Identifying Causal Marketing-Mix Effects Using a Regression Discontinuity Design

Wesley Hartmann∗ Harikesh S. Nair† Sridhar Narayanan‡

Previous Drafts: July 2008, November 2009
This version: May 2011§

Abstract

We discuss how regression discontinuity designs arise naturally in settings where firms target marketing activity at consumers, and illustrate how this aspect may be exploited for econometric inference of causal effects of marketing effort. Our main insight is to use commonly observed discontinuities and kinks in the heuristics by which firms target such marketing activity to consumers for nonparametric identification. Such kinks, along with continuity restrictions that are typically satisfied in Marketing and Industrial Organization applications, are sufficient for identification of local treatment effects. We review the theory of Regression Discontinuity estimation in the context of targeting, and explore its applicability to several Marketing settings. We discuss identifiability of causal marketing effects using the design and show that consideration of an underlying model of strategic consumer behavior reveals how identification hinges on model features such as the specification and value of structural parameters as well as belief structures. We emphasize the role of selection for identification. We present two empirical applications: the first, to measuring the effect of casino e-mail promotions targeted to customers based on ranges of their expected profitability; and the second, to measuring the effect of direct mail targeted by a B2C company to zip-codes based on cutoffs of expected response. In both cases, we illustrate that exploiting the regression discontinuity design reveals negative effects of the marketing campaigns that would not have been uncovered using other approaches. Our results are nonparametric, easy to compute, and control for the endogeneity induced by the targeting rule.

Keywords: regression discontinuity, nonparametric identification, treatment effects, targeted marketing, selection, endogeneity, casinos, direct-mail.

∗Associate Professor of Marketing, Stanford GSB, Email: hartmann_wesley@gsb.stanford.edu
†Associate Professor of Marketing, Stanford GSB, Email: harikesh.nair@stanford.edu
‡Associate Professor of Marketing, Stanford GSB, Email: sridhar.narayanan@stanford.edu
§We thank the review team, Anand Bodapati, Florian Zettlemeyer, seminar participants at Goethe University (Frankfurt), IIM (Bangalore), NYU-Stern, Washington-Olin, Yale-SOM, and at the Marketing Science (2007, 2009), SICS (2010) and QME (2010) conferences, for their helpful feedback. All errors are our own.
1 Introduction

Targeting is a ubiquitous element of firms’ marketing strategies. The advent of database marketing has made it possible for firms to tailor prices, advertising and other elements of the marketing mix to consumers based on their type (e.g., Rossi, McCulloch and Allenby by 1996). The measurement of the causal effects of such targeted marketing is however tricky. A first-order complication arises as observed correlation in the data between outcome variables and marketing activities is driven both by any causal effects of marketing and by the targeting rule, leading to an endogeneity problem in estimation. The commonly used solution of instrumental variables may be infeasible in such contexts because a good instrument, a variable that is correlated with the marketing effort, but otherwise uncorrelated with the outcome variable, may be hard, if not impossible, to obtain. In this paper, we propose utilizing heuristic rules often used by firms for targeting as a Regression Discontinuity design to nonparametrically measure the causal effects of marketing effort.

Regression Discontinuity (henceforth RD) was first introduced by Thistlethwaite and Campbell (1960) in the evaluation literature (e.g. Cook and Campbell 1979), and has become increasingly popular in program evaluation in economics. An RD design arises when treatment is assigned based on whether an underlying continuous score variable crosses a cutoff. The discontinuity induced by this treatment rule induces a discontinuity in the outcomes for individuals at the cutoff. Hahn, Todd and Van der Klaauw (2001), henceforth HTV (2001), formally showed that this discontinuity nonparametrically identifies a local average treatment effect, if the counterfactual outcomes for agents with and without treatment are also continuous functions of the score in the neighborhood of the cutoff. Under these conditions, observations immediately to one side of the cutoff act as a control for observations on the other side, facilitating measurement of a causal effect of the treatment. RD designs have now been used to study treatment effects in a variety of contexts, from education (Black 1999, Angrist and Lavy 1999); to housing (Chay and Greenstone 2005); and to voting (Lee et al. 2004) amongst others. In contrast, applications to Marketing and Industrial Organization contexts have been sparse. Notable applications include Busse et al. (2006 and 2010) who measure the effect of manufacturer promotion on automobile prices and sales using a design in which calendar time is the score variable, albeit not in a
targeted marketing context. Van der Klaauw (2008), Imbens and Lemieux (2008) and Lee and Lemieux (2010) are excellent summary papers on RD that discuss the method, its variants and applications in detail.

We believe targeted marketing contexts are particularly well-suited for the use of RD methods for two reasons. First, firms often target groups of customers with similar treatments. Even though firms face a continuous distribution of consumer types, it is common in actual business practice to allocate similar marketing interventions to groups of customers. The reasons for this bunching include menu or implementation costs, or the inherent difficulty of tracking historical information required for targeting at the individual-customer level. Second, targeting policies of firms often involve trigger rules. Marketing allocation often involves “rules-of-thumb” whereby groups of consumers obtain similar marketing levels based on whether a relevant function of their characteristics or historical behavior crosses a pre-specified cutoff. For instance, catalogs might be mailed based on cutoffs of underlying ‘RFM’ score variables, credit card promotions may be given based on cutoffs of FICO® scores, detailing calls may be made to a physician based on whether he is in specific prescription-based deciles, price discounts may be given to people above or below certain age cutoffs, etc. The ubiquity of such trigger-rules generate a wealth of discontinuity-based contexts that facilitate nonparametric identification of marketing effects using an RD design, which have previously been unexploited in the Marketing literature.

Applying RD in Marketing and Industrial Organization contexts, where theoretical and empirical models of strategic choice are abundant, naturally leads us to consider the extent to which these models relate to the identifiability of RD. We consider permutations on simple models of consumer selection to delineate a set of viable and non-viable applications for RD. First, we present a Hotelling-style model to show that if customers face sufficiently high costs of selecting, RD is valid. The model illustrates that RD can often be used to measure marketing effects under geographic targeting (i.e. high fixed costs of moving to receive the treatment), or situations where targeting is based on scores that cannot be changed, such as age-based marketing (i.e. infinite fixed cost of selection). We then apply RD to a geographic targeting example where the score variable is a function incorporating the probability of response at the zip code level. Direct mail is sent to a zip code if the probability of response is above
a cutoff.

Second, we present a detailed illustration of targeting based on past purchases. When past purchase behavior crosses a threshold, customers qualify for preferential treatment. We show that applicability of RD hinges on whether or not customers are uncertain about the exact score, the cutoff, or both. The implication is that canonical reward programs where thresholds and scores are communicated to customers have selection effects that invalidates RD. On the other hand, database marketing programs, where typically both the score and cutoff are unknown to consumers, are valid RD applications even in the presence of selection. To illustrate the value of RD in database marketing, we analyze data from a casino’s marketing efforts to members of its loyalty program. The casino uses a targeting rule that is discontinuous based on cutoffs in the average level of past gambling activity. These cutoffs are not known to the consumers and hence, they cannot self-select into preferential treatment. We estimate the effect of both the database marketing and geographic targeting applications nonparametrically using local linear regression (Fan and Gijbels 1996). We find in both cases that controlling for the endogeneity has large implications on the conclusions drawn from the analysis. In particular, we find that a naive estimate has an altogether different sign than the RD estimate.

Finally, we also formally consider time as a score. We illustrate that the validity of the RD in the timing case hinges on whether or not the estimation is conditional on selection decisions such as purchase or store visitation as well as the belief structures leading to these decisions. We discuss the role of dynamics induced by durability or storability in the interpretation and identification of treatment effects in a time-based design. An important takeaway from these analyses is that the identification conditions cannot be evaluated without consideration of an explicit structural model of behavior of agents that is representative of the underlying data generating process. Our analysis illuminates an “RD Paradox”: the design is often thought of as “atheoretic” or “assumption-less,” but identification often relies on crucial, sometimes non-transparent primitive assumptions regarding behavior.

We consider the RD design to be complementary to several alternative methods focused on uncovering causal effects. Randomized variation through experiments are ideal, but firms are often unable

---

1For instance, pharmaceutical firms use volumetric deciles of physicians to decide the number of detailing calls made to doctors. These deciles are category specific and doctors are unlikely to know their own prescription volumes relative to all other physicians for each category. Similarly, consumers are unlikely to know their RFM score or the trigger values used for targeted mailing of catalogs.
or unwilling to conduct randomized trials, due to considerations of cost, time and potential backlash from consumers not receiving preferred treatment. When experimentation is unavailable, RD may be more viable for targeted marketing applications. One popular alternative is to use instrumental variables, but such variables are hard to obtain because customer-side variables typically fail the required exclusion restrictions and cost-side variables typically do not vary by the segment the firm uses for targeting. Another alternative is to augment the analysis with a model of how firms allocate marketing efforts and to incorporate the restrictions implied by this model in estimation (e.g., see Manchanda et al. 2004; Otter et al. 2011). These authors are careful to point out that this approach is feasible only if full information is available to the analyst about how the firm allocates its marketing efforts. In the absence of such information, the analysis is sensitive to misspecification bias.

The main caveats for adopting the RD approach are three-fold. First, by its nonparametric nature, the estimator is data intensive and requires many observations on consumer behavior at the cutoff; in sparse-data situations, parametric approaches are more suitable. Second, the estimator provides a local treatment effect which is relevant only for the sub-population of consumers at the cutoff, and not globally.\(^2\) A third caveat is that, like any other alternative, the conditions for the validity of the estimator have to be carefully assessed depending on the context. We consider the last aspect especially crucial. The HTV (2001) conditions on identification are stated in terms of continuity of counterfactual outcomes at the cutoff. We discuss in detail how these conditions can be translated in practice to several commonly observed targeted marketing situations. A key point we wish to emphasize is that the validity of the RD design has to be based on a formal model of data generating process, by explicitly considering how consumers sort at the cutoff.

To summarize, this paper makes three contributions. First, we identify the ready application of the RD design to typical targeted marketing contexts. Our goal is not to present new estimators per se, but to point out how discontinuous rules-of-thumb, which are pervasive in real-world marketing situations, may be used to achieve nonparametric identification. Further, we point out that such rules-of-thumb, which have been typically treated as nuisance issues to be dealt with, are a source of

\(^2\)In many situations, this may precisely be the object of interest for inference. Measurement of treatment effects for the entire population would require more assumptions, or the restrictions from a formal model of behavior.
identification of the causal effects of marketing activities. Second, we present detailed illustrations of
the identifiability of causal marketing effects using the design, and show theoretically the conditions
under which the RD estimator may be valid in marketing contexts, considering in particular, the role
of consumer self-selection. The link to a structural model, and the treatment of identification in the
context of such a framework, is new to the RD literature. Finally, we demonstrate the utility of the
RD approach through two empirical applications, with counter-intuitive conclusions that are hard to
uncover through a naive analysis.

