Evaluating Program Sequences with Double Machine Learning: An Application to Labor Market Policies

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Abstract

Many programs evaluated in empirical research incorporate a sequential structure, where individuals may be assigned to various programs over time. While this aspect is often ignored in favor of analyzing interventions at a single point in time, this paper reviews, explains, and applies methods for program evaluation within a sequential framework. We outline the necessary assumptions required to identify treatment effects under dynamic confounding and show how dynamic policies can be exploited to construct and assess counterfactuals of high practical relevance. Additionally, recently developed methods for estimating effects across multiple treatments and time periods are explored, utilizing Double Machine Learning (DML), a flexible estimator that avoids parametric assumptions while preserving desirable statistical properties. Using Swiss administrative data, the methods are demonstrated through an empirical application assessing the participation of unemployed individuals in active labor market policies, where assignment decisions by caseworkers can be reconsidered between two periods. The analysis identifies a temporary wage subsidy program as the most effective intervention, on average, even after adjusting for its extended duration compared to other programs. Overall, DML-based analysis of dynamic policies proves to be a useful approach within the program evaluation toolkit.

JEL classification: C14, C21, J68

Keywords: Causal machine learning, Dynamic treatment effects, Active labor market policies

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1 Introduction

Program evaluations based on observational data are used by economists to assess the impact of policy measures such as training programs, public transfer schemes, and healthcare interventions (Abadie & Cattaneo, 2018). The common approach in the literature compares an outcome of choice between a treatment group of program participants and a control group of individuals that never participated in a program. If all confounding variables jointly influencing the outcome and program assignment are observed, the causal effect of the intervention can be identified. The described approach considerably simplifies the complex process of program assignment, participation, and completion observed in non-experimental settings into a single treatment state, limiting the ability to address many important questions. For example, program duration may vary between participants, which could have a strong impact on program efficiency. Furthermore, individuals may participate not only in one but in several successive measures, which makes it difficult to match them to one particular program type. Importantly, a specific program sequence may result from reassignment following the initial placement. For instance, a program may be shortened if an individual is no longer eligible, or the program may be switched if the initial program is deemed unsuccessful. After all, a particular program sequence may be effective for one person but not for another, which cannot be distinguished when only analyzing average effects across the entire population.

This paper addresses the previously mentioned challenges by adopting a framework to evaluate program sequences instead of single time-point interventions. The framework is based on ideas from biostatistics and epidemiology, originally formulated by Robins (1986, 1987) and further developed in subsequent research, as reviewed in Richardson & Rotnitzky (2014). While these methods have frequently been applied to the analysis of sequences of medical treatments (e.g. Hernán, Brumback, & Robins, 2002; Taubman, Robins, Mittleman, & Hernán, 2009; Young, Cain, Robins, O'Reilly, & Hernán, 2011, and many more), they have so far seen limited adoption in the econometric program evaluation literature. Notable exceptions include Lechner (2009) and Lechner & Wiehler (2013). Wider adoption has likely been hindered by the stricter identification and estimation requirements in the sequential setting compared to one-time interventions. A key contribution of this paper is to demonstrate that these concerns can be addressed by (1) designing dynamic policies that facilitate the credible identification of counterfactual treatment sequences using observational data and (2) leveraging recently proposed machine learning-based estimation strategies to flexibly estimate these quantities. These innovations enhance the applicability of sequential analysis for assessing policy measures commonly studied in economics, ensuring a better reflection of their sequential nature.

The key challenge in analyzing program sequences from observational data is that the complete trajectory of treatments is not predetermined prior to the start of the sequence. In particular, the program assignment mechanism underlying the observed data might be affected by time-varying feedback between treatments, covariates and outcomes. Hence, to identify the causal effect, it is essential to consider that sequential treatment assignment is a decision process potentially influenced by *dynamic confounding*. For example, when evaluating training courses for unemployed individuals, dynamic confounding may arise when observed program assignments result from caseworkers updating their decisions based on the outcomes of preceding programs. Due to the dynamic nature of the confounding, controlling for pre-treatment information only often proves inadequate when assessing the impacts of treatment sequences. Therefore, existing research has developed alternative identification strategies that rely on modified identification assumptions and also imply new challenges for effect estimation (Robins & Hernán, 2008).

Besides addressing dynamic confounding, dynamics also need to be considered in the design of counterfactual scenarios. In the sequential setting, pre-specified counterfactuals, such as "two consecutive periods of a training course," are often of limited relevance because they fail to account for the possibility of dynamic decision-making (Wager, 2024). For instance, estimating the counterfactual outcome for such a sequence requires considering a scenario in which individuals remain in the program for the entire sequence, even if they lose eligibility for participation along the way, a situation frequently encountered in the evaluation of policy interventions. In this paper, we show that more appropriate estimands can be defined by adopting a *dynamic policy* evaluation framework. In this framework, counterfactuals may depend on time-varying covariates, meaning that individuals following the same dynamic policy may end up in different program sequences. This allows to construct counterfactuals that align more closely with the assumed underlying assignment process and, consequently, enable the design of more realistic scenarios. For example, defining a dynamic policy allows the evaluation of continuing a program for a second period, specifically for individuals remaining eligible during the first period. An overview of the possible scenarios, along with accompanying examples, is given in Table 1.

Dynamic policies are commonly used in research on optimal dynamic treatment regimes (Murphy, 2003; Zhang, Tsiatis, Laber, & Davidian, 2013; Sakaguchi, 2024). This body of literature develops procedures to select an optimal dynamic policy from a class of feasible policies, with the

Table 1: The possible dynamic scenarios with accompanying examples.

		Assumptions about the underlying assignment mechanism: Feedback between treatments, covariates, and outcomes over time? NO	YES
Counterfactual of interest: Treatment assignment based on time-varying characteristics?	*NO*	**Static policy under static confounding:** *E.g.,* effect of two-period training course estimated from data with fixed initial treatment assignment.	**Static policy under dynamic confounding:** *E.g.,* effect of two-period training course estimated from data with time-varying treatment assignment.
	YES	**Dynamic policy under static confounding:** *E.g.,* effect of training course, extending to period 2 only if eligible throughout period 1, estimated from data with fixed initial treatment assignment.	**Dynamic policy under dynamic confounding:** *E.g.,* effect of training course, extending to period 2 only if eligible throughout period 1, estimated from data with time-varying treatment assignment.

Note: Overview of the possible dynamic scenarios. Depending on the assumptions about the underlying assignment mechanism and the counterfactual of interest, different estimands may be analyzed.

objective of maximizing the mean response in the population. Such approaches allow to provide individualized treatment recommendations but require strong identification assumptions which are often implausible in non-experimental settings. In contrast, the present work adopts an alternative objective. Rather than optimizing over an extensive policy class, we propose to selectively isolate specific policies that are both identifiable from observable data and of substantial policy relevance. Accordingly, instead of focusing on individualized treatment recommendations, we leverage dynamic policies as a framework to uncover aggregate and group-level effects that enable robust estimation and inference.

A major challenge for statistical inference in sequential settings is that the number of possible program sequences expands exponentially with the number of periods, leading to data sparsity for individual sequences. For example, a setting with five treatments and five time periods already yields $5^5 = 3,125$ possible treatment sequences, and ensuring sufficient observations for each becomes increasingly difficult. Hence, while nonparametric identification of dynamic policies is achievable, conventional estimation approaches, such as structural nested mean models (Robins, 1989, 1994) or marginal structural models (Robins, Hernan, & Brumback, 2000), rely on structural assumptions to extrapolate into regions with limited data support.[1] However, if a fair number of observations is available for each program sequence of interest, more flexible, machine learning-based estimators can be employed. Aggregating information across periods and/or treatments can enhance the feasibility of this approach, especially when credible informa-

[1]For an overview of these methods, see Appendix A.1

tion about the exact underlying data-generating process is lacking. Building on this approach, the present paper demonstrates the potential of recently proposed double machine learning (DML) methods (Bodory, Huber, & Lafférs, 2022; Bradic, Ji, & Zhang, 2024) for analyzing dynamic policies. The key advantages of DML methods are that they do not require parametric assumptions and that they can handle a potentially large covariate space while maintaining desirable statistical properties.

In the empirical application, we analyze which implementation aspects are most beneficial for individuals selected to participate in active labor market policies (ALMP). These interventions are designed to improve employment outcomes for unemployed individuals and are widely used by governments as policy tools.[2] In Switzerland, which is the focus of this paper, two-thirds of unemployed are assigned to these measures within the first twelve months of their unemployment period, with approximately 60% of them participating in more than one program. To the best of our knowledge, our application is the first to jointly address dynamic confounding and dynamic policies in evaluating ALMP sequences, whereas prior work (Lechner & Wiehler, 2013) addressed only the former. Specifically, we show that if a dynamic policy is designed to depend solely on intermediate outcomes, rather than other time-varying confounders, an estimand of greater practical relevance can be identified with only minor adjustments to identification assumptions. Furthermore, the application innovates by applying flexible dynamic DML estimation in the context of ALMP, offering more reliable effect estimates compared to prior sequential studies that relied on parametric estimators. Leveraging DML in combination with a large dataset containing extensive intermediate details about the unemployed also allows, for the first time, to assess group-level effect heterogeneity in a sequential ALMP evaluation. Our analysis shows that a particular temporary wage subsidy program is most effective on average, even after aligning program duration across programs. Moreover, first-time unemployed and individuals with limited language skills profit more from this program in comparison to extended training courses. The findings emphasize the practical value of employing DML-based estimation for the empirical evaluation of programs sequences, allowing policymakers to develop more nuanced and targeted policies.

The remainder of the paper is structured as follows: In the following Section we introduce the notation, the estimands of interest and their identification in the sequential setting. In Section 3, we present different estimators for DML under static and dynamic confounding and discuss their

[2] For an overview, refer to reviews and meta-studies by Card, Kluve, & Weber (2010, 2018); Crépon & Van Den Berg (2016); Vooren, Haelermans, Groot, & Maassen van den Brink (2019)

properties. In Section 4 we apply these estimators to our empirical application, before concluding in Section 5. Further derivations and additional results are provided in the Appendix.

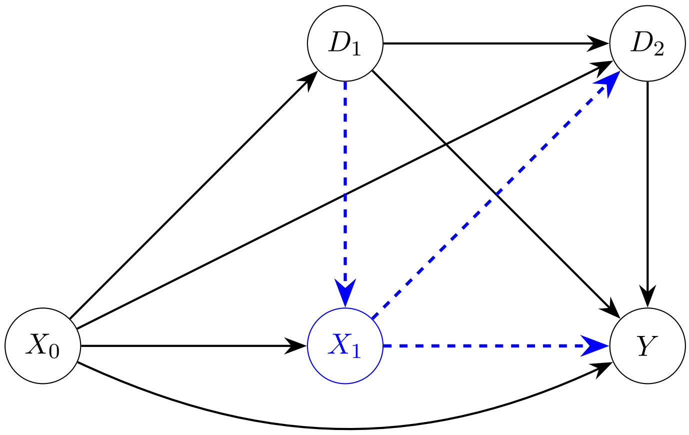
2 Identifying Effects of Sequential Policies

2.1 Notation and Problem Setup

This section introduces the framework for assessing sequential policies, drawing primarily on Hernán & Robins (2020), while employing notation more commonly used in econometrics. The framework is presented in a setting with $T = 2$ time periods, as used in the application in Section 4. To enhance clarity, we avoid using a general T, but the framework can be directly extended to settings with $T > 2$. We also note that for $T = 1$, the framework collapses to the standard setting of single time-point interventions. Let $\mathcal{D}_t = \{0, 1, ..., M_t\}$ denote the set of existing programs in period t and D_{it} a discrete random variable indicating the observed program of individual $i \in \{1, ..., N\}$ in period $t \in \{0, 1, 2\}$, where $t = 0$ denotes the pre-treatment period such that $D_{i0} = 0$ for all observations. At each point in time, we observe a collection of covariates $X_{it} \in \mathcal{X}_t$ which may contain (intermediate) outcomes $Y_{it} \in \mathcal{Y}_t$. The main outcome of interest is observed after the final treatment $Y_i := Y_{iT} = Y_{i2}$, where we drop the time subscript for the sake of readability. This variable may include outcome information from any period following the start of the final treatment. In general, capital letters (except T, M and N) denote random variables, lower case letters denote their realizations and boldface letters denote vectors of variable histories up to t, i.e. $\mathbf{X}_{it} = (X_{i0}, ..., X_{it})$ and $\mathbf{d}_t = (d_1, ..., d_t)$ such that for example $\mathbf{d}_1 = d_1$ and $\mathbf{d}_2 = (d_1, d_2)$. To simplify the notation further, we omit the individual identifier i when not explicitly needed and collect all random variables for which realizations can be observed in the vector $\mathbf{W} = (Y, \mathbf{D}_2, \mathbf{X}_1)$.

The causal diagram (Pearl, 1995) in Figure 1 illustrates our setup. In the initial period $t = 0$, we observe the pre-treatment covariates X_0. These variables may have a causal effect (arrow) on any future treatment, intermediate covariate, or outcome. In period $t = 1$, individuals are treated with D_1, which may impact their time-varying characteristics in the current and subsequent periods as well as any future treatment assignments. The same happens in period $t = 2$. The described setting is referred to as the sequential treatment effect model with dynamic confounding. In this model, the initial treatment assignment D_1 can induce changes in the covariates X_1, which subsequently may influence the second treatment D_2 and the outcome Y, as highlighted by the blue dashed arrows. If this feedback effect is absent (i.e., if one of the blue dashed arrows is removed from the graph) the setting corresponds to the sequential framework with static confounding, meaning that program assignment in all periods is predetermined conditional on covariates.[3]

Figure 1: Causal pathways in the sequential treatment effect model with two time periods



Notes: The arrows in the diagram illustrate the allowed causal pathways between the variables in the sequential model with dynamic confounding. At any point in time t, covariates X_t and treatments D_t may influence any future treatment, covariate, or outcome. If one of the blue dashed arrows is deleted from the figure, confounding is static, meaning that the treatment sequence is predetermined conditional on covariates.

To study causal effects, we make use of the potential outcome framework of Rubin (1974) and denote the hypothetical outcome under a particular treatment sequence $\mathbf{d}_2 \in \mathcal{D}_1 \times \mathcal{D}_2$ as $Y^{\mathbf{d}_2}$. Similarly, we define potential values of the time-varying covariates as $X_1^{d_1}$. The relationship between potential and observed variables follows the standard observation rule, where an observed variable is assumed to correspond to the potential variable associated with the assigned program sequence.

Assumption 2.1. *Stable Unit Treatment Value Assumption (SUTVA)*

[a] $Y = \sum_{\mathbf{d}_2 \in \mathcal{D}_1 \times \mathcal{D}_2} \mathbf{1}\{\mathbf{D}_2 = \mathbf{d}_2\} Y^{\mathbf{d}_2}$

[b] $X_1 = \sum_{d_1 \in \mathcal{D}_1} \mathbf{1}\{D_1 = d_1\} X_1^{d_1}$

This assumption implicitly requires that there are no unrepresented programs in the population of interest (everyone is assigned to a particular program sequence $\mathbf{d}_2$) and that there are no relevant interactions between individuals, meaning that the program of one individual does not

[3]In the absence of a causal pathway from D_1 to X_1, the variable X_1 can also considered to be a pre-treatment variable.

affect the final outcome and intermediate covariates of another individual.

Given potential outcomes, we can now formally define dynamic confounding:

Definition 2.1. *A sequential program assignment mechanism exhibits confounding if* $\mathbb{E}[Y^{\mathbf{d}_2}] \neq \mathbb{E}[Y|\mathbf{D}_2 = \mathbf{d}_2]$. *It is characterized by static confounding if* $\mathbb{E}[Y^{\mathbf{d}_2}|X_0] = \mathbb{E}[Y|\mathbf{D}_2 = \mathbf{d}_2, X_0]$ *and by dynamic confounding if* $\mathbb{E}[Y^{\mathbf{d}_2}|X_0] \neq \mathbb{E}[Y|\mathbf{D}_2 = \mathbf{d}_2, X_0]$ *(Hernán & Robins, 2020).*

Hence, confounding is static if the expected outcome of those treated with $\mathbf{D}_2 = \mathbf{d}_2$ equals the expected outcome if everyone were treated with $\mathbf{d}_2$, conditional on pre-treatment information. This implies that the entire treatment sequence is pre-determined given X_0. Conversely, if there is feedback between treatments, covariates, and outcomes across periods, this equality will generally not hold, and the confounding is considered dynamic.

As discussed in the introduction, the objective is to construct counterfactual scenarios that facilitate dynamic decision-making. This is accomplished by introducing the concept of dynamic policies[4] (Murphy, 2003). Let $\mathcal{R}_t \subseteq \mathcal{D}_t$ denote a subset of the possible treatments in period t. Furthermore, let $V_t \in \mathcal{V}_t$ denote a vector of random variables consisting of a subset of the covariates X_t. Based on these variables, referred to as decision variables, a dynamic policy can formally be defined as follows:

Definition 2.2. *A dynamic policy is a deterministic function* $g_t : \mathcal{V}_0 \times \ldots \times \mathcal{V}_{t-1} \rightarrow \mathcal{R}_t$ *that determines a treatment* $g_t(\mathbf{V}_{t-1}) \in \mathcal{R}_t$ *based on the history of decision variables up to time* $t-1$. *The dynamic policies in both periods are collected in a vector* $\mathbf{g}_2(\mathbf{V}_1) = (g_1(V_0), g_2(\mathbf{V}_1))$. *The potential decision variables and potential outcomes associated with a policy* $\mathbf{g}_2$ *are defined as*

$$V_1^{g_1} := \sum_{d_1 \in \mathcal{D}_1} \mathbf{1}\{g_1(V_0) = d_1\} V_1^{d_1} \quad \textit{and}$$
$$Y^{\mathbf{g}_2} := \sum_{\mathbf{d}_2 \in \mathcal{D}_1 \times \mathcal{D}_2} \mathbf{1}\{\mathbf{g}_2(\mathbf{V}_1^{d_1}) = \mathbf{d}_2\} Y^{\mathbf{d}_2} \quad \textit{with} \quad \mathbf{V}_1^{d_1} = (V_0, V_1^{d_1}).$$

The policy $\mathbf{g}_2$ is defined as a function from the decision variables to a subset of the possible treatment states. In the most flexible setting, decision variables can include all covariates, i.e. $V_t = X_t$, and the policy can return any possible program, i.e. $\mathcal{R}_t = \mathcal{D}_t$. Often, however, it is useful to align V_t and $\mathcal{R}_t$ with the requirements of the counterfactual scenario under consideration. For instance, in the subsequent application we will define V_t in a way to ensure that policies assigning

[4]Policies are also called treatment rules or regimes. Here we follow Wager (2024) and call them policies as this is the standard terminology in the econometrics literature.

ALMP can depend on the evolution of the outcome.

Example 2.1. *A dynamic policy of interest might be specified as: "Assign to program d in the first period. Continue program d in the second period if the potential intermediate outcome Y_1^d equals zero, otherwise assign program d'." This policy can be formaized as*

$$g_1(Y_0) = d \quad \textit{and} \quad g_2(\mathbf{Y}_1^d) = d \cdot \mathbf{1}\{Y_1^d = 0\} + d' \cdot \mathbf{1}\{Y_1^d \neq 0\},$$

where the potential intermediate outcome is the only decision variable and the range of the policy in the first and second period equals $\mathcal{R}_1 = \{d\}$ and $\mathcal{R}_2 = \{d, d'\}$, respectively.

A key property of dynamic policies is that two individuals following the same policy $\mathbf{g}_2$ may follow different program sequences $\mathbf{d}_2$ depending on their covariates. For instance, if in the example Y_t represented a binary indicator for employment, then both individuals unemployed in the first period with the sequence (d, d) and individuals employed in the first period with the sequence (d, d') would comply with the policy. A sequence $\mathbf{d}_2$ constitutes a specific instance of a policy, where $\mathbf{g}_2$ is expressed as a constant function. This type of policy is referred to as a *static policy*. In what follows, $\mathbf{d}_2$ is used to represent static policies exclusively, whereas $\mathbf{g}_2$ is used in contexts that encompass both static and dynamic policies. Finally, note that in our framework, dynamic policies do not take treatment assignments from previous periods as inputs, as they are deterministically determined by the history of decision variables.

2.2 Estimation Targets

Our analysis aims at evaluating effects of sequential policies at different levels of granularity. At the broadest level of the entire population, the primary target parameter is the average potential outcome (APO) under a particular policy, defined as

$$\theta^{\mathbf{g}_2} := \mathbb{E}[Y^{\mathbf{g}_2}].$$

This parameter represents the mean outcome if all individuals were assigned according to the policy $\mathbf{g}_2$ (or according to the sequence $\mathbf{d}_2$ if the policy is static). To assess heterogeneity, the focus can be switched to a specific subgroup of interest, for example to individuals that participated in a labor market program during a previous unemployment spell. This can be studied by examining the group average potential outcome (GAPO)

$$\theta^{\mathbf{g}_2}(z_0) := \mathbb{E}[Y^{\mathbf{g}_2}|Z_0 = z_0],$$

where Z_0 is a column or deterministic function of X_0 with low cardinality.[5] The following discussion of identification and estimation will focus on the parameters $\theta^{\mathbf{g}_2}$ and $\theta^{\mathbf{g}_2}(z_0)$, respectively. The average treatment effect (ATE) between implementing policy $\mathbf{g}_2$ and alternative policy $\mathbf{g}_2'$ is obtained subsequently by taking the difference between two average potential outcomes, i.e.

$$\tau^{\mathbf{g}_2,\mathbf{g}_2'} := \mathbb{E}[Y^{\mathbf{g}_2} - Y^{\mathbf{g}_2'}] = \theta^{\mathbf{g}_2} - \theta^{\mathbf{g}_2'} \quad \text{with} \quad \mathbf{g}_2 \neq \mathbf{g}_2',$$

which directly follows from the linearity of the expectation operator. Subgroup-specific average treatment effects (GATE) are similarly obtained as

$$\tau^{\mathbf{g}_2,\mathbf{g}_2'}(z_0) = \mathbb{E}[Y^{\mathbf{g}_2} - Y^{\mathbf{g}_2'}|Z_0 = z_0] = \theta^{\mathbf{g}_2}(z_0) - \theta^{\mathbf{g}_2'}(z_0) \quad \text{with} \quad \mathbf{g}_2 \neq \mathbf{g}_2'.$$

Heterogeneous treatment effects of this type are also referred to as conditional average treatment effects (CATE) in the literature. Here we follow Lechner (2018) and denote them as GATE to emphasize that we focus on large discrete subgroups of the population rather than granular individualized effects.[6]

2.3 Identification of Static Policies

The prior section defined the estimands of interest using potential outcomes. However, since each individual is observed in only one particular program sequence, the remaining potential outcomes are unobservable, necessitating additional assumptions for their identification. The assumptions required vary based on the type of confounding and the nature of the policy of interest. We begin by revisiting the identification assumptions for static policies, before turning to the framework of dynamic policies, which has thus far seen limited adoption in economics.

In the presence of static confounding, the APO of a static policy $\mathbf{g}_2 = \mathbf{d}_2$ can be identified if the following conditional independence assumption (CIA) and overlap assumption is satisfied:

[5] We focus on subgroups defined by pre-treatment covariates rather than time-varying covariates, as estimands based on the latter are not identifiable under the assumptions outlined below.

[6] More granular individualized effects are not considered because DML-based estimators of such effects lack statistical guarantees and perform poorly in finite samples under confounding, as shown for example by Lechner & Mareckova (2024) for single time-point interventions. The development of robust estimation methods for individualized effects in the sequential setting remains an open question for future research beyond the scope of this study.

Assumption 2.2. *Identification of static policies under static confounding.*

[a] CIA $\quad t = 1: \quad Y^{\mathbf{d}_2} \perp D_1 | X_0 = x_0 \; \forall \; x_0 \in \mathcal{X}_0$

$\quad t = 2: \quad Y^{\mathbf{d}_2} \perp D_2 | X_0 = x_0, D_1 = d_1 \; \forall \; x_0 \in \mathcal{X}_0$

[b] Overlap $\quad t = 1: \quad p_{d_1}(x_0) := \Pr(D_1 = d_1 | X_0 = x_0) > 0 \; \forall \; x_0 \in \mathcal{X}_0$

$\quad t = 2: \quad p_{d_2}(x_0) := \Pr(D_2 = d_2 | X_0 = x_0, D_1 = d_1) > 0 \; \forall \; x_0 \in \mathcal{X}_0$

Assumption 2.2[a] is called full conditional independence assumption in Lechner & Miquel (2001). It requires that potential outcomes are independent of program assignment in the first period for given values of the pre-treatment covariates. Thus, X_0 must include all variables that influence both the first period program assignment and the outcomes simultaneously. Additionally, the assumption asserts that allocation to a program in the second period is random, conditional on pre-treatment covariates and prior program participation. This implies that intermediate characteristics X_1 are either unaffected by previous programs or do not simultaneously influence both program assignment in the second period and potential outcomes. Furthermore, Assumption 2.2[b] requires that for a given history of previous program participation and any realization of pre-treatment characteristics, it must be possible to observe individuals with a program $D_t = d_t$. Otherwise it would be impossible to construct appropriate counterfactuals. Lechner & Miquel (2001) show that Assumption 2.2 can be re-written using basic probability theory:

Assumption 2.3. *Identification of static policies under static confounding (alternative formulation).*

[a] CIA: $\quad Y^{\mathbf{d}_2} \perp \mathbf{D}_2 | X_0 = x_0 \; \forall \; x_0 \in \mathcal{X}_0$

[b] Overlap: $\quad p_{\mathbf{d}_2}(x_0) := \Pr(\mathbf{D}_2 = \mathbf{d}_2 | X_0 = x_0) > 0 \; \forall \; x_0 \in \mathcal{X}_0$

This formulation shows that the sequential framework with static confounding is equivalent to the static single-period setting for multiple treatments (Imbens, 2000; Lechner, 2001), where sequence $\mathbf{d}_2$ is considered a single treatment state, and identification is achieved by conditioning on pre-treatment covariates only. It is simple to show that the conditional APO is identified as

$$\mathbb{E}[Y^{\mathbf{d}_2} | X_0] = \mathbb{E}[Y^{\mathbf{d}_2} | X_0, \mathbf{D}_2 = \mathbf{d}_2] = \mathbb{E}[Y | X_0, \mathbf{D}_2 = \mathbf{d}_2] =: \mu_{\mathbf{d}_2}(X_0), \tag{2.1}$$

where the first equality uses Assumption 2.2 or 2.3 and the second equality uses Assumption 2.1.

Given this result, the parameters of interest are derived using the law of iterated expectations as

$$\theta^{\mathbf{d}_2} = \mathbb{E}[Y^{\mathbf{d}_2}] = \mathbb{E}_{X_0}[\mathbb{E}[Y^{\mathbf{d}_2}|X_0]] = \mathbb{E}_{X_0}[\mu_{\mathbf{d}_2}(X_0)] \quad \text{and}$$
$$\theta^{\mathbf{d}_2}(z_0) = \mathbb{E}[Y^{\mathbf{d}_2}|Z_0 = z_0] = \mathbb{E}_{X_0}[\mathbb{E}[Y^{\mathbf{d}_2}|X_0]|Z_0 = z_0] = \mathbb{E}_{X_0}[\mu_{\mathbf{d}_2}(X_0)|Z_0 = z_0]. \tag{2.2}$$

This is the standard identification argument for single time-point interventions as seen e.g. in Rosenbaum & Rubin (1983).[7]

In observational settings, static confounding may be implausible if the assignment process generating the data depends on time-varying information. Causal effects under dynamic confounding have first been studied in epidemiology and biostatistics starting with the seminal work by Robins (1986, 1987). He demonstrated that under feedback effects between treatments and covariates, standard approaches do not allow for a causal comparison of treatment sequences, even when all pre-treatment confounding factors are controlled for. Instead, using a graphical model, Robins (1986) came up with the idea of what is called today a sequential randomized experiment and the sequential randomization assumption (Richardson & Rotnitzky, 2014). For static policies, these ideas have been introduced to econometrics by Lechner (2009) and Lechner & Miquel (2010) who termed the requirement for identification as weak dynamic conditional independence:

Assumption 2.4. *Identification of static policies under dynamic confounding.*

[a] CIA $\quad t = 1: \quad Y^{\mathbf{d}_2} \perp D_1 | X_0 = x_0 \; \forall \, x_0 \in \mathcal{X}_0$

$\quad t = 2: \quad Y^{\mathbf{d}_2} \perp D_2 | \mathbf{X}_1 = \mathbf{x}_1, D_1 = d_1 \; \forall \, \mathbf{x}_1 \in \mathcal{X}_0 \times \mathcal{X}_1$

[b] Overlap $\quad t = 1: \quad p_{d_1}(x_0) := \Pr(D_1 = d_1 | X_0 = x_0) > 0 \; \forall \, x_0 \in \mathcal{X}_0$

$\quad t = 2: \quad p_{d_2}(d_1, \mathbf{x}_1) := \Pr(D_2 = d_2 | \mathbf{X}_1 = \mathbf{x}_1, D_1 = d_1) > 0 \; \forall \, \mathbf{x}_1 \in \mathcal{X}_0 \times \mathcal{X}_1$

For $t = 1$, Assumption 2.4 is equivalent to Assumption 2.2. However, in the second period, conditioning on pre-treatment characteristics X_0 and the treatment history no longer suffices to establish independence between potential outcomes and treatment state D_2. Instead, Assumption 2.4[a] requires conditioning on the whole covariate history $\mathbf{X}_1$, which may be influenced by previous treatments. Hence, we need additional information about time-varying covariates that are expected to determine dynamic program selection. Importantly, these covariates may not be

[7]While we focus on constant static policies that are defined as a fixed program sequences $\mathbf{d}_2$, the results can be generalized to static policies $h_t : \mathcal{V}_0 \to \mathcal{D}_t$ that depend on pre-treatment decision variables V_0 but do not depend on time-varying information. Specifically, $E[Y^{\mathbf{h}_2}]$ is identified under the condition that Assumption 2.2 or 2.3 holds for all $\mathbf{d}_2$ within the range of policies $\mathbf{h}_2(V_0)$. Similar identification results are commonly applied in optimal policy learning in the single-period setting (e.g. Athey & Wager, 2021).

influenced by programs in future periods in a way that is related to the outcome variable.[8] The overlap assumption 2.4[b] also requires additional conditioning on time-varying covariates for the treatment probability in the second period.

Under Assumption 2.4, the average potential outcome under a static program sequence, conditional on all information known at the end of the first period, is identified as

$$\mathbb{E}[Y^{\mathbf{d}_2}|\mathbf{X}_1, D_1 = d_1] = \mathbb{E}[Y^{\mathbf{d}_2}|\mathbf{X}_1, \mathbf{D}_2 = \mathbf{d}_2] = \mathbb{E}[Y|\mathbf{X}_1, \mathbf{D}_2 = \mathbf{d}_2] =: \mu_{\mathbf{d}_2}(\mathbf{X}_1), \tag{2.3}$$

where the first equality uses Assumption 2.4 and the second equality uses Assumption 2.1. However, due to potential feedback between the treatments through X_1, we cannot simply average the conditional outcomes $\mu_{\mathbf{d}_2}(\mathbf{X}_1)$ over the population to determine the APO. Instead, we need an extra averaging step

$$\begin{aligned}\mathbb{E}[Y^{\mathbf{d}_2}|X_0] &= \mathbb{E}[Y^{\mathbf{d}_2}|X_0, D_1 = d_1]\\ &= \mathbb{E}_{X_1}\big[\mathbb{E}[Y^{\mathbf{d}_2}|\mathbf{X}_1, D_1 = d_1]\big|X_0, D_1 = d_1\big]\\ &= \mathbb{E}_{X_1}\big[\mu_{\mathbf{d}_2}(\mathbf{X}_1)\big|X_0, D_1 = d_1\big]\\ &=: \nu_{\mathbf{d}_2}(X_0), \end{aligned}\tag{2.4}$$

where the first equality again uses Assumption 2.4, the second equality is obtained using the law of iterated expectations and the third equality plugs in equation (2.3). Hence, averaging $\mu_{\mathbf{d}_2}(\mathbf{X}_1)$ over the distribution of covariates X_1 conditional on the baseline covariates X_0 and treatment $D_1 = d_1$, we obtain the average potential outcome conditional on pre-treatment information only. Based on result (2.4), the parameters of interest are identified using the law of iterated expectations as

$$\begin{aligned}\theta^{\mathbf{d}_2} &= \mathbb{E}[Y^{\mathbf{d}_2}] = \mathbb{E}_{X_0}[\mathbb{E}[Y^{\mathbf{d}_2}|X_0]] = \mathbb{E}_{X_0}[\nu_{\mathbf{d}_2}(X_0)] \quad \text{and}\\ \theta^{\mathbf{d}_2}(z_0) &= \mathbb{E}[Y^{\mathbf{d}_2}|Z_0 = z_0] = \mathbb{E}_{X_0}[\mathbb{E}[Y^{\mathbf{d}_2}|X_0]|Z_0 = z_0] = \mathbb{E}_{X_0}[\nu_{\mathbf{d}_2}(X_0)|Z_0 = z_0].\end{aligned}\tag{2.5}$$

Thus, the identification result for dynamic confounding is analogous to the case of static confounding, with $\mu_{\mathbf{d}_2}(X_0)$ replaced by $\nu_{\mathbf{d}_2}(X_0)$.[9] Identification through equation (2.4) is known in

[8]This exogeneity requirement for the conditional independence assumption can be explicitly stated as $X_0^{\mathbf{d}_2} = X_0^{\mathbf{d}'_2}$ and $X_1^{d_2} = X_1^{d'_2}$ $\forall$ $\mathbf{d}_2, \mathbf{d}'_2 \in \mathcal{D}_1 \times \mathcal{D}_2$, where $X_t^{\mathbf{d}_2}$ denotes the potential covariates in period t under treatment sequence $\mathbf{d}_2$. See Lechner (2008) for a discussion of exogeneity in the single-period setting.

[9]Assumption 2.4 allows to identify additional parameters such as the average potential outcome within a particular treatment group in the first period $\mathbb{E}[Y^{\mathbf{d}_2}|D_1 = d'_1]$ $\forall$ $d'_1 \in \mathcal{D}_1$. However, identification for subgroups defined by later

the literature as the g-formula (Robins, 1986, 1987) or iterated conditional expectations (Tran et al., 2019).

2.4 Identification of Dynamic Policies

As previously discussed, prior economic literature has mainly concentrated on static policies while program assignment in practice often involves dynamic decision-making. When considering dynamic policies, it is necessary to strengthen identification assumptions. Unlike static policies, a dynamic policy prescribes treatment sequences based on intermediate potential decision variables. As a result, the counterfactual scenarios of interest become more complex, since the policy can deterministically depend on these intermediate variables. For instance, the policy defined in Example 2.1 assigns an individual to program d_2 or d_2' depending on whether the potential decision variable in the first period equals one or not. These potential intermediate values need to be considered in the conditional independence assumption.

Assumption 2.5. *Identification of dynamic policies under dynamic confounding.*

[a] CIA $\quad t=1: \ \{Y^{\mathbf{d}_2}, V_1^{d_1}\} \perp D_1 | X_0 = x_0 \ \forall \ x_0 \in \mathcal{X}_0, \ \forall \ \mathbf{d}_2 \in \mathcal{R}_1 \times \mathcal{R}_2$

$\quad t=2: \ Y^{\mathbf{d}_2} \perp D_2 | \mathbf{X}_1 = \mathbf{x}_1, D_1 = g_1(V_0) \ \forall \ \mathbf{x}_1 \in \mathcal{X}_0 \times \mathcal{X}_1, \ \forall \ \mathbf{d}_2 \in \mathcal{R}_1 \times \mathcal{R}_2$

[b] Overlap $\quad t=1: \ p_{g_1}(x_0) := \Pr(D_1 = g_1(V_0)|X_0 = x_0) > 0 \ \forall \ x_0 \in \mathcal{X}_0$

$\quad t=2: \ p_{g_2}(g_1, \mathbf{x}_1) := \Pr(D_2 = g_2(\mathbf{V}_1)|\mathbf{X}_1 = \mathbf{x}_1, D_1 = g_1(V_0)) > 0 \ \forall \ \mathbf{x}_1 \in \mathcal{X}_0 \times \mathcal{X}_1$

In Assumption 2.5, we extend the notion of 'full' conditional exchangeability in Robins & Hernán (2008) by employing $\mathbf{V}_1^{d_1}$ instead of $\mathbf{X}_1^{d_1}$ and $\mathcal{R}_t$ instead of $\mathcal{D}_t$. Compared to Assumption 2.4 for static policies, Assumption 2.5 introduces two key differences. First, conditional independence and overlap must hold for all possible treatment sequences that the dynamic policy could generate, rather than just one prespecified sequence. Second, given the pre-treatment characteristics, the assignment in the first period must be independent not only of any potential final outcomes but also of the potential intermediate variables that govern the second-period treatment. This additional requirement ensures that no unmeasured confounding affects the relationship between the first-period treatment and these intermediate variables. For example, if unmeasured confounders influence D_1 and V_1 but not D_1 and Y, then $Y^{\mathbf{g}_2}$ would not be identifiable, whereas $Y^{\mathbf{d}_2}$ remains identifiable under Assumption 2.4 (Robins, 1986; Robins & Hernán, 2008).

Importantly, unlike the key leading examples in the literature that define policies as functions

periods, i.e. $\mathbb{E}[Y^{\mathbf{d}_2}|\mathbf{D}_2 = \mathbf{d}_2']$, is not possible under Assumption 2.4 if $d_1 \neq d_1'$, as noted by Lechner & Miquel (2010).

of the entire covariate vector, we define policies as functions of selected decision variables only. Consequently, even though all covariates $\mathbf{X}_1$ might influence the underlying dynamic selection, conditional independence of treatment in $t = 1$ needs to hold only with respect to the potential $V_1^{d_1}$ that are part of the dynamic policy and not with respect to all $X_1^{d_1}$. Therefore, the choice of the structure of the dynamic policy critically influences the increased restrictiveness of Assumption 2.5. For instance, if $\mathbf{g}_2$ depends exclusively on intermediate outcomes, i.e., $V_1 = Y_1$, the additional requirements may be less demanding. This is because, in many practical contexts, assuming conditional independence between the first-period treatment and the final outcome naturally extends to intermediate outcomes as well. Consequently, when the policy bases second-period treatment solely on intermediate outcomes, the added restrictiveness may be minimal. Furthermore, if the chosen dynamic policy $\mathbf{g}_2$ more closely aligns with the assignment process underlying the observed data than a static policy $\mathbf{d}_2$, the overlap condition in Assumption 2.5[b] becomes more credible than its static counterpart 2.4[b]. We provide a detailed example of such a setting below in the empirical application.

Under the additional conditions in Assumption 2.5, identification is achieved in a similar way as for static policies. First, note that

$$\begin{aligned}
\mathbb{E}[Y^{\mathbf{g}_2}|\mathbf{X}_1, D_1 = g_1(V_0)] &= \mathbb{E}\left[\sum_{\mathbf{d}_2 \in \mathcal{D}_1 \times \mathcal{D}_2} \mathbf{1}\{g_1(V_0) = d_1, g_2(\mathbf{V}_1^{d_1}) = d_2\} Y^{\mathbf{d}_2} \middle| \mathbf{X}_1, D_1 = g_1(V_0)\right] \\
&= \mathbb{E}\left[\sum_{\mathbf{d}_2 \in \mathcal{D}_1 \times \mathcal{D}_2} \mathbf{1}\{D_1 = d_1, g_2(\mathbf{V}_1) = d_2\} Y^{\mathbf{d}_2} \middle| \mathbf{X}_1, D_1 = g_1(V_0)\right] \\
&= \mathbb{E}\left[\sum_{\mathbf{d}_2 \in \mathcal{D}_1 \times \mathcal{D}_2} \mathbf{1}\{D_1 = d_1, D_2 = d_2\} Y^{\mathbf{d}_2} \middle| \mathbf{X}_1, D_1 = g_1(V_0), D_2 = g_2(\mathbf{V}_1)\right] \\
&= \mathbb{E}[Y|\mathbf{X}_1, D_1 = g_1(V_0), D_2 = g_2(\mathbf{V}_1)] \\
&=: \mu_{\mathbf{g}_2}(\mathbf{X}_1),
\end{aligned} \tag{2.6}$$

where the first equality plugs in the definition of $Y^{\mathbf{g}_2}$, the second equality uses Assumption 2.1[b] for $V_1^{d_1}$, the third equality uses 2.5 for $t = 2$, and the fourth equality applies Assumption 2.1[a].

