Explaining and Forecasting Results of The Self-Sufficiency Project

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Abstract:

This paper studies the Self-Sufficiency Project (SSP), a controlled randomized experiment concerning welfare, by estimating a model of endogenous skill accumulation, multi-dimensional job opportunities, and time-varying opportunity costs of labor market time. Methods for estimating dynamic programming models with unobserved heterogeneity are extended to account for unexpected policy interventions and endogenous sample selection and initial conditions. Parameters are identified and consistently estimated by imposing optimal responses to the exact form of the SSP earnings supplement and the experimental program, which induces exogenous variation between treatment groups and within groups as treatment progresses. The estimated model tracks primary outcomes well in and out of sample, except for under-estimating trends in the sample of new welfare applicants. Predictions from counterfactual experiments run counter to non-structural results reported elsewhere, and they suggest that details of the SSP's design are critical for interpretation of results. The separate SSP Plus treatment may have longer lasting and more generalized impacts than the in-sample impacts suggest.

JEL Classification: I3, C9, J0, C5

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The Self-Sufficiency Project (SSP) was a controlled randomized experiment conducted in Canada 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 a full-time job was acquired and the parent went off IA within one year. The hope was that inducing sustained full-time employment would generate skill accumulation, substantial wage gains, and ultimately self-sufficiency. This expectation was partially met. About one-third of the treatment group qualified for the supplement and at the peak twice as many worked full time than the controls. However, most of the impact disappeared soon after the supplement expired. 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 labour market dynamics among low-income households.

This paper provides causal evidence on the effect of labour market policies on welfare dependency. The evidence is causal in two senses. First, welfare policy is parameterized while estimating the policy-invariant (structural) parameters of a model of household behaviour. In the model the experimental treatment causes single parents to alter their labour market activity which results in complicated patterns recorded in the SSP data. The restrictions from the forward-looking model identify time-varying and heterogeneous influences on low-income single parents. The evidence is also causal in the current sense since it comes from exogenous variation in the parents' environment due to controlled random assignment to treatment.

The SSP subsidy tries to create a path out of the "welfare trap." The strong early response followed by long-term lack of impact among the treated group indicates the depth of the trap is difficult to measure since it depends on how current earnings potential relates to labour market history. That is, how much would a person earn now if they had worked more and more steadily in the past? The explanation offered here for the welfare trap combines factors present in previous empirical work but not considered jointly before and not estimated using controlled randomized variation. Four aspects of the data drive the explanation that arises from the estimated model.

1. Going on and off Income Assistance. The welfare trap is not as deep for some parents as others. A large fraction of households on long-term IA eventually leave without incentives. To explain this requires churning and friction. Some transitions come from layoffs, but single parents also classify many separations as quits. To explain this the model allows the parent's opportunity cost of labour market time to vary over time to approximate shifts in child care arrangements, commuting costs and other factors. Persistent shifts in their leisure-income tradeoff induce parents to change work hours or quit altogether and move back to IA. IA status also changes without any change in employment. In the model, parents can access private outside support that, if accepted, makes them ineligible for IA. As with the value of leisure, the value of outside support also shifts over time which can effect a change in IA status without a change in labour market status.

2. Gradual Response to Static Incentives. The SSP treatment subsidizes labour market attachment now in order to raise earnings and make work more attractive than transfers in the future. The response to the SSP incentive was gradual. Given a year to qualify for the SSP, parents did so steadily. Without labour market frictions a short-term response would happen immediately if it were to happen at all. Frictions in the form of costly job search and job-specific limits on work hours play an important role in the estimated model. Non-working parents given an incentive to work full-time must search for a suitable job. Some succeed, some do not. Not all parents working part-time at the start of the treatment immediately qualify for the supplement by increasing their hours. Work hours are not fully flexible and jobs differ in wages and flexibility.

3. Lack of Dynamic Response. After the gradual short-term response, self-sufficiency failed to happen. Wages grew at roughly the same rate in both the treatment and control groups. Once the subsidy ended the treated parents returned to being on IA at a similar rates as the controls. The model explains the short-term success and long-term failure of the SSP to alter behaviour. If the model automatically related wage growth to employment it could not combine a strong short-term response in working with no long term response in wages. So a third important element of the explanation is heterogeneity in the growth potential of jobs. Jobs are characterized by hours flexibility, an initial wage offer and whether wages on the job respond to skill accumulated through learning-by-doing. Parents can qualify for the subsidy by finding and accepting full-time jobs, but they do not discriminate enough between "dead-end" and "stepping-stone" jobs.

4. Expectations and Heterogeneity. So far broad patterns in the data have been related to key elements of the model as if the patterns were separable, but the average outcomes are surrounded by complex distributions. Heterogeneity across households is important for matching these distributions. In addition, forward-looking households do not view these responses as separate. Movement on and off IA, the gradual short-term response to treatment, and the minimal long-term response are interconnected. What glues them together is parent foresight and anticipation, captured as usual by a non-zero discount factor in a dynamic program. A myopic household facing short-run incentives to work full time will do so, even if it may take time to find such a job. But a myopic parent has weak incentives to hold out for a good job that will pull them out of the welfare trap years in the future. The SSP provides experimental variation to estimate a distribution of discount factors in the population. For most single parents the road to self-sufficiency paved by the SSP is too long for them to respond to treatment in ways that eliminate the welfare trap.

One of the motivations for developing a multifaceted explanation of the SSP is to apply it outside the experiment. Subsequent research can use the estimated model to study a larger set of counter-factual policies and experimental designs. The applications reported in this paper focus on the experiment itself. Efficient GMM estimates of the model from the first 36 months of experimental data are used to forecast out-of-sample for the last 18 months of data. Several experiments are run at the estimated parameters to 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 the results are sensitive to the details.

The role of experimental variation for estimating models is also considered. Such variation can be used for estimation or validation. To quantify the tradeoff, standard errors are re-computed at the estimates as if data on only treated or control groups were available. The size, and in some cases the direction, of change in the standard errors is surprising. The implication of this outcome is developed below.

Michalopoulos et al. (2002) and Ford et al. (2003) summarize the findings from the SSP for all measured outcomes, but the experiment and the model developed here were designed primarily to explain what might be termed intermediate effects of income assistance policy. These relate static incentives to dynamic labour market outcomes that occur over months but not years or decades. Miller and Sanders (1997), Swan (1998) and Fang and Silverman (2004) estimate models of welfare persistence using non-experimental data emphasizing similar issues. To focus on month-to-month labour market activity, the model holds location and family size fixed.[1]

The next section describes the SSP experiment and key elements of the model with minimal notation. Details of the model are provided in section 2 and of the estimation procedure in section 3. Results and implications are described in section 4 followed by a conclusion in section 5. The extensive online Supplement provides additional details of the data, the model, the solution method, parameter identification, and outputs. Material in the supplement is indicated with an S prefix.

1. Overview

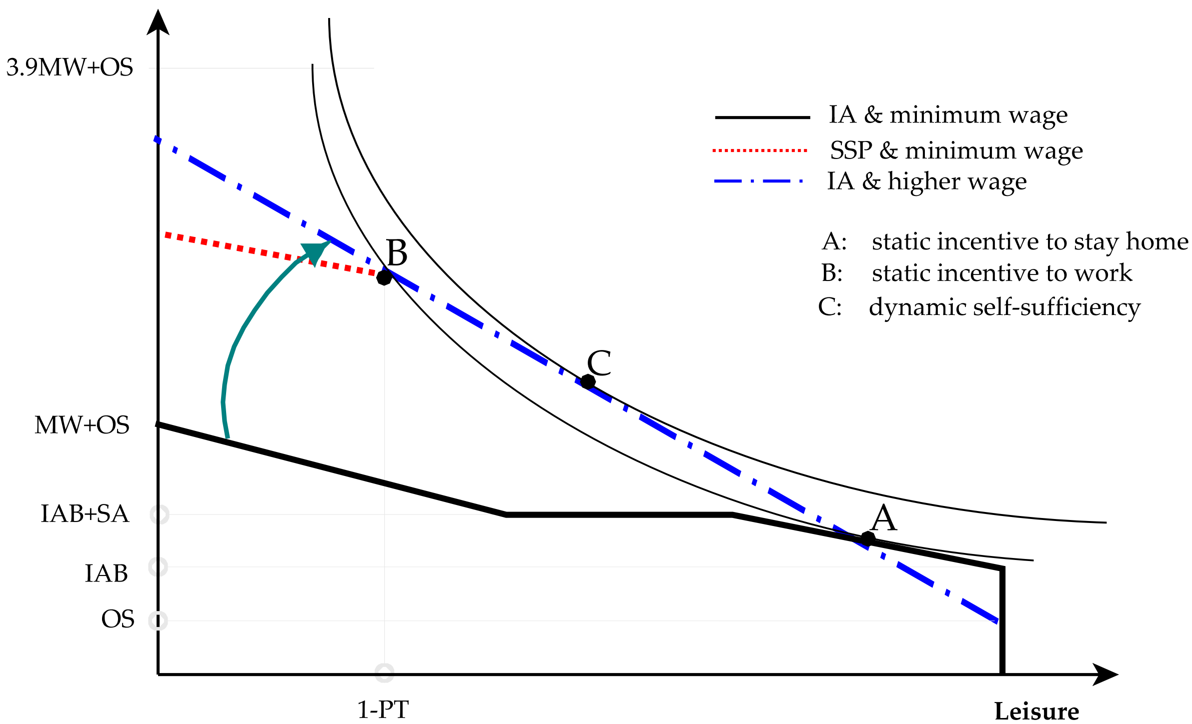
1.1 Sample Selection and Treatment in the SSP

The static tradeoffs between income and leisure created by the welfare system are illustrated in Figure 1. The details apply to Canadian income assistance programs in the 1990s but the static aspects are similar across other jurisdictions. A non-working single parent is eligible for **i**come **a**ssistance **b**enefits in amount \$IAB as long as they do not take forms of non-government **o**utside **s**upport, with value \$OS, that make them ineligible for welfare. For example, if the parent cohabitates or marries basic benefits fall substantially, possibly to \$0.[2] A working parent can **s**et **aside** earnings up to \$SA each month without a reduction in benefits. Thereafter benefits are replaced by earnings. The result is an incentive to work few or no hours (point A), especially if child care and commuting time are costly (a steep indifference curve).

1 While assumed fixed for this analysis, location and household size are treated not as strictly exogenous because the distribution of unobserved household type differs by location and household size. So policy experiments using the model can account for long run impacts in these dimensions by shifting the distributions in ways suggested by evidence from longer term studies.

2 Citing early U.S. research, Moffitt (1992) concludes, "Most exits from AFDC are not a result of an increase in earnings by the female head, but are instead the result of a change in marital status that results in the loss of AFDC eligibility."

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



IA equals $IAB when earnings are below $SA, thereafter reduced 1-for-1, discouraging full-time work (tangency A). Dynamically, this inhibits skill growth and keeps wages low. The SSP treatment subsidizes full-time work (corner solution B). It requires a parent to forgo IA, which allows them to accept ineligible transfers at value $OS. If the wage responds, the budget rotates out endogenously, making IA less attractive once treatment ends (tangency C). The estimated model allows the parent to hide a fraction of income from the authorities thereby increasing the slope of the budget. In the figure a full-time job is available, which is just one of several possible states in the model. Without a full-time job the SSP subsidy is unavailable; with no job the household's static budget is flat.

Figure 1 also displays the main SSP treatment. A treated parent retains the option of taking IA in any month, but this precludes receiving the SSP supplement. The SSP budget has slope equal to the wage until reaching full-time work hours. Then the supplement becomes available, shown as the dotted line segment and computed as half the vertical difference between earnings and 3.9 times full-time **m**inimum **w**age earnings, $MW. This particular parent is indifferent between staying on IA at point A (still a feasible choice during treatment) and working full time with the supplement at point B. This is the short-term response discussed in point 2 of the introduction.

The wage subsidy increases government transfers in order to subsidize full-time work with the premise that sustained employment induces wage growth through learning-by-doing. This dynamic response is shown as an arrow to a mixed (blue) line in Figure 1, the non-subsidized budget under a high wage job. After treatment ends the supplement budget disappears but the new optimal choice is C. The parent can balance work and home without assistance through part-time hours at a high wage rate. The temporary SSP treatment has eliminated the welfare trap through employment-induced wage growth.[3] But if the wage underlying point B does not respond to experience then the shift to the new budget does not occur, and the parent moves back to A. Which households qualify for the SSP and which ones experience wage growth depends on heterogeneity in skills, wage growth and patience (points 3 and 4 in the introduction).

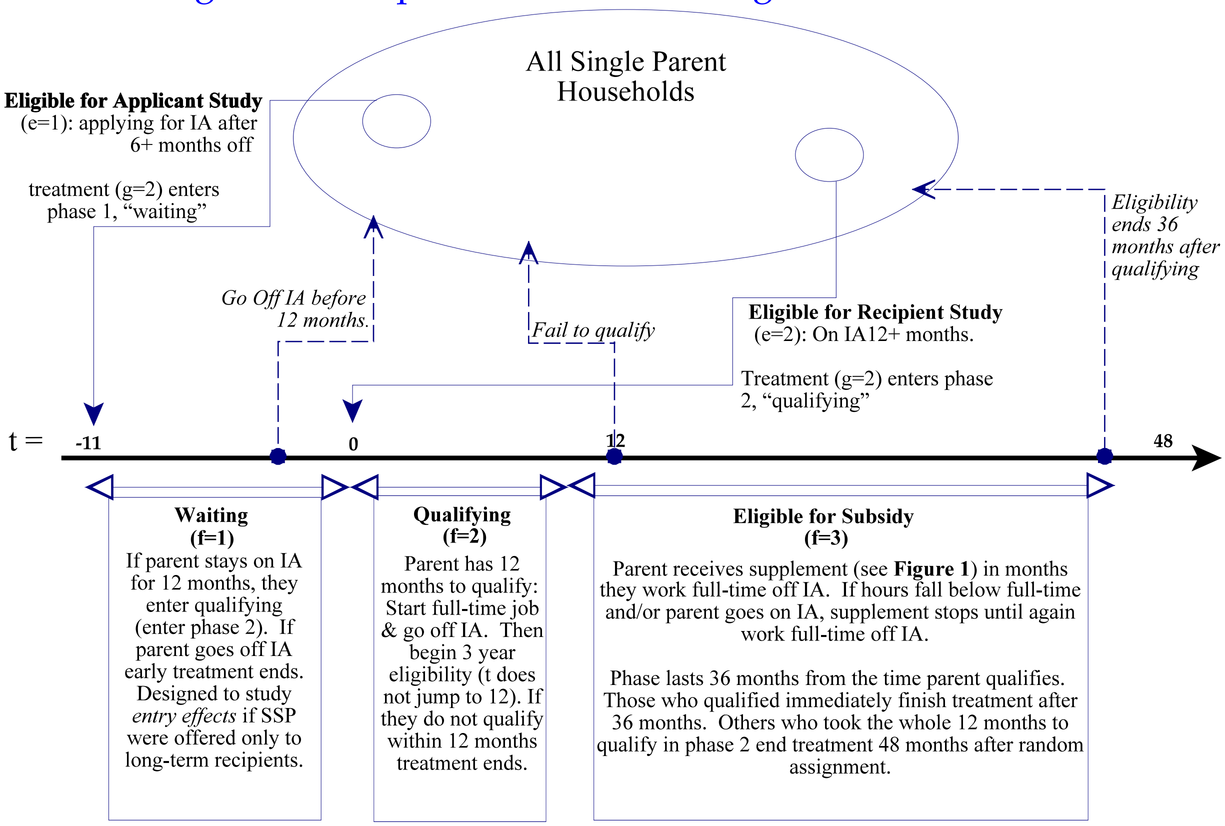
Starting with the static treatment in Figure 1 several separate experiments occurred within the SSP. First, the SSP treatment depends on minimum wages which differed between provinces. IAB (in Figure 1) depends on province and household size, and we distinguish between one and two-or-more children households. Second, parents were selected in two different ways. The Recipient Study ran in both British Columbia (BC) and New Brunswick (NB). The Applicant Study ran only in British Columbia. Within the New Brunswick Recipient Study a related treatment called SSP Plus (or SSP+) included job search counseling and other support to leave IA. Since SSP and SSP+ share a single control group, there are 4 distinct treatment groups and 3 distinct control groups across studies. That brings the total number of distinct groups subject to policy variation equal to 7x2 = 14.

Treatment took place in three phases, as shown in Figure 2. To be eligible for random assignment in the Recipient Study the parent must have been on IA for twelve or more months, shown as a subset of single parent households.[4] Treated households had twelve months to qualify for the supplement by accepting a full-time job and going off IA. Figure 2 puts the treated group on a timeline at month $t = 0$, the start of the twelve month qualifying phase. If the parent qualified they

[3] 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.

[4] This simplifies the actual condition for the Recipient Study: on IA for 12 out of the last 13 months. Kamionka and Lacroix (2003) assess non-response bias in the SSP. A possible extension to this model makes participation a choice related to the expected value of treatment relative to control. This was not included here in order to devote computational resources to correcting for endogenous selection and modeling wages and job search as realistically as possible.

Figure 2. Sample Selection and Program of Treatment



moved immediately to the eligible phase that lasted 36 months. A parent who had not gone off IA and started a full-time job within twelve months failed to qualify and their treatment ended. Otherwise, while eligible the household could return to IA and/or stop working full-time. They received no supplement in such months, and the 36-month clock continued to count down. But once working full time without IA receipt the supplement resumed. Thus the full SSP budget constraint in Figure 1 applies during the eligible phase, including the segment with IAB. After 36 months of eligibility the household returned to the status quo. The separate SSP+ treatment group followed the same schedule as the Recipient Study, so it shares the control group with the regular SSP.

A wage subsidy offered to long-term IA recipients might induce recipients to stay on IA longer so as to qualify. This entry effect would mitigate any impact observed in the experiment. Direct evidence for this effect was sought by conducting the Applicant Study. To be eligible for it a single parent had to initiate a new IA spell after six or more months away from IA. This is also graphically represented in

Figure 2 as a subset of states for the same population . The Applicant and Recipient sets do not overlap because a household could not be eligible for both in a given month (at least six months separate eligible states for a single household). Treated households in the Applicant Study were told (truthfully) that they would enter the qualifying phase if their new IA spell lasted twelve months. They are assigned to phase 1: waiting to be allowed to qualify. Since the entry point for the Recipient Study was designated $t = 0$, applicants began treatment at $t = -11$. Treatment ended if they went off IA before time 0; otherwise for $t \geq 0$ treatment was the same as in the Recipient Study.

1.2 Innovations for Analyzing Welfare and Experimental Data

The rich variation from the experiment is used to sort among several factors associated with the welfare trap. In the model the parent chooses each month whether to accept IA or not. If not, they forgo welfare stigma and can accept non-government transfers that preclude IA($OS in Figure 1). while employed the parent chooses hours of work including zero hours (quitting). While unemployed they choose whether to search actively for a job or not. Given choices and current state, the household's state next period is stochastic. It is affected by market shocks (appreciation and depreciation of skills and job offers), internal shocks (opportunity costs of labour market time), and external non-market shocks (the level of outside support).

Parents search for jobs that differ in hours flexibility, initial wages, and wage growth. Skills accumulate stochastically while working but they can also depreciate while inactive. Person-specific skill, job quality, and minimum wage levels interact in determining current wages and potential future wage growth (e.g. Gladden and Taber 2009). In short, there are multiple reasons why households end up on IA for long periods of time even while some apparently similar households come and go.

Unobserved heterogeneity is both permanent (parameters of the dynamic program) and transitory (hidden state variables). A finite mixture over types is estimated. As in Eckstein and Wolpin (1999) types differ in many dimensions and there is no interaction between exogenous observed characteristics and type-specific utility parameters. As in Ferrall (1997) the mixture proportions differ across groups, and a discount factor specific to each type is estimated. Efficient GMM estimates are obtained from means of variables conditioned on exogenous values, including time since randomization, experimental group and exogenous household characteristics. As detailed in section 2.6, eligibility for random assignment is explained with the same model as post-assignment observations. To focus on month-to-month decision making while addressing selection and incomplete histories, the household's environment outside the experiment is assumed to be stationary.[5] Within this stationary environment the finite phases of treatment create controlled exogenous variation in both budget constraints and *expectations*. For example, the "waiting" and "qualifying" phases in Figure 2 affect only expectations about contingent future utility. If single parents were completely myopic then the distributions for treated parents in these phases would be identical to their control counterparts. Instead, observed differences between these groups help identify the patience of single parents. Identification of discount factors is discussed further in section 3.2.

2. The Model

2.1 The Dynamic Program

The model of household behaviour is a discrete choice dynamic program. All parameters and state variables associated with the program are contained in a generalized state/parameter vector denoted θ. The current action of the parent is a vector α, chosen from feasible actions $\mathrm{A}(\theta)$. The combination (α, θ), referred to here as the outcome, is the argument for the one-period return or utility, $U(\alpha, \theta)$, and the stochastic transition to next month's state, $P\{\theta' \mid \alpha, \theta\}$. The combined state/parameter vector tracks different types of households assigned, unexpectedly, to a finite period of treatment.[6] It also tracks all estimated parameters of the dynamic program. Specifically, θ is composed of sub-vectors that group parameters and state variables by their role in the analysis:

$$\theta \equiv \begin{pmatrix} \theta_{\text{clock}} & \theta_{\text{exp}} & \theta_{\text{end}} & \theta_{\text{exog}} & \theta_{\text{pol}} \end{pmatrix}. \tag{1}$$

[5] *A priori* the SSP has no permanent effect in the model in the sense of lasting for eternity. But nothing stops the treatment effects from lasting, say, 40 years on average. This would be the stationary equivalent to a permanent lifetime effect. Further, several unobserved state variables are free to jump in value often or almost never. Lifecycle features such as children reaching school age can be approximated by a low probability of shifts in labour market time without tracking ages. That is, a shift in the leisure-income tradeoff is expected every sixty months this would be a stationary approximation to a five year period of child care.

[6] Many of the technical elements of how the model is constructed are available in the Supplement. In addition, Ferrall 2003 provides a complete discussion of the underlying framework.

The **end**ogenous state of the household outside the experiment is stored in θ_{end}. In a non-experimental model this would be the whole state vector. The two leftmost components of θ are added to track the experiment as illustrated in Figure 2. The vector $\theta_{\text{exp}} = (\,g \quad e\,)$ tracks the household's **exp**erimental group using indicators for **e**ntry point (Applicant or Recipient) and treatment **g**roup (control, treated, SSP+ treated). For the treated groups $\theta_{\text{clock}} = (\,r \quad f\,)$ **clock**s progress through treatment **ph**ase (f) and months **r**esiding in the current phase (r). **Exog**enous (estimated) parameters of the problem appear in θ_{exog}. **Pol**icy values (such as IAB and MW introduced earlier) appear in θ_{pol}. [7]

Using discount factor δ (an element of θ_{exog}), the value of a state θ and an outcome (α, θ) satisfy the stationary infinite horizon Bellman's equation:

$$\begin{aligned}\forall\,\alpha \in \mathbf{A}(\,\theta\,), \qquad v(\alpha,\theta) &\equiv U(\alpha,\theta) + \delta E\left[V(\theta')\right] \\ &= U(\alpha,\theta) + \delta \sum_{\theta'} P\{\,\theta' \,|\, \alpha\,,\,\theta\,\}\,V(\theta') \end{aligned} \tag{2}$$

$$\forall\,\theta, \qquad V\,(\,\theta\,) = max_{\alpha\in\mathbf{A}(\,\theta\,)} \quad v(\alpha,\theta). \tag{3}$$

Equation (2) differs from the specifications that contain additive choice-specific error terms, such as papers related to Rust (1994) in which a discrete choice dynamic program generalizes a multinomial logit. As written, the conditional probability of choosing a particular action is either 0 or 1 (ignoring exact ties). Extreme-value error terms provide a simple expression for conditional choice probabilities that are smooth in the structural parameters. With elements of (α,θ) unobserved the advantages of that specification in constructing the likelihood are lost, but smoothness is still essential for estimation. Following Eckstein and Wolpin (1999), which also allowed unobserved transitory states, choice probabilities are not smoothed by adding an error term. Instead they are smoothed directly using a logistic kernel with parameter $\rho > 0$:

$$\begin{aligned}\tilde{v}(\alpha,\theta) &\equiv \exp\Big\{\rho\big[v(\alpha,\theta) - V\,(\,\theta\,)\,\big]\Big\} \\ P\{\,\alpha\,|\,\theta\,\} &= \tilde{v}(\alpha,\theta) \quad / \sum_{\alpha^{\star}\in\mathbf{A}(\,\theta\,)} \tilde{v}(\alpha^{\star},\theta).\end{aligned} \tag{4}$$

Combining smoothed choice probabilities in (4) with exogenous outcome-to-state transitions (defined in supplement equation S12) generates the state-to-state transition, denoted $P_s\{\,\theta'\,|\,\theta\,\}$. This optimal transition is computed for status quo and for every stage of the experimental treatment.

[7] The values of policy parameters and the model of treatment phases are described in S6.2.3.

2.2 State and Decision Variables

Within the framework described above, a household's situation outside the experiment is described by nine state variables contained in the endogenous vector:

$$\theta_{\text{end}} \equiv (\, l \quad p \quad n \quad x \quad b \quad s \quad h \quad d \quad k\,). \tag{5}$$

Reading from right to left, k is the parent's unobserved type and d its observed **d**emographic group. These two values vary for a household over time. The next five variables are the key time-varying states. Briefly they are indices for: the opportunity cost of time outside the **h**ousehold; the level of available non-governmental or family **s**upport; the upper **b**ound on hours in the current job (0 if not employed); the parent's skill based on previous e**x**perience; and the ear**n**ings offer in the current job. The two leftmost variables, l and p, do not enter the utility or transition. They are extra state variables tracked solely to match SSP results and identify key parameters directly from the data. Namely they indicate the parent worked in the **p**revious month and **l**ost their previous job.

Each of the $D = 4$ demographic groups (province and household size) has a vector of policy parameters, $\Psi_p[d] \equiv (\,\text{IAB}_d \quad \text{MW}_d\,)$, illustrated in Figure 1. Within demographic group, unobserved type k is distributed across $K = 4$ types according to $\Lambda[d]$, a vector with elements $\lambda[d,k]$.[8] For example, $\lambda[1,2]$ equals the proportion of type 2 households in group 1 (New Brunswick, one child) households.

