Coordinated vs Efficient Prices: The Impact of Algorithmic Pricing on Multifamily Rental Markets

Sophie Calder-Wang* Gi Heung Kim†

Preliminary and Comments Welcome

February 26, 2024‡

Abstract

Algorithmic pricing can improve efficiency by helping firms set prices more responsive to changing market conditions. However, widespread adoption of the same algorithm could also lead to price coordination, resulting in elevated prices. In this paper, we examine the impact of algorithmic pricing on the U.S. multifamily rental housing market using hand-collected adoption decisions of property management companies merged with the data of market-rate multifamily apartments from 2005 to 2019. First, our findings suggest that algorithm adoption indeed helps building managers set more responsive prices: buildings with the software increase prices during booms and lower prices during busts, compared to non-adopters in the same market. Second, when compared across markets, we find markets with greater algorithm penetration also experienced higher rents and lower occupancy in the post-crisis period. Such empirical patterns are consistent with either price coordination through the algorithm or widespread pricing error among non-adopters. Lastly, we estimate a structural model of housing demand and perform a test of conduct to evaluate the "algorithmic coordination" hypothesis.

Keywords: pricing algorithms, real estate, competition policy

JEL Code: R21, R31, L12, L43, D83

*The Wharton School, University of Pennsylvania *sophiecw@wharton.upenn.edu*

†The Wharton School, University of Pennsylvania *robinkim@wharton.upenn.edu*

‡This paper benefited from conversations with seminar and conference participants at NBER Digital Workshop, NBER Summer Institute, IIOC, the Federal Trade Commission, Duke University, NYU IO Day, CUHK, and the Department of Housing and Urban Development. This research is supported by the Zell Lurie Real Estate Center at the Wharton School. All errors are our own.

1 Introduction

The introduction of computing technology has brought about significant changes in how firms set prices, especially towards the use of software that set prices automatically and algorithmically. These types of software often leverage high-frequency data collected across firms operating in the same industry or in the same market as well as consumer preferences to suggest optimal prices for firms. Moreover, these programs are sometimes powered by artificial intelligence (AI), raising concerns that these pricing agents might just learn to jointly play a collusive strategy rather than price competitively. Consequently, the issue of algorithmic collusion has gained significant attention from researchers (Calvano et al., 2020; Asker et al., 2022), policymakers (OECD, 2017), and antitrust agencies (Fortin, 2020; Mcsweeny and O'Dea, 2017).

The multifamily housing industry in the United States is currently facing intense scrutiny regarding the issue of algorithmic pricing. Since 2022, a total of seven class action lawsuits have been filed against RealPage, the software company, as well as landlords utilizing the software, alleging that the pricing software is responsible for raising prices "above competitive levels" (Yusupov v. RealPage, Inc et al, 2023; Navarro v. RealPage, Inc. et al, 2022; Bason v. RealPage, Inc., 2022). This issue has garnered significant media attention[1] and has also captured the interest of regulators, with the U.S. Department of Justice reportedly launching an investigation into the potential coordination of prices among landlords utilizing the software.[2]

In this paper, we empirically evaluate the impact of algorithmic pricing on the U.S. multi-family rental market. Conceptually, it is not obvious what empirical patterns can be used as evidence for algorithmic coordination, especially given that the adoption of the software may change several aspects of pricing behavior simultaneously. To disentangle various channels, we start with a stylized model of algorithmic pricing. We consider both the possibility that algorithms help building managers set prices more responsively as demand conditions change, and the possibility that algorithms help adopters to coordinate their prices.

The key intuition from the stylized model is that, while it is possible to extract evidence of responsive pricing if one finds that adopters charge lower prices and produce higher quantity during a recession, the model predictions on price and quantity during an economic boom produce identical signs for both a model of responsive pricing and a model of coordinated pricing. As a result, it becomes impossible to distinguish these two models with reduced-form regressions of price or quantity on adoption. In other words, merely observing increasing prices and decreasing quantity with increasing penetration *cannot* be used as evidence for price coordination.

Motivated by the findings of the stylized model, we proceed with (i) a building-level comparison to evaluate the evidence of responsive pricing, (ii) a market-level analysis to estimate the empirical

[1] https://www.propublica.org/article/yieldstar-rent-increase-realpage-rent

[2] https://www.propublica.org/article/yieldstar-realpage-rent-doj-investigation-antitrust

magnitude of the impact of algorithmic pricing, but not as a test of conduct, (iii) a structural model of housing demand from renters and perform a formal conduct test.

We construct a novel data set of algorithm adoption dates merged with the universe of multi-family rentals. We hand-collected the adoption decisions of management companies from a variety of sources, including unstructured data such as internet archives of industry surveys, media updates of the relevant software companies, and market intelligence reports using internet traffic. We then merge the adoption dates with a comprehensive dataset of rental information from REIS. This data consists of a long panel of rent and occupancy of all market-rate multifamily rental buildings in the top 50 metro markets from 2005 to 2019.

We find that at least 25% of buildings, or 34% of units, in the data, were using pricing algorithms as of 2019. Indeed, we find that all of the top 20 management companies have adopted pricing software. Our 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.

Now, to evaluate the role of responsive pricing, we conduct a building-level analysis by comparing the price and quantity of adopters and non-adopters in the same market over time. Specifically, to account for selection on adoption, we include a rich set of controls, including building-fixed effects, building characteristics, and submarket-quality tier-year fixed effects. We also construct an instrumental variable that leverages the intuition that software adoption is often made at the management company level, and thus can be affected by the extent of software adoption in other markets that a management company operates. We find robust evidence that adopters charged *lower* prices and experienced *higher* occupancy during the period of economic recession (2008-2010), suggesting that the pricing software helps buildings set more responsive prices. While we also find evidence that adopters charge *higher* prices and experience *lower* occupancy during the period of economic recovery (after 2013), such empirical patterns could be rationalized with a model of responsive pricing alone, and thus does not produce sufficient evidence for price coordination.

Next, because prices are strategic complements, the impact of algorithmic pricing is not only restricted to adopters, but also the equilibrium responses of non-adopters in the same market. As such, we conduct a market-level analysis to estimate the empirical magnitude of the impact of algorithmic pricing. Across markets, we find that a higher penetration of algorithm pricing software leads to higher rents and lower occupancy. We first show markets that experienced a sudden sharp increase in software adoption charge considerably higher rents and have lower occupancy, compared with markets that do not experience such jumps in adoption rates. Moreover, we find that market-average rent increases monotonically as the penetration of the algorithm increased from 2014 to 2016. In terms of the magnitude, the market average rent of a fully penetrated market is 3.0% higher than an unpenetrated market. This positive relationship is robust to controlling for observable market characteristics and local market conditions such as levels and changes in the unemployment rate, the house price index, household income, and net migration, as well as aggreated version of our instrumental variable for the building-level adoption decision. While the reduced-form analysis is informative in terms of establishing the magnitudes, we proceed to estimate a structural model of the multi-family rental market to perform a formal test of adopter conduct. To that end, we estimate a full structural model of rental demand based on a discrete choice random utility model in the Seattle market. We match on aggregate shares in the REIS data and match on micro-moments from the census-tract American Community Survey. 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. Operationally, we adopt a pair-wise testing framework (Backus et al., 2021; Rivers and Vuong, 2002) to evaluate whether a model of coordination is more or less favored than a model of own-profit-maximization. Unlike a canonical conduct test, we also allow non-adopters to be somewhat unsophisticated where they may not be charging the full amount of markup, so our pair-wise test is performed for each level of non-adopter behavior.

Overall, we find that a model of own-profit-maximization is favored over a model of full coordination in the Seattle market regardless of non-adopter sophistication. However, if non-adopter are assumed to price close to optimally, a model of coordination with a moderate level of internalization is favored over own-profit-maximization. That said, as we consider non-adopters to become more unsophisticated, it becomes harder for our tests to find evidence in favor of coordination.

Our paper's main contribution is to empirically evaluate the impact of algorithmic pricing in a high-stake context with material welfare implications. Moreover, we perform a conduct test by estimating a full structural model of demand and supply, which allows us to incorporate the notion that the adoption of a pricing algorithm may affect multiple aspects of firm's pricing behavior besides coordination. While there is a growing theoretical literature on the issue of algorithmic collusion (Calvano et al., 2020; Asker et al., 2022), to our knowledge, the only empirical study is that of Assad et al. (2020), which examines the effect of algorithmic pricing on the German retail gasoline market. Moreover, their analyses are based on reduced-form regressions alone, whereas we build a structural model of demand, allowing us to perform a conduct test formally.

Our paper finds convincing evidence that algorithmic pricing does lead to responsive prices. As such, a key insight is that any empirical test of the algorithmic collusion also has to take into account the possibility of other changes that algorithmic pricing may produce at the same time, such as more responsive prices. Indeed, theoretical literature have explored channels such as better information on the demand variations, and thus set more responsive prices (Miklós-Thal and Tucker, 2019; Harrington, 2022). But empirical analyses on responsive pricing typically rely on observing extremely high-frequency data in a specific context (Brown and MacKay, 2021) or not explicitly concerned about collusion (Leisten, 2022). As such, this study is unique in that we allow for the potential interaction of the responsive pricing channel and the coordination channel. We believe it is empirically relevant to recognize that the adoption of algorithmic pricing could entail multiple effects, and reduced-form estimates may not be able to isolate the coordination channel because it may produce observationally identical predictions as other concurrent channels.

Our test for coordination builds and extends the extensive literature on conduct tests (Bresnahan, 1987; Nevo, 2001; Miller and Weinberg, 2017; Backus et al., 2021; Magnolfi and Sullivan, 2022; Magnol et al., 2022). We highlight that the result of the conduct test of algorithmic pricing should be combined with a model of non-adopter behavior.

Lastly, understanding the economic impact of pricing power (Watson and Ziv, 2021) in the context of multifamily rentals becomes especially important given its colossal size. At least $100 billion of rent payments are made annually in our dataset alone, representing a sector that is over $2 trillion in asset size.[3] At the household level, rent payments are often the biggest share of household expenditure among renters. Consequently, even a minor percentage impact within this industry translates into substantial value differences.

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 presents a stylized model of algorithmic pricing by setting responsive or coordinated prices. It also illustrates these two models of pricing can generate same-signed predictions, making it difficult to disentangle using reduced-form analysis alone. Next, we show evidence that the algorithm helps landlords set efficient prices in Section 4.1, and we also measure its implication on the market-level rents and occupancy in Section 4.2. To conduct a test of conduct, we describe and estimate a structural model of housing demand from renters in Section 5. The procedure and the results of the conduct test are presented in Section 6. Section 7 concludes.

2 Background and Data

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

The U.S. multifamily housing industry has experienced fast-paced growth after the Great Recession, with a 158% increase in value per square feet from 2010 to 2019.[4] While it has been an attractive investment opportunity for institutional investors with 80% increase in average nominal rents and 50% decrease in vacancy rates, renters of these multifamily units spend a substantial share of their income on their rents.[5]

[3] https://cre.moodysanalytics.com/insights/market-insights/the-fed-and-banks-are-putting-the-squeeze-on-multifamil Based on REIS data and a conservative assumption of 5% cap rate.

[4] https://www.nmhc.org/research-insight/quick-facts-figures/quick-facts-investment-returns-on-apartments

[5] https://www.nmhc.org/research-insight/quick-facts-figures/quick-facts-market-conditions

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, running promotions, and performing various maintenance activities. While the media has recently paid attention to the consolidation of apartment ownership,[6] more pronounced increases in concentration have occurred among management companies. Greystar, the biggest management company in the U.S., has more than quadrupled the number of its managed units over the last ten years, reaching almost 700,000 units as of 2022.[7] Other top management companies have also experienced steady growth in the number of units they manage.

Management companies rely on IT infrastructure to streamline their operation across thousands of units in their buildings. The management companies contract with enterprise software companies to develop property management tools. These software companies provide a suite of services such as processing payments, logging maintenance requests, managing lease turnovers, monitoring vacancies, bookkeeping, etc. Besides such traditional property management services, software companies started to offer rent optimization solutions starting early the 2000s.[8] The "rent optimization" solution is an automated pricing algorithm that suggests rents in real-time by unit type and lease lengths to property managers. It aims to take the guesswork out of pricing for both new and renewal leases.

The adoption of algorithmic pricing solutions has grown rapidly. In 2011, it was reported that around 15% of apartment units had adopted a version of such pricing software.[9] In 2017, 3 million units were reportedly using RealPage's Yieldstar alone, after it acquired the largest competitor, Rainmaker LRO. Fitch reported that approximately 30% of multifamily rental units in the U.S. were using RealPage software in 2021.[10] While it not immediately clear whether all 19 million units use RealPage's pricing algorithm rather than some other tools, we believe this still suggests a great proliferation of more automated property management processes in the multifamily industry.

Although the details of how exactly the software computes optimal rents are not publicly known, a copy of RealPage's presentation slides at a housing conference provides a glimpse of the inner workings of its pricing module Yieldstar. The most notable feature is that the software estimates demand elasticity and forecasts demand at the bedroom level based on lease length and renewal probability while taking into account the prices and vacancies of selected competitors.[11] Figure A2 and A3 show the dashboard view for a property manager, which displays the price recommendations made by the software. It summarizes complex information and reduces the action space for the property manager to either "Accept Rates" or "Review Rates." ProPublica reports that managers accept recommended rents up to 90% of the time.

[6] https://www.propublica.org/article/when-private-equity-becomes-your-landlord

[7] https://www.nmhc.org/research-insight/the-nmhc-50/top-50-lists/2022-top-managers-list/

[8] Yardi RentMAXImizer, RealPage Yieldstar, and Rainmaker LRO

[9] https://web.archive.org/web/20110824021635/http://www.multifamilyrevenue.com/revenue-management-users-multifamily/

[10] https://www.fitchratings.com/research/corporate-finance/fitch-assigns-first-time-b-idr-to-realpage-inc-outlook-s

[11] See Appendix Figure A1 for the exact wordings from the slide deck.

In terms of how the pricing software is able to measure and predict market conditions, in one of its promotional videos, Yieldstar claims that they

> "leverage the statistical analysis collected from the industry's largest lease transaction database, spanning over 11 million units and millions of transactions a year. No one else has this tremendous scope of real-time data that determines daily exceptions and opportunities for maximizing rents and reducing vacancy with utmost accuracy."[12]

While it is almost certainly plausible that Yieldstar utilizes its subscribers' data to form pricing recommendations, it remains unclear whether this feature is designed to achieve competitive, optimal pricing for each user or used to coordinate and maximize the joint profits of all their users.

