The Design and Targeting of Compliance Promotions

>> PRELIMINARY AND INCOMPLETE, PLEASE DO NOT CITE <<

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Abstract

This paper considers an experimental-based approach for the optimal design and targeting of compliance promotions. Compliance promotions involve optional participation on the behalf of a customer. For example, a physician must consent to see a detailer, and consumers must redeem coupons to obtain a discount. Compliance decisions change the mix of customers participating in the promotion, and therefore how the promotion affects sales. Linking compliance decisions to sales outcomes is an especially acute problem in the context of field experiments, as policy optimization often necessitates extrapolation beyond the observed cells of the experiment to a different mix of complying customers.

Our approach to extrapolation in the context of experiments involves four components: i) an experiment to exogenously vary the parameters of the compliance promotion; ii) a means to identify which promotion features can be causally extrapolated; iii) an approach to extrapolate those causal effects, and iv) an optimization over the compliance promotion features, conditioned on the extrapolation. The extrapolation approach is easy to estimate, accommodates two-sided non-compliance due to unobserved heterogeneity, and establishes partial identification bounds of causal effects. Applying the approach to a hotel loyalty promotion where customers must visit enough hotels to earn bonus loyalty points, we find extrapolations that ignore effects of unobserved heterogeneity on compliance lead to sub-optimal promotional designs.

Keywords: Reward promotions, Field experiments, IV estimation, Marginal treatment effects.

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1 Introduction

1.1 Overview

Consider a credit card retention offer designed to prevent customers from attriting. The card issuer faces two major questions when developing this promotion: i) who should receive the offer (targeting), and ii) what the level of the retention offer should be (design). To aid these decisions, firms often conduct field experiments to ascertain how different consumer segments respond to promotions with various features (Ascarza, 2018). Upon observing customer responses in the field experiment, the firm can simply choose, for each segment, the offer with the highest expected customer profit, net of reward costs. For example, a common credit card promotion offers a retention bonus to customers who renew, in the hopes that the subset of customers encouraged by the bonus to renew then spends enough after renewal to offset the total cost of the retention bonuses. Using an experiment, the credit card issuer might learn that a $50 retention bonus generates higher net profit than a $25 bonus. However, this simple example raises the question of whether a lower ($20), intermediate ($40) or higher ($60) retention bonus might have been even more profitable. Unless the card issuer included $20, $40, and $60 reward levels as part of the original experiment, it will be necessary to interpolate or extrapolate customer spending and promotion costs to these new reward levels.¹

One option available to the card issuer is to regress customer spending on the reward levels included in the experiment, and use a linear extrapolation to predict spending at other reward levels. But such an approach might ignore how different reward levels changes the mix of customers retained due to the promotion, and how average spending after renewal varies i) across different mixes of customers for the same promotion, and ii) within the same mix of customers for different promotions. For example, customers who renew their cards when offered larger bonuses might be more price-sensitive than those who renew when offered smaller bonuses. If retention rates and the average spending among retained customers are both linear functions of the reward offered, then a linear extrapolation might make sense. But even small changes to the mix of customers who are retained might correspond with large differences in post-renewal spending, contradicting the assumed linearity and thereby biasing predictions. Moreover, unobserved factors that simultaneously

¹ Hereafter we use *extrapolation* to refer to both interpolation and extrapolation, and *interpolation* when referring exclusively to interpolation.
affect: i) which customers satisfy the terms of promotion (e.g., card renewal), and ii) the outcome of interest (e.g., average spending after renewal), can further complicate the task of extrapolation from experimental results, and thus the optimization of the promotion.

The goal of this paper is to clarify the nature of these complications, demonstrate how ignoring them can lead to suboptimal promotions, provide practical guidance for how to overcome these issues, and illustrate our approach with an empirical application. We consider the optimal design of promotions in settings where: i) the promotion’s features and unobserved customer heterogeneity jointly determine the mix of customers whose behavior satisfies the terms of the promotion; and where ii) this particular mix of customers determines the incremental, net profit generated by the promotion. We refer to such promotions as *compliance promotions* because they are characterized by what is termed *two-sided noncompliance* in field experiments—customers who are not offered the promotion may nevertheless behave in a way that satisfies its terms; and customers who are offered the promotion might not take advantage of it.

Compliance promotions in marketing are commonplace. Coupons are one example: Only a fraction of consumers who receive coupons end up using them (Venkatesan and Farris, 2012; Noble et al., 2017; Ghose et al., 2019), those who do redeem coupons may differ from those who do not, and some customers who redeem coupons might have purchased even without the coupon. Another example is pharmaceutical detailing, where physicians who consent to meet with detailers might already exhibit prescribing behavior that differs from physicians who decline to meet (Narayanan and Manchanda, 2009). Loyalty reward promotions (Kumar and Reinartz, 2018) are yet another example, where only a small portion of customers reach the promotion’s purchase threshold as a consequence of being offered a reward, with the remainder reaching or not reaching the threshold irrespective of the reward offer.

For these and other compliance promotions, the *incremental* value (revenue less selling costs) from the promotion is generated entirely (or nearly so) by the subset of customers who would satisfy the terms of the promotion if offered the coupon, meeting, or reward; but only if they are offered the promotion (otherwise their behavior would not satisfy the terms). In the literature on experiments with two-sided noncompliance, these individuals are referred to as *compliers*.\(^2\) Customers who meet

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\(^2\) Here and throughout this paper, we make the standard assumption of *monotonicity*, meaning there are no *defiers*—customers who would satisfy the promotional terms if they were not offered the promotion, but would not satisfy the terms if they were offered the promotion.
the terms of the promotion, regardless of whether they are offered it, are referred to as always-takers in the literature. Their behavior does not affect the gains from the promotion, but may contribute to the promotion’s fixed costs (e.g., if all customers making a purchase receive a reward). Thus, central to the task of optimizing most compliance promotions with data from field experiments is: i) obtaining a consistent estimate of the average change in the outcome variable (e.g., margin from purchases) generated by compliers meeting the terms of the promotion; and ii) extrapolating these estimated causal effects to promotions that were not part of the original experiment. We outline an approach to identify when extrapolation of these causal effects is feasible. We further show that approaches to extrapolation, which may be appropriate in settings where compliance is not a concern, are usually ill-suited for compliance promotions. Such approaches are typically predicated on an unconfoundedness assumption—that there are no unobserved factors that simultaneously affect: i) whether a customer satisfies the terms of the promotion, and ii) the (potential) outcomes that would arise if that customer does nor does not satisfy the promotion’s terms. We show, theoretically and empirically, that failing to account for these unobserved factors can lead to biased predictions and suboptimal promotion designs.

There are situations in which optimizing compliance promotions using data from a field experiment is not as complicated as we have characterized. First, if one can observe, in data, all of the factors that are believed to jointly affect whether a customer meets the terms of a promotion and their potential outcomes, then the unconfoundedness assumption is appropriate. One can then apply a wide array of tools to estimate (and extrapolate) causal effects, and use these to find a set of optimal promotions (see Powers et al., 2018 and Jacob, 2020 for recent summaries). Second, in settings where the optimality of a promotion is invariant to the mix of customers who meet the terms, then one can sidestep issues of two-sided noncompliance entirely by focusing on intent-to-treat (ITT) effects. Hitsch and Misra (2018), for example, use a randomized experiment to consider the effect of catalog targeting on profits across customer segments. Whether a customer uses a catalog and how much profit they generate are likely confounded, but due to random assignment within the experiment, the intent-to-treat effect of mailing catalogs is not confounded with profits. At the same time, the company incurs the cost of the promotion when they mail a catalog, not when the customer uses one to make a purchase. Because the effect of catalog use on compliers’ spending is accounted for by the ITT effects, and the promotion’s costs are independent of which customers
are compliers, the task of finding an optimal promotion can proceed by i) assuming the unobserved factors determining catalog use have approximately the same effect on spending for all customers, and ii) extrapolating incremental intent-to-treat effects to new groups of customers, subject to the incremental costs of mailing them catalogs. Third, and perhaps trivially, if there is a finite set of promotions under consideration, and if one can design a field experiment measuring the net profit from each (versus a control of no promotion offer), then the optimal design can be derived directly from the experimental data without the need for extrapolation.

In all other settings—where unobserved customer heterogeneity jointly affects who satisfies the terms of the promotion and their outcome of interest; where the profitability of the promotion depends on this mix of customers; and where extrapolation outside the cells of a field experiment is desired—in these settings, the complications just described are consequential.

Given that extrapolating causal effects to all potential levels of promotional design (not just those in the experiment) across heterogeneous customers is central to the optimal design of compliance promotions, we seek to extend the literature on heterogeneous treatment effects in marketing by considering the issues of unobservable heterogeneity and extrapolation.

1.2 Understanding and Overcoming Challenges When Optimizing Compliance Promotions

To illustrate our approach, consider a supermarket loyalty promotion in which customers who spend at least $50 during a shopping trip in the next week will earn a $10 discount on tickets to a popular, local theme park. The net profit from the promotion depends on two factors. One factor is the increase in store margin generated by compliers—customers who will spend $50 or more, but only if they are offered the promotion (otherwise they will spend less than $50). The other factor is the cost of the ticket discounts, which depends on the total number of customers who spend $50 or more, whether they are compliers or always takers. The optimal design of this compliance promotion would consider: i) the minimum level of spending (the spending hurdle), and ii) the size of the discount on the theme park tickets (the reward). The optimal targeting of this promotion would consider which designs are best for which customers. The objective of the firm is typically to first choose the target, and then optimize the design of the promotion offered to each target in order to maximize short-term profits.
Our approach assumes that the firm is able to conduct a randomized field experiment with a random sample of these (potentially) targeted customers. In this field experiment, customers are randomized, and some are offered a promotion (the *offer group*); the remainder are not offered a promotion (the *control group*). Within the offer group, customers are further randomized to be offered promotions with different features. In the supermarket example, the store might randomize the minimum spending required (the hurdle) over the values \{\$40, \$50, \$60\}, and the size of the theme park discount (the reward) over the values \{\$7.50, \$10, \$15\}. After the experiment, the store would observe, for each customer, i) which promotion (hurdle and reward) they were offered (if any); ii) their *promotion status*—whether their behavior met the terms of the promotion (spending at or above the hurdle), $D = 1$, or not, $D = 0$, regardless of whether they were actually offered the promotion or in the control group; and iii) their *outcome*—how much they spent during the promotion, $Y$. The causal effect of interest is the change in margin among compliers—customers who spent more than the hurdle because they were offered the promotion, but who would have spent less than the hurdle had they not received the offer. Furthermore, the store would like to extrapolate these causal effects (as well as the expected costs of the rewards), to other spending hurdles and reward levels that were not in the experiment. As we show, the supermarket in this example will not be able to achieve all that it wants, due to the complications described earlier.

The first challenge that arises is due to unobserved factors affecting both: i) whether customers spend enough to meet the terms of the promotion—their promotion status, $D$; and ii) how much they spend during the promotion—their outcome, $Y$. In other words, $D$ and $Y$ are very likely confounded by one or more unobserved variables. To address this issue, we use methods based on marginal treatment effects (MTEs) for the estimation and extrapolation of causal effects in the face of unobserved customer heterogeneity and experimental noncompliance (Heckman and Vytlacil, 1999; Heckman and Vytlacil, 2005; Mogstad and Torgovitsky, 2018; Mogstad et al., 2018). The MTE is defined as the expected causal effect of a particular treatment (e.g., meeting the terms of the promotion) on an outcome of interest (e.g., spending), conditioned on a specific realized level of observed and unobserved customer heterogeneity. The MTE serves as a building block for defining, estimating, and ultimately extrapolating more complex treatment effects (Heckman and Vytlacil, 2007). A treatment effect that is especially relevant for compliance promotions is the policy-relevant treatment effect (PRTE; Heckman and Vytlacil, 2001). One PRTE of interest for the supermarket
might be the incremental, net profit from offering a promotion with a particular spending hurdle and reward (versus not offering a promotion). This PRTE is increased by the incremental margin from customers who comply with the promotion offer, and decreased by the reward costs of the promotion. By comparing (extrapolated) PRTEs for promotions with different features, we can identify a subset of promotions that yield the greatest expected, incremental profit.

The presence of unobserved customer heterogeneity also leads to a second challenge. For some features of a promotion, it might not be possible to extrapolate causal effects to levels that were not part of the original experiment. In particular, we show that it is only possible to extrapolate causal effects over promotion features that are valid instrumental variables for the compliance status variable, $D$ (whether customers meet the terms of the promotion). That is, these promotion features need to satisfy an exclusion restriction, meaning they have a direct causal effect on which customers comply with the promotion, but otherwise have no direct causal effect on the outcome. In the supermarket example, the size of the theme park discount arguably has such a property: It provides an incentive to spend enough to reach the hurdle, but otherwise has no meaningful direct effect on spending. For example, we would not expect customers willing to spend $50 to receive a $10 discount to spend more than $50 if they were offered a $15 discount. By contrast, the spending hurdle in this example does not have this property: It not only affects the incentive to meet the terms of the promotion, but also has a direct effect on how much customers spend. For example, some customers who would spend $50 to receive the tickets might spend $60 if their offer had a higher hurdle.

