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Gerard J. van den Berg
University of Bristol, IFAU, University of Groningen, IZA, ZEW, CEPR, CESifo and Fellow of the Econometric Society

Johan Vikström
IFAU and UCLS Uppsala University

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Long-Run Effects of Dynamically Assigned Treatments: A New Methodology and an Evaluation of Training Effects on Earnings*

We propose and implement a new method to estimate treatment effects in settings where individuals need to be in a certain state (e.g. unemployment) to be eligible for a treatment, treatments may commence at different points in time, and the outcome of interest is realized after the individual left the initial state. An example concerns the effect of training on earnings in subsequent employment. Any evaluation needs to take into account that some of those who are not trained at a certain time in unemployment will leave unemployment before training while others will be trained later. We are interested in effects of the treatment at a certain elapsed duration compared to "no treatment at any subsequent duration". We prove identification under unconfoundedness and propose inverse probability weighting estimators. A key feature is that the weights given to outcome observations of non-treated depend on the remaining time in the initial state. We study earnings effects of WIA training in the US and long-run effects of a training program for unemployed workers in Sweden.

JEL Classification: C14, J3

Keywords: treatment effects, dynamic treatment evaluation, program evaluation, duration analysis, matching, unemployment, employment

Corresponding author:
Gerard J. van den Berg
School of Economics, Finance and Management
University of Bristol
2C3, The Priory Road Complex
Priory Road, Clifton
Bristol, BS8 1TU
United Kingdom

E-mail: gerard.vandenberg@bristol.ac.uk

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1 Introduction

By now, an extensive toolkit has been developed for treatment evaluations based on unconfoundedness assumptions (i.e., absence of unobserved confounders). This includes the construction of efficient and robust estimators, balancing tests and the imposition of common support restrictions (e.g., Imbens and Wooldridge, 2009). Much of this literature considers a static evaluation problem, where the comparison is between being treated and not being treated. Training programs and other active labor market policy (ALMP) programs have often been evaluated using such methods, especially if rich register and/or survey data are available. However, the static framework is of limited value for the evaluation of such programs. To see this, consider, more generally, a setting where eligibility for a treatment requires the individual to be in a certain state. Within this initial state, the date of entry into the treatment may vary, for example because of capacity constraints on the number of treatment slots. The duration of being in the initial state may vary as well, for example because of market frictions. Some of those who do not enter the treatment at a certain point in time will therefore leave the initial state before they are ever treated, while others will be treated before they leave. In such a setting, the evaluation needs to take the dynamic nature of the treatment assignment into account, and this complicates the selection of a proper control group. The literature contains several contributions for the case where the length of stay in the initial state is the outcome of interest (see the literature discussion below).

In this paper, we consider outcome measures that are realized after the individual has left the initial state. Such outcomes are often deemed interesting. Compared to outcomes capturing the length of stay in the initial state, they are less sensitive to short-run institutional features and market imperfections. A case in point is the evaluation of post-unemployment earnings effects of participation in a training program for unemployed individuals. In the short run, locking-in effects of program participation may dominate the effect on productivity, whereas in the longer run, outcomes may better reflect the best attainable earnings levels, and hence the estimated effect may better reflect the effect on productivity or on human capital. In addition to this, longer-run outcomes may be the key drivers of the present values of the various treatment regimes, and hence they are likely to play an important role in cost-benefits analyses.[1]

[1]Many studies of European training programs focus on transitions from unemployment to employment as the key outcome. One reason for this is the concern about relatively long unemployment durations. Moreover, with long durations, post-unemployment outcomes have often only been observable for a modest subset of the sample, leading to concerns about selective observability (Ham and

Long-run data on individuals who are treated at the elapsed duration t in the eligibility state are informative on the average potential outcome under treatment, whereas *only* the individuals who leave the initial state before treatment provide information on no-treatment potential outcomes. Ultimately, the realized treatment status depends on the realized time in the initial state. The challenge that we face here is that the non-treated group cannot be straightforwardly used to construct an average counterfactual outcome for the treated.

This may be illustrated by the example with training during unemployment and its effect on long-run earnings.[2] With a sufficiently long time horizon, the long-run earnings of the treated at t are directly observable. Under unconfoundedness among survivors in unemployment at t we can adjust for differences between treated and those who are not treated at t. However, in subsequent time periods some of the previously non-treated will receive the treatment before they leave unemployment as well. Individuals with a small probability of receiving the treatment in subsequent periods are more likely to remain non-treated. Moreover, for a given period-by-period treatment probability, the individuals with a high rate of leaving unemployment are more likely to remain non-treated. Both features (small probability of treatment in subsequent periods and high exit rate) may influence long-run earnings. To infer the causal effect of training, these features need to be controlled for.

In this paper, we develop a novel approach to resolve this, based on unconfoundedness (or conditional independence) assumptions. We define average treatment effects on long-run outcomes among those who are treated at a given elapsed time in the initial state, and we aggregate this into average treatment effects for the full program. We introduce and implement inverse probability weighting (IPW) estimators for the various average effects. Besides unconfoundedness we do not impose any structure on the assignment process. We make the usual no-anticipation assumption (see e.g., Abbring and van den Berg, 2003) on the distribution of potential outcomes. To identify

LaLonde, 1996). Recently, the interest in long-run outcomes has been increasing (see e.g., the survey of Crépon and Van den Berg, 2016). Studies of US training programs have always focused more on earnings outcome variables.

[2]Besides training and post-unemployment earnings, examples can be constructed where the treatment is the usage of a particular medication and the eligibility period is the spell of a certain illness during which the medication is prescribed. Alternatively, one may consider intergenerational settings where the treatment is an event in the life of a child (e.g., the achievement of a certain level of education) and the outcome relates to features of the parents' life after the decision not to have additional children. Some individuals may never be treated in the eligibility state, as they will be childless at high ages and are unlikely to still obtain a child.

the counterfactual, we *only* use outcome observations of those who are not treated before leaving unemployment. As it turns out, the weights given to those observations do not only depend on observed confounders but also implicitly on the time spent in unemployment. Such weights establish a balance between the treated population at t and the control group consisting of the unemployed who are non-treated throughout the entire unemployment spell. We provide identification results and investigate the theoretical properties of our estimator in detail. Simulation results confirm that the IPW estimator is asymptotically unbiased and that the proposed bootstrap inference works well.

The baseline identification and estimation results are for a setting with selection based on time-invariant observed characteristics (covariates), absence of censoring of the durations in the initial state, and with only one long-run outcome realized after the individuals left the initial state. We then generalize the results to several different settings. This includes allowing for selection on *time-varying* covariates. For instance, in evaluations of training programs, the selection to the training may depend on characteristics that change during the unemployment spell. Another extension is to allow for right-censored durations which are common in many applications, for instance, due to drop-out from the study or a limited follow-up period. We also consider joint effects on both short-run and long-run outcomes. For training programs for unemployed workers, the short-run outcomes may be earnings a couple of months after the start of the training.

These methodological contributions are related to several strands of literature. A substantial body of work faces dynamic treatment assignment, either if the outcome of interest is the sojourn time in the initial eligibility state, such as the average unemployment duration after treatment,[3] or if the outcome of interest is realized partly or fully after unemployment. For post-unemployment outcomes, Lechner (1999, 2002), Wunsch and Lechner (2008) and Lechner et al. (2011) developed the "hypothetical treatment durations" approach, where hypothetical treatment dates are generated for each non-treated individual, and the actual and hypothetical treatment dates are used

[3]In a recent study, Vikström (2017) proposes an IPW estimator of average effects on the survival probability of being in the state of interest under unconfoundedness. That estimator is not designed with an eye on long-run outcomes and it essentially uses all not-yet treated in a period as the control group for treatment in that period. Note that in our case the control group only consists of non-treated, i.e. those who leave the state without having been treated. For that reason, the weights also differ. See Crépon et al. (2009) for a similar estimator based on blocking methods for the propensity score, and Muller et al. (2017) for a comparison of existing dynamic evaluation approaches for duration outcomes.

as covariates in the propensity score.[4] Another approach compares "treatment now versus treatment later", as introduced by Sianesi (2004), with applications in e.g. Sianesi (2008), Fitzenberger et al. (2008) and Biewen et al. (2014). For a given pre-treatment duration, it uses individuals who are not treated at this duration but possibly treated later on, as a control group. Note that this redefines the treatment effect.

As mentioned above, if the post-unemployment outcome is realized strictly after completion of the eligibility period, then, depending on the circumstances, the outcome may not be observed for a substantial fraction of the full sample. Lee (2009) and Lechner and Melly (2010) propose conditions under which average effects on the outcome can be bounded. We do not impose such conditions, and we mainly focus on long-run outcomes that are observed for every sample member. This holds by approximation for outcomes that are observable at a sufficiently long period of time after entry into the eligibility state. A number of other studies use random-effects models to jointly estimate duration effects and post-unemployment effects of exposure to labor market policy programs, allowing for selection on unobservables. This approach effectively extends the Timing-of-Events (ToE) approach by Abbring and Van den Berg (2003) developed for duration effects, by adding model equations for post-unemployment outcomes. In contrast, our approach does not impose model equations and does not restrict the heterogeneity of treatment effects.

Our paper contains two full-blown empirical applications; one for the US and one for Sweden. First, we examine the WIA training program in the US (see e.g., Andersson et al., 2016), which includes counselling and training services. We follow several previous studies and estimate the effect of training relative to other WIA services, i.e. the effect of training conditional on participation in WIA. This setting is similar to the example with training in unemployment. The WIA training program has an eligibility state (being registered at a WIA job center) within which individuals can enroll in WIA training. Assignment to WIA training is a dynamic process in which training starts after some elapsed duration in WIA. Thus, some of the currently non-trained will receive training before they leave WIA.

The results show that WIA training has positive effects on earnings after exit from WIA. Comparisons with a standard static evaluation approach reveal that the static approach underestimates the WIA training effect by roughly 20% (the difference is

[4] As Lechner (1999) writes, many ways can be defined to assign values to hypothetical treatment dates, and although they are intuitively plausible, it appears difficult to derive properties of the various approaches based on hypothetical treatment dates analytically.

significant). This is because the control group in a static approach includes too many participants who only remain in WIA for a short period, leading to downward bias since this, most likely, is a group with more favorable characteristics. The new dynamic approach in this paper corrects for this selection problem.

The second empirical application considers long-run earnings effects of the flagship training program for unemployed workers in Sweden. The main purpose of this program is to improve the skills and thereby to enhance reemployment chances. Starting with Regnér (2002), studies have concluded that the program did not raise post-unemployment earnings (see also the survey in Forslund and Vikström, 2011). This has been a reason for policy makers to cut down on training program volumes.[5] Our data allow us to follow each unemployed individual in our sample for more than ten years after entry into unemployment. We find that in the short run the program decreases annual labor earnings. This reflects lock-in effects, since some of these earnings are received while still in unemployment. However, in the medium and long run, the estimated earnings effects of the program are persistently significantly positive, leading to positive effects on cumulative earnings. We conclude that previous evaluations need to be revised in the light of these substantial positive earnings effects in the long run.

The outline of the paper is as follows. Section 2 presents the causal framework. Section 3 lays out the evaluation problem, and Section 4 contains the main identification results. Section 5 discusses estimation and statistical inference. In Section 6, we extend the results to different settings. Section 7 contains the US application and Section 8 reports the results from the Swedish application. Section 9 concludes.

2 Model

Consider an individual entering a state in which (s)he is eligible for a treatment. This state is called the initial state or eligibility state. The treatment may take place at some point in time while in the initial state but the individual may also leave this state at some point in time without having been treated. We are interested in the effect of the treatment on long-run outcomes. Initially, we consider the case when the long-run outcome is realized after individuals left the initial state. In Subsection 6.1 we allow the observation period for the long-run to overlap with the initial spell.

[5] More recent evaluations from the Swedish Public Employment Office with negative employment effects of training have also contributed to the decreased program volumes (e.g., PES, 2016).

Let the time clock start at the moment of entry into the initial state. We denote the duration spent in the initial state by T_u and the duration until treatment by T_s. Note that in this framework T_s is a latent variable as it can be right-censored by T_u. The potential time in the initial state if the individual is assigned to be treated at t_s is denoted by $T_u(t_s)$. The long-run outcome is denoted by Y, and its corresponding potential outcomes by $Y(t_s)$. Thus, $T_u(\infty)$ and $Y(\infty)$ capture the potential duration and the potential long-run outcome if the individual is assigned to be "never treated". In practice, infinity may be replaced by some upper bound on the eligibility interval length. (In the next subsection we discuss the role of $T_u(t_s)$ in the long-run effects on $Y(t_s)$.)

We take time to be discrete. We assume that, within each time period t with $T_s \geq t$, the binary event governing whether $T_s = t$ vs. $T_s > t$ is realized before the binary event governing whether $T_u = t$ vs. $T_u > t$. This means that we do not impose a one-period delay in the effect of t_s on T_u.

We adopt the "no anticipation" assumption (Abbring and Van den Berg, 2003), ruling out that the timing of a future treatment affects the current probability of leaving the initial state. Specifically, for each individual,

Assumption 1 (No anticipation in exit out of the eligibility state)**.**

$$P\left(T_u(t') = t\right) = P\left(T_u(t'') = t\right), \quad \forall t < \min(t', t'')$$

This implies that $P\left(T_u(t') = t\right) = P\left(T_u(\infty) = t\right)$, $\forall t < t'$.

We also require a no-anticipation assumption regarding $Y(t_s)$. Specifically,

Assumption 2 (No anticipation in long-run outcomes)**.**

$$Y(t') = Y(t''), \quad \forall\ T_u(t') = T_u(t'') < \min(t', t'').$$

This rules out that the potential outcomes $Y(t_s)$ are affected by behavior before t_s that is driven by the future treatment date t_s, *even* if this behavior is of no consequence for the moment of leaving the eligibility state before t_s. For example, what is ruled out is the following: while in the eligibility state, the individual acts upon private knowledge of his future moment of treatment in such a way that his long-run outcome is affected but his time in the eligibility state is not. (Note that *if* the probability of leaving would be affected by such behavior before t_s then Assumption 1 is violated.) Assumption

2 only considers $T_u(t') = T_u(t'')$ because if $T_u(t') \neq T_u(t'')$ then a difference in the long-run outcomes $Y(t')$ and $Y(t'')$ may be ascribed to an effect of T_u on Y. Together, Assumptions 1 and 2 ensure that individual outcomes do not vary with the moment of treatment as long as the treatment is not realized. In the remainder of the paper we therefore often simply refer to Assumptions 1 and 2 jointly as "the" no-anticipation assumptions.

Recall that in the model structure in this section, Y is always realized after T_u. Later on, when we allow the observation period for the long-run to overlap the initial spell, we need to adopt an alternative no-anticipation assumption for $Y(t_s)$.

In the absence of right-censoring of T_u, and by invoking Assumptions 1 and 2, we can express the actual long-run outcome Y as follows,

$$Y = \sum_{t=1}^{T_u} [\mathbf{I}(T_s = t) Y(t)] + \mathbf{I}(T_s > T_u) Y(\infty), \tag{1}$$

where $\mathbf{I}()$ is the indicator function. This is the observational rule. The first part of the right-hand side of (1) states that if the individual is treated before leaving the initial state, then we observe the long-run potential outcome corresponding to the actually observed time to treatment. The second part states that if an individual exits the initial state without having been treated then the observed outcome is the outcome corresponding to the assignment "never treated".

3 The evaluation problem

A possible object of interest is the average treatment effect of t_s on Y among those who are actually treated at t_s. This is referred to as ATET(t_s):

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

This contrasts the treatment at t_s with being given the assignment to be "never treated". By aggregating this over the distribution of T_s we obtain the average effect of the program in comparison to a setting where the program is absent (ignoring equilibrium effects).

An alternative object of interest is the average treatment effect of t_s on Y among all those who, if they were assigned to t_s, would still be in the initial state at that time t_s. Contrary to ATET(t_s), this object does not depend on the actual assignment mechanism. We refer to it as ATE(t_s),

$$\text{ATE}(t_s) = E(Y(t_s) - Y(\infty) \mid T_u(t_s) \geq t_s). \tag{3}$$

In general, these average causal effects of t_s on the long-run outcome $Y(t_s)$ may include (1) a direct effect and (2) an indirect that runs by way of the time $T_u(t_s)$ spent in the initial eligibility state. For example, with training programs for unemployed workers, (1) may capture that the training program improves the participants' human capital and the ensuing labor earnings, while (2) may capture that program participation speeds up exit out of unemployment and the resulting unemployment duration is used by employers as a signal of worker quality and hence as a determinant of earnings. In the long-run, as employers gather more and more information on worker quality, (or in a flexible labor market with full information,) indirect effects are likely to lose importance, so that the over-all effect primarily represents the direct effect. However, depending on the application at hand, it cannot be ruled out that the over-all effect includes an indirect effect, especially if the treatment is only targeted at the reduction of the time in the eligibility state.

In this paper, we do not attempt to separate the direct effect from the indirect effect. In different words, we examine the over-all comprehensive effect on Y regardless of the pathway. This is why the potential outcome notation $Y(t_s)$ does not explicate a causal effect of t_u on Y. In the sequel, in line with the "matching" evaluation literature on which we build, we mainly focus attention on ATET(t_s).

We denote the set of observed individual characteristics by X. Concerning the assignment process, we assume sequential unconfoundedness. For presentation reasons we will initially explain our identification results under the assumption that unconfoundedness holds conditional on *time-invariant* covariates. Subsection 6.2 generalizes the results allowing for selection on *time-varying* covariates.

Formally, let the binary indicator $P_t := 1$ iff $T_s = t$.

Assumption 3 (Assignment)**.** For all t,

$$P_t \perp \{Y(t_s)\}_t^\infty \mid X, T_s \geq t, T_u(\infty) \geq t.$$

Here, the joint distribution of $\{Y(t_s)\}_t^\infty$ is taken to cover all values $t_s = t, t+1, \ldots, \infty$.

In fact, inference on ATET(t_s) only requires P_t to be conditionally independent of the potential outcome $Y(\infty)$ if non-treated, while the full Assumption 3 is needed for ATE(t_s).

