Long-run effects of active labor market policy programs*

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Abstract

This paper proposes and implements a new method to estimate treatment effects in settings where treatments could start at any point in time. We consider evaluation under unconfoundedness and study effects on an outcome experienced some time after the start of the program. The causal comparison is between treatment and no treatment. An important example is the effects of training for unemployed individuals on earnings some years after the program. We show that various average effects are identified and propose and implement inverse probability weighting estimators. The key innovation is that we show that the weights given to the outcome response of the non-treated should depend on both observed characteristics and time in unemployment. To illustrate this new approach, we study the long-run effects of a training program for unemployed individuals in Sweden. After an initial locking-in period the training program increases labor earnings for at least 10 years after the program. We also compare the new estimator with related estimators.

Keywords: Treatment effects; dynamic treatment assignment; program evaluation; training; unemployment duration

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1 Introduction

In cases with rich register and/or survey data training programs and other active labor market policy (ALMP) programs are often evaluated under unconfoundedness. By now there is extensive methodological evidence on various aspects of evaluation under unconfoundedness or conditional independence, such as construction of efficient and robust estimators, balancing tests and common support restrictions (see e.g. Imbens and Wooldridge 2009). A very large part of this literature considers a static evaluation problem, in which the assignment problem is a choice between taking and not taking treatment. However, in evaluations of ALMP programs for the unemployed the static framework has several limitations, because very often the program could start after many different elapsed unemployment durations. This dynamic treatment assignment means that the current non-treated unemployed might become treated later on, and this complicates the selection of a proper control group. This problem has been recognized in several recent papers (Lechner 1999; Sianesi 2004; Fredriksson and Johansson 2003; Crépon et al. 2009; Lechner et al. 2011; Kastoryano and van der Klauww 2011; Biewen et al. 2014).

The intuition behind the dynamic evaluation problem is particularly clear if the interest lies in the earnings effects of a training program for unemployed individuals. Naturally, outcome responses of the unemployed that participate in the program inform us about the average earnings under treatment, while only the non-treated unemployed provide information about the counterfactual outcome under no treatment. In a dynamic evaluation setting the non-treated are those that leave unemployment without participating in the program. The dilemma is that this non-treated group constitute a selected sample, since the treatment status depends on time in unemployment. The implication is that unemployed with relatively short unemployment durations will be over-represented among the non-treated, and this creates selection even if treatment assignments are unconfounded in each time period.

In this paper, we propose a new way to resolve this dynamic treatment assignment dilemma. Specifically, we are interested in the average effects of a treatment given in an initial state on an outcome experienced some time after the treatment. The key feature is that exits from the initial state as well as the start of the treatment could occur at any point in time. The training program for unemployed is a key example, in which the initial state is unemployment and the outcome of interest could be, for instance, labor earnings or employment status some years after the start of the unemployment spell.

This captures the medium- and long-run effects that are of key interest for comprehensive evaluations of training and other ALMP programs. Note that several studies document that most ALMP programs have locking-in effects during the actual program, leading to negative employment effects in the short-run. If the training improves participants' human capital the program is likely to have positive employment effects, which might lead to a much steeper income trajectory.[1] The medium- and long-run earnings and employment effects are, thus, crucial in evaluations of ALMP programs.

Our main contribution is that we introduce and implement inverse probability weighting (IPW) estimators for various average effects. Besides unconfoundedness and no-anticipation[2] we impose no additional structure on the assignment process. We provide both identification results and investigate the theoretical properties of our estimator in detail. Our simulation results confirm that the IPW estimator is unbiased and that the proposed bootstrap inference works well. The estimator is implemented using Swedish data that allows us to follow each unemployed individual in our sample for 13 years after they entered unemployment.

Next, consider the intuition behind our estimator. From the long-run earnings response of the treated at t we directly obtain the average earnings under treatment. Under unconfoundedness we can adjust for differences between treated and non-treated at t. However, in subsequent time periods some of the previously non-treated becomes treated, and this creates selection in the group of non-treated for two main reasons: (1) individuals with a large probability to be treated in subsequent periods are less likely to remain non-treated, and (2) for a given period by period treatment probability, individuals with longer unemployment durations are less likely to remain non-treated. Our IPW estimator corrects for both sources of selection in a very straightforward way. The innovation is that we show that the weights given to the outcome response of the non-treated should depend on both the observed characteristics and time in unemployment. The outcomes of unemployed with relatively long unemployment durations and individuals with characteristics that makes them more likely to enter treatment are given larger weight. This restores the balance between the treated population at t and the control group consisting of the unemployed that are non-treated throughout the entire unemployment spell.

[1]This is also partly supported by recent evidence on long-run effects of ALMP programs (Fitzenberger et al. 2008; Lechner et al. 2011).

[2]See e.g. Abbring and van den Berg (2003), Abbring and Heckman (2008) and Crépon et al. (2013) for discussions of the role of no-anticipation.

Our IPW approach is related to several strands of the literature. A large number of empirical papers consider the same setting as in this paper. Two very often used approaches are the hypothetical treatment durations approach and the treatment now versus waiting for treatment approach. The former originates from Lechner (1999, 2002) and is further refined by e.g. Wunsch and Lechner (2008) and Lechner et al. (2011). In this approach hypothetical treatment durations are generated for each non-treated individual[3], but we have found no full theoretical justification of this approach. The treatment now versus waiting for treatment approach was introduced by Sianesi (2004) and has been used by for example Sianesi (2008), Fitzenberger et al. (2008) and Biewen et al. (2014). For a given pre-treatment duration, this estimator uses individuals who are not treated at this duration but possibly treated later on as control group. This transforms the dynamic problem back to a static problem, but this is achieved by redefining the treatment effect.

In a recent study Vikström (2014) considers the same dynamic setting and proposes IPW estimators of average effects on the survival rate in unemployment. Here, we consider the medium- and long-run effects on outcomes experienced in a specific time period, for instance some years after the program. This not only affects the outcome measure, but also the way the control group is constructed. In Vikström (2014) the control group in a certain time period consists of all not-yet treated in that time period, whereas here the control group always consists of the non-treated, i.e. those that leave unemployment without participating in the program. For that reason, the structure of the weights also differs. Fredriksson and Johansson (2008) and Crépon et al. (2009) also consider the effect on the survival rate in unemployment.

Our paper is also related to Lee (2009) and Lechner and Melly (2007) that both consider effects on post-unemployment outcomes, such as the wage rate. If the interest lies in effect on the wage rate, one problem is that wages are only observed if the formerly unemployed individual is employed. For that reason, both Lee (2009) and Lechner and Melly (2007) propose different conditions under which the effect on the wage rate could be bounded. In this paper, we focus on long-run outcomes that are observed for everyone. This holds, for example, for employment status and labor earnings, since any non-employed individual by definition has zero labor earnings.

[3] In the most recent versions of the approach log treatment durations for the treated are regressed on the covariates. The estimated coefficients are then used together with a random draw from estimated residuals from that regression to construct the hypothetical starting dates for the non-treated. Finally, the actual and the hypothetical treatment dates are then used as covariates when estimating the propensity score.

Finally, a number of recent papers extend the Timing-of-Events (ToE) approach by Abbring and van den Berg (2003) to consider post-unemployment outcomes such as time in employment and wage rates (see e.g. Arni et al. 2013). In these papers the treatment is also allowed to occur at any point in time. The main difference compared to this paper is that the ToE approach allows the selection into treatment to be based on both observed and unobserved heterogeneity. This is achieved at the expense of imposing the mixed proportional hazard structure, whereas the IPW approach in this paper requires no additional structure on the assignment process beyond unconfounded treatment assignment.

