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Stuck Online: When Online Engagement Gets in the Way of Offline Sales[1]

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In recent years, billions of dollars have been spent by both online and offline retailers on website design aimed at increasing consumers' online engagement. We study the relationship between online engagement and offline sales, utilizing a quasi-experimental setting in which a leading premium automobile brand gradually launched a new interactive website across markets, allowing for a treatment-control comparison. This paper offers evidence of a causal effect of online engagement on offline sales, with the high-engagement website leading to a decline of approximately 12% in car sales. This negative effect is due to substitution between online and offline engagement; users of the high-engagement website exhibited a decreased tendency to seek out personal contact with a car dealer and proceed to offline engagement—a necessary stage in the car purchase funnel. We develop an analytical model of the online-to-offline sales funnel to generalize our findings and highlight the conditions under which online engagement substitutes for offline engagement and potentially decreases offline sales. Taken together, our findings suggest that while online engagement serves as a means for both product information provision and consumer persuasion, it may fall short in achieving the latter goal, as compared to the offline channel. For purely offline products, hands-on engagement is a necessary step toward purchase. Thus, increasing consumers' online engagement may not be an optimal strategy if it has the potential to halt progression down the sales funnel and reduce offline engagement.

Keywords: Online engagement, e-commerce, online-to-offline, sales funnel, quasi-experiment

Introduction

Over a decade ago, when e-commerce was in its infancy, questions and concerns arose regarding the future of brick-and-mortar stores—in particular, whether physical stores would become showrooms for e-retail, and whether and to what extent consumers would shift from offline to online shopping (Calanog 2011, Greene 2012, Minzesheimer 2011). While about half of consumers do tend to use physical stores as showrooms, the reverse situation—i.e., online product research followed by an offline purchase—appears to be more common (Shannon-Missal 2014). Indeed, given growing digitization, the online-to-offline sales funnel has become a major component of multichannel retailing (Brynjolfsson et al. 2013, Verhoef et al. 2015).

Focusing attention on the online-to-offline funnel, both online and offline retailers have been striving to improve their online presence, with website spending

[1] Bin Gu was the accepting senior editor for this paper. Hong Guo served as the associate editor.

commanding the lion's share of marketing budgets (Sorofman et al. 2016). Billions of marketing dollars spent on brand websites are aimed at increasing consumers' *online engagement*, broadly defined as the level of users' website activity, as represented by different site-visit characteristics.[2] Investment in online engagement is largely motivated by its potential in driving consumers down the sales funnel, from the initial consideration level to the decision-making level, eventually leading to purchase (Brynjolfsson et al. 2013, Venkatesh and Agarwal 2006, Wiesel et al. 2011). Indeed, one global distributor reports that online engagement increases customers' total spending across channels by 7% to 29%, and that customers displaying high online engagement patterns are much less likely to churn.[3] Online engagement is therefore an important force in the online-to-offline funnel, which managers should learn how to correctly harness in order to generate value for their business.

Motivated by this, we therefore ask: *How does online engagement affect offline sales for products that must be purchased offline?* To address this question, we first conduct an empirical investigation in a quasi-experimental setting, establishing a causal effect of online engagement on offline sales. We propose that this effect is the result of substitution between online and offline engagement, where the latter is a necessary step in the online-to-offline purchase funnel. We find evidence of such substitution and demonstrate that the end effect on offline sales may be negative. In the Appendix, we present an analytical model of the online-to-offline sales funnel to generalize our empirical findings and provide conditions under which online engagement excessively substitutes for offline engagement, thereby decreasing offline sales.

Our research setting is the automobile market. Online-to-offline purchase journeys have become the norm for *purely offline products* such as cars, real estate, and healthcare services, which are (largely) unavailable online.[4] In the automobile market, close to 90% of new car shoppers use online sources before visiting a dealership (Mohr et al. 2014).[5] This has led to a decline in the average number of dealership visits from 5 to 1.6 between 2004 and 2014[6] (LeBeau 2014, Mohr et al. 2014).

Because of this dynamic, although dealership visits have been reduced, the first dealership a car shopper visits may very well be the last. Brand dealership visits are especially powerful for purchasing because of strong customer lock-in in the physical channel (Neslin and Shankar 2009), foot-in-the-door effects (Burger 1999, Freedman and Fraser 1966), persuasive abilities of car dealers, and the importance of interpersonal communication with salespeople when making large purchases (Olshavsky 1973, Webster 1968). Considering the prominence of online engagement together with the importance of the offline channel for experience goods, it is increasingly important to understand and measure the end effect of web presence on offline sales and evaluate how it is mediated by online consumer behavior.

We partnered with a premium automobile brand to estimate the effect of increased online engagement on its local brand websites on offline car sales in the same local markets between 2011 and 2014. The increase in online engagement resulted from the staggered launch of an upgraded interactive brand website in some but not all markets in our dataset, yielding a quasi-experimental setting that we analyze using a difference-in-differences (DID) empirical strategy.[7] The website upgrade increased engagement by improving the "build-your-own" feature (or *car configurator*)—which is the central user experience in the websites of many automobile brands.[8] Increased engagement was achieved by adding 360-degree visualizations, streamlining and gamifying the configuration experience, and making the feature more prominent on the landing page.

Our empirical investigation confirms that engagement did, in fact, substantially increase as a result of the

[2] These are discussed at length in the literature review.

[3] https://www.digitalcommerce360.com/2018/09/26/how-a-global-distributor-proved-online-engagement-boosts-offline-sales/.

[4] At the time of our analysis, the vast majority of vehicle purchases were completed in offline channels. This continues to be the case, though alternatives for purchasing cars online have recently emerged.

[5] These shoppers spend 43% of their online journey at brand and dealership websites, and these constitute the final online touch point for 67% of car shoppers (Cox Automotive 2019), attesting to their importance.

[6] The dealership visit remains a necessary step toward purchase, with 90% of U.S. consumers surveyed report having conducted at least one test drive prior to purchase.

[7] We compare pre- and post-launch engagement, sales, and user activity, for treatment vs. control markets. Complete details are provided below.

[8] A car configuration feature is prominently placed on websites for leading brands, such as Citroen, Ford, Audi, Mercedes, and many others.

upgrade.[9] We analyze the impact of the high-engagement website, finding a decrease in sales of 12-13%, on average, in markets where the upgraded site launched compared to control markets. We performed several robustness tests to support our use of the DID empirical strategy and our main findings, and conducted online lab experiments, confirming that the high-engagement website indeed offered an improved user experience, ruling out the possibility that the negative impact was due to a more complicated or otherwise inferior online experience.

We propose that the mechanism driving this negative effect on sales is substitution between online and offline engagement, where the latter is necessary for a vehicle purchase. To demonstrate the existence of such substitution, we studied consumers' utilization of online sales leads—that is, their choice of online options that lead to personal contact with a car dealer and thus constitute a link between the online and offline channels. We find that high online engagement increased test drive requests, which represent clear purchase intent and thus progression to the prepurchase offline engagement stage. However, higher online engagement simultaneously decreased pre-test drive requests for contact by dealers, thereby substituting for the offline engagement of customers who have not yet decided to visit a dealership and could have potentially been persuaded by personal interaction with a salesperson. This substitution between online and offline engagement is the likely driver of the observed decrease in sales. Thus, considering the dual role of online engagement in product information provision and consumer persuasion (as detailed in the analytical model presented in the Appendix), our findings suggest that the online channel may be less effective in terms of persuasion than the offline channel.

In sum, our natural experiment setup, coupled with the context of a purely offline product, provides a unique opportunity to identify a causal effect of online engagement on offline sales. For purely offline products, high online engagement can be a double-edged sword because substituting the offline hands-on experience with increased online engagement may decrease sales. For our premium automotive brand, increased online engagement on the brand website resulted in consumers' remaining online and not continuing their car shopping journey to offline engagement with a brand dealer. This substitution between online and offline engagement reduces opportunities for salespeople to convince still deliberating customers at a dealership, thus negatively impacting sales. Managers optimizing their online-to-offline strategies should therefore carefully consider the potential for substitution between online and offline engagement, especially for high-investment experience goods for which offline engagement is a necessary step toward purchase.

[9] Based on data from Alexa.com and supported by the results of online lab experiments reported in Appendix C.

Related Literature

E-commerce platforms and brand websites have become an integral part of almost any consumption process, as consumers gather information online at every step in order to make their purchase decisions (McAfee and Brynjolfsson 2012, Montgomery et al. 2004). The growing influence of online channels in consumer decision-making has given rise to two related streams of literature: multichannel retailing and online engagement. Studying the impact of online engagement on offline sales, this work draws from both of these streams.

Multichannel Retailing

Multichannel retailing has been defined as "the design, deployment, coordination, and evaluation of channels to enhance customer value through effective customer acquisition, retention, and development" (Neslin et al. 2006). The increasing digitization of consumption processes has led retailers to consider the different online and offline channels as touch points for engagement with their brands and products and potential points of sale, suggesting a broader, dynamic view of the consumption journey (Gallino and Moreno 2019). In line with this updated view, managers in retail now face multichannel challenges of devising business strategies, improving customer satisfaction, and optimizing operations across various online and offline channels—issues that have also driven scholarly work over the last two decades (Gallino and Moreno 2019, Neslin and Shankar 2009, Verhoef 2012, Verhoef et al. 2015, Zhang et al. 2010).

Table 1. Literature on The Interplay between Online and Offline Channels

Main findings	Papers
Channel Substitution	
Main findings	**Papers**
E-commerce site visits lead to online sales and reduce offline sales.	Brynjolfsson et al. (2009), Forman et al. (2009)
Consumers' "desire for service" and offline interaction could drive them away from the online channel.	Kollmann et al. (2012)
Online information about products' in-store availability is associated with a decrease in online sales, while in-store traffic and sales increase.	Gallino and Moreno (2014)
Channel Complementarity	
Main findings	**Papers**
A conceptual framework (offered in the marketing literature) argues that: (1) offline sales benefit from online consumer activity and vice versa, and (2) online and offline channels have a combined role in driving consumers down the purchase funnel.	Wiesel et al. (2011)
Offline stores complement the online channel, as offline in-store interactions along with on-site return options reduce the risk of an online purchase and thus increase sales in the online channel.	Kumar et al. (2019)
No Channel Substitution / Complementarities	
Main findings	**Papers**
The online channel does not significantly cannibalize the offline channel.	Biyalogorsky and Naik (2003) and Gentzkow (2007)
For an online retailer opening an offline store, both substitution and complementarity between offline and online consumer activities are found.	Wang and Goldfarb (2016)

Previous work has examined the interplay between online and offline channels, exploring both channel complementarity and substitution in terms of the end effect on sales, with mixed conclusions. Table 1 summarizes these studies, showing that the relationship between online and offline consumer activities and their effect on purchase decisions is context specific and merits further study of underlying mechanisms that may shed light on cross-channel effects.

Recent work has focused on *omnichannel* (rather than multichannel) retailing, a new approach reflecting fully integrated multichannel operations and customer experience with seamless flow between channels and flexible purchase options (Bell et al. 2014, Bijmolt et al. 2021, Brynjolfsson et al. 2013, Piotrowicz and Cuthbertson 2014).

Importantly, in both multichannel and omnichannel retailing, products can be purchased in any of the channels. However, for some products, a purchase may only be completed in the offline channel. These are *purely offline* products, for which the online channel can only be used for research shopping (Verhoef et al. 2007), and more broadly, for online engagement with the product or brand (Brodie et al. 2011). To the best of our knowledge, the cross-channel process for purely offline products has yet to be studied, and thus our paper contributes to the literature by examining the online-to-offline funnel for purely offline products, identifying a causal effect of the online channel on offline sales.

For purely offline products there is no substitution or complementarity between channels in the sales dimension. In this paper, we propose that the mechanism driving the effect of the online channel on offline sales is channel substitution in the engagement dimension—that is, substitution between online and offline engagement, where the latter must take place prior to purchase.

Online Engagement

The increase in consumer-firm touch points across multiple channels has led to a growing focus on user experience and, specifically, engagement as central constructs in the marketing literature of the last decade or so (Lemon and Verhoef 2016). Consumer engagement is defined as individuals' participation in and connection with a firm or brand and its representatives (Vivek et al. 2012) and serves an important role in shaping the customer experience as part of the purchase journey (Calder et al. 2009, Lemon and Verhoef 2016).

In the traditional offline purchase funnel, consumer engagement has largely referred to interactions with salespeople and firm representatives (Barlow et al. 2004, Jaakkola and Alexander 2014), but given the ever-increasing importance of online channels, discussions of consumer engagement now mostly refer to online engagement. Table 2 summarizes the extant work on engagement, its definitions, measurement, and impact on consumption.

Overall, the research on online engagement is generally in line with Kumar et al.'s (2010) theoretical arguments stating that engagement exerts a positive influence on consumption behavior, thereby increasing firm profitability. For automotive brand websites, which are studied in this paper, the "build-your-own" feature (or car configurator) is typically the central engagement experience. Car manufacturers thus consider site visits with car configuration activity as the online equivalent of a dealership visit (Naik and Peters 2009, Nöhrer and Egyed 2013).

