Heavy User Effect in A/B Testing: Identification and Estimation

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ABSTRACT
On-line experimentation (also known as A/B testing) has become an integral part of software development. To timely incorporate user feedback and continuously improve products, many software companies have adopted the culture of agile deployment, requiring online experiments to be conducted and concluded on limited sets of users for a short period. While conceptually efficient, the result observed during the experiment duration can deviate from what is seen after the feature deployment, which makes the A/B test result highly biased. While such bias can have multiple sources, we provide theoretical analysis as well as empirical evidence to show that the heavy user effect can contribute significantly to it. To address this issue, we propose to use a jackknife-resampling estimator. Simulated and real-life examples show that the jackknife estimator can reduce the bias and make A/B testing results closer to our long term estimate.

KEYWORDS
A/B testing, external validity, jackknife, block bootstrap, causal inference

1 INTRODUCTION
1.1 Background
In the big data era, A/B testing has become the gold standard for evaluating websites, desktop and mobile applications, on-line services and operating systems, and has been widely utilized by the information technology industry [6, 7, 9, 12]. For example, the ExP platform at Microsoft [10–12] supports experimentation in Bing, MSN, Cortana, Skype, Office, XBox, Windows, Edge Browser and more, running over ten thousand experiment treatments per year. In typical online controlled experiments, when users start using a product or service (e.g., search engine), they will be randomly assigned to the new experience (treatment) or existing experience (control), and the assignment typically remains consistent throughout the experiment. During the experimental period, we collect telemetry from all users, aggregate them into metric values, and conduct statistical tests to detect differences in metrics values between the treatment and control groups which are unlikely to be observed due to random chance.

1.2 Motivation and related work
The attractiveness of controlled experiments comes from their abilities to establish causal relationships between the features being tested and the measured changes in user behaviors [8, 19]. One key touchstone of trustworthiness of experimentation is external validity [1–3, 20] – can the results observed during an experiment period still hold when the new feature being tested is rolled out to the entire user population in the future? Obviously, there can be multiple factors that affect external validity. For example, novelty effect might jeopardize external validity: after a new feature is presented to the users, if the users are unfamiliar with the new feature, they might change their behavior out of curiosity but gradually go back to their normal behavior. For another example, weekday/weekend effect could affect external validity: if users have distinct behaviors on weekdays and weekends, an A/B test shorter than a week would yield a biased result.

While some effects are well recognized and accounted for, there could still be other neglected yet common effects that play an important role in determining external validity. In this paper we highlight heavy user effect, which affects external validity significantly. To the best of authors’ knowledge, this effect has not been discussed much and its crucial relationship to external validity is largely ignored. Yet, this effect turns out to be very common in online experimentation settings. Roughly speaking, heavy user effect describes the phenomenon that heavy users are more likely to be included in an experiment than light users, rendering the estimated average treatment effect biased towards heavy users. To illustrate this point, let’s consider a simple example. Say a website has two hundred users, half are heavy users who use the website every day, and the other half are light users who use the website with 50% probability each day. Each user pays the website one dollar each day, if and only if the user uses the website. Assume that there is a new feature, which would let heavy users pay one more dollar to the website each day but has no effect on light users. Then, if deployed, this new feature would generate an increment of one hundred dollars each day. In other words, the average treatment effect is fifty cents per user. However, in a one-day online experiment, there would only be around 150 users using the website, resulting a .67 dollars per user estimated average treatment effect, which overestimates
As we will show in Section 2.2, experiments of limited lengths A/B testing, the user base can change, e.g., new users appear and old users disappear, or user types can switch gradually. Thus, it is hard to determine the size of the user population as well as users’ activeness based on the historical data.

It is worth emphasizing that, although heavy user effects exist ubiquitously, whether it causes severe bias in treatment effect estimation heavily depends on how we define and measure the treatment effect (e.g., metric type). In the aforementioned example, if we measure the treatment effect using the relative difference instead of the absolute difference, a one-day A/B test shows that the revenue increases by 66.7%, which is consistent with the true effect.

We are motivated by our day-to-day analyses of A/B tests from different perspectives, one main objective of this paper is to quantitatively characterize the extent of heavy user bias in various scenarios.

### 1.3 Scope and contribution

As we will show in Section 2.2, experiments of limited lengths tend to favor heavy users, and therefore the test results biased. To the best of our knowledge, this problem has not been thoroughly discussed in the existing data mining literature, and therefore we hope to analyze this issue rigorously and make the following contributions:

1. Proving the existence of heavy user biases in A/B tests when there is treatment effect heterogeneity between heavy users and light users, and quantifying such bias by deriving its closed-form expression under certain assumptions, and showing that it is inversely proportional to the length of the experiment;
2. Proposing an intuitive and easy-to-implement estimator based on jackknife [13, 15, 17, 18, 25, 26], which can asymptotically correct the first-order heavy user bias in general, and demonstrating the effectiveness of the proposed estimator through both simulated and real-life examples;
3. Providing practical guidance on how to apply the jackknife estimator on various types of metrics with different aggregation levels.

The rest of the paper is organized as follows. In Section 2, we introduce a framework of analyzing A/B tests and discuss some practical details, e.g., the metric types, what users to include, and how to measure the difference between the treatment group and the control group. Under the proposed framework, we discuss the unique challenges of external validity in A/B testing scenarios. In Section 3, we formally define heavy user bias within our framework, derive its closed-form expression under a fixed population user model, and propose a jackknife re-sampling estimator to adjust said bias. Section 4 and 5 examines the performance of our proposed estimator via simulated and real-life examples. Section 6 concludes the paper and discusses future directions. We relegate all the proofs and other technical details to the Supplementary materials.

