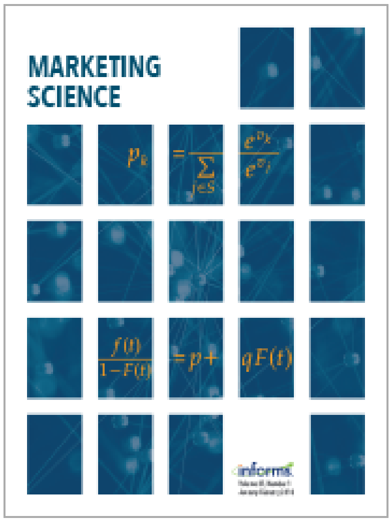

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How Does the Use of Trademarks by Third-Party Sellers Affect Online Search?

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Firms that sell via a direct channel *and* via indirect channels have to decide whether to allow third-party sellers to use trademarked brand names of products in their advertising. This question has been particularly controversial for advertising on search engines. In June 2009, Google started allowing any third-party reseller of a product to use a trademark such as "DoubleTree" in the text of its ad, even if the reseller did not have the trademark holder's permission. We study the effects of this change empirically within the hotel industry. We find some evidence that allowing third-party sellers to use a trademark in their online search advertising weakly reduced the likelihood of a consumer clicking on a trademark holder's paid search ads. However, the decrease in paid clicks was outweighed by a large increase in consumers clicking on the unpaid links to the hotelier's website within the main search results. Our evidence shows that when a third-party seller focuses on a trademarked brand in its ads, the ads become less distinct, and customers are more likely to ignore the advertised offers and buy from the direct channel.

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1. Introduction

Suppose a consumer wants to book a room at a Doubletree hotel and searches for "DoubleTree" using a search engine. Next to the main search results there will be a separate set of paid search ads that each contain a link to a website. These ads will be not only for the direct channel (DoubleTree.com) but also for third-party resellers such as HotelReservations.com. Should DoubleTree allow third parties to use the DoubleTree trademark in the text of their ads? If the use of the trademark legitimizes the third-party seller as an alternative outlet for the brand, the trademark holder may lose money. DoubleTree would have to pay a 10% commission to the agent that could have been avoided had the customer not been diverted from DoubleTree's own website. Even worse, a travel agency website may lead the consumer to book a room at a competing hotel. Such fears have led legal analysts to estimate losses of $400 million annually for the hotel industry due to use of trademarks to trigger ads and in ad copy by third-party sellers (Ripin 2007); such practices have been referred to as "poaching" (Sayedi et al. 2012).

The advertising literature has a different prediction. Work by Koch and Ullman (1985) and Itti (2005) on visual distinctness suggests that the salience of a paid search ad is not determined solely by its own design but also by the extent to which it is distinct from paid search ads. Similarity in ad features leads to competitive ad clutter (Kent and Allen 1993, Pieters et al. 2007, Danaher et al. 2008, Goldfarb and Tucker 2012), which reduces the efficacy of advertising. If third-party sellers' ads highlight the same trademark, they risk becoming less distinct, and consumers may instead choose the nonadvertised path to the direct channel. Therefore, the empirical consequences of the use of trademarks by third-party sellers are not clear-cut, making this an empirical question.

In June 2009, Google began allowing advertisers to use trademarks in the text of their paid search ads even if they did not have the permission of the trademark holder. Paid search ads appear in a separate column next to the main search results when consumers query a specific search term. Firms must pay for clicks on links in their paid ads but do not pay for clicks on links in the main results.

We compare changes in click behavior by customers who used search engines to query major U.S. hotel brand trademarks before and after Google's policy change. We use aggregate data from comScore that describes which websites U.S. consumers visited after searching Google or Yahoo! using a trademarked search term from April to August 2009. We compare how clicks changed on Google (where the policy change occurred) to Yahoo! (where there was no such change in policy).

We find little evidence of harm to the trademarks' direct channels. The trademark holders' websites did receive (marginally) fewer clicks on their paid search ads after the change in policy. However, the decrease was outweighed by a large increase in the number of clicks on the nonpaid links to the trademark holders' websites within the main search results. When third-party ads started displaying the trademarks, search engine users started clicking directly on the main links to the trademark holders' websites.

Our finding is robust to different functional forms, specifications, and control groups. We show that no such effect occurred in the previous year or for related searches that were unaffected by the policy change. We also replicate our results in the controlled conditions of an online survey, and we show that when advertising is already indistinct, no such effect occurs from the addition of trademarks. Furthermore, when a larger number of ads contribute to the clutter, the positive spillover effects are stronger.

The interdependency between paid ads and nonpaid links in search results is not a new finding: Yang and Ghose (2010) find a positive interdependence between whether a paid ad is present for a particular retailer and whether someone clicks through the retailer's nonpaid listing, and Chiou and Tucker (2012a) show that the extent of interdependence varies depending on whether the search term is a brand name. What is novel about our study is the finding of spillover effects to the nonpaid search result from *other retailers'* ads if these ads highlight the trademark. Such spillover effects are analogous to the finding by Anderson et al. (2010a) that when a catalog company shares its mailing list with a rival firm, sales actually increase for some of the firm's own products.

2. Policy Change

On May 14, 2009, Google announced that it would begin allowing advertisers to use a trademark within the text of their ads without the trademark holder's permission as long as the trademark is referred to in "a descriptive or generic way," and the advertiser either resells or offers information about the trademark holder's products. This was a major shift from Google's previous policy where an advertiser was required to remove a trademarked term from the text of the ad if the advertiser did not own the trademark.

Google began accepting such ads at 11 AM PDT on May 15th, but it did not start displaying them until June 15th. Figure 1(a) shows a mock-up of a search result for a Hyatt hotel before the policy change. Only ads with generic wording were allowed. Figure 1(b) shows how the same search result would have looked after the policy change. The search term is bolded in the text, highlighting the trademark.

The question of how trademarked terms in search ads affect advertising outcomes is a new one for the marketing literature. Earlier research such as Cohen (1986, 1991) and Krasnikov et al. (2009) has pointed out that trademarks represent a crucial part of a firm's branding efforts. Other topics have included research into how off-line search costs affect trademarks (Png and Reitman 1995) and trademark dilution (Morrin and Jacoby 2000, Morrin et al. 2006).

The use of trademarks in search is an important question for marketing because it has been claimed that the advent of search engines has turned trademark law "upside down" (Zimmerman 1999, Hursh 2004). Much of the legal discussion has focused on the question of whether or not firms should be allowed to advertise if a consumer searches a trademarked brand name (Bechtold 2011).[1] Empirically, such instances of competitive "piggybacking" have found to be rare (Rosso and Jansen 2010).

Several legal cases have also focused on the use of trademarks in the ad copy. For example, in *Edina Realty, Inc. v. TheMLSonline.com* (2006, WL 737064, Civil No. 04-4371 (D. Minn. 2006)), the Court objected that the ad by TheMLSonline.com used the Edina Realty trademark as its headline. Similarly, the recent European Court of Justice decision relating to *Hotels Méridien v. Google France* (Tribunal de grande instance, Nanterre, December 16, 2004) and *Accor v. Overture* (Cour d'Appel Versailles, 12e ch., November 2, 2006) suggests that trademarks in ad content could be problematic.[2]

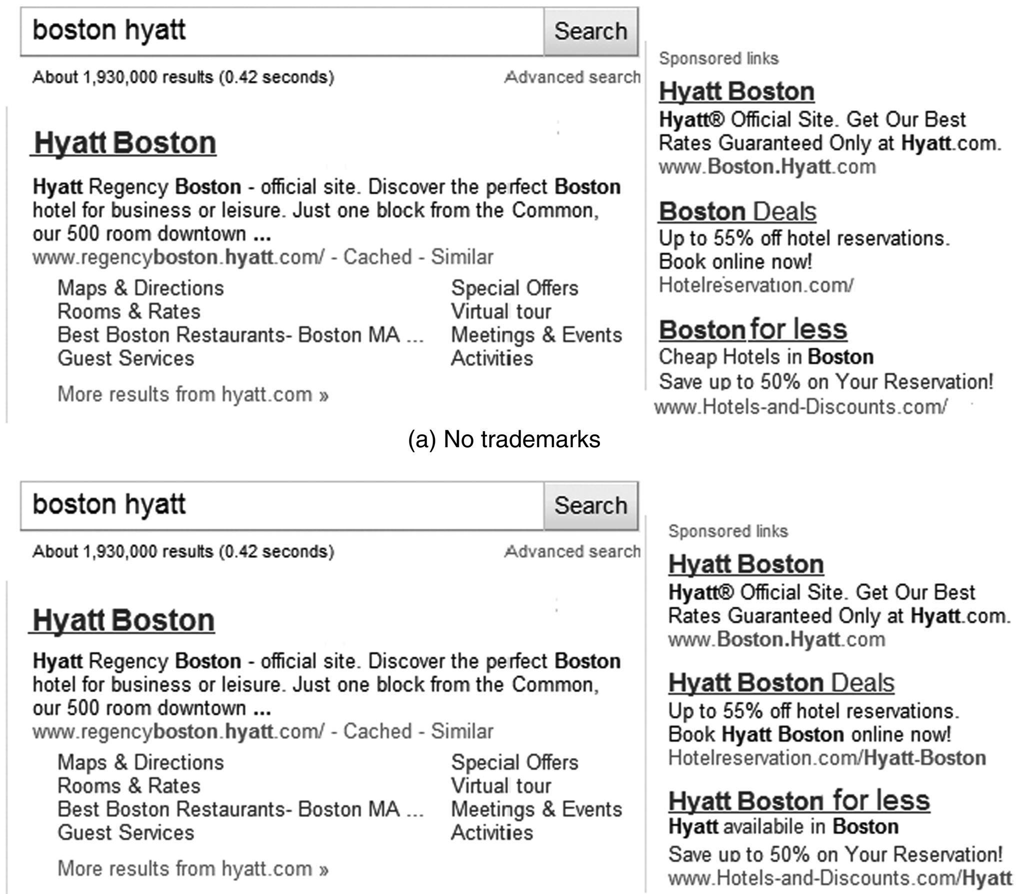
3. Conceptual Framework

This discussion shows that, in general, the legal policy literature has assumed that if third-party resellers use a trademarked term in their advertising campaign, the trademark holder will be hurt. For example, O'Connor (2007, p. 377) states that "customers are undoubtedly being diverted and urgent action is needed to reclaim hotel trademarks in the

[1] The rulings have been contradictory. For example, in *Merck & Co., Inc. v. MediPlan Health Consulting, Inc.* (425 F. Supp. 2d 402 (S.D.N.Y. 2006)), Merck lost its attempt to prevent MediPlan, a Canadian Internet pharmacy, from bidding on search terms such as "Zocor." Other rulings include *Playboy v. Netscape* (354 F.3d 1020 (9th Cir. 2004)), *GEICO v. Google* (330 F. Supp. 2d 700 (E.D. Va. 2004)), *Google v. American Blinds* (2005 WL 832398 (N.D. Cal. 2005), motion reconsidered 2007 WL 1159950 (N.D. Cal. Apr. 27, 2007)), *800-JR Cigar v. GoTo.com* (437 F. Supp. 2d 273 (D. N.J. 2006)), and *Rescuecom Corp. v. Google* (2009 WL 875447 (2d Cir. April 3, 2009)).

