EXPERIMENTATION FOR HOMOGENEOUS POLICY CHANGE

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Molly Offer-Westort  Drew Dimmery

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ABSTRACT

When the Stable Unit Treatment Value Assumption (SUTVA) is violated and there is interference among units, there is not a uniquely defined Average Treatment Effect (ATE), and alternative estimands may be of interest, among them average unit-level differences in outcomes under different homogeneous treatment policies. We term this target the Homogeneous Assignment Average Treatment Effect (HAATE). We consider approaches to experimental design with multiple treatment conditions under partial interference and, given the estimand of interest, we show that difference-in-means estimators may perform better than correctly specified regression models in finite samples on root mean squared error (RMSE). With errors correlated at the cluster level, we demonstrate that two-stage randomization procedures with intra-cluster correlation of treatment strictly between zero and one may dominate one-stage randomization designs on the same metric. Simulations demonstrate performance of this approach; an application to online experiments at Facebook is discussed.

1 Introduction

When the Stable Unit Value Assumption (SUTVA) is violated, a unit’s potential outcomes are not only a function of the treatment assigned to them directly, but also of the treatments assigned to other units, that is, there is “interference” [Cox 1958]. In such settings, the ATE is not uniquely defined, as a unit’s treatment status may be associated with multiple potential outcomes under different treatment assignments of other units. Researchers may be interested in considering alternative design-specific estimands, including a generalization of the ATE under interference, the Expected Average Treatment Effect (EATE) [Sävje et al. 2017]; decomposition of treatment effects into direct and indirect effects as well as overall causal effects of the intervention [Halloran and Struchiner 1995; Hudgens and Halloran 2008; VanderWeele and Tchetgen 2011]; and related targets, such as the treatment effect on the uniquely treated, and spillover effects on the treated and non-treated as a function of treatment saturation [Baird et al. 2018]. Aronow and Samii [2017] develop a general approach for estimating causal effects in the presence of arbitrary but known interference.

Motivated by an analytic setting at Facebook, we consider average differences in unit-level outcomes under alternative interventions, where under counterfactuals of interest all units would be assigned the same treatment. For some research designs, isolating direct effects or discerning the form and magnitude of interference may be explicit objectives. Here, we are only interested in the average effects of alternative homogeneous policies, and the decomposition of these effects is not of specific interest. Our motivating application is in consideration of the design of experiments run to tune the performance of the video player on Facebook. The substance of these experiments is minor and technical (e.g. changing how much data is pre-buffered on a video) but vital to improving the overall Facebook experience. The goal of this experimentation is to launch a single configuration to all users and all videos which will provide the best user experience.

We consider experimental design in settings with partial interference, where units are organized into mutually exclusive clusters and there is no interference between units in different clusters [Sobel 2006]. Making one video on a user’s feed play more seamlessly may hurt the performance on another video, but it is implausible for a video’s performance to affect the performance of a video for another user. Estimation under partial interference with binary treatment has been considered by [Hudgens and Halloran 2008; Tchetgen and VanderWeele 2012, and Liu and Hudgens 2014] for two-stage randomization designs, where treatment saturation is randomized at the cluster level and treatment or control is randomly assigned to units within clusters according to the relevant saturation level. [Baird et al. 2018] discuss optimal experimental design in such settings.
Our contribution is to extend discussion of estimation and experimental design to settings where the researcher intends to estimate effects of several treatment levels. We present a linear-in-means model [Moffitt et al., 2001, Bramoullé et al., 2009, Chin, 2018] for a clustered multi-treatment experiment and propose methods to estimate the HAATE. We also discuss optimal randomization procedures in such settings. Often, the standard approach to experimental design is to use a one-stage randomization procedure, either assuming no interference and randomizing at the unit level, or allowing for interference and randomizing at the cluster level so that treatment assignment within clusters is homogeneous. Given the HAATE as the estimand of interest, we identify a bias-variance trade-off and show that root mean squared error (RMSE) under two-stage randomization procedures with intra-cluster correlation of treatment strictly between zero and one may dominate one-stage randomization procedures with randomization at either the unit or cluster level, under certain conditions. We provide a principled way to make this choice by selecting from a continuum of possible intra-cluster correlations of treatments (see Figure 1). We then demonstrate performance of this approach through simulations. We discuss an application to online experiments at Facebook.

We consider randomization procedures that may be classified according to intra-cluster correlation of treatment. Above is a stylized representation of proportion of each of three treatment conditions within clusters, conditional on intra-cluster correlation of treatment in an experimental design. The column to the left represents experiments with one-stage randomization at the cluster level, while the column to the right represents one-stage randomization at the unit level. In between, procedures may have intermediate intra-cluster correlation of treatment.

