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

Sophie Calder-Wang* Gi Heung Kim†

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Abstract

This paper empirically evaluates the impact of algorithmic pricing on the U.S. multifamily rental market. We hand-collect data on management company adoption decisions of algorithmic pricing and combine it with a comprehensive database of building-level rents and occupancy from 2005 to 2019. We find strong evidence that algorithmic pricing helps building managers set prices that are more responsive to market conditions, with adopters lowering rents more rapidly than non-adopters during economic downturns. We also find that average rents are higher and average occupancies are lower in markets with greater algorithmic penetration during periods of economic recovery. Then, we estimate a structural model of housing demand to test for "algorithmic coordination." Compared to a model of own profit maximization, our pair-wise tests favor a model of joint profit maximization among adopters of the same software. We estimate that the coordination channel results in an average markup increase of $25 per unit per month, impacting about 4.2 million units nationwide. Our findings have important implications for regulators and policymakers concerned about the potential risks and trade-offs of algorithmic pricing.

Keywords: pricing algorithms, real estate, competition policy
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*The Wharton School, University of Pennsylvania *sophiecw@wharton.upenn.edu*
†The Wharton School, University of Pennsylvania *robinkim@wharton.upenn.edu*

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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 utilize 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 is currently facing intense scrutiny regarding the issue of algorithmic pricing. Since 2022, over twenty class action lawsuits have been filed against RealPage, the software company, as well as management companies that use the software, alleging that RealPage's pricing software is responsible for raising prices "above competitive levels" and is a violation of Section 1 of the Sherman Act.[1] This issue has garnered significant media attention[2] and has also captured the interest of regulators, with the U.S. Department of Justice reportedly launching an investigation.[3]

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,[4] effectively creating a

[1] US District Court, Middle District of Tennessee, https://www.tnmd.uscourts.gov/mdl-case-information, accessed July 16, 2024.

[2] New York Times, "Landlords Used Software to Set Rents. Then Came the Lawsuits." https://www.nytimes.com/2024/07/19/business/economy/rent-prices-realpage-lawsuit.html, accessed July 22, 2024.

[3] https://www.politico.com/news/2024/03/20/rental-housing-market-doj-investigation-00147333, accessed July 16, 2024.

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

monopoly with over 95% of adopters being customers of RealPage alone.

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. 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 by examining data during a downturn. However, because these two channels produces 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.

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 submarket and building quality class (Class A vs. B/C). We include an extensive set of controls as well as saturated fixed effects, including building fixed effects as well as segment-year fixed effects, capturing differential trends between luxury and non-luxury buildings in the same submarket at a given time. Both TWFE estimator and CSDID that accounts for staggered adoption (Callaway and Sant'Anna, 2020) 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 submarket and building class. 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 the impact of adoption on market-level average prices and quantities. While these findings cannot directly test coordination, they remain empirically relevant because it provides an estimate of the aggregate magnitude of rent and occupancy changes which also takes into account non-adopters' endogenous responses in the same market. We find that during the Great Recession (2008-2010), average rents were lower and average occupancies were higher in market segments with higher degrees of algorithmic penetration. Conversely, during the recovery period (2014-2016), market segments with higher algorithmic penetration saw higher average rents and lower occupancies, with averages rents about 4% higher in segments with the most algorithmic penetration. We also access the impact of RealPage's 2017 acquisition of LRO on market outcomes. However, we do not find any statistically significant relationship between the predicted changes in HHI due to the acquisition and changes in either rent or occupancy. Although such a null result could be consistent with a lack of coordination, it is also consistent with a lack of full integration between the two pieces of software even after the acquisition, which appeared to be consistent with RealPage's own disclosures.[5]

Third, given the limitations of using reduced-form regressions of price and quantity on adoption status in isolating the "algorithmic coordination" channel, we proceed to estimate a full structural model of renter demand, which allows us to recover the entire matrix of demand elasticities and use it to impose structural relationships on how markups among adopters should be determined by a given model of conduct, and therefore, allowing us to test for adopter conduct.

We estimate a full structural model of rental demand based on a discrete choice random utility

[5] "LRO is also completely separate from YieldStar and AIRM, and contrary to the allegations, LRO does not have access to the YieldStar/AIRM database, and the YieldStar/AIRM database does not have access to the LRO database," based on RealPage statement made on June 18, 2024. https://www.realpagepublicpolicy.com/realpagestatement, accessed August 8, 2024.

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 analyses 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.,[6] this aggregates to an enormous economic magnitude, 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;

[6] $(6.7\% + 3.7\%) \times 40.5 = 4.2$. 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.

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 simulations studies under a variety of different specifications and assumptions.[7]

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.[8]

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.[9] 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, 2024b) could be particularly relevant in empirically settings.

Our paper also adds to the ongoing policy and regulatory debates surrounding algorithmic pricing (Ezrachi and Stucke, 2017; Spann et al., 2024; Harrington, 2024a; Asil and Wollmann, 2024). The primary objective of the paper is to provide a positive analysis to assess whether observed prices and implied markups align more with joint or individual profit maximization. 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 anti-trust 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, when yielding positive results, could be used

[7] This includes Waltman and Kaymak (2008); Kimbrough and Murphy (2009); Cooper, Homem-de-Mello, and Kleywegt (2015); Hansen, Misra, and Pai (2021); 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).

[8] 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 out 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`.)

[9] A related literature has also investigated the issue 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.

to help meet the evidentiary threshold to warrant further investigation. 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 relevant antitrust regulations and enforcement against algorithmic collusion.

Separately, our paper contributes to the literature on the impact of technological innovation in the housing market. Despite 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[10] 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 has 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[11]—even small rent increases can lead to significant transfers from renters to owners.

The remainder of the paper proceeds as follows. Section 2 provides background on the U.S. multifamily housing market and the pricing software used. It also describes the data collection process and shows stylized facts. Section 3 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 3.2. We also measure its implication on the market-level rents and occupancy in Section 3.3. To conduct a test of conduct, we describe and estimate a structural model of housing demand from renters in Section 4. The procedure and the results of the conduct test are presented in Section 5. Section 6 concludes.

[10] 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.

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

2 Background and Data

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.[12] Approximately 21 million out of 45 million occupied rental housing units are considered to be multifamily, and they house over 40 million people.[13] The sector has experienced fast-paced valuation growth after the Great Recession, with a 135% increase in price per square foot from the lowest in Q1 2010 to Q4 2019,[14] reaching a colossal overall valuation at $4.7 trillion.[15] In the meantime, nominal rents have also increased by about 35% in the same time period,[16] with the median renter spending about 30% of their household income on rents.[17]

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 has been recent concerns about rising market power in real estate (Watson and Ziv, 2021, 2024). Within multifamily housing, the top 50 property management companies combined manage over 4 million rental units, which account for a moderate 20% of the overall 20 million multifamily rental stock. That said, these management companies still manage a large number of units in absolute counts. As of 2022, Greystar, the biggest management company in the U.S., manages almost 700,000 units, followed by Lincoln Property Company, which manages over 200,000 units.[18]

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 amounts 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.

[12] Fannie Mae Mortgage Overview, https://multifamily.fanniemae.com/media/5906/display, accessed on August 6, 2024

[13] American Community Survey 2022

[14] FRED Economic Data, https://fred.stlouisfed.org/series/BOGZ1FL075035403Q, accessed on August 6, 2024

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

[16] FRED Economic Data, https://fred.stlouisfed.org/series/CUUR0000SEHA, accessed on August 6, 2024.

[17] American Community Survey 2022

[18] National Multifamily Housing Council, https://www.nmhc.org/research-insight/the-nmhc-50/top-50-lists/2022-top-managers-list/, accessed August 7, 2024.

RealPage, one of the ERP software companies for property management, developed 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."[19] 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 in December, 2017, shortly after it received antitrust clearance from the DOJ.[20] 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 2%.

The number of apartment units who received algorithmic pricing recommendation has grown substantially overtime and consist 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.[21] At the time of LRO acquisition, LRO alone was reported to provide "optimized pricing for over 1.5 million apartments"[22] More recently, RealPage has disclosed that approximately 6.7% of rental properties across all US metropolitan statistical areas use its YieldStar and AI Revenue Management, and 3.7% uses LRO,[23] which amounts to about 4.2 million units as of 2023.[24] 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, based on an FAQ published by RealPage,[25] 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

[19] RealPage 2014 10K, https://www.sec.gov/Archives/edgar/data/1286225/000128622514000008/rp-20131231x10k.htm, accessed August 7, 2024

[20] RealPage press release on November 28, 2017, https://www.realpage.com/news/realpage-receives-antitrust-clearance-lro/, accessed August 7, 2024.

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

[22] RealPage press release on February 27, 2017, https://www.realpage.com/news/realpage-to-acquire-lease-rent-options/, accessed August 7, 2024.

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

[24] $(6.7\% + 3.7\%) \times 40.5 = 4.2$, where there are about 40.5 million rental units in all US MSAs based on 2022 ACS.

[25] 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.

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 show the dashboard view for a property manager, which displays the price recommendations made by the software, summarizing complex information and reduces 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.[26]

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 maximizes 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 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 several additional sources, capturing property ownership from Real Capital Analytics (RCA) and commercial property tax records from CoreLogic.

2.2.1 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, club house, swimming pool etc. It also records building level characteristics including the year built, building class (Class A vs. B/C), and the composition of each 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

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

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, Fannie Mae reported that there are about 17 million market-rate units nationwide in 2021.[27] Given that there were about 1.5 million new units constructed from 2019 and 2021,[28] we believe our data covers approximately half of the universe of all market-rate apartment units in the U.S. 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 structures across the same set of MSAs, it suggests that our coverage of such large apartment buildings is about 80%.[29]

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 independent variable, it leads to an attenuation bias, suggesting that our reduced form estimates is likely a lower-bound on the true magnitude. In terms of the conduct test, such misclassification also makes it harder for the conduct test to find evidence in favor of coordination, suggesting our test will be under-powered compared to a true test of coordination without such measurement error.

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

[27] https://multifamily.fanniemae.com/news-insights/multifamily-market-commentary/assessing-market-rate-affordable-multifamily-sector

[28] https://www.jchs.harvard.edu/sites/default/files/reports/files/Harvard_JCHS_The_State_of_the_Nations_Housing_2020_Report_Revised_120720.pdf

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

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.

2.2.2 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 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 remain as an adopter afterward.

We estimate about 25% of the buildings and 34% of the units in the REIS apartment have adopted one of the 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 to be adopters,[30] 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 illustrate 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%. 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 submarket building-class pair. Overtime, 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 that it allows us to record when and which software is adopted by a given management company. The natural concern is, of course, potential

[30] 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.

measurement error, in terms of both false positives and false negative. 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,[31] 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.

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[32] 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.[33] 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 our data also correctly identifies the adoption status of management companies involved in recent class action lawsuits.

2.2.3 Additional Data Sources

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

Commercial Real Estate Tax Records We obtain property-level tax amounts and tax assessments for all apartments from CoreLogic. Specifically, we merge the REIS building addresses with CoreLogic to obtain a panel of property tax paid and property tax assessment throughout the sample period.

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

[31] RealPage 2019 and 2020 10K.

[32] 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.

