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

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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. Our findings suggest that algorithm adoption helps building managers set more responsive prices: buildings with the software increase prices during booms but lower prices during busts, compared to non-adopters in the same market. However, we also find evidence that greater algorithm penetration can lead to higher prices, raising rents among both adopters and non-adopters in the same market. Such empirical patterns are consistent with either price coordination through the algorithm or widespread pricing errors before software adoption.

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1 Introduction

The recent advances in computing technology have shifted the paradigm of price-setting practices of firms through software that dynamically and automatically set prices. These types of software often leverage high-frequency data collected across firms operating in the same industry or in the same market to suggest optimal prices for managers. The algorithms also make decisions based on the past behaviors of other firms and forecast market demands, which makes them especially prone to be the hub that facilitates price coordination across firms using similar technologies. Moreover, these programs are often powered by artificial intelligence (AI), raising concerns that these pricing agents might just learn to jointly play a collusive strategy rather than price competitively. For these reasons, the technology has recently attracted lots of attention from researchers, policy-makers, and antitrust agencies alike (Fortin, 2020; Mcsweeny and O'Dea, 2017; OECD, 2017).

The industry that has recently come under heavy scrutiny of antitrust authorities for using such technology is the multifamily housing industry of the U.S. Following an article by ProPublica with the title "*Rent Going Up? One Company's Algorithm Could Be Why.*"[1] accusing the pricing software of RealPage, a multifamily management solution IT company, of pushing "prices above competitive levels," a series of class action lawsuits were filed against both the software company and landlords using the software (*Yusupov v. RealPage, Inc. et al*, 2023; *Navarro v. RealPage, Inc. et al*, 2022; *Bason v. RealPage, Inc.*, 2022). It is also reported that the Department of Justice has opened an investigation to look for evidence of price coordination among landlords using the software.[2] However, it is not known that whether the algorithm helped landlords to set efficient and competitive prices in light of the booming multifamily rental market, or it facilitated price coordination among the buildings driving up the prices. Moreover, understanding the economic impact of pricing algorithms in the context of multifamily rentals becomes especially important given its colossal size. At least \$100 billion of rent payments are made annually, representing a sector that is over \$2 trillion in asset size.[3] At the household level, rent payments are often the biggest share of household expenditure among renters.

Aside from the importance of the setting, there is a growing need for a deeper

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

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

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

understanding of the economic impact of pricing algorithms, especially in terms of how they interact with varying market environments in practice. Many theoretical studies have been conducted under synthetic environments, yet there is no theoretical consensus on whether the algorithms end up facilitating collusion (Asker et al., 2022; Calvano et al., 2020; Miklós-Thal and Tucker, 2019; Brown and MacKay, 2021). To our knowledge, the only existing extensive empirical study is that of Assad et al. (2020), which examines the effect of algorithmic pricing on the German retail gasoline market. In contrast to the previous studies that focused on the potential for the algorithms to collude, we seek to provide a more comprehensive view of the issue. The algorithms can be a channel for coordinating on prices which has negative implications on welfare, but they can also be used to efficiently set rents in response to changing market conditions which may be welfare-enhancing. We seek to disentangle the two effects in this paper.

To empirically analyze the impact of pricing algorithms on a given industry, we construct a novel data set of algorithm adoption dates merged with the universe of multi-family rentals. We hand-collected the adoption decisions of management companies from a variety of sources. It includes unstructured data from internet archives of industry surveys, media updates of the relevant software companies, and market intelligence reports using internet traffic. We then merge the adoption dates with rental information from REIS, which consists of a panel of all market-rate multifamily rental buildings in the top 50 metro markets from 2005 to 2019.

Overall, we find that at least 25% of buildings or 30% of units in the data were using pricing algorithms as of 2019. Indeed, we find that 19 out of the top 20 management companies have adopted pricing software. Our data is well-suited to our study because of its long panel structure, covering periods of varying macroeconomic conditions, as well as its rich cross-sectional variations across geographical markets with varying degrees of penetration of the technology.

With this data, we empirically assess the impact of pricing software adoption on rents. We first lay out a stylized model of competition in homogeneous good space with hard capacity constraint to show prediction of prices and quantity under different conduct assumption. Then we specify the empirical estimand of interest that characterizes the aggregate impact of the algorithm adoption across markets that can be decomposed into building-level efficiency gain from the algorithm and market-level equilibrium effect including the non-adopters sharing the market with varying share of algorithm adopters.

First, to assess the potential improvement in pricing efficiency, we use the staggered rollout of the adoption decisions to compare the difference in price and occupancy between adopters and non-adopters in the same market. We find that the adoption of algorithmic pricing indeed appears to help managers to price more efficiently, supported by its heterogeneous effect across the calendar year through business cycles. During the Great Financial Crisis, we find that the adopters *lowered* their prices compared to peers and gained occupancy when compared to non-adopters in the same market. During the boom market starting in 2013, we find that adopters increased rents while sacrificing some occupancy. The time-varying nature of the treatment effects of adoption suggests an important channel through which algorithmic pricing affects the market is by helping managers set prices that are more responsive to changing market conditions.

However, the building-level comparison within the same market is not suited for identifying the total effect because of the strategic responses from competing non-adopters. In the literature that measures the price impact of mergers, a typical threat to identification is the competitive response of the non-merged parties, which are usually competing firms in the same market (Dafny, 2009). A similar concern applies to our setting: if algorithm adopters raise their prices through coordination, it is in the non-adopters' best interest to raise prices as well to a comparable level.[4] In other words, analyses that only look at building-level variations within the same market *cannot* capture the increases in prices resulting from the potential price coordination channel. Thus, the analysis of the impact of varying degree of algorithmic penetration calls for market-level variations involving non-adopters.

When evaluating market-level outcomes, we find suggestive evidence that higher penetration of algorithm pricing software leads to higher rents. We first show markets that experienced a sudden sharp increase in software adoption charge considerably higher rents and have lower occupancies, 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. The 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. It is also robust to controlling for various fixed effects, including the metro-year fixed effect.

We find this positive relationship between market-level rent and algorithm pen-

[4]Under the assumption that the buildings are competing on prices.

etration to hold across all years in our sample period, regardless of macro-economic conditions. On aggregate, the building-level efficiency gain during the downturn is masked by low adoption rate across markets resulting in only $-.015\%$ point decrease in average rents across markets, compared to markets with zero penetration. During the boom period, however, the impact on average rent is substantial. On aggregate, markets with positive penetration spanning over 70% of all markets experience 1.5% point increase in rents. We find this is a substantial effect, because this also involves non-adopters in those markets.

While our empirical exercise cannot make statements towards whether the empirical pattern is driven by coordination without adding more assumptions and structure, our findings are largely consistent with the theoretical studies of Asker et al. (2022) and Calvano et al. (2020) as well as the empirical findings of Assad et al. (2020). The algorithm exhibits heterogeneous effects on participants in markets with varying degrees of software penetration. In addition, we find that algorithms, when used right, can be welfare-improving in helping landlords set efficient prices, which has not been empirically analyzed before.

The remainder of the paper proceeds as follows. Section 2 briefly provides background on the U.S. multifamily housing market and the pricing software used. Section 3 describes the data, shows stylized facts from the data, which motivates the stylized model presented in Section 4. We show evidence that the algorithm helps landlords set efficient prices in Section 5.2, and we also measure its implication on the market-level rents in Section 5.3, giving a rough estimate of the total impact of proliferation of the software. Finally, Section 6 concludes.

2 Multifamily Market and Pricing Software

2.1 Industry Background

The U.S. multifamily housing industry has experienced fast-paced growth after the Great Recession, with 158% increase in value per square feet from 2010 to 2019.[5] While it has been an attractive investment opportunity for institutional investors with 80% increase in average nominal rents and 50% decrease in vacancy rates,[6] renters of these multifamily units spend a substantial share of their income on their

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

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

rents.

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, setting monthly rents, managing lessees, running promotions, alongside with various maintenance activities. While there has been some noticeable consolidation of apartment *owners* that has caught the media's attention recently,[7] more pronounced concentration has happened among the management companies. Greystar, the biggest management company in the U.S., has increased its number of units managing by 337% from 2010 to 2021,[8] and other top 20 management companies as of 2022 have also shown a steady increase in the number of units managing.

2.2 Algorithmic Pricing Software

The management companies rely on IT infrastructure to streamline their operation across thousands of units in dozens of buildings they manage across states. The management companies contract with enterprise solutions who develop proprietary property management software for implementation. These software companies provide a suite of services to construct a central database, manage bookkeeping, engage with tenants, automate lease renewals, monitor vacancies, assess market conditions, etc.

In addition to these services, three major companies started to offer rent optimization solutions starting in the early 2000s.[9] The "revenue management" solution is an automated pricing algorithm that suggests optimal rents in real-time by unit type and lease lengths to property managers. It aims to take guess out of pricing rents in both new lease signing and renewal process. Investors often prioritized maintaining near full occupancy, which many property managers found as a suboptimal way of maximizing the return.[10] By 2011, around 15% of apartment units had adopted a version of pricing software,[11] and in 2017, it is reported that 3 million units were using RealPage's Yieldstar after its acquisition of the largest competitor, Rainmaker LRO. In a report from Fitch, approximately 30% of total units in the U.S. were using

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

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

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

[10] "If you're at 97% or 98% occupancy, your rents are probably too low." https://www.investors.com/news/landlords-consider-how-to-raise-rent/

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

a RealPage software in 2021.[12] While it is unclear that all 19 million units use the company's pricing algorithm, it suggests that there has been a great proliferation of algorithmic pricing across multifamily apartment management companies.

The details of how exactly the software computes optimal rents is not well-known. We were able to obtain a copy of presentation slides made in 2014 that showcases the inner workings of one of the software, RealPage's Yieldstar. The most notable point is that the software estimates demand elasticity and forecasts dynamic demand at the bedroom-level based on lease length and renewal probability by fully utilizing selected competitors' prices and vacancies. See Appendix Figure A1 for the exact wordings from the slide deck, and Figure A2 and A3 shows the dashboard views for a property manager that displays price recommendations made by the software. All of this information is purely for providing data points to manager and the cognitive burden for a manager to act on this myriad of information is minimal; a manager can simply click either the "Accept Rates" or the "Review Rates" (if something seems so out of place) button located on both top and bottom of the table. ProPublica reports that managers accept recommended rents up to 90% of the time.

The pricing software both reacts to changes in market conditions and heavily utilizes the detailed, high-frequency data of competitors down to the granularity of daily prices for individual units. The question is then, where do they get the data from? 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."[13]

While it is almost certain that Yieldstar utilizes its own clients' data to maximize other clients' profits, it is not clear whether this feature should be attributed to achieving competitive, efficient pricing or used to coordinate and maximize the joint profits of the using buildings.

