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 difference-in-differences 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 3.2% and increased occupancy by 1.7 percentage points; (ii) relative to narrow parity, a complete ban on parity clauses reduced prices by 3.7%, 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" rate 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" rate 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 of two sets of recent regulatory actions in Europe that ban the use of parity clauses. The first set of rulings banned the use of wide rate parity agreements, but left in place narrow parity. This regulatory regime was first enacted in Germany in January 2015, and subsequently expanded to all EU countries in July 2015. 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).

Policy actions that prohibit wide or narrow parity may impact hotel prices in different ways; first, by changing a hotel's incentives to undercut OTAs through their own direct channels, and second, by changing competition between OTAs in their commission fees offered to hotels. Policies that ban wide but leave in place narrow parity may induce hotels to provide discounts through offline channels, while full price parity bans further allow the hotel to set lower prices through its (perhaps more visible) online channels. Both types of policies may spur increased competition between OTAs on commission rates by allowing hotels to charge lower rates on low-commissions OTAs. While technically narrow parity clauses allow a hotel to charge different prices across different OTAs, they may still restrain price competition, as hotels may be reluctant to undercut their direct channel price on any OTA platform.[2] Removal of narrow parity may thus eliminate this additional constraint on hotels' ability

[2]This was a key part of the argument made by regulators in Germany to ban narrow parity clauses (Bundeskartellamt, 2016).

to offer lower prices to low-commission OTAs. Since different price parity regulations can impact market outcomes through multiple margins, empirical evidence related to one type of parity ban does not necessarily extend to the other type. It is therefore important to understand the impact of both types of regulatory regimes.

We use a difference-in-differences strategy that exploits variation in the timing and scope of policy governing price parity to measure the effect of wide and narrow rate parity clauses. We first investigate the impact on prices, and find that both sets of rulings cause prices to fall. In our baseline specification, we treat each policy change as a separate event. We find that policies that banned wide parity but left narrow parity clauses in place decreased average hotel prices by approximately 3%. Policy actions that moved from narrow parity to a complete ban on parity decreased prices by between 1% and 9%. In a pooled specification on each set of experiments, we find that in countries that banned wide parity but allowed narrow parity, prices dropped by 3.2%, while countries that enacted a complete ban saw prices fall by 3.7%, relative to control countries with narrow parity.

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—following Dube et al. (2023)—reveals that pre-policy trends cannot explain the estimated effect of the policy actions. Finally, we present a series of placebo tests in which we randomly assign placebo policy actions to different (untreated) cites. We find the resulting estimated effect to be symmetric around zero and not statistically different from zero in 21 out of 24 placebo tests, thus mitigating concerns that the research design is biased towards finding negative price effects.

We then explore the impact of the policy 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 lead to more consumers using the OTA to search, but then booking directly through the hotel—behavior dubbed "show-

rooming" (Wang and Wright, 2020)—OTAs may respond by reducing investment in the platform, a key concern among regulators debating parity bans (Luther, 2021). If the reduced investment increases consumer search costs, it may drive down residual demand for hotels and decrease occupancy. Our analysis suggests that effects on occupancy are mixed—policy changes that left narrow parity clauses in place increased occupancy by 1.7 percentage points, while policies that banned parity clauses had no discernible effect. Similar analysis of hotel revenue per room, which combines both price and occupancy effects, shows a positive but insignificant effect of wide-to-narrow policies, and a negative effect of parity ban policies.

Because both types of policies may impact inter-OTA competition, we cannot fully separate the mechanisms driving the drop in prices. However, 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 suggests that an important driver of the price effect of removing narrow parity is to enable hotels to set lower prices through their direct channel (for example, their website), rather than enhanced 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 impact of both wide and narrow parity clauses on prices 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.[3] 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

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

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 seven different regulatory regimes—three that move countries from wide to narrow parity, and four that move from narrow parity to a complete ban. 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 the evidence is mixed. Occupancy increases in countries that move from wide to narrow parity clauses, but we find no effect for complete parity bans, 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.[4] Crucially, our data include offline bookings, which represent over 40% of European hotel stays.[5] 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.

Understanding the overall effects of price parity regulations on market outcomes remains policy relevant, as the discussion over appropriate remedies continues in several jurisdictions. For instance, the European Commission's revised guidelines on vertical relationships, released in June 2022, removed competition law exemptions for wide rate parity clauses (Lovells,

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

[5] 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).

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).

Ex ante, the effect of price parity clauses on downstream prices is theoretically ambiguous. 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). 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 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.

Competition between OTAs (as well as new OTA entry) may 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). Direct bookings may have lower costs, as the hotel does not pay a commission to the OTA. Offering lower prices through the direct channel is particularly important if direct channel consumers differ in their demand from OTA shoppers—for example, if consumers that book a stay in advance through Expedia differ from walk-ins along demographics like age or income. All else equal, this effect would lead to a higher share of relatively high-cost OTA bookings rather than those made directly with the hotel. Edelman and Wright (2015) show that under some conditions, parity clauses can lead to "excessive intermediation"—because consumers pay the

same price on the intermediary as when they book directly, a large share book through the OTA, even though in equilibrium this leads to high prices that outweigh any benefits provided by the intermediary (e.g. search and price comparison tools). In the absence of parity, hotels may also use lower rates to incentivize last minute and walk-in bookings when demand is lower than anticipated. This channel can also be affected by both types of parity agreements; the removal of wide parity allows hotels to set lower prices in offline channels, while the further removal of narrow parity also allows hotels to undercut OTA prices on their own website.

On the other hand, literature has also argued that parity agreements may indirectly lower hotel prices by solving a holdup problem (Baker and Chevalier, 2012; Wang and Wright, 2020; Ezrachi, 2015). OTAs invest in tools that lower search costs for consumers by collecting prices from many different hotels in a single searchable database. These search tools 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. Thus, some form of parity agreements may protect investment incentives for the platform and lower search costs for consumers, making the hotel market more competitive.

Our paper provides further evidence to this debate. We find that moving from wide to narrow and from narrow to no parity both reduce prices. This suggests that relaxing parity agreements is encouraging more price competition between sales channels. However, we also find that moving from narrow parity to a full ban had no impact on occupancy, despite a drop in prices, which is consistent with a reduction in OTA investment incentives as a result of a hold-up problem. Further, in countries that banned all parity agreements, the share of bookings made through OTAs grew substantially less than in countries where narrow parity was left in place. Finally, we note that there has not been significant entry in the OTA market during this period—indeed, the OTA market has become more concentrated—and so

we conclude that low cost entrants are not a primary driver of the price effects.

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 concludes.

