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Peer-to-peer markets, collectively known as the sharing economy, have emerged as alternative suppliers of goods and services traditionally provided by long-established industries. The authors explore the economic impact of the sharing economy on incumbent firms by studying the case of Airbnb, a prominent platform for short-term accommodations. They analyze Airbnb's entry into the state of Texas and quantify its impact on the Texas hotel industry over the subsequent decade. In Austin, where Airbnb supply is highest, the causal impact on hotel revenue is in the 8%–10% range; moreover, the impact is nonuniform, with lower-priced hotels and hotels that do not cater to business travelers being the most affected. The impact manifests itself primarily through less aggressive hotel room pricing, benefiting all consumers, not just participants in the sharing economy. The price response is especially pronounced during periods of peak demand, such as during the South by Southwest festival, and is due to a differentiating feature of peer-to-peer platforms—enabling instantaneous supply to scale to meet demand.

Keywords: sharing economy, Airbnb, hotel industry, competition, peer-to-peer markets

The Rise of the Sharing Economy: Estimating the Impact of Airbnb on the Hotel Industry

The emergence of peer-to-peer platforms, collectively known as the "sharing economy," has enabled people to collaboratively make use of underutilized inventory through fee-based sharing. Consumers have so far enthusiastically adopted the services offered by firms such as Airbnb, Uber, Lyft, and TaskRabbit. The rapid growth of peer-to-peer platforms has arguably been enabled by two key factors: technology innovations and supply-side flexibility. Technology innovations have streamlined the process of market entry for suppliers, facilitated searchable listings for consumers, and kept transaction overheads low. Supply-side flexibility is another hallmark of these platforms: Uber drivers can add or remove themselves from the available supply of drivers with a swipe on an app, and similarly other suppliers can readily list and delist the selection of goods or services they offer.

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In our work, we focus on the impacts that these peer-to-peer platforms have on incumbent firms, specifically focusing on the case of Airbnb, a provider of travel accommodation and a pioneer of the sharing economy. Because Airbnb has served more than 50 million guests since it was founded in 2008 and has a market capitalization eclipsing $30 billion, we hypothesize that it has a measurable and quantifiable impact on hotel revenue in affected areas (see Airbnb 2015; Farrell and Bensinger 2016). Our hypothesis is that some stays with Airbnb serve as a substitute for certain hotel stays, thereby affecting hotel revenue, and that this impact is differentiated by geographic region, by hotel market segment, and by season. Although incumbent firms face higher fixed costs and offer less personalized products than peer-to-peer platforms, they have only recently begun to view competition from platforms such as Airbnb as a serious threat. For example, hotel executives have publicly issued largely dismissive statements regarding competitors like Airbnb, arguing either that these peer-to-peer platforms are a niche market or that they target complementary market segments from those targeted by hotel chains. Notably, Airbnb also appears to espouse this latter

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view: according to Airbnb, in many cities, over 70% of Airbnb properties are outside the main hotel districts,[1] suggesting complementarity of their offerings.

In this article, we provide empirical evidence to this debate by studying the differentiated impact of Airbnb's entry in the Texas hotel market on hotel room revenue. Our study explores the relationship between Airbnb and hotels in the state of Texas by estimating monthly hotel room revenue as a function of Airbnb entry in the market. Using data we collected from Airbnb, monthly hotel room revenue from approximately 3,000 hotels in Texas dating back to 2003, and several other auxiliary data sets to compile controls, we quantify the extent to which Airbnb's entry to the accommodation market has negatively affected hotel room revenue.

To identify the causal impact of Airbnb on hotel revenue, we employ a difference in differences (DD) empirical strategy. Specifically, because of the significant variability in both the temporal rate and the spatial density of Airbnb adoption in Texas, as well as the geographic specificity of both our hotel and Airbnb data sets, we are able to treat Airbnb market entry as a variable intervention in space and time against the hotel room revenue data. Our DD strategy identifies the Airbnb treatment effect by comparing differences in revenue for hotels in cities affected by Airbnb before and after Airbnb's entry with a baseline of differences in revenue for hotels in cities unaffected by Airbnb over the same period of time. To perform the analysis, we regress against two measures of Airbnb supply: (1) a cumulative measure that defines supply as all listings appearing prior to a given date in a given city and (2) an instantaneous measure that defines supply as those Airbnb listings active within a short (e.g., three-month) period. In all our specifications, we include a rich set of controls that vary by location and over time: population, wages, unemployment, total hotel room supply in each market, each hotel's own capacity over time, airport passenger counts, and the TripAdvisor ratings for each hotel as a proxy for quality. In addition to these measured covariates, we include city-specific trends and city-month dummies to account for seasonal variation in demand across different markets. Using our preferred cumulative specification, we find that, in Texas, each additional 10% increase in the size of the Airbnb market resulted in a .39% decrease in hotel room revenue, with similar but somewhat smaller estimated impacts using the instantaneous supply measure. These effects are primarily driven by Austin, where Airbnb inventory has grown extremely rapidly over the past few years, resulting in an estimated revenue impact of 8%–10% for the most vulnerable hotels in Austin.

We next investigate the market response to Airbnb entry and study the mechanisms whereby affected hotels might react to Airbnb's market entry both in the short run and in the long run. In the short run, likely responses could take the form of a price response or an occupancy response. Using hotel industry performance metrics as dependent variables, we find a small decrease in occupancy rate and a significant decrease in hotel room prices. Notably, such a price response benefits all consumers, not just participants in the sharing economy. With respect to longer-term responses, such as diminished investment or hotel entry and exit, we do not find evidence of an effect yet, consistent with evidence we present showing that the timescale of such a response would occur with a multiyear lag.

Our next set of results develops a more nuanced understanding of the mechanisms behind Airbnb's impact on hotel room revenue by unpacking the effects to study the differentiated impacts that Airbnb has had across hotels, cities, and time. First, given the nature of rentals currently on Airbnb, which typically provide fewer amenities and services than many hotels, we expect hotels that provide more differentiated services to be less affected. We examine three such cases in high-end hotels, chain hotels, and hotels catering to business travelers, each of which provide amenities that a typical Airbnb host does not. First, after segmenting hotels in five industry-standard price tiers (budget, economy, midprice, upscale, and luxury) we find that the impact of Airbnb is gradually magnified as we move down the price tiers. Then, through a similar analysis, using conference and meeting room space as a proxy for the extent to which a hotel caters to business travel, we find that the impact of Airbnb also falls disproportionately on hotels lacking conference facilities. Finally, we examine Airbnb's differential impact on chain hotels versus independent hotels and confirm our expectation that chain hotels will be less affected than independents for reasons ranging from chains' larger marketing budgets and stronger brands to their predictably consistent service.

In our final main result, we study the impact that Airbnb has during periods of peak demand, leveraging our instantaneous measure of supply. Use of this measure enables us first to confirm that there are significant seasonal fluctuations in city-level Airbnb supply that are correlated with periods of peak demand in those cities. We then study the impacts that Airbnb has exerted, year-over-year, during the highly popular South by Southwest (SXSW) festival in Austin and during the Texas State Fair in Dallas. Our finding is that Airbnb's ability to flexibly scale instantaneous supply in response to seasonal demand has significantly limited hotels' pricing power during periods of peak demand. Indeed, we argue that accommodating surges in demand through flexible scaling of supply is a defining feature of the sharing economy, and we interpret our result as evidence of the power of this capability, which appears difficult for incumbent firms such as hotels to directly counteract.

Finally, we conduct several robustness checks to support a causal interpretation of our estimates. First, we show that the basic set of controls included in our DD specification (i.e., hotel fixed effects and time trends) explain approximately 88% of the variation in Airbnb supply, whereas time-varying observables that could potentially drive hotel revenue have almost no additional explanatory power. Second, we check whether hotel performance drives Airbnb adoption, which would indicate that we have confused cause and effect. To the contrary, we find that a wide range of pre-Airbnb demographic and market characteristics—including, for example, hotel room prices, occupancy rates, and hotel room supply per city, which are all significant predictors of post-Airbnb hotel room revenue—are not correlated with the patterns of Airbnb adoption we see in our data. Third, we define a measure of competing Airbnb supply at a per hotel granularity, accounting for the geographic distance between the hotel and Airbnb inventory. This distance-based analysis shows a magnified negative impact from Airbnb on hotels as proximity between hotels and Airbnb inventory increases. Fourth, we show that

[1]See http://blog.airbnb.com/economic-impact-airbnb/.

our results are robust to alternative measures of Airbnb supply. Finally, in a separate analysis, we combine DD with coarsened exact matching (CEM; Iacus, King, and Porro 2012). Specifically, we match each "treated" hotel affected by Airbnb to a "control" hotel belonging to the same price tier and sharing the same affiliation (e.g., an upscale Hilton in Austin where Airbnb adoption is high, and an upscale Hilton in Dallas where Airbnb penetration is low), discarding hotels that remain unmatched. We find that our CEM estimate is similar to our main analysis. Taken together, these robustness checks provide significant support for the assumptions underlying our DD analysis. We conclude this article by discussing managerial and policy implications related to the rapid growth of Airbnb specifically and the sharing economy more broadly.

RELATED WORK

Relatively few studies have investigated competition between peer-to-peer markets and incumbent firms offering similar goods or services. In one line of recent work, Einav, Farronato, and Levin (2016) discuss the design and regulation of peer-to-peer markets and provide theoretical predictions of the effects of competition from these markets on incumbent firms. A key prediction they make, which is borne out in our data, is that peer-to-peer markets can reduce price variability by flexibly scaling supply to accommodate increased demand. As for empirical work, a handful of studies have examined the adoption and effects of car sharing; for example, two studies have used survey analysis methods to find that car sharing is associated with significant decreases in miles traveled, gasoline consumption, and car ownership (Cervero, Golub, and Nee 2007; Martin, Shaheen, and Lidicker 2010). In the domain of accommodation sharing, we find numerous opinion pieces in the popular press and on blogs, but little in the way of academic literature. Our closest comparison point is a set of short studies, commissioned by Airbnb, which claim that the Airbnb business model is complementary to the hotel industry but primarily focus on arguing for and quantifying the substantial net economic benefit to cities that Airbnb travelers provide.[2] Although our work is related to these studies, we apply a more sophisticated identification strategy, methodology, and segmentation analysis, resulting in conclusions that are both different and more nuanced. Notably, recent analyses have confirmed our initial findings in Texas in other markets; for example, Credit Suisse analysts used STR data to estimate that in New York City, Airbnb caused January 2015 revenue per hotel room to decline by 18.6%, year over year (Phillips 2015).

Our work contributes to the growing literature on multisided platform competition, as Airbnb exemplifies a two-sided platform. Much of this literature has established the economic theory of two-sided markets—for example, through structural models that establish theories of price structure and usage (Rochet and Tirole 2003; Rysman 2009; Weyl 2010), and models that connect innovations in product design to network effects (Parker and Van Alstyne 2005). Other work, more closely related to our own, has contributed empirical results to the literature that try to explain the behavior of firms and people in two-sided markets (Jin and Rysman 2012), including the role of multihoming (Landsman and Stremersch 2011), modeling response to regulation (Valverde, Chakravorti, and Fernandez 2010), and understanding the supply-side labor market (Hall and Krueger 2015). In contrast, our work empirically studies a setting in which a peer-to-peer market offers a substitute for consumer services supplied by traditional firms.