[12] https://www.fitchratings.com/research/corporate-finance/fitch-assigns-first-time-b-idr-to-realpage-inc-outlook-stable-11-02-2021

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

3 Data

To shed more light on the two conflicting channels empirically, we use two main datasets. The first contains the data of market-rate multifamily buildings in the U.S. from REIS by Moody's Analytics. The second contains the adoption year of pricing software or "revenue management" software by management companies, hand collected from various sources. We discuss the advantages and limitations of each data in the following sections, then present stylized facts to motivate the main empirical analysis.

3.1 REIS

REIS by Moody's Analytics contains information on the market-rate buildings in the U.S., from 2005 to 2019. There are 37,216 unique buildings with 7.2 million units covered in the data, covering the top 50 metro markets. According to Fannie Mae, there were approximately 375,000 market-rate properties with 17 million market-rate units in 2021.[14] Considering there were about 1.5 million new units constructed in between 2019 and 2021,[15] our data covers about half of the market-rate apartment units in the U.S. Table 1 shows summary statistics of the data.

Market-rate buildings are a particularly attractive sample since they are not subject to special subsidies or legal compensation. In the fourth quarter of each year, Moody's surveys owners and managers of these buildings and collect information on asking rents, occupancy, concessions, and various amenities. The dataset contains building-year level observations spanning 14 years from 2005 to 2019 with the identity of management companies. REIS also provides its own definitions of markets. They assign each building to one of 625 "submarkets" in one of 50 "metros." Submarkets completely partition a metro without overlaps.

The limitations come from two sources. The first source is that the management companies were based on our sample of the REIS data as of 2019, making the management company field for each building time-invariant even, which ignores prior management company changes. The decision to adopt pricing software is made at the management company level, and as mentioned in Section 2.1, and the property management industry had experienced some consolidation over the past decade. These

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

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

two facts can potentially lead to over-counting of adopters in earlier periods. Because we are misclassifying non-adopted buildings to be adopters (measurement error in the independent variable), this would lead to an attenuation bias towards zero for the estimates.

The second source of limitation is a lack of high-frequency price data. One advantage of higher frequency data is that it shed light on additional price dynamics and responses to changes in competitors' prices at a higher frequency, such as those illustrated in Figure A2. However, we believe that our annual data sample remains sufficient to investigate the potential problem of price coordination. Another advantage of high-frequency pricing data is that it can be used to detect structural breaks to infer adoption in absence of accurate adoption data as done in Assad et al. (2020). This is not a major concern either, 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.

3.2 Software Adoption Data

We collected the adoption data from three major sources. Our first source is yearly snapshots of a website that maintained and updated the list of management companies and owners who had adopted pricing software. The owner of the website solicited survey responses from participants at a major multifamily housing conference from 2008 to 2011 and updated the list every year. See Figure 1 for an example of the website snapshots.

Our second source is various media outlets. Both Rainmaker LRO and Yieldstar had active media presence announcing their major customer acquisitions. Through their main news outlets, they not only announced customer acquisitions but also major updates (or "patches") to their price optimization software. See Figure 2 for an example of an article.

Lastly, we supplement the data using the list from AppsRunTheWorld.com. This company collects data on the adoption of enterprise IT applications and sells insights to salesforces of IT companies for better targeting. We use the list of companies who use Yieldstar. While they also survey the companies and record adoption dates, it could be noisy because some of the adoption dates are inferred from web-scraped data, which may trail behind actual adoption dates. Fortunately, this data contains only a small fraction of adopters compared to the other two more credible sources.

The main limitation is, of course, the measurement error. We expect that we underestimate the number of adopters because our collection methods are designed towards identifying the adoption decision of major management companies. We conducted a validation exercise against the current list of buildings that are RealPage customers.[16] We estimate the fraction of false positives to be minimal at about 2.3%, but the fraction of false negatives may be up to 42%, likely concentrated in buildings managed by small management companies.[17] However, the presence of measurement error in the adoption decisions likely leads to attenuation bias in our estimates.

3.3 Descriptives

In this section, we present stylized facts by combining the REIS data with the hand-collected data on software adoption and discuss sample selection for the main analysis. Figure 3 shows 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.[18] Since we are focusing on the sample of market-rate buildings, we expect these buildings to adopt pricing software more aggressively than other types of multifamily apartments. Compared to the guess of 15% penetration in 2012 made by the surveyor mentioned in Section 3.2, about 19% of units in the REIS data had adopted the software. At the end of our data in 2019, about 2.4 million, or 33% of units, and 9,124, or 25% of buildings in the data had adopted the software.

Despite the concern that the hand-collected adoption data may be prone to false negatives, it is assuring that the adoption data merged with REIS covers 19 out 20 top management companies, as shown in Table 2. These management companies not only have large shares of buildings in our data but also according to the National Multifamily Housing Council (NMHC). More importantly, this subset includes management companies involved in recent lawsuits, allowing us to examine the raw rent

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

[17] We randomly selected 641 buildings in our data set and compare them to the list of RealPage customers as of 2022. Out of these buildings, we correctly identify 206 buildings (38%) as adopters and 146 buildings (22%) as non-adopters. There are minimal false positives. Only 15 (2.3%) buildings were flagged as adopters in 2019 but not using RealPage in 2022. The majority of these buildings were said to be using Rainmaker LRO, so it is possible that they decided not to switch over to RealPage following its acquisition in 2017. We find that 274 buildings (42%) were using RealPage products as of 2022 but were not flagged as adopters in 2019. However, we believe that it represents an upper bound on false negatives because RealPage's algorithmic pricing tool YieldStar is only one of the many property management products offered by RealPage.

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

and occupancy trends of those compared to non-adopters.

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, who adopted the software in 2010, raises their rent more aggressively than the rest of the market and loses occupancy during the period of economic recovery. As such, these charts hint at the presence of the efficiency channel, evidenced by lowering rents to gain occupancy during the downturn and increasing rents during the upturn. If any coordination channel exists, it will suppress rent drop during the downturn and exacerbate rent increase during the upturn.

Prior to moving onto more rigorous analysis, we compare key variables along with building characteristics between adopters and non-adopters. These differences motivate the inclusion of relevant controls in our empirical specification. Table 3 compares key variables as well as some available characteristics of buildings between adopters and non-adopters. Adopters tend to have higher rents and lower occupancy rates than non-adopters, and they seem to be comprised of buildings with higher quality. They are more recently built, tend to be taller in the number of floors, and tend to have more amenities that can be found in "luxury" apartments. It suggests that carefully controlling for building-specific covariates and covariate-specific time trend is necessary to measure the impact of adopting the pricing software on buildings.

4 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 software with the objective of joint profit maximization for its adopters. We show that greater penetrations of the software will lead to weakly increasing prices and weakly decreasing quantities when it is set to maximize profit jointly.

Here are the primitives of the model: Assume that a market is comprised of homogeneous products with no differentiation,[19] but a total capacity constraint at Q^F. Each building owner is infinitesimal and is also capacity constrained. Further, we assume that the marginal cost of operating the building is 0 up to the capacity constraint, and $+\infty$ above the capacity constraint. Let $D(p)$ denote the quantity demanded at price p and the competitive market equilibrium is achieved at p^E such that the total quantity demanded equals the capacity constraint $D(p^E) = Q^F$, where E denotes the efficient outcome.

To further parametrize the problem, we assume a linear demand system

$$D(p) = -ap + b \tag{1}$$

where we can solve the fully competitive benchmark analytically

$$p^E = \frac{b - Q^F}{a}, \quad Q^E = Q^F \tag{2}$$

where the equilibrium quantity is the capacity. Equivalent, the competitive price is the solution when the software is setting price to maximize individual firm profits:

$$\max_p \pi^E(p) = p \times Q^F \qquad s.t. \quad D(p) \leq Q^F. \tag{3}$$

4.1 A Stylized Model of Efficient Price Setting

In this subsection, we illustrate the efficiency gains from more responsive price setting when faced with changing market conditions. Consider a negative aggregate demand shock such that $D'(p) = D(p) - \mu$. For instance, one may consider a contraction of aggregate demand for housing during the great financial crisis. In this model, price is sticky $p_1 = p_0$ and quantity adjusts first. In other words, the effect of the negative demand shock is first reflected in an increase in vacancies:

$$Q_1 = D'(p_1) = D(p_0) - \mu = Q^F - \mu < Q^F \tag{4}$$

[19]The model can be readily extended to a differentiated product setting, in which case, the presence of markup itself is not evidence of price coordination. However, prices above Nash-Bertrand that are consistent with the internalization of fellow-adopters in the same market would be consistent with a model of price coordination.

where p_0 denotes the equilibrium price prior to the shock. Eventually, equilibrium is restored at a lower price at p_2 such that $D'(p_2) = Q^F$:

$$p_2 = D^{-1}(Q^F + \mu) < p_0 \tag{5}$$

as demand $D^{-1}(\dot{})$ is assumed to be downward sloping. If prices were set responsively, then social welfare would have increased by

$$DWL = \int_{Q^F-\mu}^{Q^F} D'^{-1}(q)dq \geq 0. \tag{6}$$

In this stylized model, with a positive demand shock, a more efficient price setting results in a net transfer from renters to owners and total welfare are unchanged. However, the fact that total welfare is weakly positive is an artifact of the simplifying assumption on the marginal cost. In a more realistic model of rapidly increasing marginal cost near capacity, more responsive prices following a positive demand shock will also generate strictly positive welfare gains.

Therefore, the stylized model above motivates our empirical analysis we examine the price and quantity differences between adopters and non-adopters in the *same* market across market conditions (i.e., time) to obtain estimates of the impact of the responsive price-setting channel.

4.2 A Stylized Model of Coordinated Price Setting

In this subsection, we illustrate the intuition behind how adopting a software by a fraction $h \in (0, 1]$ of the owners that jointly maximizes profit leads to higher prices and lower quantity. Moreover, we also illustrate the crucial insight that even non-adopters will best respond by increasing their prices while renting out its full capacity. In other words, because of capacity constraints, prices are strategic complements.

The competitive benchmark is denoted by p^E such that $D(p^E) = Q^F$. The monopoly benchmark is determined by profit maximization

$$\max_p \pi^M(p) = p\,D(p) \qquad s.t. \quad D(p) \leq Q^F. \tag{7}$$

Taking the first-order condition yields that the monopoly price p^M is set at where the demand elasticity is 1:

$$-\frac{D(p^M)/p^M}{\partial D(p^M)/\partial p} = 1 \tag{8}$$

For a linear demand system, we can solve the monopoly benchmark analytically and find that the monopoly price is higher than the competitive benchmark and the monopoly quantity is lower than full capacity:

$$p^M = \frac{b}{2a} \ > \ p^E \tag{9}$$

$$Q^M = \frac{b}{2} \ < \ Q^E \tag{10}$$

as long as the monopoly price does not breach capacity constraint (i.e., $b/2 \leq Q^F$).