[2] See Court of Justice of the European Union (2009).

Figure 1 How the Appearance of Search Results Changed



(b) Trademarks

search environment." This is echoed by Clemons and Madhani (2010–2011), who suggest that such practices by search engines are akin to anticompetitive behavior.

However, the effect of competing advertisers using trademarks online is not as clear-cut as the legal literature might suggest. The literature ignores the fact that paid search ads are only successful if they manage to divert consumers away from the main listing. Studies by Kent and Allen (1993) and Danaher et al. (2008) show that when similar ads are presented together, consumers perceive them as clutter and are more likely to ignore them. Eye-tracker results from Pieters et al. (2007) emphasize that the similarity of ads determines whether consumers view advertising as clutter. Theoretically, therefore, the introduction of trademarks could increase advertising clutter in two ways. First, when all advertisers focus their ad around the same trademark, consumers may experience each ad as being less distinct. Paid ads will offer a less compelling reason for the consumer to divert from the main nonpaid listing. Second, if advertisers are encouraged to start advertising because they can now use trademarks, then the number of similar ads will increase, again contributing to clutter.

The theory predicts that the number of paid clicks for the trademark holder will decrease as its ad is made less distinct relative to its competitors. However, the effect on nonpaid clicks for the trademark holder is ambiguous, and if the effects of advertising clutter are strong enough, nonpaid clicks for the trademark holder may even increase.

4. Field Studies

4.1. Data

We use data on consumer search and navigation behavior from comScore. comScore tracks the online activity of a panel of more than two million users in order to provide commercial data products. ComScore is not open about its recruitment methods, but it does claim that the panel is representative.

We had access to a database named comScore Marketer. The database records the total aggregate number of paid and nonpaid clicks that various websites received over the past two years after searches for specified search terms using major search engines were performed.[3] We extracted aggregate data on searches that contained trademarked names for major hotel brands in the United States. We focus on the hotel industry for two reasons. First, because comScore data record only whether someone visited a website and not the subsequent activity at a website, we wanted to study a sector where a visit to a company's website is meaningful in itself. Hotel brand websites currently account for 69% of all online hotel bookings in the United States (PhoCusWright 2009). Second, the hotel industry has been the setting for major litigation over trademarks and search advertising. Owners of hotel brands do not have to pay commission if they sell their rooms directly, so they have an incentive to direct Internet business to their sites (Vinhas and Anderson 2005).[4]

To determine our sample of hotel brands, we started with the top 300 hotel brands as reported by *Hotels* magazine in its July 2007 issue.[5] Of these, we identified brands that were based primarily in the United States and where comScore panel members conducted more than one search in April 2009. Our sample contains 53 such brands. The vast majority of hotel brands that we excluded were non-U.S. brands such as Barceló and Jin Jiang. We excluded non-U.S. brands because Google changed its policy only on its U.S. website, and the majority of comScore panel data members are located in the United States. In a few instances, hotels maintain explicit policies that prohibit their associated travel agents from using their trademarks in their ad copy. The major companies with these policies are Marriott and InterContinental Hotels Group. Consequently, we also exclude the brands owned by these companies from our data set.[6] In §4.4.2, we use these brands as a robustness check. The top panel of Table 1 describes the monthly aggregate statistics for each search.

For each of these 53 different branded search terms, we collected aggregate data on the number of paid and nonpaid clicks to different websites after consumers used the trademark as a search term.[7] All of the hotels in our study engage in search advertising; they pay for some of the clicks their websites receive. On average, our data suggest that hotel trademark holders pay for 18% of the clicks they receive.[8] A high correlation exists between the total number of clicks and the number of rooms that a hotel chain controls (0.74). This provides some face validity to the data. The correlation is weakest for economy motel chains such as Econo Lodge that presumably rely more heavily on "walk-in" customers than on customers who book ahead online.

Table 1 Data Summary

	Mean	Std. dev.
Search term level		
Monthly average paid clicks for search term	25,472.0	39,378.5
Monthly average nonpaid clicks for search term	109,799.6	197,655.6
Observation: Search engine–search term–website–month		
Paid clicks	865.1	5,675.8
Nonpaid clicks	3,729.0	24,858.5
Google search engine	0.50	0.50
Trademark holder's website	0.10	0.30
Number of paid ads associated with search term	4.11	4.58
Number of third-party ads associated with search term	2.67	3.71

Notes. Observations = 6,360. Summary statistics are from April 2009 to September 2009.

In addition to trademark holders' websites, people visited 66 distinct third-party websites in sufficient numbers for comScore to report data. The sites were either online travel agencies (e.g., Expedia.com, Hotels.com) or websites that direct customers to online travel agencies (e.g., TripAdvisor.com).

As comScore provides data on a monthly basis, we collected these data for April through October 2009. In our main analysis, we compare April and May 2009 with July and August 2009. We use the September and October 2009 data in our analysis of long-run effects in §4.5. We omit data from June 2009 from our empirical analysis because the date of the policy change (June 15th) fell exactly in the middle of that month, making inference difficult. We use data for the Yahoo! and Google search engines. On June 3, 2009, Microsoft rebranded its live search engine as Bing, which made it a problematic candidate for a control group.

An observation occurs at the level of search engine–search term–website–month. For example, we

[3] The aggregate nature of this commercial data set contrasts with the individual nature of the comScore data for 100,000 panelists used by researchers such as Park and Fader (2004). However, these individual-level data have only been released to researchers for 2002 and 2004, and so they cannot be used for this study.

[4] There is additional empirical evidence that hotels may even be able to command price premiums in online channels (De los Santos et al. 2012).

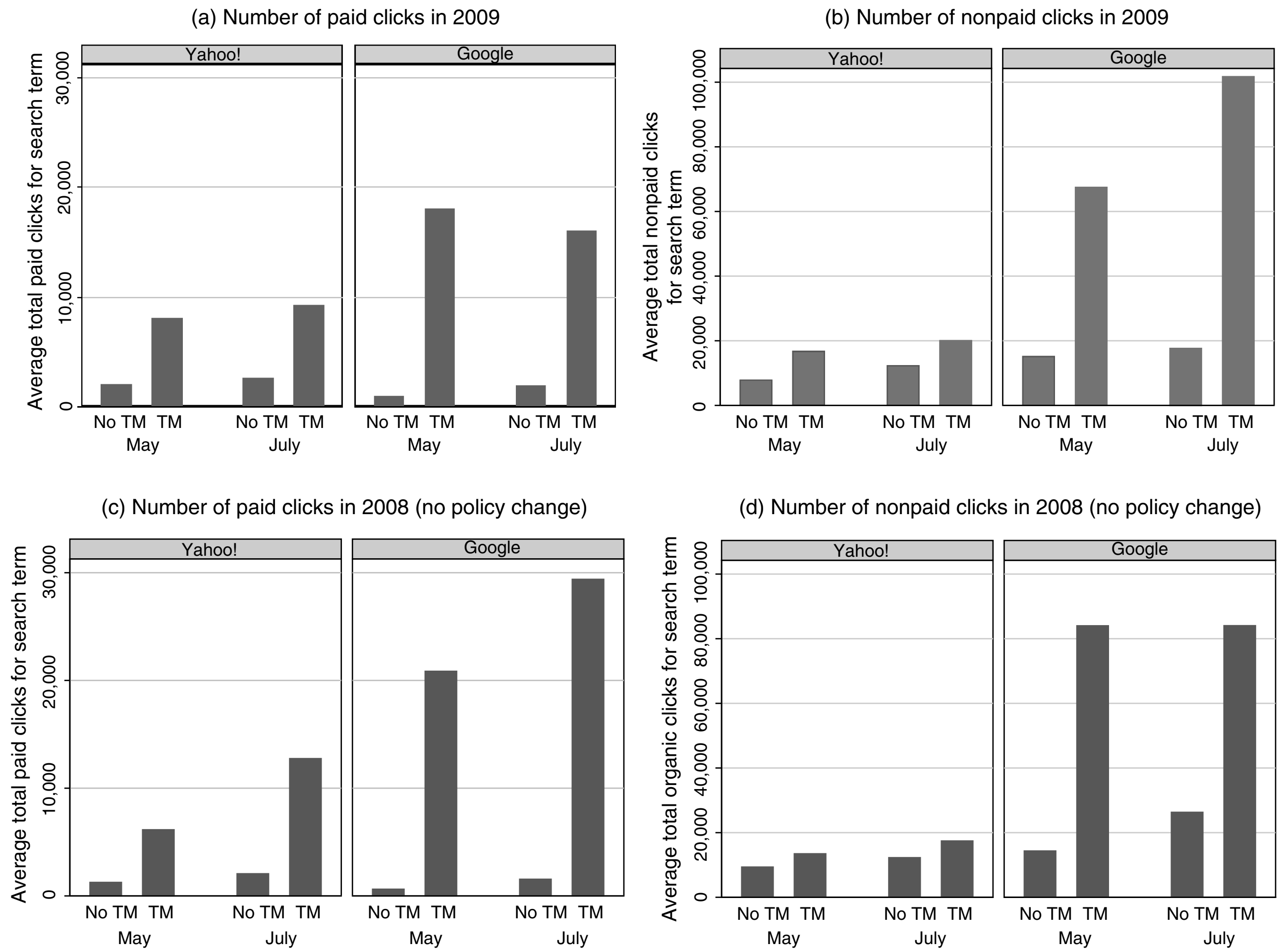
[5] The 2008 and 2009 editions of this list only reported the top 50 brands, but there appears to be substantial continuity across the years.

[6] The Marriott brands include Marriott, JW Marriott, Renaissance, Courtyard, Residence Inn, Fairfield Inn, TownePlace Suites, SpringHill Suites, Marriott Vacation Club, Ritz-Carlton, Marriott ExecuStay, and Marriott Executive Apartments. InterContinental Hotels Group's brands include Holiday Inn, Crowne Plaza, and InterContinental Hotels.

[7] We removed websites where the user was evidently not looking for information about hotels. For example, we removed results for searches containing "Hilton" that were Web pages for celebrity gossip magazines and video-sharing websites related to the celebrity Paris Hilton.

[8] Table B.1 in Appendix B records the total number of clicks by search term and the proportion of those clicks that were paid over the period we studied for each of the hotel websites.

Figure 2 How the Number of Clicks an Average Website Received Changed on Google and Yahoo!