A key insight that emerges from our approach is that it not possible to extrapolate causal effects for promotion features that have a direct effect on the outcome of interest, even if the levels of those features are manipulated experimentally. The exogeneity that comes with random assignment of promotion features is certainly valuable, as it facilitates the estimation of causal effects. But this is exogeneity with respect to factors outside the experiment. Randomization alone cannot resolve the unobserved confounding that happens inside the experiment and affects who meets the promotion’s terms, $D$, and their outcomes of interest, $Y$. Our approach thus entails extrapolating

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3 The term *comply* here refers specifically to the pattern of behavior whereby i) a customer is offered a promotion, ii) their behavior meets the terms of the promotion ($D = 1$), but iii) had they not been offered the promotion, they would not have met the promotion’s terms ($D = 0$). When we refer to the causal effect of compliance on an outcome, we mean the difference between the outcomes that would arise for one of these complying customers at $D = 1$ because they received the offer, versus their outcome at $D = 0$ because they did not receive the offer.
causal effects over promotion features that are (conditionally) excludable from the outcome (i.e. valid instruments) to levels not observed in the original experiment. But these extrapolations are, by necessity, conditional effects, given a particular instance of all other variables that do not satisfy the exclusion restriction. In the running example, the supermarket can estimate, for each spending hurdle used in the experiment, an extrapolated PRTE of the promotion at reward levels not included in the original experiment. Moreover, it can then compare these PRTEs across the different spending hurdles to obtain the promotion with the highest expected profit. But it cannot use the results of the experiment to extrapolate the profitability of promotions to hurdles that were not part of the original experiment.

Given the subtlety of this restriction on which promotion features can be used to extrapolate causal effects, we believe it is frequently violated in practice. To underscore this point, we offer two examples of experimental contexts where the unconfoundedness assumption may not hold, yet extrapolation without an instrumental variable was employed. The first example comes from Danaher (2002), who considers a subscription plan for a telecommunications product. Analyzing a field experiment to assess the effect of access fees and usage charges on retention and usage levels, Danaher (2002) extrapolates access fees and usage charges beyond the manipulated levels to optimize plan pricing. In this context, retention corresponds with promotion status, $D$, post-retention usage is the outcome, $Y$, and access fees and usage charges are the randomized promotion features. While the access fee might well be a valid instrument (affecting retention, but otherwise not directly affecting usage), the usage charge has a clear, direct effect on usage, and thus is not a valid instrument. More recently, Tian and Feinberg (2020) explore the effect of duration-based discounts for subscriptions to an online dating site, where the base price and a discount for longer subscriptions are both manipulated experimentally. In their context, subscribing constitutes promotion status, $D$, the plan that is purchased is the outcome, $Y$, and the base price and discount levels are randomized promotion features. Both base price and discounts for longer subscriptions have direct effects on the chosen plan, thus neither is a valid instrument for subscribing. In both examples, causal effects are extrapolated over variables that simultaneously alter i) the composition of customers complying with the promotion (via $D$), and ii) the causal effect among that same group of customers (via $Y$). But experiments with noncompliance due to unobserved heterogeneity cannot separately identify these effects. Thus these and similar extrapolations in the literature rest on very
strong behavioral assumptions that might not be obvious to most readers.

In sum, our approach consists of conducting an experiment to vary the promotional design, specifying the design parameters that are conditionally excludable and thus can be extrapolated, applying an MTE approach to accommodate unobserved confounding, and using the resulting causal estimates to design optimal promotions for each target. We describe this approach and illustrate its value with an empirical example, in which an international hotel chain seeks to optimize a compliance promotion using data from a field experiment.

1.3 Related Literature

A growing literature in marketing pertains to the extrapolation of treatment effects for targeted promotions, whereby causal effects are estimated from field experiments and then extrapolated to customer segments that were not directly represented in the original experiment (Ascarza, 2018; Hitsch and Misra, 2018; Dubé and Misra, 2021; Simester et al., 2020b,a). Our focus is instead on extrapolating treatment effects to new promotion designs, in settings where there is unobserved customer heterogeneity and compliance with the promotion is not mandatory. Such settings are common. For example, even though a hotel or airline might observe behavioral and demographic variables about its customers, compliance with the terms of a promotion is likely affected by a host of unobserved factors, including the motivation or ability to attain a reward (e.g., a planned vacation, family commitments, or change in work status). These factors are also likely to affect the behavior of interest for the promotion (e.g., purchasing business class tickets, reserving a suite of hotel rooms, or timing a holiday), further complicating extrapolation of causal effects and the optimization of promotions.

We apply our approach to optimizing compliance promotions in the context of loyalty promotions (see Kumar and Reinartz, 2018 for a summary). Overall, our research differs from this stream of loyalty promotion research on a number of dimensions. First, our emphasis is on optimizing short-term loyalty promotions, rather than long-term loyalty programs. Second, our approach relies on field experiments to estimate the effects of different promotion features on outcomes, where, owing to the practical limitations of such experiments, extrapolation is necessary to optimize promotion designs.

Our empirical application is most similar to that of Wang et al. (2016), who study the effects of
promotion compliance on purchase behavior after the promotion has ended. Their analysis entails measuring how different spending hurdles affect the total length of hotel stays during and after the promotion period. They experimentally manipulate spending hurdles, and recover causal effects on hotel stays using: i) a regression model of promotion status (reaching the spending hurdle), $D$, as a function of customer demographics; and ii) a Tobit model of nights stayed, $Y$, conditioned on hurdle attainment, $D$. Our application instead focuses on optimizing promotions using similar experimental data, and with a more robust approach to extrapolation (an implicit assumption in Wang et al. (2016) is that the effect of unobservables on outcomes is independent of hurdle levels, implying that the marginal customers attracted by highest hurdle levels are identical to those attracted by the lowest hurdle levels). In addition, our work emphasizes the optimization of rewards, conditional on hurdles, and we argue that extrapolation over hurdles is not valid in this setting.

A third stream of relevant research relates to the estimation of MTEs using instrumental variables (Mogstad et al., 2018; Heckman and Vytlacil, 1999). We build on this research in several ways. First, we outline conditions under which it is possible to extrapolate experimental treatment effects, and illustrate these using Pearl’s causal framework (Pearl, 2009). Second, we adapt MTE estimation approaches to the context of compliance promotions, and demonstrate the suitability of these approaches in these contexts. We find that approaches predicted on more restrictive assumptions regarding the impact of unobserved factors on outcomes (analogous to the restrictions embedded in the Heckman selection model), perform poorly in our empirical context. Third, we couple the MTE estimation of treatment effects with optimization in the context of compliance promotions to enable their use in marketing settings—settings where not only customer margins, but also promotion costs depend on the mix of customers who comply with the promotion. Fourth, we prove that commonly used linear extrapolation approaches in experimental contexts, such as linear regression assuming unconfoundedness, or linear extrapolation of nonparametric intent-to-treat effects, are likely to be badly misspecified in all but the most exceptional circumstances.

2 Data and Descriptive Analysis

Throughout the paper, we use a field experiment involving a compliance promotion to exemplify extrapolation of causal effects. Our approach is intended to generalize to most compliance pro-
motion settings, so the context is predominantly illustrative of the approach. This section first
details the empirical setting and field experiment, and reports descriptive analyses showing that
the promotional design has a substantial effect on customers’ purchases. We then apply a sim-
ple, non-parametric approach to optimizing promotional designs within the cells of the experiment,
showing profit gains across the experimental cells. The findings from this analysis serve to motivate
the potential to improve profits further via a valid extrapolation approach, which we describe in
the following sections of the paper.

2.1 Example Empirical Setting

The empirical context we use to exemplify our extrapolation procedure involves a reward promo-
tion provided by the InterContinental Hotels Group (IHG), a prominent multinational hospitality
company with $4.6B in annual revenue during 2019.⁴ IHG’s 16 brands include, among others,
Holiday Inn, Crowne Plaza, and Intercontinental. IHG offers a loyalty program which customers
may join for free. The goal of this program is to enhance IHG’s relationship with its customers
and increase their likelihood of selecting IHG brands. Customers in this program received fringe
benefits (e.g., free internet), discounts, reward points, and occasionally promotional reward offers
(our focus). Customers within the loyalty program are sorted into four different loyalty tiers based
on past stay behaviors, with higher tier customers receiving premium services.

Within the various promotional offers embedded within this broader loyalty program, we con-
sider compliance promotions whereby customers are offered a reward in the amount of \( R \) bonus
points if they reach a hurdle of staying at \( H \) different hotels, from the same brand (henceforth
Hotel A), within a defined time period. Consumers self-select into meeting the terms of a given
promotion \( (D = 1) \) or not \( (D = 0) \).

2.2 Field Experiment Design

2.2.1 Sample Size and Composition

Between January 1, 2018, and April 30, 2018, IHG conducted a field experiment involving the
compliance promotion just described. The experiment was conducted with 23,583 randomly selected
customers. Each customer belongs to one of the four aforementioned loyalty tiers, \( T \), and has an
estimated “baseline” number of stays in the absence of the promotion, \( B \). The customer’s tier is

categorical, and reflects their overall past stays and spending across all of the IHG hotel brands. A customer’s baseline is an integer, and reflects their expected stays at Hotel A; this value is computed based on a proprietary predictive algorithm.

### 2.2.2 Experimental Manipulation and Design

The 23,583 customers are randomized into an offer group (16,034 customers) and a control group (7,549 customers). We describe these two groups next.

**Offer Group.** Customers in this group are offered a reward promotion of the form “Receive $R$ bonus points for staying at $H$ different hotels of Hotel A between January 1, 2018, and April 30, 2018.” Customers in the offer group are further block randomized, based on customers’ values of $B$ and $T$, to receive different promotions. Promotions differ in their values of $H$ and $R$, and within each block, up to 4 promotions are offered. For example, a customer in tier $T = 2$ with expected baseline visits of $B = 3$ might be offered a promotion with $H = 4$ and $R = 13,200$ meaning they would receive 13,200 bonus points (valued at roughly $90) if they stay at 4 different hotels of Hotel A over the 4-month duration of the experiment.

The hurdle and reward levels vary across blocks. Customers within the higher baseline groups are offered promotions with higher hurdles (and correspondingly more bonus points) as it would otherwise be too easy for them surpass the hurdle and earn the reward. Likewise, customers in lower baseline groups are not offered promotions with higher hurdles (and correspondingly, fewer bonus points).

**Control Group.** Customers in the control group are not offered a reward promotion, so $R = 0$, and $H$ is undefined. For the purpose of comparing customers in the offer and control groups, however, it is advantageous to define a value of $H$ for customers in the control group. We infer a set of potential values of $H$ that the customer might have been assigned, had they been in the offer group, based on the empirical distribution of hurdle levels among those in the targeted group with the same baseline and tier. Using this empirical distribution, we randomly sample values of $H$ for customers in the control group. Conceptually, these customers meet the terms of the promotion if they stay at $H$ or more hotels, but they receive $R = 0$ points.
### Table 1: Summary Statistics of Baseline, Tier, Hurdle and Reward

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>S.D.</th>
<th>Median</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>2.67</td>
<td>2.04</td>
<td>2</td>
<td>0</td>
<td>17</td>
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<tr>
<td>Tier</td>
<td>2.24</td>
<td>0.89</td>
<td>2</td>
<td>1</td>
<td>4</td>
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<tr>
<td>Hurdle (H)</td>
<td>3.69</td>
<td>2.03</td>
<td>3</td>
<td>2</td>
<td>19</td>
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<tr>
<td>Reward (R) in 1,000 pts</td>
<td>6.22</td>
<td>5.54</td>
<td>5.6</td>
<td>0</td>
<td>54</td>
</tr>
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### Table 2: Comparing Customers across Experimental Groups and Promotion Status

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<td>Stays</td>
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<td>2.60</td>
<td>1</td>
<td>0</td>
<td>26</td>
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<tr>
<td>Offer Group (O)</td>
<td></td>
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<tr>
<td>Control Group (C)</td>
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<tr>
<td>Obs.=16,034</td>
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<tr>
<td>Obs.=7,549</td>
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<tr>
<td>Mean</td>
<td>2.03</td>
<td>2.63</td>
<td>1.98</td>
<td>2.55</td>
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<tr>
<td>S.D.</td>
<td></td>
<td></td>
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<tr>
<td>Difference (O-C)</td>
<td>0.065</td>
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<tr>
<td>p-val of the diff.</td>
<td>0.14</td>
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<thead>
<tr>
<th></th>
<th>Mean</th>
<th>S.D.</th>
<th>Median</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reaching Hurdle (RH)</td>
<td>5.24</td>
<td>3.37</td>
<td>1.21</td>
<td>1.54</td>
<td></td>
</tr>
<tr>
<td>Not Reaching Hurdle (NRH)</td>
<td>1.54</td>
<td>3.37</td>
<td>1.21</td>
<td>1.54</td>
<td></td>
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<tr>
<td>(Stays≥H, Obs.=9,856)</td>
<td></td>
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<tr>
<td>(Stays&lt;H, Obs.=39,194)</td>
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<tr>
<td>Mean</td>
<td>5.24</td>
<td>3.37</td>
<td>1.21</td>
<td>1.54</td>
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<tr>
<td>S.D.</td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Difference (RH-NRH)</td>
<td>4.03</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>p-val of the diff.</td>
<td>0.00</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

### 2.2.3 Experimental Outcomes

**Stays.** Summary statistics of the data are reported in Tables 1 and 2. Table 1 presents the baseline, tier, hurdle, and reward distributions across customers. Rewards are intended to incentivize more stays and are therefore set on average slightly higher than baselines, which reflect predictions of stays in the absence of a promotion. Table 2 compares the numbers of stays between those who received a promotion and the control. On average, customers in the experimentally targeted group stay more than those in the control group but the difference is not statistically significant. However, when controlling fixed effects of baseline and tier via regression, the coefficient of the targeted indicator becomes significantly positive (coef. = 0.09, s.e. = 0.03, p-value < 0.001). Thus, rewards motivate stays across the experimental cells. Conditioning on compliance, we further compare stays for customers who exceed their hurdles (compliers) with those who do not (non-compliers). The table indicates that compliers on average purchase 4 more stays than non-compliers and that the difference is statistically significant.
Offer Group (O)  Control Group (C)  Difference (O-C)  p-val of the diff.
---
Obs.=16,034  Obs.=7,549
Avg. Rate Reaching Hurdle  0.29  0.24  0.05  p<0.001

Table 3: Comparing Hurdle Achievement across Experimental Groups

Compliance. Within the targeted and control groups, we compute the compliance rate for each baseline-tier-hurdle combination (fraction of customers reaching their hurdles). Table 3 reports that the targeted group has a higher average compliance level than the control group (0.29 vs. 0.24), which suggests rewards incentivize customers to reach the hurdles.\(^5\) Figure 1 depicts compliance rate difference between the targeted and control groups for each baseline-tier-hurdle combination. The graph indicates that the target group has higher average compliance rates than the control group, especially for higher baseline-tiers and hurdle levels relative to baseline.