2 Model

In this paper, we are interested in average effects of a treatment given in an initial state on long-run outcomes experienced after the individual left the initial state. The key feature is that exits from the initial state as well as start of the treatment could occur at any discrete point in time. Below we show that this dynamic nature of the treatment assignment give rise to a number of interesting evaluation issues. One typical example that will be used throughout this paper is evaluation of training programs for unemployed. In that setting, the initial state is unemployment and the long-run outcome could be, for instance, labor earnings or employment status some years after program. Training programs also often have the dynamic structure that is discussed in this paper, since case workers often could enrol unemployed into training after many different unemployment durations.

Formally, time to treatment is denoted by T_s. The potential time in the initial state if treated at t_s is denoted by $T_u(t_s)$. Note that $T_u(\infty)$ can be viewed as the potential duration if "never treated". The observed time in the initial state is denoted by T_u. We also have the long-run potential outcome $Y(t_s)$ (also $Y(\infty)$) with observed value Y. Finally, the observed covariates are denoted by X.

We consider several average effects. The first parameter of interest is the average treatment effect on the treated of treatment at t_s

$$\text{ATET}(t_s) = E(Y(t_s)|T_s = t_s, T_u(t_s) \geq t_s) - E(Y(\infty)|T_s = t_s, T_u(t_s) \geq t_s). \quad (1)$$

This effect contrasts treatment at t_s, say training after 6 months, with never being treated. It captures the average effect of removing the entire training program. Besides the average treatment effect on the treated one might also consider the average effect on all survivors at t_s

$$\text{ATE}(t_s) = E(Y(t_s)|T_s \geq t_s, T_u(\infty) \geq t_s) - E(Y(\infty)|T_s \geq t_s, T_u(\infty) \geq t_s). \tag{2}$$

Both ATET(t_s) and ATE(t_s) capture the average effect for a certain pre-treatment duration. In section 4.1, we discuss estimation of the overall effect, i.e. the aggregated effect averaged over all (or a set of) pre-treatment durations.

In the training program for unemployed example ATET(t_s) and ATE(t_s) capture the sum of two different effects: (1) a direct effect, if the training program improves the participants human capital and if the improved human capital affects labor earnings. (2) An indirect effect, if program participation affects time in unemployment and if time in unemployment has an independent effect on subsequent earnings. Several surveys, including e.g. Heckman et al. (1999), Kluve (2010), and Card et al. (2010), document that many training programs are associated with substantial locking-in effects during the actual program and in some cases increased exit rates after the program has been completed. In this paper, we do not attempt to separate the direct effect and the indirect effect.

3 The evaluation problem

We consider identification under unconfoundedness

$$T_s \perp T_u(t_s), Y(t_s) \quad , for all t_s > 0 \quad |X. \tag{3}$$

That is conditional on a set of observed covariates treatment assignment is independent of both the potential durations in the initial state and the potential long-run outcomes. This assumption and similar conditional independence assumptions are often used in evaluations of training programs. Often the argument is that case workers are very influential in the assignment process and that the data set used in the analysis contains most of the information available when case workers decided upon enrollments.

Another key assumption is no-anticipation, which implies that any future treatment does not affect current outcomes. Formally,

$$\Pr(T_u(t') = t) = \Pr(T_u(t'') = t) \quad , \forall t < \min(t', t''). \tag{4}$$

The assumption is fulfilled if individuals are unaware of future treatments or if they do not alter their behavior as a response to knowledge of future treatments. The importance of this assumption for evaluations in dynamic settings was highlighted by Abbring and van den Berg (2003), and subsequently discussed by e.g. Abbring and Heckman (2008) and Crépon et al. 2009.

Under no-anticipation we have the following observational rule for the long-run outcome

$$Y = \sum_{t_s=1}^{T_u} [I(T_s = t_s)Y(t_s)] + I(T_s > T_u)Y(\infty). \tag{5}$$

The first part of (5) tells us that if the individual starts training before finding employment we observe the long-run potential outcome corresponding to the actually observed time to treatment. From the second part of (5) we have that if an individual exits the initial state without being treated the observed outcome equals the outcome if "never treated". Note that this only holds under no-anticipation, because if future treatments affect current job finding rates the observed outcome under $T_s > T_u$ corresponds to a treated duration.

Next, consider the evaluation problem. First, from the observed outcome of those treated at t_s we have

$$E(Y(t_s)|T_s = t_s, T_u(t_s) \geq t_s) = E(Y|T_s = t_s, T_u \geq t_s), \tag{6}$$

i.e. the first part of ATET(t_s) is directly identified from the outcomes of those actually treated at t_s. In the same way the first part of ATE(t_s) could be obtained by reweighting the outcomes of those actually treated at t_s in order to mimic the outcomes of all survivors at t_s.

Instead, the evaluation problem concerns the counterfactual outcome $E(Y(\infty)|T_s = t_s, T_u(t_s) \geq t_s)$. The observational rule in (5) illustrates the main issues. First, it is clear that only non-treated individuals, i.e. individuals with $T_s > T_u$, are informative about the counterfactual outcome under never treatment. Individuals that are not treated at t_s, but are treated later on provide no information about the counterfactual outcome under no-treatment, since because they are treated the observed response, Y, corresponds to a potential outcome under treatment.

Second, the potential control group of non-treated, defined by $T_s > T_u$, in general, constitute a selected sample. The reason is that static treatment status, $I[T_s > T_u]$, depends on survival time (i.e., the outcome), since the probability of treatment enrollment by construction increases with the time in unemployment. This problem has been acknowledged in a number of papers analyzing the effects of a treatment on time in the initial state (see discussions in e.g. Sianesi 2004, Fredriksson and Johansson 2008, Crépon et al. 2009 and Vikström 2014). The implication is that individuals with relatively short unemployment durations will be overrepresented in the group of non-treated. Thus, even if treatment assignment at t_s is unconfounded treatment assignment in subsequent time periods create selection in the group of non-treated.

In this paper, we propose a completely new approach to correct for this selection in the group of non-treated. This leads to a inverse probability weighting (IPW) approach that re-weights the outcome of the "never-treated". Before introducing the IPW estimator we consider identification in detail. Initially,

$$E(Y(\infty)|T_s = t_s, T_u(t_s) \geq t_s) = E(Y(\infty)|T_s = t_s, T_u \geq t_s) = \tag{7}$$

$$E_{X|T_s=t_s,T_u\geq t_s} E(Y(\infty)|T_s = t_s, T_u \geq t_s, X),$$

where the first equality holds under Assumption 4 and second equality follows using the law of iterated expectations. Then, under Assumption 3

$$E(Y(\infty)|T_s = t_s, T_u \geq t_s, X) = E(Y(\infty)|T_s > t_s, T_u \geq t_s, X).$$

Next,

$$E(Y(\infty)|T_s > t_s, T_u \geq t_s, X) = \tag{8}$$

$$\begin{aligned} \Pr(T_u &= t_s|T_s > t_s, T_u \geq t_s, X)E(Y(\infty)|T_s > t_s, T_u = t_s, X) + \\ \Pr(T_u &> t_s|T_s > t_s, T_u \geq t_s, X)E(Y(\infty)|T_s > t_s, T_u > t_s, X). \end{aligned}$$

That is the counterfactual outcome under never treatment could be decomposed into average outcomes for individuals with $T_u = t_s$ and $T_u > t_s$. The former group, with $T_u = t_s$, in (8) consists of non-treated individuals that exit directly in period t_s. For this group we have using the observational rule

$$E(Y(\infty)|T_s > t_s, T_u = t_s, X) = E(Y|T_s > t_s, T_u = t_s, X).$$

Also, note that the probabilities $\Pr(T_u = t_s|T_s > t_s, T_u \geq t_s, X)$ and $\Pr(T_u > t_s|T_s > t_s, T_u \geq t_s, X)$ are observed. For the latter group, with $T_u = t_s$, in (8), i.e. those that survives at least one additional time period, we have using Assumption 3

$$E(Y(\infty)|T_s > t_s, T_u > t_s, X) = E(Y(\infty)|T_s > t_s + 1, T_u \geq t_s + 1, X). \tag{9}$$

