Evaluating Programs for Social Assistance Recipients: Controlling for Confounding Using Survey and Register Data

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PRELIMINARY WORK - DO NOT QUOTE!

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Abstract

In labor market evaluation studies, causal analyses build on the assumption of no unmeasured confounding (selection on observables). The question is then whether observable information in rich administrative register data manages to cover enough aspects of individual characteristics that affect selection into a labor market program as well as post-program social assistance and labor market outcomes. In this paper we combine population-based administrative registers with survey data containing normally unobserved information on self-reported health and perceived labor market prospects to investigate to what extent the survey data contributes when controlling for confounding for estimation of labor market program effects. In a first step we test, using an algorithm building on the work by Robins (1997), if the register variables constitute a sufficient subset of the union set of register and survey variables. We find that this is the case, making the survey variables redundant. In a second step, we construct a covariate selection algorithm to select sufficient subsets from the full pool of register variables by sequentially performing variable selection using conditional independence properties for sufficient subsets. Using data from Jobcenters in the city of Stockholm targeted at recipients of social assistance, we estimate the effect of internship on future social assistance receipt and labor earnings. Our results show that internship causally reduces the amount of social assistance received up to two years after participating in the program. The effects on earnings are less clear.

Keywords: Social assistance, Activation programs, Combining survey and register data, Matching estimator, Sufficient set

1 Introduction

In Sweden, as well as in the other Nordic countries, administrative registers with individual level data constitute a high-quality observational data resource for researchers in a wide range of disciplines. In medical sciences data from population-based hospital records linked to socio-economic registers is used to both support and generate new hypotheses. In the social sciences data from linked registers is often the only reliable source when studying individuals' economic and social positions and life trajectories in society with respect to, e.g., education, the job market and welfare systems.

Register studies commonly involve drawing causal conclusions about investigated relationships. There have been a rapid development in the statistical and economtric literature in the research field of causal inference in observational studies building on the potential outcome framework (Rubin, 1974). Here, a causal effect of a treatment is defined as a contrast (e.g. the difference) between outcomes that would be observed if the treatment were taken or not taken. Estimation of causal effects under an assumption of *no unmeasured confounding*, also called selection on observables, means that if all paths of spurious association between the treatment/program/exposure variable and the outcome are adjusted for in the analysis, researchers will be able to estimate *causal effects* of the treatments. Rosenbaum and Rubin (1983) show that it is sufficient to control for the probability of being treated given the confounders, the propensity score. However, a set of confounders sufficient to condition on is not unique, there may be different subsets of confounders blocking all confounding paths between the treatment and the outcome and hence different propensity scores could be used in the analysis. In the sequel we refer to such a set as a *sufficient set* of covariates. There have been important developments and insights concerning covariate selection for sufficient subsets of covariates. To evaluate the sufficiency of a covariate set a directed acyclic graph (DAG) can be assumed and the d-separation criterion can be applied (Pearl, 2009). However using graphcial critera requires that the causal relations between the observed variables need to be specified (see also VanderWeele and Shpitser, 2011). General conditional independence properties that imply sufficiency for subsets was described by Robins (1997) and illustrated in Greenland et al. (1999) in a DAG setting. In this paper we implement the conditional independence properties by Robins in tests described through an algorithm that returns an answer to whether a pre-specified subset is sufficient or not.

Even the extensive register data bases available in the Nordic countries may fail to include all important confounders, making the selection on observable assumptions less credible. In the case of training programs, one variable that might be important is the participant's job readiness, which arguable can affect whether a case worker place him or her in a program (the selection can be both positive or negative) as well as the success of the program. Given that registers often include past employment histories and unemployment spells, it is however still possible that things like job readiness is indeed captured by register data. In this paper, we combine register data with survey data directed at potential program participants, and are thus able to investigate to what extent administrative registers "do the job" in controlling for confounders, or whether survey data is needed. We here add to the work by Caliendo et al. (2014) who investigate the importance of usually not observed variables like personality traits, job search behavior and socio-cultural characteristics when estimating effects of short- and long-term training as well as wage subsidies in the German setting. Relying on survey data from the IZA Evaluation dataset, they find that although these variables typically turn out statistically significant in the propensity score estimations and also improve the matching quality, the estimated effects do not change much as long as they also condition on individuals' labor market history. The group we study in this paper (recipients of social assistance) typically lack previous labor market experience, making it impossible to condition on the same set of confounders as Caliendo et al. (2014). In contrast to Caliendo et al. (2014), comparing different treatment effect estimates between models based on different covariate sets, our approach builds on selecting sufficient subsets of covariates using an algorithm building on the work by Robins (1997). The algorithm tests if the register variables is a sufficent set by partitioning the remaining survey variables in all possible sets of two and then sequentially tests conditional independence for the partitions. We also comment on the work by Caliendo et al. (2014) by showing that obtaining the same treatment effect estimate when using a covariate subset does not imply that a subset of the covariates is sufficient and therefore cannot be used as a valid argument for covariate selection.

The specific application that we focus on is the Jobcenters in the city of Stockholm that provide recipients of social assistance with ALMP. More specifically, we estimate the effect of internship on the participants' future welfare participation and labor earnings. The Stockholm Job centers fit well into the trend during the last decades of imposing strict activation programs for unemployed recipients of social assistance.[1] We know fairly little what effects these programs have had on the participants' future welfare and labor market outcomes, and we know even less about which type of program that works. This paper therefore also adds to the existing literature on the effect of active labor market programs for recipients of social assistance.[2]

The rest of the paper is organized as follows: In the next section follows

[1] Mandadory activaiton was, for example, an important part of the large US Welfare Reform in 1996, and has also been an important element in , e.g., *New Deal/Flexible New Deal/Work Programme* in the UK, the "Hartz" reforms in Germany, *The Youth Employment Act* in The Netherlands, different activation programs in Norway and Denmark, and in municipal activation programs in Sweden.

[2] By now, there is a relative extensive literature on the effects of different types of active labor market programs (ALMP) for unemployed, see, e.g. Kluve (2006); Card et al (2010); Forslund and Vikström (2011) for surveys. The main message from this literature is that things that seem to work are i) job search assistance and ii) subsidized employment. Also, the more similar to a real job an activity is, the better. Individuals receiving social assistance typically have a much weaker labor market attachment than those traditionally targeted by ALMP.The bulk of unemployed social assistance recipients are made up by either young people or immigrants, where both groups lack earlier job market experience from Sweden. They are therefore not entitled to unemployment insurance benefits, but need to turn to the welfare offices for financial support. Many of them have low education, it is not uncommon that they experience different types of social problems, and at least many of the foreign born speak relatively poor Swedish. Hence, it is not obvious that the same programs work for them. In addition, at least in the Swedish context, programs targeted at social assistance recipients are typically provided by the local governments, not by the public employment offices (PES).

a description of the causal model and theory. In specific, we discuss how to estimate treatment effects using observational data and how we select covariates to control for selection into treatment. Thereafter we turn to our empirical application and describe the jobcenters in Stockholm, earlier literature on ALMP for recipients of social assistance, as well as the data used in the paper. Section four presents the results and section five concludes.

2 Model and theory

We adopt the potential outcome framework of Rubin (1974) for defining a causal effect of a binary treatment, T with values $T = 1$ if an individual receives treatment and $T = 0$ if the individual does not receive the treatment. The causal effect of the treatment is defined as the difference between two potential outcomes, $Y(1) - Y(0)$, where $Y(1)$ is the potential outcome under treatment and $Y(0)$ is the potential outcome under no treatment. Since each individual cannot be both treated and not treated at the same time, either $Y(1)$ or $Y(0)$ will be missing. The observed outcome is hence defined by $Y = TY(1) + (1 - T)Y(0)$, implying that the the vector of observed variables are (X, T, Y) from which we want to make inference about a causal effect. We focus on the average treatment effect of the treated, $\text{ATT} = E[Y(1) - Y(0)|T = 1]$, although the (modified) description below also holds for the average treatment effect, $\text{ATE} = E[Y(1) - Y(0)]$. Identification of ATT in an observational study is achieved under assumptions of *no unmeasured confounding* and *overlap* where X is a vector of pre-treatment variables, not affected by T.

