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

Georgios Zervas
Questrom School of Business
Boston University
zg@bu.edu

Davide Proserpio
Marshall School of Business
University of Southern California
proserpi@marshall.usc.edu

John W. Byers
Computer Science Department
Boston University
byers@cs.bu.edu

December 29, 2016

Abstract

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. The authors analyze Airbnb's entry into the state of Texas, and quantify its impact on the Texas hotel industry over the subsequent decade. The authors estimate that in Austin, where Airbnb supply is highest, the causal impact on hotel revenue is in the 8-10% range; moreover, the impact is non-uniform, with lower-priced hotels and those hotels not catering to business travelers being the most affected. The impact manifests itself primarily through less aggressive hotel room pricing, an impact that benefits all consumers, not just participants in the sharing economy. The price response is especially pronounced during periods of peak demand, such as SXSW, and is due to a differentiating feature of peer-to-peer platforms – enabling instantaneous supply to scale to meet demand.

The emergence of peer-to-peer platforms, collectively known as the “sharing economy”, has enabled individuals to collaboratively make use of under-utilized inventory via 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, have facilitated searchable listings for consumers, and have 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 de-list the selection of goods or services they have on offer.

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. With Airbnb having served over 50 million guests since it was founded in 2008, and a market capitalization eclipsing $30 billion, we hypothesize that Airbnb has a measurable and quantifiable impact on hotel revenue in affected areas.[1] Our hypothesis is that some stays with Airbnb serve as a substitute for certain hotel stays, thereby impacting hotel revenue, and that this impact is differentiated: by geographic region, by hotel market segment, and by season. Incumbent firms, despite both facing higher fixed costs and offering less personalized products than peer-to-peer platforms, have only recently started to take competition from platforms like Airbnb as a serious threat. For example, hotel executives have publicly issued largely dismissive statements regarding competitors like Airbnb, arguing that these peer-to-peer platforms are either a niche market or that they target complementary market segments from that targeted by hotel chains. Interestingly, Airbnb appears to also espouse this latter view: according to Airbnb, in many cities, over

[1] See: http://blog.airbnb.com/wp-content/uploads/2015/09/Airbnb-Summer-Travel-Report-1.pdf and http://www.wsj.com/articles/airbnb-raises-850-million-at-30-billion-valuation-1474569670.

70% of Airbnb properties are outside the main hotel districts,[2] suggesting complementarity of their offerings.

In this paper 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 datasets to compile controls, we quantify the extent to which Airbnb's entry to the accommodation market has negatively impacted hotel room revenue.

To identify the causal impact of Airbnb on hotel revenue, we employ a difference in differences (DD) empirical strategy. Specifically, due to 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 datasets, 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, against 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: a cumulative measure that defines supply as all listings appearing prior to a given date in a given city, and an instantaneous measure that defines supply as those Airbnb listings active within a short (*e.g.*, 3-month) time 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

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

variation in demand accross different market. 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-term and in the long-term. In the short-term, 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 multi-year 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 on Airbnb today, which typically provide fewer amenities and services than many hotels, we expect those hotels providing 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 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 those 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 larger marketing budgets and stronger brands, to providing 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 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 like hotels to directly counteract.

Finally, we mention several robustness checks that we conduct 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 Airbnb adoption is driven by hotel performance, which would be a case of our confusing 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 our results are robust to alternative measures of Airbnb supply. Finally, in a separate analysis, we combine DD with coarsened exact matching (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, discarding hotels that remain unmatched (*e.g.*, an upscale Hilton in Austin where Airbnb adoption is high, and an upscale Hilton in Dallas where Airbnb penetration is low.) 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 paper by discussing managerial and policy implications related to the rapid growth of Airbnb specifically and the sharing economy more broadly.

Related Work

Relatively few papers have yet studied competition betweeen peer-to-peer markets and incumbent firms offering similar goods or services. In one line of recent work, (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, that is borne out in our data, is that peer-to-peer markets can reduce price variability by flexibly scaling supply to accomodate 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 (Nee, 2007; Lidicker, 2010). In the domain of accommodation sharing, we find a large number of 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.[3]

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

While 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, January 2015 revenue per hotel room was impacted by 18.6%, year-over-year.[4].

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

It is in this latter context that our work contributes to the literature on substitution between peer-to-peer markets and incumbent firms, as markets like Airbnb can be viewed as providing enabling technology that facilitates suppliers of niche inventory to flexibly bring their products to market. In contrast to traditional markets, Airbnb provides sufficiently low cost of revenue for individuals 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, a number of recent studies have focused on the impact of Craigslist – a website featuring free online classified ads – on the newspaper industry (Pope, 2014; Zhu, 2013).

