# Estimating the Long-Term Effects of Novel Treatments

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## Abstract

Policy makers often need to estimate the long-term effects of novel treatments, while only having historical data of older treatment options. We propose a surrogate-based approach using a long-term dataset where only past treatments were administered and a short-term dataset where novel treatments have been administered. Our approach generalizes previous surrogate-style methods, allowing for continuous treatments and serially-correlated treatment policies while maintaining consistency and root-n asymptotically normal estimates under a Markovian assumption on the data and the observational policy. Using a semi-synthetic dataset on customer incentives from a major corporation, we evaluate the performance of our method and discuss solutions to practical challenges when deploying our methodology.

## 1 Introduction

Businesses invent new ways of interacting with their customers. Marketing departments devise new marketing campaigns. Pharmaceutical companies roll out trials of new drugs. In many domains, policy makers want to understand the long-term effects of novel treatments, while only having access to short-term data following their deployment.

We propose an estimation methodology that leverages historical data, collected under earlier treatment regimes, to derive estimates of the long-term effects of novel treatments. The seminal paper of Athey et al. (2020) proposes a surrogate index as one solution to this problem. The fundamental assumption of the surrogate index is that causal effects on long-term outcomes are channeled through a set of observable short-term proxies. This assumption allows one to use the historical long-term data set to learn a mapping from short-term signals to a projected long-term reward and subsequently estimate the causal effect of novel treatments on this index of surrogates.

In practice, treatment policies are often *dynamic*: treatments are assigned repeatedly, and their assignments depend on past treatments and short-term outcomes. Unfortunately, this pattern can easily break the assumptions needed for the surrogate index to work. For example, suppose that a firm offers multiple investments to a particular customer in the historical data and these investments are auto-correlated, i.e. if a customer receives an investment this month, then they will receive an investment with high probability in one of the subsequent months. These future investments can substantially increase the long-term outcome of interest, and this increase will be attributed to the short-term proxies. The surrogate index thus formed will tend to over-predict long-run outcomes, and so when it is used to measure the treatment effect of some new treatment in the short-run data, the estimated treatment effect will be bigger (in absolute magnitude) than the truth.

The main methodological innovation of this paper is to suggest using the dynamic treatment effect analysis of Lewis & Syrgkanis (2020) on the historical data in order to create an unbiased dynamically adjusted surrogate index. Our new index takes the interpretation of the projected long-term reward in

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the absence of any future treatments. This model produces unbiased causal effect estimates of the long-term effects of the novel treatments, even with auto-correlated treatments.

A second contribution of the paper is to generalize the final causal analysis step to allow for multiple continuous treatments rather than a single binary treatment, as in Athey et al. (2020). Our new estimator for this expanded surrogate approach allows for the construction of valid confidence intervals, even when using flexible machine learning models at both stages of the estimation to deal with high-dimensional data. In short, we show how one can combine three recently developed techniques, i.e. i) the surrogate index approach of Athey et al. (2020), ii) the double machine learning approach of Chernozhukov et al. (2018a) and iii) the dynamic treatment effect estimation approach of Lewis & Syrgkanis (2020), in a single data analysis pipeline to estimate treatment effects in the presence of dynamic treatment policies.

Our work lies in the broader field of estimating causal effects with machine learning and Neyman orthogonality (Neyman, 1979; Robinson, 1988; Ai & Chen, 2003; Chernozhukov et al., 2016; Chernozhukov et al., 2018b). Moreover, it relates to the work on machine learning estimation of treatment effects in the dynamic treatment regime (Nie et al., 2019; Thomas & Brunskill, 2016; Petersen et al., 2014; Kallus & Uehara, 2019b,a; Lewis & Syrgkanis, 2020; Bodory et al.; Singh et al., 2020) and on structural nested models in biostatistics (Robins, 1986; Robins et al., 1992; Robins, 1994; Robins & Ritov, 1997; Robins et al., 2000; Lok & DeGruttola, 2012; Vansteelandt et al., 2014; Vansteelandt & Sjolander, 2016). Finally, it relates to the surrogacy literature in causal inference (Prentice, 1989; Begg & Leung, 2000; Frangakis & Rubin, 2002; Freedman et al., 1992). Our work builds on insights in these works and proposes the first complete method that combines all three lines of work, so as to estimate the long-term effect of novel treatments from observational data that stem from a dynamic observational policy and in a manner that allows for high-dimensionality.

## 2 Problem and Methodology

Though our methodology applies in many domains, for concreteness we consider the running example of a firm making investments in its customers in order to increase subsequent purchases. A firm has a number of distinct investments/treatments $T_1, T_2 \dots T_k$ it can offer to customers. At each period $t$ (e.g. the period of a month), a vector of treatments $T_{i,t} = (T_{i,t,1}, \dots, T_{i,t,k}) \in \mathbb{R}^k$, is applied to each customer $i$. We also observe a vector of $p$ characteristics $X_{i,t} = (X_{i,t,1}, \dots, X_{i,t,p}) \in \mathbb{R}^p$, some of which are constant within customer (e.g. industry) and some of which vary over time and customer (e.g. last month's revenue). We observe the outcome of interest $Y_{i,t} \in \mathbb{R}$ (e.g. monthly revenue).

