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Do Wall Street Landlords Undermine Renters' Welfare?

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We examine the recent rise of institutional investment in the single-family home rental market and its implications for renters' welfare. Using institutional mergers to identify local exogenous variation in institutional landlords' scale and market share, we show that rents increase in neighborhoods where both merging firms owned properties (i.e., overlapped neighborhoods) relative to other nonoverlapped neighborhoods. Meanwhile, the crime rate also significantly decreases in overlapped neighborhoods after mergers. Our findings suggest that while institutional landlords leverage their market power to extract greater surplus from renters, they also improve the quality of rental services by enhancing neighborhood safety. (*JEL* G20, R30)

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The foreclosure crisis from 2008 to 2010 forced millions of households out of their homes and created a new investment opportunity for institutional investors. Private equity firms, such as the Blackstone Group and Starwood Capital, acquired a large number of foreclosed properties through auctions, direct purchases from banks, and local brokers and refashioned them as rental properties. Single-family rentals (SFRs) have since become widely recognized as a new asset class for institutional investment (Sultan 2015). The emergence of institutional ownership in single-family homes is accompanied by consolidation through mergers and acquisitions. On August 10, 2017, the two publicly listed companies Invitation Homes and Starwood Waypoint merged to become the nation's largest owner and operator of SFRs, with a portfolio of 82,000 homes (BusinessWire 2017).

The unprecedented scale of the rise of institutional investors, as well as of the market share in the SFR business, raises questions about how institutional landlords affect renters' welfare. In fact, the business practices of institutional landlords have attracted media scrutiny and public outcry. For example, activist groups, such as the ACCE Institute, Americans for Financial Reform, and the Public Advocate, have criticized "Wall Street landlords" for raising rents and fees, poorly maintaining properties, and ruthlessly evicting tenants.[1] Despite reasonable concerns about the business practices of institutional landlords, whether they undermine renters' welfare is not entirely obvious (e.g., Eckbo 1983, 1992; Focarelli and Panetta 2003; Hatch and Johnson 2002).

In this paper, we study the effect of institutional landlords on rent and neighborhood quality. We first develop a simple model to show that having a large landlord in a neighborhood affects renters through two channels: A large institutional investor could utilize its market power to extract greater surplus from renters by charging a higher rent (i.e., the *market power channel*). Alternatively, it achieves economies of scale that enhance neighborhood quality compared with mom-and-pop landlords (i.e., the *scale economy channel*) (Bers and Springer 1997; Yang 2001; Ambrose et al. 2000).

We then empirically examine the causal effect of institutional ownership on rent and neighborhood quality using the three largest mergers of institutional SFR investors from 2015 to 2017. A merger creates a discrete jump in the scale and market share of the merged firm in local markets where both the acquirer and the target own properties prior to the merger. Our identification assumption is that the degree of market overlap in each local market and the resultant shift in landlord scale and market share postmerger are plausibly exogenous to local economic conditions, and this overlap and shift causally affect neighborhood rental price and quality through both the scale economy channel and the market power channel. Put differently, after controlling for observable characteristics, any systematic difference in the changes of rent and neighborhood quality of markets with low market overlap and high market overlap should be due to

[1] See, for example, Americans for Financial Reform (2018).

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synergistic gains from the merger, to which only the latter group is exposed (treatment effect).

The main challenge to our identification assumption is that mergers do not occur randomly: an acquirer may choose a specific target because the merged company expects to enjoy higher rent growth in neighborhoods with high overlap relative to other neighborhoods within the portfolios of the target and acquirer (selection effect). We extract information on the geographic distribution of each institutional landlord's properties and construct a measure for a neighborhood's exposure to mergers based on the degree of market overlap between acquirer and target properties. We then perform a neighborhood-level difference-in-differences (DiD) analysis around mergers to study the changes in renters' welfare and investigate the extent to which the results are due to the treatment or the selection effect.

We find that, in the year following completion of the mergers, neighborhoods with greater overlap of homeownership by the merging firms (we define "merging firms" as the acquirer and target) experienced an increase in rent. This result holds after controlling for all time-varying geographic characteristics at the county level and time-invariant neighborhood characteristics. Rents increased by 0.51% more for neighborhoods in which the merged firms gained more than five properties after the merger compared with the rent increases of nonoverlapped neighborhoods that are *also* covered by the merging firms. Preexisting trends, which themselves might be driven by other confounding factors, cannot explain such an increase in rent.

Next, we examine the effect of institutional ownership on neighborhood quality in terms of the crime rate. Our model predicts that institutional landlords can be incentivized by a higher scale and market share to internalize the cost of neighborhood safety and overcome the free-rider problem of public good provisions (e.g., Favara and Giannetti 2017; Chod, Lyandres, and Yang 2019). The results of our empirical analysis show that a high concentration of institutionally owned SFRs induced by mergers indeed leads to an improvement in neighborhood safety. Specifically, after completion of institutional mergers, neighborhoods in which merged firms gained more than five properties experienced a 5.23% decrease in the number of criminal incidents in the following year relative to other neighborhoods covered (but not overlapped) by merged firms. Reductions in crime include a 3.03% decrease in burglaries, 4.64% decrease in thefts, and 3.43% decrease in vandalism. Reductions in the number of reported criminal incidents cannot be explained by any preexisting trends due to other long-term confounding factors, such as gentrification or demographic shifts. Overall, our results largely support the idea that a higher scale and market share allow institutional landlords to internalize the cost of neighborhood safety.

Next, we explore the extent to which our results are driven by the ex ante rent growth expectations of acquirers when they select a target. Our empirical setting provides a unique opportunity to understand the magnitude of such selection

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effects. Note that exposure to the treatment in our empirical setting depends on not only the number of target properties in a neighborhood but also the number of acquirer properties in the same neighborhood. If the effect we document is mainly driven by a selection effect, we should expect a significant change in the postmerger rent and crime rate in areas with a large number of target properties, *regardless of the degree of overlap with the acquirers' portfolios*. Thus, we surmise that the number of target properties in a neighborhood is a metric that captures rent growth expectations. Our results indicate that increases in rent and decreases in the crime rate following the mergers are more correlated with the size of targets' portfolios in overlapped neighborhoods than in nonoverlapped neighborhoods, suggesting the causal effect of mergers we document is not mainly driven by selection.

We also provide a variety of evidence on the underlying mechanisms through which landlord mergers reduce neighborhood crime. First, we find that the hiring of private security guards ramped up in overlapped areas. Second, an analysis of detailed satellite data shows more streetlight density at nights in overlapped neighborhoods postmergers, which can lead to lower crime at nights. Third, landlord mergers appear to reduce resident turnover, thereby stabilizing the affected neighborhoods. Fourth and finally, we observe a 4.4% increase in the eviction rate in overlapped neighborhoods relative to the sample mean. These results largely support the notion that, while institutional landlords charge higher rents, they are also significantly enhancing neighborhood safety.

We also show evidence that landlord mergers increase neighborhood rent through the market power channel. Specifically, we find that both postmerger rent increases and the reduction in crime are weaker in the presence of competition from rival landlords in the same neighborhoods. Moreover, using property-level rental listing data, we find that properties owned by the merged landlords, especially those owned by the acquirers, charged higher rents relative to those of other single-family rental homes in the same neighborhood. This within-neighborhood variation in rent cannot be explained by the reduction in crime at the neighborhood level. In addition, we show evidence of an increase in the vacancy rate in overlapped neighborhoods, consistent with the idea that landlord market power reduces demand for rental properties postmergers.

When we explore the effect of mergers on house prices, we observe increases in home prices in overlapped neighborhoods in the medium run, that is, 2 years after completion of the mergers, but no significant increases in the immediate years following the mergers. Further, when decomposing neighborhood rent into a quality-predicted and a residual component (e.g., Gete and Reher 2018), we find that only the residual rent increased significantly within 1 year after mergers. These results suggest that the observed increase in rent within 1 year postmerger is likely driven by the market power channel, whereby merged landlords can charge higher listed rents immediately, while neighborhood quality improvement more gradually affects home rental prices and selling prices.

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Finally, we find that both overlapped and nonoverlapped neighborhoods covered by merged firms saw a significant increase in rent and decrease in the crime rate, relative to other neighborhoods in the same counties where neither of the merged firms owned properties, although the difference is more pronounced for overlapped neighborhoods. This result implies that rental price increases and the increased quality of *all* neighborhoods within institutional landlords' portfolios following institutional mergers are in line with both the market power and scale economy channels. Overall, our findings suggest that while institutional landlords may extract higher surplus from renters, they also offer better services compared with mom-and-pop landlords by enhancing neighborhood quality.

Our study informs the public debate about the impact of institutional landlords on consumer welfare. Since the Great Recession, the SFR market has grown exponentially (Gete and Reher 2018). In January 2017, in response to the sector's fast expansion, the Federal Housing Finance Agency (FHFA) approved Fannie Mae and Freddie Mac to operate a pilot program to purchase and/or guarantee securitized loans issued by investors in the SFR business. This initiative quickly drew controversy and criticism from fair housing advocates and legislators. One major concern is that large investors will make SFR less affordable and offer housing of questionable quality. In August 2018, amid criticism of the program, the FHFA decided to end participation of the government-sponsored enterprises (GSEs) in the SFR market. The FHFA pointed out that "it is premature to allow the Enterprises to enter this portion of the SFR market because the effects of their participation on rent growth, long-term affordability, for sale assets, and homeownership is insufficiently understood without significantly more extensive research and analysis."[2] Our findings, based on a comprehensive analysis of nationwide data with careful identification, offer important insights into the impact of institutional SFR ownership on rent growth and community safety. These results can aid policy makers in evaluating whether to allow GSEs to participate in the SFR space in the future.

Our findings also contribute to the literature on the postcrisis recovery of U.S. real estate markets (e.g., Agarwal et al. 2017a, 2017b; Di Maggio et al. 2017; Flynn, Ghent, and Tchistyi 2020; Giacoletti and Parsons 2022; Hsu, Matsa, and Melzer 2018; Mian, Sufi, and Trebbi 2015; Piskorski and Seru 2018). In particular, our study adds to the recent line of research on the institutional SFR market that emerged as a consequence of the foreclosure crisis, which permanently changed the landscape of home rental markets (Smith and Liu 2017; Allen et al. 2018; Mills, Molloy, and Zarutskie 2019; D'Lima and Schultz 2019; Ganduri, Xiao, and Xiao 2019; Lambie-Hanson, Li, and Slonkosky 2019). Our evidence shows that institutional investors filled a void in

2 See the Federal Housing Finance Agency (2018).

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a distressed housing market when repositioning foreclosed properties as rental properties. Traditional mom-and-pop landlords face significant operational frictions, such as high fixed costs of property management and the risk of high renter turnover and vacancy. By contrast, institutional landlords can exploit scale economies and reduce uncertainty through diversification and data analytics.[3] Thus, institutional investment in SFRs became a viable solution in the distressed housing market.[4]

1. Institutional Background

Mom-and-pop investors historically and still do dominate the SFR market. The wave of institutional investment in single-family homes as rental properties started in late 2011, when a large number of bank-owned foreclosed homes, also known as real-estate owned (REO), became available for sale at deep discounts, while the financial crisis limited the credit supply available to individual investors. Since then, the institutionalization of SFRs has grown exponentially. According to a report by Amherst Capital, institutional investors owned 240,000 single-family homes in the United States as of January 2019 (Bordia 2019). Recent studies, such as Allen et al. (2018), Ganduri, Xiao, and Xiao (2019), and Mills, Molloy, and Zarutskie (2019), have shown that institutional investment has contributed to the value recovery of distressed properties.

Mom-and-pop SFR investors typically own one or several income properties and act as the property managers themselves or hire an agent to handle property maintenance and/or locating tenants. By contrast, institutional SFR investors operate on a substantially larger scale, owning tens of thousands of properties at the same time and operating the rental business by employing their own property management divisions. Hence, institutional landlords have a cost advantage over individual investors with economies of scale (Bers and Springer 1997; Yang 2001; Ambrose et al. 2000). Furthermore, while institutional ownership accounts for only a minor portion of the overall SFR market, it is highly concentrated in several regional markets that experienced a large number of foreclosed homes after the Great Recession.[5] As a result, institutional investors have amassed a disproportionate market share in specific regions.

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[3] For instance, Main Street Renewal, one of the largest SFR investors, uses computer models for house hunting and cost/revenue predictions (Tully 2019).

[4] Hence, our findings match those from recent studies of the SFR market. For example, Eisfeldt and Demers (2015) show that rental yields and house price appreciation equally contribute to returns on SFR investments. Mills, Molloy, and Zarutskie (2019) suggest that the rise of institutional SFR investors in the foreclosure crisis can be explained by the large supply of properties for sale, tight credit conditions, and a decrease in costs of acquiring and managing properties, thanks to technological advances.

[5] A study by the Joint Center for Housing Studies of Harvard University shows that the national rental stock as of 2016 comprises 47.1 million units, 39% of which were single-family homes (President and Fellows of Harvard College 2017).

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Apart from purchasing homes through auctions, as well as direct purchases from banks and local brokers, institutional SFR investors quickly expanded by merging with other institutional rivals. Our study focuses on the three largest mergers in the institutional SFR market and how they affected neighborhoods.

A vast literature in finance and economics studies the implication of horizontal mergers. For the most part, the consensus in the literature is that horizontal mergers can affect consumer welfare through two channels. First, horizontal mergers lead to an efficiency gain through economies of scale and asset complementarities. Extensive evidence indicates that horizontal mergers allow firms to allocate resources efficiently.[6] In the case of home rental markets, mergers of institutional landlords could achieve efficiency gains through cost reductions and risk diversification. Mergers could also enhance service quality by combining two companies' managerial expertise and technologies. Second, horizontal mergers can have an anticompetitive effect whereby merged firms gain bargaining power over suppliers and customers.[7]

In fact, all institutional landlords in our study quote economies of scale and competition as the main considerations for their decision to merge. For example, the 2017 proxy statement by Invitation Homes states that its merger with Starwood Waypoint Homes would create "a preeminent operational and management platform combining (1) the cutting-edge technology and service platforms of INVH and SFR and (2) the premier management teams of INVH and SFR," suggesting efficiency gains through the enhancement of managerial know-how and technology.[8] The 2015 proxy statement by Starwood Waypoint Residential Trust states that "the Merger will provide a number of significant strategic and financial opportunities, including . . . the scale and density to optimize operations and reduce operating costs, . . . (and) anticipated enhanced competitive position as a result of the Combined Company's size and scale."[9]

In our empirical analysis, we examine the effect of institutional mergers on neighborhood quality in terms of crime rate. Landlords can take various measures to improve neighborhood safety. For example, the "broken windows" theory supposes that improving property and neighborhood facilities can deter criminal activities (Wilson and Kelling 1982). Thus, landlords can deter neighborhood crime by ensuring doors and windows are in good working order, installing security systems, and maintaining curbside appearance and outdoor lights. For example, Invitation Homes installs home automation systems in its properties that allow renters to control door locks remotely and monitor activities within the homes (BusinessWire 2017). Furthermore, landlords can be

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[6] See, for example, Cornaggia and Li (2019), Erel, Jang, and Weisbach (2015), Maksimovic and Phillips (2001), Maksimovic, Phillips, and Prabhala (2011), and Tate and Yang (2016).

[7] See, for example, Fairhurst and Williams (2017), Fee and Thomas (2004), Focarelli and Panetta (2003), Greene, Kini, and Shenoy (2017), Kim and Singal (1993), Shahrur (2005), Shen (2018), and Singal (1996).

[8] See https://www.sec.gov/Archives/edgar/data/1579471/000119312517310310/d398351ddefm14a.htm.

[9] See https://www.sec.gov/Archives/edgar/data/1579471/000119312515376853/d45844ddefm14a.htm.

