STAFF REPORTS

NO. 1000
DECEMBER 2021

REVISED
DECEMBER 2022

Financing Constraints and Maintenance Investments: Evidence from Apartments

Lee Seltzer

FEDERAL RESERVE BANK of NEW YORK

Financing Constraints and Maintenance Investments: Evidence from Apartments

Lee Seltzer

Federal Reserve Bank of New York Staff Reports, no. 1000
December 2021; revised December 2022
JEL classification: G3, G31, R30

Abstract

This paper studies whether renters bear the costs of building financing constraints in the form of reduced maintenance. Using a novel data set combining housing code violations from forty-five U.S. cities with apartment financing information, I show that more financially constrained buildings incur more code violations. I then exploit a natural experiment, effectively reducing financial resources for some New York City rent-stabilized buildings. Following the shock, code violations increase for affected buildings relative to controls, and the effect is concentrated among more financially constrained buildings. The results are consistent with financing constraints reducing maintenance.

Key words: corporate finance, commercial real estate, housing code violations

---

Seltzer: Federal Reserve Bank of New York (email: lee.seltzer@ny.frb.org). This paper was previously circulated under the title "The Effects of Leverage on Investments in Maintenance: Evidence from Apartments." The author thanks his dissertation committee members Jonathan Cohn (co-chair), Mike Geruso, Sam Kruger, Laura Starks, and Sheridan Titman (co-chair) for their invaluable feedback. He is also grateful to Aydogan Alti, Taylor Begley (discussant), Nicola Cetorelli, Richard Crump, Jim Costello, DJ Fairhurst, Cesare Fracassi, Iman Dolatabadi, Caitlin Gorback, Greg Hallman, Andrew Haughwout, Emirhan Ilhan, Xuewei Erica Jiang, Hyeyoon Jung, Jangwoo Lee, Will Shuo Liu, Stephan Luck, Tim Park, Alex Priest, Jo˜ao Santos, Clemens Sialm, Sarah Zebar, Albert Sol´e Oll´e (discussant), Anjan Thakor, Xiaoyu David Xu, Kristle Romero Cortes (discussant), Bob Connolly (discussant), and seminar participants at the AREUEA Virtual Seminar Series, the 2022 SFS Cavalcade Meetings, McCombs Salem Center Ph.D. Symposium, the 2020 AFBC Ph.D. Forum/2020 AFBC, the 2021 UEA North American Meetings, the University of Texas at Austin, Stevens Institute of Technology, the New York Fed, the Chicago Fed, the Philadelphia Fed, University of Oxford, Copenhagen Business School, Washington State University, Cal State Fullerton, and the OCC for useful discussion and comments. He also extends special thanks to the McCombs Real Estate Center for providing funding for this project.

1. Introduction

31% of housing units in the United States are rental units,[1] and rental housing is unique because while homeowners can make repairs themselves, renters need to rely on the property-owner to maintain their unit. From the property-owner's perspective, building maintenance is an important investment necessary to keep the asset from depreciating, and like all investments, it is only possible to maintain a building with sufficient financial resources. While previous literature has examined how financing constraints affect investment in single-family homes (Melzer, 2017), a similar analysis does not exist for rental properties. It is particularly important to understand this relationship in the context of rental housing because since moving is costly and often not possible until the termination of the lease, renters are likely to bear costs of poor building maintenance due to financing constraints.

This paper asks whether a building's maintenance is sensitive to the building's financial condition. I construct a unique hand-collected dataset on housing code violations from 45 US cities, and combine them with data on apartment mortgage loan-to-value (LTV) ratios at origination. Since LTV ratios at origination provide information on the size of building debt payments relative to the value of the building, they are a useful proxy for a building's exposure to financing constraints. Using these data, I can examine whether poor building maintenance, indicated by the presence of housing code violations, is more prevalent in more financially constrained buildings.

The analysis proceeds in two steps. First, I implement building-by-year level regressions of housing code violations on building LTV ratios. Previous literature shows that financing constraints relating to either insufficient liquidity or excessive debt lead to reductions in investment (Myers, 1977; Fazzari, Hubbard, and Petersen, 1988), and that these frictions are especially pronounced in cases where the investment primarily benefits nonfinancial stakeholders (Titman, 1984; Maksimovic and Titman, 1991). Therefore, if apartment maintenance is sensitive to financing constraints, buildings with higher LTV ratio mortgages should be less well maintained, and therefore have more code violations.

The analysis reveals an economically meaningful sensitivity of code violations to LTV ratios. In particular, a one standard deviation increase in a building's LTV ratio (14.3 percentage points)

[1] Current Population Survey

is associated with a 0.100 increase in the expected number of violations involving the building in a given year, or equivalently 9.7% of the sample mean number of violations. Given that 86% of building-year observations have no code violations, this is an economically significant increase. Moreover, appendix results using several alternative proxies for the existence of financing constraints, including the debt-service coverage ratio (DSCR), are qualitatively similar.

This initial analysis shows that more financially constrained buildings have more housing code violations. Of course though, financing constraints are not randomly assigned to buildings and may be correlated with omitted variables that are also related to building maintenance. For instance, building-owners might choose higher debt levels for lower quality buildings because future investment opportunities in these buildings are likely to be limited, reducing concerns about financial distress. As a result, unobserved heterogeneity could compromise causal interpretation of panel regression estimates.

The next step of the analysis addresses endogeneity concerns with a natural experiment in the setting of the New York City rent stabilized building stock. Owners of rent stabilized apartments in New York are allowed to pass on a portion of the cost of apartment unit improvements to their tenants through a rent increase. However, I exploit a revision to rent stabilization laws passed in 2011 which decreases the amount that owners could increase monthly rents to recoup improvement costs from one-fortieth of the costs to one-sixtieth. By decreasing future cash flows, the Rent Act effectively shocks building financing constraints by decreasing the financial resources available to spend on maintaining affected buildings.

Importantly, the law change only applies to buildings with more than 35 apartment units. I therefore use rent stabilized buildings with 35 or fewer units as controls to filter out the effects of any time-varying factors affecting the New York City rent stabilized building stock in the aggregate. Specifically, I estimate generalized difference-in-differences regressions with matched samples to compare changes in violations after the law passes in 2011 for rent stabilized buildings with more than 35 units to a group of observationally similar rent stabilized buildings with 35 or fewer units.

Consistent with the hypothesis that increases in financing constraints lead to a reduction in maintenance investment, violations per building increase by 3.76 for buildings with over 35 units relative to control buildings, or more than three-quarters of a standard deviation. The results are robust to a number of alternative specifications, including conducting the tests in the full unmatched sample, varying the difference-in-differences time window, and other variations of the difference-in-differences test construction. Overall, the results provide clear evidence that the Rent Act leads to a reduction in building maintenance.

If financing constraints lead building-owners to invest less in building maintenance, we should observe a more severe decrease in code violations after the shock for buildings that were more financially constrained prior to the shock. To test this hypothesis, I examine the heterogeneity of the effect of the Rent Act on code violations by financing constraints ex-ante by estimating the difference-in-differences regression within different LTV ratio terciles. The results are strongest in the top LTV ratio tercile and absent in the bottom LTV ratio tercile. Appendix results show that similar heterogeneities are observed when using several alternative proxies for the existence of financing constraints, including the DSCR. This provides further evidence that the effect of the Rent Act on code violations is related to a building's financial position.

Afterward, I conduct several tests to examine alternative explanations for the change in code violations. For instance, one may worry that the results could be driven by differences between buildings with more than 35 units and other buildings. However, the results are similar when conducting a test limiting the sample to narrow size-bins around the 35-unit cutoff, indicating that the effect of the shock is driven by the building size relative to the cutoff of 35 units.

It is also possible that the results could be driven by differences in the rental rates for treated and control buildings. To control for this possibility, I conduct a test where treated buildings are matched to control buildings according to their base rent right before the law passed, and the results are similar. This implies that it is unlikely the effect is primarily driven by differences in rents for treated and control buildings.

Furthermore, it is possible that the decrease in code violations for treated buildings could be driven by building-owner characteristics. To address this possibility, I match each treated building to the most similar control building within the same building-owner's portfolio in a robustness test. Even when comparing treated buildings to control buildings operated by the same building-owner, there is a large and economically significant increase in code violations for treated buildings relative to control buildings following the Rent Act. It therefore appears unlikely that the results are driven by owner characteristics.

Lastly, it is possible that the results could be driven by changes in New York City's rental markets unrelated to the Rent Act. However, I show that no change in code violations are present for market-rate buildings with more than 35 units. As these buildings are exposed to similar market conditions but not rent stabilization laws, this placebo test provides evidence that the results are driven by changes in rent stabilization laws.

To summarize, the findings in this paper show that a one standard deviation increase in a building's LTV ratio is associated with a 9.7 percentage point increase in the number of code violations relative to the sample mean. Moreover, code violations increase by more than three-quarters of a standard deviation for treated buildings relative to control buildings following the Rent Act, and the effect is concentrated in buildings with high LTV ratio mortgages. This provides evidence that the increase in code violations is driven by reductions in financial resources following the regulation change. Together, the results are consistent with tightening financing constraints leading to decreases in building maintenance.

As renters are the customers of real estate firms, this paper contributes to the literature on how financing constraints can affect a firm's customers. Financial economists have long understood that financing constraints can reduce investment,[2] with negative consequences for firm stakeholders.[3] In particular, existing work shows that insufficient financial resources can reduce product quality when examining supermarkets (Matsa, 2011), airlines (Phillips and Sertsios, 2013), hospitals (Adelino, Lewellen, and McCartney, 2021), product recalls (Kini, Shenoy, and Subramaniam, 2016), exports (Bernini, Guillou, and Bellone, 2015) and marketing services (Malshe and Agarwal, 2015).

The rental housing market has several features making it uniquely appealing for studying how financing constraints affect product markets. For instance, renters face significant non-pecuniary costs to moving due to neighborhood amenities (Bartik, Butler, and Liu, 1992) as well as social network ties (Koşar, Ransom, and Van der Klaauw, 2022). It is therefore more difficult for renters to switch to a competitor than for customers in other product markets, making them especially likely to bear costs from insufficient building maintenance. Additionally, previous work tends to study short-lived investments in product quality, whereas failing to invest in apartment maintenance leads to the asset's depreciation, which could exacerbate the reduction in building maintenance.

[2]See, for example, Myers (1977), Whited (1992), Lamont (1997), Fazzari et al. (1988), Kaplan and Zingales (1997), Kaplan and Zingales (2000), Rauh (2006), Alti (2003), Almeida, Campello, and Weisbach (2004), Opler and Titman (1994), Wittry (2021), and Giroud, Mueller, Stomper, and Westerkamp (2012).

[3]See, for example Titman (1984), Maksimovic and Titman (1991), Cohn and Wardlaw (2016), Bae, Kang, and Wang (2011), Benmelech, Bergman, and Seru (2021), Xu and Kim (2022) and Aiello (2022).

Finally, apartment buildings are homogeneous relative to other product markets, providing a useful laboratory for conducting empirical research. For these reasons, the rental housing market is ideal for studying how financing constraints affect a firm's customers.

Similarly, this paper relates to an emerging literature examining financial frictions in real estate markets. My paper complements work showing financing constraints can reduce maintenance and capital expenditure investments in single-family homes (Melzer, 2017; Li, 2016), as well as work showing that financing constraints can reduce the incentives of homeowners to work (Bernstein, 2021; Bernstein, McQuade, and Townsend, 2021; Donaldson, Piacentino, and Thakor, 2019). This paper studies the market for apartments, which unlike single-family homes tend to be financed with debt that has shorter maturity, is not fully amortizing, and is possibly not even secured by collateral (Ghent, Torous, and Valkanov, 2019). Despite these differences, I show that financing constraints also impact building maintenance in the context of apartment buildings.

This paper is especially related to Liebersohn, Correa, and Sicilian (2022), which shows highly leveraged retail buildings have lower earnings and occupancy rates. While Liebersohn et al. (2022) examines operating information for retail buildings, my novel housing code violations data allows me to observe an outcome directly tied to investment in apartment buildings, and that is especially crucial for the quality of life of renters.

Finally, this paper contributes to both the commercial real estate and urban economics literatures. While several papers examine the decision to invest in commercial real estate,[4] this paper shows that a building's financing structure affects its maintenance investment. This paper also relates to the literature on rent regulation. Previous findings show rent regulation leads to reduced property values (Autor, Palmer, and Pathak, 2014), misallocation of housing (Glaeser and Luttmer, 2003; Munch and Svarer, 2002; Favilukis, Mabille, and Van Nieuwerburgh, 2019), reduced housing supply (Diamond, McQuade, and Qian, 2019) and reduced housing quality (Downs, 1988; Moon and Stotsky, 1993; Sims, 2007). By providing evidence that reductions in housing quality are concentrated in more financially constrained buildings, this paper provides evidence that financial frictions exacerbate unintended consequences of rent regulation.

[4] See, for example Titman (1985), Grenadier (1996), Holland, Ott, and Riddiough (2000), Reher (2021), Favilukis and Van Nieuwerburgh (2021), Chinloy (1980), Arnott, Davidson, and Pines (1983), and Pavlov and Blazenko (2005).

2. Data

2.1. Code Violations Data

I identify poor maintenance of multifamily buildings using municipal code violations. In the United States, city governments create minimum standards of living for building-owners to provide their tenants. Tenants can complain to the city if they feel the building-owner has breached these standards. Examples of problems leading to complaints include infrastructure in need of repair, issues with plumbing, or infestation with rodents. After investigating to determine whether the complaint is valid, the city then serves the building-owner with a code violation, and the owner is required to remedy the issue causing the violation.

Building-owners are typically fined when they incur violations, and in some cases, penalties from violations can be severe. For instance, when building-owners fail to make repairs sufficiently quickly in New York City, the government sometimes makes the repair on their behalf and then bills the building-owner afterward.[5] The billed repair carries the same weight as a tax lien, and sometimes leads to foreclosure.[6] Other potential costs to violations include lawsuits by tenants, continued scrutiny by the municipal government, and damage to the building-owner's reputation. Mortgages also sometimes include clauses deeming either a violation of local laws, including code violations, or a failure to keep a building in good repair grounds for default.

