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

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Abstract

Algorithmic pricing can improve efficiency by helping firms set prices that are 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 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. Lastly, we estimate a structural model of housing demand and perform a test of conduct to evaluate the "algorithmic coordination" hypothesis. Our pair-wise tests favor joint profit maximization among adopters of the same software, suggesting considerable markup increases due to the coordination channel on the order of $53 per unit per month, affecting over 2.6 million units. However, our tests do not find support for coordinated prices between different algorithms. Our findings have implications for regulators and policymakers concerned about potential risks and tradeoffs associated with algorithmic pricing.

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1 Introduction

The introduction of computing technology has brought about significant changes in how firms set prices, especially towards the use of software that sets 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 (Kühn and Tadelis, 2018; Calvano, Calzolari, Denicolò and Pastorello, 2020; Asker, Fershtman and Pakes, 2022; Harrington, 2022), policymakers (OECD, 2017), and antitrust agencies (Mcsweeny and O'Dea, 2017; UK Competition and Markets Authority, 2022; Canadian Competition Bureau, 2022).

The multifamily housing industry in the United States is currently facing intense scrutiny regarding the issue of algorithmic pricing. Since 2022, over twenty class action lawsuits have been filed against RealPage, the software company, as well as landlords utilizing the software, alleging that the pricing software is responsible for raising prices "above competitive levels" and a violation of Section 1 of the Sherman Act.[1] This issue has garnered significant media attention[2] and has also captured the interest of regulators, with the U.S. Department of Justice reportedly launching an investigation into the potential coordination of prices among landlords utilizing the software.[3]

In this paper, we empirically evaluate the impact of algorithmic pricing on the U.S. multifamily 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 coordinate their prices, thus allowing them to maximize profits jointly as opposed to competing with each other.[4]

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 cannot isolate the coordination channel from the responsive pricing channel. As a result, it becomes difficult to distinguish these two models with reduced-form regressions of price or quantity on adoption. In other words, merely

[1] https://www.tnmd.uscourts.gov/mdl-case-information, accessed July 16, 2024.

[2] https://www.propublica.org/article/yieldstar-rent-increase-realpage-rent, accessed July 16, 2024.

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

[4] Note that when we use the word "coordination," we simply refer to whether implied markups based on an appropriate demand system are consistent with a model of joint profit maximization in a positive sense. We are not detecting the "act" of coordination, nor are we assessing them against the legal definition of price fixing in this paper.

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 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 multifamily 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 social media profiles of management companies. We then merge the adoption dates with a comprehensive dataset of rental information from Moody's REIS (Real Estate Information Services), a data provider for commercial real estate market information. 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 have tagged all of the top 20 management companies by units under management as adopters of algorithmic 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 submarket 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 metros where 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 do not produce sufficient evidence for price coordination.

Next, because prices are strategic complements, the impact of algorithmic pricing is restricted not only to adopters but also affects 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 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 increases from 2014 to 2016. In terms of magnitude, the market average rent of a fully penetrated market is 3.0% higher than that of 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 an aggregated 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 multifamily rental market to perform a formal test of adopter conduct. Importantly, the structural approach allows us to isolate the coordination channel from other channels of pricing change by imposing a structural relationship on how markups among adopters should be determined by a given conduct assumption together with a demand system.

To that end, we estimate a full structural model of rental demand based on a discrete choice random utility model across ten cities. We match on aggregate shares in the REIS data and match on moments from American Community Survey micro-data to capture the rich heterogeneity between household demographics and housing attributes. The estimation procedure follows Petrin (2002) and Conlon and Gortmaker (2020, 2023). The model produces sensible estimates of own-product and aggregate demand elasticities.

Then, a test of conduct amounts to testing the conditional moment restrictions where the marginal cost shocks are conditionally independent of a set of excluded instruments under the correct model of conduct (Nevo, 2001; Berry and Haile, 2014; Miller and Weinberg, 2017). Operationally, we adopt a pair-wise testing framework (Backus, Conlon and Sinkinson, 2021; Rivers and Vuong, 2002) to evaluate whether a model of coordination is more or less favored than a model of competition, given its desirable properties against model misspecifications (Magnolfi and Sullivan, 2022; Duarte, Magnolfi, Sølvsten and Sullivan, 2023). Specifically, because RealPage acquired LRO in 2017, we test the conduct of the adopters of each respective software separately and also for the two time periods separately.

Overall, we find that the markup implied by our demand model favors a model of joint-profit-maximization over own-profit-maximization among both adopters of RealPage and adopters of LRO prior to acquisition. The results are statistically significant and also robust to more conservative measures of cross-building ownership. For the merged entity, even though the test against full coordination is statistically insignificant, some intermediate level of the internalization parameter is still favored over full competition. Moreover, when conducting another pair-wise test for possible coordination between RealPage and LRO prior to the acquisition, the test does not find statistically significant evidence of coordination between different software.

Lastly, we perform several back-of-the-envelope analyses to evaluate the impact of algo pricing. If we assume that merged RealPage would eventually be able to jointly profit maximize among its adopters, then the average markup would be $53.1 higher per month per unit due to the coordination channel.

Since there are about 600,000 adopted units in our estimation sample, and over 2.6 million adopted units in our full fifty city sample, the aggregated impact could be considerable.

Our paper's main contribution is to empirically evaluate the impact of algorithmic pricing in a high-stakes context with material welfare implications. Moreover, we estimate a full structural model of demand and supply, which allows us to isolate a test of conduct from other aspects of pricing changes. While there is a growing theoretical literature on the issue of algorithmic collusion (Calvano et al., 2020; Banchio and Mantegazza, 2023), empirical work remains rather thin. To our knowledge, the most related effort to test for algorithmic collusion is Assad, Clark, Ershov and Xu (2024), which we complement and extend their work in several important ways. First, we have rich data on both price and quantity, thus allowing us to build a structural model of underlying demand. Moreover, instead of inferring adoption status, we have hand-collected data on both the adoption status and the identity of the algorithm provider, allowing for separate tests for coordination within and between different software. In addition, we devise and implement a test of conduct that could isolate the coordination channel from other possible channels in which the adoption of algorithms may impact pricing.

Indeed, 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. Theoretical literature has explored channels such as better information on the demand variations and thus set more responsive prices (Miklós-Thal and Tucker, 2019; Harrington, 2022). Empirical analyses have found evidence that algorithmic pricing has allowed some firms to set prices at higher frequencies than their competitors (Brown and MacKay, 2023) or to incorporate market information better (Leisten, 2022), but these studies do not address concerns about collusion explicitly. As such, our paper is also 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 naive reduced-form estimates may not be able to isolate the coordination channel because it may produce observationally identical predictions as other concurrent channels.

Lastly, our paper contributes to the literature on understanding the impact of technological innovation on the housing market. The size of the housing market is colossal: At least $100 billion of rent payments are made annually in our dataset alone, representing a sector that is about $3.8 trillion in asset size.[5] Consequently, even a minor percentage impact within this industry translates into substantial value differences. At the household level, rent payments are often the biggest share of household expenditure among renters. While technological innovations often promise to bring about efficiency, the net impact is likely more complex due to various market frictions (Buchak, Matvos, Piskorski and Seru, 2022; Fu, Jin and Liu, 2023; Raymond, 2024).

[5] https://www.reit.com/data-research/research/nareit-research/estimating-size-commercial-real-estate-market-us-202 accessed July 16, 2024.

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.[6] 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.[7]

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,[8] 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.[9] 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.[10] The "rent optimization" solution is an automated pricing algorithm that suggests rents in real-time by unit type and lease lengths to property

[6] https://www.nmhc.org/research-insight/quick-facts-figures/quick-facts-investment-returns-on-apartments
[7] https://www.nmhc.org/research-insight/quick-facts-figures/quick-facts-market-conditions
[8] https://www.propublica.org/article/when-private-equity-becomes-your-landlord
[9] https://www.nmhc.org/research-insight/the-nmhc-50/top-50-lists/2022-top-managers-list/
[10] Yardi RentMAXImizer, RealPage Yieldstar, and Rainmaker LRO

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.[11] 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.[12] 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.[13] 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.

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."[14]

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.

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

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

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

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

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.[15] Given that there were about 1.5 million new units constructed from 2019 and 2021,[16] 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

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

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

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. (2024). However, this is not a major concern for us because we were able to collect a reasonably confident data set of management companies who adopted 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 manual Google searches. In selected instances, management companies and their related social media sites posts information about the status of their adoption.

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.[17] 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.[18] However, the presence of measurement error in the adoption decisions likely leads to attenuation bias in our estimates.

2.2.3 Additional Data Sources

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

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

[18] 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.

Real Capital Analytics (RCA) We obtain a panel of ownership 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.

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

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

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.[19] 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 A1), 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.

[19] 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,[20] 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 changing market conditions quickly, hanging on to the price from the previous period $p_1^{NA} = p_0$.[21] Adopters, through the usage of the software, are *"alert"*, where they adjust their prices responsively to changing market conditions.

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

[21] 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.[22] 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

[22] 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,[23] 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

[23] 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 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.[24]

   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.

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).[25]

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

[24] 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.

[25] 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 various possible predictions on adopters, non-adopters, and their differences by the level of penetration, as long as we allow for some degree of product differentiation where the non-adopters are not fully pricing its optimal markup, one still cannot differentiate a model of coordinated pricing from a model of sub-optimal pricing on the part of non-adopters.

impact of adoption 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 chosse a cohort of buildings that adopted the software before the financial crisis and another cohort that adopted the software after the crisis. The outcome of interest is $y_{jt} \in \{\log(rent_{jt}), occ_{jt}\}$, 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,[26] 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.[27] 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 in which 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:

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

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.

