# Position Effects in Search Advertising:

# A Regression Discontinuity Approach

Sridhar Narayanan Kirthi Kalyanam

Stanford University Santa Clara University\*

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#### Abstract

We investigate the causal effect of position in search engine advertising listings on outcomes such as click-through rates and sales orders. Since positions are determined through an auction, there are significant selection issues in measuring position effects. A simple mean comparison of outcomes at two positions is likely to be biased due to these selection issues. Additionally, experimentation is rendered difficult in this situation by competitors' bidding behavior, which induces selection biases that cannot be eliminated by randomizing the bids for the focal advertiser. Econometric approaches to deal with the selection are rendered infeasible due to the difficulty of finding suitable instruments in this context. We show that a regression discontinuity approach is a feasible approach to measure causal effects in this important context. We apply the approach to a large and unique dataset of daily observations containing information on a focal advertiser as well as its major competitors. Our regression discontinuity estimates demonstrate that there are significant selection biases in the more naive estimates, that selection effects vary by position and that there are sharp local effects in the relationship between position and outcomes such as click through rates and orders. We further investigate moderators of position effects. Position effects are stronger when the advertiser is smaller, and the consumer has low prior experience with the keyword for the advertiser. They are weaker when the keyword phrase has specific brand or product information, when the ad copy is more specific as in exact matching options, and on weekends compared to weekdays.

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# 1 Introduction

Search advertising, which refers to paid listings on search engines such as Google, Bing and Yahoo, has emerged in the last few years to be an important and growing part of the advertising market. An example is shown in figure 1 of the results of a search for the phrase "golf clubs" on Google, the most popular search engine. The order in which these paid listings are served is determined through a keyword auction, with advertisers placing bids to get specific positions in these listings, with higher positions costing more than lower positions. It is therefore crucial for advertisers to understand what the effect of position in search advertising listings is on outcomes such as click-through rates and sales.

Search advertising has been the focus of a significant stream of literature in multiple fields including marketing, economics and information systems. The effect of position in search advertising has been specifically of interest in this literature. Position in the search advertising listings is the main decision variable for the advertiser, given the limited ability to vary the content of the advertisement itself, and because it is the only variable with any significant cost implications. Position could affect consumer click-through and purchase behavior through multiple mechanisms, including signaling (Nelson, 1974; Kihlstrom and Riordan, 1984), consumer expectations about the advertisements being ordered on the basis of relevance (Varian, 2007), sequential search (Weitzman, 1979) and behavioral mechanisms such as attention (Hotchkiss, Alston, and Edwards, 2005; Guan and Cutrell, 2007). One or more of these mechanisms could simultaneously be at play, leading to position effects of search advertising. Several empirical studies have documented the relationship between position and behavioral outcomes such as click-through rates, conversion rates and sales (Agarwal, Hosanagar, and Smith, 2007; Ghose and Yang, 2009; Kalyanam, Borle, and Boatwright, 2010; Yang and Ghose, 2010; Rutz and Trusov, 2011).

However, measuring causal effects in this context is challenging due to the lack of experimental variation in position in search advertising listings. This is because position is determined through an online auction, with competing advertisers biddings for their advertisements to appear in the listings. This leads to position being endogenous. Past studies have tried to address this issue either

by conducting experiments in which bids for the focal firm are randomized (Agarwal, Hosanagar, and Smith, 2007), or by accounting for the potential endogeneity of position through a simultaneous equations approach that incorporates a parametric selection equation (Ghose and Yang, 2009; Rutz and Trusov, 2011; Kalyanam, Borle, and Boatwright, 2010). Experimentation is difficult in this context, since randomization of bids of the focal advertiser is insufficient to achieve randomization of position. This is because position is a function not just of the bids of the focal advertiser, but those of competing firms as well. Imagine, for instance, a competing advertiser bidding for higher positions on days when it expects higher sales due to a sales promotion. The sales promotion at this competing advertiser might lower sales at the focal advertiser. On such days, the focal advertiser's position would likely be lower due to the higher bids of the competitors. Even in the absence of a true position effect, such lower sales associated with lower position may be picked up spuriously as an effect of position on sales. Thus, while randomization of bids might eliminate the selection biases induced by the bidding behavior of the focal advertiser, the selection biases induced by competitors' strategic bidding behavior are not eliminated, and one cannot make causal inferences based on such an experiment. Instruments are difficult if not impossible to find in this context, since demand side factors that are correlated with position cannot typically be excluded from consumer outcome variables, and it is hard to find cost-side instruments that vary with position. Parametric selection equations are also likely problematic in this context, since there is a set of highly complex processes through which position is determined (Jerath, Ma, Park, and Srinivasan, 2011), the nature of the selection effects could vary by position and the use of an incorrect specification would lead to unpredictable biases in estimates of position effects. Furthermore, this approach requires the availability of a valid exclusion restriction with sufficient variation, which is hard to find in practice. Added to this is the fact that the simultaneous equations approach of specifying a parametric selection equation is computationally burdensome in practice. Thus, while there is an extant literature on position effects in search advertising, there is still a gap in the literature in finding causal position effects and its moderators. Advertisers are also interested in finding a robust and easily implementable approach to finding causal estimates of positions.

We present a regression discontinuity approach to finding causal position effects in search adver-

tising. The regression discontinuity (RD) design, a quasi-experimental approach was first developed in the program evaluation literature (Thistlethwaite and Campbell, 1960; Cook and Campbell, 1979; Shadish, Cook, and Campbell, 2001) and its econometric properties formalized by Hahn, Todd, and van der Klaauw (2001). It has been applied to the measurement of causal treatment effects in a variety of domains (see Imbens and Lemieux 2008; Lee and Lemeiux 2010; van der Klaauw 2008 for recent reviews of the literature). A recent literature has applied RD to measuring promotional effects (Busse, Silva-Risso, and Zettelmeyer, 2006; Busse, Simester, and Zettelmeyer, 2010; Nair, Hartmann, and Narayanan, 2011). RD measures causal treatment effects in situations where treatment is based on whether an underlying forcing variable crosses a threshold. With the treatment being the only discontinuity at this threshold, a discontinuous jump in the outcome of interest at the threshold is the treatment effect. Thus, RD measures the treatment effect as the difference between the limiting values of the outcome on the two sides of the threshold.

In the case of search engine advertising, the position is the outcome of an auction conducted by the search engine. In the typical auction, for instance that of Google, the advertisers are ranked on a score called AdRank, which is a function of the advertisers' bids and a measure given by the search engine that is termed  $Quality\ Score\ (Varian,\ 2007)$ . This leads to a viable RD design to measure the causal effect of a movement from one position to the adjacent one. Considering the higher position as the treatment, the forcing variable is the difference in the AdRanks for the bidders in the higher and lower positions. If this crosses 0, there is treatment, otherwise not. Thus, the RD estimator of the effect of position finds the limiting values of the outcome of interest (e.g. click through rates or sales) on the two sides of this threshold of 0. This application satisfies the conditions for a valid RD design laid out in Nair, Hartmann, and Narayanan (2011) and thus we obtain valid causal effects of position.

While the search engine observes the AdRanks of all the bidders, the bidders themselves only observe their own AdRanks. They observe their own bids, and the search engine reports the Quality Score to them ex-post. Hence they can construct their own AdRanks, but they do not observe the

<sup>&</sup>lt;sup>1</sup>Other search engines such as Bing have a similar mechanism to decide the position of the advertisement. Our empirical application uses data for advertisements at Google, which is also the largest search engine in terms of market share. Hence, the rest of the discussion will focus primarily on Google.

bids or Quality Scores of their competitors. Since the forcing variable for the RD design is the difference between competing bidders AdRanks, they cannot construct the forcing variable even expost. Added to that is the fact that the typical modified second-price auction mechanism for position auctions eliminates the incentives for advertisers to second-guess what their competitors are bidding. This ensures the local randomization required for the RD design, since this non-observability of competitors' AdRanks implies that advertisers cannot precisely select into a particular position At the same time, this poses a challenge to the empirical researcher wishing to use RD in this context, who obtains data from one advertiser and only typically observes the AdRank for that advertiser - thus not allowing for the forcing variable to be observed. However, we have obtained a unique dataset that contains information on bids and AdRanks and performance information for a focal advertiser and its main competitors. All of these firms were major advertisers on the Google search engine, and we have a large number of observations where pairs of firms in our data were in adjacent positions. We have historical information from these firms for a period when they operated as independent firms, with independent advertising strategies. Thus, for a large number of observations, we have AdRanks and performance measures for advertisers in adjacent positions. We are thus able to implement a valid RD design to measure the treatment effects. This situation is similar to the type of data that would be available to a search engine, which can report causal position effects to the advertiser.