The rest of the paper proceeds as follows. We first provide a brief review of the identification
conditions for the RD estimator. We then discuss our theoretical and simulation results on identifica-
tion of marketing mix effects under specific targeting situations. We then present our two empirical
applications. The last section concludes.

2 Identification of Marketing-Mix Effects Using an RD Design

In this section, we review identification conditions from HTV (2001), Lee (2008) and Lee and Lemieux
(2010) and discuss the role of local inference in Marketing contexts.

2.1 Identification Conditions

The following describes the key identification conditions from HTV (2001). To set up the notation, let
d$_i$ indicate exposure to marketing, and let $Y_i(1)$ and $Y_i(0)$ be the potential outcomes for individual $i$,
with and without marketing. The treatment effect, $Y_i(1) - Y_i(0)$, cannot be directly estimated, as only
$Y_i = d_i Y_i(1) + (1 - d_i) Y_i(0)$ is observed by the analyst for each $i$. Instead, the focus is on measuring
an average treatment effect, $E[Y_i(1) - Y_i(0)]$, where the expectation $E(.)$ is taken over individuals
(or the density of individual-types). The RD design implies that treatment is assigned depending on
whether a continuous score, $z_i$, crosses a cutoff, $ar{z}$, i.e., $d_i = I(z_i \geq \bar{z})$. Then, the observed size of the
discontinuity in the outcome (Eq. 1) and the treatment (Eq. 2) in a neighborhood of $ar{z}$ are,

$$Y^- = \lim_{z \to \bar{z}^-} E[Y_i(0) | z_i = z] \quad \text{and} \quad Y^+ = \lim_{z \to \bar{z}^+} E[Y_i(1) | z_i = z]$$

$$d^- = \lim_{z \to \bar{z}^-} E[d_i | z_i = z] \quad \text{and} \quad d^+ = \lim_{z \to \bar{z}^+} E[d_i | z_i = z]$$

(1) (2)
Theorem 1 (HTV (2001)) Suppose (1) $d^-$ and $d^+$ exist, and $d^+ \neq d^-$ (2) $Y^-$ and $Y^+$ are continuous in $z_i$ at $z_i = \bar{z}$; then, the quantity $\beta = \frac{Y^+ - Y^-}{d^+ - d^-}$ measures the average treatment effect at $\bar{z}$, i.e. $\beta = \mathbb{E}[Y(1) - Y(0) | z_i = \bar{z}]$.

That is, to obtain a causal effect of the treatment, we estimate the discontinuity in the outcomes, and weigh it down by the discontinuity in treatments. Estimation of the size of the discontinuities can be achieved by non-parametrically estimating the limit values $(d^-, d^-, Y^-, Y^+)$ (see for e.g., Porter 2003). The interpretation of size of the discontinuity in outcomes as a treatment effect hinges crucially on the continuity conditions. The continuity of equation $\mathbb{E}[Y(1)]$, and $\mathbb{E}[Y(0)]$ in $z_i$ enables us to interpret the outcomes just below $\bar{z}$ as a valid counterfactual for the outcomes just above $\bar{z}$, thereby facilitating a relevant control group for measurement of a treatment. The condition of continuity of counterfactual outcomes is equivalent to requiring either continuity in the density of $z_i$, or continuity in the density of consumer types given $z_i$ at $z_i = \bar{z}$. Essentially, one implies the other. Letting $\theta$ index consumer types, and taking the expectation over $\theta$, note that, $\mathbb{E}_\theta [Y(0) | z = \bar{z}] = \int Y(0|\theta) h(\theta | z = \bar{z}) d(\theta)$, where $h(.)$ is the density of $\theta$ given $z$ (equivalently for $Y(1|\theta)$). If $h(\theta | z)$ is not continuous at $z = \bar{z}$, we obtain that,

$$\lim_{z \to \bar{z}^-} \int Y(0|\theta) h(\theta | z) d(\theta) \neq \lim_{z \to \bar{z}^+} \int Y(0|\theta) h(\theta | z) d(\theta)$$

Thus, continuity of counterfactual outcomes at the cutoff is violated, and the RD is invalid. By implication, the RD design is equivalently invalid if the distribution of the score $z$ given type $\theta$ is not continuous at $z = \bar{z}$. By Bayes rule, the conditional density of types given the score is $h(\theta | z) = \frac{f(z|\theta) f(\theta)}{f(z)}$, implying that a discontinuity in $f(z|\theta)$ will make $h(\theta | z)$ discontinuous as well.

The continuity condition is invalid in the context of selection. Assume that agents have control over their score. If they can precisely control their score and have an incentive to select into treatment, the distribution of the score $z$ will not be continuous at the cutoff $\bar{z}$, since the agents immediately to the right of the cutoff are those who chose to select into treatment, while those to the left of the cutoff chose not to be treated. Nevertheless, Lee (2008) and Lee & Lemieux (2010) introduce the important

---

3In a sharp RD, $d^+ = 1$ and $d^- = 0$, so the discontinuity in outcomes itself is the treatment effect. In a “fuzzy” RD, $(d^+, d^-) \in (0, 1)$, i.e. treatment is not certain if the score is crossed.

4Recall, by the definition of continuity, $h(\theta | z)$ is continuous at $z = \bar{z}$, if $\lim_{z \to \bar{z}^-} h(\theta | z) = \lim_{z \to \bar{z}^+} h(\theta | z) = h(\theta | \bar{z})$. Hence, discontinuity of $h(\theta | z)$ implies (3).
idea that when the score includes some random noise, the continuity conditions may still be satisfied and a valid RD design obtained. Formally, let the score now have a component to the utility that is not predictable and not controllable by the agent. Thus, $z = x + w$, where $x$ is the systematic part of the score that the agent can predict and can take actions to control, and $w$ is an exogenous random chance component to the score which cannot be predicted or controlled by the agent.

If $w$ has a continuous density, the distribution of $z$ at the cutoff $\bar{z}$ is locally continuous, thus validating the RD design. This results from the fact that agents are unable to select precisely into treatment. Thus, in the neighborhood of the cutoff $\bar{z}$, the distribution of agents on either side of the cutoff is the same. In the above notation, $h(\theta|z)$ is continuous at $z = \bar{z}$, validating the RD design. If, on the other hand, $w$ is either zero (i.e., there is no random, unpredictable component to the score) or is discontinuous at $\bar{z}$, then the randomness cannot validate the design.

The continuity condition is also violated if the cutoffs on the score variable that define treatment is chosen at a point of discontinuity in the score. In some Marketing contexts, for instance, this may happen because competitive promotions use the same cutoffs for treatment, or because firms use natural points of discontinuity in the score as cutoffs for assigning consumers to the treatment. The latter is typically associated with sparse data in the neighborhood of the cutoff, or the use of structural breaks in the score to decide the cutoffs.

### 2.2 Local Inference and Marketing

As previously mentioned, RD estimates an effect that is local to a particular value of the score. This could be problematic when designing a marketing policy for the entire distribution of customers in a database. If a firm were to only set one cutoff, we might worry the firm would not set that cutoff exactly where the effect of the marketing policy is largest, or even where the magnitude is expected to be the average effect. Therefore, if firms find large local effects, their optimal response is to move the cutoff to include more customers until the marginal benefit of the treatment is roughly equal to the incremental cost of the treatment.

How does such a strategic cutoff setting process affect inference? It remains true, per the preceding subsection, that the effects are valid treatment effects at the cutoffs. Yet, it is clear the treatment effects...
are not randomly sampled points from the distribution of the score. Practitioners should therefore be cautious in interpreting the results. We recommend the use of multiple cutoffs and movement of cutoffs in response to either negative marginal effects or large marginal effects. Through such a process, firms will retain the simplicity of classifying customers into a limited number of targeted groups for Marketing, while also reducing the loss associated with under- or over-incentivizing some set of customers.

3 Geographic Targeting

We begin by considering an example of geographic targeting. This simple application is instructive for two reasons. First, the requirement for identification is simply that the cost of moving residences is substantially larger than benefits of preferential marketing. Second, this model allows us to clearly illustrate the identifying conditions from HTV. We wrap up this section with an empirical analysis of geographic targeting using RD.

3.1 Model

We define a model that reflects our empirical application where consumers are targeted preferential direct-mail based on their location. The model involves two stages. Initially, each consumer is endowed with a score $z$, which can be thought of as his location on a Hotelling line. In the first stage, the customer makes a selection decision to move his location to $\tilde{z}$, which we call the “manipulated score”. If the consumer decides not to move to a new location, his manipulated score $\tilde{z}$ remains the same as his initially endowed score $z$. If $\tilde{z} \geq \bar{z}$, the consumer is eligible for the treatment. In the second stage, the customer makes a decision about the outcome of interest, conditional on his treatment eligibility. The rational consumer takes the effect of treatment on outcomes in the second stage into account when making a selection decision in the first stage. We consider the two stages of the model in reverse, considering the second stage first and then using the optimality condition from the second stage into account in solving for the optimal decision in the first stage.
3.1.1 Stage 2: Outcome

The outcome \( Y \) is a binary variable indicating whether or not an individual makes a purchase. Treatment is indicated by the binary variable \( R = \mathcal{I}(\tilde{z} \geq \bar{z}) \). We model the individual’s outcome as a random utility model \( Y = \mathcal{I}(u_1 > u_0) \), where,

\[
\begin{align*}
  u_1 &= v(X, R = 1|\beta) \mathcal{I}(\tilde{z} \geq \bar{z}) + v(X, R = 0|\beta) \mathcal{I}(\tilde{z} < \bar{z}) + \eta_1 \\
  u_0 &= \eta_0
\end{align*}
\]

(4)

Here, \( v(\cdot) \)-s indicate the non-stochastic portion of the individuals utility of choosing to purchase, and \( \eta = (\eta_1, \eta_0) \) are mean-zero unobservables (to the econometrician) that affect purchases. We introduce these unobservables so as to clarify the separate role played by unobservables affecting selection versus those affecting purchase, in ensuring identification.