[33] $(6.7\%/(1+3.3\%)^4 + 3.7\%) \times 25,514,472 = 2,550,970$, where the total number of rentals is based on 2022 American Community Survey.

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.

3 Reduced-Form Estimates of the Impact of Algorithmic Pricing

3.1 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 channels: responsive pricing and coordinated pricing. We show that it is relatively straightforward to test for the presence of the responsive pricing channel through reduced-form regressions, and discuss why it is much harder to isolate the coordination channel.

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 the changes in the market condition and update 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. By contrast, adopters are be 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, when faced with a negative demand shock, (i) adopters would charge lower prices and experience higher occupancies than non-adopters in that market; (ii) across markets, average prices would decrease and

average occupancy would increase as the degree of algorithmic penetration increases.

Conversely, Figure 4 Panel (b) illustrates the market dynamics with a positive demand shock. In this case, non-adopters are slow to increase its prices when faced with a stronger demand, leading to too few vacancies. Similarly, adopters hike their prices promptly as the algorithm is faster at recognizing the emergence of a booming market. As such, when faced with a positive demand shock, the same model of responsive pricing makes opposite predictions, (iii) adopters charge higher prices and experience lower occupancy than non-adopters in that market, and (iv) across markets, average prices are increasing and average occupancy are decreasing with the degree of 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.

Let h denote the fraction of the market who are adopters. 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 jointly profits, whereas non-adopters sets prices individually to maximize their own profits.

When $h = 1$, the price that maximizes joint profits amounts to monopoly price. In a simple model with homogeneous products, the optimal mark-up equals to 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 mark-up equals to the inverse demand elasticity of the residual demand ϵ_D^A faced by the group of adopters.[34] As the degree of algorithmic penetration h increases, the residual demand faced by adopters becomes more inelastic, allowing them to charge to higher markups and higher prices. Non-adopters can undercut by charging a just-so-slightly lower price and not withhold quality.

Therefore, a model of coordinated prices predicts that (v) adopters charge (weakly) higher prices and experience lower occupancy than non-adopters, and (vi) markets with high algorithmic penetration charge on average higher prices and have lower occupancy.

Implications for Conduct Table 6 summarizes the key predictions for building-level comparisons within the same market and market-level comparisons across different levels of penetration for both responsive and coordinated pricing. The key insight is two-fold:

1. With a negative demand shock, the predictions of a responsive pricing paradigm are the *opposite* of the predictions of a coordinated pricing paradigm for both building-level and market-level

[34] 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 (5.8). It is also worth noting that, in principle, adopters should have some incentives to deviate from the prices recommended by the algorithm to increase its profit. However, individual management companies may not deviate from recommended prices due to several factors: lack of awareness of coordination, or lack of analytical capability to evaluate the benefits from deviation combined with the experience that following the recommendations had been beneficial. Moreover, even if some sophisticated companies deviate, the capacity-constrained nature of the multifamily market limits their impact.

comparisons.

2. With a positive demand shock, the predictions of a responsive pricing paradigm are the *same* as the predictions of a coordinated pricing paradigm for both building-level and market-level comparisons.

Thus, during an economic recession, if one finds adopters charge lower prices and produce higher quantities than non-adopters in the same market (or if prices decrease and quantities increase in algorithmic penetration), it *is* evidence that the algorithm has led to more responsive pricing. However, during an economic boom, if adopters charge higher prices and produce lower quantities than non-adopters in the same market (or if prices increase and quantities decrease in algorithmic penetration), it does not isolate the coordination channel, and therefore is *not* evidence of coordinated pricing.[35]

We proceed with the empirical analysis in three steps. First, we estimate building-level differences between adopters and non-adopters to identify evidence of responsive pricing. Second, we assess the impact of adoption on market-level average prices and quantities. While these findings cannot isolate coordination, they remain empirically relevant for estimating the overall impact of algorithmic pricing. Finally, we estimate a structural model of housing demand from renters and perform a test of conduct to evaluate the "algorithmic coordination" channel.

3.2 Building-level Impact of Algorithmic Pricing

In this section, we examine the responsive pricing hypothesis by comparing adopters' prices and quantity with non-adopters in the same market across varying market conditions.

To start, in Figure 5, we examine the pricing dynamics after software adoption by two specific companies, Essex and Greystar, both involved in lawsuits for alleged price fixing in Seattle. Essex adopted the software in 2008, just before the financial crisis. Panel (a) shows that Essex aggressively dropped prices, retaining much of their occupancy amid the crisis in 2009. In contrast, the rest of Seattle experienced sharp declines in occupancy rates in the same period. Greystar, which adopted the software in 2010, raised its rent more aggressively than the rest of the market, and experienced a decline in occupancy. As such, these charts hint at the likely presence of the responsive pricing channel, evidenced by lowering rents to gain occupancy during the downturn and increasing rents during the upturn.

[35] In addition, it is plausible that there are other sources of friction leading to departures from optimal prices for non-adopters. 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. A model of algorithmic pricing that helps adopters to charge the correct markup compared to before could also produce the same empirical patterns as coordinated pricing. In addition, the current discussion focuses on the predictions at the building-level comparisons and at the market-level aggregates, the stylized models can generate additional predictions regarding other moments of the data. However, when we iterate over various possible predictions on adopters, non-adopters, and their differences by the level of penetration, as long as we allow for some degree of product differentiation where the non-adopters are not fully pricing its optimal markup, it remains difficult to isolate a model of coordinated pricing from a model of sub-optimal pricing on the part of non-adopters.

We then provide some more suggestive evidence from the difference-in-difference plots of two cohorts of adopters. To examine how the pricing impact may vary by market conditions, we examine a cohort of buildings that adopted the software before the financial crisis and another cohort that adopted the software after the crisis. The outcome of interest is y_{jt} include the log of asking rents and the occupancy rate of building j in year t. We regress both outcomes on calendar-year dummies leading up to and after the adoption as follows

$$y_{jt} = \sum_{\substack{\tau=-5 \\ \tau \neq -1}}^{5} \beta_\tau^Y \mathbb{1}\{t = Y + \tau\} a_{j,t} + \beta X_{jt} + \theta_{mqt} + \phi_j + \mu_{jt} \tag{3.1}$$

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), μ_{jt} are the residuals, and β_τ^Y are our coefficients of interest. 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 submarket and building class pairs. The inclusion of such saturated fixed effects θ_{mqt} accounts for differential trends for buildings of different quality tiers in the same submarket. In other words, it allows for the possibility that luxury buildings may be experiencing faster rent growth than non-luxury buildings in the same submarket at the same time.

Figure 6 plots the coefficient of interest β_t^Y for the 2007 and 2013 cohorts, respectively. During the Great Recession, the 2007 adoption cohort aggressively lowered their price in 2009 and gained in occupancy compared to non-adopters. By contrast, the 2013 adoption cohort exhibited significant price growth compared to non-adopters after 2014 and experienced almost 1.5 percentage points lower in occupancy. In both cases, the parallel trends before adoption appear satisfied.

Next, to analyze the full sample period, our main specification estimates the time-varying treatment effects by calendar year from 2006 to 2018. That is, we are interested in measuring the impact of the pricing software in each year τ as follows

$$y_{jt} = \sum_{\tau=2006}^{2018} \beta_\tau \mathbb{1}\{t = \tau\} a_{j,t} + \beta X_{jt} + \theta_{mqt} + \phi_j + \mu_{jt}. \tag{3.2}$$

where $a_{j,t}$ is a binary indicator for the adoption status of building j in year t. As before, in addition building-level covariates X_{jt}, we fully saturate the model with both building-level fixed effects ϕ_j and time-specific market segment fixed effects θ_{mqt}. In addition, given the staggered adoption structure of the data, we also estimate the treatment effects using CSDID (Callaway and Sant'Anna, 2020).

Figure 7 succinctly summarizes the results of the impact of algorithmic pricing at the building level for both log rent and occupancy. During the Great Recession (2008 to 2010), adopters charged lower rents and experienced higher occupancy than non-adopters. After the recession, especially after 2013, adopters charged higher rents and tolerated more vacancies than non-adopters. Estimates using

CSDID are similar to TWFE estimates, with slightly larger magnitudes in both time periods. Our main result is also robust to a variety of alternative specifications, including more granular definitions of building quality where we divide buildings in the same submarket into four quartiles. Our main finding is also robust to restricting the analysis to only ever-adopters, thus comparing the behavior of existing adopters and not-yet adopters in the same market segment at the same time. Given our discussion before, the finding that adopters charged lower prices and experience higher occupancies compared to non-adopters during the recession provides strong evidence in support of the responsive channel of algorithmic pricing.

Given that we have saturated our model with both building and segment-year fixed effects in Eq 3.2, the identifying assumption is that the adoption status $a_{j,t}$ of a building j at a given time t is orthogonal to residual building-level shocks $\mu_{j,t}$. There are several rationale in support of this assumption. First, although we have shown that there is selection into adoption based on building characteristics as shown in Table 7, the inclusion of building-fixed effects account for most of these non time-varying characteristics. Also, while it is plausible that the timing of adoption may reasonably be correlated with its unobserved shock during the year of adoption, it is much less likely that a building's continued adoption status over time remain correlated with the residual shocks in future time periods. More importantly, since a building's adoption status is typically driven by the adoption status of its management company, who tend to manage a portfolio of buildings across various locations, it becomes less likely to be correlated with the building specific shock.

That said, to the extent that there may still be concerns around residual endogeneity after controlling for the exhaustive list controls and fixed effects, 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 in 2022 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.[36] 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, weighed by the importance of that market M' to the company based on its portfolio share,

$$Adopt^{IV}_{c,M,t} = \sum_{M' \neq M} \frac{\sum_{c' \neq c} N^{A}_{c'M't}}{\sum_{c' \neq c} N_{c'M't}} \times \frac{N_{cM't}}{\sum_{M'' \neq M} N_{cM''t}} \tag{3.3}$$

[36] https://www.nmhc.org/research-insight/the-nmhc-50/top-50-lists/2022-top-managers-list/, accessed July 16, 2024.

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 $\sum N_{cM\prime t}$ 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). We find consistent patterns across all of them, 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 (3.2), we add an additional interaction term between the adopters $a_{j,t}$ and the underlying economics 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.

3.3 Market-level Impact of Algorithmic Pricing

In this section, we estimate the impact of algorithmic pricing on average rent and occupancy across markets by the degree of algorithmic penetration. It remains a useful exercise because it provides a sense of the total magnitude by capturing the equilibrium effect of adoption, including non-adopters' endogenous response.