## 2 A FRAMEWORK FOR A/B TESTING

### 2.1 Notations and assumptions

The general idea of A/B testing is simple. Assume engineers develop a new feature of the product and now we would like to evaluate how that feature will affect the user behavior. Ideally, we would like to know how users react without (A) and with the feature (B). However, we can either observe the user behavior under setting A or setting B but it is not possible to observe both simultaneously. Therefore, we need to do randomized trials and put each user randomly in either treatment or control. Then we collect data from those users and analyze the data. A more detailed introduction of A/B testing can be found in [11, 12]. To make the discussion concrete, we first introduce the following notations.

| Notation | Explanation |
|----------|-------------|
| $Y_u(t)$ | the observed outcome of user $u$ at day $t$. |
| $Z_u$ | an indicator whether the user $u$ is in treatment. |
| $R_u(t)$ | whether the user $u$ used the product at day $t$. |
| $t_u^0$ | the first time user $u$ shows up, i.e. $\min\{t \mid R_u(t) = 1\}$ |
| $\tau_u(t)$ | treatment effect for user $u$ at day $t$. |
| $c_u(t)$ | control outcome for user $u$ at day $t$. |
| $k$ | duration of the experiment (from day 1 to day $k$). |
| $N_T(k)$, $N_C(k)$ | number of users in treatment/control in the test. |

Note that the number of users in treatment/control group $N_T(k)$, $N_C(k)$ are functions of the experiment duration $k$. However, for notation ease, we omit $k$ and use $N_T$, $N_C$ in later sections. Throughout this paper, we make the following assumptions.

**Assumption 1 (stable unit treatment value assumption).**

One user’s outcome is unaffected by other users’ treatment assignments. In other words, different users do not interfere with each other.

Under Assumption 1, the potential outcomes of any user only depend on its own treatment assignment but not others’. This assumption is reasonable when there are no user interactions. For example, this assumption likely hold for search engines like Bing and Google, since each user does not interact with other users. However, this assumption could break when users can interact and communicate, such as users of Facebook or MSN. Although the heavy user effect still exists, the current framework could not handle those scenarios. It is an interesting direction to study how the heavy user effect could affect A/B tests when Assumption 1 breaks but that is not the focus of this paper.

**Assumption 2 (super population).** For each user, its behavior can be characterized as a series of triplets $(R_u(t), \tau_u(t), c_u(t))_{t=1}^T$. We assume that this series for each user is an i.i.d. sample from a super population with probability distribution $\Psi$:

$$P[R_u(t) = a_t, \tau_u(t) \leq b_t, c_u(t) \leq c_t; t = 1, \ldots, k],$$

where $a_t \in \{0, 1\}$, $b_t, c_t \in \mathbb{R}$ for $t = 1, \ldots, k$.

The super population model is a commonly used model in causal inference literature [16]. Its key feature is that it assumes that the...
potential outcome of each user is random. A natural consequence of the super population model is that the observed outcome of each user is independent. The super population model gives a good approximation in large scale A/B testing scenarios where the number of users is usually at the scale of millions. The super population model also makes the mathematical analysis more readable and intuitive. A more detailed comparison between these two frameworks could be found in [4].

Throughout the paper, we refer to user activity as the act of user using the product. We call a day \( t \) as an active day for user \( u \) if \( R_u(t) = 1 \). Then we have the following assumption regarding \( R_u(t) \) and \( Z_u \):

**Assumption 3 (incremental experiment assumption).** We assume that for each user \( u \), the activity indicator \( R_u(t) \) is independent of the treatment assignment \( Z_u \).

This assumption implies that user’s visit \( R_u(t) \) is not affected by whether a user is treated. In other words, we assume our experiment is incremental such that it does not change the frequency of users’ visits. This assumption could bring issues if a treatment significantly moves the number of days a user is active (i.e. uses the product). Therefore, it is important to test this assumption before analyzing the data using this framework. Fortunately, this assumption is not hard to test. One plausible way would be to test whether the average active days per user is the same across treatment and control group.

Under Assumptions 1–3, the observed outcome of a user is

\[
Y_u(t) = R_u(t)(Z_uT_u(t) + c_u(t)).
\]

There are several caveats about this model. First, although a unit is called “a user” in this paper, depending on the actual application scenario, it could be a user, a device, or a browser cookie. Similar ideas apply to \( R_u(t) \). Although \( R_u(t) \) is defined to be whether a user user the product at day \( t \) in this paper, it could also be thought of as any other events, such as whether a user viewed a specific page. Second, when a user did not use the product, \( Y_u(t) \) would be zero. That means there are two scenarios where \( Y_u(t) \) can be zero. The first scenario is that a user did not use the product. The second scenario is that a user used the product but has outcome zero. We can tell the difference between these two scenarios by looking at \( R_u(t) \). Therefore, setting \( Y_u(t) = 0 \) when \( R_u(t) = 0 \) will not cause any confusion.

### 2.2 External validity

External validity, also known as generalizability [22], refers to the problem about how to generalize the findings from the units in the experiment to a larger inference population. External validity is an important problem in causal inference and several papers studied the external validity under a variety of different scenarios such as politics [22] and education [23, 24].