2 Setting

Consider an experiment over a set of of clusters, indexed by \( j = 1, \ldots, J \), each composed of \( n \) individual units.\(^1\) We assume i.i.d. draws from an infinite super-population of clusters. Let \( A_j = (A_{j,1}, \ldots, A_{j,n})^T \) be the cluster-level treatment assignment vector, where \( A_{j,i} \in \{0, \ldots, M\} \) determines which of \( M + 1 \) treatments a unit \( i \) in cluster \( j \) receives, with level zero being the control or status quo. Outcomes for units in cluster \( j \) take the form \( Y_{j,i} = Y_{j,i}(A_{j,i}, A_{j,-i}) \), where \( A_{j,-i} \) represents the vector \( A_j \) with the \( i^{th} \) element removed. Then, under partial interference, \( Y_{j,i}(a_{j,i}, a_{j,-i}, a_{j'}) = Y_{j,i}(a_{j,i}, a_{j,-i}, a_{j'}) \)

\(^1\)In exposition, we fix all \( n_j \) as equal, i.e., \( n_j = n \) for all \( j \) in \( 1, \ldots, J \).
We term the homogeneous assignment unit-level treatment effect as the unit-level treatment effect of being assigned \( A \) following the distribution determined by some experimental design to find the expected average treatment effect.

We are interested, however, in comparing counterfactual outcomes under two specific designs which each allow for only one vector of homogeneous treatment assignments. Consider for example, a setting where we want to change from the status quo, under which all units receive the same treatment, to some alternative policy, which will also be implemented homogeneously across all units. In this setting, we do not need to take the step of marginalizing over all treatment vectors, as each unit has a single potential outcome under each setting. We will term these homogeneous vectors \( \bar{m} \), homogeneous under treatment \( m \), as compared to \( \bar{0} \), homogeneous under the control.

We term the homogeneous assignment unit-level treatment effect as the unit-level treatment effect of being assigned \( m \) when all other units receive assignment \( m \), as compared to being assigned zero when all other units receive assignment zero.\(^2\) This is represented as,

\[
\Psi_{j,i}(\bar{m}, \bar{0}) = Y_{j,i}(A_{j,i} = m, A_{-i} = \bar{m}_{-i}) - Y_{j,i}(A_{j,i} = 0, A_{-i} = \bar{0}_{-i}).
\]

As shorthand, we may write,

\[
\Psi_{j,i}(\bar{m}, \bar{0}) = Y_{j,i}(\bar{m}) - Y_{j,i}(\bar{0}).
\]

Taking the expectation over the distribution of units, our inferential target is the Homogeneous Assignment Average Treatment Effect (HAATE), which is defined as,

\[
\Psi^{HAATE}(\bar{m}, \bar{0}) = E \left[ Y_{j,i}(\bar{m}) - Y_{j,i}(\bar{0}) \right].
\]

Note that we could average over units within clusters and then take the expectation over the distribution of clusters for the cluster version of the HAATE, but as clusters are of uniform size, the effect is equivalent. This estimand has been considered by others, including Basse and Airoldi [2018] and Chin [2018]. This effect is well-defined. However, without assumptions on interference it is not directly estimable, as all units will be under one condition or the other, and so we can find the mean under either condition only to the exclusion of the other. Under no interference, the treatment assignment vector for other units does not affect the potential outcome, and the HAATE, the EATE, and the ATE will all be equivalent.

Having assumed partial interference, however, it is the case that a unit’s potential outcomes are only defined by the assignments of treatment within their cluster, regardless of treatment assignments in other clusters. We can thus use homogeneous random assignment at the cluster level, such that each cluster has positive probability of being assigned \( \bar{m} \) or \( \bar{0} \), to identify the HAATE. Then, under partial interference, potential outcomes are only a function of the within-block treatment assignment vector, and under random assignment, treatment is independent of potential outcomes. Thus,

\[
\Psi^{HAATE}(\bar{m}, \bar{0}) = E \left[ Y_{j,i}(\bar{m}) - Y_{j,i}(\bar{0}) \right]
\]

by linearity of expectations,

\[
= E \left[ Y_{j,i}(\bar{m}) \right] - E \left[ Y_{j,i}(\bar{0}) \right]
\]

by partial interference,

\[
= E \left[ Y_{j,i}(\bar{m}j) \right] - E \left[ Y_{j,i}(\bar{0}j) \right]
\]

by independence of potential outcomes with treatment under random assignment, and consistency,

\[
= E \left[ Y_{j,i} | A_j = \bar{m}_j \right] - E \left[ Y_{j,i} | A_j = \bar{0}_j \right].
\]

This allows us to use the difference-in-means as an unbiased estimator of the HAATE,

\[
\hat{\Psi}^{HAATE}_{DM}(\bar{m}, \bar{0}) = \frac{\sum_{j=1}^{J} \sum_{i=1}^{n} Y_{j,i} | A_{j,i} = m} {\sum_{j=1}^{J} \sum_{i=1}^{n} A_{j,i} = m} - \frac{\sum_{j=1}^{J} \sum_{i=1}^{n} Y_{j,i} | A_{j,i} = 0} {\sum_{j=1}^{J} \sum_{j=1}^{n} A_{j,i} = 0}.
\]