Using this result, it follows that

$$
\begin{aligned}
&\mathbb{E}[Y^{\mathbf{g}_2}|X_0] \\
&= \mathbb{E}\left[\sum_{\mathbf{d}_2\in\mathcal{D}_1\times\mathcal{D}_2} \mathbf{1}\{g_1(V_0)=d_1, g_2(\mathbf{V}_1^{d_1})=d_2\}Y^{\mathbf{d}_2}\middle|X_0\right] \\
&= \mathbb{E}\left[\sum_{\mathbf{d}_2\in\mathcal{D}_1\times\mathcal{D}_2} \mathbf{1}\{g_1(V_0)=d_1, g_2(\mathbf{V}_1^{d_1})=d_2\}Y^{\mathbf{d}_2}\middle|X_0, D_1=g_1(V_0)\right] \\
&= \mathbb{E}_{X_1}\left[\mathbb{E}\left[\sum_{\mathbf{d}_2\in\mathcal{D}_1\times\mathcal{D}_2} \mathbf{1}\{g_1(V_0)=d_1, g_2(\mathbf{V}_1^{d_1})=d_2\}Y^{\mathbf{d}_2}\middle|\mathbf{X}_1, D_1=g_1(V_0)\right]\middle|X_0, D_1=g_1(V_0)\right] \\
&= \mathbb{E}_{X_1}\big[\mu_{\mathbf{g}_2}(\mathbf{X}_1)\big|X_0, D_1=g_1(V_0)\big] \\
&=: \nu_{\mathbf{g}_2}(X_0)
\end{aligned}
$$

where the first equality again applies the definition of $Y^{\mathbf{g}_2}$, the second equality follows from Assumption 2.5 for $t=1$, the third equality applies the law of iterated expectations and the fourth equality plugs in result (2.6). From here, $\theta^{\mathbf{g}_2}$ and $\theta^{\mathbf{g}_2}(z_0)$ are obtained identically to (2.5) by integrating over X_0. In summary, despite the need for stronger identification conditions under dynamic policies, the logic of identification mirrors that of static policies once these conditions are met. As shown, the average outcome associated with following policy $\mathbf{g}_2(\mathbf{V}_1^{g_1})$ can be inferred from individuals whose observed treatments and covariates align with following strategy $\mathbf{g}_2(\mathbf{V}_1)$.[10]

2.5 Doubly Robust Identification

In addition to the identification results using the g-formula discussed in the previous subsections, alternative identification approaches based on Assumptions 2.1-2.5 offer favorable robustness properties through additional reweighting by propensity scores. Let $\Theta^{st}_{\mathbf{d}_2}(\mathbf{W})$ and $\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})$ denote so-called score functions for the settings under static and dynamic confounding, respectively,

[10] So far we did not consider the case of dynamic policies under static confounding. When relying on dynamic policies we often want to construct counterfactuals that are close to observed practice. Hence, in observational studies, dynamic policies usually should be accompanied by dynamic confounding in the underlying data. Dynamic policies under static confounding might become relevant in experimental settings, for example when using stratified-on-X_0 randomization. For such cases, Assumption 2.5 can be weakened by conditioning only on $X_0 = x_0$ instead of $\mathbf{X}_1 = \mathbf{x}_1$ in the second period. We omit an explicit statement of this assumption for the sake of brevity.

defined as

$$\Theta^{st}_{\mathbf{d}_2}(\mathbf{W}) := \mu_{\mathbf{d}_2}(X_0) + \frac{(Y - \mu_{\mathbf{d}_2}(X_0))\,\mathbf{1}\{\mathbf{D}_2 = \mathbf{d}_2\}}{p_{\mathbf{d}_2}(X_0)}, \tag{2.7}$$

$$\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W}) := \nu_{\mathbf{g}_2}(X_0) + \frac{(\mu_{\mathbf{g}_2}(\mathbf{X}_1) - \nu_{\mathbf{g}_2}(X_0))\,\mathbf{1}\{D_1 = g_1(V_0)\}}{p_{g_1}(X_0)} + \frac{(Y - \mu_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{\mathbf{D}_2 = \mathbf{g}_2(\mathbf{V}_1)\}}{p_{g_2}(\mathbf{X}_1, g_1)p_{g_1}(X_0)}. \tag{2.8}$$

It can be shown that the parameters of interest are identified as expectations of these scores,

$$\theta^{\mathbf{g}_2} = \mathbb{E}[\Theta^{j}_{\mathbf{g}_2}(\mathbf{W})] \qquad \text{and} \qquad \theta^{\mathbf{g}_2}(z_0) = \mathbb{E}[\Theta^{j}_{\mathbf{g}_2}(\mathbf{W})|Z_0 = z_0],$$

for $j = st$ under static confounding (Robins, Rotnitzky, & Zhao, 1994) and for $j = dy$ under dynamic confounding (Bang & Robins, 2005). The identification result under static confounding possesses the well-known double robustness property in the sense that it even identifies the APO if either $\mu_{\mathbf{d}_2}(\cdot)$ or $p_{\mathbf{d}_2}(\cdot)$ is replaced by an alternative function. Under dynamic confounding, this result extends to multiple robustness, which ensures identification if at least one component from each of the two periods (i.e. $\nu_{\mathbf{g}_2}(\cdot)$ or $p_{g_1}(\cdot)$ and $\mu_{\mathbf{d}_2}(\cdot)$ or $p_{g_2}(\cdot)$) is correctly specified.[11] The score functions (2.7) and (2.8) will become important for the estimation procedures discussed in the following section.

3 Evaluating Sequential Policies with Double Machine Learning

3.1 Causal Machine Learning Based on the Efficient Influence Function

The previous section introduced several estimands of interest and showed that, under additional assumptions, aggregates of their unobserved components can be expressed in terms of random variables, from which observations can be sampled. Given a set of such random samples, we now review different techniques proposed in the literature for estimating the parameters of interest and for conducting inference on them. In this paper, we consider flexible machine learning estimators of $\theta^{\mathbf{g}_2}$ and $\theta^{\mathbf{g}_2}(z_0)$ that do not require functional form assumptions about the underlying data generating process and allow to use high-dimensional covariates, enhancing the credibility of the identification arguments in observational settings.[12] However, machine learning estima-

[11] For a proof of identification double robustness under static confounding see e.g. Knaus (2022). Bradic et al. (2024, Lemma 1) provide a proof of double robustness of static policies under dynamic confounding. For completeness, Appendix B.1 extends their proof to dynamic policies.

[12] The focus here is on machine learning-based estimators; for an introduction to conventional parametric methods in the sequential setting we refer to Hernán & Robins (2020).

tors trade-off bias against variance to obtain a low mean squared error. Hence, naïve plug-in estimators based on (2.2) or (2.5), that use machine learning estimates of $\mu_{\mathbf{d}_2}(X_0)$ or $\nu_{\mathbf{g}_2}(X_0)$ are biased due to regularization. To address this issue, it has been shown that plug-in estimators can be de-biased using the efficient influence function (EIF) of the target parameter. Estimators based on the EIF have desirable properties, such as $\sqrt{N}$-convergence and asymptotic normality. Furthermore, an estimator needs to solve the EIF in order to be asymptotically efficient (Van der Laan & Gruber, 2012).

The average potential outcome $\theta^{\mathbf{g}_2}$, as identified by the g-formula, is a smooth one-dimensional functional whose analytic EIF is given by the difference between the doubly robust score (2.7 or 2.8) and the estimand itself (Bang & Robins, 2005; Kennedy, 2024), i.e.

$$EIF^j(\mathbf{W}, \theta^{\mathbf{g}_2}, \eta^j) = \Theta^j_{\mathbf{g}_2}(\mathbf{W}, \eta^j) - \theta^{\mathbf{g}_2} \;\; \text{for } j \in \{st, dy\}, \tag{3.1}$$

where $\eta^{st} = (\mu_{\mathbf{d}_2}(X_0), p_{\mathbf{d}_2}(X_0))$ and $\eta^{dy} = (\nu_{\mathbf{g}_2}(X_0), \mu_{\mathbf{g}_2}(\mathbf{X}_1), p_{d_2}(\mathbf{X}_1, g_1), p_{g_1}(X_0))$ denote the vectors of nuisance functions under static and dynamic confounding, respectively. For true $\theta^{\mathbf{g}_2}$ and η^j the EIF satisfies the moment condition $\mathbb{E}[EIF^j(\mathbf{W}, \theta^{\mathbf{g}_2}, \eta^j)] = 0$, as shown by the identification result in the previous section. This suggests to construct an estimator solving an empirical equivalent of this moment condition,

$$\frac{1}{N}\sum_{i=1}^{N} EIF^j(\mathbf{W}_i, \theta^{\mathbf{g}_2}, \hat{\eta}^j) = 0 \quad \Rightarrow \quad \hat{\theta}^{\mathbf{g}_2} = \frac{1}{N}\sum_{i=1}^{N} \Theta^j_{\mathbf{g}_2}(\mathbf{W}_i, \hat{\eta}^j), \tag{3.2}$$

where $\hat{\eta}^j$ refers to the estimated nuisance functions. In addition, for the group average potential outcome, $\theta^{\mathbf{g}_2}(z_0)$, the moment condition

$$\mathbb{E}[EIF^j(\mathbf{W}, \theta^{\mathbf{g}_2}, \eta^j)|Z_0 = z_0] = \frac{1}{\Pr(Z_0 = z_0)}\mathbb{E}[\Theta^j_{\mathbf{g}_2}(\mathbf{W}, \eta^j)\mathbf{1}\{Z_0 = z_0\}] - \theta^{\mathbf{g}_2} = 0$$

suggests to use the estimator

$$\hat{\theta}^{\mathbf{g}_2}(z_0) = \frac{1}{\frac{1}{N}\sum_{i=1}^{N}\mathbf{1}\{Z_{0i} = z_0\}} \sum_{i=1}^{N} \Theta^j_{\mathbf{g}_2}(\mathbf{W}_i, \hat{\eta}^j)\mathbf{1}\{Z_{0i} = z_0\}.$$

Hence, estimation of both parameters of interest can be based on the same scores (Chernozhukov, Hansen, Kallus, Spindler, & Syrgkanis, 2024).

In practice, estimation under static confounding ($j = st$) is implemented by first separately predicting $\hat{\mu}_{\mathbf{d}_2}(X_0)$ and $\hat{p}_{\mathbf{d}_2}(X_0)$ by machine learning methods, plugging them into Equation (2.7), i.e.

$$\Theta^{st}_{\mathbf{d}_2}(\mathbf{W}, \hat{\eta}^{st}) = \hat{\mu}_{\mathbf{d}_2}(X_0) + \frac{(Y - \hat{\mu}_{\mathbf{d}_2}(X_0)) \cdot \mathbf{1}\{\mathbf{D}_2 = \mathbf{d}_2\}}{\hat{p}_{\mathbf{d}_2}(X_0)}, \tag{3.3}$$

and finally averaging this score over the population of interest. For statistical inference, the variance of the scores can be used to construct a standard t-test statistic. The structure of the score $\Theta^{st}_{\mathbf{d}_2}(\mathbf{W}, \hat{\eta}^{st})$ demonstrates that the regularization bias in the estimation of $\hat{\mu}_{\mathbf{d}_2}(X_0)$ is corrected by adding an adjustment term, consisting of the residuals of the conditional outcomes $\hat{\mu}_{\mathbf{d}_2}(X_0)$, re-weighted by the inverse treatment probability. Hence, the adjustment increases as the prediction deviates further from the observed outcome and as the conditional treatment probability decreases.

DML in the single-period setting has been proposed by Chernozhukov et al. (2018). In their seminal paper, the authors demonstrate the importance of addressing regularization bias and overfitting in the estimates of the nuisance functions when employing the machine learning-based plug-in approach. This is achieved by (1) using a moment condition based on the EIF, and (2) by using a cross-fitting procedure. The EIF-based moment condition (1) allows to obtain $\sqrt{N}$-consistency even when using machine learning estimators that typically converge at relatively slow rates due to the curse of dimensionality. This is because the EIF satisfies a certain 'Neyman'-orthogonality property that makes it locally insensitive to small biases in the nuisance estimates. In analogy to the concept of identification double robustness discussed in Section 2.5, this property is also referred to as rate double robustness in the literature (Knaus, 2022). The cross-fitting procedure (2) avoids overfitting by ensuring that observations are not used to predict their own nuisance functions. Therefore, the set of observations $\mathcal{W} = \{1, ..., N\}$ is split into K equally sized subsamples $\mathcal{W}_k$. For each $k = 1, ..., K$ the nuisance parameters are first trained on the complementing subset $\mathcal{W}_{-k}$, then predicted in the subsample $\mathcal{W}_k$ and plugged into the score function. Finally, the DML estimate is obtained by taking the mean of the cross-fitted scores. In line with earlier reasoning, this procedure designed for the single-period setting, can be directly applied to sequential estimation under static confounding by treating each program sequence $\mathbf{d}_2$ as a distinct treatment state.

3.2 Sequential Double Machine Learning under Dynamic Confounding

Several recent contributions (Bodory et al., 2022; Bradic et al., 2024; Meza & Singh, 2021; Chernozhukov, Newey, Singh, & Syrgkanis, 2022) propose extensions of single-period DML to the sequential setting under dynamic confounding. All these extensions are introduced within a framework of static policies. However, once identification is established, DML-based estimation can proceed similarly for both static and dynamic policies, as detailed below. Accordingly, the procedures are presented directly using the more general notation for dynamic policies.

All mentioned contributions are based on the sequential EIF (3.1) with score

$$\begin{aligned}\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W}, \hat{\eta}^{dy}) = \hat{\nu}_{\mathbf{g}_2}(X_0) &+ \frac{(\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1) - \hat{\nu}_{\mathbf{g}_2}(X_0)) \cdot \mathbf{1}\{D_1 = g_1(V_0)\}}{\hat{p}_{g_1}(X_0)} \\ &+ \frac{(Y - \hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)) \cdot \mathbf{1}\{\mathbf{D}_2 = (g_1(V_0), g_2(\mathbf{V}_1))\}}{\hat{p}_{g_2}(\mathbf{X}_1, g_1)\hat{p}_{g_1}(X_0)},\end{aligned} \tag{3.4}$$

which now consists of two re-weighted outcome residuals, one for each period. The authors demonstrate that this score satisfies the Neyman orthogonality condition, indicating its suitability for the DML approach. However, the extension from static to dynamic confounding is complicated by the fact that $\nu_{\mathbf{g}_2}(X_0)$ cannot be directly represented by observable variables as it nests the conditional mean outcome of the second period $\mu_{\mathbf{g}_2}(\mathbf{X}_1)$. Hence, estimation of the function $\nu_{\mathbf{g}_2}(X_0)$ requires estimates $\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$ as inputs, which can be implemented in various ways.

An initial approach, as proposed by Bodory et al. (2022), is to estimate $\hat{\nu}_{\mathbf{g}_2}(X_0)$ by regression of $\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$ on X_0 for observations following $D_1 = g_1(V_0)$, i.e.

$$\hat{\nu}^{\mathrm{BHL22}}_{\mathbf{g}_2}(X_0) = \hat{\mathbb{E}}[\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)|X_0, D_1 = g_1(V_0)], \tag{3.5}$$

where $\hat{\mathbb{E}}[A|B, C = c]$ denotes predictions from a regression of A on B for observations with $C = c$. This requires an additional split of the subsamples $\mathcal{W}_{-k}$ to avoid overfitting, such that $\hat{\nu}_{\mathbf{g}_2}(X_0)$ and $\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$ are not learned from the same sample. In a setting with more than two time periods, the number of splits would increase even further. See Algorithm 3.6, column (1) for a detailed outline of the procedure.

Bradic et al. (2024) propose another method for implementing DML under dynamic confounding, introducing an additional bias correction term for estimating the nested conditional outcome. In particular, they estimate $\hat{\nu}_{\mathbf{g}_2}(X_0)$ as

$$\hat{\nu}^{\text{BJZ24}}_{\mathbf{g}_2}(X_0) = \hat{\mathbb{E}}\left[\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1) + \frac{\mathbf{1}\{D_2 = g_2(\mathbf{V}_1)\}\left(Y - \hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)\right)}{\hat{p}_{g_2}(\mathbf{X}_1, g_1)}\middle| X_0, D_1 = g_1(V_0)\right], \tag{3.6}$$

where the pseudo-outcome that is regressed on X_0 in the subsample $D_1 = g_1(V_0)$ corresponds to the doubly robust score of the treatment effect of the second period. Again, this requires second-order sample splitting, but the authors propose to regain full sample size efficiency by cross-fitting. In a first step, $\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$ is estimated using the first subsample of $\mathcal{W}_{-k}$, and predictions are made on the second subsample. These predictions are then used to estimate $\hat{\nu}^{\text{BJZ24}}_{\mathbf{g}_2}(X_0)$. The process is repeated by reversing the roles of the subsamples, resulting in two estimates of $\hat{\nu}^{\text{BJZ24}}_{\mathbf{g}_2}(X_0)$. Subsequently, the observations from fold $\mathcal{W}_k$ are applied to both estimated functions, and the predictions are averaged for each observation. The exact procedure is demonstrated in column (2) of Algorithm 1. While the original paper is only formulated for a binary treatment setting, we extend their proposed procedure to the case of multiple treatments.

For both approaches, the properties of $\sqrt{N}$-consistency and asymptotic normality extend from single-period DML to the sequential setting under extended assumptions. Besides standard regularity conditions, the theoretical guarantees rely on four (instead of two in the single-period framework) consistent nuisance parameter predictions $\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$, $\hat{\nu}_{\mathbf{g}_2}(X_0)$, $\hat{p}_{g_1}(X_0)$ and $\hat{p}_{g_2}(\mathbf{X}_1, g_1)$. In addition, Bodory et al. (2022) require three product rate conditions: The two within-period products of the rates of convergence between $\hat{\nu}_{\mathbf{g}_2}(X_0)$ and $\hat{p}_{g_1}(X_0)$, and between $\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$ and $\hat{p}_{g_2}(\mathbf{X}_1, g_1)$, as well as the cross-period product rate between $\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$ and $\hat{p}_{g_1}(X_0)$ need to be at least as fast as $N^{1/2}$ (see Assumption 4(d) therein). In Bradic et al. (2024), the additional doubly robust step reduces the number of required product rate conditions to just two. Specifically, only the within-period products need to be considered, making the cross-period product between $\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$ and $\hat{p}_{g_1}(X_0)$ no longer necessary. As will be demonstrated later, the practical significance of the weakened assumption appears minimal within the context of our empirical application.

Both discussed contributions are formulated in terms of static policies under dynamic confounding. While the identification assumptions differ between static and dynamic policies as discussed in the Section 2, both identification results have the same functional form, with $\mathbf{d}_2$ replaced by $\mathbf{g}_2(\mathbf{V}_1)$. DML is an estimator that plugs in nuisance estimates directly into the empirical representation of the identification result. These nuisance functions can be estimated in the same way for both static and dynamic policies. Moreover, dynamic policies are deterministic functions of the same covariates already appearing in the estimator for static policies under dynamic

Algorithm 1: Overview of the DML algorithms under dynamic confounding

(1) Bodory et al. (2022)	(2) Bradic et al. (2024)
Sample splitting:	
Randomly split $\mathcal{W}$ into K equally sized subsamples $\mathcal{W}_k$. Define $\mathcal{W}_{-k} := \mathcal{W} \backslash \mathcal{W}_k$ and further split $\mathcal{W}_{-k}$ into two equal-sized sets $\mathcal{W}_{-k,1}$ and $\mathcal{W}_{-k,2}$. For $\mathbf{a}_2 \in \{\mathbf{g}_2, \mathbf{g}_2'\}$, define $\mathcal{W}_{-k,a_1}$ as the subsample $\mathcal{W}_{-k}$ with $D_1 = a_1(V_0)$ and $\mathcal{W}_{-k,\mathbf{a}_2}$ as the subsample $\mathcal{W}_{-k}$ with $\mathbf{D}_2 = (a_1(V_0), a_2(\mathbf{V}_1))$. Similarly define $\mathcal{W}_{-k,1,a_1}$, $\mathcal{W}_{-k,2,a_1}$ $\mathcal{W}_{-k,1,\mathbf{a}_2}$ and $\mathcal{W}_{-k,2,\mathbf{a}_2}$, respectively.	
Estimation of nuisance functions:	
for $\mathbf{a}_2 \in \{\mathbf{g}_2, \mathbf{g}_2'\}$ **do**	**for** $\mathbf{a}_2 \in \{\mathbf{g}_2, \mathbf{g}_2'\}$ **do**
for $k = 1, ..., K$ **do**	**for** $k = 1, ..., K$ **do**
Learn $\hat{p}_{a_1}$ on $\mathcal{W}_{-k}$	Learn $\hat{p}_{a_1}$ on $\mathcal{W}_{-k}$
Learn $\hat{p}_{a_2}$ on $\mathcal{W}_{-k,a_1}$	Learn $\hat{p}_{a_2}$ on $\mathcal{W}_{-k,a_1}$
Learn $\hat{\mu}_{\mathbf{a}_2}$ on $\mathcal{W}_{-k,1,\mathbf{a}_2}$	Learn $\hat{\mu}_{\mathbf{a}_2}$ on $\mathcal{W}_{-k,\mathbf{a}_2}$
Predict $\hat{\mu}_{\mathbf{a}_2}$ on $\mathcal{W}_{-k,2,\mathbf{a}_2}$, learn $\hat{\nu}_{\mathbf{a}_2}$ on $\mathcal{W}_{-k,2,\mathbf{a}_2}$ using (3.5)	Learn $\hat{\mu}_{\mathbf{a}_2}$ on $\mathcal{W}_{-k,1,\mathbf{a}_2}$, learn $\hat{p}_{a_2}$ on $\mathcal{W}_{-k,1,a_1}$, predict $\hat{\mu}_{\mathbf{a}_2}$ and $\hat{p}_{a_2}$ on $\mathcal{W}_{-k,2,a_1}$, learn $\hat{\nu}^1_{\mathbf{a}_2}$ on $\mathcal{W}_{-k,2,a_1}$ using (3.6)
	Learn $\hat{\mu}_{\mathbf{a}_2}$ on $\mathcal{W}_{-k,2,\mathbf{a}_2}$, learn $\hat{p}_{a_2}$ on $\mathcal{W}_{-k,2,a_1}$, predict $\hat{\mu}_{\mathbf{a}_2}$ and $\hat{p}_{a_2}$ on $\mathcal{W}_{-k,1,a_1}$, learn $\hat{\nu}^2_{\mathbf{a}_2}$ on $\mathcal{W}_{-k,1,a_1}$ using (3.6)
Predict $\hat{p}_{a_1}$, $\hat{p}_{a_2}$, $\hat{\mu}_{\mathbf{a}_2}$ and $\hat{\nu}_{\mathbf{a}_2}$ on $\mathcal{W}_k$	Predict $\hat{p}_{a_1}$, $\hat{p}_{a_2}$, $\hat{\mu}_{\mathbf{a}_2}$, $\hat{\nu}^1_{\mathbf{a}_2}$ and $\hat{\nu}^2_{\mathbf{a}_2}$ on $\mathcal{W}_k$, obtain $\hat{\nu}_{\mathbf{a}_2}$ by computing average of $\hat{\nu}^1_{\mathbf{a}_2}$ and $\hat{\nu}^2_{\mathbf{a}_2}$
end for	**end for**
Compute $\hat{\Theta}^{dy}_{\mathbf{a}_2,i}$ using (3.4)	Compute $\hat{\Theta}^{dy}_{\mathbf{a}_2,i}$ using (3.4)
end for	**end for**
Computation of effects and standard errors:	
Compute $\hat{\theta}^{\mathbf{g}_2} = \frac{1}{N}\sum_{i=1}^N \hat{\Theta}^{dy}_{\mathbf{g}_2,i}$ and $\hat{\sigma}^{\mathbf{g}_2} = \sqrt{\frac{1}{N}\sum_{i=1}^N \left(\hat{\Theta}^{dy}_{\mathbf{g}_2,i} - \hat{\theta}^{\mathbf{g}_2}\right)^2}$	
Compute $\hat{\tau}^{\mathbf{g}_2,\mathbf{g}_2'} = \frac{1}{N}\sum_{i=1}^N \hat{\Theta}^{dy}_{\mathbf{g}_2,i} - \hat{\Theta}^{dy}_{\mathbf{g}_2',i}$ and $\hat{\sigma}^{\mathbf{g}_2,\mathbf{g}_2'} = \sqrt{\frac{1}{N}\sum_{i=1}^N \left(\hat{\Theta}^{dy}_{\mathbf{g}_2,i} - \hat{\Theta}^{dy}_{\mathbf{g}_2',i} - \hat{\tau}^{\mathbf{g}_2,\mathbf{g}_2'}\right)^2}$	
Let $N_{z_0} = \sum_{i=1}^N \mathbf{1}\{Z_{0i} = z_0\}$. Compute $\hat{\tau}^{\mathbf{g}_2,\mathbf{g}_2'}(z_0) = \frac{1}{N_{z_0}}\sum_{i=1}^N \mathbf{1}\{Z_{0i} = z_0\}\left(\hat{\Theta}^{dy}_{\mathbf{g}_2,i} - \hat{\Theta}^{dy}_{\mathbf{g}_2',i}\right)$ and $\hat{\sigma}^{\mathbf{g}_2,\mathbf{g}_2'}(z_0) = \sqrt{\frac{1}{N_{z_0}}\sum_{i=1}^N \mathbf{1}\{Z_{0i} = z_0\}\left(\hat{\Theta}^{dy}_{\mathbf{g}_2,i} - \hat{\Theta}^{dy}_{\mathbf{g}_2',i} - \hat{\tau}^{\mathbf{g}_2,\mathbf{g}_2'}(z_0)\right)^2}$	

confounding. Hence, the proposed estimators naturally generalize to dynamic policies, allowing estimation to proceed in the same manner regardless of whether the interest lies in $Y^{\mathbf{d}_2}$ or $Y^{\mathbf{g}_2}$.

The presented estimators offer ready-to-implement algorithms based on robust theoretical foundations under standard assumptions. While we focus on these methods in this paper, several alternative EIF-based estimators have been proposed in the literature that rely on stronger assumptions or lack practical implementability in our setting (Meza & Singh, 2021; Chernozhukov, Newey, Singh, & Syrgkanis, 2022; Van der Laan & Gruber, 2012; Lewis & Syrgkanis, 2021). We provide a detailed overview of these methods in Appendix A.2.

4 Application: Evaluation of ALMP Sequences in Switzerland

4.1 Introduction and Related Literature

ALMP are government programs provided to unemployed individuals with the objective objective of facilitating their return to the labor market. While the extensive previous literature on ALMP mostly focused on the effects of the first or longest program, here we want to exploit the sequential treatment effect framework to assess two practical details among the individuals that participate: The duration and the order of the programs. Lechner & Wiehler (2013) is the first comprehensive evaluation of ALMP in the sequential context based on the methods originally developed in Lechner & Miquel (2010) and Lechner (2009), which allow for the assessment of static sequential policies under dynamic confounding. Using Austrian data and inverse probability weighting, the authors find that jobs search assistance is more effective after a qualification program compared to the reverse order. In addition, they conclude that sequences of two qualification measures perform better than only one initial qualification program. Adopting the same approach, Dengler (2015, 2019) find similar results for public employment programs and classroom training in Germany. Vikström (2017) analyzes ALMP sequences in Sweden using a survival time framework, where the outcome of interest is the probability of remaining unemployed up to a specific period, which differs from our focus on medium-term labor market outcomes. In contrast to Lechner & Wiehler (2013) and Dengler (2015, 2019), the study concludes that there is usually no advantage to participating in a sequence of two programs over participating in a single program. However, it presents evidence that participating in a work practice program following a training program can shorten the duration of unemployment compared to participating in only the training program or no program at all.[13]

4.2 Institutional Background

In Switzerland, ALMP are nationally regulated but implemented by regional employment offices (REOs). Unemployed individuals who have worked at least 12 months in the previous two years can register at a REO to receive income maintenance based on their past salary for up to 24 months. To receive these benefits, individuals must actively search for a job and participate in assigned ALMP. The programs can be categorized into five groups:

[13]Besides the mentioned studies, there exists a stream of literature that considers dynamic assignment to ALMP in the sense that they account for potential non-randomness of program start dates (e.g. Sianesi, 2004; Crépon, Ferracci, Jolivet, & Van den Berg, 2009; Van den Berg & Vikström, 2022; Kastoryano & Van der Klaauw, 2022). While these papers adapt their identification and estimation procedures to dynamic confounding, they remain within a framework that compares single ALMP but does not allow the evaluation of program sequences.

- *Job search assistance (JA)*: Orientation measures and courses for basic job acquisition skills (e.g. job application strategies, career development workshops).
- *Training courses (TC)*: Language, computer and sector-specific vocational training courses from introductory to advanced level.
- *Employment program (EP)*: Unpaid employment outside the regular labor market (not in competition with other firms) providing individuals a meaningful activity and daily routine.
- *Temporary wage subsidy (WS)*: Monetary compensation incentivizing individuals to accept temporary jobs paying lower wage than the unemployment benefit.
- *Other programs (OP):* Small programs not included in the previous four categories, such as training grants, vocational placements, and internships.

Two thirds of individuals take part in one of these programs within the first twelve months of their unemployment spell, with approximately 60% of them participating in more than one program. Caseworkers have the authority to sanction individuals if they decline to participate.

4.3 Data and Panel Design

The analysis is based on Swiss administrative records for the period 2004 to 2018. The population under study is defined as individuals aged 25-55[14] who became eligible for programs and received unemployment benefits between April 2011 and January 2015,[15] following a minimum of three months of prior unsubsidized employment. Among these individuals, all individuals with a program lasting at least five business days and starting within 12 months of the beginning of their unemployment spell are selected.[16] For the final sample of 191,619 individuals, we observe monthly information on employment status, program participation and covariates. For details on the institutional background and data, see Mascolo, Bearth, Muny, Lechner, & Mareckova (2024), who base their study on the same public records.

The information is aggregated to a panel of two three-month periods, starting from the month of the first program. This structure has been chosen to meet two key requirements: Firstly, it should capture as good as possible the true assignment process. Secondly, it should ensure a sufficient

[14]Younger and older individuals are excluded to avoid dealing with educational and (early) retirement choices.

[15]The time frame is restricted by a major revision of Swiss unemployment insurance in early 2011 and the need for a follow-up period of at least three years after program start to measure outcomes.

[16]These restrictions ensure that assessments conducted before the allocation to a program are excluded and that there is sufficient time left to participate in a program within the entitlement period.

number of observations per program sequence to enable a meaningful econometric analysis.[17] To ensure a sufficient sample size, the panel's reference point is set to the start month of the first program, rather than the beginning of the unemployment spell. As a result, all individuals participate in a program during the first period. While this eliminates a control group of non-participants in the initial period, it substantially increases the number of individuals with identical sequences.[18] The lack of sequences beginning with non-participants is of minor concern, as the primary focus is on evaluating program combinations and durations, while addressing dynamic confounding.[19]

Another key factor influencing the range of possible program sequences is the number of periods. With each additional period, the number of sequences for a given set of programs increases exponentially. This makes it increasingly difficult to find individuals with the same sequence as the number of periods rises. In addition, the dynamic estimation methods become increasingly unstable with more periods due to extra sample splits and the multiplication of additional propensity scores in the denominators of the de-biasing terms. Consequently, the number of periods has been chosen to be two, consistent with the formal notation introduced in the previous sections.

Finally, period length is determined in a data-driven way. Our sample reveals a median interval of 3 months between program starts and a median program duration of 45 days. Opting for three-month periods maximizes the number of program starts in the second period, compared to two-month or four-month intervals. With a three-month period length, individuals have on average 2.1 appointments with their caseworker in the first period, which seems reasonable since the assignment decision is expected to be reconsidered less frequently than at every single meeting. In addition, the proportion of individuals with changes in covariates X_1 relative to X_0 increases considerably when moving from 2-month to 3-month intervals, but shows a much smaller increase when comparing 3-month to 4-month intervals. This suggests that a three-month period is sufficient for time-varying selection to occur.

[17] Since aggregation inherently introduces inaccuracies, maintaining the dataset at the highest possible level of granularity would be ideal. When opting against aggregation, an alternative approach involves using estimators relying on parametric assumptions, such as marginal structural models (Robins et al., 2000), which enable extrapolation into regions beyond observed data support. However, given limited knowledge about the true data-generating process in this application, an aggregation approach was selected instead, prioritizing flexible estimation.

[18] For example, consider two individuals who are unemployed for ten months. Individual 1 is assigned to a six-week training course in the first month of the unemployment spell. Individual 2 is assigned to the same program in the fifth month. Despite the different start times of the program, they both exhibit the same program sequence, "training course - no program".

[19] We note that alternative designs might be preferable if the focus of the evaluation is when to start programs (timing-to-treatment framework, e.g. Nie, Brunskill, & Wager, 2021) or program duration without considering dynamics (dose response framework, e.g. Imbens, 2000).

4.4 Definition of Treatments and Outcomes

The primary objective of this analysis is to evaluate the effectiveness of different program sequences. To achieve this, the outcome measure is defined as the cumulative months of employment over a 30-month period following the first program. This measure captures the medium-term impact of program sequences on employment trajectories, which aligns with the Swiss government's policy objective of promoting sustained employment through ALMP.

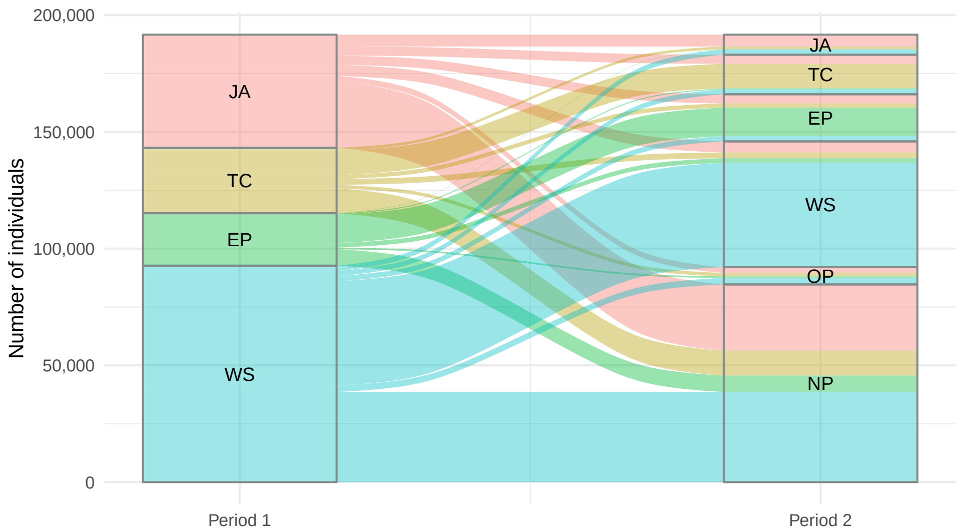
In each of the two periods of the panel, each individual is assigned to a treatment state corresponding to one of the four programs *JA*, *TC*, *EP*, or *WS* introduced above. For a program to be considered a treatment state, it must last at least five business days within the period, regardless of its start date. For instance, if an individual participates in an employment program for five months, the treatment state in the second period is *EP*, even if the program started in the first period. Individuals participating in multiple different programs within the same period are assigned to the longest program.[20] In the second period, we introduce two additional treatment states. Individuals who remain without program participation throughout the period are categorized as *No program (NP)*. This group includes both unemployed individuals who are no longer assigned to a program and individuals who are not assigned because they have exited unemployment. This additional treatment state allows analyzing sequences with a program in the first period but no program in the second period. Individuals who are assigned to *OP* in the second period remain in the sample for modeling treatment assignment in the first period. However, they are not considered in the analysis due to the small number of participants and special admission requirements for these programs, which prevent credible identification.

Figure 2 illustrates the distributions of the treatment states across the two periods, revealing insights about program size and duration. In the first period, temporary wage subsidies comprise nearly half of the beneficiaries, followed by job-search assistance. Most recipients of temporary wage subsidies in the first period continue in the second period, while recipients of job-search assistance often transition to other program states. Overall, more than half of the individuals remain in a program in the second period. The plot highlights the large variety of transitions, emphasizing the importance of sequential analysis.

When evaluating ALMP over time, program assignments critically depend on the evolution of employment status. For instance, if an individual exits unemployment during the first period, this

[20] Hence, we disregard within-period dynamics in program assignment, i.e. assignment to a second program within a period does not depend on the first program within the same period. This seems reasonable given the limited duration of the periods.

Figure 2: Alluvial plot of program sequences considered in the analysis



Notes: Program frequencies and transitions between first and second period. *JA*: Job-search assistance, *TC*: Training course, *EP*: Employment program, *WS*: Wage subsidy, *OP*: Other programs, *NP*: No program.

directly influences program allocation decisions in the second period. Consequently, evaluating counterfactuals that impose a fixed sequence of two program periods (d_1, d_2) for all individuals, without accounting for changes in employment status, may lack meaningful insights. While previous research has largely ignored this issue, the present analysis proposes a solution based on dynamic policies. Specifically, as previewed earlier in Example 2.1, the decision variable V_1, which determines program allocation in the second period, is defined as the potential intermediate outcome $Y_1^{d_1}$. Here, $Y_1^{d_1}$ is a binary indicator equal to one if an individual treated with program d_1 exits unemployment during the first period and zero otherwise. Using this decision variable, we define 20 dynamic policies of interest as

$$\mathbf{g}_2(Y_1^{d_1}) = (d_1, \mathbf{1}\{Y_1^{d_1} = 0\} \cdot d_2 + \mathbf{1}\{Y_1^{d_1} = 1\} \cdot \mathrm{NP}), \tag{4.1}$$

with $d_1, d_2 \in \{\mathrm{JA}, \mathrm{TC}, \mathrm{EP}, \mathrm{WS}\} \times \{\mathrm{JA}, \mathrm{TC}, \mathrm{EP}, \mathrm{WS}, \mathrm{NP}\}$. Note that the policies are dynamic only for the second period, while they assign a fixed program d_1 in the first period. This allows to construct counterfactuals in which every individual is initially assigned to program d_1 during the first period, with program d_2 assigned in the second period only to those who remain unemployed throughout the first period. The treatment d_2 may also be defined as *NP*, in which case the policy is fully static. Policy 4.1 is defined in terms of potential intermediate outcomes $Y_1^{d_1}$ as the assignment to the second program should depend on the employment status after the first counterfactual program d_1 and not on the employment status following the observed program D_1.

As shown above, the average outcome associated with following policy $\mathbf{g}_2(Y_1^{d_1})$ can be inferred from individuals whose observed treatments and covariates align with following strategy $\mathbf{g}_2(Y_1)$ if the identification assumptions hold.

4.5 Identification

For single time-point interventions, the literature widely acknowledges that the effects of ALMP can be plausibly identified using observational data, provided a comprehensive set of control variables is available (Caliendo, Mahlstedt, & Mitnik, 2017; Lechner & Wunsch, 2013). Since individuals are randomly assigned to caseworkers, who then determine program assignments, the primary challenge lies in accounting for all factors considered by caseworkers during this process. In the current setting, all relevant covariates identified in prior research are available. These include, among others, socio-demographic characteristics, employment and earnings histories spanning the past seven years, details about the last job and prior unemployment spells, regional labor market conditions, and caseworkers' assessments of individuals' job search efforts prior to program start. An overview of all control variables used in the analysis can be found in Appendix C.

In the analysis of sequential treatments, the identification assumptions for single time-point interventions must be extended to address static or dynamic confounding, as well as static or dynamic policies, as discussed in Section 2. Under static confounding, Assumption 2.3 requires the entire program path to be predetermined, given the available information before the start of the sequence. When working with observational data, this seems unlikely, as caseworkers may adjust their assignments over time. This concern is supported by an analysis of covariate means by program participation in Appendix C.1, which reveals substantial differences across program sequences, for both baseline and time-varying covariates (see Tables C.1 and C.2). Therefore, we focus on the setting under dynamic confounding, which permits a more flexible program assignment process in the underlying data.

For static policies under dynamic confounding, as analyzed for example in Lechner & Wiehler (2013) and Dengler (2015, 2019), Assumption 2.4[a] consists of two parts, one for every period. In the first period, the identification argument aligns with that of a standard non-sequential evaluation and should therefore be reasonable given the available information, as outlined earlier. The additional difficulty of dynamic confounding is addressed by the second part of the assumption, which requires controlling for all factors that influence both potential outcomes and program assignment in the second period, conditional on program participation in the first period. To address this assumption, five types of covariates are considered, that may change during the first period and are expected to determine dynamic selection into the second program. Firstly, it is essential to control for the intermediate outcome, employment status, as only unemployed individuals are eligible for assignment to a program in the second period. Secondly, financial aspects are taken into account by controlling for the amount of unemployment benefits and other state subsidies, as well as intermediate earnings during the first period. Thirdly, the selection likely hinges on an individual's job prospects. These are assessed through variables such as the number of applications written and caseworkers' assessments of individual's job search efforts, employability, and qualification needs. Fourthly, caseworkers' decisions might be influenced by the cooperativeness of individuals. This is measured through the number of scheduled, postponed, and canceled appointments at the REO, as well as incidents resulting in sanction days. Lastly, changes in the personal situation such as relocation, pregnancy, and the number of sickness days, are also accounted for.

A key difficulty in identifying static program sequences is that program participation depends on remaining unemployed. Consequently, both unemployment status and program assignment must be accounted for when addressing selection bias. This essentially requires that, conditional on covariates and prior program participation, not only program assignment but also the chance of remaining unemployed are random, which makes credible identification significantly more demanding. This issue can be addressed by applying dynamic policies, as defined in equation (4.1), where only individuals who remain unemployed throughout the first period are assigned to a second program. However, this advantage comes at the cost of the stricter identification requirements stated in Assumption 2.5. In particular, conditional on pre-treatment information, assignment to the first program needs to be independent of final *and intermediate* potential outcomes. Given the context of this application, the additional restrictiveness of the assumption appears to be unproblematic. So far, conditional independence has been assumed with respect to the joint distribution of potential employment status over the 30 months following the first period. The additional complexity introduced by the dynamic strategy now necessitates independence with respect to employment status during the three months of the first period as well. This restriction aligns with the assumption made in any single-period program evaluation that measures outcomes from the start of the treatment.

