Effects of Targeted Promotions: Evidence from Field Experiments*

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Abstract

The prevalence and widespread usage of email has given businesses a direct and cost effective way of providing consumers with targeted promotional offers. While targeted promotions are expected to increase the demand for the promoted products, are these promotions effective in increasing revenues? Do they have effects beyond acting as price reductions? We study these questions using individual-level data from 70 randomized experiments run by a large online ticket resale platform. We measure the impact of emailed promotions by comparing purchases by individuals who received the experimental promotions with purchases by those who did not receive the offers because of the experimental randomization. We find that the offers cause the average expenditure to increase by $3.03 (a 37.2% increase) during the promotion window. However, ninety percent of these gains are not through redemption of the offers. Interestingly, the promotion causes carryover to the week after the promotion expires; we find that spending increases by $1.55 in the week after the offer expires. Additionally, we find evidence for cross category spillovers to non-promoted products - offers not applicable to a ticket genre cause an increase in spending in that genre. We conclude that emailed promotions can serve as a form of “advertising” for the firm’s products.

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1 Introduction

Being able to target individual consumers is one of the most important objectives of marketing in general and of digital marketing in particular. Display ad networks and search engines have emerged as platforms for advertising that allow for targeting consumers based on their online browsing and search behavior. Similarly, email marketing has emerged as a channel that allows businesses to target consumers with customized messages or discounts. Consequently, using emails to communicate with current and potential buyers has become a widespread strategy; in a recent survey, 99% of marketers reported investing in it (Experian Digital Marketer, 2014).

While there has been research on issues such as how the nature of content in an email influences consumers’ responses (Ansari and Mela [2003]) or the implications of the personalization of email content (Wattal et al. [2011]), questions related to the direct impact of email marketing on revenue and profit have not been much-researched. This paper addresses these issues in the context of targeted email promotions (i.e., exclusive discounts communicated through emails) and examines how they affect the purchase behavior of consumers.

Compared to other marketing instruments such as television advertising, businesses have much more control over email marketing - who gets it, when they get it, etc. Emails are also easier to link with their recipients’ responses. However, measuring the impact of email campaigns on a firm’s revenues and profitability is complicated by the very nature of email marketing - that it is targeted. First, such targeting of email marketing renders a simple comparison of outcomes across those receiving and not receiving the email (in order to measure the effect of the email) inappropriate due to the selection inherent in targeting. Well-designed experiments can, in theory, measure the effects that are relevant for quantifying the value of consumers to a firm. Therefore, with the availability of the right resources, researchers and firms can quantify the effects of emails and also set out to uncover the mechanisms by which targeted emailed messages change consumer behavior. In practice, however, these experiments need to be executed with care because they can be non-representative, costly, and time-consuming. A second challenge with measuring the effects of emails is that because of the ability to customize the content, each campaign may comprise a small targeted set of individuals. Therefore, by itself, each campaign
may have insufficient observations to be able to precisely measure its impact on a firm’s revenues. We address these issues by reporting average effects across 70 experiments conducted by a large online ticket resale platform. Each of these experiments is a targeted email campaign run by the platform during the time period from July 2009 through December 2011. These campaigns are representative of the targeted promotions the platform offered to encourage repeat buying on the website among customers who opted-in for email communication. The data available to us are from a panel of these customers who made purchases through the platform. While the email promotions are themselves targeted based on various criteria used by the platform, for each of these targeted promotions, a random subset was “withheld” from actually receiving the promotion. Importantly, we observe the targeted group for each promotion as well as the treated subset (the group that actually received the email) and the control subset (the group for which the email promotion was withheld). For this reason we are able to causally link the emailed promotions with observed differences in purchases between treated and control conditions.

In our analysis, we first show how the effects of these campaigns can be quantified. Following that, we provide an understanding of how targeted offers sent through emails affect consumer decisions in this context. Our outcome variable of interest is the revenue generated from customers. Further, we are interested in knowing whether the increase, if any, is statistically and economically significant. Next, we examine the mechanism(s) by which targeted email promotions influence buying behavior; i.e., how should firms and policy makers think about email offers as a marketing tool? One possible mechanism driving the effect could be that email offers work purely as price reductions. According to this mechanism, offered discounts appeal to consumer price-sensitivity; reduced price increases the quantity of the promoted product purchased. The profitability of email offers in such a case would depend on the tradeoff between the gain from increased sales at a lower price versus a loss in revenue because of the redeemed discounts. Another possible mechanism is that email offers serve as “advertising” for the product and for the firm (in our case, the resale platform). Consequently, offers may cause an increase in sales even without consumers availing themselves of the discounts reflected in the email promotions. In this case the relevant tradeoff firms face is the gain in sales due to emails versus the cost of designing and sending the emails, the (possible) opportunity cost of customers paying less
attention to future emails, etc. Moreover, ads have been shown to have persistent effects. These effects can spillover to other products and positively affect products that are not advertised; specifically, ads may influence future sales or products not promoted in the email offers. In reality, both these mechanisms can play a role. Our objective is to examine whether either or both of these mechanisms are at play in the campaigns we investigate.

We find that promotional emails have a causal effect on expenditures on the platform. Further, the effect is statistically significant. We also find that there is a significant economic impact of the promotions. On average, receiving experimental offer emails increases a consumer’s spending by about $3 (on a base of $8.07 - a 37.2% increase). Importantly, a very small portion of the gains come from the actual redemption of the offers. About 90% of the gains from experimental offers is through increased spending without redemption of the offers. This evidence suggests that the experimental offers have effects beyond merely acting as price reductions.

We further investigate whether the targeted promotional email offers show carryover (over time) and spillover (to non-promoted categories) effects as if the emails acted as advertising, as described previously. We find that the offer emails have a significant effect during the week after the offer expires, increasing expenditure by $1.55 on average, although not beyond. Additionally, we find that the promotions cause increased sales of products for which the discounts do not apply. Specifically, we use the fact that many offers in our data are not applicable on purchases of Major League Baseball (MLB) tickets. For these offers we find that expenditure on MLB tickets increases significantly during promotions even though the discounts are not applicable.