Note. TM, trademark.

observe the number of paid and nonpaid clicks that Hilton.com receives in a month from people who use the search term "Hilton" on Yahoo!. There are 795 observed website and search term combinations for each search engine in each month. The bottom panel of Table 1 describes our data at this level.

4.2. Univariate Analysis

Figures 2(a) and 2(b) compare the paid and nonpaid clicks for each search term on Yahoo! and Google before and after the policy change (May and July 2009).[9] Two patterns are apparent. First, as hoteliers feared, paid clicks fell for trademark holders after the policy change on Google relative to Yahoo!. At the same time, however, a large increase occurred in nonpaid clicks for trademark holders. The small gains in paid clicks for the non-trademark holder sites do not appear to be significantly different from the patterns on Yahoo!.

To check that the variation was not simply seasonal, we collected similar data for 2008. Figures 2(c) and 2(d) show the results. Reassuringly, there was neither an upward shift in "nonpaid" clicks nor a downward shift in "paid" clicks on Google for trademark holders for similar months in the previous year. Instead, the general trend in paid clicks appeared to be upward (perhaps owing to a larger number of summer bookings) on both Yahoo! and Google, with little change in nonpaid clicks.

4.3. Empirical Analysis

We formalize the insights of Figure 2 in an econometric framework. For each website i that is potentially reached by consumers who search trademarked brand name j on search engine k in month t, we model the number of clicks as

$$clicks_{ijkt} = +\beta_1 TMHolder_{ij} \times PostChange_t \times Google_k + \beta_2 TMHolder_{ij} \times PostChange_t$$

[9] For simplicity, we look only at May and July 2009, the months surrounding the policy change. For completeness, we report the full monthly analysis in Figure B.1 in Appendix B.

$$+\beta_3 PostChange_t \times Google_k + \beta_4 TMHolder_{ij} \times PostChange_t + \beta_5 PostChange_t + \beta_6 month_t + \gamma_{ijk} + \epsilon_{ijk}.$$

The variable $TMHolder_{ij}$ is an indicator variable that equals 1 if website i is the trademark holder for hotel brand j and 0 otherwise. $Google_k$ is an indicator variable that equals 1 if the search engine is Google and 0 if the search engine is Yahoo!. $PostChange_t$ is an indicator variable that equals 1 if the month of the search occurs after June 15, 2009 and 0 if it occurs before. The vector γ_{ijk} includes fixed effects at the search engine–search term–website level. These fixed effects are collinear with the main effects of $TMHolder_{ij}$, $Google_k$, and $TMHolder_{ij} \times Google_k$, which are consequently omitted. The variable $month_t$ is an indicator variable for whether or not it is the month of May in the pretest period.[10] We estimate this model using ordinary least squares. We cluster standard errors at the search engine–search term–website level.[11]

Table 2 presents the results of this specification. Column (1) presents the estimates for the number of nonpaid clicks, which is our key variable of interest. As explained in §3, changes in the distinctiveness of paid searches may have potential spillover effects to the main results. The positive and significant coefficient estimate of 13,431.6 for $PostChange_t \times Google_k \times TMHolder_{ij}$ suggests a large increase in nonpaid clicks by users who directly navigated to the trademark holders' websites through the main search results after the change in policy on Google (relative to Yahoo!). This supports the theory that growing indistinctness of paid ads encourage users to navigate simply to the main nonpaid listing.

In column (2) of Table 2, we display results for the change in number of clicks on paid links. The (marginally) significant coefficient estimate for $PostChange_t \times Google_k \times TMHolder_{ij}$ suggests that after the policy change, trademark holder websites experienced a decrease of around 3,269 paid clicks on Google compared with Yahoo!. The result is as expected and follows conventional legal wisdom about the negative effects of permitting trademark dilution on an advertising message. When other paid ads could use the trademark, the trademark holder's ad was less distinctive and attracted fewer clicks. However, a comparison of columns (1) and (2) suggests that the decrease in paid clicks was outweighed fourfold by the increase in nonpaid clicks. Column (3) evaluates the effect of the policy change on total clicks to the website. The policy change was associated with a net increase of 10,162.7 in the number of monthly visits to the direct channel website. The lower-order interactions are insignificant in all three columns.

Table 2 Trademark Holders Lose Paid Clicks but Gain Nonpaid Clicks After the Policy Change

	(1) Nonpaid clicks	(2) Paid clicks	(3) Total clicks
PostChange × Google × TMHolder	13,431.6***	−3,269.0*	10,162.7***
	(3,635.5)	(1,744.1)	(2,997.9)
PostChange × Google	−3.908	18.56	14.65
	(78.91)	(44.08)	(92.36)
PostChange × TMHolder	−454.4	73.99	−380.4
	(893.9)	(671.5)	(1,078.8)
PostChange	148.7	14.48	163.2
	(94.53)	(46.44)	(110.7)
May indicator	6.184	−34.46	−28.28
	(159.2)	(68.68)	(186.8)
Search engine–search term–website controls	Yes	Yes	Yes
Observations	6,360	6,360	6,360
R-squared	0.176	0.154	0.179

Notes. Ordinary least squares estimates are shown. An observation is the number of clicks for a website in a month for searches using a specific trademarked term on a specific search engine. Data are from April, May, July, and August 2009. *Google × TMHolder*, *Google*, and *TMHolder* are dropped because of their collinearity with the search engine–search term–website fixed effects. Standard errors are clustered at search-term level.
$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

We then reestimate the model using a semi-log (log-linear) specification (see Table 3). We use a semi-log specification because it can be interpreted in terms of percentage changes to address the concern that our results might be driven by the difference in the absolute level of clicks between Google and Yahoo! (as observed in Figures 2(a) and 2(b)) or by extreme values. We estimate the semi-log specification using the generalized estimating equation framework (Mullahy 1999, Manning and Mullahy 2001). The logarithmic transformation inherent in this specification means that the results can be interpreted as a percentage change. These results suggest that the number of nonpaid clicks increased by 42% after the change in policy for trademark holders on Google and that, relatively, total clicks increased by 26%. The decrease in paid clicks for trademark holders on Google after the policy change is no longer significant, though the point estimate is large. In this specification, the coefficient on the indicator $PostChange_t$ is significant and positive, as one might expect demand for hotel

[10] We are limited from estimating a specification with a full set of monthly fixed effects as a result of collinearity with the $PostChange_t$ variable. The indicator variable for May tests for a preexisting trend in clicks before the policy change. The results are very similar if this indicator variable is omitted.

[11] In Table B.4 in Appendix B, we report results for a specification where we collapse the data into pre-policy and post-policy totals. This is an alternative method to clustering for addressing the concerns expressed by Bertrand et al. (2004) about using multiple-month data for policy evaluation. The results are similar, though the larger point estimates reflect the conflation of the two months.

Table 3 Log Specification: Trademark Holders Lose Paid Clicks but Gain Nonpaid Clicks After the Policy Change

	(1) Nonpaid clicks	(2) Paid clicks	(3) Total clicks
PostChange × *Google* × *TMHolder*	0.419***	−0.673	0.262**
	(0.132)	(0.493)	(0.122)
PostChange × *Google*	−0.112	0.326	−0.0747
	(0.0897)	(0.452)	(0.09)
PostChange × *TMHolder*	−0.269**	−0.218	−0.246**
	(0.113)	(0.250)	(0.0985)
PostChange	0.250***	0.207	0.234***
	(0.0757)	(0.219)	(0.0741)
May indicator	0.0229	−0.0628	0.00346
	(0.0563)	(0.0814)	(0.0535)
Search engine–search term–website controls	Yes	Yes	Yes
Observations	6,360	6,360	6,360
R-squared	0.178	0.173	0.188

Notes. Log-linear specification. An observation is the number of clicks for a website in a month for searches using a specific trademarked term on a specific search engine. Data are from April, May, July, and August 2009. *Google* × *TMHolder*, *Google*, and *TMHolder* are dropped because of their collinearity with the search engine–search term–website fixed effects. The generalized estimating equation estimates implying population-averaged effects rather than standard fixed effects. Standard errors are clustered at search-term level.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

rooms to increase during the summer months. The interaction $PostChange_t \times TMHolder_{ij}$, however, is negative and significant, which nullifies the positive effect of $PostChange_t$ and suggests no such increase for trademark holders' sites. This may reflect that the increase in clicks for summer travel was driven by leisure customers who may be more likely than business travelers to click on third-party resellers.

4.4. Robustness Checks

The results in Table 2 suggest that trademark holders actually received more clicks after the change in trademark policy. This goes against the conventional legal wisdom, so we checked the robustness and plausibility of our results in multiple ways. We discuss these checks in this section.

4.4.1. Control Group Checks. To be a valid control group, Yahoo! users must behave similarly to Google users in the absence of a policy change. We control for static differences between Yahoo! and Google, but a concern may be that the composition of users may be changing in a way that could distort our results. This process is sometimes referred to as *maturation* (Cook and Campbell 1979). This would be particularly problematic if the composition of Google users shifted toward groups of people who were more likely to simply use search engines as navigation tools and not click on ads, relative to Yahoo! users. To investigate this, we collected data from Experian Hitwise on the demographic profile of Google search and Yahoo! search users in the period we study. Table B.2 in Appendix B indicates that the income and age distribution of Google and Yahoo! users appears relatively similar and remains similar over the period we study.[12] Yahoo! has slightly more female users than Google, but this pattern did not change over the period we study. Table B.3 in Appendix B also shows that no other interface or operational changes occurred on either Yahoo! or Google.

4.4.2. Falsification Checks. We have already shown that there was no similar trend in 2008 for Google relative to Yahoo! (see Figures 2(c) and 2(d)). However, there is still the possibility of time-varying unobserved factors, or history (Cook and Campbell 1979), that were specific to 2009. For instance, perhaps Google did not publicly report a change in the search engine's algorithm, which led to hotel websites being highlighted more within the main results. To check for such possibilities, we conducted two "falsification checks."

In the first falsification check, we looked at a set of trademark holder clicks that were not affected by Google's policy change, and we examine whether they exhibited a similar pattern to that displayed in Table 2. We looked specifically at searches where consumers navigated to a trademark holder's website after searching using a competitor's trademark. Such searches were not affected by the policy change because Google only permitted advertisers who sold the specific brand to use the trademark in their ad copy. For example, Hilton could not use "Marriott" in its ad copy. If our results capture a general increase in consumer clicks to trademark holders' nonpaid links in the summer of 2009 on Google relative to Yahoo!, then these estimates should show a similar decrease in paid search activity and an increase in nonpaid searches. However, as reported in Table B.5 in Appendix B, all estimates are insignificant. This suggests that no global pattern persisted whereby customers searching for trademarks were more likely to visit brand-name sites using nonpaid links from the main search results on Google compared with Yahoo!.

