Bias-Variance Tradeoffs for Designing Simultaneous Temporal Experiments

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Abstract

We study the analysis and design of simultaneous temporal experiments, where a set of interventions are applied concurrently in continuous time, and outcomes are measured on a sequence of events observed in time. As a motivating setting, suppose multiple data science teams are conducting experiments simultaneously and independently on a ride-hailing platform to test changes to marketplace algorithms such as pricing and matching, and estimating effects from observed event outcomes such as the rate at which ride requests are completed. The design problem involves partitioning a continuous space of time into intervals and assigning treatments at the interval level. Design and analysis must account for three factors: carryover effects from interventions at earlier times, correlation in event outcomes, and effects of interventions tested simultaneously. We provide simulations to build intuition and guidance for practitioners.

Keywords: Carryover Effects, Simultaneous Intervention, Temporal Experiment

1 Introduction

In many empirical settings, it is useful to estimate the effects of interventions via time-based or *temporal* experimental designs rather than (the far more common) cross-sectional designs. Most prominently, heuristic designs colloquially known as "switchbacks" have become popular due to their applications in digital marketplaces. In these modern settings, the interference structure between units is difficult to account for and can cause bias of unknown signs and large magnitude using more traditional approaches. Prior to more recent applications, there is a long history in medicine of designing an experiment using a single unit of observation and leveraging longitudinal observations in medicine where it is known as an "n-of-1" trial (Mirza et al., 2017).

As motivation for the present work, we consider the problem of designing multiple simultaneous temporal experiments, for instance, in a ride-hailing company where multiple teams would like to measure the effects of their product changes with only a small number of available treatment units (e.g., cities or regions). In a dynamic two-sided marketplace, users exposed to new pricing and matching algorithms may change their behavior in ways that affect outcomes for other users on either side of the marketplace. There are a variety of

causal mechanisms for these spillovers, such as riders consuming available drivers, relocating drivers, or stimulating drivers to drive for longer (Chamandy, 2016).

Given the importance of digital marketplaces and the well-acknowledged need to rapidly test new ideas, the design of experiments that provide reliable estimates in the presence of marketplace-mediated interference has drawn increasing attention in recent studies (Holtz et al., 2020; Li et al., 2021; Basse and Feller, 2018; Jagadeesan et al., 2020; Johari et al., 2022). A common theme of these approaches is exploiting prior knowledge of the spillover mechanisms, and leveraging this structure to provide alternative analysis procedures or designs.

We study the design of experiments in a highly generic setting where interventions are applied in a continuous temporal space, and outcomes are measured on a sequence of events in this space. Good designs in this setting efficiently partition continuous temporal space into intervals with alternating treatments in anticipation of precisely estimating a quantity we call the global average treatment effects (GATE) of interventions from the observed event outcomes. GATE is an important estimand for decision-making that captures the difference in average outcomes when an intervention is deployed indefinitely (global treatment) versus when the intervention is absent indefinitely (global control).

Our goal is to capture realistic properties of this generic empirical setting that complicate the design and analysis of temporal experiments. First, we account for carryover effects between treatments and the outcomes of future events. Second, we account for correlation in event outcomes from unobserved (or unmodeled) factors that create nuisance dependence among measurements; outcomes of events close in time can be similar due to weather, traffic, or other external factors. Correlations do not have to be monotonic in the distance between events, as they can display periodic behavior in weekly or daily cycles. Third, we account for the irregular density of observed events, corresponding to the property that there is strong periodicity in interactions with marketplaces. Finally, we consider the presence of simultaneous experiments run by other teams on the same sequence of events, which can confound effect estimates in finite samples.

We provide a decomposition of the mean-squared error of the estimated GATE from any design under this generic setting, and show the tradeoff of various sources of bias and variance. We further conduct a simulation study that explores the role of assumptions about carryovers, outcome covariance, and event density in affecting the MSE of heuristic designs. Practitioners can use similar simulations with assumptions tailored to their specific design problem in order to design efficient experiments in their empirical settings.

1.1 Related Work

Our work is closely connected to several related literature in the experimental design space. First, there has been extensive work on the design of experiments in temporal or time-series settings, the distinguishing property of which is that outcomes are subject to carryover effects from treatments of prior time periods. As discussed above, the most common tool is the switchback design (Bojinov et al., 2020; Ni et al., 2023), in which predetermined time intervals are randomly and sequentially exposed to treatment and control variants. Alternative approaches include pulse designs (Basse and Feller, 2018) where units are treated only for one time period, or designs with irreversible treatment adoption patterns that

are based on generalized least squares (Xiong et al., 2023) or synthetic control estimators (Doudchenko et al., 2019, 2021; Abadie and Zhao, 2021).

Designing and analyzing experiments in the presence of interference has been studied in broad settings beyond temporal data. Johari et al. (2022) study how demand-randomized and supply-randomized designs can contribute different types of bias in a manner that is dependent on market balance. On network data, one common method for mitigating interference is through cluster-randomized designs (Ugander et al., 2013; Eckles et al., 2017; Candogan et al., 2021), where the clusters are chosen to minimize edges that cut across clusters. The cluster size serves an analogous role as the interval length in temporal data, governing the tradeoff between interference bias and estimation variance. Another popular method to mitigate interference is to use two-stage or multi-stage randomization, which has been used in public health (Hudgens and Halloran, 2008; Liu and Hudgens, 2014), political science (Sinclair et al., 2012), and social science (Crépon et al., 2013; Baird et al., 2018; Basse and Feller, 2018). In the spatial setting, a common approach is to conduct experiments at an aggregate level (Ni et al., 2023) or to randomly assign treatments to a set of predetermined spatial intervention points, with a focus on estimating spatial spillover effects (Aronow et al., 2020, 2021). Our general approach to the temporal problem suggests that some of these ideas may be useful here as well.