We estimate the effect of position on two main outcomes of interest - click through rates and sales orders (i.e. whether the consumer who clicked on the search advertisement purchased at that or a subsequent occasion). We find that position positively affects click-through rates, with higher positions getting greater clicks. However, these effects are highly localized, with significant effects at certain positions and no significant effects at others. We find that position effects are largely insignificant when it comes to sales orders, with the only exception being at the typical position where consumers have to scroll down to see the next advertisement. We also document several interesting findings about moderators for position effects. In general, we find stronger position effects for smaller advertisers, for keyword phrases where the advertiser generally has a weaker recognition, for keyword phrases that are less specific about the product or brand, and where the

advertiser allows the search engine to display the ad even where the advertised keyword phrase is not an exact match to the keyword phrase the consumer searches for (referred to as 'broad match' as opposed to 'exact match'). We also document interesting differences in the position effects between weekdays and weekends, with the differences being driven perhaps by different search costs on weekends vs. weekdays.

This paper makes several contributions to the literature. First, this paper tries to make causal inferences on position effects, which have been hard to make in the literature so far due to limitations in the data and in the empirical strategies used. Second, it demonstrates the nature of the selection bias that can result in these contexts, not only due to observed factors such as the advertiser, keyword, etc. but also due to other unobserved factors that drive strategic bidding behavior of firms. Third, it documents moderators to these effects across type of advertisers, type of keyword, the advertisement match-type and across weekday vs. weekend, which have not been documented before. Finally, we present a novel application of regression discontinuity to an important context where it has not been considered before, and where other ways of obtaining causal effects are typically infeasible. This approach could be applied by search engines to find position effects for their advertisers with data that they already have, with relatively simple econometric methods and without the cost and effort involved in experimentation.

The rest of the paper is organized as follows. We give some background on search advertising in general and position effects in particular in section 2. In section 3, we discuss the selection of position in search advertising contexts, and extant approaches to deal with the issue. In section 4, we discuss a regression discontinuity approach to find causal effects. We discuss the data in section 5 and the results of our empirical analysis in section 6. We conclude in section 7.

# 2 Background on search advertising

## 2.1 Overview of search advertising

Search advertising involves placing text ads on the top or side of the search results page on search engines. An example is shown in figure 1 of the results of a search for the phrase "golf clubs" on

Google the most popular search engine. Search advertising is a large and rapidly growing market. For instance, Google reported revenues of almost \$14.9 billion for the quarter ending September 30, 2013, with a growth of 12% over the same period in the previous year. The revenues from Google's sites, primarily the search engine, accounted for 68% of these revenues.<sup>2</sup> According to the Internet Advertising Bureau, \$16.9 billion was spent in the United States alone on search advertising in 2012. Search advertising is the largest component of the online advertising market, with 46% of all online advertising revenues in 2012. Despite the fact that it is a relatively new medium for advertising, search is the third largest medium after TV and Print, and surpassed Radio in 2012.<sup>3</sup>

Several features of search advertising have made it a very popular online advertising format. Search ads are triggered by specific keywords (search phrases). For example consider an advertiser who is selling health insurance for families. Some of the search phrases related to health insurance could include "health insurance", "family health insurance", "discount health insurance" and "California health insurance". The advertiser can specify that an ad will be shown only for the phrase "family health insurance". Further, these ads can be geography specific, with potentially different ads being served in different locations. This enables an advertiser to obtain a high level of targeting.

Search advertising is sold on a "pay for performance" basis, with advertisers bidding on keyword phrases. The search engine conducts an automated online auction for each keyword phrase on a regular basis, with the set of ads and their order being decided by the outcome of the auction. Advertisers only pay the search engine if a user clicks on an ad and the payment is on a per click basis (hence the commonly used term - PPC or pay per click for search advertising). By contrast, online display advertising is typically sold on the basis of impressions, so the advertiser pays even if there is no behavioral response. In search advertising, advertisers are able to connect the online ad to the specific online order it generated by matching cookies. The combination of targeting, pay for clicks and sales tracking make the sales impact of search advertising highly measurable. This creates strong feedback loops as advertisers track performance in real time and rapidly adjust their

<sup>&</sup>lt;sup>2</sup>These data were obtained from Google's earnings report for Q3 2013, available at https://investor.google.com/earnings/2013/Q3 google earnings.html (last accessed on October 31, 2013).

<sup>&</sup>lt;sup>3</sup>Internet Advertising Bureau's report on internet advertising can be accessed at http://www.iab.net/media/file/IABInternetAdvertisingRevenueReportFY2012POSTED.pdf (last accessed on October 31, 2013).

spending.

Before we move on to position effects, we discuss the auction mechanism by which search engines such as Google decide positions of advertisers. Advertisers bid on keywords, with the bid consisting of the maximum amount that the advertiser would pay the search engine every time a consumer clicked on the search ad. Since the search engine gets paid on a per click basis, revenue is maximized if the winning bidder has higher product of bid and clicks. Thus, Google ranks bidder not on their bids, but on a score called AdRank, which is the product of bid and a metric called Quality Score assigned by Google. While the exact procedure by which Google assigns a Quality Score to a particular ad is not publicly revealed, it is known that it is primarily a function of expected click through rates (which Google knows through historical information combined with limited experimentation), adjusted up or down a little bit by factors such as the quality of the landing page of the advertiser. The positions of the search ads of the winning bidders is then in descending order of their AdRanks. The winning bidder pays an amount that is just above what would be needed to win that position. Thus, the cost per click of the winning bidder in position i is given by

$$CPC_{i} = \frac{Bid_{i+1} \times QualityScore_{i+1}}{QualityScore_{i}} + \varepsilon \tag{1}$$

where  $\varepsilon$  denotes a very small number.

#### 2.2 Position effects

One of the most important issues in search advertising is the position of the ad on the page. Since the position of an ad is the outcome of an auction, higher positions cost more for the advertiser, everything else remaining equal, and hence would be justified only if they generate higher returns for the advertiser. Measurement of causal position effects are thus of critical importance to the advertiser.

A variety of mechanisms can lead to positions affecting outcomes such as clicks and sales. One mechanism could be that of signaling (Nelson, 1974; Kihlstrom and Riordan, 1984). In this mechanism, which might be most relevant for experience goods, advertisers with higher quality goods

spend greater amounts on advertising in equilibrium, and consumers take advertising expenses as a signal of product quality. Since it is well known that advertisers have to spend more money to obtain higher positions in the search advertising results, consumers might infer higher positions as a signal of higher quality.

A second mechanism might relate to consumers' learned experience about the relationship between position and the relevance of the advertisement. The auction mechanism of search engines such as Google inherently scores ads with higher relevance higher (Varian, 2007). Over a period of time, consumers might have learned that ads that have higher positions are more likely to be relevant to them. Since consumers incur a cost (in terms of time and effort) each time they click on a link, they might be motivated to click on the higher links first given their higher expected return from clicking higher links. Such a mechanism is consistent with a sequential search process followed by the consumer (Weitzman, 1979), where they start with the ad in the highest position and move down the list until they find the information they need. Chen and He (2006) show, using an analytical model, that it is viable equilibrium for advertisers with higher relevance to be positioned higher and consumers to be more likely to click on higher positions. By contrast, Katona and Sarvary (2010) and Jerath, Ma, Park, and Srinivasan (2011) discuss situations under which it may be optimal for firms to not be ranked in order of relevance or quality, and clicks to also not necessarily be higher for higher placed search ads.

A third mechanism that could drive position effects is that of attention. Several studies have pointed to the fact that consumers pay attention only to certain parts of the screen. Using eye-tracking experiments, these studies show that consumers pay the greatest attention to a triangular area that contains the top three ad positions above the organic results and the fourth ad position at the top right. Such an effect is particularly pronounced on Google and is often called the Google golden triangle (Hotchkiss, Alston, and Edwards, 2005; Guan and Cutrell, 2007). The reasons for such an effect may be due to spillovers from attention effects for organic (unpaid) search results. The organic search results are sorted on relevance to consumers, and hence consumers may focus their attention first on the top positions in the organic search results. Since search advertising results are above or by the side of organic search results, consumers' attention might be focused

on those ads that are closest to the organic results they are focused on. Thus, in addition to the economic mechanisms such as signaling and relevance, there might be behavioral mechanisms for position effects. In general, whether there are significant position effects at a particular position is an empirical question.

### 2.3 Moderators of position effects

Whether there are position effects at particular positions is an interesting first order question in itself. However, advertisers may also be interested in learning if there are moderators to these effects, across different types of advertisers, different types of keywords, etc. Additionally, advertisers in search engines such as Google have decisions to make about the nature of targeting - specifically, they need to decide whether to bid for a keyword to appear as a 'broad match' ad (where the advertisement is shown when the keyword phrase searched for by the consumer is close to but is not an exact match to the advertised keyword phrase) or an 'exact match'.

