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Advertising, Promotion, and Reviews:

Three Models to Better Understand

Internet Marketing

A dissertation submitted in partial satisfaction

of the requirements for the degree

Doctor of Philosophy in Management

by

Paul R. Hoban

2014

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ABSTRACT OF THE DISSERTATION

Advertising, Promotion, and Reviews: Three Models to Better Understand Internet Marketing

by

Paul R. Hoban

Doctor of Philosophy in Management

University of California, Los Angeles, 2014

Professor Randolph E. Bucklin, Committee Co-chair

Professor Sanjog R. Misra, Committee Co-chair

I present three essays on online advertising and promotion effectiveness, focusing on how firms make decisions, and how consumer response can be more accurately measured. In the first chapter, I present a cost effective method for measuring online display advertising's impact, net of biases stemming from individual targeting and browsing behavior. I find that the recommended approach can produce dramatically different results from standard correlational measures, and that consumer responses vary greatly with their existing relationship with the firm. In the second two chapters, I examine online daily deals, both in terms of how firms make decisions, and how these promotions impact online reviews. The results indicate that these offers have strong, negative, and temporary effects on a firm's reputation, while offering a significant increase in traffic. Further, firms trade-off between these two when deciding whether or not to offer a daily deal.

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To Allie – Thank you for your continuing love and support, without which the last five years would never have been possible.

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Ch. 1: Assessing the Effects of Internet Display Advertising: Model-Based Insights from a Randomized Field Experiment

1.1 Abstract

In this chapter, we analyze the effects of internet display advertising on website visitation using tracking data from a controlled field experiment. A random subset of individual users was exposed to banner ads for an unrelated charity instead of ads for the company, a financial tools provider. The experiment enables us to control for confounds from individual level targeting algorithms (e.g., users targeted based on likely interests) and browsing behavior (e.g., users browsing more are more likely to visit) and permits both model-free and model-based analysis. The experiment reveals that display advertising affects site visitation for users in some, but not all, stages of the purchase funnel. Next, we estimate a binary logit model of site visit as a function of advertising exposure and we harness the posterior distributions from Bayesian estimation to calculate marginal effects and ad elasticities, also by funnel stage. We find substantial potential value in reallocating display ad impressions across different funnel stages. Effects based on the experimental data also differ significantly from those computed from standard correlational approaches, highlighting implications for ad testing and retargeting strategies.

1.2 Introduction

In 2012, advertisers spent \$7.7 billion dollars on online display advertising, representing 21% of all online ad spending and 13% year-over-year growth (IAB and PricewaterhouseCoopers 2012; IAB and PricewaterhouseCoopers 2013). Each quarter in the US, there are now more than one trillion display ads delivered and nearly 300 individual advertisers spend at least \$1 million (comScore 2011). This is expected to grow in the coming years, with advertisers projected to spend \$11.7 billion on online display advertising by 2015 (eMarketer 2011).

Despite this growth, managers continue to question whether online display advertising truly affects customer behavior. General Motors recently terminated a \$10 million ad spend with Facebook while publicly questioning whether such ads could influence consumer behavior (Terlep, Vranica et al. 2012). Wine.com CEO Rich Bergsund moved his company's entire display ad spend to paid search, affiliate marketing, and comparison shopping engines. His concern was that display ad impressions, "may not convert to customers or sales," (Barr and Gupta 2012). These concerns highlight the need to develop and test reliable methods to establish and measure the links between online display advertising and consumer behavior. This is critical not only for advertisers, but also for the internet companies who depend upon display ad revenue and the online platforms which manage the sale and distribution of display advertising.

Correlating behavior outcomes (e.g., sales, leads, or other intermediate metrics such as site visits) with exposure to display ads, as is done in a number commercially available applications, can be problematic because the users exposed to the ads (versus those who are not) may have had those ads targeted to them based on measures correlated with their propensity to act in certain ways. Similarly, differences in how users browse the web can also make exposure-based

correlates problematic. This is because, other things being equal, users who visit more sites and view more pages may be more likely to have seen a given ad as well as to visit a given web site. Even when ads are not individually targeted but merely placed on different types of sites (e.g., based on context), spurious correlations can occur. For example, if users interested in financial services are more likely to browse sites with financial content, ad placement on this basis will produce biased estimates of the true lift due to display ad exposure. Thus, managers looking to evaluate online display advertising face multiple selection issues, including targeting and browsing behavior, that may bias estimates from common correlational approaches.

The purpose of this paper is to improve the analysis of internet display advertising effects using field experiment data. In particular, we harness the detailed tracking data available to us to examine not only effect sizes and elasticities, but also differences in effects across multiple stages of the purchase funnel (i.e., non-visitor, visitor, authenticated user, and converted customer). In so doing, we are able to provide implications for the allocation of ad impressions across users based on tracking cookie identification of purchase funnel stage.

In what follows, we present model-free and model-based analyses of data from a large scale field experiment conducted by a collaborating firm. This firm sells a set of online financial management tools directly to consumers. Our data provide tracking records of individual consumer exposure to display advertising along with those individuals' browsing behaviors at the firm's website, information that is identical to what is generally available to managers. In the experiment, a small proportion of individuals were randomly assigned to the control group. This group was served ads in precisely the same manner as the treatment group, but the ad copy was for an unrelated charity instead of the focal firm. For the control group, any correlation between display advertising and the outcomes of interest cannot stem from advertising effects, but would

be due to potential confounds such as individual level targeting and browsing behavior. By comparing the advertising response between the treatment and control groups, we are able to estimate the effect of display advertising while controlling for such confounds. From the within site browsing behavior, we identify four key stages in the purchase process. This enables us to explore how advertising's impact may change as individuals move through the purchase funnel.

Based on our analysis of the experimental data, we first provide model-free evidence of display advertising's efficacy. We find that its impact varies by purchase funnel stage, and is positive and significant for three of the four phases. Using an advertising response model, we then show how to uncover marginal effects and elasticities from the data while also incorporating lag effects and holding constant other factors such as timing and seasonality. We compare the model-based results with those which would be found from a simple correlational approach. We find that accounting for targeting and browsing behavior, as the experiment permits us to do, produces significantly different estimates of display ad effectiveness. We also examine the optimal allocation decisions implied by our elasticity estimates and show that they differ dramatically from the allocation actually employed as well as those implied by correlational estimates. An important feature of our approach is that we are able to reveal these differences using a relatively small control group size (1.5% of the total impressions served), an important cost consideration for implementing ad testing.

1.3 Background and Literature

The need for a clear link between online display advertising and a firm's outcomes of interest has not gone unnoticed in the literature. In some of the earliest work on display advertising,

Chatterjee, Hoffman et al. (2003) focused on understanding what drove click through rates. Using data from an online content provider, they found that new visitors and less frequent visitors to the site showed a stronger propensity to click. At about this time, click through rates dropped precipitously, and researchers sought the underlying causes and better metrics of banner ad effectiveness. Dreze and Hussherr (2003) showed that although individuals actively avoid looking at display ads, they still have a positive effect on brand awareness and advertising recall. Cho and Cheon (2004) established perceived goal impediment, the belief that the ad is not relevant to the objective at hand, as the underlying cause of display ad avoidance. This was supported by Danaher and Mullarkey (2003) who found that banner ads have more influence on individuals who are browsing than on those who are performing a goal directed activity.

Rutz and Bucklin (2012) examined and quantified the link between display ad exposure and brand interest. Using data from a third party automotive site, they showed that display ad exposure can influence within site browsing behavior. Specifically, consumers were significantly more likely to seek content on the site related to previously advertised brands than those that were not advertised. In line with previous work, they found that effect sizes varied by browsing behavior, with users who created less focused clickstreams showing a greater response to advertising.

Manchanda, Dubé et al. (2006) moved beyond the classical brand-based measures of ad effectiveness, directly linking banner ad exposure and purchase behavior. Using a semi-parametric hazard model, they found that banner ad exposure had a positive effect on purchase frequency for existing customers. This effect was greatest when consumers viewed a large number of webpages across a variety of websites (i.e., when across site browsing increased).

Lewis, Rao et al. (2011) pointed out that heavy browsers are also more likely to perform a wide variety of online behaviors independent of advertising exposure. Because the probability of ad exposure increases with browsing duration and intensity, unaccounted for correlation between browsing behavior and the outcomes of interest will create a confound (Cameron and Trivedi 2005). For example, an individual who visits 100 websites throughout the course of the day has a far greater chance, *ceteris paribus*, of receiving an ad impression than an individual who visits only 10 websites. In a series of experiments, Lewis et al. show that such individuals are also more likely to perform certain searches, visit a given site, or even sign up for online services. In their application, they find that correlational measures would have led to significant overestimation of display advertising's influence on these behaviors of interest.

In addition to browsing behavior, potential bias also stems from the individually targeted nature of much display advertising. Since the early days of online advertising, managers and researchers have sought to increase ad effectiveness by targeting individuals with relevant browsing and search histories (Sherman and Deighton 2001). In recent years, major ad servers have begun aggregating user histories across services, including email, search, and social networking (Ingram 2012). Because the targeting algorithms used by ad servers are frequently only partially observable to firms (Google 2012), accounting for such targeting is complex. For instance, firms may request that a certain ad be retargeted to prior site visitors, but they may not know how the ad server selects which prior visitors will receive impressions.

While one might assume that targeting would, at worst, have no effect on individual responses, Goldfarb and Tucker (2011) showed that targeting can negatively affect display ad effectiveness when paired with highly visible creative. Using data from a large scale field experiment, they found that display ads that were both obtrusive and targeted had less impact on

purchase intent than those that did either one or the other. They showed that this effect is most pronounced in categories generally considered private (financial products, healthcare, etc.) and for individuals who seem to most value privacy.

In sum, two selection issues may create bias in standard correlational estimates of display advertising effectiveness. First, individuals who browse more webpages are, *ceteris paribus*, more likely to see a given ad and may be more likely to perform a number of activities, such as visiting a site and buying online. Second, individual level targeting may lead to individuals with higher baseline probabilities of site visits being served more impressions. In either case, there would be a positive bias in effect size estimates. The bias, however, need not always be positive. There may exist activities, such as making critical life decisions, that are less likely to be undertaken when users are actively browsing a wide variety of webpages. Similarly, recent work has shown that certain types of advertisements perform worse when heavily targeted, with a pronounced effect in the financial and health arenas. Consequently, the two sources of bias, browsing behavior and targeting, can have opposing effects with unknown magnitude. Thus, it is unclear *a-priori* whether correlational measures will overstate or understate the effect of display advertising in any given application.

Industry researchers are aware of these issues and have developed a number of interesting solutions to handle them. comScore's "Brand Metrix" is likely the most advanced. This product leverages comScore's online panel to compare individuals exposed to a firm's advertising to otherwise similar individuals who were not exposed. Based on nearly 200 brand impact studies using this service, comScore finds that exposed subjects are more likely to search, visit, and purchase from the advertisers. Further, they find that this difference is maintained for at least four weeks following the first exposure (comScore 2008). While this is an impressive approach,

it comes with the requirement that the matching method accurately reflects, and thus controls for, the targeting algorithm and browsing behavior bias.

Because it is likely to be challenging to accurately capture the continuously evolving, increasingly complex, and unknown targeting algorithms used in display ad serving, we seek to develop an approach that avoids this limitation and will provide a more robust foundation for assessing display ad effectiveness.

1.4 Approach

Our proposed approach is based upon the conduct of a controlled field experiment and the model-free and model-based analysis of individual-level tracking data from it. In this way, we build on the long history of using field experiments to evaluate advertising, both in marketing academia and industry. Our approach is analogous to commonly used copy testing experiments, so called “A/B Testing”, with the exception that we use ads for an unrelated charity in place of firm advertising. This allows us to identify a baseline independent of the firm’s message and copy, while still following the same ad-serving (and therefore targeting) methodology.

Our data come from a large scale field experiment in which a small proportion of individuals were randomly assigned to the control group. This group is targeted in precisely the same manner as the treatment group, but is shown ad copy for an unrelated charity in place of firm advertising. Throughout the experiment, ad impressions and site visits are observed at the individual level using tracking cookies. While ad servers commonly provide such individual level tracking data to advertisers, it is generally limited to how otherwise anonymous individuals interacted with the firm’s advertising and website. By leveraging charity advertisements, we are

able to observe individual behaviors that are independent of the firm. Through our combination of random assignment, identical targeting, and individual level tracking, we are able to control for both targeting and browsing behavior biases. In the web appendix, we present further discussion of the antecedents of this problem based on the data generating process.

As an alternative to a controlled experiment, advanced correlational approaches might be used to handle endogeneity due to targeting and browsing. Instrumental variables and control functions use additional covariates to create an orthogonal relationship between the regressors and the error term (Cameron and Trivedi 2005). For targeting, this would require both knowledge of the proprietary targeting algorithms used by ad servers and access to the relevant covariates, neither of which are generally available. In the past, researchers have attempted to control for browsing behavior bias by including the number and variety of other websites an individual is observed to visit (Manchanda, Dubé et al. 2006). However, these counts are only observable when a firm's ad is served during the site visit. This can be an effective control in untargeted campaigns, as was the case for Manchanda et. al (2006). However, the correlation between this measure of browsing behavior and any unaccounted for targeting can introduce an additional source of endogeneity.

In dealing with missing variables and individual level targeting, our problem mirrors that of Manchanda, Rossi et al. (2004), who studied pharmaceutical detailing. They simultaneously estimate targeting and response models while allowing the individual level response parameter to enter the targeting algorithm. Unfortunately, their approach is not a good fit for our application. To handle the unobserved nature of browsing behavior, we would need some exogenous predictor. As mentioned above, such predictors are generally unavailable to the researcher. Second, identification of the parameters in their model requires significant individual level

variation in both targeting and response. While this may be possible for very large campaigns, we found the model to be empirically unidentified in our application.¹

The experiment was set up as follows. During their first digital interaction with the firm (either through ad exposure or site-visit), individuals were randomly assigned to either the treatment or control group. When determining when and to whom a display ad impression should be served, the ad servers did not differentiate between the two groups and the targeting was consistent between them. When selecting the ad copy to be served, the control group was always given copy unrelated to the firm; in our case, these were ads for a large multi-national charity. Thus, any apparent effect of display advertising in the control group represents the combined effect of targeting and browsing behavior. For the treatment group, the effect of display advertising is the joint effect of these factors and the actual effect of online display advertising. Thus, the effect of display advertising on behavior is the difference in the probability of the action between members of the treatment and control groups.

As we will show, the model-free evidence of ad effectiveness will be illuminating, especially as it is broken out across stages of the purchase funnel. Nonetheless, this analysis alone does not fully unlock the value of the online experimental data. We therefore augment the model-free analysis by developing a binary logit response model, estimated in a Bayesian framework. Applying a model to the experimental data allows us to accomplish several additional objectives without the need to increase the size of the control group. (Notably, we are able to estimate all of our effects while reserving only 1.5% of total impressions for the control group.)

The model allows us to control for additional covariates and incorporate lags and impression counts. We can also calculate marginal effects and elasticities for display ad exposure. Using this information, we then evaluate our approach versus more common correlational approaches.

Finally, we are able to uncover relevant, actionable findings, despite the inherently sparse nature of the available data. Overall, our approach accounts for biases stemming from both browsing behavior and targeting, reveals ad effectiveness at different stages of the purchase funnel, can be easily implemented by firms at a relatively low cost, and utilizes the types of data generally available to managers.

1.5 Data

The data for this study were provided by a large financial services firm, and focus on a single suite of consumer financial management tools. From the customer's perspective, this product line is largely independent, maintaining its own brand and website. Due to the nature of our research agreement with the company, we cannot share the name of the firm or the precise details of their business. The data are at the individual cookie level, and detail the online interactions between the firm and individuals in a mid-size Midwestern market during the six week period from February 19 to April 2, 2010.

Because our data come from a single firm advertising one product during a specific time in a given market, we do not seek to draw generalizable conclusions regarding display advertising effectiveness. Instead, we use our data to explain how our approach can be applied to reveal interesting, unexpected results, and how the resulting recommendations can vary dramatically from those produced by commonly used correlational approaches. We leave the further generalization of our findings as a topic for future research.

Our data, like all cookie data, have limitations. A tracking cookie actually identifies a unique browser. It is possible that multiple users share a browser; in this case our data are not unlike the

household level panels used in much of the marketing literature. It is also possible that an individual regularly uses multiple browsers, resulting in multiple cookies for the same individual. Toupet et. al. (2012) compare cookie data from a common analytics provider to the proprietary Nielsen panel; they find that the vast majority of individuals are associated with only a single cookie. There has also been concern that individuals who allow tracking cookies to persist on their machine differ in some critical, unknown respect from those who disable or frequently delete them. As pointed out by Chatterjee et. al. (2003), many websites block access to browsers with cookies disabled, leaving consumers with little practical choice in this regard. With respect to cookie deletion, research has also shown that most individuals delete cookies less than once per month (comScore 2007). Finally, Dreze and Zufryden (1998) used a randomized experiment to show that consumer browsing behavior is not significantly influenced by the presence of tracking cookies.

In our data, each display impression contained one of nine different designs, consisting of the same solid color background with a phrase, icon, or both in the foreground. Unfortunately, we do not have sufficient information to consistently identify the creative for each impression. While we have been assured by management that the only systematic variation in creative was by week (which we will control for), this precludes us from pursuing questions related to the efficacy of various designs or appeal types. The ads were distributed across a myriad of sites, using targeting algorithms that were only partially known to the firm. For example, the firm could control the total number of impressions per day or retarget individuals that had previously visited the site, but they were not privy to all of the individual level factors determining when and to whom impressions were served.

Based on the webpages a user visits within the firm's site, we identify four stages in the purchase funnel: non-visitor, visitor, authenticated user, and converted customer. Similar to many ecommerce websites, a consumer must proceed sequentially through these stages to complete an online transaction with the firm, though they may move through any number of them during a single visit. Non-visitors are individuals who have never interacted with the firm's website. Visitors have been to the site, but have not provided the personally identifiable information necessary to sign-up for an account. Authenticated users have signed up for an account, but no money has changed hands. Finally, a converted customer has completed a transaction.

We selected these stages of the purchase funnel for two key reasons. First, this purchase process is quite common in ecommerce. Second, we have distinct expectations regarding how consumers in each funnel stage may respond to display advertising. For example, those who have never been to the site (non-visitors) may be less familiar with the brand, and thus ads may help build awareness. In contrast, those who have been to the site and signed up (authenticated users) are likely to be very aware of the brand, and the ads may serve as a reminder to complete the transaction. While we do not propose specific hypotheses within this paper, these distinctions motivated our choice of purchase funnel structure.

We use site visit as our dependent variable of interest. We define this as any time a user visits the firm's website, regardless of the actual within-site browsing behavior. Our data come from a large provider of web-based financial management tools, and these tools are deeply integrated into the firm's website so as to provide a uniform look and feel. This means that consumers experience many of the product attributes (ease of use, clarity of messaging, etc.) immediately upon site visit. Prior research has shown that such direct product experience dominates

advertising's impact on attitudes, beliefs, and behavior (Hoch and Ha 1986; Marks and Kamins 1988; Tellis 1988; Wright and Lynch 1995). Because users begin evaluating product attributes at the point of site visit, we believe that display advertising's influence on subsequent actions such as sign-up or conversion will be overshadowed.² In addition, using site visit as the dependent variable allows us to evaluate display advertising's impact at various funnel stages, where we find significant, informative differences. Finally, site visit allows for broad generalizability of our approach across websites.

Our approach could easily be repurposed to measure the effect of advertising on other observable click stream behaviors, and we recommend that managers think carefully about the outcome for which their campaign is optimized. Firms should be primarily interested in increasing profits, and optimizing advertising for any intermediate step runs the risk of leaving money on the table. For instance, if a display ad campaign were to attract individuals who are willing to visit but unlikely to purchase, it could be an exceptionally effective traffic driver without contributing to profits. Similarly, a campaign could attract window shoppers, individuals likely to move through the free portions of the purchase funnel but who are unlikely to complete a transaction. Even if a campaign is optimized based on purchase incidence, one runs the risk of attracting low margin customers. With this in mind, managers should select an outcome of interest that reflects the advertising objective.

During our observation period, we track 133,058 cookies, 2,164 (1.6%) of which were randomly assigned to the control group. Because the data contain only observed interactions between the user and the firm, we only know that a cookie is actively tracking a user between the first and last observed events. Thus, including lag effects and funnel position effects can create an initial conditions problem. To mitigate these issues, we require an ad exposure or site visit at

least three weeks prior to our focal period and use this initialization period to determine the funnel position. We also require that each cookie survive for at least four weeks, because frequent cookie deletions would bias our estimated effects towards zero.³ We culled cookies in the top one percent of total impression counts to remove crawlers, machines programmed to download large numbers of webpages without human interaction. We also removed individuals who were not served a display ad impression, because they cannot be identified in our dataset as charity or firm. Finally, we removed observations from 795 mobile devices.

The data was discretized to the cookie-day, resulting in 4,748,020 observations. During this period, 2,216,947 display ad impressions were served, 33,096 (1.5%) of which were served to our control group. Table 1 presents the distribution of observed impression counts, showing a significant mass at 0 and a long positive tail. The vast majority of cookie days (87.5%) contain no impressions, and 98.2% of observations contain five or fewer impressions. Table 2 contains the observation counts by treatment group and funnel stage. Note that the treatment group has a larger proportion of observations in later funnel stages. This follows expectations if advertising has a positive impact.

1.6 Model-Free Results

Because of the experimental design, the only difference between the treatment and control groups within a funnel stage is the type of display ad shown. Thus, a difference in the probability of site visit between these two groups is attributable to the display ads. To examine this difference, we aggregate the cookie-day observations by treatment group and funnel stage, and calculate the probability of site visit within each grouping. As throughout this paper, site visit is

represented as a binary indicator. Thus, we assume that multiple page views on a single day are a part of the same visit. The results are presented in Figure 1. In each stage, cookies in our treatment group are more likely to visit than those in the control group. Non-visitors, visitors, authenticated users, and converted customers in the treatment group are respectively 0.07%, 0.01%, 0.99%, and 0.52% more likely to visit on any given day than their control group counterparts. While these differences may seem small, this is due to the small overall probability of site visit. In terms of odds, this translates to a respective 74.7%, 0.6%, 49.7%, and 48.2% increase between the control and treatment groups. However, this difference is only significant ($\alpha = 0.05$) for non-visitors and authenticated users, and marginally significant ($\alpha = 0.10$) for converted customers.

These results provide strong model-free evidence for the efficacy of online display advertising and differences in that efficacy along the purchase funnel. Nevertheless, there are a number of limitations. First, they do not directly control for potentially important observable factors, such as lagged effects or timing. Second, they do not account for the number of impressions served to users. Third, they do not provide insight into marginal effects or elasticities, which are needed for making allocation decisions.

These limitations are not a result of the experimental approach per se – all of these effects could be estimated non-parametrically given a sufficiently large sample. Rather, they stem from practical constraints associated with the control group cost, which would increase exponentially as experimental factors are added. In a balanced experiment, the required sample size generally increases exponentially with the number of additional factors to avoid a loss in precision. Given the disparate size of our treatment and control groups, we may be able to obtain equal precision by increasing only the control group. Unfortunately, the incremental cost of our approach is

directly proportional to the size of the control group. Measuring marginal effects further complicates matters, because each impression count would represent an additional treatment level and require a sufficiently large sample size to reliably estimate effects. Unfortunately, firms cannot dictate individual impression counts. Thus, no finite sample can guarantee reliable non-parametric estimation of marginal effects. With statistically significant results for only two of four groups, we already appear to be pushing the bounds of what can be learned non-parametrically even with 4,748,020 observations and 2,216,947 display ad impressions.

1.7 Model

To control for potentially important factors, estimate lag effects, and calculate marginal effects elasticities, we turn to a model of advertising response. Given the binary nature of our dependent variable, whether or not a site visit occurred, we turn to a standard binary logit estimated at the cookie-day level. Below, we will discuss how we use this model to account for endogeneity stemming from browsing behavior and targeting, as well as the limitations created by the sparse nature of our data.⁴

Using the standard binary logit model, we express the probability of site visit at time t conditional on a set of covariates X as:

$$Pr(visit_t|X, \beta) = \frac{\exp(X\beta)}{1 + \exp(X\beta)}. \quad (1)$$

Here, X contains the log transformed contemporaneous and lagged impression counts for both firm and charity ads, indicators for funnel stage, date controls, and interactions between the logged contemporaneous effects and funnel stage indicators.⁵ β is the vector of parameters to be estimated. The parameters for contemporaneous and lagged impression counts as well as the

related interactions differ depending upon whether the user is in the treatment or control group. We specify the remaining parameters to be equal between groups because group assignment is random and, therefore, the only difference is in the ad shown. This specification allows us to maximize the information content on which the ad response parameters are estimated, conditional on a given control group size. We log transform the impression counts to incorporate decreasing marginal effects of advertising and to reduce the influence of large impression counts on parameter estimates. This specification also provides a better fit than either linear or squared transformations, as measured by the Bayesian Information Criterion.

As we will discuss in more detail below, our estimated effect size is the difference between the fitted probability of site visit for members of the treatment and control group. While a point estimate for any set of covariate values could certainly be calculated using a frequentist approach, the Bayesian approach more readily provides us with the posterior distributions necessary to evaluate the estimated effect sizes. Further, it allows us to easily aggregate posterior distributions over groups of covariate values. In this way, we can look at the effect of display advertising by funnel stage, impression count, or both.

To obtain actual model-based effect sizes, we must deal with a series of issues. First, the effect of an interaction term is not equal to its marginal effect in non-linear models, but rather the magnitude, direction, and significance are all a function of the remaining covariate and parameter values (Ai and Norton 2003). Second, the effect of n exposures to a firm's advertising is the probability of site visit at time t conditional on those exposures minus the probability of site visit for an otherwise identical individual conditional on the same number of exposures to control ad impressions. This difference in probabilities is not easily interpreted from any one of the marginal posterior distributions.

Because the Bayesian approach produces draws from the joint posterior distribution of the parameters, we can overcome these challenges. Using the joint posterior draws, we can simulate the predictive distribution for any set of covariate values, $\Pr(\text{visit}_t | x_{t,f}, x_{t,c}, \beta, \Psi)$.⁶ Here, $x_{t,f}$ is the number of firm impressions at time t , $x_{t,c}$ is the number of control impressions, Ψ represents the vector containing value of all other covariates, and β represents a matrix containing all draws from the joint posterior distribution of the parameters in Equation (1). As discussed above, we are not directly interested in these values, but rather the change in probability of site visit, v , given n firm impressions relative to the probability of site visit given the same number of charity impressions. Let $\Delta_v(n)$ represent this difference in predictive distributions such that,

$$\Delta_v(n) = \Pr(\text{visit}_t | x_{t,f} = n, x_{t,c} = 0, \beta, \Psi) - \Pr(\text{visit}_t | x_{t,f} = 0, x_{t,c} = n, \beta, \Psi). \quad (2)$$

To include K lags, we generalize Equation (2) by calculating the difference in probability of a site visit in any of the K periods given n firm impressions less the probability of a site visit given n control impressions. This gives the following:

$$\begin{aligned} \Delta_v(n) = & \bigcup_{k=0}^K \Pr(\text{visit}_{t+k} | x_{t,f} = n, x_{t,c} = 0, \beta, \Psi) \\ & - \bigcup_{k=0}^K \Pr(\text{visit}_{t+k} | x_{t,f} = 0, x_{t,c} = n, \beta, \Psi). \end{aligned} \quad (3)$$

Note that we take the union of the probabilities over the K periods instead of the sum to avoid double counting the probability of a site visit occurring on multiple days.⁷

Given this effect size estimate, we can simulate the predictive distributions for the marginal effects and elasticities. For n impressions, the incremental effect of an additional impression, $\mathcal{M}_v(n)$, is the effect of $n + 1$ impressions minus the effect of n impressions:

$$\mathcal{M}_v(n) = \Delta_v(n+1) - \Delta_v(n). \quad (4)$$

Following the standard definition, advertising elasticity can be calculated as the percentage change in advertising effectiveness divided by the percentage change in impression count. Because impression counts are by nature an integer value, the elasticity of an additional impression reduces to the product of $\mathcal{M}_v(n)$, n , and the inverse probability of site visit given n firm impressions:

$$\eta_{x_{t,f}} = \Delta_v(n) \times \left(\frac{n}{\bigcup_{k=0}^K Pr(\text{visit}_{t+k} | x_{t,f} = n, x_{t,c} = 0, \beta, \Psi)} \right). \quad (5)$$

Following equations (2) through (5), we can simulate the predictive distributions for a given set of covariate values. However, in evaluating the size and robustness of an effect, it is often more informative to look at these distributions over a range of covariate values. To do this, let Ψ be a matrix containing the fully enumerated values over the observed span of X . With some abuse of notation, equations (3) through (5) then produce a matrix of draws from the relevant predictive distributions, where each row represents the distribution for a given set of covariate values.⁸ We use these joint predictive distributions in what follows.

1.8 Model-Based Results

We specify and estimate four versions of the model, building from rudimentary to the full specification. As with the model free evidence, each model is estimated at the cookie-day level, with site visit as the outcome of interest. Model 1 includes only contemporaneous firm and charity impressions. Model 2 adds the effects of funnel stage and date controls, while Model 3 allows for interactions between contemporaneous impressions and funnel stage. Finally, Model 4 also allows prior day impressions to influence site visit. In Table 3 we present parameter

estimates and fit criteria for the four models. Each model was estimated using 100,000 draws, with a 10,000 iteration burn-in period and retaining every 10th. As mentioned above, directly interpreting the parameter estimates for interacted terms in Table 3 can lead to incorrect conclusions regarding the significance, direction, and magnitude of their effects. However, the table does give two key pieces of information. First, the final row contains the Bayesian Information Criterion (BIC), from which we can clearly see that Model 4 is our preferred specification. Second, we can interpret the estimated effects of non-interacted terms within this model. The large negative intercept estimate (-6.960) is a result of the low average frequency of site visitation. Weekdays during the sixth week serve as our baseline, so the significant parameter estimates for weeks one (0.610), two (0.481), three (0.174), and five (0.173) indicate that subjects were generally more likely to visit during these weeks than during the sixth week, *ceteris paribus*. Similarly, the positive parameter estimate for weekend (0.108) shows that subjects were more likely to visit on Saturday or Sunday than the remaining days, *ceteris paribus*.