[26] 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.

[27] 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.[28] 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.[29] We then regress:

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

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

[28] 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.

[29] 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:

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

where N_{mt} denotes the number of buildings in market m in year t, and $Adopt^{IV}_{c(j),Metro,t}$ 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:

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

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

The empirical findings from Section 4 echo the intuitions from our stylized model presented in Section 3 and support that there are indeed two main channels through which the algorithm can raise rents and reduce occupancy. However, as noted, the reduced-form evidence alone cannot disentangle the two mechanisms of interest. In this section, we complement our descriptive findings by formally testing the pricing behaviors of adopters, which is made possible because the structural model can flexibly

incorporate the entire matrix of demand elasticities, thus allowing us to directly test for conduct.

5.1 Demand Model

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

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

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

where y_i is the household's income, p_{jt} is the effective monthly rent of the unit, which we interchangeably call "price", $\boldsymbol{X_{jt}}$ is a row vector of 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 price coefficient that varies at the household level. 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 submarket m in year t. The probability of renter i choosing to live in $j \in \mathcal{J}_{mt} \cup \{0\}$ is

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

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 $\xi_{jt} = \xi_j + \tilde{\xi}_{jt}$, where ξ_j is unit type-level fixed effects, capturing the time-invariant vertical quality of a building and its unit types. For the part of the renter utility that allows for heterogeneity in taste, we rewrite $\alpha_i = \alpha_0 + \alpha_y \log(y_i)$ so that price elasticity may vary with household income. Therefore, the estimation of these parameters is achieved by two sources of variations: (1) the observed market shares of unit types and their prices and (2) the joint distribution of building characteristics and renter demographics across markets.

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) based on building-level price and occupancy 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:

$$\hat{G}_1(\theta^D) = \frac{1}{N_{jt}} \sum_{j,t} \hat{\tilde{\xi}}_{jt}(\theta^D) z^D_{jt}. \tag{5.3}$$

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

To address such endogeneity, we use the average local property tax rate in the census tract, excluding the focal building, as an instrument for rent. While property tax may not fit the strictest definition of marginal cost, it is documented in the literature that landlords pass some of the property taxes to renters (Watson and Ziv, 2021). The validity of tax instruments relies on the assumption that property tax rates are set by local authorities *before* the realization of the contemporaneous demand shock, whereas building managers set prices after observing the tax obligations, along with other contemporaneous demand shocks. Moreover, to the extent that the focal building might take actions to lower its tax obligations by negotiating its assessed value, it is less likely that it could lower its tax rate for a given assessment. Still, to the extent that they could still affect tax rates that may be endogenous to its own unobserved demand, we take the more conservative approach to compute the average local property tax rate with the focal building excluded. We believe that once the tax rates are residualized by the product fixed effects, the remaining variation reflects idiosyncratic timings of changes in the local tax regime.

The second set of moments are "micro-moments" on the joint distribution of building characteristics and renter demographics. We use the micro-data from the American Community Survey (ACS) 1-year Estimates to construct the covariances between various household demographics and the housing characteristics chosen by these renters. While ACS provides extremely rich information about the correlation between household demographics and housing attributes, because it is anonymized at the Public Use Micro Area (PUMA) level, we do not directly observe which particular apartment a household lives in. As a result, we match the micro-moments from the model prediction on REIS apartment buildings with the covariances in the subsample of ACS that shares the same support of characteristics with REIS. Effectively, it amounts to matching moments within a subsample of ACS households who live in large multi-family buildings, defined as structures with more than 50 units, with the same support of bedroom count and rent distributions. Furthermore, we augment the demand estimation using moments from Data Axle on housing attributes that are not part of standard ACS,

namely, the designation of Class A buildings. Specifically, we also match moments that compare the ratio of household income between those who choose Class A buildings vs. Class B/C buildings based on information in Data Axle. These moments further help us pin down heterogeneous taste parameters for the number of bedrooms, building age, building type, and building class by various household characteristics, including income, household age, household size, and the presence of children.

The difference between 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 household size (hs) and bedroom count (bed) for renters in market mt is

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

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

By stacking the set of exclusion restriction moments $G_1(\theta)$ and micro-moments $G_2(\theta)$, the estimation procedure finds a 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 a set of ten cities from 2009 to 2019, including Atlanta, Charlotte, Dallas, DC, Houston, Los Angeles, Minneapolis, Portland, San Diego, and Seattle.

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

34% of the units in our estimation sample were adopters at the end of 2019, which is similar to the overall 33% for the full fifty-city REIS data.

Table 8 summarizes key moments from the demand estimation. Panel (A) contains key statistics that relate to market share and prices, which include 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. As shown in Panel (C), there are ten metros covering 203 REIS-submarkets, providing us with potentially many different types of markets with varying levels of algorithmic pricing penetration.

Table 9 summarizes our estiamted demand parameters using `pyblp` (Conlon and Gortmaker, 2020, 2023), and we use property tax rate as the demand instrument. Our estimates show that renters with higher incomes are less price sensitive. We also see households with greater household size and with the presence of children deriving higher utility from units with more bedrooms, as implied by a positive, significant coefficient on household size-bed interaction term. We find those with higher income and younger prefer newer buildings. We find that those with higher incomes also prefer Class A buildings. Lastly, renters who choose buildings in REIS tend to be households that are smaller in size, less likely to have children, and younger in age. This also makes sense given that REIS buildings tend to be multifamily apartments mostly comprised of studios, one-bedroom, and two-bedroom units, averaging 1.5 bedrooms per unit. By contrast, non-REIS dwellings in the ACS have more bedrooms, averaging 1.8 bedrooms per unit. In addition, because we allow for the price to also include a mean linear coefficient α_0, all the moments are matched perfectly by construction, which we also verify empirically.

We close the section on renter demand for housing by discussing estimated elasticities. The median own-elasticity of buildings is around -4.79, which implies that each building faces elastic residual demand. When aggregated up to REIS buildings, they face inelastic aggregate demand with elasticity of -1.93, again largely in line with the existing literature (Chen et al., 2011; Albouy et al., 2016). 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 0.78, suggesting that on average, for every household leaving a REIS building, 78% of them will choose to live in a non-REIS building and 22% will choose another apartment building in REIS. Overall, our demand-side estimation shows that while individual buildings face elastic demand, they collectively face more inelastic aggregate demand and face some competitive pressure from other landlords outside of REIS apartment buildings.

6 Supply and Testing for Conduct

6.1 Supply Model

The workhorse model for market equilibrium in differential product markets is Nash-Bertrand. Namely, each building owner f will set the price of its products j across all unit types in all buildings that it

owns to maximize its total profit.

We add two elements to the model to account for the potential impact of algorithmic pricing. First, to allow for conduct tests against potential coordination among adopters, following Backus et al. (2021), we let a firm's profit function include a portion τ^A of its fellow adopters' profits, which allows for partial internalization. A value of $\tau^A = 0$ corresponds to no coordination, and a value of $\tau^A = 1$ corresponds to full coordination[30] Thus, the overall profit function of owner f is as follows

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

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

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

Second, to capture the notion that non-adopters may depart from the Nash-Bertrand benchmark due to various sources of frictions, we model it flexibly by allowing for a time-varying τ_t^{NA} applied to the markup term. A value of $\tau_t^{NA} = 1$ suggests that non-adopters are fully "sophisticated" by charging the optimal Nash-Bertrand markup. A value greater than one would suggest their prices are too high, and a value lower than one would suggest their prices are too low. For clarity, we index non-adopter firms by l, and its first-order condition becomes

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

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

$$\boldsymbol{p_t} - \boldsymbol{mc_t} = \underbrace{-\left(\mathcal{H}_t(\tau^A, \tau_t^{NA}; \boldsymbol{D}(p)) \odot \boldsymbol{\Omega}_t(\boldsymbol{p})\right)^{-1} \boldsymbol{D}_t(\boldsymbol{p})}_{\boldsymbol{\eta}_t(\mathcal{H}_t(\tau^A, \tau^{NA}))} \tag{6.2}$$

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

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

[30] As pointed out in Backus et al. (2021), while intermediate values of τ^A are not "necessarily interpretable," it does represent partial internalization of competitor's profits.

it assumes the following block-diagonal form:

$$\mathcal{H}_t(\tau^A, \tau^{NA}) = \begin{bmatrix} \mathcal{H}_t^A(\tau^A) & 0 \\ 0 & \mathcal{H}_t^A(\tau_t^{NA}) \end{bmatrix} \tag{6.3}$$

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

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

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

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

Across adopters $j \in A_t$ and non-adopters $k \notin A_t$, $\mathcal{H}_{jkt} = 0$. The block-diagonal shape of $\mathcal{H}_t(\tau^A, \tau_t^{NA})$ arises naturally if we index all the adopters first and then index all the non-adopters. Economically, it requires that the pricing algorithm employed does not facilitate the collusion between adopters and non-adopters.[31] Since the pricing algorithm does not recommend prices to non-adopters, we believe this is a reasonable assumption.