I collect data on housing code violations for various cities throughout the United States.[7] The data are gathered via municipal open data portals if they are available. Otherwise, I submit a Freedom of Information Act (FOIA) request to the relevant city department to obtain data on code violations. The process ultimately yields data on code violations for 45 cities of varying size covering a diverse geographic region throughout the United States. Data are aggregated at the building-by-year level.

Information on the geographic distribution of data are shown in Figure IA.1 and Table IA.1. The aggregate time-series of code violations are shown in Figure IA.2. Note that more code violations are observed as time goes on. There are two main reasons for this. One is that different cities

[5] Source: NYC Government Emergency Repair Program Information

[6] Source: NYC Government Third Party Transfer (TPT) In Rem Program Information

[7] Cities are selected based on their representation in the Real Capital Analytics mortgage data, which is described in Internet Appendix IA.A.

provide data for different windows of time. The other is that the data from New York City only include violations open as of October 1, 2012, leading to a sharp increase in violations observed in New York City in 2012. At the same time, more data are available for some cities than others. Although it is unlikely the availability of code violations data for a building is correlated with both the building's financial resources and maintenance investment, I conduct several robustness checks to ensure variation in the availability of code violations data over time and between cities does not drive results. For instance, I cluster standard errors at the city-level and include zip-code-by-year fixed effects in all regressions. I also include versions of the analysis varying the sample time period, inverse-probability weighting by number of observations in the city, dropping the largest cities from the sample and double-clustering at the city and year levels. The results are broadly similar in all cases.

In some cases, the cities provide the text associated with the violation. Examples of the violations include:

> "Repair the roof so that it will not leak above the ceiling..." – New York, NY

> "Neighbor is running a barber shop out of hisgarage. garage has a waiting room with table chairs, barber chair. Customersall the time of day and night." – Tucson, AZ[8]

> "See Inspector Comments" – Chicago, IL

The examples show that while in some cases it is clear that the violation is due to a failure to maintain the building, this is not always the case. To account for these differences, I note that violations regarding insufficient maintenance investment should require a repair to mitigate. Given this insight, I parse through all violations for which text is provided and identify violations indicating the need for the building-owner to make repairs. I exclude violations indicating the need to make large-scale investments to focus on basic maintenance. However, results are similar if I include large-scale investments. I detail how violations are classified in Internet Appendix IA.A.

Note that due to the vagueness of the text, I likely underestimate the number of violations requiring repairs. For this reason, the results are likely a lower bound of the effect of LTV ratios on code violations. Additionally, for this reason, it is not informative to separately examine those not requiring repairs. More information on this issue is provided in Internet Appendix IA.A.

[8]Text shown as it appears in the violation, preserving typos by code enforcement officials.

2.2. Mortgage Data

Code violations data are merged with building and loan-level data at the building-by-year level according to the building address and zip code. I obtain apartment mortgage and transactions data from Real Capital Analytics (RCA). RCA collects data on transactions of commercial properties throughout the United States from property deeds. RCA contains information on mortgages for apartment buildings issued in transactions greater than or equal to $2.5 million associated with both property sales and mortgage refinancing activity. Data from RCA include building LTV ratios at loan-issuance, transaction prices, loan origination dates, loan interest rates, loan maturity dates, building locations, the number of units in a building, building ages, building-owners and firm types. The data also include lender and originator characteristics. I drop buildings labeled as co-ops, condos, military-owned or subsidized, as well as observations where the number of units, zip code or address is missing.

Finally, I drop observations where the loan has a cross-default provision, which is when a default in a mortgage triggers defaults in other debt on the borrower's books. By excluding mortgages with cross-default provisions, I effectively limit the sample to only properties that enjoy limited liability. This is beneficial for identification purposes as it focuses on the buildings where the building-owner has no incentive to cross-subsidize the asset using cash flows from their other buildings. The tests in this paper therefore capture the effect of building-level financial resources on building-level maintenance.

Several other data sets are used in this paper. They are described in Internet Appendix IA.A.

2.3. Summary Statistics

Table 1 displays summary statistics. The average number of violations incurred per building in each year is 1.029 and the average number of violations per 100 units for each building in a given year is 2.82. On average, each building incurs 0.502 violations requiring a repair per year, which is about half the amount for all violations. 0.14 of all building-year observations have a violation, highlighting that code violations are relatively rare occurrences and typically only happen in the case of severe problems.

3. Examining LTV ratios and code violations

3.1. Panel regressions

In this section, I test whether more financially constrained buildings tend to be less well maintained by evaluating whether code violations are increasing in LTV ratios. I first graphically examine the relationship between code violations and LTV ratios.[9] I residualize LTV ratios for mortgages and code violations at the zip-code-by-year level to control for time-varying local characteristics. I then normalize the residualized LTV ratios to be between 0 and 1, and sort all mortgages into 100 residualized LTV ratio bins (i.e., 0-0.01, 0.01-0.02, etc...). Figure 1 displays average residualized code violations within each of these bins, where the size of each point is proportional to the number of observations in each bin.

Consistent with financially constrained buildings being less well maintained, code violations are increasing in LTV ratios after controlling for zip code level time trends regardless of which measure of code violations are used. To consider violations especially related to maintenance, Figure IA.3 displays plots using only repair violations. Analogously, there is a positive relationship between violations needing a repair and LTV ratios.

The plots show that code violations and LTV ratios are positively correlated. To examine this relationship more formally, I implement the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}, \tag{1}$$

where $Violations_{it}$ is either the number of code violations for building i in year t, the number of code violations per 100 units for building i in year t or an indicator variable equal to one if building i has a code violation in year t. $LTVratio_{it-1}$ is the LTV ratio at origination for the mortgage on building i in year $t-1$. LTV ratios are standardized by subtracting the sample mean and dividing by the sample standard deviation. As the LTV ratio at origination is informative of the size of debt payments over the life of the mortgage, I use the LTV ratio at origination as my main proxy for the existence of building financing constraints. To support the conclusions from these tests, I also include several robustness checks using alternative measures of financial constraint.

[9]In these plots I limit the sample to mortgages with LTV ratios between 0 and 1.

X_{it-1} is a vector of building, loan, lender, and borrower controls. Building-level controls include the building transaction price in millions, the building's age, an indicator variable equal to one if the building is either a mid-rise or a high-rise and the number of units in the building, where RCA defines mid or high-rise buildings as buildings with four floors or more. Borrower-level controls include indicator variables equal to one if a building is owned by a public company, an institutional investor, or by a joint venture respectively and an indicator variable equal to one if there is a pre-existing relationship between the borrower and loan originator. Lender-controls include indicator variable equal to one if a loan is held by a CMBS lender and an indicator variable equal to one if the loan was made by a government lender. Loan-level controls include the loan interest rate, an indicator variable equal to one if a loan is a refinance of a pre-existing loan, an indicator variable equal to one if the mortgage is fixed rate and the mortgage time to maturity. γ_{zt}, κ_v are zip-code-by-year and mortgage issue year fixed effects. Standard errors are clustered at the city-level.

The regression coefficient β_1 can be interpreted as the predicted increase in code violations after an increase in a building's LTV ratio controlling for both zip-code-by-year and mortgage issue year fixed effects as well as building, building-owner, lender and loan controls. This test can be seen as comparing code violations in a given year for similar buildings located in the same zip code that vary based on their mortgage LTV ratios. If more financially constrained buildings are less well maintained, I expect β_1 to be positive.

Table 2 displays regression results. Column (1) displays estimates from a regression of the number of violations on the LTV ratio. A one standard deviation increase in the LTV ratio (14.3 percentage points) predicts 0.100 more violations per year, or 9.7% of the sample mean. Given the majority of buildings in the sample never have a code violation, this is a substantial effect. Examining the control variables, older buildings incur more code violations and more highly valued buildings incur fewer violations.

Column (2) uses the number of violations per 100 units as the dependent variable. A one standard deviation increase in the apartment LTV ratio predicts an increase of 0.294 violations per 100 units, or 10.4% of the sample mean. Column (3) displays results using the violation indicator, which shows that a one standard deviation increase in a building's LTV ratio is associated with a 0.6 percentage point increase in the probability of having a code violation. In all three of these regression specifications, the results are both economically and statistically significant at either the

5% level or the 1% level.

The previous results show that decreases in financing resources available to make investments predict increases in code violations. I next examine whether decreases in financing resources are associated with violations requiring a repair (i.e. new maintenance investment). Results are displayed in Table 3. Again, the estimates in all specifications are positive and statistically significant, providing evidence that more financially constrained buildings tend to be less well maintained.[10]

3.2. Robustness

Several robustness checks are presented in the Internet Appendix. Noting that building LTV ratios at origination may not be the only way to capture financial constraints, Table IA.2 uses debt-service coverage ratios (DSCR) instead. Consistent with the results using LTV ratios at issue, buildings with higher DSCRs, i.e. that can more easily cover debt payments, tend to have fewer violation. I also include results using several other calculations of building LTV ratios. To consider the effect of second mortgages, Table IA.3 includes results using combined LTV ratios. Additionally, Table IA.4 displays regression results using estimated amortized LTV ratios from information provided by RCA. Using all these variations of LTV ratios, the results are similar to those using LTV ratios at issuance. These tests provide further evidence that financially constrained buildings tend to be less well maintained.

Several tests are included to consider whether sample selection may drive the results. To account for concentration of data in certain cities, results using inverse probability weighting by the number of observations in each city are shown in Table IA.5. Table IA.6 also show results excluding the four most widely represented cities (New York, Los Angeles, Houston and Chicago) from the sample. Likewise, to examine whether greater availability of code violations data in larger years are driving the results, Table IA.7 displays results only including observations from 2012 and earlier, Table IA.8 displays results only including observations from 2013 and later, and Table IA.9 includes results double-clustering by city and year. All results are broadly similar to the baseline, indicating the findings are not driven by the composition of the sample.

Note that since code violations are a count variable, efficiency loss can occur in a regression

[10] Note examining violations not requiring repairs is not informative because due to vague text violations requiring repairs may be incorrectly categorized as violations not requiring repairs. More information on this issue is provided in Internet Appendix IA.A.

of code violations on LTV ratios (Cohn, Liu, and Wardlaw, 2022). While the regressions using violations per 100 units help correct for this problem, Table IA.10 includes results of a poisson regression of code violations on LTV ratios, and the conclusions are similar. Lastly, to better control for the quality of a building, Table IA.11 displays regression results using the building's effective age, defined as the time since the building's most recent renovation when available and the building's age otherwise, as a control in place of the building's age, and the results are similar to the baseline.

4. Identification - 2011 NYC Rent Law

4.1. Determinants of Financial Resources

Endogeneity can bias the estimates from Equation 1 since financial resources are not randomly assigned to buildings. To more clearly illustrate why this is the case, Figure 2 displays maps of New York City showing average LTV ratios and capitalization rates (i.e. commercial real estate rate of return) by zip code. Panel (a) displays zip code level apartment LTV ratios within New York City. The region on the lefthand most side of the map is Manhattan, which overall has low LTV ratio mortgages relative to the rest of the city. Additionally, Panel (b) displays the apartment capitalization rate by zip code within New York City. A comparison of the two figures reveals significant overlap between zip codes with high capitalization rates and those with high LTV ratios. This illustrates that owners of buildings in zip codes with higher capitalization rates may be less concerned with ensuring those buildings have sufficient financial resources because these buildings tend to have riskier cash flow streams and therefore have lower returns to maintenance investment.[11]

Since building-owners may endogenously choose to reserve more financial resources for buildings with lower returns to maintenance, it is not possible to causally examine the effect of financing constraints on investment in maintenance without random variation in financial resources. To obtain such variation, I exploit a natural experiment from a shock to financial resources due to a change in future cash flows for rent stabilized buildings in New York City.

[11] A more formal analysis of the determinants of the building LTV ratio is provided in greater depth in Internet Appendix IA.B.

4.2. New York Rent Act of 2011

A change to New York State's rent stabilization laws in 2011 provides such a shock. Approximately one million apartment units in New York City are rent stabilized. If a unit is rent stabilized, the building-owner must abide by the decisions from the New York City Rent Guidelines Board in setting their rent, which are updated annually based on rental market conditions. One key provision of New York rent stabilization is that if either the unit is vacant or the existing tenant agrees, building-owners can make additional increases to rent for qualifying apartment unit improvements, or Individual Apartment Improvements (IAIs). Examples of improvements qualifying as IAIs include replacing equipment such as a stove, renovating the bathroom or replacing the carpeting. Importantly, an investment can only qualify as an IAI if it plausibly increases the value of the apartment unit. This prohibits classifying basic repairs that are needed to prevent the building value from depreciating as IAIs.[12]

Up until 2011, building-owners were allowed to increase the monthly rent by one-fortieth of the value of an IAI. However, New York State revised their rent laws with the passage of the New York Rent Act of 2011 on June 24, 2011. Effective September 24, 2011, owners of buildings with more than 35 units could only raise rent by one-sixtieth of the cost of the improvement, resulting in a substantial decrease in the ability of building-owners to recover costs incurred when making IAIs.[13]

An example using a $5,000 bathroom renovation is displayed in Figure 3. Prior to the law change, building-owners could increase monthly rent by $125 regardless of the building size. After the law change, owners of buildings with more than 35 units could only increase monthly rent by $83.33. If you value a building using discounted cash flows and assume a discount rate of 10%, this reduced the value of the building by $5,000. This law change therefore decreased a building's financial resources, by decreasing its future cash flows conditional on investing in IAIs for buildings with more than 35 units, but not those with 35 or fewer. The Rent Act can therefore be viewed as providing both cross-sectional and time-series variation in the availability of financial resources

[12]More information on New York rent stabilization and IAIs can be found at https://rentguidelinesboard.cityofnewyork.us/

[13]The law also limited the number of times per year that building-owners could legally increase rent upon vacancy, and changed the circumstances under which building-owners can deregulate previously rent stabilized buildings based on either the rent charged or the income of tenants. For reference see the full text of the law here: https://rentguidelinesboard.cityofnewyork.us/wp-content/uploads/2019/10/rentact2011.pdf.

within the relatively homogeneous pool of rent stabilized buildings in New York City.[14]

To reiterate, a key aspect of the rent stabilization law is that basic maintenance investments needed to prevent code violations, such as repairing a toilet or a hole in the roof, are not classified as IAIs. IAIs are better characterized as significant improvements or renovations, which are associated with increases in building value, and even neighborhood value (Helms, 2003). As a result, the law does not directly affect the profitability of basic maintenance investments needed to avoid code violations. At the same time, since building-owners can use operating cash flows from increasing rent following IAIs to finance repairs, the law change reduced building financial resources.