The action vector has three variables,

$$\alpha \equiv (\, m \quad a \quad i\,), \tag{6}$$

representing labour **m**arket hours, **a**ctive job search, and acceptance of **i**ncome assistance. The i and m choices are observed but active job search is not. The feasible set $\mathbf{A}(\,\theta\,)$ imposes two restrictions. First, active job search while working is

[8] There is a computational tradeoff between more distinct types (greater K) and allowing more parameters to vary across types. Initially K was set to two and only a few parameters differed with k in order to locate reasonable values. An arduous process followed of alternately iterating on the objective then adding elements to the model to address major issues. For example, the bound on hours was added so that not all part-time workers are predicted to immediately qualify for the SSP by switching to full-time. A final specification was chosen that allowed nearly all parameters to vary by type. Only parameters related to unobserved household costs and outside support were held common across type. This flexibility dictated a low value of K, with 4 being at the limit of available computational resources.

ruled out: $m > 0$ or $a = 1$, but not both.[9] Second, the parent faces an upper bound on work hours: $m \leq u(b)$ where b is specific to the current job. When the parent has no job ($b = 0$) work is not available, and $u(0) = 0$. With a part-time job ($b = 1$) they can only work less than $u(1)$ =PT=75% of full-time hours. When holding a full-time job ($b = 2$) the parent can chose to work any number hours ($u(2)$ =FT= 100%). A parent with a job who sets $m = 0$ quits and loses the option to work until a new job is offered and accepted.

2.3 Utility, Skill and Wages

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

$$U(\alpha,\theta) = \text{Income}(\alpha,\theta) + \text{OS}(\alpha,\theta) - \text{C}(\alpha,\theta). \tag{7}$$

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

$$\text{Income}(\alpha,\theta) \equiv \text{TrueEarn}(\alpha,\theta) + \text{IA}(\alpha,\theta) + \text{SUP}(\alpha,\theta). \tag{8}$$

The components of income are defined as:

$$\begin{aligned}
\text{TrueEarn}(\alpha,\theta) &\equiv mW(\alpha,\theta) \\
\text{Earn}(\alpha,\theta) &\equiv (1-\beta i)\,\text{TrueEarn}(\alpha,\theta) \\
\text{IA}(\alpha,\theta) &\equiv i\max\Big\{\text{IAB} - \Big(\text{Earn}(\alpha,\theta) - \min\{\,SA\,,\,\text{Earn}(\alpha,\theta)\,\}\Big)\ ,\ 0\Big\} \\
\text{SUP}(\alpha,\theta) &= \mathcal{B}\,[f=3\ \&\ i=1\ \&\ m>\text{PT}]\max\left\{0\,,\,\frac{1}{2}\,[3.9\times\text{MW} - \text{TrueEarn}(\alpha,\theta)]\right\}.
\end{aligned} \tag{9}$$

True earnings equal work hours (as a fraction of full-time work) times the full-time equivalent wage $W(\alpha,\theta)$ defined below. When a parent is on IA ($i = 1$) earnings in the data and reported to the welfare authorities are a fraction β of TrueEarn. Underreporting of income is encouraged by the 100% implicit tax above the set aside amount, and allowing for it in (9) helps explain the sizeable fraction of households working while receiving IA. SUP(α,θ) includes the algebraic version of the supplement illustrated in Figure 1 with a factor in front that indicates the person is eligible. Here $\mathcal{B}\,[x]$ denotes the Boolean (0/1) value of x (often referred to as the indicator I_x). So the first term in SUP is one if the parent is in the eligible phase, is off IA, and is working fulltime.

[9] Job-to-job transitions are treated as the same job, so passive search while working is allowed. The model attributes growth in full-time equivalent earnings between contiguous jobs as skill acquisition.

Non-government transfers and additional utility (in dollar equivalent) from forgoing IA equals

$$\mathrm{OS}(\alpha,\theta) \equiv (1-i)s\Big[\xi\mathrm{IAB}\Big]. \tag{10}$$

The transfer component of OS is support from others that, if accepted, disqualifies the parent for IA. Marriage and cohabitation (detected by the welfare authorities) are both implicitly included in OS. 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 labour market status. A drop in s may push the parent back to receiving IA.

As with many job search models, utility is linear in income and separable in leisure. But unlike basic job search models $C(\alpha,\theta)$ is time-varying and non-linear in hours, resulting in shifting static preferences for work hours. To keep time-costs, $C()$, in the relevant range during estimation it is expressed as a fraction of maximum possible earnings for this type of person, $W_{\max}$, which is defined below. Specifically, the cost of labour market time,

$$C(\alpha,\theta) = W_{\max}\nu\left[m+\kappa a\right]^{c(h)}, \tag{11}$$

depends on work hours and search when not working, which is converted to work time by the exogenous parameter κ. These preferences explain changes in hours and related movement on and off IA as highlighted in the introduction. The curvature is determined by $c(h)$ which shifts with the state variable h. This functional form is explained in detail in S6.2.2 and can be viewed as monetary costs of child care, commuting, and other employment costs. Non-linearity allows for non-market options for child care including school time and care by relatives.[10]

2.4 Skill, Job Search and Wages

$W(\theta)$ denotes full-time equivalent monthly earnings. Wages are determined by worker skill (x), job offer index (n), and minimum wage MW. Skill takes on four values, $x = 1/4, 1/2, 3/4, 1$. From month to month x either remains constant, **a**ccumulates with probability $m\pi_a$ while working or **d**ecreases with probability π_d while not working. If $\pi_a = \pi_d = 0$, endogenous skill accumulation and depreciation

[10] The *range* of $C(\alpha,\theta)$ is $[0, W_{\max}]$ and for $\mathrm{OS}(\alpha,\theta)$ is $[0, \xi IAB]$. The forms ensure that the state-dependent values stay within their relevant ranges while exogenous parameters are varied during estimation. Otherwise current values of ξ and ν can end up in regions where choice probabilities are flat.

are eliminated, and x becomes a permanent random effect for the parent in addition to their fixed type k. The standard Mincer earnings function essentially assumes $\pi_a = 1$ and $\pi_d = 0$. That is, skill accumulates log-linearly and deterministically with experience and does not depreciate when not working. This rules out a suspected culprit for the welfare trap: the decay of potential wages due to a temporary absence from the labour force which then prolongs the absence. When $\pi_a < 1$ and $\pi_d > 0$ transient conditions can persist. Even when the parent is forward-looking and sees the skill investment value of work, the rate of wage growth and/or their degree of patience may be too slow to make work pay.

To see how $W()$ goes beyond Mincer in this and other ways, start with the case MW $= 0$, for which $W()$ collapses to $W^0()$:

$$W^0(\theta) \equiv \begin{cases} \exp\{\mu + \sigma\Phi^{-1}(n/6) + \eta x\} & \text{for } n > 0 \text{ with probability } (1-\pi_m)/5 \\ 0 & \text{for } n = 0 \text{, with probability } \pi_m. \end{cases} \quad (12)$$

$\Phi()$ is the standard normal distribution. Discrete job offers are indexed by n, which takes on values 0 to 5. Conditional on an offer, real (positive) offers occur when $n > 0$ which happens with probability $1 - \pi_m$. The offers are derived from a log-normal distribution with type-specific parameters μ and σ. This distribution is discretized by associating n with the $100(n/6)^{th}$ quantile of the log-normal distribution. Worker quality also contributes to the wage through the parent's current acquired skill, x, and the estimated return η. The offers with index $n = 0$ are jobs that pay nothing absent a minimum wage, but it will be assumed if taken the parent could still increase x while working at such a job.[11] The relatively simple form in (12) does not account for the distortion of a minimum wage. It is generalized to move the mass point from 0 to MW > 0 and distort the underlying offer distribution. Loosely speaking, the fully wage function assumes workers whose productivity is below MW are first overpaid and then, as x increases, they are under-paid. This captures some elements of a bargaining or contracting model of minimum wages such as Flinn (2006). Certain low-quality job/worker pairs pay MW.

Offers of type $n = 0$ now pay MW regardless of skill. The existence of such jobs inhibits but does not rule out self-sufficiency.[12] Let ϕ_x denote the fraction of the

[11] With nine state variables the number of values any particular variable can take on is limited. A balance was struck with n having the most distinct values (see Table). Otherwise the predicted wage distribution would be very coarse making it difficult to pin down μ and σ.

[12] This assumption allows even low-skill jobs to transmit good habits such as showing up on

underlying log-normal offer distribution below MW:

$$\phi_x = \Phi\left[\frac{\ln(\mathrm{MW}) - \eta \ln(x) - \mu}{\sigma}\right]. \tag{13}$$

For $x = 1/4$ it is assumed the lowest two regular offers (n=1,2) 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 $n = 1$ offer pays MW and it occurs 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 at θ:

$$\mathrm{M}(n,x) \quad = \quad \mathcal{B}\,[\,n=0\,] \;+\; \mathcal{B}\,[\,x=1/4\;\&\;1\leq n\leq 2\,] \;+\; \mathcal{B}\,[\,x=2/4\;\&\;n=1\,]\,. \tag{13}$$

That is, $\mathrm{M}(n,x)$ indicates the parent currently works at minimum wage. Such jobs are heterogeneous in their growth potential based on both the job characteristic n and the person characteristic x. Otherwise the wage exceeds MW, and each such offer is equally likely for a given x. Using $\tilde{n}(x) = 3+\mathcal{B}\,[x > 1/4]+\mathcal{B}\,[x > 2/4]$ to denote the number of offers above MW for skill level x, the general expression for full-time earnings that interacts skill, job quality and minimum wages is:

$$\begin{aligned} W\,(\,\theta\,) &= \mathrm{M}(n,x)\mathrm{MW}+ \\ &\qquad (1-\mathrm{M}(n,x))\left(x^{\eta}\exp\left\{\mu+\sigma\Phi^{-1}\left(\phi_x+(1-\phi_x)\,/\tilde{n}(x)\right)\right\}\right) \\ W_{\max} &\equiv \exp\left\{\mu+\sigma\Phi^{-1}\left(\phi_1+(1-\phi_1)\,/5\right)\right\}. \end{aligned} \tag{14}$$

Although this formula may seem arbitrary, it is (ex post) easy to explain and fairly intuitive. For high x only $n = 0$ offers start at MW and all other offers provide wage growth with each increment of skill (until $x = 1$). The lower a person's skill while searching the more likely they are to be offered MW initially and the longer they will stay at MW even if employed steadily. Regardless of job quality, skills do accumulate (stochastically) and eventually steady work results in high skill. But some "dead-end" jobs provide no direct return to skill, and it may be optimal to quit them to search for a better job. In addition, by fixing the job indices that pay MW for each skill level but allowing offer probabilities to differ across n, (14) has the important property that it is continuous in the estimated parameters μ and σ.

time. But to take advantage of such learned skills the parent must quit the job and find another with $n > 0$. This feature helps explain why large fractions of all groups are on minimum wages throughout the sample period.

2.5 SSP Plus and Offer Probabilities

The SSP Plus treatment ($g = 1$) offered the supplement plus employment services. This additional treatment is not represented in Figure 1 which presumes a job is available. The model allows that employment services enhance active search by raising the job offer probability. Ordinarily a month spent in active search ($a = 1$) generates an offer with probability π_j, a type-specific estimated parameter. The effectiveness of the SSP+ treatment is captured by another estimate parameter, π_p. Then, in general, the offer probability is a function of the outcome:

$$p_j(\alpha, \theta) = \begin{cases} a\left(\pi_j + \pi_p(1-\pi_j)\right) & \text{if } g = 1 \ \&\ 2 \leq f \leq 3 \\ a\pi_j & \text{otherwise.} \end{cases} \tag{15}$$

The treatment parameter determines how much the services improve the chance of a job offer. If $\pi_p = 1$ then any month of active search generates an offer ($p_j = 1$). Holding constant state-contingent choices, parents in SSP+ treatment receive more offers than in the regular treated group, but they can also respond to SSP+ employment services by searching actively in states and reject offers they would not have otherwise. The model is thus restrictive by tying all predicted differences between SSP and SSP+ treatment groups in New Brunswick to a single type-specific parameter.

2.6 Endogenous Eligibility for Random Assignment

If the SSP had selected single parents at random the initial conditions for both the treatment and control groups, the experimental results would reflect the distributions across unobserved states for the target population. Instead, as illustrated in Figure 2, both the Recipient and Applicant Studies selected parents conditional on past receipt of IA. The population eligible for random assignment is therefore endogenous to the policy and the behaviour being studied. The exogenous share of type k in group d, $\lambda[d, k]$, does not equal the share of k eligible for random assignment. And the distribution across endogenous states is different at random assignment than in the general population. Correcting parameter estimates so that they apply to the wider population of single-parent households, not just the select subsets illustrated in Figure 2, is essential for policy relevance.

To make controlling for endogenous eligibility feasible the environment is made stationary outside of treatment.[13] The state-to-state transition, $P_s\{\theta' \,|\, \theta\}$ in Supplement equation (S13), combines the primitive transition and smoothed optimal

[13] To my knowledge no previous research has used a model of forward-looking agents to deal

choice probabilities. Following Ferrall (2003) the primitive transition is specified so that, outside of treatment, the state-to-state transition is ergodic. That is, a unique stationary distribution over $\theta_{\rm end}$ exists for all exogenous parameters $\theta_{\rm exog}$ in the interior of the parameter space. Let $P_\infty\{\theta\}$ denote this distribution. Having solved the infinite horizon problem (2)- (3), this distribution is computed exactly over the discrete state space for each combination of demographic group d and unobserved type k. From this distribution eligibility criteria for the Applicant and Recipient Studies are applied sequentially.

Consider first the Recipient Study, which required at least twelve months of IA receipt ($i = 1$). Only choices in the outside world with $i = 1$ keep a parent eligible, resulting in choice probabilities that generate a state-to-state transition different than $P_s\{\,\theta' \,|\, \theta\,\}$. Starting from $P_\infty\{\theta\}$ this transition is imposed, resulting in a new distribution across endogenous states, representing households in a cross-section that have been on welfare at least one month. This transition does not preserve the size of the population as it drops households off IA ($i = 0$). The amount of leakage is recorded, and then the mass is rescaled to 1 to continue selection (equation S15). The conditional transition is applied again to form the distribution of households on welfare at least two months. Repeated twelve times, this produces the distribution of households eligible for random assignment in the Recipient Study. This sequential conditioning is repeated for all k in group d. Using the total leakage over the 12 selection months the share of type k eligible for assignment to treatment is computed (equation S16) and used in generating post-assignment predictions (section 3.2). For the Applicant Study the procedure is carried out for its seven months of criteria and implied transitions: a parent must be off welfare ($i = 0$) for six months then initiate a new claim ($i = 1$).

Thus the model accounts not just for responses to treatment but for selection on unobservables, both time-varying and time-invariant. The distribution across skills, job offers and household costs is different than in the general population of single parents and unique to each study and demographic group. The mix of permanent types eligible for the experiment is not the same as the exogenous (and estimated) type proportions in the general population. No ad hoc auxiliary model is used to account for initial conditions. Smoothed choice probabilities and

with unobserved histories and endogenous selection. Attanasio et al. (2005) estimate a model of educational attainment using a cross-section of data from the Progresa experiment. They control for initial conditions using a separate reduced-form model.

computation (not simulation) of the sequence of distributions across the finite state space maintains continuity of predictions in the estimated parameters.

3. Estimation

3.1 Estimated Parameters

All estimated parameters appear in θ_{exog} and can be grouped by which part of dynamic program they enter. Parameters of $U()$ are placed in a type-specific vector,

$$\Upsilon \equiv (\,\beta \quad \eta \quad \kappa \quad \mu \quad \nu \quad \sigma \quad \zeta \quad \xi\,)\,. \tag{16}$$

Recapping the previous discussion, β is the rate of income reporting; η is the curvature in skill; κ converts search time into work time; ν is the (scaled) income-equivalent cost of full-time work; μ and σ determine location and spread of wage offers; ζ determines the variance in the curvature of time-costs over time; and ξ is the factor on outside support.

Transition parameters are also grouped together:

$$\Pi \equiv (\,\pi_j \quad \pi_m \quad \pi_f \quad \pi_h \quad \pi_i \quad \pi_d \quad \pi_l \quad \pi_s \quad \pi_p\,)\,. \tag{17}$$

Referring to each probability by its subscript, j is the probability that active **j**ob search generates a job offer (in the absence of SSP+); m is the proportion jobs that are pure **m**inimum wage jobs; f is the proportion of job offers that are **f**ull-time jobs; h is probability that the curvature in **h**ousehold costs changes; a is the probability that skills **a**ccumulate while working; d is the probability that skills **d**ecreases while not working; l is the probability that the parent is **l**aid off exogenously; s is the probability that outside **s**upport changes; and p is the index of SSP**+** effectiveness.

Then $\Gamma[k] \equiv (\,\delta_k \quad \rho_k \quad \Upsilon[k] \quad \Pi[k]\,)$ contains all parameters that determine behaviour of type-k parents. The exogenous parameter vector contains those $K = 4$ vectors and the type proportions: $\theta_{\text{exog}} = (\,\Lambda[1] \quad \cdots \quad \Lambda[4] \quad \Gamma[1] \quad \cdots \quad \Gamma[4]\,)$. For a household of type k in group d only $\Lambda[d]$ and $\Gamma[k]$ are relevant. There are $N = 19$ parameters in $\Gamma[k]$ leading to a total of $K(D+N) = 4(4+18) = 88$ exogenous parameters. Fewer parameters are free. Three parameters in $\Gamma[k]$ are constrained to be equal across type, on the presumption that they are the least likely to be identified by variation in observables and therefore most reliant on functional form assumptions. And the four elements of $\Lambda[d]$ must sum to 1 for all d. The result is $3 + 4(19-3) + 3(4) = 79$ parameters estimated from the data, all contained in θ_{exog}.

3.2 Measurements and Sources of Identification

Ideally the exogenous vector θ_{exog} would be estimated using a long panel on individuals and observing the full outcome (α, θ) starting from fixed initial states. In this case, applying endogenous choice probabilities sequentially along a single stochastic path will form the likelihood. However, two aspects of the SSP preclude this approach. First, full histories of the subjects are not available. Second, a realistic model of the welfare trap includes elements unobserved by the econometrician, such as skill, job quality, and time-varying leisure-income tradeoffs. When the realized path is not full observed the likelihood requires multiple summation across states and choices weighted by time-varying endogenous probabilities. Here, estimation is based not on (α, θ) directly but on a vector of measurements denoted $Y(\alpha, \theta)$ and described in section 3.2. Some state and action variables are elements of Y and thus directly observed. Others, such as current skill, influence observables such as earnings but cannot be observed or inferred from Y. While not ruling out maximum likelihood estimation, this approach is a natural basis for GMM. The predicted expected are matched to observed average values of $Y(\alpha, \theta)$ conditioned on instruments. Four observed exogenous variables generate instruments, including three that are fixed for an individual: $\tilde{\theta}_{\text{cond}} \equiv (g \quad e \quad d)$. This vector includes the person's the demographic group d and the elements of θ_{exp} defined earlier and consisting of the entry (Applicant or Recipient) and treatment (control, SSP, SSP+) groups. There are 14 different values of $\tilde{\theta}_{\text{cond}}$. As shown in Table 1, 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 Study includes 292 people in New Brunswick assigned to treatment. Roughly one-half of the households had more than one child at the baseline.[14]

The fourth conditioning variable is time t, which appeared in Figure 2 but does not appear in the parent's state/parameter vector because it plays no direct role in the model solution only in the construction of predictions and observations. It is appended to the fixed conditional vector $\tilde{\theta}_{\text{cond}}$:

$$\theta_{\text{cond}} \equiv \begin{pmatrix} t & \tilde{\theta}_{\text{cond}} \end{pmatrix} = (t \quad g \quad e \quad d) \tag{18}$$

to coordinate measurements across groups and track time during endogenous el-

14 More details and descriptive discussions of the data are available in the Supplement.

Table 1. Demographic, Treatment, and Experimental Groups

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

Total observations used = 8898. Observations dropped due to invalid or missing age, high school attendance or number of children.

igibility for assignment. 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. Province and household size are exogenous by assumption. Since the distribution of preference parameters is specific to each group, d can be interpreted as lagged endogenous and possibly sensitive to certain policy changes. After controlling for endogenous eligibility for randomization entry group e and treatment group g are randomly determined. Experimental time t increments each month regardless of decisions so it also is exogenous and acts as an instrument in two ways. First, the model predicts that control groups will re-approach their stationary distributions across states, P_∞. The drift in the data is matched to the model's prediction as correlated with t. Second, t is correlated to endogenous choices that determine the experimental clock, θ_{clock}, since each phase of treatment has a maximum finite length and explicit transition rules. The model relates behaviour directly to θ_{clock} and then indirectly to t through the distribution across states at each t.

The expected observation given the conditioning vector is written:

$$E\Big[Y \mid \theta_{\text{cond}}, \theta_{\text{exog}}\Big] = \sum_{k=1}^{4} \lambda^\star\left(k, \tilde{\theta}_{\text{cond}}\right)\left[\sum_{\theta_{\text{end}}, \theta_{\text{clock}}} \Omega\{\theta \mid k, \theta_{\text{cond}}\} \sum_{\alpha \in A(\theta)} P\{\alpha \mid \theta\} Y(\alpha, \theta)\right]. \tag{19}$$

The inner right-most summation is what the parent expects their measurement vector to be at the beginning of the month, as it conditions on θ and averages over state-contingent choice probabilities, $P\{\alpha \mid \theta\}$ defined in (4). In turn this is averaged over states using the distribution $\Omega(\cdot)$ defined in equation (S15), which accounts for endogenous eligibility, unobserved states, and the evolution of states since random assignment. The term $\lambda^{\star}(\cdot)$ from (S16) is the fraction of eligible households in group $\tilde{\theta}_{\rm cond}$ of type k. It is a reweighing of $\lambda[d,k]$ that accounts for the differences across unobserved types in being eligible for random assignment in entry group e. Parents in a control group act as they do outside the experiment (conditional on their current state). For them Ω differs from the stationary distribution P_∞ because of non-random sampling. Post-assignment their choices lead Ω to converge back to P_∞. Among the treated the distribution can continue to diverge from its stationary value until all individuals are past the final stage of treatment.

Let $\hat{E}[Y \mid \theta_{\rm cond}]$ denote the vector of average observed (empirical) results for $\theta_{\rm cond}$. The 12 contemporaneous variables chosen for Y are summarized in Table 2. Many of the moments were chosen because they are key outcomes of interest in the SSP and social assistance policy in general, including earnings, rates of IA receipt, full-time work, lay-offs, and quits. Others were chosen to help identify specific parameters of the model (see S6.3). The model makes a prediction for every combination of variables in $\theta_{\rm cond}$. So a 0/1 indicator for each value of the vector is itself a valid instrument in the usual sense. Thus the number of instruments is not three or four, based on the dimension of $\theta_{\rm cond}$, but rather the number of unique values of $\theta_{\rm cond}$ in the data, over 400. Thus, there is a large amount of variation that is either strictly exogenous or conditionally exogenous. The measurement vector $Y(\alpha, \theta)$ contains twelve values that interact choices with endogenous states. Interacting measurements with $\theta_{\rm cond}$ results in a total of 5374 total moments.

With the complex utility and transitions it is not possible to demonstrate formal identification of all parameters. Consider here only the identification of the discount factors δ_k from the experimental treatment. Rust (1994) shows the discount factor is non-parametrically unidentified in the case of infinite horizon discrete choice models. In many parametric applications the discount factor is still so poorly identified it is fixed *a priori*. However, Wolpin (1995) provides a formal proof that the discount factor is parametrically identified in a job search model with an exogenous finite horizon. Intuitively the change in behaviour leading up to the final decision period reveals the patience of the decision maker if other parameters

Table 2. Experimental Results (Moments) Selected for Matching

Var.	Description	Model	Unit	Count	Mean	St.Dev	Note
earn	Reported Earnings	(1- i)mW	\$100	470	3.439	1.564	
earnsq	Earnings Sq.	earn^2	\$100^2	470	62.59	43.470	
ia	IA Received	IA	\$100	466	5.966	1.869	Fwd.2 mth
iasq	IA Recv Sq.	IA^2	\$100^2	466	57.78	27.593	Fwd.2 mth
gsu	SSP Suppl	SUP	\$100	240	1.530	0.600	Fwd.2 mth
onia	Received IA	i	0/1	470	0.708	0.161	
mwg	Min. Wage Job	(n<6-#n)(m>0)	0/1	470	0.777	0.059	wage < MW+\$.10
leftjb	Left/quit a job	p(l=0)(m=0)	0/1	456	0.003	0.004	Excl. job-to-job
lossjb	Lost 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 <= PT	0/1	470	0.130	0.023	
onXem	IA & Working	(ia) (m>0)	0/1	470	0.161	0.045	

The complete data used in estimation is available in the supplement. Count is number of cells. Total count = 5374. 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. Fwd. 2 months means values in month t come from month $t+2$ in the data to account for the lag in verifying status and then paying and recording the supplement.

are identified from other data (such as accepted wages). The fixed length of each phase of SSP treatment plays a role similar to an exogenous finite horizon. Within a treatment phase utility shifts dramatically when the phase shifts, which occurs according to rules built into the model. The experimental clock θ_{clock} is counting down in each phase of treatment. Further, we observe identical control groups with no such finite deadlines, providing additional variation on expectations.

Finite phases of treatment made running the SSP tractable with the unintended benefit of providing exactly the kind of dynamic variation required to identify how patient the single parents are when making decisions. The small standard errors on δ_k and most other parameters reported later suggest the heuristic explanations are relevant for the sample. And when computing standard errors using data from within groups the change in standard errors confirms that identification of δ_k comes from the non-stationarity of the SSP treatment.