As a further falsification test, we also checked whether any such effect existed for a hotel brand that explicitly forbade third-party sellers from using its trademark in a search. We used data on searches involving such brands that we had excluded from our main data set reported in Table 1. Table B.6 in Appendix B displays the results for the subset of brands that did appear successful in restricting their third-party sellers from advertising next

[12] We also checked that our results held if we only looked at searches that used only the trademarks, which helps us rule out time-varying heterogeneity in the nature of search terms used.

to their trademarks. As expected, the coefficient for $PostChange_t \times Google \times TMHolderSite_{ij}$ is insignificant.

4.4.3. Replication Using Different Control Group. To make sure that our results were robust to using only "within-Google" variation in behavior, we collected further data on the search behavior among people seeking hotels using generic, nonbranded search terms. Specifically, we collected data on the search outcomes of Google users who did not search for a brand but instead searched for a hotel in a specific geographic destination, e.g., someone who searched for "Atlanta Hotel" or "Atlanta Hotels." The idea is that such searchers on Google who are investigating booking a hotel but are focusing on location rather than brand should be subject to similar unobserved time-varying shocks and impulses. We collected this kind of search data for the top 10 most populous metropolitan statistical areas in the United States.[13] Because these are generic searches and city names are not subject to trademark restrictions, these types of searches were not affected by the policy change.

We then analyzed whether the trademark searches enjoyed a similar increase in clicks relative to these non-trademark searches. If there was no difference, this might suggest that our result simply reflects a shift in preferences of Google users seeking travel information toward clicking on the top main search result rather than on paid search results in the period we study. Table 4 displays our results for this new data sample. In this specification, the new indicator variable $TrademarkSearch_j$ is 1 when the search was conducted using a trademark and is 0 if the searcher used a geographical term. Even with using variation only among searchers on Google seeking hotel information, the positive coefficient for $PostChange_t \times TMHolder_{ij} \times TrademarkSearch_j$ suggests a sizable increase in the number of nonpaid clicks for branded websites in the main results for the trademark searches relative to non-trademark searches associated with the timing of the policy change. Because the regression uses a different data set, the absolute numbers cannot be directly compared to Table 4.[14]

4.5. Magnitude of the Spillover Effects

The coefficient size suggested by these log results is large, with an overall effect size of 26%. In this section, we investigate how long an effect of this size persisted and how the size of the effect varied across websites.

[13] These are New York, Chicago, Los Angeles, Dallas, Houston, Miami, Atlanta, Washington DC, Philadelphia, and Boston.

[14] The log estimates reported in the online technical appendix (available at http://dx.doi.org/10.1287/mksc.1120.0724) suggest a slightly larger positive effect proportionally for nonpaid clicks and a larger negative effect proportionally for nonpaid clicks when we analyze only within-Google variation after the policy change.

Table 4 Comparison Between Trademark Name Searches and Generic Searches on Google Only After Change in Policy

	(1) Nonpaid clicks	(2) Paid clicks	(3) Total clicks
PostChange × *TMHolder* × *TrademarkSearch*	7,885.6***	−2,370.8**	5,514.9***
	(2,068.7)	(1,148.8)	(1,627.8)
PostChange	−423.2*	−134.3	−557.5*
	(239.3)	(104.7)	(291.5)
Search term–website controls	Yes	Yes	Yes
Month controls	Yes	Yes	Yes
Observations	4,243	4,243	4,243
R-squared	0.0195	0.0254	0.0201

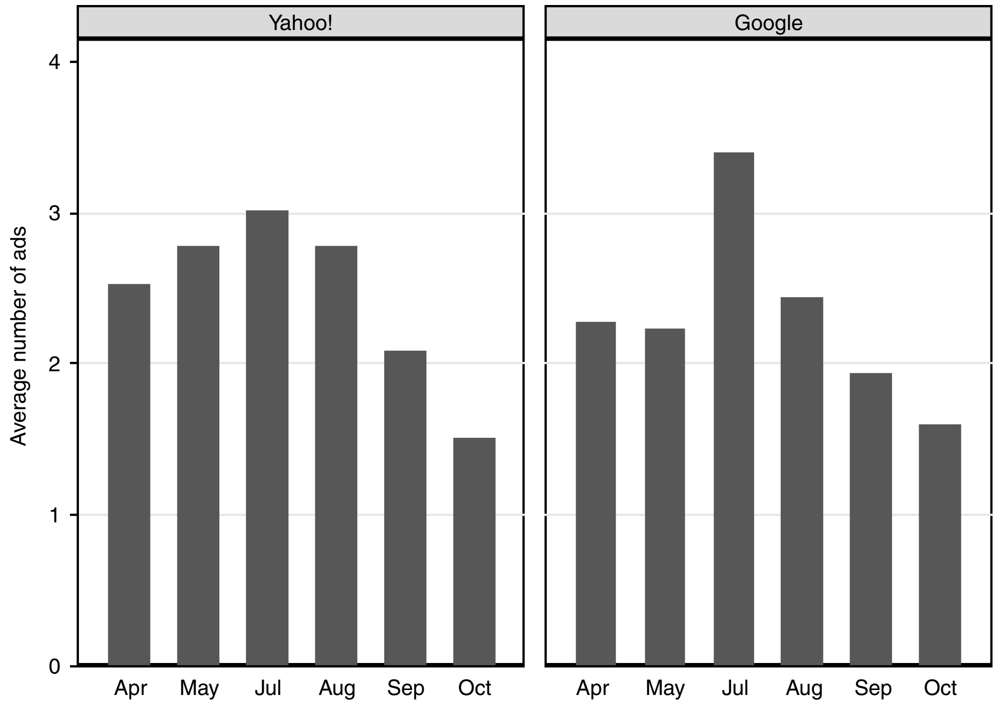
Notes. Ordinary least squares estimates are shown. An observation is the number of clicks for a website in a month for searches using a trademarked search term or a geographical (top 10 by population U.S. city) hotel search term on Google. Data are from April, May, July, and August 2009. Lower-order interactions for *TrademarkSearch* and *TMHolder* with *PostChange* are not separately identified for nonpaid clicks because the geographical searches did not produce trademark holders' websites as primary search results. Standard errors are clustered at search-term level.
$^*p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

4.5.1. Persistence of the Spillover Effects. It seems unlikely that, in equilibrium, third-party resellers would continue to highlight trademarks in their ads after it becomes evident that such a strategy is not that effective. To study this, we collected data over time on the number of ads that appeared for each branded search. We measured the number of ads by how many separate websites received paid clicks for those search terms on each search engine. Figure 3 shows how the number of ads evolved on Google compared to Yahoo! over the months surrounding the policy change. It is clear that an increase in the number of ads by non-trademark holders occurred after the policy change on the Google search engine in July 2009 as advertisers took advantage of the new opportunity to highlight a trademark. However, no such significant change occurred on Yahoo!.

Figure 3 also shows that the increase in the number of ads displayed for trademark searches on Google after the policy change relative to Yahoo! fell after the initial months. This is not surprising; as Figure 2(a) suggests, only small gains in paid clicks for non-trademark holders occured after the policy change. Many advertisers on search engines rely on automated systems that allocate their expenditures to advertising campaigns that attract the most click-throughs, as search engines' pricing algorithms penalize advertisers who do not attract sufficient clicks. Therefore, advertisers tend not to continue to run ads that do not attract significant clicks.

Therefore, large gains in nonpaid clicks to trademark holders might not have been sustained if third parties pulled the inefficient ads. To examine this, Table 5 repeats the analysis of Table 2 but includes data from September and October 2009. It compares

Figure 3 How the Average Number of Ads for Each Search Term Changed on Google and Yahoo! Across Multiple Months



the effect for July and August 2009 (captured by *PostChange*) with the incremental shift in the effect in September and October 2009 (labeled as "long-term"). The new long-term interaction is captured by an indicator variable, *Long-Term*, which is equal to 1 if it was September or October 2009. *PostChange* continues to indicate whether the month occurs after the policy change. The coefficient for *Long-Term* × *Google* × *TMHolder* is negative for nonpaid clicks, though it is only marginally significant in the linear specification and insignificant in the log specification. This suggests that the short-run effect of spillover effects from other retailers' ads on nonpaid clicks decreased after August 2009 by 4,380, though a sizable effect still exists. Given the reduction in number of competitor ads and the results of Table 6, the positive effect for the direct channel is mediated by the number of third-party seller ads (presumably) highlighting a trademark. The negative coefficient for *Long-Term* × *Google* × *TMHolder* for paid clicks does suggest that the reduction in paid clicks persisted, though again, this is marginally significant in the linear specification and is not significant in the log specification.

4.5.2. Search Engine Motivation. A remaining question is why Google would allow the use of trademarks in paid search ads if it encouraged nonpaid clicks. Because we cannot obtain pricing data, we cannot calculate the full equilibrium effect on revenues. We believe that the answer lies in the increase in prices that the advertisers paid for each of these bids. As discussed by Goldfarb and Tucker (2011), there is huge variation in the prices of ads on search engines. Ultimately, without competition, a click would only be worth around $0.10, reflecting Google's minimum bid for a click. However, with competition, and given the sealed-bid second-price auction mechanism used to price each click, the prices would rise rapidly. For example, Pfanner (2010) quotes Interflora as saying that when Google allowed other firms to bid on trademarks in the United Kingdom, the cost of buying its own name rose from 3 to 4 cents per click to as much as 42 cents per click, costing an additional $750,000 in the first year. Therefore, Google may potentially have strategically decided to trade off fewer clicks if this led to an increase in revenues through higher prices.

4.6. Mechanism

As described in §3, when consumers search a trademarked brand name, they are likely intending to navigate to the brand's website. The paid search results have to offer something compelling and distinct to distract consumers from their original purpose. However, if all ads focus around the same trademark, then they become less distinct and are more likely to be perceived as advertising clutter. We use our data to provide evidence for this mechanism in two ways. First, we show that the effect is greater when there is a larger number of ads contributing to the clutter. Second, we show that the negative effect is greater for non-trademark holders' ads that would otherwise have more strikingly different value propositions from the direct channel.