2.2 Data

We use two main datasets for our empirical analysis. The first dataset is REIS by Moody's Analytics, a comprehensive survey of rent and occupancy aimed at covering all investable 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 supplement our analysis with several additional datasets. We obtain ownership panels of individual buildings in selected metros from Real Capital Analytics (RCA), which keeps track of commercial real estate deals that are over $2.5 million in value. Lastly, we use the American Community Survey to supplement our structural estimation of rental demand.

2.2.1 REIS

Our sample of REIS by Moody's Analytics contains annual snapshots of US market-rate buildings from 2005 to 2019 in the top 50 metro markets, summarized in Table 1. REIS conducts periodic surveys on these buildings' owners and managers and collects information on asking rents, occupancy, concessions, and various amenities. The dataset contains building-year-level observations, including the name of management companies. REIS also provides its own definitions of submarkets, assigning each building to one of 625 "submarkets" in one of 50 "metros." Submarkets completely partition a metro without overlaps.

There are several strengths to this dataset. First and foremost, it provides us with extensive coverage across a long panel. There are 37,216 unique buildings with 7.2 million units covered in our data. According to Fannie Mae, there were approximately 375,000 market-rate properties with 17 million market-rate units in 2021.[13] Given that there were about 1.5 million new units constructed from 2019

[12] Yieldstar Revenue Management Overview Presentation Webinar, accessed by registration on Dec 1st, 2022.

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

and 2021,[14] we believe our data covers approximately half of the universe of all market-rate apartment units in the U.S. Second, compared to typical scraped data of posted prices, the REIS survey includes not only price but also quantity information, characterized in the occupancy data. The presence of both price and quantity is instrumental for our analysis of conduct and deriving welfare implications. Lastly, 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. Because the decision to adopt pricing software is made at the management company level, and the property management industry has experienced consolidation over the past decade, it can lead to an over-counting of adopters in earlier periods. Misclassifying non-adopted buildings as adopters (measurement error in the independent variable) will likely lead to attenuation bias in our estimates.

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, our annual data sample remains sufficient to investigate the potential impact of algorithmic pricing in the rental context. Another 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 as done in Assad et al. (2020). However, this is not a major concern for us because we were able to collect a reasonably confident data set of management companies who adopted the software along with *when* each of them had adopted.

2.2.2 Software Adoption Data

We hand-collected the adoption data from several 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 various media outlets. Both Rainmaker LRO and Yieldstar had an active media presence announcing their major customer acquisitions. Through their main news outlets, not only did they announce customer acquisitions but also major updates (or "patches") to their price optimization software. See Figure 2 for an example of an article.

Lastly, we supplement the data using the list from AppsRunTheWorld.com. This company collects data on the adoption of enterprise IT applications based on a company's technology stack, such as network infrastructure and tools. It then sells insights to salesforces of IT companies for better targeting. We use the list of companies that use Yieldstar. While one may be concerned about the

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

accuracy of this data, fortunately, it contains only a small fraction of adopters compared to the other two more credible sources.

The main limitation is, of course, measurement error. We expect to underestimate the number of adopters because our collection methods are designed to identify the adoption decision of major management companies. We conducted a validation exercise against the current list of buildings that are RealPage customers.[15] We estimate the fraction of false positives to be minimal, but the fraction of false negatives may be much higher, likely concentrated in buildings managed by small management companies.[16] However, the presence of measurement error in the adoption decisions likely leads to attenuation bias in our estimates.

2.3 Descriptives

We present stylized facts by merging the REIS data with the hand-collected data on software adoption. Figure 3 illustrates the penetration trend of pricing software across buildings in REIS. We were able to identify *which* software the management companies had adopted for most of them and the big jump in the market share of Yieldstar in 2017 is due to their acquisition of Rainmaker.[17] In 2012, we find about 19% of units in the REIS data had adopted the software, compared to the estimated penetration of 15% made by the surveyor mentioned in Section 2.2.2. This is not surprising because the REIS survey focuses on investable market-rate buildings, so they are more likely to be professionally managed and more likely to adopt pricing software compared to other types of multifamily apartments. By 2019, we find that a significant fraction have adopted the software, standing at approximately 2.4 million units, or 34% of all units, and 9,124, or 25% of buildings in our data.

Table 3 summarizes the distribution of algorithmic pricing penetration across submarkets. The adoption of algorithmic pricing started in 2005 in our data. Over time, more and more markets have shifted towards higher-valued penetration bins, while substantial variations in the extent of penetration across markets remain.

Despite the concern that the hand-collected adoption data may be prone to false negatives, it is reassuring that we found all 20 out of 20 top management to be adopters (shown in Table 2), based on a ranking produced by the National Multifamily Housing Council (NMHC). In addition, our data also correctly identifies the adoption status of management companies involved in recent class action lawsuits.

[15] https://www.realpage.com/explore,AccessedDec.2022

[16] We randomly select a sample of 641 buildings in our dataset and compare them to the list of RealPage customers as of 2022. Of these buildings, we correctly identify 38% as adopters and 22% as non-adopters. There are minimal false positives. Only 2.3% of buildings were flagged as adopters in our 2019 dataset but did not appear as RealPage customers in 2022. The majority of these buildings were identified as Rainmaker LRO customers, so it is plausible that they have not switched to RealPage following the acquisition. We find that 42% were using RealPage products as of 2022 but were not flagged as adopters in our data. However, we believe it represents a less informative upper bound on false negatives because RealPage's algorithmic pricing tool YieldStar is only *one* of many property management products RealPage offers.

[17] https://www.businesswire.com/news/home/20171204006136/en/RealPage-Closes-Acquisition-of-Lease-Rent-Options-LRO

Figure 4 illustrates the pricing dynamics following the software adoption of two specific companies. Both Essex and Greystar have appeared in multiple lawsuits accusing them of price fixing through their software, especially in the Seattle metro area. Essex adopted the software in 2008, shortly before the financial crisis. Panel (a) shows that Essex aggressively dropped prices and retained much of their occupancy amid the crisis in 2009. In comparison, 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 lost occupancy during economic recovery. 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.

3 Stylized Model

In this section, we outline a stylized model of the multi-family rental market. We first illustrate when prices are more responsive to demand changes, it produces efficiency gains. We then describe how the market functions when a fraction of the market is priced by a piece of software with the objective of joint profit maximization for its adopters. We derive the comparative statics for prices and quantities with respect to the degree of penetration.

3.1 Model Primitives

First, we describe the primitives of the model. Assume that a market is comprised of homogeneous products with no differentiation,[18] 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.

3.2 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 charging market conditions quickly, hanging on to the price from the previous period $p_1^{NA} = p_0$.[19] Adopters, through the usage of the software, are *"alert"*, where they adjust their prices responsively to changing market conditions.

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

[19] 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.

Negative Demand Shock Figure 5 Panel (b) 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.

$$D_1^{A,h}(p) = D_1(p) - (1-h)Q_1^{NA} \tag{3.1}$$

$$S_1^{A,h}(p) = hS(p) \tag{3.2}$$

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

$$p_1^{A,h} < p_1^{NA} \tag{3.3}$$

$$Q_1^{A,h} > Q_1^{NA}. \tag{3.4}$$

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.[20] 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 5 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 5 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

[20] 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.

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 (3.1) and (3.2).

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 5 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.

3.3 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 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:

$$\max_p \pi^M(p) = p\, D(p) - C(D(p)). \tag{3.5}$$

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

$$p\frac{dD}{dp} + D(p) - mc\frac{dD}{dp} = 0, \tag{3.6}$$

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

$$\frac{p^M - mc}{p^M} = \frac{1}{\epsilon_D(p^M)}, \quad \text{where} \quad \epsilon_D(p) = -\frac{dD}{dp}\frac{p}{D}. \tag{3.7}$$

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:

$$\max_p \pi^A(p) = p\, D^A(p) - C^A(D^A(p)) \tag{3.8}$$

where the residual demand D^A becomes

$$D^A(p) = D(p) - (1-h)S(p) \tag{3.9}$$

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

The cost faced by adopters supply becomes

$$C^A(p) = hC\Big(D^A(p)/h\Big) \tag{3.10}$$

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:

$$p\frac{dD^A}{dp} + D^A(p) - mc\frac{dD^A}{dp} = 0, \tag{3.11}$$

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

$$\frac{p^A - mc}{p^A} = \frac{1}{\epsilon_{D^A}(p^A)}, \quad \text{where} \quad \epsilon_{D^A}(p^A) = -\frac{dD^A}{dp}\frac{p}{D^A}. \tag{3.12}$$

Notice the direct parallel between the monopoly markup formula in (3.7) and the coordination markup in (3.12).

For any given price,[21] the residual demand becomes more and more inelastic as the share of adopters h increases

$$\frac{\partial(1/\epsilon_{D^A}(p))}{\partial h} > 0, \tag{3.13}$$

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 set the same price as adopters $p^{NA} = p^A$, but they will not restrict quantity, but will instead each 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

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

produce a lower quantity compared to non-adopters, but they all charge the same price. 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.

Implications for Conduct Test Based on the stylized models discussed, Table 4 summarizes the key predictions of both building-level comparisons (i.e., comparing adopters and non-adopters within the same market) and market-level comparisons (i.e., comparing average price and quantities by varying levels of penetration h) for each of the pricing paradigm.

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 comparisons.

   Hence, during periods of economic recession, if one finds adopters charge lower prices and produce higher quantity than non-adopters in the same market (or, decreasing prices and increasing quantity in markets with greater penetration), then it *is* evidence supporting that the algorithm has indeed led to more responsive prices.

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.[22]

   Hence, during periods of economic boom, if one finds average price increases with penetration and total quantity decreases with penetration (or, lower quantity among adopters than non-adopters), it is *not* evidence supporting that the algorithm has led to coordinated prices.

Indeed, the fact that the predictions of responsive pricing and coordinated pricing are directionally identical during economic booms is yet another conceptual reason why reduced-form "structure-conduct-performance" regressions are limited in their ability to distinguish different models of conduct, adding to the existing concerns documented in (Berry et al., 2019).[23]

Therefore, the stylized models provide us with direct guidance in terms of how to proceed with the empirical analysis in three steps. First, we estimate the building-level differences between adopters and non-adopters to find evidence of responsive pricing. Second, we estimate the impact of adoption

[22]Technically, coordinated pricing predicts that adopters and non-adopters in the same market charge the same price. However, because it is not a positive sign, it still cannot help us isolate evidence for coordination.

[23]While 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, in the appendix Figure A4, we show that when we iterate over all 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 in, one still cannot differentiate a model of coordinated pricing from a model of sub-optimal pricing on the part of non-adopters.

on market-level average prices and quantity. As the stylized model shows, while the findings cannot be used as a test for coordination, it is still empirically relevant to estimate the magnitude of the impact of the algorithmic pricing software. Lastly, we estimate a structural model of housing demand from renters and perform a test of conduct to evaluate the "algorithmic coordination" channel.

4 Measuring the Impact of Algorithmic Pricing

4.1 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.

First, we provide some suggestive evidence from the event study plots of two cohorts of adopters. To illustrate the treatment effect heterogeneity by market conditions, we choose a cohort of buildings that adopted the software before the financial crisis and another cohort that adopted the softer after the crisis. The outcome of interest is $y_{jt} \in \{\log(rent_{jt}), occ_{jt}\}$, which are the log of asking rents and occupancy rate of building j in year t, respectively. We regress both outcomes on calendar-year dummies leading up to and after the adoption. Specifically, we compare the outcomes for a cohort of buildings that adopted the software in year Y with never-adopters as follows:

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

where $a_t(j)$ is an indicator for the adoption status of building j in year t, X_{jt} are time-varying building level covariates, θ_{mqt} are market-quality-tier-year fixed effects where the quality-tier is measured by its pre-adoption rent quartile, μ_{jt} are residuals, and β_τ^Y are our coefficient of interest.

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 are satisfied.

Next, to analyze the full sample period, we estimate the 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 t on all the buildings that have adopted the software by then:

$$y_{jt} = \sum_{\tau=2006}^{2018} \beta_\tau \mathbb{1}\{t = \tau\} a_t(j) + \beta X_{jt} + \theta_{mqt} + \theta_j + \mu_{jt}. \tag{4.2}$$

To address the issue of selection in adoption,[24] we include an extensive list of controls, including the building-specific covariates X_{jt} (e.g., year built, number of floors, the presence of various amenities such as parking, doorman, clubhouse, and swimming pool). In addition, we also include an exhaustive list of fixed effects. We include building fixed-effects θ_j to account for persistent building-level unobservable quality. We also include a fully saturated market-quality-tier-year fixed effect θ_{mqt}, which accounts for the differential trends for buildings in different quality segments in the same market. In other words, it allows for the possibility that luxury buildings may be experiencing faster rent growth than non-luxury buildings in the same market at the same time. Further, given the staggered adoption structure of the data, we also estimate the treatment effects using Callaway and Sant'Anna (2020).

Figure 7 succinctly summarizes 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. The price and quantity patterns during the recession provide evidence that algorithmic pricing has resulted in more responsive prices.

In addition, to the extent that there may still be 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.[25] We expect management companies that are exposed to metros with high share 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 that a management company operates is likely relevant for a company's adoption decision but could be viewed as plausibly exogenous to the local market conditions of the focal building. Hence, we construct the instrument for the adoption status of a given building j in market m based on the extent of algorithmic penetration in all other metro markets m' that its management $c = c(j)$ company operates, weighted by the importance of that market m' to the company based on its portfolio share:

$$Adopt_{cmt}^{IV} = \sum_{m' \neq m} \frac{\sum_{c' \neq c} N_{c'm't}^{A}}{\sum_{c' \neq c} N_{c'm't}} \times \frac{N_{cm't}}{N_{ct}} \tag{4.3}$$

where $N_{c'm't}$ denotes the number of buildings managed by c' in metro m' in year t, the superscript A denotes the number of adopters, and N_{ct} denotes the total number of buildings managed by c across all metros. The variation of the instrument is at the management company-metro-year level.

[24] There is clear evidence of selection when it comes to the adoption algorithmic pricing. Table 5 shows that adopters are more likely to be newer buildings, have more floors, and have more luxury amenities.