This assumption echoes the usual CIA unconfoundedness assumptions in the evaluation literature based on matching methods, including the references cited above. Somewhat informally, it rules out that there are systematic unobserved characteristics that drive both the treatment assignment and the long-run outcomes. In the case of training programs for unemployed workers, the argument has been made that caseworkers determine the assignment and that the data set used in the analysis include the information used by the caseworkers when deciding on the assignment. The assumption is also more likely to hold in cases with detailed administrative data and/or survey data that capture individual motivation, ability, and personality traits that may affect treatment assignments as well as outcomes.

As usual in non-experimental evaluations, an overlap condition or common support condition is needed. In the basic evaluation setting, this amounts to assuming that for all t and X we have that $\Pr(P_t = 1|T_s \geq t, T_u \geq t, X) < 1$. In addition, the usual SUTVA assumption needs to hold, ruling out various types of interference between the units in the sample (see e.g. Wooldridge, 2010). In the sequel, for expositional convenience, we tacitly assume common support and SUTVA wherever applicable, and we do not spell out the corresponding requirements in the derivations.

To explain the evaluation problem, consider first the first component of the expression for ATET(t_s). From a random sample of observed outcomes Y of those treated at time T_s, this component is straightforwardly identified:

$$E(Y(t_s)|T_s = t_s, T_u(t_s) \geq t_s) = E(Y|T_s = t_s, T_u \geq t_s). \quad (4)$$

The evaluation problem concerns the second component of ATET(t_s), that is, the counterfactual outcome $E(Y(\infty)|T_s = t_s, T_u(t_s) \geq t_s)$. The observational rule in (1) illustrates the main issues. First, it is clear that only non-treated individuals, i.e. individuals with $T_s > T_u$, can be informative on the counterfactual outcome under never treatment. Individuals who are not treated at t_s, but are treated later are not informative on the counterfactual outcome under never treatment. After all, since they are ultimately treated, their observed response, Y, corresponds to a potential outcome under a treatment.

Second, the potential control group of non-treated, defined by $T_s > T_u$, in general, constitutes a selective sample. Indicators like $\mathbf{I}[T_s > T_u | T_s = t_s, T_u \geq t_s]$ depend on the survival time T_u (i.e., on an intermediate outcome which may affect the outcome of interest), since the probability of treatment enrollment by construction increases with the time in the initial state. Thus, for a given period-by-period treatment probability, individuals with longer unemployment durations are less likely to remain non-treated. This arises purely because of the dynamic nature of the treatment assignment. To phrase this differently, individuals with relatively short durations after a treatment at t_s are overrepresented in the group of non-treated. Thus, even if treatment assignment is unconfounded, treatment assignments in subsequent time periods cause the group of non-treated to be systematically different.

4 Identification

4.1 The gist of the approach in a simple setting

This subsection describes the gist of our method in a simplified setting with two periods in which the treatment can take place whereas no treatments can occur thereafter. We consider the effect of treatment in the first period, $\text{ATET}(1) = E(Y(1) - Y(\infty) \mid T_s = 1)$. (Note that for the effect of a treatment in the first period there is no need to condition on survival.) Subsequent sections generalize this setting.

As a starting point, note that $E(Y(1)|T_s = 1) = E(Y|T_s = 1)$ follows directly from the observational rule. The identification challenge is to show that the counterfactual $E(Y(\infty)|T_s = 1)$ is identified under our assumptions. If we condition on X then, by the assignment-assumption (Assumption 3), the treated and non-treated are comparable:

$$E(Y(\infty)|T_s = 1, X) = E(Y(\infty)|T_s > 1, X). \tag{5}$$

By the law of iterated expectations

$$\begin{aligned} E(Y(\infty)|T_s &> 1, X) = \\ \Pr(T_u &= 1|T_s > 1, X)E(Y(\infty)|T_s > 1, T_u = 1, X) + \\ \Pr(T_u &> 1|T_s > 1, X)E(Y(\infty)|T_s > 1, T_u > 1, X). \end{aligned} \tag{6}$$

That is, the counterfactual outcome under never treatment is decomposed into average outcomes for individuals with $T_u = 1$ and with $T_u > 1$. Note that probabilities $\Pr(T_u =$

$1|T_s > 1, X)$ and $\Pr(T_u > 1|T_s > 1, X)$ are observed.

Next, the group with $T_u = 1$ in (6) consists of non-treated individuals who exit directly in period 1. For this group, the observational rule in (1) and no-anticipation (Assumptions 1 and 2) give

$$E(Y(\infty)|T_s > 1, T_u = 1, X) = E(Y|T_s > 1, T_u = 1, X). \tag{7}$$

This follows since for non-treated individuals with $T_s > T_u$, the observed long-run outcome Y equals the non-treated long-run potential outcome $Y(\infty)$.

For the group with $T_u > 1$, in (6), i.e., those who survive at least one additional time period, we have by the assignment-assumption (Assumption 3):

$$E(Y(\infty)|T_s > 1, T_u > 1, X) = E(Y(\infty)|T_s > 2, T_u > 1, X), \tag{8}$$

i.e. among the non-treated survivors, conditional on X, those who become treated in period 2 are comparable to those who remain non-treated. Next, since there are no treatments after period 2 and under no-anticipation, those with $T_s > 2$ remain non-treated, so that for this group the observed long-run outcome Y equals the non-treated long-run potential outcome, $Y(\infty)$:

$$E(Y(\infty)|T_s > 2, T_u > 1, X) = E(Y|T_s > 2, T_u > 1, X). \tag{9}$$

Then, by (5)-(9) we have

$$\begin{aligned} E(Y(\infty)|T_s &= 1, X) = \\ \Pr(T_u &= 1|T_s > 1, X)E(Y|T_s > 1, T_u = 1, X) + \\ \Pr(T_u &> 1|T_s > 1, X)E(Y|T_s > 2, T_u > 1, X). \end{aligned} \tag{10}$$

That is, the long-run outcomes for non-treated units with $T_u = 1$ and $T_u > 1$ are weighted in a specific way, based on the exit probabilities $\Pr(T_u = 1|T_s > 1, X)$, and this allows us to recover the long-run outcome under no treatment. This deals with the fact that some of the non-treated become treated in the second period.

4.2 Identification results

The first part of the derivation below concisely generalizes the line of reasoning in the previous subsection for an arbitrary number of periods. We show that $E(Y(\infty)|T_s = t_s, T_u(t_s) \geq t_s)$ (i.e., the second component of ATET(t_s)) is identified under our assumptions. Note that the first component, $E(Y(t_s)|T_s = t_s, T_u(t_s))$, is given by (4).

We first rewrite the counterfactual outcome using Assumptions 1 and 2,

$$E(Y(\infty) \mid T_s = t_s, T_u(t_s) \geq t_s) = E(Y(\infty) \mid T_s = t_s, T_u(\infty) \geq t_s). \tag{11}$$

Next, we condition on X and apply Assumption 3 (assignment) for period t_s:

$$E(Y(\infty) \mid T_s = t_s, T_u(\infty) \geq t_s, X) = E(Y(\infty) \mid T_s > t_s, T_u(\infty) \geq t_s, X). \tag{12}$$

Then, by the law of iterated expectations

$$E(Y(\infty) \mid T_s > t_s, T_u(\infty) \geq t_s, X) = \tag{13}$$

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

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

That is, the counterfactual outcome under "never treatment" can be decomposed into average outcomes for individuals with $T_u = t_s$ and for individuals with $T_u > t_s$. The former, with $T_u = t_s$, in (13) are non-treated individuals who leave the initial state directly in period t_s. For this group, the observational rule in (1) and Assumptions 1 and 2 give

$$E(Y(\infty)|T_s > t_s, T_u(\infty) = t_s, X) = E(Y|T_s > t_s, T_u = t_s, X). \tag{14}$$

Also, note that the treatment probabilities $\Pr(T_u = t_s|T_s > t_s, T_u(\infty) \geq t_s, X) = \Pr(T_u = t_s|T_s > t_s, T_u \geq t_s, X)$ and $\Pr(T_u > t_s|T_s > t_s, T_u(\infty) \geq t_s, X) = \Pr(T_u > t_s|T_s > t_s, T_u \geq t_s, X)$ are observed.

For the other group, with $T_u > t_s$, in (13), i.e., for those who survive at least one additional time period, we have

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

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

where the first equality follows from Assumption 3 for period t_s+1, and the second equality from re-writing. Next, using (13) by replacing t_s with t_s+1 we have

$$E(Y(\infty) \mid T_s > t_s+1, T_u(\infty) \geq t_s+1, X) = \tag{15}$$

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

For the sake of presentation, introduce some auxiliary notation for the distribution of T_u:

$$h(t, X) = \Pr(T_u = t \mid T_s > t, T_u \geq t, X).$$

Using this notation, (13)-(15) and (14) for period t_s+1 give

$$\begin{aligned}E(Y(\infty)|T_s > t_s, T_u(\infty) \geq t_s, X) = h(t_s, X) \quad & E(Y|T_s > t_s, T_u = t_s, X) + \\ [1-h(t_s, X)]h(t_s+1, X) \quad & E(Y|T_s > t_s+1, T_u = t_s+1, X) + \\ [1-h(t_s, X)][1-h(t_s+1, X)] \quad & E(Y(\infty)|T_s > t_s+1, T_u(\infty) > t_s+1, X).\end{aligned}$$

All parts of this expression are observed except $E(Y(\infty)|T_s > t_s+1, T_u(\infty) > t_s+1, X)$. However, for this outcome, we can iteratively use (13) and (14) for $t_s+2, \ldots, T_u^{\max}$, where $T_u^{\max}$ is maximum time in the initial state. This gives:

$$E(Y(\infty)|T_s > t_s, T_u(\infty) \geq t_s, X) = \sum_{k=t_s}^{T_u^{\max}} h(k, X) \prod_{m=t_s}^{k-1} [1-h(m, X)] E(Y|T_s > k, T_u = k, X). \tag{16}$$

Interestingly, this is the expectation of the random outcome variable Y over a discrete-time competing risks duration distribution. In this interpretation, the competing risks are treatment and exit out of the initial state, and only the observed outcomes after the second risk (exit) are used.

Next, note that

$$E(Y(\infty)|T_s = t_s, T_u(t_s) \geq t_s) = E_{X|T_s=t_s,T_u\geq t_s}\left[E(Y(\infty)|T_s = t_s, T_u(t_s) \geq t_s, X)\right]. \quad (17)$$

Then, $E(Y(\infty) \mid T_s = t_s, T_u(t_s) \geq t_s)$ is identified from (11)-(17). This gives the second component of ATET(t_s), and (4) gives the first component of ATET(t_s).This is summarized in Theorem 1.

Theorem 1 (ATET)**.** If Assumptions 1–3 hold then

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

$$E_{X|T_s=t_s,T_u\geq t_s}\left[\sum_{k=t_s}^{T_u^{\max}} h(k,X) \prod_{m=t_s}^{k-1}[1 - h(m,X)]E(Y|T_s > k, T_u = k, X)\right],$$

where

$$h(t,X) = \Pr(T_u = t|T_s > t, T_u \geq t, X).$$

In sum, the non-treated group (consisting of individuals leaving the initial state before becoming treated or actually never would have become treated) can be used to identify the counterfactual outcome for those treated after a certain elapsed duration. Theorem 1 shows that this is achieved by giving individuals leaving the initial state a differential weight depending on the duration in that state.

The above derivation demonstrates the independent roles of the unconfoundedness assumption and the no-anticipation assumption. The unconfoundedness 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 for the not-yet treated can be used to mimic the outcomes under never treatment even if some of the non-yet treated may be treated if they remain in the initial state.

Identification of ATE(t_s) follows using similar reasoning as above. For the second component of ATE(t_s), our assumptions give

$$E(Y(\infty)|T_u(t_s) \geq t_s) = E_{X|T_u \geq t_s}\left[E(Y(\infty)|T_u(\infty) \geq t_s, X)\right] = E_{X|T_u \geq t_s}\left[E(Y(\infty)|T_s > t_s, T_u(\infty) \geq t_s, X)\right], \tag{18}$$

and from (16):

$$E(Y(\infty)|T_s > t_s, T_u(\infty) \geq t_s, X) = \sum_{k=t_s}^{T_{us}^{\max}} h(k,X) \prod_{m=t_s}^{k-1} [1-h(m,X)] E(Y|T_s > k, T_u = k, X). \tag{19}$$

Thus, from (18)-(19) we have

$$E(Y(\infty)|T_u(t_s) \geq t_s) = \tag{20}$$

$$E_{X|T_u \geq t_s}\left[\sum_{k=t_s}^{T_u^{\max}} h(k,X) \prod_{m=t_s}^{k-1} [1-h(m,X)] E(Y|T_s > k, T_u = k, X)\right].$$

For the first component of ATE(t_s):

$$\begin{aligned} E(Y(t_s)|T_u(t_s) &\geq t_s) = E(Y(t_s)|T_u(\infty) \geq t_s) \\ &= E_{X|T_u \geq t_s}\left[E(Y(t_s)|T_u(\infty) \geq t_s, X)\right] \\ &= E_{X|T_u \geq t_s}\left[E(Y(t_s)|T_s = t_s, T_u(\infty) \geq t_s, X)\right] \\ &= E_{X|T_u \geq t_s}\left[E(Y|T_s = t_s, T_u \geq t_s, X)\right], \end{aligned} \tag{21}$$

where the first equality follows from Assumptions 1 and 2, the second by the law of iterated expectations, the third by Assumption 3 and the fourth equality from the observational rule and Assumptions 1 and 2.

From (20) and (21) we thus obtain,

Theorem 2 (ATE)**.** If Assumptions 1–3 hold then

$$\text{ATE}(t_s) = E_{X|T_u \geq t_s}\left[E(Y|T_s = t_s, T_u \geq t_s, X)\right] -$$

$$E_{X|T_u \geq t_s} \left[\sum_{k=t_s}^{T_u^{\max}} h(k,X) \prod_{m=t_s}^{k-1} [1 - h(m,X)] E(Y|T_s > k, T_u = k, X) \right]$$

where

$$h(t, X) = \Pr(T_u = t | T_s > t, T_u \geq t, X).$$

ATET(t_s) and ATE(t_s) can be aggregated over the distribution of T_s to obtain the over-all average effect of the program. Specifically,

$$\text{ATET} = E_{t_s}[E(Y(t_s) - Y(\infty) \mid T_s = t_s, T_u(t_s) \geq t_s)]$$

where the outer expectation is taken over the distribution of T_s. A similar expression applies to ATE. In this context it is important to point out that all quantities above refer to (and will be quantified by data from) a world in which the program exists. In particular, before individuals are treated, they may be aware of the existence of the program and act accordingly. Assumption 1 stipulates that such ex ante behavior is equal for all individuals until the treatment occurs. Still, the behavior in the various control groups used for inference on counterfactual outcomes may be affected by such ex ante behavior. For example, the existence of a training program may reduce efforts to find jobs before treatment occurs, for every unemployed individual in the market. As such, the over-all effects ATET and ATE capture effects relative to "no treatment" in a world where the program exists.

5 Estimation

We rely on the constructive identification results of the previous section to construct estimators of the average treatment effects of interest. Specifically, in Appendix A it is shown that if Assumptions 1–3 hold, then an unbiased estimator of ATET(t_s) is provided by:

$$\widehat{\text{ATET}}(t_s) = \frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} = t_s, T_{u,i} \geq t_s} Y_i - \tag{22}$$

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

where N_t is the number of non-treated survivors at the beginning of t and

$$\begin{aligned} w^{t_s}(t_u, X) &= \frac{p(t_s, X)}{\rho_{t_s}[1-p(t_s, X)]} \frac{1}{\prod_{m=t_s+1}^{t_u}[1-p(m, X)]} \\ p(t, X) &= \Pr(T_s = t | T_s \geq t, T_u \geq t, X) \\ \rho_t &= \Pr(T_s = t | T_s \geq t, T_u \geq t), \end{aligned} \tag{23}$$

in practice the weights, w, are replaced by estimated weights, $\widehat{w}$, with estimated treatment probabilities (propensity scores), $\widehat{p}$. For ATE(t_s) we have by the results in Appendix A

$$\widehat{\text{ATE}}(t_s) = \frac{1}{\sum\limits_{i \in T_{s,i} = t_s, T_{u,i} \geq t_s} w_{ATE1}^{t_s}(X_i)} \sum_{i \in T_{s,i} = t_s, T_{u,i} \geq t_s} w_{ATE1}^{t_s}(X_i) Y_i - \tag{24}$$

$$\frac{1}{\sum\limits_{i \in T_{s,i} > T_{u,i} \geq t_s} w_{ATE0}^{t_s}(T_{u,i}, X_i)} \sum_{i \in T_{s,i} > T_{u,i} \geq t_s} w_{ATE0}^{t_s}(T_{u,i}, X_i) Y_i,$$

where

$$\begin{aligned} w_{ATE1}^{t}(X) &= \frac{1}{p(t, X)} \\ w_{ATE1}^{t_s}(t_u, X) &= \frac{1}{1-p(t_s, X)} \frac{1}{\prod_{m=t_s+1}^{t_u}[1-p(m, X)]}. \end{aligned}$$

This follows the ideas of Horvitz and Thomson (1952) and weighs 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 (22) and (24) are based on normalized weights. The reason 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 (24). 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 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 start treatment at $t_s+1,\ldots$. As a background, consider the case with only treatment assignment at t_s, so that everyone who do not receive treatment at t_s remains untreated in all time periods. We focus on non-normalized weights. Then, in large samples:

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

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

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

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

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

Next,

$$E[Y(\infty)|T_s > t_s, T_u \geq t_s, X] = \tag{25}$$

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

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 $T_u^{\max}$. 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 (25), this is an average that gives 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, because 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 above correct for this, since the weights depend on both the observed characteristics and the time in the initial state.

To estimate the over-all average effect ATET aggregated over all possible t_s, we average over the distribution of T_s, where the fraction treated after t is equal to $N_t/\sum_{m=1}^{T_u^{\max}} N_m$. Specifically, we average $\widehat{\text{ATET}}(t_s)$ over the effects for specific pre-treatment durations. This gives:

$$\widehat{\text{ATET}} = \sum_{t=1}^{T_u^{\max}} \frac{N_t}{\sum_{m=1}^{T_u^{\max}} N_m} \frac{1}{\rho_t N_t} \sum_{i \in T_{s,i}=t, T_{u,i} \geq t} Y_i, - \tag{26}$$

$$\sum_{t=1}^{T_u^{\max}} \frac{N_t}{\sum_{m=1}^{T_u^{\max}} N_m} \frac{1}{\sum\limits_{i \in T_{s,i} > T_{u,i} \geq t} w^t(T_{u,i}, X_i)} \sum_{i \in T_{s,i} > T_{u,i} \geq t} w^t(T_{u,i}, X_i) Y_i.$$

A similar approach can be used to estimate ATE.

