IF YOU CAN'T BEAT THEM, BAN THEM: HOTEL MARKET POWER AND SHORT-TERM RENTAL REGULATIONS

SOPHIE CALDER-WANG, CHIARA FARRONATO, KARAM KANG AND QUAN LE

Abstract. Incumbents' consolidation may reshape competition by influencing the regulation of new entrants. We study this possibility in the accommodations market using a new panel of short-term rental (STR) regulations across about 1,000 U.S. cities and variation in local exposure to the 2016 Marriott–Starwood merger. Among cities not yet regulated, merger exposure raises the six-month probability of adopting STR regulation by roughly 70 percent, from 3.3 to 5.6 percent. The response is stronger where municipal boundaries encompass more of the relevant hotel market, and municipal records show a shift in documented advocacy toward hotel-aligned positions. STR regulation reduces Airbnb revenues by constraining capacity, while increasing hotel revenues primarily through higher prices. For a hotel market contained within one city that initially has no STR regulation, an illustrative calculation combining these estimates implies 2.5 percent lower Airbnb capacity and 0.4 percent higher hotel prices after four years of merger exposure.

1. Introduction

Market power has direct and well-understood consequences for consumer welfare. In standard models of oligopoly, increased concentration can raise prices, reduce quantities, and lower consumer surplus. But market power may also shape the regulatory environment in ways that further soften competition. If so, changes in market structure can induce regulatory responses that restrict entry and alter competitive conditions, making market power self-reinforcing. Yet there is limited evidence on whether changes in market structure translate into regulatory outcomes and how those responses feed back into market equilibrium. We study this feedback in the

Date: November 3, 2026.
Calder-Wang (email: sophiecw@wharton.upenn.edu): University of Pennsylvania and NBER.
Farronato (email: cfarronato@hbs.edu): Harvard University, CEPR, and NBER.
Kang (email: karam.kang@wisc.edu): University of Wisconsin-Madison and NBER.
Le (email: qle@hbs.edu): Harvard University.

U.S. accommodations market, asking whether hotel mergers encourage cities to restrict short-term rentals (STRs) and how those restrictions subsequently affect both hotels and STRs.

The accommodations market offers a useful setting for linking market structure, regulation, and market outcomes. Hotels and STRs serve similar travelers but operate under different regulatory regimes. Over the past two decades, hotel consolidation has coincided with the rapid expansion of platforms such as Airbnb and Vrbo, which have introduced substantial competition for hotels. STR regulation is primarily local: cities adopt listing caps, zoning restrictions, licensing requirements, and platform-level compliance obligations. This decentralized policy environment allows us to observe regulatory decisions across cities and over time. We assemble a novel panel of STR regulations covering over 1,000 U.S. cities and exploit plausibly exogenous variation in their exposure to national hotel mergers. Because hotel chains have different pre-merger footprints across cities, the same national merger generates larger concentration increases in some cities than in others. We use this variation to estimate the effect of hotel concentration on regulatory adoption, then examine how regulation affects STR activity and hotel outcomes.

Hotel consolidation may affect these local regulatory decisions by changing both hotels' gains from STR restrictions and their incentives to pursue them. First, softer competition among hotels can increase the profits they earn from the demand diverted away from STRs. (Cowgill, Prat and Valletti, 2024). Second, consolidation can reduce free riding in political activity. Since STR restrictions benefit hotels regardless of whether they contribute to securing them, bringing a larger share of these benefits under a common decision-maker can therefore strengthen incentives to support collective advocacy (Olson, 1965; Moshary and Slattery, 2024).[1] Whether these forces translate into regulatory adoption depends on the responses of other constituencies and the weight policymakers place on competing interests, making the net regulatory effect of consolidation an empirical question.

To assess this net regulatory response, we first examine the 2016 merger between Marriott and Starwood, which created the world's largest hotel company. Using a hazard-rate design among cities that have not yet adopted regulation in a given

[1]See, for example, Pecorino (1998, 2001), and Magee (2002) on how group size, firm heterogeneity, and the distribution of policy gains shape firms' ability to sustain cooperation in lobbying, and Bombardini (2008) on how fixed costs of political organization make the firm-size distribution central to lobby formation.

semester, we find that cities exposed to merger-induced increases in the Herfindahl-Hirschman Index (HHI) of at least 100 points are significantly more likely to regulate STRs compared to cities with lower or no changes in HHI. The estimated 2.24 percentage-point increase raises the model-implied semester-level adoption probability among merger-exposed cities from 3.33% absent the merger to 5.58% with it. The effects are especially pronounced for policies that restrict STR supply or strengthen enforcement, including quantity restrictions and platform-level compliance requirements. For platform-level compliance obligations, the estimated increase is from 0.15% to 0.83% per semester. These estimates account for city fixed effects, state-by-semester fixed effects, and time-varying local housing and accommodation market conditions. The results are supported by alternative specifications, including a non-parametric hazard-rate difference-in-differences estimator following Deaner and Ku (2024). An instrumental-variables analysis using all national hotel mergers yields qualitatively similar patterns, although the estimates are less precise.

To assess whether the regulatory response is consistent with stronger hotel incentives to advocate for regulation, we test whether it is larger when municipal boundaries encompass a greater share of the relevant hotel market (*geographic alignment*). We measure geographic alignment as the share of each relevant hotel market's hotels located within the city, using hotel markets defined by Smith Travel Research and averaging across markets using their shares of the city's hotels as weights. A one-standard-deviation increase in alignment increases the estimated merger effect on any regulation by 1.99 percentage points per semester, a large increment relative to the baseline adoption probability of 3.33%. For platform liability, the corresponding increase is 0.89 percentage points, substantially higher than the baseline adoption probability.

Geographic alignment shapes the payoff hotels can expect from local regulation. When a city encompasses more of the market in which its hotels compete, STR regulations can have greater scope to reduce competitive pressure and generate gains for hotels inside the jurisdiction. When much of the market lies outside the city's jurisdiction, travelers can instead turn to STRs in nearby unregulated areas, limiting the gains that local restrictions generate for hotels. Geographic alignment may therefore strengthen the regulatory response to consolidation by increasing the benefits at stake in local policymaking. The pattern also places an additional requirement on explanations based on omitted local shocks: conditional on our controls and fixed effects, such shocks would need to generate a stronger relationship between merger

exposure and regulatory adoption in cities with greater alignment between the legal jurisdiction and the accommodations market.

To examine whether merger exposure is also associated with changes in political activity, we analyze records of city council and other municipal proceedings for a subset of our sample cities, based on the Legistar database.[2] We identify letters, testimony, petitions, and participation in policymaking, classifying activities as hotel-aligned or STR-aligned based on participants' affiliations and the policy positions expressed. Because regulatory decisions reflect both support and opposition, we examine the balance of activity between the two sides. Cities more exposed to the Marriott–Starwood merger exhibit a statistically significant shift in documented advocacy toward the hotel-aligned side relative to the STR-aligned side. The records also illustrate how these interests enter the policymaking process. In Chapel Hill for example, a working group involving "hoteliers concerned about unfair competition" and community organizations submitted a joint petition proposing STR restrictions. The American Hotel & Lodging Association similarly reported organizing "labor, affordable housing, neighborhood groups, and the broader business community" around STR policy. These findings are consistent with consolidation shaping regulation by shifting the balance of political activity.

To assess the economic stakes underlying these competing political interests, we next examine how STR regulation affects both segments of the market, using Smith Travel Research data for hotels and AirDNA data for Airbnb properties. We use two specifications: one examines the first regulatory adoption by a city within a hotel market, while the other tracks the share of the market's accommodation capacity located in cities that have adopted regulation. Restricting post-adoption observations to the first four years, the latter specification associates a move from no regulation to full market coverage with 29.4% lower Airbnb revenue and 18.4% lower Airbnb capacity, alongside 6.5% higher hotel revenue and 3.4% higher hotel prices. In dollar and capacity terms, these estimates imply an average monthly Airbnb revenue loss of $1.20 million and a hotel revenue gain of $1.96 million per market, with approximately 10,070 fewer available Airbnb listing-nights per month and hotel nightly prices higher by $4.55. These results indicate substantial economic stakes.

Finally, we combine the adoption and market-outcome estimates to illustrate how merger-induced regulation can affect competition. We consider a hotel market

[2]Legistar is Granicus's legislative-management platform through which municipalities publish agendas, meeting minutes, legislative records, and supporting documents.

contained within one city that initially has no STR regulation, so adoption brings the entire market under regulation. Using this approach, the calculation implies 4.1% lower Airbnb revenue and 2.5 percent lower Airbnb capacity after four years, alongside 0.8% higher hotel revenue and 0.4% higher hotel prices. These findings offer a new perspective on the 2023 Merger Guidelines' concern with barriers to entry or expansion (§2.6.A), highlighting a channel not explicitly discussed there: consolidation can strengthen incumbents' positions by encouraging regulatory restrictions on competing suppliers.

The interaction between market structure and political influence has long been central to political economy. Stigler (1971) formalizes regulation as the outcome of political exchange, while more recent work emphasizes the feedback between market power and political influence and the importance of market responses to policy (Callander, Foarta and Sugaya, 2022, 2024).[3] A growing empirical literature examines how market structure shapes political organization and spending (Bombardini and Trebbi, 2012; Cowgill, Prat and Valletti, 2024; Moshary and Slattery, 2024), and recent work highlights trade policy as a channel through which mergers can raise rivals' costs (Montag, 2026). We complement this work by studying how national mergers affect local governments' adoption of restrictions on competing suppliers and by linking those restrictions to outcomes in both the regulated and incumbent segments of the market. Our evidence highlights a regulatory channel through which consolidation can reshape competition.

Our paper also contributes to the literature on local land use regulation. A long line of work has measured the stringency of residential zoning across U.S. jurisdictions, including the Wharton Residential Land Use Regulatory Index (Gyourko, Saiz and Summers, 2008; Gyourko, Hartley and Krimmel, 2021) and extended more recently using natural language processing applied to municipal codes (Mleczko and Desmond, 2023; Bartik, Gupta and Milo, 2025; Sahn, 2025). We extend this measurement agenda to restrictions on the short-term rental use of residential properties, constructing a panel that records the timing and content of STR regulations in over 1,000 U.S. cities. This dataset allows us to study how local market structure relates to both the adoption of regulation and the types of restrictions cities choose.

[3]Related arguments appear in Rajan and Zingales (2003) and Wu (2011), which document how incumbents can shape regulation to entrench their market position in financial and information industries, respectively.

A related literature studies the economic impact of specific zoning instruments such as floor area ratios (Rollet, 2025), minimum lot sizes (Cui, 2023; Gyourko and McCulloch, 2023), and upzoning reforms (Diamond, Huang and McQuade, 2025); see Gyourko and Molloy (2015) and Glaeser and Gyourko (2018) for surveys. Much of this work focuses on regulations governing new residential construction. We study a complementary margin of land use regulation: restrictions on the short-term rental use of existing residential housing.[4] Our emphasis is on how these regulations arise, and specifically, how changes in the market structure of incumbent accommodation providers affect their adoption. This perspective also connects our analysis to research on political participation in local land use decisions (Einstein, Glick and Palmer, 2019). We highlight how the economic interests of incumbent businesses, alongside those of residents and property owners, can shape the restrictions on housing use.

In addition, a growing literature examines the effects of STR entry on competition with hotels (Zervas, Proserpio and Byers, 2017; Farronato and Fradkin, 2022), housing markets (Calder-Wang, 2021), and residential amenities (Almagro and Domínguez-Iino, 2025). More recently, Anastasi et al. (2025) examine how hotel interests may have influenced the STR ban in New York City. We extend this line of inquiry by examining the influence of hotel interests on STR regulation across a broad set of major U.S. markets.

The rest of the paper is structured as follows. Section 2 describes the institutional context and data. Section 3 presents a simple framework for how hotel concentration may affect STR regulation. Section 4 estimates the effect of hotel concentration on regulatory adoption. To get at mechanisms, Section 5 examines whether merger exposure shifts documented advocacy toward hotel-aligned positions. Section 6 studies the effects of STR regulation on accommodation-market outcomes. Section 7 combines our estimates to quantify the indirect effect of merger exposure through regulation. Section 8 concludes.

2. Context and Data

Our empirical setting combines three key features that make it well suited to study the interaction between market structure and regulation. First, the accommodations market consists of two competing segments—incumbent hotels and newer

[4]Research on the effects of such restrictions includes Jin, Wagman and Zhong (2024), who study Chicago's STR ordinance and highlight the importance of platform data sharing for enforcement and reductions in STR activity, and Anastasi et al. (2025), who find that enforcement of New York City's STR restrictions increased hotel prices and revenues.

short-term rentals (STRs)—that are substitutes but subject to different regulatory regimes. Second, variation in hotel market concentration is driven in part by large national mergers that generate heterogeneous changes across local markets. Third, STR regulation is determined at the local level and has expanded rapidly over time, providing rich variation in policy adoption. Together, these features allow us to link changes in market structure to regulatory responses and to subsequent market effects.

This section describes the data sources, accommodation markets, municipal STR policies, and the Marriott–Starwood merger. We also describe how individual hotels, chains, and industry associations participate in STR policymaking.

2.1. **Data Sources.** We combine a database of municipal STR regulation with hotel-market data from Smith Travel Research, STR-market data from AirDNA, and records of local political activity from Legistar. These sources allow us to connect changes in hotel market structure to regulatory adoption and accommodation-market outcomes, and to documented participation in the policymaking process.

First, we construct a database of STR regulation covering 1,058 U.S. cities. The collection begins with manual research on 323 of the largest cities, using municipal codes, legislative records, and other official sources. One research assistant collected information from public sources available online. A separate research assistant independently reviewed each entry, with disagreements resolved jointly by the two assistants. For each regulatory action, we record passage and effective dates when available and characterize licensing requirements, quantitative restrictions, platform obligations, taxation, and enforcement provisions. We date regulatory adoption using the passage date when available and the effective date otherwise.[5] We expand coverage using a large language model with integrated web search, guided by a structured protocol that prioritizes official sources and retains uncertainty when information cannot be verified.[6] Initial enactments and subsequent amendments are recorded separately. Appendix A describes the manual collection and GenAI-assisted extraction procedures. Our analysis sample comprises 1,018 cities, after excluding 35 cities with

[5]Some regulatory records report only one of these dates. Our results are robust to using the effective date when available and the passage date otherwise.

[6]We implement the LLM-based extraction both with and without supplying information from the manually collected dataset and compare the resulting regulatory records. The two approaches yield substantial agreement, providing a check on sensitivity to the supplied information. This comparison does not constitute independent validation, since both approaches may rely on the same underlying sources and share extraction errors.

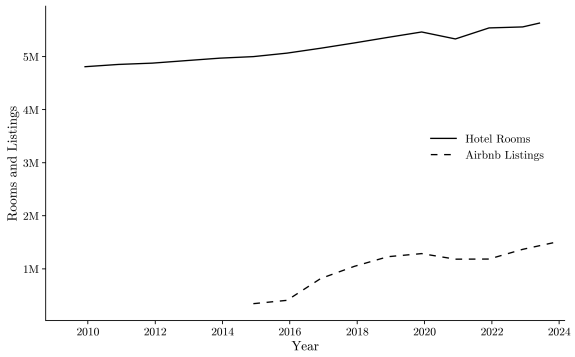
STR regulations but no recorded adoption date and 5 cities that could not be linked to the hotel-market or demographic data used in the analysis.

Second, we obtain hotel data from Smith Travel Research. The data report hotels and rooms by brand at a semester frequency between 2009 and 2024. Smith Travel Research defines geographic *submarkets*, which it calls "tracts," to approximate local areas where hotels are closer substitutes for one another than for hotels in other tracts. A submarket can encompass multiple smaller cities or portions of bigger cities. As a result, a large metropolitan area can contain several hotel submarkets. We use room counts and brand affiliations to construct hotel-market concentration. To map brands to parent companies over time, we combine merger and acquisition information from SDC Platinum with manual research on brand ownership. For cities covered by our manually collected regulation data, we also use monthly hotel performance data from Smith Travel Research. These data report revenue, rooms available, and rooms sold, aggregated by hotel scale (luxury through economy), submarket, and month.

Third, for the STR segment, we use listing-level data from AirDNA for 2014–2024. AirDNA collects information on listings offered through Airbnb and Vrbo, including location, availability, and prices. Because transactions are not publicly observed, bookings are inferred from listing-calendar information rather than measured directly from transaction records. We use these data to construct measures of STR supply, revenue, prices, and occupancy. The geographic aggregation and time windows used for the market-outcome regressions are described further below and more in depth in Appendix B.

Finally, we construct a database of documented political activity using Legistar, a platform through which municipalities publish legislative agendas, meeting minutes, and supporting documents. We collect legislative records for 164 cities and screen them for potential STR relevance. The selected records provide letters, petitions, hearing accounts, and other evidence from which an AI-assisted contextual review identifies documented activities. An activity is an identifiable action or role by a person, organization, or group in a particular proceeding, such as submitting a letter, speaking at a hearing, or participating in an advisory group. Repeated descriptions of the same activity are linked rather than counted separately. With this process, we identify 10,028 activities across 95 cities that are linked to STR rulemaking. Appendix C documents collection, classification, and construction of the outcomes used in the political-activity analysis.

FIGURE 1. Hotel Rooms and Airbnb Listings Over Time



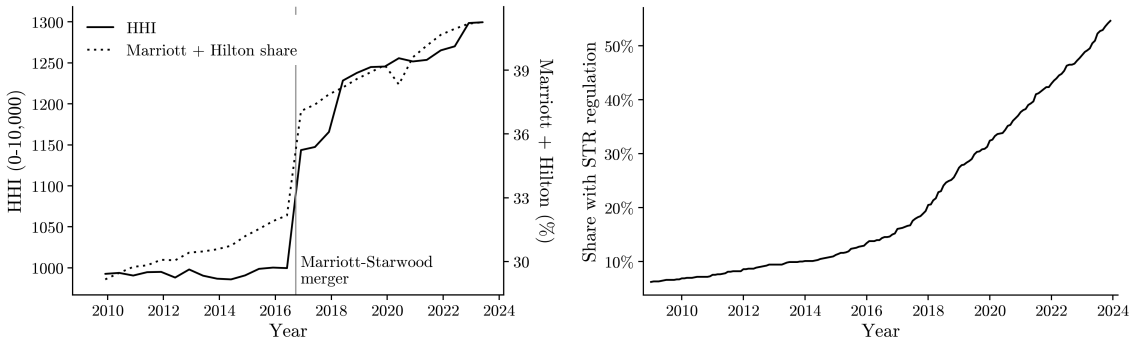
Notes: This figure plots the number of hotel rooms in the U.S. (solid line) and Airbnb listings (dashed lines). Hotel data are plotted for December of each year from 2009 to 2024, with the exception of June 2023 instead of December 2023. Airbnb is plotted for December of each year from 2014 to 2023.

2.2. **Accommodation Markets and STR Regulation.** Our data paint a clear picture of the growth of accommodations in the United States. Figure 1 shows how hotel room supply expanded by approximately 18% over nearly two decades, reaching 5.6 million rooms across nearly 64,000 properties by June 2024. More than two-thirds of these are branded rooms, and among those, 41% belong to either Marriott or Hilton, the two largest hotel brands in the U.S.

Figure 1 also highlights the expansion of short-term rentals from the beginning of our sample in mid 2010s (Airbnb was founded in 2008). By the end of 2023, the U.S. had 2.9 million listings: 52% on Airbnb, 19% on Vrbo, and 29% on both platforms. These listings generated $63 billion in revenue, with an average occupancy rate of 56% (although only 43% of nights were available) and an average nightly price of $313.

STR Regulation has become widespread, growing from 6% right before 2010 to covering 55% of the cities in our sample by December 2023 (Figure 2b). The majority of regulations involve registration requirements, whereby listings must be officially registered as short-term rentals. In addition, 40% of cities impose requirements beyond registration. Approximately 32% of cities implement quantity restrictions, such as limiting short-term rentals to owner-occupied units or capping their share of the housing stock (e.g., no more than 5%). Around 24% of cities impose explicit fines for noncompliance, with a median fine of $1,000 and an average of $2,500. Finally, 7%

FIGURE 2. Hotel Concentration and STR Regulation Over Time



(A) Hotel Concentration (B) STR Regulation

Notes: Panel A plots the national Herfindahl-Hirschman Index of hotel parent companies' shares of hotel rooms (solid line) and the share of rooms belonging to Marriott or Hilton brands (dotted line) over time. The vertical line marks the completion of the Marriott–Starwood merger in September 2016. Only rooms belonging to branded hotels are included. Panel B plots the share of U.S. cities among our sample of 1,058 cities with some form of STR regulation in effect.

of cities have introduced some form of platform liability, such as requiring platforms like Airbnb to verify compliance or prevent noncompliant listings from being posted.

We use the merger between Marriott and Starwood as a focal event that altered hotel market concentration, with effects that varied across geographies. The merger was announced in November 2015. The U.S. premerger antitrust waiting period expired on February 29, 2016, as announced by the companies on March 1, clearing a condition for completion of the transaction.[7] The transaction was completed in September 2016, creating the largest hotel company in the world, with a combined portfolio of 30 hotel brands—ranging from Ritz-Carlton to Sheraton and Courtyard—and more than 1.1 million rooms globally. Figure 2a plots U.S. hotel-industry concentration, measured by HHI, and the combined share of branded rooms held by Marriott and Hilton. Both measures rise sharply around the completion of the Marriott–Starwood merger in September 2016.

To measure hotel concentration at the local level, we assign each city to a single hotel market using the Smith Travel Research "submarkets" described above. The assigned market is the submarket containing the largest number of the city's hotel

[7]See Marriott International and Starwood Hotels & Resorts Worldwide, "Marriott and Starwood Announce Achievement of Key Milestones in Antitrust Review of Marriott's Proposed Acquisition of Starwood," March 1, 2016. Other jurisdictions, including Canada, China, and the European Union, also reviewed the transaction and ultimately cleared it.

properties in the June 2016 data, immediately before the merger.[8] We hold this assignment fixed throughout the panel, so each city inherits the concentration measures and merger exposure of the same submarket in every semester. Several cities can share an assigned market, and a city whose hotels span several submarkets is represented by its primary submarket. The city-to-market assignment therefore does not require market boundaries to coincide with municipal boundaries. The 1,018 cities in our sample correspond to 553 distinct primary submarkets.[9]

For each city now assigned to a hotel submarket, we compute the HHI by aggregating rooms across brands belonging to the same parent company.[10] Due to data limitations, the calculation uses the hotel inventory recorded for that submarket, including hotels outside the focal city's boundaries.[11] Merger exposure is the change in this market's HHI obtained by combining Marriott and Starwood under one parent while holding room inventory fixed at June 2016 levels. Thus, references to a city's HHI or merger-induced concentration change refer to its assigned hotel submarket. We examine how these market boundaries overlap city jurisdictions in Section 4.2.

Figure 3 shows that nearly all cities are in markets whose pre-merger concentration level does not raise antitrust concerns (HHI $< 1,500$) according to the merger guidelines that applied at the time. Only 30 cities (in 14 markets) raise potential concerns, as they combine moderately high concentration (HHI $> 1,500$) with increases in HHI of at least 100 points. None raises likely concerns, characterized by high concentration (HHI $> 2,500$) and increases in HHI exceeding 200 points. Cities that raise any level of concern tend to be small- to medium-size cities. The fact that the merger was cleared without remedies is consistent with this distribution.[12]

The merger nevertheless substantially expanded Marriott's local presence. Among treated cities, combining the two companies' pre-merger portfolios raises Marriott's share of rooms in the assigned hotel market by a median of 6.8 percentage points (a

[8]Ties in property counts are broken by the number of hotel rooms.