How advertising effects vary by advertiser has been the topic of considerable interest in prior research. For example, Lodish, Abraham, Kalmensen, Livelsberger, Lbetkin, Richardson, and Stevens (1995) provide an empirical generalization that advertising elasticities vary widely across brands. Advertising elasticities are higher for durables than for nondurables (Sethuraman and Tellis 1991). They also decrease over the product life cycle and hence are higher for new brands compared to existing brands (Parker and Gatignon 1996). Advertising has been found to more effective for experience goods than for search goods (Hoch and Ha 1986; Nelson 1974). However to the best of our knowledge there has been no work examining how position effects in search advertising vary across advertisers. This is an important question to study since some studies in the theoretical literature on position auctions (see for instance Varian 2009) assume that position effects (for example the ratio of click through rates across positions) are independent of advertiser. However, this assumption has not been empirically tested until now. Other theoretical studies (e.g. Jerath, Ma, Park, and Srinivasan 2011) have pointed to mechanisms by which position effects may vary for higher quality and lower quality firms. Since we have information for multiple firms in our data set that are selling similar categories in the same time period it provides a unique opportunity to empirically examine

how position effects vary across advertisers.

Another aspect of advertising that has received attention in the literature is that prior experience with the product or firm is a substitute for advertising. For example Deighton, Henderson, and Neslin (1994) show that advertising is effective in attracting consumers who have not recently purchased the brand (low recent experience) but advertising does little to change the repeat-purchase probabilities of the consumers who have just purchased the brand (high recent experience). Ackerberg (2001) reports that advertising's effect on inexperienced consumers is positive and significant whereas it has a small and insignificant effect on experienced consumers. Narayanan and Manchanda (2009) also find that experience and advertising are substitutes in the context of a learning model. In the context of our data, one of the key components of the search engine assigned Quality Score is the historic click through rates (CTR) that the firm obtains on its ads. It also reflects the overall historic CTR of all the ads and keywords for the advertiser, and the quality of the advertiser's landing page. Since a searcher who has clicked through in the past has actually experienced the firm's online offering, the different components of Quality Score together provide a plausible measure of the stock of aggregate prior digital experience that searchers have with the firm. So analyzing how position effects vary for ads with different Quality Scores allows us to examine the linkage between position effects and prior experience in the context of search advertising.

Prior research has also focused on the distinction between category and brand terms in search advertising. For example the keyword Rayban sunglasses would be classified as a brand phrase, since it contains specific brand/product information, whereas the keyword sunglasses would be categorized as a category phrase. Prior research has focused on the sequential use of category searches followed by brand searches. Lamberti (2003) reports that consumers initiated their search process with category terms and used more specific brand terms later in the purchase process. Rutz and Bucklin (2011) report that increased exposure to category advertising terms leads to increased searches of brand terms. So there is some evidence that the use of category terms precedes the use of brand terms. These findings are consistent with the literature on product category expertise. As per this literature, consumers who are early in the purchase process know little about the product category or the underlying attributes in a product or a service. They may not know what questions

to ask (Sheth, Mittal, and Newman, 1999, Ch. 14, p. 534) and have a limited consumption vocabulary (West, Brown, and Hoch, 1996). Hence the use of broad category terms early in the search process. In addition to the sequence of usage, there are also implications for how position effects vary for category terms versus brand terms. Since searchers who use category terms are early in the search process and have a lower level of knowledge and experience in the product, they might rely on the information in position more than searchers who use brand terms. This distinction in position effects has not been examined in prior empirical work.

Google and other search engines also allow advertisers to set match criteria when they bid on ads for particular keyword phrases. On Google, these match types include broad match, where the ad is displayed when there is an imprecise match between the consumer's search term and the keyword phrase that the advertiser is bidding on, and exact match, where the ad is displayed only in cases where the two match exactly. As a result the headline and ad copy of the exact match ad will match the consumer's query better when compared to a broad match ad. To put it differently, with exact match ads advertisers can provide more precise information to consumers with the headline and ad copy compared to a broad match ad. Since the information in exact match ads is higher, we can expect position effects to be less salient compared to broad match ads. This is consistent with the prior discussion regarding high versus low quality keywords, and category versus brand keywords on how information or experience substitute for position. This distinction between broad and exact match types has not been examined in the existing literature.

The distinction between weekday and weekends effects is an important one in retailing. For example Warner and Barsky (1995), in the context of offline retailing suggest that consumer search costs are lower on the weekends. If consumers search costs are lower on weekends, they are more likely to search lower down the advertising results on a search engine page before stopping. This would imply that position effects are stronger on the weekdays compared to the weekends. We examine this distinction in our empirical analysis.

## 3 Selection issues

### 3.1 Selection on observables

As we have discussed in the previous section, measuring causal position effects is of critical importance to the retailer. However, there are likely significant selection biases in naive estimates, and we discuss them in this section.

First, we discuss the selection biases that may result if we compare outcomes for different positions by pooling observations across advertisers, keywords, match-types, days etc, which is a common strategy in empirical work. Consider the case where we observe positions and outcomes for a set of keywords. It is likely that there are systematic differences in click through rates across different keywords. Due to the auction mechanism itself, keywords with high click through rates in the past would typically have higher positions because they are assigned higher Quality Scores. Since these keywords are typically also likely to get higher clicks in the future, there is a correlation between position and click through rates that is not causal, but instead driven by the auction mechanism. Similar arguments can be made about spurious effects when pooling across advertisers, match-types etc. If panel data are available, fixed effects for keywords, advertisers, match-type etc. could eliminate these selection baises.

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#### 3.2 Selection on unobservables

In addition to selection biases for observables, there is potential for selection on unobservables. For example selection may also be induced by the the bidding behavior of advertisers. Advertisers in the search advertising context often use bidding engines to decide bids for keyword phrases, since they typically deal with a very large number of keywords (for instance, the advertisers in our empirical application bid on several tens of thousands of keywords on any given day). These bidding engines can be programmed to use specific bidding rules, with adjustments made to these rules on a case by case basis. For instance, advertisers often set a fixed advertising to sales ratio for deciding advertising budgets. In the search engine context, this involves a continuous feedback loop

from performance measures to the bidding engine. As sales per click increases, the bidding engine might be programmed to automatically increase advertising budgets, which in turn increases their bid amounts and hence ensures higher positions for their ads. Similarly, as sales drop, advertising budgets and eventually position also fall. Such a mechanism would induce a positive bias in position effects, as higher position might be induced by increasing sales rather than the reverse.

A negative bias is also feasible due to other rules used by advertisers in setting their bids. Consider an advertiser who has periodical sales, with higher propensity of consumers to visit their sites even without search advertising during that period (through other forms of advertising or marketing communication, such as catalogs for instance). The advertiser may in this instance reduce their search advertising budgets if they believe that they would have got the clicks that they obtain through search advertising anyway, and without incurring the expense that search advertising entails. Thus, they may generate high clicks and sales, even though their strategy is to spend less (and hence obtain lower positions) on search advertising during this period. This mechanism would induce a negative bias on estimates of position effects.

Another potential cause for selection biases is competition. Since search advertising positions are determined through a competitive bidding process, the bidding behavior of competitors could also induce biases in naive estimates of position effects. Consider a competing bidder, who offers similar products and services as the focal advertiser, with data on the competing bidder unavailable to the latter. Due to mechanisms similar to those described above, competing bidders may place high or low bids when their sales are high. Since the competing bidder offers similar products as the focal advertiser, higher sales for the competing bidder, for instance due to a price promotion, may lower the sales for the focal advertiser. Even click through rates for the focal advertiser could be affected if the search advertising listing for the competitor mentions that there is a price promotion at that website. At the same time, the competing bidder may place a low bid on the keyword auction through a similar set of mechanisms as the ones described earlier in this subsection, thus pushing the focal advertiser higher in position. This negative correlation between position and sales for the focal advertiser induced by the price promotion at the competing advertiser's website and the unobserved strategic bidding behavior by the competitor would be picked up as a position effect

by a naive analysis. In general, any unobservables that affect positions through the bidding behavior of the competing advertiser may also affect outcomes such as sales and click through rates for the focal advertiser, and this would induce selection biases.

To sum up, there are significant selection issues that may render naive estimates of positions highly unreliable, with unpredictable signs and magnitude of the biases induced by selection on unobservables.