It is well known that IPW estimation may be sensitive to extreme values of the propensity scores, since single observations may receive too large weight (see e.g., Frölich 2004, Huber et al. 2013, Busso et al. 2014). One way to overcome this problem is trimming. One trimming approach is the three-step approach in Huber et al. (2013). For average treatment effects, their approach imposes zero weights to all treated (controls) whose share of the sum of all original weights in the treatment (control) group is larger than $c\%$ (e.g., 4%). Thereafter, normalize the weights again and finally discard observations whose propensity score is smaller (greater) than the maximum (minimum) of the minimum (maximum) scores among the treated and controls. This assures that extreme values are discarded and that the same type of individuals are discarded in both treatment arms. This approach is also applicable to our estimator, noting that in our case the weights are a function of several propensity scores.

Concerning inference, (22) and (24) 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 depend on several propensity scores instead of a single propensity score. Nevertheless, if the propensity scores are known, the large-sample variances for $\widehat{\text{ATE}}(t_s)$ can be derived using similar reasoning as in Lunceford and Davidian (2004). More generally, Hirano et al. (2003) show that the variance decreases if estimated scores are used instead of true scores. In that case, standard errors can be obtained by bootstrapping.

A simulation study on the properties of the estimator is given in Appendix B. The simulations show that the bias of our new estimator is virtually zero in all simulations, the bootstrap estimator for the standard errors has correct size, and the standard

standard error decreases by roughly 50% when the sample size is increased by a factor of four (estimator is $\sqrt{N}$-convergent). We also compare with a static IPW estimator, which as expected, is biased, and the bias is increasing in, for instance, the share of treated.

6 Extensions

6.1 Short-run and long-run outcomes

So far we have considered effects on the long-run outcome realized after the individuals left the initial state. We now consider effects on both short-run and long-run outcomes. Specifically, define Y_t as the observed outcome in period t. The corresponding potential outcomes are denoted by $Y_t(t_s)$. Here, the object of interest is the average treatment effect of treatment at t_s on the outcome in period $t_s + \tau$ (i.e., τ periods after the start of the treatment) among the treated at t_s:

$$\text{ATET}(t_s, \tau) = E(Y_{t_s+\tau}(t_s) - Y_{t_s+\tau}(\infty)|T_s = t_s, T_u(t_s) \geq t_s). \tag{27}$$

For training programs for unemployed workers, this may be the effect on earnings a certain number of periods after the start of the training, so that $\text{ATET}(t_s, \tau)$ for smaller values of τ capture any lock-in effects during the program, and subsequent $\text{ATET}(t_s, \tau)$ reflect the effects in the medium-run and the long-run.

In Section 2 we introduced the no-anticipation Assumptions 1 and 2. Since Y is now a short-term outcome, we now need a no-anticipation assumption for Y_t that is similar to Assumption 1 for T_u,

Assumption 4 (No anticipation, short-run outcomes)**.**

$$E[Y_t(t')] = E[Y_t(t'')], \quad t < \min(t', t'').$$

For completeness we also generalize the unconfoundedness assumption:

Assumption 5 (Assignment, short-run outcomes)**.** For all t,

$$P_t \perp \{Y_t(t_s), \ldots, Y_\infty(t_s)\}_t^\infty \mid X, T_s \geq t, T_u \geq t.$$

The observation rule is now

$$Y_t = \sum_{m=1}^{t} [\mathbf{I}(T_s = m) Y_t(m)] + \mathbf{I}(T_s > \min(T_{u,} t)) Y_t(\infty), \tag{28}$$

which holds in the absence of right-censoring of T_u and by invoking Assumptions 1 and 4. The first part of the right-hand side of (28) states that if the individual is treated before t, then we observe the potential outcome corresponding to the actually observed time to treatment.[6] The second part states that if an individual have left the initial state before t without having been treated or if the individual is treated after t, then the observed outcome at t is the outcome corresponding to the assignment "never treated".

The first component of ATET(t_s, τ) is identified from the observed outcomes, Y_t, of those treated at time t_s:

$$E(Y_{t_s+\tau}(t_s) | T_s = t_s, T_u(t_s) \geq t_s) = E(Y_{t_s+\tau} | T_s = t_s, T_u \geq t_s). \tag{29}$$

For the second component of ATET(t_s, τ), condition on X. Then, by Assumptions 1, 4 and 5,

$$E(Y_{t_s+\tau}(\infty) | T_s = t_s, T_u(t_s) \geq t_s, X) = E(Y_{t_s+\tau}(\infty) | T_s = t_s, T_u(\infty) \geq t_s, X) \tag{30}$$

$$= E(Y_{t_s+\tau}(\infty) | T_s > t_s, T_u(\infty) \geq t_s, X),$$

and by the law of iterated expectations and the observational rule

$$E(Y_{t_s+\tau}(\infty) | T_s > t_s, T_u(\infty) \geq t_s, X) = h(t_s, X) E(Y_{t_s+\tau}(\infty) | T_s > t_s, T_u(\infty) = t_s, X) + \tag{31}$$

$$[1 - h(t_s, X)] E(Y_{t_s+\tau}(\infty) | T_s > t_s, T_u(\infty) > t_s, X),$$

[6] In applications, if the time unit in the data is large, then it is possible that the observed Y_t (say, total earnings in t) for $t = T_s$ reflects to some extent the earnings within period t that were obtained in the sub-period prior to the treatment, and, indeed, that a realization of T_s in the middle of period t depends on earnings earlier in that same time period. This is a commonly encountered challenge in applied discrete-time dynamic treatment analyses. In our setting it can be dealt with by allowing the conditioning set X to be time-varying (see the next subsection for this approach) but only if the data provide outcomes on a finer grid than the time unit in the data. Of course it depends on the institutional setting as well as on the data whether the treatment assignment can give rise to such concerns.

where $E(Y_{t_s+\tau}(\infty)|T_s > t_s, T_u(\infty) = t_s, X) = E(Y_{t_s+\tau}|T_s > t_s, T_u = t_s, X)$, and the probability $h(t_s, X)$ also is observed. By Assumption 5 for period $t_s + 1$ and (31) by replacing t_s with $t_s + 1$ we have

$$E(Y_{t_s+\tau}(\infty)|T_s > t_s, T_u \geq t_s, X) = E(Y_{t_s+\tau}(\infty)|T_s > t_s+1, T_u(\infty) \geq t_s+1, X) = \tag{32}$$

$$h(t_s + 1, X)E(Y_{t_s+\tau}(\infty)|T_s > t_s + 1, T_u(\infty) = t_s + 1, X)$$

$$+[1 - h(t_s + 1, X)]E(Y_{t_s+\tau}(\infty)|T_s > t_s + 1, T_u(\infty) > t_s + 1, X),$$

where the first equality follows from Assumption 5 and the second from (31). Iteratively, for $t_s + 2, \ldots, t$ gives

$$E(Y_{t_s+\tau}(\infty)|T_s > t_s, T_u(\infty) \geq t_s, X) = \tag{33}$$

$$\sum_{k=t_s}^{t_s+\tau} h(k, X) \prod_{m=t_s}^{k-1} [1 - h(m, X)]E(Y|T_s > k, T_u = k, X)+$$

$$\prod_{m=t_s}^{t_s+\tau} [1 - h(m, X)]E(Y_{t_s+\tau}|T_s > t_s + \tau, T_u > t_s + \tau, X).$$

Combining (30) and (33) and averaging over X gives the second component of (27). All these results are in Theorem 3.

Theorem 3 (ATET with short-run and long-run outcomes)**.** If Assumptions 1, 4 and 5 hold then

$$\text{ATET}(t_s, \tau) = E(Y_{t_s+\tau}|T_s = t_s, T_u \geq t_s)-$$

$$E_{X|T_s=t_s,T_u\geq t_s}[\sum_{k=t_s}^{t_s+\tau} h(k, X) \prod_{m=t_s}^{k-1} [1 - h(m, X)]E(Y|T_s > k, T_u = k, X)-$$

$$\prod_{m=t_s}^{t_s+\tau} [1 - h(m, X)]E(Y_{t_s+\tau}|T_s > t_s + \tau, T_u > t_s + \tau, X)],$$

where

$$h(t, X) = \Pr(T_u = t|T_s > t, T_u \geq t, X).$$

We can also extend the estimation results in a similar way. In Appendix A, we show that if Assumptions 1, 4 and 5 hold an unbiased estimator of ATET(t_s) is:

$$\widehat{\text{ATET}}(t_s) = \frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} Y_{t_s+\tau,i} - \tag{34}$$

$$\frac{1}{\rho_{t_s} N_{t_s}} \left[\sum_{i \in T_{s,i} > T_{u,i}, t_s+\tau \geq T_{u,i} \geq t_s} w^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i} + \sum_{i \in T_{s,i} > t_s+\tau, T_{u,i} > t_s+\tau} w_\tau^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i} \right],$$

where $w^{t_s}(t_u, X)$ is given by (23) and

$$w_\tau^{t_s}(X) = \frac{p(t_s, X)}{\prod_{m=t_s}^{t_s+\tau} 1 - p(m, X)}.$$

6.2 Time-varying covariates

We now allow for selection on *time-varying* observed covariates. In evaluations of active labor market programs, this allows treatment assignments to not only depend on characteristics measured at the beginning of the unemployment spell, but also on characteristics that change during the unemployment spell. For instance, some unemployed workers divorce and local labor market conditions can change during the unemployment spell.

To rule out effects of the treatment in period t on X, the covariates determining treatment assignment in t should be measured before assignments are made. For that reason we use the notation X_{t^-} for the observed covariates at t, where t^- indicates that X is measured at least slightly before t. Note that X_{t^-} may include covariates from previous periods, even the entire vector of covariates from all previous periods. By analogy to Assumption 1, we require a "no-anticipation" assumption regarding future changes in the time-varying covariates.

With time-varying covariates our sequential unconfoundedness assumption is:

Assumption 6 (Assignment time-varying covariates)**.** For all t,

$$[P_t \perp \{Y(t_s)\}_t^\infty \mid X_{t^-}, T_s \geq t, T_u(\infty) \geq t]$$

As above, the identification problems concerns the counterfactual outcome $E(Y(\infty)|T_s = t_s, T_u(t_s) \geq t_s)$. Here, similar reasoning as above can be used. The main difference is that we now first average over $X_{t_s}^-$, then over $X_{t_s+1}^-$ and so on. Specifically, we have the following result:[7]

Theorem 4 (ATET with time-varying covariates)**.** If Assumptions 1, 2 and 6 hold then

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

$$[E_{X^-_{t_s}|T_s=t_s,T_u\geq t_s}[h(X^-_{t_s})E(Y|T_s > t_s, T_u = t_s, X^-_{t_s})+$$

$$[1 - h(X^-_{t_s})]E_{X^-_{t_s+1}|T_s>t_s,T_u>t_s,X^-_{t_s}}[h(X^-_{t_s+1})E(Y|T_s > t_s + 1, T_u = t_s + 1, X^-_{t_s+1})+$$

$$[1-h(X^-_{t_{s+1}})]E_{X^-_{t_s+2}|T_s>t_s+1,T_u>t_s+1,X^-_{t_s+1}}[h(X^-_{t_{s+2}})E(Y|T_s > t_s+2, T_u = t_s+2, X^-_{t_s+2})+\ldots+$$

$$[1 - h(X^-_{T_u^{\max}-1})]E_{X^-_{T_u^{\max}}|T_s>T_u^{\max}-1,T_u>T_u^{\max}-1,X^-_{T_u^{\max}-1}}[h(X^-_{T_u^{\max}}) \times$$
$$E(Y|T_s > T_u^{\max}, T_u = T_u^{\max}, X^-_{T_u^{\max}})]\ldots]]]].$$

where

$$h(X^-_t) = \Pr(T_u = t|T_s > t, T_u \geq t, X^-_t).$$

Proof See Appendix A.

These identification results show that average effects are identified with time-varying covariates. In terms of estimation, Appendix A shows that if Assumptions 1 and 2 and 6 hold an unbiased estimator of $\text{ATET}(t_s)$ is:

$$\widehat{\text{ATET}}(t_s) = \frac{1}{\rho_{t_s}N_{t_s}} \sum_{i\in T_{s,i}=t_s,T_{u,i}\geq t_s} Y_i - \frac{1}{\rho_{t_s}N_{t_s}} \sum_{i\in T_{s,i}>T_{u,i}\geq t_s} w^{t_s}(T_{u,i}, X^-_i)Y_i \qquad (35)$$

where

$$w^{t_s}(t_u, X^-) = \frac{p(t_s, X^-_{t_s})}{\prod_{m=t_s}^{t_u}[1 - p(m, X^-_m)]}.$$

[7] The standard overlap condition for treatment assignment now is that for all t and X_{t^-} $\Pr(P_t = 1|T_s \geq t, T_u \geq t, X_{t^-}) < 1$, taking censoring into account.

6.3 Right-censored durations

We now allow for right-censoring of the durations in the eligibility state. It is important to explain what we mean with this in the context of a long-run post-spell outcome. First, the right-censoring can only take place while the unit is in the eligibility state. Let T_c denote the (possibly individual-specific) time until the unit is right-censored. At each discrete elapsed duration in the eligibility state, the binary event governing whether $T_c = t$ vs. $T_c > t$ is realized before the binary events governing whether $T_s = t$ vs. $T_s > t$ and $T_u = t$ vs. $T_u > t$. This implies that at a given time unit, censoring is realized before treatment assignment. If $T_c = t$, any treatment realizations and exits at t and after t are unobserved. Note that we can write $T_c = \infty$ if $T_c > T_u$.

Secondly, the long-run outcome is assumed to be unobserved if the unit is right-censored, i.e., if $T_c \leq T_u$. This introduces another selection problem because any right-censored unit does not carry any information about the long-run outcomes and the durations of certain types of individuals may be censored at a higher rate. Since both treated and non-treated durations can be censored, this problem occurs for both non-treated units and the counterfactual potential outcome under no treatment, as well as for the treated units and the treated potential outcomes.

The above type of right-censoring is common in many applications. For instance, individuals may drop out from the study, or there may be a limited follow-up period after the initial sample is drawn. In the case of unemployment durations, individuals may drop out due to emigration, mortality, or exit out of the labor force. Individuals who do not enter employment after a certain time period may be transferred to welfare payments and thus end up in different administrative registers.

Since it is assumed that the realization of the right-censoring outcome is the first event within each period, we now have the following average treatment effect of t_s on Y among those who are actually treated at t_s:

$$\text{ATET}(t_s) = E(Y(t_s) - Y(\infty) \mid T_s = t_s, T_c > t_s, T_u(t_s) \geq t_s). \tag{36}$$

We adjust for right-censoring in a similar way as for treatment assignments and consider identification under unconfounded right-censoring. Formally, let the binary indicator $C_t := 1$ iff $T_c = t$. The assumption is:

Assumption 7 (Right-censored process)**.** For all t,

$$[C_t \perp \{Y(t_s)\}_t^\infty \mid X, T_s \geq t, T_c \geq t, T_u \geq t],$$

where, as before, the joint distribution of $\{Y(t_s)\}_t^\infty$ is taken to cover all values $t_s = t, t+1, \ldots, \infty$. This assumption echoes the unconfoundedness assumption for treatment assignments and rules out that there are systematic unobserved characteristics that drive both the right censoring and the long-run outcome. As for Assumption 3, the plausibility of Assumption 7 depends on the exact censoring process and on the information available.[8]

Using similar reasoning as above, we arrive at Theorem 5,

Theorem 5 (ATET with right-censored durations)**.** If Assumptions 1–3 and 7 hold then

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

$$E_{X|T_s=t_s,T_c>t_s,T_u\geq t_s}\left[\sum_{k=t_s}^{T_u^{\max}} h_{c1}(t,X,t_s)\prod_{m=t_s}^{k-1}[1-h_{c1}(t,X,t_s)]E(Y|T_s=t_s,T_c>k,T_u=k,X)\right]-$$

$$E_{X|T_s=t_s,T_c>t_s,T_u\geq t_s}\left[\sum_{k=t_s}^{T_u^{\max}} h_c(k,X)\prod_{m=t_s}^{k-1}[1-h_c(m,X)]E(Y|T_s>k,T_c>k,T_u=k,X)\right].$$

where

$$\begin{aligned} h_{c1}(t,X,t_s) &= \Pr(T_u=t \mid T_s=t_s, T_c>t, T_u\geq t, X). \\ h_c(t,X) &= \Pr(T_u=t \mid T_s>t, T_c>t, T_u\geq t, X). \end{aligned}$$

Proof See Appendix A.

These identification results show that the average effects are identified with right-censoring. The only difference is that the right-censoring introduces another selection problem, so that in addition to selection due treatment assignments, there is also selection due to right-censored durations.