These findings enable us to make several contributions to the literature. The paper shows evidence for significant causal effects of email offers and shows that a substantial portion of the gain from email promotions arises even when the offers are not redeemed by the recipients. The firm benefits from avenues not confined by the characteristics of the offer; sales increase even after the offer expires and through purchases where the offer is not applicable. In other words, advertising-like effects can even exist for marketing actions that are not explicitly ads. This finding suggests that focusing on just redemption rates of email promotions is not enough to gauge the impact of email offers. These results have direct implications for firms investing in
email communication and also policy-makers interested in evaluating the impact of prevalent targeted offers. They also raise questions (e.g., is a promotional offer even needed as part of the email, does its presence encourage consumers to open the email etc.) that future research can help answer.

The rest of the paper proceeds as follows. In the next section we provide a brief review of the relevant literature. This is followed by a section on the specific empirical setting and the data we use. Next, we provide a brief description of our empirical measurement approach. Following that we discuss our findings and then conclude with a brief discussion.

2 Literature Review

The paper relates to the extensive literature on promotions that explores how discounts and deals affect consumer decisions. Specifically, email offers are targeted to individuals; in this respect, they are similar to coupons in offline markets. Blattberg et al. [1995] summarize the early research in this area. Coupons have been primarily thought of as a tool for price discrimination (Narasimhan [1984]), allowing firms to charge a lower price to consumers who are willing to spend effort on redeeming the coupons and are likely to have a lower willingness to pay. Many studies on the characteristics of individuals who redeem coupons support this view (Bawa et al. [1997], Cronovich et al. [1997], Swaminathan and Bawa [2005], Chiou-Wei and Inman [2008]). A large section of the empirical work has focused on the impact of coupons on the promoted brand’s sales. Klein [1981] reports a field experiment where a random subset of the target population does not receive a coupon and finds coupons increase sales for the promoted brand. Bawa and Shoemaker [1987] show that after receiving direct-mail coupons, purchases for the promoted brand increase mostly through redemption of the coupons. Bawa and Shoemaker [1989] find that compared to before sending the coupons, purchase for the promoted brand increases even among individuals who did not redeem the coupons. Neslin [1990] uses household panel data to estimate a market share model (embedded in a simultaneous equation framework) and finds that the increase in sales associated with coupons is not large enough for couponing to be profitable for the firms considered in the analysis. Chiang [1995] estimates a model aimed
at measuring the effects of coupons on demand while allowing for category expansion and finds no evidence for such an effect in his detergent purchases. Gönül and Srinivasan [1996] estimate a structural model to find evidence that consumers have expectations about receiving coupons and incorporate them into their decision-making. Venkatesan and Farris [2012] use panel data to estimate a model of consumer demand to show how customized targeted coupons affect sales in a supermarket. Data patterns show that consumers who receive but do not redeem the coupons spend much more on the promoted and non-promoted brands.

Another section of the literature focuses on the impact of the characteristics of coupons on their effectiveness. Srinivasan et al. [1995] use data on store sales to find that newspaper free-standing-inserts (FSIs) with print ads increase store sales. Leclerc and Little [1997] use lab studies and panel data analysis to find that the execution of print ads along with FSIs matters; response from less brand loyal consumers increases with the informational content of the ads. Raju et al. [1994] show that the distribution of package coupons (on-pack, peel-off, or in-pack) can affect their impact on the promoted brand. Inman and McAlister [1994] focus on the pattern of coupon redemption rates over time and find that the redemption rates increase just before the expiry of coupons. Krishna and Zhang [1999] show that duration of validity of coupons co-varies with market shares of firms dropping these coupons and build a model to explain the variation.

Email offers are similar to offline coupons in that availing the offer requires active redemption from the consumer’s side. However, they are easier to customize and match with the user’s preferences, allowing for more sophisticated targeting and cost management. This paper adds to the literature which examines the effect of customized promotions on consumer purchases. Being able to send exclusive deals has been found to influence a consumer’s preference for the product (Feinberg et al. [2002], Barone and Roy [2010]). Further, email offers can cause consumers to search immediately because the websites with product information are just a click away. The resulting differences between targeted offers and coupons are evident in our data.

First, compared to 38% of offline purchases in the cereal industry using coupons (Nevo and Wolfram [2002]), we find that just about 3% of transactions in our data actually use any offer. Second, we find that 90% of the short-term gains from the email offers is not through redemption of the offers, which is significantly different from what is found for offline coupons.
Literature focused on email marketing more specifically is limited. Lewis [2004] estimates a dynamic demand model to study the effects of a loyalty program and compares it with email promotions. Hartmann [2006] studies consumption utility in a context where email promotions are used to manage excess capacity. Kumar et al. [2014] study the impact of marketing activities on the propensity of a consumer getting in and out of email marketing lists. This paper adds to the literature by providing evidence for causal effects of email promotions and describing how these effects can impact offer targeting strategy.

This paper also adds to the literature by bringing to bear data from randomized field experiments that include a randomized control group that does not receive an offer (although a potential target for it). Since offers such as those considered in this paper are highly targeted, a randomized control group is necessary for unbiased estimation of the causal effect of offers on consumer purchases. In the absence of experimental variation, comparison of purchases before and after the distribution of coupons might yield biased estimates because coupons could co-occur with other promotions. For example, Nevo and Wolfram [2002] and Anderson and Song [2004] both show that coupons and price reductions are coordinated. Closest in spirit to our paper is the study by Klein [1981]. This paper reports a field experiment on coupons with a control group not receiving coupons. However, the paper focuses on sales of the promoted brand and does not consider the potential increase in sales in the absence of a coupon redemption. Further, it does not look at the longer-term and spillover effects of coupons.

We also find that email offers have effects such as cross-category spillovers benefiting the platform which have not been examined in the literature on coupons. We find that consumers seem to be leaving substantial gains on the table, perhaps attributable to forgetting or to suboptimal decision-making. In our context we find that the firm benefits from these effects to reduce the redemption costs from the offers.