Finally, the overlap assumptions 2.4[b] and 2.5[b] require that, in both periods, there is a nonzero probability of following the (static or dynamic) policy of interest, conditional on pre-period information. This assumption is expected to hold among unemployed individuals, as all the programs considered are accessible to them, irrespective of their covariate history. Thus, although treatment probabilities may vary based on previous characteristics, any unemployed individual should, in principle, be eligible for admission to any program. Again, the fact that individuals might exit unemployment during the first period poses a challenge to the credibility of the assumption for static program sequences. For example, individuals who leave unemployment in the first period are only eligible for treatment in the second period if they re-enter unemployment within that three-month window, resulting in a very low treatment probability. This issue does not arise for dynamic strategies, as such situations are excluded under any treatment strategy of interest. Specifically, for any realization of the function $g_2(Y_1^{d_1})$, individuals employed in the first period are not assigned to a program in the second period. Hence, carefully designing the structure of the dynamic policy allows to obtain counterfactuals that are not only more practice-relevant but also more credibly identified.

4.6 Implementation

To estimate the effects of interest we apply the two dynamic estimation procedures by Bodory et al. (2022)[21] and Bradic et al. (2024) described in Section 3 with 5-fold cross-fitting. The nuisance functions are estimated by random forests using the Python package `scikit-learn` (Pedregosa et al., 2011). Following Bach, Schacht, Chernozhukov, Klaassen, & Spindler (2024), we tune the hyperparameters of the random forest on the full sample using `FLAML` (Wang, Wu, Weimer, & Zhu, 2021) with a maximum time budget of 10 minutes per nuisance estimation. The minimum number of trees used in a forest is set to 500.

Estimators based on propensity score reweighting, such as DML, are known to produce unstable estimates when estimated propensity scores assign large weights to certain observations (Khan & Tamer, 2010). Hence, even if overlap holds in the population, it may not be satisfied in the sample if the treatment probabilities for certain programs are very low for specific individuals. To address this issue, we drop observations with extreme propensity scores, extending the minmax trimming procedure proposed in Lechner & Strittmatter (2019) to the sequential setting. In particular, for the first period propensity scores $\hat{p}_{d_1}(X_0)$, we first identify the minimum value among individuals

[21]Note that our implementation of the procedure by Bodory et al. (2022) differs from their own implementation provided in the `causalweight` **R**-package. In particular, they fit $\hat{\mu}$ stratified by programs but include D_1 as a covariate when estimating $\hat{p}_{d_2}$. Instead, we stratify both $\hat{\mu}$ and $\hat{p}_{d_2}$. This adjustment is expected to reduce bias in the estimates of $\hat{p}_{d_2}$, but may increase variance if there are few individuals following a particular program d_1.

within the subgroups $D_1 = d_1$ and $D_1 \neq d_1$, respectively. Then, the largest of these two values is selected, and all individuals with a $\hat{p}_{d_1}(X_0)$ smaller than this threshold are removed from the sample. Analogously, all units with $\hat{p}_{d_1}(X_0)$ larger than the smallest maximum are also removed. For the second period propensity scores $\hat{p}_{g_2}(\mathbf{X}_1, d_1)$ the same procedure is applied for individuals with ($D_1 = d_1$ and $D_2 = g_2(Y_1)$) vs. ($D_1 \neq d_1$ or $D_2 \neq g_2(Y_1)$), separately for the subgroups with $g_2(Y_1) = d_2$ and $g_2(Y_1) = \text{NP}$, respectively. After repeating the procedure for all 20 dynamic policies of interest, 7% of the observations are dropped, resulting in a final sample size of 177,856 observations. Plots of the propensity score densities after trimming, presented in Figures D.4 and D.5 in the appendix, demonstrate that the procedure achieves satisfactory levels of overlap for all relevant sequences. The shares of trimmed observations for each policy are indicated in the results tables the following section, while the treatment frequencies in the trimmed sample are provided in Figure D.1. A comparison of pre-treatment covariate means reveals no substantial differences between the original and trimmed data, as demonstrated in Table D.1.

4.7 Results

4.7.1 Program Duration

Table 2 presents the main results of the analysis. It illustrates average treatment effects for different comparisons of policies and estimation techniques. Each column refers to a comparison of programs in the first period while in the panels below, several scenarios of second period policies are compared. In addition to the ATEs and their standard errors, the number of observations remaining after trimming under a particular policy, along with their share relative to the untrimmed sample, are reported. Panel A provides a setting where the second period programs are unrestricted. These are the results one would obtain from a standard single-period evaluation analyzing the first program only. We start with discussing these conventional results, before showing what we can learn in addition from the dynamic methods. The results indicate that *WS* is the most effective program on average, leading to significantly more months in employment in the medium term compared to all other programs. The effect sizes range from 2.68 months more employment compared to *JA* to 1.95 months more employment compared to *TC* in the 30 months following the second period. *TC* is identified as the second most effective program, yielding 0.73 and 0.63 months of increased employment compared to *JA* and *EP*, respectively. No statistically significant difference is observed between the programs *JA* and *EP*.

The median duration of the four programs, measured by the number of business days, are 23

Table 2: Average treatment effects for program duration.

	d_1 = JA d'_1 = TC	d_1 = JA d'_1 = EP	d_1 = JA d'_1 = WS	d_1 = TC d'_1 = EP	d_1 = TC d'_1 = WS	d_1 = EP d'_1 = WS
Panel A: Second period program unrestricted (single-period intervention):						
ATE (static conf.)	-0.73*** (0.13)	-0.10 (0.19)	-2.68*** (0.11)	0.63*** (0.18)	-1.95*** (0.08)	-2.57*** (0.16)
N_{d_1}	45,681 (94%)	45,681 (94%)	45,681 (94%)	26,501 (95%)	26,501 (95%)	20,636 (92%)
$N_{d'_1}$	26,501 (95%)	20,636 (92%)	85,038 (92%)	20,636 (92%)	85,038 (92%)	85,038 (92%)
Panel B: Second period without program (static policy): $g_2(Y_1^{d_1})$ = NP, $g'_2(Y_1^{d'_1})$ = NP						
ATE (BHL22)	-1.31*** (0.24)	-1.25*** (0.40)	-4.16*** (0.21)	0.06 (0.37)	-2.84*** (0.16)	-2.91*** (0.35)
ATE (BJZ24)	-1.37*** (0.24)	-1.18*** (0.39)	-4.14*** (0.21)	0.19 (0.37)	-2.77*** (0.16)	-2.96*** (0.34)
ATE (static conf.)	-1.45*** (0.17)	-2.41*** (0.30)	-4.58*** (0.13)	-0.96*** (0.30)	-3.14*** (0.14)	-2.17*** (0.28)
$N_{\mathbf{g}_2}$	26,358 (93%)	26,358 (93%)	26,358 (93%)	9,992 (94%)	9,992 (94%)	6,225 (88%)
$N_{\mathbf{g}'_2}$	9,992 (94%)	6,225 (88%)	35,231 (91%)	6,225 (88%)	35,231 (91%)	35,231 (91%)
Panel C: Same program for at least two periods (static policy): $g_2(Y_1^{d_1}) = d_1$, $g'_2(Y_1^{d'_1}) = d'_1$						
ATE (BHL22)	-1.05*** (0.31)	-0.69* (0.40)	-2.91*** (0.27)	0.36 (0.35)	-1.86*** (0.18)	-2.22*** (0.31)
ATE (BJZ24)	-1.50*** (0.42)	-0.73 (0.47)	-2.90*** (0.30)	0.77 (0.48)	-1.40*** (0.31)	-2.17*** (0.38)
ATE (static conf.)	-1.61*** (0.20)	-0.97*** (0.35)	-3.21*** (0.16)	0.63* (0.33)	-1.60*** (0.13)	-2.24*** (0.31)
$N_{\mathbf{g}_2}$	4,743 (94%)	4,743 (94%)	4,743 (94%)	9,906 (95%)	9,906 (95%)	11,490 (94%)
$N_{\mathbf{g}'_2}$	9,906 (95%)	11,490 (94%)	41,014 (92%)	11,490 (94%)	41,014 (92%)	41,014 (92%)
Panel D: Same program for at least two periods if not employed in first period (dynamic policy): $g_2(Y_1^{d_1}) = \mathbf{1}\{Y_1^{d_1} = 0\}d_1 + \mathbf{1}\{Y_1^{d_1} = 1\}\text{NP}$ and $g'_2(Y_1^{d'_1}) = \mathbf{1}\{Y_1^{d'_1} = 0\}d'_1 + \mathbf{1}\{Y_1^{d'_1} = 1\}\text{NP}$						
ATE (BHL22)	-1.24*** (0.22)	-0.57** (0.25)	-3.47*** (0.17)	0.67*** (0.24)	-2.23*** (0.15)	-2.90*** (0.20)
ATE (BJZ24)	-1.14*** (0.21)	-0.49* (0.26)	-3.39*** (0.16)	0.65*** (0.25)	-2.26*** (0.15)	-2.91*** (0.21)
$N_{\mathbf{g}_2}$	9,349 (95%)	9,349 (95%)	9,349 (95%)	11,836 (95%)	11,836 (95%)	13,268 (94%)
$N_{\mathbf{g}'_2}$	11,836 (95%)	13,268 (94%)	55,213 (93%)	13,268 (94%)	55,213 (93%)	55,213 (93%)

Note: This table presents ATEs and the number of observations for various comparisons of policies. d_1 and d'_1 represent first-period programs in the treatment and control states, respectively, while $g_2(Y_1^{d_1})$ and $g'_2(Y_1^{d'_1})$ denote second-period policies dependent on the potential intermediate outcome $Y_1^{d_1}$ or $Y_1^{d'_1}$ (1 if an individual exits unemployment in the first period). JA: Job-search assistance, TC: Training course, EP: Employment program, WS: Temporary wage subsidy, NP: No program. Outcome: Cumulative months in employment in the 30 months from start of the second period. Rows labeled ATE report effect sizes with standard errors in parentheses. *, **, *** indicate p-values below 10%, 5%, and 1%. Row labeled N show the trimmed sample size and its proportion of the untrimmed sample (in parentheses).

for *JA*, 39 for *TC*, 80 for *EP*, and 65 for *WS*. A natural question arising from these differences is whether program duration affects effectiveness, i.e., whether some programs are superior to others due to their varying lengths. One approach to address this question is to use the sequential framework, comparing sequences as if individuals only participated during the first period.[22] Recall that policies of this type are not dynamic as *NP* can occur in the second period regardless of whether an individual is unemployed or not. The results are presented in Panel B of Table 2. The first two rows of the panel present ATEs estimated using the methods of Bodory et al. (2022) and Bradic et al. (2024), respectively, while the third row shows estimates ignoring dynamic confounding.

The results show that when restricting program duration to a maximum of three months, the ranking of the programs slightly changes. While *WS* remains the most beneficial and *JA* the least beneficial program, there is no longer a significant difference between *TC* and *EP*. This implies that the effectiveness of *EP* could be improved by cutting its duration. Furthermore, aligning program duration enhances the advantage of *WS* relative to the other programs, for instance, by 1.5 months compared to *JA* and nearly one month compared to *TC*. In general, the two methods addressing dynamic confounding provide very similar results, suggesting no clear superiority of a particular method in this application. When comparing dynamic to static confounding, a systematic over-estimation of ATEs is observed for programs with a larger average duration. For example, when comparing the shortest program (*JA*) to the longest program (*EP*), the relative effectiveness of the latter appears to double when dynamic confounding is ignored (1.18-1.25 vs. 2.41). Consequently, disregarding the feedback between treatments and intermediate outcomes results in an overly optimistic assessment of longer programs.

Instead of restricting programs to the first period, another approach to align program duration is to require at least two periods of the same program. This can be implemented by using static or dynamic policies. The former, shown in Panel C of Table 2, present average outcomes for the scenario in which all individuals are assigned to the same program in both periods. This is the approach used for example in Lechner & Wiehler (2013). Alternatively, dynamic policies can be used to construct counterfactuals for the arguably more realistic scenario in which individuals are reassigned to the same program only if they remain unemployed throughout the first period. These estimates are presented in Panel D.

[22] An alternative approach to account for program duration would be to estimate a continuous treatment effect (Imbens, 2000). This framework, however, requires program duration to be determined before treatment start, while here we allow that program duration can be updated once in-between. We also consider repetitions of the same program type.

Overall, the results indicate that longer programs exhibit patterns similar to those observed for shorter or unrestricted sequences. Once again, *WS* outperforms all other programs, while extended or repeated participation in *JA* proves less advantageous than any other static or dynamic policy involving two programs of the same type. Compared to programs lasting only one period, longer programs tend to slightly narrow the differences in effectiveness. However, these reductions are modest, suggesting that program duration is unlikely to be the primary driver of the observed differences between programs. Finally, the comparison between static and dynamic policies reveals largely similar results. However, for sequences involving *WS*, static policies produce effects approximately half a month lower than dynamic policies. For example, under static policies, two periods of *WS* result in 2.9 months more employment than *JA*, whereas under dynamic policies, the difference increases to 3.4 months. This underscores that the choice of counterfactual can have economically significant implications.

4.7.2 Program Order

Besides duration or multiple participation, the sequential treatment effect framework can be exploited to obtain insights on the effective ordering of programs. As seen in Figure 2, a considerable number of individuals is assigned to different programs across the two periods. In such cases, it could be interesting to assess whether reversing the order of two programs leads to better outcomes. Table 3 presents the results of such an analysis. Panel A and B present the average treatment effects implementing a specific pair of programs, compared to implementing the same pair in reverse order, assuming static and dynamic policies, respectively.

The results for static policies indicate that when *WS* is combined with another program, it should be implemented as the second program rather than the first. This suggests that quitting a subsidized job to join an alternative program is less effective than first participating in the alternative program and then transitioning to the subsidized job. However, under dynamic policies, all effects, except for *JA-WS*, become smaller and statistically insignificant. Hence, the previous conclusion holds only if individuals remain unemployed for at least two periods, which is unknown a priori. Instead, when considering the possibility of reemployment during the first period, no clear superiority of any combination is observed. This suggests that conclusions from ALMP evaluations relying solely on static policies should be interpreted with careful consideration.[23]

[23] A challenge with analyzing the order of programs is that the second program may extend into subsequent periods, resulting again in comparisons of programs with differing durations. While a three-period setup with a final *NP* period, as in Lechner & Wiehler (2013), could address this issue, it is not used here due to our prior evidence that duration is unlikely to drive differences in effectiveness and the resulting smaller sample sizes leading to unstable estimates.

Table 3: Average treatment effects for program order.

	d_1 = JA d_1' = TC	d_1 = JA d_1' = EP	d_1 = JA d_1' = WS	d_1 = TC d_1' = EP	d_1 = TC d_1' = WS	d_1 = EP d_1' = WS
Panel A: Program order (static policy): $g_2(Y_1^{d_1}) = d_1'$, $g_2'(Y_1^{d_1'}) = d_1$						
ATE (BHL22)	0.76 (0.85)	11.71 (11.39)	0.72* (0.38)	-0.74 (0.84)	2.05*** (0.68)	1.97** (0.90)
ATE (BJZ24)	1.10 (0.89)	10.45 (10.33)	0.81** (0.38)	-0.66 (0.86)	2.19*** (0.65)	1.84** (0.85)
ATE (static conf.)	-0.42 (0.31)	-0.25 (1.21)	1.04*** (0.23)	-0.45 (0.59)	1.22*** (0.29)	1.30** (0.51)
$N_{\mathbf{g}_2}$	3,781 (97%)	3,845 (98%)	4,603 (97%)	1,735 (97%)	2,392 (96%)	1,774 (90%)
$N_{\mathbf{g}_2'}$	1,075 (95%)	108 (95%)	2,157 (95%)	402 (93%)	2,042 (95%)	2,062 (91%)
Panel B: Program order (dynamic policy): $g_2(Y_1^{d_1}) = \mathbf{1}\{Y_1^{d_1} = 0\}d_1' + \mathbf{1}\{Y_1^{d_1} = 1\}\text{NP}$ and $g_2'(Y_1^{d_1'}) = \mathbf{1}\{Y_1^{d_1'} = 0\}d_1 + \mathbf{1}\{Y_1^{d_1'} = 1\}\text{NP}$						
ATE (BHL22)	-0.44 (0.40)	5.78 (5.95)	0.41 (0.29)	-0.43 (0.72)	0.13 (0.23)	1.01 (0.90)
ATE (BJZ24)	-0.38 (0.40)	5.54 (5.71)	0.81** (0.37)	-0.51 (0.76)	0.27 (0.23)	0.99 (0.88)
$N_{\mathbf{g}_2}$	8,393 (97%)	8,453 (97%)	9,131 (97%)	3,674 (97%)	4,312 (97%)	3,534 (92%)
$N_{\mathbf{g}_2'}$	3,024 (97%)	1,930 (94%)	18,334 (94%)	2,217 (94%)	18,240 (94%)	18,276 (94%)

Note: This table presents ATEs and the number of observations for various comparisons of policies. d_1 and d_1' represent first-period programs in the treatment and control states, respectively, while $g_2(Y_1^{d_1})$ and $g_2'(Y_1^{d_1'})$ denote second-period policies dependent on the potential intermediate outcome $Y_1^{d_1}$ or $Y_1^{d_1'}$ (1 if an individual exits unemployment in the first period). JA: Job-search assistance, TC: Training course, EP: Employment program, WS: Temporary wage subsidy, NP: No program. Outcome: Cumulative months in employment in the 30 months from start of the second period. Rows labeled ATE report effect sizes with standard errors in parentheses. *, **, *** indicate p-values below 10%, 5%, and 1%. Row labeled N show the trimmed sample size and its proportion of the untrimmed sample (in parentheses).

4.7.3 Effect Heterogeneity

A key advantage of causal machine learning methods is that they allow to flexibly analyze effect heterogeneities. In the DML setting, heterogeneous effects can be obtained by aggregating the estimated scores $\hat{\Theta}_{\mathbf{g}_2}^{dy}$ for specific sub-groups of interest. Here, we focus on two types of heterogeneities: local language skill level and prior program participation in previous unemployment spells. Language skills, a proxy for migration background, are included since significant effect heterogeneity has been observed for this variable in multiple previous studies (e.g. Cockx, Lechner, & Bollens, 2023). Previous program participation is included to enable an even more detailed analysis of program combinations. Of course, the procedure could be applied to any other discrete pre-treatment characteristic, provided there is a sufficiently large sample size.

Panel A of Table 4 presents the results for heterogeneities based on local language knowledge.

Specifically, estimates of GATE-ATE are shown for dynamic policies (4.1) with $d_2 = d_1$, estimated by the method of Bodory et al. (2022). The effects are presented as the difference between GATE and ATE, where a significant deviation from zero indicates significant heterogeneity relative to the average.[24] The results indicate that for this type of long programs, *TC* is particularly effective for individuals fluent in the local language, whereas those with limited language skills benefit significantly less than the average. This finding holds true when comparing *TC* to each of the other programs. The result is surprising, given that *TC* includes language courses alongside various other training programs. The finding suggests that extended programs offering work experience may be more beneficial than extended training courses for individuals with limited language skills. No significant heterogeneity is found between the remaining programs.

Panel B of Table 4 presents the same analysis using information about unemployment spells in the five years prior to the program start. The results show that individuals who participated in *WS* during a previous unemployment spell benefit above-average from *TC* compared to further enrollment in *WS*. This finding suggest that repeated participation in *WS* across multiple unemployment spells leads to reduced program effectiveness. In addition, individuals who have not been unemployed in Switzerland before, profit less from *TC* compared to employment-related programs such as *EP* and *WS*. This observation aligns with the findings for local language knowledge, as recent immigrants are overrepresented among those experiencing first-time unemployment.

5 Conclusion

This paper reviewed, explained, and applied methods for the evaluation of program sequences and introduced the concept of dynamic policies to the econometric program evaluation literature. By summarizing the identification process under static and dynamic confounding, we demonstrated that assessing dynamic policies allows for the construction of counterfactuals with greater practical relevance, requiring only minor adjustments to identification assumptions. In addition, we illustrated how dynamic DML can be employed to flexibly estimate the effects of dynamic policies. The presented methods provide a foundation for more effective policy design in settings where program assignments depend on time-varying characteristics.

In our empirical application, we analyzed sequences of Swiss ALMP across two consecutive periods, starting at the beginning of the first program. An initial descriptive analysis revealed a

[24] As derived in Appendix B.2, standard errors for this difference can be computed as the square root of $\mathrm{Var}(\hat{\theta}(z_0) - \hat{\theta}) = \mathrm{Var}(\hat{\theta}(z_0)) + \mathrm{Var}(\hat{\theta}) - \frac{2N_{z_0}}{N}\mathrm{Var}(\hat{\theta}(z_0))$, where N_{z_0} denotes the number of observations with $Z_0 = z_0$.

Table 4: GATE-ATE by local language knowledge and previous program participation.

	d_1 = JA, d_1' = TC	d_1 = JA, d_1' = EP	d_1 = JA, d_1' = WS	d_1 = TC, d_1' = EP	d_1 = TC, d_1' = WS	d_1 = EP, d_1' = WS
	$g_2(Y_1^{d_1}) = \mathbf{1}\{Y_1^{d_1} = 0\}d_1 + \mathbf{1}\{Y_1^{d_1} = 1\}$NP; $g_2'(Y_1^{d_1'}) = \mathbf{1}\{Y_1^{d_1'} = 0\}d_1' + \mathbf{1}\{Y_1^{d_1'} = 1\}$NP					
Panel A: Local language knowledge						
None to basic	1.13**	0.75	0.04	-0.38	-1.09***	-0.72
	(0.46)	(0.61)	(0.41)	(0.53)	(0.27)	(0.48)
Intermediate	0.39	-0.57	-0.26	-0.96**	-0.65**	0.31
	(0.36)	(0.41)	(0.27)	(0.41)	(0.27)	(0.33)
Good	0.94	0.56	0.57	-0.38	-0.37	0.00
	(0.61)	(0.81)	(0.51)	(0.72)	(0.36)	(0.64)
Fluent	-0.58***	-0.10	-0.04	0.48**	0.54***	0.06
	(0.18)	(0.22)	(0.14)	(0.21)	(0.13)	(0.17)
Panel B: Unemployment (UE) and program participation in 5 years prior to current UE spell						
UE no program	-0.04	0.15	-0.12	0.19	-0.08	-0.27
	(0.20)	(0.23)	(0.15)	(0.22)	(0.14)	(0.18)
Not UE	0.92	-0.45	-0.01	-1.37*	-0.93**	0.44
	(0.60)	(0.74)	(0.41)	(0.77)	(0.46)	(0.63)
JA	-0.37	0.02	-0.26	0.38	0.10	-0.28
	(0.63)	(0.90)	(0.48)	(0.88)	(0.45)	(0.78)
TC	0.01	0.23	0.85*	0.22	0.84	0.62
	(0.72)	(0.61)	(0.46)	(0.72)	(0.59)	(0.45)
EP	-1.79*	-1.85*	-1.20	-0.06	0.59	0.65
	(1.09)	(1.07)	(0.99)	(0.63)	(0.49)	(0.45)
WS	-0.60	-0.06	0.32	0.54	0.92***	0.38
	(0.51)	(0.57)	(0.41)	(0.51)	(0.32)	(0.41)

Note: This table shows GATE-ATE by local language knowledge and previous program participation. Each column represents the comparison of two dynamic policies, where the first period program is continued in the second period if the individual remains unemployed in the first period (no program otherwise). d_1 and d_1' represent first-period programs in the treatment and control states, respectively, while $g_2(Y_1^{d_1})$ and $g_2'(Y_1^{d_1'})$ denote second-period policies dependent on the potential intermediate outcome $Y_1^{d_1}$ or $Y_1^{d_1'}$ (1 if an individual exits unemployment in the first period). JA: Job-search assistance, TC: Training course, EP: Employment program, WS: Temporary wage subsidy, NP: No program. Outcome: Cumulative months in employment in the 30 months from start of the second period. For each comparison of programs, the rows show the GATE-ATE and the standard errors in parentheses for the respective category of the heterogeneity variable. *, **, *** indicate the precision of the estimate by showing whether the p-value of a two-sided significance test is below 10%, 5%, and 1%, respectively.

significant variety of program sequences, highlighting the necessity of a sequential analysis. Using standard results from single time point interventions as a benchmark, we demonstrated how an assessment of sequential policies can provide additional insights into implementation details of ALMP. This revealed that *WS* is the most effective program on average, even after adjusting program duration across different program types. In particular, first-time unemployed and individuals with limited language skills profit more from programs related to obtaining work experience in comparison to extended training courses. In our application, disregarding dynamic confounding resulted in an overly optimistic assessment of longer programs while the choice between specific dynamic DML methods (Bodory et al. (2022) vs. Bradic et al. (2024)) did not appear to be of critical importance. Moreover, we found the choice between static and dynamic policies can lead to economically and statistically significant differences in estimated effect sizes.

Overall, DML-based estimation of effects of dynamic policies turns out to be a valuable addition to the standard program evaluation toolkit. A key limitation of any sequential analysis is the need for sufficient sample sizes for all sequences of interest to ensure robust estimation. This challenge is particularly pronounced for flexible machine learning methods, which avoid parametric structural assumptions but require large datasets to capture non-linear relationships. In our application, this constraint necessitated the aggregation of program categories into four major groups. This aggregation potentially masks distinct effects of individual sub-programs, limiting the detail of our findings. For the same reason, only a few relatively long periods could be used in the panel, which might not perfectly capture the true dynamic selection process. Although sequential methods are data-intensive, their potential is expected to increase as more extensive datasets become available. This advancement could enable extensions of the presented framework toward estimating more granular individualized treatment effects and developing personalized dynamic policy recommendations.

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A Estimators for Effects of Sequential Treatments

A.1 Conventional Estimators

To situate the machine learning-based estimators used in this paper in the broader methodological context, this section briefly overviews conventional estimation techniques commonly applied in the analysis of sequential treatments. For a comprehensive textbook introduction to these methods we refer to Hernán & Robins (2020).

The early literature proposed estimation of dynamic treatment effects based on the so-called g-formula. The g-formula is equivalent to identification result (2.5) discussed above. It is called g-formula as it is designed for the estimation of $g$eneralized treatment effects beyond the static case. The associated estimation method, referred to as g-computation (Robins, 1986), requires estimation of the mean outcomes for all treatment sequences of interest, conditional on covariates, as well as the estimation of conditional densities of all time-varying covariates. Originally, this has been implemented using a parametric plug-in procedure in which, for example, the conditional outcomes and (discrete) covariate distributions were estimated by linear and logistic regressions, respectively. Later it has been acknowledged that the latter can be avoided by exploiting a representation in terms of nested conditional expectations, which can be used as updated outcomes in an iterative estimation procedure (Robins, 2000; Murphy, 2003). Based on this idea, Lechner & Miquel (2010) propose propensity score matching, where instead of conditioning on the covariates, it is conditioned on the propensity scores.

Alternatively, causal effects of time-varying treatments can also be estimated using inverse probability weighting (IPW) (Robins et al., 2000), which goes back to Horvitz & Thompson (1952). Conceptually, while the g-formula mimics a case in which everyone receives the same treatment, the idea behind IPW is to re-establish a setting in which there is no confounding and everyone receives treatment at random (Robins & Hernán, 2008). Therefore, each observation in the sample is re-weighted by the inverse probability of receiving the treatment it actually got, given covariates. In the dynamic setting, the standard procedure is complicated by the occurrence of multiple treatments and their dependence on previous treatments and covariates. This leads to a product of propensity scores in the denominator of the weights, which might amplify the problem of extreme weights already known from the static setup. Note that in observational studies the propensity scores need to be estimated (e.g. using logistic regression), see Lechner (2009) for an application in the ALMP context.

For cases where the number of program sequences is large compared to the sample size, literature has drawn on structural mean models, which model the relationship between program assignment and the mean potential outcome. For example, a popular estimation approach based on Robins et al. (2000) are marginal structural models (MSM), which are specifically designed for cases with many periods and many levels of treatments. In its simplest form, an MSM estimates the expected potential outcome using a least-squares regression of the outcome on the cumulative number of treated periods in the pseudo-population reweighted by inverse probability weights. Another approach is the g-estimation of structural nested mean models (Robins, 1989, 1994). At each period, this approach models the effect of changing the treatment in that period, conditional on treatment and covariate history. Then, starting in the last period, the models are solved using a backward induction algorithm that recursively iterates outward and applies the sequential randomization assumption in each step. To simplify computation, a linear specification of the equations is typically assumed.

A further option to estimate dynamic treatment effects are doubly robust methods that combine the g-formula with IPW. When using parametric models for nuisance parameter estimation, doubly robust methods are useful as they remain consistent even if one of the parametric models is misspecified. Doubly robust estimation goes back to Robins et al. (1994) and Scharfstein, Rotnitzky, & Robins (1999) who showed that augmented inverse probability-weighting (AIPW) is doubly robust. Bang & Robins (2005) extended the procedure to the longitudinal setting and proposed to implement doubly robust estimation parametrically using inverse propensity scores as "clever covariates" in the nested outcome models. This procedure can be seen as a precursor of targeted minimum loss-based estimation, which is discussed in the following section. In the main body of this paper we focus on similar doubly-robust estimators that, however, do not rely on parametric models for the estimation of the nuisance functions.

A.2 Alternative EIF-based Machine Learning Estimators

Besides the DML-estimators discussed in Section 3, alternative EIF-based estimators have been proposed in the literature. Here we briefly outline these approaches and argue why they have not been implemented in our analysis.

Meza & Singh (2021) provide a very general framework, which applies to several longitudinal parameters such as sequential and mediated treatment effects or long-term effects using surrogates. Their paper can be seen as a sequential extension of Chernozhukov, Newey, & Singh (2023), and covers both ATEs and GATEs as special cases. Their theory provides a finite sample Gaussian approximation under regularity conditions, as well as $\sqrt{N}$-consistency and asymptotic normality under assumptions on the learning rate of the nuisance estimates, similar to Bodory et al. (2022). The authors do not commit to a specific estimator for the nested conditional outcome $\nu_{\mathbf{g}_2}(X_0)$, as long as it converges at a fast enough rate. However, they provide estimation theory for an adversarial nested instrumental variable regression procedure that avoids using $\hat{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$ as a pseudo-outcome. Hence, no additional sample splits within the cross-fitting folds are needed, which becomes increasingly relevant as the number of time periods increases. The following application does not consider their adversarial estimator, given that it remains within a setting limited to a maximum of two time periods and does not require instrumental variables.

Chernozhukov, Newey, Singh, & Syrgkanis (2022) propose to estimate the ATE under dynamic confounding using automatic DML (Auto-DML). Instead of directly using the EIF (3.1), Auto-DML is based on the more general score function

$$\Theta^{\text{AUTO}}_{\mathbf{g}_2}(\mathbf{W}) := \nu_{\mathbf{g}_2}(X_0) + a_1(D_1, X_0)\left(\mu_{\mathbf{g}_2}(\mathbf{X}_1) - \nu_{\mathbf{g}_2}(X_0)\right) + a_2(\mathbf{D}_2, \mathbf{X}_1)\left(Y - \mu_{\mathbf{g}_2}(\mathbf{X}_1)\right),$$

where the weights a_1 and a_2 denote the recursive Riesz representer functions. This representation is guaranteed to exist if treatments are discrete and the overlap assumption holds (Chernozhukov, Newey, & Singh, 2022). While the previously proposed estimators exploit closed-form solutions of a_1 and a_2, and plug-in estimates of the propensity scores $\hat{p}_{g_1}$ and $\hat{p}_{g_2}$, Auto-DML avoids using the analytical form and learns the Riesz representers $\hat{a}_t$ directly from numerical optimization. As the authors argue, this leads to improved behavior of the estimated weights even in settings where the functional form is known, since plugging-in estimated probabilities in the denominators of the weights can be avoided. Similar to the nested outcomes, a particular difficulty is that only the Riesz representer of the final period is directly identified from the data. For earlier periods, a_t must be learned based on the estimated representer from the previous period, which complicates the extension of the procedures developed for single time periods. While Chernozhukov, Newey, Singh, & Syrgkanis (2022) provide promising theoretical results, unsolved questions remain regarding the practical implementation of Auto-DML in the sequential setting. For example, it is unclear which estimators for the nested conditional outcomes and nested Riesz representers satisfy the theoretical requirements. In addition, the performance of the approach in finite samples is not yet well understood. Finally, the literature is still lacking empirical applications of Auto-DML even in the non-sequential setting, which leads us to consider the procedure too preliminary for our purposes.

A method closely related to DML is targeted minimum loss-based estimation (TMLE), originally proposed in Van Der Laan & Rubin (2006). In the basic setting with static policies and static confounding, TMLE updates the initial (biased) prediction of the conditional outcome $\hat{\mu}_{\mathbf{d}_2}(X_0)$ using the adjustment

$$\tilde{\mu}_{\mathbf{d}_2}(X_0) = \hat{\mu}_{\mathbf{d}_2}(X_0) + \underbrace{\frac{\frac{1}{N}\sum_{j=1}^{N} \hat{\alpha}(X_{0j}, \mathbf{D}_{2j})\left(Y_j - \hat{\mu}_{\mathbf{d}_2}(X_{0j})\right)}{\frac{1}{N}\sum_{j=1}^{N} \hat{\alpha}(X_{0j}, \mathbf{D}_{2j})^2}}_{=:\hat{\epsilon}} \hat{\alpha}(X_0, \mathbf{d}_2),$$

with $\hat{\alpha}(X_0, \mathbf{D}_2) = \mathbf{1}\{\mathbf{D}_2 = \mathbf{d}_2\}/\hat{p}_{\mathbf{d}_2}(X_0)$. Hence, $\tilde{\mu}_{\mathbf{d}_2}(X_0)$ corresponds to the conditional outcome $\hat{\mu}_{\mathbf{d}_2}(X_0)$ plus the predicted value of a regression of the residual $Y - \hat{\mu}_{\mathbf{d}_2}(X_0)$ on the "clever covariate" $\hat{\alpha}(X_0, \mathbf{D}_2)$ with regression coefficient $\hat{\epsilon}$, evaluated at $\mathbf{D}_2 = \mathbf{d}_2$. Conceptually, the key distinction between DML and TMLE lies in their approach to obtaining an estimator where the EIF equals zero. DML imposes this condition upfront and derives the de-biased estimator by solving the moment condition. In contrast, TMLE starts with the original prediction of the conditional outcome and fluctuates it until the

EIF is zero and no further de-biasing is needed. This can be seen from plugging $\tilde{\mu}_{\mathbf{d}_2}(X_{0i})$ for $\hat{\mu}_{\mathbf{d}_2}(X_{0i})$ into the empirical EIF (3.2), which results in a zero de-biasing term, i.e.

$$\tilde{\theta}^{\mathbf{g}_2} = \frac{1}{N}\sum_{i=1}^{N} \tilde{\mu}_{\mathbf{d}_2}(X_{0i}) + \frac{(Y_i - \tilde{\mu}_{\mathbf{d}_2}(X_{0i})) \cdot \mathbf{1}\{\mathbf{D}_{2i} = \mathbf{d}_2\}}{\hat{p}_{\mathbf{d}_2}(X_{0i})} = \frac{1}{N}\sum_{i=1}^{N} \tilde{\mu}_{\mathbf{d}_2}(X_{0i}).$$

Hence, the target parameter estimate can be directly obtained by averaging $\tilde{\mu}_{\mathbf{d}_2}(X_{0i})$ over the population of interest. DML and cross-fitted TMLE share the same statistical properties and are asymptotically equivalent, as shown in Chernozhukov, Newey, & Singh (2022). However, TMLE might be more stable than DML in finite samples since the working model on the "clever covariate" can be exploited to impose global constraints, for example if the outcome Y is bounded in some interval (Kennedy, 2024).

TMLE has been extended to dynamic policies and dynamic confounding, referred to as longitudinal TMLE (LTMLE). Similar to DML, the estimated (and targeted) conditional mean outcome of the second period, $\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$, is employed as a pseudo-outcome in a regression on X_0 within the subset $D_1 = g_1(V_0)$. This produces an estimate $\hat{\nu}_{\mathbf{g}_2}(X_0)$, which is subsequently targeted again to yield $\tilde{\nu}_{\mathbf{g}_2}(X_0)$. In direct extension of the previous exposition, the targeted estimates $\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$ and $\tilde{\nu}_{\mathbf{g}_2}(X_0)$ lead to a zero de-biasing term when plugged into the empirical EIF (3.2). Tran et al. (2019) show that LTMLE outperforms competing estimators of sequential effects in a simulation study. However, to the best of our knowledge, no cross-fitted version of LTMLE currently exists, and the optimal approach for implementing sample splitting in the longitudinal context remains unclear. Hence, the procedure is prone to overfitting, unless restrictive complexity constraints on the nuisance estimators are met, which exclude commonly applied machine learning estimators such as lasso or random forests (Kennedy, 2024). For this reason, the method is not adopted in the current study.

Finally, Lewis & Syrgkanis (2021) introduce DML for effect estimation of sequential treatments as an extension of the classical g-estimation of structural nested mean models (Robins, 1989, 1994). Unlike the procedures discussed in the previous section, their approach is not directly based on the identification result (2.8) but instead relies on an expansion of the potential outcome,

$$Y^{\mathbf{g}_2} = Y^{g_1,g_2} = Y + \Big(Y^{g_2} - Y\Big) + \Big(Y^{g_1,g_2} - Y^{g_2}\Big),$$

with $Y^{g_2} := \sum_{\mathbf{d}_2 \in \mathcal{D}_1 \times \mathcal{D}_2} \mathbf{1}\{D_1 = d_1, g_2(\mathbf{V}_1) = d_2\} Y^{\mathbf{d}_2}$. Hence, the target potential outcome is obtained from the observed outcome by adding the 'blip effect' of changing only the last program plus the 'blip effect' of switching the first program after having already switched the second program. This observation motivates a backward induction algorithm, which recursively estimates the 'blip effects' in a Neyman orthogonal manner using cross-fitting. Starting in the final period, the 'blip effect' is estimated and subtracted from the outcome to derive a new adjusted outcome that reflects the effect of the counterfactual program rather than the observed program in the final period. The adjusted outcome is subsequently used to estimate the 'blip effect' of changing programs in the previous period. While the 'blip effects' are non-parametrically identified by applying Assumption 2.4 at each stage, a linear parametric form is imposed on them for estimation. This eliminates the need to weight by inverse products of estimated propensity scores while ensuring $\sqrt{N}$ convergence and asymptotic normality, even when nuisance functions are estimated using machine learning methods. Their method is not used in this study due to the restrictive nature of the parametric assumptions. Nevertheless, the additional structure provided by their approach may be beneficial in scenarios involving many or continuous treatments and many time periods, where non-parametric approaches become infeasible.

B Proofs

B.1 Doubly Robust Identification

In this section, we prove identification of our target parameters $\theta^{\mathbf{g}_2}$ and $\theta^{\mathbf{g}_2}(z_0)$ by the score $\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})$ and show that it fulfills the double robustness property. We follow the proof of Lemma 1 in Bradic et al. (2024), which is consistent with earlier results by Bang & Robins (2005).