We tested a number of additional model specifications, focusing on extending the lag structure and allowing for interactions between contemporaneous funnel position and lagged impressions. Given the sparse nature of the data and the disparate size of the control and treatment groups, we must be careful in evaluating model fit. As shown in Equation (2), the effect of online display advertising is the difference in the fitted probability of site visit between otherwise identical members of the treatment and control groups. Model parameters must then be added in pairs (one each for the treatment and control groups) to identify an effect. However, standard model selection methods evaluate only the overall model fit, and remain agnostic as to which parameters drove the improvement. Given the relatively small nature of the control group

and our model specification, additional treatment group parameters may drive a sufficient increase in likelihood to offset any over-fitting resulting from their control group counterparts. This control group specific over-fitting may result in wide confidence bands around the resulting probabilities of site visit, masking our estimated effect sizes. To account for this, we tested model extensions by first adding the control group parameters, and continuing with treatment group parameters only if model fit improved as measured by BIC. We found that neither an expanded lag structure nor interactions between contemporaneous funnel position and lagged impressions improved model fit.

To examine how display advertising's effect varies by purchase funnel stage, we fully enumerate the span of our data, including up to six focal and lagged impressions.⁹ Because we are modeling the effect of display advertising conditional on funnel stage, we require funnel stage to remain constant within an observation. That is, an individual in our simulated data cannot be an authenticated user during the first period and a converted customer in the next. If such a transition were allowed, our estimates of display advertising effectiveness would be confounded with the main effects for funnel stage. This restriction is largely consistent with our data, as only 0.7% of observed impressions carry over from one funnel stage to another. We also restrict non-focal impressions (i.e., lagged impressions at time t and contemporaneous impressions at time $t + 1$) to be firm impressions. Without this constraint, the effect of focal impressions would be confounded with that of non-focal impressions. Thus, we are measuring the impact of additional firm impressions given some consistent state. Using this simulated data, our posterior draws, and equations (3) through (5), we can plot the posterior distributions for the effects, marginal effects, and elasticities of display advertising for any set of covariates.

Figure 2 contains the posterior distribution of effect size for a single display advertising impression, broken down by funnel stage. Similar to Figure 1, we see that the effect of display advertising is positive and significant for non-visitors, authenticated users, and converted customers, while it has no discernible effect on prior visitors. The null effect for this last group is likely attributable to the aforementioned dominance of product experience over advertising effects.¹⁰ In visiting the site, these individuals have been exposed to detailed firm offerings, but declined to proceed. It is unlikely that the firm's display advertising, a monochrome banner containing a single icon, tagline, or both, will contain sufficient new information to persuade these individuals to reconsider. Comparing the distributions across funnel stages, it is clear that the effect size is significantly smaller for non-visitors than for authenticated users or converted customers, with at least 99.9% of the posterior distributions for these later stages being greater than the median draw for non-visitors. Given the randomized nature of the field experiment, these differences provide additional evidence that consumer response to advertising changes with familiarity and experience.

Figures 3 and 4 contain the median and 95% confidence bands for the effect and marginal effect at each of the purchase funnel stages for up to six impressions. Each point on the plot represents the increase in the probability of site visit from one additional impression. All of the previous findings are supported, and we now see a marked difference in marginal effects between visitors and both authenticated users and converted customers. While the effect of a display ad impression is small for non-visitors, so is the rate of decreasing marginal effectiveness. For authenticated users and converted customers, the effect sizes are larger, but degrade more quickly with additional impressions.

Figure 5 plots the posterior distribution of the elasticity estimates at a single impression. The descriptive statistics for these distributions can be found below in Table 4. While the display advertising effect is smaller for non-visitors than converted customers, the median elasticity estimates are almost equal. Note that by examining the elasticity at a single impression, impression count drops out of Equation (5), resulting in elasticity being equal to the ratio of the marginal effect and the probability of site visit given a single firm impression:

$$\eta_{t,f} = \frac{\Delta_v(1)}{\sum_{k=0}^K Pr(\text{visit}_{t+k} | x_{t,f} = 1, x_{t,c} = 0, \beta, \Psi)}. \quad (6)$$

Since the marginal effect for non-visitors is significantly smaller than for converted customers, the comparable elasticity estimates are due to the relatively small probability of site visit for non-visitors.

Excluding visitors, all posterior distributions for elasticities are positive and significant, with at least 99.9% of their posterior draws greater than zero. For the three funnel stages with significant elasticity estimates, the combined 95% confidence band spans 0.05 to 0.16 with a median of 0.10. Interestingly, this is exactly in line with the average sales-to-advertising elasticity of 0.10 found in existing literature (Hanssens 2009). However, we caution that these are not sales-to-advertising elasticities, but rather site-visit-to-advertising elasticities. Further, this estimate focuses on the three responsive segments, ignoring prior visitors. The closest comparison to our measure is likely that of Rutz and Bucklin (2012), who report a page-choice-to-on-site-advertising elasticity of 0.20 for within site browsing behavior. Notably, their estimate focuses on browsing behavior within the same website, while ours focuses on driving behavior across sites.

1.8.1 Optimal Allocation & Expected Site Visits

Based on these elasticity estimates, we can calculate the optimal allocation and estimate the resulting expected number of visits using the Dorfman-Steiner condition. This condition states that, at optimality, marginal revenue must equal marginal cost for each marketing instrument (Dorfman and Steiner 1954; Lambin, Naert et al. 1975; Sridhar, Mantrala et al. 2011). Given this, it follows that the ratio of advertising elasticities for any two marketing instruments is equal to the ratio of their costs. While the Dorfman-Steiner condition is often thought to apply only to monopolies, it holds if we apply the weaker assumption of monopolistic competition (Lambin, Naert et al. 1975). That is, we need only assume that the firm's competitors will not react to the recommended shift in advertising allocation. Our recommendations for reallocation are based on a consumer's position in the purchase funnel, as determined by prior within-site browsing behavior. Because this is not observed by the firm's competition, we believe that competitors would be highly unlikely to respond to the recommended shifts in advertising spending.

To be sure, one can imagine scenarios in which a (partially) unintentional competitive response could occur. When a firm reduces the advertising to a given segment (i.e., non-visitor and visitor), these impression opportunities do not cease to exist. Rather, they are reallocated to other advertisers based on the ad server's sales and targeting processes. In a system in which all advertisers sell competitive products, such impression opportunities would certainly be filled by a competitor, and a competitive response would, by definition, occur. However, the vast majority of ad servers work with a wide variety of advertisers, most of whom are almost certainly unrelated to our focal firm. Further, these advertisers are only partially privy to the sales and

targeting algorithms used by the ad servers. Thus, the chance of a competitor filling foregone impression opportunities is quite small.

We can apply Dorfman-Steiner using only our existing data, provided we make three additional assumptions. First, we must view online display advertising that is targeted to each purchase funnel stage as a distinct marketing instrument, precluding carryover effects between stages. Given that our preferred model contains a single day lag, only 0.7% of all observed impressions carryover from one stage to the next. Second, we must assume that the cost per impression is constant across funnel stages. This assumption is necessary because management indicated that the firm was not allocating impressions following the Dorfman-Steiner condition and our data does not contain cost information. If they had been targeting impressions optimally or costs were observed, we could relax this assumption. For firms making allocation decisions, costs will generally be observable and should be used in place of impression counts in our approach. Finally, we must assume that the firm's objective is to maximize site visitation, with profit maximization as an assumed byproduct. In our application, consumers begin experiencing the product when they arrive at the site, and, as previously discussed, product experience dominates advertising as a determinant of consumer beliefs and behaviors. Further, there are near zero marginal costs related to each site visit. Therefore, maximizing site visitation is likely closely correlated with maximizing profit, at least for non-converted customers. Because this assumption does not hold for converted customers, we exclude them in the allocation recommendations that follow. Though we have made a number of assumptions to enable this analysis, some of which might be challenged, our intent remains primarily to illustrate how dramatically advertising allocation can shift when effects are understood at the funnel stage level.

Accepting the above, the Dorfman-Steiner condition states that the ratio of site-visit-to-advertising elasticities for any two segments must be equal to the ratio of their total impression counts:

$$\frac{\eta_{nv}}{\eta_{au}} = \frac{Imps_{nv}}{Imps_{au}} \text{ and} \quad (7.a)$$

$$\frac{\eta_v}{\eta_{au}} = \frac{Imps_v}{Imps_{au}}. \quad (7.b)$$

Where η is the site-visit-to-impression elasticity for a given funnel stage, $Imps$ is the corresponding impression count, nv represents non-visitors, v represents visitors, and au represents authenticated users. Further, we know that the total number of impressions available is the sum of the impressions served to each group, or:

$$IMPS = Imps_{nv} + Imps_v + Imps_{au}. \quad (8)$$

Given the elasticity and total available impressions, we solve this system of equations for the optimal allocation. The optimal allocation rules are:

$$Imps_{nv} = \frac{IMPS}{\frac{\eta_v}{\eta_{nv}} + \frac{\eta_{au}}{\eta_{nv}} + 1}, \quad (9.a)$$

$$Imps_v = \frac{IMPS}{\frac{\eta_{nv}}{\eta_v} + \frac{\eta_{au}}{\eta_v} + 1}, \text{ and} \quad (9.b)$$

$$Imps_{au} = \frac{IMPS}{\frac{\eta_v}{\eta_{au}} + \frac{\eta_{nv}}{\eta_{au}} + 1}. \quad (9.c)$$

Because the proposed approach produces a negative elasticity estimate for prior visitors, we fix this value at zero in what follows.¹¹

Table 5 contains the actual impression allocation as well as that based on the median elasticity estimates from the proposed approach. Based on our approach, the firm should dramatically reallocate impressions between funnel stages. All impressions previously served to

prior visitors should be allocated to authenticated users, along with 26% of those that were previously served to non-visitors. In the end, the optimal allocation for authenticated users based on our estimates is 497% larger than the actual allocation.

This finding deserves a bit of further discussion. The recommended reallocation may be a result of suboptimal allocation on the part of management, or it may be a result of cost differences that are unobservable to the researcher. We can't disentangle these drivers given our data. While we assume that costs are constant across purchase funnel stages, and indeed it seems unlikely that targeting any given individual would be more expensive than another, this does not fully account for untargeted impressions. Given that the low probability of site visit results in a large proportion of non-visitors relative to other funnel stages, untargeted advertising is more likely to reach non-visitors. If the cost of untargeted impressions is sufficiently small, it may be optimal to allocate more impressions to non-visitors than our approach would indicate. However, this is a result of our assumption of equal costs across funnel stages, and not a result of our estimated effect sizes. For a firm implementing our approach, such cost differences would be observable, and should be included in the optimal allocation calculations to mitigate such concerns.

Using our proposed elasticity estimates provided in Table 4, we can calculate the expected number of visits given these allocation shifts as follows:

$$VISITS = \sum_s E[visits_s] = \sum_s visits_{s,actual} \left[1 + \eta_s \left(\frac{imps_{s,opt}}{imps_{s,act}} - 1 \right) \right]. \quad (10)$$

Where $E[visits_s]$ is the expected number of visits from funnel stage s , $visits_{s,actual}$ is the number of visits observed for that funnel stage in the data set, η_s is the elasticity estimate from our proposed approach for that funnel stage, $imps_{s,opt}$ is the number of impressions served to

that funnel stage under the optimal allocation, and $imps_{s,act}$ is the observed number of impressions served to that funnel stage. Table 6 compares the actual number of site visits at each stage, with that expected given our approach. In total, our proposed allocation results in 1,241 expected additional site visits, a 9.85% increase.

Simply increasing expected visits is insufficient to justify our methodology; we must also outperform what would be expected if the 31,758 impressions that were shown to the control group¹² are instead used for firm advertising. These estimates are contained in the last column of Table 6. Given the observed proportion of impressions allocated to each funnel stage, the additional impressions increase expected visits, but only by 16 (0.12%). Given that this is a tiny proportion of the increase delivered by the optimal allocation, the firm is clearly better off following our approach.

1.8.2 Our Results Versus Correlational Estimates

As mentioned earlier, individual targeting and browsing behavior can bias common correlational estimates of display advertising's effectiveness (Goldfarb and Tucker 2011; Lewis, Rao et al. 2011). While the combined direction and magnitude of these biases is unknown *a-priori*, we can examine them *a-posteriori*. To obtain comparable standard correlational estimates, we use the same approach as above, except that the simulated control group contains no impressions. This is similar to comparing individuals who saw the firm's advertising to those who did not.¹³ In this case Equation 3 becomes:

$$\Delta_v(n) = \bigcup_{k=0}^K \Pr(\text{visit}_{t+k} | x_{t,f} = n, x_{t,c} = 0, \beta, \Psi) - \bigcup_{k=0}^K \Pr(\text{visit}_{t+k} | x_{t,f} = 0, x_{t,c} = 0, \beta, \Psi). \quad (11)$$

By excluding the effects of advertising on the control group, we give up control for potential biases associated with both browsing behavior and individual targeting. While we cannot isolate the biases individually, comparing the results from this model to that of our proposed model allows us to identify their joint effect.

Figures 6 through 8 compare the posterior distributions for each measure (effect, marginal effect, and elasticity) from this correlational model to those from our approach. In general, the correlational estimates are biased towards zero for each of the funnel stages, with visitors standing out as a possible exception. For each combination of funnel stage and measure, Table 7 reports the percent of the posterior distribution from our approach which exceeds the median from the correlational approach. For authenticated users and converted customers, the 95% confidence bands from our approach exclude the median from the correlational estimate for each measure. For non-visitors and converted customers, the evidence is less clear.

Even where the correlational estimates are significantly biased, the difference from our estimates are far smaller than the two to three orders of magnitude reported by Lewis, Rao et al. (2011) and the direction is reversed. The smaller difference is likely due to the outcome of interest. Lewis et. al. selected response measures with a high baseline probability (i.e., page views and searches on Yahoo.com), which are likely to be more highly correlated with browsing behavior than are visits to a relatively less well trafficked website. The directional difference may be due to our specific application in financial services. Activities requiring significant

concentration and/or privacy, such as making important financial decisions, may be negatively correlated with browsing a large number of webpages. Given these relatively small differences as compared to Lewis, Rao et al. (2011), a relevant question is whether the informational gain from our approach is worth the cost (i.e., the impressions that were donated to charity in creating the control group). That is, would a manager find it worthwhile to implement our approach versus simply using correlational measures?

We can measure the benefits of our approach versus correlational measures using the Dorfman-Steiner condition. Again, given our underlying assumptions and the extent to which the recommended allocations vary from actual, this analysis is primarily illustrative. We seek to show that not only are the parameter estimates from our approach significantly different from correlational measures, but the resulting recommendations for impression allocation also differ in practical significance.

Similar to Table 5, Table 8 compares the optimal allocations based on the median elasticity estimates from the correlational approach to that observed in the data and what was recommended under the proposed approach. Under the correlational approach, 373,544 non-visitor impressions are reallocated to later funnel stages, with 23.3% going to visitors and the remaining 76.7% to authenticated users. In the end, the recommended impression allocation for authenticated users based on the correlational approach is 175% larger than the actual allocation but 54% smaller than that recommended by our proposed approach.

Again, we cannot say decisively that differences between the actual allocation and that recommended by the correlational approach is a result of suboptimal allocation, because we do not directly observe costs. However, the difference between our proposed allocation and that of the correlational approach is quite interesting. Neither allocation takes cost into account, so the

difference is a direct result of the estimated ad response parameters. In short, the correlational approach seems to drastically underestimate display advertising's effect among authenticated users, and accordingly under-allocates impressions to this group.

1.9 Discussion

Our research has several implications for managers. First, building on a long history of using controlled field experiments to evaluate advertising effectiveness, we introduce a relatively low cost, implementable test and control methodology for examining display ad effectiveness in light of unobservable targeting and browsing behavior biases. Using ads for an unrelated charity in place of firm advertising, we are able to identify a baseline independent of firm intervention. We produce model free evidence that online display advertising increases the probability of site visit, even accounting for targeting and browsing behavior biases. For managers, this means that display advertising can be a productive marketing tool.

Second, we introduce a modeling approach that allows us to calculate effect sizes, marginal effects, and elasticities while limiting the size of the control group. We show that all of these can vary greatly by purchase funnel stage, suggesting that managers should carefully consider both the reach and frequency of their retargeting strategies. In our application the null effect for visitors and the strong effects for authenticated users and converted customers are evidence that retargeting strategies need to be more nuanced than simply identifying those that have previously visited the site. Overall, we provide evidence that consumer response to display advertising changes with their relationship to the firm, and recommend that targeting strategies account for this.

Third, we show that our approach produces estimates that can differ significantly from correlational estimates. This supports the work of recent display advertising researchers, especially in their call for caution when analyzing display advertising campaigns. Importantly, we show that correlational estimates can actually understate the effect of display advertising in some instances.

Finally, we show that our approach not only produces significantly different estimates than popular correlational approaches, but that allocation decisions based on our approach produce significantly more site visits. Given that the expected increase in site visits due to improved allocation decisions dominates the loss due to impressions sacrificed in creating the control group, managers can be confident in implementing our methodology. As targeting becomes increasingly complex, we expect that such an approach will become increasingly valuable.

1.10 Conclusion

Our purpose was to develop and test a managerially applicable method for estimating the effects of online display advertising, accounting for both targeting and browsing behavior biases. Further, we examined differences in display ad effectiveness by stage in the purchase funnel. We used a unique dataset containing online display ad impressions and firm interactions for 133,058 individuals over a six week period. A small subset of these individuals (2,164), were randomly assigned to a control group and shown ads for an unrelated charity in place of the firm's advertising. This control group allows us to identify the combined effect of targeting and browsing behavior biases.

In our data, we find that online display advertising has a small, positive, and significant effect for three out of the four purchase funnel stages studied. For non-visitors, the effects are small, but so is the rate of decreasing returns to increased exposure. For authenticated users and converted customers, the effects are significantly larger, but also decay much more quickly. For individuals who have previously visited the site but declined to provide identifying information, online display advertising has no discernible impact on behavior. Further, we find that the estimated effects derived from our approach and those derived from correlational methods are significantly different, both statistically and practically. Optimal impression allocation across funnel stages based on our proposed approach results in 1,241 additional expected site visits, representing a 9.85% lift in total visits. This is over three times more than would be expected using correlational estimates, even when all impression opportunities are leveraged for firm advertising.

While we have sought to develop a practical methodology for measuring online display advertising and obtaining managerial insights into its effects, there remain several opportunities to build on our research. First, we do not have information on other marketing variables (ie: offline advertising, pricing, etc.) during the time of our data. While we would expect such effects to be consistent across the randomly assigned treatment and control groups, such information could serve as a valuable control or provide insight into how effects vary based on observed heterogeneity. Second, we cannot link ad copy to a given exposure due to technical limitations in our data. While we have been assured by management that the only systematic variation was by week (which we control for), this does prevent us from pursuing interesting questions regarding ad copy effects such as how display advertising response in each funnel stage varies by appeal type. Third, we do not have any additional relevant behavior or demographic information. Such

information would be helpful in reducing uncertainty surrounding our parameter estimates, and may allow future researchers to disentangle browsing behavior from targeting biases. Finally, our data stem from a single campaign, for a single firm advertising a single product line in one market. Larger, more diverse data sets are needed before our results can be freely generalized or the underlying mechanisms driving differences in display advertising response by funnel stage are fully understood.

¹ Our attempt modeled ad response accounting only for targeting. We found that the signs of the targeting and response parameters were not empirically identifiable due to the data set's low individual level information content, a potential pitfall that Manchanda and co-authors note in their original paper.

² When running similar models using sign-up or conversion as the focal behavior, we were unable to obtain significant parameter estimates. This may be due to a lack of effect or to a decrease in observed behaviors caused by moving further through the purchase funnel.

³ Frequent cookie deletion decreases the probability that both an impression and a site visit would be associated with the same individual. Thus, including frequently deleted or "short-lived" cookies would incorrectly reduce the correlation between the two events, biasing our parameter estimates towards zero.

⁴ One immediate concern may be that logistic regression can underestimate the probability of rare events. Fortunately, the risk of bias declines dramatically as the number of observed events increases, and virtually disappears when the number of observed rare events exceeds 6,000, the upper bound published by King and Zeng (2001). With 16,433 observed site visits, we far exceed this threshold.

⁵ We add one prior to log transforming the relevant variables to avoid taking the logarithm of zero.

⁶ Note that this expression represents a distribution of probabilities given the data and the joint posterior draws of β ; it is not a single probability value.

⁷ As an example, consider the case of a one period lag effect (the result we find for our data). Our objective is to compute the lift in probability of a site visit occurring on either the day of exposure or the next day. Thus, we would add the probabilities of site visit for days one and two then subtract the product of those probabilities.

⁸ An alternative approach would be to use X in this calculation, with the assumption that future data will be similarly distributed. Given that our data are sparse (i.e., the vast majority of observed covariates are zero), the resulting plots and calculations provide little information beyond simply calculating Equation (3) once using a vector of zeros. We believe that fully enumerating the observed dataset better conveys the range and robustness of the effects.

⁹ 98.5% of our observations contain six or fewer impressions.

¹⁰ The observation counts in Table 2 make it unlikely that this null effect is due to a shortage of observations for visitors compared to the other funnel stages.

¹¹ If this were not done, the approach would recommend that this group be served a negative number of impressions. Also, we again note that the negative elasticity estimate is not significantly different from zero.

¹² This excludes the charity impressions served to the converted customers, as converted customers are not included throughout this comparison.

¹³ An alternative approach would be to discard the control group and associated parameters, and then re-estimate the model. This reduces the information content available to estimate the common parameters, while offering no additional benefits. Thus, we believe this approach to be the appropriate comparison.

1.11 Tables

Impression Count	Percent of Cookie Days
0	87.53%
1	5.77%
2	2.39%
3	1.22%
4	0.76%
5	0.49%
6+	1.84%

Table 1: Percentage of observations by observed impression count.

	Firm Ad	Charity Ad	Total
Non-Visitor	4,047,002 (86.62% / 98.30%)	70,082 (92.44% / 1.70%)	4,117,084 (86.71% / 100.00%)
Visitor	281,185 (6.02% / 99.13%)	2,470 (3.26% / 0.87%)	283,655 (5.97% / 100.00%)
Authenticated User	106,690 (2.28% / 98.69%)	1,414 (1.87% / 1.31%)	108,104 (2.28% / 100.00%)
Converted Customer	237,330 (5.08% / 99.23%)	1,847 (2.44% / 0.77%)	239,177 (5.04% / 100.00%)
Total	4,672,207 (100.00% / 98.40%)	75,813 (100.00% / 1.60%)	4,748,020 (100.00% / 100.00%)

Table 2: Observations by treatment group and funnel stage (% Column Total/% Row Total)

	Model 1	Model 2	Model 3	Model 4
Intercept	-5.931*** (-5.949,-5.913)	-6.865*** (-6.916,-6.815)	-6.962*** (-7.012,-6.910)	-6.960*** (-7.012,-6.907)
ln($x_{it,f} + 1$)	0.855*** (0.840,0.869)	0.538*** (0.522,0.555)	0.820*** (0.795,0.847)	0.869*** (0.842,0.897)
ln($x_{it,c} + 1$)	0.259* (0.010,0.473)	0.101 (-0.161,0.320)	0.280 (-0.095,0.586)	0.430* (0.059,0.745)
ln($x_{it-1,f} + 1$)				-0.159*** (-0.183,-0.134)
ln($x_{it-1,c} + 1$)				-0.501* (-0.959,-0.113)
Visitor		1.677*** (1.632,1.722)	1.869*** (1.816,1.921)	1.892*** (1.838,1.945)
Auth. User		2.774*** (2.729,2.817)	2.985*** (2.935,3.036)	3.008*** (2.958,3.056)
Conv. Cust.		2.153*** (2.112,2.194)	2.313*** (2.265,2.361)	2.336*** (2.288,2.384)
Visitor x ln($x_{it,f} + 1$)			-0.403*** (-0.448,-0.358)	-0.389*** (-0.434,-0.345)
Auth. x ln($x_{it,f} + 1$)			-0.443*** (-0.490,-0.398)	-0.432*** (-0.478,-0.387)
Conv. x ln($x_{it,f} + 1$)			-0.364*** (-0.404,-0.323)	-0.354*** (-0.395,-0.312)
Visitor x ln($x_{it,c} + 1$)			0.215 (-0.408,0.785)	0.177 (-0.452,0.746)
Auth. x ln($x_{it,c} + 1$)			-0.550` (-1.246,0.059)	-0.524` (-1.210,0.091)
Conv. x ln($x_{it,c} + 1$)			-1.271* (-3.029,-0.128)	-1.268* (-2.935,-0.110)
Week 1		0.606*** (0.550,0.662)	0.590*** (0.534,0.645)	0.610*** (0.556,0.663)
Week 2		0.451*** (0.395,0.509)	0.471*** (0.414,0.527)	0.481*** (0.423,0.539)
Week 3		0.158*** (0.098,0.218)	0.165*** (0.104,0.224)	0.174*** (0.113,0.236)
Week 4		0.028 (-0.034,0.091)	0.034 (-0.027,0.096)	0.042 (-0.019,0.103)
Week 5		0.168*** (0.107,0.228)	0.170*** (0.109,0.230)	0.173*** (0.113,0.234)
Weekend		0.118*** (0.084,0.152)	0.108*** (0.075,0.142)	0.108*** (0.074,0.142)
Log-Likelihood	-105,454 (-105,458,-105,453)	-96,636 (-96,641,-96,632)	-96,372 (-96,378,-96,367)	-96,003 (-96,010,-95,996)
BIC	210,953	193,207	192,774	192,630

*** $\alpha < 0.001$, ** $\alpha < 0.01$, * $\alpha < 0.05$, ` $\alpha < 0.10$

Table 3: Parameter estimates, model fit and associated 95% confidence bands based upon our approach.

	Median	90% Confidence Band	95% Confidence Band
Non-Visitor	0.10	(0.06, 0.16)	(0.05, 0.17)
Visitor	-0.00	(-0.10, 0.07)	(-0.13, 0.08)
Authenticated User	0.09	(0.05, 0.12)	(0.04, 0.13)
Converted Customer	0.12	(0.07, 0.16)	(0.06, 0.16)

Table 4: The second column contains the median elasticity estimate from our model for each funnel stage. The third and fourth columns report the associated confidence bands.

	Actual	Proposed Estimate
Non-Visitor	1,364,007	997,960
Visitor	389,757	0
Authenticated User	145,680	869,893

Table 5: Column two presents the actual impression allocation by funnel stage, while column three presents the recommended optimal allocation based on the Dorfman-Steiner condition.

	Existing Impressions		Incl. Charity Impressions
	Actual	Proposed Estimate	Actual
Non-Visitor	6,500	6,336	6,511
Visitor	2,914	2,914	2,914
Authenticated User	3,191	4,596	3,196
Total	12,605	13,846	12,621
Quantity Δ		1,241	16
% Change		9.85%	0.12%

Table 6: Column three reports the expected number of site visits conditional on the allocations presented in Table 5. The last two columns present a more stringent test, allowing the control group impressions to be served as firm impressions for the actual estimates.

	Effect	Marginal Effect	Elasticity
Non-Visitor	95.70%	79.40%	87.79%
Visitor	59.85%	24.58%	24.99%
Authenticated User	99.76%	98.74%	99.13%
Converted Customer	99.97%	99.53%	99.59%

Table 7: The percent of estimated posterior probability of site visit based on the proposed approach that exceeds the median posterior probability of site visit using correlational approaches.

	Actual	Proposed Estimate	Correlational Estimate
Non-Visitor	1,364,007	997,960	990,463
Visitor	389,757	0	476,979
Authenticated User	145,680	869,893	400,411

Table 8: This table builds on Table 5, adding the recommended impression allocations based on the correlational model estimates.

	Existing Impressions Only			Incl. Charity Impressions	
	Actual	Proposed Estimate	Correlational Estimate	Actual	Correlational Estimate
Non-Visitor	6,500	6,336	6,332	6,511	6,341
Visitor	2,914	2,914	2,914	2,914	2,914
Auth. User	3,191	4,596	3,688	3,196	3,701
Total	12,605	13,846	12,935	12,621	12,956
Quantity Δ		1,241	330	16	351
% Change		9.85%	2.62%	0.12%	2.78%

Table 9: This table builds on Table 6 by including the expected number of visits given the allocation recommendations resulting from the correlational approach.