[4] See "New York City hotel rooms are getting cheaper thanks to Airbnb" at `http://qz.com/341292`

Finally, our work contributes to the literature studying the impact of external shocks on the tourism and the hospitality industry. Much of the prior work though, has centered on demand shocks. For example, Gallagher (2008) study the impact of terrorism on tourism in Ireland; Coulter (2007) estimate the impact of the 2002 and 2005 terrorist attacks in Bali on the islands' vendors. Similarly, Enz (2012) examine the adverse effects of the 9/11 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, Smith Travel Research (STR), county demographics from the U.S. Census Bureau, airport passenger counts from the U.S. Bureau of Transportation Statistics (BTS), the Current Population Survey (CPS) from the U.S. Bureau of Labor Statistics (BLS), 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 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 that used the service by September

2015.[5]

Airbnb's business model currently operates with minimal regulatory controls in most locations, and as a result, hosts and guests both have incentives to use signalling mechanisms to build trust and maximize the likelihood of a successful booking. To 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, and 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 textual description, photos, capacity, availability, check-in and check-out times, cleaning fees, and security deposits. Our collected dataset contains detailed information on 10,555 distinct hosts and 13,935 distinct listings spanning a period from 2008 to August 2014.

To conduct our analysis, we must choose an appropriate level of geographic aggregation. Here, our data is 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.

[5]See `http://blog.airbnb.com/wp-content/uploads/2015/09/Airbnb-Summer-Travel-Report-1.pdf`.

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 due to "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) find that between 21% and 32% of guest requests are rejected due to 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 dataset. 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 later dedicate a section of the paper to detail and justify our approach.

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 dataset includes basic information including hotel name, address, and capacity. The raw dataset spans the period between Jan. 2003 and Aug. 2014.

Interestingly, 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." 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 dataset. This is evident from Figure 2, 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 dataset 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 dataset contains information on $3,747$ hotels in Texas metropolitan areas. After linking the STR census dataset with the Texas tax dataset, 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 RevPAR (revenue per available room), which is the product of average room price and occupancy. Because the data we obtained from the 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, or ADR in the industry) 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 (Jan. 2003 to Aug. 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 BTS. 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 is 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 MSA level from the BLS at `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 information at the county level from the U.S. Census Bureau at `census.gov`.

Quantifying Airbnb's Impact in Texas

Empirical Strategy

Airbnb has seen 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 1, which depicts the current extent of market penetration both of Airbnb properties and hotels within the state of Texas (top panels), and within the county encompassing the state capital, Austin (bottom panels). 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 patterns of Airbnb adoption, over the past eight years in the ten most populous cities in Texas, are themselves diverse, with several cities experiencing early adoption and rapid growth, while others experienced minimal Airbnb adoption. Our empirical strategy exploits this variability to identify the impact of Airbnb's rise on hotel room revenue using a difference in differences (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, against 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 have to 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 pre-specified functional form (linear or quadratic).

Our DD specification takes the following form:

$$\begin{aligned}\log \text{Hotel Revenue}_{ikt} = &\beta \log \text{Airbnb Supply}_{kt} + X'_{ikt}\gamma \\ &+ h_i + \tau_t + \text{City}_k \times \text{Month}_t + \epsilon_{ikt}.\end{aligned} \tag{1}$$

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 non-treated 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 non-treated 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. 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 subsequent to Airbnb's entry compared against a baseline of changes in hotel room revenue over the same time 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 impact 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 check-out 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 dataset 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 dataset, 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 has both descriptive value and allows 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 low season. During low season Airbnb listings face lower demand, which in turn leads to fewer reviews. Therefore, during low season, some listings that are available may receive zero reviews and thus be misclassified as unavailable.

Figure 4 compares the cumulative and instantaneous Airbnb supply measures for the four biggest 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 have to deal with is that the unit of analysis is hotel monthly room revenue, but the treatment, Airbnb adoption, occurs at the city level. This mismatch in the level at which we measure our dependent variable compared to 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 in hotel room revenue. In doing so, we follow the standard practice in the literature for analyzing panel data in a DD setting (Mullainathan, 2004; Lang, 2007). We report standard errors clustered at the city level for all subsequent regressions.

Incorporating Controls: Hotel Supply & 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 drive per-hotel room revenue down, 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 Comptroller as, 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 for increases in the supply of hotel rooms provided by competitors to impact 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, for instance, following renovations. We derive hotel capacity from the tax data, and we use TripAdvisor ratings as a proxy for quality.

Second, as we explained earlier, our DD estimate will be biased if there exist unobserved factors that vary across cities and over time, and which 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 dataset covers a long pre-Airbnb period from 2003 to 2008, allowing us to estimate these trends on a large sample of pre-treatment 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 due to 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.