Treatments may affect outcomes over many future periods, and this horizon could vary by treatment. We are therefore interested in identifying the average effect of each treatment at some period $t$, on the cumulative outcome in the subsequent $M$ periods, i.e. $\bar{Y}_{i,t} = \sum_{\kappa=0}^{M} Y_{i,t+\kappa}$. Suppose direct inference is impossible because we have not yet observed the subsequent $M$ periods following a novel treatment. Instead, we assume that for every customer $i$ and period $t$, we have access to a vector of $d$ short-term proxies/surrogates, $S_{i,t} = (S_{i,t,1}, \dots, S_{i,t,d})$. In practice, surrogates might include the next few months of revenue and other measures that are indicative of a customer's trajectory. Given these surrogate measures, Athey et al. (2020) propose the following estimation strategy:

1. Begin with a long-term *observational data set* that need only include customer characteristics $X_{i,t}$, surrogates $S_{i,t+1}$, realized $M$-period outcomes $\bar{Y}_{i,t}$. Based on this data-set we construct a model that predicts outcome $\bar{Y}_{i,t}$ from $S_{i,t}$ and $X_{i,t}$:

$$g_0(S_{i,t}, X_{i,t}) := \mathbb{E}[\bar{Y}_{i,t} \mid S_{i,t}, X_{i,t}]. \qquad \text{(surr. model)}$$

   Any machine learning estimation algorithm can be used to construct $\hat{g}$.

2. Next, use a second short-term, *experimental data set* that includes customer features, $X_{i,t}$, surrogates, $S_{i,t}$, and treatments $T_{i,t}$ (the set of customers can vary across the data-sets). This data set is restricted to the period where we can measure treatments, but only requires a few months of leading surrogates rather than $M$ periods of leading outcomes. For any such sample we calculate the surrogate index:

$$I_{i,t} := \hat{g}(S_{i,t}, X_{i,t}). \qquad \text{(predicted surrogate index)}$$

   This predicted index becomes the outcome in the final causal model.

This standard approach will be consistent for the treatment effects under two assumptions. First, the relationship between the surrogate variables and the target outcome remains unchanged over our historical sample period and the periods into which we project future outcomes. We follow Athey et al. (2020) in maintaining this assumption. The model is able to account for steady growth in a natural way: as the surrogates grow, the predicted outcomes grow too.

Second, the only causal path from the treatment to the outcome goes through the surrogates. In other words, the treatment has an effect on $M$-period outcomes if and only if it affects these surrogates. This critical "sufficiency" assumption typically requires that a few periods of post-treatment data are available to build a relatively good understanding of the $M$-period path.

Serially correlated treatment policies will often violate this second assumption. A customer who receives a treatment in period $t$ may be more likely to receive a second treatment over the next year, even controlling for observed features $X_{it}$. This positive correlation could be either because a customer that is treated subsequently becomes more salient to the firm, or because the treatment successfully drives surrogate metrics higher, thereby improving the perceived return on subsequent treatments of this customer (an example could be a firm that targets its fast-growing customers).

In either case, the standard estimated surrogate index model $\hat{g}$ will now be biased; we would wrongly estimate that even small increases in our proxy measures forecast strong growth in $M$-period outcomes on average. If we subsequently estimate the causal effect of novel treatments on the surrogate index, we will over-estimate the causal effect on $M$-period outcomes. Causal effects that should be attributed to the future treatment get doubly attributed to the current treatment. To remove this bias we include a dynamic adjustment step based on Lewis & Syrgkanis (2020) that subtracts the effect of future treatments from the target outcome before estimating our surrogate index model. To show this why this method is both necessary and sufficient for removing bias, we frame the problem and solution in approach in a two-period example before formally presenting the general case in Section 3.

### 2.1 A two-period example

As in our formal model, we collapse $X_{i,t}$ and $S_{i,t-1}$ so that the surrogates at each period and the controls in the next period are all denoted by $S_{i,t-1}$. In other words, all customer observable characteristics in the next period are candidate surrogates and become controls for the period after next. This is without loss of generality. For this illustration, we further simplify by assuming a linear Markovian model of how the random variables evolve over time and focusing on a single scalar investment. With these simplifications, the evolution of the random variables can be described in three equations:

$$\begin{aligned} S_{i,t} &= AT_{i,t} + BS_{i,t-1} + \epsilon_{i,t} && \text{(evolution of controls)} \\ Y_{i,t} &= \gamma' S_{i,t} + \zeta_{i,t} && \text{(outcome equation)} \\ T_{i,t+1} &= \kappa T_{i,t} + \lambda' S_{i,t} + \eta_{i,t} && \text{(treatment policy)} \end{aligned}$$

where $A$ is an $p \times 1$ matrix, $B$ is a $p \times p$ matrix, $\gamma, \lambda$ are $p$-dimensional vectors and $\kappa$ is a scalar. The terms $\epsilon_{i,t}, \zeta_{i,t}, \eta_{i,t}$ are exogenous mean-zero independent noise terms. For conciseness we drop the customer index $i$ in the remaining equations.