In what follows we want to show

$$
\begin{aligned}
\theta^{\mathbf{g}_2} &= \mathbb{E}[Y^{\mathbf{g}_2}] = \mathbb{E}_{X_0}[\mathbb{E}[Y^{\mathbf{g}_2}|X_0]] = \mathbb{E}_{X_0}[\mathbb{E}[\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})|X_0]] = \mathbb{E}[\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})] \\
\theta^{\mathbf{g}_2}(z_0) &= \mathbb{E}[Y^{\mathbf{g}_2}|Z_0 = z_0] = \mathbb{E}_{X_0}[\mathbb{E}[Y^{\mathbf{g}_2}|X_0]|Z_0 = z_0] = \mathbb{E}_{X_0}[\mathbb{E}[\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})|X_0]|Z_0 = z_0] \\
&= \mathbb{E}[\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})|Z_0 = z_0].
\end{aligned}
$$

Both statements hold true if $\mathbb{E}[Y^{\mathbf{g}_2}|X_0] = \mathbb{E}[\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})|X_0 = x_0]$. We can write

$$
\begin{aligned}
\mathbb{E}[\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})|X_0 = x_0] = \nu_{\mathbf{g}_2}(x_0) & \\
&+ \mathbb{E}\left[\frac{(Y - \mu_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{\mathbf{D}_2 = \mathbf{g}_2(\mathbf{V}_1)\}}{p_{g_2}(\mathbf{X}_1, g_1)p_{g_1}(X_0)}\middle| X_0 = x_0\right] \\
&+ \mathbb{E}\left[\frac{(\mu_{\mathbf{g}_2}(\mathbf{X}_1) - \nu_{\mathbf{g}_2}(X_0))\,\mathbf{1}\{D_1 = g_1(V_0)\}}{p_{g_1}(X_0)}\middle| X_0 = x_0\right].
\end{aligned}
$$

From statement 2.4 we know $\nu_{\mathbf{g}_2}(x_0) = \mathbb{E}[Y^{\mathbf{g}_2}|X_0]$. Hence, to prove identification it suffices to show that the second and third term are zero. For the second term we find

$$
\begin{aligned}
&\mathbb{E}\left[\frac{(Y-\mu_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{\mathbf{D}_2=\mathbf{g}_2(\mathbf{V}_1)\}}{p_{g_2}(\mathbf{X}_1,g_1)p_{g_1}(X_0)}\middle|X_0=x_0\right]\\
&=\mathbb{E}_{D_1}\left[\mathbb{E}\left[\frac{(Y-\mu_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{\mathbf{D}_2=\mathbf{g}_2(\mathbf{V}_1)\}}{p_{g_2}(\mathbf{X}_1,g_1)p_{g_1}(X_0)}\middle|X_0=x_0,D_1\right]\middle|X_0=x_0\right]\\
&=\Pr(D_1=g_1(V_0)|X_0=x_0)\mathbb{E}\left[\frac{(Y-\mu_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{D_2=g_2(\mathbf{V}_1)\}}{p_{g_2}(\mathbf{X}_1,g_1)p_{g_1}(X_0)}\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\mathbb{E}\left[\frac{(Y-\mu_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{D_2=g_2(\mathbf{V}_1)\}}{p_{g_2}(\mathbf{X}_1,g_1)}\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\mathbb{E}_{X_1}\left[\mathbb{E}\left[\frac{(Y-\mu_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{D_2=g_2(\mathbf{V}_1)\}}{p_{g_2}(\mathbf{X}_1,g_1)}\middle|\mathbf{X}_1,D_1=g_1(V_0)\right]\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\mathbb{E}_{X_1}\left[\Pr(D_2=g_2(\mathbf{V}_1)|\mathbf{X}_1,D_1=g_1(V_0))\mathbb{E}\left[\frac{Y-\mu_{\mathbf{g}_2}(\mathbf{X}_1)}{p_{g_2}(\mathbf{X}_1,g_1)}\middle|\mathbf{X}_1,\mathbf{D}_2=\mathbf{g}_2(\mathbf{V}_1)\right]\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\mathbb{E}_{X_1}\left[\mathbb{E}\left[Y-\mu_{\mathbf{g}_2}(\mathbf{X}_1)|\mathbf{X}_1,\mathbf{D}_2=\mathbf{g}_2(\mathbf{V}_1)\right]\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\mathbb{E}_{X_1}\left[\mathbb{E}\left[Y|\mathbf{X}_1,\mathbf{D}_2=\mathbf{g}_2(\mathbf{V}_1)\right]-\mu_{\mathbf{g}_2}(\mathbf{X}_1)\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=0.
\end{aligned}
$$

For the third term we find

$$
\begin{aligned}
&\mathbb{E}\left[\frac{(\mu_{\mathbf{g}_2}(\mathbf{X}_1)-\nu_{\mathbf{g}_2}(X_0))\,\mathbf{1}\{D_1=g_1(V_0)\}}{p_{g_1}(X_0)}\middle|X_0=x_0\right]\\
&=\mathbb{E}_{D_1}\left[\mathbb{E}_{X_1}\left[\frac{(\mu_{\mathbf{g}_2}(\mathbf{X}_1)-\nu_{\mathbf{g}_2}(X_0))\,\mathbf{1}\{D_1=g_1(V_0)\}}{p_{g_1}(X_0)}\middle|X_0=x_0,D_1\right]\middle|X_0=x_0\right]\\
&=\Pr(D_1=g_1(V_0)|X_0=x_0)\mathbb{E}_{X_1}\left[\frac{\mu_{\mathbf{g}_2}(\mathbf{X}_1)-\nu_{\mathbf{g}_2}(X_0)}{p_{g_1}(X_0)}\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\mathbb{E}_{X_1}\left[\mu_{\mathbf{g}_2}(\mathbf{X}_1)-\nu_{\mathbf{g}_2}(X_0)|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\mathbb{E}_{X_1}\left[\mu_{\mathbf{g}_2}(\mathbf{X}_1)|X_0=x_0,D_1=g_1(V_0)\right]-\nu_{\mathbf{g}_2}(x_0)\\
&=0.
\end{aligned}
$$

Hence, $\mathbb{E}[Y^{\mathbf{g}_2}|X_0]=\mathbb{E}[\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})|X_0=x_0]$ which completes the proof. We can also show that the score $\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})$ possesses so-called multiple robustness properties. Therefore, we replace the true functions $\mu_{\mathbf{g}_2}(\mathbf{X}_1)$, $\nu_{\mathbf{g}_2}(X_0)$, $p_{g_1}(X_0)$ and $p_{g_2}(\mathbf{X}_1,g_1)$ in the second and third term by arbitrary functions $\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)$,

$\tilde{\nu}_{\mathbf{g}_2}(X_0)$, $\tilde{p}_{g_1}(X_0)$ and $\tilde{p}_{g_2}(\mathbf{X}_1, g_1)$. For the second we find

$$
\begin{aligned}
&\mathbb{E}\left[\frac{(Y-\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{\mathbf{D}_2=\mathbf{g}_2(\mathbf{V}_1)\}}{\tilde{p}_{g_2}(\mathbf{X}_1,g_1)\tilde{p}_{g_1}(X_0)}\middle|X_0=x_0\right]\\
&=\mathbb{E}_{D_1}\left[\mathbb{E}\left[\frac{(Y-\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{\mathbf{D}_2=\mathbf{g}_2(\mathbf{V}_1)\}}{\tilde{p}_{g_2}(\mathbf{X}_1,g_1)\tilde{p}_{g_1}(X_0)}\middle|X_0=x_0,D_1\right]\middle|X_0=x_0\right]\\
&=\Pr(D_1=g_1(V_0)|X_0=x_0)\mathbb{E}\left[\frac{(Y-\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{D_2=g_2(\mathbf{V}_1)\}}{\tilde{p}_{g_2}(\mathbf{X}_1,g_1)\tilde{p}_{g_1}(X_0)}\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)}\mathbb{E}\left[\frac{(Y-\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{D_2=g_2(\mathbf{V}_1)\}}{\tilde{p}_{g_2}(\mathbf{X}_1,g_1)}\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)}\mathbb{E}_{X_1}\left[\mathbb{E}\left[\frac{(Y-\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1))\,\mathbf{1}\{D_2=g_2(\mathbf{V}_1)\}}{\tilde{p}_{g_2}(\mathbf{X}_1,g_1)}\middle|\mathbf{X}_1,D_1=g_1(V_0)\right]\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)}\mathbb{E}_{X_1}\left[\Pr(D_2=g_2(\mathbf{V}_1)|\mathbf{X}_1,D_1=g_1(V_0))\mathbb{E}\left[\frac{Y-\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)}{\tilde{p}_{g_2}(\mathbf{X}_1,g_1)}\middle|\mathbf{X}_1,\mathbf{D}_2=\mathbf{g}_2(\mathbf{V}_1)\right]\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)}\mathbb{E}_{X_1}\left[\frac{p_{g_2}(\mathbf{X}_1,g_1)}{\tilde{p}_{g_2}(\mathbf{X}_1,g_1)}\left(\mathbb{E}\left[Y|\mathbf{X}_1,\mathbf{D}_2=\mathbf{g}_2(\mathbf{V}_1)\right]-\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)\right)\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)}\mathbb{E}_{X_1}\left[\frac{p_{g_2}(\mathbf{X}_1,g_1)}{\tilde{p}_{g_2}(\mathbf{X}_1,g_1)}\left(\mu_{\mathbf{g}_2}(\mathbf{X}_1)-\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)\right)\middle|X_0=x_0,D_1=g_1(V_0)\right]
\end{aligned}
$$

and for the third term we find

$$
\begin{aligned}
&\mathbb{E}\left[\frac{(\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)-\tilde{\nu}_{\mathbf{g}_2}(X_0))\,\mathbf{1}\{D_1=g_1(V_0)\}}{\tilde{p}_{g_1}(X_0)}\middle|X_0=x_0\right]\\
&=\mathbb{E}_{D_1}\left[\mathbb{E}_{X_1}\left[\frac{(\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)-\tilde{\nu}_{\mathbf{g}_2}(X_0))\,\mathbf{1}\{D_1=g_1(V_0)\}}{\tilde{p}_{g_1}(X_0)}\middle|X_0=x_0,D_1\right]\middle|X_0=x_0\right]\\
&=\Pr(D_1=g_1(V_0)|X_0=x_0)\mathbb{E}_{X_1}\left[\frac{\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)-\tilde{\nu}_{\mathbf{g}_2}(X_0)}{\tilde{p}_{g_1}(X_0)}\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&=\frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)}\mathbb{E}_{X_1}\left[\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)-\tilde{\nu}_{\mathbf{g}_2}(x_0)|X_0=x_0,D_1=g_1(V_0)\right].
\end{aligned}
$$

Hence, the conditional-on-X_0 expected score can be rewritten as

$$
\begin{aligned}
\mathbb{E}[\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})|X_0=x_0]=\;&\tilde{\nu}_{\mathbf{g}_2}(x_0)\\
&+\frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)}\mathbb{E}_{X_1}\left[\frac{p_{g_2}(\mathbf{X}_1,g_1)}{\tilde{p}_{g_2}(\mathbf{X}_1,g_1)}\left(\mu_{\mathbf{g}_2}(\mathbf{X}_1)-\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)\right)\middle|X_0=x_0,D_1=g_1(V_0)\right]\\
&+\frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)}\mathbb{E}_{X_1}\left[\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)-\tilde{\nu}_{\mathbf{g}_2}(x_0)|X_0=x_0,D_1=g_1(V_0)\right].
\end{aligned}
$$

Plugging in the telescoping sums $\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)-\tilde{\nu}_{\mathbf{g}_2}(x_0)=\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)-\mu_{\mathbf{g}_2}(\mathbf{X}_1)+\mu_{\mathbf{g}_2}(\mathbf{X}_1)-\tilde{\nu}_{\mathbf{g}_2}(x_0)$ and

$\tilde{\nu}_{\mathbf{g}_2}(x_0) = \nu_{\mathbf{g}_2}(x_0) - (\nu_{\mathbf{g}_2}(x_0) - \tilde{\nu}_{\mathbf{g}_2}(x_0))$ and rearranging we get

$$
\begin{aligned}
\mathbb{E}[\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})|X_0 = x_0] &= \nu_{\mathbf{g}_2}(x_0) - (\nu_{\mathbf{g}_2}(x_0) - \tilde{\nu}_{\mathbf{g}_2}(x_0)) \\
&+ \frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)} \mathbb{E}_{X_1}\left[\frac{p_{g_2}(\mathbf{X}_1, g_1)}{\tilde{p}_{g_2}(\mathbf{X}_1, g_1)} (\mu_{\mathbf{g}_2}(\mathbf{X}_1) - \tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)) \middle| X_0 = x_0, D_1 = g_1(V_0)\right] \\
&+ \frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)} \mathbb{E}_{X_1}\left[\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1) - \mu_{\mathbf{g}_2}(\mathbf{X}_1) | X_0 = x_0, D_1 = g_1(V_0)\right] \\
&+ \frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)} \mathbb{E}_{X_1}\left[\mu_{\mathbf{g}_2}(\mathbf{X}_1) - \tilde{\nu}_{\mathbf{g}_2}(x_0) | X_0 = x_0, D_1 = g_1(V_0)\right] \\
&= \nu_{\mathbf{g}_2}(x_0) - (\nu_{\mathbf{g}_2}(x_0) - \tilde{\nu}_{\mathbf{g}_2}(x_0)) \\
&+ \frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)} \mathbb{E}_{X_1}\left[\left(\frac{p_{g_2}(\mathbf{X}_1, g_1)}{\tilde{p}_{g_2}(\mathbf{X}_1, g_1)} - 1\right) (\mu_{\mathbf{g}_2}(\mathbf{X}_1) - \tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)) \middle| X_0 = x_0, D_1 = g_1(V_0)\right] \\
&+ \frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)} (\nu_{\mathbf{g}_2}(x_0) - \tilde{\nu}_{\mathbf{g}_2}(x_0)) \\
&= \nu_{\mathbf{g}_2}(x_0) \\
&+ \frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)} \mathbb{E}_{X_1}\left[\left(\frac{p_{g_2}(\mathbf{X}_1, g_1)}{\tilde{p}_{g_2}(\mathbf{X}_1, g_1)} - 1\right) (\mu_{\mathbf{g}_2}(\mathbf{X}_1) - \tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1)) \middle| X_0 = x_0, D_1 = g_1(V_0)\right] \\
&+ \left(\frac{p_{g_1}(x_0)}{\tilde{p}_{g_1}(x_0)} - 1\right) (\nu_{\mathbf{g}_2}(x_0) - \tilde{\nu}_{\mathbf{g}_2}(x_0)).
\end{aligned}
$$

From this expression we obtain $\mathbb{E}[\Theta^{dy}_{\mathbf{g}_2}(\mathbf{W})|X_0 = x_0] = \nu_{\mathbf{g}_2}(x_0)$ if

- either $\tilde{p}_{g_2}(\mathbf{X}_1, g_1) = p_{g_2}(\mathbf{X}_1, g_1)$ or $\tilde{\mu}_{\mathbf{g}_2}(\mathbf{X}_1) = \mu_{\mathbf{g}_2}(\mathbf{X}_1)$ such that the second term equals zero

and

- either $\tilde{p}_{g_1}(X_0) = p_{g_1}(X_0)$ or $\tilde{\nu}_{\mathbf{g}_2}(X_0) = \nu_{\mathbf{g}_2}(x_0)$ such that the third term equals zero.

B.2 GATE-ATE Variance

Let $k \in \{1, ..., K\}$ denote one of K discrete outcomes of the variables Z_0 and $N_k = \sum_{i=1}^{N} \mathbf{1}\{Z_{0i} = k\}$ the number of observations corresponding to the realization k. Denote the estimate of the ATE as $\hat{\theta}$ and of the GATE in group k as $\hat{\theta}(k)$. Then we compute the variance of the difference $\hat{\theta}(k) - \hat{\theta}$ as

$$
\begin{aligned}
\mathrm{Var}(\hat{\theta}(k) - \hat{\theta}) &= \mathrm{Var}(\hat{\theta}(k)) + \mathrm{Var}(\hat{\theta}) - 2\mathrm{Cov}(\hat{\theta}(k), \hat{\theta}) \\
&= \mathrm{Var}(\hat{\theta}(k)) + \mathrm{Var}(\hat{\theta}) - 2N_k/N\, \mathrm{Var}(\hat{\theta}(k))
\end{aligned}
$$

since

$$
\begin{aligned}
\operatorname{Cov}(\hat{\theta}(k), \hat{\theta}) &= \operatorname{Cov}(\hat{\theta}(k), \sum_{j=1}^{K} \frac{N_j}{N} \hat{\theta}(j)) \\
&= \sum_{j=1}^{K} \frac{N_j}{N} \operatorname{Cov}(\hat{\theta}(k), \hat{\theta}(j)) \\
&= \frac{N_k}{N} \operatorname{Cov}(\hat{\theta}(k), \hat{\theta}(k)) \\
&= N_k/N \operatorname{Var}(\hat{\theta}(k)).
\end{aligned}
$$

The first equality follows from the definition of the ATE, the second equality follows from the properties of the covariance, the third equality follows from independence between GATEs for different groups (because observations are *iid* and groups are mutually exclusive) and the last equality follows from the definition of the variance.

C Descriptive Statistics

C.1 Covariate and Outcome Means by Program sequence

To assess covariate and outcome differences between treatment groups we compute standardized differences

$$\Delta = \frac{\left|\bar{X}_{\mathbf{d}_2} - \bar{X}_{\mathbf{d}_2'}\right|}{\sqrt{1/2\left(\mathrm{Var}(X_{\mathbf{d}_2}) + \mathrm{Var}(X_{\mathbf{d}_2'})\right)}} \cdot 100,$$

where $\bar{X}_{\mathbf{d}_2}$ and $\mathrm{Var}(X_{\mathbf{d}_2})$ indicate the sample mean and variance of variable X in the subgroup with $\mathbf{D}_2 = \mathbf{d}_2$. As the standardized difference is independent of sample size it is preferred over a t-test to compare the balance of baseline covariates across treatment groups. Imbalance is typically defined as an absolute value greater than 20 (Imbens & Rubin, 2015; Yang & Dalton, 2012).

Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
Avg add. income in months ue 1y before ref. point	15.61	16.88	18.07	30.49	17.50	16.20	21.16	23.87	50.22	24.18	13.32	32.24	24.38	50.44	38.12	57.24	63.66	72.08	116.78	99.41
	(1.23)	(0.38)	(0.35)	(6.94)		(5.07)	(1.66)	(0.16)	(11.08)		(13.78)	(2.58)	(6.62)	(5.00)		(10.63)	(9.18)	(6.97)	(3.70)	
Age at start of unemployment	39.90	40.22	39.40	39.00	39.07	40.81	38.74	39.52	39.22	39.29	39.43	39.32	39.43	38.84	38.53	38.76	39.07	38.64	39.50	37.80
	(9.47)	(13.18)	(3.79)	(0.76)		(18.11)	(6.46)	(2.71)	(0.78)		(10.46)	(9.02)	(10.00)	(3.41)		(10.93)	(14.58)	(9.43)	(19.32)	
Age at 1st income subj. to Swiss social ins. contrib.	24.11	24.37	23.47	22.99	22.75	24.80	26.75	25.40	25.74	24.71	23.23	25.06	24.33	24.20	23.38	23.51	26.53	24.31	23.74	23.12
	(18.90)	(22.48)	(10.12)	(3.58)		(1.25)	(25.34)	(8.67)	(13.10)		(2.25)	(23.07)	(13.15)	(11.59)		(5.59)	(43.69)	(16.18)	(8.59)	
Avg monthly applications in previous ue spells	2.61	2.27	3.02	2.84	2.68	2.68	2.48	2.82	3.10	2.81	2.88	3.99	3.35	4.11	3.93	2.55	2.60	3.46	2.98	3.01
	(1.72)	(10.48)	(8.36)	(3.78)		(3.45)	(8.40)	(0.22)	(6.95)		(27.62)	(1.47)	(14.29)	(4.35)		(12.12)	(10.79)	(11.58)	(0.79)	
Avg monthly applications in year before ref. point	4.04	3.60	4.59	4.34	4.23	4.47	3.69	4.53	4.44	4.24	4.93	5.66	5.23	5.92	5.66	3.41	3.25	4.38	3.67	3.77
	(4.37)	(14.81)	(8.56)	(2.59)		(5.59)	(12.77)	(6.89)	(4.63)		(19.00)	(0.08)	(11.08)	(6.91)		(8.73)	(12.70)	(14.92)	(2.63)	
Age of youngest child in month before ref. point																				
0-3	*0.06*	*0.06*	*0.06*	*0.06*	*0.06*	*0.05*	*0.06*	*0.05*	*0.04*	*0.05*	*0.04*	*0.08*	*0.06*	*0.06*	*0.07*	*0.03*	*0.03*	*0.03*	*0.06*	*0.04*
	(2.86)	*(1.98)*	*(0.65)*	*(3.24)*		*(2.13)*	*(2.66)*	*(0.27)*	*(6.55)*		*(13.05)*	*(5.29)*	*(2.91)*	*(3.24)*		*(4.10)*	*(3.92)*	*(3.82)*	*(9.13)*	
3-6	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.03*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.03*	*0.02*
	(0.66)	*(0.48)*	*(0.73)*	*(0.37)*		*(4.68)*	*(0.56)*	*(1.49)*	*(2.78)*		*(3.54)*	*(2.54)*	*(0.44)*	*(1.87)*		*(1.04)*	*(2.33)*	*(0.73)*	*(4.50)*	
6-10	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.02*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*
	(0.70)	*(0.27)*	*(3.27)*	*(0.46)*		*(0.88)*	*(2.33)*	*(4.04)*	*(1.16)*		*(4.79)*	*(4.19)*	*(1.28)*	*(4.47)*		*(3.72)*	*(2.22)*	*(1.66)*	*(4.05)*	
No children	*0.03*	*0.05*	*0.04*	*0.05*	*0.05*	*0.04*	*0.06*	*0.05*	*0.06*	*0.06*	*0.04*	*0.03*	*0.05*	*0.04*	*0.05*	*0.05*	*0.05*	*0.06*	*0.06*	*0.07*
	(8.60)	*(1.18)*	*(6.76)*	*(3.22)*		*(10.07)*	*(1.46)*	*(3.47)*	*(1.28)*		*(5.66)*	*(11.08)*	*(0.22)*	*(2.44)*		*(7.60)*	*(4.82)*	*(4.00)*	*(3.94)*	
Unknown	*0.87*	*0.85*	*0.87*	*0.86*	*0.85*	*0.87*	*0.85*	*0.87*	*0.87*	*0.85*	*0.90*	*0.85*	*0.86*	*0.87*	*0.85*	*0.89*	*0.88*	*0.88*	*0.84*	*0.87*
	(7.11)	*(1.78)*	*(5.11)*	*(3.86)*		*(5.77)*	*(0.15)*	*(4.25)*	*(6.38)*		*(15.20)*	*(1.59)*	*(2.34)*	*(5.88)*		*(5.96)*	*(5.93)*	*(4.17)*	*(6.08)*	
Avg net ue benefits in months ue 1y before ref. point	2,246.65	2,018.87	1,893.87	2,009.65	2,285.15	2,311.49	2,124.59	2,198.47	2,140.14	2,419.80	2,323.79	2,294.01	2,300.87	2,240.72	2,288.48	1,186.20	1,084.64	1,324.32	1,234.55	1,361.59
	(2.25)	(16.12)	(24.43)	(17.08)		(6.83)	(18.53)	(14.90)	(18.44)		(2.58)	(0.41)	(0.89)	(3.56)		(12.04)	(19.39)	(2.57)	(8.49)	
Cum. state subsidies in year before ref. point (in CHF 1,000)	10.81	7.52	7.59	8.34	10.33	10.09	8.99	9.87	9.39	11.78	12.38	10.82	12.78	11.42	13.28	5.40	5.08	7.19	6.51	7.41
	(3.95)	(26.75)	(26.33)	(17.31)		(15.07)	(24.92)	(17.54)	(21.23)		(7.80)	(22.59)	(4.05)	(16.36)		(21.50)	(25.13)	(2.17)	(8.77)	
Cumulative state subsidies in 2nd year before ref. point	1,806.25	1,193.87	1,630.29	1,760.14	1,616.25	1,340.36	1,526.67	1,613.09	2,569.22	2,327.85	2,157.92	1,735.06	2,349.63	3,468.49	3,120.56	2,652.12	2,710.66	3,648.78	4,047.39	4,322.43
	(2.58)	(6.57)	(0.20)	(2.02)		(13.20)	(10.70)	(9.82)	(2.81)		(12.11)	(18.00)	(9.10)	(3.81)		(16.79)	(16.16)	(6.35)	(2.47)	
Cumulative state subsidies in 3rd year before ref. point	3,662.61	2,548.41	3,288.19	3,244.07	2,955.05	2,789.46	2,856.17	3,208.78	3,765.04	3,409.02	5,295.96	2,618.64	4,342.24	4,837.90	4,510.17	4,056.13	3,573.02	5,515.54	5,197.78	4,814.60
	(6.64)	(4.26)	(3.31)	(2.88)		(6.18)	(5.37)	(1.97)	(3.33)		(6.62)	(19.48)	(1.51)	(2.95)		(6.69)	(11.24)	(5.73)	(3.13)	
Avg monthly state subsidies 5y before reference point	414.54	298.40	350.80	356.54	379.81	363.57	347.55	380.50	394.47	430.77	537.69	396.85	495.44	506.25	511.60	339.95	306.49	450.89	424.68	424.82
	(6.63)	(17.28)	(5.86)	(4.62)		(13.84)	(16.86)	(10.34)	(7.03)		(4.60)	(22.27)	(2.96)	(0.98)		(15.20)	(21.59)	(4.43)	(0.02)	
Avg monthly state subsidies 7y before start of ref. point	390.56	285.51	341.67	338.92	353.22	342.21	324.31	357.84	363.84	394.71	496.62	383.25	455.24	464.30	463.73	338.18	289.34	421.42	399.53	395.74
	(7.92)	(15.79)	(2.56)	(3.15)		(12.16)	(15.86)	(8.37)	(6.71)		(6.59)	(16.92)	(1.75)	(0.12)		(11.35)	(21.41)	(4.83)	(0.69)	
Canton of residence																				
AG	*0.13*	*0.34*	*0.09*	*0.17*	*0.18*	*0.04*	*0.03*	*0.01*	*0.03*	*0.04*	*0.01*	*0.00*	*0.01*	*0.01*	*0.01*	*0.21*	*0.04*	*0.02*	*0.07*	*0.06*
	(13.43)	*(36.54)*	*(28.58)*	*(2.19)*		*(2.52)*	*(3.35)*	*(17.01)*	*(2.83)*		*(4.86)*	*(10.16)*	*(9.26)*	*(3.42)*		*(43.67)*	*(9.31)*	*(21.02)*	*(2.32)*	
AR	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.01*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.01*	*0.01*
	(5.38)	*(1.56)*	*(1.62)*	*(1.69)*		*(8.38)*	*(1.19)*	*(6.49)*	*(2.67)*		*(7.91)*	*(1.07)*	*(2.65)*	*(5.86)*		*(3.16)*	*(3.70)*	*(2.11)*	*(0.57)*	
BE	*0.00*	*0.00*	*0.01*	*0.01*	*0.01*	*0.00*	*0.02*	*0.05*	*0.04*	*0.04*	*0.06*	*0.27*	*0.13*	*0.28*	*0.35*	*0.01*	*0.03*	*0.20*	*0.08*	*0.10*
	(7.21)	*(4.46)*	*(4.36)*	*(1.37)*		*(22.45)*	*(8.53)*	*(6.59)*	*(0.37)*		*(79.65)*	*(18.61)*	*(54.95)*	*(16.79)*		*(44.07)*	*(30.58)*	*(26.12)*	*(7.15)*	
BL	*0.01*	*0.02*	*0.04*	*0.04*	*0.04*	*0.02*	*0.01*	*0.02*	*0.03*	*0.02*	*0.00*	*0.00*	*0.02*	*0.02*	*0.02*	*0.03*	*0.03*	*0.02*	*0.04*	*0.03*
	(21.73)	*(9.75)*	*(1.44)*	*(3.76)*		*(1.10)*	*(6.67)*	*(0.36)*	*(3.42)*		*(17.51)*	*(10.17)*	*(2.15)*	*(2.30)*		*(1.33)*	*(2.53)*	*(8.85)*	*(1.61)*	
BS	*0.03*	*0.01*	*0.01*	*0.03*	*0.02*	*0.01*	*0.02*	*0.01*	*0.02*	*0.02*	*0.02*	*0.00*	*0.01*	*0.01*	*0.01*	*0.02*	*0.03*	*0.01*	*0.04*	*0.03*
	(5.13)	*(10.71)*	*(6.91)*	*(3.92)*		*(14.24)*	*(0.49)*	*(10.46)*	*(0.61)*		*(9.14)*	*(3.80)*	*(4.54)*	*(0.16)*		*(5.83)*	*(4.65)*	*(16.47)*	*(0.67)*	

Continued on next page

Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
FR	*0.03*	*0.02*	*0.07*	*0.02*	*0.02*	*0.01*	*0.03*	*0.03*	*0.02*	*0.02*	*0.12*	*0.08*	*0.07*	*0.08*	*0.07*	*0.03*	*0.02*	*0.08*	*0.04*	*0.04*
	(7.20)	*(2.86)*	*(25.44)*	*(5.57)*		*(10.32)*	*(5.64)*	*(6.89)*	*(4.90)*		*(19.04)*	*(4.62)*	*(3.81)*	*(5.51)*		*(2.97)*	*(8.35)*	*(18.86)*	*(0.86)*	
GE	*0.26*	*0.07*	*0.08*	*0.07*	*0.06*	*0.21*	*0.13*	*0.10*	*0.09*	*0.10*	*0.15*	*0.05*	*0.08*	*0.02*	*0.03*	*0.08*	*0.11*	*0.04*	*0.07*	*0.06*
	(55.57)	*(5.76)*	*(8.85)*	*(2.67)*		*(29.31)*	*(9.30)*	*(0.80)*	*(3.15)*		*(42.37)*	*(9.12)*	*(23.89)*	*(3.02)*		*(9.80)*	*(21.10)*	*(6.38)*	*(7.46)*	
GL	*0.00*	*0.00*	*0.01*	*0.01*	*0.00*	*0.00*	*0.00*	*0.01*	*0.01*	*0.00*	*0.01*	*0.02*	*0.01*	*0.01*	*0.00*	*0.00*	*0.00*	*0.01*	*0.01*	*0.00*
	(4.20)	*(0.16)*	*(3.34)*	*(3.00)*		*(6.06)*	*(0.11)*	*(3.41)*	*(7.37)*		*(6.20)*	*(12.82)*	*(1.77)*	*(5.98)*		*(0.19)*	*(3.87)*	*(1.45)*	*(0.70)*	
GR	*0.00*	*0.00*	*0.01*	*0.00*	*0.00*	*0.01*	*0.02*	*0.04*	*0.03*	*0.03*	*0.03*	*0.05*	*0.03*	*0.02*	*0.04*	*0.01*	*0.02*	*0.03*	*0.02*	*0.02*
	(7.38)	*(3.31)*	*(7.64)*	*(1.04)*		*(19.95)*	*(9.19)*	*(2.96)*	*(2.13)*		*(4.92)*	*(6.54)*	*(3.75)*	*(8.60)*		*(14.52)*	*(1.53)*	*(5.78)*	*(0.28)*	
JU	*0.00*	*0.00*	*0.01*	*0.00*	*0.00*	*0.00*	*0.01*	*0.01*	*0.01*	*0.01*	*0.05*	*0.05*	*0.01*	*0.02*	*0.02*	*0.00*	*0.01*	*0.01*	*0.01*	*0.01*
	(5.41)	*(1.69)*	*(7.76)*	*(3.50)*		*(6.97)*	*(0.45)*	*(7.54)*	*(1.47)*		*(11.93)*	*(14.62)*	*(6.91)*	*(0.75)*		*(5.27)*	*(0.40)*	*(6.51)*	*(1.12)*	
LU	*0.07*	*0.04*	*0.03*	*0.04*	*0.02*	*0.03*	*0.04*	*0.07*	*0.03*	*0.03*	*0.01*	*0.02*	*0.03*	*0.02*	*0.02*	*0.05*	*0.04*	*0.03*	*0.04*	*0.04*
	(21.72)	*(9.73)*	*(5.32)*	*(9.52)*		*(0.06)*	*(0.33)*	*(15.00)*	*(0.83)*		*(8.10)*	*(0.80)*	*(7.83)*	*(4.11)*		*(2.11)*	*(0.81)*	*(6.02)*	*(1.28)*	
NE	*0.03*	*0.06*	*0.03*	*0.02*	*0.03*	*0.04*	*0.06*	*0.02*	*0.03*	*0.03*	*0.02*	*0.03*	*0.02*	*0.01*	*0.01*	*0.03*	*0.04*	*0.01*	*0.02*	*0.02*
	(2.45)	*(13.70)*	*(2.63)*	*(2.92)*		*(7.05)*	*(17.55)*	*(1.47)*	*(0.66)*		*(7.68)*	*(13.32)*	*(8.81)*	*(1.71)*		*(7.79)*	*(13.92)*	*(5.85)*	*(1.78)*	
NW	*0.00*	*0.00*	*0.01*	*0.00*	*0.00*	*0.00*	*0.00*	*0.01*	*0.00*	*0.00*	*0.01*	*0.00*	*0.01*	*0.00*	*0.01*	*0.00*	*0.00*	*0.01*	*0.00*	*0.00*
	(2.48)	*(2.33)*	*(6.32)*	*(1.79)*		*(0.54)*	*(0.64)*	*(5.12)*	*(1.65)*		*(3.01)*	*(6.10)*	*(0.35)*	*(5.51)*		*(3.31)*	*(1.12)*	*(8.44)*	*(1.65)*	
SG	*0.10*	*0.06*	*0.07*	*0.07*	*0.07*	*0.08*	*0.08*	*0.05*	*0.08*	*0.05*	*0.06*	*0.09*	*0.06*	*0.04*	*0.03*	*0.07*	*0.06*	*0.05*	*0.05*	*0.05*
	(11.93)	*(2.75)*	*(1.13)*	*(1.86)*		*(11.31)*	*(12.74)*	*(2.30)*	*(11.01)*		*(17.26)*	*(26.95)*	*(14.24)*	*(4.92)*		*(6.83)*	*(4.11)*	*(0.94)*	*(0.08)*	
SH	*0.02*	*0.02*	*0.02*	*0.02*	*0.01*	*0.02*	*0.02*	*0.01*	*0.02*	*0.01*	*0.03*	*0.01*	*0.01*	*0.01*	*0.01*	*0.02*	*0.02*	*0.01*	*0.01*	*0.01*
	(2.45)	*(6.52)*	*(3.41)*	*(6.68)*		*(7.33)*	*(3.76)*	*(0.74)*	*(4.28)*		*(16.79)*	*(6.61)*	*(0.94)*	*(1.38)*		*(7.10)*	*(6.00)*	*(0.73)*	*(0.78)*	
SO	*0.02*	*0.02*	*0.14*	*0.06*	*0.05*	*0.00*	*0.01*	*0.03*	*0.02*	*0.01*	*0.07*	*0.05*	*0.06*	*0.10*	*0.05*	*0.03*	*0.01*	*0.07*	*0.03*	*0.03*
	(20.60)	*(16.81)*	*(30.11)*	*(4.38)*		*(7.52)*	*(1.28)*	*(12.77)*	*(8.94)*		*(8.69)*	*(1.40)*	*(1.82)*	*(16.44)*		*(1.32)*	*(11.96)*	*(17.29)*	*(0.60)*	
SZ	*0.01*	*0.01*	*0.05*	*0.02*	*0.03*	*0.01*	*0.00*	*0.01*	*0.00*	*0.00*	*0.03*	*0.00*	*0.02*	*0.01*	*0.01*	*0.02*	*0.00*	*0.01*	*0.01*	*0.01*
	(11.08)	*(18.78)*	*(10.74)*	*(4.26)*		*(3.09)*	*(3.98)*	*(6.02)*	*(1.73)*		*(10.83)*	*(11.65)*	*(3.28)*	*(0.88)*		*(3.12)*	*(7.50)*	*(0.51)*	*(1.69)*	
TG	*0.00*	*0.03*	*0.04*	*0.03*	*0.03*	*0.01*	*0.03*	*0.03*	*0.03*	*0.02*	*0.00*	*0.02*	*0.04*	*0.03*	*0.02*	*0.03*	*0.04*	*0.04*	*0.03*	*0.03*
	(22.77)	*(1.18)*	*(4.97)*	*(0.58)*		*(9.33)*	*(5.20)*	*(7.59)*	*(4.29)*		*(20.41)*	*(1.37)*	*(10.17)*	*(3.32)*		*(1.81)*	*(0.53)*	*(4.34)*	*(1.36)*	
TI	*0.04*	*0.01*	*0.07*	*0.05*	*0.04*	*0.03*	*0.02*	*0.08*	*0.04*	*0.06*	*0.05*	*0.05*	*0.13*	*0.12*	*0.06*	*0.04*	*0.02*	*0.10*	*0.06*	*0.04*
	(3.61)	*(18.35)*	*(10.37)*	*(0.55)*		*(12.55)*	*(18.43)*	*(10.32)*	*(7.32)*		*(8.04)*	*(6.45)*	*(23.01)*	*(19.19)*		*(0.88)*	*(7.44)*	*(23.36)*	*(12.48)*	
VD	*0.08*	*0.12*	*0.09*	*0.07*	*0.08*	*0.14*	*0.26*	*0.23*	*0.18*	*0.22*	*0.17*	*0.11*	*0.13*	*0.08*	*0.10*	*0.07*	*0.21*	*0.11*	*0.12*	*0.11*
	(0.61)	*(14.48)*	*(4.05)*	*(4.90)*		*(21.60)*	*(9.05)*	*(3.47)*	*(10.63)*		*(18.63)*	*(2.05)*	*(8.26)*	*(6.78)*		*(13.24)*	*(27.30)*	*(1.70)*	*(3.27)*	
VS	*0.03*	*0.02*	*0.03*	*0.03*	*0.03*	*0.05*	*0.02*	*0.06*	*0.06*	*0.07*	*0.06*	*0.04*	*0.04*	*0.05*	*0.08*	*0.03*	*0.04*	*0.06*	*0.06*	*0.08*
	(4.84)	*(0.60)*	*(3.57)*	*(4.32)*		*(10.14)*	*(24.52)*	*(3.94)*	*(4.42)*		*(6.61)*	*(15.35)*	*(17.20)*	*(13.64)*		*(21.54)*	*(16.62)*	*(9.64)*	*(7.32)*	
ZG	*0.00*	*0.01*	*0.03*	*0.01*	*0.02*	*0.00*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.02*	*0.02*	*0.01*	*0.01*	*0.01*	*0.02*	*0.01*	*0.01*
	(16.95)	*(12.07)*	*(6.13)*	*(8.11)*		*(5.51)*	*(2.88)*	*(3.15)*	*(3.04)*		*(4.26)*	*(6.19)*	*(4.31)*	*(3.37)*		*(1.06)*	*(1.47)*	*(8.57)*	*(1.57)*	
ZH	*0.15*	*0.12*	*0.07*	*0.21*	*0.24*	*0.28*	*0.17*	*0.10*	*0.22*	*0.19*	*0.04*	*0.03*	*0.05*	*0.04*	*0.03*	*0.20*	*0.19*	*0.04*	*0.16*	*0.19*
	(25.22)	*(33.23)*	*(50.25)*	*(8.90)*		*(22.14)*	*(5.05)*	*(27.63)*	*(8.38)*		*(4.44)*	*(3.27)*	*(10.69)*	*(4.24)*		*(4.30)*	*(1.89)*	*(45.81)*	*(7.07)*	
Other	*0.00*	*0.00*	*0.01*	*0.00*	*0.00*	*0.00*	*0.00*	*0.01*	*0.01*	*0.00*	*0.00*	*0.00*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*
	(3.28)	*(4.49)*	*(5.26)*	*(0.92)*		*(0.77)*	*(4.36)*	*(4.31)*	*(2.33)*		*(11.23)*	*(11.23)*	*(3.20)*	*(5.37)*		*(2.10)*	*(1.69)*	*(4.55)*	*(1.94)*	
Civil status																				
Divorced	*0.14*	*0.14*	*0.15*	*0.14*	*0.13*	*0.16*	*0.11*	*0.13*	*0.14*	*0.13*	*0.11*	*0.13*	*0.14*	*0.14*	*0.13*	*0.15*	*0.12*	*0.15*	*0.14*	*0.13*
	(3.19)	*(2.81)*	*(4.07)*	*(3.19)*		*(8.94)*	*(5.08)*	*(0.01)*	*(1.76)*		*(5.21)*	*(0.35)*	*(2.36)*	*(3.29)*		*(6.95)*	*(1.27)*	*(5.77)*	*(5.21)*	
Married	*0.54*	*0.57*	*0.51*	*0.53*	*0.49*	*0.54*	*0.59*	*0.57*	*0.59*	*0.54*	*0.48*	*0.58*	*0.52*	*0.57*	*0.51*	*0.50*	*0.59*	*0.50*	*0.53*	*0.46*
	(11.23)	*(17.61)*	*(4.63)*	*(8.43)*		*(1.67)*	*(8.99)*	*(4.98)*	*(9.50)*		*(5.60)*	*(14.60)*	*(2.09)*	*(12.01)*		*(7.46)*	*(25.18)*	*(8.11)*	*(13.35)*	
Single	*0.31*	*0.28*	*0.34*	*0.32*	*0.38*	*0.29*	*0.29*	*0.29*	*0.27*	*0.32*	*0.40*	*0.28*	*0.34*	*0.28*	*0.35*	*0.34*	*0.28*	*0.34*	*0.32*	*0.40*
	(14.41)	*(21.50)*	*(8.17)*	*(11.95)*		*(6.18)*	*(5.91)*	*(5.61)*	*(11.72)*		*(9.19)*	*(16.17)*	*(3.34)*	*(14.95)*		*(12.87)*	*(25.74)*	*(12.52)*	*(18.29)*	