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selective about potential renters by performing background checks and charging higher rents. Our conversation with one of the institutional SFR investors also revealed that they employ designated customer service centers and private security guards for the most densely owned neighborhoods.

Mergers of institutional landlords could enhance neighborhood safety for several reasons. First, some direct costs of ensuring neighborhood safety, such as those for hiring maintenance and security staff, are highly localized and average costs decrease with ownership density, whereas the benefits of investing in neighborhood safety increase with ownership density. Hence, institutional landlords are more likely to internalize the cost of safety measures after mergers. Second, maintaining neighborhood safety requires coordination across residents and owners, and a higher ownership concentration can reduce the difficulty of coordination. For example, a large landlord can take the initiative on neighborhood safety issues in the homeowners association (HOA). Third, maintaining neighborhood safety requires managerial know-how that may not be shared with rival landlords but can be transferred through mergers. Fourth, as institutional landlords have argued in their public filings, lower competition can help enhance the retention rate of renters. For example, the 2016 10-K by Colony Starwood Homes states that "competing properties may be newer, better located and more attractive to residents. Potential competitors may have lower rates of occupancy than we do or may have superior access to capital and other resources, which may result in competing owners more easily locating residents and leasing available housing at lower rental rates than we might offer at our homes. Many of these competitors may successfully attract residents with better incentives and amenities, which could adversely affect our ability to obtain quality residents and lease our single family properties on favorable terms. This competition may affect our ability to attract and retain residents and may reduce the rental rates we are able to charge."[10] Higher tenant retention can enhance neighborhood stability, which has been shown to reduce crime rates (e.g., Boggess and Hipp 2010). These reasons suggest that both (direct and indirect) fixed costs and frictions arising from competition may hinder landlords from making private investments in neighborhood safety. Therefore, only a sufficiently large landlord may profit from making such investments.

2. A Simple Model of Rental Market Competition

In this section, we outline a simple model to formalize the mechanisms through which the creation of a large landlord through a merger may affect average rent and neighborhood safety. The Internet Appendix provides the details.

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10 See https://www.sec.gov/Archives/edgar/data/1579471/000156459017002843/sfr-10k_20161231.htm.

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2.1 A two-house model

Consider a neighborhood consisting of two identical houses, 1 and 2. Let $r_i \in [0,\infty)$ denote the rent for house $i \in \{1,2\}$. Let $s \in \{H,L\}$ denote the status of neighborhood security. If any landlord hires a patrol unit, $s=H$, and the crime rate in the neighborhood lowers. Otherwise, $s=L$. The demand for house $i \in \{1,2\}$ is given by $D(r_i,r_j,s):[0,\infty)\times[0,\infty)\times\{H,L\}\rightarrow[0,1]$, where $j \in \{1,2\}\backslash\{i\}$ denotes the other house. Thus, the demand for a house (D) is a function of the house's own rent (r_i), the rent charged by the other house (r_j), and the status of neighborhood security. For the purpose of tractability, we impose the following specific functional form for the demand function of house i:

$$D(r_i,r_j,s)=\begin{cases} 1 & \text{if } r_i \leq \underline{r}_s+\alpha r_j, \\ \frac{\overline{r}_s-r_i+\alpha r_j}{\overline{r}_s-\underline{r}_s} & \text{if } \underline{r}_s+\alpha r_j < r_i < \overline{r}_s+\alpha r_j, \\ 0 & \text{if } r_i \geq \overline{r}_s+\alpha r_j, \end{cases} \tag{1}$$

for $i,j \in \{1,2\}$, $i \neq j$, and $s=\{H,L\}$. This specific functional form guarantees that demand is continuous and takes values from $[0,1]$, which captures the probability that the house gets rented. Thus, $1-D$ can be considered to be the house's vacancy rate. Parameter α captures the impact of the rent charged by the other house relative to its own rent on the demand for a house. First, we assume $\alpha>0$, so that the demand for a house is decreasing with its own rent and increasing with the rent charged by the other house. Second, we assume $\alpha<1$, so that the demand elasticity with respect to the rent charged by the other house is lower than the elasticity with respect to its own rent. When a house's own rent is sufficiently lower than the relative rent charged by the other house ($r_i < \underline{r}_s+\alpha r_j$), the house can be rented out for sure ($D=1$). When a house's own rent is sufficiently higher than the relative rent charged by the other house ($r_i > \overline{r}_s+\alpha r_j$), there is no chance it can be rented out ($D=0$). $\underline{r}_s$ and $\overline{r}_s$ can be interpreted as renters' minimum and the maximum willingness to pay for a house given that the rent of the other house is zero, respectively. Assume $\underline{r}_L < \underline{r}_H$ and $\overline{r}_L < \overline{r}_H$, which capture the fact that a secured neighborhood is more desirable.[11]

Suppose two different landlords own two houses. The two landlords first independently decide whether to hire a patrol unit (at a fixed cost) and then engage in rent competition. In the Internet Appendix, we characterize the subgame perfect Nash equilibria. We show the following results: (1) The equilibrium rent is higher when the security level of the neighborhood is higher. (2) When the cost of hiring a patrol unit is sufficiently high, no landlord would

[11] Note that in the current formulation, even if r_i is very high, the demand for house i still can be positive as long as r_j is sufficiently high as well. In reality, a house may never be rented if its own price exceeds a certain threshold, regardless of the rents of the other houses in the neighborhood. Hence, alternatively, one can assume that $D(r_i,r_j,s)=0$ if $r_i > \overline{r}_s$. Such an assumption would make the demand function not continuous, but it would not affect our main results qualitatively.

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have an incentive to hire one, resulting in an unsafe neighborhood with low rents. (3) When the cost of hiring a patrol unit is sufficiently low, one of the landlords will hire a patrol unit, whereas the other freerides, resulting in a safer neighborhood with higher rents.[12]

Suppose now the two landlords merge to become a large landlord in the neighborhood. In the Internet Appendix, we show that both the security level and average rent in the merger case are higher than those in the two-landlord case when the cost of hiring a patrol unit is within an intermediate range. Two channels drive this result. First, the merger enhances market concentration, thereby reducing the competition in rent between the two houses (i.e., the market power channel). Second, the merged landlord extracts greater benefit from neighborhood safety because of its larger scale; thus, it is more willing to internalize the cost of hiring a patrol unit (i.e., the scale economy channel). Higher neighborhood safety in turn further increases average rent in the neighborhood.

Note that the demand for rental houses (or the vacancy rate) may go in either direction postmerger. This is because the merger affects housing demand through two opposing channels. On the one hand, the higher rent charged by the merged landlord due to enhanced market power reduces housing demand. On the other hand, improved neighborhood safety due to the merged landlord's costly investment enhances housing demand. Hence, which channel is the dominant driving force for the postmerger vacancy rate is an empirical question.

2.2 Discussion

What if the merged landlord still has to compete with a third landlord? In the Internet Appendix, we consider this possibility by extending the two-house model to a three-house model in which two of the landlords, each of whom owns one house, merge to become a new landlord, whereas the third landlord remains with one house.

The analysis of the three-house model leads to several predictions. First, the level of market concentration postmerger differs across the two models because the merged landlord has exclusive market power in the two-house model, whereas it still faces competition in the three-house model. Hence, the presence of competition from a third landlord leads to a smaller postmerger increase in average rent relative to that in the two-house model. Second, there exists a region for the cost, in which the merged landlord hires a patrol unit in the two-house model, but not in the three-house model. This result implies that, without competition, the merged landlord is more likely to profit from

[12] In the model, we assume that as long as one landlord hires a patrol unit, the status of neighborhood security is $s = H$. Hence, having two patrol units in the neighborhood is socially wasteful, and each landlord has an incentive to freeride on the other. One can alternatively assume that marginal increases seen in rent by adding a patrol unit is decreasing in the number of existing patrol units, or the two landlords can bargain over the division of the cost of hiring one patrol unit. Nevertheless, none of these alternative assumptions will give qualitatively different results, albeit they are arguably more realistic. Hence, for simplicity, we stick with the current assumption.

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making an investment in neighborhood safety. Hence, in addition to economies of scale, market power can also mitigate the free-rider problem in the private provision of public goods on the margin. Third, the merged landlord would charge a higher rent than the third landlord because it has a greater market share. Thus, the market power channel suggests that merged landlords would experience greater rent growth than their local rivals within a neighborhood postmerger.

Our model generates the following empirical predictions about the effect of institutional mergers:

Hypothesis 1a (H1a). After completion of institutional mergers, average rent in overlapped neighborhoods increases relative to that in nonoverlapped neighborhoods.

Hypothesis 1b (H1b). The postmerger increase in average neighborhood rent is weaker with greater competition in the neighborhoods.

Hypothesis 1c (H1c). Within overlapped neighborhoods, the postmerger increase in rent charged by merged landlords is greater than that charged by other landlords in the same neighborhoods.

Hypothesis 2a (H2a). After completion of institutional mergers, the crime rate in overlapped neighborhoods decreases relative to that in nonoverlapped neighborhoods.

Hypothesis 2b (H2b). The postmerger reduction in the neighborhood crime rate is weaker with greater competition in the neighborhoods.

We note that a number of real-world considerations cannot be concisely incorporated into the model. For example, the reduction in crime can occur as a result of mergers for reasons other than direct investments by the merged landlords. Less landlord competition could reduce renter turnover, which may also contribute to an improvement in resident stability and neighborhood safety. Second, our model does not consider the endogenous choice of a target landlord, which could be motivated by the expected rent growth of the target's portfolio and/or the expected synergistic gains in scale and market power. This would bias toward finding a rent increase and a reduction in crime in the neighborhoods where mergers take place. We discuss the assumptions and limitations of our empirical setting in detail in Section 4.1.

3. Data

3.1 Data source

Our main analysis relies on three sources of data: (1) detailed property-level data, (2) neighborhood-level rental data, and (3) census-tract-level crime reports. In this section, we describe these sources and outline our sample construction.

We obtain data on real estate transactions from Zillow's Assessor and Real Estate Database (ZTRAX). ZTRAX contains detailed property transaction

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information, such as transaction date, sales price, identities of buyers and sellers, and foreclosure information. ZTRAX also includes detailed information about each property collected from local tax assessors, such as property type, full street address, year built, lot size and building area, and the number of bedrooms and bathrooms. We obtain detailed assessors' data as of 2016 and merge these data with the recorded transactions based on a unique parcel ID for each property.

We use the Zillow Rent Index (ZRI) for neighborhood rental price. *ZRI* is the median of the *Rent Zestimate* for individual properties of each housing type in a given area, and the *Rent Zestimate* is predicted based on a machine learning model using home characteristics and actual rental listings in the area. Thus, the *ZRI* has smoothed out idiosyncratic fluctuations in actual market rent due to variations in property characteristics and seasonality. Furthermore, the ZRI is estimated based on market-listed rent rather than the actual contract rent, which typically occurs with a delay. Therefore, changes in the *ZRI* reflect the immediate pricing decisions made by local landlords. We collect the *ZRI* for single-family homes at the Zillow-defined neighborhood level. Using the boundary shapefile provided by Zillow, we match the acquirers and targets' properties with the corresponding Zillow neighborhoods. Further details about the construction of *ZRI* are available on Zillow's (2019) website. For brevity, we will refer to Zillow neighborhoods as neighborhoods hereafter.

We scrape the data on individual criminal incidents from the LexisNexis Community Crime Map.[13] We aggregate geocoded criminal incidents to the census tract level for our analysis. Doing so results in a larger sample than for the rent analysis because the census tract sample is not limited by the availability of *ZRI*.

In addition to these data sets, which help us test the main hypotheses, we also use the following data sets to explore the various mechanisms that could explain our findings: (1) density of streetlights, (2) hiring of security guards, and (3) property-level rental listings. We provide the details of these data sets in Section 4.5.1, where we analyze the effect of mergers on amenity provisions.

3.2 Institutional SFR investors

We identify institutional investors in SFRs and their properties following Ganduri, Xiao, and Xiao (2019). We start with properties owned by companies and then identify 2,097 owner mailing addresses associated with at least 100 properties.[14] We perform a manual internet search for each address to identify the owners and filter out those not in the SFR business, such as home builders, property management companies, and government agencies. We consolidate

[13] The map is available at https://communitycrimemap.com/.

[14] The database also provides information on the names of company owners. These names, however, are often recorded with typos, abbreviations, or as various subsidiaries of a parent company. We therefore use mailing address to identify unique investors.

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different addresses associated with the same company. At the end of this process, we find 166,635 single-family homes owned by 26 institutional SFR investors as of 2016. We also identify the timing of each SFR investment based on the last recorded transaction appearing in the database.

3.3 Mergers of institutional SFR investors

Based on a web search and M&A records from the Securities Data Company (SDC) database, we find three mergers of publicly listed institutional SFR investors. Table 1 summarizes the three mergers announced and completed between 2015 and 2017. Except for Colony American Homes, all merging firms were publicly listed at the time of the mergers. Therefore, we are able to find the list of subsidiaries and the geographic distribution of their properties. Based on the information on a broader sample of institutional SFR investors in Ganduri, Xiao, and Xiao (2019) and public disclosure by the merging firms, we narrow our search to the following states: Arizona, California, Colorado, Florida, Georgia, Illinois, Indiana, Nevada, North Carolina, Ohio, South Carolina, Texas, and Washington. We match the names of the subsidiaries and addresses with the property owner names and mailing addresses from the assessment files using a keyword search and manual filtering. Table 1 reports the number of properties we find from the database and the number of properties reported in news articles. We successfully find 74%, 87%, and 87% of the reported number of properties for the three mergers. The table shows a high state-level overlap of the merging firms' portfolios. The three mergers created the two largest institutional SFR investors in the United States: American Homes 4 Rent and Invitation Homes. As of 2018, together, the two companies own more than 130,000 single-family homes in the United States, accounting for more than half of the institutional SFR market (Andrews and Sisson 2018).

3.4 Summary statistics

Table 2 provides summary statistics for the data used in our analysis. Panels A and B provide neighborhood-level and census-tract-level statistics on the number of institutional SFR properties. Zillow creates a database of around 7,500 neighborhoods in the largest cities in the United States. In our DiD analysis of rent, we include 1,439 of these neighborhoods where we find properties of the merged landlords and for which Zillow has an estimated rent index. To analyze neighborhood crime, we count the number of criminal activities at the census tract level, because the data on crime are not limited to the neighborhood areas defined by Zillow. There are 5,603 census tracts that have at least one property owned by one of the merged institutional SFR investors and available crime data. The Zillow-defined neighborhoods have a median area of 1.33 square miles in our sample, similar to that of the census tracts (1.24 square miles in our sample). By using neighborhoods and census tracts as the units of observations, we are able to exploit the variation in investors' scale and market share, rent pricing, and crime rate at a highly micro level.

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Table 1
Summary of mergers in the analysis

Acquirer				Target					
Name	Status	Properties	States	Name	Status	Properties	States	Announcement	Completion
American Homes 4 Rent	Public	29,637 (38,377)	AZ, CA, CO, FL, GA, IL, IN, NV, NC, OH, SC, TX, WA	American Residential Properties	Public	5,247 (8,938)	AZ, CA, CO, FL, GA, IL, IN, NV, NC, OH, SC, TX	Dec. 3, 2015	Feb. 29, 2016
Starwood Waypoint Residential Trust	Public	10,608 (30,000)	AZ, CA, CO, FL, GA, IL, IN, NV, NC, OH, SC, TX, WA	Colony American Homes	Private	15,537	AZ, CA, CO, FL, GA, IL, NV, NC, TX	Sep. 21, 2015	Jan. 9, 2016
Invitation Homes	Public	42,601 (50,000)	AZ, CA, CO, FL, GA, IL, NV, NC, SC, WA	Starwood Waypoint Homes	Public	28,491 (32,000)	AZ, CA, CO, FL, GA, IL, IN, NV, NC, OH, SC, TX, WA	Aug. 10, 2017	Nov. 16, 2017

This table summarizes the three horizontal mergers of institutional SFR investors. The numbers of properties are based on the records we manually find from Zillow database. The numbers of properties in parentheses are based on news articles (see Lane 2015a, 2015b, 2017).

