Meta-Analysis of Randomized Experiments with Applications to Heavy-Tailed Response Data

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Abstract

A central obstacle in the objective assessment of treatment effect (TE) estimators in randomized control trials (RCTs) is the lack of ground truth (or validation set) to test their performance. In this paper, we propose a novel cross-validation-like methodology to address this challenge. The key insight of our procedure is that the noisy (but unbiased) difference-of-means estimate can be used as a ground truth “label” on a portion of the RCT, to test the performance of an estimator trained on the other portion. We combine this insight with an aggregation scheme, which borrows statistical strength across a large collection of RCTs, to present an end-to-end methodology for judging an estimator’s ability to recover the underlying treatment effect as well as produce an optimal treatment "roll out" policy. We evaluate our methodology across 699 RCTs implemented in the Amazon supply chain. In this heavy-tailed setting, our methodology suggests that procedures that aggressively downweight or truncate large values, while introducing bias, lower the variance enough to ensure that the treatment effect is more accurately estimated.

1. Introduction

Causal inference is widely used across numerous disciplines such as medicine, technology, and economics to inform important downstream decisions (Hernan and Robins, 2020). Inferring causal relationships between an intervention and outcome requires estimating the treatment effect (TE): the difference between what happened given an intervention and what would have happened in its absence. A central difficulty is that these two events are never jointly observed (Rubin, 2005). TE estimation leverages randomized controlled trials (RCTs)—which randomly assign the items of...
interest into either the treatment or control groups—to counter selection biases and allow causal effects to be estimated via a simple differences-in-means estimate.

Indeed, the simplest “model-free” unbiased estimator of a treatment effect is the difference-in-means (DM) estimate (Rubin, 2005). Such an estimator may, however, suffer from high variance in real-world scenarios which often involve heterogeneous, high-dimensional and heavy-tailed data\(^1\). A plethora of additional information is thus often used to improve TE estimates relative to this simple baseline. For example, pretreatment regression adjustments can significantly reduce the variance of a treatment effect estimate while adding little additional bias (Angrist and Pischke, 2008; Imbens and Rubin, 2015). Similarly, a host of other regularization and robustness modifications can be used to trade off bias and variance.

As the complexity of such estimators increases, so do the assumptions (and work) needed to establish their statistical validity. One particular setting in which this becomes easier, and which we argue arises in many practical applications,\(^2\) is when large RCTs can be run on the same population. This setting provides an opportunity to get at the fundamental attributes of interest—the mean-squared error (MSE) of a given treatment effect estimator and its ability to inform treatment roll out decisions. Our simple insight is that the DM estimator can function as a noisy, but unbiased “label” for the treatment effect. Noisy estimates for a TE estimator performance can then be computed by comparing this estimator to the (unbiased) difference-in-means estimator via a simple, held-out validation estimate (see Theorem 1 and Theorem 2). Our goal in this work is to judge the performance of TE estimators by pooling noisy (but unbiased) estimates of their performance across many RCTs. Such a procedure is desirable because it targets the actual quantity of interest, the estimator MSE, in an assumption/estimator-agnostic fashion. The primary contributions of this work are as follows:

- We process a corpus of 699 genuine RCTs implemented at Amazon across several years and we highlight the heavy-tailed nature of the response and covariate variables. The unique challenges associated with heavy-tailed estimation require careful navigation of the bias-variance tradeoff which motivates the development of an objective selection procedure for TE estimation.

- We present a selection scheme which borrows statistical strength across the corpus of RCTs in order to judge the relative performance of several commonly used TE estimators, including their usefulness at defining a treatment roll out policy.

- We use this framework to argue that in the presence of heavy-tailed data—that often arise in large-scale technology and logistics applications—aggressive downweighting and truncation procedures are needed to control variance.

- We propose an extension of this methodology that allows us to use a collection of RCTs to assess the impact of different roll out policies for an RCT.

- We also use this framework to show that the generally accepted practice to use statistical significance at level \(\alpha = 0.05\) for the TE to determine the roll out policy is far from optimal for the Amazon Supply Chain and should instead be determined empirically.

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1. Such heavy-tailed data is commonplace in the large-scale RCTs which motivate our study.
2. Including AB testing of forecasting model improvements, website changes, supply-chain modifications, or a number of other interventions.
1.1 Related Work

The literature on causal inference and treatment effect estimation is vast and a comprehensive review is beyond the scope of this paper. Hernan and Robins (2020); Imbens and Rubin (2015); Angrist and Pischke (2008); Hadad (2020) and Wager (2020) provide modern perspectives on both the theory and practice of treatment effect estimation. Cross-validation (CV) also has been (and remains) a major subject of statistical inquiry as it is amongst the most widely used tools to assess the quality of an estimator and perform model selection Bayle et al. (2020); Lei (2020); Stone (1974); Geisser (1975).

Relatively little work has been done in the intersection of these two domains. Part of the difficulty stems from the fact that the standard procedure of CV breaks down for treatment effect estimation since the true treatment effect is never observed in data. Athey and Imbens (2016) and Powers et al. (2018) do provide model-specific selection methods in the context of treatment effect estimation. However, these works do not apply to arbitrary TE estimators. Closest to our work is that of Schuler et al. (2018), who use a data-splitting methodology to evaluate several risk functions to assess heterogeneous treatment effect estimators. This differs from our work in two principal ways. First, our framework targets the problem of average treatment effect estimation—in many scenarios that we are interested in, treatments cannot be individualized and must be applied in an all-or-nothing fashion to the entire population. Our statistical scheme also differs since we provide a provably unbiased estimate of the mean-squared error of a TE estimator, and we introduce an aggregation scheme to borrow statistical strength across different AB tests to compare estimators. Additionally, our work uses a large corpus of 699 actual randomized AB tests conducted at Amazon over the course of several years as our test-bed for estimator selection in contrast to synthetic data simulations.