**Target effect estimand** With two periods, the long term outcome $\bar{Y}_{i,t} = Y_{i,t} + Y_{i,t+1}$. The effect of treatment $T_t$ on $\bar{Y}_t$ can be derived from the structural model equations:

$$\bar{Y}_t = \gamma' S_t + \gamma' S_{t+1} \text{noise} = \gamma' A T_{t+1} + \gamma'(B+I)AT_t + \gamma'(B+I)BS_{t-1} + \text{noise}$$

Thus we see that the effect of $T_t$ on $Y_{t+1} + Y_t$, assuming future treatments are held constant, is:

$$\theta_0 := \gamma'(B+I)A. \tag{1}$$

**Surrogate index without dynamic adjustment** If we train a surrogate index by regressing $\bar{Y}_t$ on $S_t$, as in Athey et al. (2020), we would estimate:

$$g_0(S_t) = \mathbb{E}[\bar{Y}_t \mid S_t] = \gamma'(I+B)S_t + \gamma' A \mathbb{E}[T_{t+1} \mid S_t] \tag{2}$$

Subsequently, if we estimate the causal effect of $T_t$ on $g_0(S_t)$, then this effect would be:

$$\theta_* = \theta_0 + \gamma' A \frac{\mathbb{E}[\mathbb{E}[T_{t+1} \mid S_t]\, T_t]}{\mathbb{E}[T_t^2]} \tag{3}$$

The standard estimate contains a bias stemming from the fact that investment today can lead to higher investment tomorrow. The serial correlation can be direct if $\kappa \geq 0$ in the treatment policy, or via the surrogates if $\lambda' A \geq 0$.

**Dynamic adjustment** We propose a new first step, estimating the single-period causal effect of $T_{t+1}$ on $Y_{t+1}$, controlling for $S_t$. This conditional expectation is equal to:

$$\mathbb{E}[Y_{t+1} \mid T_{t+1}, S_t] = \gamma' A T_{t+1} + \gamma' B S_t. \tag{4}$$

We can now create an *adjusted long-term outcome*, $\bar{Y}_t^{\text{adj}} := Y_t + Y_{t+1} - \alpha'_{t+1} T_{t+1}$, where $\alpha_{t+1}$ is our estimate of $\gamma' A$ from this first step.

The *dynamically adjusted surrogate index* is the projected adjusted long-term outcome, conditional on the observed surrogates:

$$\begin{aligned} g_0^{\text{adj}}(S_t) &= \mathbb{E}[Y_t \mid S_t] + \mathbb{E}[Y_{t+1} - \gamma' A T_{t+1} \mid S_t] \\ &= \gamma' S_t + \mathbb{E}[\gamma' B S_t \mid S_{t+1}] \\ &= \gamma'(I + B) S_t \end{aligned} \tag{5}$$

This new index captures the projected $M$-period outcomes as if the customer was offered no future treatments.

When we estimate the effect of the treatment based on this adjusted surrogate index we recover:

$$\begin{aligned} \mathbb{E}[g_0^{\text{adj}}(S_t) \mid T_t, S_{t-1}] &= \gamma'(I + B)\mathbb{E}[S_t \mid T_t, S_{t-1}] \\ &= \gamma'(I + B) A T_t + \gamma'(I + B) B S_{t-1} \end{aligned} \tag{6}$$

With the dynamic adjustment, the coefficient in front of $T_t$ that we recover is the true causal effect $\theta_0 = \gamma'(I + B)A$. Lewis & Syrgkanis (2020) show how this adjustment approach can be extended to many periods, via a recursive peeling process, and also to high-dimensional surrogates and controls via a dynamic double machine learning approach.

**Estimating causal effects of new treatments** With this adjusted surrogate index in hand, we can also estimate the long-term effect of any other treatment that was introduced more recently. In this example, the stationarity assumption of both proposed surrogate approaches requires that $B$, which governs how the surrogate evolves, and $\gamma$, which governs how surrogates translate to per-period outcomes, do not change between the observational and experimental data-sets. These two parameters govern how surrogates today relate to future outcomes in the absence of any treatment.

Under such a condition, if we introduce a new treatment $T_t^{\text{new}}$, which has a different effect $A^{\text{new}}$ on the surrogates (and hence on the long-term outcome), then the effect of this treatment on the long-term outcome is $\theta_0^{\text{new}} = \gamma'(I + B)A^{\text{new}}$. This $\theta_0^{\text{new}}$ is exactly the outcome of estimating the causal effect of $T_t^{\text{new}}$ on $g_0^{\text{adj}}(S_{t+1})$ controlling for $S_t$.

## 3 Formal Guarantees

In this section we present our main results. In addition to expanding to $M$ periods, we introduce two substantive innovations over the basic strategy presented in the two period example above. First, we develop a generalization of the doubly robust estimation method in Athey et al. (2020) to the case of multiple continuous treatments. Second, we relax the linearity assumptions in the structural model and make use of orthogonal machine learning techniques Chernozhukov et al. (2018a) to allow for a rich set of potential confounders and valid analytic confidence intervals.

We now assume that we observe the full $M$-period time series, $(S_0, T_1, S_1, Y_1, T_2, S_2, Y_2, \ldots, T_M, S_M, Y_M)$, for each sample in the observational population, denoted as $o$. In contrast, we only observe $(S_0, T_1, S_1)$ for each sample from the experimental population, denoted as $e$. We assume that the data obey the Markovian assumptions depicted in the causal graph in Figure 1.

