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Tom Blake
Chris Nosko
Steven Tadelis

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ABSTRACT

Internet advertising has been the fastest growing advertising channel in recent years with paid search ads comprising the bulk of this revenue. We present results from a series of large scale field experiments done at eBay that were designed to measure the causal effectiveness of paid search ads. Because search clicks and purchase behavior are correlated, we show that returns from paid search are a fraction of conventional non-experimental estimates. As an extreme case, we show that brand-keyword ads have no measurable short-term benefits. For non-brand keywords we find that new and infrequent users are positively influenced by ads but that more frequent users whose purchasing behavior is not influenced by ads account for most of the advertising expenses, resulting in average returns that are negative.

Tom Blake
ebay Research Labs
thblake@ebay.com

Chris Nosko
Booth School of Business
University of Chicago
5807 S. Woodlawn Ave
Chicago,IL 60637
eBay Research Labs
2065 Hamilton Ave
cnosko@gmail.com

Steven Tadelis
Haas School of Business
University of California, Berkeley
545 Student Services Building
Berkeley, CA 94720
and NBER
stadelis@haas.berkeley.edu

1 Introduction

Advertising expenses account for a sizable portion of costs for many companies across the globe. In recent years the internet advertising industry has grown disproportionately, with revenues in the U.S. alone totaling $36.6 billion for 2012, up 15.2 percent from 2011. Of the different forms of internet advertising, paid search advertising, also known in industry as "search engine marketing" (SEM) remains the largest advertising format by revenue, accounting for 46.3 percent of 2012 revenues, or $16.9 billion, up 14.5 percent from $14.8 billion in 2010.[1] Google Inc., the leading SEM provider, registered $46 billion in global revenues in 2012, of which $43.7 billion, or 95 percent, were attributed to advertising.[2]

This paper reports the results from a series of controlled experiments conducted at eBay Inc., where large-scale SEM campaigns were randomly executed across the U.S. Our contributions can be summarized by two main findings. First, we argue that conventional methods used to measure the causal (incremental) impact of SEM vastly overstate its effect. Our experiments show that the effectiveness of SEM is small for a well-known company like eBay and that the channel has been ineffective on average. Second, we find a detectable positive impact of SEM on new user acquisition and on influencing purchases by infrequent users. This supports the *informative view* of advertising and implies that targeting *uninformed* users is a critical factor for successful advertising.

The effects of advertising on business performance have always been considered hard to measure. A famous quote attributed to the late 19th century retailer John Wannamaker states that "I know half the money I spend on advertising is wasted, but I can never find out which half." Traditional advertising channels such as TV, radio, print and billboards have limited targeting capabilities. As a result, advertisers often waste valuable marketing dollars on "infra-marginal" consumers who are not affected by ads to get to those marginal consumers who are. The advent of internet marketing channels has been lauded as the answer to this long-standing dilemma for two main reasons.

First, unlike offline advertising channels, the internet lets advertisers target their ads to the activity that users are engaged in (Goldfarb, 2012). For instance, when a person is reading content related to sports, like ESPN.com, advertisers can bid to have display ads

[1] These estimates were reported in the *IAB Internet Advertising Revenue Report* conducted by PwC and Sponsored by the Interactive Advertising Bureau (IAB) 2012 Full Year Results published in April 2013. See `http://www.iab.net/media/file/IAB_Internet_Advertising_Revenue_Report_FY_2012_rev.pdf`

[2] See Google's webpage `http://investor.google.com/financial/tables.html`

appear on the pages that are being read. Similarly, if a user is searching Google or Bing for information about flat-screen TVs, retailers and manufacturers of these goods can bid for paid search ads that are related to the user's query. These ads better target the intent of the user and do not waste valuable resources on uninterested shoppers.

Second, the technology allows advertisers to track variables that should help measure the efficacy of ads. An online advertiser will receive detailed data on visitors who were directed to its website by the ad, how much was paid for the ad, and using its own internal data flow, whether or not the visitor purchased anything from the website. In theory, this should allow the advertiser to compute the returns on investment because both cost and revenue data is available at the individual visitor level.

Despite these advantages, serious challenges persist to correctly disentangling causal from correlated relationships between internet advertising expenditures and sales, resulting in endogeneity concerns. Traditionally, economists have focused on endogeneity stemming from firm decisions to increase advertising during times of high demand (e.g., advertising during the Holidays) or when revenues are high (e.g., advertising budgets that are set as a percentage of previous-quarter revenue).[3]

Our concern, instead, is that the amount spent on SEM (and many other internet marketing channels) is a function not only of the advertiser's campaign, but is also determined by the *behavior* and *intent* of consumers. For example, the amount spent by an advertiser on an ad in the print edition of the New York Times is independent of consumer response to that advertisement (regardless of whether this response is correlated or causal). In contrast, if an advertiser purchases SEM ads, expenditures rise with clicks. Our research highlights one potential drawback inherent in this form of targeting: While these consumers may look like good targets for advertising campaigns, they are also the types of consumer that may already be informed about the advertiser's product, making them less susceptible to informative advertising channels. In many cases, the consumers who choose to click on ads are loyal customers or otherwise already informed about the company's product. Advertising may appear to attract these consumers, when in reality they would have found other channels to visit the company's website. We are able to alleviate this endogeneity challenge with the design of our controlled experiments.

Before addressing the general case of SEM effectiveness with broader experimentation, we begin our analysis with experiments that illustrate a striking example of the endogeneity

[3]See Berndt (1991), Chapter 8, for a survey of this literature.

problem and first test the efficacy of what is referred to as "brand" keyword advertising, a practice used by most major corporations. For example, on February 16, 2013, Google searches for the keywords "AT&T", "Macy", "Safeway", "Ford" and "Amazon" resulted in paid ads at the top of the search results page directly above *natural* (also known as *organic*) unpaid links to the companies' sites. Arguably, consumers who query such a narrow term intend to go to that company's website and are seeking the easiest route there. Brand paid search links simply intercept consumers at the last point in their navigational process, resulting in an extreme version of the endogeneity concern described above.[4]

Our first set of experiments are described in Section 3 and show that there is no measurable short-term value in brand keyword advertising. eBay conducted a test of brand keyword advertising (all queries that included the term eBay, e.g., "ebay shoes") by halting SEM queries for these keywords on both Yahoo! and Microsoft (MSN), while continuing to pay for these terms on Google, which we use as a control in our estimation routine. The results show that almost all of the forgone click traffic and attributed sales were captured by natural search.[5] That is, substitution between paid and unpaid traffic was nearly complete. Shutting off paid search ads closed one (costly) path to a company's website but diverted traffic to natural search, which is free to the advertiser. We confirm this result further using several brand-keyword experiments on Google's search platform.

The more general problem of analyzing non-branded keyword advertising is the main part of our analysis as described in Section 4. eBay historically managed over 100 million keywords and keyword combinations using algorithms that are updated daily and automatically feed into Google's, Microsoft's and Yahoo!'s search platforms.[6] Examples of such keyword strings are "memory", "cell phone" and "used gibson les paul". Unlike branded search, where a firm's website is usually in the top organic search slot, organic placement for non-branded terms vary widely. Still, even if eBay does not appear in the organic search results, consumers may use other channels to navigate to eBay's website, even by directly navigating to www.ebay.com. Hence, with non-branded search, we expect

[4]A search for the term "brand-keyword advertising" yields dozens of sites many from online ad service agencies that discuss the importance of paying for your own branded keywords. Perhaps the only reasonable argument is that competitors may bid on a company's branded keywords in an attempt to "steal" visitor traffic. We discuss this issue further in section 6.

[5]Throughout, we refer to sales as the total dollar value of goods purchased by users on eBay. Revenue is close to a constant fraction of sales, so percentage changes in the two are almost equivalent.

[6]See "Inside eBay's business intelligence" by Jon Tullett, news analysis editor for ITWeb at `http://www.itweb.co.za/index.php?option=com_content&view=article&id=60448:Inside-eBay-s-business-intelligence&catid=218`

that organic search substitution may be less of a problem but purchases may continue even in the absence of SEM. To address this question, we designed a controlled experiment using Google's geographic bid feature that can determine, with a reasonable degree of accuracy, the geographic area of the user conducting each query.[7] We designate a random sample of 30 percent of eBay's U.S. traffic in which we stopped all bidding for all non-brand keywords for 60 days. The test design lends itself to a standard difference-in-differences estimation of the effect of paid search on sales and allows us to explore heterogeneous responses across a wider consumer base, not just those searching for eBay directly.

