

# Treatment Evaluation with Selective Participation and Ineligibles

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## **Abstract**

Matching methods for treatment evaluation based on a conditional independence assumption do not balance selective unobserved differences between treated and non-treated. We derive a simple correction term if there is an instrument that shifts the treatment probability to zero in specific cases. Policies with eligibility restrictions, where treatment is impossible if some variable exceeds a certain value, provide a natural application. In an empirical analysis, we exploit the age eligibility restriction in the Swedish Youth Practice subsidized work program for young unemployed, where compliance is imperfect among the young. Adjusting the matching estimator for selectivity changes the results towards making subsidized work detrimental in moving individuals into employment.

# 1 Introduction

Matching methods for treatment evaluation compare the outcomes of treated and non-treated subjects, conditioning on observed individual and environment characteristics. The average treatment effect on the treated (ATT) is estimated by averaging observed outcome differences over the treated, with different matching methods using different sets of weights to construct the counterfactual from non-treated observations. The main assumption underlying all different versions of matching is that the conditioning ensures that the assigned treatment status is conditionally mean independent from the potential outcomes. This is usually known as “the Conditional Independence Assumption” or, in short, CIA, although in fact it concerns mean independence.<sup>1</sup>

Matching is intuitive, as it mimics randomized experiments: the distributions of behavioral determinants and indicators are balanced as closely as possible over treated and non-treated, using observational data. The use of matching has improved the policy evaluation practice by clarifying the importance of common support restrictions for the distribution of conditioning variables. By now, it is a common tool for the analysis of active labor market policies (ALMP) and programs (see e.g. the survey in Kluve, 2006). However, matching methods have the well-recognized limitation of not ensuring the balancing of the distribution of unobservable determinants of both treatment assignment and outcomes among treated and non-treated. When incapable to balance unobservables, matching methods may produce biased estimates of the treatment effects.

The first contribution of this paper deals with this problem by developing an estimation method for the average treatment effect on the treated that is robust to violations in the conditional independence assumption that underlies matching methods. We effectively correct the matching estimate with a measure of the bias due to selection on unobservables. Key to the estimation of such correction term is the availability of an instrument capable

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<sup>1</sup>See e.g. Cochrane and Rubin (1973), Rosenbaum and Rubin (1983), and Heckman, Ichimura, and Todd (1998).

of driving participation to zero at certain of its (possibly limiting) values while keeping the selection mechanism partly unexplained at other parts of its distribution. Like the matching methods, our approach matches the distribution of observed variables between treated and non-treated groups, thus effectively combining matching with the exogenous variation provided by an instrument to balance unobservables.

Alternative approaches to correct matching estimators for selection problems typically assume that the relevant unobserved variables have additive effects on the potential outcomes (see Heckman and Robb, 1985, and Andrews and Schafgans, 1998). The popular conditional difference-in-differences estimator (Heckman, Ichimura, Smith, and Todd, 1998) is also based on this. By contrast, our approach does not require additivity.

Within the same framework we also suggest a new test of the CIA. In the presence of a valid instrument, satisfying the condition introduced above, the CIA holds if and only if the correction term is zero. Thus, testing the validity of the CIA is equivalent to testing the statistical significance of the correction term.

The second contribution of this paper is to show that there are important empirical applications for this method. Consider, for instance, the case of treatment evaluation in the context of ALMP for unemployed workers. It has been recognized that individual characteristics and employment history may not capture the full range of skills and motivation that explain both treatment participation and employment-related outcomes.<sup>2</sup> However, many programs use clear eligibility rules based on observed variables and often involving boundary restrictions. Also, novel programs are introduced and others are terminated, creating variation in eligibility over calendar time. Eligibility variation can be exploited to construct an instrument capable of moving subjects in and out of treatment while otherwise being unrelated to the potential outcome(s) of interest.

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<sup>2</sup>For example, Card and Sullivan (1988), Gritz (1993), Bonnal, Fougère and Sérandon (1997) and Richardson and Van den Berg (2012) argue that this can be expected to play a major role in the empirical evaluation of ALMP, and their estimation results confirm this. Van den Berg, Van der Klaauw and Van Ours (2004) contain similar findings for the effect of punitive sanctions for welfare recipients.

The ideal setting for the application of our method is created by non-mandatory programs in the presence of boundary eligibility restrictions on personal characteristics or time. In such cases, full non-participation is observed at certain values of the instrument while compliance is imperfect at other values.<sup>3</sup> This is a relevant setting for ALMP. It is a common feature of ALMP to restrict eligibility to individuals aged above or below a certain age, or to individuals with a certain minimum or maximum amount of education, and/or to individuals with a certain minimum amount of labor market experience (see e.g. Kluve, 2006). The latter includes eligibility based on profiling systems that determine potential treatments as a deterministic function of a set of individual characteristics. Many policies only apply to certain regions, cantons or states, and as noted above, the introduction and abolition of policies leads to eligibility changes as well. (In Section 3 of the paper we give concrete examples of studies that exploit eligibility variation in instrumental variable settings.) In many of these cases, compliance among the eligible individuals is imperfect, and actual participation is selective. Individuals may influence participation, or the case worker may use her discretionary power to assign individuals based on individual characteristics that are unobserved to the researcher. The same problems arise with random experiments if compliance to the treatment is imperfect. In all these cases, matching approaches cannot be used. We propose overcoming this limitation by exploiting the eligibility boundary restriction within the matching framework.

Our approach to inference is related to Battistin and Rettore (2008), who consider a specific partially fuzzy discontinuity design where eligibility rules preclude participation on one side of a threshold for a certain variable and allow - but do not impose - participation on the opposite side (so non-compliance affects outcomes on one side of the threshold). To identify a Local Average Treatment Effect (LATE), they need the continuity assumption that is characteristic of sharp regression discontinuity (RD) designs.<sup>4</sup> In contrast, our esti-

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<sup>3</sup>Here, the word “compliance” is used in a statistical sense, meaning that some of the individuals who, according to the policy design, are eligible for treatment end up in the non-treated subpopulation.

<sup>4</sup>The idea of exploiting one-sided compliance to deal with selective participation has some history in

mator is formulated it in a matching framework, and our quantity of interest is the average treatment effect on the treated (ATT). Accordingly, the underlying assumptions are not identical. As will be made clear later on, we require an exclusion restriction that is stronger than Battistin and Rettore (2008)’s continuity assumption, but in turn we derive conditions for the identification of ATT in empirical settings with no discontinuity in participation. As a result, the applicability of the two estimators differs, and our empirical study in Section 3 illustrates the practical use of our estimator when regression discontinuity is not appropriate.

As noted above, our estimator relies on an instrumental variable, and as such it is related to the LATE approach introduced by Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996). However, here again the underlying assumptions differ. LATE relies on a monotonicity assumption to determine the impact of treatment on “compliers” for a local variation in the instrument. Instead, our estimator is based on the alternative premise that the instrument can drive participation in a program to zero. This effectively rules out the existence of “defiers” and “always-takers” when comparing observations at two values of the instrument, where the probability of participation is zero at one of these points. Making all treated “compliers” makes our assumption stronger than the LATE monotonicity assumption. We can use this to weaken the LATE independence assumptions to the standard exclusion restriction of Instrumental Variable estimation, and we can also weaken the monotonicity assumption within the subdomain of the instrument where participation probabilities are positive. In cases where the instrument is binary and participation is not possible at one of its values, LATE and ATT are equivalent, and the two methods identify the same parameter. But this is generally not true for continuous instruments or instruments assuming many values, in which case the two methods are not connected. We show

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the analysis of treatment effects on duration outcomes in Mixed Proportional Hazard types of models with endogenous treatments. See Bijwaard and Ridder (2005) and Abbring and Van den Berg (2005). Note also the similarities with the “Bloom setting”: a fully experimental setting with non-compliance on the treated side only.

how to aggregate our estimates over the distribution of the matching covariates and the instrument to obtain the ATT.

We empirically assess our approach by evaluating a major Swedish program aimed at helping young unemployed individuals to find work, the Youth Practice (YP).<sup>5</sup> YP is a subsidized work program designed for short-term unemployed individuals aged below 25. The program is not compulsory. As a result, compliance is imperfect on the lower side of the age-eligibility threshold. We may therefore apply our selectivity-adjusted matching estimator using age as the instrument. In Section 3 we argue that the exclusion restriction is valid. The subpopulation of non-treated includes those below 25 who do not participate as well as those 25 and above. Participation is not sharply discontinuous at age 25 but declines gradually before age 25. This is not a problem for our method but could complicate the application of regression-discontinuity methods. The non-compulsory nature of the program among eligibles may raise difficulties for matching to balance unobservables. We use our correction factor to assess whether this is in fact the case and to eliminate the potential selection bias.

The Swedish YP has been evaluated before (see e.g. White and Knight, 2002, Larsson, 2003, Forslund and Nordström Skans, 2006, for results). It is of particular interest that existing YP evaluations are based on the matching approach. We find that adjusting the matching estimator for selectivity changes the results to become negative when the outcome of interest is outflow into employment.

In Section 2 we develop a formal framework for the analysis. We define the objects of interest and we derive the selectivity-adjusted matching estimator. In Section 3 we discuss the Swedish YP program, the implementation of our estimator, the data, and the estimation results. Section 4 concludes.