Our goal is to isolate the causal effect of treatments in period 1, $T_1$, on the long term outcome $\bar{Y}_1 := \sum_{t=1}^{M} Y_t$. Formally, if we fix the future treatments that each sample receives and we change

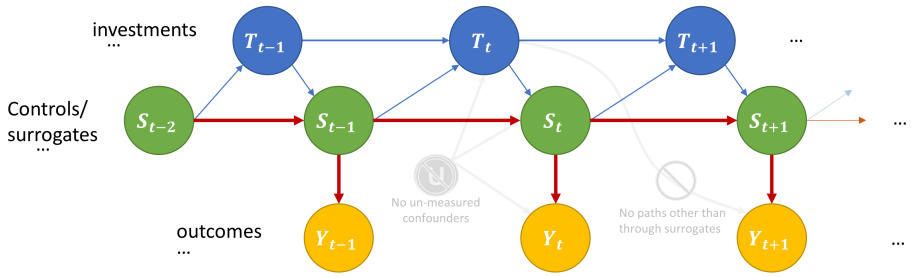


Figure 1: Causal graph representation of the main assumptions of our causal analysis, which are: i) there are no paths to future outcomes from past investments that do not go through surrogates, ii) the relationships designated with a red arrow have to remain un-changed between the long-term data-set and the short-term data-set, iii) there are no un-measured confounders at each period, i.e. variables that are not observed and which affect the treatment assignment and the outcome directly.

the treatment $T_1$ from some value $t_0$ to some other value $t_1$, the treatment effect is:

$$\tau(t_1, t_0) := \mathbb{E}_e[\mathbb{E}[\bar{Y}_1 \mid do(T_1 = t_1), T_2, \dots, T_M] - \mathbb{E}[\bar{Y}_1 \mid do(T_1 = t_0), T_2, \dots, T_M]]. \tag{7}$$

We present our theoretical results in two steps. For exposition, we begin by assuming treatments happen only at period 1. This is the setting analyzed in Athey et al. (2020), albeit only for the case of a single binary treatment $T_1$.[1] We then show how this approach can be modified to incorporate a dynamic treatment policy in the observational and experimental sample.

**Notation** Throughout, we will denote with $\mathbb{E}_e[\cdot]$ the expectation conditional on the experimental population and $\mathbb{E}_o[\cdot]$ the expectation conditional on the observational population. Moreover, for any vector-valued function $f$ that takes as input a random variable $Z$, we denote with:

$$\|f\|_{2,o} = \sqrt{\mathbb{E}_o\left[\|f(Z)\|_2^2\right]} \tag{8}$$

and analogously $\|f\|_{2,e}$. We denote with $\mathbb{E}_n[\cdot]$, the empirical expectation over all the samples, i.e. for any random variable $Z$, $\mathbb{E}_n[Z] := \frac{1}{n}\sum_i Z_i$, and with $\mathbb{E}_{e,n}$ and $\mathbb{E}_{o,n}$ the empirical expectation over the experimental and observational samples correspondingly, i.e. $\mathbb{E}_{e,n}[Z] = \frac{1}{n_e}\sum_{i\in e} Z_i$ and $\mathbb{E}_{o,n}[Z] = \frac{1}{n_o}\sum_{i\in o} Z_i$.

### 3.1 Double/Debiased Correction without Dynamic Adjustment

To begin, we assume that $T_2, \dots, T_M = 0$. Throughout, we assume a *partially linear relationship* between the treatment and the long-term outcome:

$$\mathbb{E}_e[\bar{Y}_1 \mid T_1, S_0] = \theta_0^\top \phi(T_1, S_0) + b_0(S_0) \tag{PLR}$$

for some known feature map $\phi(\cdot,\cdot)$, but arbitrary function $b_0(\cdot)$.

Formally, the *invariance of the surrogate-outcome* relationship requires that that the mean-relationship between the surrogates $S_1$ and the long-term outcome does not change between the observational and the experimental sample:

$$g_0(S_1) := \mathbb{E}_o[\bar{Y}_1 \mid S_1] = \mathbb{E}_e[\bar{Y}_1 \mid S_1]. \tag{IR}$$

[1] Athey et al. (2020) also allowed for estimation of average treatment effects, even in the case when there is arbitrary treatment effect heterogeneity. In this work, we assume that treatment effects are constant. A generalization to the case of arbitrary treatment effect heterogeneity is feasible, but would require the estimation of conditional covariance matrices, which would make the estimation algorithm more brittle and the exposition much more complex.

Under the PLR assumption and the causal graph governing our data, we have that:

$$\tau(t_1, t_0) = \theta_0^\top \mathbb{E}[\phi(t_1, S_0) - \phi(t_0, S_0)]. \tag{9}$$

We establish three consistent estimators for $\theta_0$ that follow the same intuition as Athey et al. (2020), adapted to consider linear effects of continuous treatments rather than a single binary treatment.