Tying the Two Together

While online engagement has become an important construct in the marketing and human-computer interaction literatures, its impact beyond online channels has not yet been studied. Wiesel et al.'s (2011) conceptual framework for the online-offline funnel states that online browsing and engagement move consumers from the initial consideration level to the decision-making level, and thus eventually lead to purchase. Therefore, the demonstrated potential of online engagement for shaping attitudes and affecting decisions leads us to expect an influence on subsequent behavior in non-online channels as well. Hence, we conduct the first study of the causal impact of online engagement on offline sales, drawing a new connection between the literature on online engagement and the literature on multichannel retailing. We propose that this impact is mediated by substitution between online and offline engagement, and thereby offer a new perspective on channel substitution. Namely, we argue that substitution in the engagement dimension may lead to an effect on sales of a purely offline product, since, for such products, offline engagement must precede purchase. With respect to the multichannel retailing literature, this is a hitherto unexplored mechanism by which online channels may affect offline consumption decisions. Our setting of a purely offline product is especially useful to clearly illustrate this impact.

Online Engagement's Impact on Offline Sales: Analysis of a Quasi-Experiment

We partnered with a leading premium automobile manufacturer with a substantial global presence to estimate the effect of increased online engagement on local brand websites on the company's offline car sales, between 2011 and 2014. Increased online engagement in our setting was caused by the manufacturer's launch of a new interactive brand website, replacing the previously less interactive website located at the same URL.

The auto maker's stated goal for the new website was to increase consumers' engagement with the brand and its different car models and features. The main change, compared to the previous website, was a substantially improved car configuration experience in the "Build Your Own" tool, which constitutes the central user experience in our focal brand's website, as is common for automotive brands. This tool is designed to engage users as they test out different configurations of car models, interactively displaying the full set of customizable options, accompanied by detailed information and prices for each option. Increased engagement with the configurator, and thus with the website, was achieved by adding 360-degree visualizations with improved graphics, streamlining and gamifying the build-your-own experience, and more prominently placing the feature in the website's landing page.

Table 2. Engagement: Definitions, Metrics and the Effect on Consumption and Firm Value

Engagement Definitions	
In the traditional offline purchase funnel, consumer engagement is the host of customer interactions with salespeople and firm representatives.	Barlow et al. (2004), Jaakkola and Alexander (2014)
Applying the above definition to e-commerce settings, online engagement is defined as individuals' participation in and connection with the firm or brand and its representatives in online channels.	Vivek et al. (2012)
Online engagement is the interactive dynamics between a consumer and a brand, a product, or a service on a digital platform, comprising of cognitive, affective, and behavioral components.	Brodie et al.'s (2011) and Hollebeek et al.'s (2014); see Dessart et al. (2015) for a review.
Online engagement defined as consumers' levels of participation on a website.	Lehmann et al. (2012)
Online Engagement Metrics	
Engagement should be measured along three dimensions: (1) site popularity—represented by the number of distinct users, number of site visits and page views; (2) user activity—represented by the average number of page views per visit, and session duration; and (3) user loyalty—represented by the number of days in which a user visited the site, return rate, and overall time spent on the site in the reference period.	Lehmann et al. (2012)
Engagement metrics for user activity and loyalty should further include number of events per page view, bounce rate, scrolling, number of likes, comments, and shares.	Cevallos Rojas (2014), Clifton (2012), Lehmann et al. (2017), Pletikosa Cvijikj and Michahelles (2013)
Online Engagement Effects on Consumption and Firm Value	
Consumer engagement may generate value for the firm by driving four types of customer behavior: (1) customer purchase behavior; (2) customer referral behavior (i.e., the active referral of peers to the firm); (3) customer influencer behavior (e.g., generating product and firm related word of mouth); and (4) customer knowledge behavior (e.g., writing reviews, providing product feedback).	Kumar et al. (2010)
Online engagement affects consumption by inducing more positive consumer opinions, reviews, and comments.	O'Reilly (2007)
Online engagement may improve consumer satisfaction.	Bowden (2009)
Online engagement increases trust and loyalty.	Casaló et al. (2007), Hollebeek (2011)
Online engagement increases advertising effectiveness.	(Calder et al. 2009)
Online engagement metrics improve predictions of online purchase decisions.	Mallapragada et al. (2016), Montgomery et al. (2004)

To evaluate the impact of high online engagement generated by the upgraded website, we utilize the quasi-experimental setting arising from its staggered launch, whereby the website was upgraded in some countries only and at different times. This gradual launch strategy was possible since the brand's websites are centrally designed and deployed yet maintained at local, country-specific URLs, such that all traffic from a specific country, or *market*, is automatically directed to the local URL. Specifically, between 2011 and 2014, the auto manufacturer launched the upgraded *high-engagement (HE)* website in four markets, labeled T1-T4 in our dataset. T1 occurred in December 2011, and T2-T4 in December 2012. Other markets were left unaffected by the *HE* treatment in the above time period. These markets, which retained the previous *low-engagement (LE)* website format,[10] served as our control group. We obtained data for eight of these control markets, labeled C1-C8.

We estimate the effect of the *HE* treatment by comparing pre- and post-launch engagement, sales, and user activity, for treatment vs. control markets. This is the difference-in-differences (DID) empirical strategy, whereby the effect of the treatment is measured as the change in the differences between treatment and control groups caused by the onset of the treatment (see Angrist & Pischke, 2009 for further details and discussion). More specifically, our identification strategy exploits differences in the launch times of the *HE* website across markets and the existence of control markets in which the website did not launch in our period of analysis. This setting, with multiple groups and time periods, is known in the literature as a general DID model (see, for example, Bertrand et al. 2004).[11] In the analysis below we demonstrate the validity of this identification strategy.

Our main dataset was made available by our partner premium auto manufacturer. We analyze quarterly sales data for the four-year period from 2011 to 2014, for T1-T4 and C1-C8.[12] Further analyses of the effects of launch on engagement and user activity utilize additional datasets based on subsets of these treatment and control groups and subsets of the four-year period (because of constraints in data availability as detailed below—see Appendix A for data description and summary statistics).

Manipulation Check: The HE Website Launch Increased Online Engagement

The starting point of our analysis is to confirm that the launch of the interactive brand website indeed resulted in higher online user engagement. This manipulation check is performed using data from Alexa.com, which tracks and measures global online activity.[13] We study the impact of the upgraded website on two variables, *Time-on-Site* and *Traffic Rank*. *Time-on-Site* is a well-accepted proxy for user engagement (Luo et al. 2013, Mallapragada et al. 2016, Montgomery et al. 2004) and is measured as time spent on the brand website. *Traffic Rank* is a measure of website popularity determined by Alexa.com based on global internet traffic, such that a lower rank indicates greater popularity.

Alexa.com measures engagement for the 100,000 most popular websites worldwide; therefore, *Time-on-Site* was available only for two treatment markets (T2-T3) and three control markets (C3, C4, C7). *Traffic Rank* for the brand's market-specific websites was available for all treatment markets and for seven control markets (all but C8). Both metrics were available at a monthly level for September 2012 through December 2014, i.e., starting three months before the T2-T4 launch. Note that monthly *Time-on-Site* is actually the average *Time-on-Site*, averaged across visits to the website each month.

The launch of the interactive website was not accompanied by any related promotions designed to attract more users to the brand's market-specific websites. The launch was aimed only at increasing engagement for website visitors and not at increasing website traffic. Hence, we expect to find a positive impact of launch on *Time-on-Site*, with no effect on *Traffic Rank*. Figure 1 shows that this is indeed the case. Comparing the pre- and post-launch three-month periods, the average monthly *Time-on-Site* did not significantly differ between the treatment and control markets pre-launch, substantially increased for the T2-T3 launch markets in the post-launch months, and slightly decreased for the control markets (where this decrease is not statistically significant). For *Traffic Rank*, both the treatment and control markets suffered an increase in rank (i.e., decreased traffic) in the post-launch months.

[10] At least in our period of analysis.
[11] The general DID model is estimated using a fixed-effects model, with fixed effects controlling for market-invariant and time-invariant effects. In this fixed-effects model, the coefficient of the treatment effect is referred to as the DID estimator. Further details accompany the presentation of the models below.
[12] Offline conversions are still typically measured at an aggregate level (Goic and Olivares 2019).
[13] Additional manipulation checks are performed as part of the online lab experiments described in Appendix C.

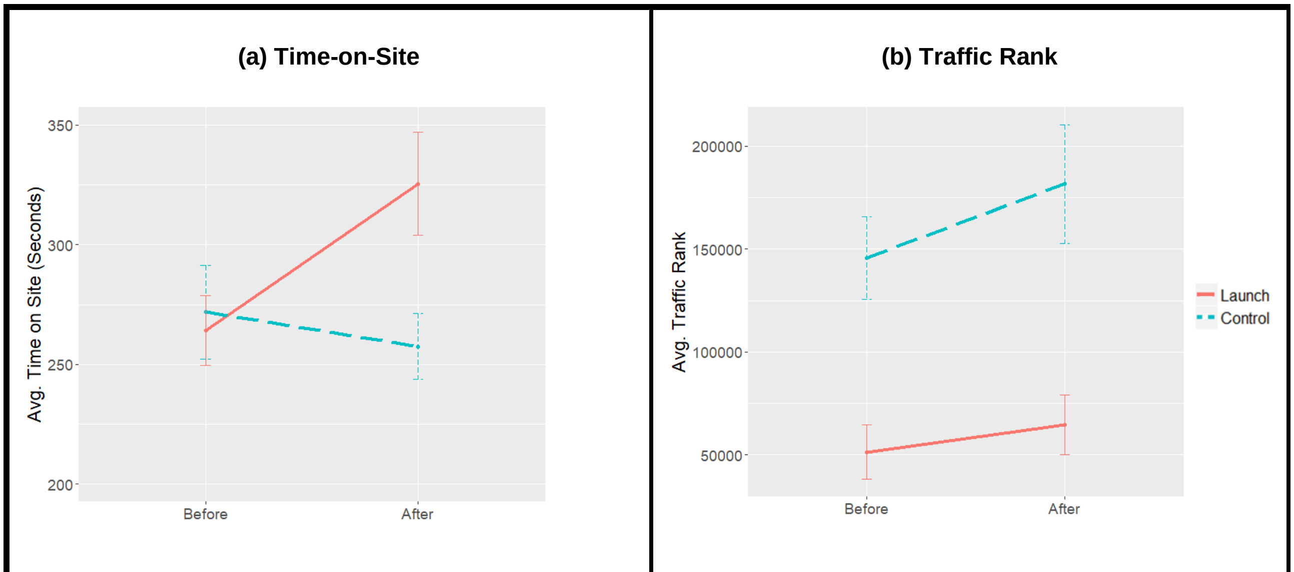

Figure 1. The Effect of Launch on Online Engagement. (a) Average Time-on-Site Three Months Before and After Launch: T2-T3 vs. Three Control Markets; (b) Average Traffic Rank Three Months Before and After Launch: T2-T4 vs. Seven Control Markets.

We estimate the effect of launch on engagement using the following general DID regression model:

$$(1) \quad TimeOnSite_{cym} = \alpha_c + \beta_y + \gamma_m + \boldsymbol{\delta} \cdot Launch_{cym} + \epsilon_{cym}$$

where cym represents $country - year - month$. The model thus has a full set of country, year, and month fixed effects represented by $\alpha_c, \beta_y,$ and γ_m, to control for differences between countries and for the trend and seasonality in the car market. The idiosyncratic error term is ϵ_{cym}. We define the binary variable *Launch* as equaling 0 until the new website was launched (in each market), and 1 from the month of launch onward. We estimate δ, which is the effect of $Launch$.[14] Estimation results[15] are reported in Column (1) of Table 3, where Column (2) presents estimation results for the effect of Launch on *Traffic Rank* (with the same model specification). Standard errors, clustered at the country level, are bootstrapped using the "wild bootstrap" method due to the small number of clusters (Cameron et al. 2008).

The results show a significant increase in consumers' engagement in the upgraded local websites and no significant change in the traffic to these websites. Specifically, launch of the interactive website increased *Time-on-Site* for the average site visit by approximately 63 seconds (p = 0.003***). These results are in line with the manufacturer's stated goal for the website's redesign: namely, higher engagement.

The Negative Effect of the HE Website Launch on Sales

We now turn to our main DID analysis—examining the impact of the *HE* website launch on sales, employing country-level quarterly panel data of the number of cars sold, available from the manufacturer for T1-T4 and C1-C8, in 2011-2014. As noted, our identification strategy exploits differences in the launch times of the *HE* website across markets, as the website upgrade occurred in 2011Q4 for T1, in 2012Q4 for T2-T4, and did not take place in our period of analysis for C1-C8, which are thus referred to as our control markets. The DID analysis hinges on the parallel trends assumption, which we discuss, test, and validate in the following subsection.

[14] This general DID regression model is therefore a fixed-effects model, in which the coefficient of interest is the DID estimator, δ.

[15] This and all the other fixed-effect models are estimated using OLS, where OLS is used on the demeaned model, i.e., after the fixed effects are projected out.