Next, we show the impact when only a fraction of the market has adopted a piece of software to perform joint profit maximization. In this case, the best response from non-adopters (NA) is to "undercut" the coordinated price set by adopters (A) by ϵ and reach its full capacity:

$$p^{NA} = p^A = p^J, \quad Q^{NA} = (1-h)Q^F \tag{11}$$

In other words, because they are capacity constrained, there is no incentive for non-adopters to charge below-market prices as they cannot sell more than their capacity.[20] It is also why building-level differences between adopters and non-adopters within the same market cannot be the test for price coordination.

The residual demand faced by adopters (A) with the fraction of adopters h becomes

$$D^A(p) = D(p) - (1-h)Q^F \tag{12}$$

When the software solves a joint maximization problem among all adopters, then it

[20] Presumably, this model implies that there will be incentives for adopters to drop out of the adoption. However, we assume the decision to adopt the software comes with a bundle of services that one cannot easily opt out of separately.

solves the following problem

$$\max_p \pi^A(p) = p\, D^A(p) = p\,(D(p) - (1-h)Q^F) \tag{13}$$

$$s.t. \quad D^A(p) \le h\, Q^F. \tag{14}$$

The solution for the joint maximization problem is characterized as follows:

$$p^J = \frac{b - (1-h)Q^F}{2a} \tag{15}$$

$$Q^J = \frac{b}{2} + \frac{1-h}{2} Q^F \tag{16}$$

when $h \ge b/Q^F - 1$ and $p^J = p^E, Q^J = Q^E$ when $h < b/Q^F - 1$.

Combined with the previous section, we have

$$p^M \ \ge p^J \ \ge \ p^E \tag{17}$$

$$Q^M \ \le Q^J \ \le \ Q^E \tag{18}$$

indicating that the optimal price set for the joint maximization problem is weakly higher than the price in the competitive equilibrium and the optimal quantity is higher than the competitive equilibrium.

Moreover, we have

$$\frac{\partial p^J(h)}{\partial h} \ge 0, \quad \frac{\partial Q^J(h)}{\partial h} \le 0. \tag{19}$$

Hence, the takeaway of the model is that *joint* maximization leads to higher prices as software penetration increases, compared to the price at the competitive benchmark, or equivalently, the price set by a software that is maximizing profit for each adopter *individually*. A second takeaway is that any price differentials between adopters and non-adopters within the same market (perhaps due to different pricing responsiveness to changing market conditions) are *not* appropriate tests for price coordination in this model. Therefore, the stylized model motivates our empirical analysis where we leverage variations in the level of software penetration at the market level to test for the price coordination channel through joint profit maximization.

5 Measuring Impact of Algorithmic Pricing

5.1 Estimand of Interest

Motivated by the stylized model, we formally describe the estimand that quantifies the total impact of algorithm penetration in a given market. Consider a geographical market m at time t and to fix idea, let the average rent of market m at time t, r_{mt}, be the outcome of interest. We are interested in the treatment effect of algorithmic penetration in market m on the market's average rent,

$$TE(s^A) = E[r_{mt} \mid s^A_{mt} = s^A] - E[r_{mt} \mid s^A_{mt} = 0],$$

where s^A is share of buildings in market mt that adopted the software. The first term, $E[r_{mt} \mid s^A_{mt} = s^A]$, is simply the weighted sum of average rent on adopters and non-adopters within market with share s^A:

$$E[r_{mt} \mid s^A] = s^A_{mt} \cdot E[r_{jmt} \mid a_t(j) = 1, s^A] + (1 - s^A_{mt}) \cdot E[r_{jmt} \mid a_t(j) = 0, s^A],$$

where $a_t(j) = 1$ if building j is an adopter at time t and 0 otherwise. Expanding the second term out and substituting it back into the expression for $TE(s^A)$, we get:

$$\begin{aligned} TE(s^A) = s^A_{mt}(&\overbrace{E[r_{jmt} \mid a_t(j) = 1, s^A] - E[r_{jmt} \mid a_t(j) = 0, s^A]}^{=:TE^A(s^A)}) \\ &+ \overbrace{E[r_{jmt} \mid a_t(j) = 0, s^A] - E[r_{jmt} \mid a_t(j) = 0, s^A = 0]}^{=:TE^{NA}(s^A)}, \end{aligned}$$

where $E[r_{mt} \mid s^A_{mt} = 0]$ can be replaced with $E[r_{jmt} \mid a_t(j) = 0, s^A = 0]$ since every building is a non-adopter in a market zero penetration, $s^A_{mt} = 0$.

Note that the first term, denoted it as $TE^A(s^A)$, is the treatment effect of adoption on the building-level, within market with identical s^A. The second term, denoted it as $TE^{NA}(s^A)$, is the market-level impact of algorithm adoption on *non-adopters* between markets with $s^A_{mt} = s^A$ and $s^A_{mt} = 0$. Then the estimand of interest is

$$TE(s^A) = s^A \cdot TE^A(s^A) + TE^{NA}(s^A).$$

Then across $\mathcal{M}$ markets, the total impact is taking the weighted sum of the impact:

$$TE_t = \sum_{m \in \mathcal{M}} w_m TE(s^A_{mt}),$$

where w_m denotes the weight of the market m such that $\sum_{m \in \mathcal{M}} w_m = 1$.

This concretely lays out the empirical objects we are after. As alluded, we estimate TA^A from the building-level regression in 5.2 and TA^{NA} from the market-level regression in Section 5.3. Because we use difference-in-differences estimators, we can only recover average treatment effect on the treated (ATT). Hence, we conduct a back-of-the-envelope exercise to get a rough estimate of the total effect.

5.2 Building-level Impact of Algorithmic Pricing

We first show that buildings that have adopted the software respond to changes in demand conditions more flexibly compared to non-adopters, suggesting that buildings using the software havejmore information on the demand conditions than those who do not, consistent with the qualitative evidence as well as the pricing pattern changes shown in Figure 4, In doing so, we follow the reduced-form analysis of Leisten (2022). Specifically, for building j in market m and time t, we run following regressions:

$$\begin{aligned}
\log(rent_{jt}) &= \theta^r_j + \beta^{r,common} X_{mt} + \beta^{r,adopt} X_{mt} \cdot a_t(j) + \mu^r_{jt} \\
Occ_{jt} &= \theta^o_j + \beta^{o,common} X_{mt} + \beta^{o,adopt} X_{mt} \cdot a_t(j) + \mu^o_{jt},
\end{aligned}$$

where θ_j is the building fixed effects, X_{mt} contains the proxies for demand shifters such as level and changes of unemployment, income, migration, and housing price index, and the indicator $a_t(j)$ is 1 if building j has adopted the software at time t and 0 otherwise. Intuitively, the co-variation of prices and occupancies from the non-interacted part, $\left(\hat{\beta}^{r,common} X_{mt}, \hat{\beta}^{o,common} X_{mt}\right)$ captures the pricing response of buildings facing the demand shifts from the "common knowledge." The interacted part in addition to the common part, $\left(\hat{\beta}^{r,adopt} X_{mt}, \hat{\beta}^{o,adopt} X_{mt}\right)$, captures the pricing schedule of adopters. If the adopters have indeed more information on demand than non-adopters, it should show more flexible price response to changes in occupancy rates.

Figure 5 visually shows the difference in slopes of price response curves between the two groups. The left-hand side imposes no sample restriction, and the right-hand side restricts to markets where there is only one adopter to condition out possible competitive response of non-adopters to adopters' pricing strategy. In both figures, the price response of adopters are more flexible than that of non-adopters; adopters lower prices more aggressively when occupancy is low, and raise prices more aggressively when occupancy approaches 100%. Restricting to markets with only one adopter shows starker information advantage that adopters have over non-adopters. Table 5 shows the difference in slopes between the two groups and statistical significance of the difference. The estimates suggest that while adopters have significant informational advantage over non-adopters, the gap closes once there are enough adopters in a given market due to the equilibrium effect that non-adopters are best responding to adopters' pricing decisions.

From the above exercise, it is straightforward to see why measuring the *average* price effect of adoption on buildings compared to non-adopters does not make much sense. First, the treatment effect of the software on buildings depends on the market's demand conditions. In a low-demand market, more responsive buildings will charge cheaper rents and fill up occupancy compared to non-adopters, and when the demand is high, these buildings will charge higher rents, staying below the 100% occupancy. Hence the relevant empirical question to measure the degree of efficient pricing is to estimate heterogenous treatment effects of the software during market downturns and upturns.

We first show two event study plots for two different, but specific cohorts of adopters to show the existence of treatment effect heterogeneity: a cohort of buildings adopted the software before the financial crisis and another cohort who adopted after the crisis. The outcome of interest is $\log(rent_{jt})$ and occ_{jt}, which are 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 the adoption, and after the adoption. Specifically, for cohort Y that adopted the software in year Y,

$$\log(rent_{jt}^{Y}) = \sum_{\tau=Y-5}^{Y-2} \beta_t^{Y,r} 1\{t=\tau\}a(j) + \sum_{\tau=Y}^{Y+5} \beta_t^{Y,r} 1\{t=\tau\}a(j) + \beta^r X_{jt} + \theta_{mt}^r + \theta_j^r + \mu_{jt}^r$$

$$occ_{jt}^{Y} = \sum_{\tau=Y-5}^{Y-2} \beta_t^{Y,o} 1\{t=\tau\}a(j) + \sum_{\tau=Y}^{Y+5} \beta_t^{Y,o} 1\{t=\tau\}a(j) + \beta^o X_{jt} + \theta_{mt}^o + \theta_j^o + \mu_{jt}^o,$$

where X_{jt} are time-varying building level covariates, θ_{mt} is market-year fixed effects, μ_{jt} are residuals, and β_t are our coefficient of interest, which is plotted in Figure

6 for the 2007 and 2013 cohorts, i.e. $Y = 2007$ and $Y = 2013$. Note that since we are estimating the treatment effects for a single adoption cohort, and comparing the outcome of interest with the never-treated buildings, we are not subject to the concerns of estimating the dynamic treatment effects in the usual staggered diff-in-diff research design.

Since adoption decisions are made at the management company-level, it is unlikely that building-specific unobservable is correlated with company-level decision. All of top 20 management companies in 2022 operates across dozens of states, and it is plausible that these adoption decisions are not driven by any one specific time-varying condition of a building.[21] Importantly, we address the concern of luxury vs. non-luxury apartments being on a different rental growth path by including metro-level, pre-adoption rent quartile, year-fixed effects through θ_{mt}. In other words, these fixed effects allow for full flexibility where buildings in the same metro market but different quality segments could have differential rent growths each year.