Continued on next page

Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference $\geqslant 20$ marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
Widowed	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	(1.60)	(3.81)	(2.49)	(4.51)		(5.35)	(0.80)	(1.35)	(0.24)		(0.45)	(3.51)	(2.97)	(1.68)		(1.08)	(0.88)	(0.15)	(3.61)	
Change in regional unemployment rate vs. last year	-0.05	-0.06	-0.05	-0.05	-0.03	-0.05	-0.04	-0.04	-0.02	-0.02	0.01	-0.00	-0.03	-0.04	0.00	-0.06	-0.07	-0.07	-0.06	-0.05
	(4.17)	(8.59)	(6.01)	(4.39)		(5.33)	(2.56)	(2.69)	(1.90)		(2.04)	(1.73)	(8.68)	(11.57)		(3.97)	(4.41)	(6.03)	(2.17)	
Desired degree of employment before ref. point	96.04	94.02	94.77	95.03	94.18	94.87	95.11	95.47	95.96	94.18	94.17	92.54	94.18	95.66	92.79	95.71	94.40	94.86	92.57	94.24
	(14.28)	(1.15)	(4.31)	(6.30)		(4.87)	(6.65)	(9.46)	(13.37)		(8.66)	(1.54)	(8.98)	(19.73)		(11.01)	(1.10)	(4.41)	(10.83)	
Degree of employment in last job	91.86	89.92	91.00	91.97	91.47	90.98	90.67	90.58	92.40	90.81	90.37	89.57	89.74	92.57	89.42	91.14	88.45	89.85	89.48	91.30
	(2.28)	(8.81)	(2.78)	(3.04)		(0.95)	(0.74)	(1.25)	(9.42)		(5.16)	(0.81)	(1.71)	(18.09)		(0.95)	(16.03)	(8.35)	(10.65)	
Months with disability ins. benefits 1y before ref. point	0.02	0.00	0.02	0.01	0.01	0.01	0.00	0.01	0.01	0.01	0.00	0.06	0.02	0.01	0.03	0.03	0.00	0.02	0.02	0.02
	(0.81)	(5.44)	(1.12)	(0.79)		(3.04)	(2.62)	(0.67)	(0.82)		(9.75)	(7.36)	(2.45)	(5.58)		(3.70)	(7.60)	(0.27)	(0.41)	
Earnings history missing for at least one year	0.30	0.29	0.29	0.27	0.28	0.29	0.24	0.29	0.26	0.28	0.26	0.29	0.30	0.28	0.30	0.27	0.26	0.28	0.29	0.27
	(4.92)	(1.77)	(2.16)	(1.71)		(0.32)	(9.07)	(0.26)	(6.37)		(9.12)	(3.11)	(0.82)	(5.05)		(0.77)	(0.75)	(2.70)	(4.96)	
Cum. earnings in year before ref. point (in CHF 1,000)	48.77	50.50	45.08	46.00	54.12	48.09	50.00	40.57	43.90	49.85	37.59	36.91	35.51	36.29	35.03	46.65	43.70	39.23	45.57	46.14
	(12.88)	(8.76)	(23.46)	(21.01)		(4.47)	(0.36)	(25.21)	(15.99)		(9.38)	(7.58)	(2.01)	(5.69)		(1.78)	(8.32)	(26.09)	(1.99)	
Cum. earnings in 2nd year before ref. point (in CHF 1,000)	59.72	56.99	51.98	53.58	65.31	57.55	54.99	48.91	49.47	58.55	51.24	47.96	48.83	45.58	47.67	48.57	42.70	42.15	47.64	48.34
	(11.92)	(18.40)	(31.20)	(27.33)		(2.51)	(8.59)	(26.40)	(24.27)		(10.30)	(0.97)	(3.73)	(7.45)		(0.74)	(18.19)	(21.20)	(2.15)	
Cum. earnings in 3rd year before ref. point (in CHF 1,000)	52.52	50.62	45.71	47.76	59.02	50.91	45.64	41.99	40.97	50.31	44.27	42.21	42.17	39.27	41.93	42.62	35.33	35.48	42.15	43.22
	(13.39)	(17.33)	(30.05)	(25.13)		(1.47)	(10.75)	(21.54)	(23.85)		(6.52)	(0.90)	(0.72)	(8.72)		(1.80)	(23.73)	(24.49)	(3.16)	
Avg monthly earnings in 5y before reference point	4,472.37	4,310.88	3,893.12	4,020.99	4,917.24	4,334.31	4,178.83	3,632.64	3,741.54	4,417.45	3,701.81	3,459.10	3,519.71	3,371.25	3,483.13	3,761.90	3,366.77	3,180.83	3,698.73	3,788.72
	(12.72)	(17.98)	(32.51)	(28.23)		(2.79)	(7.65)	(28.52)	(24.40)		(8.86)	(1.13)	(1.67)	(5.67)		(1.18)	(18.83)	(29.04)	(3.88)	
Avg monthly earnings in 7y before reference point	4,353.10	4,173.22	3,761.03	3,892.11	4,781.89	4,208.30	4,051.77	3,514.36	3,644.35	4,296.36	3,611.82	3,365.29	3,442.67	3,288.32	3,409.90	3,618.33	3,262.21	3,082.26	3,582.41	3,660.36
	(12.36)	(18.39)	(32.75)	(28.38)		(3.02)	(8.02)	(29.08)	(24.08)		(8.36)	(2.10)	(1.50)	(6.23)		(1.91)	(18.29)	(28.59)	(3.45)	
Level of education																				
0 - Unknown	0.06	0.05	0.06	0.05	0.05	0.05	0.07	0.07	0.08	0.07	0.04	0.05	0.06	0.05	0.04	0.05	0.09	0.06	0.06	0.06
	(6.14)	(3.47)	(7.62)	(3.94)		(6.64)	(2.69)	(0.90)	(4.50)		(0.04)	(5.05)	(8.58)	(7.91)		(2.99)	(11.94)	(1.01)	(3.30)	
1 - Primary	0.02	0.01	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.01	0.03	0.01	0.02	0.02	0.01	0.01	0.02	0.02	0.02	0.01
	(11.27)	(9.09)	(6.95)	(6.15)		(3.03)	(5.54)	(5.98)	(4.81)		(9.50)	(0.66)	(0.98)	(1.80)		(0.47)	(3.06)	(1.02)	(1.66)	
2 - Secondary I	0.25	0.27	0.26	0.27	0.16	0.27	0.30	0.31	0.35	0.23	0.31	0.31	0.29	0.36	0.29	0.27	0.36	0.31	0.27	0.23
	(23.63)	(26.81)	(25.21)	(27.28)		(8.81)	(16.66)	(18.82)	(26.32)		(3.65)	(4.29)	(0.93)	(14.69)		(10.00)	(28.64)	(18.77)	(9.27)	
3 - Secondary II	0.46	0.49	0.53	0.53	0.53	0.48	0.36	0.44	0.42	0.43	0.48	0.50	0.46	0.48	0.50	0.52	0.39	0.49	0.48	0.53
	(14.75)	(9.31)	(1.04)	(0.04)		(8.41)	(14.44)	(2.00)	(2.89)		(4.61)	(1.41)	(8.73)	(5.43)		(1.92)	(28.73)	(8.37)	(9.81)	
4 - Tertiary	0.21	0.18	0.14	0.13	0.26	0.20	0.24	0.16	0.14	0.26	0.15	0.13	0.18	0.10	0.16	0.14	0.14	0.12	0.17	0.17
	(11.09)	(19.13)	(30.52)	(31.93)		(14.58)	(3.87)	(25.69)	(30.88)		(2.00)	(6.70)	(5.44)	(18.22)		(7.69)	(6.14)	(12.83)	(0.29)	
Months employed in year before reference point	9.06	9.76	9.59	9.47	9.18	9.14	9.37	8.95	9.11	8.73	8.10	8.59	8.06	8.36	7.86	10.24	10.31	9.68	10.03	9.75
	(5.02)	(26.84)	(18.29)	(12.49)		(17.53)	(27.14)	(9.43)	(16.28)		(9.12)	(28.92)	(7.26)	(18.60)		(21.23)	(24.29)	(3.14)	(11.05)	
Months employed in 2nd year before reference point	10.94	11.11	10.96	10.97	11.03	11.04	10.63	10.79	10.38	10.60	10.46	10.96	10.68	10.28	10.41	10.47	9.99	10.19	10.27	10.14
	(3.37)	(3.28)	(2.60)	(2.23)		(16.36)	(0.80)	(6.84)	(7.75)		(1.86)	(20.11)	(9.57)	(4.29)		(10.51)	(4.31)	(1.78)	(4.12)	
Months employed in 3rd year before reference point	9.53	9.79	9.69	9.79	9.98	9.65	8.74	9.12	8.68	9.12	8.91	9.56	9.14	8.92	9.14	9.28	8.42	8.61	9.19	9.21
	(11.38)	(4.99)	(7.49)	(4.98)		(12.37)	(8.33)	(0.03)	(9.65)		(5.37)	(10.04)	(0.05)	(5.03)		(1.67)	(17.63)	(13.53)	(0.47)	
Months in employment in 5y before reference point	46.87	48.04	47.64	48.15	48.39	47.10	43.38	44.51	43.30	44.75	43.31	45.54	44.19	43.71	44.18	47.03	43.27	44.12	46.66	46.33
	(11.64)	(2.68)	(5.82)	(1.85)		(16.67)	(9.17)	(1.62)	(9.79)		(6.25)	(10.13)	(0.11)	(3.48)		(5.10)	(20.79)	(16.10)	(2.42)	
Months in employment in 7y before reference point	62.53	63.73	63.58	64.48	65.15	62.66	55.62	58.23	56.40	59.21	58.56	59.64	58.92	58.40	59.51	62.36	55.93	58.49	62.49	62.06
	(13.00)	(6.98)	(7.91)	(3.40)		(15.69)	(15.50)	(4.37)	(12.20)		(4.60)	(0.61)	(2.89)	(5.41)		(1.44)	(27.31)	(17.20)	(2.08)	
Language level English	2.23	2.07	1.79	1.82	2.68	2.19	2.27	1.81	1.68	2.49	1.69	1.65	1.83	1.42	1.86	1.84	1.75	1.59	1.82	1.97
	(18.98)	(26.59)	(39.43)	(38.47)		(12.35)	(9.00)	(29.39)	(34.89)		(8.27)	(9.99)	(1.39)	(21.56)		(6.11)	(10.05)	(17.70)	(6.98)	

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Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
Exempt from paying ue insurance contributions	0.00	0.00	0.01	0.00	0.01	0.00	0.00	0.00	0.00	0.01	0.01	0.00	0.01	0.00	0.01	0.01	0.01	0.01	0.01	0.02
	(5.46)	(8.10)	(3.75)	(4.34)		(11.71)	(5.57)	(3.77)	(6.31)		(3.57)	(16.24)	(6.39)	(9.90)		(5.50)	(9.11)	(1.94)	(3.28)	
Ever exempt from job search efforts 1y before ref. point	0.12	0.10	0.12	0.11	0.12	0.12	0.11	0.13	0.13	0.13	0.20	0.17	0.15	0.15	0.19	0.13	0.11	0.14	0.13	0.15
	(1.21)	(7.19)	(0.05)	(4.46)		(4.22)	(8.20)	(0.21)	(2.00)		(3.31)	(5.56)	(11.71)	(9.56)		(8.01)	(12.28)	(5.14)	(7.96)	
Ever exempt from job search eff. in prev. ue spells	0.24	0.19	0.24	0.21	0.23	0.22	0.21	0.24	0.24	0.24	0.33	0.28	0.27	0.30	0.32	0.24	0.22	0.29	0.27	0.29
	(0.90)	(9.02)	(1.03)	(4.05)		(4.22)	(7.38)	(1.26)	(0.85)		(2.01)	(9.31)	(10.85)	(5.67)		(9.80)	(16.19)	(1.18)	(4.44)	
Work exp. agricultural and forestry occupations																				
0 - Not looking for this occupation	0.98	0.98	0.98	0.98	0.99	0.98	0.98	0.98	0.97	0.98	0.99	0.98	0.97	0.97	0.97	0.97	0.97	0.96	0.97	0.96
	(1.50)	(1.14)	(6.31)	(4.58)		(1.51)	(1.05)	(0.11)	(4.56)		(17.16)	(7.13)	(2.17)	(0.40)		(3.01)	(5.95)	(0.41)	(4.75)	
3 - Less than 1 year of experience	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.00
	(1.46)	(4.81)	(3.39)	(3.40)		(1.61)	(0.78)	(1.17)	(1.44)		(8.61)	(1.95)	(0.21)	(2.64)		(2.59)	(0.48)	(3.99)	(0.19)	
4 - 1-3 years of experience	0.00	0.00	0.01	0.00	0.00	0.00	0.01	0.01	0.01	0.01	0.00	0.00	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	(2.00)	(3.60)	(2.79)	(0.99)		(5.93)	(1.30)	(0.26)	(3.03)		(13.10)	(8.15)	(1.56)	(0.69)		(2.27)	(0.90)	(2.52)	(1.58)	
5 - More than 3 years of experience	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.02	0.01	0.00	0.01	0.01	0.02	0.02	0.02	0.01	0.02	0.01	0.02
	(0.83)	(0.92)	(3.63)	(4.68)		(2.83)	(0.29)	(0.93)	(4.87)		(18.79)	(6.38)	(2.47)	(0.34)		(2.46)	(7.84)	(1.14)	(4.89)	
Other	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.01	0.01	0.00	0.00	0.00	0.01	0.01	0.01
	(1.44)	(0.13)	(3.12)	(0.87)		(3.18)	(2.19)	(2.22)	(0.27)		(5.52)	(0.46)	(1.04)	(4.09)		(4.75)	(2.84)	(2.25)	(0.96)	
Work exp. production occupations in industry and trade																				
0 - Not looking for this occupation	0.70	0.69	0.65	0.64	0.76	0.72	0.71	0.68	0.67	0.77	0.66	0.63	0.67	0.60	0.68	0.64	0.69	0.62	0.72	0.72
	(13.95)	(16.41)	(25.18)	(27.10)		(11.48)	(13.05)	(20.67)	(22.49)		(4.00)	(10.38)	(0.85)	(16.78)		(17.69)	(7.20)	(22.26)	(0.32)	
1 - Experience unknown	0.02	0.02	0.03	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.01	0.03	0.03	0.03	0.04	0.02	0.04	0.03	0.03
	(2.97)	(2.77)	(5.37)	(3.42)		(0.48)	(3.04)	(4.09)	(4.11)		(13.30)	(15.04)	(0.00)	(3.67)		(3.92)	(2.99)	(7.01)	(1.42)	
2 - No experience	0.04	0.04	0.05	0.04	0.02	0.03	0.04	0.04	0.04	0.03	0.04	0.08	0.05	0.06	0.04	0.05	0.05	0.05	0.04	0.03
	(10.78)	(10.03)	(12.31)	(10.78)		(3.92)	(8.88)	(6.73)	(8.77)		(2.35)	(17.73)	(2.21)	(8.28)		(7.28)	(7.60)	(9.81)	(1.39)	
3 - Less than 1 year of experience	0.02	0.02	0.02	0.02	0.01	0.02	0.02	0.02	0.02	0.02	0.05	0.03	0.02	0.03	0.03	0.03	0.03	0.04	0.02	0.02
	(5.29)	(4.65)	(6.36)	(7.57)		(2.31)	(3.45)	(2.19)	(3.95)		(10.65)	(2.12)	(2.40)	(4.99)		(4.39)	(6.45)	(9.13)	(0.08)	
4 - 1-3 years of experience	0.05	0.06	0.07	0.06	0.04	0.05	0.06	0.07	0.07	0.05	0.06	0.07	0.07	0.08	0.07	0.07	0.06	0.07	0.06	0.06
	(4.88)	(9.16)	(11.23)	(9.16)		(0.79)	(6.34)	(7.22)	(7.13)		(7.69)	(1.89)	(2.07)	(1.65)		(6.42)	(3.93)	(6.75)	(0.08)	
5 - More than 3 years of experience	0.16	0.16	0.19	0.20	0.14	0.16	0.14	0.18	0.18	0.12	0.19	0.18	0.16	0.20	0.15	0.18	0.14	0.18	0.14	0.14
	(5.84)	(6.91)	(13.33)	(18.03)		(11.30)	(5.10)	(15.37)	(15.90)		(10.59)	(7.32)	(2.34)	(11.33)		(10.56)	(0.81)	(10.78)	(0.93)	
Work exp. technical and information techn. occupations																				
0 - Not looking for this occupation	0.90	0.89	0.89	0.91	0.87	0.89	0.87	0.91	0.91	0.87	0.86	0.92	0.90	0.91	0.91	0.90	0.93	0.91	0.93	0.91
	(8.81)	(6.49)	(7.41)	(11.49)		(7.12)	(0.83)	(13.94)	(12.57)		(13.67)	(5.43)	(3.34)	(1.44)		(2.52)	(7.85)	(2.03)	(7.22)	
1 - Experience unknown	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.00	0.00	0.00	0.01	0.01	0.00	0.01	0.00	0.01	0.01	0.01
	(2.84)	(0.17)	(1.27)	(0.37)		(4.56)	(0.05)	(2.53)	(1.51)		(8.03)	(1.36)	(3.57)	(3.65)		(3.47)	(6.20)	(0.98)	(2.08)	
2 - No experience	0.01	0.00	0.00	0.01	0.01	0.00	0.00	0.00	0.00	0.01	0.00	0.01	0.01	0.01	0.00	0.01	0.00	0.00	0.00	0.01
	(0.84)	(2.35)	(0.84)	(0.51)		(2.36)	(2.18)	(1.86)	(2.29)		(9.84)	(5.99)	(1.28)	(0.36)		(0.11)	(1.66)	(0.96)	(1.26)	
3 - Less than 1 year of experience	0.01	0.00	0.01	0.00	0.00	0.00	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.00	0.00	0.01	0.00	0.01	0.00	0.01
	(1.91)	(0.15)	(3.14)	(1.19)		(7.61)	(1.27)	(0.12)	(1.36)		(9.33)	(8.88)	(0.54)	(6.15)		(0.67)	(1.68)	(3.72)	(1.85)	
4 - 1-3 years of experience	0.01	0.01	0.02	0.01	0.02	0.01	0.02	0.02	0.01	0.02	0.03	0.01	0.02	0.02	0.02	0.02	0.02	0.01	0.01	0.02
	(2.26)	(4.87)	(0.01)	(2.44)		(7.50)	(1.64)	(0.45)	(3.47)		(6.65)	(6.73)	(1.48)	(0.99)		(0.96)	(0.50)	(4.42)	(4.23)	
5 - More than 3 years of experience	0.07	0.08	0.07	0.06	0.10	0.09	0.10	0.06	0.07	0.10	0.11	0.04	0.07	0.06	0.06	0.06	0.04	0.05	0.05	0.06
	(9.04)	(4.71)	(8.82)	(12.84)		(4.27)	(0.89)	(16.22)	(12.29)		(16.38)	(8.78)	(3.27)	(2.06)		(2.43)	(7.11)	(1.67)	(4.94)	

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Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference $\geqslant 20$ marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**
Number of observations in sequence:	**4,743**	**3,781**	**3,845**	**4,603**	**26,358**	**1,075**	**9,906**	**1,735**	**2,392**	**9,992**	**108**	**402**	**11,490**	**1,774**	**6,225**	**2,157**	**2,042**	**2,062**	**41,014**	**35,231**
Work exp. construction and mining occupations																				
0 - Not looking for this occupation	*0.92*	*0.95*	*0.93*	*0.89*	*0.94*	*0.94*	*0.91*	*0.90*	*0.84*	*0.90*	*0.90*	*0.94*	*0.88*	*0.82*	*0.85*	*0.86*	*0.87*	*0.84*	*0.87*	*0.79*
	(7.93)	*(6.45)*	*(2.43)*	*(17.54)*		*(15.07)*	*(4.83)*	*(1.74)*	*(16.31)*		*(13.59)*	*(27.88)*	*(8.95)*	*(9.71)*		*(19.58)*	*(22.65)*	*(12.25)*	*(21.55)*	
1 - Experience unknown	*0.01*	*0.00*	*0.01*	*0.01*	*0.00*	*0.00*	*0.00*	*0.00*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.02*
	(1.60)	*(1.83)*	*(2.84)*	*(1.82)*		*(7.97)*	*(2.47)*	*(1.12)*	*(3.12)*		*(0.28)*	*(1.70)*	*(2.18)*	*(3.81)*		*(5.99)*	*(6.97)*	*(6.54)*	*(5.14)*	
2 - No experience	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.01*	*0.00*	*0.01*	*0.01*	*0.00*	*0.00*	*0.00*	*0.01*	*0.01*	*0.01*	*0.00*	*0.01*	*0.01*	*0.01*	*0.01*
	(4.28)	*(0.58)*	*(2.53)*	*(4.78)*		*(1.37)*	*(2.02)*	*(1.62)*	*(4.23)*		*(12.47)*	*(7.34)*	*(2.99)*	*(2.59)*		*(2.92)*	*(1.86)*	*(1.08)*	*(1.73)*	
3 - Less than 1 year of experience	*0.01*	*0.00*	*0.01*	*0.01*	*0.00*	*0.00*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.00*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.02*	*0.01*	*0.01*
	(2.14)	*(1.03)*	*(3.72)*	*(4.26)*		*(5.59)*	*(0.55)*	*(4.86)*	*(1.16)*		*(3.85)*	*(12.26)*	*(2.67)*	*(2.41)*		*(3.02)*	*(1.99)*	*(2.04)*	*(4.54)*	
4 - 1-3 years of experience	*0.02*	*0.01*	*0.01*	*0.02*	*0.01*	*0.01*	*0.03*	*0.03*	*0.05*	*0.03*	*0.04*	*0.01*	*0.03*	*0.04*	*0.04*	*0.03*	*0.04*	*0.04*	*0.02*	*0.04*
	(4.59)	*(1.41)*	*(0.73)*	*(7.79)*		*(10.58)*	*(0.69)*	*(1.00)*	*(11.24)*		*(0.04)*	*(15.92)*	*(6.61)*	*(2.93)*		*(2.71)*	*(2.18)*	*(2.37)*	*(8.43)*	
5 - More than 3 years of experience	*0.05*	*0.02*	*0.04*	*0.07*	*0.04*	*0.04*	*0.05*	*0.06*	*0.08*	*0.06*	*0.05*	*0.04*	*0.07*	*0.11*	*0.08*	*0.08*	*0.07*	*0.09*	*0.08*	*0.13*
	(4.78)	*(7.37)*	*(0.47)*	*(13.92)*		*(8.59)*	*(4.39)*	*(0.06)*	*(10.25)*		*(13.69)*	*(18.06)*	*(4.38)*	*(9.13)*		*(18.45)*	*(23.13)*	*(12.39)*	*(17.39)*	
Work exp. trade and transport occupations																				
0 - Not looking for this occupation	*0.68*	*0.69*	*0.66*	*0.67*	*0.65*	*0.67*	*0.75*	*0.74*	*0.73*	*0.71*	*0.69*	*0.67*	*0.71*	*0.71*	*0.70*	*0.71*	*0.75*	*0.72*	*0.72*	*0.73*
	(5.72)	*(8.68)*	*(1.46)*	*(4.91)*		*(9.89)*	*(9.09)*	*(5.35)*	*(3.55)*		*(1.40)*	*(7.36)*	*(2.92)*	*(2.18)*		*(5.37)*	*(4.44)*	*(2.26)*	*(2.99)*	
1 - Experience unknown	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.03*	*0.01*	*0.01*	*0.02*	*0.01*	*0.00*	*0.03*	*0.02*	*0.02*	*0.02*	*0.02*	*0.01*	*0.02*	*0.02*	*0.02*
	(5.15)	*(1.20)*	*(0.33)*	*(0.11)*		*(9.06)*	*(3.86)*	*(1.83)*	*(0.20)*		*(19.66)*	*(5.59)*	*(0.01)*	*(2.88)*		*(0.11)*	*(9.13)*	*(0.60)*	*(0.09)*	
2 - No experience	*0.03*	*0.03*	*0.03*	*0.03*	*0.03*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.06*	*0.04*	*0.03*	*0.04*	*0.03*	*0.03*	*0.03*	*0.04*	*0.03*	*0.03*
	(1.30)	*(0.47)*	*(3.44)*	*(0.20)*		*(1.17)*	*(1.11)*	*(1.95)*	*(1.63)*		*(10.72)*	*(2.11)*	*(2.39)*	*(1.45)*		*(2.59)*	*(0.36)*	*(4.73)*	*(1.30)*	
3 - Less than 1 year of experience	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.01*	*0.03*	*0.02*	*0.02*	*0.03*	*0.02*	*0.03*	*0.03*	*0.02*	*0.02*
	(0.70)	*(0.69)*	*(3.54)*	*(2.28)*		*(0.37)*	*(1.75)*	*(0.62)*	*(0.21)*		*(13.02)*	*(2.02)*	*(3.82)*	*(1.44)*		*(6.52)*	*(3.09)*	*(2.63)*	*(0.95)*	
4 - 1-3 years of experience	*0.06*	*0.05*	*0.07*	*0.06*	*0.06*	*0.06*	*0.05*	*0.06*	*0.05*	*0.06*	*0.06*	*0.08*	*0.06*	*0.07*	*0.06*	*0.06*	*0.05*	*0.06*	*0.06*	*0.06*
	(1.05)	*(2.92)*	*(4.00)*	*(0.43)*		*(1.76)*	*(2.22)*	*(1.69)*	*(0.30)*		*(3.13)*	*(6.46)*	*(0.23)*	*(1.89)*		*(2.05)*	*(1.33)*	*(0.57)*	*(0.50)*	
5 - More than 3 years of experience	*0.20*	*0.19*	*0.19*	*0.19*	*0.22*	*0.21*	*0.15*	*0.15*	*0.16*	*0.18*	*0.19*	*0.16*	*0.15*	*0.15*	*0.16*	*0.16*	*0.13*	*0.13*	*0.15*	*0.14*
	(4.61)	*(7.98)*	*(7.11)*	*(6.80)*		*(7.51)*	*(8.20)*	*(7.49)*	*(4.84)*		*(7.40)*	*(0.53)*	*(1.08)*	*(3.14)*		*(6.77)*	*(2.96)*	*(1.45)*	*(4.55)*	
Work exp. occupations providing personal services																				
0 - Not looking for this occupation	*0.74*	*0.71*	*0.69*	*0.71*	*0.79*	*0.70*	*0.68*	*0.63*	*0.65*	*0.74*	*0.64*	*0.64*	*0.65*	*0.65*	*0.67*	*0.69*	*0.57*	*0.62*	*0.64*	*0.72*
	(13.75)	*(19.08)*	*(23.81)*	*(20.17)*		*(7.92)*	*(12.71)*	*(23.31)*	*(19.34)*		*(7.53)*	*(6.42)*	*(4.52)*	*(5.35)*		*(6.04)*	*(30.13)*	*(20.04)*	*(16.08)*	
1 - Experience unknown	*0.02*	*0.02*	*0.02*	*0.02*	*0.01*	*0.01*	*0.02*	*0.03*	*0.02*	*0.02*	*0.04*	*0.04*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.03*	*0.02*
	(0.96)	*(3.57)*	*(5.02)*	*(2.03)*		*(2.32)*	*(0.58)*	*(6.31)*	*(2.42)*		*(7.40)*	*(8.83)*	*(1.73)*	*(6.50)*		*(0.13)*	*(0.07)*	*(0.25)*	*(2.42)*	
2 - No experience	*0.02*	*0.02*	*0.02*	*0.02*	*0.01*	*0.02*	*0.02*	*0.04*	*0.02*	*0.02*	*0.01*	*0.03*	*0.03*	*0.02*	*0.02*	*0.03*	*0.03*	*0.03*	*0.02*	*0.02*
	(5.56)	*(4.76)*	*(6.09)*	*(4.70)*		*(1.04)*	*(3.78)*	*(10.56)*	*(0.15)*		*(10.38)*	*(4.82)*	*(2.62)*	*(1.01)*		*(5.48)*	*(6.37)*	*(6.80)*	*(3.05)*	
3 - Less than 1 year of experience	*0.02*	*0.02*	*0.02*	*0.02*	*0.01*	*0.02*	*0.02*	*0.03*	*0.02*	*0.02*	*0.03*	*0.05*	*0.03*	*0.02*	*0.03*	*0.03*	*0.04*	*0.03*	*0.02*	*0.02*
	(2.19)	*(5.64)*	*(6.80)*	*(1.90)*		*(3.90)*	*(4.36)*	*(5.87)*	*(5.17)*		*(1.69)*	*(11.90)*	*(0.61)*	*(0.17)*		*(3.55)*	*(9.08)*	*(6.27)*	*(0.93)*	
4 - 1-3 years of experience	*0.06*	*0.08*	*0.08*	*0.06*	*0.04*	*0.08*	*0.09*	*0.10*	*0.10*	*0.07*	*0.14*	*0.09*	*0.08*	*0.09*	*0.07*	*0.07*	*0.13*	*0.10*	*0.08*	*0.06*
	(8.62)	*(13.44)*	*(13.84)*	*(6.99)*		*(4.37)*	*(10.45)*	*(12.27)*	*(10.94)*		*(22.04)*	*(5.76)*	*(4.58)*	*(6.47)*		*(5.61)*	*(24.20)*	*(13.72)*	*(6.94)*	
5 - More than 3 years of experience	*0.15*	*0.15*	*0.17*	*0.18*	*0.12*	*0.17*	*0.16*	*0.18*	*0.19*	*0.15*	*0.15*	*0.15*	*0.19*	*0.20*	*0.18*	*0.16*	*0.21*	*0.20*	*0.21*	*0.16*
	(8.08)	*(9.32)*	*(13.34)*	*(16.99)*		*(5.62)*	*(4.72)*	*(10.58)*	*(13.02)*		*(9.19)*	*(8.20)*	*(1.72)*	*(3.96)*		*(0.06)*	*(12.15)*	*(9.47)*	*(12.36)*	
Work exp. management, admin, banking, insurance, legal																				
0 - Not looking for this occupation	*0.66*	*0.65*	*0.70*	*0.73*	*0.59*	*0.64*	*0.70*	*0.70*	*0.78*	*0.65*	*0.69*	*0.69*	*0.73*	*0.81*	*0.73*	*0.76*	*0.79*	*0.79*	*0.77*	*0.77*
	(14.14)	*(11.79)*	*(24.37)*	*(29.52)*		*(0.35)*	*(12.73)*	*(11.97)*	*(29.79)*		*(10.37)*	*(10.09)*	*(1.14)*	*(19.14)*		*(3.12)*	*(4.14)*	*(5.24)*	*(0.18)*	
1 - Experience unknown	*0.01*	*0.02*	*0.02*	*0.01*	*0.02*	*0.01*	*0.01*	*0.02*	*0.01*	*0.01*	*0.02*	*0.02*	*0.01*	*0.01*	*0.01*	*0.02*	*0.01*	*0.01*	*0.01*	*0.02*
	(7.52)	*(4.27)*	*(3.87)*	*(6.26)*		*(2.04)*	*(3.53)*	*(2.59)*	*(4.08)*		*(6.59)*	*(7.59)*	*(3.17)*	*(3.47)*		*(0.42)*	*(9.28)*	*(7.21)*	*(1.18)*	

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Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
2 - No experience	0.02	0.02	0.02	0.01	0.02	0.01	0.01	0.02	0.01	0.01	0.05	0.02	0.02	0.02	0.02	0.02	0.01	0.02	0.01	0.02
	(3.63)	(0.44)	(0.30)	(6.55)		(0.38)	(2.67)	(7.50)	(0.12)		(15.82)	(4.51)	(0.93)	(2.41)		(2.13)	(4.13)	(4.11)	(1.66)	
3 - Less than 1 year of experience	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.03	0.02	0.02	0.01	0.01	0.01	0.01	0.02	0.01	0.01
	(3.48)	(2.65)	(0.31)	(2.32)		(2.15)	(0.73)	(2.01)	(5.01)		(8.99)	(2.09)	(0.79)	(5.31)		(1.81)	(1.22)	(3.14)	(0.10)	
4 - 1-3 years of experience	0.05	0.04	0.04	0.05	0.05	0.04	0.04	0.05	0.04	0.05	0.06	0.05	0.05	0.04	0.05	0.03	0.04	0.05	0.04	0.04
	(1.63)	(7.26)	(3.68)	(2.65)		(2.86)	(3.31)	(0.80)	(7.04)		(8.31)	(0.70)	(2.39)	(3.42)		(3.75)	(1.08)	(3.90)	(0.52)	
5 - More than 3 years of experience	0.26	0.27	0.20	0.19	0.30	0.27	0.22	0.20	0.16	0.27	0.16	0.20	0.17	0.12	0.18	0.16	0.14	0.11	0.15	0.14
	(10.39)	(7.26)	(23.73)	(26.62)		(1.63)	(10.54)	(15.50)	(27.18)		(5.54)	(6.57)	(1.53)	(17.23)		(4.16)	(1.73)	(9.30)	(1.46)	
Work exp. health, teaching, scientists, cultural occup.																				
0 - Not looking for this occupation	0.87	0.88	0.85	0.85	0.83	0.87	0.86	0.86	0.87	0.86	0.90	0.84	0.84	0.89	0.86	0.85	0.84	0.85	0.79	0.82
	(10.30)	(13.28)	(5.67)	(5.82)		(4.01)	(0.88)	(0.31)	(4.82)		(12.32)	(5.44)	(5.42)	(8.48)		(7.92)	(5.27)	(9.58)	(6.32)	
1 - Experience unknown	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.02	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	(0.75)	(5.11)	(0.51)	(1.75)		(0.69)	(1.55)	(1.01)	(0.73)		(9.65)	(6.99)	(0.22)	(0.93)		(2.94)	(3.50)	(3.05)	(0.55)	
2 - No experience	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.02	0.01	0.01	0.00	0.02	0.02	0.02	0.01	0.01	0.02	0.02	0.02	0.01
	(0.20)	(1.49)	(0.27)	(2.01)		(2.18)	(1.99)	(6.54)	(0.21)		(16.44)	(3.31)	(1.53)	(1.59)		(0.37)	(1.83)	(3.18)	(0.86)	
3 - Less than 1 year of experience	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	(0.99)	(1.07)	(3.93)	(0.48)		(4.10)	(1.49)	(0.07)	(0.07)		(2.86)	(0.21)	(1.29)	(2.53)		(0.73)	(1.38)	(0.08)	(1.83)	
4 - 1-3 years of experience	0.02	0.02	0.03	0.03	0.03	0.02	0.03	0.03	0.03	0.03	0.02	0.02	0.04	0.03	0.03	0.03	0.03	0.03	0.04	0.04
	(3.55)	(3.68)	(0.93)	(1.56)		(3.10)	(0.14)	(3.66)	(0.65)		(7.48)	(3.16)	(2.98)	(1.45)		(5.05)	(1.84)	(4.60)	(0.72)	
5 - More than 3 years of experience	0.08	0.08	0.09	0.09	0.11	0.08	0.08	0.08	0.07	0.09	0.06	0.09	0.09	0.05	0.08	0.09	0.10	0.08	0.13	0.11
	(10.57)	(11.65)	(8.73)	(7.98)		(5.41)	(2.89)	(5.83)	(6.46)		(9.36)	(4.65)	(3.85)	(9.76)		(6.38)	(4.61)	(9.67)	(6.15)	
Female	0.45	0.55	0.48	0.47	0.46	0.52	0.51	0.52	0.47	0.49	0.44	0.53	0.48	0.43	0.46	0.44	0.51	0.44	0.53	0.43
	(2.50)	(17.87)	(3.81)	(3.40)		(6.35)	(4.75)	(5.79)	(4.45)		(5.25)	(13.22)	(3.03)	(5.72)		(1.43)	(17.00)	(2.65)	(20.04)	
Language level French	2.96	2.43	2.41	2.02	2.46	2.99	2.93	2.88	2.39	3.09	3.50	2.86	2.80	2.24	2.73	2.15	2.48	2.50	2.55	2.50
	(17.98)	(1.18)	(1.84)	(16.86)		(3.42)	(5.24)	(7.10)	(24.43)		(28.57)	(4.66)	(2.47)	(18.19)		(13.17)	(0.70)	(0.13)	(1.98)	
Language level German	2.07	2.45	2.78	3.06	3.89	1.72	0.98	1.23	1.40	1.77	2.03	1.94	1.99	2.16	2.65	3.03	1.25	2.17	2.55	3.13
	(55.51)	(43.39)	(32.72)	(24.30)		(1.59)	(29.05)	(18.95)	(12.53)		(19.24)	(22.31)	(20.69)	(15.17)		(2.88)	(61.60)	(29.03)	(17.46)	
Avg monthly additional income in previous ue spells	186.48	159.60	197.11	260.41	175.38	173.73	135.65	178.82	206.68	165.53	155.08	181.37	204.35	301.43	208.62	283.06	227.41	311.96	379.90	334.27
	(2.16)	(3.21)	(4.35)	(15.35)		(1.70)	(6.38)	(2.73)	(8.29)		(11.63)	(5.64)	(0.86)	(16.44)		(8.12)	(17.62)	(3.56)	(6.62)	
Avg net monthly ue benefits in previous ue spells	1,027.62	837.77	996.49	930.52	1,057.33	929.83	769.37	799.12	812.49	966.65	1,333.18	819.08	1,053.08	1,027.77	1,104.36	995.83	738.25	1,022.66	994.68	1,037.57
	(2.03)	(15.68)	(4.34)	(9.25)		(2.68)	(14.64)	(13.12)	(11.91)		(16.60)	(23.89)	(3.97)	(6.24)		(3.38)	(25.48)	(1.25)	(3.52)	
Avg monthly sickness days in previous ue spells	0.13	0.10	0.14	0.13	0.11	0.11	0.08	0.14	0.10	0.10	0.19	0.12	0.15	0.19	0.17	0.15	0.10	0.15	0.13	0.13
	(3.95)	(2.57)	(6.59)	(4.05)		(3.04)	(4.32)	(8.65)	(0.21)		(2.69)	(11.98)	(5.16)	(2.13)		(4.18)	(5.54)	(5.89)	(0.36)	
Avg monthly waiting days in previous ue spells	0.29	0.26	0.35	0.35	0.41	0.26	0.23	0.24	0.26	0.34	0.35	0.24	0.32	0.37	0.42	0.40	0.25	0.35	0.36	0.48
	(11.50)	(14.27)	(5.58)	(5.15)		(10.42)	(12.75)	(12.55)	(10.13)		(7.85)	(18.91)	(9.62)	(5.08)		(6.87)	(22.23)	(12.53)	(11.65)	
Incidents concerning jobsearch 1y before ref. point	0.31	0.27	0.37	0.32	0.35	0.26	0.26	0.30	0.29	0.28	0.33	0.37	0.42	0.42	0.49	0.34	0.27	0.41	0.30	0.36
	(5.92)	(12.86)	(3.09)	(4.19)		(3.10)	(3.24)	(3.87)	(2.35)		(20.66)	(14.35)	(8.41)	(8.00)		(3.11)	(14.09)	(6.31)	(8.97)	
Incidents concerning misbehavior 1y before ref. point	0.06	0.05	0.09	0.07	0.07	0.05	0.06	0.09	0.06	0.07	0.19	0.12	0.13	0.15	0.17	0.06	0.06	0.11	0.06	0.08
	(3.67)	(7.07)	(6.31)	(0.63)		(7.23)	(2.99)	(6.08)	(1.90)		(2.47)	(10.09)	(9.10)	(4.74)		(6.32)	(8.24)	(7.08)	(6.44)	
Incidents concerning programs 1y before ref. point	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.05	0.00	0.01	0.02	0.01	0.01	0.00	0.01	0.00	0.01
	(0.18)	(2.43)	(3.26)	(0.30)		(0.55)	(0.44)	(3.75)	(3.22)		(16.01)	(9.29)	(1.22)	(1.01)		(0.20)	(1.64)	(2.25)	(2.39)	
Incidents concerning jobsearch in previous ue spells	0.49	0.42	0.65	0.56	0.51	0.37	0.38	0.47	0.51	0.46	0.69	0.60	0.73	0.97	0.91	0.68	0.51	0.95	0.66	0.78
	(1.93)	(7.72)	(10.27)	(3.68)		(8.41)	(7.42)	(1.16)	(4.54)		(13.59)	(19.87)	(10.44)	(3.12)		(6.57)	(17.94)	(9.51)	(7.34)	
Incidents concerning misbehavior in prev. ue spells	0.19	0.12	0.24	0.22	0.18	0.10	0.12	0.19	0.16	0.17	0.25	0.22	0.29	0.40	0.37	0.27	0.18	0.41	0.25	0.31
	(1.46)	(7.06)	(6.63)	(4.62)		(10.84)	(7.52)	(2.91)	(1.09)		(11.21)	(13.55)	(7.17)	(2.35)		(4.04)	(13.49)	(7.83)	(5.15)	