3.3 GMM Estimation

The exogenous vector is estimated by minimizing the weighted distance between observed and predicted values of the conditional moment vectors:

$$\Delta\left(\theta_{\text{cond}}\right) \equiv \hat{E}\left[Y \mid \theta_{\text{cond}}\right] - E\left[Y \mid \theta_{\text{cond}}, \hat{\theta}_{\text{exog}}\right]. \tag{20}$$

The objective in the first stage of the efficient GMM estimation procedure is

$$Z^1\left(\hat{\theta}_{\text{exog}}\right) \equiv \sum_{\tilde{\theta}_{\text{cond}}} \sum_{t=t_0(e)}^{t_{\max}(e)} \frac{n(\theta_{\text{cond}})}{265159} \Delta\left(\theta_{\text{cond}}\right)' \Sigma_0 \Delta\left(\theta_{\text{cond}}\right), \tag{21}$$

where Σ_0 is a 12×12 diagonal matrix.[15] The terms t_0 and $t_{\max}$ denote the time of random assignment in entry group e and the maximum observed month, respectively.

Let $\hat{\theta}^1_{\text{exog}}$ denote the parameters that minimize Z^1. Measurements for an individual are correlated across t. Across treatment, entry, and demographic groups the measurements are independent. So the population covariance of moments is block diagonal with the blocks defined by $\tilde{\theta}_{\text{cond}}$. $\Delta\left(\theta_{\text{cond}}\right)$ is stacked across t to form $\tilde{\Delta}\left(\hat{\theta}_{\text{exog}}, \tilde{\theta}_{\text{cond}}\right)$. The weighting matrices for these long vectors in the second stage are the inverse of the variance matrix of the moments, $\Sigma\left(\tilde{\theta}_{\text{cond}}, \hat{\theta}^1_{\text{exog}}\right)$, estimated by simulation using $\hat{\theta}^1_{\text{exog}}$.[16] The second stage objective is

$$Z^2\left(\hat{\theta}_{\text{exog}}\right) = \sum_{\tilde{\theta}_{\text{cond}}} \tilde{\Delta}\left(\hat{\theta}_{\text{exog}}, \tilde{\theta}_{\text{cond}}\right)' \Sigma\left(\tilde{\theta}_{\text{cond}}, \hat{\theta}^1_{\text{exog}}\right) \tilde{\Delta}\left(\hat{\theta}_{\text{exog}}, \tilde{\theta}_{\text{cond}}\right). \tag{22}$$

Let $D\left(\hat{\theta}_{\text{exog}}, \tilde{\theta}_{\text{cond}}\right)$ denote the matrix of gradients for the vector $\tilde{\Delta}$ with respect to the estimated parameters. The variance of the estimated parameter vector is computed as

$$\hat{Var}\left[\hat{\theta}_{\text{exog}}\right] = \left\{\sum_{\tilde{\theta}_{\text{cond}}} D\left(\hat{\theta}_{\text{exog}}, \tilde{\theta}_{\text{cond}}\right) \Sigma\left(\tilde{\theta}_{\text{cond}}, \hat{\theta}^1_{\text{exog}}\right) D\left(\hat{\theta}_{\text{exog}}, \tilde{\theta}_{\text{cond}}\right)'\right\}^{-1}. \tag{23}$$

Standard errors for $\hat{\theta}_{\text{exog}}$ are based on (23). Two other sets of standard errors are reported below. In one, control groups ($g = 3$) are excluded from the sum over $\tilde{\theta}_{\text{cond}}$; in the other set treatment groups ($g = 1, 2$) are excluded. The standard errors are still evaluated at the parameter values found when including all the data, so these calculations measure how within- and between-group variation contributes to identification of the parameters.

[15] For the monetary values in Table 2 the weight is 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 $n(\theta_{\text{cond}})$ sum to 265,159 and are listed in Table S6.13.

[16] Note that to calculate the predicted moments in (19) simulation is not used. The full distribution across all states is stored for each t starting from the stationary distribution. This preserves smoothness of the objective in the estimated parameters. Since the weights are computed only once for one vector smoothness is not required. Details of the simulation are in S6.6.2.

4. Estimates and Implications

4.1 Parameters and Predicted Outcomes

Table 3 reports the efficient GMM parameter estimates.[17] The estimated mixing probabilities 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. Types have very different levels of patience. A period is one month, so only for the first two types is δ_k close to 1.0. The other types make decisions close to a static manner: next year's outcomes have essentially no impact on today. The income reporting parameter β 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 as does the stigma associated with welfare (captured by the coefficient on outside support, χ). Full-time work has a very similar cost across type (ν), 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).

The bottom panel of Table 3 reports the transition shifters Π. 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 $n = 0$ jobs (with no on-the-job growth potential). Estimates of the home environment indicate that outside support is highly persistent (π_s is small) but household costs of work and job search are not (π_h is high). Type 1 workers get a skill increment each period and have rapid on-the-job wage growth. For other types growth is intermittent. Type 3 is the only one with further skill accumulation expected after one year of working (based on the increment probability and four skill levels). Because average wages do not accumulate in the treatment group, this suggests that the return to skill (η) reported

[17] Since there are no coefficients on observed variables few of the magnitudes are easy to compare with results based on, say, a Mincer earnings function. The discussion of the estimates themselves is brief in favour of a longer discussion of the model's prediction. The transition parameters are probabilities, but those related to skill, job offers and household costs affect unobservables, and the magnitude depends on the number of values the corresponding state variable takes on. Adding points to make the discrete grid finer would make the expected change from a jump smaller and the estimated jump probability would go up.

Table 3. $\hat{\theta}_{\text{exog}}$: Estimated Parameters

Variable			Description	Type Index (k) 1	2	3	4
Type Prop. Λ	dem. group	1	NB, One Child	0.0063	0.8216	0.0911	0.0810 -
		2	NB, Two+ Children	0.000003	0.2072	0.3253	0.4675 -
		3	BC, One Child	0.5228 *	0.00009	0.4771 *	0.0000009 -
		4	BC, Two+ Children	0.5736 *	0.00009	0.4263 *	0.00002 -
DP	Parameters	δ	Discount Factor	0.9999 *	0.934 *	0.476	0.742 *
		β	Income Reporting	0.399 *	0.413 *	0.955 *	0.391 *
		ρ	Smoothing	36.220 *	99.192	11.507	6.103 *
Utility Shifters (Υ)	Market	μ	Job Offer Mean	-1.560 *	-0.072	0.025	0.020
		σ	Job Offer St. Dev.	1.999 *	1.632 *	1.825 *	1.608 *
		η	Return to Skill	1.355 *	2.964	31.685	7.070 *
	Home	χ	Outside Support	1.427 *	1.080	1.535 *	0.825 *
		ν	Cost of FT Work	0.346 *	0.409	0.447 *	0.487 *
		κ	Cost of Job Search	0.461 *	0.0007	0.000002	0.081 *
		ζ	1 / Mean Convexity	2.997 *			
Transition Shifters (Π)	Market	j	Job Offer (b>0)	0.069 *	0.730	0.99994 *	0.831 *
		f	Prop. Full Time	0.999996 *	0.889 *	0.99955 *	0.902 *
		m	Prop. MW job (n=0)	0.131 *	0.439	0.531	0.249 *
		l	Job Loss	0.021 *	0.0011	0.018 *	0.002
		p	SSP Plus Effect	0.823 *	0.009	0.641	0.910 *
	Home	s	Support Change	0.029 *			
		h	Costs Change	0.999989 *			
	Skills	a	Accumulation	0.995 *	0.4787	0.1245	0.2601 *
		d	Depreciation	0.0006	0.0037	0.7654	0.1007 *

Efficient GMM estimates. Value of Objective = 19.426. Standard errors reported in Table 4 "all" column. Estimates with t ratios significant at the 5% level indicated with *. Type 4 proportions sum to 1 across rows, so standard errors for k=4 not computed. Parameters with an estimate only in the k=1 column were estimated as common across types.

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.

Figure 3 presents the observed and predicted results for earnings by demographic group. For each group the top panels show outcomes: data on the left, model predictions on the right. The difference between the left and right top panels enters the GMM objective as an element of $\tilde{\Delta}$. The bottom panels depict the observed and predicted impacts of the treatment. The model's vector-valued impact is defined for $g < 3$ as

$$E\left[Y\left(\theta_{\text{cond}}, \hat{\theta}_{\text{exog}}\right)\right] - E\left[Y\left(\theta_{\text{cond}}\Big|_{G}, \hat{\theta}_{\text{exog}}\right)\right], \tag{24}$$

where the notation $\Big|_{G}$ means replace g in θ_{cond} with $G = 3$, the control group, holding other elements of θ_{cond} fixed. Observed impact is similarly defined using the data. For both family sizes in New Brunswick the predictions track the data quite well. Selection and the evolution of state variables together generate 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 model fails to capture qualitatively is the slope of change in the Applicant Study (show in the left column of Figures 3-5). The starting level and impact are accurate but the selection effect is larger than the model predicts. The fraction of the BC 1 group on IA is shown in Figure 4. 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. Figure 5 shows total transfers, IA + SSP, for the BC 2+ group. From the government budget perspective, the SSP is valuable if additional transfers during treatment result in lower transfers later on. Since the impact fades, the policy is a failure in total transfers. In all groups including those not shown 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.

Figure 3. Observed vs. Predicted Earnings

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

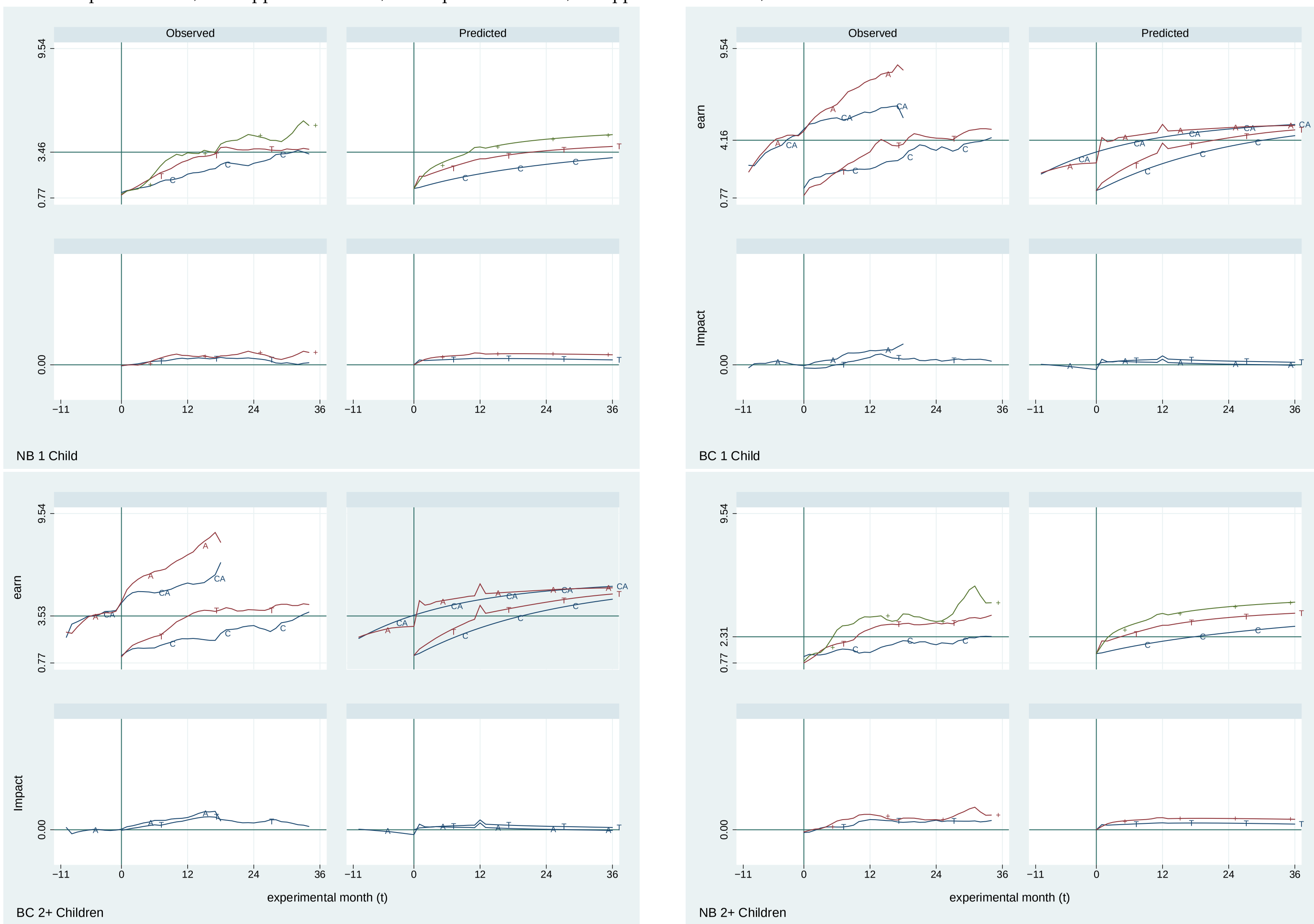
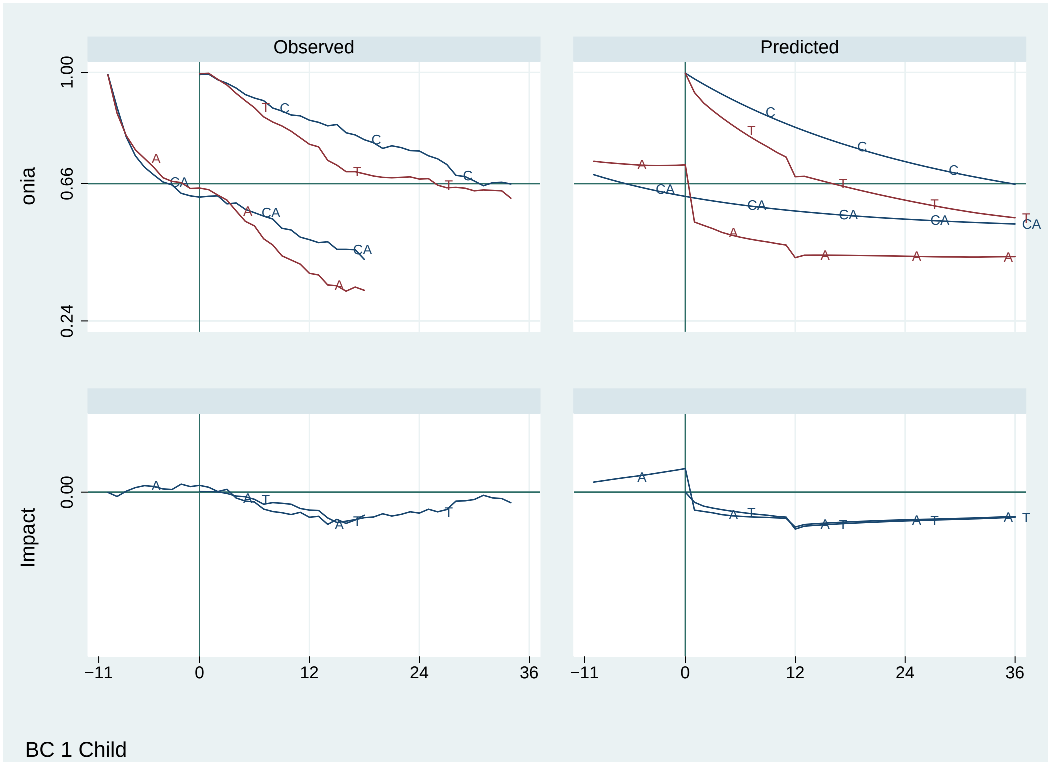


Figure 4. Observed vs. Predicted IA Participation (OnIA)

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

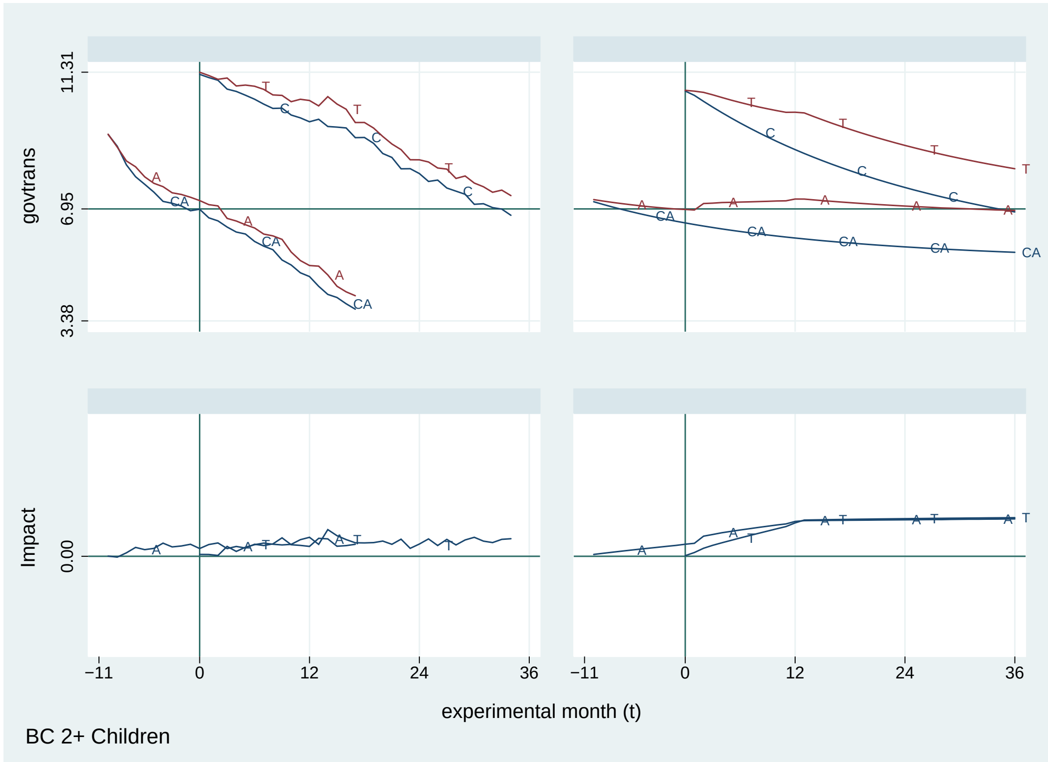


4.2 Variation from Policy, Selection and Heterogeneity

Figure 6 illustrates the combined effects of all sources of variation. Each panel shows the behaviour 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 6 is the large response to SSP+, which is a combination of a large estimate of effectiveness (π_p) and a low job offer probability (π_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

Figure 5. Observed vs. Predicted Government Transfers (IA+SSP)

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

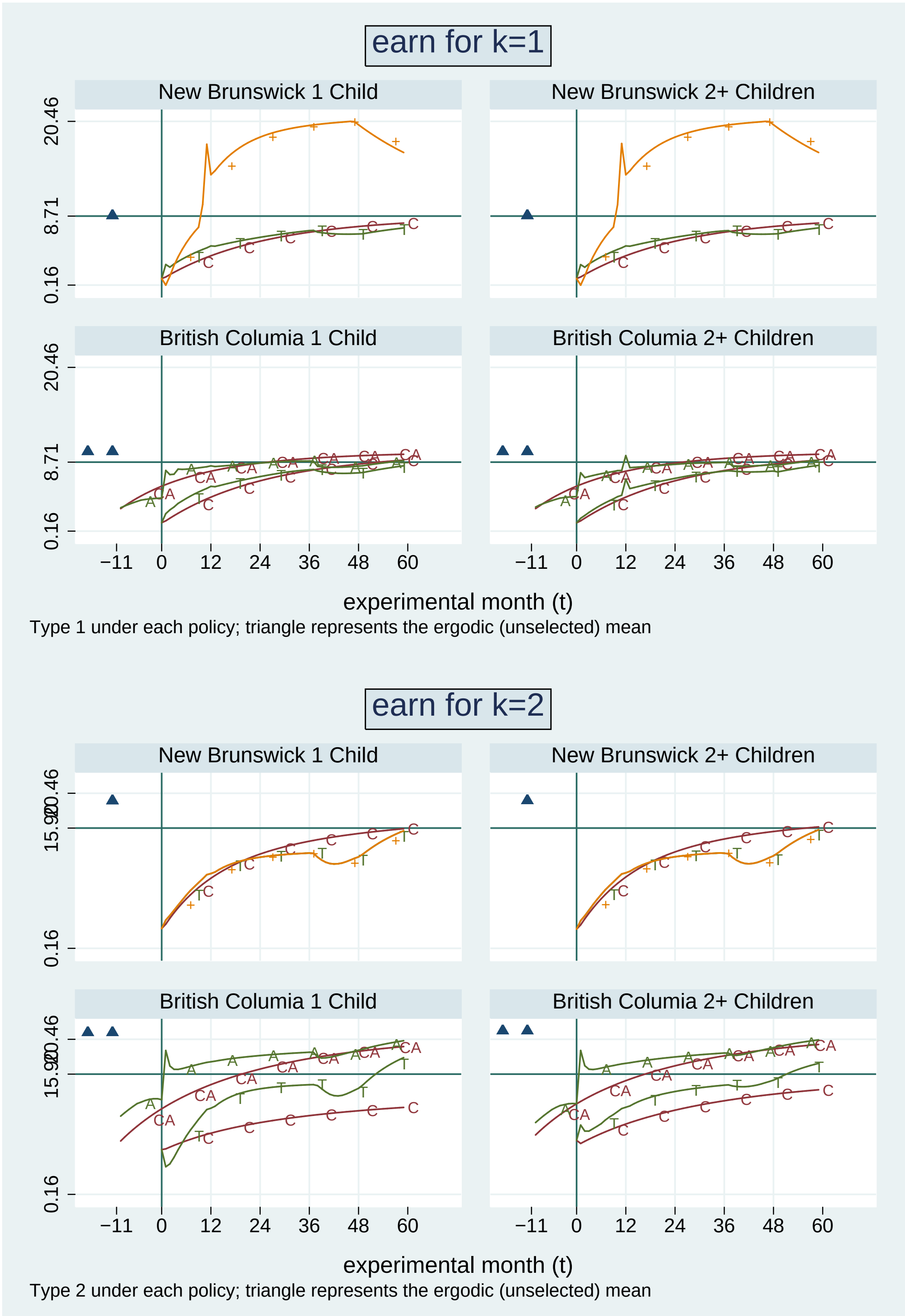


is not particularly responsive to the SSP treatment; its households are constrained by a lack of job offers which the SSP+ alleviates.

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 appears and is long lasting. However, type 2 is estimated to be a vanishingly small fraction of the population in BC.

Figure 6. Variation from Policy, Selection and Heterogeneity

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



4.3 Treatment: Identification or Validation?

The marked estimates in Table 3 and the underlying standard errors reported in Table 4 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. Lise et al. (2005) and Todd and Wolpin (2006) follow this approach by respectively calibrating and estimating models of forward-looking agents within control groups of experiments (respectively SSP and Progresa). They then use the treatment group data for out-of-sample validation. A major advantage of this approach is that behaviour 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 behaviour in populations facing similar but not identical environments.[18]

To quantify the 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 4 reports the results. Standard errors based on all the data are compared to those from the control and treatment groups alone. 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 behaviour of low-income households: the discount factor (δ), the wage offer parameters (μ and σ), the return to skill (η) and many probabilities that determine persistence in wages and other states. Thus, if the validation strategy had been 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.

18 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. Under this strategy restrictions on the final model remain due to parameters unidentified from limited variation in the control group.

Table 4. Estimated Standard Errors: Based on All Data and Within Treatment Groups

Param. / Subscript		k=1 / common			k=2			k=3			k=4		
		All	Ctrl	Treat	All	Ctrl	Treat	All	Ctrl	Treat	All	Ctrl	Treat
Λ	1	0.030	0.098	0.032	0.860	2.657	0.894	0.438	1.295	0.456			
	2	0.006	**0.148**	*0.004*	0.985	**4.187**	0.984	0.429	**1.972**	0.413			
	3	0.020	0.081	0.017	0.016	0.051	0.013	0.014	**0.057**	0.011			
	4	0.019	0.073	0.017	0.016	0.047	0.014	0.014	**0.056**	0.011			
Υ	δ	0.0005	**1.526**	0.0003	0.081	**8.610**	*0.052*	0.416	**40.816**	0.321	0.023	**1.555**	*0.014*
	β	1.081	**1114.74**	*0.750*	143.937	**6427.08**	*89.382*	7.440	**232.913**	*5.460*	2.463	**29.493**	*1.798*
	ρ	0.004	**0.039**	*0.003*	0.040	0.096	0.044	0.022	0.047	0.019	0.019	**0.164**	*0.015*
	μ	0.045	**11.413**	*0.033*	0.154	**673.94**	*0.045*	0.020	**0.896**	*0.014*	0.014	**0.913**	*0.007*
	σ	0.014	**2.282**	*0.010*	0.069	**167.62**	0.068	0.235	**5.286**	*0.152*	0.018	**0.294**	*0.013*
	χ	0.037	**1.316**	*0.025*	3.665	**146.915**	*2.508*	0.023	**0.718**	*0.017*	0.064	**1.246**	*0.044*
	ν	0.004	**0.648**	*0.003*	0.767	**192.037**	*0.515*	0.133	**3.250**	*0.086*	0.025	**0.322**	*0.017*
	κ	0.036	**41.937**	*0.027*	0.350	**524.13**	*0.222*	0.002	**0.322**	*0.001*	0.036	**0.723**	*0.022*
	η	0.155	**371.760**	*0.111*	30.406	**2711.54**	*18.156*	415.395	**3913.08**	313.367	2.352	**193.051**	*1.208*
	ζ	0.069	**3.380**	*0.050*									
Π	j	0.003	**2.565**	*0.002*	0.510	**1520.55**	*0.351*	0.060	**0.274**	0.048	0.028	**2.257**	*0.019*
	f	0.0001	**0.019**	*0.000*	0.080	**0.664**	0.075	0.206	**25.773**	*0.146*	0.066	**0.733**	*0.043*
	m	0.016	**32.530**	*0.011*	0.321	**1174.60**	*0.221*	0.512	**11.121**	0.385	0.030	**2.227**	*0.021*
	l	0.001	**0.006**	*0.001*	0.007	0.020	0.007	0.006	0.009	0.006	0.002	**0.017**	*0.001*
	s	0.002	0.004	0.001									
	h	0.002	**0.156**	*0.002*									
	p	0.015	-------	-------	0.255	-------	-------	1.060	-------	-------	0.036	-------	-------
	a	0.038	**39.199**	*0.027*	0.898	**40.614**	0.679	0.071	**2.743**	0.056	0.016	**0.595**	*0.012*
	d	0.001	**0.019**	*0.001*	0.021	**1.000**	*0.015*	1.066	**9.138**	0.870	0.009	**0.407**	0.007

"All"= standard error for the corresponding parameter in Table 3; "Ctrl" is the standard error using 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 larger than "all". *Italic* indicates smaller than 3/4 of "all". SSP+ effect is unidentified from controls, so it was excluded from both "Ctrl" and "Treat" to make those columns comparable.