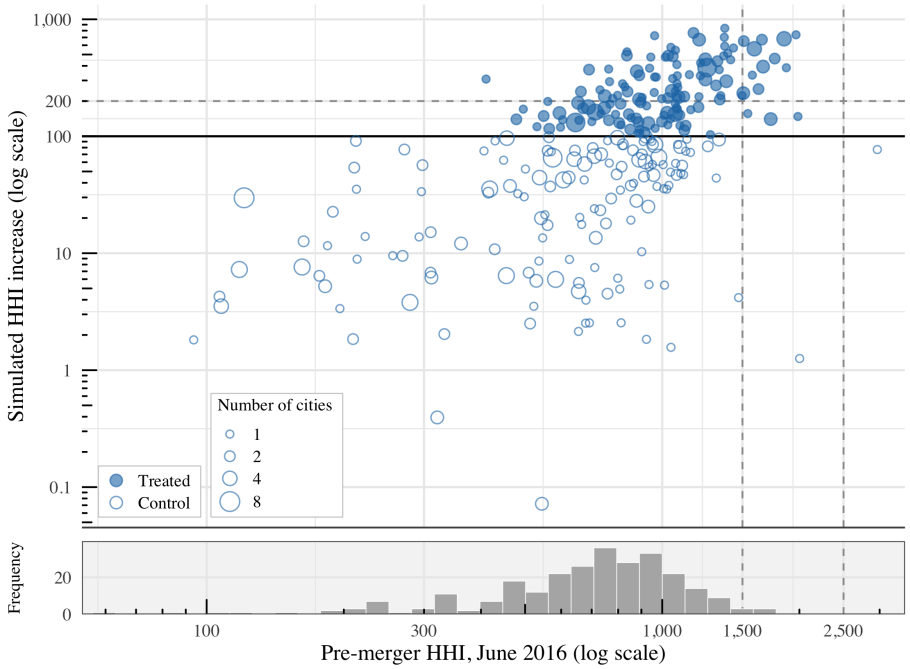
[9]The unit of observation in our regulatory-adoption panel in Section 4 is the city-semester, so cities that share a primary submarket remain separate observations.

[10]We treat each independent hotel as a separate competitor. Because we do not observe room counts for individual independent hotels, we divide the room inventory of independent hotels equally among them, assigning each hotel the average room count.

[11]Smith Travel Research only reports the inventory by chain and submarket, not by individual hotel. To build a submarket's inventory, we allocate each city's rooms and properties across the submarkets it spans, in proportion to the location of its hotels in the Smith's December 2017 property census. Appendix B details the construction of submarket hotel inventories and the resulting HHI measures.

[12]Appendix Figure F.1 provides additional histograms of the marginal distributions along the x- and y-axes in Figure 3.

FIGURE 3. Marriott–Starwood Merger and Hotel Concentration



Notes: The panel plots pre-merger HHI against the merger-induced change in HHI, holding room supply fixed at June 2016 levels and reassigning Marriott and Starwood properties to common ownership. Both axes are on a log scale. The top panel is restricted to the 274 submarkets in our sample for which we have a positive change in HHI due to the Marriott-Starwood merger. The bottom panel is a histogram of the pre-merger HHI of the 267 submarkets with no merger-induced change. The size of each market corresponds to the number of cities that market spans (recall that we have 1,018 cities corresponding to 553 distinct submarkets). Filled markers are "treated" by the merger in our preferred specification. Dashed lines mark the screening thresholds of the 2010 DOJ/FTC Horizontal Merger Guidelines: markets are unconcentrated below an HHI of 1,500, moderately concentrated between 1,500 and 2,500, and highly concentrated above 2,500; increases of more than 100 points may raise concerns in moderately concentrated markets, and increases above 200 points are presumed to raise concerns in highly concentrated markets.

median proportional increase of 41 percent). After the merger, the fraction of treated cities in which Marriott is the largest hotel chain increased from 56 to 88 percent (Appendix Figure F.2).

It is natural to question whether brand mergers translate into greater market power, given that hotels typically continue to operate under their pre-existing brand names and brands rarely own hotels directly. Indeed, properties are often owned by third parties and either managed by the parent company—more common for higher-tier hotels where maintaining quality and service standards is critical—or operated under franchise agreements.

Regardless of the organizational form, parent companies in the hotel industry exert substantial influence over pricing through centralized revenue-management systems. These systems aggregate demand data, monitor competitor prices, and generate recommended room rates (Ivanov, 2014; Talluri and Van Ryzin, 2006), while individual hotels retain formal discretion to deviate from the recommendations.

Appendix D.2 documents this arrangement in Marriott's own franchise agreements: a hotel retains formal discretion over its own rates, but Marriott supplies the pricing system and the data, may require consistent pricing across distribution channels, and, for hotels enrolled in its revenue-management program, has its own staff maintain the hotel's rates and inventory restrictions. By 2018, about 52% of Marriott's North American properties had their rates managed directly by Marriott, either because Marriott operated them or because they participated in the program.[13] Brand consolidation can therefore coordinate pricing across a larger number of properties even when ownership remains dispersed, increasing market power despite the persistence of distinct brand identities.

2.3. **Political Organization of the Hotel Industry.** Individual hotel owners and managers can participate directly in policymaking, while chains provide an organizational structure for coordinating political activity across brands and properties through government-affairs staff and networks of local managers. For example, Marriott's Business Councils bring together general managers and associates from different brands within a market. Its government-affairs staff manage outside lobbyists and coordinate grassroots advocacy through these councils and hotel properties, alongside engagement with industry associations and coalitions.[14] These arrangements enable political coordination across affiliated hotels despite differences in property ownership and management.

Chains also encourage participation in and help govern industry associations. The American Hotel & Lodging Association (AHLA) credits brand companies with promoting membership, reporting 3,240 participating Marriott properties and 533 Starwood properties in its 2016 annual report. Chain executives also serve in its

[13]Marriott reports that in 2018 its North American revenue-management program served 1,642 properties, including 227 legacy Starwood properties. Together with the 827 properties Marriott managed directly, this amounts to 52% of its 4,769 North American hotels.

[14]Marriott's job postings describe roles supporting these councils and their involvement in advocacy. See, for example, Marriott International, "Business Councils Intern," posted through the Rochester Institute of Technology, November 30, 2023, for summer 2024; and "Director, State & Local Government Affairs," reproduced by the Public Affairs Council, November 2, 2025.

leadership: in 2016, Marriott's David Grissen served on AHLA's executive committee, and Starwood's Bob Hermany served on its board of directors. AHLA's membership extends beyond chains to hotel owners, management companies, independent hotels, and state associations. Councils representing these constituencies communicate policy priorities, contribute to the association's strategy, and nominate board representatives.[15]

Industry associations support STR restrictions through research, engagement with public officials, and coalition building. Both AHLA reports and contemporaneous media coverage describe the association's coordination with state associations, labor groups, housing advocates, and neighborhood groups.[16] In New York City, Anastasi et al. (2025) describe the hotel association's collaboration with an affordable-housing coalition and city officials to secure Local Law 18. Although both hotels and STRs could lobby, Anastasi et al. (2025) document substantially greater campaign contributions from hotel interests than from STR interests.[17] Our municipal records similarly document direct lodging-industry participation in Louisville and a coalition of hoteliers, business organizations, and neighborhood advocates in Chapel Hill (Appendix C.7).

Hotel advocacy thus combines participation by individual properties with coordination by chains and industry associations. These associations facilitate collective action but need not eliminate free riding among members, as Moshary and Slattery (2024) discuss in the context of automobile dealerships. Consolidation may therefore change a chain's willingness to undertake or support advocacy by altering its returns to regulatory intervention. Section 3 develops these incentives, while Section 4 examines changes in regulatory adoption and Section 5 the balance of political participation.

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[15]See American Hotel & Lodging Association, 2016 Annual Report, pp. 1 and 14, for leadership and membership participation; and 2015 Annual Report, p. 4, for the membership and council structure.

[16]See American Hotel & Lodging Association, *2015 Annual Report: Achieving New Heights*, pp. 7–8; and *The New York Times*, "Inside the Hotel Industry's Plan to Combat Airbnb," April 16, 2017. Illustrating its municipal focus, AHLA outlined plans to identify target cities with state partners for council-level action on STR taxation, safety, and health requirements. See Sean O'Neill, "American Hotel Association to Fight Airbnb and Short-Term Rentals," April 30, 2014, available through PhocusWire.

[17]Their New York City data record almost $600,000 in hotel-industry campaign contributions over 2008–2023, compared with just over $18,000 from Airbnb and other home-sharing interests. These figures do not measure total lobbying expenditures or STR-specific spending.

3. Why Hotel Concentration May Affect STR Regulation: A Conceptual Framework

Before turning to the empirical analysis, we briefly outline why hotel concentration may affect local STR regulation. This framework is intended to organize our arguments, not to make a separate theoretical contribution. It also helps interpret our estimates of merger effects on regulation in Section 4 and our evidence on political mechanisms in Section 5.

Following Stigler (1971), we view STR regulation as the outcome of political exchange between those who seek regulation and policymakers who supply it. Hotels may favor restrictions on STRs, while hosts and platforms may oppose them. Residents, homeowners, renters, hotel workers, and other groups may support or oppose regulation for different reasons.

We represent a policymaker's objective as

$$W(r; M) = \lambda(M)\Pi_H(r; M, q_c) + V(r; M),$$

where $r \in \{0, 1\}$ indicates whether the city adopts STR regulation, M denotes hotel market structure, and Π_H is hotel profit.[18] The weight $\lambda(M)$ captures the influence assigned to hotel interests relative to those represented in V. Consolidation may change this weight by reducing free riding, facilitating coordination, or increasing the resources devoted to advocacy, without necessarily changing policymakers' underlying preferences. The term V captures the value of regulation to other stakeholders, whose interests and participation may reinforce or offset those of hotels.

The geographic reach of local regulation may also shape hotels' gains from it. Boston, for example, regulates STRs within its city limits, while travelers may choose among accommodations in Boston and neighboring jurisdictions. Let q_c denote the share of the relevant hotel market within city c's jurisdiction. When this share is small, travelers and STR suppliers may substitute toward nearby unregulated jurisdictions, limiting the effect of local restrictions on hotels' competitive conditions and profits. Greater geographical alignment (i.e., higher q_c) may therefore increase hotels' gains from regulation and, in turn, the return to incurring the fixed cost of organizing advocacy in that city.

[18]The binary adoption condition is a simplification. In practice, policymakers choose among different regulatory instruments, each with potentially different effects on hotel profits and other stakeholders. When multiple options are available, the policymaker chooses the option that maximizes $W(r; M)$.

The policymaker adopts regulation when

$$\lambda(M)\Delta\Pi_H(M, q_c) + \Delta V(M) \geq 0,$$

where $\Delta\Pi_H$ and ΔV are the changes in hotel profits and other stakeholders' value under regulation. Thus, regulatory adoption depends both on the weight placed on hotel interests and on whether local regulation can meaningfully change hotel profits. In addition, holding $\lambda(M) > 0$ and ΔV fixed, greater alignment favors adoption if it increases $\Delta\Pi_H$.

Concentration may increase hotels' incentive to seek regulation through two channels. First, restrictions on STRs may increase hotel profits, but competition among hotels can dissipate some of these gains as hotels compete them away. If consolidation softens hotel competition, hotels may retain a larger share of the gains from STR regulation, increasing its value to them. This rent-dissipation channel is emphasized by Cowgill, Prat and Valletti (2024). In a related trade-policy setting, Montag (2026) distinguishes changes in the gains from protection due to competitive behavior from gains internalized by bringing the target under common ownership. Applying the first channel to hotel-chain mergers requires consolidation to affect competitive conduct, through arrangements such as those discussed in Section 2 and Appendix D.2.

Second, concentration may reduce free riding in political activity. STR restrictions can benefit many hotels, giving each hotel an incentive to let others bear the cost of advocacy. When consolidation brings a larger share of those beneficiaries under a common decision-maker, that decision-maker may internalize more of the gains and have a stronger incentive to support political activity, even if total hotel gains from regulation do not change (Olson, 1965; Moshary and Slattery, 2024).

These channels can reinforce one another: larger gains from STR restrictions may strengthen hotels' incentives to support advocacy, while reduced free riding may make that advocacy easier to organize. Whether these forces translate into regulatory adoption depends on the interests and political responses of other constituencies and the weight policymakers place on them. Our empirical analysis estimates the net regulatory response to merger-induced concentration. We then examine changes in the balance of documented local advocacy to provide complementary evidence on the political activity accompanying this response.

4. Hotel Concentration and Short-term Rental Regulations

The framework above suggests that hotel consolidation may affect STR regulation by changing both the gains from restricting competition and the incentives to pursue those gains through political activity. In this section, we estimate the resulting regulatory response using variation in local hotel concentration induced by the Marriott–Starwood merger. Because the two chains had different pre-merger footprints across cities, the same national merger generated heterogeneous changes in local hotel concentration (Figure 3). We treat STR regulation as effectively absorbing: cities may amend their rules, but rarely repeal them. Our hazard-based difference-in-differences design thus focuses on cities that have not yet adopted regulation and estimates how merger exposure affects adoption, both overall and by specific instrument. We assess the robustness of these estimates to differences in covariate balance and pre-existing adoption trends, and implement a nonparametric hazard-rate difference-in-differences estimator as an alternative approach. We then exploit continuous variation in concentration changes generated by multiple national hotel mergers in an instrumental-variables specification. Finally, we examine whether the regulatory response is stronger when municipal boundaries encompass a larger share of the relevant hotel market. This heterogeneity provides a test of the role of local political incentives and helps distinguish our mechanism from alternative explanations based on omitted local shocks.

4.1. **Main Analysis: Exploiting the Marriott–Starwood Merger.** We begin by examining how variation in hotel market concentration, induced by the Marriott–Starwood merger, affects the adoption and stringency of STR regulation. Our empirical design focuses on the timing of regulatory adoption. Specifically, we restrict the sample to city-semester observations prior to the introduction of regulation and estimate the probability that a city adopts a regulation in a given semester, which we interpret as discrete-time hazards of regulatory adoption.[19] Figure 4a illustrates how the probability of adopting STR regulation evolves over time among cities that have not yet adopted it. We estimate the following specification:

$$y_{ct} = \beta \, \text{Treated}_c \times \text{Post}_t + \phi_c + \nu_{s(c)t} + \mathbf{x}_{ct}\theta + \varepsilon_{ct}, \tag{1}$$

[19]Although, in principle, cities could both adopt and subsequently repeal STR regulations, in practice regulation has expanded over the past 15 years with virtually no reversals. Once a city adopts STR regulation, it may amend it, but it rarely repeals it. We therefore treat regulation as an absorbing state: once a city regulates STRs, it remains regulated thereafter. This setting closely resembles a hazard framework.

where c indexes cities and t indexes semesters. We consider four adoption outcomes: any STR regulation; restrictions beyond registration or licensing alone; quantity restrictions, such as limits on rented nights or which properties can operate as STRs; and platform obligations, such as requirements to verify registrations or remove non-compliant listings, or provisions that subject platforms to fines for non-compliance.[20] Each dependent variable y_{ct} equals one in the semester of first adoption and zero in preceding semesters. The indicator Treated_c equals one if the city experiences a Marriott–Starwood merger-induced change in HHI of at least 100 points.[21]

We control for time-varying city characteristics, $\mathbf{x}_{ct}$, that may affect both hotel market structure and STR regulatory adoption. These include log population and log median household income; housing-market conditions measured by the renter share, rental vacancy rate, rent-to-income ratio, and seasonal or second-home share; and the importance of the local accommodations sector, measured by the accommodation and food services employment share and the log number of hotel properties in the city's assigned hotel submarket.[22] City fixed effects, ϕ_c, absorb persistent differences across cities, while state-by-semester fixed effects, $\nu_{s(c)t}$, account for changes common to cities within a state, including changes in statewide lodging policies.[23] The estimation sample is restricted to at-risk city–semester observations with complete covariate data; 976 of the 1,018 cities in our analysis panel are linked to Census records, and the number retained for each regression varies by its regulatory outcome.

Table 1 reports our estimates. The coefficient on $(\Delta\text{HHI} \geq 100) \times \text{Post}$ is positive and statistically significant across all specifications, indicating that cities exposed to larger merger-induced increases in hotel market concentration are more likely to adopt short-term rental (STR) regulations following the Marriott–Starwood merger. In column (1), the probability that a previously unregulated city adopts any form of STR regulation increases by 2.24 percentage points per semester, relative to cities with smaller (or no) changes in HHI. Among merger-exposed cities, the model implies a

[20]These categories overlap and may be adopted at different dates. Appendix A.4 provides the detailed definitions and explains how we assign adoption dates and handle incomplete records.

[21]The results are robust to alternative choices of pre-treatment windows and HHI change thresholds used to define treatment (Appendix Figure F.4).

[22]The demographic, housing, and employment variables come from the U.S. Census Bureau's American Community Survey five-year estimates for 2009–2023. Hotel property counts come from Smith Travel Research.

[23]State-level STR regulation is rare and generally takes one of two forms: extending lodging taxes to STRs or prohibiting cities from banning them.

TABLE 1. Effect of Marriott–Starwood Merger on STR Regulation Adoption

Dependent variable:	Any Regulation (1)	Beyond Licensing (2)	Quantity Restrictions (3)	Platform Obligations (4)
$(\Delta\text{HHI} \geq 100) \times \text{Post}$	0.0224***	0.0222***	0.0202***	0.0068***
	(0.0075)	(0.0060)	(0.0054)	(0.0024)
Time-varying city attributes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
State-semester FE	Yes	Yes	Yes	Yes
Counterfactual adoption probability	0.0333	0.0191	0.0141	0.0015
Fitted probability with merger exposure	0.0558	0.0414	0.0343	0.0083
Observations	22,167	23,968	25,061	27,795
Cities	906	926	945	974
R-squared	0.153	0.130	0.125	0.095

Notes: This table reports unweighted baseline difference-in-differences estimates of equation (1). Each outcome indicates first adoption of the specified category; subsequent observations are excluded. Columns cover (1) any STR regulation, (2) restrictions beyond licensing alone, (3) limits on rental nights or permitted rental arrangements, and (4) platform verification, blocking, or fine provisions. Appendix A.4 details the coding and adoption-date conventions. ΔHHI is the merger-induced change in hotel market concentration, expressed on the 0–10,000 HHI scale. Post equals one from the second semester of 2016 onward. All specifications include city fixed effects, state-by-semester fixed effects, and eight time-varying city controls: log population, log median household income, renter share, rental vacancy rate, second-home share, rent-to-income ratio, accommodation/food employment share, and log hotel property count. The counterfactual adoption probability is the average fitted probability among merger-exposed post-merger city-semesters still at risk of first adoption after setting the treatment effect to zero. The fitted probability with merger exposure retains the estimated treatment contribution. These quantities are post-estimation summaries for the merger-exposed post-merger risk set, not pooled outcome means. R-squared is the overall regression R-squared. Standard errors are clustered by city and reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

semester-level adoption probability of 3.33 percent absent the merger, so the estimated effect represents an increase of about 67 percent.

Columns (2)–(4) show that merger exposure increases the likelihood of adopting restrictions beyond licensing alone, quantity restrictions, and platform-based liabilities. The effect is largest relative to the baseline for platform liability (last column), which requires platforms such as Airbnb and Vrbo to help ensure compliance with local rules and may therefore strengthen enforcement. Merger exposure raises the semester-level adoption probability for these provisions by 0.68 percentage points, more than four times the baseline probability absent the merger, which is 0.15 percent. These results indicate that the regulatory response extends beyond licensing to restrictions on STR supply and obligations for platform enforcement.

4.1.1. *Robustness: Covariate Balance and Differential Adoption Trends.* A potential concern is that cities more exposed to the Marriott–Starwood merger differ from less-exposed cities in ways that predict subsequent STR regulatory adoption even in the absence of the merger. City fixed effects absorb persistent differences in adoption propensities, but they do not account for changes over time associated with those characteristics. For example, larger cities or cities with different political preferences may become more inclined to regulate STRs for reasons unrelated to hotel consolidation. We assess this concern through complementary exercises: reweighting cities to improve balance in observed pre-merger characteristics, allowing regulatory adoption to evolve differently with city and hotel-market characteristics, and allowing flexible time patterns associated with local political preferences and council composition.

Appendix Table F.1 shows that merger-exposed cities differ from less-exposed cities along several pre-merger dimensions measured in 2015. Among the 807 cities on common propensity-score support, treated cities have larger populations and higher median household incomes, fewer hotel properties in their assigned markets, lower rental vacancy rates, smaller second-home shares, and lower accommodation and food services employment shares.[24] These patterns motivate examining whether the estimated regulatory response persists when treated and control cities are made more comparable in their observed characteristics.

Appendix Table F.2 reports estimates for cities on common support, both without weighting and using weights that make control cities resemble treated cities or overlap weights that emphasize cities with comparable treatment propensities. All specifications retain the baseline controls and fixed effects. The estimated effects on any regulation, restrictions beyond licensing alone, and quantity restrictions remain positive and statistically significant across specifications. Platform-liability estimates also remain positive, although they are less precise in some specifications. These exercises support the robustness of the main findings to differences in observed pre-merger characteristics.

Next, we allow cities with different observed characteristics to experience different changes in regulatory adoption after 2016, independently of merger exposure. Appendix Figure F.5 reports specifications that separately interact the post-merger indicator with demographic, housing, and hotel-market characteristics, retaining the

[24]We estimate propensity scores using a logit model with the eight baseline characteristics and centered squared terms for log population, log income, and log hotel-market size. The model uses 972 cities with complete covariate data; common support is the intersection of the treated and control propensity-score ranges.

baseline controls and fixed effects. We find that the estimated effect on any regulation remains positive and statistically significant across specifications.

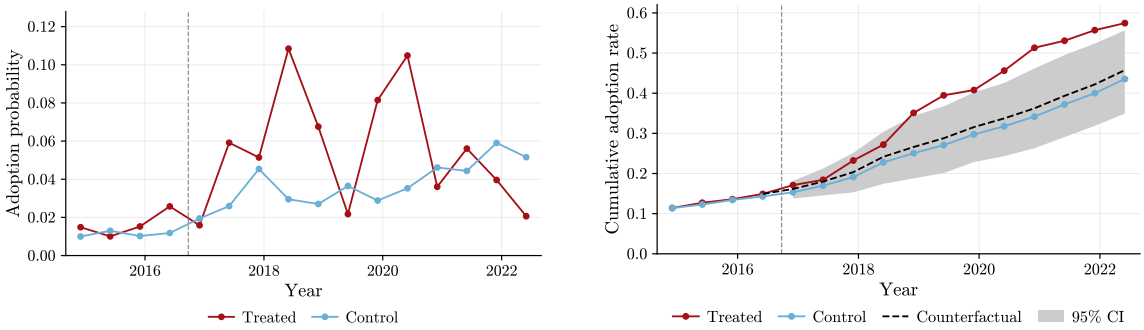
A related concern is that the chains' pre-merger locations reflect hotel-market characteristics associated with subsequent regulatory changes that demographic, housing, and other hotel-market controls do not capture. This concern is potentially relevant because, while Marriott is present across a broad range of markets, differences in pre-merger Starwood presence account for much of the variation in merger exposure (Appendix Figure F.3). We therefore also allow post-merger changes to vary with pre-merger hotel-market structure. Appendix Figure F.6 reports specifications interacting the post-merger indicator with other chains' room shares, the overall branded-room share, and hotel quality-segment shares. The estimates remain positive and most are statistically significant. These checks support the robustness of the regulatory response to differential changes associated with observed hotel-market structure, without ruling out differences associated with unobserved characteristics.

We also examine whether the results reflect differences in local political orientation. Using the American Local Government Elections Database (de Benedictis-Kessner et al., 2023), we measure cities' Democratic vote shares in the 2016 presidential election and their average Democratic shares among council members classified as Democratic or Republican over 2009–2015.[25] Appendix Table F.3 allows more flexible time patterns by interacting each political measure, either as a continuous share or an indicator for a share above 50%, with semester indicators. These interactions allow cities with different political orientations to follow different semester-by-semester adoption paths, independently of merger exposure. Because political data cover a smaller set of cities, we compare each specification with the baseline estimated on the same sample. The merger effects remain positive, statistically significant, and close to these same-sample benchmarks, suggesting that differential adoption patterns associated with local partisanship do not explain our findings.