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<sup>5</sup>There is an increasing awareness that youth unemployment may be a serious problem for society despite the fact that youth unemployment durations are relatively short. This is because of the prevalence of psychological and labor-market scarring effects which may have long-run implications for the productivity of those affected (see e.g. Burgess et al., 2003).

## 2 A correction term to matching

### 2.1 Identification of the ATT in case of selective participation and ineligibles

In what follows, we adopt standard counterfactual notation where  $Y_0$  and  $Y_1$  are individual potential outcomes associated with being assigned to non-treatment and treatment, respectively. The binary indicator,  $D$ , denotes the actual treatment status, where we use the terms “participation” and “treatment” to denote  $D = 1$  and “non-participation” and “control” to denote  $D = 0$ . The finite-dimensional vector  $X$  contains conditioning variables. The actual outcome  $Y$  satisfies  $Y = DY_1 + (1 - D)Y_0$ .

We are interested on the Average Treatment Effect on the Treated (ATT):

$$\text{ATT} = \text{E}[Y_1 - Y_0 \mid D = 1].$$

Clearly,

$$\begin{aligned} \text{ATT} &= \text{E}_{X|D=1} \text{E}[Y_1 - Y_0 \mid X, D = 1] \\ &= \text{E}_{X|D=1} \text{E}[Y_1 \mid X, D = 1] - \text{E}_{X|D=1} \text{E}[Y_0 \mid X, D = 1] \end{aligned} \tag{1}$$

where the expectations  $\text{E}_{X|D=1}$  are taken over the distribution of  $X$  among the treated. Under the unconfoundedness assumption or Conditional Independence Assumption (CIA) stating that  $Y_0 \perp D \mid X$  (and under additional SUTVA and common support assumptions; see below), the ATT is identified and can be estimated using a matching method. We do *not* make such an assumption because we do not rule out that, conditional on observed confounders  $X$ , the actual treatment assignment is related to the individual potential outcomes. The most obvious reason to expect violation of the CIA is that there may be individual characteristics that affect both the treatment status and the potential outcome in case of non-participation in the program, where some of these characteristics may be unobserved to the researcher. We therefore refer to the violation of the CIA as “selection on unobservables”.

Our proposed estimator relies on the classical “common support matching” assumption on variables  $X$ , demanding some degree of randomness in the participation rule over the domain of covariates  $X$  among the treated. This is our Assumption 1:

**Assumption 1.**  $0 < P[D = 1 | X = x] < 1$  for all  $x$  in  $\text{Supp}(X | D = 1)$ ,

where  $\text{Supp}(X | D = 1)$  is the domain of covariates  $X$  among the treated.

Instead of adopting the CIA typical of matching, we assume that there exists a variable  $Z$  satisfying the following Assumptions 2 and 3,<sup>6</sup>

**Assumption 2.**  $Y_0 \perp Z | X$ ,

**Assumption 3.** There exist points  $\{z^*, z^{**}\}$  in the domain of  $Z$  where

$$P[D = 1 | X, Z = z^*] = 0 \quad \text{and} \quad P[D = 1 | X, Z = z^{**}] > 0$$

for each  $X$  in  $\text{Supp}(X | D = 1)$ .

Assumption 2 states that  $Z$  does not explain  $Y_0$  when conditioning on the explanatory variables  $X$ . It is a common instrumental variables exclusion assumption, but notice that it is imposed on untreated outcomes only and that it is conditional on observed confounders. In contrast, the LATE estimator further requires that  $Z$  has no impact on potential outcomes when treated, and that it is exogenous in the assignment rule, to allow for the presence of “always-takers” under the considered variation in  $Z$  (Imbens and Angrist, 1994; Angrist, Imbens and Rubin, 1996).

Assumption 3 states that  $D$  is a non-trivial function of  $Z$  after conditioning on  $X$ . In particular, it ensures that participation can be driven to zero at certain values of  $Z$ .

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<sup>6</sup>Identification of the ATT also hinges on SUTVA (Stable Unit Treatment Value Assumption). SUTVA requires the potential outcomes of each individual  $i$  to be invariant to the assignment of treatment in the rest of the population (Rubin, 1980 and 1990). Under SUTVA there is no interference between units or individuals in the population. It effectively rules out effects of treatment arising through market adjustment mechanisms or social interactions.

The second part of Assumption 3, that participation is positive for some values of  $Z$ , is ensured by Assumption 1. If the participation probability is zero (positive) then we call the individual ineligible (eligible). As alluded to in the introduction, Assumption 3 is stronger than the corresponding LATE assumption in that it requires the instrument to drive participation (informative) and rules out “defiers” (i.e., imposes monotonicity). On the other hand, we do not require monotonicity within the set of values of the instrument where individuals are eligible.

Assumptions 1-3 do not rule out that participation is selective on unobservables. In particular,  $D$  may depend on  $Y_0$  if  $Z = z^{**}$  even after conditioning on  $X$ . Since Assumptions 2 and 3 can be called an exclusion restriction and an “informative instrument” assumption, it is natural to call  $Z$  an instrumental variable. Notice that Assumption 3 can be verified empirically, whereas Assumption 2 requires an external justification.

A common concern with exclusion restrictions like Assumption 2 is that there may be characteristics that affect both the instrument  $Z$  and the potential outcome  $Y_0$ , where these characteristics may be unobserved to the researcher. If the instrument is a threshold value of a personal characteristic, one needs to address whether the threshold may represent additional differences between the set of agents on one side of the threshold and the set of agents on the other side. For example, the threshold may coincide with a stratification of the labor market. Another concern with exclusion restrictions is that individuals may act upon knowledge of their personal value of  $Z$  before the treatment status is determined, and this behavior may affect the potential outcomes, leading to a violation of the restriction (Van den Berg, 2007). In Section 3 we discuss the justification of Assumption 3 in our empirical application and in similar potential applications.

In expression (1) for ATT, the term  $E_{X|D=1}E[Y_1 | X, D = 1]$  is directly identified from the mean observed outcome among the treated. The challenge is to identify the mean counterfactual outcome,  $E_{X|D=1}E[Y_0 | X, D = 1]$ .

Under Assumption 2,

$$\begin{aligned}
\mathbb{E}[Y_0 | X] &= \mathbb{E}[Y_0 | X, Z] \\
&= \mathbb{E}[Y_0 | X, Z, D = 0] \mathbb{P}[D = 0 | X, Z] + \\
&\quad \mathbb{E}[Y_0 | X, Z, D = 1] \mathbb{P}[D = 1 | X, Z].
\end{aligned} \tag{2}$$

Since this relationship holds for all possible values of  $Z$ , and in particular for  $Z = z^*$ , Assumption 3 ensures that

$$\mathbb{E}[Y_0 | X] = \mathbb{E}[Y_0 | X, Z = z^*, D = 0]. \tag{3}$$

On the other hand, the following decomposition always yields,

$$\begin{aligned}
\mathbb{E}[Y_0 | X] &= \mathbb{E}[Y_0 | X, D = 0] \mathbb{P}[D = 0 | X] + \\
&\quad \mathbb{E}[Y_0 | X, D = 1] \mathbb{P}[D = 1 | X]
\end{aligned}$$

implying

$$\begin{aligned}
&\mathbb{E}[Y_0 | X, D = 1] \\
&= \frac{\mathbb{E}[Y_0 | X] - \mathbb{E}[Y_0 | X, D = 0] \mathbb{P}[D = 0 | X]}{\mathbb{P}[D = 1 | X]} \\
&= \frac{\mathbb{E}[Y_0 | X, Z = z^*, D = 0] - \mathbb{E}[Y_0 | X, D = 0] \mathbb{P}[D = 0 | X]}{\mathbb{P}[D = 1 | X]} \\
&= \mathbb{E}[Y_0 | X, D = 0] + \frac{\mathbb{E}[Y_0 | X, Z = z^*, D = 0] - \mathbb{E}[Y_0 | X, D = 0]}{1 - \mathbb{P}[D = 0 | X]}. \tag{4}
\end{aligned}$$

where the common support Assumption 1 ensures that the denominator in the ratio expressions is non-zero.

Equation (4) is an expression for the mean counterfactual outcome  $\mathbb{E}[Y_0 | X, D = 1]$  given  $X$ . The second term in line four of the equation corrects  $\mathbb{E}[Y_0 | X, D = 0]$ , the mean counterfactual outcome given  $X$  that is used in standard matching estimation, for selection on unobservables.

The terms  $\mathbb{E}[Y_0 | X, D = 0]$  and  $\mathbb{E}[Y_0 | X, Z = z^*, D = 0]$  on the right-hand side of equation (4) are identified from the mean observed outcome among the controls at given

$X$ , and the mean observed outcome among the ineligible controls at given  $X$ , respectively. Taken together, the mean counterfactual outcome  $E[Y_0 | X, D = 1]$  given  $X$  is identified from equation (4). If there is more than one value of  $Z$  satisfying the condition for  $z^*$  in Assumption 3, then it is efficient to use them all and estimate  $E[Y_0 | X, Z \in Z^*, D = 0]$  by averaging over the observable conditional (on  $X$ ) distribution of  $Z$  in  $Z^* = \{z^* \in \text{Supp}(Z) | P(D = 1 | X, Z = z^*) = 0\}$ . In turn, by the law of iterated expectations, the mean counterfactual outcome  $E_{X|D=1}E[Y_0 | X, D = 1]$  unconditional on  $X$  is identified by averaging over the observable distribution of  $X$  given  $D = 1$ . Hence, the ATT is identified.