To summarize, our paper contributes to the targeted coupons / promotions literature by uncovering causal evidence on the effects of these promotions in the presence of targeting. Additionally, it sheds light on the role of these promotions beyond the traditional role of providing a price discount. Given this advertising role of the email campaigns, we show the presence of spillovers...
over time (longer term effects) and across other products.

3 Empirical Setting and Data

3.1 Empirical Setting

The data for this study come from a large online ticket resale platform for the United States and Canada.\textsuperscript{1} To preserve anonymity, we cannot disclose the name of the firm. Typical sellers in this marketplace are individuals reselling one or more tickets for events such as concerts or sporting events. Sellers set their own prices for and reveal attributes (e.g. venue location, date, etc.) of each ticket they post. Subsequently, potential buyers search and review these prices and ticket attributes, and decide whether or not to purchase a ticket. If a transaction occurs, the buyer pays the platform the price of the ticket, the platform takes a cut (on the order of 10% of the listed price from the buyer and around the same from the seller), and the seller receives the remainder. All else equal, the platform profits from more and/or larger transactions. Note that we only observe transaction outcomes, i.e., whether or not a purchase occurred and for how much, but the not the search process that consumers engage in. Further, there is no “social” aspect to the email offers - i.e., recipients cannot forward the emails to friends for them to redeem the promotions.

In order to retain buyers, the platform uses email promotional offers (hereafter, “offers”) extensively. A first-time buyer, upon creating an account, specifies his preferences over event genres and elects to opt into or opt out of receiving emails about events and offers. We focus on individuals who opt into receiving emails; they comprise about 50% of a random sample of individuals who have completed a transaction on the website in the past. If a user visits the platform’s website and received an offer, the website may display to her a banner highlighting the offer.

Table 1 describes a few of the observed offers. All offers are valid for a fixed time period when the offered discount is available. To avail a discount offered, the user has to actively input a

\textsuperscript{1}We thank the Wharton Consumer Analytics Initiative (WCAI) for providing us the data.
code corresponding to the offer while completing the transaction on the website. The offers are targeted; each offer is sent to a subset of the population typically segmented based on characteristics of the recipients. The nature and the extent of discount varies across the offers: some offer free shipping of tickets, others provide percentage discounts. The usage of some offers is restricted: they may be applicable to tickets of a certain genre; they may also be associated with a spending threshold (e.g., on purchases of $100 or more). Because each offer differs in its type of discount and its restrictions on usage, the offers in our data lie somewhere in the range between traditional offline coupons that are specific to one “product” (e.g., $0.50 off a box of Cheerios cereal) and general retailer discounts that are not (e.g., CVS offering a 20% discount on purchases of $20 or more).

Experiment Design

This setting is especially unique because the platform ran a large subset of its promotional offers as experiments. Just as any offer, the experimental offers were targeted, possibly based on individuals’ characteristics and past buying behavior. The number of individuals targeted with the offers also varied. But within the pool of individuals targeted for an experimental offer, individuals are randomly partitioned into a group that actually receives the offer (treatment) and a group that is held out from receiving the offer (control). We define a campaign to be any given treatment/control pair of conditions. We refer to a campaign as an experiment or an experimental campaign when it has both a treatment and a control group.

3.2 Data

Our data comprise a sample of 95,753 individuals from the population of the platform’s buyers. Out of these, we analyze the 52,043 buyers who opted into receiving emails. This sample of individuals was randomly drawn from a subpopulation of the buyers with accounts on the platform whose first purchases occurred between January 2007 and December 2010.

We observe the campaigns every individual was targeted with, as well as whether or not the individual received an offer or was held out (as part of the control group for that email offer). Hereafter, we refer to individuals who were in an experiment as “targeted” individuals, regardless
of whether they were treated or untreated. The features associated with the transactions and transacted items for all of the buyers in our sample are also observed. In particular, we observe the time and date the buyer transacted, the genre of the event purchased, the quantity of tickets purchased, the buyer’s expenditure, and whether the buyer redeemed an offer for this transaction. For a large subset of the individuals (82% of the total), we also observe coarsened average demographics in a nondisclosed region near each individuals. As a rough idea of the size of these transactions, the average transaction size in the data is $284 with a standard deviation of $458\textsuperscript{2}. Table 2 reports summary statistics comparing all buyers with just opt in buyers. Based on observables, it appears as though consumers who opted in for emails are similar to those that opted out.

We observe information on a total of 122 campaigns. Figure 1 shows the distribution of the length of the time window in which the offers are valid; most of the offers expire either in two weeks or in a month. Of the campaigns, 76 are experimental, i.e., have a treatment and a control group. Figure 2 shows the distribution of individuals by the number of experimental campaigns they are exposed to over the course of the data. More than 50% of the individuals are a part of only one experiment, with a majority of the remaining individuals being a part of between two and four experiments. Table 3 shows the distribution of the number of targeted individuals across experimental campaigns. Six of the experiments contain fewer than 100 individuals, about half of the experiments contain between 100 and 300 such individuals, and the distribution is skewed with a few experiments with more than 1500 individuals.

For this study we focus on the 70 experimental campaigns with more than 100 targeted individuals in each campaign. In total we have 71,445 instances of opt-in individuals being targeted by experimental campaigns. Because of the experimentation, 61,377 instances actually received an offer, the rest corresponded to instances in the control condition.

\textsuperscript{2}These numbers ignore the first transactions from each buyer, since buyers are not eligible for offers during their first transaction. The average is $279 with a standard deviation of $431 if the first transactions are included.
3.3 Randomization checks

If randomization were correctly implemented, the probability of getting allocated to the treatment group within an experimental campaign would not be correlated with individual characteristics and past transactions. We check for such correlation in the data by regressing a dummy indicator of whether an individual was allocated to the treatment condition on demographic information and information on past transactions: the number of past transactions, total past expenditure, number of offers redeemed and the number of past campaigns targeted. Since the check is for systematic variation in within campaign treatment probability, we add 70 campaign fixed effects to the regression. Estimates show that none of the coefficients related to demographic or past transactions are individually or jointly statistically significant (p-value = 0.6). This test supports the firm’s contention (and thus our assumption) that, within an experimental campaign, an individual’s allocation to the treatment vs. control condition is random.

