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The Dark Side of Mobile App Adoption: Examining the Impact on Customers' Multichannel Purchase

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Abstract

Firms use mobile applications to engage with customers, provide services, and encourage customer purchase. Conventional wisdom and previous research indicate that apps have a positive effect on customer spending. The authors critically examine this premise in a highly competitive institutional context by estimating how customers' multichannel spending changes after adopting a hotel group's app and identifying the factors contributing to such change. Exploiting the variation in customers' timing of app adoption and using a difference-in-differences approach, the authors find that the effect of app adoption on customers' overall spending is significant and negative. Additional analyses suggest the possibility that customers who adopt the focal app also adopt competitor apps and therefore search more and shop around, leading to decreased share of wallet with the focal hotel group. The authors also find that the negative effect on spending is smaller for customers who use the app for mobile check-in service than those who use the app for only searching, highlighting the role of app engagement in mitigating the negative effect.

Keywords

difference-in-differences, mobile apps, mobile marketing, multichannel marketing

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Mobile apps have become a mainstay of firms' omnichannel strategy to interact directly with customers, provide synergistic value to customers at all stages of the customer journey, and build customer loyalty (Boyd, Kannan, and Slotegraaf 2019). It is easy to see why. For example, 80% of mobile device owners in the United States performed mobile shopping activities via app in a given month (Sabanoglu 2020). It is not surprising, therefore, that mobile apps have become a major source of retail e-commerce revenues.

Given the ubiquitous use of mobile apps, an obvious question from a firm's perspective is whether an introduction of a mobile app provides the necessary return for the investment. Viewing the mobile app as an additional channel with anytime, anywhere access, the general expectation arising from multichannel studies is that the apps will bring in additional revenue not only through customers spending via the app but also through increased customer spending across all channels (Montaguti, Neslin, and Valentini 2015). However, we cannot simply extend the findings in other channels to mobile apps, because different channel features could lead to different consumer behavior. In particular, because the mobile device is accessible anytime and anywhere, it is ideal for information searches in the early shopping stage and is more frequently used on the go, in a public location (e.g., in stores), or for impulse purchases as compared with other channels (De Haan et al. 2018). Moreover, mobile apps can incorporate new technologies and deliver innovative services that cannot be replicated in other channels (e.g., keyless entry to hotel rooms using apps, app-based augmented reality).

Many recent studies have examined the issue of whether consumers' overall spending with a firm increases after they adopt the firm's mobile app (Table 1). All these studies show that the effect of mobile app adoption on overall customer spending is positive after controlling for customer self-selection and other biases. Many of these studies are in the retail context, such as online grocery, video games and electronics, office supplies and household appliances, and retail coalition loyalty programs. The exception is the study by Gill, Sridhar, and Grewal (2017), which is in the business-to-business (B2B) context, with a manufacturer introducing the app. However, all these studies share some common features in

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Table 1. Comparisons Between Prior Research and Our Research.

Research	Product Category	Effect of App Adoption on Purchases	App Market Competition Intensity	Ease of Using Mobile Device for Product Research
Kim, Wang, and Malthouse (2015)	A coalition loyalty program allowing customers to earn reward points for purchasing at partnering sponsors	Positive	Low	Low
Wang, Malthouse, and Krishnamurthi (2015)	An online grocery store	Positive	Low	Low
Huang, Lu, and Ba (2016)	An e-retailer of electronics, office supplies, and household appliances	Positive	Medium	Low
Gill, Sridhar, and Grewal (2017)	A manufacturer of tooling and industrial materials (B2B)	Positive	Low	Medium
Narang and Shankar (2019)	A retailer of video games, consumer electronics and wireless services	Positive	Medium	Low
The current research	A hotel group	Negative	High	High

Notes: B2B = business to business.

their research contexts—specifically, they are characterized by low to medium levels of competition in their app markets and fewer introductions of apps in their respective verticals. For example, there is limited competition for the online grocer (Wang, Malthouse, and Krishnamurthi 2015) and coalition loyalty program (Kim, Wang, and Malthouse 2015) ties in a network of retailers. The B2B study (Gill, Sridhar, and Grewal 2017) involves a monopoly context, where the options to switch apps/firms are limited for customers. The mobile app is a fairly convenient and suitable tool for product research in the category of travel products. Given that mobile screens are generally smaller than computer screens, customers usually have higher search cost using mobile devices for many product categories. However, for the travel products, customers would especially appreciate the mobility feature of mobile devices, which makes it possible for them to do product and price comparisons on the go.

In this article, we examine app adoption by customers in a highly competitive setting, in contrast to the contexts studied in prior research. We focus on mobile app adoption by customers of an international hotel group in a highly competitive vertical market in which competitive hotel groups and travel intermediaries have also introduced mobile apps. Will the introduction of a mobile app in such a setting lead to similar positive returns as observed in prior research? Or, given customers' tendency to search extensively in this market and the ease of adopting and switching among different mobile apps, could it lower the switching and search costs? Could the resultant increased competitive intensity lead to an overall decrease in customer spending and negative returns? These are the possibilities we explore as we analyze customers' mobile app adoption and its impact on customers' overall spending across all channels. We leverage a unique and large data set from the international hotel group that introduced its mobile app in August 2011. This data set covers a multiyear period between 2011 and 2015 and contains rich information about hotel reservations, customer app adoption and usage, and customer demographics. Drawing a random sample of 60,000 customers who ultimately adopted the app at six different points in time, we track the total and channel-specific spending of each customer and examine the effect of customer app adoption on their total spending with the firm as well as their spending in each channel.

A major challenge in determining how a customer's app adoption affects their spending patterns using a regression model framework is that customers self-select to adopt the mobile app. We adopt a difference-in-differences (DID) modeling approach to control for the biases. Specifically, we compare the spending of a group of customers who adopted the app at a specific point in time (treatment group) with the spending of a group of control customers who had not adopted the app at that time (but did adopt the app at a much later time). This design eliminates the impact of time-varying factors that are common to both groups (e.g., seasonality, exogenous shocks) and time-invariant factors at the customer level (e.g., business travelers, leisure travelers) on customer spending. It also accounts for time-invariant factors that affect customer app adoption decisions, as both the treatment and control groups adopted the app, albeit at different times. To account for self-selection of customers, we augment the DID approach with the Heckman-type correction and address selection on both observables (e.g., customers' past purchase pattern) and unobservables (e.g., idiosyncratic demand shock at the customer level). We also use propensity score matching (PSM) to increase the comparability between the treatment group and the control group based on customer preadoption behavior and characteristics. Finally, we replicate the analyses using multiple treatment–control pairs spread over time to account for the effect of app upgrades and conduct a variety of robustness checks.

From our analysis, we find that, contrary to extant research, customers' app adoption is negatively correlated with their total spending with the hotel group in all the treatment–control pairs we test. Although there are segments of customers for whom the app adoption can lead to spending increase (e.g., business travelers), overall the average effect is negative, especially for customers with low-level loyalty membership and customers who use the firm's nondigital channels for booking. Our results indicate that app adoption may not always lead to an increase in customer total spending, and it may depend on the specific industry and institutional context. To better understand the role of the institutional context and possible mechanisms that could drive the results, we perform additional analyses using app market information and app usage data. Many alternative explanations could exist for the result driven by the institutional context. For example, customers' need to search extensively in an intensely competitive market and the low switching cost across competitive apps could increase competition across hotel groups and travel intermediaries. This could lead to customers shopping around using different apps, thereby becoming less loyal to the focal firm and possibly decreasing their share of wallet. Alternatively, it could be the case that customers naturally become more tech-savvy as they adopt and use more hotel and travel apps and, thus, allocate their spending among more firms.

Our article primarily contributes to the emerging stream of research on mobile applications and distinguishes from prior research in several ways. Most importantly, our findings suggest a strikingly negative effect of app adoption on customer spending, which contrasts with previous findings, and we highlight that firms cannot take positive returns for granted. In addition, we suggest that the highly competitive nature of the market, the importance of search, and the derived nature of demand, all driven by the institutional context, could be reasons for such a negative outcome. However, from a substantive viewpoint, we note that there could still be some benefits of app introduction for firms, as we show that mobile apps help customers connect directly with the firm and thereby reduce their share of spending with travel intermediaries and other third-party agents and increase their share of the hotel group's channels. Finally, we provide managerial suggestions that firms could attenuate the negative impact of app adoption on spending by encouraging app engagement based on additional analyses.

Institutional Context and Background

Our focal firm of study is one of the largest international hotel groups worldwide. Given the informational nature of the purchase of hotel room nights, many of the hotel chains benefited from moving online with the advent of the internet. However, it also led to the emergence of online travel agencies (OTAs), which have become dominant in the market over the years. The industry has become very competitive in the last two decades, fueled by consumers switching to online channels for hotel booking and using search engines and OTAs to get information and better rates. With online reviews becoming common, OTAs were able to become the primary point of entry for consumers searching for hotels, and they are responsible for leveling the playing field for smaller no-name independent hotels and further increasing competition (Hollenbeck 2018). From 2011 to 2015, the ratio of bookings through direct channels to bookings through third-party channels for rooms over $100 per night fell from 4.3 to 2.7 in the United States, highlighting the intense competition (Green and Lomanno 2016). To counter the threat of OTAs, major hotel groups strengthened their loyalty programs to encourage customers to interact with their direct channels rather than use third-party channels such as the OTAs. With the advent of smartphones, mobile websites became common, but their user-friendliness for extensive search and booking remained at a level lower than that of computers. Mobile apps changed that.

The relatively low cost of development, low entry barriers, and the quick development timelines (Anthes 2011) enabled both major hotel groups and OTAs to introduce new and updated versions of their apps quickly. Two major hotel groups introduced mobile apps in early 2010, targeting their loyalty program members to strengthen customer relationships and reduce OTAs' chances of making inroads into their customer base. The OTA Booking.com (strong in European markets) introduced its mobile app in mid-2010, Orbitz followed suit in late 2010, and Expedia introduced its app in mid-2011. The OTA apps were generally of higher quality than the hotel apps and provided more features. The focal hotel group introduced its app in August 2011, and another major competing hotel group also introduced an app in late 2011, each of them making its app exclusive for its loyalty program members (Mankad, Hu, and Gopal 2018). Within a year, there were seven major apps in the market, four from large hotel groups and three from major OTAs, making the app environment very competitive. Between 2012 and 2013, other three major competing hotel groups also introduced their apps, further increasing competition in the hotel app market.

