Received: 5 March 2019 | Revised: 12 April 2021 | Accepted: 18 April 2021

DOI: 10.1002/jae.2866

WILEY Journal of APPLIED ECONOMETRICS

RESEARCH ARTICLE

Dynamic evaluation of job search assistance

Stephen Kastoryano[1] | **Bas van der Klaauw**[2]

[1]Department of Economics, University of Reading, Reading, UK

[2]Department of Economics, VU University Amsterdam, Amsterdam, The Netherlands

Correspondence
Stephen Kastoryano, Department of Economics, University of Reading, Shinfield Rd, Reading RG6 6EL, UK.
Email: s.p.kastoryano@reading.ac.uk

Funding information
Dutch Science Foundation

Summary

This paper evaluates a job search assistance program for unemployed teachers where the assignment to the program is dynamic. We discuss the methodology of estimating dynamic treatment effects and identification conditions. In the empirical analysis, we use administrative data from a unique institutional environment in which we observe all variables determining assignment to the job search assistance program. This allows us to compare results from a dynamic discrete-time evaluation model and a continuous-time duration model. All approaches show that participation in the job search assistance program reduces exit rates from unemployment, in particular when starting the program early during the spell of unemployment. The discrete-time approach makes less strict parametric assumptions, but the results are sensitive to the choice of control group and the unit of time.

KEYWORDS

dynamic treatment evaluation, labor market policy

1 | INTRODUCTION

The usual selection problem that program participants may have different characteristics than nonparticipants has been addressed extensively in microeconometrics (e.g., LaLonde, 1986). However, the evaluation of active labor market programs is often further complicated by ongoing entry into programs. In these dynamic settings, constructing an appropriate control group for individuals who enter the program at one moment is difficult because other individuals enter the program later (e.g., Sianesi, 2004). Constructing the control group by only including those individuals who are observed not to have participated in the program, for example, because they left unemployment before entering the program, implies conditioning on future outcomes (Eberheim et al., 1997; Ham & LaLonde, 1996). Because future outcomes depend on dynamic selection and dynamic selection is not accounted for in the control group, this may bias relatively simple treatment evaluation estimators (Fredriksson & Johansson, 2008).

This paper empirically evaluates a job search assistance program for unemployed primary-school teachers in the Netherlands. The outcome variable we consider is the exit from unemployment, which is the key variable of interest to policy makers.[1] We first present a dynamic potential outcomes framework and define the relevant treatment effects. The framework draws from the literature on dynamic treatment evaluation and in particular from the survey by Abbring and Heckman (2007). Within the dynamic potential outcomes framework, we show that defining counterfactual outcomes and treatment effects requires a no-anticipation assumption. This no-anticipation assumption is justified from the randomness in the moment of starting the job search assistance program and the lack of behavioral responses to the invitation

[1]Focusing, for example, on wages is less relevant in this context because a majority of the unemployed teachers return to working at primary schools where wages are determined by collective bargaining and are almost a deterministic function of the individual's age (with some extras for managerial responsibilities).

letter. Our administrative data are from a unique institutional environment where the assignment to the job search assistance program is very clearly described and is guaranteed to only depend on a limited set of characteristics that we also observe. This allows us to make a credible dynamic conditional independence assumption. Usually, such an assumption is justified from the richness of the data rather than knowledge of the institutional environment. Gerfin and Lechner (2002) and Sianesi (2004), for example, argue that information on past labor market outcomes and subjective assessments of labor market prospects justify their dynamic conditional independence assumption. Lalive et al. (2008) show that even if such information is available, program effect estimates vary substantially between estimation methods. Unlike our institutional setting, in their setting, it is unclear whether a conditional independence assumption is valid.

Our main contribution is to compare the implementation of discrete-time and continuous-time methods for estimating dynamic treatment effects. We show the performance of both estimation methods in an institutional environment where the identifying assumptions very likely hold. Our discrete-time approach is flexible in constructing outcome distributions and also deals with later enrollment in the program of individuals assigned to the control group. In particular, we evaluate all possible paths starting with treatment participation until the outcome a number of periods later, which relates to the g-computation formula introduced by Robins (1986) in the biostatistical literature (see also Gill & Robins, 2001; Robins, 1997). However, the estimated treatment effects from this discrete-time model are sensitive to the choice for the unit of time and the construction of the control group. A short unit of time implies that in each period, only few transitions are observed, which reduces the precision of the estimates. Extending time periods causes a bias because dynamics within a time period are ignored. Furthermore, we show that our estimates deviate from those obtained using the methods proposed by Sianesi (2004) and Fredriksson and Johansson (2008), who simplify the construction of the potential untreated outcomes.

The continuous-time approach builds on a proportional hazard model. The dynamic conditional independence assumption allows to keep the proportional hazard model relatively simple; that is, we do not have to allow for dependence between hazard rates as in Abbring and Van den Berg (2003). We find some differences in the estimated program effects between the discrete-time and the continuous-time approaches. This suggests that even if the identifying assumptions are very likely to hold, the specification of the empirical model is important. The functional form of the proportional hazard rate specification is restrictive, and this may affect the estimated program effects.

We show theoretically how the treatment effects for the participants at a specific moment can be translated in an average treatment effect for the treated. This can be used to assess the efficiency of modified program assignment rules assuming that changing the treatment assignment rule does not affect the treatment effects for participants at each moment, which is useful from a costs–benefit perspective of the program. However, an empirical investigation of changing the treatment assignment rule is not very useful, because both the discrete-time approach and the continuous-time approach find that participation in the program has only adverse effects on the exit rate from unemployment. We explain these adverse effects from a mismatch between the content of the programs and the ambitions of unemployed teachers. The finding is important for policy makers who often consider investments in active labor market programs as a necessary requirement to reduce moral hazard in a system with relatively generous unemployment benefits (see OECD, 2010).

The paper is organized as follows. In Section 2, we provide details about the job search assistance program. Section 3 presents a general framework for dynamic treatment evaluation. Section 4 discusses our estimation procedure. Section 5 presents the data, justifies the key assumptions, and presents the estimation results. Finally, Section 6 concludes.

2 | JOB SEARCH ASSISTANCE

Our data concern former employees of Dutch primary education institutions, who are entitled to collect unemployment insurance (UI) benefits. Most individuals aim at finding new work again in the primary education sector, but about one-third of the observed exits are towards employment outside this sector. Below, we provide a brief description of the UI system and the job search assistance program; more information can be found in the Supporting Information.

The primary education sector has their own UI agency, which provides job search assistance programs to benefits recipients. The program is offered to individuals who receive benefits for at least 8 h per week and with an entitlement period exceeding 3 months. Unemployed workers under age 60 are obliged to participate in these programs if these are offered to them. Participation in a program does not affect the entitlement to benefits.

The timing of assignment to the program differs depending on an age criteria. All individuals above age 50 (at the first day of unemployment) and (low-skilled) individuals who were previously employed in a subsidized job are supposed to enter the job search assistance program immediately after starting collecting benefits. Individuals under age 50 and who are not low-skilled enter the program only after 6 months of unemployment. The programs are offered at 11 locations. Once invited, the benefits recipients can choose the location, but 75% of the individuals accept the default. The remaining 25% almost always opt for the location nearest to their home.

The job search assistance program starts with an intake interview to determine the required activities. These range from improving language skills, providing psychological support, providing short vocational class, and offering the type of job search assistance services also included in the short program. The training takes place both in individual and group meetings. The intensity of the meetings depends on the needs of the individual. The first weeks are often more intense, with two to three meetings per week with training officers. The total time spent in these meetings is about one full working day per week. After this period, the participants usually visit the training center once a week or every other week for a few hours. During this later stage, participants receive weekly assignments to be discussed in the weekly meetings.

