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Early indication of program performance: The case of a Swedish temporary employment program

Laura Larsson
Oskar Nordström Skans

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Postal address: P.O. Box 513, 751 20 Uppsala

Visiting address: Kyrkogårdsgatan 6, Uppsala

Phone: +46 18 471 70 70

Fax: +46 18 471 70 71

ifau@ifau.uu.se

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Early indication of program performance: The case of a Swedish temporary employment program^{*}

by

Laura Larsson and Oskar Nordström Skans[♦]

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Abstract

This paper proposes an application of standard evaluation techniques to obtain an early indication of the efficiency of a social program. An explicit distinction is made between participation and treatment since non-participants in many cases can find treatment outside of the program. By estimating the amount of treatment participants would have received if there had been no program it is possible to derive the net treatment provided by the program as soon as the first participants have entered the program. Using propensity score matching to study substitutes in the recently implemented Swedish “Career Break” pilot program we obtain three main results: First, the selection into the program favours participants that would have received treatment even without the program. Second, the net treatment provided by the program is on average only about half of the expected program length. Third, a simulation shows how the net treatment could be increased by a simple change in the eligibility criteria.

Keywords: Evaluation, Propensity score matching, Social programmes, Treatment intensity

JEL: C14, J38, J68

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[♦] Institute for Labour Market Policy Evaluation (IFAU). E-mail: laura.larsson@ifau.uu.se and oskar.nordstrom_skans@ifau.uu.se.

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1 Introduction

Policy makers are showing growing interest in evaluations of social programmes. One increasingly popular method of facilitating evaluations is to conduct experiments or pilot programmes. There is, however, a common conflict-of-interest between the researchers' need for long follow-up periods and the policy-makers' need for swift answers. This paper proposes and applies a simple method to provide information on the expected efficiency of a program shortly after the start of the program.

Many social programmes provide a treatment that can be obtained without participation in the program. For example, participants in a subsidized employment program can find employment even outside the program. Thus, it is important to distinguish between participation and treatment when evaluating a program. The actual *net intensity* of treatment, defined as the extra treatment provided by the program depends on the counterfactual time-use of the program participants during the program period. The more treatment the participants would have received had they not participated, the weaker the treatment intensity of the program.

In this paper we suggest that estimation of the net intensity of treatment provides a good indicator of program performance when data on post-program outcomes are unavailable. The method should be applicable in all cases where social programmes provide a treatment that the participants can acquire elsewhere; e.g. programmes that involve subsidies of employment, day care, education, or health insurance. We further suggest that propensity score matching is a suitable tool for estimating the counterfactual time-use during participation whenever the data is rich enough to fulfil the conditional independence assumption. Matching also allows for simulations of how changes in the eligibility criteria can increase the net intensity of treatment. We illustrate the method in an empirical application by estimating the net treatment intensity on the substitutes in the Swedish career-break pilot program and by providing some simulation results showing how changes in the eligibility criteria may increase the net treatment intensity of the program.

2 The evaluation problem

2.1 The net intensity of treatment

The evaluation literature typically uses a terminology of *potential* outcomes to describe the outcomes of an individual participating in a program (Y^1) and the outcome of the *same* individual if not participating (Y^0). Denoting participation by $D = 1$ and non-participation by $D = 0$ we can write the *actual* outcome for an arbitrary individual i as:

$$(1) \quad Y_i = D_i Y_i^1 + (1 - D_i) Y_i^0.$$

Assume that we ultimately are interested in how the program affects the participants. This parameter is defined as:

$$(2) \quad \Delta = E[Y_i^1 - Y_i^0 | D = 1]$$

While Δ is the ultimate parameter of interest, estimation of that parameter requires a long follow-up period. However, we note that in many cases treatment is not limited to program participants only. The efficiency of the program is of course smaller if some of the participants would have gotten the treatment anyway (*ceteris paribus*).

We thus propose that estimating the effect of program participation on the *net treatment intensity* will provide useful information about the program efficiency early on in the implementation process. The stronger the treatment intensity, the more can we expect of the effect of the program.¹ We assume that participation in a program (*i.e.* $D = 1$) affects the outcome (Y) only through the provision of a treatment denoted by T , and that the treatment is the same within and outside the program. This gives us a two-step model in which program participation affects the (continuous) treatment (assumed to be bounded by 0 and 1) which, in turn, affects the outcome. We denote treatment for participants and non-participants by T^1 and T^0 and assume that program participants receive

¹ Making the reasonable assumption that the program is instituted with a prior of positive treatment effects for the participants. In the model below we further assume that treatment affects the outcome linearly. This assumption is, however, simply for convenience.