Notice that identification does not require any additivity assumption on the relationships between outcome, treatment, and instrument. Also, it does not require the instrument to be discrete or to be continuous. In Subsection 2.2 below we discuss, in some detail, the implementation of the estimator suggested by the above constructive identification proof. We should also point out that the results in the current subsection do not depend on the type of matching technology used to match treated and controls.

If the population of ineligibles is comparatively large, the denominator in equation (4) may mechanically yield small values. The estimates of the correction term in that equation are then sensitive to small differences in mean outcomes between non-eligibles and non-treated in the data. In the remainder of this subsection we consider a variation on our approach above, to deal with this. This alternative approach is motivated by the insight that in the absence of selection on unobservables, one may discard the ineligibles and instead use only the eligible controls to obtain the mean counterfactual outcome for the treated.

To proceed, it is useful to re-write the ATT as

$$\text{ATT} = E_{[X|D=1]} \text{ATT}(x)$$

where

$$\begin{aligned}
\text{ATT}(x) &= \mathbf{E}_{[Z|X=x,D=1]} \mathbf{E}[Y_1 - Y_0 \mid X = x, Z, D = 1] \\
&= \mathbf{E}_{[Z|X=x,D=1]} \mathbf{E}[Y_1 \mid X = x, Z, D = 1] - \\
&\quad \mathbf{E}_{[Z|X=x,D=1]} \mathbf{E}[Y_0 \mid X = x, Z, D = 1]
\end{aligned}$$

This expression explicitly defines the mean counterfactual for  $\text{ATT}(x)$  over the domain of  $Z$  among the treated with covariates  $x$  – that is, for all values  $z^{**}$  satisfying Assumption 3 with  $X = x$ . This also makes it clear that Assumption 1 on the common support is not sufficient for the identification of the ATT if we were to use only eligibles in the matching procedure, as it does not exclude the possibility of full participation for some specific values of  $(X, Z)$  in the domain of these two variables among the treated. Since  $Z$  is now being used as a matching variable, the common support assumption needs to hold for all matching covariates,  $(X, Z)$ . Thus, we strengthen the common support assumption as follows

**Assumption 1’.**  $0 < P[D = 1 \mid X = x, Z = z^{**}] < 1$  for all  $(x, z^{**})$  in  $\text{Supp}(X, Z \mid D = 1)$ .

Under Assumption 1’, participation is only partially determined by  $(X, Z)$  among the eligibles.

Let us now follow the earlier derivation of the identification of  $\mathbf{E}[Y_0 \mid X, D = 1]$ , where we now condition on  $Z = z^{**}$  as well as on  $X$ . The mean no-treatment outcome at a specific point  $(X, Z = z^{**})$  with a non-zero probability of treatment is

$$\begin{aligned}
\mathbf{E}[Y_0 \mid X, Z = z^{**}] &= \mathbf{E}[Y_0 \mid X, Z = z^{**}, D = 0] \mathbf{P}[D = 0 \mid X, Z = z^{**}] + \\
&\quad \mathbf{E}[Y_0 \mid X, Z = z^{**}, D = 1] \mathbf{P}[D = 1 \mid X, Z = z^{**}]
\end{aligned}$$

Assumptions 2 and 3 ensure that

$$\begin{aligned}
\mathbf{E}[Y_0 \mid X, Z = z^{**}] &= \mathbf{E}[Y_0 \mid X, Z = z^*] \\
&= \mathbf{E}[Y_0 \mid X, Z = z^*, D = 0]
\end{aligned}$$

for any  $z^*$  satisfying the condition  $P(D = 1 | X, Z = z^*) = 0$ . But then, the counterpart of (4) when conditioning on  $Z = z^{**}$  under Assumption 1' is

$$\begin{aligned} & E[Y_0 | X, Z = z^{**}, D = 1] \\ &= E[Y_0 | X, Z = z^{**}, D = 0] \\ &+ \frac{E[Y_0 | X, Z = z^*, D = 0] - E[Y_0 | X, Z = z^{**}, D = 0]}{P[D = 1 | X, Z = z^{**}]}. \end{aligned} \tag{5}$$

The terms  $E[Y_0 | X, Z = z^{**}, D = 0]$  and  $E[Y_0 | X, Z = z^*, D = 0]$  on the right-hand side of equation (5) are identified from the corresponding observed outcomes.<sup>7</sup> This implies that the mean counterfactual outcome  $E[Y_0 | X, Z = z^{**}, D = 1]$  at given  $X$  and  $Z = z^{**}$  is identified from equation (5). In turn, the mean counterfactual outcome  $E_{Z|X, D=1}E[Y_0 | X, Z = z^{**}, D = 1]$  can be obtained from averaging (5) over the distribution of  $Z$  conditional on  $X, D = 1$  and the ATT is the average of  $ATT(X)$  over the distribution of  $X$  conditional on  $D = 1$ .

The correction term in (5) vanishes if there is no selection on unobservables, so the only controls used to estimate the ATT are the non-treated eligibles. In this case, the alternative approach subsumes the instrument  $Z$  in the set of conditioning variables  $X$ .<sup>8</sup> With selection on unobservables, of course, the ineligible controls with  $Z = z^*$  are also used in the alternative approach to obtain the correction term.

Under Assumptions 1', 2 and 3 (together with SUTVA), the estimators implied by (4) and (5) are equally consistent for ATT. However, if the population of ineligibles is large as compared to the treated, the denominator in (4) may become excessively small, which would lead to imprecise estimates of the correction term in finite samples, by magnifying any small variation in estimates of the difference  $E[Y_0 | X, Z = z^*, D = 1] - E[Y_0 | X, D = 1]$ .<sup>9</sup> In equation (5), we tackle this by comparing treated with other eligibles only.

However, there are also costs associated with using (5) instead of (4). These arise from

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<sup>7</sup>As explained before, it is more efficient to estimate  $E[Y_0 | X, Z = z^*, D = 0]$  over the whole set of points  $z^*$  under which participation is nil conditional on  $X$

<sup>8</sup>See Heckman and Lozano (2004) for a discussion of the selection of covariates in matching.

<sup>9</sup>This problem is analytically analogous to the classical weak instrument problem but conceptually

two sources. First, the precision of inference on the standard matching counterfactual may be affected by a strong reduction in sample size resulting from the exclusion of ineligible from the sample. And second, the common support assumption is stronger. If Assumption 1' does not hold over the entire support of  $(X, Z)$  among treated; specifically if points  $(x, z)$  exist for which  $P(D = 1 | x, z) = 1$ , then the parameter identified by (5) is not the conventional ATT but instead an average of  $(X, Z)$ -specific ATTs over the observed overlapping support of  $(X, Z)$ , where  $P(D = 1 | X, Z) < 1$  is satisfied.<sup>10</sup> This is not a problem for the estimator implied by equation (4) since it does not exploit values  $z^{**}$  in the eligible region. There, the matching counterfactual is estimated using the entire non-treated sample, and the required common support Assumption 1 always holds if controls can be drawn from the non-eligible region.

In practice, the alternative approach based on equation (5) may be preferable in cases where the sample sizes of non-treated eligibles and ineligible are large, provided that the more demanding common support Assumption 1' holds. But if the sample size of treated is sufficiently large to ensure that  $P(D = 1 | X)$  is not too close to zero, then the estimator based on result (4) is preferable.

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different, as the weak instrument problem only applies among eligibles. Baiocchi et al. (2010) develop a treatment evaluation estimator that also combines matching and instrumental variables. Their approach aims specifically at improving the strength of the instrument using matching. In the first stage of their estimation procedure, they match individuals with low values of the instrumental variable to individuals with high values. Subsequently, they examine the effect of the instrument on the treatment and the effect of the instrument on the outcome. Combining this results in an estimator that can be interpreted as a LATE estimator. As in our case, controlling for covariates makes it less likely that the exclusion restriction is violated by common unobserved determinants of the instrument and the potential outcomes.

<sup>10</sup>Notice that, despite the exclusion restriction Assumption 2, the ATT may vary with  $Z$  even after conditioning on  $X$  because exclusion is not imposed on  $Y_1$  and because  $Z$  affects participation, possibly because of expected gains from treatment. Thus, for any two values of  $Z$  in the eligible region conditional on  $X$ , say  $(z_1, z_2)$  it is possible that  $E[Y_0 | X, Z = z_1, D = 0] \neq E[Y_0 | X, Z = z_2, D = 0]$ .