The unit of analysis is a market segment in a given year mqt. We categorize market segments into four bins by time period and ten bins by degrees of penetration to allow non-linearity in the treatment effects. Each time-period bin has three years: 2008-2010, 2011-2013, and 2014-2016. We exclude 2005 to 2007 due to low adoption shares, yielding noisy estimates. We also exclude the post 2017 period, as the market structure changed dramatically with RealPage's acquisition of LRO, which we analyze later separately. Markets are then binned by the share of algorithm adopters in 10 percentage point increments. We find considerable mass at 0% by our market segment definition, and these segments is considered as the baseline group.[37] Our estimating equation becomes

$$y_{mqt} = \sum_{T=1}^{T=3} \sum_{B=1}^{10} \beta_{T,B} \, \mathbb{1}\{(T(t) = T\} \mathbb{1}\{B_t(mq) = B\} + \beta X_{mqt} + \psi_{mq} + \theta_{M,t} + \mu_{mt}$$

where $T(t)$ denotes the year bin that year t belongs to, $B_t(mq)$ denotes the penetration bin a market segment mq belongs to in year t. X_{mqt} includes both average building characteristics of the segment as well as local economic conditions, which include levels and changes in unemployment rates, household

[37] Table 3 shows the variation in penetration by the binned years.

income, housing price, and net migration, which help us to control for local, time-varying demand conditions that may be correlated with algorithmic penetration. Since the unit of analysis is a segment-year pair, we can no longer include the θ_{mqt} fixed effect as the building-level analysis before. However, we still saturate the model with a segment fixed effect ψ_{mq} accounting segment level unobservables, and a metro-year fixed effect $\theta_{M,t}$ account for unobserved shocks in the broader metro at a given time t. The coefficients of interest are $\hat{\beta}_{T,B}$ for each T, B.

Figure 8 displays the coefficients for average rent and occupancy across different penetration bins and time periods. Coefficients from the periods before and during the financial crisis are shown in light blue, while those from the post-crisis period are in light red. Between 2008 and 2010, there is clear evidence that average rents decreased with higher levels of algorithmic penetration, while average occupancy increased. In sharp contrast, during the 2014 to 2016 period, average rents increased with algorithmic penetration—up to nearly 4%—while occupancy declined as penetration increased.

Moreover, we construct an analogous instrument for the penetration in segment mq in year t by aggregating the previously-defined building-level instrument to the market level. Table 10 summarizes the OLS and IV estimates of $\hat{\beta}_{T,Share}$ for each year bin T. The magnitude and signs of the IV estimates are similar to the OLS estimates. During the recession, we see rents decreasing and occupancy increasing with the share of adopters, and during the boom the opposite is true.

Lastly, we analyze the impact of the 2017 merger in which RealPage acquired LRO from the Rainmaker Group. Since RealPage and LRO had different pre-existing market shares across different markets, the resulting change in market structure also varies across different markets. In other words, condition on total algorithmic penetration, the impact of the 2-to-1 merger differs significantly between markets where RealPage was already dominant and markets where RealPage and LRO previously split more evenly. Thus, we consider the following estimating equation

$$\Delta y_{mt} = \beta \Delta \widehat{\text{HHI}_{mt}} + \gamma \, \text{Total Algo Share}_{m,t-1} + \mu_{mt} \tag{3.4}$$

where $\Delta \widehat{\text{HHI}_{mt}}$ denotes the predicted change in the Herfindahl-Hirschman Index in its market segment due to the merger. Table 11 summarizes the results. We do not find any statistically significant impact on either rents or occupancies due to the merger. The results are robust when we include additional controls such as the changes in the local economic condition or metro fixed effects. The results are also robust when we focus on the impact on the adopters alone, rather than market-level averages. These null results could be consistent with a lack of coordinated pricing. However, they could also be consistent with a lack of immediate integration between YieldStar and LRO following the merger. In fact, RealPage disclosed in June 2024 that "LRO is also completely separate from YieldStar and AIRM, and contrary to the allegations, LRO does not have access to the YieldStar/AIRM database, and the YieldStar/AIRM database does not have access to the LRO database,"[38] suggesting that they

[38] https://www.realpagepublicpolicy.com/realpagestatement, accessed August 8, 2024.

may be still far from full integration. As such, we do not use the lack of merger impact as evidence against coordinated pricing.

To summarize this section, both the building-level and market-level results show a clear and consistent picture. The estimates during the Great Recession supports the presence of responsive pricing. The estimates in the post-recession period show that rents increased and occupancy decreased with greater algorithmic penetration. Because such patterns can be consistent with both responsive pricing and coordinated pricing, they do not serve as evidence of algorithmic coordination.

4 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.

4.1 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:

$$u_{ijt} = \alpha_i p_{jt} + \boldsymbol{X_{jt}}\boldsymbol{\beta_i} + \xi_{jt} + \epsilon_{ijt} \tag{4.1}$$

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

$$s_{ijt} = \frac{\alpha_i p_{jt} + \boldsymbol{X_{jt}\beta_i} + \xi_{jt}}{1 + \sum_{j' \in \mathcal{J}_{mt}} \alpha_i p_{j't} + \boldsymbol{X_{j't}\beta_i} + \xi_{j't}}. \tag{4.2}$$

4.2 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 $\xi_{jt} = \xi_j + \tilde{\xi}_{jt}$, where ξ_j is unit type-level fixed effects, capturing the time-invariant vertical quality of a building and its unit types. 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:

$$\hat{G}_1(\theta^D) = \frac{1}{N_{jt}} \sum_{j,t} \hat{\tilde{\xi}}_{jt}(\theta^D) z^D_{jt}. \tag{4.3}$$

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 average local property tax rate in the census tract, excluding the focal building, as an instrument for rent. The exclusion restriction of the instruments hinges on the assumption that property tax rates are set by local authorities *before* the realization of the contemporaneous demand shock, whereas building managers set prices after observing the tax obligations, along with other contemporaneous demand shocks. Moreover, to the extent that the focal building might take actions to lower its tax obligations by making more assessment appeals against

the city, it is less likely that it could lower its applicable rate for a given assessment. Still, to the extent that they could affect tax rates that may be endogenous to its own unobserved demand (e.g., appealing for applicability of certain rules and regulations), we take the more conservative approach of computing the average local property tax rate with the focal building excluded. We believe that once the tax rates are residualized by the product fixed effects, the remaining variation reflects idiosyncratic timings of changes in the local tax regime.

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 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

$$G_2^t(\theta^D) = \underbrace{\sum_{i,j} w_i \mathbb{1}\{y_i = j\} \times \text{hs}_i \times \text{bed}_j}_{\text{observed ACS covariance}} - \underbrace{\sum_{i,j} w_i s_{ijt}(\theta^D) \times \text{hs}_i \times \text{bed}_j}_{\text{model predicted covariance given } \theta^D} \tag{4.4}$$

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:

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

where W is the optimal weighting matrix from two-step GMM.

4.3 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 12 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 7.1% and the 95th percentile of penetration at 67.7%.

Table 13 summarizes our estiamted 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 matched 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 -4.79, indicating that each building faces elastic residual demand. The distribution of own elasticities are shown in Appendix Figure A3. However, when aggregated to the level of all REIS apartment buildings, the elasticity becomes more inelastic, at -1.93, 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 0.78. This suggests that, on average, 78% of households leaving a REIS building opt for a non-REIS building, while the remaining 22% 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.

5 Supply and Testing for Conduct

5.1 Supply Model

The workhorse model for market equilibrium in differential 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 to 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 $\tau^A \notin \{0, 1\}$ denotes 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

$$\Pi_{ft}\left(\boldsymbol{p_{ft}}; \boldsymbol{p_{-ft}}\right) = \sum_{j \in \mathcal{J}_f} \Pi_{jt}\left(p_{jt}; \boldsymbol{p_{-jt}}\right) + \tau^A \sum_{f' \neq f} \kappa_{ff't} \sum_{j \in \mathcal{J}_{f'}} \Pi_{jt}\left(p_{jt}; \boldsymbol{p_{-jt}}\right) \tag{5.1}$$

$\kappa_{ff't}$ is an indicator for whether firm f and f' has adopted the same algorithmic pricing software at

time t. Correspondingly, the first order condition with respect to p_{jt} becomes

$$\frac{\partial \Pi_{ft}}{\partial p_{jt}} = D_{jt} + \left(\sum_{k \in \mathcal{J}_f} (p_{kt} - mc_{kt}) \frac{\partial D_{kt}}{\partial p_{jt}} \right) + \tau^A \sum_{f' \neq f} \kappa_{ff't} \left(\sum_{k \in \mathcal{J}_{f'}} (p_{kt} - mc_{kt}) \frac{\partial D_{kt}}{\partial p_{jt}} \right) = 0 \tag{5.2}$$

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

$$\frac{\partial \Pi_{lt}}{\partial p_{lt}} = \tau_t^{NA} D_{jt} + \left(\sum_{k \in \mathcal{J}_l} (p_{kt} - mc_{kt}) \frac{\partial D_{kt}}{\partial p_{lt}} \right) = 0 \tag{5.3}$$

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

$$\boldsymbol{p_t} - \boldsymbol{mc_t} = \underbrace{-\left(\mathcal{H}_t(\tau^A, \tau_t^{NA}; \boldsymbol{D}(\boldsymbol{p})) \odot \boldsymbol{\Omega}_t(\boldsymbol{p}) \right)^{-1} \boldsymbol{D}_t(\boldsymbol{p})}_{\boldsymbol{\eta}_t(\mathcal{H}_t(\tau^A, \tau^{NA}))} \tag{5.4}$$

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:

$$\mathcal{H}_t(\tau^A, \tau^{NA}) = \begin{bmatrix} \mathcal{H}_t^A(\tau^A) & 0 \\ 0 & \mathcal{H}_t^A(\tau_t^{NA}) \end{bmatrix} \tag{5.5}$$

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

$$\mathcal{H}^{A}_{jkt}(\tau^{A}) = \begin{cases} 1 & \text{if } j,k \text{ are owned by the same owner,} \\ \tau^{A} & \text{if } \kappa_{jkt} = 1 \text{ are using the same algorithmic pricing software,} \\ 0 & \text{otherwise.} \end{cases} \tag{5.6}$$

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

$$\mathcal{H}^{NA}_{jkt}(\tau^{NA}_{t}) = \begin{cases} \frac{1}{\tau^{NA}_{t}} & \text{if } j,k \text{ are owned by the same owner,} \\ 0 & \text{otherwise.} \end{cases} \tag{5.7}$$

Across adopters $j \in A_t$ and non-adopters $k \notin A_t$, $\mathcal{H}_{jkt} = 0$. The block-diagonal shape of $\mathcal{H}_t(\tau^{A}, \tau^{NA}_{t})$ 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 (5.4) separately for adopters and non-adopters, stacked as follows

$$\begin{bmatrix} \boldsymbol{p}^{A}_{t} - \boldsymbol{mc}^{A}_{t} \\ \boldsymbol{p}^{NA}_{t} - \boldsymbol{mc}^{NA}_{t} \end{bmatrix} = \begin{bmatrix} -\left(\mathcal{H}_t(\tau^{A}) \odot \boldsymbol{\Omega}^{A}_{t}(\boldsymbol{p})\right)^{-1} \boldsymbol{D}^{A}_{t}(\boldsymbol{p}) \\ -\left(\mathcal{H}_t(\tau^{NA}_{t}) \odot \boldsymbol{\Omega}^{NA}_{t}(\boldsymbol{p})\right)^{-1} \boldsymbol{D}^{NA}_{t}(\boldsymbol{p}) \end{bmatrix} \tag{5.8}$$

where $\boldsymbol{D}^{A}$ and $\boldsymbol{D}^{NA}$ denotes the demand for adopters and non-adopters respectively, and $\boldsymbol{\Omega}^{A}_{t}$ and $\boldsymbol{\Omega}^{NA}_{t}$ represents 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.[39] As such, by estimating the entire surface of demand derivatives, the structural model provides us with a direct approach to evaluating adopter conduct.