The non-brand keyword experiments show that SEM had a very small and statistically insignificant effect on sales. Hence, on average, U.S. consumers do not shop more on eBay when they are exposed to paid search ads. To explore this further, we segmented users according to the frequency and recency at which they visit eBay. We find that SEM accounted for a statistically significant increase in new registered users and purchases made by users who bought only one or two items the year before. For consumers who bought more frequently, SEM does not have a significant effect on their purchasing behavior. We calculate that the short-term returns on investment for SEM were negative because frequent eBay shoppers account for most of the sales attributed to paid search.

The heterogeneous response of different customer segments to SEM supports the *informative view* of advertising, which posits that advertising informs consumers of the characteristics, location and prices of products and services that they may otherwise be ignorant about. Intuitively, SEM is an advertising medium that affects the information that people have, and is unlikely to play a persuasive role.[8] It is possible that display ads, which appear on pages without direct consumer queries, may play more of a persuasive role, affecting the demand of people who are interested in certain topics.[9]

In particular, consumers who have completed at least three eBay transactions in the year before our experiment are likely to be familiar with eBay's offerings and value proposition, and are unaffected by the presence of paid search advertising. In contrast, more new users sign up when they are exposed to these ads, and users who only purchased

[7] This methodology is similar to one proposed by Vaver and Koehler (2011).

[8] A recent survey by Bagwell (2007) gives an excellent review of the economics literature on advertising as it evolved over more than a century. Aside from the informational view, two other views were advocated. The *persuasive view* of advertising suggests that consumers who are exposed to persuasive advertising will develop a preference for the advertised product, increasing the advertiser's market power. The *complementary view* posits that advertising enters directly into the utility function of consumers.

[9] A few papers have explored the effects of display ads on offline and online sales. See Manchanda et al. (2006), Goldfarb and Tucker (2011a) and Lewis and Reiley (2014b).

one or two items in the previous year increase their purchases when exposed to SEM. These results echo findings in Ackerberg (2001) who considers the effects of ads on the purchasing behavior of consumers and shows, using a reduced form model, that consumers who were not experienced with the product were more responsive to ads than consumers who had experienced the product. To the best of our knowledge, our analysis offers the first large scale field experiment that documents the heterogeneous behavior of consumers as a causal response to changes in advertising that are related to how informative these are for the consumers.[10]

Our results contribute to a growing literature that exploits rich internet marketing data to both explore how consumers respond to advertising and demonstrate endogeneity problems that plague the more widespread methods that have been used in industry.[11] Lewis and Reiley (2014b) examine a related endogeneity problem to the one we stress, which they call "activity bias", and which results from the fact that when people are more active online then they will both see more display-ads and click on more links. Hence, what some might interpret as a causal link between showing adds and getting consumers to visit sites is largely a consequence of this bias.[12] To illustrate the severity of this problem, we calculate Return on Investment (ROI) using typical OLS methods, which result in a ROI of over $4,100\%$ without time and geographic controls, and a ROI of over $1,400\%$ with such controls. We then use our experimental methods that control for endogeneity to find a ROI of -63%, with a 95% confidence interval of $[-124\%, -3\%]$, rejecting the hypothesis that the channel yields positive returns at all.

Of the $31.7 billion that was spent in the U.S. in 2011 on internet advertising, estimates project that the top 10 spenders in this channel account for about $2.36 billion.[13] If, as we suspect, our results generalize to other well known brands that are in most consumers'

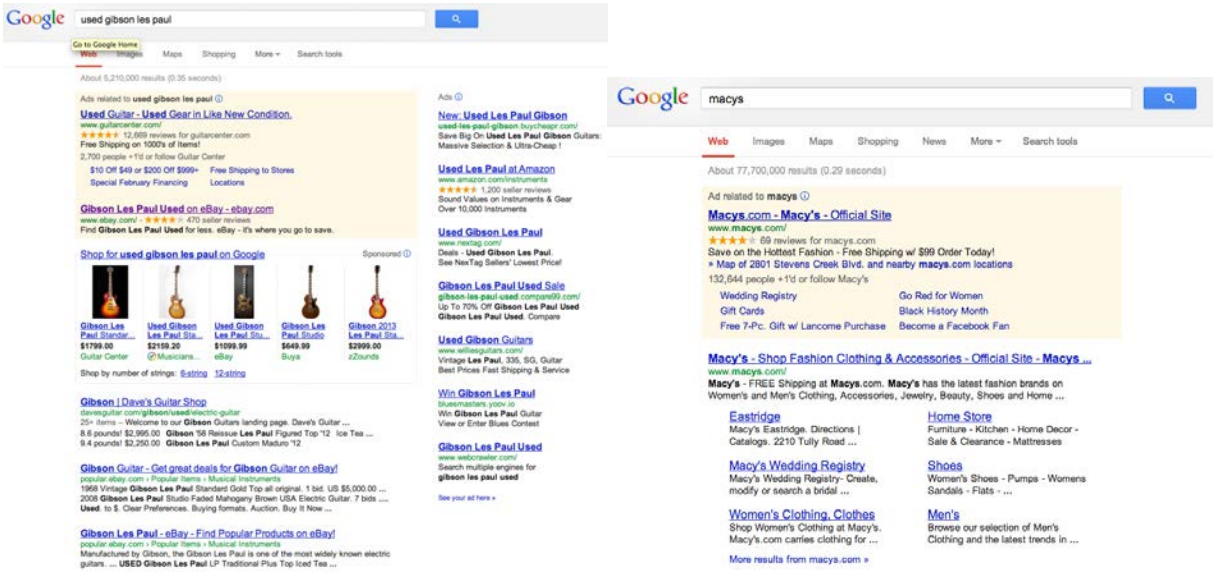
[10]Using rich internet data, other recent papers have shown heterogeneous responses of consumers along demographic dimensions such as age, gender and location. See Lewis and Reiley (2014a) and Johnson et al. (2014).

[11]See Sahni (2011), Rutz and Bucklin (2011), Yao and Mela (2011), Chan et al. (2011b), Reiley et al. (2010), and Yang and Ghose (2010) for recent papers that study SEM using other methods.

[12]Edelman (2013) raises the concern that industry measurement methods, often referred to as "attribution models", may indeed overestimate the efficacy of such ads. Lewis and Rao (2013) expose another problem with measurement showing that there are significant problems with the power of many experimental advertising campaigns, leading to wide confidence intervals.

[13]These include, in order of dollars spent, IAC/Interactive Group; Experian Group; GM; AT&T; Progressive; Verizon; Comcast; Capital One; Amazon; and eBay. See the press release by Kantar Media on 3/12/2012, `http://kantarmediana.com/sites/default/files/kantareditor/Kantar_Media_2011_Full_Year_US_Ad_Spend.pdf`

Figure 1: Google Ad Examples



(a) Used Gibson Les Paul **(b) Macys**

consideration sets, then our study suggests that much of what is spent on internet advertising may be beyond the peak of its efficacy. We conclude by discussing the challenges that companies face in choosing optimal levels of advertising, as well as some of the reasons that they seem to overspend on internet marketing.

2 An Overview of Search Engine Marketing

SEM has been celebrated for allowing advertisers to place ads that directly relate to the queries entered by consumers in search platforms such as Google, Microsoft (Bing) and Yahoo!, to name a few.[14] SEM ads link to a landing page on the advertiser's website, which typically showcases a product that is relevant to the search query.

Figure 1a shows a Google search results page for the query "used gibson les paul". The results fall into two categories: paid (or "sponsored") search ads (two in the shaded upper area, five thumbnail-photo ads below the two and seven ads on the right), and unpaid (also called "natural" or "organic") search results (the three that appear at the bottom).

[14]These differ from display (or banner) ads that appear on websites that the consumer is browsing, and are not a response to a search query entered by the consumer. We focus most of our discussion on Google primarily because it is the leading search platform.

The ranking of the unpaid results is determined by Google's "PageRank" algorithm, which ranks the results based on relevance, while the ranking of the paid search ads depend on the bids made by advertisers for appearing when the particular query is typed by a user, and on a proprietary "quality score" that depends on the click-through rate of the bidder's previous ads. For a more detailed explanation of the bidding and scoring process of SEM, see Edelman et al. (2007) and Varian (2007).