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Table 2
Summary statistics

Statistic:	N	Mean	P1	P50	P99	SD
A. Neighborhood-level statistics						
Area (square mile)	1,439	3.469	0.170	1.329	40.011	7.238
Properties (per merger)	2,556	10.000	1.000	3.000	103.000	30.467
Δ Properties (per merger)	2,556	1.444	0.000	0.000	23.000	6.675
HHI	2,532	0.581	0.173	0.510	1.000	0.279
Number of rivals	2,556	2.312	0.000	2.000	8.000	2.191
B. Census tract-level statistics						
Area (square mile)	5,603	5.237	0.188	1.237	54.194	30.616
Properties (per merger)	9,708	6.044	1.000	3.000	47.000	10.247
Δ Properties (per merger)	9,708	0.710	0.000	0.000	9.000	2.072
HHI	9,503	0.644	0.200	0.557	1.000	0.274
Number of rivals	9,708	1.643	0.000	1.000	6.000	1.596
C. Market share						
Census tract level:						
Large treatment (premerger)	278	0.027	0.004	0.020	0.145	0.027
Large treatment (postmerger)	278	0.059	0.010	0.047	0.246	0.044
Small treatment (premerger)	1,039	0.011	0.001	0.006	0.096	0.018
Small treatment (postmerger)	1,039	0.027	0.003	0.018	0.132	0.030
Census block level:						
Large treatment (premerger)	17,050	0.132	0.000	0.058	1.000	0.222
Large treatment (postmerger)	17,050	0.234	0.015	0.125	1.000	0.273
Small treatment (premerger)	29,022	0.160	0.000	0.059	1.000	0.270
Small treatment (postmerger)	29,022	0.261	0.012	0.130	1.000	0.306

(*Continued*)

Panels A and B also report summary statistics on the number of properties owned by merging firms and the number of properties gained from mergers. The average (median) numbers of properties are 10 (3) per neighborhood and 6 (3) per census tract. In the sample, 26.4% (13.6%) of neighborhoods (census tracts) have properties owned by both acquirers and targets prior to the mergers.

Panel C reports the acquirers' market shares before and after the mergers. We first calculate the market share at the census tract level based on the number of properties owned by the acquirer divided by the total number of rental housing units for the census tract reported by the Census Bureau. We also divide overlapped neighborhoods into a "large treatment" group and a "small treatment" group. This division refers to census tracts where the acquirer gained more than five properties and up to five properties after the mergers, respectively. The statistics in the first row show that, at the census tract level, the acquirer's average market share is 1.1% (2.7%) in the small (large) treatment areas prior to the mergers, and it increased to 2.7% (5.9%) after the mergers. These statistics likely understate the true market share of institutional landlords for several reasons. First, the number of rental units reported by the Census Bureau includes non-SFR properties, such as multifamily properties and apartment complexes, which account for 65% of the total rental housing stock (Eisfeldt and Demers 2015) and might not be competing with institutional landlords for the same pool of renters. Second, the total number of rental housing

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Table 2
(*Continued***)**

D. Rent (ZRI) at Zillow neighborhood level

Period:	Premerger			Postmerger		
Statistic:	N	Mean	SD	N	Mean	SD
American Home 4 Rent	6,444	1,396	443	6,444	1,449	467
American Residential Properties	2,124	1,245	337	2,124	1,286	349
Starwood Waypoint	4,536	1,546	513	4,536	1,668	568
Colony American Homes	6,552	1,676	601	6,552	1,805	661
Colony Starwood Homes	9,588	1,787	656	9,588	1,859	670
Invitation Homes	9,552	1,740	757	9,552	1,800	737
All	30,696	1,639	656	30,696	1,718	675
$I(\Delta Properties > 0)$	8,100	1,617	556	8,100	1,697	577
$I(\Delta Properties > 5)$	1,716	1,661	540	1,716	1,747	562

E. Number of crimes at census tract level

Period:	Premerger			Postmerger		
Statistic:	N	Mean	SD	N	Mean	SD
American Home 4 Rent	26,568	12.40	17.50	26,568	12.61	17.87
American Residential Properties	8,868	15.98	22.57	8,868	11.51	21.12
Starwood Waypoint	16,380	11.12	21.00	16,380	13.34	22.88
Colony American Homes	23,844	11.82	18.77	23,844	11.42	18.78
Colony Starwood Homes	35,256	11.98	20.54	35,256	12.06	20.36
Invitation Homes	35,916	11.43	21.06	35,916	11.09	20.50
All	116,688	11.86	19.68	116,688	11.77	19.78
$I(\Delta Properties > 0)$	15,816	13.19	20.53	15,816	12.74	19.78
$I(\Delta Properties > 5)$	3,336	14.31	16.95	3,336	13.52	16.59

Panels A and B report summary statistics on the size of neighborhoods and census tracts in our sample. *Properties (per merger)* is the number of properties owned by the merged firms in each neighborhood or census tract. Δ*Properties (per merger)* is the number of properties gained by the merging firms after the merger in each neighborhood or census tract. Panel C presents the acquirers' market shares in overlapped areas before and after the mergers. Market share at the census tract level is measured by the ratio of the number of properties owned by the acquirer to the number of rental units reported by the Census Bureau. Market share at the census block level is measured by the ratio of the number of properties owned by the acquirer to the number of single family homes in the census block that have rental histories on Zillow. A neighborhood is defined as "large treatment" if the merged firm gained more than five properties after the merger (i.e., $I(\Delta Properties > 5) = 1$), and "small treatment" if there is overlap in the two portfolios but the merged firm gained five or fewer properties after the merger (i.e., $I(\Delta Properties > 0) = 1$ and $I(\Delta Properties > 5) = 0$). Panel D (E) presents the average rent (number of crimes) of the neighborhoods (census tracts) covered by each of the merging firms.

units include homes occupied by long-term renters; these units are thus not active in the rental market.

To measure market share more accurately, we identify all single-family rental homes located in the same census blocks as the merged landlords' properties by scraping individual home rental listing records from Zillow. We then calculate the market share at the census block level by taking the ratio of the number of properties owned by the acquirer by the number of single-family homes with a rental history in the census block. We find that, at the census block level, an acquirer's average market share is 16.0% (13.2%) in the small (large) treatment census tracts prior to the mergers and 26.1% (23.4%) after the mergers. These numbers are substantially higher than the market shares measured at the census tract level using the total number of rental units reported by the Census Bureau. In 13.4% (10.4%) of the census blocks in the small (large) treatment group, acquirers became the dominant landlord with more than half the market share after mergers. This is consistent with other studies, such as Ganduri, Xiao, and Xiao (2019), showing that institutional landlords' portfolios are highly

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geographically concentrated. Therefore, while merged landlords are far from being monopolists at the census tract level, they may still have gained significant market power at a more local level.

Panels D and E report the average rent and crime rate for areas covered by institutional landlords in our sample. Panel D shows that the average monthly rent for all neighborhoods increased from \$1,639 to \$1,718, or by 4.8%, after merger completion. Among these neighborhoods, those in which merged landlords gained more than five homes experienced a 5.18% increase from \$1,661 to \$1,747. Hence, the rent growth rate appears higher in overlapped neighborhoods. Panel E shows that the average number of criminal incidents for all census tracts decreased by 0.76% from 11.86 to 11.77 per month after merger completion. By contrast, the average number of criminal incidents for census tracts in which merged landlords gained more than five homes decreased by 5.52% from 14.31 to 13.52 per month. Thus, the crime rate also appears to have declined more in overlapped neighborhoods, consistent with our hypothesis.

4. Empirical Analysis

4.1 Empirical design and identification assumptions

We examine the three largest mergers of institutional SFR investors. By focusing on institutional mergers, we can capture discrete jumps in the scale of institutional investors' portfolios and market share due to varying degrees of ownership overlap between acquirers and targets. This empirical setting allows us to separate the effect of institutional landlords from other confounding factors in local areas, such as gentrification and demographic shifts, which gradually take effect.

For each institutional merger, we include neighborhoods that have properties owned by at least one merging firm and find the corresponding data on neighborhood rent and crime for the 12 months before merger announcements and the 12 months after merger completion. We then estimate the following DiD models for the neighborhood rent and the crime rate:

$$ln(Rent_{m,n,t}) = \alpha + \beta_1 Post_{m,t} \times Treated_{m,n} + \beta_2 Treated_{m,n} + \beta_3 Post_{m,t} + \gamma_{c,t} + \theta_n + \varepsilon_{m,n,t}, \quad (2)$$

$$ln(Crime_{m,k,t}) = \alpha + \beta_1 Post_{m,t} \times Treated_{m,k} + \beta_2 Treated_{m,k} + \beta_3 Post_{m,t} + \gamma_{c,t} + \theta_k + \varepsilon_{m,k,t}. \quad (3)$$

$Rent_{m,n,t}$ is the *ZRI* for merger m, neighborhood n, and month t. $Post_{m,t}$ is a binary variable that equals one for neighborhood-month observations after completion of merger m. $Crime_{m,k,t}$ is the number of criminal incidents for merger m, census tract k, and month t. Zillow publishes these neighborhood rent data using their own definition of "neighborhoods." Hence, we estimate Model (2) with Zillow neighborhoods as the unit of observation. We estimate

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Table 3
Illustration of treated and control neighborhoods around mergers

Neighborhood	Number of acquirer properties	Number of target properties	Properties gained from merger	Group
North Mountain	56	18	18	Large treatment
Paradise Valley	43	17	17	Large treatment
Deer Valley	42	53	42	Large treatment
Alhambra	16	19	16	Large treatment
Maryvale	5	84	5	Small treatment
Laveen	3	64	3	Small treatment
Estrella	1	60	1	Small treatment
Camelback East	9	2	2	Small treatment
Ahwatukee Foothills	1	3	1	Small treatment
South Mountain	0	32	0	Control
Desert View	0	2	0	Control
North Gateway	0	2	0	Control
North Scottsdale	0	1	0	Control

The table shows the number of properties owned by the acquirer and target in 13 adjacent Zillow Neighborhoods of Maricopa County, Arizona, in the merger between Starwood Waypoint and Colony American Home to illustrate the definition of treated and control neighborhoods. A neighborhood is defined as a "large treatment" if the merged firm gained more than five properties after the merger (i.e., $I(\Delta Properties > 5) = 1$), "small treatment" if there is overlap in the two portfolios but the merged firm gained five or fewer properties after the merger (i.e., $I(\Delta Properties > 0) = 1$ and $I(\Delta Properties > 5) = 0$), and "control" if there is no overlap in the two portfolios (i.e., $I(\Delta Properties > 0) = 0$).

Model (3) at the census tract level so that the test is not limited by the availability of Zillow rent data.[15] As Table 2 shows, the median size of census tracts in the sample (1.24 square miles) is similar to that of Zillow neighborhoods (1.33 square miles).

$Treated_{m,n}$ and $Treated_{m,k}$ indicate the treatment variables for neighborhood n and census tract k in merger m, respectively. They are defined based on the degree of overlap between acquirer and target properties in the neighborhood. Specifically, we use three different treatment variables: (1) a binary variable, $I(\Delta Properties > 0)$, that equals one if there is any overlap between the acquirer's and the target's portfolios in the neighborhood; (2) a binary variable, $I(\Delta Properties > 5)$, that equals one if the merged firm gained more than five properties after the merger (i.e., the lesser of the number of properties owned by the acquirer and target before merger is greater than five); and (3) and a continuous variable, $\Delta Properties$, that is the number of properties the merged firm gains after the merger (i.e., the lesser of the number of properties owned by the acquirer and target before merger).

Table 3 lists a subset of 13 adjacent neighborhoods in Maricopa County, Arizona, with the number of properties involved in the merger between Starwood Waypoint and Colony American Homes to illustrate the definitions of treated and control neighborhoods. In 4 of the 13 neighborhoods, the merged firm gained more than five properties after the merger. These neighborhoods are thus designated as the "large treatment" group. For example, in Paradise Valley,

15 The empirical results on the postmerger crime changes are similar if we estimate Model (3) at the Zillow neighborhood level.

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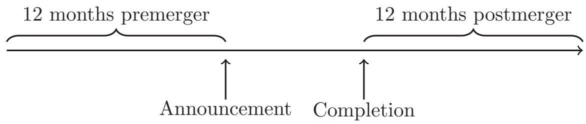

Figure 1
Time line for the difference-in-differences analysis

the acquirer and target landlords have 43 and 17 properties, respectively. The number of properties gained from the merger (Δ*Properties*), defined as the lesser of these two counts, is 17. Since Δ*Properties* is larger than five, this neighborhood is in the large treatment group. Five of the 13 neighborhoods are designated as the "small treatment" because the merged firm gained only five or fewer properties in those neighborhoods. For instance, in Laveen, the acquirer and target landlords have 3 and 64 properties, respectively. The number of properties gained from the merger in this case is three because the acquirer has fewer properties than the target. Since Δ*Properties* is smaller than five in this case, Laveen is in the small treatment group based on our definition. The remaining four neighborhoods are in the control group because only the target firm has properties in the area. Hence, the acquirer and the target do not have any market overlap, and Δ*Properties* is zero.

In both regression models, we include county $\times$ year-month fixed effects ($\gamma_{c,t}$) to account for unobservable time-varying characteristics at the county level. Hence, county-level fundamentals, such as local public policies, demographics, and economic conditions, would not confound our estimated treatment effect of landlord mergers on local rental price and the neighborhood crime rate. In Models (2) and (3), we also include neighborhood fixed effects (θ_n) and census tract fixed effects (θ_k), respectively. These fixed effects account for any unobservable time-invariant (or slowly changing) characteristics at the neighborhood level that may affect local rental prices and/or neighborhood quality, such as the natural physical environment, housing supply elasticity (e.g., Saiz 2010), and local climate. After including these fixed effects, our DiD estimates capture the differences in the postmerger changes in rent and crime rate across neighborhoods within a county. We exclude observations between the merger announcements and completion to ensure that the merger takes effect in the post-period. Figure 1 illustrates the timeline for the DiD analysis.

Our regression models estimate the treatment effect of institutional mergers on neighborhood rent levels and the crime rate based on variations in the degree of market overlap between acquirer and target properties. The identification assumption is that a merger creates a discrete jump in the scale and market share of the merged firm in overlapped neighborhoods. The degree of market overlap and the resultant shift in landlord scale and market share postmergers are plausibly exogenous to local economic conditions and could causally affect neighborhood rent levels and the crime rate through both the scale economy

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channel and market power channel that we discuss in Section 2. As such, we predict the DiD estimator β_1 to be significantly positive in Model (2) and negative in Model (3), respectively.

However, the treatment effects estimated from our DiD models could be confounded by selection bias simply because an SFR firm is likely to be targeted if its portfolio has sound fundamental value as perceived by the acquirer but is undervalued by the market due to frictions, such as financial distress and poor management.[16] In fact, acquirers in our sample express positive views about the economic prospects of the targets' portfolios. For instance, the 2015 proxy statement by Starwood Waypoint Residential Trust states that "its exposure to targeted markets with strong growth outlooks and market fundamentals, will leave the Combined Company well positioned to generate consistent growth in cash flow and earnings stability." The selection bias should be stronger in neighborhoods with a higher number of target properties, because acquirers can benefit more from strong growth in those neighborhoods.