Customers typically use mobile apps to search for hotel properties and prices, read reviews, make and review bookings, and manage their loyalty accounts. The expectation of hotel groups is that these app investments will translate into enhanced shopping and consumption experiences and thereby increase customer bookings, loyalty, and retention. There is indeed strong evidence for this expectation based on mobile app studies in other product categories (Gill, Sridhar, and Grewal 2017; Huang, Lu, and Ba 2016; Kim, Wang, and Malthouse 2015; Narang and Shankar 2019; Wang, Malthouse, and Krishnamurthi 2015) as well as general multichannel studies (Montaguti, Neslin, and Valentini 2015; Venkatesan, Kumar, and Ravishanker 2007). However, the environment is more fluid and competitive in the hospitality category because the search cost is lowered by multiple app introductions and the low switching costs encourage customers to adopt multiple hospitality apps.

There are several reasons to question whether all hotel groups will have similar gains in customer spending as a result of app introduction, even with firms targeting their loyalty program members exclusively with the mobile apps. First, in our institutional context, mobile apps are a convenient tool for customers to search for hotels, check room availability, and compare hotel characteristics and prices. Thus, by adopting and using one app, customers realize its value and, given the low switching costs, start adopting and using other apps to facilitate their shopping process across multiple hotel groups and OTAs. When customers shop around, they could reduce their share of wallet with a particular hotel group. Court et al. (2017) provide empirical evidence for this possibility and highlight that mobile shopping apps and new technologies have changed the customer decision journey at the product consideration and purchase stages. Among the 30 product categories they studied, they find that only three were loyalty driven, with customers repurchasing the same brand rather than shopping around. Therefore, from a hotel group's perspective, the competitive nature of the app environment in the hospitality category could be a limiting factor on the growth of customer spending, which they could expect with the app launch.

There is another characteristic of our institutional context that may determine the impact of apps: the derived nature of demand. The primary demand for travel could be conferences, business meetings, holiday travel, and so on, which gives rise to the demand for hotel rooms. Given this demand, the problem becomes allocating the share of the overall demand across different hotels, which depends on the number of business or leisure trips. Unlike retail categories, in which impulse purchases are more likely and the budget for spending is a softer constraint, customers generally do not extend their stay longer because hotel rate is low, nor do they make hotel reservations on impulse. In addition, given the constraint on travel time, hotel reservations and stays cannot be advanced simply because there is a good deal, as the hotel stays cannot be inventoried as retail products. Thus, we may not see an uptick in sales merely due to mobile app adoption, as we might observe in other categories. In the following sections, we undertake a systematic analysis to determine the exact nature of the impact of mobile app introduction by the focal firm using a DID approach and explore the various mechanisms that could be contributing to the results through extensive descriptive analyses.

Data Description

Our data are provided by a leading international hotel group based in the United States. The data contain detailed information on customers' hotel reservations, mobile app usage, and demographics from January 2011 to February 2015. Our focal hotel group launched its mobile app in August 2011, allowing customers to search for hotel rooms, make reservations, check upcoming reservations, and manage loyalty membership accounts. All customers in our data set adopted the hotel's mobile app before February 2015. During our data period, the hotel group did not provide monetary incentives such as deals or discounts for customers to adopt the mobile app. To promote the app, the hotel group introduced the app in reminder emails that were sent to customers who had upcoming reservations.

The app was improved over time with a series of version releases and a variety of added features. In particular, the firm announced a major app update in June 2013: the introduction of the mobile check-in feature. This app service feature allows customers to check in and check out at hotels using the app. When the room is ready, the customer will be automatically notified by an in-app alert and can avoid waiting in line at the front desk by picking up their key at a mobile check-in kiosk. At the end of their stay, the customer can check out via the app and receive a copy of the bill by email, again without waiting in line at the front desk. The hotel group aims to use the mobile check-in service as an important feature for enhancing their customers' stay experience. In our data set, we can observe whether each customer used the mobile check-in feature in any given month.

To examine the change in customer spending after app adoption, we specifically focus on existing customers of the focal hotel group rather than including customers acquired during our data period. This restriction ensures that we observe customers' spending prior to their app adoption. The total number of existing customers who adopted the mobile app before February 2015 is 1,085,609. We define six customer cohorts on the basis of customers' timing of app adoption: August 2011, April 2012, January 2013, October 2013, June 2014, and February 2015. Within each cohort, all customers adopted the app in the same month. We then construct our sample by randomly drawing 10,000 customers from each of the six cohorts, adding up to a total number of 60,000 customers. The average number of reservations by all customers from the six cohorts and the average number of reservations by the randomly selected 60,000 customers are very similar, indicating that our random sample is representative of the original data set.

We define customer cohorts on the basis of the timing of app adoption because we aim to identify the effect of app adoption for these cohorts by treating each of them as a treatment group and those cohorts that have not adopted the app at that time as control groups. We select the six cohorts in such a way that the six adoption months are spread over the entire observed time period of customer app adoption between August 2011 and February 2015 and cover the releases of different versions of the app. This rules out the possibility that our estimated effect could be biased due to various time-related shocks such as seasonality, app quality changes, and so on. In the next section, we explain the details of constructing the treatment groups and the control groups.

In total, the 60,000 customers made 1,937,124 reservations with the focal hotel group during our data period. Figure 1 shows the average monthly spending of each customer cohort. First, we can see a positive relationship between customers' timing of app adoption and their initial average spending level. That is, earlier app adopters have higher spending levels than the later adopters. In addition, the average customer spending of each cohort increases before the app adoption. It implies that, on average, customers are motivated to adopt the app when they make a reservation because they believe they will have the opportunity to use it during their upcoming stay. However, this does not mean that every customer spent at their highest level when they adopted the app, and the spending spikes shown in Figure 1 are rather due to the so-called

Figure 1. Average monthly spending by cohort ($).
Notes: The vertical dashed lines denote the app adoption month of each cohort.

aggregation bias. The Web Appendix provides detailed explanations about this issue.

Our customers' spending data include hotel reservations through both the firm-owned channels and the third-party channels. We refer to firm-owned channels as direct channels, which are directly owned and operated by the hotel group. The direct channels include the hotel's nonmobile website, property and call center, and mobile app and mobile website. The focal hotel group sets the same price and assortment across all its direct channels. The third-party channels include the global distribution system (GDS), which is primarily used by travel agents; OTAs such as Expedia and Travelocity; and other channels operated by third-party corporations. On average, 63% of the reservations were booked through direct channels as opposed to 37% through third-party channels.

Empirical Analysis

The objective of our research is to estimate the effect of customers' app adoption on their purchases (total spending). The

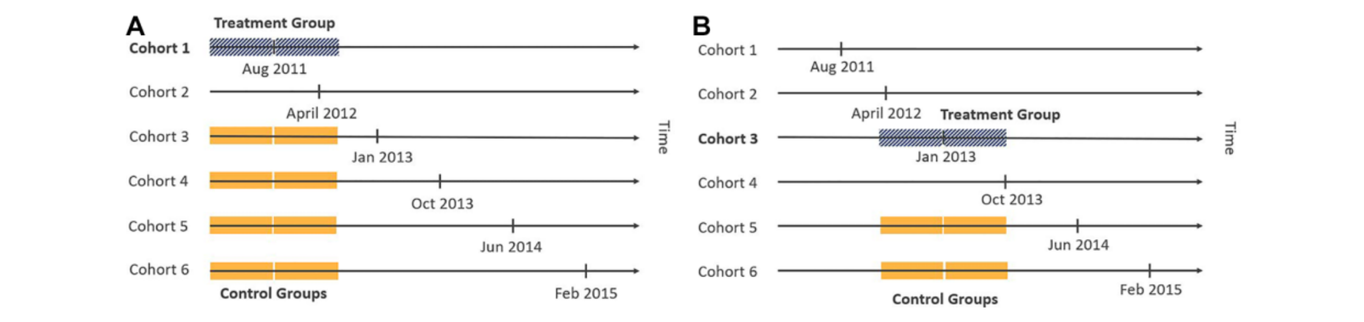

Figure 2. Example of multiple treatment and control groups.

dependent variable is the individual monthly spending constructed from the records of hotel reservations. Ideally, we are interested in comparing the spending of app adopters after the adoption to their spending if they had not adopted the app. However, the latter is a counterfactual that we cannot observe. Instead, we construct the treatment groups and the control groups using customer cohorts and then estimate the change in customer spending after app adoption by using the DID approach, which is an empirical methodology widely applied in the field of marketing (e.g., Chevalier and Mayzlin 2006; Goldfarb and Tucker 2011). To reduce self-selection bias, which is a major concern of many observational studies, we use a formal Heckman-type correction to address selection on both unobservables and observables. In addition, we use propensity score methods to match customers on the basis of their observed attributes to increase the comparability between customers in the treatment group and the control group. Our analyses in the following subsections move from a pure correlational inference toward a more causal inference. However, the app adoption decision by customers is not controlled in our research, and therefore we cannot guarantee a causal inference given the observational nature of our data and associated limitations.

Treatment and Control Groups

We have six customer cohorts that have adopted the hotel app at one time or another. For each cohort and their corresponding month of app adoption, we compare them with customers in the other cohorts that have not adopted the app yet. For example, customers in Cohort 1 adopted the app in August 2011, whereas the other cohorts had not adopted yet at that time. Therefore, we can compare customers in Cohort 1 with those in Cohorts 3, 4, 5, and 6 as Figure 2, Panel A, shows: we use Cohort 1 as the treatment group and the other cohorts as the control groups, and the analysis window is defined around the month in which Cohort 1 adopted the app (August 2011). Moreover, whereas we use Cohort 3 as a control group for Cohort 1, we also use Cohort 3 as a treatment group and compare it with Cohorts 5 and 6, as we show in Figure 2, Panel B, and the analysis window is defined around January 2013 (the app adoption month of Cohort 3).