It is in this context that our research contributes to the literature on substitution between peer-to-peer markets and incumbent firms, because markets such as Airbnb can be viewed as providing enabling technology that facilitates suppliers of niche inventory to flexibly bring their products to market. Unlike traditional markets, Airbnb provides sufficiently low cost of revenue for people to profitably list remnant inventory online; moreover, Airbnb provides enhanced reach by reducing consumer search costs (Bakos 1997). As such, our study can be viewed as investigating the consequences of an online platform lowering the barrier to entry for suppliers. Related work has studied similar examples in other domains. For example, several recent studies have focused on the impact of Craigslist—a website featuring free online classified ads—on the newspaper industry (Kroft and Pope 2014; Seamans and Zhu 2013).

Finally, our work contributes to the literature focusing on the impact of external shocks on the tourism and the hospitality industry. However, much of the prior work in this area has centered on demand shocks. For example, O'Connor, Stafford, and Gallagher (2008) study the impact of terrorism on tourism in Ireland; Baker and Coulter (2007) estimate the impact of the 2002 and 2005 terrorist attacks in Bali on the islands' vendors. Similarly, Kosová and Enz (2012) examine the adverse effects of the September 11 attack and the 2008 financial crisis on hotel performance.

DATA AND THE AIRBNB PLATFORM

For our study, we collect and combine data from various sources including the Airbnb website, the Texas Comptroller Office, STR, county demographics from the U.S. Census Bureau, airport passenger counts from the U.S. Bureau of Transportation Statistics, the Current Population Survey from the U.S. Bureau of Labor Statistics, and hotel reviews from TripAdvisor.

The Airbnb Platform

Much of the data used in our study is collected directly from the Airbnb website. Airbnb describes itself as "a trusted community marketplace for people to list, discover, and book unique accommodations around the world," and it exemplifies a peer-to-peer marketplace in the sharing economy. Prospective hosts list their spare rooms or apartments on the Airbnb platform; establish their own nightly, weekly or monthly price; and offer accommodation to guests. Airbnb derives revenue from both guests and hosts for this service: guests pay a 9%–12% service fee for each reservation they make, depending on the length of their stay, and hosts pay a 3% service fee to cover the cost of processing payments. Since its launch in 2008, the Airbnb online marketplace has experienced very rapid growth, with more than two million properties worldwide and over 50 million guests who have used the service by September 2015 (Airbnb 2015).

Airbnb's business model currently operates with minimal regulatory controls in most locations, and as a result, both hosts and guests have incentives to use signaling mechanisms to build trust and maximize the likelihood of a successful booking. To

[2] See https://www.airbnb.com/economic-impact/.

reinforce this behavior, Airbnb has built an online reputation system that enables and encourages participants to rate and review each completed stay. Guests use star ratings to rate features of their stay (e.g., cleanliness, location, communication) while both guests and hosts are encouraged to post public reviews of each stay on the platform.

Airbnb Listings Data

To estimate the extent of Airbnb's market entry, we collected consumer-facing information from Airbnb.com on the complete set of users who had listed their properties in the state of Texas for rental on Airbnb. We refer to these users as "hosts" and their properties as their "listings." Each host is associated with a set of attributes including a photo, a personal statement, their listings, guest reviews of their properties, and Airbnb-certified contact information. Similarly, each listing displays attributes including location, price, a brief description, photos, capacity, availability, check-in and checkout times, cleaning fees, and security deposits. Our collected data set contains detailed information on 10,555 distinct hosts and 13,395 distinct listings spanning a period from January 2008 to August 2014.

To conduct our analysis, we must choose an appropriate level of geographic aggregation. Here, our data are suitably granular (with location accuracy to roughly 100 meters) to permit analysis at many different scales. Our preferred specification employs city-level granularity and is driven by the observation that a city is the largest geographic unit within which we reasonably expect to see significant substitution patterns between hotels and Airbnb properties. However, distance-based measures also arguably have operational validity. We discuss these along with our other modeling decisions and robustness checks.

Another central element of our analysis is to accurately quantify Airbnb supply; however, this cannot be directly inferred from available data and is thus a highly nuanced modeling decision. Indeed, inferring instantaneous Airbnb supply is a challenging task even for Airbnb itself because of "stale vacancies" (i.e., Airbnb listings that appear to be part of available supply only because the hosts neglected to update the availability status of those listings). By analyzing proprietary Airbnb data, Fradkin (2014) finds that between 21% and 32% of guest requests are rejected as a result of this effect.

Despite imperfect information, we do have substantial data with which to construct proxies for supply—namely, the date that hosts became Airbnb members and the date for each review of each property. Significantly, Fradkin et al. (2014) report that 67% of Airbnb guests left a review about their stay across their large data set. For market entry, we can estimate the (unobservable) entry date of individual listings either by using the date their owners became Airbnb members or by the date of the first review. Similarly, we can construct proxies for both cumulative and instantaneous supply by leveraging the review histories we compile. We detail and justify our approach in a subsequent section.

Hotel Data: Revenue, Prices, and Occupancy Rates

The main dependent variable we use in our analysis is monthly hotel room revenue, which we obtained from public records furnished by the Texas Comptroller of Public Accounts, in their capacity as auditors of state tax collection. In addition to monthly hotel room revenue, the data set includes basic information including hotel name, address, and capacity. The raw data set spans the period between January 2003 and August 2014.

Notably, according to Texas law, "a hotel is considered to be any building in which members of the public rent sleeping accommodations for 15 or more per day."[3] For this reason, revenue from Airbnb properties (as well as various other vacation rental options) whose owners are in compliance with the Texas tax code is also reported in this data set. This is evident from Figure 1, which plots the number of unique tax-paying properties in Austin broken down by capacity (i.e., maximum occupancy). We conjecture that the rapid increase in low-capacity properties starting in 2008 is related to Airbnb's entry into the Texas market at the same time. To exclude non-hotel properties from our analysis of impact on hotels, we cross-reference the Texas Comptroller data set with the U.S. hotel census data provided to us by STR. The STR census includes all U.S. hotels and contains a rich attribute set for each hotel, including its opening date, price segment, capacity, operation type (chain vs. independent), and geographic location. In total, the STR data set contains information on 3,747 hotels in Texas metropolitan areas. After linking the STR census data set with the Texas tax data set, we obtain high-confidence matches for a panel of 3,619 properties (96% of STR hotels, which account for over 95% of the revenue in our data).

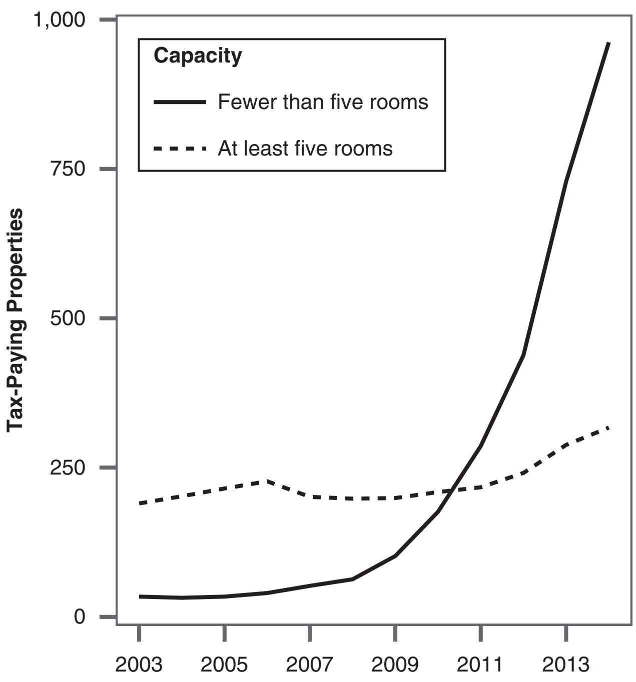
Airbnb can affect hotel room revenue through lower occupancy rates, decreased hotel room prices, or a combination of these two factors, conventionally reported within the hotel and hospitality industry as revenue per available room (RevPAR), which is the product of average room price and occupancy. Because the data we obtained from the Texas Comptroller's office does not report either occupancy rates or hotel room prices, we obtain additional data on these quantities for a subset of Texas hotels from STR. The room price, also referred to as average daily rate (ADR), and occupancy rate data from STR covers a subset of 2,584 hotels in Texas who chose to report this information to STR over the same time period (January 2003 through August 2014).

Auxiliary Data Sources

We assemble a set of control variables derived from publicly available sources. First, for each hotel we collect its entire TripAdvisor review history—a total of 424,583 reviews. We then use TripAdvisor star ratings to control for changes in hotel quality over our observation period. Second, we collect passenger arrival data for all Texas airports from the Bureau of Transportation Statistics. We then associate each city in Texas with its nearest airport and use the passenger data to control for changes in tourism demand over time that are unrelated to Airbnb. The data are a monthly panel of passenger counts, in which we exclude passengers connecting through Texas airports. Third, we obtain monthly unemployment and wage data at the metropolitan statistical area–level from the Bureau of Labor Statistics (https://www.bls.gov/). Unemployment statistics are updated monthly, while the wage data, which comes from the Occupational Employment Statistics Survey, is updated once a year. Finally, we obtain demographic

[3]See https://comptroller.texas.gov/taxes/audit/manuals/hotel/ch2.php.

Figure 1
ANNUAL COUNTS OF AUSTIN PROPERTIES THAT PAY HOTEL OCCUPANCY TAX, BROKEN DOWN BY CAPACITY



information at the county level from the U.S. Census Bureau (https://www.census.gov/).

QUANTIFYING AIRBNB'S IMPACT IN TEXAS

Empirical Strategy

Airbnb has had widely varying degrees of traction within different local, regional, and international markets both with respect to initial market entry and the rate at which it has been adopted within markets. For example, consider Figure 2, which depicts the current extent of market penetration both of Airbnb properties and hotels within the state of Texas and within the county encompassing the state capital, Austin. Unlike hotels, which have coverage throughout the state and pockets of local density, such as in downtown Austin, Airbnb has spotty coverage at best throughout the state but broader coverage across metro areas, including suburbs and exurbs. Table 1 reveals that, over the past eight years in the ten most populous cities in Texas, patterns of Airbnb adoption are themselves diverse, with several cities experiencing early adoption and rapid growth, whereas others experiencing minimal Airbnb adoption. Our empirical strategy exploits this variability to identify the impact of Airbnb's rise on hotel room revenue using a DD identification strategy. Specifically, we estimate Airbnb's impact on hotel room revenue by comparing changes in hotel room revenue before and after Airbnb enters a specific city with a baseline of changes in hotel room revenue in cities with no Airbnb presence over the same period of time.

The key identification assumption we must make to support a causal interpretation of this DD estimate is that there are no unobserved, time-varying, city-specific factors that are correlated with both Airbnb entry and hotel room revenue, resulting in endogeneity. Stated differently, we assume that unobserved factors that could potentially jointly affect both Airbnb adoption and hotel room revenue do not systematically vary both between different cities and over time. For instance, the following unobserved factors are accounted for in our estimate and do not bias our estimates: (1) city-specific, time-invariant differences in adoption rates (e.g., consumers in Austin overall being more likely to adopt Airbnb than consumers in Dallas); (2) factors that vary arbitrarily over time but do not vary across cities (e.g., a generally increasing awareness of Airbnb shared across all consumers in Texas over time); and (3) city-specific trends, which allow for unobserved confounders that vary both between cities and over time according to a prespecified functional form (linear or quadratic).

Our DD specification takes the following form:

$$\text{(1)} \quad \log \text{Hotel Revenue}_{ikt} = \beta \log \text{Airbnb Supply}_{kt} + X'_{ikt}\gamma + h_i + \tau_t + \text{City}_k \times \text{Month}_t + \epsilon_{ikt}.$$

The dependent variable is the log of monthly room revenue of hotel i in city k at time t. Our model includes hotel fixed effects h_i, and time (year-month) fixed effects τ_t. To implement the DD strategy, we define treated hotels to be those hotels in cities with an Airbnb presence, and nontreated hotel to be those hotels in cities with no Airbnb presence. The first difference is taken using the hotel fixed effects, which allow for time-invariant differences in hotel room revenue between treated hotels and nontreated hotels. The second difference in our DD specification is taken over time using year-month fixed effects τ_t, which allow for unobserved time-varying revenue differences that are common across different cities.

