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Using internal replication to establish a treatment effect

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WORKING PAPER 2006:3

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ISSN 1651-1166

Using internal replication to establish a treatment effect*

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25 April 2006

Abstract

In many cases assignment to a treatment may affect concomitant variables. I show how a concomitant variable can be used to corroborate evidence from an observational study. In the observational study two types of training programs are compared. One program is part of regular Swedish labor market training while the other program was run by Swedish industry during 1998-2000. A large and positive effect on employment is found from this latter program. In this program it was much easier to get employer contact than in the regular program. From a survey I have information about employer contacts in the two programs. I find the same positive effect on employment from employer contacts in either program and no effects from the new program when conditioning on employer contacts. I interpret this as a causal effect on employment from employer contacts. In addition, this effect is found to be more pronounced for individuals with a weak position in the labor market.

Keyword: Evaluation; Active labor market training; Information technology; Employment rate; Propensity score matching; Internal replication
JEL: C14, C52, J68

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*The author thanks Gary Barret, Xavier de Luna, Per-Anders Edin, Peter Fredriksson, Maria Vredin Johansson, Sara Martinsson two anonymous referees and seminar participants at Tinbergen Institute Amsterdam, CAFO Växjö university, Department of statistics Umeå university and the warm hospitality of the Department of Economics, UNSW, Sydney.

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1 Introduction

The objective of all evaluation studies is to estimate a causal treatment effect. However, observational studies are plagued by concerns regarding the potential selection into treatment based on potential outcomes. Internal replication (Rosenbaum, 1999) may be a way to examine whether selection is a problem.[1] To illustrate the methodology of internal replication consider the following hypothetical example. There are two essentially identical training programs and we are interested in the relative employment effects. However, the assignment processes to these two programs are entirely different. If the estimated effects are very similar across the two assignment processes, then this suggest that the estimate is causal. If not, the selection bias from both assignments need to be the same - and if the assignment process to the two programs are different then this is highly unlikely. Let us take the idea of internal replication one step further and see how it can be used to corroborate evidence from a observational study.

Consider a case with two training programs, where we have estimated a positive employment effect of one program relative to the other. The question is whether we can believe that this estimate reflects a causal effect or whether it stems from positive selection. Assume that we have knowledge about the content of treatment within the programs and that these two programs differs, albeit in only one dimension. Then this information can be useful to test if the original effect is due to selection or not.

To illustrate why, suppose we have two class room training programs and that it is possible to use computers in some classes in both programs. In one program there are more classes with access to computers than in the other program. Suppose that we have a measure of the access to computers within the two training programs. This gives us an opportunity to estimate the effect of access to computers separately for each program. If these two estimates turns out to be identical, this has two possible implications: (i) if the assignment process to computers differs across the programs, then the logic of internal replication suggest that we have estimated the causal effect of computer access; (ii) if the assignment process is identical across the two programs, then the two estimates may be biased due to selection.

[1] A number of approaches have been proposed to assess the credibility of the assumption invoked to establish causal effects with observational data (see, for instance, Rosenbaum, 1999; Pearl, 2000; Heckman and Hotz, 1989 and Rosenbaum, 1984).

But in the latter case a relative comparison between the two programs provides an unbiased estimate of the causal effect of computer access. The one potential pitfall is if selection to computer assisted training is the same within the programs but the assignment process to the programs differs. This seems like a mostly unlikely real event however.

Now, let us apply these ideas to the real world rather than this hypothetical example and present how the logic of internal replication can be used to corroborate evidence from an observational study. In this study, an active labor market program run by Swedish industry, Swit, is compared to a traditional active labor market program run by the Swedish National Labor Market Board, AMVc. I find the employment propensity to increase by almost ten percentage points if a Swit participant enters Swit instead of AMVc. From a follow-up telephone survey it is found that it is much easier to get employer contacts (EC) within Swit than in AMVc. When controlling for observed characteristics from the telephone survey, I find the effect of EC within Swit and AMVc to be of equal size; the EC effect is estimated to 18 percentage points in AMVc and 16 percentage points in Swit. I also find that the treatment effect of Swit participation after controlling for EC is no longer statistically significant. For the treatment effects of EC within AMVc and Swit to both be biased, the same bias must be attributed to both assignments; that is, the screening to EC on unobservables should be the same within AMVc and Swit. But when I control for EC there is no remaining effect from Swit. This then implies that there is no selection on unobservables to Swit. Thus there should not be a problem with the Swit/AMVc evaluation. All in all, I take this as evidence that the effect of Swit is from increased employer contacts.

The rest of the paper is structured as follows. Section 2 provides a background and compares Swit and AMVc. Section 3 describes the register data used in the estimation of the differential program effects on employment. Section 4 gives the results from the estimations. Section 5 describes the survey and how the survey information on employer contacts is used to establish causal effects. Finally, section 6 concludes.

2 Background and comparison of the programs

In the spring of 1997, the Federation of Swedish Industries approached the Swedish social democratic government about the lack of educated individuals in the area between specialists and users of information technology

(IT). As a result of these discussions, the Federation of Swedish Industries and the Federation of IT-companies set up an active labor market training program aimed at increasing the IT competence. The organization established by these federations was named SwIT (an acronym for Swedish information technology) and the labor market training program, Swit. The program was run between 1998 to 2000 (see Martinson (1999, 2000) for a description of the program). The project was funded with 0.15 billion, which covered the cost for the labor market training, salaries, administration and subsistence for the unemployed individuals in the programs. Groups traditionally underrepresented in the IT-industry was encouraged to attend the program.

All in all, allmost $11,000$ individuals entered Swit during this time period. Of these 11,000 indiduals, about 75 percent was unemployed individulas. The remaining 25 percent was employed individuals but at risk of becoming unemployed.

In this paper, I estimate the effect of entering Swit relative to IT labor market training courses run by the Swedish National Labor Market Board (AMV) on employment six months after ending either of the programs.[2,3] These "traditional" active labor market training programs are denoted AMVc. These programs have been the only alternative for the unemployed before the creation of a specialist IT programme set up by the Swedish industry and, thus, makes it the status quo obvious alternative to SwIT. This restriction for the evaluation, in addition, minimize the selection (on unobservables) problem.[4]

2.1 Comparison of the programs

The rules for eligibility were the same for the two programs. The individuals must be unemployed, at least 20 years of age and enrolled at the public

[2] The Swedish government decided that the evaluation of Swit should be based on the employment rate six months after finishing training and IFAU was responsible for the evaluation.