Essentially, we compare the app adoption group (treatment) with those who have not adopted the app at that time point (control). Because both the treatment and control groups end up adopting the app, the comparison between these two groups accounts for any unobserved time-invariant factors that drive customers to eventually adopt the app (Jung et al. 2019). This is not the case with many of the other analyses in extant literature (e.g., Gill, Sridhar, and Grewal 2017; Huang, Lu, and Ba 2016; Kim, Wang, and Malthouse 2015; Narang and Shankar 2019). In addition, we show a more reliable treatment effect of app adoption than prior research in the sense that we compare a treatment group against multiple control groups as well as have multiple treatment groups that adopted the app at different times over a wide time range.

It is worth noting that we do not pair a treatment group (e.g., Cohort 1) with its closest cohort (e.g., Cohort 2) because the time gap between the two app adoption months (e.g., August 2011 and April 2012) is too narrow, such that the posttreatment period is very close to the adoption month of the control cohort. Thus, the spending of customers in the control group could be affected by their own impending app adoption decision and disqualifies the control group from being a good counterfactual for the treatment group. However, in some sense, comparing the closest cohorts could be advantageous, as these cohorts are similar in terms of the unobservable factors affecting adoption. Therefore, we also compare the closest cohorts as a robustness check and find consistent results. Overall, we construct ten pairs of the treatment and control groups: Cohorts 1 and 3, Cohorts 1 and 4, Cohorts 1 and 5, Cohorts 1 and 6, Cohorts 2 and 4, Cohorts 2 and 5, Cohorts 2 and 6, Cohorts 3 and 5, Cohorts 3 and 6, and Cohorts 4 and 6.

Difference in Differences

We employ the DID approach, which essentially compares the change in spending of customers in the treatment group before and after app adoption with the change in spending of customers in the control group (Angrist and Pischke 2008). The change in spending of the control group is an estimate for the counterfactual change in spending of the treatment group in the absence of mobile app adoption. Thus, the treatment effect identifies whether the app adopters would have spent more (positive effect) or less (negative effect) than their actual spending if they had not adopted the app.

The comparison between these two changes enables us to account for observed and unobserved time-varying factors affecting spending that are common to all customers. A few examples of the unobserved time-varying factors include advertisement, economic shocks, and seasonal trends. Moreover, the DID approach addresses an important methodological concern—that is, the unobserved customer heterogeneity may lead to different levels in customer spending. For example, business travelers generally travel more frequently than leisure travelers and thus spend more on hotel stays. In this case, customers' traveler type could affect their spending level, but we do not observe this in our data. By taking the difference between spending before and after app adoption of the same customer, the DID approach eliminates individual-specific fixed effects that influence spending (Cameron and Trivedi 2005, pp. 768–70).

The DID model is formally given by the following specification. For customer i in period t,

$$y_{it} = \beta_0 + \beta_1 \, \text{adopt}_i + \beta_2 \, \text{post}_t + \beta_3 (\text{adopt} \times \text{post})_{it} + \mathbf{x}_{it}\gamma + \mathbf{w}_i\theta + \epsilon_{it}, \tag{1}$$

where y_{it} is the log value of spending of customer i in period t. The indicator variable adopt_i takes on a value of 1 if customer i adopted the app in the postadoption period and 0 otherwise. post_t is an indicator variable that takes on a value of 1 for the postadoption period and 0 otherwise. The coefficient of interest is β_3, which is the DID estimate of the treatment effect of app adoption on spending. In addition, we control for several observables to augment the DID model: $\mathbf{x}_{it}$ represents a vector of reservation characteristics that vary across both customer and time, and $\mathbf{w}_i$ is customer i's demographics that do not vary over time. For example, customers who make reservations for luxury hotels could have higher spending. So, we include the variable "luxury brand" to account for this. Finally, ϵ_{it} is the error term, which is assumed to be correlated across periods for each customer and independent across customers.

There are two periods: $t = 0$ denotes the period before the treatment group adopts the app, and $t = 1$ denotes the period after the treatment group adopts the app. For each treatment–control group pair, we define the preadoption period as the six months prior to the adoption month and the postadoption period as the six months after the adoption month. We do not include the month of app adoption in our analysis window for two reasons. First, we observe the adoption month but not the adoption date. As a result, we are not able to distinguish which portion of customer spending in the adoption month occurs before adoption and which portion occurs after adoption. The treatment effect could be biased if we mistakenly include customers' preadoption spending as postadoption spending or vice versa. Second, Figure 1 exhibits extraordinary spikes of spending in the adoption months, which could also produce bias.

We use a standard two-period DID model because a multiperiod DID model is likely to suffer from a severe serial correlation problem, which could produce biased standard errors (Angrist and Pischke 2008). Bertrand, Duflo, and Mullainathan (2004) provide several correction methods that can be applied to different situations depending on the length of the time series and the number of test units. Given that our time series is only 12 periods and is too short to provide reliable estimates of the data-generating processes, we follow one of Bertrand, Duflo, and Mullainathan's proposed methods, averaging the data before and after the adoption, and run a cross-sectional DID model with two periods.

A critical identifying assumption of DID is that the change in spending of customers in the control group should represent the counterfactual of customers in the treatment group. Although it is not possible to test this assumption directly, we can check whether the preadoption spending of customers in the treatment group shares a similar time trend with that of customers in the control group. If the spending of customers in the two groups follows a similar trend before app adoption, we would expect the trends of spending to continue developing similarly in the postadoption period if customers in the treatment group had not adopted the app. We evaluate the preadoption time trends for the control and treatment customers through a graphical inspection as well as a panel model estimation and find no noticeable difference between the two trends (see the Web Appendix).

Modeling Self-Selection

The major challenge for an unbiased estimation of treatment effect arises from the fact that customers self-selected themselves to adopt the mobile app. The DID approach does not control for unobserved individual factors that may affect customer spending as well as app adoption and, thus, create endogeneity bias. Therefore, we have to model self-selection formally to control for any potentially remaining bias. Specifically, we use the Heckman-type correction by Heckman (1979), which is described by a selection equation (app adoption) and an outcome equation (spending). The selection equation is a binomial probit model that characterizes the customers' decision to adopt the mobile app as a function of observed covariates prior to the adoption, $\mathbf{z}_i$:

$$\text{Prob}(\text{adopt}_i = 1) = \mathbf{z}_i\alpha + e_i. \tag{2}$$

Then, we calculate the inverse Mills ratio (IMR) for each customer i using the probit model in Equation 2 and augment our DID model in Equation 1 (outcome equation) with the IMR_i as a covariate. Instead of augmenting Equation 1 directly with IMR_i (Gill, Sridhar, and Grewal 2017; Narang and Shankar 2019), we take the difference of Equation 1 evaluated at the preperiod ($t = 0$) and the postperiod ($t = 1$) and augment the differenced equation with IMR_i to estimate the treatment effect (see Semykina and Wooldridge 2013). The coefficient estimate of IMR_i being significant indicates the existence of selection bias. Finally, the error term in the outcome equation and the error term in the selection equation are correlated and follow a bivariate normal distribution.

Instrumental variables are required to identify the two equations. To specify appropriate instrumental variables, it is important to understand the nature of unobservables in the outcome equation. For instance, such unobservables can be customers' deal proneness, tendency to search around, search costs, and so on. These factors could lead customers to search more, find better prices, and, thus, spend less. However, it could be argued that deal-prone customers and customers who tend to search around are also likely to spend more with the firm by purchasing more room nights on deal at the expense of competitive hotel groups. Thus, spending could be affected in either direction. At the same time, deal-prone customers and customers who tend to search around are more likely to adopt apps because they can use the app to find information more easily. Note that while these unobservables are customer specific, they could also change over time—that is, customers could become more deal prone and/or search around more over time. Because our selection model is static, we cannot capture these dynamics, but estimating the selection model for multiple cohort pairs over time can provide us snapshots of these changes at the customer level, which the IMR will capture.

We use three instrumental variables that enable us to capture the effect on app adoption without affecting the customer spending. First, we use a set of dummy variables capturing customers' mobile device type (e.g., iPhones, Android phones). Mobile apps generally differ across devices in terms of their launch date, version, size, rating, and so on and thus provide different app usage experience. Each pair of treatment and control cohorts focuses on a specific month of adoption, and the differences in app release status or app version can explain who did and did not adopt the app in that specific month. Because the analyses for different cohort pairs are done across time in various adoption months, the mobile device variables also capture such time-related variation that can affect adoption. However, the mobile device type is unlikely to affect customer hotel spending conditioned on the reservation characteristics such as hotel brand levels, which account for customer wealth and income level.

Second, we include a dummy variable indicating whether a customer used the hotel group's mobile website for hotel reservations before app adoption. This variable captures the unobserved customer preference toward mobile device usage for hotel bookings. Customers who access the mobile website of the focal hotel group using their smartphones and make bookings using this site are already highlighting the convenience advantage that the mobile device provides for them. So, we expect this behavioral variable to positively affect customers' adoption decision of the focal hotel app. While prior mobile website usage is likely to be correlated with unobservables such as customers' tendency to use multiple channels to shop, it should not have an effect on the spending levels. This is because using multiple channels could lead customers to spend more (Montaguti, Neslin, and Valentini 2015) or enable them to find better deals and reduce spending amount. In addition, the mobile website accounts for only 6.25% of total spending of customers who used the mobile website for hotel booking before app adoption.

Third, we use the length of booking window, which is the number of months between the date of making the reservation and the date of checking in at the hotel. The app is a convenient tool for customers to plan and manage their travels. So, one could argue that customers who like to make their travel plans early (longer booking window) are more likely to adopt the app. However, the length of booking window could affect spending in either direction. On the one hand, customers who book early may spend less because they get better rates. On the other hand, customers who book early may spend more because they get a lot of deals and increase their share of wallet with the focal hotel group at the expense of competitors. To test for the strength of the three instruments, we perform likelihood ratio tests and find significant results. In addition, we find that the three instruments provide unique variation to predict the app adoption decision, as they clearly increase the pseudo R^2 of the selection equation.

