Algorithms for Adaptive Experiments that Trade-off Statistical Analysis with Reward: Adaptively Combining Uniform Random Assignment and Thompson Sampling

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ABSTRACT
Multi-armed bandit algorithms like Thompson Sampling (TS) can be used to conduct adaptive experiments, in which maximizing reward means that data is used to progressively assign participants to more effective arms. Such assignment strategies increase the risk of statistical hypothesis tests identifying a difference between arms when there is not one, and failing to conclude there is a difference in arms when there truly is one. We tackle this by introducing a novel heuristic algorithm, called TS-PostDiff (Posterior Probability of Difference). TS-PostDiff takes a Bayesian approach to mixing TS and Uniform Random (UR): the probability a participant is assigned using UR allocation is the posterior probability that the difference between two arms is ‘small’ (below a certain threshold), allowing for more UR exploration when there is little or no reward to be gained. We evaluate TS-PostDiff against state-of-the-art strategies. The empirical and simulation results help characterize the trade-offs of these approaches between reward, False Positive Rate (FPR), and statistical power, as well as under which circumstances each is effective. We quantify the advantage of TS-PostDiff in performing well across multiple differences in arm means (effect sizes), showing the benefits of adaptively changing randomization/exploration in TS in a “Statistically Considerate” manner: reducing FPR and increasing statistical power when differences are small or zero and there is less reward to be gained, while exploiting more when differences may be large. This highlights important considerations for future algorithm development and analysis to better balance reward and statistical analysis.

CCS CONCEPTS
• Computing methodologies → Sequential decision making; • Theory of computation → Online learning algorithms.

KEYWORDS
Adaptive Experiments, Multi-armed Bandit Algorithms, Statistical Inference

1 INTRODUCTION
The impact of the web depends on understanding which experiences users consider beneficial, as well as knowing how to use the web to impact and change online behaviour of people. For example, whether version A or B of a website is more engaging [3, 13], which version of an explanation helps people learn [22], or which email causes the reader to take action [26]. Rather than make these decisions solely by intuition, A/B tests or randomized experiments are increasingly being used to gather data by randomizing users with equal probability (e.g. 50/50) to available conditions. Such field experiments raise complex questions regarding when and how to use the data collected to change experiences for future participants.

An increasingly used approach is adaptive experimentation, such as using multi-armed bandit algorithms like Thompson Sampling (TS) [6] to adapt the probability of assigning arm A versus arm B,
based on data on the impact of arms on previous participants. This approach aims to maximize the reward or benefit to the participants. However, issues arise when analyzing data from adaptive experiments. These include measurement bias in arm means [9, 16], lowered statistical power (probability of correctly detecting a difference in arms) and higher false positive rates (FPR, probability of incorrectly concluding that a difference exists) [17, 20]. This paper therefore explores how to modify these adaptive algorithms to better trade-off: (1) statistical analysis and scientific knowledge, against (2) maximizing reward and benefiting users. Specifically, we consider a modification to Thompson Sampling (TS) that increases exploration by adding steps that do uniform random (traditional experiments), or limit how large the probability of assignment to one arm can be.

We present real-world experiments that guide simulation studies, and use these to investigate: (1) A novel heuristic algorithm, called TS-PostDiff (Posterior Probability of Difference). Taking a Bayesian approach, TS-PostDiff approximates the posterior probability that the difference between two arms is ‘small’ (below a certain threshold) and uses this as the probability a new participant will be allocated to an arm using Uniform Random (UR) rather than TS; this allows for more UR exploration when there is little or no reward to be gained by exploiting. (2) Epsilon/Top-Two Thompson Sampling (e/TT-TS; [18]), adding a fixed amount of Uniform Random spread evenly over time. (3) TS-Probability Clipping [27], forcing more exploration by bounding the maximum/minimum allocation probability to each arm.

We find that all the approaches provide an arguably better trade-off than pure Uniform Random or TS. e/TT-TS shows how a fixed UR approach can improve Power and FPR in adaptive experiments, while TS-Probability Clipping achieves similar benefits through a different approach. Our results show how the TS-PostDiff approach provides benefit in performing well across a range of possible scenarios (arm differences) and adapting to different scenarios based on the intuitively specified parameter of whether a difference is small enough to forego user benefits in favour of gaining greater certainty/knowledge. The results also shed light on why and when these different approaches perform best, providing a first step in exploring the wide range of different approaches to adaptive experimentation, and outlining the issues future work needs to tackle for more “Statistically Considerate” ML algorithms.

2 RELATED WORK

Given the well-documented challenges of drawing inferences from adaptively collected data [4, 8, 16, 17, 19, 20, 24, 27], two general approaches have been taken to address this issue: developing methods to make correct inferences directly from data collected using existing adaptive algorithms, or changing the adaptive algorithm to collect better data. Work taking the first approach has primarily focused on characterizing bias from ordinary least squares estimates and reducing that bias through a variety of statistical approaches [4, 8, 16, 19, 24]; reducing the bias of an estimator is related to the final performance of a hypothesis test, but does not directly address the hypothesis testing procedure (typically evaluated through FPR and Power). We focus on statistical hypothesis testing because that is the dominant paradigm for analyzing data in digital field experiments; future work could explore the impact of adding uniform random allocation on Bayesian analysis. There is reason to believe that the issues in incorrect statistical conclusions extend beyond hypothesis tests, as the issues in bias in estimates of means [16] and unequal sample sizes are not restricted to one particular kind of test.

More closely related to this paper is work that considers changing the adaptive algorithm to balance competing goals of minimizing regret and obtaining data that is useful for drawing conclusions. In active exploration [5], the objective is to estimate the value of all arms as accurately as possible, and thus ensure low estimation errors. The tradeoff between active exploration and maximizing cumulative reward was first addressed using a heuristic algorithm [14], and later formalized as a multi-objective bandit problem integrating rewards and estimation errors [9] which lead to a rigorous, but deterministic, strategy with theoretical guarantees. The work in this paper is complementary to [9] as it focuses on randomized, rather than deterministic strategies, which are likely to be of interest to researchers who typically conduct experiments.