3.1.2 Stage 1: Selection

In the first stage, each customer can choose to manipulate his current score to \( \tilde{z} = z + m \). Changing the score is not costless. The total cost of moving has a fixed and marginal component, \( C = F + \tau m \). A consumer at \( z \) would move to the cutoff \( \tilde{z} \) if the expected value from obtaining the treatment is greater than the cost,

\[
\mathbb{E}_\eta [u_1 - u_0] = v(X, R = 1|\beta) - v(X, R = 0|\beta) \geq F + \tau (\tilde{z} - z)
\]

(5)

The marginal customer that selects into treatment is defined as \( z^* \) such that,

\[
z^* = \frac{1}{\tau} (F + \tau \tilde{z} - [v(X, R = 1|\beta) - v(X, R = 0|\beta)])
\]

(6)

3.1.3 Identification

We now consider whether an RD applied to this context is valid. Validity depends on whether continuity of the manipulated score \( \tilde{z} \) is violated at the cutoff, \( \bar{z} \). Continuity of \( \tilde{z} \) depends on whether the marginal consumer has a score \( z^* \) less than \( z \). If, \( z^* < \bar{z} \), all consumers between \( [z^*, \bar{z}] \) would move. Hence, the score would have positive mass to the right of \( \bar{z} \), but no mass just to the left of \( \bar{z} \). Thus, the distribution of \( \tilde{z} \) would jump at \( \bar{z} \), invalidating the RD. Another intuition is to note that with such selection, the
limit of the counterfactual outcome just to the left of $\tilde{z}$ does not exist. Formally, the condition for RD to be invalid, $z^* < \tilde{z}$, implies from Equation (6) that $F < [v(X, R = 1|\beta) - v(X, R = 0|\beta)]$. Intuitively, if the fixed costs of moving are not higher than the gain from moving, selection can invalidate an RD application by violating continuity in the score relevant for treatment.

**Heterogeneity**  We now consider if heterogeneity of consumer types can resolve this identification problem. For instance, heterogeneity in $\theta = (F, \tau, \beta)$ could imply that there exist at least some mass of consumers to the left of $\tilde{z}$, who may not move (for example, individuals with very high fixed costs). This ensures that the limit of the counterfactual outcome from the left exists. However, unless all customers have sufficiently large fixed costs, the RD is not valid. Mathematically, it is easier to see this in terms of checking the continuity of the counterfactual outcome $Y$ in the absence of treatment, $R = 0$. The limit of the counterfactual outcome from the left of $\tilde{z}$ is,

$$
\lim_{\tilde{z} \to \tilde{z}^-} \mathbb{E}[Y(0|\theta) | \tilde{z}] = \lim_{\tilde{z} \to \tilde{z}^-} \int Y(X, R = 0, \tilde{z}, \eta, \theta) dF_{\theta|\tilde{z}}(\theta|\tilde{z} < \tilde{z}) dF_{\eta}(\eta) \tag{7}
$$

while the limit from the right of $\tilde{z}$ is,

$$
\lim_{\tilde{z} \to \tilde{z}^+} \mathbb{E}[Y(0|\theta) | \tilde{z}] = \lim_{\tilde{z} \to \tilde{z}^+} \int Y(X, R = 0, \tilde{z}, \eta, \theta) dF_{\theta|\tilde{z}}(\theta|\tilde{z} \geq \tilde{z}) dF_{\eta}(\eta) \tag{8}
$$

In the presence of selection, the set of consumers to the right of the cutoff would have lower $\tau$ and $F$, and higher $\beta$ than those to the right. Hence, $F_{\theta|\tilde{z}}(\theta|\tilde{z} < \tilde{z}) \neq F_{\theta|\tilde{z}}(\theta|\tilde{z} \geq \tilde{z})$ and the left-hand sides of Equations ((7)) and ((8)) are not the same. Hence, heterogeneity does not guarantee validity of the RD design. This can be mitigated only if $\theta$ is such that no one moves in order to obtain treatment, which is likely if the fixed costs of moving are large enough compared to the benefits of obtaining the reward $R$.

**Discussion**

The above analysis suggests that geographic targeting will plausibly be a valid RD application because preferential marketing treatment (e.g. receipt of catalogs) is unlikely to ever be large enough to outweigh the costs of moving. However, applying RD to geographic targeting relies on an underlying model of customer selection as well as the definition and magnitude of structural parameters such as
moving costs. Moving outside of the geographic space as the underlying score variable may involve much smaller “moving” costs that could invalidate RD applications.

3.2 Direct-Mail Activity by a Direct Marketing Firm

We consider a canonical marketing problem: measuring the causal effects of direct-mail. Our application involves a direct marketing firm sending direct-mail to customers to solicit a request for contact with the company. Once the customer contacts the company (either online or via the phone), further promotions and prices are offered in order to acquire the customer. We focus on whether or not the customer contacts the company as the response variable of interest. Measuring the effect of direct-mail is not straightforward, as the firm does not randomly choose consumers to send the direct mail solicitations to. Rather, as response rates are small for this mode of marketing (of the order of 1-2%), the firm tends to send direct-mail to customers it anticipates are most likely to respond. The firm’s targeting is at the level of a zip code, i.e. it chooses to send direct mail to all consumers in the zip code or to none. Importantly, the firm decides the choice of zip codes in which to send direct mail to customers based on cutoffs on a one dimensional score variable, which is an (unknown to us) function of customer characteristics, past response histories and other features of the zip code. The question of interest is whether direct mail solicitations causally affect the number of customer contacts. We observe the score variable, the cutoff, a treatment variable indicating whether or not direct mail was sent as well as the customer contacts for each of the zip codes in six different states in the US. The dependent variable is whether or not a customer in a zip code contacts the company.

We conduct four kinds of analysis on this data to illustrate the application of RD to this geographical targeting context, and report these results in Table 1. First, we test for differences in mean customer contact rates for those zip codes in which consumers received the mail solicitation vs. zip codes in which consumers did not receive mail solicitations, and we do this separately for each of the six states in our data. These mean differences are identical to the slope coefficients for an OLS regression of direct mail solicitations on the customer contact rates. These results, reported in the top panel of the table, would seem to suggest a significantly positive effect of direct mail solicitation on customer contact rates. However, this is a naive analysis, that does not account for the fact that zip codes with
higher scores are selected for the direct mail campaign, presumably because they have higher expected customer response rates.

We next discuss the RD estimates for the effect of direct mail solicitations on customer contact rates. Specifically, we compare the limiting values of the customer contact rates for zip codes with scores in the neighborhood of the cutoff and on the two sides of it to measure the causal effect of the direct mail solicitation. The identifying assumption for the causal effect using RD in this application is that the benefits from receiving the direct mail solicitation are small compared to the fixed costs of moving to a different zip code, and hence it is implausible that consumers select into treatment. We compute the limiting values of the customer contact rates using local linear regressions on the two sides of the cutoff. In the middle panel of Table 1, we report the RD estimates with optimal bandwidth computed separately for each of the six states in our data, since the cutoffs differ by state. In any RD application, we need to choose the bandwidth around the cutoff in which the analysis is done. We focus on the bandwidth that minimizes mean squared error.

The results of the RD estimates computed for the optimal bandwidth show that the causal effect of direct mail is positive only in Tennessee, and is actually negative in Arizona and Wisconsin. The remaining three states have no significant effect of the direct mail solicitation on response rates. While the null effects in Washington, New Jersey and Minnesota are not necessarily surprising, the negative effects in Arizona and Wisconsin are. We conjecture, but cannot verify, that these may reflect an adverse reaction to direct mail activity due to heavy direct mail activity by the firm in the past. Another explanation is that in Wisconsin, for instance, consumers who were picked out for this campaign were subject to heavy direct mail by competitors.

We assess sensitivity of the results to the bandwidth selected. In the bottom panel of the Table, we report the RD estimates when the bandwidth is 50% higher than the optimal bandwidth. We find that the estimates for Arizona are not robust to the change of bandwidth, while those in Wisconsin are. This illustrates the fact that in RD designs, bandwidth selection is an important part of the process of inference.
<table>
<thead>
<tr>
<th>State</th>
<th>AZ</th>
<th>WA</th>
<th>NJ</th>
<th>MN</th>
<th>TN</th>
<th>WI</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Difference in Means</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.0051</td>
<td>0.0020</td>
<td>0.0019</td>
<td>0.0011</td>
<td>0.0023</td>
<td>0.0034</td>
</tr>
<tr>
<td></td>
<td>(0.0012)*****</td>
<td>(0.0002)*****</td>
<td>(0.0003)*****</td>
<td>(0.0005)****</td>
<td>(0.0005)*****</td>
<td>(0.0012)*****</td>
</tr>
<tr>
<td><strong>RD Estimate with Bandwidth that Minimizes MSE</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-0.0168</td>
<td>-0.0002</td>
<td>0.0000</td>
<td>0.0008</td>
<td>0.0063</td>
<td>-0.0103</td>
</tr>
<tr>
<td></td>
<td>(0.0073)**</td>
<td>(0.0012)</td>
<td>(0.0016)</td>
<td>(0.0017)</td>
<td>(0.0021)*****</td>
<td>(0.0048)**</td>
</tr>
<tr>
<td><strong>RD Estimate with 50% Increase in Bandwidth</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-0.0090</td>
<td>-0.0001</td>
<td>-0.0001</td>
<td>-0.0010</td>
<td>0.0049</td>
<td>-0.0078</td>
</tr>
<tr>
<td></td>
<td>(0.0070)</td>
<td>(0.0009)</td>
<td>(0.0011)</td>
<td>(0.0014)</td>
<td>(0.0017)*****</td>
<td>(0.0037)**</td>
</tr>
</tbody>
</table>

** is significant at 1% level, ** at 5% level and * at 10% level

**Table 1:** Results from Analysis of Direct-Mail Program

4 Targeting Based on Past Purchases

We discuss a model of history-based targeting. Such an approach is valid for RD if individuals either do not know their score or cutoff. The former is an application of the approach in Lee (2008) where a random shifter of the score is realized post-selection. Uncertainty about the cutoff however is more involved, and requires a formal derivation of agents belief structures because identification hinges on continuity of these beliefs in particular neighborhoods. A precursor to deriving each is a model with known score and cutoff. In this case, it becomes clear why a random variable known to the consumer, such as the realization of a logit shock to outcomes, is insufficient to make RD valid. This rules out RD for reward programs where firms communicate point accumulations and payoff schedules. Structural consideration of this application also allows us to illustrate how selection satisfying Lee’s criteria affects the interpretation of treatment effects. To better deal with treatment effects on selected samples, we suggest and analyze an application with multiple cutoffs.

4.1 Model

Once again, the model reflects one of our empirical applications where we consider a reward program in which a casino offers short-lived promotions to gamblers based on a score computed as a function of their past gambling activity. Unlike in a geographical targeting application, fixed costs to consumers of changing gambling amounts are likely quite low, so the validity conditions in the previous section do not apply. However, in the context of this application, we show that an RD design continues to be valid.
if consumers have uncertainty about the exact score or cutoff used by the firm to target promotions. This requires augmenting the model to allow for uncertainty and randomness at the selection stage. Our analysis reveals that the role of this randomness is subtle and context specific. We show that the extent to which such randomness can smooth out the discontinuity induced by selection depends on the nature of the randomness, and the precise details of how it affects behavior. We build on the contribution of Lee (2008), which pointed out that randomness in the score can validate RD designs, by discussing how this aspect requires a careful consideration of an underlying behavioral model for assessment.