Assumption 1. *[No unmeasured confounding]*
$Y(0) \perp\!\!\!\perp T|X$,

and

Assumption 2. *[Overlap]*
$P(T = 1|X) < 1 - \eta$, *for some* $\eta > 0$,

see e.g., Imbens and Wooldridge (2009) for a review of assumptions underlying different estimation approaches in program evaluation studies. Under these assumptions we can identify the average causal effect of the treated with the observed data by marginalizing over the conditional means

$$\begin{aligned}\text{ATT} =& E\left[Y(1) - Y(0) \mid T = 1\right] \\ =& E\left[E(Y(1) - Y(0) \mid X) \mid T = 1\right] \\ =& E\left[E(Y \mid X, T = 1) - E(Y \mid X, T = 0) \mid T = 1\right]\end{aligned} \tag{1}$$

The identification in (1) provides the theroetical justification to estimating the treatment effect of the treated by comparing outcomes for treated and controls conditional on covariates and subsequently taking the average of the differences over the covariate distribution of the treated. A propensity score matching/stratification estimator (see Stuart, 2010 for a review on matching estimators) evaluates the inner expectation in (1) by grouping treated and controls in matched sets formed on a scalar function of the covariates, $e(X) = P(T = 1|X)$, the propensity score (Rosenbaum and Rubin, 1983).

3 Covariate selection and sufficient subsets

The choice of covariates to control for in Assumption 1 should primarily be based on economic theory. However, confounding paths between treatment and outcome can be complex and although there may be theories specifying treatment and outcome equations there might not be theory covering relations between the covariates themselves. Most importantly, there may be several sets, S, such that $Y(0) \perp\!\!\!\perp T \mid S$ hold. This means that for the purpose of identifying the average causal effect of the treated there can be several sets of covariates that could be used in the analysis. We refer to a covariate set that upholds Assumption 1 as a *sufficient* covariate set. In the following we will build on theory of sufficient subsets with the important property that the observed data can be used in the search for such a set. This is the main theoretical justification for determining if a subset of the covariates can be used for the causal analysis.

We now define a sufficent subset as a set $S \subset X$ such that the following assumption hold

Assumption 3. *[No unmeasured confounding given a sufficient subset]*
$Y(0) \perp\!\!\!\perp T|S$.

If in addition the corresponding overlap assumption holds

Assumption 4. *[Overlap]*
$P(T = 1|S) < 1 - \eta$, *for some* $\eta > 0$

then the identification in (1) follows directly when replacing X with S.

Although Assumption 3 includes the potential outcome $Y(0)$, a sufficient subset can be selected with the observed data. We use the following result in the selection of a sufficient set (Robins, 1997).

Theorem 5. *Let* $X = \{W, S\}$. *Assume that* $Y_0 \perp\!\!\!\perp T|X$, *then* $Y_0 \perp\!\!\!\perp T|W$ *if there is a decomposition* $S = S_1, S_2$ *such that* $T \perp\!\!\!\perp S_1|W$ *and* $Y_0 \perp\!\!\!\perp S_2|T, W, Z_1$

We now turn to the argument that if the average causal effect of the treated is the same when conditioning on a subset then the subset is sufficient. Given that Assumption 1 holds, and that the average treatment effect of the treated is

$$E\left[E(Y \mid X, T = 1) - E(Y \mid X, T = 0)|T = 1\right] = c \tag{2}$$

For a candidate subset $S^* \subset X$ assume that

$$E\left[E(Y \mid S^*, T = 1) - E(Y \mid S^*, T = 0)|T = 1\right] = c \tag{3}$$

however, this does not imply that S^* is a sufficient subset. To show that the claim: Assumption 1, 2, and 3 $\implies Y(0) \perp\!\!\!\perp T \mid S^*$ is false, we provide a counterexample.

Example

For two confounders of $X = \{X_1, X_2\}$ of non-degenerate distributions assume that $Y(1) = \alpha_0 + \alpha_1 X_1 + \alpha_2 X_2 + \varepsilon_1$, $Y(0) = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \varepsilon_0$ where $E(\varepsilon_t|X_1, X_2) = 0$, $t = 0, 1$ and $\alpha_j, \beta_j \neq 0$, $j = 0, 1, 2$. We assume that the treatment assignment $T \sim \text{Bernoulli}(e(X_1, X_2))$ where $e(X_1, X_2) = P(T = 1|X_1, X_2) = E(T|X_1, X_2) \neq E(T|X_1)$. Since T is binary we have that $f(y(0)|T, X_1, X_2) = f(y(0)|e(X_1, X_2), X_1, X_2) = f(y(0)|X_1, X_2)$ i.e., (1) holds but $f(y(0)|T = t, X_1 = x_1) \neq f(y(0)|T = t)$. Under the above models we have that

$$
\begin{aligned}
\text{ATT} &= E\left[E(Y \mid X_1, X_2, T = 1) - E(Y \mid X_1, X_2, T = 0)|T = 1\right] \\
&= E\left[E(Y(1) - Y(0) \mid X_1, X_2)|T = 1\right] \\
&= \alpha_0 - \beta_0 + (\alpha_1 - \beta_1)E(X_1|T = 1) + (\alpha_2 - \beta_2)E(X_2|T = 1)
\end{aligned}
$$

Let $S^* = X_1$, then

$$
\begin{aligned}
&E\left[E(Y \mid S^*, T = 1) - E(Y \mid S^*, T = 0)|T = 1\right] = E\left[E(Y \mid X_1, T = 1) - E(Y \mid X_1, T = 0)|T = 1\right] \\
&= E\left[E(Y(1) \mid X_1, T = 1) - E(Y(0) \mid X_1, T = 0)|T = 1\right] \\
&= \alpha_0 - \beta_0 + (\alpha_1 - \beta_1)E(X_1|T = 1) + \alpha_2 E(X_2|T = 1, X_1) - \beta_2 E\left[E(X_2|X_1, T = 0)|T = 1\right]
\end{aligned}
$$

and (3) is equal to (2) if $(\alpha_2 - \beta_2)E(X_2|T = 1) = \alpha_2 E(X_2|T = 1, X_1) - \beta_2 E\left[E(X_2|X_1, T = 0)|T = 1\right]$ while conditioning on $S^* = X_1$ which is a contradiction.

4 Application: Estimating effects of internship

4.1 Background: The Jobcenters in the city of Stockholm

In Sweden, unemployed workers are typically covered by unemployment insurance benefits. However, in order to qualify for those, individuals must be members of an unemployment insurance fund and have worked at least six months during the year preceding the unemployment spell. This implies that individuals without earlier labor market experiences do not qualify for benefits. If needing financial aid they instead have to turn to the municipalities applying for social assistance, that is, a means tested benefit making up the final safety net. The Social Services Act states that all Swedish and foreign citizens living in Sweden have the right to apply for social assistance in the absence of other means of economic support. The benefit level should ensure a reasonable standard of living, and depend on the number and age of household members. The municipalities are free to set the exact level as long as it exceeds the minimum level set by The National Board of Health and Welfare. As opposed to the situation in many other countries (e.g., the US and UK), receiving social assistance is not dependent on having children. However, in order to be eligible for benefits, all other means, including savings and valuable assets, must be exhausted. Furthermore, the means testing is performed at the household level.

The economic recession of the 1990s and the accompanying rise in unemployment led to financial distress for many municipalities that experienced increased costs for social assistance, as well as diminishing tax revenues. At the same time, many municipalities expressed the view that the PES were not doing enough for job-seekers that were not entitled to benefits from the unemployment insurance. As a consequence, the municipalities started to build up their own active labor market programs and conditioned receiving social assistance on participation in these programs. [3]

In the city of Stockholm, all individuals applying for social assistance for which unemployment is main reason for income support, are sent to a Jobcenter.[4] Figure 1 illustrates what happens when a client enters into a

[3]The right to require participation in activation programs was formally introduced by a change in the Social Services Act in 1998. The new law made it possible for municipalities and city districts to demand participation in work-related activities, such as internships and supervised job searches, in return for social assistance for individuals below 25 as well as for older recipients if there were special needs. In 2013, the age-limit was abandoned.