### 3.3 Extant approaches to deal with selection

As mentioned in the introduction, position effects have been studied in the literature. An early study of the effect of position was Agarwal, Hosanagar, and Smith (2007), which concluded that click through rates decrease monotonically as one moves down the search advertising listings, but conversion rates go up and then down. This study controls for heterogeneity across keywords, but not across match-types, days etc. Further, it does not control for selection on unobservables. Instead, it reports a robustness check using an experimental design with randomly varying bids for a small number of observations. For robust results for even the main effect of positions, the experiment would need to be carefully designed to randomize bids across the various keywords, positions, days of week, match-type etc., and a small number of observations would typically not be sufficient. Exploring moderators for these effects would greatly increase the number of observations needed. Irrespective of the scale of the experiment, it cannot eliminate selection biases induced by strategic bidding behavior of competitors. For instance, if a competing advertiser bids lower on days when they have sales promotions than on other days, their low bids can drive the focal advertiser's position to be higher everything else remaining equal. Due to the sales promotion at this competing advertiser's website, the clicks and sales at the focal advertiser may be lower. This would lead to a negative correlation between positions and outcome variables such as clicks and sales. This spurious correlation cannot be eliminated by randomizing the bids of the focal advertiser alone<sup>4</sup> This discussion demonstrates why experimentation is difficult in this context, since it would require

<sup>&</sup>lt;sup>4</sup>In the context of advertising on the Google search engine, positions are reported only on a daily basis. Furthermore, data on clicks or sales within a shorter period of time within a day would typically have a lot of zeroes and hence lower variation. Across multiple days of experiments, it would be hard to make the case that competing advertisers do not vary their bids in a manner that induces selection in positions.

randomization of bids of all bidders - the focal bidder and its competitors, and on a large scale. This is typically infeasible for a given advertiser. A search engine could randomize positions of all advertisers, but large scale experimentation is difficult for the search engine as well without the concurrence of all the advertisers. Furthermore, it is expensive for the search engine to randomize the positions on a large scale, since experimental pages typically do not generate revenues for it.

A second approach has been to control for selection by modeling the process by which positions are determined using a parametric specification, and jointly estimating both the outcome and position equations (Ghose and Yang, 2009; Kalyanam, Borle, and Boatwright, 2010; Yang and Ghose, 2010). The selection in positions is explicitly modeled by estimating correlations between the errors of the two equations. This approach crucially depends on the validity of the parametric specification of the position equation. It might be hard to come up with a parametric specification for the position equation given that position is determined through an auction. Typically, a parametric specification is assumed for the position as a function of lagged variables for the focal advertiser, with no information on competitors. It would be difficult to control for the selection issues induced by competitors' strategic bidding behavior using such a specification. Furthermore, we have seen that there is a set of complex processes at work even within the focal bidder, with potentially different mechanisms operating at different times inducing biases of opposite signs. Such complexities would be hard to capture using a parametric specification. Added to this is the fact that such an approach is demanding computationally and requires that the researcher has access to appropriate exclusion restrictions that are necessary for identification of parameters.

A third approach, adopted by Rutz and Trusov (2011) is to instrument for the position. Since an instrumental variable that is valid and has sufficient variation is hard to come by, this study uses the latent instrumental variables approach of Ebbes, Wedel, and Bockenholt (2005) to account for the potential endogeneity of position. The method relies on several crucial assumptions - normality of the outcome equation and departures from normality for the position equation. While the latter is not problematic, the former may be more problematic. As we will see in our empirical application, outcomes such as click through rates and sales are highly non-normal. For instance, periodical sales and promotional events, if unobserved in the data, would induce the distribution of the outcome

variables to be skewed and even potentially multi-modal. This would make the approach challenging in many contexts. Further this approach relies on a single latent instrument variable model, implicitly assuming that selection effects do not vary across position. Finally this approach relies on the assumption that position effects can be modeled with a parametric specification, whereas the complex multiple mechanisms underlying selection in position (for instance, own strategic bidding, competitive bidding, with the set of competitors being potentially different across different positions) imply that the selection bias is likely to be highly local in nature, making a parametric specification subject to the potential for specification bias.

To sum up, the extant literature has either ignored the endogeneity/selection issues altogether, or taken parametric approaches to control for endogenous positions, with the first being problematic, and the second having significant limitations. Further, this is a situation where experimentation, which is the typically advocated approach for obtaining causal effects, is usually infeasible.

# 4 Applying regression discontinuity to finding position effects

## 4.1 Regression discontinuity

Regression discontinuity (RD) designs can be employed to measure treatment effects when treatment is based on whether an underlying continuous forcing variable crosses a threshold. Under the condition that there is no other source of discontinuity, the treatment effect induces a discontinuity in the outcome of interest at the threshold. Thus, the limiting values of the outcome on the two sides of the threshold are unequal and the difference between these two directional limits measures the treatment effect. A necessary condition for the validity of the RD design is that the forcing variable itself is continuous at the threshold (Hahn, Todd, and van der Klaauw, 2001) and this is achieved in the typical marketing context if the agents have uncertainty about the score or the threshold. (Nair, Hartmann, and Narayanan, 2011).

Formally, let y denote the outcome of interest, x the treatment and z the forcing variable, with  $\bar{z}$  being the threshold above which there is treatment. Further define the two limiting values of the outcome variable as follows

$$y^{+} = \lim_{\lambda \to 0} \mathbb{E}[y|z = \bar{z} + \lambda] \tag{2}$$

$$y^{-} = \lim_{\lambda \to 0} \mathbb{E}\left[y|z = \bar{z} - \lambda\right] \tag{3}$$

Then the local average treatment effect is given by

$$d = y^+ - y^- \tag{4}$$

Practical implementation of RD involves finding these limiting values non-parametrically using a local regression, often simply a local linear regression within a pre-specified bandwidth  $\lambda$  of the threshold  $\bar{z}$  and then assessing sensitivity to the bandwidth. More details on estimating causal effects using RD designs, including the difference between sharp and fuzzy RD designs, the selection of non-parametric estimators for  $y^+$  and  $y^-$ , the choice of bandwidth  $\lambda$  and the computation of standard errors can be found in Hahn, Todd, and van der Klaauw (2001) and Imbens and Lemieux (2008).

# 4.2 RD in the search advertising context

As described earlier in section 2.1, positions in search advertising listings are determined by an auction, with bidders ranked on a variable called AdRank, which in turn is the product of the bid and the  $Quality\ Score$  assigned by Google to the bidder for each specific keyword phrase for a particular match-type. The application of RD to this context relies on knowledge of the AdRank of competing bidders for a given position. Specifically, if bidder A gets position i in the auction and bidder B gets position i+1, it must be the case that

$$AdRank_i > AdRank_{i+1} \tag{5}$$

or in other words

$$\Delta AdRank_i \equiv (AdRank_i - AdRank_{i+1}) > 0 \tag{6}$$

The forcing variable for the RD design is this difference in AdRanks and the threshold for the treatment (i.e. the higher of the two positions) is 0. The RD design measures the treatment effect by comparing outcomes for situations when  $\Delta AdRank_i$  is just above zero and when it is just below zero. Thus, it compares situations when the advertiser just barely won the bid to situations when the advertiser just barely lost the bid. This achieves the quasi-experimental design that underlies RD, with the latter set of observations acting as a control for the former.

For an RD design to be valid, it should be the case that the only source of discontinuity is the treatment. One consequence of this condition is that RD is invalidated if there is selection at the threshold. If it is the case that an advertiser can select his bid so as to have an AdRank just above the threshold, the RD design would be invalid. However, what comes to our assistance in establishing the validity of RD is the modified second price auction mechanism used by Google. As per this mechanism, the winner actually pays the amount that ensures that its ex post AdRank is just above that of the losing bidder. Specially, the cost per click for the advertiser is determined as in equation 1, and this ensures that ex post, the following is true.

$$\Delta AdRank_i \equiv (AdRank_i - AdRank_{i+1}) > \varepsilon \tag{7}$$

where  $\varepsilon$  is a very small number. An important consequence of this modified second price mechanism is that it is approximately optimal<sup>5</sup> for advertisers to set bids so that they reflect what the position is worth to them as opposed to setting bids such that they are just above the threshold for the position. Thus, the second-price auction design eliminates incentives for advertisers to second guess their competitors bids and put in their own bids so as to have an AdRank just above that of their competitors.

Further, AdRanks are unobserved ex ante by the advertiser. Their own AdRanks are observed ex post, since Google reports the Quality Score on a daily basis at the end of the day, and the advertiser observes only his own bid ex ante. However, AdRanks of competitors are not observed even ex post. Thus, the advertiser cannot strategically self-select to be just above the cutoff. Occasions when the advertiser just barely won the bid and when he barely lost the bid can be considered equivalent in

<sup>&</sup>lt;sup>5</sup>See Varian (2007) for a discussion on this.

terms of underlying propensities for click throughs, sales etc. Any difference between the limiting values of the outcomes on the two sides of the threshold can be entirely attributed to the position. The fact that AdRanks of competitors are unobserved satisfies the conditions for validity of RD laid out in Nair, Hartmann, and Narayanan (2011), with the advertiser being uncertain about the score ( $\Delta AdRank$ ).

Typically, only the search engine observes the AdRanks for all advertisers. Therefore, the RD design could be applied by the search engine, but not by advertisers, or by researchers who have access to data only from one firm. Unfortunately, search engines like Google are typically unwilling to share data with researchers, partly due to the terms of agreement with their advertisers. However, we have access to a dataset where we observe AdRanks for four firms in the same category. One of these firms acquired the three other firms in this set, and hence we have access to data from all firms, including from a period where they operated and advertised independently. We describe the data in more detail in section 5.