Several recent papers have directly tackled how to collect data that is amenable to statistical hypothesis testing. The Power-constrained bandits algorithm [25] explores making multiple allocation decisions for one participant, with the bandit aiming to guarantee the Power of a later hypothesis test while also minimizing regret. Other work [23] uses a finite horizon MDP approach to minimize regret while constraining the minimum number of participants assigned to each arm; they show this leads to improved Power and lower estimation errors compared to an adaptive algorithm without this constraint. e/TT-TS is a high-performing algorithm that introduces a fixed amount of additional exploration of the second-best arm as modeled by TS [18]. In contrast to the fixed amount of Uniform Random introduced by e/TT-TS, we introduce the TS-PostDiff algorithm which adaptively varies the amount of Uniform Random and compare it to the state-of-the-art e/TT-TS.

3 MIXING MAB ALLOCATION WITH UNIFORM RANDOMIZATION

Maximizing the benefit of an experiment to participants can be modelled as a MAB problem, in which an agent has to effectively choose among K available actions or arms. At each time step t, the agent chooses an arm k ∈ {1, …, K} and receives a reward yk associated with that arm. Rewards are generally stochastic, and the expected reward might vary between arms. The agent’s goal is to maximize cumulative reward, i.e., the sum of all rewards earned from all time steps. To achieve this goal, the agent must trade off the benefits of exploring unknown or poorly explored arms to learn more about their associated reward, with exploiting already known arms by choosing the one with highest expected reward [12].

We first consider adding Uniform Random allocation to an adaptive allocation strategy by spreading the Uniform Random allocation evenly over time (fixed) with e/TT-TS. We then illustrate our proposal of combining adaptive assignment with fixed Uniform Random allocation, based on what we call Posterior Probability of Difference (PostDiff). The idea behind TS-PostDiff is to modulate the amount
of additional Uniform Random adaptively, depending on the empirical evidence about effect sizes (difference in expected reward between arms).

### 3.1 Beta-Bernoulli Thompson Sampling

Thompson Sampling (TS) is a commonly used, randomized MAB algorithm [6]. With binary rewards, a Beta-Bernoulli model of the arms is typical. The true rate of reward from each arm \( k \), denoted \( p_k^* \), is unknown. Instead, we maintain a Beta distribution over our estimate of \( p_k \), capturing uncertainty about the effectiveness of each arm. Independent beta-distributed priors with parameters \( \alpha_k = 1 \) and \( \beta_k = 1 \) (corresponding to a uniform distribution) over the estimation of each \( p_k \) are assumed. At each iteration of TS, a sample is drawn from the posterior distribution of \( p_k \) for each arm, and the arm with the largest sampled value is selected [6]. Choosing actions with TS balances exploration and exploitation in the long run, sampling from arms with the goal of converging on an optimal arm asymptotically [1]. If there are two arms with the same expected reward, then choosing either arm is equivalent from the perspective of maximizing reward, making all policies optimal. While intuitively one might believe that TS would sample both arms equally, it is not guaranteed to do so and in practice, it can still converge to only or mainly sampling from one of the arms (e.g., [17]).

### 3.2 Epsilon/Top-Two Thompson Sampling

\( \epsilon/\mathrm{TT-TS} \) [18] introduces additional fixed random exploration through a parameter \( \beta \), and achieves high performance in best-arm identification. At each time step, \( \epsilon/\mathrm{TT-TS} \) samples from the posterior distribution for each arm, and with probability \( \beta \) chooses the arm with the highest sampled value, and with probability \( 1 - \beta \) chooses the arm with second-highest sampled value. With two-arms, this corresponds to allocating with Uniform Random with probability \( \epsilon \) and with TS with probability \( 1 - \epsilon \), where \( \beta = 1 - \frac{\epsilon}{2} \).

### 3.3 Thompson Sampling Probability Clipping (TS-Probability Clipping)

TS-Probability Clipping [27], adds restriction to the maximum allocation probability to each arm through \( \text{prob} \_\text{max} \), which is a parameter takes value between 0.5 and 1. Accordingly, the minimum allocation probability is equal to \( 1 - \text{prob} \_\text{max} \) and this ensures the algorithm always has a chance to explore the seemingly worse arm. In two arm cases, at each time step, TS-Probability Clipping estimate the allocation probability to each arm, and if the maximum allocation probability exceeds \( \text{prob} \_\text{max} \), it instead allocates to the better arm with probability \( \text{prob} \_\text{max} \) and to the worse arm with probability \( 1 - \text{prob} \_\text{max} \).

### 3.4 Thompson Sampling PostDiff (TS-PostDiff) Policy

If an effect is small, the difference in reward between arms is not great, but the demand on Power in detecting this effect is large, and thus we would prefer the algorithm to assign participants to conditions closer to uniformly, to ensure we can detect this small effect. Likewise, when no effect exists, there is no reward to be gained, but MAB attempts to maximize reward can inflate FPR. We would thus want to choose actions uniformly at random in this case. Finally, as the effect size grows larger, we would like the algorithm to assign more participants to the reward-maximizing arm.

Thus, we would like to increase Uniform Random assignment when there are nonexistent or smaller effects (and less evidence for these effects), with a minimal loss in reward when there is a substantial effect and reward to be gained. Towards this end, we propose the TS-PostDiff algorithm, which operates as follows:

- with probability \( \phi_t \), choose action uniformly at random.
- with probability \( 1 - \phi_t \), choose action according to an adaptive allocation strategy (e.g., TS).

We define \( \phi_t \) to be the posterior probability (after \( t \) steps) that the difference in expected reward between the 2 arms is less than a threshold \( c \in [0,1] \) set by the experimenter, i.e., let

\[
\Delta \mathbb{E}(y_t | x, \mathbf{p}) = |\mathbb{E}(y_t | x = 1, p_1) - \mathbb{E}(y_t | x = 2, p_2)|
\]

(1)

then,

\[
\phi_t = \int_{[0,1]^2} \mathbb{1}[\Delta \mathbb{E}(y_t | x, \mathbf{p}) < c] \pi(\mathbf{p} | \mathcal{D}_t) \, d\mathbf{p}
\]

(2)

where \( \mathbb{1} \) is the indicator function, \( \mathbf{p} = (p_1, p_2) \) is the parameter vector, \( \mathcal{D}_t \) is set of available data up to time \( t \), and \( \pi(\mathbf{p} | \mathcal{D}_t) \) is the posterior distribution of the unknown \( p_k \)'s parameters given the observed data \( \mathcal{D}_t \). Specifically, we choose an action uniformly at random if \( |p_1 - p_2| < c \), and use a MAB adaptive allocation strategy otherwise, with a probability of Uniform Random allocation of \( \phi_t \). We can think of \( c \) as the effect size below which we are willing to forgo reward in favour of improved FPR and Power.

