Broad and Narrow Price Parity Agreements: Evidence from European Hotels*

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Abstract

This paper investigates the effect of "price parity" clauses in contracts between hotels and online travel agencies (OTAs). These restrictions require a hotel to set its lowest prices for a given room on a travel agency's website and have come under recent scrutiny by antitrust regulators. We use a synthetic control strategy to study a series of policy changes in Europe using novel data on transacted hotel prices and occupancy. Our analysis finds that (i) restricting the broadest form of parity clauses, but leaving in place a more limited version, reduced prices by about 1.5% and increased occupancy by 1 percentage point; (ii) relative to narrow parity, a complete ban on parity clauses reduced prices by between 0 and 4%, but had no impact on occupancy.

Keywords: Most Favored Nation clauses, hotel pricing, price parity clauses, antitrust

JEL Classification: D40, K21, L11, L42, L81

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1 Introduction

Most favored nation clauses (MFNs) in vertical contracts require that a seller provide its most favorable terms to that buyer. MFNs have been the subject of heightened scrutiny in recent years. MFNs, or similar clauses, are used extensively by online platforms—including e-commerce, e-books and online travel–and also in other markets, such as healthcare and payment systems. A sizable theoretical literature has established the potential for MFNs to raise platform fees and retail prices for consumers (Baker and Scott Morton, 2017). Correspondingly, these vertical restraints have prompted antitrust scrutiny across several jurisdictions and markets. In a recent case, for example, US regulators have challenged Amazon's use of MFNs in its relationships with suppliers (Leonard, 2023). However, empirical evidence on the effects of MFNs remains sparse.

This paper studies the effect of a type of MFN known as a "price parity" clause in the market for hotels and provides novel evidence of its effects on equilibrium prices and occupancy. In this industry, consumers can book a stay through a hotel directly or through an intermediary, such as an online travel agent (OTA). OTAs allow consumers to search for hotel availability across locations and dates, see available hotel amenities, and book rooms through the travel agent. In exchange for this service, the OTA collects a commission based on a percentage of the booking.[1] Between 20 and 30% of hotel bookings in European markets are made through OTAs (Schegg, 2020). The OTA market is highly concentrated, with three platforms—Booking.com, Expedia, and HRS—accounting for about 90% of total bookings in Europe. In this market, prices are most commonly set by the hotels themselves, rather than by the platform.

Price parity clauses require that a hotel must set its lowest price for a given room on the OTA's website. This provision explicitly prevents a hotel from offering lower rates in other channels, such as on its own website or through a competing OTA that charges lower commissions. Virtually all contracts between hotels and the three largest online travel agen-

[1]Booking.com, one of the largest OTAs, charges fees in the range of 15-20% (Lodgable, 2022).

cies contain price parity clauses, except where prohibited by recent regulatory action. Parity clauses can take different forms. "Wide" parity clauses between an OTA and a hotel prohibit the hotel from undercutting prices provided to the OTA in any other distribution channel, including through competing OTAs, the hotel's own website, and non-public indirect or offline channels (such as walk-ins, email and phone bookings, and to customers in their loyalty programs). "Narrow" parity allows the hotel to offer discounts to other OTAs and in these indirect and offline channels, but does not allow the hotel to undercut the OTA's prices on its own website.

In this paper, we study the effect on hotel prices and occupancy of two sets of recent regulatory actions in Europe that ban the use of parity clauses. The first set of interventions weakened parity clauses without imposing an immediate full ban. These include regulatory actions in Germany that began in 2014 and a subsequent EU-wide settlement in July 2015. In the EU-wide settlement, major OTAs suspended the use of wide parity agreements while leaving narrow parity in place. In Germany, the regulatory environment evolved through a sequence of overlapping policy actions between 2014 and 2016 resulted in a mixed parity regime across OTAs. The second set of rulings banned all forms of price parity clauses, including narrow parity. These policies were enacted in a subset of European countries and in a staggered way: first in France (July 2015), and later in Austria (2016), Italy (2017), and Belgium (2018).

The effects of price parity clauses on downstream prices and quantities are theoretically ambiguous—and indeed the discussion over appropriate remedies continues in several jurisdictions[2]. Price parity clauses may increase prices by dampening incentives for intermediaries to compete by lowering commission fees (Boik and Corts, 2016; Johnson, 2017; Johansen et al., 2017; Edelman and Wright, 2015).[3] Competition between OTAs (as well as new OTA entry)

[2]For instance, the European Commission's revised guidelines on vertical relationships, released in June 2022, removed competition law exemptions for wide price parity clauses (Lovells, 2022), while German courts in 2019 reversed previously instituted parity ban, ruling that narrow parity clauses were necessary to prevent free-riding (Botteman et al., 2019).

[3]Consider a hotel that lists a room on both Expedia and Booking.com. Absent price parity, if Booking.com charges a higher commission than Expedia, the hotel may respond by listing the room at a lower price on

may also be affected by both wide and narrow parity agreements. Both types of contracts make it difficult for entrants to gain initial market share by offering lower commissions, which can be an essential strategy in platform markets which are characterized by strong network externalities. Finally, parity clauses may increase prices by preventing hotels from offering lower prices to consumers that book directly (e.g. through the hotel's website, or over the phone), which may have lower costs. This channel may be particularly important if walk-in customers have different price elasticities and can lead to "excessive intermediation" with more bookings made through OTA than in an efficient equilibrium (Edelman and Wright, 2015).

On the other hand, literature has also argued that parity agreements may solve a holdup problem (Baker and Chevalier, 2012; Wang and Wright, 2020; Ezrachi, 2015) and, by improving consumer search, indirectly boost demand and lower prices. OTAs invest in tools that lower search costs for consumers by collecting prices from many different hotels in a single searchable database, which may make the hotel market more competitive. In the absence of parity agreements, hotels might offer lower rates on their own websites, where they do not pay commission fees. This could induce consumers to use the OTA's platform to conduct a search for different rooms before visiting the hotel's website to book a room—behavior dubbed "show-rooming" (Wang and Wright, 2020)—and OTAs may respond by reducing investment in the platform, a key concern among regulators debating parity bans (Luther, 2021). Thus, some form of parity agreements may protect investment incentives for the platform.

To shed light on these ambiguous empirical predictions, we estimate the effects of two sets of regulatory interventions that either partially restricted or fully eliminated parity clauses, relying on a difference-in-differences strategy implemented using synthetic control methods (Abadie and Gardeazabal, 2003). Our research design exploits variation in the timing and

Expedia relative to Booking.com. This creates an incentive for Booking.com to keep its commissions low in fear of losing consumers. When parity agreements are in place between the hotel and each intermediary, the hotel is contractually restricted from undercutting Booking.com on Expedia, and vice versa. In equilibrium, this may result in both intermediaries charging higher commissions, and the hotel charging higher downstream prices. By the same mechanism, parity agreements may suppress new OTA entry.

scope of parity regulation across European countries to measure impacts on hotel prices and occupancy rates. We first investigate the impact of these interventions on prices and find that both types of regulatory changes lead to price reductions. Policies that weakened parity clauses without imposing a full ban are associated with an average decline in hotel prices of approximately 1.5%. Policies that moved from narrow parity to a full ban on parity clauses are associated with additional price reductions ranging from 0% to 4%, depending on the country.

We present several robustness tests for these results. We show that the impacts are broadly consistent across policy changes, mitigating the concern that any specific event is driving the results, and are robust to changes in the set of fixed effects and control variables. An event-study specification shows the effects begin within a few months of the policy change and build over time, and cannot be explained by pre-policy trends. Finally, we show that our main results using synthetic controls are similar in direction and magnitude to those obtained using a standard difference-in-differences analysis.

We then explore the impact of these policies on hotel occupancy and revenue. If a weakening of price parity clauses leads to lower commission costs for hotels without changing residual demand, occupancy may increase. However, if parity bans weaken OTA investment incentives because of show-rooming concerns, consumer search costs may rise, reducing demand. Our analysis suggests that effects on occupancy are heterogeneous: interventions that weakened parity clauses without imposing a full ban are associated with modest increases in occupancy, while full parity bans have ambiguous effects that are inconsistent across countries. Analysis of hotel revenue per room, which combines price and occupancy effects, shows no significant effects of partial parity restrictions and generally negative effects following full parity bans.

The finding that full parity bans have mixed impacts on occupancy despite lowering prices is consistent with a potential reduction in OTA investment incentives arising from a hold-up problem. Additionally, we show that differences in OTA usage across countries point to

the importance of competition between a hotel's direct channels and intermediaries. Using survey data on the composition of hotel bookings by channel, we find that in countries that instituted a full ban of parity clauses, the share of OTA bookings grew much more slowly between 2013 and 2019 relative to countries that left narrow parity agreements in place. This pattern suggests that an important driver of the price effects of eliminating narrow parity may be to increase discounting through hotels' direct channels (for example, their website), rather than intensified competition between OTAs.

We make several contributions to an existing literature that studies price parity clauses in the hotel industry. First, to our knowledge, this is the first work to quantify the price effects of both partial parity restrictions and full parity bans using a single data source and research design. Prior literature has largely studied one policy change in a single country, and shows mixed results. Ennis et al. (2023) study the policy changes in France, Germany, and the EU, but focus their analysis on whether hotels vary prices across channels, rather than price levels themselves.[4] Mantovani et al. (2021) and Ma et al. (2024) study the impact of the policy change on price levels directly using the policy change in France; Mantovani et al. (2021) find a short-lived effect that fades over time, while Ma et al. (2024) find statistically significant price impacts only for offline bookings. Differences in data sources and empirical strategies across papers further complicate the comparison of results. We provide a comprehensive analysis of the price effects of moving from wide price parity to both narrow parity and no parity regimes, which is itself an important input to the policy debate. In total, our paper analyzes the impact of six different regulatory regimes: two that weakened parity clauses without imposing a full ban, and four that eliminated parity clauses entirely. This allows our analysis to speak to the effects of both of these regulatory regimes.

Second, our data allow us to study the effects of parity clauses on occupancy, which is largely unstudied in prior literature. Whether eliminating price parity agreements increases total surplus depends on transacted quantity, rather than price. Our findings suggest that

[4]Hunold et al. (2018) also study price variation across channels using the regulatory changes in Germany.

the evidence is mixed. Occupancy increases in countries that experienced partial parity restrictions, while effects following full parity bans are heterogeneous across countries, despite a decrease in prices. This effect is consistent with a shift in the demand curve faced by hotels, possibly due to increased search costs for consumers.

Finally, we differ from much of the prior work by analyzing transacted prices, rather than posted prices collected from OTA websites.[5] Crucially, our data include offline bookings, which represent over 40% of European hotel stays.[6] Offline bookings are particularly important for understanding the price effects of moving from wide to narrow parity clauses, as narrow parity clauses still restrict the rates that can be posted on the hotel's public website but not the rates that can be offered for in-person or phone bookings. Price cuts in the offline channel would not be reflected in data collected from OTAs.

The remainder of the paper is organized as follows. Section 2 discusses recent regulatory scrutiny of price parity clauses. Section 3 provides details of the data sources we use. Section 4 discusses our empirical strategy. Section 5 shows our empirical results. Section 6 presents an additional discussion of the findings together with additional evidence about the economic forces at play. Section 7 concludes.

2 Regulatory landscape

In recent years, price parity clauses in contracts between OTAs and hotels have been the subject of regulatory scrutiny by antitrust authorities around the world. Price parity was targeted in multiple lawsuits in the United States against OTAs and large hotel chains (who themselves utilize parity contracts to restrict pricing by franchised properties).[7] Federal courts ultimately ruled in 2014 that, while the agreements had the potential to be used to

[5] An exception is Ma et al. (2024), who study the policy change in France using data on transacted prices.