It would also be relevant at this stage to discuss the role of other unobservables in this approach. In our empirical application, we have observations for four firms in the category, which constitute an overwhelming share of sales and search advertising in this market. However, it is possible that there are other advertisers that we do not observe in our dataset. This is not problematic in our context, since our analysis is only conducted on those sets of observations where we observe AdRanks for pairs of firms within our dataset. Since our interest is in finding how position affects outcomes, everything else remaining constant, we conduct a within firm, within keyword, within match-type and within day-of-week analysis, with the AdRank data for the firms and competitors only used to classify which observations fall within the bandwidth for the RD design. Thus, the presence of other firms not in our dataset does not affect our analysis.

## 4.3 Implementing the RD design to measure position effects

In this sub-section, we describe how to implement the RD design to measure the effect of position on click-through rates. An analogous procedure can be easily set up to measure position effects on other outcomes such as conversion rates, sales etc. Consider the case where we wish to find the effect of moving from position i + 1 to position i on the click through rate. Note that the  $(i + 1)^{th}$  position is lower than the  $i^{th}$  position. Let  $y_j$  refer to the outcome (e.g. click through rate) for the advertisement j (which is a unique identifier of a particular keyword phrase, an advertiser, a specific day and a specific match-type).  $AdRank_j$  refer to the AdRank for that ad, and  $pos_j$  refer to the position of the ad in the search engine listings. The following steps are involved in implementing the RD design to measure the incremental click through rates of moving from position i + 1 to position i.

- 1. Select observations for which we observe AdRanks for competing bidders in adjacent positions. This is because the forcing variable  $z_j$  for the RD design is the difference between the AdRanks of adjacent advertisers, i.e.  $\Delta AdRank$ . For an advertiser in position  $i, z_j$  is the difference between that advertiser's AdRank and that of the advertiser in position i + 1 and has a positive value. For an advertiser in position  $i + 1, z_j$  is the difference between the advertiser's AdRank and that of the advertiser in position i and has a negative value.
- 2. Select the bandwidth  $\lambda$  for the RD. This could be an arbitrary small number, with the researcher checking for robustness of estimates to the choice of bandwidth. In our case, we select the optimal bandwidth using a "leave one out" criterion described later in this section.
- 3. Retain observations with score within the bandwidth  $\lambda$ . The RD design compares observations for which  $0 < z_j < \lambda$  with those for which  $-\lambda < z_j < 0$ . Thus, retain observations for which  $|z_j| < \lambda$ .
- 4. Find the position effect using a local linear regression. One could use a local polynomial regression but a local linear regression works well in this context due to its boundary properties (see for instance Fan and Gijbels 1996; Imbens and Lemieux 2008). The local linear regression is the following regression applied to the subset of the data within the bandwidth.

$$y_{j} = \alpha + \beta \cdot 1 \left( pos_{j} = i+1 \right) + \gamma_{1} \cdot z_{j} + \gamma_{2} \cdot z_{j} \cdot 1 \left( pos_{j} = i+1 \right) + f\left( j:\theta \right) + \varepsilon_{i}$$
 (8)

In this regression,  $1 (pos_j = i + 1)$  is an indicator for whether the ad is in the higher position,

 $\beta$  is the position effect of interest. The  $\gamma_1$  and  $\gamma_2$  parameters control for the variation in CTR with changes in the forcing variable, allowing the variation to be different on the two sides of the threshold through an interaction effect. The  $f(j:\theta)$  term includes a set of fixed effects with  $\theta$  being the parameter vector.

5. Find the bandwidth  $\lambda^*$  using a "leave one out cross validation" (LOOCV) optimization procedure described in Ludwig and Miller 2007 and Imbens and Lemieux (2008). This involves leaving out one observation  $y_k$  at a time, and finding the parameter estimates using the remaining observations. These estimates are then used to find a predicted value  $\hat{y}_k$  for that left out observation. Since RD estimates involve finding the limiting values of the outcomes on the two sides of the threshold, we use only observations very close to the threshold (i.e. with  $\tilde{\lambda} < \lambda$ ) for the cross validation. The criterion that is used to find the optimal bandwidth is the mean squared error of these predictions.

$$C_Y = \frac{1}{N} \sum_{\left\{k : -\tilde{\lambda} < z_k < \tilde{\lambda}\right\}} (\hat{y}_k - y_k)^2 \tag{9}$$

where N is the number of observations with forcing variable  $z_k$  lying between  $-\tilde{\lambda}$  and  $\tilde{\lambda}$ . The optimum bandwidth is then

$$\lambda^* = \arg\min_{\lambda} \left( C_Y \right) \tag{10}$$

# 5 Data Description

Our data consist of information about search advertising for a large online retailer of a particular category of consumer durables<sup>6</sup>. This firm, which is over 50 years old started as a single location retailer, expanding over the years to a nationwide chain of stores both through organic growth and through acquisition of other retailers. Since the category involves a very large number of products, running into the thousands, a brick and mortar retail strategy was dominated in terms of its economics by a direct marketing strategy. Thus, over the years, its strategy evolved to stocking

<sup>&</sup>lt;sup>6</sup>We are unable to disclose the name of the firm or details of the category due to confidentiality concerns on the part of the firm.

a relatively small selection of entry-level, low-margin products with relatively high sales rates in the physical stores, with the very large number of slower moving, high margin products being sold largely through the direct marketing channel. Recently, the firm acquired three other large online retailers. Two of the four firms are somewhat more broadly focused, while two others are more narrowly focused on specific sub-categories. However, each of them has significant overlaps with the others in terms of products sold. For a significant period of time after the acquisition, the firms continued to operate independently, with independent online advertising strategies. Our data have observations on search advertising on Google for these four firms, and crucially for the period where they operated as independent advertisers.

We have a total number of about 28.5 million daily observations over a period of 9 months in the database, of which about 13.1 million observations involve cases where two or more advertisers among the set of 4 firms bid on the same keyword. Since the keywords are often not in adjacent positions, we filter out observations where the observations are not adjacent. We also drop observations where we don't have bids and *Quality Scores* for both of the adjacent advertisements. Since the position reported in the dataset is a daily average, we also drop observations where the average positions are more than 0.1 positions away from the nearest integer. We are thus left with a total of 414310 observations where we observe advertisements in adjacent positions, spanning 22825 unique keyword phrase/match-type combinations. An overwhelming majority (79.4%) of the 22825 keywords are of the broad match-type, and the rest are of the exact match-type. There are a total of 19205 unique keywords in this analysis dataset, with most exact match-type keywords also advertised as broad match type, but not necessarily vice versa.

Table 1 has the list of variables in the analysis dataset (including variables we have constructed such as click through rates, conversion rates and sales per click) and the summary statistics for these variables. Observations are only recorded on days that have at least one impression, i.e. when the ad was served at least once. Through a tracking of cookies on consumer's computers, each click is linked to a potential order, sales value, margin etc. As per standard industry practice, a sales

<sup>&</sup>lt;sup>7</sup>Google reports only the daily average positions for any ad. Variation in position within a day may be because of different ads being served in different geographies, and a limited degree of experimentation by Google itself to calibrate *QualityScores* for all advertisers.

order is attributed to the last click from a search ad within an attribution window with previous clicks not getting credit for these sales.

To summarize, we have obtained a unique dataset, consisting of information at a daily level on keywords, type of match, bids, quality score and key performance metrics for the advertisement. To the best of our knowledge, this is the very first time that a dataset has been assembled which includes information on AdRanks, clicks and sales outcomes for competing advertisers.

# 6 Results

We conducted an analysis of the effect of position on two key metrics of interest to advertisers - click through rates (henceforth CTR) and the number of sales orders (henceforth orders). The reasons to select these two metrics is that they are the most important metrics from the point of view of the advertiser. CTR measures the proportion of consumers served the ad who clicked on it and arrived at the advertiser's website. Since the advertiser's control on the consumer's experience only begins once the consumer arrives at the website, CTR is of critical importance to the advertiser in measuring the effectiveness of the advertisement in terms of driving 'volume' of traffic. We could conduct an analysis on raw clicks instead, but it does not make any material difference to the results, and CTR is the more commonly reported metric in this industry.

The second measure we consider is the number of sales orders corresponding to that keyword. This is again a key metric for the firm since it generates revenues only when a consumer places an order. We attempted an analysis on measures like conversion rates, sales value and sales per click, but do not report these estimates since almost all the estimates were statistically insignificant. This is partly driven by the fact that the category in focus sees very infrequent purchases, reducing the statistical significance of results.