Another result is revealed in Table 4 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 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 data solely to study mean differences between treatment groups, control and treatment observations have equal value as they enter linearly in the statistic of interest. Absent other costs, splitting the overall sample evenly minimizes the variance of the impact estimate. Table 4 suggests this is not true when experimental variation will be used to identify an underlying model. An additional treated observation may be more valuable than an additional control observation because their choices reflect more exogenous variation. In other situations it may be control group observations that contain more information for estimation.

4.4 Card and Hyslop Counterfactuals

Card and Hyslop (2005) model IA participation (one of the three elements of α) as a random effect probit in the Recipient Study excluding SSP+.[19] They use their estimates to predict average IA participation by month t under two counterfactual programs of treatment. It is straightforward to replicate their experiment using the estimated model. It requires changing parameters of treatment ($\theta_{\rm pol}$), resolving behaviour among the treated, and computing new predicted values, $E\left[Y \mid \theta_{\rm cond}\,, \hat\theta_{\rm exog}\right]$.

The first counterfactual is lengthening the qualifying period (phase $f = 2$) by 3 months. Using R_f to denote the maximum length of phase f (Table S2.1), this is a policy shift from $R_2 = 12$ to 15. Forward looking behaviour implies that

[19] Card and Hyslop estimate a suite of models and specifications. The focus here is on their joint model of treatment and control groups and their preferred specification, reported in the final column of Table VI in their paper, as well as the counterfactuals in section F.

predictions can shift for all periods both before and after month 12. Parents who would have accepted any job to qualify in month 12 may now reject some jobs, expecting to get a better offer over the next three months. And parents who had searched actively since randomization may not start searching until closer to the new deadline. After month 15 the treatment is the same, but the distribution across permanent and transitory states may differ, leading to different trends. The second counterfactual considered by Card and Hyslop is lengthening the eligible period (here phase $f = 3$, in their notation $E_{it} = 1$), from $R_3 = 36$ to 48 months. In the model greater future potential income affects job search behaviour even during qualification before the supplement is payable. However, income 3 years hence is heavily discounted so the impact at the estimated parameters will be small early on. Once eligible, the longer supplement period increases the value of keeping jobs even before month 48. The impact would continue between months 37 and 49, but under the actual treatment eligibility would be ending, so a widening gap in net impact should be observed. At month 49 the longer eligibility periods start to expire and the patterns would mirror the pattern of qualifying during the first months.

There are important differences between these qualitative explanations and the Card and Hyslop econometric approach. Their panel probit for IA captures state dependence by including two lagged values of IA participation. Here state dependence and persistence are captured by assuming all state variables follow an estimated jump process. IA is itself not persistent, but the household and labour market conditions that lead to it as an optimal choice are. They include unobserved heterogeneity in coefficients, but unlike here the distribution is conditional on being eligible for randomization. That is, they do not correct for endogenous selection into the sample based on being a long-term welfare recipient under the status quo. Their specification includes a fourth-order polynomial in time-since-randomization (t) to capture drift or non-stationarity and an interaction between unobserved type and a quadratic trend in t. Here, drift and heterogeneity in drift is captured by the model with no additional free parameters. Parents in the control group continue to make choices as in the real world after $t = 0$, but since they were selected their distribution over states drifts back toward the stationary distribution. Parents in treatment drift for that reason as well but the treatment induces its own non-stationarity in choices. The same model and the same parameters capture (imperfectly) drift under two completely different selection criteria (applicant and recipient) as well as any other hypothetical selection criterion. With Card and Hyslop's method a different set of selection criteria (such as the Applicant Study) requires different coefficients on the time polynomial.

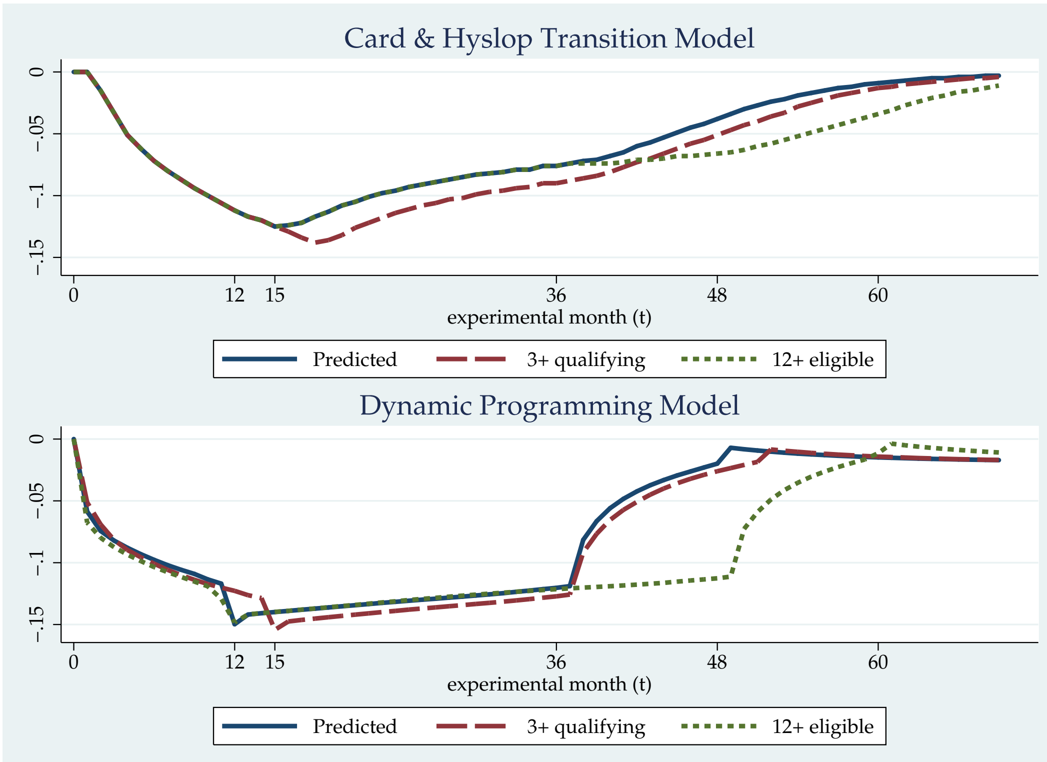
Card and Hyslop model the treatment group with separate coefficients whereas here all estimated parameters apply to both treatment and controls. They extend the panel probit by modeling qualifying (transiting from phase 2 to phase 3) with a normal hazard model. The hazard depends on time since randomization, with an implication that runs counter to the forward-looking model. Namely, the length of the qualification period (here R_2, in Card and Hyslop T_e) has no effect on the probability of qualifying in months prior to R_2. They include an arbitrary three month period of adjustment after qualifying and new state-dependency parameters that are specific to the actual treatment. As in the case of the qualifying deadline, the backward-looking state dependency in their specification means that the longer eligibility period in the second counterfactual has zero effect before month 36. Their prediction is based on extrapolating the polynomial trend terms for greater values of t than in the sample. More generally, none of their models of IA account for income; neither current nor future potential income affect the chances of going on or off IA.

With these considerations in mind, the top panel of Figure 7 recreates Figure 11 of Card and Hyslop (2005). The bottom panel shows results from the estimated model and for the same two counterfactuals. Impact on onIA is shown. The data and model predictions for onIA impact were displayed in Figure 4 for one group. To match Card and Hyslop, which did not control for province or household size, impacts were averaged across all groups weighted by sample size. The in-sample predicted impacts are similar, which is not surprising since the models used the same data. Card and Hyslop's prediction matches the observed impact better, but their flexible form model was fitted to that one dimension, whereas the structural model was fitted to several thousand moments in twelve dimensions across all groups simultaneously. One artifact of their model is the precisely zero effect before months 13 and 37 for the two respective changes. Their impact turns at month 15 because of the ad hoc adjustment period. The estimated model makes the prediction that the net impact of the first change is negligible (but not zero) early on. In months 13-15 it is non-negligible as the "take any job to qualify" decision is delayed. Parents still trying to qualify react to three extra months. From month 15 on, the Card and Hyslop net impact of longer eligibility is larger than the estimated model. Together their prediction before and after month 12 is that a longer eligibility period dominates the shorter one, in terms of this outcome and the goal of moving long-term recipients off IA. A policy maker might conclude from their analysis that a longer qualification period is strictly preferred. But the estimated model shows the impact is quite small after month 15 and is offset by the opposite impact in months 12 to .

In response to the second counterfactual of adding 12 months of subsidy, the Card and Hyslop model predicts a widening impact on IA in months 36 to 48 then a gradual decay. The dynamic programming estimates differ. Card and Hyslop argue (p. 1766) their predictions are lower bounds for the effect of this counterfactual, because it raises the value of the subsidy and would encourage more take-up, an effect their method cannot capture since their model does not include income. Here discounting implies the impact of extended eligibility during qualification is nearly but not exactly zero, suggesting the Card and Hyslop lower bound would be very tight. However, the large fraction of parents who qualified in month 1 and 2 now stay on the subsidy in months 37 to 48. So the impact shifts to the right for twelve months. The Card and Hyslop approach does not impose this type of restriction across time in their predictions. That is, when they extrapolate outside the sample, the rate at which the impact changes in month t is not constrained by the actual take-up rate in month $t-36$ or $t-48$. The dynamic program automatically tracks the fraction of households ending eligibility each t, and therefore impact is consistent over time. This means the Card and Hyslop impact is not a tight lower bound in months 36 to 48 as would be expected given the small effect of future earnings.

As discussed earlier, skill accumulation is negligible for most of the population, so the extra year of steady work induced by the longer subsidy has little marginal effect on skills and wages. Once eligibility expires outcomes for the treated approach the controls at essentially the same speed as in the actual experiment, just 12 months later. Notice that in months 54 to 70 the Card and Hyslop prediction is not a lower bound; it over-estimates the impact on IA participation. It also misses a slight rebound impact of the model at month 60. This corresponds to parents who took minimum wage jobs to qualify at month 12. They push back the impact

Figure 7. Structural Versus Non-Structural Counterfactuals



Predicted treatment impact on IA participation (OnIA). The top panel reproduces Figure 11 in Card and Hyslop (2005). The bottom panel shows the estimated model's impact, averaged by month across demographic groups.

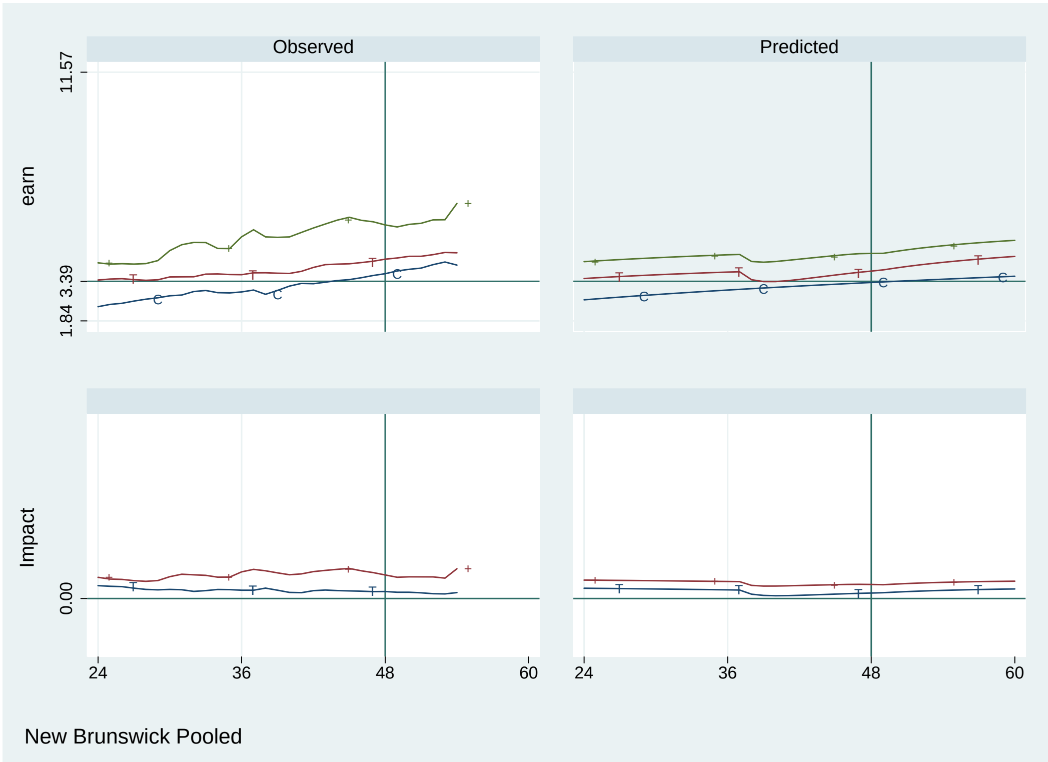
to nearly 0 by returning to IA. However, some now find other better paying jobs and go off IA again creating a modest rebound. Together the lower panel in Figure 7 implies much greater total transfer payments under extended eligibility and no improvement in self-sufficiency. The Card and Hyslop results for onIA, while providing no prediction for transfers or earnings, suggests a milder response. This is indeed a smaller overall impact, but over time their predictions do not bound the impacts either way.

4.5 Other Predictions

Figure 8 compares the model predictions to data on earnings for New Brunswick from the end of the experiment that was not available for estimation. The averages come from SRDC and are available only by province so the predictions for 1 and 2+ children are combined. Since self-sufficiency did not emerge there is little move-

Figure 8. Forecast Earnings Out of Sample

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



ment in the impact and it is not surprising that the model is able to capture that. The SSP+ continues to have a modestly larger impact because of the better quality jobs that it allowed parents to hold out for. The figures show a continual drift in all groups which the model does reasonably well capturing and that it attributes to selection and then return to the stationary distribution.

Figure 9 displays the results of two separate counterfactual experiments. Results of both experiments project through month 60, well beyond the estimation sample. First, the Applicant Study was conducted only in British Columbia and the SSP+ treatment was conducted only in New Brunswick on the Recipient sample. Using the model the missing experiments can be run: the Applicant Study in NB and the SSP+ treatment in both provinces and on both studies. The left column shows total transfers (IA+SSP) for the missing samples in the NB 1 Child and BC 2+ Children demographic groups. These counterfactuals can account for the selected populations not in the data because the estimates correct for selection while allowing each demographic group to have its own unselected mixture over parameters.

An Applicant study in NB (top left panel in Figure 9) 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 5. Namely, the model produces 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. Offering Plus treatment to the Applicants has no additional effect beyond the regular treatment.

An interesting difference is predicted to emerge if SSP+ were run in BC where job offers are a major constraint (bottom left panel of Figure 9).[20] 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 (with low supplements) are available but hard to find. Extrapolating the effects of Plus to the BC population (through the type-specific parameter π_p) the improved offer probability is a significant component. Further, because good jobs are accepted the impacts are long-lasting. This prediction is out-of-sample and is possibly an artifact of the specification in (15). Perhaps the impact of SSP+ in job offers would not be so high in BC. Further, if it were feasible to estimate allowing for more types ($K > 4$) the concentrated effect in NB may not be shared by as large a fraction of the BC population as these estimates indicate. This highlights the difficulty of drawing inferences from experiments out of sample whether it is based on a model or an impact analysis. Together with the modest negative impact in NB it also shows that the hoped-for impacts of the SSP and SSP+ are present in the model, but when accounting for all the outcomes the responsive households are not common enough in the population to generate a large and long-lived impact.

Next, consider the second counterfactual and the model's prediction under an alternative to the SSP treatment shown on the right side of Figure 9. Consider an

[20] Note that job offer rates are type-specific not province specific. So job offers are a constraint in BC indirectly because the predominant types in BC have low job offer probabilities.

experiment that would be difficult to run but may reflect a policy that is ultimately behind most reforms to welfare. Namely, suppose offering the SSP treatment while cutting IAB by 20% among the treated. 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. The figure shows the effect for BC 2+ for OnIA (top right) and total transfers (bottom right). The counterfactual results are shown on the right while the model's predictions for the actual study are shown on the left in order to display the differential impact. Recipients respond strongly to the cut in benefits. Rates on IA are much lower during the qualifying and eligible 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.

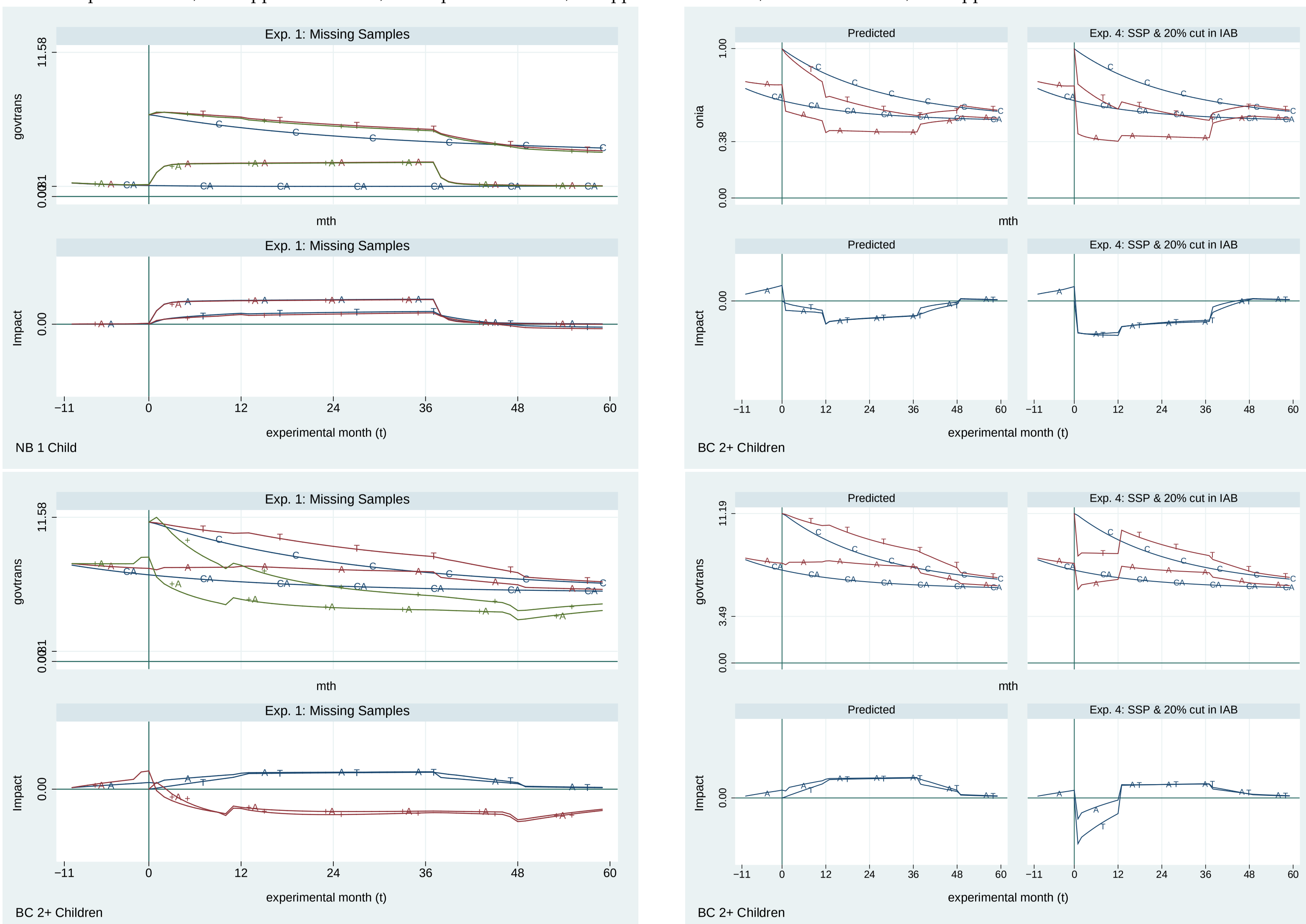
5. Conclusion

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 behavioural 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 due to 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. The model predicts stronger results may have been detected if the SSP+ had been run in British Columbia where the population mix contains a higher proportion of responsive parents.

Beyond the topic of welfare reform, this paper has explored an alternative approach to combining models and experiments. Estimated standard errors com-

Figure 9. Experiments

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



puted after removing groups demonstrate quantitatively that intra-group variation generated by the treatment is critical for identifying a rich and presumably more general model of household behaviour. The literature emphasizes either inter-group variation without any model of behaviour or a reliance on variation within the control group for identification. In the case of the SSP either of these strategies is an inefficient use of the costly output generated by the experiment. Also, counterfactual implications drawn from a non-structural model of the SSP diverge notably from the ones produced by this model. When trying to explain social experiments allowing for endogenous selection and forward-looking behaviour is challenging, but it is shown that in this case it matters quantitatively for making inferences about policy.

There are several limits to the analysis in this paper. First, in order to focus on medium-run causes and solutions to the welfare trap, equilibrium responses to welfare reform are not considered. A targeted earnings subsidy such as the SSP would also affect ineligible low-skill workers in the same labour market if it were implemented as policy. Lise et al. (2005) consider exactly this issue with an equilibrium search model calibrated using SSP data. Second, the utility function is linear in income and additively separable in leisure. As with many job search models linearity allows us to ignore consumption smoothing and assets, but a more general model may suggest that the low discount factor estimated for some types reflects borrowing constraints and not fundamental time preferences. A non-separable utility function is more general and may be able to match some aspects of the data better. Finally, the papers in SRDC (2006) demonstrate that single parents responded to the SSP in dimensions other than job search, work hours and welfare receipt, the ones modeled here. Among these would be longer term responses to welfare in terms of education, fertility and mobility across regions. Keane and Wolpin (2002) and Kennan and Walker (2010) have analyzed these issues using forward-looking models estimated on non-experimental data. Finally, the results can be applied to answer more general questions than posed in this paper. Although the SSP treatment was not very successful in eliminating the welfare trap, it did inspire Canadian provinces in subsequent reforms. Quebec implemented a reform of IA closely related to the SSP (Lacroix 2010). Applying the model estimated here to data from the Quebec experience using methods described in Ferrall (2003) is in development.

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6. Supplemental Material

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This version benefits enormously from suggestions from the individuals and audiences listed below.

- Conferences
  CERF (Ottawa); The EALE/SOLE Joint Meetings (Milan); SED (New York); The Econometrics of Strategy and Decision Making (Yale);
- Workshops
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6.2 Model Details

6.2.1 States and Actions

Table S1 summarizes the state variables and the state space. Details about each variable are given here.

Endogenous Variables. To describe the transition for each variable, let $q' = q^\star(\bar{q}, \{\pi_j\}, \{\mathbf{Q}_j\})$ 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 π_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 Γ and the mixing distribution Λ.
- Transition: $k' = k^\star(k, 0, \emptyset)$

S2. Observed Type: $d \in \{1, 2, 34\}$

Table S1. Endogenous Variables and Actions

Item	Variable	Description	Num.	Values / Calculation	Notes
	l	**L**ost job entering this month	2	{0,1}	does not affect utility or transitions
	p	worked **P**revious month	2	{0,1}	does not affect utility or transitions
	n	current earn**N**gs offer	6	{0,1,2,3,4,5}	
	x	e**X**perience level	4	{1/4,1/2,3/4,1}	
end	b	upper **B**ound on hours in job	3	{0,1,2}	Figure 1
	s	Outside **S**upport	4	{0,1/3,2/3,1}	Figure 1
	h	Opp. cost of time outside **H**ousehold	3	{1/4,2/4,3/4}	Figure 1
	d	**D**emographic group	4	{1,2,3,4}	Table 1
	k	unobserved type	4	{1,2,3,4}	
	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 clock	All states given assignment (**S**$_2$)	138,240	= **S**$_1$ * 12 * 5	Cond. on ne & ng (group assignment)
Size	x exp	All individual states (**S**$_3$)	417,024	= **S**$_1$ + **S**$_2$ *3	Control+SSP+ SSP_Plus+Applicant
	xKxD	Complete State Space (**S**)	6,672,384	= **S**$_3$ * 4 * 4	K * D
	m	labor **M**arket work hours	5	{0,1/4,1/2,3/4,1}	constrained by u(b); see Figure 1
	a	engage in **A**ctive job search	2	{0,1}	
	i	accept **I**A	2	{0,1}	see Figure 1.
	Size	Feasible Action Space (**A**)	12	= **6***2	m & a stored as one var. with **6** values
Size		Outcome Space	80,068,608	**= S * A**	

- Role: index into the policy vector θ_{pol} and the mixing distribution Λ.
- Transition: $d' = d^{\star}(k, 0, \emptyset)$

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

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

$$c(h) = -\zeta \ln(1-h)\,. \tag{S1}$$

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: (11)
- Transition: $h' = h^{\star}(h, \pi_h, H)$

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: (10)
- Transition: $s' = s^{\star}(s, \pi_s, S)$

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

- Role: constraint on work hours in current job
- Auxiliary Equations: See feasible actions below
- Transition:

$$\begin{array}{cc} \underline{b'(\alpha,\theta)} & \underline{P\{b'|(\alpha,\theta)\}} \\ 0 & \mathcal{B}\left[m>0\right]\pi_l \\ 1 & (1-p_j(\alpha,\theta))\mathcal{B}\left[b<2\right]+\mathcal{B}\left[m=0\right] \\ 2 & p_j(\alpha,\theta)\pi_f \\ 3 & p_j(\alpha,\theta)\pi_f \\ b & \mathcal{B}\left[m>0\right](1-\pi_l). \end{array} \tag{S2}$$

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

- Role: level of earnings and future growth potential
- Equations in Text: (12), (13), and (14)
- Transition:

$$x' = x^{\star}\Big(x, [m\pi_a + \mathcal{B}\left[m=0\right]\pi_d], [\min\{\max\{1/4, x + \mathcal{B}\left[m>0\right]/4 - \mathcal{B}\left[m=0\right]/4\}, 1\}]\Big) \tag{S3}$$

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

- Role: search-sensitive component of wages
- Equations in Text: (12)- (14)
- Transition: with MW$=0$,

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

With MW>0

$$\begin{aligned} n'(\alpha,\theta) =& n^{\star}\Big(n, [\,ap_j\pi_m, ap_j(1-\pi_m)\phi_x \quad ap_j(1-\pi_m)(1-\phi_x)\,], \\ &[\,\{0\} \quad \{1/6,\ldots,(5-\tilde{n}(x'))/6\} \quad \{(6-\tilde{n}(x'))/6,\ldots,5/6\}\,]\Big). \end{aligned} \tag{S4}$$

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: $l' = l^{\star}(0, \mathcal{B}[m>0]\,\pi_l, \{1\})$

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

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

Actions.