4.3. *Empirical Design: Difference-in-differences*

I compare changes in code violations for New York City rent stabilized buildings affected by the Rent Act with more than 35 units, to those with 35 or fewer units, before and after 2011 by running a difference-in-differences regression. I use a difference-in-differences design as this allows me to exploit both the cross-sectional variation from the size cutoff and the time-series variation from the timing of the passage of the law. The control group is composed of rent stabilized buildings not impacted by the Rent Act.

To control for differences between rent stabilized buildings with more than 35 units and those with 35 or fewer units and to ensure the precision of the estimates, I conduct a one-to-one nearest neighbor matching procedure with replacement. This approach matches each treated building to the control building with the shortest Mahalanobis distance calculated along observable characteristics (Mahalanobis, 1936). By utilizing this approach, I am able to compare each treated building with the most similar control building possible. However, results without matching, both with and without controls, are included in the appendix.

Matching covariates include the building LTV ratio, the most recent transaction price, the building's age, whether a building was owned by an institutional investor, and the zip code level occupancy rate, which captures renter turnover.[15] This sample is then used to estimate the following

[14] In general, rent stabilized buildings in New York City have six or more units and were built in 1974 or prior, or take advantage of certain affordable housing tax abatements. Therefore, the quality of buildings does not differ significantly amongst rent stabilized buildings. For more detail on the composition of the rent stabilized building stock, see https://rentguidelinesboard.cityofnewyork.us/resources/rent-stabilized-building-lists/.

[15] Values of the covariates as of 2010 are used. Matching is conducted using a caliper of 0.5, meaning if for a given treated building there does not exist a control building whose Mahalanobis distance is 0.5 or less (i.e. if the match is not sufficiently precise), I drop it from the sample. I use the adjustment from Abadie and Imbens (2006) to address

difference-in-differences regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it}. \tag{2}$$

In Equation 2, $Violations_{it}$ are either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to one if building i has a violation in year t. $Treat_i$ is equal to one if the building has more than 35 units, $After_t$ is equal to one if the observation is from 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. In reported results, standard errors are clustered at the building-level, but results clustering standard errors at alternative levels are included in the appendix. β_1 can be interpreted as the difference in the change in code violations after 2011 for rent stabilized buildings with more than 35 units in New York City relative to other rent stabilized buildings in New York City, controlling for matched-pair time trends and time-invariant building characteristics. I expect β_1 to be positive if decreases in financial resources available to make investments lead to decreases in maintenance spending (i.e. increases in code violations).

Difference-in-differences regressions use a pre-period of 2007-2010 and a post-period of 2011-2015. To ensure results are not contaminated by building entry and exit from the 2008 financial crisis, I restrict the sample to buildings I can observe for the full sample period. Note that versions of the test varying the difference-in-differences time period are included in the appendix.

One important identifying assumption is changes to the incentives to maintain a building after the Rent Act and basic maintenance spending decisions are only related through changes in financial resources. If large improvements substantially reduce the likelihood of maintenance problems, this compromises the ability to interpret these results causally. While this assumption is not testable, I address it by excluding violations indicating a need to make substantial improvements from the repair violations measure based on the classifications in Internet Appendix IA.A.

Table IA.12 displays summary statistics for the treated and control groups as of 2010 prior to matching, and the two groups appear very different. The difference between the two groups is what motivates the matching procedure. Table IA.13 displays summary statistics for the treated and control groups after matching. Differences between the treated and control groups are smaller in

bias resulting from calculating the Mahalanobis distance with two or more continuous variables.

magnitude than prior to the matching, indicating that the matching is effective. Additionally, I will later present results from a dynamic difference-in-differences regression, which show that there are not observable differences in pre-trends for the treated and control groups.

Next, to better understand if the law affected financial resources, I examine whether building values changed for treated buildings relative to control buildings after the Rent Act. It is extremely challenging to test this since commercial real estate is highly illiquid and transactions rarely occur. However, Figure 4 displays the average capitalization rates over time for treated and control rent stabilized buildings in New York City. Prior to the Rent Act, there was little difference in the trend in capitalization rates for treated and control buildings. However, after the law passed in 2011, capitalization rates increased by about 0.2 percentage points for treated buildings, with no observable change for control buildings. This is consistent with the market pricing in the decrease in value for buildings affected by the Rent Act. Similarly, Table IA.14 examines changes in appraisal values for treated buildings and control buildings from 2010 to 2012. The results show that appraisal values per unit decrease for treated buildings relative to control buildings, providing evidence of a decrease in building values after the Rent Act, consistent with decreasing financing resources.

4.4. Difference-in-Differences Results

Table 4 displays difference-in-differences results. Panel A displays results using all code violations. Column (1) shows results using the number of violations per building, which increase by 3.76 for treated buildings relative to control buildings after the Rent Act. In economic terms, this is more than three-quarters of a standard deviation relative to the distribution of code violations within the full sample. Similarly, column (2) reveals that violations per 100 units increase by 7.71 for treated buildings relative to controls after the Rent Act. Column (3) includes results using the violation indicator, where the probability of a code violation increases by 7.4 percentage points for treated buildings relative to controls after the Rent Act. Overall, the Rent Act appears to correspond with an increase in code violations for buildings affected by the law.

Panel B uses only violations requiring the building-owner to make a repair. Column (1) shows the number of violations requiring a repair per building increase by 2.54 for treated buildings relative to controls following the Rent Act. According to column (2), violations requiring a repair per 100 units increase by 5.60 relative to controls following the Rent Act. Finally, column (3) shows the probability of a violation increases by 9.3 percentage points.

The findings in panel B show that the Rent Act corresponds with a reduction in repairs, leading to an increase in code violations. This provides evidence that building maintenance degraded following the Rent Act, leading to code violations. At the same time, these tests also exclude violations indicating the need to make improvements, implying the result is driven by a reduction in basic maintenance spending rather than major improvements.[16]

As a whole, the results in Table 4 are consistent with affected building-owners incurring more code violations following the Rent Act, particularly when examining the total number of violations per building. Moreover, the results are consistent when examining only violations requiring repairs, implying that the change in violations was due to decreases in maintenance investment as a result of reduced access to financial resources after the Rent Act.

Next, I examine the effect of the Rent Act on treated buildings dynamically by plotting the coefficients and 95% confidence intervals estimated from running the following regression for all outcome variables:

$$Outcome = \Sigma_{j=2007, j\neq 2010}^{2015} \beta_{1j}[Treat_i \times \mathbb{1}(j = t)] + \gamma_i + \kappa_{pt} + \epsilon_{it}. \tag{3}$$

Each β_{1j} can be interpreted as the difference in changes to the outcome variable in year j for New York City rent stabilized buildings larger than 35 units relative to those with 35 units or fewer. The coefficient for 2010 is excluded from the regression, so 2010 is the base year. β_{1j} near zero for $j < 2011$ and $\beta_{1j} > 0$ $j \geq 2011$ would be consistent with the parallel trends assumption.

The results are displayed in Figure 5. Figures 5(a), (b) and (c) contain results where the outcome variables are the number of code violations, the number of code violations per 100 units and the violation indicator respectively. The coefficients are statistically indistinguishable from zero for all years prior to 2011, which is consistent with the assumption that code violations evolved similarly for treated and control buildings before the Rent Act. However, the estimates increase after 2011. In particular, treated buildings have about 2.5 more violations immediately after the law change relative to control buildings. The effect is also persistent. Results for violations requiring repairs are shown in Figure IA.4, and the conclusions are similar.

[16]See Footnote 10

As a whole, the findings in this section show that code violations increase substantially for buildings larger than 35 units relative to other buildings starting in 2011. At the same time, graphical evidence provides no reason to reject the parallel trends assumption. This is consistent with the Rent Act leading to increases in code violations, presumably driven by decreases in maintenance investment.

5. Additional Tests

The results in Section 4.4 show that code violations increase for buildings with more than 35 units after 2011, which I argue is due to decreasing financial resources to make maintenance investments from the Rent Act. In this section, I first show that the increase in violations after the Rent Act is sensitive to the building LTV ratio, providing evidence that the observed increase in code violations is driven by the reduction in financial resources. I then examine several alternative stories that could potentially explain the increase in code violations.

5.1. Sensitivity of the Change in Code Violations to Building LTV Ratio

By decreasing a building's future cash flows, the Rent Act effectively shocked a building's financial resources. Moreover, shocks to financial resources should more negatively affect maintenance investment for buildings with higher LTV ratio mortgages ex ante, as they had fewer financial resources to begin with, and financially constrained firms are more sensitive to cash flow shocks (Almeida et al., 2004). To evaluate this claim, I test whether violations increase more for treated buildings with high LTV ratio mortgages before the shock relative to other treated buildings

To consider whether the Rent Act disproportionately affects more financially constrained buildings, I divide the sample into terciles based on the building LTV ratio calculated prior to the shock. Afterward, I conduct the difference-in-differences analysis in each of these terciles.

Results are displayed in Table 5. Panel A shows the buildings in the bottom tercile of LTV ratios. For all outcome variables examined except the number of violations related to repairs indicator, there is not a statistically significant increase in code violations after the Rent Act. Panel B has regression results for buildings in the second LTV ratio tercile. The estimates are all statistically significant and larger than in Panel A.

Panel C displays results for buildings in the top LTV ratio tercile. Column (1) shows that the number of violations per building increases by 5.76 for treated buildings relative to controls after the Rent Act. Column (2) shows the number of violations per 100 units increases by 13.5 for treated buildings relative to control buildings, an increase that is about double that found using the whole sample. Column (3) uses the violations indicator, which indicates the probability of a violation increases by 11.9 percentage points for high LTV ratio treated buildings relative to high LTV ratio control buildings. In all three specifications, the results for the top LTV tercile are both larger than those observed in Table 4 as well as those observed in the two other terciles. In fact, the effect for the number of violations is more than three times the size of that observed for the bottom LTV tercile. Columns (4) through (6) display results using violations requiring repairs. For all cases except the violation indicator, the effect is once again larger than that observed for both the middle-tercile and the bottom-tercile.

These subsample results provide evidence that the Rent Act led to more significant increases in code violations for more financially constrained buildings. This supports the story that the Rent Act reduced financial resources that treated buildings needed to make important maintenance investments, and that this change in financial resources is what drove the increase in code violations.

5.2. Examining Alternative Stories

The subsample results provide evidence that building financing constraints are an important driving force of the change in code violations following the Rent Act. Next, I examine several alternative explanations for the change in code violations following the Rent Act.

5.2.1. Controlling for Differences in Size

The Rent Act of 2011 only affected buildings with more than 35 units. However, a building's size is related to both its LTV ratio and its propensity to incur code violations, so the change in code violations after the Rent Act could be driven by differences between large and small buildings.

I consider this possibility by repeating the analysis on subsamples containing buildings within narrow size ranges around the 35 unit cutoff. The intuition of this test is similar to that of a regression discontinuity design: buildings sufficiently close to the cutoff are likely very similar. I compare the change in code violations for buildings with a similar number of units at either side of the 35 unit cutoff. Results are presented in Table 6, where Panel A includes buildings with 10-to-60 units, Panel B includes buildings with 15-to-55 units, Panel C includes buildings with 20-to-50 units, Panel D includes buildings with 25-to-45 units and Panel E includes buildings with 30-to-40 units.

In every subsample, there is a positive and statistically significant increase in the probability of having a violation. Moreover, in all subsamples except Panel D, there is a positive and statistically significant increase in the number of violations per building and the number of violations per 100 units. It is even true in the very restrictive 30-to-40 unit subsample, making it appear unlikely that the results are primarily driven by outliers.

To further consider whether the results within size-bins are driven by the buildings closest to the cutoff, I implement the following regression on the subsample of buildings with no more than 75 units:

$$\begin{aligned} Violations_{it} = {} & \beta_1[\mathbb{1}(35 < Units \leq 45)] \times After_t + \beta_2[\mathbb{1}(45 < Units \leq 55)] \times After_t \\ & + \beta_3[\mathbb{1}(55 < Units \leq 65)] \times After_t + \beta_4[\mathbb{1}(65 < Units \leq 75)] \times After_t \\ & + \gamma_i + \kappa_{pt} + \epsilon_{it}. \end{aligned} \tag{4}$$

Each regression coefficient estimates the increase in code violations for each building in a given size-bin relative to buildings with fewer than 35 units. For example, β_1 is the difference in the change in code violations after 2011 for buildings with more than 35 units and less than or equal to 45 units relative to buildings with 35 or fewer units. If the results are not driven by outlier buildings, the effect should not be stronger for larger buildings in the sample relative to buildings closer to the cutoff.

Results are displayed in Table 7, and the estimates for the size-bin closest to the cutoff is statistically significant for the number of violations per building and the number of violations per 100 units. The increase in code violations is also statistically significant for buildings between 45 and 55 units. However, the effect becomes much smaller for buildings between 55 and 65 units, as well as those between 65 and 75 units, in part because there are fewer buildings with 55-to-75 units than those with 35-to-55 units.

Together, these two tests provide evidence that the results are unlikely to be driven by unusually large or small buildings in the sample. These findings provide evidence that the change in code violations after the Rent Act was driven by building size relative to the cutoff of 35 units specified by the law.