Table 5 The Spillover Effects Decreased in the Long Run

	(1) Nonpaid clicks	(2) Paid clicks	(3) Total clicks
PostChange × *Google* × *TMHolder*	15,917.6***	−1,203.1	14,714.4***
	(4,120.9)	(1,573.6)	(4,139.3)
Long-Term × *Google* × *TMHolder*	−4,340.8*	−2,196.4*	−6,537.1**
	(2,345.6)	(1,239.3)	(2,893.6)
PostChange	343.1***	56.56**	399.7***
	(85.05)	(25.57)	(89.20)
PostChange × *Google*	−188.7	14.97	−173.8
	(142.3)	(35.77)	(147.5)
PostChange × *TMHolder*	1,900.2**	1,298.9**	3,199.1**
	(890.5)	(602.9)	(1,261.2)
Long-Term	−317.1***	−73.84**	−391.0***
	(76.36)	(34.79)	(85.60)
Long-Term × *Google*	56.24	37.75	93.98
	(142.3)	(32.18)	(147.2)
Long-Term × *TMHolder*	−2,582.0***	−1,128.3*	−3,710.2***
	(966.5)	(612.7)	(1,138.0)
May indicator	−298.5**	−45.95	−344.5**
	(124.1)	(66.67)	(148.0)
Search engine–search term–website controls	Yes	Yes	Yes
Observations	11,130	11,130	11,130
R-squared	0.0744	0.173	0.0871

Notes. Ordinary least squares estimates are shown. An observation is the number of clicks for a website in a month for searches using a specific trademarked term on a specific search engine. Data are from April, May, July, August, September, and October 2009. Pre-policy months are April and May 2009. Long-term effect captures the incremental change in *PostChange* in September and October 2009. *Google* × *TMHolder*, *Google*, and *TMHolder* are dropped because of their collinearity with the search engine–search term–website fixed effects. Standard errors are clustered at search-term level.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

4.6.1. The Spillover Effects Increase in the Quantity of Advertising Clutter. A greater number of ads increases the perception of advertising clutter (Danaher et al. 2008, Pieters et al. 2007). Therefore, we would expect the effect of exposure to increase with the number of ads displayed by third-party sellers after the policy change.

Table 6 displays a specification that allows the effect of the policy change to vary with the number of ads displayed by third-party sellers. This should pick up the variation in the number of third-party seller ads observed in Figure 3. The key effect is captured by the four-way interaction $PostChange_t \times Google_k \times TMHolder_{ij} \times No.Comp.UsingTM_{ijtk}$. The positive coefficient for $PostChange_t \times Google_k \times TMHolder_{ij} \times No.Comp.UsingTM_{ijtk}$ for nonpaid clicks suggests that, as expected, the positive incremental effect of the policy change increased in the number of third-party reseller ads. Similarly, the negative coefficient for $PostChange_t \times Google_k \times TMHolder \times No.Comp.UsingTM_{ijtk}$ for paid clicks suggests that the negative effect of the policy change for paid clicks indeed increased in the number of third-party reseller ads.[15]

Table 6 Changes in Paid Search and Nonpaid Search by Number of Competitors' Ads

	(1) Nonpaid clicks	(2) Paid clicks	(3) Total clicks
PostChange × *Google* × *TMHolder* × *No.Comp.UsingTM*	5,719.9***	−1,961.3***	3,758.6***
	(560.1)	(225.9)	(577.5)
PostChange × *Google* × *TMHolder*	6,256.7***	−891.5	5,365.3***
	(1,412.7)	(569.8)	(1,456.6)
PostChange × *Google* × *No.Comp.UsingTM*	−29.66	16.50	−13.16
	(173.4)	(69.93)	(178.8)
No.Comp.UsingTM	−52.46	7.081	−45.37
	(164.0)	(66.16)	(169.1)
PostChange × *No.Comp.UsingTM*	40.53	0.133	40.67
	(117.8)	(47.53)	(121.5)
Google × *No.Comp.UsingTM*	34.55	−8.828	25.72
	(216.5)	(87.31)	(223.2)
TMHolder × *No.Comp.UsingTM*	972.8	−464.0*	508.8
	(623.1)	(251.3)	(642.5)
PostChange × *TMHolder* × *No.Comp.UsingTM*	253.7	34.16	287.9
	(382.9)	(154.4)	(394.8)
Google × *TMHolder* × *No.Comp.UsingTM*	−1,484.5*	1,747.9***	263.5
	(836.5)	(337.4)	(862.5)
PostChange	62.29	8.865	71.15
	(397.7)	(160.4)	(410.1)
PostChange × *Google*	69.22	−9.286	59.94
	(506.3)	(204.2)	(522.0)
PostChange × *TMHolder*	−1,153.0	128.4	−1,024.6
	(1,044.7)	(421.4)	(1,077.2)
May indicator	6.431	−39.50	−33.08
	(236.6)	(95.44)	(244.0)
Search engine–search term–website controls	Yes	Yes	Yes
Observations	6,360	6,360	6,360
R-squared	0.291	0.0160	0.287

Notes. Ordinary least squares estimates are shown. An observation is the number of clicks for a website in a month for searches using a specific trademarked term on a specific search engine. Data are from April, May, July, and August 2009. *Google* × *TMHolder*, *Google*, and *TMHolder* are dropped because of their collinearity with the search engine–search term–website fixed effects. Standard errors are clustered at search-term level.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 6 suggests that the effect of the policy change for trademark holders was indeed moderated by the number of non-trademark holder ads that appeared after the policy change on Google.

4.6.2. Negative Spillovers for Third Parties with the Most Distinct Message Pre-Policy. We then investigated whether the negative effects of this policy were felt hardest by websites that potentially could have put forward the most distinctive advertising

[15] We also estimated this specification to explore how the policy affected click outcomes for third-party resellers, but our estimates were imprecise.

Table 7 Websites That Focused on Offering Discounted Prices Received Fewer Paid Clicks After the Policy Change

	(1) Nonpaid clicks	(2) Paid clicks	(3) Total clicks
PostChange × *Google* × *TMHolder*	13,425.4***	−3,318.3*	10,107.2***
	(3,636.3)	(1,744.5)	(2,998.8)
PostChange × *Google* × *BargainSite*	−43.91	−351.9**	−395.8**
	(104.8)	(149.0)	(183.1)
PostChange × *Google*	2.242	67.85	70.09
	(91.30)	(45.52)	(104.5)
PostChange × *TMHolder*	−478.1	117.7	−360.4
	(894.5)	(671.5)	(1,079.3)
PostChange	172.4*	−29.21	143.2
	(98.92)	(44.00)	(113.6)
PostChange × *BargainSite*	−169.2***	311.9**	142.7
	(55.59)	(143.2)	(153.2)
May indicator	6.184	−34.46	−28.28
	(159.2)	(68.69)	(186.8)
Search engine–search term–website controls	Yes	Yes	Yes
Observations	6,360	6,360	6,360
R-squared	0.176	0.152	0.179

Notes. Ordinary least squares estimates are shown. An observation is the number of clicks for a website in a month for searches using a specific trademarked term on a specific search engine. Data are from April, May, July, and August 2009. *Google* × *TMHolder*, *Google* × *BargainSite*, *Google*, *TMHolder*, and *BargainSite* are dropped because of their collinearity with the search engine–search term–website fixed effects. Standard errors are clustered at search-term level.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

messages. To explore this, we identified websites that had a very salient "low-price" brand messages. If these websites changed the text of their ads to reflect the trademarks, they might have lost the opportunity to make their offerings distinct, and their ads may have been more likely to be viewed as advertising clutter and ignored. As documented by Anderson et al. (2010b), such price-oriented marketing messages online often halt consumer searches as consumers focus on the "cheap" offerings.

We reran the specification in Table 2 with a new interaction for non-trademark holder sites whose websites' URLs contain the word "cheap," "bargains," "discounts," or "deal." This is represented by the new indicator variable *BargainSite*, which is equal to 1 if the URL contains one of those words and 0 otherwise. No trademark holders' websites were classified as bargain sites. This allowed us to distinguish third-party sellers that are price focused. As shown in Table 7, the negative coefficient for $PostChange_t \times Google_k \times BargainSite_i$ for paid search clicks suggests that these paid clicks decreased for these "bargain" websites relative to third-party websites on Google after the policy change.[16] This occurs despite the fact that the coefficient for $PostChange_t \times BargainSite_i$ is positive, which suggests a time trend, as one might expect, for more clicks on such sites during the summer months. Websites that were most likely to have the largest shift in their advertising emphasis if they emphasized trademarks suffered the most from the change of policy. Of course, we do not observe how advertising content changed as a result of the policy change, so this is somewhat speculative.[17] To obtain more conclusive evidence with full knowledge of what ads are being shown, we turned to the lab.

5. Lab Experiment

Because the empirical results suggest a sizable positive effect that runs against conventional legal wisdom, we replicated our results and obtained direct behavioral evidence of the mechanism in the laboratory.

We conducted the lab experiment online and used Amazon Mechanical Turk to recruit 346 survey takers.[18] They were randomly allocated to one of six scenarios [(Baseline, Indistinct, Many ads) × (Trademarks, No trademarks)].[19] The stimuli for each of these conditions are presented in Figure 4. In the "Trademarks" conditions, the third-party ads displayed trademarks; in the "No trademarks" conditions, they did not.

In the "Baseline" conditions, we aim to replicate the main field experiment. In the "Indistinct" conditions, we changed the third-party ads so that they no longer mentioned their price advantage but instead said something nonspecific and indistinct about quality. In such scenarios there should be less of an effect of the introduction of trademarks because the ads will already be perceived as clutter. In the "Many ads" conditions, we changed the number of ads that respondents saw.[20] Pieters et al. (2007) argue that larger numbers of indistinct ads increase advertising clutter. Therefore, we expect the spillover effects to be largest in the condition with trademarks and more ads.

[16] There were very few nonpaid clicks for these bargain websites, which made precision difficult in a regression with nonpaid clicks as a dependent variable.