The general goal is that after 2 months of participating in the program, individuals start making successful job applications. However, participation in the program does not lower the job search requirements. The job search assistance program should not last longer than 1 year, and individuals who start a new job during the program are offered to finish the program while working. The cost of the program is 4000 euro for individuals above age 50 and for low-skilled individuals and 3750 euro for individuals below age 50. These costs are paid by the benefits agency to the commercial agencies offering the programs.

The UI agency does not assign benefits recipients directly to programs but outsources this task to a separate firm. This firm never has personal contact with unemployed workers and receives only a limited amount of information when assigning them to a program. The information consists of the social security number, gender, age, an indication for being low-skilled (i.e., previously in a subsidized job), the length of entitlement to benefits, number of weekly hours of collecting benefits, and an indicator code for the previous employer. Two weeks prior to the start of the program, the individual receives a letter explaining that she should enroll in a program. This letter allows the possibility for individuals to select one of the 11 locations. The policy is to avoid having individuals previously employed at the same institution in the same meeting groups.

In practice, the policy guidelines concerning the timing of entering the job search assistance program were not followed strictly. This was due to administrative and communication issues between the UI agency and the external firm. In the Netherlands, all individuals applying for UI benefits apply at the nationwide UI administration. This administration forwards files of workers from the primary education sector to the specific UI agency, which already causes a delay ranging from a few days to a few weeks. In addition, there are cases where records were lost, where information was provided too late, and where notification letters were never sent. As we will show in the next section, this creates substantial variation in the assignment of the program. And, because the external firm never had any contact with benefits recipients, the variation in program assignment should be exogenous conditional on observed individual characteristics. We exploit the latter in the empirical analysis.

3 | THEORETICAL FRAMEWORK FOR DYNAMIC TREATMENT EVALUATION

In this section, we present a framework for dynamic treatment evaluation where the outcome of interest is leaving unemployment and treatment participation only occurs while being unemployed. Early empirical studies of this dynamic setting are by Ham and LaLonde (1996) and Eberheim et al. (1997). More theoretical developments to models for dynamic treatment evaluation are provided by Abbring and Van den Berg (2003), Sianesi (2004), Fredriksson and Johansson (2008), and Vikström (2017). In this section, we provide a framework that is a special case of Abbring and Heckman (2007) and relates to the biostatistical literature on dynamic treatment effects (e.g., Robins, 1997), which was first introduced to economics by Lechner (2009).

Consider the case where we observe for each individual the duration $T > 0$ of unemployment. We define the binary variable Y_t as indicator for being unemployed ($Y_t = 0$) or employed ($Y_t = 1$) after t periods, so $Y_t = I(T \leq t)$. The outcome variable describes the survival in unemployment, $\mathrm{E}[Y_t] = 1 - \Pr(T > t)$. Individuals can participate in a single treatment only once during the period of unemployment. This setting is discussed by Abbring (2003), Abbring and Van den Berg (2003), Sianesi (2004), Fredriksson and Johansson (2008), and Vikström (2017).[2] All individuals are eligible for

[2]Robins (1997) and Lechner (2009) present frameworks where a binary treatment choice is made in each (discrete) period, whereas Gill and Robins (2001) consider changing the treatment intensity over time.

having treatment. However, the timing of treatment differs between individuals.

Individuals can only start treatment when being unemployed, and they may leave unemployment before actually starting treatment. Let $S > 0$ be a random variable describing the elapsed unemployment duration at the moment of entering treatment. Young et al. (2011) refer to this type of treatment assignment as a randomized static treatment plan. In a randomized static treatment plan, treatment at s does not depend causally on previous outcomes or time-varying intermediate individual characteristics. Robins (1997), Gill and Robins (2001), and Lechner (2009) allow the treatment decision in each time period to depend on earlier outcomes. In our case, only the yet untreated survivors in unemployment can enter the job search assistance program. Treatment assignment at time s thus depends on $Y_s = 0$. However, one may argue that the moment of treatment $S = s$ is assigned at the start of unemployment $t = 0$ and treatment is only realized when the individual's unemployment duration T exceeds s.

Gill and Robins (2001) suggest building a counterfactual space on top of the factual outcomes. Let $Y^*_{1,t}(s)$ denote the potential unemployment status after t periods had the individual been treated at s under a given policy regime. Usually, a consistency assumption is made to link potential outcomes to observed outcomes. This assumption implies that if we observe $S = s$, the random variable describing the outcome Y_t equals the potential outcome $Y^*_{1,t}(s)$. Even though we only consider a single treatment, it may have different effects when initiated at different moments s.

To construct treatment effects, it is necessary to define nontreated potential outcomes. This is less obvious in a dynamic setting because untreated survivors are eligible to start treatment later. Therefore, we adopt the no-anticipation assumption described by Abbring and Van den Berg (2003), which states that treatment participation can only affect later outcomes

$$Y^*_{1,t}(s) = Y^*_{1,t}(s') \qquad \text{if} s \neq s' \qquad \forall t < s, s'. \tag{1}$$

Abbring and Heckman (2007) refer to this assumption as no causal dependence of outcomes on future treatments. This no-anticipation assumption imposes that the effect of an intervention does not precede the start of the intervention, which is useful for defining untreated potential outcomes. Because of the no-anticipation assumption,

$$Y^*_{0,t} = Y^*_{1,t}(s) \qquad \forall s > t. \tag{2}$$

The no-anticipation assumption is often explicitly made in cases where treatment participation is only observed for individuals who are still unemployed (e.g., Vikström, 2017). It ensures that all outcomes of yet untreated individuals can be considered as potential untreated outcomes, rather than being affected by some future treatment participation. Therefore, our framework is not useful when studying, for example, benefits drops when reaching the end UI entitlement. The literature often shows spikes in job finding just prior to exhausting UI benefits, which indicates that outcomes are affected by future treatment (Katz & Meyer, 1990; Moffitt, 1985).

Most microeconometric applications (e.g., Vikström, 2017) aim at estimating the average treatment effect on the surviving treated, which is defined as,

$$\Delta_{\text{ATEST}}(t, s) = \mathrm{E}\left[Y^*_{1,t}(s) - Y^*_{0,t} | S = s, Y_s = 0\right] \qquad \text{with} \ \ t > s. \tag{3}$$

This treatment effect denotes the effect of providing treatment at s on exit to work between s and t for those who survived in unemployment for s periods. This is the ex post effect of the treatment, so the effect of actually participating in the treatment on future outcomes. The size of this treatment effect can vary by the moment of the intervention s for two reasons (assuming that the time period between t and s is held constant). First, the treatment can have different effects when being imposed at different moments and second the composition of survivors in unemployment changes with s.

The average treatment effect on the surviving treated $\Delta_{\text{ATEST}}(t, s)$ provides a series of effects for different values of s and t. Policy makers interested in the overall effectiveness of a treatment on all (recent) participants additionally require knowledge about the treatment assignment mechanism. Let $f(s)$ denote the density function of starting treatment at time s and $S^*_0(s)$ the potential survivor function in unemployment until time s for individuals who do not participate in treatment before s. We define the average treatment effect on the treated t time periods after entering unemployment as

$$\Delta_{\text{ATET}}(t) = \frac{\int_{s=0}^{t} f(s) S^*_0(s) \mathrm{E}\left[Y^*_{1,t}(s) - Y^*_{0,t} | S = s, Y_s = 0\right] ds}{\int_{s=0}^{t} f(s) S^*_0(s) ds}. \tag{4}$$

This average treatment effect weighs the treatment effects by treatment participation after each duration s and describes the average treatment effect on those individuals who started participating in the treatment before time period t. This average treatment effect on the treated can be used for a cost–benefit analysis if the main policy interest is having individuals leave unemployment within t time periods. Alternatively, after estimating $S_0^*(s)$ and $\mathrm{E}\left[Y_{1,t}^*(s) - Y_{0,t}^*|S = s, Y_s = 0\right]$, the empirical treatment assignment rule $f(s)$ can be replaced by an alternative treatment assignment rule $g(s)$ to predict how such an alternative assignment affects exit from unemployment within t time periods. This would assume that the treatment assignment rule does not affect potential outcomes, which is not guaranteed by no-anticipation. Substantial changes in the policy regime may affect individual optimization.