A further 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.[6]. Conceivably, an increase in the unemployment rate

[6] See "How Airbnb helps users save their homes", August 2012, `http://finance.fortune.cnn.com/2012/08/16/Airbnb-foreclosure/`

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 a controls in X_{itk}.

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 Mogstad (2013), 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 pre-treatment 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 pre-treatment factors. To perform this identification check, for each city, we compute its most recent pre-treatment (2007) population, unemployment rate, employment in the accommodation sector, hotel room supply, hotel room prices, and hotel occupancy rates. We then interact these pre-determined factors ($Z_{k,2007}$) with a vector of post-treatment 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:

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

Each coefficient in the vector of coefficients θ is interpreted as a correlation between a specific pre-treatment characteristic and Airbnb adoption in each post-treatment period (from January 2008 onwards). Figure 3 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. Visually, 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 pre-treatment 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 onwards.

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 discussed above in the first column of Table 3. 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), although we note that our estimate for β without any controls (not presented) is comparable ($\beta = -.035$, $p < .01$). As stated earlier, 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 4 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 once trends are included, our estimate becomes negative and significant. To explain this observation, we hypothesize that city-specific demand trends drive both 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 upwards. 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: since moving from quadratic to cubic trends our estimates remain unchanged, 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 city in Texas with the highest Airbnb penetration, we estimate that the impact of Airbnb over the past 5 years is roughly 10% of hotel room revenue (the 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 3, we find that a 10% increase in the supply of hotel rooms in Texas is associated with a roughly 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, as 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. And, 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 shortly.

Next, we estimate Equation 1 using our instantaneous Airbnb supply measure. We present these results in the second and third columns of Table 3. In the second column we use a TTL of 3 months, while in the third, we use a TTL of 6 months. In both cases, we obtain negative and significant estimates, though the 3-month TTL estimate for β is smaller than the 6-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 too. For instance, in our data, we estimate that instantaneous Airbnb supply during SXSW has historically been approximately 60% higher than 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 $\log(1.6) * .035$).

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 might be 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 see evidence that Airbnb's impact in Texas is observable through the lens of both cumulative and instantaneous supply measures. We further see that while 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 later refine this top-level analysis to study how the economic impacts are differentiated across types of hotels in, and further unpack the effects of supply flexibility on the peak pricing power of hotels.

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

So far, we have measured Airbnb's impact in terms of hotel revenue. Now we turn to the nature of responses by incumbent hotels to Airbnb market entry. In the short term, 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 impacting market entry and exit. All of 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 average daily room price (ADR) during that same period of time. 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 via 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 re-estimate 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 dataset 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 5. 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, where hotels set prices accordingly 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's 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 impacted hotel prices by roughly 6%.

Both the price and occupancy effects we investigated above constitute immediate responses to Airbnb. In the longer 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 dataset (from STR) that records all currently 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 dataset, we computed the average time it takes to transition from one phase to the next, which we diagram in Figure 5. The average estimated time between pre-planning and projected opening is approximately 4 years, although 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, which we do not report, 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' longer-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 while Airbnb can surely sometimes provide a suitable alternative to hotels, one can hardly expect it to be a perfect substitute for all travel needs. As 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, as 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 different 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, 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:

$$\begin{aligned}\log \text{Hotel Revenue}_{ikt} = & \beta_1 \text{log Airbnb Supply}_{kt} \\ & + \beta_2 \text{log Airbnb Supply}_{kt} \times \text{Hotel Type}_i \\ & + X'_{ikt}\gamma + \alpha_i + \tau_t + \epsilon_{ikt}.\end{aligned} \tag{3}$$

The coefficients of interest are β_2, which capture 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 one 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 7. 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 unaffected by Airbnb, motivated by the observation that these hotels are least comparable to Airbnb based on average room price and also by their amenities (*e.g.,* pools, conference rooms, concierge). We find the negative impact of Airbnb increasing as we step down 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 segment (the latter being the reference level, and hence 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. While 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 7, 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. We do note though that, seeing a growth opportunity in the business travel segment, Airbnb recently launched an initiative to attract more business travelers.[7] An interesting open question going forward is the extent to which 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 which should make them less vulnerable to competition. In addition, many chains provide a more predictable standard of service, which further differentiate them from both Airbnb and independent hotels. We present this analysis in the third column of Table 7, using chain hotels as a reference level. The overall effect due to 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 based on 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. But we now show that Airbnb supply is more than just a partial substitute for low-end hotel

[7] See `http://bits.blogs.nytimes.com/2014/07/28/airbnb-expands-into-business-travel/`.