One of our main motivations is to highlight the unique challenges associated with heavy-tailed data often present in applications arising at large-scale technology and logistics companies. Semiparametric TE estimators for heavy-tailed datasets inspired by similar applications have been explored Fithian and Wager (2014) and Taddy et al. (2016). However, these works do not address the problem of model selection which is our central focus. Specifically, we focus on methods to select among simple estimators (with few to no tuning parameters) that are widely used in practice.

1.2 Preliminaries

We work within the Rubin potential outcomes model (Rubin, 2005) where we imagine we are given a domain of objects $\mathcal{Y}$ and a target variable of interest $Y(\cdot)$ given a possible intervention. For a fixed intervention $I$, our goal is to estimate the population average treatment effect (ATE):

$$\Delta = \mathbb{E}[Y(1) - Y(0)],$$

where $Y(1)$ corresponds to the value of an experimental unit—in our case a product in the supply chain—given the treatment and $Y(0)$ its unobserved counterfactual control (and vice versa). In general, we also allow the existence of other covariates in our model $X \in \mathcal{X}$. In a given AB test, we first randomly sample an equal number of items into a treatment group, $T$, and a control group $C$. We further let the $(X_i, T_i, Y_i)$ be the covariates, treatment dummy, and value of the $i$th item. By a standard argument, using the assumption of randomization (independence of $\{Y_i(1), Y_i(0)\}$ and $T_i$),

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3. Leveraging the unbiased nature of the DM estimator.
the differences-in-means estimator,
\[
\hat{\Delta}_{DM} = \frac{1}{|T|} \sum_{i \in T} Y_i(1) - \frac{1}{|C|} \sum_{i \in C} Y_i(0),
\]

provides an unbiased estimate of \( \Delta \) (Rubin, 2005). A primary benefit of the DM estimator is that it is “model-free.” That is, it makes no explicit assumptions on the data-generation process for \( Y_i \) as a function of the other covariates.

1.3 Dataset Description

We use 699 RCTs that were run at Amazon since 2017 on a population of products. The interventions in each RCT consist of various modifications and (potential) improvements to the way in which products are processed through the supply chain. The RCTs are most often constructed with 50% of products in an RCT randomly placed in the treatment group and 50% in the control group, though some are not evenly balanced. The RCTs vary in size from tens of thousands of products to those with several millions. Each RCT is run over the course of approximately 27 weeks with the intervention instituted at a trigger date at 10 weeks in the treatment group.

At each week in an RCT, the response variable generated from each product is computed. Each RCT was preprocessed to contain the averaged pretreatment response (denoted \( X \)), a strictly nonnegative averaged pretreatment auxiliary covariate (denoted \( D \)), averaged posttreatment response (denoted \( Y \)), and binary treatment indicator (denoted \( T \)) for each item. Auxiliary covariates (such as \( D \)) often arise in naturally occurring applications where it is feasible to forecast a related quantity to \( Y \) (such as the number of expected products needed in a time period to satisfy user demand).

2. Heavy Tails and Hard Estimation Case Study

The difficulties associated with treatment effect estimation of an intervention in large-scale commerce RCT datasets are many fold. The most salient difficulty for our consideration is that the response distribution over the range of products has a heavy tail. Similar heavy-tailed distributions are known to exist in user revenue distributions as well as user engagement metrics at large-scale technology companies (Fithian and Wager, 2014; Taddy et al. 2016). Estimation in this setting is difficult and requires balancing several considerations when considering the pros and cons of various estimation techniques. Our exploration of these issues serves a dual purpose: (1) to highlight the ubiquitous occurrence of such heavy tails in naturally occurring data, and (2) to motivate the need for a model selection procedure to navigate the bias-variance tradeoff.

Let us investigate the data inside a single RCT to assist in further making this point. The RCT under consideration consists of millions of distinct products. This RCT (a representative choice) displays significant heavy-tail behavior, as shown in Fig. 2.

We implement the Hill estimator to obtain an estimate of the power-law behavior \( \eta \) in the right tail distribution of \( \sim y^{-\eta} \) across all the RCTs under consideration. The Hill cutoff hyperparameter is chosen to discard points near the center of the distribution (i.e., near zero) and allows the formulation of a bias-variance tradeoff (Drees et al., 2000). We avoid a more sophisticated data-driven choice of this cutoff since the precise Hill value is not of particular interest in our setting. Rather, it is

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4. Indeed we have tens of thousands of points in all RCTs, so small-sample difficulties associated with “Hill horror plots” seem not to arise.
Figure 1: Gini plot of a single RCT showing the cumulative share of demand vs. product population share ordered by descending popularity. Demand is heavy-tailed with the top 20% most popular products accounting for nearly 80% of the demand share.

Figure 2: Hill plot of the right tail of the response variable distribution in a single RCT versus the Hill cutoff hyperparameter. The Hill values are an estimate of the power $\eta$ in the asymptotic tail behavior of the response distribution variable, $Y$, $p(y) \sim y^{-\eta}$.

apparent the power $\eta$ can be conservatively judged to be between 1 – 3 in Fig. 2. Analyzing the response distribution across the entire corpus of 699 RCTs and choosing the Hill cutoff parameter at the 5th percentile shows that the average decay exponent is $\approx 2.32$ with a standard deviation of 0.79, and median of 2.1476.

The difficulties seen in this case study reinforce the conclusion that handling the heavy tails inherent in our data likely requires more sophisticated (regularized) estimators than the DM estimator. Ultimately this boils down to balancing the tradeoff between bias and variance in estimation. Navigating this bias-variance tradeoff is one of the primary motivations for our aggregation methodology for TE estimator selection.