As a result, since the inverse of a block diagonal matrix is also block diagonal, we can re-write the first-order condition from Eq 6.2 separately for adopters and non-adopters, stacked as follows

$$\begin{bmatrix} \boldsymbol{p}_t^A - \boldsymbol{mc}_t^A \\ \boldsymbol{p}_t^{NA} - \boldsymbol{mc}_t^{NA} \end{bmatrix} = \begin{bmatrix} -\left(\mathcal{H}_t(\tau^A) \odot \boldsymbol{\Omega}_t^A(\boldsymbol{p})\right)^{-1} \boldsymbol{D}_t^A(\boldsymbol{p}) \\ -\left(\mathcal{H}_t(\tau_t^{NA}) \odot \boldsymbol{\Omega}_t^{NA}(\boldsymbol{p})\right)^{-1} \boldsymbol{D}_t^{NA}(\boldsymbol{p}) \end{bmatrix} \equiv \begin{bmatrix} \boldsymbol{\eta}_t^A(\mathcal{H}_t(\tau^A)) \\ \boldsymbol{\eta}_t^{NA}(\mathcal{H}_t(\tau_t^{NA})) \end{bmatrix} \tag{6.4}$$

where $\boldsymbol{D}^A$ and $\boldsymbol{D}^{NA}$ denotes the demand for adopters and non-adopters respectively, and $\boldsymbol{\Omega}_t^A$ and $\boldsymbol{\Omega}_t^{NA}$ represents their respective derivatives with respect to prices. Consequently, by estimating the entire surface of demand derivatives, unlike the naive reduced-form regressions, the structural model can provide us with a direct approach to evaluating adopter conduct and non-adopter sophistication.

[31] Since the vast majority of the buildings are owned by single-building owners, adoption status is uniform within owners. Moreover, multi-building owners tend to use the same management company and thus have the same adoption status. The case where multi-building owners have a set of buildings with different adoption statuses is exceedingly rare. Therefore, we are not concerned about the internalization of profits between adopters and non-adopters.

6.2 Conduct Test

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

$$\boldsymbol{mc_t}^A = \boldsymbol{p_t}^A - \boldsymbol{\eta}_t^A(\mathcal{H}_t(\tau^A)) \tag{6.5}$$

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

$$\mathbb{E}[\omega_{jt}|\boldsymbol{z}_t^S] = 0. \tag{6.6}$$

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

$$mc_{jt}^A(x_{jt}, q_{jt}) = h(x_{jt}) + \omega_{jt}. \tag{6.7}$$

where, without loss of generality, we assume $\boldsymbol{x}_t$ to be part of the $\boldsymbol{z}_t^S$.

In the multi-family rental setting, rather than using a fully flexible function h, we think it is a reasonable approximation to consider a flat marginal cost curve up until the capacity constraint. As such, we assume $h(x_{jt})$ to capture various fixed effects. Alternatively, one might also be concerned that occupancy is likely the main driver of the slope of marginal cost, perhaps capturing the intuition that the marginal cost curve has a hockey-stick shape, namely, increasing in occupancy and asymptotes to infinity right above capacity, which we parametrize as

$$mc_{jt}^A(x_{jt}, occ_{jt}) = h(x_{jt}, occ_{jt}) + \omega_{jt} = \frac{\kappa_a}{(occ_{jt} - 1 - \delta_\epsilon)^2} + \delta_j + \omega_{jt}. \tag{6.8}$$

where occ_{jt} denotes occupancy (i.e., quantity divided by capacity), where δ_ϵ is a modeling tool to make sure the marginal cost function is always well-behaved even if occupancy is at 100%.[32]

For the conditional moment condition in Eq (6.6), 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 construction of new buildings (or demolition of old buildings). To the extent that development

[32]While the inclusion of occupancy in marginal cost is economically motivated, it produces an econometric problem in that it introduces another endogeneity problem in estimating κ_a. As such, we use the same set of instruments for the conduct test to instrument for the quantity term in front of κ_a .

activities nearby are typically driven by unobserved demand shocks (e.g., an area is becoming gentrified) as opposed to marginal cost shocks of the focal building, it should still generate variations in the demand for the focal building.

With the supply model and the marginal cost shocks described above, we adopt 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. 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

$$\mathbb{E}[(\omega_{j,t}^{M1})' A(\boldsymbol{z}_t^S)] - \mathbb{E}[(\omega_{j,t}^{M2})' A(\boldsymbol{z}_t^S)] \tag{6.9}$$

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

$$A(\boldsymbol{z}^S) = \mathbb{E}[\Delta\eta_{jt}^{1,2} | \boldsymbol{z}^S], \tag{6.10}$$

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.

While it may seem straightforward to directly estimate the coordination parameter τ^A through GMM (Nevo, 2001; Miller and Weinberg, 2017), we primarily focus on the two-sided RV test for several reasons: (i) The main advantage of pair-wise testing over conduct parameter estimation is its desirable properties against possible model misspecification (Magnolfi and Sullivan, 2022; Duarte, Magnolfi, Sølvsten and Sullivan, 2023). While the estimation approach is directly sensitive to misspecification of the demand model and functional form of marginal cost, the pair-wise testing focuses on which model of conduct is "more favored" by the data, given the possible misspecifications. (ii) The estimation framework is more valuable if intermediate values of the internalization parameter $\tau^A \in (0, 1)$ is the key object of interest, but given our focus is primarily on *whether* algorithms facilitated joint maximization among its adopters, the pair-wise testing framework is suitable for our context. That said, when it comes to the sophistication parameter τ_t^{NA} for non-adopters, since the quantification of its impact is driven by its magnitude, we approach τ_t^{NA} through the standard GMM framework.

6.3 Testing Results

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

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

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

Algorithm 1 Testing Procedure

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

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

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

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

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

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

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

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

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

5. Compute the test statistic

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

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

Table 10 summarizes the result of the conduct test. The pair-wise test compares model 1 of competition (i.e., own profit maximization) vs. model 2 of coordination (i.e., joint profit maximization). A value of $\tau_2^A = 1$ means that we are testing a model of full competition against a model of full coordination. A value of $\tau_2^A < 1$ means that we are testing a model of full competition against a model of partial internalization. A negative test statistic implies that data favors model 1, namely, competition. A positive test statistic implies that data favors model 2, namely, partial or full coordination.

For the pre-acquisition time period (2009-2017), the test statistics for LRO adopters and RealPage adopters for full coordination are 2.67 and 2.93, respectively, suggesting that data favors a model of coordination over competition among adopters. The test statistics of other intermediate levels of internalization τ^A also generate significant test statistics in favor of some degree of joint maximiza-

tion.[33] As such, the test finds that the markups implied by our structural model of demand are more consistent with a model of joint profit maximization among the adopters of both software rather than a model of own profit maximization in the 2009-2017 period.

For the post-acquisition time period (2017-2018), the test statistic for full coordination among all adopters of the merged RealPage is 0.59, suggesting that data cannot differentiate the two models with any statistical significance. The test statistics of other intermediate levels of internalization τ^A generate significant test statistics when it goes below 0.4. These findings suggest that the markups post acquisition do not seem to be consistent with a model of full coordination, yet, some degree of coordination seems preferred by the data over competition. We think this result is intuitive if the completion of the acquisition in the legal entities does not mean that the operations of the two underlying software were combined immediately.

To consider the robustness of the results, one might be concerned that possible coordination might also be driven by joint ownership that is not fully controlled for by the observed ownership data: Even though RCA attempts to look through the various shell companies to capture the underlying economic owner, mismeasurment could imply that one may under-estimate the extent of joint ownership. As a result, we also perform a more conservative conduct test by controlling for shared management company, as there are likely many more buildings that share management companies than buildings that share underlying owners. In other words, we test a model of competition of management companies against a model of coordination among algorithmic adopters. However, even in this more conservative test, we still find that the test statistics appear positive and significant for the 2009-2017 period, suggesting that the markups are more consistent with a model of joint maximization.

Given these results, as well as the theoretical interest in terms of whether algorithms are able to learn to coordinate among themselves Calvano et al. (2020), we also consider a test of coordination *between* RealPage and LRO prior to the acquisition from 2009 to 2017. In this case, we test a model of within-software coordination against a model of across-software coordination. Table 11 summarizes the pair-wise test results on whether the "cross-diagonal" terms τ^A_{RL} between RealPage and LRO is favored by coordination. In this case, the test statistics is always insignificant (less than 1.96), suggesting that we do not find statistically significant evidence of coordination between RealPage and LRO prior to the acquisition.

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

6.4 Implications

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

Table 12 summarizes the results. The first column shows that, within the estimation sample, the fraction of adopters grew steadily from 6.5% in 2009 to about 25% in 2019. Consequently, the estimated markup difference between the two models of conduct also increased from \$13.6 per month on average in 2009 to \$24.9 per month in 2019, or \$53.1 per month if we assume that merged RealPage eventually would be able to jointly profit maximize among its adopters. The average also masks the extent of heterogeneity across markets, where the average impact per month could be as high as \$78 per month for submarkets in the 75th percentile.

As a back-of-the-envelope calculation, we could also compute the sum-product of the markup difference with the building capacity among adopters across all these ten cities in the estimation sample. When summed across all buildings, the total estimated impact amounts to about \$33mm per month in the fully merged RealPage. Given that our estimation sample accounts for only about 600,000 adopted units in 2019, but we tagged over 2.4 million units across the top fifty metros in the US, the total impact could reach \$130mm per month, or over \$1.5bn per year. We also note that the calculation here is based only on the partial equilibrium of comparing markups under the two models of conduct among adopters. To fully capture the welfare impact, it would require a full re-computation of the new equilibrium for both adopters and non-adopters. However, because we do not have a structural model of how non-adopters would behave under such hypothetical market environments, we do not compute such counterfactual equilibrium.[34] That said, in terms of the direction of the bias, if anything, a full-fledged counterfactual would likely produce an even higher estimate because non-adopters would endogenously respond to adopter coordination to some degree, whereby prices are strategic complements.