4.1.2. *Robustness: Non-parametric Specification.* As a non-parametric complement to our linear hazard specification, we implement the hazard-rate difference-in-differences estimator of Deaner and Ku (2024). This approach compares adoption probabilities

[25]The 2016 presidential vote share captures local political orientation around the merger. Although the election followed the merger's completion, presidential voting is unlikely to respond to hotel-industry consolidation.

FIGURE 4. Adoption Probabilities and Counterfactual Adoption Path



(A) Empirical adoption probabilities (B) Cumulative adoption and counterfactual

Notes: Panel (A) plots, for each semester, the share of cities without STR regulation that adopt one by the next semester. Panel (B) plots the cumulative share of cities with STR regulation, along with the counterfactual adoption path for treated cities constructed using the hazard-rate difference-in-differences estimator of Deaner and Ku (2024). Cities are treated if the resulting HHI increase from the Marriott-Starwood merger exceeds 100. The vertical line in each panel indicates the timing of the merger. Shaded bands represent 95% bootstrap confidence intervals from 500 bootstrap draws of cities.

among cities that remain unregulated, accommodating the absorbing nature of regulatory adoption. Identification requires that, absent the merger, the gap in time-averaged hazard rates between treated and control cities would have remained constant. The assumption permits the gap in *cumulative* adoption to widen mechanically as the at-risk pool shrinks, without attributing that widening to treatment.

We estimate the pre-treatment hazard gap using the four semesters before the merger. Adding this gap to the observed control-group hazards yields counterfactual hazards for treated cities, which we then use to construct their cumulative adoption path absent the merger. Figure 4 plots semester-level adoption probabilities in panel (A) and cumulative adoption in panel (B). Following the merger, cumulative adoption among treated cities rises above its estimated counterfactual, with the gap growing over time.

4.1.3. *Extension: Using Multiple National Mergers.* Appendix E extends the analysis using continuous variation in hotel concentration generated by national hotel mergers recorded in our data. We instrument lagged hotel-market HHI with simulated merger-induced concentration changes accumulated over a rolling five-year window. Appendix Table E.1 reports estimates from a linear IV hazard model with city and

state-by-semester fixed effects, paralleling the baseline specification, and a control-function Probit with state-by-year and semester fixed effects. Both specifications retain the full set of baseline controls. The instrument strongly predicts concentration, and the estimated effects on regulatory adoption are positive across outcomes and specifications. However, none of the linear IV estimates is statistically significant at conventional levels; only platform liability reaches significance at the 10% level in the Probit.

4.2. **Heterogeneity by Geographic Alignment of Political vs. Market Boundaries.** We next examine whether the regulatory response to merger exposure varies with the geographic alignment between city boundaries and hotel markets. The framework in Section 3 suggests that greater alignment may increase hotels' gains from local STR restrictions and strengthen their incentives to support regulation. We therefore test whether merger exposure has a larger effect on regulatory adoption in cities that encompass a greater share of their relevant hotel markets.

4.2.1. *Measuring Geographic Alignment.* We measure geographic alignment using U.S. Census place boundaries and Smith Travel Research hotel submarkets.[26] For each submarket containing hotels in a city, we calculate the share of its hotels located within that city. We then average these shares, weighting each submarket by its share of the city's hotels:

$$\text{Alignment}_c = \sum_j \frac{n_{cj}}{N_c} \frac{n_{cj}}{M_j},$$

where n_{cj} is the number of hotels in city c and submarket j, $N_c = \sum_j n_{cj}$ is the city's total hotel count, and M_j is the submarket's total hotel count. Higher values indicate that more of the city's relevant hotel markets lie within its jurisdiction. Appendix Figure F.7 illustrates the construction for Dallas: its hotels span six hotel submarkets, yielding an alignment index of 0.69.

For 803 of the 897 cities with an observed alignment index, all hotels belong to a single submarket.[27] In these cases, alignment is simply the fraction of that submarket's hotels located within the city: a city containing 10 of a submarket's

[26]We use the 2017 hotel census data, which is the closest snapshot we have to the Marriott–Starwood merger with complete latitude–longitude information. The hotel census data report each property's location and its assignment to a submarket defined by Smith Travel Research.

[27]The geographic construction retains Census places classified as cities, towns, or villages. Of the 976 cities linked to Census records, 71 fall outside these retained place types. Eight additional cities have no hotels matched within their boundaries in the 2017 Smith Travel Research hotel census, leaving 897 cities with an observed alignment index.

100 hotels has an index of 0.10. Appendix Figure F.8(A) shows that alignment is generally low, with a median of 0.183 and a mean of 0.267. Weighting cities by their hotel counts raises the mean to 0.488, indicating that cities with more hotels tend to encompass larger shares of their relevant hotel markets.

Appendix Table F.4 compares cities at or below the median alignment index with those above it. Low-alignment cities have smaller populations (approximately 58,000 versus 189,000 residents) and fewer hotels within their boundaries (12.6 versus 41.7), but their assigned hotel markets contain more properties (147.3 versus 81.8). They also share their hotel markets with more cities (17.3 versus 5.6, weighted by local hotel shares). Income and housing characteristics differ less markedly. Cities with low alignment tend to belong to broad regional hotel markets rather than be part of densely clustered municipalities. Indeed, low-alignment cities have fewer nearby city boundaries within 1 km on average (2.5 versus 4.3). Geographic alignment differs across major U.S. regions: mean alignment ranges from 0.184 in the Northeast to 0.299 in the South, with means of 0.247 in the West and 0.263 in the Midwest.[28]

4.2.2. *Heterogeneity in the Merger Effects on STR Regulations.* We extend Equation (1) by interacting merger exposure with the standardized alignment index. We also include an interaction between alignment and the post-merger indicator, allowing post-merger changes in adoption to vary with alignment independently of merger exposure. All specifications retain the baseline controls and fixed effects.

Table 2 shows that geographic alignment is associated with both statistically significant and economically meaningful differences in the regulatory response to merger exposure.[29] A one-standard-deviation increase in alignment increases the estimated merger effect on any regulation by 1.99 percentage points per semester, an increment that is about 60% of the baseline adoption rate of 3.33% from Table 1. For platform liability, the corresponding increase is 0.89 percentage points, almost six times its baseline mean of 0.15%. Both interactions are significant at the 1% level. The interactions for licensing and quantity restrictions are positive but imprecisely estimated. At mean alignment, the estimated merger effects are positive for all four outcomes,

[28]Appendix Figure F.8(B) illustrates the substantial variation that also exists within a region: neighboring cities in the Dallas–Fort Worth area range from low to nearly complete alignment.

[29]Appendix Table F.5 replaces continuous alignment with an indicator for alignment above the sample median. The pooled specification retains common control slopes and state-by-semester fixed effects and includes the indicator interacted with Post. The estimated high-minus-low differences are positive for any regulation and platform liability, statistically significant at the 10% level.

TABLE 2. Geographic Alignment and STR Regulation Adoption

Dependent variable:	Any Regulation (1)	Beyond Licensing (2)	Quantity Restrictions (3)	Platform Obligations (4)
$(\Delta\text{HHI} \geq 100) \times \text{Post}$	0.0124	0.0156**	0.0164***	0.0033
	(0.0076)	(0.0062)	(0.0057)	(0.0022)
$(\Delta\text{HHI} \geq 100) \times \text{Post} \times \text{Alignment}$	0.0199***	0.0064	0.0025	0.0089***
	(0.0072)	(0.0057)	(0.0045)	(0.0030)
$\text{Post} \times \text{Alignment}$	0.0047	0.0041*	0.0022	0.0007
	(0.0032)	(0.0023)	(0.0020)	(0.0008)
Time-varying city attributes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
State-semester FE	Yes	Yes	Yes	Yes
Observations	20,400	22,094	23,084	25,525
Cities	833	852	869	893
R-squared	0.156	0.130	0.124	0.101

Notes: This table reports unweighted difference-in-differences estimates of regulatory adoption, allowing treatment effects to vary with standardized geographic alignment. Treatment is a merger-induced HHI increase of at least 100 points on the 0–10,000 scale; Post begins in the second semester of 2016. City-semesters with regulation already in place in the preceding semester are excluded; the adoption semester is retained. Alignment uses the 2017 hotel-submarket index, standardized across 897 cities (mean 0.267; standard deviation 0.234). The treatment × post coefficient is the effect at mean alignment. All regressions include city and state-semester fixed effects and eight time-varying controls as the baseline specifications in Table 1. City-clustered standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

but statistically significant only for beyond-licensing requirements and quantity restrictions.

Because alignment covaries with city population and hotel inventory, we also allow adoption patterns and merger effects to vary across groups defined by these characteristics. Appendix Tables F.6 and F.7 report specifications using population and hotel-inventory tertiles and quintiles. The estimated effect of merger exposure on any regulation continues to increase with alignment across these specifications, although the interaction becomes imprecise with hotel-inventory quintiles.

5. Mechanisms Linking Hotel Consolidation to STR Regulation

To assess the political channel behind the results in the previous section, we now examine whether the regulatory response to hotel consolidation is accompanied by changes in political activity. As discussed in Section 3, consolidation may increase

hotels' gains from STR restrictions and strengthen their incentives to support advocacy, either directly or by nudging participation from other constituencies. We use municipal legislative records from Legistar to test whether merger exposure shifts the balance of documented advocacy toward positions aligned with hotel interests relative to those aligned with STR interests.

The institutional evidence in Section 2 illustrates how hotels participate directly and through associations and broader coalitions. Our legislative records capture some of these activities, but participants' affiliations are often unreported (Appendix C.7). We therefore examine advocacy aligned with hotel or STR interests, without assuming that all hotel-aligned activity is organized or financed directly by the hotel industry.

We classify activities using participants' identified affiliations and the policy positions expressed. Activities involving hotels or hotel associations are classified as hotel-aligned, while those involving STR platforms, hosts, operators, or their associations are classified as STR-aligned. When affiliations are unavailable, we classify activities according to whether their predominant direction favors STR restrictions or STR availability.[30] This procedure identifies 4,092 hotel-aligned activities (40.8%) and 4,076 STR-aligned activities (40.6%); activities with mixed or unclear alignment remain unassigned (Appendix Table C.2). We therefore examine the broader balance of policy positions, rather than limiting attention to participants explicitly identified as industry representatives.

We count unique political activities documented in the legislative records by city, semester, and advocacy side. Outcomes distinguish all activities, written submissions, and oral or other active participation. Written submissions include letters and submitted comments; oral or other active participation includes hearing testimony, petitions, advisory roles, consultations, negotiations, and collective advocacy.[31] Our main sample requires at least four semesters of qualifying Legistar availability before the second semester of 2016. A semester qualifies when all six months appear in the recorded coverage calendar and at least one meeting has a link to the corresponding records. We retain qualifying semesters for eligible cities, yielding an initial sample

[30] Affiliations or roles cannot be established for any participant in 81.4% of policy-related activities. See Appendix C for details.

[31] Written submissions and oral or other active participation are mutually exclusive categories. Activities with any oral or other active component are assigned to the latter.

of 48 cities and 960 city–semesters.[32] Within eligible periods, a side with no identified activity receives a zero count, which denotes no documented activity rather than verified absence of participation.

For each outcome, we estimate a Poisson pseudo-maximum-likelihood model on a panel stacked by advocacy side:

$$\mathbb{E}[Y_{sct} \mid c, t, s] = \exp[\alpha_{cs} + \lambda_{ts} + \delta \,\text{Treated}_c\text{Post}_t + \beta \,\mathbf{1}\{s = H\}\text{Treated}_c\text{Post}_t] \,, \quad (2)$$

where Y_{sct} is the count of documented political activities for side s in city c and semester t, and $s \in \{H, S\}$ distinguishes hotel-aligned from STR-aligned activity. As before, the indicator Treated_c equals one for cities with a merger-induced HHI increase of at least 100 points on the 0–10,000 scale, and Post_t equals one beginning in the second semester of 2016. City-by-side fixed effects, α_{cs}, absorb persistent differences in each side's recorded activity, while semester-by-side fixed effects, λ_{ts}, allow each side to follow its own common time path.

The coefficient δ captures the treatment-associated change in the STR-aligned side's log expected count, while $\delta + \beta$ captures the corresponding change for the hotel-aligned side. Our primary parameter, β, measures the differential response, and $\exp(\beta) - 1$ is the proportional change in the hotel-to-STR ratio of expected political activity. The stacked model estimates this relative response without constructing observed ratios, which would be undefined when the STR-aligned count is zero. A causal interpretation requires that, absent the merger, treated and control cities would have experienced parallel proportional changes in each side's expected activity, with no treatment-related changes in documentation that differentially affect the two sides.

Our focus on relative advocacy (β) reflects both strategic and measurement considerations. Because supporters and opponents can respond to one another, changes in one side's stakes or organization may alter both sides' activity. The balance of participation therefore captures a dimension of the political response that the hotel side's level alone would miss. Cities also differ in how extensively they document participation. Comparing the two sides can reduce sensitivity to these differences when more extensive documentation increases the recorded activity of both sides proportionately. This comparison does not eliminate concerns about differences in how the two sides' activities are recorded or changes in the composition of observed participation.

[32]Eligibility does not require observing political activity or regulatory adoption. The broader available-records sample applies the same semester-level availability rule without requiring four qualifying pretreatment semesters. It contains 115 cities and 1,568 city–semesters before estimation-specific exclusions. Results appear in Appendix Table F.8.

TABLE 3. Balance of STR Advocacy: Pre-Treatment Coverage Sample

	Any involvement	Written	Oral/Active
(ΔHHI $\geq$100) $\times$ Post	-1.142	-0.831	-1.200
	(1.002)	(1.458)	(0.957)
Hotel-aligned $\times$ (ΔHHI $\geq$100) $\times$ Post (β)	1.203**	0.522	0.774
	(0.530)	(0.749)	(0.567)
Relative proportional effect ($e^{\beta}-1$)	2.331**	0.685	1.169
	[0.13, 8.80]	[-0.65, 7.01]	[-0.32, 5.91]
City $\times$ side fixed effects	Yes	Yes	Yes
Semester $\times$ side fixed effects	Yes	Yes	Yes
Observations (city–semester–side)	975	672	891
Distinct city–semesters	578	385	530
Cities	33	22	30
Treated cities	15	10	12
Pseudo-R^2	0.406	0.543	0.317

Notes: Each column reports the Poisson pseudo-maximum likelihood regression estimates of equation (2), where the outcome variable is the count of documented political activities based on local legislative records from Legistar, with separate observations for hotel-aligned and STR-aligned advocacy in each city–semester. Δ HHI is the merger-induced change in hotel market concentration, expressed on the 0–10,000 HHI scale. Post equals one beginning in the second half of 2016. The STR-aligned side is the reference category. The sample requires all six months of a semester to fall within the recorded Legistar collection window and at least one posted minutes link during that semester. Cities must additionally have at least four qualifying semesters before the second semester of 2016. The initial sample contains 960 city–semesters in 48 cities; reported sample sizes reflect estimator exclusions. Standard errors clustered by city are in parentheses, and 95% confidence intervals are in brackets. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

After estimation-specific exclusions, the specification for all political activities retains 33 cities, including 15 merger-exposed cities. Table 3 reports a statistically significant shift toward hotel-aligned advocacy relative to STR-aligned advocacy. For all political activities, the estimated interaction coefficient is 1.203, implying a 233% increase in the hotel-to-STR ratio of expected activity relative to its counterfactual.[33] The corresponding estimates for written submissions and oral or other active participation imply increases of 69% and 117%, respectively, but neither is statistically significant at conventional levels.

These findings associate merger exposure with a shift in documented participation toward hotel-aligned interests, but do not establish an increase in hotels' own advocacy. The individual-side estimates, $\widehat{\delta}=-1.142$ for STR-aligned activity and

[33]The broader available-records sample yields a similar estimated increase of 224% (Appendix Table F.8).

$\widehat{\delta}+\widehat{\beta} = 0.061$ for hotel-aligned activity, suggest lower STR-aligned participation alongside approximately unchanged hotel-aligned participation, although neither response is statistically distinguishable from zero. The results thus complement the regulatory adoption results by linking consolidation to changes in the balance of competing voices in local policymaking.

6. Effects of Short-Term Rental Regulations

The preceding sections show that hotel consolidation increases the likelihood of STR regulatory adoption and provide evidence of an associated shift in documented advocacy toward hotel-aligned positions relative to STR-aligned positions. We now examine the consequences of STR regulation for the accommodations market. We ask whether regulation reduces STR activity and generates gains for hotels, providing evidence on the economic stakes that may shape regulatory decisions. We begin with the effects on STR and hotel revenues, then examine changes in capacity, occupancy, and prices.

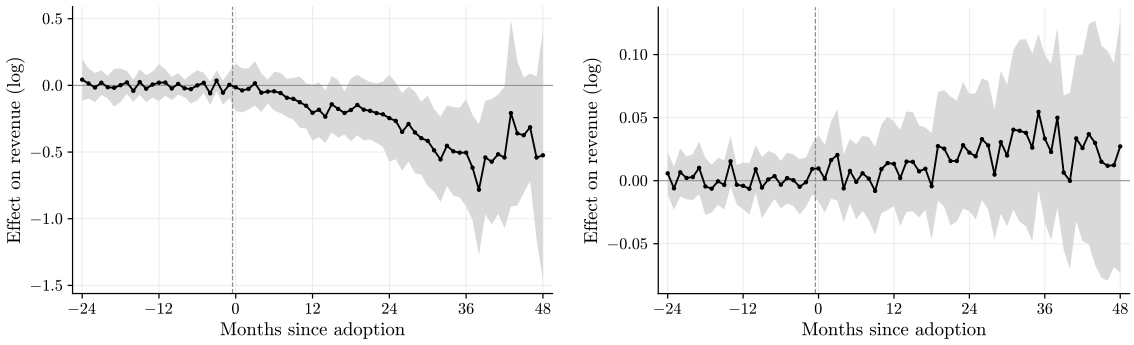
Our unit of analysis is the hotel submarket-month, using the geographic submarkets defined by Smith Travel Research. We aggregate hotel performance data from Smith Travel Research and STR activity from AirDNA to this level. The AirDNA sample includes properties listed on Airbnb, including properties cross-listed on Airbnb and Vrbo, and excludes Vrbo-only properties.[34]

We analyze revenue, average daily rate (ADR), capacity, and occupancy, expressing revenue and ADR in 2019 dollars. Capacity is available room-nights for hotels and reserved plus available listing-nights for Airbnb; occupancy is nights sold divided by capacity. The hotel sample spans June 2010–January 2020 and the Airbnb sample January 2015–January 2020, both ending before the COVID-19 pandemic. To ensure sufficient pre-treatment data, we retain submarkets first regulated in June 2013 or later for hotels and January 2016 or later for Airbnb, along with never-treated submarkets. Appendix Table F.9 summarizes these samples.[35] Average monthly revenue per submarket is $19.4 million for hotels and $1.34 million for Airbnb, with substantial right-skewness in both segments.

[34]To limit sample changes from AirDNA's addition of Vrbo coverage in June 2017, we retain only properties with an Airbnb identifier and include all their observed months, even those listed only on Vrbo, because outcomes are reported by property rather than platform.

[35]These statistics cover all sample observations, whereas the level calculations below use only post-adoption observations in treated submarkets.

FIGURE 5. Event-study Estimates for Airbnb and Hotel Revenue



(A) Airbnb revenue (B) Hotel revenue

Notes: The figure plots Callaway–Sant'Anna (2021) event-study estimates for seasonally adjusted log revenue, measured in 2019 dollars before taking logs. Airbnb includes Airbnb-only and Airbnb–Vrbo cross-listed properties, counted once per property-month; Vrbo-only properties are excluded. Event time is months relative to the first regulatory adoption by a city overlapping the hotel submarket. Estimates allow six months of anticipation and use never-treated and not-yet-treated submarkets as comparisons. At each event time, cohort-specific effects are averaged with weights proportional to cohort size; contributing cohorts may vary across event times. Shaded areas are 95% simultaneous confidence bands from a multiplier bootstrap clustered by submarket. The panels display months −24 through 48; effects are in log points.

Because hotel submarkets may span multiple cities, we date treatment to the first STR regulation adopted within a submarket. The estimates capture changes following this first adoption, including any subsequent expansion in regulatory coverage. We average available effects over the first 48 months after adoption, without requiring four years of follow-up for every submarket. The sample includes 403 Airbnb submarkets and 332 hotel submarkets, of which 164 and 154, respectively, experience adoption.

We use the estimator of Callaway and Sant'Anna (2021), comparing adopting submarkets with never-treated and not-yet-treated submarkets and allowing for six months of anticipation.[36] We seasonally adjust all outcomes and estimate effects on revenue, ADR, and capacity in logs and occupancy in percentage points.[37] Standard errors are clustered by hotel submarket. Figure 5 plots the dynamic effects on Airbnb and hotel revenue. Airbnb revenue declines after adoption, with estimates of approximately −0.21 log points after one year, −0.25 after two years, and −0.50 after three years. Converting these estimates using $100[\exp(\widehat{\theta}) - 1]$ gives proportional declines

[36]Appendix Figure F.9 examines sensitivity to individual adoption cohorts. The average revenue estimates are similar when each cohort is omitted in turn.

[37]The results are similar without seasonal adjustment, except that the first-adoption Airbnb ADR estimate becomes positive and significant (Appendix Table F.10).

of approximately 19, 22, and 40 percent. Hotel revenue point estimates suggest a gradual increase, but the confidence bands are wide.

Columns (1) and (2) of Table 4 report average first-adoption effects through month 48. The Airbnb estimates imply declines of 16.9 percent in revenue and 11.6 percent in capacity, with no statistically significant changes in ADR or occupancy. For hotels, the point estimates imply increases of 1.5 percent in revenue, 1.0 percent in ADR, and 0.9 percent in capacity, alongside a 0.20 percentage-point decline in occupancy. Although none of these hotel estimates is statistically distinguishable from zero at conventional levels, complementary Poisson-imputation estimates show significant increases in hotel revenue and ADR.[38]

To account for regulation expanding across cities within a market, we define $\text{RegulatedShare}_{jt}$ as the share of baseline accommodation capacity in submarket j covered by STR regulation in month t.[39] For revenue, ADR, and capacity, we estimate

$$\log Y_{jt} = \beta \, \text{RegulatedShare}_{jt} + \alpha_j + \lambda_t + \varepsilon_{jt},$$

where outcomes are seasonally adjusted and α_j and λ_t are submarket and year-month fixed effects. We estimate the same specification for occupancy in levels. We use the first-adoption sample, retaining post-adoption observations through month 48.[40]

Columns (3) and (4) of Table 4 report the estimates. Moving from zero to full coverage is associated with 29.4 percent lower Airbnb revenue and 18.4 percent lower capacity, alongside 6.5 percent higher hotel revenue, 3.4 percent higher ADR, and 3.1 percent greater hotel capacity. The hotel capacity estimate is significant at the 10 percent level; Airbnb ADR and occupancy in both segments show no statistically significant changes. Full coverage exceeds the roughly one-half average coverage observed after first adoption, but the larger hotel estimates may also reflect differences in estimators.

[38]Appendix Table F.11 reports Poisson-imputation estimates following Wooldridge (2023) and Moreau-Kastler (2025). The first-adoption estimates imply increases of 3.6 percent in hotel revenue and 4.7 percent in ADR, significant at the 5 and 1 percent levels, respectively. The larger hotel estimates may partly reflect differences in weighting, as well as differences in the outcome model and estimation sample.

[39]Airbnb weights use listings created before January 2016, including cross-listed properties, assigned to the city of their nearest hotel. Hotel weights use rooms in June 2016. We hold weights fixed so coverage changes reflect regulatory adoption rather than shifts in supply.

[40]Unlike the first-adoption analysis, this specification treats the six months before adoption as untreated, without allowing for anticipation.