**Theorem 3.1** (Identification)**.** *Denote the residual surrogate index and the residual treatment with:*

$$\tilde{g}_0(S_1) := g_0(S_1) - \mathbb{E}_e[g_0(S_1) \mid S_0] \tag{10}$$

$$\tilde{T}_1 := \phi(T_1, S_0) - \mathbb{E}_e[\phi(T_1, S_0) \mid S_0] \tag{11}$$

*Then under assumptions (PLR) and (IR):*

$$\theta_0 = \mathbb{E}_e[\tilde{T}_1 \tilde{T}_1^\top]^{-1} \mathbb{E}_e[\tilde{T}_1 \tilde{g}_0(S_1)] \qquad \text{(surrogate index rep.)}$$

$$\theta_0 = \mathbb{E}_e[\tilde{T}_1 \tilde{T}_1^\top]^{-1} \mathbb{E}_o\left[\frac{\Pr(e \mid S_1, S_0)}{\Pr(o \mid S_1, S_0)} \frac{\Pr(o)}{\Pr(e)} \mathbb{E}_e[\tilde{T}_1 \mid S_1, S_0] \bar{Y}_1\right] \qquad \text{(surrogate score rep.)}$$

$$\theta_0 = \mathbb{E}_e[\tilde{T}_1 \tilde{T}_1^\top]^{-1} \left(\mathbb{E}_e[\tilde{T}_1 \tilde{g}_0(S_1)] + \mathbb{E}_o\left[\frac{\Pr(e \mid S_1, S_0)}{\Pr(o \mid S_1, S_0)} \frac{\Pr(o)}{\Pr(e)} \mathbb{E}_e[\tilde{T}_1 \mid S_1, S_0](\bar{Y}_1 - g_0(S_1))\right]\right) \qquad \text{(orthogonal rep.)}$$

The core estimation challenge that the surrogate approach resolves is that the treatments and outcome of interest are not observed in a single dataset. Intuitively, the first **surrogate index representation** approaches this challenge by using realized treatments from the experimental sample and, in place of realized outcomes, substitutes the expected outcome conditional on the surrogates, $g_0(S)$, which can be identified from the observational sample and then constructed in the experimental sample.

The second **surrogate score representation** reverses this substitution. The second term pairs an expectation of the treatment conditional on surrogates, $\mathbb{E}_e[\tilde{T}_1 \mid S_1, S_0]$, with the realized outcomes from the observational sample. This representation requires an added ratio of probabilities of appearing in each sample, $\Pr(e)$ and $\Pr(o)$, to adjust for sampling variation across the two datasets.

The third **orthogonal representation** blends the first two representations and satisfies *Neyman orthogonality*, which allows the construction of confidence intervals and double robustness.

The parameter identified in the equations in Theorem 3.1 is interpretable even if the partially linear assumption is violated. In this case, the equations are identifying the best linear projection of the variation in the long-term outcome that is not explained by the initial state. We formulate the estimation of $\theta_0$ based on the third orthogonal representation as a $Z$-estimator based on a vector of moment equations that depends on a vector of nuisance functions $f_0$, i.e.:

$$m(\theta_0; f_0) := \mathbb{E}[\psi(Z; \theta_0, f_0))] = 0 \tag{12}$$

and such that it satisfies the Neyman orthogonality condition:

$$D[m(\theta_0; f_0), f - f_0] := \frac{\partial}{\partial t} m(\theta_0; f_0 + t\,(f - f_0))\Big|_{t=0} = 0$$

Subsequently, this will allow us to invoke the results in Chernozhukov et al. (2018b), to derive an asymptotic normal estimator, even when high-dimensional, regularized approaches are used to estimate the nuisance functions $f_0$.

**Theorem 3.2** (Orthogonal Moment Formulation)**.** *Let $f = (g, q, p, h)$, denote a set of nuisance functions and define:*

$$q_0(S_1, S_0) := \frac{\Pr(e \mid S_1, S_0)}{\Pr(o \mid S_1, S_0)} \mathbb{E}_e[\tilde{T}_1 \mid S_1, S_0]$$

$$p_0(S_0) := \mathbb{E}_e[\phi(T_1, S_0) \mid S_0]$$

$$h_0(S_0) := \mathbb{E}_e[g(S_1) \mid S_0]$$

*Then $\theta_0$ is the solution to the moment equation:*

$$m(\theta_0; f_0) := m_e(\theta_0; f_0) + m_o(\theta_0; f_0) = 0$$

$$m_e(\theta; f) := \mathbb{E}\Big[1\{e\}\,(g(S_1) - h(S_0) - \theta^\top(\phi(T_1, S_0) - p(S_0))) \cdot (\phi(T_1, S_0) - p(S_0))\Big]$$

$$m_o(\theta_0; f) := \mathbb{E}\left[1\{o\}\, q(S_1, S_0)(\bar{Y}_1 - g(S_1))\right]$$

*Moreover, the moment $m$ satisfies the Neyman orthogonality property with respect to $f$. Furthermore, it satisfies a stronger double robustness property with respect to $g$ and $q$, i.e. if we denote with $\hat{\theta}$ the solution to $m(\theta; \hat{g}, \hat{q}, p_0, \hat{h}) = 0$, then:*

$$\left\| \mathbb{E}_e[\tilde{T}_1 \tilde{T}_1^\top] \left( \hat{\theta} - \theta_0 \right) \right\|_2 \leq \frac{\Pr(o)}{\Pr(e)} \|g_0 - \hat{g}\|_{2,o} \, \|q_0 - \hat{q}\|_{2,o}$$

We estimate $\theta_0$ by method of moments, solving the empirical analogues of these moment conditions, after estimating the nuisance functions $f$. In Theorem 3.3 below, which covers a more general case, we show that this estimator is asymptotically normal under relatively mild rate conditions on the estimation of the nuisances.