[6] Data from the report conducted by the competition authorities of ten European countries reports that 45% of 2016 bookings came from offline channels, 41% from OTAs, and 14% from direct online bookings (EU Competition Authorities, 2016).

[7] Relevant lawsuits include *Turik v. Expedia Inc*, *James Smith v. Orbitz Worldwide*, *Woodell v. Expedia Inc*, and others. These were ultimately combined into a class action suit involving 24 individuals across 13 states that named 22 travel brands as defendants (O'Neil, 2014).

facilitate collusion, the plaintiffs were unable to establish that the OTAs and hotel chains were engaged in a conspiracy to fix prices, leaving the agreements in place (O'Neil, 2014).

In Europe, however, complaints targeting price parity agreements by OTAs have received more favorable treatment by regulators. Investigations against the largest European OTAs, including Booking.com, Expedia, and HRS, have proceeded in several countries, resulting in substantial cross-country variation in regulations. Our data include three different regulatory regimes. Wide price parity contracts, in which hotels are restricted from undercutting the OTA price on all sales channels, were allowed in all European countries prior to 2014, and continue to be used in the United States. Narrow price parity contracts, where a hotel is restricted from pricing below the OTA on its own website but can offer lower prices to offline bookings and on competing OTAs, were implemented by Booking.com and Expedia across European markets beginning in July 2015 (Booking.com, 2015).[8] In subsequent legislation, several European countries took further action by prohibiting the use of both wide and narrow price parity clauses, including France in July 2015, Germany in January 2016, Austria in August 2016, Italy in September 2017, and Belgium in July 2018. In these countries, OTAs are prohibited from restricting the prices that hotels can set in any channel. Our empirical strategy will focus on the analysis of hotel prices in major European markets to exploit this variation in policy governing the use of parity clauses. We summarize the evolution of the regulatory environment in each sample country in Figure 1 and Table 1, and include details of the rulings in each country below.

Germany was the first European country to take policy action against parity agreements in the hotel sector. On December 20, 2013, the German Federal Cartel Office (FCO) ruled that Hotel Reservation Service (HRS), the leading OTA in Germany with more than 50% of OTA bookings, was required to remove all parity clauses (both wide and narrow) from its contracts effective March 2014 (Bundeskartellamt, 2012, 2013). Following this decision,

[8] An exception is the United Kingdom, where Booking.com, Expedia, and InterContinental Hotels Group committed to replacing wide parity with narrow parity in January 2014 following an Office of Fair Trading investigation (Hunold et al., 2018).

HRS operated without parity clauses, while Booking.com continued to apply wide parity clauses through the end of 2014. In early 2015, Booking.com also removed wide parity clauses, resulting in a period in which the two largest OTAs in Germany operated without parity restrictions (Botteman et al., 2019). Booking.com subsequently reintroduced narrow parity clauses for a limited period beginning in July 2015, before being ordered to remove all parity clauses again effective January 2016 (Bundeskartellamt, 2016).[9] As a result, the period between 2014 and 2016 in Germany encompasses a sequence of regulatory regimes, including wide parity, narrow parity, and no parity, which sometimes applied asymmetrically across platforms. Accordingly, we refer to the German regulatory environment over this period as a mixed parity regime rather than a clean transition between parity types.

The policy debate and regulatory actions in Germany prompted subsequent investigations by regulators in France, Italy and Sweden. These investigations culminated in coordinated commitments in several European countries, under which Booking.com, and in some jurisdictions Expedia, suspended wide parity agreements and moved to narrow parity clauses beginning in July 2015 (Booking.com, 2015; Konkurrensverket, 2015). The effects of this partial ban on price parity have been widely debated in the travel industry. Some hotels, including Accor, the largest hotel chain in Europe, championed the change as increasing transparency in the market and returning pricing control to hotels. Other operators, such as Intercontinental Group, continued to push for stricter rules, including a complete ban of parity agreements, which would allow hotels to control pricing on their own websites.

However, this intermediate narrow parity regime proved short lived in a number of countries. Several national authorities and legislatures, including those in Germany, France, Italy, Austria, and Belgium, subsequently moved to prohibit both wide and narrow parity clauses during our sample period, resulting in a full ban on parity restrictions. On July 9, 2015,

[9]Booking.com appealed the FCO's decision, and it was subsequently annulled in June 2019, opening the door for Booking.com to use narrow parity agreements in Germany (Botteman et al., 2019), until a 2021 ruling again disallowed narrow parity (Bundesgerichtshof, 2021). In its 2019 ruling, the FCO acknowledged that narrow parity agreements are useful to prevent customers from free-riding on the Booking.com platform. While these decisions appear after the end of our sample, they suggest that regulators continue to grapple with the optimal set of policies.

the French National Assembly, within the "Macron Law", effectively prohibited any form of price parity between OTAs and hotels effective August 8th, 2015 (Roskis and Strange, 2015). The decision was condemned by Booking.com and other intermediaries, who argued that the ruling could ignite a price war and hurt hotel margins. The Italian Parliament voted on similar legislation that amends article 1 (166) of the Annual Competition Law to ban all forms of parity contracts in August 2017 (Osborne Clarke, 2017; Marasa, 2018). Austria and Belgium passed similar laws, effective November 2016 and July 2018, respectively (Van Bael and Bellis, 2016; HOTREC, 2018).

The evolution of the regulatory environment across different countries creates useful variation to study the impact of parity clauses on the prices faced by consumers. A potential concern, however, is that the policy actions restricting or eliminating parity clauses may have coincided with other regulatory changes affecting hotel markets. These could include policies such as changes in VAT rates, subsidies to the hotel sector, or regulations targeting short-term rental (STR) platforms. Such coincident reforms would be particularly problematic if they were implemented at the same time that parity clauses were restricted, as they may confound the treatment effects that we measure.

Several features of the policy environment suggest that parity restrictions were not bundled with other policies that affected hotel markets. In two of the six policy changes we study (the action in Germany and the EU-wide settlement with Booking.com and Expedia), the changes resulted from antitrust investigations narrowly focused on the use of parity clauses by OTAs. Courts and competition authorities involved in these cases lack statutory authority to enact broader fiscal or housing policies, making it unlikely that these interventions were bundled with other measures affecting hotel markets. Moreover, while a number of European cities introduced or debated regulations targeting short-term rentals during this period, enforcement of these policies typically occurred well after the parity-related interventions we study.

In France, Austria, Italy, and Belgium, the prohibition of price parity clauses was enacted

through legislation that in some cases included other reforms. For example, the French parity ban was part of the broader "Macron Law," which also addressed labor markets, professional licensing, and transportation regulation (Vogel, 2017). Similarly, Italy's parity ban was included in the 2017 Annual Competition Law, which encompassed a range of pro-competition measures (Global Compliance News, 2017). Austria implemented its parity ban through amendments to competition and price labeling statutes.

Several markets included in our sample adopted regulations governing short-term rentals, including Amsterdam, Barcelona, Berlin, London, Madrid, Paris, and Rome. However, these regulations were largely not enforced until at least 2017 and therefore do not coincide with our treatment windows.[10] Where relevant, such STR regulations would tend to restrict accommodation supply outside the hotel sector and thus increase hotel prices, biasing our estimates of price effects toward zero.

3 Data

Our primary source of data on hotel prices comes from STR Global, a firm that collects price and occupancy data for hotel stays. They conduct a global hotel survey covering approximately 50,000 hotels in which a panel of hotel operators report performance data. We use an extract from this survey that contains daily market-level data for 13 European cities and 7 cities in the United States, broken out by hotel class (quality level) between January 1, 2012 and December 31, 2018.[11] The data includes the average daily rate (ADR) and occupancy rate for the sample of hotels that they survey, as well as the total number of hotel rooms in the market (city-class combination). Price and occupancy are reported daily while the total number of hotel rooms varies at the month level. Hotels are divided into six

[10] While the *Zweckentfremdungsverbot* policy restricting STRs in Berlin was passed in 2014, its enforcement did not begin until May 2016 (The Independent, 2016; Duso et al., 2024), which is after the end of our sample window.

[11] The sample cities are Amsterdam, Barcelona, Berlin, Brussels, Chicago, Dusseldorf, London, Los Angeles, Madrid, Miami, Milan, Munich, New York, Orlando, Paris, Prague, Rome, San Francisco, Vienna, and Washington DC.

classes based on their average daily rate: economy, midscale, upper midscale, upscale, upper upscale, and luxury.

Our baseline estimation uses data aggregated to the market-day-class level. Within a market-day-class, prices and occupancy rates are computed as the weighted averages of hotels included in the survey. If fewer than five hotels contributed prices within a segment on a given day, STR does not report a price or occupancy rate for that observation. About 10% of observations have missing price and occupancy data. To deal with this missing data, our empirical specifications include market-class fixed effects. We are not able to disaggregate prices paid by which channel consumers used to book the stay. Our data do allow us to study the movement of average prices over time, as they include stays across all channels, including through OTAs, the hotel's website, and in offline channels.

We supplement the price and occupancy data with Google search volumes data at the city-week level as a proxy for hotel demand for a city. For each city, we record the (normalized) number of worldwide searches for the search terms "<city name> hotels" and "<city name>". We demean each time series and control for a common linear trend. Therefore, the variation captured by the residual is given by both movement around a trend and by the differential time trend across different cities.

We present summary statistics of the key variables used in our analysis by market in Table 2. The most expensive markets in our sample are New York, Paris, and San Francisco. The cheapest hotels were in Prague, Berlin, and Madrid. Occupancy rates across countries ranged between 66% (Milan) to 83% (San Francisco and London).

Finally, we supplement our analysis with data from an additional survey of European hotels conducted by a trade association (Schegg, 2020). The survey asked hotels about the share of bookings they received from various sales channels including direct bookings, OTAs, and other sources, as well as other questions related to interactions between hotels and intermediaries. The survey was administered across multiple countries in 2013, 2015, 2017, and 2019.

4 Empirical Strategy

Our empirical strategy leverages variation induced by the policy changes described in Section 2. Changes in price parity rules by country give rise to a series of natural experiments that we use to measure the effect of weakening and eliminating parity agreements on hotel prices and occupancy. Our baseline specification uses a synthetic control approach, which constructs a weighted combination of untreated markets to serve as the counterfactual for each treated group. This allows us to account for heterogeneous seasonal patterns across markets and better match pre-treatment dynamics. For robustness, we also estimate standard differences-in-differences (DD) specifications that compare market outcomes in treated countries to those in non-treated markets.

A growing econometrics literature has raised methodological concerns with staggered DD designs in which multiple experiments result in units that are treated at different times. In particular, Goodman-Bacon (2021) show that the commonly used two-way fixed effects model uses earlier-treated units as a control for later-treated units. When treatment effects are heterogeneous across experiments, this approach may fail to measure the treatment effect of interest. In this context, there are a number of reasons that the treatments may be heterogeneous across markets due to differences in the competitive environment, policy implementation, or enforcement. Given this, our analysis treats each policy action as a separate experiment with its own control group, rather than including them in a single regression.

Our first set of analyses measures the effect of weakening price parity agreements without fully eliminating them. We study two cases: Germany and the coordinated European commitments implemented in mid 2015. In the European case, the settlement with Booking.com and Expedia represents a relatively clean transition from wide parity clauses to narrow parity clauses across participating countries. By contrast, the regulatory environment in Germany between 2014 and 2016 evolved through a sequence of overlapping interventions rather than a single discrete policy change. During this period, different OTAs operated under wide parity, narrow parity, and no parity at different times (see Section 2 for details). We therefore char-

acterize Germany's policy environment over this interval as a mixed parity regime. Given the close timing and overlapping nature of these interventions, we estimate a single treatment effect beginning in March 2014. The estimated effect for Germany should be interpreted as the average impact of operating under this mixed parity regime, rather than as a clean comparison between wide and narrow parity clauses.