full treatment while non-participants get some a priori unknown level of treatment:

$$(3) \quad \begin{aligned} T_i^1 &\equiv T_i \Big|_{D_i=1} = 1 \\ T_i^0 &\equiv T_i \Big|_{D_i=0} \in [0,1] \end{aligned}$$

The outcome of participants and non-participants depend linearly on treatment as a weighted average of two underlying outcome variables Y^T and Y^{NT} . Thus, using equation (3), we may write the outcome of participants and non-participants as:

$$(4) \quad \begin{aligned} Y_i^1 &\equiv Y_i \Big|_{D_i=1} = Y_i^T \\ Y_i^0 &\equiv Y_i \Big|_{D_i=0} = T_i^0 Y_i^T + (1 - T_i^0) Y_i^{NT} \end{aligned}$$

Using (1) and (4) we see that actual outcome is related to participation and treatment as:

$$(5) \quad Y_i = D_i Y_i^T + (1 - D_i) [T_i^0 Y_i^T + (1 - T_i^0) Y_i^{NT}]$$

Rewriting our ultimate parameter of interest (equation 2) as a function of treatment we get:

$$(6) \quad \Delta = E[(T_i^1 - T_i^0)(Y_i^T - Y_i^{NT}) | D = 1] = E[(1 - T_i^0)(Y_i^T - Y_i^{NT}) | D = 1]$$

However, since we do not have data on outcomes, the parameter to be estimated in this paper is the *net treatment intensity* for the participants defined as:

$$(7) \quad \Lambda \equiv E(T_i^1 - T_i^0 | D = 1) = E(1 - T_i^0 | D = 1)$$

In words, equation (6) states that the effect of participation for each participant is the net treatment intensity of the program times the effect on the outcome of

each unit of treatment. Equation (7) denotes the *expected value* of the net treatment intensity that is to be estimated.² Assuming that the effect of actual treatment is positive, the average program effect (*i.e.* Δ) is increasing in $(1-T_i^0)$ and in the limiting (trivial) case, where the participants would have received full treatment even in absence of the program, the program effect will always be zero. Furthermore, in empirical applications the net treatment intensity (A) may be a function of some covariates. Estimation of Λ will thus allow for an analysis of how the net treatment intensity varies with the covariates, and thus how a change in the eligibility criteria would affect net treatment.

2.2 Matching estimator

To obtain the net treatment intensity, we need an estimate of the counterfactual time-use of the participants, $E(T_i^0|D=1)$. Thus the objective here, as in all non-experimental studies, is to construct a comparison group that is as close as possible to an experimental, randomly assigned, comparison group. A possible method used for solving this problem is matching.³ In short, matching involves pairing participating individuals with a comparison group that is similar in terms of observable characteristics.

The crucial assumption behind matching is that all differences between the participants and the comparison group affecting both the selection and the outcome are captured by characteristics (X) that are observed by the evaluator. This conditional independence assumption (CIA) is formalized as:

$$(8) \quad \{Y^0, Y^1\} \perp\!\!\!\perp D | X = x, \forall x \in \mathcal{X},$$

² The practical value of this estimate may vary between implementations since there is a potential covariance between the effect of the treatment and the net treatment intensity. Defining the maximum treatment effect (that would have been the effect if only the participants received treatment) as $\tilde{\Delta} \equiv E[Y_i^T - Y_i^{NT} | D=1]$ we can rewrite the actual treatment effect as

$\Delta = \Lambda \tilde{\Delta} + \text{cov}(\Lambda, \tilde{\Delta})$. Thus, there may be applications with a large program effect despite low treatment intensity if the covariance is sufficiently negative, *i.e.* if workers receiving a small net treatment also have a large effect of treatment. Whether this is likely or not has to be evaluated from case to case.

³ See Rubin 1977; Rosenbaum & Rubin 1983, 1984, 1985; Rubin & Thomas 1992.

where χ denotes the set of covariates for which the average treatment effect is defined. In words, the CIA requires that, *given all the relevant characteristics* (X), the individual participation decision is not based on the actual outcomes.

When estimating the net treatment intensity instead of the long term effect of the program, the outcome is the *treatment* T . Thus, the CIA must yield independence between the participation D and the treatment T :

$$(9) \quad \{T^0, T^1\} \perp\!\!\!\perp D | X = x, \forall x \in \chi.$$

The assumption requires that, given all the observable characteristics, the participation decision is not based on how much treatment the individual will receive when participating or not in the program. Moreover, in order for the average participation effect to be identified, the probability of participation must be strictly between zero and one:

$$(10) \quad 0 < P(x) < 1, \text{ where } P(x) = P(D = 1 | X = x)$$

When these two assumptions are fulfilled the counterfactual outcome, $E(T_i^0 | D = 1, X = x)$, can be obtained by simply matching the participants with *identical* (with respect to X) non-participants, and then taking the average of the non-participants' outcomes: $E(T_i^0 | D = 0, X = x)$.