Our treatment variables, which measure the degree of portfolio overlap, depend on not only the number of target properties in a neighborhood but also the number of acquirer properties in the same neighborhoods. Specifically, conditional on the same (nonzero) number of target properties, the degree of overlap increases with the number of acquirer properties, implying a stronger treatment effect on neighborhood rent and crime rate through the scale economy and market power channels based on our hypotheses. For example, in the merger between Invitation Homes and Starwood Waypoint Homes, around 15% of properties owned by Starwood Waypoint Homes were located in Texas, while Invitation Homes had no properties in Texas prior to the merger. Hence, while the decision of Invitation Homes to acquire Starwood Waypoint Homes may be endogenous to the growth prospect of the Texas housing market, Texas is in the control group of our empirical setting because the acquirer and the target do not have any portfolio overlap.

Admittedly, to the extent that the degree of portfolio overlap is correlated with the size of the target portfolio, selection bias could still confound our DiD estimates. While we cannot completely rule out this possibility, in Section 4.4 we conduct further analysis to gauge the magnitude of this possible selection bias.

Other than taking advantage of the anticipated growth in the target's markets, an acquirer can also select a target precisely to maximize synergistic gains from market power and cost efficiency. In fact, acquirers are aware of the benefits of market overlap. For example, the 2015 proxy statement by Starwood Waypoint Residential Trust states that "SWAY and CAH have portfolios with substantial market overlap, and the Combined Company Home Portfolio is characterized by a significant number of homes in each of its markets. Management believes this

16 Consistent with this possibility, American Residential Properties experienced a 10% decline in its stock price in the 6-month period prior to its acquisition by American Homes 4 Rent.

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market overlap and density will create operating efficiencies due to economies of scale." Thus, the choice of target could be endogenous to the degree of market overlap between the acquirer and target's portfolios.[17] In this case, our interpretation of the empirical estimates is still internally valid, in that our DiD estimator captures the causal effect of mergers on local rental prices and neighborhood safety. However, the endogenous choice of targets based on market overlap implies that the average treatment effect on the treated estimated in our empirical setting would be larger than the average treatment effect, that is, the predicted effect of landlord concentration on rent and neighborhood safety outside the context of mergers.

Finally, in a frictionless market, when landlords raise rent or provide poor rental services, renters can freely move to other neighborhoods. However, a household's choice of neighborhood heavily depends on factors unrelated to landlords, such as school quality, commuting distance, and cultural proximity to neighbors. Renters may also have developed neighborhood-specific social capital, such as a local network of friends and family. Households with these geographical preferences and local social capital may have low mobility and face a high risk of rent appreciation and rental quality deterioration (Diamond, McQuade, and Qian 2019). Therefore, our analysis assumes that home rental markets are, to a certain extent, segmented at the neighborhood level.

4.2 Institutional landlords and neighborhood rent

In this subsection, we examine the effect of institutional mergers on neighborhood rent by estimating Model (2). Based on our empirical prediction $H1a$, β_1 should be significantly positive if institutional mergers significantly increase average rent in overlapped neighborhoods. Table 4 presents the DiD estimates for Model (2). The estimates show that β_1 is significantly positive. This is consistent with our hypothesis $H1a$, whereby landlords raise rent when there is less competition. The coefficients for *Treated* and $Post$ cannot be perfectly absorbed by neighborhoods and time fixed effects, because their definitions are specific to mergers that affect different neighborhoods at different times. The adjusted R^2, which accounts for the explanatory power of both the DiD estimator and the fixed effects, is 99.7%. This suggests that the county × year-month fixed effects and the neighborhood fixed effects have absorbed most of the variation in neighborhood rent. Thus, the scope for an omitted variable is relatively small.[18]

Figure 2 presents the rent difference between overlapped and nonoverlapped neighborhoods in the 24-month window around mergers. The figure confirms the parallel trend assumption, in that the trend in rent between treated and

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17 See https://www.sec.gov/Archives/edgar/data/1579471/000119312515376853/d45844ddefm14a.htm.

18 For example, the within-R^2 for the estimate in column 1 of Table 4 is 0.7%, suggesting that the fixed effects can explain 99% of the variation in neighborhood rent levels. Specifically, the incremental R^2s contributed by the neighborhood fixed effects and county × year-month fixed effects are 40.1% and 58.9%, respectively.

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Table 4
Change in neighborhood rent around mergers

Dependent variable:	ln(ZRI)		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post × Treated	0.0043***	0.0051**	0.0002***
	(3.18)	(2.55)	(2.95)
Post	−0.0030***	−0.0013***	−0.0011***
	(−4.66)	(−3.07)	(−2.87)
Treated	−0.0069***	−0.0039**	−0.0001
	(−5.31)	(−2.37)	(−1.36)
County × Year-month FEs	Yes	Yes	Yes
Neighborhood FEs	Yes	Yes	Yes
Adjusted R^2	.997	.997	.997
Observations	61,239	61,239	61,239

This table presents estimates of DiD regressions from a sample of neighborhood-month observations from 12 months before the announcement to 12 months after completion of the three horizontal mergers of institutional investors. The sample includes neighborhoods covered by either of the merging firms. We exclude observations between the announcement and completion dates. The dependent variable is the natural logarithm of the Zillow Neighborhood Rental Index (ZRI) for single-family residences. In column 1, *Treated* is a binary variable that equals one if the merging firms gain at least one property after the merger. In column 2, *Treated* is a binary variable that equals one if the merging firms gain more than five properties after the merger. In column 3, *Treated* is the number of properties the merging firms gain after the merger. *Post* is a binary variable that equals one after completion of the mergers. We include county × year-month fixed effects and neighborhood fixed effects in the regressions. We report t-statistics using standard errors clustered by neighborhood in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

control neighborhoods did not diverge until completion of a merger. One may be concerned that our sample time period is too short to capture any pre-trend over the longer term. Figure A.2 in the Internet Appendix shows that the parallel trends in rent hold even over a 3-year period before the merger announcements. This pattern supports the idea that the greater increase in rent for overlapped neighborhoods is driven by the merger, rather than by other confounding local characteristics. Note that month −1 indicates the month prior to the *announcement* of the mergers, and month +1 indicates the month after the *completion* of the mergers. Table 1 shows an average 3-month gap between the announcement and completion of the mergers, and, thus, the increase in rent did not occur as suddenly as it appears on the figure. In Figure A.4 in the Internet Appendix, we include the period between a merger announcement and completion in the sample. The figure shows that rents in overlapped neighborhoods significantly increased only after mergers were completed. The absence of an effect during the period between the announcement and completion in Figure A.4 and the subsequent large increase observed in Figure 2 suggest that rent increases are not driven by other landlords expecting an improvement in neighborhood quality. Instead, rents appear to increase once acquirers take over their targets, in line with the market power channel. In Section 4.6, we will further explore the possibility that landlord mergers increase neighborhood rent through the market power channel.

Note that the construction method of the *ZRI* can further explain some of the patterns in Figure 2. First, the price trend in Figure 2 is smooth because the *ZRI*

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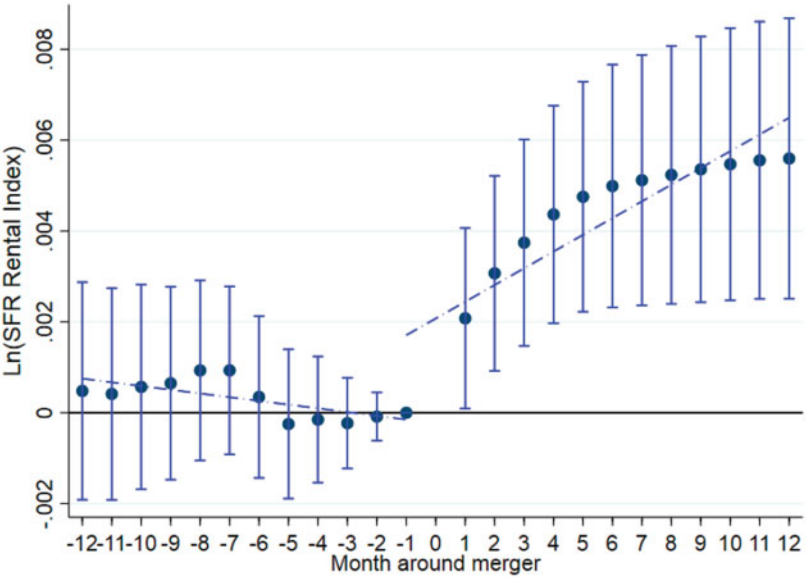

Figure 2
Difference in rent between overlapped and nonoverlapped neighborhoods around mergers
The sample includes neighborhood-month observations for neighborhoods covered by the merging firms. A neighborhood is defined as treated if there is any overlap between the merging firms prior to the merger. The horizontal axis refers to 12 months before the announcement to 12 months after completion of the mergers. We exclude observations between the announcement and completion dates. Hence, there is an average 3-month gap between month −1 and month 1 in the figure. The vertical axis represents the difference between the treated and control groups in terms of the natural logarithm of the Zillow Neighborhood Rental Index (ZRI) for single-family residences.

is an estimate from a predictive model that filters out idiosyncratic variation due to home characteristics and seasonality. Second, the *ZRI* immediately increases after completion of the mergers because it is an estimate of listed rental prices, which reflect the immediate pricing decisions of local landlords.

Based on the estimates in column 2 of Table 4, we find that, after mergers are completed, the ZRI of neighborhoods where the merged firms gained more than five properties increased by 0.51% relative to other neighborhoods. For average treated neighborhoods ($1,661 per month prior to a merger, see Table 2), this effect translates into an $8.47 increase in monthly rent. The DiD estimate also represents an increase in *ln(ZRI)* by 1.6% of standard deviation (0.325). Since the average growth rate of *ZRI* in the year prior to the mergers in our sample is 4.5%, the DiD estimate implies an additional 11% increase in the rent growth rate for treated neighborhoods compared with control neighborhoods after the mergers. Thus, while postmerger rent increases may not seem large in absolute magnitude, they represent a sizable increase relative to the historical growth rate.

This finding is robust to several alternative specifications, which we present in the Internet Appendix. In Table A1, we estimate the regressions in each

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merger separately and find that the DiD estimates have a consistent sign and magnitude across mergers. Thus, all three mergers are associated with postmerger rent increases.[19] Table A2 shows that the postmerger rent increases remain significant if we estimate the model with an annual frequency.

Finally, we investigate whether landlord mergers generate only a one-time increase in rent or a persistent rent increase over time. Specifically, we extend our dynamic DiD analysis to 2 years after merger completion to examine the dynamics of rental price change over a longer horizon. Unfortunately, because we only have the *ZRI* data up to 2018, and Zillow has since stopped publishing the neighborhood-level ZRI data, we could only perform this analysis for the first two mergers. Nevertheless, in the extended dynamic DiD analysis, the results of which are reported in Figure A.3 of the Internet Appendix, we show that the rent level of overlapped neighborhoods continues to increase beyond the first year postmerger and is about 1.4% higher than the premerger level compared with the control group in the 24th month after completion of the mergers. This result suggests that merged landlords raise rent continuously over time. Therefore, even though the rent increase estimated from our baseline DiD model is relatively small, such a small but persistent increase would still lead to a material effect on rental market prices as it accumulates over time.

4.3 Institutional landlords and neighborhood crime

Next, we test our model's prediction $H2a$ and use the institutional merger setting to examine the effect of institutional landlords on neighborhood crime. We scrape data from the LexisNexis Community Crime Map on criminal activities for all counties involved in institutional mergers and count the number of criminal incidents in each census tract and month for the 12 months before the merger announcements and 12 months after completion of the mergers. We then estimate Model (3), presented in Section 4.1.

Table 5 presents the estimates of Model (3) in a sample of census tracts that have at least one property owned by either of the merging firms.[20] The coefficient for $Post_t \times Treated_{m,k}$ is consistently negative across three definitions of treatment variables and is significant in columns 2 and 3. The estimates in column 2 suggest that, for census tracts where the merged firms gained more than five properties upon the merger, the number of criminal incidents declines by 5.23% in the 12-month period after merger completion.

19 Since the definitions of postmerger period and treated neighorhood are fixed within each merger, the time and census tract fixed effects in the single-merger regressions absorb the stand-alone effect of *Post* and *Treated*.

20 The sample of Zillow neighborhoods is smaller as the ZRI is available for a selected set of neighborhoods only, and, thus, many of the census tracts included in the crime analysis have no available information on neighborhood rent. Table A3 in the Internet Appendix shows that the postmerger reduction in the crime rate remains robust and similar in magnitude if we count the number of criminal incidents at the Zillow neighborhood level and use the same sample as the one used in the rent model. Therefore, our main findings on the postmerger rent increase, and the reduction in crime is not sensitive to the choice of samples.

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Table 5
Change in neighborhood crime rate around mergers

A. All crimes included			
Dependent variable:	ln(Crime)		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post × Treat	−0.0108	−0.0523**	−0.0048***
	(−0.94)	(−2.57)	(−2.91)
Post	−0.0052	−0.0025	−0.0004
	(−1.21)	(−0.78)	(−0.11)
Treated	−0.0092	0.0297	0.0026
	(−0.63)	(1.29)	(1.39)
County × Year-month FEs	Yes	Yes	Yes
Census tract FEs	Yes	Yes	Yes
Adjusted R^2	.886	.886	.886
Observations	232,397	232,397	232,397

B. Crimes by type						
Dependent variable:	ln(Crime)					
Type of crime:	Assault (1)	Burglary (2)	Robbery (3)	Theft (4)	Drug (5)	Vandalism (6)
Post × Treat	−0.0198	−0.0303*	−0.0143	−0.0464***	−0.0118	−0.0343***
	(−1.60)	(−1.81)	(−1.63)	(−2.59)	(−1.04)	(−2.75)
Post	0.0003	0.0006	0.0014	−0.0006	−0.0006	−0.0012
	(0.18)	(0.27)	(1.03)	(−0.24)	(−0.42)	(−0.73)
Treated	0.0143	0.0079	0.0130**	0.0299*	0.0042	−0.0125
	(1.21)	(0.53)	(2.00)	(1.77)	(0.42)	(−1.03)
County × Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Census tract FEs	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	.730	.753	.404	.803	.654	.623
Observations	232,397	232,397	232,397	232,397	232,397	232,397

This table presents estimates of DiD regressions from a sample of census-tract-month observations from 12 months before the announcement to 12 months after completion of the three horizontal mergers of institutional investors. The sample includes census tracts covered by either of the merging firms. We exclude observations between the announcement and completion dates. The dependent variable is the natural logarithm of one plus the number of criminal incidents in the census tract. Panel A includes all types of crime. In column 1, *Treated* is a binary variable that equals one if the merging firms gain at least one property after the merger. In column 2, *Treated* is a binary variable that equals one if the merging firms gain more than five properties after the merger. In column 3, *Treated* is the number of properties the merging firms gain after the merger. *Post* is a binary variable that equals one after completion of the mergers. Panel B presents estimates for different types of crime. *Treated* is a binary variable that equals one if the merging firms gain more than five properties after the merger. We include county × year-month fixed effects and census tract fixed effects in the regressions. We report t-statistics using standard errors clustered by census tract in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

For the average treated census tract, this effect amounts to a decrease in the monthly number of criminal incidents from 14.31 to 13.56, or by 0.75 cases. The DiD estimate also represents a decrease in *ln(Crime)* by 3.5% of standard deviation (1.476). Since the average growth rate in the number of criminal incidents in the year prior to the mergers in our sample is 3.0%, the DiD estimate implies a 174.3% decrease in the crime growth rate for the treated neighborhoods compared with the control neighborhoods after the mergers. Overall, the DiD estimate suggests an economically significant reduction in the neighborhood crime rate postmerger. The adjusted R^2, which accounts for the explanatory power of both the DiD estimator and the fixed effects, is

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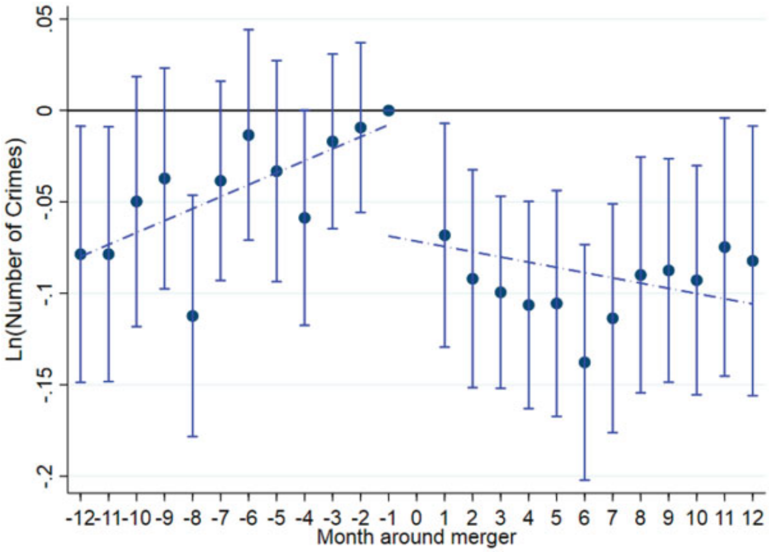

Figure 3
Difference in the number of crimes between overlapped and nonoverlapped census tracts around mergers
The sample includes census tract-month observations for census tracts covered by the merging firms. A census tract is defined as treated if the merging firms gain more than five properties after completion of the merger. The horizontal axis refers to 12 months before the announcement to 12 months after completion of the mergers. We exclude observations between the announcement and completion dates. Hence, there is an average 3-month gap between month -1 and month 1 in the figure. The vertical axis represents the difference between the treated and control groups in terms of the natural logarithm of one plus the number of crimes in a census tract.