In addition to the instruments, we include a set of reservation characteristics (e.g., hotel brand level, hotel location) and customer characteristics (e.g., customer loyalty membership, age, gender). These variables describe customer profiles and can predict customers' future interactions with the firm. We also use them in the outcome equation. Finally, we use several additional covariates that are not included in the outcome equation to improve the prediction of app adoption. First, we include the share of spending through each shopping channel. One would expect that customers who mostly use the digital channels are more likely to adopt the app. Second, we include the number of room nights because frequent travelers may value the hotel's app more highly given that they have more opportunities to use it than infrequent travelers. Third, to capture the predictive power of customers' tendency to increase their spending prior to adoption (as shown in Figure 1), we include the following variable: $\Delta\,\text{Spending} = \text{Spending}_{m-1} - \text{Spending}_{m-4}$, where m is the month of adoption.[1]

Increasing Comparability Between Treatment and Control Groups

Another concern that could complicate our DID identification strategy is that the treatment effect may vary across customers as a function of observed covariates. For example, the treatment effect of app adoption may be more consequential for customers who stay at the focal hotel frequently than for those who stay occasionally. Suppose there are some particularly frequent customers among those who adopted the app (treatment groups). Problems may emerge for our DID estimates if

[1] For robustness check purposes, we estimate the DID model with Heckman-type correction using three model specifications: (1) Δ Spending is not included in either equation, (2) Δ Spending is included in the selection equation only, and (3) Δ Spending is included in both equations. The three models generate very similar treatment effects and different IMR coefficients.

there are no comparable frequent customers among those who did not adopt the app (control groups).

Our task is then to increase the comparability between the treatment group and the control group and thereby eliminate bias in the estimated treatment effects due to observable heterogeneity. In principle, we would like to construct a control group by finding nonadopters that have similar observed attributes to those of the app adopters. However, the "curse of dimensionality" problem will arise if the vector of observed attributes $\mathbf{z_i}$ is high-dimensional. Instead of matching on a high-dimensional vector $\mathbf{z_i}$, we use PSM methods to match on a scalar—the "propensity score" $P(\mathbf{z})$ (Rosenbaum and Rubin 1983).

The propensity score $P(\mathbf{z})$ is the probability of the customer adopting the mobile app in our context. We estimate the propensity score for each customer by estimating the predicted values of the adoption probability from a binomial logit model as a function of the customer's historical shopping behavior and individual characteristics.

$$P(\mathbf{z}_i) = \text{Prob}(\text{adopt}_i = 1|\mathbf{z}_i) = \mathbf{z}_i\alpha + \xi_i, \tag{3}$$

where the error term ξ_i follows a standard logistic distribution. $\mathbf{z}_i$ is a vector of preadoption covariates that is identical to what we use in the selection equation of Heckman-type correction. Customers' past purchase behavior is an important predictor of their app adoption decision and is commonly used for matching in previous literature (Huang, Lu, and Ba 2016; Kim, Wang, and Malthouse 2015; Wang, Malthouse, and Krishnamurthi 2015). To clarify, the matching variables $\mathbf{z}_i$ are constructed using data from the beginning of our sample period, January 2011, to the month prior to the treatment cohort's adoption. In a robustness check, we perform the matching using only the first six months of our sample period (January 2011 to June 2011) and find consistent results.

Then, using the estimated propensity scores $\hat{P}(\mathbf{z})$, we identify the control and treatment customers on the basis of how sufficiently close the scores are across the two groups. As a common practice, we exclude all control and treatment customers whose propensity score lies outside the common support of the propensity scores of the two groups. Then we use 1:1 matching by finding the nearest neighbor in terms of $\hat{P}(\mathbf{z})$ in the set of nonadopters to each of the adopters. Matches were accepted only if the nonadopter is within .01 units of standard deviation of $\hat{P}(\mathbf{z})$ from an adopter. As a result, we dropped approximately 30% of the customers from the original sample.

The PSM method ensures that all customers are similar in terms of the predicted probability of app adoption based on the observed attributes—customer past purchase behavior and demographics. We first check the quality of PSM by comparing the distribution of $\hat{P}(\mathbf{z})$ of the treatment group and that of the control group before and after matching. By visual inspection, there was negligible difference in the distribution of propensity scores of the two groups after matching. Second, we perform a Kolmogorov–Smirmov test to verify that the distribution of estimated propensity scores of customers from the treatment and the control groups are roughly the same after matching. Finally, we check the mean values of the covariates of the treatment group and the control group and find that the covariate balance is fairly good after matching (we report details in the Web Appendix).

Results

Treatment Effects on Customer Spending

We present the estimated treatment effects of the standard DID model, the DID model combined with Heckman-type correction, and the DID model on matched samples in Table 2. The results are for customers' total spending across all channels as well as spending through the direct channels and spending through the third-party channels. Each of the ten columns reports the treatment effects based on one treatment–control group pair. For customers' total spending across all channels, we also report the full list of covariates and estimation results of the outcome equation of the DID model with Heckman-type correction in Table 3.

Most importantly, we find a very strong negative effect of customer app adoption on customers' total spending from all model specifications across all treatment and control groups. In general, the treatment customers have an over 10% negative effect on their total spending after app adoption compared with the control group. The magnitudes of the treatment effects are different across cohorts, which could relate to the fact that the focal app was upgraded over time and each treatment cohort adopted different versions of it. Yet the DID estimates remain consistently negative. Note that the treatment effect identifies whether the app adopters would have spent more (positive effect) or less (negative effect) than their actual spending if they had not adopted the app. So, even if the treatment effect is negative, the app adopters' spending may still increase over time because they may have spent more if they had not adopted the app.

From a channel-specific perspective, the treatment effects for both the direct channels and third-party channels are mostly negative. Furthermore, the negative effects of app adoption are stronger on the third-party channels than on the direct channels. As a result, there is a positive association between the share of spending through direct channels and app adoption. This indicates that app introduction could still provide some benefits for firms, as mobile apps help customers connect directly with the firm and thereby reduce their share of spending with travel intermediaries and other third-party agents. We also report the various treatment effects for individual shopping channels in Table 4. The treatment effects are significant and negative for the firm's online website, which is the major booking channel of the focal hotel group. For most customer cohorts, the treatment effects for the property/call center and GDS are negative. However, there are generally insignificant treatment effects for the OTAs.

Treatment Effects by Customer Segments

Although our main analysis shows an average negative effect based on all customers, we wonder whether there is

Table 2. Treatment Effects of Customer App Adoption on Spending.

	(1) Cohort 1 vs. 3	(2) Cohort 1 vs. 4	(3) Cohort 1 vs. 5	(4) Cohort 1 vs. 6	(5) Cohort 2 vs. 4	(6) Cohort 2 vs. 5	(7) Cohort 2 vs. 6	(8) Cohort 3 vs. 5	(9) Cohort 3 vs. 6	(10) Cohort 4 vs. 6
All Channels										
DID	−.137**	−.126**	−.0613*	−.101**	−.310**	−.222**	−.188**	−.230**	−.195**	−.377**
	(.0251)	(.0254)	(.0258)	(.0258)	(.0258)	(.0262)	(.0263)	(.0261)	(.0261)	(.0268)
DID-Heckman	−.444**	−.391**	−.177**	−.124**	−.727**	−.355**	−.140**	−.629**	−.243**	−.253**
	(.0889)	(.0672)	(.0541)	(.0426)	(.0927)	(.0657)	(.0462)	(.0829)	(.0523)	(.0631)
DID-Matching	−.153**	−.173**	−.146**	−.190**	−.300**	−.215**	−.214**	−.223**	−.186**	−.379**
	(.0293)	(.0320)	(.0350)	(.0392)	(.0298)	(.0326)	(.0375)	(.0306)	(.0342)	(.0333)
Direct Channels										
DID	−.0626*	−.0559*	−.00428	−.0156	−.184**	−.105**	−.0900**	−.0927**	−.0505*	−.195**
	(.0252)	(.0250)	(.0252)	(.0251)	(.0253)	(.0254)	(.0254)	(.0254)	(.0252)	(.0258)
DID-Heckman	−.298**	−.338**	−.0455	.0833*	−.602**	−.200**	.0242	−.302**	.0777	.117
	(.0901)	(.0670)	(.0535)	(.0418)	(.0919)	(.0643)	(.0450)	(.0808)	(.0507)	(.0612)
DID-Matching	−.0438	−.0639*	−.0537	−.123**	−.154**	−.0908**	−.142**	−.0849**	−.0995**	−.244**
	(.0295)	(.0313)	(.0337)	(.0376)	(.0293)	(.0313)	(.0357)	(.0296)	(.0323)	(.0316)
Share of Direct Channels										
DID	.0162**	.0224**	.0229**	.0322**	.00909	.0126*	.00807	.0148**	.0246**	.00764
	(.00484)	(.00515)	(.00556)	(.00562)	(.00528)	(.00563)	(.00576)	(.00555)	(.00572)	(.00564)
DID-Heckman	.00334	.00284	.0106**	.0145**	.0102*	.0182**	.0228**	.0253**	.0278**	.0292**
	(.00470)	(.00344)	(.00265)	(.00207)	(.00449)	(.00303)	(.00212)	(.00372)	(.00231)	(.00275)
DID-Matching	.00777**	.00563**	.00290	.00268	.00690**	.00430**	−.0000595	.00545**	−.00120	−.00155
	(.00147)	(.00149)	(.00148)	(.00161)	(.00139)	(.00139)	(.00149)	(.00130)	(.00134)	(.00130)
Third-Party Channels										
DID	−.140**	−.139**	−.126**	−.154**	−.159**	−.132**	−.106**	−.171**	−.169**	−.180**
	(.0286)	(.0285)	(.0279)	(.0275)	(.0288)	(.0280)	(.0277)	(.0277)	(.0273)	(.0290)
DID-Heckman	.0444	.0701	−.148*	−.197**	.0730	−.0485	−.160**	−.392**	−.309**	−.335**
	(.103)	(.0764)	(.0595)	(.0460)	(.103)	(.0705)	(.0489)	(.0875)	(.0547)	(.0681)
DID-Matching	−.207**	−.203**	−.183**	−.207**	−.213**	−.147**	−.0714	−.155**	−.109**	−.166**
	(.0328)	(.0346)	(.0361)	(.0394)	(.0328)	(.0340)	(.0377)	(.0319)	(.0344)	(.0350)

*$p < .05$.
**$p < .01$.
Notes: Clustered standard errors are in parentheses.

heterogeneity in the treatment effects across different customer segments. To gain a comprehensive understanding of how customer spending changes after app adoption, we conduct a latent class analysis on our DID model. In particular, we allow the latent customer segments to have heterogeneous coefficients on the DID estimator (β_3) and the dummy variables, Adopt and Post (β_1 and β_2). The optimal number of latent segments for each treatment–control pair is determined by the Bayesian information criterion. On average, we find that 79.2% of app adopters have a negative treatment effect ranging between -6.6×10^{-8} and -2.23, and 21.8% of app adopters have a positive treatment effect ranging between 2.66×10^{-7} and 1.20 (for details, see the Web Appendix).