The key empirical problem is estimating $\Delta_{\text{ATEST}}(t, s)$. Therefore, unemployed workers treated at s should be compared to similar unemployed workers who (possibly) receive treatment after t. To deal with the usual selection problem, it is often assumed that conditional on a set of observed characteristics X, there are no unmeasured confounders jointly determining treatment participation and future employment status. In our dynamic setting, this results in the conditional independence assumption

$$Y_{1,t}^*(s') \perp I(S = s) \,|\, Y_s = 0, S \geq s, X = x \qquad \forall s \geq 0 \text{ and } s' \geq s \text{ and } t > s. \tag{5}$$

This is an extension of the conditional independence assumption in static settings and asserts that, conditional on some observed characteristics X, at any duration s, treatment is randomly assigned among the yet untreated survivors in unemployment. This dynamic conditional independence assumption is similar to the assumptions made by Robins (1997), Gill and Robins (2001), Sianesi (2004), Fredriksson and Johansson (2008), Lechner (2009), and Vikström (2017).

In Section 5.1, we provide a justification for the assumptions discussed above, and the next section presents two empirical approaches based on the framework in this section. The first empirical specification is a flexible discrete-time approach, but discretizing time requires additional assumptions when restricting the length of a time period. The second is a continuous time approach, which imposes a stronger functional form assumption (i.e., a proportional hazard structure).

4 | EMPIRICAL MODELS

We consider two possible approaches to estimate the treatment effects defined in the previous section. First, we describe a discrete-time model, which has some similarities with the approaches taken by Robins (1997) who defines a treatment regime as a vector of assignment rules mapping the history of treatment and observed variables to a current treatment and Vikström (2017) who relies on matching and weighting observation. Second, we present a continuous-time duration model.

4.1 | Discrete-time approach

In our discrete-time approach, one unit of time is denoted by κ. This defines the interval cut-off points $\tau_{k+1} = \tau_k + \kappa$ with $\tau_0 = 0$ the moment at which the unemployment spell starts. Then Y_{τ_k} is the observed employment status after k units of time with $Y_{\tau_0} = 0$. Discretizing time has two consequences for the potential outcomes. First, it is only relevant in which time interval treatment starts. Because the exact timing within the interval does not matter, $Y_{1,\tau_k}^*(s) = Y_{1,\tau_k}^*(s')$ if both s and s' lie within the same time interval $\langle \tau_j, \tau_{j+1}]$. Second, the start of treatment can only affect the outcomes from the subsequent time interval onwards, which implies $Y_{1,\tau_k}^*(s) = Y_{0,\tau_k}^*$ if $s > \tau_{k-1}$.

Because potential outcomes only depend on the time interval in which treatment starts, we define

$$\tilde{S} = \kappa \sum_{j=1} I(S \leq \tau_j).$$

So $\tilde{S}$ describes the end of the time interval in which treatment starts, which means that $\tilde{S} = \tau_j$ implies that treatment started in the jth time interval, that is, $\langle \tau_{j-1}, \tau_j]$.

In discrete-time, the dynamic version of the conditional independence assumption becomes

$$Y_{1,\tau_k}^*(s) \perp\!\!\!\perp I(\tilde{S} = \tau_j) | \tilde{S} > \tau_{j-1}, Y_{\tau_j} = 0, X = x \qquad \forall k > j > 0 \text{ and } s > \tau_{j-1}. \tag{6}$$

In addition, we follow Robins (1997) and Lok et al. (2004) by imposing that treatment sequences can be evaluated,

$$0 < \Pr(\tilde{S} = \tau_k | \tilde{S} > \tau_{k-1}, Y_{\tau_k} = 0, X = x) < 1 \;\text{ if }\; \Pr(\tilde{S} > \tau_{k-1}, Y_{\tau_k} = 0 | X = x) > 0 \quad \forall k > 0 \text{ and } x. \tag{7}$$

This common support assumption guarantees that at any moment, exposure to the job search assistance program is not deterministically allocated among untreated survivors.

We consider start of treatment in the jth time interval, that is, $\tau_{j-1} < s \leq \tau_j$ and $\tilde{S} = \tau_j$. Then we can rewrite the treatment effects of interest from the previous section in discrete-time notation as

$$\begin{aligned}\Delta_{\text{ATEST}}(\tau_k, \tau_j) =& \mathrm{E}\left[Y^*_{1,\tau_k}(\tau_j) - Y^*_{0,\tau_k} | \tilde{S} = \tau_j, Y_{\tau_j} = 0\right] \\ =& \int_x \mathrm{E}[Y^*_{1,\tau_k}(\tau_j) | \tilde{S} = \tau_j, Y_{\tau_j} = 0, X = x,] f_X(x | \tilde{S} = \tau_j, Y_{\tau_j} = 0) dx \\ &- \int_x \mathrm{E}[Y^*_{0,\tau_k} | \tilde{S} = \tau_j, Y_{\tau_j} = 0, X = x] f_X(x | \tilde{S} = \tau_j, Y_{\tau_j} = 0) dx,\end{aligned} \tag{8}$$

where $F_X(x | \tilde{S} = \tau_j, Y_{\tau_j} = 0)$ is the distribution of observed covariates for the untreated survivors at τ_j who enter the program in the interval $<\tau_{j-1}, \tau_j]$.

Under the assumptions above, we can write

$$\begin{aligned}&\mathrm{E}[Y^*_{1,\tau_k}(\tau_j) | \tilde{S} = \tau_j, Y_{\tau_j} = 0, X = x] \\ &= 1 - \prod_{l=j+1}^{k} \Pr(Y_{\tau_l} = 0 | \tilde{S} = \tau_j, Y_{\tau_{l-1}} = 0, X = x),\end{aligned} \tag{9}$$

and for the untreated outcome,

$$\begin{aligned}&\mathrm{E}[Y^*_{0,\tau_k} | \tilde{S} = \tau_j, Y_{\tau_j} = 0, X = x] \\ &= 1 - \prod_{l=j+1}^{k-1} \left(\Pr(Y_{\tau_{l+1}} = 0 | \tilde{S} > \tau_l, Y_{\tau_l} = 0, X = x) \cdot \Pr(\tilde{S} > \tau_l | \tilde{S} > \tau_{l-1}, Y_{\tau_l} = 0, X = x)\right) \\ &\quad \cdot \Pr(Y_{\tau_{j+1}} = 0 | \tilde{S} > \tau_j, Y_{\tau_j} = 0, X = x),\end{aligned} \tag{10}$$

if $k > j+1$ and $\Pr(Y_{\tau_{j+1}} = 0 | \tilde{S} > \tau_j, Y_{\tau_j} = 0, X = x)$ if $k = j+1$. The treatment effect of interest $\Delta_{\text{ATEST}}(\tau_k, \tau_j)$ can therefore be written in terms of a series of transition probabilities. This is a special case of Robins' (1986) g-computation formula, which recursively models the outcome in terms of the history of confounders and treatment. It evaluates all possible paths leading to the observed outcome Y_{τ_k} and starting from not observing program enrollment before τ_j. For the estimation, we use matched control samples for those starting the program at any moment. Next, we estimate Kaplan–Meier estimates for the survivor probabilities both for the program participants and the (matched) control group. To compute standard errors, we apply subsampling on the untreated survivor population (Politis & Romano, 1994). Supporting Information contains the proof for the g-computation formula, the propensity score weighting version of the g-computation formula, and details about the actual estimation.

