When Can Experimental Evidence Mislead?
A Re-Assessment of Canada's Self Sufficiency Project*

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Introduction

There is an ongoing debate about whether labour market and social programs can reliably be evaluated with non-experimental methods (Lalonde, 1986; Burtless, 1995; Heckman and Smith, 1995; Heckman, Lalonde and Smith, 1999). The key advantage of random assignment is that the treatment and control groups are statistically equivalent (not statistically significantly different) in terms of observed and unobserved characteristics. Thus the behavior of the control group provides an unbiased estimate of the counterfactual behavior of the treatment group – the behavior that would have been observed in the absence of treatment. Any difference in outcomes between treatments and controls can thus be attributed to the causal effects of the intervention. Indeed, the credibility of evidence from randomized trials is often cited as one of the major advantages of social experiments (Burtless, 1995).

However, social experiments have limitations, some of which may affect the internal validity of the experimental evidence (Heckman and Smith, 1995; Heckman, Lalonde and Smith, 1999). Non-random attrition can result in treatment and control groups that differ, even though the two groups had very similar characteristics at the baseline. Those assigned to the control group may obtain services similar to those provided to the treatment group, resulting in "substitution bias" that generally results in under-estimating the impact of the intervention (Heckman, Hohmann, Khoo and Smith, 2002).

In this project we examine another potential problem with social experiments – a form of contamination that we refer to as "counterfactual bias." Counterfactual bias occurs when the policy or economic environment changes after treatment has begun, and does so in a way that has a different effect on the treatment group than on the control group. In these circumstances the behavior of the control group may no longer provide an appropriate counterfactual for the altered treatment group. This section discusses conditions under which counterfactual bias can occur.

Under random assignment the treatment and control groups have very similar characteristics (observed and unobserved) at the point of randomization. However, once treatment is underway the characteristics of the two groups may – indeed, generally will -- diverge. A change in the economic, social or policy environment -- which for simplicity

we will refer to as a "policy change" -- that takes place after treatment has begun may thus exert a different effect on the treatment group than on the control group. The behavior of the control group will continue to provide an estimate of the way in which the behavior of the <u>original</u> treatment group would have been affected by the policy change. However, given that the characteristics of the treatment group have been altered as a result of the treatment, the control group's behavior may no longer provide a suitable counterfactual for the behaviour of the <u>altered</u> treatment group. In such circumstances standard experimental estimates based on simple treatment – control differences will be biased estimates of the true effect of the intervention. That is, standard experimental estimates may confound two effects: (i) the impacts of the intervention and (ii) the differential impacts of the policy change on the treatment and control groups.

It is important to note that not all changes in the policy or economic environment result in bias. Indeed, the principal purpose of the control group is to provide an unbiased estimate of the counterfactual – the way the treatment group would have responded to policy or economic changes in the absence of the treatment. However, after the treatment has begun the characteristics of the treatment and control groups may change, so that policy changes that take place after random assignment could have a differential effect on the two groups. In these circumstances the response of the control group to the policy change does not necessarily provide an appropriate estimate of the counterfactual.

We can illustrate the potential for counterfactual bias in the context of welfare-to-work initiatives evaluated with an experimental design. To do so we combine all the factors (those observed by the researcher and those not observed) that influence the probability of leaving welfare into a single index, which we call "propensity to leave welfare" or "job readiness." Those with higher values of job readiness are more likely to exit welfare. At the baseline t_0, when random assignment takes place, all members of the experimental sample are receiving welfare. Half of the experimental sample is randomly assigned to the treatment group, which receives an intervention that provides an incentive to leave welfare and enter the workforce. The remainder is randomly assigned to the control group. Treatment status is independent of the observed and unobserved characteristics of both groups at time t_0. Thus the distributions of job readiness in the treatment and control groups are identical (see the top panel in Figure 1).

For the purposes of exposition we will assume that the treatment is effective, so that the welfare exit rate of the treatment group exceeds that of the control group in the post-baseline period. Subsequently, at time $t^* > t_0$, the existing welfare policy is changed in a way that encourages recipients to exit welfare and enter the workforce. This change can be thought of as a second treatment, the first being the incentive offered to those assigned to the treatment group and the second being the new policy that applies to members of both the treatment and control groups who remain on welfare at time t^*. However, while the initial incentive treatment was independent of the characteristics of the treatment and control groups, the second policy change "treatment" is not independent of the characteristics of the two groups. In particular, at time t^*, compared to the control group a smaller proportion of the treatment group remains on welfare and those in the treatment group who still receive welfare have a lower average propensity to exit. (This is illustrated in the bottom panel in Figure 1, where the area to the left of the vertical line indicates the fraction of each group that remains on welfare at t^*.) At time t^* the control group has more job ready welfare recipients than does the treatment group. Thus the policy change introduced at t^* may have a larger impact on the exit rate of the control group than on that of the treatment group. In these circumstances the behavior of the control group after t^* no longer provides an appropriate counterfactual.

The policy change at time t^* may also influence the probability of re-entry on to welfare. This effect on the re-entry rate could also differ between the experimental treatment and control groups because of the differences in the observed and unobserved characteristics of the two groups at time t^*.

In summary, randomization ensures that the treatment and control groups are statistically equivalent at the baseline. However, once treatment begins the characteristics of the two experimental groups may diverge. Subsequent changes to the policy or economic environment may exert different impacts on the two groups. In such circumstances the behavior of the control group may no longer provide an appropriate counterfactual for the behavior of the altered treatment group. In the next section we present evidence that strongly suggests that this type of contamination occurred in the SSP demonstration.`

Welfare Reform and the Self-Sufficiency Project

A frequent criticism of income assistance (welfare) programs is that they provide little incentive for recipients to seek employment. Under many such programs, recipients who enter the workforce are required to forego benefit payments by the amount of their labour market earnings -- implying that earnings are taxed at a rate of 100 percent. The implicit tax rate may even exceed 100% if, for example, those leaving income assistance are no longer eligible for medical benefits or subsidized housing.

