Algorithmic Pricing in Multifamily Rentals: Efficiency Gains or Price Coordination?

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Abstract

This paper evaluates the impact of algorithmic pricing on the U.S. multifamily rental market using hand-collected data on management company adoption and building-level rents and occupancies from 2005 to 2019. We find algorithmic pricing allows managers to set prices that are more responsive to changing market conditions, resulting in lower rents during downturns and higher rents during recoveries. Based on a structural model of rental demand, our pair-wise conduct test favors a model of joint profit maximization among adopters of the same software, leading to an average markup increase of $25 per unit monthly, affecting 4.2 million units nationwide.

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1 Introduction

The rise of computing technology has significantly transformed how firms set prices, particularly with the adoption of algorithmic pricing software, which often utilizes extensive internal and external data to recommend optimal prices. Moreover, these systems are increasingly powered by artificial intelligence (AI), raising concerns that they may learn to engage in collusive strategies rather than competing with each other. As a result, the issue of algorithmic collusion has gained significant attention from researchers (Calvano, Calzolari, Denicolò, and Pastorello, 2020b; Klein, 2021; Asker, Fershtman, and Pakes, 2022; Harrington, 2022), policymakers (OECD, 2017), and antitrust agencies such as the Federal Trade Commission (Mcsweeny and O'Dea, 2017) and others (Canadian Competition Bureau, 2018; Competition and Markets Authority, 2022).

The multifamily housing industry in the United States has been facing intense scrutiny regarding the issue of algorithmic pricing. Beginning in 2022, numerous class action lawsuits were filed against RealPage and management companies that use its software, alleging anticompetitive pricing in violation of the Sherman Act.[1] Following private lawsuits, in August 2024, the U.S. Department of Justice filed a civil antitrust lawsuit against RealPage, alleging that the company "engaged in an unlawful scheme to decrease competition among landlords," which resulted in a settlement in November 2025.[2] Additional litigation by state Attorneys General and against other algorithmic pricing providers such as Yardi Systems remain ongoing.

In this paper, we empirically evaluate the impact of algorithmic pricing on the U.S. multifamily rental market. To begin, we construct a novel panel of adoption status of algorithmic pricing over time for a comprehensive set of market-rate multifamily buildings. We hand-collected the adoption decisions of management companies from a variety of unstructured sources, including internet archives of industry surveys, press releases from relevant software companies, and social media profiles of management companies.

We document a rapid growth of algorithmic pricing in multifamily from 2005 to 2019. By the end of 2019, we find that approximately 25% of the apartment buildings in our dataset had adopted algorithmic pricing, representing about 10% of all rental housing, or 30% of rental units in apartment buildings with 20 or more units. We have also tagged all top twenty management companies as adopters of algorithmic pricing software. The industry structure is found to be very concentrated. Up until 2017, the market for algorithmic pricing was dominated by only two pieces of software, YieldStar owned by RealPage, and Lease Rent Option (LRO) owned by the Rainmaker group, each with comparable market shares. In December 2017, RealPage acquired LRO from Rainmaker,[3] effectively creating a monopoly with over 95% of adopters being customers of RealPage alone. That said, according to

[1] These cases were consolidated into multidistrict litigation in the U.S. District Court for the Middle District of Tennessee. See https://www.tnmd.uscourts.gov/mdl-case-information, accessed July 16, 2024.

[2] U.S. v. RealPage, Inc., No. 1:24-cv-00710 (M.D.N.C. Aug. 23, 2024).

[3] RealPage Form 8K, filed on December 4, 2017. https://www.sec.gov/edgar/browse/?CIK=1286225, accessed August 6, 2024

RealPage's own disclosures, the two systems were not immediately integrated, at least not during our sample period.[4] Thus, we proceed with our main analysis assuming that LRO continued to serve its legacy users separately.

We then merge the adoption status with an extensive dataset of rental information from Moody's REIS (Real Estate Information Services), a data provider for commercial real estate market information covering the vast majority of professionally-managed market-rate multifamily rental buildings across the US. Our sample from REIS consists of a long panel of all market-rate buildings from 2005 to 2019 across top 50 metros, comprised of 666 non-overlapping submarkets. The data includes information on both building level attributes (e.g., property address, unit-type composition, management company, and building amenities) and on annual rents and occupancies of each unit type. This data is well-suited to our study because of its long panel structure, covering periods of varying macroeconomic conditions, as well as its rich cross-sectional variations across geographical markets with varying degrees of software penetration.

Before we dive into the empirical analysis, to help interpret patterns in the data, we begin with a stylized model on how a hypothetical algorithm can influence pricing through two possible channels. (i) Algorithmic pricing can help adopters set prices optimally by overcoming existing information or pricing frictions, an example of which can be to help adopters set prices that are more responsive to changing market conditions. We call it the "responsive pricing" channel. (ii) Algorithmic pricing can also recommend adopters prices that maximize their profits jointly as opposed to individually, as though adopters were coordinating. We call it the "algorithmic coordination" channel. It is important to clarify that, when we use the word "coordination" or "coordinated prices," we simply refer to whether implied markups based on an appropriate demand are consistent with a model of joint profit maximization in a purely positive sense. We do not detect the "act" of coordination, nor do we assess them against any legal definition of price fixing or collusion in this paper.

We now consider the predictions of these two channels on observed price and quantities by comparing adopters with non-adopters in the same market. Under the responsive pricing channel, when an economic recession hits, adopters would react faster, thus charging lower prices and experiencing higher occupancy rates than non-adopters. Conversely, when an economic boom arrives, adopters would also update rents more quickly, charging higher prices and experiencing lower occupancies. On the other hand, under the algorithmic coordination channel, if the algorithm recommends prices for joint profit maximization, adopters would also charge (weakly) higher prices and experience lower occupancies than non-adopters, producing a set of predictions that is directionally the same as a model of responsive pricing during a boom. As a result, because a model of responsive pricing produces opposite predictions from that of coordinated pricing during a recession, it is straightforward to test for responsive pricing

[4] According to RealPage statement made on June 18, 2024, "LRO is also completely separate from YieldStar and AIRM [...] LRO does not have access to the YieldStar/AIRM database, and the YieldStar/AIRM database does not have access to the LRO database." Empirically, we also do not find any statistically significant relationship between the predicted changes in HHI due to the acquisition and changes in either rent or occupancy.

by examining data during a downturn. However, because these two channels produce directionally the same prediction during a boom, it is more challenging to isolate the coordination channel by running such regressions of price and quantity on adoption status.

We also consider the predictions of these two channels by comparing how adopter outcomes vary with the degree of algorithmic penetration across markets. Under responsive pricing, as the fraction of adopters in a market increases, more buildings adjust prices in the correct direction, shifting the residual demand per adopter. As a result, adopter price and occupancy move in the *same* direction with penetration. By contrast, under coordinated pricing, higher penetration makes the residual demand faced by all adopters more inelastic, allowing higher markups, so adopter prices *increase* while occupancy *decreases* with penetration. Thus, finding opposite signs on price and occupancy with respect to penetration cannot be generated by responsive pricing alone, potentially providing a way to detect coordinated pricing. However, such patterns could also arise from correlated marginal cost shocks, which is why reduced-form evidence on penetration alone cannot be interpreted conclusively without additional assumptions.

Given the insights from the stylized model, we proceed with the empirical analysis in three steps. First, we estimate building-level differences between adopters and non-adopters to identify possible evidence of responsive pricing. Our main specification estimates the time-varying treatment effects of algorithmic pricing by comparing adopters and non-adopters in the same market segment, defined by a pair of metro and building quality quartile. We include an extensive set of covariates as well as market segment indicators, capturing differential time-varying trends between higher and lower quality buildings in the same metro at a given time. Both the difference-in-differences estimator from Callaway and Sant'Anna (2021) (CSDID), which specifically accounts for staggered adoption, and traditional two-way fixed effects estimator (TWFE) show clear patterns of time-varying effects. During the Great Recession (2008-2010), adopters of the algorithm lowered rents and increased occupancy compared to non-adopters in the same market segment. Conversely, during the economic recovery (2014-2016), adopters increased rents and reduced occupancy compared to non-adopters. This pattern is robust to alternative regression specifications, market definitions, and when instrumenting a building's adoption with its management company's exposure to algorithmic pricing in other metros. Overall, given that the results consistently show that adopters charged lower rents and achieved higher occupancy rates than non-adopters during the recession, they provide strong evidence in favor of the responsiveness channel of algorithmic pricing.

Second, we assess how adopter outcomes vary with the degree of same-software algorithmic penetration across neighborhoods. We find that adopters of both YieldStar and LRO in neighborhoods with higher same-software penetration charge higher rents and experience lower occupancy. These opposite signs on price and occupancy cannot be generated by responsive pricing alone, which predicts that price and occupancy move in the same direction with penetration. In addition, cross-software penetration effects are not statistically significant, suggesting limited internalization between the two software groups. While these patterns are suggestive of coordinated pricing within the users of the same software, reduced-form comparisons alone cannot fully separate conduct from cost.

Third, to more formally test the "algorithmic coordination" channel, we estimate a full structural model of renter demand, which allows us to recover the entire matrix of demand elasticities and impose structural relationships on how markups among adopters should be determined by a given model of conduct, and therefore, to test for adopter conduct directly.

We estimate a full structural model of rental demand based on a discrete choice random utility model across a sample of ten cities. We match on aggregate shares from the REIS data and also on micro-moments from American Community Survey micro-data to capture the rich heterogeneity between household demographics and housing attributes. The estimation procedure follows Petrin (2002) and Conlon and Gortmaker (2020, 2023). The model produces sensible estimates of own-product and aggregate demand elasticities. Then, a test of conduct amounts to testing the conditional moment restrictions where the marginal cost shocks are conditionally independent of a set of excluded instruments under the correct model of conduct (Berry and Haile, 2014). Operationally, we adopt the pair-wise testing framework developed in Backus, Conlon, and Sinkinson (2021) to evaluate whether a model of coordination is more or less favored than a model of competition. Such pair-wise testing framework provides us with desirable properties against possible model misspecifications (Magnolfi and Sullivan, 2022; Duarte, Magnolfi, Sølvsten, and Sullivan, 2023). Moreover, because RealPage acquired LRO in 2017, we test the conduct of the adopters of each respective software separately, and also for the two time periods separately.

We find that the pair-wise test favors a model of joint-profit-maximization over own-profit-maximization among both adopters of RealPage and adopters of LRO prior to acquisition. The results are statistically significant and also robust to more conservative measures of cross-building ownership. For the merged entity, the test against full coordination is statistically insignificant, a result consistent with a lack of full integration even after the acquisition. Moreover, when conducting another pair-wise test for possible coordination between RealPage and LRO prior to the acquisition, the test does not find statistically significant evidence of coordination *between* users of different companies. Therefore, the overall finding is that, compared to a model of own profit maximization, our pair-wise tests favor a model of joint profit maximization among users of the same software, but not across users of different software, supporting a more contained notion of "algorithmic coordination."

Lastly, we perform a back-of-the-envelope analysis to evaluate the impact of such algorithmic coordination. We estimate the average markup difference between joint profit maximization and own profit maximization is on average \$25 per month per unit in 2019. Given that algorithmic pricing is adopted by about 4.2 million units across the U.S.,[5] this aggregates to an enormous economic magnitude,

[5] $(6.7\% + 3.7\%) \times 40.5 = 4.2$. RealPage disclosed that 6.7% and 3.7% of all rentals in US MSAs are adopters of YieldStar (and its newer product AIRM) and LRO respectively based on its statement made on June 18, 2024, https://www.realpagepublicpolicy.com/realpagestatement, accessed August 7, 2024. We estimate about 40.5 million rental units in all US MSAs based on 2022 ACS.

amounting to about \$1.5 billion annually in increased markups.

Our paper's main contribution is to empirically evaluate the impact of algorithmic pricing and test for algorithmic collusion in a high-stakes, economically-significant setting. Research on algorithmic collusion has been rapidly expanding, driven by the increasing sophistication of learning algorithms and the ease of observing competitor behavior online. In the theoretical literature, researchers have investigated how various types of reinforcement learning algorithms give rise to algorithmic collusion (Calvano, Calzolari, Denicolò, and Pastorello, 2020b; Klein, 2021; Asker, Fershtman, and Pakes, 2022; Banchio and Mantegazza, 2023; Possnig, 2024). Research in related fields such as operations research and marketing have also documented the emergence of algorithmic collusion in simulation studies under a variety of different specifications and assumptions.[6]

Despite such burgeoning theoretical literature on algorithmic pricing, empirical work assessing whether it arises in practice is notably sparse. Kühn and Tadelis (2018) and Schwalbe (2019) have argued that there may be practical constraints limiting its feasibility. To our knowledge, the only empirical research on algorithmic collusion is Assad, Clark, Ershov, and Xu (2024), which found evidence in favor of it in the context of German gasoline markets, where post-adoption prices are found to rise in duopoly markets but not in monopoly markets.[7]

We develop and extend the empirical literature on algorithmic collusion, especially Assad et al. (2024), in two important ways. First, we address the empirical challenge that algorithmic pricing can influence market outcomes through multiple channels, not just collusion, such as by providing prices that are more responsive to market conditions (Miklós-Thal and Tucker, 2019). We address this by performing a conduct test using a structural model of rental demand, estimated using detailed data on prices, quantities and choices, which allows us to impose structural relationships between implied markup and a specific model of conduct. Second, we highlight the empirical relevance of "algorithmic coordination" when a significant number of adopters use the same pricing algorithm from a common software provider.[8] We are able to address this issue because our hand-collected adoption data includes the identity of the adopting software. Since software services are often outsourced due to economies of scale, the issue of algorithmic pricing with a common third-party (Harrington, 2022) could be particularly relevant in empirical settings.

[6]This includes Waltman and Kaymak (2008); Kimbrough and Murphy (2009); Cooper, Homem-de-Mello, and Kleywegt (2015); Hansen, Misra, and Pai (2021a); Kastius and Schlosser (2022); Eschenbaum, Mellgren, and Zahn (2022); Sanchez-Cartas and Katsamakas (2022); Meylahn and V. den Boer (2022); Abada and Lambin (2023); Abada, Lambin, and Tchakarov (2024).

[7]Another related, but distinct, set of empirical work concerns the behavior of repricers, which use "automated rule mapping a rival's price to the firm's own price" (Leisten, 2024) and typically do not have any learning abilities. Brown and MacKay (2023) and Musolff (2022) have both found that the use of repricers can lead to price increases. However, such repricer models do not fit our setting, because RealPage is explicitly designed *not* to be a "comparables-based" pricing software. (See RealPage FAQ `https://www.realpage.com/storage/files/pages/faqs/pdfs/2022/10/revenue-management-faqs.pdf`.)

[8]A related literature has also investigated the issue of common subcontracting in airlines (Aryal, Campbell, Ciliberto, and Khmelnitskaya, 2024) and in marketing (Bernheim and Whinston, 1985; Decarolis and Rovigatti, 2021), whereas our focus is on algorithms that are directly used for setting prices.

Our paper also adds to the ongoing policy and regulatory debates surrounding algorithmic pricing (Ezrachi and Stucke, 2017; Spann et al., 2024; Harrington, 2024; Asil and Wollmann, 2024). While the DOJ's antitrust case against RealPage has resulted in a settlement, the broader economic questions raised by algorithmic pricing remain far from settled. Our paper provides a set of empirical frameworks that can shed light on the possibility of coordinated pricing, even in the face of other channels through which algorithmic pricing affects market outcomes. Since we do not have access to the underlying algorithms, we do not and cannot explain how such "algorithmic coordination" arises, nor can we assess its legality. However, if antitrust authorities could audit or subpoena the algorithms, as proposed in Calvano, Calzolari, Denicolò, Harrington, and Pastorello (2020a), then tests such as ours can provide a useful analytical tool that, when yielding positive results, could help meet the evidentiary threshold to warrant further investigation or be considered as a plus factor (Harrington, 2025). Additionally, our finding that algorithmic pricing can also lead to more responsive prices highlights the importance of preserving pro-competitive benefits of algorithmic pricing when considering antitrust regulations and enforcement.

Separately, our paper contributes to the literature on the impact of technological innovation in the housing market. Despite the real estate sector's slow adoption of technology in general, algorithmic pricing has become more prevalent, altering the information environment for market participants. Buchak, Matvos, Piskorski, and Seru (2022) finds algorithmic pricing deployed by iBuyers[9] produces more accurate predictions in liquid housing markets, allowing for automated intermediation. Raymond (2024) finds that algorithmic valuation can have market-level effects, closing historical racial gaps in housing value. Fu, Jin, and Liu (2023) shows that Zillow's automated valuations ("Zestimates") influence human behavior, affecting both listing and sale. Other technological solutions enabled by short-term rental platforms such as Airbnb have allowed hosts to be more "responsive to market conditions" by expanding supply more flexibly (Farronato and Fradkin, 2022).

Lastly, our paper assesses the issue of market power in real estate, traditionally arising from ownership concentration or supply constraints. In single-family rentals, while increased institutional ownership may boost rental availability (Coven, 2023), research has mostly found that ownership concentration has led to higher rents (Gurun, Wu, Xiao, and Xiao, 2022; Austin, 2022; Gorback and Qian, 2024). By contrast, our paper highlights that beyond ownership concentration, market power could also emerge from widespread algorithmic pricing adoption, where the degree of concentration in algorithmic adoption far exceeds that of ownership in many markets. Given the US multifamily sector's enormous scale, home to over 40 million renters and valued at over \$4.7 trillion,[10] even small rent increases can lead to significant transfers from renters to owners.

The remainder of the paper proceeds as follows. Section 3 provides background on the U.S. mul-

[9] iBuyers stands for "instant buyer," referring to real estate companies "that uses algorithms and technology to buy and resell homes quickly." `https://www.nerdwallet.com/article/mortgages/understanding-ibuyers`, accessed August 12, 2024.

[10] National Multifamily Housing Council, `https://www.nmhc.org/research-insight/quick-facts-figures/quick-facts-investment-returns-on-apartments`, accessed on August 6, 2024.

tifamily housing market and the pricing software used. It also describes the data collection process and shows stylized facts. Section 4 starts with a stylized model of algorithmic pricing by setting responsive or coordinated prices. We then run regressions at the building level and provide evidence that the algorithm helps landlords set efficient prices in Section 4.2. We also measure its implication on the market-level rents and occupancy in Section 4.3. To conduct a test of conduct, we describe and estimate a structural model of housing demand from renters in Section 5. The procedure and the results of the conduct test are presented in Section 6. Section 7 concludes.

2 Industry Background

2.1 Background on U.S. Multifamily Industry and Pricing Software

Multifamily housing in the U.S. is typically defined as any rental housing having five or more dwelling units.[11] Approximately 22 million out of 46 million occupied rental housing units are considered to be multifamily, and they house over 41 million people.[12] The sector has experienced fast-paced valuation growth after the Global Financial Crisis, with a 160% increase in price per square foot from the lowest in Q4 2009 to Q4 2019,[13] reaching a colossal overall valuation at $5.1 trillion.[14] In the meantime, nominal rents have also increased by about 75% in the same time period,[15] with an average of 30% of household expenditure allocated to rents.[16]

Owners, especially institutional investors, often outsource the day-to-day operation of buildings in their portfolio to a management company. The management companies then "run" the buildings, including setting monthly rents, managing leases, and performing various maintenance activities. Although the real estate sector generally exhibits limited market concentration, compared to other financial services industries such as banking and insurance (Kwon, Ma, and Zimmermann, 2024), there have been recent concerns about rising market power in rental markets (Watson and Ziv, 2021, 2025; Ramoutar, 2024). Within multifamily housing, the top 50 property management companies combined manage almost 5 million rental units, which account for a moderate 23% of the overall 22 million multifamily rental stock. That said, these management companies still manage a large number of units in absolute counts. As of 2025, Greystar, the biggest management company in the U.S., manages 950,000 units, followed by Asset Living managing 290,000 units and Willow Bridge (formerly known

[11] Fannie Mae Multifamily Selling and Servicing Guide, `https://mfguide.fanniemae.com/node/3116`, accessed on Feb 2, 2026

[12] American Community Survey 2024.

[13] FRED Economic Data, `https://fred.stlouisfed.org/series/BOGZ1FL075035403Q`, accessed on Feb 2, 2026.

[14] National Multifamily Housing Council, `https://www.nmhc.org/research-insight/quick-facts-figures/quick-facts-investment-returns-on-apartments`, accessed on Feb 2, 2026, combined with the price index from FRED.

[15] FRED Economic Data, `https://fred.stlouisfed.org/series/CUUR0000SEHA`, accessed on Feb 2, 2026.

[16] U.S. Bureau of Labor Statistics.

as Lincoln Property) managing 220,000 units.[17]

Management companies typically rely on enterprise resource planning (ERP) software systems to conduct their operation. These ERP software companies develop property management tools, providing a suite of services such as processing payments, logging maintenance requests, managing lease turnovers, monitoring vacancies, providing accounting services, etc. Besides such traditional property management services, starting early 2000s, some of the software companies started to offer real-time pricing recommendations to property managers. They are sometimes labeled as yield management, asset optimization, or AI revenue management. Since such tools amount to an automated pricing algorithm that suggests rents in real-time by unit type and lease lengths, we refer to them as "algorithmic pricing" in this paper.

RealPage, one of the ERP software companies for property management, offered a product called YieldStar, which, according to its company 10K, "[u]ses current customer and market data and statistically derived supply/demand forecasts and price elasticity models to calculate and present optimal prices for each rental unit."[18] Another major player in this market, Lease Rent Option (LRO), also provided pricing optimization for apartments from 2005 to 2017. LRO was subsequently acquired by RealPage from its parent company, the Rainmaker Group, in December, 2017.[19] Lastly, Yardi, another software company serving the real estate sector, developed a product called RENTmaximizer, although by our accounts, its market share is less than 5% in our sample.

The number of apartment units that received algorithmic pricing recommendations has grown substantially over time and consists of a meaningful fraction of the multifamily market. Back in 2011, it was reported that around 15% of apartment units had already adopted such pricing software.[20] RealPage has also disclosed that approximately 6.7% of rental properties across all US metropolitan statistical areas use its YieldStar and AI Revenue Management, and 3.7% use LRO,[21] which amounts to a total of 4.2 million units as of 2023.[22] Although it only accounts for 10.4% of overall rentals in the US, it accounts for about 21% of multifamily rentals, and even more strikingly, over 30% of buildings with 20 or more units, which are more likely to be managed professionally and users of such ERP systems.

Although the details of how exactly the software computes optimal rents is not publicly known,

[17] National Multifamily Housing Council, https://www.nmhc.org/research-insight/the-nmhc-50/top-50-lists/2025-top-managers-list/, accessed Feb 2, 2026.

[18] RealPage 2014 10K, https://www.sec.gov/Archives/edgar/data/1286225/000128622514000008/rp-20131231x10k.htm, accessed Feb 2, 2026

[19] Closing press release, RealPage Closes Acquisition of Lease Rent Options, December 4, 2017, https://www.sec.gov/Archives/edgar/data/1286225/000128622517000046/exhibit991lroclose-01.htm, accessed Feb 2, 2026.

[20] https://web.archive.org/web/20110824021635/http://www.multifamilyrevenue.com/revenue-management-users-multifamily/, accessed December 1, 2022.

[21] RealPage statement made on June 18, 2024, https://www.realpagepublicpolicy.com/realpagestatement, accessed August 7, 2024

[22] $(6.7\% + 3.7\%) \times 40.5 = 4.2$, where there are about 40.5 million rental units in all US MSAs based on 2022 ACS.

based on an FAQ published by RealPage,[23] the most notable feature is that its software does not just provide a "comparables-based" pricing, but instead it emphasizes that "a property's internal supply/demand dynamics are much more important than a competitor's rents," and as a result, it relies on estimates of demand elasticity:

> "the software uses aggregated, anonymized rent data from a variety of sources to help determine price elasticity of demand—that is, how sensitive demand is to upward or downward adjustments in price—and thus the appropriate magnitude of change in price."

A copy of RealPage's presentation slides at a housing conference also provides a glimpse of the inner workings of its pricing module YieldStar. The presentation starts with an emphasis that the software "[b]alances supply and demand via price" and "calibrates elasticity for each bedroom type," see Appendix Figure A1. Appendix Figure A2 shows the dashboard view for a property manager, which displays the price recommendations made by the software, summarizing complex information and reducing the action space for the property manager to either "Accept Rates" or "Review Rates." In addition, RealPage emphasizes that their "pricing recommendation may be followed, modified, or ignored by an apartment provider." That said, their recommendations are typically followed by its subscribers, where managers reportedly accept recommended rents 80 to 90% of the time.[24]

While it is almost certainly likely that RealPage utilizes its subscribers' data to form pricing recommendations to better forecast demand conditions, it remains unclear whether this feature achieves competitive prices that maximize individual user profits, or is used to achieve supracompetitive prices that maximize the total profits of all users combined. In other words, it is unclear whether its optimal pricing calculation uses the residual demand elasticity faced by each individual user, or the aggregate demand elasticity faced by the group of all users, which would be more inelastic.

2.2 A Lack of Immediate Post-Acquisition Integration

Although RealPage completed the acquisition of LRO in 2017, we have several pieces of evidence suggesting that there was a lack of immediate integration between LRO and YieldStar, at least not within the two years post-acquisition in our sample. Thus, we do not leverage the timing of the acquisition and induced market structure changes as the identifying variation in our empirical design.