Table 3. The Effect of *HE* Website Launch on Time-on-Site and Traffic Rank

	Dependent Variable:	
	Time-on-Site (1)	Traffic Rank (2)
Launch	62.67*** (19.78)	-4,950.67 (22792)
Constant	264.52*** (7.91)	134919.21*** (2714.56)
Observations	139	308
Adjusted R^2	0.43	0.80

Note: Fixed effects for country, year and month included. Cluster robust standard errors shown in parentheses (Clustered on country).*** *p < 0.01, ** p < 0.05, * p < 0.1*

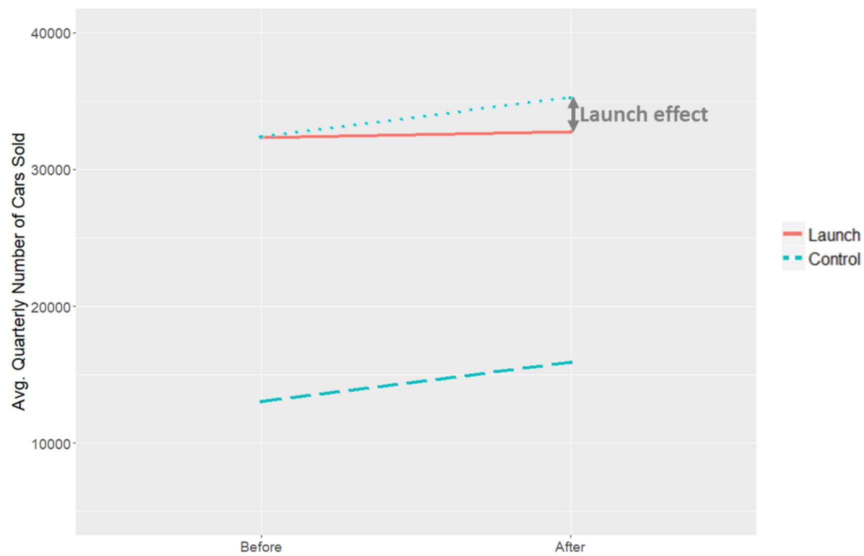

Figure 2. The Effect of the *HE* Launch on Sales. Average Quarterly Sales for T2-T4 Launch Markets and All Control Markets, One Year before and after the December 2012 Launch.

Figure 2 plots the average quarterly sales, in terms of numbers of cars sold, one year before and one year after the December 2012 launch for T2-T4 and all control markets. The dashed light blue line represents the parallel trend assumption, by showing the hypothetical change in sales for treatment markets had they continued to follow the same trend as control markets (i.e., absent treatment). The "launch effect" marked in Figure 2 is the change in the differences between the control and treatment markets' average quarterly sales, comparing the year prior to the year after the launch. We observe a negative effect, as the *HE* launch group exhibits a smaller increase in average quarterly sales compared to control markets.

To formalize this result, we estimate the launch effect using the following model:

$$(2) \quad \log(Sales_{cyq}) = \alpha_c + \beta_y + \gamma_q + \boldsymbol{\delta} \cdot Launch_{cyq} + \epsilon_{cyq}$$

Where $\log(Sales_{cyq})$ is the natural logarithm of quarterly number of cars sold in country c in year y and quarter q. The variables $\alpha_c, \beta_y,$ and γ_q represent fixed effects for country, year, and quarter, controlling for these sources of variation. As both launches occurred toward the end of a quarter, we defined the binary variable *Launch* as equal to 0 until the quarter in which the new website launched (in each market), and 1 from the quarter following launch onward. This further accounts for the pace of the new car market, where typically 1-3 months pass from initial inquiry to the supply of a new vehicle (Nuttall 2015, Putsis et al. 1994). Due to this supply lag, we tested a second model specification where the dependent variable is the one-quarter lead of sales, considering the possibility of a delayed impact.

To these base specifications, we added the variable *TotalRegistered*, which provides the total quarterly number of noncommercial vehicles registered in each country, allowing for better control for country-level trends in automobile sales. We further added search trends data represented by the variable *GoogleTrends*, to control for the level of interest in the brand in each market (McAfee and Brynjolfsson 2012). Search trends data has also been shown to be an accurate predictor of automotive sales (Choi and Varian 2012, Du and Kamakura 2012, Geva et al. 2017).

Estimation results for these six specifications are reported in Table 4. Standard errors, clustered at the country level, are bootstrapped using the wild bootstrap method due to the small number of clusters (Cameron et al. 2008). We focus our attention on δ, the effect of $Launch$.

Our results show a significant negative effect of the *HE* launch on sales, such that post-launch quarterly sales were, on average, approximately 12-14% lower in the treatment markets compared to the control markets, using same-quarter sales (Columns (1), (3) and (5)), and approximately 11% lower using next-quarter sales (Columns (2), (4) and (6)).

To rule out the possibility that this negative effect is due to some exogenous shock to the premium segment of the market in the treatment countries, we compared sales for our premium brand to two close competitors in the treatment markets before and after launch. This robustness test, reported in Appendix B, provides further support to our main findings.

Robustness: Validity of the Control Group

The manufacturer chose to deploy the *HE* website gradually and continued its roll-out in the same manner in other markets after our period of analysis. Reportedly, the order of launch markets was chosen based on internal considerations and was not based on previous web activity or sales in these markets. This supports the soundness of our treatment-control comparison, in our quasi-experimental setting. However, to ensure the validity of our DID empirical strategy, we conducted several tests of the parallel trends assumption.[16] This assumption requires that treatment and control groups follow a similar pre-intervention trend, such that they are indeed comparable, and any divergence in trend for the treatment group in the post-intervention period is due to the onset of treatment.

Analyzing pre-launch trends: Figure 3 visually presents the sales trend and shows parallel pre-launch trends for T2-T4 and all control markets. Post-launch, we observe a small downward vertical shift in treatment markets' sales, and a difference in trends, as the treatment group's growth rate is now slower compared to that of the control group.

Formally, we estimate the following model as a direct test for differences in pre-launch trends (as in Gallino and Moreno 2014):

$$(3)\quad \log(Sales_{cyq}) = \alpha_c + \beta_1 \cdot Trend_{yq} + \boldsymbol{\beta_2} \cdot Trend_{yq} \cdot Treatment_c + \epsilon_{cyq}$$

Where $Trend_{yq}$ is the index of quarter q in year y, and $Treatment_c$ is an indicator variable that equals 1 if country c is in T1-T4, and 0 otherwise. Other variables are defined as in Model (2). We also tested specifications that include $TotalRegistered_{cyq}$ and $GoogleTrends_{cyq}$ as additional controls. In all specifications, β_2 is not statistically significant ($p > 0.1$; see Table B3 in Appendix B), implying that there is no difference in pre-launch trends between the treatment and control groups.

Relative Time Model[17]: We conducted a second robustness test, as in Autor (2003) and Greenwood and Agarwal (2016). This falsification test confirmed that *HE* launch status predicted sales only after—and not before—launch, where a finding of no pre-treatment effect provides further evidence of no pre-launch differences in trends for treatment and control markets. For this test, we created a set of dummy variables, indicating the quarter relative to the *HE* launch. Specifically, we used indicator variables for 1-3 quarters before launch, and 0-5 quarters after launch, labeled $RelLaunch_{c,t}$, where $t \in \{-3, -2, .., +5\}$; and an indicator for the sixth quarter and onward after launch, labeled $RelLaunch_{c,+6onward}$.

[16] Additional tests are reported in Appendix B.

[17] Also known as the Granger Causality Test (Granger 1969).

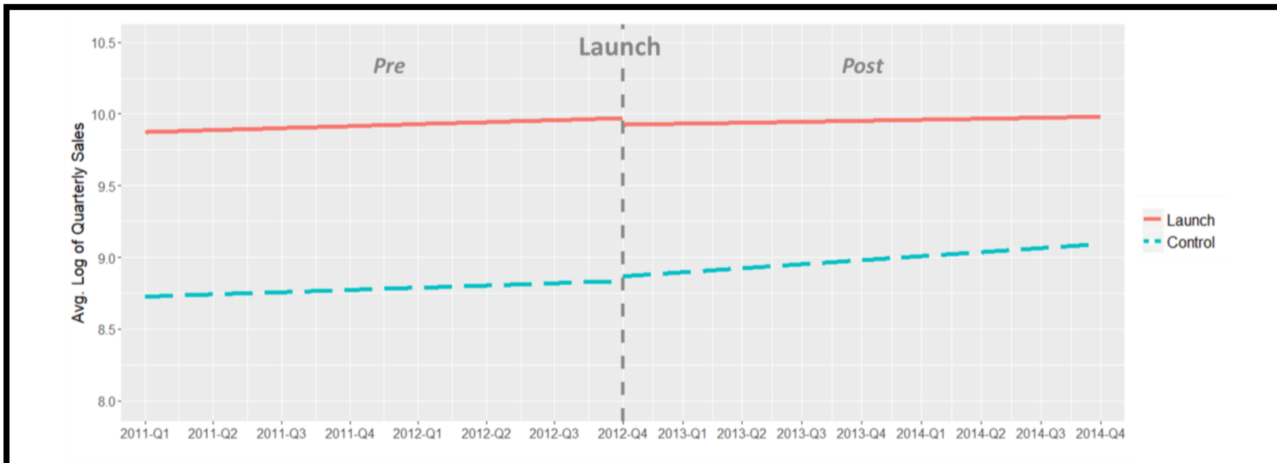

Figure 3. Linear Trend of $Log(Sales)$ for Treatment (T2-T4) vs. Control Markets (C1-C8), Pre- and Post-Launch.

Table 4. The Effect of the HE Website Launch on Sales

	Dependent Variable:					
	$\log(Sales_q)$ (1)	$\log(Sales_{q+1})$ (2)	$\log(Sales_q)$ (3)	$\log(Sales_{q+1})$ (4)	$\log(Sales_q)$ (5)	$\log(Sales_{q+1})$ (6)
Launch	-0.14** (0.07)	-0.12** (0.06)	-0.13** (0.07)	-0.12** (0.07)	-0.15*** (0.06)	-0.12* (0.07)
TotalRegistered			0.0000 (0.0000)	-0.0000 (0.0000)	0.0000 (0.0000)	-0.0000 (0.0000)
GoogleTrends					0.004 (0.004)	-0.001 (0.005)
Constant	9.05*** (0.02)	9.09*** (0.03)	8.84*** (0.14)	9.14*** (0.06)	8.58*** (0.30)	9.18*** (0.26)
Observations	192	192	192	192	192	192
Adjusted R^2	0.98	0.98	0.98	0.98	0.98	0.98

Note: Fixed effects for country, year and quarter included. Cluster robust standard errors shown in parentheses (Clustered on country).*** $p < 0.01$,** $p < 0.05$,* $p < 0.1$

These variables allow for a possible effect of launch before and after the actual *HE* launch, and further allow us to examine the dynamics of the *HE* impact—whether the effect increases over time or remains stable. The following Model (4) incorporates these variables, that replace $Launch$ in Model (2), and further includes country and quarter fixed effects (α_c and γ_q) as well as control for market specific trends in car sales, represented by $TotalRegistered$.

(4) $$\log(Sales_{cyq}) = \alpha_c + \gamma_q + \sum_{t\in\{-3,..+4\}} \delta_t \cdot RelLaunch_{c,t} + \delta_{+5onwards} RelLaunch_{c,+5onwards} + TotalRegistered_{cyq} + \epsilon_{cyq}$$

Estimation results are reported in Table 5. The results confirm that there are no anticipatory effects, that is, the differences between treatment and control markets do not appear prior to the *HE* launch, in support of the parallel trends assumption.[18] Furthermore, we observe the negative impact of the *HE* website increasing in magnitude in the periods following launch.

[18] For additional robustness, we employ a coarsened exact matching (CEM) procedure (Iacus et al. 2012) to increase the ex ante comparability of our treatment and control markets (Appendix B), as well as a placebo treatment model that further supports the validity of our empirical strategy (Appendix B).

Table 5. Anticipated Effects Test (Granger-Causality Test)

	Dependent Variable: $\log(Sales)$
TotalRegistered	0.0000 (0.0000)
d_PreLaunch3	0.11 (0.10)
d_PreLaunch2	-0.08 (0.07)
d_PreLaunch1	-0.07 (0.06)
d_Launch0	*Omitted Base Case*
d_PostLaunch1	-0.05 (0.08)
d_PostLaunch2	-0.15* (0.09)
d_PostLaunch3	-0.17** (0.08)
d_PostLaunch4	-0.13* (0.07)
d_PostLaunch5	-0.11 (0.07)
PostLaunch6onward	-0.25*** (0.08)
Constant	8.95*** (0.14)
Observations	192
Adjusted R^2	0.98

Note: Fixed effects for country and quarter included. Cluster robust standard errors shown in parentheses (Clustered on country).
*** p <0.01, ** p <0.05, * p <0.1

Mechanism: Substitution between Online and Offline Engagement

The automobile brand's website is designed to affect sales by driving interested and deliberating shoppers to personally interact with a car dealer at a dealership where they can test drive a car and complete the purchase. Therefore, online engagement is meant to complement offline engagement by driving consumers to engage offline with a dealer. We conjecture that the negative effect that the upgraded website had on offline sales is due to the substitution between online and offline engagement that manifested instead of the intended complementarity. We now describe online options that constitute sales leads intended to lead consumers from the online channel to offline interaction with a salesperson, and then provide evidence of substitution between online and offline engagement based on the utilization of online sales leads. On the brand website, sales leads are actions that indicate consumer interest in further information about a brand model, a desire to interact with a dealer to receive a dealership offer, or interest in scheduling a test drive. After choosing an online sales lead, the interested consumer is contacted by a car dealer and online engagement is followed by offline engagement. This is the case for the pre- and post-launch website.