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[8], but they cannot materially increase supply, due to high fixed costs of new inventory. In contrast, many of the micro-entrepreneurs 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 serve to 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 will 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 will refer to peak demand months as the *high season*, and the remaining months as the *low season*. For each hotel i, we will denote high season prices during year y by $p_{i,y}^H$ and low season prices by $p_{i,y}^L$. Given these two quantities, we will define hotel i's peak pricing power as:

$$P_{i,y} = \log p_{i,y}^H - \log p_{i,y}^L, \tag{4}$$

[8] For example, see evidence of surge pricing coinciding with the annual shareholders' meeting of Berkshire Hathaway in Omaha, "Buffett's revenge," The Economist, 1/9/16.

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

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

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

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

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

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 season, it is unlikely that they are jointly driven by Airbnb hosts flexibly scaling supply to accommodate peak demand during specific months of the year; hence, in this case, $\Delta P_{i,y}$ will be estimated to be zero.

To study changes in peak pricing power of hotels in our dataset, we considered the impact of two large events that take place annually in Texas: the South by Southwest (SXSW) festival in Austin in March, and the Texas State Fair (TSF) in Dallas in October. Both events draw a very large number of out-of-town visitors, and have a substantial impact on the bottom line of area hotels as a result. Both events have also grown in popularity in the past decade, but with the much smaller SXSW festival growing more rapidly in percentage terms. Figure 8 displays attendance for SXSW Interactive, which together with SXSW Film and SXSW Music, are the major components of SXSW. March and October represent the peak months for demand of hotels in Austin and Dallas respectively, measured both in terms of occupancy and ADR (average daily room rate). 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, where 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 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 6 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 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 has not changed significantly compared to the remainder of the year.

Next, we consider Austin. With the very rapid growth in SXSW, one could naturally conjecture that the rate at which peak pricing power grows would outstrip that of non-peak periods. Consider the data plotted in Figure 7, where we depict the year-over-year percentage changes in SXSW prices for March (solid line), in comparison to 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 to prices during the same month one year ago. Unlike our visualization, where 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:

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

where March_t is a dummy for March hotel-months, the Year_t are year fixed effects, and Austin_i is an indicator for hotels in Austin. In addition to these explicit controls, seasonal differencing wipes out both hotel fixed effects, as well as hotel-month-specific linear trends in year-over-year prices changes (*e.g.*, a specific hotel increasing March prices by 5% every year, April prices by 2% every year and so on.) 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 a) March-specific changes in pricing power outside Austin and b) non-March-specific changes in Austin. Figure 9 displays the coefficients β_6 and their associated 95% confidence intervals. Our conclusions here mirror our earlier observations: SXSW pricing power has significantly declined as Airbnb popularity grew, despite the fact the SXSW attendance has continued to steadily grow over time.

Unlike our earlier 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. Better understanding this phenomenon with both more sophisticated modeling and data spanning more large events is future work.

In closing, we compare and contrast our observations with another sharing economy study that observes flexible supply entering the Uber market during peak periods (Nosko, 2015). In this work, researchers study the effectiveness of surge pricing on Uber, whereby drivers are incented 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 seen 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 high season. While 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. Interestingly, Airbnb is moving towards a variable pricing model, where it dynamically adjusts listing prices in response to demand.[9]

[9] See "Airbnb shakes up pricing model to meet surging demand" at `http://www.ft.com/cms/s/0/bc875c4c-88ee-11e5-9f8c-a8d619fa707c.html`. We thank Avi Goldfarb for pointing out this connection.

Robustness checks

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

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.) While this seems reasonable for smaller cities in our dataset, 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 (Wooldridge, 2005, 2008, 2009), allows for correlation between the hotel-specific trend component and time-varying observables. The specific model we use takes the following form:

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

Following standard practice (see, *e.g.*, Wooldridge (2005)), we eliminate the hotel-specific quadratic trends by second-differencing our data (to see this, note that first-differencing transforms the linear trend to a constant, and the quadratic trend to a linear one: $a_{i1}t + a_{i2}t^2 - a_{i1}(t-1) - a_{i2}(t-1)^2 = a_{i1} + a_{i2}(2t-1)$. 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.) 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:

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

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 1 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 5 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 is directly affecting 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 non-parametrically 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, 1000+ Airbnb units. Specifically, we estimate:

$$\begin{aligned}\log \text{Hotel Revenue}_{ikt} =& \beta_1 \text{I(Airbnb Supply 1-99)}_{kt} + \beta_2 \text{I(Airbnb Supply 100-999)}_{kt} \\ &+ \beta_3 \text{I(Airbnb Supply 1000+)}_{kt} + h_i + \tau_t + X'_{ikt}\gamma + \epsilon_{ikt},\end{aligned} \tag{11}$$

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

This model allows for the effect of Airbnb to vary depending on the level of Airbnb listings present in each city during a given period. In addition, it provides easier to interpret estimates compared to the log-log estimates of Equation 1. 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 8. 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 1000 rooms, the estimate $(-.085, p < .05)$, indicates (since 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.