3. Validation Procedure for Treatment Effect Estimators

In this section, we present the key idea behind the validation procedure we use to assess the quality of an arbitrary treatment effect estimator, $\hat{\Delta}_E(\cdot, \cdot)$, in the RCT denoted $I$. Let $\Delta$ denote the population ATE shown in (1). Given the groups $T$ and $C$, we first randomly partition them into disjoint groups $T_1$, $T_2$ and $C_1$, $C_2$. Now, consider the (potentially complicated) treatment effect estimator $\hat{\Delta}_E(T_1, C_1)$ trained on the first fold of data. We can obtain an estimate of its performance by how well it targets the difference-of-means estimator computed on the hold-out set $\hat{\Delta}_{DM}(T_2, C_2)$:

$$\hat{\text{MSE}}_{I,E}(\hat{\Delta}_E(T_1, C_1), \hat{\Delta}_{DM}(T_2, C_2)) = (\hat{\Delta}_E(T_1, C_1) - \hat{\Delta}_{DM}(T_2, C_2))^2.$$ (3)

A simple argument shows that this quantity is a noisy but unbiased MSE of the estimator (and thus it permits the relative comparison of two different estimators).

**Lemma 1** Given two different treatment effect estimators $A$ and $B$ in the aforementioned setting, we have:

$$\mathbb{E}[(\hat{\Delta}_A(T_1, C_1) - \hat{\Delta}_{DM}(T_2, C_2))^2] \leq \mathbb{E}[(\hat{\Delta}_B(T_1, C_1) - \hat{\Delta}_{DM}(T_2, C_2))^2] \implies (4)$$

$$\mathbb{E}[(\hat{\Delta}_A(T_1, C_1) - \Delta)^2] \leq \mathbb{E}[(\hat{\Delta}_B(T_1, C_1) - \Delta)^2].$$

5
we believe a relative improvement of estimator \( \hat{A} \) and \( \hat{B} \). However, simply using this estimator on a single RCT provides a (potentially very) noisy estimate of the population error, not the population error itself. Indeed, if the estimator \( \hat{\Delta}_{DM}(T_2, C_2) \) is sufficiently good to estimate \( \Delta \), why even bother to use another estimator? Said another way, the error estimate in (3) will always suffer at least the variance of the unbiased estimate (2). In practice we use a cross-validated version of (3) to reduce the subsampling variance due to the random train/test splits (see Appendix C). This procedure will not decrease the variance of the DM estimator arising from the underlying heavy-tailed data however.

Our proposal for resolving this conundrum is to note that in many situations we have access to \textit{multiple} RCTs from the same underlying population or process given different interventions. Thus, aggregating the set of error estimates

\[
\hat{A} = \{ \text{MSE}_{I_1, A}((T_1, C_1), (T_2, C_2)), \ldots, \text{MSE}_{I_N, A}((T_1, C_1), (T_2, C_2)) \}
\]

and comparing to

\[
\hat{B} = \{ \text{MSE}_{I_1, B}((T_1, C_1), (T_2, C_2)), \ldots, \text{MSE}_{I_N, B}((T_1, C_1), (T_2, C_2)) \},
\]

for various interventions \( \mathcal{L} = \{I_1, \ldots, I_N\} \), can allow us to pool information across RCTs. We sidestep the methodological complexities of performing this aggregation and instead turn to an investigation of simple, practically-motivated schemes.

### 3.1 An Aggregation Scheme

Aggregating the mean-squared errors requires handling a practical consideration. Since the RCTs and interventions across RCTs themselves may be different, the overall scales of the MSEs between different RCTs may be different. As an example, consider a corpus of two RCTs on which estimator \( A \) obtain errors \( \{1, 10\} \) and estimator \( B \) obtains errors \( \{2, 9\} \). Simply averaging the errors or doing a rank-based test of performance would indicate both estimators are equivalent. However, intuitively we believe a relative improvement of estimator \( B \) from 10 to 9 on the second RCT does not outweigh the degradation from 1 to 2 on the first RCT.

This observation motivates the definition of a normalized score to compare the estimators \( A \) vs \( B \), as a function of the vectors of their noisy errors

\[
S_i(\hat{A}_i, \hat{B}_i) = \frac{\hat{B}_i - \hat{A}_i}{\hat{B}_i + \hat{A}_i},
\]

for \( \hat{A}_i \in \hat{A} \) and \( \hat{B}_i \in \hat{B} \). Where \( \hat{A} \) and \( \hat{B} \) are defined according to (5) and (6) respectively.

This normalized score vector (which we denote by \( S(\hat{A}, \hat{B}) \)) implicitly binarizes each of its elements to bound them in the range \([-1, 1]\). Each element of this vector is a noisy score of estimator \( A \)'s performance relative to \( B \) on \textit{one RCT in the corpus}.\(^6\) If the estimator has many elements that are positive, it suggests that estimator \( B \) has larger errors than estimator \( A \). In this case, we would expect estimator \( A \) to be better than estimator \( B \).

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\(^5\) As noted earlier, in practice each error estimate is averaged over several resampled train/test splits, but we suppress this extra notation for clarity.

\(^6\) Our notion of a normalized score vector is element-wise transitive. That is, if \( \frac{a-b}{a+b} > 0 \) and \( \frac{c-b}{c+b} > 0 \) imply \( \frac{a-c}{a+c} > 0 \).
To formalize this intuition, we use the following heuristic which implicitly treats each RCT equally independent of size. We use a two-sided one-sample $t$-test applied to this normalized score vector to test the null that the "population mean" of the $\hat{S}$ "distribution" is 0, i.e., that the performance of estimator $A$ is indistinguishable from the performance of estimator $B$. Overall, this procedure interpolates between two extremes. A purely rank-based test of performance might only count the number of RCTs for which $A$ is better than $B$ irrespective of how much better one is in a particular RCT. Meanwhile, a procedure which only looks at the raw (unnormalized) RCT errors has the property that RCTs with large MSE values for both estimators would drown out signal from RCTs with small MSE values. We stress that the $t$-test heuristic provides a simple way of converting the information contained in $\hat{S}(\hat{A}, \hat{B})$ to a single number, but we recommend looking at the score histograms for a more complete picture.