Advertisers pay only when a user clicks on the paid search ad, implying that ad expenses are only incurred for users who respond to the ad. Furthermore, because firms pay when a consumer clicks on their ad, and because they must bid higher in order to appear more prominently above other paid listings, it has been argued that these "position auctions" align advertiser incentives with consumer preferences. Namely, lower-quality firms that expect clicks on their ads not to convert will not bid for positions, while higher-quality firms will submit higher bids and receive higher positions, expecting more satisfied users who will convert their clicks to purchases.[15]

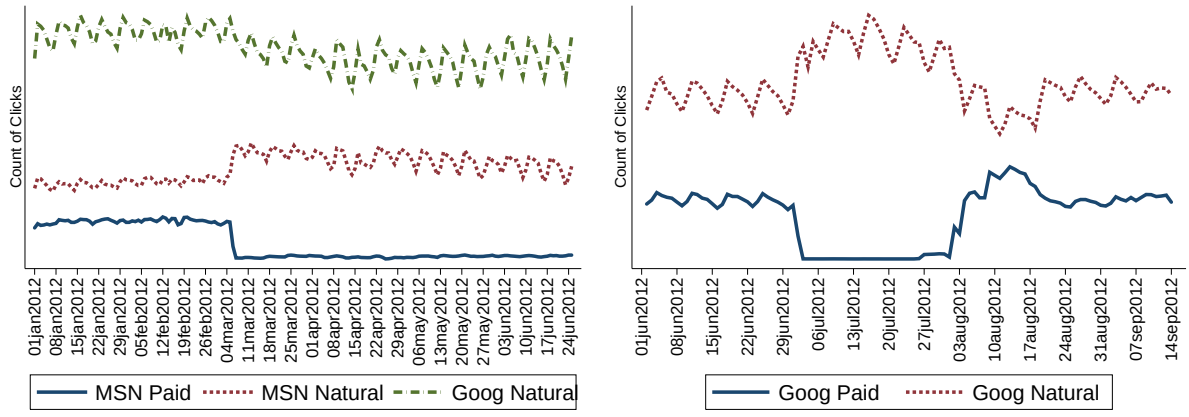
The example in Figure 1a describes what is referred to as a non-brand keyword search, despite the fact that a particular branded product (Gibson Les Paul) is part of the query, because many retailers with their own brand names will offer this guitar for sale. This is in contrast to a branded keyword such as "macys". Figure 1b shows the results page from searching for "macys" on Google, and as the figure shows there is only one paid ad that links to Macy's main webpage. Notice, however, that right below the paid ad is the natural search result that links to the same page. In this case, if a user clicks on the first paid search result then Macy's will have to pay Google for this referral, while if the user clicks on the link below then Macy's will attract this user without paying Google.

3 Brand Search Experiments

In March of 2012, eBay conducted a test to study the returns of brand keyword search advertising. Brand terms are any queries that *include* the term eBay such as "ebay shoes." Our hypothesis is that users searching for "eBay" are in fact using search as a navigational tool with the intent to go to ebay.com. If so, there would be little need to advertise for these terms and "intercept" those searches because the natural search results will serve

[15] Athey and Ellison (2011) argue that "sponsored link auctions create surplus by providing consumers with information about the quality of sponsored links which allows consumers to search more efficiently." (p. 1245)

Figure 2: Brand Keyword Click Substitution



(a) MSN Test **(b) Google Test**

Note: MSN and Google click traffic is shown for two events where paid search was suspended (Left) and suspended and resumed (Right).

as an almost perfect substitute. That said, and as we explain in the introduction, many companies choose to use this advertising channel under the belief that it is a profitable advertising channel. To test the substitution hypothesis, eBay halted advertising for its brand related terms on Yahoo! and MSN. The experiment revealed that almost all (99.5 percent) of the forgone click traffic from turning off brand keyword paid search was immediately captured by natural search traffic from the platform, in this case Bing. That is, substitution between paid and unpaid traffic was nearly complete.[16]

Figure 2a plots the paid and natural clicks originating from the search platform. Paid clicks were driven to zero when advertising was suspended, while there was a noticeable uptake in natural clicks. This is strong evidence that the removal of the advertisement raises the prominence of the eBay natural search result. Since users intend to find eBay, it is not surprising that shutting down the paid search path to their desired destination simply diverts traffic to the next easiest path, natural search, which is free to the advertiser.

To quantify this substitution, Table 1 shows estimates from a pre-post comparison as well as a difference-in-differences across search platforms. In the pre-post analysis we

[16]The 0.5 percent of all clicks lost represents about 1.5 percent of all paid clicks. In a recent paper, Yang and Ghose (2010) similarly switched off and back on paid search advertising for a random set of 90 keywords. We find much smaller differences in total traffic, most likely because we experimented with a brand term where the substitution effect is much larger.

regress the log of total daily clicks from MSN to eBay on an indicator for whether days were in the period with ads turned off. Column 1 shows the results which suggest that click volume was 5.6 percent lower in the period after advertising was suspended.

Table 1: Quantifying Brand Keyword Substitution

	MSN		Google
	(1)	(2)	(3)
	Log Clicks	Log Clicks	Log Clicks
Period	-0.0560***		-0.0321*
	(0.00861)		(0.0124)
Interaction		-0.00529	
		(0.0177)	
Google		5.088	
		(10.06)	
Yahoo		1.375	
		(5.660)	
Constant	12.82***	11.33*	14.34***
	(0.00583)	(5.664)	(0.00630)
Date FE		Yes	
Platform Trends		Yes	
N	118	180	120

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This simple pre-post comparison ignores the seasonal nature of sales that may bias its conclusions. We use data on eBay's clicks from Google as a control for seasonal factors because during the test period on MSN, eBay continued to purchase brand keyword advertising on Google. With this data, we calculate the impact of brand keyword advertising on total click traffic. In the difference-in-differences approach, we add observations of daily traffic from Google and Yahoo! and include in the specification search engine dummies and trends.[17] The variable of interest is thus the interaction between a dummy for the MSN platform and a dummy for treatment (ad off) period. Column 2 of Table 1 show a much smaller impact once the seasonality is accounted for. In fact, only 0.529 percent of the click traffic is lost so 99.5 percent is retained. Notice that this is a lower bound of retention because some of the 0.5 percent of traffic that no longer comes through Google may be switching to non-Google traffic (e.g. typing "ebay.com" into the browser).

[17]The estimates presented include date fixed effects and platform specific trends but the results are very similar without these controls.

These results inspired a follow-up test on Google's platform that was executed in July of 2012 which yielded similar results. Figure 2b shows both the substitution to natural traffic when search advertising was suspended and the substitution back to paid traffic when advertising resumed. Column 3 of Table 1 show the estimated impacts: total traffic referred by Google dropped by three percent. It is likely that a well constructed control group would reduce this estimate as was evident in the MSN test. During this test, there was no viable control group because there was no other contemporaneous paid search brand advertising campaign. In an online Appendix we describe a test in Germany that preserved a control group, which confirms the results described here.

In summary, the evidence strongly supports the intuitive notion that for brand keywords, natural search is close to a perfect substitute for paid search, making brand keyword SEM ineffective for short-term sales. After all, the users who type the brand keyword in the search query intend to reach the company's website, and most likely will execute on their intent regardless of the appearance of a paid search ad. This substitution is less likely to happen for non-brand keywords, which we explore next.

4 Non-Brand Terms Controlled Experiment

When typing queries for non-brand terms, users may be searching for information on goods or wish to purchase them. If ads appear for users who do not know that these products are available at the advertiser's website, then there is potential to bring these users to the site, which in turn might generate sales that would not have occurred without the ads.

Because eBay bids on a universe of over 100 million keywords, it provides an ideal environment to test the effectiveness of paid search ads for non-brand keywords. The broad set of keywords place ads in front of a wide set of users who search for queries related to millions of products. Measuring the effects of the full keyword set more directly addresses the value of informative advertising because we can examine how consumers with different levels of familiarity with the site respond to advertising. In particular, we have past purchase behavior for users who purchased items on eBay and we can use measures of past activity to segment users into groups that would be more or less familiar with eBay's offerings. Non-brand ads can attract users that are not directly searching for eBay but the endogeneity problem persists because the ads may attract users who are already familiar with eBay and may have visited eBay even if the ad were not present.

4.1 Experimental Design and Basic Results

To determine the impact of advertising on the broader set of search queries we designed and implemented a large scale field experiment that exposes a random subset of users to ads and preserves a control group of users who did not see ads.[18] We use Google's relatively new geographic bid feature that can determine, with a reasonable degree of accuracy, the Nielsen Designated Market Area (DMA) of the user conducting each query. There are 210 DMAs in the United States, which typically correspond to large metropolitan areas. For example, San Francisco, Oakland, and San Jose, CA, comprise a large DMA while Butte and Bozeman, MT, comprise a smaller DMA.