Our second set of analyses measures the effect of moving from environments in which parity clauses were at least partially permitted to regimes in which parity clauses of all forms were explicitly prohibited. We observe these policy changes in four sample countries: France, where the Macron Law instituted a full parity ban beginning in July 2015, Austria (November 2016), Italy (September 2017), and Belgium (July 2018). For each case, we use the synthetic control method to construct a counterfactual for each treated market, selecting the donor pool from all non-treated markets, which includes European countries that had no additional policy action (Czech Republic, Netherlands, and Spain) or did not enact policy actions until later on (e.g. Belgium), as well as US hotel markets.[12] Because none of these countries transitioned directly from wide parity to a full ban in isolation, the estimated effects should be interpreted as the incremental impact of banning parity entirely, relative to the parity regime prevailing in contemporaneous control markets.[13]

A key decision in our empirical strategy concerns the construction of the control group for each of the events that we study. Our sample contains a diverse set of hotel markets across many countries, which may be subject to distinct seasonal patterns and idiosyncratic demand shocks over time. These demand fluctuations are potential confounders in our analysis, as they could result in changes in prices or occupancy rates that are unrelated to the policy

[12] As discussed in Section 2, the United Kingdom experienced a transition from wide to narrow parity in January 2014. We therefore exclude UK markets from the donor pool in our analysis of the policy changes in the EU, Germany, and France, where this change would overlap with the treatment window.

[13] France moved from wide parity to a full ban in July 2015, but this change coincided with the EU-wide settlement under which many control countries simultaneously moved from wide to narrow parity. The estimated effect for France therefore reflects the impact of a full parity ban relative to a narrow parity environment. By contrast, Austria, Italy, and Belgium implemented full bans after narrow parity clauses were already in place, so the estimated effects in those cases capture a transition from narrow parity to a complete ban.

change. To address this issue, we implement a two-step procedure. First, we residualize the outcome variables with respect to market-class, market by day-of-week, and market by month-of-year fixed effects, as well as contemporaneous and lagged measures of Google search intensity for "<city> hotels" interacted with a Europe indicator. This step removes predictable seasonal patterns and common demand shocks while preserving the within-market variation relevant for the policy analysis.[14] Second, we construct synthetic controls using these residualized outcomes, following the methodology of Abadie and Gardeazabal (2003) and Abadie et al. (2010). We estimate separate synthetic control weights for each outcome variable (prices, occupancy, and revenue), so that the pre-treatment fit is optimized for the outcome under consideration. The synthetic control is therefore chosen to match treated and untreated markets on their residualized pre-treatment trajectories, rather than on raw levels that reflect mechanical seasonality or scale differences across markets.

As in any DD specification, our identification assumption is that hotel prices in the treated groups and control groups would have followed parallel trends in the absence of the policy change. In Figures 2-3, we plot hotel prices and occupancy rates for each policy experiment (marked with a red vertical line) together with its synthetic control group. The plots show visual evidence that the synthetic controls (shown in dashed blue) appear to closely track the outcomes in the treated markets prior to the policy change. Figure 2 shows that, after implementation, prices in treatment markets relative to controls appear to fall for five out of six of the policy changes. One exception is Belgium, where prices in control markets stay fairly flat while prices in treatment markets rise modestly. Figure 3 shows analogous plots for occupancy rate. Here, the evidence is more mixed. Occupancy appears to rise in treatment jurisdictions relative to the control group in Germany, Italy, and Belgium, but the effects are somewhat noisier and less consistent across countries. As we discuss in the introduction, the effect of weakening price parity agreements on equilibrium quantities is theoretically

[14]Several recent papers note that residualizing or demeaning outcomes prior to synthetic control construction can improve pre-treatment fit in settings with highly disaggregated data or strong deterministic components. See, for example, Ferman and Pinto (2021), Ben-Michael et al. (2021), and Abadie and L'hour (2021).

ambiguous, and the weaker evidence of the policy changes on this variable is consistent with this observation.

We compare the trends in prices and occupancy rates using the synthetic control approach to analogous plots that compute the raw average from unweighted control groups, shown in Figures A1-A2. These alternative graphs show greater divergence in pre-treatment trends and more volatile post-treatment dynamics. The unweighted comparisons suggest that untreated markets are subject to different demand shocks and seasonal patterns, which may confound estimates of the treatment effect. In contrast, the synthetic controls are explicitly constructed to match the pre-policy trajectory of the treated markets, improving the credibility of the counterfactual. For this reason, we rely primarily on the synthetic control estimates in our main analysis and present the standard DD estimates as a robustness check.

An additional concern is that because our data are only available in aggregate form, we are not able to control for other factors that could affect hotel prices, such as how far in advance a stay was booked or how many nights were included in the stay. Such variation should not bias our estimates of the effect of the policy if it does not differ systematically within markets over time.

Specifically, we estimate the treatment effect of each policy change by computing:

$$(\hat{\beta}^{\mathrm{SC}}, \hat{\mu}, \hat{\alpha}_t) = \arg\min_{\beta,\mu,\alpha} \left\{ \sum_{j=1}^{N} \sum_{t=1}^{T} \left(y_{jct} - \mu - \alpha_t - \beta D_{jt}\right)^2 \hat{\omega}_j^{\mathrm{SC}} \right\} \tag{1}$$

where y_{jct} is the residualized outcome variable in city j and hotel class c on week t, D_{jt} is an indicator equal to one if the market is in a treated country after the regulation change, and $\hat{\omega}_j^{SC}$ are the outcome-specific synthetic control weights constructed in a first step. The coefficient $\hat{\beta}^{SC}$ therefore captures the average post-treatment difference between treated markets and their synthetic controls. As outcome variables, we use log daily prices, occupancy rates, and log daily revenue. We cluster standard errors at the market-class level in all specifications.

5 Results

5.1 Effects of parity agreements on hotel prices

We first consider the impact of the policy changes on hotel prices. We find clear evidence that reducing the stringency of price parity clauses lowers hotel prices. In four of the six regulatory interventions we study, prices decline following the policy change, with economically meaningful and statistically significant effects. In the remaining two cases, estimated price effects are small and statistically indistinguishable from zero. Overall, the pattern across countries indicates that weakening or eliminating parity clauses tends to reduce prices, with larger and more robust declines associated with full parity bans.

Table 3 reports average post-treatment effects from our preferred synthetic control specification. Policies that weakened parity clauses without imposing a full ban are associated with modest but precisely estimated price declines. In Germany, which experienced a mixed parity regime over this period, prices fell by approximately 1.3%, while the EU-wide settlement reduced prices by about 1.7%.

Full parity bans are associated with larger price reductions relative to narrow parity regimes. We estimate statistically significant declines of of 4% in Austria and 2.9% in Italy. Estimates for France and Belgium are smaller in magnitude and imprecisely estimated. Overall, the table confirms the pattern in Figure 2: reducing the stringency of parity clauses lowers hotel prices, with larger effects when parity is fully eliminated.

We then estimate a dynamic version of equation 1, allowing treatment effects to vary flexibly by month relative to the policy change. Figure 4 reports the resulting event study coefficients from one year before to one year after each intervention. Panel (a) pools policy changes that weakened parity clauses without imposing an immediate full ban, while Panel (b) pools full parity bans. In both panels, outcomes in the synthetic controls closely track those in treated markets prior to the policy change, providing no evidence of differential pre-trends. Following the interventions, prices in treated markets decline relative to their

synthetic controls, with effects that emerge gradually and strengthen over time.

The event study plots for both sets of policy changes show a gradual emergence of treatment effects. This may reflect an adjustment process on the part of hotels. Prices may not adjust immediately following the removal of parity clauses due to limited awareness of the policy change, or because they have an existing stock of reserved bookings made at previous prices. Even once informed, hotels may undergo an experimentation process to understand optimal pricing across distribution channels absent the constraint imposed by parity clauses. Additionally, hoteliers may adopt a wait-and-see approach, adjusting prices only after observing changes in competitor behavior or realizing shifts in demand. These adjustment lags could partially explain why the treatment effects appear to build over the first year following the policy implementation.[15]

5.2 Effects of parity agreements on occupancy and revenues

In contrast to prices, the effects of parity restrictions on occupancy are heterogeneous and less precisely estimated. *Ex ante*, given that the policy results in a decline in hotel prices, we might expect occupancy to increase if hotels' residual demand remains constant. However, other forces may partially offset this effect. Critics of parity bans argue that they lead to "show-rooming" by consumers, who may use the OTA to search, but book directly through the hotel. OTAs may respond to lower commission volumes by reducing their investments—for example, by spending less on search advertising or other marketing efforts. If this in turn increases consumer search costs, it may lower residual demand for hotels and decrease occupancy, partially offsetting the impact of a decline in hotel costs.

Table 4 reports average treatment effects on occupancy. Weakening parity without a full ban is associated with small but statistically significant increases in occupancy in both Germany and the EU-wide settlement of about 1 percentage point, shown in columns (1) and

[15]This finding is also consistent with Ma et al. (2024), who show that the impact of the policy change in France builds over the first year after implementation. Similarly, Armona et al. (2024) measure the effects of new hotel entry on demand for nearby incumbent hotels and show that the impacts build over the first 6-12 months after the entry date.

(2). By contrast, full parity bans (columns (3)-(6)) produce mixed effects across countries, with no consistent direction or magnitude. Taken together, these results suggest that occupancy responses depend on how parity clauses are relaxed and may reflect offsetting forces operating through demand and platform investment.

These set of estimates, with small and heterogeneous impacts of the six policy changes on occupancy rates, are consistent with the presence of the two offsetting forces we describe above. These patterns are also consistent with discussion among some regulators that showrooming may be of particular concern when parity clauses are fully eliminated, as in the policy changes in France, Austria, Italy, and Belgium.[16]

Figure 5 presents event study estimates for occupancy. For policies that weakened parity without a full ban (Panel (a)), occupancy rises modestly (between 1 and 2 percentage points) following the intervention. For full parity bans (Panel (b)), we detect no systematic post-treatment pattern. Pre-treatment trends are stable in both panels.

Because parity restrictions reduce prices and have mixed effects on occupancy, the net effect on revenues is theoretically ambiguous. Table 5 reports estimates using log daily revenue per room. We find no statistically significant revenue effects for policies that weakened parity without a full ban, consistent with price declines being offset by higher occupancy. In contrast, full parity bans are associated with revenue declines of 3-5% in several countries, reflecting price reductions combined with weak or negative occupancy responses.

Overall, our results show that reducing the stringency of parity clauses lowers hotel prices. Policies that weaken parity clauses without eliminating them are associated with modest price reductions accompanied by small increases in occupancy, indicating effects along both price and quantity margins. By contrast, full parity bans generate additional price reductions relative to narrow parity regimes but are associated with heterogeneous and less precisely estimated occupancy responses across countries. Taken together, these findings highlight a potential trade-off faced by policymakers between achieving further price reductions and

[16]For example, prevention of show-rooming was a key rationale in the 2019 decision by German courts to allow Booking.com to reinstate narrow parity clauses (Luther, 2021).

preserving mechanisms that may support demand or platform investment. These patterns suggest that parity clauses affect prices primarily through cost and competitive channels, while their effects on quantities depend on additional forces such as platform investment incentives and consumer search behavior. We explore these mechanisms further in Section 6.

5.3 Alternative Empirical Frameworks

Our baseline empirical strategy uses the synthetic control method to construct a comparison group for each treated market. This approach selects a combination of untreated markets that closely matches outcomes in the treated group prior to the policy change. In this section, we explore the robustness of our results to alternative specifications.