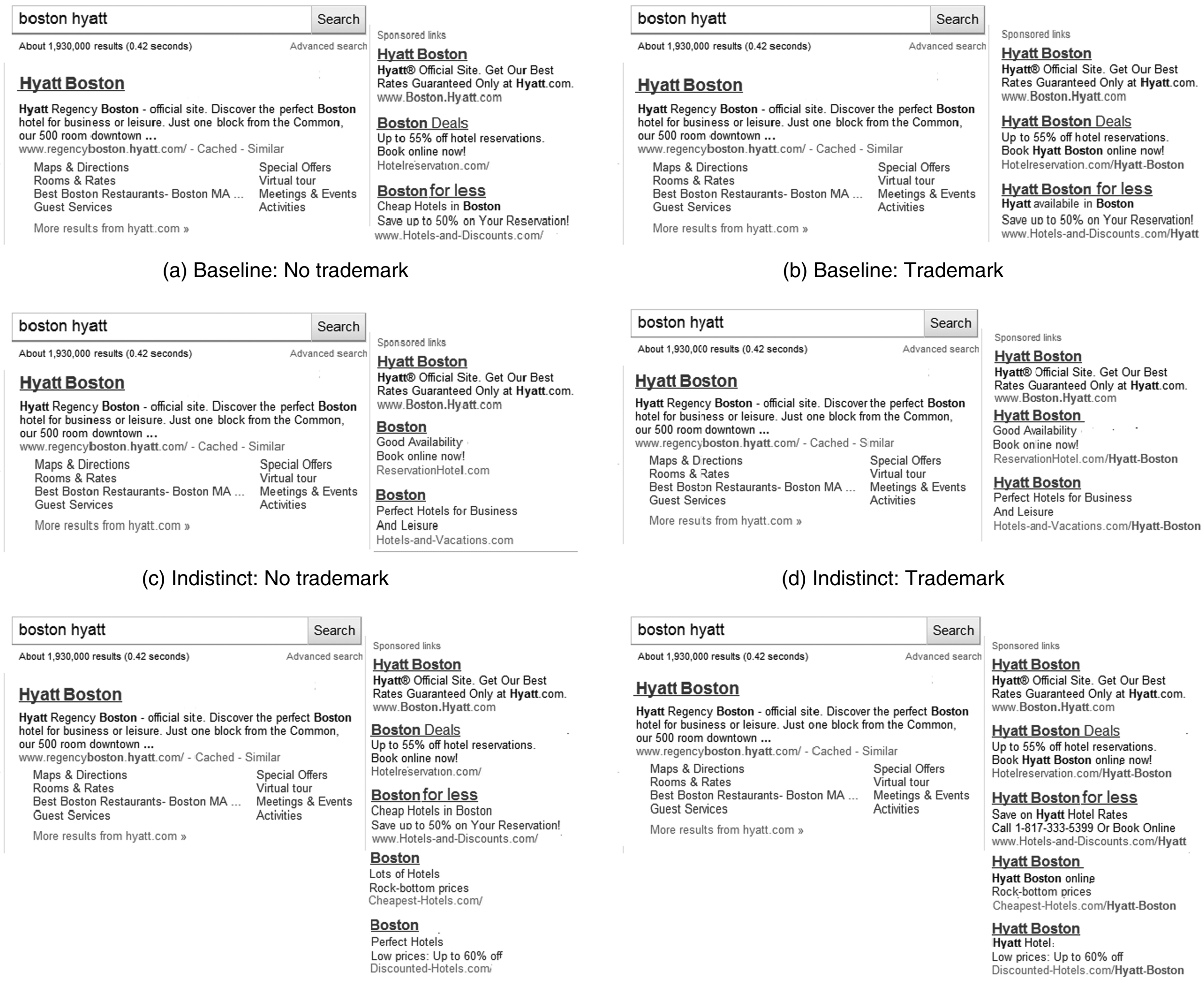
[17] It is also possible that the removal of ad messages emphasizing price reduced the relative perceptions of quality for the trademark holders' paid links. However, this is not consistent with the observed increase in the number of unpaid clicks.

[18] Ferraro (2008) suggests that Amazon Mechanical Turk respondents are more likely to be female, to be of South Asian descent, and to have a college degree relative to the representative American in the 2000 Census, but the respondents' typical characteristics also tend to be typical of heavy Internet users.

[19] We removed responses from 24 survey takers whose IP addresses suggested that they originated from the same address.

[20] Table 6 presents evidence that our results are moderated by the number of ads. However, in the field data, the number of ads was endogenous to the introduction of the policy, rendering it not a clean test.

Figure 4 Lab Experiment Stimuli



(a) Baseline: No trademark

(b) Baseline: Trademark

(c) Indistinct: No trademark

(d) Indistinct: Trademark

(e) Multiple ads: No trademark

(f) Multiple ads: Trademark

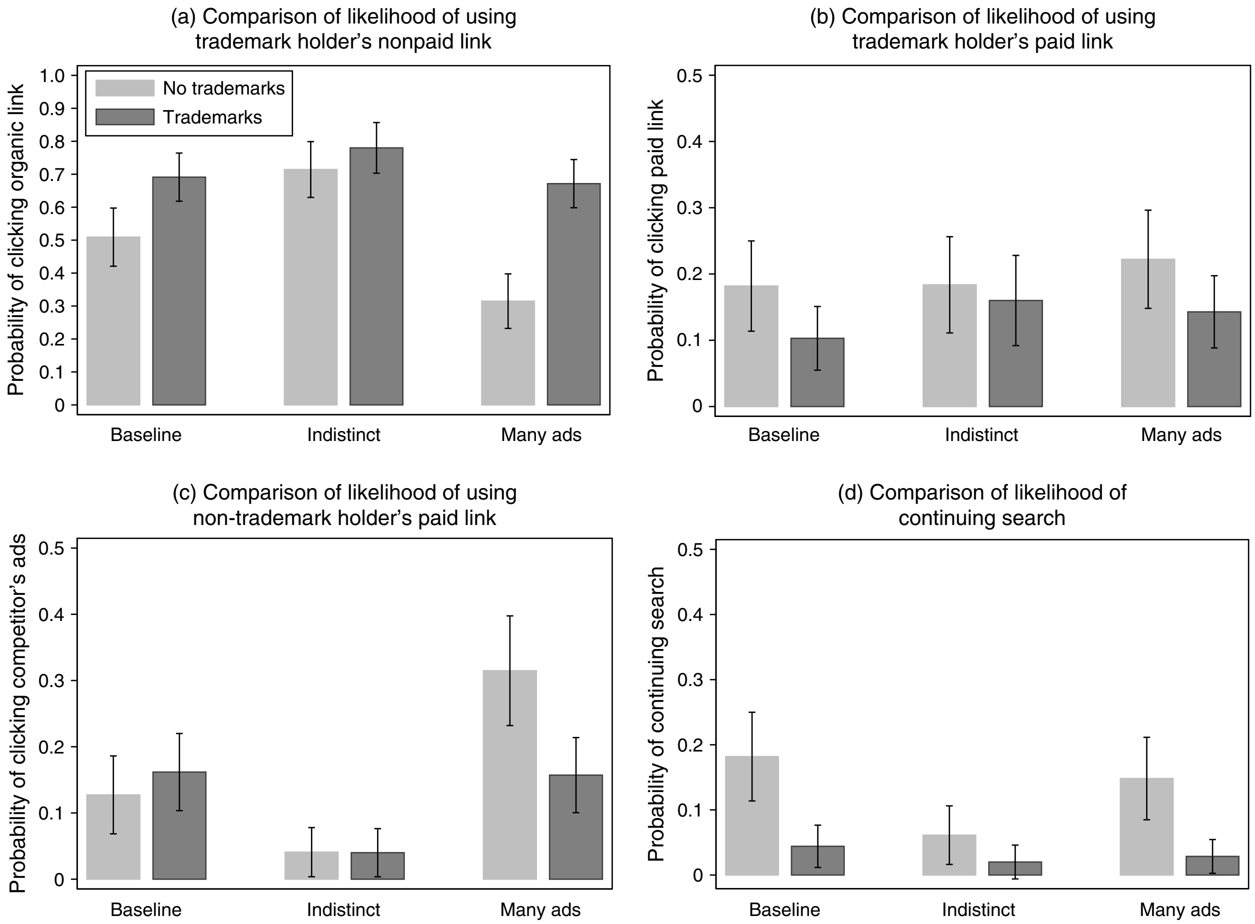
In each condition, above the screenshot, we stated, "Imagine you are trying to book a hotel room which you have to pay for. You use a search engine to search for 'Boston Hyatt' and see the following search result." We then asked respondents which option they would use to book their hotel room. The options were the trademark holder's nonpaid link, the trademark holder's paid link, the third parties' paid link, or continuing to search for further information.

Figure 5(a) presents the outcomes of the experiment for whether the respondents would use the trademark holder's nonpaid link to book their hotel in each of the conditions. In the Baseline scenario, a higher proportion of respondents said they would book a room using the main nonpaid link if trademarks were present (51% versus 69%; $t = 2.07$, p-value $= 0.04$). In the Indistinct scenario, as predicted, there was no change in the proportion of people who were prepared to use a third party's link to book their website (71% versus 78%; $t = 0.74$, p-value > 0.1). In the Multiple ads scenario, a higher proportion of respondents said they would book a hotel using the main nonpaid link (31% versus 67%; $t = 4.18$, p-value < 0.01) when trademarks are present. The difference is larger and more significant than in the Baseline scenario, where there were fewer listings.

We then examined the effects of the change on the likelihood of a respondent using the trademark holder's paid link. As illustrated by Figure 5(b), there was no significant difference across subjects who saw trademarks and those who did not in any of the scenarios. This insignificance echoes the lack of precision of the effect of the policy change on paid clicks for the trademark holder reported in §4.3.

Figure 5(c) summarizes the results for the proportion of people who would use a third party's link to book their room. In the Baseline and Indistinct scenarios, there was no significant difference, but in the Many ads scenario, there was a drop in the number of people who were prepared to use a third party's

Figure 5 Lab Experiment



link to book their website (31% versus 16%; $t = 2.10$, p-value $= 0.04$) when trademarks were present, which is suggestive that the combination of trademarks with multiple ads is particularly off-putting.

Figure 5(d) shows how the proportion of users who decided to continue searching changed in each of the scenarios. There was no significant change in the Indistinct scenario. However, in the Baseline and Many ads scenarios, the change in the proportion of people who chose to continue searching for other deals was smaller in the presence of trademarks (18.1% versus 4.4%; $t = 2.51$, p-value $= 0.013$ and 14% versus 2%; $t = 2.46$, p-value $= 0.015$, respectively).

Our experiment also allowed us to verify that the presence of trademarks increased perceptions of clutter. When asked, "Is this Web page cluttered?," more people indeed believed the page was cluttered when trademarks were present (59% versus 68%; $t = 2.09$, p-value $= 0.04$).

6. Implications

This paper explores how marketing outcomes are affected by the use of trademarks in ads by third-party sellers who compete with a firm's direct channel. We use data from a natural experiment where Google changed its policy to align with that of other major search engines by permitting the use of trademarks in ad copy. Our results suggest that, surprisingly, this policy change benefited trademark holders. Although trademark holders lost paid clicks, this decrease was outweighed by a fourfold increase in nonpaid clicks. We present evidence that shows when third-party sellers highlight brands in their ads, they reduce their sellers' ability to convey messages distinct from the other ads, such as offering lower prices. As a result, consumers are less likely to be diverted by paid ads and more likely to click on the main nonpaid link.

Firms have often tried to restrict third-party sellers contractually from competing with their direct channels in digital advertising. For example, in 2004, InterContinental Hotels required third-party distributors to agree not to bid on InterContinental's trademarks on search engines. InterContinental even severed relationships with Expedia for three years after it did not agree to these terms. In January 2010, Carnival Cruise Lines, Cunard Line, Holland America Line, Princess Cruises, and Seabourn Cruises threatened similarly harsh penalties for travel agencies that bid on trademarked terms (Jainchill 2010). Our results suggest, however, that such draconian action may be unnecessary. Instead, firms' direct channels may be beneficial if third parties feature their trademarks prominently in ads. When these third-party sellers focus on the focal brand in their advertising, they inadvertently encourage customers to purchase from the direct channel as their message, such as a low price, becomes less distinct. This finding, that by loosening their hold on intellectual property online firms can benefit from marketing spillovers, is echoed in the Chiou and Tucker (2012b) finding that replication of content by non-copyright holders can help promote the copyright holder's website. This implication, of course, rests on the assumption that, as happened in the case we study, an increase in trademark use by competitors in their advertising can lead to increased ad clutter (in terms of both the nature of ads and the number of rival ads).