Figure 2
GEOGRAPHICAL DISTRIBUTION OF HOTELS AND AIRBNB LISTINGS IN THE STATE OF TEXAS AND IN TRAVIS COUNTY, TEXAS IN 2013

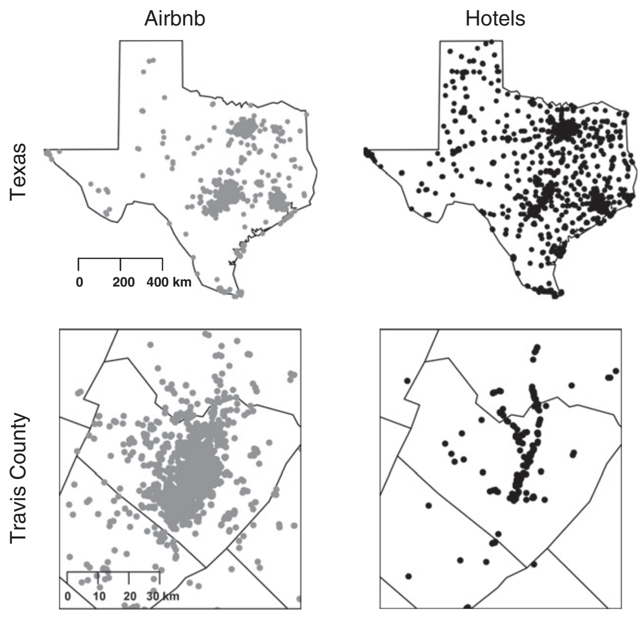


Table 1
AIRBNB'S SPATIAL AND TEMPORAL PENETRATION

	Houston	San Antonio	Dallas	Austin	Fort Worth	El Paso	Arlington	Corpus Christi	Plano	Laredo
Population (millions)	2.16	1.38	1.24	.84	.78	.67	.38	.31	.27	.24
No. of Airbnb listings in ...										
2008	1	9	0	25	0	0	0	0	0	0
2009	6	13	7	146	2	0	1	0	0	0
2010	39	22	23	468	10	0	3	0	1	0
2011	169	72	109	1,862	34	3	19	7	5	1
2012	425	171	271	5,158	68	8	27	24	20	1
2013	695	271	422	7,489	93	23	36	49	33	1
2014	891	346	526	8,575	114	31	52	60	44	2

Notes: This table presents cumulative counts of Airbnb listings per year in the ten most populous Texas cities.

The coefficient of interest is β, which has the usual DD interpretation: it is an estimate of the percentage change in hotel room revenue in treated (Airbnb-adopting) cities after Airbnb's entry compared with a baseline of changes in hotel room revenue over the same period in untreated (non-adopting) cities. We interpret a statistically significant negative coefficient on Airbnb supply as indicating that Airbnb listings lead to Airbnb bookings that substitute for hotel stays and affect hotel room revenue. We interpret a coefficient that is not statistically significantly different from zero as indicating that Airbnb listings having no effect on hotels. We interpret a positive coefficient (though implausible) as indicating that Airbnb listings benefit hotels. Next, we elaborate several measures of Airbnb supply that we employ in Equation 1 and the various economic impacts each measure can identify.

Modeling Airbnb Supply

Our first approach uses a cumulative measure of Airbnb supply, quantified at the granularity of individual cities: for a given city and date, we count the number of distinct listings that have cumulatively appeared on Airbnb in that city prior to that date. We approximate the unobservable entry date of individual listings by using the displayed date their owners became Airbnb members. By construction, a weakness of the cumulative measure of Airbnb supply is that it ignores listing exit, which we do not observe in our data. Therefore, our estimate of Airbnb's impact will be consistent if the unobserved fraction of active Airbnb listings is not endogenously correlated with cumulative listing supply and hotel revenue. To demonstrate when the (observed) cumulative supply and (unobserved) actual monthly supply yield the same consistent estimate, we relate cumulative supply to actual supply through a set of (unobserved) multipliers $f_{kt} \in [0, 1]$, such that Actual Airbnb $\text{Supply}_{kt} = f_{kt} \times$ Cum. Airbnb Supply_{kt}. Here, f_{kt} is the fraction of Airbnb listings that entered the market prior to time t and are still actively in the market at time t. Because we work with a log-log specification, f_{kt} becomes an unobserved quantity that enters the error term additively. Therefore, only residual variation in f_{kt} after controlling for observables, fixed effects, and trends that is correlated with residual cumulative supply, will cause bias.

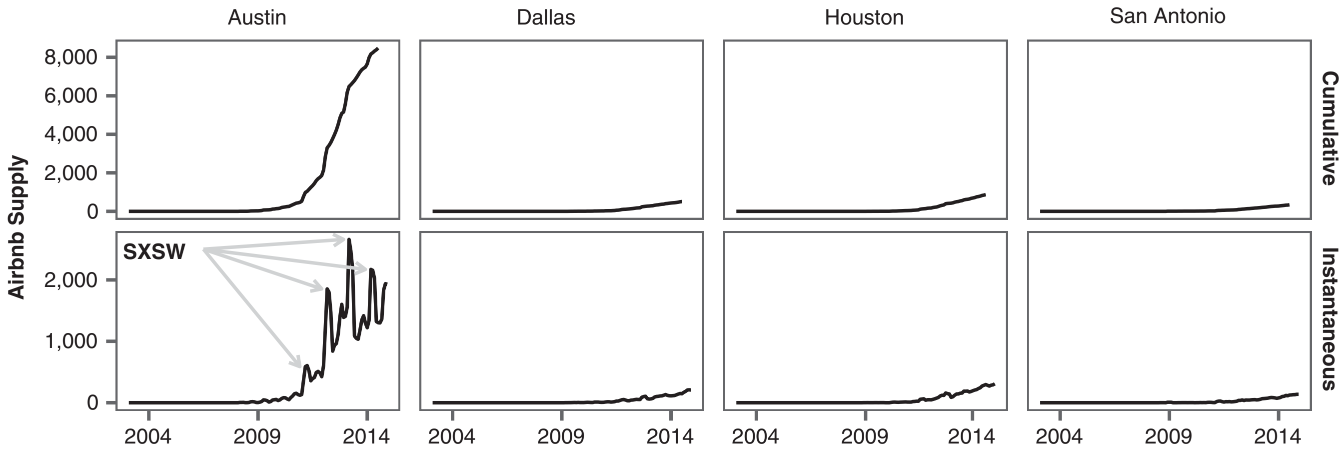
Our second approach employs an instantaneous proxy measure of actual Airbnb supply. To build an instantaneous measure, we exploit the fact that Airbnb requires guests who wish to submit a review to do so within 14 days of a stay and reports the checkout date (with monthly precision) in each review; thus, listings that receive a review must be on the market at that time. Moreover, the incidence of reviewing is high: Fradkin et al. (2014) report that 67% of Airbnb stays in their large data set resulted in a review. Taken together, these two facts indicate that a time series of Airbnb reviews reflects time-varying supply. For each Airbnb listing in our data, we observe its entire historical record of reviews, which includes reviews for the listing as well as reviews for each guest (by the host). Using the review data set, we apply the following heuristic to determine when each Airbnb listing was active: When an Airbnb listing enters the market, we assume that it remains active for m months, which we refer to as the listing's time to live (TTL); whenever a listing is reviewed, its TTL is extended by m months from the date of the review; if a listing exceeds its TTL, it exits the market; finally, listings become active again after exiting the market if they receive a new review.

The main advantage of the instantaneous supply measure is that it can capture a key differentiating feature of Airbnb: its ability to scale supply. This measure both has descriptive value and enables us to confirm that our results are not driven by our choice of a cumulative supply measure. A limitation of the instantaneous supply measure, arising from the way we construct it, is that it may underestimate Airbnb inventory in the low season. During low season, Airbnb listings face lower demand, which in turn leads to fewer reviews. Therefore, during the off-season, some listings that are available may receive zero reviews and thus be misclassified as unavailable.

Figure 3 compares the cumulative and instantaneous Airbnb supply measures for the four largest cities in our data. We see that our instantaneous Airbnb supply measure fluctuates significantly over time, differentiating it from our cumulative supply measure. Moreover, its pattern of variation over time correlates with periods when we would expect Airbnb supply to be highest, such as March in Austin, when the SXSW festival takes place.

A final issue that pertains to both measures of Airbnb supply that we must address is that although the unit of analysis is hotel monthly room revenue, the treatment, Airbnb adoption, occurs at the city level. This mismatch in the level at which we measure our dependent variable compared with the treatment variable can result in understating the standard error of the estimate of Airbnb's impact, because it is likely that hotel room revenue is serially correlated over time within a city. We correct for this mismatch by clustering standard errors at the city level, which lets us account for possible serial correlation

Figure 3
CUMULATIVE VERSUS INSTANTANEOUS AIRBNB



Notes: The seasonal peaks of the Austin instantaneous supply curve correspond to SXSW.

in hotel room revenue. In doing so, we follow the standard practice in the literature for analyzing panel data in a DD setting (Bertrand, Duflo, and Mullainathan 2004; Donald and Lang 2007). We report standard errors clustered at the city level for all subsequent regressions.

Incorporating Controls: Hotel Supply and Quality, Demand Shifters, and Demographics

An initial identification challenge we face is that increased demand for accommodation is likely correlated with increases in both Airbnb supply and hotel room supply. Concretely, it is plausible that over our decade-long observation period, hotel firms have been strategically developing new properties in areas of anticipated high demand. As high demand could also correlate with increased Airbnb adoption, this pattern of competition could bias our estimation, because city-specific increases in hotel room supply could decrease per hotel room revenue, and this effect could be misattributed to increased Airbnb adoption. To guard against this concern, we construct a control variable Hotel Room Supply_{-ikt}, which measures the total supply of hotel rooms in the same city as hotel i (but excluding hotel i itself, thus the −i in the subscript) for each time t. To construct this variable, we rely on the same monthly panel of tax reports provided by the Texas Comptroller because, in addition to revenue, taxpayers have to report the capacity of their properties with each filing. Therefore, Hotel Room Supply_{-ikt} captures changes in competitors' total room supply over time, including changes resulting from hotels expanding or shrinking and entering or exiting the market. This control, which we also incorporate in X_{ikt}, allows increases in the supply of hotel rooms provided by competitors to affect the room revenue of each hotel in our data, much as we hypothesize an increase in Airbnb rooms does. In addition, we control for hotel i's own capacity and quality over time, both of which may change (e.g., following renovations). We derive hotel capacity from the tax data, and we use TripAdvisor ratings as a proxy for quality.

Second, as we explained previously, our DD estimate will be biased if there exist unobserved factors that vary across cities and over time and jointly influence Airbnb entry and hotel room revenue—most notably, demand for accommodation. This type of bias likely works against finding a negative Airbnb effect: both Airbnb supply and hotel revenue should respond positively to shifts in accommodation demand, which implies that if we omit a control for demand, then Airbnb supply will absorb its effect and become biased upwards.

We use three types of controls to account for variation in accommodation demand across different cities. First, we include quadratic city-specific trends as a control in X_{ikt}. The inclusion of these trends relaxes the DD assumption of no cross-city time-varying unobservables that are correlated with both Airbnb supply and hotel revenue. A concern with the inclusion of city-specific time-trends is that they can be confounded with hotels' response to Airbnb (Wolfers 2006). Fortunately, our data set covers a long pre-Airbnb period from 2003 to 2008, allowing us to estimate these trends on a large sample of pretreatment observations. Second, we include city-month (e.g., Austin-March) fixed effects to control for differences in seasonal demand patterns across the different cities. For instance, March in Austin is especially popular because of the SXSW festival. The city-month fixed effects control for such seasonal differences. Finally, we associate each city in our data with the nearest airport and use the (log of the) number of passengers disembarking at that airport as their final destination as a control.