The external validity of A/B tests could be affected by a variety of effects other than heavy user effects. For example, there could be novelty or primacy effects [21], where a user’s treatment effect is affected by its length of exposure to the product. For novelty effects, a user’s treatment effect could be high at first but as time goes on, its treatment effect will approach to zero. For primacy effects, the user’s treatment effect is low at first but as time goes on, its treatment effect will be higher. To avoid the novelty effect, we could either run the experiment long enough so that the novelty effect goes away or we could adopt the approach of “reverse” A/B tests, exposing all the users to the new feature for a long time and then run an experiment by exposing users to the original feature. Intuitively, reverse A/B tests eliminate the novelty/primacy effect of the new feature and would provide a more accurate estimate of the long term effect. For another example, there could be weekday/weekend effects: Users’ treatment effect during weekdays and weekends are different. To avoid the weekday/weekend problem, we could set the experiment duration to be multiples of a week. While those effects are well recognized and have some quick fixes, there could be many overlooked factors affecting external validity that would be crucial to take into account to make A/B testing results more trustworthy.

In A/B testing, a crucial question is what users should be included in the experiment. Users might appear at different days while the experiment duration is usually limited. Microsoft’s ExP platform uses continuous analysis. In continuous analysis, we include every user who visits the product during the experiment period. If a user visited at the first day of the experiment, the analysis will include that user’s data till the end of the experiment. In contrast, if a user did not appear until the last day of the experiment, the analysis will include the data of that user for the last day. The advantage of continuous analysis is “no data left behind,” where we used all the user data during the experiment period at hand.

### 2.3 Metrics

Metrics are the quantities that are of interest for people to make decisions based on A/B testing. Depending on the particular feature and the goal of the decision makers, the metrics of interest could be quite different. For example, Click-through rate (CTR) is a common metric for search engines to measure the effectiveness of online advertisements. While many different types of metrics can be computed for analyzing A/B tests, we list the definitions of the metrics types that are most commonly used:

- **Single average metric** is the sum of observed daily outcomes per user in the treatment/control group:

  \[
  \mathbb{E} \left\{ \sum_{t=1}^{k} Y_u(t) \mid Z_u = z \right\} \quad (z = 1, 0),
  \]

  where the expectations are with respect to the data generating mechanism in (1). A common example is revenue per user. For this metric, the average treatment effect (ATE) is the difference between the metric for the treatment group and that for the control group:

  \[
  \Delta_{\text{single}} = \mathbb{E} \left\{ \sum_{t=1}^{k} R_u(t)T_u(t) \right\}.
  \]

  Since ATE is the difference between the two groups, we also call it *delta*. It is worth mentioning that, Assumption 3 plays a crucial role in deriving (3).

  For non-negative responses (e.g., ad revenue), this ATE is always increasing with duration \( k \). That will make it harder to compare across different durations of an experiment (different \( k \)). One way to mitigate this issue is to...
consider the delta of the scaled single average metric:
\[
\Delta_{\text{scaled}} = \mathbb{E}\left\{ \frac{1}{k} \sum_{t=1}^{k} R_u(t) Y_u(t) \right\}.
\]
Another solution is the percentage delta – the ratio between the delta and the metric values for the control group:
\[
\%\Delta_{\text{scaled}} = \mathbb{E}\left\{ \frac{1}{k} \sum_{t=1}^{k} R_u(t) Y_u(t) \right\} / \mathbb{E}\left\{ \sum_{t=1}^{k} R_u(t) c_u(t) \right\}.
\]
- Double average metric is the total metric sum per user per active day in the treatment/control group:
\[
\mathbb{E}\left\{ \frac{\sum_{t=1}^{k} Y_u(t)}{\sum_{t=1}^{k} R_u(t)} Z_u = z \right\} \quad (z = 1, 0).
\]
Examples of double average metrics are CTR and revenue per user per active day. The ATE (or delta) of this metric is the difference between treatment and control:
\[
\Delta_{\text{double}} = \mathbb{E}\left\{ \frac{\sum_{t=1}^{k} R_u(t) Y_u(t)}{\sum_{t=1}^{k} R_u(t)} \right\}.
\]
Again, Assumption 3 plays a crucial role in ensuring the validity of (6). Note that this metric does not increase with \(k\), so we do not need to re-scale. Similarly, we define the percentage delta of the double average metric to be:
\[
\%\Delta_{\text{double}} = \mathbb{E}\left\{ \frac{\sum_{t=1}^{k} R_u(t) Y_u(t)}{\sum_{t=1}^{k} R_u(t)} \right\} / \mathbb{E}\left\{ \sum_{t=1}^{k} R_u(t) c_u(t) \right\}.
\]

3 METHODOLOGY

3.1 Point estimation

We present the point estimators of the (percentage) deltas of the aforementioned metrics using the simple difference-in-means estimator. Due to practical importance, we focus on:
- The scaled single average metric. We estimate its delta (i.e., \(\Delta_{\text{scaled}}\)) by the corresponding difference-in-sample-means:
\[
\omega_{\text{scaled}} = \frac{\sum_{u,Z_u=1}^{k} Y_u(t) - \sum_{u,Z_u=0}^{k} Y_u(t)}{kN_T}.
\]
Similarly, we estimate its percentage delta by
\[
\%\omega_{\text{scaled}} = \omega_{\text{scaled}} / \left( \sum_{u,Z_u=0}^{k} Y_u(t) / (kN_C) \right);
\]
- The double average metric. We estimate its delta (i.e., \(\Delta_{\text{double}}\)) by the corresponding difference-in-sample-means:
\[
\omega_{\text{double}} = N_T^{-1} \sum_{u,Z_u=1}^{k} Y_u(t) - N_C^{-1} \sum_{u,Z_u=0}^{k} Y_u(t).
\]
Similarly, we estimate its percentage delta by
\[
\%\omega_{\text{double}} = \omega_{\text{double}} / \left( \sum_{u,Z_u=0}^{k} Y_u(t) / N_C \right).
\]