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 1000 (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 re-estimating the model gives a negative but statistically insignificant impact. Even though a few cities in our data saw 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 defended from Airbnb in those other cities. Second, our earlier findings indicate that hotels facing seasonal tourism demand have a structural susceptibility to Airbnb that is universal, not local to Austin.[10]

[10] A recent report by CBRE Hotels' Americas Research ranks Austin as the 13th most vulnerable city to Airbnb in the US (NYC 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://cbrepkfcprod.blob.core.windows.net/downloads/store/12Samples/An_Analysis_of_Airbnb_in_the_United_States.pdf.

A matching estimate using CEM

Since Airbnb adoption is clearly not random by design, to provide evidence in support of the DD identification assumptions, we showed that observed pre-treatment 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 (Navarro-Lozano, 2004). While various matching methods exist, here we use the Coarsened Exact Matching (CEM) procedure (Porro, 2012), because it is intuitive and works well with categorical data (like most hotel characteristics).

CEM takes places 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, or 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, where treatment intensity varies, we make this distinction by defining hotels in cities which see 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; while 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 re-normalize 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 (CEM 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 units within each stratum are similar, and discarding too many observations. Our results our robust to alternate matching criteria.) Finally, we re-estimate the DD specification in Equation 1 on the subset of matched hotels using the CEM weights. Conceptually, DD 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 8. 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 3.

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 gives evidence that Airbnb supply is differentiated from hotel supply, as evidenced both by Airbnb supply-side flexibility and carrying through to the impact on hotel peak pricing power.

Our work has some limitations which could be addressed in future work. 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 which 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 like HomeAway and VRBO. We do not believe that our results are significantly affected by these competitors, since these firms primarily serve the smaller vacation rental market; moreover, they have not experienced the extremely rapid growth of Airbnb. Nevertheless, one could investigate the impact of all of these firms in aggregate, or individually. A final limitation of our study pertains to the precise characterization of hotels' response: here we have analyzed two metrics, price and occupancy rate, that managers can invoke as a response in the short-term. 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 re-positioning 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 a near 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 to 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 via 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 harder to measure. Quantifying the net impact of peer-to-peer platforms remains an interesting direction for future research.

Returning to the thesis that the sharing economy has the potential to transformatively increase social welfare, as evangelized by Botsman (2012) and others, we assert that a large population of individuals worldwide have indeed benefited from Airbnb: not only hosts that 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, 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 paper.

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Table 1: Airbnb's spatial and temporal penetration. Cumulative counts of Airbnb listings per year in the ten most populous Texas cities.

(Pop.)	Houston 2.16M	San Antonio 1.38M	Dallas 1.24M	Austin 0.84M	Fort Worth 0.78M	El Paso 0.67M	Arlington 0.38M	Corpus Christi 0.31M	Plano 0.27M	Laredo 0.24M
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	1862	34	3	19	7	5	1
2012	425	171	271	5158	68	8	27	24	20	1
2013	695	271	422	7489	93	23	36	49	33	1
2014	891	346	526	8575	114	31	52	60	44	2

Table 2: City-months that experience positive Airbnb entry

	2008	2009	2010	2011	2012	2013	2014
Jan.	0	3	11	39	70	100	121
Feb.	0	3	11	40	74	104	123
Mar.	1	4	12	46	77	107	126
Apr.	1	6	13	49	79	109	127
May	1	6	16	49	84	109	128
Jun.	1	6	17	49	86	111	128
Jul.	1	6	23	58	89	112	130
Aug.	2	6	26	60	90	114	–
Sep.	2	10	28	65	93	117	–
Oct.	2	10	29	65	96	118	–
Nov.	2	11	30	66	96	120	–
Dec.	3	11	34	67	98	120	–

Table 3: Difference-in-differences 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	0.034	0.034	0.034
	(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	0.049	0.061	0.030
	(.33)	(.42)	(.21)
log Airline Passengers	0.150***	0.138***	0.148***
	(3.24)	(2.94)	(3.23)
Is Reviewed	−.057***	−.056***	−.057***
	(−3.03)	(−2.94)	(−2.98)
TripAdvisor Star-Rating	0.031***	0.031***	0.031***
	(6.93)	(6.81)	(6.86)
N	294383	294383	294383
Within R^2	.013	.011	.012

Note: 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.

Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 4: Difference-in-differences 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	0.009	−.025**	−.039***	−.039***
	(1.26)	(−2.48)	(−4.40)	(−3.29)
N	294383	294383	294383	294383
Adj. Within R^2	.024	.012	.013	.011

Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 5: Difference-in-differences 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	0.075***	−.007
	(5.98)	(−.43)
log Median Annual Wage	−.263	−.050
	(−1.65)	(−.26)
Unemployment Rate	−.025***	−.016**
	(−4.50)	(−2.47)
log Population	−.004	0.140**
	(−.09)	(2.02)
log Airline Passengers	0.012	0.044**
	(.80)	(2.22)
Is Reviewed	−.060	−.129**
	(−1.34)	(−2.33)
TripAdvisor Star-Rating	0.002	0.008**
	(.86)	(2.59)
N	264172	264172
Within R^2	.018	.012

Note: 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 shown in parentheses. All specifications include hotel fixed effects, year-month fixed effects, city-month fixed effects, and a city-specific quadratic time trend.

Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

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)	0.006	0.016
	(.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)	0.174***	0.176***
	(5.49)	(5.57)
Δ^2(Is Reviewed)	0.043***	0.043***
	(2.65)	(2.67)
Δ^2(TripAdvisor Star-Rating)	−.007*	−.007*
	(−1.66)	(−1.67)
N	285187	285187
Within R^2	.0011	.0011

Note: 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.

Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 7: Difference-in-differences estimates of heterogeneity in Airbnb's impact on hotel room revenue.

	(1) Price segment	(2) Meeting space	(3) Operation
log Cum. Airbnb Supply	−.016	−.033***	−.038***
	(−1.61)	(−3.58)	(−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***	−.158***	−.158***
	(−6.26)	(−6.27)	(−6.26)
log Capacity	0.034	0.035	0.033
	(1.49)	(1.53)	(1.50)
log Median Annual Wage	−.225	−.219	−.215
	(−.64)	(−.62)	(−.61)
Unemployment Rate	−.060***	−.060***	−.060***
	(−4.46)	(−4.46)	(−4.47)
log Population	0.086	0.058	0.047
	(.63)	(.39)	(.31)
log Airline Passengers	0.151***	0.150***	0.150***
	(3.28)	(3.26)	(3.24)
Is Reviewed	−.032**	−.047***	−.056***
	(−2.12)	(−2.64)	(−2.97)
TripAdvisor Star-Rating	0.026***	0.029***	0.031***
	(7.15)	(6.94)	(7.00)
N	294383	294383	294383
Within R^2	.018	.014	.013

Note: 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.

Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 8: Robustness checks: 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.

	(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)	
1000+ listings	−.085**	
	(−2.16)	
log Hotel Room Supply	−.152***	−.151***
	(−6.06)	(−5.70)
log Capacity	0.034	0.075**
	(1.50)	(2.40)
log Median Annual Wage	−.432	−.246
	(−1.15)	(−.66)
Unemployment Rate	−.059***	−.055***
	(−3.87)	(−3.55)
log Population	0.128	0.152
	(.78)	(1.12)
log Airline Passengers	0.127***	0.165***
	(2.65)	(3.52)
Is Reviewed	−.056***	−.050***
	(−2.93)	(−2.72)
TripAdvisor Star-Rating	0.031***	0.034***
	(6.79)	(6.40)
N	294383	188818
Within R^2	.011	.015
CEM Sample	No	Yes

Note: 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.

Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

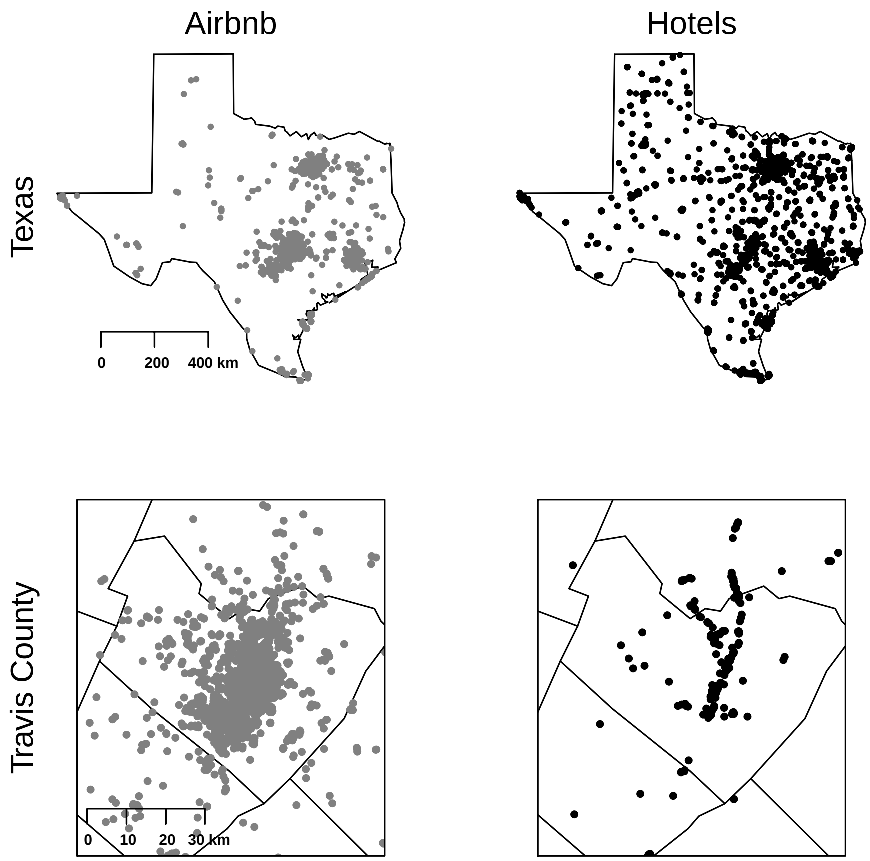

Figure 1: Geographical distribution of hotels and Airbnb listings in the state of Texas (top) and in Travis County, TX (bottom) in 2013.

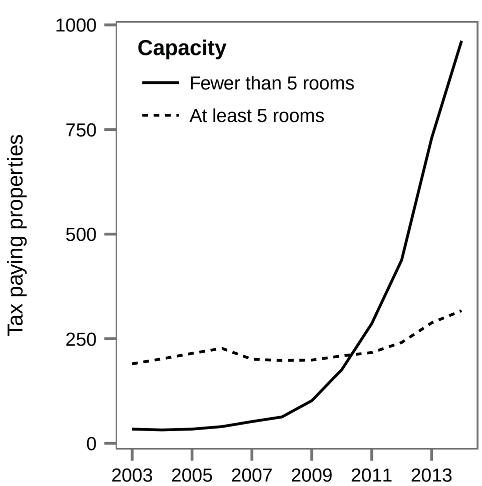

Figure 2: Annual counts of Austin properties that pay hotel occupancy tax, broken down by capacity.

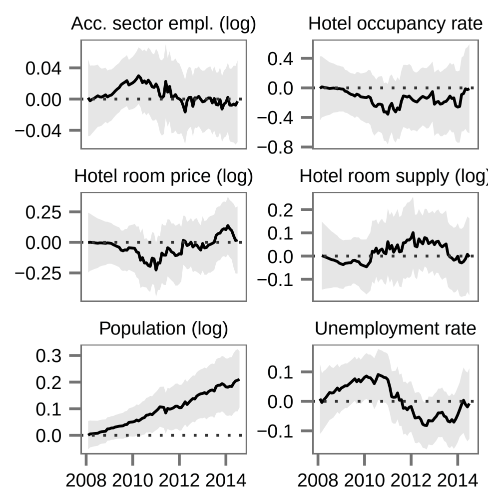

Figure 3: Correlation between Airbnb supply and pre-Airbnb (year 2007) city characteristics, with 95% confidence intervals.

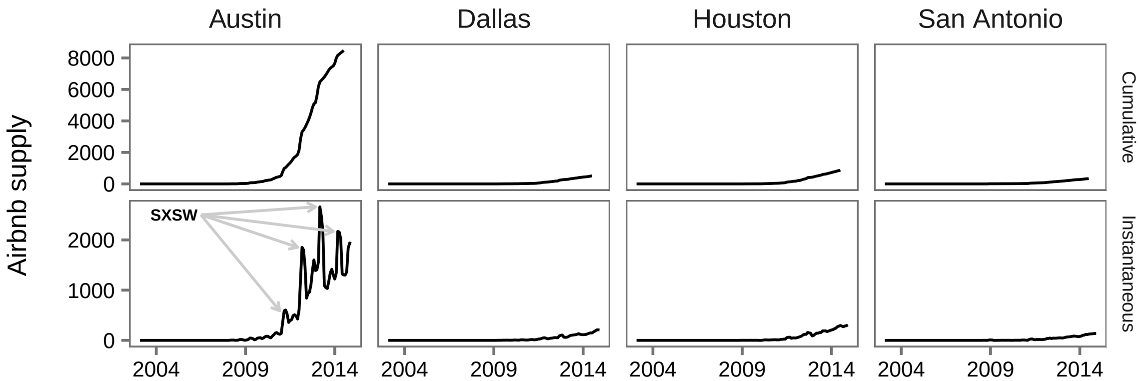

Figure 4: Cumulative vs. instantaneous Airbnb. The seasonal peaks of the Austin instantaneous supply curve correspond to SXSW.

Figure 5: Average time between various stages in the hotel pipeline construction.