Both cohorts as well as the aggregate event study across cohorts do not exhibit pre-trend, further supporting the parallel trend assumption.

The figure shows the clear trade-off that the algorithm makes between occupancy and asking rents. During the market downturn of 2009, the 2007 cohort aggressively lowers their price and gains more in occupancy compared to the non-adopters. The cohort who adopted the software in 2013, however, exhibits significant price growth compared to non-adopters but with almost 1.5% point less occupancy.

To further highlight that the algorithm may help buildings set efficient prices in response to demand conditions, we plot the estimates of the calendar-year treatment effects across cohorts in Figure 7. That is, we are measuring the impact of adoption in year t on the buildings that were using the software in that year. We measure this by simply regressing outcomes of interest on calendar year dummies, interacted with whether the building had adopted by then:

$$\log(rent_{jt}) = \sum_{\tau=2006}^{2018} \beta_t^r 1\{t = \tau\} a_\tau(j) + \beta^r X_{jt} + \theta_{mt}^r + \theta_j^r + \mu_{jt}^r$$
$$occ_{jt} = \sum_{\tau=2006}^{2018} \beta_t^r 1\{t = \tau\} a_\tau(j) + \beta^o X_{jt} + \theta_{mt}^o + \theta_j^o + \mu_{jt}^o.$$

Since this regression estimates treatment effects across all cohorts, we also show the

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

coefficients estimated using the procedure of Callaway and Sant'Anna (2020) along with the coefficients estimated from the regression above. This highlights the aforementioned point that the algorithms help buildings price more efficiently. In between 2008 and 2010, the algorithm advised buildings to lower their rents and gain more occupancy. During the boom period starting in 2013, the adopters were charging higher rents, and allows for some level of vacancy compared to the non-adopters. The fact that the adoption leads to higher prices than non-adopters is not a direct evidence that the algorithm grants market power to the treated. In fact, this may suggest that buildings using the software are optimally pricing during the time of high-demand, which may be socially optimal.

5.3 Market-level Impact of Algorithmic Pricing

We found that within-market, across-buildings, the adoption decision leads to heterogeneous treatment effect across time periods. To get at the market-level effect of adoption among non-adopters, we first show an evidence that the penetration of algorithm into a market raises rents and lowers occupancy regardless of the market condition. We define our "market" as the interaction between the submarket predefined by REIS and the quartile of rents. This mimics how the building managers and the algorithm picks competitors to benchmark their rents as shown in Section 2.2. We leverage variations coming from sudden increase in share of adopters in a given market. We define the event as more than 30% point increase in the share over one year period. That is, over a third of the buildings in a market become adopters in one year.

The parallel trend assumption needs to be justified. Again, we lean on the fact that the adoption decisions are made on the company-level, and any specific market trend would not be correlated with the decision of the adoption. To sufficiently control for local, time-varying demand conditions that might have induced some companies to base their adoption decision off of, we control for levels and changes in unemployment rate, personal income, housing price index, and net migration. Then we run following regressions for market m in year t:

$$\log(rent_{mt}) = \sum_{\tau=-5}^{-2} \beta_\tau^r 1\{t(m) - t = \tau\} + \sum_{\tau=0}^{5} \beta_\tau^r 1\{t(m) + t = \tau\} + \beta^r X_{mt} + \theta_t^r + \theta_m^r + \mu_{mt}^r$$
$$occ_{mt} = \sum_{\tau=-5}^{-2} \beta_\tau^o 1\{t(m) - t = \tau\} + \sum_{\tau=0}^{5} \beta_\tau^o 1\{t(m) + t = \tau\} + \beta^o X_{mt} + \theta_t^o + \theta_m^o + \mu_{mt}^o, \tag{1}$$

where $t(m)$ denotes the year of treatment for market m, X_{mt} contains vector of market-level economic conditions, θ_m are market fixed-effects, θ_t are year fixed-effects, and μ_{mt} are residuals. The coefficients of interest are β_τ, which is plotted in Figure 8, along with those estimated from the procedure of Callaway and Sant'Anna (2020).

In both specifications, we do not see a strong evidence of pre-trend, and if anything, the pre-trend gets even weaker with the robust method of Callaway and Sant'Anna (2020). This suggests that there is an instantaneous increase in rents which keeps on increasing after 5 years from the event. The effect on occupancy rates show up slowly but it gets evident by the fourth year after the treatment. This is not driven by additional "treatment." We find that on average, post-treatment increase in adopter share is much less than that of non-treated markets. Hence, just one-time, but significant increase in the share of adopters leads to a persistent increase in rents and a decrease in occupancy compared to the markets with lower shares of adopters.

Supported by the evidence, we examine differential treatment effects of the software across markets by the degree of its penetration. The "market" definition we consider is a submarket-rent quartile. We categorize the markets into four bins by time periods and ten bins by the degrees of penetration. 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 period that may suffer the most from the attenuation bias coming from actual adopters flagged as non-adopters. Cross-sectional markets are binned by their absolute share of algorithm adopters in 10% point increments. Table 6 shows the variation in penetration by the binned years. We find considerable mass to be at 0% by our market definition, and these markets will be considered as the "control" group. We then regress:

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

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 market-level economic conditions as well as average building characteristics in the market. For the outcomes of interest, $\log(rent_{mt})$ and Occ_{mt}, we plot $\hat{\beta}_{T,B} + \hat{\beta}_B$ for each T, B as shown in Figure 9. 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 that the market-level adoption had zero to negative effect on rents and strong, positive effect on occupancy gain, compared to markets with zero adoption. From 2014 to 2016, the data shows a stark contrast; we find that rent increases rapidly with the penetration of the algorithm, but occupancy decreases with the penetration. Going back to our original hypothesis in Section 4, this speaks to our hypothesis that the efficient prices is lower than the jointly-maximized prices and efficient occupancy is higher than the occupancy realization under joint maximization. From the building-level evidence, we find that 2008-2010 period may be the period with substantial evidence of efficiency gain; this pattern matches both the stylized model and the building-level effect across calendar years.

To get at the estimand of interest mentioned in Section 5.1, we run the same regression but on the sample containing only non-adopters. Figure 10 shows the coefficients. In general, regardless of the periods, the coefficients tend to increase with the penetration of the software up to 4%, meaning that the greater the penetration is, the larger the increase in average rents of the market. The positive relationship between increase in average market rents of non-adopters and software penetration is robust to a variety of alternative specifications. For example, the pattern is robust to the market definition. We show coefficients from the same regression specifications when the market(m) is defined by simple zip code in Figure A4. The relationship is also robust to additional inclusions of metro-year fixed effects, suggesting that the relationship is not driven by unobservable differential trends between metros.

To further check the robustness of the sign and magnitude of the regression, we construct an instrumental variable for building-level adoption decision to instrument for the share of adopters in a given market. We exploit the fact that management companies have presence in multiple areas across the nation, but decision to adopt software is rather a centralized decision. Our conjecture is that the more exposure that a management company has to markets with large share of adopters, the more likely the management company itself decides to adopt one. This is the intuition behind the relevance condition of the instrument. A management company's decision to adopt in a given market is driven only through its exposure to adopters in *other* markets. The exposure instrument's exclusion restriction is met as long as the exposure in other markets is not correlated with the unobservable of the focal market we are instrumenting for. Hence, it is crucial to define the market scope of exposure to be large enough such that there is minimal possibility of spillover of demand shocks across markets, as well as controlling for any nation or regional-level time-trend.

Our unit of analysis is submarket-rent quartile, year pairs, but we construct the IV based on metro-level exposure. Metro is a lot coarser way of defining markets compared to submarket-rent quartile, hence we believe that this is the most conservative way of constructing the instrument. We proceed similarly as constructing a "leave-one-out shift share" instrument; we take the weighted sum of the management company's share of buildings multiplied by share of adopters net of the focal management company's adopted buildings, if they are flagged as adopted, across metros and leaving out that of the focal metro. We predict the probability of a building j's adoption at year t using this variable through a probit regression, recovering $\hat{a}_t(j)$. We then construct the instrument for the penetration in market m in year t as

$$AlgoShare^{IV}_{mt} = \frac{\sum_{j:m(j)=m} \Pr\left(\hat{a}_t(j)\right)}{N_{mt}},$$

where N_{mt} is number of buildings in market m in year t. We then run following two-stage least square regression:

$$\begin{aligned} AlgoShare_{mt} &= \alpha^{1st} + \beta^{1st} AlgoShare^{IV}_{mt} + \beta^{1st,X} X_{mt} + \theta^{1st}_m + \theta^{1st}_t + \mu^{1st}_{mt} \\ \log(Rent^{NA}_{mt}) &= \alpha + \beta \widehat{AlgoShare}_{mt} + \beta^X X_{mt} + \theta_m + \theta_t + \mu_{mt}, \end{aligned}$$

where the first row is regression equation for the first-stage regression, and $\log Rent^{NA}_{mt}$ is average log asking rent of non-adopters in mt. Table 7 shows the estimates of $\hat{\beta}^{1st}$ and $\hat{\beta}$ when controlling for nation-wide time trend, θ_t and when controlling for metro-wide time trend for each submarket-renttile belongs to, $\theta_{Metro(m),t}$. As expected, we have positive and significant $\hat{\beta}^{1st}$. The signs of the main coefficient of interest, $\hat{\beta}$, across the OLS and the 2SLS specifications are consistent and does not lose the significance, showing strong evidence towards penetration raising rents. The magnitude of the 2SLS is larger in general than that of OLS, suggesting that companies may have been targeting buildings in rather struggling markets than booming markets to implement the software.

Given our estimates of $\widehat{TE}^{A}$ and $\widehat{TE}^{NA}$ for each time-period and bins of algorithm penetration, we assess the total impact of the algorithm. Recall that across $\mathcal{M}$ markets, the total impact is taking the weighted sum of the impact:

$$TE_t = \sum_{m \in \mathcal{M}} w_m \left(s^A \cdot TE^A(s^A) + TE^{NA}(s^A) \right).$$

Figure 11 shows the decomposition of the effects for the 2008 to 2010, and the 2014 to 2016 period estimates. Visually, they show impact of adoption weighted up to s^A, and not including w_m. The market-level effect coming from non-adopters seems to dominate the building-level effect, consistent with the stylized model. We weigh the impact by the number of buildings in each market with corresponding penetration levels in Table 6 to account for w_m. The 08-10 period shows that building-level effect on adopters is negative, but it visually seems net positive effect on rents. In fact, the weighted total effect is -0.15% point decrease in rents. This is consistent with the fact that 1) there were few markets were few markets above 30% penetration and 2) adopters' treatment effect was negative compared to non-adopters. In addition, the effect likely be even greater in magnitude once accounting for the noisy estimates above 30%. However, for the 14-16 period, the total effect is roughly 1.5% increase in rents across all buildings in markets with non-zero penetration of the algorithm. While this may not seem substantial, we find this as a substantial increase in average rents considering around 70% of the markets had positive penetration during the period.