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Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
Incidents concerning programs in previous ue spells	0.03	0.02	0.03	0.03	0.02	0.01	0.01	0.01	0.02	0.02	0.05	0.04	0.03	0.04	0.05	0.03	0.02	0.03	0.03	0.03
	(2.09)	(2.80)	(4.11)	(4.20)		(1.53)	(2.43)	(1.79)	(0.30)		(0.26)	(0.22)	(3.70)	(0.67)		(1.78)	(4.21)	(1.11)	(1.82)	
Insured earnings before start of first program	5,312.05	5,057.50	4,749.42	4,895.88	5,609.46	5,180.85	5,063.43	4,602.79	4,766.90	5,315.23	4,647.16	4,391.73	4,561.06	4,514.90	4,576.71	4,744.52	4,356.96	4,303.87	4,682.45	4,843.73
	(12.70)	(24.13)	(39.70)	(33.66)		(5.92)	(10.79)	(33.72)	(26.31)		(3.45)	(9.84)	(0.81)	(3.53)		(5.59)	(27.55)	(31.99)	(8.92)	
Ever insufficient jobsearch efforts 1y before ref. point	0.26	0.26	0.30	0.28	0.29	0.24	0.23	0.26	0.26	0.24	0.28	0.29	0.31	0.30	0.32	0.29	0.26	0.32	0.27	0.29
	(5.91)	(7.39)	(3.07)	(3.02)		(0.02)	(0.78)	(4.58)	(5.81)		(9.01)	(7.18)	(1.89)	(4.39)		(0.73)	(6.64)	(5.98)	(4.58)	
Ever insufficient job search eff. in prev. ue spells	0.29	0.29	0.34	0.31	0.32	0.26	0.27	0.28	0.30	0.27	0.35	0.31	0.35	0.35	0.36	0.36	0.33	0.39	0.34	0.35
	(5.04)	(5.73)	(4.22)	(0.50)		(2.25)	(0.04)	(3.07)	(7.32)		(1.36)	(11.14)	(0.89)	(1.03)		(2.67)	(4.78)	(8.33)	(2.45)	
Language level Italian	0.91	0.79	1.01	0.96	0.94	0.97	0.86	1.16	1.14	1.15	1.00	1.01	1.35	1.26	1.10	0.96	1.02	1.20	1.11	0.97
	(1.20)	(7.40)	(3.51)	(0.98)		(7.95)	(13.84)	(0.67)	(0.28)		(4.76)	(4.45)	(10.94)	(7.07)		(0.68)	(2.48)	(10.37)	(6.35)	
Number of child allowances before reference point	0.21	0.20	0.20	0.21	0.18	0.24	0.21	0.24	0.24	0.21	0.34	0.23	0.22	0.27	0.23	0.13	0.11	0.16	0.12	0.14
	(3.69)	(3.30)	(3.07)	(3.67)		(3.95)	(0.81)	(4.21)	(4.56)		(15.23)	(0.54)	(1.27)	(5.82)		(1.71)	(7.15)	(3.18)	(3.59)	
Experience in last job																				
0 - Experience unknown	*0.06*	*0.07*	*0.08*	*0.09*	*0.08*	*0.06*	*0.06*	*0.08*	*0.08*	*0.06*	*0.08*	*0.10*	*0.08*	*0.09*	*0.08*	*0.08*	*0.08*	*0.09*	*0.09*	*0.09*
	(6.03)	*(3.09)*	*(2.69)*	*(3.64)*		*(1.48)*	*(1.78)*	*(5.36)*	*(5.19)*		*(2.11)*	*(9.35)*	*(0.44)*	*(4.94)*		*(2.13)*	*(4.13)*	*(1.77)*	*(1.46)*	
1 - No experience	*0.02*	*0.02*	*0.01*	*0.03*	*0.02*	*0.03*	*0.03*	*0.03*	*0.03*	*0.02*	*0.06*	*0.03*	*0.02*	*0.03*	*0.02*	*0.03*	*0.03*	*0.04*	*0.03*	*0.03*
	(0.22)	*(4.21)*	*(6.29)*	*(1.37)*		*(1.63)*	*(2.07)*	*(4.33)*	*(5.74)*		*(15.60)*	*(4.46)*	*(0.23)*	*(4.66)*		*(0.22)*	*(1.64)*	*(3.93)*	*(0.29)*	
2 - Less than 1 year of experience	*0.05*	*0.05*	*0.06*	*0.05*	*0.05*	*0.04*	*0.05*	*0.05*	*0.06*	*0.06*	*0.07*	*0.09*	*0.06*	*0.07*	*0.07*	*0.07*	*0.08*	*0.10*	*0.06*	*0.07*
	(1.70)	*(1.95)*	*(2.09)*	*(1.73)*		*(7.20)*	*(0.86)*	*(3.66)*	*(3.12)*		*(0.81)*	*(7.32)*	*(4.04)*	*(2.37)*		*(0.70)*	*(3.91)*	*(9.47)*	*(3.72)*	
3 - 1-3 years of experience	*0.20*	*0.19*	*0.22*	*0.20*	*0.18*	*0.22*	*0.24*	*0.23*	*0.23*	*0.21*	*0.25*	*0.24*	*0.23*	*0.24*	*0.23*	*0.21*	*0.27*	*0.23*	*0.21*	*0.20*
	(6.45)	*(3.88)*	*(10.20)*	*(5.42)*		*(0.96)*	*(7.52)*	*(4.91)*	*(5.35)*		*(4.70)*	*(1.52)*	*(0.92)*	*(3.34)*		*(1.22)*	*(15.27)*	*(7.16)*	*(0.67)*	
4 - More than 3 years of experience	*0.67*	*0.68*	*0.63*	*0.64*	*0.67*	*0.66*	*0.62*	*0.61*	*0.59*	*0.65*	*0.54*	*0.53*	*0.60*	*0.57*	*0.60*	*0.60*	*0.54*	*0.54*	*0.61*	*0.60*
	(1.29)	*(0.65)*	*(9.23)*	*(7.79)*		*(2.61)*	*(5.91)*	*(6.86)*	*(10.81)*		*(11.82)*	*(12.29)*	*(1.10)*	*(6.02)*		*(0.51)*	*(12.34)*	*(11.83)*	*(2.13)*	
Function in the last job																				
Auxiliary function	*0.26*	*0.31*	*0.33*	*0.33*	*0.20*	*0.27*	*0.33*	*0.37*	*0.40*	*0.26*	*0.31*	*0.45*	*0.39*	*0.50*	*0.41*	*0.35*	*0.40*	*0.43*	*0.33*	*0.30*
	(14.03)	*(23.59)*	*(29.48)*	*(28.60)*		*(0.27)*	*(13.90)*	*(23.19)*	*(28.26)*		*(20.01)*	*(8.48)*	*(4.78)*	*(17.08)*		*(10.94)*	*(20.86)*	*(27.74)*	*(7.23)*	
Management function	*0.06*	*0.06*	*0.03*	*0.05*	*0.10*	*0.06*	*0.05*	*0.02*	*0.03*	*0.06*	*0.06*	*0.02*	*0.03*	*0.02*	*0.04*	*0.03*	*0.02*	*0.02*	*0.04*	*0.04*
	(12.43)	*(12.90)*	*(25.68)*	*(19.78)*		*(1.97)*	*(6.50)*	*(18.48)*	*(15.17)*		*(9.86)*	*(5.95)*	*(1.03)*	*(8.60)*		*(3.21)*	*(7.93)*	*(13.26)*	*(0.45)*	
Other function	*0.01*	*0.00*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.00*	*0.00*	*0.01*	*0.00*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*
	(0.94)	*(3.04)*	*(1.40)*	*(1.25)*		*(1.08)*	*(0.35)*	*(1.32)*	*(4.63)*		*(0.28)*	*(4.83)*	*(0.60)*	*(4.69)*		*(3.67)*	*(3.78)*	*(1.07)*	*(2.90)*	
Technical function	*0.67*	*0.63*	*0.62*	*0.62*	*0.69*	*0.67*	*0.62*	*0.60*	*0.57*	*0.67*	*0.62*	*0.52*	*0.57*	*0.48*	*0.55*	*0.60*	*0.57*	*0.54*	*0.62*	*0.65*
	(5.27)	*(13.70)*	*(14.19)*	*(15.43)*		*(0.57)*	*(10.19)*	*(14.70)*	*(20.09)*		*(15.25)*	*(5.57)*	*(4.97)*	*(13.36)*		*(10.23)*	*(16.97)*	*(23.12)*	*(6.30)*	
Type of last job																				
Agricultural and forestry occupations	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.02*	*0.01*	*0.02*	*0.02*	*0.01*	*0.01*	*0.01*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*
	(3.80)	*(0.17)*	*(3.70)*	*(4.32)*		*(4.42)*	*(0.29)*	*(1.07)*	*(3.40)*		*(8.10)*	*(7.43)*	*(3.33)*	*(0.14)*		*(1.93)*	*(1.84)*	*(3.05)*	*(4.88)*	
Construction and mining occupations	*0.06*	*0.03*	*0.04*	*0.08*	*0.04*	*0.04*	*0.06*	*0.07*	*0.12*	*0.08*	*0.09*	*0.05*	*0.08*	*0.14*	*0.11*	*0.09*	*0.09*	*0.11*	*0.10*	*0.17*
	(8.42)	*(4.39)*	*(1.11)*	*(15.85)*		*(15.51)*	*(5.70)*	*(1.40)*	*(14.48)*		*(4.69)*	*(19.15)*	*(8.55)*	*(9.28)*		*(22.09)*	*(22.65)*	*(15.01)*	*(20.61)*	
Health, teaching, scientists, cultural occup.	*0.07*	*0.06*	*0.08*	*0.08*	*0.09*	*0.07*	*0.07*	*0.07*	*0.08*	*0.08*	*0.06*	*0.08*	*0.10*	*0.05*	*0.08*	*0.09*	*0.09*	*0.08*	*0.14*	*0.12*
	(8.10)	*(13.01)*	*(3.42)*	*(4.42)*		*(6.17)*	*(2.49)*	*(2.32)*	*(2.04)*		*(11.28)*	*(1.73)*	*(4.19)*	*(12.42)*		*(11.09)*	*(11.54)*	*(15.16)*	*(4.26)*	
Management, admin, banking, insurance, legal	*0.25*	*0.26*	*0.19*	*0.19*	*0.29*	*0.26*	*0.22*	*0.20*	*0.15*	*0.26*	*0.18*	*0.22*	*0.17*	*0.12*	*0.18*	*0.16*	*0.14*	*0.13*	*0.15*	*0.15*
	(9.45)	*(6.72)*	*(23.20)*	*(23.83)*		*(0.13)*	*(10.53)*	*(13.88)*	*(27.85)*		*(1.00)*	*(10.40)*	*(1.31)*	*(16.09)*		*(2.61)*	*(2.89)*	*(5.28)*	*(0.81)*	
Not classifiable	*0.05*	*0.08*	*0.06*	*0.07*	*0.04*	*0.04*	*0.05*	*0.06*	*0.05*	*0.03*	*0.02*	*0.05*	*0.06*	*0.06*	*0.04*	*0.06*	*0.05*	*0.06*	*0.04*	*0.04*
	(2.41)	*(14.23)*	*(7.87)*	*(9.48)*		*(6.75)*	*(9.68)*	*(13.49)*	*(10.33)*		*(14.74)*	*(1.48)*	*(4.99)*	*(9.10)*		*(11.65)*	*(5.44)*	*(9.79)*	*(4.36)*	
Occupations providing personal services	*0.18*	*0.19*	*0.19*	*0.19*	*0.13*	*0.20*	*0.23*	*0.24*	*0.24*	*0.18*	*0.25*	*0.21*	*0.23*	*0.24*	*0.21*	*0.20*	*0.32*	*0.26*	*0.25*	*0.19*
	(14.05)	*(18.00)*	*(19.04)*	*(16.62)*		*(4.39)*	*(12.80)*	*(14.70)*	*(14.75)*		*(9.38)*	*(0.85)*	*(5.18)*	*(8.16)*		*(3.34)*	*(29.09)*	*(16.19)*	*(15.01)*	

Continued on next page

Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**
Number of observations in sequence:	**4,743**	**3,781**	**3,845**	**4,603**	**26,358**	**1,075**	**9,906**	**1,735**	**2,392**	**9,992**	**108**	**402**	**11,490**	**1,774**	**6,225**	**2,157**	**2,042**	**2,062**	**41,014**	**35,231**
Production occupations in industry and trade	*0.14*	*0.14*	*0.16*	*0.17*	*0.11*	*0.12*	*0.12*	*0.16*	*0.14*	*0.10*	*0.20*	*0.17*	*0.14*	*0.17*	*0.14*	*0.16*	*0.14*	*0.17*	*0.12*	*0.12*
	(7.01)	*(8.74)*	*(13.72)*	*(15.08)*		*(4.65)*	*(6.83)*	*(15.97)*	*(12.83)*		*(16.05)*	*(7.21)*	*(0.34)*	*(6.90)*		*(12.14)*	*(4.59)*	*(13.77)*	*(1.80)*	
Technical and information techn. occupations	*0.06*	*0.07*	*0.07*	*0.05*	*0.08*	*0.07*	*0.09*	*0.05*	*0.06*	*0.09*	*0.06*	*0.05*	*0.06*	*0.05*	*0.06*	*0.06*	*0.04*	*0.05*	*0.04*	*0.06*
	(7.64)	*(6.29)*	*(6.21)*	*(11.52)*		*(7.10)*	*(1.20)*	*(14.84)*	*(12.21)*		*(1.62)*	*(6.02)*	*(0.02)*	*(5.72)*		*(1.49)*	*(9.89)*	*(4.09)*	*(6.60)*	
Trade and transport occupations	*0.18*	*0.16*	*0.18*	*0.17*	*0.20*	*0.19*	*0.14*	*0.13*	*0.14*	*0.16*	*0.13*	*0.16*	*0.14*	*0.14*	*0.15*	*0.15*	*0.13*	*0.13*	*0.14*	*0.13*
	(4.24)	*(10.24)*	*(3.93)*	*(8.61)*		*(8.84)*	*(6.30)*	*(9.36)*	*(6.02)*		*(6.41)*	*(1.31)*	*(2.51)*	*(2.32)*		*(4.91)*	*(2.01)*	*(2.08)*	*(2.40)*	
Language level local language	**5.39**	**5.23**	**5.72**	**5.60**	**6.13**	**5.05**	**4.21**	**4.81**	**4.40**	**5.06**	**5.48**	**5.21**	**5.37**	**5.20**	**5.51**	**5.52**	**4.29**	**5.27**	**5.45**	**5.66**
	(40.15)	**(48.23)**	**(24.13)**	**(30.55)**		**(0.67)**	**(34.13)**	**(10.57)**	**(27.05)**		**(1.46)**	**(14.31)**	**(7.06)**	**(15.16)**		**(6.65)**	**(60.34)**	**(18.86)**	**(10.09)**	
Sector of last employer																				
Agriculture, forestry and fishing	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*
	(4.21)	*(0.89)*	*(3.00)*	*(0.95)*		*(3.26)*	*(1.32)*	*(0.81)*	*(1.86)*		*(0.39)*	*(0.32)*	*(0.98)*	*(2.13)*		*(0.28)*	*(0.75)*	*(0.19)*	*(2.92)*	
Construction	*0.06*	*0.05*	*0.05*	*0.08*	*0.05*	*0.06*	*0.07*	*0.07*	*0.12*	*0.08*	*0.07*	*0.05*	*0.08*	*0.12*	*0.10*	*0.09*	*0.08*	*0.10*	*0.09*	*0.14*
	(3.32)	*(2.73)*	*(0.16)*	*(10.89)*		*(9.03)*	*(4.86)*	*(2.99)*	*(12.38)*		*(9.90)*	*(17.70)*	*(6.86)*	*(6.04)*		*(18.05)*	*(21.07)*	*(11.79)*	*(15.82)*	
Financial and insurance activities	*0.09*	*0.06*	*0.05*	*0.04*	*0.08*	*0.09*	*0.06*	*0.05*	*0.02*	*0.07*	*0.04*	*0.04*	*0.04*	*0.02*	*0.04*	*0.03*	*0.02*	*0.02*	*0.03*	*0.03*
	(2.17)	*(8.81)*	*(12.29)*	*(17.11)*		*(7.38)*	*(3.93)*	*(6.02)*	*(20.99)*		*(0.05)*	*(3.95)*	*(0.89)*	*(11.99)*		*(2.52)*	*(2.30)*	*(1.47)*	*(0.28)*	
Information and communication	*0.03*	*0.03*	*0.02*	*0.02*	*0.05*	*0.03*	*0.04*	*0.02*	*0.02*	*0.04*	*0.04*	*0.01*	*0.02*	*0.01*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*
	(9.33)	*(7.32)*	*(11.49)*	*(13.67)*		*(6.21)*	*(0.78)*	*(10.12)*	*(11.17)*		*(7.71)*	*(8.51)*	*(0.22)*	*(10.05)*		*(0.12)*	*(2.10)*	*(4.83)*	*(0.46)*	
Manufacturing, mining, quarrying, other industry	*0.18*	*0.21*	*0.20*	*0.20*	*0.18*	*0.18*	*0.16*	*0.18*	*0.16*	*0.14*	*0.25*	*0.22*	*0.18*	*0.19*	*0.19*	*0.18*	*0.13*	*0.16*	*0.12*	*0.12*
	(1.40)	*(7.87)*	*(5.65)*	*(5.20)*		*(9.48)*	*(5.62)*	*(10.75)*	*(5.33)*		*(14.52)*	*(6.60)*	*(3.21)*	*(1.02)*		*(16.53)*	*(2.13)*	*(9.77)*	*(0.00)*	
Other services	*0.07*	*0.08*	*0.07*	*0.07*	*0.07*	*0.07*	*0.08*	*0.08*	*0.07*	*0.08*	*0.11*	*0.09*	*0.08*	*0.07*	*0.07*	*0.08*	*0.11*	*0.09*	*0.11*	*0.09*
	(0.65)	*(3.87)*	*(1.28)*	*(0.43)*		*(0.94)*	*(2.60)*	*(0.48)*	*(0.75)*		*(16.00)*	*(8.92)*	*(4.85)*	*(3.20)*		*(3.13)*	*(5.50)*	*(0.28)*	*(5.29)*	
Prof., scientific, technical and admin services	*0.15*	*0.16*	*0.14*	*0.14*	*0.15*	*0.15*	*0.18*	*0.15*	*0.18*	*0.17*	*0.11*	*0.11*	*0.14*	*0.14*	*0.13*	*0.18*	*0.22*	*0.16*	*0.17*	*0.17*
	(0.55)	*(1.88)*	*(4.10)*	*(1.56)*		*(5.31)*	*(0.87)*	*(6.39)*	*(0.91)*		*(6.35)*	*(5.31)*	*(2.48)*	*(2.63)*		*(0.68)*	*(10.82)*	*(3.87)*	*(1.06)*	
Publ. admin, defence, educ., health, social work	*0.09*	*0.07*	*0.11*	*0.10*	*0.10*	*0.08*	*0.08*	*0.09*	*0.08*	*0.09*	*0.08*	*0.10*	*0.12*	*0.08*	*0.11*	*0.09*	*0.10*	*0.10*	*0.13*	*0.12*
	(3.81)	*(9.64)*	*(2.05)*	*(1.16)*		*(5.96)*	*(3.73)*	*(1.57)*	*(4.67)*		*(8.67)*	*(3.91)*	*(4.50)*	*(10.71)*		*(8.38)*	*(6.93)*	*(5.63)*	*(4.20)*	
Real estate activities	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.00*	*0.02*	*0.01*	*0.01*	*0.01*	*0.01*	*0.00*	*0.01*	*0.01*	*0.01*
	(2.19)	*(0.43)*	*(2.28)*	*(1.07)*		*(2.45)*	*(1.15)*	*(7.22)*	*(4.86)*		*(12.72)*	*(10.12)*	*(0.92)*	*(2.78)*		*(0.91)*	*(2.44)*	*(2.25)*	*(2.12)*	
Wholesale, retail, transport, accomodation, food	*0.32*	*0.33*	*0.35*	*0.34*	*0.31*	*0.33*	*0.31*	*0.34*	*0.32*	*0.30*	*0.29*	*0.34*	*0.32*	*0.34*	*0.32*	*0.31*	*0.31*	*0.33*	*0.31*	*0.28*
	(0.97)	*(4.65)*	*(7.32)*	*(6.12)*		*(5.21)*	*(0.64)*	*(7.40)*	*(4.56)*		*(7.81)*	*(3.77)*	*(1.10)*	*(3.94)*		*(5.82)*	*(6.19)*	*(9.30)*	*(5.78)*	
Mandatory job applications during 1y before ref. point	**0.53**	**0.43**	**0.46**	**0.58**	**0.54**	**0.54**	**0.47**	**0.61**	**0.59**	**0.61**	**0.83**	**0.61**	**0.76**	**0.79**	**0.79**	**0.43**	**0.35**	**0.54**	**0.41**	**0.49**
	(0.54)	**(9.75)**	**(6.52)**	**(2.99)**		**(5.33)**	**(11.54)**	**(0.37)**	**(1.96)**		**(2.58)**	**(11.14)**	**(1.64)**	**(0.30)**		**(4.42)**	**(11.65)**	**(3.26)**	**(6.00)**	
Mandatory job applications in previous ue spells	**0.75**	**0.61**	**0.91**	**0.80**	**0.70**	**0.71**	**0.71**	**0.84**	**0.92**	**0.81**	**1.29**	**0.78**	**1.11**	**1.40**	**1.27**	**0.98**	**0.87**	**1.37**	**1.08**	**1.10**
	(2.38)	**(4.37)**	**(8.67)**	**(4.85)**		**(4.22)**	**(4.35)**	**(1.30)**	**(4.04)**		**(0.60)**	**(16.92)**	**(5.03)**	**(4.00)**		**(4.25)**	**(8.37)**	**(8.73)**	**(0.71)**	
Maternity benefits during year before ref. point	**0.04**	**0.04**	**0.04**	**0.04**	**0.04**	**0.03**	**0.04**	**0.04**	**0.02**	**0.03**	**0.03**	**0.06**	**0.04**	**0.04**	**0.04**	**0.02**	**0.02**	**0.02**	**0.04**	**0.02**
	(4.66)	**(1.51)**	**(0.08)**	**(3.28)**		**(2.81)**	**(2.70)**	**(0.78)**	**(7.30)**		**(8.18)**	**(6.57)**	**(2.69)**	**(3.50)**		**(3.15)**	**(1.69)**	**(1.17)**	**(8.36)**	
Max daily allowances for current unemployment spell	**390.89**	**394.81**	**391.69**	**388.98**	**389.73**	**393.50**	**382.01**	**386.34**	**377.52**	**380.40**	**372.41**	**385.42**	**382.26**	**377.39**	**372.66**	**380.70**	**373.71**	**373.78**	**379.84**	**372.58**
	(1.96)	**(8.75)**	**(3.31)**	**(1.27)**		**(20.57)**	**(2.43)**	**(9.07)**	**(4.22)**		**(0.36)**	**(18.72)**	**(13.77)**	**(6.66)**		**(11.75)**	**(1.61)**	**(1.69)**	**(10.32)**	
Months in basic course 1y before current ue spell	**0.01**	**0.00**	**0.00**	**0.00**	**0.00**	**0.01**	**0.00**	**0.01**	**0.01**	**0.01**	**0.01**	**0.01**	**0.01**	**0.02**	**0.01**	**0.01**	**0.02**	**0.02**	**0.02**	**0.02**
	(2.10)	**(1.38)**	**(4.35)**	**(0.42)**		**(3.98)**	**(7.47)**	**(4.22)**	**(0.48)**		**(4.15)**	**(3.40)**	**(0.28)**	**(2.08)**		**(6.97)**	**(1.35)**	**(1.46)**	**(2.00)**	
Months in basic course 5y before current ue spell	**0.26**	**0.15**	**0.23**	**0.20**	**0.18**	**0.19**	**0.16**	**0.20**	**0.21**	**0.19**	**0.41**	**0.18**	**0.27**	**0.26**	**0.21**	**0.21**	**0.21**	**0.26**	**0.22**	**0.20**
	(8.98)	**(4.51)**	**(5.64)**	**(2.30)**		**(0.20)**	**(4.34)**	**(1.25)**	**(2.02)**		**(18.97)**	**(4.02)**	**(6.14)**	**(5.70)**		**(1.05)**	**(0.96)**	**(6.96)**	**(2.18)**	
Months in employment program 1y before current ue spell	**0.00**	**0.01**	**0.00**	**0.00**	**0.01**	**0.00**	**0.00**	**0.01**	**0.02**	**0.02**	**0.04**	**0.02**	**0.03**	**0.05**	**0.06**	**0.01**	**0.01**	**0.04**	**0.02**	**0.03**
	(1.39)	**(0.44)**	**(1.19)**	**(1.34)**		**(10.33)**	**(6.73)**	**(1.10)**	**(0.53)**		**(6.21)**	**(9.91)**	**(7.38)**	**(2.58)**		**(8.40)**	**(6.45)**	**(4.33)**	**(2.83)**	
Months in employment program 5y before current ue spell	**0.23**	**0.15**	**0.33**	**0.21**	**0.17**	**0.14**	**0.18**	**0.36**	**0.22**	**0.22**	**0.78**	**0.40**	**0.60**	**0.57**	**0.61**	**0.24**	**0.24**	**0.64**	**0.31**	**0.27**
	(4.99)	**(2.43)**	**(12.77)**	**(3.61)**		**(8.35)**	**(4.02)**	**(10.73)**	**(0.17)**		**(8.76)**	**(13.03)**	**(0.29)**	**(2.04)**		**(2.75)**	**(2.16)**	**(23.55)**	**(3.02)**	

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Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
Avg duration of previous employment spells (in months)	49.98	53.38	48.46	50.30	51.48	51.63	44.02	45.09	43.07	45.34	39.07	46.34	43.07	40.48	40.41	43.30	40.94	37.87	44.44	39.97
	(4.02)	(5.00)	(8.15)	(3.14)		(16.93)	(3.70)	(0.69)	(6.28)		(3.88)	(16.41)	(7.53)	(0.19)		(9.51)	(2.81)	(6.19)	(12.72)	
Months from start of ue spell to start of 1st program	2.87	2.17	2.32	2.41	2.72	2.82	2.51	2.90	2.65	3.07	3.84	3.32	3.77	3.37	3.85	1.51	1.36	1.85	1.56	1.82
	(5.89)	(25.01)	(17.95)	(13.58)		(10.41)	(23.54)	(7.23)	(17.57)		(0.43)	(20.74)	(3.10)	(17.41)		(13.68)	(20.67)	(0.99)	(10.82)	
Avg duration of previous unemployment spells (in months)	4.87	3.97	4.60	4.27	4.23	4.46	4.01	4.58	4.23	4.43	5.86	4.77	5.58	5.17	5.22	4.47	3.97	5.09	5.06	4.16
	(16.28)	(6.87)	(9.27)	(1.03)		(0.74)	(11.34)	(3.97)	(5.43)		(16.69)	(11.99)	(9.27)	(1.16)		(6.57)	(3.91)	(19.27)	(16.91)	
Months in training course 1y before current ue spell	0.01	0.01	0.00	0.01	0.01	0.00	0.01	0.01	0.03	0.03	0.01	0.00	0.01	0.02	0.02	0.01	0.03	0.02	0.02	0.02
	(0.36)	(2.41)	(3.53)	(0.18)		(13.02)	(5.27)	(6.54)	(0.83)		(6.29)	(9.32)	(4.00)	(1.73)		(6.64)	(1.68)	(1.26)	(2.56)	
Months in training course 5y before current ue spell	0.24	0.27	0.24	0.20	0.17	0.33	0.32	0.32	0.30	0.31	0.32	0.30	0.30	0.29	0.26	0.24	0.35	0.34	0.26	0.22
	(7.69)	(9.39)	(7.73)	(3.45)		(2.21)	(1.21)	(1.15)	(0.59)		(6.06)	(3.64)	(3.56)	(2.37)		(1.77)	(11.51)	(10.66)	(4.20)	
Mother tongue (9 categories)																				
Albanian	*0.08*	*0.08*	*0.09*	*0.09*	*0.05*	*0.07*	*0.08*	*0.07*	*0.08*	*0.05*	*0.10*	*0.07*	*0.07*	*0.09*	*0.08*	*0.08*	*0.08*	*0.08*	*0.07*	*0.06*
	(13.43)	*(12.55)*	*(13.83)*	*(13.87)*		*(7.19)*	*(11.40)*	*(7.41)*	*(12.06)*		*(8.47)*	*(3.04)*	*(1.56)*	*(5.13)*		*(9.73)*	*(9.66)*	*(7.06)*	*(4.61)*	
English	*0.02*	*0.02*	*0.01*	*0.01*	*0.01*	*0.03*	*0.04*	*0.02*	*0.02*	*0.04*	*0.00*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.02*	*0.01*	*0.01*	*0.01*
	(4.35)	*(2.61)*	*(2.80)*	*(3.18)*		*(7.23)*	*(2.23)*	*(9.32)*	*(9.99)*		*(15.19)*	*(0.95)*	*(0.88)*	*(2.37)*		*(0.59)*	*(7.02)*	*(3.81)*	*(0.60)*	
French	*0.24*	*0.19*	*0.17*	*0.13*	*0.15*	*0.25*	*0.26*	*0.24*	*0.19*	*0.27*	*0.23*	*0.25*	*0.21*	*0.15*	*0.19*	*0.14*	*0.20*	*0.17*	*0.18*	*0.17*
	(23.32)	*(12.61)*	*(5.27)*	*(3.99)*		*(3.69)*	*(1.35)*	*(5.79)*	*(19.61)*		*(11.42)*	*(16.07)*	*(5.73)*	*(9.89)*		*(6.87)*	*(8.77)*	*(0.42)*	*(3.80)*	
German	*0.27*	*0.32*	*0.38*	*0.41*	*0.53*	*0.23*	*0.13*	*0.17*	*0.19*	*0.24*	*0.29*	*0.25*	*0.27*	*0.28*	*0.35*	*0.40*	*0.17*	*0.29*	*0.34*	*0.43*
	(53.86)	*(43.84)*	*(30.81)*	*(24.08)*		*(1.52)*	*(28.49)*	*(18.22)*	*(12.35)*		*(12.65)*	*(21.93)*	*(16.99)*	*(13.95)*		*(4.83)*	*(59.44)*	*(29.29)*	*(17.60)*	
Italian	*0.06*	*0.04*	*0.07*	*0.06*	*0.06*	*0.06*	*0.05*	*0.09*	*0.09*	*0.08*	*0.06*	*0.06*	*0.11*	*0.10*	*0.08*	*0.05*	*0.08*	*0.09*	*0.08*	*0.06*
	(0.98)	*(5.65)*	*(6.29)*	*(3.08)*		*(6.72)*	*(10.62)*	*(1.93)*	*(1.90)*		*(3.99)*	*(7.17)*	*(10.73)*	*(8.96)*		*(3.05)*	*(5.37)*	*(11.38)*	*(5.95)*	
Other	*0.18*	*0.20*	*0.15*	*0.17*	*0.12*	*0.17*	*0.21*	*0.19*	*0.19*	*0.14*	*0.14*	*0.21*	*0.16*	*0.17*	*0.14*	*0.16*	*0.19*	*0.18*	*0.15*	*0.11*
	(17.19)	*(21.97)*	*(10.92)*	*(15.75)*		*(8.87)*	*(18.29)*	*(13.42)*	*(12.09)*		*(0.53)*	*(18.63)*	*(6.38)*	*(9.19)*		*(14.59)*	*(21.23)*	*(18.50)*	*(11.17)*	
Portuguese	*0.08*	*0.07*	*0.08*	*0.07*	*0.04*	*0.10*	*0.12*	*0.14*	*0.16*	*0.11*	*0.10*	*0.09*	*0.11*	*0.13*	*0.12*	*0.08*	*0.17*	*0.12*	*0.10*	*0.11*
	(15.89)	*(10.71)*	*(14.25)*	*(11.37)*		*(5.24)*	*(3.32)*	*(7.53)*	*(14.02)*		*(4.23)*	*(7.54)*	*(1.25)*	*(3.28)*		*(11.16)*	*(15.34)*	*(2.99)*	*(2.60)*	
Spanish	*0.04*	*0.04*	*0.02*	*0.03*	*0.02*	*0.05*	*0.06*	*0.05*	*0.05*	*0.04*	*0.05*	*0.03*	*0.03*	*0.04*	*0.03*	*0.03*	*0.07*	*0.03*	*0.03*	*0.03*
	(9.85)	*(12.08)*	*(2.79)*	*(3.89)*		*(2.95)*	*(8.52)*	*(4.03)*	*(4.91)*		*(10.45)*	*(0.43)*	*(3.57)*	*(5.10)*		*(1.16)*	*(19.09)*	*(3.26)*	*(3.56)*	
Turkish	*0.03*	*0.04*	*0.03*	*0.03*	*0.02*	*0.04*	*0.03*	*0.03*	*0.03*	*0.02*	*0.03*	*0.03*	*0.03*	*0.03*	*0.02*	*0.03*	*0.03*	*0.03*	*0.03*	*0.02*
	(4.42)	*(10.10)*	*(6.38)*	*(4.71)*		*(7.63)*	*(6.36)*	*(4.80)*	*(6.33)*		*(3.26)*	*(5.92)*	*(1.80)*	*(6.75)*		*(9.62)*	*(6.73)*	*(8.84)*	*(5.60)*	
Months with wage subsidy 1y before current ue spell	0.04	0.03	0.03	0.07	0.04	0.04	0.04	0.06	0.11	0.06	0.01	0.09	0.08	0.16	0.11	0.11	0.17	0.22	0.28	0.23
	(0.11)	(1.66)	(1.37)	(6.42)		(4.87)	(4.79)	(0.22)	(8.04)		(21.99)	(2.69)	(5.40)	(7.27)		(13.61)	(6.91)	(1.61)	(3.90)	
Months with wage subsidy 5y before current ue spell	0.84	0.69	0.86	1.17	0.70	0.73	0.64	0.93	1.18	0.79	0.86	0.88	1.17	1.67	1.12	1.44	1.45	2.06	2.41	1.74
	(4.41)	(0.63)	(5.24)	(14.24)		(2.16)	(5.15)	(4.49)	(11.50)		(8.11)	(7.36)	(1.25)	(13.86)		(7.07)	(6.55)	(6.67)	(12.88)	
Number of canceled appointments in year before ref. point	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.01	0.03	0.03	0.03	0.02	0.02	0.03	0.02	0.03
	(0.51)	(2.61)	(0.82)	(0.65)		(2.92)	(2.27)	(1.76)	(0.60)		(14.56)	(12.00)	(1.85)	(1.48)		(5.35)	(2.61)	(1.69)	(1.81)	
Canceled job center appointments before current ue spell	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.02	0.01	0.02	0.01	0.01	0.02	0.01	0.02
	(1.95)	(1.70)	(2.34)	(1.20)		(4.18)	(0.53)	(0.45)	(0.82)		(6.04)	(10.40)	(0.78)	(1.38)		(4.93)	(1.90)	(1.22)	(1.63)	
Job center appointments before current ue spell	5.54	4.75	5.49	5.39	4.77	5.23	4.69	5.44	5.51	5.02	7.37	5.11	6.43	6.69	6.00	6.18	5.67	7.30	6.72	6.31
	(10.81)	(0.35)	(10.39)	(8.90)		(2.88)	(4.85)	(5.79)	(6.74)		(16.55)	(12.32)	(5.27)	(8.41)		(1.60)	(8.08)	(11.55)	(4.89)	
No-show job center appointments before current ue spell	0.36	0.25	0.38	0.37	0.30	0.23	0.26	0.36	0.32	0.30	0.46	0.36	0.43	0.61	0.51	0.49	0.38	0.62	0.44	0.51
	(6.28)	(5.01)	(7.95)	(6.56)		(7.38)	(4.37)	(6.46)	(2.25)		(4.02)	(12.81)	(6.71)	(7.20)		(2.03)	(11.08)	(7.79)	(5.55)	
Number of no-show appointments in year before ref. point	0.09	0.06	0.10	0.09	0.09	0.08	0.08	0.09	0.09	0.08	0.14	0.12	0.13	0.14	0.16	0.10	0.10	0.13	0.08	0.11
	(0.45)	(8.05)	(3.48)	(0.24)		(2.43)	(1.20)	(2.27)	(0.92)		(3.76)	(8.66)	(5.61)	(2.89)		(3.83)	(4.48)	(3.71)	(7.36)	
Postponed jobcenter appointments before current ue spell	0.48	0.39	0.48	0.51	0.43	0.43	0.40	0.45	0.56	0.46	0.66	0.63	0.59	0.82	0.66	0.65	0.60	0.82	0.71	0.70
	(4.52)	(4.50)	(4.46)	(6.89)		(2.63)	(5.26)	(0.34)	(8.81)		(0.48)	(2.35)	(5.29)	(10.98)		(3.96)	(7.47)	(7.75)	(0.54)	

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Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
Number of postponed appointments in year bef. ref. point	0.35	0.26	0.30	0.31	0.31	0.33	0.31	0.34	0.36	0.35	0.45	0.45	0.43	0.41	0.46	0.33	0.30	0.39	0.32	0.35
	(5.18)	(7.75)	(1.85)	(0.37)		(3.05)	(4.94)	(0.58)	(2.47)		(0.42)	(0.89)	(3.34)	(5.47)		(3.26)	(7.12)	(4.50)	(3.90)	
Number of appointments by phone in year before ref. point	0.06	0.03	0.06	0.05	0.05	0.05	0.05	0.06	0.05	0.06	0.13	0.07	0.09	0.09	0.08	0.04	0.04	0.05	0.05	0.05
	(3.08)	(6.30)	(4.67)	(1.85)		(0.12)	(2.42)	(2.07)	(0.33)		(12.67)	(1.20)	(1.79)	(2.28)		(2.16)	(2.36)	(1.18)	(1.52)	
Job center appointments by phone before current ue spell	0.02	0.01	0.03	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.03	0.03	0.03	0.03	0.03	0.02	0.03	0.03	0.02	0.02
	(0.39)	(2.36)	(6.51)	(2.54)		(0.30)	(1.56)	(0.74)	(0.77)		(0.90)	(0.58)	(1.36)	(2.33)		(2.53)	(2.46)	(2.73)	(0.25)	
Number of appointments in year before ref. point	3.71	3.26	3.24	3.38	3.54	3.91	3.55	3.84	3.75	3.89	4.67	3.89	4.24	3.80	3.95	2.72	2.65	2.81	2.66	2.78
	(8.05)	(14.18)	(14.66)	(7.51)		(0.78)	(16.60)	(2.65)	(6.49)		(27.19)	(2.80)	(11.25)	(6.03)		(2.92)	(6.19)	(1.16)	(5.32)	
3-level nationality swiss/EU/non-EU																				
EU	*0.27*	*0.25*	*0.26*	*0.25*	*0.28*	*0.28*	*0.34*	*0.33*	*0.36*	*0.34*	*0.26*	*0.26*	*0.31*	*0.30*	*0.30*	*0.26*	*0.36*	*0.31*	*0.29*	*0.32*
	(1.58)	*(4.70)*	*(3.59)*	*(4.97)*		*(11.19)*	*(1.70)*	*(1.94)*	*(4.90)*		*(9.47)*	*(9.04)*	*(1.07)*	*(1.41)*		*(12.24)*	*(7.56)*	*(3.29)*	*(6.70)*	
Swiss	*0.48*	*0.48*	*0.50*	*0.51*	*0.58*	*0.48*	*0.36*	*0.40*	*0.38*	*0.48*	*0.54*	*0.43*	*0.46*	*0.43*	*0.49*	*0.48*	*0.36*	*0.43*	*0.51*	*0.52*
	(18.78)	*(20.23)*	*(14.75)*	*(13.42)*		*(1.16)*	*(24.42)*	*(14.73)*	*(20.38)*		*(9.37)*	*(11.50)*	*(6.44)*	*(12.75)*		*(8.14)*	*(34.21)*	*(17.65)*	*(3.40)*	
Non-EU	*0.25*	*0.27*	*0.24*	*0.24*	*0.15*	*0.23*	*0.30*	*0.27*	*0.26*	*0.19*	*0.20*	*0.31*	*0.24*	*0.28*	*0.21*	*0.25*	*0.29*	*0.26*	*0.21*	*0.16*
	(25.33)	*(30.13)*	*(22.72)*	*(22.59)*		*(11.26)*	*(26.19)*	*(19.64)*	*(18.48)*		*(1.07)*	*(22.54)*	*(6.56)*	*(16.34)*		*(23.99)*	*(32.01)*	*(25.56)*	*(12.43)*	
Months w/o income subj. to contrib. 5y before ref. p.	4.17	4.46	3.81	3.45	3.38	4.73	8.17	5.84	7.31	5.90	4.19	4.82	4.38	4.44	3.92	4.25	8.15	4.82	3.84	4.08
	(8.47)	(11.43)	(4.70)	(0.76)		(10.53)	(18.28)	(0.51)	(11.59)		(2.79)	(9.18)	(4.79)	(5.38)		(1.74)	(34.40)	(7.27)	(2.44)	
Months w/o income subj. to contrib. 7y before ref. point	9.16	9.97	8.30	7.89	7.60	10.33	17.29	12.92	15.53	12.56	8.56	11.01	9.82	9.97	8.82	9.25	16.82	10.62	8.49	8.93
	(9.29)	(13.90)	(4.30)	(1.83)		(11.32)	(22.01)	(1.80)	(14.00)		(1.51)	(12.28)	(5.78)	(6.60)		(1.84)	(38.56)	(9.44)	(2.52)	
Number of kids (only available for women)	0.13	0.13	0.13	0.12	0.15	0.12	0.13	0.11	0.09	0.13	0.07	0.15	0.13	0.12	0.16	0.09	0.08	0.09	0.14	0.10
	(4.70)	(5.17)	(3.22)	(6.46)		(4.37)	(2.14)	(6.37)	(11.38)		(21.07)	(1.66)	(5.24)	(8.51)		(3.12)	(6.02)	(4.13)	(8.48)	
Number of previous unemployment spells	1.89	1.70	1.96	1.94	1.89	1.86	1.66	1.92	1.83	1.92	2.38	2.05	2.18	2.36	2.47	1.83	1.50	2.05	1.80	2.07
	(0.44)	(13.98)	(4.94)	(2.94)		(4.38)	(18.86)	(0.00)	(6.23)		(4.82)	(24.49)	(16.21)	(5.76)		(13.18)	(32.21)	(0.72)	(14.69)	
Months out of labor force in 3 years before ref. point	2.07	2.00	1.99	1.87	1.83	2.12	3.41	2.54	3.19	2.74	2.10	2.17	2.15	2.32	2.31	2.38	3.81	2.51	2.07	2.36
	(5.08)	(3.61)	(3.36)	(0.83)		(11.47)	(11.20)	(3.71)	(7.59)		(4.04)	(2.84)	(3.29)	(0.19)		(0.34)	(23.85)	(2.99)	(5.71)	
Open positions in desired job in canton per 100k pop.	9.16	11.87	8.96	12.63	10.34	10.61	11.64	11.03	14.07	12.37	8.39	8.48	8.85	10.34	8.93	17.12	18.05	9.84	14.40	17.26
	(3.34)	(3.89)	(4.09)	(5.55)		(4.95)	(1.89)	(3.72)	(4.07)		(2.05)	(1.61)	(0.28)	(4.11)		(0.24)	(1.33)	(16.01)	(5.15)	
Language level best non-native language	0.75	0.63	0.65	0.64	0.63	0.81	0.70	0.57	0.75	0.62	0.76	0.58	0.61	0.56	0.54	0.63	0.66	0.56	0.60	0.58
	(7.36)	(0.43)	(0.93)	(0.31)		(10.98)	(5.02)	(3.15)	(7.79)		(13.45)	(2.70)	(4.30)	(0.88)		(2.94)	(5.10)	(1.71)	(1.41)	
Type of work permit																				
B	*0.22*	*0.23*	*0.21*	*0.21*	*0.17*	*0.24*	*0.40*	*0.32*	*0.36*	*0.30*	*0.19*	*0.29*	*0.25*	*0.28*	*0.24*	*0.24*	*0.39*	*0.28*	*0.23*	*0.24*
	(11.49)	*(15.30)*	*(9.59)*	*(9.53)*		*(12.35)*	*(20.59)*	*(4.22)*	*(13.75)*		*(11.30)*	*(10.76)*	*(1.83)*	*(8.27)*		*(1.83)*	*(31.57)*	*(8.31)*	*(2.76)*	
C	*0.30*	*0.29*	*0.29*	*0.28*	*0.25*	*0.27*	*0.25*	*0.28*	*0.26*	*0.23*	*0.27*	*0.28*	*0.29*	*0.30*	*0.27*	*0.28*	*0.26*	*0.28*	*0.26*	*0.23*
	(10.75)	*(8.85)*	*(8.06)*	*(6.66)*		*(11.32)*	*(5.19)*	*(12.34)*	*(8.35)*		*(0.05)*	*(2.21)*	*(5.40)*	*(6.04)*		*(11.11)*	*(5.18)*	*(11.73)*	*(6.65)*	
Other	*0.48*	*0.48*	*0.50*	*0.51*	*0.58*	*0.48*	*0.36*	*0.40*	*0.38*	*0.48*	*0.54*	*0.43*	*0.46*	*0.43*	*0.49*	*0.48*	*0.36*	*0.43*	*0.51*	*0.52*
	(18.78)	*(20.23)*	*(14.75)*	*(13.42)*		*(1.16)*	*(24.42)*	*(14.73)*	*(20.38)*		*(9.37)*	*(11.50)*	*(6.44)*	*(12.75)*		*(8.14)*	*(34.21)*	*(17.65)*	*(3.40)*	
Placeability (last evaluation before ref. point)																				
0 - not available	*0.51*	*0.39*	*0.44*	*0.37*	*0.34*	*0.45*	*0.42*	*0.42*	*0.38*	*0.39*	*0.52*	*0.47*	*0.45*	*0.47*	*0.51*	*0.52*	*0.54*	*0.56*	*0.57*	*0.55*
	(33.95)	*(10.16)*	*(18.85)*	*(5.52)*		*(11.18)*	*(6.16)*	*(6.21)*	*(2.69)*		*(2.46)*	*(8.21)*	*(12.08)*	*(6.99)*		*(6.52)*	*(1.38)*	*(2.83)*	*(3.78)*	
1 - difficult	*0.11*	*0.12*	*0.11*	*0.09*	*0.08*	*0.10*	*0.14*	*0.14*	*0.11*	*0.10*	*0.16*	*0.15*	*0.14*	*0.14*	*0.12*	*0.08*	*0.09*	*0.09*	*0.07*	*0.06*
	(11.71)	*(15.10)*	*(10.40)*	*(6.61)*		*(0.01)*	*(11.46)*	*(11.66)*	*(3.39)*		*(11.35)*	*(8.42)*	*(5.70)*	*(6.60)*		*(8.43)*	*(12.12)*	*(14.67)*	*(3.79)*	
2 - medium	*0.28*	*0.39*	*0.38*	*0.39*	*0.40*	*0.31*	*0.34*	*0.35*	*0.37*	*0.37*	*0.23*	*0.34*	*0.33*	*0.31*	*0.31*	*0.30*	*0.28*	*0.28*	*0.27*	*0.27*
	(26.81)	*(1.82)*	*(5.36)*	*(1.93)*		*(13.39)*	*(6.85)*	*(4.12)*	*(0.48)*		*(17.12)*	*(6.10)*	*(4.71)*	*(0.22)*		*(5.53)*	*(0.69)*	*(1.18)*	*(1.27)*	
3 - easy	*0.10*	*0.09*	*0.08*	*0.14*	*0.18*	*0.15*	*0.10*	*0.09*	*0.14*	*0.14*	*0.09*	*0.05*	*0.09*	*0.08*	*0.07*	*0.11*	*0.09*	*0.06*	*0.10*	*0.12*
	(21.43)	*(25.17)*	*(28.79)*	*(9.73)*		*(2.33)*	*(10.55)*	*(15.21)*	*(0.12)*		*(8.93)*	*(6.74)*	*(7.20)*	*(4.49)*		*(4.22)*	*(8.85)*	*(20.09)*	*(7.07)*	