[25] https://www.nmhc.org/research-insight/the-nmhc-50/top-50-lists/2022-top-managers-list/

Table 6 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. Overall, we believe the building-level regression results during the recession from 2008 to 2010 provide robust evidence that the adoption of the software has led to more responsive prices.

4.2 Market-level Impact of Algorithmic Pricing

In this section, we estimate the impact of algorithmic pricing across markets by the degree of its penetration across different time periods. It is an important exercise because it not only gives a sense of the total magnitude of rent and occupancy changes but also captures the equilibrium effect of adoption, including non-adopters' strategic response.

The "market" definition we consider is a submarket-building class.[26] We categorize the markets 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, 2014-2016, and 2017-2019. We drop 2005 to 2007 due to low adoption shares, yielding noisy estimates, and drop 2017 to 2019 which likely suffer the most from false negatives (i.e., actual adopters flagged as non-adopters). Markets are then binned by the share of algorithm adopters in 10 percentage point increments. We find considerable mass at 0% by our market definition, and these markets will be considered as the baseline group.[27] We then regress:

$$\begin{aligned} y_{mt} = &\sum_{T=2}^{T=3}\sum_{B=1}^{10}\beta_{T,B}\mathbb{1}\{(T(t)=T\}\mathbb{1}\{B_t(m)=B\} + \sum_{B=1}^{10}\beta_B\mathbb{1}\{B_t(m)=B\} \\ &+ \beta X_{mt} + \theta_m + \theta_t + \mu_{mt}, \end{aligned}$$

where $T(t)$ denotes the year bin that year t belongs to, $B_t(m)$ denotes the binned share of adopters that market m belongs to in year t. X_{mt} includes average building characteristics as well as local economic conditions. To sufficiently control for local, time-varying demand conditions that may be correlated with adoption decisions, we include levels and changes in unemployment rates, household income, housing price, and net migration. The coefficients of interest are $\hat{\beta}_{T,B} + \hat{\beta}_B$ for each T, B.

Figure 8 plots the coefficients on average rent and occupancy for each penetration bin and time period. The coefficients that belong to the periods before and during the financial crisis are in light blue, and the coefficients for the post-crisis period are plotted in light red. From 2008 to 2010, we find clear evidence that average rents decreased significantly with the degree of algorithmic penetrations, whereas average occupancy increased with penetration. From 2014 to 2016, in sharp contrast, we find that rent increased rapidly with the penetration of the algorithm up to nearly 5%, whereas occupancy

[26] A building is classified as either Class A or Class B/C. The choice of submarket-class is intended to capture the group of buildings that are reasonable substitutes. Our results are robust to various alternative market definitions.

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

decreased with the penetration.

Moreover, to examine the robustness of the effect of algorithm penetration at the market level, we construct an instrument for the penetration in market m in year t by aggregating the previous-defined building-level instrument to the market level:

$$AlgoShare_{mt}^{IV} = \frac{1}{N_{mt}} \sum_{j:m(j)=m} Adopt_{c(j),Metro(m),t}^{IV} \tag{4.4}$$

where N_{mt} denotes the number of buildings in market m in year t, and $Adopt_{c(j),Metro,t}^{IV}$ is constructed above as shown in Equation (4.3). We then run the following two-stage least square regression to estimate the time-varying treatment effects:

$$\begin{aligned} AlgoShare_{mt} &= \alpha^{1st} + \beta^{1st} AlgoShare_{mt}^{IV} + \beta^{1st,X} X_{mt} + \theta_m^{1st} + \theta_t^{1st} + \mu_{mt}^{1st} \\ y_{mt} &= \sum_{T=2}^{T=3} \beta_{T,Share} \mathbb{1}\{(T(t) = T\} \widehat{AlgoShare}_{mt}^{IV} + \beta_{Share} \widehat{AlgoShare}_{mt}^{IV} \\ &+ \beta X_{mt} + \theta_m + \theta_{Metro(m)t} + \mu_{mt}, \end{aligned} \tag{4.5}$$

where the first row is the regression equation for the first-stage regression, and the outcomes of interest are market-level average rent and occupancy, $\log(rent_{mt}), occ_{mt}$.

Table 7 shows the IV estimates of $\hat{\beta}_{T,Share} + \hat{\beta}_{Share}$ for each year bin T, while controlling for metro-year fixed effects $\theta_{Metro(m),t}$ as well as time-invariant submarket-class fixed effects θ_m. The magnitude and signs of the IV estimates are similar to the OLS estimates and also consistent with the estimates shown in Figure 8: during the recession, we see rents decreasing and occupancy increasing with the share of adopters, and during the boom the opposite is true.

Overall, both the building-level and market-level results show a clear and consistent picture. The estimates during the Great Recession point toward the presence of responsive pricing. The estimates after the recession show that rents increased and occupancy decreased with the penetration of algorithmic pricing. Because such patterns can be consistent with both responsive pricing and coordinated pricing, it is not evidence of coordination. Hence, we proceed to estimate a full structural model of demand and supply and conduct a formal test of conduct.

5 Renter Demand For Multifamily Housing

In this section, we estimate a structural model of rental housing demand, which can be used to test conduct. We estimate the demand side while remaining agnostic on the model of pricing or competition among multifamily buildings on the supply side.

5.1 Demand Specification

We model renters' decisions as a multinomial discrete choice problem across differentiated rental housing units. We first lay out a general model of the renter's choice and then proceed to specify the empirical implementation of the model. We consider a renter's choice set as all the rental units in a (REIS-defined) submarket m and in year t. We define "product" as bedroom type in building b, $j(b)$ (which we call "unit type," in short). For brevity, we use j instead of $j(b)$ unless otherwise noted since j is unique to a building, and we use jt instead of $jm(j)t$ to denote 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:

$$u_{ijt} = \alpha_i \log(y_i - p_{jt}) + X_{jt}\beta_i + \xi_{jt} + \epsilon_{ijt} \tag{5.1}$$

where y_i is the household's income, p_{jt} is the effective monthly rent of the unit, which we interchangeably call "price", X_{jt} is the observable characteristics of the unit type, and ξ_{jt} is the unobservable quality or demand shock to the unit type unobservable to econometrician. β_i is a vector of households' valuation of observable characteristics in X_{jt}, and α_i is the marginal utility of income net of rent. These preference parameters are a function of a set of renter demographics, d_i. We consider the outside good $j = 0$ all the 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 market m in year t. The probability of renter i choosing to live in $j \in \mathcal{J}_{mt} \cup \{0\}$ is

$$s_{ijt} = \frac{\alpha_i \log(y_i - p_{jt}) + X_{jt}\beta_i + \xi_{jt}}{\sum_{j' \in \mathcal{J}_{mt} \cup \{0\}} \alpha_i \log(y_i - p_{j't}) + X_{j't}\beta_i + \xi_{j't}}. \tag{5.2}$$

5.2 Demand Estimation Procedure

Our estimation goal is to recover parameters governing the demand system $\theta^D := (\boldsymbol{\alpha}, \boldsymbol{\beta}, \boldsymbol{\xi})$. We follow the literature in estimating a multinomial logit demand system with heterogeneous tastes (Berry et al., 1995; Nevo, 2001; Petrin, 2002; Conlon and Gortmaker, 2020).

In terms of model specification, we assume the unobserved quality of a unit type can be decomposed into $\xi_{jt} = \xi_j + \xi_{mt} + \tilde{\xi}_{jt}$, where ξ_j is unit type-level fixed effects, and ξ_{mt} submarket, year-level fixed effects. The unit type-fixed effects, ξ_j, captures 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 $\alpha_i \log(y_i - p_{jt})$ as its first-order approximation, $\alpha_i(y_i - \frac{p_{jt}}{y_i})$, which reduces down to $-\alpha_i \frac{p_{jt}}{y_i}$, and let marginal utility of residual income, α_i, to vary across five annual income bins of renters (in \$1,000s): $\{[0, 20), [20, 35), [35, 50), [50, 75), [75, \infty)\}$, which flexibly allows for non-homothetic preference for housing. We also allow one's household size $d_i = [hs_i]$ to affect their preference for the number of bedrooms and the outside good. Hence the full empirical utility specification becomes

$$u_{ijt} = -\alpha_{I(y_i \in I_y)} \frac{p_{jt}}{y_i} + [1 \quad bed_j]\, \boldsymbol{\beta}_{hs_i} + \xi_j + \xi_{mt} + \tilde{\xi}_{jt} + \epsilon_{ijt}, \tag{5.3}$$

where I_y denotes one of the income bins specified above, and $\beta(hs_i) = \beta \cdot hs_i$.

Estimation of these parameters is achieved by two sources of variations: (1) the observed market shares of unit types and their prices $(\boldsymbol{s}_t, \boldsymbol{p}_t)$ and (2) the joint distribution of building characteristics and renter demographics across markets, $\mathcal{F}_t(p_j, X_j, d_i)$.

The first set of moments helps to identify demand parameters by inverting the observed market shares à la Berry (1994) and Berry et al. (1995) using price and quantity data from REIS. To impose the exclusion restriction, it requires a set of instruments z^D to be independent of unobserved demand shock $\tilde{\xi}_{jt}$, namely $\mathbb{E}[\tilde{\xi}_{jt} z_{jt}] = 0$. Hence, we construct a sample analog to the moment condition:

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

Given that we already include rich building-level and submarket-year fixed effects $\xi_j + \xi_{mt}$ in our model, the candidate instruments in z^D should be uncorrelated with time-varying demand shocks that are specific to buildings or neighborhoods. As Nevo (2000) noted, once product fixed effects are included, the source of endogeneity becomes demand shocks to specific products or markets. An example of such endogeneity in our context could be changes in the provision of public amenities at a given time, such as the addition of a subway station or a park, raising the rents of nearby buildings.

To address such endogeneity, we use the amount of local property tax paid by each building each year as instruments for rent. While the tax paid by each building every year may not fit the strictest definition of marginal cost, it is well documented that landlords pass some of the property taxes to renters (Watson and Ziv, 2021). The validity of tax instruments relies on the assumption that the assessed value of the building measured by local authorities is set *before* the realization of the contemporaneous demand shock, but building managers set prices after observing the tax obligations calculated from the assessed value, along with other contemporaneous demand shocks. We believe that once the amount of tax is residualized by the product fixed effects and the market-year fixed effects, the remaining variation reflects idiosyncratic timings of assessment value change and changes in the local tax regime.

The second set of moments are "micro-moments" on the joint distribution of building characteristics and renter demographics. We use census tract-level estimates from the 5-year American Community Survey (ACS) to construct the covariances between demographics (y_i, hs_i) and the characteristics (p_i, bed_i) chosen by such renters. For each census tract, we observe the joint distribution of renter households' income and fraction spent on rent, and the joint distribution of households' size and occupancy per room. Once tracts are aggregated to the submarket level, this gives two micro-moments to target for each market: $Cov_{mt}(y_i, p_i)$ and $Cov_{mt}(hs_i, bed_i)$. These moments further help us pin down α_i and heterogeneous taste for the number of bedrooms by household size.

The difference between observed moments and corresponding moments given a set of parameters form a criterion function $\hat{G}_2(\theta^D) = v - \hat{v}(\theta^D)$, where v denotes a statistical moment from the data, and $\hat{v}(\theta^D)$ denotes corresponding, predicted moment as a function of parameters from estimation. For example, the criterion function that targets the covariance between income and income spent on rent for renters in market mt is

$$G_2^{mt}(\theta^D) = \overbrace{\frac{1}{I_{mt}^{ACS}}\left(\sum_i p_i y_i - \sum_i p_i \sum_i y_i\right)}^{\text{“observed” ACS } Cov_{mt}(y_i, p_i)} - \underbrace{\left(\sum_{i,j} w_i s_{ijt}(\theta^D) p_j y_i - \sum_{i,j} w_i s_{ijt}(\theta^D) y_i \cdot \sum_{i,j} w_i s_{ijt}(\theta^D) p_j\right)}_{\text{computed } Cov_{mt}(y_i, p_i^*) \text{ given } \theta^D},$$

where w_i denotes sampled individual's weight and s_{ijt} is renter's choice probability as explained above. 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 rent paid and income as observed from the ACS data.

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

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

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

5.3 Demand Estimation Results

We focus our estimation of renter demand and the subsequent tests of conduct on the Seattle market from 2011 to 2018 for several practical reasons. First, the Seattle market has been at the center of litigation with a significant degree of algorithmic pricing penetration. Second, we were able to hand-collect a detailed ownership panel of most buildings in Seattle from Real Capital Analytics to account for existing underlying ownership of buildings.

Table 8 summarizes key moments from the Seattle market. Panel (A) contains key statistics that relate to market share and prices, which includes the unit-type effective rent and occupancy. Panel (B) contains key moments from the ACS that form the basis of the micro-moments G_2. The advantage of using the ACS is that REIS buildings are a proper subset. Hence, we get a precise estimate of the entire market size and how renters choose across units in general. As shown in Panel (C), there are 319 census tracts covering 16 REIS-submarkets, providing us with a level of granularity to construct the covariance moments for each submarket. Summary statistics from the ownership data in Panel (D) resolve one of our key underlying concerns that the building ownership may be concentrated. We match individual buildings to transactions recorded by Real Capital Analytics (RCA) from 2000 to 2020. We were able to match 75% of buildings in Seattle. Among the 876 matched buildings, we found 607 different owners, implying a Herfindahl-Hirschman index (HHI) of less than 600, which is well below even a "moderately" concentrated industry as defined by the Department of Justice.[28] Hence, we do not consider ownership concentration to be a major concern for the Seattle market. The fraction of units priced by the software is considerably high at 43%.

We estimate the demand parameters using `pyblp` (Conlon and Gortmaker, 2020, 2023), summarized in Table 9. The first column shows the estimates using the tax IV, and the second column is estimated with the approximated demand "optimal instruments" in the spirit of Chamberlain (1987) implemented by Conlon and Gortmaker (2020). Note that the optimal IV is approximated without any supply-side restriction, hence also agnostic to supply-side assumptions.