We propose that the mechanism leading to the decrease in sales under *HE* is high engagement in the online channel substituting for offline engagement rather than complementing it. We therefore expect to find a negative effect of launch on online sales leads, suggesting that the *HE* website was less effective than the *LE* version in driving consumers to the offline engagement stage and thus resulted in decreased sales. Importantly, the utilization of online sales leads represents a lower bound for the actual level of ensuing offline engagement, since they are optional features embedded in the website, and their use is not mandated in any way. Hence, not all consumers who engage online with the brand and decide to proceed in the purchase funnel perform a sales lead action to indicate their intention to visit a dealership; rather, they may simply arrive at a dealership and interact with a dealer.

We studied the effect of launch on sales leads using a panel of quarterly sales leads for T3 and three control markets, between 2012 and 2014.[19] Online sales leads are captured by the following three variables: (1) *BD*—brochure downloads, where a brochure includes all possible options for model configuration along with their

[19] The automaker provided limited data for online sales leads. To validate the analysis of the limited dataset, we demonstrate that the markets for which sales leads data is available are indeed representative of the markets for which it is not. See Appendix D.

price (for a single chosen model); (2) *RFO*—requests for an offer; and (3) *TDA*—test drive applications. *TDA* indicate a commitment to arrive at a dealership and are thus performed by customers who likely have a strong purchase intent. On the other hand, *BD* is an initial sales lead, representing an early stage in the car purchase journey, where the customer is still gathering information and deliberating. The *RFO* represents an intermediate stage when the deliberating customer seeks out personal contact with a dealer.

Figure 4 illustrates the effect of launch on these three variables. Comparing the difference between control markets to T3 in *BD, RFO,* and *TDA* in the year before T3's *HE* launch to the year after, we observe a positive effect of launch on *BD* and *TDA*, and a large negative effect on *RFO*. The effect is estimated in the following DID model:

$$(5)\quad \log\left(SalesLead_{cyq}\right) = \alpha_c + \beta_y + \gamma_q + \boldsymbol{\delta} \cdot Launch_{cyq} + \epsilon_{cyq}$$

where $SalesLead$ is one of $\{BD, RFO, TDA\}$ and the remaining variables are the same as in specification (2). Estimation results are reported in Table 6. The results indicate that the launch of the *HE* website increased the number of BD and TDA, while reducing the number of RFO.[20] The results continue to hold when we further controlled for total car registrations in each market (reported in Appendix D).[21]

Our results suggest that higher online engagement leads to increased information gathering online, as consumers highly engaged with their model of interest were more likely to download the model's brochure in order to retain a record of the model they spent a long time configuring with all the configurable options. Furthermore, *HE* helped move customers with a strong purchase intent down the purchase funnel, by increasing TDA. Yet, at the same time, higher online engagement resulted in fewer, pre-test drive interactions with dealers, represented by the decrease in RFO. This reduction in personal offline contacts with customers who are still in the deliberation stage is in line with the conjectured substitution between online and offline engagement following launch of the upgraded site and is the likely driver of the decrease in sales.

However, we acknowledge that the analysis of online sales leads' utilization provides only limited evidence to the proposed mechanism, as we cannot demonstrate a direct relationship between an online sales lead and a subsequent offline sale with our available data. This limitation is further discussed in the concluding remarks section. In Appendix E, we present a theoretical model of the online-to-offline sales funnel and analytically derive conditions under which online engagement substitutes for offline engagement and identify when this may decrease offline sales. The model thus provides a complementary methodology, supporting our proposed mechanism.

Concluding Remarks

Online product search and engagement play an increasingly important role in firms' omnichannel strategies. Yet the impact of online engagement beyond online channels has not been studied and online engagement has remained a neglected construct in the multichannel retailing literature. Despite the billions of marketing dollars spent on highly engaging brand websites, a causal effect of online engagement on offline sales has not been demonstrated.

Filling this gap, we study the impact of online engagement on the online-to-offline sales funnel, by exploiting a unique quasi-experiment, arising from a premium automaker's gradual launch of a new website in four local markets. We find a negative effect of increased online engagement on offline sales, and propose that the mechanism leading to this effect is substitution between online and offline engagement in contexts in which offline interaction and engagement must precede a car purchase. We find evidence of substitution between online and offline engagement by analyzing the utilization of online sales leads on the new *HE* website and the previous *(LE)* format. Consumers' higher online engagement helped move customers with a strong purchase intent down the purchase funnel, but higher online engagement simultaneously resulted in reduced personal interactions between deliberating customers and salespeople, i.e., lower offline engagement. Thus, higher online engagement resulted in a loss of opportunities to persuade undecided shoppers offline and therefore lowered sales for the brand.

[20] We refrain from comparing the magnitude of these effects to the magnitude of the effect on sales, as this analysis is based on a limited dataset.

[21] The DID parallel trends assumption holds. For the three type of sales leads, there was no difference in pre-launch trends between the treatment and control groups (see Appendix D).

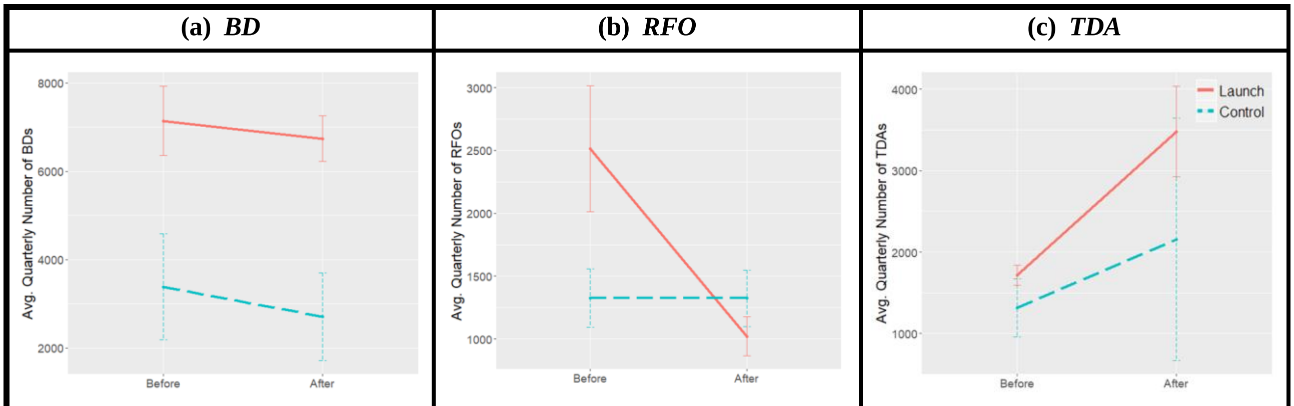

Figure 4. The Effect of HE Launch on Online Sales Leads: (a) BD; (b) RFO; (c) TDA. Comparing T3 to Control Markets, a Year before and after the T3 Launch, We Observe a Positive Effect of Launch on BD and TDA, and a Negative Effect on RFO.

Table 6. *HE* Launch effect on Online Sales Leads

	Dependent Variable:		
	log(BD) (1)	log(RFO) (2)	log(TDA) (3)
Launch	0.33***	-0.78***	0.92**
	(0.15)	(0.06)	(0.38)
Constant	7.65***	7.59***	6.78***
	(0.06)	(0.07)	(0.09)
Observations	36	36	48
Adjusted R^2	0.98	0.69	0.81

Note: Fixed effects for country, year and quarter included. Cluster robust standard errors shown in parentheses (Clustered on country).
*** p < 0.01, ** p < 0.05, * p < 0.1

The negative effect of online engagement on offline sales we find is substantial in magnitude because of the combination of three distinguishing features of our setting. First, the launch led to a substantial and economically significant increase of more than 20% in online engagement. Second, this increase in engagement centered around a focal milestone in the car shopping journey, as automotive brand websites play an important and growing role in shoppers' purchase paths. Finally, since online engagement is becoming more central to this industry, the number of dealership visits has been decreasing, infusing the relatively few remaining visits with greater weight and power in swaying consumer decisions and brand choices. Taken together, these features imply that substitution between online and offline engagement for a premium automotive brand may indeed lead to a nontrivial effect on sales, thereby supporting the face validity of the effect we identify.

Our work contributes to the research on the relationship between consumer behavior in online and offline channels and the impact of online engagement in omnichannel retailing. Our empirical results provide the first evidence of a causal effect of online engagement on purely offline sales. For managers, the negative effect we identify suggests that setting high online engagement goals is not a "one size fits all" strategy and must be carefully considered, especially when a hands-on experience is a necessary step toward purchase, and online engagement may substitute for offline engagement with the product. Our findings are therefore generalizable to products that are primarily sold in physical channels, largely consumed offline, or those that involve a substantial offline component. Examples of such products are healthcare services, real estate, and various consumer experiences commonly found in the hospitality and tourism industries.

Based on our conclusions, managers should further consider personalizing the online experience to help move undecided shoppers (especially consumers of high-investment experience goods) down the sales funnel. One such option would be nudging website users who appear "stuck" at the online engagement stage by initiating a chat with a sales representative. This would create a sales lead that may help prevent the potential loss of sales opportunities arising from excessive substitution between online and offline engagement.

Considering the potential limitations of this work, we note that our empirical analysis focuses on the automobile market, and specifically on premium cars. It is possible that the substitution between online and offline engagement is especially strong for this type of product, and that the negative effect identified is therefore context specific. To alleviate this concern, in Appendix E we offer a theoretical framework that considers the online-to-offline purchase funnel for products that are only available offline and study the role of online and offline engagement within this model. Further generalizability may come from future work examining the impact of online engagement in other omnichannel markets.

An additional limitation concerns the data available for our analysis of the launch effect on sales leads. While this analysis supports our proposed mechanism, we cannot demonstrate a direct relationship between interpersonal communication following an RFO and a subsequent offline sale. We thus provide a theoretical model of the online-offline sales funnel, in which we analyze the impact of online engagement on consumers' decisions to proceed to offline engagement and the effect on offline sales (in Appendix E). This complementary methodology provides additional support of the mechanism for cases in which the empirical evidence is not ideal.

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Appendix A

Data Description

Alexa.com Data Used in the Manipulation Check

Monthly *Time-on-Site* data is available for T2, T3, C3, C4 and C7. The data spans September 2012-December 2014, except C4 for which one month (September 2012) is missing. Monthly *Traffic Rank* data for T1-T4, and C1-C7, for September 2012-December 2014.

Table A1: Summary Statistics of Alexa.com Data

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Time-on-Site	139	283.28	48.56	168.50	251.56	308.41	413.71
Traffic Rank	308	138,953.10	147,750.00	7,402.87	39,059.46	189,014.60	944,551.90
Launch	308	0.33	0.47	0	0	1	1
Year	308	2,013.29	0.70	2012	2013	2014	2014
Month	308	7.07	3.52	1	4	10	12

Sales Data Provided by Our Premium Automotive Brand

Quarterly sales data 2011-2014 is available for C1-C8, T1-T4.

Table A2. Summary Statistics of Sales Data

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Sales	192	18,163.61	23,809.48	1,405	4,377.5	17,645	103,147
TotalRegistered	192	458,318.00	485,330.20	20,879	99,600.5	690,309.8	2,080,920
GoogleTrends	192	71.79	12.49	42	64.1	81.5	90
Launch	192	0.19	0.39	0	0	0	1
Year	192	2,012.50	1.12	2011	2011.8	2013.2	2014
Quarter	192	2.50	1.12	1	1.8	3.2	4

Registration Data for Focal and Competing Brands

Monthly registration data 2011-2014 for focal brand and its two closest competitors is available for T2 and T3.

Table A3. Summary Statistics of Competing Brands Data

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Registrations	288	16,463.28	7,946.07	2,147	8,791.2	23,597.2	30,965
Launch	288	0.17	0.38	0	0	0	1
year	288	2,012.5	1.12	2011	2011.8	2013.2	2014
month	288	6.5	3.46	1	3.8	9.2	12

Data on Online Sales Leads Provided by Our Premium Automotive Brand

Quarterly sales leads data of all sales leads (BD, RFO, TDA) for 2012-2014 is available for C3, C4 and T3, and only TDA data for 2012-2014 is available for C5.

Table A4. Summary Statistics of Online Sales Leads Data

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
BD	37	4,291.78	3,103.44	146.00	331.00	6,754.00	9,250.00
RFO	36	1,572.78	1,241.40	494.00	802.75	1,827.25	7,502.00
TDA	48	2,874.90	4,264.72	249	396.2	3,605	18,420
Launch	48	0.17	0.38	0	0	0	1
Year	48	2,013.00	0.83	2012	2012	2014	2014
Quarter	48	2.50	1.13	1	1.8	3.2	4

Appendix B

Quasi Experiment: Robustness Tests

Robustness: Comparison to Major Competitors

We compare sales for our premium brand to two close competitors in the treatment markets, before and after launch, as another robustness test of the negative effect of the *HE* launch on sales identified in the market-level DID analysis. This comparison will rule out the possibility that the negative effect we find is due to some exogenous negative shock to the premium segment in the treatment countries, which is not related to the launch of the *HE* website. The brand's two closest competitors were identified by the company. We use new vehicle registration data as a close proxy for sales, as we do not have access to internal sales data for the competing brands. We thus analyze a brand-level panel of monthly vehicle registrations, for three brands — the focal brand and its two main rival brands — focusing on the two largest markets T2 and T3, in which registration data is publicly available.