[3] Johansson and Martinson (2000) also considered differential program effects on earnings. This comparison was based on the earnings stated in the survey and resulted in the same conclusion as for the employment outcome.

[4] In addition to reducing the selection on unobservables problem, the relative comparison helps generate a starting date for the comparison individual. For the estimation problem when estimating the effect of treatment against non treatment, see Abbring and van den Berg (2004) and Fredriksson and Johansson (2004).

employment service. During the training, participants in AMVc and Swit received an equally large amount for subsistence. The weekly cost for Swit and AMVc, respectively, was, on average 273 and 289 (Näringsdepartementet (1999); SwIT-yrkesutbildning (2000)). This cost excludes subsistence for the participants. As was discussed above there was an exception on the unemployment requirement for 25 percent of the Swit participants. This population is not considered in the evaluation and therefore I do not either include them in the comparison below.

The large organizational difference between the two program was that within SwIT, a project leader was responsible for the whole labor market training process (e.g. the selection of individuals to training, advising on labor market training courses, providing a host company etc.) while these functions were shared by many different employees within the AMV.

AMV's procurement of vocational active labor market training courses is based on a biennial forecast of the labor market, performed by county labor boards. The forecast is a collective judgement of the labor market in the county. As a basis for the forecast − beyond the statistics and a judgement of the present situation − a survey is distributed to employers with more than 100 employees within the county. Most county labor boards have staff responsible for keeping contacts with the local industry. As a collaboration between the industry and the county labor board, there also exist regional competence boards and local employment service committees. Project leaders in Swit contacted companies (by e.g. visits) and identified the need for competence. Based on the companies' needs, the project leader suggested a labor market training course, which was then approved and procured by the SwIT organization secretariat in Stockholm.

The quality of the courses provided by the AMV should be similar to the courses provided by the SwIT organization, since these were bought from the same private training companies. The types of IT courses in Swit and AMVc are displayed in Table 1. The similarities of the two programs are apparent. Thus, despite the differences in procurement between the two organizations (SwIT and AMV), there do not seem to be any large differences in the type of labor market training courses provided.

The degree of contact between Swit participants and project leaders varies during the period of training. In most cases, project leaders payed a visit to the course and they were also supposed to discuss the quality of the labor market training with the trainee (SwIT-yrkesutbildning, 2000). Within AMVc, an AMV employee was supposed to pay a visit a few weeks

Table 1: The frequency distribution of the courses within the two programs. The sample consists of respondents to the survey (see Section 5).

	AMVc ($n = 796$)	Swit ($n = 794$)
Programmer	32	27
Computer technician	31	29
Application support	10	16
IT-pedagogue	2	6
IT-entrepreneur	1	3
Other	17	15
Missing	7	4

into the labor market training course to decide on the quality of the course. However, according to (Ds 2000:38), this practice was rarely followed.

The fundamental idea with Swit was to increase the contacts between program participants and employers. One means of achieving this was to provide a host company for the trainee,[5] which could take an active part in the training, e.g. by suggesting a labor market training course. The trainee was encouraged to keep in contact with the host company, for example by visits. The trainee could discuss the training and future needs of competence with the host company and its employees.

During the training, participants in both Swit and AMVc could get job practice with one or several employers. The job practice was, in many cases, arranged by the private training company and it was assumed to provide an understanding of the topics of the course in real life situations. Naturally, the contents of the job practice vary with the course and the company responsible for the training. Preferably, it should be "hands-on" job practice rather than idle observations. I do not have any information on whether Swit trainees with a host company also get job practice within this company; however, I believe this to be highly likely.

The Swit project leader performed a test and an interview with the applicant. The test was supposed to measure the applicant's motivation and ability, not the applicant's previous knowledge and experience within

[5] From the survey, I know that not all participants consider that they had a host company. The results from the questions in the survey are further discussed in Section 5.

the IT-area (Martinson, 1999). The individual's motivation and ability were also of great importance for AMV's selection method. The selection rule of the Public Employment Service (PES) differs between the type of labor market program and the specific needs of the employers. It has been documented (Ds 2000:38) that all county labor boards use tests to select applicants for qualified labor market programs such as those studied here. Most often, a test is followed up by an interview performed by the responsible training company. Applicants are then divided into groups according to their degree of suitability. Thus, the selection of participants into the two programs differs to some extent. However, in contrast with many other evaluations of active labor market training programs the selection was very similar.[6]

3 Data and selections

The register data is collected from AMV. This data contains information about all individuals registered at the public employment service since 1991. It provides detailed information on each individual's labor market status over time, together with important characteristics of the unemployed.

3.1 Selections

I have data on all Swit and AMVc participants from January 1, 1998 to May 30, 2000. Disregarding the employed Swit participants, the sample consists of 23,442 individuals: 8,055 in Swit and 15,387 in AMVc. In Table 2, descriptive statistics for the total sample, divided into Swit and AMVc, are given. From this table, it can be seen that Swit participants are significantly younger, better educated and less vocationally disabled. There are no significant differences between the two groups in the proportion of women and the proportion of non-Nordic citizens.

In the following evaluation, I discard individuals that: i) had less than six months of follow up duration from when ending the training (3,178 for Swit and 2,828 for AMVc), ii) had zero days in either Swit or AMVc (58 and 209, respectively), ii) had previously attended Swit or AMVc

[6] Personal communication with officials at the county labor board revealed that the test used to screen individuals by the SwIT organization had been considered. However, this was not used since they believed their actual test to be equally good.

(162 and $2,215$, respectively) or iv) had, within the evaluation period, started another labor market training program after finishing either Swit or AMVc (956 and $3,776$, respectively). The reason for the last exclusion is that AMV's programs can consist of a planned sequence of different courses, with different codes in the register (e.g. a person who enters a labor market training program to become a salesman can first take an IT course and thereafter enter a course in customer services etc.). Swit, on the other hand, was purely oriented toward IT. If the probability of returning to a labor market training program that has not been decided before starting the program is different for the two programs, this exclusion is of course not correct. When including these individuals in the analysis, I get the same results as those presented in next section, however.