**Example: TS-PostDiff.** As an example, we show how can we apply the PostDiff mixture strategy to TS. The procedure, which we call **TS-PostDiff**, is illustrated in Algorithm 1.

Note the inclusion of a resampling step.\(^1\)

\(^1\)This step is used to prevent using the same \( \mathbf{p} \) sample to determine if a difference exceeds \( c \). and for action selection, as this can result in TS behaving too exploitatively, choosing the estimated best arm too frequently.
4 REAL-WORLD ADAPTIVE EXPERIMENTS

We considered a range of real-world online experiments that could be adapted using algorithms that try to trade-off helping participants with statistical analysis [3]. These ranged from website conversions [21], messages to get people vaccinated [15] [10], and optimizing instructor explanations [22]. We conducted 10 real-world educational experiments during 2021, where we collected data side by side using both UR (traditional RCT/experiment) and an adaptive experiment using the TS-PostDiff algorithm. Figure 1 shows illustrative examples from these real-world deployments. To evaluate the performance of different algorithms for statistical analysis, we need to know how often these cases occur: specifically, the false positive rate and the power. Case (1) is TS-PostDiff producing closer to Uniform Random allocation when there is little evidence for a difference in arm means, reducing the chances of getting a false positive with no—or minimal—cost to reward; (2) TS-PostDiff doing TS reward maximization when there seems to be a difference in arm means that is large relative to the parameter for small differences (below which maximizing reward is not a high priority).

Of course, a few real-world examples of case studies does not reveal the behaviour of the algorithms over many adaptive experiments. We therefore specified ground truth scenarios with arm differences (below which there is a difference between the arms (i.e. reward for arms is identical (i.e. $H_0: \mu_1 = \mu_2$), and the alternative hypothesis as there is a difference between the arms (i.e. $H_1: \mu_1 \neq \mu_2$).

5 EVALUATING ALGORITHMS FOR ADAPTIVE EXPERIMENTS

Algorithms that increase uniform random (UR) exploration are likely to allow us to draw more accurate conclusions from statistical hypothesis testing, but their impact on the trade-off between false positive rate (FPR), statistical power, and reward (or outcome) may vary. The impact of each algorithm is likely to vary depending on the environment in which it is deployed, including the total number of time steps and the actual effect size (or difference between treatment arms). Since the actual difference between arms is not known, it would be beneficial to have an algorithm that performs well across multiple effect sizes. We use simulations to explore the impact of adding fixed amounts of UR allocation ($\epsilon/\text{TT-TS}$), UR based on the estimated effect size was small compared with the parameter $c$. Case 2 (Bottom): 584 participants were randomly allocated to the arms, showing an effect size of 0.045; the other 587 students were allocated by a TS-PostDiff policy with $c = 0.02$, getting an effect size of 0.062, showing that TS-PostDiff chose to exploit the arm with better mean given that the estimated effect size was large compared with the parameter $c$.

Table 3 shows our key metrics to understand the behaviour of these four algorithms. Below we explain our methods for obtaining each of these metrics in Table 3.

5.1 Methods

Following standard practice for hypothesis testing for differences in means, we set our null hypothesis as the case where the expected reward for arms is identical (i.e. $H_0: \rho^*_1 = \rho^*_2$), and the alternative hypothesis as there is a difference between the arms (i.e. $H_1: \rho^*_1 \neq \rho^*_2$).
We use the common Wald z-test statistic, which relies on a comparison of the sample means and their respective standard errors to reject or accept the null hypothesis.  

Effect Sizes (Differences in Arm Means) and Sample Sizes: Let \( \omega \) be the effect size (or the difference in the mean rewards of the arms), which could be equal to 0, 0.1, 0.2, 0.3, and 0.5. In our simulations, we consider a 2-armed Bernoulli bandit setting with arm means set as \( p_1^\ast = 0.5 + \omega, \) \( p_2^\ast = 0.5 - \omega \). More precisely, we set \( p_1^\ast = p_2^\ast = 0.5 \) for effect size 0, \( p_1^\ast = 0.55, p_2^\ast = 0.45 \) for effect size 0.1, \( p_1^\ast = 0.6, p_2^\ast = 0.4 \) for effect size 0.2, etc. In our experiments, we analyze sample size, which is the sample size required for the statistical power of 0.8 for a given effect size using a fixed uniform random (UR) allocation. This results in \( n = 197 \), and \( n = 785 \), for effect sizes 0.2, and 0.1 respectively. To see the change in power and reward when the effect size increases, we also included effect sizes 0.3 and 0.5 for \( n = 197 \) and effect sizes 0.2 and 0.3 for \( n = 785 \). When there is no effect, we use the same sample sizes as when there is an effect.

Optimal Arm and ’Superior’ Arm: We refer to the arm with better mean reward as the Optimal Arm (if there is no difference in arms, then both arms are optimal). At the end of a study or simulation trial, we also look at the arm with higher estimated mean reward, and we call it the ‘Superior’ arm. The Optimal arm is determined and fixed once we know the true mean rewards of the two arms; while the ‘Superior’ arm could change from trial to trial (even under the same simulation setting).

Algorithm and Parameter Settings: An important part of our methodology is a basis for comparing these different algorithms, each of which has free parameters, which can lead to different complex trade-offs between reward, FPR and statistical power. We therefore fix specific FPR rates, enabling a comparison of the algorithms in terms of reward and power. This corresponds to how adaptive experiments would be run in the real world, where the experimental design will typically specify the desired FPR.

Specifically, under the assumption of no difference between arms, for each sample size, to get four FPRs close to (but not exceeding) 6%, 7%, 8%, and 10%, we pick the corresponding four triples of the key parameter for each algorithm: \( c \) in TS-PostDiff, \( \beta \) in e/T-TS, and prob_max in TS-ProbClip. We provide the specific numbers in the Appendix in Section B. For all results, we average across the outcomes of 10,000 simulations.

5.1.1 Metrics for Algorithm Comparison. In our simulation study, we use the following metrics to evaluate and analyze the performance of different algorithms. In Table 3, the results of those metrics are listed in the order as is mentioned below.

Mean Reward: We are interested in measuring performance with respect to reward maximization. To this end, we look at mean reward (referred to as reward) and the proportion of optimal allocations for a given sample size.