We first re-estimate treatment effects using a standard difference-in-differences framework. Rather than constructing synthetic controls, this approach uses all available untreated markets as the comparison group. For policy changes in Germany, France, Italy, Austria, and Belgium, we use other European markets as controls, while for the EU-wide settlement we use US cities. Each policy change is analyzed separately, and all specifications include a rich set of fixed effects to flexibly control for market-specific seasonality and demand patterns, as well as Google search volume controls. Table A1 summarizes the treatment and control groups used in each regression.

We show results for hotel prices in Table A2. Our findings are broadly similar both in direction and magnitude to those using synthetic controls. We find negative and statistically significant coefficients for four policy changes (Germany, France, Austria, and Italy) and negative but insignificant results in Europe and Belgium. Effect sizes range between -0.1% (Belgium) and -5.1% (France). In all six regressions, the 95% confidence intervals from the standard DD estimates overlap with the confidence intervals produced from the synthetic control specification in Table 3.

We repeat the analysis for occupancy and revenue, which we show in Tables A3 and A4. As in our main specification, our results on occupancy are mixed across policy events, with

no consistent pattern emerging across countries. Revenue effects mirror the combination of price declines and mixed occupancy responses. Overall, the DD estimates reinforce the central conclusion from our preferred specification: parity restrictions lower prices, while quantity responses are more variable and context dependent.

We also test the sensitivity of our baseline synthetic control estimates to the inclusion of different sets of controls. Our main specification includes week fixed effects, market-class fixed effects, market x day-of-week fixed effects, market x month-of-year fixed effects, and Google search volume controls. Appendix Table A5 shows alternative synthetic control estimates when we successively drop the search controls and the market x month and market x day of week fixed effects in the residualization step. Our results for prices, shown in the top panel of Table A5, are negative and significant across most specifications and similar in magnitude to our baseline estimates. The second two panels show estimates from these alternative specifications for occupancy and revenue, which are also broadly similar to those obtained using our baseline specification.

Finally, we examine the sensitivity of our results to the construction of synthetic control weights across outcomes. Our baseline specification estimates outcome-specific synthetic control weights, allowing the pre-treatment fit to be optimized separately for prices, occupancy, and revenue. Appendix Table A6 reports alternative estimates in which the synthetic control weights constructed from the price outcome are applied to occupancy and revenue. Comparing these estimates to those obtained using outcome-specific weights shows that the qualitative patterns are unchanged, and differences in magnitudes are modest.

6 Channels

This section discusses potential channels for our empirical findings. Understanding these channels is relevant for policy design, as different parity regimes restrict competition along distinct margins which may have different implications for market outcomes and welfare.

However, given the aggregate nature of the data and the limited number of policy interventions, the evidence in this section should be interpreted as suggestive, rather than a formal test of each specific mechanism.

Theoretical analysis of price parity clauses predicts that they may increase prices by limiting competition between OTAs, or decrease them through beneficial impacts related to solving a hold-up problem. Parity clauses may increase prices by reducing competitive pressure in the OTA market, since they prevent hotels from setting lower prices on platforms that charge lower commissions (Boik and Corts, 2016). Inter-OTA competition (which may also occur through new platform entry) may be restrained by both wide and narrow parity clauses along different margins. While narrow parity agreements allow hotels to vary prices across OTA channels, they prohibit the hotel from undercutting any OTA price on its website. A complete ban of parity clauses, including narrow versions, removes the restriction that a hotel's website price be at least as high as the OTA price and may spur additional competition. As a consequence, regulatory interventions that partially restrict parity clauses and those that eliminate parity clauses altogether may affect competition through distinct mechanisms.

Parity clauses may also maintain high prices by affecting competition between intermediaries and hotels' direct sales channel. Wide parity clauses prevent the hotel from offering lower prices through their offline sales channels, including walk-ins, phone and email reservations, which are an important source of bookings in Europe. Both wide and narrow parity clauses prevent the hotel from setting lower prices on its own website (where it pays no commission) than through the OTA. These commissions will be partially passed through to prices. All else equal, this mechanism would result in a higher share of hotel bookings occurring through an intermediary under price parity, even if consumers get limited value from the OTA ("excessive intermediation" in the terminology of Edelman and Wright (2015)). Both of these channels—increased OTA competition and direct channel substitution—would suggest that parity bans lead to lower prices and higher occupancy through a decline in hotel marginal costs.

On the other hand, removing parity clauses may weaken incentives for OTAs to invest in advertising and platform quality due to concerns about consumer show-rooming. If OTAs respond to parity bans by reducing investment in search engine advertising or other marketing efforts, consumer search costs may increase, potentially reducing hotel demand and offsetting the direct effect of lower prices.

Our empirical results are consistent with these competing forces. We find that policy changes that weakened parity clauses without imposing an immediate full ban are associated with lower prices and modest increases in occupancy, while full parity bans further reduce prices but have mixed and heterogeneous effects on occupancy. While both types of interventions plausibly affect prices through increased competition and direct channel substitution, our empirical design does not allow us to isolate these mechanisms cleanly. The absence of a uniform increase in occupancy following full parity bans is suggestive of additional offsetting forces, such as reduced OTA investment, but we refrain from drawing strong conclusions given the heterogeneity of the occupancy estimates across markets.

To provide suggestive evidence of some of the mechanisms that may drive our results, we examine variation in the share of hotel bookings originating from OTAs across countries. In countries that ban all parity clauses, hotels can undercut OTAs on their own websites, which would reduce the OTA share of bookings, as well as average prices. In Figure 6, we plot OTA booking shares over time for eight countries that banned wide parity agreements but left narrow parity in place (Spain, Hungary, Norway, Sweden, Czech Republic, Switzerland, Finland, and Greece), as well as three countries that banned all parity clauses (Germany, Italy, and Austria).[17] The data show that while OTA booking shares increased in all countries during this period, the increase was smaller in countries that banned all parity clauses (18.1% in 2013 to 26.1% in 2019) relative to those that allowed narrow parity (15.8% in 2013 to 29.7% in 2019).[18]

[17] OTA booking shares were collected in a survey administered every two years between 2013 and 2019 in 11 EU countries by HOTREC, an industry association (Schegg, 2020).

[18] This is echoed in the findings by Ma et al. (2024), who show that the share of OTA bookings fell by about 2% (relative to control countries) after the policy change in France.

These patterns are consistent with a role for increased discounting through hotels' direct sales channels following full parity bans and aligns with findings in Hunold et al. (2018) and Ennis et al. (2023), who show that hotels set lower prices in their direct channel relative to OTAs following the policy interventions in France and Germany. At the same time, we acknowledge several important caveats to this analysis: the booking share data is aggregated and somewhat sparse, and OTA booking shares grew modestly faster in countries that would later allow narrow parity relative to those that banned parity prior to the policy action. We therefore interpret this evidence as suggestive rather than conclusive.

Finally, we note that there was little entry into the OTA market during this period, which instead saw increasing concentration. This suggests that entry by low-cost platforms is unlikely to explain the observed price effects.

These patterns suggest that the incremental effects of tightening parity regulation may differ depending on whether restrictions target inter-platform competition, direct-channel pricing, or platform investment incentives. As a result, regulatory approaches that relax the most restrictive parity provisions without eliminating parity altogether may generate different trade-offs than full bans.

7 Conclusion

This paper provides novel empirical evidence on a question which has received some attention in the theoretical literature, as well as in policy circles: how do price parity agreements affect equilibrium prices and quantities? We leverage a series of policy changes which induced quasi-random variation in the regulatory environment across European countries over a seven year period. We find that policies that weakened parity clauses without imposing an immediate full ban led to lower prices and modest increases in occupancy. Further restrictions that eliminate parity clauses entirely generate additional price reductions relative to narrow parity regimes but had ambiguous impacts on occupancy. These findings contrast with prior studies that

found limited additional effects from eliminating narrow parity clauses.

Amid the ongoing policy debate over parity clauses, our findings have implications for policymakers and antitrust authorities charged with protecting consumer welfare in platform markets. They suggest that the stringency of parity regulation matters: prohibiting the most restrictive forms of parity can reduce prices while preserving mechanisms that may support demand, whereas further tightening may yield additional price reductions but with less clear quantity responses. At the same time, the absence of uniform occupancy gains following full parity bans highlights potential trade-offs between achieving lower prices and maintaining incentives for platform investment. While we do not conduct a formal welfare analysis, our results underscore the importance of considering how different regulatory approaches to parity clauses affect competition, pricing, and market dynamics through multiple channels. Further research quantifying the role of platform investment and consumer search frictions in this market would be valuable for assessing the broader welfare implications of alternative regulatory regimes.

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Table 1: Policy change details

Country	Effective Date	Sample Markets	Ruling	Notes
Germany	3/1/14	Berlin, Dusseldorf, Munich	HRS ordered to remove all parity clauses	Bundeskartellamt announces statement of objections against HRS in February 2012, subsequently rules that use of price parity clauses by HRS violates competition law on 12/20/13. HRS required to eliminate all forms of parity clauses effective 3/1/2014 (Bundeskartellamt, 2012, 2013).
Germany	1/9/15	Berlin, Dusseldorf, Munich	Booking.com suspends wide parity clauses	German courts reject HRS appeal to 2014 ruling, other major OTAs relax wide parity clauses (Botteman et al., 2019).
EU	7/1/15	Amsterdam, Barcelona, Berlin, Brussels, Dusseldorf, London, Madrid, Milan, Munich, Paris, Prague, Rome, Vienna	Booking.com and Expedia agree to eliminate wide parity but keep narrow parity clauses	Booking.com reaches settlement with regulators from France, Sweden, and Italy in April 2015 to eliminate wide rate parity clauses but leave in place narrow parity, effective 7/1/2015 (Botteman et al., 2019; Booking.com, 2015; Konkurrensverket, 2015). Expedia also commits to switch to narrow parity in 2015 (European Commission, 2020).
France	8/8/15	Paris	OTAs prohibited from using all parity clauses	French National Assembly passes "Macron Law" on 7/9/15, which prohibits all forms of parity clauses including narrow, effective 8/8/15 (Roskis and Strange, 2015).
Germany	1/31/16	Berlin, Dusseldorf, Munich	Booking.com and Expedia ordered to suspend all parity clauses	Bundeskartellamt rejects Booking.com and Expedia settlement to leave in place narrow parity clauses, orders them to remove all parity clauses on 12/22/15, effective 1/31/16 (Bundeskartellamt, 2016).
Austria	11/20/16	Vienna	OTAs prohibited from using all parity clauses	Austrian Parliament bans all parity clauses in the Austrian Federal Act against Unfair Competition 1984 and Austrian Price Labeling Act (Van Bael and Bellis, 2016; Chambers, 2016).
Italy	8/29/17	Milan, Rome	OTAs prohibited from using all parity clauses	Italian Parliament bans all price parity clauses in article 1(166) of Annual Competition Law (Osborne Clarke, 2017; Marasa, 2018).
Belgium	7/19/18	Brussels	OTAs prohibited from using all parity clauses	Belgian parliament passed a law that outlaws price parity clauses between OTAs and hotels, effective 7/19/18 (HOTREC, 2018; Alliance, 2023).

The table lists details of the policy changes that we study in Section 5. The second column includes the markets in our sample that were affected by each policy change.