Profits from Promotions. We next assess how the experimental outcomes for a given target set of customers can be used to improve the profitability of a promotion. To assess this, we first construct a policy relevant treatment effect (PRTE). As shown in Appendix XXX, this PRTE is derived from a more primitive local average treatment effect (LATE) among compliers for the promotion defined by a hurdle of \(H\) stays and reward of \(R\) bonus points. Limiting attention for now to values of \(R\) and \(H\) that were included in the experiment, this PRTE is: i) the expected difference in margin from customers in tier \(T\) with baseline expected stays \(B\), when offered a promotion with a spending hurdle of \(H\) stays and a reward of \(R\) bonus points (versus not being offered a promotion); minus ii) the expected cost of the reward points for the promotion. Given target set of customers and spending hurdle, denoted by \(X \equiv \{T, B, H\}\), we can write the PRTE as a function of \(R = r\),

\[
PRT E_{0 \rightarrow r}^{II}(X) = N \cdot (E[Y|X, R = r] - E[Y|X, R = 0]) \cdot mgn - N \cdot cpp \cdot r \cdot E[D|X, R = r] \quad (1)
\]

\(^5\) As some baseline-tier-hurdle combinations have sparse observations, we only compute the compliance rates for those combinations with at least 10 customers in both the target and the control groups.
Figure 1: Average Rate of Hurdle Achievement across Baselines, Tiers, and Hurdles. The y-axis is mislabeled and should read “Difference in proportion reaching hurdle (O-C)” where the “O” means the offer group, and “C” means the control group. To make it easier to compare these differences across groups of customers receiving different hurdles, we pool differences in groups based on the difference between hurdle and baseline (H-B in the plot).
where \( cpp \) and \( mgn \) are the expected cost per reward point and expected margin per stay used by IHG, and \( N \) is the total number of customers in the target group with baseline \( B \) and tier \( T \). Due to random assignment of \( H \) and \( R \) within blocks defined by values of \( B \) and \( T \), nonparametric estimators for the expectations of \( Y \) and \( D \), conditional on \( X = x \) and \( R = 0 \) or \( R = r \), can be obtained from their conditional averages in the experimental data.

The steps involved to compute this estimator are as follows:

1. Obtain information regarding the expected margin per stay, \( mgn \), and the expected cost per reward point, \( cpp \), from IHG.

2. Estimate the total profits, \( \hat{\Pi}_{x,r} \equiv \text{PRTE}^I_{0 \rightarrow r}(X = x) \), for each \( X = x \) and \( R = r \) in the experiment (including \( R = 0 \), using observations from the control group). These estimates are based on average stays, \( Y \), and compliance, \( D \), observed in the data.

3. For each target set of customers and spending hurdle, \( X = x \), compare the average profit across reward levels, \( R \), to ascertain which reward level from the experiment yields the highest expected profit for the target, \( r^*_x \equiv \arg \max_r \hat{\Pi}_{x,r} \).

4. Compute the counterfactual total profits if all targeted customers had been assigned to the most profitable reward level for their cohort, \( \hat{\Pi}^*_x \equiv \hat{\Pi}_{x,r^*_x} \).

5. Finally, compare the profit levels obtained from Step 4 to the profit levels in the control groups to evaluate the profit lift, \( \hat{\Pi}^*_x - \hat{\Pi}_{x,0} \).

Table 4 presents the results of this analysis in terms of profit lift. With 85 baseline-tier-hurdle cohorts, we focus on 10 cohorts to facilitate exposition, and aggregate the results across the remaining baseline-tier-hurdle cohorts via a weighted average. Overall, redesigning the reward promotion would yield a profit gain of 6.8%, showing substantial potential for profit optimization within the range of the experimental variation.

To further illustrate the insights from the non-parametric PRTEs, we zoom in on the case of \( B = 2, T = 2, \) and \( H = 3 \) (the modal cohort in the experiment). The results from this target set

---

6. Our PRTE example considers only changes in reward points, but not changes hurdles. As we explain in Section 3.2, extrapolations of the PRTE for changes in hurdles is not identified in our data context. More generally, identification of extrapolated PRTEs requires that the policy relevant treatment variables are conditionally excludable from outcomes. See Section 3.2.1.

7. Actual profit levels are scaled to preserve confidentiality.
<table>
<thead>
<tr>
<th>Baseline</th>
<th>Tier</th>
<th>Hurdle</th>
<th>Profit Gain Percentage</th>
<th>Number of Customers</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>1</td>
<td>2</td>
<td>2.77</td>
<td>2629</td>
</tr>
<tr>
<td>1</td>
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</tr>
<tr>
<td>1</td>
<td>3</td>
<td>2</td>
<td>14.30</td>
<td>725</td>
</tr>
<tr>
<td>1</td>
<td>4</td>
<td>2</td>
<td>0.00</td>
<td>127</td>
</tr>
<tr>
<td>2</td>
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</tr>
<tr>
<td>2</td>
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<td>3</td>
<td>0.00</td>
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</tr>
<tr>
<td>2</td>
<td>2</td>
<td>3</td>
<td>1.84</td>
<td>4297</td>
</tr>
<tr>
<td>2</td>
<td>3</td>
<td>3</td>
<td>9.45</td>
<td>1255</td>
</tr>
<tr>
<td>2</td>
<td>4</td>
<td>3</td>
<td>18.59</td>
<td>285</td>
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</tbody>
</table>

<table>
<thead>
<tr>
<th>Baseline≠1 and 2</th>
<th>Weighted Profit Gain Percentage</th>
<th>Number of Customers</th>
</tr>
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<tr>
<td>-</td>
<td>-</td>
<td>11.48</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>All Baseline-Hurdle</th>
<th>Weighted Profit Gain Percentage</th>
<th>Number of Customers</th>
</tr>
</thead>
<tbody>
<tr>
<td>-</td>
<td>-</td>
<td>6.80</td>
</tr>
</tbody>
</table>

Table 4: Gains by Reassigning to the Most Profitable Reward Levels

| Points in 1,000 (r) | E[Y|R = r] | E[Y|R = 0] | E[D|R = r] | E[D|R = 0] | LATE | PRTE II |
|---------------------|-----------|-----------|-----------|-----------|------|---------|
| 7.2                 | 1.342     | 1.304     | 0.1660    | 0.1467    | 1.958| 0.9453  |
| 10.8                | 1.363     | 1.304     | 0.1851    | 0.1467    | 1.543| 1.357   |

Table 5: Profit Lift for B = 2, T = 2, and H = 3. All expectations and treatment effects are non-parametric estimates. \( LATE \) is the local average treatment effect, in total stays, from offering \( R = r \) points versus \( R = 0 \) points. \( PRTE II \) is expressed per customer in the segment, with \( mgn \) and \( cpp \) scaled to preserve anonymity.

of customers are captured in Table 5. Comparing the two rows, we observe that the expected increase in profits is higher ($1.36 per person) when 10,800 points are offered, and that the number of stays (1.36) and proportion of customers meeting the spending hurdle (18.5%) are also higher. Compared to the promotion offering a reward of 7,200 bonus points, the promotion offering 10,800 is expected to be more profitable.

Yet the range of reward points in the experiment is limited, and the findings raise the question of whether profits would continue to increase were points increased beyond 10,800. Alternatively, it may be the case that the optimal level of rewards lies between 7,200 and 10,800 bonus points. There may be considerable potential to further improve outcomes via extrapolation, making it
necessary to determine when and how extrapolation can be used in experimental contexts. We discuss extrapolation in the context of general compliance promotions next.

3 Approach

Our modeling and estimation approach consists of two steps. The first step conceptualizes customer outcomes and promotion status (whether they meet the terms of the promotion) as consequences of unobserved customer heterogeneity. Depending on the compliance promotion, the outcome $Y$ and promotion status $D$ might arise simultaneously or sequentially. The sequential view simplifies exposition, so here we describe outcomes in terms of i) self-selection into meeting the promotion’s terms or not (3.1.1), and ii) an outcome conditioned on that promotion status (Section 3.1). We emphasize that this conditioning can be a statistical convenience, and there is no requirement that $D$ and $Y$ arise as a sequence of events. The model describing self-selection into meeting the promotion’s terms links the promotion’s design (e.g., the hurdle and reward offered) to a customer’s promotion status, $D$. The model describing the outcome (e.g. hotel stays), $Y$, conditional on promotion status, $D$, combines the both of these, as well as promotion features, using marginal treatment effects (MTEs). The joint model can then be estimated, extrapolated, and used in policy evaluation; that is, to find the most profitable promotional design for any given target (Section 3.3).

3.1 Marginal Treatment Effects

In what follows, we adapt and describe the MTE approach from Mogstad et al. (2018) to the context of compliance promotions, in order to make policy recommendations on promotional designs. Interested readers are referred to Mogstad et al. (2018) and Mogstad and Torgovitsky (2018) for a more complete treatment of the MTE approach.

3.1.1 Selection into Promotion Status and Effect on Outcomes

We assume that whether a customers meets the terms of the promotion (promotion status, $D$) is a binary state, with $D = 1$ when the terms are met, and $D = 0$ otherwise; and emphasize that a customer who is not offered a promotion can nevertheless have $D = 1$ if their behavior satisfies the terms of the promotion. We consider two potential outcomes of interest: the outcome that is observed if the customer meets the promotion terms, $Y_1$; and the outcome if the customer does not, $Y_0$. Every customer’s observed outcome, $Y$, is related to these potential outcomes through a
switching equation:

\[ Y = DY_1 + (1 - D)Y_0 \]  \hspace{1cm} (2)

Because customers either meet the terms of the promotion or not, only \( Y_1 \) or \( Y_0 \) is observed for any customer. Individual-level treatment effects are therefore never observed in the data, so we focus on estimating expected treatment effects among particular subsets of customers.

The selection equation for promotion status can be written compactly as a function of an unobserved variable and a propensity function (Heckman and Vytlacil, 1999; Heckman and Vytlacil, 2007),

\[ D = I[U \leq p(X, Z)], \]  \hspace{1cm} (3)

where: i) \( p(X, Z) \) is a propensity function, ii) \( X \) is a set of variables affecting both promotion status and outcomes, iii) \( Z \) is a set of variables that weakly increases (or decreases) the likelihood a customer meets the terms of the promotion, but otherwise has no direct effect on the outcome (i.e., \( Z \) contains instruments that are conditionally excludable from \( Y \)), and iv) \( U \) encapsulates the effect of unobserved heterogeneity on the outcome and promotion status, and is normalized to be uniformly distributed for identification purposes, as is standard (i.e., \( U | (X = x) \sim Unif[0, 1] \)). Under the standard monotonicity assumption, Equation 3 implies that higher values of \( U \) correspond with lower rates of meeting the promotion terms, which is standard in the literature on experiments with two-sided non-compliance.\(^8\) In the context of compliance promotions, the unobserved variable \( U \) will typically affect both the promotion status and the outcome, as customers who meet the terms of a promotion are typically different from those who do not. Customers’ promotion statuses, \( D \), are therefore confounded with their outcomes, \( Y \).

3.1.2 Marginal Treatment Effects and Marginal Treatment Response Functions

The MTE function provides the foundation for the extrapolation approach we use. The marginal treatment effect (Heckman and Vytlacil, 1999; Heckman and Vytlacil, 2005) is defined as:

\[ MTE(u, x) = E[Y_1 - Y_0 | U = u, X = x] \]  \hspace{1cm} (4)

\(^8\) Normalizing \( U \) to be uniformly distributed, conditional on \( X \) and \( Z \), and relating higher values of \( U \) to lower rates of compliance are both standard in the literature (see Heckman and Vytlacil, 2007, §3, for further discussion). The normalization arises after positing a more fundamental, unobserved primitive \( V \) with a conditional c.d.f. \( F_{V|X} \), and then defining \( U = F_{V|X}(V) \). In typical marketing applications, higher values of an unobservable variable \( \epsilon \) correspond with a higher choice likelihood. Thus, it might help intuition to think of \( \epsilon = -U \).
The intuition behind the MTE in our example is that it represents the expected difference in hotel stays when reaching or not reaching the hurdle, for a customer with observed characteristics \( X = x \) and unobserved characteristics \( U = u \). Because \( u \) varies continuously, equation 4 implies that treatment effects can vary on the margin for any customer represented by the pair \( \{x, u\} \), and that there is both observed and unobserved heterogeneity in the MTE.

The MTE function can be re-written as the difference in expected potential outcomes, \( Y_1 \) and \( Y_0 \). These expectations are called marginal treatment response (MTR) functions, and are defined as

\[
\begin{align*}
m_0(u, x) &= E[Y_0 | U = u, X = x] \quad \text{and} \quad m_1(u, x) = E[Y_1 | U = u, X = x],
\end{align*}
\]

where \( MTE(u, x) \equiv m_1(u, x) - m_0(u, x) \). The MTR functions can be quite general; as described below, we use a polynomial basis function with interactions between observed \((x)\) and unobserved \((u)\) variables for the MTRs.