The analytical sample consists of $3,760$ Swit trainees and $6,941$ AMVc trainees. The descriptive statistics (see Table 3) are similar to the total sample. From Table 3, it appears that participants in Swit are, to a larger extent, male, younger, better educated, Nordic citizens and less vocationally disabled than AMVc participants.[7] Swit participants also have fewer previous days in unemployment, subsidized employment and labor market programs (including computer active centers). Furthermore, Swit participants have a larger number of days in previous labor market programs focusing on IT courses. On average, the duration in Swit is somewhat longer than the duration in AMVc (174 and 150 days, respectively). There are no regional differences in the Swit/AMVc participation ratio (not reported).[8]

The last row in Table 3 gives the proportion of participants employed six months after finishing the program.[9] Table 3 shows that 57 and 43 percent of the Swit and the AMVc trainees were employed six months after finishing the respective program. The difference is statistically significant at all reasonable levels of significance.

[7] Standard t-tests are given in column 2 in Table 6.

[8] Sweden consists of 25 counties. In the analysis, I control for differences in local labor market conditions by including a county factor.

[9] An individual is defined as employed if he/she is de-registered at AMV as i) employed, ii) employed for a limited period of time, iii) employed part time or iv) registered at AMV as employed at an hourly basis or temporarily.

Table 2: Descriptive statistics (mean and standard deviation, st.dev.) for the inflow into Swit ($n = 8,055$) and AMVc ($n = 15,387$) from January 1, 1998 to May 30, 2000.

	Swit		AMVc	
Variable	mean	st.dev	mean	st.dev
Age	32.99	8.70	34.64	9.55
MEN (%)	64	48	63	48
Vocational disability (%)	4	21	9	28
Non-nordic citizen (%)	8	27	9	28
Less than 10 years of schooling (%)	14	35	17	38
10-13 years of schooling (%)	67	47	67	47
University degree (%)	19	39	16	37

Table 3: Descriptive statistics (mean and standard deviation, st.dev.) for the analytical samples used in the evaluation.

		Swit (n = 3,760)		AMVc (n = 6,941)	
Variable	Description	mean	st.dev	mean	st.dev
AGE	Age	33	8.64	35	9.42
MEN	Men (%)	64	48	61	49
VD	Vocational disability (%)	5	22	9	29
CITIZ	Non-Nordic citizen (%)	8	27	9	29
EL 1	Less than 10 years of schooling (%)	14	35	19	39
EL 2	10-13 years of schooling (%)	67	47	66	47
EL 3	University (%)	18	39	14	35
DAYS	Number of days in Swit/AMVc	174	70	150	105
UNEPD	Number of days in unemployment	611	468	778	477
PROGD	Number of days in subsidized employment	158	220	219	259
LMTD	Previous number of days in active labor market programs (excluding IT courses)	94	177	118	202
CACD	Number of days in a computer active center	13	33	23	43
CCD	Number of days in IT courses	31	78	22	70
Y	Employed six months after finishing **a** program (%)	57	49	43	49

4 Estimating the relative effect

The interest lies in the effect on employment of joining Swit relative to AMVc for Swit participants.[10] For Swit participants, the outcome if joining AMVc is not observed. The evaluation problem consists of estimating this counterfactual outcome. In the following, I discuss the evaluation problem and estimation, using the "potential outcome" framework (Rubin, 1974).

Define the employment if participating in AMVc and Swit by $Y(0)$ and $Y(1)$, respectively. Let $D = \{0, 1\}$ denote the actual program assignment, such that $D_i = 1$ if individual i participated in Swit.

The evaluation parameter of interest is the average treatment of the treated effect

$$\Delta = E(Y(1) - Y(0)|D = 1) = E(Y(1)|D = 1) - E(Y(0)|D = 1). \quad (1)$$

The first term, the average outcome for a Swit program participant, is observed in the data. This is not the case for the average outcome from attending AMVc for the Swit participant, however. Identifying assumptions need to be invoked to overcome the fundamental missing data problem, since no individual can participate both in AMVc and Swit in the same time period. I assume treatment assignment (to Swit or AMVc) to contain no informative about employment propensities if in AMVc when conditioning on observed covariates $\mathbf{x}$, that is:

$$Y(0) \perp\!\!\!\perp D|\mathbf{X} = \mathbf{x}, \forall \mathbf{x} \in \Xi. \quad (2)$$

Here, $\Xi \subseteq R^K$ defines the set of $\mathbf{X}$ for which the treatment effect is defined. Assumption (2) is denoted as conditional independence in Lechener (1999).[11]

Let the conditional probability to enter Swit given $\mathbf{x}$ be $p(\mathbf{x}) = \Pr(D = 1|\mathbf{x})$. In the literature, this is denoted as the propensity score. Furthermore, assume that there is some random element to be treated among the treated for each value of $\mathbf{x}$, thus:

$$p(\mathbf{x}) < 1, \ \forall \mathbf{x} \in \Xi. \quad (3)$$

[10] The reason why I focus on the effect for those entering the program (i.e., the treatment of the treated effect) is that there was no government intention to make this program permanent.

[11] Imbens (2000) denotes it as a weak uncounfoundness assumption as it is a weaker condition than the unconfoundedness assumption in Rosenbaum and Rubin (1983) where the assignment is ignorable for the outcomes if both treated and not treated when conditioning on observed covariates.

Given (2), (3) and that Swit participants do not affect the outcomes for AMVc participants (this is the stable-unit-treatment-value assumption (SUTVA) see, e.g., Rubin (1990)) the unobserved counterfactual can be identified as

$$E(Y(0)|D=1) = E_{\mathbf{X}}[E(Y(0)|D=1,\mathbf{x})] = E_{\mathbf{X}}[E(Y(D=0),\mathbf{x})].$$

The inner expectation is identified using the conditional independence assumption (CIA) (2) and the outer expectation is taken with respect to the distribution of $\mathbf{X}$ for Swit participants.

I order to evaluate wether the Swit participants affects the outcome of the AMVc participants (i.e. SUTVA) we need to consider the volume of trainee's within the two programs and also to consider the demand for IT personal at this time period. The volume of Swit trainees was 8,500 unemployed individulas. The volume of AMVc trainees was at this time period around 20,000 unemployed individulas. Thus, Swit constitute an approximately 40 percent increase in educated individuals directed towards IT. According the Federation of Swedish Industries there was at this time period an excess demand of IT educated individuals. If this demand was large the SUTVA may be appropriate, otherwise SUTVA is less likely appropriate.