TABLE 4. Effects of STR Regulation on Market Outcomes

	First adoption		Regulatory coverage	
	Airbnb (1)	Hotels (2)	Airbnb (3)	Hotels (4)
Revenue (log)	−0.185***	0.015	−0.348***	0.063***
	(0.053)	(0.010)	(0.069)	(0.021)
ADR (log)	0.004	0.010	0.027	0.034***
	(0.017)	(0.006)	(0.018)	(0.011)
Occupancy rate (pp)	−0.289	−0.200	−0.798	0.163
	(0.473)	(0.337)	(0.565)	(0.431)
Capacity (log)	−0.123***	0.009	−0.203***	0.031*
	(0.032)	(0.006)	(0.045)	(0.016)
Treated submarkets	164	154	164	154
Control submarkets	239	178	239	178
Submarkets	403	332	403	332
Submarket-months	24,527	38,086	24,527	38,086
Estimator	CS	CS	TWFE	TWFE

Notes: Columns (1)–(2) use Callaway–Sant'Anna (CS) estimates with six months of anticipation and never-treated and not-yet-treated comparisons, averaging effects over months 0–48 after adoption. Columns (3)–(4) use two-way fixed effects (TWFE) on the same sample, treating pre-adoption months as untreated. Coverage is measured using fixed baseline listing shares for Airbnb and room shares for hotels. All outcomes are seasonally adjusted. Revenue, ADR, and capacity are in natural logs; occupancy effects are in percentage points (pp). Standard errors, in parentheses, are clustered by submarket, using a multiplier bootstrap for CS estimates. Counts refer to the revenue sample. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Higher hotel capacity alongside little change in occupancy is consistent with hotel supply expanding to accommodate additional demand.[41] STR restrictions could encourage new hotel entry or investment in existing hotels without converting existing STR properties into hotels.[42] A separate spatial-matching exercise finds no

[41]Appendix Figure F.10 shows a gradual increase in independent hotels' share of capacity after regulatory adoption, although the estimates are imprecise and do not establish a clear change in the composition of hotel supply.

[42]HotelSpaces' "The Airbnb Math Problem Behind the World's First AI Hotel" (March 23, 2026) links Las Vegas's Otonomus Hotel to STR restrictions and financing constraints. Woodlock Capital's "200–210 Spring Street" project page (accessed October 30, 2026) promotes hotel-use entitlements over STR use for a proposed 152-unit Charleston development.

strong evidence of increased conversion into independent hotels, but does not measure prospective entrants' choices between hotel and STR projects.[43]

Table 5 translates the estimates into dollar and capacity changes by applying the estimated proportional effects to implied outcomes absent regulation, assuming a common proportional response across submarkets and months. For first adoption, the calculation implies monthly Airbnb revenue losses of approximately $675,000 and hotel revenue gains of $470,000 per submarket, although the hotel estimate is imprecise. Moving from zero to full regulatory coverage implies monthly losses of $1.20 million for Airbnb and gains of $1.96 million for hotels. Airbnb capacity falls by approximately 6,345 listing-nights per month under first adoption and 10,070 under full coverage, while hotel ADR rises by $1.34 and $4.55, respectively, with only the latter statistically significant. The implied hotel revenue gains amount to roughly $0.70 per dollar of Airbnb revenue lost under first adoption and $1.63 under full coverage, suggesting potentially substantial gains for hotels; however, these comparisons use different submarket samples and should not be interpreted as direct transfers of spending from Airbnb to hotels.

Our findings in Sections 4 and 5 underscore that regulatory adoption is an endogenous policy choice, shaped by hotel market structure and competing political interests. This raises two distinct concerns. First, effects may differ between markets that adopt regulation and those that do not. For example, selective lobbying may make adoption more likely where hotels expect larger gains from restricting STRs. The first-adoption design targets effects among adopting markets, which need not match the effects of imposing the same policies on randomly selected markets. This limits the generalizability of the estimates, but does not by itself threaten identification of the effect on treated markets.

Second, identification of that effect requires that, absent regulation, outcomes in adopting markets would have evolved in parallel with those in comparison markets. This assumption may fail if the conditions that lead cities to regulate also predict changes in accommodation-market outcomes. For example, cities may be more likely to restrict STRs where policymakers or incumbent hotels anticipate substantial Airbnb expansion. If STR activity in adopting markets would otherwise have grown faster than in comparison markets, our estimates would understate the magnitude

[43]Matching the 2017 and 2024 hotel censuses to Airbnb-only listings within approximately 100-meter cells yields regulatory effects of 2.5 percentage points (SE 1.8) on independent hotels' same-year overlap and 1.9 points (SE 3.3) on newly observed independent hotels' overlap with 2017 listings. Neither is statistically significant, and spatial overlap need not indicate conversion.

TABLE 5. Changes in Outcome Levels Implied by STR Regulation

	First adoption		Full regulatory coverage	
	Change (%)	Change (level)	Change (%)	Change (level)
Panel A. Hotels				
Revenue (2019$)	1.5	469,647	6.5***	1,963,478***
	(1.0)	(320,841)	(2.2)	(644,290)
ADR (2019$)	1.0	1.34	3.4***	4.55***
	(0.6)	(0.86)	(1.1)	(1.49)
Occupancy rate (pp)	—	−0.200	—	0.163
		(0.337)		(0.431)
Capacity (room-nights)	0.9	2,556	3.1*	8,849*
	(0.6)	(1,588)	(1.6)	(4,537)
Panel B. Airbnb				
Revenue (2019$)	−16.9***	−674,866***	−29.4***	−1,204,065***
	(4.4)	(209,828)	(4.9)	(251,617)
ADR (2019$)	0.4	0.59	2.8	4.33
	(1.7)	(2.62)	(1.8)	(2.83)
Occupancy rate (pp)	—	−0.289	—	−0.798
		(0.473)		(0.565)
Capacity (listing-nights)	−11.6***	−6,345***	−18.4***	−10,070***
	(2.8)	(1,728)	(3.7)	(2,296)
Hotel gain / Airbnb loss ($)		0.70		1.63
		(0.52)		(0.63)

Notes: Calculations use the estimates in Table 4 and 3,966 Airbnb and 4,480 hotel treated-post submarket-months through month 48. For log outcomes, percentage changes are $100[\exp(\beta) - 1]$. First-adoption level changes are $\overline{Y}_T[1 - \exp(-\beta)]$, where $\overline{Y}_T$ uses the cohort-months and weights of the CS estimate. Regulatory-coverage level changes are $\overline{Y \exp(-\beta D)}[\exp(\beta) - 1]$, where D is each observation's actual regulatory coverage; they describe a move from zero to full coverage. Counterfactual mean monthly revenues are $31,185,580 (hotels) and $3,990,015 (Airbnb) for first adoption, and $30,368,759 and $4,098,129 for the dose calculation. These conversions assume a common proportional first-adoption effect or regulatory-coverage response across submarket-months. Occupancy entries are the estimated percentage-point effects from Table 4. Standard errors use the delta method, holding observed outcomes and doses fixed and allowing the dose coefficient to affect the counterfactual mean. Ratio standard errors treat hotel and Airbnb estimates as independent. Ratios compare different submarket samples and are not spending-transfer shares. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

of any regulation-induced decline.[44] More generally, the direction of bias depends on how the conditions prompting adoption would have affected each outcome absent

[44]Another concern is that merger exposure may affect accommodation-market outcomes directly as well as predict regulatory adoption. If these direct effects differ between adopting and comparison markets, the estimates may partly capture merger effects unrelated to regulation.

regulation. The continuous-coverage regressions additionally impose a common linear response in log outcomes to coverage, and their two-way fixed-effects estimates need not identify an average causal response to regulatory coverage under heterogeneous effects.

The revenue event studies in Figure 5 provide reassuring evidence: we find no clear differential trends before the six-month anticipation window. However, similar pre-adoption trends cannot rule out selection based on expected future market conditions, or departures from parallel trends that the estimates are too imprecise to detect. Additional sensitivity analyses will help assess how strongly our conclusions depend on these assumptions and on the construction of the event-study estimates.[45]

With these qualifications, the estimates indicate that STR regulation reduces Airbnb capacity and revenue and provide evidence of gains to hotels, although the magnitude and precision of the hotel responses vary across specifications. These findings help explain the economic stakes surrounding STR regulation. In the next section, we combine the estimated market responses with the effect of merger exposure on regulatory adoption to quantify the merger's indirect consequences through regulation.

7. Quantifying the Indirect Regulatory Channel

We combine the estimated effect of merger exposure on STR regulatory adoption with the market-outcome estimates in Section 6 to illustrate the merger's indirect regulatory channel. We consider an initially unregulated hotel submarket contained within a single city, so that the city's adoption moves the market from zero to full regulatory coverage. We report changes in expected market outcomes four years after merger exposure, relative to the counterfactual without the estimated merger-exposure effect. These are effects prevailing at that horizon, rather than cumulative changes in revenue or capacity over the intervening years.

7.1. Merger Exposure and the Probability of Regulation.

We treat regulation as an absorbing state and hold semester-level adoption probabilities constant over the four-year horizon. Among merger-exposed city–semesters after the merger, Table 1 implies an adoption probability of $h_0 = 0.0333$ absent the estimated merger effect and a fitted probability of $h_1 = 0.0558$ with it. Conditional on the city being initially

[45] In the next revision, we plan to assess sensitivity to departures from parallel trends using the approach of Rambachan and Roth (2023), and examine alternative anticipation windows and a common set of adoption cohorts across event times.

unregulated, its probability of having adopted by semester t under scenario $m \in \{0, 1\}$ is

$$p_m(t) \equiv \Pr(R_t(m) = 1 \mid R_0 = 0) = 1 - (1 - h_m)^t,$$

where $m = 0$ excludes the estimated merger-exposure effect and $m = 1$ includes it. At four years, or eight semesters, these probabilities are approximately 23.7 and 36.8 percent. The merger-induced increase in the probability of regulation is therefore $\Delta P_8 = p_1(8) - p_0(8) = 0.1306$, or 13.06 percentage points. Because the market lies within one city, this is also the increase in its probability of full regulatory coverage.

7.2. **Mapping Regulatory Exposure into Market Outcomes.** Table 6 reports two calculations using the first-adoption and regulatory-coverage estimates in Table 4. The latter evaluates a move from zero to full coverage, matching the regulatory transition in our single-city illustration. The first-adoption calculation provides a benchmark, although first adoption need not cover an entire market in the estimation sample. Both calculations assume that responses estimated across the broader sample apply to merger-induced regulation in single-city markets.

For revenue, ADR, and capacity, let $\theta_Y = \exp(\beta_Y^q) - 1$ denote the proportional effect under either specification. We hold expected unregulated outcomes fixed across scenarios and assume a common proportional effect that begins at adoption and remains constant thereafter. Expected outcomes under scenario m are then proportional to $1 + p_m(8)\theta_Y^q$, giving the four-year indirect effect

$$\text{Indirect Effect}_8^Y = 100\,\frac{\Delta P_8\,\theta_Y}{1 + p_0(8)\theta_Y}. \tag{3}$$

The denominator accounts for regulation that would occur without the additional merger-exposure effect. For occupancy, we multiply the estimated percentage-point effect by ΔP_8. Standard errors use the delta method, holding the counterfactual adoption probability fixed and treating the adoption-probability and market-outcome estimates as independent.

The regulatory-coverage calculation implies that merger-induced regulation reduces Airbnb activity while benefiting hotels. After four years, Airbnb revenue and capacity are 4.13 and 2.51 percent lower, respectively (Panel B), while hotel revenue rises by 0.83 percent, ADR by 0.44 percent, and capacity by 0.40 percent (Panel C). The hotel revenue and ADR effects are significant at the 5 percent level, and the capacity effect at the 10 percent level. The first-adoption calculation also implies lower Airbnb revenue and capacity, by 2.30 and 1.56 percent, but yields small, statistically

TABLE 6. Indirect Effects of Merger Exposure After Four Years

	Four-year indirect effect	
	First adoption (1)	Full regulatory coverage (2)
Panel A. Merger-induced regulatory exposure		
Additional adoption probability (pp)	13.06***	13.06***
	(4.00)	(4.00)
Panel B. Airbnb outcomes		
Revenue (%)	−2.30**	−4.13***
	(0.94)	(1.46)
Average daily rate (%)	0.05	0.36
	(0.22)	(0.26)
Occupancy rate (pp)	−0.04	−0.10
	(0.06)	(0.08)
Capacity (%)	−1.56**	−2.51***
	(0.61)	(0.93)
Panel C. Hotel outcomes		
Revenue (%)	0.20	0.83**
	(0.15)	(0.38)
Average daily rate (%)	0.13	0.44**
	(0.09)	(0.20)
Occupancy rate (pp)	−0.03	0.02
	(0.04)	(0.06)
Capacity (%)	0.12	0.40*
	(0.08)	(0.24)

Notes: The calculation considers an initially unregulated market contained within a single city. Adoption moves coverage from zero to one. Panel A uses the semester-level adoption probabilities from Table 1: 0.0333 absent the merger effect and 0.0558 with it. Panels B–C report effects prevailing after eight semesters using equation (3). Column (1) applies the CS first-adoption estimates in Table 4; column (2) applies the regulatory-coverage estimates on the same sample, evaluated at full coverage. Both use Airbnb including cross-listed properties and a post-adoption window through month 48. These coefficients are estimated on the broader sample, not exclusively on single-city markets. Revenue, ADR, and capacity use $\theta = \exp(\beta) - 1$; occupancy uses the direct percentage-point effect multiplied by the additional adoption probability. Standard errors in parentheses use the delta method, holding the counterfactual adoption probability fixed and treating the merger and market-outcome estimates as independent. The semester-level merger effect is 0.0224 (standard error 0.0075). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

insignificant hotel effects. Thus, both calculations point to a contraction in competing Airbnb supply, while the evidence of hotel gains depends on the specification. Neither specification implies statistically significant changes in Airbnb ADR or occupancy in either segment.

These illustrative calculations replace the dynamic responses in the event studies with constant effects after adoption and abstract from differences in regulatory stringency, spillovers to neighboring markets, and other equilibrium responses. Subject to these assumptions, they link consolidation to lower Airbnb capacity and revenue and, under the regulatory-coverage specification, higher hotel prices and revenue. They illustrate a potential channel for merger effects through the regulation of rival suppliers, without measuring welfare effects or its importance relative to the merger's direct effects.

8. Conclusion

This paper studies how hotel consolidation affects the regulation of competing short-term rentals. Exploiting differences in local exposure to the national hotel merger of Marriott and Starwood in 2016, we find that merger-induced increases in hotel concentration raise the likelihood that cities adopt STR regulation. The response is stronger where municipal boundaries encompass a larger share of the relevant hotel market, consistent with the gains from local restrictions shaping incentives to seek regulation. Municipal legislative records provide complementary evidence of a shift in documented advocacy toward hotel-aligned interests. We also find that STR regulation reduces Airbnb capacity and revenue while raising hotel prices and revenue. Combining the adoption and market-outcome estimates in an illustrative calculation connects consolidation to a contraction in competing supply through regulation. Taken together, the findings suggest that consolidation can reshape competition by changing the rules facing competing suppliers.

For antitrust authorities, these findings suggest a broader perspective on how mergers may raise barriers to rivals' entry or expansion: consolidation can change incumbents' incentives and capacity to influence the rules governing competing suppliers. Merger assessment may therefore benefit from considering how consolidation shapes subsequent regulatory decisions. For local policymakers, they underscore the importance of evaluating STR restrictions in light of both their stated objectives and their competitive consequences. Our results do not establish that these restrictions reduce welfare: policies that limit STR activity may benefit residents through housing availability or neighborhood conditions even as they restrict accommodation supply and benefit hotels. A welfare analysis would weigh these potential benefits against the effects on travelers and accommodation providers, accounting for spillovers across municipal boundaries. Understanding when restrictions address local concerns, and

how incumbent interests shape their design, remains an important direction for future research.

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Appendix A. Construction of STR Regulation Data

This appendix describes the construction of our municipal short-term rental (STR) regulation database and the regulatory adoption measures used in the analysis. We first explain the manual and GenAI-assisted collection of regulatory histories, including initial enactments and subsequent amendments, and compare the records obtained through the two approaches. We then describe how we classify policy provisions, assign adoption dates, and construct city–semester indicators for four regulatory categories used in our analysis: any STR regulation, regulation beyond licensing, quantity restrictions, and platform obligations. Throughout, we discuss the treatment of incomplete information and the limitations of the resulting measures.

A.1. **Manual Collection.** We assemble a database of municipal short-term rental (STR) regulations that records the timing and content of initial enactments and subsequent policy changes. We combine manual collection for an initial set of cities with GenAI-assisted collection covering a broader municipal sample, described below. Recording regulatory actions separately allows us to distinguish initial adoption from later amendments and to identify when different policy provisions were introduced.

The manually collected dataset covers 323 U.S. cities and contains 633 city–regulation records. One research assistant collected and coded information from municipal codes, city council records, and official government websites. A second research assistant independently reviewed the collected information against the source documents. The two research assistants discussed discrepancies and reached a joint decision. For each regulatory action, the dataset records passage and effective dates when available, substantive policy provisions, and supporting source links and notes. The recorded provisions include registration and licensing requirements, restrictions on STR operation and availability, platform obligations, and enforcement mechanisms.

The manual records distinguish unclear provisions from provisions not mentioned in the collected material. A provision is coded as "unclear" when relevant text is identified but cannot be classified, while a zero indicates that the provision is not mentioned. Such a zero does not necessarily establish that no applicable requirement exists. These provision-level records are distinct from the analytical policy categories, whose construction we describe below. The manual dataset also serves as a benchmark for comparisons with the GenAI-assisted collection.

A.2. **GenAI-Assisted Collection.** We extend the geographic coverage of the regulatory database through GenAI-assisted collection. The broader roster of 1,058 cities was assembled by selecting cities with large hotel-room inventories and adding cities from the manual collection that were not already included. The collection framework uses a large language model with integrated web search to identify municipal STR regulations and extract their timing, substantive provisions, and supporting sources.[46]

During development, collection was conducted both with and without hints from the manual records. The hints supplied previously collected ordinance dates and source links as starting information for the model's searches. The processed dataset selected for constructing the analytical outcomes comes from the collection without these hints: the model received city identifiers but was not supplied with the manually collected ordinance dates or source links.

A.2.1. *Regulatory scope and policy histories.* The collection protocol sought to reconstruct each city's regulatory history. The model was instructed to identify STR-specific ordinances, material amendments, administrative rules, emergency orders, and court orders affecting STR legality or enforcement. Where no STR-specific ordinance existed, the instructions also covered transient-lodging or hotel regulations applicable to STRs. Initial enactments and amendments changing legality, zoning coverage, quantitative limits, platform obligations, or enforcement were to be recorded as separate policy versions. For each version, the requested dates included passage, effectiveness, the start of enforcement when distinct, and repeal or supersession when applicable.

A.2.2. *Sources and documentary support.* The protocol emphasized official documents as the basis for identifying and characterizing regulatory actions. The instructions prioritized municipal code libraries, followed by city clerk and legislative records, official STR-program and planning-department pages, and relevant court documents. Each policy version was to include at least one official source, preferably more than one, with quotations from relevant code sections when available. The model was also instructed to document state-level preemption affecting municipal regulatory authority. These source requirements were intended to make the extracted provisions traceable to the underlying documents; they are distinct from independent verification of the returned records.

[46]The archived collection code uses OpenAI's Responses API with web search and specifies `gpt-5` as its default model.

A.2.3. *Substantive provisions.* The extraction schema captured differences in rules across rental arrangements, geographic areas, and regulated parties. It distinguished hosted and unhosted rentals, owner-occupied and non-owner-occupied properties, tenant hosting, and accessory dwelling units. Requested provisions included annual rental-night caps, minimum-stay and primary-residence requirements, restrictions involving rent-controlled properties, and limits on the concentration of STRs. To represent variation within a municipality, the schema distinguished a citywide baseline from zone-specific rules and exceptions.

Host obligations were recorded separately from obligations imposed on booking platforms. Requested host requirements included registration, licenses with inspections, discretionary permits, fees and renewal intervals, insurance, local contacts, and the display of registration numbers. Platform requirements included verifying registrations, blocking noncompliant listings, sharing data, displaying registration numbers, and joint liability. Enforcement fields covered the responsible agency, fines per violation or per day, criminal penalties, and license-revocation provisions. These fields describe regulatory content more broadly than the analytical categories used in the paper; their mapping into those categories is explained below.

A.2.4. *Structured records and uncertainty.* A common output schema organized the extracted information into comparable policy-version records. The model was instructed to retain missing values when dates or provisions could not be verified rather than infer an answer. The schema also requested a confidence score from 0 to 100 for each policy version, with lower scores for incomplete documentation, ambiguous dates, missing substantive fields, or unresolved conflicts across sources. These scores are model-reported assessments rather than calibrated measures of accuracy. The returned records were subsequently processed into a city–regulation dataset for constructing the analytical outcomes. The next subsection describes comparisons with the manually collected records.

A.3. **Comparison Across Collection Approaches.** Comparisons with the manually collected records indicate broad agreement on the presence of municipal STR regulation, alongside differences in adoption dates and amendment coverage. In an initial comparison covering the 323 manually collected cities, the two collections agreed on regulatory status for 281 cities (87%): both identified regulation in 208 cities and no regulation in 73 cities. A smaller development test of a revised collection without

hints yielded agreement for 33 of 43 cities (77%). These exercises concern different development samples and extraction versions and should not be interpreted as successive estimates of accuracy for the final dataset.

Agreement across these approaches provides evidence of consistency, but its interpretation has limits. The hinted collection draws directly on the manual records, and even the unhinted collection may rely on the same municipal documents consulted by the research assistants. Shared omissions or ambiguities can therefore persist across approaches. We accordingly view these comparisons as supporting the broad correspondence between collection approaches, rather than establishing the accuracy of every recorded date or policy provision.

A.4. **Policy categories and adoption dates.** We construct four indicators summarizing the adoption of STR regulation and its principal provisions: any regulation, regulation beyond licensing, quantity restrictions, and platform obligations. Of the 1,058 cities covered by the collection, 40 lack identifiable regulatory adoption dates or data links and are excluded, leaving 1,018 cities in the dated analysis panel. Individual regression samples may be smaller because of additional data requirements and outcome-specific risk sets.

A.4.1. *Policy categories.* Table A.1 describes the coding criteria for the four indicators. The categories are not mutually exclusive: an ordinance may introduce operating restrictions and platform obligations simultaneously, while later amendments may add provisions absent from the initial enactment. We therefore identify the first qualifying adoption separately for each category. The beyond-licensing indicator distinguishes records classified as licensing or registration alone from other retained records; it does not directly measure whether a licensing requirement exists.

A.4.2. *Adoption dates and incomplete information.* We prioritize passage dates and use effective dates when passage dates are unavailable. Where the effective-date field is missing, the construction also permits the recorded enforcement-start date as a fallback. Dates reported only as a year and month are assigned to the first day of that month; dates reported only as a year in the date fields are assigned to January 1. When these fields do not yield a usable year, the construction uses specified corrections and year information recovered from ordinance titles or version identifiers. Records with a recovered year but no usable date are assigned to June 30 of that year. These conventions permit panel construction but do not imply that the underlying documents establish the imputed day or month.

TABLE A.1. Definitions of Regulatory Categories

Category	Coding criteria
Any STR regulation	A retained record explicitly identifies STR regulation.
Beyond licensing	A retained record is not classified as imposing licensing or registration requirements alone. The construction includes records whose license-only flag is false or missing.
Quantity restrictions	A record explicitly restricts a covered rental arrangement in some or all zones, or records a numerical annual cap on unhosted rental nights. Covered arrangements include owner-occupied and non-owner-occupied rentals, hosted and unhosted rentals, tenant hosting, and accessory dwelling units.
Platform obligations	A record explicitly authorizes fines on booking platforms or requires platforms to verify registrations or block noncompliant listings. Data-sharing requirements alone do not trigger this indicator.

Missing classifications are handled according to the rule for each category. Quantity restrictions and platform obligations require an affirmative trigger in the relevant extracted fields; an unclassified field does not by itself trigger adoption. The beyond-licensing category differs because its construction includes missing license-only flags. A zero in an analytical indicator therefore denotes the absence of a qualifying recorded adoption under these rules, rather than independently verified absence of every potentially applicable restriction.