1.12 Figures

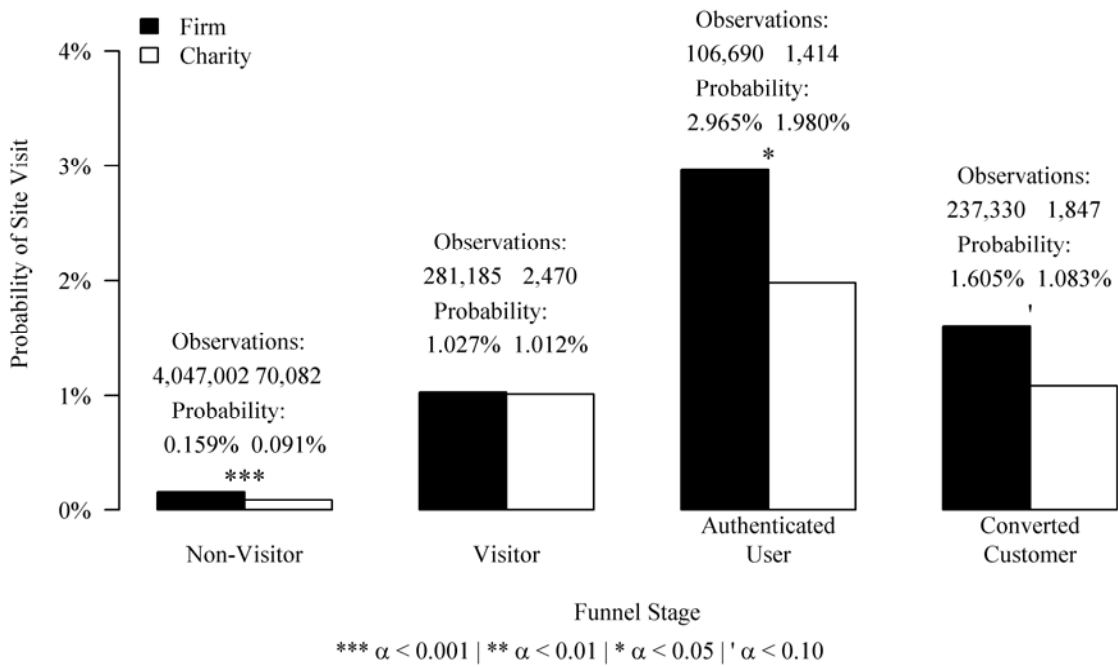


Figure 1: Probability of site visit by funnel stage & treatment group

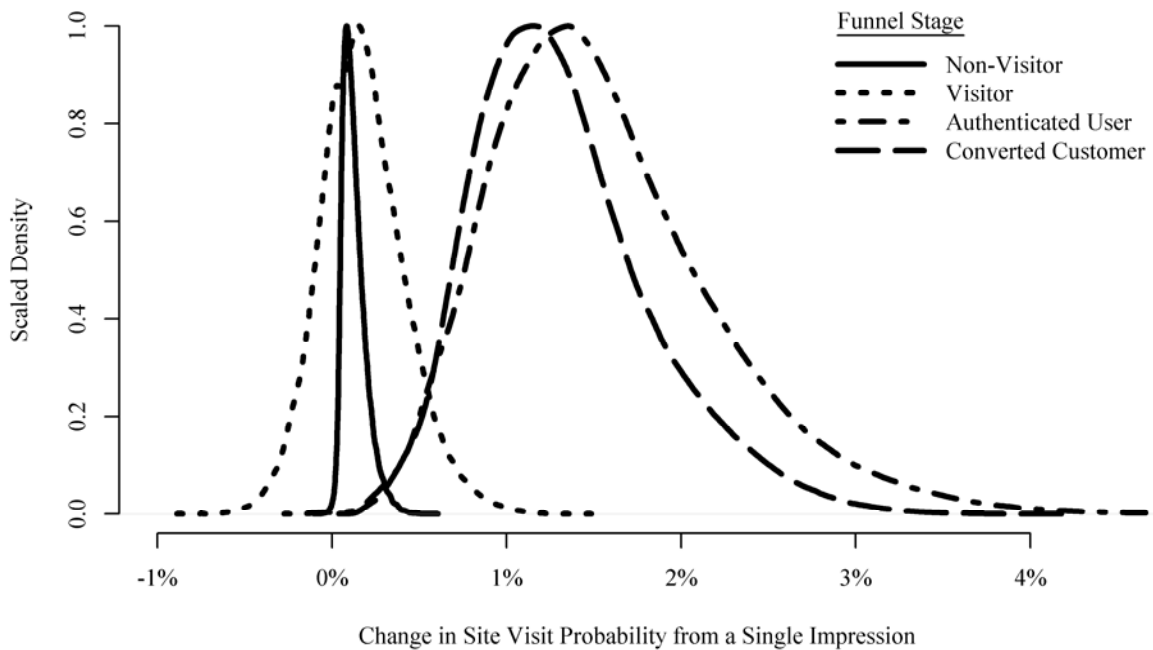


Figure 2: Effect of a single impression by funnel stage

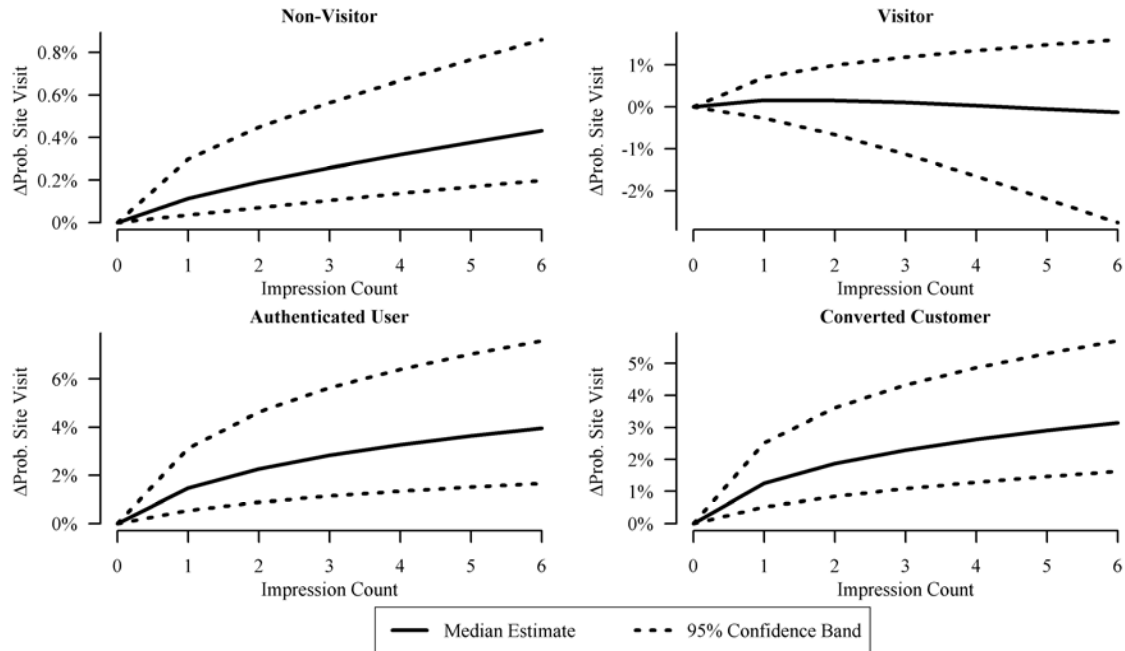


Figure 3: Median & 95% confidence bands for the estimated effects (1-6 impressions). The solid lines reflect the median effect size estimate resulting from our approach, while the dashed lines represent the 95% confidence bands.

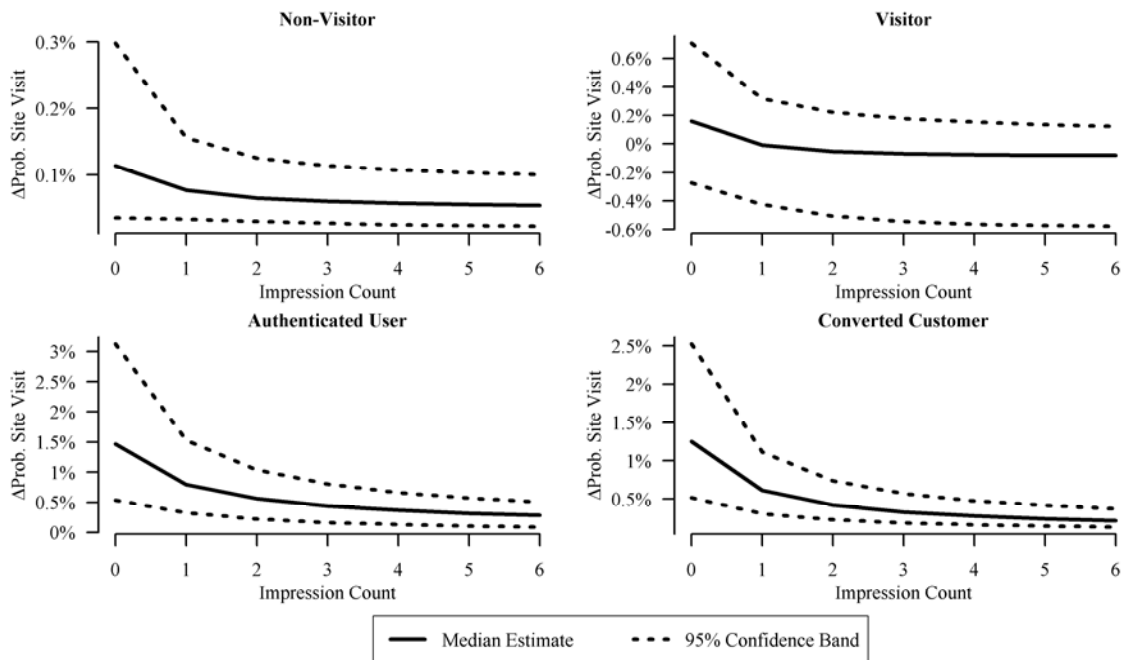


Figure 4: Median & 95% confidence bands for marginal effects (1-6 impressions). The solid lines reflect the median effect size estimate resulting from our approach, while the dashed lines represent the 95% confidence bands.

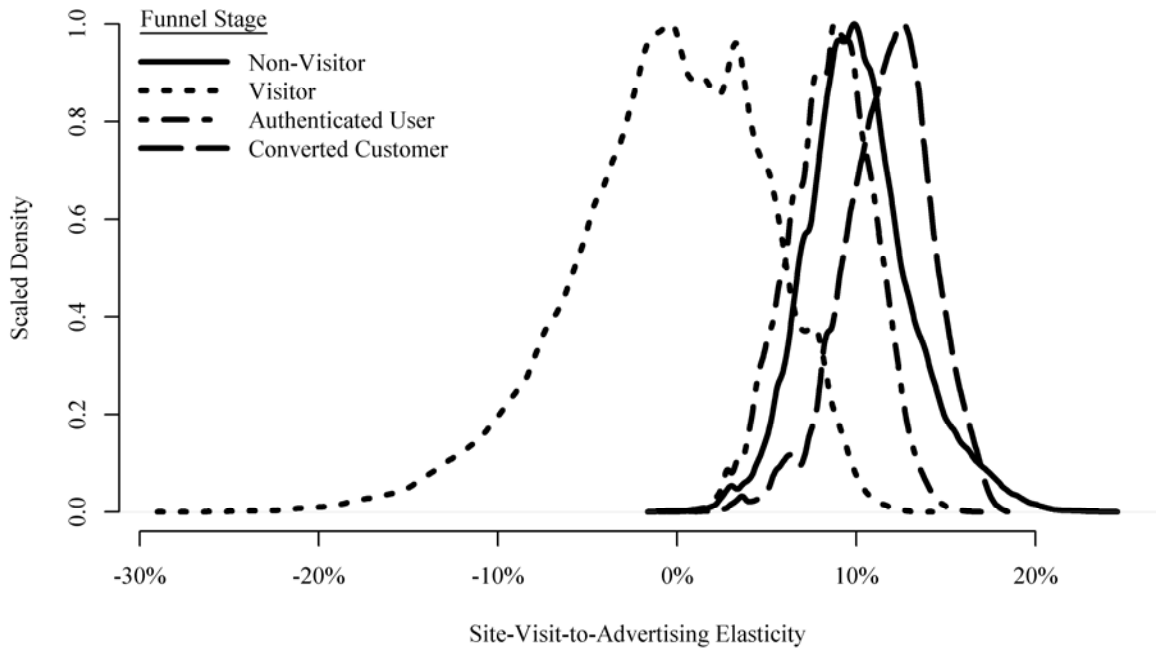


Figure 5: Elasticity of moving from one to two impressions.

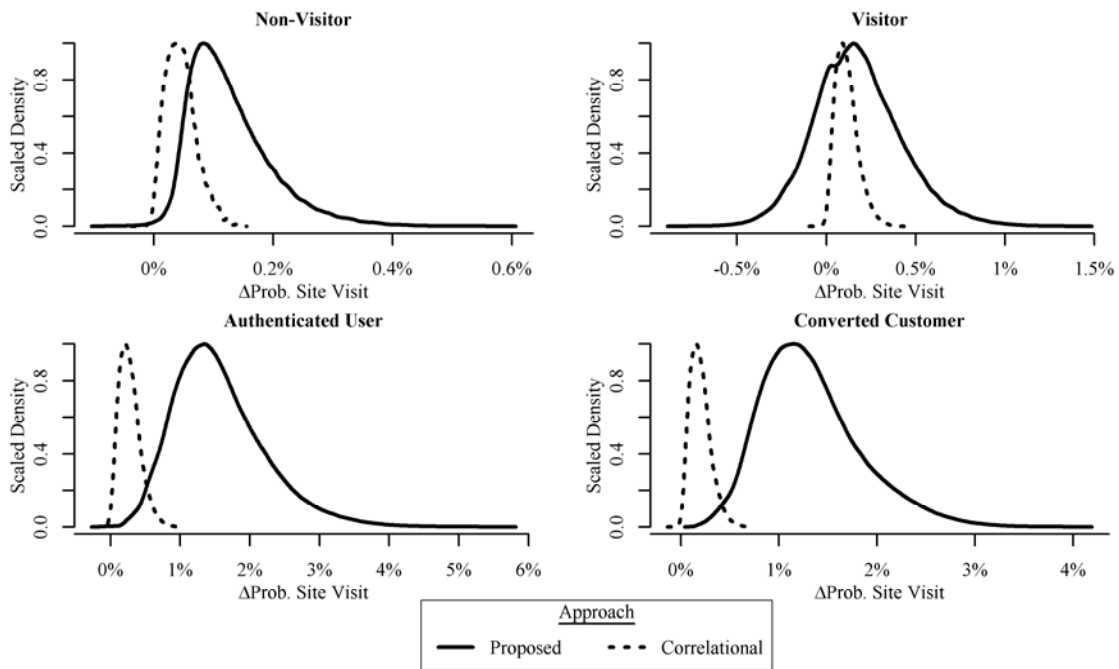


Figure 6: Comparison of effect of a single impression between our approach and a correlational approach for each of the funnel stages.

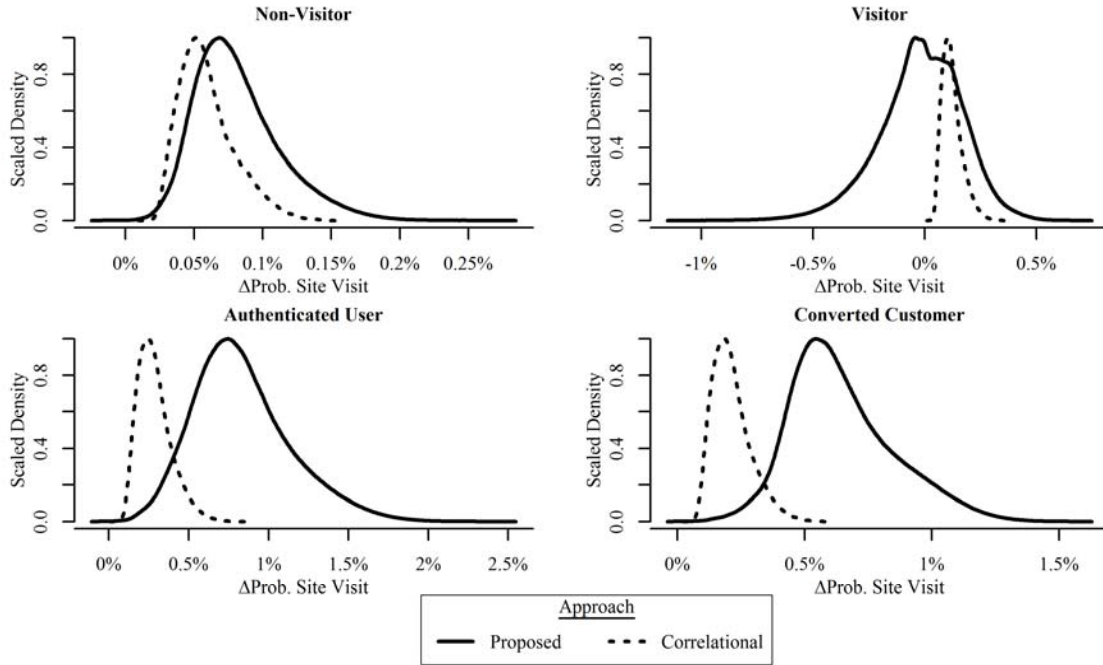


Figure 7: Comparison of marginal effect from one to two impressions resulting from our approach and a correlational approach for each of the funnel stages.

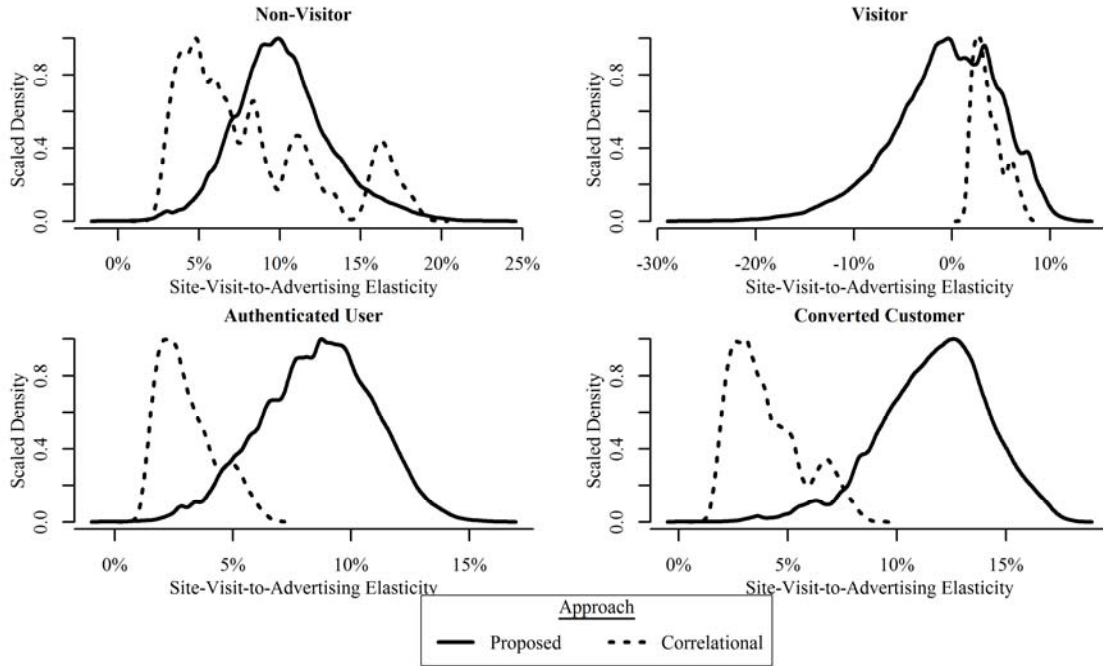


Figure 8: Comparison of elasticity from one to two impressions resulting from our approach and a correlational approach for each of the funnel stages.

1.13 Appendix – Bayesian Inference

To obtain the necessary marginal posterior distributions, we estimated our model using a Bayesian approach. The full-conditional distributions were derived from the joint density and the chosen priors. We obtained the marginal posterior distributions by sequentially drawing 100,000 samples from the full-conditional distributions. Starting points were selected based on a classically estimated model. We kept every 10th iteration to reduce auto-correlation, and used a 10,000 iteration burn-in period to mitigate the effects of the selected starting values.

β is distributed $MVN(\mu_\beta, V_\beta)$. Thus, the full conditional distribution for β is:

$$\Pr(\beta | \mu_\beta, V_\beta, X) \propto \left[\prod \frac{\exp(X\beta)}{1 + \exp(X\beta)} \right] \left[-0.5(\beta - \mu_\beta)' V_\beta^{-1} (\beta - \mu_\beta) \right]$$

Because the full-conditional distribution for β is known only up to a proportionality constant, we use a random walk Metropolis-Hastings algorithm. Candidate values on iteration n are drawn as $\beta^c = \beta^{n-1} + \varepsilon_\beta$, where $\varepsilon_\beta \sim MVN(0, sV_\varepsilon)$. V_ε is the covariance matrix for β estimated asymptotically. s is a scalar chosen to achieve a rejection rate of 50-70%. The candidate value is accepted with probability:

$$\min \left\{ \frac{\Pr(\beta^c | \mu_\beta, V_\beta, X)}{\Pr(\beta^{n-1} | \mu_\beta, V_\beta, X)}, 1 \right\}$$

We set $\mu_\beta = 0$ and $V_\beta = 25I$.

1.14 Appendix – Data Generating Process

The random assignment of individuals to the treatment and control groups is central to our approach. To understand the critical role of the control group, it helps to understand the data generating process. Figure 9 depicts this process. There are several complicating factors at play that make existing correlational approaches problematic. First, there is a feedback loop between prior site visit and both future site visits and advertising. Specifically, product familiarity obtained during a prior site visit may make an individual more likely to revisit through product

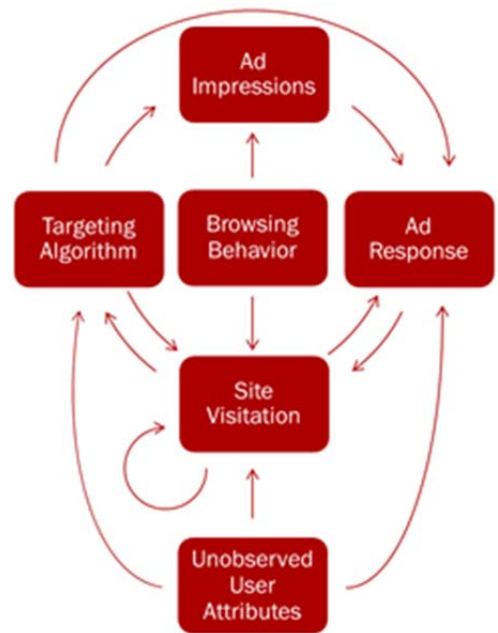


Figure 9: Display Ad Response Data Generating Process.

experience. The visit itself may make the individual more likely to receive advertising impressions based on the ad servers targeting algorithm. Further, prior site visit may change how individuals react to such advertising. Research has shown that brand familiarity influences ad perceptions (Campbell and Keller 2003), and that such influence may not always be positive (Goldfarb and Tucker 2011). Second, the aforementioned targeting algorithms generally include a number of factors that are unobserved by the researcher. Existing methods include everything from browsing behavior on third party sites to the content of emails received by individuals (Ingram 2012). Even algorithms based on mood and body language are in the works (Delo 2012). Third, these unobservable factors may directly impact the probability of site visit in addition to impacting the probability of receiving an ad impression. An email from a friend

recommending that an individual visit a given site may increase the probability that they visit the site, the probability that they receive advertising for that site, both, or neither. Finally, browsing behavior likely impacts both the probability of seeing an ad and the probability of performing any action online, creating a confound unless appropriately measured. That is, when individuals spend more time online, they may be both more likely to be served a given ad and to perform any given activity, *ceteris paribus*, even if the two events are unrelated from the consumer's perspective.

The data commonly available to managers for evaluating display advertising campaigns further complicates the analysis. Firms frequently use tracking cookies to compile individual histories across browsing sessions. Webpages, display ads, and other downloadable content can contain tracking code that looks for a unique cookie ID stored within the browser. This identifier is then sent to a third party along with information such as a time stamp, IP address, the visited URL, and identifiers for the downloaded content (i.e., a number uniquely identifying the display ad shown) (Google 2012). When aggregated over a large number of websites and browsing sessions, these data can provide a rich history of online consumer behavior. However, this full dataset is almost never available to individual firms due to privacy agreements between the third parties and consumers. The subset that firms generally receive is limited to how otherwise anonymous users interacted with only their ads and website. The result is a dataset that is sparse and devoid of individual level demographic information.

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Ch. 2: Trading Reputation for Traffic:

An Examination of Restaurant Daily Deal

Decisions

2.1 Abstract

Online daily deal sites provide a marketplace in which local merchants sell vouchers for goods and services to geographically targeted consumers. After the steep discount and provider's cut, the merchant's take is generally 25% of listed prices. While there are certainly a number of merchants for whom daily deals have been effective, stories of failure are rife in the popular press. In this paper, I examine when and why merchants participate in daily deals, and what influence such participation has on their business. To address these questions, I compiled a unique dataset containing all restaurants in the Los Angeles area, their characteristics and customer reviews as reported by Yelp.com, and their participation in daily deals from March 2010 through July 2012. Using a joint model of participation and outcomes, I am able to control for the selectivity inherent in the data. Further, I explore competitive interactions by leveraging recent advances in empirical games. I find that daily deal launch and offer expiration coincides with a dramatic spike in the volume of reviews and a significant drop in valence, consistent with existing research. Further, I find positive spillover effects such that focal restaurant traffic increases when competitors offer daily deals. I also find positive strategic interactions, such that

a merchant is more likely to participate in a daily deal if they expect their competitors to do so. Finally, I find that merchants consider the potential impact on word of mouth when deciding whether to offer a daily deal. Specifically, merchants appear to make a tradeoff between traffic and reputation, accepting some negative word of mouth in exchange for a sufficient increase in traffic.

2.2 Introduction

Online daily deals represent one of the newest, fastest growing, and most controversial marketing tools available. In 2012, the two largest daily deal providers, Groupon and LivingSocial, reported combined revenues of \$2.87 billion, up 54% year over year (Amazon.com 2013; Groupon 2013). In 2015, consumers will spend an estimated \$5.5 billion on daily deals in the US alone, a 202% increase from 2011 (Pacheco and Udowitz 2012). These numbers are even more impressive when one considers that neither Groupon nor LivingSocial existed prior to 2007.

By building large subscriber bases and managing websites on which offers are listed, daily deal providers serve as market makers for daily deal offers. The listed offers generally come from small, local firms, and represent a 40-60% discount on goods and services (Byers, Mitzenmacher et al. 2012). The offers are listed for a short period of time, generally one to three days, and expire at a prespecified date, usually six months in the future. As their fee, providers take an average of 45% of a voucher's selling price (Dholakia 2012), leaving merchants with 22-33% of the voucher's face value as revenue. For participating merchants, these offers constitute

the single largest annual marketing expense (Dholakia 2011). Given the resulting slim margins, it may be unsurprising that the popular press is rife with stories of daily deals gone bad; including a cupcake shop losing a year's worth of profits (MSNBC 2011), a niche grocer requiring charitable donations to stay afloat (Gelles 2012), and a waffle house being driven out of business (Kurtzleben 2012). Daily deals have been referred to as, "The single worst decision I have ever made as a business owner," (Caldwell 2012) and "... the equivalent of a loan sharking business," (Agrawal 2011).

Reading through these firsthand accounts and speaking with managers, several themes become apparent. First, merchants find that many daily deal customers are less satisfied with their products and services. To the extent that the discounted prices attract marginal consumers, this follows directly from standard economic theory. The result is that these less satisfied customers are unlikely to return at full prices or spend beyond the voucher's value. Second, loyal customers frequently purchase vouchers. This can have negative effects in both the short and long term. In the short term, voucher sales to loyal customers may cannibalize future full price sales. In the long term, such discounts can damage the brand by increasing price sensitivity (Kaul and Wittink 1995), decreasing reference prices (Kalyanaram and Winer 1995), and signaling lower quality (Erdem, Keane et al. 2008). Finally, capacity constraints are real and binding. When merchants don't sufficiently limit the number of available vouchers, they are frequently unable to meet the resulting demand while maintaining existing quality and service levels. This impacts not only the daily deal customers, but also their existing customer base. The result is that daily deals can not only fail to succeed, but they can also do long term damage.

Despite these pitfalls, there are merchants that report great success with these promotions. One restaurant owner in Richmond, VA said, “There is no question that [our daily deal offer] has paid for itself better than any other advertising medium we have ever used,” (Friedman 2011). Such sentiments seem to be supported by the emerging research, which shows that roughly half of past participants intend to run another daily deal, and a third of managers view them as a sustainable business practice (Yarrow 2011; Dholakia 2012). While this falls well below the 80% and 97% merchant repeat rates claimed by LivingSocial and Groupon respectively (Lancellotti-Young 2011; Heine 2013), it does provide evidence that at least some subset of merchants finds daily deals to be beneficial.

Among the positive stories, there are two themes; awareness and trial. The majority of participating merchants are smaller, local businesses, with limited advertising budgets and little in the way of brand awareness. Because providers send daily emails to their subscriber bases highlighting the deals offered, daily deals represent an opportunity for significant exposure at no upfront cost. Beyond simply driving awareness, daily deals increase trial by offering consumers an opportunity to test a product or service at a dramatically lower price. While repeat rates are heavily debated, few argue against daily deals as a driver of trial.

With the significant debate surrounding the efficacy of daily deals, it is interesting to consider how merchants make the decision whether or not to participate. In this paper, I explore three critical factors that influence the merchant participation decision; traffic (as measured through review volume), reputation (as measured through review volume and ratings), and strategic interactions. I also examine how daily deals ultimately impact traffic and reputation, accounting for the endogenous firm decision. My approach utilizes a joint model of firm

participation and outcome, based on existing research into word of mouth, promotion effects, and empirical games. I assume that firms are rational agents, selecting actions in an effort to maximize expected profits. In addition to stable restaurant attributes and the firm's offer decision, I allow these profits to be impacted by the volume and valence of online reviews. This link between word of mouth, sales, and profits is well established within the literature, as will be discussed in section 2.3. I also allow online reviews to be impacted by the merchant's offer decision, in line with the extensive literature on promotion effects. Finally, I allow both firm profits and reviews to be impacted by the daily deal decisions of a firm's competitors, following from the literature on promotion effects and empirical games.