[4]The Jobcenters that we analyze were introduced in 2008. Already before 2008, there existed a number of local Jobcenters within the city, but in 2008 the organization was streamlined, implying that the many local Jobcenters were merged into five big centers which all worked in a similar fashion. Also, individuals further away from the labor market that formerly had been exempted from activation were now sent to the Jobcenters.

Jobcenter. After an introductory period of approximately four weeks the participants are placed into different type of ALMP, including internship, language training, programs at the PES, or other activities. Some participants also stay at the Jobcenters performing "basic activities" such as own job search and meetings with coaches.

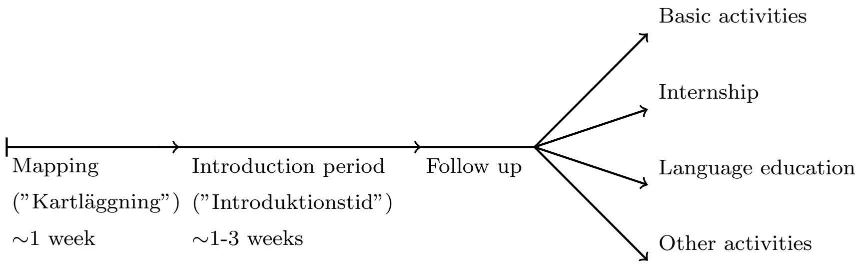


Figure 1: Illustration of activities at Jobcenters

The activities at the Jobcenters can be grouped as follows: Basic activities (including introduction period; the clients mainly search for job by themselves); internship; Swedish as a second language (SFI) and other language courses; other activities at the Jobcenters; activities at the public employment office (PES).

In this paper, we will compare those in internship with those in basic activities. We would expect internship and the work experience that follows to increase the employability of the individuals since these activities can increase labor market skills, promote work habits and provide general encouragement for the individuals. Whether the effects of internship has a more positive effect on employment than the basic activities at *Jobbtorg* remains to be seen.

4.2 Previous literature

The previous literature evaluation activation programs for recipients of social assistance can be divided into two strands; first papers examining activation in a broad sense, and second papers that focus on different types of activation programs. Among the papers in the first strand are Dahl (2003) on activation programs in Norway; Rosholm and Svarer (2009) on early meetings and activations in Denmark; Persson and Vikman (2014) on activation programs in Stockholm and Markussen and Røed (2016) on Qualification Programs in Norway. Our paper falls mainly in the second strand. Given the large emphasis on mandatory participation in ALMP for recipients of social assistance the empirical evidence of the effects of different type of programs is surprisingly scarce. Below we summarize the existing literature.

Cockx and Ridder (2001) evaluate the Belgian Social Employment (BSE) program using a grouping/IV estimator i order to handle selection into programs. The BSE-program is a public sector work experience program where the welfare offices employ unemployed welfare recipients in order to provide community services, such as care, meal provision and cleaning, to households and institutions. The participants earn the minimum wage during the program and are entitled to unemployment insurance benefits after taking part in the program. Cockx and Ridder find that the program actually increases participants' welfare duration.

There are several studies from Denmark, which are relevant for us, especially since there are several similarities between the way ALMP for recipients of social assistance is organized in Sweden and Denmark. Two of them, Clausen et al. (2009) and Heinesen et al. (2013) focus specially at non-western immigrants, whereas Bolvig et al. (2003) do not separate between natives and immigrants.

Clausen et al. (2009) investigate the effects of active labor market programs and language courses supplied by Danish municipalities for newly arrived immigrants (refugees or family reunified). Besides the "classical" background characteristics they also have data over number of visits to the doctor, whether the immigrant lives in a socially deprived household and initial proficiency in Danish and progression in proficiency. Besides language training, immigrants receiving social assistance are also offered a number of different ALMP (employment with a wage subsidy in a private firm, direct employment programs in the public sector, education and training, mixed special programs, counseling an upgrading, and special employment programs in the private sector). Conducting a timing of events duration model along the lines suggested by Abbring and Van Den Berg (2003), they find lock in effects of ALMP, especially if combined with language training. However, the lock-in effects are not present for private sector wage subsidies. Also, private sector wage subsidies shorten the time to employment by 14-24 weeks, whereas mixed special programs and counselling and upgrading lengthen the duration until employment. In addition, their results indicate that language skills are important; improvements in language proficiency have positive effects on the time to employment for those taking part in language programs.

Heinesen et al. (2013) do not focus on newly arrived immigrants, but instead on all non-western immigrants Denmark, receiving social assistance. Many of them have been for a long time in Denmark (more than 60 percent for more than 5 years, and 37 percent more than 10 years) but still have very weak labor market attachment; less than 18 percent of the females and less than 30 percent of the males had at least one year of working experience in Denmark. Applying the same empirical strategy as Clausen et al. (2009) but with a follow up period of five years instead of four, Heinesen et al find positive post-program effects of all type of programs, including subsidized employment in private firms, direct employment programs in the public sector and other programs including education, training and counselling, where the effects are largest for subsidized employment programs. Somewhat surprisingly, they also find positive in-program effect, i.e. no lock-in effects. The effects are largest for those entering into programs after six month of unemployment.

Bolvig et al. (2003) compare employment programs (subsidized employment in a private or public firm or in a municipal employment project) with training programs targeted at recipients of social assistance in the Danish municipality of Aarhus 1997-1999. Estimating a competing risk model for duration until program participation they find considerably lock-in effects as well as negative post-program effects of training whereas the employment programs have positive post-program effects.

Another Nordic country with similar labor market conditions as Sweden is Finland. Sarvimäki and Hämäläinen (2016) examine how integration plans aimed at non-working immigrants that had been in Finland for less than 3 years affected their earnings and uptake of social assistance. The idea with the plan was that the mix of ALMP would be more targeted to the specific individual. This individual targeting resulted in an increased focus on language training, whereas the length of the training period or the resources spent on ALMP did not change. The effects of the plan turned out to be quite positive; earnings increased with 47 percent and social assistance benefits decreased with 13 percent over a follow-up period of ten years.

Summarizing the above evidence, it seems like much of the findings from the literature evaluation traditional ALMP are true also for the programs targeted at recipients of social assistance; wage subsidies in private firms are most promising when it comes to helping people find employment. The same message comes through in a meta-analysis conducted by Butschek and Walter (2014) comparing training, job search assistance, wage subsidies and subsidised public sector employment targeted at immigrants in Europe; only wage subsidies have positive employment effects. The experiences from the Finnish integration plans on the other hand, as well as evidence in Clausen et al. (2009) also point at the importance of acquiring language skills.

4.3 Data

A main purpose of this paper is to investigate to what extent data collected via surveys makes the assumption of no unmeasured confounding more likely to hold. We therefore combine register data from Statistics Sweden and the city of Stockholm with survey data collected directly at the Jobcenters at the city of Stockholm. Below, we discuss these data sets in turn

4.3.1 Register data

The register data on the Jobcenter-participants comes from registers at the city of Stockholm and at Statistics Sweden. Background characteristics, such as region of origin, as well as spell data over activities at the Jobcenter are available in the *FLAI* register that covers the years 2008–2014 whereas information on monthly payments of social assistance to households is taken from the *Paraplyet* register that covers the years 2009–2014. To these two registers from the city of Stockholm we merge information from Statistics Sweden over, e.g., the individuals' educational level, age, number of kids (LISA) and when the individuals' immigrated to Sweden (InUt), from which we can calculate time in Sweden for those born outside Sweden.[5]

4.3.2 Survey data

The survey data was collected on site at three out of then five Jobcenters in the spring of 2010.[6] We randomly selected 500 clients that were registered at each of the three Jobcenters at a certain date (this information was taken from *FLAI*)[7] These 1,500 clients were called to a meeting at the Jobcenter [8] to answer a written questionnaire. The questionnaire was translated into several languages and, at some of the meetings with the clients, interpreters were present. 681 individuals completed the survey, amounting to a response rate of 45 percent. The final sample includes 581 individuals that answered the survey questions we use in the analysis, appeared in the register data and did not work during the survey month.

The first column in Table 1 shows the total pool of clients that could have been randomly selected to take part in the survey and how we end up with the final survey sample. The second column in Table 1 shows how

[5]These registers are described in more detail in the Appendix.