The outcome variables Y_{τ_k} may be latent when the unemployment spell is right censored prior to τ_k. Right censoring occurs when the unemployed teacher does not leave the benefits system before the end of entitlement to UI benefits or when the unemployed teacher still collects benefits at the end of the observation period. Our approach accounts for right-censoring but assumes that right censoring is exogenous.

4.2 | Continuous-time approach

The previous approach imposes discrete time intervals even though we observe unemployment durations and program enrollment at a daily level. A hazard-rate model allows to model job finding in continuous time, which avoids defining a unit of time and making assumptions on the order in which events occur within a time interval. The disadvantage of the hazard-rate model is that it requires stronger functional-form assumptions. In particular, Elbers and Ridder (1982) show that the hazard-rate model is identified when proportionality is imposed. We follow this specification, which is also the most often used in empirical research.

Consider an individual collecting benefits for t units of time. The exit rate from unemployment $\theta(t|x,v,s)$ follows the mixed proportional hazard specification

$$\theta(t|x,v,s) = \lambda(t)\exp(x'\beta + \delta \cdot I(s<t) + v), \tag{11}$$

in which x denotes observed characteristics, v unobserved characteristics, and s is the moment of entering the job search assistance program. The variable $I(s<t)$ indicates whether the individual already started participating in the job search assistance program after t periods of unemployment.

In this model, the conditional independence assumption for identifying the treatment effect δ is $S \perp V|X$, so conditional on the observed characteristics X, the moment of starting the program is independent on unobserved characteristics V. Because our focus in on the effect of the program, our interest in not in a causal interpretation of β. Therefore, we do not consider imposing independence between V and X as restrictive. When we choose a distribution $G(v)$ for the unobserved heterogeneity and a functional form for the duration dependence pattern $\lambda(t)$, we can estimate all parameters in the hazard rate.

The parameter δ is the multiplicative effect of program participation on the exit rate from unemployment. To translate this multiplicative effect into the treatment effect defined in Section 4, we need to estimate the entry rate into the job search assistance program. Therefore, we again specify a mixed proportional hazard rate

$$\theta_s(t|x,v_s) = \lambda_s(t)\exp(x'\beta_s + v_s), \tag{12}$$

and $G_s(v_s)$ is the distribution of unobserved heterogeneity in the entry rate into the program.[3]

Because the continuous-time model fully specifies the hazard rates to the job search program and out of unemployment, we can obtain an estimator for the treatment effect $\Delta_{\text{ATEST}}(t,s)$. We first define for unemployed worker i with observed characteristics x_i,

$$\mathrm{E}[Y^*_{1,t}(s) - Y^*_{0,t}|Y_s = 0; x_i, v] = \frac{\exp\left(-\int_0^t \theta(z|x_i,t,v)dz\right) - \exp\left(-\int_0^t \theta(z|x_i,s,v)dz\right)}{\exp\left(-\int_0^s \theta(z|x_i,s,v)dz\right)}. \tag{13}$$

To translate this into the average treatment effect on the treated $\Delta_{\text{ATEST}}(t,s)$, we then condition on the rate of receiving treatment after s periods. This allows us to estimate the average treatment effect on the surviving treated from

$$\Delta_{\text{ATEST}}(t,s) = \frac{\int_v\int_{v_s}\sum_i f(s|x_i,v,v_s)\mathrm{E}[Y^*_{1,t}(s) - Y^*_{0,t}|Y_s=0;x_i,v]dG_s(v_s)dG(v)}{\int_v\int_{v_s}\sum_i f(s|x_i,v,v_s)dG_s(v_s)dG(v)}, \tag{14}$$

where $f(s|x_i,v,v_s) = \theta_s(s|x_i,v_s)\exp\left(-\int_0^s \theta(z|x_i,v,s) + \theta_s(z|x_i,v_s)dz\right)$ is the rate at which individual i enters the job search assistance program after s periods. We use the delta method to compute standard errors around the treatment effects.

The job finding rate and the entry rate in the program are estimated using maximum likelihood estimation. Therefore, we parameterize the duration dependence functions $\lambda(t)$ and $\lambda_s(t)$ as piecewise constant, and for the distribution functions of the unobserved heterogeneity $G(v)$ and $G_s(v_s)$, we use discrete mass point specifications. These functional form specifications are relatively flexible.

[3] Abbring and Van den Berg (2003) allow for correlation between the unobserved heterogeneity in the job finding rate and the entry rate in the program to allow for endogeneity in starting the program. Because of the conditional independence assumption, this is not necessary in our setting. When we follow Abbring and van den Berg (2003), our estimates show that the correlation between the unobserved heterogeneity terms is insignificant (see Kastoryano & van der Klaauw, 2011).

5 | EMPIRICAL ANALYSIS

In this section, we focus on applying our empirical models to evaluate the job search assistance programs for former employees in the primary education sector. Before discussing the estimation results, we present the data and justify the no-anticipation assumption and the dynamic conditional independence assumption, which are crucial for our empirical models. Finally, we compare our estimated treatment effects with treatment effects that are obtained using related discrete-time methods.

5.1 | Data and justification of the main assumptions

In the empirical analysis, we use administrative data from the UI agency for workers in the primary education sector. Our data concern individuals who started collecting UI benefits between August 1, 2006, and April 1, 2008, and who meet the criteria for participation in the job search assistance program. The latter means that we leave out individuals who are above 60 years old, claimed benefits for less than 8 h per week, and started collecting benefits more than 30 days after being laid off. We also exclude individuals above age 60 because for them, participation in the job search assistance program is voluntary. Individuals are followed until benefits payments end (due to finding work or exhausting the entitlement period) or until March 12, 2009.

The data contain 3064 individuals for which we only consider the first observed unemployment spell.[4] Over 60% of the individuals are entitled to benefits for more than 1 year, and 40% have an entitlement period exceeding 2 years. Almost 50% of the inflow occurs in August, which is the start of a new school year (see Figure D1 in the Supporting Information). The outflow is much more spread over the year, although there is a decreasing trend over the school year. The median unemployment duration is about 21 weeks (see Figure D2a in the Supporting Information for a descriptive Kaplan–Meier estimate for leaving benefits).

Of the 3064 individuals, 940 entered the job search assistance program. We can distinguish two groups, those who should enter a program immediately (either older than 50 or low-skilled) and those who should enter after 6 months of unemployment (below 50 and not low-skilled). The data clearly show that the latter group enters the program, on average, later during the unemployment spell (Figure D2b in the Supporting Information contains for both groups the Kaplan–Meier estimate for entering a program). Also within groups, there is substantial variation in the moment of entering. This confirms that the external firm did not manage to correctly implement the rules for program assignment. It also confirms the common support assumption that for both assignment rules to the job search assistance program, there are individuals entering the program at each unemployment duration.

The data contain a limited set of individual characteristics. In Table 1, we show that when weighting for the characteristics used for the assignment of the program, the individual characteristics are balanced between individuals who participated in a program during unemployment (participants) and those who did not (nonparticipants). The p-values in the third column show no significant differences between the participants and the nonparticipants, also not for the characteristics that are not used in the assignment of the program by the external firm. The balancing of individual characteristics allows us to make a strong case that, conditional on the variables used for the assignment to the job search assistance program, there are no unmeasured confounders jointly determining assignment to the job search assistance program and future employment outcomes.

Dynamic selection can cause that our assumptions are violated. This occurs when individuals anticipate treatment and, therefore, leave the state just before the actual start of treatment. Such behavior is observed by, for example, Black et al. (2003), who find that unemployed workers are more likely to leave the benefits program once they have been informed about the actual start of a job search assistance program. A similar result is found by Crépon et al. (2018). In their setting, the average time between notification and entry in the program is almost 3 months, which is much longer than in our empirical setting.