## 2.2 Inference

Our estimation method for the ATT closely follows the above identification proofs. For the sake of brevity we focus on the method for the first approach discussed above, which uses equation (4) to obtain the mean counterfactual outcome among the treated  $E(Y_0 | D = 1)$ . This is also our preferred method for as long as the sample of ineligibles is not too large in comparison to that of treated, as it relies on a weaker common support assumption. Equation (4) is conditional on  $X$  and  $D = 0$ , but we need to average it over the observable distribution of  $X$  given  $D = 1$  to obtain  $E(Y_0 | D = 1)$ . For this purpose we estimate a propensity score for  $P(D = 1 | X)$ , using the full sample. Next, we match each treated individual to non-treated individuals, using propensity-score kernel-matching. However, contrary to the standard matching approach to treatment evaluation, we do not take the difference between the outcome of the treated and the matched (weighted mean) outcome of the controls. Instead we take the difference between the outcome of the treated and the matched (weighted mean) value of the right-hand side of equation (4). On the right-hand side, the separate terms are kernel-smoothed for this purpose, using propensity scores as well. Notice that in the alternative approach based on equation (5), the main propensity score does not only depend on  $X$  but also on  $Z$  for values  $Z = z^{**}$  satisfying Assumption 3.<sup>11</sup> In Subsection 3.4 we discuss practical implementation issues for our estimator, in the context of the evaluation of the Swedish Youth Practice (YP) program.

The standard errors are estimated with bootstrapping. Each replication samples from the original data – before matching is performed – and reproduces exactly the whole estimation routine. This procedure accounts for all steps in estimation, including estimation of the propensity score. Standard errors are computed directly on the estimated ATT, not

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<sup>11</sup>The ATT estimates suggested by the two alternative approaches are not necessarily identical. This provides scope for the construction of a general specification test. However, it remains to be seen whether such a test has satisfactory power since the underlying estimates are driven by outcomes from overlapping subsamples that may not coincide, as discussed towards the end of the previous subsection.

as a combination of standard errors of different terms that may form the ATT.<sup>12</sup> Because the number of matches increases with sample size and the estimator is asymptotically linear as shown in Heckman, Ichimura and Todd (1998), bootstrapping is expected to provide correct inference when applied to kernel matching (this is at odds with the results for nearest neighbor matching derived in Abadie and Imbens, 2008). It is also much simpler to implement and expected to lead to more reliable inference than the asymptotic variance derived in Heckman, Ichimura and Todd (1998).

We may also use the results of the previous subsection to design tests of the usual CIA assumption that  $Y_0 \perp D \mid X$  in the overlapping support, provided that Assumptions 2 and 3 apply. The standard matching method assumes the CIA which implies that the first term of the right-hand side of (4) captures  $E(Y_0 \mid X, D = 1)$ . As already pointed out, the second term can be labelled a correction term due to selection on unobservables. Thus, the usual CIA assumption holds iff the correction term is zero for any possible  $X$ , so iff

$$E[Y_0 \mid X, Z \in Z^*, D = 0] = E[Y_0 \mid X, D = 0] \quad (6)$$

for any  $X$ . In the alternative approach (see (5)), this is replaced by

$$E[Y_0 \mid X, Z \in Z^*, D = 0] = E[Y_0 \mid X, Z = z^{**}, D = 0]. \quad (7)$$

for any possible  $X$  and  $z^{**}$ . This can again be aggregated over  $X$  and  $z^{**}$ . These equalities can be used to test the usual CIA assumption in standard matching estimation. Alternatively, we may test directly whether the correction terms are zero, because these are a by-product of the ATT estimation.

Battistin and Rettore (2008) propose a selection test based on the bias term

$$E[Y_0 \mid X, Z, D = 0] - E[Y_0 \mid X, Z, D = 1] \quad (8)$$

defined in regions of  $Z$  where participation is not deterministic. Under their RD design with one-sided imperfect compliance, the bias term in (8) can be computed at the eligibility

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<sup>12</sup>This is important as various terms, such as the standard matching estimate and the correction terms, may be correlated.

cutoff point. The statistical significance of this term at that specific point provides some information of what may happen elsewhere. In contrast, the matching setup that we explore allows us to directly test the CIA on a larger part of the domain of  $Z$ , and therefore on a larger population. This is not empirically irrelevant as sample sizes often preclude meaningful analysis in local discontinuity estimation.

### **3 Empirical Application: Evaluating Youth Practice**

We study the impact of a Swedish youth employment program, the Youth Practice (YP), on the employment probability of young men. The aim of this program is to ease the flow of young unemployed into work by providing work experience. The main focus of our evaluation is its impact on transitions into employment. In what follows we discuss the program, the data, the definition of the treatment and the outcome of interest, the choice of the instrument, the estimation procedure, and the results.

#### **3.1 The program**

YP is a Swedish large-scale subsidized-work program targeted at the 18-24 years old unemployed. It was launched in July 1992, in response to the adverse labor market conditions of the early 1990s recession in Sweden. In October 1995 it was subsumed into an extended policy program for youth unemployment.

The YP program was primarily intended for unemployed individuals with a high school diploma. Officially, two rules determined eligibility: (1) that the individual is aged 18 to 24 at the time of enrollment into YP, and (2) that prior to enrollment she/he has been registered with the employment office for a minimum duration of 4 months for the 20-24 years old and 8 weeks for the 18-19 years old. Empirical data show that the second eligibility requirement was not respected: almost 20 percent of 20-24 years old participants enter YP within 1 month of registering, and over 60 percent enter before completing the

first 4 months. In contrast, the age eligibility rule is strictly respected: participants are always 24 or younger at the moment of enrolling into YP.

The treatment consisted of a job placement in the private or public sector for 6 months with a possible extension to 12 months. While at work, YP participants received an allowance below the prevailing wage rate. The employer paid at most a small fraction of the allowance. The job was supposed to be supplementary, in the sense that it should not displace regular employment, and to include a mixture of work and training leading to human capital accumulation. In addition to work, participants were also expected to spend four to eight hours per week actively seeking regular employment at the employment office. In practice, however, the latter guidelines may not have been strictly adhered to. No-displacement and job-search guidelines seem to have been ignored regularly. Most job search requirements were simple, and the proportion of time in training was negligible (see Larsson, 2003, and references therein).

Participation was not compulsory. YP was one among several non-compulsory treatments that agents could enter. Notably, agents could try to participate in Labor Market Training. This is an expensive program that mostly consists of vocational training. However, this program was primarily intended for displaced workers in need of a new type of occupation (Richardson and Van den Berg, 2012). Indeed, YP was by far the most common treatment among young unemployed individuals. In over 22% of the new registration spells of eligible individuals aged 20 to 24 during the YP period, YP is the first reported event after registration at the employment office, whereas the other possible treatments amount to only 16%, of which just over a third concerns Labor Market Training.<sup>13</sup>

Assignment to YP follows a complex selection mechanism. Similarly to all alternative treatments offered by the Swedish employment offices at the time, assignment to YP was decided in mutual agreement between the unemployed and the employment officer. Ac-

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<sup>13</sup>We consider exits from the first unemployment period after registration with the employment office. The reported figures refer to individuals aged 20 to 24 when first registering with the employment office between July 1992 and September 1994.

ording to the accounts of job seekers and employment officers alike, YP was generally regarded as providing real work experience (see Larsson, 2003, Forslund and Nordström Skans, 2006). Moreover, candidates interested in YP were encouraged to find a placement on their own, to build initiative, job search and representation skills. Together, these two conditions support the view that participants may have been better equipped to find a job than non-participants. In some occasions, however, employment officers offered YP placements to job seekers. The allocation of such treatments may be driven by officers prioritizing candidates in special need of help or close to benefits eligibility exhaustion. Both processes challenge the ability of matching to deal effectively with selection. Our proposed method can be used to assess the presence of residual selection after matching and to correct it if a valid instrument is available.

## **3.2 Data**

### **3.2.1 Register and sample**

We use the Swedish unemployment register called *Händel*. This is an administrative dataset that comprises information from August 1991 onwards on registration spells with the employment offices, labor market status of registered individuals (individuals in low-paid jobs may register), program participation and the subsequent labor market status of those who deregister (e.g. employment, education or inactivity). All individuals with registration spells since 1991 are included in the dataset and their registration and unemployment history can be followed over time. *Händel* also includes demographic information on age, gender, citizenship, area of residence and education.

The *Händel* dataset required considerable cleaning and selection work, mainly due to the high incidence of negative and overlapping spells. The criteria applied to construct the final dataset are described in the appendix.

To obtain a sample of eligibles, we select observations of male individuals aged 20 to 24 when first registering with the employment office between July 1992 and September 1994

(or some subgroup thereof). Men aged 18-19 at registration are excluded due to a range of differences with the policy regime for those below 20 (see Forslund and Nordström Skans, 2006, and the other references in Section 1, for details on YP and youth unemployment in Sweden). Observations for older men (age 25 to 29) are used to construct the control group of ineligible. Estimation relies on either all new registration spells, independently of the employment status of the new applicant, or the subsample of new registration spells classified as open unemployment for comparison purposes.<sup>14</sup> We consider a single registration spell per individual during the period of interest, namely the earliest one. Registration spells starting after September 1994 are disregarded, as YP take-up slowed substantially from then onwards until extinction in October 1995.<sup>15</sup> Details on the construction of treatment and comparison groups, sample sizes and sample properties are in Subsections 3.2.2 and 3.5 below.

### 3.2.2 Treatment status and outcomes of interest

We aim to measure the impact of YP on subsequent individual labor market outcomes. The potential participants, or *eligibles*, are men starting a new registration spell with the employment office between July 1992 and September 1994 while aged 20 to 24. In contrast, *ineligibles* are men who start a new registration spell when aged 25 or above during the same period. *Treatment* is defined as enrolling into YP as the first destination after registration, if enrollment occurs during the first 3 or during the first 6 months of the spell (we define “time to treatment” to be the duration of the registration spell before the enrollment in YP). Obviously, enrollment is only possible for eligibles.