5.2.2. Controlling for Differences in Rental Rates

While after the Rent Act, code violations increased for buildings with more than 35 units relative to others, it is possible that differences in rental rates between large and small buildings could drive the results. For instance, if units in rent-stabilized buildings with more than 35 units tend to have lower rents, it is possible their investment decisions could be more sensitive to the distortions induced by the Rent Act, and this increased sensitivity would also be related to the returns on investment for the buildings. If this is the case, the results should be biased upward.

To control for such a possibility, I collect data on rent for rent stabilized buildings from the CoStar Group. I then construct a sample where I match according to the building's rent in 2010 in addition to the covariates used in previous specifications.[17] Another benefit of using this data set is that I can limit the sample to buildings where I observe growth of rents of no more than 2% at the time the law was passed, which allows me to ensure they are complying with the rent stabilization laws.

I repeat the difference-in-differences analysis in Table 8, and find qualitatively similar results to the baseline specification. This provides evidence that the results are not driven by differences in the rental rates of the assets.

5.2.3. Does Building LTV Ratio Proxy for Owner Characteristics?

It is possible that building-owners whose portfolios have larger apartment buildings may have other traits making code violations more likely. For instance, some building-owners may specialize in operating buildings with low returns to investment, and may optimally choose higher debt levels for these buildings. To control for owner-characteristics, I implement difference-in-differences

[17] To conduct this matching a caliper of 1 is used instead of .5 to allow for a larger sample as rental data is only available for a subset of buildings.

regressions on a sample matched within building-owner.[18] This test effectively compares the change in code violations for buildings with more than 35 units after the Rent Act to a control building with the same owner, that has 35 or fewer units, thereby controlling for any systematic differences between building-owners.

Results are displayed in Table 9 and are qualitatively similar to those in the main test. Based on these findings, the increase in violations surrounding the Rent Act is likely not driven by owner-level characteristics.

5.2.4. Are the Results Driven by New York City's Rental Market Conditions

It is possible that the results could be due to other changes in the New York City rental market occurring in 2011 unrelated to the Rent Act. To examine whether results could be driven by New York City rental market conditions, I conduct a placebo test using market rate buildings in New York City, which are subject to similar market conditions but not rent stabilization laws. Results are shown in Table 10. In all specifications, the estimate of β_1 is close to zero and is not statistically significant. From this placebo test, it appears unlikely that the change in code violations following the Rent Act was due to other market conditions.

5.3. Other Robustness Checks

A battery of robustness checks are included to ensure the results are not sensitive to empirical choices made in implementing the difference-in-differences design. To consider whether the choice to proxy for financing constraints with the LTV ratio at origination is driving the results in the subsample analysis, results are presented using alternative measures. Table IA.15 displays results using the DSCR, Table IA.16 displays results using the combined LTV ratio and Table IA.17 displays results using an amortized LTV ratio. In all tests, the change in violations after the Rent Act is absent in the least financially constrained tercile, and in most cases it is strongest in the top tercile.

Rent stabilized buildings with more than 35 units may have lower quality than those that those with 35 or fewer units, which may make building-owners more sensitive to the reduction in cash

[18] Matching specification is the same as in main tests except the institutional investor indicator is excluded since matching is done within owner.

flows from the Rent Act. To better control for this possibility, I conduct a test where I match on a building's effective age, defined as the time since the most recent building renovation when available and a building's age otherwise. Results are displayed in Table IA.18, and are qualitatively similar to those in the main specification, providing further evidence that the results are not driven by differences in building quality for treated and control buildings.

As the number of violations is a count variable, results using it as a dependent variable may have reduced efficiency (Cohn et al., 2022). Although using the violations per 100 units helps with this, Table IA.19 includes results of the difference-in-differences analysis using a poisson regression, and the conclusions are broadly similar.

The results from Section 4 use buildings from the RCA database, which only covers buildings sold in transactions worth over $2.5 million, which could introduce selection bias. To examine a more general sample of buildings, I merge code violations with a list of all buildings required to register with the New York Department of Housing Preservation and Development (HPD). The list contains the number of units in each building, allowing me to conduct the analysis using all rental buildings in New York City. Results are displayed using all rent stabilized buildings registered with the HPD in Table IA.20. For all outcome variables, the results are qualitatively similar to those in the main specifications, showing the findings generalize to a broad population of buildings.

Additionally, the results in the difference-in-differences regressions are robust to variations in the empirical design. For instance, Table IA.21 displays tests using several different time windows. Table IA.22 displays results on the full unmatched sample, both with and without controls. Lastly, Table IA.23 shows results clustering standard errors at the zip-code-level instead of the building-level, while Table IA.24 shows results double-clustering standard errors and the building and year levels. In all of these cases, the conclusions are qualitatively similar.

While the subsample analyses show that the Rent Act had a larger effect on buildings with higher LTV mortgages, it does not show if this difference is statistically significant. To more formally compare treatment effects for buildings in the highest LTV ratio tercile relative to those in the bottom LTV tercile, I implement the comparison of changes in code violations after the Rent Act along the building LTV ratio as a triple-difference regression in Table IA.25, where the "treated" group is buildings in the top LTV tercile and the "control" group is buildings in the bottom LTV tercile. The regression results show statistically significant and negative estimates for the triple-difference coefficient in four out of six specifications, providing further evidence that changes in code violations for treated buildings after the Rent Act is sensitive to building financial constraints.

6. Conclusion

Given the large population of renters in the United States, it is important for researchers and policy-makers to understand how the financing of rental properties can impact those living in the properties. The results in this paper make it clear that financially constrained apartment buildings tend to have lower levels of maintenance investment.

Up until now, there has been little work examining the implications of corporate finance policies on apartment housing. However, this paper makes it clear that building financing constraints have important implications for apartment renters. More broadly, this paper highlights an example of an interesting and socially relevant situation where a firm's financial decisions has notable consequences for its customers. Moreover, since there are frictions to moving from one rental unit to another, this setting is one where the customers are especially likely to bear the costs of financing frictions.

By showing that apartment buildings with higher LTV ratio mortgages have lower basic maintenance investment, these results provide evidence that financial constraints reduce the incentives for firms to make investments that are beneficial for both long-term firm value and stakeholders, but not profitable in the short-run. In this way, this paper highlights an important channel through which the financial structure of an asset can incentivize short-termist decision-making by managers. The findings in this paper also highlight the importance of understanding financing policies of apartment owners for long-term asset values. In particular, this paper speaks to the effects of financial constraints on building depreciation, as well as on the living experience of tenants.

References

Abadie, Alberto, and Guido W Imbens, 2006, Large sample properties of matching estimators for average treatment effects, *Econometrica* 74, 235–267.

Adelino, Manuel, Katharina Lewellen, and William Ben McCartney, 2021, Hospital financial health and clinical choices: Evidence from the Financial Crisis, *Management Science* Forthcoming.

Aiello, Darren J, 2022, Financially constrained mortgage servicers, *Journal of Financial Economics* 144, 590–610.

Almeida, Heitor, Murillo Campello, and Michael S Weisbach, 2004, The cash flow sensitivity of cash, *Journal of Finance* 59, 1777–1804.

Alti, Aydoğan, 2003, How sensitive is investment to cash flow when financing is frictionless?, *Journal of Finance* 58, 707–722.

Arnott, Richard, Russell Davidson, and David Pines, 1983, Housing quality, maintenance and rehabilitation, *Review of Economic Studies* 50, 467–494.

Autor, David H, Christopher J Palmer, and Parag A Pathak, 2014, Housing market spillovers: Evidence from the end of rent control in Cambridge, Massachusetts, *Journal of Political Economy* 122, 661–717.

Bae, Kee-Hong, Jun-Koo Kang, and Jin Wang, 2011, Employee treatment and firm leverage: A test of the stakeholder theory of capital structure, *Journal of Financial Economics* 100, 130–153.

Bartik, Timothy J, John Scott Butler, and Jin-Tan Liu, 1992, Maximum score estimates of the determinants of residential mobility: implications for the value of residential attachment and neighborhood amenities, *Journal of Urban Economics* 32, 233–256.

Benmelech, Efraim, Nittai Bergman, and Amit Seru, 2021, Financing labor, *Review of Finance* 25, 1365–1393.

Bernini, Michele, Sarah Guillou, and Flora Bellone, 2015, Financial leverage and export quality: Evidence from france, *Journal of Banking & Finance* 59, 280–296.

Bernstein, Asaf, 2021, Negative equity, household debt overhang, and labor supply, *Journal of Finance* 76, 2963–2995.

Bernstein, Shai, Timothy McQuade, and Richard R Townsend, 2021, Do household wealth shocks affect productivity? Evidence from innovative workers during the Great Recession, *Journal of Finance* 76, 57–111.

Chinloy, Peter, 1980, The effect of maintenance expenditures on the measurement of depreciation in housing, *Journal of Urban Economics* 8, 86–107.

Cohn, Jonathan B, Zack Liu, and Malcolm I Wardlaw, 2022, Count (and count-like) data in finance, *Journal of Financial Economics* 146, 529–551.

Cohn, Jonathan B., and Malcolm I. Wardlaw, 2016, Financing constraints and workplace safety, *Journal of Finance* 71.

Diamond, Rebecca, Timothy McQuade, and Franklin Qian, 2019, The effects of rent control expansion on tenants, landlords, and inequality: Evidence from San Francisco, *American Economic Review* 109, 3365–3394.

Donaldson, Jason Roderick, Giorgia Piacentino, and Anjan Thakor, 2019, Household debt overhang and unemployment, *The Journal of Finance* 74, 1473–1502.

Downs, Anthony, 1988, Residential rent controls, Technical report.

Favilukis, Jack, Pierre Mabille, and Stijn Van Nieuwerburgh, 2019, Affordable housing and city welfare, Technical report, National Bureau of Economic Research.

Favilukis, Jack, and Stijn Van Nieuwerburgh, 2021, Out-of-town home buyers and city welfare, *Journal of Finance* 76, 2577–2638.

Fazzari, Steven, R Glenn Hubbard, and Bruce C Petersen, 1988, Financing constraints and corporate investment, *Brookings Papers on Economic Activity* 1988, 141–206.

Ghent, Andra C, Walter Torous, and Rossen I Valkanov, 2019, Commercial real estate as an asset class, *Annual Review of Financial Economics* 11, 153–171.

Giroud, Xavier, Holger M Mueller, Alex Stomper, and Arne Westerkamp, 2012, Snow and leverage, *The Review of Financial Studies* 25, 680–710.

Glaeser, Edward L, and Erzo FP Luttmer, 2003, The misallocation of housing under rent control, *American Economic Review* 93, 1027–1046.

Grenadier, Steven R, 1996, The strategic exercise of options: Development cascades and overbuilding in real estate markets, *Journal of Finance* 51, 1653–1679.

Helms, Andrew C, 2003, Understanding gentrification: An empirical analysis of the determinants of urban housing renovation, *Journal of Urban Economics* 54, 474–498.

Holland, A Steven, Steven H Ott, and Timothy J Riddiough, 2000, The role of uncertainty in investment: An examination of competing investment models using commercial real estate data, *Real Estate Economics* 28, 33–64.

Kaplan, Steven N, and Luigi Zingales, 1997, Do investment-cash flow sensitivities provide useful measures of financing constraints?, *The Quarterly Journal of Economics* 112, 169–215.

Kaplan, Steven N, and Luigi Zingales, 2000, Investment-cash flow sensitivities are not valid measures of financing constraints, *The Quarterly Journal of Economics* 115, 707–712.

Kini, Omesh, Jaideep Shenoy, and Venkat Subramaniam, 2016, Impact of financial leverage on the incidence and severity of product failures: Evidence from product recalls, *Review of Financial Studies* 30, 1790–1829.

Koşar, Gizem, Tyler Ransom, and Wilbert Van der Klaauw, 2022, Understanding migration aversion using elicited counterfactual choice probabilities, *Journal of Econometrics* 231, 123–147.

Lamont, Owen, 1997, Cash flow and investment: Evidence from internal capital markets, *Journal of Finance* 52, 83–109.

Li, Lingxiao, 2016, The role of foreclosures in determining housing capital expenditures, *Journal of Real Estate Finance and Economics* 53, 325–345.

Liebersohn, Jack, Ricardo Correa, and Martin Sicilian, 2022, Debt overhang and the retail apocalypse, Technical report.

Mahalanobis, Prasanta Chandra, 1936, On the generalized distance in statistics, National Institute of Science of India.

Maksimovic, Vojislav, and Sheridan Titman, 1991, Financial policy and reputation for product quality, *Review of Financial Studies* 4, 175–200.

Malshe, Ashwin, and Manoj K Agarwal, 2015, From finance to marketing: The impact of financial leverage on customer satisfaction, *Journal of Marketing* 79, 21–38.

Matsa, David A, 2011, Running on empty? Financial leverage and product quality in the supermarket industry, *American Economic Journal: Microeconomics* 3, 137–73.

Melzer, Brian T., 2017, Mortgage debt overhang: Reduced investment by homeowners at risk of default, *Journal of Finance* 72.

Moon, Choon-Geol, and Janet G Stotsky, 1993, The effect of rent control on housing quality change: a longitudinal analysis, *Journal of Political Economy* 101, 1114–1148.

Munch, Jakob Roland, and Michael Svarer, 2002, Rent control and tenancy duration, *Journal of Urban Economics* 52, 542–560.

Myers, Stewart C, 1977, Determinants of corporate borrowing, *Journal of Financial Economics* 5, 147–175.

Opler, Tim C, and Sheridan Titman, 1994, Financial distress and corporate performance, *Journal of Finance* 49, 1015–1040.

Pavlov, Andrey, and George W Blazenko, 2005, The neighborhood effect of real estate maintenance, *Journal of Real Estate Finance and Economics* 30, 327–340.

Phillips, Gordon, and Giorgo Sertsios, 2013, How do firm financial conditions affect product quality and pricing?, *Management Science* 59, 1764–1782.