3.4 Raw data comparison of treatment and control means

We check in the raw data, for any evidence of the impact of the experimental offers across the experiments. We plot the distribution of average expenditures in the treatment and control groups across the 70 experiments in Figure 4. The plot shows that the distribution of the mean expenditures shifts to the right because of the experimental offers. This provides us with some reassurance that the effects of the offers are meaningful in a generalizable way across the experiments. In the rest of the paper, we present our approach and analysis to quantify the average causal effects across the offers and statistically test a variety of hypothesis relating to how they affect the consumer purchase behaviors.

4 Empirical Approach

As the platform’s profits depend directly on users’ expenditures, our primary aim is to understand the impact of the email promotions on users’ total expenditures during the period of validity of the promotion (denoted by $Y$) and to compare the difference in expenditures across
treatment and control groups with costs associated with the discounts given. It may, prima
facie, seem more intuitive to look at metrics that are more specific to the promotions such as
the redemptions and expenditures associated with the promoted items. As noted above, how-
ever, the lack of specificity in several of the promotions makes this more challenging. Later we
will look at genre-specific promotions (for which we can track expenditures) as well as redemp-
tion rates. Before we proceed to the analysis, we highlight the following points related to our
empirical strategy.

First, note that we cannot estimate the impact of these promotions on individuals who were
never in an experiment run by the platform. Indexing individual users by $i$ and experimental
campaigns by $j$, we aim to estimate

$$
\theta \equiv E_{ij} (Y_{ij}^1 - Y_{ij}^0 | D_{ij} = 1)
$$

(1)

where $Y_{ij}^1$ and $Y_{ij}^0$ are the expenditures for $i$ in the presence and the absence of an offer and
$D_{ij}$ is a dummy indicator of whether the individual $i$ is a part of experiment $j$. In terms of
the notation in the treatment effects literature, $\theta$ would be referred to as the average treatment
effect on the treated population.

Second, the proportion of individuals who get allocated to the treatment group varies across
experiments. Figure 3 shows the distribution of the treatment propensities across experiments.
Note that the treatment propensity across the 70 experiments varies significantly, ranging from
0.31 to 0.97 with an average of 0.85. This could be a consequence of the firm managing costs
associated with experimentation. For example, if the target population is very receptive to offers,
keeping a larger control group is more expensive because of the opportunity cost - foregone sales
due to sending no offers. Because of this variation in the data, we estimate $\theta$ by estimating
a separate treatment effect ($\theta_j$) for every experiment $j$, and compute a sample weighted mean
effect.$^3$ Since every experiment $j$ is a randomized experiment, comparison of the treatment and
the control group will give an unbiased estimate of the experiment specific treatment effect on

$^3$An alternative approach would be to pool data from all experiments and estimate a regression model with one
coefficient for the treatment effect and fixed effects for all the experiments. Appendix A shows that systematic
variation in treatment proportions across experiments can cause such an estimation to produce biased estimates.
the population which was targeted for the experimental offer \( j \). Mathematically,

\[
\theta_j = E_i \left( Y_{ij}^1 - Y_{ij}^0 | D_{ij} = 1 \right)
\]

Suppose the sampling distribution of \( \theta_j \) is estimated to be

\[
\theta_j \sim N \left( \hat{\theta}_j, \hat{\sigma}_j^2 \right)
\]

Given the estimated sampling distribution of \( \theta_j \), we can obtain the sampling distribution of \( \theta \):

\[
\theta = \frac{\sum n_j \theta_j}{\sum n_j} \sim N \left( \frac{\sum n_j \hat{\theta}_j}{\sum n_j}, \frac{\sum n_j^2 \hat{\sigma}_j^2}{(\sum n_j)^2} \right)
\]

where \( n_j \) is the number of individuals in experiment \( j \).

5 Results

In this section we discuss our findings from the data. First, we focus on the impact of experimental offers on expenditure for targeted individuals during the time period when the offers were valid. If the offers are able to incentivize the individuals to purchase from the platform, we should see an increase in the expenditure for the individuals who get the offers relative to individuals who are held out.

5.1 Expenditure while offers are active

Following the approach highlighted in the previous section, we first compute the sampling distribution of the difference in the expenditure by individuals in the treatment group and the control group for every experimental campaign \( j \). This effect is estimated by the OLS estimate of the coefficient \( \theta_j \) in

\[
y_{ij} = \alpha_j + \theta_j T_{ij} + \epsilon_{ij}
\]
\( \theta_j \) for all \( j \) are estimated jointly and the standard errors are robust, clustered by individual; \( T_{ij} \) indicates whether individual \( i \) was in the treatment group for experimental campaign \( j \). We find that the \( \hat{\theta}_j \) are jointly statistically significant across \( j \) (\( p<0.01 \)). We take an average of the sampling distribution of \( \theta_j \) using (2) to get an estimate of \( \theta \), the effect of the experimental offers across campaigns on average. Table 4 shows the average effect. On average, the experimental offers cause the individuals who are targeted to spend \$3.03 more during the time when the offer is active.\(^4\) This estimate is statistically significantly different from zero. In other words, there exists evidence in the data suggesting that, on average, offers do work by increasing the expenditure and, consequently, revenues to the platform from buyers who received the offers relative to from buyers who did not. Individuals in the control group spent \$8.07 on average. Therefore, the experimental offers caused a 37% increase in the spending on the website.