2 Regulatory landscape

In recent years, rate parity clauses in contracts between OTAs and hotels have been the subject of regulatory scrutiny by antitrust authorities around the world. Rate 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).[6] Federal courts ultimately ruled in 2014 that, while the agreements had the potential to be used to 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 rate 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 rate 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 in Germany starting in January 2015 and later in all European markets beginning in July 2015 (Booking.com, 2015). In subsequent legislation, several European countries prohibited the use of both wide and narrow rate parity clauses, including France in July 2015, Germany in January 2016, Austria

[6]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).

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 the use of parity agreements. 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). Other OTAs (namely Booking.com and Expedia) committed to replacing their wide parity clauses with narrow versions in Germany by January 2015 (Botteman et al., 2019). Following subsequent legal action by the FCO, Booking.com was ordered to remove narrow parity clauses as well as of January 2016 (Bundeskartellamt, 2016).[7]

The policy debate and regulatory actions in Germany prompted subsequent investigations by regulators in France, Italy and Sweden. These investigations culminated in Europe-wide settlements by the two largest intermediaries, Booking.com and Expedia (who held a combined market share of 64% in 2012 according to Phocuswright). Under the settlement, Expedia and Booking.com suspended wide parity agreements and instead moved to narrow rate clauses across Europe, beginning on July 1, 2015 (Booking.com, 2015; Konkurrensverket, 2015). The effects of this partial ban on rate 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

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

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.

Following the EU-wide settlement with Booking.com and Expedia, the legislatures of several European countries enacted more stringent regulations of price parity, striking down narrow parity. On July 9, 2015, the French National Assembly, within the "Macron Law" (French Law 2015-990), effectively prohibited any form of rate parity between OTAs and hotels effective August 8th (Roskis and Strange, 2015). The decision was condemned by Booking.com and other intermediaries, who threatened that the ruling could ignite a price war and hurt hotel margins. In Italy, the Senate 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 possible concern, however, is that the policy actions which eliminated parity simultaneously induced other changes in these markets, such as reductions in VAT rates or subsidies to the hotel sector. This would be of particular concern if the policy was part of a broader subsidy or support package for the hotel market in general. Details of the policy implementation suggest that this is unlikely. In three of the seven policy changes we study (the two actions in Germany and the EU-wide settlement with Booking.com and Expedia), the change is the result of actions taken by antitrust authorities following specific investigations into the use of price parity clauses by OTAs. Given the narrow focus of these inquiries–and the fact that the courts lack statutory power to change tax rates or provide subsidies–they seem unlikely to have been bundled with other changes that affected hotel markets.

In France, Austria, Italy, and Belgium, the prohibition of price parity clauses was the result of legislation which in some cases included other changes. For instance, the policy

change in France was part of the "Macron Law", which also reduced entry restrictions in bus transportation and legal services, reformed labor markets, simplified housing regulations, and impacted corporate governance rules (Vogel, 2017). In Italy, parity clauses were banned with the passage of the Annual Competition Law of 2017, which included other competitive reforms (Global Compliance News, 2017). In Austria, parliament banned parity clauses through changes to the Austrian Federal Act Against Unfair Competition and the Austrian Price Labeling Act. However, these other reforms appear to be largely unrelated to the hotel sector.[8]

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.[9] 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 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

[8]This is consistent with the detailed discussion of these reforms in Franck and Stock (2020), who suggest that the passage of the policies in these four countries was primarily motivated by concern about the market power of large OTAs relative to the more fragmented hotel market.

[9]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.

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.[10] 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 country in Table 2. The most expensive markets in our sample are located in France (Paris), the United States (which include New York and San Francisco), and the United Kingdom (London). The cheapest hotels were in the Czech Republic, Germany, and Austria. Occupancy rates across countries ranged between 67% (Belgium) to more than 80% (in 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.

[10] STR Global also produces a more aggregated series at the market-day level, which is sufficiently aggregated that there is no censoring.

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 wide and narrow parity agreements on hotel prices and occupancy. To measure these effects, we employ a differences-in-differences (DD) strategy that compares hotel prices in the treated country 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 base specification treats each policy action as a separate experiment, rather than stacking them in a single regression. We also report results that pool the two sets of experiments using the procedure developed in Borusyak et al. (2024), which is robust to the issues identified in Goodman-Bacon (2021). To illustrate the dynamic impacts of the treatment, we implement the local projection technique developed in Dube et al. (2023). This approach uses only clean control units to avoid the pitfalls associated with standard event study designs when treatments are staggered.

Our first set of analyses measures the effect of moving from wide rate parity agreements to narrow parity. There are three such policy changes; the settlements in Germany (with HRS in February 2014, and with other large OTAs in January 2015) and the Expedia and Booking.com settlement in July 2015, which applied to all European markets. To test the effect of the ruling in Germany, we take hotels in Berlin, Dusseldorf, and Munich as the treated group, and all other European markets as the control group. To measure the effect of the Europe-wide settlement, we use hotels in European markets as the treated group and

those in US markets as the control.

Our second set of analyses measures the effect of moving from a regime that allows narrow rate parity clauses to one in which parity clauses of all forms are banned. We observe these legal changes in three sample countries: Austria (November 2016), Italy (September 2017), and Belgium (July 2018). In France, the Macron Law resulted in a change from wide parity prior to July 2015 to a complete parity ban in the period that followed. In our DD specifications using these four countries, we use hotel prices in untreated European countries as a control group (Czech Republic, Netherlands, Spain, and the UK).[11] Because the total parity ban in France is concurrent with the move from wide to narrow parity in the rest of the Europe, the effect that we measure should be interpreted as the incremental effect of a complete parity ban relative to moving to narrow parity.

A key decision in our empirical strategy concerns the construction of the control group for each of the events that we study. Following the insights from Borusyak et al. (2024), we are careful to define the control group for each event in a way that avoids including previously treated units as controls (so-called "forbidden comparisons"). For the six out of seven policy changes that affect markets in a single country, we use observations from other European markets that were unaffected as controls. Because the Booking.com settlement affects all European markets simultaneously, no European markets are available as control units. In this case, we includes US markets as the control group. In Table 3, we list the exact markets and time periods that are used as treatment and control samples for each analysis.

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 for each policy experiment (marked with a red vertical line) together with its control groups. While the monthly price data are subject to seasonal patterns and some volatility, the control and treatment groups largely appear to exhibit parallel trends.

[11] While these countries had previously been subject to the EU-wide settlement with Booking.com in 2015, there were no other regulatory changes in the period after the ruling.