A.4.3. *Panel construction and adoption patterns.* For each city and category, the adoption date is the earliest qualifying date among the retained records. The indicator equals zero before that date and one thereafter. These indicators are absorbing: subsequent amendments or repeals do not reverse an earlier adoption. They measure whether a city has adopted a category of regulation, rather than the complete regulatory regime in force at each date. In the adoption-hazard analyses, each category has its own risk set, retaining observations through first adoption and excluding subsequent periods.

Table A.2 summarizes adoption patterns in the full 1,018-city panel, including observations after adoption. The annual average share with any recorded STR regulation rises from 7.0% in 2009 to 54.4% in 2023. Over the same period, the share classified as having regulation beyond licensing rises from 4.7% to 39.0%, and the

TABLE A.2. Recorded STR Regulatory Adoption by Year

Year	Any regulation	Beyond licensing	Quantity restrictions	Platform obligations
2009	7.0	4.7	2.8	0.0
2010	7.5	4.8	2.9	0.0
2011	8.3	5.3	3.2	0.0
2012	9.5	6.0	3.8	0.0
2013	10.3	6.5	4.0	0.0
2014	11.1	7.0	4.3	0.0
2015	12.9	8.8	5.6	0.4
2016	15.1	10.3	6.5	0.5
2017	18.7	12.6	8.5	0.8
2018	25.5	17.8	13.1	1.5
2019	31.0	22.5	17.1	2.7
2020	36.4	26.9	21.5	3.5
2021	42.1	30.3	24.3	4.6
2022	47.9	34.3	27.7	5.4
2023	54.4	39.0	31.1	6.4

Notes: Entries report the percentage of city–semester observations with a recorded adoption in each category. The sample contains the same 1,018 cities throughout, excluding 35 cities without identifiable regulatory adoption dates and five without the required data links. Annual means use June and December observations, except for 2009, which contains December only. Categories are defined in Table A.1 and are not mutually exclusive. Indicators remain equal to one after first adoption. Unlike the adoption-hazard regressions, this table includes post-adoption observations.

share with quantity restrictions rises from 2.8% to 31.1%. Platform obligations remain less common, reaching 6.4% in 2023. These figures describe the cumulative adoption of the respective categories, rather than annual adoption probabilities.

APPENDIX B. VARIABLE CONSTRUCTION FOR ANALYSIS

This appendix describes the data sources and geographic crosswalks used to construct measures of hotel concentration, regulatory exposure, and accommodation-market outcomes. The construction links three distinct geographic units: municipal jurisdictions, which adopt STR regulations; city labels reported by the data providers; and hotel submarkets defined by Smith Travel Research, which serve as our measures of local accommodation markets. These units need not coincide: a municipality may overlap several hotel submarkets, and a submarket may span several municipalities.

B.1. **Data Sources.** Our analysis combines hotel inventories and performance measures, STR listing data, municipal regulatory records, and Census data. These sources

differ in their geographic units and temporal coverage; we describe each before explaining how we link them.

B.1.1. *Hotel establishments, inventories, and performance.* We use three datasets from Smith Travel Research. The first is an establishment-level census of U.S. hotels, observed in December 2017 and October 2024. Each census records hotels' street addresses, coordinates, city labels, and assignments to Smith Travel Research markets and submarkets. Submarkets are nested within markets and are used by industry participants to benchmark hotel performance. We use these submarkets as the geographic units of hotel competition and refer to them as hotel markets in the main text.

The establishment census also reports hotel names, opening dates, brand and parent-company affiliations, chain scales/classes, room counts, operating arrangements, management and ownership companies, and amenities. Operating arrangements distinguish franchised, chain-managed, and independent hotels. We standardize establishment-level city labels using the October 2024 census; the December 2017 census provides the geographic distribution used to allocate hotel inventories across submarkets.

The second dataset is a semiannual inventory of hotel properties and rooms by chain and city label. It covers June and December of each year from December 2009 through June 2024, except December 2023. For each chain–city observation, the inventory reports the chain's parent company and its numbers of properties and rooms. Unlike the establishment census, this inventory does not provide hotel coordinates, so its city labels must be linked to municipalities and submarkets using the procedures described below.

The third dataset contains monthly hotel-performance aggregates by market and submarket from January 2010 through December 2025. It reports occupancy, average daily rates, revenue per available room, available room-nights, booked room-nights, and room revenue. Occupancy and average daily rates are based on reporting hotels rather than a census of all hotels in a submarket. The data include property and room counts for both the reporting sample and the full submarket. The estimation samples use the time windows specified in the main text.

B.1.2. *Municipal STR regulations.* Our regulatory database combines manually collected information for 323 municipalities with GenAI-assisted research covering 1,058 municipalities. For each regulatory action, we record passage and effective dates when

available and classify the restrictions imposed, including registration and licensing requirements, limits on rental nights, zoning and host-type restrictions, and platform obligations. Initial enactments and subsequent amendments are recorded separately. Appendix A describes the collection and verification procedures.

The relevant geographic unit is the municipality exercising regulatory authority. This jurisdiction may differ from the area associated with the same city name in the hotel or STR data. We retain this distinction when constructing geographic links and measures of regulatory exposure.

B.1.3. *STR listings and performance.* AirDNA provides listing characteristics and monthly performance measures for properties advertised on Airbnb and Vrbo. Listing records include coordinates, ZIP codes, city labels, host identifiers, property types, bedroom and bathroom counts, price tiers, and dates of creation and most recent observation. AirDNA flags coordinates as exact for approximately 12% of listings. Its city labels agree with Smith Travel Research's labels in approximately 95% of ZIP codes.

Monthly listing records report revenue, average daily rates, occupancy, reservations, and available and blocked days. Bookings are inferred from listing-calendar information rather than observed directly from transaction records. Although the underlying data cover both Airbnb and Vrbo, the market-outcome analysis in Section 6 uses Airbnb listings to avoid changes in measured activity arising from the expansion of platform coverage.

B.1.4. *Geographic boundaries and municipal characteristics.* We use the U.S. Census Bureau's 2017 cartographic boundary files for incorporated places and counties. For the geographic construction, we retain places classified by the Census Bureau as cities, towns, or villages (LSAD codes 25, 43, and 47). This restriction excludes Census-designated places and other place types, including boroughs and certain consolidated jurisdictions. The files provide geographic polygons and land areas. We use these boundaries to locate hotels within municipal jurisdictions and identify municipalities that overlap the same hotel submarket. State boundaries also support the geographic restriction to the 50 states and the District of Columbia.

Municipal demographic, housing, and employment characteristics come from the American Community Survey five-year estimates for 2009–2023, indexed by the final year of each five-year period. The underlying variables include population; housing units by tenure and vacancy status; median household income and gross rent; and

total employment and employment in accommodation and food services. From these variables, we construct log population, log median household income, the renter share, the rental vacancy rate, the seasonal or second-home share, the rent-to-income ratio, and the accommodation and food services employment share.

B.2. **Geographic Matching.** We link municipal jurisdictions to Smith-reported city labels and assign hotels and STR listings to a common set of hotel submarkets. These links allow us to relate municipal regulatory decisions to market-level concentration and outcomes while accounting for differences between political and market boundaries.

B.2.1. *Linking municipalities to cities as reported by Smith.* Short-term rental regulations are recorded for municipalities, while Smith's semiannual inventory reports hotels only by a city label, without coordinates. For our first step, we therefore link each municipality in our sample to the city with the same name.

The city as reported by Smith and municipality rarely differ for hotels. In the December 2017 census, 96% of the hotels located inside a sample municipality with the same name as their Smith-reported city. The Smith-reported city picks up some hotels outside of the municipality, with roughly 18% of hotels being in neighboring areas that use the same name. Henceforth, we refer to "municipality" and "Smith-reported city" interchangeably as "city."

B.2.2. *Allocating hotel inventories across submarkets.* The semiannual inventory records hotel properties and rooms by chain and city, while our preferred measure of competition is defined at the submarket level. We allocate each city's chain-level inventory across submarkets using its geographic distribution in the December 2017 establishment census. For other inventory dates, this procedure holds fixed the within-city distribution of each chain's hotels across submarkets at its December 2017 proportions.

We assign each city a primary submarket, defined as the submarket containing the largest number of its hotel properties as of June 2016, with ties broken by room counts. This assignment links municipal regulatory decisions to concentration in the hotel market most closely associated with the city's hotel sector.

B.2.3. *Assigning STR listings to hotel submarkets.* To measure hotel and STR outcomes within a common geography, we assign each AirDNA listing to the submarket of its nearest hotel in the December 2017 establishment census. Nearest hotels are

identified using great-circle distances between listing and hotel coordinates. The median distance is 1.1 kilometers, and 99% of listings are within 20 kilometers of their nearest hotel. We exclude 85,214 listings located more than 25 kilometers from the 50 states and the District of Columbia, including listings in Puerto Rico, the U.S. Virgin Islands, and other countries.

B.3. **Construction of Analysis Variables.** Using these geographic links, we construct measures of hotel concentration, merger exposure, and accommodation-market outcomes for our analyses.

B.3.1. *Hotel concentration and merger exposure.* We measure hotel concentration using room-capacity shares because hotel-level bookings are unavailable.[47] For each submarket and inventory date, the Herfindahl–Hirschman Index (HHI) is the sum of squared percentage shares of room capacity across competitors, expressed on the 0–10,000 scale. Brand affiliations are linked to parent companies over time using merger and acquisition information and manual research on ownership. Each independent hotel is treated as a separate competitor, with room capacity imputed by dividing the total independent-hotel room inventory equally among independent properties in the submarket. Capacity-based concentration may differ from concentration measured using realized bookings when occupancy rates vary across hotels. Each city is assigned the concentration measure of its primary submarket. Merger exposure is the increase in HHI implied by combining the merging companies' capacity shares while holding market capacity fixed. We also construct an alternative measure using capacity shares within the provider-reported city rather than its primary submarket.

B.3.2. *Regulatory exposure.* For the market-outcome analysis, we link each hotel submarket to the cities represented by its hotels. The binary treatment indicator turns on in the first month in which any overlapping city adopts an STR regulation. This definition measures exposure to the first regulatory adoption within a hotel market; it does not require regulation to cover the entire market.

We also construct a continuous measure of exposure equal to the share of a submarket's hotel rooms located in regulated cities. This measure captures the extent of regulatory coverage as additional cities within a market adopt restrictions. As described in Section 6, the continuous-exposure analysis applies the hotel-room-based measure to both hotel and Airbnb outcomes.

[47]This potentially underestimates the HHI of the submarket if occupancy is higher for chain hotels.

B.3.3. *Accommodation market outcomes.* Hotel outcomes are observed at the submarket–month level in the Smith Travel Research performance data. AirDNA listing-level records are assigned to the same submarkets using the nearest-hotel procedure described above and aggregated to construct monthly STR outcomes. This common geography allows us to examine how regulation affects revenue, capacity, occupancy, and average daily rates in both segments of the accommodation market.

Appendix C. City Council Legislative Data

This appendix describes the construction of our database of local legislative deliberations concerning short-term rentals (STRs). We collect meeting and agenda metadata—structured information describing meetings and legislative items, such as dates, titles, legislative bodies, recorded actions, and links to supporting documents. We then retrieve supporting documents and meeting minutes, and identify evidence of political participation concerning hotels, platforms, STR hosts, residents and other stakeholders. The database captures public records of the policy process, including committee discussions, correspondence, petitions, and advisory-group reports. It is distinct from the regulatory-adoption database described in Appendix A: a discussion need not culminate in an ordinance, and one proposed ordinance may appear at several meetings.

C.1. **Identifying cities and collecting legislative metadata.** We begin with a roster of 1,058 cities and identify municipal legislative systems hosted on Legistar. Legistar organizes records by a jurisdiction-specific identifier, or *client*, which need not match the city's name. We construct candidate identifiers from normalized city names, city–state combinations, and variants such as "cityof" prefixes and Saint/St. substitutions. We test these identifiers against Legistar's public application programming interface (API), at `https://webapi.legistar.com/v1`. A usable client must return a successful, interpretable response from the legislative-body endpoint. Additional requests assess the availability of events, agenda items, legislative matters, and attachments, and record meeting-date bounds and location information.

We supplement this search with portal checks and corrections for aliases. For example, Ann Arbor, Philadelphia, and San Francisco use the identifiers `a2gov`, `phila`, and `sfgov`. Because cities in different states can share a name, a successful API response does not by itself establish a correct geographic match. We use portal and

meeting-location information to resolve ambiguous assignments and retain a city–state–client crosswalk.

The expanded search identifies 169 portals, of which 168 have usable APIs. The consolidated metadata contain records for 164 cities (Table C.1). Four cities marked API-usable—Council Bluffs, Fayetteville in Arkansas, Minneapolis, and Mooresville—do not appear in the consolidated metadata. API availability does not establish complete historical coverage: jurisdictions differ in when they began using Legistar, which legislative bodies they publish, and the supporting materials they retain.

For each client, we request meeting records from January 1, 2008 onward, using paginated requests, retries, and locally saved intermediate results. We retain event identifiers, dates, legislative-body names, locations, and agenda and minutes URLs. For each event, we retrieve agenda items and associated legislative matters, including their identifiers, titles, agenda and minutes notes, item ordering, recorded actions, and attachment names and URLs. Coverage includes committees and other bodies available through the portal, as well as full council meetings. Where meeting-linked agenda items are unavailable, we retrieve legislative matters directly, including their titles, introduction dates, statuses, and supporting documents. These records provide evidence of legislative business but do not establish that a hearing occurred on the introduction date.

The consolidated metadata contain 280,824 events from 157 cities and 5,731,024 agenda-item or fallback-matter records from 164 cities. Of the latter, 5,627,925 are event-linked agenda items and 103,099 are fallback matters. These totals cover all topics. Repeated appearances of a matter are retained because they document different stages of a legislative process; they should not be interpreted as distinct ordinances.

C.2. **From Legislative Records to Documented Political Activity.** We construct observations of documented political activity in three steps. First, we identify legislative records potentially concerning STRs and retrieve their supporting documents. Second, we select excerpts from the available text for contextual review. Third, we identify the underlying activities, distinguishing separate acts from repeated descriptions of the same act.

C.2.1. *Step 1: Selecting legislative records and recovering documents.* We screen legislative metadata for potential STR relevance. For agenda items, the searchable fields combine titles, agenda and minutes notes, matter names, and attachment names. For

TABLE C.1. Legistar Coverage and Identification of Political Activity

Collection stage	Count
A. Cities covered	
Cities searched	1,058
Cities with an identified Legistar portal	169
Cities with a usable data-access interface (API)	168
Cities with legislative records in the collected data	164
Cities with records selected as potentially STR-related	135
B. Legislative records collected and screened	
Meetings recorded in Legistar, covering all topics	280,824
Agenda items linked to meetings, covering all topics	5,627,925
Legislative matters retrieved without meeting links, all topics	103,099
Legislative records selected as potentially STR-related	7,036
Agenda items linked to meetings	6,662
Legislative matters retrieved without meeting links	374
Meetings linked to the selected agenda items	3,953
C. Documents supporting the selected records	
Distinct attachment files	15,604
Distinct linked meeting-minutes files	2,461
D. Review of evidence and identification of activities	
Text excerpts selected for review (candidate passages)	39,101
Excerpts for which the evidence remains unresolved	3,583
Documented political activities identified	14,247
Cities with at least one documented political activity	98

Notes: Counts cover the full collected corpus before actor and policy-relevance classification. The corpus includes recorded dates from February 12, 2008 to July 23, 2026. The units differ across panels and should not be added together. A *legislative record* describes an item of government business, such as a proposed ordinance or permit application. We collect both agenda items linked to particular meetings and legislative matters. Selection as potentially *STR-related* reflects a text screen, not confirmed substantive relevance. We then obtain *documents* that are linked to each selected legislative record. They include attachments—such as staff reports, letters, petitions, and presentations—and minutes that report meeting proceedings. The same attachment can be linked to several records, so attachment links outnumber distinct files. Available text includes text recovered through additional retrieval and optical character recognition (OCR). A *candidate passage* is a contiguous excerpt of source text selected because it may describe political participation. Review assesses the excerpt and, when necessary, additional context. A passage may describe several activities, while several passages may describe the same activity. Unresolved means that the reviewed evidence was insufficient to settle whether or how qualifying participation occurred. A *political activity*represents an identifiable action or role by a person, organization, joint set of actors, or documented group in a particular proceeding—for example, a submitted letter, hearing testimony, or advisory-group participation.

fallback legislative matters, they include names, titles, and types. The screen recognizes short-term and vacation-rental language, home-sharing terms, platform names including Airbnb, Vrbo, HomeAway, and FlipKey, and the standalone, case-sensitive abbreviations STR and STRs. It also includes illegal-hotel, unlicensed-rental, and transient-occupancy language. Hotel, lodging, room, or transient-occupancy tax terms qualify when accompanied by rental, platform, host, or other STR context.

The screen selects 7,036 candidate legislative records from 135 cities. These records potentially concern rulemaking, taxation, enforcement, or individual applications. Selection is deliberately broader than ordinance adoption and does not establish substantive relevance: unrelated rental assistance, nonresidential rentals, and incidental STR references can also satisfy the screen.

For selected records, we retrieve linked attachments and the minutes of associated meetings. Attachments include staff reports, proposed ordinances, presentations, letters, emails, petitions, and other supporting materials. The selected records contain 29,920 attachment links referring to 15,604 distinct attachment files. The same document can accompany several legislative records, so these links do not represent independent submissions.

We extract text from PDFs and supported office documents, supplementing the initial extraction through targeted document recovery, link repairs, optical character recognition (OCR), and extraction from embedded files. After recovery, text is available for 14,798 attachments (94.8 percent) and all 2,461 linked minutes files. Text availability does not establish complete readability: missing pages, handwriting, flattened tables, and OCR errors can leave relevant information unresolved.

C.2.2. *Step 2: Selecting excerpts for review.* We use the term *candidate passage* to mean a contiguous excerpt of extracted source text selected for review because it may describe political activity. A passage is not necessarily a paragraph, a complete letter, or a single person's statement. Its boundaries are initially determined by the retrieval rules described below, and it may contain several speakers or activities. Conversely, different passages may describe the same activity.

The principal retrieval rule searches for expressions suggesting participation, such as letters or written comments, testimony or speaking, petitions, task forces, consultations, organizing, and negotiation. An occurrence qualifies for retrieval when it lies within 1,800 characters of an STR-related expression, when the source title contains STR-related language, or when it appears in a linked non-minutes source no longer

than 8,000 characters. Around each qualifying expression, the procedure extracts up to 500 preceding characters and 700 following characters. Overlapping excerpts are combined when their combined length does not exceed 6,000 characters.

Supplementary rules address participation descriptions that lack an explicit activity expression. References to actors such as hotels, lodging associations, chambers of commerce, host associations, and neighborhood or tenant associations generate excerpts when they occur within 1,400 characters of STR-related language. Letter openings and closings, such as "I am writing" or "Sincerely," provide additional retrieval cues. When a non-minutes document has a title suggesting comments, correspondence, petitions, testimony, or an advisory group but produces no initial excerpt, we select its first 2,000 characters for review.

We also retrieve messages from attachments containing email bundles using sender and date headers. Each such excerpt extends from a message header to the next detected header, subject to a 6,000-character limit. This rule helps capture individual messages whose text does not repeat the keywords used in the principal search. Recovered text and targeted source inspection supplement the review material where necessary.

These rules determine what text is brought to review. AI-reviewers consult additional source context when an excerpt is insufficient to identify the activity, speaker, or proceeding. In particular, participation described in meeting minutes must be linked to the relevant agenda item rather than attributed to every STR-related item at that meeting.

C.2.3. *Step 3: Establishing political activity.* By reviewing the candidate passages, we construct a database of *documented political activities*, defined as an identifiable action or role by an actor, joint set of actors, or documented group of participants in a particular STR-related proceeding. Examples include a resident's written comment, an operator's testimony at a hearing, an association's submitted petition, or a stakeholder's participation in a policy working group. An observation in this database need not correspond to one council meeting, because a letter can precede a hearing, and an advisory role can extend across several meetings.

We consider six types of documented political activities: (i) written submissions, (ii) oral participation, (iii) formal petitions, (iv) advisory or working-group participation, (v) meetings, consultations or negotiations, and (vi) collective advocacy. A

written submission requires evidence that a substantive comment or supporting material entered an identified proceeding. Oral participation requires evidence of actual speaking, presentation, or substantive responses. Petitions must have been submitted, while advisory participation requires an actual appointment, membership, or contribution. Meetings, consultations, and negotiations require documented substantive discussion, including applicant-led neighborhood meetings required by a permit process, or bargaining/exchanged proposals. Collective advocacy requires evidence of organizing or coordinating political action, rather than merely expressing similar opinions.

Specifically, for each candidate passage, an AI-assisted contextual review evaluates whether the retrieved evidence documents one of the above qualifying activities, contains no such activity, or remains unresolved. Routine government processing and legislative actions, incidental actor references, and planned activities that have not occurred do not by themselves qualify. For example, a name on a mailing list does not establish participation, and a prepared statement does not establish that its author delivered it orally. Uncertain attribution, activity dates, and substantive scope are retained as limitations rather than resolved through unsupported inference. The completed first pass assigns decisions to 39,101 candidate passages, including 3,583 unresolved cases. These are counts of reviewed excerpts, not counts of political activities. The review covers the selected excerpts and additional context consulted to resolve them; it is not an exhaustive reading of every page in every source.

We then construct political activity observations from the substantive evidence, rather than assigning one observation to each retrieved passage or document. For example, if several passages reproduce or summarize the same submitted letter, they support one observation. Separate actions by the same actor can produce separate observations. For example, a written submission and a subsequent oral statement are distinct when the evidence establishes both acts. For each observation, we retain the actor's name and stated affiliation when available, a description of the activity, its date or date uncertainty, the relevant proceeding, and the supporting excerpts and source references.

This procedure identifies 14,247 political-activity observations across 98 cities before the subsequent classification of actors and STR-policy relevance. The observations measure documented activities, not unique individuals or the number of people represented by joint submissions. Repeated accounts are linked when the evidence establishes that they concern the same act, but possible duplicate descriptions and

overlap between aggregate reports and individual activities remain documented limitations.

C.3. **Restricting Activities to STR Rulemaking and Policy.** Our analysis concerns political activity over the rules governing STRs, rather than all activity involving an STR property or business. We therefore distinguish activities concerning STR rulemaking and policy from activities concerning the application or enforcement of STR rules. The first category includes participation over the adoption, amendment, repeal, or general design of STR policies, including entry restrictions, operating requirements, taxation, and enforcement frameworks. The second concerns decisions under an existing framework, such as an individual permit application, renewal, appeal, or enforcement case. For example, testimony about a citywide permit requirement concerns rulemaking, whereas testimony about whether a particular property should receive a permit generally concerns the application of existing rules.[48] We also distinguish activities that mention STRs without an established regulatory connection from those for which the available information is insufficient to determine relevance. A reference to Airbnb in a broader housing or development discussion does not by itself establish participation over STR regulation. Conversely, an activity need not repeat STR-related terminology if its description and the identified proceeding establish that connection.

We retain an activity when at least one of its linked descriptions is classified as STR rulemaking and policy, counting the activity only once and using its policy-classified descriptions for subsequent aggregation. This restriction retains 10,028 of the 14,247 identified activities, or 70.4 percent, across 95 cities. Panel A of Table C.2 describes the forms of activity in this policy sample. Each indicator equals one if the activity has the corresponding category in any of its policy-classified descriptions. Categories can overlap: for example, a submitted petition, for example, can be both a written submission and a formal petition.

C.4. **Classifying Participating Actors.** We classify participating actors into hotels, hotel associations, other business associations, tourism organizations, platforms, hosts and operators, residents, and other identified actor types. Actor type remains unknown when the recorded information does not establish a substantive category.

[48]The distinction between general policy and individual application is substantive but not always unambiguous. For example, a comment on a particular permit may also request a change in citywide rules.