Several reforms have been proposed to deal with this incentive problem. One strategy is to raise the market wage of income assistance (IA) recipients through training and employment programs, thus making work more attractive relative to welfare. Another approach improves work incentives by reducing the implicit tax rate on market earnings. Examples of this approach include the negative income tax, earnings disregards, and income supplementation policies such as the Working Income Tax Benefit in Canada and the Employment Income Tax Credit in the U.S.[1] A third strategy attempts to alter the preferences of recipients, either by raising the stigma associated with welfare receipt or enhancing the perceived value of work.

Some policies combine elements of two or more of these approaches. An interesting example is a temporary earnings supplement for welfare recipients who enter the workforce. During the period the supplement is in place, this policy has the work incentive features of many income supplementation schemes. Labour market earnings are implicitly taxed at a rate less than 100% and program participants receive income (market earnings plus the supplement) that exceeds welfare benefits. By encouraging recipients to leave welfare and enter the workforce for at least the period of the supplement, former welfare recipients may gain work experience and enhance their skills, thus raising their market earnings. The experience of working for an extended period of time may also alter individual's preferences between welfare and work. As a result of enhanced earnings capacity and/or altered preferences toward work, a temporary financial incentive may have lasting effects on income assistance receipt and labour force participation.

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[1] Earnings disregards refer to market earnings that IA recipients are allowed to receive without a reduction in their IA benefits.

The Self-Sufficiency Project (SSP), carried out in the provinces of British Columbia (B.C.) and New Brunswick (N.B.) during the 1990s, was a demonstration project designed to provide a rigorous test of a temporary earnings supplement. The impacts of the policy were evaluated using a random assignment design. Members of the program group were offered a generous earnings supplement if they left welfare to take full-time employment. Those taking up the supplement offer could receive the earnings supplement for up to three years provided they continued to meet the eligibility criteria. Program group members could also return to welfare at any time. Those randomly assigned to the control group received nothing from the SSP -- they could remain on income assistance or enter the workforce.

A key objective of the SSP Demonstration was to determine whether financial incentives lead to reductions in welfare use among long-term welfare recipients, and whether the magnitudes of program impacts on welfare use and earnings are sufficient to support this approach to welfare reform. The demonstration focused on single parents with children, the group with the lowest exit rate from income assistance.[2] "Long-term" was defined as having been on welfare for at least 12 of the previous 13 months.

Another key objective of the SSP was to test whether a temporary financial incentive could have lasting effects on welfare receipt and work activity. Accordingly, treatment group members who left welfare and took up full-time employment could receive a substantial earnings supplement for up to three years providing they maintained full-time work.

The SSP findings indicate that financial incentives can be effective even for long-term welfare recipients. About one-third of the treatment group qualified for the earnings supplement, and left IA to take up full-time employment. The exit rate of the treatment group substantially exceeded that of the control group, especially during the first 18 months of the SSP demonstration. However, taken at face value the SSP results relating to any "permanent" effects of a temporary earnings supplement are clearly negative. Although a large treatment – control gap in income assistance receipt and employment was evident during most of the experimental period, the offer of a generous but

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[2] In Canada welfare is not restricted to single parents with children. Anyone (including singles and couples without children) with demonstrated need can qualify for income assistance.

temporary financial incentive appears to have had no lasting impact on welfare dependence or labour force attachment. For example, 54 months after random assignment – by which time the three-year supplement period had ended for all eligible participants -- the mean employment rates of the treatment and control groups were almost identical, and were not statistically significantly different (Michalopoulos et al 2002). Similarly, the two groups' participation rates in income assistance had converged by month 69 after random assignment (Card and Hyslop, 2005). Figure 2 shows the pattern of income assistance rates over the first 54 months of the SSP demonstration. On the surface, therefore, the SSP findings suggest that temporary financial incentives have little to offer in the on-going attempt to re-design income support programs to "make work pay."

The behavior of the SSP control group during the demonstration was also revealing. As shown in Figure 2, despite the experiment's focus on single parents with children who had been on welfare for an extended period of time, these long-term IA recipients displayed a gradual but substantial exit from welfare over time.

The purpose of this research project is to re-assess the key experimental findings of the SSP demonstration. We argue that such a re-assessment is needed because the experiment was subject to potentially important biases that pose a threat to the internal validity of the SSP evidence. The nature of this threat to internal validity and its relevance to the SSP evidence is discussed in the next section.

Policy change during the SSP demonstration

During the 1990s the Government of Canada funded an innovative demonstration project, the SSP, designed to provide evidence on the effects of a financial incentive on long-term welfare recipients.[3] The SSP demonstration was carried out in British Columbia and New Brunswick, and focused on single parents with dependent children who had been on income assistance (IA) for at least 12 of the previous 13 months.[4] Among those who agreed to participate, one-half were randomly assigned to the treatment group that was eligible for the earnings supplement; the rest were assigned to the control group. Random assignment took place between February 1992 and November

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[3] For details of the SSP demonstration see Michalopoulos et. al (2002).
[4] About three-quarters of the SSP sample had been receiving IA for more than 2 years, and more than 40% had received IA for more than the previous 3 years.

1995. Those in the treatment group were offered a financial incentive to leave welfare and take up full-time employment.[5] The financial incentive was generous, approximately doubling income from work for the typical participant and providing total income substantially higher than welfare benefits.