An additional issue relates to the unobserved incentives of consumers who choose to list their homes on Airbnb. For example, Airbnb touts the help it provides to struggling or unemployed homeowners in paying their mortgage (Primack 2012). Conceivably, an increase in the unemployment rate could simultaneously drive Airbnb adoption and independently cause demand for hotels to soften. Therefore, failure to control for cross-city differences in the demographics could potentially bias our estimation. In this case, the bias likely works in favor of finding a negative Airbnb impact. To address this concern, we incorporate unemployment rate, the median annual wage, and population as controls in X_{ikt}.

Identification Checks

Before proceeding with estimation, we conduct a series of identification checks to assess whether our proposed empirical strategy is capable of recovering Airbnb's causal impact on hotel room revenue. Our DD identification strategy relies on randomness in Airbnb adoption with respect to unobserved city-specific time-varying factors (ϵ_{ikt}) that are also correlated with changes in hotel room revenue (conditional on the control variables we include). As with any study relying on observational data, there is no conclusive test of this assumption. However, we can exploit the richness of our data to check if this assumption is likely to hold in practice. Similar to Akerman, Gaarder, and Mogstad (2015), we perform two checks that support the basis for our key identification assumption.

First, we show that most variation in Airbnb adoption is explained by regressing (the log of) Airbnb supply on time-invariant city-specific factors, time fixed effects, and city-specific trends—all of which are part of the DD model. These factors explain 88% of the variation in Airbnb adoption, suggesting that our modeling assumption has a sound basis in practice. Next, we repeat this regression with the addition of city-specific time-varying observables that could potentially be correlated with hotel room revenue: population, unemployment rate, and employment in the accommodation sector. The inclusion of these factors does not increase the explanatory power of the regression.

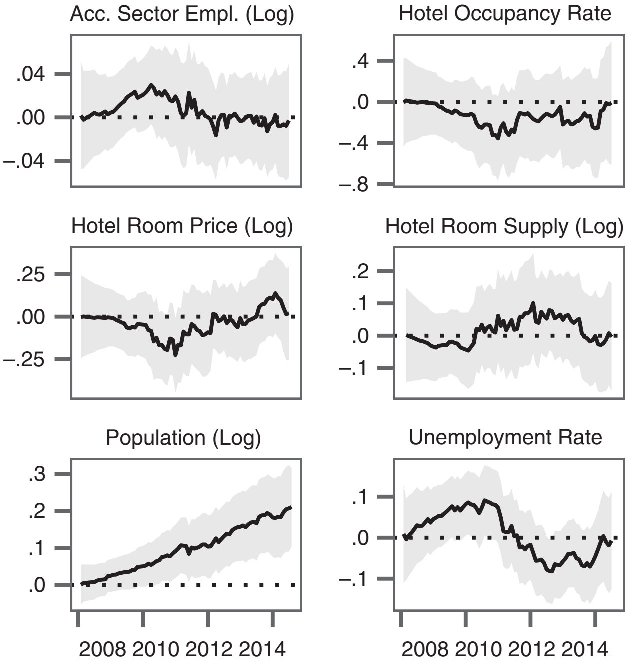
Second, we perform a randomization check by testing whether pretreatment city characteristics predict future Airbnb supply, where the time of treatment is taken to be 2008, when Airbnb entered the Texas market. The idea behind this test is that, assuming Airbnb adoption is exogenous (with respect to hotel revenue), it should not be correlated with pretreatment factors. To perform this identification check, for each city, we compute its most recent pretreatment (2007) population, unemployment rate, employment in the accommodation sector, hotel room supply, hotel room prices, and hotel occupancy rates. We then interact these predetermined factors ($Z_{k,2007}$) with a vector of posttreatment year-month fixed effects (τ_t) and regress them on Airbnb supply. Concretely, with the units of analysis being post-2007 city-months, we estimate:

$$\text{(2)} \qquad \log \text{Airbnb Supply}_{kt} = \text{City}_k + \left(\tau_t \times Z_{k,2007}\right)'\theta + e_{kt}.$$

Each coefficient in the vector of coefficients θ is interpreted as a correlation between a specific pretreatment characteristic and Airbnb adoption in each posttreatment period (from January 2008 onward). Figure 4 presents the estimated coefficients θ for each characteristic together with their 95% confidence intervals. The only significant association we find is between pre-Airbnb population and subsequent Airbnb adoption, and, for this reason, we include population as a control in all our specifications. There also appears to be a weak correlation with pre-Airbnb unemployment rate, further justifying the inclusion of county-level unemployment rates as a control in Equation 1. Beyond these associations, we find no other discernible trend in the remaining coefficients (whose 95% confidence intervals always include the zero point, or no effect). It is especially reassuring that the pretreatment hotel industry structure—as captured by hotel room supply, occupancy rates, room prices, and accommodation sector employment in 2007—do not predict Airbnb supply from 2008 onward.

Figure 4

CORRELATION BETWEEN AIRBNB SUPPLY AND PRE-AIRBNB (2007) CITY CHARACTERISTICS, WITH 95% CONFIDENCE INTERVALS



Here, we have shown that various factors potentially affecting hotel room revenue, including demographic trends as well as the structure and performance of the hospitality industry across different cities, are not correlated with local patterns of Airbnb adoption. These checks increase our confidence that the identification assumptions needed to estimate Airbnb's causal impact on hotel room revenue hold in our data.

Results and Economic Significance

We report the results of estimating Equation 1 using our cumulative Airbnb supply measure and incorporating the control variables in the first column of Table 2. We estimate the coefficient $\beta = -.039$ $(p < .01)$ or, equivalently, a 10% increase in Airbnb listings is associated with a statistically significant .39% decrease in monthly hotel room revenue. The estimated coefficients for the controls have the signs and magnitudes one would expect (e.g., increased hotel room supply and unemployment are both associated with decreased hotel room revenue), though we note that our estimate for β without any controls (not presented) is comparable ($\beta = -.035$, $p < .01$). As stated previously, we interpret a negative coefficient β as indicative of some Airbnb stays substituting for hotel stays in cities with an established Airbnb presence.

Our estimates are sensitive to the functional form of the city-specific time trends. Table 3 compares models without city-specific trends and with city-specific trends of increasing order. Without time trends, we estimate a positive (but insignificant) effect, whereas when trends are included, our estimate becomes negative and significant. To explain this observation, we hypothesize that city-specific demand trends drive both

Table 2
DD ESTIMATES OF THE IMPACT OF AIRBNB ON HOTEL ROOM REVENUE USING DIFFERENT MEASURES OF AIRBNB SUPPLY

	(1) Revenue	(2) Revenue	(3) Revenue
log Cum. Airbnb Supply	−.039***		
	(−4.40)		
log Inst. Airbnb Supply (TTL 3 mo.)		−.025***	
		(−2.82)	
log Inst. Airbnb Supply (TTL 6 mo.)			−.035***
			(−3.92)
log Hotel Room Supply	−.157***	−.154***	−.156***
	(−6.25)	(−6.12)	(−6.21)
log Capacity	.034	.034	.034
	(1.50)	(1.50)	(1.50)
log Median Annual Wage	−.212	−.364	−.290
	(−.60)	(−1.01)	(−.82)
Unemployment Rate	−.060***	−.058***	−.058***
	(−4.48)	(−3.98)	(−4.10)
log Population	.049	.061	.030
	(.33)	(.42)	(.21)
log Airline Passengers	.150***	.138***	.148***
	(3.24)	(2.94)	(3.23)
Is Reviewed	−.057***	−.056***	−.057***
	(−3.03)	(−2.94)	(−2.98)
TripAdvisor Star Rating	.031***	.031***	.031***
	(6.93)	(6.81)	(6.86)
N	294,383	294,383	294,383
Within R^2	.013	.011	.012

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: The dependent variable is log Hotel Revenue_{ikt}. Cluster-robust t-statistics (at the city level) are shown in parentheses. All specifications include hotel fixed effects, year-month fixed effects, city-month fixed effects, and a city-specific quadratic time trend.

hotel revenue and Airbnb supply. Therefore, when we omit city-specific trends from the model, Airbnb supply stands in for the omitted trend and becomes biased upward. In other words, increasing Airbnb supply is a sign of increasing demand for accommodation. This analysis guides the functional form we choose to control for city-specific trends: because our estimates remain unchanged when we move from quadratic to cubic trends, we settle for the simpler quadratic form.

The economic significance of this estimate is best understood in the context of Airbnb's growth. For instance, in Austin, the Texas city with the highest Airbnb penetration, we estimate that the impact of Airbnb over the past five years is approximately 10% of hotel room revenue (this calculation is based on an increase in cumulative Airbnb supply from approximately 450 listings in 2010 to over 8,500 listings in 2014, yielding a revenue impact of $1-(8,500/450)^{-.039}$). Considering the high fixed costs associated with operating a hotel, this figure could represent a significant fraction of hotel profits.

An alternative way to assess the economic significance of Airbnb is through a direct comparison of the effects of Airbnb and hotel room supply on hotel revenue. By interpreting the coefficient of log Hotel Room Supply in the first column of Table 2, we find that a 10% increase in the supply of hotel rooms in Texas is associated with an approximately 1.6% decrease in Texas hotel room revenue, as compared with the smaller .39% decrease associated with a 10% increase in Airbnb supply. It makes intuitive sense that increasing Airbnb supply has a smaller impact than increasing hotel room supply, because we do not expect all Airbnb stays to substitute for a hotel room stay. Nevertheless, the two effects are surprisingly comparable in size: an increase in Airbnb supply has one-fourth the negative revenue impact of a corresponding increase in hotel room supply. Taken at face value, this suggests that incremental Texas Airbnb inventory does weakly substitute for incremental hotel inventory. In addition, although the impact of additional Airbnb supply is not as large, the significantly higher costs associated with increasing hotel room supply implies that hotels are less likely to be able to expand inventory as rapidly, an issue we return to subsequently.

Next, we estimate Equation 1 using our instantaneous Airbnb supply measure. We present these results in the second and third columns of Table 2. In the second column, we use a TTL of three months, while in the third column, we use a TTL of six months. In both cases, we obtain negative and significant estimates, though the three-month TTL estimate for β is smaller than the six-month TTL estimate (−.025 vs. −.035, $p < .01$ for both estimates). Our conclusion is that regressing on either a cumulative or an instantaneous measure of Airbnb supply captures a significant effect on hotel revenues due to Airbnb.

This analysis reveals an additional insight regarding Airbnb's economic impact: the significant fluctuation in instantaneous Airbnb supply suggests that Airbnb's impact on hotel revenue will vary significantly over time. For instance, in our data, we estimate that instantaneous Airbnb supply during SXSW has historically been approximately 60% higher than

Table 3
DD ESTIMATES OF THE IMPACT OF AIRBNB ON HOTEL ROOM REVENUE USING CITY-SPECIFIC TRENDS OF INCREASING ORDER

	(1) No Trends	(2) Linear	(3) Quadratic	(4) Cubic
log Cum. Airbnb Supply	.009	−.025**	−.039***	−.039***
	(1.26)	(−2.48)	(−4.40)	(−3.29)
N	294,383	294,383	294,383	294,383
Adj. within R^2	.024	.012	.013	.011

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

the rest of the year. In turn, this suggests that Airbnb impact on hotel revenue is approximately 1.5 percentage points larger during SXSW (calculated as $\log(1.6) \times .035$).