The parameters governing the marginal utility of income for each income group are significant and sensible across two columns. Since the coefficient divided by income can be interpreted as the price coefficient of individual $-\frac{\alpha_I}{y_I}$, our estimates show that renters in higher income bins are less price sensitive than those in lower income bins. Renters who choose buildings in REIS tend to be smaller households, as implied by a significant negative coefficient on the interaction term between the household size and the inside-good indicator. This also makes sense given that REIS buildings tend to be high-rise multifamily apartments mostly comprised of studios, one-bedroom, and two-bedroom units, averaging 1.5 bedrooms per unit. In contrast, non-REIS dwellings have more bedrooms than REIS buildings from the ACS data, averaging 1.9 bedrooms per unit. Lastly, we see renters with greater household size deriving higher utility from units with more bedrooms, as implied by a positive, significant coefficient on household size-bed interaction term.

We close the section on renter demand for housing by discussing estimated elasticities. The median own-elasticity of buildings ranges from -2.5 to -3, which implies that each building faces elastic residual demand. This estimate is similar to the recent findings of Watson and Ziv (2021), where they also find median elasticity of -2.2 to -3.5. When aggregated up to REIS buildings, they face inelastic aggregate demand with elasticity of -0.58 to -0.47, again largely in line with the existing literature (Chen et al., 2011; Albouy et al., 2016; Watson and Ziv, 2021). The degree of substitution from REIS buildings to non-REIS rental buildings is captured by the mean diversion ratio to the outside good, $-\mathbb{E}[\frac{\partial s_{0t}}{\partial p_{jt}} / \frac{\partial s_{jt}}{\partial p_{jt}}]$, which is around 0.57, suggesting that on average, for every person leaving a REIS building, 0.57 choose to live in a non-REIS building. Overall, our demand-side estimation shows that, while individual buildings face elastic demand, they collectively fact inelastic aggregate demand and face some competitive pressure from other landlords not present in the REIS dataset.

[28] RCA tracks transactions involving more than \$2.5M. It also attempts to uncover the ownership based on various public sources beyond deed records. If we treat unmatched buildings under the RCA-size threshold as individual owners, the implied HHI is less than 400.

6 Testing Alternative Models of Conduct and Pricing

6.1 Testing Procedure

With the demand estimates recovered, we proceed to test across alternative models of coordination among adopters, adopting a pair-wise testing procedure based on Backus et al. (2021). Their approach builds upon the intuition from Berry and Haile (2014) and 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.

To provide intuition behind the testing procedure, we write each unit type's rent p_{jt} as being decomposed into two parts: marginal cost mc_{jt} and markup η_{jt}, neither of which is directly observable to the researcher:

$$p_{jt} = mc_{jt} + \eta_{jt}.$$

Given that we assume buildings are pricing according to a static pricing game, the first order condition gives rise to a vector of marginal costs and markups such that:

$$\boldsymbol{p}_t = \boldsymbol{mc}_t + \overbrace{\left(-\mathcal{H}^M \odot \frac{\partial \boldsymbol{s}_t}{\partial \boldsymbol{p}_t}\right)^{-1} \boldsymbol{s}_t}^{\boldsymbol{\eta}_t(\mathcal{H}^M)}, \tag{6.1}$$

where $\mathcal{H}^M$ is a $N_{jt} \times N_{jt}$ "internalization matrix" under a given conduct assumption, $\frac{\partial s_t}{\partial p_t}$ is a $N_{jt} \times N_{jt}$ matrix of own- and cross- derivatives of shares with respect to price, and $\odot$ denotes element-by-element product of those two matrices.

Notably, because the demand derivatives $\frac{\partial s_t}{\partial p_t}$ are separately estimated already, the internalization matrix $\mathcal{H}^M$ becomes the sole determinant of markup. As such, we treat marginal cost mc_{jt} as a residual, namely, $mc_{j,t}^M = p_{j,t} - \eta_{j,t}(\mathcal{H}^M)$. Intuitively, to test for conduct, what we have to do is to find instruments $\boldsymbol{z}_t^S$ that affect demand and markup but otherwise orthogonal to marginal cost shocks, where the corresponding moment condition would hold under that correct model of conduct

$$\mathbb{E}[\omega_{jt}|\boldsymbol{z}_t^S] = 0. \tag{6.2}$$

Here, we extend the canonical testing literature by further parameterizing the internalization matrix $\mathcal{H}$ to encapsulate both the possibility of coordination by the adopters and the possibility of mispricing by non-adopters. Such mispricings on the part of non-adopters take the form of charging less than their full markup.[29] To fix ideas, consider three buildings each with single unit-type $j = 1, 2, 3$. $j = 1, 2$

[29] We believe allowing for some degree of sub-optimal pricing among non-adopters is an important empirical consideration of this market. Besides stale prices that we have discussed before, there may be various reasons that may lead non-adopters not to charge their full markup, ranging from information friction, bounded rationality, agency problems, risk aversion, etc. While we do not pinpoint the exact source of the friction, it is encapsulated in the model parameter τ^{NA}, namely, the fraction of markup that non-adopters charge.

are adopters of the software and $j = 3$ is not. If all firms operate independently and price to compete with one another, the true internalization matrix $\mathcal{H}$ would look like the one on the left. If the adopters coordinate to a degree of $\tau^A \in (0, 1]$, and non-adopters price optimally, then the internalization matrix would look like the one in the middle. Finally, if non-adopters are pricing sub-optimally to charge only a fraction $\tau^{NA} \in (0, 1]$ of their full markup, the internalization matrix would look like the far-right one.

$$\begin{pmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{pmatrix} \qquad \begin{pmatrix} 1 & \tau^A & 0 \\ \tau^A & 1 & 0 \\ 0 & 0 & 1 \end{pmatrix} \qquad \begin{pmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & \frac{1}{\tau^{NA}} \end{pmatrix}$$

Together, we write the corresponding internalization matrix as a function of (τ^A, τ^{NA}) as follows:

$$\mathcal{H}(\tau^A, \tau^{NA}) = \begin{pmatrix} 1 & \tau^A & 0 \\ \tau^A & 1 & 0 \\ 0 & 0 & \frac{1}{\tau^{NA}} \end{pmatrix}$$

While it may seem straightforward to directly estimate (τ^A, τ^{NA}) through GMM, there are several cautions against it. Such estimation of the conduct parameter requires strong instruments for markup (Nevo, 1998), and could be sensitive to misspecification of demand model and functional form of marginal cost (Magnolfi and Sullivan, 2022; Duarte et al., 2023). Therefore, we take the more conservative approach of pair-wise testing (Backus et al., 2021). For any two models of conduct M_1 and M_2, the null hypothesis is that they fit the data equally well. The two-sided alternative hypothesis is that one model is "favored" over the other. To the extent that the underlying model may be misspecified, the pair-wise approach asks which model of conduct is "less wrong".

Because we have a two-dimensional mechanism of interest including both coordination and mispricing, we hold the degree of mispricing fixed while testing for coordination. Therefore, we ask whether a model of coordination at level τ^A is more or less favored than a model of own-profit maximization, holding fixed a given level of non-adopter behavior at τ^{NA}.

To perform the test, we model the marginal cost of each building as a non-parametric function plus an additively separable unobservable shock:

$$mc_{jt} = f(x_{jt}, w_{jt}, occ_{jt}) + \omega_{jt}, \tag{6.3}$$

where x_{jt} is characteristics of the unit type that enters both renter's demand and the building's marginal cost, such as the number of bedrooms, w_{jt} is a vector of characteristics that only enters cost, such as tax, and $occ_{jt} = \frac{q_{jt}}{K_j} \in [0, 1]$ is the occupancy, which is the number of occupied units, q_{jt} divided by the building's capacity of each unit type, K_j.

While it may be reasonable that quantity does not affect marginal cost in other contexts such as

Backus et al. (2021), in the housing context, however, given the binding nature of building capacity, we believe that it is paramount that occupancy itself enters into the marginal cost to match the empirical fact that buildings cannot be rented at above 100% occupancy. 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 $\hat{f}(\cdot)$. As such, it requires us to also instrument for building occupancy. Moreover, because we want to maintain a flexible function form in $f(\cdot)$, instead of using a two-stage least square, we adopt a non-parametric IV sieve estimation procedure by Newey and Powell (2003) to address this concern.

Given two competing models characterized by differing internalization matrix $\mathcal{H}^{M_1}$ and $\mathcal{H}^{M_2}$, the pair-wise testing amounts to computing a test statistic based on the difference of unconditional moment restriction

$$\mathbb{E}[(\omega_{j,t}^{M1})' A(\boldsymbol{z}_t^S)] - \mathbb{E}[(\omega_{j,t}^{M2})' A(\boldsymbol{z}_t^S)] \tag{6.4}$$

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

$$A(\boldsymbol{z}^S) = \mathbb{E}[\Delta\eta_{jt}^{1,2}|\boldsymbol{z}^S], \tag{6.5}$$

where $\Delta\eta_{jt}^{1,2} = \eta_{jt}(\mathcal{H}^{M_1}) - \eta_{jt}(\mathcal{H}^{M_2})$ and the rest of the details are described in Algorithm 1.

We can use the same set of $\boldsymbol{z}^S$ to address endogeneity in occ_{jt} and for the moment condition in Eq (6.2). The exclusion restriction of candidate instruments is that they affect markup and quantity but are not correlated with unobserved marginal cost shocks. Following Backus et al. (2021), we use the demand-side optimal IV approximated from the demand estimates to capture efficient non-linear combinations of individual demand instruments. We also consider other instruments, such as census-tract level capacity. Any change in nearby capacity is due to the completion of new buildings or demolition of old buildings, where the exact timings of such events are plausibly exogenous to nearby buildings' marginal *cost* shocks. Yet, it should generate variations in markups in the nearby buildings as they change the set of close competitors in the local market.

6.2 Results of the Conduct Tests

As mentioned above, we test whether a model of adopter coordination is more or less favored by data than a model of adopter own-profit maximization while holding fixed the level of mispricing by non-adopters.

Figure 9 summarizes the result of our test using data from Seattle. The X-axis denotes the conduct test, namely, whether data favor a model of coordination at level τ^A or a model of own-profit maximization. A value of $\tau_A = 1$ indicates we are testing full coordination vs. own-profit maximization. A value of $\tau_A = 0.1$ indicates we are testing coordination where adopters internalize 10% of their fellow

Algorithm 1 Testing Procedure

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

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

2-1. For each cost estimate $\hat{mc}^M$, fit $f(x_{jt}, w_{jt}, occ_{jt})$ using a non-parameteric estimation procedure and compute the residual, $\hat{\omega}^M$:

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

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 as a function of candidate instrumental variables, $\boldsymbol{z}^S$:

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

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

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

---

adopters' profit. The y-axis denotes the level of non-adopter sophistication τ^{NA}. At $\tau^{NA} = 1$, non-adopters are sophisticated and maximize their own profit by charging the full markup. At $\tau^{NA} = 0$, non-adopters do not charge any markup and are marginal-cost pricers. We fill out the entire matrix by illustrating the range of possible non-adopter behavior. The color of the figure indicates the sign, and the depth of the color indicates its statistical significance. A shade of green indicates a positive-valued test statistic, whereby a model of coordination is favored over a model of own-profit-maximization. A shade of blue indicates a negative-valued test statistic, whereby a model of own-profit-maximization is favored over a model of coordination. Table 10 shows the value of test statistics for each cell in Figure 9.

We make several observations: First, with fully sophisticated non-adopters ($\tau^{NA} = 1$), our test favors a model of own-profit-maximization over full coordination ($\tau^A = 1$), as indicated by the blue color in the top right corner of the chart. Second, at any level of non-adopter sophistication, our test still favors a model of own-profit-maximization over full coordination, as indicated by the blue color in the right-most column. Third, with fully sophisticated non-adopters ($\tau^{NA} = 1$), our test favors a model of moderate-degree- coordination over own-profit-maximization, as indicated by the green shades in the top row.[30] Lastly, as we allow for less sophisticated non-adopters, it becomes harder to find evidence that favors coordination, as indicated by the increasing amount of blue shades as one goes down in the vertical direction.

Intuitively, we think that the gradient of test statistic over what we assume of non-adopters and the conduct of adopters makes sense. If we assume non-adopters are unsophisticated and charging closer to their marginal cost, the difference in prices between adopters and non-adopters can then be rationalized by adopter charging their full markup. So it becomes harder for us to find evidence of coordination. However, if non-adopters are already charging the full markup, then price difference between adopters and non-adopters are better explained by the coordination channel.

Currently, our test does not prove there is evidence for coordination, nor does it exonerate the algorithm from coordination. Nonetheless, our series of tests provides us with a range of assumptions on non-adopter behavior in which a model of some coordination may be favored over own-profit-maximization. Given our reduced form evidence of the responsive pricing channel, it is unlikely that non-adopters are behaving optimally, suggesting the scope for coordination may be more limited than what is implied from a full rational benchmark.

7 Conclusion

In this paper, we examine the impact of algorithmic pricing software adoption on the U.S. multifamily housing industry. We hand-collect a dataset of management company adoption status from a variety of 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 price more responsively. The treatment effect of the algorithm at the building level is heterogeneous across time periods. During the great recession from 2009 to 2010, the adopters of the algorithm lowered rents and increased occupancy, compared to comparable non-adopters in the same submarket. Conversely, during a period of economic recovery from 2014 to 2017, the adopters of the algorithm increased rents and reduced occupancy.

Second, to measure the aggregate impact, we estimate the market-level treatment effect of algorithmic pricing penetration. Again, during periods of economic recovery, we find that across markets, higher levels of penetration have led to significantly higher rents and lower occupancy. This pattern is

[30] To the extent that a model of coordination at a moderate level is favored when non-adopters are sophisticated, it still begs the question of the incentive compatibility problem. In other words, it is rather inconceivable that building owners can transfer profits to other adopter building owners. As such, this may be another reason that high levels of coordination may lead to some adopters being worse off, which is not consistent with the incentive of the software company, which presumably attempts to maximize the number of subscribers.

robust across alternative market definitions, regression specifications, and instrumenting a building's adoption with its management company's exposure to algorithmic pricing in other markets.

While both the building-level and the market-level results provide us evidence that the software helps adopters set more responsive prices, we caution against using the reduced-form results as a conclusive test for conduct. This is because a model of coordination produces directionally the same results as a model of responsive pricing during an economic boom.

Lastly, to test for conduct, we take a structural approach. We estimate a model of housing choices from renters based on housing characteristics and household demographics. A pair-wise testing procedure shows that a model of own-profit-maximization is favored over a model of full coordination in the Seattle market. However, if non-adopter are assumed to price optimally, a model of coordination at a moderate level is favored over own-profit-maximization. That said, to the extent that we consider non-adopters to be somewhat unsophisticated, the test becomes more likely to favor own-profit-maximization than coordination.