The SSP demonstration incorporated two important time limits. Members of the treatment group were given up to 12 months following random assignment to obtain full-time employment. Once they had qualified, participants could continue to receive the supplement for three years providing they maintained full-time employment. Those in the control group could remain on welfare or enter the workforce. Card and Hyslop (2005) show that the two SSP time limits generated an "establishment" incentive to find a full-time job and exit welfare within 12 months after random assignment, and an "entitlement" incentive to choose work over welfare once eligibility was established.

The experimental findings are summarized in the SSP Final Report (Michalopoulos et. al., 2002). More than one-third of the treatment group obtained full-time employment and qualified for the earnings supplement. During the eligibility period, the treatment group experienced gains in earnings and employment and reduced welfare use relative to the control group. The largest impacts were observed during the first 12-15 months following random assignment. After this time the differences in outcomes between the treatment and control groups gradually narrowed. By the end of the 3-year period of supplement eligibility treatment-control differences in employment, earnings and welfare receipt were small. In particular, by the 54 month point there was no difference in full-time employment rates, part-time employment rates and average earnings between the two experimental groups (Michalopoulos et. al., 2002, chapter 3). Similarly, treatment-control differences in income assistance receipt had faded to zero by month 69 (Card and Hyslop, 2005).

Another striking finding was that the long-term welfare recipients in the SSP control group left welfare steadily, and at a moderately substantial rate, throughout the period of the demonstration. This finding was particularly noteworthy because the demonstration project was targeted on single parents with dependent children with a

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[5] Full-time employment was defined as at least 30 hours per week and could be achieved by combining two or more part-time jobs.

substantial history of welfare dependence – a group with relatively low exit rates from income assistance.

Within the treatment group, the characteristics of the single parents who obtained full-time employment and established eligibility for the earnings supplement differed from those of the recipients that did not establish eligibility. Specifically, the "eligible subset" had characteristics associated with higher wages and being more "job ready." Compared to the treatment group members who did not establish eligibility, the eligible subset were more highly educated, had more previous work experience, were more likely to be working at the baseline and were less likely to have been on income assistance continuously for the past three years (Card and Hyslop, 2005). Thus within the treatment group the most "job ready" left welfare for full-time employment and established eligibility for the earnings supplement and the least "job ready" remained on income assistance.

However, a major policy change was made in BC that may have changed welfare use for individuals in that province. In the mid-1990s the B.C. government made sweeping changes to the provincial income assistance program.[6] These changes strongly encouraged welfare recipients to leave income assistance and enter the workforce. In particular, "employable" recipients were required to actively seek work, and to remain eligible for IA could only refuse a job under a strict set of conditions. The key changes affecting single parents with dependent children were introduced in December 1995 (when the definition of "employable" was broadened considerably) and January 1996 (when the exemption from the requirement to actively seek work was changed from exempting single parents with a dependent child under 12 years of age to those with a dependent child under 7 years of age). Other substantive changes in 1996 raised the incentives to work (or to demonstrate that one is actively looking for work) by providing financial incentives including sizeable day care subsidies and dental, vision and other extended health benefits to an individual's dependents ('Healthy Kids Act'). Particularly

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[6] Although these changes culminated in the passage of the B.C. Benefits Act in October 1996, many important policy changes were introduced prior to the passage of the new Act through revisions to the regulations under the previous legislation governing the income assistance program (i.e., in December 1995 and January 1996).

strong rules were imposed on individuals under age 25.[7] Further changes meant to facilitate the transition to work including a wave of training, education-related and other active labour market reforms (including for example earnings exemptions) followed in March 1997. Raw welfare caseloads declined substantially in 1996-98 (Ministry of Human Resources[8]). While the various SRDC reports on the SSP do note the policy changes made in BC and NB, to our knowledge no analysis of the impacts of these policy changes on the experimental findings has been carried out.[9]

Empirical analysis and results

In Figures 2 and 3 we present evidence that these policy changes accelerated the exit from welfare of members of the SSP, and that they appear to have had a different effect on the control group than on the treatment group. To do so, we show income assistance rates of SSP entering cohorts by calendar month. Experimental analysis of impacts on outcomes such as income assistance receipt, employment and earnings typically examines behaviour by month from random assignment rather than by calendar month (see, for example, Michalopoulos et. al. 2002). Doing so is the appropriate approach for many purposes, but it obscures the possible effects of policy changes because it pools together cohorts that entered SSP at different points in time. We also present these Figures in the Appendix. As is now well know, IA rates in BC converged while there has always been a small., seemingly permanent effect in NB. Pooled together the SSP appears to have had virtually no statistically discernable effect on welfare rates.

Figure 2 shows B.C. cohorts that were randomly assigned between January and June 1993. The one-year eligibility period for this group had expired by June 1994. Note the large treatment-control gap in IA receipt that opens up during the period June 1993 to June 1994. After the initial eligibility period IA receipt continues to decline for both groups, and although the gap narrows somewhat it nonetheless remains substantial. IA receipt by the control group declines fairly steadily over the period from May/June 1993 to December 1995. However, beginning in January 1996, at the time the new BC policies

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[7]At the time of the relevant regulation this group only constitutes about 10% of the BC sample and thus we ignore these individuals.

[8] This ministry has changed substantially twice since. Also see the Canada Welfare to Work Project, University of Western Ontario (insert website)

[9] See, for example, the discussion in the Final Report (Michalopoulos et. al. 2002, pp. 6-9).

came into effect for single parents with dependent children, the control group's exit rate accelerates. By early 1997 the IA rates of the treatment and control groups are almost equal.