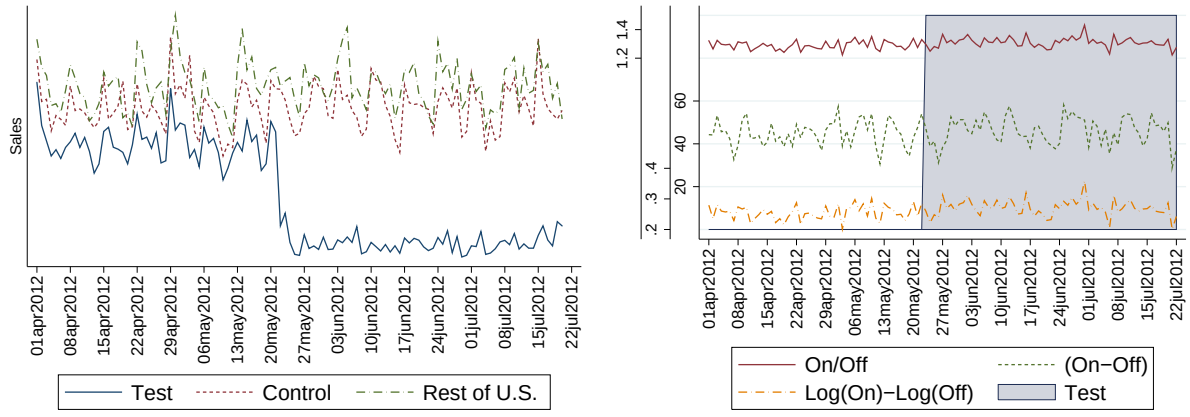
For the test, ads were suspended in roughly 30 percent of DMAs. This was done to reduce the scope of the test and minimize the potential cost and impact to the business (in the event that the ads created considerable profits). A purely random subsample of DMAs were chosen as candidates for the test. Next, candidate DMAs were divided into test and control DMAs using an algorithm that matched historical serial correlation in sales between the two regions. This was done to create a control group that mirrored the test group in seasonality. This procedure implies that the test group is not a purely random sample, but it is certainly an arbitrary sample that does not exhibit any historical (or, ex post) difference in sales trends. The test design therefore lends itself neatly to a standard difference-in-differences estimation of the effect of paid search on sales.

Figure 3a plots total *attributed* sales for the three regions of the U.S.: the 68 test DMAs where advertising ceased, 65 matched control DMAs, and the remaining 77 control DMAs (yielding a total of 142 control DMAs). Attributed sales are total sales of all purchases to users within 24 hours of that user clicking on a Google paid search link.[19] Note that attributed sales do not completely zero out in the test DMAs after the test was launched. The remaining ad sales from test DMAs are an artifact of the error both in Google's ability to determine a user's location and our determination of the user's location. We use the user's shipping zip code registered with eBay to determine the user's DMA and whether or not the user was exposed to ads. If a user makes a purchase while traveling to

[18] Whereas the previous section referred to a test of advertising for branded keywords and their variants, this test specifically excluded brand terms. That is, eBay continued to purchase brand ads nationally until roughly 6 weeks into the geographic test when the brand ads were halted nationwide.

[19] The y-axis is suppressed to protect proprietary sales data. It is in units of dollars per DMA, per day.

Figure 3: Non-Brand Keyword Region Test



(a) Attributed Sales by Region **(b) Differences in Total Sales**

a region exposed to ads but still has the product shipped to her home, we would assign the associated sales to the off region. Attributed sales fall by over 72 percent.[20]

A very simple assessment of the difference-in-differences results is plotted in Figure 3b. We plot the simple difference, ratio, and log difference between daily average sales in the designated control regions where search remained on and the test regions where search is off. As is apparent, the regions where search remained on are larger (about 30 percent) than the regions switched off.[21]

This is an artifact of the selection algorithm that optimized for historical trends. There is no noticeable difference between the pre and post experimental period demonstrating the muted overall impact of paid search. To quantify the impact of paid search, we perform a difference-in-differences calculation using the period of April through July and the full national set of DMAs. The entire regime of paid search adds only 0.66 percent to sales. We then estimate a difference-in-differences and generalized fixed effects as follows:

$$ln(Sales_{it}) = \beta_1 \times AdsOn_{it} + \beta_2 \times Post_t + \beta_3 \times Group_i + \epsilon_{it} \quad (1)$$

$$ln(Sales_{it}) = \beta_1 \times AdsOn_{it} + \delta_t + \gamma_i + \epsilon_{it} \quad (2)$$

[20]This classification error will attenuate the estimated effect towards zero. However, the Instrumental Variables estimates in Table 3 measure an effect on the intensive margin of spending variation, which overcomes the classification error problem.

[21]The Y-axis is shown for the ratio, the log difference, and in differences in thousands of dollars per day, per DMA.

Table 2: Diff-in-Diff Regression Estimates

	Daily		Totaled	
	(1)	(2)	(3)	(4)
	Log Sales	Log Sales	Log Sales	Log Sales
Interaction	0.00659	0.00659	0.00578	0.00578
	(0.00553)	(0.00555)	(0.00572)	(0.00572)
Experiment Period	-0.0460***		0.150***	0.150***
	(0.00453)		(0.00459)	(0.00459)
Search Group	-0.0141		-0.0119	
	(0.168)		(0.168)	
DMA Fixed Effects		Yes		Yes
N	23730	23730	420	420

Standard errors, clustered on the DMA, in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

In this specification, i indexes the DMA, t indexes the day, $Post_t$ is a indicator for whether the test was running, $Group_i$ is an indicator equal to one if region i kept search spending on and $AdsOn_{it}$ is the interaction of the two indicators. In the second specification, the base indicators are subsumed by day and DMA fixed effects. The β_1 coefficient on the interaction term is then the percentage impact on sales because the $Sales_{it}$ is the log of total sales in region i on day t. We restrict attention to sales from fixed-price transactions because auctions may pool users from both test and control DMAs, which in turn would attenuate the effect of ads on sales.[22] We control for inter-DMA variation with DMA clustered standard errors and DMA fixed effects.

Columns (1) and (2) in Table 2 correspond to Equations (1) and (2) respectively where an observation is at the daily DMA level, resulting in 23,730 observations. Columns (3) and (4) correspond to equations (1) and (2) respectively where an observation is aggregated over days at the DMA level for the pre and post periods separately, resulting in 420 observations. All regression results confirm the very small and statistically insignificant effect of paid search ads.

We now examine the magnitude of the endogeneity problem. Absent endogeneity problems we could estimate the effect of ad spending on sales with a simple regression:

$$ln(Sales_{it}) \quad = \quad \alpha_1 \times ln(Spend_{it}) + \epsilon_{it} \tag{3}$$

[22]The results throughout are quantitatively similar even if we include auction transactions.

Table 3: Instrument for Return, Dep Var: Log Revenue

	OLS		IV	
	(1)	(2)	(3)	(4)
Log Spending	0.885***	0.126***	0.00401	0.00188
	(0.0143)	(0.0404)	(0.0410)	(0.00157)
DMA Fixed Effects		Yes		Yes
Date Fixed Effects		Yes		Yes
N	10500	10500	23730	23730

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Columns 1 and 2 of Table 3 show the estimates of a regression of log revenue on log spending during the period prior to our test. As is evident, the simple OLS in column (1) yields unrealistic returns suggesting that every 10 percent increase in spending raises revenues by 9 percent. The inclusion of DMA and day controls in column (2) lowers this estimate to 1.3 percent but still suggests that paid search contributes a substantial portion of revenues. Industry practitioners use some form of equation 3 to estimate the returns to advertising, sometimes controlling for seasonality, which reduce the bias, but still yields large positive returns. However, as we argue above, the amount spent on ads depends on the search behavior of users, which is correlated with their intent to purchase. It is this endogeneity problem that our experiment overcomes.

Columns 3 and 4 of Table 3 show estimations of spending's impact on revenue using the difference-in-differences indicators as excluded instruments. We use two stage least squares with the following first stage :

$$ln(Spend_{it}) \quad = \quad \tilde{\alpha_1} \times AdsOn_{it} + \tilde{\alpha_2} \times Post_t + \tilde{\alpha_3} \times Group_i + \epsilon_{it} \tag{4}$$

The instruments isolate the exogenous experimental variation in spending to estimate the causal impact of spending on changes in revenue. True returns are almost two orders of magnitude smaller and are no longer statistically different from zero suggesting that even eBay's large spending may have no impact at all.