Next, using (8) by replacing t_s with $t_s + 1$ we have

$$E(Y(\infty)|T_s > t_s + 1, T_u \geq t_s + 1, X) =$$

$$\begin{aligned}
\Pr(T_u &= t_s + 1|T_s > t_s + 1, T_u \geq t_s + 1, X)E(Y(\infty)|T_s > t_s + 1, T_u = t_s + 1, X) + \\
\Pr(T_u &> t_s + 1|T_s > t_s + 1, T_u \geq t_s + 1, X)E(Y(\infty)|T_s > t_s + 1, T_u > t_s + 1, X),
\end{aligned}$$

where using the same reasoning as above all parts of this expression are observed except $E(Y(\infty)|T_s > t_s + 1, T_u > t_s + 1, X)$. However, for this outcome we can iteratively use (9) and (8) for $t_s + 2, \ldots, T_u^{\max}$, where $T_u^{\max}$ is maximum time in the initial state. This gives

$$E(Y(\infty)|T_s = t_s, T_u \geq t_s, X) = \sum_{k=t_s}^{T_s^{\max}} f(k, X)E(Y|T_s > k, T_u = k, X) \tag{10}$$

with the observed probabilities

$$\begin{aligned}
f(k, X) &= \Pr(T_u = t_k|T_s > t_k, T_u \geq t_k, X) \quad , k = t_s \\
f(k, X) &= \Pr(T_u = t_k|T_s > t_k, T_u \geq t_k, X) \prod_{m=s}^{k-1} \Pr(T_u > t_m|T_s > t_m, T_u \geq t_m, X) \quad , k > t_s.
\end{aligned}$$

Thus, using (6) for the first part of (1), and (7) and (10) for the first part of (1) we have

$$\text{ATET}(t_s) = \tag{11}$$

$$E(Y|T_s = t_s, T_u \geq t_s) - E_{X|T_s=t_s,T_u \geq t_s} \sum_{k=t_s}^{T_s^{\max}} f(k, X)E(Y|T_s > k, T_u = k, X).$$

In sum, the identification results in this section, shows that the non-treated group consisting of individuals leaving the initial state before becoming treated or actually never would have become treated could be used to identify the counterfactual outcome for those treated after a certain elapsed duration. Equation (11) also indicates that this could be achieved by giving individuals leaving the initial after different elapsed durations differential weight. The identification results above also demonstrates the independent use of the unconfoundedness assumption and the no-anticipation assumption. The selection on observables assumption relates to the allocation of treatment across individuals, and assures that the treated and the not-yet treated have similar potential outcomes. The no-anticipation assumption concerns the relationship between different potential outcomes for a given individual, and assures that the outcomes the non-treated could be used to mimic the outcomes under never treatment even if some of the non-yet treated would have been treated if they remained in the initial state.

4 Estimation

The identification results above show that average effects could be identified using the outcomes of treated individuals and individuals who remain untreated the entire time in the initial state. The latter group, also referred to as the non-treated, however as the identification results show, constitutes a selected group. The selection arises since individuals not treated at t_s might become treated at $t_s + 1, \ldots$. This creates selection for two main reasons: (1) Individuals with a large probability to be treated in subsequent periods are less likely to remain non-treated. (2) For a given period by period treatment probability, individuals with longer unemployment durations are less likely to remain non-treated. The first source of selection is similar to the static evaluation problem, whereas the second problem arises purely because of the dynamic nature of the evaluation problem. However, it is important to notice that in both cases it is the period by period treatment assignment that creates the selection. Under unconfounded treatment assignment it is therefore possible to construct an estimator that corrects for both sources of selection. Specifically, in the appendix we show that unbiased estimators of $ATET_t(s)$ and $ATET_t(s)$ are:

$$\widehat{\text{ATET}}(t_s) = \frac{1}{N^1_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} Y_i - \tag{12}$$

$$\frac{1}{\sum_{i \in T_{s,i}>T_{u,i}, T_{u,i} \geq t_s} w^{t_s,ATET}(T_{u,i}, X_i)} \sum_{i \in T_{s,i}>T_{u,i}, T_{u,i} \geq t_s} w^{t_s,ATET}(T_{u,i}, X_i) Y_i$$

and

$$\widehat{\text{ATE}}(t_s) = \frac{1}{\sum\limits_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} w^{t_s}(X_i)} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} w^{t_s}(X_i) Y_i - \tag{13}$$

$$\frac{1}{\sum\limits_{i \in T_{s,i}>T_{u,i}, T_{u,i} \geq t_s} w^{t_s,ATE}(T_{u,i}, X_i)} \sum_{i \in T_{s,i}>T_{u,i}, T_{u,i} \geq t_s} w^{t_s,ATE}(T_{u,i}, X_i) Y_i,$$

where $N_{t_s}^1$ is the number of treated at t_s, and

$$\begin{aligned} w^{t_s}(X_i) &= \frac{1}{p(t_s, X_i)} \\ w^{t_s,ATET}(T_{u,i}, X_i) &= \frac{p(t_s, X_i)}{1-p(t_s, X_i)} \frac{1}{\prod_{m=t_{s+1}}^{T_{u,i}} [1 - p(m, X)]} \\ w^{t_s,ATE}(T_{u,i}, X_i) &= \frac{1}{1-p(t_s, X_i)} \frac{1}{\prod_{m=t_{s+1}}^{T_{u,i}} [1 - p(m, X)]} \\ p(t, X_i) &= \Pr(T_s = t | T_s \geq t, T_u \geq t, X_i). \end{aligned} \tag{14}$$

This follows the ideas of Horvitz and Thomson (1952) and weights the outcome responses of the treated and non-treated toward the target population (either the treated population at t_s or the full population of survivors at t_s). Both (12) and (13) are based on normalized weights. The reasons for this is that without the normalization the weights not always add up to one.

Consider the intuition behind the estimator ATE(t_s) in (13). Only non-treated individuals, provide information about the counterfactual outcome under never treatment for those treated at t_s. However, for the reasons discussed above, the outcomes of the non-treated need to be weighted for several reasons. The first part of the weights, $\frac{1}{1-p(t_s,X_i)}$, follows from IPW estimators in the static evaluation literature (see e.g. Wooldridge, 2010). Under Assumption 3 this adjusts for for covariate differences between the treated t_s and those still waiting for treatment at t_s.

The second part of the weights corrects for the fact that some non-treated at t_s starts treatment at $t_s + 1, \ldots$. As background briefly consider the case with only treatment assignment at t_s, so that everyone who do not receive treatment at t_s remain untreated in all time periods. Then, also noticing that in large samples we have

$$\widehat{\text{ATE}} = \frac{1}{N_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} \frac{Y_i}{p(t_s, X_i)} - \frac{1}{N_{t_s}} \sum_{i \in T_{s,i}>T_{u,i}, T_{u,i} \geq t_s} \frac{Y_i}{1 - p(t_s, X_i)} =$$

$$\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} \geq t_s, T_{u,i} \geq t_s} \left[\frac{I[T_s = t_s, T_u \geq t_s] Y_i}{p(t_s, X_i)} - \frac{I[T_s > t_s, T_u \geq t_s] Y_i}{1 - p(t_s, X_i)} \right],$$

i.e. the same structure and weights as static IPW estimators (see e.g. Wooldridge, 2010). The average effect is obtained by re-weighting the responses of the treated and not treated t_s. Notice that for the second part of this expression we have conditional on X

$$E\left[\frac{I[T_s > t_s, T_u \geq t_s]Y}{[1 - p(t_s, X)]} | X\right] = E\left[\frac{I[T_s > t_s, T_u \geq t_s]Y(\infty)}{[1 - p(t_s, X)]} | X\right] =$$

$$\frac{[1 - p(t_s, X)] E[Y(\infty)|X, T_s > t_s, T_u \geq t_s]}{[1 - p(t_s, X)]} = E[Y(\infty)|X, T_s > t_s, T_u \geq t_s].$$

Next,

$$E[Y(\infty)|X, T_s > t_s, T_u \geq t_s] = \tag{15}$$

$$\sum_{t_u = t_s}^{T_u^M} \Pr(T_u = t_u | T_s > t_s, T_u \geq t_s) E[Y(\infty)|X, T_s > t_s, T_u = t_u],$$

i.e. the average outcomes of the untreated survivors at t_s could be expressed as an average over the average outcome of the non-treated for each duration between t_s and T_u^M. Thus, with only treatment assignment at t_s the outcomes of the not treated are weighted using the IPW weights from the static evaluation problem. Following (15) this is an average giving individuals with the same X the same weight regardless of time in the initial state. With treatment assignment at $t_s, \ldots$ additional weighting is necessary, since in this case individuals with a high treatment probability and/or individuals remaining in the initial state for a long-time are less likely to remain non-treated. The IPW estimators in (12) and (13) correct for this , since the weights depend on both the observed characteristics and the time in the initial state.