In terms of estimation, in Appendix A it is shown that if Assumptions 1–3, and 7

[8] An overlap condition for the censoring process is also needed. For all t and X $\Pr(C_t=1|T_c\geq t, T_u\geq t, X)<1$. Formally, the standard overlap condition for treatment assignment now is: for all t and X $\Pr(P_t=1|T_s\geq t, T_c>t, T_u\geq t, X)<1$, taking censoring into account.

hold, then an unbiased estimator of $\text{ATET}(t_s)$ is:

$$\widehat{\text{ATET}}(t_s) = \frac{1}{\sum_{i \in T_{s,i}=t_s, T_{c,i}>T_{u,i}, T_{u,i}\geq t_s} w_{c1}^{t_s}(T_{u,i}, X_i)} \sum_{i \in T_{s,i}=t_s, T_{c,i}>T_{u,i}, T_{u,i}\geq t_s} w_{c1}^{t_s}(T_{u,i}, X_i) Y_i - \tag{37}$$

$$\frac{1}{\sum_{i \in T_{s,i}>T_{u,i}, T_{c,i}>T_{u,i}, T_{u,i}\geq t_s} w_{c0}^{t_s}(T_{u,i}, X_i)} \sum_{i \in T_{s,i}>T_{u,i}, T_{c,i}>T_{u,i}, T_{u,i}\geq t_s} w_{c0}^{t_s}(T_{u,i}, X_i) Y_i,$$

where $N_{t_s}^c$ is the number of non-treated survivors at the beginning of t_s with durations censored after t_s and

$$\begin{aligned}
w_{c1}^{t_s}(t_u, X) &= \frac{1}{\prod_{m=t_s+1}^{t_u}[1 - e_{c1}(m, t_s, X)]} \\
w_{c0}^{t_s}(t_u, X) &= \frac{p_c(t_s, X)}{\rho_{t_s}^c [1 - p_c(t_s, X)] \prod_{m=t_s+1}^{t_u}[1 - p_c(m, X)][1 - e_{c0}(m, X)]} \\
\rho_t^c &= \Pr(T_s = t | T_u \geq t, T_c > t, T_s \geq t) \\
p_c(t, X) &= \Pr(T_s = t | T_s \geq t, T_c > t, T_u \geq t, X) \\
e_{c1}(t_c, t_s, X) &= \Pr(T_c = t_c | T_s = t_s, T_c \geq t_c, T_u \geq t_c, X) \\
e_{c0}(t, X) &= \Pr(T_c = t | T_s \geq t, T_c \geq t, T_u \geq t, X).
\end{aligned}$$

7 WIA training in the US

7.1 Background

The Workforce Investment Act (WIA) of 1998 replaced the Job Training Partnership Act (JTPA). The WIA program provides federal resources for employment services at the state and the local level. One important goal of WIA was to consolidate services for unemployed workers. The coordinated services now take place at around 2,500 One-Stop centers (American Job Centers) nationwide. All participation in WIA is voluntary, but caseworkers at the local job center need to authorize all staff-assisted services, including training. The services are provided through three funding streams aimed at adult workers, dislocated workers, and youths.[9]

The services are offered at three levels. All WIA participants receive core services,

[9] Adults are individuals 18 years satisfying certain criteria, dislocated workers are comprise adults who recently lost a job, and the youth program target youths in ages 18-21.

which include staff-assisted job search, placement and labor market information, and basic counseling. The second level involves intensive services with more comprehensive assessment and counseling, career planning, and in some case shorter courses.[10] The third level involves training services, such as on-the-job training, apprenticeships, training at vocational schools, and community colleges. The idea is that the participants should start with the core services and if no job is found proceed to the intensive services and/or training, However, in a national evaluation of the implementation of WIA, D'Amico et al. (2004) find extensive local and state variation in the implementation of WIA. Some participants follow the intended sequence with more and more intensified services, while others almost immediately proceed to training services.[11]

Several previous studies evaluate the service offered within the WIA program. In a recent study, Andersson et al. (2016) compare WIA participants who receive training to other WIA participants, and estimate the effect of training conditional on WIA participation. Their preferred estimates (data for 1999–2005) reveal significantly positive effects of WIA training on earnings and employment for workers served under the adult funding stream, but mostly negative effects for dislocated workers. Heinrich et al. (2013) find positive effects on earnings from WIA training for workers in the WIA adult program. However, for dislocated workers they find negative short-run effects and modest positive effects in the long-run. Hollenbeck et al. (2005) conclude that WIA services increase employment rates and that WIA training generates positive effects in comparison with other WIA services. Ao et al. (2016) study the effects of the length of WIA participation. Finally, McConnell et al. (2016) report evidence from a randomized trial with. One result from the 15-month impact evaluation is that WIA training did not increase earnings when compared to other WIA services.

7.2 The dynamic evaluation setting

We follow Andersson et al. (2016) and Heinrich et al. (2013) and study the impact of WIA training conditional on WIA registration. That is, we compare WIA participants who receive training and perhaps also other types of services to WIA participants who received only core and/or intensive services. In this setting, there are two durations. First, T_u, is time in the WIA program, i.e. WIA registration time. An individual be-

[10]The implementation of the intensive services vary across local area. Some areas provide a rich set of intensive services, while other areas mainly use the intensive services as preparation for training and/or exclusively as individualized job search assistance.

[11]D'Amico et al. (2004) and Barnow and King (2005) provide detailed descriptions of WIA.

comes eligible for WIA services when (s)he enters WIA and remains eligible while in the WIA program. In a sense this resembles the classic European training program eligibility where individuals typically are eligible for training while unemployed. Second, the treatment duration, T_s, is the time from WIA registration to training. The outcome of interest, Y, is earnings measured some time after the WIA training.

Note that the WIA selection process is a dynamic process. Some participants start with core services and then proceed with training, while other participants start training in the first or second month after WIA registration. This gives rise to extensive variation in the time from WIA registration to training, which is illustrated in Figure 1, which shows the distribution of the time to WIA training in months.[12] The figure shows that a rather large fraction of all training episodes start in the first month after WIA registration, but 23.1% of the training participants wait more than three months before they enroll in training. Those who start training after several months are workers who remain on WIA services after several months, most likely, are a selected sample of less successful participants. The implication is that treatment status as either treated (WIA training) or non-treated (WIA services but no training) depends on the time in WIA, which creates the dynamic treatment assignment problem addressed in this paper.

7.3 Data and estimation details

To evaluate WIA training, the WIASRD public use files are used. It contains information on all WIA participants. We select all WIA registration spells that started between April 1, 2011 and March 31, 2013. We follow previous studies and focus on the adult program and the dislocated workers program, and consider the effect of the first training program within the first 12 months in WIA. As outcomes we use the available information in the WIASRD public use files and study wages over four quarters after WIA.

The WIA selection process involves both WIA participants and the WIA staff. WIA participants' desires for training will depend on, for instance, their age, motivation, ability to learn, perception of the job market and beliefs about the value of training. All these factors will also influence the actions taken by the WIA staff. They will also take the budget set of the local WIA into account. Based on detailed information on this

[12]When WIA was initially implemented it placed large focus on a first-work policy, but later guidelines placed more emphasizes on training (Barnow and King, 2005). This is why training often starts in the first months.

selection process and using insights from studies, such as Lechner and Wunsch (2013) and Caliendo et al. (2017), that compare alternative sets of conditioning variables, Andersson et al. (2016) select a number of variables to control for in the analysis. This includes variables that are directly observed (e.g., age), variables that are proxies for motivation and ability (e.g., pre-program wages), and geographic indicators to control for the state of the local economy. All this allows Andersson et al. (2013) to argue that unconfoundedness holds.[13] They also argue that other WIA participants represent a natural comparison group for WIA training participants, and that there is a substantial amount of WIA caseworker discretion over the different WIA services, which lends further support to the unconfoundedness assumption. Heinrich et al. (2013) use a similar set of background characteristics and also evaluate under unconfoundedness.

We also analyze the WIA training based on unconfounded treatment assignment. The conditioning variables include sex, race (3 categories), age (10 categories), education (4 categories), disability indicator, veteran indicator, three calendar quarters of pre-WIA wages, registration year and quarter indicators, and a full set of state dummies. Compared to Andersson et al. (2016), the main difference is that we do not have information on geographic location within each state (the local One-Stop office at which the participant enrolled), because this information is excluded from the public use files. This is unfortunate, since Andersson et al. (2016) finds that controlling for the geographic location of the WIA offices is important. Since our analysis is restricted to WIASARD administrative data we also have somewhat less information on pre-WIA wages.[14,15]

Table 1 presents sample statistics for training participants and the untreated. The sample includes roughly 1.5 million WIA registration spells of which 231,540 (13.0%) concern participation in training. The reported background characteristics show that there are more females, blacks and younger workers among the training participants.

[13] Andersson et al. (2016) also compare their main selection on observed variables with difference-in-differences type of estimators that control for time invariant unobserved heterogeneity, but find very small differences.

[14] Andersson et al. (2016) also use matched employer data on from the Longitudinal Employer-Household Dynamics to obtain characteristics of the last employer before registration, including information on industry, size and turnover rates. They conclude that the additional firm variables do not pass a researcher cost-benefit test in this context based on what they add in terms of the credibility of the CIA.

[15] Besides unconfoundedness, no-anticipation also needs to hold. Since there are several sources of uncertainty in the assignment process we believe that this assumption holds. For instance, as already mentioned, WIA staff have large influence over enrollment decisions, making it difficult for the workers to predict the exact timing of the training.

Concerning estimation details, our dynamic IPW estimator involves estimation of a series of propensity scores, one for each time period. These propensity scores are estimated using logistic regression models. Due to the lower number of treated after four months on WIA, the propensity scores are estimated jointly from the fourth month and onwards (with indicators for each duration month). Standard errors are obtained using bootstrap (99 replications).[16]

We compare the dynamic estimator to a static IPW approach. Here, treated individuals are those who obtain training within the first 12 months in WIA. We adjust for differences between treated and non-treated using a standard IPW approach:

$$\delta_{StaticIPW} = \frac{1}{n_1}\sum_{i=1}^{N} W_i Y_i - \frac{1}{n_0}\sum_{i=1}^{N}\left(\frac{1}{n_0}\sum_{i=1}^{N}\frac{\hat{e}(x_i)(1-W_i)}{1-\hat{e}(x_i)}\right)^{-1}\frac{\hat{e}(x_i)(1-W_i)Y_i}{1-\hat{e}(x_i)}, \quad (38)$$

where $\hat{e}(X_i)$ is the estimated scores for the probability that $W = 1$, n_1 the number of treated observations and n_0 the number of untreated observations.

7.4 Estimation results

Table 2 presents the main estimation results. The first column provides a raw comparison of the treated and the non-treated workers without adjusting for any observed variables. It shows that training participants on average have \$1737 higher earnings than if they had been non-treated, for the first quarter after WIA. Adjusting for observed variables using a static IPW estimator reduces the estimate to \$1008 (Column 2).

With our dynamic IPW estimator, this estimate increases to \$1233, which is 22.3% larger than the static IPW estimates, and this difference is significant (Columns 3-6).[17] All this confirms the expected patterns. With a static approach, the control group includes too many participants who only remain in WIA for a short period. This is, most likely, a group with more favorable characteristics, and this leads to an effect estimate that is biased downwards. Our dynamic approach corrects for this positive selection of the control group.

[16]We also explore common support restrictions using a variant of the three-step approach in Huber et al. (2013). We set an upper bound on the weight given to the outcome for a certain individual (1%), but due to the rather large sample size, this common support restriction is not very binding.

[17]The standard error for the difference between the two estimates is obtained using bootstrap.

Table 2 also reveals similar patterns for earnings in the second, third, and fourth quarters after exit from WIA.

8 Long-run effects of Swedish training programs

8.1 Background and data

The main aim of the Swedish training program called AMU is to improve the skills of unemployed workers. The training courses in this program are 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. The most common courses offer training towards manufacturing (11.6% of the participants), machine operators (9.8%), office/warehouse work (15.1%), health care (6.1%) and computer skills (15.1%).[18]

While the training program is for unemployed workers, it is important to know the long-run effects on earnings. This fits into the dynamic evaluation problem studied in this paper. For treated workers, the long-run earnings are unobserved, and under the unconfoundedness assumption the treated and the not-yet treated at t are comparable. However, some non-treated receive treatment in subsequent periods, and this creates a dynamic evaluation problem because some workers have a higher treatment probability, and since for a given period-by-period individuals with a high rate of leaving unemployment are more likely to remain non-treated.

Several register-based datasets are used in the analysis. From the Swedish employment offices, we have day-by-day information on the unemployment status and on participation in training and other labor markets programs. These data also include a number of personal characteristics. 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 unemployment history. Additional background characteristics are obtained from an annual population register, including indicators for children in different age brackets, marital status, and labor earnings history. The population register also includes information on long-run labor earnings (all cash compensation paid by employers, consumer price adjusted) up to ten years after the training.

[18]Evaluations of the effect of the program on unemployment durations include Harkman and Johansson (1999) and van den Berg and Richardson (2013).

For estimation reasons, the daily data is aggregated into monthly intervals. To be able to study long-run effects, we sample all unemployment spells that start in the period 1995–1998. We focus on unemployed individuals with no previous unemployment record within 180 days before the unemployment spell, and restrict the analysis to workers in ages 25–55 at the time of entry into unemployment.[19] We consider the effect of the first training program during the unemployment. All propensity scores are estimated using logistic regression models and standard errors are obtained by bootstrapping (99 replications).[20]

Table 3 presents sample statistics. In total, the analysis sample includes 735,581 unemployment spells of which 47,411 (6.1%) concern participation in the training program. We also see that there are more females, high-school educated, university educated, married, and parents among the participants.

As in the WIA application, the analysis rests on an unconfoundedness assumption. We control for socio-economic characteristics, previous income and unemployment, and regional indicators. This selection of variables is based on several previous studies. Heckman et al. (1998, 1999) stress that it is important to control for previous unemployment, lagged earnings, and local labor market conditions. More recently, Lechner and Wunsch (2013) and Biewen et al. (2014) obtain similar results. Here, we adjust for both previous unemployment and earnings, and regional labor market conditions (through regional dummies). As further support of the unconfoundedness assumption, Eriksson (1997) and Carling and Richardson (2001) show that caseworkers have a large influence and large degree of discretionary power over enrollment decisions, which suggests that individual self-selection into the training program is less of a problem.

8.2 Estimation results

The main results are presented in Table 4. Column 3 reports the results from our dynamic estimator. It shows that in earnings, on average, decrease by about 14,500 SEK in the year that the program starts. This is because of so called lock-in effects during the program (Gerfin and Lechner, 2002, van Ours, 2004, and Sianesi, 2004). In

[19]The reason for the latter is that the benefits entitlement rules and active labor market policy programs were different for persons aged below 25 or above 55 during the period studied here. Individuals 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.

[20]When using the three-step common support approach as in Section 7, we obtain similar results.

the long-run, the effects are much more positive with sizable and persistent positive effects on yearly labor earnings. For instance, five years after the program, the effect is 6543 SEK or 4.6%.

Table 4 also reveals striking differences between a static IPW approach (Column 2) and our dynamic IPW approach. For instance, five years after the training, the dynamic approach gives a positive training effect, while the static approach suggests a large negative effect of training. Needles to say, this has very different policy implications. It also means that adjusting for the dynamic treatment assignment is relatively more important for this Swedish training program than for the WIA training program, although the differences is sizeable and significant also in the WIA case. There are two explanations to this. First, Figure 2 shows that a larger share of the participants in the Swedish training program start training after many months of unemployment. Since the static approach do not adjust for differences in pre-treatment durations, the control group includes too many short-term unemployed workers. This leads to substantial downward bias as short-term unemployed workers tend to have more favourable characteristics. For WIA training, a larger share starts training in the first months (see Figure 1), and this leads to a somewhat less pronounced dynamic evaluation problem.

Second, in the Swedish case, the abundance of short-term unemployed workers in the static approach's control group is especially problematic because of the large observed earnings differences between short-term and long-term unemployed workers. Average earnings five years after the unemployment spell are about 185,000 for the previously short-term unemployed (less than four months of unemployment) and 129,000 for the previously long-term unemployed (more than 12 months of unemployment). In the WIA case, individuals who participate in WIA for only a short period of time are more similar to individuals who stay a bit longer, which makes for a less pronounced dynamic problem.

9 Conclusions

This paper presents new identification results and proposes new estimators for treatment evaluations based on unconfoundedness assumptions in a dynamic setting. We apply this to programs from both the US and Sweden. The US application evaluates the WIA program and compares participation in WIA training with participation in other WIA services. Here, the initial state is being registered in the WIA program and the dynamic evaluation problem arises because the date of entry into the training varies. In the Swedish application, we estimate the long-run earnings effects of a flagship training program for unemployed workers in Sweden. Training programs for unemployed workers is a classic dynamic evaluation setting, where training occurs after different elapsed unemployment durations.

In both applications, we find positive earnings effects of training, and taking the dynamic treatment assignment into account turns out to be important. For the WIA program, a standard static evaluation approach underestimates the WIA training effect by roughly 20% when it is compared to our new dynamic approach. For the Swedish program, the difference between a static and our dynamic approach is even more striking as the static approach leads to estimates with an incorrect sign (negative effect instead of positive effects on earnings in the long-run). Indeed, for reasonable discount rates, our results lead to a reassessment of the over-all benefits of the program.

The analyses in this paper do not explicitly distinguish between the direct causal effect of the treatment and an indirect effect that may run through the length of stay in the initial eligibility state. Often one would expect the former to dominate in the longer run. However, we view it as an interesting topic for further research to develop a formal statistical framework for mediation analysis that distinguishes between the two channels.

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Tables and Figures

Figure 1: Distribution of time to WIA training, by months since WIA registration

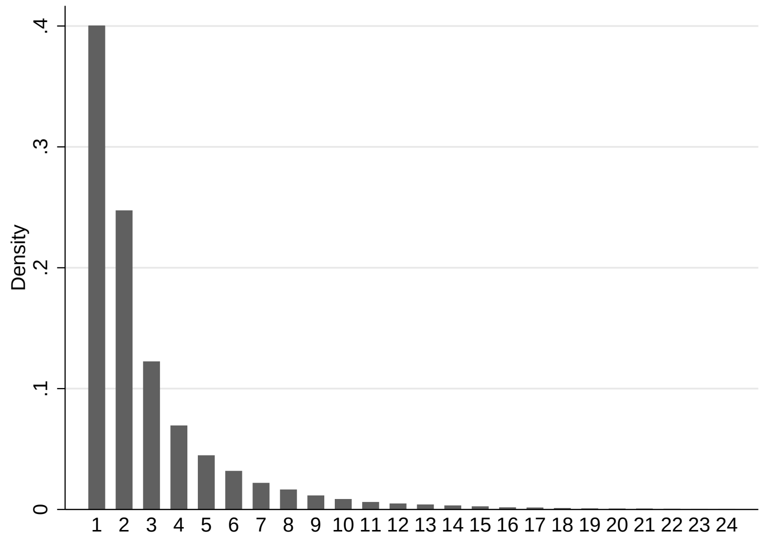


Table 1: Sample statistics for WIA training participants and other WIA participants

	No training	Training
# observations	1551313	231540
Mean survival time	4.9	12.4
Outcomes		
Earnings first quarter after WIA	3626	5355
Earnings second quarter after WIA	3864	5622
Earnings third quarter after WIA	3910	5698
Earnings fourth quarter after WIA	3888	5778
Background characteristics		
Male	54.0	48.0
Race		
White	62.1	53.4
Black	19.5	26.4
Other	0.2	0.2
Age at registration		
–20	5.6	4.9
20–25	14.4	15.4
26–30	14.0	14.6
31–35	12.2	13.0
36–40	10.5	11.5
41–45	10.7	11.5
46–50	11.0	11.2
51–55	10.2	9.8
56–60	7.6	6.2
Education		
Less than High School	3.1	0.9
High School	58.6	53.2
Some College	24.1	32.7
College or more	14.2	13.2
Disabled	4.1	2.5
Veteran	7.6	7.0
Pre-program earnings, quarter before registration		
First	4815.5	3837.0
Second	5029.3	4407.9
Third	5026.8	4682.9
Year of registration		
2011	34.7	36.7
2012	53.6	52.3
2013	11.8	11.0
Quarter of registration		
First	28.4	25.3
Second	23.0	26.0
Third	24.5	26.3
Fourth	24.1	22.3

Note: Background characteristics recorded at registration. Earnings in 2012 US dollars.