3.2 Bias of an A/B test

In general, the expectations of the aforementioned point estimators do not equal the corresponding (percentage) deltas. We define bias of an A/B test to be the difference between expectation of the point estimators and the corresponding metric. To be more specific:
- For scaled single average metric, because only users who appear between day 1 and day \(k\) will be included in the experiment, the expectation of the estimated delta is:
\[
\mathbb{E}\left\{ \omega_{\text{scaled}} \right\} = \frac{1}{k} \sum_{t=1}^{k} \mathbb{E}\left\{ \tau_u(t) R_u(t) \right\} \left\{ \sum_{t=1}^{k} \tau_u(t) \right\};
\]
and the corresponding bias is \(\mathbb{E}\left\{ \omega_{\text{scaled}} - \Delta_{\text{scaled}} \right\}\). Similarly, to calculate the bias of percentage delta \(\%\omega_{\text{scaled}}\), we approximate its expectation by
\[
\mathbb{E}\left\{ \omega_{\text{scaled}} \right\} = \frac{1}{k} \sum_{t=1}^{k} \mathbb{E}\left\{ \tau_u(t) R_u(t) \right\} \left\{ \sum_{t=1}^{k} \tau_u(t) \right\} \left\{ \sum_{t=1}^{k} \tau_u(t) \right\}.
\]

3.3 User model

Before presenting the user model, we first briefly discuss the rationale behind it. For long-standing on-line applications and services, the user population tend to change slowly over time. Therefore, we assume a fixed population during the experiment length. Within the population, we assume that there exists user heterogeneity, for both activity frequency and responses. The model below demonstrates the point that heavy user bias still exists and therefore needs to be considered.

Model 1 (Fixed population with user heterogeneity). In many experiments, users’ treatment effect and their activity can be

\footnote{This is only an approximation because, in general the expectation of the ratio of two random variables is not the same as the ratio of their expectations. However, this approximation is accurate when both random variables have small variances [18]. In our case, because the numbers of users in both treatment and control are large, the variances of both the numerator and denominator of \(\%\omega_{\text{scaled}}\) tend to be small.}
correlated, e.g. heavy user and light users react differently. We use the following model to reflect the heterogeneity:

- \( R_u(t) \) for a user \( u \) on day \( t \) is i.i.d. from a Bernoulli random variable with success probability \( p \sim f(\cdot) \).
- The expectation of the treatment effect for user \( u \) is \( \mathbb{E}_{\tau_0}(t) = \tau(p) \). It implies that the treatment effect could be different for users with different activity parameter \( p \) but remains the same across all days.
- The expected control outcome of a user is \( \mathbb{E}_{c_u}(t) = c(p) \).

While we acknowledge that the above model may contain strong assumptions, our purpose is to illustrate the existence of heavy user biases and provide baselines/heuristics in empirical examples. Under this model the delta of the scaled single average metric in (4) could be written as:

\[
\Delta_{\text{scaled}} = \int_{0}^{1} \tau(p)p f(p) dp.
\] (11)

On the other hand, based on (7), the expectation of the scaled single average metric (7) in continuous analysis would be (detailed proof in the Appendix):

\[
\mathbb{E}\left(\hat{\Delta}_{\text{scaled}}\right) = \int_{0}^{1} \frac{\tau(p)p f(p) dp}{f(\cdot)c(p)\int_{0}^{1} c(p) f(p) dp}.
\] (12)

Intuitively, the expectation of the point estimate \( \mathbb{E}\left(\hat{\Delta}_{\text{scaled}}\right) \) differs from the actual delta \( \Delta_{\text{scaled}} \) because light users are less likely to show up during the experiment duration therefore the ATE on heavy users has been exaggerated. If we can run the experiment for very long (\( k \to \infty \)), then

\[
\lim_{k \to \infty} \mathbb{E}\left(\hat{\Delta}_{\text{scaled}}\right) = \int_{0}^{1} \tau(p)p f(p) dp = \Delta_{\text{scaled}}.
\]

However, for a finite period \( k \), the expectations are different. This is because different users have different probability of being included in the experiment. In particular, heavy users tend to have higher chances being included, and light users tend to have lower chances. To be more specific, the probability that a user with activity probability \( p \) shows up during the experiment is \( 1 - (1 - p)^k \), which is increasing with respect to \( p \). Therefore, the sample tends to have more heavy users than light users compared to the whole population. Similarly, we could analyze the heavy user bias of percentage delta. The following proposition shows that, for scaled single average metric, the heavy user biases of the delta and the percentage delta under the aforementioned model are \( O(k^{-2}) \) and \( O(k^{-2}) \), respectively.

**Proposition 1.** Under Model 1, if \( f(\cdot), \tau(\cdot) \) has gradient and their gradients are continuous, i.e. \( f(\cdot), \tau(\cdot) \in C^1([0, 1]) \), then the heavy user bias of the delta of a scaled single average metric is

\[
\mathbb{E}\left(\hat{\Delta}_{\text{scaled}}\right) - \Delta_{\text{scaled}} = \Delta_{\text{scaled}} \frac{\int_{0}^{1} \tau(0)p f(p) dp}{\int_{0}^{1} c(p) f(p) dp} k^{-1} + O(k^{-2}).
\]

Furthermore, if \( f(\cdot), \tau(\cdot), c(\cdot) \in C^1([0, 1]) \) and the control outcome \( c(\cdot) \) is positive, then the heavy user bias of the percentage delta is

\[
\mathbb{E}\left[\frac{\Delta_{\text{double}}}{\sum_{k=1}^{k} R_u(t)c_u(t)}\right]_{0 \leq k} - \Delta_{\text{double}} = \frac{\int_{0}^{1} \tau(0)p f(p) dp}{\int_{0}^{1} c(p) f(p) dp} k^{-1} + O(k^{-2}).
\] (13)

**Remark 1.** When there is no extremely light users \( f(0) = 0 \), it can be seen from the proposition that the first order bias of continuous analysis would be zero. Note that \( f(0) \) is not the proportion of users who has activity probability 0. If a user has probability 0 of showing up, it will never appear in the experiment. \( f(0) \) should be thought of as the limit \( \lim_{q \rightarrow 0} P(p \leq q)/q \), which (approximately) represents users with very few activities.