4. Validation Procedure for RCT-Driven Decision Making Policies

Though estimating the treatment effect helps teams better understand the inner workings of the Amazon supply chain, the fundamental motivation for running an RCT is typically to determine whether a planned change (a new algorithm, system, or intervention) should be rolled out to all treatment units. Practically, this means deciding whether to deploy a new algorithm, system or other intervention to all products that run through the supply chain. The traditional approach of relying on the treatment effect estimate and its statistical significance to make this decision runs into two types of problems:

1. Statistical significance can be difficult to achieve, especially in the presence of heavy-tailed response data.

2. The traditional threshold for statistical significance at level $\alpha = 0.05$ is arbitrary and emphasizes controlling for type I errors over type II errors. However, businesses are equally concerned about forgoing opportunities to improve efficiency (type II error) as they are about deploying changes that are not actually beneficial (type I error), so there is no strong reason to favor the status quo.

This is not to say that the optimal roll out policy is to simply deploy any treatment for which the estimated ATE is positive, i.e. $\hat{\Delta}_E > 0$. Rather, the estimated treatment effect, and associated statistics such as standard error, t-statistic, etc., should be used to guide a binary decision of the form $D(\hat{\Delta}_E) = D_E \in \{0, 1\}$ for deploying a treatment in production. The important insight here is that we are not interested in making only one decision in isolation, but a sequence of decisions over time on the basis of multiple RCTs that share a common metric. In this setting, exploiting commonality between treatment effects allows us to pool information across RCTs and improve our decisions. From this perspective, it is easier to reason about the problem from a Bayesian perspective by considering the ATE $\Delta_i$ for each RCT is drawn from a prior distribution $\Delta_i \sim \mathcal{P}(\Delta)$. The estimated treatment effect is then given by $\mathbb{P}(\hat{\Delta}_E | \Delta_i)$, and in that sense our goal can be understood as determining $\mathbb{P}(\Delta_i > 0 | \hat{\Delta}_E)$, where $\Delta_E = (\hat{\Delta}_{E, I_1}, \ldots, \hat{\Delta}_{E, I_N})$.

Formulating a fully Bayesian perspective to the problem is neither necessary nor our intention in this paper. We took this detour to stress the fact that $\hat{\Delta}_E > 0$ is not a priori a sensible decision rule even if we consider type I and type II errors to be equally important. In our setting, the goal remains to maximize the cumulative financial impact (CFI) across the corpus of RCTs $\mathcal{I}$, and this
can be formalized as the objective function:

$$F_E = \sum_{i \in I} M_i \Delta_i D_{E,i}$$  \hspace{1cm} (8)

where \(M_i\) is the size of the treatment target population for RCT \(i\), i.e. the population to which the treatment would be applied if rolled out.

Directly maximizing (8) for the best decision rule \(D_{E,i}\) is not possible, however, since it would require knowledge of the true ATE \(\Delta_i\) for all RCTs. Fortunately, \(F_E\) is linear in \(\Delta_i\), and we can produce an unbiased estimate of it using an unbiased estimate for the ATE (provided by the DM estimator \(\Delta_{DM,i}\)). To ensure independence between the estimated ATE \(\Delta_{DM,i}\) and the decision rule \(D_{E,i}\) we can again rely on splitting the treatment and control into two groups \(T_1, T_2\) and \(C_1, C_2\). Doing this for every RCT allows us to construct the empirical objective (see Appendix C for details)

$$\hat{F}_E = \sum_{i \in I} M_i \hat{\Delta}_{DM,i}(T_2, C_2) D_{E,i}(T_1, C_1)$$  \hspace{1cm} (9)

which is unbiased for \(F_E\):

**Lemma 2** \(\hat{F}_E\) is unbiased for \(F_E\) in (8) for a decision policy based on sub-sampled data \((T_1, C_1)\).

Again seen Appendix A for the proof.

We constructed \(\hat{F}_E\) with the Amazon supply chain in mind, but it generalizes to any context in which the following apply:

1. The target metric is additive across units in the population and treatments applied.
2. The treatments do not interact with each other\(^7\).
3. The decision made on the basis of each treatment is binary (i.e. apply treatment or do not).
4. The RCTs are statistically independent in that each RCT control/treatment group assignment is independently randomized.

These requirements are satisfied in many settings. Examples range from field experiments in education aiming to increase standardized testing results in schools, to continuous improvements to user interfaces focused on customer engagement, to medical interventions targeting a specific outcome such as reduced cholesterol, amongst others. Indeed, \(\hat{F}_E\) is a relevant quantity in any domain where repeated experiments are the common.

One nuance worth noting given our lengthy discussion of normalization in Section 3.1 above is that in defining \(F_E\), we do not normalize; rather, we track the additive effect of each RCT in proportion to the relevant population to which it would be applied, meaning that RCTs with low ATE whose decision would impact a large population will contribute more than those with high ATE but low corresponding population size—which is sensible from the decision-making perspective.

\(^7\) In our setting, we believe this assumption to be reasonable in for two reasons. First, since most treatments result in treatment effects of small magnitude, any mutual interactions are approximately locally linear. Second, even for potentially large interactions, such interactions are likely to incoherently add and cancel across RCTs, since there is limited coordination between RCTs and their purpose.
5. Results

In this section we detail several simple and commonly used estimators for TE estimation and subsequently compare their relative performance.

5.1 Estimators

For the following estimators, we note that each admits a “Winsorization” which can be used to trade off bias and variance. To do this, we can simply Winsorize the covariates and targets, $X, D, Y$, in only the training fold, to reduce variance. The test folds are always left untrimmed/Winsorized so Theorem 1 remains valid. Explicitly we define Winsorization at level $0.001$ to Winsorize the $X,Y$ distributions at $P_{0.1}, P_{99.9}$ and the (positive) auxiliary $D$ distribution at $P_{99.9}$.