More broadly, our results provide empirical evidence on the policy question of trademarks and search advertising. In the United States, the possibility of trademark infringement has been proposed by researchers such as Clemons and Madhani (2010–2011) as a major justification for the regulation of search engines. Many lawsuits have been filed in the United States over the use of trademarks in search advertising, and court decisions have been contradictory. Recently in Europe, two cases related to the hotel industry, *Hotels Méridien v. Google France* (2004) and *Accor v. Overture* (2004), resulted in search engines paying large fines for allowing competitors to advertise next to trademarks. These cases have led to attempts to clarify the law at the European level. Poiares Maduro, the Advocate General of the European Court of Justice, ruled that "Google has not committed a trademark infringement by allowing advertisers to select, in AdWords, search terms corresponding to trademarks" (Court of Justice of the European Union 2009, p. 2). However, in what is crucial for our study, the decision suggested that this exemption did not apply to the use of trademarks as *content* featured in ads.[21] It is precisely this use of trademarks in the content of ads that we study in this paper.

There are limitations to our findings. First, the policy change we study is confined to changes in the ability of a brand's partners to use a trademark in their ad copy on search engine ads. This makes it harder to draw conclusions about other potential trademark usage restrictions, such as restricting other firms from bidding on a competitor's brand trademark as a search term, or the effect of policies off-line. Second, we do not have data on the cost of paid searches before the policy change. The increase in the number of bidders on a particular search term that was occasioned by the policy change may have increased the cost per click for trademark holders in ways we cannot measure, so we do not know how this change affected search engine revenues. Third, we measure only the number of clicks each website receives—we cannot measure how the policy change affected reservations. Last, it is not clear how our results extend to other sectors of the economy where direct sales are less crucial to the brand owner's business model. These limitations notwithstanding, our empirical analysis does highlight an unexpected consequence of trademark usage in the digital age with significant implications for firms' online advertising strategies.

[21] See Court of Justice of the European Union (2009).

Electronic Companion
An electronic companion to this paper is available as part of the online version at http://dx.doi.org/10.1287/mksc.1120.0724.

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Appendix A. Announcement of Google Change in Policy
The following text was posted by Dan Friedman on the *Inside AdWords* blog (owned by Google), May 14, 2009 (http://adwords.blogspot.com/2009/05/update-to-us-ad-text-trademark-policy.html).

Update to U.S. Ad Text Trademark Policy
Imagine opening your Sunday paper and seeing ads from a large supermarket chain that didn't list actual products for sale; instead, they simply listed the categories of products available—offers like "Buy discount cola" and "Snacks on sale." The ads wouldn't be useful since you wouldn't know what products are actually being offered. For many categories of advertisers, this is the problem they have faced on Google for some time.

That is why, in an effort to improve ad quality and user experience, we are adjusting our trademark policy in the U.S. to allow some ads to use trademarks in the ad text. This change will bring Google's policy on trademark use in ad text more in line with the industry standard. Under certain criteria, you can use trademark terms in your ad text in the U.S. even if you don't own that trademark or have explicit approval from the trademark holder to use it. This change will help you to create more narrowly targeted ad text that highlights your specific inventory.

For example, under our old policy, a site that sells several brands of athletic shoes may not have been able to highlight the actual brands that they sell in their ad text. However, under our new policy, that advertiser can create specific ads for each of the brands that they sell. We believe that this change will help both our users and advertisers by reducing the number of overly generic ads that appear across our networks in the U.S.

Please note that this policy update will only apply to ads served in the U.S. on Google.com and to U.S. users on the Search and Content Networks. Also, while we will start accepting new ads that contain trademark terms as of 11 AM PDT on May 15th, those ads will not begin showing until June 15th.

If you have ads in your account which were previously disapproved for trademark policy and that comply with the new policy, you may submit those ads for re-review and eligible ads may begin showing in the U.S. starting June 15th. For instructions on editing your ad text, click here.

In order to help advertisers understand whether their landing pages meet our policy guidelines we've added some new functionality to our Search Based Keyword Tool. If you visit www.Google.com/sktool and enter your website URL, you may see a list of brands on the left side of the page if your site contains those brands. When you click on any of those brands you'll notice a column titled "Extracted from webpage." Those landing pages may be opportunities for you to show re-sale or informational ads.

We believe that this change will offer you the opportunity to provide users with more relevant information, choice and options while respecting the interests of trademark holders.

Appendix B. Further Empirical Tables and Data Description

Table B.1 Summary of Hotel Trademark Search Terms and the Associated Number of Clicks

No.	Brand	Beds	Total clicks	Percentage of clicks the advertiser paid for
1	Best Western	315,401	2,243,275	18
2	Hilton	172,605	7,736,176	14
3	Days Inn	151,438	2,142,488	14
4	Hampton Inn	138,481	3,059,937	16
5	Sheraton	135,900	2,466,953	21
6	Super 8	126,175	647,511	21
7	Comfort Inn	110,877	2,661,719	23
8	Ramada Inn	105,986	634,901	17
9	Motel 6	90,243	951,294	34
10	Radisson	90,080	1,039,602	18
11	Crowne Plaza	75,632	655,368	24
12	Quality Inn	72,054	991,570	28
13	Hyatt Regency	69,733	814,748	21
14	La Quinta Inn	61,570	545,764	27
15	Westin	54,200	1,330,296	24
16	Econo Lodge	49,679	114,342	19
17	Americas Best Value Inn	45,672	82,680	14
18	Embassy Suites	45,172	1,759,598	16
19	Howard Johnson	44,432	542,052	13
20	Hilton Garden Inn	41,669	876,008	11
21	Extended Stay America	40,434	430,036	19
22	Travelodge	37,468	315,035	4
23	Red Roof Inn	36,339	467,829	17
24	Comfort Suites	33,976	591,059	30
25	Country Inn and Suites	32,827	493,340	15
26	Sleep Inn	24,575	347,982	21[ab]
27	Clarion Hotel	23,945	170,833	25
28	Wyndham Hotels	22,582	46,348	3
29	Fairmont Hotels	22,407	60,876	26
30	Four Points by Sheraton	21,900	61,259	17
31	Homestead Studio Suites	21,141	64,608	15
32	Knights Inn	16,892	51,747	12
33	Grand Hyatt	16,429	213,530	14
34	Omni Hotels	14,384	19,291	8
35	Rodeway Inn	14,168	176,946	22
36	Candlewood Suites	14,149	418,425	20
37	Doubletree Hotel	14,149	484,530	15
38	Wingate by Wyndham	14,146	67,708	13
39	Drury Inn	14,000	41,716	7
40	Baymont Inn	12,377	241,617	17
41	Studio 6	9,385	21,654	7
42	Hawthorn Suites	8,735	40,519	14
43	Suburban Extended Stay	7,984	16,802	14
44	Park Plaza	7,197	8,128	46
45	ResortQuest	6,000	51,677	14
46	Millennium Hotel	5,041	47,321	27
47	Jameson Inn	5,000	44,667	0
48	Hyatt Place	3,794	375,352	26
49	Waldorf Astoria	3,780	41,397	25
50	Hilton Grand Vacation Club	3,740	29,703	2
51	Sandals Resorts	3,234	265,744	25
52	Hyatt Summerfield Suites	3,024	19,476	2
53	Peabody Hotel	1,773	217	100

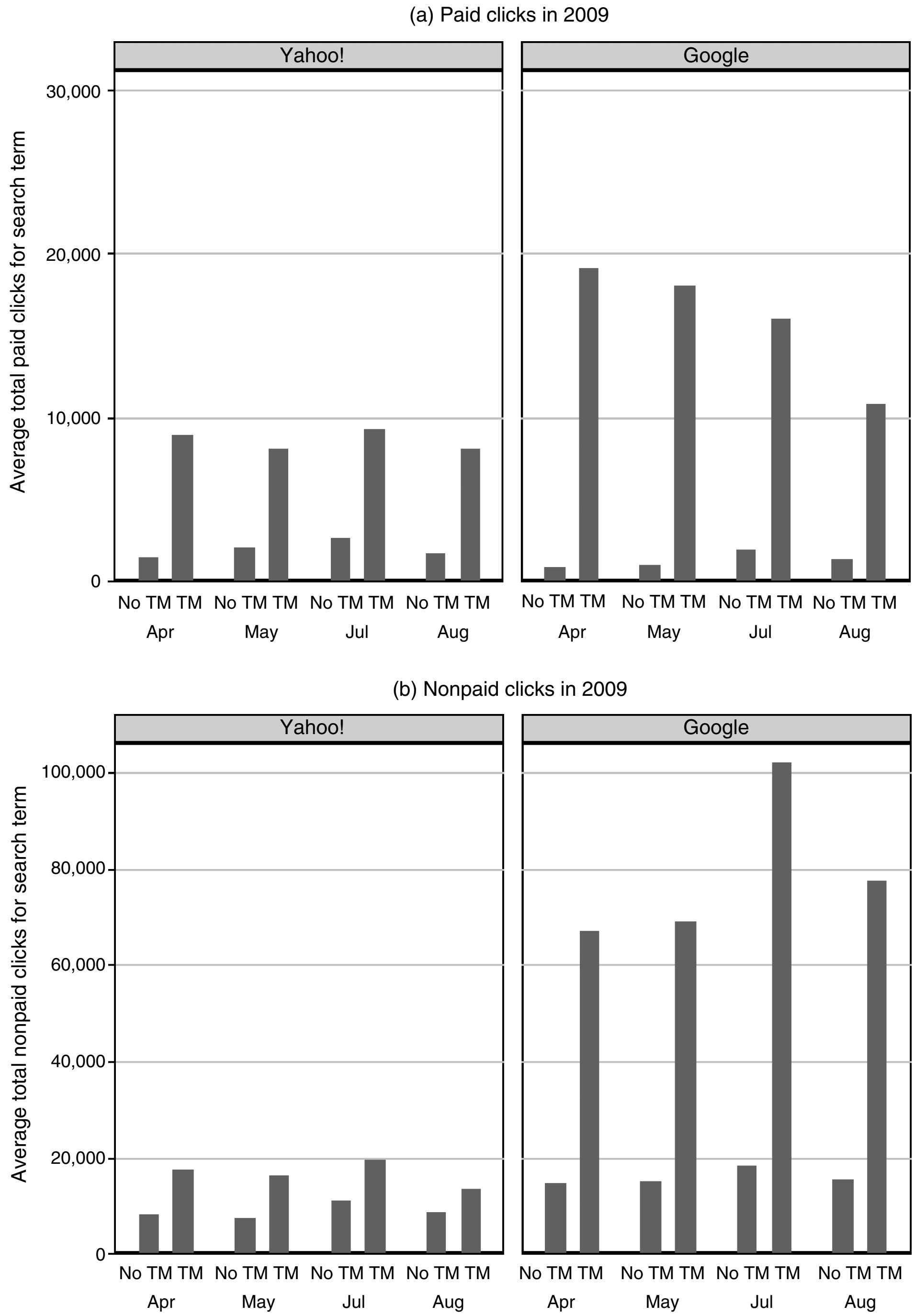
Notes. Sample consists of 53 hotel brands names ranked by number of beds. Total clicks calculated from April 2009 to August 2009. Number of beds from *Hotels* magazine "Top 300 Hotel Brands" (July 2007). We only have data on whether users visited a hotel website rather than whether or not they booked a hotel room through it. To understand how likely it is that a click led to a booking, we obtained separate data from Experian Hitwise, a company that also tracks the behavior of consumers on the Internet, about which websites people visited after visiting an accommodation website. Most people navigated to tangential sites, suggesting that they had completed their product search. However, it is evident that a certain amount of leakage occurred. For 22.5% of the time, consumers went to alternative accommodation websites; 9.6% of the time they went to alternative travel agencies, and 6.4% of the time they returned to a search engine.