[6]The three Jobcenters are Kista, Farsta and Vällingby. The survey was carried out by Matz Dahlberg, Eva Mörk and Katarina Hjertner Thorén. A report in Swedish, Dahlberg et al. (2013), provides a descriptive analysis on the collected data.

[7]We excluded clients that had been registered less than 10 days or were recently arrived immigrants.

[8]The survey in Kista was not taken place at the Jobcenter, but instead at Kista Träff, a conference venue in downtown Kista.

	(1)	(2)
	count	count
Pool of possible survey participants	3543	3543
Called to survey	1498[a]	
In register	1479	3542
Turned up	680	
Respondents	600	
Do not work	581	3368

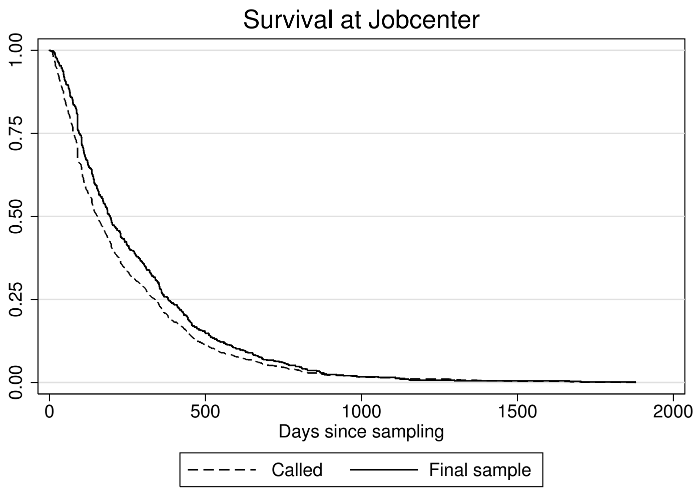
[a] Two individuals were called twice since they had changed jobcenter.

Table 1: Flow table, sample selection

many out of the total pool of clients that could have been randomly selected to take part in the survey that appeared in the registers and that did not work during the month in which the survey took place.

A first question is to what extent our final survey sample is representative for those registered at the Jobcenter. Table 8 in the Appendix shows summary statistics for those 1479 originally called to the Jobcenter and in registers and the 581 individuals in our final sample. Looking at the table it seems like women, those born outside Sweden and married individuals are somewhat misrepresented in our final sample. Also, individuals in the final sample are somewhat older, have been on social assistance for more months and have been a shorter time in Sweden (those born outside Sweden).

Figure 2 instead compare how long individuals in the two groups stay at the Jobcenter. Note that there are several reasons for being deregistered at the Jobcenter; you could find a job or for some other reason no longer receive social assistance, but you could also be referred back to the social office if the personnel at the Jobcenter assess that you are too far away from the labor market. From the figure it seems like those in our final sample remain at the Jobcenter for a longer period. One understanding of this is that the non-respondents were already more involved in different activities than the respondents at the time of the survey and found it harder to come to answer the survey.[9]



Note: Time is measured since sampling.

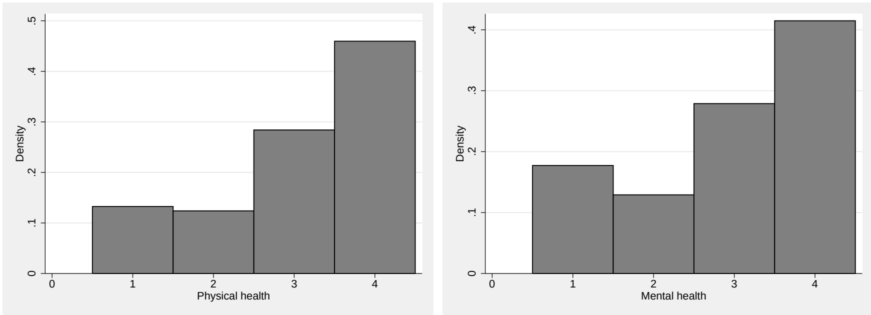
Figure 2: Time at the Jobcenter for called and final sample

With the survey we wanted to capture things that are observed by the caseworker that decides on the treatment but are not typically captured in registers. We here focus on self-rated health status (both physical and mental), the predicted probability of finding a job in the near future, and the perceived reason for being unemployed.

Regarding their health, the clients were asked to describe their physical and mental health on a discrete four-point scale; Very poor, Poor, Good, or Very good. The distributions of the self-rated health variables are presented in Figure 3. The results for subjective physical health (presented in the diagram in the left part of the figure) show that over 70 percent of the respondents consider themselves to be in a very good or good physical shape. From the diagram in the right part of Figure 3, it is also clear that the majority of the responding clients (approximately 70 percent) also think that they have a very good or good mental health.

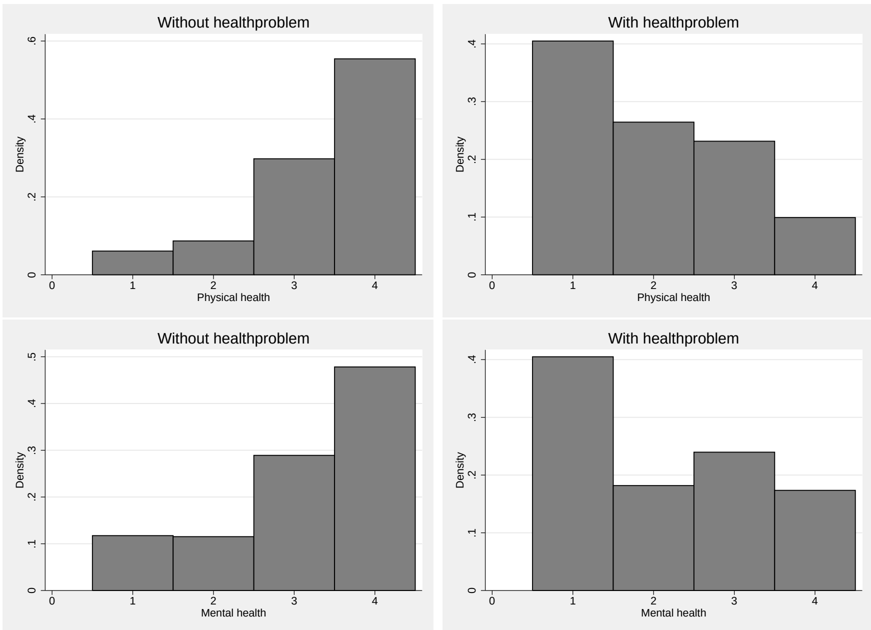
For their labor market situation, the clients were asked to rate, on a

[9]The invitation letter to the survey clearly stated that the participants were allowed to miss their ordinary activity in order to attend the survey. Certificates of presence were also distributed to those who required such.



Note: 1 = Very poor, 2 = Poor, 3 = Good, and 4 = Very good

Figure 3: Distributions of self-rated health



Note: 1 = Very poor, 2 = Poor, 3 = Good, and 4 = Very good

Figure 4: Physical and mental health by saying that health is a problem for work

discrete four-point scale, how likely they thought they were to find a job in the near future; Very unlikely, Unlikely, Likely, or Very likely. They were also asked to state, from different given alternatives, why they considered themselves unemployed (e.g., because they do not speak Swedish very well). Table 3 shows the self-rated job probability for each of five different obstacles for employment. The first column (Total) shows the percentage of the respondents that reported each obstacle (since the respondents could report more than one obstacle, the rows do not need to sum to 100). The next four columns shows how these responses are distributed over different self-rated job probability (the four columns in each row sum to the total). Overall, the respondents are not very optimistic about their job-finding probabilities; approximately 70 percent of the responding clients think they are very unlikely or unlikely to find a job in the near future. Also, language problems seems to be the most common obstacle for employment (31 percent of the respondents reported this as one of the obstacles for employment), followed by health problems and lack of adequate eduction (21 percent). One can also note that those that have most pessimistic over the chances of getting a job are those stating the latter two reasons for unemployment.

The first two columns in Table 3 show which activities the clients responding to the survey participated in when the survey was conducted; the first of the two columns presents the number of clients that *only* have a specific activity and the second of the two columns presents the number of clients that have a specific activity *in combination* with some other activity (so, e.g., 66 clients take part solely in internship and 94 in combination with some other activity). The last two columns in Table 3 show the same statistics for the clients in the total pool of potential candidates fulfilling the two criteria of being eligible to the survey and did not work in the survey month (c.f. the 3,368 in Table 1).