6 Conclusion

In this paper, we examined the impact of algorithmic pricing software adoption on the U.S. multifamily housing industry using hand-collected, unique datasets. We show that the equilibrium impact of the penetration of the pricing algorithms can be decomposed into two estimable, empirical objects.

First, we find robust evidence that the algorithm helps building managers price more efficiently. The adopters exhibit a more responsive pricing function to the changes in local demand conditions. The treatment effect of the algorithm at the building-level is heterogeneous across calendar years: in a down market, the algorithm lowers rents and increases occupancy, and vice-versa during the boom.

Second, to measure the market-level, equilibrium impact of algorithm adoption, we measure the market-level treatment effect of penetration. We find that across markets, higher levels of penetration lead to higher rents. This pattern is robust across time-periods, market definitions, and regression specifications.

Lastly, we aggregate two effects and compute the total impact across time periods and markets. We find that during the bust period, the net effect on rent across markets is negative, and during the boom period, the effect is positive. While this is consistent with the efficient pricing by the adopters and non-adopters playing the best-response, we conclude with a note that this can also be consistent with the pattern of coordinated pricing, shown by the empirical pattern of price increase and quantity restriction of the non-adopters as well as the previous theoretical and empirical work on algorithmic pricing.

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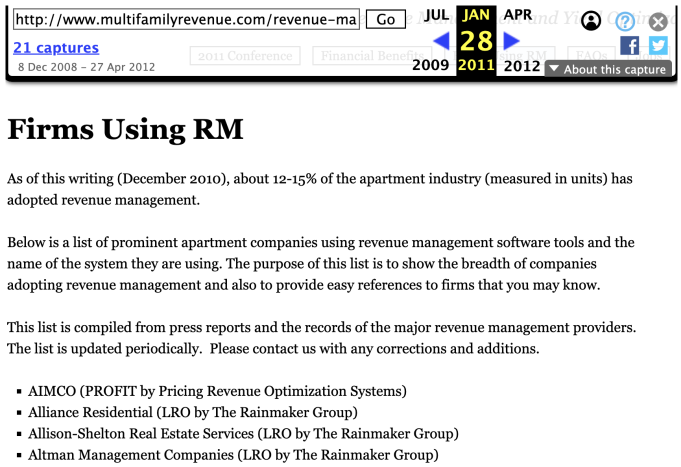
7 Figures

Figure 1. Snapshot of Surveyed List of Adopters


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

Firms Using RM

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

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

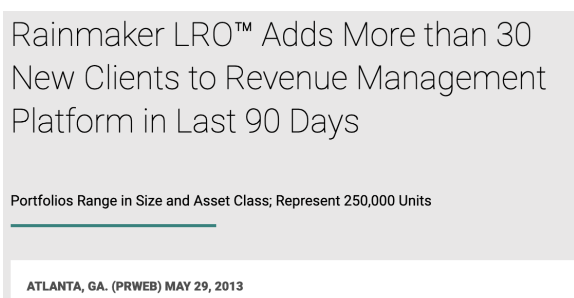
This list is compiled from press reports and the records of the major revenue management providers. The list is updated periodically. Please contact us with any corrections and additions.

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

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

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

(a) Rainmaker


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

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

ATLANTA, GA. (PRWEB) MAY 29, 2013

(b) Yieldstar


RealPage Announces that Wilkinson Selects YieldStar Price Optimizer

By RealPage News | June 21, 2011

Decision follows substantial improvements in rent and occupancy during trial

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

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

Figure 3. Share of Adoption by Software

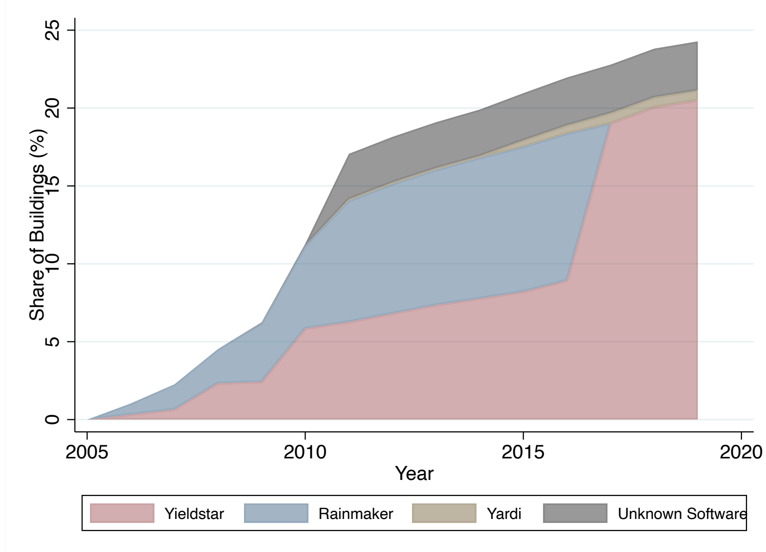
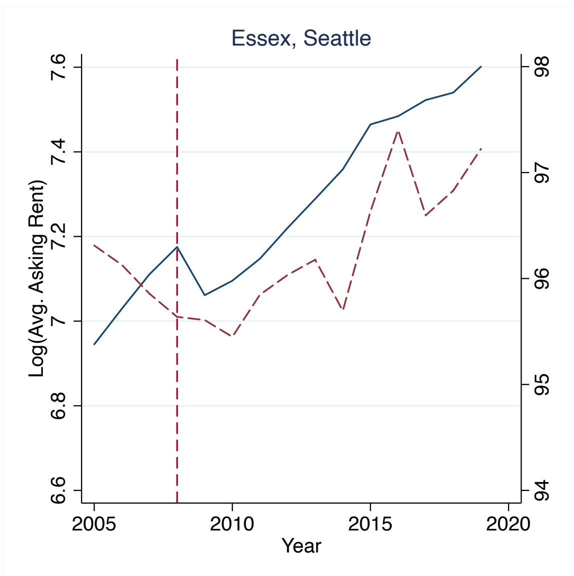
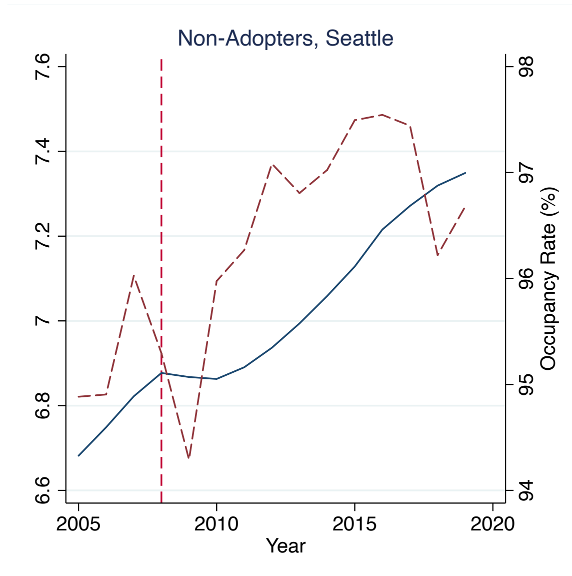


Figure 4. Case Study: Pricing and Occupancy Trend of Companies Adopted the Software