Figure 3 shows the IA rates of BC cohorts randomly assigned from January to December 1994. This group had just completed their initial eligibility period when the new BC Benefits regime was introduced. Again, the exit rate from IA among control group members accelerates with the introduction of the policy changes. Figures 2 and 3 are very similar in two respects: (i) treatment – control differences during the 12-month eligibility period and in the gap from the last cohort entering the figure to the last month of eligibility for the SSP program group and (ii) the sharp decline in IA rates following January 1996. Perhaps of most interest is the difference between the 1993 and 1994 cohorts in the period between the last of month of eligibility and January 1996. This ultimately is a key source of variation for our analysis. For the 1993 cohort there is a year and a half of IA information in this window period that does not exist for the 1994 cohort since the latter has both a more extended period of intake months and is immediately impacted by the new legislation.

The visual evidence suggests that the 1996 policy change had a larger impact on the control group than on the treatment group. Such a differential effect is consistent with the control group having a larger proportion of relatively "job ready" members than the treatment group at the time the new policies were introduced. Because of this differential effect the experimental evidence is likely to under-estimate the true impact of the SSP financial incentive on income assistance receipt. Figures 2 and 3 also suggest that the behavior of the BC control group is likely to over-state the rate at which long term welfare recipients leave IA and enter the workforce on their own (i.e. without financial or other incentives to do so). In our empirical work below we use New Brunswick as a falsification test since the time period (January 1996) relevant for British Columbia should have had no effect on individuals in New Brunswick.

We now turn to a simple empirical framework for analyzing income assistance patterns in BC that will allow us to simulate what would have happened if the BC Benefits Legislation had not been passed. Specifically, we estimate the following regression:

$$IA_{i,t} = f(\alpha + \delta*LAW + \beta_1 t + \beta_2 t^2 + \beta_3 t^3 + \beta_3 SSPTIME + \beta_3 MONTH + \beta_3 YEAR + \varepsilon)$$

$$(1)$$

where IA =1 if individual i was receiving welfare in month (since onset of program) t. Note that we estimate equation (1) separately by experimental group since the transitions over time are fundamentally different due to the incentives introduced by the wage subsidy. SSPTIME are 69 SSP time since onset dummies, MONTH and YEAR are calendar monthly and yearly dummies to capture seasonality. Our identification strategy of course rests on the notion that baseline interview dates are exogenous to welfare – that is, that individuals with early exposure to the new law can be used as a counterfactual for those with later exposure to the new law. This underlying stationarity assumption could be violated if there were seasonality effects; for instance, if labour market conditions changed. Finally, we estimate (1) for New Brunswick as a falsification test; LAW should not be statistically significant.

Equation (1) includes a third order polynomial time trend, SSP time dummies and then the key policy variable LAW, which equals one when a given baseline interview date becomes covered by the legislation; i.e., the month in SSP time (1 to 70) when calendar time equals January 1996. In addition to the 'natural' time trend in IA being different across experimental groups (due to the incentive of the SSP offer), the law is expected to effect the two groups differently as discussed above. The effect, if any, should be larger for the control group since relatively more of the program group left IA prior to the BC policy changes in response to the SSP offer. Note that there could be an effect for the program group since those who had not left IA became subject to the new welfare regime as was the case for individuals in the control group who had not yet left IA. If the BC Benefits Act (i.e., January 1996 on) had no differential effect across baseline cohorts then δ should be zero.

Before proceeding to the results, it is useful to emphasize what variation is used to identify the policy variable. The LAW variable only differs across baseline interview cohorts when the BC Benefits Act affects those individuals included in a given cohort. This is the same type of variation we see when comparing Figures 2 and 3; the IA path for the control group decreases much more quickly following January 1996 for all cohorts, but there is a substantial difference across cohorts in time since random

assignment when the January 1996 changes affect them. Similarly, we could imagine conducting a series of 'natural experiments' where we compare, for example, the January 1993 cohort with the January 1994 cohort. In the latter case, the earlier cohort will have an extra year in their IA path before becoming covered by the new legislation. Any difference in relative IA use in that year is what identifies δ. This is the only variation exploited in the regression equation above. We could also use New Brunswick as a comparison group, which would always equal zero for the interaction term. When we do this, our estimated policy effects are somewhat larger, but for now our preferred specification is (1) since this only relies on one source of variation: the timing of when a baseline cohort becomes covered under the legislation.

Table 1 presents the regression results and Figure 4 presents the predicted IA rates for time since onset to compare to the standard SSP figure in the Appendix. The BC Benefits law reduced IA rates for both the Program Group and the Control Group, but by a much larger amount for the Control Group. The policy variable had no effect in New Brunswick with positive, statistically insignificant coefficients.

Figure 4 asks the following counterfactual: what would have IA rates over time looked like if the BC Benefits Act had not been passed? We simulate this by predicting IA rates if the LAW variable was always zero. Not surprisingly, given the coefficients on the policy interaction from Table 2, we see sizeable effects. We note that our model predicts IA rates to be identical to actual IA rates. By the 70 month point post random assignment, IA rates in BC are estimated to be 10 percentage points higher than the actual rates (50% vs. 40%) for the control group if BC Benefits had not been passed, and 5 percentage points higher for the program group (45% vs. 40%) leaving us with a 5 percentage points gap between the two groups at month 70. Recall that actual monthly welfare caseloads in B.C. declined by about 60% for the welfare-to-work group (those over 25 without an exemption). In addition to suggesting that BC Benefits led to IA rates in the SSP that have been misinterpreted to be too low, the differential effects of the law across experimental groups are such that there may have been a permanent effect of the SSP in B.C. as well; in the neighborhood of 5 percentage points. We emphasize this is not 4.5 years post-baseline as in the previous literature but almost 6 years post-baseline. Moreover, our model simulations suggest that without BC Benefits, the experimental

effect would have been quite constant around 5 percentage points over the final *3 years*, suggestive of a steady, permanent effect. Further, this is consistent with the 1994 cohorts for NB – where the bulk of the observations are for that province – where over the last 2 to 3 years there was a fairly steady monthly percentage point difference of 5% to 9% between treatments and controls.[10]