Table B.2 Comparison of Demographics of Yahoo! and Google Users

	April–May 2009		July–August 2009	
	Google	Yahoo!	Google	Yahoo!
Household income (%)				
$> \$150{,}000$	8.53	7.68	8.18	8.66
\$100,000–\$149,999	14.52	12.40	14.45	12.69
\$60,000–\$99,999	27.47	25.86	27.66	24.23
\$30,000–\$59,999	29.29	31.45	29.55	30.82
$< \$30{,}000$	20.19	22.60	20.15	23.60
Age				
18–24	19.39	20.17	17.61	18.56
25–34	20.82	23.83	20.73	22.45
35–44	22.32	21.17	21.96	20.48
45–54	20.03	17.66	20.38	18.8
55+	17.45	17.18	19.32	19.72
Gender				
Male	50.3	46.92	51.81	47.98

Source. Experian Hitwise.

Figure B.1 How the Number of Clicks Changed on Google and Yahoo!: Monthly Analysis



Note. TM, trademark.

Table B.3 Relevant Changes to Search Engines Operations, March 2009–August 2009

Date	Change
March 2009	Beta testing starts for new AdWords interface. (This is the interface for the Web page where advertisers bid for their ads.)
March 24, 2009	Two changes were made to how nonpaid results were presented. The first change was an expanded list of useful related searches. The second change was the addition of longer search result descriptions.
April 6, 2009	Google Maps was adjusted so that it presented results even if the user did not type in a location, based on an algorithm designed to pinpoint a user's location.
April 18, 2009	AdWords system maintenance. Does not affect display of campaigns.

Table B.3 (Cont'd.)

Date	Change
May 16, 2009	AdWords system maintenance. Does not affect display of campaigns.
May 20, 2009	Google announces increased personalization for suggestions entered into the Google search box.
May 14, 2009	Google announces "search options" product. This was an optional navigational toolbar that allowed users to see results for a certain time frame and divide video and Web page results.
June 1, 2009	Google announces increased efficiency for the comma-separated value import function for its external AdWords editor.
June 13, 2009	AdWords system maintenance. Does not affect display of campaigns.
June 17, 2009	Yahoo! introduces new toolbar that allows users to jump to sites such as Flickr, Yahoo! Mail, and eBay.
July 22, 2009	Announcement that advertisers would be able to start using location extensions in the coming few weeks, which meant that they would not have to type in an ad for the local section separately.
June 26, 2009	Michael Jackson's death causes flood of traffic onto search engines. Some reports of slow response times.
July 11, 2009	AdWords system maintenance. Does not affect display of campaigns.
July 29, 2009	Announcement that Microsoft will now power Yahoo! search while Yahoo! will become the exclusive worldwide relationship sales force for both companies' premium search advertisers. This change would be effected in late 2011.
July 30, 2009	Yahoo! increases amount of information available on local business searches to include photos and details of amenities.
August 8, 2009	AdWords system maintenance. Does not affect display of campaigns.
August 24, 2009	Yahoo! announces rollout of increasingly integrated home page for Yahoo! users and search results.

Source. Yahoo! and Google press releases from March to August 2009.

Table B.4 Robustness Checks Using Collapsed Data

	Linear specification			Log specification		
	(1) Nonpaid clicks	(2) Paid clicks	(3) Total clicks	(4) Nonpaid clicks	(5) Paid clicks	(6) Total clicks
PostChange × *Google* × *TMHolder*	26,863.1***	−6,538.0*	20,325.3***	0.416***	−0.678	0.262**
	(7,272.6)	(3,489.1)	(5,997.2)	(0.131)	(0.475)	(0.122)
PostChange × *Google*	−7.817	37.12	29.30	−0.108	0.331	−0.0743
	(157.9)	(88.18)	(184.8)	(0.0896)	(0.432)	(0.0927)
PostChange × *TMHolder*	−908.8	148.0	−760.8	−0.265**	−0.204	−0.246**
	(1,788.2)	(1,343.4)	(2,158.2)	(0.112)	(0.244)	(0.0985)
PostChange	291.2***	63.42	354.7***	0.234***	0.224	0.233***
	(95.14)	(60.61)	(112.6)	(0.0696)	(0.208)	(0.0691)
Search engine–search term–website controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,180	3,180	3,180	3,180	3,180	3,180
R-squared	0.176	0.154	0.179	0.184	0.186	0.194

Notes. An observation is the number of clicks for a website in a two-month period for searches using a specific trademarked term on a specific search engine. Data are from April + May (combined) and July + August (combined) 2009. Ordinary least squares estimates are shown in columns (1)–(3). Log-linear estimates are shown in columns (4)–(6) (generalized estimating equation estimates with population-averaged effects rather than standard fixed effects). *Google* × *TMHolder*, *Google,* and *TMHolder* are dropped because of their collinearity with the search engine–search term–website fixed effects. Standard errors are clustered at search-term level.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table B.5 Trademark Holders' Sites Reached Through Competitor Trademarks, Where Such Combinations Were Not Affected by the Policy Change

	Linear specification			Log specification		
	(1) Nonpaid clicks	(2) Paid clicks	(3) Total clicks	(4) Nonpaid clicks	(5) Paid clicks	(6) Total clicks
PostChange × *Google* × *TMHolderSite*	−105.4	−96.54	−201.9	0.0225	5.389	1.623
	(196.1)	(177.6)	(299.6)	(28.48)	(1,099.3)	(27.47)
PostChange × *Google*	−28.59	26.19	−2.398	−0.117	0.735	−0.0930
	(90.15)	(39.96)	(98.24)	(0.114)	(0.560)	(0.107)
PostChange × *TMHolderSite*	−179.3***	−146.0**	−325.3***	−0.353	−7.713	−2.171
	(54.84)	(73.52)	(91.67)	(28.47)	(1,099.3)	(27.46)
PostChange	140.7**	60.60*	201.3***	0.188*	0.365*	0.219**
	(65.92)	(34.81)	(74.24)	(0.105)	(0.220)	(0.0970)

Table B.5 (Cont'd.)

	Linear specification			Log specification		
	(1) Nonpaid clicks	(2) Paid clicks	(3) Total clicks	(4) Nonpaid clicks	(5) Paid clicks	(6) Total clicks
May indicator	−75.58	10.84	−64.74	−0.0675	−0.00654	−0.0542
	(66.02)	(19.71)	(68.77)	(0.0661)	(0.239)	(0.0655)
Search engine–search term–website controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,104	7,104	7,104	7,104	7,104	7,104
R-squared	0.00129	0.0000546	0.00238	0.00673	0.00438	0.00631

Notes. Sample consists of hotel brand and search term combinations where the branded search term did not correspond to the website but includes cases where the website did own a trademark for *another* brand. This is a different sample from Table 2, which excluded such terms. Ordinary least squares estimates are shown in columns (1)–(3). Log-linear estimates are shown in columns (4)–(6) (generalized estimating equation estimates with population-averaged effects rather than standard fixed effects). An observation is the number of clicks for a website in a month for searches using a specific trademarked term on a specific search engine. Data are from April, May, July, and August 2009. *Google* × *TMHolder*, *Google*, and *TMHolder* are dropped because of their collinearity with the search engine–search term–website fixed effects. Standard errors are clustered at search-term level.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table B.6 Results for Trademark Holders' Sites Where the Trademark Holder Forbade Third-Party Sellers from Using the Trademark as a Contractual Condition

	Linear specification			Log specification		
	(1) Nonpaid clicks	(2) Paid clicks	(3) Total clicks	(4) Nonpaid clicks	(5) Paid clicks	(6) Total clicks
PostChange × *Google* × *TMHolder*	−2,864.9	−2,879.0	−5,743.8	−0.825	−0.366	−0.611
	(4,966.0)	(3,504.4)	(7,129.0)	(0.933)	(1.557)	(1.034)
PostChange × *Google*	−177.7	54.89	−122.8	0.320	−0.0488	0.334
	(193.1)	(80.97)	(212.2)	(0.378)	(0.173)	(0.414)
PostChange × *TMHolder*	−3,670.8	−1,042.1	−4,713.0	0.750	−0.495	0.386
	(2,679.7)	(1,047.6)	(3,336.8)	(0.860)	(1.423)	(0.955)
PostChange	−122.5	−9.316	−131.8	−0.350	0.0475	−0.243
	(285.4)	(128.9)	(248.4)	(0.266)	(0.102)	(0.281)
May indicator	−164.1	−42.80	−206.9	0.156	−0.140	0.134
	(553.0)	(257.1)	(475.8)	(0.255)	(0.119)	(0.253)
Search engine–search term–website controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	496	496	496	496	496	496
R-squared	0.414	0.390	0.446	0.218	0.648	0.496

Notes. Sample consists of trademark searches for brands that appeared successful at preventing their third-party sellers from advertising (InterContinental, Courtyard by Marriott, Ritz-Carlton, Springhill Suites, Towne Place Suites) that were excluded from the original sample in Table 2 as the policy change did not apply. Ordinary least squares estimates are in shown columns (1)–(3). Log-linear estimates are shown in columns (4)–(6). An observation is the number of clicks for a website in a month for searches using a specific trademarked term on a specific search engine. Data are from April, May, July, and August 2009. *Google* × *TMHolder*, *Google*, and *TMHolder* are dropped because of their collinearity with the search engine–search term–website fixed effects. Standard errors are clustered at search-term level.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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CORRECTION

In this article, "How Does the Use of Trademarks by Third-Party Sellers Affect Online Search?" by Lesley Chiou and Catherine Tucker (published in *Marketing Science*, Vol. 31, No. 5, September–October 2012, pp. 819–847, http://dx.doi.org/10.1287/mksc.1120.0724), Figure 2(b) has been corrected to show the correct panel.