\(^2\)This definition is flexible to fixing any treatment level as the control, and can be used for comparison of arbitrary treatment levels.
3 Linear-in-means model

We assume a simple linear-in-means model as the underlying data generating process \cite{Manski1993, Bramoullé2009}. In such models, a unit’s outcome is a linear function of the mean characteristics of their group, and possibly a set of the unit’s own characteristics. \cite{Chin2018} has developed such models to account for interference in Bernoulli randomized trials with covariates that are arbitrary functions of the treatment assignment vector; \cite{Baird2018} consider regression models for randomized treatment saturation designs with binary treatments and a fixed set of saturations determined by the researcher.

We follow in the spirit of \cite{Baird2018} and \cite{Chin2018}, with generalization to the multiple treatment setting. The model proposed by \cite{Baird2018} includes an intercept and separate indicators for directly treated units and untreated units in treated clusters at each assigned saturation level. They estimate slopes separately for treated and untreated units in treated clusters as the difference in coefficients on indicators at two different treatment saturation levels, divided by the difference in saturation levels. To allow for more flexible design selection in the multiple treatment setting, we will include slope coefficients directly in the model.

We assume that for all treatment vectors, the individual expected potential outcome is a function of direct treatment \(a_{j,i}\) received and the realized proportion of each of the treatment conditions within the cluster, \(p_{j[n]}(A_{j})\) \(= \frac{1}{n} \sum_{i=1}^{n} \{A_{j,i} = m\}\). That is,

\[
E[Y_{j,i}(A_{j})] = E[Y_{j,i}(a_{j,i}; p_{j[0]}(A_{j}), \ldots, p_{j[M]}(A_{j}))].
\]

Following \cite{Baird2018}, this is a modification of the stratified interference assumption \cite{Hudgens2008}, allowing realized potential outcomes to depend on the units that receive each treatment assignment, but constraining expected potential outcomes to simplify representation of standard errors without network knowledge.

We also follow \cite{Baird2018} in imposing homoskedasticity over potential outcomes, with intra-cluster correlation of errors such that for all treatment vectors, the variance covariance matrix is characterized by,

\[
\text{Var}[Y_{j,i}(a_{j})] = \sigma^2 + \tau^2,
\]

\[
\text{Cov}[Y_{j,i}(a_{j}), Y_{j,i'}(a_{j})] = \begin{cases} \tau^2 & \text{for } i \neq i', \\ 0 & \text{for } j \neq j'. \end{cases}
\]

Intra-cluster correlation of errors is then

\[
\rho_u = \frac{\tau^2}{\tau^2 + \sigma^2}.
\]

These two assumptions mirror \cite{Baird2018} Assumptions 1 and 2.

We propose a model,

\[
Y_{j,i} = \sum_{m=0}^{M} \beta_m \times \{A_{j,i} = m\} + \sum_{m=0}^{M} \sum_{\ell=1}^{M} \delta_{m,\ell} \times p_{j[\ell]} \times \{A_{j,i} = m\} + \varepsilon_{j,i}.
\]

The error term is the difference between the realized unit potential outcome as a function of the treatment vector, and the unit expected potential outcome for that treatment vector. Under the assumptions imposed above, \(E[\varepsilon_{j,i}|A_{j}] = 0\) \cite{Baird2018}, Lemma 1, and the OLS estimate is unbiased.

Relating the model to the potential outcomes framework, the expected outcome under homogeneous control, \(E[Y_{j,i}(\bar{0}_{j})]\), is represented by \(\beta_0\), and the expected outcome under homogeneous treatment \(m\), \(E[Y_{j,i}(\bar{m}_j)]\), is represented by \(\beta_m + \delta_{m,m}\). If the model is appropriately specified, then the HAATE is

\[
\psi^{HAATE}(\bar{m}, \bar{0}) = (\beta_m + \delta_{m,m}) - \beta_0.
\]

When randomization is not homogenous at the cluster level, the model demonstrates why the difference-in-means estimator is biased for the HAATE in this setting \cite{Chin2018} makes a similar observation). Under the difference in means estimator, the expected outcome for a unit assigned treatment \(m\) is,

\[
E[Y_{j,i}|A_{j,i} = m] = \beta_m + \sum_{\ell=1}^{M} \delta_{m,\ell} \times E[p_{j[\ell]}|A_{j,i} = m].
\]

\(\delta_{m,m}\) is represented by \(\delta_{m,m} = \frac{E[j\times i]}{m^2} \times \{A_{j,i} = m\}\) \cite{Baird2018}.

To avoid indexing by units as well, and because the slope effect is modeled separately for each direct treatment condition, we include all units in the proportion so that each unit in the same cluster shares the same values of \(p_{j[m]}\) for all \(m\) in \(0, \ldots, M\).