5.2 Conduct Test

Given the first order condition given in Eq (5.4), because the demand system $\boldsymbol{D}_t(\boldsymbol{p})$ is already estimated, its derivatives can 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-

[39] To be precise, an additional requirement for this to be true is that the marginal cost functions for adopters, mc^{A}_{t}, and non-adopters, mc^{NA}_{t}, are allowed to differ, so that the left-hand-side of Eq (5.8) can also be stacked. This is fairly reasonable considering that the marginal cost of a building may change after adoption.

order condition in Eq (5.8) allows us to back out the marginal cost of adopters as follows

$$\boldsymbol{mc_t}^A = \boldsymbol{p_t}^A - \boldsymbol{\eta}_t^A(\mathcal{H}_t(\tau^A)) \tag{5.9}$$

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

$$\mathbb{E}[\omega_{jt}|\boldsymbol{z}_t^S] = 0. \tag{5.10}$$

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

$$mc_{jt}^A(x_{jt}, q_{jt}) = h(x_{jt}) + \omega_{jt}. \tag{5.11}$$

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

$$mc_{jt}^A(x_{jt}, occ_{jt}) = h(x_{jt}, occ_{jt}) + \omega_{jt} = \frac{\kappa_a}{(occ_{jt} - 1 - \delta_\epsilon)^2} + \delta_j + \omega_{jt}. \tag{5.12}$$

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%.[40]

For the conditional moment condition in Eq (5.10), 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

$$\mathbb{E}[(\omega_{j,t}^{M1})' A(\boldsymbol{z}_t^S)] - \mathbb{E}[(\omega_{j,t}^{M2})' A(\boldsymbol{z}_t^S)] \tag{5.13}$$

[40] 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 $1/(occ_{jt} - 1 - \delta_\epsilon)^2$.

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

$$A(\boldsymbol{z}^S) = \mathbb{E}[\Delta\eta_{jt}^{1,2}|\boldsymbol{z}^S], \quad \Delta\eta_{jt}^{1,2} = \eta_{jt}(\mathcal{H}^{M_1}) - \eta_{jt}(\mathcal{H}^{M_2}) \tag{5.14}$$

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

5.3 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 14 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.[41] 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 (2017-2018), 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 previous results from the merger analysis, where it seems that the acquisition of the legal entities did not imply that 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, mismeasurment 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.[42] Now, with this more conservative test, Appendix Table A5 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

[41] 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.

[42] 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.

within-software coordination against a model of across-software coordination. Table 15 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 is 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."

5.4 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.

Table 16 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 2.2.2), 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.[43] 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.

[43] In other words, although the conduct parameter τ^{NA}_t among adopters is structural in nature, the sophistication parameter τ^{NA}_t 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^{NA}_t arises from economic fundamentals, we do not know the values of the sophistication parameter τ^{NA}_t in such counterfactuals.

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 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.

5.5 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. Another possibility is that price experimentation at the cluster level, designed to avoid treatment-control in-

terference (Holtz, Lobel, Lobel, Liskovich, and Aral, 2024), could produce an estimated elasticity that reflects a more aggregate, inelastic demand.

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 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.[44] 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.

[44]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 (5.8). 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.

6 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 estimate the market-level treatment effect of algorithmic pricing penetration to assess the aggregate impact. Again, during periods of economic recovery, higher levels of penetration across markets led to significantly higher average rents and lower occupancy. While both building-level and market-level results indicate that the software helps adopters set more responsive prices, these reduced-form results cannot conclusively test for conduct, as a model of coordination produces directionally the same results as a model of responsive pricing during an economic boom.

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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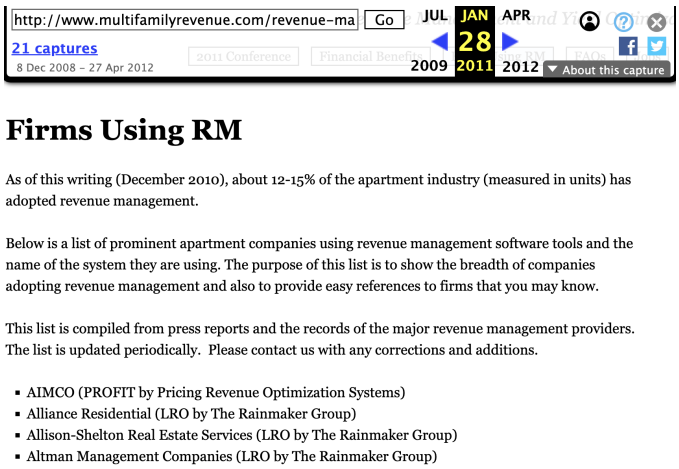
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7 Figures

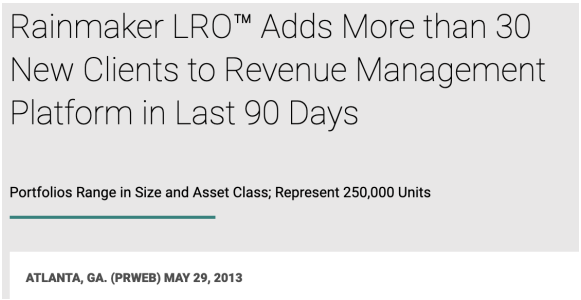
Figure 1: Snapshot of Surveyed List of Adopters



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) 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

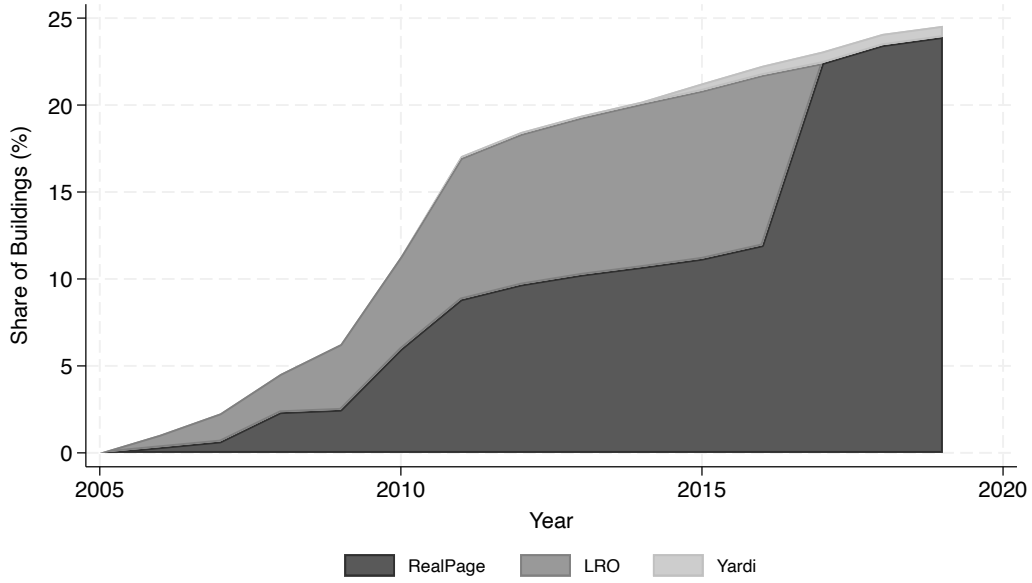
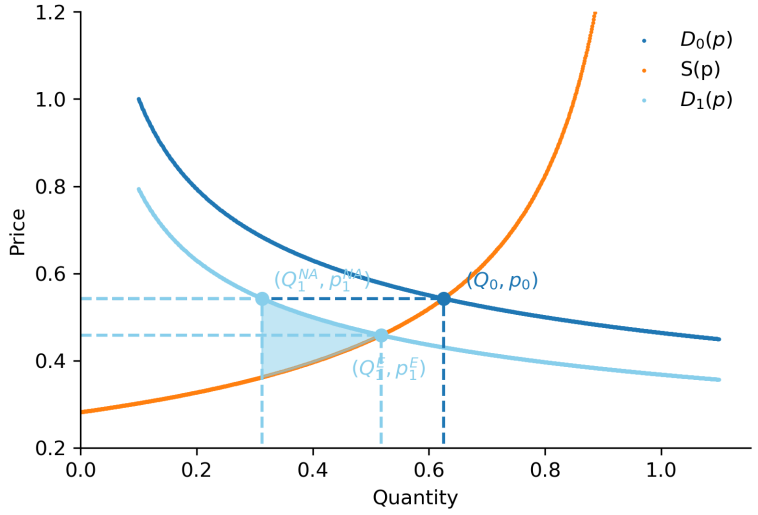
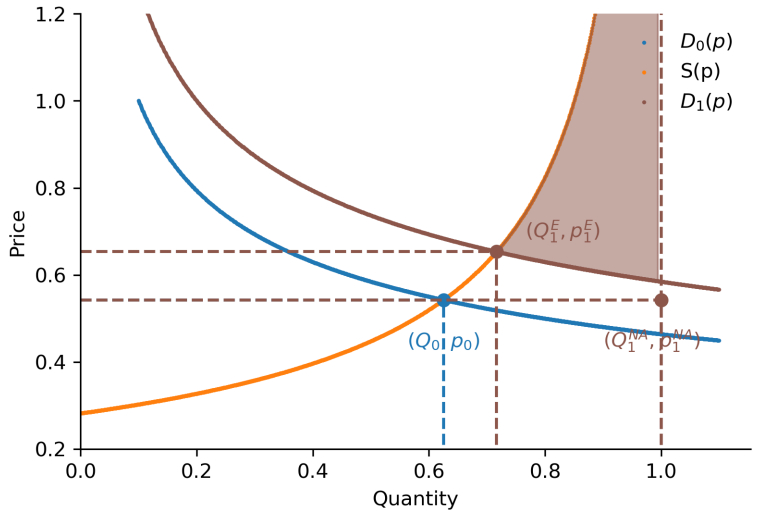