2 Setting

Suppose K decision makers are simultaneously running experiments on the same time interval. For example, each decision-maker could be on a different team within the same company. Let $T \in \mathbb{R}$ be the experiment duration. The ℓ-th decision maker runs an experiment to study the effect of intervention ℓ, for $\ell \in [K]$, where $[K] = \{1, \cdots, K\}$. For example, the interventions could be pricing, matching, or routing algorithms that are all being tested within the same marketplace in a single region or city. We assume the K interventions are different from one another, and K is finite.

For each intervention $\ell \in [K]$, let $w_{\ell,t} \in \{0, 1\}$ be the treatment status at time $t \in [0, T]$, where $w_{\ell,t} = 1$ indicates that the marketplace is exposed to intervention ℓ (treatment) at time t, and $w_{\ell,t} = 0$ indicates otherwise (control).

Each decision maker ℓ chooses the treatment design of intervention ℓ for the whole experiment horizon, i.e., $\boldsymbol{W}_\ell = \{W_{\ell,t}, \forall t \in [0, T]\}$, pre-experiment. The treatment decisions of all the interventions are made simultaneously. As the treatment decisions are made in a continuous time interval, the decision maker first partitions experimental horizon $[0, T]$ into M disjoint intervals and then randomizes the treatment assignment of each interval. For intervention ℓ, let $0 \leq t_{\ell 0} \leq t_{\ell 1} \leq \cdots \leq t_{\ell,M-1} \leq t_{\ell M} = T$ be the endpoints that define the M intervals, $\mathcal{I}_{\ell m} = [t_{\ell,m-1}, t_{\ell,m}]$ be the m-th interval, and $|\mathcal{I}_{\ell m}| = t_{\ell m} - t_{\ell,m-1}$ be the length of the m-th interval. For any two interventions, the intervals of one intervention may overlap but not be identical to the intervals of another intervention.[1]

1. Without loss of generality, assume M is the same for all interventions by allowing the interval length to have measure zero.

As the treatment decisions are made at the interval level, the treatment assignments are the same for all times within an interval, i.e.,

$$w_{\ell,t} = w_{\ell,t'}, \qquad \text{for all } t, t' \in \mathcal{I}_{\ell m}, \ \text{ for all } m \text{ and all } \ell\,.$$

Special cases of these treatment designs include the commonly used fixed duration switchbacks where intervals are of equal size and treatment assignments are randomized. Our setup allows for more general designs with varying interval lengths.

The raw data available for analyzing the effect of each intervention are at the event level, where each event could be a rider opening the app and checking the price. Suppose there are n events occurring in the marketplace between time 0 and time T. The outcomes of these n events are available to all decision-makers. Let $Y^{(i)}$ be the outcome of event i that occurred at time t_i, where we assume the occurred time t_i is a random variable. For example, $Y^{(i)}$ could be a binary variable indicating whether the rider requests a ride or not. Let $f(t) : [0, T] \to \mathbb{R}^+$ be the density function from which events are sampled. For example, if events are equally likely to occur at any time in the experiment, then $f(t) = 1/T$ for all $t \in [0, T]$. We assume that $f(t)$ is bounded from below and from above for all t.

We additionally define the marketplace outcome at time t as Y_t. The marketplace outcome Y_t can be viewed as the average outcome of all users in the marketplace, such as the average request rate at time t. The event outcomes are noisy measurements of the marketplace outcomes, i.e., for all i,

$$Y^{(i)} = Y_{t_i} + \varepsilon^{(i)}\,,$$

where the measurement error $\varepsilon^{(i)}$ has mean zero and bounded variance. When the binary $Y^{(i)}$ indicates whether rider i requests a ride, we think of $Y^{(i)}$ as a random draw from the Bernoulli distribution with probability $\mathbf{P}(Y^{(i)} = 1) = Y_{t_i}$ of being 1. We allow measurement errors of events that are close in time to be correlated:

$$\mathrm{Cov}[\varepsilon^{(i)}, \varepsilon^{(j)}] \neq 0 \qquad \text{for } \ t_i \neq t_j\,.$$

The correlation can be caused by external factors like weather, supply conditions, and traffic. This correlation creates a nuisance dependence between event outcomes, affecting the variance of treatment effect estimates.

Furthermore, we define the potential outcomes of the marketplace at time t as

$$Y_t(\boldsymbol{w}_1, \boldsymbol{w}_2, \cdots, \boldsymbol{w}_K)\,,$$

where $\boldsymbol{w}_\ell = \{w_{\ell,t},\, \forall t \in [0, T]\}$ is a realization of $\boldsymbol{W}_\ell$.[2] The noisily measured marketplace outcome satisfies $Y_t = Y_t(\boldsymbol{W}_1, \cdots, \boldsymbol{W}_K)$. Conditional on treatment designs $\boldsymbol{W}_1, \cdots, \boldsymbol{W}_K$ and event occurrence time t_i, there is no randomness in Y_t anymore, and the randomness in $Y^{(i)}$ purely comes from the measurement error $\varepsilon^{(i)}$.

Note that the definition above generalizes the standard, binary definition of potential outcomes under the stable unit treatment value assumption (SUTVA) in two aspects. First,

2. Note that intervention ℓ is not applied to times outside of the experiment duration, i.e., $w_{\ell,t}$ is always 0 for $t \notin [0, T]$. Therefore, there are no carryover effects of intervention ℓ from times outside of the experiment duration, $\mathbb{R}\backslash[0, T]$, to the experiment duration, $[0, T]$. It is then reasonable to define potential outcomes only by $\boldsymbol{w}_\ell = \{w_{\ell,t} : \forall t \in [0, T]\}$.

this definition allows potential outcomes to be jointly affected by K interventions. Second, this definition allows for the existence of carryover effects: the potential outcome of t is not only affected by the treatment status at t but also the treatment statuses at other times.