Table 2: Effects of WIA training on earnings

	Raw [1]	Static IPW [2]	Dynamic IPW [3]	Difference [3]-[2] [4]	Diff. in percent [5]	p-value (Diff.) [6]
First quarter	1737	1008	1233	225	22.3	0.000
	(13.3)	(18.8)	(25.5)	(18.2)		
Second quarter	1767	1138	1353	215	18.9	0.000
	(13.2)	(18.0)	(24.2)	(18.3)		
Third quarter	1794	1176	1393	217	18.4	0.000
	(15.5)	(21.3)	(24.4)	(17.0)		
Fourth quarter	1897	1219	1397	178	14.6	0.000
	(15.3)	(24.4)	(28.5)	(19.6)		

Note: Quarterly earnings in 2012 US dollars. Dynamic IPW is the dynamic IPW estimator introduced in this paper. Raw makes no covariate adjustments and Static IPW is a standard static IPW estimator with normalized weights.

Table 3: Sample statistics for Swedish labor market training participants and non-participants

	Untreated	Treated
# observations	735581	47411
Mean survival time	8.7	23.8
Outcomes		
Earnings program year	69354	56865
Earnings +1 years	79886	50721
Earnings +2 years	101121	84456
Earnings +3 years	119704	106003
Earnings +4 years	133278	120001
Earnings +5 years	142343	129455
Earnings +6 years	147861	135164
Earnings +7 years	152404	138984
Earnings +8 years	158090	143800
Background characteristics		
Male	47.4	47.8
Age at the start of the spell		
25–34	57.8	50.9
35–44	26.4	30.5
45–54	15.8	18.6
Married	36.5	40.7
Eduation		
Less than High school	0.2	0.2
High school education	51.9	58.0
University education	26.4	21.7
Child in ages 0-3	25.6	30.1
Child in ages 4-6	17.7	19.2
Child in ages 7-15	24.7	27.6
UI eligiable	78.0	80.6
Rest. search area	16.6	19.4
Pre-program earnings and unemployment		
Days unemployed year -1	58.0	63.8
Days unemployed year -2	81.1	94.1
Earnings year -1	72092	73124
Earnings year -2	74197	73128
Earnings year -3	77117	76035
Year of inflow		
1996	24.9	20.7
1997	24.6	19.1
1998	26.0	26.6
Area of residence		
Stockholm MSA	21.1	17.4
Gothenburg MSA	16.4	13.7
Skane MSA	13.3	12.9
North	14.2	15.3
South	11.8	13.4

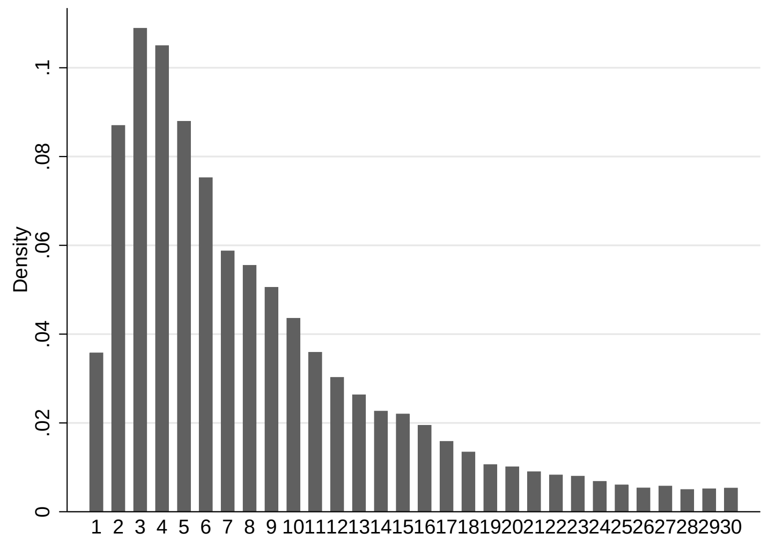
Note: Controls recorded at the start of the unemployment spell. Earnings is in SEK.

Table 4: Effects of Swedish AMU training on yearly labor earnings

	Raw [1]	Static IPW [2]	Dynamic IPW [3]	Difference [3]-[2] [4]	Diff. in percent [5]	p-value (Diff.) [6]
Prog. year	-27823	-27756	-14455	13301	47.9	0.000
	(266.0)	(281.2)	(280.8)	(410.9)		
Prog. + 1 years	-14279	-12282	-5196	7086	57.7	0.000
	(365.4)	(376.4)	(461.5)	(519.5)		
Prog. + 2 years	-7525	-4405	7295	11700	265.6	0.000
	(442.7)	(436.4)	(613.8)	(576.3)		
Prog. + 3 years	-7799	-3999	8899	12899	322.5	0.000
	(483.3)	(453.1)	(720.8)	(673.9)		
Prog. + 4 years	-9019	-5084	7697	12782	251.4	0.000
	(497.0)	(475.2)	(794.1)	(683.1)		
Prog. + 5 years	-9274	-5699	6543	12242	214.8	0.000
	(534.7)	(508.2)	(754.1)	(671.9)		
Prog. + 6 years	-10127	-6616	5223	11839	178.9	0.000
	(578.8)	(557.2)	(746.2)	(745.9)		
Prog. + 7 years	-10976	-7162	5000	12162	169.8	0.000
	(567.8)	(568.7)	(785.1)	(875.5)		
Prog. + 8 years	-11531	-7255	5357	12612	173.8	0.000
	(619.1)	(594.4)	(888.8)	(1002.8)		
Prog. + 9 years	-11805	-6904	6005	12909	187.0	0.000
	(608.5)	(577.4)	(1019.8)	(1165.3)		

Note: Outcome labor earnings in SEK. Dynamic IPW is the dynamic IPW estimator introduced in this paper. Raw makes no covariate adjustments and Static IPW is a standard static IPW estimator with normalized weights. Bootstrapped standard errors (99 replications)

Figure 2: Distribution of time to Swedish AMU training, by months since in unemployment



Appendix A. Proofs and derivations

Estimation of ATET(t_s)

We show that if Assumptions 1–3 hold, the IPW estimator, $\widehat{\text{ATET}}$, is an unbiased estimator of $\text{ATET}(t_s) = E(Y(t_s) - Y(\infty) \mid T_s = t_s, T_u(t_s) \geq t_s)$.

For the first part of ATET(t_s), the estimator without the normalization is:

$$\frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} = t_s, T_{u,i} \geq t_s} Y_i,$$

for which we have

$$\begin{aligned}
& E\left[\frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} = t_s, T_{u,i} \geq t_s} Y_i\right] = \\
& \frac{1}{\rho_{t_s}} E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} \geq t_s, T_{u,i} \geq t_s,} \mathbf{I}(T_{s,i} = t_s) Y_i\right] = \\
& \frac{1}{\rho_{t_s}} E\left[\mathbf{I}(T_s = t_s) Y | T_s \geq t_s, T_u \geq t_s\right] = \\
& \frac{1}{\rho_{t_s}} \Pr(T_s = t_s | T_u \geq t_s, T_s \geq t_s) E\left[Y | T_s = t_s, T_u \geq t_s\right] = \\
& E\left[Y | T_s = t_s, T_u \geq t_s\right] = \\
& E\left[Y(t_s) | T_s = t_s, T_u(t_s) \geq t_s\right],
\end{aligned} \tag{A.1}$$

where the last equality follows by Assumptions 1 and 2 and the observational rule. Note that $\rho_{t_s} = \Pr(T_s = t_s | T_s \geq t_s, T_u \geq t_s)$.

For the second part of ATET(t_s), the estimator without the normalization is:

$$\frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} > T_{u,i} \geq t_s} w^{t_s}(T_{u,i}, X_i) Y_i, \tag{A.2}$$

for which we have

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

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

$$E\left[\frac{1}{\rho_{t_s}N_{t_s}}\sum_{i\in T_{s,i}\geq t_s, T_{u,i}\geq t_s}\sum_{t_u=t_s}^{T_u^{\max}} w^{t_s}(t_u,X_i)\mathbf{I}(T_{s,i}>t_u, T_{u,i}=t_u)Y_i\right]=$$

$$\frac{1}{\rho_{t_s}}E\left[\sum_{t_u=t_s}^{T_u^{\max}} w^{t_s}(t_u,X)\mathbf{I}(T_s>t_u, T_u=t_u)Y|T_s\geq t_s, T_u\geq t_s\right]=$$

$$E_{X|T_s\geq t_s,T_u\geq t_s}\left[\frac{1}{\rho_{t_s}}E\left[\sum_{t_u=t_s}^{T_u^{\max}} w^{t_s}(t_u,X)\mathbf{I}(T_s>t_u, T_u=t_u)Y|T_s\geq t_s, T_u\geq t_s, X\right]\right].$$

For sake of presentation, use the notation

$$h(t,X)=\Pr(T_u=t|T_s>t,T_u\geq t,X).$$

Next, using Assumptions 1–3, and that $w^{t_s}(t_u,X)=\frac{p(t_s,X)}{\prod_{m=t_s}^{t_u}[1-p(m,X)]}$:

$$E\left[w^{t_s}(t_u,X)\mathbf{I}(T_s>t_u,T_u=t_u)Y|T_s\geq t_s,T_u\geq t_s,X\right]= \tag{A.3}$$

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

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

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

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

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

Note that the second equality follows from the definition of $w^{t_s}(t_u,X)$, the third equality by simplifying, the fourth equality by Assumptions 1 and 2, and the fifth equality by applying Assumption 3 for $t_s,...,t_u$.

From (A.2) and (A.3)

$$E\left[\frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} > T_{u,i}, T_{u,i} \geq t_s} w^{t_s}(T_{u,i}, X_i) Y_i\right] = \tag{A.4}$$

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

For sake of presentation, introduce the notation

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

Using this notation we have using that by construction $h(T_u^{\max}, X) = 1$

$$h(T_u^{\max}, X) \prod_{m=t_s}^{T_u^{\max}-1} [1-h(m, X)] y(T_u(\infty) = T_u^M, X) = \prod_{m=t_s}^{T_u^{\max}-1} [1-h(m)] y(T_u(\infty) = T_u^M, X). \tag{A.5}$$

Next, for time periods $T_u^{\max} - 1$ and $T_u^{\max} - 2$

$$\prod_{m=t_s}^{T_u^{\max}-1} [1-h(m, X)] y(T_u(\infty) = T_u^M, X) + h(T_u^M - 1, X)] \prod_{m=t_s}^{T_u^{\max}-2} [1-h(m, X)] y(T_u(\infty) = T_u^M - 1, X) = \tag{A.6}$$

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

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

$$\prod_{m=t_s}^{t} [1-h(m, X)] y(T_u(\infty) > t, X) + h(t)] \prod_{m=t_s}^{t-1} [1-h(m, X)] y(T_u(\infty) = t-1, X) = \tag{A.7}$$

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

Thus, using (A.5) for $T_u^{\max}$, (A.6) for $T_u^{\max}-1$ and (A.7) for $t_s, ..., T_u^{\max}-2$ we have

$$\sum_{t_u=t_s}^{T_u^{\max}} h(t_u, X) \prod_{m=t_s}^{t_u-1} [1-h(m,X)] E[Y(\infty)|T_s=t_s, T_u(\infty)=t_u, X] = \tag{A.8}$$

$$\sum_{t_u=t_s}^{T_u^{\max}} h(t_u, X) \prod_{m=t_s}^{t_u-1} [1-h(m,X)] y(T_u(\infty)=t_u, X) \overset{(A.5)}{=}$$

$$\prod_{m=t_s}^{T_u^{\max}-1} [1-h(m,X)] y(T_u(\infty)=T_u^{\max}, X) + \sum_{t_u=t_s}^{T_u^{\max}-1} h(t_u, X) \prod_{m=t_s}^{t_u-1} [1-h(m,X)] y(T_u(\infty)=t_u, X) \overset{(A.6)}{=}$$

$$\prod_{m=t_s}^{T_u^{\max}-2} [1-h(m,X)] y(T_u(\infty)\geq T_u^{\max}-1, X) + \sum_{t_u=t_s}^{T_u^{\max}-2} h(t_u, X) \prod_{m=t_s}^{t_u-1} [1-h(m,X)] y(T_u(\infty)=t_u, X) \overset{(A.7)}{=}$$

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

Thus, from (A.4) and (A.8)

$$E\left[\frac{1}{N_{t_s}} \sum_{i\in T_{s,i}>T_{u,i}, T_{u,i}\geq t_s} w^{t_s, ATE}(T_{u,i}, X_i) Y_i\right] = \tag{A.9}$$
$$E_{X|T_s\geq t_s, T_u\geq t_s}\left[\frac{p(t_s, X)}{\rho_{t_s}} y(T_u(\infty)\geq t_s, X)\right] =$$
$$\frac{1}{\rho_{t_s}} E_{X|T_s\geq t_s, T_u\geq t_s}\left[p(t_s, X) E[Y(\infty)|T_s=t_s, T_u(\infty)\geq t_s, X]\right] =$$
$$\frac{1}{\rho_{t_s}} E_{X|T_s\geq t_s, T_u\geq t_s}\left[\Pr(T_s=t_s|T_s\geq t_s, T_u\geq t_s, X) E[Y(\infty)|T_s=t_s, T_u(\infty)\geq t_s, X\right] =$$
$$\frac{1}{\rho_{t_s}} \Pr(T_s=t_s|T_s\geq t_s, T_u\geq t_s) E[Y(\infty)|T_s=t_s, T_u(\infty)\geq t_s] =$$
$$E[Y(\infty)|T_s=t_s, T_u(\infty)\geq t_s].$$

This averaging over X is admitted by the common support assumption.

Finally, (A.1) and (A.9) imply that $E\left[\widehat{\text{ATET}}(t_s)\right] = \text{ATET}(t_s)$.

Estimation of ATE(t_s)

We now show that if Assumptions 1–3 hold, $\widehat{\text{ATE}}(t_s)$ is an unbiased estimator of $\text{ATE}(t_s) = E(Y(t_s) - Y(\infty) \mid T_s \geq t_s, T_u(\infty) \geq t_s)$.

For the first part of ATE(t_s), the estimator without the normalization is:

$$\frac{1}{N_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} w^{t_s}_{ATE1}(X_i) Y_i,$$

under Assumptions 1–3 we have

$$\begin{aligned}
& E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} w^{t_s}_{ATE1}(X_i) Y_i\right] = \\
& E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} \geq t_s, T_{u,i} \geq t_s,} w^{t_s}_{ATE1}(X_i) \mathbf{I}(T_{s,i} = t_s) Y_i\right] = \\
& E\left[w^{t_s}_{ATE1}(X) \mathbf{I}(T_s = t_s) Y | T_s \geq t_s, T_u \geq t_s\right] = \\
E_{X|T_s \geq t_s, T_u \geq t_s} & \left[E\left[w^{t_s}_{ATE1}(X) \mathbf{I}(T_s = t_s) Y | T_s \geq t_s, T_u \geq t_s, X\right]\right] = \\
E_{X|T_s \geq t_s, T_u \geq t_s} & \left[\frac{1}{p(t_s, X)} p(t_s, X) E\left[Y | X, T_s = t_s, T_u \geq t_s, X\right]\right] = \\
E_{X|T_s \geq t_s, T_u \geq t_s} & \left[E\left[Y(t_s) | T_s = t_s, T_u(\infty) \geq t_s, X\right]\right] = \\
E_{X|T_s \geq t_s, T_u \geq t_s} & \left[E\left[Y(t_s) | T_s \geq t_s, T_u(\infty) \geq t_s, X\right]\right] = \\
& E\left[Y(t_s) | T_s \geq t_s, T_u(\infty) \geq t_s\right],
\end{aligned} \tag{A.10}$$

where the first three equalities follow by re-writing, the fourth by substituting for $w^{t_s}_{ATE1}(X) = \frac{1}{p(t_s, X)}$, the fifth by Assumptions 1 and 2 and the observational rule, the sixth equality by Assumption 3 for period t_s, and the seventh by averaging over X.

For the second part of ATE(t_s), the estimator without the normalization is:

$$\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} > T_{u,i} \geq t_s} w^{t_s}_{ATE0}(T_{u,i}, X_i) Y_i,$$

using similar reasoning as in (A.2) we have

$$E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} > T_{u,i} \geq t_s} w^{t_s}_{ATE0}(T_{u,i}, X_i) Y_i\right] = \tag{A.11}$$

$$E_{X|T_s \geq t_s, T_u \geq t_s} \left[E \left[\sum_{t_u = t_s}^{T_u^{\max}} w_{ATE0}^{t_s}(t_u, X) \mathbf{I}(T_s > t_u, T_u = t_u) Y | T_s \geq t_s, T_u \geq t_s, X \right] \right].$$

Under Assumptions 1–3, and using the fact that $w_{ATE0}^{t_s}(t_u, X) = \frac{1}{\prod_{m=t_s}^{t_u}[1 - p(m, X)]}$:

$$E\left[w_{ATE0}^{t_s}(t_u, X) \mathbf{I}(T_s > t_u, T_u = t_u) Y | T_s \geq t_s, T_u \geq t_s, X \right] = \tag{A.12}$$

$$w_{ATE0}^{t_s}(t_u, X) \Pr(T_s > t_u, T_u = t_u | T_u \geq t_s, T_s \geq t_s, X) E[Y | T_s > t_u, T_u = t_u, X] =$$

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

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

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

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

Note that the second equality follows from the definition of $w_{ATE0}^{t_s}(t_u, X)$, the third equality by simplifying, the fourth equality by Assumptions 1 and 2, and the fifth equality by applying Assumption 3 for $t_s, ..., t_u$.