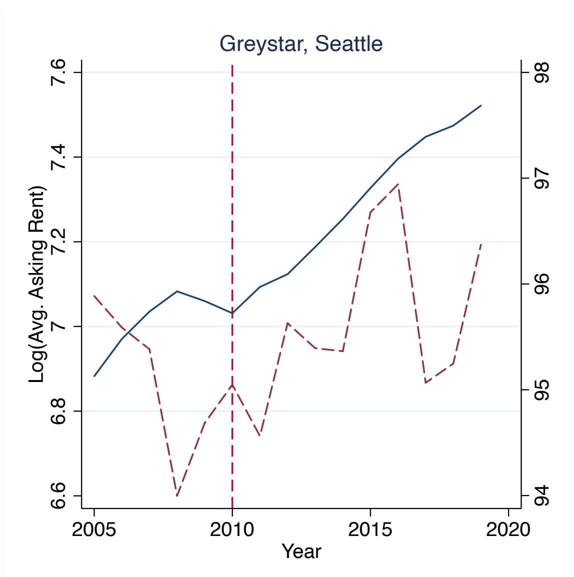
(a) Essex



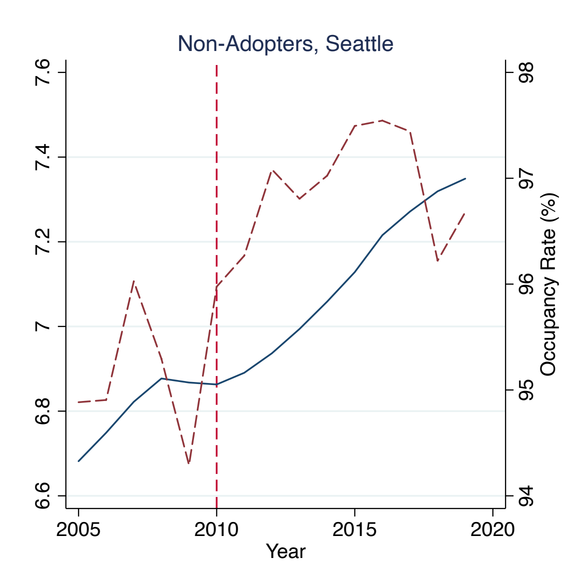
(b) Non-Adopters



(c) Greystar



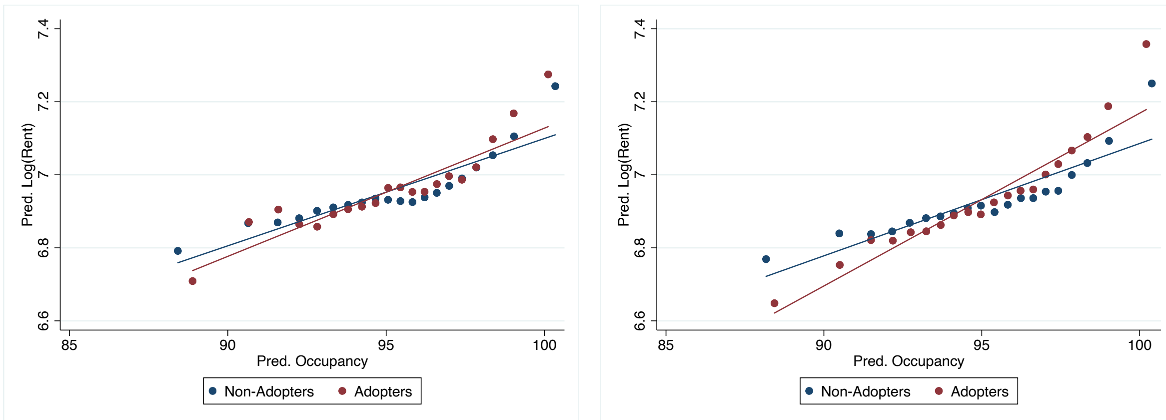
(d) Non-Adopters



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

Figure 5. Pricing Schedule of Adopters vs. Non-Adopters

(a) All Adopters vs. Non-Adoters (b) Only Adopter in the market vs. Non-Adoters



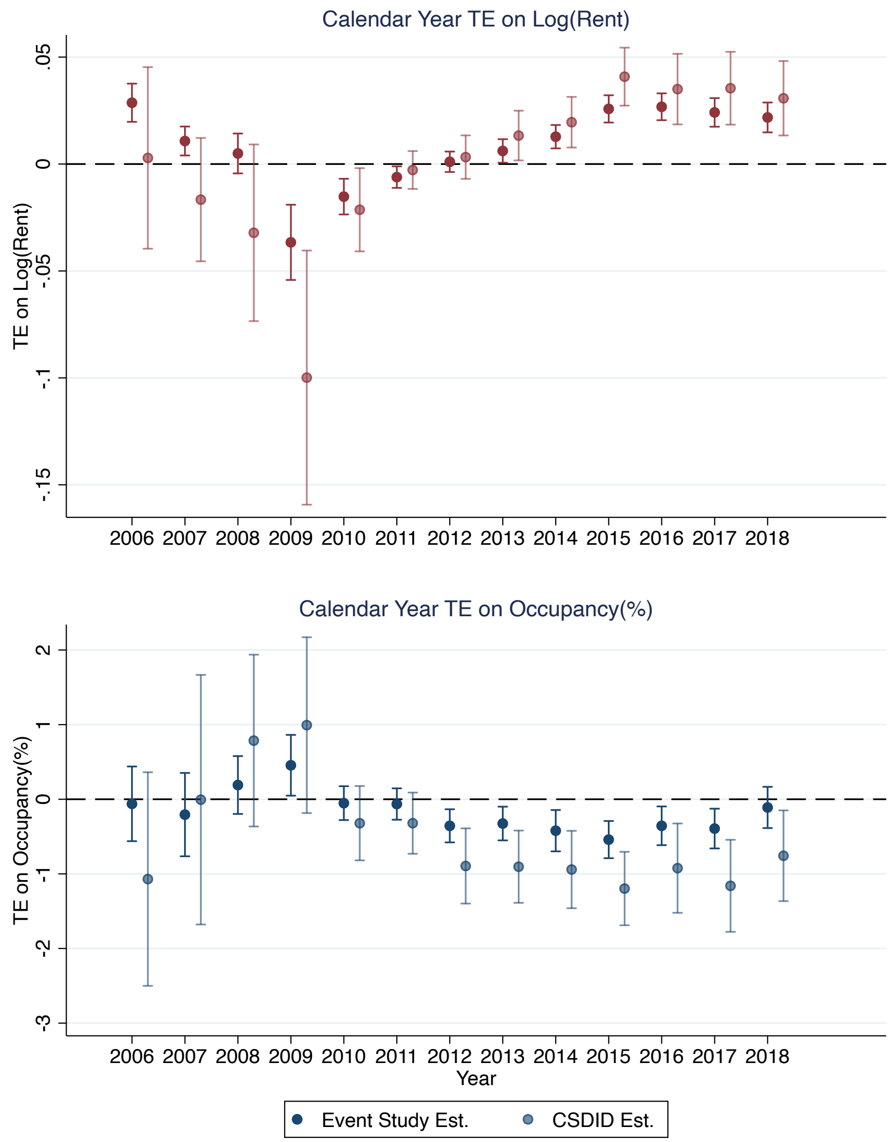
Binned scatterplots of predicted Log(Asking Rent) and occupancy as a function of market-level variables including levels and changes of housing price index, unemployment rate, net migration, average individual income, asking rents, and vacancies net of the building's. These variables are also interacted with whether the building had adopted the software or not. The "market" is the pre-defined submarket segmented by quartile of asking rents.

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.

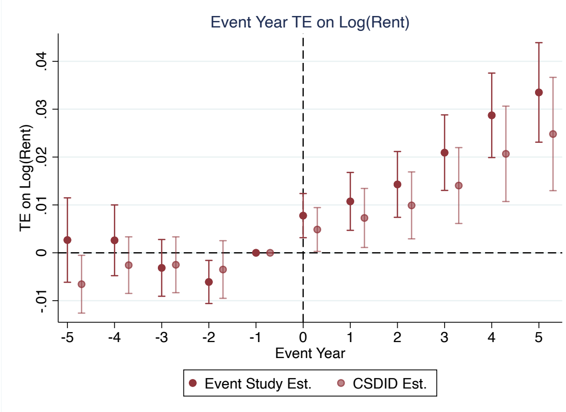
Figure 7. Calendar Year TE on Adopted Buildings



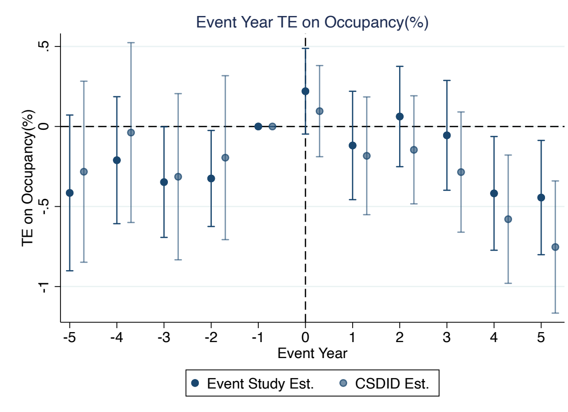
Sample restricted to buildings built before 2005. Building-level and time trend (year) fixed effects for the building's metro and pre-treatment period rent quartile are included. Controls include months of free rent offered, average concession offered in the submarket. For the Callaway and Sant'Anna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

(a) On Log(Asking Rent)



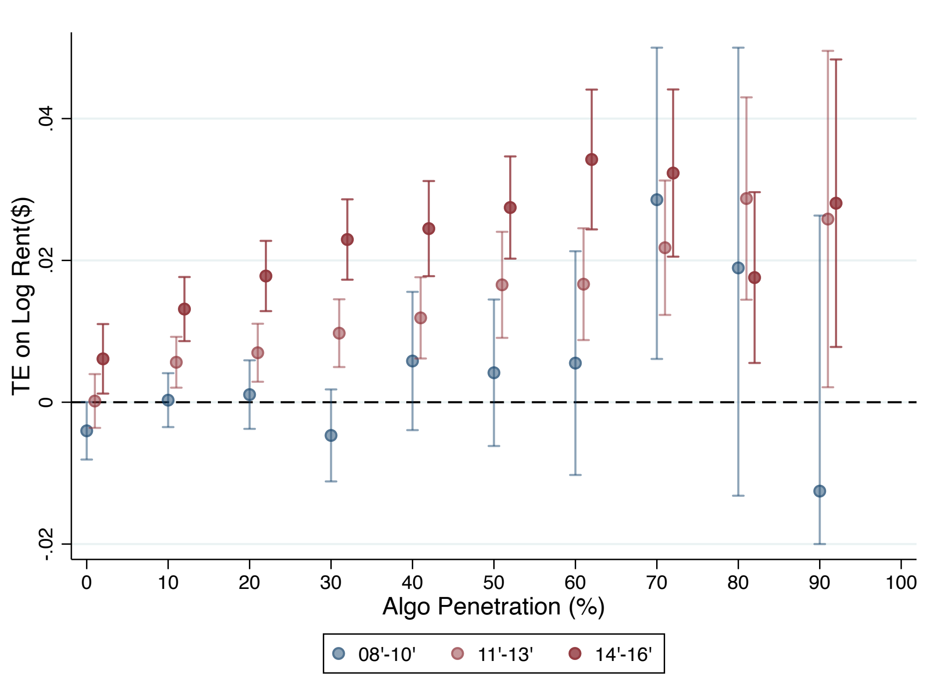
(b) On Occupancy Rate



Sample restricted to buildings built before 2005. Market is the pre-defined submarket segmented by quartile of asking rents. The event is defined as more than 30% point increase over one year in share of adopters relative to all buildings in a given market. Controls include levels and changes of housing price index, unemployment rate, net migration, average individual income, average characteristics of buildings in the market, as well as market and year two-way fixed effects. For the Callaway and Sant'Anna (2020) specification (CSDID), market level characteristic time trends are also controlled through the doubly robust estimator in addition to the fixed effects. Standard errors are clustered at the market level for both specifications.

Figure 9. Market-Level Treatment Effects by Degree of Penetration, Submarket-Rent Quartile

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



(b) $Y = Occ$

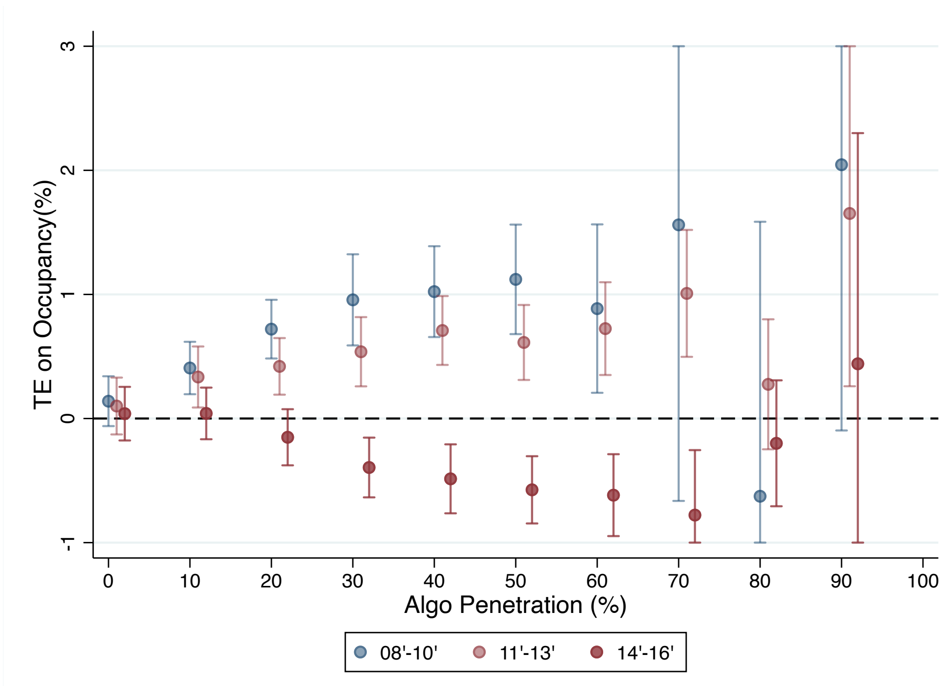


Figure 10. Market-Level Treatment Effects on Non-adopters by Degree of Penetration, Submarket-Rent Quartile