Post-experiment, each decision maker ℓ use observed event outcomes $\{Y^{(i)}\}_{i\in[n]}$ and treatment assignments $\boldsymbol{W}_\ell$ to estimate the effect of intervention ℓ.

2.1 Estimands

Our main object of interest is the *global average treatment effect* (GATE), which measures the difference in average outcomes over time when an intervention is deployed indefinitely (global treatment) versus when an intervention is absent (global control). We formally define the GATE of intervention ℓ as

$$\delta_\ell^{\text{gate}} = \int \delta_{\ell,t}^{\text{gate}} f(t) dt\,,$$

which is the average of total treatment effect $\delta_{\ell,t}^{\text{gate}}$ at time t weighted by the event density $f(t)$. The total treatment effect $\delta_{\ell,t}^{\text{gate}}$ at time t is defined as

$$\delta_{\ell,t}^{\text{gate}} = Y_t(\mathbf{0}, \cdots, \underbrace{\mathbf{1}}_{\boldsymbol{W}_\ell}, \cdots, \mathbf{0}) - Y_t(\mathbf{0}, \cdots, \underbrace{\mathbf{0}}_{\boldsymbol{W}_\ell}, \cdots, \mathbf{0})\,,$$

where $\mathbf{1} = \{w_{\ell,t} = 1,\, \forall t \in [0,T]\}$ and $\mathbf{0} = \{w_{\ell,t} = 0,\, \forall t \in [0,T]\}$ denote global treatment and global control of intervention ℓ, respectively.

We additionally define the average instantaneous and carryover effects, which are building blocks of GATE. The average instantaneous effect $\delta_\ell^{\text{inst}}$ is defined as

$$\delta_\ell^{\text{inst}} = \int \delta_{\ell,t}^{\text{inst}} f(t) dt\,,$$

where $\delta_{\ell,t}^{\text{inst}}$ is the instantaneous treatment effect at time t that is defined as

$$\delta_{\ell,t}^{\text{inst}} = Y_t(\mathbf{0}, \cdots, \underbrace{e_t}_{\boldsymbol{W}_\ell}, \cdots, \mathbf{0}) - Y_t(\mathbf{0}, \cdots, \underbrace{\mathbf{0}}_{\boldsymbol{W}_\ell}, \cdots, \mathbf{0})$$

and e_t is a one-hot-encoded vector with the entry of time t to be 1 and all the remaining entries to be 0

$$e_t = \big(0 \;\cdots\; 0 \;\underbrace{1}_{\text{time } t}\; 0 \;\cdots\; 0\big)\,.$$

The average carryover effect $\delta_\ell^{\text{co}}(\boldsymbol{w}_\ell)$, given treatment assignments $\boldsymbol{w}_\ell$, is defined as

$$\delta_\ell^{\text{co}}(\boldsymbol{w}_\ell) = \int \delta_{\ell,t}^{\text{co}}(\boldsymbol{w}_\ell) f(t) dt\,,$$

where $\delta_{\ell,t}^{\text{co}}(\boldsymbol{w}_\ell)$ is the carryover effect at time t that is defined as

$$\delta_{\ell,t}^{\text{co}}(\boldsymbol{w}_\ell) = Y_t(\mathbf{0}, \cdots, \boldsymbol{w}_\ell, \cdots, \mathbf{0}) - Y_t(\mathbf{0}, \cdots, \boldsymbol{w}_\ell \circ e_t, \cdots, \mathbf{0})$$

and "$\circ$" denotes the entry-wise product. Let $\delta_\ell^{\text{co}} \coloneqq \delta_\ell^{\text{co}}(\mathbf{1})$ be the average treatment effect under global treatment. Then we can decompose the GATE as

$$\delta_\ell^{\text{gate}} = \delta_\ell^{\text{inst}} + \delta_\ell^{\text{co}}\,.$$

2.2 Post-Experiment Estimation

Post-experiment, decision makers estimate the GATE using observed event outcomes and treatment designs and decide whether to deploy the intervention indefinitely. We propose to use the Horvitz-Thompson (HT) estimator for $\delta_\ell^{\text{gate}}$ (Horvitz and Thompson, 1952), and we analyze the statistical properties of the HT estimator in Section 3.

$$\hat{\delta}_\ell^{\text{gate}} = \frac{1}{n}\sum_i \left(\frac{W_{\ell,t_i}}{\pi_\ell} - \frac{1-W_{\ell,t_i}}{1-\pi_\ell}\right) Y^{(i)} = \frac{1}{n}\sum_i \alpha_{\ell,t_i} Y^{(i)}\,, \tag{1}$$

where $\alpha_{\ell,t_i} = \frac{W_{\ell,t_i}-\pi_\ell}{\pi_\ell(1-\pi_\ell)}$ is a normalized weight, and

$$\pi_\ell = \int_{t\in[0,T]} \mathbf{E}[W_{\ell,t}] f(t) dt$$

is the fraction of treated times under intervention ℓ.

We use the HT estimator for three reasons. First, it does not rely on an assumption about carryover mechanisms. Second, it does not rely on assumptions about how the outcomes are correlated in time. Third, it does not require the knowledge of treatment assignments of simultaneous interventions. Due to these three reasons, the HT estimator is flexible and broadly applicable to a wide range of settings in practice.

However, the flexibility of this estimator comes at a cost. First, the HT estimator could be biased due to the carryover effect of the same treatment at other times. The HT estimator approximates the outcomes under global treatment and global control by the event outcomes in treated intervals and control intervals, respectively. When the carryover effect is zero, i.e., $\delta_{\ell,t}^{\text{co}}(\boldsymbol{w}_\ell) = 0$, the approximation error is zero. For general cases, the approximation error is non-zero, and the HT estimator is biased. The bias scales with the size of the carryover effect. Second, the HT estimator can have a large variance as the event outcomes at different times are correlated and the HT estimator does not optimally weight observations that account for the correlation structure. Third, the HT estimator could have a confounding bias from simultaneous interventions when the treatment designs of two interventions are correlated in finite samples.