Our data contain some information on invitation letters for the job search assistance program, which should be sent about 2 weeks prior to the start of the program. However, this information is very incomplete. Letters are only recorded since April 2008, so no information is available on the first 2 years of the observation period. There is also no guarantee

[4]We drop three individuals who very often entered and exited unemployment during the observation period. We exclude 43 observations for which the date of entering the job search assistance program is unknown or prior to becoming unemployed. The latter might occur if the individual was still in the program from an earlier unemployment spell. Finally, we exclude 37 observations with an hourly wage in the previous job below 3 euro, which is far below the legally binding minimum wage.

TABLE 1 Descriptive statistics

	Program participants	Non participants	*p*-value
Number of observations	940	2124	
Median unemployment duration (in days)	369	96	
Median duration to program start (in days)	156		
Benefits level (daily)	€101.9	€100.1	0.323
Benefits entitlement less than 2 years	23.6%	24.0%	0.833
Female	63.8%	63.0%	0.733
Age 20–35	9.1%	9.2%	0.935
Age 35–50	45.8%	58.1%	0.295
Age 50–60	45.1%	42.6%	0.271
Low-skilled	34.6%	34.6%	1.000
Job loss during summer	47.6%	51.7%	0.077
West	37.3%	37.1%	0.937
Rest of the Netherlands	62.7%	62.9%	0.937

Note: Means of explanatory variables are weighted by being low-skilled, being older than 50 years old and UI entitlement period. For each participant, weights are assigned to the nonparticipants who were still unemployed at the moment the participant entered the program. The *p*-values relate to a *t*-test for similar (weighted) means.

that for the later period the information on the letters is complete. In total, the data contain 279 letters. In almost all cases, 14–20 days elapsed between the sending of a letter and the start of a job search assistance program. No one left unemployment in the period after receiving the letter and starting the job search assistance program, whereas four individuals left unemployment in the 2 weeks prior to receiving the letter. This shows that anticipation to the start of the program is unlikely to cause substantial dynamic selection.

Our framework does not exclude the possibility that individuals know they are exposed to the risk of participating in treatment and, therefore, behave differently than in a system in which treatment is absent (Heckman & Navarro, 2007). Individuals may be informed about the treatment assignment rules. An individual above age 50 may know that she should enter the job search assistance program as soon as possible. However, individuals should not choose the timing of reemployment given exact knowledge of the future program participation. Individuals should also not manipulate their assignment to the job search assistance program. Given the assignment through the external firm, it is unlikely that individuals can either manipulate or obtain exact knowledge about their actual assignment date. Recall that unemployed workers do not know about the existence of the external firm, and the external firm only received limited information about each unemployed worker. The data also do not show a spike in job finding just prior to 6 months for those individuals that should enter the program after 6 months of unemployment. This thus also does not hint towards anticipation.

5.2 | Average treatment effects on the treated survivors

Table 2 presents the estimated treatment effects using both the discrete-time and the continuous-time approaches. We consider the effects of entering the job search assistance program in the second, fourth, and eighth month of unemployment on exit within 3 or 8 months after starting the program. This gives insight in both the effects of entering the program early and late and on short-run and longer-run outcomes of program participation.

Both models estimate that participation in the job search assistance program does not stimulate the exit rate from unemployment. According to the discrete-time model, individuals who enter the program during the second month of unemployment are about 10 percentage points less likely to exit unemployment between the beginning of the third month and the end of the fifth month. And the probability of leaving unemployment between the third and ninth month is almost 17 percentage points lower. The negative program effects are smaller when starting the program at a later moment and become insignificant when starting in the eighth month of unemployment. The continuous-time model finds significantly negative effects of participating in the job search assistance program at all moments of entering and for all outcome lengths. This is not surprising because the continuous-time model specifies the program effect as a homogeneous mul-

TABLE 2 Average treatment effects on the treated survivors

(s, t) (in months)	(2, 5)	(2, 10)	(4, 7)	(4, 12)	(7, 10)	(7, 15)
Discrete-time logit						
$E[Y^*_{0,t} \mid S = s, Y_s = 0]$	0.126	0.302	0.130	0.260	0.180	0.449
	(0.013)	(0.028)	(0.021)	(0.046)	(0.024)	(0.056)
$E[Y^*_{1,t}(s) \mid S = s, Y_s = 0]$	0.025	0.137	0.059	0.243	0.170	0.403
	(0.011)	(0.027)	(0.017)	(0.043)	(0.026)	(0.034)
$\Delta_{\mathbf{ATEST}}(\mathbf{t,s})$	−0.100***	−0.166***	−0.071**	−0.017	−0.010	−0.046
	(0.017)	(0.036)	(0.028)	(0.062)	(0.034)	(0.061)
N	2411	2411	1588	1588	771	771
N (treated)	90	90	136	136	87	87
Continuous-time hazard rate model						
$E[Y^*_{0,t} \mid S = s, Y_s = 0]$	0.178	0.363	0.184	0.348	0.183	0.470
	(0.010)	(0.017)	(0.010)	(0.017)	(0.012)	(0.025)
$E[Y^*_{1,t}(s) \mid S = s, Y_s = 0]$	0.123	0.267	0.126	0.252	0.124	0.349
	(0.010)	(0.017)	(0.010)	(0.016)	(0.010)	(0.018)
$\Delta_{\mathbf{ATEST}}(\mathbf{t,s})$	−0.055***	−0.096***	−0.058***	−0.096***	−0.059***	−0.121***
	(0.011)	(0.020)	(0.012)	(0.020)	(0.012)	(0.026)
N	3064	3064	3064	3064	3064	3064
N (treated)	788	788	788	788	788	788

Note: The unit of time in the discrete-time model is 30 days. Standard errors for the discrete-time model are obtained using 300 subsample replications and for the continuous model using the Delta method. All models include covariates as *gender*, *age*, *age*^{2} *log(pre-unemployment wage)*, *duration until exhaustion of benefits*, *I(unemployed in July or August)*, *I(age above 50)*, and *I(low skilled)*.

***Indicates significance at the 1% level.

**Indicates significance at the 5% level.

*Indicates significance at the 10% level.

tiplicative effect on exit rates, which means the effect of treatment is averaged over all moments of entrance.[5] Given that both approaches provide only negative treatment effects, investigating the consequences on changing the program assignment rule to optimize the effectiveness of the program does not make sense.

The discrete-time and continuous-time approaches show some differences in estimated treatment effects, which are related to differences in estimated potential outcomes. The continuous-time model estimates the potential untreated outcomes $Y^*_{0,t}$ higher than the discrete-time model, but the differences in treatment effects are mainly driven by differences in the estimated potential treated outcomes $Y^*_{1,t}(s)$. Part of the difference may be because the continuous-time model relies on proportional hazard rates, implying that the piecewise constant baseline hazard accounts for all changes in exit rates during the unemployment spell. The discrete-time estimates do not impose such a strong parametric assumption on the effect of the treatment and covariates on exit from unemployment. The discrete-time model may, therefore, be better in capturing time-varying heterogeneity in job finding rates of both treated and nontreated individuals. We estimated more complicated models, for example, by stratifying the duration dependence on different covariates, but these did not show significant improvements of the model. Supporting Information shows treatment effects separately for regular and low-skilled workers and for workers above and below age 50. There is some heterogeneity in treatment effects, mainly between low-skilled and regular worker. More sensitivity analyses for the continuous-time model can be found in the earlier working paper version Kastoryano and Van der Klaauw (2011).