### 3.2 Double/Debiased Correction with Dynamic Adjustment

We now consider the case of potentially serially correlated treatments in all periods. To achieve consistent estimates of the effects of period 1 treatments in this setting we need to again assume that all the relationships are linear, albeit potentially high-dimensional:

$$\begin{aligned} S_t &= AT_t + BS_{t-1} + \epsilon_t \\ Y_t &= CS_t + \eta_t \\ T_{t+1} &= DT_{t-1} + GS_{t-1} + \zeta_t \end{aligned} \tag{13}$$

where $\epsilon_t, \eta_t, \zeta_t$ are i.i.d. random shocks.

In this case, the main identification result in Lewis & Syrgkanis (2020), shows that:

$$\tau(t_1, t_0) = \sum_{t=1}^{M} \theta_{1,t}^\top (t_1 - t_0) = \theta_0^\top (t_1 - t_0) \tag{14}$$

where $\theta_0 := \sum_{t=1}^{M} \theta_{1,t}$ and $\theta_{1,t}$ are interpreted as the dynamic effect of treatment at period 1 on the outcome at period $t$, controlling for all future treatments. This quantity is equivalent to the effect assuming that all future treatments are zero, since the effects of each period treatment under this linear model are linearly separable. This property simplifies both the estimation and the interpretation of the effect quantity as many causal quantities of potential interest collapse in this setting to the same object.

The identification results in Lewis & Syrgkanis (2020) also imply that we can identify the quantity $\theta_{1,t}$ by estimating the effect of $T_1$ on the multi-period dynamically adjusted target outcome:

$$\bar{Y}_1^{\text{adj}} = \sum_{t=1}^{M} \left[ Y_t - \sum_{\tau=2}^{t} \theta_{\tau,t}^\top T_t \right] = \theta_0^\top T_1 + \beta_0 S_0 + \mu_1 \tag{15}$$

for some exogenous mean-zero random shock variable $\mu_1$. This is the setup that we analyzed in the previous section, albeit with $\bar{Y}_1$ replaced by $\bar{Y}_1^{\text{adj}}$ and with $\phi(T_1, S_0) = T_1$.The PLR assumption with respect to this dynamically adjusted target immediately holds by the equation above.

Furthermore, we need to assume that the invariance of the surrogate-outcome assumption holds, albeit now for the dynamically adjusted long-term outcome:

$$g_0^{\text{adj}}(S_1) := \mathbb{E}_o[\bar{Y}_1^{\text{adj}} \mid S_1] = \mathbb{E}_e[\bar{Y}_1^{\text{adj}} \mid S_1] \tag{dynIR}$$

This assumption is more permissive in practice than the standard invariance assumption as we no longer require that the dynamic treatment policy in the observational data be the same as in the experimental data, but only that the adjusted outcomes retain the same relationship with the surrogates. For estimation, we can apply the same analysis as in the previous section. The differences are that 1) we now have this new target long-term outcome and 2) we must additionally account for the variance of the $\hat{\theta}_t$ terms that are being estimated by the dynamic adjustment algorithm. We show below that the method of moments estimator with plug in nuisance estimates is asymptotically normal:

**Theorem 3.3** (Surrogates with Dynamic Adjustment: Estimation and Asymptotic Normality)**.** *Let:*

$$\psi_0(Z;\theta,f_0) := 1\{e\} \underbrace{\left(\tilde{g}_0^{adj}(S_1) - \theta_0^\top \tilde{T}_1\right) \tilde{T}_1}_{\psi_{0,e}(Z;\theta,f_0)} + 1\{o\} \underbrace{\left(\bar{Y}_1^{adj} - g_0^{adj}(S_1)\right) q_0(S_1,S_0)}_{\psi_{0,o}(Z;\theta,f_0)}$$

$$\forall\, 2 \le \tau \le t \le M : \psi_{\tau,t}(Z;\theta,f_0) := 1\{o\} \left(\tilde{Y}_{t,\tau} - \sum_{\kappa=\tau+1}^{t} \theta_{\kappa,t}^\top \tilde{T}_{\kappa,\tau} - \theta_{\tau,t}^\top \tilde{T}_{\tau,\tau}\right) \tilde{T}_{\tau,\tau}$$

$$\forall\, 2 \le t \le M : \psi_t(Z;\theta_0,f_0) = \left[\psi_{t,t}(Z;\theta,f_0) \quad \dots \quad \psi_{t,2}(Z;\theta,f_0)\right]^\top$$