We estimate a DID model similar to specification (2), where the control groups are competing brands. We study the effect of launch on both a one- and two-month lead for sales, to account for the pace of the car market as well as a possible lag between purchase and registration. The results reported in Table B1 and Figure B1 show a significant decrease of approximately 9-10% ($p < 0.01^{***}$) in sales following the *HE* website launch, compared to the control brands. We therefore reaffirm our main finding that high online engagement led to a decrease in car sales. The soundness of this comparison is ensured, as we find no difference in pre-launch trends between the treatment and control groups (details of this robustness test and results are reported in the next subsection and in Table B2).

Table B1. The Effect of *HE* Launch on Sales: Comparison to Competing Brands

	Dependent Variable:					
	$\log(Sales_{t+1})$			$\log(Sales_{t+2})$		
	Control Brand1	**Control Brand2**	**Both**	**Control Brand1**	**Control Brand2**	**Both**
	(1)	(2)	(3)	(4)	(5)	(6)
	-0.09***	-0.11***	-0.10***	-0.09***	-0.09***	-0.09***
	(0.003)	(0.01)	(0.01)	(0.001)	(0.002)	(0.003)
Constant	8.92***	8.88***	8.90***	10.05***	9.99***	10.00***
	(0.02)	(0.04)	(0.02)	(0.009)	(0.0007)	(0.008)
Observations	192	192	288	192	192	288
Adjusted R^2	0.78	0.83	0.80	0.78	0.83	0.80

Note: Fixed effects for brand, country, year and month included.
Cluster robust standard errors shown in parentheses (Clustered on brand).
*** $p < .01$,** $p < .05$,* $p < .1$

Table B2. Pre-Launch Trend: Comparison to Major Competitors

	Dependent Variable: $\log(Sales)$
Trend	0.002*** (0.0003)
*Trend * Treatment*	0.01 (0.005)
Constant	9.45*** (0.01)
Observations	144
Adjusted R^2	0.63

Note: Fixed effects for country, year and quarter included.
Cluster robust standard errors shown in parentheses (Clustered on country).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

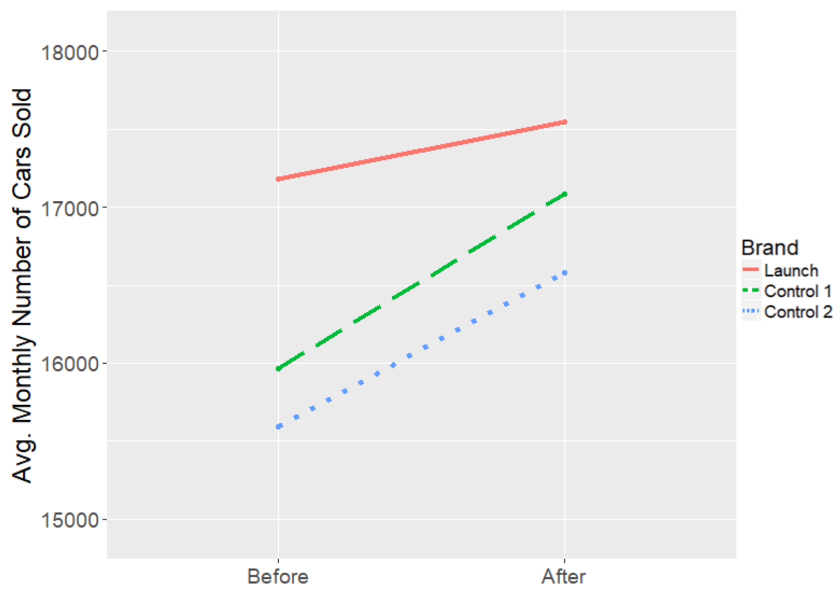


Figure B1. The Effect of *HE* Launch on Sales: Comparison to Competing Premium Brands, Two Years Before and Two Years After the *HE* Launch in T2 and T3.

Comparison to Competitors: Validity of DID Empirical Strategy

To validate the DID empirical strategy employed in the comparison to competing brands, we conduct a formal test of differences in pre-launch trends between the treatment brand and its two major competitors, focusing on the two largest markets T2 and T3, in which registration data is publicly available. We estimate the following model specification (as in Gallino and Moreno, 2014):

$$\log(Sales_{bcyq}) = \alpha_c + \gamma_b + \beta_1 \cdot Trend_{yq} + \boldsymbol{\beta_2} \cdot Trend_{yq} \cdot Treatment_{bc} + \epsilon_{bcyq}$$

Where $Trend_{yq}$ is the index of quarter q in year y, and $Treatment_{bc}$ is an indicator variable that equals 1 if brand b is the treatment brand, and 0 otherwise. We further include country and brand fixed effects represented by α_c and γ_b. We are interested in the estimate for β_2, where a non-zero, statistically significant estimate would indicate that the treatment brand and its main competitors were characterized by different sales trends pre-launch.

Estimation results are reported in the table below showing that the estimate for β_2 is not statistically significant ($p > 0.1$), implying that there is no difference in pre-launch trends between the treatment and control groups. This rules out the possibility that the firm launched the website in countries where it faced stronger competition.

DID Validity: Pre-Treatment Trend Comparison of Treatment vs. Control Markets

As an additional robustness test of our main specification, we conduct a second test for differences in pre-launch trends between the treatment and control markets, now controlling for market size using *TotalRegistered*, and for interest in the brand as reflected by relative search intensity on Google using *GoogleTrends*.
Estimation results are reported in Table B3 and show no significant difference in pre-launch trends between the treatment and control markets.

Table B3: Pre-Trend Comparison with Additional Controls

	Dependent Variable: $log(Sales)$		
	(1)	(2)	(3)
Trend	0.02 (0.02)	0.01 (0.02)	0.01 (0.02)
*Trend*Treatment*	-0.002 (0.02)	0.004 (0.02)	0.004 (0.02)
TotalRegistered		0.0000 (0.0000)	0.0000 (0.0000)
GoogleTrends			0.003 (0.01)
Constant	8.96^{***} (0.04)	8.83^{***} (0.09)	8.66^{***} (0.36)
Observations	92	92	96
Adjusted R^2	0.98	0.98	0.98

Note: Fixed effects for country included.
Cluster robust standard errors shown in parentheses (Clustered on country).
**** p < 0.01, ** p < 0.05, * p < 0.1*

Coarsened Exact Match (CEM)

To increase the comparability of the treatment and control markets, we employ a coarsened exact matching (CEM) procedure (Iacus et al. 2012), thereby limiting ex-ante differences between the two groups. We match treatment and control markets on three different criteria: *TotalRegistered* as a measure of the automobile market size, *GoogleTrends* as a measure of brand interest, and period index.[22] We then replicate our main DID analysis, as specified in model (2), on the matched sample. The results (reported in Table B4) continue to show a significant negative effect of the *HE* website launch on offline car sales, thereby providing further support for our conclusions.

Table B4: The Effect of the *HE* Website Launch on Sales, for CEM Matched Data

	Dependent Variable:	
	$log(Sales_q)$ **(1)**	$log(Sales_{q+1})$ **(2)**
Launch	-0.11^{*} (0.08)	-0.11^{*} (0.08)
Constant	9.15^{***} (0.01)	9.18^{***} (0.02)
Observations	148	148
Adjusted R^2	0.98	0.98

Note: Fixed effects for country, year and quarter included.
Cluster robust standard errors shown in parentheses (Clustered on country).
**** p < 0.01,** p < 0.05,* p < 0.1*

[22] Adding more matching variables further reduces the size of the sample, thus limiting our ability to draw robust inferences.

Placebo Treatment Model

As a final robustness test of the DID results, we estimate a placebo treatment model to demonstrate that the observed effect on sales cannot be attributed to chance. For this exercise, we use pre-launch data for T2-T4 and control markets, and estimate the effect of a placebo (fake) launch starting December 2011, using the same specifications as in Model (2). The effect of the placebo treatment is not statistically significant, as expected ($p > 0.1$; estimation results are reported in Table B5).

Table B5. Placebo Treatment Model

	Dependent Variable:					
	$\log(Sale_q)$ (1)	$\log(Sales_{q+1})$ (2)	$\log(Sales_q)$ (3)	$\log(Sales_{q+1})$ (4)	$\log(Sale_q)$ (5)	$\log(Sales_{q+1})$ (6)
Placebo	0.05 (0.09)	0.07 (0.09)	0.09 (0.10)	0.06 (0.09)	0.08 (0.10)	0.07 (0.09)
TotalRegistered			0.0000^{*} (0.0000)	-0.0000 (0.0000)	0.0000 (0.0000)	-0.0000 (0.0000)
GoogleTrends					0.002 (0.01)	-0.002 (0.003)
Constant	8.98^{***} (0.03)	9.06^{***} (0.03)	8.71^{***} (0.15)	9.14^{***} (0.09)	8.60^{***} (0.36)	9.26^{***} (0.16)
Observations	88	88	88	88	88	88
Adjusted R^2	0.98	0.98	0.99	0.98	0.99	0.98

Note: Fixed effects for country, year and quarter included. Cluster robust standard errors shown in parentheses (Clustered on country).
**** p < 0.01, ** p < 0.05, * p < 0.1*

Appendix C

Robustness: Website Upgrade and User Experience—Online Lab Experiments

Experiment Design and Analysis

We perform two online lab experiments to test the upgraded website's impact on various user experience characteristics. The *engagement/information (E/I)* experiment is designed to verify that engagement indeed increased following the upgrade, and further test for possible confounding factors in the *HE* website—information levels and choice overload (Scheibehenne et al. 2010) —which may provide alternative explanations for the effect on sales.

An additional alternative explanation for the negative impact of the *HE* website could be faulty web design that influenced key user-experience parameters, such as site usability, information quality, and interactivity features (Jiang et al. 2010, Liang and Lai 2002, Palmer 2002, Zviran et al. 2006). The *user-experience (UX)* experiment additionally tests these parameters, to ensure that the *HE* website does not provide an unintentionally inferior user experience.

In both experiments, participants were randomly assigned to either the *HE* or *LE* version of the brand website to complete tasks and then answer a follow-up survey. The experiments were carried out on the brand's live local websites (in one treatment market and one control market) that are in the same language. Specifically, participants were asked to browse the manufacturer's website and perform the following three tasks, associated with purchase intention: (a) design their own car using the "Build your own car" feature of the website (BYO); (b) locate and download a brochure of their selected model (BD); and (c) locate and complete the test drive application form (TDA). The experiment tasks were submitted as human intelligence tasks (HITs) to Amazon Mechanical Turk (MTurk). Each participant was required to complete the entire set of website tasks and the follow-up survey in order to receive payment (of $1).

In the *E/I* experiment, we compare 1002 users' experience on the *HE* (post-launch) and *LE* (pre-launch) websites using both completion times for each website task and survey responses. Comparing tasks' completion times, we find that participants assigned to the *HE* website spent more time customizing their "own car" than the *LE* participants (BYO: 405 seconds vs. 319 seconds, $p < 0.01^{***}$). Furthermore, *HE* participants could locate and download brochures and submit test drive requests significantly faster than their *LE* counterparts (BD: 143 seconds vs. 196 seconds, $p < 0.01^{***}$; TDA: 97 seconds vs. 129 seconds, $p < 0.01^{***}$). These results are presented in Figure C1 below.

To disentangle engagement from information level, we asked the *E/I* experiment participants to evaluate website information levels along three different dimensions, on a 5-point Likert scale: (1) price information for the selected model, (2) information about features of the selected model, and (3) the general "look and feel" of the model. As shown in Table C1, we found no significant difference in the informativeness scores between the *HE* and *LE* websites along all three dimensions, thereby ruling out site informativeness as an alternative explanation.

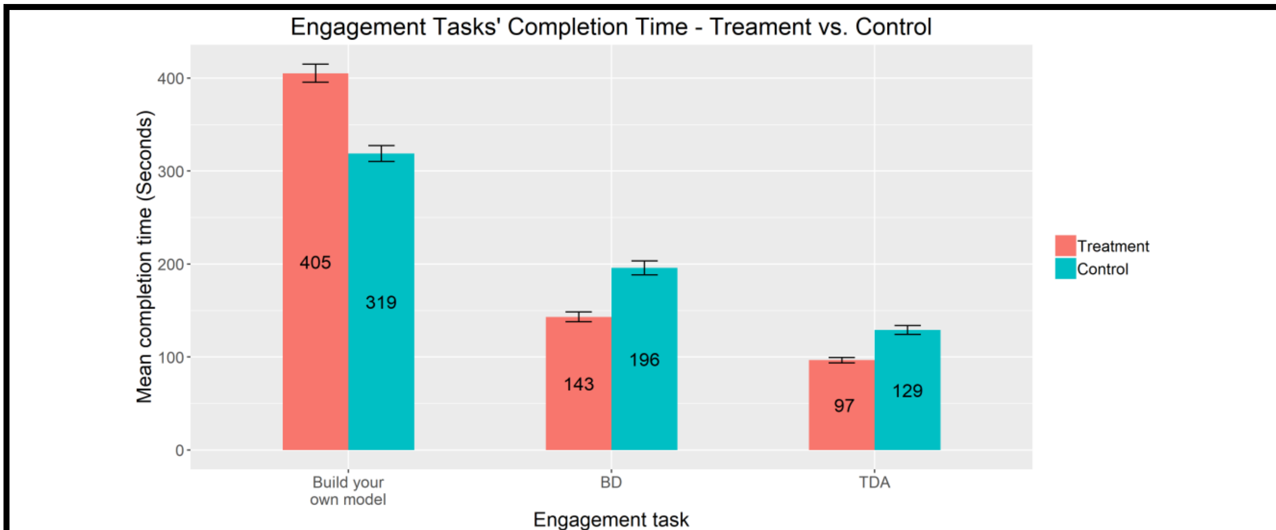


Figure C1. Comparison of the HE (Post-Launch) and LE (Pre-Launch) Websites Efficiency Using Different Tasks Completion Times.