First, in its own disclosure, RealPage acknowledged that YieldStar, as well as its subsequent product AI Revenue Management (AIRM) developed in 2020,[25] are run and maintained separately from LRO, stating that "LRO is also completely separate from YieldStar and AIRM" and "LRO does not have access to the YieldStar/AIRM database, and the YieldStar/AIRM database does not have access to

[23] Frequently Asked Questions about Revenue Management Software, Page 3-4, https://www.realpage.com/storage/files/pages/faqs/pdfs/2022/10/revenue-management-faqs.pdf accessed on December 1, 2022.

[24] https://www.propublica.org/article/yieldstar-rent-increase-realpage-rent, accessed August 7, 2024.

[25] RealPage Announces AI Revenue Management, Feb 27, 2020, https://www.realpage.com/news/realpage-announces-ai-revenue-management/, accessed Feb 2, 2026.

the LRO database."[26]

Second, based on the DOJ lawsuit, the plaintiff has also acknowledged that LRO functions differently and is run separately from YieldStar and AIRM: "AIRM and YieldStar are built upon similar code and leverage competitive data in similar ways. LRO, on the other hand, was originally developed outside of RealPage and takes a different approach," and "LRO does not require the same type and quantity of nonpublic, transactional data pulled from competitors' property management software." Operationally, "[t]here are separate user group meetings for LRO and for YieldStar and AIRM."

Third, based on the disclosed numbers, legacy LRO users also did not seem to have migrated to YieldStar or AIRM, at least not in material numbers. The disclosed number of LRO users appears unchanged from 2017 to 2023. At the time of acquisition in 2017, LRO was reported to provide "optimized pricing for over 1.5 million apartments"[27] Fast forward to May 2023, RealPage disclosed that about 3.7% of 40.5 million U.S. rental units are LRO users, which also amounted to about 1.5 million units.[28] That said, the plaintiff did also note that "RealPage has stopped offering LRO to new clients" and "has made plans to sunset both YieldStar and LRO by the end of 2024." However, this planned sunset date falls well beyond our study period.

Lastly, when we directly estimate the impact of the 2017 acquisition, we find no statistically significant impact on either price or quantity in the subsequent year based on the predicted market structure change. Specifically, since YieldStar and LRO had different pre-existing market shares, the resulting change in market structure from the acquisition also varies across markets. In other words, conditional on total algorithmic penetration, the impact of the 2-to-1 merger differs significantly between markets where YieldStar was already dominant and markets where YieldStar and LRO previously split more evenly. Thus, we consider the following estimating equation

$$\Delta y_{mt} = \beta \Delta \widehat{\text{HHI}_{mt}} + \gamma \, \text{Total Algo Share}_{m,t-1} + \mu_{mt} \tag{2.1}$$

where $\Delta \widehat{\text{HHI}_{mt}}$ denotes the predicted change in the Herfindahl-Hirschman Index in its market segment due to the merger and Δy_{mt} indicates the change in either market rent or occupancy.

Reported in Appendix Table A8, we do not find any statistically significant impact on either rent or occupancy due to the acquisition. This null result is robust when we include additional controls, such as changes in local economic conditions or metro fixed effects. The null result is also robust when we focus on the impact on adopters alone rather than on market-level averages. While this finding could be consistent with a model of no coordinated pricing within software groups, it is also consistent with a model in which no immediate integration took place within our sample period.

[26] RealPage statement made on June 18, 2024, https://www.realpagepublicpolicy.com/realpagestatement, accessed Feb 2, 2026

[27] RealPage press release on Feb 27, 2017, https://web.archive.org/web/20250329221436/https://www.realpage.com/news/realpage-to-acquire-lease-rent-options/, accessed Feb 2, 2026.

[28] RealPage statement made on Jun 18, 2024, https://www.realpagepublicpolicy.com/realpagestatement, accessed Feb 2, 2026

Therefore, given the available evidence, all of which is suggestive of a lack of immediate integration between LRO and YieldStar, we do not use the induced market structure changes at the time of the acquisition as a source of identifying variation for our main empirical design. Consequently, we focus our primary empirical analysis on the pre-acquisition period. In addition, we ensure that our main findings are robust to the alternative sample that includes the post-acquisition period, while treating the two software groups separately.

3 Data

We use two main datasets for our empirical analysis. The first dataset is from REIS by Moody's Analytics, a comprehensive database of rent and occupancy aimed at covering all market-rate multifamily buildings in the US. The second dataset documents the year of algorithmic pricing adoption by management companies, hand collected by us from various, largely unstructured, sources. In addition, we use household-level information from the American Community Survey and Data Axle to estimate our structural model of rental demand. Lastly, we supplement our analysis with property ownership information from Real Capital Analytics.

3.1 REIS

Real Estate Information Services (REIS) by Moody's Analytics aims to cover investable commercial real estate assets in the US. REIS conducts regular surveys on these buildings' owners and managers and collects information on asking rents, occupancy, concessions, and various amenities such as the presence of a doorman, elevator, parking garage, clubhouse, swimming pool, etc. It also records building level characteristics including the year built, building class (Class A vs. B/C), and the composition of unit types (e.g., the number of 1-bedroom units, 2-bedroom units etc.) REIS provides its own definitions of submarkets, assigning each building to one of 666 "submarkets" in one of 50 metros, where submarkets completely partition a metro without overlap. As such, a typical metro area contains about a dozen submarkets. Our sample from REIS contains annual snapshots of US market-rate multifamily buildings from 2005 to 2019 in the top 50 metro markets, summarized in Table 1.

There are several strengths to this dataset. First and foremost, it provides us with extensive coverage across a long panel. Our data covers a significant portion of all US market-rate apartments and the vast majority of apartment units in large buildings across these metros, including 37,216 unique buildings with 7.2 million units as of 2019. Meanwhile, there are about 17 million market-rate units nationwide in 2021,[29] suggesting our data covers approximately half of the universe of all market-rate apartment

[29] https://web.archive.org/web/20240714162515/https://multifamily.fanniemae.com/news-insights/multifamily-market-commentary/assessing-market-rate-affordable-multifamily-sector, accessed Feb 13, 2026.

units in the US at the time.[30] Moreover, the buildings captured in the REIS dataset tends to be professionally managed apartments with at least 20 units in the building. Given that there are about 8.3 million occupied rental units in such 20-plus unit structures across the same set of MSAs, it suggests that our coverage of such large apartment buildings is about 80%.[31]

Second, compared to typical scraped data of posted prices, REIS reports not only price but also quantity information, characterized in the occupancy data. The presence of both price and quantity is instrumental for our estimation of demand and the subsequent conduct test. Moreover, market-rate buildings are a particularly attractive sample because they are not subject to special subsidies or additional rent regulation.

That said, there are several limitations as well. The first is that the management company field is backfilled based on the REIS data as of 2019, making it time-invariant and ignoring prior management company changes. As the property management industry has experienced consolidation over the past decade, it can lead to an over-counting of adopters in earlier periods. Nonetheless, because such misclassification of non-adopters as adopters amounts to measurement error in the treatment variable, it leads to an attenuation bias, suggesting that our reduced-form estimates on the difference between adopters and non-adopters are likely a lower bound on the true magnitude. In terms of the conduct test, such misclassification would lead to higher predicted markup under coordination, but would not affect the predicted markup under competition, making it harder for the test to find evidence in favor of coordination relative to competition, and thus unlikely to overturn any test results in favor of coordination.

The second source of limitation is a lack of high-frequency price data. One advantage of such data is that it can shed light on additional pricing dynamics and responses to changes in competitors' prices at a higher frequency. However, given our long panel spanning over 14 years, our annual data sample still remains sufficient for us to investigate the potential impact of algorithmic pricing in the rental context as well as for our demand estimation. Another potential advantage of high-frequency pricing data is that it can be used to detect structural breaks to infer adoption in the absence of accurate adoption data (Assad, Clark, Ershov, and Xu, 2024). However, this is not a major concern for us because we were able to collect a reasonably confident data set of management companies who adopted algorithmic pricing along with *when* and *which* software each of them had adopted.

3.2 Software Adoption Data

We hand-collected the adoption data from several unstructured sources. Our first source is based on survey responses from participants at a major multifamily housing conference from 2008 to 2011. We obtained snapshots of its archived website, which maintained and updated the list of management

[30] Note that there were about 1.5 million new units constructed from 2019 and 2021, `https://www.jchs.harvard.edu/sites/default/files/reports/files/Harvard_JCHS_The_State_of_the_Nations_Housing_2020_Report_Revised_120720.pdf`.

[31] American Community Survey 2022, 7.2/(8.3/0.95) = 82% assuming an average rental vacancy rate of 5%.

companies and owners who had adopted pricing software. See Figure 1 for an example of the website snapshots. Our second source is from marketing updates. Both Rainmaker (the parent group of LRO prior to its acquisition) and RealPage had an active media presence announcing their major customer acquisitions, presumably for marketing purposes. See Figure 2 for an example of such articles from Rainmaker and RealPage respectively. Lastly, we supplement the data using manual Google searches. In select instances, management companies and their related social media sites posts information announcing their adoption of these software. Throughout this process, once we tag a management company as an adopter in a given year, we assume that the company remains as an adopter afterward.

We estimate about 25% of the buildings and 34% of the units in REIS apartments have adopted algorithmic pricing software by 2019, as shown in Table 2. The fact that the penetration at the unit level is higher than the building level suggests that larger buildings are more likely to be adopters. Moreover, we tagged all 20 out of 20 top management companies to be adopters,[32] highlighting its popularity among large, professional management companies. Indeed, Table 7 shows that adopters are more likely to be newer buildings, have more floors, and have more luxury amenities.

Figure 3 illustrates the adoption trend over time visually. Notice that the market share of RealPage and LRO were roughly comparable from 2005 to 2016 until RealPage acquired LRO in 2017. Yardi's RENTmaximizer picked up some market share starting 2015, but remained significantly smaller than RealPage at about only 1% in our sample period. As a result, we consider the market for apartment pricing software to be well approximated by a duopoly prior to the acquisition and essentially a monopoly afterwards. Table 3 summarizes the distribution of algorithmic pricing penetration across market segments, as defined by a metro quality-quartile pair. Over time, more and more market segments have shifted towards higher penetration levels, with a significant number of market segments having the majority of the buildings tagged as adopters. In Appendix Table A2, we show that the cities with the highest levels of penetration include Raleigh-Durham, Seattle, Charlotte, DC, and Austin. The cities with the lowest levels of penetration in our dataset include Columbus, Cleveland, New Orleans, Cincinnati, and Milwaukee.

The primary strength of such hand-collected data is that it allows us to record when and which software is adopted by a given management company. The natural concern is, of course, potential measurement error, in terms of both false positives and false negatives. We believe the extent of false positive is fairly small. It seems unlikely that sources such as public media updates or survey respondents from management companies would be mischaracterizing their adoption status. Moreover, based on RealPage's public disclosure, its users renewed at an average rate of 96.5% in 2019 and 96.6% in 2020,[33] suggesting our assumption on continued usage upon adoption is reasonable. Such renewal patterns seem sensible because the integration of a software product into a company's operation often requires organizational and personnel changes that are hard to reverse.

[32] This list is shown in Appendix Table A1, based National Multifamily Housing Council (NMHC) ranking, `https://www.nmhc.org/research-insight/the-nmhc-50/top-50-lists/2019-managers-list/`, accessed August 8, 2024.

[33] RealPage 2019 and 2020 10K.

Next, while it is reasonable to be concerned about false negatives, we perform a benchmark analysis based on disclosed adoption counts from RealPage, suggesting the scope of false negatives to be limited. As mentioned before, RealPage has disclosed that 6.7% and 3.7% of all rentals in US MSAs are adopters of YieldStar (and its newer product AIRM) and LRO respectively, suggesting a total of 4.2 million adopted units as of 2023. If we apply a similar adoption rate for our sample of 50 metros, it would imply a total of 2.7 million adopted units. Assuming a moderate organic growth rate of YieldStar[34] and no growth of the legacy LRO, it would imply a total of 2.55 million adopted units in 2019, which is quite close to our estimate of 2.42 million units in the same set of metros.[35] Similarly, since LRO is reported to account for about 3.7% of all rentals, it implies approximately 944,000 units in these 50 metros, which is also quite close to our estimate of 888,000 units prior to the acquisition in 2016, as shown in Table 4. Besides the benchmark analysis, it is also reassuring that our data correctly identifies the adoption status of management companies involved in recent class action lawsuits.

3.3 Additional Data Sources

Besides REIS and adoption status, we supplement our analysis with a few additional sources.

Real Capital Analytics (RCA) We obtain a panel of ownership of individual buildings in ten select metros from Real Capital Analytics (RCA), which keeps track of commercial real estate deals that are over $2.5 million in value.

American Community Survey (ACS) We use ACS 1-year micro-level data from 2009 to 2019. For household demographics, we obtain information such as household income, household size, presence of children, and the age of the household head. For housing attributes, we obtain information such as contract rent, number of bedrooms, age of the building, and type of building (units in structure). We focus on households who are renters.

Data Axle (formerly Info Group) provides an anonymized sample of households with exact address information, allowing us to identify the households who choose to live in a REIS building. Because the Data Axle sample is not guaranteed to be representative across the US, unlike the ACS, and the fact that household demographic information is relatively limited, we use only Data Axle to generate additional micro-moments on correlations to augment the demand estimation.

[34] We assume an organic growth rate of 3.3% based on RealPage's public disclosure where its 6.7% increase is "primarily attributable to our 2020 acquisitions, which accounted for approximately 3.3% of total ending on demand units, as well as organic unit growth". Source: RealPage 2020 10K.

[35] $(6.7\%/(1+3.3\%)^4+3.7\%)\times 25,514,472 = 2,550,970$, where the total number of rentals is based on 2022 American Community Survey.

4 Reduced-Form Estimates of the Impact of Algorithmic Pricing

4.1 Stylized Model

Before delving into the regressions, in this subsection, we first present a stylized, verbal model of how algorithmic pricing could affect market outcomes through various channels. We consider at least two possible, non-mutually exclusive channels: responsive pricing and coordinated pricing. By "responsive pricing," in this paper, we focus on the idea where the pricing algorithm can help set prices that are more responsive to changing underlying demand conditions.[36] By "coordinated pricing," we focus on the case where adopters of algorithmic pricing charge prices that maximize their profits jointly as opposed to individually, as though adopters were coordinating.

A Stylized Model of Responsive Pricing Consider a multifamily rental market with a set of buildings who each has a fixed capacity and a marginal cost curve that asymptotically approaches infinity at capacity. In a simple two-period model, we start with a market in equilibrium at $T = 0$. Then, in $T = 1$, demand conditions change. Due to various frictions (e.g., cost of information acquisition, menu costs, or agency frictions), when faced with a demand shock, non-adopters appear "sleepy," in that they may be slow to recognize the change, and are slow to update their prices. On the other hand, adopters are much more "alert," in that the algorithm more promptly recognizes changes in market conditions and updates their prices more swiftly.

Figure 4 provides a simple illustration. Panel (a) illustrates the market dynamics with a negative demand shock. When faced with a contraction in aggregate demand, non-adopters charge a stale price based on an outdated equilibrium price from the previous period that is otherwise too high, resulting in excess vacancies (i.e., under-production). By contrast, adopters are able to lower their prices much more rapidly with help from algorithmic pricing. As a result, the shaded area denotes the net efficiency gain when algorithmic pricing leads to more responsive prices. Such model of responsive pricing predicts that, (i) when faced with a negative demand shock, adopters would charge lower prices and experience higher occupancies than non-adopters in that market.

Conversely, Figure 4 Panel (b) illustrates the market dynamics with a positive demand shock. In this case, non-adopters are slow to increase their prices when faced with stronger demand, leading to too few vacancies. Similarly, adopters hike their prices promptly as the algorithm is faster at recognizing

[36] Note that researchers may also use the phrase "responsive pricing" to refer to a different type of responsiveness, namely when algorithmically-set prices become more "responsive" to changes in rival's prices such as Brown and MacKay (2023, 2026). In our setting, we instead focus on responsiveness to changing demand for several reasons: First, fluctuating demand conditions due to national, local, and seasonal housing demand shocks constitute an important, practical challenge in setting rents faced by landlords, and it is plausible that a dedicated third-party pricing algorithm could be better at detecting such changes than individual landlords. Second, while it is entirely conceivable that the pricing algorithms in our setting also improve responsiveness to rival prices, our annual panel on rents and occupancies is ill-positioned to answer this question conclusively. Third, it seems plausible that responsiveness to changing demand is more likely to constitute an efficiency gain worth investigating, whereas the welfare impact of responsiveness to rival prices is theoretically ambiguous.

the emergence of a booming market. As such, the same model of responsive pricing makes opposite predictions, namely, (ii) when faced with a positive demand shock, adopters charge higher prices and experience lower occupancy than non-adopters in that market.

In addition to the comparison between adopters and non-adopters *within* a market, such a model of responsive pricing also generates comparative statics with respect to the degree of algorithmic penetration *across* markets. Under a negative demand shock, as the fraction of adopters in a market, denoted by h, increases, the fraction of under-producing non-adopters declines, thus shrinking the residual demand per adopter and driving down their price and occupancy.[37] As $h \to 1$, the price and occupancy of adopters lower to a level that fully restores the competitive equilibrium, effectively tracing down the supply curve, as shown by the blue arrow in Figure 4 Panel (a). Conversely, under a positive demand shock, as h increases, the fraction of over-producing non-adopters declines, thus driving up the price and occupancy of adopters, shown by the brown arrow in the bottom Panel (b).

Therefore, in a model of responsive pricing, comparing across markets, (iii) when faced with the same negative shock, adopter price and occupancy both decrease with algorithmic penetration, and (iv) when faced with the same positive shock, adopter price and occupancy both increase with algorithmic penetration.

A Stylized Model of Coordinated Pricing Next, we consider a model of coordinated pricing, where the algorithm helps its adopters to set prices to maximize their joint profits.

For simplicity, consider the case where there is only one software company and we also shut down the responsive pricing channel for now. In this case, the algorithm allows the group of adopters to set prices to maximize their joint profits, whereas non-adopters set prices individually to maximize their own profits.

When the degree of algorithm penetration $h = 1$, the price that maximizes joint profits amounts to monopoly price. In a simple model with homogeneous products, the optimal markup equals the inverse demand elasticity of the aggregate demand ϵ_D, and the monopoly quantity is lower than the competitive quantity. When the fraction of adoption $h < 1$, the optimal markup equals the inverse demand elasticity of the residual demand ϵ_D^A faced by the group of adopters.[38] As the degree of algorithmic penetration h increases, the residual demand faced by adopters becomes more inelastic, allowing them to charge higher markups and higher prices. Non-adopters respond by charging a just-so-slightly lower price and not withholding quality.

Therefore, a model of coordinated prices predicts that, (v) within a market, adopters charge (weakly) higher prices and experience lower occupancy than non-adopters, and (vi) across markets, adopters in

[37]Said differently, when markets are experiencing negative demand shocks, a greater degree of algorithm penetration means that more buildings adjust their prices down quickly in that market, leading to a more contracted residual demand curve per adopter.

[38]See Appendix Section C for more details of the derivation. In addition, in a model with differentiated products, a similar pricing formula arises as we show later in Eq (6.8).

markets with high algorithmic penetration charge on average higher prices and have lower occupancy.

Implications for Conduct Table 6 summarizes the main predictions produced by different models of algorithmic pricing. The key insights are two-fold:

1. *A comparison between adopters and non-adopters within a market* can isolate responsive pricing but not coordinated pricing. During downturns, responsive pricing predicts that adopters charge lower prices and experience higher occupancy, while coordinated pricing predicts higher prices and lower occupancy regardless of market conditions. During booms, both models predict higher prices and lower occupancy for adopters. Thus, finding lower prices and higher occupancy for adopters during recessions *is* indicative of responsive pricing, but finding higher prices and lower occupancy during booms is *not* indicative of coordination.

2. *A comparison of adopters across markets with varying levels of penetration* can potentially be indicative of coordinated pricing, assuming levels of penetration are not correlated with unobserved cost shocks. Responsive pricing predicts that adopter price and occupancy move in the *same* direction with penetration h, while coordinated pricing predicts they move in *opposite* directions (price increasing, occupancy decreasing with h). Thus, finding that adopter price increases while occupancy decreases with penetration cannot be generated by responsive pricing alone.[39] However, such patterns could also arise from correlated marginal cost shocks, which is why hypotheses about conduct cannot be separated from hypotheses about marginal cost without appropriate exclusion restrictions.

Thus, we proceed with the empirical analysis in three steps.[40] First, we estimate the difference between adopters and non-adopters within the same market to identify evidence of responsive pricing. Second, we assess how adopter price and occupancy vary across markets by algorithmic penetration, with the important caveat that the findings there cannot be interpreted conclusively without additional assumptions on marginal cost shocks. Finally, we estimate a structural model of housing demand from renters and perform a test of conduct to evaluate the "algorithmic coordination" channel.

[39] Note that algorithmic pricing could reduce other sources of pricing friction beyond responsive pricing, but fixing those also tend to generate predictions where adopter price and occupancy still move in the *same* direction with penetration. For instance, a common pricing heuristic used in the real estate industry is to "keep the building full," which can result in lower-than-optimal prices in certain cases, given some locational monopoly power is naturally present. In this case, a model of algorithmic pricing that helps adopters raise rents to the optimal markup could also result in adopters having higher prices and higher occupancy as penetration increases, because algorithmic adoption reduces the number of overproducing firms in the market.

[40] In addition, beyond the comparisons outlined above, the stylized models can generate additional predictions regarding other moments of the data. However, when we iterate through various possible predictions on adopters, non-adopters, their differences, and their aggregates, we do not derive additional insights beyond what are listed here.

4.2 Within-market Comparisons between Adopters and Non-adopters

In this section, we compare adopters' prices and quantity with non-adopters in the same market across varying market conditions as a possible strategy to examine the responsive pricing hypothesis.

First, we offer some suggestive evidence by examining the difference between adopters and non-adopters over time across adoption cohorts. To see how the impact of algorithmic pricing varies by market conditions, we include both cohorts of buildings that adopted algorithmic pricing before the Great Recession and cohorts that adopted after the recession. We employ a dynamic two-way fixed effect (TWFE) specification as follows

$$y_{jt} = \sum_{\substack{\tau=-5 \\ \tau\neq-1}}^{5} \beta_\tau^Y \, \mathbb{1}\{t = Y + \tau\} \, a_{j,t} + \beta X_{jt} + \theta_{mqt} + \phi_j + \mu_{jt}, \tag{4.1}$$

where $a_{j,t}$ is a binary indicator for the adoption status of building j in year t, X_{jt} are building-level covariates (e.g., building age and other time-varying amenities), and β_τ^Y are our coefficients of interest. The outcomes of interest y_{jt} include the log of asking rents and the occupancy rate of building j in year t. We include building fixed-effects ϕ_j to account for persistent building-level quality. We also include time-specific market segment fixed effects θ_{mqt}, where market segments mq are defined by metro and quality-quartile pairs.[41] The inclusion of such rich fixed effects θ_{mqt} accounts for differential trends for buildings of different quality tiers in the same metro. In other words, it allows for the possibility that luxury buildings may be experiencing faster rent growth than non-luxury buildings in the same metro in a given year. While cohort-level analysis naturally results in identical treatment timing, we also consider the Callaway and Sant'Anna (2021) difference-in-differences estimator (CSDID) as it allows covariates and market segment effects to enter more flexibly.

Figure 5 presents the cohort-specific estimates for the four largest adoption cohorts: 2008, 2009, 2010, and 2011. For the early adoption cohorts (2008 and 2009) who adopted just before or during the Great Recession, we observe that adopters charged *lower* rents than non-adopters in the years immediately following adoption, consistent with adopters lowering prices in response to weakening demand. By contrast, for later adoption cohorts (2010 and 2011) who adopted during the recovery period, adopters charged higher rents than non-adopters for the entire post-adoption period. The occupancy results in Appendix Figure A3 paint a directionally consistent picture, namely, cohorts who adopted prior to the recession experienced some increase in occupancy after adoption relative to non-adopters, whereas cohorts who adopted post-recession only saw a reduction in occupancy post adoption, though these estimates are statistically weaker. As such, these charts are suggestive of the presence of the responsive pricing channel, as evidenced by lowering rents to gain occupancy during the downturn and increasing rents during the upturn.

Next, to leverage the full sample, our main specification estimates the time-varying treatment effects

[41] A building's quality quartile is determined by its rent quartile in a given metro at the beginning of the sample.

by calendar year from 2007 to 2019. Analogously, we can estimate the effects via traditional two-way fixed effects (TWFE) as follows

$$y_{jt} = \sum_{\tau=2007}^{2019} \beta_\tau \mathbb{1}\{t=\tau\}\, a_{j,t} + \beta X_{jt} + \theta_{mqt} + \phi_j + \mu_{jt}. \tag{4.2}$$

However, given that adoption is staggered over time, we prefer the Callaway and Sant'Anna (2021) difference-in-differences estimator (CSDID) rather than traditional two-way fixed effects (TWFE) for estimating the treatment effects by calendar-year. CSDID avoids the bias that could arise when TWFE uses already-treated units as controls, and instead estimates cohort-specific average treatment effects on the treated, using never-treated (and not-yet-treated) units as valid controls.

Importantly, to ensure that we compare adopters and non-adopters within the same market segment, our CSDID estimator conditions on segment indicators, which are defined as metro by quality-quartile pairs, effectively comparing adopters and non-adopters within the same metro-quality segment. In addition, we also include building characteristics as covariates,[42] thus allowing for these characteristic-specific time trends as well.