Furthermore, we explore the differences between latent customer segments with negative effect and latent customer segments with positive effect. First, although we do not observe customers' traveler type, we do observe whether their hotel stays are during weekends and holidays. Among customers with negative effect, 54% of them travel only on weekdays and/or nonholidays (i.e., likely to be business trips) and 46% travel on weekends and holidays (i.e., likely to be leisure trips). However, among customers with positive effect, about 83% of them travel only on weekdays and/or nonholidays, indicating that business travelers are more likely to have positive effect than leisure travelers. In addition, we investigate the geographic distribution of the two types of customer segments and find that customers in Arkansas, Mississippi, Nevada, and Wyoming are less likely to have negative effect.

Finally, we construct three pairs of predefined customer segments: (1) elite customers who have high-level membership and nonelite customers who have low-level membership, (2) heavy spenders whose total spending is among the top 25% of all customers and light spenders whose total spending is among the bottom 25% of all customers, and (3) customers who have used the firm's digital channels for making reservations and customers who have never used digital channels for making reservations. We find that the negative treatment effects are stronger for nonelite customers than for elite customers and stronger for light spenders than for heavy spenders. In addition, both customers who do and do not use digital channels have negative treatment effects, although the nondigital channel users have much stronger negative treatment effects than the digital channel users.

Table 3. Estimation Results of Heckman Outcome Equation (All Channels).

Variables	**(1) Cohort 1 vs. 3**	**(2) Cohort 1 vs. 4**	**(3) Cohort 1 vs. 5**	**(4) Cohort 1 vs. 6**	**(5) Cohort 2 vs. 4**	**(6) Cohort 2 vs. 5**	**(7) Cohort 2 vs. 6**	**(8) Cohort 3 vs. 5**	**(9) Cohort 3 vs. 6**	**(10) Cohort 4 vs. 6**
Adopt × Post	−.444**	−.391**	−.177**	−.124**	−.727**	−.355**	−.140**	−.629**	−.243**	−.253**
Online website	.327**	.351**	.337**	.309**	.424**	.437**	.350**	.394**	.329**	.420**
Property & call center	.900**	.905**	1.063**	1.193**	1.134**	1.200**	1.233**	1.331**	1.335**	1.211**
GDS	.755**	.671**	.773**	.719**	.529**	.624**	.696**	.606**	.623**	.513**
OTAs	1.570**	1.722**	1.656**	1.484**	1.751**	1.794**	1.812**	1.915**	2.000**	1.556**
Luxury brand	.754**	1.026**	.846**	.952**	.868**	.589**	.739**	.914**	1.216**	1.159**
Premium brand	.615**	.766**	.719**	.795**	.865**	1.039**	.929**	.974**	1.167**	1.162**
Upscale brand	.0737	.0212	.0809	.0305	.273*	.433**	.250	.244*	.292*	.421**
Economy brand	.0179	−.0700	−.0900	−.00526	.0312	.0648	−.0461	.1000	.174	.186
Franchise hotel	−.0459	−.121	−.267**	−.204*	−.263**	−.321**	−.271**	−.112	−.138	−.312**
US hotel	3.974**	4.114**	4.537**	4.542**	4.261**	4.604**	4.681**	4.441**	4.589**	4.527**
Weekend stay	1.726**	1.744**	1.926**	1.936**	1.802**	2.003**	2.077**	1.980**	2.103**	2.129**
Holiday stay	.727**	.725**	.665**	.698**	.528**	.444**	.574**	.659**	.690**	.723**
Airport area	−.547**	−.568**	−.719**	−.766**	−.286**	−.463**	−.466**	−.563**	−.752**	−.706**
Downtown area	−.0773	.00256	−.0364	.00771	.199	.198	.267*	.386**	.284*	.348**
Resort area	.423**	.557**	.515**	.557**	.943**	.901**	1.297**	1.285**	1.416**	.834**
Suburban area	−.481**	−.432**	−.627**	−.570**	−.401**	−.565**	−.525**	−.388**	−.499**	−.580**
Expressway area	−.448**	−.493**	−.619**	−.613**	−.478**	−.510**	−.533**	−.411**	−.486**	−.648**
Metro area	−.285**	−.327**	−.429**	−.396**	−.248*	−.340**	−.301**	−.371**	−.555**	−.461**
Platinum member	.198**	.235**	.230**	.212**	−.166	−.218*	−.278**	−.213*	−.225*	−.235*
Gold member	.174**	.126*	.121*	.154**	−.0919	−.134*	−.162*	−.155*	−.160*	−.190**
Silver member	.297**	.288**	.211**	.263**	.0947	.0848	.0941	.0337	.0450	−.102
Basic member	.593**	.600**	.463**	.513**	.469**	.343**	.385**	.529**	.528**	.125*
IMR	.217**	.200**	.0839*	.0184	.303**	.117**	−.0162	.289**	.0589	−.0669
Constant	.0535	.0235	−.0845**	−.0960**	.365**	.125**	.0126	.374**	.161**	.0122

*$p < .05$.
**$p < .01$.

Table 4. Treatment Effects of Customer App Adoption on Spending (Multichannel).

	(1) Cohort 1 vs. 3	**(2) Cohort 1 vs. 4**	**(3) Cohort 1 vs. 5**	**(4) Cohort 1 vs. 6**	**(5) Cohort 2 vs. 4**	**(6) Cohort 2 vs. 5**	**(7) Cohort 2 vs. 6**	**(8) Cohort 3 vs. 5**	**(9) Cohort 3 vs. 6**	**(10) Cohort 4 vs. 6**
Online Website										
DID	−.0721**	−.0653**	−.0454*	−.0454*	−.182**	−.130**	−.0921**	−.0804**	−.0527*	−.162**
DID-Heckman	−.654**	−.410**	−.241**	−.0949**	−.539**	−.325**	−.152**	−.353**	−.195**	−.200***
DID-Matching	−.0462	−.0614*	−.0356	−.0828**	−.163**	−.0865**	−.0756**	−.0747**	−.0168	−.146**
Property and Call Center										
DID	−.0390	−.0444*	−.0177	−.0455*	−.122**	−.0858**	−.0751**	−.0843**	−.0594**	−.0957**
DID-Heckman	.423**	.0526	.131**	.0977**	−.131	−.0632	−.00894	−.0691	.0954*	−.0382
DID-Matching	−.0630*	−.0558*	−.0646*	−.130**	−.106**	−.0953**	−.103**	−.0834**	−.108**	−.111**
GDS										
DID	−.0378*	−.0641**	−.0359*	−.0428**	−.0815**	−.0824**	−.0617**	−.0405*	−.0450**	−.0675**
DID-Heckman	.0817	.107*	.0700*	−.0331	−.123*	−.138**	−.0968**	−.126*	−.0554	−.120**
DID-Matching	−.0690**	−.111**	−.0940**	−.0889**	−.0963**	−.0516**	−.0528*	−.0428*	−.0462*	−.0626**
OTAs										
DID	−.00651	−.00182	−.00653	−.00139	−.0131*	−.00289	−.00327	−.0017	−.00593	−.0111
DID-Heckman	.00543	−.00562	−.00845	.00334	−.00569	−.0108	−.00827	.00480	−.0105	.00493
DID-Matching	−.00756	−.00245	−.00469	−.00902	−.0121	−.0000208	.00155	.000527	−.00472	−.00858

*$p < .05$.
**$p < .01$.
Notes: This table uses clustered standard errors.

Mobile App Adoption

In the Heckman selection equation, we modeled customer app adoption decisions using the probit regression. There are several significant explanatory variables for mobile app adoption, and the coefficient estimates reported in Table 5 are consistent with our expectations. First, the dummy variables capturing mobile device type have strong predictive power for

Table 5. Estimation Results of Heckman Selection Equation (Probit Regression for App Adoption).