4.1.1 Setup

Consider a simplified promotion program that provides a reward $R$ to consumers if an index of their past outcomes, $z$, crosses a cutoff, $\bar{z}$. In the casino application, $R$ can represent play credits given to consumers, and $z$ can represent an index of the dollar amount a consumer has played with the casino in the past (we present specific details in section 4.2). Here, $R$ is the treatment, $z$ is the score and $\bar{z}$ is the cutoff at which a discontinuity arises. The econometrician wishes to measure a treatment effect of the reward on current outcomes (e.g. how much the consumer plays today) by comparing the behavior of consumers just to the right of $\bar{z}$ to those just to the left of $\bar{z}$. Consumers are forward-looking, know the reward $R$ and initially also know the cutoff $\bar{z}$ and their score, $z$. Customers have an incentive to play to earn the reward when their current score is below the cutoff, but a reduced incentive to play with the objective of earning a reward when their current score already exceeds the cutoff.5

As before, we consider a two-stage model. In the first stage, the consumer makes a selection decision. He enters the first stage with no rewards, and with score $z$. Based on $z$, $R$ and his characteristics, he evaluates whether to self-select and play in the first stage in order to earn a reward in the second. Selection adds $m$ to $z$, and the manipulated score is then $\tilde{z} = z + m$. If the consumer chooses to not select into treatment, the manipulated score remains the same as the original score, i.e. $\tilde{z} = z$.

In stage two, those consumers with manipulated score $\tilde{z} \geq \bar{z}$ obtain the reward $R$. Then, condi-

5Such incentives have been shown to be nontrivial empirically - for instance, Nunes and Dreze (2010) document that airline consumers just below the 25,000 miles cutoff are significantly more likely to fly to earn a frequent-flyer reward than those just above. As we show, this type of discontinuity invalidates RD applications in the case of frequency reward programs.
tioning on their treatment status, all consumers make an outcome decision denoted as \( Y = Y(\tilde{z}, R, \eta) \), where \( \eta \) is an unobservable (to the econometrician).

The econometrician observes \( \{Y, \tilde{z}\} \) across a sample of consumers in the neighborhood of \( \tilde{z} \), and wants to estimate the effect of the treatment \( R \) on outcomes \( Y \). Note that the action of “playing” is both a selection action as well as an outcome of interest, albeit at two different stages.

In contrast to the previous model, we now allow for some randomness. We allow selection to be stochastic by introducing a shock \( \varepsilon \) that represents random events in the casino that affect consumer’s selection decisions. This shock is unobserved to the econometrician, but observed by consumers. To reflect the dependence on \( \varepsilon \), we denote the selection decision by an indicator \( y = y(m, z, R, \varepsilon) \).\(^6\) Thus, this setup builds on that in section (3) by a) allowing \( \varepsilon \) to influence the selection decision; and b) making actions at the selection stage discrete.

The manipulated score, \( \tilde{z} \), can be written as,

\[
\tilde{z} = z + m \times y(m, z, R, \varepsilon) \quad (9)
\]

Following a canonical discrete-choice set-up, we assume that \( y \) is determined based on an inequality condition involving consumer’s type, his state, \( z \), and the realization of the error, \( \varepsilon \),

\[
y = I(f(m, z, R) + \varepsilon > 0) \quad (10)
\]

We do not explicitly write out the deterministic component, \( f(m, z, R) \), but implicitly, this would be the difference between the choice-specific value function associated with playing and earning a reward and that for not playing. Consider a consumer 1 with \( z \) just to the left of \( \tilde{z} \) such that \( z \in [\tilde{z} - m, \tilde{z}) \). Given selection, the induced distribution of his manipulated score \( \tilde{z} \) is,

\[
\tilde{z} = \begin{cases} 
  z + m & \text{w.p. } \Pr(y = 1|z < \tilde{z}) \\
  z & \text{w.p. } \Pr(y = 0|z < \tilde{z})
\end{cases}
\]

To examine the implications of selection for the induced distribution of \( \tilde{z} \), note that in general, we expect that \( f(m, z, R|z < \tilde{z}) > f(m, z, R|z \geq \tilde{z}) \), as we expect that consumers who do not have the reward but are close to it derive a net value from self-selecting that is higher than that derived

\(^6\)To be clear, \( \varepsilon \) is an unobservable that affects the selection decision, \( y \), while \( \eta \) is an unobservable that affects the outcome decision, \( Y \).
by those who already have the reward. This implies that post selection, the manipulated score for consumers who start just below the cutoff will jump to have higher mass to the right of the cutoff. As the proportion of consumers who purchase at the selection stage is higher just below the cutoff than just above, the density of the manipulated score has a discontinuity at the cutoff. A discontinuity in the density of the manipulated score at the cutoff implies a discontinuity in the types just to the left and the right of $\bar{z}$, and RD is invalidated by selection. This intuition is demonstrated graphically in Figure 1.

Discussion We just demonstrated that selection in a typical reward program invalidates the RD design. Recent literature has suggested that random factors that drive outcomes (such as $\epsilon$), can make RD designs valid even in the presence of selection. Casual intuition may suggest that if random events shift consumer’s gambling amounts around a known cutoff, it could be that the distribution of types at the cutoff is “as if it were randomized”, and the RD may be valid. Here, we show that this intuition is not general and does not hold in the example shown above. Specifically, it fails because, as Lee (2008) suggests, the realization of the randomness to the agent must occur after selection for the randomness to smooth out the discontinuity in scores. The above analysis makes this intuition specific.

4.1.2 Mitigating Selection: Uncertainty

Uncertainty about the Score Now we consider if uncertainty (from the consumer’s perspective) in the manipulation of the score can mitigate the issues raised by selection. We generate the uncertainty by adding an additive shock to the manipulated score. This shock represents aspects of the score that cannot be controlled by the consumer at the selection stage. We assume this uncertainty has a continuous density. We therefore modify equation (9) to include a random term, $w$,

$$\tilde{z} = z + m \times y(m, z, R, \epsilon) + w$$

7This can be seen from the fact that the value of selecting is a function of the additional utility derived from getting the treatment, weighted by the increase in probability of treatment that comes from choosing to play at the selection stage. For consumers with $z < \bar{z}$, the probability of getting treatment increases when the consumer selects. However, for consumers with $z > \bar{z}$, this is not true.

8In principle, the RD estimate finds the limits of the outcomes for consumers on two sides of the cutoff, and therefore the distribution of $w$ can have infinitesimally small variance. In practice, since RD estimates involve comparing consumers in a certain bandwidth on two sides of the cutoff, we assume that the distribution of $w$ needs to have sufficiently large bandwidth relative to this bandwidth for the purpose of this discussion.
The key addition in this specification, $w$, is an error term that represents the consumer’s uncertainty about the eventual realization of the score. This may occur, for instance, if consumers forget their past play since it has been too long, or if consumer’s do not know the exact score used by the firm. Assume that $w$ has a continuous density with full support over $(\bar{z} - h, \bar{z} + h)$, where $h$ is the bandwidth defining the neighborhood of the cutoff used for estimation. While the econometrician does not observe either $\varepsilon$ or $w$, the consumer observes $\varepsilon$ prior to the selection decision, but not $w$. Introduction of $w$ thus removes the ability of agents to sort precisely around the cutoff $\bar{z}$. Following the intuition proposed in Lee (2008), we illustrate how this aspect restores the validity of the RD in spite of selection.

Consider two individuals in a neighborhood of $\bar{z}$, such that individual 1 lies close to the left and individual 2 to the right of the cutoff i.e., $z_1 < \bar{z}$ and $z_2 > \bar{z}$. Consider the distribution of the

Figure 1: Discontinuity due to selection: The top panel depicts a continuous distribution of the score, $f(z)$, chosen to be uniform for convenience. Four consumers, A-D, are shown. The second panel depicts the probability of selection as a function of $z$. The probability is higher for those in region 2: $[\bar{z} - m, \bar{z})$. The third panel depicts how the discontinuous incentives in the second panel led the otherwise continuously distributed individuals from the first panel to be discontinuously distributed around the cutoff $\bar{z}$. There are four relevant types of customers within a small bandwidth of the cutoff: A’ are those from region 1 that chose $y$; B’ are those from region 2 that chose $y$ with the added incentive $m$ that put them across the cutoff; C are those just below the cutoff who, despite an increased incentive to move, did not choose $y$; and D are those just above the cutoff that also did not chose $y$.

9 Or alternatively, the firm may induce some uncertainty. For example, the firm informs all consumers who are close to, or have just earned a reward, that they are enrolled in a lottery for potential miles.

10 Note the implicit requirement that the randomness $w$ has full support in the region $(\bar{z} - h, \bar{z} + h)$ of the cutoff. For instance, suppose the casino never changes its reward rules, and hence, those to the right of the cutoff (individual 2 in the example above) can never forfeit their earned reward. In this case $w$ has only positive support for individuals 3 and 4, and the distribution of $\tilde{z}_2$ will be truncated below at $\bar{z}$. This generates a discontinuity in the distribution of $\tilde{z}$ across all individuals at $\bar{z}$, and RD is invalid. Continuity of the density of $w$ is also important. Obviously, if the density of $w$ is discontinuous, the required smoothing is not achieved.
manipulated score $\tilde{z}_1$ for individual 1 implied by the modified score determination rule in Equation (11). We can think of the distribution of $\tilde{z}_1$ induced by Equation (11) as the following mixing distribution,

$$
\tilde{z}_1 = \begin{cases} 
z_1 + m + w & \text{w.p. } \Pr (y = 1|z_1 < \tilde{z}, x_1) \\
z_1 + w & \text{w.p. } \Pr (y = 0|z_1 < \tilde{z}, x_1)
\end{cases}
$$

(12)

Thus, the distribution of $\tilde{z}_1$ is obtained by taking a weighted average of the PDF of $w$, evaluated at the translated location parameters $z_1 + m$ or $z_1$, and weighted by the probabilities that $y = 1$ or 0, given that $z_1 < \tilde{z}$. If one observes individual 1 in repeated trials, his manipulated score will accumulate mass to the right of $\tilde{z}$ as long as $\Pr (y = 1|z_1 < \tilde{z}, x_1) > 0$. However, due to the additional density of $w$, this distribution will be smooth. The distribution of the score for individual 2 to the right of $\tilde{z}$, will analogously be determined as in Equation (12), except the weighting probabilities are evaluated at the right of the cutoff, e.g., $\Pr (y = 1|z_2 \geq \tilde{z})$. The implication of the additional source of uncertainty can now be clarified. We see that randomness in $w$ makes the distribution of $\tilde{z}$ smooth at $\tilde{z}$ for every individual. The distribution across individuals is a weighted average of the distribution for each individual. Because the distribution for every individual is smooth and continuous at $\tilde{z}$, the distribution of $\tilde{z}$ across all individuals will also be smooth and continuous at $\tilde{z}$. Continuity implies the distribution of types just to the left and right of $\tilde{z} = \tilde{z}$ is the same, and hence it is as if there is local randomization at the cutoff. Consequently, the RD design is now valid. To the extent that such randomness is plausible in many contexts, RD designs maybe considered very similar to quasi-randomized experiments (see for e.g., Lee and Lemieux 2009). Note that the validity of the RD depends crucially on a structural element of the model, i.e., the beliefs of consumers about the score.