Reported obstacles	Total	(1) Very low	(2) Rather low	(3) Rather high	(4) Very high
		Which have reported	Self-rated job	probability as	
Language problems	0.31	0.44	0.30	0.23	0.03
Health problems	0.21	0.54	0.28	0.12	0.06
Not enough work experience	0.18	0.41	0.35	0.20	0.05
Problems social situation	0.06	0.41	0.31	0.22	0.06
Not right education	0.21	0.49	0.33	0.16	0.02
Observations	581	220	191	128	42

Table 2: Reported obstacle for employment by self-rated job probability.

	Survey sample		Large sample	
Activities	Only	In combinations	Only	In combinations
Basic	236		1409	
Internship	66	94	362	527
SFI	52	144	373	882
Language	18	70	59	334
Other	43	72	187	313
PES	55	85	378	550

Table 3: Number of participants

4.3.3 Empirical specification

In this paper we will estimate the effect of participating in internship compared to only participating in the basic activities at the Jobcenter. The outcomes that we will study are the probability of receiving social assistance in 2011 or 2012, the sum of social assistance as well as earnings in 2011 and 2012.[10]

Of the 581 individuals who answered the questions and that we use in the analysis, there are 94 that during the survey month participated in internship (constituting the treatment group) and 236 that only had basic activities (constituting the control group; c.f. the first two columns in Table 3). For the larger pool of potential candidates (c.f. the first two columns in Table 3), 527 individuals are in the treatment group (Internship) and 1,409 are in the control group (Basic activities). Descriptives of the treatment and control groups are shown in Table 4. The corresponding descriptives, for Table 4, for the large population is shown in the Appendix.

Table 4: (4) Descriptives, mean values for treated (Internship) and control (Basic) group.

	(1) Internship		(2) Basic	
	mean	sd	mean	sd
Socio-demographic				
Woman	0.45	0.50	0.50	0.50
Age in 2010	39.86	11.99	40.76	11.80
From Sweden	0.23	0.43	0.25	0.43
From Africa	0.29	0.45	0.19	0.39
From Asia	0.34	0.48	0.39	0.49
From rest of the world	0.14	0.35	0.17	0.38

Continued on next page

[10]When estimating the propensity scores, we will use logs (we add 1 to zeros) of earnings and social assistance, whereas we will estimate the effects in 100 SEK.

	(1)		(2)	
	Internship		Basic	
Continued from last page	mean	sd	mean	sd
Compulsory school	0.45	0.50	0.38	0.49
High school	0.32	0.47	0.34	0.48
University	0.23	0.43	0.27	0.44
No children	0.55	0.50	0.52	0.50
1-2 children	0.29	0.45	0.35	0.48
3 or more children	0.16	0.37	0.14	0.34
Married	0.48	0.50	0.41	0.49
1 year in Sweden	0.01	0.10	0.05	0.21
2 years in Sweden	0.11	0.31	0.02	0.14
3 years in Sweden	0.07	0.26	0.03	0.18
4 years in Sweden	0.09	0.28	0.05	0.22
5 or more years in Sweden	0.72	0.45	0.85	0.36
Information from Jobcenter				
JT Vallingby	0.46	0.50	0.39	0.49
JT Kista	0.21	0.41	0.27	0.45
JT Farsta	0.33	0.47	0.34	0.47
Time at JT when surveyed	410.65	249.04	275.01	240.47
Month with SA in Sthlm previous year	8.81	3.44	7.68	3.89
Registered at JT in 2008	0.56	0.50	0.47	0.50
Registered at JT in 2009	0.90	0.30	0.80	0.40
Before Survey activity internships	0.33	0.47	0.22	0.42
SFI Before survey	0.16	0.37	0.05	0.22
Before Survey activity language except sfi	0.10	0.30	0.06	0.23
Before Survey activity other	0.22	0.42	0.24	0.43
Before Survey activity at PES	0.12	0.32	0.11	0.31
Survey questions				
Very low job probability	0.41	0.50	0.33	0.47

Continued on next page

	(1)		(2)	
	Internship		Basic	
Continued from last page	mean	sd	mean	sd
Rather low job probability	0.24	0.43	0.35	0.48
Rather high job probability	0.24	0.43	0.23	0.42
Very high job probability	0.10	0.30	0.10	0.30
Very bad physical health	0.11	0.31	0.11	0.32
Rather bad physical health	0.07	0.26	0.16	0.36
Rather good physical health	0.37	0.49	0.30	0.46
Very good physical health	0.45	0.50	0.43	0.50
Very bad mental health	0.12	0.32	0.18	0.38
Rather bad mental health	0.14	0.35	0.14	0.34
Rather good mental health	0.29	0.45	0.33	0.47
Very god mental health	0.46	0.50	0.36	0.48
Language problems	0.22	0.42	0.19	0.40
Health problems	0.19	0.40	0.19	0.40
Not enough work experience	0.14	0.35	0.20	0.40
Family difficulties or problems social situation	0.02	0.15	0.07	0.26
Not right education	0.13	0.34	0.22	0.42
Previous earnings and SA (1000 SEK				
Earnings in 2005	16.56	45.99	16.49	59.88
Earnings in 2006	19.73	44.18	24.88	71.18
Earnings in 2007	20.26	45.47	26.97	71.45
Earnings in 2008	16.99	39.54	27.27	68.28
Earnings in 2009	7.24	19.52	20.50	62.07
Having SA in 2005	0.45	0.50	0.58	0.49
Having SA in 2006	0.52	0.50	0.61	0.49
Having SA in 2007	0.67	0.47	0.67	0.47
Having SA in 2008	0.84	0.37	0.74	0.44
Having SA in 2009	0.96	0.20	0.88	0.32

Continued on next page

	(1)		(2)	
	Internship		Basic	
Continued from last page	mean	sd	mean	sd
SA in 2005	15.83	24.26	25.38	32.90
SA in 2006	22.64	29.14	28.13	33.68
SA in 2007	29.66	30.14	29.13	33.49
SA in 2008	38.09	31.66	31.04	33.16
SA in 2009	51.09	35.01	40.20	32.31
Observations	94		236	

4.3.4 Selecting covariates

The decision to include/not include the survey variables is based on the result in Theorem 5. We implement the theorem in an algorithm performing all possible partitions of the survey set into sets S_1, S_2 and subsequently perform the tests 1) $T \perp\!\!\!\perp S_1|W$ and 2) $Y_0 \perp\!\!\!\perp S_2|T, W, S_1$. The first test is evaluated through a logistic regression model where the treatment is the dependent variable and S_1 and W (the register variables) are the independent variables. The second test $Y_0 \perp\!\!\!\perp S_2|T, W, S_1$ is evaluated similarly by either logistic regression or linear regression depending on if the response is binary or continuous.

After the evaluation of the survey variables we use a search algorithm (Algorithm B in de Luna et al., 2011) to further reduce the remaining covariate set. If the conclusion is that the survey variables are needed for the analysis, we search for a sufficient subset among the total pool of register and survey data for the 581 clients that answered the survey. If the conclusion is that the survey variables are *not* needed for the analysis, we search for a sufficient subset among the total pool of register data for the 3,368 clients that were potential candidates for being sampled for answering the survey. The algorithm first selects covariates predicting the outcome for the different treatment groups respectively and, thereafter, from the chosen covariates selects a subset predicting treatment, see also Häggström et al. (2015) for an implementation in the statistical software R R Core Team (2016). We employ a matching estimator, following Abadie and Imbens (2016), for the ATET. Here, each treated individual is matched to four control (with replacement) neighbours based on the estimated propensity score $\hat{e}(X)$. For the implementation we use the package `teffects` in Stata and the function `psmatch`. Individuals that lack overlap in covariates are discarded, see Section 4.3 for further details. For continuous outcomes we use a linear regression model and for the binary outcomes and the propensity score we assume a linear logistic regression model.

All models are selected with lasso (Tibshirani, 1996) which, integrated in the regression analysis, include parameter estimation criteria that performs both variable selection and regularization. The regularization parameter is selected with cross-validation, using the glmnet package and the function cv.glmnet (Friedman et al., 2010).