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

- Equations in Text: (11)

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

- Equations in Text: (15), (11)

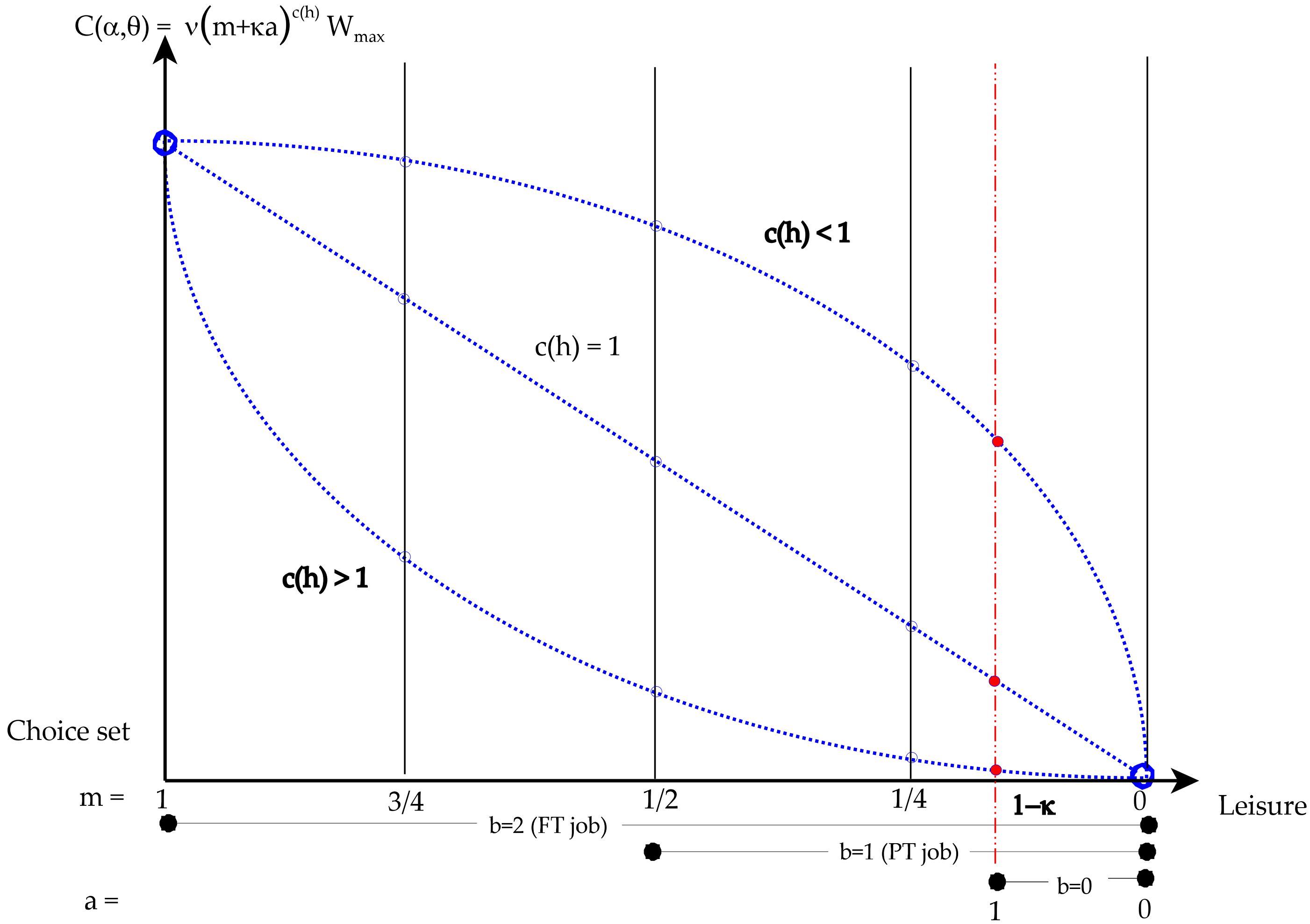
A3. Accept Income Assistance: $i \in \{0,1\}$

- Equations in Text: (10), (9), (9)

Feasible Actions

$$\mathbf{A}(\theta) \equiv \{(\,m \quad a \quad i\,) \in \{\mathbf{M} \times \{0,1\} \times \{0,1\}\} \,:\, m < b \,\&\, ma = 0\}. \tag{S5}$$

Figure S.1. Cost of Work Hours



A single-parent household faces a shifting tradeoff between income and leisure (non-labor market time) driven by the state variable h. When h is large costs are convex to the origin. The parent prefers part-time work, perhaps because children are in school or family members are available to provide part-time daycare. If these arrangement breaks down costs become concave. The parent now faces fixed costs of work, for example paid day care plus commuting time. Now the parent prefer either full-time work or no labor market activity (all else constant). The ability to respond to the SSP or to spread fixed costs across more hours depends on whether the parent has a job that allows full-time work ($b = 2$) or only has a part-time job ($b = 1$).

6.2.2 Cost of Work Hours

Three possible sets of feasible work hours (depending on b) are shown in Figure S.1 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 curvature of costs is determined by $c(h)$. When not working the 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 π_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.

6.2.3 The SSP Experimental Design

A subject's treatment group in the SSP is indexed by the sub-vector $\theta_{\text{exp}} = (\,e \quad g\,)$, where e is the experimental sample and g is the randomly assigned treatment status within samples. The Recipient Study ($e = 2$) includes parents that had been on IA for at least one year. The Applicant Study ($e = 1$) includes parents initiating (applying for) a new spell of receiving IA after a period of at least six months without IA. The treatment variable g takes on three values: control ($g = 3$), treatment ($g = 2$), and SSP Plus treatment ($g = 1$). A subject's status in the treatment program is defined by the sub-vector $\theta_{\text{clock}} = (\,r \quad f\,)$, where f is the current phase of treatment, r is the number of periods the subject has resided in that phase.

Each treatment group has associated with it an initial post-assignment clock setting, a pre-assignment selection period and a sequence of feasible histories:

$$\Psi_x[e] = \left(\bar{\theta}_{\text{clock}} \quad T \quad \mathcal{H}\left[y;\theta_{\text{cond}}\right]\right). \tag{S7}$$

The elements of Ψ_x are listed in Table S2.1. 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.

Time 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

Table S2.1. Program of Treatment

			Transition Rules		
f	phase name	R_f	f_n=0	otherwise	default at r=R_f
0	pre-random assignment	1			
1	entry	12	stay on welfare (i=1)	f_n=4	f_n=2
2	qualification for SSP	12	i=1 or m<=PT	f_n=3	f_n=4
3	eligibility for SSP	36	automatic		f_n=4
4	post-treatment	1			

In the actual experiment each twelve-month period of eligibility was a distinct phase, because two months per year the recipient could receive the supplement when hours fell below the full-time requirement (to smooth within-month interruptions in employment). This is not included in model for simplicity so phases 3-5 are collapsed to a single phase 3 that lasts 36 months.

clock setting has $r = 2$. Formally the selection criteria can be represented several different ways. Table S2.1 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.

The SSP program of treatment is defined by a vector of parameters,

$$\Psi_t[g] = \begin{pmatrix} R_0 & \cdots & R_4 & f_n(y) & PT & TB & UL \end{pmatrix}. \tag{S9}$$

Both $f = 0$ and $f = 4$ correspond to the real, non-experimental world, before random assignment and after treatment has ended. By definition, control groups ($g = 3$) transit immediately from phase 0 to phase 4. The treatment groups transit from phase 0 to the initial phase for their treatment group (listed in Table S2.2). Ultimately they reach phase 4 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. R_f is the maximum duration of treatment phase f. 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 S.1 summarizes the selection, assignment, and transition rules in the SSP.

The remaining elements of Ψ_t determine the value of the SSP supplement, SUP(α, θ), which enters utility defined in (S7) through income defined in (8). The full equation for SUP(α, θ) appears in (9). 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

Table S2.2. Policy Vectors Contained in θ_{pol}

$\Psi_p[d]$

d	IAB	MW[a]	SA	CB
1	712			
2	755	650	200	100%
3	982			
4	1175	780		

$\Psi_t[g]$

g	PT	TB	UL
3	0%	0%	0.00
2	75%	50%	3.90
1	75%	50%	3.90

$\Psi_x[e]$

					$H[\text{y};\theta_{\text{cond}}]$												
e	Init. r,f	T	t_0	t_{min}	-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							

[a] 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); $\sim$ i means i=0 (off IA).

take only 4 months. After seven months the first parent's clock would read $(\,7 \quad 2\,)$, the second $(\,3 \quad 3\,)$, and for all parents assigned to the control group it would read $(\,1 \quad 6\,)$. 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. With all of the policy vectors introduced the policy sub-vector defined as

$$\theta_{\text{pol}} = (\,\Psi_p \quad \Psi_x \quad \Psi_t\,) \tag{S11}$$

is summarized in Table S.3.

6.2.4 Transitions and Conditional Distributions

Given the transitions for each of the variables defined above, the primitive transition function can be written

$$P\{\,\theta' \,|\, \alpha\,,\,\theta\,\} = \prod_{q\in\theta}\left[\mathcal{B}\left[q'=\bar{q}\right]\left(1-\sum_j \pi_j\right)+\sum_j \mathcal{B}\left[q'\in \mathbf{Q}_j\right]\frac{\pi_j}{\#\mathbf{Q}_j}\right]. \tag{S12}$$

This means take the product over all state variables q. Each state contributes the probability that it takes on the value in θ', 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 $P\{\,\theta' \,|\, \alpha\,,\,\theta\,\}$.

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 θ_{cond}. Choose an unobserved type k and use the corresponding ergodic distribution as the starting value: $\Omega\{\,\theta' \mid k, \theta_{\text{cond}}\} = P_\infty\{\theta\}$. 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.

$$P^{\star}\{\,\theta' \,|\, \theta\,\} = \sum_{\alpha} P\left\{\theta'\Big|_{\mathcal{B}[t=t_0]\bar{\theta}_{\text{clock}}} \mid \alpha,\theta\right\}\mathcal{H}\left[Y(\alpha,\theta);\theta_{\text{cond}}\right]P\{\alpha \mid \theta\}\,. \qquad (S13)$$

The notation $\Big|_x$ 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:

$$\omega\left(k;\theta_{\text{cond}}\Big|_{t+1}\right) = \omega\left(k;\theta_{\text{cond}}\right)\left[\sum_{\theta'}\sum_{\theta}P^{\star}\{\,\theta' \,|\, \theta\,\}\,\Omega\{\,\theta \mid k,\theta_{\text{cond}}\}\right]. \qquad (S14)$$

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:

$$\Omega\left\{\,\theta' \mid k,\theta_{\text{cond}}\Big|_{t+1}\right\} = \frac{\omega\left(k;\theta_{\text{cond}}\right)}{\omega\left(k;\theta_{\text{cond}}\Big|_{t+1}\right)}\sum_{\theta}P^{\star}\{\,\theta' \,|\, \theta\,\}\,\Omega\left\{\,\theta \mid k,\theta_{\text{cond}}\Big|_{t-1}\right\}. \qquad (S15)$$

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:

$$\lambda^{\star}\left(k,\tilde{\theta}_{\text{cond}}\right) \equiv \lambda[k,d]\frac{\omega\left(k;\theta_{\text{cond}}\Big|_{t_0}\right)}{\sum_{k'=1}^{K}\omega\left(k';\theta_{\text{cond}}\Big|_{t_0}\right)}. \qquad (S16)$$

Since the clock was set properly at t_0, the updating rules (S13) - (S15) apply for $t>t_0$ as well. Since all actions are feasible after random assignment in the SSP, $\omega\left(k;\theta_{\text{cond}}\Big|_{t+1}\right)$ becomes constant and the correction factor on $\Omega\left\{\,\theta' \mid k,\theta_{\text{cond}}\Big|_{t+1}\right\}$ becomes one. This assumes that attrition is uncorrelated with unobserved types (and unobserved states).

6.3 Identification Redux

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 $\theta_{\rm cond} = (t \quad g \quad e \quad d)$. Different loadings on these four 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 π_l. In the model job loss occurs exogenously and the SSP survey records reasons why a parent stops working. These were grouped into losses and quits. Thus the proportion of working parents losing a job each month is available in the data and is directly determined by the value of π_l. Since the observed proportions differ across demographic groups it is feasible to consider unobserved heterogeneity in π_l with different mixtures across groups. Of course, the estimates of π_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 and the jump probability for outside support, π_s. How the quit rate correlates with labor market earnings helps identify the distribution of $c(h)$ and thus ζ. 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 δ.

The final parameter to discuss is the smoothing factor ρ. 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 behaviour 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 Λ will be identified from the data along with differences in the underlying parameter vectors $\Gamma[k]$.

6.4 Data

6.4.1 Notes on Measurement

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.

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.

6.4.2 Impact and the Correlation Across Moments

Observed impact is the difference in mean results between a treated group and its control:

$$\hat{\Delta}\left(\theta_{\text{cond}}\right) \equiv \hat{Y}\left(\theta_{\text{cond}}\right) - \hat{Y}\left(\theta_{\text{cond}}\Big|_{G}\right). \tag{S17}$$

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

$$\check{\Delta}\left(\theta_{\text{cond}}\right) \equiv E\left[Y\left(\theta_{\text{cond}}\right)\right] - E\left[Y\left(\theta_{\text{cond}}\Big|_{G}\right)\right]. \tag{S18}$$

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 $\lambda^{\star}\left(k, \tilde{\theta}_{\text{cond}}\right)$ 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_{∞} 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 in the model 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.

Table S.3 reports relative impacts ($\check{\Delta}\left(\theta_{\text{cond}}\right)/Y\left(\theta_{\text{cond}}\Big|_{G}\right)$) 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 are 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 Study.

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 S.4 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 S.4 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 applicants. 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 δ 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 S.4 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

Table S.3. Relative Impacts on Selected Moments in Months -11,1,13,25

		NB / 1 Child		NB / 2+		BC / 1 Child		BC / 2+	
		Recpients		Recpients		Appl.	Recpients	Appl.	Recpients
Var.	t	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 the corresponding Table S6, divided by the Ctrl column. Negative impacts in () and in red. Largest absolute impact within the table shaded for each moment.

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.

6.5 Additional Results and Experiments

6.5.1 Out-of-Sample Predictions

Figure S.3 compares the model predictions to data on earnings and OnIA by province from the end of the experiment that was not available for estimation. Not surprisingly, the impacts continue to fade toward zero as treatment ends and all subjects return to the status quo. The model's prediction is similar in trend but it continues to miss the level of earnings in the Applicant Study. One intriguing pattern is that the impact of the SSP+

Table S.4. Contemporaneous Correlations Across Selected Results

Applicants (e=1)

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

Recpients (e=2)

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

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.

6.5.2 Stock Versus Flow Sampling

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 as policy 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 Study an entry condition of just six months on welfare is used to enhance the effects. 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

Figure S.1. Observed vs. Predicted IA Participation (OnIA)

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

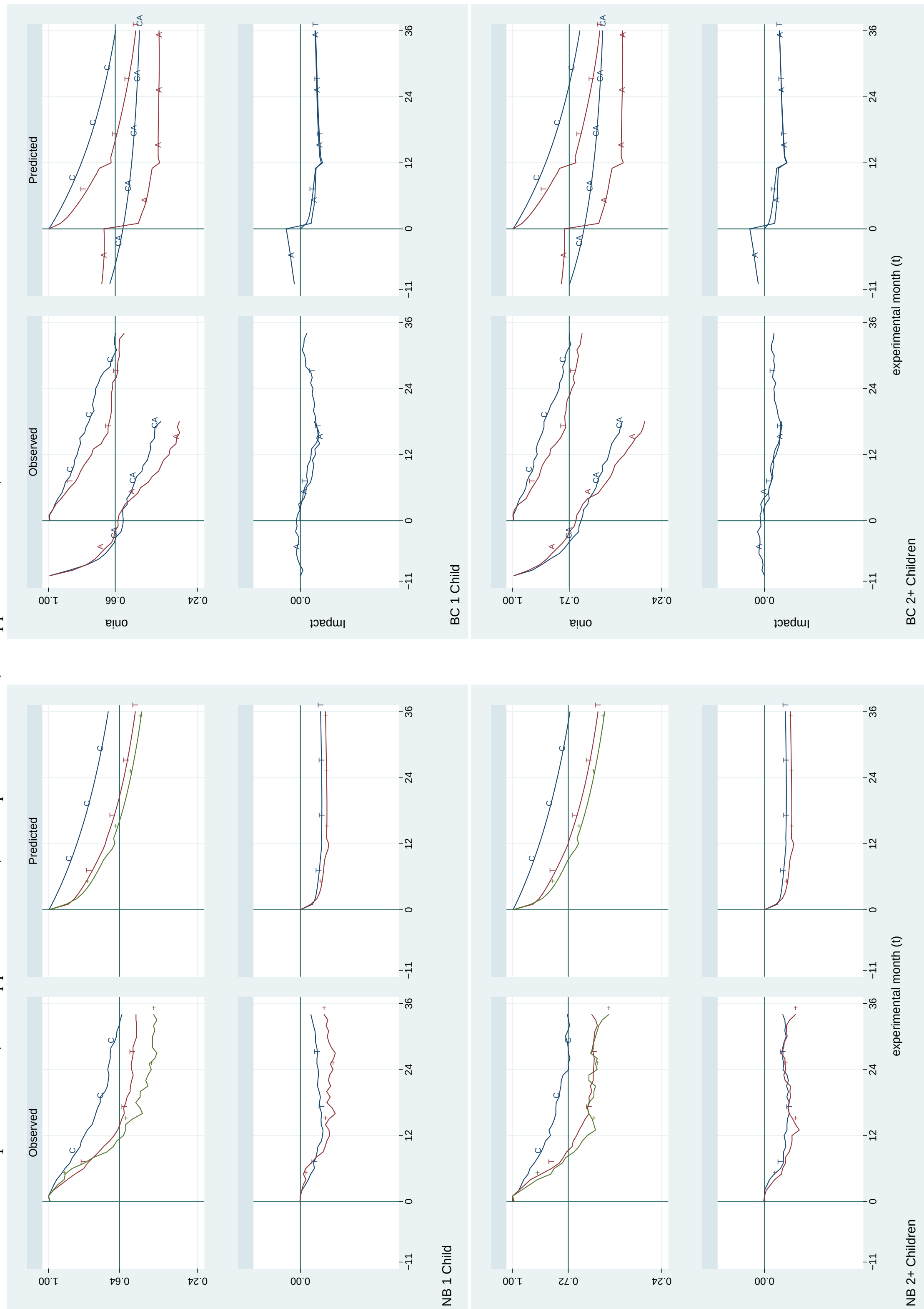


Figure S.2. Observed vs. Predicted Transfers (IA+SSP)

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

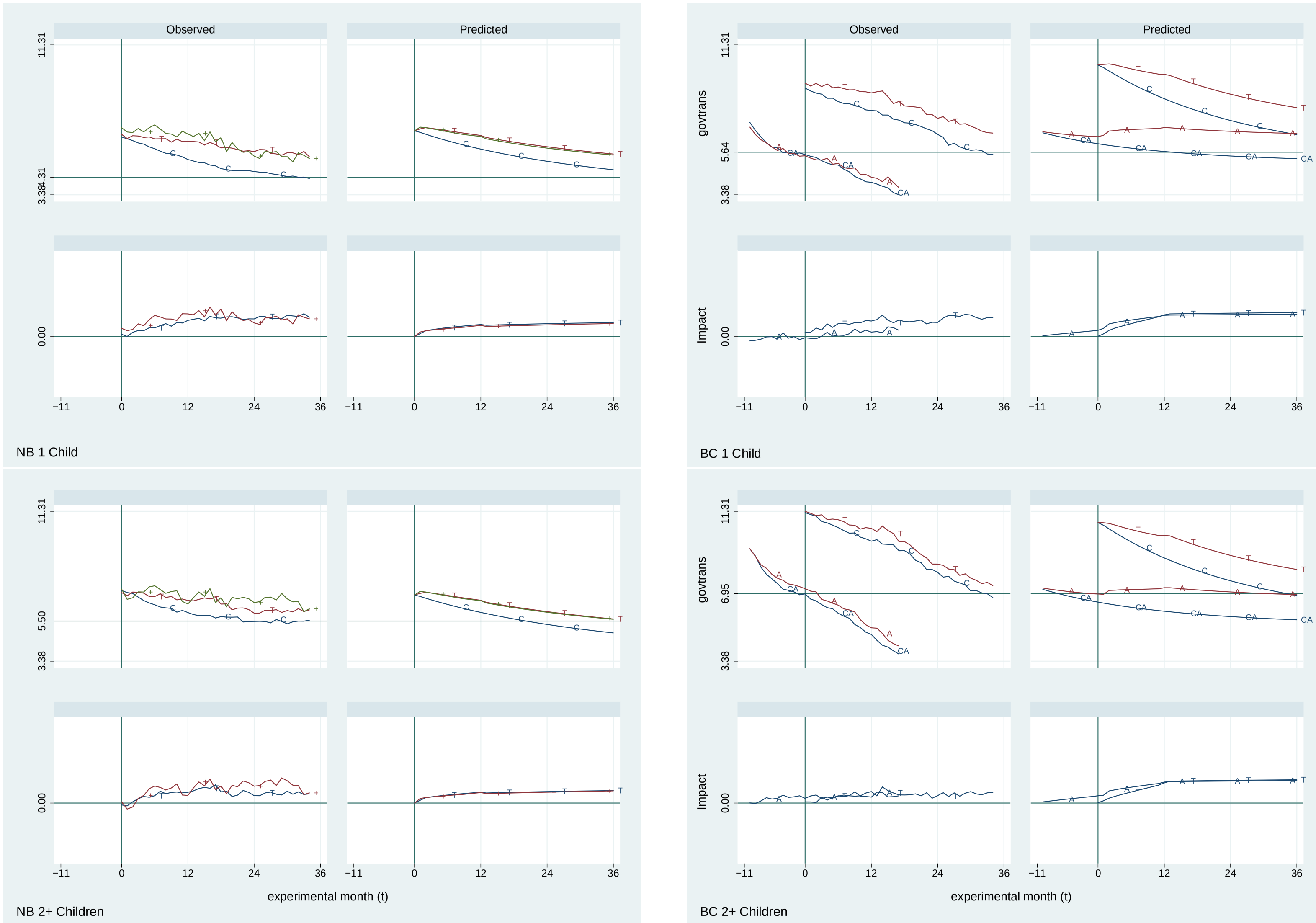
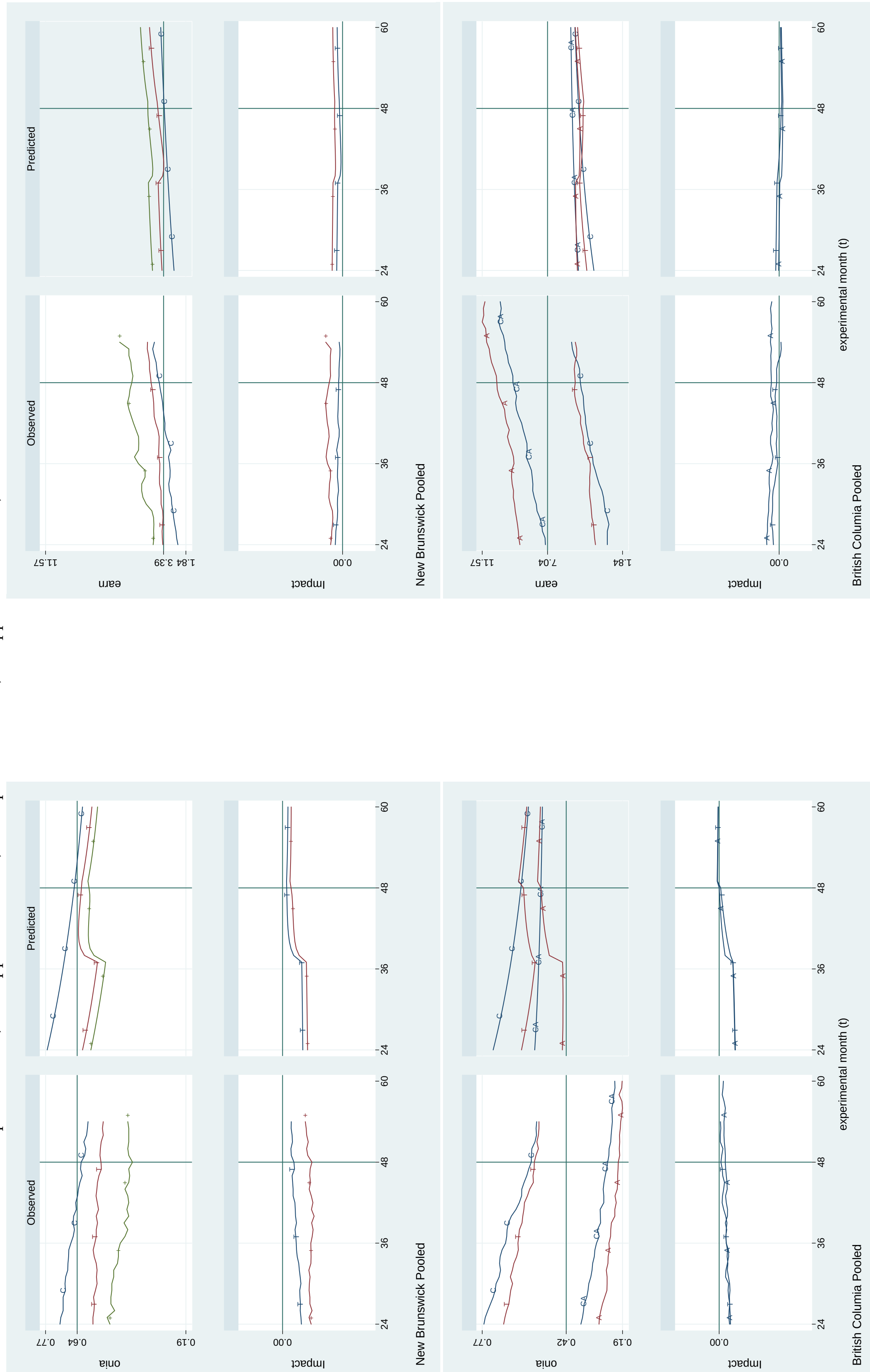


Figure S.3. Forecast Earnings and On IA by Province

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



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 S2. 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.