### 5.1.1 Redemption of the offers

The increase in expenditure is at the expense of the discounts given to buyers who would have bought in the absence of the offer. To assess this cost we next focus on the redemption of the offers. Table 5 shows statistics on experimental offer redemption. In total we observe 61,377 instances where the experimental offers were sent to an individual in the treatment group. In this group 1,475 transactions occur and just 57 involve redemption of the offers. This implies a redemption rate of \( \frac{57}{61,377} \approx 0.1\% \). These transactions where offers are redeemed account for total expenditure of \$20,690. The total expenditure in instances of offer redemption is just 11% of the total increase in expenditures caused by the offers as estimated in the previous section (\$3.03\times\#Offers sent=\$185,972). We repeat the calculation of the average effect of the experimental campaigns, ignoring the transactions where offers were redeemed, to compute \( \theta_{\text{No redemption}} \). The estimate reported in Table 5 indicates on average there is an increase of \$2.81 in the expenditure of individuals in the treatment group even when they do not redeem the offers. This analysis suggests that offers increase the expenditure for the individuals beyond

\(^4\)A pertinent question is: what does the estimate look like when we use the alternative approach of estimating one coefficient across all experiments (while still retaining experiment-specific fixed effects)? That approach yields a higher (by 25%) estimate; the estimated effect is \$4, and is statistically significant. Therefore, our estimate of \$3.03 is more conservative. Appendix A discusses the differences between the two approaches and why our approach is more appropriate for this context.
the amount spent on the transactions where they are redeemed.

For each instance of an offer being redeemed, we use the redeemed offers’ descriptions to compute the discount given.\(^5\) In total, we calculate that the 57 instances of offer redemption were given $1,178 in discounts. Therefore, from the perspective of the firm, the experimental campaigns are short-run profitable if an increase in buyer expenditure of $185,972 justifies a spend of $1,178.

We were concerned that some users fail to apply the discount online and call later to avail the discounts. This could lead to an under estimation of the redemption rate observed in our data. To reassure ourselves we contacted our data provider for information on post-purchase redemption. According to the company only 0.253\% of all discounts (0.253\% of 57) were later reimbursed over the phone, i.e., these are instances where the buyer might have inadvertently missed including the discount code from a targeted promotion. Given its very small proportion, we do not think that our results are meaningfully affected by such instances.

**Description of the effects**

Next we attempt to describe how the causal effects across the 70 experimental campaigns vary with characteristics of their target populations. Specifically, we focus on the recency (\(r\)) , frequency (\(f\)) and monetary value (\(m\)) of the past transactions made by the target population.

For every experimental campaign we compute the average \(r, f, m\) of the target population and regress the estimated causal effect of the experimental offer on these factors. Column I in Table 6 shows the results from this regression. The coefficient for \(m\) is statistically significant and positive. This suggests that the offers targeted to individuals with a history of larger purchases with the websites lead to a larger increase in spending. To describe the redemption of the offers in a similar manner, we regress the redemption rates of the offers on these target population characteristics. Column II of Table 6 shows the estimates. We find that the average recency of past transactions plays a significant role. Specifically, the redemption rates of the offers decrease as the time since last transaction increases for the targeted population. The other two factors are not significantly associated with the redemption rates. Since the campaign specifics, such as characteristics of the target population, are decided by the firm non-randomly, we do not

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\(^5\)In cases where the individual had multiple promotional options within an offer, we assume the person chose the best option to get an upper bound on the discounts given by the firm.
interpret these correlations as causal effects.

So far, evidence presented in this section suggests that the experimental offer campaigns have an effect which is different from the traditional view on price reductions. Price reductions are likely to increase purchases by low-valuation consumers by providing them direct incentives due to lower effective price. The consumers in our setting, however, are spending more without actually using the price discounts. Therefore, the prominent mechanism driving the increase in expenditure in our context is different and echoes the “advertising” effect of targeted email offers. We proceed by exploring other aspects of how the email offers impact the increase in expenditure.

5.2 Longer term effects

The above analysis shows that the experimental offers, during the time they are valid, cause an increase in expenditure on the website. The short term increases in expenditure due to the experimental offers, however, may be offset by long term decreases if consumers substitute across time by accelerating their purchases to be during the offer window. Such purchase acceleration has been noted in other contexts in the marketing literature (e.g., Blattberg et al. [1995], Simester et al. [2009]). On the other hand, if the advertising effect of the offers is strong, there may be increases in expenditure in the longer term as well.

To investigate the impact of email offers beyond the time duration while they are active, we compare the expenditures for individuals in the treatment and the control condition after the offers expire. The average increase in expenditures due to email offers is estimated using the same approach as before. For each experimental offer $j$ we estimate the parameters of

$$\tilde{y}_{ij} = \alpha_j + \tilde{\theta}_j T_{ij} + \epsilon_{ij}$$

using OLS with robust standard errors, where $\tilde{y}_{ij}$ is $i$’s total expenditure during a period after offer $j$ expires. We find that $\tilde{\theta}_j$ are jointly statistically significant across $j$, indicating the presence of a long-term effect. Next, we compute the average of the sampling distribution of $\tilde{\theta}_j$ to get the estimate of the average effect, again using (2).
Figure 5 shows the estimates and 90% confidence intervals for average changes in expenditure over longer periods of time while comparing between treatment and control groups across all the experiments. In the week after the offers expire, there is on average a $1.55 increase in expenditure due to treatment. This estimate is statistically significantly greater than zero. Therefore, even after the offer expires and can no longer be used to get discounts, it causes an increase in expenditure. This evidence is also consistent with the advertising effects of the email offers. Beyond a week after expiration, we do not find a significant difference between the treatment and the control groups. So we do not find evidence that offers cause purchase acceleration.