5 Results

5.1 Descriptive analysis

The purpose of this paper is to investigate whether survey data adds anything to traditional register data when controlling for confounders. This section contains some descriptive analysis that will give a first indication to whether this is the case.

Do the respondents do a god job when predicting their job probabilities? In order to test this we estimate the likelihood that an individual gets a job within six months using only register data.[11] We then plot the predicted job probabilities for each of the four possible responses to the question "How likely is it that you will find a job soon?" (very low, rather low, rather high and very high), see Figure 5. It is clear from the figure that those that

[11]We defined employment as having earnings from employment within 6 months after the survey months and included all variables that is listed in table 4, except the survey questions, as regressors.

responded that it the chance of finding a job soon was very low also have considerably lower predicted likelihood of being employed within six months.

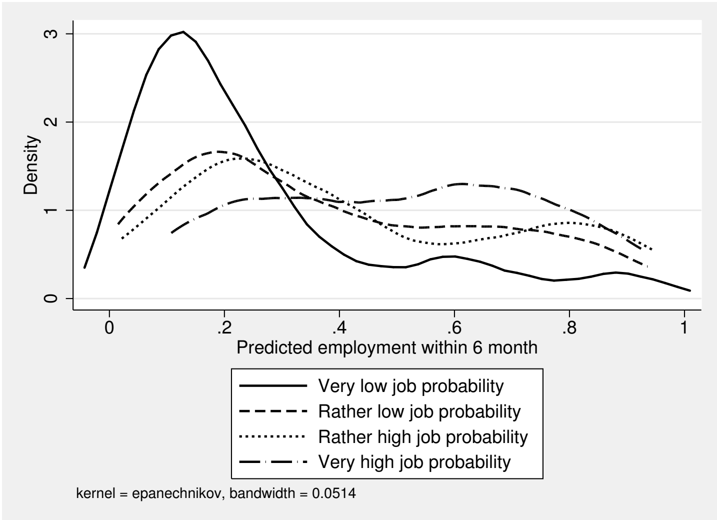


Figure 5: Predicted job probabilities (using register data) and self-rated job probability

Next we also add variables from survey data when predicting whether an individual was employed within six months. Figure 6 shows the resulting distributions when i) only using register data, ii) adding the self-rated job probability, and iii) also adding the other variables from survey data (health, and obstacle for employment) for each of the responses on the job probability-question. The survey data turns out to be most relevant for predicting the job probability for those that responded that their chance of finding a job soon was very high. The question is then whether these survey variables are important to condition on in the analysis, or if the information it contains is picked up by some of the variables in the registers. This will be determined by applying Theorem 5 on the data.

5.2 Selection of covariates

The selection of covariates hence follows a two-step procedure. In the first step, we have to determine if the survey variables are necessary to condition on. Applying an algorithm that implements Theorem 5, we find that there

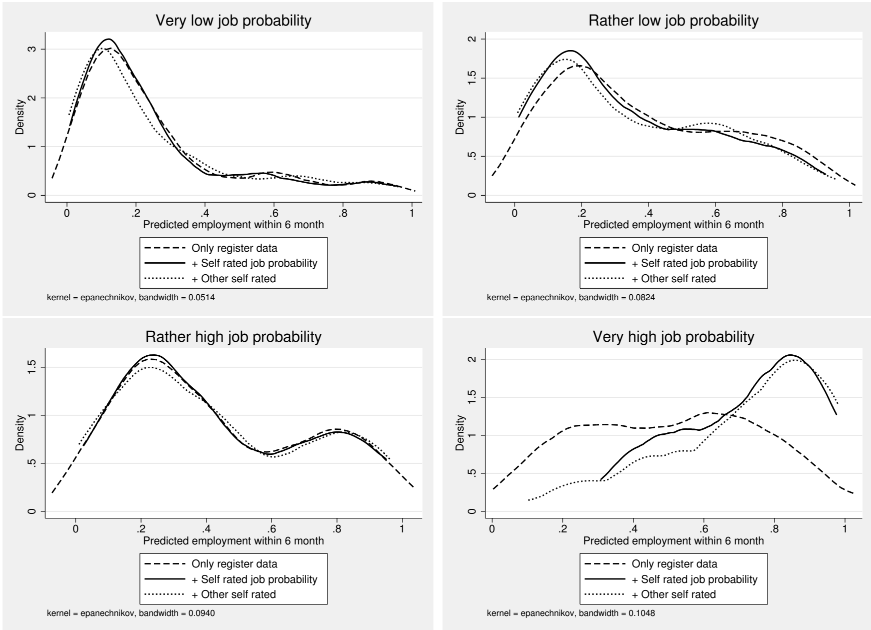


Figure 6: Predicted job probabilities using both register and survey data.

exists at least one partition of the survey variables in which we cannot reject that both 1) $T \perp\!\!\!\perp S_1 | W$ and 2) $Y_0 \perp\!\!\!\perp S_2 | T, W, S_1$ holds (for a full list of the survey variables on which the partitions are conducted, see Table 4). The implications of this is that the register variables constitute a sufficient subset of covariates, and that we do not need to condition on the survey variables.

In the second step, we have to determine which of the variables from the register data to include as potential covariates. A variable should be included if it potentially affect *both* outcome and treatment. The type of variables we include are background characteristics (such as sex, country of origin, education, number of children, marital status, and years in Sweden), labor market and social assistance history (such as time at the Jobcenter when surveyed, measured via registration in 2008 and 2009, labor earnings and social assistance in Sweden 2005–2009, and months of social assistance in the previous twelve months). The full list of register variables that we included as potential covariates are presented in Table 4.

The variables actually chosen for each outcome and year in the covariate selection procedure described in the econometric section are presented in Table 5.

5.3 Overlap and balancing of covariates

Figure 7 shows the overlap. In the analysis we define good overlap as having a neighbour within 0.2 times the standard deviation of the propensity score. A visual inspection indicates that we do not have a problem with poor overlap. From Table 6 it is also clear that there are very few observations that have no overlap according to this definition. The observations without overlap isn't outliers in any separate observable characteristics. Even if some observations are removed from the analysis, we still match on the original estimated propensity score using teffects nnmatch in Stata.

Figure 8 shows the standardized bias before and after matching. A first thing to note is that, even without matching, the standardized biases are relatively low (typically below 0.3 and always below 0.4). Also, matching reduces them considerably. Our reading is that the matching procedure has worked well.

5.4 Average treatment effects

The estimated average treatment effects on the treated (ATET) are presented in Table 7. Starting with the effects on social assistance take up, we note that being on internship in 2010 significantly reduces the probability of receiving any social assistance in 2011 and 2012; the statistical significance level is somewhat lower in 2012, but the point estimates in the two years are fairly similar (internship reduces the probability of receiving social assistance by approximately 6.3 percent in 2011 and by approximately 5.6 percent in 2012).

Being on internship in 2010 also significantly (at the one percent significance level) reduces the sum of social assistance received in 2011 and 2012; in 2011 the sum is reduced by 5,234 SEK, and in 2012 the sum is reduced by 6,086 SEK.