Figure 4: An Illustrative Model of Responsive Pricing

(a) Negative Demand Shock

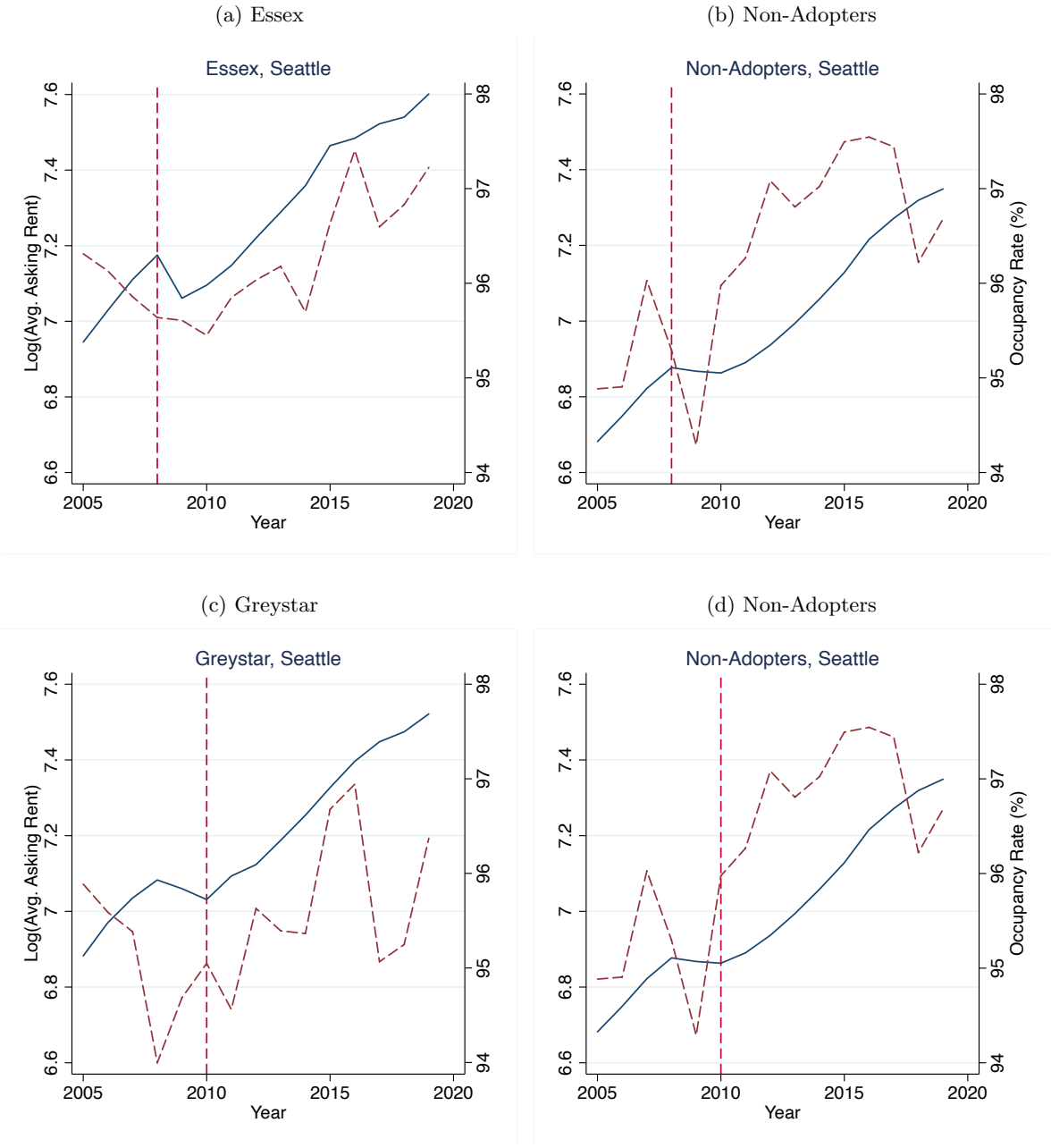


(b) Positive Demand Shock



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 are indicated by the brown shaded region.

Figure 5: Case Study: Pricing and Occupancy of Select Adopted Companies

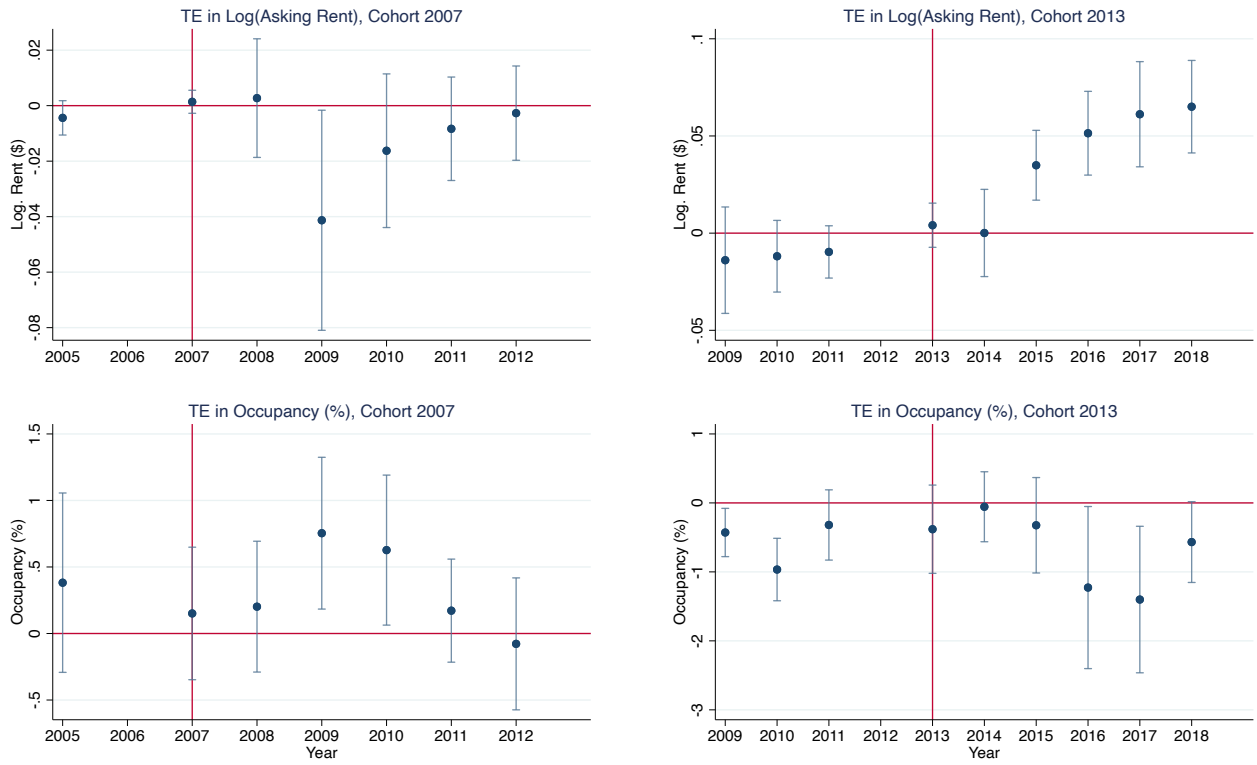


Notes: The analysis is restricted to Seattle metro-area apartments. The solid navy line represents Log(Rent), while the dashed red line represents the occupancy rate. The vertical dashed line marks the year of adoption for each management company.

Figure 6: Building-Level Treatment Effects of Algorithmic Pricing by Adoption Cohorts

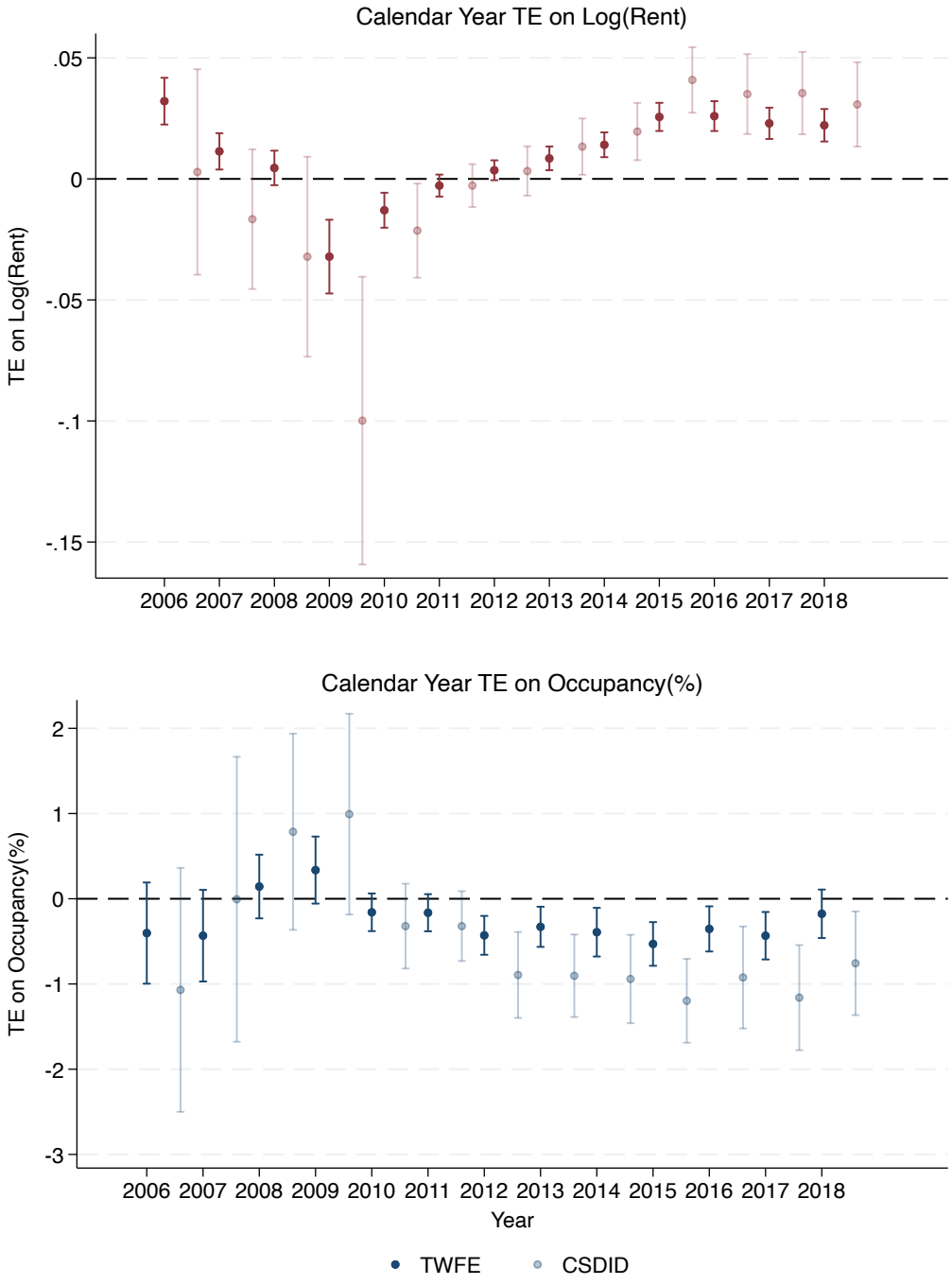
(a) 2007 Adopters

(b) 2013 Adopters



Notes: 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 submarket and building class pairs. Standard errors are clustered at the management company level.