False Positive Rate (FPR) and Statistical Power: In conducting our hypothesis test, we are concerned with two performance metrics:

1) FPR, which is the probability that we conclude there is a difference between groups when there is no difference; and 2) statistical power, which is the probability that we correctly detect that an effect exists.

In our simulations, we approximate the FPR as the proportion of times we incorrectly reject the null hypothesis when \( H_0 \) is true and power as the number of times we correctly reject the null hypothesis when \( p_1^\neq p_2^\ast \) and \( H_0 \) is false. We use a significance level of 0.05, which is commonly used in web experimentation and in social and behavioral sciences.

Distribution Intervals of Posterior Probability of an Arm being Optimal (P(Optimal)) and Probability of Arm Assignment (P(Assign)): Thompson Sampling (TS) assigns arms proportional to the posterior probability that an arm is the optimal arm. But that posterior diverges from the posterior obtained under uniform random (UR), just as the FPR and power do. To understand how different algorithms impact data collection and statistical inference, we look at distribution intervals of posterior probability of an arm being optimal (P(Optimal)) and probability of assignment (P(Assign)).

First, note that Posterior Probability refers to the probability calculated at the end of a simulation trial. In addition, in (two arm settings) each of those probabilities consists of two values (for arm 1 and arm 2, respectively) that add up to 1. For simplicity, we only look at the probability of one arm: when there is no arm difference, we look at such probabilities for the ‘Superior’ arm (which is not a fixed arm); whereas when there is a difference, we focus on the Optimal arm. Here are two examples to illustrate this:

- **Example 1** In Table 3 (where \( p_1=0.55, p_2=0.45, n = 785 \), P(Optimal) refers to the probability that ‘Arm 1 is better than Arm 2’, given data collected by a certain algorithm at the end of a trial.

- **Example 2** In Table 3 (where \( p_1=0.5, p_2=0.5, n = 785 \), P(Assign) refers to the probability that a certain algorithm will assign the ‘Superior’ arm to the next (786th) participant at the end of a trial.

We are interested in the distribution of P(Optimal) and P(Assign) in 10,000 simulation trials. For the distribution interval [0.5, 0.6], we calculate the percent of time those probabilities are between 0.5 and 0.6. Similarly, we compute in the distribution intervals [0.6, 0.9] and [0.9, 1].

Proportion of Assignments to Optimal Arm and Proportion of Assignments to ‘Superior’ Arm: In the case where arm difference is 0 (As referred in Table 3), we calculate Proportion of Assignments to ‘Superior’ Arm (named “Prop. Sup.” in the chart), which is the average percent of times the superior arm is chosen. This allows us to examine the proportion of people assigned to the arm with the larger sample mean, and see how much an algorithm tends away from exploration and UR towards assigning an arm, even when no difference exists.

In the case where arm difference is not 0 (As shown in Table 3), we calculate Proportion of Assignments to Optimal Arm (named “Prop. Opt.” in the chart), which is the average percent of times the optimal arm is chosen.

Proportion of non-TS steps: The three main algorithms under comparison are TS-PostDiff, e/T-TS, and TS-ProbClip. Each of these algorithms ‘mixes’ TS with UR with their unique mechanisms.
To better understand how these algorithms compare, we calculate the percent of time each of these mechanisms triggers and hence make these algorithms not do an exact TS-like step. To be more specific: for TS-PostDiff and e/TT-TS, this is when they follow UR policy; and for TS-ProbClip, this is when the allocation probability of an arm exceeds the clipping probability $p_{\text{max}}$.

We should also notice that, for TS-PostDiff and e/TT-TS, this proportion can be viewed as proportion of UR, while for TS-ProbClip this is not exactly the case (but it still reflects the proportion to some extent, with higher Proportion of non-TS steps, more UR is allocated).

**Estimation Biases (Bias in arm 1, Bias in arm 2):** In addition to hypothesis testing, we also want to measure the ‘quality’ of data collected by each algorithm in terms of Estimation Bias. As shown in Table 3, we calculate three measurements. ‘Bias in Arm 1’ and ‘Bias in Arm 2’ refers to the average bias in the estimation of mean reward of arm 1(or arm 2) using data collected by a certain algorithm. $|\hat{p}_1 - \hat{p}_2|$ refers to the expected value of the estimated effect size, which is ideally equal or close to the true arm difference.

![Figure 2: Power-reward plots for Uniform Random, TS, TS-PostDiff, e/TT-TS and TS-ProbClip under sample size 197 and 785. The blue dashed lines separate simulation results for the different effect sizes/arm differences.](image)

### Table 2: Total average of Power and Mean Reward for Uniform Random, TS, TS-PostDiff, e/TT-TS, and TS-ProbClip across all the different settings of Arm Difference and Sample Size.

| Algorithm     | Power | Reward |
|---------------|-------|--------|
| Uniform Random| 0.930 | 0.500  |
| TS            | 0.800 | 0.620  |
| TS-PostDiff   | 0.919 | 0.600  |
| e/TT-TS       | 0.872 | 0.591  |
| TS-ProbClip   | 0.836 | 0.596  |

(2) e/TT-TS: A fixed amount of additional UR exploration. (3) TS-PostDiff: A data adaptive amount of UR exploration, proportional to the probability the arm difference is below a specified size.

### 5.3 Overall Trade-off between Maximizing Reward and Statistical Analysis.

Table 2 shows the average performance of different algorithms across all of our simulation setting where arm difference is greater than 0, for parameter choices that ensured matched FPR. We see that, over all, TS-PostDiff, e/TT-TS, and TS-ProbClip achieve intermediate Power and Reward compared to TS and Uniform Random, matching the goal of taking both metrics into consideration. All 3 algorithms provide trade-offs that will be arguably better in many web applications. They achieve this because each of them add a mechanism beside TS that enables and forces more exploration.

### 5.4 Performance and Behaviour of data-adaptive (TS-PostDiff) UR exploration vs fixed (e/TT-TS) UR exploration vs exploitation clipping (TS-ProbClip)

Now we analyse the performance of the three algorithm in more depth.

#### 5.4.1 TS-PostDiff adapts based on effect size to achieve a good trade-off between Reward and Power.

First, we compare the algorithms under three simulation settings:

- no arm difference $p_1 = p_2 = 0.5$;
- smaller arm difference $(p_1 = 0.55, p_2 = 0.45)$;
- larger effect size $(p_1 = 0.65, p_2 = 0.35)$.