Variation in instantaneous supply is not the only reason why Airbnb's impact could be more pronounced during SXSW or during other large events. Perhaps it is the case that Airbnb is especially appealing to SXSW participants but has little or no appeal to travelers the rest of the year. However, we find that this is not the case: when we censor SXSW from our data, the elasticity that we estimate is unchanged ($\beta = -.039$, $p < .05$) using our cumulative supply measure. This result suggests that Airbnb's impact is not solely due to idiosyncratic preferences of the SXSW demographic.

In summary, we find evidence that Airbnb's impact in Texas is observable through the lens of both cumulative and instantaneous supply measures. We further find that although its impact is most strongly concentrated in Austin and has maximum impact correlated with periods of peak demand, the impacts are present year-round. Using the instantaneous measure, we attributed seasonal variation in impact to a feature that is unique to the sharing economy: supply flexibility. We subsequently refine this top-level analysis to study how the economic impacts are differentiated across different types of hotels and further unpack the effects of supply flexibility on the peak pricing power of hotels.

Hotels' Responses to Airbnb: Price, Occupancy, Entry, and Exit

So far, we have measured Airbnb's impact in terms of hotel revenue. Next, we turn to the nature of responses by incumbent hotels to Airbnb market entry. In the short run, hotels could plausibly respond to Airbnb market entry through a price response, an occupancy response, or both. In the long run, Airbnb could cause hotel investments to change course, ultimately affecting market entry and exit. All these impacts can be investigated naturally by measuring alternative dependent variables other than revenue.

Recall that hotel room revenue is the product of two quantities: average occupancy rate within a given time period and ADR during that same period. A hotel that exerts no response to a supply shock would exhibit a reduction in occupancy, whereas an active manager could alternatively maintain occupancy levels through a price response. A notable difference between the two responses is that the latter response, reduced prices, is a net benefit for all consumers seeking accommodations, whether they use Airbnb or not.

To estimate these effects, we reestimate the DD specification in Equation 1, substituting the dependent variable first with occupancy rate and then with the log of ADR, and retaining the controls. Similar to the room revenue analysis, these two quantities vary by hotel and by month. The price and occupancy data set that we use masks individual hotel identities; therefore, we cannot link it with the TripAdvisor data on a hotel-by-hotel basis. Instead, we control for changes in hotel quality at the city level using the average hotel rating and fraction of reviewed hotels in each city. We report these results in Table 4. As reported in the first column of this table, we find a small and weakly significant ($p < .1$) negative connection between increased Airbnb listings and occupancy rate. (Note that, in contrast to our other dependent variables, occupancy rate is already expressed as a percentage and therefore we do not log-transform it. Therefore, the coefficient of this regression has a level-log interpretation.) In the second column, we regress against ADR, and we find that a 10% increase in Airbnb supply is associated with a statistically significant ($p < .01$) price decrease of .19%. This suggests that affected hotels actively respond by lowering their prices. Note that this behavior is consistent with basic hotel revenue management practices, whereby hotels set prices according to the level of occupancy rates observed. Indeed, the hospitality industry has high fixed costs and low marginal costs, and therefore the thinking is that it is better to "put a head in a bed"—at a low price—than not at all. To understand the economic significance of these results, we can repeat the same calculation performed in the previous section, which suggests that in Austin, Airbnb negatively affected hotel prices by approximately 6%.

Both the price and occupancy effects we investigated constitute immediate responses to Airbnb. In the long run, Airbnb may also affect hotels' entry, exit, and investment decisions. To better understand the decision-making process and timetables of hotel development, we assembled a proprietary data set (from STR) that records all ongoing Texas hotel projects, including both new construction and renovations (we do not have access to the historical record of completed renovations). STR records the dates that projects enter their various phases of development. Using this data set, we computed the average time it takes to transition from one

Table 4

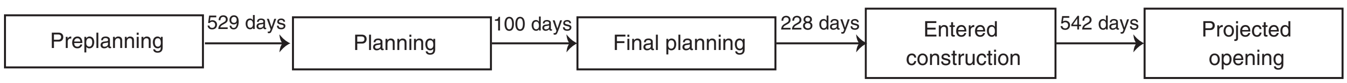
DD ESTIMATES OF THE IMPACT OF AIRBNB ON HOTEL OCCUPANCY RATES AND PRICES

	(1) Occupancy Rate	*(2) Room Price*
log Cum. Airbnb Supply	−.005*	−.019***
	(−1.66)	(−2.84)
log Hotel Room Supply	−.132***	−.060***
	(−8.36)	(−4.26)
log Capacity	.075***	−.007
	(5.98)	(−.43)
log Median Annual Wage	−.263	−.050
	(−1.65)	(−.26)
Unemployment Rate	−.025***	−.016**
	(−4.50)	(−2.47)
log Population	−.004	.140**
	(−.09)	(2.02)
log Airline Passengers	.012	.044**
	(.80)	(2.22)
Is Reviewed	−.060	−.129**
	(−1.34)	(−2.33)
TripAdvisor Star Rating	.002	.008**
	(.86)	(2.59)
N	264,172	264,172
Within R^2	.018	.012

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

Notes: The dependent variable is Occupancy Rate_{ikt} in column 1 and log Hotel Room Price_{ikt} in column 2. Cluster-robust t-statistics (at the city level) are in parentheses. All specifications include hotel fixed effects, year-month fixed effects, city-month fixed effects, and a city-specific quadratic time trend.

Figure 5
AVERAGE TIME BETWEEN VARIOUS STAGES IN THE HOTEL PIPELINE CONSTRUCTION



phase to the next, which we diagram in Figure 5. The average estimated time between preplanning and projected opening is approximately four years, though there exists significant variation depending on the project type. Therefore, hotel projects that were completed or were ongoing during our observation period were likely conceived before Airbnb became a concern for the hotel industry. Indeed, basic Poisson regressions of hotel entry and exit against Airbnb supply (not reported here) yielded no correlation. As Airbnb continues to become more established and hotels have time to incorporate Airbnb in their investment strategies, studying the nature of hotels' long-term response will be worth revisiting.

VARIATION OF IMPACT ACROSS HOTELS AND ACROSS TIME

Which Hotels Are Most Affected and Why?

We have provided evidence that Airbnb has a negative impact on hotel room revenue in Texas, treating hotels as a homogeneous set. In this section, we investigate various mechanisms through which Airbnb could exhibit *heterogeneous* impacts across different types of hotels and provide supporting empirical evidence. To motivate this analysis, we observe that although Airbnb can surely sometimes provide a suitable alternative to hotels, one can hardly expect it to be a perfect substitute for all travel needs. Because Airbnb has its roots in casual stays, including those involving shared accommodations, we expect it to be a more attractive option for travelers on a budget. Conversely, business travelers and vacationers who frequent high-end hotels are examples of consumer groups we argue are less likely to substitute a hotel stay with an Airbnb stay. Business travelers in particular are often less price-sensitive because they are typically reimbursed for their travel; moreover, they also make use of business-related hotel amenities not typically provided by Airbnb properties. Following this logic, we further isolate the impact of Airbnb on hotel room revenue by partitioning hotels in three ways—each dividing hotels into segments that we expect to be less vulnerable to Airbnb's entry and other segments that we expect to be more vulnerable—then estimating the additional interaction effects in our original DD specification. In our first partition, we segment hotels by price tier, following the STR hotel census, which divides hotels into five tiers: budget, economy, midprice, upscale, and luxury. In our second partition, we differentiate hotels by their customer base: those that target business travelers versus those that do not. Finally, we consider the differentiated impact on chain hotels versus independents.

To estimate heterogeneous treatment effects, we estimate a new specification that adds an interaction effect between hotel types and Airbnb supply to the DD specification in Equation 1:

(3)

$$\begin{aligned}\log \text{Hotel Revenue}_{ikt} = {} & \beta_1 \log \text{Airbnb Supply}_{kt} \\ & + \beta_2 \log \text{Airbnb Supply}_{kt} \times \text{Hotel Type}_i \\ & + X'_{ikt}\gamma + \alpha_i + \tau_t + \epsilon_{ikt}.\end{aligned}$$

The coefficient of interest is β_2, which captures the differential impact of Airbnb on the various segmentations by hotel type that we investigate. For our first segmentation, we define Hotel Type_i as a categorical variable identifying each of the hotel price segments used by STR. In the second and third segmentations, we define Hotel Type_i to be a binary indicator: whether or not hotel i has conference or meeting space and whether or not it is a chain, respectively.

The results of these analyses appear in Table 5. We start with price segmentation, presented in the first column. We estimate Equation 3, interacting hotel price segments with Airbnb supply. Here, we use luxury hotels as a reference category least affected by Airbnb, motivated by the observation that these hotels are least comparable to Airbnb based on average room price and amenities (e.g., pools, conference rooms, concierge). We find the negative impact of Airbnb increasing as we move down the price tiers, with statistically significant interaction coefficient estimates at the 1% level for each of the three lowest tiers (midprice, economy, and budget). In contrast, we find only a small negative and insignificant effect for the upscale and luxury segments (the latter being the reference level and thus its being captured by the main effect). From a managerial standpoint, this result has direct import: even though lower-end hotels in Texas account for a disproportionately small amount of room revenue as compared with upmarket hotels, they nevertheless bear the brunt of the impact of the market entry of Airbnb. Our evidence suggests that consumers are increasingly substituting Airbnb stays for lower-end hotels in Texas, possibly identifying the former as offering better value at a similar price point. Although this increased competition affords consumers greater choice, it also places lower-end hotels in regions with high Airbnb penetration at greater risk.

In the second column of Table 5, we report the results of the segmentation of hotels catering to business travelers. We use those hotels having conference and meeting space as the reference category. The estimated coefficient β_2 for the interaction between Airbnb supply and the indicator variable denoting absence of meeting space is negative and statistically significant ($-.015$, $p < .01$), suggesting that hotels lacking business facilities are more affected by Airbnb. These results are consistent with our prior segmentation as well as with Airbnb's marketing strategy to date, which has primarily targeted vacation travel. However, we note that, seeing a growth opportunity in the business travel segment, Airbnb

Table 5

DD ESTIMATES OF HETEROGENEITY IN AIRBNB'S IMPACT ON HOTEL ROOM REVENUE

	(1) Price Segment	(2) Meeting Space	(3) Operation
log Cum. Airbnb Supply	−.016 (−1.61)	−.033*** (−3.58)	−.038*** (−4.23)
Price segment × log Cum. Airbnb Supply (ref. Luxury) Budget	−.039*** (−5.39)		
Economy	−.031*** (−6.02)		
Midprice	−.020*** (−5.20)		
Upscale	−.007 (−1.45)		
w/o Meeting Space × log Cum. Airbnb Supply		−.015*** (−4.28)	
Independent × log Cum. Airbnb Supply			−.008** (−2.53)
log Hotel Room Supply	−.158*** (−6.26)	−.158*** (−6.27)	−.158*** (−6.26)
log Capacity	.034 (1.49)	.035 (1.53)	.033 (1.50)
log Median Annual Wage	−.225 (−.64)	−.219 (−.62)	−.215 (−.61)
Unemployment Rate	−.060*** (−4.46)	−.060*** (−4.46)	−.060*** (−4.47)
log Population	.086 (.63)	.058 (.39)	.047 (.31)
log Airline Passengers	.151*** (3.28)	.150*** (3.26)	.150*** (3.24)
Is Reviewed	−.032** (−2.12)	−.047*** (−2.64)	−.056*** (−2.97)
TripAdvisor Star Rating	.026*** (7.15)	.029*** (6.94)	.031*** (7.00)
N	294383	294383	294383
Within R^2	.018	.014	.013

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: The dependent variable is log Hotel Revenue$_{ikt}$. Cluster-robust t-statistics (at the city level) are shown in parentheses. All specifications include hotel fixed effects, year-month fixed effects, city-month fixed effects, and a city-specific quadratic time trend.

recently launched an initiative to attract more business travelers (Isaac 2014). An interesting open question going forward is to what extent business travel will continue to differentiate the impact of Airbnb on hotels.