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Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
Population of municipality of residence (in 1,000)	63.42	38.08	34.23	49.42	54.15	71.81	61.96	47.53	54.31	57.29	42.10	30.64	36.89	27.38	29.98	49.57	57.76	32.27	51.34	51.54
	(9.12)	(17.32)	(22.34)	(4.67)		(13.71)	(4.76)	(10.78)	(3.07)		(20.00)	(1.24)	(11.57)	(4.95)		(2.08)	(6.59)	(24.39)	(0.22)	
Pregnant during year before the ref. point	0.03	0.04	0.04	0.03	0.05	0.03	0.04	0.03	0.02	0.04	0.01	0.04	0.04	0.03	0.05	0.02	0.02	0.02	0.04	0.03
	(9.67)	(6.23)	(4.36)	(7.33)		(8.58)	(2.95)	(8.29)	(14.19)		(22.72)	(2.01)	(5.37)	(8.97)		(8.16)	(5.36)	(7.22)	(5.03)	
Qualification needs (last evaluation before ref. point)																				
0 - none	*0.90*	*0.85*	*0.91*	*0.89*	*0.92*	*0.90*	*0.87*	*0.87*	*0.87*	*0.89*	*0.92*	*0.86*	*0.90*	*0.89*	*0.90*	*0.91*	*0.90*	*0.91*	*0.94*	*0.94*
	(6.99)	*(21.39)*	*(1.54)*	*(9.47)*		*(1.23)*	*(8.26)*	*(6.29)*	*(7.54)*		*(4.77)*	*(13.83)*	*(0.05)*	*(5.48)*		*(12.49)*	*(16.17)*	*(11.62)*	*(1.77)*	
1 - basic qualification	*0.05*	*0.09*	*0.05*	*0.06*	*0.04*	*0.05*	*0.09*	*0.07*	*0.08*	*0.06*	*0.05*	*0.06*	*0.05*	*0.06*	*0.05*	*0.05*	*0.06*	*0.05*	*0.03*	*0.03*
	(6.12)	*(18.98)*	*(4.42)*	*(7.58)*		*(3.30)*	*(9.40)*	*(3.94)*	*(5.70)*		*(1.26)*	*(6.77)*	*(0.12)*	*(4.27)*		*(8.73)*	*(13.33)*	*(7.52)*	*(0.82)*	
2 - professional qualification	*0.05*	*0.06*	*0.04*	*0.05*	*0.04*	*0.05*	*0.05*	*0.05*	*0.05*	*0.04*	*0.04*	*0.08*	*0.05*	*0.06*	*0.05*	*0.04*	*0.04*	*0.04*	*0.03*	*0.03*
	(3.48)	*(9.85)*	*(2.56)*	*(5.44)*		*(1.87)*	*(1.06)*	*(4.75)*	*(4.58)*		*(5.44)*	*(12.02)*	*(0.05)*	*(3.26)*		*(8.57)*	*(8.81)*	*(8.60)*	*(1.67)*	
Sanction days concerning jobsearch 1y before ref. point	1.51	1.34	1.65	1.59	1.81	1.23	1.27	1.47	1.31	1.43	1.44	1.77	1.99	1.73	2.27	1.20	1.06	1.49	1.04	1.45
	(6.42)	(10.55)	(3.44)	(4.56)		(4.65)	(3.76)	(1.09)	(2.79)		(15.65)	(8.01)	(4.59)	(9.21)		(5.40)	(8.41)	(0.81)	(9.19)	
Sanction days concerning misbehavior 1y before ref. point	0.17	0.11	0.18	0.20	0.21	0.11	0.14	0.22	0.17	0.19	0.94	0.27	0.37	0.40	0.53	0.16	0.12	0.30	0.16	0.27
	(2.58)	(6.75)	(2.07)	(1.03)		(6.15)	(3.40)	(1.76)	(1.69)		(8.19)	(10.58)	(6.24)	(4.86)		(5.23)	(8.13)	(1.29)	(5.34)	
Sanction days concerning programs 1y before ref. point	0.02	0.02	0.05	0.03	0.04	0.03	0.02	0.05	0.02	0.02	0.36	0.00	0.07	0.08	0.07	0.03	0.02	0.05	0.02	0.03
	(2.66)	(2.42)	(1.59)	(0.62)		(2.06)	(0.91)	(4.21)	(0.35)		(13.82)	(8.53)	(0.32)	(0.46)		(0.25)	(2.33)	(2.42)	(1.98)	
Sanction days related to jobsearch in previous ue spells	1.45	1.03	1.73	1.61	1.31	0.87	0.94	1.10	1.30	1.22	2.06	1.27	1.99	2.41	2.65	2.07	1.34	2.68	1.87	2.35
	(2.06)	(4.69)	(5.74)	(4.30)		(6.08)	(4.68)	(2.08)	(1.24)		(6.27)	(15.24)	(6.67)	(2.35)		(3.17)	(13.18)	(3.36)	(5.62)	
Sanction days related to misbehavior in prev. ue spells	0.65	0.44	0.72	0.67	0.55	0.21	0.28	0.51	0.49	0.45	1.10	0.37	0.79	1.12	0.98	0.79	0.50	1.08	0.70	0.96
	(2.28)	(2.25)	(3.97)	(2.82)		(8.47)	(5.73)	(1.62)	(1.22)		(2.11)	(14.18)	(3.69)	(2.32)		(3.00)	(9.82)	(2.24)	(4.91)	
Sanction days related to programs in previous ue spells	0.15	0.10	0.16	0.15	0.11	0.08	0.07	0.05	0.06	0.10	0.36	0.28	0.20	0.27	0.28	0.19	0.07	0.23	0.14	0.16
	(1.96)	(1.03)	(2.67)	(2.18)		(1.21)	(1.96)	(4.25)	(2.66)		(3.04)	(0.08)	(3.27)	(0.35)		(1.60)	(5.83)	(3.31)	(1.30)	
Avg sickness days in months ue 1y before ref. point	0.21	0.20	0.19	0.15	0.17	0.15	0.14	0.20	0.16	0.16	0.44	0.24	0.22	0.20	0.24	0.12	0.12	0.14	0.11	0.13
	(4.19)	(3.39)	(2.16)	(2.41)		(0.66)	(1.57)	(4.47)	(0.35)		(13.62)	(0.57)	(1.73)	(4.05)		(1.06)	(0.68)	(1.36)	(1.96)	
Size of labor force in region of residence (in 1,000)	94.09	102.25	84.07	96.45	99.28	90.92	84.51	74.77	83.17	81.32	68.60	67.31	72.44	72.06	69.29	98.52	85.43	73.60	85.40	85.12
	(9.32)	(5.23)	(27.99)	(4.93)		(17.48)	(6.02)	(12.64)	(3.44)		(1.52)	(4.19)	(6.63)	(5.68)		(23.85)	(0.58)	(22.18)	(0.52)	
Months with social assistance 1y before ref. point	0.32	0.27	0.35	0.25	0.22	0.37	0.33	0.44	0.33	0.26	0.88	0.55	0.50	0.45	0.58	0.33	0.33	0.53	0.33	0.28
	(6.15)	(3.32)	(8.05)	(2.20)		(6.71)	(3.99)	(10.45)	(4.62)		(11.58)	(1.49)	(3.67)	(6.64)		(3.07)	(3.30)	(13.46)	(3.14)	
Assigned to special consultation 1y before ref. point	0.05	0.04	0.03	0.04	0.04	0.04	0.03	0.03	0.03	0.03	0.03	0.04	0.03	0.02	0.04	0.03	0.02	0.02	0.02	0.02
	(4.89)	(0.05)	(6.48)	(1.03)		(4.59)	(2.10)	(0.09)	(1.60)		(5.59)	(2.31)	(2.58)	(7.44)		(4.25)	(2.81)	(2.78)	(0.07)	
Assigned to special consultation in previous ue spells	0.02	0.03	0.02	0.02	0.03	0.01	0.02	0.02	0.02	0.02	0.01	0.02	0.02	0.02	0.02	0.03	0.02	0.02	0.02	0.02
	(2.39)	(0.57)	(2.24)	(4.04)		(3.83)	(2.93)	(1.59)	(0.61)		(10.28)	(1.47)	(0.59)	(0.37)		(4.18)	(3.15)	(0.48)	(1.26)	
Months with supplementary benefits 1y before ref. point	0.03	0.04	0.02	0.02	0.01	0.02	0.03	0.04	0.03	0.01	0.00	0.06	0.03	0.03	0.03	0.03	0.03	0.03	0.02	0.01
	(4.52)	(4.88)	(3.00)	(3.16)		(1.76)	(3.22)	(5.58)	(3.37)		(7.26)	(4.47)	(0.28)	(0.37)		(3.38)	(2.92)	(3.96)	(1.69)	
Months with survivors ins. benefits 1y before ref. point	0.06	0.08	0.08	0.09	0.05	0.13	0.04	0.05	0.06	0.06	0.11	0.09	0.06	0.05	0.07	0.06	0.03	0.03	0.06	0.04
	(2.14)	(4.33)	(4.32)	(4.56)		(6.19)	(3.10)	(1.51)	(1.15)		(4.24)	(2.28)	(0.79)	(2.05)		(2.93)	(1.51)	(0.69)	(3.06)	
Registered at job center while still working	0.24	0.27	0.30	0.26	0.21	0.21	0.23	0.24	0.25	0.20	0.27	0.37	0.27	0.32	0.30	0.28	0.28	0.33	0.36	0.28
	(6.82)	(13.90)	(19.64)	(10.34)		(1.23)	(5.67)	(9.67)	(11.48)		(6.35)	(15.09)	(5.26)	(4.37)		(1.02)	(0.56)	(10.81)	(17.60)	
Months in unemployment in 5y before reference point	6.70	5.17	6.22	6.05	5.92	6.00	5.58	6.83	6.46	6.68	9.91	6.94	8.82	9.00	8.98	6.00	5.37	8.09	7.12	6.82
	(10.51)	(10.90)	(4.11)	(1.80)		(9.48)	(15.32)	(2.00)	(2.88)		(10.63)	(26.57)	(1.81)	(0.27)		(9.76)	(17.54)	(14.17)	(3.29)	
Months in unemployment in 7y before reference point	8.79	6.84	8.47	8.03	7.68	7.86	6.99	8.68	7.96	8.30	12.39	9.47	11.18	11.32	11.24	8.19	6.94	10.41	9.34	8.77
	(11.50)	(9.24)	(8.45)	(3.74)		(4.78)	(14.18)	(3.92)	(3.54)		(10.54)	(17.03)	(0.53)	(0.72)		(5.39)	(17.49)	(14.53)	(5.06)	

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Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**
Number of observations in sequence:	**4,743**	**3,781**	**3,845**	**4,603**	**26,358**	**1,075**	**9,906**	**1,735**	**2,392**	**9,992**	**108**	**402**	**11,490**	**1,774**	**6,225**	**2,157**	**2,042**	**2,062**	**41,014**	**35,231**
Month of start of unemployment																				
01 - January	*0.12*	*0.12*	*0.12*	*0.14*	*0.13*	*0.13*	*0.13*	*0.13*	*0.15*	*0.13*	*0.11*	*0.13*	*0.13*	*0.16*	*0.12*	*0.11*	*0.11*	*0.11*	*0.12*	*0.13*
	(1.63)	*(4.25)*	*(3.07)*	*(4.08)*		*(1.55)*	*(0.06)*	*(2.02)*	*(7.88)*		*(3.27)*	*(3.07)*	*(2.20)*	*(9.69)*		*(8.55)*	*(6.64)*	*(8.23)*	*(3.16)*	
02 - February	*0.05*	*0.04*	*0.05*	*0.06*	*0.06*	*0.05*	*0.05*	*0.06*	*0.07*	*0.06*	*0.05*	*0.05*	*0.06*	*0.06*	*0.05*	*0.05*	*0.05*	*0.06*	*0.06*	*0.06*
	(2.22)	*(6.97)*	*(2.88)*	*(2.55)*		*(3.06)*	*(0.19)*	*(2.78)*	*(5.24)*		*(3.09)*	*(2.63)*	*(2.27)*	*(3.86)*		*(4.64)*	*(4.97)*	*(0.02)*	*(1.21)*	
03 - March	*0.06*	*0.05*	*0.06*	*0.06*	*0.05*	*0.06*	*0.05*	*0.05*	*0.05*	*0.06*	*0.02*	*0.06*	*0.05*	*0.05*	*0.04*	*0.05*	*0.04*	*0.05*	*0.06*	*0.05*
	(0.36)	*(3.01)*	*(2.42)*	*(1.09)*		*(0.79)*	*(3.28)*	*(0.73)*	*(1.59)*		*(14.74)*	*(8.03)*	*(3.56)*	*(4.85)*		*(0.54)*	*(6.36)*	*(1.37)*	*(2.24)*	
04 - April	*0.07*	*0.10*	*0.09*	*0.08*	*0.08*	*0.08*	*0.08*	*0.09*	*0.08*	*0.08*	*0.07*	*0.10*	*0.08*	*0.07*	*0.08*	*0.07*	*0.08*	*0.08*	*0.08*	*0.08*
	(3.28)	*(5.52)*	*(1.00)*	*(2.02)*		*(1.08)*	*(2.72)*	*(2.67)*	*(1.29)*		*(2.10)*	*(8.58)*	*(1.85)*	*(2.41)*		*(4.47)*	*(0.88)*	*(0.23)*	*(1.44)*	
05 - May	*0.08*	*0.09*	*0.08*	*0.07*	*0.08*	*0.08*	*0.07*	*0.08*	*0.07*	*0.08*	*0.08*	*0.07*	*0.07*	*0.08*	*0.08*	*0.09*	*0.08*	*0.08*	*0.08*	*0.08*
	(1.23)	*(4.45)*	*(0.06)*	*(1.34)*		*(0.73)*	*(0.29)*	*(0.96)*	*(2.10)*		*(0.68)*	*(4.46)*	*(3.36)*	*(1.98)*		*(5.49)*	*(0.86)*	*(0.34)*	*(1.83)*	
06 - June	*0.08*	*0.07*	*0.07*	*0.07*	*0.07*	*0.09*	*0.08*	*0.07*	*0.07*	*0.06*	*0.11*	*0.07*	*0.08*	*0.06*	*0.07*	*0.09*	*0.09*	*0.09*	*0.07*	*0.07*
	(2.84)	*(0.20)*	*(1.79)*	*(3.04)*		*(9.91)*	*(6.55)*	*(3.35)*	*(1.72)*		*(14.74)*	*(2.21)*	*(2.66)*	*(5.67)*		*(7.92)*	*(8.28)*	*(7.29)*	*(2.32)*	
07 - July	*0.09*	*0.09*	*0.09*	*0.07*	*0.08*	*0.10*	*0.09*	*0.08*	*0.07*	*0.08*	*0.12*	*0.08*	*0.09*	*0.08*	*0.08*	*0.08*	*0.09*	*0.07*	*0.08*	*0.07*
	(3.29)	*(3.33)*	*(2.71)*	*(5.08)*		*(4.94)*	*(1.09)*	*(0.29)*	*(6.95)*		*(14.38)*	*(1.72)*	*(3.43)*	*(0.29)*		*(3.83)*	*(7.37)*	*(1.08)*	*(1.86)*	
08 - August	*0.09*	*0.08*	*0.08*	*0.08*	*0.09*	*0.08*	*0.09*	*0.08*	*0.06*	*0.07*	*0.07*	*0.06*	*0.09*	*0.07*	*0.08*	*0.10*	*0.10*	*0.09*	*0.08*	*0.08*
	(0.94)	*(1.36)*	*(2.73)*	*(4.09)*		*(4.11)*	*(6.93)*	*(2.04)*	*(6.23)*		*(1.09)*	*(5.80)*	*(3.61)*	*(1.83)*		*(7.68)*	*(8.07)*	*(3.57)*	*(2.29)*	
09 - September	*0.08*	*0.09*	*0.09*	*0.08*	*0.09*	*0.09*	*0.09*	*0.09*	*0.08*	*0.08*	*0.08*	*0.08*	*0.08*	*0.08*	*0.09*	*0.10*	*0.09*	*0.09*	*0.08*	*0.07*
	(0.72)	*(2.19)*	*(3.10)*	*(0.75)*		*(1.60)*	*(3.53)*	*(3.27)*	*(2.39)*		*(0.94)*	*(2.30)*	*(0.61)*	*(0.70)*		*(9.03)*	*(7.41)*	*(5.58)*	*(3.62)*	
10 - October	*0.09*	*0.10*	*0.10*	*0.09*	*0.09*	*0.08*	*0.09*	*0.10*	*0.09*	*0.09*	*0.09*	*0.07*	*0.09*	*0.08*	*0.09*	*0.10*	*0.10*	*0.10*	*0.09*	*0.09*
	(1.31)	*(3.40)*	*(2.59)*	*(0.92)*		*(4.86)*	*(0.17)*	*(2.31)*	*(1.31)*		*(0.35)*	*(8.05)*	*(1.31)*	*(3.08)*		*(6.31)*	*(6.16)*	*(5.51)*	*(0.25)*	
11 - November	*0.08*	*0.08*	*0.09*	*0.10*	*0.09*	*0.08*	*0.09*	*0.08*	*0.11*	*0.10*	*0.11*	*0.10*	*0.09*	*0.10*	*0.10*	*0.10*	*0.10*	*0.10*	*0.09*	*0.10*
	(2.99)	*(3.19)*	*(0.01)*	*(2.86)*		*(6.78)*	*(2.63)*	*(6.53)*	*(4.25)*		*(4.06)*	*(0.54)*	*(1.48)*	*(1.87)*		*(1.84)*	*(0.18)*	*(1.62)*	*(1.03)*	
12 - December	*0.09*	*0.08*	*0.08*	*0.10*	*0.09*	*0.09*	*0.09*	*0.08*	*0.11*	*0.11*	*0.07*	*0.12*	*0.09*	*0.10*	*0.12*	*0.06*	*0.07*	*0.09*	*0.10*	*0.13*
	(0.79)	*(2.09)*	*(1.36)*	*(3.43)*		*(7.39)*	*(8.58)*	*(10.88)*	*(0.22)*		*(15.73)*	*(0.38)*	*(10.70)*	*(5.36)*		*(22.87)*	*(21.11)*	*(12.40)*	*(9.69)*	
Year of start of unemployment	**2,012.63**	**2,012.65**	**2,012.63**	**2,012.66**	**2,012.68**	**2,012.62**	**2,012.63**	**2,012.59**	**2,012.58**	**2,012.57**	**2,012.44**	**2,012.56**	**2,012.59**	**2,012.59**	**2,012.57**	**2,012.56**	**2,012.56**	**2,012.56**	**2,012.56**	**2,012.49**
	(4.25)	**(2.16)**	**(4.44)**	**(1.30)**		**(4.44)**	**(5.02)**	**(1.08)**	**(0.90)**		**(10.89)**	**(0.23)**	**(2.02)**	**(1.86)**		**(5.43)**	**(5.94)**	**(5.29)**	**(5.79)**	
Months unemployed in year before reference point	**2.85**	**2.16**	**2.31**	**2.43**	**2.72**	**2.80**	**2.52**	**2.95**	**2.77**	**3.15**	**3.88**	**3.36**	**3.85**	**3.55**	**4.01**	**1.59**	**1.52**	**2.17**	**1.84**	**2.08**
	(5.53)	**(26.77)**	**(18.93)**	**(12.92)**		**(15.74)**	**(27.59)**	**(8.88)**	**(16.67)**		**(5.02)**	**(26.39)**	**(6.14)**	**(17.33)**		**(22.07)**	**(25.38)**	**(3.72)**	**(9.82)**	
Months unemployed in 2nd year before reference point	**0.47**	**0.37**	**0.45**	**0.50**	**0.44**	**0.37**	**0.47**	**0.56**	**0.72**	**0.64**	**0.83**	**0.48**	**0.72**	**1.00**	**0.91**	**0.77**	**0.83**	**1.11**	**1.11**	**1.14**
	(2.06)	**(4.24)**	**(0.55)**	**(3.55)**		**(15.61)**	**(9.60)**	**(4.12)**	**(4.10)**		**(3.39)**	**(21.47)**	**(8.40)**	**(4.02)**		**(14.71)**	**(12.18)**	**(0.91)**	**(1.23)**	
Months unemployed in 3rd year before reference point	**1.08**	**0.82**	**1.01**	**0.97**	**0.82**	**0.89**	**0.87**	**1.08**	**1.14**	**1.01**	**1.71**	**0.89**	**1.40**	**1.57**	**1.36**	**1.26**	**1.11**	**1.72**	**1.50**	**1.33**
	(9.69)	**(0.34)**	**(7.43)**	**(5.90)**		**(4.63)**	**(5.44)**	**(2.74)**	**(4.74)**		**(11.17)**	**(17.33)**	**(1.32)**	**(6.49)**		**(2.09)**	**(7.47)**	**(12.13)**	**(5.48)**	
Unemployment rate in region of residence	**3.69**	**3.31**	**3.19**	**3.19**	**3.20**	**3.75**	**3.81**	**3.64**	**3.54**	**3.68**	**3.64**	**3.12**	**3.36**	**3.03**	**3.04**	**3.17**	**3.55**	**3.08**	**3.33**	**3.24**
	(38.93)	**(9.65)**	**(1.03)**	**(0.78)**		**(4.69)**	**(9.45)**	**(3.30)**	**(10.75)**		**(43.21)**	**(6.29)**	**(24.27)**	**(0.50)**		**(5.26)**	**(23.58)**	**(12.55)**	**(7.21)**	
Urban-rural classification of municipality of residence																				
Intermediate	*0.13*	*0.19*	*0.16*	*0.18*	*0.18*	*0.16*	*0.15*	*0.16*	*0.15*	*0.17*	*0.19*	*0.21*	*0.16*	*0.17*	*0.19*	*0.19*	*0.14*	*0.18*	*0.17*	*0.18*
	(12.77)	*(4.02)*	*(4.84)*	*(1.59)*		*(2.73)*	*(7.09)*	*(4.46)*	*(6.95)*		*(1.03)*	*(5.26)*	*(8.53)*	*(5.39)*		*(1.26)*	*(11.04)*	*(1.30)*	*(2.66)*	
Rural	*0.07*	*0.09*	*0.12*	*0.10*	*0.10*	*0.07*	*0.09*	*0.11*	*0.10*	*0.10*	*0.11*	*0.12*	*0.13*	*0.14*	*0.15*	*0.10*	*0.09*	*0.13*	*0.11*	*0.12*
	(11.88)	*(1.82)*	*(4.96)*	*(0.85)*		*(11.85)*	*(4.67)*	*(3.02)*	*(1.55)*		*(12.44)*	*(9.87)*	*(6.92)*	*(3.97)*		*(6.56)*	*(10.70)*	*(3.83)*	*(3.27)*	
Urban	*0.80*	*0.71*	*0.73*	*0.72*	*0.72*	*0.77*	*0.77*	*0.73*	*0.75*	*0.73*	*0.69*	*0.67*	*0.71*	*0.69*	*0.66*	*0.71*	*0.77*	*0.69*	*0.72*	*0.70*
	(18.59)	*(2.26)*	*(0.60)*	*(0.79)*		*(9.87)*	*(9.10)*	*(1.67)*	*(4.74)*		*(8.12)*	*(2.70)*	*(12.16)*	*(7.41)*		*(3.43)*	*(16.61)*	*(1.67)*	*(4.53)*	
Vocational degree	**0.47**	**0.50**	**0.52**	**0.54**	**0.58**	**0.47**	**0.37**	**0.43**	**0.42**	**0.46**	**0.50**	**0.51**	**0.46**	**0.47**	**0.52**	**0.54**	**0.38**	**0.48**	**0.49**	**0.55**
	(22.18)	**(15.47)**	**(10.56)**	**(6.51)**		**(2.06)**	**(17.15)**	**(4.73)**	**(7.35)**		**(3.67)**	**(1.19)**	**(10.92)**	**(9.78)**		**(2.33)**	**(33.88)**	**(13.23)**	**(12.25)**	

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Table C.1: Pre-treatment covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
Avg waiting days in months ue 1y before ref. point	2.06	2.36	1.99	2.04	2.57	2.19	2.15	1.73	1.76	2.18	1.25	1.47	1.43	1.38	1.48	1.54	1.22	1.20	1.21	1.60
	(15.78)	(6.34)	(18.22)	(16.72)		(0.11)	(1.13)	(16.13)	(15.03)		(10.51)	(0.30)	(2.24)	(4.25)		(2.23)	(14.56)	(16.27)	(15.21)	
Willing to move for new job	0.02	0.01	0.02	0.02	0.02	0.01	0.02	0.04	0.02	0.03	0.05	0.04	0.04	0.05	0.06	0.02	0.02	0.03	0.03	0.03
	(3.55)	(6.38)	(1.72)	(3.82)		(8.42)	(1.97)	(4.75)	(3.08)		(4.28)	(7.48)	(6.92)	(3.53)		(6.64)	(4.18)	(1.98)	(1.21)	

Table C.2: Intermediate covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
Canceled appointment in period 1	0.01	0.01	0.01	0.02	0.05	0.01	0.01	0.01	0.01	0.05	0.04	0.01	0.01	0.01	0.07	0.02	0.02	0.02	0.02	0.07
	(22.96)	(24.13)	(22.75)	(20.09)		(24.14)	(22.56)	(21.52)	(21.31)		(14.98)	(27.86)	(29.07)	(28.04)		(21.25)	(24.49)	(19.75)	(24.86)	
No-show appointment in period 1	0.04	0.03	0.04	0.04	0.05	0.03	0.03	0.04	0.04	0.04	0.06	0.04	0.04	0.06	0.07	0.07	0.06	0.07	0.06	0.08
	(6.30)	(7.76)	(2.97)	(3.84)		(5.24)	(5.05)	(0.52)	(0.23)		(1.51)	(14.00)	(10.88)	(2.20)		(5.39)	(10.96)	(3.95)	(9.45)	
Avg number of applications per month in period 1	5.44	5.09	6.44	5.95	4.96	5.96	4.83	5.74	5.64	4.52	6.22	6.84	6.68	7.44	5.55	5.29	5.04	6.34	5.14	3.71
	(9.81)	(2.50)	(30.93)	(19.88)		(29.39)	(6.42)	(24.92)	(23.07)		(15.39)	(30.74)	(25.47)	(45.19)		(35.71)	(30.10)	(63.57)	(33.99)	
Postponed appointment in period 1	0.23	0.23	0.21	0.25	0.27	0.24	0.23	0.24	0.27	0.26	0.29	0.20	0.25	0.27	0.27	0.33	0.30	0.32	0.34	0.36
	(7.11)	(8.70)	(12.13)	(3.01)		(5.01)	(6.95)	(4.55)	(2.21)		(2.81)	(17.17)	(6.48)	(1.62)		(4.59)	(12.79)	(6.48)	(3.19)	
Phone appointment in period 1	0.04	0.03	0.05	0.05	0.05	0.04	0.04	0.04	0.06	0.06	0.08	0.06	0.06	0.07	0.08	0.05	0.05	0.07	0.06	0.08
	(5.60)	(11.43)	(0.01)	(2.70)		(8.15)	(10.84)	(9.65)	(1.63)		(1.45)	(7.73)	(8.36)	(2.72)		(10.22)	(9.35)	(2.22)	(6.73)	
Avg net unemployment benefit in months ue in period 1	3,472.26	3,339.18	3,107.03	3,103.77	3,494.72	3,417.03	3,297.65	3,073.18	3,020.47	3,333.17	3,119.48	3,000.30	3,052.30	2,810.16	2,808.32	1,950.95	1,876.23	1,873.88	1,542.94	1,529.78
	(1.39)	(9.86)	(25.81)	(26.30)		(5.60)	(2.31)	(18.49)	(22.11)		(22.82)	(14.92)	(18.62)	(0.15)		(36.31)	(30.42)	(30.69)	(1.17)	
Change of job center in period 1	0.03	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.02	0.01	0.01	0.01	0.02	0.03	0.03	0.02	0.02	0.02
	(5.07)	(2.61)	(1.38)	(0.40)		(2.18)	(1.71)	(5.59)	(6.20)		(1.62)	(1.18)	(2.84)	(3.86)		(6.57)	(4.43)	(0.49)	(1.97)	
Change in place of residence in period 1	0.02	0.02	0.01	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.04	0.01	0.02	0.02	0.02	0.02	0.03	0.03	0.02	0.02
	(0.92)	(0.17)	(2.06)	(1.74)		(3.25)	(4.68)	(6.09)	(7.56)		(9.62)	(4.49)	(3.28)	(1.14)		(3.18)	(5.26)	(4.80)	(1.47)	
Exempted from job search efforts in period 1	0.03	0.01	0.01	0.02	0.19	0.02	0.05	0.06	0.04	0.24	0.10	0.04	0.03	0.03	0.32	0.04	0.03	0.03	0.03	0.34
	(52.52)	(62.71)	(61.04)	(56.68)		(68.76)	(58.85)	(54.34)	(60.17)		(54.77)	(76.64)	(82.29)	(80.00)		(81.67)	(87.25)	(87.17)	(87.76)	
Whether incident concerning jobsearch in period 1	0.08	0.05	0.10	0.08	0.07	0.05	0.05	0.08	0.06	0.05	0.11	0.06	0.08	0.12	0.10	0.13	0.10	0.17	0.12	0.13
	(2.88)	(7.34)	(10.88)	(4.71)		(1.51)	(2.06)	(8.94)	(4.33)		(2.47)	(15.01)	(8.11)	(4.58)		(0.53)	(10.35)	(11.74)	(3.07)	
Whether incident concerning misbehavior in period 1	0.03	0.03	0.04	0.04	0.04	0.02	0.03	0.04	0.04	0.03	0.09	0.04	0.04	0.06	0.06	0.06	0.04	0.08	0.05	0.07
	(2.61)	(4.44)	(2.36)	(0.75)		(7.48)	(3.96)	(4.43)	(1.04)		(12.57)	(10.25)	(8.91)	(0.20)		(0.84)	(9.22)	(5.03)	(4.86)	
Whether incident concerning programs in period 1	0.01	0.01	0.02	0.01	0.01	0.01	0.01	0.02	0.02	0.01	0.06	0.01	0.02	0.02	0.04	0.02	0.01	0.01	0.01	0.01
	(0.11)	(4.50)	(7.27)	(1.38)		(3.71)	(4.37)	(5.17)	(3.54)		(11.60)	(16.85)	(14.36)	(8.46)		(8.83)	(3.27)	(3.62)	(1.95)	
Insufficient job search efforts in period 1	0.06	0.05	0.08	0.07	0.07	0.05	0.05	0.06	0.06	0.05	0.10	0.05	0.07	0.09	0.09	0.12	0.10	0.14	0.11	0.14
	(0.80)	(9.02)	(5.03)	(0.36)		(3.40)	(3.83)	(0.92)	(2.43)		(2.75)	(18.19)	(9.43)	(1.99)		(4.48)	(13.67)	(1.47)	(7.35)	
Child allowance in period 1	0.16	0.15	0.15	0.14	0.13	0.17	0.15	0.17	0.15	0.14	0.20	0.17	0.15	0.13	0.14	0.10	0.09	0.09	0.02	0.05
	(7.34)	(6.86)	(6.08)	(1.78)		(7.77)	(4.04)	(8.04)	(2.23)		(16.24)	(8.72)	(1.23)	(4.37)		(18.78)	(16.20)	(15.40)	(13.37)	
Mandatory job applications in period 1	0.49	0.54	0.56	0.68	0.50	0.52	0.48	0.61	0.61	0.46	0.41	0.46	0.49	0.66	0.39	0.52	0.46	0.59	0.39	0.35
	(0.84)	(3.82)	(5.60)	(15.22)		(5.11)	(1.50)	(13.71)	(12.87)		(1.44)	(7.17)	(9.46)	(21.68)		(17.55)	(11.56)	(22.38)	(5.00)	
Avg monthly state subsidies (with WS-earnings) period 1	3,812.17	3,640.24	3,392.15	3,369.39	3,738.69	3,740.84	3,678.37	3,331.31	3,406.44	3,643.71	3,356.25	3,226.49	3,337.22	3,267.06	3,009.79	3,850.14	3,667.40	3,506.81	4,076.16	3,368.77
	(3.86)	(5.40)	(19.86)	(21.49)		(5.36)	(1.86)	(18.71)	(14.05)		(21.99)	(14.41)	(21.49)	(17.90)		(26.10)	(16.76)	(7.85)	(36.87)	

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Table C.2: Intermediate covariate means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference ⩾ 20 marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP	JA	TC	EP	WS	NP
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
Avg monthly earnings (excl. WS-earnings) in period 1	7.83	4.83	9.46	30.32	371.99	7.10	2.67	9.42	16.87	436.93	0.00	15.19	3.66	51.79	506.46	72.61	54.82	38.04	78.75	976.98
	(50.25)	(50.91)	(48.43)	(46.07)		(43.24)	(43.80)	(42.99)	(42.11)		(69.55)	(66.75)	(68.88)	(59.79)		(89.16)	(92.11)	(94.91)	(88.32)	
Number of appointments in period 1	2.17	2.13	2.39	2.26	2.02	2.19	2.19	2.26	2.22	2.00	2.79	2.30	2.18	2.25	1.96	2.36	2.49	2.47	2.24	1.97
	(16.25)	(12.25)	(37.44)	(25.59)		(19.20)	(18.94)	(26.31)	(21.93)		(53.04)	(30.09)	(19.26)	(24.27)		(35.55)	(46.86)	(42.79)	(25.05)	
Change of caseworker in period 1	0.11	0.11	0.11	0.12	0.11	0.10	0.10	0.11	0.11	0.09	0.13	0.08	0.10	0.12	0.11	0.27	0.26	0.27	0.29	0.24
	(0.54)	(0.07)	(2.41)	(4.38)		(2.70)	(2.26)	(5.35)	(5.75)		(7.05)	(8.46)	(1.84)	(2.78)		(6.13)	(4.58)	(5.32)	(10.15)	
Change of interviewer in period 1	0.16	0.17	0.18	0.17	0.15	0.16	0.15	0.16	0.16	0.13	0.23	0.16	0.15	0.16	0.16	0.36	0.36	0.33	0.36	0.32
	(2.55)	(5.26)	(6.76)	(4.11)		(6.71)	(5.56)	(8.42)	(6.96)		(19.18)	(0.93)	(1.12)	(2.09)		(9.70)	(8.05)	(3.34)	(8.68)	
Out of unemployment at least once during period 1	0.00	0.00	0.00	0.02	0.18	0.00	0.00	0.01	0.01	0.20	0.01	0.02	0.00	0.04	0.29	0.05	0.04	0.03	0.05	0.46
	(62.66)	(62.93)	(62.37)	(53.50)		(67.58)	(68.37)	(65.19)	(62.15)		(86.22)	(81.08)	(88.95)	(74.07)		(107.49)	(110.73)	(114.39)	(106.78)	
Placeability (most recent evaluation before period 2)																				
0 - not available	*0.44*	*0.30*	*0.38*	*0.30*	*0.27*	*0.38*	*0.35*	*0.37*	*0.32*	*0.34*	*0.47*	*0.42*	*0.41*	*0.42*	*0.47*	*0.33*	*0.35*	*0.43*	*0.39*	*0.40*
	(35.32)	*(5.94)*	*(22.01)*	*(5.30)*		*(6.81)*	*(0.82)*	*(5.90)*	*(6.05)*		*(0.05)*	*(10.98)*	*(13.51)*	*(10.12)*		*(12.89)*	*(9.85)*	*(6.41)*	*(1.98)*	
1 - difficult	*0.13*	*0.15*	*0.13*	*0.11*	*0.09*	*0.12*	*0.16*	*0.16*	*0.13*	*0.11*	*0.19*	*0.16*	*0.16*	*0.16*	*0.14*	*0.11*	*0.13*	*0.13*	*0.09*	*0.07*
	(13.75)	*(17.42)*	*(13.22)*	*(7.17)*		*(2.44)*	*(14.53)*	*(14.97)*	*(5.96)*		*(15.45)*	*(6.92)*	*(6.95)*	*(6.49)*		*(12.80)*	*(18.57)*	*(19.33)*	*(7.05)*	
2 - medium	*0.31*	*0.45*	*0.40*	*0.43*	*0.44*	*0.33*	*0.37*	*0.37*	*0.40*	*0.39*	*0.26*	*0.37*	*0.34*	*0.33*	*0.32*	*0.40*	*0.39*	*0.36*	*0.38*	*0.36*
	(27.60)	*(1.88)*	*(8.06)*	*(1.76)*		*(12.64)*	*(3.86)*	*(4.64)*	*(1.73)*		*(13.67)*	*(9.35)*	*(4.22)*	*(2.89)*		*(8.34)*	*(6.91)*	*(0.12)*	*(4.08)*	
3 - easy	*0.11*	*0.10*	*0.09*	*0.16*	*0.19*	*0.17*	*0.12*	*0.09*	*0.15*	*0.15*	*0.07*	*0.05*	*0.09*	*0.08*	*0.07*	*0.16*	*0.13*	*0.08*	*0.14*	*0.17*
	(21.71)	*(26.46)*	*(30.44)*	*(9.75)*		*(5.51)*	*(10.32)*	*(17.59)*	*(0.13)*		*(1.87)*	*(6.02)*	*(8.19)*	*(5.14)*		*(4.24)*	*(11.80)*	*(27.62)*	*(8.16)*	
Pregnant in period 1	0.01	0.01	0.01	0.01	0.02	0.01	0.02	0.01	0.01	0.02	0.00	0.00	0.01	0.01	0.02	0.01	0.01	0.01	0.01	0.02
	(8.21)	(7.23)	(8.58)	(13.18)		(12.25)	(5.31)	(11.03)	(14.41)		(20.08)	(16.52)	(5.35)	(12.64)		(6.68)	(5.20)	(6.28)	(2.07)	
Qualification needs (last evaluation before period 2)																				
0 - none	*0.87*	*0.83*	*0.90*	*0.88*	*0.91*	*0.88*	*0.85*	*0.86*	*0.86*	*0.89*	*0.89*	*0.85*	*0.90*	*0.88*	*0.89*	*0.88*	*0.85*	*0.89*	*0.92*	*0.93*
	(11.75)	*(25.51)*	*(2.59)*	*(10.60)*		*(0.62)*	*(11.57)*	*(7.57)*	*(8.69)*		*(0.85)*	*(11.50)*	*(1.55)*	*(4.52)*		*(18.51)*	*(26.12)*	*(13.68)*	*(5.25)*	
1 - basic qualification	*0.07*	*0.10*	*0.05*	*0.06*	*0.04*	*0.06*	*0.10*	*0.08*	*0.09*	*0.06*	*0.05*	*0.07*	*0.05*	*0.07*	*0.06*	*0.07*	*0.09*	*0.05*	*0.04*	*0.04*
	(10.15)	*(21.72)*	*(4.13)*	*(8.47)*		*(1.63)*	*(12.98)*	*(5.82)*	*(8.62)*		*(4.01)*	*(6.02)*	*(1.55)*	*(4.78)*		*(13.57)*	*(21.02)*	*(6.48)*	*(3.96)*	
2 - professional qualification	*0.06*	*0.07*	*0.04*	*0.06*	*0.04*	*0.06*	*0.05*	*0.06*	*0.06*	*0.05*	*0.06*	*0.08*	*0.05*	*0.06*	*0.05*	*0.06*	*0.06*	*0.06*	*0.04*	*0.03*
	(5.87)	*(12.54)*	*(0.70)*	*(6.05)*		*(2.62)*	*(1.78)*	*(4.46)*	*(2.84)*		*(4.86)*	*(9.63)*	*(0.58)*	*(1.33)*		*(11.83)*	*(14.36)*	*(12.32)*	*(3.30)*	
Sanction days in period 1	0.90	0.56	1.21	0.98	0.99	0.56	0.50	0.69	0.74	0.69	2.02	0.49	0.84	1.04	1.69	1.70	1.14	1.91	1.36	1.67
	(1.81)	(10.12)	(4.27)	(0.24)		(3.39)	(5.21)	(0.05)	(1.23)		(5.05)	(22.62)	(15.25)	(10.97)		(0.51)	(10.47)	(3.84)	(5.88)	
Avg sickness days in months ue in period 1	0.36	0.22	0.28	0.26	0.29	0.36	0.33	0.41	0.35	0.41	1.50	0.85	0.85	0.85	1.26	0.20	0.17	0.23	0.12	0.15
	(6.81)	(8.10)	(0.72)	(2.78)		(4.40)	(7.31)	(0.16)	(5.69)		(9.69)	(19.90)	(20.35)	(19.95)		(6.75)	(2.29)	(9.86)	(4.03)	
Social assistance in period 1	0.04	0.04	0.05	0.04	0.03	0.05	0.04	0.05	0.04	0.03	0.08	0.06	0.06	0.06	0.07	0.05	0.05	0.08	0.05	0.04
	(3.36)	(2.92)	(8.00)	(3.76)		(8.81)	(4.94)	(10.22)	(3.11)		(6.51)	(0.61)	(3.26)	(4.58)		(5.70)	(6.56)	(15.32)	(2.60)	
Assigned to special consultation in period 1	0.03	0.03	0.02	0.03	0.03	0.02	0.01	0.01	0.01	0.01	0.04	0.03	0.01	0.02	0.02	0.03	0.02	0.02	0.01	0.01
	(1.35)	(2.75)	(5.18)	(0.32)		(8.05)	(1.39)	(3.21)	(0.47)		(12.91)	(10.47)	(3.41)	(1.82)		(12.29)	(5.02)	(6.67)	(3.25)	
Avg waiting days in months ue in period 1	0.26	0.27	0.27	0.27	0.34	0.20	0.23	0.17	0.19	0.22	0.25	0.06	0.12	0.16	0.16	0.68	0.51	0.48	0.63	0.86
	(6.43)	(6.26)	(6.50)	(5.77)		(1.31)	(1.18)	(5.07)	(2.73)		(9.21)	(15.36)	(5.25)	(0.17)		(11.70)	(24.13)	(24.88)	(15.48)	

Table C.3: Outcome means and standardized differences (vs. first program - NP) in trimmed sample. Standardized difference $\geqslant 20$ marked in violet.