The necessary condition to identify the average treatment of the treated estimand (1) above is that the covariates for the treated constitute a subset of those for the untreated. If there are regions where the support of $\mathbf{X}$ does not overlap for the two groups, matching must be performed over the region of common support; the estimated treatment effect is then the average treatment of the treated effect within the common support.[12]

Rosenbaum and Rubin (1983) showed that $D \perp\!\!\!\perp \mathbf{x}|p(\mathbf{x})$. This means that the propensity score provides a parsimonious[13] way of adjusting for differences in a (generally large) set of pre-program variables between Swit- and AMVc-participants. Under the CIA and the restriction (3), the counterfactual can be estimated as

$$E(Y(0)|D=1) = E_{p(\mathbf{x})|D=1}(E(Y(D=0),p(\mathbf{x}))),$$

[12] Observe that if the treatment effect varies over individuals, restricting the interpretation to individuals with common support may change the interpretation of the parameter estimated.

[13] $p(\mathbf{x})$ and $\mathbf{x}$ are the coarsest and the finest balancing score, respectively. A balancing score is a a function of $\mathbf{x}$, $b(\mathbf{x})$ such that $D \perp \mathbf{x}|b(\mathbf{x})$.

where $E_{p(\mathbf{x})|D=1}$ is the expectation with respect to $p(\mathbf{x})$ of the Swit trainees. Hence, the employment propensity experienced by the matched pool of AMVc participants identifies the counterfactual employment propensity for Swit participants had they instead participated in AMVc.

Different matching algorithms have been suggested in the literature (see e.g. Rubin (1973, 1979) and Rosenbaum (1995) for estimators not re-using the comparison sample and Heckman, Ichimura and Todd (1998) for a kernel based matching estimator). Here, a kernel based matching estimator is used:

$$\widehat{\Delta} = \frac{1}{n^*_{Swit}} \sum_{s=1}^{n^*_{Swit}} \left[y_s - \sum_{j=1}^{n_{AMVc}} k(\widehat{p}(\mathbf{x}_s), \widehat{p}(\mathbf{x}_j)) y_j \right], \tag{4}$$

where y_s and $\widehat{p}(\mathbf{x}_s)$ are the outcome and the estimated propensity score for the Swit trainee s, y_j and $\widehat{p}(\mathbf{x}_j)$ are the the corresponding variables for the AMVc trainee j, $k(\widehat{p}(\mathbf{x}_s), \widehat{p}(\mathbf{x}_j))$ is a kernel estimator (see e.g. Härdle, 1990) and n^*_{Swit} is the number of Swit participants with the same support as the individuals within AMVc.

The generalized additive model (GAM) (Hastie and Tibshirani, 1990) is employed in the estimation of the propensity score, $p(\mathbf{x})$. The variables used as covariates in the model are age (AGE), gender (MEN), vocational disability (VD), level of education (EL 1 and EL 2), citizenship (CITIZ), labor market history (UNEPD, PROGD, LMTD, CACD and CCD), and the county where the unemployed is registered (a factor in 25 levels). For the continuous variables (labor market history and age), a locally weighted running-line smoother (loess) with the bandwidth 2/3 (see e.g. Hastie and Tibshirani, 1990, chapter 2) is used. Parameter estimates from the GAM together with estimates from a standard logit model are presented in Table 4. Given the descriptive statistics displayed in Table 3, all parameter estimates have the expected signs. For example, the propensity for non-Nordic citizens and vocationally disabled to enter Swit was small while it was higher for well-educated men. Hence, groups with a traditionally better position in the labor market entered Swit rather than AMVc.

From Table 5, it can be seen that the predictions of the GAM improve on the standard logit model, especially when it comes to predicting Swit participation. Histograms for the propensity scores for Swit and AMVc participants are displayed in Figure 1. It can be seen that the degree of common support is large and the distribution for AMVc participants

Table 4: Parameter estimates (Estimate, standard error, s.e., and t-value = Estimate./s.e.) of the propensity score, using the GAM. As a reference, the parameter estimates from a standard logit (LOGIT) model are also included.

	LOGIT[1]			GAM[1]		
Variable	Estimate	s.e.	t-value	Estimate	s.e.	t-value
(Intercept)	0.832	0.118	7.061	-0.5604	0.0710	-7.894
UNEPD	-0.002	5.3e-5	-11.344	[2]		
PROGD	-5.4e-4	1.0e-4	-5.353	[2]		
LMTD	1.3e-4	1.2e-4	0.114	[2]		
CACD	-0.006	6.1e-4	-10.621	[2]		
CCD	0.002	2.9e-4	7.979	[2]		
AGE	-0.023	0.002	-8.966	[2]		
EL 1	0.176	0.060	2.935	0.176	0.060	2.912
EL 2	0.503	0.076	6.612	0.495	0.076	6.476
CITIZ	-0.240	0.079	-3.053	-0.271	0.079	-3.435
VD	-0.493	0.091	-5.438	-0.516	0.091	-5.671
MEN	0.104	0.044	2.364	0.107	0.044	2.420

[1] A regional (county) factor (in 25 levels) is also included in the models.

[2] The effect of the continuous variables in the GAM is estimated with a loess smoother with a bandwidth of 2/3.

Table 5: Prediction with the semiparametric GAM and the logit model.

Model	GAM		LOGIT	
Predicted\Observed	AMVc	Swit	AMVc	Swit
$\mathbf{1}(\widehat{p}(\mathbf{x}) < 0.5)$	57.7	23.8	57.7	24.6
$\mathbf{1}(\widehat{p}(\mathbf{x}) \geq 0.5)$	7.2	11.4	7.1	10.6
Percentage correctly predicted	69.0		68.3	

Note: Individuals predicted with values larger than **or** equal to 0.5 are classified as Swit participants and individuals with a prediction of less than 0.5 are classified as AMVc participants is more right skewed than is the distribution for Swit participants. The largest value for the propensity score is 0.85 for a Swit participant and the largest propensity score for an AMVc participant is 0.84.

In the matching, I use a Gaussian kernel with a bandwidth estimated using cross validation (the estimated bandwidth is 0.10). The number of Swit participants with the same support as AMVc participants is $n^*_{Swit} = 3,741$ (i.e. over $\widehat{p}(\mathbf{x}) < 0.79$). This gives $\widehat{\Delta} = 0.096$. Calculated using bootstrap[14], the standard error is 0.0102. Thus, for Swit participants, Swit increases the employment chances by 9.6 percentage points as compared to participating in AMVc.[15]

To see whether the GAM and the kernel based matching estimator remove the imbalance in observed covariates seen in Table 3, I present standardized difference measures before and after matching in Table 6.[16] The denominator in the before and after comparison is simply the standard error of the differences in the means. Thus, in the before comparison, the standardized difference is the unmatched t-test. All variables differ significantly in background before matching, with one exception. After matching, the standardized difference is reduced from a low 36 percent to a high 95 percent and, for almost all variables, there is no significant difference.[17] Hence, the GAM and the kernel based matching estimator seem sufficient for removing the pre-program differences in observed characteristics.