The third distinction that we explore, which relates primarily to hotel operation, is between chain hotels (including franchises) and independent hotels. Unlike independent hotels, chain hotels allocate large marketing budgets to advertising, brand building, guest loyalty programs, and other tactics that should make them less vulnerable to competition. In addition, many chains provide a more predictable standard of service, which further differentiates them from both Airbnb and independent hotels. We present this analysis in the third column of Table 5, using chain hotels as a reference level. The overall effect from Airbnb remains negative and statistically significant (−.038, $p < .01$), suggesting that hotels of both operation structures were affected. However, the estimated interaction coefficient for independent hotels (−.008, $p < .05$) is also negative and statistically significant, suggesting that Airbnb has indeed had a slightly larger impact on independent hotels.

Overall, we find that independent hotels, hotels that do not cater to business travelers, and lower-end hotels are all more heavily affected by Airbnb than our respective reference categories, hotels without these characteristics. While these results help us better understand the most vulnerable hotel segments, and are certainly of importance to hoteliers, they also serve as robustness checks to our primary finding, in that the heterogeneous substitution effects they reveal align with the effects we hypothesized on the basis of the value proposition to consumers that Airbnb offers.

Airbnb and Peak Pricing Power of Hotels

Our analysis so far has focused on quantifying the extent to which Airbnb supply substitutes for hotel room supply and its differentiated impact across various hotels segments. Next, we show that Airbnb supply is more than just a partial substitute for low-end hotel supply by proposing and empirically evaluating mechanisms whereby changes in Airbnb supply exhibit fundamental differences from changes in hotel room supply. In particular, we investigate the ability of Airbnb suppliers to exhibit a more flexible response to peak seasonal demand and, in so doing, crimp operating margins of hotel operators during these peak periods.

During localized periods of peak demand, it is well understood that hotels can respond by raising prices,[4] but they cannot materially increase supply, because of high fixed costs of new inventory. In contrast, many of the microentrepreneurs providing Airbnb supply can elect to take inventory on and off the market on very short time scales and with near-zero cost. Thus, the aggregate decisions of Airbnb providers comprise both a price response and a supply response. Our subsequent analysis is therefore motivated by the hypothesis that during localized periods of peak demand, regions with flexible Airbnb supply more effectively absorb high seasonal demand than regions in which Airbnb is not present. If the hypothesis is operative, the managerial implication is that the hotel industry's ability to command high rents during peak periods, which we refer to as their "peak pricing power," has become diminished in regions where Airbnb has actively entered the market, as compared with other locales where Airbnb is less prevalent.

To motivate our definition of peak pricing power, consider that city-specific travel patterns are highly seasonal, and many periods of peak demand predictably recur with an annual frequency. Therefore, for each hotel-year in our data, we refer to peak demand months as the "high season," and the remaining months as the "low season." For each hotel i, we denote high-season prices during year y by $p^H_{i,y}$ and low-season prices by $p^L_{i,y}$. Given these two quantities, we define hotel i's peak pricing power as

$$P_{i,y} = \log p^H_{i,y} - \log p^L_{i,y}, \tag{4}$$

which can be interpreted as the percentage increase in prices during high season.

Because we are interested in understanding *changes* in—rather than absolute levels of—hotel pricing power as Airbnb adoption increases, the quantity we analyze is the first difference of peak pricing power:

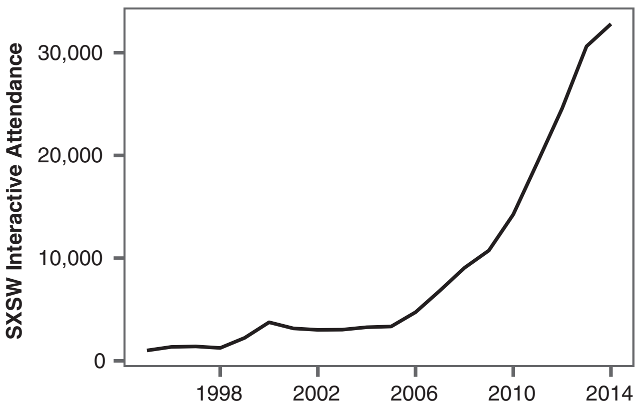
$$\Delta P_{i,y} = \left(\log p^H_{i,y} - \log p^L_{i,y}\right) - \left(\log p^H_{i,y-1} - \log p^L_{i,y-1}\right), \tag{5}$$

which can be interpreted as the year-over-year change in a hotel's ability to increase prices during the high season. Rearranging the terms in Equation 5 gives us the more convenient form:

$$\begin{aligned}\Delta P_{i,y} &= \left(\log p^H_{i,y} - \log p^H_{i,y-1}\right) - \left(\log p^L_{i,y} - \log p^L_{i,y-1}\right)\\ &= \Delta \log p^H_{i,y} - \Delta \log p^L_{i,y},\end{aligned} \tag{6}$$

[4]For example, see evidence of surge pricing coinciding with the annual shareholders' meeting of Berkshire Hathaway in Omaha (*The Economist* 2016).

Figure 6
SXSW INTERACTIVE ATTENDANCE

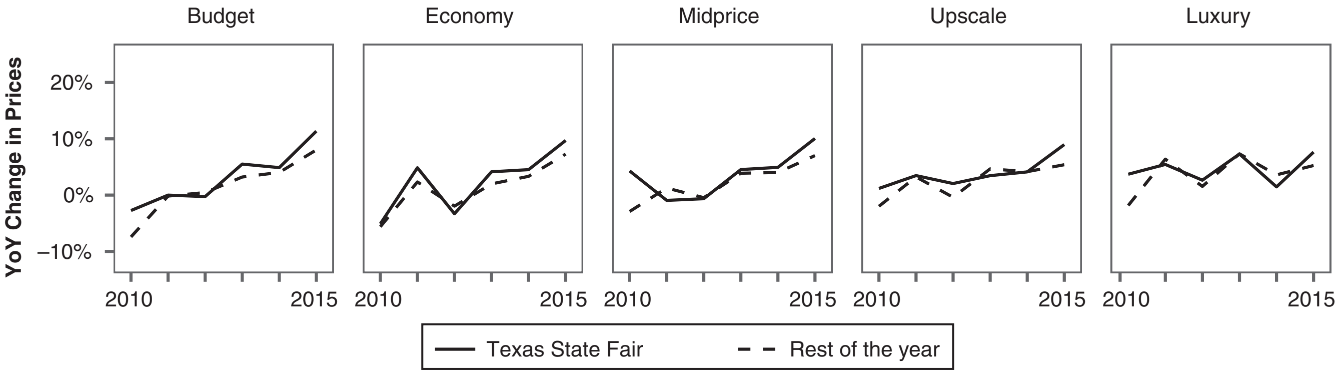


which is the difference between year-over-year changes in high-season prices and low-season prices. Intuitively, double differencing allows us to adjust changes in high-season pricing (likely related to flexible scaling of Airbnb supply) using low-season changes in pricing (likely unrelated to flexible Airbnb supply) as a baseline. For instance, if year-over-year percentage price changes are equal during high and low seasons, it is unlikely that they are jointly driven by Airbnb hosts flexibly scaling supply to accommodate peak demand during specific months of the year; thus, in this case, we estimate $\Delta P_{i,y}$ to be zero.

To study changes in peak pricing power of hotels in our data set, we considered the impact of two large events that take place annually in Texas: the SXSW festival in Austin in March, and the Texas State Fair in Dallas in October. Both events draw a large number of out-of-town visitors and have a substantial impact on area hotels' bottom line as a result. Both events have also grown in popularity in the past decade, though the much smaller SXSW festival has grown more rapidly in percentage terms. Figure 6 displays attendance for SXSW Interactive. Together with SXSW Film and SXSW Music, these are the major components of SXSW. March and October represent the peak months for demand of hotels in Austin and Dallas, respectively, measured in terms of both occupancy and ADR. In both cases, ADR and occupancy range between 8%–15% above the corresponding values for the rest of the year, consistently over the past decade. However, Airbnb has grown much faster in Austin than it has in Dallas, suggesting that if Airbnb affects peak pricing power, this effect will be more pronounced in Austin.

We begin our analysis by visualizing changes in peak pricing power. Motivated by our previous results, in which we found that Airbnb has a stronger impact on lower-end hotels, we segment hotels by price category and consider year-over-year changes in pricing power for the high season versus all other months combined. Following Equation 6, for each hotel, we compute year-over-year changes in high- and low-season prices (i.e., $\Delta \log p^H_{i,y}$ and $\Delta \log p^L_{i,y}$). Figure 7 displays the annual average of these quantities in Dallas for the period 2010–2014. The gap between the solid line (changes in high season prices) and the dashed line (changes in low season

Figure 7
YEAR-OVER-YEAR CHANGES IN DALLAS HOTEL PRICES, BROKEN DOWN BY HOTEL PRICE LEVEL



Notes: The solid line displays changes during the State Fair of Texas (October) while the dashed line displays changes for the rest of the year.

prices) can be interpreted as the year-over-year change in hotel pricing power during periods of peak demand. Visually, we see little discernible difference between the two lines, with the gap between them always close to zero. This suggests that the pricing power of hotels in Dallas during the state fair does not change significantly compared with the remainder of the year.

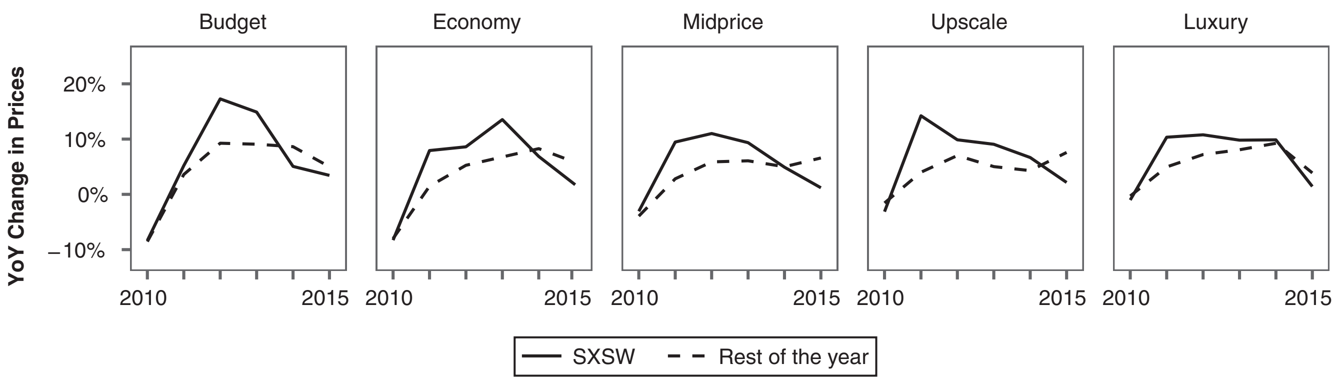
Next, we consider Austin. With the very rapid growth in SXSW, one could conjecture that the rate at which peak pricing power grows would outstrip that of nonpeak periods. Consider the data plotted in Figure 8, in which we depict the year-over-year percentage changes in SXSW prices for March (solid line) compared with changes in prices during the remaining months of the year (dashed line). During the initial period (roughly 2010–2012), visual evidence suggests the hotel pricing power for SXSW increased faster than during the rest of the year, consistent with rapid growth in SXSW. In the second half of the period, 2012–2014, a new phenomenon is at work. The gap between high- and low-season price changes starts to narrow as hotels lose the ability to exert the same pricing power, despite the continued growth of SXSW. This effect is especially pronounced for lower-end hotels, as our previous results would predict. Overall, these visualizations are consistent with an explanation of flexible Airbnb supply coming online during SXSW to accommodate peak demand, thereby crimping the peak pricing power of lower-end hotels specifically.