Lastly, we also provide a decomposition analysis to disentangle the efficiency and the coordination channel of algorithmic pricing in a partial equilibrium. Specifically, we directly estimate the sophistication parameters of non-adopters τ_t^{NA} for three different time periods, namely, from 2009 to 2011, from 2012 to 2015, and from 2016 to 2019. The segmentation of the time period is motivated by the reduced form finding where the impact of responsive pricing is likely time-varying, depending on the underlying market conditions. The estimated values are 1.49 for 2009-2011, 0.79 for 2012-2015, and 0.71 for 2016-2019. Recall that $\tau_t^{NA} > 1$ means that non-adopters charge too high of a price compared to the optimal markup, and vice versa. With the estimated time-varying sophistication parameters

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

τ_t^{NA}, we could also compute how coordinated and responsive pricing contributes to the overall price levels of the adopters. We first compute the markup difference due to coordination. We then compute the impact of responsive pricing by computing a second markup difference where we set $\tau_t^{NA} = 1$.

In Figure 9, the top panel shows that coordination had a small price impact in Seattle in 2009 when total adoption was still very low, but responsive pricing played a much larger role in bringing *down* the prices of adopters. The net impact of algorithmic pricing is that adopters charged lower prices than non-adopters. By contrast, the bottom panel of Figure 9 shows a much bigger markup impact due to coordination in 2019 when algo penetration had grown significantly by then. In addition, the responsive channel added another price bump. In net, the adoption of algorithmic pricing meant that adopters charged significantly higher prices due to the adoption of algorithmic pricing in 2019.

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 various sources and merge it with a comprehensive database of building-level rents and occupancy across 50 metro areas.

First, we find robust evidence that the algorithm helps building managers set prices 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, we estimate the market-level treatment effect of algorithmic pricing penetration to measure the aggregate impact. 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 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 market-level results provide evidence that the software helps adopters set more responsive prices, we cannot directly use such reduced-form results as a conclusive test for conduct because a model of coordination produces directionally the same results as a model of responsive pricing during an economic boom. To test for conduct, we take a structural approach. The structural approach allows us to isolate the test for coordination by imposing the structural relationship on how markups among adopters would be determined by a given conduct assumption together with a demand system.

For the structural approach, we first estimate a model of housing choices from renters based on housing characteristics and household demographics across a panel of ten cities, leveraging detailed

price and quantity data at the building level. Then, we adopt a pair-wise testing framework and find that it favors coordination among adopters of RealPage and LRO from 2009-2017, supporting joint profit maximization over competitive pricing. Post-acquisition tests (2017-2018) show weaker results but still favor some degree of coordination. This coordinated pricing leads to higher markups, increasing costs for renters. In 2019, if under full coordination, the average markup difference due to joint maximization among the adopters could reach $53.1 per month per unit, affecting about 2.4 million units across the top fifty metros in the US.

Overall, our findings and empirical approach have significant implications. The real estate industry is colossal, with an estimated asset value of multifamily at around $3.8 trillion dollars.[35] Consequently, even a minor percentage impact within this industry translates into substantial value differences. As algorithmic pricing becomes increasingly prevalent across various sectors, price efficiency can be improved if algorithms are better at acquiring and incorporating market information. On the other hand, given the market concentration of such business services software, the concern around "algorithmic collusion" also looms. Balancing the tradeoff between the two seems a complex yet pressing concern for businesses, consumers, and regulators alike.

[35] https://www.reit.com/data-research/research/nareit-research/estimating-size-commercial-real-estate-market-us-202 accessed July 16, 2024.

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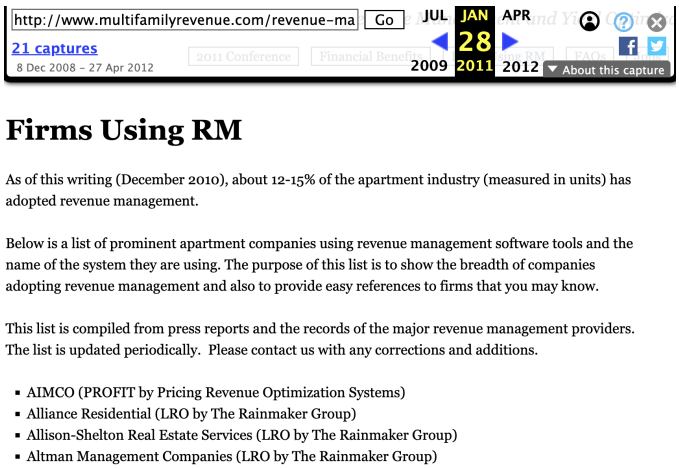
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UK Competition and Markets Authority, "Algorithms: how they can reduce competition and harm consumers," 2022.

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

8 Figures

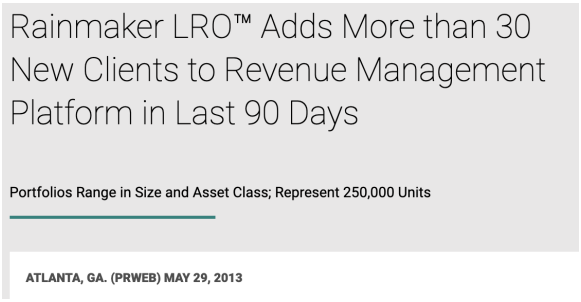
Figure 1: Snapshot of Surveyed List of Adopters



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



(b) Yieldstar



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

Figure 3: Share of Adoption by Software

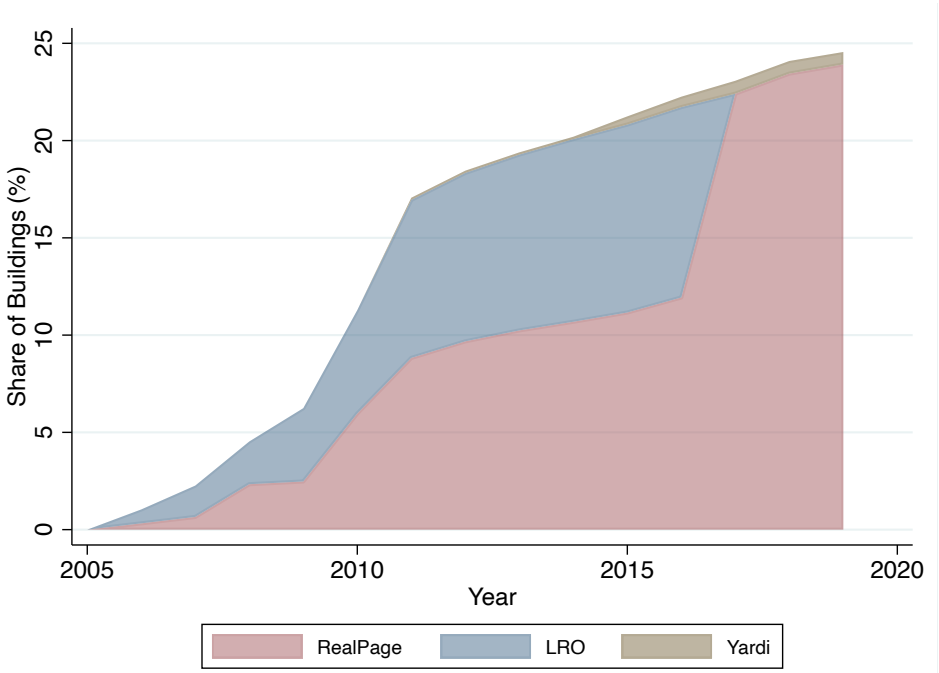
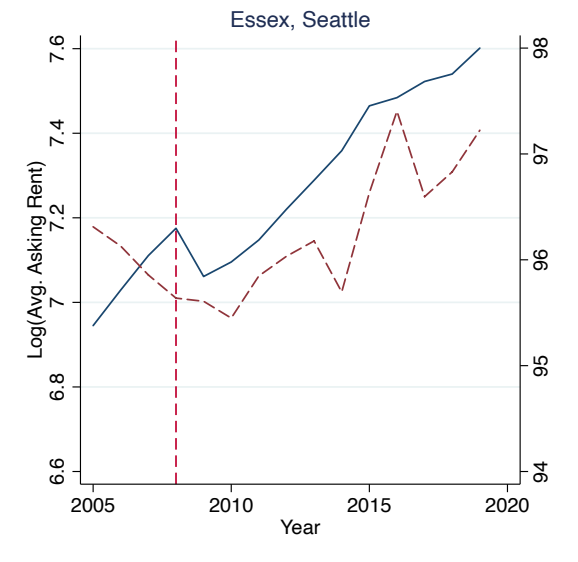
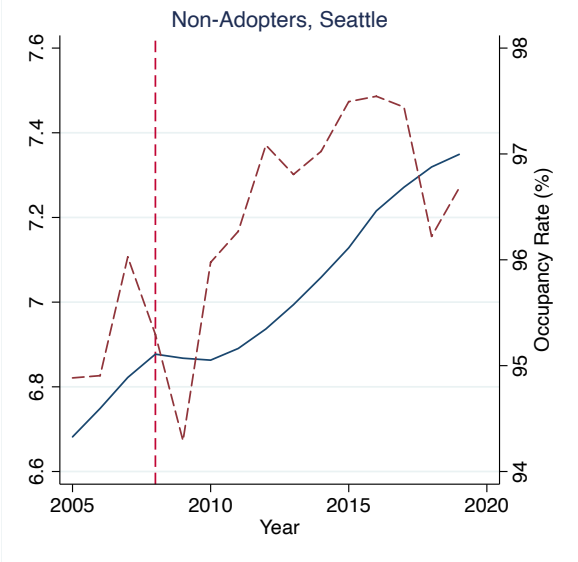


Figure 4: Case Study: Pricing and Occupancy Trend of Companies Adopted the Software