For simplicity we only present results for sample size 785, and, except TS and UR, we only consider TS-PostDiff with $c = 0.1$, e/TT-TS with $e = 0.125$ and TS-ProbClip with $p_{\text{max}} = 0.9375$. Their parameters are chosen such that, under the case $p_1 = p_2 = 0.5$ and sample size 785, they all have FPR close to 0.08. Detailed results for all metrics introduced in Section 5.1.1.

To understand how those algorithms ‘adapt’ based on effect size, we calculated ‘Prop. non-TS’, which tells us how their respective mechanism is working across different effect sizes. For TS-PostDiff, as effect size increases, we see it’s ‘non-TS’ proportion is decreasing.
Table 3: Comparison of performance between different algorithms under Arm Difference 0.0 (i.e., $p_1 = 0.5, p_2 = 0.5$), Arm Difference 0.1 (i.e., $p_1 = 0.55, p_2 = 0.45$), and Arm Difference 0.3 (i.e., $p_1 = 0.65, p_2 = 0.35$), with sample size = 785. We measure the performance of each algorithm under multiple matrices. The explanation to those metrics can be found in section 5.1.1. For TS-PostDiff, $\epsilon/TT-TS$ and TS-ProbClip, we choose their parameters in the way that they produce a similar False Positive Rate (FPR) when there’s no arm difference and $p_1 = 0.5, p_2 = 0.5$, sample size = 785.
When there is in fact an Arm Difference (e.g. 0.1 or 0.3). As shown with TS having has 1.8 times as much Bias as TS-ProbClip (-0.013 vs -0.023).

We consider the behaviour of an algorithm in terms of the trade-off it provides between UR and TS, based on Table 2 that averages over multiple Arm Differences and Sample Sizes. TS-ProbClip gives 80.00% = (0.596 – 0.5)/(0.620 – 0.5) of the Reward advantage TS has over UR (A reward of 0.596, vs 0.500 for UR and 0.620 vs 0.500 for TS). For giving up only 3.87% = (0.620 – 0.596)/0.620 of the TS Reward advantage, TS-ProbClip gains 72.31% = (0.164 – 0.070)/(0.200 – 0.07) of the FPR advantage UR has over TS (FPR of 0.164 vs UR 0.070 and TS 0.200) when Arm Difference is 0.0. Table 3 illustrates the mechanisms behind this. TS-ProbClip does not allow P(Assign) of the seemingly inferior arm to get too close to 0- (in table 3, p_max of 0.9375 means there is a minimum 6.25% chance of a seemingly worse arm being sampled). This additional exploration relative to TS means a reduction in the bias that TS adaptive sampling induces in the sample estimates of arm means, with TS having has 1.8 times as much Bias as TS-ProbClip (-0.013 vs -0.023).

TS-ProbClip has 72.31% = (0.836 – 0.930)/(0.800 – 0.930) of the Power advantage UR has over TS (Power of 0.836 vs UR 0.930 and TS 0.800). The mechanisms underlying higher Power are subtle and complex, with two opposing contributions of adaptive sampling when there is in fact an Arm Difference (e.g. 0.1 or 0.3). As shown in Table 3, adaptive sampling increases the uneven assignment to arms, which reduces Power as with less data in one arm there is less evidence against the null hypothesis that the observed difference in the sample means is due to noise. TS-ProbClip decreases the Prop. Opt (proportion of participants in the experiment assigned to the optimal arm 1), which increases power. However, adaptive sampling can counter-intuitively increase Power, by increasing bias in estimates of the lower arm mean (Bias in arm 2 is over ten times the bias in Arm 1). TS-ProbClip reduces this bias by adding more exploration of arms that looked worse than they actually were, due to random lows. This means that there is a smaller (but more accurate) observed difference in arm means, which reduces power. Even though TS has much lower Power than UR, this is largely due to the much fewer samples in the lower arm overwhelming the effect of estimates of the lower arm, which (incorrectly) suggest arm differences are bigger than they are, but provide more evidence for there being a difference. Arguably, getting the ‘correct’ answer (there is a difference) for the ‘wrong’ reason - the difference in arms seeming bigger than it truly is. Note that it is still a problem for statistical analysis that estimation of these parameters is less accurate - hypothesis testing and estimation questions are related but distinct, in complex ways.

When exploration is added in TS-ProbClip, it both increases confidence in the estimate of the worse arm (increasing power) and reduces bias in the estimate (reducing power). Which of these two competing contributions ‘wins out’? For Arm Difference of 0.1 TS-ProbClip has slightly worse Power than TS (0.0533 vs 0.566), but for Arm Difference of 0.3 TS-ProbClip has slightly better Power than TS (0.998 vs 0.973).

In order to show that our previous analysis applies to those algorithms with different choice of parameters, we plotted results for all algorithms under multiple parameters in Figure 2. For TS-PostDiff, larger c indicates that extra Uniform Random should be added for a larger range of effect sizes; for ε/TT-TS, a higher β will decrease the amount of Uniform Random it allocates; for TS-ProbClip, it behaves more similar to Uniform Random when prob_max is closer to 0.5.

In our figure, we suggest our reader to compare PostDiff, TT-TS and Clip results that’s in the same color, as they correspond to similar FPR under no effect case. From this point of view, we see that PostDiff has better power and worse reward when effect size is low, and better reward when effect size is large.

TS-PostDiff has Increased Power for small effect sizes Firstly, we find that TS-PostDiff almost always has better Power than ε/TT-TS and TS-ProbClip. Such an advantage is bigger when the effect size is relatively small (for example, when effect size = 0.2 and sample size = 197). In other words, in the situation where the sample size is not quite enough to detect differences in treatments, TS-PostDiff tends to increase the proportion of UR and thus achieve a higher Power, with minimal loss to reward. However, this is due to two factors.

The first is a statistically desirable and justified reason of putting more participants in the arm with lower sample mean, so there is more confidence in the sample mean estimate. The second is counterintuitive. The power is higher because there is *more* bias in the lower arm mean and so differences look larger than they actually are. As shown in Table 3b and 3c, the bias is closer to bias in TS than in ε/TT-TS or TS-ProbClip. This is due to: when the worse arm has bad estimation at the beginning, PostDiff mechanisms are less likely to happen (negative correlation). And for TT-TS and Clip, this correlation is 0/positive.