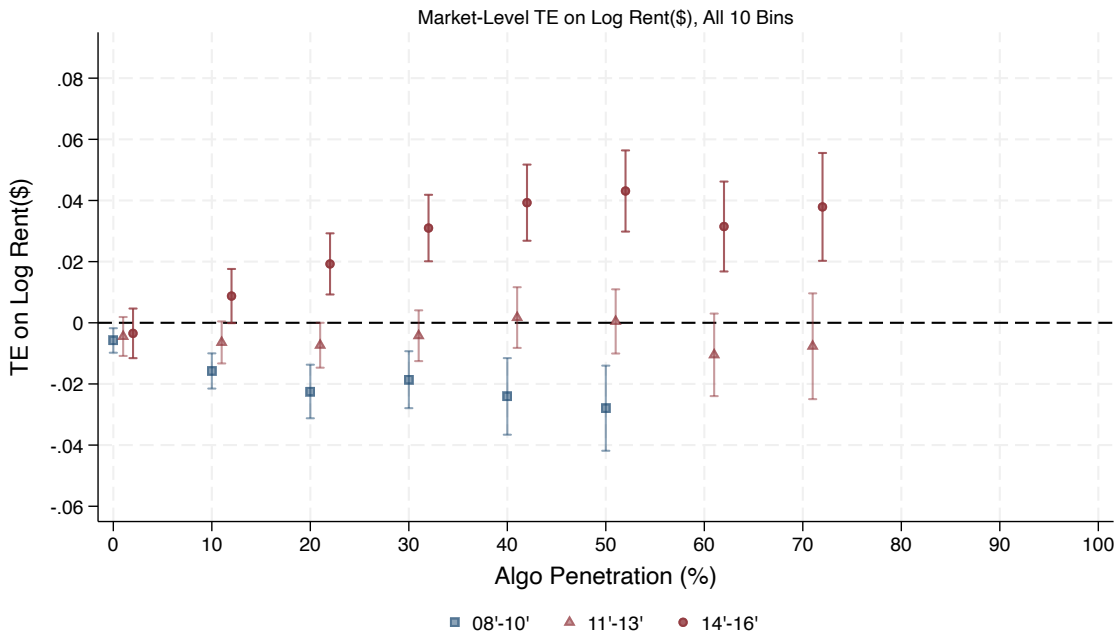
Figure 7: Building-Level Treatment Effects of Algorithmic Pricing by Calendar Year



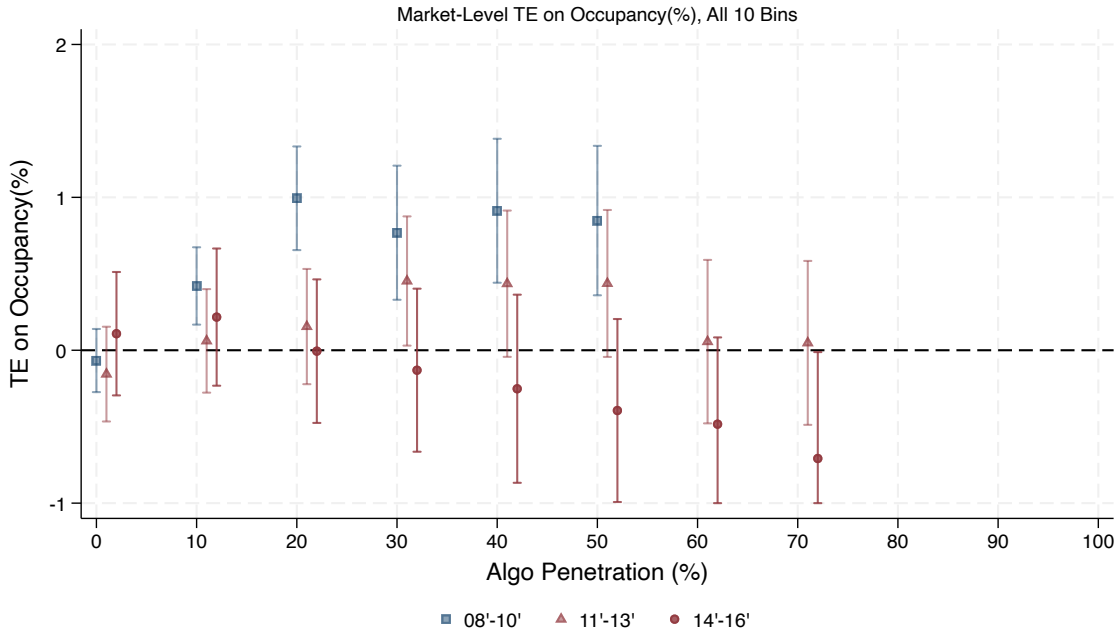
Notes: The sample is restricted to buildings constructed before 2005. The model includes building-level fixed effects and segment-year fixed effects, with market segments defined by submarket and building class pairs. For CSDID, building-level characteristic-specific time trends are also controlled through the doubly robust estimator in addition to the fixed effects. Standard errors are clustered at the management company level for both specifications.

Figure 8: Market-Level Treatment Effects by Degree of Algorithmic Pricing Penetration

(a) $Y = \log(Rent)$



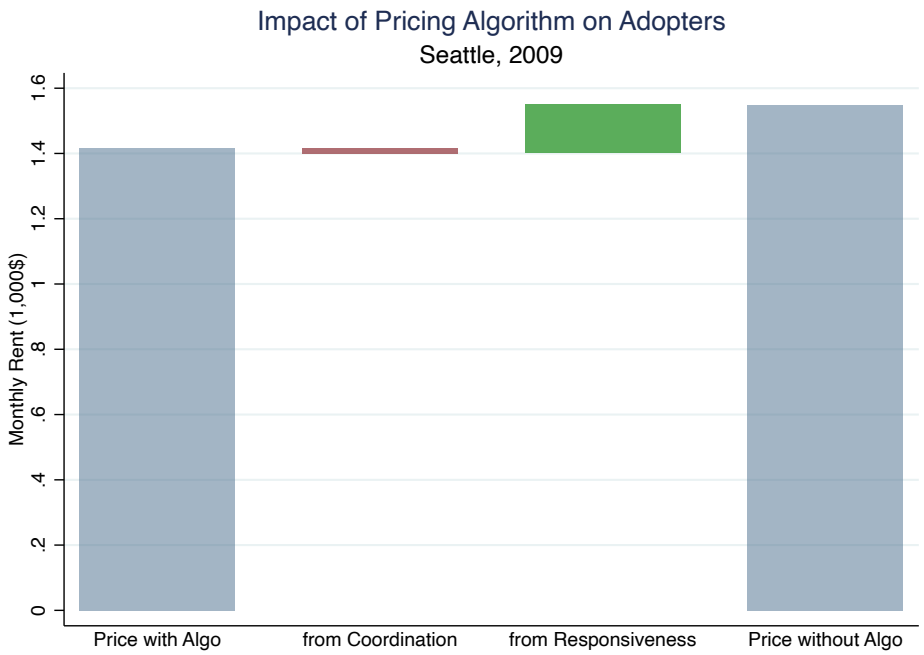
(b) $Y = Occupancy(\%)$



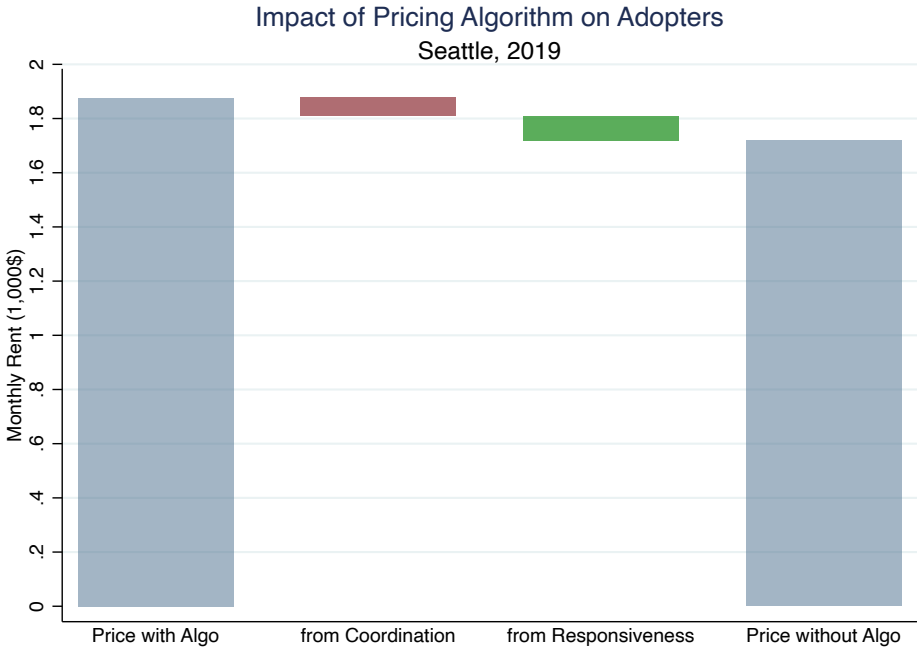
Notes: The sample is restricted to buildings constructed before 2005. The model includes segment fixed effects and metro-year fixed effects, with market segments defined by submarket and building class pairs. Standard errors are clustered at the segment level.

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

(a) Seattle 2009



(b) Seattle 2019



8 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	1217	0	0	0	0	0	0	0	0	0	0	1217
2006	1052	111	34	19	0	1	2	0	0	0	0	1219
2007	923	165	76	31	18	2	5	0	0	0	0	1220
2008	756	216	122	56	36	17	14	5	1	0	0	1223
2009	627	259	149	80	55	26	21	6	3	0	2	1228
2010	405	267	217	125	71	53	51	24	6	4	6	1229
2011	267	230	218	167	109	59	89	45	21	6	18	1229
2012	239	231	218	163	113	71	96	46	27	9	17	1230
2013	226	222	220	173	117	74	91	56	24	11	18	1232
2014	222	217	212	182	119	78	97	53	24	13	15	1232
2015	216	213	214	168	121	90	108	55	20	14	15	1234
2016	197	211	214	156	136	90	116	67	22	13	14	1236
2017	186	210	214	161	137	96	118	70	22	11	12	1237
2018	178	199	220	167	127	107	127	62	28	10	14	1239
2019	171	192	227	173	135	116	117	62	24	8	14	1239

Notes: Each segment is defined by a submarket and building class (Class A or Class B/C).

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: Building-Level 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: Market-Level Comparative Statics with Penetration h			
	Responsive Pricing (Bust Period)	Responsive Pricing (Boom Period)	Coordinated Pricing (All Periods)
p	$\searrow$	$\nearrow$	$\nearrow$
Q	$\nearrow$	$\searrow$	$\searrow$

Notes: The table above summarizes the model predictions for each of the pricing paradigms based on the stylized models. The top panel summarizes the building-level differences in price and quantity between adopters and non-adopters in the same market. Note that $+^*$ denotes weakly positive. The bottom panel summarizes the market-level comparative statics in terms of how average price and total quantity changes as the adoption penetration h increases. Notably, there are no predictions for a model coordinated of pricing that cannot be generated from a model of responsive pricing.