As the number of observations per cluster is discrete, it is not necessary to assume continuous potential outcomes.
Only when the bias in the expected outcome for a unit assigned treatment \( m \) and the bias in the expected outcome for a unit assigned the control condition are perfectly offset is the estimator unbiased for the HAATE. Alternatively, if there is no interference, all of the \( \sigma_{m,\ell} \) terms are zero.

In this model, we have assumed that interference is a linear function of proportion of each type of treatment within a cluster; this can be broadened to consider the type of general “interference control variables,” discussed in Chin [2018], Section 3, which are a function of the vector \( A_{j,-i} \). We could define such a deterministic function, following Chin [2018], as \( X_i(A_{j,-i}) \), to account for non-linearities in interference, endogenous effects, or to encode further information about the network structure or other ways that interference is channeled. This requires independence of potential outcomes with treatment assignment, and equivalent exogeneity assumptions on the errors as those imposed by our assumptions above.

4 Randomization procedures

We wish to estimate the HAATE for each of \( M \) treatment conditions in comparison to the control condition; our objective is to select a design to minimize average root mean squared error (RMSE) across the \( M \) estimates, with a fixed experimental sample size. RMSE is defined as

\[
\sqrt{\left( \Psi - \hat{\Psi} \right)^2}.
\]

We generalize the concept of coverage or saturation in the binary treatment setting [Hudgens and Halloran 2008] to intra-cluster correlation of treatment in the multiple treatment setting.

We consider two-stage experimental designs, where all units have positive probability of being assigned to any of the treatment conditions in \( 0, \ldots, M \). The researcher first randomly and independently assigns a treatment probability vector to each cluster, defined by \( \pi_j = (\pi_j[0], \ldots, \pi_j[M]) \), such that \( \pi_j[m] \in (0,1) \) for each respective treatment condition, and \( \sum_{m=0}^{M} \pi_j[m] = 1 \). Treatment is then randomly and independently assigned to units within each cluster following the relevant treatment probability vector. The distribution of treatment probability vectors is set by the researcher under some experimental design.

We implement this procedure as cluster-level draws from a Dirichlet distribution, followed by within-cluster randomization according to a Multinomial distribution, parameterized by the Dirichlet draw. Distribution of treatment within clusters then follows a Dirichlet-multinomial distribution with \( n \) trials; each condition follows a Beta-binomial distribution with \( n \) trials. The Dirichlet distribution is parameterized by \( \alpha = (\alpha_0, \ldots, \alpha_M) \).

We set all \( \alpha_m \) as equal at \( \tilde{\alpha} \) so that unconditional expected probability of success of a given condition is \( \frac{1}{M+1} \), regardless of the magnitude of each \( \alpha_m \). This means that we only consider experimental designs where assignment probabilities are balanced, although in practice a researcher may wish to select from designs where, for example, a larger proportion of units are assigned to the control condition. Lower values of \( \tilde{\alpha} \) are associated with greater overdispersion relative to the Multinomial distribution.

Defining an indicator for each treatment level, intra-cluster correlation of treatment as a function of \( \tilde{\alpha} \) is the same for each treatment condition at a given level of \( \tilde{\alpha} \), and takes the form,

\[
\rho_m(\tilde{\alpha}) = \frac{1}{\sqrt{(M+1)\tilde{\alpha} + 1}}.
\]

This allows us to consider permitted experimental designs along a spectrum. At one end of the spectrum, when \( \tilde{\alpha} \) is very small, randomization approximates cluster-level randomization, where each draw from the Dirichlet distribution results in a Multinomial distribution with probability approaching one on any given condition, and probability approaching zero on all other conditions. Intra-cluster correlation for any treatment indicator is near one. At the other end of the spectrum, when \( \tilde{\alpha} \) is very large, randomization approaches unit-level randomization, where each draw from the Dirichlet distribution results in a Multinomial distribution with probability of approximately \( \frac{1}{M+1} \) on each condition. Intra-cluster correlation for any treatment indicator is near zero.

Suppose we are in a setting where there may be both partial interference and intra-cluster correlation of errors. With randomization at the cluster level, the difference-in-means estimator will be unbiased for the HAATE. With randomization at the unit level, the difference-in-means estimator will produce an estimate with a smaller variance, but will not generally produce one that is unbiased for the HAATE, but rather for the average direct causal effect defined by Hudgens and Halloran [2008].
In the absence of interference, however, with randomization at the unit level, the difference-in-means estimator will be unbiased for the HAATE, and variance of the estimate will be decreased relative to cluster-level homogeneous randomization. In the absence of both interference and intra-cluster correlation of errors, the two approaches are equivalent for the difference-in-means estimator. In the presence of both interference and intra-cluster correlation of errors, intermediate designs may be preferable in terms of RMSE.