The simple difference-of-means estimator,

$$\hat{\Delta}_{DM} = \frac{1}{|T|} \sum_{i \in T} Y_i(1) - \frac{1}{|C|} \sum_{i \in C} Y_i(0),$$

as defined before is the first estimator we consider. We also consider the Difference-of-Median-of-Means (mom) estimator

$$\hat{\Delta}_{DMoM} = \text{MoM}(\{Y_i(1)\}_{i=1}^{|T|}, B) - \text{MoM}(\{Y_i(0)\}_{i=1}^{|C|}, B).$$

Where $\text{MoM}(\{Y_i(1)\}_{i=1}^{|T|}, B)$ indicates we bucket the data into $B$ blocks, compute the mean in each block, and the median across all the blocked means. We use $\text{mom}1000$ in our experiments to denote the median-of-means estimator chosen with 1000 total blocks. Next we also consider what we refer to as the Generalized Difference-in-Differences (gen_dd) estimator which assumes access to a pretreatment item-specific covariate $X_i$ corresponding to the response value $Y_i$. So, assuming the model,

$$Y = \alpha + T \cdot \Delta + X \cdot \beta + \epsilon,$$

we can estimate the ATE for a binary treatment by (least-squares) regressing $Y_i$ onto $(1, T_i, X_i)$, where $\epsilon_i$ represents a general conditionally mean-zero noise term (which may depend on $X_i$). If the covariates $X_i$ are strongly correlated with the response value $Y_i$, incorporating them into the regression can significantly reduce the variance.

Finally we consider a reweighted version of the previous estimator we refer to as the Weighted Generalized LR (and Generalized Difference-in-Differences) (gen_dd_w1) estimator. That is,

$$\frac{1}{n} \sum_{i=1}^n \frac{1}{(1 + D_i)^\gamma} (Y_i - \alpha - \Delta T_i - \beta_i X_i)^2.$$ 

To estimate $\alpha \beta$, and most importantly the TE $\Delta$. In practice, the covariate $D$ is taken as an auxiliary covariate, which serves as positive surrogate capturing the shape of the distribution of $Y$. In this case the weighting has the effect of downweighting large values of $Y$ which can be useful to regularize heavy-tailed distributions.
5.2 Estimator Comparisons

In this section, we present results obtained from a corpus of 699 RCTs performed at Amazon over several years as described in Section 1.3. We compare estimators by their out-of-sample MSE computed via the cross-validation procedure described in Section 3.

We begin by studying several of the normalized score histograms to facilitate the comparison of our estimators; additional results are provided in Appendix B. In judging two estimators $A, B$ via their score distribution $\hat{S}(A, B)$, we note that a left-skewed score distribution indicates $B$ is a better estimator (in terms of its MSE) than $A$.

![Figure 3: Histogram of the score distribution for dm vs Winsorized (at 0.001) dm estimator.](image1)

![Figure 4: Histogram of the score distribution for dm vs gen_dd estimator.](image2)

![Figure 5: Histogram of the score distribution for dm vs gen_dd_w1 estimator.](image3)

In Table 1, we use the $t$-test heuristic from Section 3.1 to summarize each score histogram. For the sake of brevity, we do not display all the methods tested in the table. Overall, we see several phenomena that accord with our expectations. First, adjusting for the pretreatment covariate reduces variance (i.e., gen_dd is better then dm). Second, downweighting large values of $Y$ provides significant value: inverse weighting by $D$ and Winsorization performs generically the best under our metric (gen_dd_w1 and all Winsorized estimators perform well). We also see that the dm estimator is dominated by every other method in Table 1, such as the median of median-of-means estimator (mom1000), whose robustness underlies its improved performance.

We summarize this table by converting it into a table of pairwise comparisons of wins/losses/ties using a $p$-value to determine the significance of the win or loss. The question of extracting an ordered ranking from the table of wins/losses is a classic problem. The natural procedure of simply summing up the number of row-wise wins is commonly referred to as the Copeland/Borda counting method (see (Saari and Merlin, 1996) and references within).

Applying such a method by inspection returns the following rankings:

$gen_dd_w1_wins.001 > gen_dd_wins.001 > dm_wins.001 \approx gen_dd_w1 > gen_dd > mom1000 > dm$

Overall, these results suggest that aggressively Winsorizing and/or downweighting heavy tails can profitably trade variance for some additional bias.

5.3 Estimator statistic and Roll Out Policies

We now turn to the question of evaluating the effectiveness of a roll out policy $D_E$. A common policy, which we use internally, is to roll out treatments that are positive and statistically significant
Table 1: Comparison of Estimators via one-sample t-test applied to their normalized score vector. Easiest to read row-wise. The index \((A, B)\) of the table computes the pair of the \((t\text{-statistic}, p\text{-value})\) associated with the score \(S(A, B)\). A large positive \(t\)-statistic at index \((A, B)\) indicates estimator \(A\) is better then estimator \(B\) and vice-versa.