Figure 6 summarizes the main results. During the Great Recession from 2008 to 2010, and especially during the deepest part of the recession in 2009, adopters charged lower rents and experienced higher occupancy than non-adopters. After the recession, especially after 2013, adopters charged higher rents and experienced more vacancies than non-adopters. Estimates using TWFE in our setting are similar to CSDID estimates, with slightly tighter standard errors. Given our discussion before, the finding that adopters charged lower prices and experienced higher occupancies compared to non-adopters during the recession provides strong evidence in support of the responsive channel of algorithmic pricing.

Our main result is robust to a variety of alternative specifications, including alternative definitions of market segment such as metro-class or submarket (Appendix Figure A4). While our main specification uses never-adopters as the control group, our finding remains robust when we also include not-yet-adopters in the control group (Appendix Figure A5). Furthermore, although statistically weaker in later years, our main finding is also robust to a comparison using only ever-adopters, thus comparing the behavior of existing adopters and not-yet adopters (Appendix Figure A6).

The validity of difference-in-difference methods rests on parallel trends, which are inherently untestable. However, here are a few considerations that improve its plausibility. First, in our CSDID specification, we include rich covariates for both building characteristics and narrow segment indicators, thus requiring only conditional parallel trends by accounting for flexible time trends that vary by these covariates and market segments. Second, we can examine both cohort-specific and aggregate pre-trends in our event-study estimates. Among cohort-level results in Figure 5, they do not appear to exhibit pre-trends. More formally, Appendix Figure A7 aggregates all adoption cohorts and plots

[42] The list of building characteristics includes building age, number of floors, and indicators for having a doorman, parking garage, pool, tennis court, and clubhouse.

the treatment effect across event time, showing no evidence of pre-treatment trends in either rent or occupancy. Given that the dynamic treatment effects can vary in both sign and magnitude across cohorts, as shown in our cohort-level estimates, we focus on the pre-treatment period when assessing parallel trends rather than on the post-treatment dynamics.

That said, to the extent that there may still be concerns that adoption decisions are endogenous and thus the parallel trend assumptions may not hold, we also implement an instrumental variable strategy leveraging the notion that the adoption decisions are typically made at the management company level rather than at the individual building level. All of the top 20 management companies operate across multiple states, so it is plausible that these adoption decisions are not driven by any one specific time-varying condition of a building. We expect management companies that are exposed to metros with higher shares of adopters to be more likely to adopt the software, driving buildings under their portfolio in other metros to become adopters. As such, the extent of software penetration in other metro markets in which a management company operates is likely relevant for a company's adoption status but could be viewed as plausibly exogenous to the local market conditions of the focal building.

We construct the instrument for the adoption status of a given building j in a metro M based on the extent of overall algorithmic penetration in *other* metros $M' \neq M$ that its management $c = c_j$ company operates, weighted by the importance of that market M' to the company based on its portfolio share,

$$Adopt^{IV}_{c,M,t} = \sum_{M' \neq M} \frac{{}_{c' \neq c} N^{A}_{c'M't}}{{}_{c' \neq c} N_{c'M't}} \times \frac{N_{cM't}}{{}_{M'' \neq M} N_{cM''t}} \tag{4.3}$$

where N_{cMt} denotes the number of buildings managed by c in metro M in year t, the superscript A denotes the number of adopters, and $N_{cM''t}$ denotes the total number of buildings managed by c across all metros outside of the focal metro. The variation of the instrument is at the management company-metro-year level. Table 8 summarizes the estimated coefficients on price and occupancy for all three specifications (TWFE, 2SLS, and CSDID). For the 2SLS specification, the instrument is strong with a first stage partial F stat at 55.1. Overall, we find consistent patterns across all three specifications, where adopters charge lower prices during busts and higher prices during booms.

Lastly, to the extent that the estimated pricing impact is driven by the intensity of the market shock, we also conduct a heterogeneity analysis on the estimated coefficient by changes in market-level economic fundamentals. Specifically, based on the main specification Eq (4.2), we add an additional interaction term between the adopters $a_{j,t}$ and the underlying economic conditions at the time in a metro $Z_{M,t}$. Table 9 summarizes the findings, where we find that, compared to non-adopters in the same market segment, adopters' prices are lower in metros with larger increases in unemployment rates, larger decreases in household income, and larger decreases in the home price index, which lends further support that algorithmic pricing recommends prices that are more responsive to market conditions.

4.3 Across Market Comparison of Adopter Outcomes

Next, we turn to how adopter outcomes vary with the degree of algorithmic penetration across neighborhoods. Recall from Table 6 that coordinated pricing predicts that adopter prices increase and occupancy decreases with penetration h, whereas responsive pricing predicts that price and occupancy move in the *same* direction. Thus, the sign pattern on both outcomes can potentially distinguish the two channels.

The key source of variation we exploit is cross-neighborhood differences in algorithm penetration within the same metropolitan area. We do not use variation across metros, as demand conditions are difficult to hold comparable across different metropolitan areas. Instead, all regressions include segment-year fixed effects, where a segment is defined as a metro-by-rent-quartile pair, so that we absorb common shocks at the market-segment level. Identification then comes from the fact that, conditional on being in the same segment and year, neighborhoods differ in their degree of algorithmic penetration. We verify that there is indeed substantial variation at the local level: Figure 7 plots the distribution of tract-level and zip-level penetration shares for each software group, showing a wide range of penetration especially at the tract level.

Our main specification is

$$y_{jt} = \beta^{YS}\, h^{YS}_{n(j),t} \cdot \mathbb{1}\{j \in YS\} + \beta^{LRO}\, h^{LRO}_{n(j),t} \cdot \mathbb{1}\{j \in LRO\} + \phi_j + \theta_{mqt} + \mu_{jt}, \tag{4.4}$$

where y_{jt} is the log rent or occupancy rate of building j in year t, $h^{s}_{n(j),t}$ is the algorithm penetration share of software s in neighborhood $n(j)$ (tract or zip code),[43] ϕ_j are building fixed effects, and θ_{mqt} are segment-year fixed effects. The inclusion of both building and segment-year fixed effects means that identification comes from differential changes in local penetration across neighborhoods within the same segment over time.

Table 10 presents the results. In the first four columns, which use tract-level penetration, we find that adopters in tracts with higher same-software penetration charge significantly higher rents and experience significantly lower occupancy. The last four columns use zip-level penetration; results are directionally similar but statistically weaker, consistent with zip codes being a coarser geographic unit with less variation. These results are robust to alternative definitions of market segment such as metro-class or submarket (Appendix Table A7).

In Table 10, we also include specifications with cross-software penetration terms (even columns within each group), which asks whether a YieldStar building's outcome responds to the penetration of LRO in the same neighborhood, and vice versa. The cross-software coefficients are not statistically significant. While this is not a formal test of cross-software coordination, it speaks against the hypothesis that the two software groups coordinate with each other; we return to this question with a

[43] We also restrict the sample to tracts (or zip codes) with more than one building, as the effect of neighborhood penetration is difficult to interpret for a building that is the sole occupant of its tract.

formal structural test in Section 6.

Lastly, we also estimate the penetration effect year by year by interacting the penetration shares with year indicators. Figure 8 plots these year-by-year coefficients for both YieldStar and LRO. The coefficients on log rent are consistently positive across nearly all years and for both software groups, while the coefficients on occupancy are consistently negative, reinforcing that the results in Table 10 are not driven by any particular sub-period.

The joint finding that adopter rents increase while occupancy decreases with penetration can be viewed as potentially indicative of coordinated pricing, but with some important caveats. First, the opposite signs on rent and occupancy cannot be generated by responsive pricing alone, which predicts that price and occupancy move in the *same* direction with penetration, as described in the stylized model in section 4.1. Second, the inclusion of both price and occupancy as outcomes helps address a natural concern about omitted demand shocks: markets that are growing are more likely to have more algorithm adopters (e.g., more new buildings), which could generate a spurious positive correlation between penetration and rents. However, if higher penetration were simply proxying for stronger unobserved demand, we would also expect higher occupancy in high-penetration neighborhoods. The fact that we instead find *lower* occupancy is difficult to reconcile with this alternative explanation, and is more consistent with adopters pricing closer to joint-profit maximization.

That said, interpreting these patterns as evidence of coordination requires that penetration is plausibly exogenous to marginal cost shocks. While it seems unlikely that a focal building's marginal cost responds to the adoption decisions of other buildings in the same neighborhood, we cannot fully rule this out. More broadly, as emphasized by Berry and Haile (2014), hypotheses about conduct cannot be tested separately from assumptions about marginal cost without formal exclusion restrictions. This motivates our structural approach in the next section, where we estimate a full model of housing demand and use instruments that can capture the markup differences from different models but are otherwise excluded from cost shocks, allowing us to test the coordination hypothesis more formally and efficiently.

To summarize, the comparison between adopters and non-adopters within the same market provides evidence of responsive pricing, as adopters charged lower rents and experienced higher occupancy during the Great Recession. However, such within-market comparisons cannot readily isolate the coordination channel. On the other hand, the comparison of adopters across neighborhoods with varying levels of penetration reveals that adopter prices are increasing and occupancy is decreasing in the degree of algorithmic penetration, which could be indicative of the presence of coordinated pricing. However, given the fundamental challenge remains that such reduced-form comparisons cannot separate conduct from cost, we turn to a structural approach in the next section.

5 A Structural Model of Renter Demand For Multifamily Housing

In this section, we estimate a structural model of rental housing demand. We estimate the demand side while remaining agnostic to the model of competition among multifamily buildings on the supply side. Compared to the regressions of price and quantity on adoption status conducted in the previous section, a full structural model of renter demand incorporates additional moments from household characteristics and choices, allowing us to recover the entire matrix of demand elasticities and thus test for conduct.

5.1 Demand Model

We model renters' decisions as a multinomial discrete choice problem across differentiated rental housing units, following the literature in McFadden (1977); Bayer, Ferreira, and McMillan (2007); Calder-Wang (2021). We first lay out a general model of the renter's choice before proceeding to specify the empirical implementation of the model. We consider a renter's choice set as all the rental units in a submarket m and in year t. We define "product" $j(b)$ as bedroom-type b in building j, which we call "unit type" in short. For brevity, we use j instead of $j(b)$ unless otherwise noted. We use jt instead of jmt to denote a product-market pair since a building is unique to a geographical market.

We specify household i's utility from unit type j in market t as:

$$u_{ijt} = \alpha_i p_{jt} + \boldsymbol{X_{jt}} \boldsymbol{\beta_i} + \xi_{jt} + \epsilon_{ijt} \tag{5.1}$$

where p_{jt} denotes the monthly rent of the unit, which we interchangeably call "price." ξ_{jt} denotes the unobservable quality or demand shock to the unit type. $\boldsymbol{X_{jt}}$ is a row vector of observable attributes of the unit type, such as the number of bedrooms, building age, building type, and building class. $\boldsymbol{\beta_i}$ is a vector of households' preferences for the vector observable attributes in $\boldsymbol{X_{jt}}$, and α_i is the price coefficient. These preference parameters vary at the household level, modeled as a function of a set of renter demographic characteristics, such as household income y_i, age of the household head, household size, and household type. We consider the outside good $j = 0$ all other non-REIS rental units in the submarket. The idiosyncratic taste parameter ϵ is i.i.d. type 1 extreme value. A renter chooses a unit type $j \in \mathcal{J}_{mt} \cup \{0\}$ to maximize their utility u_{ijt}, where $\mathcal{J}_{mt}$ denotes the set of unit types present in submarket m in year t. The probability of renter i choosing to live in $j \in \mathcal{J}_{mt} \cup \{0\}$ is

$$s_{ijt} = \frac{\exp(\alpha_i p_{jt} + \boldsymbol{X_{jt}} \boldsymbol{\beta_i} + \xi_{jt})}{1 + \sum_{j' \in \mathcal{J}_{mt}} \exp(\alpha_i p_{j't} + \boldsymbol{X_{j't}} \boldsymbol{\beta_i} + \xi_{j't})}. \tag{5.2}$$

5.2 Demand Estimation Procedure

Our estimation goal is to recover parameters governing the demand system $\theta^D := (\boldsymbol{\alpha}, \boldsymbol{\beta}, \boldsymbol{\xi})$. We follow the literature in estimating a multinomial logit demand system with heterogeneous tastes (Petrin, 2002; Berry, Levinsohn, and Pakes, 2004; Conlon and Gortmaker, 2020, 2023).

In terms of model specification, we assume the unobserved quality of a unit type can be decomposed into

$$\xi_{jt} = \xi_j + \xi_{mt} + \tilde{\xi}_{jt} \tag{5.3}$$

where ξ_j is unit type-level fixed effects, capturing the time-invariant vertical quality of a building and its unit types, and ξ_{mt} denote submarket-year fixed effects, capturing time-varying demand shocks at the submarket level. For the part of the renter utility that allows for heterogeneity in taste, we rewrite $\alpha_i = \alpha_0 + \alpha_y \log(y_i)$ so that price elasticity may vary with household income. The estimation of these parameters is achieved by two sources of variations: (1) the observed market shares of unit types and their prices and (2) the joint distribution of building attributes and renter demographics.

The first set of moments helps to identify demand parameters by inverting the observed market shares based on building-level price and occupancy data from REIS (Berry, 1994; Berry, Levinsohn, and Pakes, 1995). Moreover, the share of the outside option is also observed directly because we observe the total number of rental units in each market from the ACS. Since price is likely endogenous, it requires a set of instruments z^D to be mean independent of unobserved demand shock $\tilde{\xi}_{jt}$. As such, we construct a sample analog to the moment condition:

$$\hat{G}_1(\theta^D) = \frac{1}{N_{jt}} \sum_{j,t} \hat{\tilde{\xi}}_{jt}(\theta^D) z^D_{jt}. \tag{5.4}$$

Given that we already include building-level fixed effects ξ_j in our model, the candidate instruments in z^D should be uncorrelated with time-varying demand shocks that are specific to buildings or neighborhoods. An example of such endogeneity in our context could be changes in the neighborhood amenities at a given time, such as the addition of a nearby park, raising the rents of surrounding buildings.

To address such endogeneity, we use the "differentiation IVs" proposed by Gandhi and Houde (2019). The intuition is that a product's markup depends on how close its rivals are in characteristics space:[44] products with many close substitutes face stronger competitive pressure and lower markups, while more isolated products can sustain higher prices. The instruments capture this by measuring, for each product, the distance in characteristics to rival products in the same market. We further interact them with average household demographics in the submarket to generate additional instruments.

The second set of moments are "micro-moments" on the joint distribution of housing attributes and renter demographics. We use the micro-data from the American Community Survey (ACS) 1-year Estimates to construct the covariances between various household demographics and the housing attributes chosen by these renters. While ACS provides extremely rich information about the correlation

[44] The building characteristics we use include bedroom count, building age, and Class A designation.

between household demographics and housing attributes, because it is anonymized at the Public Use Micro Area (PUMA) level, we do not directly observe which particular apartment a household lives in. As a result, we match the micro-moments from the model prediction on REIS apartment buildings with the covariances in the subsample of ACS that shares the same support of building attributes with properties in REIS. Effectively, it amounts to matching moments within a subsample of ACS households who live in large multi-family buildings, defined as structures with more than 50 units, and share the same support of bedroom counts and rent distributions.

Furthermore, we augment the demand estimation using moments from Data Axle on housing attributes that are not part of standard ACS, namely, the designation of Class A buildings. Specifically, we match moments on the ratio of household income between those who choose Class A buildings vs. Class B/C buildings based on information in Data Axle. These moments help us pin down heterogeneous taste parameters for the number of bedrooms, building age, building type, and building class by various household characteristics, including income, household age, household size, and the presence of children.

The difference between the empirical moments and their corresponding predicted moments at a given parameters form a criterion function $\hat{G}_2(\theta^D) = v - \hat{v}(\theta^D)$, where v denotes the empirical moment based on data, and $\hat{v}(\theta^D)$ denotes the corresponding, predicted moment, as a function of parameters. For example, the criterion function that targets the covariance between household size (hs) and bedroom count (bed) for renters in market t is

$$G_2^t(\theta^D) = \underbrace{\sum_{i,j} w_i \mathbb{1}\{y_i = j\} \times \text{hs}_i \times \text{bed}_j}_{\text{observed ACS covariance}} - \underbrace{\sum_{i,j} w_i s_{ijt}(\theta^D) \times \text{hs}_i \times \text{bed}_j}_{\text{model predicted covariance given } \theta^D} \tag{5.5}$$

where w_i denotes the sampled household's weight, y_i denotes the actual choice of household i, and s_{ijt} denotes household i's probability of choosing product j given the parameter. All household demographics are standardized. Intuitively, the estimation procedure finds some set of parameters θ^D that gives rise to the choice probabilities of renters for each building, $\hat{s}_{ijt}(\theta^D)$ that yields a similar pattern of covariance between household demographics and housing attributes as observed from data.

By stacking the set of exclusion restriction moments $G_1(\theta)$ and micro-moments $G_2(\theta)$, the estimation procedure finds a vector of parameters that minimizes the stacked criteria:

$$\arg\min_{\theta^D} \mathcal{G}(\theta^D) = G(\theta^D)' W G(\theta^D), \qquad G(\theta^D) = [G_1(\theta^D); G_2(\theta^D)],$$

where W is the optimal weighting matrix from two-step GMM. To further improve efficiency, we use the GMM estimates obtained with the differentiation IVs to construct approximated "optimal instruments" in the spirit of Chamberlain (1987), and re-estimate the model via two-step GMM with these instruments.

5.3 Demand Estimation Results

We focus our estimation of renter demand and the subsequent tests of conduct on a set of ten cities from 2009 to 2019, including Atlanta, Charlotte, Dallas, DC, Houston, Los Angeles, Minneapolis, Portland, San Diego, and Seattle.

These cities were chosen for several practical reasons and data limitations based on the following selection criteria. First, we rank all available cities by the number of multifamily units. Next, we removed several cities where we could not obtain reliable commercial property tax rates from our CoreLogic records, namely cities in Arizona, Illinois, and Pennsylvania. Third, we removed New York because of known widespread rent stabilization laws even within "market-rate" units and, as a result, low algorithmic adoption rates. Lastly, we add both Seattle and Charlotte because they have been subjects of ongoing litigation with a significant degree of algorithmic pricing penetration. These ten cities account for about 25% of our entire sample in terms of affected units. Moreover, on average, about 32% of the units in our estimation sample are adopters at the end of 2019, which is similar to the overall 34% for our overall REIS data.

Table 11 summarizes key moments from the demand estimation. Panel (A) contains key statistics that relate to market share and prices, which include the unit-type rent and occupancy. Panel (B) contains key moments from the ACS that form the basis of the micro-moments. Panel (C) shows that there are ten metros covering 203 REIS-submarkets. Panel (D) shows that this set of submarkets provide us with varying levels of algorithmic pricing penetration, with the 5th percentile of penetration at 2.8% and the 95th percentile of penetration at 69.0%.

Table 12 summarizes our estimated demand parameters using `pyblp` (Conlon and Gortmaker, 2020, 2023). Our estimates show that renters with higher income are less price sensitive. We also see households with greater household size and with the presence of children deriving higher utility from units with more bedrooms, as implied by a positive, significant coefficient on household size-bed interaction term. We find households with higher income and younger households prefer newer buildings. We find that households with higher incomes also prefer Class A buildings. Lastly, renters who choose buildings in REIS tend to be households that are smaller in size, less likely to have children, and younger in age. This makes sense given that REIS buildings tend to be multifamily apartments mostly comprised of studios, one-bedroom, and two-bedroom units, averaging 1.5 bedrooms per unit. By contrast, non-REIS dwellings in the ACS have on average 1.8 bedrooms per unit. In addition, because we allow the price coefficient to also include a mean coefficient α_0, all the moments, including market share moments, are designed to match perfectly by construction, which we also verify empirically.

We end the section on renter demand by discussing the estimated elasticities. The median own-elasticity for individual buildings is approximately -3.26, indicating that each building faces elastic residual demand. The distribution of own elasticities is shown in Appendix Figure A8. However, when aggregated to the level of all REIS apartment buildings, the elasticity becomes more inelastic, at -1.30, which is mostly in line with existing findings in the literature (Chen, Clapp, and Tirtiroglu, 2011). The degree of substitution from REIS to non-REIS rental buildings is reflected in the mean diversion ratio to the outside option, $-\mathbb{E}[\frac{\partial s_{0t}}{\partial p_{jt}}/\frac{\partial s_{jt}}{\partial p_{jt}}]$, which is around 0.79. This suggests that, on average, 79% of households leaving a REIS building opt for a non-REIS building, while the remaining 21% move to another REIS apartment. In summary, our demand-side estimation reveals that while individual apartment buildings face fairly elastic demand, collectively, they face a more inelastic aggregate demand, yet they continue to face considerable competitive pressure from other rentals outside REIS apartment buildings.

6 Supply and Testing for Conduct

6.1 Supply Model

The workhorse model for market equilibrium in differentiated product markets is Nash-Bertrand. Namely, each building owner f sets the price of its products j across all unit types in all buildings that it owns to maximize its total profit.

We add two elements to the model to account for the potential impact of algorithmic pricing. First, to allow for potential coordination among adopters, following Backus, Conlon, and Sinkinson (2021), we let a firm's profit function internalize a fraction τ^A of its fellow adopters' profits. A value of $\tau^A = 0$ corresponds to a model of own profit maximization, i.e., no coordination, and a value of $\tau^A = 1$ corresponds to a model of joint profit maximization, i.e., full coordination. Any intermediate values $\tau^A \notin \{0, 1\}$ denote partial internalization of competitor profits and is not directly interpretable as any specific model of conduct. Thus, the overall profit function of owner f is as follows

$$\Pi_{ft} \;\; \boldsymbol{p_{ft}}; \boldsymbol{p_{-ft}} \;\; = \sum_{j\in\mathcal{J}_f} \Pi_{jt} \;\; p_{jt}; \boldsymbol{p_{-jt}} \;\; + \tau^A \sum_{f'\neq f} \kappa_{ff't} \sum_{j\in\mathcal{J}_{f'}} \Pi_{jt} \;\; p_{jt}; \boldsymbol{p_{-jt}} \tag{6.1}$$

$\kappa_{ff't}$ is an indicator for whether firm f and f' have adopted the same algorithmic pricing software at time t. Correspondingly, the first order condition with respect to p_{jt} becomes

$$\frac{\partial \Pi_{ft}}{\partial p_{jt}} = D_{jt} + \left(\sum_{k\in\mathcal{J}_f} (p_{kt} - mc_{kt}) \frac{\partial D_{kt}}{\partial p_{jt}} \right) + \tau^A \sum_{f'\neq f} \kappa_{ff't} \left(\sum_{k\in\mathcal{J}_{f'}} (p_{kt} - mc_{kt}) \frac{\partial D_{kt}}{\partial p_{jt}} \right) = 0 \tag{6.2}$$

Second, to account for non-adopters potentially deviating from the Nash-Bertrand benchmark due to various frictions, we model this flexibly by introducing a time-varying τ_t^{NA} applied to the markup term. A value of $\tau_t^{NA} = 1$ indicates that non-adopters are fully "sophisticated" by charging the optimal Nash-Bertrand markup. A value greater than one suggests their prices are too high, while a value less than one suggests their prices are too low. For example, in the context of our previous model of responsive pricing, if non-adopters are slow to raise prices during a boom, it could correspond to

$\tau_t^{NA} < 1$, and if they are slow to lower prices during a bust, it could correspond to $\tau_t^{NA} > 1$. In general, τ_t^{NA} flexibly captures the extent to which non-adopter prices deviate from the optimal Nash-Bertrand levels, regardless of the nature of the pricing friction they face.

For clarity, we index non-adopter firms by l, and its first-order condition becomes

$$\frac{\partial \Pi_{lt}}{\partial p_{lt}} = \tau_t^{NA} D_{jt} + \left(\sum_{k \in \mathcal{J}_l} (p_{kt} - mc_{kt}) \frac{\partial D_{kt}}{\partial p_{lt}} \right) = 0 \tag{6.3}$$

Taken together, we can write the system of first-order conditions as follows

$$\boldsymbol{p_t} - \boldsymbol{mc_t} = -\underbrace{\left(\mathcal{H}_t(\tau^A, \tau_t^{NA}) \odot \boldsymbol{\Omega}_t(\boldsymbol{p})\right)^{-1} \boldsymbol{D}_t(\boldsymbol{p})}_{\boldsymbol{\eta}_t(\mathcal{H}_t(\tau^A, \tau^{NA}))} \tag{6.4}$$

where $\boldsymbol{D}_t(\cdot)$ denotes the vector of demand for all products at a given price. $\boldsymbol{\Omega}_t(\boldsymbol{p})$ represent the demand derivatives where $\Omega_{jkt} = \partial D_{jt}/\partial p_{kt}$. $\odot$ represent the element-wise (Hadamard) product between the "internalization matrix" $\mathcal{H}_t$ and the matrix of demand derivatives. $\boldsymbol{\eta}_t(\mathcal{H}_t(\tau^A, \tau^{NA}))$ denotes the markup implied by the parameter (τ^A, τ^{NA}) for a given demand system $\boldsymbol{D}_t$.