4.1 Considerations for experimental design

For the linear-in-means model, the variance can be appropriately estimated using the cluster robust generalization of the sandwich estimator [Eicker et al., 1963; Huber, 1967; White, 1980], or with nonparametric bootstrapping at the cluster level. Greenwald [1983] and Moulton [1986] demonstrate that increases in intra-cluster correlation of errors and intra-cluster correlation of the regressors are associated with upward adjustments to the conventional OLS variance estimate. In our setting, the intra-cluster correlation of treatment is decreasing with \( \alpha \), as we move from cluster to unit-level randomization. This does not mean, however, that holding fixed intra-cluster correlation of errors the variance of the estimator is necessarily monotonically decreasing with \( \alpha \).

Indeed, the distribution of the design matrix also changes with \( \alpha \), and the precision of the estimate of the HAATE from the linear-in-means model also depends on our ability to estimate the slopes (in our case, the \( \delta_{m,\ell} \) values). Baird et al. [2018] demonstrate the competing design features in estimating slope effects in randomized saturation designs for binary treatments. In this case, it is only necessary to have a minimum of two different interior saturations (i.e., non-homogenous clusters) in the experimental design to estimate slope effects. With a greater distance between saturations, all else equal, the slope is more precisely estimated. However, all else is not equal; the number of untreated units in high-saturation clusters and treated units in low-saturation clusters is decreasing as the distance between saturations becomes more extreme. These factors represent a trade-off, and will also interact with intra-cluster correlation of errors, \( \rho_u \). At high values of \( \rho_u \), there will be less unique information learned from additional observations from within the same cluster, and so the unequal number of observations at low and high saturations will become less important. In our design setting, with low levels of \( \alpha \), we will be in the scenario where we observe clusters with both very low and very high proportions of the respective treatment and control variables. As \( \alpha \) increases, the distribution of treatment conditions within clusters will be more balanced, with very little overdispersion in proportion of each treatment.

For the difference-in-means estimator, we also estimate effects under an OLS model to facilitate comparable variance estimation,

\[
Y_{j,i} = \sum_{m=0}^{M} \beta_m \times \{A_{j,i} = m\} + \eta_{j,i}.
\]

The difference in means is estimated as \( \hat{\beta}_m - \hat{\beta}_0 \). Using cluster-robust variance estimates, we will see changes in the variance of the estimate due to the interaction of intra-cluster correlation of errors and intra-cluster correlation of treatment as discussed above. But even without intra-cluster correlation of errors, if the true data generating process is as described in the linear-in-means model, the variance of the difference-in-means estimator will change in other ways with the design of the experiment; at each value of \( \alpha \), each \( \hat{\beta}_m \) under the difference-in-means model will absorb the amount of \( \delta_{m,\ell} \) associated with \( E[p_{j,\ell}\mid A_{j,i} = m] \). However, the conditional distribution of \( p_{j,\ell}\mid A_{j,i} = m \) will also change.

These competing factors mean that there is not a one-size fits all solution for experimental design. The optimal design for a given sample size will depend on the form and magnitude of interference, as well as the intra-cluster correlation of errors.

5 Simulations

To demonstrate a range of scenarios, we develop simulations with varying intra-cluster correlation of errors and magnitude of interference. We assume 100 clusters, each with \( n = 50 \). Errors are normally distributed with mean zero and \( \sigma^2 \) fixed at one; \( \tau^2 \) varies following \( \rho_u/(1 - \rho_u) \) according to the values in Table 1. Magnitude of interference is defined by \( \tau^2 \), values of which are also listed in the Table. We run 1,000 simulations at each combination of \( \rho_u \times c \times \alpha \) parameter values.

We consider a setting with three treatment conditions, motivated by the Facebook application discussed below, where there is a control condition, as well as a “high” treatment condition (treatment condition one) and a “low” treatment condition (treatment condition two). Compared to the homogeneous control setting, homogeneous treatment under the “high” treatment condition results in the highest outcomes; homogeneous treatment under the “low” treatment condition results in lowest outcomes. Fixing direct treatment, outcomes are linearly increasing with with the proportion of “high”
treatment assignments in a cluster, and linearly decreasing with proportion of “low” treatment assignments in a cluster. We let the data generating process follow the linear model with slope effects, as below:

\[ Y_{j,i} = \beta_0 \times \{A_{j,i} = 0\} + \beta_1 \times \{A_{j,i} = 1\} + \beta_2 \times \{A_{j,i} = 2\} + \delta_{0,1} \times p_j[1] \times c \times \{A_{j,i} = 0\} + \delta_{0,2} \times c \times p_j[2] \times \{A_{j,i} = 0\} + \delta_{1,1} \times p_j[1] \times c \times \{A_{j,i} = 1\} + \delta_{1,2} \times c \times p_j[2] \times \{A_{j,i} = 1\} + \delta_{2,1} \times p_j[1] \times c \times \{A_{j,i} = 2\} + \delta_{2,2} \times c \times p_j[2] \times \{A_{j,i} = 2\} + \varepsilon_{j,i}. \]