### 3.1.3 Target Parameters

The MTRs serve as the building blocks for constructing “target parameters” (Mogstad et al., 2018), including many causal effects (e.g., typical estimands such as the average treatment effect, ATE; conditional average treatment effect, CATE; local average treatment effect, LATE; and PRTE) and empirical moments (e.g., typical estimators such as treatment coefficients from OLS and IV regressions) (Heckman and Vytlacil, 2005, 2007). Denoting these target parameters \( \beta \), and indexing them according to their respective estimators or estimands by \( s \), all of the causal effects and empirical moments discussed in this paper can be expressed using the following weighted function of the MTRs:

\[
\begin{align*}
\beta_s &= E\left[ \int_0^1 m_0(u, x) \omega_s^0(u, x, z) du \right] + E\left[ \int_0^1 m_1(u, x) \omega_s^1(u, x, z) du \right] \quad (6)
\end{align*}
\]

The weights, \( \omega_s^d \), \( d \in \{0, 1\} \), are specific to a particular estimator or estimand, \( s \), and are given by

\[
\begin{align*}
\omega_s^0(u, x, z) &\equiv s(0, x, z) \mathbb{1}[u > p(x, z)] \quad \omega_s^1(u, x, z) \equiv s(1, x, z) \mathbb{1}[u \leq p(x, z)].
\end{align*}
\]

The expectation in (6) is taken with respect to \( X \) and \( Z \), but one can construct target parameters that are conditional on \( X = x \) by modifying the expectation operator. An example of the latter is the conditional on \( X = x \) LATE (CLATE). An example of the former is a homogeneous treatment coefficient from an instrumental variables regression (i.e., one that is adjusted for \( X \), but not interacted with \( X \)).

---

9 The expectation in (6) is taken with respect to \( X \) and \( Z \), but one can construct target parameters that are conditional on \( X = x \) by modifying the expectation operator. An example of the latter is the conditional on \( X = x \) LATE (CLATE). An example of the former is a homogeneous treatment coefficient from an instrumental variables regression (i.e., one that is adjusted for \( X \), but not interacted with \( X \)).
To provide intuition on the role of the weights in defining the target parameters, consider the example of the average treatment effect on the treated (ATT), $E[Y_1 - Y_0 | D = 1]$. The weighting functions for the ATT are given by $\omega^{\text{ATT}}_1 \equiv 1[u \leq p(x,z)]/ \Pr[D = 1 | X = x] = -\omega^{\text{ATT}}_0$ (Heckman and Vytlacil, 2005). As $u$ decreases, meeting the terms of the promotion becomes more likely, and the numerator for $\omega^{\text{ATT}}_1$, $1[u \leq p(x,z)]$, increases. Correspondingly, as $\omega^{\text{ATT}}_1$ increases, the weight placed on $m_1$ increases, and thus the expected value of $Y_1$ contributes more to the ATT target parameter. Accordingly, the ATT target parameter places greater weight on the expected outcomes among those who are more likely to self-select into treatment, as one would expect.

Most common estimators or estimands can be expressed as weighted averages of the MTR functions using (6). This observation immediately suggests a means for one to construct bounds or estimates for the MTRs from the various estimators, such as IV coefficients, and to use the recovered MTRs to construct estimates of causal effects, such as a LATE or PRTE (Mogstad et al., 2018).

To illustrate, consider the IV estimator for $D$ obtained by regressing $Y$ on $D$ with a scalar instrumental variable $Z$. Expressing the IV regression parameter for $D$ as a target parameter yields:

$$\beta^{IV}_D = E_{X,Z} \left[ \int_0^1 m_0(u,x)\omega^{IV}_0(u,x,z)du \right] + E_{X,Z} \left[ \int_0^1 m_1(u,x)\omega^{IV}_1(u,x,z)du \right]$$

$$\omega^{IV}_0(u,x,z) \equiv 1[u > p(x,z)] \frac{z - E[Z]}{\text{Cov}[D,Z]} \quad \omega^{IV}_1(u,x,z) \equiv 1[u \leq p(x,z)] \frac{z - E[Z]}{\text{Cov}[D,Z]}$$

(8)

The expectation of $Z$ and the covariance of $D$ and $Z$ can be estimated from the data, the indicator $1[u > p(x,z)]$ can be computed from an estimated propensity function, and the term $\beta^{IV}_D$ on the left hand side can be estimated from an IV regression. Hence, the only unknowns in (8) are the MTR functions, $m_0(u,x)$ and $m_1(u,x)$, which can be recovered by matching the left and right hand sides. As we discuss next, multiple empirical estimators in the set $\beta^s$ can be used to construct moment equations for estimating or placing restrictions on the MTRs.

### 3.1.4 Relating Target Parameters Representing Empirical Moments and Causal Effects

Given point estimates or bounds for the MTRs, $m_0(u,x)$ and $m_1(u,x)$, it then becomes possible to use these (with an estimated propensity function) to estimate many common causal effects.
For example, one parameter of interest in our context is the conditional on \( X = x \) local average treatment effect (CLATE), which represents how outcomes differ among complying customers with \( X = x \)—those whose promotion status changes from \( D = 0 \) to \( D = 1 \) with a change in the promotion feature \( Z \) from \( z_0 \) to \( z_1 \), under the monotonicity assumption of \( p(x, z_1) > p(x, z_0) \). The CLATE is equal to \( \mathbb{E}[Y_1 - Y_0|X = x, p(x, z_0) < U \leq p(x, z_1)] \), where \( p(x, z_0) < U \leq p(x, z_1) \) denotes the marginal subset of customers in a specific range of unobserved heterogeneity, \( u \), who select into becoming compliers with a change in \( Z \) from \( z_0 \) to \( z_1 \). This subset excludes customers who have i) such a small \( u \) that they would comply even with \( Z \leq z_0 \) (always-takers), or ii) such a high \( u \) that they would not comply even with \( Z > z_1 \) (never-takers). This CLATE target parameter can also be expressed in the form of (6) (Heckman and Vytlacil, 1999):

\[
LATE_{z_0 \rightarrow z_1}(x) = \mathbb{E} \left[ \int_0^1 m_0(u,x) \omega_{LATE}^0(u,x,z) du \right] + \mathbb{E} \left[ \int_0^1 m_1(u,x) \omega_{LATE}^1(u,x,z) du \right]
\]

\[
\omega_{LATE}^1(u,x,z) = \frac{1[p(x, z_0) < u < p(x, z_1)]}{p(x, z_1) - p(x, z_0)} \equiv -\omega_{LATE}^0(u,x,z).
\]

Recall, in Equation (8), the target parameter (the IV coefficient for \( D \)) was known, but the MTRs were not known. In Equation (9), the target parameter (LATE) is not known, but the MTRs are (in the sense that they have been estimated or restricted using (8)). In sum, the MTE approach first uses various estimators to recover or bound the MTR functions, and then uses the recovered MTRs to compute causal effects.

### 3.1.5 Estimation of the MTRs

We estimate causal effects of interest by applying the approach described in Mogstad et al. (2018), which is implemented in the ivmte R package (Shea and Torgovitsky, 2020). This procedure uses a numerical optimization routine to recover point estimates, or upper and lower bounds, of causal effects such as the CLATE; such that these estimates respect the restrictions placed on the MTRs by matching them to empirical moments, such as the coefficients from IV and OLS regressions. The previous discussion is agnostic about the functional specification of i) the MTRs, which link unobserved heterogeneity, \( U \), and customer observables, \( X \), to outcomes, \( Y \); and ii) the propensity function, \( p(x, z) \), which links customer observables, \( X \), and a valid instrument, \( Z \), to promotion status, \( D \). In this section, we briefly describe the general form of the MTR functions that can be used with Mogstad et al.’s approach. In section 3.2.2, we describe the MTRs and propensity
function used in our loyalty promotion example.

Letting \( d \in \{0, 1\} \) index the MTR functions (\( m_0 \) if \( d = 0 \) or \( m_1 \) if \( d = 1 \)), the MTRs are defined (or approximated) as finite-dimensional, linear basis functions of \( X \) and \( U \) (Mogstad et al., 2018):

\[
m_d(u, x) = \sum_{k=0}^{K_d} \theta_{dk} b_{dk}(u, x). \tag{10}
\]

Further restrictions can also be placed on the shape and domain of the MTRs (e.g., in our loyalty promotion example, the domains of \( m_1 \) and \( m_0 \) are theoretically constrained by the spending hurdle needed to meet the terms of the promotion).

To estimate a causal effect, we must choose a set of empirical moments that the MTRs defined in (10) must be able to generate. These connections to observable data restrict the set of feasible MTRs, which then limits the range of causal effects that are consistent with equation (10). Substituting the linear basis functions in equation (10) into the target parameter function in equation (6) yields, for each of these empirical moments \( s \), an expression of the form

\[
\beta_s = \sum_{d=0}^{1} \sum_{k=0}^{K_d} \theta_{dk} \mathbb{E}_{X,Z} \left[ \int_0^1 b_{dk}(u, x) \omega_s^d(u, x, z) du \right]. \tag{11}
\]

The definitions of the weights \( \omega_s^d \) (see Equation (7)) depend on the set of empirical moments, \( s \), that are to be matched (one can use many such moments to bound the MTRs). Just as with any method of moments procedure, by using multiple regression coefficients (i.e., moments), one can make more information available to restrict the MTRs and the causal effect of interest.

As previously alluded, it is not guaranteed that a specification of the MTR functions and a given set of empirical moments will produce a unique point estimate of a causal effect. Instead, the upper bound for the target parameter (e.g., the LATE), \( \overline{\beta}^{TP} \) can be computed as

\[
\overline{\beta}^{TP} = \sup_{\theta \in \Theta} \sum_{d=0}^{1} \sum_{k=0}^{K_d} \theta_{dk} \mathbb{E}_{X,Z} \left[ \int_0^1 b_{dk}(u, x) \omega_s^d(u, x, z) du \right], \tag{12}
\]

subject to the constraint

\[
\sum_{s \in S} \left| \beta_s - \sum_{d=0}^{1} \sum_{k=0}^{K_d} \theta_{dk} \mathbb{E}_{X,Z} \left[ \int_0^1 b_{dk}(u, x) \omega_s^d(u, x, z) du \right] \right| \leq \kappa, \tag{13}
\]
where: i) \( S \) represents the set of empirical moments to match, and ii) \( \kappa \) is the maximum allowed discrepancy between the empirical moments, \( \hat{\beta}_s \), obtained from standard estimators and their (nearly) equivalent values derived from the MTRs. An analogous definition determines the lower bound \( \underline{\beta}^{TP} \). The upper and lower bounds for the causal effect are thus the greatest and least values of the target parameter that can be generated from the subset of MTR functions that is “consistent with the data”—where “consistent with the data” means that the set of MTR functions must also be capable of producing the empirical moments \( s \in S \), either exactly or with minimal error.\(^{10}\)

3.2 Application and Model Specification

In this section we apply the MTE approach to our hotel loyalty example to illustrate its utility in the context of compliance promotions. We organize our discussion using the following procedure: i) discuss identification, ii) specify the functional form and variables used in the MTRs \( m_1 \) and \( m_0 \) as given in Equation (10), iii) specify the functional form and variables used in the propensity function, \( p(X,Z) \), per Equation (3), and iv) detail the specification and rationale behind the empirical moments (Equation (13)) used to estimate the MTRs.

3.2.1 Identification

Overview. To optimize the compliance promotion through extrapolation, we must define the variables \( X \) and \( Z \) introduced in Section 3.1.1. The first set, \( X \), contains variables that define a target set of customers in terms of i) background variables that are relevant to \( D \) and/or \( Y \), and ii) experimentally manipulated features of the promotion that are believed to have a direct effect on the outcome, \( Y \). The second set, \( Z \), contains any experimentally manipulated promotion features that are valid instruments for \( D \)—meaning they affect a customers’ promotion statuses, \( D \), but otherwise do not have a direct effect on the outcome, \( Y \). As stated above, the LATE in equation 9 is defined over a change in \( Z \), not \( X \), meaning it is not possible to extrapolate over the \( X \). We will use our example application to show why the LATE can only be extrapolated over \( Z \), when it is possible to extrapolate from promotion variables in an experimental design, and when it is not.

Figure 2 depicts a directed acyclic graph (DAG) for the data in our application, with arrows indicating the possibility of a direct causal effect, and the lack of an arrow between nodes encoding

\(^{10}\) The value of \( \kappa \) is bounded below by the set of MTRs that minimize the left-hand side of Equation (13). Setting \( \kappa \) to be greater than this infimum expands the set of MTRs that are deemed consistent with the data, in turn allowing the upper and lower bounds for the target parameter to be wider.
Figure 2: Directed Acyclic Graph. All causal effects are conditional on baseline $B$ and tier $T$; these dependencies are not depicted. The variable $U$ represents unobserved confounding affecting both promotion status, $D$, and the outcome of interest, $Y$. The effect of $R$ on $Y$ is mediated through $D$, and thus indirect. $R$ is therefore a valid instrument for $D$. The total effect of $H$ on $Y$ includes an effect mediated through $D$, as well as a direct effect. $H$ is therefore not a valid instrument for $D$. an assumed absence of a direct effect. All variables shown in the DAG are affected by i) customers’ expected baseline stays in the absence of a promotion, $B$, and ii) customers’ loyalty tiers, $T$. To simplify Figure 2, these implicit dependencies are not depicted. All variables are observed, except for $U$, which is depicted with an open circle. The unobserved confounding due to $U$ is depicted by the dashed arrows from $U$ to both $D$ and $Y$. As compliance features, the number of bonus points, $R$, and the spending hurdle, $H$, both exert direct effects on promotion status, $D$. Because the spending hurdle $H$ establishes upper and lower bounds on the number of hotel stays that define the promotion status variable, there is an arrow from $H$ to $Y$; $Y$ is directly affected by $H$.

**Reward bonus points are excludable and valid for extrapolation.** There is no arrow pointing from $R$ to $Y$, because in this empirical setting, bonus reward points, $R$, are plausibly excludable from customers’ potential outcomes, $Y_1$ and $Y_0$, conditional on the size of the spending hurdle. Consider the potential outcome for hotel stays when a customer meets or exceed the terms of the promotion, $Y_1$. Whether the customer visits exactly $Y_1 = H$ hotels or exceeds the hurdle by visiting $Y_1 > H$ hurdles, they still receive the reward of $R$ bonus points. Conditional on visiting at least $H$ hotels, increasing or decreasing the reward should have no effect on how many hotels they visit. Similarly, the potential outcome for hotel stays when a customer does not meet the terms of the promotion, $Y_0$, should also be unaffected by the points offered. A customer who does not reach the hurdle visits $0 \leq Y_0 < H$ hotels and does not receive the reward. Conditional on not receiving the reward, increasing or decreasing the reward should have no impact on visits.