**Uncertainty About the Cutoff** While an astute customer may occasionally be able to perfectly know their score, it is much less likely that a customer knows the cutoffs in a given database marketing program. In this case consumers may have a continuous belief distribution over the cutoffs. We now consider whether uncertainty about the exact cutoff at which rewards may be earned may be enough to ensure the validity of the RD design even in situations where consumers observe their score perfectly. Analyzing this aspect is more complicated, as it requires us to be more explicit about the choice-specific value functions that generate the function $f(m, z, R)$ in Equation (10).

We start with the basic model without other sources of randomness (i.e. no $w$) i.e. $\tilde{z} = z + my$. 19
To define a customer’s selection/plan decision, we begin by specifying 
\( f(m, z, R) = \mathcal{V}_1(m, z, R|\beta) - \mathcal{V}_0(m, z, R|\beta) \), where, the choice-specific value functions, \( \{ \mathcal{V}_1, \mathcal{V}_0 \} \), are defined as,

\[
\mathcal{V}_1(m, z, R|\beta) = v_1(R|\beta) + \delta \mathbb{E} \mathcal{V} (\tilde{z}, R|m, z, y = 1) + \varepsilon_1
\]

\[
\mathcal{V}_0(m, z, R|\beta) = 0 + \delta \mathbb{E} \mathcal{V} (\tilde{z}, R|m, z, y = 0) + \varepsilon_0
\]

Here, \( v_1(.) \) is the deterministic component of the per-period value from playing which depends on whether the consumer has a reward or not, and the \( \varepsilon \)-s are stochastic unobservables (to the econometrician) as before. \( \delta \) is the consumer’s discount rate. The expected future value from choosing the action \( y \) is,

\[
\mathbb{E} \mathcal{V} (\tilde{z}, R|m, z, y) = \int \int [\mathcal{I} (\tilde{z} \geq \bar{z}) \max \{ v_1(R|\beta) + \eta_1, \eta_0 \}] \\
+ [\mathcal{I} (\tilde{z} < \bar{z}) \max \{ v_1(0|\beta) + \eta_1, \eta_0 \}] d\mathcal{F}_\eta(\eta_1, \eta_0) dG_{\bar{z}} (\bar{z})
\]

That is, if the consumer is able to cross the required cutoff by choosing \( y \) today, \( \mathcal{I} (\tilde{z} \geq \bar{z}) = 1 \), and he will have the reward \( R \) and \( \tilde{z} = z + my \) tomorrow. If he chooses to play tomorrow, he obtains payoff \( v_1 (R|\beta) + \eta_1 \); otherwise, he obtains only \( \eta_0 \). The value from the best possible action tomorrow conditional on earning the reward is the maximum of these two payoffs. If on the other hand, he is unable to cross the cutoff by selecting to play today, \( \mathcal{I} (\tilde{z} < \bar{z}) = 1 \), the value from the best action tomorrow is analogously the maximum of the two payoffs, but evaluated at \( R = 0 \). However, \( \bar{z} \) and \( \eta = (\eta_1, \eta_0) \) are unknown at the time of selection. Hence, the future value involves integrating out \( \bar{z} \) and \( \eta \) over the consumer’s beliefs over these variables. In Equation (13) the consumer’s beliefs over the cutoff \( \bar{z} \) is represented by a continuous density, \( G_{\bar{z}} (\bar{z}) \), and his beliefs over the random shocks \( \eta \) by the density \( \mathcal{F}_\eta(\eta_1, \eta_0) \). Moving the integration into the brackets, and noting that \( \Pr (\bar{z} \geq \tilde{z}|m, z, y) \equiv G_{\bar{z}} (\bar{z}) \), we can write Equation (13) as,

\[
\mathbb{E} \mathcal{V} (\tilde{z}, R|m, z, y) = G_{\bar{z}} (\bar{z}) \mathbb{E}_\eta [\max \{ v_1(R|\beta) + \eta_1, \eta_0 \}] \\
+ [1 - G_{\bar{z}} (\bar{z})] \mathbb{E}_\eta [\max \{ v_1(0|\beta) + \eta_1, \eta_0 \}]
\]

where \( G_{\bar{z}} (\bar{z}) \) represents the cumulative density function of the consumer’s beliefs about \( \bar{z} \).

With some abuse of notation, let, \( \Omega (z + m, z|R, \beta) = \mathbb{E} \mathcal{V} (\tilde{z}, R|m, z, y = 1) - \mathbb{E} \mathcal{V} (\tilde{z}, R|m, z, y = 0) \), the relative expected future value from selection versus not. We can evaluate Equation (14) at \( y = \)
(1, 0)\textsuperscript{11} to obtain,

\begin{equation}
\Omega (z + m, z| R, \beta) = [G_{\bar{z}} (z + m) - G_{\bar{z}} (z)]
\times \left[ \mathbb{E}_\eta \max \{v_1 (R|\beta) + \eta_1, \eta_0\} - \mathbb{E}_\eta \left[ \max \{v_1 (0|\beta) + \eta_1, \eta_0\} \right] \right]
\end{equation}

Intuitively, Equation (15) implies that the future component of the incentive to select/purchase is the difference in expected utility under treatment and not, weighted by the increase in the probability of receiving treatment that is due to adding \( m \) to the score, i.e. \( G_{\bar{z}} (z + m) - G_{\bar{z}} (z) \). The decision to select is given now as,

\begin{equation}
\Pr (y = 1) = \Pr (v_1 (R|\beta) + \delta \Omega (z + m, z| R, \beta) + \varepsilon_1 - \varepsilon_0 > 0)
\end{equation}

We can represent the distribution induced by this type of selection on the manipulated score \( \tilde{z} \) as,

\begin{equation}
\tilde{z} = \begin{cases}
    z + m & \text{w.p. } \Pr (y = 1|m, z, R) \\
    z & \text{w.p. } \Pr (y = 0|m, z, R)
\end{cases}
\end{equation}

**Proposition 2** The distribution of \( \tilde{z} \) is continuous at the true cutoff. Hence, the RD is valid.

**Proof.** First, fix the value of the cutoff actually used by the firm at \( \bar{z} \). Now note that the density of \( \tilde{z} \) will be continuous at the true cutoff \( \bar{z} \) if the mass of \( \tilde{z} \) that piles up to the left and right of \( \bar{z} \) due to selection is the same. This will be the case if, (1) \( \Pr (y = 1|m, z, R) \) is the same just to the left and to the right of \( z = \bar{z} - m \), and, (2) \( \Pr (y = 0|m, z, R) \) is the same just to the left and to the right of \( z = \bar{z} \).

Note from Equation 15 above that \( z \) affects the probabilities only through the difference in cumulative densities, \( [G_{\bar{z}} (z + m) - G_{\bar{z}} (z)] \). Hence, (1) and (2) will be satisfied if the limit of \( [G_{\bar{z}} (z + m) - G_{\bar{z}} (z)] \) from the left and the right is the same at \( z = \bar{z} - m \), and \( z = \bar{z} \). To see that this is the case, note that \( G_{\bar{z}} (z) \) is the marginal c.d.f. of the random variable \( \bar{z} \) evaluated at the value \( z \), prior to selection. As consumers do not know the true \( \bar{z} \), this function is continuous at all \( z \) (by the primitive assumption), including at \( z = \bar{z} - m \), and \( z = \bar{z} \). The difference between two continuous functions is also continuous. Hence, \( [G_{\bar{z}} (z + m) - G_{\bar{z}} (z)] \) is also continuous. Q.E.D. \( \blacksquare \)

It is interesting to contrast this with the situation where the true cutoff is known. In the typical frequency reward program, \( G_{\bar{z}} \) has mass 1 at the true value of \( \bar{z} \), because the firm communicates cutoffs

\textsuperscript{11}Remember that at \( y = 0 \), \( \tilde{z} = z \) and at \( y = 1 \), \( \tilde{z} = z + m \)
to customers to incentivize purchase. In this case, note that,
\[
\lim_{z \to \bar{z}^{-}} G_{z} (z + m) = 1 \quad \text{and} \quad \lim_{z \to \bar{z}^{-}} G_{z} (z) = 0
\] (18)
because selection moves a customer from no-treatment to treatment with perfect certainty if he moves
the score by \(m\) from below the known cutoff (contrast this with the case when the cutoff is unknown,
where this cannot be known for sure). From the right of \(\bar{z}\), we have that,
\[
\lim_{z \to \bar{z}^{+}} G_{z} (z + m) = 1 \quad \text{and} \quad \lim_{z \to \bar{z}^{+}} G_{z} (z) = 1
\] (19)
as the consumer on the right gets the reward for sure. Hence, with a known cutoff, \(G_{z} (z + m) - G_{z} (z)\)
jumps from 1 to 0, as one moves from the left to the right of \(\bar{z}\). Essentially, the smoothing generated
by the continuity of the density \(G_{z} (.)\) is lost.

This intuition is depicted in Figure 2, which is analogous to Figure 1 that depicted the situation
with a known cutoff. In this figure, we assume for the sake of simplicity, that \(z\) is uniformly distributed
and that the uncertainty about the cutoff also has a uniform distribution. In other words, the true
cutoff \(\tilde{\bar{z}} = \bar{z} + w\), where \(w \sim Uniform[-\bar{w}, \bar{w}]\). Consumers do not know what \(w\) is but know its
distribution. Figure 2 shows that the induced distribution of \(\tilde{z}\) is smooth, thereby ensuring the validity
of the RD.

Uncertainty about \(m\) Another form of uncertainty could be uncertainty about \(m\), or in other words,
the effect of the selection decision on the score. We discuss this situation in Appendix 2 for the purpose
of keeping this brief, but we demonstrate there that this kind of uncertainty is insufficient to make
the RD design valid in the presence of self-selection. This underscores the importance of considering
the specific nature of uncertainty and its implications on consumer behavior to assess if uncertainty
or randomness more generally can resolve the identification issues in an RD context in the presence of
selection.