Magnitude of interference is implemented as a multiplier of slope effects, \(c\); larger multipliers are associated with increased levels of interference, while a multiplier of zero is associated with no interference. Consequently, expected potential outcomes under homogeneous treatment are 5 under control, 7.5 + \(c\) under the “high” treatment, and 2.5 – 2.5\(c\) under the “low” treatment.

| Parameter | Value |
|-----------|-------|
| \(\beta_0\) | 5     |
| \(\delta_{0,1}\) | 0.5\(c\) |
| \(\delta_{0,2}\) | -0.5\(c\) |
| \(\beta_1\) | 7.5   |
| \(\delta_{1,1}\) | \(c\) |
| \(\delta_{1,2}\) | -\(c\) |
| \(\beta_2\) | 2.5   |
| \(\delta_{2,1}\) | 2.5\(c\) |
| \(\delta_{2,2}\) | -2.5\(c\) |
| \(\sigma^2\) | 1     |
| \(\rho_u\) | \{0, .1, .3, .5, .8\} |
| \(c\) | \{0, .01, .5, 1\} |
| (M+1) \(\bar{\alpha}\) | \{.001, .01, .05, .1, .2, .3, .4, .5, .6, .7, .8, .9, 1, 2, 3, 10, 1000\} |

We estimate the HAATE both under the correctly specified linear-in-means model and the difference-in-means model, using OLS with cluster-robust variance estimates, implemented in the \texttt{sandwich} package [Zeileis, 2006; Berger et al., 2017] in R.

For varying levels of intra-cluster correlation of errors, \(\rho_u\), and magnitude of interference, \(c\), Table 2 reports the “optimal” \(\rho_{m}(\bar{\alpha})\) as a function of \(\bar{\alpha}\) as the design with the lowest RMSE. Results from simulations with different values of \(\rho_{m}(\bar{\alpha})\) are reported in Appendix Table 4.
Table 2: Simulation results for minimum RMSE design over intra-cluster correlation of errors and magnitude of interference

| \( \rho_u \) | \( c \) | \( \Psi(1,0) \) | \( \Psi(2,0) \) | Optimal \( \rho_m(\hat{\alpha}) \) | Bias | RMSE | Estimated SE | Coverage |
|---|---|---|---|---|---|---|---|---|
| 0 | 0 | 2.5 | -2.5 | 0.999 | 0.725 | 0.000 | 0.002 | 0.035 | 0.034 | 0.034 | 0.035 | 0.956 | 0.957 |
| 0.1 | | | | 0.999 | 0.018 | -0.003 | 0.001 | 0.088 | 0.036 | 0.088 | 0.036 | 0.960 | 0.961 |
| 0.8 | 0.1 | 2.6 | -2.75 | 0.999 | 0.999 | 0.000 | 0.000 | 0.034 | 0.034 | 0.034 | 0.034 | 0.955 | 0.955 |
| 0.8 | 0.5 | 3 | -3.75 | 0.999 | 0.999 | 0.000 | 0.000 | 0.035 | 0.035 | 0.035 | 0.035 | 0.962 | 0.962 |
| 0.8 | 1 | 3.5 | -5 | 0.999 | 0.999 | 0.000 | 0.000 | 0.036 | 0.036 | 0.036 | 0.036 | 0.960 | 0.960 |
| 0.8 | 0.1 | | | 0.999 | 0.999 | 0.000 | 0.001 | 0.163 | 0.163 | 0.163 | 0.163 | 0.955 | 0.955 |
| 0.8 | 0.5 | | | 0.999 | 0.999 | 0.008 | 0.008 | 0.163 | 0.163 | 0.163 | 0.163 | 0.955 | 0.955 |

The “LM” columns represent the linear-in-means estimator, the “DM” columns the difference-in-means model. Empirical bias, RMSE, standard errors from cluster-robust variance estimates, and coverage of 95 percent confidence intervals for each estimator average over the two estimates, \( \hat{\Psi}(1,0) \) and \( \hat{\Psi}(2,0) \), and over the 1,000 iterations.
Figure 2: Simulated RMSE on intra-cluster correlation of treatment as a function of $\bar{\alpha}$

Magnitude of interference, $c$, is fixed at 0.1; intra-cluster correlation of errors $\rho_u$ varies over $\{0.1, 0.3, 0.5\}$. Vertical dashed lines represent the value of intra-cluster correlation of treatment, $\rho_m(\bar{\alpha})$, with minimum RMSE. The upper and lower panel do not share the same scaling on the y-axis.
In this setting, the HAATE is generally estimated with higher RMSE under the correctly specified linear-in-means estimator as compared to the difference-in-means estimator when interference is low to moderate (here, $c \leq .5$). This is largely attributable to the greater variance of the linear-in-means estimator. However, the linear-in-means estimator generally has lower bias and correct coverage, while the difference-in-means estimator has poor to very poor coverage in the presence of interference when randomization is not at the cluster level, as demonstrated in Appendix Table B. As well, when the magnitude of interference is relatively high (here, $c \geq 1$), the bias component begins to outweigh the variance in RMSE, and the unbiased linear-in-means estimator becomes more preferable. These simulations were all conducted with fixed $n$ and $J$; with increasing sample sizes, the fixed bias would eventually outweigh the decreasing variance at all non-zero levels of $c$.