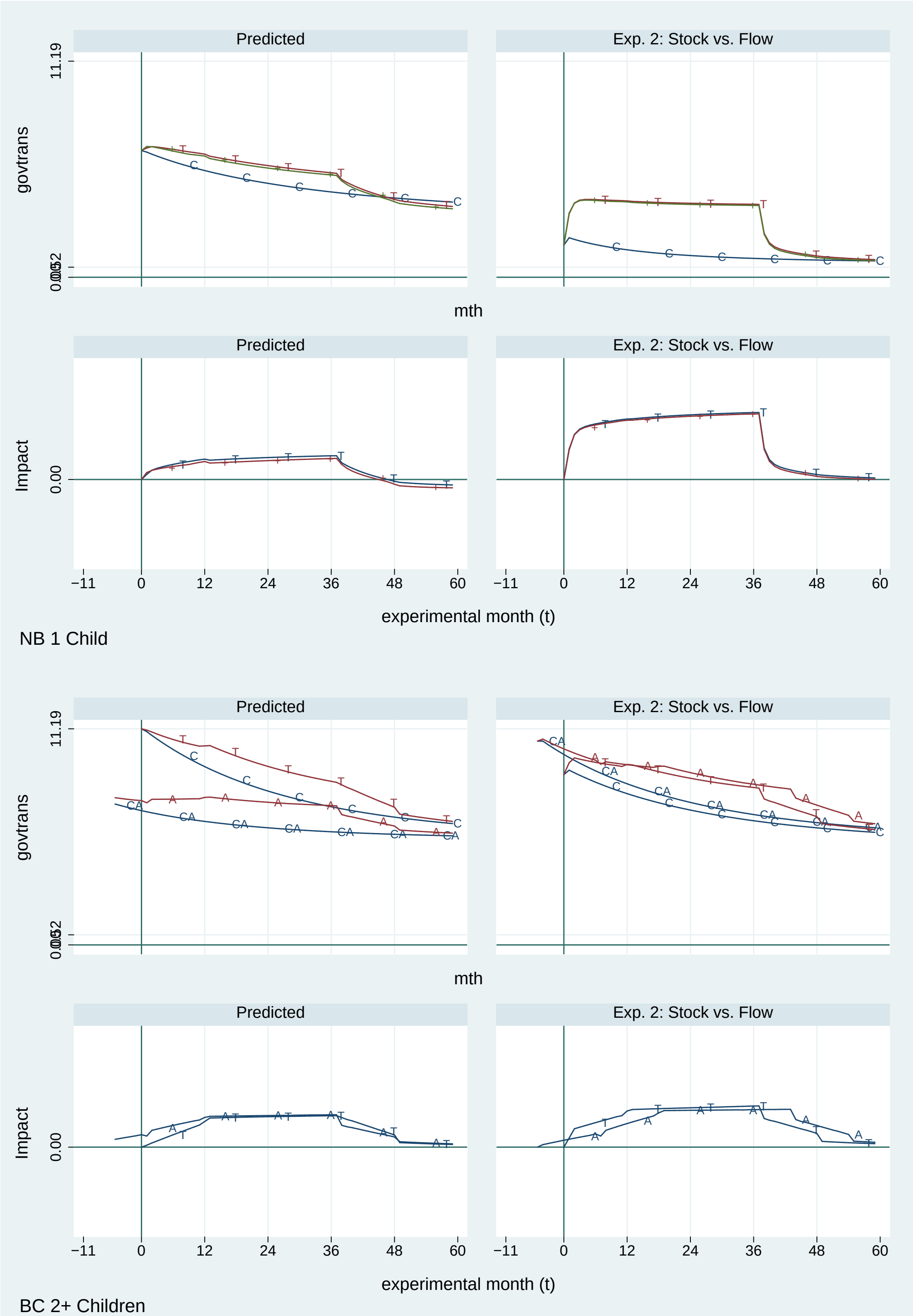
6.6 Computation

The size of the model and some technical details of the solution are listed in Table S.5. 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 (S2). For example, the household is not affected by the values of l and p, and a currently not employed worker ($b = 0$) does not care about values of n.

Since a stationary distribution P_∞ 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, 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 (S3) 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 δ. 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

Figure S2. Total Transfers under Stock/Flow Sampling Reversal



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

Table S.5. 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	

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 completed in approximately an hour.

6.6.1 Steps in Model Solution

Steps in Computation.

A0. Set $\theta_{\text{exog}} = \theta^0_{\text{exog}}$ 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 3 to convergence.
  - **C1.** Once converged, loop one more time over θ_{end} to compute choice probabilities ($P\{\alpha\,|\,\theta\}$ in 4) and $E[Y\mid\theta]$.
  - **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_∞, compute the first value of $\omega(k;\theta_{\text{cond}})$ and $\Omega\{\theta|k,\theta_{\text{cond}}\}$.

D1. Increase t by 1. Update Ω and ω by looping through all transitions.

D2. Repeat previous step until $t = t_0$.

D3. Store Ω to be used for all g given e, k, d.

C3. 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 $E[Y \mid \theta]$.

E2. Decrease r by 1. Return to E1 until $r = 0$.

E3. Repeat the previous two steps until $f = 0$.

C3. Compute expected outcomes given k. Set $t = t_0$ and restore Ω.

F0. Loop through θ_{end} and setting the clock to $\bar{\theta}_{\text{clock}}$. Compute $E[Y|k, \theta_{\text{cond}}]$ and update Ω for the next period.

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

B0. Decrease g by 1. If $g > 0$ set $f = F$ and return to section E.

B1. Decrease e by 1. If $e > 0$ then reset $g = 2$ and return to section D.

B2. Decrease k. If $k > 0$ return to B0.

B3. 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 (20).

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

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

A3. Use the optimizer to minimize the objective with respect to θ_{exog}.

A4. Iterate on the weighting matrix Σ, return to previous steps to compute $\hat{\theta}_{\text{exog}}$.

6.6.2 Simulating the second stage weighting matrix

To simulate the covariance of the stacked moment vectors at the first stage estimates, first fix $\tilde{\theta}_{\text{cond}}$ and k. Draw a θ_{end} from the corresponding ergodic distribution P_∞ followed by a sequence of conditional actions and next states using choice probabilities consistent with the entry group's eligibility criteria (that is simulate an individuals who ends up being eligible for group e). At random assignment the process continues but now all choices are feasible. Let $\tilde{Y}^r(\tilde{\theta}_{\text{cond}})$ denote the rth simulated path with vectors concatenated across experimental time t.

Compute the weighted deviation from mean $u_r = \lambda^\star\left(k, \tilde{\theta}_{\text{cond}}\right)\left(Y^r(\tilde{\theta}_{\text{cond}}) - E\left[\tilde{Y} \mid \tilde{\theta}_{\text{cond}}, \hat{\theta}^1_{\text{exog}}\right]\right)$. Repeat the process across k and r and compute the outer product of deviations averaged across simulations. The resulting matrix is a consistent estimate of the covariance of the block of moments. The inverse,

$$\Sigma\left(\tilde{\theta}_{\text{cond}}, \hat{\theta}^1_{\text{exog}}\right) = \left(\frac{1}{R}\sum_{r=1}^{R}\sum_{k=1}^{4} u_r {u_r}'\right)^{-1},$$

enters the second-stage objective in 22.

6.7 Complete list of observed moments

Table S6.1 . Earnings (earn)

"earn"

	NB / 1 Child			NB / 2+ Child			BC / 1 Child				BC / 2+ Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-11							1.8922	2.1144			2.3689	1.8482		
-10							2.2972	2.6892			2.5788	2.2727		
-9							2.8021	2.6612			2.5071	3.0483		
-8							3.2467	3.0492			2.8824	3.1892		
-7							3.5927	3.4052			3.1947	3.3488		
-6							3.9674	3.5989			3.4775	3.5250		
-5							4.2358	3.7348			3.5820	3.5237		
-4							4.3171	3.8718			3.6350	3.6061		
-3							4.4502	4.2045			3.7186	3.7888		
-2							4.4602	4.3951			3.7043	3.8036		
-1							4.4189	4.4632			3.8027	3.8473		
0	0.9927	0.9475	1.0903	0.8627	0.7696	1.1457	4.6981	4.8075	0.9185	1.3405	4.3663	4.2889	1.1285	1.2020
1	1.1789	1.1779	1.2066	1.1959	0.9604	1.2774	5.1578	5.1047	1.3752	1.8268	5.0716	4.6648	1.4907	1.4344
2	1.2464	1.2998	1.2509	1.3354	1.1725	1.2404	5.5088	5.1608	1.5011	1.9714	5.4262	4.8912	1.8008	1.5981
3	1.3001	1.4762	1.3437	1.3791	1.4716	1.2420	5.7803	5.3013	1.5715	2.0096	5.6813	4.9623	1.9553	1.6465
4	1.5404	1.6738	1.3908	1.6812	1.6729	1.2944	5.9756	5.3754	1.8066	2.1854	5.8747	4.9536	2.0759	1.6291
5	1.8365	1.8491	1.4508	2.1767	1.7914	1.4026	6.0913	5.4429	2.0931	2.2116	5.9753	4.9347	2.2024	1.6425
6	2.2201	2.0475	1.5678	2.6998	1.8921	1.5211	6.2636	5.4739	2.3017	2.2882	6.1399	4.8804	2.3167	1.6503
7	2.6227	2.2413	1.7224	2.9487	1.9544	1.5849	6.6192	5.3487	2.5137	2.4079	6.1970	4.9302	2.3884	1.7918
8	2.9489	2.3605	1.8236	2.9819	2.0223	1.5663	7.0016	5.4061	2.7649	2.3714	6.2845	5.0171	2.6372	1.8937
9	3.1421	2.4955	1.8300	3.0787	2.1264	1.5098	7.1382	5.5521	2.9040	2.4147	6.5429	5.0971	2.9011	1.9851
10	3.3353	2.7021	1.8945	3.3386	2.4549	1.3476	7.2939	5.6839	3.1164	2.4638	6.7551	5.2463	3.1841	2.1209
11	3.2556	2.8736	1.9832	3.4773	2.6379	1.4024	7.5417	5.8130	3.2936	2.4530	6.9133	5.3667	3.3420	2.1932
12	3.4207	2.9771	2.1708	3.4652	2.7602	1.3853	7.6938	5.7715	3.4771	2.4750	7.1148	5.4596	3.5098	2.1856
13	3.3883	3.1245	2.2440	3.4954	2.8870	1.5297	7.7743	5.8823	3.9308	2.5758	7.2834	5.3878	3.6926	2.2084
14	3.3759	3.2005	2.2676	3.5343	2.9828	1.6891	8.0288	6.0626	4.2047	2.7503	7.6407	5.4334	3.8023	2.1797
15	3.5695	3.2077	2.3320	3.3092	3.0171	1.7808	8.1143	6.0823	4.0501	2.8959	7.9045	5.4848	3.8616	2.1356
16	3.4881	3.2557	2.4499	3.2167	3.0449	1.8385	8.1488	6.1440	3.8564	2.9360	8.1267	5.7154	3.8443	2.0948
17	3.4496	3.3543	2.4908	3.2734	3.0184	1.9663	8.5817	6.1720	3.8249	2.9626	8.4277	5.9438	3.7965	2.0942
18	3.9325	3.7313	2.7314	3.6463	3.0777	2.0761	8.2789	5.4827	3.9256	3.1594	7.8511	6.6560	3.8854	2.4951
19	4.0780	3.7534	2.8606	3.6291	3.0962	2.0537			4.3171	3.5300			4.0134	2.7182
20	4.1365	3.6833	2.8029	3.4856	3.0140	1.9230			4.5444	3.6611			3.9481	2.7407
21	4.1653	3.6068	2.7620	3.4672	3.0104	2.0086			4.4733	3.8883			3.8156	2.7762
22	4.3232	3.5895	2.7051	3.3448	3.0328	2.0110			4.3803	3.8275			3.8276	2.8865
23	4.4963	3.5910	2.6652	3.2548	3.0747	1.8991			4.3230	3.6563			3.8981	2.9365
24	4.4335	3.6523	2.8059	3.2029	3.1228	1.8390			4.2820	3.5660			3.8842	2.9646
25	4.3562	3.6554	2.8800	3.2366	3.0598	1.9483			4.2717	3.7729			3.8596	2.8220
26	4.2866	3.6369	2.9511	3.4103	3.1109	1.9213			4.2459	3.6642			3.8543	2.7420
27	4.1554	3.5807	3.0574	3.6451	3.0678	1.8856			4.1918	3.5176			3.9579	2.6115
28	4.1467	3.5646	3.3136	4.2050	3.2385	2.0734			4.4165	3.6312			4.1572	2.8035
29	4.0773	3.5545	3.3571	4.5618	3.2899	2.1329			4.5969	3.9268			4.2129	3.1271
30	4.2989	3.6495	3.3732	5.0596	3.4089	2.2520			4.7385	3.9959			4.2148	3.1844
31	4.5812	3.6146	3.4476	5.2834	3.4326	2.2416			4.7817	4.0572			4.1377	3.2699
32	5.0132	3.6127	3.5598	4.7309	3.3818	2.3217			4.8352	4.0877			4.1397	3.4566
33	5.2969	3.6799	3.4686	4.2912	3.4617	2.3366			4.8358	4.1846			4.2410	3.6208
34	5.0272	3.6313	3.3574	4.3035	3.5690	2.3233			4.8001	4.3059			4.1996	3.7530

Table S6.2 . Earnings Squared (earnsq)

"earnsq"'

	NB/1Child			NB/2+Child			BC/1Child				BC/2+Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-11							25.454	35.357			42.532	27.673		
-10							36.473	51.717			41.639	36.667		
-9							59.477	53.248			42.222	62.248		
-8							73.774	65.193			49.344	69.829		
-7							83.942	76.396			56.403	72.809		
-6							97.758	83.814			67.901	77.397		
-5							102.165	85.654			69.020	77.039		
-4							100.948	86.114			72.316	78.459		
-3							104.979	93.470			73.285	86.482		
-2							94.330	103.233			69.524	83.980		
-1							94.972	102.287			72.839	87.245		
0	6.089	7.120	7.887	4.985	5.155	22.507	107.719	107.401	7.927	15.072	88.248	97.527	12.246	15.950
1	8.923	9.237	9.023	8.661	6.851	24.984	117.206	105.956	13.806	23.160	107.140	103.901	17.744	19.264
2	8.622	10.903	9.957	11.187	9.139	23.984	127.340	109.156	16.979	28.795	117.922	109.247	35.685	23.383
3	9.856	12.758	11.789	11.308	12.277	23.620	132.639	117.005	18.293	29.591	123.004	110.429	38.295	24.633
4	10.536	15.445	12.445	14.035	14.378	24.040	137.921	118.549	21.442	34.048	126.545	106.155	40.263	24.719
5	13.672	17.403	12.740	18.557	15.623	25.841	136.618	120.608	25.813	35.069	128.776	102.807	40.956	26.006
6	18.780	19.756	14.784	27.432	15.777	27.700	139.191	121.287	29.208	36.673	132.371	101.394	44.364	26.486
7	22.355	22.119	17.211	35.288	16.723	28.760	147.622	118.519	32.585	40.120	133.614	99.742	45.892	29.417
8	27.709	23.425	22.165	37.122	18.206	28.491	156.270	120.938	35.766	39.545	133.434	103.860	49.106	30.323
9	27.843	25.278	32.956	37.439	19.451	28.069	160.775	124.879	37.966	40.424	143.238	110.258	54.169	30.481
10	29.883	28.303	34.201	39.183	24.507	14.307	163.527	127.509	40.472	43.840	144.678	115.403	60.129	33.869
11	28.043	31.046	34.961	39.970	26.703	15.420	169.032	129.007	42.812	40.548	146.716	118.970	63.838	35.690
12	30.926	31.765	37.614	40.325	27.897	14.714	173.204	128.560	45.268	40.603	152.380	122.225	63.569	34.877
13	29.728	33.040	38.167	40.774	27.710	16.339	174.787	129.145	52.286	43.452	152.420	114.947	65.350	35.851
14	29.526	33.860	37.973	35.172	29.080	19.051	182.510	131.441	59.470	50.590	159.550	115.088	66.923	36.141
15	32.151	33.927	34.901	31.397	29.549	19.954	183.845	133.585	60.149	53.089	174.898	111.491	67.897	33.908
16	32.974	34.778	33.664	30.132	29.494	20.746	188.552	136.991	59.624	54.273	183.917	114.890	69.384	32.558
17	32.769	34.923	34.857	31.346	29.514	22.116	206.750	136.539	59.670	52.863	193.477	120.749	70.593	32.025
18	38.616	40.742	39.043	37.352	30.877	25.250	222.152	123.636	57.161	56.866	155.564	135.570	74.590	80.399
19	41.230	42.790	40.533	38.958	33.135	25.634			71.526	65.272			79.737	93.894
20	43.846	43.703	39.550	37.391	31.115	24.919			95.748	72.305			75.963	96.699
21	46.689	40.873	39.250	36.042	30.687	25.638			94.149	94.137			74.593	97.045
22	48.694	41.278	38.347	34.969	32.043	25.308			94.223	94.213			74.533	100.334
23	51.181	40.713	37.831	34.779	33.042	23.816			91.988	85.853			75.846	100.432
24	49.525	41.728	41.508	34.658	34.824	23.040			92.174	84.718			75.780	103.075
25	49.064	41.283	43.198	35.837	33.078	24.322			90.086	89.918			74.552	98.156
26	45.621	41.800	43.857	39.413	33.225	24.082			88.053	83.653			75.589	96.640
27	41.475	40.890	46.487	46.397	33.502	24.790			90.214	66.749			77.122	91.696
28	45.469	42.327	54.645	53.929	38.739	28.789			97.611	65.973			81.740	154.303
29	44.734	42.644	55.874	56.098	40.884	30.365			108.562	75.207			87.529	165.304
30	50.345	43.842	56.067	68.301	42.841	32.612			111.976	76.557			85.672	166.328
31	58.668	41.957	56.131	77.912	42.927	32.858			113.829	90.976			85.917	168.621
32	65.626	41.808	57.496	59.401	43.450	34.460			114.418	92.045			85.576	172.178
33	71.272	45.172	55.390	53.500	44.406	34.819			114.925	95.920			89.010	175.469
34	67.248	44.225	54.112	53.925	45.922	34.983			109.300	98.517			86.911	178.007

Table S6.3 . IA Received (ia)

"ia"'

	NB/1Child			NB/2+Child			BC/1Child				BC/2+Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-11							7.5656	7.9439			9.7910	9.6669		
-10							6.9632	7.2136			9.3351	9.3276		
-9							6.5735	6.7883			8.9235	8.9517		
-8							6.2961	6.4415			8.4811	8.3570		
-7							6.1241	6.1384			8.2967	7.9789		
-6							5.9110	5.9111			7.9750	7.7369		
-5							5.7269	5.8666			7.7695	7.4872		
-4							5.8350	5.6033			7.6626	7.1928		
-3							5.5580	5.6437			7.4662	7.1310		
-2							5.5629	5.5595			7.4141	7.0463		
-1							5.4264	5.5952			7.3242	6.8944		
0	6.9306	6.5785	6.4386	7.1962	7.0116	7.1217	5.4473	5.5109	9.2903	9.0299	7.2193	6.9421	11.3119	11.2438
1	6.7056	6.3399	6.3476	6.6544	6.8273	7.0115	5.2440	5.4204	9.0912	8.8770	6.9907	6.6684	11.1598	11.1410
2	6.5091	6.1652	6.2502	6.5574	6.6132	6.9667	4.9411	5.3563	8.8701	8.7635	6.7862	6.5710	10.8084	11.0552
3	6.4089	5.8792	6.1279	6.0702	6.2275	6.7724	4.8053	5.1867	8.5722	8.7070	6.1141	6.3746	10.6960	10.7735
4	6.1267	5.6179	6.0633	6.0026	5.9467	6.5893	4.6986	5.0587	8.4134	8.4890	5.8603	6.2149	10.3053	10.6993
5	5.6878	5.4451	5.9183	5.8490	5.5762	6.4629	4.3005	4.9695	8.0789	8.4921	5.7202	6.1525	10.0963	10.5788
6	5.3622	5.2666	5.8196	5.6534	5.4969	6.3643	4.1679	4.9555	7.9435	8.3120	5.4762	5.9152	9.9870	10.4510
7	5.0104	5.1024	5.6917	5.3504	5.4510	6.2471	3.8607	4.7411	7.8877	8.2177	5.2668	5.7650	9.8255	10.2940
8	4.7144	4.9794	5.5798	5.2120	5.2876	6.2016	3.6767	4.5990	7.6827	8.2004	5.0959	5.6516	9.6460	10.1570
9	4.6026	4.8108	5.5501	5.1426	5.2055	6.1748	3.6195	4.3527	7.5909	8.1132	4.7256	5.3230	9.4320	10.1656
10	4.4034	4.6309	5.4867	4.8857	5.0691	5.9746	3.2691	4.2253	7.3833	8.0234	4.3748	5.1639	9.1813	9.9466
11	4.4471	4.5046	5.3891	4.4656	4.9851	6.0670	3.1756	4.0706	7.1766	7.8746	4.1170	4.9158	9.0590	9.8555
12	4.4673	4.4290	5.2490	4.6222	4.8441	5.9552	2.9386	4.0413	6.9913	7.8458	3.8877	4.7952	8.8332	9.7318
13	4.0862	4.3677	5.1690	4.6641	4.7518	5.8473	2.8420	3.9510	6.6832	7.8225	3.6648	4.4851	8.4775	9.8116
14	3.8107	4.2439	5.0916	4.7126	4.5351	5.7929	2.6847	3.8355	6.5414	7.6037	3.3083	4.2261	8.5362	9.5808
15	3.8374	4.2542	5.0762	4.8408	4.6251	5.7987	2.7478	3.7483	6.4380	7.6013	3.0875	4.1284	8.3820	9.5610
16	4.0102	4.1789	4.9564	4.6849	4.5612	5.7992	2.6267	3.4949	6.3190	7.3863	2.9307	3.9276	8.4233	9.5348
17	3.7874	4.1086	4.9121	4.4213	4.5156	5.6647	2.4900	3.3809	6.3756	7.3349	2.7748	3.7555	8.2233	9.2251
18	3.8210	4.0166	4.7669	4.3903	4.4291	5.7378			6.3376	7.1936			8.1966	9.2309
19	3.5122	3.9983	4.7153	4.3617	4.4581	5.7127			6.2017	7.1732			8.0314	9.0437
20	3.6549	3.9732	4.6715	4.6311	4.3811	5.7091			6.2428	7.1014			7.9211	8.7198
21	3.5529	3.9636	4.6598	4.5686	4.3561	5.7187			6.2147	6.9970			7.7317	8.5939
22	3.3963	3.9628	4.6709	4.5480	4.3983	5.4539			6.0887	6.8722			7.4375	8.2322
23	3.3646	3.9743	4.6605	4.6653	4.3856	5.4803			5.9593	6.7736			7.1836	8.2354
24	3.3500	3.9305	4.6133	4.6655	4.3464	5.4858			5.8128	6.5807			7.2326	8.0803
25	3.1730	3.9630	4.5801	4.6821	4.3607	5.4929			5.7787	6.4172			7.1104	7.8286
26	3.3922	3.9528	4.5800	4.6279	4.4225	5.4786			5.6659	6.0061			6.9398	7.8723
27	3.2721	3.8773	4.5003	4.5617	4.3508	5.4078			5.6825	6.1146			6.8972	7.6187
28	3.1794	3.8136	4.4561	4.4401	4.3287	5.5877			5.5470	5.9115			6.7132	7.5187
29	3.1542	3.8282	4.4092	4.2789	4.3137	5.4814			5.6593	5.8202			6.7762	7.4185
30	3.1933	3.8147	4.3394	4.2858	4.2941	5.3592			5.5214	5.7355			6.5592	7.0995
31	3.1706	3.8738	4.3730	4.1874	4.3428	5.4631			5.4640	5.7645			6.4119	7.1181
32	3.1759	3.8675	4.3104	4.0391	4.4722	5.5025			5.2575	5.7194			6.3488	6.9933
33	3.2967	3.9367	4.3175	4.0116	4.4919	5.4978			5.2007	5.5405			6.3554	6.9466
34	3.2097	3.8924	4.2447	4.1309	4.4465	5.5476			5.1597	5.5239			6.1872	6.7496

Table S6.4 . IA Received Squared (iasq)

"iasq"'