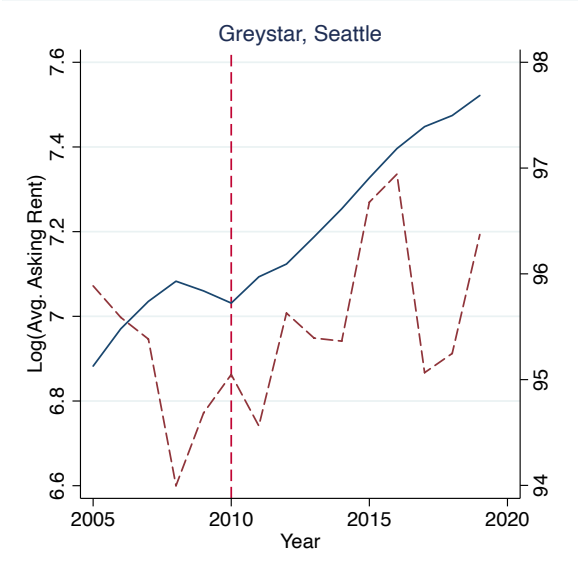
(a) Essex



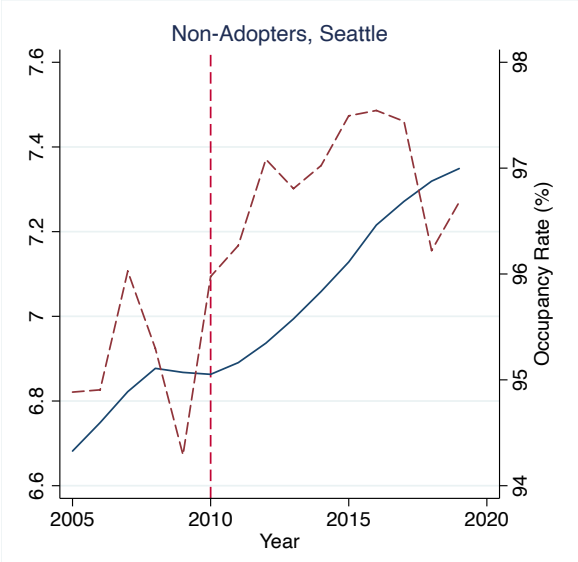
(b) Non-Adopters



(c) Greystar



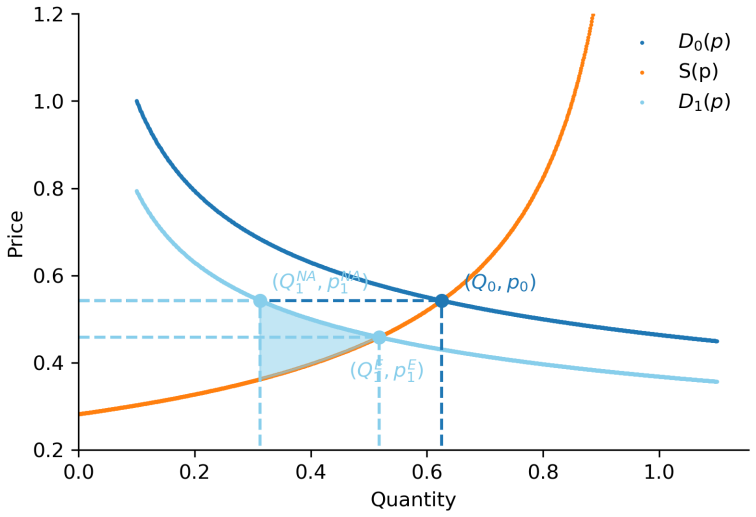
(d) Non-Adopters



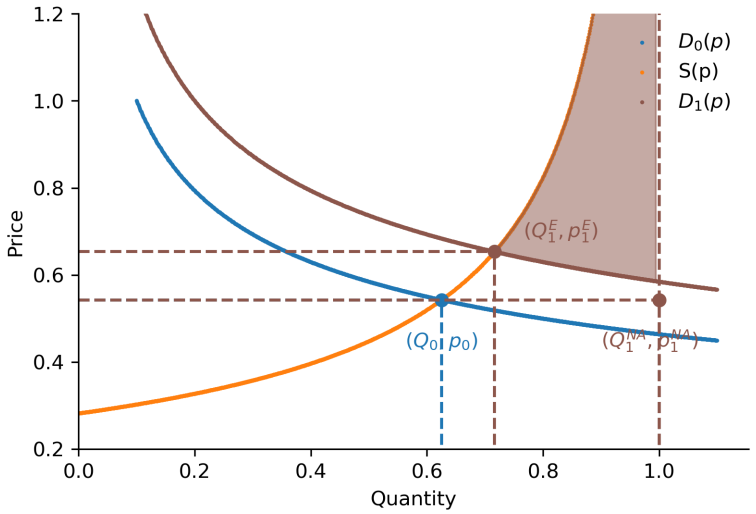
Restricting samples to Seattle metro-area apartments. Solid, navy line follows Log(Rent) and dashed, red line follows occupancy rate. The vertical dashed line indicates the year of adoption of each management company.

Figure 5: Responsive Pricing to Demand Shocks

(a) Negative Demand Shock



(b) Positive Demand Shock

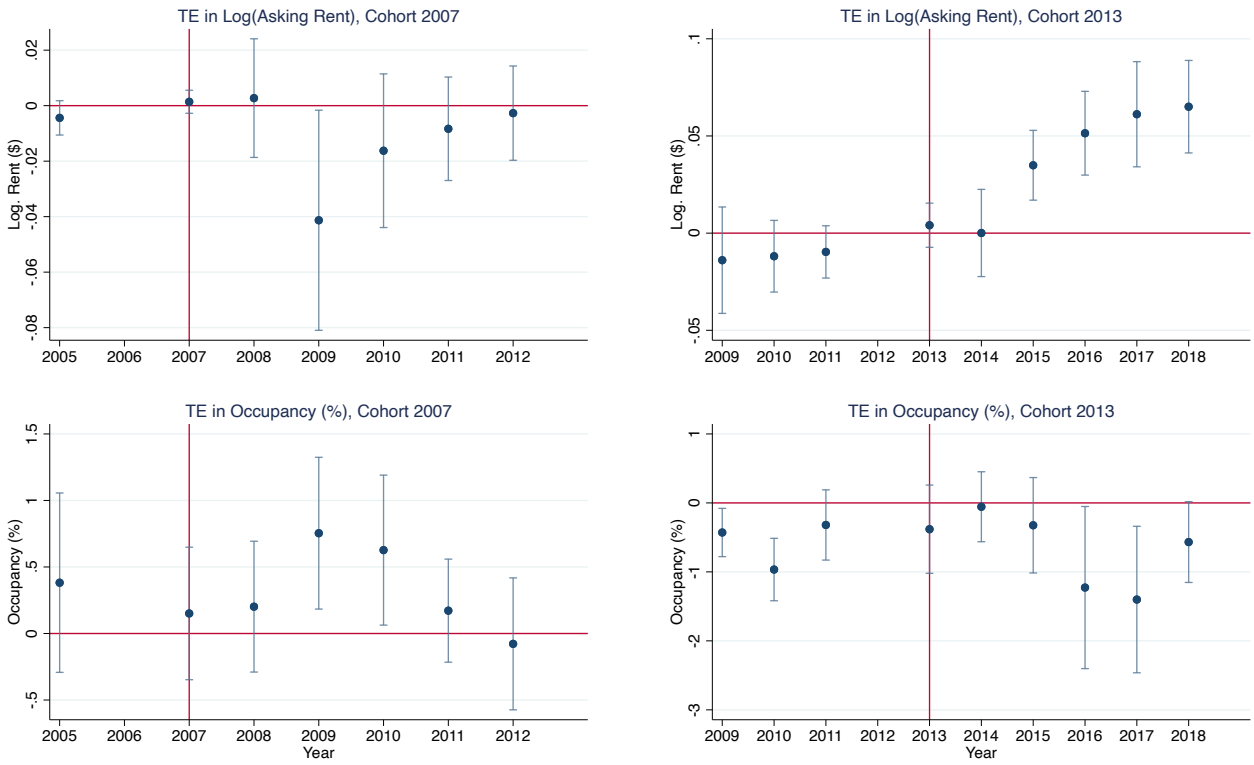


A stylized model of responsive pricing. In the top panel, with a negative demand shock, stale prices that are too high relative to the new equilibrium prices lead to excessive vacancies. Lowering prices more quickly results in welfare gains that are indicated by the blue shaded region. In the bottom 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