Conclusions

Three major conclusions have been reached on the basis of the experimental evidence from the SSP demonstration: (i) long-term IA recipients do respond to financial incentives to leave welfare and enter the workforce, (ii) over time a substantial fraction of IA recipients who are single parents with dependent children leave welfare on their own, without financial or other incentives to do so, and (iii) temporary financial incentives to leave IA have temporary but no lasting impacts on welfare receipt and labour force participation.

This study makes two principal contributions:
(i) Our investigation contributes to the ongoing debate over the advantages and disadvantages of experimental versus non-experimental methods for analyzing social policies. By analyzing the consequences of a substantial policy change that was introduced during the period that a major demonstration project was underway, we provide new evidence on the extent to which the validity of the findings of social experiments may be threatened by such policy changes.
(ii) Our study sheds new light on the conclusions that have been reached to date on the basis of the SSP evidence. On the basis of our preliminary investigations, we believe that the SSP experimental estimates are unduly pessimistic about the long-term consequences of temporary financial incentives on welfare dependence. It also appears that the observed behavior of the experimental control group in B.C. over-estimates the extent to which long-term welfare recipients with dependent children leave IA in the absence of incentives to do so.

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[10] This does not change if the three 1995 cohorts are included in the NB figure.

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Table 1: Estimated coefficients for the probability of being on welfare

Variable	British Columbia		New Brunswick Falsification Test	
	Control Group	**Program Group**	**Control Group**	**Program Group**
BC Benefits Act (January 1996)	-.048*** (.006)	-.017* (.009)	.017 (.016)	.011 (.015)
Time	-.012*** (.001)	-.025*** (.001)	-.035*** (.001)	-.021*** (.001)
Time^2 (*10)	.002*** (.000)	.006*** (.000)	.009*** (.000)	.004*** (.000)
Time^3 (*100)	-.002*** (.000)	-.004*** (.000)	-.007*** (.000)	-.006*** (.000)
69 SSP Month dummies	Yes			
11 Calendar month dummies	Yes			
6 Calendar year dummies	Yes			
R-squared	.18	.15	.13	.10
F-stat	552.1	404.4	324.0	236.9
N	87080	90580	80360	81620

NOTES: Standard errors are in parentheses and adjust for clustering on the baseline interview date.

Figure 1

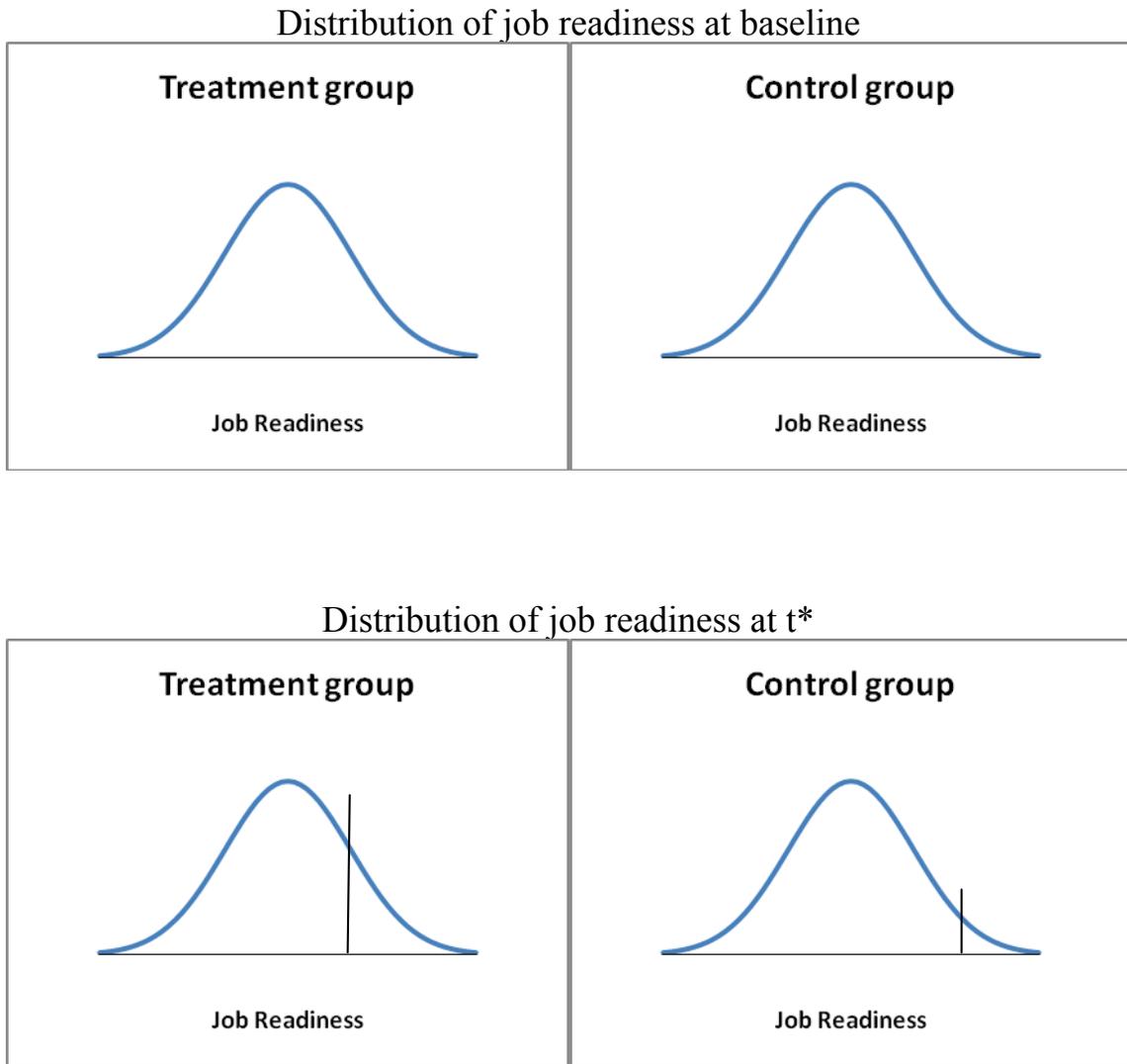Distribution of job readiness at baseline