Concerning inference, (12) and (13) are similar to the IPW estimator proposed by Hirano et al., (2003) and also discussed by Lunceford and Davidian (2004). One important difference is that here the weights depends on several propensity scores instead of a single propensity score. Nevertheless, if the propensity scores are known and following the same reasoning as in Lunceford and Davidian (2004), the large sample variances for $\widehat{\text{ATE}}(t_s)$ is

$$\sigma^2_{\widehat{\text{ATE}}(t_s)} = E\left[\frac{[Y-\mu(t_s)]^2}{p(t_s,X_i)} + \frac{[Y-\mu(t_s,\infty)]^2}{\prod_{m=t_s}^{T_{u,i}}[1-p(m,X)]}\right], \tag{16}$$

where the expectation is taken over the population with $T_s \geq t_s, T_u \geq t_s$. Also, $\mu(t_s) = E[Y|T_s = t_s, T_u \geq t_s]$ and $\mu(t_s,\infty) = E[Y|T_s > T_u, T_u \geq t_s]$.Also, Hirano et al. (2003) show that the variance decreases if estimated scores are used instead of true scores. In that case one way to obtain standard errors is bootstrapping

4.1 The aggregated effect

In the previous section we proposed estimators for average effects for a given pre-treatment duration. Here, we extend these results into the overall or aggregated effect over all pre-treatment durations. Formally, we define the aggregated effect treatment effect on the treated as follows:

$$\text{ATET} = \sum\nolimits_s g(t_s)\text{ATET}(t_s) \tag{17}$$

where $g(t_s) = \frac{N^1_{t_s}}{\sum_{m=1}^{T_u^M} N^1_m}$, i.e, $g(t_s)$, is the fraction in the sample of treated starting treatment at t_s. Equation (17) suggests that the aggregated effect could be estimated by averaging over the effects for specific pre-treatment durations. We have

$$\widehat{\text{ATET}} = \frac{1}{\sum_{m=1}^{T_u^M} N^1_m} \sum_{m=1}^{T_u^M} \sum_{i \in T_{s,i}=m, T_{u,i}\geq m} Y_i - \tag{18}$$

$$\frac{1}{\sum\limits_{m=1}^{T_u^M} \sum\limits_{i\in T_{s,i}>T_{u,i}, T_{u,i}\geq m} w^{t_s,ATET}(T_{u,i},X_i)} \sum_{m=1}^{T_u^M} \sum_{i\in T_{s,i}>T_{u,i},T_{u,i}\geq m} w^{m,ATET}(T_{u,i},X_i)Y_i =$$

$$\frac{1}{N^1}\sum_{i\in T_{u,i}\geq T_{s,i}} Y_i - \frac{1}{\sum\limits_{i\in T_{s,i}>T_{u,i}} w^{ATET}(T_{u,i},X_i)} \sum_{i\in T_{s,i}>T_{u,i}} w^{ATET}(T_{u,i},X_i)Y_i,$$

where $N^1 = \sum_{m=1}^{T_u^M} N_m^1$ and $w^{ATET}(T_{u,i}, X_i)$ is the weighted sum of the weights for each t_s given to a specific individual. Specifically,

$$w^{ATET}(T_{u,i}, X_i) = \sum_{m=1}^{T_{u,i}} w^{m,ATET}(T_{u,i}, X_i).$$

5 Monte Carlo simulation

This section presents a simulation study that examines the properties of the weighted estimator introduced in this paper, and compares it with related estimators. We use the following notation for the exit rate, $\theta_{T_u}(t) = \Pr(T_u = t_u | T_u \geq t_u)$, and the treatment rate, $\theta_{T_s}(t) = \Pr(T_s = t_s | T_u \geq t_s, T_s \geq t_s)$. We consider the following discrete time DGP:

$$\begin{aligned}
\theta_{T_u}(t) &= f(-2.5 + \beta_u X + v_u) \\
\theta_{T_s}(t) &= f(-3.0 + \beta_s X + v_s) \quad , t \leq 12 \\
\theta_{T_s}(t) &= 0 \quad , t > 12 \\
Y &= 100 + \beta_y X + \delta I(T_s \geq T_u) + 2v_u + v_y \\
&\text{with} \quad X, v_u, v_s \sim \text{unif}(-1,1), v_y \sim N(0,5),
\end{aligned} \tag{19}$$

with X, v_u, v_s, v_y all independently of each other, and $f(h) = [1 + \exp(-h)]^{-1}$, i.e. we use a logistic model for the exit rate and the treatment rate. We vary the parameters of the model in different ways: (i) δ is set equal to 0 or 1, i.e. we explore data generating processes with and without a long-run effect of the treatment. (ii) We set $\beta_u = \beta_s = \beta_y = \beta$ with β equal to 1 or 2, i.e. $\theta_{T_u}(t)$, $\theta_{T_u}(t)$ and Y will either be positively correlated or strongly positively correlated.

This model has several properties worth noticing. First, Y captures the long-run outcome, i.e. e.g. earnings in the training example. Second, treatment could start at any point during the first 12 time periods. This corresponds to a training program in place during the first two years of unemployment (if the time period is a month). Third, both durations, T_u, T_s, and the outcome, Y, depend on observed and unobserved characteristics, but since the unobserved effect in the treatment equation is uncorrelated with the other unobserved effects the unconfoundedness assumption holds. Fourth, the unobserved effect in the duration time equation also appears in the long-run outcome equation. This is consistent with the idea that some unobserved characteristics determine both time in the initial state and the long-run outcome equation. In the training for unemployed example this might be unobserved motivation and/or ability.

We mainly focus on the aggregated effect ATET, but also report estimates for the average effect for specific pre-treatment durations. Here we label this estimator $\delta_{Dyn.IPW}$. The propensity scores are estimated with a correct logistic model specification. The standard errors are calculated using both the formula applicable with true propensity scores in (16) and using bootstrap (99 replications). In both cases we use estimated scores.

We compare our IPW estimators to several related estimators. The first approach is a naive model (δ_{naive}), were we make no adjustments for the observed covariates and base the entire analysis on the observed static treatment status $W = I(T_s \geq T_u)$. That is we compare the average long-run outcome for individuals that is observed to start treatment before leaving the initial state with the average long-run outcome for treated individuals.

The second approach is also based on the observed static treatment status W, but with covariate adjustment using a standard static IPW estimator:

$$\delta_{naive,IPW} = \sum_{i=1}^{N} \frac{W_i Y_i}{e(x_i)} / \frac{W_i}{e(x_i)} - \sum_{i=1}^{N} \frac{(1-W_i)Y_i}{1-e(x_i)} / \frac{1-W_i}{1-e(x_i)}, \tag{20}$$

where $e(X_i)$ is the estimated scores for the probability that $W = 1$, N the number of observations. The score is estimated using a logistic model. See e.g. Hirano, Imbens and Ridder (2003) and Imbens and Wooldridge 2009) for discussions of this estimator.