Table 2: Price and occupancy rates by market

Country	Market	ADR (Euros)	Occupancy Rate	# Rooms
Austria	Vienna	86.5	0.74	31,828
Belgium	Brussels	106.0	0.67	16,956
Czech Republic	Prague	66.3	0.74	32,148
France	Paris	197.7	0.76	47,568
Germany	Berlin	79.0	0.76	68,748
Germany	Dusseldorf	94.0	0.68	12,029
Germany	Munich	109.9	0.76	36,038
Italy	Milan	119.7	0.66	31,916
Italy	Rome	116.8	0.70	36,854
Netherlands	Amsterdam	120.3	0.76	32,357
Spain	Barcelona	112.7	0.75	46,135
Spain	Madrid	80.5	0.70	49,137
United Kingdom	London	131.4	0.83	128,898
United States	Chicago	109.9	0.69	111,381
United States	Los Angeles	130.4	0.79	98,965
United States	Miami	155.8	0.78	51,964
United States	New York	216.2	0.86	107,510
United States	Orlando	91.1	0.74	121,257
United States	San Francisco	174.0	0.83	51,548
United States	Washington DC	119.8	0.70	108,651

The table shows summary statistics for our analysis sample by market. Our data contain daily hotel prices and occupancy rates for 20 markets (13 European markets and 7 US markets) between January 2012 and December 2018. For each city, we average across markets and hotel quality levels within a country. ADR is average daily rate and is recorded in Euros. Occupancy rate is the number of rooms sold divided by rooms available. The last column gives the average number of hotel rooms in each market.

Table 3: Effects of parity bans on prices using synthetic controls

	(1) Germany	(2) EU	(3) France	(4) Austria	(5) Italy	(6) Belgium
VARIABLES	OLS Log ADR	OLS Log ADR	OLS Log ADR	OLS Log ADR	OLS Log ADR	OLS Log ADR
Germany x After Mar 2014 (treated)	-0.013**					
	[0.005]					
Europe x After Jul 2015 (treated)		-0.016**				
		[0.007]				
France x After Aug 2015 (treated)			-0.008			
			[0.015]			
Austria x After Nov 2016 (treated)				-0.040**		
				[0.017]		
Italy x After Sep 2017 (treated)					-0.029***	
					[0.010]	
Belgium x After July 2018 (treated)						0.003
						[0.011]
Observations	17,664	9,882	8,820	9,282	11,780	7,695

The table shows regression estimates for the effects of the six regulatory changes during our sample period that partially or completely ban parity agreements. The dependent variable in each regression is the natural log of average daily price. The first two columns show the effects of policies that banned wide parity clauses but left narrow parity in place (Germany and EU), while the last four show the effects of full parity bans (France, Austria, Italy and Belgium). For each policy change, we residualize the data using month fixed effects, market-class fixed effects, market x day-of-week fixed effects, and market x month-of-year fixed effects. We then construct a synthetic control group using markets that were untreated during the sample period. We describe the regulatory regime changes in more detail in Section 2 and provide additional detail on the empirical strategy in Section 4. Standard errors are clustered at the market-class level.

Table 4: Effects of parity bans on occupancy using synthetic controls

	(1) Germany	(2) EU	(3) France	(4) Austria	(5) Italy	(6) Belgium
VARIABLES	OLS Occupancy	OLS Occupancy	OLS Occupancy	OLS Occupancy	OLS Occupancy	OLS Occupancy
Germany x After Mar 2014 (treated)	0.012**					
	[0.005]					
Europe x After Jul 2015 (treated)		0.008*				
		[0.005]				
France x After Aug 2015 (treated)			-0.020***			
			[0.005]			
Austria x After Nov 2016 (treated)				0.001		
				[0.006]		
Italy x After Sep 2017 (treated)					-0.006	
					[0.008]	
Belgium x After July 2018 (treated)						0.018**
						[0.009]
Observations	17,664	9,882	8,820	9,282	11,780	7,695

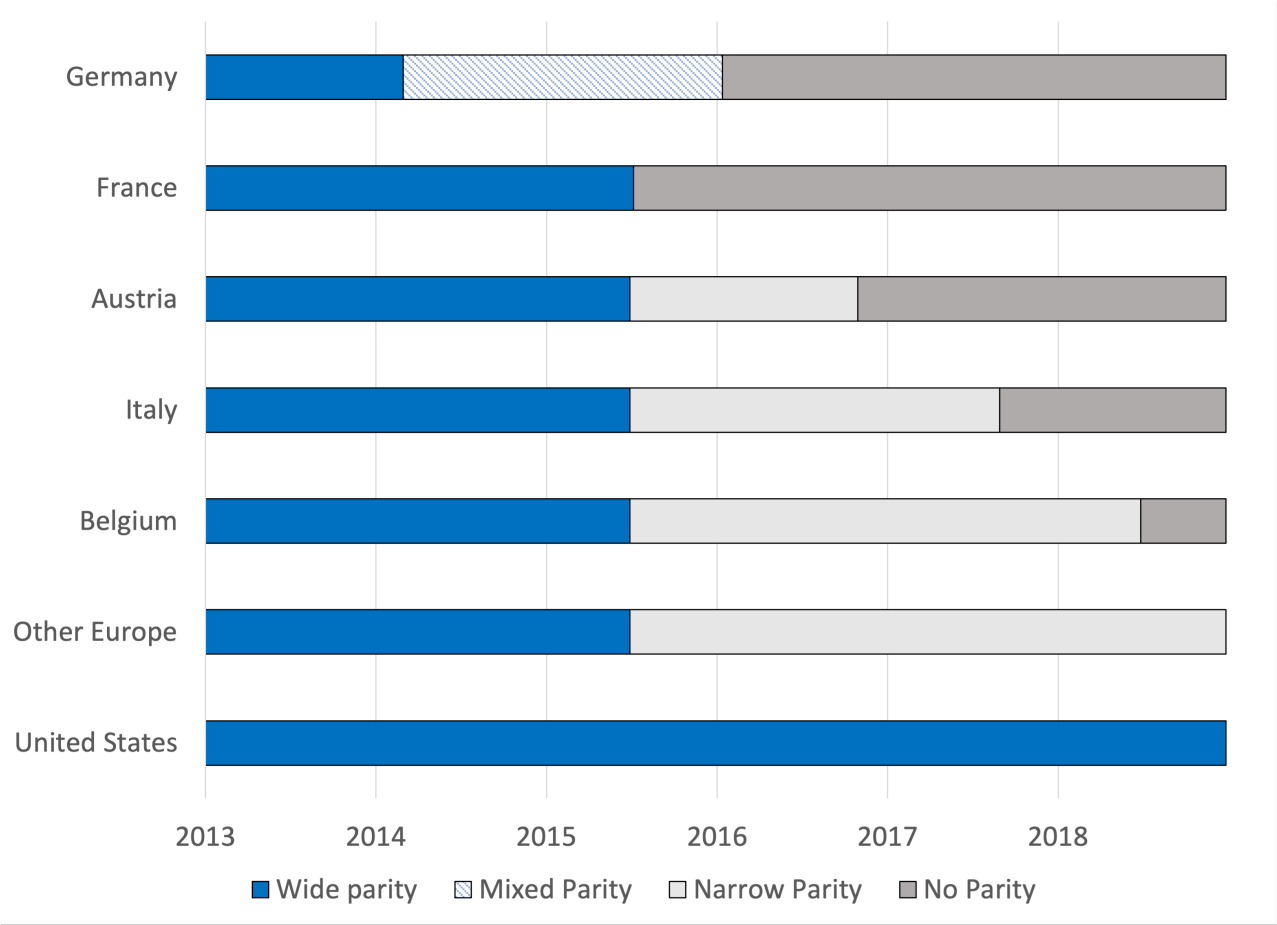
The table shows regression estimates for the effects of the six regulatory changes during our sample period that partially or completely ban parity agreements. The dependent variable in each regression is the occupancy rate. The first two columns show the effects of policies that banned wide parity clauses but left narrow parity in place (Germany and EU), while the last four show the effects of full parity bans (France, Austria, Italy and Belgium). For each policy change, we residualize the data using month fixed effects, market-class fixed effects, market x day-of-week fixed effects, and market x month-of-year fixed effects. We then construct a synthetic control group using markets that were untreated during the sample period. We describe the regulatory regime changes in more detail in Section 2 and provide additional detail on the empirical strategy in Section 4. Standard errors are clustered at the market-class level.

Table 5: Effects of parity bans on revenue using synthetic controls

	(1) Germany	(2) EU	(3) France	(4) Austria	(5) Italy	(6) Belgium
VARIABLES	OLS Log Revenue	OLS Log Revenue	OLS Log Revenue	OLS Log Revenue	OLS Log Revenue	OLS Log Revenue
Germany x After Mar 2014 (treated)	0.005					
	[0.009]					
Europe x After Jul 2015 (treated)		0.002				
		[0.011]				
France x After Aug 2015 (treated)			-0.038***			
			[0.014]			
Austria x After Nov 2016 (treated)				-0.033*		
				[0.019]		
Italy x After Sep 2017 (treated)					-0.048***	
					[0.015]	
Belgium x After July 2018 (treated)						0.024
						[0.018]
Observations	17,664	9,882	8,820	9,282	11,780	7,695

The table shows regression estimates for the effects of the six regulatory changes during our sample period that partially or completely ban parity agreements. The dependent variable in each regression is the log of daily revenue. The first two columns show the effects of policies that banned wide parity clauses but left narrow parity in place (Germany and EU), while the last four show the effects of full parity bans (France, Austria, Italy and Belgium). For each policy change, we residualize the data using month fixed effects, market-class fixed effects, market x day-of-week fixed effects, and market x month-of-year fixed effects. We then construct a synthetic control group using markets that were untreated during the sample period. We describe the regulatory regime changes in more detail in Section 2 and provide additional detail on the empirical strategy in Section 4. Standard errors are clustered at the market-class level.

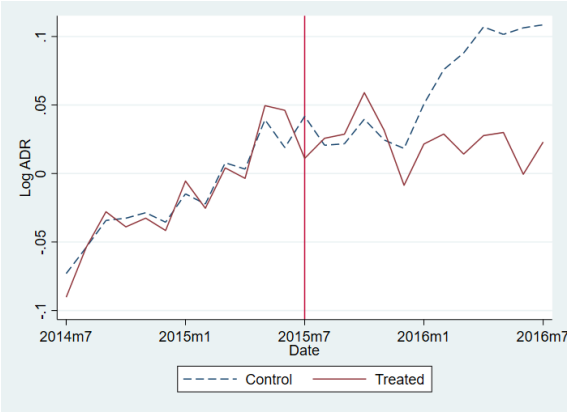
Figure 1: Timeline of regulatory environment in sample countries



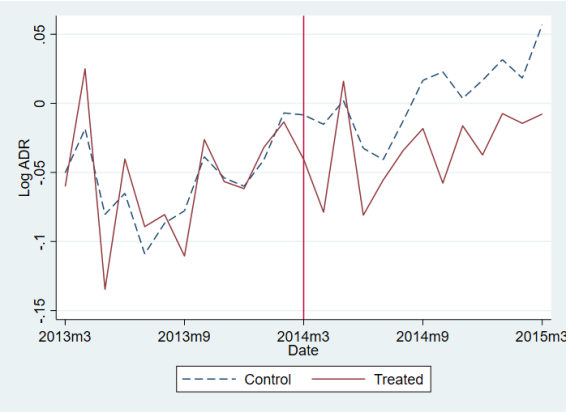
The figure shows the evolution of the regulatory environment surrounding price parity clauses in the US and Europe. We detail the evolution of the regulation of these contracts in Section 2.