4.2 Consumer Response Heterogeneity

The scale of our experiment allows us to separate outcomes by observable user characteristics because each subset of the user base is observed in both treatment and control DMAs. Econometrically, this can be accomplished by interacting the treatment dummy with dummies for each subgroup which produces a set of coefficients representing the total average effect from the advertising regime on that subgroup.

We examine user characteristics that are common in the literature: recency and frequency of a user's prior purchases. First, we interact the treatment dummy with indicators for the number of purchases by that user in the year leading up to the experiment window.[23] We estimate the following specification:

$$ln(Sales_{imt}) \quad = \quad \beta_m \times AdsOn_{imt} \times \theta_m + \delta_t + \gamma_i + \theta_m + \epsilon_{it} \tag{5}$$

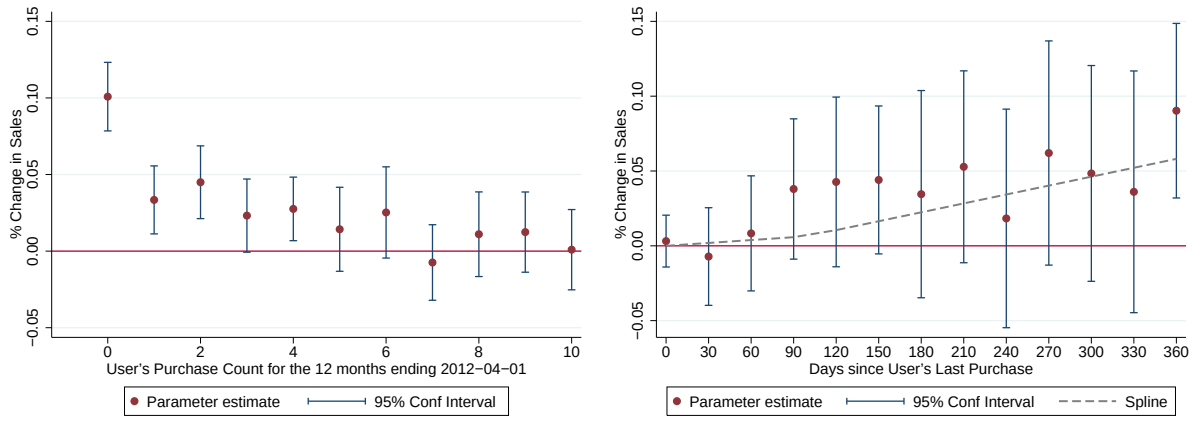
where $m \in \{0, 1, ..., 10\}$ indexes user segments. New registered users are indexed by $m = 0$, those who purchased once in the prior year by $m = 1$, and so on, while $Sales_{imt}$ is the log of total sales to all users in segment m in period t and DMA i. This produces 11 estimates, one for each user segment.[24] Figure 4a plots the point estimates of the treatment interactions. The largest effect on sales was for users who had not purchased before on eBay. Interestingly, the treatment effect diminishes quickly with purchase frequency as estimates are near zero for users who buy regularly (e.g. more than 3 times per year).

Second, Figure 4b plots the interactions by time since last purchase. Estimates become noisier as we look at longer periods of inactivity because there are fewer buyers that return after longer absences which causes greater variance in the sales. The estimates are tightly estimated zero impacts for zero days and consistently centered on zero for 30 and 60 day absences, suggesting that advertising has little impact on active and moderately active customer. However, the effect then steadily rises with absence and impacts are once again large and statistically significant when looking at customers who have not purchased in

[23] Transaction counts exclude the pre-test period used for the difference-in-differences analysis.

[24] This approach is similar to running 11 separate regressions, which produces qualitatively similar results.

Figure 4: Paid Search Impact by User Segment



(a) User Frequency **(b) User Recency**

over a year.[25] We estimate a spline with a break at the arbitrarily chosen 90 day mark and estimate the treatment effect to be 0.02 percentage points larger per month of absence.[26]

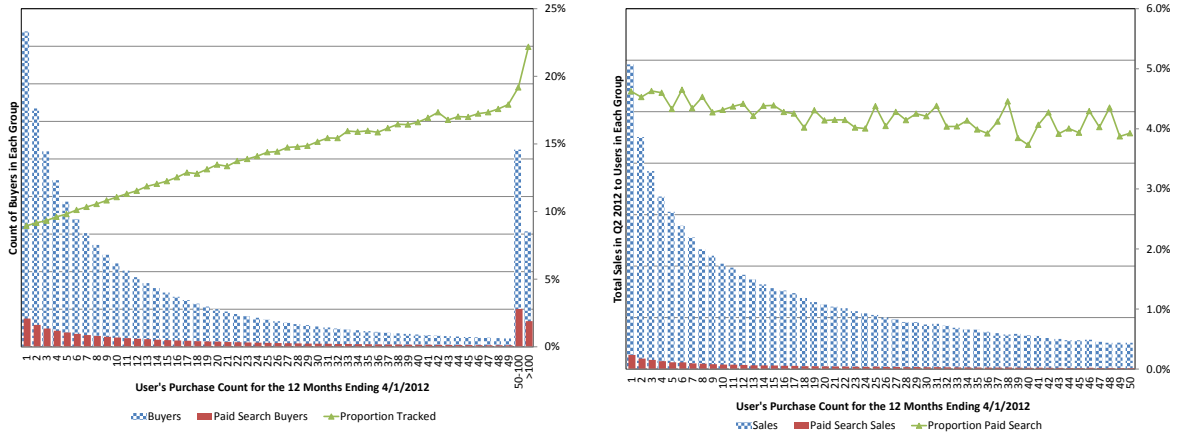
Figure 4 implies that search advertising works only on a firm's least active customers. These are traditionally considered a firms "worst" customers, and advertising is often aimed a high value repeat consumers (Fader et al., 2005). This evidence supports the informative view where ads affect consumption only when they update a consumer's information set. Bluntly, search advertising only works if the consumer has no idea that the company has the desired product. Large firms like eBay with powerful brands will see little benefit from paid search advertising because most consumers already know that they exist, as well as what they have to offer. The modest returns on infrequent users likely come from informing them that eBay has products they did not think were available on eBay. Active consumers already know this and hence are not influenced.

While the least active customers are the best targets for search advertising, we find that most paid search traffic and attributed sales are high volume, frequent purchasers. Figure 5 demonstrates the relationship between attribution and user purchase frequency, and

[25] Gönül and Shi (1998) study a direct mail campaign and find that it is recent individuals are not influenced by mailing because they are likely to buy anyway while mailing dollars are best spent on customers who have not recently purchased.

[26] This estimate is derived by replacing separate coefficients for each user segment to Equation 5 with interactions of the treatment dummy and the number of days since purchase. This is statistically distinguishable from zero, with a standard error of .00004577 so that pooling across user segments provides better evidence of the trend than the noisier separate coefficients.

Figure 5: Paid Search Attribution by User Segment



(a) Buyer Count Mix **(b) Transaction Count Mix**

highlights the tendency of industry-used attribution models to mistakenly treat purchases as causally influenced by ads. Our experiment sheds light on *which* customers are causally influenced as compared to those who are mistakenly labeled as such.

Figure 5a plots the count of buyers by how many purchases they made in a year. The counts are shown separately for all buyers and for those that buy, at any point in the year prior to the experiment, *after* clicking on a paid search ad. The ratio of the two rises with purchase frequency because frequent purchasers are more likely to use paid search at some point. Figure 5b shows the same plot for shares of transaction counts. Even users who buy more than 50 times in a year still use paid search clicks for 4 percent of their purchases. The large share of heavy users suggests that most of paid search spending is wasted because the majority of spending on Google is related to clicks by those users that would purchase anyway. This explains the large negative ROI computed in Section 5.

We have searched for other indicators of consumer's propensity to respond in localized demographic data. Although randomization was done at the DMA level, we can measure outcomes at the zip code level, and so we estimate a zip code level regression where we interact zip code demographic data with our treatment indicator. We find no differential response that is statistically significant across several demographic measures: income, population size, unemployment rates, household size, and eBay user penetration. The coefficient on eBay user penetration (the proportion of registered eBay users per DMA) is

negative, which complements the finding that uninformed and potential customers are more responsive than regular users.

4.3 Product Response Heterogeneity

As argued above, a consumer's susceptibility to internet search ads depends on how well informed they are about where such products are available. Given that the availability of products varies widely, the effectiveness of paid search may vary by product type. As a large e-commerce platform, eBay's paid search advertising campaigns present an opportunity to test the returns to advertising across product categories which vary in competitiveness, market thickness and general desirability. To our surprise, different product attributes did not offer any significant variation in paid search effectiveness.