Thus, from (A.11) and (A.12)

$$E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} > T_{u,i}, T_{u,i} \geq t_s} w_{ATE0}^{t_s}(T_{u,i}, X_i) Y_i \right] = \tag{A.13}$$

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

Next, using similar reasoning as for (A.8) we have

$$\sum_{t_u = t_s}^{T_u^{\max}} h(t_u, X) \prod_{m=t_s}^{t_u - 1}[1 - h(m, X)] E[Y(\infty) | T_s \geq t_s, T_u(\infty) = t_u, X] = \tag{A.14}$$

$$E[Y(\infty)|T_s \geq t_s, T_u(\infty) \geq t_s, X],$$

so that from (A.13) and (A.14)

$$E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} > T_{u,i}, T_{u,i} \geq t_s} w^{t_s}_{ATE0}(T_{u,i}, X_i) Y_i\right] = \tag{A.15}$$
$$E_{X|T_s \geq t_s, T_u \geq t_s}\left[E[Y(\infty)|T_s \geq t_s, T_u(\infty) \geq t_s, X]\right] =$$
$$E[Y(\infty)|T_s \geq t_s, T_u(\infty) \geq t_s].$$

Finally, (A.10) and (A.15) imply that $E\left[\widehat{\text{ATE}}(t_s)\right] = \text{ATE}(t_s)$.

Estimation of ATET(t_s) with short-run outcomes

Consider estimation of ATET(t_s, τ) and the estimator in (34)

$$\text{ATET}(t_s, \tau) = E(Y_{t_s+\tau}(t_s) - Y_{t_s+\tau}(\infty)|T_s = t_s, T_u(t_s) \geq t_s).$$

For the first part of ATET(t_s, τ), the estimator without the normalization is:

$$\frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} = t_s, T_{u,i} \geq t_s} Y_{t_s+\tau,i}.$$

By similar reasoning as for (A.1) we have

$$E\left[\frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} = t_s T_{u,i} \geq t_s} Y_{t_s+\tau,i}\right] = E\left[Y_{t_s+\tau}(t_s)|T_s = t_s, T_u(t_s) \geq t_s\right]. \tag{A.16}$$

For the second part of ATET(t_s, τ) the estimator without the normalization is:

$$\frac{1}{\rho_{t_s} N_{t_s}}\left[\sum_{i \in T_{s,i} > T_{u,i}, t_s+\tau \geq T_{u,i} \geq t_s} w^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i} + \sum_{i \in T_{s,i} > t_s+\tau, T_{u,i} > t_s+\tau} w^{t_s}_{\tau}(T_{u,i}, X_i) Y_{t_s+\tau,i}\right].$$

Initially, using similar reasoning as for (A.2)

$$\left[\frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} > T_{u,i}, t_s+\tau \geq T_{u,i} \geq t_s} w^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i}\right] = \tag{A.17}$$

$$E_{X|T_s \geq t_s, T_u \geq t_s} \left[\frac{1}{\rho_{t_s}} E\left[\sum_{t_u=t_s}^{t_s+\tau} w^{t_s}(t_u, X)\mathbf{I}(T_s > t_u, T_u = t_u) Y_{t_s+\tau} | T_s \geq t_s, T_u \geq t_s, X\right]\right],$$

and using similar reasoning as for (A.3), we have that if Assumptions 1 and 3 hold, then

$$E\left[w^{t_s}(t_u, X)\mathbf{I}(T_s > t_u, T_u = t_u) Y_{t_s+\tau} | T_s \geq t_s, T_u \geq t_s, X\right] = \tag{A.18}$$

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

Next,

$$E\left[\frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} > t_s+\tau, T_{u,i} > t_s+\tau} w_\tau^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i}\right] = \tag{A.19}$$

$$E\left[\frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} \geq t_s, T_{u,i} \geq t_s} w_\tau^{t_s}(T_{u,i}, X_i)\mathbf{I}(T_{s,i} > t_s+\tau, T_{u,i} > t_s+\tau) Y_{t_s+\tau,i}\right] =$$

$$E\left[\frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} \geq t_s, T_{u,i} \geq t_s} \sum_{t_u > t_s+\tau}^{T_u^{\max}} w_\tau^{t_s}(t_u, X_i)\mathbf{I}(T_{s,i} > t_s+\tau, T_{u,i} = t_u) Y_{t_s+\tau,i}\right] =$$

$$\frac{1}{\rho_{t_s}} E\left[\sum_{t_u > t_s+\tau}^{T_u^{\max}} w_\tau^{t_s}(t_u, X)\mathbf{I}(T_s > t_s+\tau, T_u = t_u) Y_{t_s+\tau} | T_s \geq t_s, T_u \geq t_s\right] =$$

$$E_{X|T_s \geq t_s, T_u \geq t_s} \left[\frac{1}{\rho_{t_s}} E\left[\sum_{t_u > t_s+\tau}^{T_u^{\max}} w_\tau^{t_s}(t_u, X)\mathbf{I}(T_s > t_s+\tau, T_u = t_u) Y_{t_s+\tau} | T_s \geq t_s, T_u \geq t_s, X\right]\right],$$

Then, Assumptions 1 and 3 and the fact that $w_\tau^{t_s}(X) = \frac{p(t_s,X)}{\prod_{m=t_s}^{t_s+\tau}[1-p(m,X)]}$ imply that

$$E\left[w_\tau^{t_s}(X)\mathbf{I}(T_s > t_s+\tau, T_u = t_u)Y_{t_s+\tau}|T_s \geq t_s, T_u \geq t_s, X\right] = \tag{A.20}$$

$$w_\tau^{t_s}(X)\Pr(T_s > t_s+\tau, T_u = t_u|T_u \geq t_s, T_s \geq t_s, X)E[Y_{t_s+\tau}|T_s > t_s+\tau, T_u = t_u, X] =$$

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

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

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

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

From (A.17)-(A.20) we have

$$E\left[\frac{1}{\rho_{t_s}N_{t_s}}\left[\sum_{\substack{i\in T_{s,i}>T_{u,i},\\ t_s+\tau\geq T_{u,i}\geq t_s}} w^{t_s}(T_{u,i}, X_i)Y_{t_s+\tau,i} + \sum_{\substack{i\in T_{s,i}>t_s+\tau,\\ T_{u,i}>t_s+\tau}} w_\tau^{t_s}(T_{u,i}, X_i)Y_{t_s+\tau,i}\right]\right] = \tag{A.21}$$

$$E_{X|T_s\geq t_s, T_u\geq t_s}\left[\frac{p(t_s, X)}{\rho_{t_s}}\sum_{t_u=t_s}^{T_u^{\max}} h(t_u, X)\prod_{m=t_s}^{t_u-1}[1-h(m,X)]E[Y_{t_s+\tau}(\infty)|T_s = t_s, T_u(\infty) = t_u, X]\right].$$

Then, using similar reasoning as for (A.8)

$$\sum_{t_u=t_s}^{T_u^{\max}} h(t_u, X)\prod_{m=t_s}^{t_u-1}[1-h(m,X)]E[Y_{t_s+\tau}(\infty)|T_s = t_s, T_u(\infty) = t_u, X] = \tag{A.22}$$

$$E[Y_{t_s+\tau}(\infty)|T_s = t_s, T_u(\infty) \geq t, X],$$

and, thus from (A.21) and (A.22)

$$E\left[\frac{1}{\rho_{t_s}N_{t_s}}\left[\sum_{\substack{i\in T_{s,i}>T_{u,i},\\ t_s+\tau\geq T_{u,i}\geq t_s}} w^{t_s}(T_{u,i},X_i)Y_{t_s+\tau,i}+\sum_{\substack{i\in T_{s,i}>t_s+\tau,\\ T_{u,i}>t_s+\tau}} w^{t_s}_{\tau}(T_{u,i},X_i)Y_{t_s+\tau,i}\right]\right]= \quad \text{(A.23)}$$

$$\frac{1}{\rho_{t_s}}E_{X|T_s\geq t_s,T_u\geq t_s}\left[p(t_s,X)E[Y_{t_s+\tau}(\infty)|T_s=t_s,T_u(\infty)\geq t_s,X]\right]=$$

$$\frac{1}{\rho_{t_s}}E_{X|T_s\geq t_s,T_u\geq t_s}\left[\Pr(T_s=t_s|T_s\geq t_s,T_u\geq t_s,X)E[Y_{t_s+\tau}(\infty)|T_s=t_s,T_u(\infty)\geq t_s,X\right]=$$

$$\frac{1}{\rho_{t_s}}\Pr(T_s=t_s|T_s\geq t_s,T_u\geq t_s)E[Y_{t_s+\tau}(\infty)|T_s=t_s,T_u(\infty)\geq t_s]=$$

$$E[Y_{t_s+\tau}(\infty)|T_s=t_s,T_u(\infty)\geq t_s].$$

Finally, (A.16) and (A.23) imply that $E\left[\widehat{\text{ATET}}(t_s)\right]=\text{ATET}(t_s)$.

Identification with time-varying covariates

Consider identification of $\text{ATET}(t_s)=E(Y(t_s)-Y(\infty)\mid T_s=t_s,T_u(t_s)\geq t_s)$. For the first component, as before

$$E(Y(t_s)|T_s=t_s,T_u(t_s)\geq t_s)=E(Y|T_s=t_s,T_u\geq t_s). \quad \text{(A.24)}$$

For the second component, by Assumptions 1 and 2, and averaging over $X^-_{t_s}$

$$E(Y(\infty)|T_s=t_s,T_u(t_s)\geq t_s)=E(Y(\infty)|T_s=t_s,T_u(\infty)\geq t_s)= \quad \text{(A.25)}$$

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

Then, by Assumption 6 for period t_s,

$$E(Y(\infty)|T_s=t_s,T_u(\infty)\geq t_s,X^-_{t_s})=E(Y(\infty)|T_s>t_s,T_u(\infty)\geq t_s,X^-_{t_s}).$$

Next, using the notation $h(X^-_t)=\Pr(T_u=t|T_s>t,T_u\geq t,X^-_t)$ we have

$$E(Y(\infty)|T_s>t_s,T_u(\infty)\geq t_s,X^-_{t_s})=h(X^-_{t_s})E(Y(\infty)|T_s>t_s,T_u(\infty)=t_s,X^-_{t_s})+ \quad \text{(A.26)}$$

$$[1-h(X^-_{t_s})]E(Y(\infty)|T_s > t_s, T_u(\infty) > t_s, X^-_{t_s}) =$$

$$h(X^-_{t_s})E(Y|T_s > t_s, T_u = t_s, X^-_{t_s}) + [1-h(X^-_{t_s})]E(Y(\infty)|T_s > t_s, T_u(\infty) > t_s, X^-_{t_s}),$$

where the second equality follows from Assumption 2 and the observational rule. Note that we also use that under Assumption 1 $\Pr(T_u = t|T_s > t, T_u(\infty) \geq t, X^-_t) = \Pr(T_u = t|T_s > t, T_u \geq t, X^-_t) = h(X^-_t)$, and the treatment probability $h(X^-_{t_s})$ is observed. Next,

$$\begin{aligned} E(Y(\infty)|T_s &> t_s, T_u(\infty) > t_s, X^-_{t_s}) = \\ E_{X^-_{t_s+1}|T_s>t_s,T_u>t_s,X^-_{t_s}}[E(Y(\infty)|T_s &> t_s, T_u(\infty) > t_s, X^-_{t_s+1})] = \\ E_{X^-_{t_s+1}|T_s>t_s,T_u>t_s,X^-_{t_s}}[E(Y(\infty)|T_s &> t_s+1, T_u(\infty) > t_s, X^-_{t_s+1})] = \\ E_{X^-_{t_s+1}|T_s>t_s,T_u>t_s,X^-_{t_s}}[E(Y(\infty)|T_s &> t_s+1, T_u(\infty) \geq t_s+1, X^-_{t_s+1})], \end{aligned} \tag{A.27}$$

where the first equality follows from the law of iterated expectations, the second equality from Assumption 6 for period t_s+1, and the third equality by rewriting. Here, the covariates $X^-_{t_s+1}$ may include $X^-_{t_s}$. From (A.26), by replacing t_s with t_s+1:

$$E(Y(\infty)|T_s > t_s+1, T_u(\infty) \geq t_s+1, X^-_{t_s+1}) = \tag{A.28}$$

$$\begin{aligned} h(X^-_{t_{s+1}})E(Y(\infty)|T_s &> t_s+1, T_u(\infty) = t_s+1, X^-_{t_s+1}) + \\ [1-h(X^-_{t_{s+1}})]E(Y(\infty)|T_s &> t_s+1, T_u(\infty) > t_s+1, X^-_{t_s+1}). \end{aligned}$$

Next, from (A.26) and (A.28)

$$E(Y(\infty)|T_s > t_s, T_u(\infty) \geq t_s, X^-_{t_s}) = h(X^-_{t_s})E(Y|T_s > t_s, T_u = t_s, X^-_{t_s}) +$$

$$[1-h(X^-_{t_s})]E_{X^-_{t_s+1}|T_s>t_s,T_u>t_s,X^-_{t_s}}[h(X^-_{t_s+1})E(Y|T_s > t_s+1, T_u = t_s+1, X^-_{t_s+1}) +$$

$$[1-h(X^-_{t_s+1})]E(Y(\infty)|T_s > t_s+1, T_u(\infty) > t_s+1, X^-_{t_s+1})].$$

Then, using (A.27) for t_s+1 gives

$$E(Y(\infty)|T_s > t_s, T_u \geq t_s, X^-_{t_s}) = h(X^-_{t_s})E(Y|T_s > t_s, T_u = t_s, X^-_{t_s}) +$$

$$[1-h(X^-_{t_s})]E_{X^-_{t_s+1}|T_s>t_s,T_u>t_s,X^-_{t_s}}[h(X^-_{t_s+1})E(Y|T_s > t_s+1, T_u = t_s+1, X^-_{t_s+1}) +$$

$$[1-h(X^-_{t_{s+1}})]E_{X^-_{t_s+2}|T_s>t_s+1,T_u>t_s+1,X^-_{t_s+1}}[E(Y(\infty)|T_s > t_s+2, T_u(\infty) \geq t_s+2, X^-_{t_s+2})]],$$

and (A.26) for $t_s + 2$ gives

$$E(Y(\infty)|T_s > t_s, T_u \geq t_s, X_{t_s}^-) = h(X_{t_s}^-)E(Y|T_s > t_s, T_u = t_s, X_{t_s}^-)+$$

$$[1 - h(X_{t_s}^-)]E_{X_{t_s+1}^-|T_s>t_s,T_u>t_s,X_{t_s}^-}[h(X_{t_s+1}^-)E(Y|T_s > t_s + 1, T_u = t_s + 1, X_{t_s+1}^-)+$$

$$[1-h(X_{t_{s+1}}^-)]E_{X_{t_s+2}^-|T_s>t_s+1,T_u>t_s+1,X_{t_s+1}^-}[h(X_{t_{s+2}}^-)E(Y|T_s > t_s+2, T_u = t_s+2, X_{t_s+2}^-)+$$

$$[1 - h(X_{t_{s+2}}^-)]E(Y(\infty)|T_s > t_s + 2, T_u(\infty) > t_s + 2, X_{t_s+2}^-)]],$$

and (A.27) for $t_s + 2$ gives

$$E(Y(\infty)|T_s > t_s, T_u \geq t_s, X_{t_s}^-) = h(X_{t_s}^-)E(Y|T_s > t_s, T_u = t_s, X_{t_s}^-)+$$

$$[1 - h(X_{t_s}^-)]E_{X_{t_s+1}^-|T_s>t_s,T_u>t_s,X_{t_s}^-}[h(X_{t_s+1}^-)E(Y|T_s > t_s + 1, T_u = t_s + 1, X_{t_s+1}^-)+$$

$$[1-h(X_{t_{s+1}}^-)]E_{X_{t_s+2}^-|T_s>t_s+1,T_u>t_s+1,X_{t_s+1}^-}[h(X_{t_{s+2}}^-)E(Y|T_s > t_s+2, T_u = t_s+2, X_{t_s+2}^-)+$$

$$[1-h(X_{t_{s+2}}^-)]E_{X_{t_s+3}^-|T_s>t_s+2,T_u>t_s+2,X_{t_s+2}^-}[E(Y(\infty)|T_s > t_s+3, T_u(\infty) \geq t_s+3, X_{t_s+1}^-)]]].$$

and iteratively using (A.26) and (A.27) for $t_s + 3, \ldots, T_u^{\max}$ we have

$$E(Y(\infty)|T_s > t_s, T_u \geq t_s, X_{t_s}^-) = h(X_{t_s}^-)E(Y|T_s > t_s, T_u = t_s, X_{t_s}^-)+ \tag{A.29}$$

$$[1 - h(X_{t_s}^-)]E_{X_{t_s+1}^-|T_s>t_s,T_u>t_s,X_{t_s}^-}[h(X_{t_s+1}^-)E(Y|T_s > t_s + 1, T_u = t_s + 1, X_{t_s+1}^-)+$$

$$[1-h(X_{t_{s+1}}^-)]E_{X_{t_s+2}^-|T_s>t_s+1,T_u>t_s+1,X_{t_s+1}^-}[h(X_{t_{s+2}}^-)E(Y|T_s > t_s+2, T_u = t_s+2, X_{t_s+2}^-)+\ldots+$$

$$[1-h(X_{T_u^{\max}-1}^-)]E_{X_{T_u^{\max}}^-|T_s>T_u^{\max}-1,T_u>T_u^{\max}-1,X_{T_u^{\max}-1}^-}[p(X_{T_u^{\max}}^-)E(Y|T_s > T_u^{\max}, T_u = T_u^{\max}, X_{T_u^{\max}}^-)]\ldots]].$$

Finally, combining (A.25) and (A.29) second component together with for (A.24) give the result in Theorem 4.