While the conduct test is powerful for testing coordination for a given level of non-adopter behavior, as it currently stands, it is not well-suited to test the degree of non-adopter sophistication. Hence, an appropriate next step is to extend the structure model to incorporate the extent of non-adopter responsiveness and sophistication in a more unified conduct test framework.

Overall, our findings and empirical approach have far-reaching implications. The real estate industry is colossal, with an estimated asset value exceeding 2 trillion dollars within our dataset alone. Consequently, even a minor percentage impact within this industry translates into substantial value differences. Moreover, as intelligent algorithmic pricing becomes increasingly prevalent across various sectors, and given the market concentration of such business services software, the proper treatment and regulation of "algorithmic coordination" emerge as pressing concerns for businesses, consumers, and regulators alike.

References

Albouy, David, Gabriel Ehrlich, and Yingyi Liu, "Housing Demand, Cost-of-Living Inequality, and the Affordability Crisis," Technical Report w22816, National Bureau of Economic Research, Cambridge, MA November 2016.

Asker, John, Chaim Fershtman, and Ariel Pakes, "Artificial Intelligence, Algorithm Design, and Pricing," *AEA Papers and Proceedings*, May 2022, *112*, 452–456.

Assad, Stephanie, Robert Clark, Daniel Ershov, and Lei Xu, "Algorithmic Pricing and Competition: Empirical Evidence from the German Retail Gasoline Market," *SSRN Electronic Journal*, 2020.

Backus, Matthew, Christopher Conlon, and Michael Sinkinson, "Common Ownership and Competition in the Ready-To-Eat Cereal Industry," *Working paper*, 2021.

Bason v. RealPage, Inc., October 2022.

Berry, Steven and Philip A. Haile, "Identification in Differentiated Products Markets Using Market Level Data," *Econometrica*, 2014, *82* (5), 1749–1797.

_ , James Levinsohn, and Ariel Pakes, "Automobile Prices in Market Equilibrium," *Econometrica*, July 1995, *63* (4), 841.

_ , Martin Gaynor, and Fiona Scott Morton, "Do Increasing Markups Matter? Lessons from Empirical Industrial Organization," *Journal of Economic Perspectives*, August 2019, *33* (3), 44–68.

Berry, Steven T., "Estimating Discrete-Choice Models of Product Differentiation," *The RAND Journal of Economics*, 1994, *25* (2), 242.

Bresnahan, Timothy F., "Competition and Collusion in the American Automobile Industry: The 1955 Price War," *The Journal of Industrial Economics*, 1987, *35* (4), 457–482. Publisher: Wiley.

Brown, Zach Y and Alexander MacKay, "Competition in Pricing Algorithms," 2021, p. 65.

Callaway, Brantly and Pedro H.C. Sant'Anna, "Difference-in-Differences with multiple time periods," *Journal of Econometrics*, December 2020, p. S0304407620303948.

Calvano, Emilio, Giacomo Calzolari, Vincenzo Denicolò, and Sergio Pastorello, "Artificial Intelligence, Algorithmic Pricing, and Collusion," *American Economic Review*, October 2020, *110* (10), 3267–3297.

Chamberlain, Gary, "Asymptotic efficiency in estimation with conditional moment restrictions," *Journal of Econometrics*, March 1987, *34* (3), 305–334.

Chen, Yong, John M. Clapp, and Dogan Tirtiroglu, "Hedonic estimation of housing demand elasticity with a markup over marginal costs," *Journal of Housing Economics*, December 2011, *20* (4), 233–248.

Conlon, Christopher and Jeff Gortmaker, "Best practices for differentiated products demand estimation with PyBLP," *The RAND Journal of Economics*, 2020, *51* (4), 1108–1161.

_ and _ , "Incorporating micro data into differentiated products demand estimation with PyBLP," 2023. Working paper.

Duarte, Marco, Lorenzo Magnolfi, Mikkel Sølvsten, and Christopher Sullivan, "Testing Firm Conduct," January 2023. arXiv:2301.06720 [econ].

Fortin, John, "Algorithms and Conscious Parallelism: Why Current Antitrust Doctrine is Prepared for the Twenty-First Century Challenges Posed by Dynamic Pricing," *SSRN Electronic Journal*, 2020.

Harrington, Joseph E., "The Effect of Outsourcing Pricing Algorithms on Market Competition," *Management Science*, September 2022, *68* (9), 6889–6906.

Leisten, Matthew, "Informational Difference Among Rival Firms: Measurement and Evidence from Hotels," 2022.

Magnolfi, Lorenzo and Christopher Sullivan, "A comparison of testing and estimation of firm conduct," *Economics Letters*, March 2022, *212*, 110316.

Magnol, Lorenzo, Daniel Quint, Christopher Sullivan, and Sarah Waldfogel, "Falsifying Models of Firm Conduct," *Working paper*, 2022.

Mcsweeny, Terrell and Brian O'Dea, "The Implications of Algorithmic Pricing for Coordinated Effects Analysis and Price Discrimination Markets in Antitrust Enforcement," 2017.

Miklós-Thal, Jeanine and Catherine Tucker, "Collusion by Algorithm: Does Better Demand Prediction Facilitate Coordination Between Sellers?," *Management Science*, April 2019, *65* (4), 1552–1561.

Miller, Nathan H. and Matthew C. Weinberg, "Understanding the Price Effects of the MillerCoors Joint Venture," *Econometrica*, 2017, *85* (6), 1763–1791. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.3982/ECTA13333.

Navarro v. RealPage, Inc. et al, November 2022.

Nevo, Aviv, "Identification of the Oligopoly Solution Concept in a Differentiated Products Industry," *Economics Letters*, 1998, pp. 391–395.

_ , "A Practitioner's Guide to Estimation of Random-Coef cients Logit Models of Demand," *Journal of Economics and Management Strategy*, 2000, p. 36.

_ , "Measuring Market Power in the Ready-to-Eat Cereal Industry," *Econometrica*, 2001, *69* (2), 307–342. Publisher: [Wiley, Econometric Society].

Newey, Whitney K. and James L. Powell, "Instrumental Variable Estimation of Nonparametric Models," *Econometrica*, September 2003, *71* (5), 1565–1578.

OECD, "Algorithms and Collusion - Note by the United States," 2017.

Petrin, Amil, "Quantifying the Benefits of New Products: The Case of the Minivan," *Journal of Political Economy*, August 2002, *110* (4), 705–729.

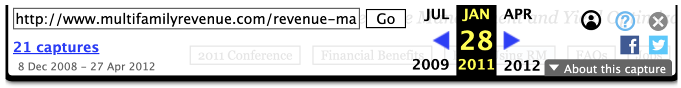
Rivers, Douglas and Quang Vuong, "Model selection tests for nonlinear dynamic models," *The Econometrics Journal*, 2002, *5* (1), 1–39. Publisher: [Royal Economic Society, Wiley].

Watson, C. Luke and Oren Ziv, "Is the Rent Too High? Land Ownership and Monopoly Power," *SSRN Electronic Journal*, 2021.
Yusupov v. RealPage, Inc et al

Yusupov v. RealPage, Inc et al, January 2023.

8 Figures

Figure 1: Snapshot of Surveyed List of Adopters



Firms Using RM

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

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

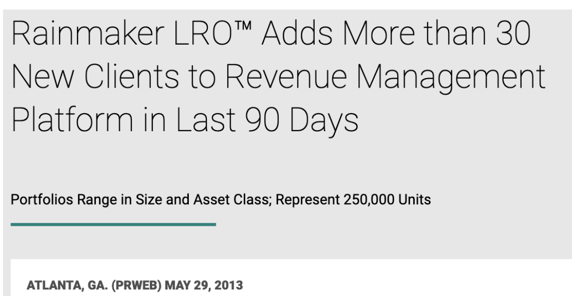
This list is compiled from press reports and the records of the major revenue management providers. The list is updated periodically. Please contact us with any corrections and additions.

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

https://web.archive.org/web/20110128035809/http://www.multifamilyrevenue.com/revenue-management-users-multifamily/

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

(a) Rainmaker



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

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

ATLANTA, GA. (PRWEB) MAY 29, 2013

(b) Yieldstar

RealPage Announces that Wilkinson Selects YieldStar Price Optimizer

By RealPage News | June 21, 2011

Decision follows substantial improvements in rent and occupancy during trial

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

(Rainmaker) https://www.prweb.com/releases/rainmakerlro/adds30newcompanies/prweb10779081.htm
(Yieldstar) https://www.realpage.com/news/realpage-announces-that-wilkinson-selects-yieldstar-price-optimizer/

Figure 3: Share of Adoption by Software

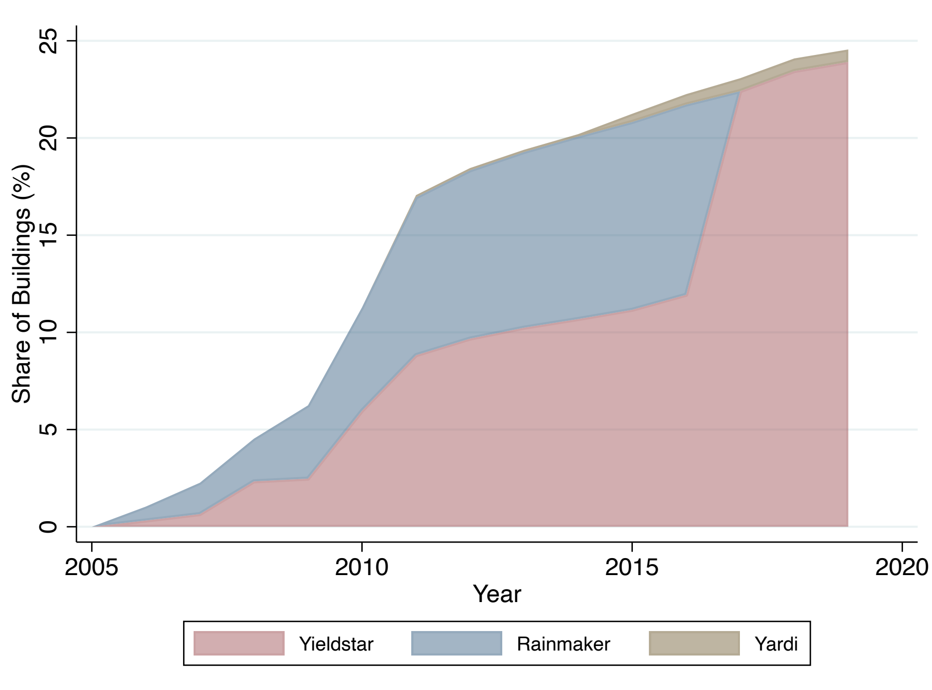
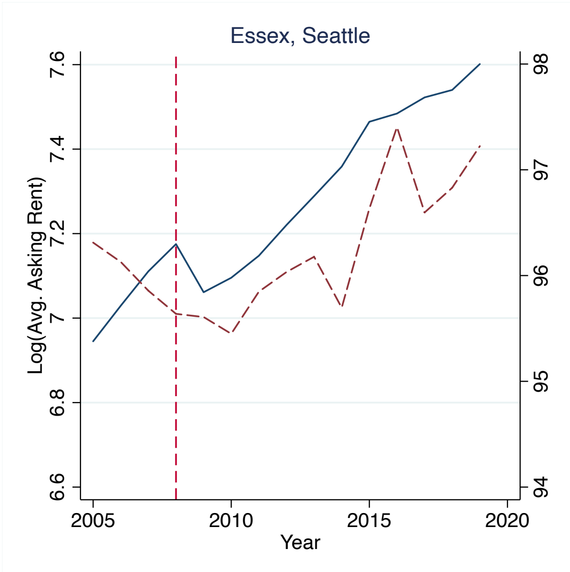
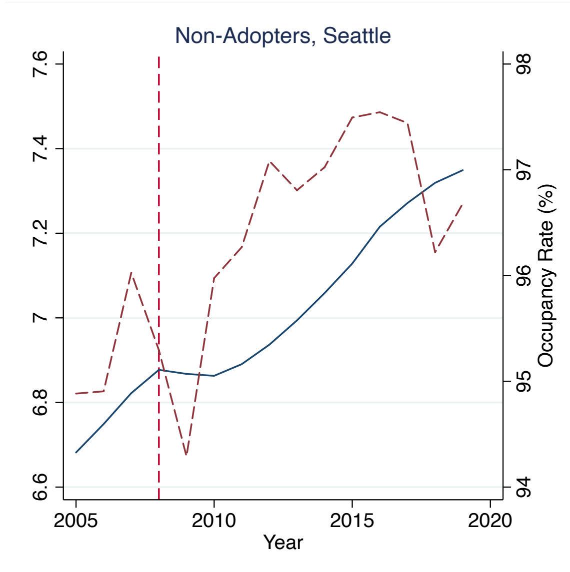


Figure 4: Case Study: Pricing and Occupancy Trend of Companies Adopted the Software