Rewards are therefore assumed to be a valid instrument for promotion status. Given a particular subset of targeted customers with the same baseline, $B$, and loyalty tier, $T$; and who are offered
a promotion with a spending hurdle, $H$—i.e., conditional on $X = \{B, T, H\}$—we can leverage the (experimentally manipulated) exogenous variation in the instrument $Z = \{R\}$ to recover a causal effect of promotion compliance on hotel stays. This causal effect has a limited interpretation as a conditional local average treatment effect (CLATE) when changing the bonus reward points from $R = r_0$ to $R = r$, and it only pertains to the subset of customers who are defined not just by their shared value of $X$, but also by their shared behavior under different promotions. Specifically, these customers do not meet the terms of the promotion when offered $r_0$ bonus points, but they do when offered $r > r_0$ bonus points. Conditional on our specification of the MTR and propensity functions, we can estimate bounds for extrapolated CLATEs at reward levels that were not observed in the experiment.

**Spending hurdles are not excludable and not valid for extrapolation.** As mentioned above, the potential outcomes $Y_1$ and $Y_0$ are directly affected by the spending hurdle, with $Y_1 \geq H$ and $0 \leq Y_0 < H$. Thus, $H$ cannot serve as an instrument for promotion status, and we cannot estimate or extrapolate CLATEs comparing promotions with different spending hurdles. Rather, we can only estimate CLATEs for changes in bonus points, conditional on a particular spending hurdle. Thus, even though one can ascertain intent-to-treat effects of hurdles on stays over the observed experimental cells, it is not possible to extrapolate treatment effects to spending hurdles outside the experiment, *even though spending hurdles were manipulated experimentally.*

To understand why extrapolation isn’t valid, consider that, as the value of the spending hurdle changes, two things happen: i) the mix of customers who comply with the promotion changes, and ii) the average number of hotel visits among complying and non-complying customers also changes. For example, say we have a promotion that offers $R = r$ bonus points for reaching the hurdle $H = h$. Let $D$ represent the set of customers who meet the terms of this promotion, and say that the average number of hotel visits for this group is equal to $h + \frac{1}{2}$. What happens if we lower the spending hurdle by 1, to $h' = h - 1$? Most likely, all of the original customers in $D$ would continue to meet the terms of the promotion, but also, a new group of customers $C$ would now meet the terms as well. To simplify things, say we know this new group of compliers, $C$, is equal in size to the original group, $D$ (in other words, the number of customers with $D = 1$ has doubled). Further, say this new group of customers, $C$, has an average number of visits equal to $h' + \frac{1}{2} = h - \frac{1}{2}$. If we consider the average number of visits among *all* customers who meet the
terms of this new promotion, \( D \cup C \), one might expect it to be equal to \( h \) (the average of \( h + \frac{1}{2} \) and \( h - \frac{1}{2} \)). But that would only be correct if the average hotel visits for customers in \( D \) wasn’t affected by the new, lower spending hurdle. More likely, the average stays among customers in the original compliance group \( D \) would decrease with the lower spending hurdle. Say the average hotel visits for the customers in \( D \) changes to \( h' + 1 = h \) under the new promotion with a hurdle of \( h' \). That would mean the average stays among all customers with \( D = 1 \) (\( D \cup C \)) is \( h' - \frac{1}{4} \) (the average of \( h \) and \( h - \frac{1}{2} \)).

And this is the source of the problem. Although we observe the average stays among all customers with \( D = 1 \), we cannot separately identify the average number of stays within each of the groups, \( C \) and \( D \). We have no way of estimating how the average stays in group \( D \) changes when we lower the spending hurdle, but this is exactly what we need to know in order to extrapolate to new values of \( H \)!

Unfortunately, even if we are willing to model how spending changes, there is nothing in the experiment that will allow us to estimate the parameters of this model, because the experiment never generates the necessary variation. One would have to impose the extrapolation model by fiat.

This principle holds in general for field experiments involving compliance promotions. If there is a manipulated variable that is not conditionally excludable from customers’ potential outcomes (i.e., fully mediated by their promotion status, \( D \)), then one cannot extrapolate experimental effects related to that variable. Stated differently, unless the analyst is willing to assume a promotion feature has a zero (or negligible) effect on the outcome, or they are willing to impose a model of behavior that does not depend on the outcome of the experiment, then extrapolation of treatment effects is not possible for that feature of the promotion.

**Threats to excludability of reward bonus points.** Before proceeding, we mention two potential concerns about the assumed excludability of bonus points in the empirical setting of IHG’s compliance promotion. One concern is purchase acceleration: If consumers delay travel during the promotion so they can use bonus points on those postponed visits after the promotion, then the number of reward points would have a direct effect on stays, in violation of the exclusion restriction.

We assume that if purchase acceleration occurs, its effects are negligible, and we rationalize this assumption in three ways. First, research suggests consumer weekly discount rates are quite low, around .9 per week (Yao et al., 2012). With a four month window for the promotion, the present
value of the postponed visits would be discounted over 80%. Second, purchase delays would only occur among a subset of customers who would otherwise exceed the hurdle by enough to support a purchase delay (otherwise purchase delays would cause them not to achieve the hurdle), and so may not be that common. Third, IHG management believes such behavior to be rare.

The other concern is there may be customers who try to comply with the promotion, but fail. If there are, then the concern is that the offer of higher rewards might lead to higher stays among customers who do not reach the hurdle. If higher values of \( R \) can shift potential outcomes \( Y_0 \) to be closer to \( H \), that would also violate the exclusion restriction. Such an outcome, however, would imply a pattern of “bunching” of stays just below the hurdle in the offer group, but not in the control group. We test for this pattern and fail to observe it. Specifically, we compare the average stays among the subset of customers who do not reach their hurdles between the offer and control groups (recall customers in the control group are randomly assigned a hurdle for the purposes of this and similar comparisons). We conduct two tests:

- **First**, bunching behavior suggests that average stays among customers in the offer group who do not reach the hurdle should be closer to their hurdle than those in the control group. We use a Chi-squared test to evaluate whether the offer group has proportionately more customers who visited one less hotel than required by the hurdle; that is, whether stays of \( Y = H - 1 \) are disproportionately common in the offer group. The test has a p-value of 0.96, suggesting no evidence of bunching. We also repeat the test for \( Y = H - 1 \lor Y = H - 2 \). The p-value of that test is 0.82, also suggesting no evidence of bunching.

- **Second**, we explore whether offers with higher rewards, \( R \), are associated with higher proportions of customers with stays just below the hurdle (among all customers who didn’t meet the hurdle). Specifically, for each baseline-tier-hurdle, we regress the fraction of customers with \( Y = H - 1 \) on \( R \), controlling for baselines-tiers-hurdle fixed effects. The resulting coefficient for the reward level \( R \) is not significant (coef. = -0.0007, s.e. = 0.0004, p-value = 0.11), failing to provide evidence that the fraction of customers with \( Y = H - 1 \) increases with \( R \).

Note that bunching above the cutoff is not a concern, and is consistent with \( R \) being conditionally excludable. In fact, patterns such as bunching above the threshold highlight a distinct advantage of the MTE approach. Flexibility in specifying the MTRs as functions of both observed and
unobserved heterogeneity enables the MTE approach to estimate treatment effects that arise from complex patterns of customer behavior, something more restrictive models such as Heckman’s switching regression cannot accommodate.

3.2.2 MTR Functions

As discussed above, the hurdle \( H \) restricts the upper and lower values of the potential outcomes for hotel visits. That is, \( Y_1 \geq H \) and \( 0 < Y_0 \leq H - 1 \) (recall \( Y \) is integer valued). In order to incorporate this information during estimation, we model a transformed outcome variable, \( Y^* \equiv Y - H \). This leads to the restrictions \( Y^*_1 \geq 0 \) and \( Y^*_0 \leq -1 \), which we impose during estimation as restrictions on the domains of the MTR functions: \( m_1 \geq 0 \) and \( m_0 \leq -1 \).

To facilitate presentation of the model, and without loss of generality, we write the MTR functions for \( d \in \{0, 1\} \) in terms of two, additively separable functions,

\[
m_d(u, x) = \mu_d(b, t, h) + \nu_d(u, b, t, h),
\]

where \( x \equiv \{b, t, h\} \) represents particular values of baseline, tier, and hurdle; \( \mu_d \) is a function of only the observed variables in \( x \); and \( \nu_d \) is a function of both \( x \) and the unobserved variable \( u \). Importantly, the function \( \nu_d \) allows for interactions between the observed variables in \( x \), and the unobserved variable \( u \). In our full specification (described further below), \( \nu_d \) is a function with interactions between powers of \( u \) and the observed variables in \( x \). Two nested models are worth mentioning here. First, a model that ignores unobserved heterogeneity by assuming unconfoundedness can be represented by defining \( \nu_d(u, b, t, h) \equiv 0 \). In that case, \( m_d(u, x) = \mu_d(b, t, h) \). Second, a model that accounts for unobserved heterogeneity, but restricts its effects to be additively separable from the observed variables, can be represented by defining \( \nu_d(u, b, t, h) \equiv \nu_d(u) \). In that case, \( m_d(u, x) = \mu_d(b, t, h) + \nu_d(u) \).

We specify the first component of the MTR functions as

\[
\mu_d(b, t, h) = \\
\phi_d^0 + \phi_d^{B_0} \mathbb{1}(b = 0 \wedge d = 0) + \phi_d^B b + \phi_d^H h + \phi_d^{T_2} \mathbb{1}(t = 2) + \phi_d^{T_3} \mathbb{1}(t = 3) + \phi_d^{T_4} \mathbb{1}(t = 4)
\]

This specification is linear in baseline stays, \( b \), and hurdle, \( h \); and it includes fixed effects for each
loyalty tier (level \( t = 1 \) is normalized at \( \phi_{dT}^1 = 0 \)). It also includes a fixed effect for the special case when \( b = 0 \) and \( d = 0 \). The reason for this is that values of \( Y \) are bounded below at \( Y = 0 \), as are baseline predictions, \( B \). Hence, the subset of customers with predicted baseline stays of \( B = 0 \) have an average value of \( Y \) that is greater than their value of \( B \). Without this dummy variable for the case when \( B = 0 \), regression coefficients for \( B \) will underestimate the (positive) relationship between \( B \) and \( Y \). We include this dummy in \( m_0 \) and the regression equations, but we do not include it in \( m_1 \), because \( Y_1 \geq H \) and the minimum value of \( H \) in the experiment is 2).

Turning to the second component of the MTRs, we specify a polynomial basis function for \( u \) of order 3, with all terms interacting with the observables (except for the \( B = 0 \) dummy variable),

\[
\nu_d(u, t, b, h) = \sum_{q=1}^{3} \left( \psi_{dq}^0 + \psi_{dq}^B b + \psi_{dq}^H h + \psi_{dq}^{T} \mathbb{1}(t = 2) + \psi_{dq}^{T2} \mathbb{1}(t = 3) + \psi_{dq}^{T3} \mathbb{1}(t = 4) \right) u^q.
\]  

(16)

The structure of this function is motivated by two considerations. First, Mogstad et al.’s (2018) numerical optimization procedure relies on the polynomial structure of the MTRs to efficiently calculate the integral in Equation (13). Second, a 3rd degree polynomial of \( u \) is highly flexible, allowing for non-monotonic marginal effects in the unobservable \( u \), and capable of approximating a normally distributed unobservable (a common assumption, e.g., in the Heckman switching regression).

### 3.2.3 Propensity Function

The propensity function isolates the effect of \( Z \equiv \{R\} \) on meeting the terms of the promotion, conditional on \( X \equiv \{B, T, H\} \). We specify the following logit propensity function:

\[
p(x, z) \equiv \Pr[D = 1|X = x, Z = z] \\
= \Pr[D = 1|B = b, T = t, H = h, R = r] \\
= \exp(V)/(1 + \exp(V)) = \exp(V)/(1 + \exp(V)) \\
V = \delta_{bt}^{RT} + \delta^H h + \delta^{H^{-1}} h^{-1} + \delta^B h \cdot b + \delta^{B^{-1}} b \cdot h^{-1} + \delta_t^{RT} f(r)
\]  

(17)

where: i) \( \delta_{bt}^{RT} \) represents fixed effects for each unique combination of baseline, \( B \), and tier, \( T \), and thus an intercept for each experimental cell; ii) \( \delta^H, \delta^{H^{-1}}, \delta^B, \) and \( \delta^{B^{-1}} \) are coefficients for hurdle, its inverse, and interactions between these and baseline (the absence of a coefficient
for baseline is due to the inclusion of the baseline-tier fixed effects); and iii) \( \delta_{t}^{RT} \) are tier-specific coefficients for a transformation of reward points, \( f(r) = \sinh^{-1}(r/12000) \). This transformation allows \( f(0) = 0 \), and an approximately logarithmic relationship for \( r > 0 \). The coefficients \( \delta_{t}^{RT} \) are different for each tier, as we believe sensitivity to reward points may vary across the four tiers.\(^{11}\)

The inclusion of the inverse of the hurdle (specifically, the term \( \delta^{B-H^{-1}} b \cdot h^{-1} \)) is motivated by compliance being less likely when there is a large discrepancy between the baseline prediction for a customer’s hotel visits and their hurdle, as this is more of a stretch goal for a consumer.

3.2.4 Model Summary

The full model can be succinctly summarized as follows:

1. There is an unobserved variable, \( U \), that affects i) how many hotels would be visited if the hurdle were to be met, \( Y_{1} \); ii) how many hotels would be visited if the hurdle were not to be met, \( Y_{0} \); and iii) whether the hurdle is in fact reached, \( D \) (Equations 2 and 3).