The third approach is the treatment now vs. waiting for treatment estimator introduced by Sianesi (2004) and used by e.g. Sianesi (2008), Fitzenberger et al. (2008) and Biewen et al. (2014). For a given pre-treatment duration this estimator uses individuals who are not treated at this duration but possibly treated later on as control group. One advantage of this approach is that it transforms the dynamic problem into a static problem, since under unconfoundedness those still waiting for treatment at t_s are comparable to those treated at t_s. However, note that this estimator captures the effect of treatment now vs. waiting and not the treatment vs. never treatment effect. We still use this estimator for comparison, since it is often used. Concerning estimation details, we use standard IPW as in (20) and one-to-on propensity score matching to calculate the effect for each pre-treatment duration. In both cases, the score is estimated using a logistic model. These estimators are denoted by $\delta_{Sianesi,IPW}$ and $\delta_{Sianesi,PS}$.

The fourth approach originates from Lechner (1999, 2002) and was refined by e.g. Wunsch and Lechner (2008) and Lechner et al. (2011). In this approach hypothetical treatment durations are generated for each non-treated individual (i.e. those with $W = 0$). Specifically, we first regress the log treatment durations for the treated on X. The estimated coefficient are then used together with a random draw from estimated residuals from that regression to construct the hypothetical starting dates for the non-treated. The actual and the hypothetical treatment dates are then used as covariates when estimating the propensity score. In practice, we implement this using either static IPW methods and/or one-to-on propensity score matching. In both cases, the score is estimated using a logistic model. These estimators are denoted by $\delta_{Lechner,IPW}$ and $\delta_{Lechner,PS}$.

For all estimators we impose common support by observations whose propensity score is smaller (larger) than the maximum (minimum) of the minimum (maximum) scores among the treated and the non-treated.

Table 1 presents the bias of the various estimators. Here we use samples of sizes 10,000 and 20,000, and the number of replications is 2000. We see that the naive estimator, δ_{naive}, and the static IPW estimator, $\delta_{naive,IPW}$, both are severely biased. As expected the bias is larger for larger effect of the observed covariate ($\beta = 2$) compared to when $\beta = 1$. The bias of our dynamic IPW estimator, $\delta_{Dyn.IPW}$, is virtually zero, and more than 100 times smaller compared to the bias of the naive estimators. This holds both with ($\delta = 1$) and without ($\delta = 0$) a treatment effect. As expected the treatment now vs. treatment possibly later estimators, $\delta_{Sianesi,IPW}$ and $\delta_{Sianesi,PS}$, perform well if there is now treatment effect, but not with a treatment effect. Naturally under a treatment effect these estimators will underestimate the true effect, since some of the controls will be treated later on. Finally, the bias of the two random hypothetical dates estimators, $\delta_{Lechner,IPW}$ and $\delta_{Lechner,PS}$, is considerably smaller compared to the naive estimators, but almost 10 times as large compared to the dynamic IPW estimator introduced in this paper.

Tables 2 presents the bias, variance and size of our dynamic IPW estimator. Size is for a test with nominal size of 5%. We use samples of sizes 10,000 and 20,000, and the number of replications is 500. We report results for the average effect for specific pre-treatment durations. Panel A provides results with bootstrapped standard errors and Panel B results using the inference procedure based on true scores. For the bootstrap inference the size is close to 5% in all cases, whereas the size for the true score inference is lower than 5%. Based on the results of Hirano, Imbens and Ridder (2003) the latter result was expected since we use estimated scores instead of true scores. We also see that the variance of estimates decreases as the sample size is increased from 10,000 to 20,000. Finally, as expected from the results in Table 1 the bias is small in all cases.

6 Long-run effects of Swedish training programs

This section implements the estimator proposed in this paper using data from a Swedish training program called AMU. Previous evaluations of the program include e.g. Harkman and Johansson (1999), de Luna et al. (2008) and van den Berg and Richardson (2013). The main purpose of the program is to improve the skills of the unemployed and thereby enhance their chances of obtaining a job. There are two types of AMU training: vocational and non-vocational. Vocational training courses are provided by education companies, universities, and municipal consultancy operations. The local employment office or the county employment board pay these organizations for the provision of courses. The contents of the courses should be directed towards the upgrading of skills or the acquisition of skills that are in short supply or that are expected to be in short supply. In the 1990s, most courses concerned computer skills, technical skills, manufacturing skills, and skills in services and medical health care. Non-vocational training (basic general training) concerns participation in courses within the regular educational system, i.e. at adult education centers and universities. In this paper, we will focus on the overall effect of the training program and do not attempt to separate the effects of the vocational and non-vocational program. During the training, participants receive a training grant. Those who are entitled to unemployment insurance (UI) receive a grant equal to their UI benefits level.

We use several register based datasets in our analysis. The main register is called HÄNDEL and contains detailed information from the Swedish employment offices. It covers all registered unemployed persons and contain day-by-day information on the unemployment status as well as the reason for the unemployment spell to end (as a rule, this is re-employment, but some times it is a transition into education or other insurance schemes). It also contains detailed information on all labor market program episodes, including type of program and start and end date. For estimation reasons we aggregate the daily data into monthly intervals. The individuals are regarded as unemployed until they find and retain full-time or part-time for at least 30 days.

In order to be able to follow the unemployed over a long time period we sample all unemployment spells that start between January 1, 1995 and December 1, 1998. For these unemployment spells we consider the effect of the first training program after 4-24 months months of unemployment.[4] Later training episodes are ignored, so that nonparticipants are everyone who are unemployed at least 3 months and do not participate in training during the first 2 years of unemployment. We further restrict the analysis to unemployed in ages 25-55 at the time of entry into unemployment. The reason for this is that the benefits entitlement rules and active labor market policy programs were are different for persons aged below 25 or above 55 during the period studied here.[5]

HÄNDEL also includes a number of personal characteristics. In our analysis we use gender, age dummies (3 categories), level of education (3 categories), regional of residence (6 areas), inflow year dummies, and indicators for UI entitlement and if the unemployed only search for a job in the local area. The employment office data is also used to construct information on previous unemployment. Using unemployment spell data back until January 1, 1993 we obtain information on the number of unemployment days in the last 2 years. We further obtain additional background characteristics from the annual population register LOUISE. Specifically, we use indicators for at least indicators for at least one child in different age brackets (0-3, 4-6 and 7-15) and marital status. The population register also contains detailed information on labor earnings and income from various insurance schemes. We control for labor earnings one, two and three years before the unemployment spell.

The characteristics used in our analysis, thus, include a large number of socio-economic characteristics as well as controls for previous income and unemployment and regional indicators. Concerning unconfoundedness, notice that this set of controls is supported by several studies that have examined the importance if including different types of control variables when evaluating training programs. Besides using basic socioeconomic variables Heckman et al. (1998), Heckman and Smith (1999) and Dolton and Smith (2011) stress that it is important to control for previous unemployment, lagged earnings, and local labor market characteristics. More recently, Lechner and

[4]The reasons for this is that the most of those starting training during the first three months of unemployment do this after a period with several short periods of unemployment and employment, so that it may be difficult to find comparable non-participants.

[5]Persons below 25 must participate in a program after 100 days of unemployment, or otherwise they lose their unemployment benefits. They can use special programs that are not available for other age groups. Persons over 55 receive unemployment benefits for 450 days.

Wunsch (2013) and Biewen et al. (2014) obtain similar results. We control for all these types of variables. As further support of the unconfoundedness assumption Eriksson (1997) and Carling and Richardson (2001) show that caseworkers have large influence and large degree of discretionary power over enrollment decisions.

Besides unconfoundedness the other key assumption is no-anticipation. It holds if the unemployed are unable to predict the exact timing of the treatment. There are several reason why we believe that this assumption is fulfilled. There are several sources of uncertainty in the assignment process. This includes the above mentioned fact that caseworkers have large influence over enrollment decisions, which makes it difficult for the unemployed to predict the exact timing of the start of the treatment. Moreover, the unemployed are often informed about the training shortly before the start of the program, and this limits the impact of any anticipation effects.