Distribution of job readiness at t*
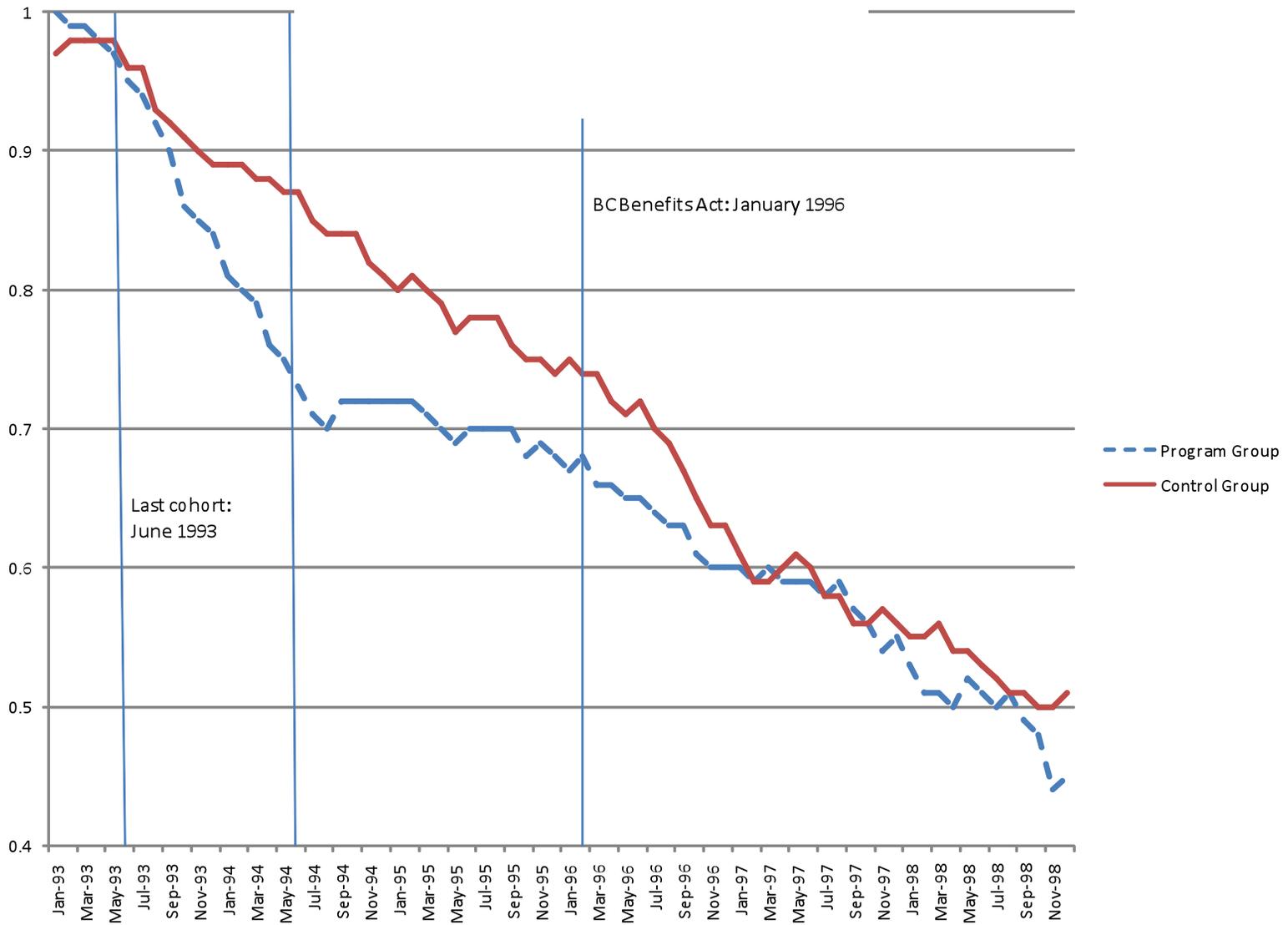
Figure 2: IA Rates, BC, 1993 Cohorts