$\mathcal{H}_t(\tau^A, \tau_t^{NA})$ denotes the "internalization matrix" that encapsulates both the possibility of coordination among adopters and the possible lack of sophistication among non-adopters. Specifically, it assumes the following block-diagonal form:

$$\mathcal{H}_t(\tau^A, \tau^{NA}) = \begin{bmatrix} \mathcal{H}_t^A(\tau^A) & 0 \\ & \\ 0 & \mathcal{H}_t^A(\tau_t^{NA}) \end{bmatrix} \tag{6.5}$$

Among two products that are adopters $j, k \in A_t$, where A_t denotes the set of products that are using algorithmic pricing in market t, we define

$$\mathcal{H}_{jkt}^A(\tau^A) = \begin{cases} 1 & \text{if } j,k \text{ are owned by the same owner,} \\ \tau^A & \text{if } \kappa_{jkt} = 1 \text{ are using the same algorithmic pricing software,} \\ 0 & \text{otherwise.} \end{cases} \tag{6.6}$$

Among two non-adopters $j, k \notin A_t$, we define

$$\mathcal{H}_{jkt}^{NA}(\tau_t^{NA}) = \begin{cases} \frac{1}{\tau_t^{NA}} & \text{if } j,k \text{ are owned by the same owner,} \\ 0 & \text{otherwise.} \end{cases} \tag{6.7}$$

Across adopters $j \in A_t$ and non-adopters $k \notin A_t$, $\mathcal{H}_{jkt} = 0$. The block-diagonal shape of $\mathcal{H}_t(\tau^A, \tau_t^{NA})$ arises naturally if we index all the adopters first and then index all the non-adopters. Conceptually, it requires that the pricing algorithm does not facilitate coordination between adopters and non-adopters, which is reasonable given that the pricing algorithm does not recommend prices to non-adopters.

As a result, because the inverse of a block diagonal matrix is also block diagonal, we can re-write the first-order condition from Eq (6.4) separately for adopters and non-adopters, stacked as follows

$$\begin{bmatrix} \boldsymbol{p}_t^A - \boldsymbol{mc}_t^A \\ \boldsymbol{p}_t^{NA} - \boldsymbol{mc}_t^{NA} \end{bmatrix} = \begin{bmatrix} -\left(\mathcal{H}_t(\tau^A) \odot \boldsymbol{\Omega}_t^A(\boldsymbol{p})\right)^{-1} \boldsymbol{D}_t^A(\boldsymbol{p}) \\ -\left(\mathcal{H}_t(\tau_t^{NA}) \odot \boldsymbol{\Omega}_t^{NA}(\boldsymbol{p})\right)^{-1} \boldsymbol{D}_t^{NA}(\boldsymbol{p}) \end{bmatrix} \tag{6.8}$$

where $\boldsymbol{D}^A$ and $\boldsymbol{D}^{NA}$ denote the demand for adopters and non-adopters respectively, and $\boldsymbol{\Omega}_t^A$ and $\boldsymbol{\Omega}_t^{NA}$ represent their respective derivatives with respect to prices. Note that the block-diagonal nature of the internalization matrix, which is based on the observation that the pricing algorithm does not recommend prices to non-adopters, is what allows us to construct the first order conditions separately for adopters and non-adopters. From a testing perspective, it has the key benefit of allowing us to perform pair-wise testing for adopter conduct irrespective of non-adopters' pricing behavior.[45] As such, by estimating the entire surface of demand derivatives, the structural model provides us with a direct approach to evaluating adopter conduct.

6.2 Conduct Test

Given the first order condition given in Eq (6.4), because the demand system $\boldsymbol{D}_t(\boldsymbol{p})$ is already estimated, its derivatives can be evaluated at the observed prices, and the markup term $\boldsymbol{\eta}_t(\mathcal{H}(\tau^A, \tau^{NA}))$ becomes solely determined by the parameters (τ^A, τ^{NA}). Moreover, as discussed, the stacked first-order condition in Eq (6.8) allows us to back out the marginal cost of adopters as follows

$$\boldsymbol{mc_t}^A = \boldsymbol{p_t}^A - \boldsymbol{\eta}_t^A(\mathcal{H}_t(\tau^A)) \tag{6.9}$$

To differentiate different models of conduct, as discussed in Berry and Haile (2014), what one has to do is to find excluded instruments $\boldsymbol{z}_t^S$ so that the corresponding conditional moment condition holds under the correct model of conduct

$$\mathbb{E}[\omega_{jt}|\boldsymbol{z}_t^S] = 0. \tag{6.10}$$

Here, the marginal cost shocks ω_{jt} are defined as the component of marginal costs residualized on observables x_{jt}

$$mc_{jt}^A(x_{jt}, q_{jt}) = h(x_{jt}) + \omega_{jt}. \tag{6.11}$$

In the real estate setting, rather than using a fully flexible function h, we think it is a reasonable approximation to consider a hockey-stick shaped marginal cost curve, which is mostly flat up until its occupancy approaches its capacity, for example in Farronato and Fradkin (2022). To allow for

[45] To be precise, an additional requirement for this to be true is that the marginal cost functions for adopters, mc_t^A, and non-adopters, mc_t^{NA}, are allowed to differ, so that the left-hand-side of Eq (6.8) can also be stacked. This is fairly reasonable considering that the marginal cost of a building may change after adoption.

flexibility in both the curvature and the level, we parametrize the marginal cost function as follows

$$mc^A_{jt}(x_{jt}, occ_{jt}) = h(x_{jt}, occ_{jt}) + \omega_{jt} = \frac{\kappa_a}{(occ_{jt} - 1 - \delta_\epsilon)^2} + \delta_j + \omega_{jt}. \tag{6.12}$$

where occ_{jt} denotes occupancy (i.e., quantity divided by capacity), where δ_ϵ is a modeling device to make sure the marginal cost function is always well-behaved even if occupancy is reported at 100%.[46]

For the conditional moment condition in Eq (6.10), the exclusion restriction of candidate instruments is that they affect markup and quantity but are not correlated with unobserved marginal cost shocks. We use the demand-side optimal IV approximated from the demand estimates developed in Conlon and Gortmaker (2020) because it can efficiently capture non-linear combinations of individual demand instruments as discussed in Backus, Conlon, and Sinkinson (2021).

With the supply model and the marginal cost shocks described above, we adopt a pair-wise testing procedure based on Backus, Conlon, and Sinkinson (2021). The approach builds upon the intuition from the non-nested testing framework of Rivers and Vuong (2002), which compares two models of conduct and asks which one is "favored" over the other. Given two competing models characterized by different internalization matrices $\mathcal{H}^{M_1}$ and $\mathcal{H}^{M_2}$, the pair-wise testing amounts to computing a test statistic based on the difference in the unconditional moment restriction

$$\mathbb{E}[(\omega^{M1}_{j,t})' A(\boldsymbol{z}^S_t)] - \mathbb{E}[(\omega^{M2}_{j,t})' A(\boldsymbol{z}^S_t)] \tag{6.13}$$

where $A(\boldsymbol{z}^S)$ denotes the expected markup difference conditional on the instruments

$$A(\boldsymbol{z}^S) = \mathbb{E}[\Delta\eta^{1,2}_{jt} | \boldsymbol{z}^S], \quad \Delta\eta^{1,2}_{jt} = \eta_{jt}(\mathcal{H}^{M_1}) - \eta_{jt}(\mathcal{H}^{M_2}) \tag{6.14}$$

The rest of the details are described in the Appendix Algorithm 1.

While it may seem straightforward to directly estimate the internalization parameter τ^A through GMM (Nevo, 2001; Miller and Weinberg, 2017), we primarily focus on the two-sided RV test for several reasons: (i) The main advantage of pair-wise testing over parameter estimation lies in its robustness to potential model misspecification (Magnolfi and Sullivan, 2022; Duarte, Magnolfi, Sølvsten, and Sullivan, 2023). While an estimated parameter is directly sensitive to misspecification of the demand model and functional form of marginal cost, pair-wise testing focuses on which model of conduct is "more favored" by the data, despite such misspecifications. (ii) The estimation framework is more valuable when intermediate values of the internalization parameter $\tau^A \in (0, 1)$ are the key focus, but since our primary interest is in *whether* algorithms facilitated joint profit maximization among its adopters as opposed to individual profit maximization, the pair-wise testing framework is particularly well-suited to our context. On the other hand, a direct estimation approach may be more appropriate

[46] While the inclusion of occupancy in marginal cost is economically motivated, it produces an econometric problem in that it introduces another endogeneity problem in estimating κ_a. As such, we use the same set of instruments for the conduct test to instrument for the quantity term $1/(occ_{jt} - 1 - \delta_\epsilon)^2$.

for settings where the exact magnitude of the internalization parameter is of greater economic interest.

6.3 Testing Results

We conduct the pair-wise test for adopters as described in the previous section. Specifically, given that RealPage acquired its main competitor, Lease Rent Options (LRO), from the Rainmaker Group at the end of 2017, we flexibly allow for three parameters to test whether observed prices are consistent with joint profit maximization for the adopters of each entity as follows

(i) τ_L^A for LRO users pre-acquisition (2009-2017)

(ii) τ_R^A for RealPage users pre-acquisition (2009-2017)

(iii) τ_M^A for the merged RealPage users post-acquisition (2018-2019)

Table 13 summarizes the result of the conduct test. The pair-wise test compares model 1 of competition (i.e., own profit maximization) vs. model 2 of coordination (i.e., joint profit maximization). A value of $\tau_2^A = 1$ means that we are testing a model of full competition against a model of full coordination. A value of $\tau_2^A < 1$ means that we are testing a model of full competition against a model of partial internalization. A negative test statistic implies that data favors model 1, namely, competition. A positive test statistic implies that data favors model 2, namely, partial or full coordination.

For the pre-acquisition time period (2009-2017), the test statistics for LRO adopters and RealPage adopters for full coordination are 2.67 and 2.93, respectively, suggesting that data favors a model of coordination over competition among adopters. The test statistics of other intermediate levels of internalization τ^A also generate significant test statistics in favor of some degree of joint maximization.[47] As such, the test finds that the markups implied by our structural model of demand are more consistent with a model of joint profit maximization among the adopters of each software rather than a model of own profit maximization in the 2009-2017 period.

For the post-acquisition time period (2018-2019), the test statistic for full coordination among all adopters of the merged RealPage is 1.48, suggesting that data cannot differentiate the two models with statistical significance. Yet, some degree of coordination seems preferred by the data over competition, given that we obtain significant test statistics at lower levels of τ^A. This finding is consistent with our

[47] It is also useful to note that the test statistic does not necessarily decline when the value of τ^1 decreases, suggesting any amount of joint maximization is preferred over full competition. While this result may appear counter-intuitive, we think it is because the weakening of the test statistics only happens when the two models under testing are both closer to the true model of conduct. For example, in Backus, Conlon, and Sinkinson (2021), given that the result of their conduct test favors competition, it becomes harder for the test to differentiate $\tau^A = 0$ vs. $\tau^A = 0.1$. On the other hand, given that the result of our conduct test favors coordination, it only becomes harder for the test to differentiate $\tau^A = 0.9$ vs. $\tau^A = 1$. Concretely, when we change the test to be a model of partial internalization against a model of full coordination, the test statistics indeed weaken to 0.8 and 2.0 respectively when testing $\tau^A = 0.9$ vs. $\tau^A = 1$. Intuitively, if one is interested in testing whether the slope of a GMM objective is statistically different from zero between two points, it becomes harder when located in the flat region of the objective.

previous results from the merger analysis, where it seems that the acquisition of the legal entities did not imply an immediate integration of the operations of the two underlying pieces of software.

To consider the robustness of the results, one might be concerned that possible coordination might also be driven by joint ownership that is not fully controlled for by the observed ownership data. Even though RCA reportedly looks through various shell holding companies to capture the underlying economic owner, mismeasurement here could imply that one may underestimate the extent of joint ownership. As a result, we also perform a more conservative conduct test by controlling for shared management company, namely, we test a model of competition between management companies against a model of coordination among them.

It is useful to note that the degree of documented ownership concentration is quite low, where the HHI, when calculated based on all REIS apartments in a metro, is below 80 in our estimation sample, as shown in Appendix Table A4. The HHI for management companies is slightly higher, but still always below 300. By sharp contrast, the HHI for algorithmic adoption is much higher, with the estimated HHI in Seattle approaching 3,000, and could be substantially higher in certain submarkets.[48] Now, with this more conservative test, Appendix Table A9 still finds that the test statistics to be positive and significant for the 2009-2017 period, suggesting that the markups are more consistent with a model of joint maximization.

Lastly, given these results, as well as the theoretical interest in terms of whether algorithms developed by different firms are able to learn to coordinate, we also consider a test of coordination *between* RealPage and LRO prior to the acquisition from 2009 to 2017. In this case, we test a model of within-software coordination against a model of across-software coordination. Table 14 summarizes the pair-wise test results on whether the "cross-diagonal" terms τ^{A}_{RL} between RealPage and LRO favors coordination or competition. In this case, the test statistics are always insignificant (less than 1.96), suggesting that the test cannot statistically distinguish a model of coordination between adopters of RealPage and LRO prior to the acquisition from a model of competition between these two groups of adopters. Therefore, the overall finding is that we generally favor a model of joint profit maximization among adopters of the same software, but not across different software, supporting a more contained notion of "algorithmic coordination."

6.4 Implications

Given the testing results, in this section, we provide several back-of-the-envelope calculations of the impact due to coordination among adopters. Specifically, we compute the differences in the implied markup between a model of joint profit maximization and a model of own profit maximization over time and across markets.

[48] U.S. Department of Justice considers "markets in which the HHI is between 1,000 and 1,800 points to be moderately concentrated, and consider markets in which the HHI is in excess of 1,800 points to be highly concentrated." https://www.justice.gov/atr/herfindahl-hirschman-index, accessed August 10, 2024.

Table 15 summarizes the results. The first column shows that, within the estimation sample, the fraction of buildings that adopted algorithmic pricing grew steadily from 6.5% in 2009 to about 25% in 2019. Consequently, the estimated markup difference between the two models of conduct also increased from \$13.6 per month per unit on average in 2009 to \$24.9 per month per unit in 2019, or \$53.1 per month per unit if we assume that merged RealPage eventually would be able to jointly profit maximize among all of its customers. The average also masks the extent of heterogeneity across markets, where the estimated markup difference could be as high as \$78 per month per unit for submarkets in the 75th percentile.

Given that we estimate about 4.2 million adopted units in the US (see Section 3.2), even without full integration between YieldStar and LRO, we estimate an average markup difference of \$24.9 per month per unit, which implies an aggregate impact of approximately \$105 million per month, or \$1.5 billion per year, due to algorithmic coordination.

We also note that the calculation here is likely a lower bound because it is based only on the markup differences of adopters in a partial equilibrium. To fully capture the general equilibrium impact, it would require a full re-computation of a new equilibrium for both adopters and non-adopters. However, because we do not have a structural model of how non-adopters would behave under such hypothetical market environments, we do not compute such counterfactual equilibrium.[49] That said, in terms of the direction of the bias, because prices are strategic complements, a full-fledged counterfactual would likely produce an even higher estimate because non-adopters would endogenously respond to adopter coordination by raising prices.

Lastly, we also provide a back-of-the-envelope decomposition analysis to disentangle the efficiency and the coordination channel of algorithmic pricing in a partial equilibrium. Specifically, we estimate the sophistication parameters of non-adopters τ_t^{NA} for three different time periods, namely, from 2009 to 2011, from 2012 to 2015, and from 2016 to 2019. The segmentation of the time period is motivated by the reduced form finding where the impact of responsive pricing is likely time-varying. The estimated values are 1.49 for 2009-2011, 0.79 for 2012-2015, and 0.71 for 2016-2019. Recall that $\tau_t^{NA} > 1$ means that non-adopters charge too high of a price compared to the optimal markup, and vice versa. With the estimated time-varying sophistication parameters τ_t^{NA}, we compute how coordinated and responsive pricing contributes to the overall price levels of the adopters. We first compute the markup difference due to coordination. We then compute the impact of responsive pricing by computing a second markup difference where we set $\tau_t^{NA} = 1$.

In Figure 9, the top panel shows that coordination had a small price impact in Seattle in 2009 when total adoption was still very low, but responsive pricing played a much larger role in bringing *down* the prices of adopters. The net impact of algorithmic pricing is that adopters charged lower

[49] In other words, although the conduct parameter τ^A among adopters is structural in nature, the sophistication parameter τ_t^{NA} for non-adopters is not a deep structural parameter in that they are estimated only off the observed market realizations. Without a model of how τ_t^{NA} arises from economic fundamentals, we do not know the values of the sophistication parameter τ_t^{NA} in such counterfactuals.

prices than non-adopters in 2009. By contrast, the bottom panel of Figure 9 shows a much bigger markup impact due to coordination in 2019 when the penetration of algorithmic pricing had grown significantly by then. In addition, the responsive channel added another price bump. In net, it means adopters charged significantly higher prices due to the adoption of algorithmic pricing in 2019.

6.5 Discussions and Limitations

While our pair-wise test is a powerful tool to assess whether observed prices are favored by a model of joint profit maximization or a model of individual profit maximization, we *do not, and also cannot, assess how such coordinated prices arise.*

One could certainly speculate on how coordinated prices arise, and there are many possibilities. We list a few conjectures here as illustrative examples. First, coordinated pricing could emerge spontaneously due to reinforcement learning algorithms deployed at the management company level (Calvano et al., 2020b; Banchio and Mantegazza, 2023). Second, algorithmic naivety could also result in supracompetitive prices. For instance, if a learning algorithm is based on a misspecified monopolist demand model that ignores strategic responses from competitors—a common model used in the revenue management literature (Gallego and Van Ryzin, 1994; Besbes and Zeevi, 2009)—prices could exceed competitive levels when used by firms that would otherwise compete (Cooper, Homem-de-Mello, and Kleywegt, 2015; Meylahn and V. den Boer, 2022).

On the other hand, supracompetitive prices could also result from improperly specified econometric analyses. For example, a naive regression of quantity on price without instrumenting for price could suffer from the usual simultaneity problem, likely leading to attenuated quantity responses to price changes (Wright, 1928) and resulting in the estimated demand elasticity being too low. Other possibilities include performing experiments that are correlated with competitors (Hansen, Misra, and Pai, 2021b) or performing them at the cluster level that reflects a more aggregate, inelastic demand (Holtz, Lobel, Lobel, Liskovich, and Aral, 2024).

Lastly, joint maximization might be explicitly coded into the algorithm itself. However, Harrington (2022) argues that third-party providers should have the incentive to recommend competitive prices to encourage adoption, as supracompetitive prices could deter adoption. Yet, it is also possible that by recommending coordinated prices, which would increase with a growing base of adopters, the third party provider could continue to demonstrate its "value" to existing customers and retain them better.

Because we do not have access to the underlying algorithms used, we cannot determine the specific mechanism, and therefore, our findings cannot be used to assess the legality of the outcome. Instead, if an anti-trust authority has the ability to subpoena such information from the third-party provider, it could better assess the mechanism and possibly develop more targeted remedies. In this sense, we view our testing framework as adding another analytical tool, and when the test yields positive results, it can contribute to meeting the evidentiary threshold for further investigation.

In addition, it is worth noting that our conduct test rests on a static model of differentiated Nash-Bertrand pricing, without explicitly incorporating dynamic considerations. We believe that dynamic pricing in revenue management problems, such as those used in airlines or hotels, is driven by two market properties that may be less relevant for long-term rentals. First, in markets where the good is perishable after a deadline—like a seat on a flight or a hotel room on a specific date—the pricing path leading up to that deadline is crucial (Gallego and Van Ryzin, 1994). For long-term rentals, although there is also an opportunity cost to leaving an apartment vacant for an additional day, it only accounts for a small fraction of the good because the lease length is typically for a much longer term (often 12 or 24 months), resulting in less variability in listed rents as the lease start date approaches. Second, in much of the travel market, high-valuation consumers often book later, leading to strategic reallocation of capacity from early to late bookers (Betancourt, Hortaçsu, Öry, and Williams, 2024). However, we do not view this correlation between arrival time and valuation as first-order in the long-term rental market.[50] Therefore, we believe our static model provides a reasonable approximation for the pricing problems in the long-term rental market, but we caution against its use as a test for conduct in genuinely dynamic settings, which we leave for future research.

7 Conclusion

In this paper, we examine the impact of algorithmic pricing software adoption on the U.S. multifamily housing industry. We hand-collect data on management company adoption status from various sources and merge it with a comprehensive database of building-level rents and occupancy across 50 metro areas.

First, we find robust evidence that the algorithm helps building managers set prices that are more responsive to market conditions. The treatment effect of the algorithm at the building level varies across time periods. During the Great Recession (2009-2010), adopters of the algorithm lowered rents and increased occupancy compared to non-adopters in the same submarket and building class. Conversely, during the economic recovery (2014-2017), adopters increased rents and reduced occupancy. This pattern is robust to alternative market definitions, regression specifications, and when instrumenting a building's adoption with its management company's exposure to algorithmic pricing in other metros.

Second, we compare adopter outcomes across neighborhoods with varying levels of algorithmic penetration. We find that adopters in neighborhoods with higher same-software penetration charge

[50] That said, there is a different type of dynamic concern in this market: leasing a unit today for 12 months renders it unavailable next month, when demand may be forecast to be stronger. If these demand changes are due to predictable forces like seasonality, then algorithmic pricing, by improving seasonality predictions, could enhance profitability by recommending a matrix of price and lease lengths to better cater to these patterns, leading to a more efficient allocation of apartment units over time. We model this in an agnostic manner by allowing the curvature of marginal costs to differ between adopters and non-adopters, as described in Eq (6.8). Unfortunately, we lack detailed data on non-standard lease lengths and cannot evaluate the extent of such reallocation across time due to algorithmic pricing in detail.

higher rents and experience lower occupancy. The opposite signs on price and occupancy cannot be generated by responsive pricing alone, suggesting potentially coordinated pricing. However, since reduced-form comparisons using variation in penetration levels still cannot fully separate conduct from cost, we turn to a structural approach to test this more formally.

To test for conduct, we adopt a structural approach by estimating a system of rental demand and assessing exclusion restrictions on residual marginal costs backed out from a given model of conduct. We first estimate a model of housing choices from renters across a panel of ten cities, using detailed building-level price and quantity data, as well as household-level choice data. Our overall finding is that prices are more consistent with a model of joint profit maximization among adopters of the same software, but not across adopters of different software. The coordinated pricing results in higher markups, increasing costs for renters. In 2019, the average markup difference due to joint maximization among adopters is estimated at \$24.9 per month per unit, affecting about 4.2 million adopted units across the U.S.

Our findings and empirical approach have significant implications. Given that the multifamily sector houses over 40 million people in the U.S. and is valued at over \$4 trillion, even minor effects in this industry can lead to substantial impact. As algorithmic pricing becomes more widespread across various industries, price efficiency may improve through better acquisition and incorporation of information. However, with the market concentration of business service software, concerns about "algorithmic coordination" also loom large. Balancing these trade-offs is a complex yet urgent question for businesses, consumers, and regulators alike.

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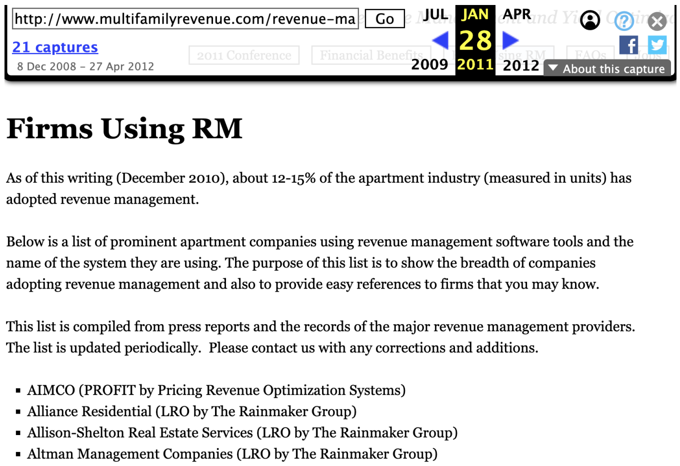
8 Figures

Figure 1: Snapshot of Surveyed List of Adopters


http://www.multifamilyrevenue.com/revenue-ma Go JUL JAN APR
21 captures
8 Dec 2008 – 27 Apr 2012
28
2009 2011 2012
About this capture

Firms Using RM

As of this writing (December 2010), about 12-15% of the apartment industry (measured in units) has adopted revenue management.

Below is a list of prominent apartment companies using revenue management software tools and the name of the system they are using. The purpose of this list is to show the breadth of companies adopting revenue management and also to provide easy references to firms that you may know.

This list is compiled from press reports and the records of the major revenue management providers. The list is updated periodically. Please contact us with any corrections and additions.

- AIMCO (PROFIT by Pricing Revenue Optimization Systems)
- Alliance Residential (LRO by The Rainmaker Group)
- Allison-Shelton Real Estate Services (LRO by The Rainmaker Group)
- Altman Management Companies (LRO by The Rainmaker Group)

https://web.archive.org/web/20110128035809/http://www.multifamilyrevenue.com/revenue-management-users-multifamily/, accessed December 1, 2022.

Figure 2: Example Articles of Client Acquisition Updates Made by Software Companies

(a) Rainmaker (b) Yieldstar


Rainmaker LRO™ Adds More than 30 New Clients to Revenue Management Platform in Last 90 Days

Portfolios Range in Size and Asset Class; Represent 250,000 Units

ATLANTA, GA. (PRWEB) MAY 29, 2013

RealPage Announces that Wilkinson Selects YieldStar Price Optimizer

By RealPage News | June 21, 2011

Decision follows substantial improvements in rent and occupancy during trial

(June 21, 2011)—RealPage, Inc. (NASDAQ: RP), today announced that Wilkinson Real Estate Advisors, an Atlanta-based privately held property management company, has selected YieldStar® revenue management to deploy across its entire portfolio of more than 7,000 units

(Rainmaker LRO) https://www.prweb.com/releases/rainmakerlro/adds30newcompanies/prweb10779081.htm
(RealPage Yieldstar) https://www.realpage.com/news/realpage-announces-that-wilkinson-selects-yieldstar-price-optimizer/, accessed December 1, 2022.