Rauh, Joshua D, 2006, Investment and financing constraints: Evidence from the funding of corporate pension plans, *Journal of Finance* 61, 33–71.

Reher, Michael, 2021, Finance and the supply of housing quality, *Journal of Financial Economics* 142, 357–76.

Sims, David P, 2007, Out of control: What can we learn from the end of Massachusetts rent control?, *Journal of Urban Economics* 61, 129–151.

Titman, Sheridan, 1984, The effect of capital structure on a firm's liquidation decision, *Journal of Financial Economics* 13, 137–151.

Titman, Sheridan, 1985, Urban land prices under uncertainty, *American Economic Review* 75, 505–514.

Whited, Toni M, 1992, Debt, liquidity constraints, and corporate investment: Evidence from panel data, *Journal of Finance* 47, 1425–1460.

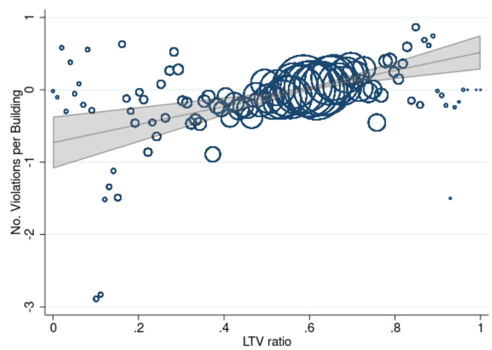
Wittry, Michael D, 2021, (Debt) overhang: Evidence from resource extraction, *The Review of Financial Studies* 34, 1699–1746.

Xu, Qiping, and Taehyun Kim, 2022, Financial constraints and corporate environmental policies, *The Review of Financial Studies* 35, 576–635.

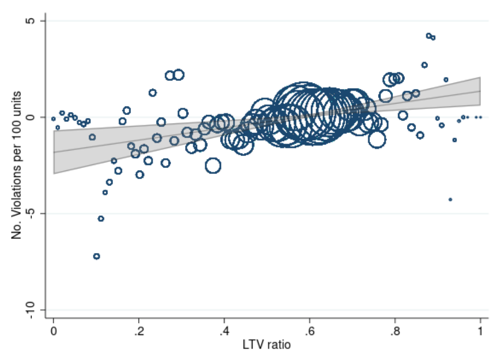
Fig. 1. Correlation Between LTV and Code Violation

Code violations (measured using the number of code violations in a given year, the number of code violations per 100 units in a given year, or an indicator variable equal to one if a property incurs a code violation in a given year) graphed in 100 LTV ratio percentile bins, where the y-axis shows average code violations in a given percentile bin. The size of each dot indicates the number of observations in each bin. Both LTV ratios and code violations are residualized at the zip-code-by-year level. The black lines are from regressions of each code violations outcome on LTV ratios, and the shaded region is the 95% confidence interval. Both scatterplots and lines weighted by number of observations in each bins. Property data are sourced from Real Capital Analytics and code violations data are provided by various municipal governments.

(a) Number of Violations



(b) Violations per 100 units



(c) Violation Indicator

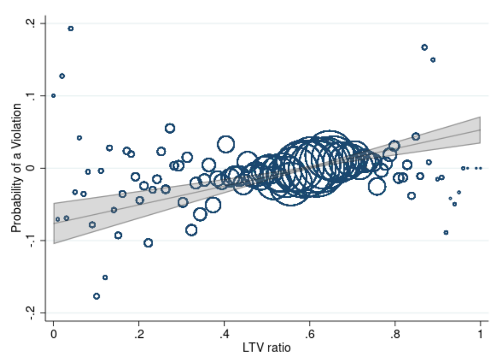
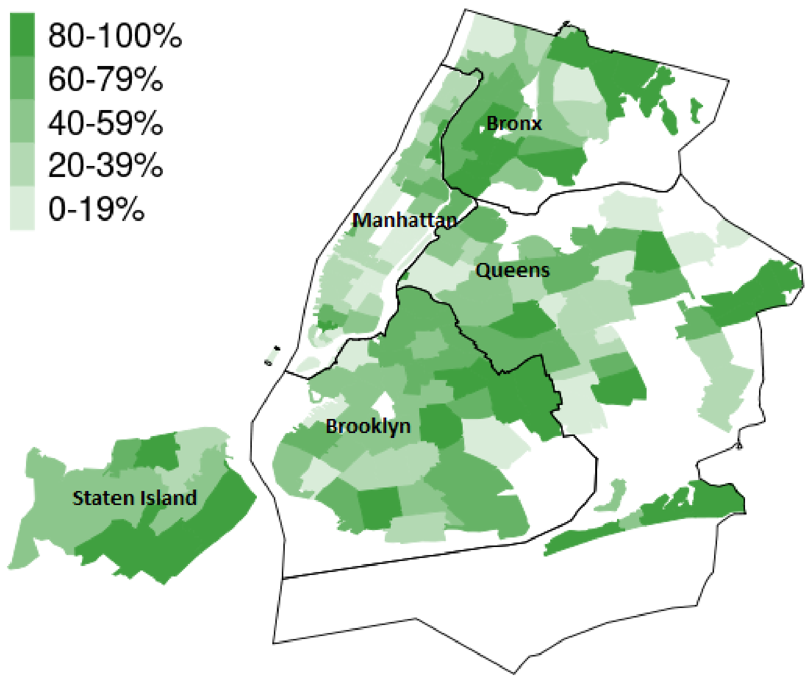


Fig. 2. Within Zip Code Variation in LTV – New York City

Average LTV ratio and capitalization rates across different zip codes in New York City. Property data are sourced from Real Capital Analytics.

(a) Average LTV ratios by NYC Zip Codes



(b) Average Capitalization Rates by NYC Zip Codes

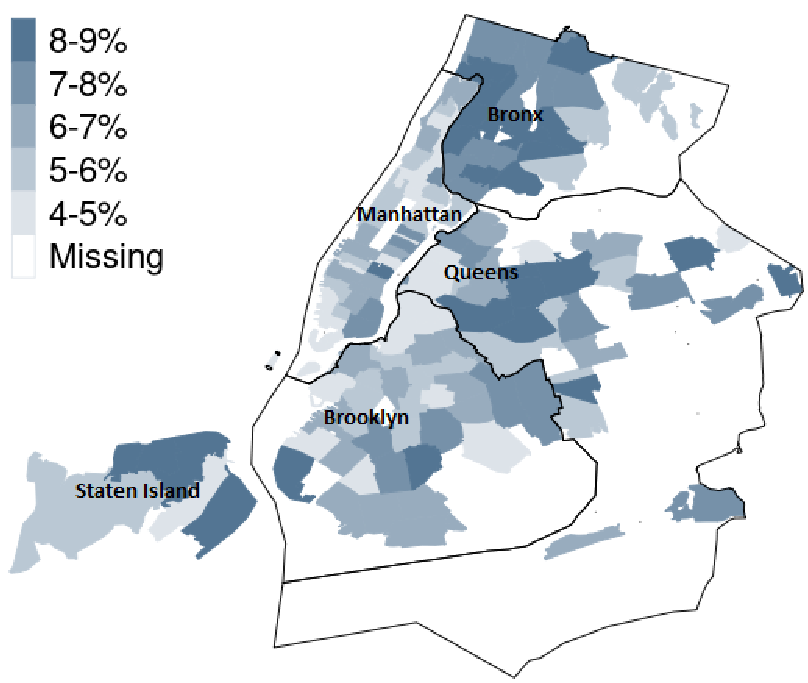


Fig. 3. Impact of Rent Act of 2011 on $5,000 bathroom renovation

This figure is meant to illustrate the effect of the Rent Act on the value of two hypothetical buildings: one with 35 or fewer units and the other with over 35 units. The illustration assumes an improvement equal to $5,000.

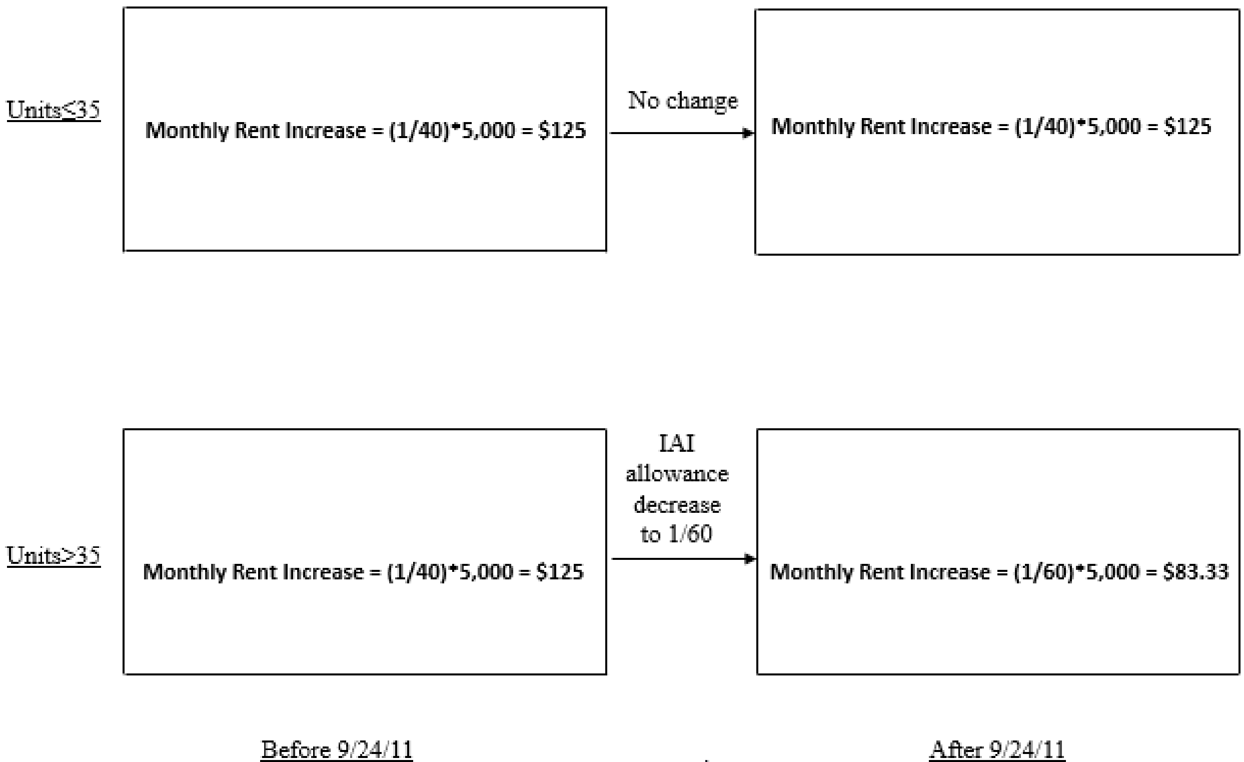


Fig. 4. Change in Capitalization Rates After the Rent Act

Average capitalization rates for treated buildings (i.e., rent stabilized buildings with over 35 units) relative to control buildings (i.e., rent stabilized buildings with 35 or fewer units). Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age, an indicator variable equal to one if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. All covariates are taken as of 2010. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

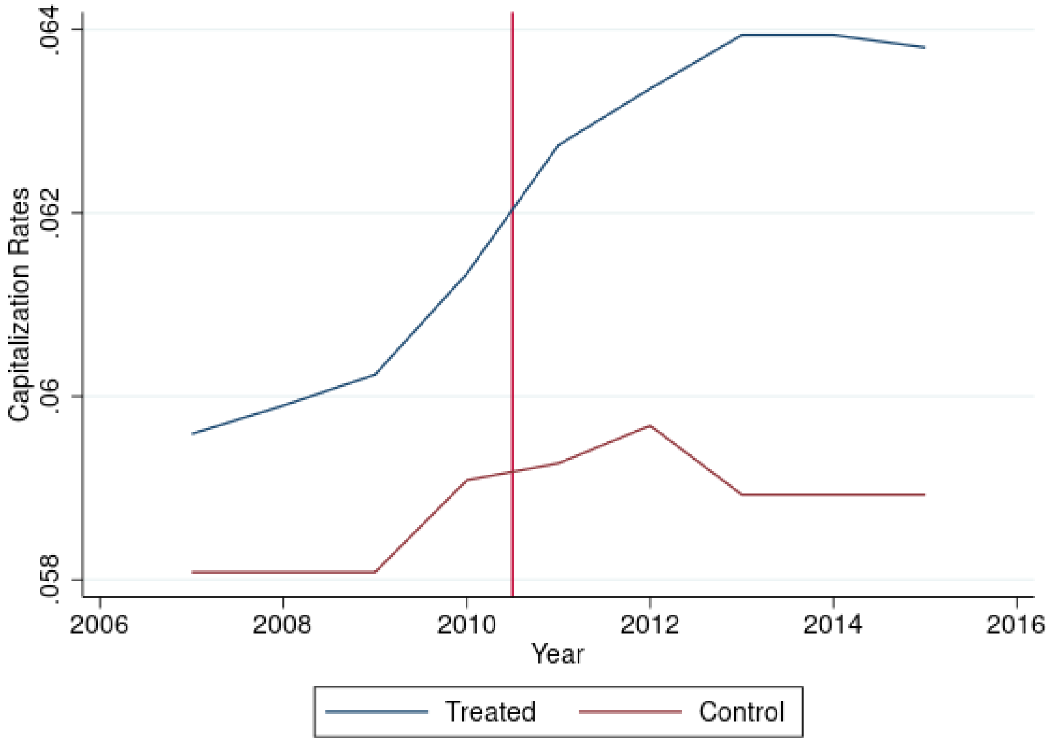
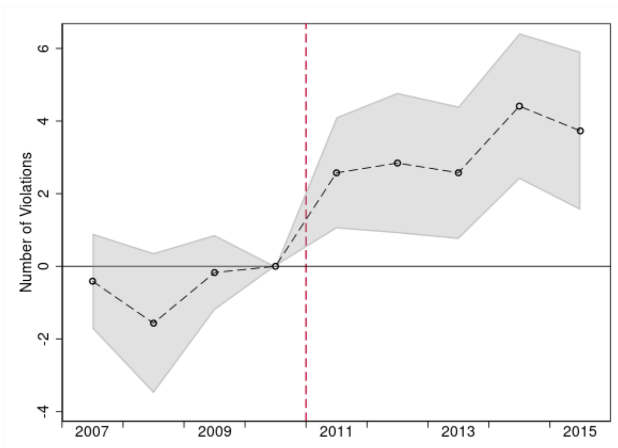