88.6%. This again suggests that the county $\times$ year-month fixed effects and the census tract fixed effects can explain most of the variation in neighborhood crime rate.

Figure 3 confirms the parallel trend assumption, as overlapped census tracts do not witness a reduction in crime relative to the other census tracts until completion of a merger. Figure A.5 in the Internet Appendix also shows that the parallel trend assumption holds over a 3-year period prior to a merger announcement. Hence, the decline in the crime rate postmerger is not driven by a preexisting differential time trend between overlapped and nonoverlapped neighborhoods. Months -1 and +1 indicate the months prior to the announcement and the month after completion of the mergers, respectively; this is similar to Figure 2. Hence, there is an average 3-month gap between month -1 and +1, and the reduction in the crime rate is not as sudden as it appears on the figure. In Figure A6 in the Internet Appendix, we include the period between the merger announcement and completion in the sample. The figure shows that the decline in the crime rate in overlapped neighborhoods occurred only after the mergers were completed.

In panel B of Table 5, we examine the following types of crime separately: assault, burglary, robbery, theft, drugs, and vandalism. The estimates show

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that after completion of the mergers, assaults decrease by 1.98%, burglaries by 3.03%, robberies by 1.43%, thefts by 4.64%, drug cases by 1.18%, and vandalism by 3.43% in overlapped neighborhoods, where merged firms gained more than five properties. A reduction in crime is statistically significant for burglaries, thefts, and vandalism. These estimates indicate a decline across a broad range of criminal activities in overlapped neighborhoods. Overall, the results are consistent with our prediction $H2a$, whereby mergers of institutional landlords lead to a lower level of neighborhood crime in areas in which merged landlords experience gains in scale and market share.

One concern about the results in Table 5 could be that the decline in the number of criminal incidents in overlapped neighborhoods may be driven by weaker legal enforcement, which reduces the likelihood of a crime being spotted. If this were true, then the number of non-criminal-enforcement cases should also significantly decline. To test this possibility, we reestimate Model (3) for the number of cases of driving under the influence (DUI) and other noncriminal activities. The estimates reported in Table A4 in the Internet Appendix reveal no decrease in the number of noncriminal cases in overlapped neighborhoods relative to others after the mergers. This is inconsistent with the explanation that weakened enforcement drives the decline in criminal cases in overlapped neighborhoods.

Another concern could be that neighborhood crimes are simply displaced rather than eliminated; that is, institutional landlords may force crimes into someone else's backyard without reducing the overall crime rate in a broader region. To address this concern, we compare the crime rate of overlapped census tracts with their adjacent census tracts. Figure A7 in the Internet Appendix shows that, consistent with our DiD analysis, highly overlapped census tracts (i.e., those gaining more than five homes postmergers) witnessed a noticeable decline in the number of criminal incidences after completion of the mergers. Meanwhile, the neighboring census tracts did not experience an increase in the crime rate. Therefore, the decline in the crime rate in the institutional landlords' neighborhoods does not seem to result in a higher crime rate for the adjacent neighborhoods. Finally, we also estimate Model (3) for each merger separately and report the estimates in Table A5 in the Internet Appendix. We find a negative change in the crime rate for all three mergers, though the effect is statistically significant and economically stronger in the first and second mergers.

Overall, the results presented in this section suggest that increasing an institutional landlord's ownership in the neighborhood is accompanied by a significant improvement in neighborhood safety. This is consistent with a large owner internalizing the cost of public goods and suggests a "bright side" of higher homeownership concentration. Although large landlords may charge higher rents after merging, they also enhance the quality of their rental service, which might have been priced into raised rents.

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4.4 Endogeneity of merger decisions

As discussed above, an SFR firm is likely to be targeted if an acquirer perceives the firm's portfolio to have sound fundamental value but to be undervalued by the market due to frictions, such as financial distress and poor management. Acquirers make merger decisions that depend on their expectations for future rent growth in overlapping neighborhoods, as well as synergies they can create after the merger. In this section, we explore the extent to which this selection plays a role in explaining our results. For this purpose, we exploit the ingredients of our measure, portfolio overlap, which contains information on both the number of target and acquirer properties in a given neighborhood.

All else being equal, we expect the selection effect to be the dominant explanation if significant changes in the postmerger rent and crime rate coincide with a large number of target properties. Put differently, if our DiD estimator is driven purely by selection bias, we should expect a significant change in the postmerger rent and crime rate in areas with a large number of target properties, regardless of the degree of overlap with the acquirers' portfolios. To examine this possibility, we estimate a modified version of Models (2) and (3) in which we interact the postmerger dummy with the number of target properties in the neighborhood and estimate this interaction separately in overlapped and nonoverlapped neighborhoods. Table 6 presents the estimates of these modified models. In column 1, where we estimate the DiD model of neighborhood rent, the coefficient for $Post \times TargetProperties$ is significantly positive only in overlapped neighborhoods. In column 2, where we estimate the DiD model of neighborhood crime rate, the coefficient for $Post \times TargetProperties$ is significantly negative in both overlapped and nonoverlapped neighborhoods, but the magnitude is greater in overlapped neighborhoods. The results suggest that the postmerger increase in neighborhood rent and decreases in the crime rate are more correlated with the size of targets' portfolios in overlapped neighborhoods. Thus, our empirical results using the degree of portfolio overlap likely capture the causal effect of mergers distinct from a pure selection effect.

Another possibility is that the acquirer chose its target to hedge against expected declines in rent growth within its existing portfolio. If this is the case, we would expect a negative historical correlation in rent growth between the acquirer and target's portfolios. In Table 7, we calculate the historical correlation of rent between the acquirer and target's portfolio over the 3-year period before the merger announcements. Columns 1 to 5 show the distribution of the correlation at the county level. Specifically, within each county, we calculate the average rent growth for the acquirer and target's portfolios using the first difference of neighborhood-level *ln(ZRI)* weighted by the number of properties in each neighborhood. We then calculate the correlation using the weighted-average rent growth over the 36 months prior to the merger announcements. In column 6, we calculate the historical correlation using the weighted-average rent growth across the acquirer and target's entire portfolios. The overall correlations in the three mergers are 0.356, 0.958, and 0.774.

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Table 6
Target properties and postmerger changes in rent and crime rate

Dependent variable:	ln(ZRI) (1)	ln(Crime) (2)
Post × Target properties (nonoverlapped)	0.0000	−0.0019*
	(0.52)	(−1.88)
Post × Target properties (overlapped)	0.0001***	−0.0031***
	(5.87)	(−3.85)
Target properties (nonoverlapped)	0.0000	0.0037***
	(0.03)	(3.19)
Target properties (overlapped)	0.0000	0.0023**
	(0.84)	(2.57)
Post	−0.0008**	0.0064
	(−2.27)	(1.51)
County × Year-month FEs	Yes	Yes
Neighborhood FEs	Yes	
Census tract FEs		Yes
Adjusted R^2	.997	.887
Observations	61,239	232,397

This table shows that the postmerger increase (decrease) in rent (crime rate) is more correlated with the number of target properties in overlapped neighborhoods than in nonoverlapped neighborhoods. Column 1 presents estimates of DiD regressions from a sample of neighborhood-month observations from 12 months before the announcement to 12 months after completion of the three horizontal mergers of institutional investors. The sample includes neighborhoods covered by either of the merging firms. The dependent variable is the natural logarithm of the Zillow Neighborhood Rental Index (ZRI) for single-family residences. Column 2 presents estimates of DiD regressions from a sample of census-tract-month observations from 12 months before the announcement to 12 months after completion of the three horizontal mergers of institutional investors. The sample includes census tracts covered by either of the merging firms. The dependent variable is the natural logarithm of one plus the number of criminal incidents. In both samples, we exclude observations between the announcement and completion dates. *Target properties (nonoverlapped)* is the number of target properties in a neighborhood multiplied by a binary variable that equals one for nonoverlapped neighborhoods. *Target properties (overlapped)* is the number of target properties in a neighborhood multiplied by a binary variable that equals one for overlapped neighborhoods. In column 1 (2), we include county × year-month fixed effects and neighborhood (census tract) fixed effects in the regressions, and report t-statistics using standard errors clustered by neighborhood (census tract) in parentheses. *$p<.1$; **$p<.05$; ***$p<.01$.

At the county level, the correlation in the three mergers ranges from 0.150 to 0.884, 0.190 to 0.902, and 0.002 to 0.891, with the median correlation being 0.263, 0.741, and 0.483, respectively. We do not observe a negative correlation between the acquirer and target's portfolios in any of the mergers. Thus, acquirers do not appear to significantly benefit from these mergers in terms of risk diversification.[21]

Therefore, our estimated treatment effects are unlikely to be solely driven by endogeneity because of (1) expected future rent growth in target properties or (2) expected benefits of hedging against future rent declines in acquirer properties. Having highlighted these items, we also point out that acquirers may select targets to maximize synergies (i.e., the treatment effect) from market overlap, an action that is consistent with the statements in the acquirers' public filings. In this case, our empirical estimate would still capture the causal effect of mergers on neighborhood rent and safety, although the local treatment effect in this case

[21] We thank an anonymous referee for the suggestion to consider risk diversification as a potential benefit of landlord mergers.

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Table 7
Historical correlation between acquirer's and target's portfolios

Merger	Distribution of correlations within counties					Overall correlation
	Min (1)	p25 (2)	p50 (3)	p75 (4)	Max (5)	(6)
American Homes 4 Rent – American Residential Properties	.150	.206	.263	.596	.884	.356
Starwood Waypoint Residential Trust – Colony American Homes	.190	.589	.741	.839	.902	.958
Invitation Homes – Starwood Waypoint Homes	.002	.204	.483	.727	.891	.774

This table shows the historical correlation in rent between the acquirer's and the target's portfolios. Columns 1 to 5 show the distribution of correlation at the county level. Specifically, within each county, we calculate the average rent growth for the acquirer's and the target's portfolios using the first difference of neighborhood-level *ln(ZRI)* weighted by the number of properties in each neighborhood. We then calculate the correlation using the weighted-average rent growth over the 36 months prior to the merger announcements. In column 6, we calculate the historical correlation using the weighted-average rent growth across the entire acquirer's and target's portfolios.

is likely greater than the average treatment effect for a hypothetical merger in which the target is randomly selected.

4.5 Evidence for neighborhood quality improvement

In this subsection, we provide further evidence that institutional landlords mergers lead to changes in the neighborhood that reduce crime rate. As we discussed in Section 1, mergers increase benefits and reduce the average costs of investments in neighborhood amenities. This leads to an improvement in neighborhood amenities, which deter crime. Furthermore, mergers will lead to lower competition for renters, resulting in lower resident turnover and better neighborhood stability. In what follows, we present evidence on the validity of these claims.

4.5.1 Postmerger change in amenities. In this section, we investigate whether the neighborhood amenities changed significantly around the merger event. For this purpose, we use two different sets of granular data sets that help us measure (1) security guard hiring and (2) density of streetlights.

Our first analysis focuses on a mechanism that can plausibly affect the level of crime in a given region. Specifically, we use a large online job postings database to test whether job postings for security guards differ in areas that witnessed a merger. Our data come from Burning Glass Technologies online job postings database. Burning Glass collects data on online job postings from over 40,000 online job boards and company websites. The data set contains over 100 million electronic job postings since 2010 and helps us identify both the county (FIPS code) and standardized occupation codes (SOC) associated with job postings. From this data set, we restrict our analysis to 967,805 job postings that explicitly specify the job function as a security guard (i.e., SOC: 33-9032). The job description for this particular SOC is given as guarding, patrolling, or monitoring premises to prevent theft, violence, or infractions of

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rules.[22] Unfortunately, while this data set allows us to observe the job posting incidents, we cannot determine the exact number of positions per posting or the ultimate employer. With this drawback, we use the aggregate number of job postings in a given county to create an amenity measure. The results reported in the first column of panel A of Table 8 show that the number of job postings for security guard positions in the treated areas increases by 8.57 (t = 2.30) after the mergers, which corresponds to an economically meaningful amount compared to the sample average of security guard positions, 29.68.

The second analysis is based on the streetlights data, which come from the Visible Infrared Imaging Radiometer Suite (VIIRS). These data allow us to measure nighttime streetlight radiance, which may affect both people's quality of life and their perception of nighttime safety (Boyce et al. 2000).[23] To examine whether the density of streetlights in affected neighborhoods changed following the mergers, we first identify (nonhighway) local roads and streets from satellite data within each census tract. We then calculate the average monthly nighttime streetlight radiance in the 20-meter bandwidth around the roads and streets and use it as the dependent variable for our DiD model. The estimates reported in panel B of Table 8 show that nighttime streetlight radiance increases by 0.59% in the 1-year period after completion of the mergers. Improved streetlighting has been considered a feasible and efficient method of reducing crime (e.g., Farrington and Welsh 2002; Xu et al. 2018). Therefore, we interpret this evidence as support for the idea that mergers of institutional landlords lead to an improvement in local amenities, thereby helping to reduce the crime rate, albeit the economic magnitude appears to be small.

A question that arises from the above findings is how mergers allow landlords to increase investments in neighborhood amenities without compromising other expenses, such as those on improving home quality, given budget constraints. As our theoretical model in Section 2 shows, both scale and market share are important factors in landlords' decisions to spend on neighborhood amenities. Increasing the number of homes owned by a landlord in a neighborhood would increase the benefit of improving neighborhood safety, while reducing the average cost of public amenities per property. The enhancement in market power postmerger also allows merged landlords to further increase their profit margin and hence increase their budget for investments in neighborhood amenities. Therefore, provided that a merger creates sufficient cost synergies and market power, a merged landlord could invest more in neighborhood amenities without necessarily cutting other expenses.

4.5.2 Postmerger change in renter turnover. In this section, we investigate whether tenant turnover exhibits a change following institutional landlord

[22] For the description of SOC codes, see https://www.bls.gov/soc/2018/soc_2018_definitions.pdf.

[23] See Elvidge et al. (2017) for a technical summary of the VIIRS data and Donaldson and Storeygard (2016) for a survey on the application of satellite data to economic research.