A primary threat to this identification strategy is the existence of time-varying shocks that impact hotel prices in US and various European countries differentially, independent of changes in the country's policy regarding price parity. These could result in changes in prices that are correlated with the timing of the policy changes that may confound our measurement. We address this by including a robust set of controls and fixed effects. In our main specification, we include average daily occupancy rate - a measure of how full hotels were in a particular time period. Our research goal is to measure the effect of the partial ban of price parity clauses on hotel pricing policies. If recent regulatory action had any price effect, it should have induced hotels to change their behavior because of changes in contractual agreements with OTAs, either by lowering the costs of providing and selling accommodations incurred by hotels (through better terms offered from OTAs or lower shares of OTAs booking) or by lowering hotels markups thanks to renewed incentives to lower prices (e.g., because of the possibility to undercut prices offered on OTAs to walk-in costumers). Therefore, we are interested in estimating a supply-side equation. Since both occupancy rate and price may be affected by unobservable shocks, we instrument for occupancy rate with lagged Google search volumes for "<city> hotels". Google search volumes are a valid instrument as long as variation over time in consumers' search decisions are not influenced by price (for instance, if consumers decide to research a city because they know it has cheap hotels in a particular week). We also include additional controls in the form of month fixed effects, market-class fixed effects, market x day-of-week fixed effects, and market x month-of-year fixed effects, which control for possible seasonal variation in demand across markets.

A second 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.

As the plots of the price data show, hotel prices are volatile, even when aggregated at

the month level. To deal with this volatility, we test a number of specifications that include day-of-week effects, trends over time, seasonality, and other demand fluctuations. In our preferred specification, we estimate the following equation:

$$\ln p_{jtc} = \mu_t + \mu_{jd} + \mu_{jc} + \delta_{jm} + \beta Occ_{jtc} + \alpha T_{jt} + \varepsilon_{jtc} \tag{1}$$

$\ln p_{jtc}$ is the natural log of the price (average daily rate) of rooms in city j on date t belonging to hotel class c. μ_t, μ_{jd}, μ_{jc}, and δ_{jm} are fixed effects for t's year-month, city-day-of-week combination jd, city-class combination jc, and city-month of the year combination jm respectively. Occ_{jtc} is the occupancy rate, measured as occupied rooms divided by available rooms in sample hotels. T_{jt} is one if the market is in a treated country after the regulation change. A negative α coefficient indicates that the policy change had a negative effect on prices. We cluster standard errors at the market-class level in all specifications.

To shed further light on the impact of the parity bans, we also implement additional empirical specifications, where we either substitute the dependent variable of equation (1) with the occupancy rate or with the daily revenues (occupancy times price); in another exercise, we aggregate the daily prices at the monthly-level–after extracting the full set of fixed effects–to investigate price dynamics before and after the policy changes.

5 Results

5.1 Effects of parity agreements on hotel prices

We first report results from the set of regressions that measure the effect of moving from wide to narrow parity. Results from our preferred specification are in Table 4. In Germany, the move to narrow parity in January 2015 was associated with a price decrease of approximately 3.4% in the IV specification, while the Europe-wide settlement led to a decrease in prices of about 2.8%. We find no effect of the March 2014 HRS settlement on hotel prices in Germany.

In both regressions, our IV and OLS estimates yield similar estimates of the price effects of the policy, although the IV strategy yields a larger coefficient on hotel occupancy.

In Table 5, we report results from the regressions that measure the impact of a complete ban on parity clauses on hotel prices. Because the control group is other European markets where the largest OTAs were already prohibited from using wide parity clauses, our treatment coefficients should be interpreted as the incremental effect of a full parity ban relative to a narrow parity regime. We find evidence of sizable price effects of these policies. In France, our IV estimates indicate that a full parity ban was associated with a statistically significant drop in prices of 8.6%. In Italy and Belgium, we estimate that the parity bans were associated with price drops of 5.0% and 3.9% respectively. We find a price effect of 1.7% in Austria, although the effect is not statistically significant at the 5% confidence level.

As a way of summarizing our results across multiple experiments, we estimate a pooled specification using the DD estimator developed in Borusyak et al. (2024). We report the results in Table 6. In column (1), we show estimates of the effect of moving from wide to narrow parity. Our treatment coefficient implies a price decrease of 3.2%, consistent with the estimates shown in Table 4. Column (3) shows estimates of the effect of moving from narrow to no parity; the treatment effect for these experiments is 3.7%. Both estimates are statistically significant at the 1% confidence level.

We then use this same framework to test for heterogeneity in treatment effect along two dimensions. We first examine how the price effects vary across hotel classes, which we consolidate into three groups: economy, midscale/upscale (which includes midscale, upper midscale, upscale, and upper upscale), and luxury. We estimate a pooled specification with these interaction terms, again using the estimator from Borusyak et al. (2024). Column (2) of Table 6 shows the results from the wide to narrow parity experiments. We see the strongest effects for economy class hotels, whose prices drop by more than 13%. We also find a 3% drop among luxury class hotels. Both coefficients are significant at the 1% confidence level. In contrast, we find a positive (but insignificant) effect for midscale hotels.

We show results for the same specification using the events that move from narrow parity to a ban on parity in Column (5) of Table 6. We again find negative and statistically significant results for economy and luxury class hotels, and are unable to reject the null for the midscale and upscale classes. In this set of experiments, the effects for the economy and luxury classes are of similar magnitudes, and we cannot reject the null that the treatment coefficients are the same. The heterogeneity we find in our data provides an interesting contrast to the results in prior literature. Mantovani et al. (2021) find the largest price effects for highly rated hotels (those with 3, 4, or 5 stars) and chains, but do not look directly at hotel class. Ennis et al. (2023) report larger results for budget hotels, although we note that their analysis focuses on the probability that hotels set different prices across channels, rather than the absolute level of prices.

We note that the legislative changes only affected parity agreements between hotels and major OTAs, but left intact retail price management agreements and other vertical restraints between chains and their franchisees. If budget hotels are disproportionately independent, then this may be the group most able to respond. Budget hotels may also be more likely to receive offline bookings than higher end properties. On the other hand, hotels in the luxury class may have the most to gain from price discrimination across different hotel channels, which is greatly facilitated by the removal of parity agreements.

In the second heterogeneity exercise, we explore how price effects vary with demand conditions. In the absence of parity clauses, hotels may set different prices across sales channels as a tool for price discrimination. For example, during times of low demand, a hotel who ends up with excess capacity may wish to offer discounts to price sensitive consumers. Consumers who book at the last minute, or through offline walk-in or phone channels, may differ in their income or the flexibility of their travel plans relative to consumers who book in advance. Parity clauses would require the hotel to offer the same discount it offers to direct booking consumers through the OTA, which may make this discounting strategy more costly (for example, if it induces future consumers to wait until the last minute). On the other

hand, a decline in anti-competitive behavior may have a larger impact when there is more room to increase markups—that is, when demand is high and there is little excess capacity.

To test these two opposing effects, we create a measure of unanticipated demand by residualizing Google search volumes for the query "<market> hotels" on month, market x class, market x month-of-year, and market x day-of-week fixed effects. We then create indicator variables corresponding to terciles of residualized search volumes, which we interact with the treatment dummy. We show the results in Table 6 in columns (3) and (6). We find generally larger price effects during high demand periods in both sets of experiments, although the treatment effects are not statistically different from each other for either set of policy experiments. This suggests that an increased ability to respond to unanticipated low demand is not driving the price declines that we measure.