In their seminal paper on matching, Rosenbaum & Rubin (1983) show that if the CIA is valid for X , it is also valid for a function of X called the *balancing score* $b(X)$, such that $X \perp\!\!\!\perp D | b(X)$. The main advantage of the balancing score property is the decrease in dimensionality: instead of conditioning on all the observable covariates, it is sufficient to condition on some function of the covariates. In the case of one treatment, the balancing score with the lowest dimension is the propensity score $P(x) = E[P(D = 1 | X = x)]$.

Thus, propensity score matching is a suitable tool for estimating the net treatment intensity whenever the data are rich enough to fulfil the CIA between the treatment and the participation. This in turn should be a reasonable assumption whenever we are willing to assume that the “traditional” CIA between the outcome and the participation. Of course, the validity of this assumption should be considered carefully in each empirical application.

3 Empirical application

3.1 The substitutes in the Swedish Career Break program

The Swedish career-break program was instituted as a pilot program in 12 Swedish municipalities starting in February 2002. The program provides public financing for an employed individual during 3-12 months leave-of-absence.⁴ Employer consent is required, and the employer is free to choose any substitute as long as he or she is registered as unemployed at the unemployment office. That is, the substitutes do not have to be long-term unemployed.

The pilot is scheduled to run until the end of 2004 whereupon the program is to be evaluated. However, during negotiations following the 2002 general elections it was decided that the career-break program should be introduced on a permanent and national basis starting in 2005. Since the final details on the design of the national program were left for discussions during 2003 and 2004, a need arose for some results regarding the consequences of the program before the end of the piloting scheme.

One of the targets, and thereby one of the expected effects of the program is to improve the future labour market situation of the substitutes. The treatment to achieve this is the temporary employment created by the program. As an application of the net treatment intensity parameter, we will estimate the extent to which the substitutes would have been employed even in the absence of the program. Thus, we are able to calculate the fraction of average program length that the participants actually received treatment *due to* the program.

3.2 Data

The analysis uses the register data base from the National Labour Market Board (Händel) that contains information on all registered unemployed individuals as well as employed job-seekers who wish to use the services of the unemployment offices.⁵ In addition to the detailed description of each

⁴ The person on leave is compensated with 85 percent of the person's unemployment insurance (UI) benefits which in turn are 80 percent of his or her wage, with a cap of 80 percent of SEK 20,075 per month ($\approx$ € 2,150 in August, 2003). Similar programmes are, or have been, in place also in Finland, Denmark and Belgium.

⁵ The data base is administrated by the National Labour Market Board (Ams).

individual's labour market history the data set includes information on basic individual background characteristics (see *Table 1*).

The stock of substitutes consists of 2,131 individuals that started their participation during the first eight months of the piloting scheme, *i.e.* between February and September 2002. The substitutes must be registered at an unemployment office in order to be eligible for the program. After removal of individuals with unknown starting dates and inconsistencies in their registered unemployment histories 1,847 substitutes remain in the sample.⁶

Ideally, we would match the substitutes to other unemployed workers at the time the program started, but then we would have to wait until data on the first cohorts who have completed the program is available. Instead, we match the participants to a comparison group from the previous year. The main advantage is that this facilitates estimation of the treatment intensity for all participants who have entered the program. In theory, the net intensity can be estimated as soon as the first participants have entered the program, thus making it possible to answer one of the policy makers' key questions much faster than conventionally.

The use of a comparison group from the previous year can be justified in two ways. First, in this particular case it appears that the economic conditions were very similar in 2001 and 2002.⁷ Second (and more generally), the results can be interpreted as estimates of what the treatment intensity would have been, *if* the program had existed in 2001 *and* the selection process would have been the same as it was in 2002.

The comparison group is chosen from all unemployed individuals registered at any of the unemployment offices in the 12 participating municipalities between February and September 2001. In total 133,279 individuals were registered in one of the participating municipalities during 2001.

3.3 Matching

To avoid problems with seasonality we match each substitute to a worker who was unemployed during the month that the substitute started to participate (but

⁶ 3 % of the individuals were dropped from the sample since their recorded unemployment histories contained fundamental contradictions (see Benmarker et al, 2000). 8 % were excluded due to unknown starting dates for their program participation.