Table C1. Comparison of the Information Level Between the HE and LE Websites.

	Mean (seconds)		***P*-value**
Parameter	**Treatment (*HE*)**	**Control (*LE*)**	
Price Information	4.18	4.13	0.43
Features Information	4.13	4.04	0.14
Look and Feel	4.01	4.03	0.75

We further asked participants to report their purchase intent on a 7-point Likert scale following online engagement. Since differences in the number of choices are known to affect purchase intent (Scheibehenne et al. 2010), we expect that a possible choice overload in the *HE* website will lead to lower average purchase intent for the *HE* participants. However, we find no significant difference between the reported purchase intent of the *HE* and *LE* participants (with an average score of 4.78 for *HE* vs. 4.68 for *LE* participants, $p = 0.35$), thus ruling out the possibility of choice overload in the *HE* website.[23]

In the *UX* experiment, we randomly assigned 335 participants to either the *HE* or *LE* version of the brand website to complete tasks similar to the ones in the *E/I* experiment. Following task completion, participants answered a ten-item survey on website perceived usefulness and ease of use (Barnes and Vidgen 2002, Pavlou and Fygenson 2006), with responses on a 7-point Likert scale (see Figure C3 in Appendix C for the complete evaluation questionnaire).

[23] However, note that experiment participants were recruited from the general population of Amazon Mechanical Turk workers; therefore, they were likely not in the market for purchasing a new car.

Analysis of participants' engagement tasks yields similar results to those found in the *E/I* experiment, further supporting our finding that the *HE* website increases users' engagement and facilitates the creation of online sales leads. The *HE* website further dominates in usefulness and ease-of-use evaluations, based on participants' survey responses. Namely, participants ranked the post-launch version of the brand website significantly higher than its pre-launch version in all survey items, except for one (the only exception was appearance attractiveness, where there was no significant difference in mean scores). Figure C2 shows the comparison of mean survey scores for each item, for the *HE* (treatment) and *LE* (control) website versions.

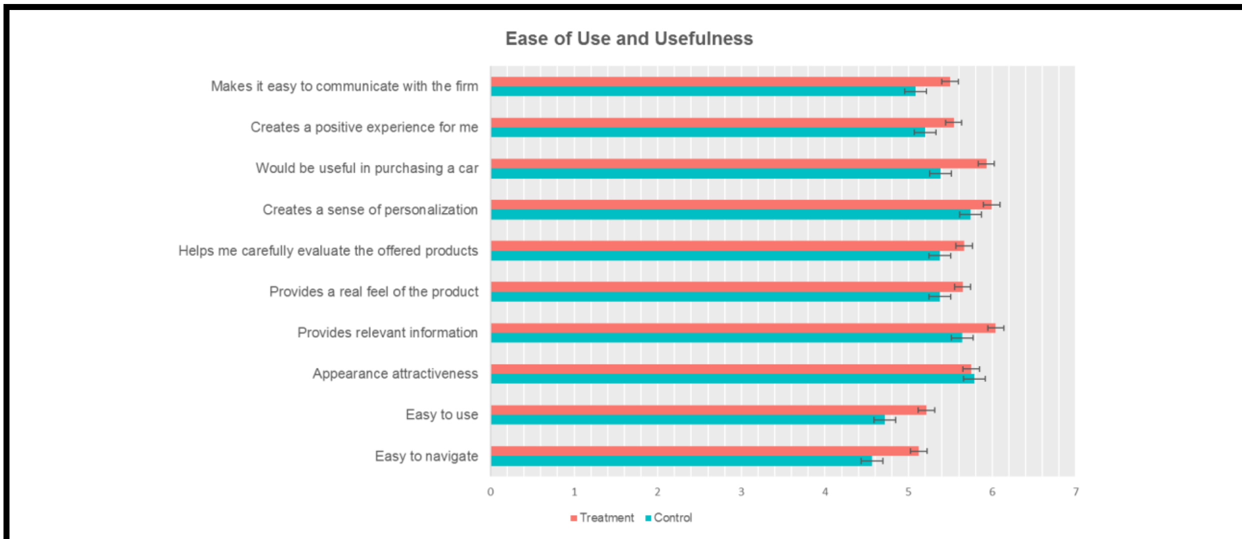
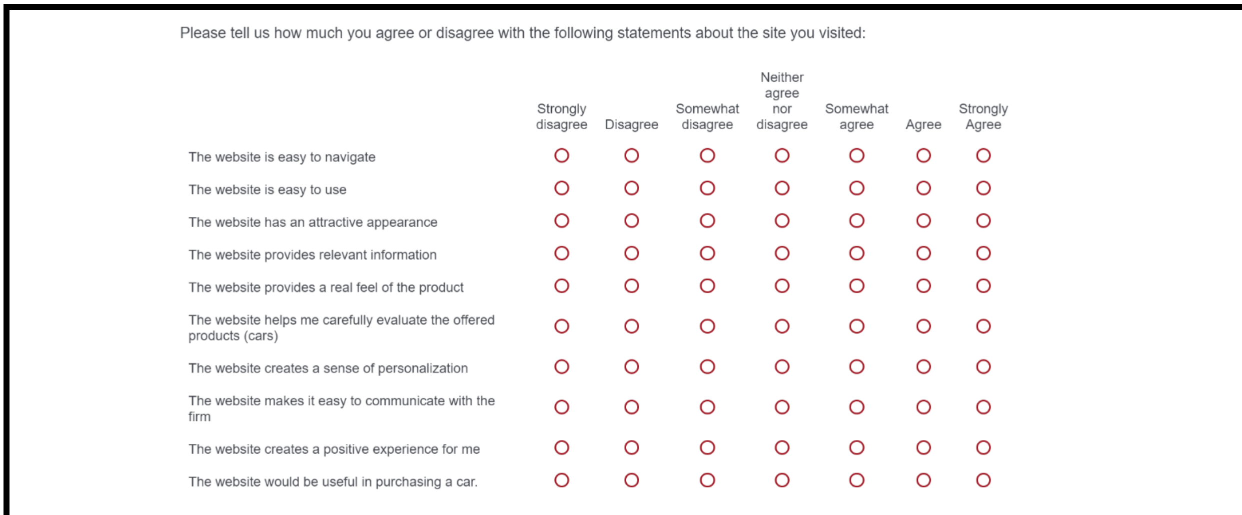


Figure C2. Online Lab Survey Results - Comparison of the *He* (Post-Launch) And *Le* (Pre-Launch) Websites Ease of Use and Usefulness

These findings rule out the alternative explanation that the upgraded website was (unintentionally) inferior. In fact, the *UX* experiment demonstrates that indeed, the *HE* website enhanced and improved consumers' online experience.

Website Evaluation Questionnaire



Please tell us how much you agree or disagree with the following statements about the site you visited:

	Strongly disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly Agree
The website is easy to navigate	○	○	○	○	○	○	○
The website is easy to use	○	○	○	○	○	○	○
The website has an attractive appearance	○	○	○	○	○	○	○
The website provides relevant information	○	○	○	○	○	○	○
The website provides a real feel of the product	○	○	○	○	○	○	○
The website helps me carefully evaluate the offered products (cars)	○	○	○	○	○	○	○
The website creates a sense of personalization	○	○	○	○	○	○	○
The website makes it easy to communicate with the firm	○	○	○	○	○	○	○
The website creates a positive experience for me	○	○	○	○	○	○	○
The website would be useful in purchasing a car.	○	○	○	○	○	○	○

Figure C3. Website Evaluation Questionnaire, Completed by Experiment Participants Following the Experimental Tasks on the Live Brand Websites.

UX Experiment: Robustness Test

To increase the robustness of our findings, in the *UX* experiment, participants were asked to perform tasks associated with purchase intention (similar to *E/I* experiment reported above). The *UX* experiment engagement tasks results (see Figure C4 below) are very similar to the *E/I* results, providing additional support to the main findings that the *HE* website increases users' engagement and facilitates the creation of online sales leads.

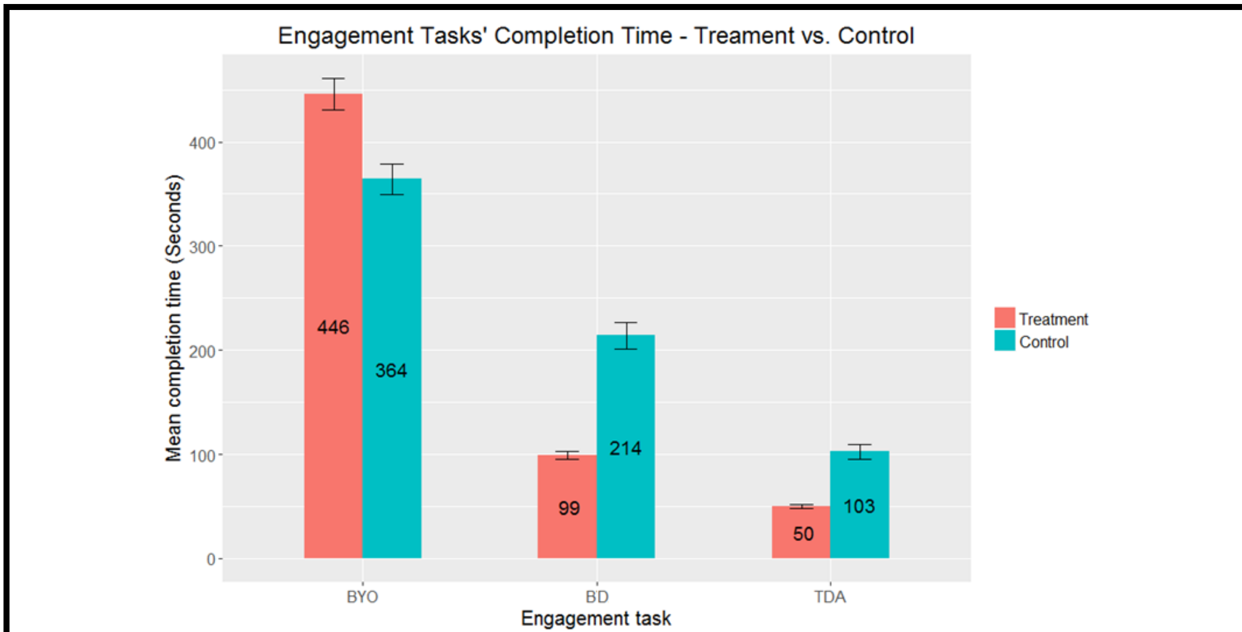


Figure C4. Comparison of the *HE* (post-launch) and *LE* (pre-launch) Websites Efficiency Using Different Tasks' Completion Times.

Appendix D

Robustness Tests for the Analysis of the HE Impact on Sales Leads

The brand's data collection before the launch of the *HE* website was not uniform across markets and was standardized following the website upgrade. Specifically, sales leads data were only collected in some markets before the launch of the *HE* website, and therefore the analysis of the launch effect on these variables is conducted on a limited dataset. To validate the analysis of the limited dataset, we demonstrate that the markets for which sales leads data is available are indeed representative of the markets for which it is not. Formally, we test the possibility of differences in pre-launch sales trends between T3, for which sales leads data is available, and the remaining treatment markets, and similarly between C3, C4, C5 and the remaining control markets using the following specification -

$$\log\left(Sales_{cyq}\right) = \alpha_c + \beta_1 \cdot Trend_{yq} + \boldsymbol{\beta_2} \cdot Trend_{yq} \cdot dSalesLeadsData_c + \epsilon_{cyq}$$

Where $Trend_{yq}$ is the index of quarter q in year y, and $dSalesLeadsData_c$ is an indicator variable that equals 1 if sales leads data is available for country c, and 0 otherwise. The variable α_c represent fixed effects for country, and ϵ_{cyq} is the idiosyncratic error term. We further estimate additional specifications in which $TotalRegistered_{cyq}$ and GoogleTrends $_{cyq}$ are the dependent variables, which test for differences in the trend for total vehicle registrations and the online searches for the brand, between markets with and without sales leads data (in the treatment group, and in the control group). These specifications thus capture both brand sales trends, trends in online interest in the brand as well as trends for the entire automobile market. We used 2 years of pre-launch data for the first specification ($Sales_{cyq}$) and 4 years of pre-launch data for the two additional specifications ($TotalRegistered_{cyq}$ and $\mathrm{GoogleTrends}_{cyq}$). Estimation results, reported in the tables below, show that β_2 is not statistically significant at a 0.05 significance level ($p > 0.1$ in specifications (1), (2), (3), (5), and (6) and $p > 0.05$ in specification (4)), thus alleviating concerns regarding a difference in pre-launch trends between treatment markets with and without sales leads data (T3 vs. T1, T2, T4) and a difference between control markets with and without these data (C3, C4, C5 vs. C1, C2, C6, C7, C8).