TABLE C.2. Characteristics of Political Activities Concerning STR Rulemaking and Policy

Characteristic	Activities	Indicator mean
A. Activity categories (nonexclusive)		
Written submission	7,099	0.7079
Oral participation	2,495	0.2488
Formal petition	21	0.0021
Advisory or working-group participation	89	0.0089
Discussion, consultation, or negotiation	348	0.0347
Collective advocacy	53	0.0053
B. Identified actor types (nonexclusive)		
Hotels	23	0.0023
Hotel associations	12	0.0012
Other business associations	103	0.0103
Tourism organizations	27	0.0027
Platforms	57	0.0057
Hosts and operators, including their associations	694	0.0692
Residents, including neighborhood associations	931	0.0928
Other identified actor types	235	0.0234
Actor type unknown	8,162	0.8139
C. Analytical alignment (mutually exclusive)		
Hotel-aligned	4,092	0.4081
STR-aligned	4,076	0.4065
Mixed policy direction	148	0.0148
Unclear policy direction	1,712	0.1707
Total activities	10,028	
Cities represented	95	

Notes: The sample contains all identified activities with at least one linked description classified as STR rulemaking and policy, based on our Legistar data with recorded dates from February 12, 2008 to July 23, 2026. Each activity is counted once. Indicator means equal the number of activities with the characteristic divided by 10,028. In Panels A and B, an indicator equals one if the category or actor type appears in any policy-classified description of the activity. These indicators can overlap and their means need not sum to one. "Actor type unknown" means that no substantive actor category is identified in any policy-classified description; it does not necessarily mean that the participant's name is unavailable. Panel C reports the mutually exclusive assignment, which combines identified industry affiliation with reviewed policy direction for other actors.

Hotels include identified hotel businesses and their representatives; hotel associations are recorded separately. Hosts and operators include STR owners, managers, and host or operator associations. Other business associations include chambers of commerce and restaurant, merchant, realtor, and similar associations, excluding hotel

and STR-host associations. Tourism organizations include visitor bureaus and destination or tourism bodies. Residents include identified residents, neighbors, and neighborhood or homeowner associations. The residual "other" category covers identified roles outside these groups, such as independent legal or consulting representatives.

Classification prioritizes recorded names and stated affiliations, supplemented by the activity description and contextual notes. Text-pattern rules and targeted review distinguish evidence of an actor's role from an incidental reference to an industry or organization. Mentioning Airbnb does not establish that a speaker represents the platform, and supporting STR restrictions does not establish a hotel affiliation. Likewise, an employment affiliation qualified as participation in a personal capacity is not automatically treated as organizational representation. When an activity lacks sufficient actor information, other recorded descriptions can supply supporting evidence. The implemented procedure prioritizes descriptions of the same activity and also permits an exact-name match within the same city and year when the available actor classifications agree and no recorded qualification precludes the assignment.

Panel B of Table C.2 shows that actor type is unknown for 81.4 percent of the policy-related activities. This does not necessarily mean that the participant is unnamed: a named letter writer may have no recorded affiliation or role. Conversely, an unnamed participant may have an identifiable role, such as an STR host. Activities involving identified hotels or hotel associations are rare: their union contains 33 activities. These are counts of documented representation, not evidence that hotels were uninvolved in other activities or that restriction-oriented residents acted on their behalf.

C.5. **Classifying Policy Alignment.** We assign each activity to one of four categories: hotel-aligned, STR-aligned, mixed, or unclear. The assignment combines identified industry affiliation with the policy direction expressed by other participants. It is designed to measure the balance of advocacy associated with the two sides of STR regulation.

We first use identified industry affiliation. Activities involving hotels or hotel associations are assigned to the hotel-aligned side, while activities involving platforms, STR hosts, operators, or their associations are assigned to the STR-aligned side. Hotel representation takes precedence when both industry groups are identified in the same activity. These assignments reflect the actor's industry affiliation rather than a separate judgment about the direction of every statement.

For the remaining 9,251 activities, an AI-assisted review assesses policy direction using the recorded activity description and contextual information, including legislative titles and descriptions of the relevant policy proceeding. The judgment concerns the statement attributable to the relevant actor, rather than positions expressed by other speakers in the same document. The review uses the collected evidence without additional web research into participants' affiliations or preferences.

An activity is classified as hotel-aligned when its predominant direction favors restricting STR availability or strengthening operating requirements or enforcement. It is classified as STR-aligned when its predominant direction favors STR availability, continued operation, or reduced restrictions. Conditional proposals can receive either label when their overall direction is discernible. For example, support for allowing STRs subject to specified safeguards need not imply a mixed position if the principal request is to permit their operation. An activity is classified as mixed when it advances substantive requests in opposing directions without a discernible predominant direction. It is classified as unclear when the available evidence does not establish a direction. Generic procedural statements, or support for an ordinance whose substantive content is unidentified, are insufficient to determine alignment. An unclear classification therefore does not imply neutrality.

Panel C of Table C.2 reports the resulting assignments. Of the 10,028 activities concerning STR rulemaking and policy, 4,092 are hotel-aligned and 4,076 are STR-aligned. Another 148 have mixed direction and 1,712 have unclear direction. The latter two groups are excluded from both sides' regression counts.

C.6. **Constructing the City–Semester Panel.** We aggregate documented political activities to a city–semester panel from the second half of 2009 through the second half of 2023. Each activity is counted once, regardless of the number of supporting passages, documents, or linked legislative records. Outcomes therefore measure unique documented activities, rather than document appearances. We assign dates using the recorded activity date when it can be parsed. When that date is unavailable or unparseable, we use the linked meeting date where available. If an activity has multiple linked descriptions, we take the earliest date obtained after applying this rule to each description. We then assign the activity to the corresponding calendar semester.[49]

[49]The 44 activities without a usable activity or meeting date cannot be assigned to the panel.

For each advocacy side (hotels vs. STRs), we construct counts of all activities. For our analysis, we construct two mutually exclusive activity types: written involvement and oral or active involvement. Active involvement includes petitions, advisory participation, discussions, consultations, negotiations, and collective advocacy.[50] Across the full policy-related activity data, this assignment yields 7,045 written activities and 2,983 oral or active activities.

Counts are constructed separately for hotel-aligned and STR-aligned activities. Activities with mixed or unclear alignment contribute to neither side's count. Within an eligible city–semester, a side with no identified activity receives a count of zero. These zeros indicate that no qualifying activity was identified in the collected records; they do not establish an absence of political participation.

C.6.1. *Documentary coverage and sample selection.* For regression analysis, recall that sample requires evidence of documentary coverage before treatment. A semester satisfies the underlying availability rule when all six months appear in the city's Legistar coverage calendar and at least one meeting during the semester has a posted minutes URL.[51] We retain cities with at least four semesters satisfying this availability rule before the second half of 2016, counting pretreatment semesters within the regression panel. For those cities, we retain only semesters that themselves satisfy the availability rule. This pretreatment-coverage sample contains 48 cities and 960 city–semester observations before estimation-specific exclusions.

An alternative approach applies the same semester-level availability rule but does not require four qualifying pretreatment semesters. This sample contains 115 cities and 1,568 city–semester observations before estimation-specific exclusions. Comparing the two samples assesses sensitivity to the requirement for an established pretreatment documentary record. Note that neither rule guarantees complete observation of participation: a posted minutes URL does not establish that all relevant correspondence or other supporting materials are available.

C.6.2. *Observed political activities and regulatory adoption.* Table C.3 compares documented political activity across cities with and without a recorded first STR-regulation adoption during 2008–2024, based on the 115 cities with the Legistar data availability. Among the 82 cities adopting during this period, 64 have identified policy-related

[50]We consolidate overlapping activity categories using the priority active or oral over written.

[51]The coverage calendar spans the first through last recorded meeting or legislative introduction date; its presence alone does not establish continuous or complete collection.

TABLE C.3. Regulatory Adoption and Documented Political Activity

	STR-regulation adoption in 2008–2024	
	Yes	No
Number of cities	82	33
Cities with any documented political activity	64	8
Mean semesters with activity per city	3.60	0.52
City–semesters with any activity	295	17
Mean activities per city–semester, conditional on any activity		
Hotel-aligned activities	11.81	1.94
STR-aligned activities	8.95	1.35
Written activities	17.44	0.65
Oral or active activities	8.68	5.06

Notes: The sample comprises the 115 cities covered by Legistar coverage with at least one posted minutes URL. Adoption groups are defined using the recorded first STR-regulation adoption date. Four cities in the "No" column first adopted regulation before 2008. *Political activity* refers to unique documented activities classified as STR rulemaking and policy and dated during 2008–2024. The mean number of semesters with activity includes all cities in each column, assigning zero to cities without identified activity during the period. Conditional activity means use only city–semesters with at least one identified activity, including those with mixed or unclear alignment, and weight each such city–semester equally.

activity, compared with eight of the other 33 cities. Adopting cities average 3.60 semesters with documented activity, compared with 0.52 semesters among the remaining cities. Conditional on observing any activity in a city–semester, adopting cities also have more activity on both sides: an average of 11.81 hotel-aligned and 8.95 STR-aligned activities, compared with 1.94 and 1.35, respectively. The difference is particularly pronounced for written involvement, which averages 17.44 activities in adopting cities versus 0.65 in the other group.

C.7. **Direct Hotel Participation and Coalition-Based Advocacy.** Identifying hotel participation from municipal records is difficult: affiliations are often unreported, and only 33 activities in our policy sample explicitly identify hotels or hotel associations. The records that do identify these actors nevertheless provide a concrete picture of their involvement, including testimony, detailed regulatory proposals, and joint petitions with community organizations. We illustrate these activities through Louisville and Chapel Hill, then turn to external evidence on hotel industry coalition building. Together, these examples explain why counting only participants explicitly identified with hotels can miss some industry-supported advocacy.

C.7.1. *Louisville, 2015–2016.* In Louisville, lodging businesses and associations participated directly in the deliberations over STR regulation in 2015. The Public Safety Committee's August 19 record identifies Patrick Gregory of the Greater Louisville Hotel and Lodging Association and Steve Porter of the Louisville Bed & Breakfast Association among those discussing the proposed ordinance. A written statement dated August 5 from Herb Warren, owner of Old Louisville Inns and a representative of the Kentucky Bed & Breakfast Association, makes the substance of this advocacy particularly clear. Warren opposed relaxing existing requirements for rentals marketed online and proposed a specific enforcement program: registration, a temporary opportunity to pay back taxes without penalties, financing enforcement from recovered revenues, and requiring Airbnb to collect and remit taxes. This example shows established lodging interests supplying officials with concrete administrative proposals, alongside arguments that STR operators should face obligations comparable to those of existing lodging businesses.[52]

C.7.2. *Chapel Hill, 2018–2021.* Chapel Hill illustrates how hotel participation can develop into a joint policy initiative with community organizations. According to the coalition's own account, local hoteliers raised STR concerns with the chamber of commerce in 2018, and a working group emerged in early 2019. Hotel owners and managers subsequently worked with the Chamber for a Greater Chapel Hill–Carrboro and the Chapel Hill Alliance for a Livable Town (CHALT). Their joint petition, transmitted on June 10 for the June 12, 2019 council meeting, reported more than 150 hours of work and combined concerns about hotel competition and unequal regulatory obligations with housing affordability and neighborhood cohesion. The group supported hosted rentals and limited short absences by primary residents, but opposed dedicated vacation rentals without a permanent resident. It also requested an advisory process and proposed registration, operating, and tax-compliance requirements. The petition thus documents both hotel participation in preparing policy proposals and an explicit coalition linking commercial and neighborhood interests.[53]

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[52]Louisville Public Safety Committee, August 19, 2015, discussion of O-131-15, `http://legistar2.granicus.com/louisville/meetings/2015/8/5136_M_Public_Safety_Committee_15-08-19_Action_Summary.pdf`; Herb Warren, written statement dated August 5, 2015, `https://louisville.legistar1.com/model/attachments/aa2e2372-5ea0-49ad-92ed-56d615e860a2.pdf`.

[53]Study Group on Short-Term Rentals, joint petition dated June 12, 2019, transmitted June 10, including the supplemental "Background Sequence" and proposed regulatory framework, `https://chapelhill.legistar1.com/chapelhill/attachments/9ec0b903-6216-4f57-9993-1d3b6977bb93.pdf`.

C.7.3. *Hotel industry's broad coalition building strategy.* Industry documents indicate that such coalition building also formed part of a broader advocacy strategy. In its 2015 annual report, the American Hotel & Lodging Association (AHLA) described building and educating a coalition encompassing bed-and-breakfast organizations, labor, affordable-housing advocates, neighborhood groups, and the wider business community. The report situated this effort within its campaign over STR regulation and described accompanying media efforts emphasizing illegal hotels and consumer safety. These activities extend beyond hotel representatives' appearances at public hearings: they involve bringing different constituencies into a common policy campaign and developing arguments that connect industry concerns with housing and neighborhood issues.[54]

Reporting by *The New York Times* provides additional evidence on the resources and mobilization behind this strategy. Drawing on internal association documents, its April 2017 account described AHLA-funded research, alliances with affordable-housing groups, neighborhood associations, and hotel labor unions, and plans for a testimonial campaign featuring people adversely affected by home sharing.[55]

Such efforts provide a mechanism through which industry resources can support advocacy publicly expressed by residents or community organizations. Resident identity and industry-supported mobilization can therefore coexist: a participant can have independent neighborhood concerns while contributing to a campaign supported by hotel interests. Municipal records may identify that participant as a resident without describing the campaign's funding or organizational support. This evidence motivates examining the broader balance of hotel-aligned and STR-aligned advocacy, without establishing that particular resident activities in our data were hotel-supported.

Appendix D. The Marriott–Starwood Merger: Timeline and Pricing Control

D.1. **Timeline of the integration.** Table D.1 documents events related to the Marriott–Starwood merger and how the two firms integrated operations. This table draws on various public filings, press releases, earnings-call transcripts, franchise disclosure agreements, and other related sources.

[54]American Hotel & Lodging Association, *2015 Annual Report: Achieving New Heights*, pp. 7–8, `https://ahla.com/sites/default/files/Annual_2015.pdf`.
[55]*The New York Times*, "Inside the Hotel Industry's Plan to Combat Airbnb," April 16, 2017.

We highlight three key dates. First, the merger was announced in November 2015. Second, the merger was closed in September 2016. Third, pricing systems were integrated between September and December 2018.

D.2. **Pricing control under Marriott's franchise agreements.** As most hotels under the Marriott-Starwood parent brand are franchised or managed for third-party owners, it is important to document to what degree the parent brand has control over hotels' pricing and operations.

Marriott's franchise agreement for the Sheraton chain (originally under Starwood), in the form dated March 31, 2017, states that Marriott's rate recommendations are "not mandatory." However, the same agreement permits Marriott to "require that Franchisee price consistently in all distribution channels," to "require Franchisee to participate in Inventory Management" (defined as inventory-management services Marriott provides under revenue-management agreements) or to "act as Sales Agent for Franchisee" with the hotel's inventory consigned to Marriott, and to designate, modify or replace the reservation and yield-management systems the hotel must use and in which the hotel must load and honor its rates (Marriott International, Inc., 2017*b*). The accompanying disclosure states that Marriott will require Sheraton hotels to use its One Yield yield-management system. Similar statements are present in the franchise agreements for Westin and Four Points hotel chains.

For hotels that join Marriott's revenue-management program (Revenue Management Advisory Services, called Revenue Management Remote Solutions in 2017), Marriott's own revenue managers operate its reservation system, MARSHA. The program agreement reserves to the hotel "any and all decisions relating to pricing" and a right to override, while appointing Marriott the hotel's "limited agent" to take inventory-management actions "as Franchisor deems appropriate." These actions include "maintaining MARSHA inventory through restrictions, authorizations and rates" and implementing "all sales strategies in MARSHA," with such actions "deemed to have [been] approved" unless the hotel objects at a periodic consultation (Marriott International, Inc., 2017*b*, 2024). Participation is "generally optional, but may be required," for example when a hotel has no Marriott-certified revenue manager, and only certified revenue managers may access the systems and their data (Marriott International, Inc., 2024). By 2023 Marriott's disclosures add that it "limit[s] the inventory of guestroom nights franchisees may make available to customers through each distribution channel" (Marriott International, Inc., 2023).

TABLE D.1. Timeline of the Marriott–Starwood merger

Month	Event	Source
Nov 2015	Merger announced. Marriott's CEO lists among the cost advantages "the need to develop only one revenue management system for example or maintain only one revenue management system as opposed to multiple systems."	Marriott International, Inc. and Starwood Hotels & Resorts Worldwide, Inc. (2015); Marriott International, Inc. and Starwood Hotels & Resorts Worldwide, Inc. (2016)
Sep 2016	Merger closes. Starwood hotels are now owned, managed or franchised by Marriott. Each chain still runs its own reservation and yield-management systems (Starwood's Valhalla and Revenue Optimization System; Marriott's MARSHA and One Yield).	Marriott International, Inc. (2016); Marriott International, Inc. (2017*b*)
Early 2017	U.S. legacy Starwood hotels are part of Marriott's online-travel-agency contract terms.	Marriott International, Inc. (2017*a*); Marriott International, Inc. (2018*b*)
Apr 2017	Over 100 company-operated U.S. Starwood hotels moved to Marriott's shared-service model for finance and accounting; Starwood-owned and managed hotels in North America moved to Marriott's procurement system.	Marriott International, Inc. (2017*a*); Leber and Kreiman (2017)
Mar–May 2017	Marriott's first franchise disclosure documents for the Starwood brands state that it is "in the process of transitioning" them to MARSHA and "will require" them to use One Yield, with aims to finish in "late 2018."	Marriott International, Inc. (2017*b*); Marriott International, Inc. (2017*a*)
Early 2018	North American sales organizations of the two chains combined.	Marriott International, Inc. (2018*a*); Marriott International, Inc. (2019*c*)
Jun 2018	Three former Starwood revenue managers (New York, Seattle, Houston) take Marriott revenue-management titles in the same month, according to their LinkedIn profiles.	LinkedIn (2026)
Aug 2018	Loyalty programs merged, allowing members "to earn, book, and redeem across more than 6,700 hotels."	Marriott International, Inc. (2018*d*)
Sep–Dec 2018	"In five waves from September to December," Marriott moves "11 brands encompassing roughly 1,500 managed and franchised Starwood hotels onto Marriott's platforms, including systems for reservations, revenue management, and sales & catering." The two chains' hotels now run on one reservation system and one revenue-management system.	Marriott International, Inc. (2018*c*); Marriott International, Inc. (2018*d*); Bouharb (2019); Marriott International, Inc. (2019*b*); Marriott International, Inc. (2019*a*)

Notes: "Early 2017" and "early 2018" are Marriott's characterizations. Marriott's revenue-management advisory service separately reports supporting 1,642 hotels in 2018 "including 227 SPG [Starwood Preferred Guest] properties." The five waves of integrating Starwood properties are dated in a Marriott IT director's account of the sales-and-catering system conversion: September 16 (Four Points), October 7 (Four Points, Tribute Portfolio), October 27 (Sheraton), November 24 (Westin, Design Hotels, Le Méridien, W) and December 17, 2018 (Luxury Collection, St. Regis, Aloft, Element).

Reporting on 2018, Marriott said it had "reduced the amount of discounting at Legacy-Starwood hotels"; by May 2019, with the legacy Starwood hotels on "one reservation system" under a "unified revenue management strategy," it had "brought the Starwood Hotels in sort of one fell swoop onto a little bit more aggressive approach" to yielding inventory away from online travel agencies (Marriott International, Inc., 2019*b*,*a*).

On the whole, we take these communications as evidence that the hotel's formal discretion over its rates is exercised within a system Marriott controls: Marriott supplies the pricing system, the data, the channel rules and, for program hotels, the staff who set the rates.

Appendix E. Using All National Mergers

We extend the analysis beyond the Marriott–Starwood merger by using variation in hotel concentration generated by national hotel mergers recorded in our data. As in the baseline analysis, each outcome uses city–semester observations through the first adoption of the corresponding regulation, retaining the adoption semester. Because hotel concentration may respond to local shocks that also affect regulatory adoption, we instrument lagged HHI with simulated concentration changes induced by national mergers.

Let $H_{c,t-1}$ denote lagged HHI in city c's assigned hotel submarket. The instrument, Z_{ct}, sums positive simulated merger-induced HHI changes over semesters $t-11$ through $t-2$, using pre-merger room inventories to isolate changes attributable to ownership consolidation.[56] Our preferred linear specification is estimated by two-stage least squares:

$$H_{c,t-1} = \pi Z_{ct} + \alpha_c + \eta_{s(c)t} + \mathbf{x}'_{ct}\theta + u_{ct}, \tag{E.1}$$

$$y_{ct} = \beta H_{c,t-1} + \phi_c + \nu_{s(c)t} + \mathbf{x}'_{ct}\gamma + \varepsilon_{ct}. \tag{E.2}$$

Both stages include city and state-by-semester fixed effects and the eight time-varying controls used in Table 1. This specification identifies the concentration effect from within-city variation after accounting for these controls and fixed effects. Its causal interpretation requires the merger instrument to affect regulatory adoption only through hotel concentration and to be uncorrelated with the remaining determinants of adoption in the estimation sample.

[56]The instrument sums available lags within this window; observations with no available lag are excluded. Lags are constructed before restricting the sample to cities at risk of adoption.

As a complementary specification, we estimate a control-function probit. We first regress lagged HHI on the instrument and controls, using state-by-year and semester fixed effects. We then include the estimated first-stage residual, $\widehat{u}_{ct}^{P}$, in the adoption equation:

$$\Pr(y_{ct} = 1 \mid H_{c,t-1}, \mathbf{x}_{ct}, \widehat{u}_{ct}^{P}, s(c), t) = \Phi\big(\beta_P H_{c,t-1} + \rho \widehat{u}_{ct}^{P} + \alpha_{s(c),\text{year}(t)}^{P} + \lambda_t^{P} + \mathbf{x}_{ct}'\gamma_P\big). \tag{E.3}$$

This specification omits city fixed effects and additionally relies on the control-function probit assumptions. Both control-function stages use the sample retained after removing fixed-effect groups with no outcome variation. The linear specification retains eligible observations from these groups, so the two approaches differ in sample coverage as well as functional form and fixed effects.

Table E.1 reports the results. In Panel A, a 100-point increase in lagged HHI raises the estimated semester-level adoption probability by 0.34 percentage points for any regulation, 0.44 points for quantity restrictions, and 0.20 points for platform liability. All three estimates are positive but statistically insignificant at conventional levels. Panel B also yields positive estimates across outcomes, with platform liability significant at the 10% level. Because the probit coefficients are index effects, their magnitudes are not directly comparable to the probability effects in Panel A.

Overall, the all-mergers estimates are consistent with concentration increasing regulatory adoption, but they are less precise than the baseline results. Note that they do not estimate the same parameter as Table 1: the IV specifications estimate a slope on lagged concentration, whereas the baseline estimates the effect of binary exposure to the Marriott–Starwood merger.

Appendix F. Additional Tables and Figures

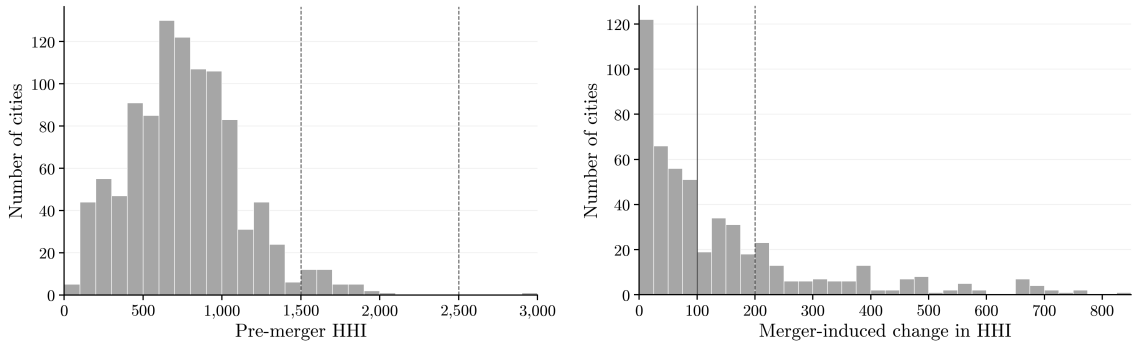
This appendix provides additional descriptive evidence and robustness analyses for the relationship between hotel concentration and STR regulation. We document the distributions of pre-merger concentration and merger-induced changes in HHI, assess covariate balance and estimates under alternative weighting schemes, and examine sensitivity to the definition of merger exposure, the pretreatment window, and differential trends across cities. These exercises assess whether the baseline findings depend on particular sample comparisons or specification choices.