In addition to this average treatment of the treated effects, I also estimate treatment effects conditioned on the propensity score. The employment propensities conditional on the propensity score $\widehat{p}(\mathbf{x})$ are estimated using a cubic B-spline smoother (Hastie and Tibshirani, 1990, chapter 2). The result from the estimation is shown in Figure 2. Under the simplifying assumption of $\widehat{p}(\mathbf{x})$ being known, the 95 percent confidence intervals are

[14] The bootstrap is performed with 500 replications with sampling with 100 percent replacement and with a bandwidth equal to 0.10.

[15] When using all individuals in Swit, i.e. $n^*_{Swit} = 3,760$, I obtain the same estimate. Hence, the condition of common support is not important for the obtained result.

[16] The standardized difference in Rosenbaum and Rubin (1985) differs from this. In their definition, the denominator is given by the square root of the average of the sample variances in the treated and non treated group.

[17] The results in the table remain the same if I use the new variance for the matched mean.

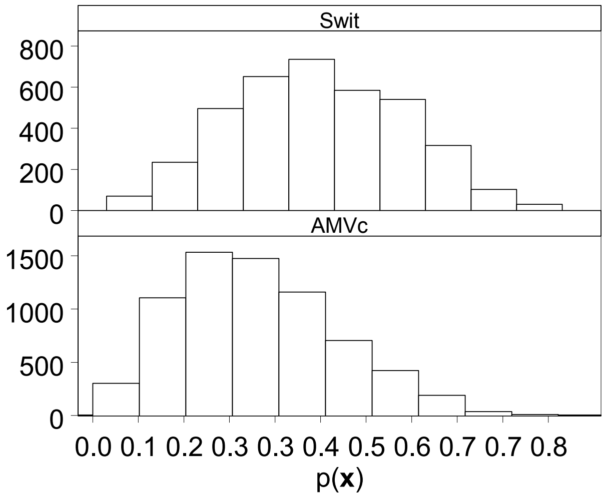

Figure 1: Histogram of the propensity scores from the GAM for Swit and AMVc participants, respectively.

calculated as pointwise standard error ($s.e$) bands

$$\widehat{\mu}(\widehat{p}(\mathbf{x})) \pm 1.96s.e,$$

where $\widehat{\mu}(\widehat{p}(\mathbf{x}))$ are the predicted means for $E(Y(0)|\widehat{p}(\mathbf{x}), D = 1)$ and $E(Y(1)|\widehat{p}(\mathbf{x}),\ D = 1)$, respectively. The standard errors are calculated using jackknifed residuals (see e.g. Venables and Ripley, 1997). The most obvious feature of Figure 2 is the positive slope, i.e. that individuals with a high propensity to enter Swit have a better chance of becoming employed, also absent the program. It is also noteworthy that the gradient differs between the two programs. For low values of $\widehat{p}(\mathbf{x})$, there is a large difference between the programs, while this difference gradually disappears at larger values of $\widehat{p}(\mathbf{x})$, as shown in Figure 4. The difference is $20 - 0$ percentage points and it is statistically significant (at the five-percent level) over the interval $0 < \widehat{p}(\mathbf{x}) < 0.5$.[18]

To see how this conditional estimator compares with the average treatment of the treated estimate, I also estimate the average treatment by averaging $\widehat{\mu}(\widehat{p}(\mathbf{x}))$ over the density distribution of $\widehat{p}(\mathbf{x})$ for Swit participants. This distribution is estimated using a Gaussian kernel with an unbiased cross validated bandwidth (see Figure 3). For completeness, I have also included the density distribution of $\widehat{p}(\mathbf{x})$ for AMVc participants.[19] The mean effect is estimated using this truncated density distribution (evaluated at 161 points) over the region $\widehat{p}(\mathbf{x}) < 0.79$.[20] In Figure 4, I have also included this (re-scaled and truncated) density distribution. This estimation procedure gives a mean effect of $\widehat{\Delta} = 10.2$ percentage points of entering Swit instead of AMVc for Swit participants.

The two estimators give practically the same point estimates. However, the second method also provides estimates of $\Delta(p(\mathbf{x}))$, which is useful when I interpret the results.

The estimates in Figure 4 can be given two interpretations: i) compared to AMVc, Swit creates better job opportunities for individuals with

[18] Since I assume that $\widehat{p}(\mathbf{x})$ is known, the confidence interval is most likely too narrow. One option might be to base the confidence intervals on a bootstrap estimator. The properties of the bootstrap estimator are not known in this setting, however.

[19] These distributions can be compared with the histogram in Figure 1.

[20] This means that I exclude the region with less than five individuals with common support. Estimations have also been performed with different degrees of common support; from ($\widehat{p}(\mathbf{x}) < 0.60$) to no restriction on the common support at all. The average treatment effect estimates are all, on the third decimal, the same.

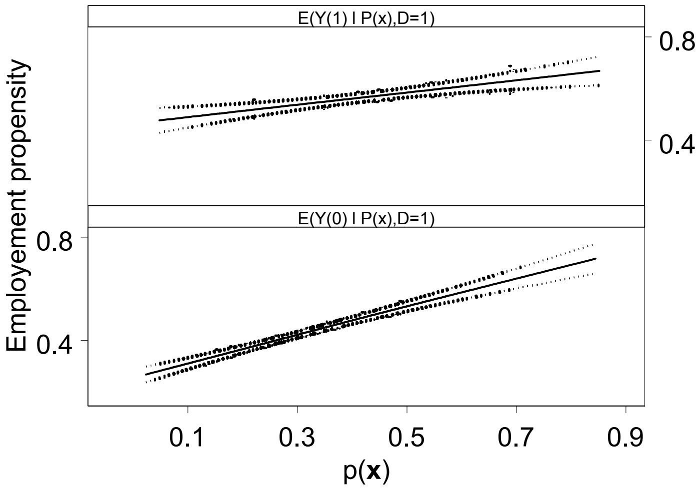

Figure 2: The conditional on $p(\mathbf{x})$ estimated employment propensity together with a 95 percent confidence interval of the estimate. A cubic B-spline smoother is employed and the smoothing parameters are estimated using cross validation.