Table D1: Pre-Trend Comparison within Treatment Markets

	Dependent Variable:		
	$\log(Sales_q)$ (1)	$\log(TotalRegistered_q)$ (2)	$GoogleTrends_q$ (3)
Trend	0.01*** (0.003)	-0.0004 (0.01)	0.56 (0.69)
Trend SalesLeadsData*	-0.005 (0.003)	-0.003 (0.01)	-0.29 (0.69)
Observations	28	60	60
Adjusted R^2	0.99	0.96	0.52

Note: Fixed effects for country included.
Cluster robust standard errors shown in parentheses (Clustered on country).
**** p < 0.01, ** p < 0.05, * p < 0.1*

Table D2. Pre-Trend Comparison within Control Markets

	Dependent Variable:		
	$\log(\mathbf{Sales_q})$ (4)	$\log(\mathbf{TotalRegistered_q})$ (5)	$\mathbf{GoogleTrends_q}$ (6)
Trend	0.0002 (0.02)	0.01 (0.01)	0.13 (0.36)
Trend SalesLeadsData*	0.04* (0.02)	-0.005 (0.01)	-0.52 (0.44)
Observations	64	128	128
Adjusted R^2	0.98	0.97	0.60

Note: Fixed effects for country included.
Cluster robust standard errors shown in parentheses (Clustered on country).
**** p < 0.01, ** p < 0.05, * p < 0.1*

We further conduct a formal test of differences in sales leads' pre-launch trends between T3 and C3, C4, C5, to validate the DID analysis of the impact of *HE* on sales leads (as in Gallino and Moreno, 2014):

$$\log(SalesLead_{cyq}) = \alpha_c + \beta_1 \cdot Trend_{yq} + \boldsymbol{\beta_2} \cdot Trend_{yq} \cdot Treatment_c + \epsilon_{cyq}$$

Where $Trend_{yq}$ is the index of quarter q in year y, and $Treatment_c$ is an indicator variable that equals 1 if country c is in the treatment group, and 0 otherwise. We further include country fixed effects represented by α_c. We are interested in the estimate for β_2, where a non-zero, statistically significant estimate would indicate that treatment and control countries were characterized by different trends for sales leads in the pre-launch period.

Estimation results reported in the table below show that the estimate for β_2 is not statistically significant ($p > 0.1$), implying that there is no difference in pre-launch trends between the treatment and control groups.

Table D3. Pre-Trend Test for Website Activity Variables

	Dependent Variable		
	log(BD) (1)	**log(RFO) (2)**	**log(TDA) (3)**
Trend	-0.20*** (0.03)	-0.20* (0.10)	0.03 (0.18)
*Trend * Treatment*	0.03 (0.03)	-0.14 (0.10)	-0.13 (0.18)
Constant	8.26*** (0.03)	7.91*** (0.10)	6.96*** (0.21)
Observations	12	12	16
Adjusted R^2	1.00	0.82	0.84

Note: Fixed effects for country, year and quarter included.
Cluster robust standard errors shown in parentheses (Clustered on country).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Finally, we estimate the model in Equation (5), further controlling for *TotalRegistered*. Estimation results reported in the table below are similar to those in reported in Table 6, and provide further robustness to our findings.

Table D4. Launch effect on Website Activity Variables (Controlling for *TotalRegistered*)

	Dependent variable		
	log(BD) (1)	**log(RFO) (2)**	**log(TDA) (3)**
Launch	0.30** (0.14)	-0.94*** (0.10)	0.79** (0.38)
TotalRegistered	0.0000 (0.0000)	0.0000*** (0.0000)	0.0000*** (0.0000)
Constant	7.47*** (0.12)	6.72*** (0.06)	6.01*** (0.18)
Observations	36	36	48
Adjusted R^2	0.98	0.73	0.81

Note: Fixed effects for country, year and quarter included.
Cluster robust standard errors shown in parentheses (Clustered on country).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix E

Analytical Model: From Online Engagement to Offline Engagement and Purchase ■

We model the purchase process for a purely offline product that begins with online information gathering and engagement in the brand or product website, and may proceed to offline engagement and purchase. We specifically consider products for which offline search costs are high (e.g., cars, real estate) such that all purchase processes effectively begin online. The model provides a general framework to consider consumers' movement down the online-offline sales funnel, and highlights the conditions under which consumers will and will not proceed to engage offline. Not tailored specifically to the automobile sales funnel, the model provides generalization to our empirical findings, illuminating the conditions under which high online engagement may be detrimental to the sales of an offline product. The model considers imperfectly informed consumers who enter the purchase funnel by visiting the product website. These consumers' online experiences shape their perceived fit with the product, thereby determining whether or not they proceed down the funnel to engage offline, where they may purchase the product.

The model highlights two roles of online engagement, in the spirit of those attributed to traditional advertising: providing product information and persuasion (Bagwell 2007). The first is modeled as uncertainty reduction regarding consumers' fit with the product, and the second as the introduction of a non-negative product bias. These effects counteract when consumers' uncertainty regarding product fit is relatively high and match probability with the product is low. When the share of consumers who match with the product is relatively low[24], lower online engagement, which maintains high uncertainty levels, is a stronger driver of movement down the sales funnel than high online engagement, which biases toward purchase, yet reduces uncertainty. The next subsection presents the details of our modeling framework, and the following subsection presents the analysis and derives conditions under which higher online engagement will lead to lower offline sales. The third and final subsection is dedicated to discussing the relationship between our general analytical framework and empirical results.

The Model

There is a mass 1 of consumers with unit demand interested in the product (or brand). These interested consumers are characterized by the value of their match with the product, denoted $m \in \{0,1\}$, where $m = 1$ (0) represents a match (no match) with the product, such that given perfect information about product attributes, including price, they will always (never) buy it. The probability of a match for an interested consumer is $\Pr[m = 1] = \tau,\ \tau \in (0,1)$. Since the mass of consumers is 1, τ also represents the share of interested consumers who match with the product.

Interested consumers are imperfectly informed about their match with the product, such that they are uncertain regarding m and assign probability σ to the opposite type, where $\sigma \sim U[0,\overline{\sigma}]$ is iid across consumers and $\overline{\sigma} < 1$. Consumers thus enter the market with a *perceived match value* defined as:

$$\tilde{t}_m^0 = (1-\sigma)m + \sigma(1-m)$$

where superscript 0 represents the initial perceived match value *before* online engagement has taken place. The initial perceived match value is therefore $\tilde{t}_1^0 = 1-\sigma$ when $m = 1$, and $\tilde{t}_0^0 = \sigma$ when $m = 0$. We refer to σ as consumers' *uncertainty* parameter. Consumers begin their product search online, visiting the product or brand website to gather information and reduce their uncertainty regarding m.

Online engagement: Consumers engage online at the brand website. Their online engagement level depends on the website, and can be either high or low, denoted $e_{on} \in \{L, H\}$. High engagement allows the consumer to learn about his match value by reducing σ, while also creating a non-negative bias toward the product. Low engagement does not improve consumer information, and does not introduce a bias. Formally, let $\sigma_{e_{on}}$ be the revised uncertainty parameter, such that $\sigma_H = 0$ and $\sigma_L = \sigma$,[25] and let $b_{e_{on}}$ denote the bias created by online engagement, where $b_L = 0$ and $b_H \sim U[0,\overline{b}]$ is *iid* across consumers, with $\overline{b} < 1$.

[24] Quite likely for premium cars, with the segment comprising approximately 13% of total car sales, http://www.thecarconnection.com/news/1104264_do-ugly-may-car-sales-mean-a-recession-is-coming.

[25] The results would qualitatively hold under a more general assumption that high engagement reduces σ, but not necessarily to zero.

We thus write the updated perceived match value following online engagement, which depends on the engagement level and true match value, $\tilde{t}_m^{e_{on}}$, for $m \in \{0,1\}$ as

$$\tilde{t}_0^{e_{on}} = \min(\sigma_{e_{on}} + b_{e_{on}}, 1)$$

$$\tilde{t}_1^{e_{on}} = \min(1 - \sigma_{e_{on}} + b_{e_{on}}, 1)$$

This revised perceived match value determines the probability that a consumer moves down the sales funnel to the next stage of offline engagement (e.g., physically examining real estate, arriving at a car dealership). Specifically, assume that when the perceived match value exceeds some threshold $T \geq 0.5$, the online consumer seeks offline contact with the product, such that $\Pr[\tilde{t}_m^{e_{on}} \geq T]$ is the probability of offline engagement for a consumer with true match value m and online engagement level e_{on}.

Offline engagement and purchase: The product can only be purchased offline following some offline engagement with the product and a sales representative. Examples of offline engagement include interaction with a car dealer or realtor, physical inspection of the product (e.g., a car, real estate), and, for automobiles, a test drive.

We assume that offline engagement reduces uncertainty regarding m, and introduces non-negative bias toward the product under consideration (i.e., offline engagement operates similarly to high online engagement). This non-negative bias is due to behavioral effects created by factors such as persuasion by the sales representative, financing deals, channel specific customer lock-in (Neslin and Shankar 2009), and the documented foot-in-the-door effect (e.g., Burger 1999, Freedman and Fraser 1966).

Formally, let $b_{off} \sim U[0, \hat{b}]$ denote the bias introduced by offline engagement and $\tilde{t}_m^{off}$ denote the perceived match value for a type m consumer following offline engagement. This perceived match value is given by

$$\tilde{t}_m^{off} = \min\{m + b_{e_{on}} + b_{off}, 1\}$$

Such that $\tilde{t}_0^{off} = \min\{b_{e_{on}} + b_{off}, 1\}$ and $\tilde{t}_1^{off} = 1$. Note that the bias introduced at the offline engagement stage is assumed to be independent of the bias introduced by high online engagement.

The probability of purchase for a consumer in the offline engagement stage equals $\tilde{t}_m^{off}$, the consumer's perceived match value following offline engagement. Thus, online engagement affects purchase probability both directly via $b_{e_{on}}$, which lingers in the offline stage, and indirectly via its effect on movement down the sales funnel. As we will see, this impact can be substantial.

The expected purchase probability for a consumer with match value m, denoted $\Phi_m^{e_{on}}$, is the probability that the consumer proceeds down the sales funnel to offline engagement, times his expected purchase probability, given that he has moved down the funnel

$$\Phi_m^{e_{on}} = \Pr[\tilde{t}_m^{e_{on}} > T] \cdot E[\tilde{t}_m^{off} \mid \tilde{t}_m^{e_{on}} > T]$$

And the expected mass of purchasers, $Q^{e_{on}}$, is given by:

$$Q^{e_{on}} = \tau\Phi_1^{e_{on}} + (1 - \tau)\Phi_0^{e_{on}}$$

Since we consider consumers who buy one unit of the product at most, $Q^{e_{on}}$ is also the expected level of sales, in terms of number of products sold.

Analysis: When High Online Engagement Reduces Offline Sales

In the above online-to-offline model, high online engagement has two effects: it reduces uncertainty about consumers' match with the product and biases consumers toward the product. While the bias effect drives all consumers to the offline channel, the uncertainty reduction effect only drives offline engagement by consumers who match with the product. Bias and uncertainty-reduction thus exert opposing effects for non-matching consumers' offline engagement. The model and its main intuitions are summarized in the following Table E1.

Table E1. Model Summary and Intuitions					
Match value: m	**Share in population**	**Engagement: e_{on}**	**Perceived match value following online engagement: $\tilde{t}_m^{e_{on}}$**	**Proceed offline $w.p.$ $\Pr[\tilde{t}_m^{e_{on}} \geq T]$**	**Perceived match value following offline engagement: $\tilde{t}_m^{off}$**
$m = 0$	$1 - \tau$	L	$\tilde{t}_0^L = \sigma$	Proceed if uncertainty is high	$\tilde{t}_0^{off} = b_{off}$
		H	$\tilde{t}_0^H = b_H$	Proceed if bias is high	$\tilde{t}_0^{off} = \min\{b_H + b_{off}, 1\}$
$m = 1$	τ	L	$\tilde{t}_1^L = 1 - \sigma$	Proceed if uncertainty is low	$\tilde{t}_1^{off} = 1$
		H	$\tilde{t}_1^H = 1$	Always proceed	

The result is that offline engagement levels and subsequent sales may be higher when online engagement is low. This is the case when the average uncertainty level is high, such that it creates more conversions to offline engagement than the introduction of a positive bias. High uncertainty is a strong driver of conversions when the share of non-matching consumers and the bias introduced in the offline stage are both relatively high. These results are formally derived below.

We begin by deriving the mass of consumers proceeding to offline engagement. This mass is denoted $q^{e_{on}}$ and given by:

$$q^{e_{on}} = \tau \Pr\left[\tilde{t}_1^{e_{on}} > T\right] + (1 - \tau) \Pr\left[\tilde{t}_0^{e_{on}} > T\right]$$

Comparing q^H and q^L we find the conditions for which low online engagement creates higher offline engagement levels. These are summarized in proposition 1.