Figure 3: Share of Algorithmic Pricing Adoption by Software

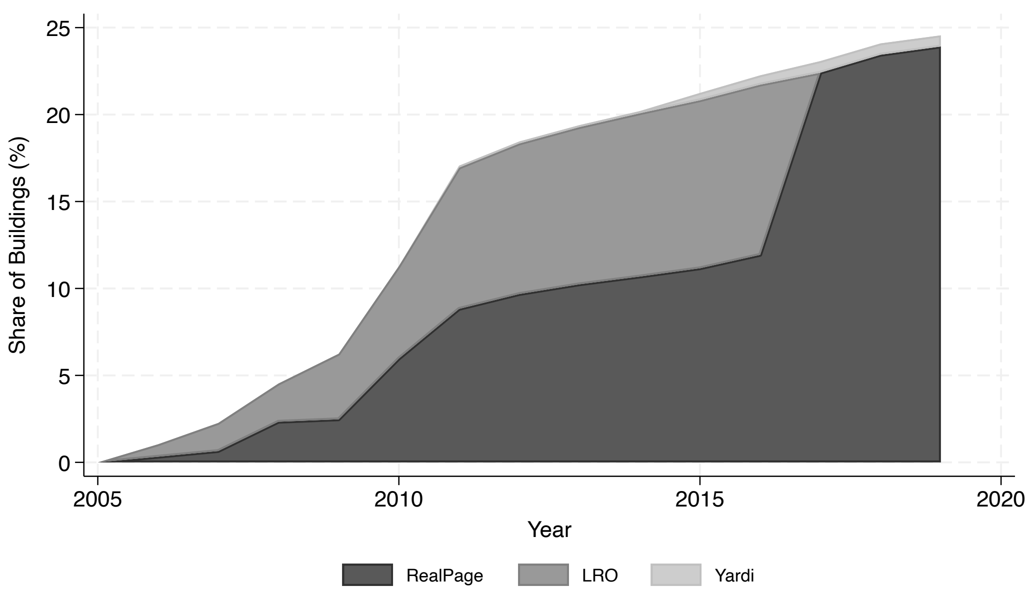
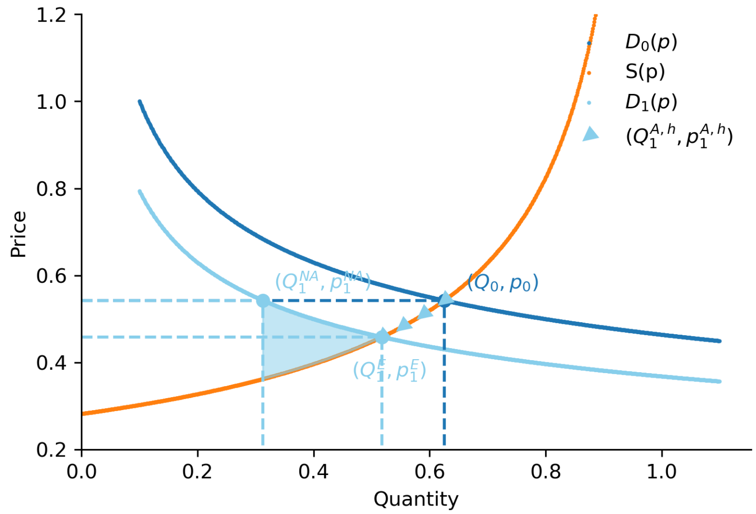
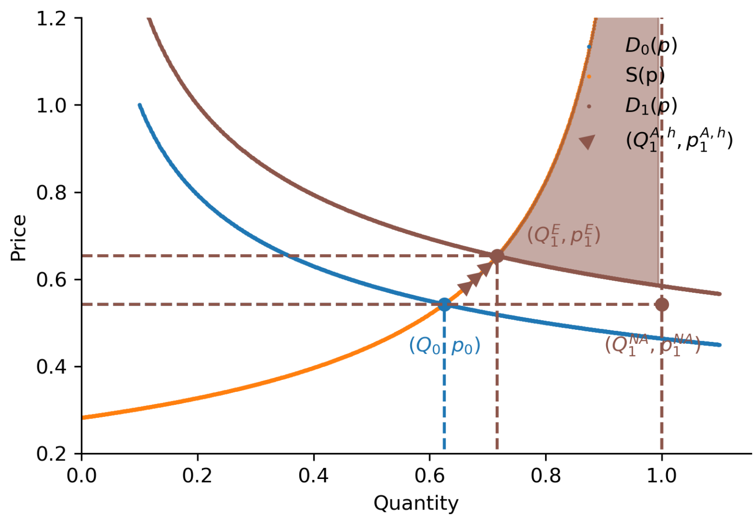


Figure 4: An Illustrative Model of Responsive Pricing

(a) Negative Demand Shock



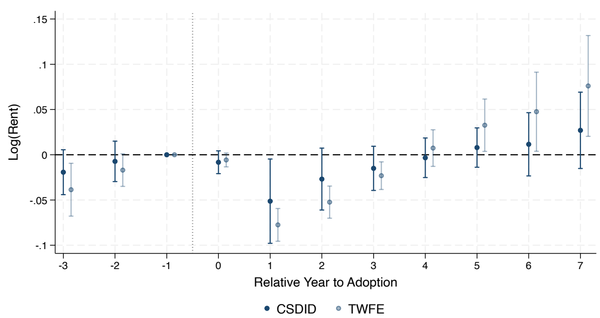
(b) Positive Demand Shock



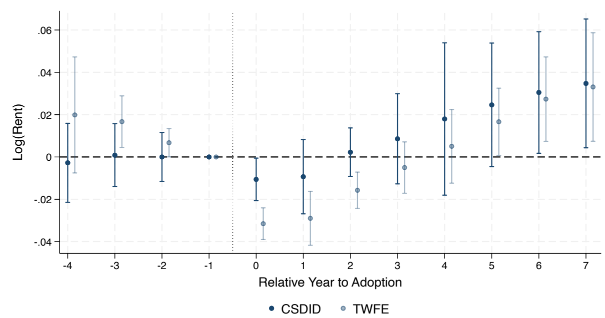
Notes: A stylized model of responsive pricing is illustrated here. In the top panel, with a negative demand shock, prices become stale and are too high relative to the new equilibrium prices, leading to excessive vacancies. Lowering prices more quickly results in welfare gains, indicated by the blue shaded region. In the bottom panel, with a positive demand shock, prices become stale and are too low relative to the new equilibrium prices, leading to a shortage. Increasing prices more rapidly leads to an increase in net social welfare that is indicated by the brown shaded region.

Figure 5: Treatment Effects of Algorithmic Pricing on Rent by Adoption Cohort

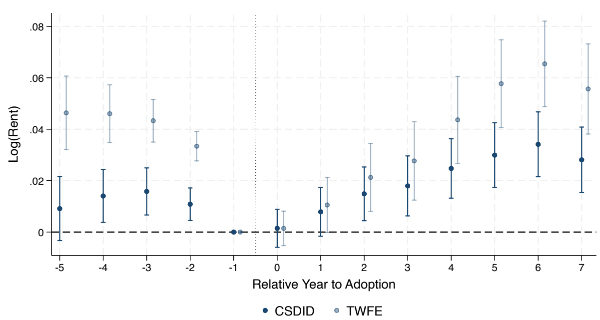
(a) 2008 Adopters



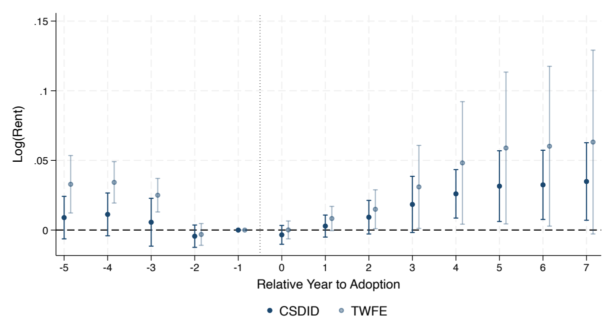
(b) 2009 Adopters



(c) 2010 Adopters



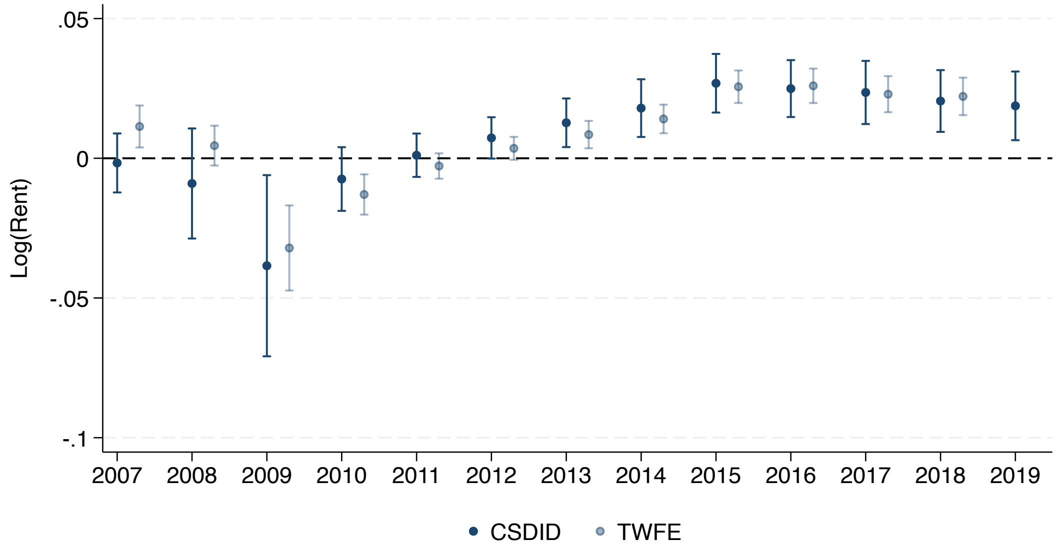
(d) 2011 Adopters



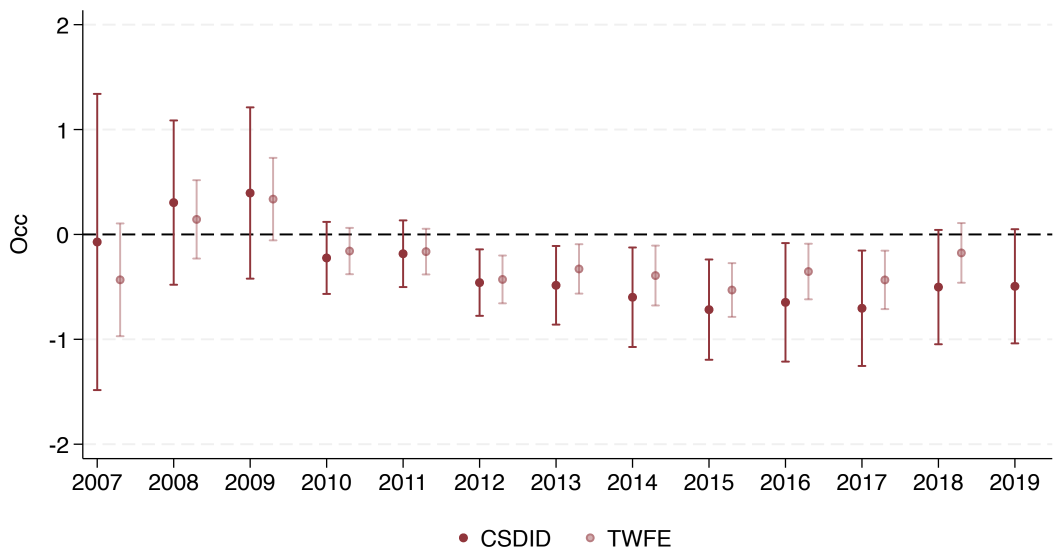
Notes: This figure presents cohort-specific event study estimates for log rent. Each panel shows the estimated treatment effect for a specific adoption cohort. The sample is restricted to buildings constructed before 2005. CSDID estimator (Callaway and Sant'Anna, 2021) uses never-adopters as the control group and includes building characteristics and market segment indicators as covariates, where market segments are defined by metro and quality-quartile pairs. TWFE includes building fixed effects, year fixed effects, and segment-year fixed effects. Standard errors are clustered at the management company level.

Figure 6: Average Treatment Effects of Algorithmic Pricing by Calendar Year

(a) By Calendar Year on Log(Rent)



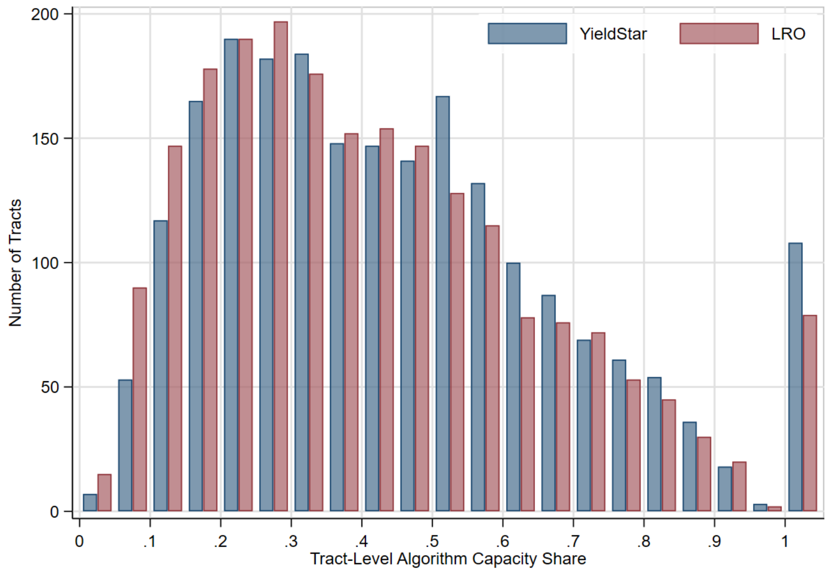
(b) By Calendar Year on Occupancy (%)



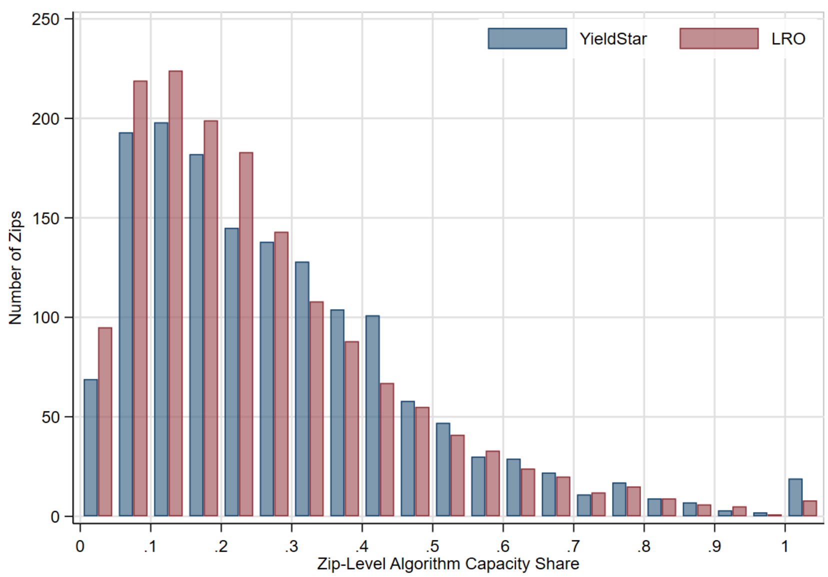
Notes: This figure presents average treatment effects on the treated (ATT) aggregated by calendar year using the Callaway and Sant'Anna (2021) difference-in-differences estimator (CSDID, dark shades). It also reports the treatment effect estimates using traditional two-way fixed effects estimator (TWFE, light shades). The sample is restricted to buildings constructed before 2005. CSDID uses never-adopters as the control group and includes building characteristics and market segment indicators as covariates, where market segments are defined by metro and quality-quartile pairs. TWFE includes building fixed effects, year fixed effects, and segment-year fixed effects. Standard errors are clustered at the management company level.

Figure 7: Distribution of Zip/Tract Level Penetration by Software

(a) Distribution of Tract-Level Algorithm Penetration (2019)



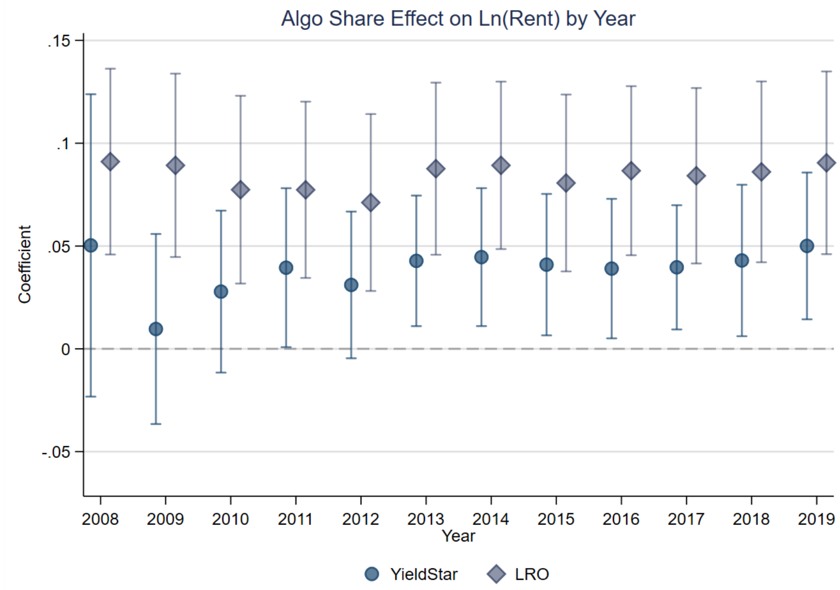
(b) Distribution of Zip-Level Algorithm Penetration (2019)



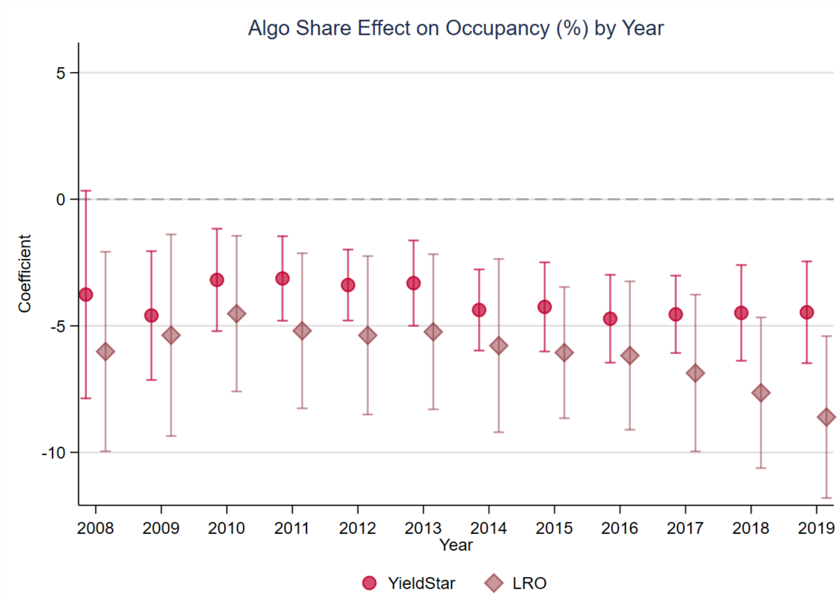
Notes: This figure presents histograms of penetration levels by software at the end of the sample period. The top panel shows the distribution at the census tract level, and the bottom panel at the zip code level. Single-building zips and tracts are excluded. Each plot conditions on zips or tracts with strictly positive penetration for the respective software group.

Figure 8: Algorithm Penetration Effects by Year

(a) Effect on Log(Rent)



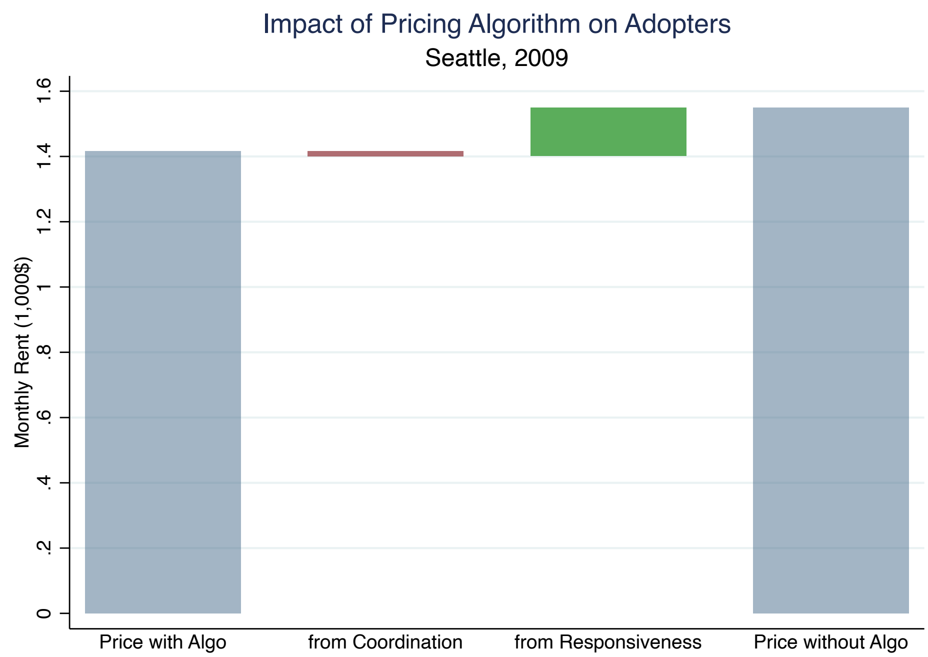
(b) Effect on Occupancy



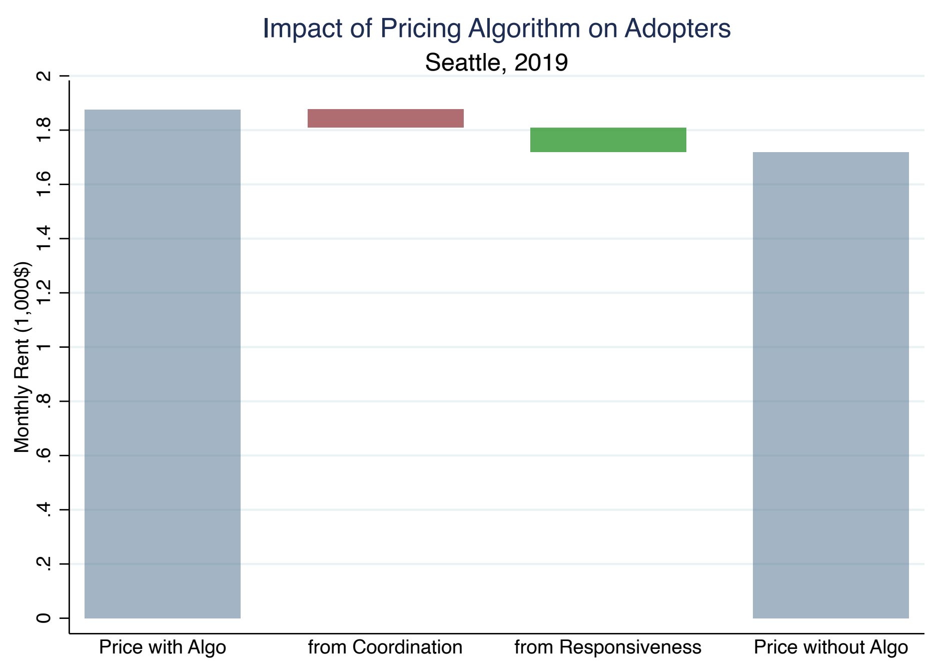
Notes: This figure plots year-by-year coefficients from regressing log rent (top panel) and occupancy (bottom panel) of adopters on tract-level algorithm penetration shares interacted with year indicators, separately for YieldStar and LRO. The sample includes current algorithm adopters in tracts with more than one building. All regressions include building and segment-year fixed effects, with standard errors two-way clustered by management company and census tract. Bars represent 95% confidence intervals.

Figure 9: Back-of-the-Envelope Decomposition of the Implication of Algorithmic Pricing

(a) Seattle 2009



(b) Seattle 2019



9 Tables

Table 1: REIS Summary Statistics

Avg. Asking Rent($)	1373.9
	(825.8)
Occupancy Rate(%)	93.15
	(7.807)
Avg. Units Per Building	193.9
	(168.4)
$N_{building}$	37,216
$N_{company}$	11,523
N_{state}	30
N_{metro}	50
N_{submkt}	666

Table 2: Summary Statistics on Algorithmic Pricing Adoption (As of 2019)

	By Building	By Unit
Adopter Count	9,138	2,424,176
Total Count	37,216	7,215,831
Fraction Adopted	25%	34%

Notes: Sample based on REIS market-rate apartment buildings in top 50 metros.