To do all of this, I assembled a unique dataset combining daily deal information from the two largest providers, Groupon and LivingSocial, and word of mouth information from the popular review site Yelp.com. The resulting dataset contains information on 14,620 restaurants in the Los Angeles area, including 1,333 unique offers and 832,701 reviews. My focus on daily deals and use of online reviews to measure outcomes makes restaurants a particularly appealing category. In addition to representing approximately 12% of daily deal industry revenue (Yipit 2012), restaurants are also the second most frequently reviewed category on Yelp.com (Yelp 2013). While other local merchants such as spas may offer a wide variety of products and services, restaurants have a single general offering, prepared and served food, making them easier to categorize and compare. There is also a strong established relationship between restaurant sales and profits and online reviews (Luca 2011; Anderson and Magruder 2012), providing assurance that my outcome measures reflect firm performance.

Based on this approach, I reach four key conclusions. First, traffic increases and reputation decreases significantly when a daily deal is offered. Second, firms anticipate these outcomes when making their decision, and appear to make a tradeoff between reputation and traffic. They are willing to accept some negative word of mouth for additional customers, but the larger the negative reputational impact, the greater the increase in traffic demanded. Third, frequent offers negatively impact perceived quality, with increased offer rates resulting in decreased average review ratings. Fourth, there exist both positive spillover effects on traffic and positive strategic interactions from daily deals. When a firm's competition offers a daily deals, traffic increases regardless of the focal firm's promotion decision, indicating that category expansion effects dominate brand switching in my data. However, simultaneous offers result in greater increases in traffic, and additional profits for the focal firm.

This paper makes several contributions to the existing literature. I am able to extend previous work showing that word of mouth impacts firm outcomes, by showing that firms are aware of this relationship and make critical decisions with reputational ramifications in mind. I am also able to build on the extensive literature on promotion effects, showing that category expansion effects can dominate brand switching in a large and important industry, and providing direct evidence that repeated promotions negatively impact perceived quality. Finally, I leverage recent advances in empirical games to provide evidence of positive strategic interactions in price promotions.

The remainder of this paper is organized as follows. In section 2.3, I review the relevant research on promotion effects, word of mouth, and strategic interactions. In section 2.4, I discuss the data collection and setup. Section 2.5 presents some stylized facts. In section 2.6, I discuss

the modeling approach, identification, and estimation strategy. Section 2.7 discusses the results, and section 2.8 concludes with implications and directions for future research.

2.3 Related Literature

2.3.1 Word of Mouth

There is considerable research showing that both the volume and valence of online reviews drive merchant sales and profits. For instance, Chevalier and Mayzlin (2006) find that more reviews and a higher average rating are positively correlated with sales rank and market share at both BarnesandNoble.com and Amazon.com. Dellarocas, Zhang et al. (2007) find a similar effect for movies, showing that both the volume and valence of reviews posted during the opening week are important predictors of total box office sales. With regard to restaurants, our area of empirical focus, two recent studies have shown strong ties between online reviews, sales, and profits. Noting that Yelp displays average ratings rounded to the nearest half star, both Luca (2011) and Anderson and Magruder (2012) use a regression discontinuity approach to show that a half a star improvement in average Yelp rating leads to 19% more sellouts and 5-9% more revenue. Further, these results are driven by the independent restaurants, about which consumers have few other available information sources (Luca 2011).

These smaller, local firms comprise the core merchant base for daily deal providers, making it critical that we understand the interplay between daily deal offerings and online word of mouth. On a positive note, recent research has shown that review volume tends to spike around daily deal launch and expiration (Byers, Mitzenmacher et al. 2012), and that this pattern

coincides with the redemption of daily deal vouchers (Song, Park et al. 2012). Further, daily deal reviews¹ tend to be for merchants in a novel location or line of business from the reviewers perspective, indicating customer trial (Byers, Mitzenmacher et al. 2012). In contrast to this positive impact on review volume, daily deals appear to be negatively correlated with review valence. Average rating tends to drop around the launch and expiration of a deal, driven in part by daily deal reviews that are in general 10% lower than others (Byers, Mitzenmacher et al. 2012). Thus, daily deals appear to be positively correlated with review volume and negatively correlated with valence.

There are three critical conclusions from the existing word of mouth literature. First, online reviews impact firm performance, with greater review volume and valence leading to improved sales and profits. Second, this link is especially strong for small and medium size restaurants, which comprise a core merchant base for daily deal providers. Third, daily deals are positively correlated with review volume, but have a strong negative correlation with valence. I hope to build on this literature by examining the role of anticipated word of mouth on firm decision making, especially as related to daily deals.

2.3.2 Promotion Effects

In offering deep discounts on products and services, daily deals can be thought of as an extreme form of price promotion, which has long been a core focus of marketing research. In the short term, price promotions have been shown to positively impact sales through category expansion, brand switching, and accelerated consumption (Ailawadi and Neslin 1998; Nijs, Dekimpe et al.

¹ Daily deal reviews are defined as reviews mentioning a daily deal provider in the text.

2001; Pauwels, Hanssens et al. 2002). In the medium term, some of these positive effects are mitigated by purchase acceleration and stockpiling, which can result in a post promotion dip (Gupta 1988; Blattberg, Briesch et al. 1995; Pauwels, Hanssens et al. 2002). In the long term, price promotions have been found to have little persistent effect on sales, especially in the absence of company inertia (Nijs, Dekimpe et al. 2001; Pauwels 2004). However, when such inertia is present, increasing promotion frequency, consumer perceptions are negatively impacted through increased price sensitivity (Kaul and Wittink 1995; Mela, Gupta et al. 1997), decreased reference prices (Kalyanaram and Winer 1995), and diminished perceived quality (Erdem, Keane et al. 2008).

My data provides an opportunity to further study two key questions regarding price promotions. First, by combining promotion information with consumer reviews, I can examine the impact of frequent price promotions on perceived quality, when the latter is directly observed. In past research, perceived quality has been defined as an unobserved heterogeneous term, impacted by price, advertising, and past product purchase (Erdem, Keane et al. 2008). If frequent promotions negatively impact perceived quality, one would expect that average ratings are lower for restaurants that have more frequently offered deals than for their less deal prone counterparts. The second critical question is when category switching effects may dominate those from brand switching?² If category expansion effects dominate, one would expect to see firm traffic increase when a competitor extends an offer. If brand switching dominates, firm traffic should decrease as existing customers accept competitor offers. Understanding these

² Note, stockpiling is not relevant given our empirical application to restaurants. For other daily deal participants such as grocers, this may be a greater concern.

relationships will help us better understand the impact that promotions have on consumer perceptions and firm outcomes.

2.3.3 Strategic Interactions

Past empirical examinations of competitor reactions to promotions has produced mixed, and somewhat conflicting results. Pauwels (2004) finds that passive responses to competitor promotions are rare, with firms generally reacting in either a cooperative or an aggressive manner. In later work, he shows that firms most frequently discount prices in response to a competitor price promotion, partially mitigating the offer's impact. Meanwhile, Nijs, Dekimpe et al. (2001) find that the dominant form of reaction to a competitive price promotion is no reaction. Thus, the existence of strategic interactions in price promotion decisions remains an open question, upon which I hope to shed some light.

To do this, I draw on a rich empirical literature in discrete games, dating back to Bresnahan and Reiss (1991; 1991). In leveraging the incomplete information framework to break a system of equations into single agent discrete choice problems, I follow the work of Bajari, Hong et al. (2010). Using related recent advances in incorporating outcome data into static discrete games (Ellickson and Misra 2012), I am able to jointly model the firm's daily deal decision, as well as the resulting review volume and valence. This joint estimation allows me to see the interdependencies between merchant daily deal decisions and word of mouth outcomes, accounting for anticipated competitor actions.

2.4 Data Collection & Construction

2.4.1 Collection

On the daily deals side, I focus on the two dominant providers, Groupon and LivingSocial, who comprise 70-80% of the market (Pepitone 2012). Using a list of URLs from daily deal aggregator Yipit, I collected information on all 1,333 offers extended by restaurants in the Los Angeles area through either of these providers during the 29 month period from March 3, 2010 to July 31, 2012.³ I gathered offer specific information such as when the deal was posted and when it expired, as well as merchant specific information such as name, address, phone number, merchant website, and Yelp URL where available.

With 86 million unique monthly visitors and roughly 7.5 million restaurant reviews, the review site Yelp.com serves as a valuable source of digital word of mouth information (Yelp 2013). From this site, I collected restaurant attributes such as type of fare and price range, as well as individual review information including when the review was posted and the assigned numeric rating. I focused first on those merchants that had offered a daily deal. The vast majority of these merchants were matched to the daily deal data using either the Yelp URL found on the daily deal site or an exact string match between both the listed name and address. Those that could not be matched programmatically were manually matched based on a combination of name, phone number, address, and website. All merchants were matched using one of these two methods.

³ There is a period from Groupon's Los Angeles launch on July 1, 2010 to the start of our data in which we do not observe daily deal participation by merchants (Groupon 2009).

I then gathered the population of potential competing restaurants using Yelp's "Browse Nearby" tool. For a given merchant, this tool provides a list of Yelp webpages for geographically proximate restaurants. For each of the firms that offered a daily deal, I used this tool to collect information on all restaurants in their vicinity. This process was repeated several times, plugging each new restaurant from the last iteration into the "Browse Nearby" tool, and collecting information on each of the listed alternatives. The result was 832,701 reviews for 14,620 unique restaurants in the Los Angeles area.^{4 5}

Finally, I geocoded each of the restaurants using Microsoft's Map API. Unlike other geocoding tools, Microsoft provides both a location type (i.e., address, intersection, or city center) and match confidence (i.e., low, medium, or high) in their results. These attributes were used to ensure that the latitude and longitude estimates were for a specific address, and returned with high confidence.

2.4.2 Data Setup

Offering a daily deal is not an instantaneous process. Rather, merchants and providers work together to plan the promotion for some future period, usually a few weeks off. Frequently, the merchant is only guaranteed that the deal will run within some prespecified window, meaning advance planning is necessary (Gupta, Weaver et al. 2011). With this in mind, I discretize the data to the merchant-month level, resulting in 423,980 observations.

⁴ Because we only observe daily deals within the Los Angeles Area, we limit the pool of potential competitors to those within a zip code that is at least partially within Los Angeles County. This reduces the risk that a competitor ran a daily deal which we do not observe.

⁵ A large number of reviews posted prior to March 3, 2010 were also collected. I use these to calculate the relevant state variables and apply the necessary filters as discussed in section 2.4.2.

For restaurants, two characteristics of the data make analyzing daily deal effects difficult. First, Yelp data is largely user generated, resulting in a fair number of stub files. These stub files commonly stem from user error (i.e., incorrectly identifying a business name). Second, new restaurants have a high failure rate, with 26% closing their doors in the first year (Parsa, Self et al. 2005). This failure rate is higher for independent restaurants than for chains, and frequently has nothing to do with the firm's performance (Parsa 2003). In an effort to mitigate the impact of these factors on the results, I filtered out merchants with fewer than 5 total reviews. This represented 0.6% of reviews and 19.4% of restaurants, including 3.3% of restaurants that extended offers. After applying this filter, the data contains 311,758 observations on 11,791 restaurants, including 827,794 reviews and 1,276 offers.

Three additional covariates cause a reduction in the total number of observations. First, average rating is not defined for merchant-months in which no reviews are posted. This impacted 111,464 observations, representing 35.8% of my data. Similarly, the average of all past ratings requires at least one past review. This affected 2,374 (0.8%) of the observations. Finally, pricing information was unavailable for a small number of restaurants, further reducing the data by 2,669 observations (0.9%). Taken together, these result in a 43% reduction in the number of observations. The final data set contains 197,427 observations on 11,614 merchants, including 815,932 reviews and 1,152 offers.

2.4.3 Defining the Competitive Set

Examining strategic interactions requires a clearly defined competitive set. I use three key restaurant attributes to define the competitive set; cuisine type, price, and location. Within a Yelp

listing, restaurants are tagged with the types of cuisine they serve. Common examples include Italian, Chinese, and pizza. Note, these categories are not mutually exclusive; a restaurant could be categorized as serving Italian food and pizza simultaneously. Within a cuisine type, there is considerable variability with regard to the price point at which restaurants operate. Price likely helps define the competitive set directly by narrowing the customer set for which the restaurant competes, and indirectly through its correlation with quality. While both Taco Bell and Rick Bayless' Red O Restaurant serve Mexican food, the disparity in price makes it unlikely that consumers view them as substitutes. Consequently, it is unlikely that they compete with each other. Finally, location impacts competition. I assume a trading radius of 1 mile, meaning that firms must be no more than 1 mile apart to be deemed competitors. While this may seem like a tight radius in terms of distance, the population of restaurants is drawn from the Los Angeles metropolitan area, meaning this trading radius covers a considerable distance in terms of travel time. As a robustness check, I have also analyzed the data based on a two mile trading radius, with no meaningful impact on the results.

Because of both the trading radius assumption and the fact that cuisine types are not mutually exclusive, the competitive relationships are not transitive. Figure 1 depicts this non-transitive relationship. Here, restaurant A serves both Japanese food and sushi, restaurant B is a sushi bar located a half mile West of restaurant A, and restaurant C is a Japanese restaurant located a half mile East of restaurant A. Despite being located within one mile of each other and both competing with restaurant A, restaurants B and C are not considered competitors because they do not share a cuisine type. Further, restaurant D is a sushi bar located a half mile east of restaurant C. Despite competing with restaurant A and sharing a cuisine type with restaurant B, it does not

compete with restaurant B because it is outside restaurant B's one mile trading radius. This non-transitive nature of the competitive sets will be used to identify the beliefs over competitor actions, as discussed in section 2.6.4.

2.5 Stylized Facts

Table 1 contains summary statistics for some key variables, drawn from the estimation data set. The first four rows contain covariates aggregated to the merchant level, including number of competitors, number of reviews, average rating, and number of offers. The next three rows are similar, except aggregated to the month instead of merchant level. This table provides some critical insights into the nature of the data. First, offering a daily deal is a relatively rare event, with only 6.46% of merchants extending an offer. Second, there is a fairly significant amount of variation in average rating across restaurants, with a range of 1.00 to 5.00. Finally, most restaurants face only a handful of direct competitors.⁶

In line with recent research, my data supports a strong relationship between daily deals and word of mouth, with review volume increasing and valence decreasing during the offer period. Figure 2 reflects this correlational relationship within the entire dataset. The line represents the thirty day moving average of rating, reset at the start and end of the deal. At these points, the drop in average rating is clear. Note that the dip following deal expiration likely reflects a delay in review posting by those that visited just before deal expiration. The gray bars at the bottom represent the daily review volume, where dramatic increases are apparent at the beginning and

⁶ This is admittedly a byproduct of the tight trading radius assumption. While expanding the trading radius increases the average number of competitors, it does not materially impact the findings.

end of the deal period. The black bars represent those reviews that specifically mention a daily deal provider. The corresponding increase in the volume of these reviews provides additional evidence that the shift in reviews is driven by the daily deals.

Beyond these immediate effects on word of mouth, there are also indications that over time daily deals can negatively impact customer perceptions. Table 2 summarizes the results from a simple regression of average rating on the number of past offers. The significant, negative coefficient on the number of past offers indicates that firms with multiple past offers receive worse ratings than their counterparts. This is in line with the findings of Erdem, Keane et al. (2008), discussed in section 2.3.

In addition to negatively impacting perceived quality, daily deal offers can also positively impact traffic at competing firms. Table 3 reports results from a similar linear regression of the number of reviews posted for a firm on a binary indicator of whether the firm offered a daily deal and a count of the number of competitor deals offered. The positive, significant coefficient on the number of competitor offers suggests positive spillovers in firm traffic as a result of competitor offers.

Despite these potentially negative outcomes from offering a daily deal, I find evidence that at least some subset of merchants find daily deals to be beneficial. Figure 3 contains the number of offers per month across all 423,980 observations. The gray portion represents offers by restaurants that have not previously participated in a daily deal, while the black portion represents offers from those that have. Two facts are immediately apparent. First, the number of deals offered in a given month has been roughly constant since the beginning of 2011. Second, the percentage of offers extended by restaurants with previous daily deal experience has

increased steadily over this same period. This growth in offers from past participants indicates that firms have found past offers to be profitable, and are thus willing to repeat. This latter point runs contrary to the majority of anecdotal evidence in the popular press, though it is in line with the recent research on daily deals discussed in section 2.2.

Strategic interactions also appear to impact the merchant offer decision. Table 4 reports the results from a linear probability model, with a binary indicator of firm offer regressed on the number of reviews posted for the focal firm, the average rating of these reviews, the log transformed number of competitors⁷, and the number of offers extended by competitors. Controlling for the focal firm outcomes and competitive set size, competitor offers are positively correlated with daily deal offers, indicating positive strategic interactions.

While interesting, this model free evidence is not without its limitations. These correlations largely ignore the complex system of relationships between daily deals, word of mouth, and firm decision making. Further, the firm's offer decision is not random, but rather involves various trade-offs in an effort to maximize expected profits. Understanding these trade-offs requires estimating counterfactuals, the expected outcomes given the alternative choice, at each decision point for each firm. In order to estimate these counterfactuals, I turn to a joint structural model of review volume, average ratings, and firm profits.

⁷ I add one prior to taking the logarithm in order to avoid taking the logarithm of zero when no competitors are observed.

2.6 Approach

2.6.1 The Firm's Decision

At the start of each time period t , the firm must decide whether to offer a daily deal ($k = 1$) or not ($k = 0$).⁸ Let π_{it}^k represent the profits earned by merchant i during period t from selecting action k . Let profits be a linear function of the firm's daily deal decision (a_{it}), a set of restaurant attributes ($x_{\pi it}$), traffic (T_{it}), reputation (R_{it}), competitor daily deal decisions (a_{jt}), and a private information term (η_{it}), such that

$$\pi_{it}^k = x'_{\pi it} \beta_x^k + T_{it}^k \beta_T + R_{it}^k \beta_R + \left[\sum_{j \in C_i} a_{jt} \right] \beta_A^k + \eta_{it}^k \quad (1)$$

Where C_i is the competitive set for firm i . Moving forward, let A_{-it} represent the number of competitor offers, such that $A_{-it} = \sum_{j \in C_i} a_{jt}$. I assume that the merchants are rational agents, seeking to maximize expected profits, and thus selecting an action such that

$$\mathbb{E}[\pi_{it}^k] \geq \mathbb{E}[\pi_{it}^{k'}] \quad \forall k, k'$$

There exist three critical determinants of firm profits that are revealed only after the firm selects an action; traffic (T_{it}), reputation (R_{it}), and competitor actions (A_{-it}). Because these are not known in value *a-priori*, actions must be selected based on an expectation of each. Thus, the firm's decision is based on its expected profit function

$$\mathbb{E}[\pi_{it}^k] = x'_{\pi it} \beta_x^k + \mathbb{E}[T_{it}^k] \beta_T + \mathbb{E}[R_{it}^k] \beta_R + \mathbb{E}[A_{-it}] \beta_A^k + \eta_{it}^k \quad (2)$$

⁸ This empirical setup reflects reality quite closely. As discussed in section 2.4.2, running a daily deal is not an instantaneous process, and frequently the merchant is only promised a window in which the offer will be published.

These expectations are discussed in detail in section 2.6.2, but first I turn to the precise specification of the firm's decision function.

Note that these expected profits are never directly observed, but rather differences in expected profits are revealed through merchant actions. Further, these differences are only identified up to some scaling constant. The change in expected profit associated with running a daily deal is then

$$\begin{aligned}\Delta\mathbb{E}[\pi_{it}^1] &= \mathbb{E}[\pi_{it}^1] - \mathbb{E}[\pi_{it}^0] \\ &= x'_{\pi it}\Delta\beta_x + \Delta\mathbb{E}[T_{it}]\beta_T + \Delta\mathbb{E}[R_{it}]\beta_R + \mathbb{E}[A_{-it}]\Delta\beta_A + \Delta\eta_{it}^k\end{aligned}\quad (3)$$

Where,

$$\begin{aligned}\Delta\beta_x &= \beta_x^1 - \beta_x^0 \\ \Delta\mathbb{E}[T_{it}] &= \mathbb{E}[T_{it}^1] - \mathbb{E}[T_{it}^0] \\ \Delta\mathbb{E}[R_{it}] &= \mathbb{E}[R_{it}^1] - \mathbb{E}[R_{it}^0] \\ \Delta\beta_A &= \beta_A^1 - \beta_A^0 \\ \Delta\eta_{it} &= \eta_{it}^1 - \eta_{it}^0\end{aligned}$$

I specify η_{it}^k as the sum of two components; $\xi_{\pi it}^k$ which follows a standard normal distribution and ε_{it}^k which is distributed extreme value type I. As is standard in the choice literature, I normalize the profit from one option (in this case the no offer option) to zero. Integrating over $\Delta\varepsilon_{it}^k$, the probability that firm i selects action k at time t is a binary logit mixture model, such that

$$\Pr(a_{it} = 1 \mid A_{-it}, x_{\pi it}, \Delta\mathbb{E}[T_{it}], \Delta\mathbb{E}[R_{it}], \Delta\xi_{\pi it}; \beta) = \frac{\exp(\Delta\mathbb{E}[\tilde{\pi}_{it}^1])}{1 + \exp(\Delta\mathbb{E}[\tilde{\pi}_{it}^1])}\quad (4)$$

Where $\Delta\mathbb{E}[\tilde{\pi}_{it}^1] = \Delta\mathbb{E}[\pi_{it}^1] - \Delta\varepsilon_{it}^1$.

2.6.2 Specifying Firm Expectations

2.6.2.1 Traffic

Let T_{it}^k represent the traffic for restaurant i during period t conditional on action k . Because sales figures are not available at the merchant level, I use the number of reviews posted during a period as a proxy for traffic. As discussed in section 2.3, review volume and sales are, at a minimum, significantly correlated. Thus, I assume $T_{it}^k = n_{it}^k$, where n_{it}^k is the number of reviews posted for restaurant i during period t given action k .

Because the number of reviews posted is by definition a count variable, I model it as a Poisson process with mean λ_{it}^k . I allow λ_{it}^k to be influenced by several state variables (x_{nit}), the number of competitors offering a deal (A_{-it}), and an iid normal, mean zero private information term (ξ_{Tit}^k). By incorporating the number of competitor offers, I allow the model sufficient flexibility to capture shifts in focal restaurant traffic as a direct result competitor offers. If category expansion dominates brand switching, competitor offers will increase focal merchant traffic, and this coefficient will be positive and significant. If brand switching plays a dominant role, competitor offers will negatively impact focal restaurant traffic, and this coefficient will be negative and significant. Given these inputs, review count is assumed to follow a Poisson process with mean λ_{it}^k , such that

$$\log(\lambda_{it}^k) = x'_{Tit}\theta_x^k + A_{-it}\theta_A^k + \xi_{Tit}^k \quad (5)$$

2.6.2.2 *Reputation & Ratings*

Merriam-Webster (2013) defines reputation as the, “Overall quality or character as seen or judged by people in general.” Accordingly, I specify reputation to be a function of both the number of reviews posted during a period and the average rating of those reviews. The average rating reflects the holistic evaluation provided by individuals, and the number of reviews reflects how generalizable these opinions are.⁹

It is important that ratings during any period be measured relative to some baseline for two reasons. First, ratings on Yelp are bounded between one and five, meaning the raw scores are always positive. However, a one star rating is certainly not a positive outcome for a firm. Second, the relative value of a review to the firm may depend on their existing ratings. For example, a three star review may improve the reputation of a restaurant with a two star average, but harm that of a restaurant with a four star average.

Review volume is assumed to magnify the reputational impact of average ratings by increasing their generalizability. The number of reviews posted reflects the sample size from which average rating is calculated, and thus review volume is negatively related to the associated margin of error. To the extent that consumers recognize this relationship, additional reviews should increase confidence that the observed average is an accurate reflection of the true average, magnifying its impact.

⁹ I am looking for an accurate assessment of a firm’s reputation, which may differ from more objective measures of firm quality. For more on this distinction, see Hu, Pavlou et al. (2006)

Given an action k , I specify the firm's reputation during any period to be the volume weighted difference between the average rating of reviews posted during that period and the cumulative average rating over all prior periods, or

$$R_{it}^k = n_{it}^k (r_{it}^k - \bar{r}_{it-}) \quad (6)$$

Where n_{it}^k is the number of reviews, r_{it}^k is the average rating of those reviews, and \bar{r}_{it-} is the average rating for all reviews up to period t . Note that the firm's promotion decision is expected to impact reputation through both review volume and valence, as represented by the superscripted k .

With the exception of \bar{r}_{it-} , the inputs to reputation are unobserved by the firm prior to selecting an action. Consequently, they must act based on some expected value of each. Because review count proxies for traffic, $T_{it}^k = n_{it}^k$, and I use the model as described in section 2.6.2.1 to predict review volume. I assume that average ratings are normally distributed, and cast them as a linear function of the restaurant attributes (x_{rit}), a private information term (ξ_{rit}^k), and an expectation error (η_{rit}^k) such that

$$r_{it}^k = x'_{rit} \gamma_x^k + \xi_{rit}^k + \eta_{rit}^k \quad (7)$$

I assume that the error components in equation (7) (ξ_{rit}^k and η_{rit}^k) are iid normal with mean zero, and are independent of each other.

2.6.2.3 *Handling Self Selection & Correlated Private Information*

Because I only observe ratings information for the chosen action, there is a selectivity problem. Merchants take the action that maximizes expected profits, based in part on the private

information that impacts traffic and ratings. Consequently, it is generally true that $\mathbb{E}[\xi_{Tit}^1 | a_{it} = k] \neq \mathbb{E}[\xi_{Tit}^0 | a_{it} = k] \neq \mathbb{E}[\omega_{rit}^1 | a_{it} = k] \neq \mathbb{E}[\omega_{rit}^0 | a_{it} = k] \neq 0 \forall k \in \{0,1\}$. Because expectation errors are independent of private information, the ratings inequalities clearly stem from ξ_{rit}^1 , and ξ_{rit}^0 . Further, one could imagine that the private information influencing firm profits, traffic, and ratings may be correlated. For example, the upcoming addition of a talented new chef may be expected to positively impact all three. Under such a scenario, $\mathbb{E}[\Delta\xi_{\pi it} | \xi_{Tit}^1, \xi_{Tit}^0, \xi_{rit}^1, \xi_{rit}^0] \neq 0$ in general.

To account for these biases, I allow the firm's normally distributed private information with regard to profits ($\Delta\xi_{\pi it}$) and traffic (ξ_{Tit}^1 and ξ_{Tit}^0) to be jointly normally distributed with the error components in ratings (ω_{rit}^1 and ω_{rit}^0)¹⁰, such that

$$\begin{pmatrix} \omega_{rit}^1 \\ \omega_{rit}^0 \\ \xi_{Tit}^1 \\ \xi_{Tit}^0 \\ \Delta\xi_{\pi it} \end{pmatrix} \sim \mathcal{N} \left(\begin{pmatrix} 0 \\ 0 \\ 0 \\ 0 \\ 0 \end{pmatrix}, \begin{bmatrix} \sigma_{r_1}^2 & \sigma_{r_1 r_0} & \sigma_{r_1 T_1} & \sigma_{r_1 T_0} & \sigma_{r_1 \pi} \\ & \sigma_{r_0}^2 & \sigma_{r_0 T_1} & \sigma_{r_0 T_0} & \sigma_{r_0 \pi} \\ & & \sigma_{T_1}^2 & \sigma_{T_1 T_0} & \sigma_{T_1 \pi} \\ & & & \sigma_{T_0}^2 & \sigma_{T_0 \pi} \\ & & & & \sigma_{\pi}^2 \end{bmatrix} \right) \quad (8)$$

Because the firm's offer decision is mutually exclusive (i.e., a firm cannot simultaneously offer and not offer a daily deal), $\sigma_{r_1 r_0}$, $\sigma_{r_1 T_0}$, $\sigma_{r_0 T_1}$, and $\sigma_{T_1 T_0}$ are not estimable, and I set each to zero. Because the profit equation parameters are identified only up to a scaling constant, I fix the variance of σ_{π}^2 at one, as is standard in the discrete choice literature. Under these normalizations and letting Σ represent the covariance matrix in equation (8),

¹⁰ I assume that $\Delta\varepsilon_{\pi it}$, the logit error described in section 2.6.1, is independent of the other private information terms, and that all expectation errors are independent of each other and the firm's private information.

$$\Sigma = \begin{bmatrix} \sigma_{r_1}^2 & 0 & \sigma_{r_1 T_1} & 0 & \sigma_{r_1 \pi} \\ & \sigma_{r_0}^2 & 0 & \sigma_{r_0 T_0} & \sigma_{r_0 \pi} \\ & & \sigma_{T_1}^2 & 0 & \sigma_{T_1 \pi} \\ & & & \sigma_{T_0}^2 & \sigma_{T_0 \pi} \\ & & & & 1 \end{bmatrix}$$

2.6.2.4 Beliefs over Competitor Actions

Let s_t be a vector of commonly observed state variables describing both the focal firm and its competition entering period t , and let ζ_{it} represent the focal firm's private information signal, a combination of $\Delta\eta_{it}^k$, $\Delta\xi_{\pi it}$, ξ_{rit}^1 , ξ_{rit}^0 , ξ_{Tit}^1 , and ξ_{Tit}^0 . As described in equation (1), firm profits are impacted by competitor actions and the private information signal which is observed by the focal firm alone. Because of this private information term, firms cannot perfectly predict competitor actions. Given this setup, the firm's decision rule, $a_{it} = \delta_{it}(s_t, \zeta_{it})$, is a function of the common state vector and its own private information, but not competitors' private information, ζ_{-it} , which are, by definition, unobserved by the focal firm. Thus, from the perspective of its competitors, the probability that firm i offers a deal at time t is

$$\Pr_{it}(a_{it} = k | s_t) = \int 1\{\delta_{it}(s_t, \zeta_{it}) = k\} dF(\zeta_{it}) \quad (9)$$

Where $1\{\delta_{it}(s_t, \zeta_{it}) = k\}$ is an indicator function equal to one if firm i selects action k at time t and zero otherwise. Let \mathbb{P}_{it} represent this probability moving forward.