5.2 Robustness

A key threat to our identification strategy is the potential for time-varying shocks to hotel markets that are unrelated to the bans of parity clauses. To investigate this, we study the dynamics of the treatment coefficient on prices using an event study specification following the local projection methods developed in Dube et al. (2023).[12] The resulting impulse response functions–together with 95 percent confidence intervals–are reported in Figures 4a (for the wide-to-narrow policy changes) and 4b (for the parity ban changes).

Prior to the wide parity bans, treated and untreated markets were on similar price trends (but for the one-month-before coefficient). After the policy, however, prices stabilize at a lower level in markets impacted by the policy, consistent with the estimates reported in Table 4, which indicate a price change of approximately 3%. Markets impacted by a full parity ban, instead, were on a upward trajectory with respect to control markets in the quarter before the policy. After the policy, however, the prices in treated markets begin to

[12]The use of local projections to estimate dynamic effects in DD settings has been shown to have several desirable properties, including simplicity and flexibility in estimating consistent causal estimates conditional on controls (Dube et al., 2023).

decline beginning in the first treated month, before settling at a level that is 3-6% lower about 12 months after policy implementation, mirroring the results in Table 5.

As an additional robustness exercise, we perform a series of placebo tests. Specifically, we take the set of European control countries in which narrow parity clauses were never banned (which include Spain, the Netherlands, United Kingdom, and Czech Republic) and simulate a set of fake policies using the implementation dates of the policy actions in France, Austria, Italy, and Belgium. We then re-estimate the pooled regression model shown in Table 6 with the fake indicators for each possible choice of control countries (a total of 24 placebo tests). We plot the treatment coefficients from each placebo test with the 95% confidence intervals in Figure 5. The coefficients are centered around zero and are statistically insignificant at the 95% confidence level for 21 of 24 placebo tests.

We also test the sensitivity of our estimates to the inclusion of different sets of controls. In Appendix Table A1, we show results that exclude occupancy as a control variable, and instead include Google search volumes as a proxy for demand. The results are qualitatively similar to the main specification, although estimated with less precision. Appendix Table A2 shows results using alternative sets of fixed effects, including using week instead of month fixed effects and dropping market x month of year and market x day of week controls. Again, while there are small differences across specifications, our findings are largely consistent with the baseline results in Tables 4-5.

Finally, as discussed in Section 2, some of the policy actions stem from actions by antitrust regulators or court rulings while others were enacted in broader legislative actions. Because of the narrower scope of rulings enacted by antitrust authorities and courts, their rulings seem less likely to have also enacted broader reforms which could have impacted the hotel sector through other channels. It is therefore reassuring that the estimated impact associated with the court-driven policy actions are qualitatively similar to those enacted through legislation.

5.3 Effects of parity agreements on occupancy

Finally, we investigate the impacts of price parity clauses on hotel occupancy. *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.

We show the results of the occupancy analysis in Table 7, which uses a similar pooled specification as the regressions in Table 6. Column (1) shows that policy changes that left narrow parity clauses in place increased occupancy by 1.7 percentage points (significant at the 95% confidence level), while column (3) shows that full parity bans had no effect. The net zero impact on occupancy in column (3) may be the result of the two offsetting forces we describe above. This is consistent with discussion among some regulators that show-rooming may be of particular concern when narrow parity clauses are banned.[13] Finally, in columns (2) and (4), we study the effect of the two sets of policy changes on revenue per room, which combines the effects of occupancy and price. We find a positive, but statistically insignificant, effect for wide-to-narrow policy changes, and a negative effect for parity bans.

5.4 Channels

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

[13]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).

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, policies that ban and wide narrow parity clauses may impact competition between OTAs in different ways.

Parity clauses may also maintain high prices by affecting competition between the intermediary and the upstream firm's direct 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. However, narrow parity clauses and full parity bans will impact these channels through different margins.

On the other hand, price parity agreements may mitigate a free-riding problem. If hotels consistently set their lowest prices in their direct channels, consumers may use an OTA to search, but book on the hotel's website, a phenomenon known as "show-rooming" (Wang and Wright, 2020). The removal of parity clauses may thus undermine incentives for OTAs to attract consumers and invest in their platforms (for example, OTA platforms may reduce

search engine advertising or marketing efforts in a country without parity agreements). If this effect increases search costs for consumers, the net result may be lower demand for hotels, pushing down both prices and occupancy.

Our empirical results show that policies that moved from wide to narrow parity decreased prices—conditional on demand—and increased occupancy, while those that banned all parity clauses decreased prices with no impact on occupancy. As we argue above, both types of regulatory changes can impact prices through inter-OTA competition and direct channel substitution (albeit through different margins). Thus, our exercise does not allow us to cleanly separate the mechanisms outlined above. However, the fact that occupancy remains unchanged after parity ban policies may point to a role for a reduction in OTA investment in those countries.

To shed more light on the mechanisms driving our results, we look at variation in the source of hotel bookings across countries. In countries that ban all types of 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 show the shares of bookings received from OTAs 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).[14] The results show that while the share of bookings from OTAs increased across the board, they increased by much less in countries that banned all parity agreements (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).[15] We interpret this as evidence that part of the price effects that we observe from parity clause bans are the result of discounting through a hotel's direct sales channels. Finally, we note that this is consistent with the findings of Hunold et al. (2018) and Ennis et al. (2023), who show that hotels set lower prices in their direct channel relative to OTAs

[14]OTA 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).

[15]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.

following the policy interventions in France and Germany.

Our empirical analysis highlights that both wide and narrow parity clauses tend to increase the prices paid by consumers. This provides empirical support for a number of mechanisms outlined in the theoretical literature. However, our analysis also provides suggestive evidence that narrow price agreements may limit "show-rooming" and preserve investment incentives for platforms.

6 Conclusion

Our 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 eliminating wide parity clauses but leaving narrow parity in place reduces prices and increases occupancy, while a full parity clause ban further reduces prices but leaves occupancy unchanged. This finding contrasts with prior studies that found limited evidence of the additional removal of narrow parity agreements (Mantovani et al., 2021).

Amid the ongoing policy debate over parity clauses, our work has implications for policymakers and antitrust authorities charged with protecting consumer welfare. It suggests that in markets characterized by vertical agreements between a platform and a network of suppliers, contractual restrictions can have important implications for downstream agents. We note, however, that the anticompetitive impact of these restrictions on prices should be weighed against possible efficiency rationales for these contracts, including preserving investment incentives by OTAs. Indeed, our mixed results on occupancy are consistent with possible reduced investment incentives by OTAs. We view additional research quantifying the impact of "show-rooming" in this market as a potential avenue for future work.