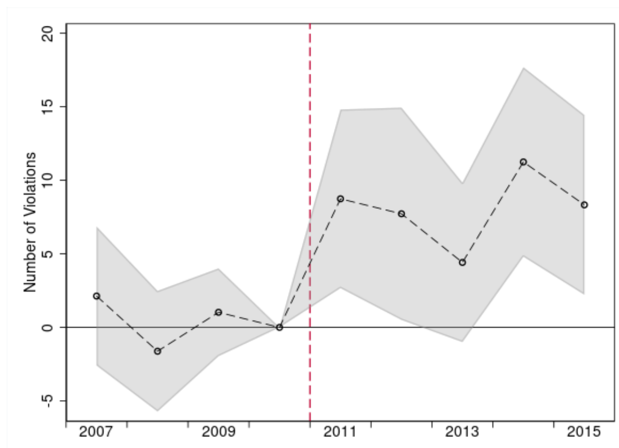
Fig. 5. Dynamic Difference-in-Differences Results – All Code Violations

Regression coefficients from dynamic difference-in-differences regressions comparing trends in code violations for treated buildings (i.e., rent stabilized buildings with over 35 units) relative to control buildings (i.e., rent stabilized buildings with 35 or fewer units). Regressions are run at the annual frequency. Coefficients to the right of the red-dotted line are for 2011 or later. 2010 is excluded from the regression, so estimates can be interpreted as differences in the change in code violations from 2010 until year j for treated relative to control buildings. The shaded region is the 95% confidence interval. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age, an indicator variable equal to one if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. All covariates are taken as of 2010. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

(a) Number of Violations



(b) Violations per 100 units



(c) Violation Indicator

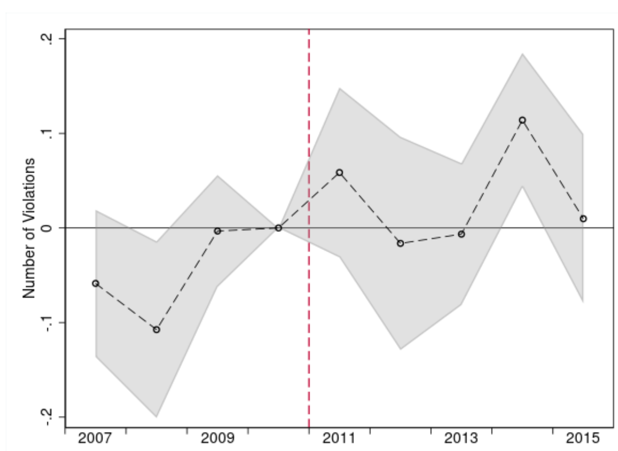


Table 1: Summary Statistics

Number of violations, number of violations per 100 units, number of repair violations, number of repair violations per 100 units, LTV ratios, Combined LTV ratios, interest rates, number of units per building, building ages, Zillow index, DSCR and occupancy rates are winsorized at the 1% and 99% levels. Data are at the building-by-year level. Property data are provided by Real Capital Analytics, and code violations data are from various municipal governments.

Variable	Obs	Mean	Std. Dev.	Min	Max
Number of Violations	62,628	1.029	4.321	0.000	36
Violations per 100 Units	62,628	2.817	13.433	0.000	106.667
Violation Indicator	62,628	0.140	0.347	0.000	1.000
Number of Repair Violations	55,856	0.502	2.566	0.000	23
Repair Violations per 100 Units	55,856	1.501	8.004	0.000	66.667
Repair Violation Indicator	55,856	0.075	0.263	0.000	1.000
LTV Ratio at Issue	62,628	0.654	0.143	0.052	1.259
Combined LTV Ratio	62,628	0.663	0.155	0.052	1.457
Amortized LTV Ratio	62,628	0.620	0.155	0.037	1.256
DSCR	60,399	1.548	0.744	0.000	9.290
Transaction Price (MM)	62,628	12.728	17.874	0.654	107.5
Building Age	62,628	50.463	32.941	1.000	120
Mid/High Rise Indicator	62,628	0.320	0.467	0.000	1.000
Number of Units in Building	62,628	120.709	131.942	5.000	628
Public Owner	62,628	0.009	0.093	0.000	1.000
Institutional Owner	62,628	0.091	0.287	0.000	1.000
Joint Venture	62,628	0.056	0.231	0.000	1.000
Borrower-Originator Relationship	62,628	0.424	0.494	0.000	1.000
CMBS Indicator	62,628	0.587	0.492	0.000	1.000
Loan Held by Government Lender	62,628	0.571	0.495	0.000	1.000
Refinance Indicator	62,628	0.759	0.428	0.000	1.000
Fixed-Rate Indicator	62,628	0.947	0.224	0.000	1.000
Interest Rate	62,628	0.051	0.011	0.023	0.079
Time to Maturity	62,628	7.039	5.358	0.000	40.417
Time Since Most Recent Renovation	19,362	10.72	11.582	0.000	127
Property Capitalization Rate at Origination	34,499	0.062	0.015	0.011	0.130
Property Occupancy Rate at Origination	53,969	0.945	0.061	0.300	1.000
Zip Code Zillow Index	55,975	446,626	389,381.3	34,400	3,338,500

Table 2: Relationship Between LTV Ratios at Issue and all Code Violations.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}.$$

$Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to one if building i incurs a code violation in year t. $LTVratio_{it-1}$ is the LTV ratio at issue for building i in year $t-1$. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v and are zip-code-by-year and mortgage issue year fixed effects. LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. Number of violations, number of violations per 100 units, LTV ratios, transaction prices, building age, number of units per building and interest rates are winsorized at the 1% and 99% levels. Standard errors, clustered at the city level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
LTV Ratio	0.100***	0.294***	0.006**
	(0.016)	(0.093)	(0.002)
Building Controls			
Transaction Price	-0.007**	-0.030**	-0.001***
	(0.003)	(0.012)	(0.000)
Building Age	0.005***	0.023***	0.000***
	(0.001)	(0.007)	(0.000)
Mid/High Rise Indicator	0.421**	0.209	0.031***
	(0.204)	(0.628)	(0.010)
Number of Units in Building	0.001**	-0.003	0.000***
	(0.001)	(0.002)	(0.000)
Building-Owner Controls			
Public Owner	0.019	-0.036	-0.013
	(0.165)	(0.448)	(0.024)
Institutional Owner	-0.025	0.376	0.004
	(0.040)	(0.276)	(0.007)
Joint Venture	0.326*	1.054*	0.034**
	(0.186)	(0.546)	(0.015)
Borrower-Originator Relationship	0.064**	0.156*	0.006
	(0.030)	(0.082)	(0.004)
Lender Controls			
CMBS Indicator	-0.144***	-0.129	-0.001
	(0.049)	(0.111)	(0.007)
Loan Held by Government Lender	-0.326**	-0.368	-0.010*
	(0.137)	(0.375)	(0.006)
Loan Controls			
Interest Rate	4.096	13.338	0.608*
	(2.699)	(11.141)	(0.335)
Refinance Indicator	-0.245	-0.658	-0.017*
	(0.196)	(0.479)	(0.009)
Fixed-Rate Indicator	-0.124	0.237	-0.008
	(0.093)	(0.300)	(0.006)
Time to Maturity	-0.012***	0.013	-0.002***
	(0.003)	(0.014)	(0.000)
FE	Zip-Year	Zip-Year	Zip-Year
	Issue Year	Issue Year	Issue Year
SE Cluster	City	City	City
Adjusted R^2	0.153	0.145	0.208
Observations	62,628	62,628	62,628

Table 3: Relationship between LTV Ratios at Issue and Code Violations Requiring Repairs.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations requiring repairs for building i in year t, the number of violations requiring repairs per 100 units for building i in year t or an indicator variable equal to one if building i incurs a code violation requiring repairs in year t. $LTVratio_{it-1}$ is the LTV ratio at issue for building i in year $t-1$. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v are zip-code-by-year and mortgage issue year fixed effects. LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. Number of violations, number of violations per 100 units, LTV ratios, transaction prices, building age, number of units per building and interest rates are winsorized at the 1% and 99% levels. Standard errors, clustered at the city level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are provided by Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations (1)	Violations/ 100 units (2)	Has Violation (3)
LTV Ratio	0.057***	0.193***	0.003**
	(0.014)	(0.061)	(0.002)
Building Controls			
Transaction Price	-0.001	-0.012**	-0.000***
	(0.002)	(0.005)	(0.000)
Building Age	0.002***	0.013***	0.000***
	(0.000)	(0.004)	(0.000)
Mid/High Rise Indicator	0.255*	0.242	0.032***
	(0.140)	(0.435)	(0.009)
Number of Units in Building	0.000	-0.002	0.000*
	(0.000)	(0.001)	(0.000)
Building-Owner Controls			
Public Owner	-0.017	-0.010	0.005
	(0.072)	(0.147)	(0.016)
Institutional Owner	-0.005	0.273	0.002
	(0.019)	(0.198)	(0.006)
Joint Venture	0.224*	0.720**	0.031**
	(0.124)	(0.344)	(0.013)
Borrower-Originator Relationship	0.050**	0.113***	0.008***
	(0.020)	(0.036)	(0.002)
Lender Controls			
CMBS Indicator	-0.081***	-0.032	0.003
	(0.029)	(0.079)	(0.005)
Loan Held by Government Lender	-0.148	-0.044	0.002
	(0.091)	(0.204)	(0.004)
Loan Controls			
Interest Rate	2.061	8.264	0.400*
	(1.267)	(6.815)	(0.222)
Refinance Indicator	-0.188	-0.525	-0.016
	(0.129)	(0.335)	(0.010)
Fixed-Rate Indicator	-0.038	0.222	-0.006
	(0.028)	(0.188)	(0.004)
Time to Maturity	-0.007***	0.011	-0.001***
	(0.001)	(0.007)	(0.000)
FE	Zip-Year	Zip-Year	Zip-Year
	Issue Year	Issue Year	Issue Year
SE Cluster	City	City	City
Adjusted R^2	0.158	0.133	0.146
Observations	55,856	55,856	55,856

Table 4: Change in Code Violations After the Rent Act of 2011.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age, an indicator variable equal to 1 if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. All covariates are taken as of 2010. Panel A displays results using all code violations and Panel B displays results using only those code violations requiring repairs. Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
$Treat_i \times After_t$	3.764***	7.708***	0.074***
	(0.710)	(2.109)	(0.027)
Adjusted R^2	0.480	0.446	0.620
Observations	5,526	5,526	5,526
Panel B – Repair Violations			
$Treat_i \times After_t$	2.541***	5.600***	0.093***
	(0.472)	(1.387)	(0.027)
Adjusted R^2	0.464	0.440	0.580
Observations	5,526	5,526	5,526
FE	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year
S.E Cluster	Building	Building	Building

Table 5: Change in Code Violations After the Rent Act of 2011 by LTV Ratio.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age, an indicator variable equal to 1 if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. All covariates are taken as of 2010. Panel A displays results examining buildings in the bottom-tercile of LTV ratios, Panel B displays results examining buildings in the middle-tercile of LTV ratios and Panel C displays results examining buildings in the top-tercile of LTV ratios. LTV ratio terciles are assigned based on the LTV ratio of buildings prior to 2011. Number of violations, number of repair violations, number of violations per 100 units and number of repair violations per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Violation type	All Violations			Repair Violations		
Variable	# Violations	Violations/ 100 units	Has Violation	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Bottom LTV Tercile, N=1,746						
$Treat_i \times After_t$	1.583	0.487	0.046	1.279*	1.468	0.044
	(1.302)	(3.949)	(0.043)	(0.762)	(2.249)	(0.051)
Adjusted R^2	0.472	0.440	0.569	0.450	0.427	0.530
Panel B – Mid LTV Tercile, N=1,494						
$Treat_i \times After_t$	4.342***	9.059***	0.059	2.769***	6.329***	0.123***
	(1.045)	(2.553)	(0.043)	(0.635)	(1.573)	(0.038)
Adjusted R^2	0.456	0.402	0.610	0.431	0.403	0.589
Panel C – Top LTV Tercile, N=1,620						
$Treat_i \times After_t$	5.762***	13.548***	0.119***	3.722***	8.801***	0.118**
	(1.314)	(3.696)	(0.046)	(0.931)	(2.609)	(0.047)
Adjusted R^2	0.530	0.489	0.646	0.514	0.474	0.597
FE	Building	Building	Building	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year	Pair-Year	Pair-Year	Pair-Year
S.E Cluster	Building	Building	Building	Building	Building	Building

Table 6: Change in Code Violations After Rent Act of 2011 – With Size Restrictions.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age, an indicator variable equal to 1 if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. All covariates are taken as of 2010. Panel A displays results using buildings with 10-to-60 units, Panel B displays results using buildings with 15-to-55 units, Panel C displays results using 20-to-50 units, Panel D displays results using 25-to-45 units and Panel E displays results using 30-to-40 units. Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – 10-60 units, N=4,635			
$Treat_i \times After_t$	3.335***	7.225**	0.072**
	(0.881)	(2.828)	(0.036)
Adjusted R^2	0.462	0.455	0.615
Panel B – 15-55 units, N=4,075			
$Treat_i \times After_t$	3.708***	8.427***	0.100***
	(0.986)	(3.134)	(0.037)
Adjusted R^2	0.458	0.459	0.621
Panel C – 20-50 units, N=3,492			
$Treat_i \times After_t$	2.996***	7.534**	0.097**
	(1.060)	(3.453)	(0.041)
Adjusted R^2	0.451	0.471	0.607
Panel D – 25-45 units, N=2,187			
$Treat_i \times After_t$	1.958	4.013	0.099**
	(1.336)	(4.016)	(0.042)
Adjusted R^2	0.451	0.451	0.596
Panel E – 30-40 units, N=1,435			
$Treat_i \times After_t$	3.802**	10.097**	0.110**
	(1.668)	(4.819)	(0.048)
Adjusted R^2	0.488	0.488	0.623
FE	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year
S.E. Cluster	Building	Building	Building

Table 7: Change in Code Violations After Rent Act of 2011 by Size-Bin.