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

	Non-Adopters	Adopters
Log(Avg. Asking Rent($))	7.03	7.27
	(0.47)	(0.47)
Occupancy Rate(%)	93.64	91.63
	(7.37)	(8.84)
Free Rent(Month)	0.03	0.04
	(0.02)	(0.02)
Num. Floors	3.88	4.97
	(4.38)	(6.11)
Year Built	1979.67	1995.05
	(23.93)	(19.47)
Frac. Pool	0.64	0.83
	(0.48)	(0.38)
Frac. Doorman	0.03	0.05
	(0.18)	(0.21)
Frac. Tennis Court	0.00	0.01
	(0.07)	(0.08)
Frac. Parking Garage	0.04	0.09
	(0.21)	(0.29)
Frac. Clubhouse	0.35	0.65
	(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%

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	0.032***	-0.387	-0.253	6.356	0.003	-1.069
	(0.005)	(0.302)	(0.304)	(11.649)	(0.022)	(0.730)
2007	0.011***	-0.418	0.050	-11.444	-0.017	-0.006
	(0.004)	(0.274)	(0.059)	(8.168)	(0.015)	(0.853)
2008	0.004	0.158	0.004	-2.542**	-0.032	0.787
	(0.004)	(0.189)	(0.016)	(1.294)	(0.021)	(0.587)
2009	-0.032***	0.352*	-0.115***	-0.516	-0.100***	0.993*
	(0.008)	(0.200)	(0.027)	(1.094)	(0.030)	(0.601)
2010	-0.013***	-0.144	-0.046***	-0.538	-0.021**	-0.321
	(0.004)	(0.112)	(0.012)	(0.493)	(0.010)	(0.254)
2011	-0.003	-0.148	-0.021***	-0.892**	-0.003	-0.321
	(0.002)	(0.111)	(0.007)	(0.367)	(0.005)	(0.209)
2012	0.003	-0.409***	-0.003	-1.101***	0.003	-0.894***
	(0.002)	(0.117)	(0.006)	(0.377)	(0.005)	(0.257)
2013	0.008***	-0.311***	0.004	-1.266***	0.013**	-0.904***
	(0.003)	(0.121)	(0.006)	(0.368)	(0.006)	(0.247)
2014	0.013***	-0.369**	0.028***	-1.231***	0.020***	-0.942***
	(0.003)	(0.146)	(0.006)	(0.360)	(0.006)	(0.265)
2015	0.025***	-0.509***	0.054***	-1.519***	0.041***	-1.197***
	(0.003)	(0.131)	(0.008)	(0.335)	(0.007)	(0.251)
2016	0.026***	-0.313**	0.057***	-0.987***	0.035***	-0.923***
	(0.003)	(0.134)	(0.008)	(0.335)	(0.008)	(0.305)
2017	0.022***	-0.389***	0.058***	-0.836**	0.035***	-1.160***
	(0.003)	(0.140)	(0.009)	(0.351)	(0.009)	(0.315)
2018	0.021***	-0.134	0.062***	-0.387	0.031***	-0.757**
	(0.003)	(0.144)	(0.010)	(0.357)	(0.009)	(0.310)
Building FE	Y	Y	Y	Y	Y	Y
Segment-Year FE	Y	Y	Y	Y	Y	Y
F-Stat			50.9	50.9		
N_{obs}	413,850	413,850	413,850	413,850	413,850	413,850

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 submarket and building class 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 Macro Economic Conditions

$\log(Rent)$	(1)	(2)	(3)
ΔUnemployment Rate$_{M,t} \times$ Adopter$_{j,t}$	-0.0306**		
	(0.00935)		
ΔHousehold Income$_{M,t} \times$ Adopter$_{j,t}$		0.0544**	
		(0.0192)	
ΔHome Price Index$_{M,t} \times$ Adopter$_{j,t}$			0.0481**
			(0.0167)
Time Varying Treatment Effects	Y	Y	Y
Building FE	Y	Y	Y
Segment-Year FE	Y	Y	Y
N_{obs}	385,341	382,591	373,939

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 submarket and building class pairs. Standard errors are clustered at the management company level in all specifications.

Table 10: Market-level Impact of Algorithm Penetration on Rent and Occupancy

	(1) Log(Rent) OLS	(2) Log(Rent) IV	(3) Occupancy (%) OLS	(4) Occupancy (%) IV
06-07 × AlgoShare	0.091***	-0.007	0.063	-0.212
	(0.033)	(0.029)	(1.210)	(1.184)
08-09 × AlgoShare	-0.036***	-0.090***	1.856***	1.432
	(0.013)	(0.024)	(0.562)	(1.161)
10-11 × AlgoShare	-0.010	-0.054***	3.052***	3.845***
	(0.010)	(0.018)	(0.338)	(0.830)
12-13 × AlgoShare	0.032***	0.007	0.507*	0.628
	(0.009)	(0.016)	(0.280)	(0.700)
14-15 × AlgoShare	0.052***	0.029**	-1.469***	-1.797***
	(0.010)	(0.015)	(0.360)	(0.695)
16-17 × AlgoShare	0.011	-0.018	-2.568***	-2.919***
	(0.011)	(0.015)	(0.439)	(0.650)
Metro-Year FE	Y	Y	Y	Y
Segment FE	Y	Y	Y	Y
N_{mqt}	17441	17441	17441	17441

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ The sample is restricted to buildings constructed before 2005. The model includes segment fixed effects and metro-year fixed effects. All regressions are weighed by number of buildings in each market. Standard errors are clustered at the market segment level for all specifications.

Table 11: 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.

Table 12: Sample Summary Statistics for Demand Estimation

Panel A: Price and Quantity	
Rent ($1000)	1.429
	(0.630)
Occupancy (%)	95.4
	(8.1)
Panel B: ACS Moments	
$Cov(\text{income}_i, \text{rent}_j)$	0.240
$Cov(\text{hh size}_i, \text{bed}_j)$	0.430
$Cov(\text{has child}_i, \text{bed}_j)$	0.360
$Cov(\text{income}_i, \text{building age} < 10_j)$	0.110
$Cov(\text{hh age} > 35_i, \text{building age} < 10_j)$	-0.069
$\mathbb{E}[\text{income}_i \vert \text{Inside Good}]/\mathbb{E}[\text{income}_i]$	1.30
$\mathbb{E}[\text{hh size}_i \vert \text{Inside Good}]/\mathbb{E}[\text{hhsize}_i]$	0.73
$\mathbb{E}[\text{has child}_i \vert \text{Inside Good}]/\mathbb{E}[\text{income}_i]$	0.46
$\mathbb{E}[\text{hh age} > 35_i \vert \text{Inside Good}]/\mathbb{E}[\text{income}_i]$	0.87
$\mathbb{E}[\text{income}_i \vert \text{class a}]/\mathbb{E}[\text{income}_i]$	1.30
Panel C: Geographical Markets	
N_{metro}	10
N_{submkt}	203
Panel D: Fraction of Units Adopted (as of 2019)	
Mean	32.4%
P5	7.1%
P25	17.7%
P75	44.1%
P95	67.7%

Notes: Estimation sample contains a set of ten cities from 2009 to 2019, including Atlanta, Charlotte, Dallas, DC, Houston, Los Angeles, Minneapolis, Portland, San Diego, and Seattle.

Table 13: Estimated Demand Parameters

Panel A: Price Coefficients	
α_0 :	-12.0
	(0.35)
α_y : ($\times$ log(Income))	1.0
	(0.0037)
Panel B: Other Heterogenous Coefficients	
Bed x HH Size	0.87
	(0.012)
Bed x Has Child	0.27
	(0.0082)
New Bld x Income	0.13
	(0.015)
New Bld x Age $>$ 35	-0.29
	(0.010)
Class A x Income	0.27
	(0.0092)
Inside Good x Income	0.75
	(0.0074)
Inside Good x HH Size	-0.84
	(0.0099)
Inside Good x Has Child	-0.0056
	(0.0073)
Inside Good x Age $>$ 35	-0.29
	(0.0047)
N_{mt}	2019
N_{bld}	11,573
N_j	29,689
N_{jt}	215,517
Unit-type FE	Y
Panel C: Estimated Elasticities	
Mean Own Elasticity	-4.94
Median Own Elasticity	-4.79
Median Aggregate Elasticity	-1.93
Median Outside Good Diversion	0.78

Notes: Estimation sample contains a set of ten cities from 2009 to 2019, including Atlanta, Charlotte, Dallas, DC, Houston, Los Angeles, Minneapolis, Portland, San Diego, and Seattle.

Table 14: 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 15: 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* uses 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 16: Estimated Markup Differences Due to Coordination

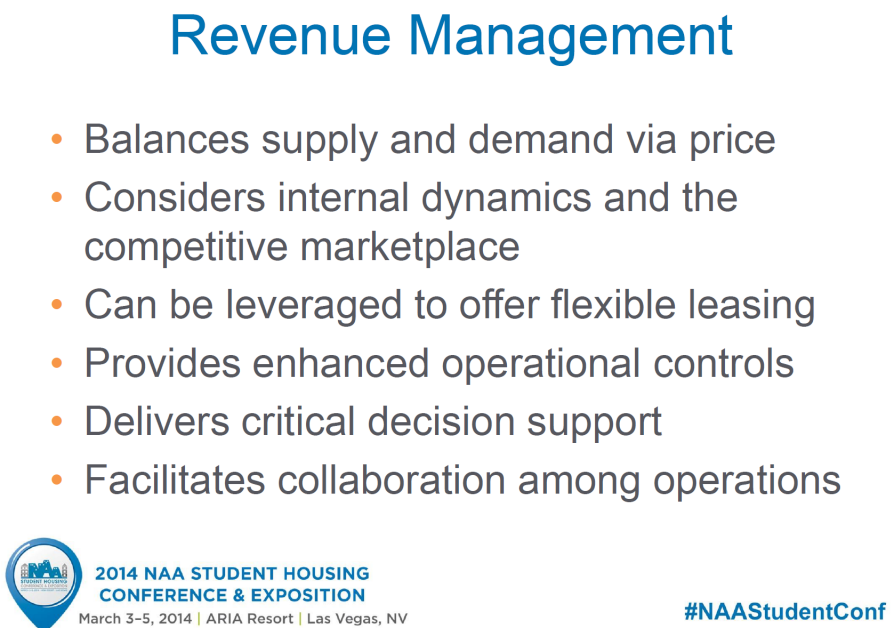
Year	Algo Adoption Fraction (% Bld)	Prices ($/mo)	Markup Difference ($/mo)	p(25)	p(50)	p(75)
2009	6.5	1724.4	13.6	2.5	7.6	17.6
2010	11.8	1517.5	11.3	1.5	5.5	15.7
2011	20.8	1314.3	12.3	3.0	9.1	15.9
2012	19.3	1527.3	11.9	2.0	7.4	15.7
2013	20.5	1531.1	22.6	6.4	16.1	32.3
2014	21.5	1595.0	22.7	7.3	17.3	34.1
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 compeition (i.e., own profit maximization) among the RealPage and LRO adopters from 2009 and 2019 in our sample of cities. The last two rows (2018* and 2019*) compute the hypothetical markup difference if the merged RealPage led to a full coordination among its adopters.