Anticipated competitor actions enter the firm's profit function in two places. First, the expected number of competitors to offer a daily deal enters directly, as seen in equation (1). Because the profit function is assumed to be linear, $\mathbb{E}_{A_{-it}}[\pi_{it}^k(\cdot, A_{-it})] = \pi_{it}^k(\cdot, \mathbb{E}[A_{-it}])$,

meaning that the expectation can be plugged in to the profit function. From the firm's perspective, this expectation is

$$\mathbb{E}[A_{-it}] = \mathbb{E} \left[\sum_{j \in C_i} a_{jt} \right] = \sum_{j \in C_i} \Pr_{it}(a_{jt} = k \mid s_t) \quad (10)$$

The forecasted number of competitors to offer a daily deal also enters the profit function indirectly through its impact on expected traffic (and consequently expected reputation). While it is unclear *a-priori* whether this indirect impact will be positive (i.e., category expansion) or negative (i.e., shifted share), it is clear that the model should be able to accommodate such an indirect effect. Unfortunately, traffic is not a linear function, and we cannot appeal to the linearity of the expectation operator as above. That is $\mathbb{E}_{A_{-it}}[T_{it}^k(\cdot, A_{-it})] \neq T_{it}^k(\cdot, \mathbb{E}[A_{-it}])$. However, the Poisson distribution does have a convenient structure such that $\mathbb{E}_{A_{-it}}[T_{it}^k(\lambda_{it}^k)] = \mathbb{E}_{A_{-it}}[\lambda_{it}^k]$. Because A_{-it} is a discrete random variable with support $\{0, \dots, |C_i|\}$, where $|C_i|$ is the number of firms competing with firm i

$$\mathbb{E}_{A_{-it}}[\lambda_{it}^k] = \sum_{c=0}^{|C_i|} \lambda_{it}^k(\cdot, \mathbb{P}_{it}) \Pr(\mathbb{P}_{it} | s_t) \quad (11)$$

The calculation of $\Pr(\mathbb{P}_{it} \mid s_t)$, the last term in equation (11) above, is detailed in section 2.6.5.1 below.

2.6.3 Pseudo-Likelihood

For notational simplicity, let the likelihood contribution based on the firm's choice, the observed average rating, and the observed traffic outcomes be represented by $\Psi_{it}(k)$, $\Gamma_{it}(k)$, and $\Omega_{it}(k)$.

Let these functions be

$$\begin{aligned}\Psi_{it}(k) &= \Pr(a_{it} = k \mid \mathbb{E}[A_{-it}], \Delta\mathbb{E}[T_{it}], \Delta\mathbb{E}[R_{it}], x_{\pi it}, \Delta\xi_{\pi it}; \beta) \\ \Gamma_{it}(k) &= f(r_{it}^k \mid x_{rit}, \Delta\xi_{\pi it}, \xi_{T_{it}}^1, \xi_{T_{it}}^0; \gamma^k) \\ \Omega_{it}(k) &= \Pr(T_{it}^k \mid A_{-it}^*, x_{T_{it}}, \xi_{T_{it}}^k; \theta^k)\end{aligned}$$

Where A_{-it}^* is the number of competitors observed to offer a daily deal. Conditional on the firm beliefs over competitor actions, the likelihood of the remaining parameters can then be written as

$$L = \prod_i \prod_t \int_{\Delta\xi_{\pi it}} \int_{\xi_{T_{it}}^1} \int_{\xi_{T_{it}}^0} \left[[\Psi_{it}(1)\Gamma_{it}(1)\Omega_{it}(1)]^{1(a_{it}^*=1)} \times [\Psi_{it}(0)\Gamma_{it}(0)\Omega_{it}(0)]^{1(a_{it}^*=0)} \right] dF(\Delta\xi_{\pi it}, \xi_{T_{it}}^1, \xi_{T_{it}}^0) \quad (12)$$

Where, a_{it}^* represents the observed choice.

It is important to consider the information upon which each of the three parts (traffic, ratings, and offer decision) are estimated in equation (12). The ratings parameters are estimated based on the focal firm's selected action ($a_{it}^* = k$), a set of restaurant attributes (x_{rit}), and the observed average rating conditional on the selected action (r_{it}^k). Similarly, the traffic parameters are based on the focal firm's selected action ($a_{it}^* = k$), the observed number of competitor offers (A_{-it}^*), the focal restaurant attributes $x_{\pi it}$, and the resulting number of reviews conditional on the selected action (T_{it}^k). Note that for both of these portions of the pseudo-likelihood, the *a-posteriori* observed values are used as inputs.

In contrast, the expected profit function parameters are estimated based on the focal restaurant attributes ($x_{\pi it}$), the observed merchant decision (a_{it}^*), the firm's expectations over

the number of competitors that will extend an offer $\mathbb{E}[A_{-it}]$, and the anticipated change in traffic and reputation resulting from each scenario ($\Delta\mathbb{E}[T_{it}^k]$ and $\Delta\mathbb{E}[R_{it}^k]$ respectively). This is in line with the information available to the firm at the decision point, as discussed in section 2.6.1.

The relationship between expected and realized outcomes creates three distinct feedback loops within the likelihood function. First, the observed average rating enters the likelihood directly through $\Gamma_{it}(k)$, but also indirectly through its impact on the firm's decision via the change expected reputation. Second, traffic enters the likelihood in three places; directly through $\Omega_{it}(k)$, indirectly through its impact on the firm's decision, and indirectly through its impact on reputation. Third, expected competitor actions enter the likelihood in four places; the same three places as traffic enters, and indirectly through their influence on the firm's decision.

2.6.4 Identification

Identification of the strategic effects requires an explicit exclusion restriction across firms. Put more simply, there must exist one or more (preferably continuous) covariates that impact the firm's belief over competitive offer probabilities, but do not impact the firm's profit directly. Because the competitive sets are intransitive, market characteristics are idiosyncratic to each firm. Thus, a competitor's idiosyncratic market level parameters, such as number of competitors, aggregate review count, and aggregate average rating, influence the focal firm's beliefs over their actions, but have no direct impact on the focal firm's profit.

In addition to the across firm restrictions necessary to identify the strategic effects, within firm exclusion restrictions are needed to separately identify the profit function parameters from

those of the traffic and rating equations.¹¹ First, I need a covariate that impacts firm profits directly, but not contemporaneous traffic or ratings. Competitive intensity, measured here as the log transformed number of competitors, meets this criterion. Standard economic theory indicates that prices and profits both decrease as competition intensifies. Thus, competitive intensity influences firm profits directly. However, theory also reveals that these price reductions coincide with increases in volume, suggesting that competitive intensity directly impacts traffic. However, capacity is relatively fixed for restaurants in all but the long term.¹² Thus, restaurant traffic is not expected to be influenced by contemporaneous changes in competitive intensity. Conditional on visiting a restaurant, it is unclear why competitive intensity would impact a customer's experience, and consequently rating. Beyond competitive intensity, I use the number of competitor offers to separately identify the profit and ratings parameters. Clearly, competitor offers could impact traffic at the focal firm, either positively through spillover or negatively through brand switching. It may also impact firm profits directly through strategic interactions. However, competitor offers are unlikely to impact a customer's experience once they are at the focal merchant.

In addition to these exclusion restrictions for profit, I need a covariate that impacts traffic and ratings directly, but firm profits only indirectly through these relationships. Serving as a signal of restaurant quality, the average rating of all prior reviews meets this criterion. As discussed in section 2.3.1, past research has consistently shown that review valence impacts future sales

¹¹ In a strict sense, there is no collinearity issue with regard to restaurant traffic. The assumed functional form and error distributions are sufficient to separately identify the parameter sets. I utilize available exclusion restrictions only to strengthen this identification.

¹² I assume here that time of day plays a critical role in consumer dining decisions (i.e., dinner at midnight and dinner at 7:00 pm are not substitutes). Thus, extended hours are not the same as expanded capacity.

volume, represented here by restaurant traffic. To the extent that past quality and current quality are correlated, past reviews are also expected to impact customer experience, and through this relationship average rating. Except through its impact on the number of customers and the quality of their experience, it is unclear how past average ratings would impact firm profits.

It is important to note that I have assumed contemporaneous traffic and ratings to be conditionally independent. That is, average rating in a given period is not influenced by the number of visitors, and vice versa, conditional on the included covariates. As described above, restaurant quality, and consequently past average ratings, are expected to influence both current period traffic and ratings. Similarly, the restaurant's promotion decision is expected to influence both. As a result, ratings and traffic are allowed to vary with both past average ratings and the daily deal decision.

2.6.5 Estimation

The estimation strategy proceeds in two steps, following much of the recent literature on static games (Bajari, Hong et al. 2010; Ellickson and Misra 2012). In the first step, I estimate firm beliefs over competitor actions via conditional choice probabilities (CCPs). In the second stage, I jointly estimate the firm's profit, traffic, and rating parameters.

2.6.5.1 Conditional Choice Probabilities

As in the extant literature, it is critical that the first stage produce consistent estimates of the CCPs, as these are used to construct firm beliefs over competitor actions. Given all discrete variables or a small number of continuous variables, there are numerous nonparametric methods

by which these could be estimated. However, the inclusion of a large number of continuous covariates precludes such an approach, and consequently I turn to a highly flexible, semi-parametric approach. Similar to Ellickson and Misra (2012), I estimate the first stage using a highly flexible binary logit specification, including higher order terms and a full set of bivariate interactions.

As discussed in section 0, these beliefs over competitor actions enter the profit function directly, as well as indirectly through the expected traffic. The direct component is straightforward, and I calculate the expected number of competitors to offer a deal using equation (10). The indirect component requires calculating $\Pr(\mathbb{P}_{it} | s)$, the probability of observing any number of competitors entering, from equation (11). Note that this probability contains two sources of variability. The first is the probability that any given competitor offers a daily deal, A_{-it} . The second is the number of competitors that will extend an offer (i.e., if there are three competitors, multiple combinations can result in two competitors extending offers). To account for both of these sources, I simulate 100,000 entry opportunities for each competitor at each observation, and sum the result over competitors to create random draws from the distribution. I then use this empirical distribution to calculate $\Pr(\mathbb{P}_{it} | s)$.

Because forecasted competitor actions impact firm profits, missing values within the competitive set present a special challenge. One alternative would be to ignore these observations when estimating firm beliefs regarding competitors. This assumes that merchant actions are only impacted by firms for which a full set of covariates are available, and all others are ignored. This seems somewhat unrealistic. More likely, merchants use available information, such as the population average, to infer some value for the missing covariates, and base their

actions on the resulting beliefs. While I have estimated both approaches with no meaningful differences in the results, I present results based on the latter approach. Specifically, I estimate the CCP parameters based on complete observations. Missing observations are then replaced with the population average prior to calculating CCPs. In this way, merchant promotion decisions are allowed to vary based on the beliefs over all competitors.

2.6.5.2 *Structural Parameters*

Given the beliefs over competitor actions, equation (12) is maximized by simulated maximum likelihood. For a given set of parameters, each likelihood calculation proceeds as follows. First, 500 multivariate Halton sequence draws of $\Delta\xi_{\pi it}$, $\xi_{T it}^1$, and $\xi_{T it}^0$ are generated from their joint marginal density

$$\begin{pmatrix} \xi_{T it}^1 \\ \xi_{T it}^0 \\ \Delta\xi_{\pi it} \end{pmatrix} \sim \mathcal{N} \left(\begin{pmatrix} 0 \\ 0 \\ 0 \end{pmatrix}, \begin{bmatrix} \sigma_{T_1}^2 & 0 & \sigma_{T_1\pi} \\ & \sigma_{T_0}^2 & \sigma_{T_0\pi} \\ & & 1 \end{bmatrix} \right)$$

for each observation.

Conditional on these draws, I calculate the expected traffic and ratings outcomes for each observation under each scenario. Given the draws of ξ_{it}^k , the restaurant attributes, the anticipated competitor actions, and the current parameter values, calculating the expected traffic follows directly from equation (5). Calculating the expected ratings conditional on these draws is less straightforward. From equation (7), it is clear that

$$\mathbb{E}[r_{it}^k | x_{rit}, \gamma_x^k, \Delta\xi_{\pi it}, \xi_{T it}^1, \xi_{T it}^0] = x'_{rit} \gamma_x^k + \mathbb{E}[\omega_{rit}^k | \Delta\xi_{\pi it}, \xi_{T it}^1, \xi_{T it}^0]$$

Given these draws and the joint distribution of ω_{rit}^1 , ω_{rit}^0 , ξ_{Tit}^1 , ξ_{Tit}^0 , and $\Delta\xi_{\pi it}$ in equation (8), the derivation of the marginal conditional expectation of ω_{rit}^1 and ω_{rit}^0 is detailed in Appendix 2.11. Because private information and expectation shocks are assumed independent and $\mathbb{E}[\eta_{rit}^k] = 0$, $\mathbb{E}[\omega_{rit}^k | \Delta\xi_{\pi it}, \xi_{Tit}^1, \xi_{Tit}^0] = \mathbb{E}[\xi_{rit}^k | \Delta\xi_{\pi it}, \xi_{Tit}^1, \xi_{Tit}^0]$. Consequently, I can recover the firm's private information for both the selected and unselected actions.

Given these expectations, I can calculate the likelihood contribution from the selected action and resulting traffic and ratings outcomes. For the firm's decision ($\Psi_{it}(k)$) and the observed traffic ($\Omega_{it}(k)$), all of the inputs are available, and this calculation is straight forward. Observed average rating ($\Gamma_{it}(k)$) requires a change of variables.

$$\begin{aligned}\Gamma_{it}(k) &= f(r_{it}^k | x_{rit}, \Delta\xi_{\pi it}, \xi_{Tit}^1, \xi_{Tit}^0; \gamma^k) \\ &= f(x'_{rit}\gamma_x^k + \omega_{rit}^k | x_{rit}, \Delta\xi_{\pi it}, \xi_{Tit}^1, \xi_{Tit}^0; \gamma^k) \\ &= \mathcal{N}\left(x'_{rit}\gamma_x^k + \mathbb{E}[\xi_{rit}^k | \Delta\xi_{\pi it}, \xi_{Tit}^1, \xi_{Tit}^0], \Sigma(\omega_{rit}^k | \Delta\xi_{\pi it}, \xi_{Tit}^1, \xi_{Tit}^0)\right)\end{aligned}$$

Where the second equality follows from equation (7), and the third follows from the conditional distribution of ω_{rit}^k described in Appendix 2.11 and the discussion immediately above.

2.7 Results

2.7.1 Average Rating

As in the model free evidence, I find that offering a daily deal detracts significantly from average ratings. Table 5 contains the parameter estimates for this portion of the model, as well as the associated standard errors and significance levels. The parameter estimates for the offer and no

offer scenarios are in columns two and three respectively. All coefficients are significant at the $\alpha = 0.05$ level. Column four contains the difference in these estimates, all of which are significant at the $\alpha = 0.001$ level.

Between the offer and no offer scenarios, the difference in coefficients for *PastOfferCount*, the number of offers previously extended by the merchant, is particularly noteworthy. When a deal is offered, past daily deal experience has a positive impact on ratings. There are two distinct explanations for this effect. First, frequent offers may motivate loyal customers to sign up with the relevant daily deal providers, resulting in an increasingly large proportion of existing consumers participating in each promotion. Not only are these customers more likely to be satisfied than the marginal consumers discussed in section 2.2, but to the extent that these sales cannibalize full price visits this represents a direct welfare shift from the firm to these individuals. This increased satisfaction among participating customers is observed as higher average ratings during the promotion period. The second explanation is that firms learn how to more effectively execute offers as they gain experience. For example, a restaurant could increase staffing levels, or better structure the offer to avoid capacity constraints. Unfortunately, I do not observe voucher redemption by customer type, and consequently I cannot disentangle these two hypotheses.

When a deal is not run, increasing the number of past offers has a negative impact on ratings, indicating that frequent promotions negatively impact perceived quality. This effect is likely driven by the relationship between past prices, reference prices, and consumer perceptions. Reference prices are known to impact consumer choice, and past prices influence reference prices (Kalyanaram and Winer 1995). Facing repeated, deep discounts from frequent daily deal

offers, consumers may adjust their reference prices downward. Because price serves as a quality-signaling mechanism, these lowered reference prices negatively impact perceived quality (Erdem, Keane et al. 2008). Further, once reference prices are adjusted downward, non-deal period prices are perceived as a price increase, and coded as a loss (Kalyanaram and Winer 1995). In both cases, the result is a drop in average ratings.

The difference in the impact of *ActiveOffer* between the offer and no-offer scenarios is also telling. While vouchers for a daily deal are generally sold for only a few days, they may be valid for several months. The potential for unexpired vouchers from a previous deal is captured by *ActiveOffer*, a binary indicator equal to one when a previous offer by the merchant has not yet expired and zero otherwise. Under the offer and no-offer scenarios, the presence of an active offer has a negative impact on average rating. However, this effect is magnified when offers overlap, likely reflecting the binding capacity constraints faced by these merchants. If a single offer strains the standard service level, multiple simultaneous offers should be expected to have an even more deleterious effect.

Using the estimates in table 5, I calculated the anticipated average rating for each observation under the offer and no-offer scenarios. A histogram of the expected change resulting from an offer is plotted in figure 4. Nearly the entire mass of this distribution falls below zero, with an average expected decline of 0.11 stars. This provides strong evidence that daily deals negatively impact ratings in a predictable manner. As discussed above, I have found strong evidence that

this decline is driven by supply side constraints resulting in lower quality, and repeated price promotions negatively impacting consumer perceptions.¹³

2.7.2 Traffic

In line with the model free evidence, I find that traffic increases when a daily deal is offered. Table 6 contains the relevant parameter estimates, standard errors, and significance levels. Again, columns one and two contain the relevant measures for the offer and no offer scenarios respectively, and column four contains the differences in parameter estimates. With the exception of the number of competitor offers extended some category controls, all parameters are significant at the $\alpha = 0.001$ level. With regard to differences between the two scenarios, the intercept, the effect of past cumulative average ratings, and the number of past offers extended by the firm are all significant, while the difference in the number of competitor offers is marginally significant.

In the absence of a daily deal offer by the focal restaurant, I find strong evidence for positive spillover effects as a result daily deals, indicating category expansion effects dwarf brand switching. *CompetitorOffer*, a count of the competing restaurants that extend offers, is significantly and positively related to the number of reviews posted for the focal firm. Thus, restaurants seem to benefit directly from competitor daily deals, with each additional offer increasing the expected traffic at the focal firm. This provides extremely strong support for the dominance of category expansion. If category expansion were not occurring, any excess

¹³ Given that I do not observe voucher redemption, I cannot speak for or against the standard marginal consumer argument discussed in section 2.2.

competitor demand captured by a focal firm would only partially offset losses due to brand switching. The fact that restaurant traffic *increases* in the face of a competitor offer means that category expansion entirely offsets the losses due to brand switching, and drives additional restaurant traffic. Importantly, this impact disappears when the focal merchant extends a simultaneous offer. This speaks to the relatively fixed capacity constraints faced by the restaurants being studied. When restaurants offer a deal of their own, they have little flexibility with which to meet spillover demand resulting from competitor offers.

As with the coefficients on average rating, the overall difference in traffic can be hard to decipher from table 6. Figure 5 contains the anticipated percentage change in review volume for each observation in the dataset. For the vast majority of observations (97.8%), traffic is expected to increase when a daily deal is offered. The average anticipated increase in traffic is an impressive 63.5%, which again speaks to the potential for demand to exceed capacity when a daily deal is offered.

2.7.3 Reputation

Recall that ratings do not enter the firms profit function directly. Rather, changes in reputation, the volume weighted difference between the average rating during a period and the average of all prior ratings, impact firm decision making. Figure 6 plots the anticipated change in reputation between the offer and no offer scenarios. For 98.5% of the observations, daily deals are expected to have a negative impact on reputation, with an average decline of 0.79 points. Consequently, if merchants are considering reputational effects when making the daily deal decision, it is nearly

certain that those offering a daily deal are willing to accept some negative reputational effects in exchange for the large increases in traffic discussed above.

2.7.4 Firm Profits & Decision Making

Figure 7 plots the anticipated shifts in traffic and reputation conditional on offer decision. The black dots represent observations in which offers were extended, while the gray dots represent observations in which they were not. The horizontal dashed line represents the average anticipated shift in reputation from offering a daily deal, while the vertical line represents no shift in anticipated traffic. Two trends are apparent. First, the majority of the black dots fall above the horizontal line, indicating that while merchants are willing to accept some negative word of mouth from a daily deal offer, those that ultimately extend offers anticipate a smaller than average decline in reputation. Second, there appears to be a tradeoff between traffic increases and negative reputational consequences. When the anticipated bump in traffic is sufficiently large, restaurants appear willing to accept greater than average harm to their reputation.

Table 7 contains the estimates, standard errors, and significance levels for the profit equation parameters. The key covariates of interest, *CompetitorOffer*, $\Delta E[Traffic]$, and $\Delta E[Reputation]$, are all positive and significant. The significant, positive effect of *CompetitorOffer* indicates positive strategic interactions, such that firms find it more profitable to offer these promotions when they anticipate their competitors doing so. This is above and beyond the spillover effects discussed in section 2.7.2, which enters indirectly through anticipated changes in traffic. These positive strategic interactions may indicate that firms are actively combating potential brand

switching by lowering their own prices. Similar effects have been shown for consumer packaged goods, with competitor actions reducing promotional elasticity by 10% (Pauwels 2007). The significant, positive estimate for $\Delta E[Traffic]$ indicates that offers are more likely to be extended when merchants anticipate greater increases in traffic as a result. In combination with Figure 6, the positive, significant parameter estimate for $\Delta E[Reputation]$ indicates that merchants are more likely to offer a daily deal when anticipated reputational damage is lower. This provides evidence that not only do word of mouth effects impact firm outcomes, but that merchants actively manage with these effects in mind.

2.8 Discussion

Building on past research in word of mouth, promotions, and strategic interactions, I have examined three critical factors influencing merchant decisions regarding daily deals. I find that merchants are more likely to offer a daily deal when the increase in traffic is greatest and the reputational harm is lowest. To the best of my knowledge, this is the first paper to show that firms are making critical promotion decisions with word of mouth effects in mind. Further, I find positive strategic interactions, such that merchants are more likely to offer a daily deal when they anticipate their competitors will do so. This is likely an effort to offset brand switching effects through aggressive action, supporting earlier work on price promotions.

I have also been able to examine the impact of daily deals on firm outcomes, where I find three interesting results. First, daily deal offers lead to a drop in average rating, but a spike in review volume. This indicates that while deals drive restaurant traffic, customers are on average

less satisfied. Second, I find that category expansion effects dominate brand switching. When firms do not offer a daily deal, they see an increase in traffic when competitors do so. However, when they extend a simultaneous offer, this effect disappears. This is likely a result of the fixed, binding capacity constraints faced by the small restaurants under study. Finally, I find that frequent offers negatively impact perceived quality, in line with existing research on promotions and reference price effects.

These findings have important implications for managers and academics alike. For managers, daily deal promotions need to be carefully considered. Daily deals can drive dramatic increases in customer traffic. However, scaling to meet the resulting demand can be difficult, and decreased service levels impact both daily deal customers and loyal patrons. Even if existing quality levels are maintained, repeated offerings can negatively impact reference prices, and consequently perceived quality. The result is more negative reviews, which can persist far longer than the promotion itself. For academics, there are three key insights. First, the positive strategic interaction effects provide additional evidence of aggressive competitor response in promotion decisions. Second, category expansion effects can dominate brand switching, especially when capacity constraints are small and fixed. Finally, managers are taking word of mouth implications into account when making decisions. While we have long known that word of mouth impacts firm outcomes, this is the first evidence that they are being actively managed.

This research is not without its limitations. First, I have examined only a single category of merchants using a particular promotion vehicle, so the results may not be generalizable across categories or promotion types. Restaurants have a number of unique attributes which may drive my results, with fixed capacity constraints being the most prominent. Further, daily deals extend

beyond what some researchers consider a standard price promotion. The offers are emailed to a large subscriber base daily, and contain not only information on the deal, but also the merchant offering it. In many ways, this walks a line between advertising and promotion, and may drive awareness above and beyond standard price promotions. Second, because I lack financial information for all restaurants in the Los Angeles area, I have used review volume to proxy for restaurant traffic. While the existing literature has found these to be highly correlated, observed financial outcomes would clearly be preferable. Third, I have modeled the firm's decision as a single period static game, which places a number of potentially strong assumptions on firm behavior. Most importantly, it ignores the dynamic nature inherent in both daily deal offers and reviews. As discussed in the introduction, one of the key benefits of a daily deal is to drive trial. To the extent that repeat occurs beyond the one month window, it is not captured by my model. Further, online word of mouth has a persistent effect, with customer reviews remaining visible for many periods. Thus, merchants may seek positive reviews not just for their current term benefits, but also for their long term value. This is an assumption that I am working to relax in an extension to this work.

In conclusion, I have found evidence that daily deals drive a significant increase in traffic, but can damage a firm's reputation. When these offers are frequently extended, the reputational damage is magnified as consumers' perception of merchant quality is lowered. Further, firm's anticipate these outcomes, and offer deals only when traffic increases are sufficiently large to overcome reputational concerns. Finally, there exist both positive spillover effects and positive strategic interactions, such that restaurants see additional traffic when competitors extend offers, but are also more likely to extend an offer if they anticipate their competitors will do the same.

2.9 Tables

Statistic	Obs.	Min.	Q1	Median	Mean	Q3	Max
<u>per Merchant</u>							
Competitors	11,614	0.00	1.00	3.00	6.54	7.00	129.70
Reviews	11,614	1	13	34	70	80	3,546
Avg. Rating	11,614	1.00	3.19	3.61	3.53	3.97	5.00
Offers	11,614	0.00	0.00	0.00	0.10	0.00	8.00
<u>per Month</u>							
Reviews	29	18,810	22,230	29,800	28,140	32,450	36,710
Avg. Rating	29	3.68	3.69	3.70	3.70	3.72	3.73
Offers	29	14.00	31.00	43.00	39.72	49.00	62.00

Table 1: Summary statistics for key covariates. First four rows contain covariates aggregated to the merchant level. Last three rows contain covariates aggregated to the month level.

Variable	Estimate
Intercept	3.606 ^{***}
PastOfferCount	-0.058 ^{***}
R²	0.0004
Adj. R²	0.0004
Num. obs.	197,427
^{***} $\alpha < 0.001$, ^{**} $\alpha < 0.001$, [*] $\alpha < 0.001$, $\alpha < 0.001$	

Table 2: Results from a linear regression of average rating on number of past offers, using the estimation data set. The number of past offers is significantly, negatively correlated with average rating.

Variable	Estimate
Intercept	4.082***
Firm Offer	2.827***
Competitor Offer Count	1.544***
R²	0.003
Adj. R²	0.003
Num. obs.	197,427
*** $\alpha < 0.001$, ** $\alpha < 0.001$, * $\alpha < 0.001$, $\alpha < 0.001$	

Table 3: Results from a linear regression of review count on a binary indicator of firm offer, and the number of offers extended by competitors, using the estimation data set. Competitor offers are significantly, positively correlated with focal firm reviews, indicating a spill-over effect in restaurant traffic.

Variable	Estimate
Intercept	0.008***
ReviewCount	0.001***
AverageRating	-0.001***
LogCompetitorCount	-0.001***
CompetitorOfferCount	0.002 [*]
R²	0.002
Adj. R²	0.002
Num. obs.	197,427
*** $\alpha < 0.001$, ** $\alpha < 0.001$, * $\alpha < 0.001$, $\alpha < 0.001$	

Table 4: Results from a linear probability model predicting firm offers based on the number of reviews posted for the focal firm, the average rating of those reviews, the log transformed number of competitors, and the number of offers extended by competitors, using the estimation data set. Competitor offers are positively correlated with focal firm offers (though only marginally significant), indicating positive strategic interactions.

Variable	Offer	No Offer	$\gamma_O - \gamma_{NO}$
Intercept	0.390 ^{***} (0.080)	0.840 ^{***} (0.013)	-0.45 ^{***} (0.081)
ActiveOffer	-0.321 ^{***} (0.048)	-0.040 [*] (0.018)	-0.281 ^{***} (0.051)
PastOfferCount	0.099 ^{***} (0.019)	-0.026 ^{**} (0.008)	0.125 ^{***} (0.021)
CumAvgRating	0.852 ^{***} (0.018)	0.760 ^{***} (0.004)	0.092 ^{***} (0.019)

*** $\alpha < 0.001$, ** $\alpha < 0.001$, * $\alpha < 0.001$, $\alpha < 0.001$

Table 5: Average rating parameter estimates. Columns two and three contain the parameter estimates for average rating in the offer and no offer scenario respectively. The associated standard errors and significance levels are also included. Column four contains the difference in these two scenarios for each parameter.