2. The expected change in hotel stays due to a customer with \( X = x \) and \( U = u \) complying with the promotion, the marginal treatment effect, is the difference in their marginal treatment responses when meeting versus not meeting the promotions terms, \( MTE = m_{1}(u, x) - m_{0}(u, x) \) (Equations 4, 5, 14, 15, and 16). This parametric model will be constrained by empirical moments in the data, and then used to search for upper and lower bounds for causal effects.

3. Whether the hurdle is reached further depends the propensity function, \( p(X, Z) \), where \( X = \{B, T, H\} \) contains the baseline and tier customer variables, and the spending hurdle; and \( Z = \{R\} \) contains the reward level (Equations 3 and 17).

We make two remarks about the model. First, another way to characterize the point 1 above is that each customer’s observed outcome depends on: i) a realization of the random triple \( \{Y_{1}, Y_{0}, U\} \), whose distribution depends on \( X \); and ii) a realization of the promotion feature \( Z \) which is randomly assigned. Second, whether \( U \) reflects static customer traits or transient purchase propensities is

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\(^{11}\) This transformation encodes our assumption i) that there is a non-linear response to more reward points on promotion status—if the reward is too small, it is not motivating; and there are diminishing marginal returns to larger rewards—and ii) that IHG designed the experiment with an understanding (perhaps implicit) of this response function. Accordingly, we chose the scaling factor of \( \alpha = 12000 \) to rationalize the observed reward levels used in the experiment. Specifically, we conducted a linear regression of \( f_{a}(r) \) on hurdle, and fixed effects for each unique combination of baseline and tier. We then searched over values of \( \alpha \) in increments of 1000 until we identified the value with the highest \( R^{2} \).
irrelevant to the econometric specification; $U$ simply represents an indexable agglomeration of unobserved factors arising simultaneously with potential outcomes.

### 3.2.5 Moments

The model we just described encodes our assumptions about customers’ behaviors during the field experiment, and thus the empirical data that the experiment produced. Next, we specify which features of the data (i.e., regression coefficients) should be used to restrict the model, and thus place bounds on the causal effects we seek to recover.

It is our experience that the estimates of the model are sensitive to the specification of the moments. If too many moments are selected, the model risks over-fitting the data. If too few are specified, or the specified moments do not well reflect the topology of the data or the structure of the MTRs, then treatment effect estimates will have wide bounds, and the extrapolation forecasts will vary little with the $X$.

We consider empirical moments produced by three regression equations. The moments that are used to restrict the MTRs are a subset of coefficients from these regressions (some coefficients are highly collinear across regressions; in such cases we include only one of these in the moment matching procedure). The first two regression equations are closely related—one is an IV regression, and the other is its OLS counterpart; both are estimated using the entirety of the experimental data. The third regression seeks to characterize the behavior of customers in the absence of a promotion, and is estimated via OLS, using only data from customers in the control condition. All three regressions are motivated by i) IHG’s use of the baseline variable, $B$, to forecast stays in the absence of promotions, ii) the earlier discussion about forecasting error in the special case when $B = 0$, and iii) the transformation $Y^* = Y - H$.

The IV regression has the following structure

$$y^* = \beta^{IV}_0 + \beta^{IV}_1 1(b = 0) + \beta^{IV}_2 b + \beta^{IV}_3 h + \beta^{IV}_{4t} d + \beta^{IV}_5 d \cdot b + \beta^{IV}_6 d \cdot h + \beta^{IV}_7 d \cdot 1(t = 2) + \beta^{IV}_8 d \cdot 1(t = 3) + \beta^{IV}_9 d \cdot 1(t = 4) + e^{IV}$$  

(18)

This regression formula includes a main effect for baseline (as well as the dummy for $B = 0$), but does not include main effects corresponding with customers’ loyalty tiers, as any effects of loyalty tier on hotel visits (if not spending at or above the hurdle) are already accounted for by the
baseline prediction. This regression equation also includes a main effect for hurdle to account for the transformation \( y^* = y - h \). All other variables in Equation (18) are interacted with promotion status, \( d \), and thus reflect heterogeneous treatment effects (based on observed covariates) varying by tier, baseline, and hurdle. In the IV regression, the endogenous variable \( d \) is instrumented using the variables defined on the right-hand side of the propensity function in Equation (17).

The OLS version of the IV regression estimates Equation (18) without instrumenting for promotion status. Although the coefficients from an OLS regression are typically biased as causal estimates, they are nevertheless functions of the MTRs that can be observed in data, and thus provide information about the MTR functions. Indeed, any discrepancy between an OLS and IV coefficient for the same treatment variable constrains the MTR functions at different levels of the unobserved variable, \( U \).

The third regression, also estimated with OLS, seeks to capture the relationship between baseline predictions and hotel visits in the absence of a promotion (i.e., among customers in the control group). This regression is specified as

\[
y^* = \beta_0^C + \beta_1^C b + \beta_2^C b + \beta_3^C h + \epsilon^C
\]

As in (18), we include a dummy for the case when the baseline prediction is \( B = 0 \), and include the hurdle to account for the transformation \( y^* = y - h \). This regression is estimated using the subset of observations from the control group.

### 3.3 The Optimization of Reward Points

Recall from equation (1) that the non-parametric estimate of profits as a function of rewards conditional on \( X \) can be written as

\[
PRTE^\Pi_{0 \rightarrow r}(X) = N \cdot (\mathbb{E}[Y|X, R = r] - \mathbb{E}[Y|X, R = 0]) \cdot mgn - N \cdot cpp \cdot r \cdot \mathbb{E}[D|X, R = r],
\]

where \( cpp \) is the cost per point of reward, \( mgn \) is the margin per visit, and \( N \) is the number of customers in the segment with characteristics \( X \). This expression emerges as a consequence of a more primitive causal effect among compliers—those whose promotion status is \( D = 0 \) when offered a promotion with \( R = 0 \) bonus points, but \( D = 1 \) when offered \( R = r \) bonus points. We
can rewrite the PRTE in terms of this CLATE by noting two things (see Appendix A for the derivation). First, the expression $E[Y|X,R = r] - E[Y|X,R = 0]$ is equal to the (conditional on $X$) intent-to-treat effect of offering $R = r$ points versus $R = 0$ points (i.e., no promotion), which we write as $ITT_{0\to r}(X)$. Second, (and suppressing the dependence on $X$) this ITT is related to the LATE through the identity $ITT_{0\to r} = LATE_{0\to r} \cdot \pi_{0\to r}^C$, where $\pi_{0\to r}^C$ represents the proportion of compliers in a promotion offering $R = r$ bonus points (versus $R = 0$). Hence, we can rewrite (1) as:

$$PRTE_{0\to r}^\Pi(X) = \frac{N \cdot \pi_{0\to r}^C(X)}{\text{Number complying}} \cdot LATE_{0\to r}(X) \cdot mgn - \frac{N \cdot cpp \cdot r \cdot E[D|X,R = r]}{\text{Total promotion cost}}$$

(20)

This expression highlights a number of ideas. One is that the policy relevant treatment effect can be decomposed into three parts: i) the number of compliers affected by the promotion, ii) the average increase in margin due to these compliers meeting the terms of the promotion; and iii) the total cost of the promotion, which depends on the number of customers reaching the hurdle when offered $R = r$ points, regardless of whether they are compliers or would have achieved the hurdle without the promotion (always-takers).

Equation (20) also emphasizes that the primitive causal effect relevant to the promotion is the CLATE. Extrapolation of the PRTE depends on an appropriate extrapolation of the CLATE that is consistent with changes in the proportion of compliers and the cost of the promotion. By expressing the PRTE as a function of the ITT, Equation (1) hides the need for consistency between the margin lift from complying customers and the cost of the promotion (below we discuss how a linear extrapolation of the ITT is unlikely to correspond with an extrapolation of the CLATE).

For values of $R$ that were used in the experiment, one can calculate the PRTE non-parametrically using Equation (1) (as described previously). To extrapolate the PRTE to values of $R = r$ outside the experiment, we use i) the MTE approach to estimate $LATE_{0\to r}(X)$, and ii) logistic regression of the propensity to estimate $\pi_{0\to r}^C(X)$ and $E[D|X,R = r]$. We perform this estimation for many values of reward points in a set of candidate values, $r \in R$. Conditional on $X$, the level of rewards that leads to the highest profits is the one generating the greatest value of the PRTE.

$$\pi^*_r(r,X) = \max_{r \in R} PRTE_{0\to r}^\Pi(X).$$

(21)
If all the PRTEs are negative, then not offering a promotion is optimal.

We note several implicit assumptions and approximations underpinning the PRTE in Equation (20). First, consistent with IHG’s practice, we assume that the cost per reward point, \( c_{pp} \), is independent of promotion status, potential outcomes, and background variables (baseline and loyalty tier). Points can be redeemed for merchandise and future stays, and IHG assigns the same cost for reward points regardless of how they are redeemed. Of course, a generalization of (20) could, for example, replace the terms \( r \cdot c_{pp} \) with a conditional on \( X \) expected cost function. Second, and again consistent with IHG’s practice, we assume a similar set of independences for the margin per customer stay, \( m_{gn} \), and in particular, we assume that the margin per hotel visit is not endogenous with promotion status. This rules out situations whereby consumers stay in cheaper hotels than they otherwise would during the promotion in order to meet its terms. Again, IHG uses a constant expected margin per stay for its internal calculations, but it is possible to generalize this approach by changing the outcome variable from hotel stays to total margin per customer. The degree of bias in our estimates due to these assumptions depends on the extent of heterogeneity in these margin and cost terms. If customers choose to stay in cheaper hotels, for example, we will over-estimate the profit effects of compliance.

3.4 Linear Interpolation

A common practice in industry, as noted in the motivating example, is to perform a linear interpolation (LI) of intent-to-treat effects in order to optimize promotion features. This is most easily accomplished by using OLS to regress total stays \( Y \) on reward points \( R \) (conditional on \( X \)). Suppressing the dependence on \( X \) in the discussion that follows, such a regression might take the form \( y = \eta_0 + \eta_1 r + e^{LI} \). This approximation implies that under linear interpolation, the intent to treat effect of reward points on stays is \( ITT_{0 \rightarrow r} = \eta_1 r \). In Appendix A, we show that in the case of i) a logit propensity function of the form \( p(r) = \text{logit}^{-1}(\alpha + \beta r) \), and i) an arbitrary set of MTRs, we can also express \( ITT_{0 \rightarrow r} \) as

\[
ITT_{0 \rightarrow r} = \int_0^r \frac{(m_1(p(\rho)) - m_0(p(\rho)))\beta}{2 + 2 \cosh(\alpha + \beta \rho)} d\rho.
\]

Comparing the two expressions for \( ITT \) suggests that they are equal to each other when: i) there is no effect of reward points on total stays, \( \beta = \eta = 0 \); or ii) the integrand, \( \frac{(m_1(p(\rho)) - m_0(p(\rho)))\beta}{2 + 2 \cosh(\alpha + \beta \rho)} = \eta \),
is a constant for any reward level \( \varrho \). The latter of these two conditions might occur when changes in \( \varrho \) are so small that the \( \cosh(\alpha + \beta \varrho) \) and \( m_d(p(\varrho)) \) functions are locally linear in \( \varrho \)—i.e., their ratio is constant in \( \varrho \). In other words, linear interpolation approximates the ITT only for very small differences in reward. Since one of the end points in \( ITT_{0\rightarrow r} \) is at \( R = 0 \), it is unlikely that local linearity would hold for any values of \( R = r \) that are managerially meaningful.

Another way of considering when a linear interpolation might be valid is to specify i) flexible MTR functions, while ii) allowing for an arbitrary propensity function (as opposed to the above paragraph, with a logit propensity function and arbitrary MTR functions). In Appendix A we show that when \( m_d = \sum_{k=0}^{K} \theta_d k u^k \) is specified as a linear polynomial function of \( u \) given \( x \), then we obtain

\[
ITT_{0\rightarrow r} = \int_{p(x,0)}^{p(x,r)} \left[ m_1(u) - m_0(u) \right] du \\
= \sum_{k=0}^{K} \theta_k x \left[ p(x,r)^{k+1} - p(x,0)^{k+1} \right] / (k+1) \\
= \theta_0 x (p(x,r) - p(x,0)) + \sum_{k=1}^{K} \theta_k x \left[ p(x,r)^{k+1} - p(x,0)^{k+1} \right] / (k+1)
\]

Again contrasting this expression with \( \eta_1 r \) from an OLS regression, we see that \( ITT_{0\rightarrow r} \) is potentially linear in \( r \) when: i) compliance \( p(x,r) \) is linear in \( r \); and ii) there is no unobserved heterogeneity in the treatment effect on stays, \( K = 0 \). Linearity in \( p \) is plausible for small changes in \( r \), but if propensities are estimated using a linear regression, there is a risk of violating the overlap condition (propensities cannot be exactly 0 or 1). Note however that if \( K = 0 \), then the expression for the CLATE becomes \( \bar{\theta}_0 x [p(x,r) - p(x,0)] / [p(x,r) - p(x,0)] = \bar{\theta}_0 x \)—a constant—implying that the treatment effects are identical among customers who might be very different—for example, among those who require very few and those who require very many points to comply with the promotion.