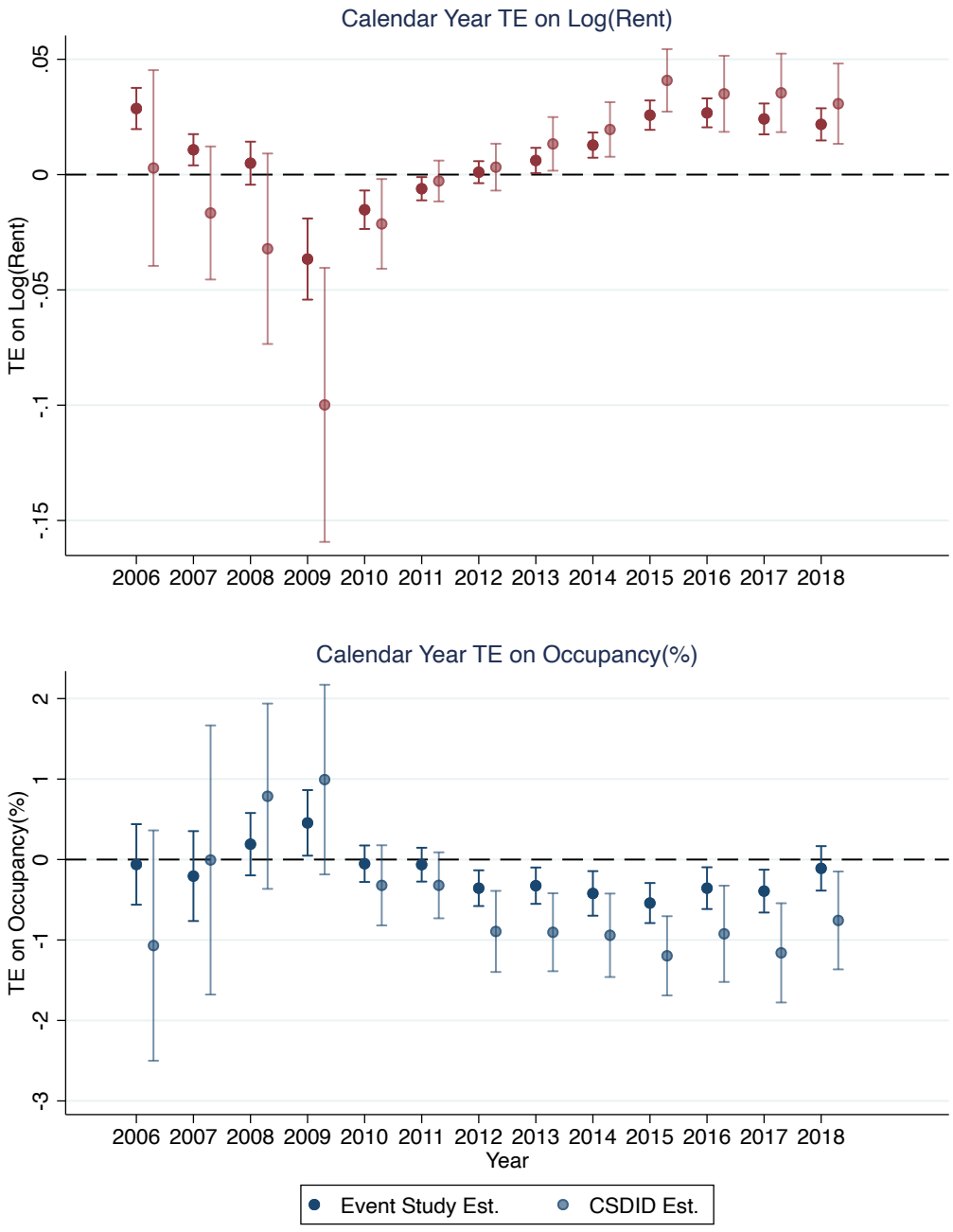
(a) 2007 Adopters

(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.

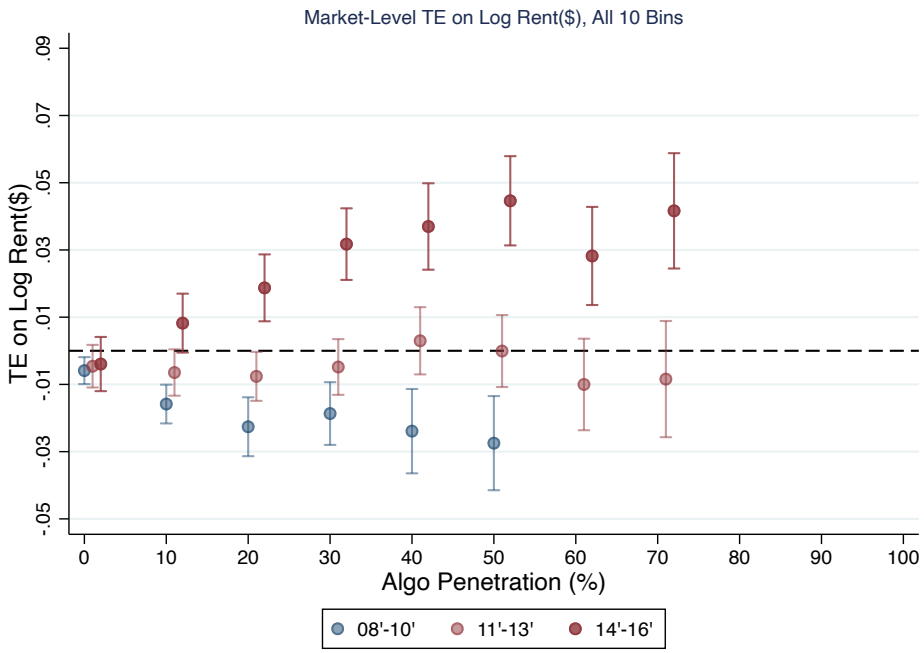
Figure 7: Calendar Year TE on Adopted Buildings



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) $Y = Occ$

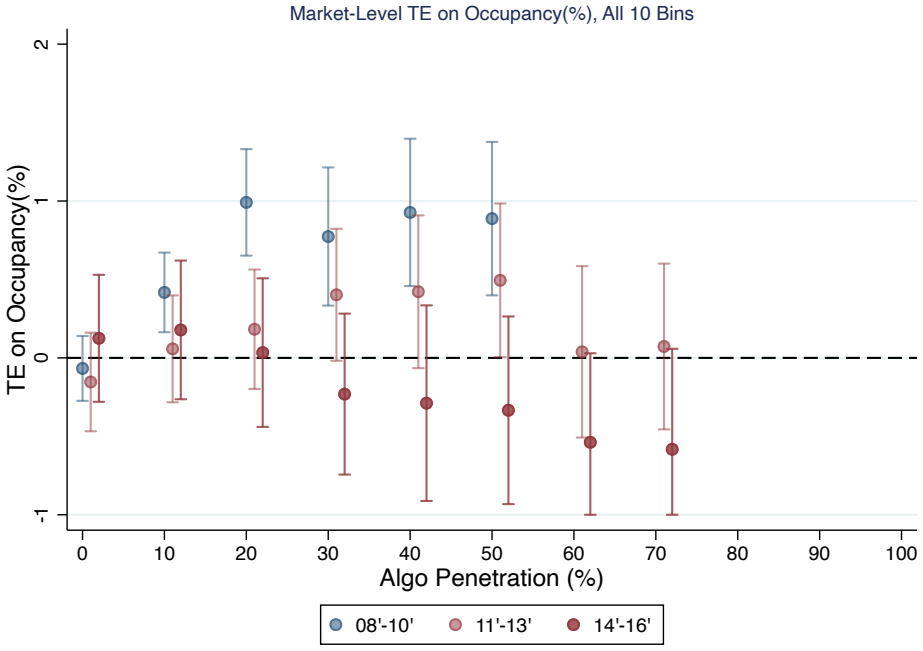
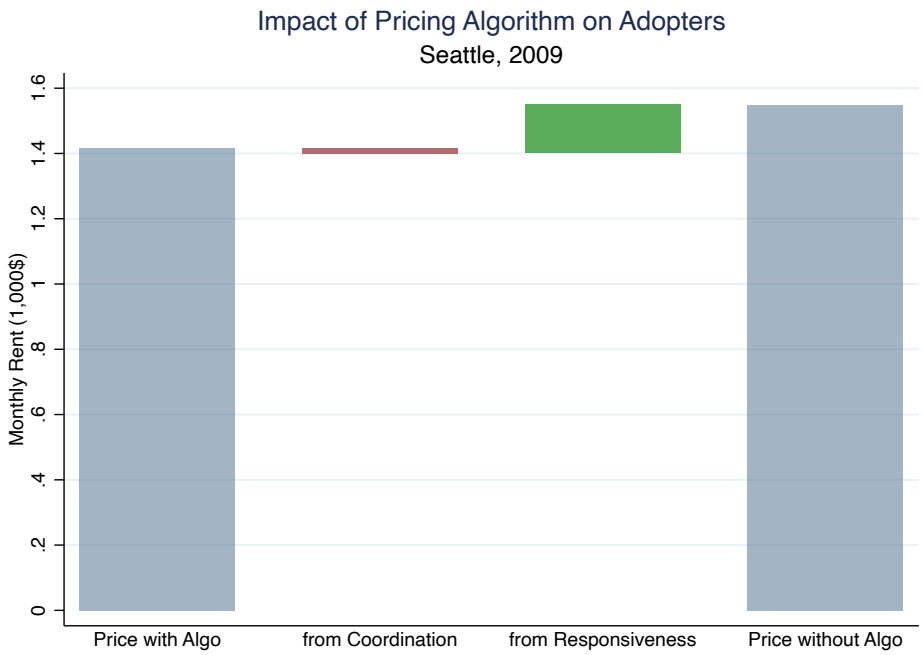
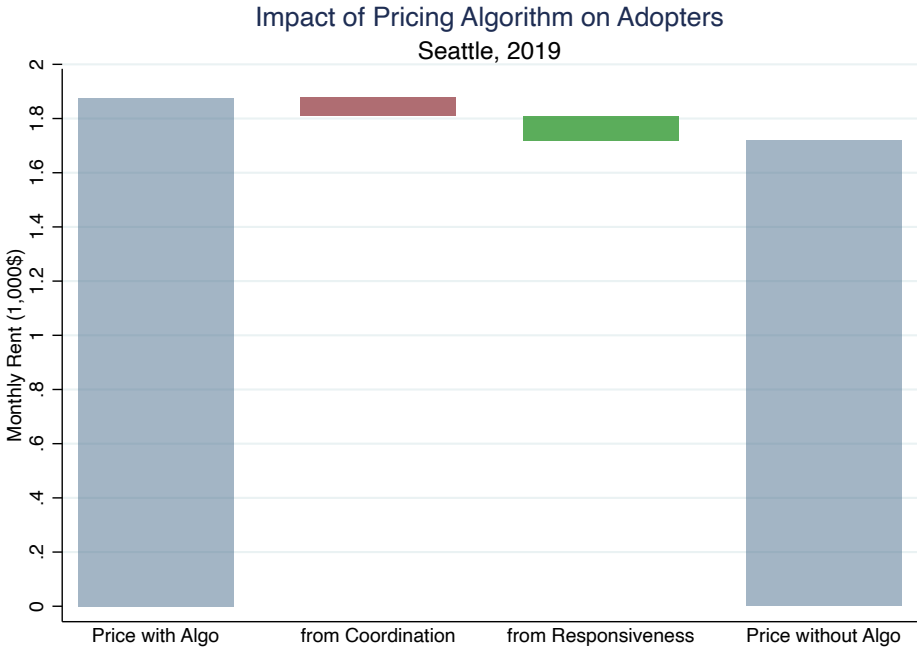