Variable	Offer	No Offer	$\theta_O - \theta_{NO}$
Intercept	0.918 ^{***} (0.095)	0.096 ^{***} (0.009)	0.822 ^{***} (0.095)
CompetitorOffer	0.085 (0.057)	0.196 ^{***} (0.006)	-0.111 (0.057)
CumAvgRating	0.254 ^{***} (0.025)	0.361 ^{***} (0.002)	-0.107 ^{***} (0.025)
ActiveOffer	0.124 ^{***} (0.032)	0.178 ^{***} (0.008)	-0.054 (0.033)
PastOfferCount	0.075 ^{***} (0.012)	0.138 ^{***} (0.004)	-0.063 ^{***} (0.013)
Category Controls	Present	Present	Present

*** $\alpha < 0.001$, ** $\alpha < 0.001$, * $\alpha < 0.001$, $\alpha < 0.001$

Table 6: Traffic parameter estimates. Columns two and three contain the parameter estimates for traffic in the offer and no offer scenario respectively. The associated standard errors and significance levels are also included. Column four contains the difference in these two scenarios for each parameter.

Variable	Estimate
Intercept	-6.102 ^{***} (0.264)
CompetitorOffer	1.438 ^{***} (0.412)
ΔE[Traffic]	0.480 ^{***} (0.118)
ΔE[Reputation]	0.732 ^{***} (0.138)
MSize	-0.128 ^{***} (0.035)
ActiveOffer	2.945 ^{***} (0.286)
Category Controls	Present
Price Controls	Present

*** $\alpha < 0.001$, ** $\alpha < 0.001$, * $\alpha < 0.001$, $\alpha < 0.001$

Table 7: Parameter estimates, standard errors, and significance levels for the critical covariates in the profit function. Category and price controls were included in the estimation, but are excluded here for the sake of exposition.

2.10 Figures

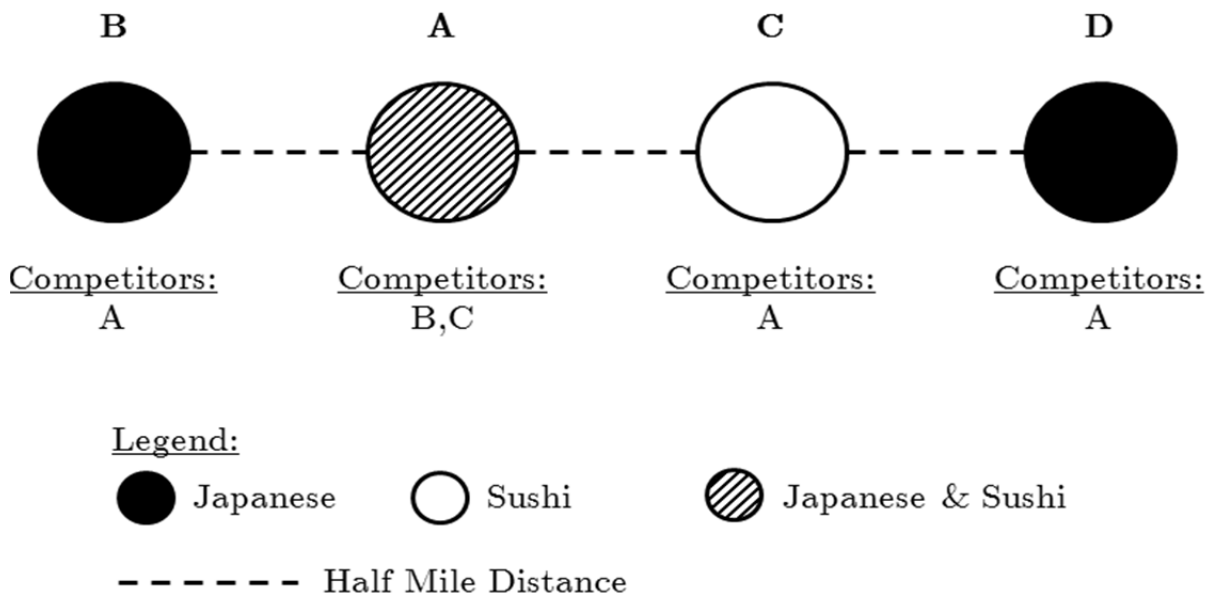


Figure 1: This figure contains a simple example of the non-transitive nature of the competitive sets. All firms compete with restaurant A, but none compete with each other. This is based either on distance (B and D are separated by 1.5 miles) or cuisine type (B and C have no overlapping cuisine types).

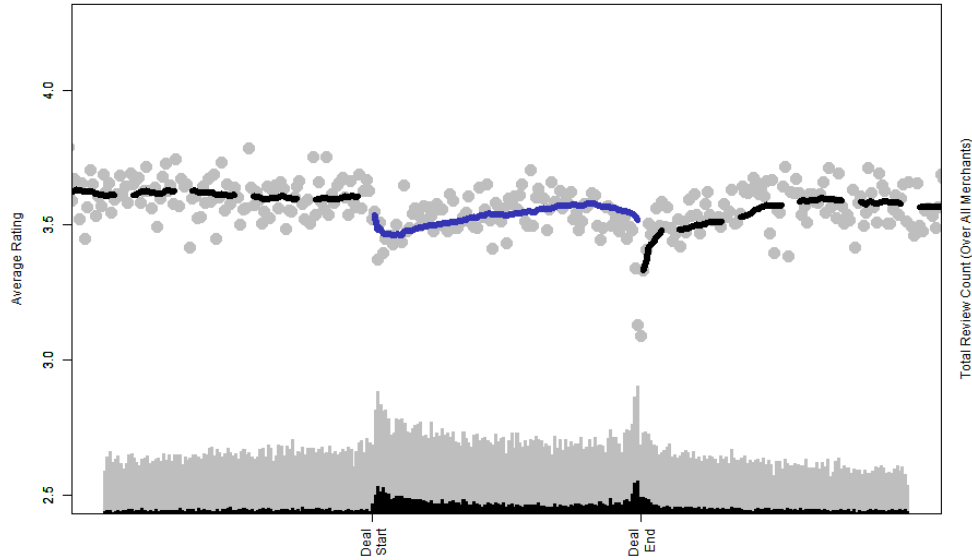


Figure 2: Relationship between daily deals and Yelp reviews. This plot shows the average drop in review valence at the start and end of the deal period, as well as the spike in review volume, based on all 423,980 available observations.

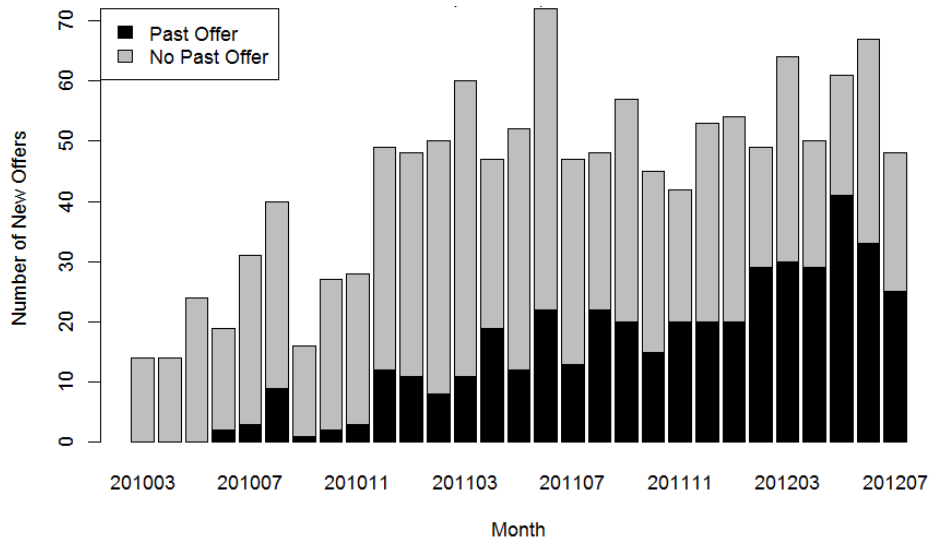


Figure 3: Offer growth over time, by past offer experience. Total bar height represents the number of new offers in each month. Black represents offers extended by merchants that have previously offered a daily deal, where gray represents offers by first time participating merchants.

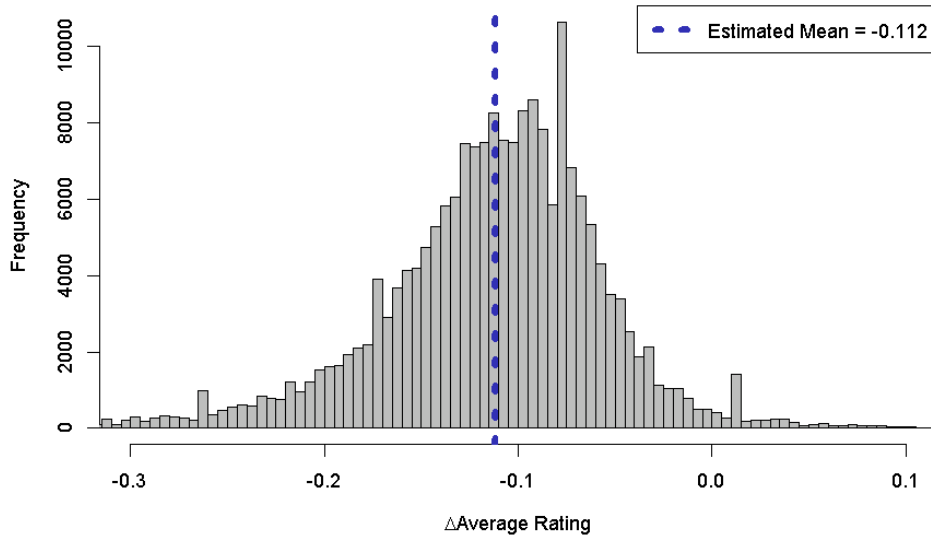


Figure 4: Anticipated change in average rating between offer and no offer scenarios. For the vast majority of merchants, average rating is expected to drop when an offer is extended.

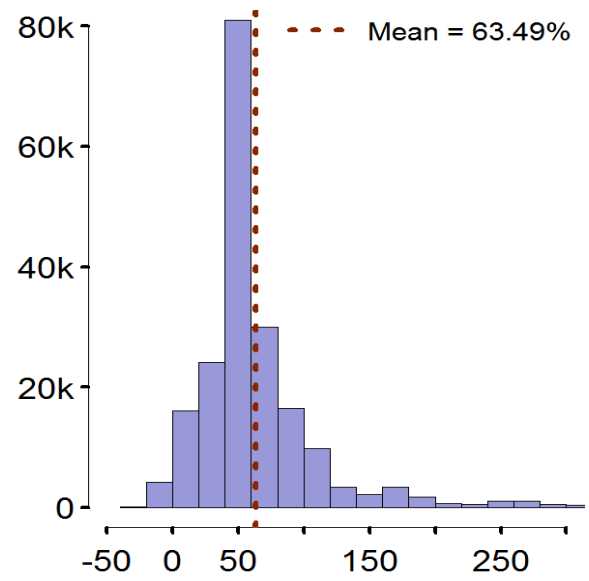


Figure 5: Anticipated percentage change in review count from extending an offer. Review volume is expected to increase 63.5% when a daily deal is offered, and is nearly always positive.

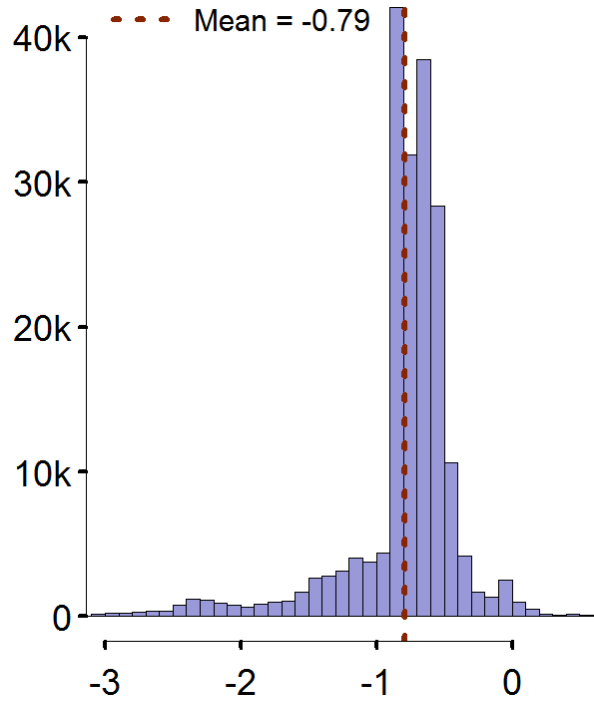


Figure 6: Anticipated change in reputation from offering a daily deal. When a daily deal is extended, nearly all merchants anticipate negative reputational effects, with an average decline of 0.79 points.

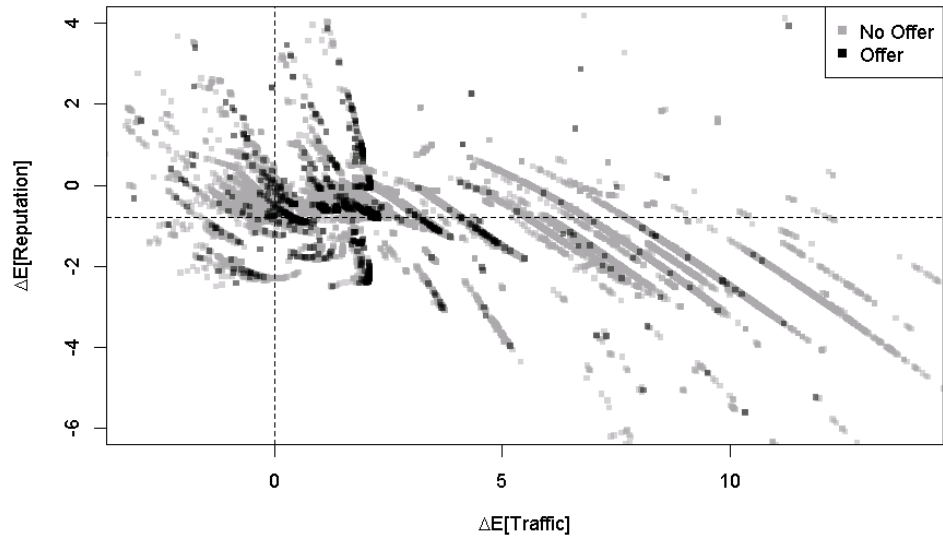


Figure 7: Choices by anticipated outcome. The gray dots represent observations in which no offer was extended. Black dots correspond to observations in which offers were extended. The horizontal dashed line indicates the average change in anticipated reputation. With relatively few black dots below this line, it appears that daily deals are avoided when merchants would take disproportionately large reputation hits. However, as the expected change in traffic increases, merchants appear more willing to make this tradeoff.

2.11 Appendix – Marginal Conditional Distribution of ω_{rit}^k

Because ω_{rit}^1 , ω_{rit}^0 , ξ_{Tit}^1 , ξ_{Tit}^0 , and $\Delta\xi_{\pi it}$ are jointly distributed as in equation (8), the marginal distribution of ω_{rit}^0 , ξ_{Tit}^1 , ξ_{Tit}^0 , and $\Delta\xi_{\pi it}$ can be written as

$$\begin{pmatrix} \omega_{rit}^0 \\ \xi_{Tit}^1 \\ \xi_{Tit}^0 \\ \Delta\xi_{\pi it} \end{pmatrix} \sim N \left(\begin{pmatrix} 0 \\ 0 \\ 0 \\ 0 \end{pmatrix}, \begin{bmatrix} \sigma_{r_0}^2 & \sigma_{r_0 T_1} & \sigma_{r_0 T_0} & \sigma_{r_0 \pi} \\ & \sigma_{T_1}^2 & \sigma_{T_1 T_0} & \sigma_{T_1 \pi} \\ & & \sigma_{T_0}^2 & \sigma_{T_0 \pi} \\ & & & \sigma_{\pi}^2 \end{bmatrix} \right)$$

Let

$$\xi_{it} = \begin{pmatrix} \xi_{Tit}^1 \\ \xi_{Tit}^0 \\ \Delta\xi_{\pi it} \end{pmatrix}$$

$$\Sigma_{21} = \begin{pmatrix} \sigma_{r_0 T_1} \\ \sigma_{r_0 T_0} \\ \sigma_{r_0 \pi} \end{pmatrix}$$

$$\Sigma_{22} = \begin{bmatrix} \sigma_{T_1}^2 & \sigma_{T_1 T_0} & \sigma_{T_1 \pi} \\ & \sigma_{T_0}^2 & \sigma_{T_0 \pi} \\ & & \sigma_{\pi}^2 \end{bmatrix}$$

Then the marginal conditional distribution of ω_{rit}^0 is

$$(\omega_{rit}^0 | \xi_{it}) \sim \mathcal{N} \left(\mathbb{E}[\omega_{rit}^0 | \xi_{it}], \Sigma_{(\omega_{rit}^0 | \xi_{it})} \right)$$

Where

$$\mathbb{E}[\omega_{rit}^0 | \xi_{it}] = \mathbb{E}[\omega_{rit}^0] + \Sigma'_{21} \Sigma_{22}^{-1} (\xi_{it} - \mathbb{E}[\xi_{it}])$$

$$\Sigma_{(\omega_{rit}^0 | \xi_{it})} = \sigma_{r_0}^2 - \Sigma'_{21} \Sigma_{22}^{-1} \Sigma_{21}$$

The marginal conditional distribution for ω_{rit}^1 can be calculated similarly.

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Ch. 3: Dynamic Examination of Daily Deal Decisions

3.1 Abstract

Online daily deal sites provide a marketplace in which local merchants sell vouchers for goods and services to geographically targeted consumers. After the steep discount and provider's cut, the merchant's take is generally 25% of listed prices. While there are certainly a number of merchants for whom daily deals have been effective, stories of failure are rife in the popular press. In this paper, I examine when and why merchants participate in daily deals, and what influence such participation has on their business. To address these questions, I compiled a unique data set containing all restaurants in the Los Angeles area, their characteristics and customer reviews as reported by Yelp.com, and their participation in daily deals from March 2010 through July 2012. Using a dynamic model of firm participation and outcomes, I explore the trade offs firms make when running these promotions. I find that daily deal launch and offer expiration coincide with a dramatic spike in the volume of reviews and a significant drop in valence, consistent with existing research. Despite these dramatic contemporaneous effects, I find no detectible, persistent effect on perceived quality. I also find that these offers tend to be extended by new merchants, which is indicative of firms using daily deals as an advertising vehicle to drive awareness and trial. Finally, I find that currently active offers dramatically increase the probability of a new offer, supporting the hypothesis that firms participating in daily deal offers run the risk of becoming dependent upon them.

3.2 Introduction

Online daily deals represent one of the newest, fastest growing, and most controversial marketing tools available. In 2012, the two largest daily deal providers, Groupon and LivingSocial, reported combined revenues of \$2.87 billion, up 54% year over year (Groupon 2013; Amazon.com 2013). By some estimates, consumers will spend consumers will spend \$5.5 billion annually on daily deals in the US alone by 2015, a 202% increase from 2011 (Pacheco and Udowitz 2012). These numbers are even more impressive when one considers that neither Groupon nor LivingSocial existed prior to 2007.

By building large subscriber bases and managing websites on which offers are listed, daily deal providers serve as market makers for daily deal offers. The listed offers generally come from small, local firms, and represent a 40-60% discount on goods and services (Byers, Mitzenmacher, and Zervas 2012a). The offers are listed for a short period of time, generally one to three days, and expire at a pre-specified date, usually six months in the future. As their fee, providers take an average of 45% of a voucher's selling price (Dholakia 2012), leaving merchants with 22-33% of the voucher's face value as revenue. For participating merchants, these offers constitute the single largest annual marketing expense (Dholakia 2011). Given the resulting slim margins, it may be unsurprising that the popular press is rife with stories of daily deals gone bad; including a cupcake shop losing a year's worth of profits (NBCNews.com 2011), a niche grocer requiring charitable donations to stay afloat (Gelles 2012), and a waffle house being driven out of business (Kurtzleben 2012). Past participating merchants have described daily deals as, "The single worst decision I have ever made as a business owner," (Caldwell 2012).

Reading through these firsthand accounts and speaking with managers, several themes become apparent. First, merchants find that many daily deal customers seem less satisfied with their products and services. To the extent that the discounted prices attract marginal consumers, this follows directly from standard economic theory. The result is that these less satisfied customers are unlikely to return at full prices or spend beyond the voucher's value. Second, loyal customers frequently purchase vouchers. This can have negative effects in both the short and long term. In the short term,

voucher sales to loyal customers may cannibalize future full price sales. In the long term, such discounts can damage the brand by increasing price sensitivity (Kaul and Wittink 1995), decreasing reference prices (Kalyanaram and Winer 1995), and signaling lower quality (Erdem, Keane, and Sun 2008). Finally, capacity constraints are real and binding. When merchants don't sufficiently limit the number of available vouchers, they are frequently unable to meet the resulting demand while maintaining existing quality and service levels. This impacts not only the daily deal customers, but also their existing customer base. The result is that daily deals can not only fail to succeed, but they may also do long term damage.

Despite these pitfalls, there are merchants that report great success with these promotions. One restaurant owner in Richmond, VA said, "There is no question that [our daily deal offer] has paid for itself better than any other advertising medium we have ever used," (Friedman 2011). Such sentiments seem to be supported by the emerging research, which shows that roughly half of past participants intend to run another daily deal, and a third of managers view them as a sustainable business practice (Dholakia 2012; Yarow 2011). While this falls well below the 80% and 97% merchant repeat rates claimed by LivingSocial and Groupon respectively (Heine 2013; Lancellotti-Young 2011), it does provide evidence that a subset of merchants finds daily deals to be beneficial.

Among the positive stories, there are two themes; awareness and trial. The majority of participating merchants are smaller, local businesses, with limited marketing budgets and little in the way of brand awareness. Because providers send daily emails to their subscriber bases highlighting the deals offered, daily deals represent an opportunity for significant exposure at no upfront cost. In this way, daily deals are similar to advertising with strong price based message. In conjunction with this increase in awareness, daily deals increase trial by offering consumers an opportunity to test a product or service at a dramatically lower price. While repeat rates are heavily debated, few argue against daily deals as a driver of trial.

With the significant debate surrounding the efficacy of daily deals, it is interesting to consider how merchants make the decision whether or not to participate. In this paper, I explore how daily deals impact online reviews, as well as whether and to what extent merchants consider this impact

when undertaking such offers. My approach utilizes a dynamic model of firm participation and outcome, based on existing research into word of mouth, promotion and advertising effects, and dynamic models. I assume that firms are rational agents, selecting actions that maximize the present value of all expected future profits. In addition to stable restaurant attributes and the firm's offer decision, I allow these profits to be impacted by the valence of online reviews. This link between word of mouth, sales, and profits is well established within the literature, as will be discussed in section 3.3. I also allow online reviews to be impacted by the merchant's offer decision, in line with the extensive literature on advertising and promotion effects. In this way, I explore the complex relationship between daily deals, online reviews, and the decisions of forward looking managers.

To do all of this, I assembled a unique data set combining daily deal information from the two largest providers, Groupon and LivingSocial, and word of mouth information from the popular review site Yelp.com. The resulting data set contains information on 21,552 restaurants in the Los Angeles area, including 1,204 unique offers and 1,051,925 reviews. My focus on daily deals and use of online reviews to measure outcomes makes restaurants a particularly appealing category. In addition to representing approximately 12% of daily deal industry revenue (Yipit 2012), restaurants are also the second most frequently reviewed category on Yelp.com (Yelp 2013). While other local merchants such as spas may offer a wide variety of products and services, restaurants have a single general offering, prepared and served food, making them easier to categorize and compare. Further, there is a strong, established relationship between restaurant sales and profits and online reviews (Anderson and Magruder 2012; Luca 2011) providing assurance that my outcome measures impact firm performance.

Based on this approach, I reach four key conclusions. First, daily deals negatively impact the valence of online reviews, largely focused at deal launch and expiration. Second, this effect is not persistent, with no discernible impact on reviews posted following the offer. Third, these offers tend to be extended by new merchants, which is indicative of firms using daily deals as an advertising vehicle to drive awareness and trial. Finally, currently active offers dramatically increase the probability of a new offer, supporting the claim that firms participating in daily deal

offers run the risk of becoming dependent upon them.

This paper makes two key contributions to the existing literature. First, I am able to extend previous work showing that word of mouth impacts firm outcomes, by showing that firms are aware of this relationship and make critical decisions with reputational ramifications in mind. I also build on the extensive literature on promotion effects, showing significant short term effects with little persistent effect on perceived quality.

The remainder of this paper is organized as follows. In section 3.3, I review the relevant research on advertising and promotion effects and online word of mouth. In section 3.4, I discuss the data collection and setup. Section 3.5 presents some stylized facts on the relationship between daily deals and online reviews as well as factors impacting restaurant closure. In section 3.6, I discuss the modeling approach, which continues with the estimation strategy in section 3.8. Section 3.9 discusses the results, and section 3.10 concludes with implications and directions for future research.

3.3 Related Work

3.3.1 Word Of Mouth

There is considerable research showing that the valence of online reviews drives merchant sales and profits. For instance, Chevalier and Mayzlin 2006 finds that a higher average rating is positively correlated with sales rank and market share at both BarnesandNoble.com and Amazon.com. Dellarocas, Zhang, and Awad 2007 finds a similar effect for movies, showing that the valence of reviews posted during the opening week is an important predictor of total box office sales. With regard to restaurants, my area of empirical focus, two recent studies have shown strong ties between online reviews, sales, and profits. Noting that Yelp displays average ratings rounded to the nearest half star, both Luca 2011 and Anderson and Magruder 2012 use a regression discontinuity approach to show that a half a star improvement in average Yelp rating leads to 19% more sellouts and 5-9% more revenue. Further, these results are driven by independent restaurants, about which

consumers have few other available information sources (Luca 2011).

These smaller, local firms comprise the core merchant base for daily deal providers, making it critical that we understand the interplay between daily deal offerings and online word of mouth. Recent research has shown that review volume tends to spike around daily deal launch and expiration (Byers, Mitzenmacher, and Zervas 2012a), and that this pattern coincides with the redemption of daily deal vouchers (Song et al. 2012). Further, daily deal reviews tend to be for merchants in a novel location or line of business from the reviewers perspective, indicating customer trial. However, reviews posted during daily deal offerings tend to be significantly more negative than those posted during non-deal periods, driven in part by daily deal reviews that are in general 10% lower than others (Byers, Mitzenmacher, and Zervas 2012b). Thus, daily deals appear to drive significant customer trial and create an environment in which review volume spikes and valence drops. This creates the potential for a strong negative effect on the cumulative average rating.

There are three critical conclusions from the existing word of mouth literature. First, online reviews impact firm performance, with greater valence leading to improved sales and profits. Second, this link is especially strong for small and medium size restaurants, which comprise a core merchant base for daily deal providers. Third, daily deals are positively correlated with review volume, but have a strong negative correlation with valence, which may lead to significant, negative effects on overall ratings. I hope to build on this literature by examining the role of anticipated word of mouth on firm decision making, especially as related to daily deals.

3.3.2 Promotion & Advertising Effects

Daily deals combine aspects of both price promotion, by offering steep discounts on products and services, and advertising, by marketing these discounts to large subscriber bases through daily email blasts and on providers' websites. In the long term, price promotions have been found to have little persistent effect on sales in the absence of company inertia (Nijs et al. 2001; Pauwels 2004). However, when such inertia is present, increasing promotion frequency, or when the promotion is accompanied by price based advertising, consumer perceptions are negatively impacted through

increased price sensitivity (Kaul and Wittink 1995; Mela, Gupta, and Lehmann 1997), decreased reference prices (Kalyanaram and Winer 1995), and diminished perceived quality (Erdem, Keane, and Sun 2008).

By combining promotion information with consumer reviews, my data provide an opportunity to further study this effect. I can examine the impact of frequent advertised price promotions on perceived quality, when the latter is directly observed. In past research, perceived quality has been defined as an unobserved heterogeneous term, impacted by price, advertising, and past product purchase (Erdem, Keane, and Sun 2008). If frequently advertised promotions negatively impact perceived quality, one would expect that average ratings are lower for restaurants that have more frequently offered deals than for their less deal prone counterparts, even when no deal is being offered. Understanding this relationship will help us better appreciate the impact that such promotions have on consumer perceptions and firm outcomes.

There is also an opportunity to examine whether merchants view daily deals predominantly as a price promotion or an advertising vehicle by examining the types of merchants that run them. Promotions have been shown to favor established, high quality brands through increased brand switching (Blattberg, Briesch, and Fox 1995). Meanwhile, advertising elasticities are greatest for new brands (Vakratsas and Ambler 1999). Therefore, one can examine whether the majority of the benefit from daily deals is through its role as a price promotion tool or advertising medium, by exploring the roles of quality and restaurant age in the firm's daily deal decision.

3.4 Data Collection & Construction

3.4.1 Collection

On the daily deals side, I focus on the two dominant providers, Groupon and LivingSocial, who comprise 70-80% of the market (Pepitone 2012). Using a list of URLs from daily deal aggregator Yipit, I collected information on all 1,433 offers extended by restaurants in the Los Angeles area through either of these providers during the 28 month period from March 3, 2010 to July 31, 2012.

I gathered offer specific information such as when the deal was posted and when it expired, as well as merchant specific information such as name, address, phone number, merchant website, and Yelp URL where available.

With 86 million unique monthly visitors and roughly 7.5 million restaurant reviews, the review site Yelp.com serves as a valuable source of digital word of mouth information (Yelp 2013). From this site, I collected restaurant attributes such as type of fare and price range, as well as individual review information including when the review was posted and the assigned numeric rating. I focused first on those merchants that had offered a daily deal. The vast majority of these merchants were matched to the daily deal data using either the Yelp URL found on the daily deal site or an exact string match between both the listed name and address. Those that could not be matched programmatically were manually matched based on a combination of name, phone number, address, and website. All merchants were matched using one of these two methods.