| Method   | \(\text{dm}\) | \(\text{mom1000}\) | \(\text{gen}_{dd}\) | \(\text{gen}_{dd\_w1}\) | \(\text{dim\_wins\_0.01}\) | \(\text{gen}_{dd\_wins\_0.01}\) | \(\text{gen}_{dd\_w1\_wins\_0.01}\) |
|----------|----------------|---------------------|-------------------|-------------------|------------------|------------------|------------------|
| \(\text{dm}\) | \((-3.58, 0.000363)\) | \((-12.68, 2.38e-33)\) | \((-22.36, 3.6e-84)\) | \((-28.19, 7.99e-118)\) | \((-25.33, 2.96e-101)\) | \((-24.96, 4.11e-89)\) | \((-25.33, 2.96e-101)\) |
| \(\text{mom1000}\) | \((13.68, 2.38e-33)\) | \((-12.12, 0.0342)\) | \((-11.89, 7.32e-30)\) | \((-15.51, 3.78e-37)\) | \((-14.61, 1.94e-42)\) | \((-15.72, 5.33e-48)\) | \((-23.15, 2.6e-75)\) |
| \(\text{gen}_{dd}\) | \((22.36, 3.6e-84)\) | \((21.1, 4.73e-77)\) | \((-21.1, 4.36e-37)\) | \(-9.56, 1.87e-07\) | \((-5.39, 9.62e-08)\) | \((-6.4, 8.5e-05)\) | \((-6.12, 4.2e-05)\) |
| \(\text{gen}_{dd\_w1}\) | \((28.19, 7.99e-118)\) | \((19.01, 2e-65)\) | \((-9.56, 1.87e-07)\) | \((-15.72, 5.33e-48)\) | \((-15.72, 5.33e-48)\) | \((-15.72, 5.33e-48)\) | \((-15.72, 5.33e-48)\) |
| \(\text{dim\_wins\_0.01}\) | \((23.49, 1.14e-90)\) | \((14.61, 1.94e-42)\) | \((-5.12, 3.87e-07)\) | \((-9.56, 1.87e-07)\) | \((-5.39, 9.62e-08)\) | \((-6.4, 8.5e-05)\) | \((-6.12, 4.2e-05)\) |
| \(\text{gen}_{dd\_wins\_0.01}\) | \((25.33, 2.96e-101)\) | \((14.61, 1.94e-42)\) | \((-5.12, 3.87e-07)\) | \((-9.56, 1.87e-07)\) | \((-5.39, 9.62e-08)\) | \((-6.4, 8.5e-05)\) | \((-6.12, 4.2e-05)\) |
| \(\text{gen}_{dd\_w1\_wins\_0.01}\) | \((24.96, 4.11e-99)\) | \((15.72, 3.33e-48)\) | \((-9.56, 1.87e-07)\) | \((-5.39, 9.62e-08)\) | \((-6.4, 8.5e-05)\) | \((-6.12, 4.2e-05)\) | \((-6.12, 4.2e-05)\) |

at level \(\alpha = 0.05\). To date, Amazon has used the Generalized Difference-in-Differences estimator, meaning that the standard decision rule has been:

\[
D_{\text{standard}} = 1 \left[ \frac{\hat{\Delta}_{\text{gen}_{dd}}}{\sigma_{\text{gen}_{dd}}} > 1.96 \right] \quad (14)
\]

Because the decision rule depends on the choice of estimator and \(t\)-critical \(t_c\) decision threshold, it is worth comparing outcomes of various potential combinations, as we do in Fig. 6. We plot the normalized CFI \(\bar{F}_E/\bar{F}_{\text{standard}}\) for estimator \(E\) as a function of \(t_c\), where the decision rule takes the general form:

\[
D_E = 1 \left[ \frac{\hat{\Delta}_E}{\sigma_E} > t_c \right]. \quad (15)
\]

Specifically Fig. 6 shows the normalized CFI, as a function of \(t_c\), for 3 estimators: the Difference-in-Means; the Generalized Difference-in-Differences; and the Weighted Generalized Difference-in-Differences with \(\gamma = 0.6\). We included the first two estimators because they correspond to two important baselines: the unbiased “target” used to construct \(\bar{F}_E\) and the estimator for our standard policy, respectively; we chose the last because it performed best at maximizing \(\bar{F}_E\) among the estimators we considered. Fig. 7 shows the normalized CFI, along with 95% confidence bands, for the Weighted Generalized Difference-in-Differences with \(\gamma = 0.6\). These confidence bands are computed via cross-validation by re-sampling \((C_1, T_1)\) and \((C_2, T_2)\) 100 times.

To interpret Fig. 6 and Fig. 7 recall that for the policies of the form (15), we roll out any treatments that achieve a \(t\)-statistic equal to or greater than \(t_c\) for that ATE estimator. In other words, the Figures show the financial impact of requiring a low evidence threshold to roll out a treatment as \(t_c \to -\infty\) and a large evidence threshold as \(t_c \to \infty\). For the specific case of Amazon Supply Chain, Fig. 7 implies that the most significant improvement of the decision policy comes from adjusting \(t\)-critical to somewhere around \(-1.2\) with a confidence interval of \((-2.4, 0.4)\). This change, and the roll outs that follow, could more than double the estimated cumulative financial impact of decisions made on the basis of our RCTs.

When it comes to the choice of estimator, the evidence is less convincing, at least for the three estimators shown in Fig. 6. We have not found the paired difference in normalized CFI for these estimators to be statistically significant when using \(t_c = -1.2\). This is not too surprising given the width of the confidence bands in Fig. 7.

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8. As explained in Bengio and Grandvalet (2004), confidence intervals computed via cross-validation should be interpreted cautiously.
Up until this point, we have limited our investigation to roll out policies of the form (15) based on a $t$-critical threshold for the ATE. A priori there is no reason to expect that decision rules of this form will be the best choice to optimize (9) and so we also consider a more general framework and expand the policy space to regression models of the form $D = D(X)$. Here the covariates $X$ include pre-RCT variables, such as per unit profit and population size, but the as well as different ATE estimates and their associated $t$-statistics $\hat{\Delta}_E\hat{\sigma}_E$. The plot in Fig. 8 shows that using a Random Forest for $D = D(X)$ achieves the highest normalized CFI and this improvement over the second best model, the Weighted Generalized Difference-in-Differences with a $t$-critical of $-1.2$, is statistically significant.

One lingering question however is how well this approach generalizes: that is, can a roll out policy learned on the 699 RCTs be safely used on future experiments? To answer this question, we revised the computation of (9) to capture the fact that the RCTs are not performed all at once, but rather they are ordered in time based on when each RCT ended. As such, optimizing $\hat{F}_E$ can be treated as an online regression problem where the outcome of the first $N$ RCTs should guide the policy (say regression coefficients) for the $N + 1$st RCT. This both ensures the policy can adjust over time—which is important if the the ATE prior $F(\Delta)$ experiences a distributional shift—but also that ensures that any claimed cumulative financial impact improvement is evaluated out-of-sample.