Dynamic selection<sup>16</sup> is beyond the scope of our study. Thus, our definition of treatment

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<sup>14</sup>Employed individuals looking for a new job may register with the employment office; they account for less than 4% of all new spells for the population we are considering.

<sup>15</sup>Among eligibles, YP occurred in only 3% of registration spells starting after September 1994 and under 1% of registration spells starting after January 1995.

<sup>16</sup>Controlling for the duration of unemployment prior to enrollment into treatment would call for a dynamic framework, which has problems of its own, in particular since we do not rule out that there is

status is *unconditional* on time to treatment other than through the time window requirement described above. Likewise, non-treated eligibles and non-eligibles are not selected on time in unemployment before moving into some alternative treatment or deregistering. The severity of any resulting bias from disregarding time to treatment will depend on the time window allowed before enrollment. We consider only relatively short durations prior to enrollment into treatment, of up to 3 or 6 months, and compare results to assess the importance of our choices.<sup>17</sup> By excluding YP participants enrolling later in their registration spell, we eliminate selection due to the imminent exhaustion of entitlement to unemployment benefits (which occurs after 14 months in the claiming count). We notice that the treated, or participants, do move quickly into YP after registration, with just under 50% and 83% doing so during the first 3 and 6 months of the new spell, respectively.

To assess the impact of enrolling into YP, we measure employment status after registration. We focus on the take-up of regular employment, or, alternatively, on having deregistered to move to any destination, after 12 or after 24 months of registration.<sup>18</sup>

### 3.3 Instrumental variable

As noted above, our instrument is the age of the individual. Specifically, we explore the clear change in eligibility status at age 25. The exclusion restriction entails that the potential outcome in case of non-treatment should not systematically depend on whether the individual's age at registration is above or below 25 years, conditional on  $X$ . We provide some arguments in support of this. First, notice that although employers may find it relevant whether an applicant for a regular job is above or below 25 years old, they are less selection on unobservables; see Abbring and Van den Berg (2003, 2005). See Fredriksson and Johansson (2008) and Crépon et al. (2009), for the use of matching methods that deal explicitly with dynamic enrollment.

<sup>17</sup>Sample sizes become forbiddingly small when further tightening the enrollment window.

<sup>18</sup>Since we do not follow individuals throughout their out-of-the-register periods, all we know is their destination upon leaving. When considering the employment status, we implicitly assume it has not changed after registration.

likely to be interested in whether the individual was above or below 25 at the time of potential enrollment into YP. As we shall see, we use samples with rather narrow age intervals around 25, so at the time at which the outcomes are realized, many treated individuals are older than 25. Notice also that individuals who are aged around 25 and who enter unemployment often have a relatively low level of education. Such individuals have not yet achieved a high degree of specialized work experience. Hence, their baseline position in the job market is not likely to be strongly dependent on the exact age within the age interval we consider. More in general, the age range within which individuals compete with each other in the job market is probably broader than just a few years.

The use of age eligibility thresholds as instrumental variables is widespread in the empirical evaluation literature. For example, Angrist and Krueger (1991) use age eligibility thresholds for compulsory schooling to estimate the impact of education on earnings, while Gelbach (2002) uses it to estimate the impact of children's enrollment in public education on mother's labor supply. Stancanelli and Van Soest (2011) use retirement age thresholds to study the effect of the partner's retirement on home production. Dickens, Riley and Wilkinson (2010), Olssen (2011) and others use age discontinuities of mandatory minimum wages to study effects of youth minimum wages on individual labor market outcomes. Calendar time thresholds (usually involving the introduction of a new policy) are also widely used as instrumental variables in evaluation studies. Examples are Blundell et al. (2004) and Van den Berg, Bozio and Costa Dias (2010), who use the introduction of a job search assistance program to study participation effects on individual labor market outcomes.<sup>19</sup>

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<sup>19</sup>In a sensitivity check of our empirical results, we exploit the introduction of YP in 1992 as an alternative (calendar time)instrument. In our setting, it has the drawback that macro-economic conditions were deteriorating around 1992. Yet, the results are fairly aligned to those obtained when using the age instrument. Notice that in the presence of heterogeneity in the impact of the treatment, the two instruments may identify different parameters as they implicitly define different subsamples of treated.

### 3.4 Estimation procedure

The application of our method requires two control groups to be defined. With the counterfactual of interest as described in equation (4), the *first* control group is as in standard matching approaches, i.e. it is drawn from the population of non-participants ( $D = 0$ ) to reproduce the distribution of the matching variables  $X$  among the treated. Since the instrument  $Z$  is not in  $X$ , non-participants comprise both non-eligible individuals ( $Z = z^*$ , for whom  $D = 0$  always) and eligible individuals that opted out of YP as their first activity after registration within the considered unemployment duration ( $Z = z^{**}$ ,  $D = 0$ ). The *second* control group is required to compute the correction term and draws exclusively from the population of ineligibles ( $Z = z^*$ ), again reproducing the distribution of the matching variables  $X$  among the treated.

The alternative estimator described in equation (5) includes the instrument in the set of matching variables when computing the standard matching counterfactual. In this case, the *first* control group is that of eligible non-participants ( $Z = z^{**}$ ,  $D = 0$ ) and is drawn exclusively from the population of eligibles who did not enroll in YP as their first activity shortly after registration. The *second* control group is as defined above.

Estimation uses propensity score matching with Epanechnikov kernel weights. The propensity score is estimated on all observable characteristics apart from the instrumental variable (age), namely citizenship, education, region of residence, quarter of entry and labor market history during the year preceding the start of the unemployment spell.<sup>20</sup> Moreover, the instrument age is included in the conditioning set to estimate the standard matching counterfactual using the estimator in equation (5). Matching is performed with replacement in all cases, so each observation in the control group can be used to match multiple treated observations. The choice of bandwidth is based on a global cross-validation exercise. All standard errors are bootstrapped using 200 replications.

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<sup>20</sup>The use of variables describing past (un)employment history in matching is widely regarded as good practice in labor economics as these are possibly the best conveyors of information on unobserved ability and preferences for work (see Heckman, Ichimura and Todd, 1997).

## 3.5 Empirical analysis and results

### 3.5.1 Descriptives

For ease of exposition, we summarize the setting as described above. We explore the cut-off point in eligibility at the 25th birthday and consider the instrument  $Z$  to be 0 or 1 depending on whether the individual is past his 25th birthday at registration or not, respectively. The analysis is restricted to men starting a new registration spell during the strong YP period, from its inception in July 1992 to the start of its phasing down period in September 1994. Only one registration spell is considered for each individual, the first to be observed starting on or after July 1992. So eligibles are men registering for the first time with the employment office during the observation window of July 1992 to September 1994 while aged 24 or younger. Non-eligibles are men registering for the first time with the employment office during the observation window of July 1992 to September 1994 while aged 25 or older.

The *treated group* is composed of eligibles who select into YP as their first activity after registering. We consider alternative treatment groups depending on two dimensions:

1. duration of registration spell prior to enrollment into the YP: up to 3 and 6 months;
2. and distance in days to 25th birthday at registration - up to 6 months, 1 year and 2 years. We decided not to tighten this requirement given the small number of treated observations close to the age cutoff point.<sup>21</sup>

Table 1 reports sample sizes by eligibility and participation status for different age groups. Participants are individuals moving into YP as the first activity after registration, irrespective of time to treatment. Column 3 shows that the number of program participants

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<sup>21</sup>We also estimated the impact of treatment on the sample of individuals as far as 5 years away from their 25th birthday but the ensuing increase in the sample size causes the procedure to become forbiddingly slow when it comes to estimate the precision of the effect. It is also conceivable that our exclusion restriction does not hold for very wide age groups.

increases more than proportionally with distance to 25th birthday. Among individuals within 3 months of turning 25 at registration, only 81, or 1.5% of the eligibles in this group, become participants; this proportion rises to 24.5% among eligibles within 5 years of their 25th birthday. This happens despite the whole population of treated being used and despite the comparatively high take-up rate among eligibles. The explanation may be a mechanical assignment issue. Although YP is the most popular treatment among young individuals in the registrar, eligibles at the verge of completing 25 years of age at inflow have a short time to enroll into the program. On the contrary, younger agents have comparatively more time, and therefore better chances, to be allocated a place.

This variation in participation rates by age is shown in figure 1. It displays the rate of transition into YP by time since registration among individuals aged 24 at the moment they register and depending on whether they are at more (red curve) or less (green curve) than 4 months from completing 25 years of age. The figure shows that participation rates for the youngest cohort is steadily above zero straight from inflow, peaks at 4 months and starts declining after that. It also provides further detail to the pattern described in table 1 showing that, for the older cohort, participation is concentrated over the first months in unemployment and is never as high as for younger cohorts. As a result, the overall hazard rates are much lower for the whole population of 24 years old at inflow (blue curve) than among those younger than 4 months from completing 25 years of age.

This pattern of participation by age creates a gradual decline in participation rates with age at inflow. Figure 2 depicts it. There is no visible discontinuity to be explored. This is not an ideal empirical setting for an application of regression discontinuity. It may also raise concerns that the selection process depends on distance to 25th birthday at registration. In the empirical analysis below we show that our identification assumptions hold and that the estimated effects of YP are robust to changes in the considered age interval and to the exclusion of individuals close to the age cutoff point.