Figure 3: IA Rates, BC, 1994 Cohorts

Last month of eligibility for last cohort: December 1995

Last cohort:
December 1994

- - - - Program Group
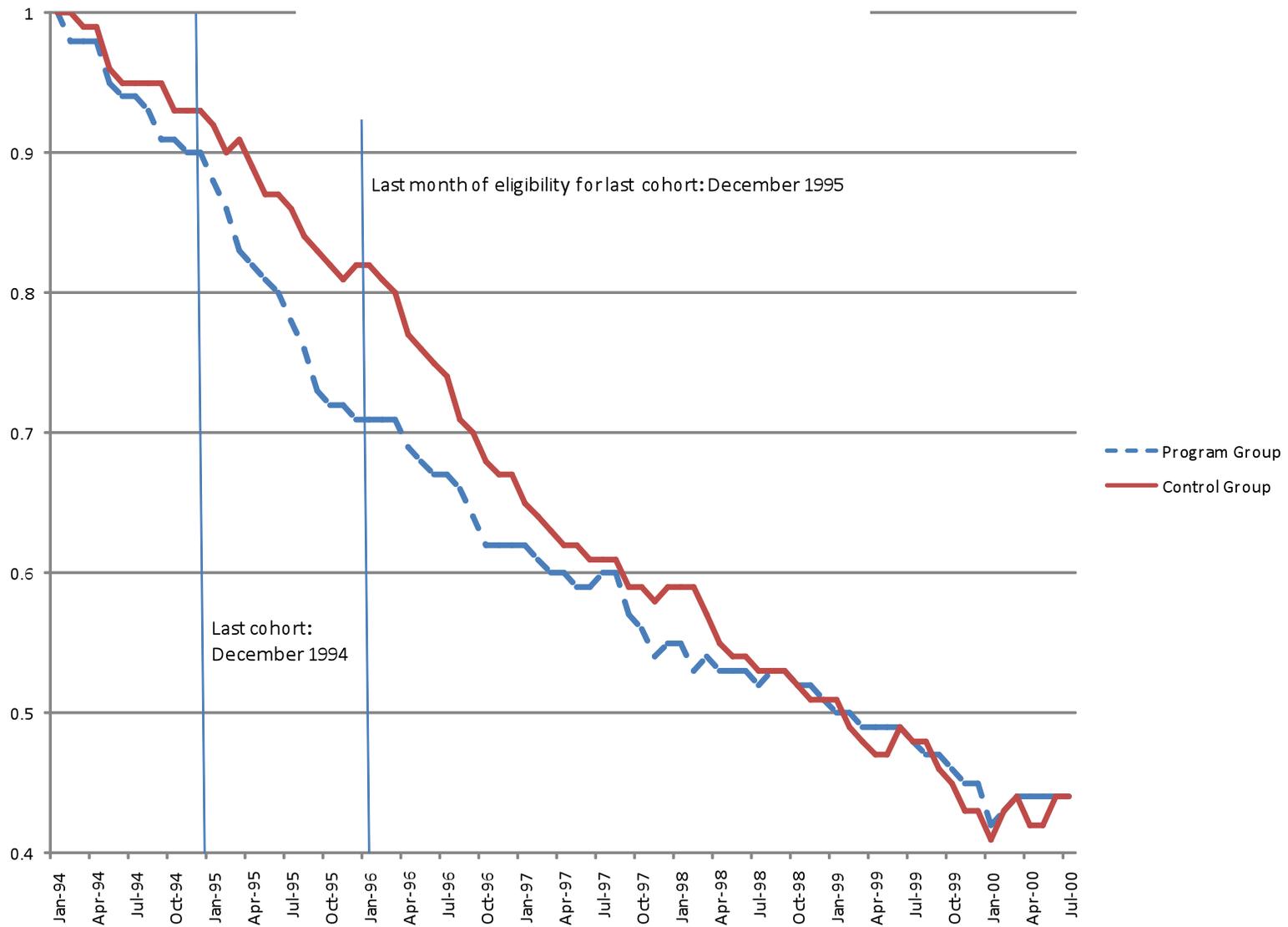