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Table 1: Policy change details

Country	Effective Date	Ruling	Notes
Germany	3/1/14	HRS ordered to removes 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 to be eliminated effective 3/1/2014 (Bundeskartellamt, 2012, 2013).
Germany	1/9/15	Booking.com and Expedia suspend 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	Booking.com and Expedia agree to eliminate wide parity but keep narrow parity clauses	Booking.com and Expedia reach 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).
France	8/8/15	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	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 on 1/31/16 (Bundeskartellamt, 2016).
Austria	11/20/16	OTAs prohibited from using all parity clauses	Austrian Parliament bans all parity clauses (11/20/16) in the Austrian Federal Act against Unfair Competition 1984 and Austrian Price Labeling Act (VBB, 2016; Chambers, 2016).
Italy	8/29/17	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	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.

Table 2: Price and occupancy rates by country

Country	ADR (Euros)	Occupancy Rate	# Rooms
Austria	86.5	0.74	31,828
Belgium	106.0	0.67	16,956
France	197.7	0.76	47,568
Germany	89.6	0.75	116,816
Italy	118.0	0.68	68,770
Netherlands	120.3	0.76	32,357
Spain	95.4	0.72	95,273
Czech Republic	66.3	0.74	32,148
United Kingdom	131.4	0.83	128,898
United States	137.4	0.76	651,275

The table shows summary statistics for our analysis sample by country. 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 covered by markets in our sample (summed across markets within a country and averaged across dates).

Table 3: Control and treatment markets

Experiment	Control markets	Treated markets	Treatment date	Time period
Germany HRS ruling	Amsterdam, Barcelona, Brussels, London, Madrid, Milan, Paris, Prague, Rome, Vienna	Berlin, Dusseldorf, Munich	3/1/14	1/1/2012-6/30/2015
Germany ban on wide parity	Amsterdam, Barcelona, Brussels, London, Madrid, Milan, Paris, Prague, Rome, Vienna	Berlin, Dusseldorf, Munich	1/9/15	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, London, 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, Milan, Munich, Prague	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, treatment dates, and sample periods for the seven policy changes that we study in this paper.

Table 4: Effects of narrow parity on prices

	(1) Germany	(2) Germany	(3) EU	(4) EU
VARIABLES	OLS Log ADR	IV Log ADR	OLS Log ADR	IV Log ADR
Germany x After March 2014 (treated)	0.001	-0.005		
	[0.008]	[0.009]		
Germany x After Jan 2015 (treated)	-0.032	-0.034		
	[0.008]	[0.008]		
Europe x After Jul 2015 (treated)			-0.029	-0.028
			[0.015]	[0.015]
Occupancy rate	0.653	1.183	0.642	0.708
	[0.036]	[0.078]	[0.036]	[0.152]
Observations	81,349	80,035	157,092	155,265
R-squared	0.981	0.090	0.983	0.239

The table shows regression estimates for the three regulatory changes during our sample period that remove wide parity agreements, but leave in place narrow parity agreements. The first two columns measure the effect of two policy changes in Germany, while the second two columns measure the effect of the Europe-wide settlement with Booking.com and Expedia. 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. Columns (1) and (3) show estimates from an OLS regression, while in (2) and (4) we instrument for the occupancy rate with lags of Google search volumes. All specifications include 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.

Table 5: Effects of parity bans on prices

	(1) France	(2) France	(3) Austria	(4) Austria	(5) Italy	(6) Italy	(7) Belgium	(8) Belgium
VARIABLES	OLS Log ADR	IV Log ADR	OLS Log ADR	IV Log ADR	OLS Log ADR	IV Log ADR	OLS Log ADR	IV Log ADR
France x After Aug 2015 (treated)	-0.036	-0.086						
	[0.015]	[0.024]						
Austria x After Nov 2016 (treated)			-0.017	-0.017				
			[0.011]	[0.010]				
Italy x After Sep 2017 (treated)					-0.044	-0.050		
					[0.018]	[0.022]		
Belgium x After July 2018 (treated)							-0.033	-0.039
							[0.018]	[0.018]
Occupancy rate	0.584	0.248	0.748	0.606	0.782	1.238	0.782	0.817
	[0.041]	[0.140]	[0.040]	[0.106]	[0.042]	[0.169]	[0.042]	[0.148]
Observations	89,400	88,371	49,019	49,019	70,720	70,720	62,414	62,414
R-squared	0.986	0.150	0.981	0.226	0.982	0.159	0.983	0.244

The table shows regression estimates for the four regulatory changes during our sample period that eliminate all parity agreements. 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. Columns (1), (3), (5), and (7) show estimates from an OLS regression, while in (2), (4), (6), and (8) we instrument for the occupancy rate with lags of Google search volumes. All specifications include 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.

Table 6: Pooled regressions - effects on prices

VARIABLES	(1) OLS Log ADR	(2) OLS Log ADR	(3) OLS Log ADR	(4) OLS Log ADR	(5) OLS Log ADR	(6) OLS Log ADR
Treated x Post	-0.032			-0.037		
	[0.013]			[0.011]		
Treated x Post x Economy		-0.135			-0.050	
		[0.027]			[0.007]	
Treated x Post x Midscale		0.031			-0.029	
		[0.022]			[0.034]	
Treated x Post x Luxury		-0.032			-0.036	
		[0.012]			[0.010]	
Treated x Post x Low demand			-0.026			-0.027
			[0.014]			[0.019]
Treated x Post x Mid demand			-0.031			-0.035
			[0.013]			[0.015]
Treated x Post x High demand			-0.045			-0.040
			[0.015]			[0.010]
Occupancy rate	0.705	0.705	0.705	0.820	0.820	0.820
	[0.038]	[0.038]	[0.038]	[0.048]	[0.048]	[0.048]
Observations	180,243	180,243	180,243	80,627	80,627	80,627
Sample	Narrow parity	Narrow parity	Narrow parity	Parity ban	Parity ban	Parity ban