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Table 8
Change in neighborhood amenities around mergers

	A		
Dependent variable:	Number of job posts for security guards		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post × Treated	8.5742**	9.5413**	0.0319**
	(2.30)	(2.23)	(2.55)
Post	−5.0386***	−4.7936***	−0.4450
	(−2.84)	(−2.74)	(−0.32)
Treated	−4.7184*	−4.3365	0.0217
	(−1.70)	(−1.51)	(1.40)
County FEs	Yes	Yes	Yes
Year-month FEs	Yes	Yes	Yes
Adjusted R^2	.807	.807	.813
Observations	10,254	10,254	10,254

	B		
Dependent variable:	ln(streetlight radiance)		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post × Treated	0.0059**	0.0084	0.0010*
	(1.98)	(1.09)	(1.92)
Post	−0.0016*	−0.0007	−0.0014*
	(−1.79)	(−1.03)	(−1.79)
Treated	−0.0072***	−0.0129***	−0.0013***
	(−2.79)	(−2.85)	(−3.03)
County × Year-month FEs	Yes	Yes	Yes
Census tract FEs	Yes	Yes	Yes
Adjusted R^2	.989	.989	.989
Observations	66,437	66,437	66,437

Panel A presents estimates of DiD regressions from a sample of county-year-month observations from 12 months before the announcement to 12 months after completion of the three horizontal mergers of institutional investors. The sample includes counties covered by either of the merging firms and exclude observations between the announcement and completion dates. The dependent variable is the number of job posts for security guards in the county. We include county fixed effects and year-month fixed effects, and report t-statistics using standard errors clustered by county in parentheses. Panel B presents estimates of DiD regressions from a sample of census-tract-month observations from 12 months before the announcement to 12 months after completion of the three horizontal mergers of institutional investors. The sample includes census tracts covered by either of the merging firms and exclude observations between the announcement and completion dates. We also require each observation to have the average cloud-free coverage (i.e. the number of observations for each pixel) above 2. The dependent variable is the natural logarithm of average nighttime streetlight radiance of local streets in the neighborhoods. We include county × year-month fixed effects and census tract fixed effects in the regressions, and report t-statistics using standard errors clustered by census tract in parentheses. For both panels, in column 1, *Treated* is a binary variable that equals one if the merging firms gain at least one property after the merger. In column 2, *Treated* is a binary variable that equals one if the merging firms gain more than five properties after the merger. In column 3, *Treated* is the number of properties the merging firms gain after the merger. *Post* is a binary variable that equals one after completion of the mergers. $^*p < .1$; $^{**}p < .05$; $^{***}p < .01$.

mergers. Here, the main idea is to understand the net effect of increased rents and improved amenities (i.e., reduced crime) on tenants' decisions to stay or leave. Conducting this analysis is a challenge because we do not have access to tenant rosters. To overcome this challenge, we create a data set of renter turnover by parsing property rental listing data from Zillow. More specifically, we search for each property owned by the merged landlords on Zillow's website and find the complete rental listing history on the property's web page. We then

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Table 9
Change in resident turnover around mergers

Dependent variable:	ln(turnover)		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post × Treated	−0.0326***	−0.0570***	−0.0075***
	(−4.75)	(−2.99)	(−3.40)
Post	−0.0202***	−0.0333***	−0.0191***
	(−7.49)	(−14.30)	(−5.90)
Treated	0.1188***	0.2309***	0.0256***
	(15.93)	(12.60)	(9.97)
County × Year-month FEs	Yes	Yes	Yes
Census tract FEs	Yes	Yes	Yes
Adjusted R^2	.303	.304	.307
Observations	232,397	232,397	232,397

This table presents estimates of DiD regressions from a sample of census-tract-month observations from 12 months before the announcement to 12 months after completion of the three horizontal mergers of institutional investors. The sample includes census tracts covered by either of the merging firms. We exclude observations between the announcement and completion dates. In panel A, the dependent variable is the natural logarithm of one plus the number of renter turnovers for properties owned by the merged landlords. In column 1, *Treated* is a binary variable that equals one if the merging firms gain at least one property after the merger. In column 2, *Treated* is a binary variable that equals one if the merging firms gain more than five properties after the merger. In column 3, *Treated* is the number of properties the merging firms gain after the merger. *Post* is a binary variable that equals one after completion of the mergers. We include county × year-month fixed effects and census tract fixed effects in the regressions. We report t-statistics using standard errors clustered by census tract in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

treat each rental listing as a turnover event. Doing so allows us to identify the number of turnovers for the merged landlords' properties in each census tract and year-month.

In Table 9, we estimate a census-tract-level DiD regression using the logarithm of the number of renter turnovers for merged landlords' homes as the dependent variable, and we find that merged landlords' properties significantly declined in the number of turnovers in overlapped neighborhoods relative to other neighborhoods. This result suggests that institutional landlord mergers are associated with a reduction in resident turnover for their own properties that may have contributed to improvements in neighborhood stability.

4.5.3 Postmerger change in eviction policy. With enhanced market powers, merged landlords may possibly evict renters more frequently. Frequent evictions could enhance neighborhood safety by removing crime-prone tenants, although the threat of eviction could also create disruptions to the lives of renters. We examine this possibility by estimating the postmerger change in neighborhood eviction rates. The eviction rate data from the Eviction Lab end in 2016, and, thus, we are only able to examine the postmerger change in eviction rates for the first two mergers, which occurred in 2015.

Table 10 presents the estimates from DiD models using data on annual eviction rates at the census tract level from 1 year before the announcement to 1 year after completion of the first two institutional mergers. The estimates show that the eviction rate of overlapped neighborhoods increased by

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Table 10
Change in eviction rate around mergers

Dependent variable:	Eviction rate		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post × Treated	0.1311	0.1073	0.0199
	(0.93)	(0.26)	(0.61)
Treated	−0.0732	−0.0558	−0.0108
	(−0.93)	(−0.26)	(−0.61)
County × Year FEs	Yes	Yes	Yes
Census tract FEs	Yes	Yes	Yes
Adjusted R^2	.817	.817	.817
Observations	5,032	5,032	5,032

This table presents estimates of DiD regressions from a sample of census-tract-year observations from 1 year before the announcement to 1 year after completion of the first two horizontal mergers of institutional investors. The sample includes census tracts covered by either of the merging firms. We exclude the year of merger announcement. The dependent variable is the eviction rate (in percentage points) for a census tract reported by the Eviction Lab. In column 1, *Treated* is a binary variable that equals one if the merging firms gain at least one property after the merger. In column 2, *Treated* is a binary variable that equals one if the merging firms gain more than five properties after the merger. In column 3, *Treated* is the number of properties the merging firms gain after the merger. *Post* is a binary variable that equals one after completion of the mergers. We include county × year fixed effects and census tract fixed effects in the regressions. Because the first two mergers occur in the same year (2015), *Post* is perfectly absorbed by the year dummies. We report t-statistics using standard errors clustered by census tract in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

0.131 percentage points relative to other neighborhoods. This amounts to a 4.4% increase from the mean level of eviction rate in our sample (2.97 percentage points). Thus, the change in eviction rate appears to be economically important, albeit being statistically insignificant.[24] Our empirical results show that merged landlords have both reduced the overall turnover rate and increased the eviction rate. A plausible interpretation is that merged landlords have achieved a net reduction in turnover rate, while adjusting the composition of renters through evictions.

4.6 Evidence for market power effect

In this subsection, we show additional evidence that, other than neighborhood quality improvement, the enhanced market power of merged landlords contributes to the postmerger rent increase.

4.6.1 Postmerger competition. First, we test our model's predictions $H1b$ and $H2b$ that both postmerger rent increases and the reduction in crime will be weaker with the presence of competition from other landlords. To test prediction $H1b$, we add to Model (2) a triple interaction between the treatment variable,

[24] Evictions in home rental markets have been an important subject of sociological research. In the best-selling book *Evicted: Poverty and profit in the American city*, Desmond (2017) documents the lives of evicted families in-depth and points out systematic issues related to U.S. eviction policies. More recently, the eviction crisis during the COVID-19 pandemic has also made eviction policies a central subject of political debate. Contributing to this policy issue, our study is among the first one that provides a quantitative study on the implications of institutional ownership of rental homes to evictions.

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the postmerger dummy, and measures of neighborhood market competition, such as the Herfindahl-Hirschman index of investor concentration (HHI), the number of rival institutional landlords in the neighborhoods, and postmerger market share.[25] Table 11 reports the coefficient estimates. The estimates show that rent increases in overlapped neighborhoods are smaller if the market concentration in a neighborhood is lower, if more rival investors are present in the neighborhood, or if the postmerger market share is lower. These results are consistent with the market power channel illustrated in our theoretical model. The postmerger rent change in overlapped neighborhoods increases from 0.08% to 0.55% when the postmerger market share increases from the bottom quartile to the top quartile.[26] Thus, the market power channel appears to explain a significant proportion of the rent increase.

Based on our prediction $H2b$, we believe that local market competition postmerger could also affect the reduction in the crime rate if competition limits the profits that merged landlords can gain from investing in neighborhood safety. To test this prediction, we include a triple interaction between the treatment indicator, the post-merger-completion indicator, and measures of local competition; this is similar to Table 11. The estimates we report in Table 12 show that the reduction in crime in overlapped census tracts is not affected by the presence of other institutional landlords in the same neighborhood, but is related to the postmerger market share. Specifically, the postmerger reduction in crime appears stronger in overlapped neighborhoods, where the postmerger market share is higher. The regression results are consistent with the market power channel influencing institutional landlords to invest in neighborhood safety.

4.6.2 Evidence on property-level rent. To further differentiate the rent increase driven by market power from that driven by neighborhood quality, we examine whether merged landlords raise rent more than their neighbors (see prediction $H1c$). We scrape the rental listing histories for each of the merged landlords' homes, as well as the neighboring single-family homes in the same census block (data from Zillow). We then estimate property-level regressions in which the dependent variable is the natural logarithm of the listed rent, and the independent variable of interest is the interaction between the treated neighborhood dummy (defined similarly to those in the baseline models), the postmerger dummy, and the merged landlord home dummy.

[25] Since we do not have the number of rental housing units for a Zillow neighborhood, we estimate the number of rental units based on the census-tract-level data. Specifically, for each area overlapped between a census tract and a Zillow neighborhood, we estimate the number of rental units (population) using the number of rental units (population) of the census tract multiplied by the ratio of overlapped areas to the census tract's total area. Then, across all the census tracts that overlap with the same neighborhood, we calculate the population-weighted number of rental units. While the calculated market share is inherently noisy, as we discussed in Section 3, as long as the measurement error is randomly distributed across neighborhoods, the cross-sectional variation in the measure still can be informative about the institutional landlords' gain in market share through the merger.

[26] We quantify the difference in rent increase by estimating Model (2) in subsamples sorted based on quartiles of postmerger market share, which for brevity are not reported.

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Table 11
Local market competition and change in neighborhood rent around mergers

A

Dependent variable:	ln(ZRI)		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post × Treated	0.0037**	0.0057**	0.0003***
	(2.48)	(2.57)	(4.10)
Post	−0.0032***	−0.0017***	−0.0017***
	(−4.83)	(−3.81)	(−3.88)
Treated	−0.0065***	−0.0044**	−0.0001
	(−4.10)	(−2.30)	(−1.02)
HHI	0.0004	−0.0039	−0.0055
	(0.05)	(−0.63)	(−0.90)
Post × HHI	−0.0099***	−0.0102***	−0.0102***
	(−3.71)	(−4.22)	(−4.25)
Treated × HHI	−0.0047	−0.0163**	−0.0003
	(−0.80)	(−2.35)	(−1.11)
Post × Treated × HHI	0.0052	0.0195**	0.0010***
	(1.00)	(2.36)	(3.25)
County × Year-month FEs	Yes	Yes	Yes
Neighborhood FEs	Yes	Yes	Yes
Adjusted R^2	.997	.997	.997
Observations	60,681	60,681	60,681

B

	(1)	(2)	(3)
Post × Treated	0.0023	0.0065	0.0005**
	(1.04)	(1.55)	(1.98)
Post	−0.0073***	−0.0064***	−0.0064***
	(−7.22)	(−7.64)	(−7.58)
Treated	−0.0076***	−0.0086**	−0.0007***
	(−3.17)	(−2.32)	(−3.01)
Number of rivals	0.0005	0.0016**	0.0014*
	(0.57)	(2.02)	(1.66)
Post × Number of rivals	0.0022***	0.0024***	0.0022***
	(6.50)	(7.77)	(7.62)
Treated × Number of rivals	0.0009	0.0018**	0.0001***
	(1.55)	(2.50)	(3.24)
Post × Treated × Number of rivals	−0.0002	−0.0016*	−0.0001**
	(−0.32)	(−1.90)	(−2.04)
County × Year-month FEs	Yes	Yes	Yes
Neighborhood FEs	Yes	Yes	Yes
Adjusted R^2	.997	.997	.997
Observations	61,239	61,239	61,239

(*Continued*)

We also include census tract × year-month fixed effects in the regressions, and, thus, the Diff-in-Diff-in-Diff estimate captures within-neighborhood variations in the postmerger rent changes between the merged landlords' properties and the other rental properties.

Supportive of prediction $H1c$, the estimates in panel A of Table 13 show that the postmerger rent increase is larger for properties owned by the merged landlords, and the difference is statistically significant. The estimate in column 1 suggests that the merged landlords raise the monthly rent by 1.19% more than do the neighboring landlords postmerger. The magnitude of the rent increase at the property level is larger than that estimated at the neighborhood level

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Table 11
(Continued)

C

Dependent variable:	ln(ZRI)		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post × Treated	0.0038***	0.0042**	0.0002*
	(2.72)	(2.01)	(1.72)
Post	−0.0024***	−0.0007	−0.0009*
	(−3.39)	(−1.39)	(−1.91)
Treated	−0.0066***	−0.0034*	−0.0002***
	(−4.72)	(−1.91)	(−3.27)
Market share	0.0236*	0.0181	0.0050
	(1.67)	(1.63)	(0.69)
Post × Market share	−0.0240***	−0.0197***	−0.0130**
	(−3.71)	(−3.28)	(−2.31)
Treated × Market share	−0.0179	−0.0159	0.0002**
	(−1.38)	(−1.54)	(2.08)
Post × Treated × Market share	0.0232***	0.0215***	0.0002*
	(3.33)	(3.19)	(1.69)
County × Year-month FEs	Yes	Yes	Yes
Neighborhood FEs	Yes	Yes	Yes
Adjusted R^2	.997	.997	.997
Observations	61,239	61,239	61,239

This table presents estimates of DiD regressions from a sample of neighborhood-month observations from 12 months before the announcement to 12 months after completion of the three horizontal mergers of institutional investors. The sample includes neighborhoods covered by either of the merging firms. We exclude observations between the announcement and completion dates. The dependent variable is the natural logarithm of the Zillow Neighborhood Rental Index (ZRI) for single-family residences. In column 1, *Treated* is a binary variable that equals one if the merging firms gain at least one property after the merger. In column 2, *Treated* is a binary variable that equals one if the merging firms gain more than five properties after the merger. In column 3, *Treated* is the number of properties the merging firms gain after the merger. *Post* is a binary variable that equals one after completion of the mergers. *HHI* is the (demeaned) Herfindahl-Hirschman index measuring the concentration of institutional SFR investors in the neighorbood. *Number of rivals* is the number of rival institutional SFR investors in the neighborhood. *Market share* is the postmerger market share, defined as the ratio of the number of properties owned by the merged landlord to the imputed number of rental units for the neighborhood. We include county × year-month fixed effects and neighborhood fixed effects in the regressions. We report t-statistics using standard errors clustered by neighborhood in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

(0.43% in column 1 of Table 4). A possible explanation is that the rent increase for homes owned by the merged landlords may have been averaged out in the estimate of the *ZRI*, resulting in a relatively moderate increase observed at the neighborhood level. Since the reduction in neighborhood crime should affect the rental price of all properties, neighborhood quality improvements cannot explain this within-neighborhood variation.