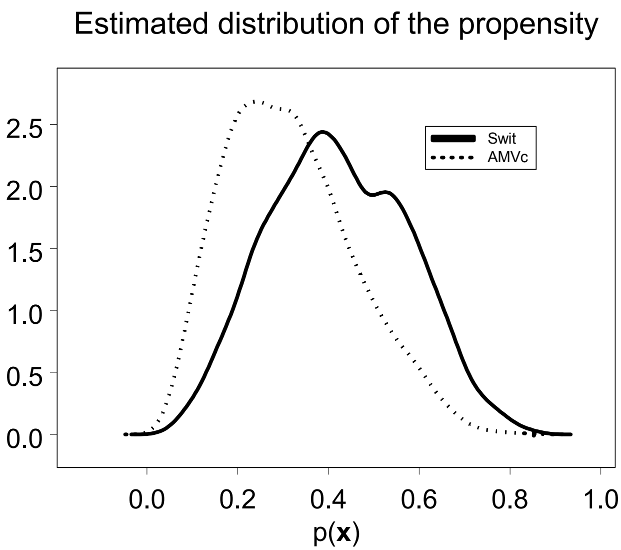

Figure 3: The estimated density distribution of the propensity to enter the Swit-program for Swit and AMVc participants, respectively. The estimation is performed using a Gaussian kernel with cross validated bandwidths (0.110 and 0.097 for Swit and AMVc, respectively).

Table 6: Imbalance of the covariates before and after matching.

	Before	After	Reduction (%)
AGE	-11.23	-1.81	83.9
MEN	3.66	1.14	68.7
VD	-12.12	-2.24	81.5
CITIZ	-2.47	-1.59	35.8
EL 1	-8.19	-2.04	75.0
EL 2	1.29	-0.06	95.3
EL 3	6.02	2.02	66.4
UNEPD	-4.13	-1.32	68.0
PROGD	-4.62	-1.43	68.9
LMTD	-2.78	-0.94	66.1
CACD	-12.35	-2.58	79.1
CAC	4.59	1.19	74.1

Note: Standardized differences are $(\overline{\mathbf{x}}_1 - \overline{\mathbf{x}}_j)/(Var(\overline{\mathbf{x}}_1) + Var(\overline{\mathbf{x}}_0))^{1/2}$, where $\overline{\mathbf{x}}_j$ is either, $\overline{\mathbf{x}}_0$ or $\overline{\mathbf{x}}_{0M}$, $\overline{\mathbf{x}}_1$ and $\overline{\mathbf{x}}_0$ are the mean values for the Swit and the AMVc, respectively and $\overline{\mathbf{x}}_{0M}$ are the mean values after matching, calculated, for each $j = 1, ..., K$, as $\overline{x}_{0Mj} = n_{Swit}^{*-1} \sum_{s=1}^{n_{Swit}^{*}} \sum_{j=1}^{n_{AMVc}} k(\widehat{p}(x_s), \widehat{p}(x_j))x_{j0}$,

a traditionally weak position on the labor market (e.g. individuals with a vocational disability, a large number of days in unemployment and less educated individuals) or ii) in the selection process, the SwIT organization has succeeded in picking individuals with high unobserved ability.[21] Hence, in the latter case, it is only the SwIT's enrollment test that makes the difference, not the difference in quality of the labor market program.

In the following section, this will be further explored using the additional information from the telephone survey.

[21] The argument for ii) is based on the assumption that: 1) the estimated propensity score measures ability (or motivation) of the unemployed individuals, 2) unobserved ability (or motivation) is positively correlated with the propensity score, and 3) additive separability of the two factors when regressing differences (Swit-AMVc) in employment on the estimated propensity score.

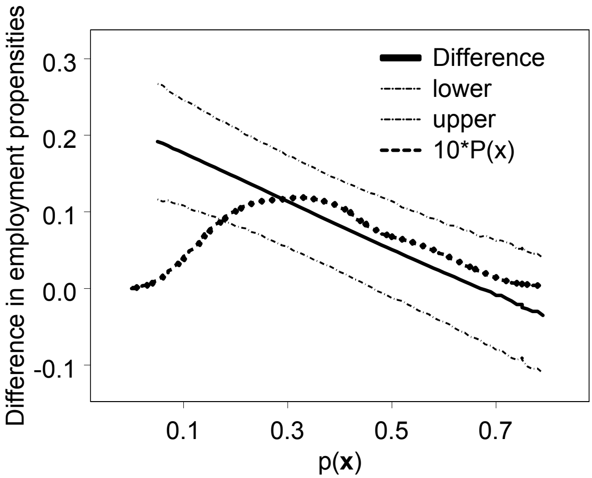

Figure 4: The difference (together with a 95 percent confidence interval – lower and upper) in employment propensities between Swit and AMVc and the re-scaled and truncated estimated density distribution.

5 The telephone survey

The purpose of the survey was above all to get comparable statistics between Swit and AMVc. The two organizations (SwIT and AMV) delivered different statistics on the number of Swit participants, the timing of participation and the employment rate. The survey was conducted in June 2000 on $1,000$ program participants from either program. The sample was taken from those ending the programs in November − December 1999 and only the unemployed Swit participants were selected. The response rates were 79.4 and 79.6 percent for participants in Swit and AMVc, respectively.

The survey contained a total of 19 questions. These concerned i) the individual's background, ii) the individual's labor market training and iii) the individual's present (i.e. in June 2000) labor market situation (see Johansson and Martinson (2000) for a thorough description of the survey conducted). Information on the individual's labor market training which contains information on job practice, type of course and how satisfied she was with the course was of special interest. In the following, the information on job practice and if Swit participants had contacts with an host company will be used to evaluate whether the effect of Swit is genuine or merely the effect of selection on unobservables.

5.1 Selection or effect?

According to the survey, 64 and 52 percent of the Swit and AMVc participants, respectively, were employed six months after having finished their labor market training (see Table 7). The level of employment is about five percentage points higher in the survey as compared with the register information. One reason for this quite large difference is that the Public employment Service (PES) lack information about when the unemployed become employed. When an unemployed individual has not been heard of for three months, the PES send an inquiry asking whether the person is employed. If there is no response, the person is recorded as "work status unknown" instead of employed. For a lengthy discussion about these problems, see (Ds: 2000:38).[22]

The difference in employment rates is very similar, though: 12 and 14

[22] For further discussion on this issue, see Bring and Carling (2000) and Sianesi (2004, 2002).

percentage points for the survey and the register data, respectively. Hence, the register data for these two programs is not systematically incorrect.[23]

The most interesting information obtained from the survey concerns the question of whether the training included job practice (JP). From Table 7, it can be seen that 69.5 percent of the Swit participants and, only, 52 percent of the AMVc participants stated that they participated in JP.