Figure 2: Hotel prices vs. synthetic controls

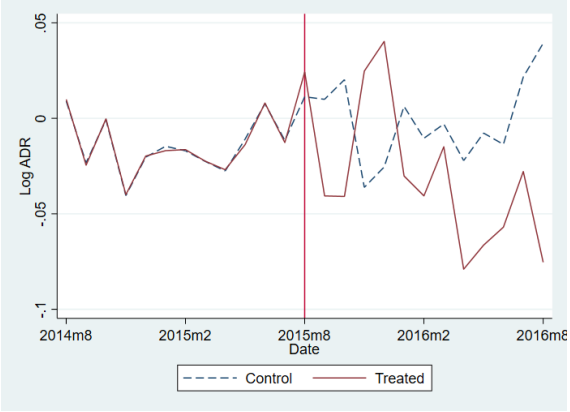
(a) EU-wide Booking.com Settlement



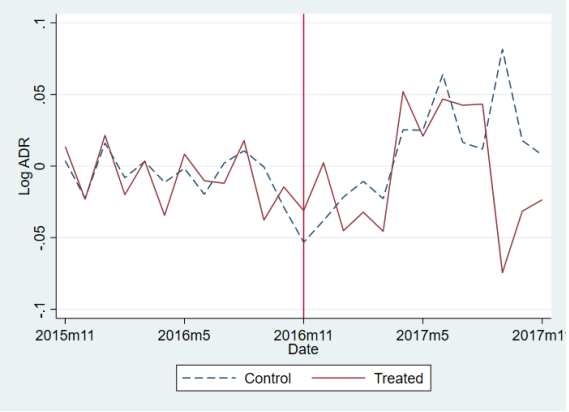
(b) Germany HRS Settlement



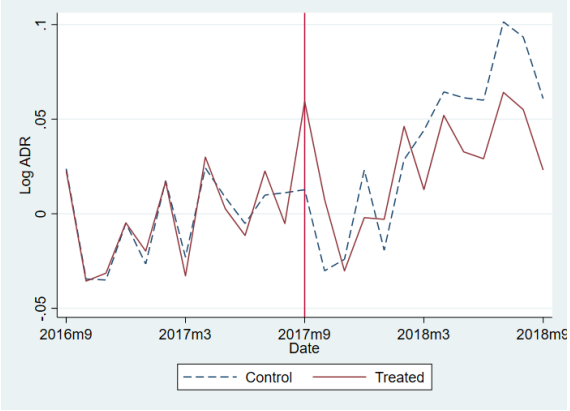
(c) France



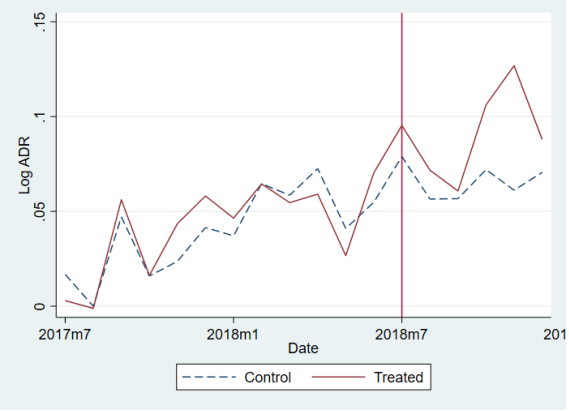
(d) Austria



(e) Italy



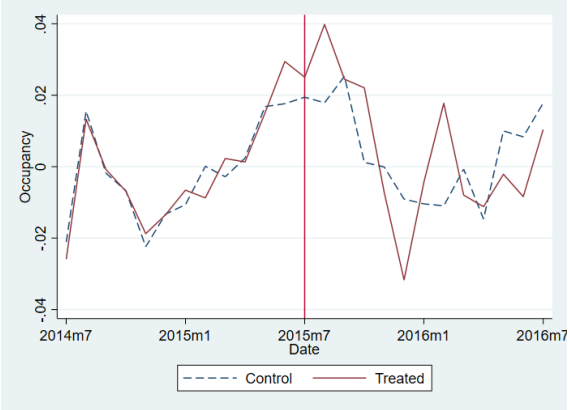
(f) Belgium



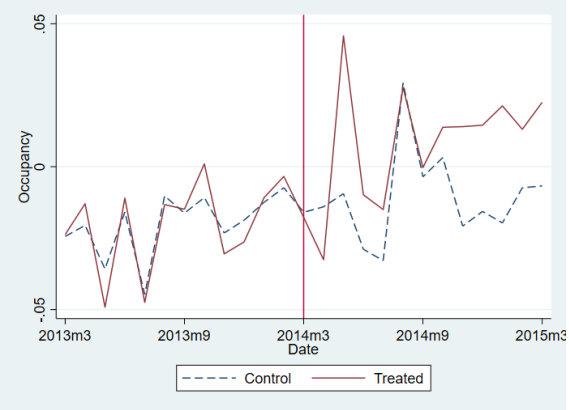
The figure shows average monthly hotel prices in Euros over time for each jurisdiction that implemented a partial or complete parity clause ban along with its synthetic control group (which is comprised of untreated markets). The first two graphs show jurisdictions that banned wide parity clauses but left narrow parity in place, while the last four graphs show event that banned all parity clauses. We provide additional details in Section 4.

Figure 3: Hotel occupancy vs. synthetic controls

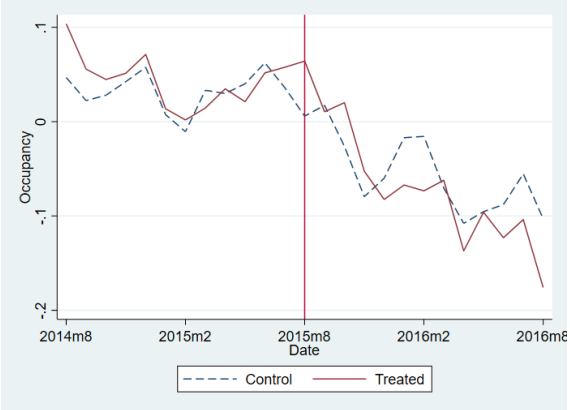
(a) EU-wide Booking.com Settlement



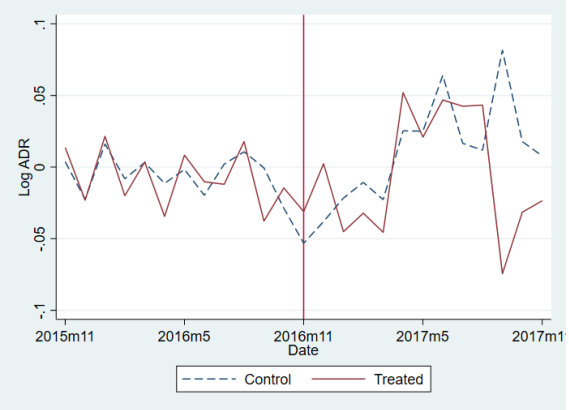
(b) Germany HRS Settlement



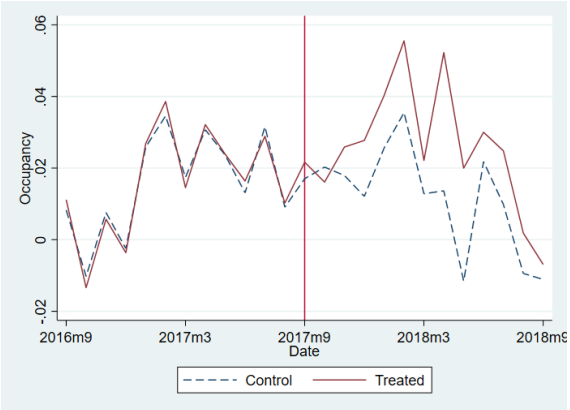
(c) France



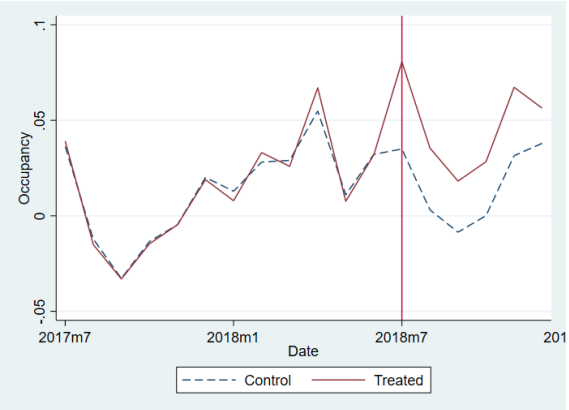
(d) Austria



(e) Italy



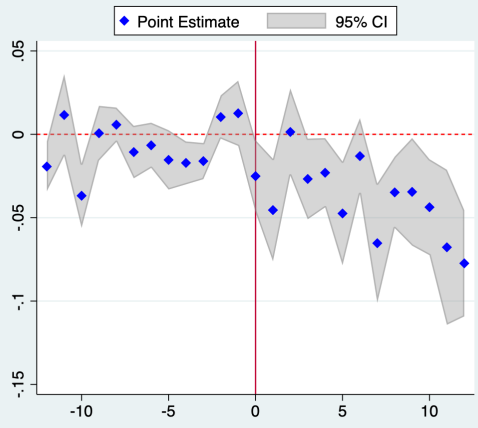
(f) Belgium



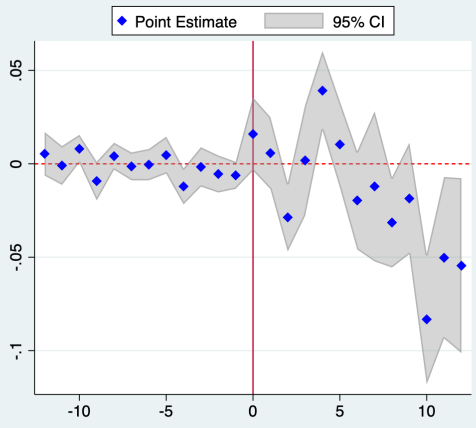
The figure shows average monthly occupancy rates over time for each jurisdiction that implemented a partial or complete parity clause ban along with its synthetic control group (which is comprised of untreated markets). The first two graphs show jurisdictions that banned wide parity clauses but left narrow parity in place, while the last four graphs show event that banned all parity clauses. We provide additional details in Section 4.

Figure 4: Event study specification - prices

(a) Partial Parity Restrictions



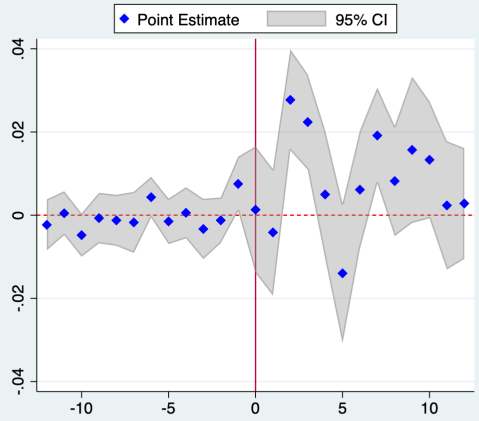
(b) Full Ban



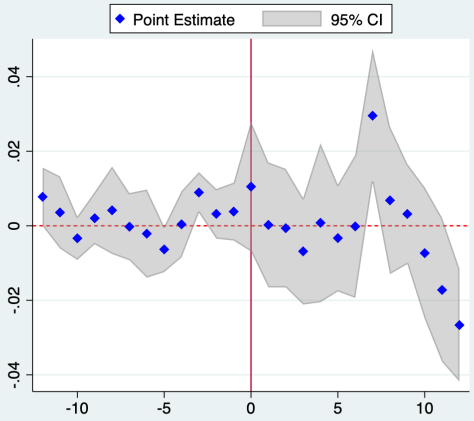
The figure shows results from an event study specification of the dynamic effects of parity restrictions. Panel (a) shows pooled event study estimates for policy interventions that weakened parity clauses without imposing an immediate full ban. This panel combines the EU-wide settlement, which represents a transition from wide to narrow parity clauses, with Germany, where the regulatory environment between 2014 and 2016 reflects a mixed parity regime involving the removal, suspension, and temporary reintroduction of parity clauses. Panel (b) shows pooled estimates for countries that enacted full parity bans (France, Italy, Austria, and Belgium). In each panel, we construct a synthetic control for each treated jurisdiction and estimate pooled monthly treatment effects relative to the policy change. Outcomes are residualized using month, market-class, market by day-of-week, and market by month-of-year fixed effects prior to constructing synthetic controls. Shaded areas denote 95% confidence intervals based on standard errors clustered at the market-class level. The vertical red line indicates the timing of the policy intervention. We provide additional details in Section 5.