⁷ The monthly unemployment numbers as measured by the Labour Force Surveys are extremely similar in 2001 and 2002; see e.g. SCB (2003).

in 2001). Thus, a substitute starting the program in a given month in 2002 is matched to an unemployed worker registered in the 15th of the *same* month in 2001.⁸

The propensity scores are estimated by probit. *Table 1* shows descriptive statistics on the variables included in the probit estimations in the May-sample.⁹ All time-varying variables (such as the length of the unemployment spell prior to participation) are measured at the start of the program for the substitute. For the comparison persons, they are measured at the 15th during the relevant month.

Besides the propensity, we match on unemployment prior to participation as it is considered as a crucial variable explaining both participation *and* the probability of employment, *i.e.* the treatment. The distance between the estimated propensity and the length of the unemployment spell is measured by a Mahalanobis distance metric.¹⁰ Nearest neighbour matching is applied with replacement.¹¹ Columns (1) and (3) in *Table 1* illustrate the matching quality in the May-sample.¹²

⁸ 15th each month is chosen simply to pick a random date.

⁹ It may be interesting to note that we tested matching on the entire vector of X instead of the propensity, but in spite of the high number of potential comparisons it was impossible to find exact matches to all substitutes.

¹⁰ The Mahalanobis distance metric is frequently used in the matching literature, see e.g. Lechner (2001) and Larsson (2003).

¹¹ However, due to the large size of the stock of unemployed in 2001 only very few persons are matched more than once. Thus, the standard errors are not adjusted for matching with replacement.

¹² A more detailed description and results of the matching procedure can be obtained from the authors by request. A description in Swedish is found in Fröberg et al (2003).

Table 1 Results of the matching procedure (May)

	<i>May substitutes</i>	<i>Entire stock</i>	<i>Matched comparisons</i>
N	224	72 893	224
Length of current registration spell at start of participation or May 15	393	947	391
Days of registration the last 2 years	369	512	359
Days of registration in total until start of participation or May 15	1456	1 740	1467
Status as registered at start of participation or May 15 (%)			
Open unemployment	25,0	29,0	28,6
Part time unemployed	17,0	13,0	19,6
Employed on hourly basis	23,2	10,1	19,2
Temporary employment	8,9	2,9	8,9
On-the-job search	7,1	5,5	5,8
Miscellaneous	18,8	39,5	17,9
Number of spells of registration until start of participation or May 15	3,6	3,0	3,8
Fraction females (%)	62,9	50,4	62,9
Age	34,9	39,4	34,1
Citizenship (%)			
Swedish	94,2	83,9	95,6
Nordic	0,9	2,3	1,3
Non-Nordic	4,9	13,8	3,1
Education (%)			
Compulsory schooling only	20,1	29,6	14,3
High school	58,0	48,6	62,9
Tertiary education	21,9	21,8	22,8
Disability (%)	3,6	19,5	3,6
Searching outside local area (%)	11,2	13,1	13,8
Desired working time (%)			
Full time (%)	57,2	44,9	57,6
Part time (%)	2,2	5,6	3,1
Both	40,6	49,5	39,3
Benefits (%)			
UI	77,2	71,6	77,7
Cash allowance	6,3	6,9	8,5
No benefits	16,5	21,5	13,8

3.4 Results

Treatment is measured by the number of employment days during the average program period of 10 months, or 304 days.¹³ For the substitutes, it is by definition equal to unity. The measure we actually use is the number of days that the matched comparison group spent away from unemployment.¹⁴ Thus, in interpreting the results we equalize non-registration with employment and disregard possible transitions out of the labour force.

Table 2 shows the amount of treatment (in days) for the matched comparison group and the entire stock of unemployed during the average program length (Columns 1 and 2). The results clearly show that persons with good expected labour market outcomes are selected into the program. For example, substitutes starting in the program in June would have been employed approximately 150 of the subsequent 304 days without the program.

Since the matched comparison groups receive much treatment, it is obvious that the net treatment intensity of the program is far from unity. Column (3) shows that without the program, the typical substitute is expected to receive treatment during about half of the average program duration. Thus, strong treatment effects are required from each unit of (net) treatment for the program to pass a cost benefit analysis.¹⁵

¹³ Ten months is the average program length for substitutes in the first cohort, *i.e.* those who already had completed their participation by the time of the data collection. If the net treatment intensity is to be estimated directly after the introduction of a new program, this information is not available and some estimated average program length must be used.

¹⁴ Unemployment is defined as being registered as a job-seeker in a category that warrants compensation from unemployment insurance or other comparable sources (such as compensation for participating in labour market programmes). Thus, only the registration categories *on the job search* and *in temporary employment* are considered as employment. Using a more generous definition of employment that would include even some of the labour market programmes would imply that our estimate of the net treatment intensity would be even lower.