As a final step in understanding the statistical significance of the effect we visualized, we estimate a descriptive model of changes in peak pricing power. The dependent variable we analyze is the seasonal price difference for each hotel i and year-month t, which is defined as follows:

$$\nabla_{12} \log p_{i,t} = \log p_{i,t} - \log p_{i,t-12}, \tag{7}$$

where ∇_D is the seasonal difference operator of order D. As before, the interpretation of this quantity is the percentage change in prices for hotel i compared with prices during the same month the previous year. Unlike our visualization, in which we lumped all low-season months together, here we separately difference each month in our data. The model we estimate takes the following triple-differences form:

Figure 8
YEAR-OVER-YEAR CHANGES IN AUSTIN HOTEL PRICES BROKEN DOWN BY HOTEL PRICE LEVEL



Notes: The solid line displays changes during SXSW (March) while the dashed line displays changes for the rest of the year.

$$
\begin{aligned}
(8) \quad \nabla_{12} \log p_{i,t} = {} & \beta_1 \text{Austin}_i + \beta_2 \text{March}_t + \text{Year}_t \\
& + \beta_3 \text{Austin}_i \times \text{March}_t + \beta_4 \text{Austin}_i \times \text{Year}_t \\
& + \beta_5 \text{March}_t \times \text{Year}_t \\
& + \beta_6 \text{Austin}_i \times \text{March}_t \times \text{Year}_t \\
& + \epsilon_{i,t},
\end{aligned}
$$

where March_t is a dummy for March hotel-months, Year_t are year fixed effects, and Austin_i is an indicator for hotels in Austin. In addition to these explicit controls, seasonal differencing eliminates both hotel fixed effects, as well as hotel-month-specific linear trends in year-over-year price changes (such as a specific hotel increasing March prices by 5% every year, April prices by 2% every year, etc.). The coefficients of interest are contained in the vector β_6, and they can be interpreted as changes in SXSW pricing power. Intuitively, the model estimates March-specific changes in pricing power in Austin and then adjusts these estimates for (1) March-specific changes in pricing power outside Austin and (2) non-March-specific changes in Austin. Figure 9 displays the coefficients β_6 and their associated 95% confidence intervals. Our conclusions here mirror our previous observations: SXSW pricing power has significantly declined as Airbnb popularity has increased, despite the fact the SXSW attendance has continued to grow steadily over time.

Unlike our previous analyses, the results in this section are descriptive. When jointly interpreted with our causal estimates of Airbnb on hotel revenue, they paint a picture of Airbnb reducing hotel pricing power during periods of peak demand, consistent with our hypothesis that the flexible provisioning of inventory to accommodate peak demand is a distinguishing feature of the sharing economy. Further research should create a better understanding of this phenomenon with both more sophisticated modeling and data that span other large events.

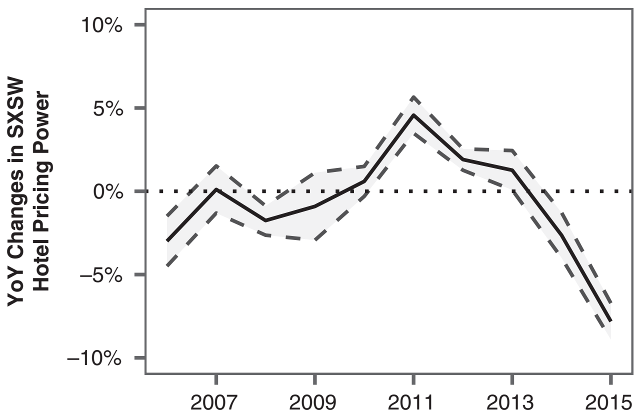
In closing, we compare our observations with another sharing economy study that observes flexible supply entering the Uber market during peak periods. Hall, Kendrick, and Nosko (2015) study the effectiveness of surge pricing on Uber, whereby drivers are incentivized to drive at peak times through higher payment multipliers. The study reports that the surge pricing mechanism is effective and leads to reduced wait times during periods of peak demand, comparable to levels observed in low-demand periods. In comparison, our work shows that a similar incentive drives Airbnb suppliers to scale room supply during periods of peak demand, when they can command higher rents. We witness this effect indirectly, through decreased peak pricing power of hotels in the high season. Whereas Uber directly incentivizes increased supply through central setting of price multipliers, a similar effect arises in Airbnb without direct control but instead through the collective, decentralized decision making of its suppliers. Notably, Airbnb is moving toward a variable pricing model, in which it dynamically adjusts listing prices in response to demand.[5]

ROBUSTNESS CHECKS

We perform three checks to reinforce the causal interpretation of our DD estimate: (1) a distance-sensitive definition of the Airbnb supply variable, (2) a specification test using an alternative functional form of Airbnb supply, and (3) a matching method, which we use as a more stringent alternative in defining (otherwise similar) treated and untreated properties.

Figure 9

PERCENTAGE CHANGES IN YEAR-OVER-YEAR AUSTIN HOTEL PEAK PRICING POWER (SXSW VS. REST OF THE YEAR)



Distance-Based Market Definition

In our analyses so far, we have assumed that travelers may substitute between hotels and Airbnb properties within the same city irrespective of the distance between properties (Hollenbeck [2014], who analyzes the same Texas data, also uses city-level markets to model competition between hotels). Although this seems reasonable for smaller cities in our data set, it is likely a less reliable approximation of how travelers form consideration sets when visiting sprawling cities like Houston. To test the sensitivity our results to narrower market definitions, we next analyze a proximity-based Airbnb supply measure. Specifically, for each hotel in our data, we measure the cumulative number of Airbnb rooms within a fixed radius at any given point in time. This measure allows different hotels in the same city to face different levels of competition by Airbnb. To be consistent with this market definition, we also define hotel competition in the same way: for each hotel, we measure the number of hotel rooms within the same fixed radial distance.

Using a hotel-specific market definition introduces a new type of endogeneity concern that we need to address. For any given hotel, increased competition by nearby Airbnb properties or hotels is likely correlated with increased demand for that hotel. In other words, even within the same city, new hotel rooms and Airbnb properties are more likely to be located near hotels that are facing growing demand by travelers. The city-specific trends we previously included do not allow for correlation between local measures of Airbnb and within-city hotel revenue variation. Therefore, we estimate a model that includes hotel-specific quadratic trends. This model, known as the correlated random trends, or random growths, model (Murtazashvili and Wooldridge 2008; Wooldridge 2005, 2009), allows for correlation between the hotel-specific trend

[5]See *Financial Times* (2015). We thank Avi Goldfarb for pointing out this connection.

component and time-varying observables. The specific model we use takes the following form:

$$\text{(9)}\quad \log \text{Hotel Revenue}_{ikt} = \beta \log \text{Local Airbnb Supply}_{ikt} + X'_{ikt}\gamma + h_i + a_{i1}t + a_{i2}t^2 + \tau_t + \text{City}_k \times \text{Month}_t + \epsilon_{ikt}.$$

Following standard practice (see, e.g., Wooldridge 2005), we eliminate the hotel-specific quadratic trends by second-differencing our data.[6] Second-differencing requires the sacrifice of the first two monthly observations of each hotel. Our decade-long panel is sufficient to comfortably accommodate this transformation. The final model we estimate is

$$\text{(10)}\quad \Delta^2(\log \text{Hotel Revenue}_{ikt}) = \beta\Delta^2(\log \text{Local Airbnb Supply}_{ikt}) + \Delta^2(X'_{ikt})\gamma + a_{i2} + \tau_t + \text{City}_k \times \text{Month}_t + \epsilon_{ikt},$$

where Δ^2 is the second-difference operator. Note that differencing also eliminates the hotel fixed effect h_i. The model can be estimated using the within transformation to eliminate the hotel-specific intercepts a_{i2}. We continue to cluster errors at the city level.

Table 6 displays our results. In the first column, we display our results using a radius of one mile around each hotel. We estimate a significant Airbnb effect with magnitude −.032 ($p < .05$), similar to our prior estimates. In the second column, we experiment with a larger radius of five miles. Our estimate is again significant; however, it is now smaller in magnitude (−.025, $p < .05$). One natural interpretation for the difference between these two estimates is that the greater the distance between Airbnb listings and hotels within a city, the less likely travelers are to substitute between the two. Overall, our results support our prior hypothesis that Airbnb directly affects hotel revenue, while producing the additional insight that this impact is sensitive to the distance between hotels and Airbnb listings within a city.

Alternative Functional Form for Airbnb Supply

The second robustness check we perform guards against a functional specification concern in Equation 1: regressing the log of Airbnb supply on the log of hotel room revenue implicitly assumes a constant elasticity relationship between the two quantities. While this might be a reasonable assumption in data with limited variation in Airbnb supply, the constant elasticity assumption is likely violated in our setting, as it is implausible that doubling Airbnb supply from 1 to 2 units will have the same effect on hotel room revenue as doubling Airbnb supply from 100 to 200 units. To ensure that our results are not driven by this modeling choice, we model Airbnb supply nonparametrically using a categorical variable, which takes on one of the following (roughly log-binned) values: 0 Airbnb units, 1–99 Airbnb units, 100–999 Airbnb units, and 1,000+ Airbnb units. Specifically, we estimate:

[6]To understand this, note that first-differencing transforms the linear trend to a constant, and the quadratic trend to a linear trend: $a_{i1}t + a_{i2}t^2 - a_{i1}(t-1) - a_{i2}(t-1)^2 = a_{i1} + a_{i2}(2t-1)$ Taking a second difference, we arrive at $a_{i1} + a_{i2}(2t-1) - a_{i1} - a_{i2}[2(t-1)-1] = 2a_{i2}$, which has a_{i2} as a hotel-specific intercept.

Table 6

LOCAL MEASURES OF AIRBNB AND HOTEL ROOM SUPPLY

	(1) *Within 1 Mile*	*(2)* *Within 5 Miles*
Δ^2(log Local Cum. Airbnb Supply)	−.032**	−.025**
	(−1.97)	(−2.15)
Δ^2(log Local Hotel Room Supply)	.006	.016
	(.95)	(.69)
Δ^2(log Hotel Room Supply)	−.005	−.004
	(−.19)	(−.18)
Δ^2(log Capacity)	−.026	−.026
	(−1.18)	(−1.18)
Δ^2(log Median Annual Wage)	−.131	−.129
	(−.38)	(−.37)
Δ^2(Unemployment Rate)	−.017*	−.017*
	(−1.88)	(−1.86)
Δ^2(log Population)	−.156	−.155
	(−.89)	(−.89)
Δ^2(log Airline Passengers)	.174***	.176***
	(5.49)	(5.57)
Δ^2(Is Reviewed)	.043***	.043***
	(2.65)	(2.67)
Δ^2(TripAdvisor Star-Rating)	−.007*	−.007*
	(−1.66)	(−1.67)
N	285187	285187
Within R^2	.0011	.0011

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: The dependent variable is Δ^2(log Hotel Revenue_{ikt}), where Δ^2 is the second difference operator. Cluster-robust t-statistics (at the city level) are shown in parentheses. All specifications include hotel fixed effects, year-month fixed effects, city-month fixed effects, and a city-specific quadratic time trend.

$$\text{(11)}\quad \log \text{Hotel Revenue}_{ikt} = \beta_1 I(\text{Airbnb Supply } 1-99)_{kt} + \beta_2 I(\text{Airbnb Supply } 100-999)_{kt} + \beta_3 I(\text{Airbnb Supply } 1{,}000+)_{kt} + h_i + \tau_t + X'_{ikt}\gamma + \epsilon_{ikt},$$

where the I(.) are dummy indicators for the corresponding ranges of Airbnb supply.