The potential treated outcomes in the discrete-time estimation are based on all individuals in the data who are observed to enter the program in a particular month. This makes the sample of treated individuals relatively small, which affects the precision of the estimated potential outcomes. The size and precision of the estimates also depend on the unit of time. A shorter unit of time means that within each time period, fewer transitions are observed, which causes estimates to become more noisy. Longer time intervals imply aggregating many transitions which, therefore, overlooks dynamics occurring within a time interval. This may bias the estimates. For the main estimates of the discrete-time model, the unit of time is fixed to 30 days.

[5]Parameter estimates are presented in Table D1 in the Supporting Information. To evaluate possible lock-in effects, we also show parameter estimates for a model allowing for different effects depending on the time since beginning the training program. The effects on the hazard are, however, are not significantly different from each other or from a continuous homogeneous effect.

In the upper panel of Table 3, we show the robustness of the estimated treatment effects of the discrete-time model with respect to the choice for the unit of time. The pattern is that the estimated treatment effects become more negative and less precise when the unit of time is reduced to 15 days and less negative with smaller standard errors when the unit of time is increased to 60 days. So, not only the standard errors strongly depend on the unit of time, but also differences between the size of the estimates are substantial. To deal with the trade-off between bias and variance, a cross-validation exercise can be constructed to find the unit of time that minimizes the mean squared error. The mean squared error is computed on leaving out rotating equally sized subsamples of the full sample. The optimal unit of time for a treatment effect may differ between the moment of starting the treatment s and the moment of evaluating the outcomes t.

The bottom panel of Table 3 shows the robustness of the continuous-time model with respect to the covariates included in the hazard rate. In the main specification, all covariates that the external firm had access to when assigning unemployed workers to the job search assistance program are included. In the robustness check, we restrict the set of covariates to only those covariates that should be relevant for the assignment of the program. Restricting the set of covariates makes the estimated treatment effects somewhat more negative. An explanation may be that the firm that assigns unemployed workers to the program also used the other covariates when assigning unemployed workers to the job search assistance program. An alternative explanation is that the proportional hazard specification is too restrictive, which causes a bias in the estimated treatment effects.

The data contain some information that can explain why the job search assistance program has a negative impact on job finding. About 31% of the participants find work when being in the program, and 45% finish the program without finding work. The remaining 24% of the participants quit the program early without finding work. The main reasons for quitting the program early are that the demands and ambitions of the unemployed worker do not match with the aims of the program that there is a lack of collaboration by the unemployed worker or that the unemployed worker is transferred to another program. A final major reason for quitting early is sickness of the unemployed worker.

Although we do not have comparisons to other programs or populations, it seems that a high share of participants consider the job search assistance program as unsuitable for them. Recall that the job search assistance program is an externally provided program that is targeted at the full population of unemployed workers. The program thus aims at developing general job application skills, which are maybe different from the skills necessary to find a teaching job at a

TABLE 3 Robustness of the estimated treatment effects

(s, t) (in months)	**(2, 5)**	**(2, 10)**	**(4, 7)**	**(4, 12)**	**(7, 10)**	**(7, 15)**
Discrete-time logit—robustness with respect to unit of time						
Unit of time is 30 days (baseline)						
$\Delta_{\mathbf{ATEST}}(\mathbf{t,s})$	−0.100***	−0.166***	−0.071**	−0.017	−0.010	−0.046
	(0.017)	(0.036)	(0.028)	(0.062)	(0.034)	(0.061)
Unit of time is 60 days						
$\Delta_{\mathbf{ATEST}}(\mathbf{t,s})$	−0.068***	−0.102***	−0.046**	0.040	−0.007	−0.022
	(0.013)	(0.025)	(0.022)	(0.038)	(0.019)	(0.035)
Unit of time is 15 days						
$\Delta_{\mathbf{ATEST}}(\mathbf{t,s})$	−0.120***	−0.181***	−0.125**	−0.159	−0.024	−0.072
	(0.023)	(0.042)	(0.032)	(0.068)	(0.040)	(0.053)
Continuous-time hazard rate model—robustness with respect to set of covariates						
All covariates						
$\Delta_{\mathbf{ATEST}}(\mathbf{t,s})$	−0.055***	−0.096***	−0.058***	−0.096***	−0.059***	−0.121***
	(0.011)	(0.020)	(0.012)	(0.020)	(0.012)	(0.026)
Limited set of covariates						
$\Delta_{\mathbf{ATEST}}(\mathbf{t,s})$	−0.077***	−0.119***	−0.079***	−0.121***	−0.079***	−0.148***
	(0.012)	(0.020)	(0.012)	(0.020)	(0.013)	(0.025)

Note: Standard errors for the discrete-time model are obtained using 300 subsample replications and for the continuous model using the Delta method. All covariates are *gender*, *age*, *age*^{2} *log(pre-unemployment wage)*, *duration until exhaustion of benefits*, *I(unemployed in July or August)*, *I(age above 50)*, and *I(low skilled)*. The limited set includes *duration until exhaustion of benefits*, *I(age above 50)*, *I(low skilled)*. The discrete-time model uses all covariates.

***Indicates significance at the 1% level.

**Indicates significance at the 5% level.

*Indicates significance at the 10% level.

TABLE 4 Comparing matching estimators for ATEST in the full sample

(s,t) (in months)	**(2, 5)**	**(2, 10)**	**(4, 7)**	**(4, 12)**	**(7, 10)**	**(7, 15)**
Discrete-time logit (baseline)						
$E[Y^*_{0,t}\|S=s,Y_s=0]$	0.126	0.302	0.130	0.260	0.180	0.449
	(0.013)	(0.028)	(0.021)	(0.046)	(0.024)	(0.056)
$E[Y^*_{1,t}(s)\|S=s,Y_s=0]$	0.025	0.137	0.059	0.243	0.170	0.403
	(0.011)	(0.027)	(0.017)	(0.043)	(0.026)	(0.034)
$\Delta_{\mathbf{ATEST}}(\mathbf{t,s})$	−0.100***	−0.166***	−0.071**	−0.017	−0.010	−0.046
	(0.017)	(0.036)	(0.028)	(0.062)	(0.034)	(0.061)
N	2411	2411	1588	1588	771	771
N (treated)	90	90	136	136	87	87
No heterogeneity in exit probabilities						
$E[Y^*_{0,t}\|S=s,Y_s=0]$	0.135	0.341	0.147	0.288	0.192	0.475
	(0.015)	(0.033)	(0.025)	(0.050)	(0.029)	(0.057)
$E[Y^*_{1,t}(s)\|S=s,Y_s=0]$	0.025	0.137	0.059	0.243	0.170	0.403
	(0.011)	(0.027)	(0.017)	(0.043)	(0.026)	(0.034)
$\Delta_{\mathbf{ATEST}}(\mathbf{t,s})$	−0.110***	−0.204***	−0.088***	−0.044	−0.022	−0.072
	(0.019)	(0.040)	(0.031)	(0.065)	(0.035)	(0.062)
N	2411	2411	1588	1588	771	771
N (treated)	90	90	136	136	87	87
Including later entry in treatment						
$E[Y^*_{0,t}\|S=s,Y_s=0]$	0.133	0.334	0.125	0.281	0.201	0.468
	(0.015)	(0.027)	(0.018)	(0.033)	(0.027)	(0.040)
$E[Y^*_{1,t}(s)\|S=s,Y_s=0]$	0.026	0.145	0.059	0.327	0.171	0.440
	(0.011)	(0.028)	(0.018)	(0.050)	(0.027)	(0.037)
$\Delta_{\mathbf{ATEST}}(\mathbf{t,s})$	−0.108***	−0.189***	−0.066***	0.046	−0.030	−0.027
	(0.018)	(0.038)	(0.025)	(0.060)	(0.039)	(0.049)
N	2411	2411	1588	1588	771	771
N (treated)	90	90	136	136	87	87
Excluding later treated from control group						
$E[Y^*_{0,t}\|S=s,Y_s=0]$	0.143	0.606	0.174	0.515	0.253	0.602
	(0.016)	(0.056)	(0.030)	(0.074)	(0.038)	(0.072)
$E[Y^*_{1,t}(s)\|S=s,Y_s=0]$	0.026	0.145	0.059	0.327	0.172	0.446
	(0.011)	(0.028)	(0.018)	(0.046)	(0.027)	(0.037)
$\Delta_{\mathbf{ATEST}}(\mathbf{t,s})$	−0.117***	−0.460***	−0.115***	−0.188**	−0.081	−0.156**
	(0.019)	(0.062)	(0.034)	(0.091)	(0.046)	(0.076)
N	2245	1870	1447	1205	656	591
N (treated)	90	90	136	136	87	87

Note: The unit of time in the discrete-time model is 30 days. Standard errors for the discrete-time model are obtained using 300 subsample replications. All models include covariates *as gender*, *age*, *age*^{2} *log(pre-unemployment wage)*, *duration until exhaustion of benefits*, *I(unemployed in July or August)*, *I(age above 50)*, and *I(low skilled)*.