Variables	(1) Cohort 1 vs. 3	(2) Cohort 1 vs. 4	(3) Cohort 1 vs. 5	(4) Cohort 1 vs. 6	(5) Cohort 2 vs. 4	(6) Cohort 2 vs. 5	(7) Cohort 2 vs. 6	(8) Cohort 3 vs. 5	(9) Cohort 3 vs. 6	(10) Cohort 4 vs. 6
Instrumental Variables										
iPhone	.0765**	.304**	.618**	2.166**	.432**	.761**	2.313**	.568**	2.163**	1.971**
Android	.190**	.366**	.621**	2.429**	.358**	.639**	2.475**	.490**	2.385**	2.216**
Blackberry	.570**	1.377**	1.703**	1.434**	1.497**	1.792**	1.419**	1.341*	.918*	−.282
Other mobile device	−.159**	−.269**	−.363**	−.218**	−.175**	−.272**	−.118*	−.249**	−.0562	.00806
Mobile website booking	.605**	.904**	.862**	.876**	.666**	.673**	.564**	.301**	.112	.0666
Booking window (×30 days)	.132**	.149**	.203**	.114**	.0581	.126**	.0535	.146**	.0538	.202**
Additional Predictors (Not Used in the Outcome Equation)										
% of online website spending	.390**	.437**	.443**	.432**	.395**	.430**	.453**	.255**	.326**	.456**
% of property & call center spending	.106**	.195**	.156**	.137**	.279**	.237**	.160**	.192**	.135**	.123**
% of GDS spending	.163**	.137**	.155**	.299**	.191**	.242**	.359**	.197**	.372**	.470**
% of OTA spending	.113	.189	.410**	.233*	.246*	.365**	.158	.346**	.203	.175
Number of room nights	.0199**	.0344**	.0283**	.0405**	.0270**	.0316**	.0508**	.00743	.0316**	.0547**
Δ Spending (×1,000 $)	.0345**	.0552**	.0624**	.0849**	.0792**	.0863**	.0890**	.0615**	.0846**	.145**
Control Variables (Used in the Outcome Equation)										
Luxury brand	−.435*	−.250	−.286	−.0430	.142	.0485	.327	−.0218	.0572	.170
Premium brand	−.0770	−.173	−.00673	.393**	−.139	.304	.492*	.0423	.203	1.398**
Upscale brand	−.112	.0660	.162	.438**	.192	.400*	.511*	.123	.114	.191
Economy brand	−.0663	.162	.223	.572**	.240	.449*	.547*	.225	.522*	.366
Franchised hotel	.137	.0841	.0568	.0760	.0444	.157	−.0355	.272*	.0510	.0716
US hotel	.379**	.163	.422**	.134	.124	.299*	.181	.208	.0567	.589**
Weekend stay	.310**	.451**	.316**	.468**	.457**	.421**	.495**	.243*	.450**	.245
Holiday stay	.177	.0137	.120	.134	.346*	.253	.346*	.769**	.813**	−.826**
Airport area	−.0351	−.0436	−.212*	−.490**	−.328**	−.595**	−.753**	−.380*	−.399*	−.610**
Downtown area	.217**	.148	.0475	−.158	−.156	−.331*	−.418*	−.300	−.213	−.493*
Resort area	.0438	−.215	−.296	−.0188	−.488*	−.668*	−.0537	−.566	.307	−.332
Suburban area	.0612	.0345	−.0528	−.271*	−.274*	−.403**	−.436**	−.265	−.160	−.259
Expressway area	−.0783	.0755	−.227	−.291*	−.0133	−.447**	−.475*	−.433*	−.511*	−.607*
Metro area	.0132	−.0673	−.101	−.264*	−.230	−.369**	−.427*	−.369*	−.431*	−.391
Platinum member	−.0271	.212**	.431**	.325**	−.0937	.0596	.00746	.272**	.156*	−.438**
Gold member	−.135**	.130*	.211**	.189**	.0132	.0151	.0789	.213**	.228**	−.360**
Silver member	−.129**	.0272	.0559	−.0371	.0203	−.00836	−.0336	.0535	.00543	−.344**
Basic member	−.207**	−.236**	−.310**	−.345**	−.165**	−.264**	−.265**	−.246**	−.299**	−.325**
Age	−.00804**	−.00908**	−.0127**	−.0165**	−.00610**	−.0106**	−.0156**	−.00669**	−.0111**	−.0105**
Female	−.147**	−.238**	−.300**	−.243**	−.0710**	−.134**	−.0687**	−.162**	−.110**	−.0323
Constant	−.0841	−.154**	−.0836	.0309	−.204**	−.121**	.00934	−.118*	−.0352	.0137
# of customers	20,000	20,000	20,000	20,000	20,000	20,000	20,000	20,000	20,000	20,000
Pseudo R^2	.0720	.128	.204	.340	.0632	.132	.292	.0774	.219	.160

*$p < .05$.
**$p < .01$.
Notes: This table uses robust standard errors.

app adoption decision. Second, customers are more likely to adopt the app if they have used the mobile website for booking prior to app adoption. Third, customers who tend to make hotel reservations early are more likely to adopt the app. Fourth, the likelihood of app adoption is higher if customers increase their spending before adoption and if customers have larger share of spending through the firm's online website, indicating that mobile app adoption is most likely correlated with consumers' past online purchase. Fifth, customers who booked domestic hotels and/or booked for weekend stays are more likely to adopt the mobile app. The probability of older and female customers adopting the app is lower than younger or male customers. The logit regression of PSM produces similar results.

Robustness Checks

To ensure that our findings are robust, we conduct a variety of robustness checks. First, although it may produce biased standard errors, it is still worthwhile to test for the treatment effects using the multiperiod panel DID model. We take each month as one time period and have six periods for the preadoption window and six periods for the postadoption window. Again, we account for the potential endogeneity concerns using the three instrumental variables: mobile device type, mobile website booking, and length of booking window. The panel DID model is given by

$$y_{it} = \beta_0 + \beta_1 \,\text{adopt}_i + \sum_{\tau=1}^{11} \beta_{2,\tau}\, \text{month}_\tau + \beta_3(\text{adopt} \times \text{post})_{it} + \mathbf{x}_{it}\gamma + \mathbf{w}_i\theta + \nu_i + \epsilon_{it}, \tag{4}$$

where month_τ is a set of dummy variables with $\tau = 1, 2, \ldots, 11$; and ν_i captures unobserved individual fixed effects. We find that the treatment effects are significantly negative for all treatment–control pairs.

Second, we investigate whether the negative effect of app adoption will fade away after a long period of time. We add a linear time trend to the DID interaction term, adopt × post, in the panel DID model:

$$y_{it} = \beta_0 + \beta_1 \,\text{adopt}_i + \sum_{\tau=1}^{11} \beta_{2,\tau}\, \text{month}_\tau + \beta_3(\text{adopt} \times \text{post})_{it} + \beta_4(\text{timetrend} \times \text{adopt} \times \text{post})_{it} + \mathbf{x}_{it}\gamma + \mathbf{w}_i\theta + \nu_i + \epsilon_{it}. \tag{5}$$

We apply the model to two treatment–control pairs, Cohort 1 versus Cohort 5 and Cohort 1 versus Cohort 6, because these two comparisons allow for longer time periods than other cohort pairs. We find that both β_3 and β_4 are negative, suggesting that, instead of fading away, the negative effect of app adoption gradually strengthens over time.

Third, approximately 80% of customers in our sample provided their geographic information, so we can use it to proxy income as an additional robustness check. In particular, we match customers with the county-level median household income data from American Community Survey by the U.S. Census Bureau. The matching takes into account the fact that each cohort pair's analysis covers a different time window: for example, we use the income data of 2011 when the sample period of DID analysis is in 2011. We find robust treatment effects after including the income variable in our DID model.

Fourth, we did not include the month of app adoption in our DID analysis window, because we do not observe the exact date of app adoption. Given that we eventually find negative effects on customer spending, if we treat customer spending in the adoption month as preadoption spending, our negative effect will be even stronger as customer preadoption spending becomes higher. If we treat customer spending in the adoption month as postadoption spending, our treatment effects will be conservative estimates because we might have considered some preadoption spending as postadoption spending. However, it could be the case that customers used the app immediately after their downloads and made reservations in the adoption month. To resolve this concern, we conduct a robustness check and assume that all adoption occurs on the first day of the adoption month, and thus we treat the spending in the adoption month as postadoption spending. We find consistently negative treatment effects.

Fifth, to address the sample selection concerns, we apply the DID analysis to alternative random samples of 4,000 customers from each of the six cohorts. The alternative samples do not overlap with the sample we use in the main analysis. We also apply the DID analysis to four alternative customer cohorts as additional treatment groups: (1) alternative Cohort 1 adopted the app in December 2011, (2) alternative Cohort 2 adopted the app in August 2012, (3) alternative Cohort 3 adopted the app in June 2013, and (4) alternative Cohort 4 adopted the app in February 2014. We find significantly negative treatment effects for all of these different samples.

In addition, we find robust results using three alternative matching methods: individual PSM, PSM with control cohort consolidation (i.e., all control cohorts are merged as one single control group), and matching on monthly spending with control cohort consolidation. We also perform placebo tests by constructing fake treatment groups to replace the real treatment groups. Specifically, we randomly assign customers to the control groups and treatment groups and apply the same models at the same treatment window as in our main analysis. We find that the signs of the treatment effects are random, and none are significant. Finally, we use the absolute value of customer spending as the dependent variable (i.e., without log-transformation), and the treatment effects are consistent with our main findings.

Examining Underlying Mechanisms

Contrary to previous findings, our research shows that the treatment effect of customer app adoption on their spending is negative in our institutional context. Consequently, it is important to understand the underlying mechanisms driving these results. In this section, we offer possible explanations for the negative effect and systematically examine them with further data and analyses.

Regression-to-the-Mean Effect

One possible explanation is that the negative effect of app adoption on spending is due to the regression-to-the-mean effect. For example, travel and associated spending within a year may peak at a specific point, around which customers adopt the app. The spending returns to a normal or even lower level after the peak. To investigate this possibility, we conduct two additional analyses: (1) we extend the time frame of our sample from six months before app adoption and six months after app adoption to one year before app adoption and one year after app adoption so that the analysis window covers a two-year period and (2) we extend the length of the treatment gap window from one month to five months. That is, we drop out not only the adoption month but also the two months preceding and succeeding that month. This longer gap window disregards the possible peak spending prior to app adoption, which could exacerbate the regression-to-the-mean effect. However, we still find significantly negative treatment effects and thus rule out the regression-to-the-mean effect as a possible explanation.

Ease of Finding Lower Prices

One might argue that the negative effect is merely caused by the lower hotel rates after app adoption, as it becomes easier for customers to find better deals or lower-end hotels by using the app. Although this is not likely to be a major concern in our context, because prices do not vary across direct channels, we repeat our DID analysis with two additional dependent variables: the number of room nights and the number of bookings (in log values), which are alternative measurements of customer purchases. We find that the treatment effects for these two dependent variables are also significantly negative for all customers. If the negative effects are only driven by lower price, then we would observe customers making more reservations with the focal hotel at that lower price. However, our results show that customers in fact made fewer bookings with the focal hotel than they would have if they had not adopted the app. It suggests that customers are turning to other hotels as it is unlikely that customers' travel demand shrinks after app adoption.

App Adopters May Not Use the App Actively

In our main analysis, we do not distinguish whether app adopters actively use the app after installing it. This gives rise to the possibility that the negative effect is not attributable to app adoption if customers do not use the app after adoption. We address this concern by restricting our sample to customers who are active app users (top 50%), which is measured by customer usage of in-app search and the mobile check-in feature. We show that the treatment effects are significantly negative for active app users in all cohorts, though the effect size could be smaller than that for the inactive app users.