The outcome of interest is yearly labor earnings from, which is available from the population register up until 2010. It is based on tax records and captures all cash compensation paid by employers. We focus on both the overall average effect on the treated and the average effect on the treated for specific pre-treatment durations. Concerning estimation details, the treatment propensity scores and censoring probabilities are estimated using logistic regression models, and standard errors are obtained using bootstrap (99 replications). We impose common support by excluding unemployed whose propensity score is smaller (larger) than the maximum (minimum) of the minimum (maximum) scores among the participants and the non-participants. This is imposed for all months within the 4-24 month period.

In Table 3 we present sample statistics for participants and nonparticipants. In total our analysis sample includes 870,699 unemployment spells of which 97367 (11,2%) concerns participation in the training program. The training program starts after many different elapsed unemployment durations. For instance, 38,865 training episodes starts after 4-6 months and 15,011 starts after 13-18 months. This confirms that enrollment into the training program is a dynamic process, in which training could start after any unemployment duration. Table 3 also shows that on average nonparticipants have higher earnings than the participants. This holds for every year between 3-13 years after the start of the unemployment spell.

Table 3 also sample statistics on all covariates used in the analysis. The statistics show that there are more females, high-school educated, university educated, married and parents, and less big city residents (Stockholm and Gothenburg) among the the participants. The participants also have more extensive unemployment record and lower previous labor earnings. The table also reports statistics by the timing of the program. These statistics show that except for unemployment record and previous labor earnings those enrolling early are relatively similar to those enrolling later into the training program.

Our main results are presented in Table 4. Each row of the table presents a separate estimate. The first row of column 1 shows that averaged over all pre-treatment durations the program on average decreases labor earnings 3 years after the start the unemployment spell. This is consistent with pronounced locking-in effects during and shortly after the end of the program documented in several previous studies (see e.g. Gerfin and Lechner 2002, van Ours 2004 and Sianesi 2004). One year later we find positive effects of the program and 5-13 years after the start of the unemployment spell we find that the program leads to a remarkably stabile increase in yearly earnings with about 6500 SEK (about \$950). In relative terms this amounts to about a 5.1% increase in year five and and a 4.4% increase in year 10. In total, the program increases earnings with 56,000 SEK (about \$8500) over 3-13 years after the start of the unemployment spell. We regard these as substantial effects, especially considering how persistent the effect is.

Table 3 also present results for specific pre-treatment durations. From this we see that the average effect of the programs is higher for unemployed enrolling relatively late into the training program. This holds both in the medium-run and in the long-run. As illustration, in year 10 the programs increases earnings with on average 9970 SEK, while the effect for those starting training after 6 months is 6700 SEK.

In Table 5, we compare our estimates with estimates using various other estimators. Initially in columns 2 and 3 we present estimates using a naive estimator with no adjustments for the observed covariates and a standard static IPW estimator. Both estimators and the other estimators are described in more detail in Section 5. In all cases we estimate the average effect on the treated using IPW and impose common support. Interestingly we obtain the result that the two naive estimators give estimates of incorrect sign. We conclude that properly taking the dynamic treatment assignment into account is very important.

Next, columns 4 and 5 of Table 5 display results for the commonly used treatment now vs. waiting for estimator as of Sianesi (2004, 2008) and also applied by e.g. Fitzenberger et al. (2008) and Biewen et al. (2014), and the random hypothetical treatment durations estimator as of Lechner (1999, 2002), Wunsch and Lechner (2008) and Lechner et al. (2011). We see that both these approaches underestimates the training effect. For instance, 10 years after the start of the program our dynamic IPW estimator shows that effect is about 6700, while the estimate from the treatment now vs. waiting estimators is 5700 and the estimate from hypothetical treatment dates estimator is 6100. Both these results are inline with the results from the Monte Carlo simulation that also indicated that both these estimators tend to underestimate any positive treatment effect.

7 Conclusions

We propose and implement a new way to estimate treatment effects in a dynamic treatment assignment setting in which treatment could start at any point in time. We consider effects on medium- and long-run outcomes experienced some time after the start of the program. The identification and estimation approach is based unconfoundedness, which is a common assumption in evaluations using rich register and/or survey data. We consider identification in detail and demonstrate that besides unconfoundedness one key assumptions is no-anticipation. We propose and implement inverse probability weighting estimators. The key innovation is that we show that the weights given to the outcome response of the non-treated should depend on both the observed characteristics and time in unemployment. We apply our method to a training program in Sweden. We follow each unemployed for up to 13 years after the start of the unemployment spell. We conclude that the training program has sizeable effects on labor earnings for at least 10 years after the program.

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Tables and Figures

Table 1: Simulated bias of various estimators

	δ_{naive}	$\delta_{naive,IPW}$	$\delta_{Dyn.IPW}$	$\delta_{Sianesi,IPW}$	$\delta_{Sianesi,PS}$	$\delta_{Lechner,IPW}$	$\delta_{Lechner,PS}$
10,000 observations							
$\delta = 1,\ \beta = 1$	-.09	-.32	.0019	-.22	-.22	-.09	-.078
$\delta = 0,\ \beta = 1$	-.089	-.32	-.0017	-.0013	-.000021	-.089	-.074
$\delta = 1,\ \beta = 2$	.46	-.37	.0023	-.26	-.26	-.22	-.21
$\delta = 0,\ \beta = 2$	.46	-.37	-.0012	-.003	.00025	-.22	-.21
20,000 observations							
$\delta = 1,\ \beta = 1$	-.091	-.32	.0013	-.23	-.22	-.09	-.076
$\delta = 0,\ \beta = 1$	-.089	-.32	-.0018	-.00015	-.00096	-.089	-.073
$\delta = 1,\ \beta = 2$	.46	-.37	-.0016	-.26	-.26	-.22	-.21
$\delta = 0,\ \beta = 2$	.46	-.37	-.00072	.00069	-.0014	-.22	-.21

Note: Bias for models with zero treatment effect ($\delta = 0$) and positive treatment effect ($\delta = 1$). Full generating processes described in the text. δ_{naive} makes no covariate adjustments. $\delta_{naive,IPW}$ is standard evaluation based on the observed static treatment status and $\delta_{Dyn.IPW}$ is the dynamic IPW estimator introduced in this paper. $\delta_{Sianesi,IPW}$ and $\delta_{Sianesi,PS}$ are the treatment now vs. waiting for treatment estimators inspired by Sianesi (2004). $\delta_{Lechner,IPW}$ and $\delta_{Lechner,PS}$ are the random hypothetical treatment durations as of Wunsch and Lechner (2008) and Lechner et al. (2011). The results are based on 2000 replications.

Table 2: Bias, size and variance of the dynamic IPW estimator. No treatment effect

	Treatment after								
	1 month			5 months			10 months		
	size [1]	bias [2]	var [3]	size [4]	bias [5]	var [6]	size [7]	bias [8]	var [8]
Panel A: Bootstrap inference									
10,000 observations									
$\beta = 1$	.052	.0032	.014	.06	-.0063	.0283	.064	.017	.0573
$\beta = 2$	.064	-.0099	.0285	.052	-.0066	.068	.072	.025	.119
20,000 observations									
$\beta = 1$	.058	.002	.00711	.072	-.011	.0155	.066	-.0033	.0276
$\beta = 2$	.058	.0079	.0146	.05	.015	.0345	.05	.015	.0597
Panel B: True score inference									
10,000 observations									
$\beta = 1$	.044	.0032	.014	.052	-.0063	.0283	.054	.017	.0573
$\beta = 2$	.048	-.0099	.0285	.048	-.0066	.068	.052	.025	.119
20,000 observations									
$\beta = 1$	.046	.002	.00711	.074	-.011	.0155	.056	-.0033	.0276
$\beta = 2$	.048	.0079	.0146	.044	.015	.0345	.04	.015	.0597

Note: Model with no treatment effect. Full generating processes described in the text. IPW estimates with bootstraped standard errors (99 replications) and assuming true scores, respectively. Size is for 5% level tests. The results are based on 500 replications.