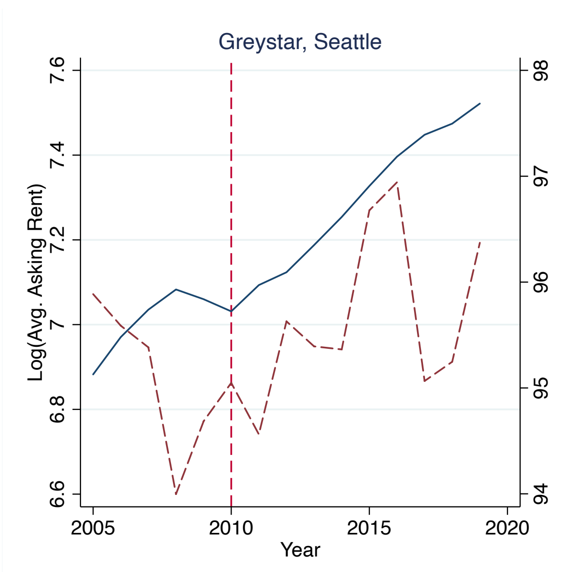
(a) Essex



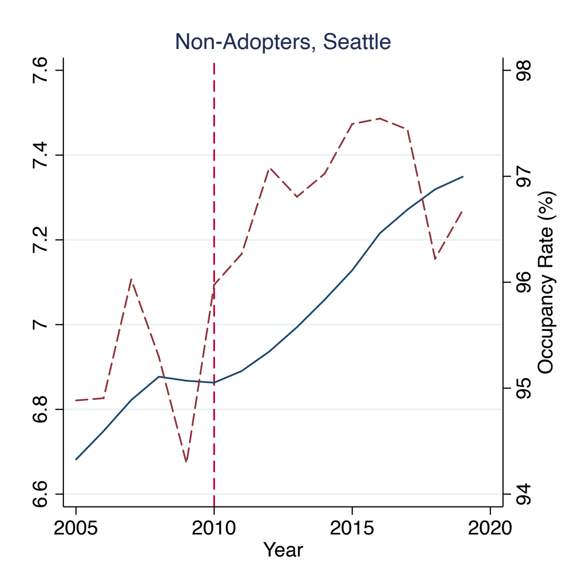
(b) Non-Adopters



(c) Greystar



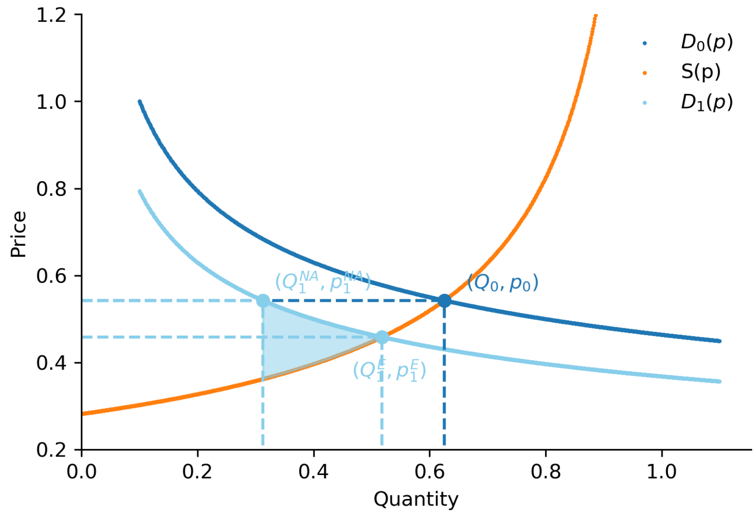
(d) Non-Adopters



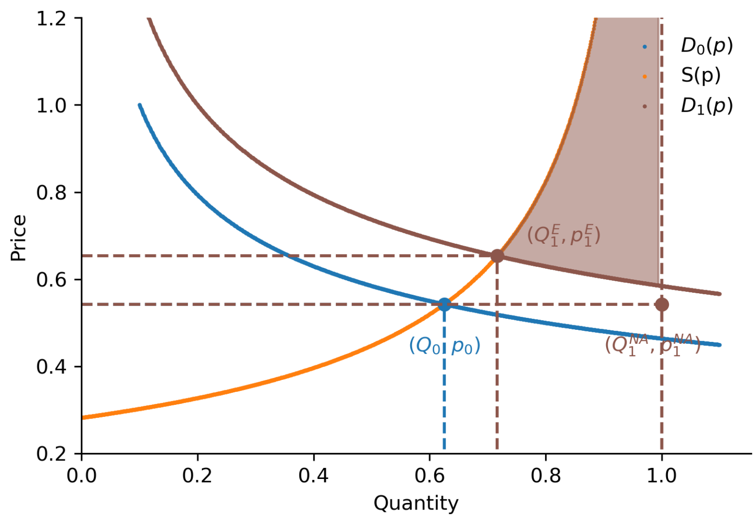
Restricting samples to Seattle metro-area apartments. Solid, navy line follows Log(Rent) and dashed, red line follows occupancy rate. The vertical dashed line indicates the year of adoption of each management company.

Figure 5: Responsive Pricing to Demand Shocks

(a) Negative Demand Shock



(b) Positive Demand Shock

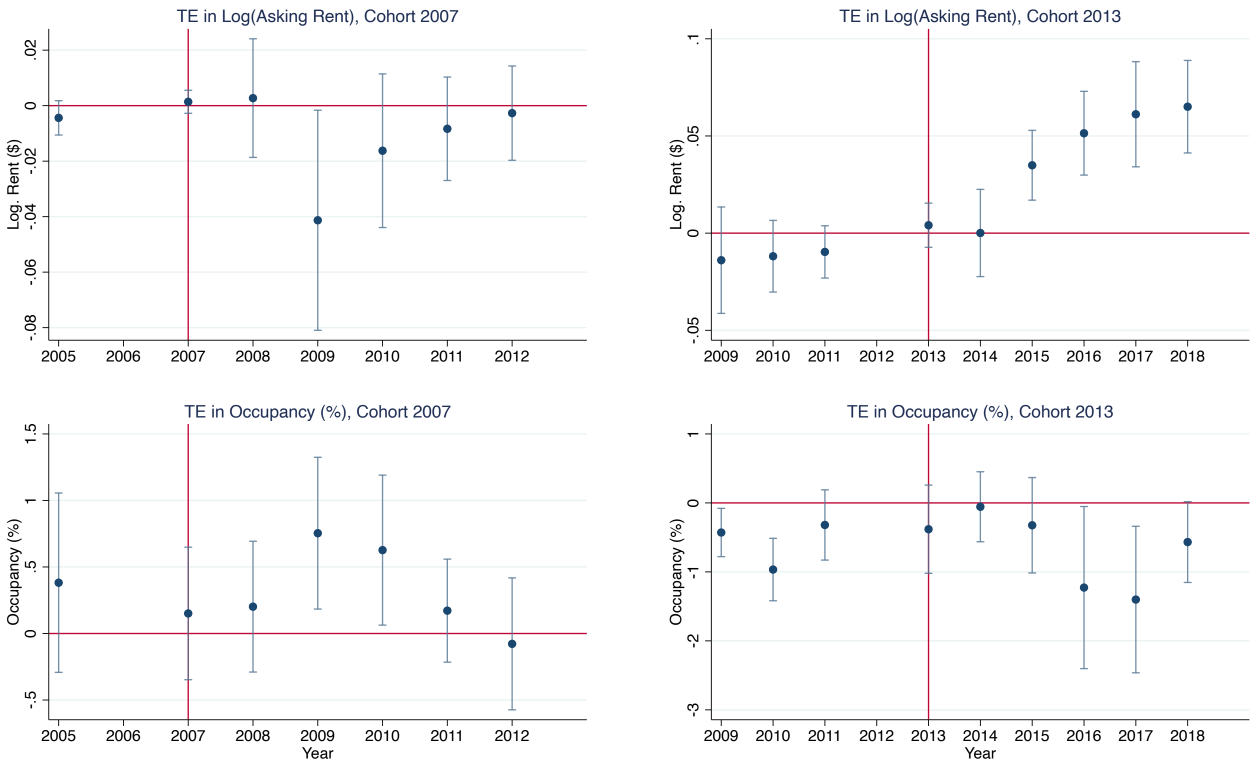


A stylized model of responsive pricing. In the top panel, with a negative demand shock, stale prices that are too high relative to the new equilibrium prices lead to excessive vacancies. Lowering prices more quickly results in welfare gains that are indicated by the blue shaded region. In the bottom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

Figure 6: Dynamic TE of Adoption on Log(Asking Rent) and Occupancy Rate

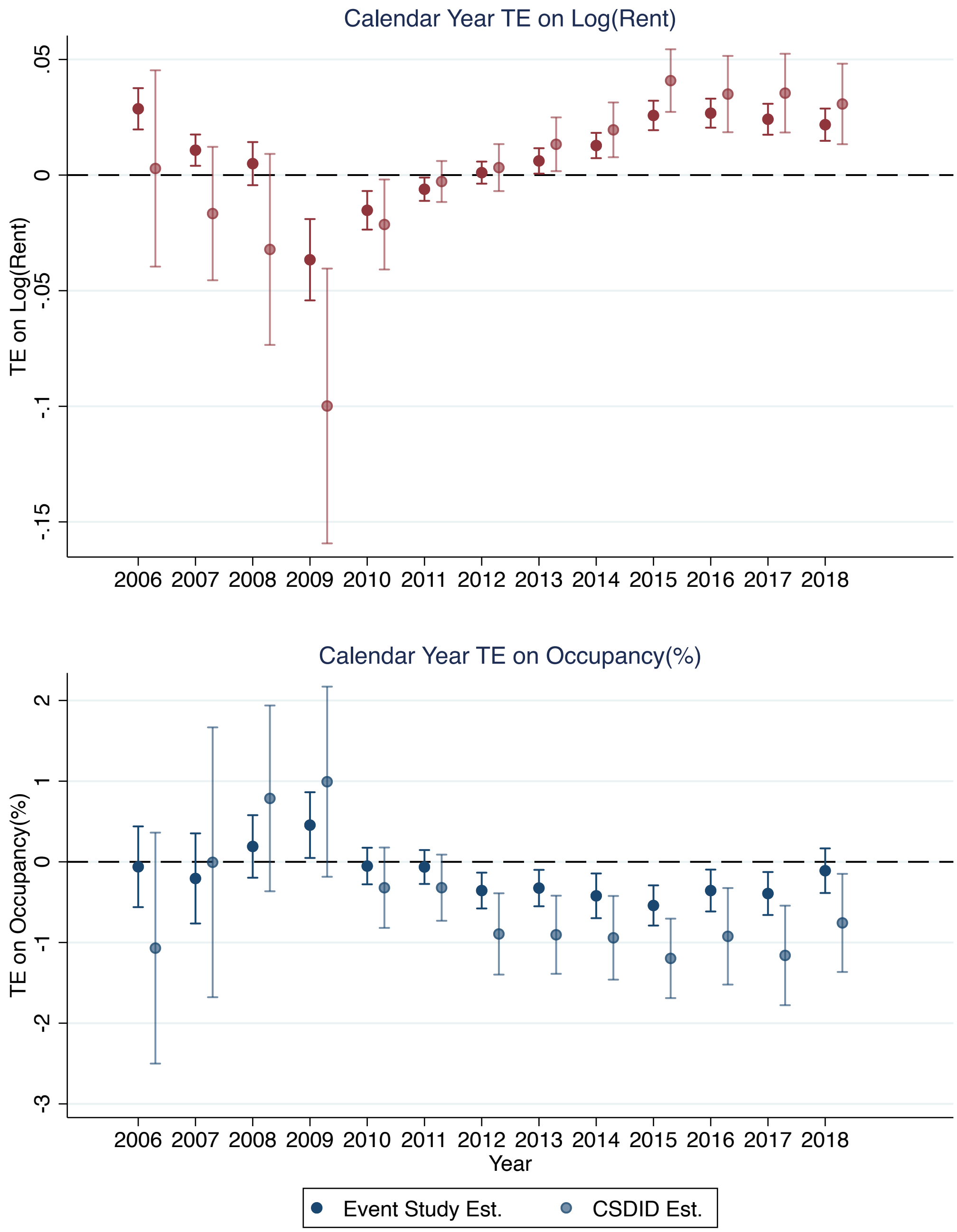
(a) 2007 Adopters

(b) 2013 Adopters



Sample restricted to buildings built before 2005. Building-level and time trend (year) fixed effects for the building's metro and pre-treatment period rent quartile are included. Controls include months of free rent offered, average concession offered in the submarket. Standard errors are clustered at the management company level.

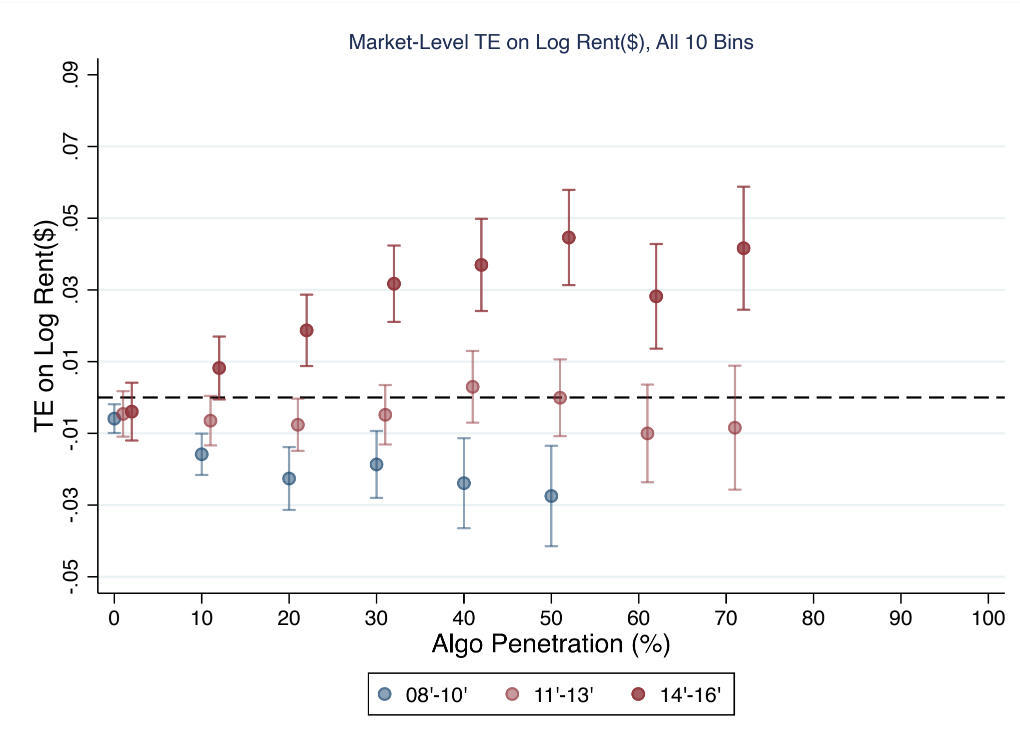
Figure 7: Calendar Year TE on Adopted Buildings



Sample restricted to buildings built before 2005. Building-level and time trend (year) fixed effects for the building's metro and pre-treatment period rent quartile are included. Controls include months of free rent offered, average concession offered in the submarket. For the Callaway and Sant'Anna (2020) specification (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 Penetration, Submarket-Rent Quartile