	(1) P(SA 2011)	(2) P(SA 2012)	(3) log(SA 2011)	(4) log(SA 2012)	(5) log(wage 2011)	(6) log(wage 2012)
Internship						
Age in 2010	-0.0145***	-0.0156***	-0.0138***	-0.0143***	-0.0185***	-0.0141***
	(0.00516)	(0.00517)	(0.00522)	(0.00525)	(0.00543)	(0.00524)
Month with SA in Sthlm previous year	0.0145					
	(0.0201)					
Log of SA in 2008	0.0663	0.0440	0.0926**	0.0407	0.0482	0.0777*
	(0.0422)	(0.0447)	(0.0431)	(0.0449)	(0.0456)	(0.0431)
Log of SA in 2009	0.113*	0.0539	0.0421	0.0741	0.0404	0.0658
	(0.0614)	(0.0579)	(0.0583)	(0.0570)	(0.0579)	(0.0572)
Log of earnings in 2009	-0.0746*	-0.0910**	-0.0894**	-0.0869**	-0.0964**	-0.0803**
	(0.0388)	(0.0386)	(0.0385)	(0.0384)	(0.0402)	(0.0384)
From Africa	0.153	0.105	0.126	0.106		
	(0.124)	(0.124)	(0.124)	(0.125)		
From rest of the world	-0.264	-0.243	-0.222	-0.228	-0.238	-0.276*
	(0.164)	(0.165)	(0.165)	(0.165)	(0.163)	(0.162)
5 or more years in Sweden	0.0242		-0.147	-0.0320	-0.0552	0.0131
	(0.166)		(0.184)	(0.159)	(0.173)	(0.169)
1 year in Sweden	0.343		0.245			0.310
	(0.314)		(0.321)			(0.313)
University	-0.130		-0.168	-0.148	-0.166	-0.153
	(0.141)		(0.142)	(0.142)	(0.142)	(0.141)
Before Survey activity internships	0.366***	0.283**	0.326***	0.274**	0.170	0.280**
	(0.122)	(0.123)	(0.121)	(0.123)	(0.131)	(0.122)
Before Survey activity language except sfi	0.496***	0.388**		0.415**	0.372**	0.411**
	(0.175)	(0.173)		(0.176)	(0.180)	(0.177)
Having SA in 2005	-0.372***					
	(0.133)					
Log of SA in 2005		-0.0893***	-0.107***	-0.103***	-0.0891**	-0.106***
		(0.0326)	(0.0375)	(0.0372)	(0.0377)	(0.0374)
No children		0.141	0.130			
		(0.115)	(0.115)			
2 years in Sweden		0.429			0.460	
		(0.267)			(0.281)	
Married		0.136	0.198	0.113	0.116	0.130
		(0.121)	(0.122)	(0.119)	(0.121)	(0.118)
Registered at JT in 2008		0.178		0.170	0.0581	
		(0.130)		(0.130)	(0.139)	
Registered at JT in 2009		0.783***	0.856***	0.766***	0.682***	0.788***
		(0.203)	(0.200)	(0.202)	(0.206)	(0.200)
3 years in Sweden			-0.341			
			(0.253)			
Time at JT when measured					0.000828***	
					(0.000303)	
Log of earnings in 2006					0.0562	
					(0.0352)	
1-2 children					-0.173	
					(0.117)	
JT Vallingby					0.132	
					(0.124)	
Before Survey activity at PES					-0.207	
					(0.173)	
Before Survey activity other						0.126
						(0.128)
Constant	-0.994***	-1.489***	-1.405***	-1.406***	-1.241***	-1.458***
	(0.248)	(0.250)	(0.299)	(0.279)	(0.290)	(0.284)
N	1936	1936	1936	1936	1936	1936
r2_p	0.0399	0.0497	0.0471	0.0484	0.0554	0.0481
ll	-1088.2	-1077.1	-1080.0	-1078.6	-1070.7	-1078.9

Bold text and figures are self-rated variables. Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Estimated propensity score for internship using variables chosen by the lasso.

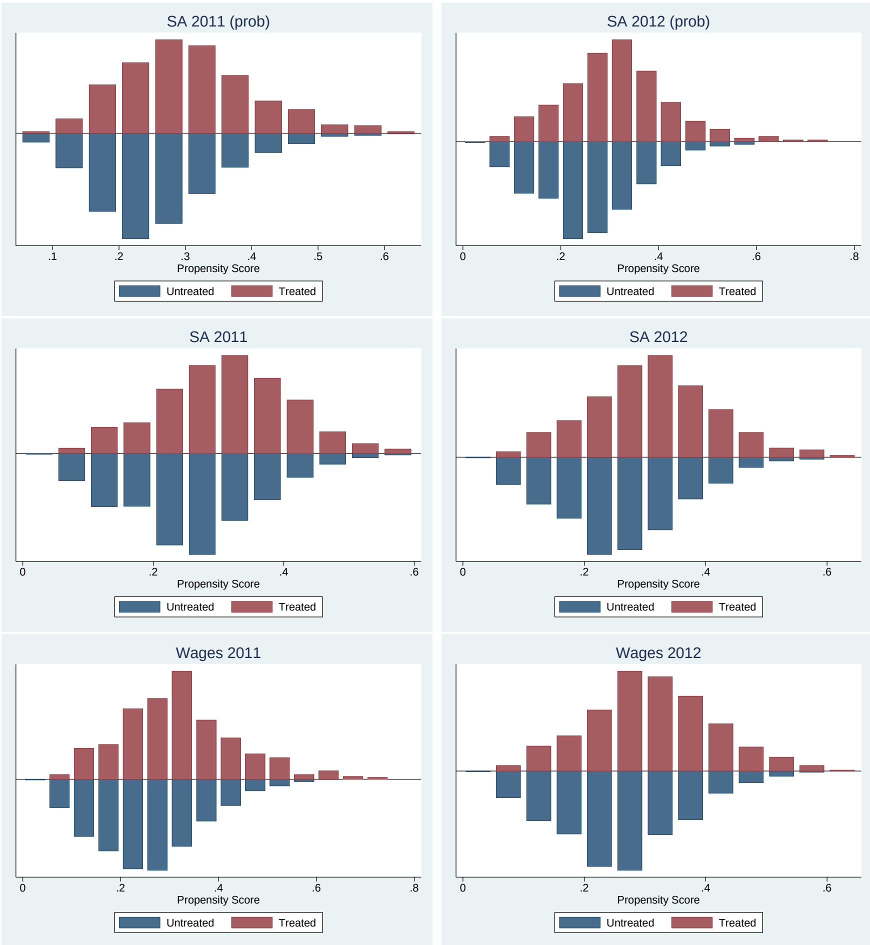


Figure 7: Common support of the propensity score

		Without overlap	
Outcome	$0.2 * sd(PS)$	Control	Treated
P(SA 2011)	.0192	1	0
P(SA 2012)	.0212	2	5
SA 2011	.0205	2	0
SA 2012	.0208	2	2
Wage 2011	.0225	1	5
Wage 2012	.0207	2	0
At least one time		3	7

Table 6: Observations without overlapp within $\pm 0.2 * sd(PS)$

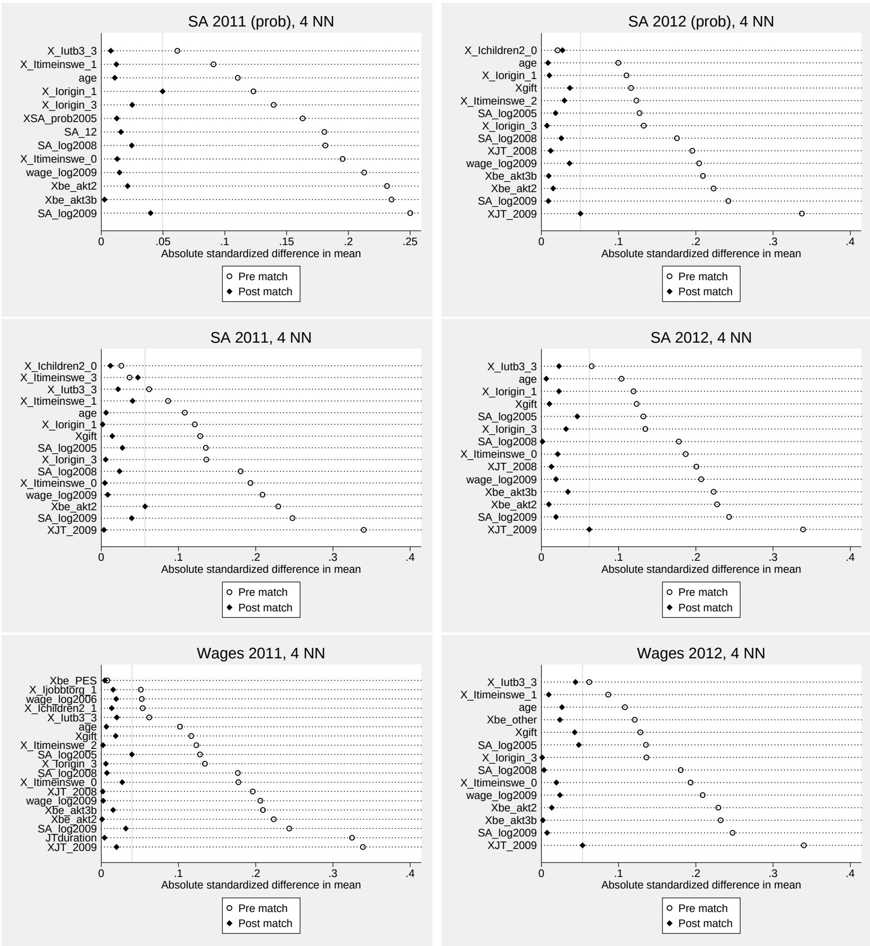


Figure 8: Love plot of absolute standardized difference, before and after matching, with 4 NN

Finally, it can be noted that internship seems to have a positive effect on earnings, even though the effect is not very clear from a statistical point of view; while the positive effect in 2011 of approximately 9,000 SEK is statistically significant at the ten percent level, the point estimate of approximately 8,800 SEK in 2012 is not statistically significant at at least a ten percent level.