Estimation with time-varying covariates

We show that the if Assumptions 1, 2 and 6 hold, the estimator in (35), is an unbiased estimator of

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

For the first part of ATET(t_s), we have from (A.1)

$$E\left[\frac{1}{\rho_{t_s}N_{t_s}}\sum_{i\in T_{s,i}=t_s,T_{u,i}\geq t_s}Y_i\right]=E\left[Y(t_s)|T_s=t_s,T_u(t_s)\geq t_s\right]. \tag{A.30}$$

For the second part of ATET(t_s), the estimator without the normalization is:

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

using similar reasoning as for A.2 we have

$$E\left[\frac{1}{\rho_{t_s}N_{t_s}}\sum_{i\in T_{s,i}>T_{u,i}\geq t_s}w^{t_s}(T_{u,i},X_i^-)Y_i\right]= \tag{A.31}$$

$$E_{X_{t_s}^-|T_s\geq t_s,T_u\geq t_s}\left[\frac{1}{\rho_{t_s}}E\left[\sum_{t_u=t_s}^{T_u^{\max}}w^{t_s}(t_u,X^-)\mathbf{I}(T_s>t_u,T_u=t_u)Y|T_s\geq t_s,T_u\geq t_s,X_{t_s}^-\right]\right].$$

We use the notation

$$h(t,X_t^-)=\Pr(T_u=t|T_s>t,T_u\geq t,X_t^-).$$

If Assumptions 1 and 2 and 6 hold, and since $w^{t_s}(t_u,X^-)=\frac{p(t_s,X_{t_s}^-)}{\prod_{m=t_s}^{t_u}[1-p(m,X_m^-)]}$:

$$E\left[w^{t_s}(t_s+1,X^-)\mathbf{I}(T_s>t_s+1,T_u=t_s+1)Y|T_s\geq t_s,T_u\geq t_s,X_{t_s}^-\right]= \tag{A.32}$$

$$\begin{aligned}&w^{t_s}(t_s+1,X^-)\Pr(T_s>t_s,T_u>t_s|T_u\geq t_s,T_s\geq t_s,X_{t_s}^-)\times\\&\Pr(T_s>t_s+1,T_u=t_s+1|T_s>t_s,T_u>t_s,X_{t_s}^-)E[Y|T_s>t_s+1,T_u=t_s+1,X_{t_s}^-]=\end{aligned}$$

$$\begin{aligned}&\frac{p(t_s,X_{t_s}^-)}{\prod_{m=t_s}^{t_s+1}[1-p(m,X_m^-)]}[1-h(t_s,X_{t_s}^-)][1-p(t_s,X_{t_s}^-)]\times\\\Pr(T_s\ &>\ t_s+1,T_u=t_s+1|T_s>t_s,T_u>t_s,X_{t_s}^-)E[Y|T_s>t_s+1,T_u=t_s+1,X_{t_s}^-]=\end{aligned}$$

$$\frac{p(t_s,X^-_{t_s})[1-h(t_s,X^-_{t_s})]}{[1-p(t_s,X^-_{t_s+1})]}\Pr(T_s > t_s+1,T_u=t_s+1|T_s>t_s,T_u>t_s,X^-_{t_s})\times$$
$$E[Y|T_s > t_s+1,T_u=t_s+1,X^-_{t_s}].$$

Next,

$$\frac{\Pr(T_s>t_s+1,T_u=t_s+1|T_s>t_s,T_u>t_s,X^-_{t_s})}{1-p(t_s,X^-_{t_s+1})} = \tag{A.33}$$
$$E_{X^-_{t_s+1}|T_s>t_s,T_u>t_s,X^-_{t_s}}\left[\frac{\Pr(T_s>t_s+1,T_u=t_s+1|T_s>t_s,T_u>t_s,X^-_{t_s+1})}{1-p(t_s,X^-_{t_s+1})}\right] =$$
$$E_{X^-_{t_s+1}|T_s>t_s,T_u>t_s,X^-_{t_s}}\left[\frac{1-p(t_s,X^-_{t_s+1})\Pr(T_u=t_s+1|T_s>t_s+1,T_u>t_s,X^-_{t_s+1})}{1-p(t_s,X^-_{t_s+1})}\right] =$$
$$E_{X^-_{t_s+1}|T_s>t_s,T_u>t_s,X^-_{t_s}}\left[\Pr(T_u=t_s+1|T_s>t_s+1,T_u>t_s,X^-_{t_s+1})\right] =$$
$$E_{X^-_{t_s+1}|T_s>t_s,T_u>t_s,X^-_{t_s}}\left[\Pr(T_u=t_s+1|T_s>t_s,T_u>t_s,X^-_{t_s+1})\right] =$$
$$\Pr(T_u=t_s+1|T_s>t_s,T_u>t_s,X^-_{t_s}) = h(t_s+1,X^-_{t_s}).$$

Note that the fourth equality follows from Assumption 6. Then, by (A.32) and (A.33), and using Assumptions 2 and 6

$$E\left[w^{t_s}(t_s+1,X^-)\mathbf{I}(T_s>t_s+1,T_u=t_s+1)Y|T_s\geq t_s,T_u\geq t_s,X^-_{t_s}\right] = \tag{A.34}$$

$$p(t_s,X^-_{t_s})h(t_s+1,X^-_{t_s})[1-h(t_s,X^-_{t_s})]E[Y|T_s>t_s+1,T_u=t_s+1,X^-_{t_s}] =$$

$$p(t_s,X^-_{t_s})h(t_s+1,X^-_{t_s})[1-h(t_s,X^-_{t_s})]E[Y(\infty)|T_s>t_s+1,T_u(\infty)=t_s+1,X^-_{t_s}] =$$

$$p(t_s,X^-_{t_s})h(t_s+1,X^-_{t_s})[1-h(t_s,X^-_{t_s})]E[Y(\infty)|T_s=t_s,T_u(\infty)=t_s+1,X^-_{t_s}],$$

where the second equality follows from Assumption 2 and the third by Assumption 6.

By similar reasoning as for (A.32)-(A.34) we have

$$E\left[w^{t_s}(t_u,X^-)\mathbf{I}(T_s>t_u,T_u=t_u)Y|T_s\geq t_s,T_u\geq t_s,X^-_{t_s}\right] = \tag{A.35}$$

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

Thus, from (A.31)-(A.35):

$$E\left[\frac{1}{\rho_{t_s}N_{t_s}}\sum_{i\in T_{s,i}>T_{u,i}\geq t_s} w^{t_s}(T_{u,i},X^-_i)Y_i\right] = \tag{A.36}$$

$$E_{X_{t_s}^-|T_s\geq t_s,T_u\geq t_s}\left[\frac{p(t_s,X_{t_s}^-)}{\rho_{t_s}}\sum_{t_u=t_s}^{T_u^{\max}} h(t_u,X_{t_s}^-)\prod_{m=t_s}^{t_u-1}[1-h(m,X_{t_s}^-)]E[Y(\infty)|T_s=t_s,T_u(\infty)=t_u,X_{t_s}^-]\right].$$

Next, by similar reasoning as for (A.5)-(A.8) we have

$$\sum_{t_u=t_s}^{T_u^{\max}} h(t_u,X_{t_s}^-)\prod_{m=t_s}^{t_u-1}[1-h(m,X_{t_s}^-)]E[Y(\infty)|T_s=t_s,T_u(\infty)=t_u,X_{t_s}^-] = \tag{A.37}$$

$$E[Y(\infty)|T_s=t_s,T_u(\infty)\geq t_s,X_{t_s}^-],$$

so that from (A.36) and (A.37)

$$E\left[\frac{1}{\rho_{t_s}N_{t_s}}\sum_{i\in T_{s,i}>T_{u,i}\geq t_s} w^{t_s}(T_{u,i},X_i^-)Y_i\right] = \tag{A.38}$$

$$E_{X_{t_s}^-|T_s\geq t_s,T_u\geq t_s}\left[\frac{p(t_s,X_{t_s}^-)}{\rho_{t_s}}E[Y(\infty)|T_s=t_s,T_u(\infty)\geq t_s,X_{t_s}^-]\right] =$$

$$\frac{1}{\rho_{t_s}}E_{X_{t_s}^-|T_s\geq t_s,T_u\geq t_s}\left[\Pr(T_s=t_s|T_s\geq t_s,T_u\geq t_s,X_{t_s}^-)E[Y(\infty)|T_s=t_s,T_u(\infty)\geq t_s,X_{t_s}^-\right] =$$

$$\frac{1}{\rho_{t_s}}\Pr(T_s=t_s|T_s\geq t_s,T_u\geq t_s)E[Y(\infty)|T_s=t_s,T_u(\infty)\geq t_s] =$$

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

Finally, (A.30) and (A.38) imply that $E\left[\widehat{\text{ATET}}(t_s)\right]=\text{ATET}(t_s)$.

Identification with right-censored durations

Consider identification of $\text{ATET}(t_s)=E(Y(t_s)-Y(\infty)\mid T_s=t_s,T_c>t_s,T_u(t_s)\geq t_s)$ under Assumptions 1-3 and 7. First, consider $E(Y(\infty)|T_s=t_s,T_c>t_s,T_u(t_s)\geq t_s)$. Initially, by Assumptions 1 and 2 and the law of iterated expectations:

$$E(Y(\infty)\mid T_s=t_s,T_c>t_s,T_u(t_s)\geq t_s)=E(Y(\infty)\mid T_s=t_s,T_c>t_s,T_u(\infty)\geq t_s)= \tag{A.39}$$

$$E_{X|T_s=t_s,T_c>t_s,T_u\geq t_s}\left[E(Y(\infty)\mid T_s=t_s,T_c>t_s,T_u(\infty)\geq t_s,X)\right],$$

where the averaging over X is possible given common support. Next, if Assumption 3 holds for period t_s we have

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

Then, by the law of iterated expectations

$$E(Y(\infty) \mid T_s > t_s, T_c > t_s, T_u(\infty) \geq t_s, X) = \tag{A.40}$$

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

$$\Pr(T_u > t_s | T_s > t_s, T_c > t_s, T_u(\infty) \geq t_s, X) E(Y(\infty) | T_s > t_s, T_c > t_s, T_u(\infty) > t_s, X),$$

decomposing the counterfactual outcome under never treatment into average outcomes for individuals with $T_u = t_s$ and $T_u > t_s$. For the group with $T_u = t_s$ in (A.40), we have by Assumption 2

$$E(Y(\infty) | T_s > t_s, T_c > t_s, T_u = t_s, X) = E(Y | T_s > t_s, T_c > t_s, T_u = t_s, X), \tag{A.41}$$

and the probabilities $\Pr(T_u = t_s | T_s > t_s, T_c > t_s, T_u(\infty) \geq t_s, X)$ and $\Pr(T_u > t_s | T_s > t_s, T_c > t_s, T_u(\infty) \geq t_s, X)$ are also observed.

For the group, with $T_u > t_s$, in (A.40), we have

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

where the first equality follows from Assumption 7 for period $t_s + 1$, the second from Assumption 3 for period $t_s + 1$ and the third equality by rewriting. Next, for sake of presentation, let us introduce some auxiliary notation:

$$h_c(t, X) = \Pr(T_u = t \mid T_s > t, T_c > t, T_u \geq t, X).$$

Using this notation and using (A.40) by replacing t_s with t_s+1 we have

$$E(Y(\infty) \mid T_s > t_s+1, T_c > t_s+1, T_u \geq t_s+1, X) = \tag{A.42}$$

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

$$[1-h_c(t_s+1,X)]E(Y(\infty)|T_s > t_s+1, T_c > t_s+1, T_u > t_s+1, X),$$

so that (A.40)-(A.42) give

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

$$[1-h_c(t_s,X)]h_c(t_s+1,X)E(Y|T_s > t_s+1, T_c > t_s+1, T_u = t_s+1, X)+$$

$$[1-h_c(t_s,X)][1-h_c(t_s+1,X)]E(Y(\infty)|T_s > t_s+1, T_c > t_s+1, T_u > t_s+1, X).$$

Then, iteratively using (A.40) and (A.41) for $t_s+2,\ldots,T_u^{\max}$ we have

$$E(Y(\infty)|T_s > t_s, T_c > t_s, T_u \geq t_s, X) = \tag{A.43}$$

$$\sum_{k=t_s}^{T_{us}^{\max}} h_c(k,X) \prod_{m=t_s}^{k-1} [1-h_c(m,X)]E(Y|T_s > k, T_c > k, T_u = k, X).$$

Then, from (A.39)-(A.43)

$$E(Y(\infty) \mid T_s = t_s, T_c > t_s, T_u(t_s) \geq t_s, X) = \tag{A.44}$$

$$E_{X|T_s=t_s,T_c>t_s,T_u\geq t_s}\left[\sum_{k=t_s}^{T_{us}^{\max}} h_c(k,X) \prod_{m=t_s}^{k-1} [1-h_c(m,X)]E(Y|T_s > k, T_c > k, T_u = k, X)\right].$$

Second, for $E(Y(t_s)|T_s = t_s, T_c > t_s, T_u(t_s) \geq t_s)$, Assumptions 1 and 2 and the law of iterated expectations give

$$E(Y(t_s) \mid T_s = t_s, T_c > t_s, T_u(t_s) \geq t_s) = E(Y(t_s) \mid T_s = t_s, T_c > t_s, T_u \geq t_s) = \tag{A.45}$$

$$E_{X|T_s=t_s,T_c>t_s,T_u\geq t_s}\left[E(Y(t_s) \mid T_s = t_s, T_c > t_s, T_u \geq t_s, X)\right].$$

Then, by the law of iterated expectations

$$E(Y(t_s) \mid T_s = t_s, T_c > t_s, T_u \geq t_s, X) = \tag{A.46}$$

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

as above decomposing the outcome of interest into average outcomes for individuals with $T_u = t_s$ and $T_u > t_s$.

Next,

$$E(Y(t_s)|T_s = t_s, T_c > t_s, T_u = t_s, X) = E(Y|T_s = t_s, T_c > t_s, T_u = t_s, X). \quad \text{(A.47)}$$

For sake of presentation, let us introduce some additional auxiliary notation:

$$h_{c1}(t, X, t_s) = \Pr(T_u = t \mid T_s = t_s, T_c > t, T_u \geq t, X).$$

Then, using this notation iteratively using (A.46) and (A.47) for $t_s + 2, \ldots, T_u^{\max}$ we have

$$E(Y(t_s)|T_s = t_s, T_c > t_s, T_u \geq t_s, X) = \quad \text{(A.48)}$$
$$\sum_{k=t_s}^{T_{us}^{\max}} h_{c1}(t, X, t_s) \prod_{m=t_s}^{k-1} [1 - h_{c1}(t, X, t_s)]E(Y|T_s = t_s, T_c > k, T_u = k, X),$$

so that by (A.45)-(A.48)

$$E(Y(t_s) \mid T_s = t_s, T_c > t_s, T_u(t_s) \geq t_s) = \quad \text{(A.49)}$$
$$E_{X|T_s=t_s,T_c>t_s,T_u\geq t_s} \left[\sum_{k=t_s}^{T_{us}^{\max}} h_{c1}(t, X, t_s) \prod_{m=t_s}^{k-1} [1 - h_{c1}(t, X, t_s)]E(Y|T_s = t_s, T_c > k, T_u = k, X) \right].$$

Finally, (A.44) and (A.49) gives the result in Theorem 5.

Estimation with right-censored durations

We now show that if Assumptions 1-3 and 7 hold, the estimator in (37) is an unbiased estimator of ATET(t_s). First, for the first component of ATET(t_s), the estimator without the normalization is:

$$\frac{1}{\rho^c_{t_s} N^c_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{c,i}>T_{u,i}, T_{u,i} \geq t_s} w^{t_s}_{c_1}(T_{u,i}, X_i) Y_i.$$

Using similar reasoning as above we have

$$E\left[\frac{1}{\rho^c_{t_s} N^c_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{c,i}>T_{u,i}, T_{u,i} \geq t_s} w^{t_s}_{c_1}(T_{u,i}, X_i) Y_i\right] = \tag{A.50}$$

$$E\left[\frac{1}{\rho^c_{t_s} N^c_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{c,i}>t_s, T_{u,i} \geq t_s} w^{t_s}_{c_1}(T_{u,i}, X_i) \mathbf{I}(T_{c,i} > T_{u,i}) Y_i\right] =$$

$$E\left[w^{t_s}_{c_1}(T_u, X) \mathbf{I}(T_c > T_u) Y | T_s = t_s, T_c > t_s, T_u \geq t_s\right] =$$

$$E\left[\sum_{t_u=t_s}^{T_u^{\max}} w^{t_s}_{c_1}(t_u, X) \mathbf{I}(T_c > t_u, T_u = t_u) Y | T_s = t_s, T_c > t_s, T_u \geq t_s\right] =$$

$$E_{X|T_s=t_s, T_c>t_s, T_u \geq t_s}\left[E\left[\sum_{t_u=t_s}^{T_u^{\max}} w^{t_s}_{c_1}(t_u, X) \mathbf{I}(T_c > t_u, T_u = t_u) Y | T_s = t_s, T_c > t_s, T_u \geq t_s, X\right]\right].$$

Introduce the notation

$$h_c(t, X) = \Pr(T_u = t | T_u \geq t, T_c > t, T_s > t, X).$$

Then, if Assumptions 1–3 and 7 hold, and noting that $w^{t_s}_{c_1}(t_u, X) = \frac{1}{\prod_{m=t_{s+1}}^{t_u}[1-e_{c1}(m,t_s,X)]}$:

$$E\left[w^{t_s}_{c_1}(t_u, X) \mathbf{I}(T_c > t_u, T_u = t_u) Y | T_s = t_s, T_c > t_s, T_u \geq t_s, X\right] = \tag{A.51}$$

$$\begin{aligned} w^{t_s}_{c_1}(t_u, X) \Pr(T_c &> t_u, T_u = t_u | T_u = t_s, T_c > t_s, T_s \geq t_s, X) \times \\ E[Y | T_s &= t_s, T_c > t_u, T_u = t_u, X] = \end{aligned}$$

$$\begin{aligned} \frac{1}{\prod_{m=t_s+1}^{t_u}[1 - e_{c1}(m, t_s, X)]} \qquad h_c(t_u, X) \prod_{m=t_s}^{t_u-1} [1 - h_c(m, X)] \prod_{m=t_s+1}^{t_u} [1 - e_{c1}(m, t_s, X)] \times \\ E[Y | T_s = t_u, T_c > t_u, T_u = t_u, X] = \end{aligned}$$

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

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

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

where the second equality follows from the definition of $w_{c0}^{t_s}(t_u, X)$, the third equality by simplifying, the fourth equality by Assumptions 2, and the fifth equality by applying Assumption 7 for $t_s, ..., t_u$.