4.1.3 What is the composition of agents for whom treatment is measured?

While the identification conditions laid out in Section 2 define when a treatment effect can be measured
for those agents clustered around the cutoff, the composition of those agents can be very difficult for
researchers to know. Consider the previous example of uncertainty about the cutoff as depicted in Figure 2. Graphically, we depicted the types of agents who move to a neighborhood of the cutoff as a mix of A/B and C/D. However, we cannot characterize who A/B or C/D types are, or know the relative proportion of A/B vs. C/D consumers without actually writing down the true structural model and belief structure, $G_{z}(\cdot)$, of each agent-type. Beliefs are well-known to be elusive to researchers. Therefore, we have a treatment effect that is averaged across a sample of individuals; we cannot hone in on the specific sub-population for whom this treatment effect is relevant without further assumptions. While this challenge is not solvable without a structural model and the elusive belief data, we suggest using applications where there are many cutoffs to learn about the distribution of treatment effects across types. Our empirical application of casino promotions includes five separate cutoffs, providing us the ability to find the treatment effect of casino promotions for consumers of different profiles, albeit locally at these cutoffs.
Discussion The above analysis documents that traditional reward programs where the cutoffs are communicated to customers are not valid RD applications. However, targeted marketing based on purchase histories in which there exists uncertainty about the program, the scores or cutoffs are viable RD applications, as long as consumers have a continuous distribution of beliefs about the uncertainty. Uncertainty about the value of rewards does not restore the validity of the design. In our casino application below, identification of the RD estimator is provided by both uncertainty about the score as well as that about the cutoff. Customers are unlikely to know their score because their past gaming behavior is affected by their luck and the casino’s algorithm for adjusting their expected worth for luck. This score was not communicated to consumers either. Further, the program involves offers sent to customers based on cutoffs that were not provided to the public. Customers may not even know that they would get these offers, or that they differ from other customers in the nature of the offers they receive, but even if they suspected this, they would not know what cutoff the firm used for determining preferential treatment.

4.2 Targeted Promotions Emailed by a Casino

In this section, we apply the regression discontinuity approach to assess a casino’s database marketing program. Two reasons make our application to marketing activity at casinos compelling. First, targeted marketing is an important component of customer management at casinos (Lal and Carrolo, 2001). Second, casino-based applications are particularly data-rich and thus well suited for application of nonparametric methods. Casinos typically send offers to casinos either by direct mail or email, offering packages that include discounted room rates, show tickets, dining credits, and promotional credits. Consumers are classified as “low-rollers” and “high-rollers”, with more lucrative offers to the high-rollers. Importantly, the classification of consumers into low or high-rollers, and the subsequent allocation of promotional offers to these tiers is determined on the basis of a one dimensional, continuous score, referred to as the “Average Daily Win” (henceforth ADW). The ADW is the casino’s best estimate of the average revenue the casino can earn from the customer per day of his visit, after controlling for luck. We have access to the ADW for all customers in the data, but not the proprietary algorithm

\[12\] The control for luck incorporates the role of randomness in past play behavior. For example, two identical customers willing to spend $100 may actually spend very different amounts during the day if one customer won $1000 on their first
that generated the ADW. In addition, we also have access to the cutoff of ADW on the basis of which customers are sorted into tiers. While consumers are aware that more play will move them into higher tiers and earn them more comps, they are unaware of the exact definition of the ADW, or the ADW-specific cutoffs that generate sorting into tiers. This aspect mitigates selection concerns in this application. In Table 2 we provide an example of a casino mailing to 79,419 customers in which the offer depends on the casino’s calculation of the average daily win (ADW) they expect from the customer.\(^{13}\)

Table 2: Example of Tiered Email Promotion Sent by the Casino

<table>
<thead>
<tr>
<th>Tier</th>
<th>Minimum ADW</th>
<th>Maximum ADW</th>
<th>Price of Standard Room</th>
<th>Price of Room on Value Day</th>
<th>Show Tickets</th>
<th>Dining Credits</th>
<th>Promotional Credits</th>
<th>Individuals Mailed</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>$1,000</td>
<td>$2,500</td>
<td>$0</td>
<td>$0</td>
<td>2 free</td>
<td>100</td>
<td>500</td>
<td>3,600</td>
</tr>
<tr>
<td>1</td>
<td>$500</td>
<td>$999</td>
<td>$0</td>
<td>$0</td>
<td>2 free</td>
<td>50</td>
<td>300</td>
<td>8,975</td>
</tr>
<tr>
<td>2</td>
<td>$300</td>
<td>$499</td>
<td>$0</td>
<td>$0</td>
<td>2 for $22</td>
<td>50</td>
<td>50</td>
<td>12,848</td>
</tr>
<tr>
<td>3</td>
<td>$200</td>
<td>$299</td>
<td>$99</td>
<td>$0</td>
<td>2 for $22</td>
<td>25</td>
<td>50</td>
<td>12,249</td>
</tr>
<tr>
<td>4</td>
<td>$100</td>
<td>$199</td>
<td>$159</td>
<td>$79</td>
<td>2 for $22</td>
<td>25</td>
<td>50</td>
<td>23,116</td>
</tr>
<tr>
<td>5</td>
<td>$50</td>
<td>$99</td>
<td>$179</td>
<td>$99</td>
<td>2 for $22</td>
<td>25</td>
<td>25</td>
<td>18,631</td>
</tr>
</tbody>
</table>

We observe four such mailings between January and September 2006. All mailings are identical except in terms of the show ticket offerings, which vary across mailings. Further, the first mailing is only sent to the top two tiers. Importantly for the subsequent analysis, the pattern of higher tiers obtaining superior offers reflected in Table 2 holds across mailings (i.e., tiers 0 and 1 receive one offer and tiers 2 through 5 receive a different offer that is inferior to the one received by the top two tiers). This systematic targeting is one reason why comps and subsequent play will be positively correlated.

We wish to measure the causal effect of comps and promotions. We consider two outcome variables that are relevant to the casino, viz. whether the customer visited the casino (Trip) and the casino’s expected win from the customer (Theoretical Win). The theoretical win is similar to ADW in that it adjusts for the customers luck. It differs in that it recalculates the spending on a given occasion as opposed to providing a measure of the expected spending on any given day. These are summarized, by tier, in Table 3. We see from Table 3 that customers in the top two tiers arrive with about 23% play and the other lost all $100 on their first play.

\(^{13}\)Note that the highest rollers, customers with ADW above $2,500, are not included, as the casino deals with such customers on a one-to-one basis. Customers with ADW less than $50 are sent emails but do not receive special offers such as discounted rooms or credits.
<table>
<thead>
<tr>
<th>Trip</th>
<th>ADW Range</th>
<th>N</th>
<th>Mean</th>
<th>Std. Dev.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Tier 0</td>
<td>$1,000 to $2,500</td>
<td>18,236</td>
<td>0.229</td>
<td>0.420</td>
</tr>
<tr>
<td>Tier 1</td>
<td>$500 to $999</td>
<td>45,219</td>
<td>0.171</td>
<td>0.377</td>
</tr>
<tr>
<td>Tier 2</td>
<td>$300 to $499</td>
<td>48,165</td>
<td>0.126</td>
<td>0.332</td>
</tr>
<tr>
<td>Tier 3</td>
<td>$200 to $299</td>
<td>43,932</td>
<td>0.083</td>
<td>0.276</td>
</tr>
<tr>
<td>Tier 4</td>
<td>$100 to $199</td>
<td>90,747</td>
<td>0.075</td>
<td>0.263</td>
</tr>
<tr>
<td>Tier 5</td>
<td>$50 to $99</td>
<td>83,355</td>
<td>0.075</td>
<td>0.263</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Theoretical Win</th>
<th>Tier 0</th>
<th>Tier 1</th>
<th>Tier 2</th>
<th>Tier 3</th>
<th>Tier 4</th>
<th>Tier 5</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$1,000 to $2,500</td>
<td>$500 to $999</td>
<td>$300 to $499</td>
<td>$200 to $299</td>
<td>$100 to $199</td>
<td>$50 to $99</td>
</tr>
<tr>
<td></td>
<td>18,236</td>
<td>45,219</td>
<td>48,165</td>
<td>43,932</td>
<td>48,165</td>
<td>83,355</td>
</tr>
<tr>
<td></td>
<td>$616.70</td>
<td>$328.17</td>
<td>$135.52</td>
<td>$64.11</td>
<td>$64.11</td>
<td>$10.08</td>
</tr>
<tr>
<td></td>
<td>$2,576.50</td>
<td>$997.59</td>
<td>$514.04</td>
<td>$295.45</td>
<td>$295.45</td>
<td>$69.86</td>
</tr>
</tbody>
</table>

Table 3: Summary Statistics of Casino Outcomes by Tier

<table>
<thead>
<tr>
<th>Changes in promotions when moving between tiers</th>
<th>5 to 4</th>
<th>4 to 3</th>
<th>3 to 2</th>
<th>2 to 1</th>
<th>1 to 0</th>
</tr>
</thead>
<tbody>
<tr>
<td>ADW Cutoff</td>
<td>$100</td>
<td>$200</td>
<td>$300</td>
<td>$500</td>
<td>$1,000</td>
</tr>
<tr>
<td>Promotion Categories</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Regular Room</td>
<td>$179 to $159</td>
<td>$159 to $99</td>
<td>$99 to Free</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Room on Value Day</td>
<td>$99 to $79</td>
<td>$79 to Free</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Show Tickets</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>B to A</td>
<td>-</td>
</tr>
<tr>
<td>Dining Credits</td>
<td>-</td>
<td>-</td>
<td>$25 to $50</td>
<td>-</td>
<td>$50 to $100</td>
</tr>
<tr>
<td>Promo Credits</td>
<td>$25 to $50</td>
<td>-</td>
<td>-</td>
<td>$50 to $300</td>
<td>$300 to $500</td>
</tr>
<tr>
<td>OLS</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Visit</td>
<td>0.008***</td>
<td>0.05***</td>
<td>0.09***</td>
<td>0.15***</td>
<td>0.15***</td>
</tr>
<tr>
<td>Theoretical Win</td>
<td>10.99**</td>
<td>53.01***</td>
<td>124.16***</td>
<td>196.67***</td>
<td>487.34***</td>
</tr>
</tbody>
</table>

Table 4: OLS Regressions of Casino Outcomes on Tier

probability, while customers in the bottom tier only arrive with 7.5% probability. There are also substantial differences in spending by tier, with the bottom tier having theoretical wins averaging only $10 while customers in the top tier have theoretical wins that average $617.

4.2.1 Analysis: Correlational Effects

One obvious pattern from Table 3 is that both outcome variables are increasing in the tiers. A pure correlational analysis that does not control for this targeting rule would pick up this positive correlation and falsely infer it as an effect of the promotion. As a benchmark, we start by regressing the outcome variables on tier fixed effects, which implicitly capture the effect of changing a promotion from the base tier to those for that tier. We pool observations across all four mailings while including period fixed effects to account for differences across the timing of mailings. Table 4 presents the results.
The OLS estimates with visit and theoretical win as the dependent variables are listed in the last two rows of Table 4. The visit variable is coded as 1 in case of a visit and 0 otherwise.\textsuperscript{14} To obtain unconditional (of visit) effects, the Theo variable for a customer who does not visit is included as a 0 in the regression. For ease of interpretation, the incremental difference in the actual promotions when going from tier $\tau$ to $(\tau + 1)$ are also listed in the top panel. Looking at Table 4, it would appear that the promotions have strongly positive effects, with higher tiers having higher visit probabilities and higher theoretical wins. Moving to a non-linear model would not fundamentally alter these results as they are driven by the underlying correlations between the tiers and the two dependent variables.