# 6.1 Effect of position on click through rates

The pooled results of all advertisements in the analysis sample, with fixed effects for advertiser, keyword, match-type and day of week are reported in table 2, showing the OLS estimates (which in

essence are the same as mean comparisons across pairs of adjacent positions), fixed effect estimates ( with fixed effects added for advertiser, keyword, match type and day of week to control for selection in observables) and the RD estimates. The RD estimates are reported at the optimum bandwidth as described in section 4.3. We report the baseline click through rates for each position, which is the mean click through rate for the lower position in the pair. The baseline is the same for the raw and the fixed effects estimates. The baseline for the RD estimates are different and are mean click through rates for the lower position for observations within the optimized bandwidth. One point to note is that these comparisons should only be conducted on a pairwise basis. For instance, the observations in position 2 that are used for analyzing the shift from position 2 to 1 are not the same as the observations used to compare 3 to 2. Hence, it will not be the case that the baseline for position 2 is the sum of the baseline for position 3 and the effect of moving from position 3 to 2.

When we look at the RD estimates we see significant effects across multiple positions. The RD estimates are significant from position 2 to 1. As seen in figure 1, the topmost position is often above the organic search results and hence distinctive relative to the other ads. Thus, the effect at position 1 is to be expected. There is no significant position effect between positions 3 and 2. However, there is a significant and positive effect when moving from position 4 to position 3. Such an effect is consistent with the Google golden triangle effect<sup>8</sup>, which has been postulated to be due to attention effects and documented in eye tracking studies (Hotchkiss, Alston, and Edwards, 2005; Guan and Cutrell, 2007) as well as using advertising and sales data (Kalyanam, Borle, and Boatwright 2010). Further, there seem to be significant effects when moving from positions 6 to 5 and 7 to 6. These positions are typically below the page fold and often require consumers to scroll down (whether position 6 or 5 appears below the fold depends on the size of the browser window, the number of ads that appear above the organic results, etc.).

For Position 2 to 1, the OLS and fixed effects estimates are also significant. The OLS estimate shows an increase in CTR of 2.4413 which is an increase of 133.22%. The fixed effect estimate is 0.4286 which is an increase of 23.38%. The RD estimate shows an increase of 0.4610 which is a 22.1% increase. The key point here is that are very significant biases in both the baseline and

<sup>&</sup>lt;sup>8</sup>The Google golden triangle effect refers to the finding that consumers focus on the top organic listing, top advertising listing and then focus on results lower down, skipping some positions in between.

the Position 1 effect in the OLS and fixed effects estimates. There is a similarly large bias for the Position 3 to 2 effect where the RD estimates are not significant, but the OLS and fixed effects estimates are. For position 4 to 3, the RD estimate is 0.1115 which is an increase of 10.12%. The OLS estimate is much higher than this with an increase of 63.99%. The fixed effects estimate is 5.38% which is lower than the RD estimate. The fixed effect estimates are insignificant for Position 6 to 5 and Position 7 to 6. The OLS estimate is significant for Position 6 to 5 but the percentage increase is much higher than that for the RD estimate.

The differences between the OLS, fixed effects and RD estimates are important, since they indicate the nature of the selection in positions. The OLS estimates are generally highly positively biased and demonstrate the selection on observables (e.g. keyword, advertiser) as well as unobservables. The fixed effects estimates, which correct for selection on observables such as keyword, advertiser, match-type and day of week on the other hand are generally downward biased. This suggests that in this context, the selection on unobservables causes a negative bias This can result from advertisers or their competitors' strategic behavior, as indicated earlier. Further, the effect of selection differs significantly by position, with the bias of the OLS estimates ranging as high as 133.22% at Position 1. This last point has some important implications. Parametric approaches to control for selection need to be able to allow for selection effects to vary by position. Instrumental variable approaches need to allow for the possibility that the choice of instrument varies by position.

The causal position effects are not just statistically significant, but have large economic significance as well. For instance, the causal effect at position 1 as a proportion of the baseline click through rate is 22.1%. They are 10.12%, 24.5% and 33.54% respectively at positions 3, 6 and 7, and hence of large magnitude even at these positions. Thus, it seems like in this category at least, if the objective of search advertising is to drive up clicks, there are opportunities at these positions and by a large magnitude. Further there is little agreement between the OLS, fixed effect and RD estimates with the OLS estimates generally overstating the true effect and the fixed effects estimates generally understating them. The magnitude of the bias is quite significant and varies by position indicating that selection effects vary by position.

### 6.2 Effect of position on sales orders

We next investigate if the position in search advertising results causally affects the number of sales orders that are generated, and report the RD estimates in table 3. We find that the OLS and the fixed effect estimates are once again misleading. They suggest that there are positive incremental effects on sales only when moving to the top position from the next one. By contrast, the RD estimates suggest that the only significant effect is in moving to position 5, with no significant differences between pairs of positions above that. This suggests that the nature of the mechanisms that may cause position to affect sales, such as quality signaling really play out only below the top 5 positions. In terms of economic significance, these effects are even stronger than for click through rates, with sales orders jumping up by over 133% relative to the baseline.

#### 6.3 Advertiser-level effects

We next investigate how position effects vary across advertisers. Table 4 reports position effects for the keywords advertised by three of the four firms in the data. We were unable to conduct advertiserlevel analysis for the fourth firm because of the significantly smaller number of observations in that case. While we are unable to name the firms, the firms labeled 1 and 2 are smaller than firm 3, which is the largest firm in the category. We find that the position effects for firms 1 and 2 are largely very similar to the effects for the pooled analysis reported earlier, with significant effects at positions 1, 3, 5 and 6. However, firm 3 has largely insignificant effects, except at position 5, which is typically the position at which consumers have to scroll down the page to see the next ad. Thus, firm 3, which is the largest of the three firms has much weaker position effects than firms 1 and 2, which are smaller. This could reflect the substitutability between advertising and other sources of information about quality, in this case the firm size. Consumers may be using the position to infer something about quality of the product advertised or the relevance of the ad to them from the position when the firm is smaller, but when the firm is larger, it is possible that firm size provides them this information and hence advertising has weaker or no effects. Another possibility is that consumers, on average, have greater prior experience with the larger firm than the two smaller firms, and this results in weaker position effects for the bigger firm because of the substitutability between advertising and prior experience. These are very interesting results, however it is hard to conduct a more detailed analysis about position effects and differences in quality or experience at the advertiser level since there are only three advertisers in our analysis. However since we have lots of keywords in our data set a more detailed analysis is possible at the keyword level.

We further investigate the relationship between prior consumer experience and position effects by using information on the quality scores for keyword/advertising combinations to proxy for prior consumer experience. Since the search engine assigns a quality score to a particular advertiser for a given keyword based on past click through history, an advertiser on whose ad more consumers have clicked on in the past receives on average a higher quality score than another advertiser with fewer clicks. We therefore split the data into two parts - one where the quality score of the advertiser for the keyword is higher than the overall median quality score, and one where it is below the median. These estimates are reported in Table 5. We find that position effects for the low quality score keywords look similar to the pooled results, with significant position effects at position 1, 3 and 5. On the other hand, the position effects are largely insignificant for the high quality score keywords, with significant effects only at position 6. To the extent that quality score is a proxy for prior consumer experience, this provides support to the argument that prior experience and advertising act as substitutes. Note that this is an argument about consumer experience in the aggregate sense, not for a particular consumer.

#### 6.4 Brand vs. Category Keywords

We next investigate moderation in position effects based on the degree of specificity of the keyword phrases themselves. For the purpose of this analysis, we first classified keyword phrases as brand phrases if there was any reference to a specific brand or product in the keyword phrase. All other phrases were termed as category phrases. For example the keyword "Rayban sunglasses" would be classified as a brand phrase whereas the keyword "Sunglasses" would be categorized as a category phrase. We did this classification using a textual analysis of the keyword phrases, with the algorithm looking for occurrences of brand names and specific product identifiers (product name,

model number etc.) in the keyword phrase. <sup>9</sup> Table 6 reports the position effects separately for brand and category keywords. Consistent with our expectation, we find weaker effects for brand keywords than for category keywords. For brand keywords, there are significant effects only at positions 5 and 6, which are the typical positions where consumers have to scroll down to view the next listing. For category keywords, in addition to these positions, there are significant effects at positions 1 and 3, mirroring the pooled results. Furthermore, the magnitudes of the effects both in absolute terms and as a percentage of the baseline are higher for category keywords than brand keywords. These findings are consistent with the notion that consumers searching for more specific products and brands have on average more information about what they are looking for than when they search using more general keyword phrases. Given that they have other information, they may use the position in the search advertising listings to a lesser extent than when their information is less specific.

### 6.5 Broad vs. exact match types

We report the RD estimates for broad and exact match types for click through rates in table 7. The comparisons of these two types of match types are consistent with our expectations. For broad match types, there are significant effects at position 3, 5 and 6 only but not at position 1. For exact match types, on the other hand, the only significant effect is at position 1. Generally speaking position effects are more pervasive for broad match ads compared to exact match ads. This is consistent with our expectation that the headline and ad copy in exact match ads is more targeted compared to a broad match ad. To the best of our knowledge, this is the first time an empirical study has documented the differences between advertising response for broad and exact match ads.