TS-PostDiff inherits some limitations of TS that may prevent even lower FPR The increase in bias when Arm Differences are greater than zero are especially noteworthy, because for Arm Difference of 0, TS-postdiff has less bias than ε/TT-TS and TS-ProbClip. This reveals that the lower bias and greater number of participants in either arm (closer to UR) for TS-PostDiff are therefore not resulting in as great a decline in FPR, as could be warranted.

Our investigation revealed that this occurs because there is a set of trials in which there is early divergence with one arm looking much higher, and so the PostDiff UR step (activated when the probability of arm difference being less than small difference parameter is high) isn’t activated frequently enough. And so, compared to ε/TT-TS
and TS-ProbClip, there are a few trials with much larger bias that increase FPR, even if on average there are more trials with less bias. This is quite revealing as to the strengths of these different methods, and the when the effects in coming up with algorithms that do an exploration-exploitation trade-off that is better for analysis. This is where a fixed UR strategy like \( e/\text{TT-TS} \) has the advantage of providing data that could help identify these extreme cases. Future work could explore different ways of incorporating a certain amount of fixed UR, such as adaptively setting an initial fixed period of UR (related to explore-then-exploit algorithms, [11]) before engaging TS or TS-PostDiff.

TS-PostDiff prioritizes reward for larger effect sizes. On the other hand, when the effect size is relatively easy to detect, TS-PostDiff provides good Reward while maintaining Power. For example, when effect size = 0.5 and sample size = 197, TS-PostDiff has similar Power as \( e/\text{TT-TS} \) and TS-ProbClip (all of them are close to 1), while the Reward of TS-PostDiff is higher. When the posterior distribution of the two arms are distinct, the Uniform Random re-sampling step will happen less frequently, which means that TS-PostDiff will behave closer to the TS algorithm and explore more to get higher reward. This suggests that TS-PostDiff can adapt to the sample size and effect size, giving a good trade-off.

Table 3 further illustrates the point we just made, which is, TS-PostDiff tends to explore more and can achieve better power relative to \( e/\text{TT-TS} \) and TS-ProbClip when there’s lack of evidence to conclude that one arm is better than the other, and it adapts to exploit the seemingly better arm more and get better reward as it collects more and more information.

To understand how the differences in outcomes for TS-PostDiff, \( e/\text{TT-TS} \), and TS-ProbClip vary based on true effect size, we present their Power, Reward, Prop Opt., and Prop Sup in table 3. We consider three effect sizes (0.0, 0.1, and 0.3) and the parameters for those three algorithms are chosen such that they all have false positive rates close to 0.08 when the effect size is 0. For better comparison, we also include the result for TS and Uniform Random.

From Table 3 we see that, when there is no effect, the Prop Sup. for TS-PostDiff (0.529) is the closest to 0.5. This shows that, compared to \( e/\text{TT-TS} \), and TS-ProbClip, TS-PostDiff acts more similar to Uniform Random and allocates participants more evenly to the two arms. When the effect size is small (effect size = 0.1), the Prop Opt. for TS-PostDiff is smaller than that for \( e/\text{TT-TS} \), and TS-ProbClip, which means TS-PostDiff will explore different arms more, and accordingly, it achieves a much better power compared to \( e/\text{TT-TS} \), and TS-ProbClip algorithm. When the effect size gets large (effect size = 0.3), the Prop Opt. for TS-PostDiff quickly approaches what TS produces, and that gives TS-PostDiff better reward than \( e/\text{TT-TS} \) and TS-ProbClip. By design, the maximum proportion for sampling the optimal arm for \( e/\text{TT-TS} \) and TS-ProbClip algorithm is restricted to be equal to the value of \( \beta \) or \( \text{prob}_{\text{max}} \), respectively, and that prevents these two algorithms from getting more rewards when effect size is large. However, TS-PostDiff on the other hand doesn’t have such limitations. When effect size is significantly greater than its parameter \( c \), the chance for TS-PostDiff mechanism being triggered will approach 0 as sample size increases and thus its behaviour can be infinitely close to TS.

Table 4 and Appendix table 5 gives detailed results for Power and Mean Reward for Uniform Random, TS, TS-PostDiff, \( e/\text{TT-TS} \), and TS-ProbClip that matches what is plotted in figure 2. In table 2 we take the average for Power and Mean Reward across all different settings we considered (across different sample sizes, different effect sizes, different parameter choices) for each algorithm. From this perspective, we see that TS-PostDiff has better Power and Reward than \( e/\text{TT-TS} \) and TS-ProbClip.

5.4.2 Understanding Behavior of TS-PostDiff and Choosing \( c \). We now address the question of how to choose the \( c \) parameter for the TS-PostDiff algorithm. We present two approaches to choosing \( c \) which reflect different user perspectives. To give the first recommendation we first analyze the behaviour of TS-PostDiff.

Understanding Behaviour of TS-PostDiff: In order to give our first recommendation, we examine how \( \phi_{\text{in}} \) in Equation 2 evolves as more participants are seen. In Figure 4, we show \( \phi_{\text{in}} \) for various choice of \( c \), for effect sizes 0.0 and 0.1. We approximate \( \phi_{\text{in}} \) as \( \phi_{\text{in}} \) by taking the proportion of times \( |p_1 - p_2| < c \) across 10,000 simulations for the given sample size. When \( c \) is above the true effect size, we see that \( \phi_{\text{in}} \) is increasing with sample size towards 1. When \( c \) is less than the true effect size, we see that \( \phi_{\text{in}} \) is decreasing towards 0. These are non-trivial results, as they indicate that the additional UR allocation of TS-PostDiff is able to overcome the bias induced by TS. For example, the above shows that the tendency of TS to lead to overestimating the size of the difference in arms does not prevent \( \phi_{\text{in}} \) from converging to 1 when the effect size is 0.0.

First Recommendation for Choosing \( c \): This discussion of the behaviour of TS-PostDiff leads us to our first recommendation for choosing \( c \); we recommend choosing \( c \) as an effect size which is small enough that we are willing to forgo reward in favor of improving data analysis capabilities.