*where we use the short-hand notation* $\tilde{Y}_{t,\tau} = Y_t - \mathbb{E}[Y_t \mid S_{\tau-1}]$ *and* $\bar{Y}_1^{adj} = \sum_{t=1}^{M}\left(Y_t - \sum_{\tau=2}^{t} \theta_{\tau,t}^\top T_\tau\right)$ *and* $\tilde{T}_{t,\tau} = T_t - \mathbb{E}[T_t \mid S_{\tau-1}]$*. Let* $h_{t,\tau}(S_{\tau-1}) = \mathbb{E}[Y_t \mid S_{\tau-1}]$ *and* $p_{t,\tau}(S_{\tau-1}) = \mathbb{E}[T_t \mid S_{\tau-1}]$*. Let* $f = \{g, q, p, h, \{h_{\tau,t}, p_{\tau,t}\}_{2\le\tau\le t\le M}\}$*, denote the set of all nuisance functions and let* $f_0$ *denote their true value. Consider the estimator based on the empirical version of the orthogonal moment, with plug-in nuisance estimates* $\hat{f}$ *trained on a separate sample, i.e.* $\hat{\theta}$ *solves the empirical moment equation:*

$$m_n(\hat{\theta};\hat{f}) = 0 \qquad\qquad m_n(\theta;f) := \mathbb{E}_n[\psi(Z;\theta,f) := \begin{bmatrix} \mathbb{E}_n\left[\psi_0(Z;\theta,f)\right] \\ \mathbb{E}_n\left[\psi_2(Z;\theta,f)\right] \\ \dots \\ \mathbb{E}_n\left[\psi_M(Z;\theta,f)\right] \end{bmatrix} \tag{16}$$

*If* $\|\hat{r} - r_0\|_{2,o} = o_p(n^{-1/4})$*, for* $r \in \{\{h_{\tau,t}, p_{\tau,t}\}_{2\le\tau\le t\le M}\}$ *and if* $\|\hat{h} - h_0\|_e = o_p(n^{-1/4})$*,* $\|\hat{p} - p_0\|_e = o_p(n^{-1/4})$ *and* $\|g_0 - \hat{g}\|_{2,o} \cdot \|q_0 - \hat{q}\|_{2,o} = o_p(n^{-1/2})$*, then:*

$$\sqrt{n}\left(\hat{\theta}_0 - \theta_0\right) \to_d N(0, J_e^{-1}\left(\Sigma_e + \Sigma_{o,1} + \Sigma_{o,2}\right) J_e^{-1})$$

*where* $J_e, \Sigma_e, \Sigma_{o,1}, \Sigma_{o,2}$ *are defined in the appendix.*

# 4 Semi-Synthetic Experimental Evaluation

We evaluate the performance of our proposed estimation strategy on a semi-synthetic dataset. The semi-synthetic data retain qualitative characteristics of data on real-world incentive investments in customers at a major corporation, although all data series and relationships have been perturbed to retain confidentiality.

The semi-synthetic dataset, like the real-world dataset on which it is based, displays several patterns that are common across many potential applications. The treatments, in this case incentive investments, are lumpy: in most periods most customers get no investments. Proxies, which include single period values of the outcome of interest, are highly auto-correlated over time. Treatments are also auto-correlated, and correlated with past values of proxies. Finally, we include a set of time-invariant controls that affect both proxies/outcomes and treatments.

To build the semi-synthetic data we estimate a series of moments from a real-world dataset: a full covariance matrix of all proxies, treatments, and controls in one period and a series of linear prediction models (lassoCV) of each proxy and treatment on a set of 6 lags of each treatment, 6 lags of each proxy, and time-invariant controls. Using these values, we draw new parameters from distributions matching the key characteristics of each family of parameters. Finally, we use these new parameters to simulate proxies, treatments, and controls by drawing a set of initial values from the covariance matrix and forward simulating to match intertemporal relationships from the transformed prediction models. For further details on the data generation process, see the appendix.

We compare multiple possible approaches for estimating the effects of our three synthetic treatments on a long-term outcome. To construct this outcome we select one proxy to be the outcome of interest. We consider the effect of each treatment in period $t$ on the cumulative sum of the outcome from period $t$ to $t+3$, four periods, or $t$ to $t+7$, eight periods. We can calculate the true treatment effects in the synthetic data as a function of parameters from the linear prediction models.

Because we construct a single, long synthetic dataset for this exercise it is possible to estimate the treatment effects on realized long-term outcomes directly, unlike the typical use case for a surrogate

approach. We estimate the effect of each treatment at time $t$ on outcomes over the next 4 or 8 periods using double machine learning and controlling for invariant customer characteristics and *contemporaneous and lagged* values of all proxies and other treatments. The blue, "total" bars in each panel of Figure 2 show the distribution of the $\ell_2$ estimation error ($\|\hat{\theta} - \theta_0\|_2$) in the estimated treatment effects from this method across 100 simulated datasets. The top row plots the estimation error when estimating the effect on four periods of outcomes, increasing the sample size of each simulation from left to right, while the bottom row shows the same for the effect on eight periods of outcome. As predicted, the auto-correlation in treatments causes this method, which does not control for *future* treatments, to substantially overestimate treatment effects relative to their true values.

We then estimate the same set of treatment effects using the unadjusted surrogate approach described in Section 3.1. The distribution of estimation errors from this approach is represented in the orange "surrogate" bars in each panel of Figure 2. Since this approach still fails to control for future treatments when estimating the surrogate index, the estimated treatment effects are still substantially larger than the true effects on average. Note that the surrogate model exhibits slightly less bias than the direct "total" approach. Intuitively, because the surrogate approach is only capturing the relationship between treatment and outcome that passes through the surrogates it picks up less of the bias resulting from future correlated treatments than the direct approach.