	NB/1Child			NB/2+Child			BC/1Child				BC/2+Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-11							71.373	77.236			117.202	114.371		
-10							66.146	70.310			112.174	111.363		
-9							62.797	66.934			107.154	107.714		
-8							59.141	63.269			99.714	100.141		
-7							58.179	60.401			99.228	95.228		
-6							57.196	58.475			94.678	90.734		
-5							54.163	57.680			92.368	88.490		
-4							57.686	54.658			90.916	86.016		
-3							52.892	56.803			89.230	85.093		
-2							53.554	54.632			88.683	84.187		
-1							50.702	55.427			88.060	81.591		
0	52.304	46.966	45.610	57.431	53.604	55.288	52.203	53.478	92.292	89.297	86.366	82.042	136.922	136.954
1	50.039	45.375	44.733	52.529	52.351	54.428	49.622	53.024	90.579	87.124	83.530	78.511	136.933	137.154
2	48.425	44.434	43.987	51.820	51.042	54.256	46.533	52.535	87.807	85.508	81.975	77.105	131.234	135.304
3	47.695	42.295	43.286	47.769	47.909	52.604	46.510	50.796	83.915	86.916	73.090	75.246	132.802	130.662
4	45.071	40.621	43.034	47.509	45.947	50.836	45.412	49.208	82.983	83.151	68.809	74.226	126.740	130.093
5	41.756	39.160	41.912	47.236	43.136	49.971	40.913	48.273	79.328	83.672	68.816	73.862	123.488	129.798
6	39.265	38.078	41.313	45.656	42.578	49.492	40.352	48.550	79.379	82.509	65.205	68.270	123.034	126.891
7	37.146	37.088	40.474	43.168	42.614	48.655	36.786	46.043	79.370	81.128	62.359	67.179	121.279	124.573
8	35.295	36.292	39.985	42.211	41.588	48.262	34.922	44.286	76.318	82.588	59.342	64.874	117.672	123.328
9	34.296	35.136	39.882	42.147	40.684	48.213	35.127	41.737	75.864	80.451	53.805	59.867	114.154	123.744
10	33.136	33.780	39.487	40.557	39.735	46.785	31.030	40.305	74.067	80.208	49.553	60.395	111.309	120.780
11	34.009	32.970	38.837	36.799	39.294	47.682	29.581	38.284	70.728	78.203	45.921	53.346	109.247	119.978
12	34.364	32.517	37.906	38.380	38.083	46.859	26.985	37.490	70.299	78.772	43.350	51.844	107.522	116.394
13	30.747	32.107	37.330	38.608	37.392	46.013	26.075	37.370	66.588	78.742	39.975	47.261	101.875	119.508
14	29.039	30.996	36.668	38.777	35.351	45.710	24.491	35.071	65.564	75.573	35.535	43.631	105.388	115.321
15	29.056	31.139	36.744	39.626	35.949	45.572	24.277	33.890	63.675	76.481	31.616	42.560	102.643	116.161
16	30.403	30.571	35.712	37.834	35.709	45.630	23.313	31.058	62.530	73.984	29.322	38.570	103.138	117.072
17	28.236	29.998	35.125	35.200	35.120	44.501	21.549	30.009	64.064	73.562	26.863	36.799	98.471	111.203
18	28.554	29.435	34.373	35.111	34.601	45.423			63.781	71.938			99.120	112.335
19	26.341	29.353	34.193	34.983	34.927	45.386			61.082	71.540			95.287	109.213
20	27.756	29.147	33.912	37.379	34.494	45.574			61.591	69.989			93.775	103.698
21	27.066	29.314	34.147	36.556	34.336	45.993			61.118	69.143			92.906	104.292
22	25.847	29.227	34.148	37.954	34.835	43.827			59.484	67.159			86.824	94.657
23	25.469	29.325	34.140	38.987	34.781	44.241			57.462	66.427			82.099	96.606
24	25.820	29.068	33.908	41.540	34.465	44.332			55.981	63.430			83.203	95.509
25	23.837	29.710	33.707	37.632	34.475	44.351			56.289	61.706			81.462	89.878
26	26.099	29.204	33.591	37.791	35.415	44.194			54.225	56.782			78.801	90.941
27	24.521	28.728	32.882	37.929	34.666	43.132			55.182	60.412			77.668	86.120
28	23.341	28.244	32.817	36.204	34.311	44.941			52.441	56.561			74.649	84.253
29	23.208	28.279	32.407	34.162	34.085	44.031			54.120	56.163			77.114	84.690
30	23.544	28.420	31.871	34.117	34.257	43.147			51.567	53.791			72.397	77.930
31	23.977	29.290	32.502	33.538	34.434	44.065			51.237	54.076			68.675	78.178
32	23.649	28.772	32.076	32.878	35.601	44.711			49.250	54.058			68.108	75.741
33	25.253	29.918	32.425	32.344	36.143	44.483			48.710	51.919			69.220	76.073
34	24.652	29.494	31.654	33.560	35.609	45.058			48.399	51.814			65.509	72.990

Table S6.5 . Supplement Received (gsu)

"gsu"'

t	NB/1Child Recipient SSP+	NB/1Child Recipient SSP	NB/1Child Recipient Ctrl	NB/2+Child Recipient SSP+	NB/2+Child Recipient SSP	NB/2+Child Recipient Ctrl	BC/1Child Applicants SSP	BC/1Child Applicants Ctrl	BC/1Child Recipient SSP	BC/1Child Recipient Ctrl	BC/2+Child Applicants SSP	BC/2+Child Applicants Ctrl	BC/2+Child Recipient SSP	BC/2+Child Recipient Ctrl
1	0.0000	0.0209		0.0000	0.0171		4.6995		0.0465		5.8810		0.0464	
2	0.1673	0.3421		0.1772	0.4241		8.2531		0.4078		7.0100		0.2772	
3	0.4798	0.6134		0.9779	0.7988		8.1776		0.5347		8.4733		0.4307	
4	0.5856	0.7992		1.0373	1.0321		8.5824		0.8339		7.9033		0.5684	
5	1.2443	1.0013		1.4646	1.2294		8.0338		0.9653		7.8193		0.8056	
6	1.7194	1.0733		1.7174	1.3043		8.3089		1.1433		8.4566		0.8776	
7	1.8480	1.2436		1.8189	1.4981		8.3805		1.1136		8.1639		0.9398	
8	1.9171	1.3720		1.7614	1.4661		8.6342		1.2513		7.8010		0.9416	
9	1.9920	1.3603		1.9258	1.5973		8.6709		1.3524		8.7624		1.1396	
10	2.0437	1.6938		2.2201	1.5562		8.3407		1.4581		8.4177		1.1909	
11	2.3004	1.6953		2.0763	1.6847		8.4749		1.6560		8.5107		1.3901	
12	2.1316	1.7837		1.7857	1.7483		8.4346		1.7815		8.1999		1.5765	
13	2.3791	1.8337		2.0777	1.8185		8.1478		2.1513		8.6428		1.7605	
14	2.8205	1.9313		2.3299	2.1158		8.3075		2.3363		8.2041		1.9974	
15	2.4134	1.7612		1.9761	2.1023		8.3552		2.1397		8.3235		1.9150	
16	2.7021	1.9773		2.5343	2.1128		7.8229		1.8932		7.9548		1.7083	
17	2.4008	1.9233		2.0524	2.2257		7.8954		1.9480		7.9795		1.4842	
18	2.5865	1.8466		2.3536	1.9737		8.4065		1.7488		8.2439		1.5121	
19	2.1581	1.8883		1.9034	1.9557				1.8569				1.5003	
20	2.4937	1.8786		2.1263	1.7176				1.7858				1.3418	
21	2.2631	1.8086		2.1141	1.8325				1.7674				1.2908	
22	2.2359	1.7252		2.2097	1.7977				1.5414				1.4054	
23	2.3085	1.7422		2.0219	1.7286				1.6686				1.3367	
24	2.0796	1.7346		1.8173	1.5771				1.6134				1.2856	
25	2.1312	1.8140		1.8490	1.5650				1.7413				1.3386	
26	2.3002	1.8125		2.1317	1.6561				1.6009				1.3172	
27	2.3447	1.7026		2.2080	1.7063				1.6712				1.3258	
28	2.4915	1.7307		2.1777	1.7790				1.5684				1.2112	
29	2.2454	1.6513		2.6854	1.6465				1.4838				1.2263	
30	2.2030	1.7938		2.3907	1.7670				1.4963				1.2178	
31	1.9578	1.7279		2.3337	1.6441				1.4334				1.2470	
32	2.4243	1.6397		2.4945	1.6893				1.4929				1.1344	
33	2.1737	1.7393		1.9897	1.5146				1.4690				1.1960	
34	2.0937	1.5025		2.0203	1.6474				1.4874				1.1895	

Table S6.6 . Receiving IA (onia)

"onia"'

	NB/1Child			NB/2+Child			BC/1Child				BC/2+Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-11							0.9260	0.9186			0.9169	0.9299		
-10							0.9923	0.9935			0.9927	0.9931		
-9							0.8750	0.8928			0.9169	0.8997		
-8							0.8061	0.8023			0.8615	0.8516		
-7							0.7628	0.7442			0.8265	0.8104		
-6							0.7360	0.7093			0.8003	0.7624		
-5							0.7092	0.6860			0.7711	0.7349		
-4							0.6773	0.6641			0.7434	0.7129		
-3							0.6658	0.6550			0.7216	0.6896		
-2							0.6620	0.6292			0.7099	0.6621		
-1							0.6441	0.6214			0.6851	0.6607		
0	0.9943	0.9935	0.9911	0.9915	1.0000	0.9945	0.6454	0.6176	0.9960	0.9933	0.6764	0.6497	0.9947	0.9931
1	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	0.6403	0.6202	0.9973	0.9947	0.6720	0.6415	0.9987	0.9972
2	0.9771	0.9765	0.9860	0.9576	0.9694	0.9687	0.6237	0.6214	0.9786	0.9774	0.6545	0.6374	0.9908	0.9834
3	0.9543	0.9400	0.9733	0.9153	0.9388	0.9558	0.6084	0.5969	0.9613	0.9667	0.6414	0.6154	0.9683	0.9751
4	0.9200	0.9035	0.9580	0.8729	0.9083	0.9429	0.5753	0.5995	0.9359	0.9521	0.6166	0.6099	0.9314	0.9640
5	0.9143	0.8644	0.9351	0.8051	0.8543	0.9208	0.5434	0.5801	0.9132	0.9321	0.5627	0.5893	0.9169	0.9447
6	0.8800	0.8201	0.9160	0.7881	0.8022	0.9116	0.5293	0.5698	0.8919	0.9214	0.5423	0.5742	0.9011	0.9322
7	0.8171	0.8005	0.8906	0.7458	0.7608	0.8895	0.4898	0.5594	0.8638	0.9134	0.5204	0.5659	0.8826	0.9281
8	0.7714	0.7718	0.8779	0.7288	0.7410	0.8711	0.4707	0.5504	0.8478	0.8908	0.5000	0.5577	0.8654	0.9170
9	0.7029	0.7432	0.8575	0.6864	0.7248	0.8545	0.4375	0.5220	0.8358	0.8815	0.4869	0.5398	0.8575	0.8990
10	0.6686	0.7184	0.8384	0.6610	0.6960	0.8416	0.4247	0.5168	0.8198	0.8695	0.4781	0.5426	0.8483	0.8921
11	0.6514	0.6884	0.8321	0.6441	0.6906	0.8324	0.4120	0.4948	0.7997	0.8668	0.4563	0.5165	0.8325	0.8907
12	0.6171	0.6649	0.8168	0.6186	0.6727	0.8066	0.3839	0.4871	0.7797	0.8535	0.4286	0.5082	0.8100	0.8741
13	0.6057	0.6467	0.8015	0.5763	0.6601	0.8140	0.3788	0.4780	0.7717	0.8469	0.4111	0.5014	0.8061	0.8797
14	0.6057	0.6349	0.7774	0.5847	0.6439	0.7974	0.3482	0.4806	0.7303	0.8362	0.3892	0.4931	0.7784	0.8728
15	0.5714	0.6258	0.7672	0.5932	0.6295	0.7864	0.3457	0.4574	0.7156	0.8402	0.3717	0.4753	0.7559	0.8603
16	0.5200	0.6128	0.7583	0.6102	0.6097	0.7790	0.3291	0.4574	0.6956	0.8149	0.3440	0.4547	0.7414	0.8479
17	0.5314	0.6154	0.7494	0.6186	0.6169	0.7790	0.3418	0.4561	0.6956	0.8083	0.3324	0.4492	0.7282	0.8396
18	0.5543	0.6050	0.7354	0.6102	0.6079	0.7772	0.3316	0.4264	0.6889	0.7936	0.3251	0.4396	0.7322	0.8396
19	0.5314	0.5984	0.7354	0.5847	0.6097	0.7624			0.6822	0.7843			0.7335	0.8271
20	0.5314	0.5828	0.7112	0.5847	0.5953	0.7587			0.6782	0.7670			0.7269	0.8147
21	0.4914	0.5815	0.6985	0.5763	0.6025	0.7532			0.6769	0.7750			0.7256	0.8050
22	0.5029	0.5750	0.6947	0.6102	0.5881	0.7514			0.6782	0.7696			0.7203	0.7870
23	0.4914	0.5671	0.6908	0.6102	0.5881	0.7422			0.6796	0.7603			0.7058	0.7732
24	0.4743	0.5750	0.6972	0.5678	0.5863	0.7127			0.6729	0.7590			0.6979	0.7621
25	0.4857	0.5776	0.6908	0.5763	0.5845	0.7164			0.6742	0.7443			0.6834	0.7607
26	0.4571	0.5711	0.6845	0.5678	0.5827	0.7072			0.6542	0.7350			0.6913	0.7469
27	0.4457	0.5671	0.6845	0.6017	0.5881	0.7145			0.6462	0.7177			0.6807	0.7400
28	0.4686	0.5684	0.6807	0.5847	0.5917	0.7164			0.6475	0.6844			0.6741	0.7441
29	0.4686	0.5593	0.6629	0.5847	0.5863	0.7201			0.6449	0.6804			0.6689	0.7317
30	0.4686	0.5489	0.6527	0.5763	0.5827	0.7311			0.6368	0.6671			0.6636	0.7303
31	0.4571	0.5502	0.6501	0.5678	0.5791	0.7182			0.6395	0.6525			0.6715	0.7192
32	0.4629	0.5489	0.6387	0.5593	0.5665	0.7090			0.6382	0.6618			0.6544	0.7026
33	0.4457	0.5528	0.6336	0.5424	0.5755	0.7145			0.6368	0.6631			0.6504	0.7109
34	0.4629	0.5528	0.6247	0.5085	0.5953	0.7201			0.6142	0.6578			0.6451	0.7095

Table S6.7 . Worked at Minimum Wage (mwg)

"mwg"'

	NB/1Child			NB/2+Child			BC/1Child				BC/2+Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-11							0.8393	0.8295			0.8163	0.8324		
-10							0.8214	0.8191			0.7974	0.8146		
-9							0.8087	0.8178			0.7959	0.7871		
-8							0.7959	0.8127			0.7784	0.7898		
-7							0.7883	0.8023			0.7697	0.7830		
-6							0.7564	0.8049			0.7653	0.7761		
-5							0.7589	0.7972			0.7595	0.7802		
-4							0.7577	0.7933			0.7668	0.7761		
-3							0.7538	0.7752			0.7638	0.7788		
-2							0.7309	0.7791			0.7522	0.7706		
-1							0.7385	0.7791			0.7551	0.7802		
0	0.8686	0.8618	0.8601	0.8644	0.8867	0.8821	0.7551	0.7506	0.8825	0.8668	0.7551	0.7802	0.8760	0.8894
1	0.8514	0.8383	0.8486	0.8390	0.8723	0.8711	0.7449	0.7429	0.8465	0.8349	0.7157	0.7569	0.8496	0.8741
2	0.8171	0.8475	0.8537	0.8475	0.8615	0.8766	0.7436	0.7455	0.8465	0.8216	0.7201	0.7624	0.8470	0.8617
3	0.8343	0.8318	0.8461	0.8390	0.8381	0.8766	0.7309	0.7494	0.8465	0.8216	0.7099	0.7596	0.8391	0.8589
4	0.8057	0.8214	0.8372	0.8136	0.8219	0.8674	0.7270	0.7481	0.8331	0.8123	0.7114	0.7541	0.8325	0.8575
5	0.7771	0.8083	0.8282	0.7627	0.8237	0.8564	0.7156	0.7545	0.8211	0.8083	0.7070	0.7569	0.8232	0.8603
6	0.7429	0.7862	0.8142	0.7373	0.8058	0.8471	0.7117	0.7519	0.8117	0.8096	0.7085	0.7651	0.8193	0.8631
7	0.6971	0.7679	0.8079	0.7288	0.8112	0.8435	0.7066	0.7636	0.7957	0.8123	0.7099	0.7637	0.8153	0.8575
8	0.6629	0.7627	0.8053	0.7373	0.8130	0.8471	0.6939	0.7636	0.7744	0.8123	0.7041	0.7692	0.7942	0.8479
9	0.6171	0.7484	0.8181	0.7373	0.8004	0.8527	0.6913	0.7584	0.7690	0.8149	0.7012	0.7665	0.7797	0.8409
10	0.6229	0.7340	0.8155	0.6949	0.7752	0.8564	0.6811	0.7468	0.7517	0.8083	0.6822	0.7582	0.7625	0.8409
11	0.6229	0.7275	0.7952	0.6780	0.7590	0.8600	0.6722	0.7481	0.7490	0.8056	0.6822	0.7555	0.7665	0.8368
12	0.6457	0.7158	0.7863	0.6949	0.7464	0.8637	0.6569	0.7519	0.7330	0.8003	0.6676	0.7514	0.7533	0.8340
13	0.6800	0.6988	0.7850	0.7119	0.7320	0.8398	0.6518	0.7403	0.6943	0.7976	0.6604	0.7486	0.7375	0.8285
14	0.6914	0.7080	0.7888	0.7034	0.7320	0.8343	0.6531	0.7364	0.6903	0.7963	0.6487	0.7473	0.7388	0.8409
15	0.6857	0.7119	0.7748	0.7034	0.7410	0.8306	0.6429	0.7261	0.6983	0.7843	0.6458	0.7390	0.7414	0.8326
16	0.7200	0.7197	0.7710	0.7288	0.7392	0.8306	0.6505	0.7326	0.7183	0.7896	0.6472	0.7253	0.7467	0.8354
17	0.7314	0.7210	0.7723	0.7288	0.7536	0.8287	0.6824	0.7700	0.7303	0.7870	0.7026	0.7692	0.7665	0.8299
18	0.7143	0.7249	0.7621	0.7203	0.7626	0.8361	0.9375	0.9599	0.7356	0.7923	0.9490	0.9492	0.7757	0.8271
19	0.7486	0.7301	0.7634	0.7288	0.7860	0.8361			0.7423	0.7856			0.7902	0.8326
20	0.7771	0.7419	0.7646	0.7373	0.8022	0.8453			0.7570	0.7936			0.7876	0.8465
21	0.7829	0.7432	0.7774	0.7203	0.7986	0.8416			0.7624	0.8016			0.8021	0.8506
22	0.7657	0.7497	0.7875	0.7288	0.8058	0.8398			0.7704	0.8069			0.7995	0.8479
23	0.7600	0.7471	0.7901	0.7373	0.7932	0.8471			0.7717	0.8043			0.7982	0.8465
24	0.7429	0.7497	0.7888	0.7542	0.7986	0.8545			0.7757	0.8109			0.7995	0.8451
25	0.7543	0.7445	0.7901	0.7542	0.8058	0.8545			0.7770	0.8096			0.7995	0.8479
26	0.7600	0.7419	0.7875	0.7458	0.7986	0.8545			0.7770	0.8123			0.8047	0.8589
27	0.7486	0.7392	0.7799	0.7373	0.8076	0.8656			0.7810	0.8123			0.8021	0.8631
28	0.6971	0.7392	0.7710	0.6780	0.7788	0.8545			0.7730	0.8029			0.7810	0.8603
29	0.6971	0.7379	0.7761	0.6525	0.7806	0.8508			0.7677	0.7936			0.7704	0.8520
30	0.6971	0.7223	0.7710	0.6271	0.7644	0.8324			0.7650	0.7923			0.7652	0.8437
31	0.6743	0.7197	0.7659	0.6102	0.7608	0.8361			0.7597	0.7896			0.7757	0.8451
32	0.6514	0.7145	0.7519	0.6356	0.7590	0.8195			0.7570	0.7870			0.7718	0.8340
33	0.6400	0.7197	0.7532	0.6695	0.7536	0.8269			0.7597	0.7830			0.7691	0.8285
34	0.6457	0.7158	0.7570	0.6610	0.7464	0.8287			0.7597	0.7776			0.7639	0.8257

Table S6.8 . Lost Job (lost)

"lost"'

	NB/1Child			NB/2+Child			BC/1Child				BC/2+Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-10							0.0000	0.0000			0.0000	0.0014		
-9							0.0013	0.0013			0.0102	0.0014		
-8							0.0038	0.0090			0.0073	0.0082		
-7							0.0077	0.0065			0.0029	0.0055		
-6							0.0051	0.0026			0.0044	0.0055		
-5							0.0077	0.0039			0.0029	0.0055		
-4							0.0064	0.0078			0.0058	0.0041		
-3							0.0128	0.0065			0.0058	0.0055		
-2							0.0051	0.0065			0.0058	0.0041		
-1							0.0102	0.0052			0.0175	0.0096		
0							0.0140	0.0103			0.0029	0.0069		
1	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0089	0.0078	0.0000	0.0000	0.0044	0.0027	0.0000	0.0000
2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0102	0.0065	0.0000	0.0000	0.0058	0.0041	0.0000	0.0000
3	0.0000	0.0013	0.0000	0.0000	0.0000	0.0000	0.0038	0.0078	0.0000	0.0013	0.0015	0.0055	0.0000	0.0000
4	0.0000	0.0013	0.0000	0.0000	0.0018	0.0000	0.0064	0.0026	0.0000	0.0000	0.0000	0.0055	0.0000	0.0000
5	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0026	0.0039	0.0027	0.0000	0.0029	0.0000	0.0000	0.0000
6	0.0000	0.0000	0.0013	0.0000	0.0000	0.0000	0.0051	0.0013	0.0000	0.0000	0.0044	0.0055	0.0000	0.0000
7	0.0000	0.0000	0.0000	0.0000	0.0000	0.0018	0.0051	0.0026	0.0000	0.0000	0.0044	0.0027	0.0000	0.0000
8	0.0000	0.0013	0.0000	0.0000	0.0000	0.0000	0.0051	0.0039	0.0000	0.0000	0.0044	0.0041	0.0000	0.0000
9	0.0000	0.0013	0.0000	0.0000	0.0036	0.0018	0.0013	0.0039	0.0013	0.0000	0.0044	0.0041	0.0000	0.0000
10	0.0057	0.0000	0.0013	0.0000	0.0000	0.0000	0.0026	0.0039	0.0000	0.0000	0.0058	0.0041	0.0000	0.0000
11	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0064	0.0052	0.0027	0.0013	0.0029	0.0055	0.0013	0.0000
12	0.0000	0.0013	0.0000	0.0000	0.0000	0.0000	0.0051	0.0039	0.0013	0.0000	0.0058	0.0014	0.0000	0.0000
13	0.0000	0.0013	0.0000	0.0000	0.0000	0.0018	0.0051	0.0052	0.0013	0.0000	0.0058	0.0041	0.0000	0.0000
14	0.0000	0.0026	0.0000	0.0000	0.0000	0.0000	0.0089	0.0103	0.0013	0.0000	0.0058	0.0027	0.0013	0.0014
15	0.0000	0.0000	0.0000	0.0000	0.0018	0.0018	0.0038	0.0052	0.0027	0.0000	0.0015	0.0027	0.0000	0.0000
16	0.0000	0.0000	0.0013	0.0000	0.0000	0.0000	0.0089	0.0052	0.0013	0.0027	0.0102	0.0055	0.0000	0.0000
17	0.0000	0.0013	0.0013	0.0000	0.0018	0.0000	0.0038	0.0052	0.0000	0.0000	0.0058	0.0055	0.0000	0.0014
18	0.0000	0.0000	0.0013	0.0000	0.0036	0.0018	0.0013	0.0013	0.0000	0.0000	0.0000	0.0027	0.0013	0.0000
19	0.0000	0.0039	0.0038	0.0000	0.0072	0.0074			0.0027	0.0000			0.0013	0.0041
20	0.0057	0.0052	0.0076	0.0085	0.0054	0.0092			0.0040	0.0013			0.0053	0.0028
21	0.0171	0.0078	0.0064	0.0000	0.0000	0.0037			0.0040	0.0040			0.0053	0.0000
22	0.0057	0.0091	0.0064	0.0000	0.0072	0.0037			0.0067	0.0013			0.0026	0.0014
23	0.0057	0.0039	0.0076	0.0085	0.0018	0.0074			0.0027	0.0027			0.0026	0.0014
24	0.0057	0.0052	0.0089	0.0000	0.0054	0.0074			0.0067	0.0027			0.0040	0.0000
25	0.0057	0.0052	0.0051	0.0085	0.0090	0.0000			0.0053	0.0027			0.0026	0.0069
26	0.0057	0.0091	0.0013	0.0000	0.0054	0.0018			0.0040	0.0013			0.0066	0.0055
27	0.0057	0.0065	0.0064	0.0000	0.0072	0.0184			0.0027	0.0027			0.0040	0.0041
28	0.0114	0.0065	0.0076	0.0000	0.0108	0.0055			0.0053	0.0027			0.0013	0.0055
29	0.0114	0.0104	0.0102	0.0085	0.0090	0.0074			0.0040	0.0000			0.0066	0.0014
30	0.0000	0.0065	0.0051	0.0000	0.0036	0.0037			0.0040	0.0027			0.0053	0.0028
31	0.0171	0.0052	0.0115	0.0000	0.0144	0.0037			0.0013	0.0053			0.0026	0.0028
32	0.0057	0.0000	0.0076	0.0169	0.0090	0.0055			0.0040	0.0053			0.0119	0.0041
33	0.0000	0.0117	0.0140	0.0000	0.0018	0.0129			0.0080	0.0107			0.0066	0.0028
34	0.0286	0.0091	0.0115	0.0000	0.0090	0.0111			0.0040	0.0040			0.0040	0.0028

Table S6.9 . Left Job (left)

"left"'