As in Section 4.2, we decompose the response by interacting the treatment indicator with dummies for sub-groupings of revenue using the category of sales. We found no systematic relationship between returns and category. The estimates center around zero and are generally not statistically significant. At the highest level, only one category is significant, but with 38 coefficients, at least one will be significant by chance.

We explored multiple layers of categorization, ranging from the broadest groupings of hundreds of categories. The extensive inventory eBay offers suggests that some categories would generate returns because customers would be unaware of their availability on eBay. However, we have looked for differential responses in a total of 378 granular product categories and found no consistent pattern of response. Less than 5 percent of categories are statistically significant at the 5 percent confidence level. Moreover, in an examination of the estimates at finer levels of categorization, we found no connection between ordinal ranking of treatment impact product features like sales volume or availability. It is thus evident that for a well known company like eBay, product attributes are less important in search advertising than user intent and, more importantly, user information.

4.4 Where did the Traffic Go?

The brand query tests demonstrated that causal (incremental) returns were small because users easily substituted paid search clicks for natural search clicks. Metaphorically, we closed one door and users simply switched to the next easiest door. This substitution was expected because users were using brand queries as simple navigational tools. Unbranded

queries are not simply navigational because users are using the search platform to find *any* destination that has the desired product. Only experimental variation can quantify the number of users who are actually directed by the presence of search advertising.

Experimentation can also quantify the substitution between SEM and other channels. For example, in Figure 1a we showed Google's search results page for the query "used gibson les paul". Notice that the second ad from the top, as well as the center image of the guitar below it, are both paid ads that link to eBay, while the two bottom results of the natural search part of the page also link to eBay. Hence, some substitution from paid to natural search may occur for non-brand keywords as well. Also, users who intend to visit eBay and do not see ads will choose to directly navigate to www.ebay.com.

In the case of brand term searches, we showed that 99.5 percent of clicks where retained in the absence of paid ads. However, clicks to eBay decline more significantly in the absence of non-brand ads. Advertising clicks dropped 41 percent, and total clicks fell 2 percent as a result of the non-brand experiment.[27] The total loss in clicks is roughly 58 percent of the number of lost paid search clicks, suggesting that 42 percent of paid search clicks are newly acquired. Advertising does increase clicks above and beyond what is taken from natural search. This conforms to expectations based on studies from Google that find the majority of lost paid search clicks would not have been recouped by natural search (Chan et al., 2011a).

But clicks are just part of what generates sales. To make meaningful statements about internet traffic, we need to make an important distinction in the nature of visits. eBay servers are able to distinguish between referring *clicks* (clicks from other sites that lead to an eBay page) and total *visits* (clusters of page visits by the same user). In the course of a single shopping session users will have many clicks referring from other websites because their search takes them on and off eBay pages. Put simply, users will travel to eBay from Google multiple times in one sitting. We found that clicks are lost but revenue is not, so the question remaining is whether paid search increases the number of sessions, the number of users visiting? We define a paid search visit as a session that begins with a paid search click and compare the substitution to comparably defined natural search visits.

We measure this potential traffic substitution by regressing the log of eBay visit counts from either organic search or from direct navigation on the log of eBay visit counts from paid search, using the experiment as an instrument. We find that a 1 percent drop in paid

[27]Natural clicks are a much larger denominator and therefore the total percentage drop is smaller.

search visits leads to a 0.5 percent increase in natural search visits and to a 0.23 percent increase in direct navigation visits. These substitution results suggest that most, if not all, of the 'lost' traffic finds its way back through natural search and direct navigation.

5 Deriving Returns on Investment

To demonstrate the economic significance of our results and interpret their implications for business decisions, we compute the implied short-term return on investment (ROI) associated with spending on paid search. Imagine that the amount spent on paid search was S_0 associated with revenues equal to R_0. Let $\Delta R = R_1 - R_0$ be the difference in revenues as a consequence of an increase in spending, $\Delta S = S_1 - S_0$, and by definition, $ROI = \frac{\Delta R}{\Delta S} - 1$.

Let $\beta_1 = \Delta ln(R)$ be our estimated coefficient on paid search effectiveness from either equations (1) or (2), that is, the effect of an increase in spend on log-revenues as estimated in Table 2. Using the definition of ROI and setting $S_0 = 0$ (no spending on paid search) some algebraic manipulation (detailed in the online appendix) yields,

$$ROI \approx \frac{\beta_1}{(1+\beta_1)} \frac{R_1}{S_1} - 1 \tag{6}$$

For the OLS and IV estimates of Table 3, we translate the coefficient $\alpha_1 = \frac{\Delta ln(Sales)}{\Delta ln(Spend)}$ from equation (3) to a measure comparable to β_1 by multiplying by the coefficient $\tilde{\alpha_1} = \Delta ln(Spend)$ estimated from Equation 4, the first stage in the IV. This converts the IV estimates to reduced form estimates and approximates estimates derived from direct estimation of the difference-in-differences procedure. Both the derived and directly estimated β_1's can then be used to compute an ROI with Equation 6.

In order to calculate the ROI from paid search we need to use actual revenues and costs from the DMAs used for the experiment, but these are proprietary information that we cannot reveal due to company policy. Instead, we use revenues and costs from public sources regarding eBay's operations. Revenue in the U.S. is derived from eBay's financial disclosures of Marketplaces net revenue prorated to U.S. levels using the ratio of gross market volume (sales) in the U.S. to global levels, which results in U.S. gross revenues

Table 4: Return on Investment

	OLS		IV		DnD	
	(1)	(2)	(3)	(4)	(5)	
Estimated Coefficient	0.88500	0.12600	0.00401	0.00188	0.00659	A
(Std Err)	(0.0143)	(0.0404)	(0.0410)	(0.0016)	(0.0056)	B
Δln(Spend) Adjustment	3.51	3.51	3.51	3.51	1	C
Δln(Sales) (β)	3.10635	0.44226	0.01408	0.00660	0.00659	D=A*C
Spend (Millions of $)	$ 51.00	$ 51.00	$ 51.00	$ 51.00	$ 51.00	E
Gross Revenue (R_1)	2,880.64	2,880.64	2,880.64	2,880.64	2,880.64	F
ROI	4173%	1632%	-22%	-63%	-63%	G=A/(1+A)*(F/E)
ROI Lower Bound	4139%	697%	-2168%	-124%	-124%	
ROI Upper Bound	4205%	2265%	1191%	-3%	-3%	

of $2,880.64 million.[28] We next obtain paid search spending data from the release of information about the expenditures of several top advertisers on Google. We calculate eBay's yearly paid search spending for the U.S. to be $51 million.[29]

Table 4 presents the ROI estimates using our analysis together with the publicly available reports on revenue and spending. As is evident, simple OLS estimation of α_1 yields unrealistic returns of over 4000 percent in Column (1) and even accounting for daily and geographic effects implies returns that are greater than 1500 percent, as shown in Column (2). The IV estimation reduces the ROI estimate significantly below zero and our best estimate of average ROI using the experimental variation is negative 63 percent as shown in Columns (4) and (5). This ROI is statistically different from zero at the 95 percent confidence level emphasizing the economic significance of the endogeneity problem.