This model allows the effect of Airbnb to vary depending on the number of Airbnb listings present in each city during a given period. In this model, each of three estimated coefficients associated with the three levels of the categorical Airbnb supply variable we use represents a percentage change in hotel revenue. We estimate this model by replacing Airbnb supply with this new categorical variable in Equation 1 using zero Airbnb units as the reference level. We present our results for cumulative Airbnb supply in the first column of Table 7. These estimates provide directly interpretable estimates of Airbnb's economic impact. We find that increasing levels of Airbnb penetration have proportionally larger impacts on hotel room revenue, as we would expect. For example, at Airbnb adoption rates exceeding 1,000 rooms, the estimate (−.085, $p < .05$), indicates (because we are now working with a log-level specification) an average impact of 8.5% on hotel room revenue. These results are in line with our main specification estimates.

Table 7
ROBUSTNESS CHECKS

	(1) *Revenue*	*(2)* *Revenue*
log Cum. Airbnb Supply		−.043*** (−4.25)
Cum. Airbnb Supply (ref. 0)		
1–99 listings	−.020 (−1.40)	
100–999 listings	−.063** (−2.05)	
1,000+ listings	−.085** (−2.16)	
log Hotel Room Supply	−.152*** (−6.06)	−.151*** (−5.70)
log Capacity	.034 (1.50)	.075** (2.40)
log Median Annual Wage	−.432 (−1.15)	−.246 (−.66)
Unemployment Rate	−.059*** (−3.87)	−.055*** (−3.55)
log Population	.128 (.78)	.152 (1.12)
log Airline Passengers	.127*** (2.65)	.165*** (3.52)
Is Reviewed	−.056*** (−2.93)	−.050*** (−2.72)
TripAdvisor Star Rating	.031*** (6.79)	.034*** (6.40)
N	294,383	188,818
Within R^2	.011	.015
CEM sample	No	Yes

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: The first column tests an alternative functional form for cumulative Airbnb supply; the second column estimates Airbnb's impact using a CEM-matched subset of hotels. The dependent variable is log Hotel Revenue_{ikt}. All specifications include hotel fixed effects, year-month fixed effects, city-month fixed effects, and a city-specific quadratic time trend.

It is reassuring that we find no statistically significant effect at low levels of Airbnb supply. In fact, this model clarifies that Austin is the primary driver of the Airbnb effect we estimate—no other city in our data had more than 1,000 (cumulative) Airbnb listings during our observation period, and therefore the 8.5% decrease in revenue we estimate for Airbnb penetration at this level is driven by Austin. Indeed, we also find that our estimate between 100–999 listings is also primarily driven by Austin—deleting Austin from the data and reestimating the model gives a negative but statistically insignificant impact. Even though a few cities in our data experienced Airbnb adoption at this level, those cities were larger, had more hotel rooms, and did so late in our observation period (see Table 1). Therefore, it would be rather surprising had Airbnb exerted a statistically significant impact on hotel revenues outside of Austin. Does this result suggest that the Airbnb effect is specific to Austin? We think this is unlikely for two reasons. First, Airbnb is popular in many other destinations outside of Texas both nationally and globally, and we do not have reason to believe the incumbent hotel industry is better protected from Airbnb in those other cities. Second, our findings indicate that hotels facing seasonal tourism demand have a structural susceptibility to Airbnb that is universal, not local to Austin.[7]

A Matching Estimate Using CEM

Because Airbnb adoption is clearly not random by design, to provide evidence in support of the DD identification assumptions, we showed that observed pretreatment demographic and market characteristics do not correlate with the patterns of Airbnb adoption we observe in our data, which is what we would expect with exogenous Airbnb entry. Here, we combine DD with matching to further limit the potential for unobserved confounders biasing our estimates. To explain the matching approach, first recall our source of identification: roughly speaking, for each "treated" hotel (i.e., a hotel affected by Airbnb competition), our DD analysis constructs a counterfactual outcome using a set of "untreated" hotels (i.e., hotels unaffected by Airbnb). The intuition behind matching is that the more similar treated and untreated hotels are in their observed characteristics, the less likely they are to differ in unobserved ways, including bias-inducing factors. Matching methods aim to reduce endogeneity concerns by ensuring comparability between treated and untreated units (Heckman and Navarro-Lozano 2004). Although various matching methods exist, here we use the CEM procedure (Iacus, King, and Porro 2012) because it is intuitive and works well with categorical data (like most hotel characteristics).

Coarsened exact matching occurs in two steps. First, hotels are stratified based on observed characteristics; we use price segment (budget to luxury), operation (independent or chain), and hotel chain affiliation (e.g., Hilton, Marriott), if any. After this first step, each stratum contains hotels that are identical on the basis of these characteristics. For instance, a single stratum contains all upscale Marriott hotels, some of which are eventually treated and some of which are not. In a setting with a binary treatment indicator, it is clear which units are eventually treated. In our case, in which treatment intensity varies, we make this distinction by defining hotels in cities that have no Airbnb penetration by the end of our observation period as untreated, and the remaining hotels as treated. One could argue that this definition of treatment is too permissive; although we do not present these results for brevity, we found our CEM analysis to be robust to alternative definitions of treated units, such as hotels in cities that eventually have at least 100 Airbnb listings. In the second step of CEM, we discard strata containing only treated or untreated hotels and renormalize weights of observations in the remaining strata to place equal weight on treated and untreated units in each stratum. Applying CEM to our data leaves us with 1, 946 hotels.[8] Finally, we reestimate the DD specification in Equation 1 on the subset of matched hotels using the CEM weights. Conceptually, DD

[7]A recent report by CBRE Hotels' Americas Research ranks Austin as the 13th most vulnerable city to Airbnb in the United States (New York City ranks first), taking into account both the ratio of Airbnb units to hotel rooms in each market, as well as hotel room and Airbnb prices (see http://rss.hsyndicate.com/file/152006083.pdf).

[8]Coarsened exact matching entails a trade-off between matching granularity and the number of discarded observations. We chose our matching criteria to strike a reasonable balance between ensuring that units within each stratum are similar and discarding too many observations. Our results our robust to alternate matching criteria.

on the CEM sample estimates a treatment effect within each stratum of comparable treated and untreated hotels, then averages these treatment effects to arrive at a final estimate. We report this estimate in the second column of Table 7. We find that the effect of Airbnb on hotel room revenue is robust to CEM, attaining a magnitude ($\beta = -.043$, $p < .01$) that is highly comparable to our original estimate ($\beta = -.039$) reported in column 1 of Table 2.

DISCUSSION AND CONCLUSIONS

The sharing economy has recently emerged as a viable alternative to fulfilling a variety of consumer needs, ranging from prepared meals to cars to overnight accommodations, that were previously provided primarily by firms rather than entrepreneurial individuals. As the size of the sharing economy has grown, so has the magnitude of its economic impacts. Our work is among the first to provide empirical evidence that the sharing economy is significantly changing consumption patterns, as opposed to generating purely incremental economic activity. Focusing on the case of Airbnb, a pioneer in shared accommodations, we estimate that its entry into the Texas market has had a quantifiable negative impact on local hotel room revenue. The substitution patterns we observe strongly suggest that Airbnb provides a viable, but imperfect, alternative for certain traditional types of overnight accommodation. Our analyses pinpoint lower-end hotels, and hotels not catering to business travelers, as those that are most vulnerable to increased competition from rentals enabled by firms like Airbnb. Moreover, our work provides evidence that Airbnb supply is differentiated from hotel supply, as shown by Airbnb supply-side flexibility and carrying through to the impact on hotel peak pricing power.

Our work has some limitations that could be addressed in further research. First, one must recognize that our findings are representative of the state of Texas; directly generalizing them to other markets may not be appropriate given the varying of dynamics of supply and demand for accommodation across different regional markets. Additional studies that model the impact of Airbnb across these markets could be a useful contribution. A second limitation of work is that we analyze properties listed only on Airbnb, but not properties available through related vacation rental platforms such as HomeAway and VRBO. We do not believe that our results are significantly affected by these competitors, because these firms primarily serve the smaller vacation rental market; moreover, they have not experienced the extremely rapid growth of Airbnb. Nevertheless, researchers could investigate the impact of all these firms in aggregate, or individually. A final limitation of our study pertains to the precise characterization of hotels' response: in this article, we have analyzed two metrics, price and occupancy rate, that managers can invoke as a response in the short run. On longer time scales, hotels have other ways of responding to Airbnb, including alterations to their investment schedules, to their entry and exit decisions, and to their marketing campaigns. New promotions, advertising campaigns, and even repositioning to provide more personalized Airbnb-like services are all options. Work that either informs or interprets the shape of the response by hotels in the longer run will address interesting open questions.

Our results have direct implications for hotels, travelers, and policy makers. For hotel managers, the competition their firms face from peer-to-peer platforms has several unique features that differentiate it from competition with other firms. First, the Airbnb platform has near-zero marginal cost, in that a new room can be incrementally added to (or removed from) the platform with negligible overhead. Because of this, Airbnb can scale supply in an almost frictionless manner to meet demand, even on short timescales. By contrast, increasing hotel room supply involves buildout, causing significant marginal costs for hotel chains. As we have shown, this unique feature of Airbnb has already significantly affected hotel's pricing power during periods of peak demand. Second, Airbnb offers a much wider range of products and services than hotels: Airbnb users can rent anything from an apartment to a yurt. More importantly, because Airbnb leverages existing housing inventory, it can potentially expand supply wherever houses and apartment buildings already exist. This is in contrast to hotels, which must be built at locations in accordance with local zoning requirements. Therefore, competition by Airbnb is potentially harder for incumbents to adapt to, compared with competition by other hotel firms.

Turning to consumers, we show that hotels in areas where Airbnb has an established presence have responded to increased competition by lowering their prices, which harms their revenues but benefits travelers, even those who do not use Airbnb. In addition to reduced prices, consumers also benefit from increased variety provided through peer-to-peer platforms. Furthermore, consumers on the supply side benefit through additional income generated by providing goods and services through peer-to-peer platforms.

Finally, our results have implications for policy makers. Municipal revenues rely in part on tax receipts from well-regulated industries such as hotels and taxicabs. With demand shifting away from these incumbent firms, and to the extent that regulation and taxation of peer-to-peer platforms proves to be more challenging, the bottom line of cities with an established Airbnb presence could be hurt in the short run. Of course, peer-to-peer platforms can also bring about increased demand, which would provide direct benefit to cities, making the net impact on cities more difficult to measure. Quantifying the net impact of peer-to-peer platforms remains a fruitful direction for future research.

Returning to the thesis that the sharing economy has the potential to transformatively increase social welfare, as espoused by Botsman (2012) and others, we assert that a large population of people worldwide have indeed benefited from Airbnb—not only hosts who derive incremental income by renting properties through Airbnb and guests who select an Airbnb rental as an alternative to a hotel stay but also those consumers who benefit from lower prices and increased competition in the accommodation industry. More broadly, one can weigh the positive change the sharing economy can bring about not only by providing imperfect substitutes for existing products but also, through an application of Say's Law, by generating demand that did not previously exist, through the supply of new products and services. Harkening back to arguments Airbnb has made, the supply of inexpensive accommodations can increase travel and tourism spend overall, and thus, the sharing economy could be a net producer of new jobs. However, these positives must be evaluated against various costs, including those estimated in this article.

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