**Remark 2.** The heavy user bias of the percentage delta is of higher order to that of the delta, implying that it is a more robust estimate in the presence of user heterogeneity.

**Proposition 2.** If \( f(\cdot), \tau(\cdot), c(\cdot) \in C^1([0, 1]) \), then the heavy user bias of the delta of a double average metric is

\[
\mathbb{E}\left[\hat{\Delta}_{\text{double}}\right] - \Delta_{\text{double}} = \left[\Delta_{\text{double}} - \tau(0) f(0) \cdot k^{-1} + O(k^{-2})\right] .
\]

Furthermore, if \( f(\cdot), \tau(\cdot), c(\cdot) \in C^1([0, 1]) \) and the control outcome \( c(\cdot) \) is positive, the heavy user bias of the percentage delta is

\[
\mathbb{E}\left[\frac{\Delta_{\text{double}}}{\sum_{k=1}^{k} R_u(t)c_u(t)}\right]_{0 \leq k} - \Delta_{\text{double}} = \frac{\int_{0}^{1} \tau(0)p f(p) dp}{\int_{0}^{1} c(p) f(p) dp} k^{-1} + O(k^{-2}).
\]

**Remark 3.** While the heavy user bias of the percentage delta of single average metric is \( O(k^{-2}) \), that of the percentage delta of a double average metric is \( O(k^{-2}) \) unless \( \tau(0) = c(0)\Delta_{\text{double}}/\int_{0}^{1} c(p) f(p) dp \).

One special case when these two will equal is that there is no heterogeneous relative treatment effect, i.e. \( \tau(p)/c(p) \) is a constant. In that case, \( \Delta_{\text{double}}/\int_{0}^{1} c(p) f(p) dp = \tau(0)/c(0) \).

To end this section, we use Table 1 to summarize our findings.

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**Table 1: External Biases under Model 1.** All \( O(k^{-2}) \) terms are omitted to save space.

|               | \( \Delta \) | \%\( \Delta \) |
|---------------|--------------|---------------|
| scaled average | \( \frac{\int_{0}^{1} \tau(0)p f(p) dp}{\int_{0}^{1} c(p) f(p) dp} k^{-1} + O(k^{-2}) \) | \( \tau(0)\Delta_{\text{double}}/\int_{0}^{1} c(p) f(p) dp \) |
| double average | \( \frac{\int_{0}^{1} \tau(0)p f(p) dp}{\int_{0}^{1} c(p) f(p) dp} k^{-1} + O(k^{-2}) \) | \( \tau(0)\Delta_{\text{double}}/\int_{0}^{1} c(p) f(p) dp \) |

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### 3.4 Jackknife estimators

In the previous section we showed the existences of heavy user biases even under a quite simple model. To the best of our knowledge, our paper is the first to point out this phenomenon using a rigorous mathematical model. Then the natural question to ask would be how to correct the bias. If we know the underlying user behavior model and the treatment effect model, we could use maximum likelihood estimation (MLE) to estimate the parameters and then do the correction. Unfortunately, modeling the user behavior...
can be a harder problem compared to estimating the heavy user bias because user behaviors could be very complex. MLE based method might not be robust if the assumed model deviated from the real model. It is also not straightforward to extend the MLE base methods to different metric types or different user models. That motivates us to consider a non-parametric method that has good performance for simple models and also has potential to extend to cases where the models don’t necessarily hold. If the MLE based method is not desirable, what other methods could be applicable? It turns out that we can treat this question as a time series prediction problem. For any fixed experiment duration \( k \), let \( h(k) \) denote the expected cumulative effect of some metric. Recall our theoretical analysis in Proposition 1 and 2, if the data is generated from Model 1, for scaled single average metric and double average metric, \( h(k) \) can be approximated using a parametric formula, \( h(k) = a + b/k \), where \( a, b \) are two parameters whose specific values depend on the metric types and the model parameters \( r(\cdot), f(\cdot) \). The heavy user bias for \( k \) is \( \frac{b}{k} \) and the true effect is \( a \). If we have an unbiased estimator for \( h(k-1) \) and \( \tilde{h}(k) \), denoted as \( \hat{h}(k-1) \) and \( \tilde{h}(k) \), i.e. \( \mathbb{E} \tilde{h}(k-1) = h(k-1) \) and \( \mathbb{E} \tilde{h}(k) = h(k) \), then we could get an unbiased estimator for \( a \):

\[
\hat{a} = k \cdot \tilde{h}(k) - (k - 1) \cdot \tilde{h}(k-1).
\]

It is not hard to show that \( \mathbb{E} \hat{a} = a \). Then it boils down to get reasonable estimators \( \hat{h}(k-1) \) and \( \tilde{h}(k) \). For a metric \( F \), let’s consider its difference-in-means estimator as this is the only estimator we use in this paper. The difference-in-means estimator using all the data is a good choice of \( \tilde{h}(k) \): \( \tilde{h}(k) = F(\{(R_u(t), Y_u(t), Z_u) | Z_u = 1, t \in [1, k]\}) - F(\{(R_u(t), Y_u(t), Z_u) | Z_u = 0, t \in [1, k]\}) \). For \( \hat{h}(k-1) \), we can use the difference-in-means estimator using the data of first \( k-1 \) days: \( \hat{h}(k-1) = \hat{F}(\{(R_u(t), Y_u(t), Z_u) | Z_u = 1, t \in [1, k-1]\}) - \hat{F}(\{(R_u(t), Y_u(t), Z_u) | Z_u = 0, t \in [1, k-1]\}) \).