### 3.5.2 Common support

Recovering the ATT and testing the CIA requires the support of  $X$  among the treated to be represented among non-treated ( $D = 0$ ) *and* non-eligibles ( $Z = z^*$ ). If such an extended common support assumption does not hold, the identified effects will represent the treatment effects in the overlapping support region only. To empirically check the common support assumption, figure 3 plots the distribution of the predicted propensity scores by treatment and eligibility status when age is excluded from the covariates set. The population being depicted is that of 24 and 25 years old at registration with the employment office, where treated are individuals moving into YP in the first 6 months of the new registration spell.

Enrollment into treatment seems to be partly dependent on the observable characteristics but the distribution of the propensity score exhibits very little dependence on the eligibility status. In fact, the covariates are relatively balanced between the treated and alternative non-treated groups, even before matching, with a maximum bias of 22%. Matching on the propensity score succeeds in improving balancing for all observables, reducing the bias very substantially in most cases and to a maximum of below 4%.<sup>22</sup>

The estimation excludes observations lying below the highest 5th percentile and above the lowest 99th percentile of the distributions of the propensity scores among treated and comparison groups. This selection procedure restricts attention to the overlapping support while moving away from the lower part of the distribution of the propensity score. Equation (4) justifies this asymmetric trimming of the distribution as the estimates of the correction term can be very imprecise for very low values of the propensity score.

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<sup>22</sup>For the discrete dummy covariates, this test ensures almost perfect alignment between treated and non-treated or non-eligible groups. For continuous covariates, graphical inspection shows that the common support assumption holds for each of them taken independently and that all the post-matching distributions are closer to the distributions among treated. Results available from the authors.

### 3.5.3 Some empirical evidence concerning exclusion restriction (Assumption 2)

Conditional on the matching variables  $X$ , Assumption 2 requires age to have no impact on the potential outcome if untreated. While this is an untestable assumption since only treated outcomes are observed among the treated, we may use observations for non-eligibles to test whether this assumption holds in the absence of YP. We explore two alternative comparisons. The first uses information on spells starting during August 1991, the earliest data period, and compares outflows into all destinations and regular employment after 10 months of registration. This is the latest we observe these individuals before YP is introduced and justifies our choice. The second contrasts outflows after 12 and 24 months of registration for older ineligible individuals during the YP period. We compare men close to either their 26th or 27th birthday. Table 2 shows the results of t-tests for the comparison of means (columns 1 and 2) and Kolmogorov-Smirnov tests for the comparison of the entire distributions of untreated outcomes.<sup>23</sup> All statistics were computed after matching on the set of characteristics used to estimate the treatment effect.

Overall there is no evidence that Assumption 1 is violated in this application, at least when considering populations not exposed to the YP. All  $p$ -values are above the standard 5% significance level and most are well above. However, these results do not exclude the possibility of Assumption 2 being violated once YP is introduced given its age-eligibility rule that in practice means younger individuals can wait longer before enrolling. We explore this possibility below with some sensitivity analysis of our results to different choices of comparison groups.

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<sup>23</sup>We omit test results on outflows after 12 months of registration as they are qualitatively identical to the results on outflows after 24 months. They can be requested from the authors.

### 3.5.4 Results

Our preferred sample comprises males registering with the employment office within 1 year of their 25th birthday while YP is operating in full (between July 1992 and September 1994). This sample is large enough to support precise estimates while ensuring that age differences between eligibles and ineligibles do not compromise the validity of Assumption 2. Among eligibles at inflow, just over 2% (511 observations) flow into YP within 1 month, 5.5% (1,182 observations) within 3 months and almost 9% (1,887 observations) within 6 months of registering with the employment office. Our main estimates use the latter group of participants for the sake of sample size. However, we will also present alternative comparisons using different age groups and unemployment durations before enrolling into YP to ensure our results are not driven by dynamic selection mechanisms related with age.

Table 3 displays the estimates of the ATT on the probability of finding a regular job within 12 and 24 months of registering with the employment office.<sup>24</sup>

Row 1 in the table displays the main set of estimates, based on individuals aged 24

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<sup>24</sup>Estimates for males only. Sample selection criteria varies by row as detailed in row titles. All estimates based on sample of new registrations with the employment office. “Treatment” in rows 1 to 5 stands for flowing into YP within 6 months of registering with employment office as first destination after registration. Row 1 compares treated aged 24 at registration with non-treated aged 24 or 25 at registration. Row 2 restricts the sample to those registering as open unemployed. Row 3 restricts the sample of eligibles to 24 years old at more than 4 months from their 25th birthday at registration. Row 4 restricts the control group in standard matching to the eligibles (aged 24 at registration). Finally, row 5 redefines “treatment” as flowing into YP as first destination within 3 months of registration and compares treated aged 24 at inflow with non-treated aged 24 or 25. The impact of treatment is estimated on the probability of moving into employment within 12 months (columns 1 to 3) and 24 months (columns 4 to 6) of registration. Columns 1 and 4 display standard matching estimates. Columns 2 and 5 display the correction term as specified in the right-hand side of equation (4) or, for row 4, of equation (5). Columns 3 and 6 display the corrected matching estimates using the counterfactuals as specified in equation (4) or, for row 4, in equation (5). The number of observations in column 7 are for the treated group only. Matching on the propensity score using kernel Epanechnikov weights with a bandwidth of 0.02 for a probability ranging in the unit interval. Bootstrapped standard errors based on 200 replications in brackets below the estimate.

and 25 at registration and defining treatment as flowing into YP during the first 6 months as the first destination after registration. Standard matching estimates suggest YP has a null effect on the probability of moving into employment within 12 (column 1) and 24 (column 4) months of registration. The corrected matching estimates corroborate this result when applied to the 12 months' outcome. However, the figure regarding outflows within 24 months of registration is significantly different. The correction term suggests that treated are not randomly selected once observables have been controlled for. Instead, the treated seem to be comparatively better positioned to find a job in the absence of treatment than similar non-treated. This is in line with the fact that treated are encouraged to find their own job positions and that such selection mechanism might dominate in the beginning of the registration spells. The consequence is the large and significantly negative effect of treatment on outflows to employment identified by the corrected matching estimator.

To assess the robustness of this result, we tried several alternative comparisons. Some of the results are displayed in the other rows of Table 3. We restrict the sample to those registering as open unemployed in row 2. We exclude eligibles at less than 4 months of completing 25 years of age in row 3 to control for potential bias arising from older individuals in the eligible group to be rushed into YP while still eligible. And we restrict the control group in standard matching to be composed only of non-treated eligibles in row 4. All results are consistent with those shown in row 1. Only in row 4 are the corrected estimates after 24 months of registering not statistically significant at 5% significance level, but the exhibited pattern is similar to all other cases.<sup>25</sup>

The last row of Table 3 considers treatment to be 'starting YP within first 3 months after registering'. If dynamic selection issues were important at these relatively short durations, we would expect the results to show some response to such change in the definition

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<sup>25</sup>We also considered using other groups: restricting the sample to Swedish citizens; focusing on individuals with vocational training only (the largest educational group with registration spells); and including exits to registered employment as a positive outcome. All results are consistent with those displayed in Table 3 and are available from the authors under request.

of treatment. However, estimates in row 5 are very similar in size and pattern to those displayed in the other rows of the same table. The robustness of these results suggests our preferred time window is sufficiently narrow to keep time of treatment exogenous in this analysis.

We further investigate the sensitivity of these results to age in Table 4 by varying the width of the age interval around the 25th birthday at registration. Columns 1 to 3 display estimates of the effect of YP on the odds of finding a job within 24 months of registration. For comparison purposes, the first row repeats the last three columns in the first row of Table 3. The following two rows display results for the population of men up to 2 years (row 2) and half year (row 3) away from their 25th birthday at registration. Neither widening or narrowing the age interval changes the pattern of the results. However, results in row 3 are substantially larger but very imprecisely estimated given the small sample size.

Columns 4 to 6 display results on an alternative outcome, deregistration within 24 months of first registering. The classical matching estimate for 24-25 years old (row 1, column 4) suggests a negative overall impact of the program, maybe due to an extended lock-in effect or to the extension of eligibility to benefits as a consequence of treatment take-up.<sup>26</sup> A similar result holds for 23-26 (row 2). In both cases, however, the correction points to the opposite direction and the resulting effect is found to be positive and statistically significant when the larger age group is used (row 2). Again here, sample size precludes a clear pattern to emerge from the analysis of the narrower age group (row 3).<sup>27</sup>

Results for both outcomes are considerably stable across age groups. Such lack of variation is consistent with an homogeneous effect of treatment by age for the interval being considered.

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<sup>26</sup>The Swedish welfare system provides unemployment insurance for a limited amount of time after a transition from employment into unemployment. However, this period can be extended by participation in the programs made available by the employment offices, of which YP is one example. Repeated participation would, in principle, allow the unemployed to remain out of work and on benefits indefinitely.

<sup>27</sup>We have estimated effects on all outflows on other samples as in table 3 and applying the alternative estimator as defined in equation (5). Results are similar to those discussed here.