For the linear-in-means estimator, in this setting to minimize RMSE for the HAATE it is nearly always preferable to set the design as close to cluster-level randomization as possible ($\rho_u(\bar{a}) \approx 1$). In this case, the slopes are often dropped from the linear model and are not estimated, and consequently the linear-in-means estimator and the difference-in-means estimator are effectively the same. This is the case even when there is no intra-cluster correlation of errors or interference.

For the difference-in-means estimator, when there are high levels of interference (here, $c \geq .5$), the minimum RMSE is obtained with designs that randomize closer to the cluster level. When there is no interference but errors are correlated, it is preferable to set designs with randomization closer to the unit level. When there is no interference and no intra-cluster correlation of errors, the choice of $\bar{a}$ does not matter. However, when there is an intermediate level of interference (here, $c = .1$), the design trade-offs become evident at different levels of $\rho_u > 0$, as is illustrated in Figure A. For $\rho_u \in \{0.1, 0.3, 0.5\}$, the optimal $\rho_u(\bar{a})$ is an interior value, but it is decreasing with $\rho_u$.

6 Application: Facebook video playback experiments

These methods were applied to online experiments at Facebook, where treatments were combinations of engineering parameters determining the behavior of video playback on the platform. When a user scrolls past or interacts with videos on their Facebook timeline, multiple videos may load and play in quick succession; these parameters ensure that videos load and play with minimal error. Parameters were tuned according to the typical process for testing configuration changes for the Facebook web video player, as such, they would typically be invisible to the user.

Here, the unit of observation is the user-video. When conducting experiments to tune these parameters, the researcher can randomize configurations at the unit level, the user-video, or they may randomize at the cluster level, the user. For this analysis, we deployed both designs for comparison. In this setting, treatment assigned to one video may interfere with outcomes of another video for the same user, as a data-intensive configuration assigned to one video may result in degraded performance for other videos. The most likely form this will take is that videos that load at nearly the same time may saturate the user’s network connection, reducing the speed at which a single video would otherwise load. However, it is unlikely that treatments assigned to videos for one user will affect outcomes associated with videos for another user. Consequently, the partial interference assumption is likely supported, with clustering at the user level.

Figure B illustrates estimates of treatment effects from both unit and cluster randomized experiments for one representative outcome variable, stall counts. Both types of experiments were run over four days, on approximately equal numbers of clusters. Ten treatment conditions were assigned in addition to the control, the status quo configuration of engineering parameters. These treatment conditions can broadly be categorized into “high” and “low” data usage treatments, relative to data usage under the control condition.

The difference-in-means estimator is unbiased for the HAATE under partial interference when treatment is randomized at the cluster level. However, estimates are much less precise than the estimates from the unit-randomized experiment. For the stall counts outcome, the standard error is three times larger under the cluster-randomized design as compared to the unit-randomized design. Additionally, the effects are measured to be around one and a half times more extreme under the unit-randomized design. Thus, while the unit level design appears to induce bias, it provides the prospect of large gains in precision.

The direction of bias is also systematic with type of treatments: high data usage treatments were estimated as being even more effective under unit-randomized experiments as compared to cluster-randomized experiments, and low data usage treatments were estimated as being even less effective. This may be based on the nature of data usage: when all videos for a user are assigned a low data usage configuration, there is relatively little competition for bandwidth. When a given video is assigned a low data usage configuration and all of the other videos are assigned a balanced distribution of low and high data usage configurations, there is increased competition for bandwidth, and the given low data usage video may exhibit deterioration in performance relative to performance under user homogeneous assignment. When all videos for a user are assigned a high data usage configuration, there is a high degree of competition for bandwidth. When a
Effect estimates on stall count from user-level randomization are represented in circles, user-video randomization in triangles. The five high data usage configurations are on the left; the five low data usage configurations are on the right. Treatment effects are estimated as ratios with standard errors estimated by the delta method, but are otherwise unadjusted.

Given video is assigned a high data usage configuration and all of the other videos are assigned a balanced distribution of low and high data usage configurations, there is relatively decreased competition for bandwidth, and the given high data usage video may exhibit improved performance relative to performance under user homogeneous assignment.