Figure 9: Decomposition of Algorithmic Pricing Implication

(a) Seattle 2009



(b) Seattle 2019



9 Tables

Table 1: REIS Summary Statistics

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

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

	By Building	By Unit
Adopter Count	9,124	2,416,779
Total Count	37,216	7,216,420
Fraction Adopted	25%	33%

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
$\text{Shr}_{building}$	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

Panel A: Price and Quantity	
Eff. Rent ($1000)	1.429
	(0.630)
Occupancy (%)	95.4
	(8.1)
Panel B: ACS Moments	
$Cov(\text{income}_i, \text{rent}_j)$	0.240
$Cov(\text{hh size}_i, \text{bed}_j)$	0.430
$Cov(\text{has child}_i, \text{bed}_j)$	0.360
$Cov(\text{income}_i, \text{building age} < 10_j)$	0.110
$Cov(\text{hh age} > 35_i, \text{building age} < 10_j)$	-0.069
$\mathbb{E}[\text{income}_i \vert \text{Inside Good}]/\mathbb{E}[\text{income}_i]$	1.30
$\mathbb{E}[\text{hh size}_i \vert \text{Inside Good}]/\mathbb{E}[\text{hhsize}_i]$	0.73
$\mathbb{E}[\text{has child}_i \vert \text{Inside Good}]/\mathbb{E}[\text{income}_i]$	0.46
$\mathbb{E}[\text{hh age} > 35_i \vert \text{Inside Good}]/\mathbb{E}[\text{income}_i]$	0.87
$\mathbb{E}[\text{income}_i \vert \text{class a}]/\mathbb{E}[\text{income}_i]$	1.30
Panel C: Geographical Markets	
N_{metro}	11
N_{submkt}	203
Panel D: Fraction of Units Adopted (as of 2019)	
Mean	32.4%
P5	7.1%
P25	17.7%
P75	44.1%
P95	67.7%

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

Table 9: Estimated Demand Parameters

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

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

Table 10: Test for Coordination

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

Notes: The result of the pair-wise RV test comparing a model of coordination at level τ^A and a model of own-profit-maximization. The standard errors are computed based on 50 draws of the bootstrap whereby we redraw the management companies in the market.

Table 11: Test for Coordination Between Algorithms (2009-2017)

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

Notes: The result of the pair-wise RV test comparing a model of joint maximization *between* software at level τ^A and a model of joint maximization *within* adopters of the same software. The standard errors are computed based on 50 draws of the bootstrap whereby we redraw the management companies in the market.

Table 12: Markup Differences

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

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

A Appendix Figures

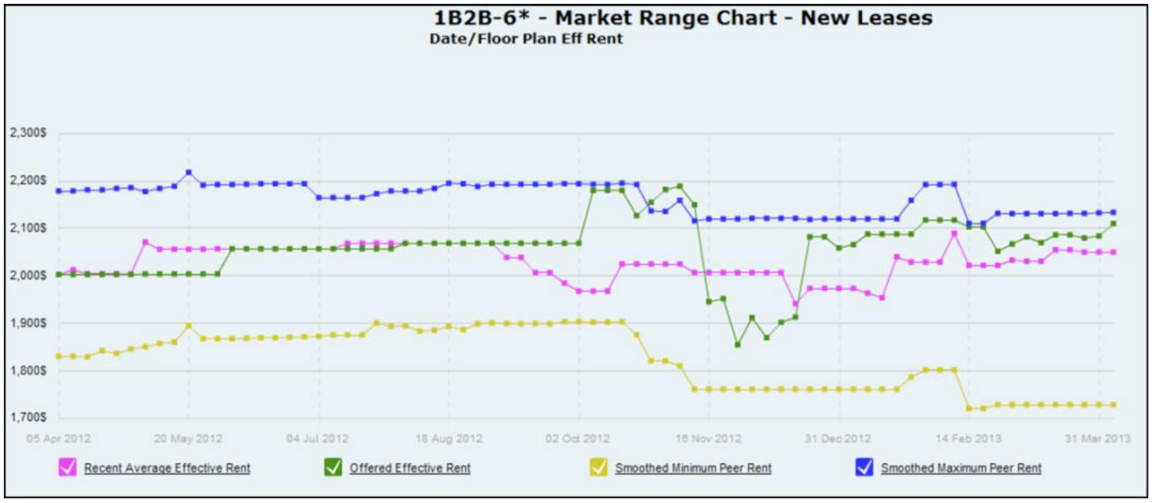
Appendix Figure A1: How Yieldstar optimizes rents

Bedroom Level Pricing

How the tool utilizes the competitive data:

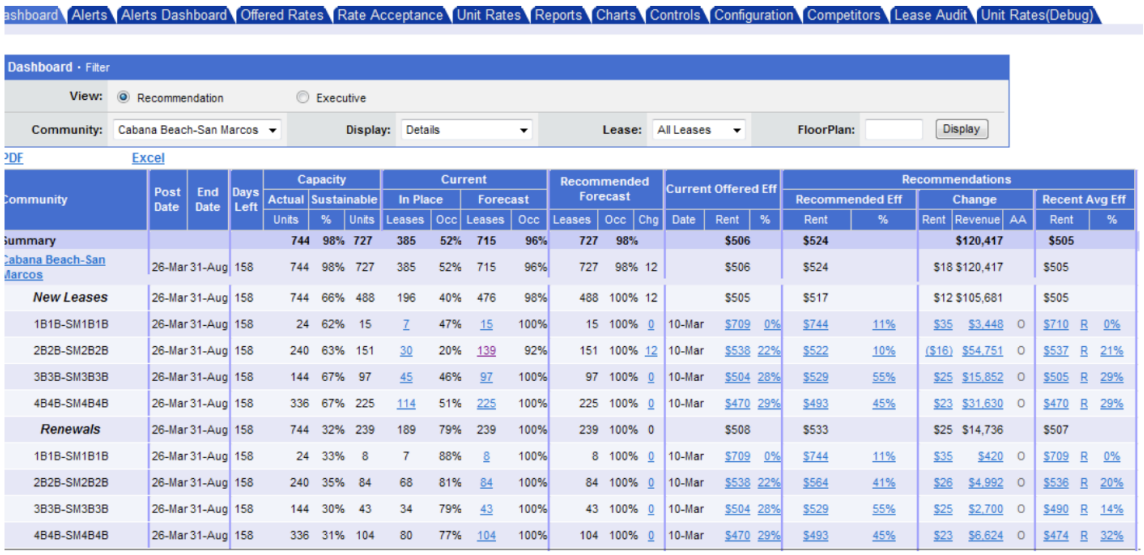
- Starts with your market survey, Operations approves the comps
- Dynamically calibrates elasticity for each bedroom type by:
  - Reading each lease and lease application for your asset
  - Determining the effective rent (net of all appropriate concessions)
  - Comparing the effective rent you achieve 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



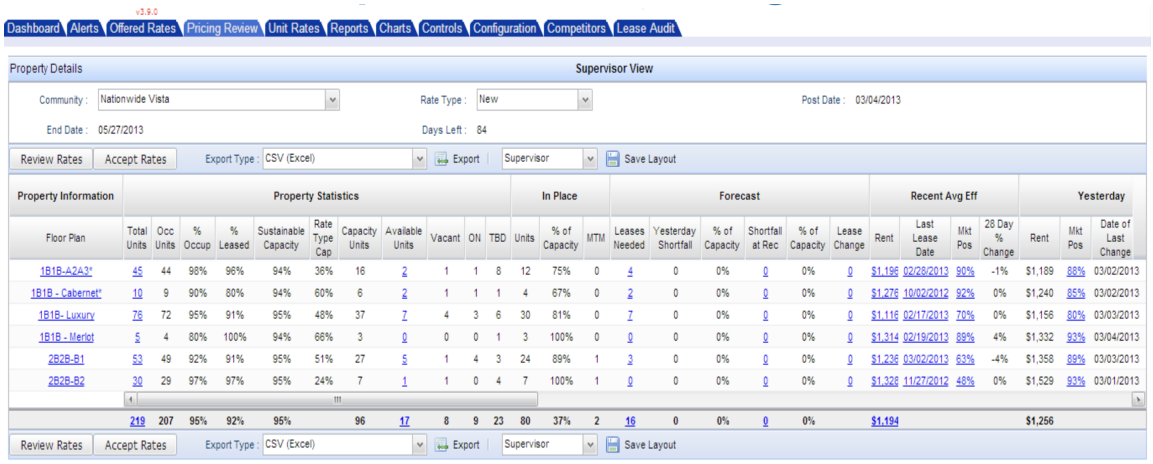
Appendix Figure A3: Manager's view of Yieldstar pricing dashboard