Overall, both tables suggest that standard matching may not be identifying the correct causal effect of interest (i.e., the ATT). Standard-matching results suggest that the program has no effect on the probability of finding a job and a small negative effect on the overall odds of leaving unemployment. Correcting for the potential selection bias in matching changes the picture quite substantially. The program seems to strongly reduce employment take-up in the medium run, after 24 months of registration. With regard to the overall impact on the odds of leaving unemployment, our estimation strategy suggests YP has either a zero or a small positive effect, within the same time frame. Analysis of other outcomes suggest that the possible positive effect of YP on deregistration is driven by exits into formal education (these estimates are available under request from the authors).<sup>28</sup>

## 4 Conclusion

We have developed and applied an evaluation method for the effects of program participation (or policy exposure) on individual outcomes, if participation is selective but individuals are ineligible in case of a certain value of some observed instrumental variable. From a practical point of view this is a common setting, in particular for active labor market policies for young individuals. In those cases, participation may be selective because individuals

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<sup>28</sup>Swedish subsidized work programs have been the focus of other studies. In particular, Sianesi (2004) analyzes the overall impact of the Swedish ALMP system and the differential impact of each of the numerous available treatments for adults (so this excludes YP). She finds that subsidized employment is the best performer in terms of moving unemployed back into work, and that the positive effect of subsidized employment seems to last. All other programs have either a zero or a negative impact, possibly arising through the renewed eligibility to benefits as a consequence of program participation. Larsson (2003) studies specifically the effects of YP on exits to employment and finds negative effects 12 months after treatment using standard matching techniques. More generally, youth programs have often shown disappointing results (see surveys by Heckman, LaLonde and Smith, 1999, and Bergemann and Van den Berg, 2008). A noticeable positive exception are programs that mix improved job-search assistance and tougher job-search monitoring such as the British New Deal for Young People (e.g. Blundell et al., 2004, De Giorgi, 2005, Anderson, 2000, Van den Berg et al., 2004, and Van den Berg and Van der Klaauw, 2006).

can choose between different programs and/or because the duration until enrollment is not deterministically set. Program participation is only possible if the individual is aged below a certain age. With selective participation, if the CIA is violated, matching cannot be used. For the same reason, one cannot simply compare those below the threshold who are treated to those above the threshold (who are all non-treated). However, our novel method, which exploits the eligibility boundary restriction within the matching framework, provides consistent estimates of the average treatment effect on those who are treated.

Our approach relies critically on the availability of an instrument satisfying Assumptions 2 and 3 in Section 2. Assumption 3 is automatically satisfied in our practical application of a policy that allows for selective participation only on certain values of an observed variable. To obtain precise estimates of our correction term, however, we also require a reasonable take-up of treatment to ensure that the participation probability,  $P(D = 1 | X)$ , is not too close to zero as estimating the correction term requires dividing by this probability.

The application to the Swedish Youth Practice program shows that our method can deliver evaluation results that differ from those based on standard matching methods. The standard matching estimates for the effect on re-employment are zero, whereas the estimates based on our method can be negative. Using age as instrument, the difference between the estimates is systematically significant when the outcome of interest is “finding a job within 24 months of becoming unemployed”. The effects on the overall exit probabilities out of unemployment are invariably estimated to be smaller than those based on matching, although the differences here are not significant. Our results confirm suspicions that treated differ systematically from comparable non-treated, suggesting they are drawn from a more employable group. As a result, we are more pessimistic about the effect of subsidized work on the rate of finding work than if we had incorrectly based ourselves on the matching estimates. Other the other hand, the overall exit rates from unemployment are less negative (even possibly positive) than if we had relied on standard matching alone. The latter result mostly involves outflows into formal education. From a policy point of view, our results suggest that perhaps the optimism about the use of subsidized work

programs to bring unemployed youth back to work should be tempered.

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## Appendix: Data cleaning and selection

Händel is an administrative dataset comprising information on all registered unemployment spells from August 1991 onwards. It details longitudinal information on the whole population of registered spells, including any undertaken treatments, the history of earned subsidies, destination on leaving the registrar and demographics such as age, citizenship, education and usual occupation.

The main obstacle in using Händel is the frequency of negative and overlapping spells. We have dealt with these occurrences in a conservative way to minimize any resulting bias introduced by data handling.

To start with, we created a condensed variable describing labor market status while in registrar. The four broad categories considered are: unemployment, registered employment, YP, all other possible treatments. Using these, we collapsed all overlapping spells in the same broad category. Spells in different broad categories overlapping by no more than 2 weeks were corrected by setting the exit date of the earliest equal to the entry date of the latest as exit dates are generally more imprecise. Zero duration spells were discarded. At last, individual histories with a remaining error were censored from the time of the error onwards and a censoring indicator was created to correct estimates for the possibility of censored histories.

Data selection followed a number of criteria. First, we used only males. Then we selected individuals starting a new registered *unemployment* spell during the period YP was more popular, between July 1992 and September 1994. Of all the selected spells, we kept only the first one and followed the corresponding individuals over time to find out about treatment take up and labor market outcomes. We considered individuals aged between 20 and 29 at the time of registration and classified as eligibles those aged 24 or younger.

In running the estimation procedure, we also focused on more narrowly defined groups as defined by the following variables: (*i*) Distance to 25th birthday at inflow: less than 2 years, less than 1 year, between 12 and 4 months (this latter condition applies to eligibles

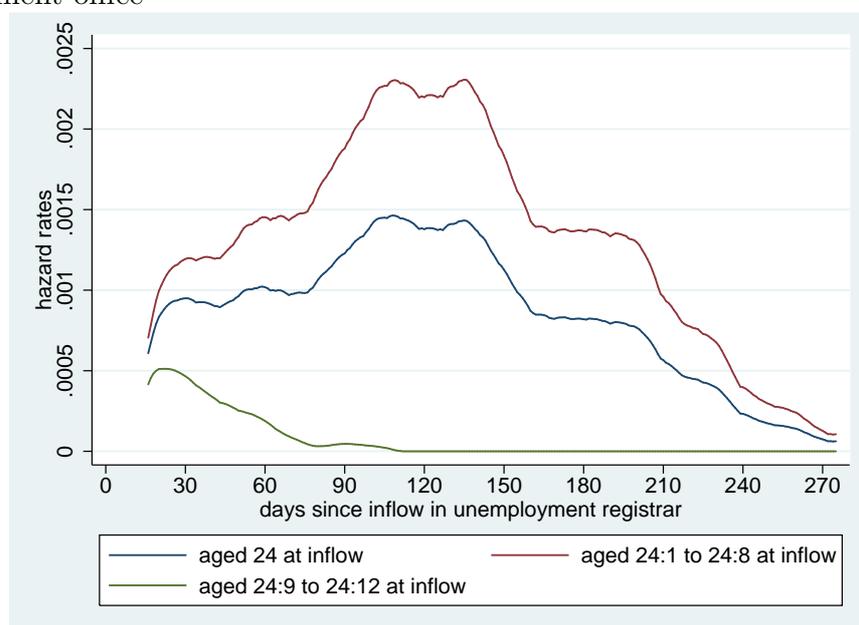
only, and compares with ineligibles less than 1 year away from 25th birthday at inflow); (*ii*) Nationality: Swedish nationals only or all new unemployed; (*iii*) Employment status at registration: whether or not registering as open unemployed; and (*iv*) Education attainment: vocational training or all levels of education.

Table 1: Number of observations by age group and eligibility/treatment status; instrument is age; men only

Distance to 25th birthday at inflow	ineligibles	eligibles (under 25)		Total
	(over 25)	non-participants	participants	
	(1)	(2)	(3)	(4)
(1) up to 3 months	5,444	5,240	81	10,765
(2) up to 6 months	11,029	10,157	448	21,634
(3) up to 1 year	21,950	19,428	2,029	43,407
(4) up to 2 years	43,683	37,118	6,064	86,865
(5) up to 5 years	102,450	112,501	32,528	247,479

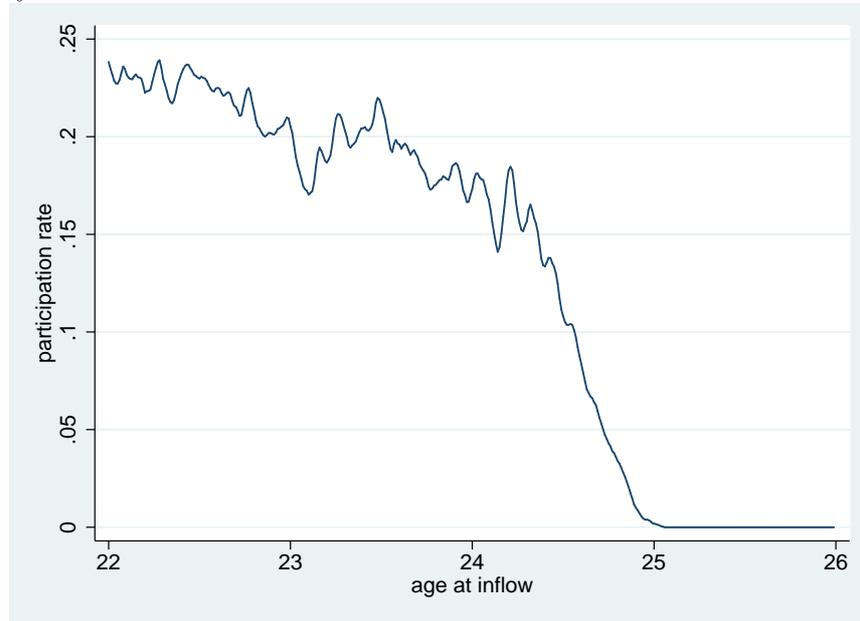
Notes: Population of males close to the 25th birthday when registering with the employment office between July 1992 and September 1994. Age groups defined by distance to 25th birthday when first registering within the time frame. Each individual is represented only once in the sample, the first registering. Eligibles (ineligibles) are those aged 24 and below (25 and above) at registration. Participants are those taking YP as the first event after registration.