We note that the bias and variance of the HT estimator depend on the treatment design, as shown in Section 3 below. It is then possible to choose a better design to lower the estimation error of the HT estimator, and we formalize the decision problem in Section 2.3. In Section 4, we conduct a simulation study to show how the estimation error of heuristic designs varies with the assumptions on carryovers, outcome covariance, and event density, which can then be used to guide choosing a treatment design in practice.

2.3 Design of Temporal Experiments

Before the experiment starts, each decision maker ℓ chooses the treatment design of intervention ℓ, seeking that $\delta_\ell^{\text{gate}}$ can be estimated as precisely as possible, post-experiment. Formally, the decision maker ℓ chooses interval endpoints $t_{\ell m}$ for $m \in \{1, \cdots, M-1\}$, aiming to minimize the mean-squared error of $\hat{\delta}_\ell^{\text{gate}}$, i.e.,

$$\mathbf{E}_{W,\varepsilon,t}\left[(\hat{\delta}_\ell^{\text{gate}} - \delta_\ell^{\text{gate}})^2\right], \tag{2}$$

where the expectation is taken with respect to the treatment designs of all the interventions $\boldsymbol{W}_1, \cdots, \boldsymbol{W}_K$, the measurement errors in event outcomes $\varepsilon^{(1)}, \cdots, \varepsilon^{(n)}$, and the event occurrence times $t_1, \cdots, t_n$. Here we focus on the randomized designs, where each interval is equally likely to be treated or untreated, i.e., $\pi_\ell = \mathbf{P}(W_{\ell,t} = 1) = 1/2$ for all ℓ and t.

Later on, we provide the expression of MSE as a function of the interval endpoints, where the interval endpoints can be arbitrarily chosen. The MSE is a complex and non-convex function of the interval endpoints, so finding the global optimal solution to the optimization problem (2) is generally not feasible. Instead, in Section 4, we provide some general principles for choosing the endpoints, which can help reduce the objective function value of problem (2).

3 Analysis of Temporal Experiments

In this section, we provide the bias-variance decomposition of the MSE of $\hat{\delta}_\ell^{\text{gate}}$ from the HT estimator. The decomposition provides insights into how carryovers from interventions at earlier times, correlation in event outcomes, and effects of simultaneous interventions affect the MSE of $\hat{\delta}_\ell^{\text{gate}}$. The insights can then be used as guidance to optimize $\{\boldsymbol{W}_\ell\}_{\ell \in [K]}$ in practice.

3.1 Interval-Level Statistics

We first introduce several interval-level statistics that quantify carryover effects, correlation in measurement errors, confounding effects from simultaneous interventions, and other components at the interval level. These interval-level statistics are building blocks of the mean-squared error decomposition in Section 3.2, and are important quantities to be considered in the partition of intervals.

Fraction of events. Let

$$\mu_\ell^{(m)} = \int_{t_i \in \mathcal{I}_{\ell m}} f(t_i) dt_i$$

be the fraction of events occurring in the interval $\mathcal{I}_{\ell m}$. $\mu_\ell^{(m)}$ ranges from 0 to 1 and the sum of $\mu_\ell^{(m)}$ over m equals to 1.

Mean control outcome. Let

$$\mu_{\ell, Y^{\text{ctrl}}}^{(m)} = \int_{t_i \in \mathcal{I}_{\ell m}} Y_{t_i}(\mathbf{0}, \cdots, \mathbf{0}) f(t_i) dt_i$$

be the integrated global control outcome $Y_{t_i}(\mathbf{0}, \cdots, \mathbf{0})$ over times t_i in the interval $\mathcal{I}_{\ell m}$.

Variance and covariance of measurement errors. Let the variance of the measurement error of event i occurred at time t_i be (measurement error has mean zero)

$$\text{Var}_{\sigma, t_i} = \mathbf{E}_\varepsilon \left[(\varepsilon^{(i)})^2 \mid t_i \right]$$

and let the corresponding integrated variance of any event occurred in the interval $\mathcal{I}_{\ell m}$ be

$$V_\ell^{(m)} = \int_{t_i \in \mathcal{I}_{\ell m}} \mathrm{Var}_{\sigma, t_i} f(t_i) dt_i \,.$$

Furthermore, let the covariance between the measurement errors of event i and j occurred at time t_i and t_j be

$$\mathrm{Cov}_{\sigma, t_i, t_j} = \mathbf{E}_\varepsilon \left[\varepsilon^{(i)} \varepsilon^{(j)} \mid t_i, t_j \right]$$

and let the corresponding integrated covariance between measurement errors of any two events that occurred both in the interval $\mathcal{I}_{\ell m}$ be

$$C_\ell^{(m)} = \int_{t_i, t_j \in \mathcal{I}_{\ell m}} \mathrm{Cov}_{\sigma, t_i, t_j} f(t_i) f(t_j) dt_i dt_j \,.$$

When the measurement errors of events at different times are correlated, $C_\ell^{(m)}$ is generally nonzero. In practical settings, there are often some patterns of how the covariance $\mathrm{Cov}_{\sigma, t_i, t_j}$ varies with t_i and t_j, e.g., decays monotonically or periodically in the distance between t_i and t_j. Therefore, we can use a kernel function to parameterize and capture the patterns inherited in $\mathrm{Cov}_{\sigma, t_i, t_j}$. See two examples in Figure 1.

Integrated total treatment effects. Let

$$\Xi_\ell^{(m)} = \int_{t_i \in \mathcal{I}_{\ell m}} \delta_{\ell, t_i}^{\mathrm{gate}} f(t_i) dt_i$$

be the integrated total treatment effect $\delta_{\ell, t_i}^{\mathrm{gate}}$ over times t_i in the interval $\mathcal{I}_{\ell m}$. Following the definition of $\delta_\ell^{\mathrm{gate}}$, the sum of $\Xi_\ell^{(m)}$ over m equals to $\delta_\ell^{\mathrm{gate}}$. Moreover, if treatment effects $\delta_{\ell, t}^{\mathrm{gate}}$ are constant in t, then $\Xi_\ell^{(m)} = \delta_\ell^{\mathrm{gate}} \mu_\ell^{(m)}$ for any m.