I then gathered the population of Los Angeles restaurants (as listed on Yelp.com) by iterating between Yelp's merchant and user pages. For each Yelp review posted on a merchant's page, there is a link to the user that posted it. Within each user profile, there is a complete list of all reviews they have posted, including the businesses for which the reviews were created. At each iteration, I collected the users that were not already in my database, and then from these users identified any previously unobserved merchants in Los Angeles County. This process continued until no new merchants or users were produced. The end result was 4,697,739 reviews for 201,237 merchants.

I geo-coded each of the merchants using Microsoft's Map API. Unlike other geo-coding tools, Microsoft provides both a location type (i.e., address, intersection, or city center) and match confidence (i.e., low, medium, or high) in their results. These attributes were used to ensure that the latitude and longitude estimates for a specific address or intersection were returned with high confidence.

Not all of the merchants listed on Yelp are restaurants, and this is reflected in my database. Because I only have information on daily deal offerings by restaurants, I refine my data set using the categories attributed to each merchant in Yelp. Specifically, I identified 141 restaurant related

categories from the 711 categories listed in my data. To be considered a restaurant, a merchant had to be assigned to at least one of these restaurant related categories. After geo-coding all merchants and separately identifying restaurants, the data contains information on 44,033 unique restaurants.

3.4.2 Data Setup

For restaurants, two characteristics of the data make analyzing daily deal effects difficult. First, Yelp's data is largely user generated, resulting in a fair number of stub files. These stub files commonly stem from user error, such as incorrectly identifying a business name. Second, new restaurants have a high failure rate, with 26% closing their doors in the first year. This failure rate is higher for independent restaurants than for chains, and is frequently a function of quality of life issues for the owner rather than the restaurant's financial performance (Parsa et al. 2005). In an effort to mitigate the impact of these factors on the results, I filtered out merchants with fewer than 5 total reviews. This represented 1.8% of reviews and 34.8% of restaurants, including 2.7% of restaurants that extended offers.

Offering a daily deal is not an instantaneous process. Rather, merchants and providers work together to plan the promotion for some future period. Frequently, the merchant is only guaranteed that the deal will run within some pre-specified window, usually a few weeks long (Gupta, Weaver, and Rood 2011). With this in mind, I discretize the data to the merchant-month level, resulting in 543,357 observations spanning March 2010 through July 2012, the period during which I observe daily deals. The final data set contains 1,051,925 reviews for 21,552 restaurants, and includes 2,068 instances of restaurant closure and 1,204 daily deal offers.

3.5 Stylized Facts

3.5.1 Daily Deals & Online Reviews

As reflected in figure 1, my data support a strong relationship between daily deals and word of mouth, with review volume increasing and valence decreasing during the offer period. Because not every deal is of the same length (though the majority last six months), each point on this plot has been normalized to span one percent of the total deal duration. This allows deal launch and expiration to be aligned across offers. The dots at the top represent the average rating of all new reviews posted at a given time, and show clear discontinuities at deal launch and expiration. The lines represents a thirty period moving average, reset at the start and end of the deal, and make this discontinuity even more clear. Note that the dip following deal expiration likely reflects a delay in review posting by those that visited just before deal expiration.¹ The bars at the bottom represent the total volume of reviews posted at each time, with those posted during the daily deal period highlighted in black. Dramatic increases are apparent at the beginning and end of the deal period, coinciding with the drop in average ratings. While I do not directly observe restaurant traffic, this is in line with daily deal redemption patterns (Song et al. 2012), and provides additional support for the link between review volume and sales. Because a firm's average rating is calculated across all previous reviews, this increase in review volume magnifies the impact of the relatively negatively valenced reviews posted during the daily deal period.

Despite the potentially negative long term impact of lower ratings, I see considerable repeat on the part of the merchants. Figure 3 contains the number of offers per month across all 543,357 observations. The gray portion represents offers by restaurants that have not previously participated in a daily deal, while the black portion represents offers from those that have. Two facts are immediately apparent. First, the number of deals offered in a given month has been roughly constant since the beginning of 2011. Second, the percentage of offers extended by restaurants with previous daily deal experience has increased steadily over this same period. One explanation

¹This may also represent individuals left holding expired vouchers after the deal period, though this is not supported by reading through a random sample of such reviews.

for this growth is that firms have found past offers to be profitable, and are thus willing to repeat. This runs contrary to the majority of anecdotal evidence in the popular press, though it is in line with the recent research on daily deals discussed in section 3.3. An alternative explanation is that restaurants become dependent upon these deals to fill their space. Daily deals are known to be an extremely effective traffic driver, and demand can far outpace supply during peak periods. As such, existing, loyal customers may be displaced by daily deal customers, and, over time, repeat may not be sufficient to make up for any permanent loss in loyal customers. Once a deal is run, this relative dearth of loyal customers may, ironically, make repeat deals both more profitable and necessary.

Participating merchants earn very slim (if not negative) margins from daily deal vouchers. If the latter reasoning were to hold, we would expect to see those merchants depending on daily deals to stay afloat fail at a greater rate. Figure 3 plots the probability of firm closure relative to the past number of deals launched. There is a clear spike in restaurant failure following the second deal, which supports this dependent merchant hypothesis. However, we observe zero closures among merchants that have participated in more than 2 daily deals. While this may be due to the relative dearth of observations, it is also consistent with a filtering process in which dependent merchants are quickly removed from the market, while others are able to earn greater overall profits from daily deals.

3.5.2 Exploring the Simple Explanations

Several simple explanations have been posited as to why merchants may offer daily deals despite the largely negative popular opinion. These arguments frequently center on participating merchants being of lower quality, having fewer customers, or having less experience. Using the data available, I can examine each of these. The purpose of this section is not to show that these factors play *no* role in the firm's decision, but rather that each appears insufficient to explain the observed behavior on their own.

It is frequently proposed that daily deals are offered by lower quality merchants. We can assess

this claim by comparing the cumulative average rating for merchants that offer daily deals, just prior to deal launch, to those that never offer daily deals. Figure 4 presents this comparison. While the average ratings among the daily deal merchants has a lower variance, both the mean and median ratings are slightly greater for this group (3.66 and 3.69 versus 3.58 and 3.64 respectively). Thus, it doesn't appear that daily deals are only offered by low quality merchants.

It has also been proposed that daily deals are offered by managers with relatively empty restaurants in an attempt to draw traffic. I can examine this effect if we allow review count to proxy for traffic. The use of this proxy is well supported in the literature, with past research showing showing a strong correlation between review volume and sales. This has been found between reviews and book sales on Amazon (Chevalier and Mayzlin 2006), online reviews and movie box office sales (Dellarocas, Zhang, and Awad 2007; Liu 2006), and (more directly to my application) Yelp reviews and restaurant sales (Lu et al. 2013). I also observe this correlation in the subset of my data for which I have both sales information. While it is not feasible to obtain sales data from all restaurants in my data set, I was able to obtain scaled revenue information from three restaurants. The correlation between review volume and sales among these three was 0.88.

Figure 5 compares median and 95% confidence bands for the number of reviews over the prior six months between observations in which a deal is offered and those with no deal offering. While there are no significant differences between the two groups, any slight, directional effect would seem to favor merchants offering deals.² This indicates that it is not just low traffic restaurants that are running these deals.

Finally, it has been proposed that daily deals are only offered by new merchants, in an attempt to drive awareness and trial. Age is defined as the number of months since the firm is first observed in my data, through either a review posting or daily deal offer. With the first observed review dated October 13, 2004, this can dramatically precede the first offer. Figure 6 shows the relationship between restaurant age and daily deal decision. While age has an overall upward trend, there is little discernible difference between those that offer daily deals and those that do not. Importantly,

²The dramatic increase in variance is a result of the dramatically lower observation count for daily deal offers.

this does not rule out age (or quality or traffic) as a factor in the firm's decision, it simply argues that these are unlikely to be the only factors.

3.5.3 Factors Impacting Restaurant Closure

One important recent advancement in estimation of dynamic models has been the two stage estimator produced by Hotz and Miller 1993 and expanded upon by Arcidiacono and Miller 2011. Their approach dramatically simplifies the computational burden, allowing for large, continuous state spaces. To fully leverage these approaches, one needs to observe a terminal state, and factors influencing entry into that state. In this paper, firm exit will serve that role. While restaurant closure appears to be influenced by past daily deal offers, it is also correlated with a number of other relevant, observed metrics. Figure 7 presents the probability of restaurant closure by category across the top 10 most popular categories.³ The dramatic variation is clear from this plot, with Japanese restaurants being 2.17 times more likely to close than fast food restaurants.

Figure 8 contains the probability of closure by restaurant age across all merchants. Here, age is measured as the number of years from the first observation (offer or review posting) for that merchant in the full data set. As seen in the data, the probability of closure increases 76.7% in the first year, but steadily declines thereafter. This is consistent with financing requirements in the industry that require owners to have several months of operating costs on hand before financing can be obtained.⁴

3.6 Model

Based on the model free evidence, I construct a model of daily deal decisions among restaurants. I assume that the dynamic process unfolds in discrete time over an infinite horizon. In each period $t \in \{1, 2, \dots, T\}$, $T \leq \infty$, managers at restaurant i make a decision, d_{it} , to either exit the market,

³A restaurant on Yelp can be tagged with multiple categories. This may be why similar trends exist among similar restaurant styles (i.e., Fast Food and Burgers).

⁴These closure rates fall well short of the 26% discussed in Parsa et al. 2005, which may be largely due to the imposed five review limit.

offer a daily deal, or continue without a daily deal, such that:

$$d_{it} = \begin{cases} 0 & \text{If exit} \\ 1 & \text{If daily deal offered} \\ 2 & \text{Otherwise} \end{cases} \quad (1)$$

This decision impacts both current period profits and the state variables defining the environment in which the restaurant operates moving forward.

Let $u(\omega_{it}, d_{it})$ represent the flow of profits to firm i at time t conditional on the firm's contemporaneous decision and a vector of state variables, ω_{it} . These state variables are partitioned into two groups. The first is a group of variables, z_{it} , that are known to both the restaurant and econometrician. The second is a choice-specific shock, $\epsilon(d_{it})$, that is revealed to the firm at the start of period t but remains unobserved by the econometrician. In line with much of the dynamic discrete choice literature (Arcidiacono and Ellickson 2011; Arcidiacono and Miller 2011; Ellickson, Misra, and Nair 2012), I assume that these two components are additively separable.⁵ Thus, firm i 's profit in period t is represented by

$$u(z_{it}, d_{it}) + \epsilon(d_{it}). \quad (2)$$

The firm's decision also influences the value of future period decisions through its impact on the subsequent, observed state variables, $z_{i,t+1}$. I assume that $z_{i,t+1}$ is Markov and independent of the unobserved decision shock conditional on the observed state variables and firm decision at time t .⁶ I denote the PDF of $z_{i,t+1}$ conditional on z_{it}, d_{it} as

$$f(z_{i,t+1} \mid z_{it}, d_{it}). \quad (3)$$

⁵Keane and Wolpin 1994 present a great discussion of how this assumption can be relaxed. Though, their approach proves considerably more computationally expensive.

⁶These assumptions are standard in nearly all dynamic discrete choice research. However, it is worth noting that Arcidiacono and Miller 2011 develops a method by which the conditional independence assumption can be partially relaxed, and Yoganarasimhan 2013 has recently taken this to data.

I assume that firms act rationally, selecting the action, δ_{it}^* that maximizes the expected present value of all future profit flows conditional on the current state variables and a common discount rate, $\beta \in [0, 1]$:

$$\delta_{it}^* = \arg \max_{d_{it}} \mathbb{E} \left(\sum_{t'=t}^T \beta^{t'-t} [u(z_{it'}, d_{it'}) + \epsilon(d_{it'}) \mid z_{it}, \epsilon(d_{it})] \right), \quad (4)$$

where the expectation is taken over all possible future states and actions resulting from d_{it} . The solution to this problem is given by the value function

$$V(z_{it}, d_{it}) = \max_{d_{it}} \left\{ u(z_{it}, d_{it}) + \epsilon(d_{it}) + \mathbb{E}_{d_{it}} \left(\sum_{t'=t+1}^T \beta^{t'-t} [u(z_{it'}, d_{it'}) + \epsilon(d_{it'})] \right) \right\}. \quad (5)$$

Because $\epsilon(d_{it})$ is unobserved, we define the ex-ante value function, \bar{V}_{it} , as the continuation value of being in state z_{it} just before $\epsilon(d_{it})$ is revealed to the firm. This is given by integrating equation 5 over $\epsilon(d_{it})$,

$$\bar{V}_{it}(z_{it}) = \int V(z_{it}, d_{it}) g(\epsilon(d_{it})) d\epsilon(d_{it}), \quad (6)$$

where $g(\epsilon(d_{it}))$ is the density function for $\epsilon(d_{it})$. Given a set of state variables and the ex-ante value function, I can write the value function as

$$v(z_{it}, d_{it}) \equiv u(z_{it}, d_{it}) + \epsilon(d_{it}) + \beta \int \bar{V}_{i,t+1}(z_{i,t+1}) f(z_{i,t+1}, d_{i,t+1}) dz_{i,t+1}. \quad (7)$$

I assume that $\epsilon(d_{it}) \stackrel{iid}{\sim} T1EV$. As outlined in Arcidiacono and Ellickson 2011, this provides two key advantages. First, there exist closed form solutions for the choice probabilities and the ex-ante value function, dramatically easing the computational burden. Second, it allows us to easily map from choice probabilities to ex-ante value functions. In particular, we can rewrite equation 6

as

$$\bar{V}_{it}(z_{it}) = -\ln[p(d_{it}^* | z_{it})] + v(z_{it}, d_{it}^*) + \gamma, \quad (8)$$

where d_{it}^* represents an arbitrary reference choice and γ is Euler's constant. Substituting equation 8 evaluated at $t + 1$ into equation 7, we can now write the choice specific value function as

$$\begin{aligned} v(z_{it}, d_{it}) \equiv & u(z_{it}, d_{it}) + \epsilon(d_{it}) + \\ & \beta \int \{-\ln[p(d_{i,t+1}^* | z_{i,t+1})] + v(z_{i,t+1}, d_{i,t+1}^*)\} \\ & \times f(z_{i,t+1} | z_{it}, d_{it}) dz_{i,t+1} + \beta\gamma. \end{aligned} \quad (9)$$

The future value term in equation 9 is now a function of the transition kernel for the state variables, the conditional choice probability for an arbitrary choice, and the associated conditional value function. Importantly, the choice of $d_{i,t+1}^*$ is arbitrary, and wholly at the researchers discretion. Setting $d_{i,t+1}^*$ to a terminal choice, beyond which no further decisions are made, allows the continuation value to be parameterized as a function of the per-period payoffs. This eliminates the need to solve the dynamic programming problem in equation 9, dramatically reducing the computational burden. This will be particularly helpful because my state space is high dimensional and continuous. I will detail the precise estimation routine in section 3.8, but first I turn to econometric assumptions and model specification used to take the model to my data.

3.7 Econometric Assumptions & Specification

I assume that the deterministic portion of contemporaneous profit, $u(z_{it}, d_{it})$ is a linear function of the observed state variables and associated parameters, modeled as

$$u(z_{it}, d_{it}) = z_{it}'\theta_1. \quad (10)$$

The vector of state variables, z_{it} , is comprised of three distinct groups of variables describing restaurant attributes, the status of any currently active offers, and past experience with daily deals. Specifically, z_{it} contains the restaurant's past average rating, $\log(\text{active offer count})$, $\log(\text{age})$, the log transformed number of months since the first daily deal observation, and indicators for the top 10 most common restaurant categories.⁷

Logarithmic transforms of merchant age, the number of active offers, and the number of months since the first daily deal offer were used for both qualitative and empirical reasons. Qualitatively, each of these is expected to have diminishing marginal returns to firm profits, largely related to awareness and trial. *Ceteris-paribus*, new restaurants tend to have lower awareness, and consequently lower sales, vis-à-vis their more established counterparts. Regardless of offer status, trial rates are likely higher among new restaurants, as there exist fewer consumers that have previously dined at the restaurant. During a daily deal offer, new restaurants are likely to benefit disproportionately from the associated increase in trial and awareness. The number of active offers captures the number of overlapping deals running at any one time. With two dominant daily deal providers, offers run in rapid succession, even across different providers, are likely to be seen multiple times by many consumers. This redundancy may result in declining response rates, and lower profit contribution from each subsequent deal, in line with what we observe for price promotions (Blattberg, Briesch, and Fox 1995). In contrast, deals that are run later in our dataset may be more effective vis-à-vis earlier offers due to the two sided nature of the daily deals marketplace. As more consumers subscribe to the daily email blasts, each deal offers the participating merchant greater reach. However, this growth may eventually lead to a less well targeted customer base (as more geographically distant consumers subscribe), and may slow overall as providers exhaust the pool of potential consumers. This is supported by figure 2, which shows rapid growth in daily deal offers followed by a stable period beginning in early 2011. The number of months since the first observed offer is intended to control for this temporal effect. Beyond this qualitative reasoning, the model fit with these logged covariates produced better fit, as measured by both AIC and BIC, than either

⁷One was added prior to computing the logarithm of any covariates to avoid issues associated with taking the logarithm of zero.

linear or squared alternatives.⁸

Conditional on the firm's decision, past average rating, $\overline{AR}_{i,t+1}$, is the only stochastic future state variable. I model this as a linear function of the observed state variables, x_{it} , and the firm's decision, d_{it} . Similar to z_{it} , x_{it} is a vector of covariates describing attributes, offer status, and past daily deal experience. Specifically, x_{it} contains past average rating, \log (expired offer count), \log (active offer count), and indicators for the top 10 most common restaurant categories. Let s_{it} be a vector containing both these state variables and the firm's decision ($s_{it} = \{x_{it}, d_{it}\}$), then

$$\overline{AR}_{i,t+1} = s'_{it}\theta_2 + \eta_{it}, \quad (11)$$

where I assume $\eta_{it} \stackrel{iid}{\sim} \mathcal{N}(0, \sigma^2)$. One concern with this assumption is that predicted values may fall outside of observed bounds, one to five stars. I could alternatively consider a truncated normal distribution, but this would be computationally costly. Instead, I will show in the results section show that the residual standard error resulting from the model is extremely small relative to the estimated values, making the normal distribution a good approximation to the truncated normal in this instance.

The argument in support of logarithmic transforms in the transition model follows closely to that given for the profit function. Qualitatively, these are supported by the diminishing marginal returns associated with daily deals, while empirically this specification offered a better fit, as measured by both BIC and AIC, than those fit with linear or squared terms.

The separate identification of the parameters influencing the transition function in equation 11 and the firm's per period profits in equation 10 relies on the standard exclusion restriction argument. In this case, I need a state variable that influences the evolution of average ratings directly, but influences profit only through its impact on average ratings. \log (expired offer count) fulfills this role. Past offers may impact current period average ratings through changes in perceived quality related to reference prices as discussed in section 3.3, or simply because average rating is an equally

⁸In my data, the maximum observed number of active offers when a restaurant closes is one, precluding estimation of a squared term for this covariate. The comparison is based on the model without this squared term.

weighted mean of all past reviews. It is less clear why a past offer would directly impact future period profits.

As mentioned at the end of section 3.6, the choice of $d_{i,t+1}^*$ is arbitrary and the estimation simplifies dramatically if it is set to a terminal state. As discussed in section 3.5, I observe firm exit and a number of factors influencing it, and therefore set $d_{i,t+1}^* = 0$. With this normalization,

$$\begin{aligned}
 v(z_{it}, d_{it}) \equiv & u(z_{it}, d_{it}) + \epsilon(d_{it}) + \\
 & \beta \int -\ln[p(d_{i,t+1}^* | z_{i,t+1})] \\
 & \times f(z_{i,t+1} | z_{it}, d_{it}) dz_{i,t+1} + \beta\gamma.
 \end{aligned} \tag{12}$$

Importantly, the choice specific value function is defined by firm's contemporaneous profits, the probability of exit in the next state given a future state, and the probability of that future state given the current state and the firm's decision. The future value can be fully parameterized in the contemporaneous profit flows.⁹ Under the above assumptions, consistent estimates of θ_1 , θ_2 , and β can be recovered without solving the fully dynamic programming problem.

3.8 Estimation

I leverage the two stage estimator originally developed in Hotz and Miller 1993 and extended by Arcidiacono and Miller 2011. In the first stage, I recover estimates for the CCP function representing firm exit, $\hat{p}(d_{i,t+1}^* | z_{i,t+1})$, and transition function in equation 3, $\hat{f}(z_{i,t+1} | z_{it}, d_{it})$. In the second stage I estimate the value function parameters taking these two as given.

As in the extant literature, it is critical that the first stage produce consistent estimates of the CCPs, as these are used to construct firm beliefs. Given all discrete state variables or a small number of continuous variables, there are numerous non-parametric methods by which these could be estimated. However, the inclusion of several continuous covariates precludes such an approach.

⁹Because exit costs are not separately identified from fixed costs of continuing operations, I normalize scrap value to zero.

Given a sufficiently large data set and an exhaustive set of parameters, CCP estimates from logit models are similar to those obtained non-parametrically (Arcidiacono and Miller 2011; Bajari et al. 2010). Consequently, I follow examples in the extant literature (Ellickson and Misra 2012; Yoganarasimhan 2013), and estimate the first stage using a highly flexible binary logit specification, including higher order terms and a full set of bivariate interactions. The exact specification is available upon request.

As discussed in section 3.7, past average rating is the only stochastic state variable conditional on the observed firm choice. I recover the associated parameters, both θ_2 and σ^2 , using standard OLS regression based on equation 11. By construction, these parameters define the distribution of future average ratings,

$$\overline{AR}_{i,t+1} \sim \mathcal{N}(s'_{it}\theta_2, \sigma^2). \quad (13)$$

I can now simulate a draw from $f(x_{i,t+1} | x_{it}, d_{it})$, for any combination of state variables and firm decision, by combining draw from equation 13 with the non-stochastic elements of $x_{i,t+1}$. I use a series of 100 such draws at each observation to numerically integrate the future value term in equation 12 for every observed combination of states and all possible firm choices over the distribution of potential future states.

Given the above results, estimating the structural parameters impacting per-period profit flows, θ_2 , and the discount rate, β , simplifies to estimating a standard multinomial logit. Given my application and approach, the ability to estimate, rather than assume, β is particularly important, but caution must be taken in interpreting the estimate. My data covers many small business that may not have frictionless access to capital. Consequently, assuming a discount rate equal to some common rate of return, as is frequently done, may be misleading and result in biased estimates. Even if such a discount rate could be reasonably assumed, it is unclear how to implement it given the approach. A well known feature of discrete choice models is scale-invariance. That is, the parameters are only identified up to some scaling constant, and reflect the effect of the observed covariates relative to the standard deviation of the unobserved variables (Train 2009). This means that β will not be interpretable relative to common cost of capital measures, and it becomes unclear how any

assumed value should enter. That said, by estimating the discount rate, I will be able to examine the valuation placed on future profits *relative* to other observed factors in the model, and I avoid biasing other parameter estimates.

3.9 Results

As stated above, the conditional exit probabilities were estimated using a highly flexible, binary logit. Because the parameter estimates from such an estimation are not particularly insightful and only the fitted values are used in what follows, I refrain from presenting the estimates here.¹⁰ Instead, I focus on the parameter estimates from the transition and value functions.

3.9.1 Transition Function

Table 1 contains the parameters estimates and model fit from equation 11. The first column contains the estimates without controlling for each firm's past average rating, while the second column controls for this lagged effect. As one might expect, a firm's past average rating is the strongest predictor of its future average rating, increasing R^2 from 0.05 to 0.97. This stems from any unobserved correlation between quality and rating, as well as the cumulative nature of Yelp's displayed average rating (i.e., there is no weighting associated with recency). Importantly, controlling for the dominant effect of a firm's past average rating clarifies the impact that daily deals have on a firm's future rating. This can be seen by comparing the parameter estimates for $\log(\text{ActiveOfferCount})$ and StatusActiveOffer between models 1 and 2 in table 1.

Before continuing to interpret the parameter estimates, I return to the concerns discussed in section 3.7 stemming from the assumption that $\eta_{it} \stackrel{iid}{\sim} \mathcal{N}(0, \sigma^2)$ while average ratings are bounded between one and five. Because the distribution of η_{it} has support over all real numbers, a relevant concern is that predicted values resulting from this model may fall outside the feasible bounds. As seen in figure 9, the distribution of predicted values is bounded from below by 1.03 and above by

¹⁰Parameter estimates and associated exit probabilities are available from the author upon request.

4.96, falling entirely within the feasible range.

Because we are using equation 11 to simulate over the distribution of future states, and additional concern is that these simulated values may fall outside the feasible bounds. As seen in table 1, $\sigma^2 = 0.11$, which is extremely small relative to the fitted values discussed above. This indicates that simulated values are unlikely to fall outside the reasonable range. Thus, the normal distribution is a reasonable approximation to the truncated normal in this case.

Returning to table 1, the significant, negative parameter estimates for `LogActiveOfferCount` and `StatusNewOffer` indicate that offering a daily deal detracts significantly from average ratings, as was discussed in section 3.5. The larger negative effect for `StatusNewOffer` reflects the steep drop in average rating associated with the initial offer. The smaller effect for `LogActiveOfferCount` indicates that average ratings for new reviews remain slightly lower throughout the deal period.

While these estimates are highly significant in a statistical sense, a relevant question is whether these are economically meaningful. On each restaurant's page, Yelp displays the average rating rounded to the nearest half star. This means that a restaurant with an average rating of 4.24 is shown as a four star establishment, while one with a score of 4.26 appears to have 4.5 stars. As discussed in section 3.3, a half a star increase in average Yelp rating leads to 19% more sellouts and a 5-9% increase in restaurant revenue. Thus, even small changes in average rating can have a dramatic impact if the merchant sits near one of these thresholds. Within my data, a new offer is expected to cause a restaurant to drop below one of these thresholds 3.6% of the time.

$\log(\text{Expired Offer Count})$ was included to test for any persistent effect of past daily deals on a firm's future reviews. To the extent that all reviews factor into a firm's average rating, any short term impact is by definition persistent. The remaining question is whether daily deals can impact reviews posted *after* the deal ends. As discussed above, research has shown that past prices influence reference prices (Kalyanaram and Winer 1995; Blattberg, Briesch, and Fox 1995). One concern surrounding such steep price promotions has been that the size of the discount may result in consumers lowering their reference prices (Blattberg, Briesch, and Fox 1995). Because price serves as a quality-signaling mechanism, these lowered reference prices may negatively impact

perceived quality even after a deal ends (Erdem, Keane, and Sun 2008). Consequently, daily deals could have a persistent, negative effect on a firm's ratings. The lack of significance on \log (Expired Offer Count) reflects the lack of a detectable, persistent effect of daily deals on a firm's future reviews. While this does not necessarily run counter to the belief that price promotions impact reference prices, it does seem to indicate that such promotions do not ultimately have a persistent impact on perceived quality.

3.9.2 Value Function

Table 2 contains the estimated parameters comprising the firm's value function, and conveys several critical insights. First, offers tend to be extended by new merchants, which is indicative of firms using daily deals to drive awareness and trial. Second, greater past average ratings lead to greater profits, and consequently a lower probability of restaurant closure, but have no impact on the firm's daily deal decision beyond that. Third, currently active offers dramatically increase the probability of a new offer, supporting popular press claims that participating firms run the risk of becoming dependent upon daily deals.

The significant negative parameter estimate for \log (Age) under a daily deal indicates that these offers tend to be more profitable for newer merchants. Daily deals are marketed through geographically targeted email blasts and webpages, and have the potential to significantly increase awareness and trial. This is one of the key differences between these offers and more standard price promotions. As discussed in section 3.7, newer merchants are likely to have lower awareness and fewer past customers, and thus stand to more greatly benefit from these aspects of a daily deal. This finding stands in contrast to the standard price promotion literature, where better known national brands tend to derive greater benefit from discounting (Blattberg, Briesch, and Fox 1995), and supports the argument that firm's view daily deals predominantly as an advertising vehicle.

The positive, significant parameter estimates for past average ratings indicates that merchants with higher average ratings are less likely to close. This is in line with past research (Anderson and Magruder 2012; Luca 2011), showing that greater average ratings lead to larger sales and profits.

Interestingly, there is little difference between the parameter estimates under the status quo and daily deal regimes, indicating that the decision between running a daily deal or simply maintaining the status quo is largely independent of the firm's rating. This runs contrary to the belief that daily deals are usually run by low quality merchants, and also contradicts past findings on price promotions where high quality brands are found to benefit disproportionately (Blattberg, Briesch, and Fox 1995). Taken together with the finding that newer restaurants tend to run daily deals, this offers compelling evidence that daily deals are run more for their advertising impact than the promotional aspects.

The large, positive, and significant parameter estimate for $\log(\text{ActiveOfferCount})$ under the daily deal regime indicates that existing active offers make new offers more profitable. Closely sequencing daily deal offers seems to run counter to what we would expect from past research on advertising or price promotion, which both exhibit decreasing marginal returns (Blattberg, Briesch, and Fox 1995; Vakratsas and Ambler 1999). When this is seen in the price promotions literature, it is frequently attributed to shifts in consumer price sensitivity and managerial inertia (Pauwels 2004). However, the magnitude of the demand shift associated with daily deals in combination with the binding capacity constraints of small restaurants offers a more troubling explanation. When existing loyal customers are displaced by daily deal customers, they may fail to return to the merchant. When the initial spike in traffic subsides, such a firm would face a lower baseline level of business if repeat rates from daily deal customers prove insufficient to offset this loss. Ironically, this lower baseline reduces one potential cost of daily deals, that existing full price customers may be displaced, and increases the profitability of subsequent offers.