TABLE E.1. Hotel Concentration and STR Regulatory Adoption: IV Estimates

	Any Regulation	Quantity Restrictions	Platform Obligations
Panel A: Linear IV hazard model			
100-point increase in lagged HHI	0.339	0.438	0.198
	(0.384)	(0.286)	(0.131)
Merger instrument (first stage)	0.715***	0.705***	0.678***
	(0.036)	(0.033)	(0.030)
First-stage Wald F	404.4	468.9	518.5
City fixed effects	Yes	Yes	Yes
State × semester fixed effects	Yes	Yes	Yes
Observations	19,519	22,266	24,911
Cities	902	946	975
Adoptions	469	276	58
Panel B: Control-function Probit			
100-point increase in lagged HHI	0.0188	0.0210	0.0841*
	(0.0166)	(0.0198)	(0.0490)
Merger instrument (first stage)	1.386***	1.452***	1.424***
	(0.062)	(0.072)	(0.133)
First-stage Wald F	501.9	535.4	152.8
City fixed effects	No	No	No
State × year fixed effects	Yes	Yes	Yes
Semester fixed effects	Yes	Yes	Yes
Observations	10,154	8,106	2,896
Cities	891	909	525
Adoptions	468	276	58
Eight baseline controls, both panels	Yes	Yes	Yes

Notes: Each outcome retains the semester of first adoption and excludes subsequent observations. HHI is measured on the 0–10,000 scale. The instrument sums available positive simulated merger-induced HHI changes over lags 2–11 semesters; observations with no available lag are excluded. Both stages include the eight baseline controls listed in Table 1. Panel A reports two-stage least-squares estimates, expressed as percentage-point changes in adoption probability for a 100-point increase in lagged HHI. Panel B reports control-function probit coefficients for the same HHI increase; these are index effects, not probability effects. Standard errors in Panel A are clustered by city. Panel B reports standard errors from 399 city-level bootstrap draws that re-estimate both stages; stars use normal-reference Wald tests. The first-stage F statistic in each panel is the city-clustered Wald statistic for the excluded instrument, calculated from the original-sample first-stage regression. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

FIGURE F.1. Marriott–Starwood Merger and Hotel Concentration



(A) Hotel Concentration Before the Merger (B) Merger-Induced Change in HHI

Notes: The left panel plots the distribution of the Herfindahl–Hirschman Index (HHI) immediately preceding the Marriott–Starwood merger (June 2016) across the 1,018 cities for which we have STR regulation data. According to the merger guidelines in effect at the time, 96% of these cities are unconcentrated (HHI $< 1,500$), 4% are moderately concentrated ($1,500 \leq$ HHI $< 2,500$), and one city is highly concentrated (HHI$\geq 2,500$). Smaller cities tend to exhibit substantially higher levels of concentration. The right panel plots the distribution of merger-induced changes in HHI for the 51% of cities that experience an increase in concentration. These changes are computed holding the supply of rooms fixed at June 2016 levels and reassigning Marriott and Starwood rooms to common ownership. Larger markets are more likely to experience increases in concentration. The median increase in HHI is 85 points, while the mean increase is 145 points.

We also provide further evidence on geographic alignment and political activity. Maps and summary statistics illustrate how municipal boundaries overlap hotel submarkets, while additional specifications examine whether the heterogeneity by alignment persists after accounting for differences in city population and hotel inventory. We then report estimates of the balance of hotel-aligned and STR-aligned advocacy using a broader sample of available municipal records. Finally, we present estimates of the effects of STR regulation on accommodation-market outcomes without the seasonal adjustment used in the main analysis.

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TABLE F.1. Pre-treatment Covariate Balance

Pre-treatment characteristic	Treated mean	Control mean	Raw std. diff.	ATT-weighted control mean	Weighted std. diff.
Log population	11.271	10.859	0.314	11.303	-0.024
Log median income	11.040	10.818	0.647	11.037	0.009
Rental vacancy rate	0.060	0.076	-0.279	0.059	0.008
Log hotel market size	4.179	4.442	-0.660	4.217	-0.095
Renter share	0.434	0.444	-0.075	0.435	-0.004
Second-home share	0.022	0.038	-0.209	0.022	-0.009
Accommodation/food employment share	0.083	0.096	-0.335	0.084	-0.030
Rent-to-income ratio (%)	22.246	22.936	-0.137	22.546	-0.060

Notes: Statistics use one observation per city and the same common-support sample of 807 cities (209 treated and 598 controls) in every column. Treatment indicates merger-induced HHI change of at least 100 points on the 0–10,000 scale. Propensity scores are estimated by logit on the eight observed characteristics in the table and the three centered squared terms of log population, median income, and hotel market size, using 972 covariate-complete cities. Common support is the intersection of the treated and control propensity-score ranges. Annual characteristics use their 2015 values; log hotel market size is the mean log hotel-property count across the June and December 2015 snapshots. ATT weights are 1 for treated cities and $p/(1-p)$ for controls, without capping. For both raw and weighted standardized differences, the denominator is the unweighted common-support pooled standard deviation, $s_P = \sqrt{(s_T^2 + s_C^2)/2}$; numerators are respectively $\bar{X}_T - \bar{X}_C$ and $\bar{X}_T - \bar{X}_{C,w}$.

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TABLE F.2. Propensity-score Robustness: Merger Exposure and STR Regulation Adoption

Dependent variable:	Any Regulation	Beyond Licensing	Quantity Restrictions	Platform Obligations
Common-support DD	0.0218***	0.0217***	0.0187***	0.0064***
	(0.0077)	(0.0062)	(0.0056)	(0.0024)
Propensity-score weighted DD (ATT)	0.0285***	0.0230***	0.0193***	0.0056
	(0.0093)	(0.0078)	(0.0072)	(0.0037)
Propensity-score overlap-weighted DD	0.0224***	0.0196***	0.0168***	0.0047*
	(0.0083)	(0.0069)	(0.0063)	(0.0027)
Observations	18,464	19,979	20,710	22,970
Cities	756	771	782	805
Treated cities	194	196	199	208
Control cities	562	575	583	597

Notes: Entries report coefficients on treated × post; city-clustered standard errors are in parentheses. Treatment indicates merger-induced HHI change of at least 100 points on the 0–10,000 scale. All regressions include city fixed effects, state-by-semester fixed effects, and the eight baseline controls in Table 1. The squared terms enter the propensity model, not the DD regression controls. Propensity scores use the 2015 characteristics and centered squared log population, log income, and log hotel market size. All three estimators use that model's common-support cities. Common-support DD is unweighted; ATT weights are 1 for treated cities and $p/(1-p)$ for controls; overlap weights are $1-p$ for treated cities and p for controls. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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TABLE F.3. Political Characteristics and the Effect of the Marriott–Starwood Merger on STR Regulation Adoption

Dependent variable:	Any Regulation (1)	Beyond Licensing (2)	Quantity Restrictions (3)	Platform Obligations (4)
Panel A: Presidential Democratic vote share, 2016				
Same-sample baseline	0.0338***	0.0279***	0.0228***	0.0098***
	(0.0100)	(0.0078)	(0.0070)	(0.0037)
Share × semester dummies	0.0291***	0.0262***	0.0210***	0.0082**
	(0.0102)	(0.0079)	(0.0072)	(0.0037)
$\mathbf{1}\{\text{Share} > 0.5\}\times$ semester dum.	0.0316***	0.0278***	0.0232***	0.0094**
	(0.0103)	(0.0080)	(0.0072)	(0.0038)
Observations	12,167	13,016	13,442	14,695
Cities	499	504	508	517
Panel B: Council Democratic share, 2009–2015				
Same-sample baseline	0.0381**	0.0290**	0.0168*	0.0168***
	(0.0157)	(0.0112)	(0.0095)	(0.0058)
Share × semester dummies	0.0377**	0.0269**	0.0160*	0.0158***
	(0.0157)	(0.0110)	(0.0096)	(0.0056)
$\mathbf{1}\{\text{Share} > 0.5\}\times$ semester dum.	0.0382**	0.0280**	0.0163*	0.0165***
	(0.0158)	(0.0111)	(0.0096)	(0.0057)
Observations	5,112	5,623	5,913	6,522
Cities	222	224	226	231
Time-varying city attributes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
State-semester FE	Yes	Yes	Yes	Yes

Notes: Each coefficient reports the effect of $(\Delta\text{HHI} \geq 100) \times \text{Post}$ from a separate unweighted difference-in-differences regression. ΔHHI is the merger-induced change in concentration on the 0–10,000 scale; Post begins in the second semester of 2016. Each panel restricts the Table 1 specification to cities with the indicated political measure. Within each column and panel, all specifications use identical city–semester observations. The adoption semester is retained; observations with the corresponding regulation already in place in the preceding semester are excluded. Share denotes the November 2016 Democratic presidential vote share in Panel A and the mean Democratic share among council members classified as Democratic or Republican over usable 2009–2015 service years in Panel B. Relative to the same-sample baseline, subsequent rows add the indicated political-share or binary-share interactions with semester dummies. The binary measure equals one for shares strictly above 0.5. All specifications retain the eight time-varying controls in Table 1, city fixed effects, and state-semester fixed effects. Standard errors clustered by city are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

TABLE F.4. City Characteristics by Geographic Alignment

	Low alignment	High alignment	All
City characteristics			
Alignment index	0.102	0.431	0.267
Population	58,051	189,030	123,467
Median household income (USD)	57,891.8	55,701.8	56,799.2
Renter share	0.432	0.465	0.449
Rental vacancy rate	0.076	0.077	0.076
Second-home share	0.050	0.036	0.043
Rent-to-income ratio	22.782	22.497	22.639
Accommodation/food employment share	0.099	0.097	0.098
Hotel inventory and market coverage			
Hotel properties inside city boundaries	12.6	41.7	27.2
Hotel properties in assigned market	147.3	81.8	114.6
Number of markets containing city hotels	1.04	1.37	1.21
Cities with hotels in only one STR tract (%)	96.9	82.1	89.5
Geographic context			
City boundary area (km^2)	63.0	169.5	116.2
Other cities sharing markets (weighted)	17.3	5.6	11.5
Other city boundaries within 1 km	2.5	4.3	3.4
Other city boundaries within 10 km	8.3	11.7	10.0
Cities	449	448	897

Notes: Entries are available-observation city means for the 897 cities with observed alignment in the June 2017 panel. Low alignment includes ties at the median (0.183); high alignment is strictly above the median. Population and socioeconomic characteristics are measured in the June 2017 snapshot. Median household income, rental vacancy, and accommodation/food employment each have 896 observations; rent-to-income has 895; all other rows have 897. A market is a "tract" as defined by the Smith Travel Research. Hotel locations and markets come from the 2017 hotel data from the Smith Travel Research.

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TABLE F.5. Geographic Alignment and STR Regulation Adoption: High versus Low Alignment

Dependent variable:	Any Regulation (1)	Beyond Licensing (2)	Quantity Restrictions (3)	Platform Obligations (4)
$(\Delta\text{HHI} \geq 100) \times \text{Post}$	0.0031	0.0164*	0.0165*	0.0019
	(0.0115)	(0.0096)	(0.0085)	(0.0031)
$(\Delta\text{HHI} \geq 100) \times \text{Post} \times \text{HighAlignment}$	0.0277*	0.0044	0.0025	0.0089*
	(0.0153)	(0.0124)	(0.0109)	(0.0050)
$\text{Post} \times \text{HighAlignment}$	0.0060	0.0065	0.0034	0.0010
	(0.0059)	(0.0044)	(0.0039)	(0.0013)
Implied treatment effect: high alignment	0.0307***	0.0208**	0.0190***	0.0108***
	(0.0105)	(0.0082)	(0.0073)	(0.0038)
Time-varying city attributes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
State-semester FE	Yes	Yes	Yes	Yes
Observations	20,400	22,094	23,084	25,525
Cities	833	852	869	893
R-squared	0.155	0.130	0.124	0.098

Notes: This table reports unweighted DD estimates allowing the effect of merger exposure to differ between high- and low-alignment cities. Treatment indicates a merger-induced HHI increase of at least 100 points on the 0–10,000 scale. Post equals one from December 2016 onward. The dependent variable indicates adoption of the specified STR regulation. Observations with that regulation already in place in the preceding semester are excluded; the adoption semester is retained. HighAlignment equals one when the city's 2017 hotel-tract alignment index is strictly above the city-level median of 0.183. Exact ties are assigned to the low-alignment group. The initial sample contains 448 high-alignment and 449 low-alignment cities. All regressions include city fixed effects, state-by-semester fixed effects, and eight time-varying city controls used in Table 1. Sample sizes vary across outcomes because of their respective risk sets, missing controls, and singleton removal. Standard errors are clustered by city and reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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TABLE F.6. Geographic Alignment and Regulation Adoption: Population Adjustments

Dependent variable:	Any Regulation	Beyond Licensing	Quantity Restrictions	Platform Obligations
A. No population-heterogeneity				
Treatment × Post	0.0124	0.0156**	0.0164***	0.0033
	(0.0076)	(0.0062)	(0.0057)	(0.0022)
Treatment × Post × Alignment	0.0199***	0.0064	0.0025	0.0089***
	(0.0072)	(0.0057)	(0.0045)	(0.0030)
B. With heterogeneity in population tertiles				
Treatment × Post	0.0097	0.0048	0.0078	-0.0033
	(0.0130)	(0.0099)	(0.0092)	(0.0027)
Treatment × Post × Alignment	0.0214***	0.0067	0.0024	0.0071**
	(0.0076)	(0.0061)	(0.0050)	(0.0030)
C. With heterogeneity in population quintiles				
Treatment × Post	-0.0026	0.0025	0.0061	-0.0040*
	(0.0145)	(0.0118)	(0.0109)	(0.0023)
Treatment × Post × Alignment	0.0211***	0.0087	0.0038	0.0060*
	(0.0081)	(0.0065)	(0.0055)	(0.0031)
Observations	20,400	22,094	23,084	25,525
Cities	833	852	869	893

Notes: Each panel reports a separate heterogeneous difference-in-differences specification for each outcome. Treatment indicates a merger-induced HHI increase of at least 100; Post begins in December 2016. The outcome indicates regulatory adoption among city-semesters with no regulation in the preceding semester; the adoption semester is retained. Alignment is the 2017 hotel-tract index, standardized across 897 unique cities (raw mean 0.267; sample standard deviation 0.234). Coefficients are in probability units; the alignment interaction is per one standard deviation. All models include Post × Alignment, eight time-varying controls used in Table 1, city fixed effects, and state-semester fixed effects. Panel A's specification includes no additional population interactions. Panels B and C add Treatment × Post and Post interactions with indicators for 2015 population tertiles and quintiles, respectively, omitting the middle group (tertile 2 and quintile 3, respectively). Thus, the base Treatment × Post coefficient is evaluated at mean alignment and, in Panels B and C, for the middle population group; it is not an overall average treatment effect. Sample counts apply to all three panels. City-clustered standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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TABLE F.7. Geographic Alignment and Regulation Adoption: Hotel Inventory Adjustments

Dependent variable:	Any Regulation	Beyond Licensing	Quantity Restrictions	Platform Obligations
A. No inventory-heterogeneity				
Treatment × Post	0.0124	0.0156**	0.0164***	0.0033
	(0.0076)	(0.0062)	(0.0057)	(0.0022)
Treatment × Post × Alignment	0.0199***	0.0064	0.0025	0.0089***
	(0.0072)	(0.0057)	(0.0045)	(0.0030)
B. With heterogeneity in hotel inventory tertiles				
Treatment × Post	0.0271*	0.0285**	0.0305**	-0.0076**
	(0.0149)	(0.0128)	(0.0122)	(0.0033)
Treatment × Post × Alignment	0.0325***	0.0199**	0.0106	0.0052
	(0.0117)	(0.0094)	(0.0081)	(0.0047)
C. With heterogeneity in hotel inventory quintiles				
Treatment × Post	0.0275	0.0244	0.0175	-0.0029
	(0.0217)	(0.0168)	(0.0159)	(0.0051)
Treatment × Post × Alignment	0.0225	0.0109	0.0049	-0.0052
	(0.0178)	(0.0124)	(0.0121)	(0.0074)
Observations	20,400	22,094	23,084	25,525
Cities	833	852	869	893

Notes: Each panel reports a separate difference-in-differences specification for each outcome. Treatment indicates a merger-induced HHI increase of at least 100; Post begins in December 2016. The outcome indicates regulatory adoption among city-semesters with no regulation in the preceding semester; the adoption semester is retained. Alignment is the 2017 hotel-tract index, standardized across 897 unique cities (raw mean 0.267; sample standard deviation 0.234). Coefficients are in probability units; the alignment interaction is per one standard deviation. Hotel inventory is the number of hotels physically inside city boundaries in the 2017 hotel census. Panels B and C add Treatment × Post and Post interactions with inventory-group indicators, omitting the middle group. Tertiles contain 1–13, 14–23, and 24–652 hotels; quintiles contain 1–10, 11–15, 16–20, 21–32, and 33–652 hotels. Cutoffs use unique cities and preserve ties. The base Treatment × Post coefficient is evaluated at mean alignment and, in Panels B and C, for the middle inventory group. Inventory-group coefficients are included in estimation but not displayed. All models include Post × Alignment, eight time-varying controls used in Table 1, city fixed effects, and state-semester fixed effects. Sample counts apply to all panels. City-clustered standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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TABLE F.8. Balance of STR Advocacy: Available-Records Sample

	Any involvement	Written	Oral/Active
(ΔHHI $\geq$100) $\times$ Post	-1.384	-0.648	-1.340
	(1.019)	(1.303)	(0.942)
Hotel-aligned $\times$ (ΔHHI $\geq$100) $\times$ Post (β)	1.175**	-0.039	0.945
	(0.516)	(0.633)	(0.582)
Relative proportional effect ($e^{\beta}-1$)	2.238**	-0.039	1.572
	[0.15, 8.09]	[-0.73, 2.45]	[-0.20, 7.25]
City $\times$ side fixed effects	Yes	Yes	Yes
Semester $\times$ side fixed effects	Yes	Yes	Yes
Observations (city–semester–side)	1,516	1,127	1,370
Distinct city–semesters	898	640	817
Cities	62	44	56
Treated cities	29	22	24
Pseudo-R^2	0.559	0.641	0.312

Notes: Each column reports Poisson pseudo-maximum-likelihood estimates of equation (2), where the outcome is the count of documented political activities from Legistar, with separate observations for hotel-aligned and STR-aligned advocacy in each city–semester. ΔHHI is the merger-induced change in hotel concentration on the 0–10,000 scale. Post equals one beginning in the second semester of 2016. The STR-aligned side is the reference category. The sample requires all six months of a semester to appear in the recorded Legistar coverage calendar and at least one posted minutes link during that semester. Unlike Table 3, no minimum pretreatment-coverage requirement is imposed. The initial sample contains 1,568 city–semesters in 115 cities over the period of study; reported sample sizes reflect estimator exclusions. Standard errors clustered by city are in parentheses and 95% confidence intervals are in brackets. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

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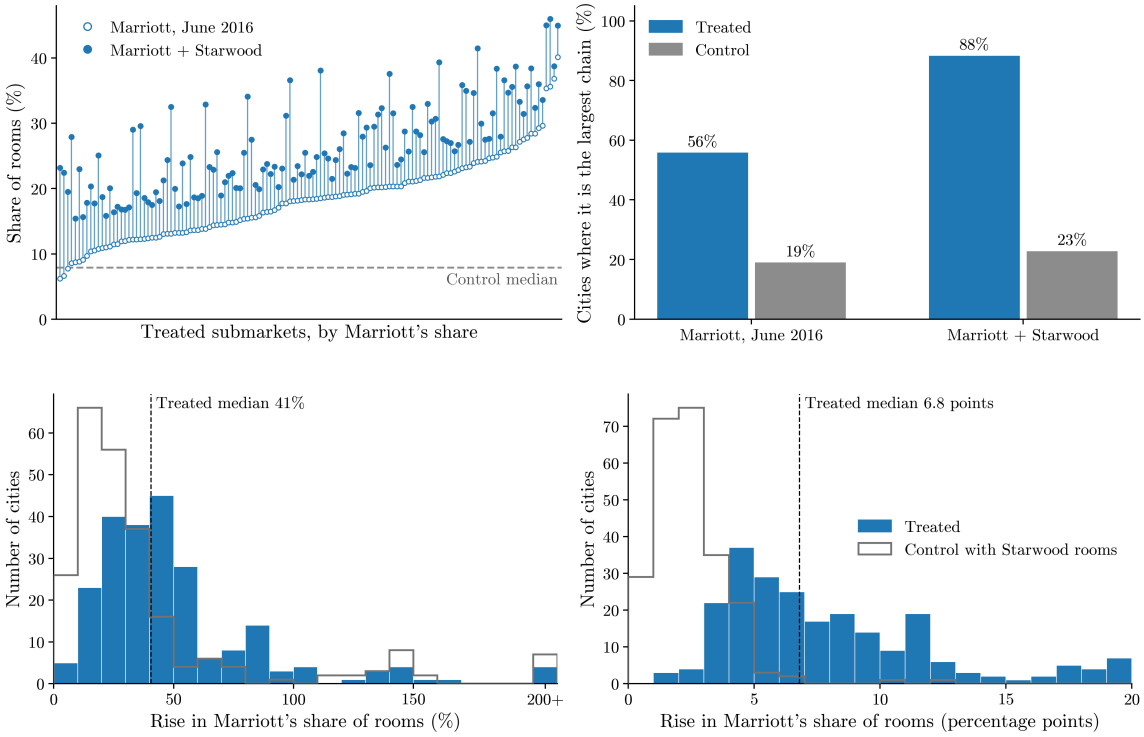
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TABLE F.9. Accommodation-Market Outcomes: Summary Statistics

	Observations	Mean	Median	Std. dev.
Panel A. Hotels				
Revenue (2019$)	38,086	19,415,352	12,448,902	30,844,564
ADR (2019$)	38,086	111.19	99.42	38.59
Occupancy rate (%)	38,086	64.36	64.47	11.55
Capacity (room-nights)	38,086	236,225	198,617	223,873
Demand (room-nights sold)	38,086	156,388	124,378	182,382
Panel B. Airbnb				
Revenue (2019$)	24,527	1,339,934	297,935	3,819,180
ADR (2019$)	24,527	134.64	118.70	61.80
Occupancy rate (%)	24,527	25.69	24.80	12.40
Capacity (listing-nights)	24,527	23,560	10,399	40,732
Demand (listing-nights sold)	24,527	7,467	2,520	15,927

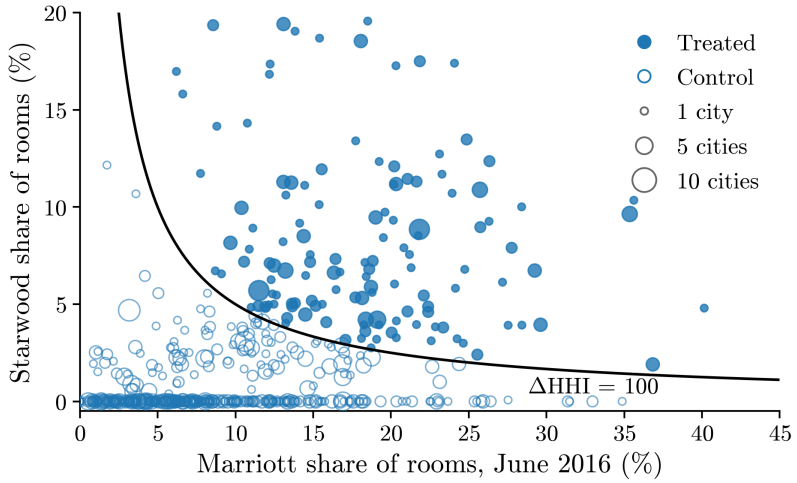
Notes: Statistics describe the sample used for Table 4: 332 hotel submarkets and 403 Airbnb submarkets, with cross-listed properties counted once per property-month. Outcomes are in unadjusted levels, using positive-outcome observations before adoption, through month 48 after adoption, and in control submarkets. Hotel data span June 2010–January 2020 and Airbnb data January 2015–January 2020; the earliest included adoptions are June 2013 and January 2016, respectively. Revenue and ADR are in 2019 dollars. Capacity is available room-nights for hotels and reserved plus available listing-nights for Airbnb. Occupancy is nights sold divided by capacity, expressed as a percentage.