If one is willing to accept \( x \) loss in expected reward (\( |p_1 - p_2| \)) for a sub optimal allocation in favor of improved data analysis, we have strong empirical evidence from Figure 4 that if we choose \( c = x \), when the true effect size is such that \( c > |p_1 - p_2| \), we will converge to always using UR allocation, and if the true effect size is such that \( |p_1 - p_2| < c \), we will converge to TS. In other words, if the true effect size is below what we are willing accept in expected loss in reward for a sub optimal allocation in favour of improved FPR and Power, TS-PostDiff will likely converge to choosing all actions with UR allocation. Similarly, when the true effect size is greater than what we are willing accept in expected loss in reward for a sub optimal allocation, TS-PostDiff will likely converge to TS.

Example Choosing \( c \) Based on Admissible Loss in Reward: For instance, perhaps we are interested in testing whether design A or design B of a website button results in a greater click through rate (CTR). We will run an experiment with 1000 users, and we do not want to miss out on an improvement in CTR of over 10% from the optimal design (i.e. design A’s CTR is 20% compared to design B’s CTR of 10%) over the course of our adaptive A/B test, if this led us to reliably discover this effect. We would favor discovering the effect, since in the short term the decrease in CTR is not very large, but by more reliably discovering relatively small, we can deploy the superior design in the long run where this design choice is used for months, and used by millions of users. We thus set \( c = 0.1 \) and run the experiment.
It turns out that the true increase in CTR for the optimal design is 9.6%. Since the effect size is smaller than what we have deemed admissible through our choice of $c$, TS PostDiff will adapt and perform a large amount of UR allocation, thereby increasing our chance of discovering this effect, and only facing a decrease in CTR which we have deemed admissible.

By contrast, let’s imagine that the difference in click through rate was actually 20%, which is greater than what we are willing to give up during the course of our experiment. Since the true effect size exceeds $c$, TS-PostDiff will perform more allocations according to TS than UR allocation, thereby respecting that we would like to improve CTR during the course of our adaptive experiment.

6 DISCUSSION, LIMITATIONS & FUTURE WORK

6.1 Summary

Analysis in the paper has a focus on performance in the ubiquitous applications where there are two arms, between 200 and 800 participants, and binary outcomes. Moreover, it is also analyzed on where it is important to trade-off helping users quickly during the experiment, as well as drawing reliable statistical conclusions, as interpretations of the data from one experiment can impact what decisions other stakeholders make, or what experiments are running. For example, when arm difference is small, using an algorithm that’s over exploitative (such as TS) can result in greatly reduced Statistical Power. For the purpose of trading off between user experience and statistical inference, first, we consider two algorithms proposed in the literature on bandit algorithms, but with fewer reported applications: $\epsilon$TT-TS and TS-ProbClip. These algorithms add exploration steps by setting parameter $\epsilon$ and $p_{\text{max}}$, which adds more UR exploration or reduces TS exploitation, respectively. Then we introduce a novel algorithm, TS-PostDiff, which sets the parameters considering what differences are small enough that one may be less concerned in the experiment stage. It also allows the addition of exploration steps to be adaptive to the data, just as TS itself is adaptive to the data (but in a way that has been shown to be potentially problematic). It is reported in the paper, that on our methodology for evaluating and comparing these different approaches, which can be useful to those interested in adaptive experimentation on the web in a more general way. Empirical results are provided from a simulation environment constructed from real experiments we have run, and attuned to the applications where there are two arms, between 200 and 800 participants, and binary outcomes. These provide insight into the strengths and weaknesses of these different approaches for adding additional exploration, so that practitioners can consider these in choosing which to apply and when. We also show the value of the data-adaptive ‘small difference’ approach in TS-PostDiff, while also highlighting its drawbacks. It is also introduced in the paper of how to choose the parameter for TS-PostDiff.

6.2 Limitations & Future Work

There are limitations to the current paper which point towards future directions. One limitation of the current work is that we focus on 2-arm trials. While these are some of the most ubiquitous trial designs, and are highly important in their own right, it could be valuable for future work to understand how an algorithm like TS-PostDiff can be extended to trials with 3 or more arms. How can one define the hypothesis tests and the criteria for “small” effect sizes, when the differences between 3 or more arms might be considered? How will combining UR and TS impact the FPR/Power/Reward trade-off as the number of arms increases? For example, when might improving Power to identify the best arm of 3 or more arms also improve Reward, vs reduce Reward?

We know that TS and UR are the two best-established designs for regret minimization and acquiring reliable data for knowledge generalizability, and so interpolating between them is a natural solution to balancing our competing objectives. But formalizing an objective function for adaptive experimentation algorithms could be extremely beneficial to the development of TS-PostDiff and other algorithms for adaptive experimentation. Defining a function which is a weighted combination of statistical Power, False Positive Rate, and expected Reward could both provide a common standard by which to compare the performance of different adaptive experimentation algorithms and help experimenters choose an algorithm and set of hyperparameters based on how important each of Reward,
Power, and False Positive Rate is to them. Of course, formalizing such an objective function is challenging due to Power requiring assumptions about the likely effect sizes, and the wide variety of possible tests, estimators, and experimental goals.

6.3 Conclusion

In digital field experiments, experimenters are often simultaneously concerned with achieving good False Positive Rate/Power and Reward maximization. The issue of balancing reward maximization against statistical inference is important, unsolved, and poses many exciting challenges. We propose interpolating between Uniform Random and Thompson Sampling as an approach to solving this problem, and introduce the TS-PostDiff algorithm, which follows from this approach. Our approach and the TS-PostDiff algorithm provide building blocks for future work to better balance Reward maximization and statistical inference.

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Appendices

A CONSIDERATIONS IN CHOICE OF c FOR TS-POSTDIFF

We consider some of the issues at play in choosing the c parameter for the PostDiff algorithm. We focus on TS-PostDiff, although the same considerations apply when using the PostDiff strategy to add Uniform Random allocation to other adaptive algorithms. Values in brackets are standard errors.

Understanding Behaviour of TS-PostDiff: The choice of c impacts how \( \hat{\phi} \) in Equation 1 of the main text evolves as more participants are seen. In Figure 4, we show \( \hat{\phi}_t \) for various choice of c, for effect sizes 0.0 and 0.1. We approximate \( \phi_t \) as \( \hat{\phi}_t \) by taking the proportion of times \( |p_1 - p_2| < c \) across 10,000 simulations for the given sample size. When c is above the true effect size, \( \hat{\phi} \) increases with sample size towards 1. When c is less than the true effect size, \( \hat{\phi} \) decreases towards 0. These are non-trivial results, as they indicate that the additional Uniform Random allocation of TS-PostDiff is able to overcome the bias induced by TS. For example, the above shows that the tendency of TS to overestimate the size of the difference in arms does not prevent \( \hat{\phi} \) from converging to 1 when the effect size is 0.0.