4 Results

In this section we begin by discussing the results of our compliance (propensity) model and then report the findings of our MTE model of stays. Within our discussion of stays, we outline a null model of CATEs on stays and compare it to the LATEs estimated via the MTEs. We conclude
Table 6: Propensity model

<table>
<thead>
<tr>
<th>Term</th>
<th>Estimate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline-Tier fixed effects, $\delta^{BT}$</td>
<td>Yes</td>
</tr>
<tr>
<td>Hurdle, $\delta^H$</td>
<td>-0.194</td>
</tr>
<tr>
<td>Baseline · Hurdle, $\delta^{B\cdot H}$</td>
<td>0.012</td>
</tr>
<tr>
<td>$1/Hurdle$, $\delta^{H^{-1}}$</td>
<td>-1.868</td>
</tr>
<tr>
<td>Baseline/Hurdle, $\delta^{B\cdot H^{-1}}$</td>
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</tr>
<tr>
<td>$f(R)$, Tier 1, $\delta^{RT}_1$</td>
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</tr>
<tr>
<td>$f(R)$, Tier 2, $\delta^{RT}_2$</td>
<td>0.342</td>
</tr>
<tr>
<td>$f(R)$, Tier 3, $\delta^{RT}_3$</td>
<td>0.286</td>
</tr>
<tr>
<td>$f(R)$, Tier 4, $\delta^{RT}_4$</td>
<td>0.179</td>
</tr>
<tr>
<td></td>
<td>(0.441)</td>
</tr>
<tr>
<td></td>
<td>(0.026)</td>
</tr>
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<td></td>
<td>(3.757)</td>
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<tr>
<td></td>
<td>(2.763)</td>
</tr>
<tr>
<td></td>
<td>(0.217)</td>
</tr>
<tr>
<td></td>
<td>(0.082)</td>
</tr>
<tr>
<td></td>
<td>(0.084)</td>
</tr>
<tr>
<td></td>
<td>(0.093)</td>
</tr>
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</table>

this section with a discussion of how optimal promotional designs vary across target segments and the profit implications.

4.1 The Propensity Function

Table 6 reports the main coefficients of interest for the propensity function recovered from a logistic regression with promotion status as the dependent variable. Overall, the propensity model appears to fit the data well. The mean (median) absolute error between values of $\mathbb{E}[D|B, T, H, R]$ estimated non-parametrically from the data, and fitted values of $p(X, Z)$ obtained from the logistic regression, is 0.11 (.05). Coefficients for reward points are positive and estimated with precision, suggesting a small lift in compliance from reward points. For tiers 1 through 4, the expected increase in compliance from 0 to 10,000 points is .12%, 3.4%, 4.1%, and 2.8% respectively; the next 10,000 points is expected to raise compliance by an additional .08%, 2.7%, 3.0%, and 2.0% respectively.

Figure 3 depicts the contour of the estimated propensity model. Each line represents a unique combination of baseline, tier, and hurdle, and includes interpolated reward levels ranging from $R = 0$ to 20% above the maximum value of $R$ in that experimental cell. Some of these lines are a approximately linear, whereas others are more convex. For the latter, linear interpolation or extrapolation based on intent-to-treat effects would problematic.
4.2 Model Comparisons

In this subsection we consider a number of alternative models to ascertain whether the proposed model better explains the data, and to help refine the model specification to be used in optimizing promotional design. We begin by comparing alternative MTR functions, then contrast our model with an inverse propensity weighting approach that ignores the effect of unobservables in compliance both in specification and estimation.

4.2.1 MTR Comparisons

A common approach to estimating treatment effects for compliance promotions is Heckman’s correction model (see Heckman (1979)). The standard Heckman correction approach, however, embeds a number of strong assumptions that may be difficult to justify. One is the assumption that the effect of the unobserved variable $U$ on $Y$ is additively separable from the other factors $X$ that also influence the outcome. Another assumption is that the effect of $U$ on $Y$ is (weakly) monotonically increasing or decreasing. Section 4.4.1 discusses monotonicity (of the MTR, not the “monotonicity assumption”), while this section suggest the separability assumption is not tenable in our context.

Separability allows for unobservable factors to influence both compliance with the promotion and hotel stays. But separability also requires that the same unobserved factors that make it more or less likely to meet a hurdle $H$ have an effect on stays that, conditional on meeting the hurdle, does
not depend on, for example, the actual hurdle. Rather, it implies that the effect of these unobserved factors on stays are constant no matter the expected baseline stays, tier, or hurdle. Separability would be violated in our context if leisure travelers are both more likely to comply with the hurdle (perhaps rewards are worth more to the more price sensitive leisure segment) and (conditional on compliance) more sensitive to hurdles in their stays (perhaps due to the more limited nature of leisure travel).

Separability is nested within our MTRs, but not required. To explore the impact of separability on the estimated treatment effects, as well as the impact of assuming selection on the observables only, we consider two alternative specifications for the MTR function in Section 3.2.2. One of these assumes that there is no effect of the unobserved variable \( U \) on stays (\( \nu_d = 0 \) in equation 14). This selection on observables model is roughly comparable to an inverse compliance weighted (ICW) regression, with weights derived from the propensity regression. Causal estimates from this model are constants for all values of reward points, thus the estimates are conditional on \( X \) average treatment effects (CATEs), and not CLATEs. The other specification assumes additive separability, meaning there are no interactions between observables and unobservables in the MTRs (i.e., \( \psi_{dq}^H, \psi_{dq}^{T2}, \psi_{dq}^{T3}, \psi_{dq}^{T4} \) in equation 16 are all equal to zero). Except for the influence of the IV moments on the MTR function estimates, the first model would imply no endogeneity bias arising from compliance decisions (conditional on compliance propensities). The no interaction model explores whether the marginal consumers in a compliance decision differ in their response to how observables affect stays. Table 7 reports the moment error bounds for each of these models for each respective moment outlined in Section 3.2.5 (for example, equation 8 illustrates how MTRs match the IV moments).\(^{12}\)

Several insights emerge from 7, which reports coefficient estimates for the three regressions estimated via standard IV and OLS procedures, and discrepancies between these moments and their MTE counterparts. First, neither the no interaction model nor the no unobservable model can recover most of the moments without error. This implies that the full model, which accounts for unobservables and their dependence on the observed data, reproduces the moments of the observed data substantially better than the restricted models. The improvement in fit is also indicated by

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\(^{12}\) Not all of the potential moments are reported. First, some moments are so collinear (redundant) with others that the IVMTE routine dropped them. Second, only one of the moments for the main effect of \( H \) is included, in order to control for the transformation of the outcome variable to \( Y - H \) (e.g., \( \beta_3^C \)). See Section 3.2.5.
<table>
<thead>
<tr>
<th>Regression</th>
<th>Coefficient</th>
<th>Full Model</th>
<th>No Interactions</th>
<th>No Unobservables</th>
</tr>
</thead>
<tbody>
<tr>
<td>IV</td>
<td>Intercept*, β_{0}^{IV}</td>
<td>-0.16</td>
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<td>0.36</td>
<td></td>
<td></td>
</tr>
<tr>
<td>IV</td>
<td>Baseline*, β_{2}^{IV}</td>
<td>0.61</td>
<td></td>
<td></td>
</tr>
<tr>
<td>IV</td>
<td>Hurdle, β_{3}^{IV}</td>
<td>-1.02</td>
<td>-0.17</td>
<td>-0.15</td>
</tr>
<tr>
<td>IV</td>
<td>Compliance, β_{4}^{IV}</td>
<td>-1.03</td>
<td>0.12</td>
<td>0.90</td>
</tr>
<tr>
<td>IV</td>
<td>Compliance×Baseline, β_{5}^{IV}</td>
<td>-0.69</td>
<td>0.07</td>
<td>0.06</td>
</tr>
<tr>
<td>IV</td>
<td>Compliance×Hurdle, β_{6}^{IV}</td>
<td>1.35</td>
<td></td>
<td></td>
</tr>
<tr>
<td>IV</td>
<td>Compliance×(T = 2), β_{7}^{IV}</td>
<td>0.70</td>
<td></td>
<td></td>
</tr>
<tr>
<td>IV</td>
<td>Compliance×(T = 3), β_{8}^{IV}</td>
<td>0.92</td>
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</tr>
<tr>
<td>IV</td>
<td>Compliance×(T = 4), β_{9}^{IV}</td>
<td>1.25</td>
<td></td>
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<tr>
<td>OLS</td>
<td>Intercept, β_{0}^{OLS}</td>
<td>-0.43</td>
<td>-0.02</td>
<td>-0.02</td>
</tr>
<tr>
<td>OLS</td>
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<td>0.33</td>
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</tr>
<tr>
<td>OLS</td>
<td>Baseline, β_{2}^{OLS}</td>
<td>0.47</td>
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<td>-0.03</td>
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<tr>
<td>OLS</td>
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</tr>
<tr>
<td>OLS</td>
<td>Compliance×Baseline, β_{5}^{OLS}</td>
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<td>-0.28</td>
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<td>0.83</td>
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<td>0.33</td>
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<tr>
<td>OLS</td>
<td>Compliance×(T = 2), β_{7}^{OLS}</td>
<td>0.17</td>
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</tr>
<tr>
<td>OLS</td>
<td>Compliance×(T = 3), β_{8}^{OLS}</td>
<td>0.16</td>
<td></td>
<td>-0.07</td>
</tr>
<tr>
<td>OLS</td>
<td>Compliance×(T = 4), β_{9}^{OLS}</td>
<td>0.13</td>
<td></td>
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<tr>
<td>OLS (control)</td>
<td>Intercept, β_{0}^{C}</td>
<td>-0.03</td>
<td></td>
<td></td>
</tr>
<tr>
<td>OLS (control)</td>
<td>Baseline=0, β_{1}^{C}</td>
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<tr>
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<td>Baseline, β_{2}^{C}</td>
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<td>OLS (control)</td>
<td>Hurdle*</td>
<td>1.20</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Sum of Absolute Errors 0.02 1.0 2.0.

Table 7: Moment Error Bounds. The three regressions used in the MTE estimation are specified in Section 3.2.5, and their coefficient estimates (moments) are given above. ε and ¯ε are differences between moments obtained from OLS or IV regressions and their MTR counterparts, at the lower and upper bounds of the treatment estimated effect, respectively. Moments named in italics and marked with an asterisk are not used in the MTE procedure; blank cells indicate the MTRs fully reproduce the data moments, that is, the error is zero. No Unobservables means that U is not included in the MTR functions, and No Interactions means U is not interacted with X in the MTR functions.

13 Though moments are estimated with statistical error, Mogstad et al. (2018) show that the MTR estimator is consistent.
the sum of absolute errors across moment conditions (which decrease in the full model even though there are more parameters). Second, some of the errors in the restricted models are quite large. For example, the error in the “no unobservables” model for the IV compliance moment (corresponding with tier $T = 1$), $\beta^4_{IV} = 1.03$, is 0.90 or 87% of the parameter value. Third, the errors are particularly large for the IV moments under the no unobservables model because the assumption of no unobservables from compliance entering the outcomes makes it hard to reconcile the MTRs with an IV regression that does not make this assumption. This assumption of no endogeneity in the MTR function is clearly problematic, because the IV estimate for compliance (-1.03) reverses sign from the OLS estimate (0.72). Thus, compliance is endogenous with stays. Because the no unobservables model assumes this endogeneity away, it cannot reproduce both the IV and OLS parameter estimates, and thus produces estimates that fare well for neither. In sum, the MTR model improves fit with the moments of the data substantially compared to models that ignore or incompletely account for endogeneity and (lack of) separability.

Next, Figure 4 compares causal effects for the full, no interaction, and no unobservables MTR specifications. Additional insights emerge. First, the estimated causal effects are quite different across the models. We interpret this discrepancy as evidence of the models failing to fit the moments also producing biased estimates of the CLATEs. For example the CLATE estimates for $T = 2$, $B = 2$, and $H = 3$ vary widely across models, differing by nearly 1.5 stays. Second, the simplest model (no unobservables) has the smallest LATE estimate bounds. With fewer terms in the MTR functions, the parameter space that meets the moment constraint shrinks (see equation 13). This suggests a trade-off. On the one hand, a more complex MTR can more easily meet the moment constraints, but on the other hand a poorly specified MTR or too many parameters can lead to large bounds, making the MTE model less useful for policy analysis. Specifically, as the bounds grow, so too does the range of potential optimal policies making it less clear which, amongst the set of optimal policies, to choose.

### 4.2.2 Doubly Robust Inverse Propensity Score Weighting Comparison

Next, we describe a null model of CATEs that assumes unconfoundedness (as is common in the experimental literature in marketing). This means that the unobserved heterogeneity affecting compliance does not have an impact on stays. We use a doubly-robust regression estimator (Bang and Robins, 2005). First, observations are weighted by the inverse of their propensities (IPW),
Figure 4: Comparison of Causal Effects Across Models for $H = 2 \ldots 5$ Among Customers with $B = H - 1$. Causal effects are in units of stays, and consider a change from no promotion to a promotion with $R$ equal to the smallest reward used in each baseline-hurdle-tier experimental cell. Vertical bars shows upper and lower bounds estimates. In cases where the bounds are very narrow or point estimates, dots are plotted at the midpoint of bounds/point estimate.
thus \( w = \frac{D}{p(X,R)} + \frac{1-D}{1-p(X,R)} \). Second, covariates that appear in the propensity function, but not in the MTRs, are also included in the regression equation to further adjust outcomes for these determinants of compliance (we do not include reward points in the regression). The regression equation is thus

\[
y^* = \beta_0^W + \beta_1^W I(b = 0) + \beta_2^W b + \beta_3^W h + \beta_4^W d + \beta_5^W d \cdot b + \beta_6^W d \cdot h + \beta_7^W d \cdot I(t = 2) + \beta_8^W d \cdot I(t = 3) + \beta_9^W d \cdot I(t = 4) + \beta_{10,b,t}^W I(b = 1) + \beta_{11}^W h^{-1} + \beta_{12}^W b \cdot h + \beta_{13}^W b \cdot h^{-1} + e^W \tag{22}
\]

where \( \beta_0^W \) through \( \beta_9^W \) correspond with covariates from the MTRs and IV moments, and \( \beta_{10}^W \) through \( \beta_{13}^W \) correspond with additional covariates from the propensity regression.

Figure 4 reports the estimated CATEs, labeled as “DR IPW” (doubly robust, inverse propensity weighted). The CATEs estimated with this approach are nearly identical to the MTR model that also omits unobservables in outcomes. The slight differences between the two estimators are due to the use of IV moments and domain restrictions on \( m_1 \) and \( m_0 \) in the MTE approach, and the inclusion of additional adjustment covariates in the DR IPW approach.