Given the thin margins associated with daily deals, merchants that become stuck in such a cycle are at considerable risk of failure. It is interesting to note the probability of restaurant failure nearly doubles following the launch of the second offer, as seen in figure 3. This is not the only explanation for such repeat rates. It may be that daily deals are profitable for some portion of restaurants, and this is also supported in figure 3 by the dramatic decline in closure rates following the third offer launch. However, it does make evident the risk faced by participating firms.

3.10 Conclusion

In this paper, I examined the trade offs firms make when offering a daily deal, and the impact that these deals have on online word of mouth. To do this, I assembled a unique data set combining 1,051,925 reviews for 21,552 restaurants in Los Angeles County from Yelp with the 1,204 daily deal offers run by these restaurants on the two largest daily deal providers, Groupon and LivingSocial. I then developed a framework of firm behavior in which firms decide whether to exit, offer a daily deal, or proceed without an offer with the goal of maximizing the present value of all future cash flows.

I find that a dramatic spike in review volume and a significant drop in valence coincide with daily deal launch and expiration, consistent with existing research. Despite these dramatic contemporaneous effects, I find no detectible, persistent link between such promotions and perceived quality. With regard to the firm's decision, I find that these offers tend to be extended by new merchants, and I find no significant difference in quality between participating and non-participating restaurants. From the restaurant's perspective, these deals appear to be viewed as advertising, and are favored by those who stand to benefit disproportionately from the increased awareness and trial. Finally, I find that currently active offers dramatically increase the probability of a new offer, supporting the hypothesis that firms participating in daily deal offers run the risk of becoming dependent upon them.

Despite the interest of these findings, there remain several avenues for future research. First, my approach assumes homogeneity in both the firm's decision function and the evolution of the state variables. Future researchers may want to relax this assumption, in line with Arcidiacono and Miller 2011. Second, I do not account for potential strategic interactions between firms. Given the findings in chapter 2, it may be worthwhile to extend the dynamic demand model presented here to a dynamic game. Third, I assume that the value function is constant over time, precluding learning on the part of firms. Because daily deals were a relatively new phenomenon at the time of my data, a model allowing for learning on the merchant's part may offer additional insight. Finally, I do not directly observe profits, revenues, or costs, forcing a more descriptive approach and precluding an

easily interpreted discount factor. Given such data, it would be interesting to explore the extent to which small business are behaving myopically when extending daily deal offers.

3.11 Tables

	<i>Dependent variable:</i>	
	Model 1	Model 2
PastAverageRating		0.973*** (0.0002)
LogExpiredOfferCount	-0.014 (0.010)	0.003 (0.002)
LogActiveOfferCount	-0.004 (0.012)	-0.006** (0.002)
StatusNewOffer	0.016 (0.018)	-0.019*** (0.003)
Constant	3.669*** (0.001)	0.097*** (0.001)
Category Effects	Included	Included
Observations	541,289	541,289
R ²	0.052	0.973
Adjusted R ²	0.052	0.973
Residual Std. Error	0.636 (df = 541275)	0.106 (df = 541274)
F Statistic	2,298.000*** (df = 13; 541275)	1,419,662.000*** (df = 14; 541274)

Note: *p<0.05; **p<0.01; ***p<0.001

Table 1: Transition function estimates and model fit.

	<i>Dependent variable:</i>
	Status
NewOffer:LogAge	-0.211*** (0.050)
StatusQuo:LogAge	0.011 (0.035)
NewOffer:(intercept)	-7.805*** (1.684)
StatusQuo:(intercept)	6.237*** (1.057)
NewOffer:PastAverageRating	0.554*** (0.058)
StatusQuo:PastAverageRating	0.478*** (0.032)
NewOffer:LogActiveOfferCount	3.376*** (0.432)
StatusQuo:LogActiveOfferCount	-0.303 (0.422)
CValue	0.375*** (0.036)
Category Effects	Included
Time Trend	Included
Observations	543,357
Log Likelihood	-21,159.000
LR Test	1,954.000*** (df = 31)

Note: *p<0.05; **p<0.01; ***p<0.001

Table 2: Structural estimates from the firm's value function.

3.12 Figures

Relationship Between Daily Deals & Reviews

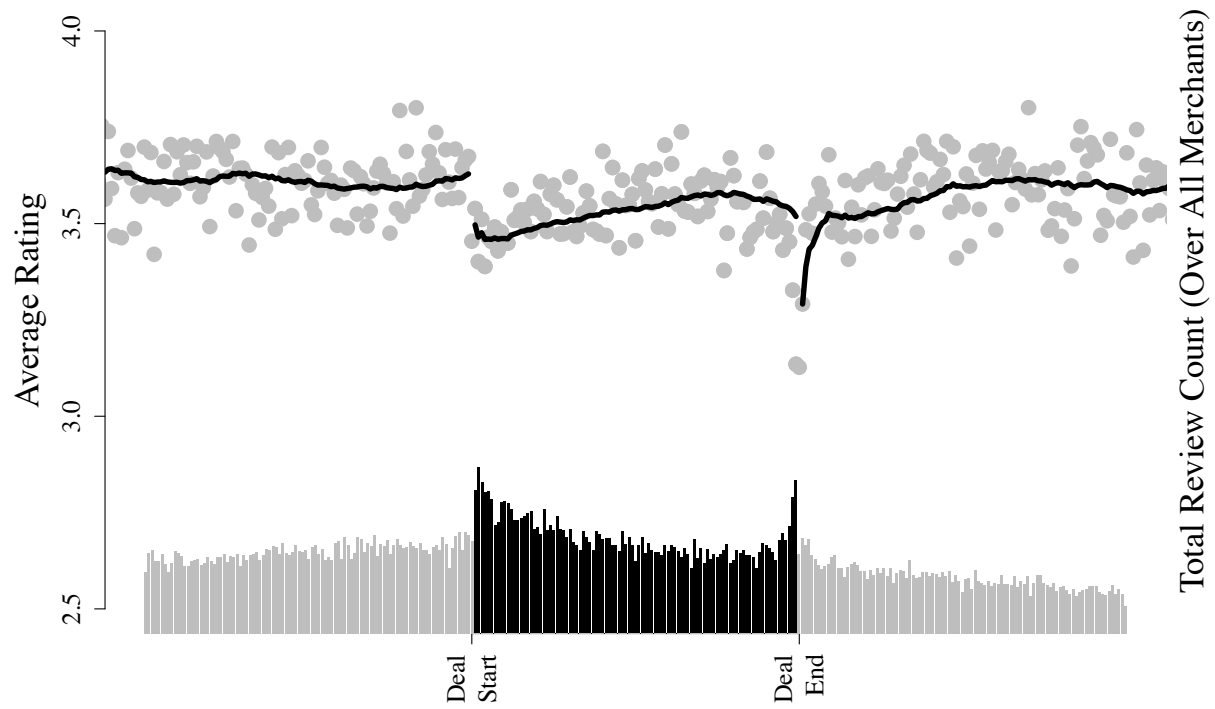


Figure 1: The impact of offering a daily deal on review volume and average rating.

Offer Growth Over Time By Past Offer Experience

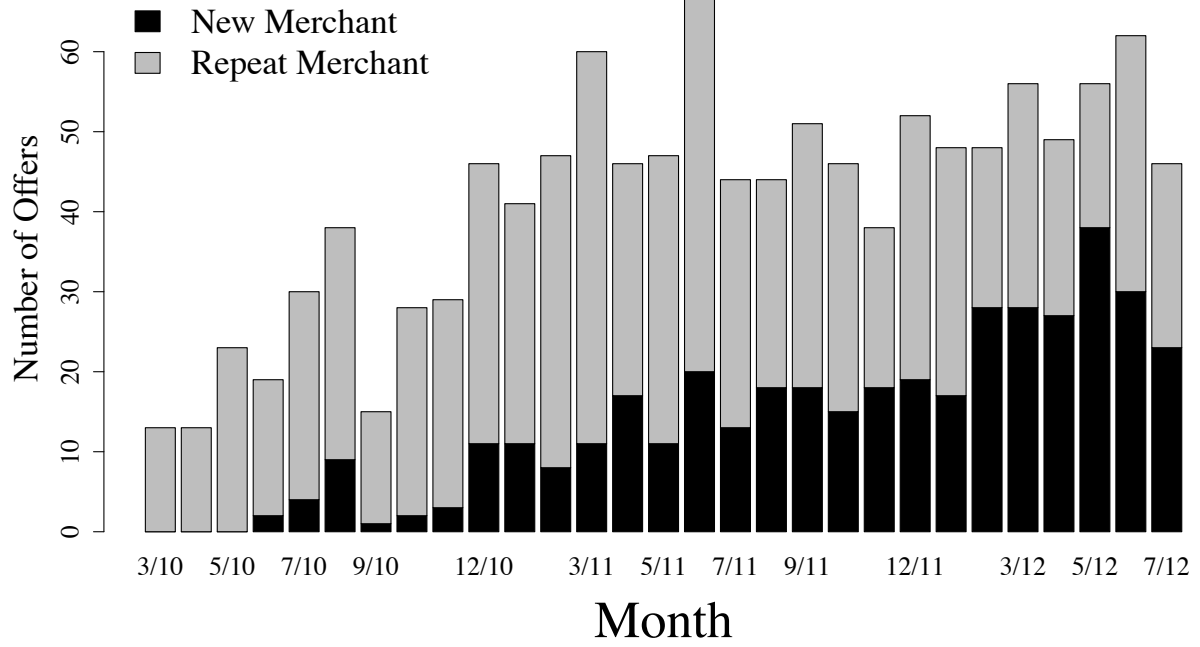


Figure 2: Offers per month among new and repeat merchants.

Prob. of Restaurant Closure Given Deal Experience

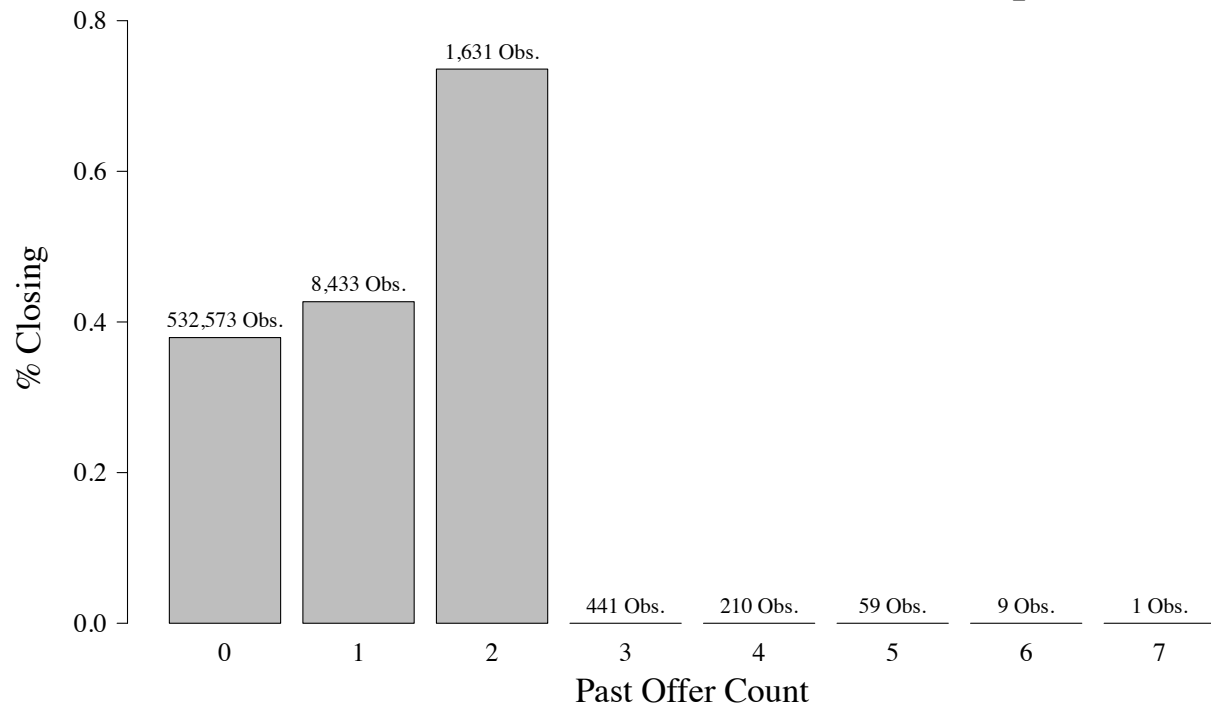


Figure 3: Probability of restaurant closure conditional on the number of prior offers.

Distribution of Average Ratings

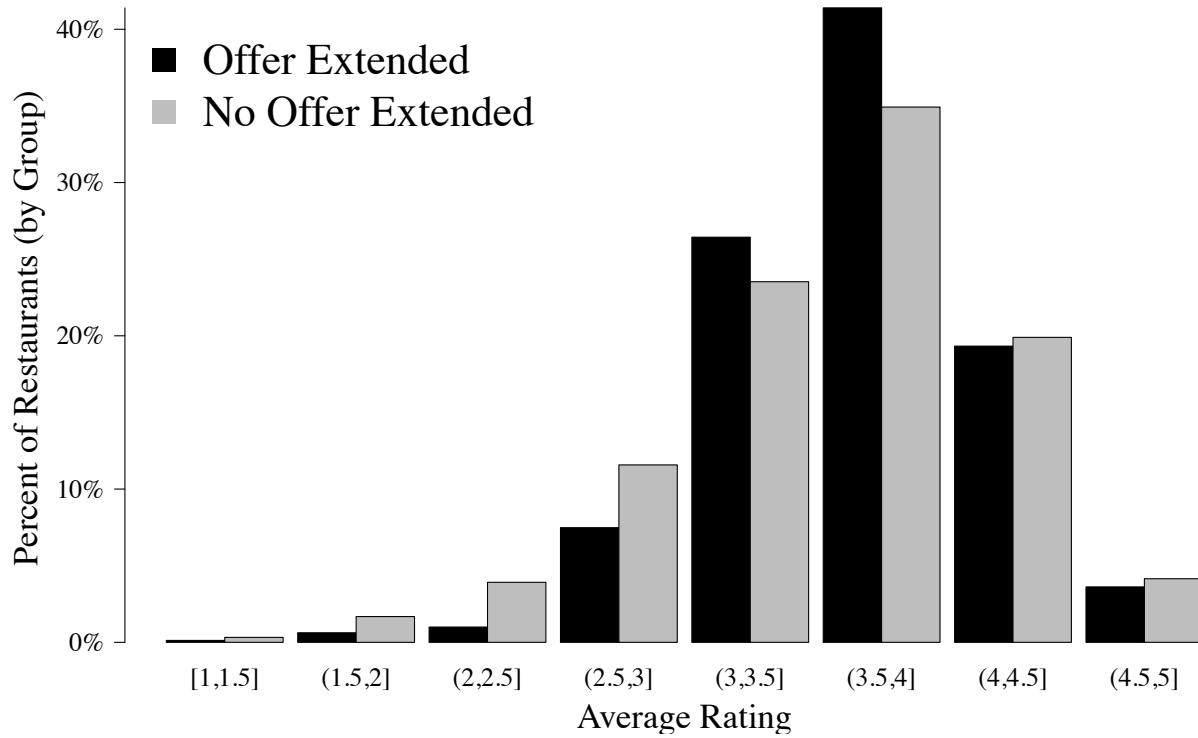


Figure 4: Final observed rating for merchants with no daily deal offers compared to those of merchants just before they offer their first daily deal.

Six Month Lagged Rolling Average of Review Count by Offer Choice

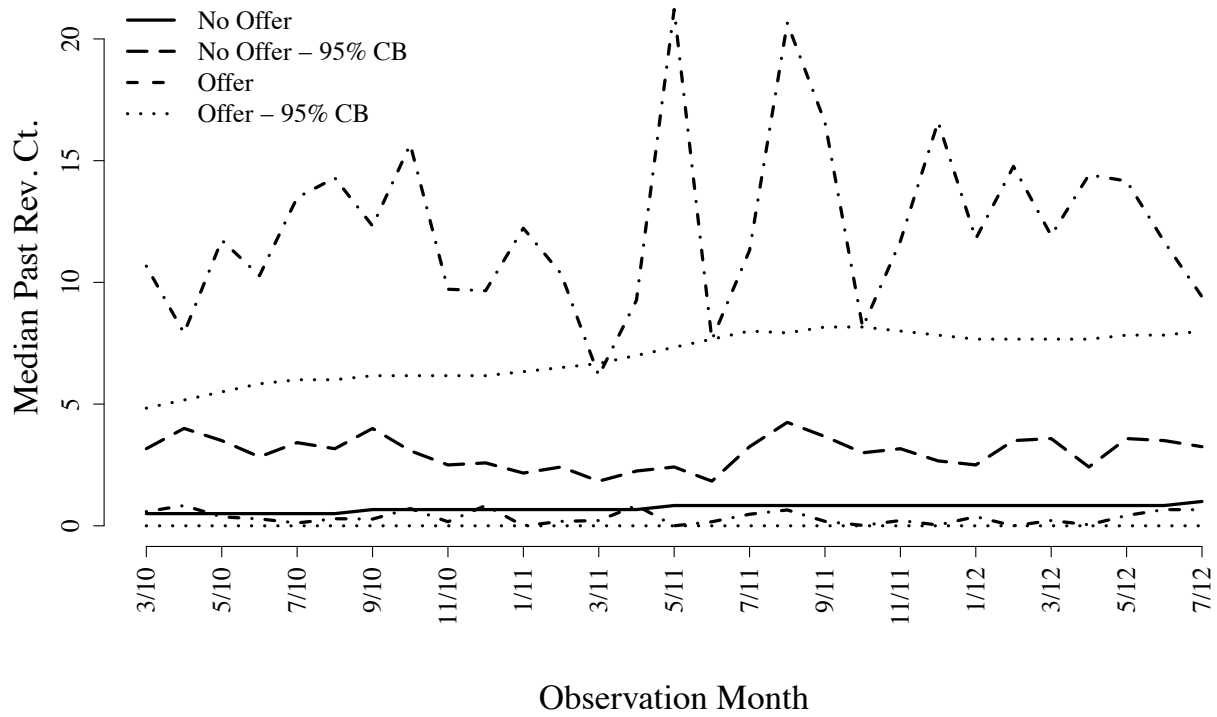


Figure 5: A rolling six month past average of monthly review counts, split by deal and no deal observations.

Restaurant Age by Offer Choice

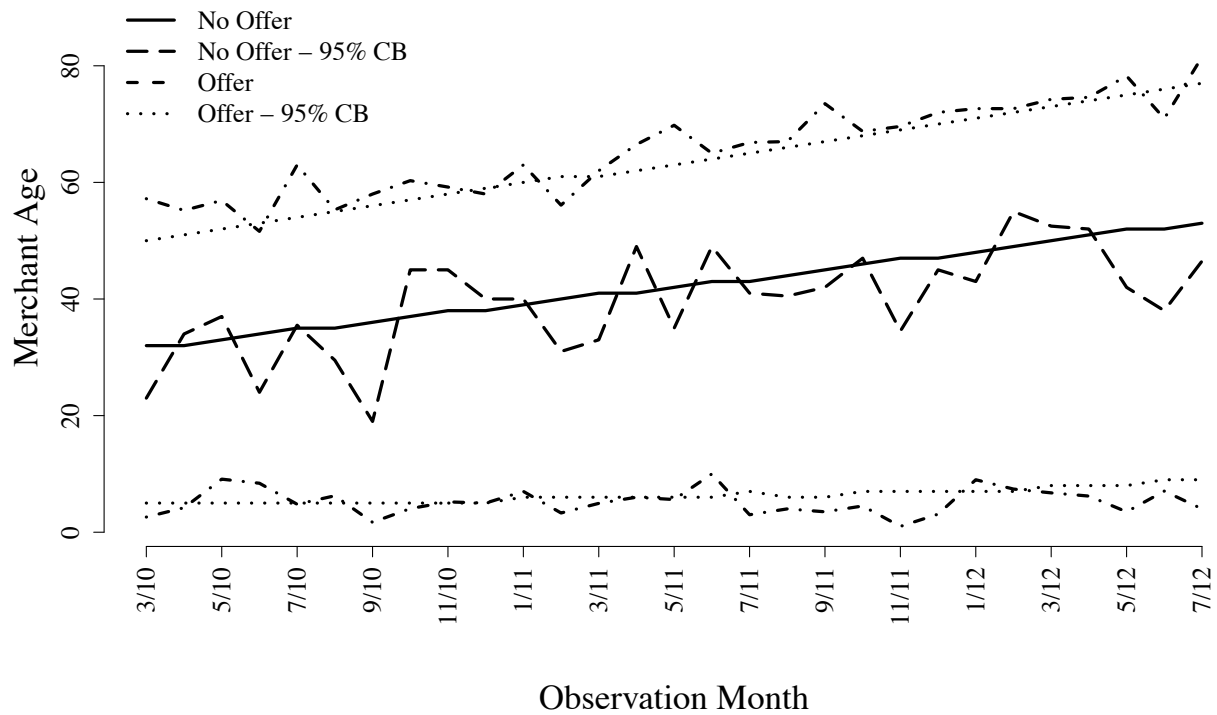


Figure 6: Average merchant age, defined as time since the first review or offer observation (whichever came earlier), split by deal and no deal observations.

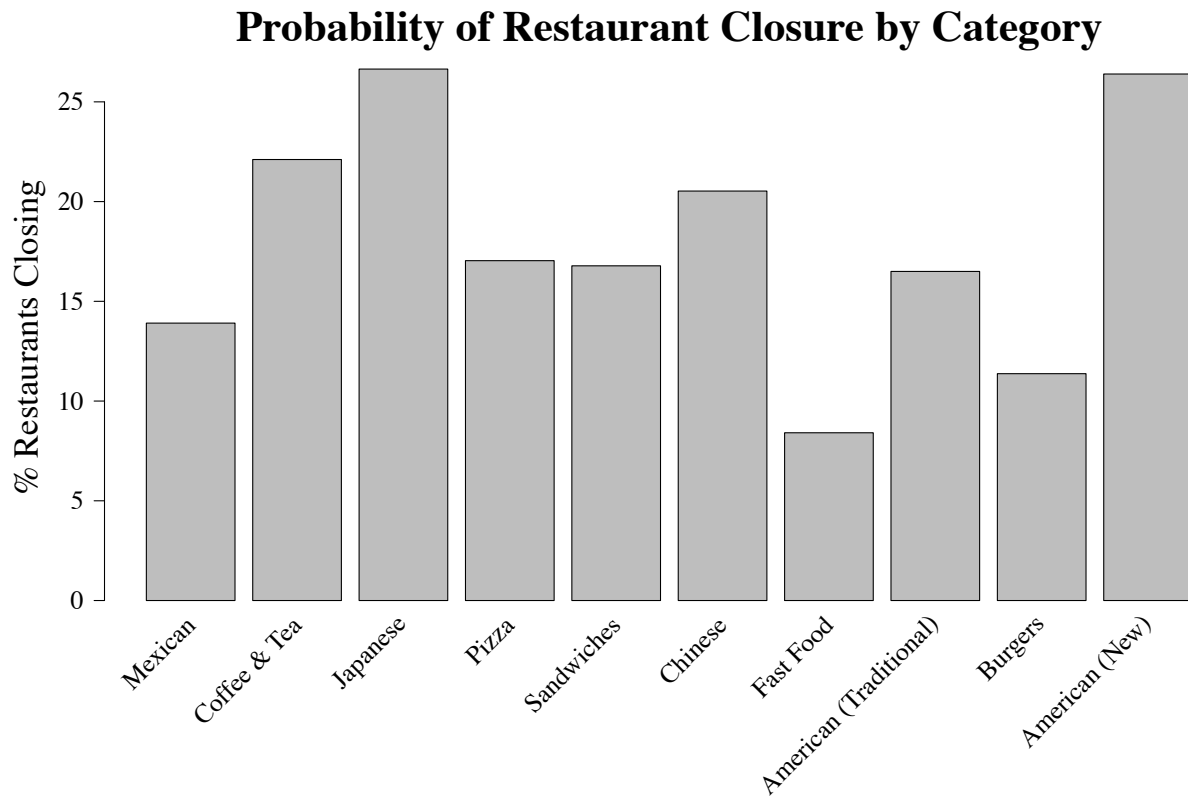


Figure 7: Probability of restaurant closure given category.

Probability of Restaurant Closure by Age

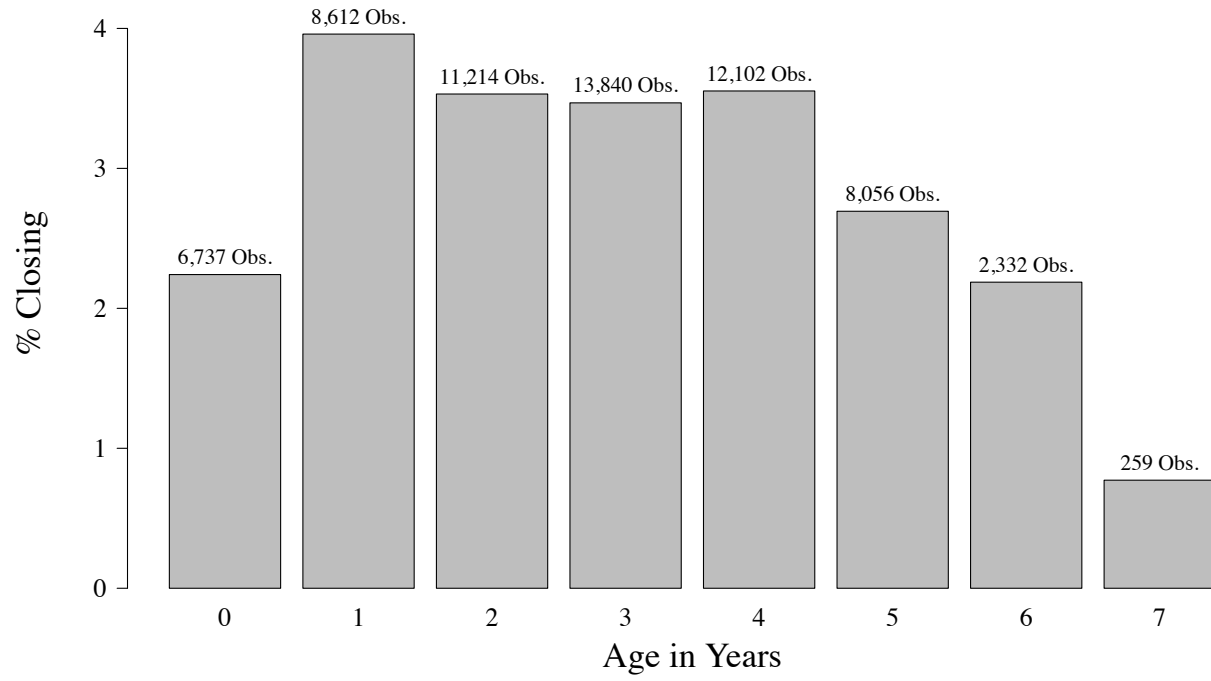


Figure 8: Probability of restaurant closure by restaurant age.

Distribution of Fitted Past Average Rating Values

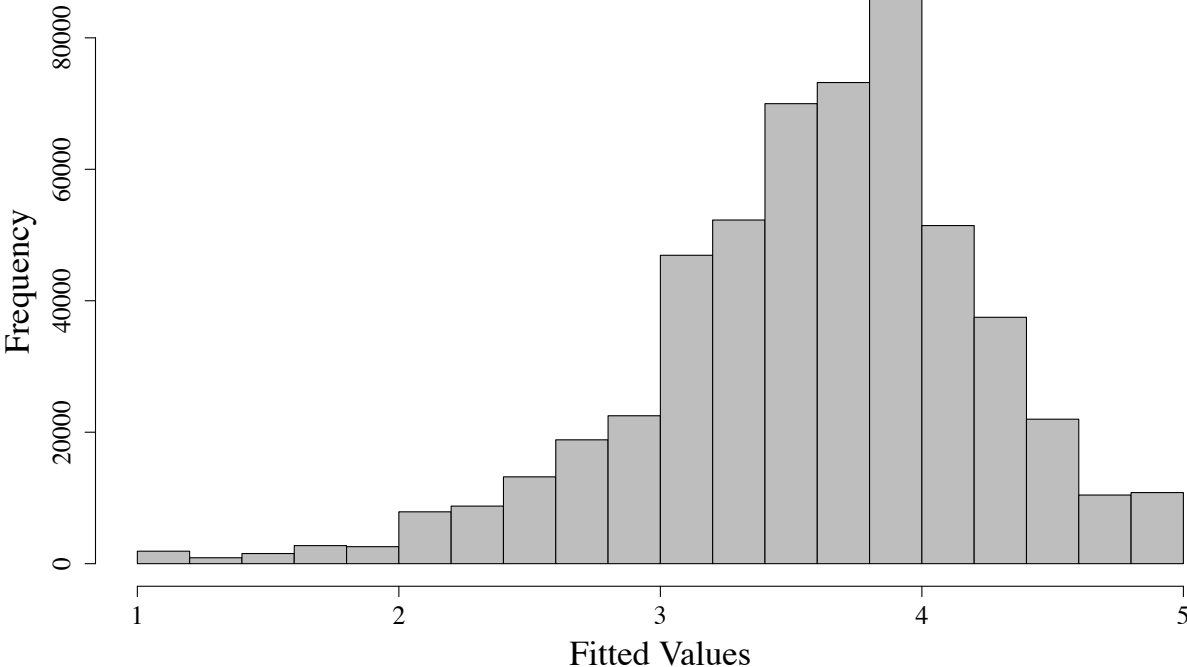


Figure 9: The distribution of predicted past average ratings resulting from fitting the transition function to the observed data.

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