The employment rates for the two programs with and without JP are shown in Table 7. For the group with JP, the employment rate for the Swit and AMVc participant is 66.6 percent and 61.4 percent, respectively. This difference is five percentage points and is not significant at the ten percent level. For the group of individuals without JP, the corresponding fraction with employment is 59.3 percent and 42.8 percent, respectively. For Swit participants, this implies a seven percentage point difference in employment rates between the two groups, while for AMVc, the statistically significant difference is 18 percentage points. One explanation for this observed large difference within AMVc but not within Swit could be that Swit participation increased the contacts with employers, also absent JP. One way of achieving this was to provide a host company for the trainee. From the survey, I find that among the 30 percent (232 individuals) in Swit that did not get any JP, 24 percent stated that they had been in contact with a host company.[24]

I have sub-divided the Swit sample into those with employer contacts (EC) and no employer contacts. Those with JP or a host company, or both, have an EC. From Table 7, it can be seen that more than 77 percent of the Swit participants have an EC. Among those with an EC, 67.2 percent are employed while only 54.9 percent of those without an EC are employed.

Within Swit, the difference in employment for those with and without EC is more than 12 percentage points, while within AMVc the corresponding difference is 18 percentage points. The difference between these "effects" in Swit and AMVc is 6.2 percentage points and this is not a statistically significant difference. The pattern within Swit is now very similar to that within AMVc. For both samples, there is a statistical significant difference between those with and those without EC (see Table

[23] This observation is confirmed for other types of programs. Forslund et al. (2004) do not find any systematic difference in the coding of employment in the AMV register between openly unemployed and individuals in subsidized employment programs.

[24] In total, 43 percent of the Swit trainees had been in contact with a host company.

9, columns 2 and 3 and columns 6 and 7). The question is then whether these observed differences can be attributed to effects of EC or to sample selections.

The problem with the survey data is that it does not contain the same information about the individuals as the register data and unfortunately, it was not possible to match the register data with the survey for confidentiality reasons.[25]

First, I turn to the observed differences between the Swit and the AMVc sample. From Table 7, it can be seen that compared to the AMVc, the Swit sample seems to have better opportunities of finding a job also absent any labor market training (less disabled, more educated and more people from Stockholm). When comparing the survey sample with the register sample, it might be possible to see that the Swit survey sample is a positively selected sample (see VD and EL 3). However, this does not seem to pertain to the AMVc survey sample (there are more individuals with a VD, but there are also more with ED 3). Thus, the survey sample seems to differ from the register data to some extent. This is especially true for the Swit sample.

Turning to the difference between those with and without EC, it is possible to see from Table 7 that within Swit, those without EC seem to be a positively selected group (see VD and EL 3). However, no selection can be seen within AMVc.

The interest is to see i) if the pattern of effects from EC in Swit and AMVc remains if I control for observed covariates and also ii) if the effects from Swit are reduced (or even non existing) when I control for EC and covariates. Since the sample sizes for the different sub-divisions are quite small, linear probability models are used in the estimation.[26] First, I discuss the results from the effect of Swit, thereafter I discuss the effects of EC

From Table 8, it can be seen that when I control for the observed covariates (including a region factor), the effects of Swit on employment

[25] However, using area code number, gender and labor handicap, 121 and 270 Swit and AMVc participants can be identified in the registers. Based on this matched data, I performed the same analysis as below but with the same control variables as in the Swit evaluation. The estimates (estimated with lower precision) from this analysis are remarkably similar to those below.

[26] I use ordinary least squares estimators and standard errors are adjusted for heteroskedasticity. Logit regression and an exact matching estimator have also been used. The results do not change qualitatively with the method used.

are reduced to half of the original effect for both groups (EC and not EC) and that both effects are not statistically significant.

When I regress employment on EC and control for observed covariates (including a region factor), I find (see Table 9) a 16 and 18 percentage point effect within Swit and AMVc, respectively. Within Swit, the effect increases from 12 to 16 percentage points, while for the AMVc sample, the effects of EC remain at 18 percentage points. Thus, when controlling for observed covariates, the effect of EC is basically the same within Swit and AMVc.

6 Discussion

I find that joining Swit increased the job chances for Swit participants by 20 percent as compared to participation in similar courses within the "traditional" active labor market training programs. From a non-parametric regression of the propensity score on employment, I also find that the differential effect is largest for individuals with a traditionally weak position on the labor market. As was discussed in Section 4 this pattern, of a large effect for those with a weak position, may indicate that the SwIT organization was good at selecting the most able and motivated individuals and that there was no effect of the program in itself.

From a complementary telephone survey, an almost identical effect (as the effects found between the two programs) of job practice within AMVc was found. For the Swit sample, no statistically significant effect of job practice was found, however. One fundamental idea with Swit was to increase the contacts between employers and program participants, e.g. by providing a host company. When further dividing the Swit sample into people with employer contacts and no employer contacts and also controlling for covariates, an almost equally large effect of employer contacts is found within Swit and AMVc. Moreover, when controlling for employer contacts and observed covariates from the survey, I find no statistically significant effect of Swit.

If the two treatment effects of EC should be biased estimates of the true treatment effect, then the same bias must be attributed to both treatment assignments. Thus, I have quite strong evidence that the effect of Swit stems from increased employer contacts and not from differences in screening on (for us) unobserved characteristics or from organizational differences between SwIT and AMV. That the effect of EC is smaller within Swit than within AMVc may be that the provision of a host company was just one way of increasing employer contacts; thus also absent a host company or job practice, there may have been more contacts between employers and Swit program participants than between employers and AMVc participants.