Carryover effects. We assume that for every t, the carryover effect can be written as

$$\delta_{\ell, t}^{\mathrm{co}}(\boldsymbol{w}_\ell) = \delta_{\ell, t}^{\mathrm{co}} \cdot \int w_{\ell, t'} \cdot d_{\ell, t}^{\mathrm{co}}(t') f(t') dt' \,,$$

where $d_{\ell, t}^{\mathrm{co}}(t')$ is a carryover kernel that measures the intensity of the effect of intervention ℓ at time t' on the outcome at time t and satisfies $\int d_{\ell, t}^{\mathrm{co}}(t') f(t') dt' = 1$. Then let

$$I_\ell^{(m,k)} = \int_{t_i \in \mathcal{I}_{\ell m}, t' \in \mathcal{I}_{\ell k}} \delta_{\ell, t_i}^{\mathrm{co}} d_{\ell, t_i}^{\mathrm{co}}(t') f(t_i) f(t') dt_i dt'$$

be integrated carryover effect of treatments at times in the interval $\mathcal{I}_{\ell k}$ on outcomes at times in the interval $\mathcal{I}_{\ell m}$. For notation simplicity, we let $I_\ell^{(m)} = I_\ell^{(m,m)}$ be the integrated carryover effect of treatments on outcomes in the same interval. The integrated carryover effect $I_\ell^{(m,k)}$ increases with the length of both $\mathcal{I}_{\ell m}$ and $\mathcal{I}_{\ell k}$, and increases with the size of carryover effect $\delta_{\ell, t}^{\mathrm{co}}$ for $t \in \mathcal{I}_{\ell m}$. The sum of $I_\ell^{(m,k)}$ over both m and k, which is the integrated carryover effect of the treatment of all intervals on the outcomes of all intervals, is equal to the average carryover effect $\delta_\ell^{\mathrm{co}}$. Moreover, if the carryover effect $\delta_{\ell, t}^{\mathrm{co}}$ is constant in

t, then the sum of $I_\ell^{(m,k)}$ over k, which is the integrated carryover effect of the treatment of all intervals on the outcomes in the interval $\mathcal{I}_{\ell m}$, is equal to $\delta_\ell^{\text{co}}\mu_\ell^{(m)}$. Therefore, we can view $I_\ell^{(m,k)}$ as a useful building block of δ_ℓ^{co}.

Confounding effects from simultaneous interventions. For intervention ℓ, let

$$S_\ell^{(m)} = \int_{t_i \in \mathcal{I}_{\ell m}} \left[\sum_{\ell' : \ell' \neq \ell} \delta_{\ell', t_i}^{\text{gate}} \right] f(t_i) dt_i$$

be the confounding effects of all the simultaneous interventions on outcomes at times t_i in the interval $\mathcal{I}_{\ell m}$. Furthermore, let

$$S_{\ell,\ell'}^{(m,m')} = \int_{t_i \in \mathcal{I}_{\ell m} \cap \mathcal{I}_{\ell' m'}} \delta_{\ell', t_i}^{\text{inst}} f(t_i) dt_i + \int_{t_j \in \mathcal{I}_{\ell m}, t' \in \mathcal{I}_{\ell' m'}} \delta_{\ell', t_j}^{\text{co}} d_{\ell', t_j}^{\text{co}}(t') f(t') f(t_j) dt_j dt'$$

be the confounding effect of employing intervention ℓ' in the interval $\mathcal{I}_{\ell' m'}$ on outcomes at times t_i in the interval $\mathcal{I}_{\ell m}$. The confounding effect consists of instantaneous and carryover confounding effects. The instantaneous confounding effect only comes from employing intervention ℓ' in the overlapping interval $\mathcal{I}_{\ell m} \cap \mathcal{I}_{\ell' m'}$, while the carryover confounding effect comes from employing intervention ℓ' in the full interval $\mathcal{I}_{\ell' m'}$. Note that if $\mathcal{I}_{\ell m}$ does not overlap with $\mathcal{I}_{\ell' m'}$, then the instantaneous confounding effect is zero.

3.2 Decomposition of MSE

Using the interval-level statistics, the MSE of $\hat{\delta}_\ell^{\text{gate}}$ can be decomposed as follows.

$$\begin{aligned}\mathbf{E}_{W,\varepsilon,t}\left[\left(\hat{\delta}_\ell^{\text{gate}} - \delta_\ell^{\text{gate}}\right)^2\right] =& [\text{Bias}_\ell(\text{carryover})]^2 + \text{Var}_\ell(\text{meas}) + \text{Var}_\ell(\text{inst} + \text{carryover}) \\ &+ \text{Var}_\ell(\text{simul}) + 2\,\text{Cov}_\ell(\text{inst} + \text{carryover}, \text{simul})\,.\end{aligned}$$

The bias term $\text{Bias}_\ell(\text{carryover})$ in the decomposition equals to

$$\text{Bias}_\ell(\text{carryover}) = \sum_{m=1}^{M} I_\ell^{(m)} - \delta_\ell^{\text{co}}\,.$$

This term arises when we use direct treated and control outcomes to approximate globally treated and control outcomes, respectively, in the HT estimator. This term increases with M and switching less frequently reduces the bias from carryover effects.