Table 3: Sample statistics for training participants and non-participants

	Controls	Training after					
		All	4-6 months	7-9 months	10-12 months	13-18 months	19-24 months
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
# observations	870669	97367	38865	22765	14329	15011	6397
Mean survival time	10.7	23.9	18.5	22.7	26.7	31.3	37.6
Outcomes							
Earnings +3 years	95381	72157	88636	76055	63895	49337	30222
Earnings +4 years	111127	97479	109891	99046	90874	81932	67767
Earnings +5 years	122731	112042	121699	113013	106919	100072	89490
Earnings +6 years	130287	120927	129162	121987	116373	110303	102257
Earnings +7 years	138993	128977	137092	130661	124334	117705	110653
Earnings +8 years	142727	132330	140680	133385	128214	121127	113407
Earnings +9 years	147619	137171	146214	137561	132400	126239	117176
Earnings +10 years	154404	143341	152741	143552	138676	131869	122807
Earnings +11 years	161528	150128	159716	150857	145515	137710	128717
Earnings +12 years	167530	154948	164621	155559	150291	142178	134299
Earnings +13 years	172089	158527	168437	159104	154193	144922	137768
Controls							
Male (%)	51.4	50.6	50.5	49.3	50.6	51.6	53.2
Ages 35-44	27.3	31.4	30.9	31.1	31.6	32.5	32.7
Ages 45-54	18.3	19.3	17.4	19.5	20.4	21.6	21.9
Married (%)	35.8	40.9	40.0	41.3	40.8	42.1	42.1
High school education (%)	53.5	57.2	58.3	57.2	57.1	55.6	54.4
University education (%)	22.9	21.1	20.7	21.0	21.5	21.9	21.8
Child in ages 0-3 (%)	23.8	27.0	26.9	27.2	26.6	27.4	27.0
Child in ages 4-6 (%)	17.0	19.0	19.1	19.1	18.6	19.4	18.5
Child in ages 7-15 (%)	24.4	27.7	27.3	27.8	27.2	28.7	27.8
UI eligiable (%)	80.4	80.9	80.4	80.5	81.5	81.6	82.1
Rest. search area (%)	18.1	20.0	20.4	20.1	19.8	19.4	19.4
Stockholm MSA (%)	20.1	16.6	16.5	17.0	16.8	16.2	15.8
Gothenburg MSA (%)	17.0	14.9	13.2	15.2	16.1	16.6	18.0
Skane MSA (%)	13.2	14.0	13.5	14.1	14.6	15.0	13.5
North (%)	15.2	14.8	15.8	14.2	13.8	14.2	15.1
South (%)	11.5	12.7	13.0	12.4	12.5	12.7	12.2
Days unemployed year -1	89.7	100.7	99.4	95.7	101.4	106.3	112.3
Days unemployed year -2	100.3	112.2	107.4	110.7	113.9	119.5	126.0
Earnings year -1	71880	66719	68690	67642	66405	64713	56873
Earnings year -2	72871	66542	69226	67262	65631	63426	57030
Earnings year -3	76212	70443	73022	70950	69211	66936	63953
Inflow 1996 (%)	26.2	22.0	20.4	20.9	21.6	25.3	29.1
Inflow 1997 (%)	25.0	21.7	19.9	20.5	22.3	25.5	26.5
Inflow 1998 (%)	23.5	24.5	27.7	26.3	23.5	19.2	14.2

Note: Controls recorded at the start of the unemployment spell. Earnings is in SEK.

Table 4: Estimates of earnings effects of training

	Aggregated	Training after 4 months	6 months	8 months	10 months	12 months
	[1]	[2]	[3]	[4]	[5]	[6]
Earnings + 3 years	-6492.7	567.9	-1925.7	-3598.8	-9036.4	-8722.0
	(281.7)	(691.6)	(794.6)	(899.3)	(969.8)	(1145.8)
Earnings + 4 years	3409.7	5210.5	6084.4	4315.5	3169.2	3276.0
	(305.9)	(759.3)	(893.0)	(1019.1)	(1144.2)	(1380.5)
Earnings + 5 years	6279.1	5083.0	6784.4	6656.0	7273.8	8502.0
	(374.4)	(785.3)	(935.1)	(1067.8)	(1230.0)	(1511.7)
Earnings + 6 years	6839.3	4350.7	6349.3	7390.0	7015.5	9791.1
	(366.9)	(816.2)	(949.2)	(1112.2)	(1266.7)	(1593.4)
Earnings + 7 years	6780.3	3578.1	6083.4	7950.2	7774.6	9502.7
	(308.3)	(811.2)	(958.7)	(1123.4)	(1307.0)	(1633.5)
Earnings + 8 years	6204.0	2768.2	5627.9	7004.6	7794.0	8710.0
	(366.2)	(826.3)	(977.9)	(1156.0)	(1330.3)	(1655.1)
Earnings + 9 years	6671.2	3945.6	6440.7	6851.8	7032.9	11234.6
	(399.1)	(850.9)	(1006.2)	(1183.6)	(1358.3)	(1684.8)
Earnings + 10 years	6740.9	3403.2	6701.9	5577.0	8056.1	9974.5
	(448.6)	(881.5)	(1049.9)	(1216.0)	(1393.8)	(1728.2)
Earnings + 11 years	7215.1	3506.6	7225.7	6504.4	9062.4	10289.3
	(398.6)	(909.9)	(1091.1)	(1256.1)	(1440.8)	(1762.0)
Earnings + 12 years	6775.4	2438.0	6776.3	6454.1	8087.2	10653.6
	(441.0)	(935.7)	(1107.0)	(1317.1)	(1471.2)	(1816.7)
Earnings + 13 years	6176.1	2417.4	4882.5	5185.4	7841.8	9945.7
	(393.0)	(973.9)	(1134.0)	(1337.6)	(1530.8)	(1931.3)

Note: Outcome is yearly labor earnings in SEK. IPW estimates with bootstraped standard errors (99 replications) in parenthesis. Aggregated effect is over training episodes after 4-24 months.

Table 5: Estimates of earnings effects of training

	$\delta_{Dyn.IPW}$ [1]	δ_{naive} [2]	$\delta_{naive,IPW}$ [3]	$\delta_{Sianesi,IPW}$ [4]	$\delta_{Lechner,IPW}$ [5]
Earnings + 3 years	-6492.7	-22460.9	-19138.6	-5429.9	-7393.9
Earnings + 4 years	3409.7	-12914.2	-9270.0	3334.6	2699.5
Earnings + 5 years	6279.1	-10112.6	-6405.7	5798.5	5230.4
Earnings + 6 years	6839.3	-8912.8	-5520.3	6246.1	6115.6
Earnings + 7 years	6780.3	-9589.2	-5432.6	6174.0	6077.5
Earnings + 8 years	6204.0	-9994.6	-5888.2	5637.0	5099.0
Earnings + 9 years	6671.2	-10026.3	-5606.1	6057.5	5734.9
Earnings + 10 years	6740.9	-10619.8	-5862.4	6100.5	5718.9
Earnings + 11 years	7215.1	-10945.5	-5696.0	6557.1	6335.5
Earnings + 12 years	6775.4	-12109.8	-6421.4	6143.1	5935.8
Earnings + 13 years	6176.1	-13079.8	-7307.9	5579.1	5527.8

Note: Outcome is yearly labor earnings in SEK. Earnings effects Aggregated effect over training episodes after 4-24 months. $\delta_{Dyn.IPW}$ is the dynamic IPW estimator introduced in this paper. δ_{naive} and $\delta_{naive,IPW}$ is the standard evaluation based on the observed static treatment status. $\delta_{Sianesi,IPW}$ is the treatment now vs. waiting for treatment estimators inspired by Sianesi (2004). $\delta_{Lechner,IPW}$ is the random hypothetical treatment durations as of Wunsch and Lechner (2008) and Lechner et al. (2011).