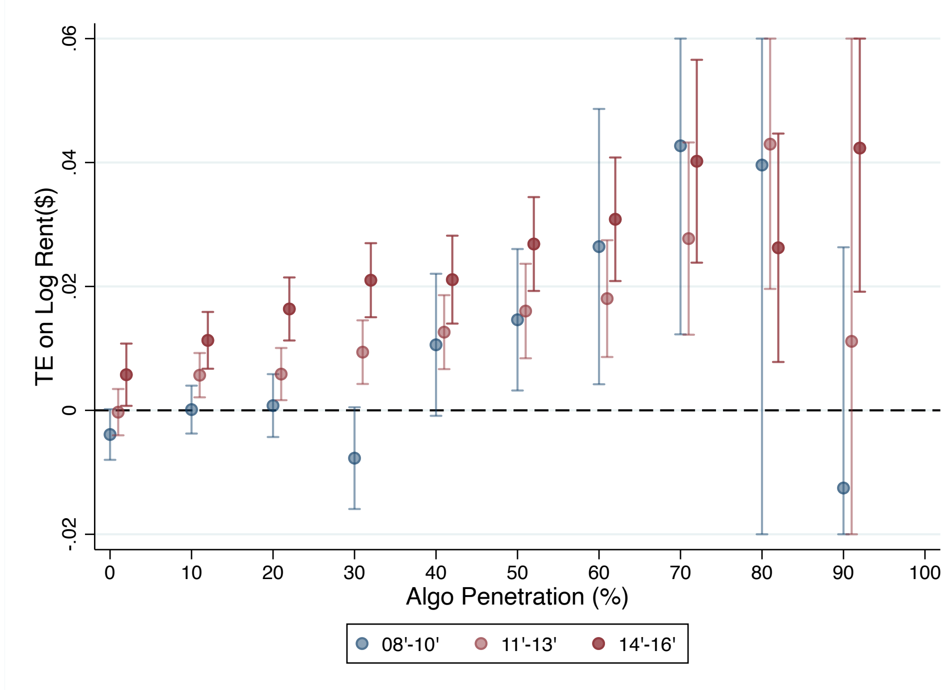
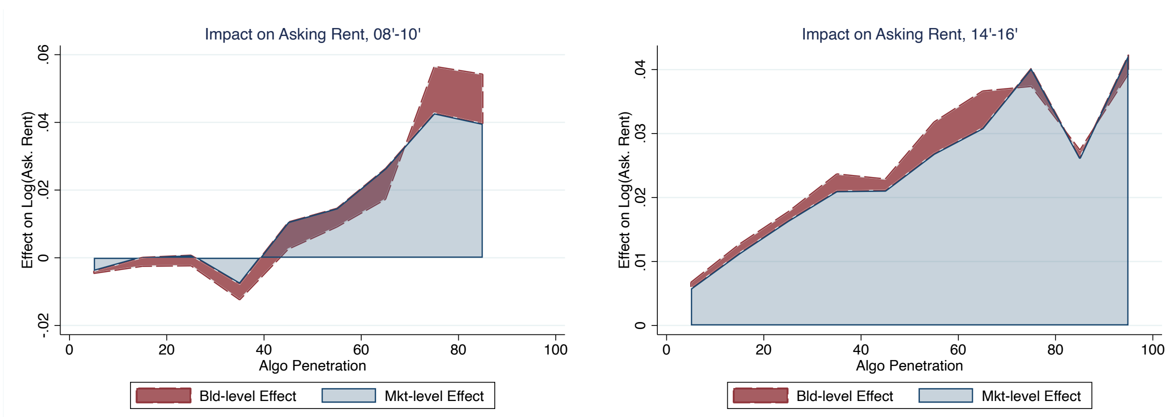


Figure 11. Decomposing Efficiency vs. Market-level Effect on Log(Asking Rent)

(a) 08-10

(b) 14-16



Sample restricted to buildings built before 2005. Market is the pre-defined submarket segmented by quartile of asking rents. For each period bin, markets are grouped into 10 absolute bins based on the percentage of adopted buildings relative to the total buildings in the market-year.

8 Tables

Table 1. REIS Summary Statistics

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

Table 2. Top Multifamily Building Management Companies

Company	Units Managed	Adoption	Adoption Date	NMHC Ranking (2019)
Greystar	320,598	1	2010	1
Lincoln Property Mgmt	123,920	1	2009	2
Pinnacle	91,977	1	2010	3
MAA	81,641	1	2007	7
Alliance Residential	74,281	1	2011	4
Equity Residential	70,979	1	2006	10
BH Management	63,650	1	2010	8
Avalon Bay	58,377	1	2008	11
Essex	54,361	1	2008	18
Camden	54,170	1	2006	21
Irvine Company	53,796	1	2010	17
Bozzuto	52,203	1	2010	12
United Dominion Realty	45,576	1	2007	30
Cortland	43,889	1	2013	26
Morgan Properties	42,527	1	2011	28
ZRS	36,594	1	2010	32
Bell Partners	35,979	1	2008	31
FPI Management	35,729	1	2011	5
Highmark Residential	32,490	0	-	19
Avenue5	32,353	1	2018	20

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

Table 3. 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)
$N_{building}$	28,092	9,124
$\text{Shr}_{building}$	75.5%	24.5%
N_{unit}	4,807,230	2,408,601
Shr_{unit}	66.6%	33.4%

Table 4. Building-level Calendar Year TE of Pricing Software Adoption

	TWFE		CSDID	
Year	Log(Ask Rent)	Occ(%)	Log(Ask Rent)	Occ(%)
2006	0.029***	-0.061	0.003	-1.069
	(0.005)	(0.255)	(0.022)	(0.730)
2007	0.011***	-0.206	-0.017	-0.006
	(0.003)	(0.285)	(0.015)	(0.853)
2008	0.005	0.191	-0.032	0.787
	(0.005)	(0.198)	(0.021)	(0.587)
2009	-0.037***	0.455**	-0.100***	0.993*
	(0.009)	(0.208)	(0.030)	(0.601)
2010	-0.015***	-0.052	-0.021**	-0.321
	(0.004)	(0.116)	(0.010)	(0.254)
2011	-0.006**	-0.064	-0.003	-0.321
	(0.003)	(0.107)	(0.005)	(0.209)
2012	0.001	-0.356***	0.003	-0.894***
	(0.002)	(0.113)	(0.005)	(0.257)
2013	0.006**	-0.325***	0.013**	-0.904***
	(0.003)	(0.115)	(0.006)	(0.247)
2014	0.013***	-0.421***	0.020***	-0.942***
	(0.003)	(0.141)	(0.006)	(0.265)
2015	0.026***	-0.541***	0.041***	-1.197***
	(0.003)	(0.127)	(0.007)	(0.251)
2016	0.027***	-0.356***	0.035***	-0.923***
	(0.003)	(0.132)	(0.008)	(0.305)
2017	0.024***	-0.392***	0.035***	-1.160***
	(0.003)	(0.136)	(0.009)	(0.315)
2018	0.022***	-0.110	0.031***	-0.757**
	(0.004)	(0.141)	(0.009)	(0.310)
N_{obs}	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 5. Slope of Price Response Curves, Adopters vs. Non-Adopters

Sample	Estimate
All	0.0057***
	(0.0010)
Num. Adopt = 1	0.0166***
	(0.0019)
1 < Num. Adopt ≤ 5	0.0157***
	(0.0012)
Num. Adopt > 5	0.0047***
	(0.0015)

Table 6. 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 7. Market-level Impact of Algorithm Penetration on Rent of Non-Adopters

Outcome: $\log(Rent^{NA})$	1st Stage	OLS	2SLS	1st Stage	OLS	2SLS
Algo. Share		0.052***	0.134***		0.033***	0.057***
		(0.006)	(0.018)		(0.005)	(0.015)
Share IV	0.804***			0.799***		
	(0.029)			(0.029)		
Submkt-Rentile FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	-	-	-
Metro-Year FE	-	-	-	Y	Y	Y
F-Stat			755.5			611.3
N	22803	22803	22803	22798	22803	22803

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

Sample restricted to never-treated units between 2008 and 2016. Controls include market average months of free rent offered, average concession offered, building characteristics, as well as levels and changes of macro variables such as household income, unemployment, net migration, and house price index. The reported F-Stat is Kleinbergen-Paap rk Wald F statistics. Standard errors are clustered at the submarket-rent quartile level for both specifications.

A Appendix Figures

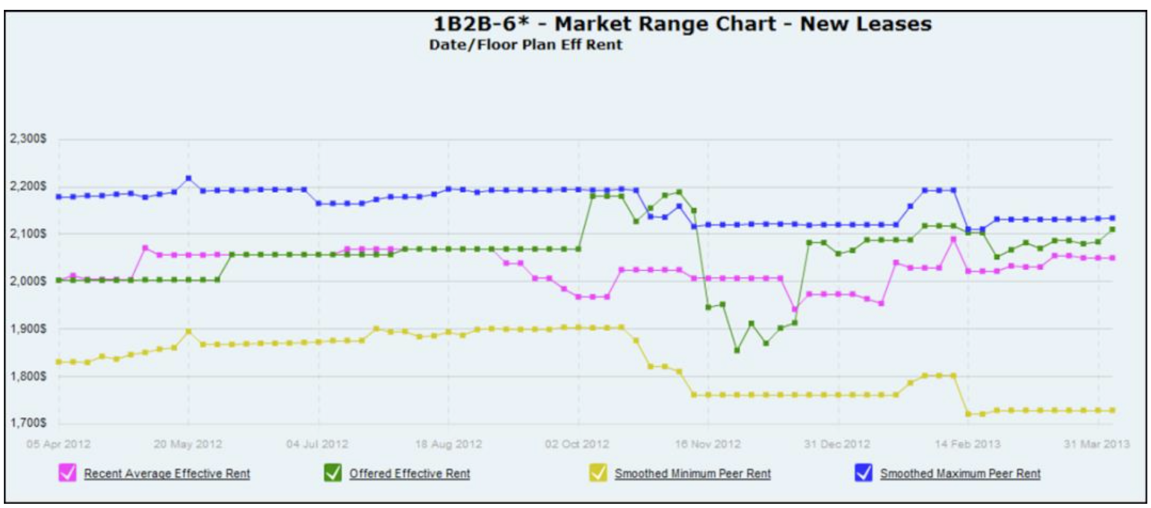
Appendix Figure A1. How Yieldstar optimizes rents