Fig. 9 shows the expected outcome of this procedure as RCTs accumulate for 4 policies: the standard policy from (14), an online Random Forest (RF), an online $t_c$ roll out policy using Weighted General Difference-in-Differences, and the optimal $t_c = -1.2$ value for the Weighted General Difference-in-Difference. Not surprisingly, the optimal $t_c = -1.2$ in hindsight outperforms the online models. More important is the fact that both online models achieve higher normalized

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9. That is to say the $t_c$ value used to decide RCT $N + 1$ is based on the optimal value for the previous $N$ RCTs.
Figure 8: Boxplot comparing the normalized CFI for 4 roll out policies: the standard policy with \( t_c = 1.96 \); the Weighted Generalized Difference-in-Differences policy with \( t_c = -1.20 \); a linear regression (OLS) policy; and random forest (RF) based policies. The distributions are estimated using cross-validation.

CFI than the standard policy (14) by a factor greater than two over the course of the RCT corpus, as shown in Fig. 10. In other words, even without the benefit of hindsight applying either of these online policies would have lead to substantial improvement in CFI.

6. Conclusion

In this work, we develop a simple methodology for treatment effect model/estimator selection which pools the performance of estimators across RCTs. The methodology allows us to compare estimators on a held-out data fold in an unbiased way. The results align with a priori intuitions of estimator performance for our data corpus. One insight is that we should be trading off variance for more bias to reduce the MSE of treatment effect estimation in problems with heavy tails. Further investigation into better estimators (as judged by their held-out MSE) and their coverage is warranted. The methodology also naturally lends itself to the question of when to roll out treatments, by allowing the comparison of different roll out policies based based on their estimated cumulative financial impact. We found that the standard policy of rolling out treatments for which the estimated ATE is significantly positive is far from optimal for our RCT corpus. In particular, a much more aggressive roll out policy can more than double the financial impact of decisions based on the RCTs run at Amazon.

While our corpus consists of RCTs at Amazon run over several years, we hope our primary methodological contribution – to propose a cross-validation-like methodology to evaluate TE estimators and their corresponding decisions – can be used to objectively evaluate causal inference techniques in settings where large corpora of RCTs are available.
The authors thank Robert Stine, Edo Airoldi, and Kenny Shirley for their valuable comments and feedback.
A. Proofs of Estimator Validation Lemmas

First, we present the proof of Theorem 1.

**Proof** [Proof of Theorem 1] We simplify the MSE of a treatment effect estimator $E$ by centering the DM estimator around its mean and expanding the square:

$$E[(\hat{\Delta}_A(T_1, C_1) - \Delta)^2] = E[(\hat{\Delta}_A(T_1, C_1) - \Delta + \Delta - \hat{\Delta}_DM(T_2, C_2))^2] =$$

$$E[(\hat{\Delta}_A(T_1, C_1) - \Delta)^2] + E[(\Delta - \hat{\Delta}_DM(T_2, C_2))^2] + 2E[(\Delta - \hat{\Delta}_DM(T_2, C_2))(\hat{\Delta}_A(T_1, C_1) - \Delta)] =$$

$$E[(\Delta - \hat{\Delta}_DM(T_2, C_2))^2] = \sum_{i \in I} M_i \Delta_i E[D_{E,i}(T_1, C_1)],$$

(16)

where the cancellation uses the independence of the first/second folds of data to factor the expectation over the two terms, and the unbiased estimation property of the DM estimator over the second fold (Rubin, 2005). We then obtain the following variances for two estimators $A$ and $B$:

$$E[(\hat{\Delta}_A(T_1, C_1) - \Delta)^2] - E[(\Delta - \hat{\Delta}_DM(T_2, C_2))^2] =$$

$$E[(\Delta - \hat{\Delta}_DM(T_2, C_2))^2] = \sum_{i \in I} M_i \Delta_i E[D_{E,i}(T_1, C_1)],$$

(17)

(18)

from which the claim follows.

Next, we present the proof of Theorem 2.

**Proof** [Proof of Theorem 2] The proof is very much in keeping with 1 and relies on the unbiasedness of $\hat{\Delta}_{DM,i}$ and independence of the two splits $(T_1, C_1)$ and $(T_2, C_2)$:

$$E[\hat{F}_E] = \sum_{i \in I} M_i \hat{\Delta}_{DM,i}(T_2, C_2) E[D_{E,i}(T_1, C_1)] = \sum_{i \in I} M_i E[\hat{\Delta}_{DM,i}(T_2, C_2)] E[D_{E,i}(T_1, C_1)]$$

$$= \sum_{i \in I} M_i \Delta_i E[D_{E,i}(T_1, C_1)],$$

(19)

as claimed.

B. Additional Results

First we present several additional estimator histograms.

In this section we present additional results from our aggregation methodology to explore their stability under using different bootstrapped train/test splits to compute the normalized score vectors $\hat{A}$ and $\hat{B}$. Tables 2 and 3 show consistent results.

C. Cross-Validation Methodology

The cross-validation methodologies described in Section 3 and Section 4 are for the most part intuitive; nonetheless, it is worthwhile to present all the details of how we partition $T$ and $C$ as well