The carry-over effects identified above may be indirect effects of the offers. For example, offers may cause individuals to purchase while the offers are valid and then these individuals may repurchase in the future due to inertia or a positive consumption experience. In the data, we do not observe whether individuals in the treatment group actually remember the offers or spend more due to experiences while the offer is active. We can, however, observe how the carry-over effects vary between individuals who transacted and individuals who did not during the offers. Therefore, we next estimate the interaction of the treatment with a dummy indicator, $X_{ij}$, which takes the value 1 if a transaction occurred during the period of the offer. This is implemented by estimating the coefficients for the following specification:

$$\tilde{y}_{ij} = \alpha_j + \delta_j T_{ij} + \beta_j T_{ij} \times X_{ij} + \gamma_j X_{ij} + \epsilon_{ij}$$

We find that the coefficients are jointly significant across $j$. The estimated average coefficients are displayed in Column III of Table 7. The coefficient on $X_{ij}$, which is an indicator of purchase while the offer was active, is positive and statistically significant. This is expected since individuals who transacted on the website in the past are likely to have a higher preference for the products offered and are therefore likely to spend more in the future. The interaction term is positive and significant too. This suggests that individuals who transacted while the offer was valid are more likely to spend in the week after the offer than individuals who transacted but did not receive the offer. Finally, the coefficient for the treatment term is also positive and marginally
significantly different from zero. Individuals who received the offer but did not transact are spending about $1.05 more than those who did not receive the offer due to the experiment. Therefore, there exists evidence suggesting that the direct effect of the experimental offers may persist, and affect the consumers’ propensity for purchasing at the platform over a period of time after offer expiration.

5.3 Spillovers across categories

In this section, we investigate whether genre specific discounts impact the sales of tickets for other genres. Most of the observations in the data are not for offers which are specific to a genre. There exist many experimental offers, however, which are not applicable for Major League Baseball (MLB) tickets. If the impact of the experimental offers is specific to the genre on discount, we could see substitution by individuals away from the excluded genre to the genre on sale. This would imply that, for offers which exclude MLB, we would expect to see a negative impact of these offers on expenditures on MLB tickets. On the other hand, if the offers broadly drive individuals towards the platform, we may find that these offers increase expenditure on MLB tickets, the excluded genre.

We have 51 offers and 25,154 observations of targeted buyers in campaigns that were not applicable for MLB. For this subset of offers, we estimate the effect of offer treatment on expenditure on MLB or on non-MLB tickets. Table 8 shows the estimates. Note that the estimates are not precise as before because the number of observations is one-third of the total observations in the data. In these data, getting the offer increases the expenditure by $4.04 on average, as shown by Column I in Table 8. Splitting these effects across non-MLB and MLB ticket purchases (Columns II and III), we find there is an increase in expenditure in both categories. We find an average increase of $1.27 in expenditure on MLB tickets despite the fact that the offers were not applicable. This evidence indicates the presence of cross category spillovers effects, similar to those documented in advertising (e.g. Erdem and Sun [2002]).

These spillovers could exist because the offers draw individuals to the retail platform, but these individuals end up buying tickets for the genre they prefer. In this case, we expect the spillovers
to MLB tickets to be driven by individuals who have a prior high propensity to purchase MLB tickets. To check for consistent evidence in the data, we further examine the characteristics of consumers who drive the estimated spillover effects. Specifically, we estimate a model where we interact offer treatment with an indicator of whether the consumer purchased baseball tickets on the website in the past ($\text{Past}_\text{MLB}_{ij}$)

$$y_{ij} = \alpha_j + \delta_j T_{ij} + \beta_j T_{ij} \times \text{Past}_\text{MLB}_{ij} + \gamma_j \text{Past}_\text{MLB}_{ij} + \epsilon_{ij} \quad (3)$$

We find that both sets of coefficients $\beta_j$s and $\delta_j$s are jointly significant. Column IV of Table 8 shows that, on average, the interaction term’s coefficient is positive and statistically significant. The main effect is still positive and marginally statistically significant. These estimates suggest that the experimental offers increase purchases of MLB tickets, mainly for individuals who have made a purchase in this genre in the past; these individuals increase their spending from $1 to $3 in this genre.

6 Discussion and Conclusion

This research improves our understanding of the effect of targeted emailed promotions, finding that such offers can have an advertising-like effect on consumer behavior. In addition to the 37.2% increase in total expenditure during the promotion window, we find that few users redeem offers. We find evidence for carryover effects (expenditure increases even after offer expiry) and cross-category spillovers (expenditure increases on non-promoted products). Though the emailed promotions were targeted, we use only the experimental treatment variation within each targeted group in our estimation. Therefore, we can attribute these effects entirely and directly to the platform’s emailed promotions.

One implication of our research is that firms can gain from the targeted promotions even without having to incur the costs associated with the discounts. A natural question that arises is how these effects change with increased levels of promotion - the size of the discounts or repetition of the promotions. With our data it is hard to answer these questions because (1) we observe little
variation in discount levels within an experiment, and (2) we also see little exogenous variation in the number of promotion emails received. The ability to vary promotion campaigns on these dimensions might allow future studies to examine further the advertising effect we find.

Another area of future research is the investigation of the impact of targeted promotions on consumer search both within and across retail websites, thereby generating more detailed implications for firms. If promotions reduce search across competing retailers, they might have implications for pricing for the platform. At the same time promotions might either inhibit, or increase search on a retailer’s own platform. While our findings on cross category spillovers shed some light on this aspect, detailed search data can potentially yield further insights.
References


A Regression with pooled data

A possible alternate approach for analyzing data from multiple experiments is to pool all the observations and estimate one average treatment effect for all experiments, controlling for differences between experiments using fixed effects. In this section we show that this approach can lead to biased inference. Although each experiment is itself randomized, the experiment designs may be endogenous. For example an experimenter may face different costs of treatment across different segments of consumers. As a result the observed treatment proportion in an experiment may depend on the type of the target population, causing the treatment probability in an experiment to be correlated with preferences of the target population.