6 Discussion

Our results show that for a well-known brand like eBay, the efficacy of SEM is limited at best. Paid-search expenditures are concentrated on consumers who would shop on eBay regardless of whether they were shown ads. Of the $31.7 billion that was spent in the U.S. in 2011 on internet advertising, the top 10 spenders in this channel account for about $2.36 billion. These companies generally use the same methods and rely on the same external

[28]Total revenues for 2012 were $7,398 and the share of eBay's activity in the U.S. was $26,424/$67,763. See `http://files.shareholder.com/downloads/ebay/2352190750x0x628825/e8f7de32-e10a-4442-addb-3fad813d0e58/EBAY_News_2013_1_16_Earnings.pdf`

[29]Data from Google reports a monthly spend of $4.25 million, which we impute to be $51 million. See `http://mashable.com/2010/09/06/brand-spending-google/`.

support to design their ad campaigns, suggesting many reasons to believe that the results we presented above would generalize to these large and well known corporations. This may not be true for small and new entities that have no brand recognition.[30]

This begs the question: why do well-known branded companies spend such large amounts of money on what seems to be a rather ineffective marketing channel? One argument is that there are long term benefits that we are unable to capture in our analysis. That said, the fact that the lion's share of spending is on customers who seem to be well aware of the site, and who continue to visit it in lieu of ads being shown to them, suggests that the long term benefits are limited and small. Furthermore, for brand-keyword advertising it is obvious that the user searched for the brand name and hence is well aware of it, making brand-keyword advertising redundant. Arguments have been made that brand-keyword advertising acts as a defense against a competitor bidding for a company's brand name. This implies that brand-keyword advertising allows competing companies to play a version of the Prisoner's Dilemma. A company and its competitor would both be better off not buying any brand-keywords, but each cannot resist the temptation to pinch away some of their competitor's traffic, and in the process, the ad platforms benefit from this rent-seeking game. It should be noted, however, that since eBay stopped bidding on its brand-keywords, such behavior by potential competitors was not observed.[31]

Our experience suggests that the reason companies spend vast amounts on SEM is the challenges they face in generating causal measures of the returns to advertising. As the results in Table 5 demonstrate, typical regressions of sales on advertising spend result in astronomical ROI estimates that vastly overestimate the true ROI, which can only be estimated using controlled experiments. This is in line with results obtained recently by Lewis et al. (2011) regarding the effectiveness of display ads.[32]

[30]If you were to start a new online presence selling a high quality and low-priced widget, someone querying the word "widget" would still most likely not see your site. This is a consequence of the PageRank algorithm that relies on established links to webpages. Only after many websites link to your site, related to the word widget, will you stand a chance of rising to the top of the organic search results.

[31]Some retailers have been bidding, both before and after ebay's response, to "broad" brand phrases such as "ebay shoes". It is also interesting to note that several advertisers pushed unsuccessfully for litigation to prevent their competitors from bidding on their trademark keywords, suggesting that some companies understand the Prisoner's Dilemma nature of this activity.

[32]Using a controlled experiment they demonstrated that displaying a certain brand ad resulted in an increase of 5.4 percent in the number of users performing searches on a set of keywords related to that brand. They then compared the experimental estimate to an observational regression estimate, which resulted in an increase ranging from 871 percent to 1198 percent.

In the absence of causal measures, the industry relies on 'attribution' measures which correlate clicks and purchases. By this measure, eBay performed very well as shown in Figure 3a and Table 4. eBay's ads were very effective at earning clicks and associated purchases. Our findings here suggest, however, that these incremental clicks do not translate into incremental sales. This is an important way in which our methodology differs from the one used in studies released by Google. Chan et al. (2011a) report that experimental studies performed at Google proved that about 89% of paid search clicks were deemed to be incremental, that is, would not have happened if companies would not pay for search. As Section 4.4 shows, our results confirm that a majority of ebay's paid searches are not recovered when ebay stops paying for them. Nonetheless, the majority of these clicks did not result in incremental sales, which in turn is the reason that paid search was ineffective as clicks alone are not a source of revenues.

It is precisely for this reason that scholars resort to natural experiments (e.g. Goldfarb and Tucker, 2011b) or controlled experiments (e.g. Lewis et al., 2011), in order to correctly assess a variety of effects related to internet marketing. This approach, however, is not the norm in industry. Not only do most consulting firms who provide marketing analytics services use observational data, recommendations from Google offer analytical advice that is not consistent with true causal estimates of ad effectiveness. As an example, consider the advice that Google offers its customers to calculate ROI:

> "Determining your AdWords ROI can be a very straightforward process if your business goal is web-based sales. You'll already have the advertising costs for a specific time period for your AdWords account in the statistics from your Campaigns tab. The net profit for your business can then be calculated based on your company's revenue from sales made via your AdWords advertising, minus the cost of your advertising. Divide your net profit by the advertising costs to get your AdWords ROI for that time period."[33]

This advice is akin to running a regression of sales on AdWords expenditures, which is not too different from the approach that results in the inflated ROIs that we report in Columns (1) and (2) of Table 5. It does not, however, account for the endogeneity concern that our study highlights where consumers who use paid search advertising on their way to a purchase may have completed that purchase even without the paid search ads. The

[33] From `http://support.google.com/adwords/answer/1722066` during March 2013

only way to circumvent this concern is to adopt a controlled test that is able to distinguish between the behavior of consumers who see ads and those who do not. Once this is done correctly, estimates of ROI shrink dramatically, and for our case become negative.[34]

It is interesting to note that the incentives faced by advertising firms, publishers, and even analytics consulting firms, are all aligned with increasing the advertising budgets of advertisers. This may not be, however, aligned with the bottom line of these advertisers because the bias in the methods used inflates the efficacy of advertising, which in turn can justify an increase in advertising budgets.

The experimental design that we use exploits the ability of an advertiser to geographically control the ad expenses across DMAs, thus yielding a large-scale test-bed for ad effectiveness. The idealized experiment would have been conducted on a more refined user-level randomization, which would allow us to control for a host of observable user-level characteristics.[35] Still, given the magnitude of our experiment and our ability to aggregate users by measures of recency, frequency, and other characteristics, we are able to detect heterogeneous responses that shed light on the way users respond to ads. As targeting technology is developed further in the near future, individual-level experiments can be performed and more insights will surely be uncovered.

[34] As mentioned earlier, this is only a short term estimate. Still, to overcome the short-term negative ROI of 63% it is possible to calculate the lifetime value of a customer acquired that would be required to make the investment worthwhile. This is beyond the scope of our current research.

[35] For example, Anderson and Simester (2010) analyze results from experiments using direct mailing catalogs which are addressed to specific individuals for which the company has lots of demographic information. This level of targeting is not currently feasible with online ads.

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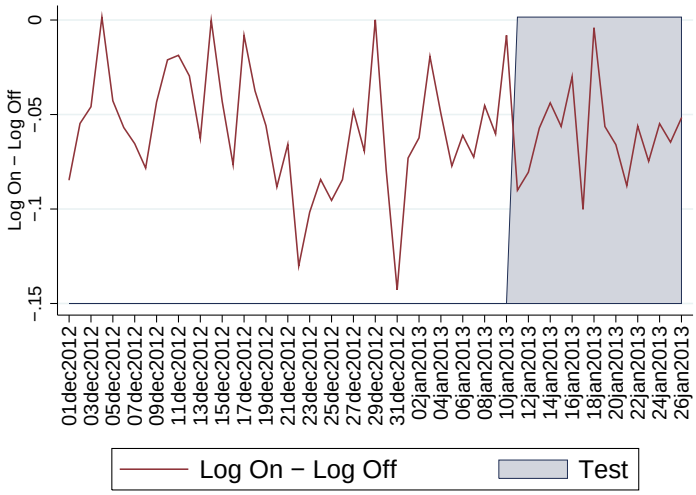
Appendix

Conrolled Brand-Keyword Experiments

In January 2013, we conducted a follow-up test of the brand term paid search, specifically keyword eBay, using geographic variation. Google offers geographic specific advertising across Germany's 16 states. So we selected a random half of the country, 8 states, where brand keyword ads were suspended. This test design preserves a randomly selected control group which is absent from the simple pre-post analysis shown in Section 3.

As was predicted by the earlier tests, there was no measurable impact on revenues. The sample size for this analysis is smaller because there are fewer separable geographical areas and the experiment window is shorter. Figure 6 shows the log difference between means sales per day in the on and off states. The treatment group is 5 percent smaller, on average, than the control because there are few states so any random division of states generates a baseline difference. The plot shows that there is no change in the difference once the experiment begins. The plot also shows the large variation in daily differences of the means which suggests that detecting a signal in the noise would be very difficult.

Figure 6: Brand Test Europe: Difference in Total Sales



We perform a difference-in-differences estimation of the effect of brand advertising on sales and find no positive impact. Table 5 presents the results from three specifications: the baseline model from Section 4.1 in Column (1), the same specification with the (less noisy) subset of data beginning January 1, 2013 in Column (2), and the smaller subset

with state specific linear time trends in Column (3). All results are small, statistically insignificant, and negative. Narrowing the window and controlling for state trends reduces the magnitude of the estimate, which is consistent with a zero result. This test is noisier than the U.S. test because there are substantially fewer (16) geographic regions available for targeting. This makes the confidence intervals larger. We also lack public estimates of spending for Germany and are therefore unable to derive a confidence interval around the ROI which is likely to be large anyway due to the smaller total spending levels for branded advertising. The negative point estimates support the findings of the U.S. brand spending changes and lead us to conclude that there is no measurable or meaningful short run return to brand spending.