#### 6.6 Weekends vs. Weekdays

The results for the position effects separated by weekday and weekend are reported in Table 8. The weekday results are largely similar to the pooled results, with a significant effect at position 1, 3 and 5. The weekend effects are less significant in general but also show differences in the position <sup>9</sup>Specific details on the procedure used for this classification are available from the authors on request.

effects. The only significant results are at positions 4 and 6, which typically are below the usual zone that consumers pay the most attention to. The absence of significant position effects may reflect the differences in search costs of consumers between weekdays and weekends. If consumers search costs are lower on weekends, they are more likely to search lower down the advertising results before stopping, giving rise to the effects we estimate. Thus, these results are consistent with the explanation for weekend effects in offline retail categories in Warner and Barsky (1995). The weekend effects described here also provide indirect support for the search cost explanation for position effects per se, while not conclusively proving its existence or ruling out the presence of other explanations simultaneously. If position effects are driven even partially by a sequential search mechanism, with consumers sequentially moving down the list of search advertising results until their expected benefit from the search is lower than their cost of further search, it is a logical conclusion that they would search more when search costs are lower. Since search costs are plausibly lower on weekends, due to greater availability of time, this would lead to position effects lower down the list on weekends than on weekdays, which is what we find in our analysis.

#### 6.7 Robustness

In comparing the position effects across firms, one possibility is that the different firms advertise on different keyword phrases, and hence the across-firm differences are reflective of differences across the keywords in reality. To rule out this explanation, we redid the analysis for a set of keywords that all three firms had advertised on. The downside to this analysis is that the number of observations for analysis is reduced. We are thus unable to report the estimates for the 7th and 6th positions due to the paucity of observations in these positions. We find that the results for the top 5 positions are consistent with the analysis presented earlier with significant position effects at the same positions as before (except at position 5 for firm 3, which is significant at the 90% level instead of the earlier 95% level). We conduct similar robustness checks for the analysis of broad vs. exact match ads, and for weekdays vs. weekends, and find that the results are robust to keeping the set of keywords fixed across match type and weekday/weekend respectively.

## 7 Conclusion

In this paper, we investigate the important question of the causal effect of position in search advertising on outcomes such as website visits and sales. We present a novel regression discontinuity based approach to uncovering causal effects in this context. The importance of this approach is particularly high in this context due to the difficulty of experimentation and the infeasibility of other approaches such as instrumental variable methods.

We obtain a unique dataset of advertising in a durable goods category by a focal advertiser as well as its major competitors on the Google search engine. The application of regression discontinuity requires that the researcher observe the AdRanks of competing retailers, which is the score used by Google to decide position. Typically, only Google observes the AdRanks of competing advertisers and hence we would not be able to apply RD to measuring causal position effects using data from only one advertiser who observes only his own AdRank. However, our data contains information including AdRanks of firms that competed with this firm in Google's search advertising listings, and this allows us to set up an RD design.

We find that there are significant position effects, and that these would be significantly misstated by analysis that does not account for the selection of position and even by more sophisticated analysis that accounts for selection on observables. We find that the position effects are of economic significance, increasing the click through rates by about 10-20% in positions where they are significant, but a lot lower than the naive estimates that do not account for selection We document moderators for these effects, with position effects in general being weaker for smaller firms, for keywords with a lower amount of prior consumer experience, and where the specificity of the search phrase is higher. We find important differences in these effects between broad and exact match keywords, with significant effects only at the top most position for exact match, and significant effects only in lower positions for broad match keywords. Finally, we document important weekend effects in this context. Position effects are weaker on the weekend and this result is consistent with the idea that consumers' search costs are lower during the weekends.

The results of our empirical analysis would be of great interest to managers who are setting

firms' online advertising strategies. Further, they should be of interest to the academic audience since we point to significant selection issues in this context and point to a viable way to correct for them. The finding that value varies by position and that position effects vary by firm should be of interest to theoretical work in this area. The methodological innovation should be of interest to search engines as well, who might be interested in viable alternatives to experimentation, which tends to be difficult and expensive especially at the scale necessary for uncovering the impact of various moderators. We hope that this study leads researchers to pay greater attention to concerns about selection and endogeneity, which have received less than the necessary attention in the field of online advertising specifically and web analytics in general.

Finally, we discuss the limitations of this paper. In general, RD designs can be demanding on the data. Fortunately, our empirical analysis is in a data-rich environment, allowing us to conduct a relatively rich analysis of both main effects and moderators. However, in spite of the large data set, we do have some constraints in what can be estimated. For instance, the sales orders results cannot be estimated for all the positions for which click through rate results can be estimated due to smaller volumes of sales orders relative to click throughs. The advertiser specific effects can be estimated reliably for only 3 of the 4 firms. The robustness checks, where we compare position effects across advertisers (or other such comparisons) with a common set of keywords cannot be reliably estimated for all positions. Since there is variation in the amount of data available for the various estimates present, comparisons of estimates based on their significance levels needs to account for the amount of data available for the estimates. We have attempted to make it transparent to the reader the volume of data that is used for every estimate that we present, so that the reader can draw their conclusions from the estimates with full information. Finally, the results we present here are based on the analysis of one large category of consumer durables. While there is some degree of generalizability of the results, particularly in the sense that the economic underpinnings of the effects and their moderators are similar, the reader should exercise caution in extrapolating the specific results to other categories. Future research using other data sets and other methodologies could be helpful in investigating the effects that we have not been able to investigate in our paper.

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Figure 1: Example of search advertising results

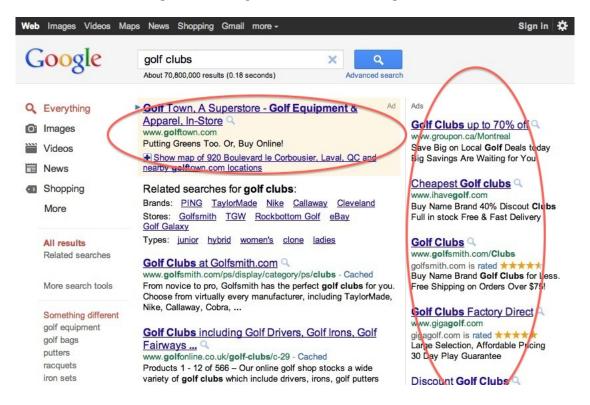


Table 1: Summary statistics of the data

Variable	Mean	Std. Dev
Impressions	45.9050	224.6952
Clicks	0.5452	2.5371
Click through rate (%) (Clicks/Impressions)	1.8932	6.6629
Number of orders	0.0046	0.0733
Conversion rate (% of non-zero clicks that resulted in orders)	0.7530	7.3889
Sales (\$)	0.4850	16.7715
Average sales per (non-zero) click (\$)	0.7446	20.7283
Average sales per (non-zero) order (\$)	107.3475	225.7770
Gross margin %	47.03%	24.93%
Bid (\$ per click)	0.3947	0.8231
Quality score	5.9811	1.2473
AdRank	2.3402	5.0966

Table 2: Position effects on click through rates

Position	]	Naive Estima	ates (CTR %	)	RD	Estimates (C	CTR %)
	Baseline	Num. of Obs.	OLS Estimate (p-value)	Fixed Eff. Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)
2 to 1	1.8325	141492	2.4413 (0.0000)	0.4286 (1.39e- 23)	2.3260	4736	0.4796 (0.0049)
3 to 2	1.1597	192346	0.9217 (0.0000)	0.0643 (0.0138)	1.2864	14134	0.0597 $(0.2884)$
4 to 3	0.8732	124488	0.5588 (0.0000)	0.0470 (0.0690)	1.0348	14350	0.1109 (0.0147)
5 to 4	0.7280	63754	0.2642 (7.82e- 09)	-0.0406 (0.1679)	0.8621	9318	0.0570 (0.2069)
6 to 5	0.6405	26564	0.1394 (0.0198)	-0.0318 (0.4126)	0.7746	4532	0.1294 $(0.0147)$
7 to 6	0.4998	11258	0.1458 (0.1729)	0.0479 (0.4286)	0.6345	1864	0.1239 (0.0474)
8 to 7	0.2740	4960	0.1708 (0.0703)	0.0583 (0.3185)	0.3855	426	-0.0058 (0.9601)

Table 3: Position effects on number of sales orders

Position	]	Naive Estima	ites (CTR %)	)	RD	Estimates (C	CTR %)
	Baseline	Num. of Obs.	OLS Estimate (p-value)	Fixed Eff. Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)
2 to 1	0.0045	141492	0.0049 (4.75e- 13)	0.0009 (0.0550)	0.0042	4338	0.0006 (0.7299)
3 to 2	0.0023	192346	0.0036 (8.93e- 15)	0.0005 (0.1757)	0.0023	12116	0.0007 (0.3656)
4 to 3	0.0021	124488	0.0018 (0.0001)	-0.0002 (0.5034)	0.0018	13332	-0.0009 (0.1549)
5 to 4	0.0019	63754	0.0006 (0.2628)	-0.0002 (0.6777)	0.0017	9206	0.0011 (0.1695)
6 to 5	0.0011	26564	0.0001 (0.8470)	-0.0005 (0.3203)	0.0008	4630	0.0017 (0.0409)