Appendix

First, for the first part of the estimator we have without the normalization

$$
\begin{aligned}
& E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} w^{t_s}(X_i) Y_i\right] = \\
& E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} \geq t_s, T_{u,i} \geq t_s,} w^{t_s}(X_i) I[T_{s,i}=t_s] Y_i\right] = \\
& E\left[w^{t_s}(X) I[T_s=t_s] Y | T_s \geq t_s, T_u \geq t_s\right] = \\
E_{X|T_s \geq t_s, T_u \geq t_s} \quad & E\left[w^{t_s}(X) I[T_s=t_s] Y | T_s \geq t_s, T_u \geq t_s, X\right] = \\
E_{X|T_s \geq t_s, T_u \geq t_s} \quad & \frac{1}{p(t_s, X)} p(t_s, X) E\left[Y | X, T_s=t_s, T_u \geq t_s, X\right] \overset{A.1}{=} \\
E_{X|T_s \geq t_s, T_u \geq t_s} \quad & E\left[Y(t_s) | T_s=t_s, T_u(\infty) \geq t_s, X\right] \overset{A.2}{=} \\
E_{X|T_s \geq t_s, T_u \geq t_s} \quad & E\left[Y(t_s) | T_s \geq t_s, T_u(\infty) \geq t_s, X\right] = \\
& E\left[Y(t_s) | T_s \geq t_s, T_u(\infty) \geq t_s\right].
\end{aligned} \tag{21}
$$

Second, for the second part of the estimator we have without the normalization

$$
\begin{aligned}
& E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} > T_{u,i}, T_{u,i} \geq t_s} w^{t_s, ATET}(T_{u,i}, X_i) Y_i\right] = \\
& E\left[\frac{1}{N_{t_s}} \sum_{ii \in T_{s,i} \geq t_s, T_{u,i} \geq t_s} w^{t_s, ATET}(T_{u,i}, X_i) I[T_{s,i} > T_{u,i}] Y_i\right] = \\
& E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} \geq t_s, T_{u,i} \geq t_s} \sum_{t_u=t_s}^{T_u^M} w^{t_s}(t_u, X_i) I[T_{s,i} > t_u, T_{u,i}=t_u] Y_i\right] = \\
& E\left[\sum_{t_u=t_s}^{T_u^M} w^{t_s}(t_u, X) I[T_s > t_u, T_u=t_u] Y | T_s \geq t_s, T_u \geq t_s\right] = \\
& E_{X|T_s \geq t_s, T_u \geq t_s} E\left[\sum_{t_u=t_s}^{T_u^M} w^{t_s}(t_u, X) I[T_s > t_u, T_u=t_u] Y | T_s \geq t_s, T_u \geq t_s, X\right].
\end{aligned} \tag{22}
$$

Next, for a given X, and using Assumptions (3) and (4)

$$E\left[w^{t_s}(t_u,X)I[T_s>t_u,T_u=t_u]Y|T_s\geq t_s,T_u\geq t_s,X\right]= \tag{23}$$

$$w^{t_s}(t_u,X)\Pr(T_s>t_u,T_u=t_u|T_u\geq t_s,T_s>t_s,X)E[Y|T_s>t_u,T_u=t_u,X]=$$

$$\frac{1}{\prod_{m=t_s}^{t_u}1-p(m,X)}h(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h(m,X)]\prod_{m=t_s}^{t_u}[1-p(m,X)]E[Y|T_s>t_u,T_u=t_u,X]=$$

$$h(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h(m,X)]E[Y|T_s>t_u,T_u=t_u,X]\overset{A.1}{=}$$

$$h(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h(m,X)]E[Y(\infty)|T_s>t_u,T_u(\infty)=t_u,X]\overset{A.2}{=}$$

$$h(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h(m,X)]E[Y(\infty)|T_s\geq t_s,T_u(\infty)=t_u,X].$$

so that using (22) and (23)

$$E\left[\frac{1}{N_{t_s}}\sum_{i\in T_{s,i}>T_{u,i},T_{u,i}\geq t_s}w^{t_s,ATET}(T_{u,i},X_i)Y_i\right]= \tag{24}$$

$$E_{X|T_s\geq t_s,T_u\geq t_s}\sum_{t_u=t_s}^{T_u^M}h(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h(m,X)]E[Y(\infty)|T_s\geq t_s,T_u(\infty)=t_u,X].$$

For sake of presentation, introduce the notation

$$\begin{aligned}
y(T_u(\infty)=t,X)&=E[Y(\infty)|T_s\geq t_s,T_u(\infty)=t,X]\\
y(T_u(\infty)>t,X)&=E[Y(\infty)|T_s\geq t_s,T_u(\infty)>t,X]\\
y(T_u(\infty)\geq t,X)&=E[Y(\infty)|T_s\geq t_s,T_u(\infty)\geq t,X].
\end{aligned}$$

Using this notation we have using that by construction $h(T_u^M)=1$

$$h(T_u^M)\prod_{m=t_s}^{T_u^M-1}[1-h(m)]y(T_u(\infty)=T_u^M,X)=\prod_{m=t_s}^{T_u^M-1}[1-h(m)]y(T_u(\infty)=T_u^M,X). \tag{25}$$

Next, for time periods T_u^M and $T_u^M - 1$

$$\prod_{m=t_s}^{T_u^M-1}[1-h(m)]y(T_u(\infty)=T_u^M,X)+h(T_u^M-1)]\prod_{m=t_s}^{T_u^M-2}[1-h(m)]y(T_u(\infty)=T_u^M-1,X)= \tag{26}$$

$$\prod_{m=t_s}^{T_u^M-2}[1-h(m)]y(T_u(\infty)\geq T_u^M-1,X),$$

and for arbitrary time periods t and $t-1$

$$\prod_{m=t_s}^{t}[1-h(m)]y(T_u(\infty)>t,X)+h(t)]\prod_{m=t_s}^{t-1}[1-h(m)]y(T_u(\infty)=t-1,X)= \tag{27}$$

$$\prod_{m=t_s}^{t-1}[1-h(m)]y(T_u(\infty)\geq t-1,X).$$

Thus, using (25) for T_u^M, (26) for $T_u^M - 1$ and (27) for $t_s, ..., T_u^M - 2$ we have

$$\sum_{t_u=t_s}^{T_u^M} h(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h(m,X)]E[Y(\infty)|T_u(\infty)=t_u,T_s=t_s,X]= \tag{28}$$

$$\sum_{t_u=t_s}^{T_u^M} h(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h(m,X)]y(T_u(\infty)=t_u,X)\overset{(25)}{=}$$

$$\prod_{m=t_s}^{T_u^M-1}[1-h(m)]y(T_u(\infty)=T_u^M,X)+\sum_{t_u=t_s}^{T_u^M-1}h(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h(m,X)]y(T_u(\infty)=t_u,X)\overset{(26)}{=}$$

$$\prod_{m=t_s}^{T_u^M-2}[1-h(m)]y(T_u(\infty)\geq T_u^M-1,X)+\sum_{t_u=t_s}^{T_u^M-2}h(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h(m,X)]y(T_u(\infty)=t_u,X)\overset{(27)}{=}$$

$$y(T_u(\infty)\geq t_s,X).$$

Thus, from (24) and (28)

$$E\left[\frac{1}{N_{t_s}}\sum_{i\in T_{s,i}>T_{u,i},T_{u,i}\geq t_s} w^{t_s,ATET}(T_{u,i},X_i)Y_i\right]=E_{X|T_s\geq t_s,T_u\geq t_s}y(T_u(\infty)\geq t_s,X)=$$

$$E_{X|T_s \geq t_s, T_u \geq t_s} E[Y(\infty)|T_s \geq t_s, T_u(\infty) \geq t_s, X] = E[Y(\infty)|T_s \geq t_s, T_u(\infty) \geq t_s].$$