This table displays results from the following regression:

$$\begin{aligned} Violations_{it} = &\beta_1[\mathbb{1}(35 < Units \leq 45)] \times After_t + \beta_2[\mathbb{1}(45 < Units \leq 55)] \times After_t \\ &+ \beta_3[\mathbb{1}(55 < Units \leq 65)] \times After_t + \beta_4[\mathbb{1}(65 < Units \leq 75)] \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it}, \end{aligned} \tag{5}$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $\mathbb{1}(35 < Units \leq 45)$ is an indicator variable equal to 1 if building i has more than 35 units and 45 or fewer units, $\mathbb{1}(45 < Units \leq 55)$ is an indicator variable equal to 1 if building i has more than 45 units and 55 or fewer units, $\mathbb{1}(55 < Units \leq 65)$ is an indicator variable equal to 1 if building i has more than 55 units and 65 or fewer units and $\mathbb{1}(65 < Units \leq 75)$ is an indicator variable equal to 1 if building i has more than 65 units and 75 or fewer units. $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. Sample limited to buildings with 75 or fewer units. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age, an indicator variable equal to 1 if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. All covariates are taken as of 2010. Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
$\mathbb{1}(35 < Units \leq 45) \times After_t$	3.359***	8.416**	0.104**
	(1.074)	(3.352)	(0.042)
$\mathbb{1}(45 < Units \leq 55) \times After_t$	4.170***	8.232***	0.063
	(1.112)	(3.172)	(0.041)
$\mathbb{1}(55 < Units \leq 65) \times After_t$	1.477	2.192	0.020
	(1.126)	(3.212)	(0.039)
$\mathbb{1}(65 < Units \leq 75) \times After_t$	3.145**	6.908	0.046
	(1.522)	(4.640)	(0.048)
FE	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year
S.E. Cluster	Building	Building	Building
Adjusted R^2	0.477	0.459	0.628
Observations	5,067	5,067	5,067

Table 8: Change in Code Violations After Rent Act of 2011 – Controlling for Rent.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it}.$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age, an indicator variable equal to 1 if a building was owned by an institutional investor, zip code level occupancy rates and building rent as covariates. All covariates are taken as of 2010. Panel A displays results examining all violations, Panel B displays results examining repair violations. Number of violations, number of repair violations, number of violations per 100 units and number of repair violations per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are provided by Real Capital Analytics, rent data are provided by CoStar Group and code violations data are provided by the New York City government.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
$Treat_i \times After_t$	4.002***	8.837***	-0.001
	(0.834)	(2.284)	(0.036)
Adjusted R^2	0.595	0.556	0.705
Observations	1,170	1,170	1,170
Panel B – Repair Violations			
$Treat_i \times After_t$	2.311***	4.951***	0.009
	(0.544)	(1.557)	(0.030)
Adjusted R^2	0.591	0.555	0.687
Observations	1,170	1,170	1,170
FE	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year
S.E Cluster	Building	Building	Building

Table 9: Change in Code Violations After Rent Act of 2011 – Match Within Owner.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age and zip code level occupancy rates as covariates. All covariates are taken as of 2010. Matching is conducted within building-owner. Panel A displays results using all code violations and Panel B displays results using only those code violations requiring repairs. Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A: All Violations			
$Treat_i \times After_t$	3.821**	8.196*	0.035
	(1.527)	(4.410)	(0.053)
Adjusted R^2	0.540	0.520	0.610
Observations	1,098	1,098	1,098
Panel B: Repair Violations			
$Treat_i \times After_t$	2.711***	6.202**	0.073
	(0.897)	(2.544)	(0.061)
Adjusted R^2	0.517	0.493	0.631
Observations	1,098	1,098	1,098
FE	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year
S.E. Cluster	Building	Building	Building

Table 10: Placebo Test Using Market-Rate Buildings in New York.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age and zip code level occupancy rates as covariates. All covariates are taken as of 2010. Matching is conducted within building-owner. Panel A displays results using all code violations and Panel B displays results using only those code violations requiring repairs. Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
$Treat_i \times After_t$	1.109	1.577	-0.031
	(0.843)	(2.332)	(0.021)
Adjusted R^2	0.438	0.351	0.674
Observations	1,836	1,836	1,836
Panel B – Repair Violations			
$Treat_i \times After_t$	0.765	1.354	-0.008
	(0.502)	(1.383)	(0.023)
Adjusted R^2	0.409	0.321	0.647
Observations	1,836	1,836	1,836
FE	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year
S.E Cluster	Building	Building	Building

Internet Appendix

IA.A. Other Data Information

Violations Relating to Repairs

I collect data on housing code violations for 45 cities.[1] For 41 of these cities, there is a description of the violations. In some cases, this is the actual text of the violation, while in others, there is an ordinance number given referring to the relevant ordinance in the city code of ordinances. I read through several hundred descriptions to determine words indicating that the violation is due to a need to make repairs. The words I identify are:

Improve	Repair	
Improve	Battered	Heat
Install	Boiler	Heater
New	Broken	Heating system
Reconstruct	Busted	Hot water
Rehabilitate	Collapsed	Janitor
Rehabilitation	Crack	Leak
Renovate	Crumbled	Lighting
Renovation	Crumbling	Maintenance
Replace	Crushed	Neglected
Restore	Damaged	Paint
	Decaying	Pave
	Decrepit	Ramshackle
	Defective	Repair
	Demolished	Rickety
	Derelict	Run down
	Dilapidated	Run-down
	Dingy	Seedy
	Electricity	Water in basement
	Fractured	Water supply
	Fragmented	Wreck

To be classified as a violation requiring repairs in my tests I also require that a violation is not classified as a violation requiring improvements so as to address endogeneity concerns in the difference-in-differences analysis in section 4. I parse through the text in stata to check for the appearance of any of the above strings. If no description is available but instead an ordinance is provided, I read through the code of ordinances for the city to identify violations of ordinations including these strings. For Seattle, Greenville SC, Cleveland, although there is neither a detailed description nor is the ordinance included, a vague descriptor or the department that handled the violation is included. If this is the case, I designate violations as relating to repairs as well as possible.

Note that as some of the violation descriptions in the data are very vague, it is inevitable that some violations related to building maintenance will be classified as unrelated to repairs. As

[1] For example, the NYC data can be found at https://data.cityofnewyork.us/Housing-Development/Housing-Maintenance-Code-Violations/wvxf-dwi5.

a result, it is less feasible to determine if a violation is unrelated to repairs than if it is related to repairs. This is readily apparent when examining the examples of code violations provided in subsection 2.1. The first example is clearly related to building maintenance, and would be classified as such because it contains the word "repair". One the other hand, the second violation is unrelated to building maintenance, and accordingly would not be classified as requiring repairs. However, the third violation contains no useful information about the content of the violation, and could either be related to building maintenance or unrelated to building maintenance. Nonetheless, it will be classified as not requiring repairs as it does not contain any words indicating a repair must be made. For this reason, separately examining violations not requiring repairs is not a particularly informative test.

Other Data Sources

I also collect panel data on rental rates and occupancy rates for rent stabilized apartment buildings in New York City from the CoStar Group. CoStar provides operating information on commercial real estate assets, including multifamily buildings. I merge this data with the sample of rent stabilized buildings in New York City by zip code, address and year to use in the difference-in-differences analysis. I identify rent stabilized buildings as of 2011 using data posted publicly at https://github.com/clhenrick/dhcr-rent-stabilized-data that was obtained in a FOIL request.

Data are also merged with zip code level Zillow indices for analyses in the appendix. Additionally, a list of all apartments under the jurisdiction of the New York Department of Housing Preservation and Development is used in the appendix.

IA.B. Drivers of Apartment Financing Decision

In this section, I examine what drives the apartment financing decision. The key takeaway from this analysis is that building owners use more mortgage debt to finance buildings that they expect to have lower returns to maintenance. Therefore, building owners choose to preserve less financial capacity for buildings with lower returns to maintenance.

Figure 2 shows that buildings in zip codes with high capitalization rates tend to have higher LTV ratio mortgages, but these results raise the question of what explains variation in LTV ratios within zip codes. I argue that landlords anticipate investing less in lower quality buildings as those buildings have lower returns to investment.

To further examine the cross-sectional determinants of financing constraints, I run regressions of apartment LTV ratios at-issuance on hypothesized drivers of leverage:

$$LTVratio_{it} = \beta_1 X_{1,it} + \beta_2 X_{2,it} + \beta_3 X_{3,it} + \beta_4 X_{4,it} + FE + \epsilon_{it}, \tag{6}$$

where $LTVratio_{it}$ is the LTV ratio for the mortgage issued on building i in year t, $X_{1,it}$ are building characteristics, $X_{2,it}$ are local zip code level characteristics, $X_{3,it}$ are building-owner characteristics, and $X_{4,it}$ are loan characteristics. LTV ratios and control variables are measured at the time of mortgage-issuance. The vector $X_{1,it}$ includes building age, the number of units in a building, an indicator variable equal to 1 if a building is a mid or high-rise and the most recent transaction price for a building. In one specification I also include the time since the most recent renovation, although I exclude it in other specifications as it is not well-populated. $X_{2,it}$ includes the zip code level capitalization rate, the zip code level occupancy rate and the zip code level Zillow Home Values Index (ZHVI). $X_{3,it}$ includes an indicator variable equal to one if building i is owned by a public company and an indicator variable equal to one if building i is owned by an institutional investor. $X_{4,it}$ includes an indicator variable equal to one if the mortgage was made by a government lender, an indicator variable equal to one if a mortgage is fixed-rate, an indicator variable equal to one if a mortgage was a refinancing of a pre-existing mortgage, the mortgage time to maturity and the mortgage interest rate.

Table IA.26 displays cross-sectional regression results. Column (1) displays results using no fixed effects. Older buildings have higher LTV ratio mortgages, perhaps because the returns to investing in an older building are lower. Mortgages on larger buildings also tend to have higher LTV ratios. This could be because buildings with more units have a more diversified source of cash flows.

Examining the effect of local economic characteristics, buildings in higher capitalization rate zip codes tend to have bigger mortgages, which is consistent with the results in Figure 2b. Surprisingly though, buildings in zip codes with higher occupancy rates tend to have larger mortgages. This could be since those investments may be less risky since they have a more stable cash flow stream, reducing costs of borrowing and therefore allowing borrowers to take on more debt. Lastly, properties in zip codes with higher home values tend to have smaller mortgages, consistent with buildings owners using more debt to finance buildings that they anticipate having lower returns to investment. Owner characteristics are displayed below, where buildings owned by public companies tend to have lower LTV ratio mortgages. This could be since those investors have other sources of capital to choose from, and therefore need to rely less on mortgage financing.

Column (2) adds zip code and mortgage issue year fixed effects to the regression in order to control for time-varying macroeconomic conditions at the time the mortgage was issued and local time-invariant characteristics. For the most part, the results are very similar. The effects of both age and units on the mortgage LTV ratios are now statistically insignificant, indicating that these effects are largely driven by local zip code characteristics. The coefficient on the mid/high rise indicator is also now negative and statistically significant, perhaps because these buildings tend to be luxury apartments which may have higher returns to investment. In this specification, the government lender indicator is no longer statistically significant, providing evidence that government lenders may provide loans in zip codes that tend to have lower LTV ratio mortgages.

The results in columns (1) and (2) make it clear that time-varying zip code level characteristics are an important determinant of building financing, so column (3) includes zip-code-by-year fixed effects to control for these zip code level time trends. When using zip-code-by-year fixed effects, the estimates of the effects of transaction prices are now negative and statistically significant. This indicates that when looking within zip code, more expensive buildings tend to have lower LTV ratio mortgages. The coefficient on the number of units is also once again positive and statistically significant. Furthermore, the R^2 of the regression increases from 0.423 in column (2) to 0.619 in column (3), indicating that a significant portion of the variation in apartment mortgage LTV ratios are explained by zip code time trends. For this reason, including zip-code-by-year fixed effects significantly improves the reliability of the panel regressions. This indicates that by controlling for zip-code-by-year fixed effects it is possible to control for a significant amount of unobserved heterogeneity in LTV ratios. Lastly, in Column (4), the time since the most recent renovation is included to better proxy for building quality. Buildings that have been renovated less recently tend to have lower LTV ratios. This could be since building owners borrow to finance renovations.[2]

Columns (1) through (4) display results using all of the mortgages in RCA. Columns (5) through (8) only display results for the portion of the sample for which there is code violations data available (i.e. cities referenced in Table IA.1). For the most part, the results are qualitatively similar. The only exceptions are that the estimates on the number of units and the ownership indicators are statistically insignificant in all specifications. This is likely due to the reduced sample size when limiting the data to cities where information on code violations is available.

Overall, the findings in this section provide evidence that LTV ratios are not chosen randomly. In particular, zip code level characteristics are an important determinant of building LTV ratios, as are the building's size, age, owner, and quality. For this reason, an identification strategy is necessary to test how building financing constraints affect building maintenance.

[2]While there are some differences in the results in this column relative to others, this is largely due to the significant decrease in the sample size when including the time since renovation variable.

IA.C. Additional Results

Fig. IA.1. Geographic Distribution of Data

Map displaying the geographic composition of the data. The size of each point is proportional to the number of observations in that MSA. The shade of blue corresponds to the number of code violations per observations (i.e. cities with more code violations are darker shades of blue). Code violations data are from various municipal governments.