Table 5: Brand Test Europe: Difference-in-Differences Estimates

	(1)	(2)	(3)
	Log Sales	Log Sales	Log Sales
Interaction	-0.00143	-0.00422	-0.000937
	(0.0104)	(0.0132)	(0.0140)
State Fixed Effects	Yes	Yes	Yes
Date Fixed Effects	Yes	Yes	Yes
Post Jan 1		Yes	Yes
State Trends			Yes
N	912	416	416

Standard errors, clustered on the state, in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Randomization Procedure Detail

The treatment assignment used a stratification procedure to ensure common historical trends between treatment and control. This means that the treatment dummy is not a simple random variable but instead lends itself to a difference-in-differences estimation. The test regions, or DMAs, were chosen in two steps. First, 133 of the 210 U.S. regions were selected to be candidates for treatment purely at random. Of these, only about half were alloted to be treated (advertising turned off). To minimize historical variation between test and control, groups of 68 were drawn at random and then the historical weekly serial correlation was computed. Several draws with very low historical correlation were discarded before the current draw of 68 in one group and 65 in another. Which group was actually turned off was decided by a coin flip. The 68 regions were then turned off at an arbitrary date (based largely on engineering availability). This procedure lends itself

perfectly to a difference-in-differences estimation where the core underlying assumption is common trends.

Candidacy IV estimation

The assignment of DMAs into treatment cells for the non-brand keyword experiment was stratified on historical trends. This stratification lacks the clarity of a total random assignment but the methodology admits an alternative approach that leverages the completely random assignment to the set of DMAs eligible for testing. We use the assignment to the *candidate* group as an instrument for whether or not a DMA was assigned to the treatment group. We collapse the data to the DMA level and use two stage least squares to estimate the effect of treatment assignment and advertising spending on revenue. We include pre-period sales in both stages to control for variations in DMA size. The first and second stages are as follows:

$$ln(Sales_i) = \beta_0 + \beta_1 \times AdsOn_i + \beta_2 \times ln(PreSales)_i + \epsilon_i \quad (7)$$

$$AdsOn_i = \alpha_0 + \alpha_1 \times Candidate_i + \mu_i \quad (8)$$

The estimates are shown in Table 6. The coefficients on both extensive ($AdsOn$) and intensive (spending level) are smaller than those in Tables 2 and 3, respectively. If the stratification assignment introduce a bias into the treatment effect, it is a positive bias which makes our primary estimates an upper bound on the true impact of advertising on paid search. The standard errors in this IV approach are larger than primary specifications making this method less precise. The loss of precision in the IV stems from the reduction in observations from 420 in Table 2 to 210 because the difference-in-differences approach uses the exogenous timing of the treatment. Moreover, the stratification was in fact designed to reduce inter-temporal variance across treatment cells.

Candidacy Difference-in-Differences

To further check the randomization procedure we run estimate the difference-in-differences using the candidacy indicator as the treatment dummy. This would estimate the diluted effect on all DMAs that were considered candidates for testing. Column 1 of Table 7 shows the results. The negative coefficient here is expected since 'candidate' DMAs were

Table 6: Candidate DMA Instrument

	(1) ln(Test Period Sales)	(2) ln(Test Period Revenue)
Ads On	0.00207	
	(0.0108)	
ln(Test Period Cost)		0.000795
		(0.00350)
ln(Pre Period Sales)	1.007***	
	(0.00226)	
ln(Pre Period Revenue)		0.997***
		(0.0113)
ln(Pre Period Cost)		0.00877
		(0.0109)
Constant	0.0436	0.102**
	(0.0354)	(0.0520)
Observations	210	210

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 7: Candidate DMA Diff-in-Diff

	(1) Log Sales	(2) Log Sales
Interaction	-0.00247	0.00124
	(0.00526)	(0.00653)
Candidate for Off DMA	0.167***	1.194***
	(0.00288)	(0.00358)
Experiment Period	-0.199***	-0.195***
	(0.0117)	(0.0150)
Constant	11.87***	11.87***
	(0.00738)	(0.00924)
Date Fixed Effects	Yes	Yes
DMA Fixed Effects	Yes	Yes
Ads On Only DMAs		Yes
N	23730	16046

Standard errors, clustered on the DMA, in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

candidates for turning ads off. Thus, the 0.25 percent is just under half the magnitude of the main estimates we present.

To check the randomization, we repeat this estimation on the sub-sample of DMAs that were not selected into treatment. These DMAs were all controls in the main estimation

and ad spending was on throughout the experiment. The coefficient therefore represents the change in the candidate control regions over the non-candidate control regions during the test. Column 2 of Table 7 presents the results, a small positive coefficient which is statistically zero. We conclude that the stratification procedure did not create any evidence of a biased treatment assignment.

ROI Results in Levels

The preferred specification reports regressions in logged dependent variable because the dependent variable is very right skewed (some DMAs are very large and some days have large positive shocks). The simple difference-in-differences estimate would estimate the average increase in sales by region per day requires context. Given the large variance across days and DMAs, the average is not particularly meaningful. The IV approach of Equaitons 3 and 4 permits a more straightforward estimation of ROI using level regressions. Table 8 presents the result of an estimation of Equaitons 3 and 4 where ln(Spend) and ln(Rev) is replace with spend and revenue, respectively, in dollars. The coefficient can be interpreted as the dollar increase in revenue for every dollar spent. A coefficient of 0.199 implies an ROI of -80 percent, comparable but slightly more negative than the primary results of -63 percent. The confidence interval of this estimate excludes the break-even point of 1. The levels estimate is qualitatively similar to the log results and so we present the more conservative estimation as our preferred specification.

Table 8: ROI in Levels

	(1)
	Revenue ($)
Cost ($)	0.199
	(0.161)
DMA Fixed Effects	Yes
Date Fixed Effects	Yes
N	23730

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

ROI Calculations

Recall that ROI is defined as

$$ROI = \frac{R_1 - R_0}{S_1 - S_0} - 1 \equiv \frac{\Delta R}{\Delta S} - 1. \tag{9}$$

Let R_i be the revenue in DMA i and let $D_i = 1$ if DMA i was treated (paid search off) and $D_i = 0$ if it was not (paid search on). The basic difference-in-difference regression we ran is

$$\ln(R_{it}) = \beta_1 D_i + \delta_t + \gamma_i + \epsilon_{it}$$

where δ_t and γ_i are time and DMA fixed effects. Using the natural logarithm $\ln R_{it}$ implies that for small differences in $R_1 - R_0$ the coefficient β_1 in the regression is aproximately the percent change in revenue for the change in the spend level that results from the experimental treatment. This means that for two revenue levels R_1 and R_0 we can write

$$\beta_1 \approx \frac{R_1 - R_0}{R_0},$$

or,

$$R_0 \approx \frac{R_1}{1 + \beta_1} \tag{10}$$

Because the spend in the "off" DMAs is $S_0 = 0$ (or close to it) and in the "on" DMAs is some S_1, then using (10) and (9) we can derive the approximate ROI as,

$$ROI = \frac{R_1 - R_0}{S_1 - S_0} - 1 \approx \frac{R_1 - \frac{R_1}{1+\beta_1}}{S_1} - 1 = \frac{\beta_1}{(1 + \beta_1)} \frac{R_1}{S_1} - 1. \tag{11}$$

Thus, the equation in 11 gives a well defined and financially correct estimate of the ROI based on the difference-in-difference estimate of the experimental results when $S_0 = 0$.

Unlike the difference-in-difference estimates, the OLS and IV estimates were derived from the regression

$$\ln(R_{it}) = \alpha_1 \ln(S_{it}) + \epsilon_{it}$$

where the first stage of the IV estimation used the regression

$$\ln(S_{it}) = \tilde{\alpha_1}[AdsOn_{it}] + \tilde{\alpha_2}[Post_t] + \tilde{\alpha_3}[Group_i] + \epsilon_{it}$$

From these, we find $\alpha_1 = \frac{\Delta ln(Sales)}{\Delta ln(Spend)}$ and $\tilde{\alpha_1} = \Delta ln(Spend)$ for which the approximation to a percentage change is poor since the change in spend was large. Therefore, to make use of the log-log regression coefficients, it is possible to translate them into a reduced form effect because,

$$\alpha_1 * \tilde{\alpha_1} = \frac{\Delta ln(Sales)}{\Delta ln(Spend)} * \Delta ln(Spend) = \Delta ln(Sales) = \beta_1$$

which we can substitute into (11). Thus, for the estimated α_1 of the OLS estimates, we can use $\tilde{\alpha_1}$ to derive a comparable β_1 which can be used to compute an ROI that is comparable across all specifications.