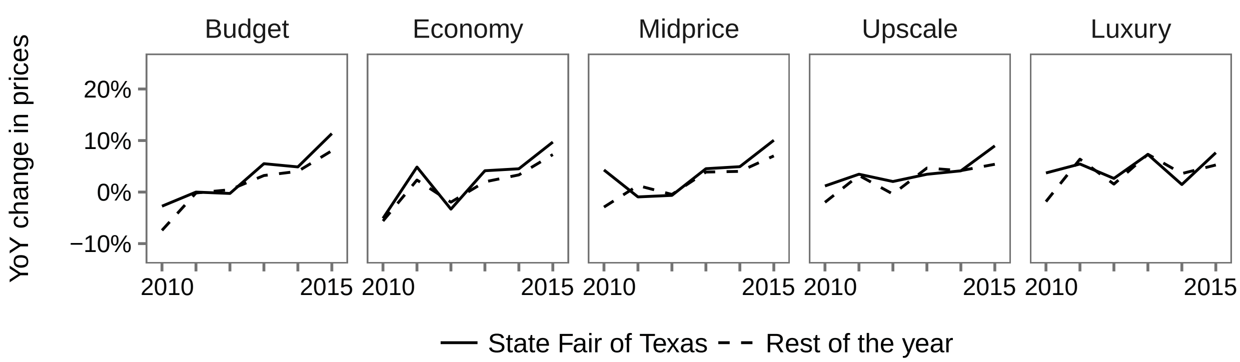

Figure 6: Year-over-year changes in Dallas hotel prices broken down by hotel price level. The solid line displays changes during the State Fair of Texas (October) while the dashed line displays changes for the rest of the year.

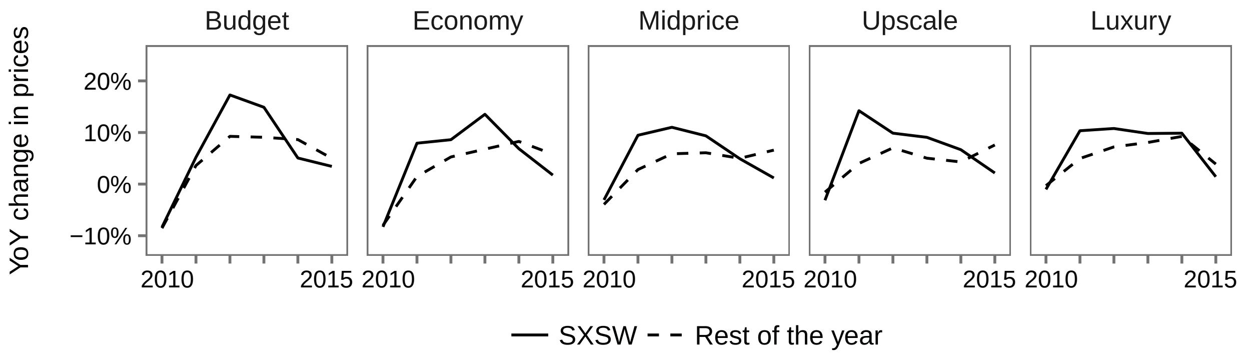

Figure 7: Year-over-year changes in Austin hotel prices broken down by hotel price level. The solid line displays changes during SXSW (March) while the dashed line displays changes for the rest of the year.

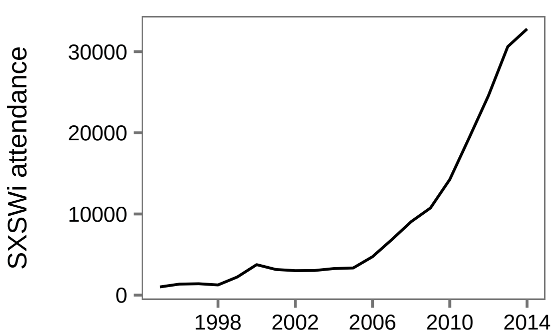

Figure 8: SXSWi attendance

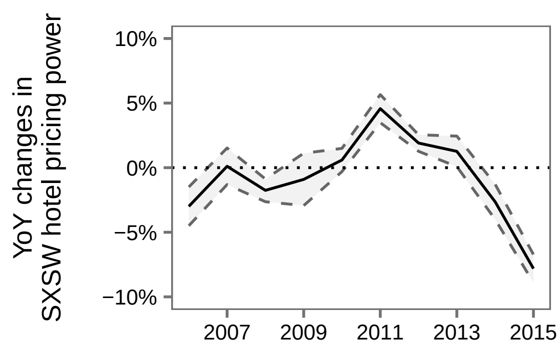

Figure 9: Percentage changes in year-over-year Austin hotel peak pricing power (SXSW vs. rest of the year).