To illustrate the bias consider estimating the average treatment effect denoted by \( \theta \) by estimating the following linear fixed effects model

\[
y_{ij} = \theta T_{ij} + \sum_{k=1}^{J} \gamma_k D_{ijk} + \epsilon_{ij}
\]

where a unit of observation is an individual \( i \) in an experiment \( j \). \( T_{ij} \) is a dummy indicator of whether \( i \) was treated in \( j \) and \( D_{ijk} \) is an indicator variable for \( j = k \). The coefficient \( \gamma_k \) is the fixed effect for experiment \( k \), and \( \epsilon_{ij} \) represents the idiosyncratic factors. The OLS estimated coefficient is

\[
\hat{\theta} = \frac{E(T'Qy)}{E(T'QT)}
\]

where \( Q = I - D(D'D)^{-1}D' \) is the projection matrix to the space orthogonal to \( D \). But, if the true model has experiment specific sensitivities to \( T \), then

\[
y_{ij} = \theta_j T_{ij} + \sum_{k=1}^{J} \gamma_k D_{ijk} + \epsilon_{ij}
\]

\[
\implies y_{ij} = \bar{\theta} T_{ij} + \sum_{k=1}^{J} \gamma_k D_{ijk} + (\theta_j - \bar{\theta}) T_{ij} + \epsilon_{ij}
\]
Combining (5) and (6) in the matrix form we get

\[
\hat{\theta} = \frac{E(T'Q (T\hat{\theta} + D\gamma + T(\theta_j - \bar{\theta}) + \epsilon))}{E(T'QT)}
\]

\[
= \bar{\theta} + \frac{E(T'QD)\gamma}{E(T'QT)} + \frac{E(T'Q (T(\theta_j - \bar{\theta})))}{E(T'QT)} + \frac{E(T'Q\epsilon)}{E(T'QT)}
\]

The transition from the second to the third step follows because \(QD = 0\) and \(T'Q\epsilon = T'\epsilon = 0\) by definition of \(Q\). Therefore the OLS estimate \(\hat{\theta}\) from (4) provides a biased estimate of the average treatment effect \(\bar{\theta}\), where the bias, \(\frac{E(T'Q (T(\theta_j - \bar{\theta})))}{E(T'QT)}\), is non-zero if the likelihood of treatment (given participation in an experiment) is correlated with the treatment sensitivities, \(\theta_j\), of the segments of individuals in each experiment.

The above discussion shows that consistency of the estimates from the regression model (4) is dependent on whether the decision for the “degree” of experimentation is correlated with heterogeneity in treatment sensitivity across different experiments. Since in the data we observe some aggregate population characteristics for the areas where the individuals live in, we can investigate the relation between these observed characteristics of the target population and the treatment proportions. For every experimental offer \(j\) we compute the average target population characteristics \(\bar{X}_j\) and the proportion of the target population treated \(t_j\). In order to understand the relationship between \(t_j\) and observed characteristics, we regress \(t_j\) on \(\bar{X}_j\). Table 9 shows the coefficients of the regression. Note that several estimated partial correlations are statistically significant. When the average household income of the target population increases, the size of the treatment group relative to the control group decreases. In such a case where the experiment treatment proportion is correlated with characteristics of the population, it may also be correlated with treatment sensitivities of the individuals. Therefore, the above approach of pooling the data across experiments and using only experiment-level fixed effects as controls may lead to a biased estimated average treatment effect.
<table>
<thead>
<tr>
<th>Starting Date</th>
<th>Ending Date</th>
<th>Description of Offer</th>
</tr>
</thead>
<tbody>
<tr>
<td>7/1/2010</td>
<td>10/1/2010</td>
<td>You have 3 OfferCodes in your account, each good for free delivery off of any transaction.</td>
</tr>
<tr>
<td>8/13/2010</td>
<td>9/13/2010</td>
<td>Spend $200 or more and get free delivery (excluding MLB tickets). OR Spend $300 or more and get 10% off (excluding MLB tickets). OR Spend $400 or more and get 15% off (excluding MLB tickets).</td>
</tr>
<tr>
<td>9/9/2010</td>
<td>10/10/2010</td>
<td>Purchase one time in the next 30 days and get 10% off (excluding MLB tickets). Purchase a 2nd time within 30 days and get 15% off (excluding MLB tickets).</td>
</tr>
<tr>
<td>1/28/2011</td>
<td>2/11/2011</td>
<td>5% Off Offer Group</td>
</tr>
</tbody>
</table>

Table 1: Examples of promotional offers. Offer descriptions provided by retailer.

<table>
<thead>
<tr>
<th></th>
<th>All Users</th>
<th>Opt-In Users</th>
</tr>
</thead>
<tbody>
<tr>
<td># Transactions/Year</td>
<td>0.461 (1.546)</td>
<td>0.490 (1.684)</td>
</tr>
<tr>
<td>$ Expenditure/Year</td>
<td>125.09 (955.30)</td>
<td>135.12 (941.56)</td>
</tr>
<tr>
<td># Tickets/Year</td>
<td>1.255 (4.616)</td>
<td>1.332 (5.028)</td>
</tr>
<tr>
<td>Region Population</td>
<td>19,896.5 (16,420.3)</td>
<td>19,945.3 (16,536.7)</td>
</tr>
<tr>
<td>% Black</td>
<td>3.50% (3.95%)</td>
<td>3.51% (3.96%)</td>
</tr>
<tr>
<td>% Non-Family</td>
<td>9.93% (18.24%)</td>
<td>9.03% (17.75%)</td>
</tr>
<tr>
<td>Avg. Family Size</td>
<td>2.915 (0.319)</td>
<td>2.930 (0.319)</td>
</tr>
<tr>
<td>Avg. HH Income</td>
<td>$ 48,311.9 ($ 32,908.5)</td>
<td>$ 46,850.3 ($ 32,746.3)</td>
</tr>
<tr>
<td>Total # Offers</td>
<td>1.049 (1.098)</td>
<td>1.297 (1.154)</td>
</tr>
<tr>
<td>Total # Experiments</td>
<td>1.136 (1.097)</td>
<td>1.383 (1.136)</td>
</tr>
<tr>
<td>Total # Experimental Offers</td>
<td>0.980 (1.024)</td>
<td>1.190 (1.062)</td>
</tr>
<tr>
<td>N</td>
<td>95,753</td>
<td>52,043</td>
</tr>
</tbody>
</table>

Table 2: Mean of coarsened (to preserve anonymity) buyer demographics (standard deviations in parentheses) comparing all buyers with buyers who opted in for emails.