	NB/1Child			NB/2+Child			BC/1Child				BC/2+Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-10							0.0000	0.0000			0.0000	0.0000		
-9							0.0102	0.0090			0.0146	0.0096		
-8							0.0051	0.0078			0.0058	0.0110		
-7							0.0026	0.0078			0.0058	0.0124		
-6							0.0051	0.0103			0.0073	0.0041		
-5							0.0064	0.0078			0.0073	0.0082		
-4							0.0115	0.0065			0.0131	0.0110		
-3							0.0089	0.0039			0.0044	0.0124		
-2							0.0089	0.0103			0.0073	0.0069		
-1							0.0077	0.0078			0.0044	0.0069		
0							0.0051	0.0090			0.0087	0.0041		
1	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0217	0.0181	0.0000	0.0000	0.0044	0.0110	0.0000	0.0000
2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0077	0.0078	0.0000	0.0000	0.0087	0.0124	0.0000	0.0000
3	0.0000	0.0013	0.0000	0.0000	0.0000	0.0000	0.0051	0.0078	0.0000	0.0000	0.0044	0.0069	0.0000	0.0000
4	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0077	0.0065	0.0000	0.0000	0.0029	0.0055	0.0000	0.0000
5	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0077	0.0052	0.0000	0.0000	0.0044	0.0082	0.0000	0.0014
6	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0064	0.0039	0.0013	0.0000	0.0058	0.0069	0.0000	0.0000
7	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0051	0.0129	0.0013	0.0000	0.0044	0.0027	0.0000	0.0000
8	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0038	0.0052	0.0013	0.0000	0.0029	0.0027	0.0000	0.0000
9	0.0000	0.0000	0.0013	0.0000	0.0000	0.0000	0.0064	0.0039	0.0013	0.0000	0.0029	0.0096	0.0000	0.0000
10	0.0057	0.0000	0.0000	0.0000	0.0000	0.0000	0.0064	0.0000	0.0000	0.0000	0.0015	0.0041	0.0000	0.0000
11	0.0000	0.0000	0.0000	0.0000	0.0018	0.0000	0.0026	0.0039	0.0000	0.0000	0.0029	0.0041	0.0026	0.0000
12	0.0000	0.0013	0.0000	0.0000	0.0018	0.0000	0.0077	0.0052	0.0000	0.0000	0.0044	0.0055	0.0000	0.0014
13	0.0000	0.0013	0.0000	0.0000	0.0000	0.0000	0.0102	0.0052	0.0000	0.0013	0.0058	0.0055	0.0013	0.0000
14	0.0000	0.0000	0.0013	0.0000	0.0000	0.0000	0.0089	0.0039	0.0013	0.0000	0.0029	0.0069	0.0000	0.0014
15	0.0000	0.0013	0.0013	0.0000	0.0018	0.0000	0.0013	0.0078	0.0013	0.0013	0.0015	0.0055	0.0013	0.0000
16	0.0057	0.0013	0.0025	0.0000	0.0018	0.0000	0.0064	0.0103	0.0013	0.0000	0.0073	0.0041	0.0000	0.0000
17	0.0000	0.0013	0.0013	0.0085	0.0018	0.0037	0.0038	0.0039	0.0040	0.0000	0.0073	0.0027	0.0026	0.0014
18	0.0057	0.0039	0.0000	0.0000	0.0018	0.0000	0.0000	0.0026	0.0013	0.0013	0.0000	0.0014	0.0000	0.0000
19	0.0057	0.0000	0.0025	0.0000	0.0072	0.0037			0.0027	0.0040			0.0040	0.0055
20	0.0057	0.0078	0.0038	0.0000	0.0054	0.0037			0.0093	0.0053			0.0040	0.0028
21	0.0057	0.0039	0.0089	0.0085	0.0036	0.0018			0.0067	0.0040			0.0066	0.0028
22	0.0057	0.0026	0.0038	0.0085	0.0072	0.0018			0.0093	0.0080			0.0040	0.0014
23	0.0000	0.0065	0.0013	0.0000	0.0072	0.0074			0.0040	0.0053			0.0079	0.0055
24	0.0057	0.0104	0.0000	0.0085	0.0036	0.0055			0.0040	0.0080			0.0053	0.0069
25	0.0057	0.0065	0.0038	0.0000	0.0036	0.0000			0.0053	0.0067			0.0066	0.0055
26	0.0000	0.0065	0.0102	0.0085	0.0036	0.0055			0.0013	0.0080			0.0066	0.0055
27	0.0000	0.0052	0.0038	0.0000	0.0108	0.0111			0.0107	0.0067			0.0079	0.0041
28	0.0000	0.0065	0.0051	0.0000	0.0054	0.0055			0.0053	0.0067			0.0040	0.0014
29	0.0000	0.0091	0.0064	0.0000	0.0090	0.0037			0.0053	0.0040			0.0066	0.0028
30	0.0114	0.0039	0.0038	0.0085	0.0054	0.0074			0.0067	0.0080			0.0092	0.0041
31	0.0000	0.0052	0.0051	0.0000	0.0036	0.0166			0.0147	0.0067			0.0119	0.0069
32	0.0000	0.0156	0.0076	0.0085	0.0072	0.0018			0.0040	0.0027			0.0013	0.0014
33	0.0000	0.0039	0.0051	0.0254	0.0054	0.0055			0.0040	0.0107			0.0040	0.0041
34	0.0000	0.0130	0.0089	0.0085	0.0036	0.0018			0.0067	0.0067			0.0053	0.0028

Table S6.10. Employment Full-Time (emft)

"emft"'

	NB/1Child			NB/2+Child			BC/1Child				BC/2+Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-11							0.1148	0.1370			0.1152	0.1223		
-10							0.1377	0.1489			0.1305	0.1329		
-9							0.1551	0.1421			0.1396	0.1571		
-8							0.1765	0.1598			0.1487	0.1571		
-7							0.1832	0.1735			0.1609	0.1629		
-6							0.2166	0.1817			0.1684	0.1686		
-5							0.2206	0.1954			0.1745	0.1657		
-4							0.2299	0.2036			0.1745	0.1743		
-3							0.2326	0.2186			0.1806	0.1714		
-2							0.2423	0.2230			0.1824	0.1753		
-1							0.2395	0.2304			0.1917	0.1718		
0	0.1200	0.0678	0.0814	0.0593	0.0558	0.0939	0.2525	0.2474	0.0507	0.0759	0.2122	0.2063	0.0633	0.0636
1	0.1034	0.0863	0.0933	0.0940	0.0778	0.1032	0.2815	0.2648	0.0746	0.0990	0.2425	0.2168	0.0928	0.0767
2	0.1264	0.1036	0.0946	0.1026	0.1130	0.1032	0.2863	0.2720	0.0887	0.1004	0.2530	0.2254	0.1064	0.0839
3	0.1379	0.1275	0.1012	0.1197	0.1389	0.1051	0.3010	0.2568	0.1000	0.1032	0.2715	0.2238	0.1173	0.0839
4	0.1724	0.1434	0.1025	0.1453	0.1500	0.1126	0.3084	0.2568	0.1239	0.1172	0.2782	0.2333	0.1241	0.0810
5	0.1897	0.1607	0.1130	0.2222	0.1611	0.1069	0.3304	0.2553	0.1493	0.1158	0.2816	0.2302	0.1378	0.0781
6	0.2126	0.1806	0.1170	0.2308	0.1870	0.1144	0.3348	0.2644	0.1620	0.1269	0.3002	0.2270	0.1405	0.0781
7	0.2529	0.2005	0.1301	0.2479	0.1870	0.1163	0.3480	0.2568	0.1817	0.1227	0.2951	0.2365	0.1487	0.0897
8	0.2701	0.2112	0.1406	0.2393	0.1981	0.1201	0.3686	0.2614	0.1944	0.1172	0.3137	0.2381	0.1664	0.0984
9	0.3046	0.2271	0.1340	0.2564	0.2111	0.1182	0.3686	0.2614	0.1958	0.1227	0.3187	0.2397	0.1869	0.1085
10	0.3161	0.2523	0.1393	0.3162	0.2463	0.1163	0.3818	0.2675	0.2211	0.1269	0.3356	0.2444	0.1992	0.1172
11	0.3448	0.2709	0.1564	0.3248	0.2648	0.1144	0.3935	0.2872	0.2352	0.1367	0.3474	0.2492	0.2169	0.1172
12	0.3563	0.2895	0.1695	0.3077	0.2870	0.1144	0.4038	0.2918	0.2535	0.1367	0.3609	0.2540	0.2374	0.1245
13	0.3448	0.3068	0.1748	0.3333	0.3093	0.1257	0.4082	0.3009	0.2873	0.1450	0.3811	0.2508	0.2578	0.1187
14	0.3391	0.3147	0.1761	0.3248	0.3111	0.1351	0.4170	0.3085	0.2901	0.1437	0.3912	0.2571	0.2538	0.1114
15	0.3448	0.3094	0.1892	0.3077	0.3056	0.1388	0.4242	0.3024	0.2845	0.1534	0.4030	0.2648	0.2551	0.1114
16	0.3276	0.3059	0.1958	0.3162	0.3222	0.1482	0.4125	0.2972	0.2638	0.1566	0.4142	0.2732	0.2483	0.1085
17	0.3276	0.3209	0.1910	0.3162	0.3123	0.1528	0.4315	0.3016	0.2652	0.1559	0.4074	0.2757	0.2397	0.1126
18	0.3500	0.3403	0.1984	0.3130	0.3031	0.1532	0.4474	0.2540	0.2749	0.1652	0.3804	0.3482	0.2429	0.1140
19	0.3585	0.3362	0.2014	0.3130	0.2899	0.1423			0.2786	0.1847			0.2326	0.1196
20	0.3522	0.3205	0.2036	0.3043	0.2774	0.1328			0.2775	0.1772			0.2391	0.1180
21	0.3333	0.3167	0.1911	0.3130	0.2854	0.1489			0.2681	0.1804			0.2284	0.1236
22	0.3459	0.3124	0.1897	0.2870	0.2774	0.1509			0.2625	0.1793			0.2264	0.1308
23	0.3585	0.3153	0.1897	0.2696	0.2834	0.1429			0.2673	0.1729			0.2325	0.1407
24	0.3648	0.3153	0.1925	0.2696	0.2754	0.1449			0.2657	0.1696			0.2310	0.1374
25	0.3522	0.3167	0.1897	0.2783	0.2754	0.1529			0.2705	0.1777			0.2325	0.1407
26	0.3522	0.2981	0.2036	0.2783	0.2814	0.1549			0.2738	0.1745			0.2295	0.1308
27	0.3711	0.2967	0.2134	0.2870	0.2774	0.1449			0.2576	0.1809			0.2371	0.1275
28	0.3396	0.2981	0.2218	0.3478	0.2774	0.1549			0.2657	0.1842			0.2462	0.1358
29	0.3333	0.2953	0.2218	0.3826	0.2794	0.1529			0.2657	0.2036			0.2523	0.1457
30	0.3522	0.3053	0.2190	0.4174	0.2954	0.1650			0.2754	0.2068			0.2523	0.1573
31	0.3585	0.3067	0.2287	0.4087	0.2934	0.1610			0.2802	0.2019			0.2416	0.1507
32	0.4025	0.3167	0.2329	0.3739	0.2834	0.1590			0.2882	0.2068			0.2386	0.1606
33	0.4151	0.3010	0.2218	0.3652	0.2974	0.1529			0.2850	0.2084			0.2462	0.1705
34	0.4088	0.2981	0.2064	0.3826	0.3034	0.1549			0.2882	0.2100			0.2447	0.1738

Table S6.11. Employment Part-Time (empt)

"empt"'

	NB/1Child			NB/2+Child			BC/1Child				BC/2+Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-11							0.1020	0.0736			0.1210	0.1003		
-10							0.1243	0.0874			0.1335	0.1343		
-9							0.1297	0.1107			0.1275	0.1300		
-8							0.1257	0.1011			0.1396	0.1371		
-7							0.1283	0.1052			0.1381	0.1386		
-6							0.1324	0.1066			0.1396	0.1443		
-5							0.1350	0.1161			0.1411	0.1429		
-4							0.1324	0.1148			0.1366	0.1457		
-3							0.1310	0.1270			0.1396	0.1443		
-2							0.1406	0.1286			0.1459	0.1509		
-1							0.1442	0.1299			0.1406	0.1557		
0	0.1086	0.1434	0.1616	0.1610	0.1205	0.0884	0.1349	0.1526	0.1242	0.1278	0.1431	0.1344	0.1121	0.0954
1	0.1322	0.1673	0.1813	0.1538	0.1352	0.0957	0.1158	0.1331	0.1549	0.1618	0.1505	0.1345	0.1282	0.1158
2	0.1322	0.1421	0.1721	0.1453	0.1093	0.0938	0.1116	0.1261	0.1394	0.1520	0.1450	0.1254	0.1146	0.1100
3	0.1264	0.1381	0.1695	0.1368	0.1185	0.0938	0.1189	0.1307	0.1197	0.1478	0.1484	0.1238	0.1132	0.1129
4	0.1207	0.1262	0.1774	0.1453	0.1296	0.0957	0.1160	0.1353	0.1141	0.1423	0.1568	0.1254	0.1119	0.1172
5	0.1494	0.1248	0.1748	0.1368	0.1204	0.1032	0.1160	0.1322	0.1042	0.1464	0.1551	0.1254	0.1064	0.1201
6	0.1667	0.1301	0.1866	0.1538	0.1185	0.1032	0.1204	0.1307	0.1000	0.1381	0.1518	0.1286	0.1078	0.1114
7	0.1494	0.1288	0.1853	0.1538	0.1167	0.0994	0.1204	0.1292	0.1014	0.1423	0.1568	0.1317	0.1078	0.1071
8	0.1667	0.1248	0.1800	0.1368	0.1130	0.0957	0.1204	0.1292	0.1099	0.1520	0.1518	0.1317	0.1173	0.1085
9	0.1724	0.1248	0.1748	0.1197	0.1093	0.0863	0.1233	0.1353	0.1099	0.1478	0.1518	0.1286	0.1160	0.1129
10	0.1667	0.1089	0.1643	0.1026	0.0981	0.0863	0.1233	0.1413	0.1085	0.1464	0.1518	0.1302	0.1160	0.1172
11	0.1379	0.1023	0.1590	0.1282	0.0944	0.0863	0.1233	0.1231	0.1099	0.1381	0.1501	0.1302	0.1037	0.1187
12	0.1379	0.1049	0.1577	0.1453	0.0926	0.0844	0.1292	0.1231	0.1099	0.1409	0.1535	0.1333	0.1050	0.1143
13	0.1494	0.1142	0.1590	0.1026	0.1000	0.0938	0.1278	0.1307	0.1239	0.1367	0.1551	0.1413	0.1119	0.1274
14	0.1667	0.1049	0.1590	0.1111	0.1056	0.0994	0.1204	0.1216	0.1183	0.1353	0.1619	0.1460	0.1187	0.1216
15	0.1609	0.1116	0.1524	0.1111	0.1019	0.1088	0.1252	0.1292	0.1169	0.1409	0.1602	0.1531	0.1201	0.1274
16	0.1667	0.1157	0.1656	0.1026	0.1019	0.1088	0.1291	0.1362	0.1227	0.1469	0.1525	0.1707	0.1241	0.1259
17	0.1437	0.1123	0.1713	0.0855	0.0967	0.1170	0.1334	0.1371	0.1191	0.1601	0.1502	0.1730	0.1178	0.1287
18	0.1813	0.1162	0.1821	0.1130	0.1216	0.1297	0.1140	0.1190	0.1293	0.1563	0.1739	0.1161	0.1157	0.1356
19	0.1635	0.1182	0.1833	0.1043	0.1164	0.1283			0.1269	0.1634			0.1230	0.1340
20	0.1635	0.1197	0.1855	0.0957	0.1238	0.1187			0.1228	0.1646			0.1158	0.1262
21	0.1572	0.1213	0.1785	0.1043	0.1218	0.1147			0.1220	0.1546			0.1104	0.1186
22	0.1572	0.1241	0.1799	0.1130	0.1218	0.1147			0.1143	0.1551			0.1170	0.1225
23	0.1635	0.1213	0.1841	0.1130	0.1178	0.1127			0.1176	0.1664			0.1185	0.1126
24	0.1572	0.1270	0.1855	0.1043	0.1317	0.1087			0.1159	0.1648			0.1170	0.1142
25	0.1635	0.1312	0.1911	0.0783	0.1357	0.1107			0.1143	0.1535			0.1216	0.1142
26	0.1698	0.1427	0.1855	0.0783	0.1417	0.1167			0.1127	0.1583			0.1231	0.1159
27	0.1572	0.1398	0.1855	0.0870	0.1317	0.1107			0.1256	0.1502			0.1216	0.1192
28	0.1698	0.1312	0.1799	0.0783	0.1357	0.1087			0.1176	0.1502			0.1170	0.1126
29	0.1572	0.1255	0.1674	0.0783	0.1198	0.1066			0.1208	0.1438			0.1094	0.1126
30	0.1447	0.1241	0.1799	0.0783	0.1118	0.1087			0.1159	0.1405			0.1064	0.1142
31	0.1321	0.1298	0.1771	0.1043	0.1078	0.1107			0.1095	0.1454			0.1033	0.1209
32	0.1321	0.1270	0.1813	0.1043	0.1038	0.1328			0.1111	0.1486			0.1094	0.1291
33	0.1321	0.1355	0.1897	0.0783	0.1058	0.1368			0.1095	0.1405			0.1064	0.1291
34	0.1195	0.1369	0.1953	0.0783	0.1018	0.1348			0.1079	0.1470			0.1216	0.1341

Table S6.12. Employed and on IA (oniaXem)

"onXem"'

	NB/1Child			NB/2+Child			BC/1Child				BC/2+Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-11							0.1837	0.1899			0.2055	0.2074		
-10							0.2580	0.2350			0.2625	0.2629		
-9							0.2059	0.1885			0.2215	0.2243		
-8							0.1898	0.1557			0.2140	0.2043		
-7							0.1778	0.1407			0.2109	0.1914		
-6							0.2045	0.1298			0.1988	0.1843		
-5							0.1979	0.1311			0.1882	0.1757		
-4							0.1858	0.1311			0.1654	0.1714		
-3							0.1751	0.1475			0.1639	0.1571		
-2							0.1834	0.1341			0.1702	0.1537		
-1							0.1823	0.1243			0.1592	0.1674		
0	0.2286	0.2086	0.2379	0.2203	0.1763	0.1805	0.1879	0.1496	0.1749	0.1997	0.1727	0.1563	0.1741	0.1563
1	0.2356	0.2537	0.2746	0.2479	0.2130	0.1989	0.1921	0.1604	0.2282	0.2580	0.1990	0.1582	0.2210	0.1925
2	0.2414	0.2324	0.2576	0.2308	0.2037	0.1895	0.1777	0.1641	0.2155	0.2343	0.1855	0.1508	0.2169	0.1823
3	0.2414	0.2377	0.2576	0.2137	0.2241	0.1839	0.1836	0.1444	0.1972	0.2301	0.2024	0.1333	0.2128	0.1852
4	0.2414	0.2112	0.2536	0.2479	0.2130	0.1914	0.1630	0.1489	0.1972	0.2287	0.1973	0.1397	0.1924	0.1780
5	0.2759	0.1939	0.2549	0.2393	0.1815	0.1857	0.1512	0.1353	0.1944	0.2176	0.1602	0.1349	0.1883	0.1664
6	0.2874	0.1846	0.2589	0.2393	0.1648	0.1914	0.1410	0.1353	0.1859	0.2134	0.1484	0.1190	0.1855	0.1534
7	0.2701	0.1886	0.2576	0.2137	0.1333	0.1726	0.1233	0.1261	0.1887	0.1980	0.1366	0.1190	0.1733	0.1635
8	0.2471	0.1740	0.2562	0.1795	0.1370	0.1689	0.1219	0.1201	0.1887	0.1980	0.1315	0.1190	0.1883	0.1563
9	0.2241	0.1647	0.2378	0.1453	0.1259	0.1707	0.1057	0.1125	0.1831	0.1897	0.1298	0.1016	0.2005	0.1664
10	0.2241	0.1633	0.2260	0.1624	0.1389	0.1632	0.1131	0.1140	0.1986	0.1841	0.1366	0.1063	0.2101	0.1708
11	0.2069	0.1527	0.2352	0.1966	0.1426	0.1482	0.1116	0.1049	0.1944	0.1869	0.1248	0.0984	0.2005	0.1722
12	0.2011	0.1580	0.2392	0.1709	0.1519	0.1388	0.1043	0.1003	0.1944	0.1883	0.1180	0.1063	0.2019	0.1722
13	0.1839	0.1607	0.2365	0.1453	0.1704	0.1538	0.1057	0.0957	0.2183	0.1869	0.1248	0.1127	0.2196	0.1881
14	0.1897	0.1514	0.2273	0.1453	0.1593	0.1576	0.0837	0.0957	0.1859	0.1729	0.1079	0.1143	0.2005	0.1679
15	0.1552	0.1355	0.2208	0.1453	0.1426	0.1557	0.0839	0.0775	0.1718	0.1869	0.0961	0.1100	0.1787	0.1621
16	0.1092	0.1343	0.2234	0.1282	0.1315	0.1595	0.0712	0.0805	0.1523	0.1818	0.0832	0.1138	0.1705	0.1563
17	0.1264	0.1337	0.2240	0.1368	0.1152	0.1679	0.0711	0.0841	0.1489	0.1896	0.0576	0.1127	0.1466	0.1623
18	0.1625	0.1331	0.2255	0.1565	0.1255	0.1788	0.0702	0.0476	0.1575	0.1873	0.0870	0.0714	0.1514	0.1680
19	0.1384	0.1296	0.2361	0.1043	0.1243	0.1663			0.1486	0.2046			0.1467	0.1643
20	0.1509	0.1168	0.2232	0.0957	0.1257	0.1449			0.1419	0.1835			0.1459	0.1557
21	0.1195	0.1113	0.2162	0.1130	0.1277	0.1469			0.1316	0.1820			0.1377	0.1516
22	0.1321	0.1170	0.2092	0.1130	0.1198	0.1489			0.1337	0.1729			0.1353	0.1424
23	0.1447	0.1198	0.2050	0.1130	0.1198	0.1388			0.1433	0.1777			0.1383	0.1358
24	0.1384	0.1241	0.2078	0.0696	0.1198	0.1368			0.1320	0.1761			0.1307	0.1374
25	0.1384	0.1298	0.2106	0.0609	0.1337	0.1449			0.1465	0.1696			0.1337	0.1358
26	0.1258	0.1284	0.2050	0.0696	0.1397	0.1388			0.1304	0.1664			0.1383	0.1258
27	0.1447	0.1255	0.2148	0.0783	0.1297	0.1328			0.1256	0.1583			0.1307	0.1209
28	0.1384	0.1184	0.2064	0.1043	0.1397	0.1288			0.1304	0.1454			0.1277	0.1275
29	0.1195	0.1113	0.1855	0.1043	0.1218	0.1288			0.1385	0.1519			0.1246	0.1225
30	0.1258	0.1084	0.1911	0.1217	0.1158	0.1288			0.1401	0.1502			0.1201	0.1209
31	0.1069	0.1241	0.1925	0.1217	0.1138	0.1227			0.1304	0.1454			0.1140	0.1225
32	0.1258	0.1255	0.1855	0.0957	0.0958	0.1388			0.1353	0.1486			0.1125	0.1308
33	0.1132	0.1213	0.1757	0.0696	0.1078	0.1429			0.1224	0.1454			0.1140	0.1407
34	0.1006	0.1241	0.1757	0.0696	0.1178	0.1388			0.1079	0.1470			0.1246	0.1457

Table S6.13. Cell Counts ($n(\theta_{\text{cond}})$)

"n"

	NB / 1 Child			NB / 2+ Child			BC / 1 Child				BC / 2+ Child			
	Recipient			Recipient			Applicants		Recipient		Applicants		Recipient	
t	SSP+	SSP	Ctrl	SSP+	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl	SSP	Ctrl
-11							784	774			686	728		
-10							748	732			659	700		
-9							748	732			659	700		
-8							748	732			659	700		
-7							748	732			659	700		
-6							748	732			659	700		
-5							748	732			659	700		
-4							748	732			659	700		
-3							748	732			659	700		
-2							747	731			658	696		
-1							735	716			647	681		
0	172	757	778	116	551	540	697	675	740	732	608	640	752	713
1	170	741	747	115	531	525	682	661	697	693	598	632	720	680
2	172	747	749	116	534	527	681	658	702	708	593	630	728	683
3	172	748	748	116	535	525	681	658	702	704	593	630	726	685
4	172	750	748	116	535	524	681	658	701	703	593	630	725	682
5	172	748	747	116	535	524	681	658	699	703	593	630	726	680
6	172	747	745	116	537	524	681	658	700	702	593	630	728	683
7	173	747	743	117	536	525	681	658	698	702	593	630	726	684
8	173	747	743	117	534	526	681	658	698	702	593	630	725	684
9	172	746	739	116	534	525	681	658	700	701	593	630	722	684
10	172	746	740	116	533	524	681	658	701	700	593	630	719	682
11	172	745	740	115	531	522	681	658	700	697	593	630	715	678
12	171	742	739	116	532	523	681	658	701	695	593	630	716	677
13	171	740	739	117	533	524	681	658	699	695	593	630	713	676
14	172	740	741	117	533	526	681	658	699	695	593	630	712	676
15	172	741	741	117	534	525	679	658	697	691	593	627	712	676
16	173	741	742	117	536	523	674	646	695	689	577	615	709	675
17	173	736	742	117	534	516	577	547	688	682	486	497	706	669
18	157	704	716	115	510	497	114	126	651	649	92	112	677	632
19	155	693	705	115	499	492			630	627			659	609
20	155	694	702	115	493	489			611	607			648	593
21	156	694	703	115	492	489			607	600			646	591
22	155	697	701	115	493	491			606	593			643	588
23	155	698	697	115	493	491			604	590			639	588
24	155	698	697	115	493	489			603	591			635	589
25	154	698	699	115	490	489			598	595			635	588
26	154	697	699	115	490	487			596	595			636	589
27	152	697	699	114	491	486			597	593			636	588
28	150	691	696	114	491	481			593	591			635	588
29	151	690	699	114	490	484			591	592			635	587
30	151	687	698	114	491	481			592	593			630	581
31	152	683	699	113	491	479			591	592			629	581
32	152	684	698	113	493	479			588	592			628	578
33	152	686	698	111	495	480			587	594			628	581
34	152	689	697	111	495	481			587	593			628	580