	First program: JA					First program: TC					First program: EP					First program: WS				
Second program:	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**	**JA**	**TC**	**EP**	**WS**	**NP**
Number of observations in sequence:	4,743	3,781	3,845	4,603	26,358	1,075	9,906	1,735	2,392	9,992	108	402	11,490	1,774	6,225	2,157	2,042	2,062	41,014	35,231
Cum. months employed in 30 months after 1st period	13.25	13.40	13.17	16.34	17.63	13.51	14.07	13.42	16.42	17.97	13.26	14.80	13.76	16.05	17.73	15.05	14.95	14.85	16.68	22.15
	(45.92)	(45.26)	(47.46)	(14.16)		(48.02)	(41.18)	(48.99)	(16.95)		(45.95)	(31.56)	(40.98)	(18.17)		(85.47)	(86.91)	(87.22)	(64.31)	

D Overlap

Table D.1: Pre-treatment covariate means and standardized differences in trimmed and untrimmed sample.

Variable	untrimmed	trimmed	Variable	untrimmed	trimmed
Avg add. income in months ue 1y before ref. point	68.19	62.24	Insured earnings before start of first program	5,001.77	4,938.37
		(1.66)			(3.04)
Age at start of unemployment	38.99	38.94	Ever insufficient jobsearch efforts 1y before ref. point	0.28	0.28
		(0.46)			(0.37)
Age at 1st income subj. to Swiss social ins. contrib.	23.75	23.77	Ever insufficient job search eff. in prev. ue spells	0.32	0.33
		(0.20)			(0.44)
Avg monthly applications in previous ue spells	2.97	2.94	Language level Italian	1.04	1.05
		(0.69)			(0.66)
Avg monthly applications in year before ref. point	4.10	4.09	Number of child allowances before reference point	0.17	0.17
		(0.08)			(0.37)
Age of youngest child in month before ref. point			Experience in last job		
0-3	0.05	0.05	0 - Experience unknown	0.08	0.08
		(0.54)			(0.35)
3-6	0.02	0.02	1 - No experience	0.03	0.03
		(0.31)			(0.40)
6-10	0.01	0.01	2 - Less than 1 year of experience	0.06	0.06
		(0.19)			(0.21)
No children	0.06	0.06	3 - 1-3 years of experience	0.21	0.21
		(0.04)			(0.59)
Unknown	0.86	0.86	4 - More than 3 years of experience	0.63	0.62
		(0.51)			(0.65)
Avg net ue benefits in months ue 1y before ref. point	1,784.91	1,779.78	Function in the last job		
		(0.31)	Auxiliary function	0.31	0.31
Cumulative state subsidies in year before ref. point	8,935.17	8,765.00			(0.15)
		(1.51)	Management function	0.05	0.05
Cumulative state subsidies in 2nd year before ref. point	3,119.68	2,956.97			(2.33)
		(1.70)	Other function	0.01	0.01
Cumulative state subsidies in 3rd year before ref. point	4,345.09	4,143.81			(0.10)
		(1.74)	Technical function	0.63	0.64
Avg monthly state subsidies 5y before reference point	423.71	411.86			(0.94)
		(2.07)	Type of last job		
Avg monthly state subsidies 7y before start of ref. point	393.85	384.36	Agricultural and forestry occupations	0.02	0.02
		(1.84)			(0.12)
Canton of residence			Construction and mining occupations	0.09	0.09
AG	0.08	0.08			(0.03)
		(0.92)	Health, teaching, scientists, cultural occup.	0.11	0.11
AR	0.00	0.00			(0.06)
		(0.23)	Management, admin, banking, insurance, legal	0.20	0.19
BE	0.09	0.07			(0.92)
		(5.77)	Not classifiable	0.04	0.05
BL	0.03	0.03			(0.21)
		(0.73)	Occupations providing personal services	0.20	0.20
BS	0.03	0.03			(0.14)
		(0.03)	Production occupations in industry and trade	0.12	0.13
FR	0.04	0.04			(0.72)
		(0.91)	Technical and information techn. occupations	0.06	0.06
GE	0.08	0.08			(0.08)
		(0.10)	Trade and transport occupations	0.15	0.15
GL	0.00	0.00			(0.11)
		(0.29)	Language level local language	5.49	5.48
GR	0.02	0.02			(0.58)
		(0.48)	Sector of last employer		
JU	0.01	0.01	Agriculture, forestry and fishing	0.01	0.01
		(0.35)			(0.10)
LU	0.03	0.04	Construction	0.09	0.09
		(0.89)			(0.13)
NE	0.03	0.03	Financial and insurance activities	0.05	0.04
		(0.84)			(1.17)
NW	0.00	0.00	Information and communication	0.03	0.03
		(0.25)			(0.58)
SG	0.06	0.06	Manufacturing, mining, quarrying, other industry	0.15	0.15
		(1.35)			(0.70)
SH	0.01	0.01	Other services	0.08	0.09
		(0.37)			(0.14)
SO	0.04	0.04	Prof., scientific, technical and admin services	0.16	0.16
		(0.62)			(0.22)
SZ	0.01	0.01	Publ. admin, defence, educ., health, social work	0.11	0.11
		(0.23)			(0.05)
TG	0.03	0.03	Real estate activities	0.01	0.01
		(0.56)			(0.10)

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Table D.1: Pre-treatment covariate means and standardized differences in trimmed and untrimmed sample.

Variable	untrimmed	trimmed	Variable	untrimmed	trimmed
TI	0.06	0.06	Wholesale, retail, transport, accomodation, food	0.31	0.31
		(0.82)			(0.13)
VD	0.12	0.12	Mandatory job applications during 1y before ref. point	0.51	0.52
		(0.95)			(0.92)
VS	0.05	0.05	Mandatory job applications in previous ue spells	0.96	0.96
		(0.62)			(0.18)
ZG	0.01	0.01	Maternity benefits during year before ref. point	0.03	0.04
		(0.20)			(0.47)
ZH	0.17	0.16	Max daily allowances for current unemployment spell	380.62	381.09
		(1.91)			(0.69)
Other	0.01	0.01	Months in basic course 1y before current ue spell	0.01	0.01
		(0.38)			(0.09)
Civil status			Months in basic course 5y before current ue spell	0.21	0.21
Divorced	0.13	0.13			(0.06)
		(0.39)	Months in employment program 1y before current ue spell	0.02	0.02
Married	0.51	0.51			(0.04)
		(0.27)	Months in employment program 5y before current ue spell	0.29	0.29
Single	0.35	0.35			(0.12)
		(0.59)	Avg duration of previous employment spells (in months)	45.18	44.91
Widowed	0.01	0.01			(0.74)
		(0.13)	Months from start of ue spell to start of 1st program	2.31	2.32
Change in regional unemployment rate vs. last year	-0.04	-0.04			(0.62)
		(0.69)	Avg duration of previous unemployment spells (in months)	4.59	4.58
Desired degree of employment before ref. point	94.03	93.98			(0.21)
		(0.31)	Months in training course 1y before current ue spell	0.02	0.02
Degree of employment in last job	90.71	90.60			(0.03)
		(0.65)	Months in training course 5y before current ue spell	0.24	0.25
Months with disability ins. benefits 1y before ref. point	0.01	0.01			(0.27)
		(0.19)	Mother tongue (9 categories)		
Earnings history missing for at least one year	0.28	0.28	Albanian	0.07	0.07
		(0.28)			(0.16)
Cumulative earnings in year before reference point	47,733.75	46,441.46	English	0.02	0.02
		(3.53)			(0.51)
Cumulative earnings in 2nd year before reference point	53,788.46	52,520.31	French	0.19	0.19
		(3.20)			(1.01)
Cumulative earnings in 3rd year before reference point	47,429.86	46,288.49	German	0.37	0.36
		(2.79)			(1.78)
Avg monthly earnings in 5y before reference point	4,104.61	3,999.74	Italian	0.07	0.07
		(3.62)			(0.64)
Avg monthly earnings in 7y before reference point	3,984.91	3,880.43	Other	0.14	0.14
		(3.67)			(0.07)
Level of education			Portuguese	0.09	0.10
0 - Unknown	0.06	0.06			(0.83)
		(0.68)	Spanish	0.03	0.03
1 - Primary	0.01	0.01			(0.10)
		(0.23)	Turkish	0.02	0.02
2 - Secondary I	0.24	0.25			(0.24)
		(0.69)	Months with wage subsidy 1y before current ue spell	0.16	0.15
3 - Secondary II	0.49	0.49			(1.88)
		(0.75)	Months with wage subsidy 5y before current ue spell	1.52	1.44
4 - Tertiary	0.20	0.19			(1.94)
		(2.16)	Number of canceled appointments in year before ref. point	0.02	0.02
Months employed in year before reference point	9.40	9.41			(0.27)
		(0.21)	Canceled job center appointments before current ue spell	0.01	0.01
Months employed in 2nd year before reference point	10.49	10.52			(0.13)
		(1.10)	Job center appointments before current ue spell	5.89	5.86
Months employed in 3rd year before reference point	9.30	9.31			(0.39)
		(0.40)	No-show job center appointments before current ue spell	0.41	0.41
Months in employment in 5y before reference point	46.25	46.25			(0.16)
		(0.04)	Number of no-show appointments in year before ref. point	0.10	0.10
Months in employment in 7y before reference point	61.78	61.71			(0.17)
		(0.31)	Postponed jobcenter appointments before current ue spell	0.60	0.59
Language level English	2.12	2.07			(0.69)
		(2.10)	Number of postponed appointments in year bef. ref. point	0.34	0.34
Exempt from paying ue insurance contributions	0.01	0.01			(0.12)
		(0.70)	Number of appointments by phone in year before ref. point	0.05	0.05
Ever exempt from job search efforts 1y before ref. point	0.13	0.13			(0.46)
		(0.36)	Job center appointments by phone before current ue spell	0.02	0.02
Ever exempt from job search eff. in prev. ue spells	0.26	0.26			(0.31)
		(0.33)	Number of appointments in year before ref. point	3.20	3.22
Work exp. agricultural and forestry occupations					(1.24)
0 - Not looking for this occupation	0.97	0.97	3-level nationality swiss/EU/non-EU		
		(0.19)	EU	0.30	0.30
3 - Less than 1 year of experience	0.00	0.00			(0.55)
		(0.12)	Swiss	0.51	0.50

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Table D.1: Pre-treatment covariate means and standardized differences in trimmed and untrimmed sample.

Variable	untrimmed	trimmed
4 - 1-3 years of experience	0.01	0.01
		(0.20)
5 - More than 3 years of experience	0.01	0.01
		(0.15)
Other	0.00	0.00
		(0.13)
Work exp. production occupations in industry and trade		
0 - Not looking for this occupation	0.72	0.72
		(0.96)
1 - Experience unknown	0.03	0.03
		(0.20)
2 - No experience	0.03	0.04
		(0.22)
3 - Less than 1 year of experience	0.02	0.02
		(0.17)
4 - 1-3 years of experience	0.06	0.06
		(0.45)
5 - More than 3 years of experience	0.14	0.14
		(0.67)
Work exp. technical and information techn. occupations		
0 - Not looking for this occupation	0.90	0.90
		(0.04)
1 - Experience unknown	0.01	0.01
		(0.08)
2 - No experience	0.00	0.00
		(0.14)
3 - Less than 1 year of experience	0.00	0.00
		(0.28)
4 - 1-3 years of experience	0.02	0.02
		(0.18)
5 - More than 3 years of experience	0.07	0.07
		(0.28)
Work exp. construction and mining occupations		
0 - Not looking for this occupation	0.87	0.87
		(0.15)
1 - Experience unknown	0.01	0.01
		(0.05)
2 - No experience	0.01	0.00
		(0.07)
3 - Less than 1 year of experience	0.01	0.01
		(0.01)
4 - 1-3 years of experience	0.03	0.03
		(0.16)
5 - More than 3 years of experience	0.08	0.08
		(0.12)
Work exp. trade and transport occupations		
0 - Not looking for this occupation	0.71	0.70
		(0.26)
1 - Experience unknown	0.02	0.02
		(0.09)
2 - No experience	0.03	0.03
		(0.09)
3 - Less than 1 year of experience	0.02	0.02
		(0.21)
4 - 1-3 years of experience	0.06	0.06
		(0.46)
5 - More than 3 years of experience	0.17	0.17
		(0.05)
Work exp. occupations providing personal services		
0 - Not looking for this occupation	0.70	0.70
		(0.42)
1 - Experience unknown	0.02	0.02
		(0.18)
2 - No experience	0.02	0.02
		(0.05)
3 - Less than 1 year of experience	0.02	0.02
		(0.16)
4 - 1-3 years of experience	0.07	0.07
		(0.29)
5 - More than 3 years of experience	0.17	0.17
		(0.20)
Work exp. management, admin, banking, insurance, legal		
0 - Not looking for this occupation	0.71	0.71
		(0.88)
		(1.00)
Non-EU	0.20	0.20
		(0.62)
Months w/o income subj. to contrib. 5y before ref. p.	4.33	4.39
		(0.54)
Months w/o income subj. to contrib. 7y before ref. point	9.49	9.61
		(0.67)
Number of kids (only available for women)	0.13	0.13
		(0.58)
Number of previous unemployment spells	1.93	1.93
		(0.11)
Months out of labor force in 3 years before ref. point	2.24	2.26
		(0.38)
Open positions in desired job in canton per 100k pop.	12.81	12.93
		(0.27)
Language level best non-native language	0.61	0.62
		(0.40)
Type of work permit		
B	0.24	0.24
		(0.67)
C	0.25	0.26
		(0.49)
Other	0.51	0.50
		(1.00)
Placeability (last evaluation before ref. point)		
0 - not available	0.49	0.48
		(1.28)
1 - difficult	0.08	0.08
		(0.63)
2 - medium	0.31	0.32
		(1.23)
3 - easy	0.12	0.12
		(0.33)
Population of municipality of residence (in 1,000)	52.12	50.26
		(2.01)
Pregnant during year before the ref. point	0.04	0.04
		(0.31)
Qualification needs (last evaluation before ref. point)		
0 - none	0.92	0.92
		(0.80)
1 - basic qualification	0.04	0.04
		(0.60)
2 - professional qualification	0.04	0.04
		(0.50)
Sanction days concerning jobsearch 1y before ref. point	1.43	1.46
		(0.50)
Sanction days concerning misbehavior 1y before ref. point	0.22	0.22
		(0.04)
Sanction days concerning programs 1y before ref. point	0.03	0.03
		(0.17)
Sanction days related to jobsearch in previous ue spells	1.78	1.77
		(0.19)
Sanction days related to misbehavior in prev. ue spells	0.70	0.70
		(0.13)
Sanction days related to programs in previous ue spells	0.14	0.14
		(0.06)
Avg sickness days in months ue 1y before ref. point	0.15	0.15
		(0.14)
Size of labor force in region of residence (in 1,000)	87.20	86.21
		(1.81)
Months with social assistance 1y before ref. point	0.32	0.32
		(0.15)
Assigned to special consultation 1y before ref. point	0.03	0.03
		(0.05)
Assigned to special consultation in previous ue spells	0.02	0.02
		(0.04)
Months with supplementary benefits 1y before ref. point	0.02	0.02
		(0.04)
Months with survivors ins. benefits 1y before ref. point	0.05	0.05
		(0.15)
Registered at job center while still working	0.28	0.28
		(0.13)
Months in unemployment in 5y before reference point	6.89	6.80
		(1.09)

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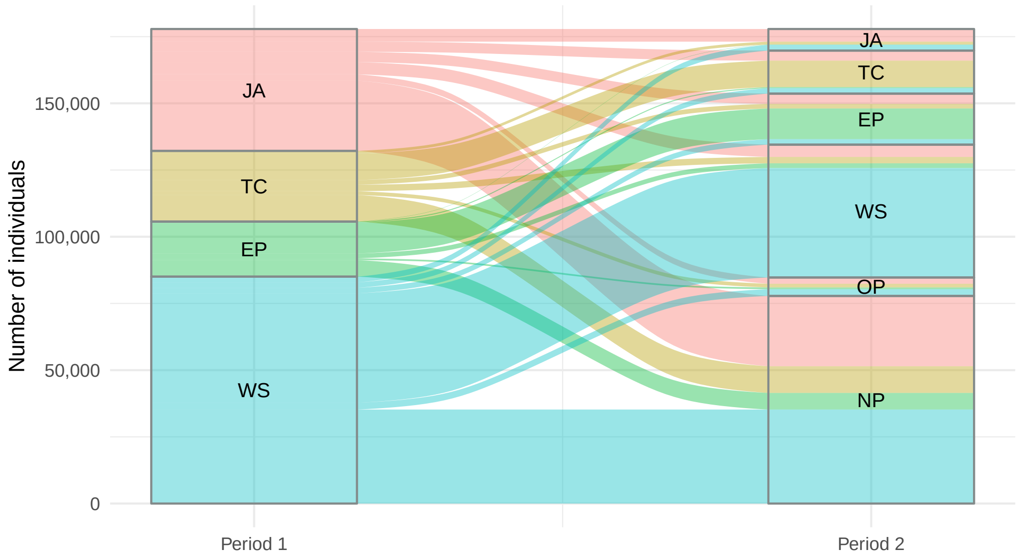
Table D.1: Pre-treatment covariate means and standardized differences in trimmed and untrimmed sample.

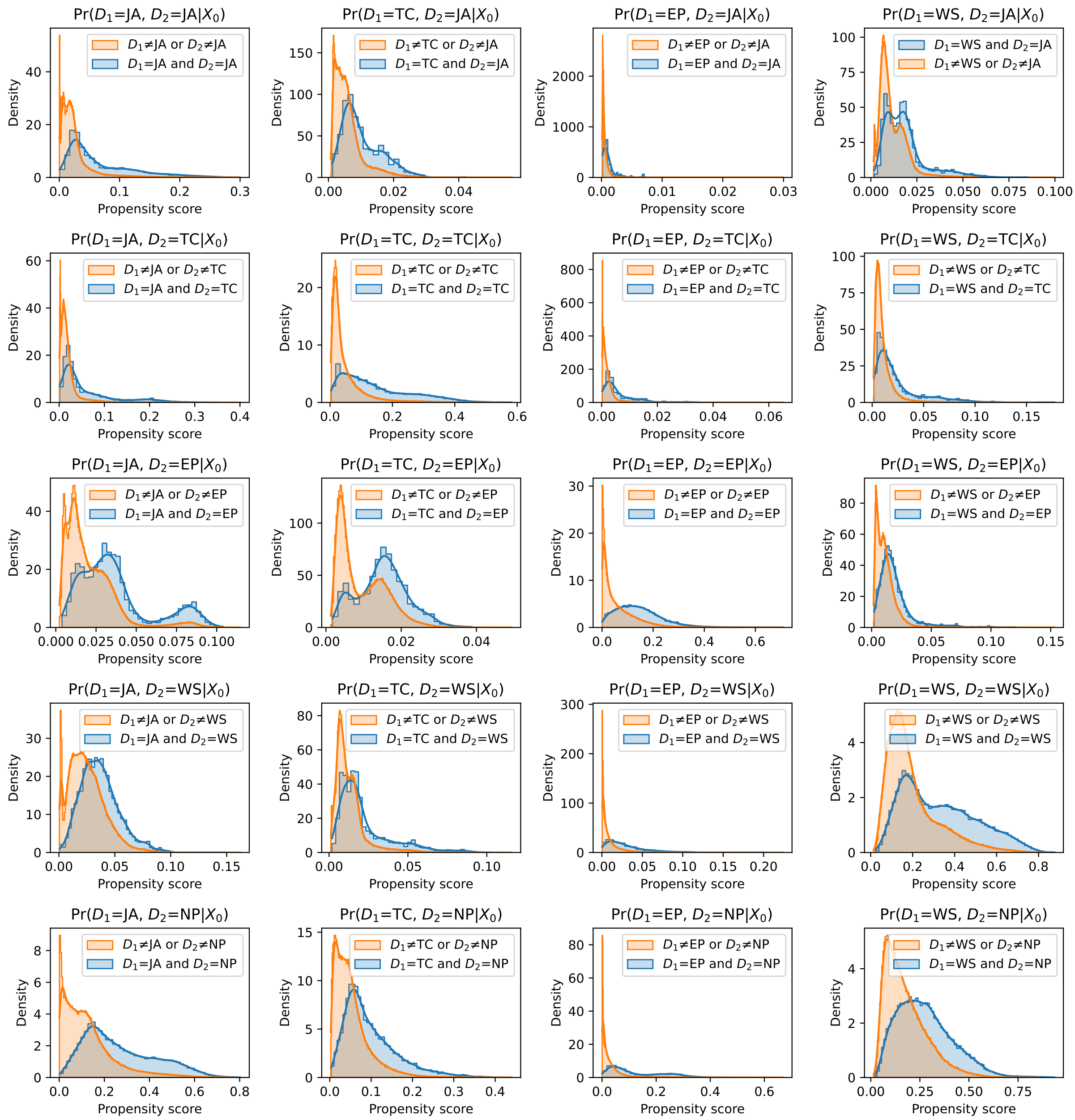
Variable	untrimmed	trimmed	Variable	untrimmed	trimmed
1 - Experience unknown	0.02	0.02	Months in unemployment in 7y before reference point	8.87	8.77
		(0.07)			(0.89)
2 - No experience	0.02	0.02	Month of start of unemployment		
		(0.12)	01 - January	0.13	0.13
3 - Less than 1 year of experience	0.01	0.01			(0.07)
		(0.17)	02 - February	0.06	0.06
4 - 1-3 years of experience	0.04	0.05			(0.16)
		(0.22)	03 - March	0.05	0.05
5 - More than 3 years of experience	0.20	0.20			(0.06)
		(1.14)	04 - April	0.08	0.08
Work exp. health, teaching, scientists, cultural occup.					(0.11)
0 - Not looking for this occupation	0.83	0.83	05 - May	0.08	0.08
		(0.12)			(0.06)
1 - Experience unknown	0.01	0.01	06 - June	0.07	0.07
		(0.12)			(0.06)
2 - No experience	0.01	0.01	07 - July	0.08	0.08
		(0.20)			(0.15)
3 - Less than 1 year of experience	0.01	0.01	08 - August	0.08	0.08
		(0.11)			(0.02)
4 - 1-3 years of experience	0.03	0.03	09 - September	0.08	0.08
		(0.08)			(0.16)
5 - More than 3 years of experience	0.11	0.11	10 - October	0.09	0.09
		(0.19)			(0.12)
Female	0.48	0.48	11 - November	0.09	0.09
		(0.55)			(0.07)
Language level French	2.61	2.62	12 - December	0.10	0.10
		(0.52)			(0.08)
Language level German	2.71	2.65	Year of start of unemployment	2,012.59	2,012.58
		(1.88)			(0.41)
Avg monthly additional income in previous ue spells	266.81	264.05	Months unemployed in year before reference point	2.47	2.47
		(0.46)			(0.16)
Avg net monthly ue benefits in previous ue spells	1,000.76	995.01	Months unemployed in 2nd year before reference point	0.85	0.82
		(0.44)			(1.43)
Avg monthly sickness days in previous ue spells	0.12	0.12	Months unemployed in 3rd year before reference point	1.24	1.21
		(0.27)			(1.27)
Avg monthly waiting days in previous ue spells	0.38	0.38	Unemployment rate in region of residence	3.34	3.35
		(0.02)			(0.73)
Incidents concerning jobsearch 1y before ref. point	0.33	0.34	Urban-rural classification of municipality of residence		
		(0.21)	Intermediate	0.17	0.17
Incidents concerning misbehavior 1y before ref. point	0.08	0.08			(0.38)
		(0.07)	Rural	0.11	0.11
Incidents concerning programs 1y before ref. point	0.01	0.01			(0.59)
		(0.13)	Urban	0.72	0.72
Incidents concerning jobsearch in previous ue spells	0.64	0.63			(0.73)
		(0.39)	Vocational degree	0.50	0.51
Incidents concerning misbehavior in prev. ue spells	0.24	0.24			(0.43)
		(0.21)	Avg waiting days in months ue 1y before ref. point	1.76	1.76
Incidents concerning programs in previous ue spells	0.03	0.03			(0.04)
		(0.00)	Willing to move for new job	0.03	0.03
					(0.99)

Figure D.1: Alluvial plot of program sequences in the trimmed sample



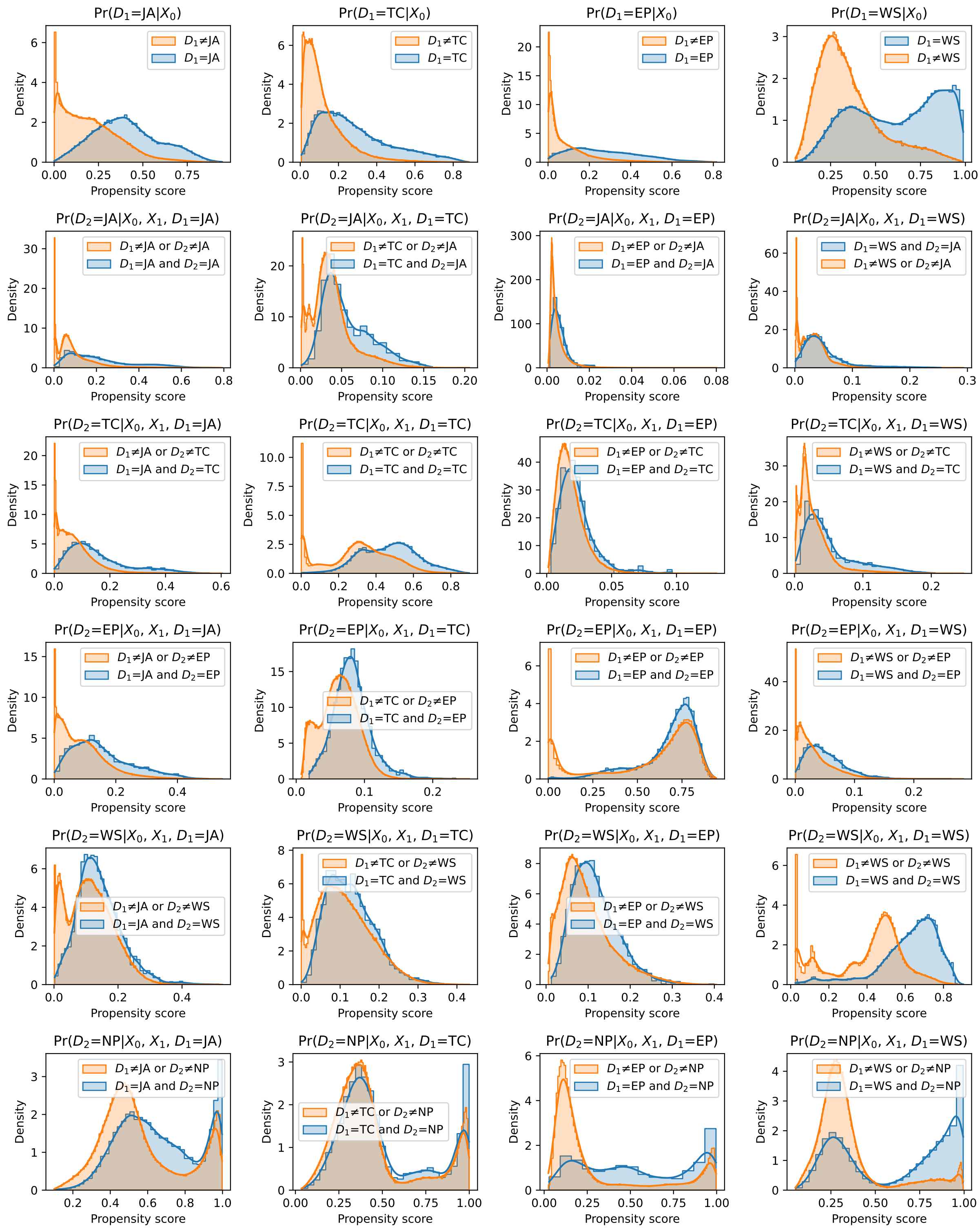
Notes: Program frequencies and transitions between first and second period. *JA*: Job-search assistance, *TC*: Training course, *EP*: Employment program, *WS*: Wage subsidy, *OP*: Other programs, *NP*: No program.

Figure D.2: Overlap plots for static policies under static confounding



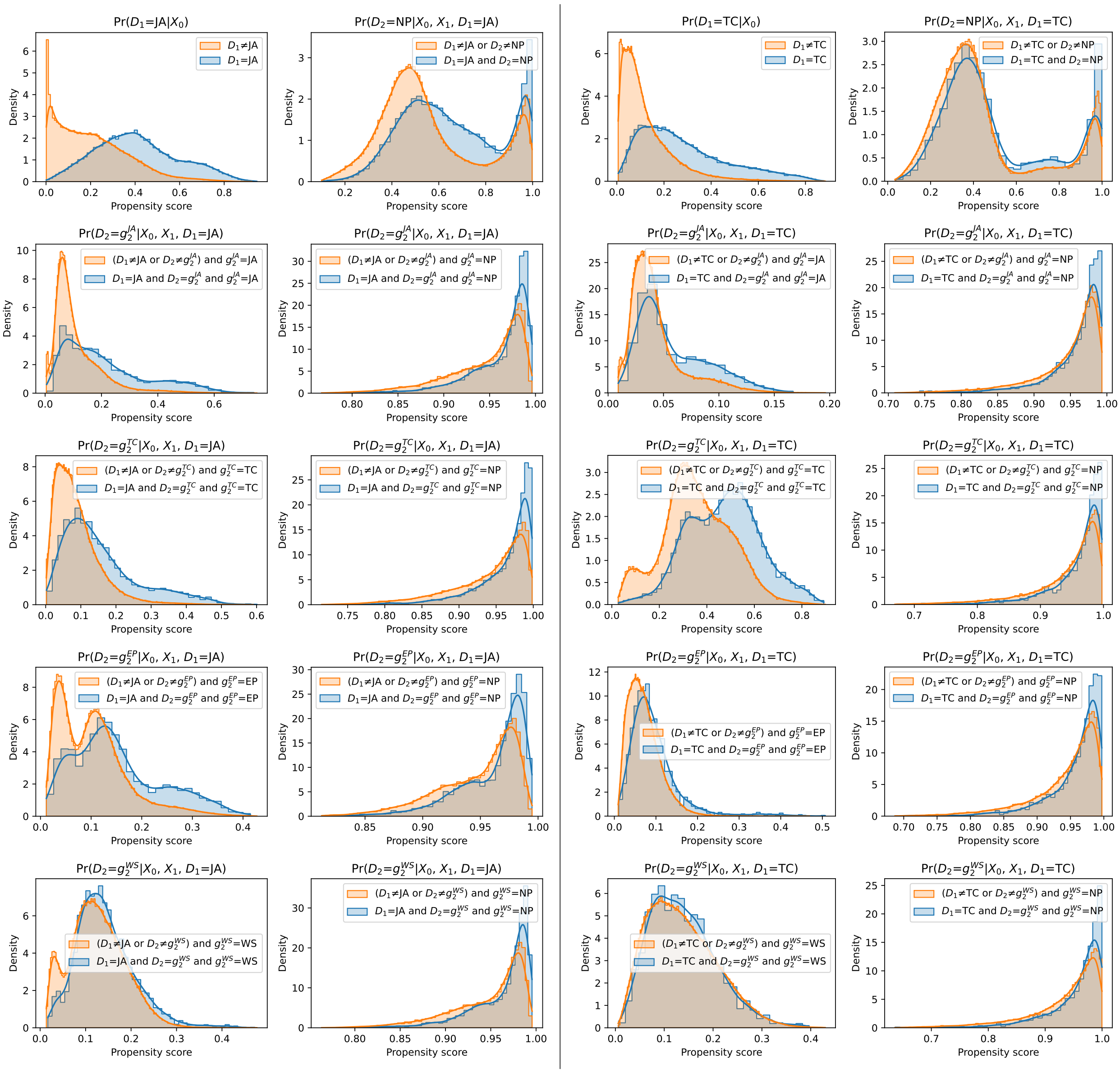
Notes: This figure plots propensity score densities after trimming for all static policies considered in the main analysis, when assuming static confounding. For each static policy, densities are plotted separately for individuals following the policy versus those not following the policy. *JA*: Job-search assistance, *TC*: Training course, *EP*: Employment program, *WS*: Wage subsidy, *NP*: No program.

Figure D.3: Overlap plots for static policies under dynamic confounding



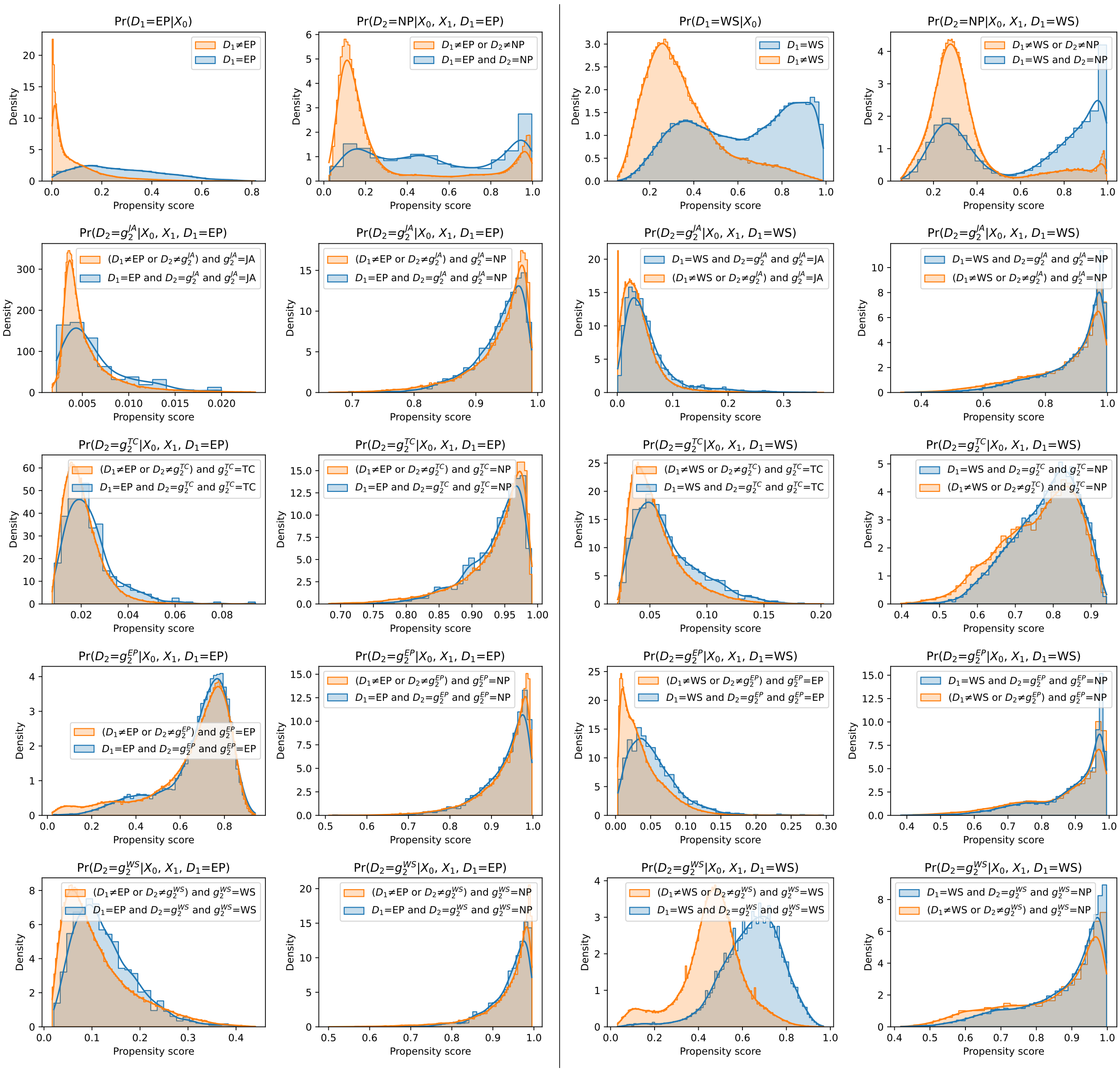
Notes: This figure plots propensity score densities after trimming for all static policies considered in the main analysis, when assuming dynamic confounding. For each static policy, densities are plotted separately for individuals following the policy versus those not following the policy. *JA*: Job-search assistance, *TC*: Training course, *EP*: Employment program, *WS*: Wage subsidy, *NP*: No program.

Figure D.4: Overlap plots of dynamic policies starting with *JA* or *TC* under dynamic confounding.



Notes: This figure plots propensity score densities after trimming for all dynamic policies starting with *JA* or *TC* considered in the main analysis. g_2^{XX} refers to the dynamic policy $g_2(Y_1)$ defined in equation 4.1 with $d_2 = XX$. For each policy, densities are plotted separately for individuals following policy $\mathbf{g}_2(Y_1)$ versus those not following the policy and separately for the different realizations of the function g_2^{XX}. *JA*: Job-search assistance, *TC*: Training course, *EP*: Employment program, *WS*: Wage subsidy, *NP*: No program.

Figure D.5: Overlap plots of dynamic policies starting with *EP* or *WS* under dynamic confounding.



Notes: This figure plots propensity score densities after trimming for all dynamic policies starting with *EP* or *WS* considered in the main analysis. g_2^{XX} refers to the dynamic policy $g_2(Y_1)$ defined in equation 4.1 with $d_2 = \text{XX}$. For each policy, densities are plotted separately for individuals following policy $\mathbf{g}_2(Y_1)$ versus those not following the policy and separately for the different realizations of the function g_2^{XX}. *JA*: Job-search assistance, *TC*: Training course, *EP*: Employment program, *WS*: Wage subsidy, *NP*: No program.