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10. Throughout we also implicitly use the fact the subfolds are (uniformly) randomly sampled from the treatment and control groups—so the expectation over the subfold is equivalent to the expectations over the entire treatment/control groups.
as how we repeat the procedure to cross-validate (3) and (9). We start with a formal definition of treatment $T$ and control $C$ groups. Let $i$ be some lab in $I$; then, the treatment group for this lab is the set of outcomes $Y_{i,a}$ and features $X_{i,a}$ for each product $a$ under the in the treatment arm $T_{i,a} = 1$, that is $T_i = \{(Y_{i,a}, X_{i,a})|T_{i,a} = 1\}$ where $|T_i| = K_i$ is the number of products that were assigned to the treatment arm $T_{i,a} = 1$. Similarly, the control group is given by $C_i = \{(Y_{i,a}, X_{i,a})|T_{i,a} = 0\}$ where $|C_i| = M_i - K_i$ is the number of products assigned to the control arm $T_{i,a} = 0$ and $M_i$ is the total number of products in the lab.
Table 2: Comparison of Estimators via one-sample t-test applied to their normalized score vector. This table was computed using error vectors from only 50 resampled train/test splits to feed into A and B. Easiest to read row-wise. The index \((A, B)\) of the table computes the pair of the \((t\)-statistic, \(p\)-value) associated with the score \(S(\hat{A}, \hat{B})\). A large positive \(t\)-statistic at index \((A, B)\) indicates estimator \(A\) is better then estimator \(B\) and vice versa.

| Method     | \(\text{dim}\) | \(\text{mom1000}\) | \(\text{gen_dd}\) | \(\text{gen_dd_w1}\) | \(\text{gen_dd_w_norm}\) | \(\text{mom1000}\) | \(\text{gen_dd}\) | \(\text{gen_dd_w1}\) | \(\text{gen_dd_w_norm}\) |
|------------|----------------|---------------------|-------------------|---------------------|---------------------|----------------|----------------|----------------|----------------|
| \(\text{dim}\) | \(x\)          | \(-12.4, 0.000000\) | \(-20.4, 3.05e-73\) | \(-20.4, 3.05e-73\) | \(-22.1, 5.73e-83\) | \(-17.7, 8.54e-38\) | \(-25.1, 2.11e-100\) | \(-24.9, 2.95e-99\) | \(-27.5, 5.03e-114\) |
| \(\text{mom1000}\) | \(-12.4, 0.000000\) | \(-20.4, 3.05e-73\) | \(-20.4, 3.05e-73\) | \(-22.1, 5.73e-83\) | \(-17.7, 8.54e-38\) | \(-25.1, 2.11e-100\) | \(-24.9, 2.95e-99\) | \(-27.5, 5.03e-114\) |
| \(\text{gen_dd}\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) |
| \(\text{gen_dd_w1}\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) |
| \(\text{gen_dd_w_norm}\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) | \(-20.0, 3.05e-73\) |

Table 3: Comparison of Estimators via one-sample t-test applied to their normalized score vector. This table was computed using error vectors from only 50 resampled train/test splits to feed into \(\hat{A}\) and \(\hat{B}\) distinct from those in previous table. Easiest to read row-wise. The index \((A, B)\) of the table computes the pair of the \((t\)-statistic, \(p\)-value) associated with the score \(S(\hat{A}, \hat{B})\). A large positive \(t\)-statistic at index \((A, B)\) indicates estimator \(A\) is better then estimator \(B\) and vice versa.

The goal of our methodology is to find an optimal estimator \(\hat{\Delta}\), for the ATE \(\Delta\), or an optimal roll out policy \(D_i\) under some objective function \(L\). This means finding a function of \(T\) and \(C\) such that \(f(T, C) \in \mathbb{R}\) or \(f(T, C) \in \{0, 1\}\) respectively, and that optimizes the expected objective:

\[
\mathbb{E}[L(f(T, C), \Delta)]
\]

for \(f\) in some functional space \(\mathcal{F}\). As discussed in Section 3 and Section 4, to do this in the context of an RCT where \(\Delta\) is unknown, we rely on the fact that the difference-in-means estimator \(\hat{\Delta}(T, C)\) is unbiased for the ATE \(\Delta\). Specifically, for any lab \(i\), we randomly split the treatment and control group using two random subsets of product indices \(S_i = \{1, \ldots, K_i\}\) and \(R_i = \{K_i + 1, \ldots, M_i\}\) so that we end up with the four following sets:

- \(T_{i,1} = \{(Y_{i,a}, X_{i,a}) | T_{i,a} = 1 \text{ and } a \in S_i\}\)
- \(C_{i,1} = \{(Y_{i,a}, X_{i,a}) | T_{i,a} = 0 \text{ and } a \in R_i\}\)
- \(T_{i,2} = \{(Y_{i,a}, X_{i,a}) | T_{i,a} = 1 \text{ and } a \notin S_i\}\)
- \(C_{i,2} = \{(Y_{i,a}, X_{i,a}) | T_{i,a} = 0 \text{ and } a \notin R_i\}\).

We also pick the size of \(S_i\) and \(R_i\) so that the split proportion \(p\) is constant across treatment, control, and labs:

\[
\frac{|S_i|}{K_i} = \frac{|R_i|}{M_i - K_i} = p.
\]
With this splitting methodology, we can now replace (20) with the empirical mean of the objective over all the labs in $\mathcal{I}$:

$$
\frac{1}{|\mathcal{I}|} \sum_{i \in \mathcal{I}} L(f(T_i,1,C_i,1), \hat{\Delta}_{DM}(T_i,2,C_i,2)).
$$

We can now optimize empirical objective for $f$ similarly to empirical risk minimization for supervised learning. We can also “cross-validate” the empirical mean of the objective to reduce the subsampling variance and to get confidence intervals, as in Fig. 7. To do this we simply repeat the splitting procedure multiple times so that every random index set $S_i$ and $R_i$ is now also indexed by a split $b \in \{1, \ldots, B\}$. Putting all of this together, we now have:

$$
\frac{1}{|\mathcal{I}|} \sum_{i \in \mathcal{I}} \frac{1}{B} \sum_{b=1}^{B} L(f(T_{i,b,1},C_{i,b,1}), \hat{\Delta}_{DM}(T_{i,b,2},C_{i,b,2})).
$$

This is how we estimated (3) and (9) in the paper, using $p = 0.5$ and $B = 100$. It is worth noting that in the case of (3), we ended up replacing the outer sum of (21) with the aggregation methodology of Section 3.1 to deal with the heavy-tailed nature of $M_i$, i.e. to ensure that the largest labs did not dominate the value of (21).

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