### 4.2.2 Analysis: Causal Effects

The OLS estimates reported in Table 4 are problematic due to two reasons. First, the estimation does not control for the targeting employed by the casino, thereby overstating the effect of the promotion. This induces a classic endogeneity bias into measurement. Second, by imposing a (linear) functional form to hold globally across all types of customers and tiers, the estimator leverages the information from all of the observations to learn about the promotional effects, despite the fact that the variation in the offers really only occurs at ADW levels of $100, $200, $300, $500, and $1,000. This results in misspecification bias of an unknown form. We address both issues with the RD estimator. Table 5 presents estimates of the RD estimator applied to these data. As in the previous analysis, we pool across all four mailings to estimate the treatment effects using the RD design, while controlling for period fixed effects.\textsuperscript{15} Following the suggestion of Imbens and Lemieux (2008), we estimate these nonparametrically using a rectangular kernel, using observations within a bandwidth of size $h$ on either side of each cutoff. Because there are six different tiers, and five different ADW cutoffs, we estimate five different regression discontinuity specifications focusing on each tier cutoff. In practice, this turns out to be least squares estimators using only observations lying in the neighborhood defined by the bandwidth on either side of each of the cutoffs. An advantage of this approach is that the

\textsuperscript{14} We choose OLS to illustrate the pitfalls of not accounting for the endogeneity. More sophisticated statistical specifications (e.g. count models, or discrete link functions) will not change the message from this analysis, as long as they interpret the full correlation of outcomes and tier-status as effects of the promotion.

\textsuperscript{15} As Lee and Lemieux (2009) notes, the panel aspect of the mailings does not add anything to the identification of the RD estimator, except to reduce the variance of the estimate. Further, we do not expect the repeated treatment of individuals to affect the estimation because we expect that the firm has accounted for past promotion/treatment, when calculating ADW.
standard errors of the rectangular kernel are the same as robust standard errors available for least squares. We also estimated the RD using local linear regression (not reported), and found the results to be similar. As in all nonparametric applications, an important aspect of the estimation is correct choice of the bandwidth. We choose the band-width by cross-validation. We conduct a search for the band-width of ADW that minimizes the mean square error across the five cutoffs. We also present the estimates for an arbitrarily chosen bandwidth of $20.

Referring to Table 5, we see that effects of the marketing program on visit probabilities are all insignificant except for the transition from tier 2 to tier 1 (ADW = $500) and 4 to 5 (ADW = $100). However, both these negative effects are not robust to changes in the size of the bandwidth. We therefore conclude that the casino offers are neither increasing nor decreasing the probability that a customer visits the casino. This is surprising because there is substantial variation in the price of a room. However, elasticities may be low because of other substantial costs of visiting a casino in Las Vegas or because the room is only a small part of the expenses of these gamblers.

We see similar patterns when analyzing the theoretical win of the customers. Most notably, most of the effects are negative or insignificant. A negative effect is plausible, as provision of dining credits and show tickets can draw customers away from the slots and the gambling tables. We conjecture these credits may substitute for actual gaming. Using a band-width of $20 for the ADW, we see there is a positive, but insignificant, jump in theoretical win at ADW = $500. But under the optimal
band-width, which is smaller, we find that evidence reverses: there is a large negative effect of the increased promotions when moving from Tier 2 to Tier 1. This reversal also underscores importance of considering robustness to choices of band-width, and the dangers of pooling data across very dissimilar observations. To reiterate the point, we present a plot of theoretical win at ADW = $1,000, where there is a change of tiers from 1 to 0.

Overall, while the general relationship between theoretical win and ADW is positive, in the limit exploring the variation in a neighborhood of the discontinuities, the effects are either not significant or negative. One interpretation of the results is that comps and hotel credits are not significant drivers of consumer’s decisions to visit or to play at the casino, but competition forces the casino to continue marketing in spite of the low returns. Alternatively, comps affect long-run considerations like building loyalty to the casino, even though in the short-run, they do little to shift visits or play. The other interpretation of the results is that they provide some evidence of short-term inefficient marketing decision making at the firm. The bottom-line is that the analysis reveals that the marketing-program we analyzed at the casino is not working: the comps are either ineffective or losing money. This is a significantly different conclusion from the previous correlational analysis. Nevertheless, it is possible that such an ineffective program may be a necessary competitive response to other casinos that offer similar programs. Our analysis can only speak to the profitability of the parameters of the program, as opposed to the need for the program as whole.

4.3 Discussion

The RD approach in our two applications revealed several null, or negative effects. The reader should not infer this to imply that the approach will yield null effects always; the right inference is that much of the observed positive correlation in the data in both contexts is due to the targeting rule, and not due to the marketing activity. It is important to point out that null effects for the direct-mail company (and the casino) are not necessarily bad. A null effect at a cutoff defining a tier implies the firm can do just as well in terms of marketing by merging the adjacent tiers together. Similarly, large differences in effects across various cutoffs imply the firm may want to consider a finer classification of consumers into tiers on the basis of the scoring variable. An interesting open question is what the optimal score
should be. It is unlikely that researchers will be able to answer this question credibly unless they have detailed data on the constraints or information asymmetries that forced firms to sort consumers on the basis of the observed score in the first place, rather than on the basis of their full range of characteristics and history. Some progress has been made in limited contexts. For instance, Huang (2009) shows that when willingness-to-pay is log-normally distributed, the optimal sorting score may be determined on the basis of their explanatory power in regressions of willingness-to-pay. But this remains an open question for future research.

5 Purchase Timing as a Score Variable

RD naturally extends to situations where the score variable is, or relates to, time. An RD with time as a score is a “Before-After” design. “Before-After” contexts are typically plagued by the concern that outcomes in the “after” period arise due to time-varying unobservables that are unrelated to the treatment. The RD design is able to address this concern because these time varying unobservables are unlikely to drop discontinuously at the exact moment of the treatment. Hence, the discontinuity in outcomes post the treatment is not due to these unobservables. Researchers in Marketing and Industrial Organization have used RD with time as a score to analyze causal effects of price promotions (Busse et. al., 2006; Busse et. al. 2010; Fong et. al. 2010); and service guarantees (Chen et. al. 2009).

We discuss two aspects of the design that are relevant to time-based RD-s. The first relates the nature of the outcome being measured, and the second, to demand-side effects flowing from the type of product considered.

Nature of Outcome being Measured To understand this point, consider analyzing data from an initial decision and then a conditional decision by consumers. Assume a continuous distribution of customers over time, \( f(z) \) (the top panel in Figure 3), where \( z \) denotes time. Let the first decision be indicated by \( y_z = I(g(R_z|\theta) + \varepsilon_z > 0) \). To fix ideas, call it the store visit decision. \( R_z \) denotes whether there is an increased incentive (e.g. a price discount for some product at the store) that makes customers more likely to visit at time \( z \). \( g(R_z|\theta) \) is the non-stochastic portion of a customer’s payoff for this choice. This payoff is similar to Equation (10) except that time is the score. \( \theta \) includes
parameters for the store choice decision as well as parameters for any conditional decisions, such as purchase. $\varepsilon_z$ is an unobservable (to the researcher) that affects the store visit decision. The potential for RD arises when the incentive, $R_z$, discontinuously changes at $z = \bar{z}$. Such a discontinuity in the incentive induces a discontinuity in the probability of the store visit as depicted in the second panel of the figure. The treatment effect of the change in $R_z$ on the store visit decision is identified by RD because the distribution of the score, $f(z)$, is continuous at the cutoff.

Next, consider any outcome conditional on having visited the store, such as whether or not to purchase. In terms of empirical implementation, this would entail analyzing data only on those consumers who visited the store. The RD estimator in this case will analyze the purchase decisions of only those customers that entered the store just before and after the price change, i.e. consumers for whom $g(R_z|\theta) + \varepsilon_z > 0$ for $z = \bar{z} \pm h$, where $h$ is a small bandwidth of time. The manipulated score in this case is the timing of the store visit, $\tilde{z}$. Together, the distribution of the initial score (top panel) and the store visit probability (second panel) generate a manipulated score, $f(\tilde{z})$, (bottom panel). The distribution of this manipulated score is discontinuous because $g(R_z|\theta)$ changes discontinuously and consequently, $Pr[g(R_z|\theta) + \varepsilon_z > 0]$, and $f(\tilde{z})$ also become discontinuous. As shown before, a discontinuity in the score invalidates RD. When measuring treatment effects on an outcome conditional on visit, the only case in which RD with time as a score is valid is when all consumers visit the store with no precise knowledge of whether the promotion is in effect or not - both before and after the promotion begins. In such a case, the first (visit) decision is unaffected by the promotion and RD is valid.

Finally, to close the discussion, consider estimating the effect of the promotion on the joint outcome of visiting the store and purchasing. In terms of empirical implementation, this would entail analyzing the demand data of all consumers who could potentially visit the store (i.e., the demand of those who did not visit should be included as a zero in the estimation data set). The relevant score is now time itself, and the RD validly measures the effect of the promotion on demand. The key insight here is that what is being measured matters: RD designs work when the outcomes are unconditional on timing decisions by consumers, but may not when analyzing outcomes conditional on a timing chosen by customers, depending on the nature of consumer beliefs.
Nature of Product and Dynamics  The second aspect to consider is the nature of the product for which we measure promotion effects. Busse et al. (2006) point out that if the good is durable or storable, forward-looking and price sensitive consumers will tend to queue up after previous promotions as in a Sobel-style model (Sobel 1984). The promotion occurs at time $\bar{z}$. At time $\bar{z} - h$, only those customers who cannot wait for the next promotion, visit the store (and make purchase decisions). However, at $\bar{z} + h$, all customers who would have potentially bought since the past promotion visit. Revisiting our analysis, the store visit effect is identified. However, the purchase decision conditional on visiting at $\bar{z} - h$ vs. $\bar{z} + h$, compares two different types of customers: price-insensitive or impatient consumers who buy prior to promotion with price-sensitive or patient consumers who wait for the promotion. More generally, forward-looking behavior and expectations accentuate issues related to dynamic selection. Assuaging dynamic effects is important to validate time-based RD designs.