Table 3: Number of Market Segments by Degrees of Algorithmic Pricing Penetration

Year	0%	0-10%	10-20%	20-30%	30-40%	40-50%	50-60%	60-70%	70-80%	80-90%	90-100%	Total
2005	200	0	0	0	0	0	0	0	0	0	0	200
2006	141	56	2	1	0	0	0	0	0	0	0	200
2007	118	68	10	4	0	0	0	0	0	0	0	200
2008	62	102	20	12	4	0	0	0	0	0	0	200
2009	39	109	34	16	1	0	1	0	0	0	0	200
2010	21	97	31	28	16	5	1	1	0	0	0	200
2011	10	72	44	32	19	15	6	1	1	0	0	200
2012	8	67	44	33	21	17	6	4	0	0	0	200
2013	6	66	46	28	23	13	14	4	0	0	0	200
2014	8	62	48	28	17	19	15	3	0	0	0	200
2015	6	65	38	29	24	16	17	3	2	0	0	200
2016	5	65	35	29	24	20	16	5	1	0	0	200
2017	4	60	38	28	26	18	21	3	2	0	0	200
2018	3	56	36	33	26	18	21	6	1	0	0	200
2019	3	54	38	29	31	16	23	5	1	0	0	200

Notes: Each segment is defined by a metro area and pre-period rent quartile pair.

Table 4: Adoption of Algorithmic Pricing Over Time (By Unit)

Year	Total	RealPage		LRO		Yardi		All Adopter	
	N	N	%	N	%	N	%	N	%
2005	5,409,743	0	0	0	0	0	0	0	0
2006	5,486,701	33,923	1	58,318	1	0	0	92,241	2
2007	5,571,685	67,085	1	133,772	2	0	0	200,857	4
2008	5,672,969	214,145	4	190,257	3	0	0	404,402	7
2009	5,792,266	227,320	4	339,060	6	0	0	566,380	10
2010	5,876,078	520,595	9	477,301	8	0	0	997,896	17
2011	5,908,455	771,728	13	694,395	12	9,043	0	1,475,166	25
2012	5,972,479	838,338	14	735,287	12	9,210	0	1,582,835	27
2013	6,094,138	908,790	15	772,997	13	9,210	0	1,690,997	28
2014	6,255,269	962,429	15	810,964	13	9,926	0	1,783,319	29
2015	6,431,098	1,022,130	16	855,321	13	42,793	1	1,920,244	30
2016	6,618,142	1,116,586	17	888,224	13	51,787	1	2,056,597	31
2017	6,823,664	2,126,859	31	0	0	65,624	1	2,192,483	32
2018	7,052,178	2,274,283	32	0	0	65,944	1	2,340,227	33
2019	7,215,831	2,357,967	33	0	0	66,209	1	2,424,176	34

Table 5: Adoption of Algorithmic Pricing Over Time (By Building)

Year	Total	RealPage		LRO		Yardi		All Adopter	
	N	N	%	N	%	N	%	N	%
2005	29,041	0	0	0	0	0	0	0	0
2006	29,375	95	0	210	1	0	0	305	1
2007	29,719	194	1	479	2	0	0	673	2
2008	30,141	704	2	662	2	0	0	1,366	5
2009	30,620	758	2	1,154	4	0	0	1,912	6
2010	30,941	1,847	6	1,645	5	0	0	3,492	11
2011	31,112	2,746	9	2,528	8	34	0	5,308	17
2012	31,427	3,042	10	2,721	9	36	0	5,799	18
2013	31,990	3,276	10	2,892	9	36	0	6,204	19
2014	32,727	3,497	11	3,073	9	40	0	6,610	20
2015	33,553	3,745	11	3,242	10	141	0	7,128	21
2016	34,423	4,108	12	3,369	10	184	1	7,661	22
2017	35,399	7,933	22	0	0	235	1	8,168	23
2018	36,465	8,548	23	0	0	237	1	8,785	24
2019	37,216	8,900	24	0	0	238	1	9,138	25

Table 6: Model Predictions of Different Pricing Paradigms

Panel A: Within-Market Comparison Between Adopters and Non-Adopters

	Responsive Pricing (Bust Period)	Responsive Pricing (Boom Period)	Coordinated Pricing (All Periods)
$p^A - p^{NA}$	$-$	$+$	$+^*$
$Q^A - Q^{NA}$	$+$	$-$	$-$

Panel B: Across-Market Comparative Statics with Penetration h for Adopters

	Responsive Pricing (Bust Period)	Responsive Pricing (Boom Period)	Coordinated Pricing (All Periods)
p^A	$\searrow$	$\nearrow$	$\nearrow$
Q^A	$\searrow$	$\nearrow$	$\searrow$

Notes: The table above summarizes the model predictions for each of the pricing paradigms based on the stylized models. The top panel summarizes the predicted differences in price and quantity between adopters and non-adopters within the same market. Note that $+^*$ denotes weakly positive. The bottom panel summarizes the comparative statics in terms of how price and quantity for adopters changes as the adoption penetration h increases across markets.

Table 7: Building Characteristics Comparison: Adopters vs. Non-adopters

	Non-Adopters	Adopters	T-test Difference
Log(Avg. Asking Rent($))	7.03	7.27	0.24***
	(0.47)	(0.47)	
Occupancy Rate(%)	93.64	91.63	-1.97***
	(7.37)	(8.84)	
Free Rent(Month)	0.03	0.04	0.00606***
	(0.02)	(0.02)	
Num. Floors	3.88	4.97	1.07***
	(4.38)	(6.11)	
Year Built	1979.67	1995.05	15.41***
	(23.93)	(19.47)	
Frac. Class A	0.35	0.68	0.34***
	(0.48)	(0.46)	
Frac. Pool	0.64	0.83	0.20***
	(0.48)	(0.38)	
Frac. Doorman	0.03	0.05	0.01***
	(0.18)	(0.21)	
Frac. Tennis Court	0.00	0.01	0.00**
	(0.07)	(0.08)	
Frac. Parking Garage	0.04	0.09	0.05***
	(0.21)	(0.29)	
Frac. Clubhouse	0.35	0.65	0.30***
	(0.48)	(0.48)	
$N_{building}$	28,078	9,138	
$\text{Shr}_{building}$	75.4%	24.6%	
N_{unit}	4,791,655	2,424,176	
Shr_{unit}	66.4%	33.6%	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Building-level Treatment Effects of Adoption by Calendar Year

	TWFE		2SLS		CSDID	
Year	Log(Rent)	Occ(%)	Log(Rent)	Occ(%)	Log(Rent)	Occ(%)
2006	0.030***	-0.493**	-0.082	3.901	0.003	-0.482
	(0.004)	(0.203)	(0.163)	(8.968)	(0.022)	(0.728)
2007	0.011***	-0.181	0.021	-6.813	-0.002	-0.072
	(0.004)	(0.245)	(0.037)	(4.793)	(0.005)	(0.720)
2008	0.006	-0.042	-0.020	-1.716*	-0.009	0.303
	(0.005)	(0.148)	(0.015)	(0.956)	(0.010)	(0.400)
2009	-0.036***	0.262**	-0.173***	-0.507	-0.038**	0.395
	(0.009)	(0.129)	(0.038)	(0.822)	(0.017)	(0.417)
2010	-0.015***	-0.191*	-0.073***	-0.697*	-0.007	-0.224
	(0.004)	(0.113)	(0.016)	(0.387)	(0.006)	(0.175)
2011	-0.006**	-0.173	-0.040***	-0.922***	0.001	-0.184
	(0.003)	(0.107)	(0.008)	(0.302)	(0.004)	(0.162)
2012	0.002	-0.388***	-0.023***	-1.120***	0.007*	-0.459***
	(0.002)	(0.104)	(0.007)	(0.289)	(0.004)	(0.162)
2013	0.007**	-0.175	-0.015**	-0.715***	0.013***	-0.485**
	(0.003)	(0.108)	(0.006)	(0.276)	(0.004)	(0.191)
2014	0.013***	-0.305**	0.006	-0.829***	0.018***	-0.599**
	(0.003)	(0.124)	(0.006)	(0.283)	(0.005)	(0.242)
2015	0.026***	-0.355***	0.036***	-0.953***	0.027***	-0.717***
	(0.003)	(0.115)	(0.007)	(0.260)	(0.005)	(0.244)
2016	0.027***	-0.407***	0.039***	-0.854***	0.025***	-0.648**
	(0.003)	(0.116)	(0.007)	(0.256)	(0.005)	(0.288)
2017	0.025***	-0.352***	0.040***	-0.620**	0.024***	-0.704**
	(0.003)	(0.122)	(0.007)	(0.258)	(0.006)	(0.281)
2018	0.023***	-0.280**	0.043***	-0.414	0.020***	-0.502*
	(0.004)	(0.124)	(0.008)	(0.269)	(0.006)	(0.278)
Control Group	Pooled	Pooled	Pooled	Pooled	Never	Never
Building FE	Y	Y	Y	Y	Y	Y
Metro-Renttile-Year FE	Y	Y	Y	Y	Y	Y
F-Stat			55.1	55.1		
N_{obs}	413,850	413,850	413,850	413,850	392,991	392,991

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The sample is restricted to buildings constructed before 2005. TWFE and 2SLS use pooled control groups (including pre-treatment observations of treated units). CSDID uses never-treated control (excluding pre-treatment observations of treated units), and therefore has a smaller effective sample size. 2SLS uses metro-level algorithmic adoption rate as the instrument. The model includes building-level fixed effects and metro-renttile-year fixed effects, with market segments defined by metro and pre-period rent quartile pairs. For the CSDID specification, building-level characteristic-specific time trends are also controlled using the doubly robust estimator, in addition to the fixed effects. Standard errors are clustered at the management company level in all specifications.

Table 9: Treatment Heterogeneity by Macroeconomic Conditions

$\log(\mathit{Rent})$	(1)	(2)	(3)
ΔUnemployment Rate$_{M,t}$ $\times$ Adopter$_{j,t}$	-0.0374***		
	(0.0102)		
ΔHousehold Income$_{M,t}$ $\times$ Adopter$_{j,t}$		0.0767***	
		(0.0201)	
ΔHome Price Index$_{M,t}$ $\times$ Adopter$_{j,t}$			0.0406*
			(0.0167)
Constant	6.964***	6.962***	6.930***
	(0.000295)	(0.000297)	(0.000295)
Time Varying Treatment Effects	Y	Y	Y
Building FE	Y	Y	Y
Segment-Year FE	Y	Y	Y
N_{obs}	385,939	383,207	374,533

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The sample is restricted to buildings constructed before 2005. The model includes building-level fixed effects and segment-year fixed effects, with market segments defined by metro and pre-period rent quartile pairs. Standard errors are clustered at the management company level in all specifications.

Table 10: Adopter Behavior by Algorithm Penetration

	Tract-Level Penetration				Zip-Level Penetration			
	(1) ln(p)	(2) ln(p)	(3) occ	(4) occ	(5) ln(p)	(6) ln(p)	(7) occ	(8) occ
is YS =1 × YS share (tract)	0.0405***	0.0406***	-4.0090***	-3.7444***				
	(0.0154)	(0.0154)	(0.7144)	(0.8082)				
is LRO=1 × LRO share (tract)	0.0711***	0.0742***	-5.5832***	-5.1862***				
	(0.0198)	(0.0180)	(1.5464)	(1.4221)				
is YS =1 × LRO share (tract)		-0.0025		1.5127				
		(0.0140)		(1.7581)				
is LRO=1 × YS share (tract)		0.0108		1.3036				
		(0.0154)		(0.9276)				
is YS =1 × YS share (zip)					0.0239	0.0236	-0.9967	-0.7436
					(0.0148)	(0.0149)	(0.9786)	(1.0006)
is LRO=1 × LRO share (zip)					0.0567***	0.0560***	-5.5636***	-5.1545***
					(0.0176)	(0.0180)	(1.5442)	(1.3695)
is YS =1 × LRO share (zip)						0.0154		-2.8091**
						(0.0172)		(1.3548)
is LRO=1 × YS share (zip)						-0.0073		2.6891**
						(0.0172)		(1.1632)
Building FE	Y	Y	Y	Y	Y	Y	Y	Y
Segment-Year FE	Y	Y	Y	Y	Y	Y	Y	Y
N	53,408	53,408	53,408	53,408	63,277	63,277	63,277	63,277
R-sq	0.974	0.974	0.424	0.424	0.973	0.973	0.419	0.420

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The sample includes current algorithm adopters in tracts (or zip codes) with more than one building. The dependent variable is log rent in odd columns and occupancy rate in even columns. Penetration is measured as the capacity-weighted algorithm adoption share at the tract or zip level, separately for YieldStar and LRO. All regressions include building and segment-year fixed effects, with standard errors two-way clustered by management company and census tract. Regressions are weighted by unit count.

Table 11: Sample Summary Statistics for Demand Estimation

Panel A: Price and Quantity		
Rent (\$1000)	1.431	
	(0.767)	
Occupancy (%)	94.8	
	(8.4)	
Panel B: ACS Moments		
	Data	*Model*
$\text{Inc}_i \times \text{Price}_j$	0.24	0.23
$\mathbb{E}[\text{Inc}_i \mid j>0]/\mathbb{E}[\text{Inc}_i]$ (ACS)	1.3	1.3
$\mathbb{1}\{\text{Has Child}_i\} \times \text{Bed}_j$	0.36	0.35
$\text{Inc}_i \times \text{New Bld}_j$	0.11	0.099
$\text{HH Size}_i \times \text{Bed}_j$	0.43	0.42
$\mathbb{1}\{\text{Age}_i > 35\} \times \text{New Bld}_j$	-0.062	-0.058
$\mathbb{E}[\text{HH Size}_i \mid j>0]/\mathbb{E}[\text{HH Size}_i]$ (ACS)	0.74	0.74
$\mathbb{E}[\mathbb{1}\{\text{Has Child}_i\} \mid j>0]/\mathbb{E}[\mathbb{1}\{\text{Has Child}_i\}]$ (ACS)	0.47	0.48
$\mathbb{E}[\mathbb{1}\{\text{Age}_i > 35\} \mid j>0]/\mathbb{E}[\mathbb{1}\{\text{Age}_i > 35\}]$ (ACS)	0.88	0.88
$\mathbb{E}[\text{Inc}_i \mid \text{Class A}_j,\ j>0]/\mathbb{E}[\text{Inc}_i \mid j>0]$	1.3	1.3
Panel C: Geographical Markets		
N_{metro}	14	
N_{submkt}	203	
Panel D: Fraction of Units Adopted (as of 2019)		
Mean	32.0%	
P5	2.8%	
P25	16.0%	
P75	43.7%	
P95	69.0%	

Notes: Estimation sample contains ten metropolitan areas from 2007 to 2019: Atlanta, Charlotte, Dallas, DC, Houston, Los Angeles, Minneapolis, Portland, San Diego, and Seattle. Panel A reports mean rent and occupancy with standard deviations in parentheses. Panel B compares data moments computed from ACS microdata with model-predicted moments from the estimated demand system. Panels C and D report market structure and algorithm adoption statistics.

Table 12: Estimated Demand Parameters

Panel A: Price Coefficients	
α_0 :	-5.95
	(0.0863)
α_y : ($\times$ log(Income))	0.42
	(0.0023)
Panel B: Other Heterogenous Coefficients	
Inside Good x Income	0.65
	(0.0050)
Inside Good x HH Size	-0.75
	(0.0084)
Inside Good x Has Child	-0.01
	(0.0064)
Inside Good x Age > 35	-0.25
	(0.0041)
Bed x HH Size	0.80
	(0.0090)
Bed x Has Child	0.21
	(0.0072)
New Bld x Income	0.05
	(0.0065)
New Bld x Age > 35	-0.22
	(0.0074)
Class A x Income	0.35
	(0.0028)
N_{mt}	2,353
N_{bld}	11,613
N_j	30,128
N_{jt}	285,940
Unit-type FE	Y
Market (Submarket-Year) FE	Y
Panel C: Estimated Elasticities	
Mean Own Elasticity	-3.26
Median Own Elasticity	-3.00
Median Aggregate Elasticity	-1.30
Median Outside Good Diversion	0.79
Panel D: First Stage (Excluded IV $\rightarrow$ Price)	
First-stage F-stat	168.3
Partial R^2	0.0020
N Excluded Instruments	3

Notes: Estimation sample contains ten metropolitan areas from 2007 to 2019: Atlanta, Charlotte, Dallas, DC, Houston, Los Angeles, Minneapolis, Portland, San Diego, and Seattle. Panel D reports first-stage statistics for the original Gandhi and Houde (2019) quadratic differentiation instruments (across-firm only). With PyBLP optimal instruments, the first-stage F-stat is 283.8 (partial R^2 = 0.0122, 11 instruments).

Table 13: Test of Own vs. Joint Profit Maximization Among Adopters

	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0
$\tau^A_{L,1} = 0$ vs $\tau^A_{L,2}$	2.97	2.85	2.82	2.74	2.71	2.61	2.46	2.50	2.24	2.21
$\tau^A_{R,1} = 0$ vs $\tau^A_{R,2}$	3.39	3.53	3.31	3.19	3.13	3.15	3.08	3.00	2.92	2.82
$\tau^A_{M,1} = 0$ vs $\tau^A_{M,2}$	3.85	3.63	3.45	3.21	2.96	2.70	2.45	2.15	1.82	1.48

Notes: The result of the pair-wise RV test comparing a model of joint profit maximization at level τ^A (model 2) against a model of own profit maximization (model 1). A significant negative test statistic implies that it favors a model of own-profit-maximization, namely, competition. A significant positive test statistic implies that it favors a model of joint profit maximization, namely, partial or full coordination. The standard errors are computed based on 50 draws of the bootstrap whereby we redraw the management companies in the market.

Table 14: Test of Own vs. Joint Profit Maximization Among Software (2009-2017)

	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0
$\tau^A_{RL,1} = 0$ vs $\tau^A_{RL,2}$	1.92	1.68	1.76	1.65	1.50	1.37	1.07	1.19	0.86	0.46

Notes: The result of the pair-wise RV test comparing a model of joint maximization *between* users of different software at level τ^A and a model of joint maximization *within* adopters of the same software. A significant negative test statistic implies that it favors a model of competition between users of the two software companies, whereas a significant positive test statistic implies that it favors a model of coordination between them. The standard errors are computed based on 50 draws of the bootstrap whereby we redraw the management companies in the market.

Table 15: Estimated Markup Differences Due to Coordination

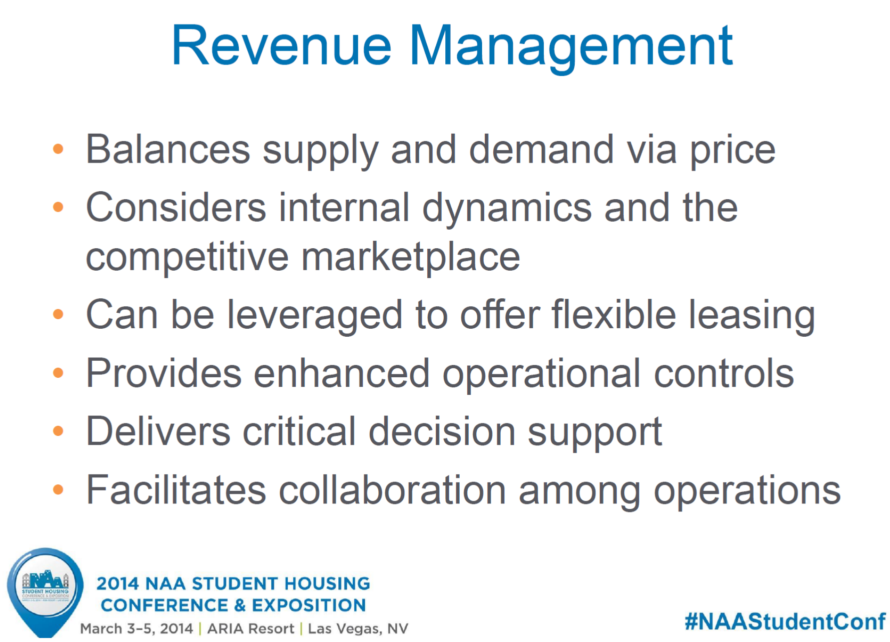
Year	Algo Adoption Fraction (% Bld)	Prices ($/mo)	Markup Difference ($/mo)	p(25)	p(50)	p(75)
2009	6.5	1724.4	13.6	2.5	7.6	17.6
2010	11.8	1517.5	11.3	1.5	5.5	15.7
2011	20.8	1314.3	12.3	3.0	9.1	15.9
2012	19.3	1527.3	11.9	2.0	7.4	15.7
2013	20.5	1531.1	22.6	6.4	16.1	32.3
2014	21.5	1595.0	22.7	7.3	17.3	34.1
2015	21.5	1722.7	23.2	8.2	19.7	32.7
2016	22.8	1740.6	24.3	8.5	19.9	35.7
2017	23.5	1759.2	25.4	8.6	20.9	36.9
2018	23.9	1773.4	24.0	8.8	21.1	35.5
2019	24.5	1798.2	24.9	8.9	21.7	36.8
2018*	23.9	1773.4	51.3	19.3	45.5	75.0
2019*	24.5	1798.2	53.1	20.4	46.3	78.0

Notes: The table tabulates the average and the distribution of markup differences between a model of coordination (i.e., joint profit maximization) and a model of competition (i.e., own profit maximization) among the RealPage and LRO adopters from 2009 to 2019 in our sample of cities. The last two rows (2018* and 2019*) compute the hypothetical markup difference if the merged RealPage led to full coordination among its adopters.

A Appendix Figures

Appendix Figure A1: YieldStar Presentation

(a) Overview of Revenue Management

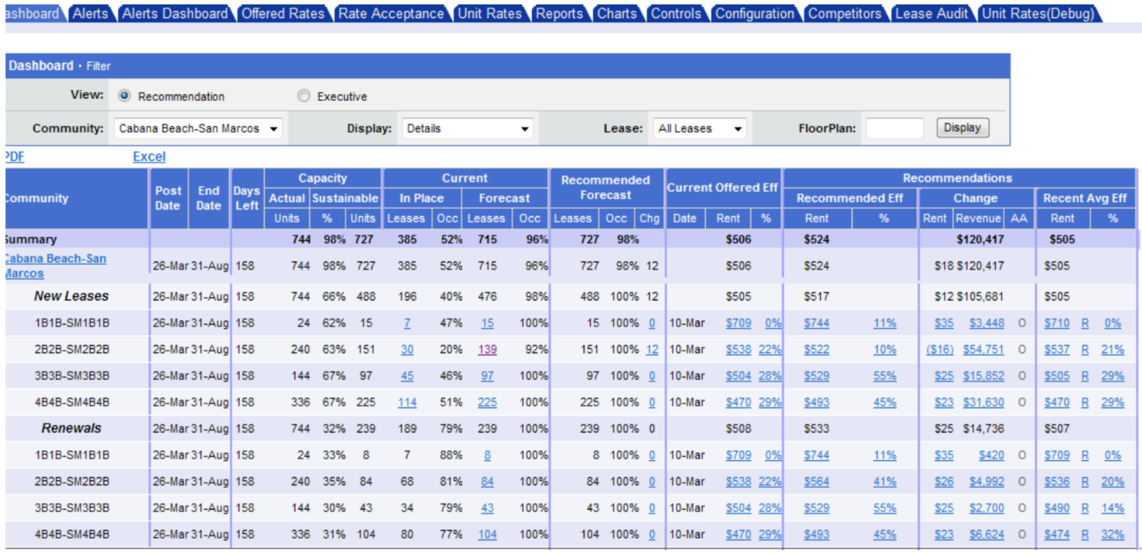


(b) Details

Driving Outperformance: Ensuring Success with Revenue Management, Presentation by Keith Dunkin of Yieldstar, at the 2014 NAA Student Housing Conference & Exposition https://web.archive.org/web/20221104163118/https://www.naahq.org/sites/default/files/naa-documents/meetings/student-housing/D1-Ensuring-your-success.pdf, accessed December 1, 2022.