The effect of increased job search assistance has been studied in a few social experiments (see Meyer (1995) for a thorough review and also Ashenfelter, Ashmore and Deschêns (1999)). The evidence from these experiments appears to be that increased service and work search requirements have positive effects on employment propensity (e.g. decreasing the time an individual spends on unemployment insurance). The question is whether economic incentives (stricter enforcement) or increased matching (increased service) is the key determinant in reducing the time on unemployment insurance. The bottom line according to (Ashenfelter et al. 1999, p. 3) is that

"*... the results of both sets of experiments imply that providing workers with subsidized job search assistance may be a relatively inexpensive way to provide cost effective, but small, benefits for both workers and society.*"

To my knowledge, there are no previous studies of the effect of increased employer contacts or job practice within active labor market training programs. Increasing employer contacts within programs can be seen as an increase in the matching between unemployed individuals and potential employers. If so, increasing the employer contacts for the unemployed is likely to be beneficial for society as well as for the unemployed. This is consistent with results from unemployment insurance experiments. The employer contact is also found to be more valuable for unemployed with a weak position on the labor market, which is what will be observed if statistical discrimination prevails in the Swedish labor market. Evidence of statistical discrimination in the Swedish labor market has been empirically established (see Edin and Lagerström, 2002).

The Swit trial was conducted in the IT-sector which was characterized by large growth at that time. This is also a sector where the employees are the main company assets. Thus, the effects of increased matching were likely to be large in this sector at that time. Even so, the estimated effect is very large and it is plausible that increased employer contacts within active labor market training in other sectors of the labor market will also increase the chances of employment.

Methodologically, this paper demonstrates how a follow-up survey can be useful in suggesting likely causes (or *mediating variables*) for estimated effects and how to refute ostensible effects using the logic of internal replication: I have one treatment "employer contacts" and two treatment assignments. For the two treatments to be both biased estimates, the same bias must be attributed to both assignments and yield the pattern anticipated from an actual effect.

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Table 7: Descriptive statistics (mean and standard error (se)) for Swit and AMVc survey data. The description is further sub-divided into sub-samples with and without employer contacs and with and without job practise (JP) (Swit sample only). EL2, EL3 and VD are the self-reported level of education and vocational disability with the definition given in Table 2. Weeks is total number of weeks in program, including number of weeks in job practice, (W JP). Sthlm means that an indivdual is registered in Stockholm.

Variable	EL 2	EL 3	MEN	VD	CITIZEN	Weeks	JP	W JP	Sthlm	Y
					Swit ($n = 794$)					
Mean	63.48	31.61	59.32	4.03	5.78	27.51	69.55	5.83	29.70	64.30
se	1.71	1.65	1.74	0.69	0.85	0.32	1.67	0.16	1.66	1.74
					Job practise ($n = 530$)					
Mean	66.42	29.25	60.94	4.52	5.09	28.96	100	5.83	23.58	66.60
se	2.05	1.98	2.12	0.9	0.96	0.34	—	0.16	1.85	2.05
					No job practise ($n = 232$)					
Mean	59.05	35.78	56.47	2.59	7.36	24.20	0.00	—	43.72	59.31
se	3.24	3.15	3.26	1.04	1.72	0.68	—	—	3.27	3.24
t-ratio[3]	1.96	-1.79	1.30	1.39	-1.15	5.77	—	—	-5.36	1.90
					Employer contacts ($n = 586$)					
Mean	66.55	29.35	59.73	4.26	5.12	28.31	90.44	5.83	24.74	67.24
se	1.95	1.88	2.03	0.08	0.91	0.35	1.22	0.16	1.78	1.94
					No employer contacts ($n = 175$)					
Mean	56.00	37.71	58.86	2.86	8.00	24.85	0.00	—	46.29	54.86
se	3.76	3.67	3.73	1.26	2.06	0.75	—	—	3.78	3.77
t-ratio[2]	2.49	-2.03	0.20	0.93	-1.28	3.49	—	—	-5.15	2.92
					AMVc ($n = 796$)					
Mean	67.34	24.75	65.45	14.95	59.80	32.56	52.04	6.49	17.39	52.45
se	1.66	1.53	1.69	1.26	0.087	0.58	1.84	0.24	1.40	1.84
t-ratio[1]	-1.39	2.64	-2.05	-6.95	-0.16	-7.97	7.08	-32.80	5.68	4.72
					Employer contacts = job practice ($n = 383$)					
Mean	69.71	23.76	65.8	12.53	6.27	35.76	100.00	6.50	16.19	61.36
se	2.35	2.18	2.43	1.69	1.24	0.83	—	2.39	1.88	2.49
t-ratio[4]	-1.03	1.94	-1.02	-4.38	-0.74	-8.48	-7.86	-4.12	3.30	1.86
					No employer contacts = No job practice ($n = 353$)					
Mean	65.16	26.63	63.46	15.86	5.67	29.09	0.00	—	18.70	42.78
se	2.54	2.36	2.57	1.95	1.23	0.76	—	—	2.08	2.64
t-ratio[2]	1.32	-0.89	0.66	-1.29	0.34	5.88	—	—	-0.89	5.12

notes : [1]Swit against AMVc, [2]EC against not EC within AMVc or Swit, [3]JP against not JP within Swit, [4]Swit against AMVc given EC

Table 8: Parameter estimates (coefficients and t-ratios) from linear probabilily regressions of employment conditional on employer contact or not. Standard errors (se) are calculates using a White heteroskedastic consistent covariance estimator and t = est./se.

	est.	t	est.	t	est.	t	est.	t
	Employer contacts				No employer contacts			
Swit	5.88	1.88	3.26	0.98	12.10	2.63	6.03	1.16
EL 2			-5.16	-0.74			23.69	2.82
EL 3			-7.23	-0.97			19.61	2.16
MEN			-3.59	-1.13			-4.65	-1.04
VD			-20.39	-3.30			-18.25	-2.59
CITIZEN			-6.40	-0.95			-6.47	-0.75
Regional factor			yes				yes	
R^2 and model p-value	0.36	0.06	6.14	0.00	1.30	0.01	11.26	0.00

Table 9: Parameter estimates (coefficients and t-ratios) from linear probability regressions of employment conditional Swit and AMVc. Standard errors (se) are calculated using White heteroskedastic consistent covariance estimator and t = est./se.

	est.	t	est	t	est.	t	est	t
	Swit				AMVc			
EC	12.40	3.01	15.97	3.77	18.60	5.13	18.32	4.91
EL 2			9.47	1.14			3.84	0.55
EL 3			7.37	0.85			1.27	0.17
MEN			-0.63	-1.75			-3.39	-0.89
VD			-7.66	-0.86			-23.73	-4.55
CITIZEN			-10.01	-1.29			-2.71	-0.36
Regional factor			yes				yes	
R^2 and model p-value	1.18	0.00	8.31	0.00	3.46	0.00	11.49	0.00

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