Proposition 1: Offline engagement may be higher under $e_{on} = L$, when the average σ is relatively high, and higher than the average b_H, and when τ is sufficiently low. The conditions for $q^L > q^H$ are given by:

(a) $\overline{\sigma} > \overline{b} > T$ and $\tau < \frac{T(\overline{\sigma} - \overline{b})}{\overline{\sigma}T - \overline{b}(1 - \overline{\sigma})}$.

(b) $\overline{\sigma} > T > \overline{b}$ and $\tau < \frac{\overline{\sigma} - T}{2\overline{\sigma} - 1}$.

Proof: See Appendix E4.

Quite intuitively, when the share of matching types is low and uncertainty levels are high, consumer uncertainty is a more powerful tool for driving movement down the sales funnel than introducing a positive bias.

This is in line with our empirical results, and specifically, the documented decrease in requests for an offer (*RFO*) following the upgrade to the *HE* website. Recall that an *RFO* is a sales lead that represents interest in moving down the sales funnel. The documented decrease in *RFO*s thus represents a decrease in the number of consumers proceeding from the online engagement to the offline engagement stage, i.e., $q^L > q^H$, which is driven by consumers who do not match with the product, when their share in the population (i.e, $1 - \tau$) is sufficiently high.[26]

[26] Note that for matching individuals, movement down the funnel is always higher under high online engagement.

Such a decrease in the number of consumers proceeding to engage offline with their product of interest may result in an overall decrease in sales. Analyzing the impact of online engagement on offline sales, we note that sales are higher with low online engagement, $Q^L > Q^H$, whenever

(1) $\quad (1-\tau)[\Phi_0^L - \Phi_0^H] > \tau[\Phi_1^H - \Phi_1^L]$

That is, when the expected purchase probability for non-matching consumers is higher under low online engagement (such that $\Phi_0^L > \Phi_0^H$), and their share in the population $(1-\tau)$ is sufficiently high such that overall sales are higher due to this segment's behavior. Further substituting $\tilde{t}_0^H = b, \tilde{t}_1^H = 1, \tilde{t}_0^L = \sigma, \tilde{t}_1^L = 1-\sigma$, and $\tilde{t}_0^{off} = b_{off}, \tilde{t}_1^{off} = 1$ in the expression for $\Phi_t^{e_{on}}$, and using the resulting expected purchase probabilities in (1), we derive the condition for $Q^L > Q^H$:

(2) $\quad (1-\tau)\{\Pr[\sigma > T]\, E\big[b_{off}\big] - \Pr[b_H > T]\, E\big[\min\big(b_H + b_{off}, 1\big) \mid b_H > T\big]\} > \tau\{1 - \Pr[1-\sigma > T]\}$

Intuitively, sales may be higher with a low engagement brand website when: (1) non-matching consumers are a sufficiently large segment, i.e. $(1-\tau)$ is sufficiently high; (2) the threshold T is sufficiently low such that uncertainty can drive some consumers to engage offline, i.e., $\bar{\sigma} > T$ and thus $\Pr[\sigma > T] > 0$; (3) the uncertainty effect is stronger, on average, than the online bias effect, i.e., $E[\sigma] > E[b_H]$; and (4) persuasion of non-matching consumers at the offline stage can generate more sales than would be generated by matching types when engagement is high, i.e., $E\big[b_{off}\big]$ is sufficiently high and enough non-matching consumers engage offline. Proposition 2 shows that $Q^L > Q^H$ is non-empty, by showing that there exist parameter ranges such that condition (2) holds.

Proposition 2: Expected sales may be higher under $e_{on} = L$. Specifically, $Q^L > Q^H$ whenever inequality (2) holds.

Proof: See Appendix E4.

We have thus proposed a general model of the online-to-offline sales funnel for a purely offline product. The model's intuitions provide useful guidelines to consider when high online engagement may or may not be harmful for a product or brand, and may be applied beyond the automobile market.

According to the model, a high engagement brand website may be detrimental to offline sales of products that have a relatively small segment of matching consumers, and for which uncertainty is a strong driver to offline engagement, where the persuasion of non-matching consumers can generate more sales than would be generated by matching types when online engagement is high.

These conditions fit well our setting of a premium car, in that: (1) there are many "non-matching" consumers that are considering a premium car, even though a mid-tier brand is likely a better fit to their budgets and lifestyle; (2) Uncertainty (regarding the actual desirability of a premium car, the added benefits compared to mid-tier competitors, financing options and available discounts) may be a strong driver down the funnel, to a dealership, and more powerful than bias due to a positive site experience; (3) the potential for offline persuasion is particularly high (due to the combination of foot-in-the-door effects and persuasive dealers).

These results are generalizable to products for which uncertainty is a strong driver to offline engagement, and offline engagement may be used to persuade deliberating consumers (such as elective medical procedures, real estate, high-end restaurants). The results further suggest that for products where the above conditions are not met, e.g., when the average uncertainty is low and offline persuasion plays a limited role (such as dining at a McDonald's restaurant) then high online engagement may be beneficial to the brand.

The Impact of Online Engagement on Sales Leads

The above model has been formulated to provide a general framework to consider the impact of online engagement on offline sales for a product that may only be purchased offline. For the sake of generalizability, the model has not been specifically tailored to the details of the sales funnel in the automobile market, and does not explicitly capture the various sales leads present in our data.

We thus offer further discussion on the relationship between our model and empirical results. First, note that the model focuses on consumers' transition from online to offline engagement and thus does not capture an impact of online engagement on *BDs,* which is a strictly online activity. Second, and as noted following proposition 1, *RFOs* represent consumer interest in engaging offline with a sales representative, i.e., their propensity of proceeding to the offline stage. Therefore, the potential of lower offline engagement when online engagement is high demonstrated in the preceding subsection, is in line with the documented decrease in *RFOs* due to the website change.

It remains to relate the model to the observed increase in *TDAs* resulting from the upgrade to the *HE* website. *TDAs* are requests for scheduling a test drive, indicating high purchase intent by consumers who are planning to arrive at a dealership. *TDAs* thus represent a high commitment appointment for offline engagement (rather than a regular indication of movement down the sales funnel).

To capture these in our model, we add a second threshold $\hat{T} < 1$, where $\hat{T} > T$ and the probability of a *TDA* following online engagement is $\Pr\left[\tilde{t}_m^{e_{on}} > \hat{T}\right]$. Table E2 summarizes the *TDA* probability for matching and non-matching individuals under high and low online engagement.

Table E2. Probability of *TDA*.

Match value: m	$\boldsymbol{e_{on} = L}$	$\boldsymbol{e_{on} = H}$
$m = 0$	$\Pr\left[\sigma > \hat{T}\right]$	$\Pr\left[b_H > \hat{T}\right]$
$m = 1$	$\Pr\left[1 - \sigma > \hat{T}\right]$	$\Pr\left[1 > \hat{T}\right]$

We assume that $E[\sigma] > E[b_H]$, such that the effect of uncertainty is a stronger driver of movement down the sales funnel than a positive brand bias.[27] This implies that the probability of performing a TDA is lower for non-matching individuals, and higher for matching individuals when the online engagement is high. An overall increase in the number of *TDAs* will be driven by the segment of matching consumers, when $\hat{T}$ is sufficiently high to provide appropriate screening for this consumer type.

Formally, the number of *TDAs* generated for $e_{on} \in \{L, H\}$ is given by:

$$TDA^{e_{on}} = (1 - \tau)\Pr\left[\tilde{t}_0^{e_{on}} > \hat{T}\right] + \tau \Pr\left[\tilde{t}_1^{e_{on}} > \hat{T}\right]$$

The number of *TDAs* is higher under high online engagement when $TDA^H > TDA^L$, which is equivalent to:

$$\tau\left\{1 - \Pr\left[1 - \sigma > \hat{T}\right]\right\} > (1 - \tau)\left\{\Pr\left[\sigma > \hat{T}\right] - \Pr\left[b_H > \hat{T}\right]\right\}$$

Note that when $\bar{\sigma} \leq \hat{T}$, the $RHS = 0$, and the inequality holds. This represents the extreme case, where only matching consumers conduct *TDAs* (for both high and low online engagement), and their *TDA* activity increases following the website upgrade. By continuity, there exists a range for $\hat{T}$, such that the inequality will continue to hold for $\bar{\sigma} > \hat{T}$, whenever the increase in *TDAs* conducted by matching consumers is sufficiently large. This range of $\hat{T}$ will depend on the relative sizes of $\tau, \bar{\sigma}, \bar{b}$.

We have thus shown that *TDAs* are represented in the model when it is updated to include a second threshold representing a high commitment transition to the offline stage, which is mostly characteristic of consumers who match with the product. Under this specification, the increase in *TDAs* is driven by matching consumers who learn their type following high online engagement, while overall movement down the sales funnel may still decrease, along with offline sales.

Proofs of Propositions

Proposition 1: Offline engagement may be higher under $e_{on} = L$, when the average σ is relatively high, and higher than the average b_H, and when τ is sufficiently low. The conditions for $q^L > q^H$ are given by:

(c) $\bar{\sigma} > \bar{b} > T$ and $\tau < \frac{T(\bar{\sigma} - \bar{b})}{\bar{\sigma}T - \bar{b}(1 - \bar{\sigma})}$.

(d) $\bar{\sigma} > T > \bar{b}$ and $\tau < \frac{\bar{\sigma} - T}{2\bar{\sigma} - 1}$.

[27] This is a necessary condition to ensure that $Q^L > Q^H$ is nonempty. We thus assume it holds throughout this subsection.

Proof of proposition 1: Using the definitions of $\tilde{t}_0^{e_{on}}$ and $\tilde{t}_1^{e_{on}}$, we substitute $\tilde{t}_0^H = b_H, \tilde{t}_1^H = 1$, $\tilde{t}_0^L = \sigma$ and $\tilde{t}_1^L = 1 - \sigma$ in the above equation. This yields $q^H = \tau + (1-\tau)\Pr[b_H > T]$ and $q^L = \tau \Pr[\sigma < 1 - T] + (1-\tau)\Pr[\sigma > T]$. Since $\sigma \sim U[0, \overline{\sigma}]$ and $b_H \sim U\left[0, \overline{b}\right]$:

$$q^H = \begin{cases} \tau & for\ \overline{b} \leq T \\ 1 - \dfrac{(1-\tau)T}{\overline{b}} & for\ \overline{b} > T \end{cases}$$

And

$$q^L = \begin{cases} \tau & for\ \overline{\sigma} \leq 1 - T \\ \dfrac{\tau(1-T)}{\overline{\sigma}} & for\ \overline{\sigma} \in (1-T, T] \\ (1-\tau) + \dfrac{\tau - T}{\overline{\sigma}} & for\ \overline{\sigma} > T \end{cases}$$

Comparing q^H and q^L in the different domains of $\left(\overline{b}, \overline{\sigma}\right)$, we derive the conditions in (a) and (b). ■

Proposition 2: Expected sales may be higher under $e_{on} = L$. Specifically, $Q^L > Q^H$ whenever inequality (2) holds, and (2) is non-empty.

Proof of proposition 2: We show that there exist parameter ranges such that inequality (2) holds.

First note that the $RHS \geq 0$, with $RHS = 0 \Leftrightarrow 1 - T \geq \bar{\sigma}$. Since $T \geq 0.5$, we have $\bar{\sigma} \leq 1 - T \leq T$, which implies $\Pr[\sigma > T] = 0$, and the inequality will not hold. We thus require $\bar{\sigma} > T$, i.e., the threshold T is sufficiently low such that uncertainty can drive some consumers to engage offline.

Considering the LHS, note that if $\bar{b} \leq T$ then $\Pr[b_H > T] = 0$, i.e., bias generated by high online engagement is not sufficient to generate movement down the sales funnel. In this case, $Q^L > Q^H$ when $\bar{\sigma} > T$ and $(1-\tau)\Pr[\sigma > T]\, E\left[b_{off}\right] > \tau\{1 - \Pr[1 - \sigma > T]\}$.

Otherwise, if $\bar{b} > T$, then $\Pr[b_H > T] > 0$, and we derive conditions to ensure that the $LHS > 0$.

Using the upper bound $E\left[\min\left(b_H + b_{off}, 1\right) \mid b_H > T\right] < 1$, we note that

$$LHS > (1-\tau)\left\{\Pr[\sigma > T]\, E\left[b_{off}\right] - \Pr[b_H > T]\right\}$$

Therefore, $E\left[b_{off}\right] > \frac{\Pr[b_H > T]}{\Pr[\sigma > T]}$ is a sufficient condition for $LHS > 0$, and we require $E[\sigma] > E[b_H]$ since $b_{off} \leq \hat{b} < 1$.

Summarizing, (2) is non-empty for:

A. $\bar{\sigma} > T \geq \bar{b}$, when $(1-\tau)$, $E[\sigma]$, and $E\left[b_{off}\right]$ are sufficiently large.
B. $\bar{\sigma} > \bar{b} > T$, when $E\left[b_{off}\right] > \frac{\Pr[b_H > T]}{\Pr[\sigma > T]}$, $E[\sigma] > E[b_H]$ and $(1-\tau)$, $E[\sigma]$, and $E\left[b_{off}\right]$ are sufficiently large.

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