(a) Price recommendation made by Yieldstar



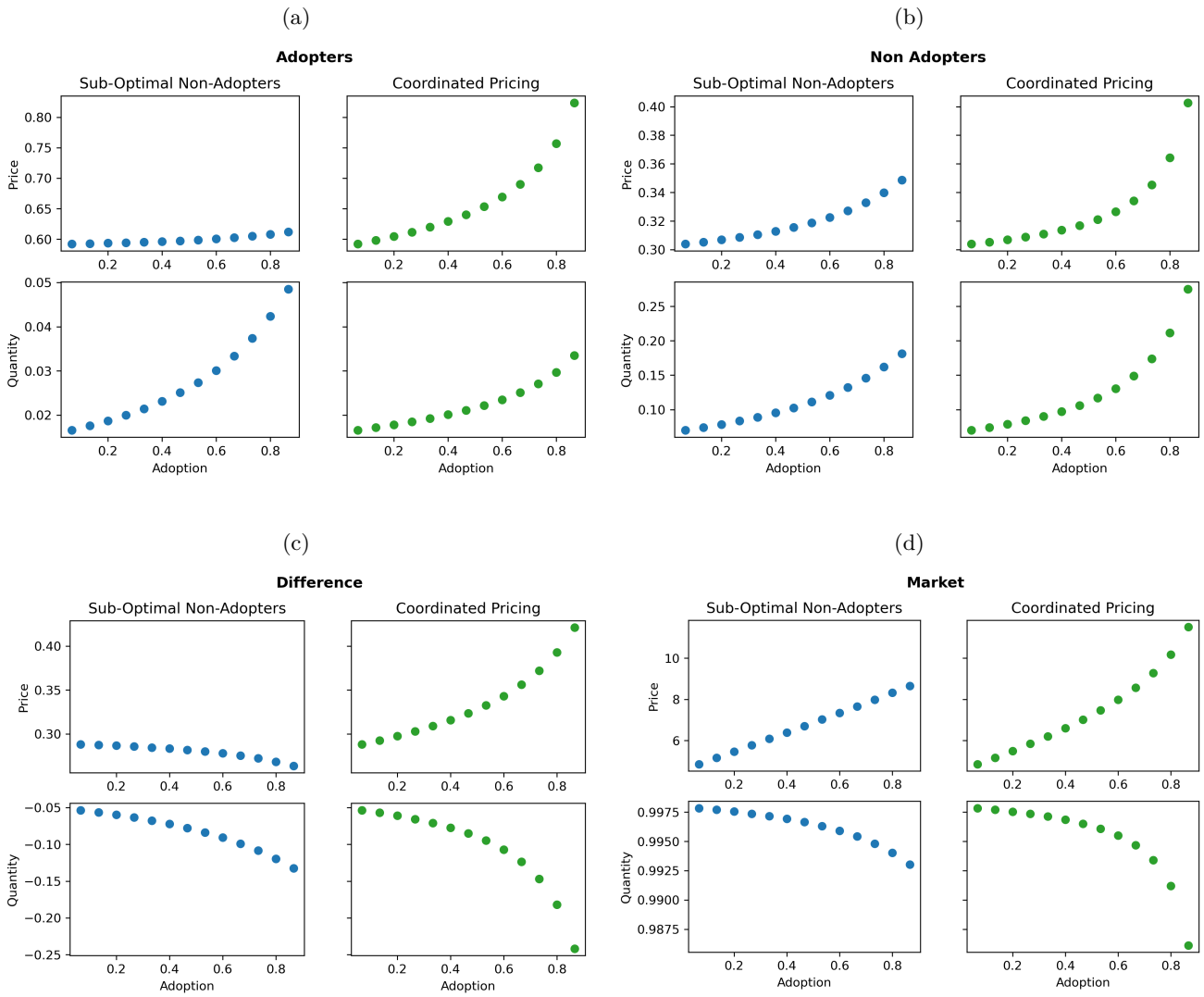
Community	Post Date	End Date	Days Left	Capacity Actual Units	Capacity Actual %	Capacity Sustainable Units	Current In Place Leases	Current In Place Occ	Current Forecast Leases	Current Forecast Occ	Recommended Forecast Leases	Recommended Forecast Occ	Recommended Forecast Chg	Current Offered Eff Date	Current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

(b) Competitor data and recommendation acceptance



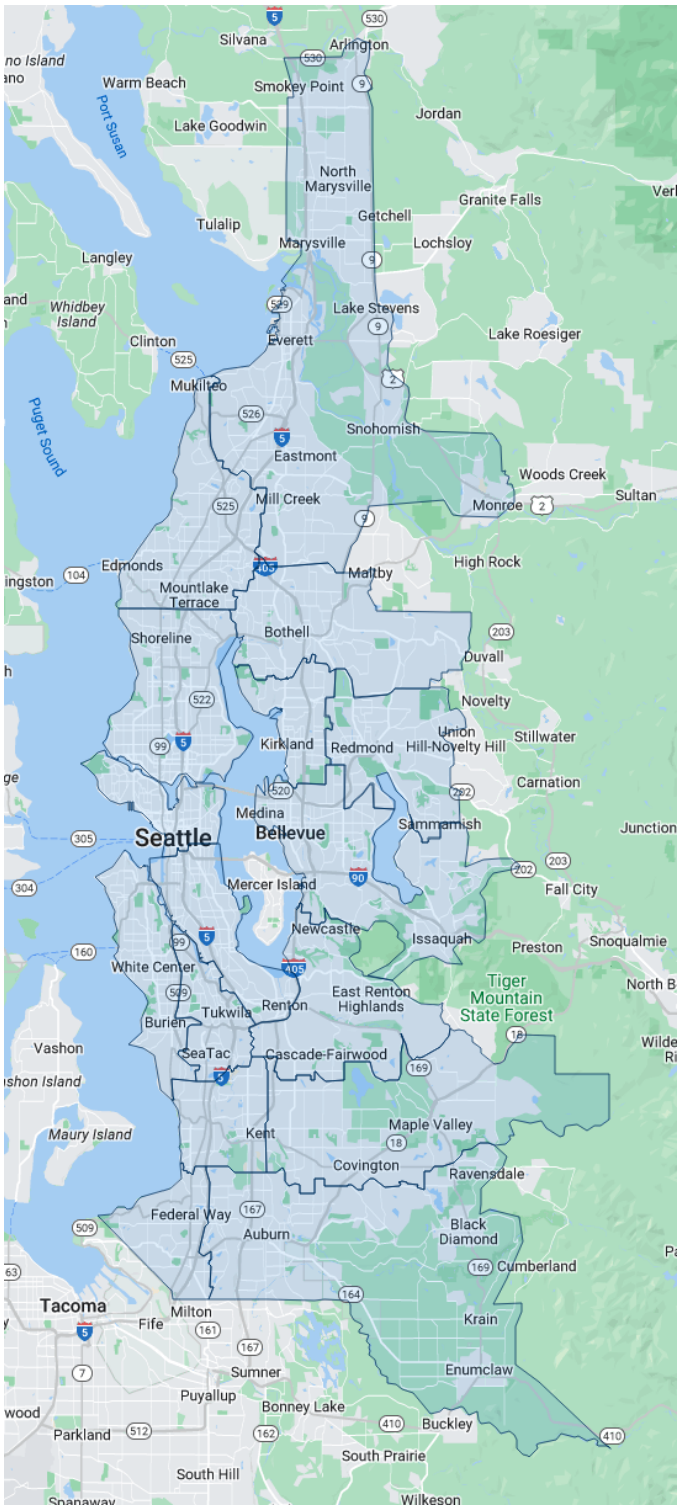
Property Information: Floor Plan	Property Statistics: Total Units	Occ Units	% Occup	% Leased	Sustainable Capacity	Rate Type Cap	Capacity Units	Available Units	Vacant	ON	TBD	In Place: Units	In Place: % of Capacity	In Place: MTM	Forecast: Leases Needed	Forecast: Yesterday Shortfall	Forecast: % of Capacity	Forecast: Shortfall at Rec	Forecast: % of Capacity	Forecast: Lease Change	Recent Avg Eff: Rent	Recent Avg Eff: Last Lease Date	Recent Avg Eff: Mkt Pos	Recent Avg Eff: 28 Day % Change	Yesterday: Rent	Yesterday: Mkt Pos	Yesterday: Date of Last Change
1B1B-A2A3*	45	44	98%	96%	94%	36%	16	2	1	1	8	12	75%	0	4	0	0%	0	0%	0	$1,196	02/28/2013	90%	-1%	$1,189	88%	03/02/2013
1B1B - Cabernet*	10	9	90%	80%	94%	60%	6	2	1	1	1	4	67%	0	2	0	0%	0	0%	0	$1,276	10/02/2012	92%	0%	$1,240	85%	03/02/2013
1B1B- Luxury	76	72	95%	91%	95%	48%	37	7	4	3	6	30	81%	0	7	0	0%	0	0%	0	$1,116	02/17/2013	70%	0%	$1,156	80%	03/03/2013
1B1B - Merlot	5	4	80%	100%	94%	66%	3	0	0	0	1	3	100%	0	0	0	0%	0	0%	0	$1,314	02/19/2013	89%	4%	$1,332	93%	03/04/2013
2B2B-B1	53	49	92%	91%	95%	51%	27	5	1	4	3	24	89%	1	3	0	0%	0	0%	0	$1,236	03/02/2013	63%	-4%	$1,358	89%	03/03/2013
2B2B-B2	30	29	97%	97%	95%	24%	7	1	1	0	4	7	100%	1	0	0	0%	0	0%	0	$1,326	11/27/2012	48%	0%	$1,529	93%	03/01/2013
	219	207	95%	92%	95%		96	17	8	9	23	80	37%	2	16	0	0%	0	0%		$1,194				$1,256		

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



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 Multifamily Building Management Companies

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

Notes: The adoption year is approximated based on various sources of unstructured information collected. NMHC Ranking from https://www.nmhc.org/research-insight/the-nmhc-50/top-50-lists/2019-managers-list/

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

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

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

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