Figure 5: Event study specification - occupancy

(a) Wide Parity Ban (Wide to Narrow)

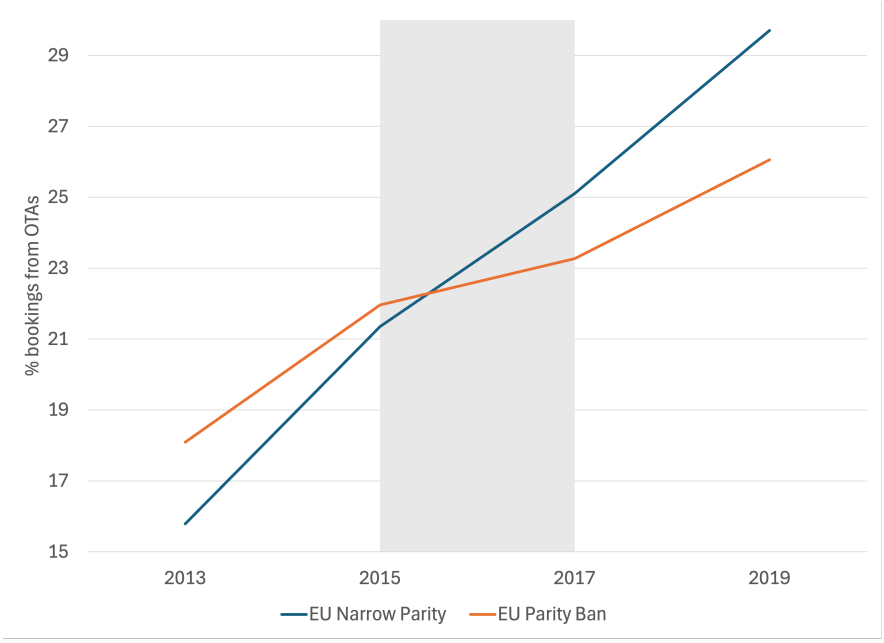


(b) Full Ban



The figure shows results from an event study specification of the dynamic effect of wide parity ban (panel a) or full parity ban (panel b) on occupancy rate using synthetic controls. We construct a synthetic control for each treated jurisdiction and estimate the pooled treatment coefficients by month across each group of experiments (EU and Germany in panel (a) and France, Italy, Belgium, and Austria in panel (b)) after residualizing the data using month, market-class, market x day-of-week, and market x month-of-year fixed effects. Standard errors are clustered at the market-class level. The red line indicates the date of the policy change. We provide additional details in Section 5.

Figure 6: Share of hotel bookings from OTAs, 2013-2019

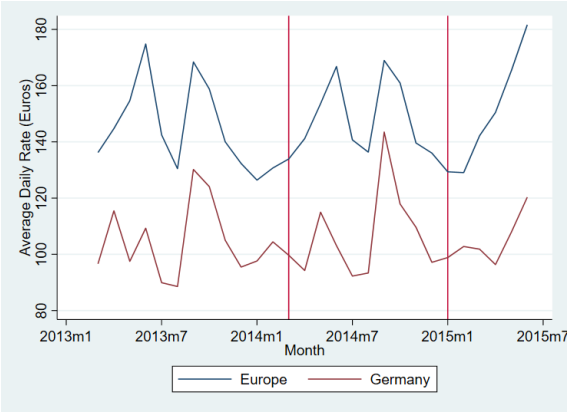


The figure shows the share of OTA bookings between 2013 and 2019 for two groups of EU countries: one group that allowed narrow parity agreements per the 2015 settlement with Booking.com and Expedia (Spain, Norway, Sweden, Czech Republic, Switzerland, Finland, and Greece) and one group that banned all parity clauses (Germany, Italy, and Austria). The grey shaded region indicates the timing of the parity bans in Germany (2016), Austria (2016), and Italy (2017). The data come from a survey administered by Schegg (2020).

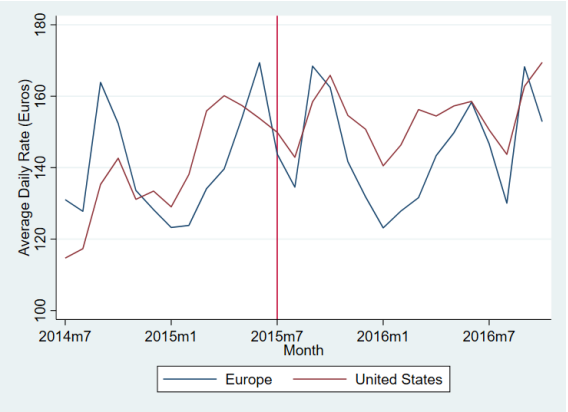
Online Appendix

Appendix Figure A1: Hotel prices after policy changes

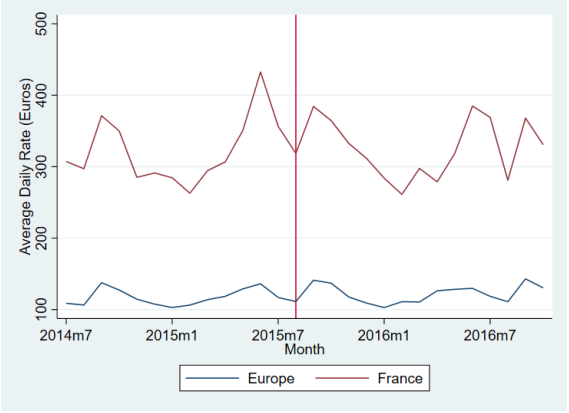
(a) Germany



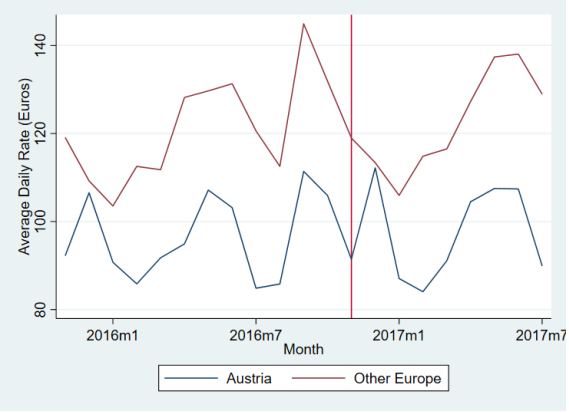
(b) EU



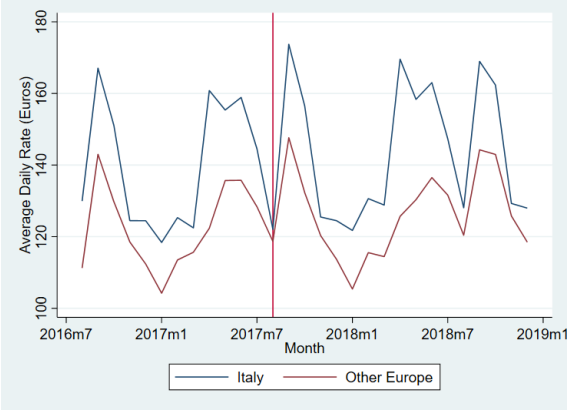
(c) France



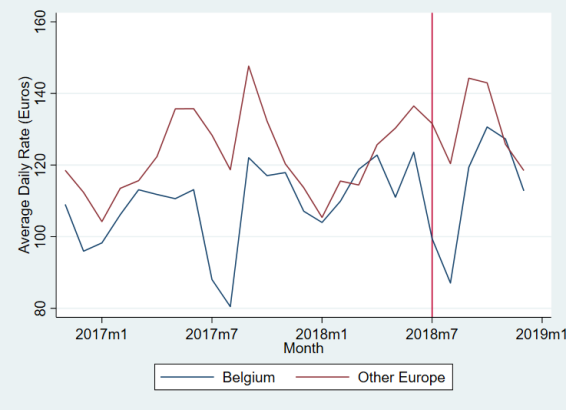
(d) Austria



(e) Italy



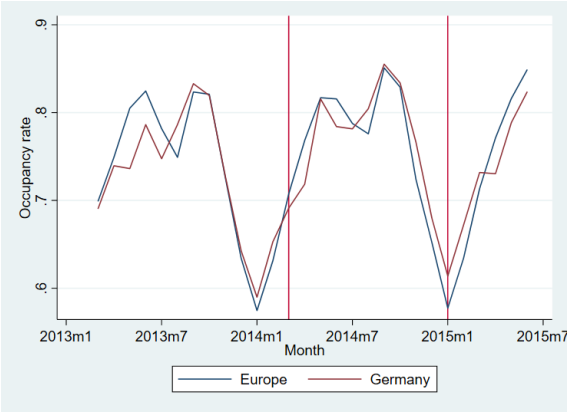
(f) Belgium



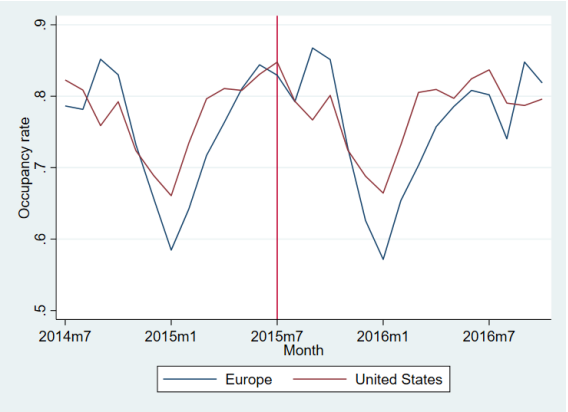
The figure shows average monthly hotel prices in Euros around each of the policy experiments used in our analysis. We also plot prices in a sample of control markets used for each experiment. The red line indicates the date of the policy change.