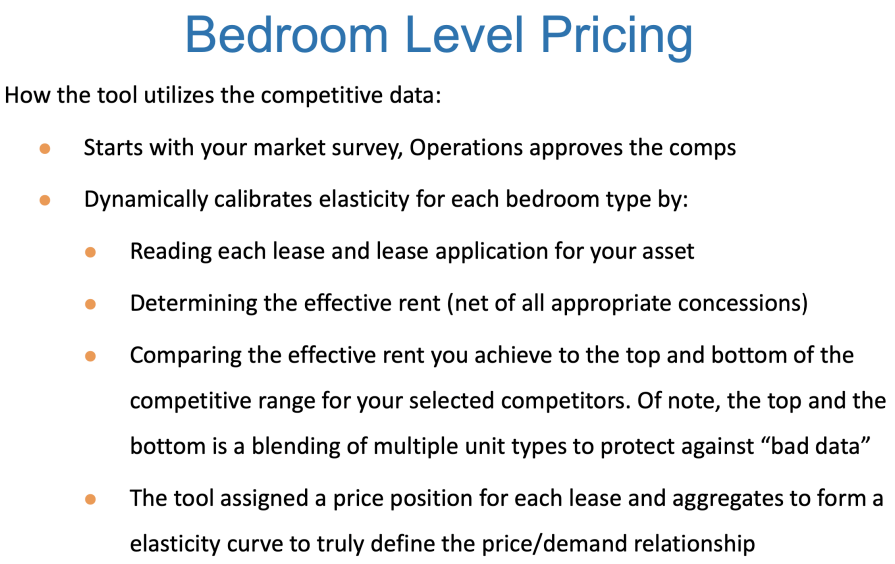
A Appendix Figures

Appendix Figure A1: YieldStar Presentation

(a) Overview of Revenue Management



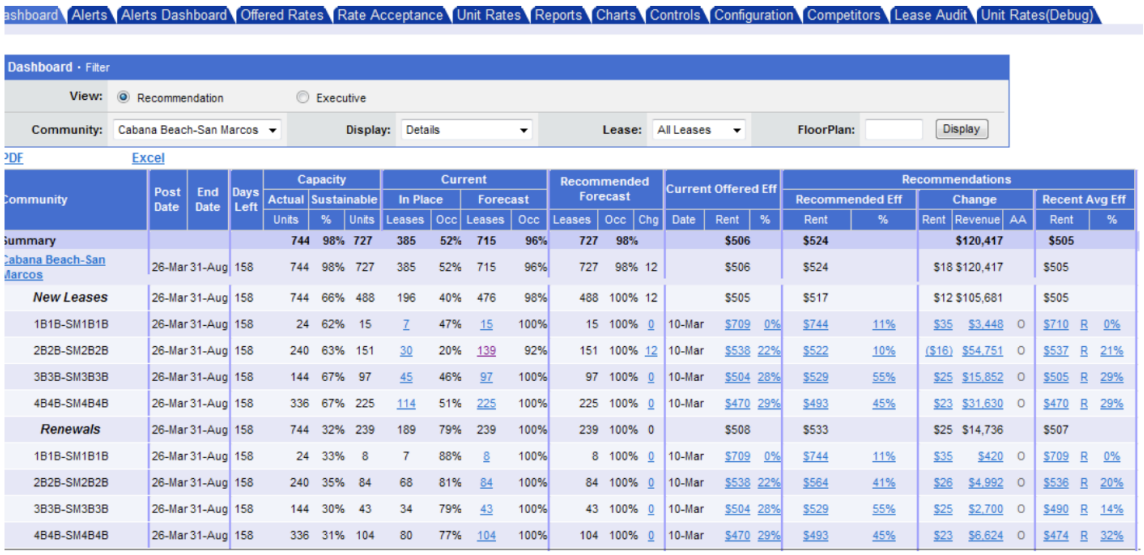
(b) Details



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.

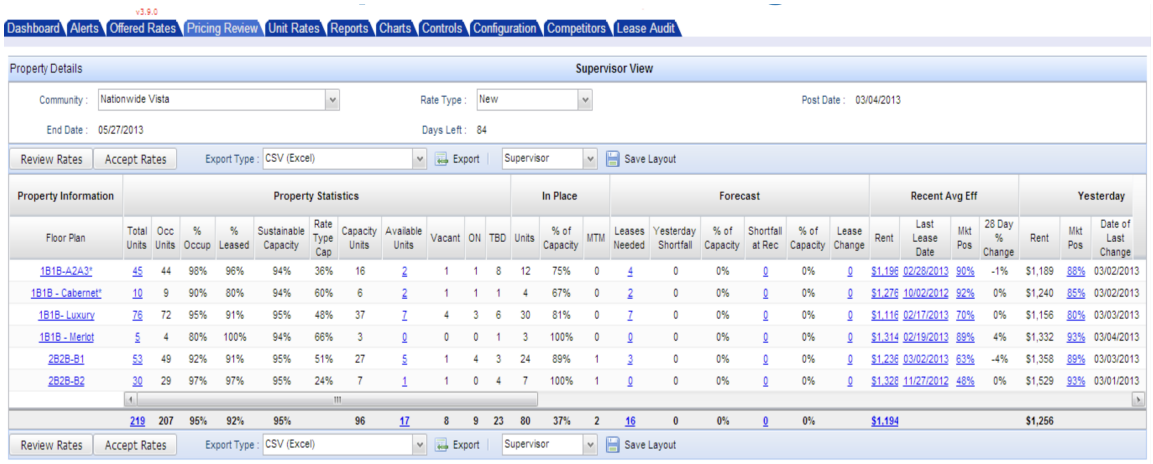
Appendix Figure A2: Manager's View of Yieldstar Pricing Dashboard

(a) Price recommendation made by Yieldstar



Community	Post Date	End Date	Days Left	Capacity Actual Units	Capacity Actual %	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

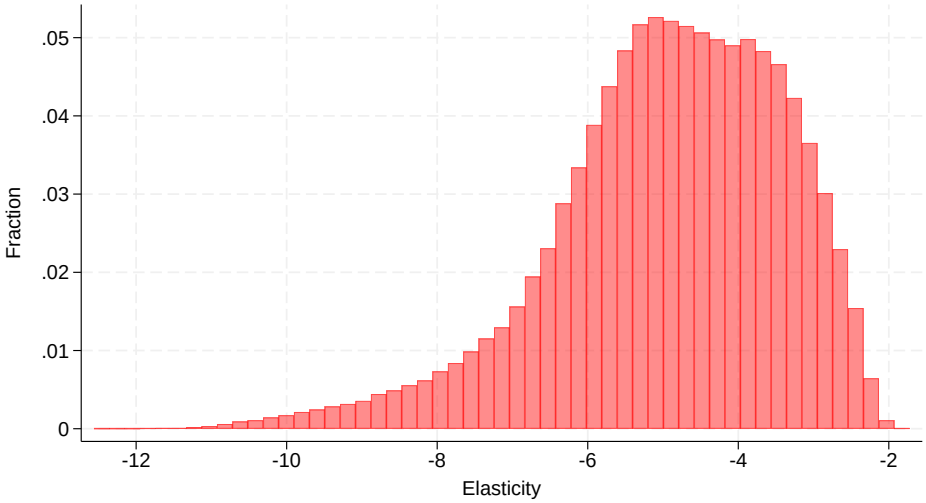


Property Information	Property Statistics											In Place			Forecast					Recent Avg Eff				Yesterday		
Floor Plan	Total Units	Occ Units	% Occup	% Leased	Sustainable Capacity	Rate Type Cap	Capacity Units	Available Units	Vacant	ON	TBD	Units	% of Capacity	MTM	Leases Needed	Yesterday Shortfall	% of Capacity	Shortfall at Rec	% of Capacity	Lease Change	Rent	Last Lease Date	Mkt Pos	28 Day % Change	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	$1,326	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		

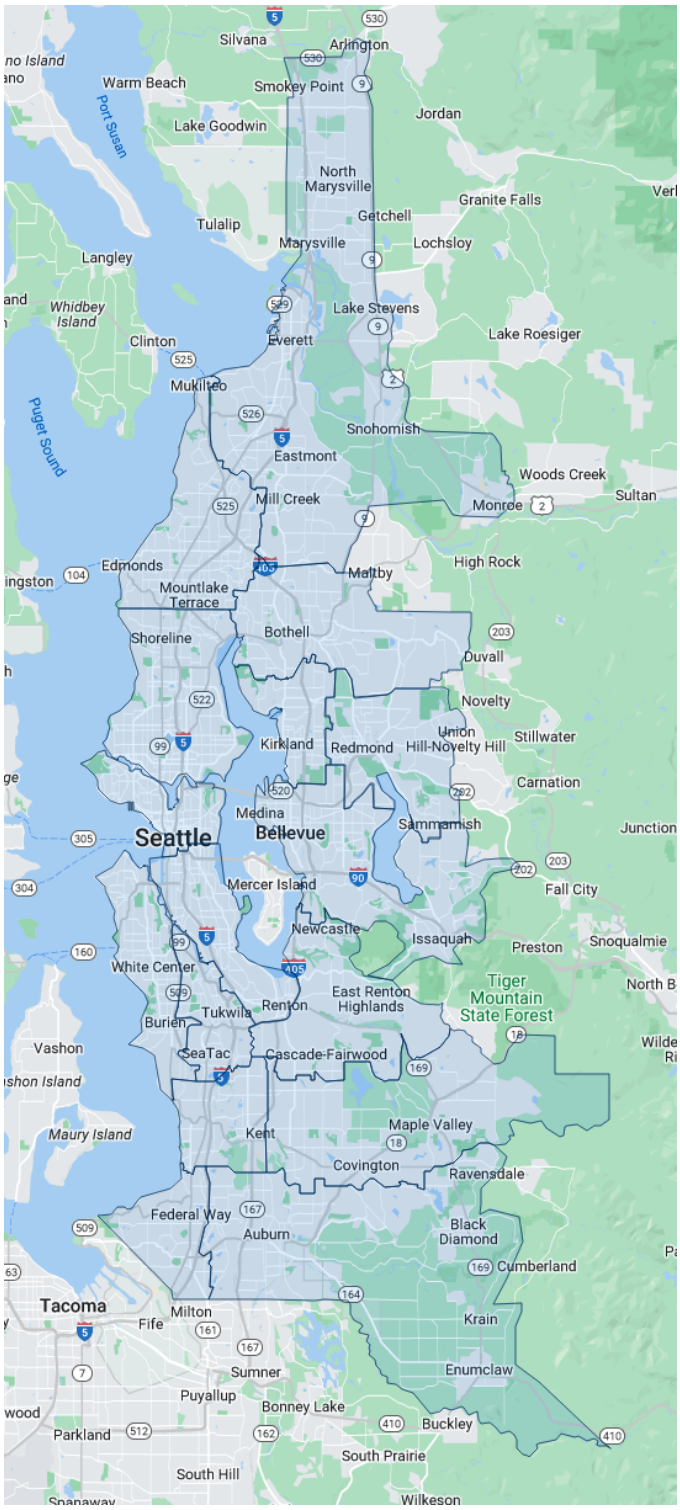
Review Rates Accept Rates Export Type: CSV (Excel) Export Supervisor Save Layout

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: Distribution of Estimated Own Elasticities



Appendix Figure A4: REIS Defined Submarket, Seattle Metro



Source: REIS Web Portal

B Appendix Tables

Appendix Table A1: Top Multifamily Management Companies

Company	Units Managed	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 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 December 1, 2022.

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: 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,[45] 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 $p_1^{NA} = p_0$.[46] 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

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

[46] 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 good model because each supplier is capacity-constrained.

As the fraction of adopters h increases, the price of the adopters approaches the full competitive equilibrium.[47] 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 model predicts that adopters charge lower prices and produce higher quantities than non-adopters within a market. Consequently, across markets with a negative demand shock, average market prices decrease with the share of adopters h, and total quantity increases with h.

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 its 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 of the adopters approaches the 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. Consequently, across markets with a positive demand shock, average market prices increase with the share of adopters h and total quantity decreases with h.

[47] In fact, 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.

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 the 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

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

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

Taking the derivative with respect to price yields the following the 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,[48] 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, average market price increases with the share of adopters h and total quantity decreases with h.

[48] 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

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