***Indicates significance at the 1% level.

**Indicates significance at the 5% level.

*Indicates significance at the 10% level.

primary school. Participation in the program may thus distract from more effective job search for teaching jobs, which causes a type of lock-in effect.

A recent literature shows that externally provided job search assistance programs more often have adverse effects on the labor market prospects of participants. Muller et al. (2020) find a negative effect on job finding for an externally provided program in the Netherlands. Krug and Stephan (2016), Behaghel et al. (2014), and Cottier et al. (2015) find similar results for other countries. In our case, the lump-sum payment for program participation does not incentivize the program provider to help program participants to find work. To quantify the negative effect on job finding, we can compare to using the budget for the program for extending the benefits entitlement period. The budget for the program allows to extend the benefits entitlement period by almost 50 weeks, which according to the results of De Groot and Van der Klaauw (2019) causes that Dutch UI recipients find work on average between 29 and 44 days later. The negative impact we find for program participation is at least twice as large.

5.3 | Comparing to other methods

The discrete-time approach requires constructing a control group. In this subsection, we show how estimated treatment effects change when we use related approaches for constructing the control group.

In Table 4, we compare our baseline discrete-time results with those from models with different control groups. The first panel shows our baseline results. The second panel presents the results from Fredriksson and Johansson (2008). The control group in this method omits the terms $\Pr(Y_k = 0|D_{k-1} = 0, Y_s = 0, X = x)$ when estimating the untreated potential outcomes. This induces a bias if transitions into treatment and out of unemployment depend on an overlapping set of covariates as is the case in our setting. The results indicate that omitting endogenous entrance into the job search program during the intervals $[\tau_{s+1}, \tau_{t-1}]$ increases the size of the treatment effects.

In the third panel, we present results following Sianesi's (2004.)definition of the control group, which includes all individuals treated in the intervals $[\tau_{s+1}, \tau_{t-1}]$. Including these individuals will bias the estimates unless there is (on average) no effect of the treatment in this interval. As opposed to the two previous approaches, this approach does not cumulatively build up exit probabilities but estimates them over the entire period $[\tau_{s+1}, \tau_t]$. In general, we find increases in the treated exit probability. This is because the estimation is taken over the entire period $[\tau_{s+1}, \tau_t]$, which means censored observations are excluded, and no information from those individuals is used when estimating exit probabilities. Because censored observations are more likely to be longer spells, ignoring these will produce an upward bias on the estimate of the treated exit probability. Because the exit probabilities are not cumulatively built up, the missing at random assumption on censored observations is also more likely to be violated. These problems also influence the nontreated exit probability. However, the upward bias due to excluding censored observations is counteracted by including the later treated in the control group whose treatment effect is negative. As a result, the nontreated exit probability is similar or smaller than in the second panel.

In the bottom panel, we look at results when excluding from the control group unemployed workers who receive later treatment. As mentioned previously, excluding these individuals will remove longer unemployment spells from the control group. This is evident in the results where we see an upward bias in the nontreated exit probabilities leading to treatment effects far larger in magnitude. This approach also does not generate exit probabilities cumulatively, which results in the same upward bias in exit outcomes as in the third panel.

6 | CONCLUSIONS

In this paper, we used data from a unique institutional setting to evaluate the effectiveness of a job search assistance program for unemployed teachers. The setting allows us to convincingly impose a conditional independence assumption. We use this in the empirical analysis to compare two different methods for dynamic treatment evaluation, a discrete-time method and a continuous-time hazard rate approach. Both methods show negative effects of participation in the job search assistance program on the exit from unemployment. However, the size of the estimated treatment effects sometimes differs between both methods.

We show that the discrete-time approach is sensitive to the choice for the unit of time. A short unit of time causes that few transitions are observed in each time period, which reduces the precision of the estimated treatment effects. A longer unit of time implies that we miss some dynamics that occur within time periods, which induces a bias on the estimated treatment effects. We compare our estimation methods to existing approaches and show that choosing the appropriate control group is important. Our continuous-time approach uses a mixed proportional hazard rate model, which has a restrictive functional form. We find that the estimation results are somewhat sensitive to the set of covariates that is included in the hazard rate.

Recall that in our application, we focus on individuals in the primary education sector collecting UI benefits. Unemployed workers from the primary school sector differ from other unemployed workers, for example, in composition and where they search for new employment. For this group of unemployed workers, we find that participating in the job search assistance program does not stimulate exit from unemployment. However, the job search assistance program is a general program provided by commercial training agencies, and many unemployed workers in the private sector also participate in this program. The poor performance might be the consequence of a mismatch between the program and workers in the primary education sector rather than the program being ineffective in general. For example, the program might press participants to search for work in the general labor market, while unemployed workers in the primary education sector mainly search for teaching jobs at primary schools. An alternative explanation for the poor performance might be the lump-sum costs of participating in this program; that is, the benefits administration pays a fixed amount to the commercial training agencies when assigning a benefits recipient to the program. This creates lower incentives for the commercial agency to ensure that program participants find work than pay-for-performance schemes, which are usually offered by benefits agencies.

As a consequence of the results discussed in this paper, the job search assistance program has been modified in a number of ways. First, after 2 months of unemployment, there is now an introductory meeting in which individuals are informed about the program. The benefits agency indicates that this reduces the resistance to participate in the program. In the new setup, individuals only enter the program after having collected benefits for 8 months. This is later than in the previous setup, and there is no difference anymore for individuals below and above age 50. The previous results indicate that the program effect is less negative if entry is later during the unemployment spell and expenditures also decreased because less individuals actually enter the program. Finally, the job search assistance program now has some voluntary elements, so individuals have some discretion to choose their degree of assistance.

ACKNOWLEDGMENTS

Thanks to Pierre Koning and seminar participants at the University of Tilburg, University of Zurich, University College London, University of Lausanne, Bristol University, Netherlands Bureau of Economic Policy Analyses, University of Essex, EALE/SOLE-meeting in London, the ESWM in Shanghai, the IZA/IFAU conference on Labor Market Policy Evaluation 2010, the editor, and three anonymous reviewers for useful comments. van der Klaauw acknowledges financial support from a Vici grant from the Dutch Science Foundation (NWO).

CONFLICT OF INTEREST

The authors declare that they have no conflicts of interest.

OPEN RESEARCH BADGES

This article has earned an Open Data Badge for making publicly available the digitally-shareable data necessary to reproduce the reported results. The data is available at [http://qed.econ.queensu.ca/jae/datasets/kastoryano001/].

REFERENCES

Abbring, J. H. (2003). Dynamic econometric program evaluation, Discussion Paper 804, IZA Bonn.