**Algorithm 1** jackknife re-sampling estimator for a metric \( F \)

1. \( \hat{\Lambda} \leftarrow F(\{(R_u(t), Y_u(t), Z_u) | Z_u = 1, t \in [1, k]\}) - F(\{(R_u(t), Y_u(t), Z_u) | Z_u = 0, t \in [1, k]\}) \).
2. For \( j = 1, \ldots, k - T + 1 \) do
3. Get new data sets by excluding data between day \( j \) to \( j + T - 1 \):
4. \( D_1 \leftarrow \{(R_u(t), Y_u(t), Z_u) | Z_u = 1, t \in [1, k]\} \}
5. \( D_2 \leftarrow \{(R_u(t), Y_u(t), Z_u) | Z_u = 0, t \in [1, k]\} \}
6. \( \hat{\Lambda}_{(j)} \leftarrow F(D_1) - F(D_2) \).
7. \( \hat{\Lambda}_{(j)} \leftarrow \frac{1}{k-T+1} \sum_{j} \hat{\Lambda}_{(j)} \).
8. \( \hat{\Delta}_{jack} \leftarrow \frac{k}{k-T} \hat{\Lambda} - \frac{k-T}{k} \sum_{j} \hat{\Lambda}_{(j)} \).
9. return estimated mean \( \hat{\Delta}_{jack} \) and its variance \( \frac{k}{k-T} \sum_{j=1}^{k} \hat{\Delta}_{(j)} - \bar{\Delta}^2 \).

**Proposition 3.** For the delta of scaled single average metric, the heavy user bias of the jackknife re-sampling estimator in Algorithm 1 with \( T = 1 \) is \( \mathbb{E} \hat{\Lambda}_{jack} - \Delta_{scaled} = O(k^{-2}) \), under Model 1.

As mentioned in Proposition 1, because the heavy user bias of the percentage delta for the scaled single average metric is 0, no adjustment would be needed. Similarly, for (percentage) delta of the double average metric, one can show that the corresponding jackknife estimators eliminate the first-order bias.

### 4 SIMULATION STUDIES

In this section, we conduct simulation studies to examine the performance of our proposed methodology. We compare our method with the original continuous analysis and block bootstrap [13], and we consider both the delta and the percentage delta of metrics.

#### 4.1 Simulation 1

The data is generated as the following. For any user \( u \) and any day, it uses the product with probability \( p \). \( p \) is generated from a uniform distribution on \([0, 1]\). If a user uses the product, its outcome would be

\[
Y_u(t) = \begin{cases} 1 + p & \text{if treated} \\
1 + N(0, 0.01^2) & \text{otherwise}
\end{cases}
\]

We have conducted exploratory analyses indicating that jackknife works under more general models (e.g., with time heterogeneity). We did not include such analyses due to space constraint. In addition, we acknowledge that there exists other re-sampling techniques such as bootstrap [5] that might also be useful. In the next section we conduct simulation studies where shows jackknife is more favorable.
The number of people in the treatment is 1000 and the number of people in the control is 1000. The experiment period for the continuous analysis is 14 days (k = 14). We repeat the data generation process 100 times and compute the original difference-in-means estimator and the jackknife adjusted estimator. We compare the estimated average treatment effect in terms of delta and percentage delta. The results are shown in Table 2. It shows that compared to the original difference-in-means estimator, the jackknife re-sampling estimator has a smaller bias with a slightly larger variance in terms of delta. As for percentage delta, since the original estimator already has very small bias, jackknife does not help.

Table 2: Simulation 1 results: The first row contains the underlying truth of the (percentage) delta. The next three rows contain the bias and standard error of the original estimator, our proposed jackknife estimator and the block bootstrap estimator, respectively. The bold results are the best result among three with 90% significance.

|                | \(\Delta_{\text{scaled}}\) | \(\%\Delta_{\text{scaled}}\) | \(\Delta_{\text{double}}\) | \(\%\Delta_{\text{double}}\) |
|----------------|-----------------------------|------------------------------|-----------------------------|------------------------------|
| truth          | 1/3                         | 2/3                          | 1/2                         | 1/2                          |
| bias of original | 0.0200(0.0025)              | -0.15%(0.49%)                | 0.0310(0.0013)              | 2.99%(0.13%)                 |
| bias of jackknife | -0.0022(0.0026)            | 0.04%(0.61%)                 | 0.0044(0.0015)              | 0.34%(0.15%)                 |
| bias of bootstrap | 0.0080(0.0025)             | -0.14%(0.55%)                | 0.0170(0.0014)              | 1.54%(0.14%)                 |