Figure 1: Hazard rates into YP by duration of unemployment spell and age at registration with employment office



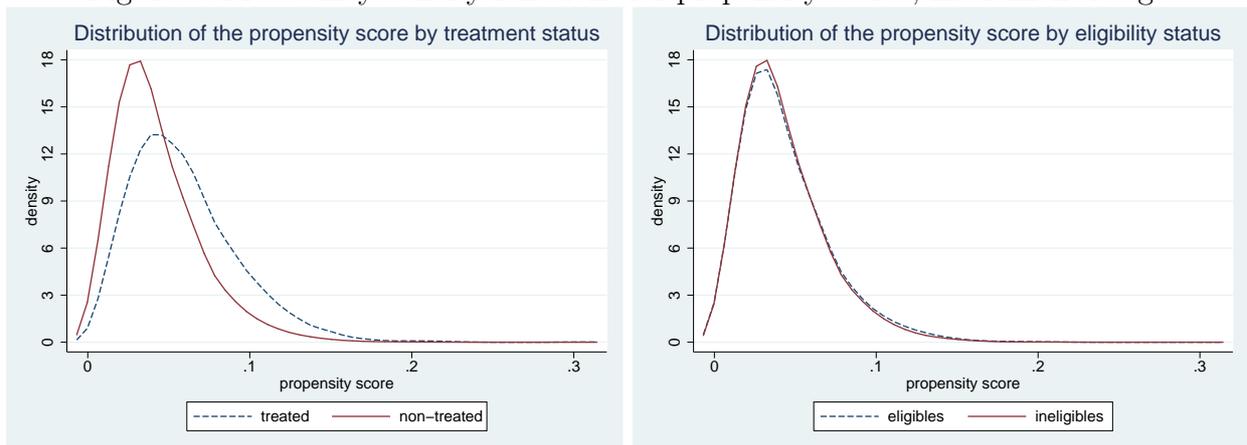
Notes: Plotted curves are smoothed Kaplan Meyer hazard rates using Local Linear Regression with a bandwidth of 15 days. Population of males aged 24 when registering with employment office for the first time between July 1992 and September 1994.

Figure 2: Probability of participation by age at inflow into new registered unemployment spell; men only



Notes: Population of males aged 22 to 26 when registering with the employment office for the first time between July 1992 and September 1994. 'Participation' means flowing into YP as first event after registration.

Figure 3: Probability density functions for propensity scores; instrument is age



Notes: Plotted curves are probability density functions of propensity scores estimated on the population of men aged 24 and 25 when registering with the employment office between July 1992 and September 1994. Treated are 24 years old moving into YP as first destination within 6 months of inflow. Non-treated are 24 and 25 years old not participating in YP as first event within 6 months of inflow. Eligibles (ineligibles) are those aged 24 (25) at registration.

Table 2: P-values for tests of Assumption 1; instrument is age

		equality of means		equality of distribution		
		deregistration	employment	deregistration	employment	observ.
		(1)	(2)	(3)	(4)	(5)
Outflows after 10 months of registration for spells starting during Aug 1991						
Comparing men aged 24 or younger with men aged 25 or older at registration						
(1)	24 versus 25	0.082	0.814	0.990	0.605	3,959
(2)	24:7-12 vs 25:1-6	0.826	0.516	0.938	0.843	1,928
(3)	24:1-8 vs 25	0.100	0.898	0.591	0.266	3,323
(4)	23-24 vs 25-26	0.091	0.910	0.948	0.988	7,913
Outflows after 24 months of registration for spells starting during YP period						
Comparing men aged close to 25 or 26 at registration						
(5)	25 versus 26	0.264	0.289	0.385	0.811	27,625
(6)	25:7-12 vs 26:1-6	0.138	0.904	0.961	0.401	13,921
(7)	25-26 vs 27-28	0.739	0.927	0.071	0.996	54,458

Notes: Columns 1 and 2 display the p-values for the difference in means. Columns 3 and 4 display the p-values for the Kolmogorov-Smirnov test of equality of distributions. Rows 1 to 4 use registration spells starting in August 1991 and follows them for 10 month, till just before the launch of YP. Rows 5 to 7 use registration spells of older individuals during the YP period. Bootstrapped standard errors using 200 replications.

Table 3: ATT on the outflows to regular employment

Outcome: employment							
12 months after registration			24 months after registration			nr of observ.	
classical matching	correction term	adjusted matching	classical matching	correction term	adjusted matching		
(1)	(2)	(3)	(4)	(5)	(6)	(7)	
<i>Treated: 24 years old moving into YP within 6 months of registration</i>							
<i>Controls: ineligible (25 years old) and eligible (24 years old) non-treated</i>							
(1)	-0.009 (0.011)	-0.015 (0.045)	0.006 (0.047)	-0.008 (0.012)	0.096* (0.048)	-0.104* (0.050)	1,699
<i>Treated: 24 years old moving into YP within 6 months of registration - open unemployed</i>							
<i>Controls: ineligible (25 years old) and eligible (24 years old) non-treated - open unemployed</i>							
(2)	-0.012 (0.011)	-0.022 (0.045)	0.010 (0.048)	-0.010 (0.013)	0.100* (0.050)	-0.109* (0.051)	1,606
<i>Treated: 24:1 to 24:8 years old moving into YP within 6 months of registration</i>							
<i>Controls: ineligible (25 years old) and eligible (24:1 to 24:8 years old) non-treated</i>							
(3)	-0.011 (0.012)	-0.002 (0.036)	-0.008 (0.038)	-0.004 (0.014)	0.070* (0.035)	-0.073* (0.036)	1,579
<i>Treated: 24 years old moving into YP within 6 months of registration</i>							
<i>Controls: eligible (24 years old) non-treated</i>							
(4)	-0.015 (0.013)	-0.041 (0.048)	0.026 (0.047)	-0.007 (0.014)	0.042 (0.052)	-0.049 (0.051)	1,563
<i>Treated: 24 years old moving into YP within 3 months of registration</i>							
<i>Controls: ineligible (25 years old) and eligible (24 years old) non-treated</i>							
(5)	0.006 (0.014)	0.005 (0.070)	0.001 (0.071)	0.004 (0.017)	0.154* (0.075)	-0.150* (0.075)	1,049

Notes: Standard errors in parenthesis below the estimate.

\* : Statistically different from zero at 5% significance level. See footnote 24 for additional explanations.

Table 4: ATT on outflows to employment and deregistration within 24 months of registration

	Outcome: employment			Outcome: deregistration			
	classical matching	correction term	adjusted matching	classical matching	correction term	adjusted matching	nr of observ.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Treated: 24 years old moving into YP within 6 months of registration</i>							
<i>Controls: ineligible (25 years old) and eligible (24 years old) non-treated</i>							
(1)	-0.008 (0.012)	0.096* (0.048)	-0.104* (0.050)	-0.032* (0.012)	-0.075 (0.049)	0.043 (0.051)	1,699
<i>Treated: 23-24 years old moving into YP within 6 months of registration</i>							
<i>Controls: ineligible (25-26 years old) and eligible (23-24 years old) non-treated</i>							
(2)	0.006 (0.008)	0.082* (0.028)	-0.076* (0.028)	-0.031* (0.008)	-0.151* (0.030)	0.120* (0.030)	4,468
<i>Treated: 24:7 to 24:12 years old moving into YP within 6 months of registration</i>							
<i>Controls: ineligible (25:1 to 25:6 years old) and eligible (24:7 to 24:12 years old) non-treated</i>							
(3)	-0.001 (0.026)	0.187 (0.152)	-0.188 (0.154)	-0.011 (0.026)	0.098 (0.147)	-0.109 (0.150)	401

Notes: Estimates for males only. Sample selection criteria varies by row as detailed in row titles. All estimates based on sample of new registrations with the employment office. "Treatment" stands for flowing into YP within 6 months of registering with employment office as first destination after registration. Row 1 compares treated aged 24 at registration with non-treated aged 24 or 25 at registration. Row 2 uses the sample of individuals at less than 2 years from 25th birthday on registration. Row 3 uses only individuals at less than 6 months from their 25th birthday at registration. The impact of treatment is estimated on the probability of moving into employment within 24 months (columns 1 to 3) and deregistering within 24 months (columns 4 to 6) of inflow. Columns 1 and 4 display standard matching estimates. Columns 2 and 5 display the correction term as specified in the right-hand side of equation (4). Columns 3 and 6 display the corrected matching estimates using the counterfactuals as specified in equation (4). Matching on the propensity score using kernel Epanechnikov weights with a bandwidth of 0.02 for a probability ranging in the unit interval. Bootstrapped standard errors based on 200 replications in brackets below the estimate.

\* Statistically different from zero at 5% significance level.