Abbring, J. H., & Heckman, J. J. (2007). Econometric evaluation of social programs, part III: Distributional treatment effects, dynamic treatment effects and dynamic discrete choice, and general equilibrium policy evaluation. In J. J. Heckman & E. Leamer (Eds.), *Handbook of Econometrics* (Vol. *6B*) (pp. 5145–5303). Amsterdam: Elsevier Science.

Abbring, J. H., & van den Berg, G. J. (2003). The nonparametric identification of treatment effects in duration models. *Econometrica*, *71*(5), 1491–1517. https://doi.org/10.1111/1468-0262.00456

Behaghel, L., Crépon, B., & Gurgand, M. (2014). Private and public provision of counseling to job seekers: Evidence from a large controlled experiment. *American Economic Journal: Applied Economics*, *6*(4), 142–174.

Black, D., Smith, J., Berger, M., & Noel, B. (2003). Is the threat of reemployment services more effective than the services themselves? Evidence from random assignment in the UI system. *American Economic Review*, *93*(4), 1313–1327. https://doi.org/10.1257/000282803769206313

Cottier, L., Kempeneers, P., Flückiger, Y., & Lalive, R. (2015). Does Outsourcing Job Search Assistance Help Job Seekers Find and Keep Jobs?, IZA Discussion Paper.

Crépon, B., Ferracci, M., Jolivet, G., & van den Berg, G. J. (2018). Information shocks and the empirical evaluation of training programs during unemployment spells. *Journal of Applied Econometrics*, *33*(4), 594–616. https://doi.org/10.1002/jae.2621

de Groot, N., & van der Klaauw, B. (2019). The effects of reducing the entitlement period to unemployment insurance benefits. *Labour Economics*, *57*, 195–208. https://doi.org/10.1016/j.labeco.2019.02.003

Eberheim, C., Ham, J. C., & LaLonde, R. J. (1997). The impact of being offered and receiving classroom training on the employment histories of disadvantaged women. *Review of Economic Studies*, *64*(4), 655–682.

Elbers, C., & Ridder, G. (1982). True and spurious duration dependence: the identifiability of the proportional hazard model. *Review of Economic Studies*, *49*(3), 403–409. https://doi.org/10.2307/2297364

Fredriksson, P., & Johansson, P. (2008). Dynamic treatment assignment: the consequences for evaluations using observational data. *Journal of Business & Economic Statistics*, *26*(4), 435–445. https://doi.org/10.1198/073500108000000033

Gerfin, M., & Lechner, M. (2002). A microeconometric evaluation of the active labour market policy in Switzerland. *Economic Journal*, *112*(482), 854–893. https://doi.org/10.1111/1468-0297.00072

Gill, R. D., & Robins, J. M. (2001). Causal inference for complex longitudinal data: the continuous case. *Annals of Statistics*, *29*(6), 1785–1811.

Ham, J. C., & LaLonde, R. L. (1996). The effect of sample selection and initial conditions in duration models: evidence from experimental data on training. *Econometrica*, *64*(1), 175–205. https://doi.org/10.2307/2171928

Heckman, J. J., LaLonde, R. L., & Smith, J. A. (1999). The economics and econometrics of active labor market programs. In O. Ashenfelter & D. Card (Eds.), *Handbook of Labor Economics* (Vol. *3*) (pp. 1865–2097). Amsterdam: Elsevier Science.

Heckman, J. J., & Navarro, S. (2007). Dynamic discrete choice and dynamic treatment effects. *Journal of Econometrics*, *136*(2), 341–396. https://doi.org/10.1016/j.jeconom.2005.11.002

Imbens, G. W., & Wooldridge, J. M. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature*, *47*(1), 5–86. https://doi.org/10.1257/jel.47.1.5

Kastoryano, S., & van der Klaauw B. (2011). Dynamic Evaluation of Job Search Assistance, Discussion Paper 5424, IZA Bonn.

Katz, L., & Meyer, B. (1990). Unemployment insurance, recall expectations and unemployment outcomes. *Quarterly Journal of Economics*, *105*(4), 973–1002. https://doi.org/10.2307/2937881

Krug, G., & Stephan, G. (2016). Private and public placement services for hard-toplace unemployed—An evaluation of a randomized field experiment using register data. *Industrial and Labor Relations Review*, *69*(2), 471–500. https://doi.org/10.1177/0019793915615334

Lalive, R., van Ours, J. C., & Zweimüller, J. (2008). The impact of active labour market programmes on the duration of unemployment in Switzerland. *Economic Journal*, *118*(525), 235–257. https://doi.org/10.1111/j.1468-0297.2007.02111.x

LaLonde, R. J. (1986). Evaluating the econometric evaluations of training programs with experimental data. *American Economic Review*, *76*(4), 604–620.

Lechner, M. (2009). Sequential causal models for the evaluation of labor market programs. *Journal of Business and Economic Statistics*, *27*(1), 71–83. https://doi.org/10.1198/jbes.2009.0006

Lechner, M., & Miquel, R. (2010). Identification of the effects of dynamic treatment treatments by sequential conditional independence assumptions. *Empirical Economics*, *39*(1), 111–137. https://doi.org/10.1007/s00181-009-0297-3

Lechner, M., & Wunsch, C. (2009). Are training programs more effective when unemployment is high? *Journal of Labor Economics*, *27*(4), 653–692. https://doi.org/10.1086/644976

Lok, J., Gill, R., van der Vaart, A., & Robins, J. (2004). Estimating the causal effect of a time-varying treatment on time-to-event using structural nested failure time models. *Statictica Neerlandica*, *58*(3), 271–295. https://doi.org/10.1111/j.1467-9574.2004.00123.x

Lok, J. J, Hernán, M. A., & Robins, J. M. (2007). Optimal start of treatment based on time-dependent covariates, JSM Proceedings 1149-1160.

Moffitt, R. (1985). Unemployment insurance and the distribution of unemployment spells. *Journal of Econometrics*, *28*(1), 85–101. https://doi.org/10.1016/0304-4076(85)90068-5

Muller, P., van der Klaauw, B., & Heyma, A. (2020). Empirically evaluate activation programs for job seekers. *Journal of Applied Econometrics*, *35*(5), 526–547. https://doi.org/10.1002/jae.2763

OECD. (2010). *Employment Outlook*. Paris: OECD.

Politis, N., & Romano, J. (1994). Large sample confidence regions based on subsamples under minimal assumptions. *Annals of Statistics*, *22*, 2031–2050.

Robins, J. M. (1986). A new approach to causal inference in mortality studies with sustained exposure periods – application to control of healthy worker survivor effect. *Mathematical Modelling*, *7*(9–12), 1393–1512. https://doi.org/10.1016/0270-0255(86)90088-6

Robins, J. M. (1997). Causal inference from complex longitudinal data. In M. Berkane (Ed.), *Latent Variable Modelling and Applications to Causality. Lecture Notes in Statistics (120)* (pp. 69–117). New York: Springer-Verlag.

Sianesi, B. (2004). An evaluation of the Swedish system of active labor market programs in the 1990's. *Review of Economics and Statistics*, *86*(1), 133–155. https://doi.org/10.1162/003465304323023723

Vikström, J. (2017). Dynamic treatment assignment and evaluation of active labor market policies. *Labour Economics*, *49*, 42–54. https://doi.org/10.1016/j.labeco.2017.09.003

Young, J. G., Cain, L. E., Robins, J. M., O'Reilly, E. J., & Hernán, M. A. (2011). Comparative effectiveness of dynamic treatment regimes: An application of the parametric g-formula. *Statistics in Biosciences*, *3*(1), 119–143. https://doi.org/10.1007/s12561-011-9040-7

SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

How to cite this article: Kastoryano, S., and van der Klaauw, B. (2022). Dynamic evaluation of job search assistance. *Journal of Applied Econometrics*, *37*(2), 227-241. https://doi.org/10.1002/jae.2866