4.2 Simulation 2

In this simulation, we consider how novelty effects affect the Jackknife estimator. For any user \(u\) and any day, its probability of visit at that day is \(p\). \(p\) is generated from a uniform distribution from \([0, 1]\). If a user uses the product someday, its outcome would be

\[
Y_u(t) = \begin{cases} 
1 + (1 + \frac{1}{10}U_{u}(t)) \cdot p + N(0, 0.01^2) & \text{if treated} \\
1 + N(0, 0.01^2) & \text{otherwise}
\end{cases}
\]

where \(U_{u}(t)\) is the number of days the user \(u\) used the product. For example, the treatment effect for a user who used the product in days \(t\) is \(1 + 1/10\); the first term is large and the second term is the novelty effect and goes to zero as \(t\) increases. The number of people in the treatment is 1000 and the number of people in the control is 1000. The experiment period for the continuous analysis is 14 days (k = 14). We repeat the data generation process 100 times and compute the original estimators and the jackknife adjusted estimators. We compare the estimated delta and percentage delta. The results are shown in Table 3. In this case, all methods have higher heavy user biases than in Simulation 1. Nonetheless, Jackknife still gives the smallest bias except for delta percentage of scaled single average. That suggests that Jackknife has reasonable performance even with moderate novelty effects. However, if we further increase the novelty effect to be \(1/U_{u}(t)\) \(10\) times bigger in the simulation, we observe that all the methods break down. That means if the novelty effect is very large, Jackknife is not helpful.

5 EMPIRICAL STUDIES

Based on our experience, heavy user effects are very common in A/B tests. In this section, we pick two experiments recently run on the Microsoft Exp platform that have heavy user effects. We demonstrate how jackknife re-sampling estimators give us some insights regarding those experiments and give better estimates of the average treatment effect.

5.1 An experiment from Bing

We analyze an experiment where the treatment makes changes to the display of search advertisements. This experiment is randomized on User ID (UID). UID is essentially a random string that is written to a browser cookie. In this experiment, we study the metric of actual revenue per user per active day (double average).

Table 3: Simulation 2 results (see Table 2 header for detailed definition of each row and column.)

|                | \(\Delta_{\text{scaled}}\) | \(\%\Delta_{\text{scaled}}\) | \(\Delta_{\text{double}}\) | \(\%\Delta_{\text{double}}\) |
|----------------|-----------------------------|------------------------------|-----------------------------|------------------------------|
| bias of original | 0.0370(0.0020)              | 2.00%(0.49%)                 | 0.0500(0.0012)              | 4.85%(0.11%)                 |
| bias of jackknife | 0.0132(0.0023)            | 2.15%(0.59%)                 | 0.0228(0.0014)              | 2.28%(0.13%)                 |
| bias of bootstrap | 0.0232(0.0022)             | 2.03%(0.52%)                 | 0.0351(0.0013)              | 3.54%(0.12%)                 |
Figure 1: The delta of total revenue per user per active day between treatment and control. The error bar is the 95% confidence interval.

Figure 2: The delta for users with different activity. The error bar is the 95% confidence interval.

Figure 3: The percentage delta for users with different activity. The percentage delta remains roughly the same for users with different activity.

5.2 An experiment from Office

We studied an experiment on the search box functionality in office applications. The treatment uses a magnifier as an icon with the description "Tell me what you want to do". The control uses a light bulb with the description "Search". Screen shots for this experiment can be found in Fig. 4. This experiment was randomized by device and run on all major Office products. In this experiment, we study the proportion of users who clicked the search box.

As shown in Figure 6, the delta is increasing as the experiment duration increases. This is likely due to the heavy user bias. From Figure 7, we see that while the treatment effect for light users is positive, the treatment effect for heavy users is negative. These users are also over represented in the experiment sample for small experiment duration. We have seen that the proportion of users who have at least two active days in a 14 day period rapidly decrease as the experiment period increases. Another observation is that the treatment effect has a clear weekly pattern (Fig. 5). The treatment effect is higher on the weekends compared to weekdays.

Now let’s try to use the jackknife estimator to estimate the long term delta. As the duration of the experiment increases, we expect that the proportion of heavy users to further drop and the delta to increase. As the treatment effect varies on weekdays and weekends, we use jackknife with the parameter \( T = 7 \). The final prediction using jackknife is 0.0015 with standard error (0.0002). The 14 day continuous analysis estimate of the treatment effect is 0.0012 with standard error 0.0001. Since this experiment has actually been running for 5 weeks now, so when we can compare the result with the continuous analysis result using the whole data set, which is 0.0018 with standard deviation 0.0001. Jackknife estimator is closer to the long term delta.

We also repeat this exercise for percentage delta. We see that, there is user heterogeneity in the percentage delta very similar to delta in this case. The jackknife estimate of percentage delta is 5.4% with standard deviation 0.9%. The 14 day continuous analysis estimate of percentage delta is 5.0% with standard deviation 0.3%. The percentage delta using the whole dataset is 5.1% with standard deviation 0.3%. In this case, the jackknife point estimator is actually further away from the 5-week estimator than the 14 day continuous analysis estimator. But the confidence intervals for these three estimators are all wide, implying that we need to run a long-term experiment to compare the three estimators.

Figure 4: Treatment (left) and control (right) diagram.

Figure 5: The outcome under treatment and control for each day in the experiment. During weekends, their difference (delta) is significantly higher.
Heavy User Effect in A/B Testing: Identification and Estimation

6 CONCLUDING REMARKS

We show that heavy user bias can exist in A/B testing due to the limited length of an experiment, and proposed jackknife re-sampling estimators to estimate the bias. We emphasize that heavy user bias could contribute significantly to the violation of external validity. Under user and time heterogeneity models, we show that jackknife estimators could correct the first order heavy user bias. Simulation studies as well as empirical studies demonstrate that jackknife would be a useful tool to estimate such bias.