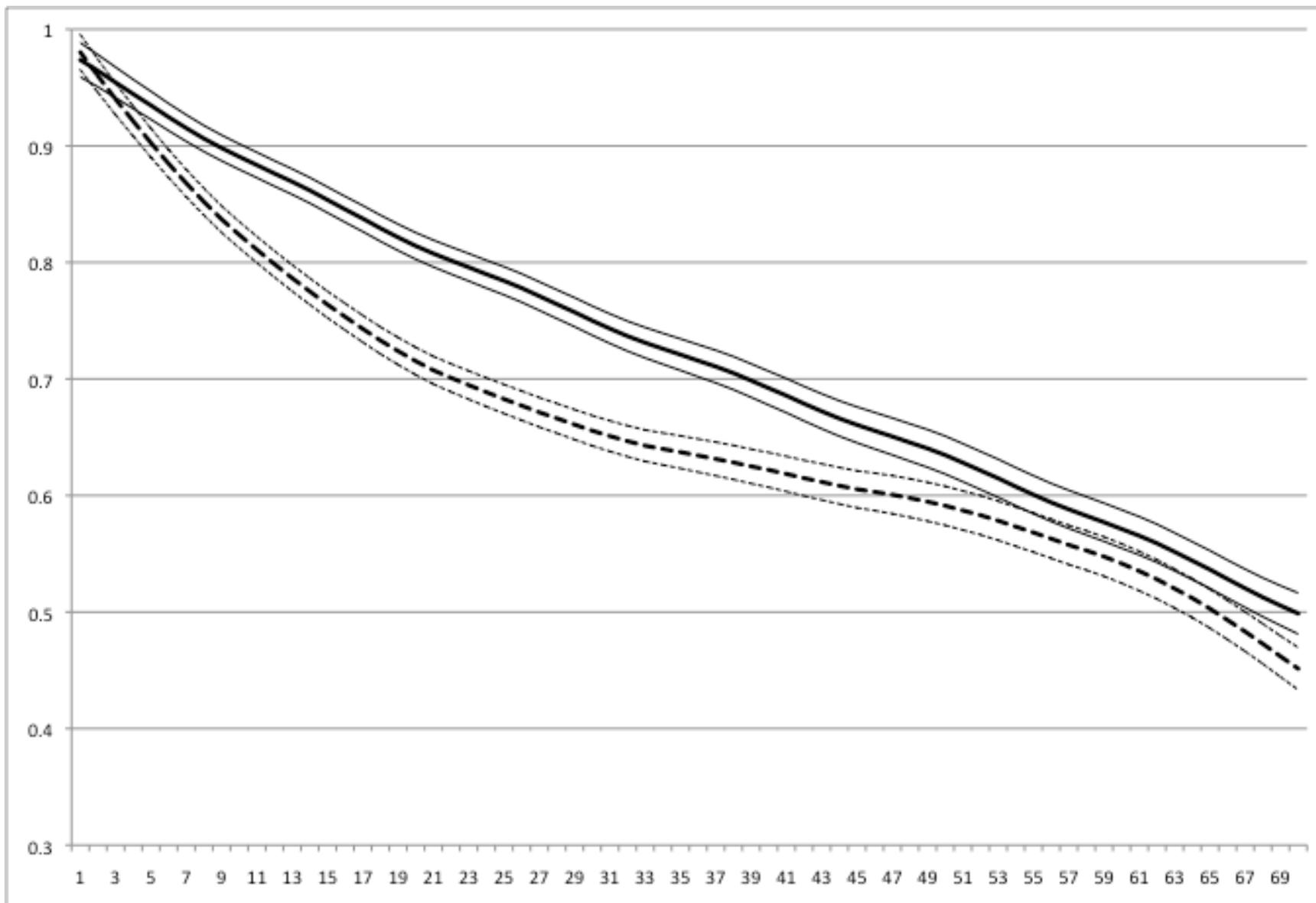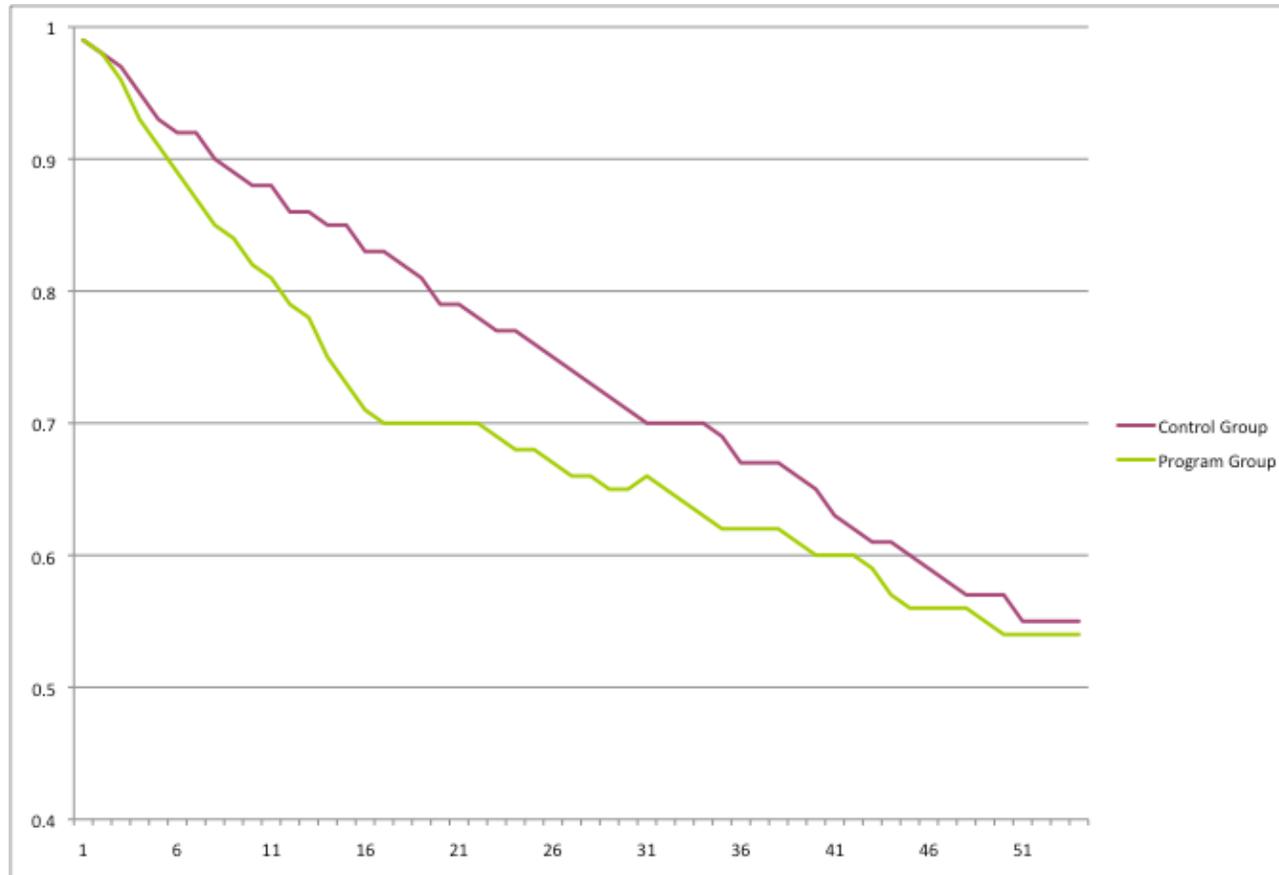
———— Control Group

Figure 4: Simulated IA Rates, BC

Appendix
Standard SSP Figures of IA Rates over SSP 'Time Since Onset'

(a) B.C

(b) NB