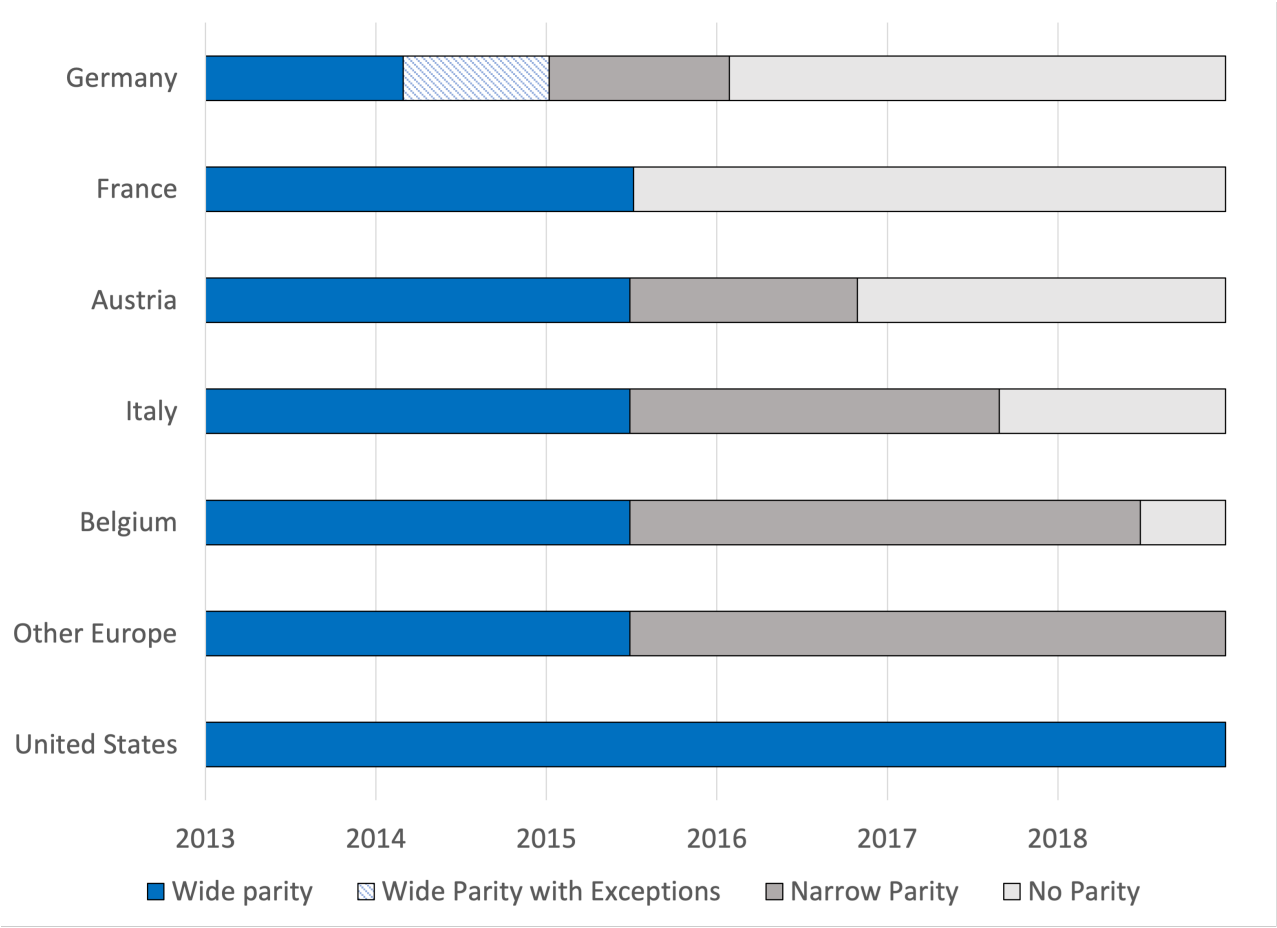
The table shows pooled regression estimates for the two sets of regulatory changes during our sample period. We describe these regulatory regime changes in more detail in Section 2. The dependent variable in each regression is the natural log of average daily price. Columns (1)-(3) show estimates using the three policy changes that ban wide parity agreements but leave in place narrow parity, while columns (4)-(6) use changes that ban all parity agreements. We use the imputation estimator developed in Borusyak et al. (2024). In columns (2) and (5), we interact the treatment with dummies for hotel class, while in (3) and (6), we interact the treatment with dummies for terciles of (residualized) Google search volume. All specifications include 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.

Table 7: Pooled regressions - effects on occupancy and revenue

VARIABLES	(1) OLS Occupancy	(2) OLS Log RevPAR	(3) OLS Occupancy	(4) OLS Log RevPAR
Treated x Post	0.017	0.014	0.003	-0.031
	[0.006]	[0.018]	[0.007]	[0.011]
Observations	180,243	180,243	80,627	80,627
Sample	Narrow parity	Narrow parity	Parity ban	Parity ban

The table shows pooled regression estimates of the effects of the two sets of regulatory changes on occupancy and revenue per available room (RevPAR). We describe these regulatory regime changes in more detail in Section 2. Columns (1)-(2) show estimates using the three policy changes that ban wide parity agreements but leave in place narrow parity, while columns (3)-(4) use changes that ban all parity agreements. We use the imputation estimator developed in Borusyak et al. (2024). All specifications include month fixed effects, market-class fixed effects, market x day-of-week fixed effects, and market x month-of-year fixed effects, as well as controls for Google search volumes for the search terms "[city name]" and "[city name] hotels". Standard errors are clustered at the market-class level.

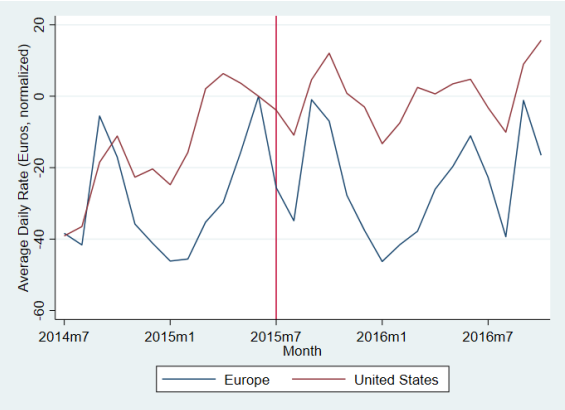
Figure 1: Timeline of regulatory environment in sample countries



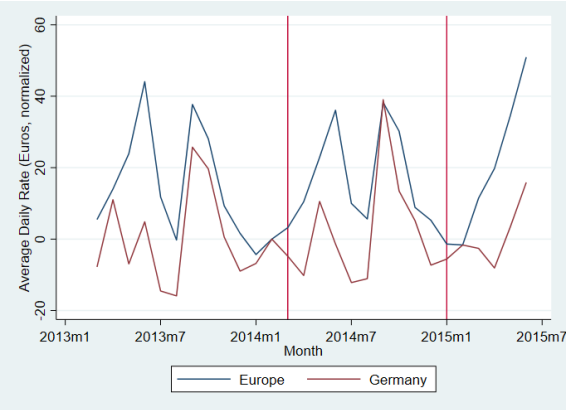
The figure shows the evolution of the regulatory environment surrounding price parity clauses in the US and Europe. We detail the evolution of the regulation of these contracts in Section 2.