In panel B of Table 13, we further divide merged landlords' homes into acquirer homes and target homes. The estimates show that the additional increase in rent is significant only for acquirer properties. These results also help address the endogeneity concern regarding the selection of targets; otherwise, one would expect the target properties to have a greater increase in rent postmergers.[27]

[27] An interesting observation is that the coefficient for $Post \times Target$ is significantly positive, meaning that target properties had an increase in rent postmergers regardless of the degree of overlap. This observation likely reflects

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Table 12
Local market competition and change in neighborhood crime rate around mergers

A

Dependent variable:	ln(Crime)		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post × Treated	−0.0058	−0.0377**	−0.0040**
	(−0.54)	(−2.09)	(−2.49)
Post	0.0047	0.0085*	0.0091*
	(0.90)	(1.88)	(1.88)
Treated	−0.0233	0.0047	−0.0005
	(−1.35)	(0.15)	(−0.19)
HHI	0.1786***	0.1627***	0.1795***
	(2.74)	(2.66)	(2.85)
Post × HHI	0.0406	0.0383	0.0366
	(1.47)	(1.47)	(1.35)
Treated × HHI	−0.0250	−0.1404	−0.0136
	(−0.38)	(−1.07)	(−1.32)
Post × Treated × HHI	0.0146	0.0942	0.0041
	(0.33)	(1.07)	(0.56)
County × Year-month FEs	Yes	Yes	Yes
Census tract FEs	Yes	Yes	Yes
Adjusted R^2	.886	.886	.886
Observations	227,538	227,538	227,538

B

	(1)	(2)	(3)
Post × Treated	0.0161	0.0194	0.0019
	(0.88)	(0.49)	(0.51)
Post	0.0151	0.0178*	0.0156
	(1.52)	(1.90)	(1.61)
Treated	−0.0286	−0.0565	−0.0074
	(−0.95)	(−0.87)	(−1.18)
Number of rivals	0.0134	0.0152	0.0113
	(1.17)	(1.39)	(1.01)
Post × Number of rivals	−0.0125**	−0.0120**	−0.0114**
	(−2.30)	(−2.41)	(−2.22)
Treated × Number of rivals	0.0062	0.0237	0.0026*
	(0.68)	(1.37)	(1.71)
Post × Treated × Number of rivals	−0.0062	−0.0164	−0.0014
	(−0.78)	(−1.16)	(−1.29)
County × Year-month FEs	Yes	Yes	Yes
Census tract FEs	Yes	Yes	Yes
Adjusted R^2	.887	.887	.887
Observations	232,397	232,397	232,397

(*Continued*)

4.6.3 Postmerger change in vacancy. To the extent that the supply of rental properties at the local level is inelastic, the vacancy rate should reflect the market demand for rental properties. As our theoretical model shows, the effect of mergers on the market demand for rental properties is ambiguous. On the one hand, rent increases due to market power could reduce the market demand for rental properties. On the other hand, improvements in neighborhood quality

the idea that the targets' portfolios in general had an upward price trend that was (endogenously) expected by the acquirer. At the same time, acquirers utilized their market power to raise the rent of their own properties postmergers.

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Table 12
(Continued)

	C		
Dependent variable:	ln(Crime)		
Treatment variable:	$I(\Delta Properties>0)$ (1)	$I(\Delta Properties>5)$ (2)	$\Delta Properties$ (3)
Post × Treated	0.0053	0.0039	−0.0000
	(0.35)	(0.11)	(−0.00)
Post	−0.0055	−0.0043	−0.0037
	(−1.04)	(−0.96)	(−0.77)
Treated	−0.0173	0.0016	0.0017
	(−0.93)	(0.04)	(0.50)
Market share	−0.5537*	−0.5372**	−0.5694**
	(−1.66)	(−2.36)	(−2.48)
Post × Market share	−0.3614	−0.2763	−0.2626
	(−1.20)	(−1.18)	(−1.14)
Treated × Market share	0.5137	0.7259	0.0318
	(1.22)	(1.47)	(1.10)
Post × Treated × Market share	−0.2485	−0.7708*	−0.0415**
	(−0.60)	(−1.65)	(−2.22)
County × Year-month FEs	Yes	Yes	Yes
Census tract FEs	Yes	Yes	Yes
Adjusted R^2	.887	.887	.887
Observations	232,349	232,349	232,349

This table presents estimates of DiD regressions from a sample of census-tract-month observations from 12 months before the announcement to 12 months after completion of the three horizontal mergers of institutional investors. The sample includes census tracts covered by either of the merging firms. We exclude observations between the announcement and completion dates. The dependent variable is the natural logarithm of one plus the number of criminal incidents. In column 1, *Treated* is a binary variable that equals one if the merging firms gain at least one property after the merger. In column 2, *Treated* is a binary variable that equals one if the merging firms gain more than five properties after the merger. In column 3, *Treated* is the number of properties the merging firms gain after the merger. *Post* is a binary variable that equals one after completion of the mergers. *HHI* is the (demeaned) Herfindahl-Hirschman index measuring the concentration of institutional SFR investors in the census tract. *Number of rivals* is the number of rival institutional SFR investors in the census tract. *Market share* is the postmerger market share, defined as the ratio of the number of properties owned by the merged landlord to the number of rental units reported by the Census Bureau. We include county × year-month fixed effects and census tract fixed effects in the regressions. We report t-statistics using standard errors clustered by census tract in parentheses. $*p<.1$; $**p<.05$; $***p<.01$.

could increase the demand for properties. Therefore, whether landlord mergers result in a net increase or decrease in the demand for local rental properties (and thus the vacancy rate) depends on which one of the opposing effects is stronger. Table 14 presents estimates of DiD models using data on the quarterly vacancy rate of residential properties at the census tract level from four quarters before the announcement to four quarters after completion of institutional mergers, to examine the postmerger change in the vacancy rate.

The estimates show that after completion of the mergers, overlapped neighborhoods saw an increase of 0.0455 percentage points in the vacancy rate compared with other neighborhoods. This amounts to a 2.5% increase from the mean level of the vacancy rate (1.81 percentage points). The evidence on the vacancy rate suggests that the market power effect of mergers appears more pronounced than the quality effect, resulting in a net decrease in demand and an increase in the vacancy rate.

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Table 13
Change in property-level rent around mergers

A

Dependent variable:	ln(Rent)		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post	0.0058*	0.0049**	0.0066***
	(1.77)	(2.04)	(2.83)
Treated	−0.0080*	−0.0117***	−0.0004*
	(−1.72)	(−3.59)	(−1.87)
Post × Treated	−0.0028	−0.0025	−0.0005**
	(−0.69)	(−0.59)	(−2.12)
Merged landlord	0.0270***	0.0249***	0.0254***
	(9.81)	(10.59)	(10.94)
Post × Merged landlord	−0.0048	−0.0019	−0.0018
	(−1.48)	(−0.69)	(−0.68)
Treated × Merged landlord	−0.0029	0.0021	0.0000
	(−0.77)	(0.52)	(0.05)
Post × Treated × Merged landlord	0.0119***	0.0093*	0.0005**
	(2.59)	(1.95)	(2.25)
Census tract × Year-month FEs	Yes	Yes	Yes
Adjusted R^2	.679	.679	.679
Observations	152,427	152,427	152,427

B

	(1)	(2)	(3)
Post	0.0056*	0.0051**	0.0070***
	(1.72)	(2.10)	(2.92)
Treated	−0.0082*	−0.0117***	−0.0004*
	(−1.77)	(−3.60)	(−1.85)
Post × Treated	−0.0025	−0.0026	−0.0005**
	(−0.62)	(−0.63)	(−2.23)
Acquirer	0.0290***	0.0252***	0.0256***
	(8.94)	(9.23)	(9.64)
Post × Acquirer	−0.0091**	−0.0076**	−0.0077**
	(−2.13)	(−2.19)	(−2.32)
Treated × Acquirer	−0.0071	0.0003	−0.0001
	(−1.58)	(0.07)	(−0.24)
Post × Treated × Acquirer	0.0145**	0.0153***	0.0009***
	(2.49)	(2.60)	(3.15)
Target	0.0230***	0.0243***	0.0252***
	(5.07)	(6.47)	(7.07)
Post × Target	0.0024	0.0073*	0.0073*
	(0.49)	(1.84)	(1.96)
Treated × Target	0.0048	0.0047	0.0001
	(0.85)	(0.80)	(0.46)
Post × Treated × Target	0.0074	−0.0002	0.0000
	(1.12)	(−0.03)	(0.06)
Census tract × Year-month FEs	Yes	Yes	Yes
Adjusted R^2	.679	.679	.679
Observations	152,427	152,427	152,427

This table presents estimates of DiD regressions of property-level rent. The sample includes rental listings for the merged landlords, as well as the neighboring single-family rental homes in the same census block from 12 months before the announcement to 12 months after completion of the three mergers. We exclude observations between the announcement and completion dates. The dependent variable is the natural logarithm of listed rent. In column 1, *Treated* is a binary variable that equals one if the merging firms gain at least one property after the merger. In column 2, *Treated* is a binary variable that equals one if the merging firms gain more than five properties after the merger. In column 3, *Treated* is the number of properties the merging firms gain after the merger. *Post* is a binary variable that equals one after completion of the mergers. *Merged landlord* is a binary variable that equals one for homes listed by the merged landlords. *Acquirer* (*Target*) is a binary variable that equals one for homes listed by the acquirer (target). We include census tract × year-month fixed effects in the regressions. We report t-statistics using standard errors clustered by census tract in parentheses. $*p < .1$; $**p < .05$; $***p < .01$.

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Table 14
Change in the vacancy rate around mergers

Dependent variable:	Vacancy rate		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post × Treated	0.0455**	0.0402*	0.0026
	(2.55)	(1.82)	(1.38)
Post	0.0174***	0.0076*	0.0132***
	(2.89)	(1.96)	(2.99)
Treated	0.0765***	0.0652***	0.0074***
	(5.16)	(3.85)	(5.66)
County × Year-Quarter FEs	Yes	Yes	Yes
Census tract FEs	Yes	Yes	Yes
Adjusted R^2	.947	.947	.947
Observations	154,905	154,905	154,905

This table presents estimates of DiD regressions from a sample of census-tract-quarter observations from four quarters before the announcement to four quarters after completion of the three horizontal mergers of institutional investors. The sample includes census tracts covered by either of the merging firms. We exclude observations between the announcement and completion dates. The dependent variable is the vacancy rate for a census tract (in percentage) reported by the U.S. Postal Service. In column 1, *Treated* is a binary variable that equals one if the merging firms gain at least one property after the merger. In column 2, *Treated* is a binary variable that equals one if the merging firms gain more than five properties after the merger. In column 3, *Treated* is the number of properties the merging firms gain after the merger. *Post* is a binary variable that equals one after completion of the mergers. We include county × year-quarter fixed effects and census tract fixed effects in the regressions. We report t-statistics using standard errors clustered by census tract in parentheses. $*p<.1$; $**p<.05$; $***p<.01$.

4.6.4 Neighborhood home value. If the postmerger increase in neighborhood rent is purely driven by improvements in neighborhood quality, then the selling prices of local properties should simultaneously increase. However, if the rent increase is mutually driven by market power and neighborhood quality, one would expect the increase in rent to be quicker than the increase in home selling prices because the merged landlords could raise the listed rent immediately using their market power before the enhanced neighborhood quality is priced in. To test this possibility, we reestimate Model (2) but replace $ln(ZRI)$ with the natural logarithm of the Zillow Neighborhood Home Value Index for single-family homes ($ln(ZHVI)$) as the dependent variable. The estimates reported in panel A of Table 15 show reveal no significant increase in home value for overlapped neighborhoods in the 1 year after completion of the mergers. Since improved neighborhood safety as a result of the mergers may eventually contribute to higher home value in the longer term, we reestimate the DiD model of home value by including 24 months of observations postmerger in the sample to examine the long-term change in home value. The estimates in panel B of Table 15 show that home value in overlapped neighborhoods significantly increased after the first year following the merger (i.e., t+1 to t+2). Therefore, both home rental prices and selling prices in overlapped neighborhoods eventually increased after the mergers. The more gradual increase in home value is consistent with improved neighborhood quality, while the rent increase might be the joint effect of improved quality and landlords' market power in the local rental market.

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Table 15
Change in neighborhood home value around mergers

A. Change in home value 1-year postmerger

Dependent variable:	ln(ZHVI)		
Treatment variable:	$I(\Delta Properties > 0)$ (1)	$I(\Delta Properties > 5)$ (2)	$\Delta Properties$ (3)
Post × Treated	0.0026	−0.0008	0.0001
	(1.15)	(−0.23)	(1.13)
Post	−0.0041***	−0.0011	−0.0008
	(−3.46)	(−1.58)	(−1.30)
Treated	−0.0138***	−0.0062**	−0.0001
	(−4.99)	(−2.32)	(−1.31)
County × Year-month FEs	Yes	Yes	Yes
Neighborhood FEs	Yes	Yes	Yes
Adjusted R^2	.995	.995	.995
Observations	68,920	68,920	68,920

B. Change in home value two years postmerger

	(1)	(2)	(3)
Post × Treated	0.0062**	0.0009	0.0003**
	(2.31)	(0.21)	(2.27)
Post	−0.0045***	−0.0011	−0.0009
	(−3.38)	(−1.43)	(−1.33)
Treated	−0.0135***	−0.0067***	−0.0002**
	(−5.01)	(−3.00)	(−2.44)
County × Year-month FEs	Yes	Yes	Yes
Neighborhood FEs	Yes	Yes	Yes
Adjusted R^2	.993	.993	.993
Observations	109,038	109,038	109,038

This table presents estimates of DiD regressions in which the dependent variable is the natural logarithm of Zillow Neighborhood Home Value Index (ZHVI) for single-family residences. Panel A (B) uses a sample of neighborhood-month observations from 12 months before the announcement to 12 (24) months after completion of the three horizontal mergers of institutional investors. Both samples include only neighborhoods covered by either of the merging firms and exclude observations between the announcement and completion dates. In column 1, *Treated* is a binary variable that equals one if the merging firms gain at least one property after the merger. In column 2, *Treated* is a binary variable that equals one if the merging firms gain more than five properties after the merger. In column 3, *Treated* is the number of properties the merging firms gain after the merger. *Post* is a binary variable that equals one after completion of the mergers. We include county × year-month fixed effects and neighborhood fixed effects in the regressions. We report t-statistics using standard errors clustered by neighborhood in parentheses. $*p<.1$; $**p<.05$; $***p<.01$.

4.6.5 Rent increase due to market power versus neighborhood safety. As we illustrated in Section 2, postmerger rents could increase for two reasons. First, merged landlords can raise rent through the market power channel. Second, a lower crime rate can increase the market demand for rental homes. In this section, we estimate the relative contribution of neighborhood quality and market power to observed postmerger rent increases using quality-adjusted neighborhood rent (see, e.g., Reher 2021). We use a neighborhood-year-month panel data set with neighborhoods covered by any of the merging landlords from 2014 (i.e., the year prior to the first merger) to 2018 (i.e., the year after the third merger) to estimate a quasi-hedonic regression of neighborhood rent. The dependent variable is $ln(ZRI)$. The independent variables are $ln(Crime)$ lagged by 1 month and local demographic characteristics, including median household income (in thousands); the poverty rate; and the percentage of

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non-Hispanic whites, Black, Hispanic, Asian, American Indian, and Alaskan Indigenous populations.[28] We include county × year-month fixed effects in the regression.[29] Table A6 in the Internet Appendix presents the hedonic estimates. In column 1, we present a scaled-back model with $ln(Crime)$ as the only independent variable. The estimate shows that a 1% reduction in the neighborhood crime rate is related to a 0.05% increase in neighborhood rent. In column 2, once we control for other neighborhood characteristics, the negative relation between the neighborhood crime rate and rent becomes weaker: a 1% reduction in the neighborhood crime rate is related to a 0.007% increase in neighborhood rent.