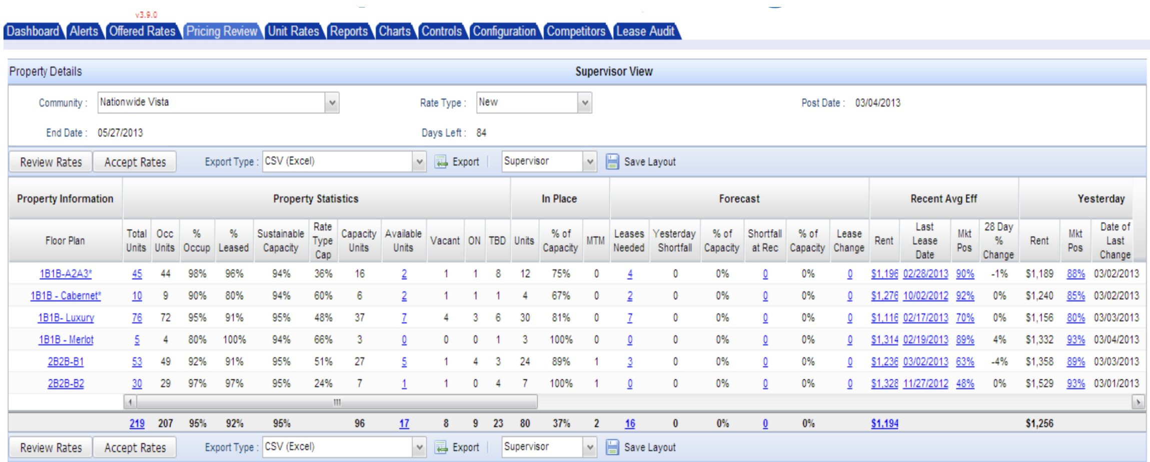
Appendix Figure A2: Manager's View of Yieldstar Pricing Dashboard

(a) Price recommendation made by Yieldstar



Community	Post Date	End Date	Days Left	Capacity: Actual Units	Capacity: Sustainable %	Capacity: Sustainable Units	Current: In Place Leases	Current: In Place Occ	Current: Forecast Leases	Current: Forecast Occ	Recommended Forecast: Leases	Recommended Forecast: Occ	Recommended Forecast: Chg	Current Offered Eff: Date	Current Offered Eff: Rent	Current Offered Eff: %	Recommendations: Recommended Eff Rent	Recommendations: Recommended Eff %	Recommendations: Change Rent	Recommendations: Change Revenue	Recommendations: Change AA	Recommendations: Recent Avg Eff Rent		Recommendations: Recent Avg Eff %
Summary				**744**	**98%**	**727**	**385**	**52%**	**715**	**96%**	**727**	**98%**			**$506**		**$524**			**$120,417**		**$505**		
Cabana Beach-San Marcos	26-Mar	31-Aug	158	744	98%	727	385	52%	715	96%	727	98%	12		$506		$524		$18	$120,417		$505		
New Leases	26-Mar	31-Aug	158	744	66%	488	196	40%	476	98%	488	100%	12		$505		$517		$12	$105,681		$505		
1B1B-SM1B1B	26-Mar	31-Aug	158	24	62%	15	7	47%	15	100%	15	100%	0	10-Mar	$709	0%	$744	11%	$35	$3,448	O	$710	R	0%
2B2B-SM2B2B	26-Mar	31-Aug	158	240	63%	151	30	20%	139	92%	151	100%	12	10-Mar	$538	22%	$522	10%	($16)	$54,751	O	$537	R	21%
3B3B-SM3B3B	26-Mar	31-Aug	158	144	67%	97	45	46%	97	100%	97	100%	0	10-Mar	$504	28%	$529	55%	$25	$15,852	O	$505	R	29%
4B4B-SM4B4B	26-Mar	31-Aug	158	336	67%	225	114	51%	225	100%	225	100%	0	10-Mar	$470	29%	$493	45%	$23	$31,630	O	$470	R	29%
Renewals	26-Mar	31-Aug	158	744	32%	239	189	79%	239	100%	239	100%	0		$508		$533		$25	$14,736		$507		
1B1B-SM1B1B	26-Mar	31-Aug	158	24	33%	8	7	88%	8	100%	8	100%	0	10-Mar	$709	0%	$744	11%	$35	$420	O	$709	R	0%
2B2B-SM2B2B	26-Mar	31-Aug	158	240	35%	84	68	81%	84	100%	84	100%	0	10-Mar	$538	22%	$564	41%	$26	$4,992	O	$536	R	20%
3B3B-SM3B3B	26-Mar	31-Aug	158	144	30%	43	34	79%	43	100%	43	100%	0	10-Mar	$504	28%	$529	55%	$25	$2,700	O	$490	R	14%
4B4B-SM4B4B	26-Mar	31-Aug	158	336	31%	104	80	77%	104	100%	104	100%	0	10-Mar	$470	29%	$493	45%	$23	$6,624	O	$474	R	32%

(b) Competitor data and recommendation acceptance

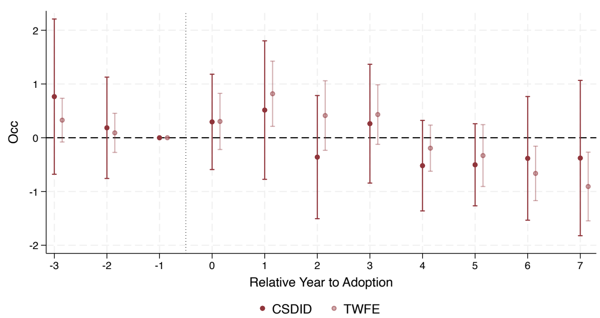


Property Information: Floor Plan	Property Statistics: Total Units	Occ Units	% Occup	% Leased	Sustainable Capacity	Rate Type Cap	Capacity Units	Available Units	Vacant	ON	TBD	In Place: Units	% of Capacity	MTM	Forecast: Leases Needed	Yesterday Shortfall	% of Capacity	Shortfall at Rec	% of Capacity	Lease Change	Recent Avg Eff: Rent	Last Lease Date	Mkt Pos	28 Day % Change	Yesterday: Rent	Mkt Pos	Date of Last Change
1B1B-A2A3*	45	44	98%	96%	94%	36%	16	2	1	1	8	12	75%	0	4	0	0%	0	0%	0	$1,196	02/28/2013	90%	-1%	$1,189	88%	03/02/2013
1B1B - Cabernet*	10	9	90%	80%	94%	60%	6	2	1	1	1	4	67%	0	2	0	0%	0	0%	0	$1,276	10/02/2012	92%	0%	$1,240	85%	03/02/2013
1B1B- Luxury	76	72	95%	91%	95%	48%	37	7	4	3	6	30	81%	0	7	0	0%	0	0%	0	$1,116	02/17/2013	70%	0%	$1,156	80%	03/03/2013
1B1B - Merlot	5	4	80%	100%	94%	66%	3	0	0	0	1	3	100%	0	0	0	0%	0	0%	0	$1,314	02/19/2013	89%	4%	$1,332	93%	03/04/2013
2B2B-B1	53	49	92%	91%	95%	51%	27	5	1	4	3	24	89%	1	3	0	0%	0	0%	0	$1,236	03/02/2013	63%	-4%	$1,358	89%	03/03/2013
2B2B-B2	30	29	97%	97%	95%	24%	7	1	1	0	4	7	100%	1	0	0	0%	0	0%	0	$1,328	11/27/2012	48%	0%	$1,529	93%	03/01/2013
	219	**207**	**95%**	**92%**	**95%**		**96**	**17**	**8**	**9**	**23**	**80**	**37%**	**2**	**16**	**0**	**0%**	**0**	**0%**		**$1,194**				**$1,256**		

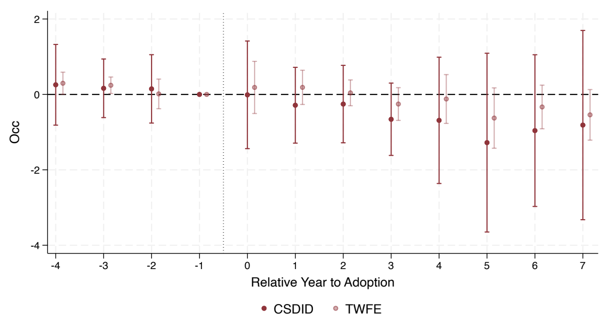
Driving Outperformance: Ensuring Success with Revenue Management, Presentation by Keith Dunkin of Yieldstar, at the 2014 NAA Student Housing Conference & Exposition `https://web.archive.org/web/20221104163118/https://www.naahq.org/sites/default/files/naa-documents/meetings/student-housing/D1-Ensuring-your-success.pdf`, accessed December 1, 2022.

Appendix Figure A3: Treatment Effects of Algorithmic Pricing on Occupancy by Adoption Cohort

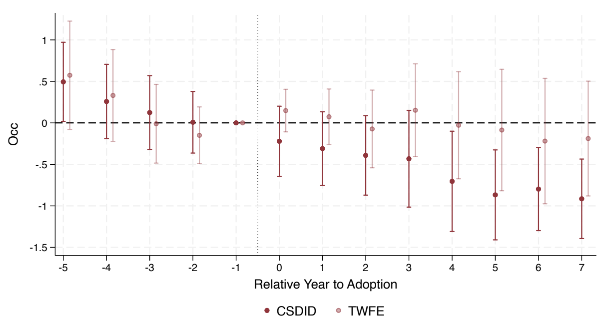
(a) 2008 Adopters



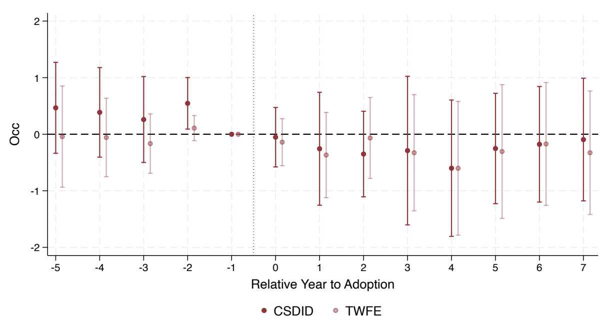
(b) 2009 Adopters



(c) 2010 Adopters



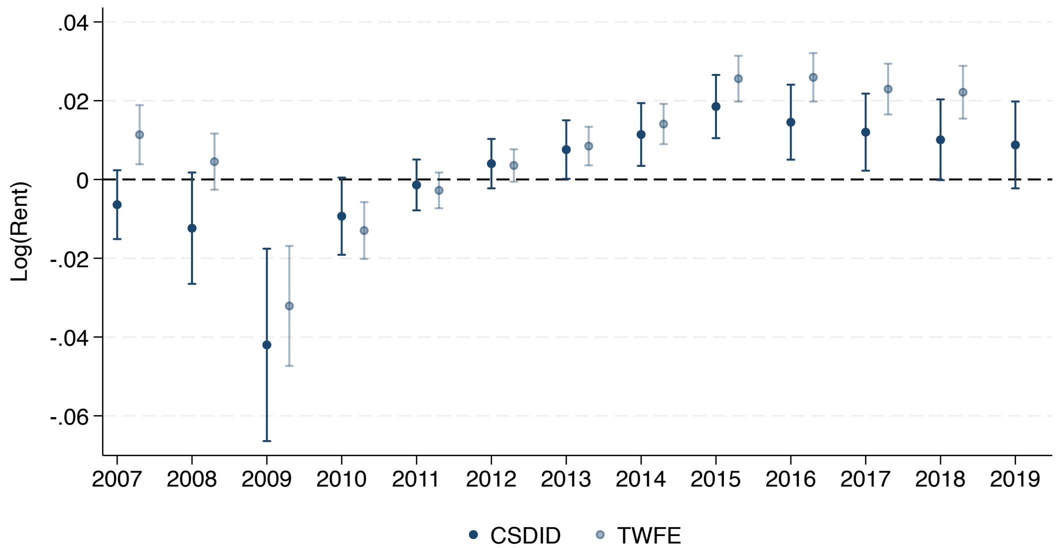
(d) 2011 Adopters



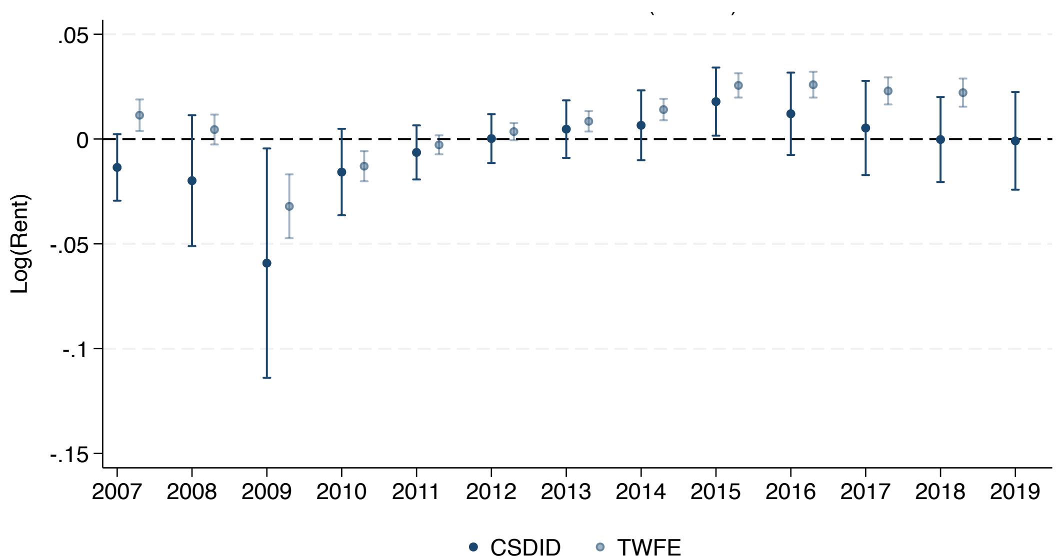
Notes: This figure presents cohort-specific event study estimates for occupancy. Each panel shows the estimated treatment effect for a specific adoption cohort. The sample is restricted to buildings constructed before 2005. CSDID estimator (Callaway and Sant'Anna, 2021) uses never-adopters as the control group and includes building characteristics and market segment indicators as covariates, where market segments are defined by metro and quality-quartile pairs. TWFE includes building fixed effects, year fixed effects, and segment-year fixed effects. Standard errors are clustered at the management company level.

Appendix Figure A4: Treatment Effects by Calendar Year: Robustness to Alternative Segment Definition

(a) Market Segment Defined as Metro-Building Class



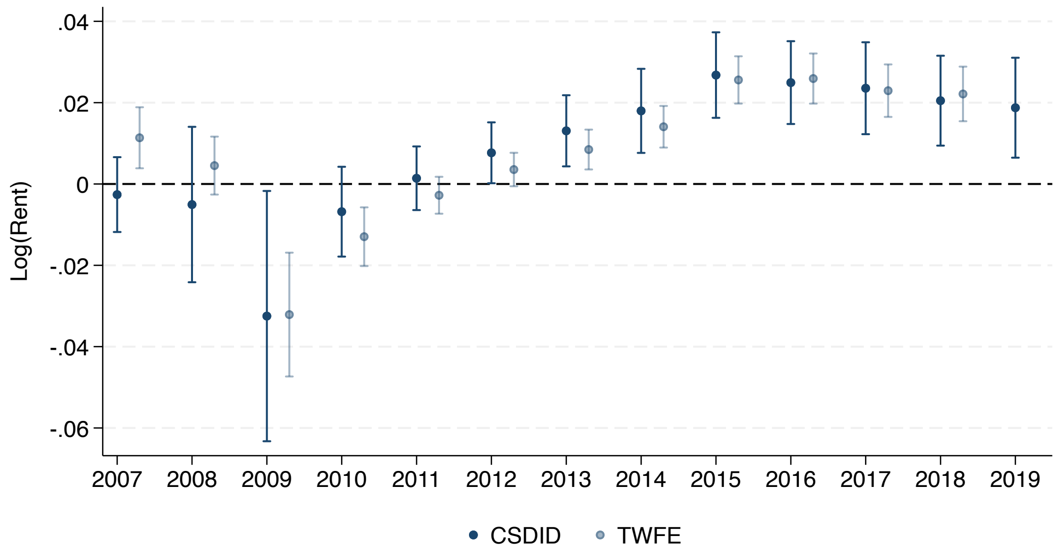
(b) Market Segment Defined as Submarkets



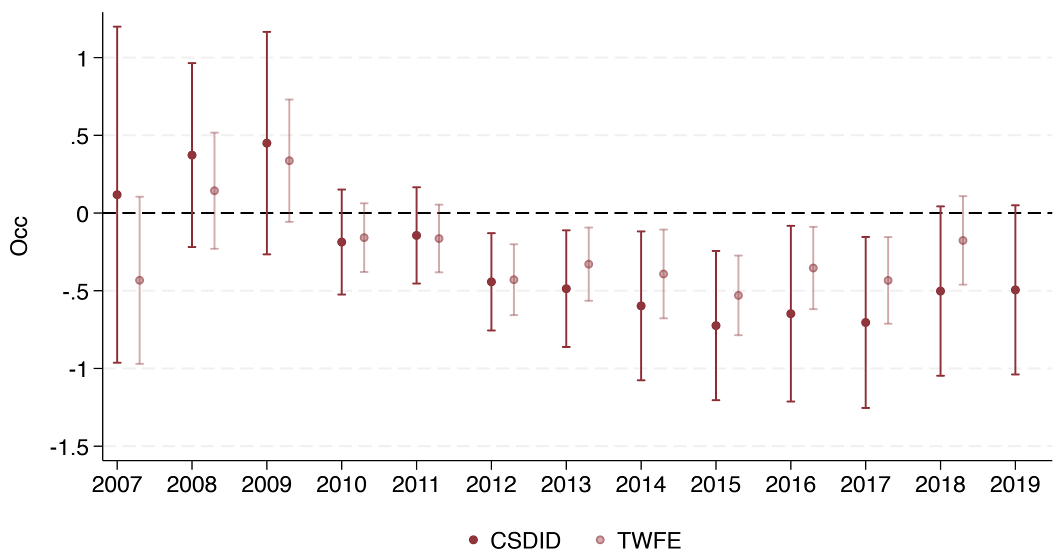
Notes: This figure presents average treatment effects on the treated (ATT) aggregated by calendar year using the Callaway and Sant'Anna (2021) difference-in-differences estimator (CSDID, dark shades). It also reports the treatment effect estimates using traditional two-way fixed effects estimator (TWFE, light shades). The sample is restricted to buildings constructed before 2005. CSDID uses never-adopters as the control group and includes building characteristics and market segment indicators as covariates. TWFE includes building fixed effects, year fixed effects, and segment-year fixed effects. Standard errors are clustered at the management company level.

Appendix Figure A5: Average Treatment Effects by Calendar Year: Alternative Control Group

(a) By Calendar Year on Log(Rent); Never Adopter and Not-Yet-Adopter as Controls



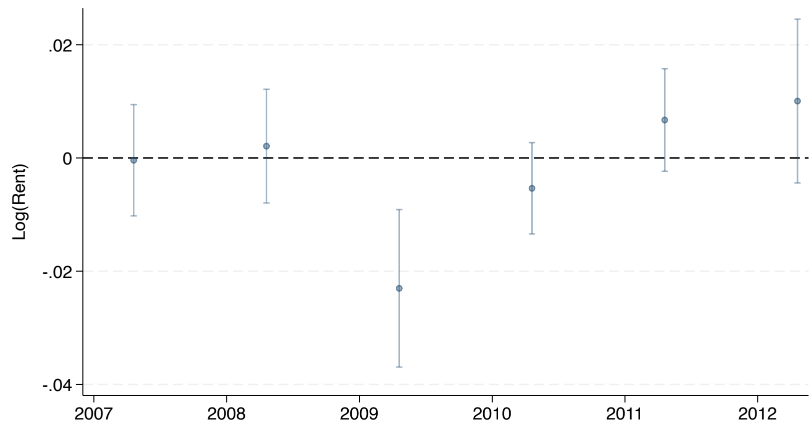
(b) By Calendar Year on Occupancy (%); Never Adopter and Not-Yet-Adopter as Controls



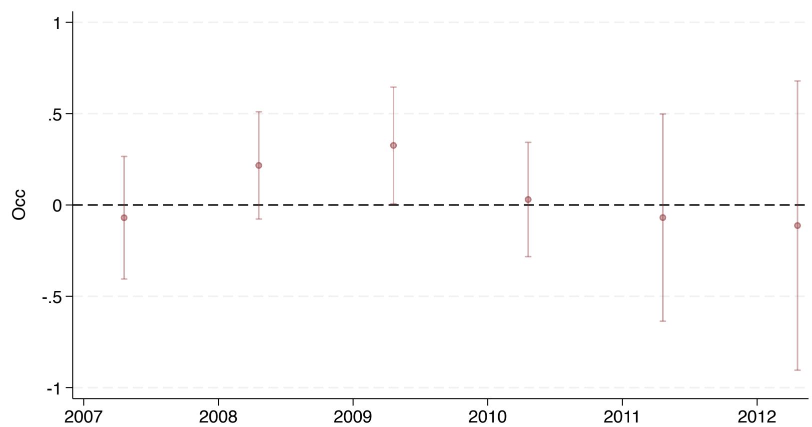
Notes: This figure presents average treatment effects on the treated (ATT) aggregated by calendar year using the Callaway and Sant'Anna (2021) difference-in-differences estimator (CSDID, dark shades). It also reports the treatment effect estimates using traditional two-way fixed effects estimator (TWFE, light shades). The sample is restricted to buildings constructed before 2005. CSDID uses never-adopters and not-yet-adopters as the control group and includes building characteristics and market segment indicators as covariates, where market segments are defined by metro and quality-quartile pairs. TWFE includes building fixed effects, year fixed effects, and segment-year fixed effects. Standard errors are clustered at the management company level.

Appendix Figure A6: Treatment Effects by Calendar Year: Ever-Adopters Only

(a) By Calendar Year on Log(Rent); Adopters vs. Not-Yet-Adopters



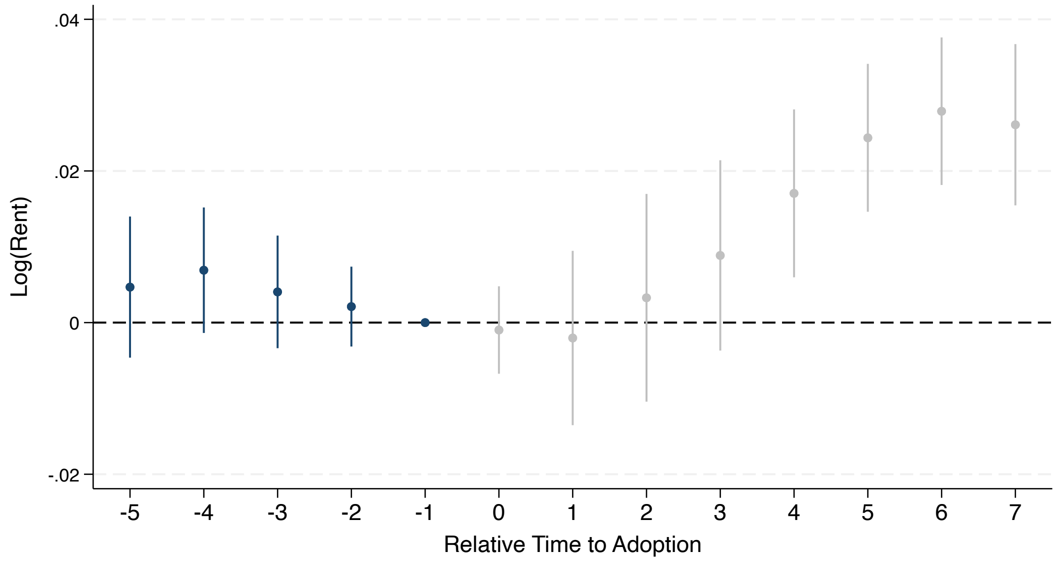
(b) By Calendar Year on Occupancy (%); Adopters vs. Not-Yet-Adopters



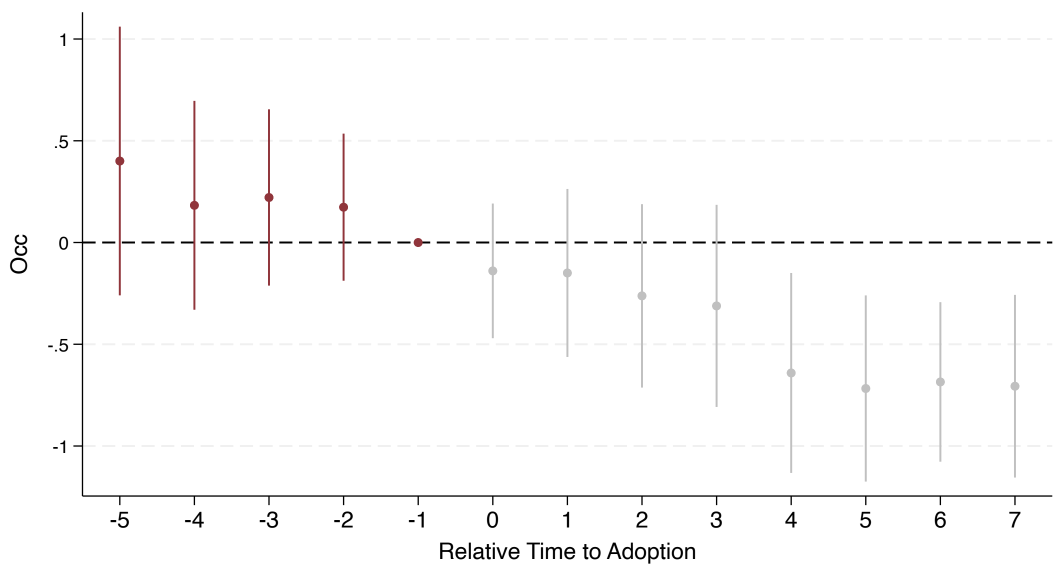
Notes: This figure presents treatment effect estimates by calendar year using the traditional two-way fixed effects estimator (TWFE), restricting the sample to ever-adopters only. The sample is restricted to buildings constructed before 2005. TWFE includes building fixed effects, year fixed effects, and segment-year fixed effects, where market segments are defined by metro and quality-quartile pairs. Standard errors are clustered at the management company level.