There are three variance terms in the MSE decomposition of $\hat{\delta}_\ell^{\text{gate}}$. The first variance term $\text{Var}_\ell(\text{meas})$ equals to

$$\text{Var}_\ell(\text{meas}) = 4\sum_{m=1}^{M}\left(V_\ell^{(m)}/n + C_\ell^{(m)} \cdot (n-1)/n\right)\,.$$

This term measures how the event measurement errors affect the MSE of $\hat{\delta}_\ell^{\text{gate}}$. $\text{Var}_\ell(\text{meas})$ consists of two parts: the first part $V_\ell^{(m)}$ measures the variance of measurement error of

any event and the second part $C_\ell^{(m)}$ measures the covariance of measurement errors of any two events. As the number of events grows, the first part shrinks to zero and the second part dominates. We can show that $C_\ell^{(m)} = O(1/M^2)$ and $\mathrm{Var}_\ell(\text{error}) = O(1/M)$, implying that switching more frequently reduces the covariance of measurement errors.

The second variance term $\mathrm{Var}_\ell(\text{inst} + \text{carryover})$ equals to

$$\mathrm{Var}_\ell(\text{inst} + \text{carryover}) = \sum_{m=1}^{M} \left(\Xi_\ell^{(m)} + 2\mu_{\ell,Y^{\text{ctrl}}}^{(m)}\right)^2 + \sum_{m=1}^{M} \sum_{m' \neq m} \left(\left[I_\ell^{(m,m')}\right]^2 + I_\ell^{(m,m')} I_\ell^{(m',m)}\right).$$

This term comes from the estimation variance of both instantaneous and carryover effects. The estimation variance of instantaneous effect only contributes to the term $\left(\Xi_\ell^{(m)} + 2\mu_{\ell,Y^{\text{ctrl}}}^{(m)}\right)^2$, while the estimation variance of carryover effect contributes to all the terms. The estimation variance encompasses a trade-off in choosing the number of intervals M. On the one hand, increasing M can increase the variation in treatment assignments at different times, helping to reduce the value of $\sum_{m=1}^{M}\left(\Xi_\ell^{(m)} + 2\mu_{\ell,Y^{\text{ctrl}}}^{(m)}\right)^2$, which is at the order of $1/M$. On the other hand, increasing M tends to decrease the length of each interval and increase the carryover effects across intervals, hence increasing the value of $I_\ell^{(m,m')}$ for $m' \neq m$. Then increasing M increases the value of the other terms in $\mathrm{Var}_\ell(\text{inst} + \text{carryover})$. Therefore, the optimal value of M balances these two competing effects of M on the variance $\mathrm{Var}_\ell(\text{inst} + \text{carryover})$.

The third variance term $\mathrm{Var}_\ell(\text{simul})$ equals to

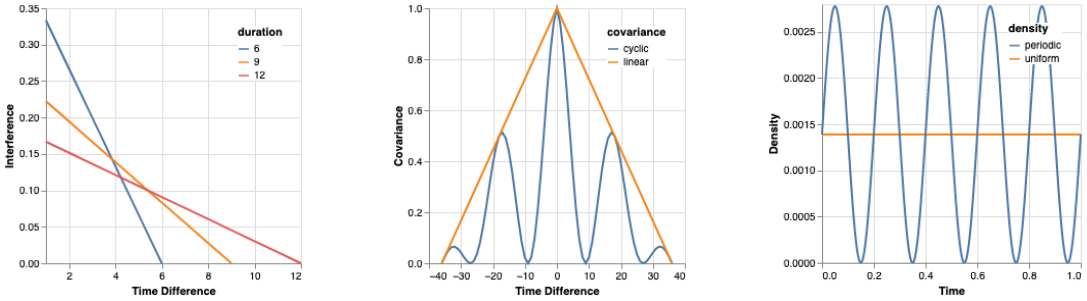
$$\mathrm{Var}_\ell(\text{simul}) = \sum_{m=1}^{M} \left[S_\ell^{(m)}\right]^2 + \sum_{m=1}^{M} \sum_{m'=1}^{M} \sum_{\ell':\ell' \neq \ell} \left[S_{\ell,\ell'}^{(m,m')}\right]^2.$$

This term comes from and increases with confounding effects from simultaneous interventions. As $S_\ell^{(m)}$ is at the order of $1/M$, the first part of $\mathrm{Var}_\ell(\text{simul})$ is at the order of $1/M$, which can be reduced by increasing M. In the second part of $\mathrm{Var}_\ell(\text{simul})$, the term $S_{\ell,\ell'}^{(m,m')}$ varies with how much $\mathcal{I}_{\ell m}$ overlaps with $\mathcal{I}_{\ell' m'}$. $S_{\ell,\ell'}^{(m,m')}$ is the largest when the interval endpoints of intervention ℓ and ℓ' are the same. Therefore, staggering the switching times of different interventions can help reduce the variance.

There is an additional covariance term $\mathrm{Cov}_\ell(\text{inst} + \text{carryover}, \text{simul})$ in the MSE decomposition, that equals to

$$\mathrm{Cov}_\ell(\text{inst} + \text{carryover}, \text{simul}) = \sum_{m=1}^{M} \left(\Xi_\ell^{(m)} + 2\mu_{\ell,Y^{\text{ctrl}}}^{(m)}\right) S_\ell^{(m)}.$$

This term measures the expected product of simultaneous effects and the sum of instantaneous and carryover effects. To reduce this covariance term, it is useful to increase M, following the same reason as how increasing M reduces $\mathrm{Var}_\ell(\text{inst} + \text{carryover})$ and $\mathrm{Var}_\ell(\text{simul})$.



(a) Carryover kernel (b) Covariance kernel (c) Event density

Figure 1: Simulation setup: carryover and covariance kernels, and event density. Time difference denotes $t'-t$ in the carryover kernel $d^{\text{co}}_{\ell,t}(t')$. If $t'-t<0$, then $d^{\text{co}}_{\ell,t}(t')=0$. The interpretation of time difference is analogous to the covariance kernel.