FIGURE F.2. Local Expansion of Marriott's Hotel Portfolio Following the Merger



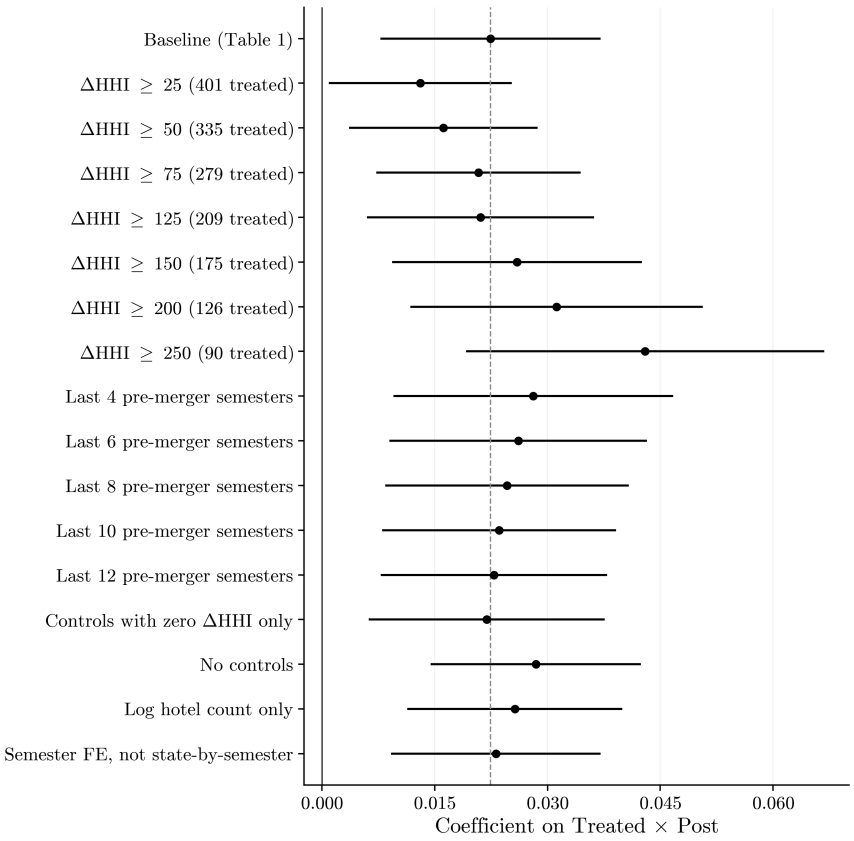
Notes: The figure combines Marriott's and Starwood's June 2016 room inventories, holding total market capacity fixed. Shares refer to rooms affiliated with each parent company. The upper-left panel compares Marriott's pre-merger share with the combined share in treated submarkets, ordered by Marriott's initial share; the dashed line marks the median of the combined Marriott and Starwood's initial share for control cities. The upper-right panel reports the fraction of treated and control cities whose assigned hotel market has Marriott as its largest chain before and after combining the portfolios. The lower panels show the proportional increase in Marriott's share (left) and the percentage-point increase (right), counting cities rather than distinct submarkets; controls in these panels are restricted to cities with Starwood rooms in their assigned markets. The respective treated-city medians are 41 percent and 6.8 percentage points. The percentage-point increase equals Starwood's pre-merger share, and the proportional increase is that share divided by Marriott's pre-merger share. Treated cities have a merger-induced HHI increase of at least 100 points. These changes reflect chain consolidation rather than new room construction.

FIGURE F.3. Pre-merger Marriott and Starwood Presence and Merger Exposure



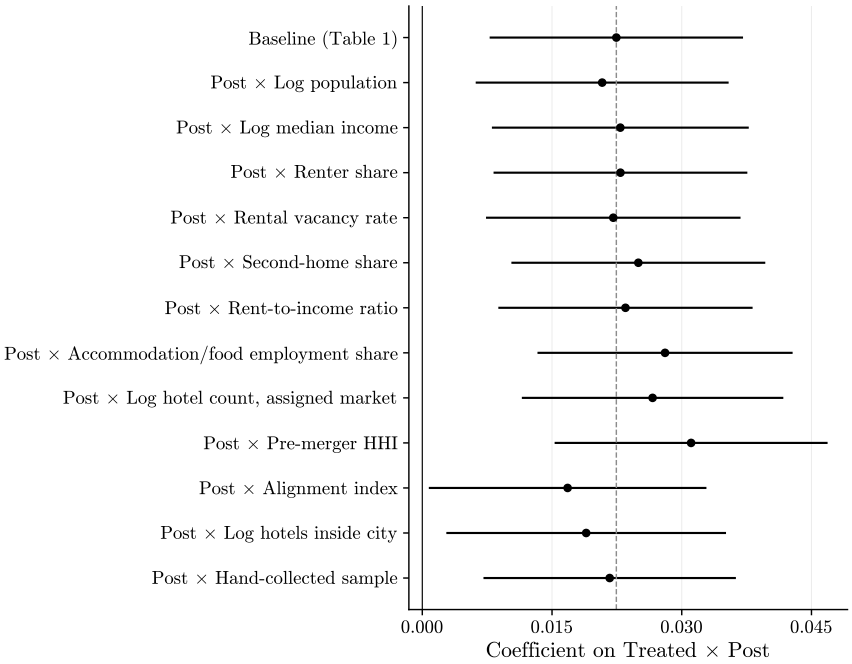
Notes: Each point represents a hotel submarket, with marker size indicating the number of sample cities assigned to it. The axes report Marriott's and Starwood's shares of rooms in June 2016, in percent. Filled markers denote treated markets and open markers controls. Holding room inventories fixed, the merger-induced HHI increase is $2s_M s_S$, where s_M and s_S are the two companies' percentage shares. The curve marks the 100-point treatment threshold. Marriott has a broad pre-merger presence, while many control markets have no Starwood rooms; differences in Starwood presence therefore account for much of the separation between treated and control markets.

FIGURE F.4. Sensitivity to Merger Exposure and Pre-period Window



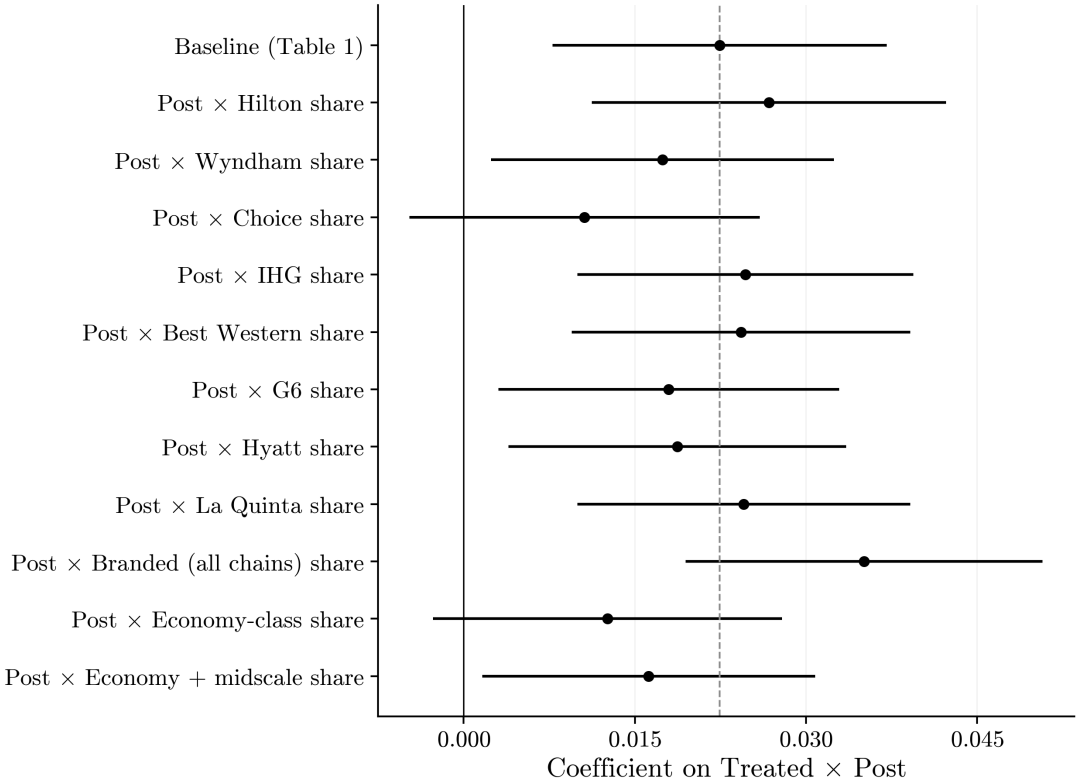
Notes: This figure examines the sensitivity of the baseline hazard-rate estimates to alternative definitions of merger exposure and pre-period windows. The dependent variable is an indicator equal to one if a previously unregulated city adopts any STR regulation in semester t. Treatment is defined by whether the Marriott–Starwood merger-induced increase in hotel-market HHI exceeds the threshold shown on the horizontal axis. The figure plots treatment-effect estimates from the baseline specification across alternative exposure thresholds and alternative numbers of pre-treatment semesters. Points denote estimates and vertical bars denote 95% confidence intervals with standard errors clustered at the city level. All specifications include city fixed effects, state-by-semester fixed effects, and time-varying city controls unless otherwise indicated. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE F.5. Sensitivity to Differential Time Trends by Covariates



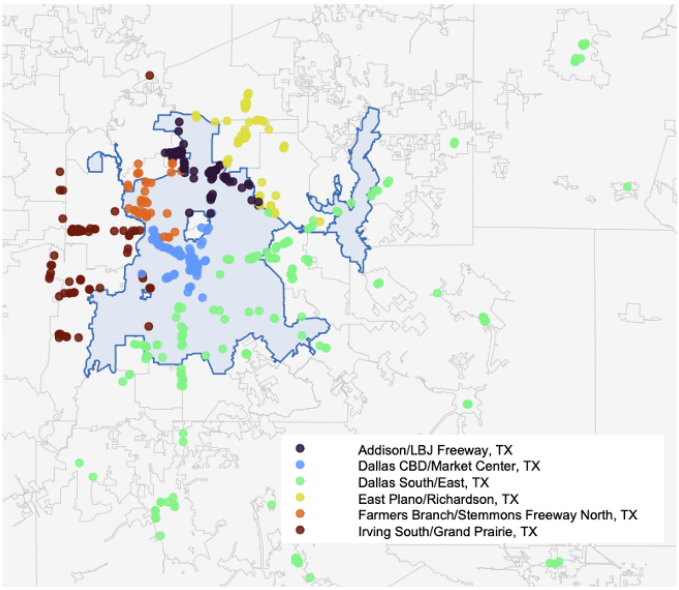
Notes: This figure examines whether the baseline merger difference-in-differences estimate is sensitive to differential post-merger trends by observable city and market characteristics. The dependent variable is an indicator equal to one if a previously unregulated city adopts any STR regulation in semester t. Each row reports the coefficient on $\text{Treated}_c \times \text{Post}_t$ from a specification that adds an interaction between the post-merger indicator and the covariate listed on the left. The dashed vertical line indicates the baseline estimate without these additional covariate-specific post trends. Horizontal bars denote 95% confidence intervals with standard errors clustered at the city level. All specifications include city fixed effects, state-by-semester fixed effects, and the baseline set of time-varying city controls.

FIGURE F.6. Sensitivity to Differential Trends by Hotel-Chain Presence and Quality Segment



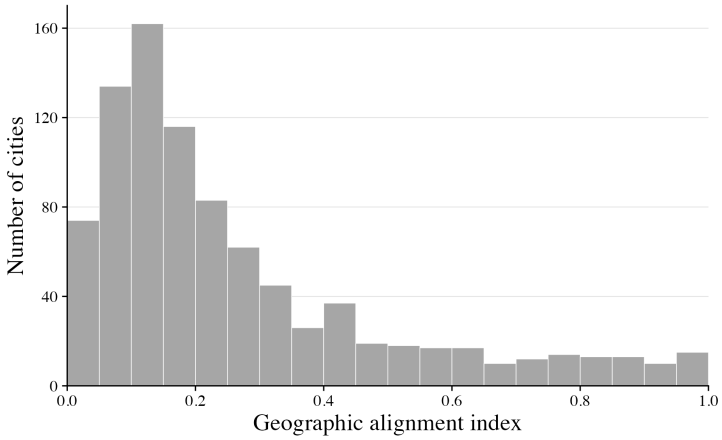
Notes: Points report the coefficient on $\text{Treated}_c \times \text{Post}_t$ for adoption of any STR regulation. The first row reproduces the baseline estimate in Table 1. Each subsequent row adds a separate interaction between the post-merger indicator and the pre-merger share of hotel rooms in the city's assigned hotel market affiliated with the indicated chain or belonging to the indicated quality segment. The branded share includes all hotel chains; the economy-plus-midscale share combines those two quality segments. The specifications retain the baseline controls, city fixed effects, and state-by-semester fixed effects. Horizontal bars show 95% confidence intervals. The dashed vertical line marks the baseline estimate, and the solid line marks zero. Coefficients are in probability units, so 0.01 represents a one-percentage-point change in the semester-level adoption probability.

FIGURE F.7. Geographic Alignment Example: Dallas, TX

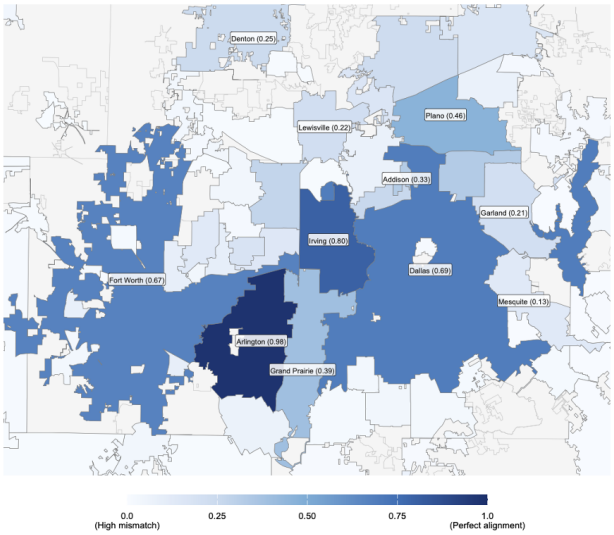


Notes: This figure shows all hotels located within hotel submarkets, as defined by the Smith Travel Research (STR) database of 2017, that contain at least one hotel in the City of Dallas in Texas. The blue shaded area denotes the Dallas city boundary. Each point represents a hotel, with colors indicating the hotel submarket to which the hotel belongs. Hotels in Dallas belong to six STR submarkets. Averaging the share of each submarket's hotels that fall within the city limits, weighted by each submarket's share of Dallas hotels, yields an alignment index of 0.69.

FIGURE F.8. Geographic Alignment Between Cities and Hotel Markets



(A) Distribution of alignment index



(B) Alignment across adjacent cities in the Dallas area

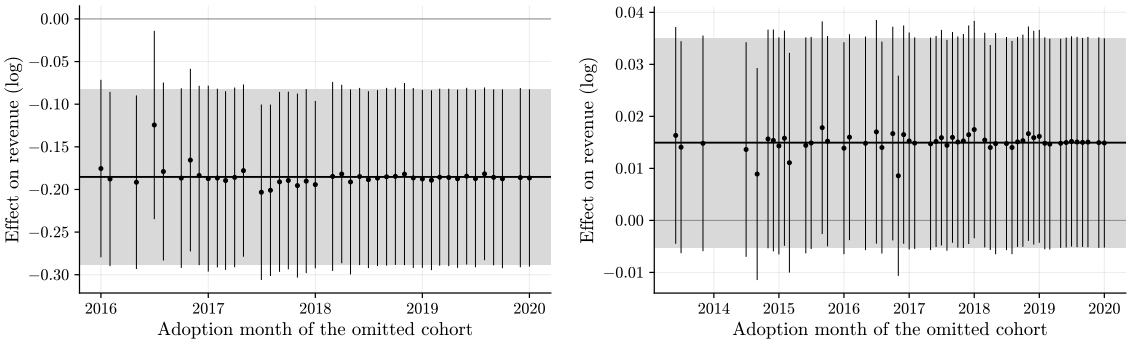
Notes: Panel (A) shows the distribution of the geographic alignment index across 897 cities. The mean is 0.267, the standard deviation is 0.234, and the median is 0.183. The index measures the average share of hotels in a city's hotel submarkets that are located within the city boundary, weighting each submarket by its share of the city's hotels. Panel (B) illustrates geographic variation in the index across adjacent cities in the Dallas–Fort Worth area. Darker shading indicates higher alignment.

TABLE F.10. Effects of STR Regulation on Market Outcomes, Without Seasonal Adjustment

	First adoption		Regulatory coverage	
	Airbnb (1)	Hotels (2)	Airbnb (3)	Hotels (4)
Revenue (log)	-0.156^{***}	0.016	-0.362^{***}	0.062^{***}
	(0.050)	(0.017)	(0.071)	(0.021)
ADR (log)	0.037^{**}	0.007	0.030	0.034^{***}
	(0.014)	(0.011)	(0.019)	(0.011)
Occupancy rate (pp)	−0.314	0.132	−0.732	0.076
	(0.569)	(0.545)	(0.581)	(0.446)
Capacity (log)	-0.124^{***}	0.009	-0.202^{***}	0.031^{*}
	(0.033)	(0.005)	(0.045)	(0.016)
Treated submarkets	164	154	164	154
Control submarkets	239	178	239	178
Submarkets	403	332	403	332
Submarket-months	24,527	38,086	24,527	38,086
Estimator	CS	CS	TWFE	TWFE

Notes: Same specifications and samples as Table 4, without seasonal adjustment. Columns (1)–(2) use Callaway–Sant'Anna (CS) estimates with six months of anticipation and never-treated and not-yet-treated comparisons, averaging effects over months 0–48 after adoption. Columns (3)–(4) use two-way fixed effects (TWFE) on the same sample, treating pre-adoption months as untreated. Coverage is measured using fixed baseline listing shares for Airbnb and room shares for hotels. Outcomes are not seasonally adjusted. Revenue, ADR, and capacity are in natural logs; occupancy effects are in percentage points (pp). Standard errors, in parentheses, are clustered by submarket, using a multiplier bootstrap for CS estimates. Counts refer to the revenue sample. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

FIGURE F.9. Cohort-by-Cohort Decomposition of the Effect of STR Regulation on Revenue



(A) Airbnb revenue (B) Hotel revenue

Notes: Each point is the average effect of the first STR regulation on log revenue, as in Table 4, recomputed without one monthly adoption cohort and plotted at that cohort's adoption month. Effects are averaged over months 0–48 after adoption. The group-time estimates are held fixed, and the remaining cohorts are reweighted as in the simple aggregation. Vertical bars are 95% confidence intervals from the multiplier bootstrap clustered by submarket. The solid line and shaded band are the full-sample estimate and its 95% confidence interval. Revenue is seasonally adjusted, in logs of 2019 dollars.

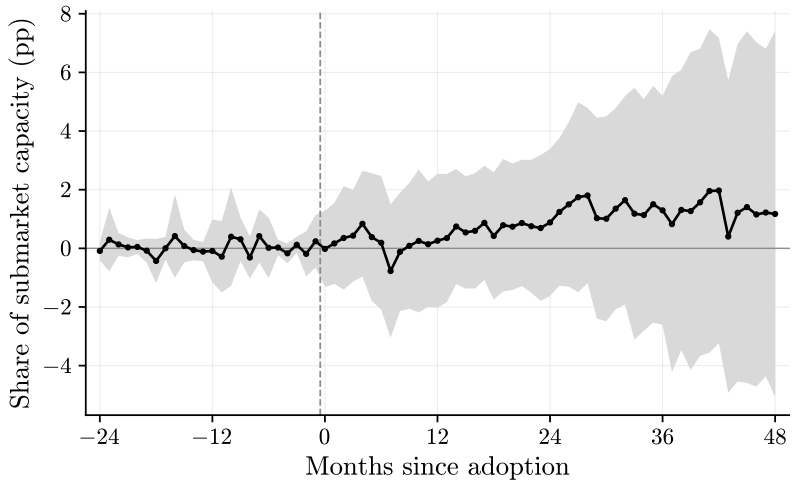
TABLE F.11. Effects of STR Regulation on Market Outcomes: Poisson Specification

	Any regulation		Regulatory coverage	
	Airbnb	Hotels	Airbnb	Hotels
	(1)	(2)	(3)	(4)
Revenue (2019$)	-0.526^{***}	0.035^{**}	-0.362^{***}	0.047^{*}
	(0.140)	(0.017)	(0.100)	(0.024)
ADR (2019$)	0.004	0.046^{***}	0.056^{**}	0.028^{**}
	(0.024)	(0.012)	(0.023)	(0.013)
Capacity (nights)	-0.320^{***}	0.003	-0.320^{***}	0.029^{*}
	(0.088)	(0.011)	(0.071)	(0.017)
Treated submarkets	163	154	181	155
Control submarkets	240	178	280	166
Submarkets	403	332	461	321
Observations	24,570	38,512	28,121	37,236
Estimator	PPML imp.	PPML imp.	PPML TWFE	PPML TWFE

Notes: This table reports complementary Poisson estimates of the outcomes in Table 4. Outcomes enter estimation in levels; revenue and average daily rates (ADR) are measured in 2019 dollars. Columns (1) and (2) use Poisson imputation following Wooldridge (2023) and Moreau-Kastler (2025). We estimate untreated outcomes using submarket and year-month fixed effects on never-treated and pre-adoption observations, excluding the six months before adoption. The reported effect is the log ratio of summed observed outcomes to summed predicted untreated outcomes over the first four years after adoption. This aggregation weights proportional responses by predicted untreated outcomes, giving greater weight to submarket–months with higher counterfactual revenue, ADR, or capacity, respectively.

Columns (3) and (4) use Poisson two-way fixed effects in multi-city submarkets, with coefficients corresponding to a change from zero to full regulatory coverage, measured by the share of hotel rooms in regulated cities. Samples differ across specifications and from Table 4. All specifications adjust for seasonality through a submarket-specific calendar-month profile estimated before the earliest included adoption cohort and included as a multiplicative offset. Estimates are reported in log points. Standard errors, in parentheses, use a submarket bootstrap with 1,000 draws in columns (1) and (2) and are clustered by submarket in columns (3) and (4). Counts refer to the revenue sample. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

FIGURE F.10. Independent Hotels' Share of Capacity Around STR Regulation



Notes: The figure plots Callaway–Sant'Anna event-study estimates for the independent-hotel share of capacity in the hotel submarket, in percentage points, using the hotel specification of Table 4: six months of anticipation, never-treated and not-yet-treated comparisons, and seasonally adjusted outcomes. Smith Travel Research reports independent hotels' capacity only for submarkets with at least four independent properties, so the sample is the submarket-months with a reported share: 152 of the 332 hotel submarkets, 81 of them treated. Event time is measured in months relative to the first regulatory adoption by a city overlapping the hotel submarket. The shaded area shows 95% simultaneous confidence bands from a multiplier bootstrap clustered by submarket. The panel displays months −24 through 48.