<table>
<thead>
<tr>
<th>Num. of individuals in the experiment</th>
<th>Num. Experiments</th>
<th>Treatment probability</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Mean</td>
</tr>
<tr>
<td>&lt;100</td>
<td>6</td>
<td>68%</td>
</tr>
<tr>
<td>100 to 200</td>
<td>20</td>
<td>85%</td>
</tr>
<tr>
<td>200 to 300</td>
<td>11</td>
<td>82%</td>
</tr>
<tr>
<td>300 to 1000</td>
<td>16</td>
<td>85%</td>
</tr>
<tr>
<td>1000 to 1500</td>
<td>13</td>
<td>87%</td>
</tr>
<tr>
<td>&gt;1500</td>
<td>10</td>
<td>86%</td>
</tr>
</tbody>
</table>

Table 3: Distribution of the number of observations across experiments.
<table>
<thead>
<tr>
<th>Estimate</th>
<th>Std Err</th>
</tr>
</thead>
<tbody>
<tr>
<td>Avg. effect on expenditure across experiment offers</td>
<td>3.03**</td>
</tr>
<tr>
<td>Avg. effect across experiment offers (ignoring transactions where an offer was redeemed)</td>
<td>2.81**</td>
</tr>
<tr>
<td>Avg. expenditure in the control group</td>
<td>8.07**</td>
</tr>
</tbody>
</table>

Table 4: Sampling distributions of the average effect of the experimental offers on dollar expenditure (difference between treatment and control groups) and of control group expenditure for relative comparison.

| # Individuals who get the offer | 61,377 |
| # Transactions | 1,475 |
| #Transactions redeeming the offers | 57 |
| Redemption rate: $\frac{\#\text{Transactions redeeming}}{\#\text{Offers sent}}$ | 0.1% |
| Expenditure in transactions with offer redemption | $20,690 |
| Estimate of the discounts given for the 57 redeemed offers | $1,178 |
| Total increase in expenditure due to offers: $\theta \times \#\text{Offers sent}$ | $3.03 \times 61,377 = $185,972 |

Table 5: Information on transactions with or without redemption of offers to compare the increased revenue from experimental offers with the redemption costs of providing these offers.

<table>
<thead>
<tr>
<th>Column I</th>
<th>Column II</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dep Measure: Estimated causal effect of the experimental offer</td>
<td>Dep Measure: Redemption rate of the experimental offer (%)</td>
</tr>
<tr>
<td>Coef</td>
<td>Std Err</td>
</tr>
<tr>
<td>Recency of the past transaction $(r)$ in days</td>
<td>-0.01</td>
</tr>
<tr>
<td>Number of past transactions with the website $(f)$</td>
<td>-2.45</td>
</tr>
<tr>
<td>Total monetary value of past transactions $(m)$ in dollars</td>
<td>0.02**</td>
</tr>
<tr>
<td>N=70</td>
<td></td>
</tr>
</tbody>
</table>

Table 6: Correlations of estimated effects of offers on expenditure with characteristics (average recency, frequency, and monetary value) of their respective target populations over 70 experimental offers.
Table 7: Measuring the impact of experimental offers on expenditure in the week after experimentation. The estimates are further split between buyers who purchased during the offer and those who did not.

<table>
<thead>
<tr>
<th>Treatment</th>
<th>I</th>
<th>II</th>
<th>III</th>
</tr>
</thead>
<tbody>
<tr>
<td>Coef</td>
<td>se</td>
<td>Coef</td>
<td>se</td>
</tr>
<tr>
<td>1.55**</td>
<td>0.45</td>
<td>1.65**</td>
<td>0.25</td>
</tr>
<tr>
<td>24.37**</td>
<td>5.58</td>
<td>17.40*</td>
<td>9.08</td>
</tr>
</tbody>
</table>

Table 8: Investigating the treatment effect for the subset of experimental offers which were not applicable for MLB tickets. Further interacted with expenditures on MLB or non-MLB tickets, showing that genre spillovers are present.

<table>
<thead>
<tr>
<th>Column I Total Expenditure</th>
<th>Column II On Non-MLB tickets</th>
<th>Column III On MLB tickets</th>
<th>Column IV On MLB tickets</th>
</tr>
</thead>
<tbody>
<tr>
<td>Coef</td>
<td>Se</td>
<td>Coef</td>
<td>Se</td>
</tr>
<tr>
<td>Treatment</td>
<td>4.04** (1.96)</td>
<td>2.77 (1.89)</td>
<td>1.27** (0.37)</td>
</tr>
<tr>
<td>Treatment x (MLB Purchase in the past)</td>
<td>3.10** (0.36)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>MLB Purchase in the past</td>
<td></td>
<td></td>
<td>0.32* (0.18)</td>
</tr>
<tr>
<td>No Treatment</td>
<td>10.92** (1.77)</td>
<td>9.81** (1.68)</td>
<td>1.11** (0.39)</td>
</tr>
</tbody>
</table>

Table 9: Relationship between the proportion of the population treated in an experiment with the observed characteristics of the population.

<table>
<thead>
<tr>
<th>Dep var: proportion of treatment in the experiment</th>
</tr>
</thead>
<tbody>
<tr>
<td>% Black population</td>
</tr>
<tr>
<td>Log Av. Income</td>
</tr>
<tr>
<td>Log Population</td>
</tr>
<tr>
<td>Av. Family size</td>
</tr>
<tr>
<td>Constant</td>
</tr>
</tbody>
</table>

*** p<0.01, ** p<0.05, * p<0.1
Figure 1: Distribution of the number of days the offers are valid.

Figure 2: Distribution of the number of times targeted (assignment to treatment or control groups) across opt-in buyers in the data.
Figure 3: Distribution of treatment probabilities across experiments.
Figure 4: Kernel-smoothed (bandwidth = $3$) PDF of average expenditure in treatment or control groups across the 70 experiments where the sample size was greater than 100.

Figure 5: Average difference in expenditure between the treatment and control groups across experiments during and after offer validity.