Table 4: Advertiser level position effects

Position	Fir	m 1 (CTR	%)	Fir	m 2 (CTR	%)	Fir	m 3 (CTR	%)
	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)
2 to 1	2.2190	1157	0.4614 (0.0108)	1.9488	781	0.4821 (0.0177)	2.8730	2176	0.3555 (0.2314)
3 to 2	1.1711	3703	0.1609 (0.4004)	1.0441	4467	-0.1431 (0.1846)	1.6288	5209	0.0833 (0.4999)
4 to 3	0.9814	5002	0.1588 (0.0406)	1.1225	5520	0.1182 (0.0423)	0.9428	3433	0.0840 (0.1520)
5 to 4	0.7479	3390	0.0756 (0.3014)	0.9126	3910	0.0970 (0.1582)	0.7809	1809	0.0363 (0.3116)
6 to 5	0.7627	1649	0.1862 (0.0596)	0.8391	1928	0.0544 (0.0400)	0.7291	715	0.1589 (0.0431)
7 to 6	0.7409	712	0.1421 (0.0685)	0.8062	792	0.1120 (0.0319)	0.4803	266	0.2575 (0.1652)
8 to 7	0.2246	326	0.0574 (0.8062)	0.5721	392	0.1578 (0.2116)	-0.2922	118	-0.4110 (0.1755)

Table 5: Position effects on click through rates: Low vs. High Quality Score Keyword Phrases

Position	Low Qu	ality Score (	CTR %)	High Qu	ality Score (	CTR %)
	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)
2 to 1	1.2323	2370	0.5725 (0.0308)	4.1504	2510	0.2819 (0.4691)
3 to 2	0.8887	6930	-0.0118 (0.8908)	2.1678	7310	0.0884 (0.4754)
4 to 3	0.6503	7608	0.0809 (0.0188)	1.5447	7334	0.1955 (0.1567)
5 to 4	0.5732	4686	-0.0279 (0.5770)	1.3554	4710	0.0906 (0.3659)
6 to 5	0.4070	2244	0.1761 (0.0185)	1.1410	2324	-0.0041 (0.9677)
7 to 6	0.2814	974	-0.0112 (0.8946)	0.7245	902	0.3351 (0.0653)
8 to 7	0.2158	220	0.2734 (0.2505)	0.6057	186	-0.1199 (0.5986)

Table 6: Position effects on click through rates: Brand vs Category Keywords

Position	Brand	Keywords (C	CTR %)	Category	Keywords (	(CTR %)
	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)
2 to 1	2.4785	2626	0.3480 (0.1252)	2.2894	1930	0.6177 (0.0211)
3 to 2	1.3927	9276	0.2397 (0.7604)	1.2318	5574	0.0491 (0.5868)
4 to 3	1.0449	8418	0.1505 (0.1274)	1.0017	5938	0.0740 (0.0281)
5 to 4	0.9350	5320	0.0112 (0.8570)	0.8219	4188	0.1547 (0.1711)
6 to 5	0.8255	2630	0.0975 (0.0505)	0.7498	1768	0.1325 (0.0465)
7 to 6	0.6952	1158	0.0550 (0.0525)	0.5507	654	0.2870 (0.0398)
8 to 7	0.4970	276	-0.0843 (0.5768)	0.3589	210	0.1134 (0.5905)

Table 7: RD estimates of position effects on click through rates: broad vs. exact match

Position	Broad	Match (C	TR %)	Exact	Match (C'	ΓR %)
	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)
2 to 1	1.7456	3262	0.2296 (0.1899)	3.1752	1580	0.8675 (0.0173)
3 to 2	1.16665	12352	0.0497 (0.2300)	2.2306	1670	0.1508 (0.4845)
4 to 3	0.9915	13164	0.1034 (0.0178)	1.4834	1084	0.1037 (0.6027)
5 to 4	0.8316	8858	0.0486 (0.2579)	1.1118	338	-0.1014 (0.6239)
6 to 5	0.7368	4298	0.1113 (0.0347)	0.7879	218	0.1999 (0.4463)
7 to 6	0.5807	1700	0.1515 (0.0488)	0.7241	142	-0.3060 (0.6988)
8 to 7	0.3220	340	0.0231 (0.8641)	0.4415	64	-0.1394 (0.8101)

Table 8: RD estimates of position effects on click through rates: weekday vs. weekend

Position	Wee	kday (CTF	R %)	Wee	kend (CTI	R %)
	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)
2 to 1	2.4659	3374	0.4994 (0.0145)	1.9813	1356	0.4267 (0.1687)
3 to 2	1.2464	10128	0.1018 (0.1356)	1.4592	3998	-0.0190 (0.8770)
4 to 3	1.0104	10168	0.1493 (0.0072)	1.1172	3978	0.0314 (0.7008)
5 to 4	0.8240	6844	0.0236 (0.6579)	0.9557	2540	0.1811 (0.0279)
6 to 5	0.7637	3384	0.1508 (0.0140)	0.7843	1166	0.0721 (0.4919)
7 to 6	0.5989	1300	0.1049 (0.2058)	1.0221	516	0.2005 (0.0485)
8 to 7	0.3302	328	-0.0340 (0.8199)	0.5391	142	0.0153 (0.4645)

Table 9: Firm-specific position effects for keywords advertised by firms 1-3

Position	All F	All Firms (CTR %)	R %)	Fir	Firm 1 (CTR %)	%)	Firr	Firm $2~(\mathrm{CTR}~\%)$	%)	Fir	Firm 3 (CTR %)	%)
	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)
2 to 1	2.5248	1210	0.4765 (0.0103)	1.9906	201	0.5317 (0.0484)	2.4748	258	0.4362 (0.0518)	3.5878	519	0.5292 (0.4704)
3 to 2	1.4315	5874	0.1461 (0.8892)	1.3710	885	0.1674 (0.4487)	1.1795	2332	0.0787 (0.6251)	2.1735	1418	0.1308 (0.6738)
4 to 3	1.0207	7406	0.1881 (0.0081)	1.1911	1137	0.2906 (0.0187)	1.0973	3292	0.0716 (0.0273)	0.8955	1289	0.1752 (0.1820)
5 to 4	0.7977	5084	0.1077 (0.2530)	0.8624	803	0.0978 (0.8722)	0.7407	2396	0.1642 (0.5110)	0.6187	778	0.1015 (0.2802)
6 to 5	0.7639	2404	0.2086 (0.0074)	0.7919	402	0.4438 (0.0016)	0.6881	1155	0.1564 (0.0043)	0.6106	278	0.2241 (0.0663)

Table 10: Position effects for keywords advertised for both broad and exact match

Position	Pooled B	Estimates (	CTR %)	Broad	Match (C'	TR %)	Exact	Match (C'	ΓR %)
	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)
2 to 1	2.7278	2342	0.4701 (0.0047)	1.7058	1060	0.3427 (0.2133)	3.3658	1256	0.8598 (0.0409)
3 to 2	1.6270	6394	0.0546 (0.5992)	1.13388	4830	0.0577 (0.6178)	2.2120	1444	0.2060 (0.3767)
4 to 3	1.1770	4348	0.0439 (0.0551)	0.9514	3468	0.0907 (0.0311)	1.4221	918	0.0115 (0.6107)
5 to 4	0.8677	2246	0.1732 (0.6055)	0.8371	1886	0.1019 (0.7691)	0.9412	288	0.3739 (0.8665)
6 to 5	0.7342	892	0.1562 (0.0632)	0.7041	706	0.1363 (0.0890)	0.7956	190	0.1788 (0.5306)

Table 11: Position effects for keywords advertised both on weekdays and weekends

Position	Pooled I	Estimates (	CTR %)	Wee	kday (CTI	R %)	Wee	kend (CTF	R %)
	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)	Baseline	Num. of Obs.	Estimate (p-value)
2 to 1	2.3034	4504	0.4913 (0.0052)	2.4294	3168	0.5197 (0.0142)	1.9813	1356	0.4267 (0.1687)
3 to 2	1.2905	13202	0.0520 (0.4082)	1.2404	9262	0.0854 (0.2466)	1.4592	3998	-0.0190 (0.8770)
4 to 3	1.0441	13264	0.0943 (0.0453)	1.0119	9388	0.1225 (0.0326)	1.1172	3978	0.0314 (0.7008)
5 to 4	0.8706	8764	0.0634 (0.2836)	0.8475	6382	0.0150 (0.7925)	0.9557	2540	0.1811 (0.0279)
6 to 5	0.7715	4158	0.1121 (0.0424)	0.7608	3030	0.1391 (0.0310)	0.7843	1166	0.0721 (0.4919)
7 to 6	0.6437	1644	0.1536 (0.0673)	0.5834	1166	0.1441 (0.1289)	1.0221	516	0.2005 (0.0485)