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



(b) $Y = Occ$

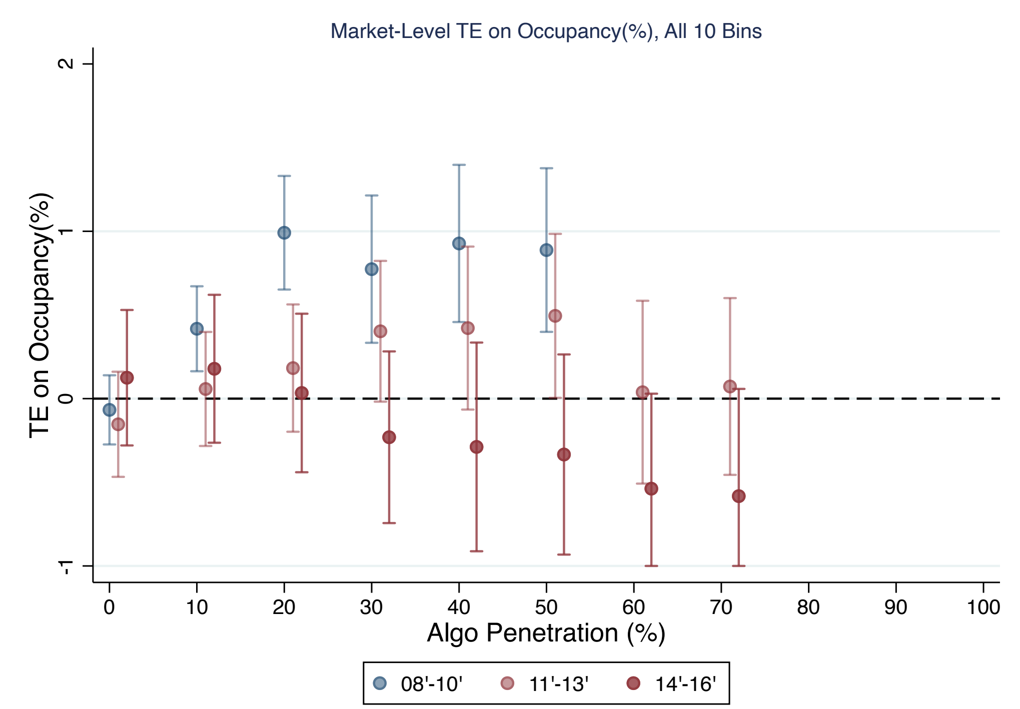
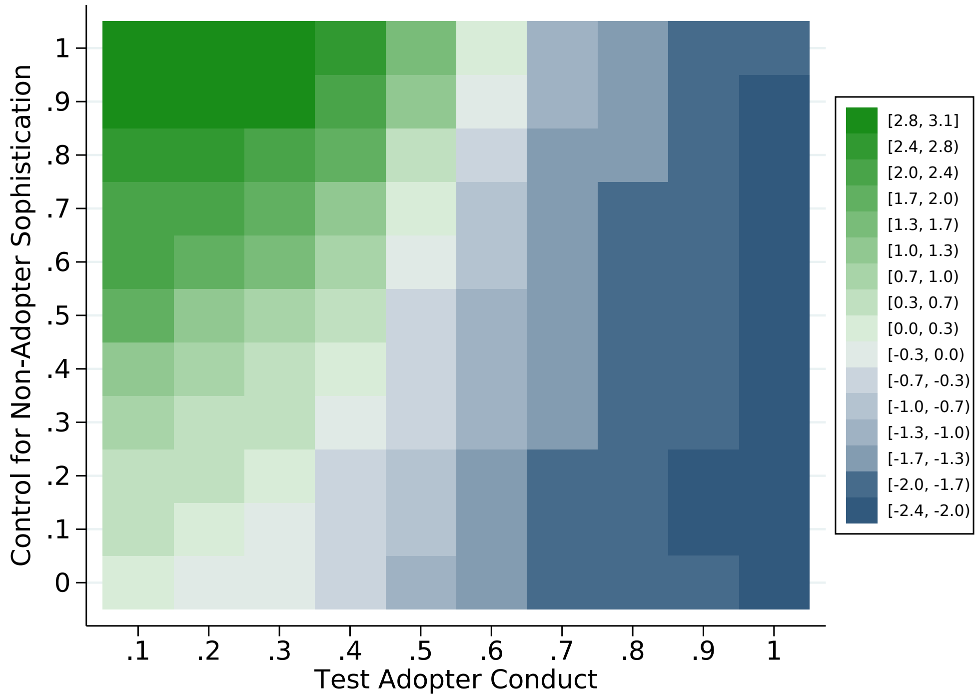


Figure 9: Test for Coordinated Pricing, Sieve 2SLS



Notes: The result of the RV test comparing a model of coordination at level τ^A and a model of own-profit-maximization, controlling for the level of non-adopter pricing sophistication. At $\tau^{NA} = 1$, non-adopters are sophisticated and maximize their own profit by charging the full markup. At $\tau^{NA} = 0$, non-adopters do not charge any markup and are essentially marginal-cost pricers. The color of the figure indicates the sign and the depth of the color indicates its statistical significance. A shade of green indicates a positive test statistics, whereby a model of coordination is favored over a model of own-profit-maximization. A shade of blue indicates a negative test statistic, whereby a model of own-profit-maximization is favored over a model of coordination. The standard errors are computed based on 1000 draws of the bootstrap whereby we redraw the management companies in the market.

9 Tables

Table 1: REIS Summary Statistics

Avg. Asking Rent($)	1373.9
	(825.8)
Occupancy Rate(%)	93.15
	(7.807)
Avg. Units	193.9
	(168.4)
$N_{building}$	37,216
$N_{company}$	11,523
N_{state}	30
N_{metro}	50
N_{submkt}	663

Table 2: Top Multifamily Building Management Companies

Company	Units Managed	Adoption	Adoption Date	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: NMHC Ranking from https://www.nmhc.org/research-insight/the-nmhc-50/top-50-lists/2019-managers-list/

Table 3: Distributions of Markets by Penetration of Adopters

Year	0%	0-10%	-20%	-30%	-40%	-50%	-60%	-70%	-80%	-90%	-100%	Total
2005	2,506	0	0	0	0	0	0	0	0	0	0	2,506
2006	2,289	104	69	34	5	2	3	1	0	0	0	2,507
2007	2,108	142	135	83	20	9	8	2	0	0	0	2,507
2008	1,864	196	188	144	48	27	27	7	3	3	1	2,508
2009	1,627	256	266	197	76	33	37	8	3	3	2	2,508
2010	1,225	276	368	267	121	103	84	44	8	6	5	2,507
2011	896	277	388	326	198	128	140	85	25	27	18	2,508
2012	846	259	415	340	197	127	154	86	34	25	24	2,507
2013	799	258	413	345	198	155	164	94	28	32	22	2,508
2014	798	250	370	350	208	180	168	88	41	33	22	2,508
2015	771	256	377	322	229	146	172	123	56	34	22	2,508
2016	730	231	394	311	239	168	190	134	50	38	22	2,507
2017	684	244	385	324	247	195	196	115	53	40	25	2,508
2018	634	228	397	346	270	184	188	143	58	34	26	2,508
2019	596	252	371	374	271	177	216	147	53	33	18	2,508

The market definition used is submarket, rent quartile pair.

Table 4: 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)
$p^A - p^{NA}$	$-$	$+$	0
$Q^A - Q^{NA}$	$+$	$-$	$-$

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. 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 5: Building characteristics comparison between 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)
$N_{building}$	27,991	9,225
Shr$_{building}$	75.2%	24.8%
N_{unit}	4,774,640	2,441,191
Shr$_{unit}$	66.2%	33.8%

Table 6: Building-level Calendar Year TE of Pricing Software Adoption

	TWFE		2SLS		CSDID	
Year	Log(Ask Rent)	Occ(%)	Log(Ask Rent)	Occ(%)	Log(Ask 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
Submkt-Tier-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

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Sample restricted to buildings built before 2005. Building-level and time trend (year) fixed effects for the building's metro and pre-treatment period rent quartile are included. Controls include months of free rent offered, average concession offered in the submarket. For the Callaway and Sant'Anna (2020) specification (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.

Table 7: 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
Submkt-Class FE	Y	Y	Y	Y
F-stat		1108.59		1108.59
N_{mt}	17441	17441	17441	17441

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Sample restricted to buildings built before 2005. Controls include avg. number of floors, building age, fraction of buildings with pool, doorman, tennis court, parking garage and clubhouse in the submarket, as well as submarket-level level and changes of house price index, unemployment rate, income, and net migration. All regressions are weighted by number of buildings in each market. The reported F-stat is Kleinberg-Paap rk Wald F Statistics from the 2SLS regression of outcome on AlgoShare intrumented with the IV but without the year-dummy interaction terms. Standard errors are clustered at the submarket-class level for all specifications.

Table 8: Estimation Sample Statistics, Seattle 2011-2018

Panel A: Price and Quantity	
Eff. Rent (100$)	14.81
	(6.294)
Occupancy (%)	94.75
	(9.203)
Panel B: ACS Moments	
Outside Good Share s_0	0.805
$Cov(income_i, rent_j)$	11.79
$Cov(hs_i, bed_j)$	0.163
Panel C: Geographical Markets	
N_{submkt}	16
N_{tract}	319
Panel D: Concentration	
N_{bld}	1163
N_{owner}	1012
HHI	376.9
$N_{bld}^{matched}$	876
$N_{owner}^{matched}$	607
$HHI^{matched}$	562.5
Frac. j by Adopters	0.430

Table 9: Estimated Demand Parameters, Seattle, 2011-2018

	Tax IV	Optimal IV
$\alpha : y_i < \$20,000$	9.363	8.352
	(3.678)	(2.586)
$\alpha : \$20,000 \leq y_i < \$35,000$	7.274	7.428
	(1.4)	(1.286)
$\alpha : \$35,000 \leq y_i < \$50,000$	9.954	7.627
	(1.875)	(1.255)
$\alpha : \$50,000 \leq y_i < \$75,000$	9.673	7.888
	(2.218)	(1.454)
$\alpha : y_i > \$75,000$	15.175	10.635
	(3.645)	(2.013)
	HH Size	HH Size
Inside Good	-5.168	-2.996
	(0.607)	(0.194)
Bed	1.959	0.28
	(0.392)	(0.094)
N_{mt}	128	128
N_{bld}	1,163	1,163
N_j	3,065	3,065
N_{jt}	18,638	18,638
Unit-type FE	Y	Y
Submkt-year FE	Y	Y
Median Own Elas.	-3.032	-2.521
Median Agg Elas.	-0.584	-0.478
Mean Outside Good Div.	0.57	0.548

Table 10: Test for Coordination, Seattle 11-18

$\tau^A = 0$	.1	.2	.3	.4	.5	.6	.7	.8	.9	1
$\tau^{NA} = 1$	3.08	3.00	2.92	2.64	1.52	0.00	-1.05	-1.53	-1.75	-1.91
$\tau^{NA} = .9$	2.85	2.81	2.88	2.19	1.04	-0.28	-1.16	-1.59	-1.82	-1.96
$\tau^{NA} = .8$	2.56	2.58	2.35	1.67	0.65	-0.52	-1.31	-1.64	-1.86	-2.00
$\tau^{NA} = .7$	2.25	2.12	1.84	1.18	0.23	-0.73	-1.38	-1.71	-1.87	-2.01
$\tau^{NA} = .6$	1.97	1.77	1.32	0.73	-0.09	-0.87	-1.45	-1.76	-1.92	-2.04
$\tau^{NA} = .5$	1.76	1.23	0.93	0.39	-0.31	-1.07	-1.54	-1.80	-1.92	-2.06
$\tau^{NA} = .4$	1.20	0.93	0.57	0.08	-0.51	-1.17	-1.58	-1.85	-1.94	-2.05
$\tau^{NA} = .3$	0.85	0.54	0.33	-0.12	-0.68	-1.22	-1.61	-1.83	-1.96	-2.04
$\tau^{NA} = .2$	0.63	0.36	0.10	-0.33	-0.83	-1.33	-1.67	-1.86	-1.98	-2.06
$\tau^{NA} = .1$	0.39	0.15	-0.11	-0.49	-0.93	-1.36	-1.71	-1.90	-1.99	-2.02
$\tau^{NA} = 0$	0.22	-0.00	-0.27	-0.60	-1.02	-1.42	-1.73	-1.83	-1.90	-2.03

The result of the RV test comparing a model of coordination at level τ^A and a model of own-profit-maximization, controlling for the level of non-adopter pricing sophistication. At $\tau^{NA} = 1$, non-adopters are sophisticated and maximize their own profit by charging the full markup. At $\tau^{NA} = 0$, non-adopters do not charge any markup and are essentially marginal-cost pricers. The standard errors are computed based on 1000 draws of the bootstrap whereby we redraw the management companies in the market.