We stated above that RD estimates a valid treatment effect when time itself is the score, but not when the score is a timing chosen by the customer. However, a vexing issue is that both cases measure effects that are relevant to a sub-population selected by dynamic considerations: the Rd's treatment effect is a function of the distribution of the state variables for the consumers who visit. The consumer’s states are a function of the firm’s past actions, which makes comparing the estimated treatment effects across studies difficult, unless the firm’s history is held constant across studies. For instance, in a storable good model, the key state affecting purchases is inventory, which is a function of past promotions. Fong et. al. (2010) find that the RD estimand over time using historical data on promotions does not perform well compared to a randomized experiment in which base-prices across products and stores are changed for storable, grocery goods. The RD application is valid because time itself is the score, not a timing chosen by customers. But, the measured treatment effects are a function of inventories realized at the time of the temporary price changes (for the RD), or the base-price change (for the experiment). For the random experiment and the RD to yield the same results, the researcher would have to ensure the firm’s promotion history prior to both the RD and the experiment are the same. This is difficult to ensure in practice. Of course, this is the case for comparing any two estimators with state-dependent treatment effects.
Figure 3: Time as a score: The top panel depicts a continuous distribution of customers over time, $f(z)$. Comparing A and B at $\bar{z}$, we see that continuity is maintained at the cutoff. The second panel depicts how a discontinuous change in an incentive $R_z$ at $\bar{z}$ results in a discontinuous change in the probability of an initial decision (e.g. a store visit). RD can validly estimate any such effect because of the continuous distribution of the score, $f(z)$. But, RD is invalid when estimating any outcome conditional on the initial decision, because the score then becomes the timing of the first decision, $\tilde{z}$. We see in the bottom panel that the manipulated score (a combination of the first two panels) changes discontinuously from A to B at $\tilde{z}$.

Discussion Overall, our discussion has highlighted the different aspects an analyst contemplating an RD application in a targeted marketing context needs to consider in order to ensure the design is valid. Even though the RD design fits into the “reduced-form causal effect” literature, we believe its validity in Marketing situations cannot be established without specifying a clearly-articulated structural model of behavior. Our detailed analysis underscores the importance of formally specifying consumer’s information sets and incentives to establish the validity or lack thereof, of an RD-based analysis. Furthermore, we have illustrated in both purchase history targeting and time as a score that even when the RD estimates a valid treatment effect, it can easily be for an unknown population whose selection is determined by how state variables and beliefs pass their way through a structural model.

6 Conclusions

This paper illustrates the use of regression discontinuity techniques in targeted marketing and Industrial Organization applications. To the best of our knowledge, we are unaware of other targeted marketing applications that have exploited the identification enabled by the rules-of-thumb and heuris-
tics pervasive in marketing practice. These heuristics had previously been thought of as a “nuisance” issue that had to be dealt with in estimation by researchers, or as evidence of inefficient marketing decision-making by firms. Here we show that the heuristics actually aid estimation by facilitating identification, and are also useful to firms as they enable credible measurement of the return-on-investment on their marketing spends. We illustrate the approach using two empirical applications. Our quasi-experimental approach reveals negative effects of marketing mix variables that are not easily uncovered otherwise.

We expect our approach to controlling for the endogeneity to be used in conjunction with other approaches for understanding demand under targeting. Treatment effects obtained via the design may be combined with estimates from structural models to improve or audit results (e.g. Khwaja et al. 2010 for the case of a matching estimator). Further, parametric models of heterogeneous demand can provide the continuous representation of heterogeneity that firms could use to better define cutoffs when using group-level targeting. Better measures of heterogeneity will improve heuristic thumb-rules used for targeting, even if firms are unable to implement the individual-level policies that can be suggested by individual-level models. Parametric methods for solving the endogeneity that enable pooling are also essential in sparse-data situations. In data-rich environments, we hope this paper encourages further exploration of the use of nonparametric methods to facilitate optimal marketing mix allocation.

7 References


Appendix 1: Heterogeneity in Types Does Not Mitigate Selection

Here, we demonstrate graphically why heterogeneity in types does not guarantee validity of the RD design in the presence of selection. Consider a situation where there are two types of consumers, one type with sufficiently high fixed costs, such that they would not satisfy the fixed costs condition in Section 3.1.3 above. The second type of consumers has fixed cost such that a fraction of them with \( z > z^* \) move to the right of the cutoff \( \bar{z} \), with \( z^* \) as defined early. Figure 4 graphically depicts this situation. Consumers of the first type are lightly shaded. None of these consumers select into the treatment group. Type 2 consumers are represented in dark shaded boxes and as can be seen in the picture, a proportion of these consumers select into treatment. This is a situation where the limit of the outcome to the left and right of the cutoff (\( \bar{z} \)) exist. However because some of the consumers of the second type have incentives to select to be on the right of the cutoff, there is a discontinuity of types and consequently a discontinuity in outcomes at the cutoff. Thus, even if there are some consumers of this second type, the conditions for identification of RD are violated.

\[
\tilde{z} = \begin{cases} 
  z + m \times y(m, z, R, \varepsilon) & \text{w.p. } p \\
  z & \text{w.p. } (1-p)
\end{cases}
\]

Consider a consumer to the right of the cutoff. This consumer already has the reward and therefore has no incentive to select to fly. Now consider an individual with a score in the neighborhood of the cutoff and to the left of it. Specifically, this individual has a score,

\[
z_1 \in [\bar{z} - m, \bar{z}]
\]

Appendix 2: Uncertainty About Value of Reward Does Not Mitigate Selection

Following the model sketched in section 4.1.2, we consider a situation where there is some uncertainty in \( m \) itself, i.e. the consumer can choose \( y \), but does not know how much the score will move by his actions. One example may be a casino reward program where the consumer does not know the exact weights via which the casino translates his play at slots, tables and other games into the score used for targeting. Another is of a hypothetical program where consumers who choose to purchase are entered into a lottery to earn points. Thus, \( m \) is zero with some probability and positive with some other probability. A moment’s reflection reveals that this sort of uncertainty does not make the RD design valid, because consumers to the right of the cutoff still have no incentives to gamble in order to earn the reward (they already have the reward). However, as long as \( m \) is positive with some probability, consumers just to the left of the cutoff will have an incentive to select that consumers to the right do not face. This will cause a discontinuity at the cutoff, invalidating the RD design.

To see this formally, consider a situation where \( m \) is discrete. Let the random variable \( m = m(>0) \) with some probability \( p \) and \( m = 0 \) with probability \( (1-p) \). Thus, the manipulated score can be rewritten as,
The manipulated score for this individual has the following distribution induced by Equation 20,

$$\tilde{z}_1 = \begin{cases} 
    z_1 + m & \text{w.p. } p \times \Pr(y = 1|m = m, z_1, R, \varepsilon) \\
    z_1 & \text{w.p. } 1 - [p \times \Pr(y = 1|m = m, z, R, \varepsilon)] 
\end{cases} \tag{21}$$

As long as the conditions described in section 4.1.1 hold, $\Pr(y = 1|m = m, z_1, R, \varepsilon)$ is non-zero. Thus, if there is any probability $p$ that $m$ takes a positive value, the distribution of $\tilde{z}_1$ will accumulate more mass just to the right of the cutoff $\bar{z}$ relative to the left, both because of consumers initially on the right of $\bar{z} - m$ ending up to the right of $\bar{z}$, and consumers initially just to the left of $\bar{z}$ leaving and ending up to the right of $\bar{z}$. This will cause a discontinuity in the distribution of the score, invalidating the RD design.

**Continuous $m$** One might argue that the discrete distribution of $m$ drives the discontinuity in the distribution of the manipulated score. Hence, we now consider a change in the model above in which $m$ has a continuous distribution. Let the pdf of $m$ be $g(\cdot)$. Modifying the discussion for a discrete $m$, the manipulated score for the individual is

$$\tilde{z}_1 = \begin{cases} 
    z_1 + m & \text{w.p. } \Pr(y = 1|m, z_1, R, \varepsilon)g(m) \\
    z_1 & \text{w.p. } \Pr(y = 0|m, z, R, \varepsilon)g(m) 
\end{cases} \tag{22}$$

Once again, as long as $g(\cdot)$ is any continuous density and the conditions described in 4.1.1 hold, the incentives of consumers in the region $[\bar{z} - m, \bar{z})$ to select in order to receive the reward (i.e. $\tilde{z}_1 = z_1 + m$), will be higher than those in regions (1) and (3). This, as discussed earlier, causes a discontinuity in the distribution of the manipulated score $\tilde{z}$, invalidating the RD approach.

**Appendix 3: Some Examples of Potential RD Applications in Marketing**

Regression discontinuity designs could potentially be applied to a variety of marketing contexts. Table 6 gives examples of some potential RD applications in marketing.
<table>
<thead>
<tr>
<th>Industry/Firm</th>
<th>Score</th>
<th>Treatment</th>
<th>Outcome</th>
<th>Discontinuity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cataloging</td>
<td>RFM (Recency, Frequency, Monetary Value)</td>
<td>Targeted catalogs to zip-codes</td>
<td>Purchase, Response</td>
<td>Catalogs are be mailed to households based on cutoffs of RFM scores.</td>
</tr>
<tr>
<td>Credit Cards</td>
<td>FICO (Fair-Issacs) Credit Score</td>
<td>APR, Credit-Lines</td>
<td>Sign-Up for Card, Default</td>
<td>APR-s and Credit Limits are assigned based on cutoffs of FICO Scores.</td>
</tr>
<tr>
<td>Insurance</td>
<td>Company-specific Risk Score</td>
<td>Premiums</td>
<td>Sign-up for policy, Renewal, Satisfaction</td>
<td>Premiums are assigned based on cutoffs of company-specific risk scores that capture driving, health risk.</td>
</tr>
<tr>
<td>Services</td>
<td>Inactive time</td>
<td>New offers, Initiate contact</td>
<td>Whether customer churns</td>
<td>In many services settings, prolonged periods of inactivity by customers may trigger promotions to prevent churn. Examples include value-added account offers by banks. In such settings, the effect of the promotion can be measured by comparing outcomes for those on either side of the discrete cutoffs of inactive time.</td>
</tr>
<tr>
<td>Rentals (e.g. Netflix)</td>
<td>Time since last rental</td>
<td>Promotions</td>
<td>Retention</td>
<td>Free-rental offers by Netflix to inactive customers are based on cutoffs of inactive time</td>
</tr>
<tr>
<td>Banking (e.g. Mint.com)</td>
<td>Amount in savings account</td>
<td>Suggestions for better investment options</td>
<td>Take-up of offer</td>
<td>Personal Financial Aggregator firms offers suggestions for higher-return investment options based on amounts deposited in lower-return options like savings accounts.</td>
</tr>
<tr>
<td>Airlines</td>
<td>Number of “Premier” passengers who arrived</td>
<td>Offer of Economy-Plus seat</td>
<td>Future flying of premier customers, Satisfaction with Mileage Program</td>
<td>Airlines like United upgrade its Premier status fliers to Economy Plus seats, based on availability determined by Economy Plus seats sold and the number of Premiers on the flight. Premiers do not know the cutoffs and cannot self-select into flights.</td>
</tr>
<tr>
<td>Hotels</td>
<td>Current Reward Points</td>
<td>Offer upgrades</td>
<td>Future hotel stays</td>
<td>Upgrades (next-in-class rooms) often offered based on cutoffs of past history/accumulated points</td>
</tr>
</tbody>
</table>

**Table 6: Examples of Potential RD Applications in Marketing**