4 Simulation Study

In this section, we present estimates of mean-squared error of heuristic designs under a simulated problem structure in order to characterize the tradeoffs involved. Evaluating a design through simulation requires the following inputs:

- Carryover kernel $d^{\text{co}}_{\ell,t}(t')$: we use a linear decay kernel in all simulations.
- Covariance kernel $\mathbf{E}_{\varepsilon}\left[\varepsilon^{(i)}\varepsilon^{(j)} \mid t_i, t_j\right]$: we consider two regimes, a triangular kernel with height 1, and a periodic covariance kernel which is the product of a triangular kernel and cosine function capturing seasonal patterns.
- Event density $f(t)$: we consider two regimes, uniform density and periodic density $f(t) \propto \sin(\alpha t)$, where events are clustered in time according to a known seasonal pattern.

We restrict our evaluation to two heuristic designs:

- Fixed duration switchback with period p and offset q.
- Poisson switchback with mean period λ.

Figure 1 graphically depicts our design choices for the simulations. Additionally, we vary parameters governing the size of the instantaneous and carryover effects $\delta^{\text{inst}}_{\ell}$ and $\delta^{\text{co}}_{\ell}$, which affect bias from carryover effects. Since these parameters are arbitrary and must be assumed, we choose them such that the resulting bias is on the same scale as the variance.

4.1 Carryover Bias and Variance Tradeoffs

Figure 2 summarizes the most fundamental tradeoff of temporal experiments. Switching frequently generates more comparisons that reduce variance from measurement errors, but also increases carryover bias from previous intervals. When the carryover effect $\delta^{\text{co}}_{\ell}$ is small,

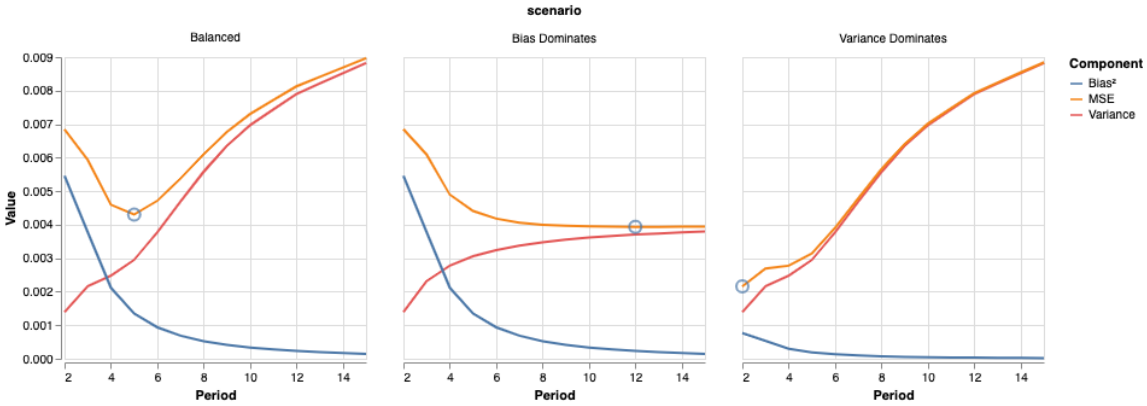


Figure 2: Tradeoffs under different regimes for a fixed duration switchback. The x-axis denotes period p in the fixed duration switchback with offset $q = 0$. The period p with the smallest MSE is circled in blue.

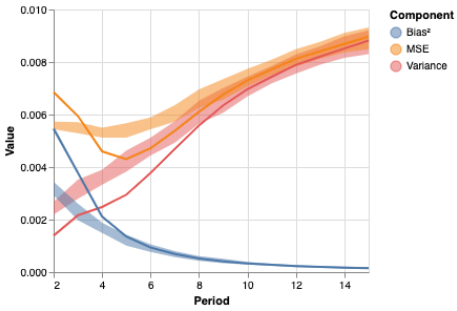
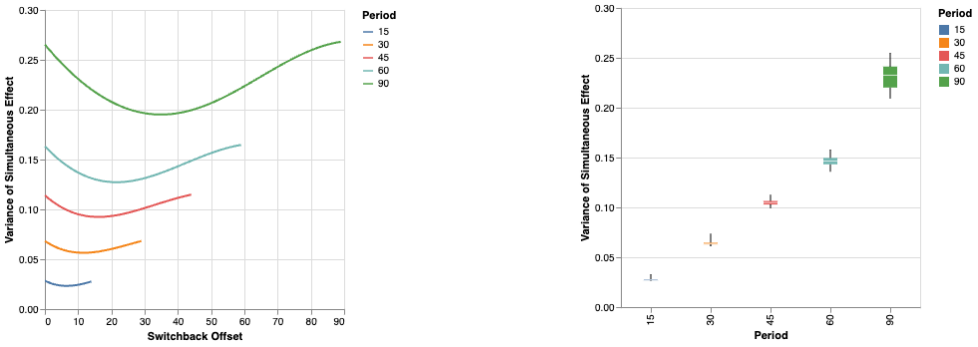


Figure 3: Poisson vs. fixed duration switchback. Solid lines denote fixed duration switchback. Shaded bands denote Poisson switchback. The x-axis denotes period p in the fixed duration switchback and λ in the Poisson switchback.

switching as quickly as possible results in the most efficient design, and when it is large, we improve the design by lengthening the switching period. We focus most of our ensuing discussion on settings where these two error components are on a similar scale and result in an interesting tradeoff.

4.2 Poisson versus Fixed Duration Designs

Figure 3 compares fixed duration designs with various periods to a distribution of errors resulting from different random draws of the stochastic policy. We find that the stochastic switchback generally results in designs with lower bias and increased variance for most values of λ. The randomization generates some longer periods between switching, which helps improve the estimator performance with respect to bias from interference.



(a) Fixed duration switchback

(b) Poisson switchback

Figure 4: Effects of simultaneous experiments. Two simultaneous experiments are run. In Figure 4a, period $p \in \{15, 30, 45, 60, 90\}$ is used in both designs with offset $q = 0$ in one design and varying offset q (x-axis in Figure 4a) in another design. In Figure 4b, we show distributions of variance produced using the Poisson switchback with $\lambda \in \{15, 30, 45, 60, 90\}$ for both designs. The Poisson switchback can be more effective unless the fixed duration designs are staggered well.