¹⁵ As a reference point it can be noted that the budget calculation assumed a net treatment of between 65 and 95 percent (Statskontoret, 2001).

Table 2 Counterfactual unemployment during participation

	<i>Counterfactual treatment in days (t^0)</i>		<i>Net treatment intensity in % of 304 Days (λ)</i>	<i>Number of comparisons</i>	
	(1)	(2)	(3)	(4)	(5)
Starting month	Matched sample	Entire stock	$\lambda = 1 - t^0/304$	Match	Stock
February	134.7	91.8	0.557	62	75 433
March	137.0	89.3	0.549	111	74 433
April	110.8	90.9	0.636	226	74 148
May	132.5	92.1	0.564	224	72 893
June	149.9	100.4	0.507	228	79 090
July	152.1	105.5	0.500	144	80 999
August	165.6	112.3	0.455	438	82 084
September	141.4	95.4	0.535	414	73 416
Average ¹	143.7	97.5	0.527		
(St.error)	(2.67)		(0.009)		

Note: Counterfactual treatment in days is calculated as the average program length (304 days) minus days of unemployment during this period. Counterfactual treatment is defined the number of days *not* registered in Händel (Händel registrations in the categories “in temporary employment“ and “on-the-job search” are counted as employment) from the 15th each month during the following 304 days. “Net Treatment” is calculated as the increase (in percent) in time spent as non-registered *due* to the program during the (average) program length.

¹The averages are weighted by the number of observations each month.

One aspect that has been discussed in relation to the career-break substitutes is whether there should be a minimum requirement for the number of days of registration prior to the start of the program. Since we match not only on propensity score but also on the length of the unemployment spell prior to participation, it is possible to study how the net treatment effect varies with the spell length. This is done in *Figure 1*. It shows clearly that the net intensity of treatment is increasing with prior unemployment, suggesting that the net treatment could be increased by restricting eligibility to workers with a reasonably long unemployment history.

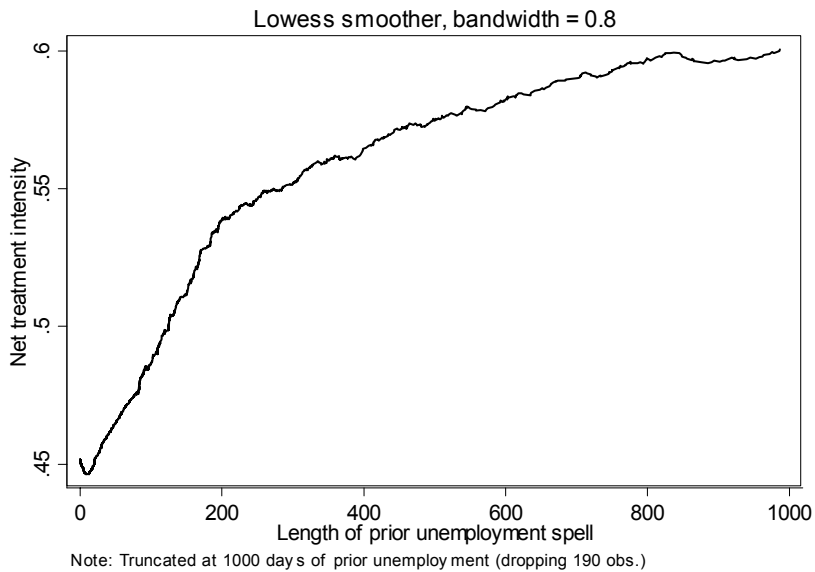


Figure 1 Net treatment intensity and prior unemployment

Finally, there may still be an upward bias in the results. Many of the participants were unemployed for only a short spell before starting the program. The unemployment spell was shorter than a week for approximately 17 % of the sample. It is possible that at least some of the substitutes register as unemployed *only* in order to become eligible for the programme. In that case, their employment without the program would probably have been higher (and the true net treatment intensity lower) than estimated.

4 Summary

This paper proposes an application of standard evaluation techniques that provides useful information about a program as soon as the first participants have entered the program. The specific application estimates the net treatment intensity on the substitutes in the Swedish career-break program. Three main results emerge: First, the selection of participating substitutes favours those that would have received a large amount of treatment even without the program.

Second, the net treatment intensity is far from 1; the net treatment provided by the program was only in the order of half the length of the program. Third, the net treatment was larger for participants with a longer preceding spell of unemployment, suggesting that a change in the eligibility criteria could increase the efficiency of the program.

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