Figure 2: Hotel prices after move from wide to narrow parity

(a) EU-wide Booking.com Settlement



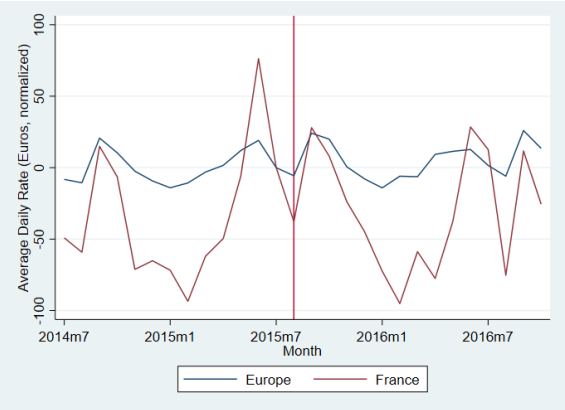
(b) Germany HRS Settlement



The figure shows average monthly hotel prices in Euros over time for all of Europe and Germany around the date that each jurisdiction banned wide parity agreements. We also plot prices in the relevant control group used in each experiment. Prices are normalized to zero in the month prior to the treatment date for both groups. The red line indicates the date of the policy change. We provide additional details in Section 4.

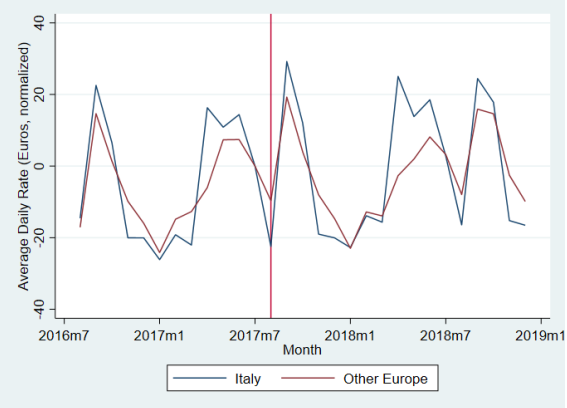
Figure 3: Hotel prices after move from narrow parity to no parity

(a) France

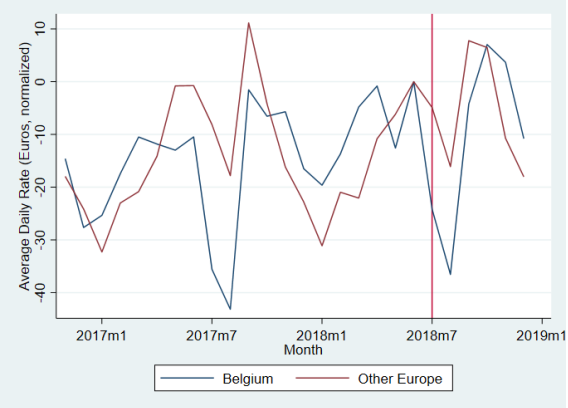


(b) Austria

(c) Italy



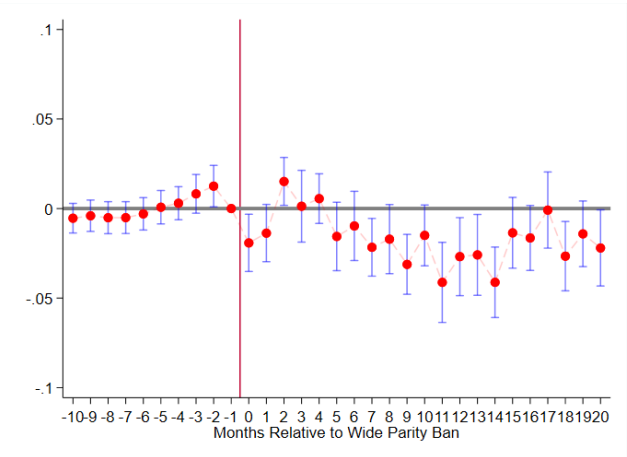
(d) Belgium



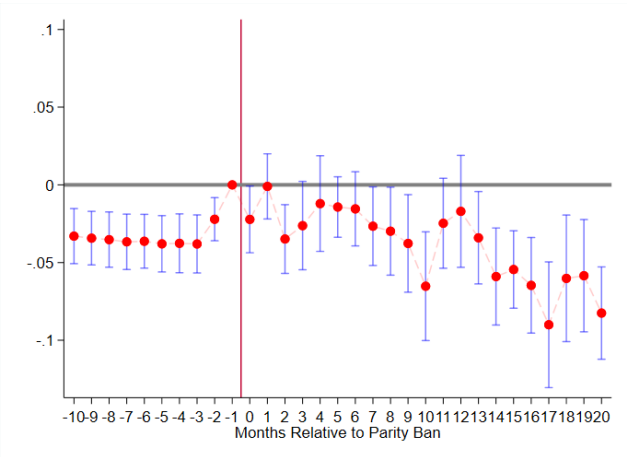
The figure shows average monthly hotel prices in Euros over time for four countries that banned all parity agreements: France, Austria, Italy, and Belgium. We also plot prices in the relevant control group used in each experiment. Prices are normalized to zero in the month prior to the treatment date for both groups. The red line indicates the date of the policy change. We provide additional details in Section 4.

Figure 4: Event study dynamic specification

(a) Wide Parity Ban (Wide to Narrow)