We used data from the user- and video- randomized experiments to estimate intra-cluster correlations of errors, and mean and variance of number of videos per user, as clusters were not of uniform size. Based on domain knowledge, we estimated interference as a linear effect of proportion of high data usage videos in a cluster. Table 3 reports estimates of parameters for the stall count outcome, averaged over all treatments. We used this information to conduct simulations and select a value of $\bar{\alpha}$ for the intra-cluster correlation of treatment that would minimize RMSE, and conducted a second round of experiments following an adapted version of the Dirichlet-multinomial procedure described above. The video playback configurations we sought to optimize had five continuous parameters; in the second stage experiment, we tested 30 unique configurations, with a balanced distribution of high and low data usage configurations.

| ICC | SEs, cluster | SEs, unit | Bias term | Var ($n_j$) | $\bar{n_j}$ |
|-----|--------------|-----------|-----------|-------------|------------|
| 0.1 | 0.00522      | 0.00180   | 1.46      | 282.31      | 9.25       |

Table 3: Parameter estimates from cluster and unit randomized experiments

The table reports estimated intra-cluster correlation of error, average standardized SEs under the cluster and user-level randomized experiment, bias as average absolute magnitude of effects under unit-randomized experiments as a multiple of cluster-level randomized effects, and variance and mean size of clusters.

In the Facebook setting it was not practical to assign a unique draw from a Dirichlet distribution to every user in the sample, so we implemented randomization by pre-computing a low number of quasi-random draws from the Dirichlet distribution which were then deployed at scale. These draws were selected based on low-discrepancy Sobol sequences [Sobol', 1967] to more evenly cover the sample space as compared to alternative pseudo-random sampling procedures. Within clusters, treatment was then randomly assigned according to the relevant Multinomial distribution.

We used this approach to optimize the engineering configurations for video playback, which we validated in a user-level experiment. By reducing the variance of effect estimates through design selection, we were then able to use constrained Bayesian optimization to select an optimal configuration from the continuous space of engineering parameters [Letham et al., 2019]. The proposed configuration resulted in a large reduction in stall counts without otherwise degrading measured outcomes.
7 Conclusion

Researchers may wish to estimate the average effect of a homogeneous treatment policy change in the presence of interference, and so will be concerned with both experimental design and estimator. When using RMSE as a metric with a finite sample, it may be preferable to use the difference-in-means estimator, even when the linear-in-means model is correctly specified. In many applied settings, it may be much more practical to use difference-in-means estimators when deploying experiments at scale and so this may be taken as a reassurance to practitioners that biased estimation using difference-in-means estimators may still have lower RMSE than correctly specified linear models that account for interference.

As a general guideline when using the difference-in-means estimator, when there is low or no interference and $\rho_u > 0$, lowest RMSE is obtained by randomizing at the unit level. When $\rho_u = 0$, lowest RMSE is obtained by assigning treatment at the cluster level. In the presence of sufficiently high intra-cluster correlation of errors and sufficiently low interference, researchers can locate an optimal intra-cluster correlation of treatment in terms of RMSE which is between these two extremes.

However, estimated coverage will not be correct for the HAATE in such cases, and so if correct coverage is also desired, randomization should be implemented at the cluster level. If the objective of analysis is to test for or measure interference, an approach more similar to that proposed by [Baird et al. [2018]], Propositions 1 and 2, would be recommended, using a model informed by domain knowledge and minimizing standard errors on estimates of slope effects.

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### Table 4: Simulation results over intra-cluster correlation of errors, magnitude of interference, and experimental design intervals for each estimator average over the two estimates.

| $\rho_u$ | $c$ | $\rho_{0}$ | $\Psi(1, 0)$ | $\Psi(2, 0)$ | Bias | RMSE | Estimated SE | Coverage |
|----------|-----|-----------|--------------|--------------|-------|-------|-------------|----------|
| 0.5      | 0.01| -0.01    | 0.05        | 0.05         | 0.05  | 0.05  | 0.05        | 0.05     |
| 0.1      | 0.01| 0.00     | 0.01        | 0.00         | 0.00  | 0.00  | 0.00        | 0.00     |
| 0.3      | 0.01| 0.00     | 0.00        | 0.00         | 0.00  | 0.00  | 0.00        | 0.00     |
| 0.5      | 0.01| 0.00     | 0.00        | 0.00         | 0.00  | 0.00  | 0.00        | 0.00     |
| 0.1      | 0.00| 0.00     | 0.00        | 0.00         | 0.00  | 0.00  | 0.00        | 0.00     |
| 0.3      | 0.00| 0.00     | 0.00        | 0.00         | 0.00  | 0.00  | 0.00        | 0.00     |
| 0.5      | 0.00| 0.00     | 0.00        | 0.00         | 0.00  | 0.00  | 0.00        | 0.00     |

The "LM" columns represent the linear-in-means estimator, the "DM" columns the difference-in-means model. Empirical bias, RMSE, standard errors from cluster-robust variance estimates, and coverage of 95 percent confidence intervals for each estimator average over the two estimates, $\hat{\Psi}(1, 0)$ and $\hat{\Psi}(2, 0)$, and over the 1,000 iterations.