A Appendix Figures

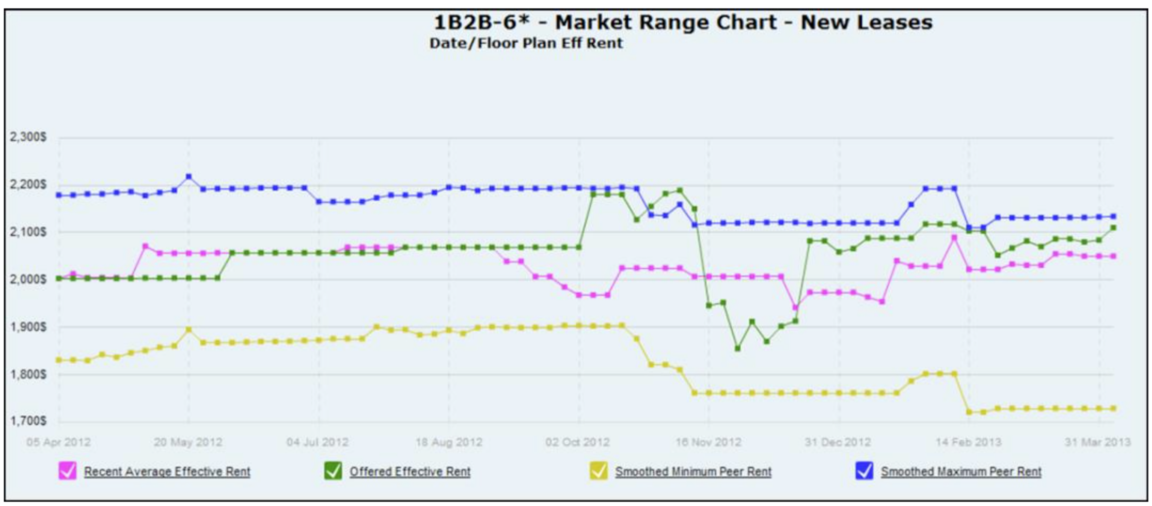
Appendix Figure A1: How Yieldstar optimizes rents

Bedroom Level Pricing

How the tool utilizes the competitive data:

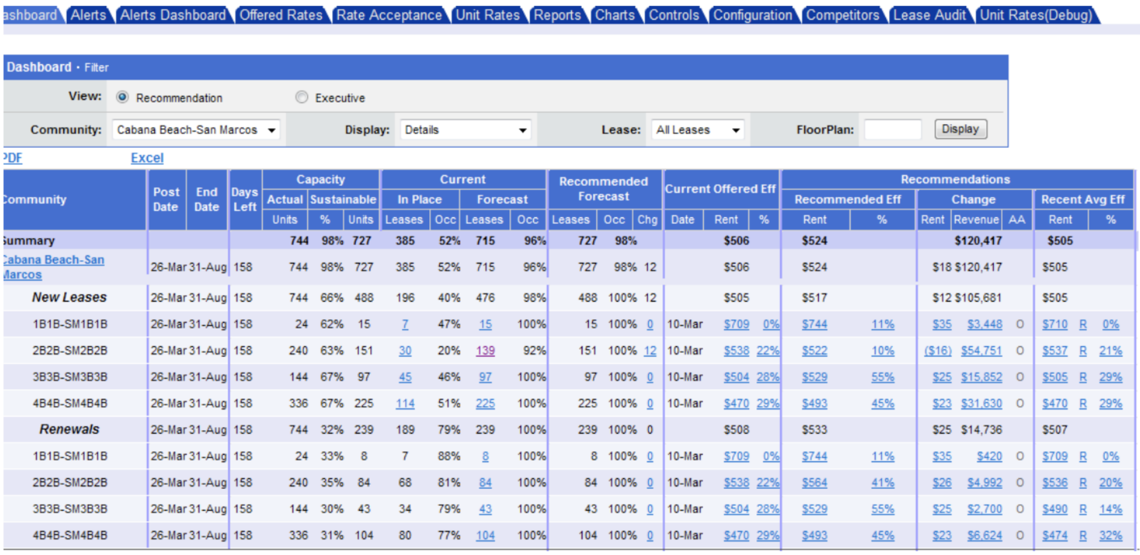
- Starts with your market survey, Operations approves the comps
- Dynamically calibrates elasticity for each bedroom type by:
    - Reading each lease and lease application for your asset
    - Determining the effective rent (net of all appropriate concessions)
    - Comparing the effective rent you achieve to the top and bottom of the competitive range for your selected competitors. Of note, the top and the bottom is a blending of multiple unit types to protect against “bad data”
    - The tool assigned a price position for each lease and aggregates to form a elasticity curve to truly define the price/demand relationship

Appendix Figure A2: Manager’s view of dynamic pricing by Yieldstar



Appendix Figure A3: Manager's view of Yieldstar pricing dashboard

(a) Price recommendation made by Yieldstar



ashboard | Alerts | Alerts Dashboard | Offered Rates | Rate Acceptance | Unit Rates | Reports | Charts | Controls | Configuration | Competitors | Lease Audit | Unit Rates(Debug)

Dashboard • Filter

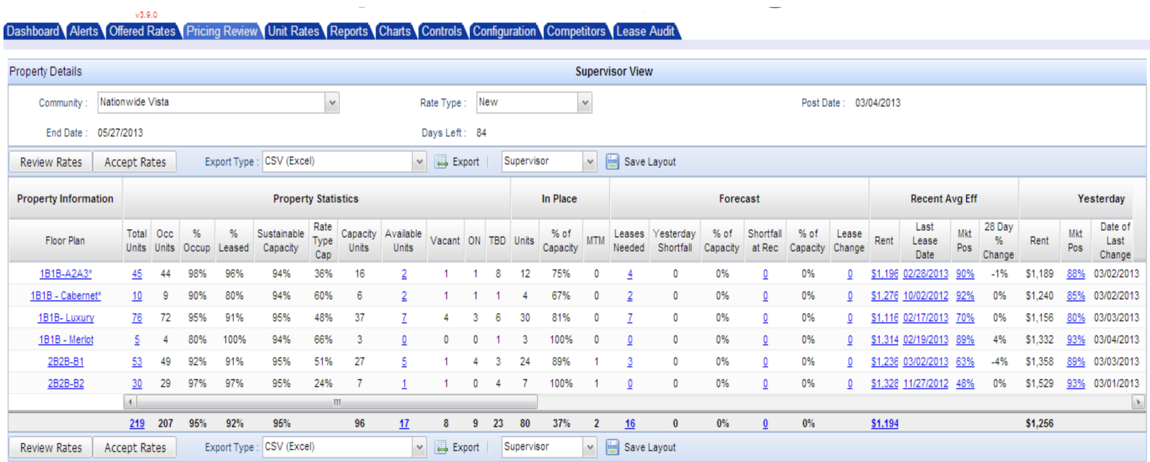
View: Recommendation Executive

Community: Cabana Beach-San Marcos Display: Details Lease: All Leases FloorPlan: Display

?DF Excel

ommunity	Post Date	End Date	Days Left	Capacity			Current				Recommended Forecast			Current Offered Eff			Recommendations							
				Actual	Sustainable		In Place		Forecast								Recommended Eff		Change			Recent Avg Eff		
				Units	%	Units	Leases	Occ	Leases	Occ	Leases	Occ	Chg	Date	Rent	%	Rent	%	Rent	Revenue	AA	Rent		%
ummary				744	98%	727	385	52%	715	96%	727	98%			$506		$524			$120,417		$505		
abana Beach-San larcos	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



v3.9.0

Dashboard | Alerts | Offered Rates | Pricing Review | Unit Rates | Reports | Charts | Controls | Configuration | Competitors | Lease Audit

Property Details Supervisor View

Community : Nationwide Vista Rate Type : New Post Date : 03/04/2013

End Date : 05/27/2013 Days Left : 84

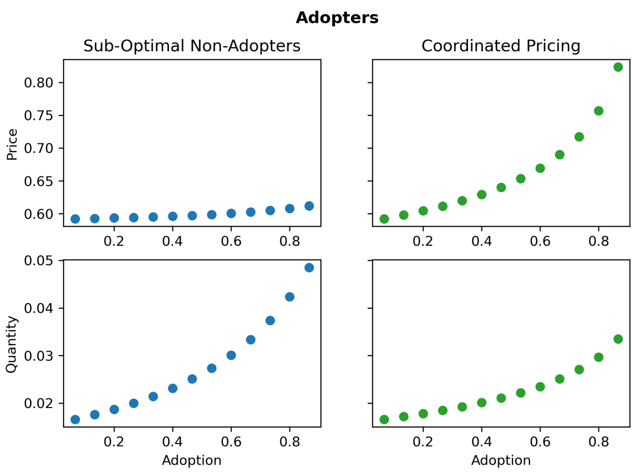
Review Rates Accept Rates Export Type : CSV (Excel) Export Supervisor Save Layout

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	$1,328	11/27/2012	48%	0%	$1,529	93%	03/01/2013
	219	207	95%	92%	95%		96	17	8	9	23	80	37%	2	16	0	0%	0	0%		$1,194				$1,256		

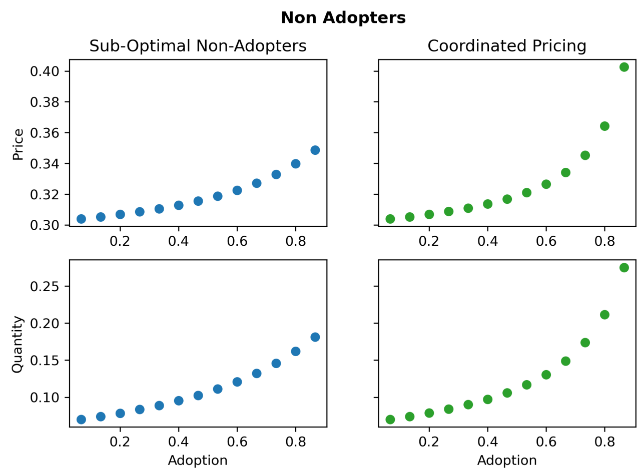
Review Rates Accept Rates Export Type : CSV (Excel) Export Supervisor Save Layout

Appendix Figure A4: Model Predictions of Different Pricing Paradigms with Differentiated Products

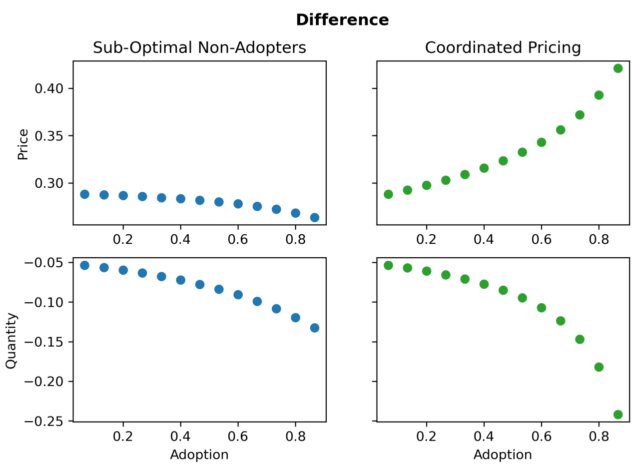
(a)



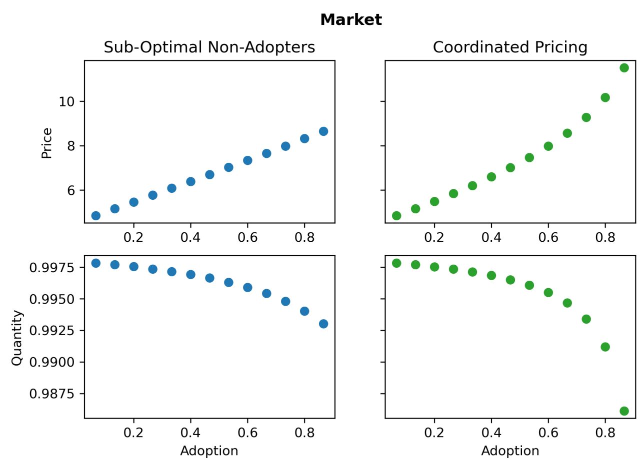
(b)



(c)

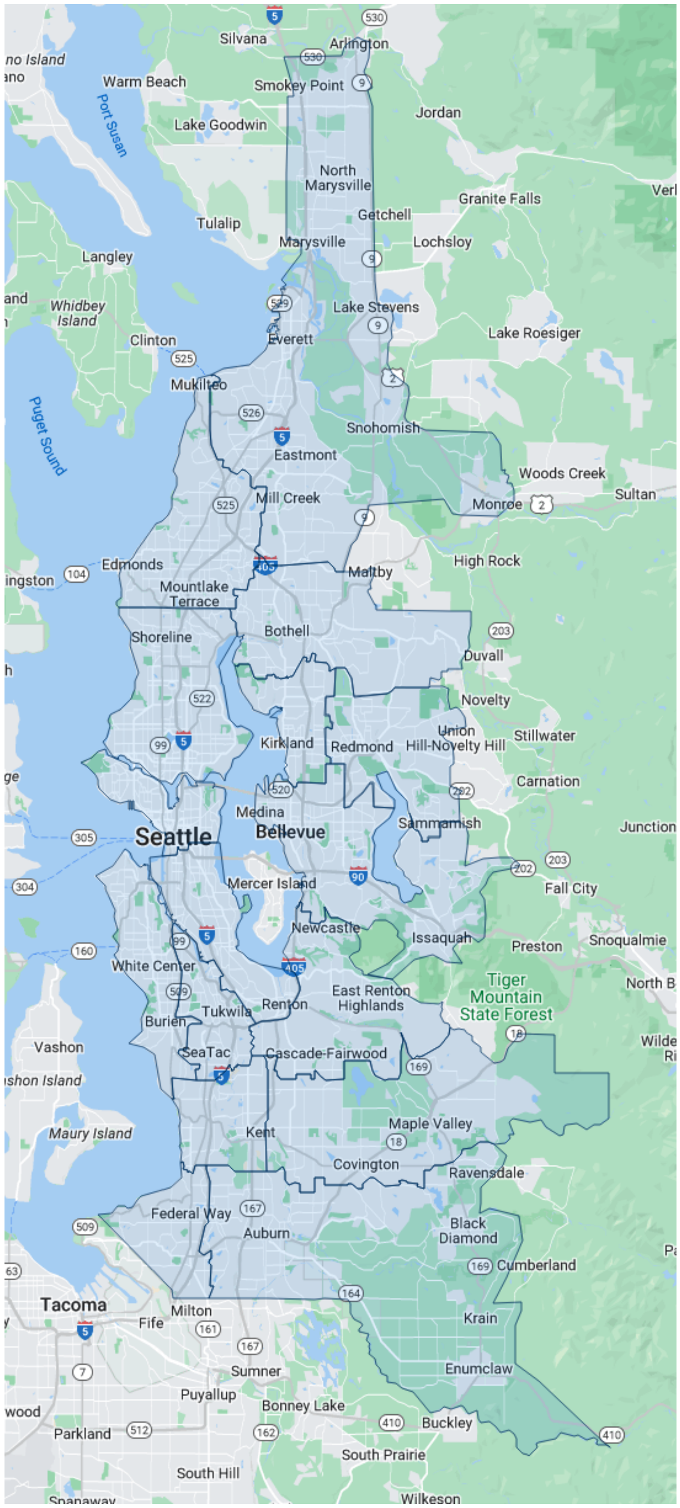


(d)



Notes: Simulation based on a stylized model of differentiated product markets. It evaluates the predictions of two pricing paradigms. On the left, it plots the predictions by penetration when non-adopters is not charging their full markup, and the effects of the software adoption allows adopters to charge their full markup. On the right, it plots the predictions by penetration when adopters are coordinating on price via adopting the same pricing algorithm. Across all moments for adopters, non-adopters, differences, and market aggregates, they generate the same sign for the comparative statics. The only exception is the price difference, but the sign difference is a function of the marginal cost function. As such, it remains impossible to find evidence of coordination by reduced-form analysis alone.

Appendix Figure A5: REIS Defined Submarket, Seattle



B Appendix Tables

Appendix Table A1: 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 A2: 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