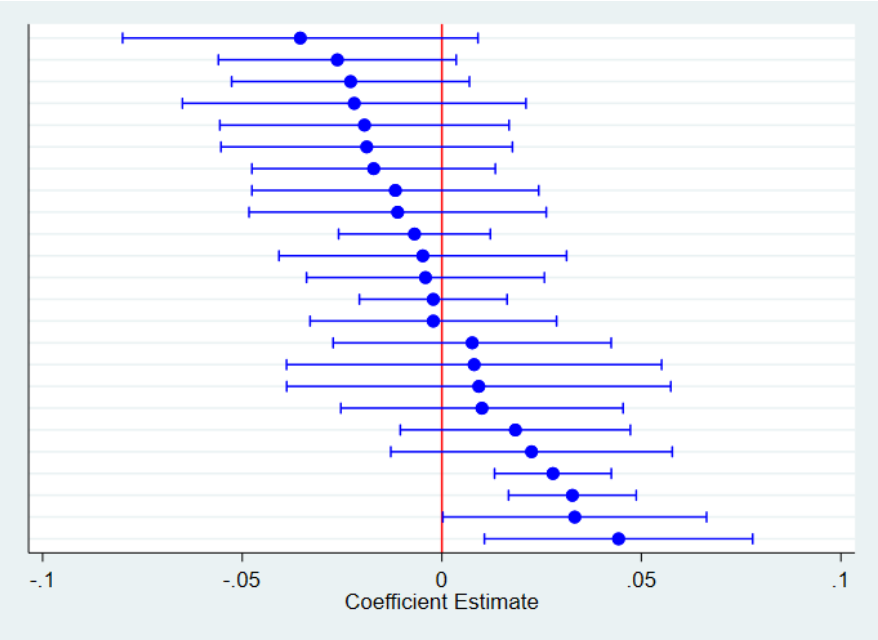


(b) Full Ban



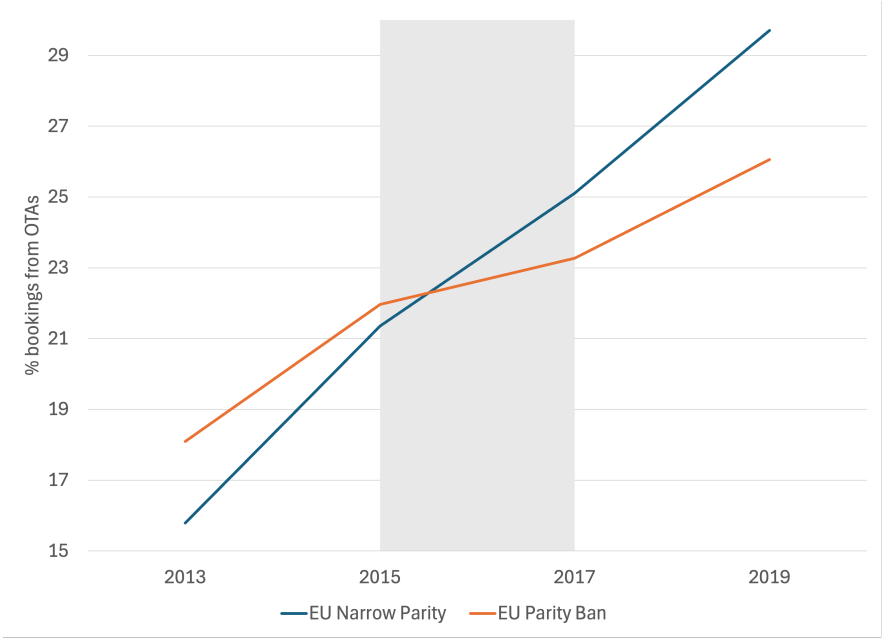
The figure presents the local projection estimates (Dube et al., 2023) of the dynamic effect of wide parity ban (panel a) or full parity ban (panel b). Daily prices are aggregated at the monthly level after being residualized using month, market-class, market x day-of-week, and market x month-of-year fixed effects. Then, the linear projection specification is estimated: for each horizon $h = -10, ...20$ the impulse response parameter β_h is estimated from the regression $p_{t+h,m} - p_{t+1,m} = \beta_h T_{t,m} + \tau_t + \gamma_h X_{t,m} + \epsilon_{t,m}$ where $p_{t,m}$ is the average (residualized) log monthly price in market m, $T_{t,m}$ is a treatment binary variable equal to one if and only if the market m was treated exactly in month t, τ_t is a month fixed effect, and $X_{t,m}$ is a set of controls, including lagged (i.e. pre-treatment) log price and occupancy rate in the market. Observations are included only if they are just treated or not treated yet up to month $t + h$ (Dube et al., 2023). 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 5: Results of placebo tests



The figure shows the results of a series of placebo tests. To conduct the placebo analysis, we simulate fake treatments in control markets using the implementation dates of the policy actions in France, Austria, Italy and Belgium. We then estimate the pooled analysis using these fake treatments. The figure reports the estimated coefficient and the associated 95% confidence interval for each placebo test. 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 Table A1: Effects of policy changes on prices - no occupancy 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 March 2014 (treated)	0.013					
	[0.008]					
Germany x After Jan 2015 (treated)	-0.024					
	[0.008]					
Europe x After Jul 2015 (treated)		-0.016				
		[0.016]				
France x After Aug 2015 (treated)			-0.131			
			[0.015]			
Austria x After Nov 2016 (treated)				-0.023		
				[0.009]		
Italy x After Sep 2017 (treated)					-0.040	
					[0.014]	
Belgium x After July 2018 (treated)						0.010
						[0.011]
Observations	81,349	157,092	89,400	49,019	70,720	62,414
R-squared	0.976	0.978	0.983	0.976	0.977	0.978

The table shows regression estimates for all of the regulatory changes during our sample period without occupancy as a control variable. 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 A2: Effects of policy changes on prices - alternative specifications

	(1)	(2)	(3)	(4)
OLS				
Germany x After March 2014	0.001	-0.005	0.002	-0.005
	(0.009)	(0.010)	(0.009)	(0.010)
Germany x After Jan 2015	-0.031	-0.039	-0.033	-0.039
	(0.008)	(0.012)	(0.008)	(0.012)
Europe x After Jul 2015	-0.028	-0.036	-0.028	-0.035
	(0.015)	(0.016)	(0.015)	(0.016)
France x After Aug 2015	-0.041	-0.029	-0.032	-0.027
	(0.015)	(0.014)	(0.015)	(0.014)
Austria x After Nov 2016	-0.016	0.003	-0.016	0.003
	(0.011)	(0.011)	(0.011)	(0.011)
Italy x After Sep 2017	-0.044	-0.045	-0.044	-0.045
	(0.018)	(0.017)	(0.018)	(0.017)
Belgium x After July 2018	-0.029	-0.058	-0.036	-0.060
	(0.018)	(0.020)	(0.019)	(0.020)
IV				
Germany x After March 2014	-0.005	-0.006	-0.004	-0.005
	(0.010)	(0.010)	(0.009)	(0.010)
Germany x After Jan 2015	-0.036	-0.038	-0.034	-0.039
	(0.008)	(0.012)	(0.008)	(0.012)
Europe x After Jul 2015	-0.028	-0.041	-0.028	-0.041
	(0.015)	(0.016)	(0.015)	(0.016)
France x After Aug 2015	-0.104	-0.056	-0.086	-0.055
	(0.029)	(0.023)	(0.024)	(0.023)
Austria x After Nov 2016	-0.016	0.001	-0.017	0.001
	(0.011)	(0.010)	(0.010)	(0.010)
Italy x After Sep 2017	-0.054	-0.050	-0.050	-0.050
	(0.024)	(0.020)	(0.022)	(0.020)
Belgium x After July 2018	-0.034	-0.067	-0.039	-0.068
	(0.018)	(0.021)	(0.018)	(0.021)
Time FE	Week	Month	Month	Month
Month of year x Market FE	Yes	No	Yes	No
Day of week x Market FE	Yes	Yes	No	No

The table shows regression estimates for all of the regulatory changes during our sample period using alternative sets of fixed effects. 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. Our baseline specification (in Tables 4-5) includes month fixed effects, market-class fixed effects, market x day-of-week fixed effects, and market x month-of-year fixed effects. Column (1) of this table shows parameter estimates using week rather than month fixed effects. Columns (2) and (3) drop month of year x market and day of week x market fixed effects, respectively, while Column (4) drops both. Panel (a) shows OLS results, while panel (b) shows results where we instrument for occupancy using Google search volumes. Standard errors in all regressions are clustered at the market-class level.