App Adopters May Increase Spending Before App Adoption

Another possible reason for the negative effect of app adoption on spending is that there is a spending spike in the treatment group prior to app adoption. We investigate this explanation by comparing nonadopters with adopters who had no such spending spikes. We define adopters without a spending spike as adopters whose ΔSpending is less than the mean value of ΔSpending of nonadopters plus one unit of the standard deviation of it. Again, the results show a consistent negative effect on customer total spending after app adoption.

App Quality

If a firm's mobile app does not possess a superior design and quality, then customers may be dissatisfied with their app usage experiences, which could have a negative spillover across other channels. As a result, customers could reduce their spending with the focal firm. While this could be a possible explanation in the early stages after app introduction, it is unlikely that a perception of poor app quality could have remained constant throughout the four-year period of our analysis because the apps are updated very frequently to fix problems and improve quality in the hospitality industry. In particular, our focal hotel group had more than 20 app updates during our period of study.

However, to examine this possibility, we look for evidence in empirical data of customer app usage. We find that, in contrast to the negative trend in total spending, customers' usage of the app to search for hotel rooms has remained steady over time. This is reflected in Figure 3. Notably, customers who adopted the app earlier make more searches over time than those who adopted later. This is clearly inconsistent with the explanation of poor app quality, as customers would be unlikely to keep using the app for a long time if they thought its quality was poor. Yet the decreasing trend in customer total spending is consistent across all cohorts.

Shopping-Around Behavior

Finally, there is also a possible explanation that customers who are comfortable using one app may find mobile apps in general to be good product research tools for comparing hotel accommodation options and, therefore, are more likely to adopt and use competitors' apps. Then, customers who use multiple apps are more likely to shop around and shift some of the spending they would have made at the focal hotel group to competitors as they find better options. We use additional information on the hospitality app market and conduct two analyses to provide empirical evidence for this "shopping-around" mechanism that we put forth for the observed negative effect.[2]

[2] To understand customer adoption of hotel apps, we also conducted an online survey with 448 complete responses in May 2017. The survey results suggest that customers who adopted the focal hotel app use more hotel and OTA apps than customers who did not adopt the focal hotel app.

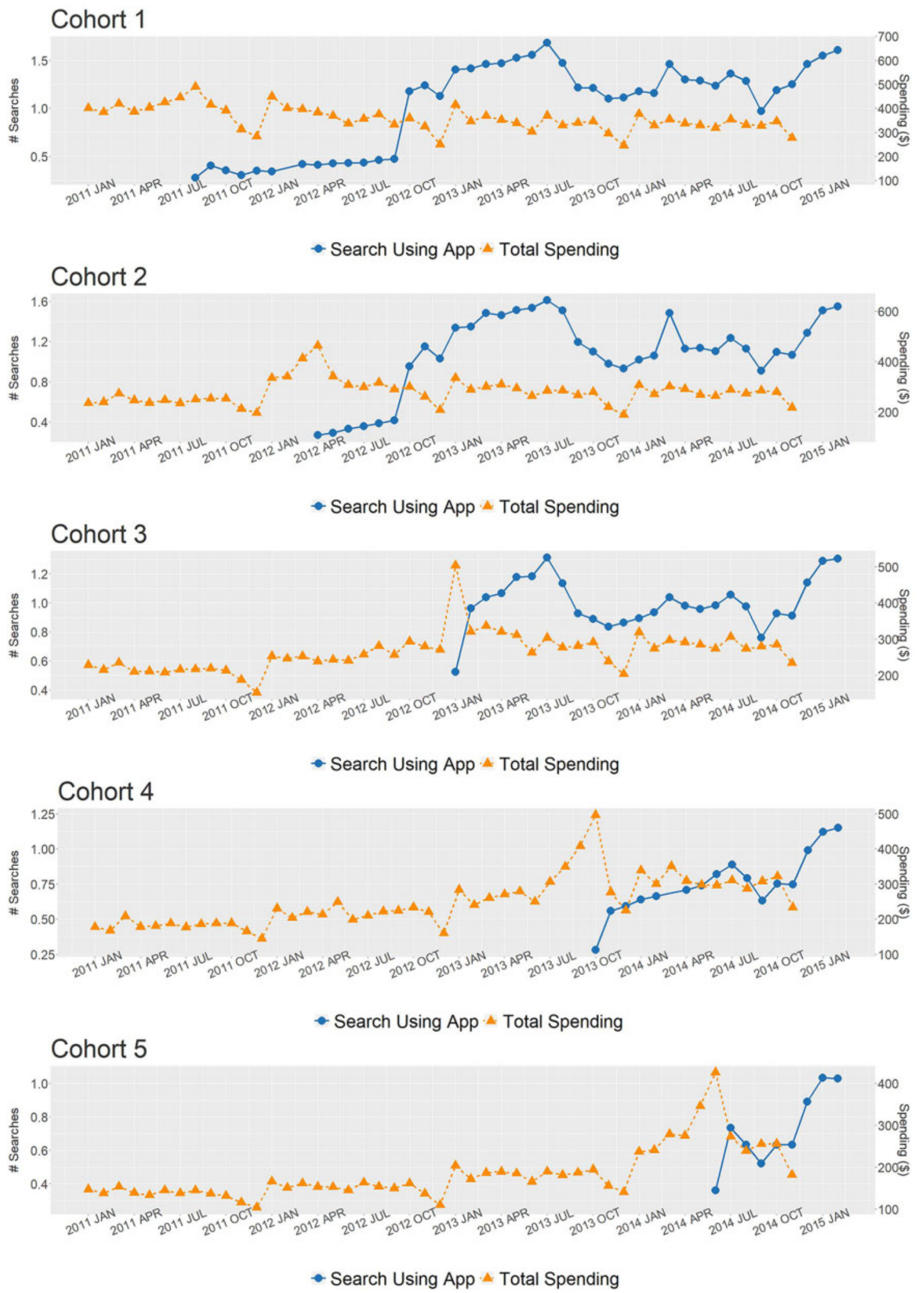

Figure 3. Average monthly number of in-app search and total spending by cohort.

First, we leverage the fact that other hotel groups and travel websites also introduced their mobile apps over time. In particular, two major hotel groups and six major travel websites introduced their apps before the launch of the focal hotel app, and four major hotel groups and one major travel website introduced their apps after the launch of the focal app. In our extended analysis, we estimate a panel DID model by including the number of hotel and travel website apps that are available on the market and its interaction with Adopt × Post. As Table 6 shows, the negative effect of customers' app adoption on their spending becomes stronger when there are more hotel and travel website apps available on the market. It may indicate that, as the app market becomes more competitive, customers are more likely to shop around because more product search tools are available on the market.

We also investigate whether the negative treatment effects are related to how customers use the mobile app as a new channel. If customers use the app as a search tool instead of

Table 6. Treatment Effects with the Number of Apps on the Market.

	(1) Cohort 1 vs. 3	(2) Cohort 1 vs. 4	(3) Cohort 1 vs. 5	(4) Cohort 1 vs. 6	(5) Cohort 3 vs. 5	(6) Cohort 3 vs. 6	(7) Cohort 4 vs. 6
Adopt × Post	.263**	.303**	.347**	.335**	.377**	.416**	.0912
	(.0693)	(.0674)	(.0655)	(.0648)	(.118)	(.116)	(.130)
Adopt × Post × Number of apps	−.0300**	−.0345**	−.0396**	−.0383**	−.0294**	−.0327**	−.00646
	(.00781)	(.00760)	(.00738)	(.00730)	(.0103)	(.0101)	(.0103)
Number of apps	−.00521	−.00417	−.00265	−.00225	.00966	.0143	−.0117**
	(.00361)	(.00339)	(.00330)	(.00321)	(.00937)	(.00895)	(.00450)
Reservation characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Customer demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*$p < .05$.
**$p < .01$.
Notes: Clustered standard errors are in parentheses. There is no change in the number of hotel and travel website apps during Cohort 2's analysis window, and thus the coefficients cannot be identified.

Table 7. Treatment Effects for Substituting and Nonsubstituting Customers.

	(1) Cohort 1 vs. 3	(2) Cohort 1 vs. 4	(3) Cohort 1 vs. 5	(4) Cohort 1 vs. 6	(5) Cohort 2 vs. 4	(6) Cohort 2 vs. 5	(7) Cohort 2 vs. 6	(8) Cohort 3 vs. 5	(9) Cohort 3 vs. 6	(10) Cohort 4 vs. 6
Substituting Customer Versus Control Customers										
Treatment effect	.361**	.411**	.515**	.537**	.0238	.171*	.233**	.292**	.411**	.248**
	(.0570)	(.0601)	(.0659)	(.0686)	(.0640)	(.0702)	(.0735)	(.0777)	(.0837)	(.0885)
Nonsubstituting Customer Versus Control Customers										
Treatment effect	−.159**	−.149**	−.0850**	−.124**	−.322**	−.236**	−.203**	−.248**	−.215**	−.390**
	(.0256)	(.0259)	(.0262)	(.0263)	(.0263)	(.0266)	(.0267)	(.0265)	(.0265)	(.0271)

*$p < .05$.
**$p < .01$.
Notes: Clustered standard errors are in parentheses.

a purchase channel, it is likely to be the case that customers tend to shop around more after app adoption. In particular, we identify customers in the treatment cohorts who substituted the existing channel(s) with the mobile channel after their app adoption. We define "substituting customers" as those who increase their number of bookings through the mobile channel while decreasing their number of bookings through the existing channels after app adoption. The rest of the treatment customers are regarded as "nonsubstituting customers." The percentage of substituting customers in each treatment cohort is 5.74%, 4.91%, 3.84%, and 3.85%, respectively. We then apply our DID model to (1) substituting customers versus control customers and (2) nonsubstituting customers versus control customers. Table 7 shows that app adoption has a positive effect for substituting customers and a negative effect for nonsubstituting customers. Because nonsubstituting customers are over 90% of the app adopters, the negative effect on spending dominates in the main sample. These results suggest that the negative effects may not exist if customers use the app as a substitution purchase channel rather than just a search tool.