Although identifying and estimating heavy user effect is an important first step, we acknowledge the need of additional work to achieve the holy grail of ensuring external validity. First, the current “standard” method to estimate the variance of jackknife re-sampling estimator produces a conservative upper bound, and it is important to find a more nuanced approach to calculate the variance more accurately. Bootstrapping the users can be a reasonable alternative, however it is often computationally intensive in practice. Second, as mentioned there can be multiple effects that affect external validity. Using simulated examples, we show that our proposed estimators provided a more accurate estimate of average treatment effect than the naive A/B test result with presence of heavy user effect and even the novelty effect. However, we might need to consider other possible effects in practice. Third, in this paper we assume the users are independent, which is reasonable for many Microsoft products. However, it would be interesting to extend the current study to the network setting where users could interfere with each other. We leave the above for future research.

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APPENDIX: PROOFS

Proof of Proposition 1. Let’s first prove the first equation about delta. As shown in (12), the expectation of $\hat{\Delta}_{\text{scaled}}$ is

$$\mathbb{E}\left\{ \hat{\Delta}_{\text{scaled}} \right\} = \frac{\Delta_{\text{scaled}}}{1 - \int_{0}^{1} f(p)(1-p)^k dp}. \quad (14)$$

Because $f \in C^1[0, 1]$, its gradient is continuous $f' \in C[0, 1]$. Because $[0, 1]$ is compact, $f'$ will be bounded. Denote the bound by $C$. Therefore, we would have the inequality: $|f(p) - f(0)| \leq C \cdot p$. Based on this inequality, we could have a bound for $\int_{0}^{1} f(p)(1-p)^k dp$:

$$\left| \int_{0}^{1} f(p)(1-p)^k dp - \int_{0}^{1} f(0)(1-p)^k dp \right| \leq \int_{0}^{1} C \cdot p(1-p)^k dp$$

$$\iff \left| \int_{0}^{1} f(p)(1-p)^k dp - \frac{f(0)}{k+1} \right| \leq \frac{1}{(k+1)(k+2)} = O(k^{-2}). \quad (15)$$

Based on (14) and (15), we have

$$\mathbb{E}\left\{ \hat{\Delta}_{\text{scaled}} \right\} = \Delta_{\text{scaled}} + f(0)\Delta_{\text{scaled}} k^{-1} + O(k^{-2}).$$

Now, let’s prove the second equation about percentage delta:

$$\mathbb{E}\{\%\hat{\Delta}_{\text{single}}\} = \frac{\mathbb{E}\{\hat{\Delta}_{\text{single}}\}}{\mathbb{E}\{\sum_{t=1}^{k} R_u(t)c_u(t)|c_u^0 \leq k\}} = \frac{\int_{0}^{1} \tau(p)f(p)dp}{\int_{0}^{1} c(p)f(p)dp}$$

It is obvious that $\%\hat{\Delta}$ has no bias. That completes the proof. \hfill \Box

Proof of Proposition 2. Based on formula (9), the expectation of $\hat{\Delta}_{\text{double}}$ would be $E\{\hat{\Delta}_{\text{double}}\} = \int_{0}^{1} \int_{0}^{1} \tau(p)f(p)(1-(1-p)^k)dp$. Compared to the expectation of the scaled single average metric, both the numerator and denominator of this formula do not have term $p$. Then all the rest of the proofs are essentially the same as the scaled single average metric. We leave the rest of proof to the reader. \hfill \Box

Proof of Proposition 3. After deleting one day, the obtained sample can be viewed as a $k-1$ day experiment. Thus we could apply Proposition 1 to obtain its expectation. Using the notation in Algorithm 1, the continuous analysis result after deleting $j$-th day data is denoted $\hat{\Delta}_{(j)}$, by Proposition 1, its expectation is:

$$\mathbb{E}\{\hat{\Delta}_{(j)}\} = \Delta_{\text{scaled}} + \frac{1}{k-1}\Delta_{\text{scaled}} f(0) + O(k^{-2}).$$

Then the expectation of the jackknife estimator would be:

$$\mathbb{E}\{\hat{\Delta}_{\text{jack, scaled}}\} = \mathbb{E}\{\hat{\Delta}_{\text{scaled}}\} - \frac{(k-1)}{k} \sum_{j} \mathbb{E}\{\hat{\Delta}_{(j)}\} = \Delta_{\text{scaled}} + O(k^{-2}).$$

\hfill \Box

Proof of Eq. (12). Based on (7),

$$\mathbb{E}\left\{ \hat{\Delta}_{\text{scaled}} \right\} = \mathbb{E}\{\tau_u(1)R_u(1)|c_u^0 \leq k\} = \int_{0}^{1} \frac{\tau(p)}{1-(1-p)^k} f(p)c_u^0 \leq k) dp. \quad (16)$$

$$= \int_{0}^{1} \frac{\tau(p)}{1-(1-p)^k} f(p)c_u^0 \leq k) dp. \quad (17)$$
where $f(p|\tau^0_u \leq k)$ is the density of the user activity probability $p$ conditioned on $\tau^0_u \leq k$. Using the Bayes' formula, we have

$$
\begin{align*}
f(p|\tau^0_u \leq k) &= \frac{f(p)P(\tau^0_u \leq k|p)}{\int_0^1 f(p)P(\tau^0_u \leq k|p)dp} \\
&= \frac{f(p)(1 - (1 - p)^k)}{\int_0^1 f(p)(1 - (1 - p)^k)dp}.
\end{align*}
$$

(18)

Combining (18) with (17), we could have (12). That completes the proof. □