4.3 Validation Against Non-Parametric Estimates

Figure 5 compares the MTE estimates of LATEs from the full model to the non-parametric estimates based on the Wald estimator, as described in Section 3.3. As the non-parametric estimates can be computed only for the observed cells we plot treatment effects for each baseline-hurdle-reward combination in the experiment with hurdles less than or equal to 5. Ten of the non-parametric estimates are not shown because they are negative or undefined (e.g., the cell with \( B = 0, H = 2, \) and \( T = 4 \) has only two customers). The MTE estimator does not suffer from this limitation. Overall, the figure suggests that the chosen moments and functional forms for the MTRs generate estimated LATEs that are consistent with their non-parametric counterparts.

4.4 The Profitability of Reward Structures

4.4.1 Optimal Design and Targeting

Next, we consider a key goal of this research; extrapolating PRTEs to find the optimal promotion design. Figure 6 depicts the estimated effect of rewards on conditional stays, costs, and profits
Figure 5: Comparison of MTE and Non-parametric (Wald) Estimates of Experimental Assignments on Stays. Points are horizontally jittered. MTE bounds estimates are plotted at the midpoint between the upper and lower bounds. Wald estimates are not shown if they are negative or violate the monotonicity assumption.

for the baseline-tier-hurdle combinations with the greatest number of observations for each of the four tiers, and for each of the MTR functions described in Section 4.2. In each plot, we depict the optimal level of rewards for each MTR function as a vertical line. Because we estimate the profit function using bounds estimators, we select the optimal reward level using a minimax criterion (Handel et al., 2013).

We first consider the upper left panel in Figure 6, where \( T = 1, B = 1, \) and \( H = 2 \). As the baseline and tier are low, these customers are infrequent visitors and one might expect them to be relatively insensitive to rewards. Consistent with this, our results suggest that rewards have little effect on compliance or stays. As the net revenue attributable to the promotion depends on the share of customers whose compliance status is changed by the reward, rewards generate almost no incremental revenue among this group of customers. Reward points, however, must be awarded to any customers who meet or exceed the hurdle, whether or not they were motivated to do so by the reward points. Because rewards are costly and have little effect on stays, their marginal value is negative and the optimal level of promotion is 0—meaning it is optimal not to offer this group any promotion at all.

Turning next to the upper right panel, where \( T = 2, B = 2, \) and \( H = 3 \), we find that higher
Figure 6: Optimal Promotional Designs. For each tier $T = 1, \ldots, 4$, the combination of $B$ and $H$ with the most observations is chosen for extrapolation and optimization. Profits on the vertical axis are scaled to preserve confidentiality. For each tier, the LATE for hotel stays and total profit from the promotion is shown for promotions offering reward points (given along the horizontal axis in units of thousands). Bound estimates are shown. The optimal reward points are indicated with arrows.
rewards drive higher levels of compliance. The effect of rewards on stays (the LATE) differs over the model specifications. Recall, rewards are excludable from stays conditioned on compliance. Hence, the only effect rewards can have on stays is indirect, via the unobservables in the compliance equation. With no unobservables in the MTR function, the estimated value of the LATE does not depend on the level of reward points offered. This can be seen in the flat line for the LATE with no unobservables. The profits under no unobservables, therefore, are driven only by changes in compliance with an optimal level of 35,000 points. Comparing the full model to the two restricted models, we observe that the expected profits in the restricted models are off by nearly 100% and they are far too optimistic (because their estimated LATEs are much too large). The model with no interactions predicts scaled profits of $8.52 per customer (in the segment), while the full model predicts profits of $3.71 per customer. Differences in the cost of the promotion can also be substantial, as the optimal reward points for this segment is 35,000 points in a model with no unobservables but only 25,000 points in a model with observables. In other words, the flexible MTR specifications in the proposed model have a material difference on policy design and profit outcomes that can’t be captured in models that ignore separability (e.g., the classic Heckman correction model) or unconfoundedness (e.g., IPW regression).

Next, we consider $T = 3, B = 2$, and $H = 3$ in the lower left. Again we note rewards affect compliance, but their effect on outcomes only matters when accounting for unobservables. Unlike the case of $T = 2, B = 2$, and $H = 3$, differences in the profit functions are small because estimates for the LATEs are similar across specifications. Optimal rewards differ for the model with no interactions owing to i) the large estimation bounds and the use of the minimax criterion to set levels, ii) the high profit per visit, which amplifies small differences in the LATE for visits into large differences in profit, and iii) the relative flatness of the profit curve. As a result even though the optimal design differs across models, the predicted profits are roughly the same. Even if the wrong model were to be used in this instance, profits would not be substantially affected (ranging between $5 and $6 per customer over the range of 17,500 points to 27,500 points).

The last case is presented in the lower right panel with $T = 4, B = 3$, and $H = 4$, and represents results for members of the highest loyalty tier—those who stay at the hotel chain most often. As in the case of the upper right panel, where $T = 2, B = 2$, and $H = 3$, the choice of model again has a substantial effect on profits and the LATE. However, in this case the effect is reversed; the full model
in the lower right panel predicts higher rather than lower profits, whereas the full model in the upper right panel predicts lower rather than higher profits. This result is suggestive that a model where unobservables can interact with observables can capture many patterns of behavior (that is, the model need not assume the marginal consumer is the same as rewards increase). The scaled profit in the model without unobservables when $T = 4, B = 3,$ and $H = 4$ is $3.01$ per customer, compared to the full model’s prediction of $4.39$, an error of about 30%. The optimal reward levels are similar, however, because marginal revenues are roughly proportional across models.

Finally section 4.2 notes that the Heckman correction model assumes separability and monotonicity, and offers evidence that the separability assumption does not support the data. Here we reference monotonicity. This assumption is also violated, because the LATE for $T = 2, B = 2,$ and $H = 3$ is increasing, the LATE for $T = 3, B = 2,$ and $H = 3$ is flat, and the LATE for $T = 4, B = 2,$ and $H = 4$ is decreasing.

In conclusion, we make two points. First, the assumptions of separability and selection are materially important when setting rewards and forecasting returns, but are often ignored in the literature on compliance promotions. Second, the hotel rewards example used to illustrate our framework suggests its potential utility for extrapolation in the context of compliance promotion design in other experimental settings.

5 Conclusion

In this paper, we outline an approach for promotional design and targeting of compliance promotions and apply it to the context of a hotel’s loyalty rewards promotion. Given the rapid growth of field experimental methods for policy evaluation in marketing, the utility of extrapolating beyond those experimental cells to improve promotional outcomes is growing rapidly, for example deciding who to target with a promotion and what the optimal design parameters of that promotion should be. Often these optimal designs do not align with the values used in the promotional experiment. Yet there is little to date in the marketing literature available to guide these targeting and design decisions in experimental contexts where compliance is not guaranteed.

The MTE approach we outline offers several advantages over past approaches used in marketing. First, it does not assume away unobservable factors that can affect compliance and outcomes (that is, it does not assume compliance is unconfounded). Unobservables complicate the task
of extrapolation and our data suggest unobservables matter. Simply assuming their effect away during extrapolation biases causal estimates and leads to sub-optimal policy outcomes. Second, in the face of unobservables, we argue causal extrapolation is only possible for promotion features that can function as valid instruments for compliance—in other words, features whose impact on the outcome are fully mediated by compliance decisions. Moreover, this requirement is true, even if the design parameters are manipulated experimentally. This observation is relevant in marketing, as prior research has extrapolated non-fully-mediated manipulations. Third, in the case where such mediation exists and design parameters are conditionally excludable from outcomes, we show how the MTE approach can be adapted to extrapolation in the context of compliance promotions. This approach is easy to use (as it is implemented in R) and does not require strong assumptions on the error structure (e.g., separability or monotonicity) or restrictions on functional form of the outcome equation. In our context, the monotonicity and separability assumption are not supported by the data and flexibility in the outcome equation (the MTR functions) is necessary to capture patterns in the data. Fourth, we show that simple interpolation methods commonly used in industry, where levels of outcomes are interpolated between observed experimental manipulations, are only locally valid for very small changes in design parameters. Applying this approach to a loyalty reward promotion experiment implemented by IHG, we find that extrapolation is only valid for rewards, but not hurdle levels, and that traditional approaches to extrapolation can over- or underestimate the effects of reward promotions. Further we show reward designs can be optimized for specific targets.

Given the growth in machine learning approaches used to estimate heterogeneous treatment effects for purposes of targeting in the face of a large number of observable covariates, an obvious next step is to integrate MTE approaches with machine learning to enable promotional design and targeting in the face of unobservables. We hope this research will be an initial step to enable marketers and researchers to extrapolate for policy evaluation and develop new approaches for the targeting and design of compliance promotions.
References


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A Extrapolation

A.1 Policy-Relevant Treatment Effect

The PRTE for a promotion offering $R$ bonus points is derived from the more primitive LATE of meeting the terms of the promotion by visiting $H$ or more hotels ($D = 1$) on the outcome $Y$, among customers with baseline $B$ and tier $T$ who are compliers when offered $R = r$ points. For these compliers, $D = 1$ when $R = r$, and $D = 0$ when $R = 0$ (conditional on $X$, which we subsequently suppress in the notation), and denote this more compactly as $D(r) = 1, D(0) = 0$. Using the potential outcomes notation introduced in Section 3.1, we can write this (conditional on $X$) causal effect as:

$$LATE_{0\rightarrow r} \equiv E[Y_1|D(r) = 1, D(0) = 0] - E[Y_0|D(r) = 1, D(0) = 0]$$

Let $\pi^C_{0\rightarrow r}$ denote the proportion of customers who are compliers when offered $R = r$ points, but not when offered 0 points, and $\pi^1_0$ denote the proportion of customers who would have $D = 1$ even if offered 0 points (i.e., without a promotion). Further, let $N$, $mgn$, and $cpp$ be the number of targeted customers, the expected margin per stay, and expected cost per point as defined in the main text. We can use these terms to define the expected, incremental profit from this promotion:

$$PRTE_{0\rightarrow r}^H = \frac{N \cdot \pi^C_{0\rightarrow r}}{\text{Number of compliers}} \cdot \frac{LATE_{0\rightarrow r} \cdot mgn}{\text{Incremental margin from compliers}} - \frac{N \cdot (\pi^1_0 + \pi^C_{0\rightarrow r})}{\text{Number of customers receiving reward}} \cdot \frac{cpp \cdot r}{\text{Per-customer cost of reward}}$$

We note two identities. First, $LATE_{0\rightarrow r}$ is equal to the ratio of the intent-to-treat effect of $R = r$ points (versus $R = 0$) on $Y$, times the proportion of compliers when $R = r$ (versus $R = 0$). Second, $\pi^1_0 + \pi^C_{0\rightarrow r}$ is equal to the expected proportion of customers in the offer group who meet the terms of the promotion ($D = 1$) when offered $R = r$ bonus points. Thus, we can rewrite the PRTE as

$$PRTE_{0\rightarrow r}^H = N \cdot \left( \frac{E[Y|R = r] - E[Y|R = 0]}{\text{ITT effect of } R = r \text{ points on } Y} \right) \cdot mgn - N \cdot cpp \cdot r \cdot \frac{E[D|R = r]}{\text{Proportion receiving reward}}$$

Given random assignment of $R$ (conditional on $X$), a non-parametric estimator of this PRTE is available at values of $R$ observed in the experiment.
A.2 Logit Propensity and General MTR Function

Conditional on $X$ (and suppressing $X$ in the notation), the intent-to-treat effect (ITT) of a promotion with $R = r$ points (versus $R = 0$ points—i.e., no promotion) is given by

$$ITT_{0\rightarrow r} = \int_{p(0)}^{p(r)} [m_1(u) - m_0(u)]du$$

Change the variable of integration from $u$ to $\varrho$, where $u = p(\varrho) \iff \varrho = p^{-1}(u)$ and $d\varrho/du = 1/p'(\varrho) \iff p'(\varrho)d\varrho = du$:

$$ITT_{0\rightarrow r} = \int_{0}^{r}[m_1(p(\varrho)) - m_0(p(\varrho))]p'(\varrho)d\varrho$$

Specify the propensity function as $p(\varrho) = \text{logit}^{-1}(\alpha + \beta \varrho)$. The derivative of the propensity function with respect to $\varrho$ is

$$p'(\varrho) = \beta [1 - \text{logit}^{-1}(\alpha + \beta \varrho)] [\text{logit}^{-1}(\alpha + \beta \varrho)]$$

$$= \beta [2 + 2 \cosh(\alpha + \beta \varrho)]^{-1}$$

thus the expression above becomes

$$ITT_{0\rightarrow r} = \int_{0}^{r} \frac{[m_1(p(\varrho)) - m_0(p(\varrho))]\beta}{2 + 2 \cosh(\alpha + \beta \varrho)}d\varrho.$$ 

A.3 General Propensity and Polynomial MTR Function

Let the MTRs be given by the polynomial function $m_d = \sum_{k=0}^{K} \theta_{dk}xu^k$. Then (again, suppressing $X$ in the notation), the ITT can be expressed as

$$ITT_{0\rightarrow r} = \int_{p(0)}^{p(r)} [m_1(u) - m_0(u)]du$$

$$= \int_{p(0)}^{p(r)} \left[ \sum_{k=0}^{K} \theta_{1k}xu^k - \sum_{k=0}^{K} \theta_{0k}xu^k \right]du$$

$$= \sum_{k=0}^{K} \frac{\theta_{1k}(p^{k+1}(r) - p^{k+1}(0))}{k+1}$$

$$= \bar{\theta}_0 x(p(r) - p(0)) + \sum_{k=1}^{K} \bar{\theta}_k x(p^{k+1}(r) - p^{k+1}(0))$$

where $\bar{\theta}_q = \theta_1^q - \theta_0^q$. 

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