Bedroom Level Pricing

How the tool utilizes the competitive data:

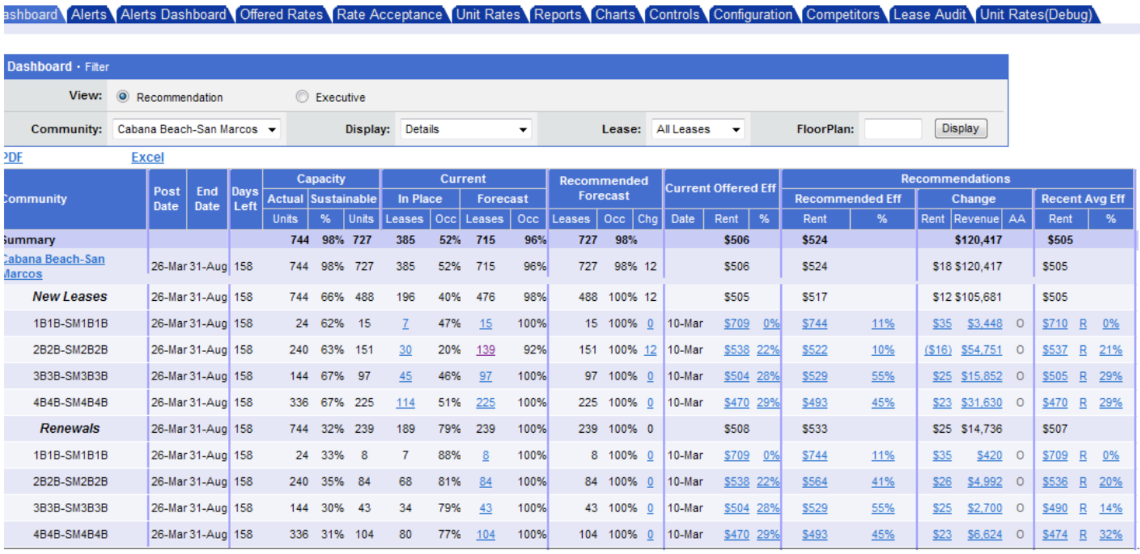
- Starts with your market survey, Operations approves the comps
- Dynamically calibrates elasticity for each bedroom type by:
  - Reading each lease and lease application for your asset
  - Determining the effective rent (net of all appropriate concessions)
  - Comparing the effective rent you achieve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

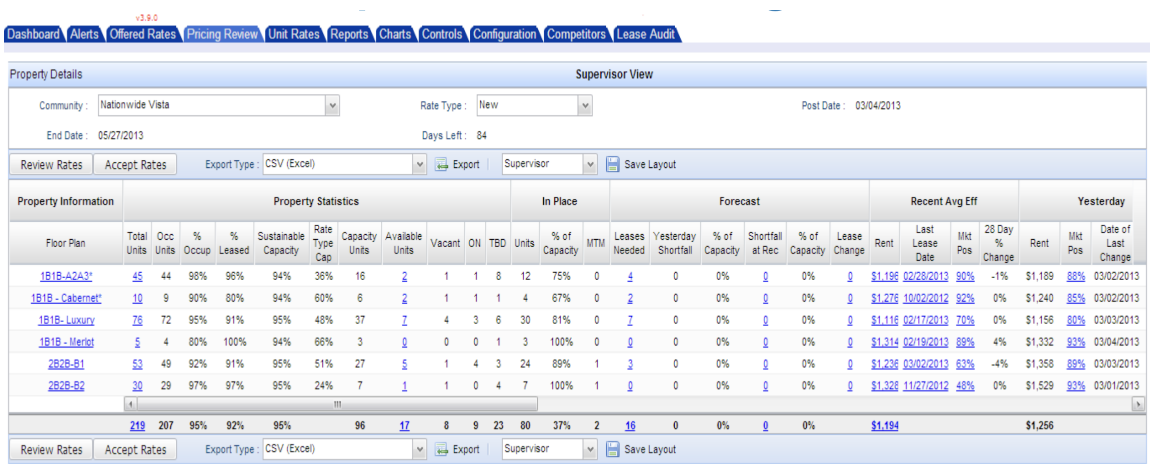


Appendix Figure A3. Manager's view of Yieldstar pricing dashboard

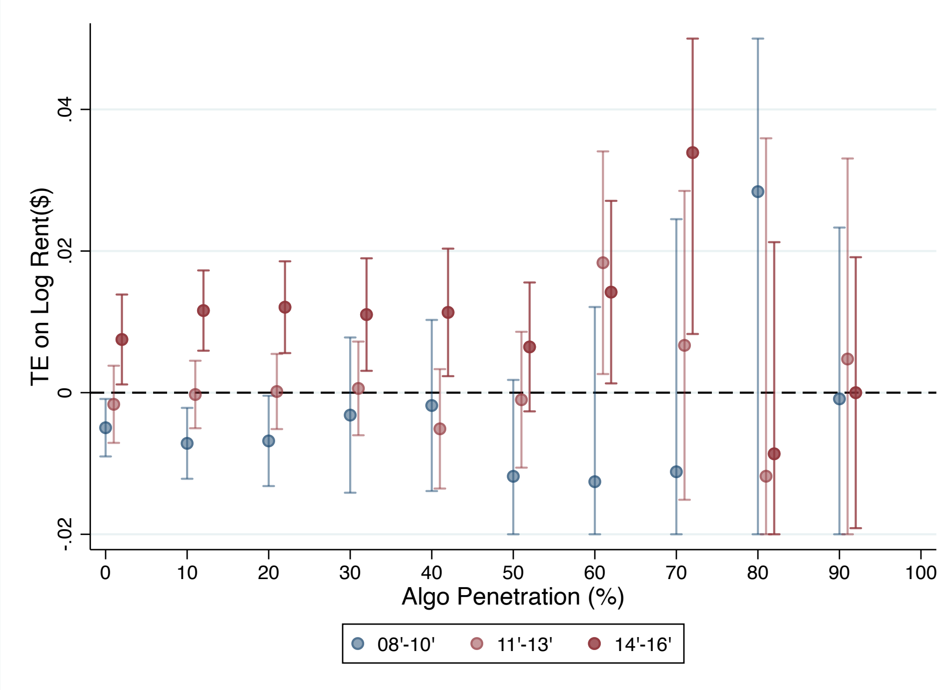
(a) Price recommendation made by Yieldstar



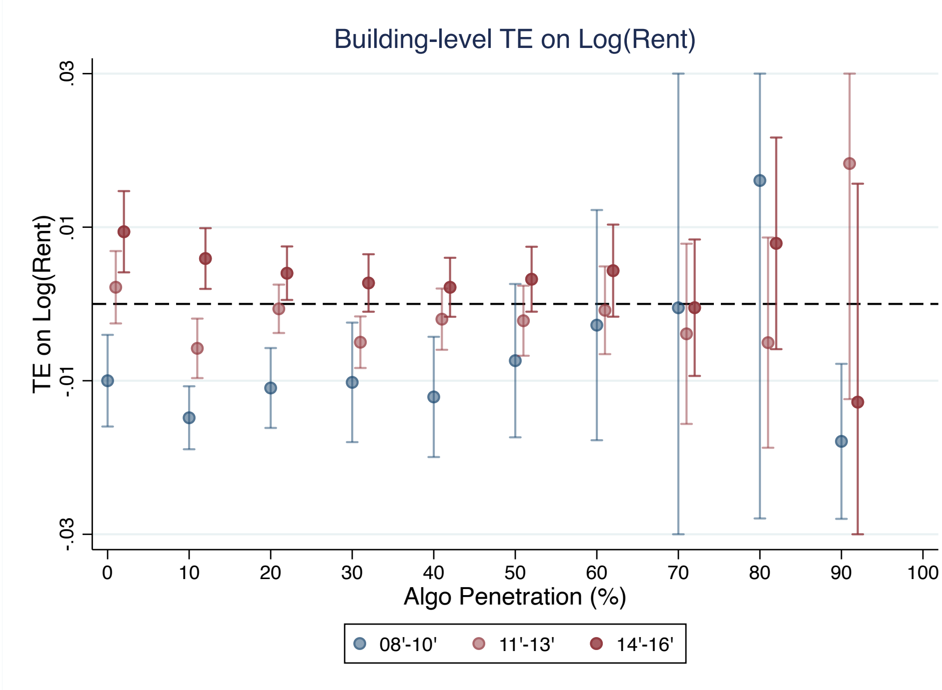
(b) Competitor data and recommendation acceptance



Appendix Figure A4. Market-Level Treatment Effects on Non-adopters by Degree of Penetration, Zip



Appendix Figure A5. Building-Level Treatment Effects by Degree of Penetration of Software, Submarket-Rent Quartile



B Appendix Tables

Appendix Table A1. Top and Bottom 5 Metro Areas by Penetration, as of 2019

Metro	Adopted Blds	Total Blds	Penetration(%)
	Top 5 Metros		
Raleigh-Durham	219	504	43
Seattle	573	1331	43
Suburban Virginia	236	580	41
Charlotte	221	546	40
Austin	283	734	39
	Bottom 5 Metros		
Columbus	44	565	8
Cleveland	14	363	4
New Orleans	8	209	4
Cincinnati	16	486	3
Milwaukee	13	400	3

Appendix Table A2. Top and Bottom 10 Submarket Areas by Penetration, as of 2019

Metro	Submarket	Adopted Blds	Total Blds	Penetration(%)
		Top 10 Submarkets		
Orange County	Irvine	72	78	92
Orange County	Newport Beach	14	17	82
Austin	Far Northwest	35	47	74
Fort Lauderdale	Plantation	20	27	74
Orange County	Mission Viejo	34	48	71
Denver	Arapahoe County	15	22	68
Austin	Near South Central	17	25	68
Dallas	Central Dallas	67	101	66
Seattle	Redmond	43	65	66
Charlotte	Carmel	33	50	66
		Bottom 10 Submarkets		
Cincinnati	North	0	25	0
St. Louis	Airport/I-70	0	43	0
Memphis	Frayser	0	8	0
Milwaukee	Greenfield/Greendale	0	54	0
New Orleans	Southeast Orleans	0	10	0
Cincinnati	Highway 27/127	0	37	0
New Orleans	Kenner	0	13	0
New Orleans	Jefferson/River Ridge	0	23	0
Kansas City	North Kansas City	0	31	0
Fort Worth	Central Arlington	0	61	0