Appendix Figure A7: Average Treatment Effects of Algorithmic Pricing by Event Year

(a) By Event Year on Log(Rent)

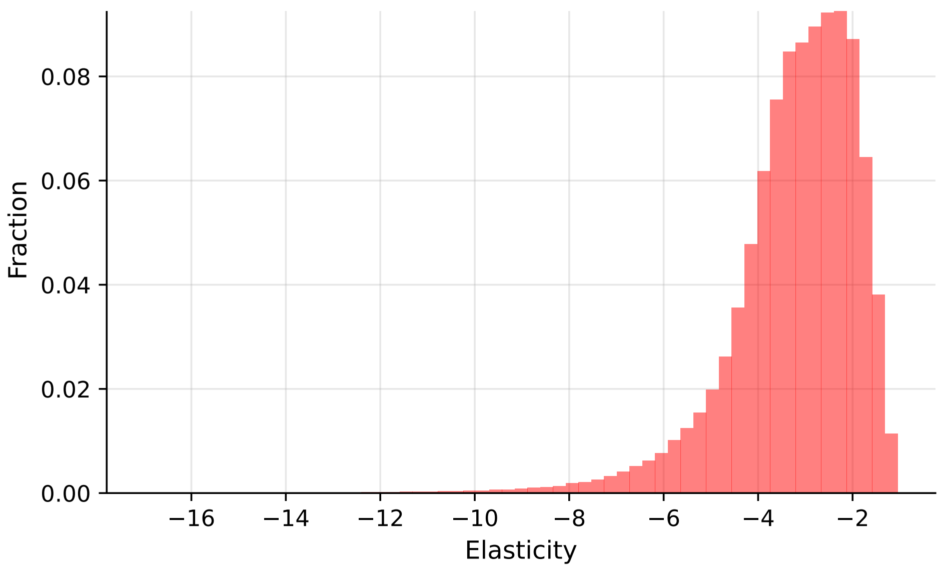


(b) By Event Year on Occupancy (%)

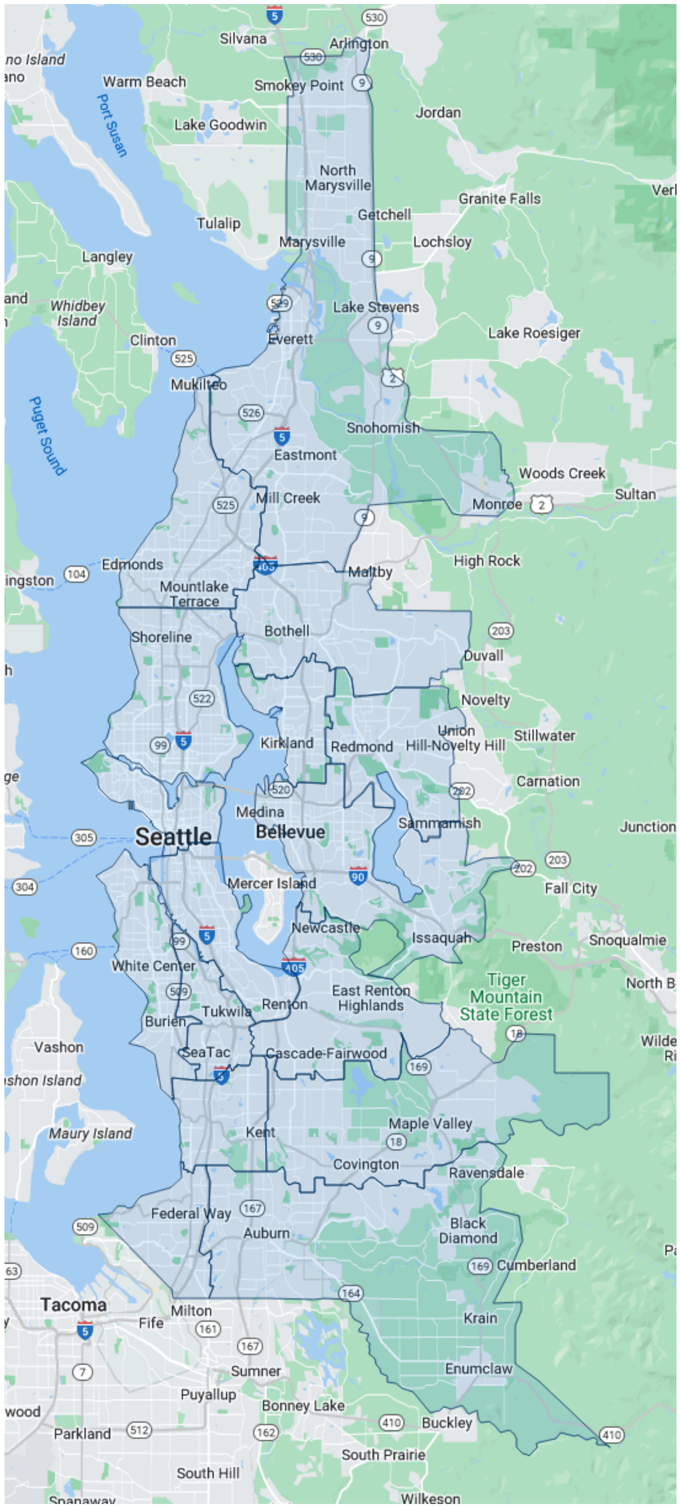


Notes: This figure presents average treatment effects on the treated (ATT) aggregated by event year using the Callaway and Sant'Anna (2021) difference-in-differences estimator. The sample is restricted to buildings constructed before 2005. CSDID uses never-adopters as the control group and includes building characteristics and market segment indicators as covariates, where market segments are defined by metro and quality-quartile pairs. Standard errors are clustered at the management company level.

Appendix Figure A8: Distribution of Estimated Own Elasticities



Appendix Figure A9: REIS Defined Submarket, Seattle Metro



Source: REIS Web Portal

B Appendix Tables

Appendix Table A1: Top Multifamily Management Companies

Company	Units Managed (in Sample)	Adoption Status	Adoption Year	NMHC Ranking (2019)
Greystar	320,598	1	2010	1
Lincoln Property Mgmt	123,920	1	2009	2
Pinnacle	91,977	1	2010	3
MAA	81,641	1	2007	7
Alliance Residential	74,281	1	2011	4
Equity Residential	70,979	1	2006	10
BH Management	63,650	1	2010	8
Avalon Bay	58,377	1	2008	11
Essex	54,361	1	2008	18
Camden	54,170	1	2006	21
Irvine Company	53,796	1	2010	17
Bozzuto	52,203	1	2010	12
United Dominion Realty	45,576	1	2007	30
Cortland	43,889	1	2013	26
Morgan Properties	42,527	1	2011	28
ZRS	36,594	1	2010	32
Bell Partners	35,979	1	2008	31
FPI Management	35,729	1	2011	5
Highmark Residential	32,490	1	2012	19
Avenue5	32,353	1	2018	20

Notes: The number of units managed tabulates the number of market-rate multifamily units managed under a given management company within our data sample of top 50 metros as of 2019 based on the REIS data. This does not account for units outside of these metros, and also excludes non-market rate (affordable) units or student housing. The adoption year is approximated based on various sources of unstructured information collected. NMHC Ranking from https://www.nmhc.org/research-insight/the-nmhc-50/top-50-lists/2019-managers-list/, accessed Feb 2, 2026.

Appendix Table A2: Top and Bottom 5 Metro Areas by Penetration, as of 2019

Metro	Adopted Blds	Total Blds	Penetration(%)
	Top 5 Metros		
Raleigh-Durham	224	504	44
Seattle	573	1331	43
Charlotte	229	546	42
Suburban Virginia	236	580	41
Austin	287	734	39
	Bottom 5 Metros		
Columbus	44	565	8
Cleveland	14	363	4
New Orleans	8	209	4
Cincinnati	16	486	3
Milwaukee	13	400	3

Appendix Table A3: Top and Bottom 10 Submarket Areas by Penetration, as of 2019

Metro	Submarket	Adopted Blds	Total Blds	Penetration(%)
		Top 10 Submarkets		
Orange County	Irvine	72	78	92
Orange County	Newport Beach	14	17	82
Fort Lauderdale	Plantation	21	27	78
Austin	Far Northwest	35	47	74
Orange County	Mission Viejo	34	48	71
Charlotte	Carmel	35	50	70
Denver	Arapahoe County	15	22	68
Austin	Near South Central	17	25	68
Dallas	Central Dallas	67	101	66
Seattle	Redmond	43	65	66
		Bottom 10 Submarkets		
Memphis	East Memphis/University	0	14	0
Milwaukee	Greenfield/Greendale/Franklin	0	54	0
Cleveland	Beachwood	0	25	0
Pittsburgh	Monroeville/Mckeesport/White Oak	0	20	0
San Francisco	Russian Hill/Embarcadero	0	20	0
St. Louis	Airport/I-70	0	43	0
Memphis	Frayser	0	8	0
Pittsburgh	Wilkinsburg/Penn Hills	0	30	0
New Orleans	Kenner	0	13	0
Milwaukee	City West	0	45	0

Appendix Table A4: HHI by Ownership, Management, and Algorithmic Adoption

MSA	Algo Adoption Fraction (%)	HHI Ownership	HHI Management Co	HHI Algo Adoption
Atlanta	45.6	66	198	1692
Charlotte	40.7	76	168	1381
Dallas	35.6	30	117	1067
District of Columbia	35.6	67	171	1152
Houston	24.5	31	101	505
Los Angeles	33.9	31	90	1029
Minneapolis	12.2	32	104	149
Portland	26.1	28	148	582
San Diego	34.1	27	186	1055
Seattle	56.4	61	287	2796

Notes: The table tabulates the Herfindahl-Hirschman Index to measure the concentration in ownership, in management company, and in the adoption of algorithmic pricing software. Each market is defined as the universe of REIS apartment units in a given MSA.

Appendix Table A5: Number of Submarkets by HHI

Year	HHI $< 1{,}000$	HHI $\in [1{,}000, 1{,}800)$	HHI $\geq 1{,}800$	Total
2005	576	68	20	664
2006	577	68	19	664
2007	575	71	19	665
2008	559	86	20	665
2009	554	89	22	665
2010	514	114	37	665
2011	440	148	77	665
2012	430	154	81	665
2013	414	168	83	665
2014	415	164	86	665
2015	406	169	90	665
2016	409	173	84	666
2017	413	171	82	666
2018	408	177	81	666
2019	420	171	75	666
2018*	320	147	199	666
2019*	325	147	194	666

Notes: This table tabulates the number of submarkets in each Herfindahl-Hirschman Index bin across the sample period, treating buildings using the same software as under common ownership, while treating non-adopters as separately owned by building. The last two rows (2018* and 2019*) present HHI values computed under the assumption that merged RealPage adopters are under single ownership. The HHI thresholds follow the Merger Guidelines §2.1 from the Department of Justice and Federal Trade Commission. Submarkets with a small number of buildings exhibit high concentration in earlier years despite having zero software penetration.

Appendix Table A6: First-Stage Regression Results, Pooled and Year-by-Year Estimates

	Pooled	By Year
$Adopt^{IV}_{c,M,t}$	1.854***	
	(0.246)	
2006		-0.373
		(0.580)
2007		0.921
		(0.568)
2008		2.215**
		(0.678)
2009		2.047***
		(0.543)
2010		2.434***
		(0.487)
2011		2.309***
		(0.296)
2012		2.265***
		(0.279)
2013		2.215***
		(0.263)
2014		2.145***
		(0.251)
2015		2.103***
		(0.233)
2016		2.005***
		(0.223)
2017		1.946***
		(0.216)
2018		1.863***
		(0.208)
2019		1.846***
		(0.206)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents coefficients from the first-stage regression of $\mathbb{1}\{Adopt_{j(c)t}\}$ on the adoption exposure instrument, $Adopt^{IV}_{c,M,t}$, with building-level controls, building fixed effects and market segment fixed effects. The first column reports results from a pooled regression across all sample periods, and the second column reports coefficients separately for each year. Standard errors are clustered at the management company level in all specifications.

Appendix Table A7: Adopter Behavior by Algorithm Penetration: Alternative Segment Definitions

	Metro × Rent-tile		Metro × Class		Submarket	
	(1) ln(p)	(2) occ	(3) ln(p)	(4) occ	(5) ln(p)	(6) occ
is YS =1 × YS share (tract)	0.0405***	-4.0090***	0.0277*	-4.4402***	0.0201	-2.7655***
	(0.0154)	(0.7144)	(0.0159)	(1.0338)	(0.0166)	(0.8520)
is LRO=1 × LRO share (tract)	0.0711***	-5.5832***	0.0412***	-5.7077***	0.0435***	-3.9985***
	(0.0198)	(1.5464)	(0.0155)	(1.3348)	(0.0160)	(1.4297)
Building FE	Y	Y	Y	Y	Y	Y
Segment-Year FE	Y	Y	Y	Y	Y	Y
N	53,408	53,408	53,821	53,821	52,798	52,798
R-sq	0.974	0.424	0.974	0.402	0.977	0.470

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The sample includes current algorithm adopters in tracts with more than one building. The dependent variable is log rent in odd columns and occupancy rate in even columns. Penetration is measured as the capacity-weighted algorithm adoption share at the tract level, separately for YieldStar and LRO. All regressions include building fixed effects. Columns (1)–(2) include metro × rent-quartile × year fixed effects, columns (3)–(4) include metro × class × year fixed effects, and columns (5)–(6) include submarket × year fixed effects. Standard errors are two-way clustered by management company and census tract. Regressions are weighted by unit count.

Appendix Table A8: Impact of RealPage and LRO Merger on Rent and Occupancy

	(1) $\Delta\log(Rent)$	(2) $\Delta Occupancy(\%)$	(3) $\Delta\log(Rent)$	(4) $\Delta Occupancy\%$
$\Delta\widehat{HHI}_{mt}$	-0.00000171	-0.0000502	-0.00000139	-0.0000199
	(0.00000198)	(0.0000922)	(0.00000183)	(0.0000841)
$AlgoShare_{m,t-1}$	0.0000591	0.0107	-0.000234	0.0102
	(0.000238)	(0.0112)	(0.000212)	(0.0102)
Δ Macro Controls			Y	Y
Metro-FE			Y	Y
N_{obs}	666	666	650	650

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The sample is restricted to 2017 and 2018. The unit of observation is a submarket.

Appendix Table A9: Test of Own vs. Joint Profit Maximization Among Adopters (Robustness)

	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0
$\tau^A_{L,1} = 0$ vs $\tau^A_{L,2}$	2.62	2.68	2.51	2.45	2.47	2.26	2.35	2.19	2.16	2.14
$\tau^A_{R,1} = 0$ vs $\tau^A_{R,2}$	2.54	2.54	2.48	2.38	2.41	2.38	2.32	2.39	2.30	2.21
$\tau^A_{M,1} = 0$ vs $\tau^A_{M,2}$	3.71	3.54	3.42	3.19	3.02	2.82	2.61	2.35	2.10	1.81

Notes: The result of the pair-wise RV test comparing a model of joint profit maximization at level τ^A (model 2) against a model of own profit maximization (model 1) accounting for shared management companies. A significant negative test statistic implies that it favors a model of own profit maximization, namely, competition. A significant positive test statistic implies that it favors a model of joint profit maximization, namely, partial or full coordination. The standard errors are computed based on 50 draws of the bootstrap whereby we redraw the management companies in the market.

C Stylized Model

In this section, we provide further details on the previously described stylized model, offering intuitive explanations for the comparative statics of responsive and coordinated pricing, respectively.

C.1 Model Primitives

Assume that a market is comprised of homogeneous products with no differentiation,[51] but a capacity constraint at K. Without loss of generality, assume the mass of suppliers is 1. Each supplier is infinitesimal and is also capacity constrained. Further, we assume that the marginal cost of operating the building is the same for all suppliers and it goes to $+\infty$ once above the capacity constraint. Let $D(p)$ denote the quantity demanded at price p. Lastly, assume that a fraction h of the suppliers are adopters, and a fraction of $1-h$ are non-adopters.

C.2 A Stylized Model of Responsive Pricing

To model the responsiveness of prices to market conditions, we consider a two-period model $T = 0, 1$. At $T = 0$, the competitive market equilibrium is achieved at (p_0, Q_0) such that the total quantity demanded equals supply. At $T = 1$, demand conditions change. Non-adopters are *"sleepy"* where they do not adjust their prices to changing market conditions quickly, hanging on to the price from the previous period $p_1^{NA} = p_0$.[52] Adopters, through the usage of the software, are *"alert,"* where they adjust their prices responsively to changing market conditions.

Negative Demand Shock Figure 4 Panel (a) illustrates the market dynamics with a negative demand shock. At $T = 1$, consider a contraction of aggregate demand from D to D_1, whereas supply is unchanged. A fully competitive model would generate a new market clearing price p_1^E and market clearing quantity $Q_1^E = D_1(p_1^E)$.

However, in our model, the non-adopters do not readily update their price $p_1^{NA} = p_0$ and experience a much-reduced quantity at Q_1^{NA}. The adopters, with the help of the software, set prices $p = p_1^{A,h}$ responsively so that their residual demand equals their supply.

$$D_1^{A,h}(p) = D_1(p) - (1-h)Q_1^{NA} \tag{C.1}$$

$$S_1^{A,h}(p) = hS(p) \tag{C.2}$$

With a negative demand shock, because non-adopters are under-producing compared to the competitive benchmark $Q_1^{NA} < Q_1^E$, it means that adopters will price *lower* than non-adopters and produce

[51] The model can be readily extended to a differentiated product setting, as we do in the actual estimation.

[52] Implicitly, in this simple two-period setting, we are making the extreme assumption that the non-adopters do not adjust their prices after the demand has changed. Yet, more realistically, with multiple periods, non-adopters will still learn about the changes in demand, albeit at a rate that is slower than adopters.

a quantity *higher* than non-adopters to clear the market:

$$p_1^{A,h} < p_1^{NA} \tag{C.3}$$
$$Q_1^{A,h} > Q_1^{NA}. \tag{C.4}$$

Note that the price difference between non-adopters and adopters can exist even in this homogeneous product model because each supplier is capacity-constrained.

As the fraction of adopters h increases, the price of the adopters approaches the full competitive equilibrium. For any intermediate level of adoption $h < 1$, the adopters charge a lower price than the non-adopters, but a higher price than the full competitive equilibrium $p_1^E < p_1^{A,h} < p_1^{NA}$. The reason that adopters do not necessarily go all the way down to p_1^E is that a non-zero fraction of $1-h$ non-adopters are under-producing. As h increases, the adopters' price and quantity follow the expansion along its supply curve. With $h = 1$, it restores the full competitive price where $p_1^{A,h=1} = p_1^E$ and $Q_1^{A,h=1} = Q_1^E$.

The shaded area in Figure 4 Panel (a) indicates the welfare gains that are achieved when all suppliers price responsively compared to when all suppliers are unresponsive, in the form of increased surplus accrued to renters.

To summarize, with a negative demand shock, a model of responsive pricing predicts that adopters charge lower prices and produce higher quantities than non-adopters within a market. Across markets, assuming the same severity of negative demand shock, a model of responsive pricing predicts that adopter price and quantity both decrease with the degree of algorithm penetration.

Positive Demand Shock Figure 4 Panel (b) illustrates the market dynamics with a positive demand shock. As such, with an outward-shifted demand, the full equilibrium is indicated by p_1^E and Q_1^E.

Much analogous to the negative demand shock, we consider non-adopters to be "sleepy" and stick with their old prices $p_1^{NA} = p_0$ and experience a much greater quantity than the full competitive benchmark (but may be limited by their capacity constraint) $Q_1^{NA} = \min\{D_1(p_1^{NA}), K\} > Q_1^E$. On the other hand, the adopters set responsive prices to balance their residual demand and supply as described in Equation (C.1) and (C.2).

With a positive demand shock, because non-adopters are over-producing compared to the competitive benchmark, it means that a model of responsive pricing will lead to adopters pricing *higher* than non-adopters, and producing a quantity *lower* than non-adopters. Just as before, as the fraction of adopters h increases, the price and quantity of the adopters increase to approach the level at full competitive equilibrium.

The shaded area in Figure 4 Panel (b) indicates the net welfare gains that are achieved when all suppliers price responsively compared to when all suppliers are unresponsive, in the form of reduced losses accrued to the non-adopters (net of some consumer surpluses accrued to the over-production).

To summarize, with a positive demand shock, a model of responsive pricing makes exactly the opposite prediction to the negative demand shock: adopters charge higher prices and produce lower quantities than non-adopters within a market. Across markets, assuming the same severity of positive demand shock, responsive pricing predicts that adopter price and quantity both increase with the degree of algorithm penetration.

C.3 A Stylized Model of Coordinated Pricing

Next, we derive the markup formula when a fraction h of the market becomes adopters of algorithmic pricing where the algorithm sets a *coordinated* price for them to maximize profits jointly.

Monopoly Benchmark It is instructive to first consider the full monopoly benchmark, which corresponds to a scenario where the fraction of adopters $h = 1$ with a model of coordinated prices. In this case, the sole supplier sets the price to maximize profit:

$$\max_p \pi^M(p) = p\,D(p) - C(D(p)). \tag{C.5}$$

Taking the derivative with respect to price yields the following first-order condition

$$p\frac{dD}{dp} + D(p) - mc\frac{dD}{dp} = 0, \tag{C.6}$$

which yields a monopoly price where the percentage mark-up equals the inverse demand elasticity:

$$\frac{p^M - mc}{p^M} = \frac{1}{\epsilon_D(p^M)}, \quad \text{where} \quad \epsilon_D(p) = -\frac{dD}{dp}\frac{p}{D}. \tag{C.7}$$

Adopter Coordination Consider a market where a fraction h of the suppliers are adopters of algorithmic pricing software that coordinates the pricing among all adopters. As such, the algorithm maximizes the profit of all adopters combined:

$$\max_p \pi^A(p) = p\,D^A(p) - C^A(D^A(p)) \tag{C.8}$$

where the residual demand D^A becomes

$$D^A(p) = D(p) - (1-h)S(p) \tag{C.9}$$

as the remaining non-adopters will supply competitively up to $S(p)$.

The cost faced by adopters supply becomes

$$C^A(p) = hC\ \ D^A(p)/h \tag{C.10}$$

as the demand normalized for each adopter is $D^A(p)/h$.

Taking the derivative with respect to price yields the following first-order condition:

$$p\frac{dD^A}{dp} + D^A(p) - mc\frac{dD^A}{dp} = 0, \tag{C.11}$$

which yields a coordinated price among adopters where the percentage mark-up equals the inverse demand elasticity of the residual demand:

$$\frac{p^A - mc}{p^A} = \frac{1}{\epsilon_{D^A}(p^A)}, \quad \text{where} \quad \epsilon_{D^A}(p^A) = -\frac{dD^A}{dp}\frac{p}{D^A}. \tag{C.12}$$

Notice the direct parallel between the monopoly markup formula in (C.7) and the coordination markup in (C.12).

For any given price,[53] the residual demand becomes more and more inelastic as the share of adopters h increases

$$\frac{\partial(1/\epsilon_{D^A}(p))}{\partial h} > 0, \tag{C.13}$$

which implies that mark-up increases with h. For any marginal cost function that is weakly increasing in quantity, it also implies that as the adoption share h increases, price increases and quantity decreases.

For non-adopters, given that this is a model of homogeneous products, they will try to undercut the adopters ever-so-slightly by charging right below p^A. They will not restrict quantity, but instead offer the competitive supply at $S(p^A)$. Note that $S(p^A) > D(p^A) > D^A(p^A)/h$, which is higher than what each adopter supplies.

To summarize, a model of coordinated pricing predicts that, within the same market, adopters charge a weakly higher price and produce a lower quantity compared to non-adopters. Moreover, the model predicts that mark-up increases with the adoption share h. Consequently, when compared across markets, adopter price increases with the share of adopters h and total quantity decreases with h.

[53] As we can expand the elasticity of the residual demand as $\frac{1}{\epsilon_{D^A}(p)} = \frac{1-(1-h)\frac{S(p)}{D(p)}}{\epsilon_D(p)+(1-h)\frac{S(p)}{D(p)}\epsilon_S(p)}$.

D Pair-wise Testing Algorithm

The testing algorithm described below follows Backus, Conlon, and Sinkinson (2021) with the notation updated to be consistent with the present paper.

Algorithm 1 Testing Procedure

1. Recover marginal cost mc^M from implied markups under each model $M = M_1, M_2$, $\eta(\mathcal{H}^M)$:

$$mc_{jt}^M = p_{jt}^{obs} - \eta_{jt}(\mathcal{H}^M)$$

2-1. For each cost estimate $\hat{mc}^M$, estimate $h^M(x_{jt}, occ_{jt})$ using an IV regression and compute the residual:

$$\omega_{jt}^M = \hat{mc}_{jt}^M - \hat{h}^M(x_{jt}, occ_{jt})$$

2-2. Compute the difference between markups $\Delta\eta_{jt}^{1,2} := \eta_{jt}(\mathcal{H}^{M_1}) - \eta_{jt}(\mathcal{H}^{M_2})$ and fit another flexible function of candidate instrumental variables, $\boldsymbol{z}^S$:

$$\Delta\eta_{jt}^{1,2} = g(x_{jt}, \boldsymbol{z}^S) + \zeta_{jt}$$

3. With $(\hat{\omega}_{jt}^{M_1}, \hat{\omega}_{jt}^{M_2}, \hat{g}(\cdot))$, compute the moment criterion value for each model M:

$$\tilde{Q}(\eta^M) = \left(N_{jt}^{-1} \sum_{j,t} \hat{\omega}_{jt}^M \cdot \hat{g}(\cdot) \right)^2$$

4. Repeat Steps 1 to 3 on bootstrapped samples and estimate the standard error, $\hat{se}$, of the difference between M_1 and M_2, $\tilde{Q}(\eta^{M_1}) - \tilde{Q}(\eta^{M_2})$ across bootstrap iterations.

5. Compute the test statistic

$$T = \frac{\tilde{Q}(\eta^{M_1}) - \tilde{Q}(\eta^{M_2})}{\hat{se}} \sim \mathcal{N}(0, 1).$$