Appendix Figure A2: Hotel occupancy rates after policy changes

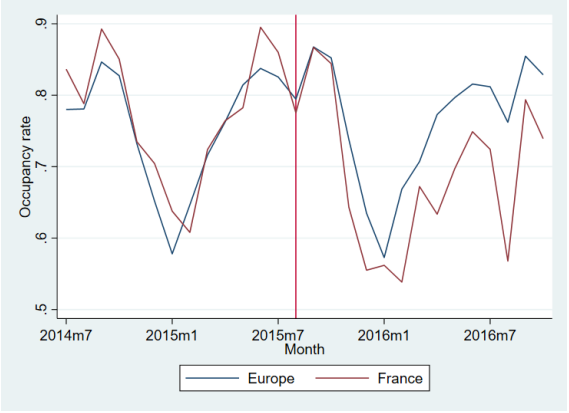
(a) Germany



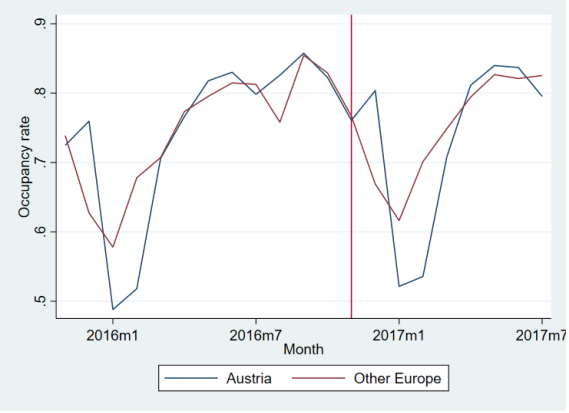
(b) EU



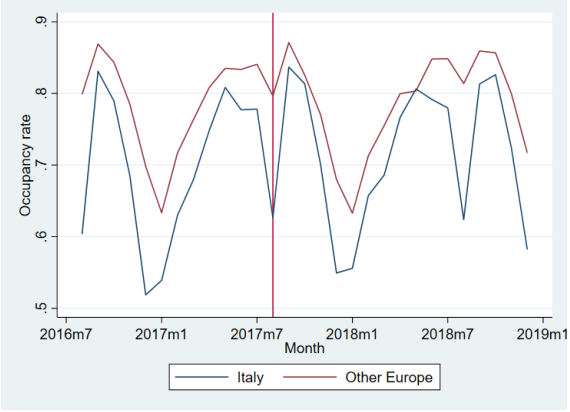
(c) France



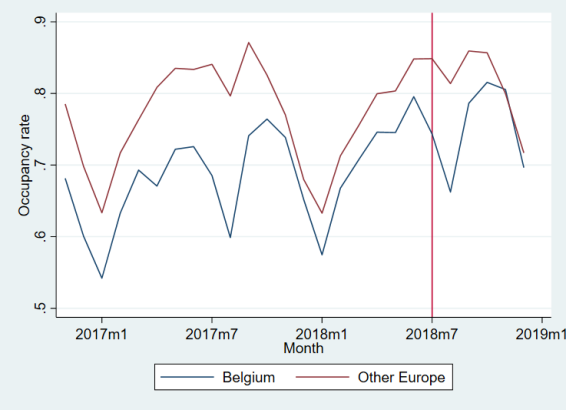
(d) Austria



(e) Italy



(f) Belgium



The figure shows average monthly occupancy rates around each of the policy experiments used in our analysis. We also plot prices in a sample of control markets used for each experiment. The red line indicates the date of the policy change.

Appendix Table A1: Control and treatment markets

Experiment	Control markets	Treated markets	Treatment date	Time period
Germany HRS ruling	Amsterdam, Barcelona, Brussels, Madrid, Milan, Paris, Prague, Rome, Vienna	Berlin, Dusseldorf, Munich	3/1/14	1/1/2012-6/30/2015
Europe-wide Booking.com settlement	Chicago, Los Angeles, New York, San Francisco, Washington DC	Amsterdam, Barcelona, Brussels, London, Madrid, Milan, Prague, Rome, Vienna	7/1/15	1/1/2012-10/31/2016
France parity ban	Amsterdam, Barcelona, Brussels, Madrid, Milan, Prague, Rome, Vienna	Paris	8/8/15	1/1/2012-10/31/2016
Austria parity ban	Amsterdam, Barcelona, Berlin, Brussels, Dusseldorf, London, Madrid, Milan, Munich, Prague, Rome, Vienna	Vienna	11/20/16	7/1/2015-7/31/2017
Italy parity ban	Amsterdam, Barcelona, Berlin, Dusseldorf, London, Madrid, Munich, Prague	Milan, Rome	8/29/17	7/1/2015-12/31/2018
Belgium parity ban	Amsterdam, Barcelona, Berlin, Dusseldorf, London, Madrid, Milan, Munich, Prague	Brussels	7/19/18	7/1/2015-12/31/2018

The table lists the control and treatment markets used in the standard difference-in-differences analysis shown in Tables A2-A4.

Appendix Table A2: Effects of policy changes on prices - standard DID

	(1) Germany	(2) EU	(3) France	(4) Austria	(5) Italy	(6) Belgium
VARIABLES	OLS Log ADR	OLS Log ADR	OLS Log ADR	OLS Log ADR	OLS Log ADR	OLS Log ADR
Germany x After March 2014 (treated)	-0.016**					
	[0.008]					
Rest of EU x After Jul 2015 (treated)		-0.001				
		[0.014]				
France x After Aug 2015 (treated)			-0.051***			
			[0.014]			
Austria x After Nov 2016 (treated)				-0.021**		
				[0.009]		
Italy x After Sep 2017 (treated)					-0.045***	
					[0.014]	
Belgium x After July 2018 (treated)						-0.002
						[0.011]
Observations	80,035	155,265	88,371	49,019	70,720	62,414
R-squared	0.977	0.978	0.984	0.976	0.977	0.978

The table shows regression estimates for all of the regulatory changes during our sample period. We describe these regulatory regime changes in more detail in Section 2. The dependent variable in each regression is the natural log of average daily price. All specifications include Google search volume controls as well as month fixed effects, market-class fixed effects, market x day-of-week fixed effects, and market x month-of-year fixed effects. Standard errors are clustered at the market-class level.

Appendix Table A3: Effects of policy changes on occupancy - standard DID

	(1) Germany	(2) EU	(3) France	(4) Austria	(5) Italy	(6) Belgium
VARIABLES	OLS Occupancy	OLS Occupancy	OLS Occupancy	OLS Occupancy	OLS Occupancy	OLS Occupancy
Germany x After March 2014 (treated)	0.002					
	[0.005]					
Rest of EU x After Jul 2015 (treated)		0.028***				
		[0.006]				
France x After Aug 2015 (treated)			-0.080***			
			[0.009]			
Austria x After Nov 2016 (treated)				-0.006		
				[0.007]		
Italy x After Sep 2017 (treated)					0.002	
					[0.009]	
Belgium x After July 2018 (treated)						0.045***
						[0.015]
Observations	80,035	155,265	88,371	49,019	70,720	62,414
R-squared	0.622	0.666	0.650	0.635	0.617	0.598

The table shows regression estimates for all of the regulatory changes during our sample period. We describe these regulatory regime changes in more detail in Section 2. The dependent variable in each regression is occupancy rate. All specifications include Google search volume controls as well as month fixed effects, market-class fixed effects, market x day-of-week fixed effects, and market x month-of-year fixed effects. Standard errors are clustered at the market-class level.

Appendix Table A4: Effects of policy changes on revenue - standard DID

	(1) Germany	(2) EU	(3) France	(4) Austria	(5) Italy	(6) Belgium
VARIABLES	OLS Log Revenue	OLS Log Revenue	OLS Log Revenue	OLS Log Revenue	OLS Log Revenue	OLS Log Revenue
Germany x After March 2014 (treated)	-0.013					
	[0.012]					
Rest of EU x After Jul 2015 (treated)		0.040**				
		[0.017]				
France x After Aug 2015 (treated)			-0.170***			
			[0.023]			
Austria x After Nov 2016 (treated)				-0.034**		
				[0.014]		
Italy x After Sep 2017 (treated)					-0.035**	
					[0.014]	
Belgium x After July 2018 (treated)						0.069**
						[0.026]
Observations	80,035	155,265	88,371	49,019	70,720	62,414
R-squared	0.931	0.938	0.949	0.937	0.942	0.943

The table shows regression estimates for all of the regulatory changes during our sample period. We describe these regulatory regime changes in more detail in Section 2. The dependent variable in each regression is the natural log of average daily revenue. All specifications include Google search volume controls as well as month fixed effects, market-class fixed effects, market x day-of-week fixed effects, and market x month-of-year fixed effects. Standard errors are clustered at the market-class level.

Appendix Table A5: Synthetic control estimates - alternative specifications

	(Baseline)	(1)	(2)	(3)
Log ADR				
Germany	-0.013**	-0.013**	-0.025***	-0.025***
	(0.005)	(0.005)	(0.006)	(0.006)
Europe	-0.016**	-0.016**	-0.002	-0.002
	(0.007)	(0.007)	(0.011)	(0.011)
France	-0.008	-0.008	-0.016	-0.016
	(0.015)	(0.015)	(0.011)	(0.011)
Austria	-0.040**	-0.041**	-0.039**	-0.039**
	(0.017)	(0.017)	(0.016)	(0.016)
Italy	-0.029***	-0.029***	-0.028***	-0.028***
	(0.010)	(0.010)	(0.010)	(0.010)
Belgium	0.003	0.005	-0.023**	-0.022**
	(0.011)	(0.011)	(0.010)	(0.010)
Occupancy				
Germany	0.012**	0.012**	0.007	0.007
	(0.005)	(0.005)	(0.005)	(0.005)
Europe	0.008*	0.008*	0.009	0.009
	(0.005)	(0.005)	(0.006)	(0.006)
France	-0.020***	-0.049***	-0.054***	-0.054***
	(0.005)	(0.016)	(0.016)	(0.016)
Austria	0.001	0.001	0.002	0.002
	(0.006)	(0.006)	(0.008)	(0.008)
Italy	-0.006	-0.006	0.003	0.004
	(0.008)	(0.008)	(0.009)	(0.009)
Belgium	0.018**	0.019**	0.002	0.003
	(0.009)	(0.009)	(0.009)	(0.009)
Log Revenue				
Germany	0.005	0.005	-0.011	-0.011
	(0.009)	(0.009)	(0.009)	(0.009)
Europe	0.002	0.002	0.021*	0.021*
	(0.011)	(0.011)	(0.012)	(0.012)
France	-0.038***	-0.110***	-0.100***	-0.100***
	(0.014)	(0.034)	(0.029)	(0.029)
Austria	-0.033*	-0.034*	-0.056***	-0.056***
	(0.019)	(0.018)	(0.020)	(0.020)
Italy	-0.048***	-0.047***	-0.027***	-0.025***
	(0.015)	(0.015)	(0.009)	(0.009)
Belgium	0.024	0.027	-0.023	-0.019
	(0.018)	(0.019)	(0.024)	(0.024)
Search controls	Yes	Yes	Yes	Yes
Month of year × Market FE	Yes	Yes	No	No
Day of week × Market FE	Yes	No	Yes	No

The table shows regression estimates for all of the regulatory changes during our sample period using the synthetic control method with alternative sets of fixed effects. We describe these regulatory regime changes in more detail in Section 2. The baseline specification includes week fixed effects, market-class fixed effects, market x day-of-week fixed effects, market x month-of-year fixed effects, and Google search volume controls. The three panels show results for log prices, occupancy rate, and log revenues, respectively. Column (1) of this table drops market x day of week fixed effects, column (2) drops market x month of year fixed effects, and column (3) drops both market x day-of-week and market x month-of-year fixed effect. Standard errors in all regressions are clustered at the market-class level.

Appendix Table A6: Synthetic control estimates using weights from price regression

	Germany	Europe	France	Austria	Italy	Belgium
Occupancy effects						
Using price weights	0.0021	-0.0001	-0.0228***	0.0084	-0.0072	0.0122
	(0.0060)	(0.0077)	(0.0052)	(0.0124)	(0.0138)	(0.0212)
Using occupancy weights	0.0123***	0.0067	-0.0204***	0.0011	-0.0058	0.0179
	(0.0046)	(0.0044)	(0.0052)	(0.0083)	(0.0129)	(0.0270)
Revenue effects						
Using price weights	-0.0095	-0.0185	-0.0388**	-0.0307	-0.0341	0.0233
	(0.0122)	(0.0160)	(0.0187)	(0.0408)	(0.0338)	(0.0563)
Using revenue weights	0.0052	0.0015	-0.0419**	-0.0332	-0.0478	0.0242
	(0.0127)	(0.0227)	(0.0198)	(0.0399)	(0.0425)	(0.0564)

This table reports synthetic control estimates of the effects of each regulatory change on occupancy and revenue using alternative weighting schemes. For each outcome, we report estimates obtained using (i) synthetic control weights constructed to match pre-treatment price dynamics ("price weights") and (ii) outcome-specific synthetic control weights constructed to match the pre-treatment trajectory of the outcome being analyzed ("occupancy weights" or "revenue weights"). All specifications follow the empirical strategy described in Section 4. Standard errors clustered at the market-class level are reported in parentheses.