From (A.50) and (A.51)

$$E\left[\frac{1}{\rho_{t_s}^c N_{t_s}^c} \sum_{i \in T_{s,i}=t_s, T_{c,i}>T_{u,i}, T_{u,i}\geq t_s} w_{c1}^{t_s}(T_{u,i}, X_i) Y_i\right] = \tag{A.52}$$

$$E_{X|T_s=t_s, T_c>t_s, T_u\geq t_s}\left[\sum_{t_u=t_s}^{T_u^{\max}} h_c(t_u, X) \prod_{m=t_s}^{t_u-1} [1 - h_c(m, X)] E[Y(\infty)|T_s = t_s, T_c > t_s, T_u(\infty) = t_u, X]\right].$$

Next, by similar reasoning as for (A.8) we have

$$\sum_{t_u=t_s}^{T_u^M} h_c(t_u, X) \prod_{m=t_s}^{t_u-1} [1 - h_c(m, X)] E[Y(\infty)|T_s = t_s, T_c > t_s, T_u(\infty) = t_u, X] = \tag{A.53}$$

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

so that from (A.52) and (A.53)

$$E\left[\frac{1}{\rho_{t_s}^c N_{t_s}^c} \sum_{i \in T_{s,i}=t_s, T_{c,i}>T_{u,i}, T_{u,i}\geq t_s} w_{c1}^{t_s}(T_{u,i}, X_i) Y_i\right] = \tag{A.54}$$
$$E_{X|T_s=t_s, T_c>t_s, T_u\geq t_s}\left[E[Y(\infty)|T_s = t_s, T_c > t_s, T_u(\infty) \geq t_s, X]\right] =$$
$$E[Y(\infty)|T_s = t_s, T_c > t_s, T_u(\infty) \geq t_s].$$

Second, for the second component of ATET(t_s) the estimator without the normalization is:

$$\frac{1}{N_{t_s}^c} \sum_{i \in T_{s,i}>T_{u,i}, T_{c,i}>T_{u,i}, T_{u,i}\geq t_s} w_{c0}^{t_s}(T_{u,i}, X_i) Y_i.$$

Using similar reasoning as for (A.2) we have

$$E\left[\frac{1}{N_{t_s}}\sum_{i\in T_{s,i}>T_{u,i},T_{c,i}>T_{u,i},T_{u,i}\geq t_s} w_{c0}^{t_s}(T_{u,i},X_i)Y_i\right] = \qquad \text{(A.55)}$$

$$E_{X|T_s\geq t_s,T_c>t_s,T_u\geq t_s}\left[E\left[\sum_{t_u=t_s}^{T_u^{\max}} w_{c0}^{t_s}(t_u,X)\mathbf{I}(T_s>t_u,T_c>t_u,T_u=t_u)Y|T_s\geq t_s,T_c>t_s,T_u\geq t_s,X\right]\right].$$

Next, if Assumptions 1–3 and 7 hold, and since

$$w_{c0}^{t_s}(t_u,X)=\frac{p_c(t_s,X)}{\rho_{t_s}^c[1-p_c(t_s,X)]\prod_{m=t_s+1}^{t_u}[1-p_c(m,X)][1-e_{c0}(m,X)]}$$

we have

$$E\left[w_{c0}^{t_s}(t_u,X)\mathbf{I}(T_s>t_u,T_c>t_u,T_u=t_u)Y|T_s\geq t_s,T_c>t_s,T_u\geq t_s,X\right] = \qquad \text{(A.56)}$$

$$\begin{aligned} w_{c0}^{t_s}(t_u,X)\,\Pr(T_s &> t_u,T_c>t_u,T_u=t_u|T_u\geq t_s,T_c>t_s,T_s\geq t_s,X)\times \\ E[Y|T_s &> t_u,T_c>t_u,T_u=t_u,X] = \end{aligned}$$

$$\frac{p_c(t_s,X)}{\rho_{t_s}^c[1-p_c(t_s,X)]\prod_{m=t_s+1}^{t_u}[1-p_c(m,X)][1-e_{c0}(m,X)]}h_c(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h_c(m,X)]\times$$

$$[1-p_c(t_s,X)]\prod_{m=t_s+1}^{t_u}[1-p(m,X)][1-e_{c0}(m,X)]E[Y|T_s>t_u,T_c>t_u,T_u=t_u,X]=$$

$$\frac{p_c(t_s,X)}{\rho_{t_s}^c}h_c(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h_c(m,X)]E[Y|T_s>t_u,T_c>t_u,T_u=t_u,X]=$$

$$\frac{p_c(t_s,X)}{\rho_{t_s}^c}h_c(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h_c(m,X)]E[Y(\infty)|T_s>t_u,T_c>t_u,T_u(\infty)=t_u,X]=$$

$$\frac{p_c(t_s,X)}{\rho_{t_s}^c}h_c(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h_c(m,X)]E[Y(\infty)|T_s=t_s,T_c>t_u,T_u(\infty)=t_u,X]=$$

$$\frac{p_c(t_s,X)}{\rho_{t_s}^c}h_c(t_u,X)\prod_{m=t_s}^{t_u-1}[1-h_c(m,X)]E[Y(\infty)|T_s=t_s,T_c>t_s,T_u(\infty)=t_u,X],$$

where the second equality follows from the definition of $w_{c0}^{t_s}(t_u, X)$, the third equality by simplifying, the fourth equality by Assumptions 2, the fifth equality by applying Assumption 3 for $t_s, ..., t_u$, and the sixth equality by applying Assumption 7 for $t_s, ..., t_u$.

From (A.55) and (A.56)

$$E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} > T_{u,i}, T_{c,i} > T_{u,i}, T_{u,i} \geq t} w_{c0}^{t_s}(T_{u,i}, X_i) Y_i\right] = \tag{A.57}$$

$$E_{X|T_s \geq t_s, T_c > t_s, T_u \geq t_s}[\frac{p_c(t_s, X)}{\rho_{t_s}^c} \sum_{t_u=t_s}^{T_u^{\max}} h(t_u, X) \prod_{m=t_s}^{t_u-1} [1 - h(m, X)] \times .$$
$$E[Y(\infty)|T_s \;=\; t_s, T_c > t_s, T_u(\infty) = t_u, X].$$

Next, by similar reasoning as for (A.8) we have

$$\sum_{t_u=t_s}^{T_u^{\max}} h(t_u, X) \prod_{m=t_s}^{t_u-1} [1 - h(m, X)] E[Y(\infty)|T_s = t_s, T_c > t_s, T_u(\infty) = t_u, X] = \tag{A.58}$$
$$E[Y(\infty)|T_s = t_s, T_c > t_s, T_u(\infty) \geq t_s, X].$$

Thus, from (A.57) and (A.58)

$$\begin{aligned}
&E\left[\frac{1}{N_{t_s}} \sum_{i \in T_{s,i} > T_{u,i}, T_{c,i} > T_{u,i}, T_{u,i} \geq t} w_{c0}^{t_s}(T_{u,i}, X_i) Y_i\right] = \qquad \text{(A.59)}\\
&E_{X|T_s \geq t_s, T_c > t_s, T_u \geq t_s} \left[\frac{p_c(t_s, X)}{\rho_{t_s}^c} E[Y(\infty)|T_s = t_s, T_c > t_s, T_u(\infty) \geq t_s, X]\right] = \\
&\frac{\Pr(T_s = t_s | T_u \geq t_s, T_c > t_s, T_s \geq t_s)}{\rho_{t_s}^c} E[Y(\infty)|T_s = t_s, T_c > t_s, T_u(\infty) \geq t_s] = \\
&E[Y(\infty)|T_s = t_s, T_c > t_s, T_u(\infty) \geq t_s],
\end{aligned}$$

since $\rho_{t_s}^c = \Pr(T_s = t_s | T_u \geq t_s, T_c > t_s, T_s \geq t_s)$.

Finally, (A.54) and (A.59) imply that $E\left[\widehat{\text{ATET}}(t_s)\right] = \text{ATET}(t_s)$.

Appendix B. Monte Carlo simulation

Simulation design

This simulation study examines properties of the estimator introduced in the paper. We use the following notation for the conditional exit probability out of the initial state: $\theta_{T_u}(t) = \Pr(T_u = t_u | T_u \geq t_u)$, and the conditional treatment probability: $\theta_{T_s}(t)$ $= \Pr(T_s = t_s | T_u \geq t_s, T_s \geq t_s)$. We consider the following discrete time DGP:

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

with X, v_u, v_s, v_y all independently distributed of each other, and $f(h) = [1+\exp(-h)]^{-1}$, i.e. we use a logistic model for the conditional exit and treatment probabilities.

This model has several properties worth noticing. First, the treatment can start at any point during the first 12 time periods, corresponding to a treatment in place during the first year (if the time period is a month). Second, both durations, T_u and T_s, and the outcome, Y, depend on observed and unobserved characteristics. However, since the unobserved effect in the treatment equation is uncorrelated with the other unobserved effects, the unconfoundedness assumption holds. Third, 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 may be unobserved motivation and/or unobserved ability.

In the baseline setting, the correlation between the unobserved characteristics in the exit and long-outcome equations β_{v_u} is 1, the baseline treatment probability parameter α_s is -3.0, the impact of the covariate on treatment β_s is 1, the treatment effect on the long-run outcome δ is 0, and impact of the covariate on the long-run outcomes β_Y is set to 1. These parameters are then varied in four different ways. Model A varies the baseline treatment parameter (α_s between -4.5 and -1.5), and Model B varies the impact of the covariate on treatment (β_s between 0 and 2). With $\alpha_s =$ -4, the conditional treatment probability in each period is 0.021 while with $\alpha_s =$ -2 this is 0.13. If β_s equals

0.5 the conditional treatment probability varies between 0.029 and 0.076; and if $\beta_s = 1.5$, this probability varies between 0.011 and 0.18. Model C varies the correlation between the unobserved characteristics in the exit and long-outcome equations (β_u between 0 and 2). Finally, Model D allows the treatment effect on the long-run outcome, δ, to vary between 1 and 10.

We focus on the aggregated effect ATET. All propensity scores are estimated with a correct logistic model specification. We initially study the bias of each estimator. The sample size is set to 10,000 and the number of replications is 2000. Common support is imposed through the above-mentioned variant of the three-step approach from Huber et al. (2013), with the upper limit on the weight given to a certain observation set to 1%. After this we study the size and variance of the dynamic estimator, using bootstrapped standard errors (99 replications).

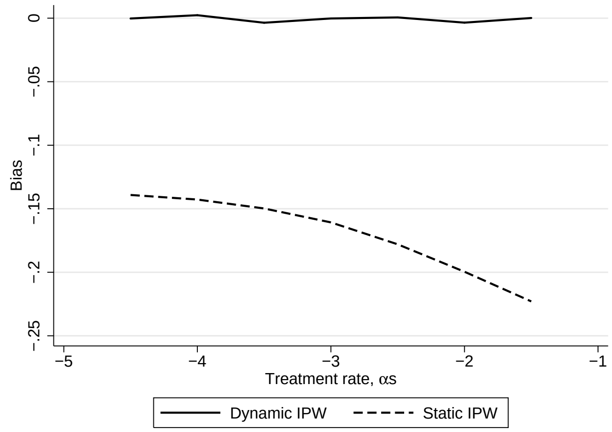
Simulation results

We compare the dynamic IPW estimator and a static IPW estimator. Figure B.1 reports how the bias of the two estimators are related to the baseline treatment rate. As expected, the static IPW estimator is biased, and the bias is increasing in the treatment parameter (higher α_s). This is because a higher conditional treatment probability implies more extensive dynamic treatment assignment. The bias of the dynamic IPW estimator, on the other hand, is virtually zero for all treatment probabilities and roughly 100 times smaller than for the static IPW estimator.

Figure B.2 also shows that the bias of the static IPW estimator increases with the variance of the treatment probability across units (larger β_s), while the dynamic approach is unbiased for all values of β_s. From Figure B.3, it can also be seen that the bias of the static approach is increasing in the correlation between the unobserved characteristics in the exit and long-outcome equations, β_u. Again, the bias of our dynamic approach is virtually zero.

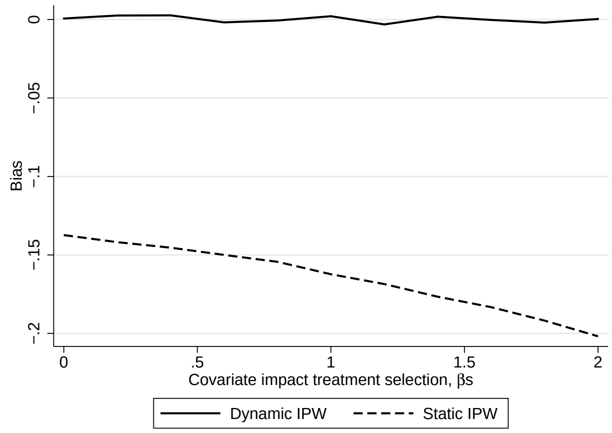
Finally, Table B.1 presents the bias, variance and size of our dynamic IPW estimator. The simulation results are for sample sizes of 1000 and 4000. We vary the parameters of the DGP in a similar was for Models A-D, but we only report simulation results for the baseline case and one additional case for each model. First, as expected based on the results in Figures B.1–B.4, the bias is small in all cases. Size is for a test with nominal size of 5%, so that the IPW estimator roughly has correct size (Columns 3 and 6). The tables also shows that standard error decreases by roughly 50% when the sample size is increased by a factor of four from 1000 to 4000, suggesting that the estimator is $\sqrt{N}$-convergent.

Figure B.1: Simulated bias for our dynamic IPW estimator and a static IPW estimator. Model A: baseline treatment rate



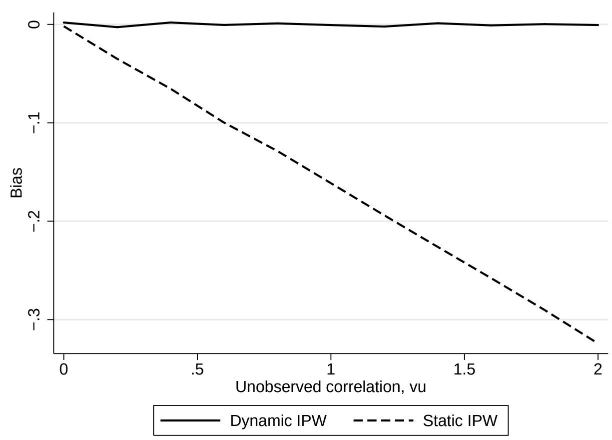
Note: α_s is the conditional treatment probability parameter. The data generating processes for the logistic simulation models described in the text. Bias for aggregated effect of treatment over the first 12 months. Dynamic IPW is the estimator introduced in this paper. Static IPW is a standard static IPW estimator with normalized weights. Results are based on 2,000 replications.

Figure B.2: Simulated bias for our dynamic IPW estimator and a static IPW estimator. Model B: impact of the covariate on the conditional treatment probability



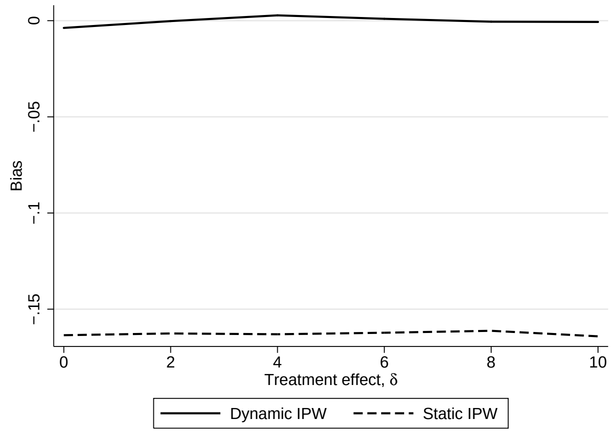
Note: β_s is the impact of the covariate on treatment. The data generating processes for the logistic simulation models described in the text. Bias for aggregated effect of treatment over the first 12 months. Dynamic IPW is the estimator introduced in this paper. Static IPW is a standard static IPW estimator with normalized weights. Results are based on 2,000 replications.

Figure B.3: Simulated bias for our dynamic IPW estimator and a static IPW estimator. Model C: correlation between the unobserved characteristics in the exit and long-outcome equations



Note: β_u determines the correlation between the unobserved characteristics in the exit and long-outcome equations. The data generating processes for the logistic simulation models described in the text. Bias for aggregated effect of treatment over the first 12 months. Dynamic IPW is the estimator introduced in this paper. Static IPW is a standard static IPW estimator with normalized weights. Results are based on 2,000 replications.

Figure B.4: Simulated bias for our dynamic IPW estimator and a static IPW estimator. Model D: treatment effect on the long-run outcome



Note: δ is the treatment effect. The data generating processes for the logistic simulation models described in the text. Bias for aggregated effect of treatment over the first 12 months. Dynamic IPW is the estimator introduced in this paper. Static IPW is a standard static IPW estimator with normalized weights. Results are based on 2,000 replications.

Table B.1: Simulated bias, size and variance of the dynamic IPW estimator

	1000 observations			4000 observations		
	bias [1]	se [2]	size [3]	bias [4]	se [5]	size [6]
Baseline model	-0.005	0.044	0.065	0.001	0.011	0.061
Unobserved correlation, β_u=2	-0.003	0.049	0.058	0.001	0.012	0.048
Treatment rate, α_s=-2	-0.005	0.027	0.046	-0.003	0.007	0.049
Treatment selection, β_s=-2	-0.000	0.047	0.054	-0.001	0.012	0.065
Treatment effect, δ_s=5	-0.000	0.047	0.054	-0.001	0.012	0.065

Note: Model with no treatment effect. Full generating processes described in the text. IPW estimates with bootstrapped standard errors (99 replications) and assuming true scores, respectively. Size is for 5% level tests. The results are based on 2000 replications.