	(1)	(2)	(3)
	SA prob	SA sum	Wage sum
ATET			
2011	-0.0635**	-5.234***	9.058*
	(0.0263)	(1.978)	(5.204)
N	1935	1934	1930
ATET			
2012	-0.0559*	-6.086***	8.815
	(0.0294)	(2.041)	(5.630)
N	1929	1932	1934

Abadie-Imbens standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Average treatment effects on the treated, all potentially called

6 Conclusions

To be written.

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Appendix

Table 8: Descriptives, mean values for called and final sample.

	(1) Called		(2) Final sample	
	mean	sd	mean	sd
Socio-demographic				
Woman	0.48	0.50	0.52	0.50
Age in 2010	37.97	11.41	40.51	11.22
Share aged 17-25	0.19	0.39	0.13	0.33
Share aged 26-35	0.25	0.43	0.22	0.41
Share aged 36-45	0.28	0.45	0.30	0.46
Share aged 46-55	0.21	0.41	0.25	0.43
Share aged 56-65	0.08	0.26	0.11	0.31
From Sweden	0.21	0.41	0.17	0.38
From Africa	0.27	0.45	0.25	0.43
From Asia	0.37	0.48	0.43	0.50
From rest of the world	0.14	0.35	0.15	0.36
Compulsory school	0.45	0.50	0.42	0.49
High school	0.33	0.47	0.32	0.47
University	0.21	0.40	0.24	0.43
No children	0.51	0.50	0.49	0.50
1-2 children	0.34	0.47	0.35	0.48
3 or more children	0.15	0.36	0.16	0.37
Married	0.41	0.49	0.48	0.50
Years in Sweden for immigrants	9.51	7.75	8.73	7.15
1 year in Sweden	0.05	0.22	0.06	0.24
2 years in Sweden	0.05	0.22	0.06	0.23
3 years in Sweden	0.08	0.28	0.08	0.27
4 years in Sweden	0.09	0.29	0.09	0.29
5 or more years in Sweden	0.73	0.45	0.71	0.46
Information from Jobcenter				
JT Vallingby	0.34	0.47	0.39	0.49
JT Kista	0.33	0.47	0.28	0.45
JT Farsta	0.33	0.47	0.33	0.47
Time at JT when surveyed	322.96	251.21	358.40	263.50
Month with SA in Sthlm previous year	7.83	3.73	8.36	3.56
Registered at JT in 2008	0.52	0.50	0.57	0.50
Registered at JT in 2009	0.87	0.33	0.87	0.34
Before Survey activity internships	0.24	0.42	0.26	0.44
SFI Before survey	0.12	0.32	0.12	0.32
Before Survey activity language except sfi	0.16	0.37	0.18	0.39

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	(1)		(2)	
	Called		Final sample	
Continued from last page	mean	sd	mean	sd
Before Survey activity other	0.22	0.42	0.25	0.43
Before Survey activity at PES	0.12	0.32	0.13	0.34
Work during survey month	0.03	0.18	0.00	0.00
Previous earnings and SA				
Earnings in 2005	13.02	45.34	13.04	49.33
Earnings in 2006	14.78	45.03	16.55	54.26
Earnings in 2007	18.17	50.12	18.57	56.43
Earnings in 2008	18.08	46.80	17.83	51.93
Earnings in 2009	11.80	35.97	12.30	44.00
Mean earnings 2005-2009, 100 SEK	15.17	36.56	15.66	45.15
Having SA in 2005	0.50	0.50	0.50	0.50
Having SA in 2006	0.59	0.49	0.57	0.50
Having SA in 2007	0.66	0.47	0.67	0.47
Having SA in 2008	0.75	0.43	0.78	0.41
Having SA in 2009	0.92	0.26	0.93	0.25
Years with SA 2005-2009	3.43	1.58	3.45	1.57
SA in 2005	21.11	30.76	21.34	31.05
SA in 2006	24.83	31.18	25.78	32.02
SA in 2007	28.49	31.80	30.19	32.07
SA in 2008	33.89	33.66	35.97	33.75
SA in 2009	43.75	33.96	45.31	32.50
Mean SA 2005-2009, 100 SEK	30.41	25.53	31.72	25.72
Observations	1479		581	

Table 9: Descriptives, mean values for treated (Internship) and control (Basic) group using sample of all possible called to survey.

	(1)		(2)	
	Internship		Basic	
	mean	sd	mean	sd
Socio-demographic				
Woman	0.47	0.50	0.47	0.50
Age in 2010	36.61	11.53	37.87	11.41
From Sweden	0.25	0.44	0.27	0.45
From Africa	0.29	0.45	0.23	0.42
From Asia	0.34	0.47	0.33	0.47
From rest of the world	0.12	0.32	0.16	0.37
Compulsory school	0.45	0.50	0.41	0.49

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	(1)		(2)	
	Internship		Basic	
Continued from last page	mean	sd	mean	sd
High school	0.35	0.48	0.36	0.48
University	0.18	0.38	0.20	0.40
No children	0.54	0.50	0.53	0.50
1-2 children	0.30	0.46	0.33	0.47
3 or more children	0.16	0.37	0.14	0.35
Married	0.39	0.49	0.33	0.47
1 year in Sweden	0.05	0.21	0.03	0.17
2 years in Sweden	0.06	0.24	0.03	0.16
3 years in Sweden	0.07	0.25	0.06	0.23
4 years in Sweden	0.10	0.30	0.07	0.26
5 or more years in Sweden	0.73	0.44	0.81	0.39
Information from Jobcenter				
JT Vallingby	0.26	0.44	0.24	0.42
JT Kista	0.36	0.48	0.36	0.48
JT Farsta	0.37	0.48	0.40	0.49
Time at JT when measured	342.63	243.39	262.98	232.26
Month with SA in Sthlm previous year	7.97	3.63	7.29	3.89
Registered at JT in 2008	0.56	0.50	0.46	0.50
Registered at JT in 2009	0.93	0.26	0.81	0.39
Before Survey activity internships	0.31	0.46	0.21	0.40
SFI Before survey	0.12	0.32	0.09	0.28
Before Survey activity language except sfi	0.15	0.36	0.08	0.26
Before Survey activity other	0.25	0.43	0.20	0.40
Before Survey activity at PES	0.11	0.32	0.11	0.31
Previous earnings and SA (1000 SEK				
Earnings in 2005	14.99	49.06	16.10	47.12
Earnings in 2006	16.26	43.06	19.67	51.34
Earnings in 2007	17.87	44.17	23.17	55.64
Earnings in 2008	14.05	35.20	23.21	54.28
Earnings in 2009	7.99	24.35	14.86	40.57
Having SA in 2005	0.46	0.50	0.55	0.50
Having SA in 2006	0.57	0.50	0.60	0.49
Having SA in 2007	0.65	0.48	0.66	0.48
Having SA in 2008	0.76	0.43	0.71	0.46
Having SA in 2009	0.94	0.24	0.88	0.32
SA in 2005	19.02	28.90	22.58	31.98
SA in 2006	24.18	30.94	25.00	31.61
SA in 2007	28.81	31.68	27.05	32.26
SA in 2008	36.10	34.43	29.80	32.90

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	(1)		(2)	
	Internship		Basic	
Continued from last page	mean	sd	mean	sd
SA in 2009	47.19	34.97	39.59	34.18
Observations	527		1409	