Our results so far suggest the possibility that customers are using the app for upper purchase funnel activities such as search. Given the intense competition in the hospitality app market, adopters of the focal hotel app are also likely to possess apps of other hotels and travel websites and search and book hotel rooms on those apps. The mobile technology enables the ease of access of search functionality in apps and leads to less concentrated booking at any one hotel group and a corresponding drop in the share of wallet for the focal hotel group. The findings validate the concern that mobile app adoption may not lead to positive outcomes on customer spending as firms would expect.

Managerial Implications

What can the firm do to combat this tendency among customers to increase search? We certainly are not advocating that the firm discontinue its mobile apps. As an increasing number of firms adopt technologies that make it easier for customers to interact and engage with the firm, customers expect such technologies to be provided by all firms. Technologies such as mobile apps have thus become less of a differentiator and more of a threshold cost of doing business. Rather than eschew mobile apps, we argue that the firm could mitigate the dark side of mobile app adoption by encouraging customers to fully use all the functionalities of the app and thus increase their engagement with the firm (Gill, Sridhar, and Grewal 2017; Rutz, Aravindakshan, and Rubel 2019; Van Heerde, Dinner, and Neslin 2019).

Formally, we investigate how customers' spending differs by their app usage behavior and the extent of their app engagement. Among customers who made hotel bookings after their app adoption, we compare the total spending of customers who used their mobile app for search only (lower level of app engagement) with the total spending of customers who used their mobile for search as well as for mobile check-in (higher level of engagement). There is, however, a self-selection issue involved in customers who choose to use the mobile check-in feature. When comparing customers who used the mobile check-in (the treatment group) with customers who did not use the mobile check-in (the control group), the estimated effect on customer spending could be a combined effect of mobile check-in usage and the self-selection bias.

However, not all hotel properties introduced the mobile check-in service, and there is randomness in hotels' choice of introduction. This implies that there are two types of customers among those who did not use the mobile check-in service: (1) a group of customers booked at hotels with the mobile check-in service, but did not use it, and (2) a group of customers who booked only at hotels without mobile check-in service and thus did not have the option to use this feature. Some of these customers could have self-selected themselves to use the mobile check-in feature if they have had the option. We use the group of customers who had the option to use the mobile check-in feature but did not use it as one control group (control group 1) and the group of customers who did not have the option to use the mobile check-in feature as another control group (control group 2) against which to compare the treatment group.

We leverage the variation in mobile check-in service introduction by the hotel properties to our advantage in isolating the self-selection bias and obtaining an unbiased estimate of the impact of mobile check-in usage (for details, see the Web Appendix). Given that there are multiple time points of the treatment, we use the DID approach in a panel setting (Dranove et al. 2003) for the two comparisons: (1) the treatment group versus control group 1 and (2) the treatment group versus control group 2.

$$y_{it} = \beta_0^c + \beta_1^c \,\text{check-in}_i + \sum_{\tau=1}^{T-1} \beta_{2,\tau}^c \,\text{month}_\tau + \beta_3^c \,(\text{check-in} \times \text{post}^c)_{it} + x_{it}\gamma^c + w_i\theta^c + \sum_{\kappa=1}^{5} \lambda_\kappa^c \,\text{cohort}_\kappa + \mu_i + \epsilon_{it}^c, \tag{6}$$

where y_{it} is the log value of customer i's total spending in period t. The indicator variable $\text{check}-\text{in}_i$ takes the value of 1 if customer i ever used mobile check-in feature and 0 otherwise. The indicator variable post_t^c takes the value of 1 for the periods after customer i's first usage of mobile check-in and 0 otherwise. $\mathbf{x}_{it}$ is a vector of reservation characteristics, and $\mathbf{w}_i$ is customer i's demographics. The indicator variable month_τ is the month dummy variable, and cohort_κ is the cohort dummy variable. μ_i is the individual-specific error term, and ϵ_{it}^c is the serially correlated error term.

Our results show that, for customers who made hotel bookings through any shopping channel, the treatment effects of using the mobile check-in feature are significantly positive ($\hat{\beta}_{mc} = .0450$, $p < .01$). Similarly, for customers who made hotel bookings through the mobile channel, the treatment effects of using the mobile check-in feature are significantly positive ($\hat{\beta}_{mc} = .0479$, $p < .01$). Therefore, encouraging customers to use app services such as the mobile check-in feature could help increase customer engagement with the app and the hotel. Then the increased customer engagement may enhance their loyalty and reduce the negative effect of app adoption on customer spending. This positive effect of increased app engagement is consistent with previous findings in Gill, Sridhar, and Grewal (2017).

Limitations

There are several limitations of this research that need to be highlighted. First, this research is based on observational data and not on data from a field experiment with customers randomly assigned as app adopters and nonadopters. In the DID analysis, we compare the customers who adopt the app at a specific time with the customers who have not adopted at that time, using them as proxy for the counterfactual of adoption (i.e., "if had they not adopted the app"). We use later adopters as control units for the earlier adopters, but these two groups could be very different, which could bias the results. We use PSM to increase the comparability between these two groups, but this does not solve for the self-selection problem. We use Heckman-type correction to account for self-selection issues, but this model does not account for the time-variant factors across customers (e.g., rate of maturation with technology, travel experience with other hotels). Even with the use of different pairs of treatment and control groups over time, such dynamics are not captured in the Heckman model, as the correction provides only an instantaneous snapshot of these omitted variables at these various points in time. As a result, our findings could be just a correlational pattern between customers' app adoption and their decreased spending rather than a clear causal relationship. A randomized field experiment could be a solution to establish causality but would be difficult to implement.

Second, the mechanism we propose for the decrease in spending is that the adopters of the focal app also adopt other apps. While our proposed mechanism is a likely explanation for the negative effects and is consistent with our empirical analyses, we note that there are alternative explanations that cannot be completely ruled out without further data and analyses. Therefore, the mechanism we proposed should be treated with circumspection. One possible alternative explanation is that as customers become more tech-savvy over time, they may adopt the hotel app and become more likely to search for hotels broadly. This possible change in customer behavior is

Table 8. Limitations and Future Research Directions.

Limitations	Future Research Directions
• The app adoption decision is endogenous: our estimated treatment effects of app adoption on customer spending could be a correlational pattern	• A randomized field experiment could be a solution to establish causality but would be difficult to implement
• The data are observational: our proposed mechanism is a likely explanation for the negative treatment effect, and there are other alternative explanations that cannot be completely ruled out	• Use additional data to provide a direct explanation for the negative effect of app adoption
• This research does not have the data to show what would have happened if the app adopters had not adopted the focal app but the other apps	• Estimate a structural model and conduct counterfactual analyses
• This research only uses customers' usage of the app search feature and the mobile check-in feature to measure customers' app engagement level	• Provide more general implications about how the app engagement can affect customer spending based on comprehensive data on customer app usage
• This research only focuses on the existing customers	• Investigate how the introduction of mobile app affects new customer acquisition and firm revenues

confounded with the customer app adoption decision as well as customer shopping habits. However, it is not observed by researchers and thus cannot be investigated in this article. Another possible alternative explanation is that there are exogenous shocks that lead a customer to adopt one app, and this app's adoption has a spillover effect on other apps' adoption. Owing to data limitations, we do not observe customer adoption of other hotels' apps, and therefore we are unable to test for this explanation. In addition, it is not clear if the negative effect is simply the result of the behavior of adopters with different personalities manifesting itself at various stages of the process. In general, our current data cannot support an analysis that completely rules out alternative explanations for the treatment effect. The availability of additional data will enable future researchers to provide a direct explanation for the negative effect of app adoption we uncover in this research.

Third, we show that app adoption leads to a negative effect on customer spending, even if it is correlational. Even if the app adopters had not adopted the focal hotel app, their spending cannot be more positive than the spending of the nonadopters. However, it could have been more positive than the case if they had not adopted the focal hotel app but adopted other apps. Unfortunately, we do not have the data to show this. Future researchers could tackle this avenue of study using a structural model and estimating such counterfactuals.

Fourth, we only use customer usage of the search feature and the mobile check-in feature to measure customers' app engagement level, which could limit the generalizability of our managerial implications on other app features. In the future, researchers could provide more general recommendations for firms if they have comprehensive data on customer app usage. In addition, our analysis shows a correlational relationship instead of a causal relationship between mobile check-in usage and customer spending given our inability of running a field experiment on app feature usage.

Finally, another limitation of our study is that we focus only on the existing customers. It is entirely possible that the introduction of new technology may lead to increases in new customer acquisition, which could potentially have a positive effect on the hotel group's total revenues. However, it might be difficult to establish whether a potential customer becomes a customer of the firm merely due to the availability of the app. We leave this important question for future research. We summarize the limitations and future research directions in Table 8.

Conclusions

Many firms use mobile apps as a synergistic new channel for selling products, offering services, and increasing customer engagement (e.g., De Haan et al. 2018). Customers' purchase behavior clearly changes after app adoption. From the firm's perspective, understanding the nature of this phenomenon is critical, especially because accountability for investments in new technologies has become increasingly important in the digital era. In this article, we examine this purchase behavior change in the context of a major international hotel group, which launched its mobile app in mid-2011.

While the conventional wisdom based on recent academic work and practitioner articles is that mobile app adoption has a positive effect on customer spending and possibly retention, in practice, the revenue benefits may not always be realized. In this research, we highlight a striking case in which customer spending and the adoption of mobile apps may actually have a negative relationship. We propose a possible mechanism to explain the negative effect of app adoption, which is due to the intense competition in the hospitality market, the suitability of mobile apps as a research tool for travel products, and the nature of derived demand of the hotel offering.

Consistent with this mechanism, similar negative effects could be likely in product categories in which search is important and customers tend to adopt multiple apps in the same category. So, service industries such as airlines, ride-sharing services, and car rentals are natural extensions. Likewise, retail banking services wherein incentives are given to open new accounts along with banking apps could face similar situations. In addition, some retail services could also face decrease in share of wallet as customers adopt more apps in that vertical. From this viewpoint, our research contributes to the emerging literature on mobile apps by highlighting the interesting mechanisms that could lead to negative outcome of customer app adoption. We believe the strength of our article is the counterintuitive results we find, which should be documented because they are managerially relevant. We hope future research will focus on similar or different settings to establish the boundary conditions under which mobile apps lead to a positive impact.

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