Next, we take the predicted values and the residual values of $ln(ZRI)$ from the estimates in column 2 of Table A6 and use the residual $ln(ZRI)$ as a measure of quality-adjusted neighborhood rent. Table A7 presents estimates of Model (2) using the predicted values and the residual values of $ln(ZRI)$ as the dependent variables. The estimates show that, while the postmerger change in quality-adjusted rent in overlapped neighborhoods remains significantly positive, the change in the quality-predicted rent is not significant.

Collectively, the evidence in Section 4.6 suggests that the increase in neighborhood rent that occurs within the first year after mergers is mainly driven by the market power channel. While the home rental market may eventually price the improvements in neighborhood safety into rents in the longer term, the short-term price changes are more likely an outcome of the merged landlords using their market power to raise listed prices immediately.[30]

4.7 Postmerger changes in rent and crime relative to areas unaffected by mergers

So far, we have focused on the geographic variation *within* the union set of neighborhoods covered by the merging firms in order to isolate the causal effect of institutional landlords' scale and market share from the selection bias of mergers. However, the effects of the scales of economies and market power brought about by institutional mergers can be further reaching than overlapped neighborhoods defined by an econometrician. In this subsection, we extend the sample to all neighborhoods and census tracts within a county covered by the merging landlords.

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[28] Racial groups that are left out from the hedonic regression and used as the base group are "Native Hawaiian" and "Other Pacific Islander alone and not Hispanic or Latino."

[29] We collect census tract level demographic information from the American Community Survey 5-Year Estimates. Since neighborhood rent is measured at the Zillow neighborhood level, we impute the demographic values by taking the population-weighted average across census tracts that overlap with the neighborhoods.

[30] A caveat to our quasi-hedonic regression is that institutional landlords may improve neighborhood quality in ways not measured in our model. For example, institutional landlords could affect the composition of local residents and create new amenities our covariates do not capture. This is in line with the observations in the gentrification literature (e.g., Guerrieri, Hartley, and Hurst 2013).

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Table 16
Comparing rent between neighborhoods with and without merging firms

Dependent variable:	ln(ZRI)	
	(1)	(2)
Post	−0.0015***	−0.0017***
	(−4.35)	(−4.78)
$I(Properties > 0)$	0.0030***	
	(3.45)	
Post × $I(Properties > 0)$	0.0069***	
	(7.04)	
$I(\Delta Properties > 0)$		0.0004
		(0.27)
Post × $I(\Delta Properties > 0)$		0.0095***
		(6.44)
$I(Properties > 0\ \&\ \Delta Properties = 0)$		0.0036***
		(4.00)
Post × $I(Properties > 0\ \&\ \Delta Properties = 0)$		0.0061***
		(5.92)
County × Year-month FEs	Yes	Yes
Neighborhood FEs	Yes	Yes
Adjusted R^2	.996	.996
Observations	182,932	182,932

This table presents estimates of DiD regressions from a sample of neighborhood-month observations from 12 months before the announcement to 12 months after completion of the three horizontal mergers of institutional investors. The sample includes all neighborhoods (with available data) in the counties covered by either of the merging firms. We exclude observations between the announcement and completion dates. The dependent variable is the natural logarithm of the Zillow Neighborhood Rental Index (ZRI) for single-family residences. $I(Properties > 0)$ is a binary variable that equals one if either of the merging firms has properties in the neighborhood. $I(\Delta Properties > 0)$ is a binary variable that equals one if both of the merging firms have properties in the neighborhood. $I(Properties > 0\ \&\ \Delta Properties = 0)$ is a binary variable that equals one if only one of the merging firms has properties in the neighborhood. We include county × year-month fixed effects and neighborhood fixed effects in the regressions. We report t-statistics using standard errors clustered by neighborhood in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

We reestimate Models (2) and (3) and identify neighborhoods and census tracts covered by at least one of the merging firms as the treated group ($I(Properties > 0) = 1$) and the rest of the neighborhoods and census tracts as the control group. There are 3,248 neighborhoods with rent data available and 13,836 census tracts with crime data available in this sample.

Table 16 reports the new estimates for Model (2) in the larger sample. Column 1 shows that the ZRI for the treated neighborhood increased by 0.69% after completion of the mergers. Figure 4 shows that the time trend in rent of the treated neighborhoods is in line with that of the control neighborhoods before merger announcements but slopes sharply upward after merger completion.[31] In column 2, we further decompose the treated neighborhoods into overlapped ($I(\Delta Properties > 0) = 1$) and nonoverlapped neighborhoods ($I(Properties > 0\ \&\ \Delta Properties = 0) = 1$) and find that both overlapped and nonoverlapped

[31] Figure A8 in the Internet Appendix also shows that the treated neighborhoods exhibit a continuous increase in rent over the 2-year period postmergers compared with the control neighborhoods. This observation is line with that in Figure A3 and suggests that landlord mergers lead to persistent increases in rent that accumulate over time. Note that we perform this analysis using only the first two mergers because of the limited availability of the *ZRI* data.

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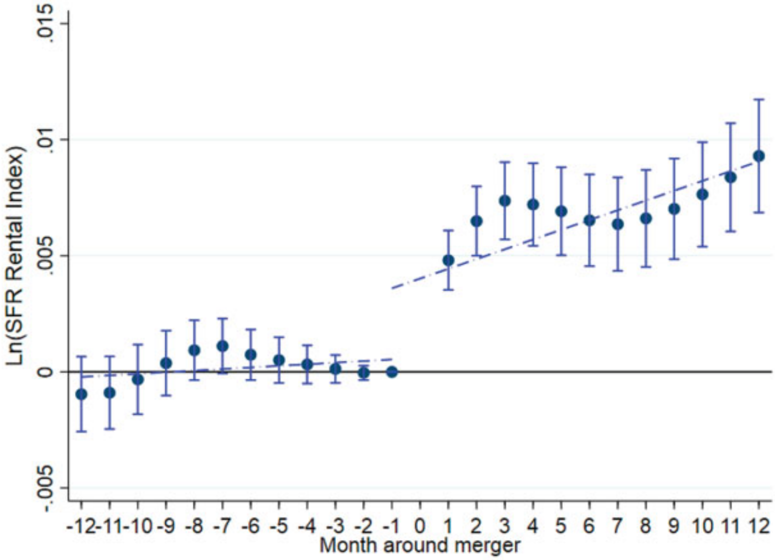

Figure 4
Difference in rent between neighborhoods covered and not covered by merging firms around mergers
The sample includes neighborhood-month observations for all neighborhoods in the counties covered by the merging firms. A neighborhood is defined as treated if it was covered by either of the merging firms prior to the merger. The horizontal axis refers to 12 months before the announcement to 12 months after completion of the mergers. We exclude observations between the announcement and completion dates. Hence, there is an average 3-month gap between month -1 and month 1 in the figure. The vertical axis represents the difference between the treated and control groups in terms of the natural logarithm of the Zillow Neighborhood Rental Index (ZRI) for single-family residences.

neighborhoods experienced an increase in rent relative to the control group. The increase is greater for overlapped neighborhoods, consistent with the findings in Table 4.

Table 17 presents the estimates of Model (3) for the crime rate in the larger sample. Panel A shows that the crime rate of the census tracts covered by the merged firms (the treated group) is 2.78% higher than that of the control group prior to the merger. After the merger, various types of criminal activities significantly decline across the treated census tracts. The total number of criminal incidents decreased by 7.17% in the treated group relative to the control group.

Figure 5 also shows that the crime rate in the treated group sharply declined only after completion of the mergers. In panel B, we again decompose the treated group into overlapped and nonoverlapped census tracts. Since we previously found that the differential change in crime is significant only when the merged firms gained more than five properties in the census tract, we set the cutoff to five properties when defining overlapped neighborhoods in panel B. The results show that both overlapped and nonoverlapped census tracts witness a significant reduction in crime relative to the control group. Consistent with

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Table 17
Comparing crime rate between neighborhoods with and without merging firms

A

Dependent variable:	ln(Crime)						
Type of crime:	All (1)	Assault (2)	Burglary (3)	Robbery (4)	Theft (5)	Drug (6)	Vandalism (7)
Post	0.0203***	0.0083***	0.0105***	0.0049***	0.0148***	0.0033***	0.0027**
	(7.63)	(6.66)	(7.36)	(6.94)	(7.04)	(3.52)	(2.53)
$I(Properties > 0)$	0.0278***	0.0177***	0.0147***	0.0090***	0.0190***	0.0064**	−0.0061**
	(3.75)	(4.98)	(4.07)	(4.96)	(3.47)	(2.36)	(−2.22)
Post × $I(Properties > 0)$	−0.0717***	−0.0248***	−0.0369***	−0.0155***	−0.0532***	−0.0101***	−0.0167***
	(−8.51)	(−6.27)	(−8.09)	(−6.92)	(−7.90)	(−3.45)	(−4.97)
County × Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census tract FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	.843	.702	.723	.419	.771	.623	.622
Observations	843,240	843,240	843,240	843,240	843,240	843,240	843,240

(*Continued*)

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Table 17
(Continued)

B

Dependent variable:	ln(Crime)						
Type of crime:	All (1)	Assault (2)	Burglary (3)	Robbery (4)	Theft (5)	Drug (6)	Vandalism (7)
Post	0.0205***	0.0084***	0.0105***	0.0050***	0.0150***	0.0033***	0.0025**
	(7.73)	(6.78)	(7.41)	(7.09)	(7.16)	(3.55)	(2.42)
$I(\Delta Properties > 5)$	0.0575**	0.0317**	0.0217	0.0219***	0.0506**	0.0092	−0.0183
	(2.22)	(2.51)	(1.39)	(2.95)	(2.51)	(0.97)	(−1.49)
Post × $I(\Delta Properties > 5)$	−0.1117***	−0.0418***	−0.0640***	−0.0255***	−0.0891***	−0.0208*	−0.0480***
	(−4.42)	(−3.23)	(−3.69)	(−2.74)	(−4.01)	(−1.84)	(−3.80)
$I(Properties > 0 \ \& \ \Delta Properties <= 5)$	0.0273***	0.0175***	0.0144***	0.0088***	0.0185***	0.0063**	−0.0064**
	(3.67)	(4.91)	(3.97)	(4.86)	(3.38)	(2.31)	(−2.32)
Post × $I(Properties > 0 \ \& \ \Delta Properties <= 5)$	−0.0708***	−0.0244***	−0.0362***	−0.0152***	−0.0524***	−0.0099***	−0.0160***
	(−8.35)	(−6.12)	(−7.89)	(−6.74)	(−7.73)	(−3.35)	(−4.76)
County × Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census tract FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	.843	.702	.723	.419	.771	.623	.622
Observations	843,240	843,240	843,240	843,240	843,240	843,240	843,240

This table presents estimates of DiD regressions from a sample of census tract-month observations from 12 months before the announcement to 12 months after completion of the three horizontal mergers of institutional investors. The sample includes all census tracts (with available data) in the counties covered by either of the merging firms. We exclude observations between the announcement and completion dates. The dependent variable is the natural logarithm of one plus the number of crimes in the census tract. $I(Properties > 0)$ is a binary variable that equals one if either of the merging firms has properties in the census tract. $I(\Delta Properties > 5)$ is a binary variable that equals one if the merging firms gain more than five properties in the census tract after the merger. $I(Properties > 0 \ \& \ \Delta Properties <= 5)$ is a binary variable that equals one if only one of the merging firms has properties in the census tract or the postmerger gain is equal or less than five properties after the merger. *Post* is a binary variable that equals one after completion of the mergers. We include county × year-month fixed effects and census tract fixed effects in the regressions. We report t-statistics using standard errors clustered by census tract in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

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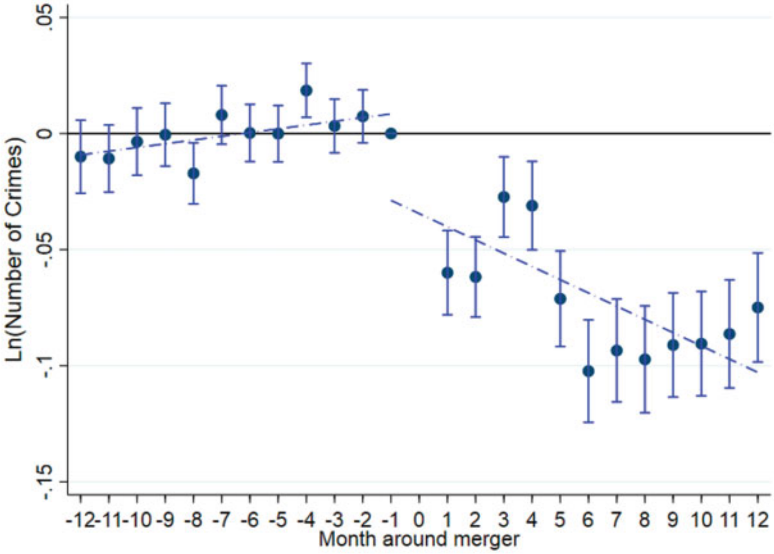

Figure 5
Difference in the number of crimes between census tracts covered and not covered by merging firms around mergers
The sample includes census tract-month observations for all census tracts in the counties covered by the merging firms. A census tract is defined as treated if it was covered by either of the merging firms prior to the merger. The horizontal axis refers to 12 months before the announcement to 12 months after completion of the mergers. We exclude observations between the announcement and completion dates. Hence, there is an average 3-month gap between month −1 and month 1 in the figure. The vertical axis represents the difference between the treated and control groups in terms of the natural logarithm of one plus the number of crimes in a census tract.

the results in Table 5, the number of criminal incidents decreased by 11.17% in overlapped neighborhoods and by 7.08% in nonoverlapped neighborhoods. Therefore, the effect of institutional mergers and the associated gains in scale and market share on rent and neighborhood quality are manifest in all neighborhoods covered by merged firms. These results also further strengthen the external validity of our estimates based on within-portfolio variation.

A plausible explanation for these results is that, to the extent that some nonoverlapped neighborhoods are in close proximity to overlapped neighborhoods, the merged landlords gain scale and market power even in nonoverlapped neighborhoods. In other words, some nonoverlapped neighborhoods can be considered overlapped at a broader level of geography. To gauge this possibility, we further divide the sample based on the degree of overlap at the county level. In Table A8 in the Internet Appendix, we find that the postmerger rent increase and the reduction in crime happen only in overlapped counties. This suggests that the postmerger changes in rent and the crime rate in the broader geographical area still might be related to the gains in local scale and market share, consistent with our proposed explanation.

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5. Conclusion

The recent rise of institutional investors in the single-family home rental market in the United States raises an important question about whether their presence undermines renters' welfare. Using the three largest mergers of institutional investors in the SFR market and granular data on the acquirers and targets' properties, we find that postmerger gains in scale and market share result in an economically moderate raise in rent. At the same time, these neighborhoods also witness a significant reduction in the crime rate, suggesting that large institutional landlords play a beneficial role in enhancing neighborhood safety, while internalizing the cost of the safety measures.

Our evidence provides new insights into the public debate about the impact of institutional landlords on U.S. neighborhoods. Activist groups raise concerns that corporate landlords undermine renters' welfare through higher rent and fees, poor property maintenance, and ruthless evictions. Policy makers have been hesitant to provide financial support to the institutionalization of SFR because of the unclear implications on renters' welfare. Our study provides a more nuanced view of the effect of large institutional landlords on renters' welfare in the postcrisis U.S. neighborhoods: institutional landlords leverage market power to extract greater surplus from renters, while improving the quality of rental services by enhancing neighborhood safety.

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