The third set of green "adj. total" bars plot the distribution of estimation errors when estimating treatment effects on adjusted realized outcomes using the method of Lewis & Syrgkanis (2020). When a dataset containing both all treatments of interest and realized long-run outcomes is available, this should be the preferred approach. This third methodology, which removes the effects of future treatments from the long-run outcome in a first step, exhibits significantly less bias than the first two methods, particularly for reasonably large samples in the right two columns.

The final two bars in each panel of Figure 2 illustrate the success of the adjusted surrogate approach described in Section 3.2. We recommend this approach in the case when treatments are serially-correlated, as in the synthetic data, and it is not possible to collect a single dataset that contains both long-term outcomes and all the treatments of interest. As illustrated by the red "adj. surrogate" bars, this adjusted surrogate approach is highly accurate in predicting long-term effects with a performance comparable to that of having access to the raw long-term outcome itself. The final purple "new treat." bars show that the approach works equally well when considering the effect of a novel treatment that appears only in the experimental sample and was not part of the dynamic adjustment. Overall, this methodology overcomes a common data limitation when considering long-term effects of novel treatments and expands the surrogate approach to consider a common, and previously problematic, pattern of serially correlated treatments.

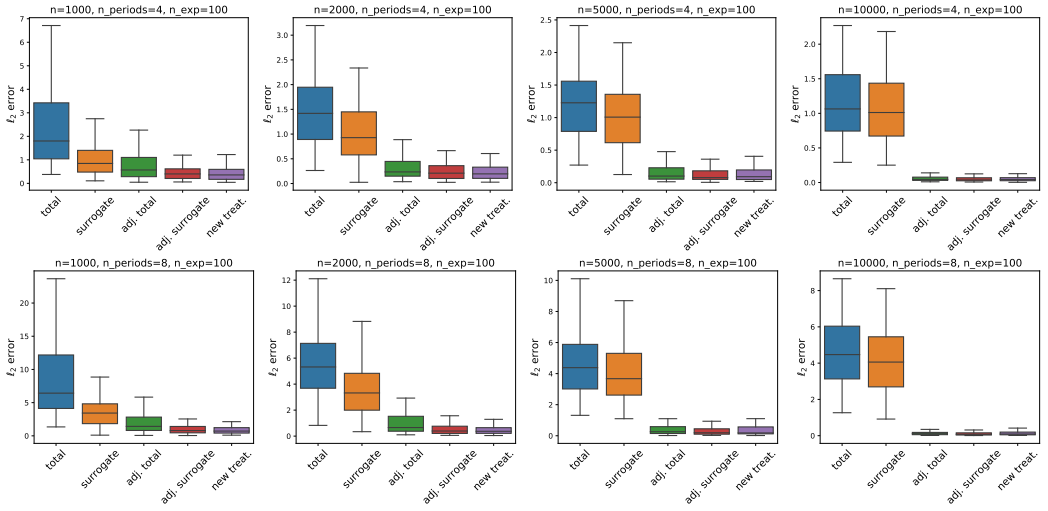


Figure 2: Experimental performance for $M = 4$ periods and $M = 8$ periods.

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## Checklist

1. For all authors...
   (a) Do the main claims made in the abstract and introduction accurately reflect the paper's contributions and scope? [Yes]
   (b) Did you describe the limitations of your work? [Yes]
   (c) Did you discuss any potential negative societal impacts of your work? [Yes]
   (d) Have you read the ethics review guidelines and ensured that your paper conforms to them? [Yes]
2. If you are including theoretical results...
   (a) Did you state the full set of assumptions of all theoretical results? [Yes] Some proofs and assumptions are detailed in the Appendix, to be submitted.
   (b) Did you include complete proofs of all theoretical results? [Yes] Some proofs and assumptions are detailed in the Appendix, to be submitted.
3. If you ran experiments...
   (a) Did you include the code, data, and instructions needed to reproduce the main experimental results (either in the supplemental material or as a URL)? [N/A]
   (b) Did you specify all the training details (e.g., data splits, hyperparameters, how they were chosen)? [N/A]
   (c) Did you report error bars (e.g., with respect to the random seed after running experiments multiple times)? [N/A]
   (d) Did you include the total amount of compute and the type of resources used (e.g., type of GPUs, internal cluster, or cloud provider)? [N/A]
4. If you are using existing assets (e.g., code, data, models) or curating/releasing new assets...
   (a) If your work uses existing assets, did you cite the creators? [Yes] No real world data or existing proprietry code or models was used. Real world data were only used to derive intuition and summary statistics of the synthetic generation of fake data as is discussed in the experimental section
   (b) Did you mention the license of the assets? [N/A]
   (c) Did you include any new assets either in the supplemental material or as a URL? [Yes] Code in supplemental material

(d) Did you discuss whether and how consent was obtained from people whose data you're using/curating? [N/A]

(e) Did you discuss whether the data you are using/curating contains personally identifiable information or offensive content? [N/A]

5. If you used crowdsourcing or conducted research with human subjects...

(a) Did you include the full text of instructions given to participants and screenshots, if applicable? [N/A]

(b) Did you describe any potential participant risks, with links to Institutional Review Board (IRB) approvals, if applicable? [N/A]

(c) Did you include the estimated hourly wage paid to participants and the total amount spent on participant compensation? [N/A]