4.3 Simultaneous Experiments

In Figure 4, we show the estimation variance from simultaneous effects when two experiments are run simultaneously. When fixed duration switchbacks are used, Figure 4a shows that the estimation variance is affected by both the interval duration and offset in switching times between two experiments. Shortening the interval duration decreases the variance. Moreover, properly staggering two designs also decreases the variance, and the effect is more obvious when the interval duration is longer due to the increased finite-sample correlation between the two designs. Though not depicted, we note that the variance also increases with the number of simultaneous experiments. When Poisson switchbacks are used, Figure 4b shows how the mean period length affects the variance. The Poisson switchback can be more effective unless the fixed duration designs are staggered well.

4.4 Periodic Event Density

In many realistic settings, the density of events will exhibit periodic patterns due to the seasonality of human behavior. For instance, in ride-hailing, many ride requests occur during commute times, and relatively few occur during the late evening on weeknights. These daily and weekly cycles create opportunities for improving the design of temporal experiments and motivate simulations with a simple periodic density function. Figure 5 shows results from a periodic density using a fixed duration switchback. When the design has a period that aligns with density ($p \in \{6, 12\}$), the offset parameter q determines how the alignment alters the bias and variance. For $p = 12$, an offset of 3 (blue dots and lines) yields a design with the lowest variance by switching at an area of maximum density. This results in more events having natural "matches" in an adjacent interval. An offset of 10 (yellow dots and lines) minimizes bias by switching directly after a period with low event

density, which minimizes interference from the preceding interval. Knowledge of the density of events can improve the efficiency of the design by leveraging the best absolute times for bias- or variance-minimizing switching points.

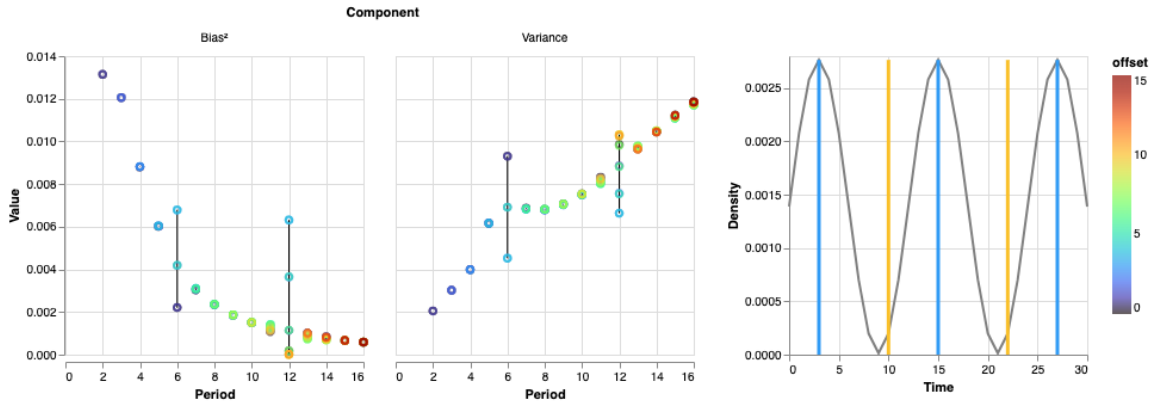


Figure 5: Bias and variance estimates for fixed duration switchback in a setting a density with a 12-period cycle. The color of points varies with offset parameter q. In all periods except 6 and 12, the offsets result in almost identical bias and variance.

4.5 Takeaways

Although our simulation results do not allow us to construct an optimal design directly, they point to the properties that better designs would tend to have and the fundamental constraints implied by the noise and causal structure of the setting.

First, as we learned in Section 3, the mean period of the design trades off variance by increasing correlation and bias by decreasing interference from previous periods. We can see that the MSE-minimizing period can vary substantially depending on assumptions about covariance (which are testable), the magnitude of effects, and carryover structure encoded by $d^{\text{co}}_{\ell,t}(t')$, $\delta^{\text{co}}_{\ell}$, and $\delta^{\text{inst}}_{\ell}$ (which must usually be assumed).

Second, stochastic designs exhibit lower carryover bias, but do incur some additional variance to achieve this. Randomization has the additional benefit of observing intervals with different lengths, which can help test if the treatment causes longer carryovers than were assumed in the design phase.

Third, simultaneous experiments are an important source of error under reasonable assumptions, which is quite a different regime than traditional A/B testing with user-level randomizations, which can generally support many simultaneous tests. In general, the throughput of multiple temporal experiments with substantive effects is something a centralized platform should manage to prevent a "tragedy of the commons" result. Ensuring that simultaneous experiments have designs that are uncorrelated in finite samples is likely to be valuable. It could be validated pre-experiment as proposed in Gupta et al. (2018) ("Seedfinder") or restricted randomizations (Simon, 1979).

Fourth, periodic behavior in both event density and covariance structure implies that there may be benefits and costs to cleverly choosing absolute switching times and periods

between switching. A more sophisticated search process could be applied to designing temporal experiments that could leverage estimates of density and the covariance kernel to provide better designs.

5 Conclusion

This paper studies the sources of error in the design and analysis of simultaneous temporal experiments. We provide an analysis of how the bias and variance of the Horvitz-Thompson estimator of the GATE are affected by three factors: carryovers from interventions at earlier times, correlation in event outcomes, and effects of interventions tested concurrently. We provide simulation examples that show how these three factors trade off each other and provide insights into how one can design efficient temporal experiments. Perhaps the most general conclusion we can draw is that designing experiments in this context involves considering a complex set of tradeoffs and critically depends on the assumptions experimenters would make using prior knowledge.

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