Possible Approaches to Choosing c: One approach is to set c to an effect size which is “small” enough that maximizing Reward for participants in the experiment is less important than getting better analysis of the data. More precisely, consider an experiment where one is willing to accept x loss in expected reward (\( |p_1^* - p_2^*| \)). Figure 4 shows that if we choose \( c = x \), if c is greater than the true effect size \( |p_1^* - p_2^*| \), the algorithm converges to always using Uniform Random allocation. If the true effect size \( |p_1^* - p_2^*| \) is less than c, the algorithm will converge to TS.

What if one isn’t sure what reward they are willing to give up for improved data analysis? Or, it might be hard for a social-behavioral scientist to make that decision without greater clarity about how much is lost or gained on each dimension of Reward, FPR, and Power. Another approach could be to choose c as the best non-zero guess for the true effect size.

We motivate the recommendation of choosing c as a guess for the true effect size based on results shown in Figure 3, which compares FPR, Power and percentage of TS Reward for a range of c values and effect sizes of 0 and 0.1. Figure 3 shows diminishing returns in Power when c exceeds the effect size of 0.1, whereas the percentage of TS Reward decreases roughly linearly. FPR also reaches diminishing returns, but a c value of 0.1 is close to the value where we see such diminishing returns. Choosing a c value of 0.1 is thus a reasonable choice, or in general a c value equal to the effect size when effect sizes are smaller. If the effect size is larger though, then the loss in Reward will be greater. Though the percentage of TS Reward is decreasing linearly, if TS Reward is large, then this linear decrease will be more costly. Following the thresholds in behavioural science for small and medium effect sizes [7], we don’t recommend setting c higher than 0.2.

In sum, we can view the choice of c from two perspectives. We can use c to minimize using TS when effect sizes are small enough that we would favour improved inference over reward maximization, and we can also use c as a way to influence how TS-PostDiff trades-off between reward maximization and Uniform Random exploration. These two perspectives (and a combination of them) can thus guide a user in choosing c.

B DETAILED SIMULATION RESULTS

Appendix Table 4 and Appendix Table 5 summarizes simulation results used in Figure 2.

Received 20 February 2007; revised 12 March 2009; accepted 5 June 2009
| Algorithm       | Power | Reward |
|-----------------|-------|--------|
| Uniform Random  | 0.804 | 0.500  |
| TS              | 0.586 | 0.572  |

| Algorithm       | FPR = 0.06 | FPR = 0.07 | FPR = 0.08 | FPR = 0.1 | Mean across FPRs |
|-----------------|------------|------------|------------|-----------|------------------|
| Power           | Reward     | Power      | Reward     | Power     | Reward           | Power | Reward     | Reward |
| TS-PostDiff     | 0.801      | 0.529      | 0.798      | 0.540     | 0.772            | 0.550 | 0.739      | 0.559  | 0.777      | 0.545  |
| TS-ProbClip     | 0.687      | 0.554      | 0.625      | 0.561     | 0.619            | 0.562 | 0.549      | 0.569  | 0.620      | 0.562  |

| Algorithm       | FPR = 0.06 | FPR = 0.07 | FPR = 0.08 | FPR = 0.1 | Mean across FPRs |
|-----------------|------------|------------|------------|-----------|------------------|
| Power           | Reward     | Power      | Reward     | Power     | Reward           | Power | Reward     | Reward |
| TS-PostDiff     | 0.986      | 0.580      | 0.981      | 0.595     | 0.971            | 0.607 | 0.948      | 0.615  | 0.972      | 0.599  |
| TS-ProbClip     | 0.940      | 0.590      | 0.898      | 0.603     | 0.879            | 0.605 | 0.824      | 0.617  | 0.885      | 0.604  |

| Algorithm       | FPR = 0.06 | FPR = 0.07 | FPR = 0.08 | FPR = 0.1 | Mean across FPRs |
|-----------------|------------|------------|------------|-----------|------------------|
| Power           | Reward     | Power      | Reward     | Power     | Reward           | Power | Reward     | Reward |
| TS-PostDiff     | 1.000      | 0.709      | 0.999      | 0.717     | 0.998            | 0.722 | 0.995      | 0.726  | 0.998      | 0.719  |
| TS-ProbClip     | 1.000      | 0.658      | 0.999      | 0.681     | 0.998            | 0.687 | 0.983      | 0.710  | 0.995      | 0.684  |

| Algorithm       | Power | Reward |
|-----------------|-------|--------|
| Uniform Random  | 1.000 | 0.500  |
| TS              | 0.954 | 0.733  |

| Algorithm       | FPR = 0.06 | FPR = 0.07 | FPR = 0.08 | FPR = 0.1 | Mean across FPRs |
|-----------------|------------|------------|------------|-----------|------------------|
| Power           | Reward     | Power      | Reward     | Power     | Reward           | Power | Reward     | Reward |
| TS-PostDiff     | 1.000      | 0.709      | 0.999      | 0.717     | 0.998            | 0.722 | 0.995      | 0.726  | 0.998      | 0.719  |
| TS-ProbClip     | 1.000      | 0.658      | 0.999      | 0.681     | 0.998            | 0.687 | 0.983      | 0.710  | 0.995      | 0.684  |

Table 4: Comparison of Power and Mean Reward for Uniform Random, TS, TS-PostDiff, $\epsilon/TT$-TS, and TS-ProbClip with sample size = 197 and three effect sizes (0.2, 0.3, and 0.5). For TS-PostDiff, $\epsilon/TT$-TS, and TS-ProbClip, we choose four different parameters for each of them that gives a false positive rate of 0.06, 0.07, 0.08 and 0.1, respectively.
Table 5: Comparison of Power and Mean Reward for Uniform Random, TS, TS-PostDiff, $\epsilon/TT-TS$, and TS-ProbClip with sample size $n = 785$ and three effect sizes (0.1, 0.2, and 0.3). For TS-PostDiff, $\epsilon/TT-TS$, and TS-ProbClip, we choose four different parameters for each of them that gives a false positive rate of 0.06, 0.07, 0.08 and 0.1, respectively.