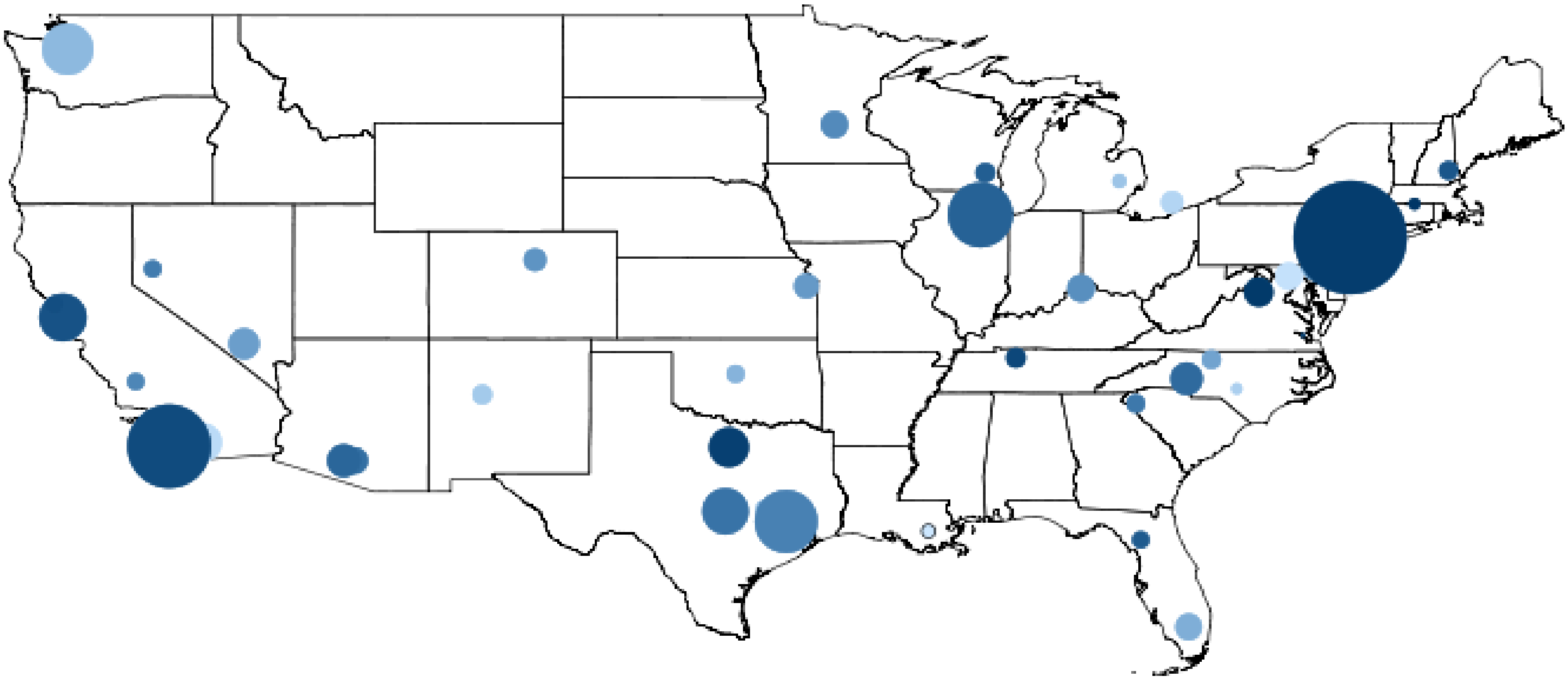


Fig. IA.2. Code Violations over Time

Number of code violations observed in the data per year. Code violations data are from various municipal governments.

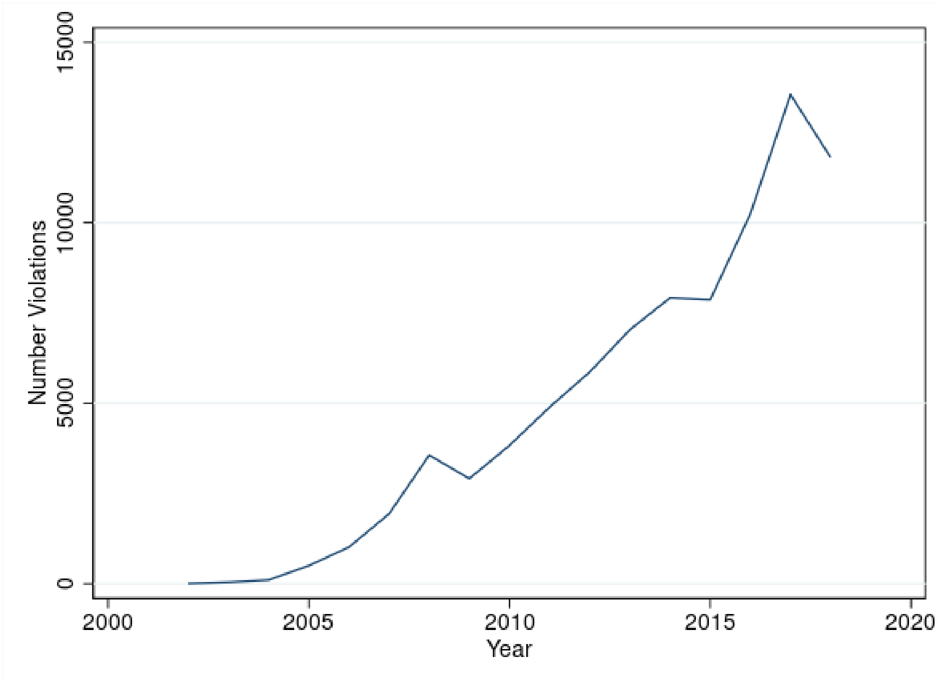
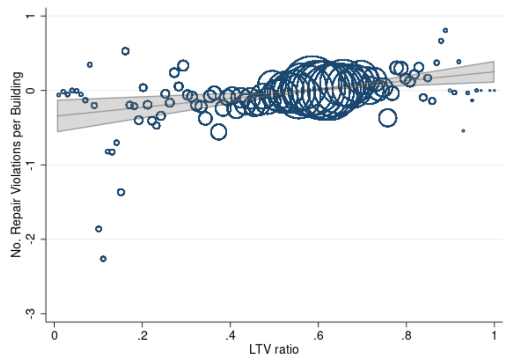


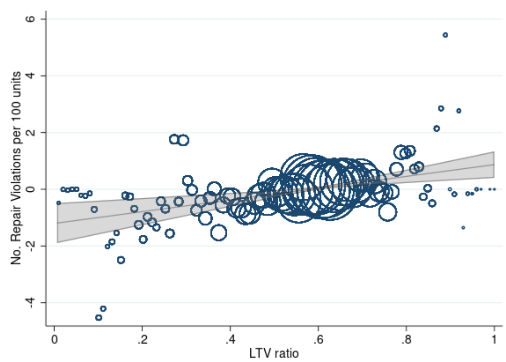
Fig. IA.3. Correlation Between LTV Ratios and Code Violation Requiring a Repair

Code violations requiring a repair (measured using the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t) graphed in 100 LTV ratio percentile bins, where each bin is the average number of code violations in a given percentile bin. Both LTV ratios and code violations are residualized at the zip-code-by-year level. The black lines are from regressions of each code violations outcome on LTV ratios, and the shaded region is the 95% confidence interval. Both scatterplots and lines weighted by number of observations in each bins. Property data are sourced from Real Capital Analytics and code violations data are provided by various municipal governments.

(a) Number of Repair Violations



(b) Repair Violations per 100 units



(c) Repair Violation Indicator

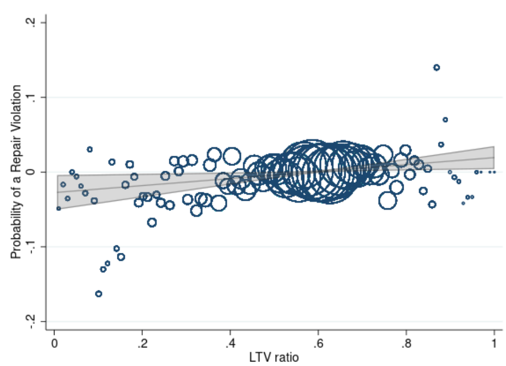
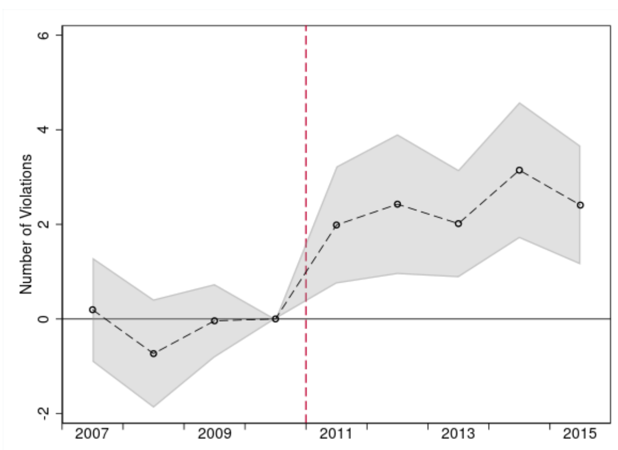


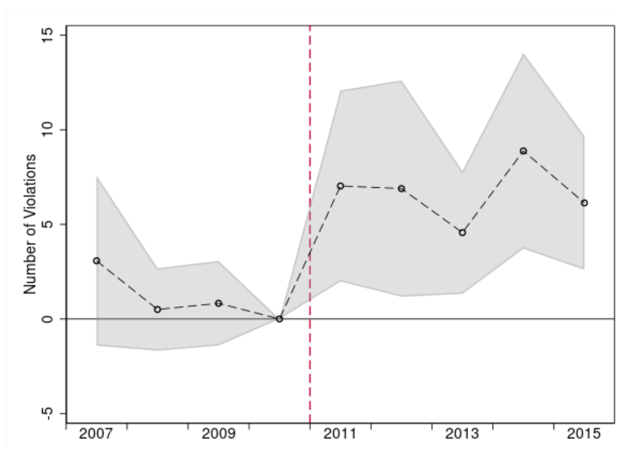
Fig. IA.4. Dynamic Difference-in-Differences Results – Code Violations Requiring Repairs

Regression coefficients from dynamic difference-in-differences regressions comparing trends in code violations requiring repairs for treated buildings (i.e., rent stabilized buildings with over 35 units) relative to control buildings (i.e., rent stabilized buildings with 35 or fewer units). Regressions are run at the annual frequency. Coefficients to the right of the red-dotted line are for 2011 or later. 2010 is excluded from the regression, so estimates can be interpreted as differences in the change in code violations from 2010 until year j for treated relative to control buildings. The shaded region is the 95% confidence interval. Sample constructed using one-to-one nearest neighbor matching to assign NYC rent stabilized buildings with more than 35 units to those with 35 or fewer units according to average building LTV ratios over the pre-period, most recent transaction prices as of 2010, building ages as of 2010, and indicator variable equal to 1 if a building was owned by an institutional investor in 2010, an indicator variable equal to 1 if the mortgage on a building in 2010 was a refinance, and zip code level occupancy rates as of 2010. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

(a) Number of Repair Violations



(b) Repair Violations per 100 units



(c) Repair Violation Indicator

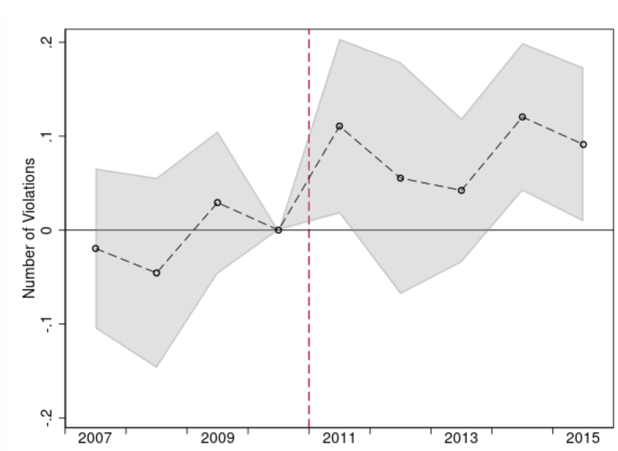
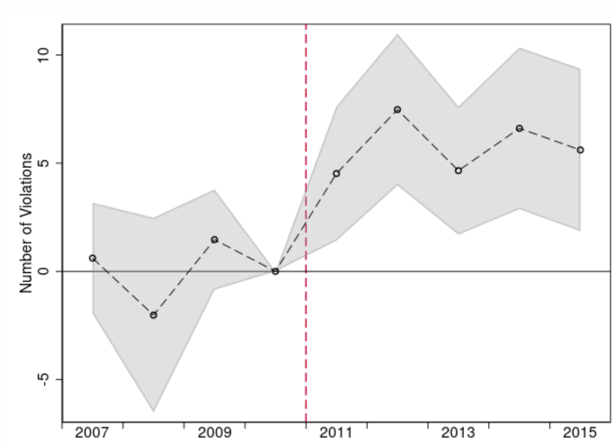


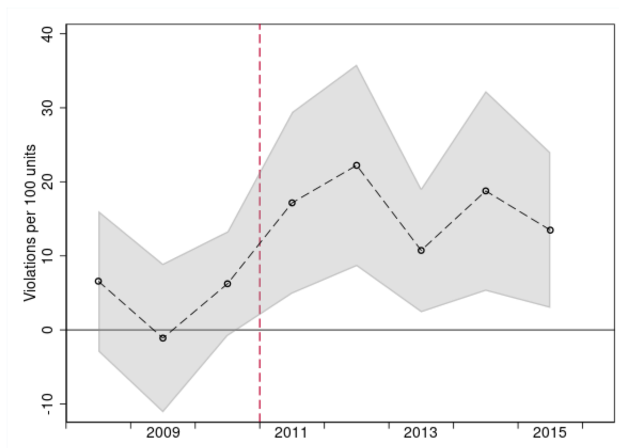
Fig. IA.5. Dynamic Difference-in-Differences Results – Top LTV Tercile

Regression coefficients from dynamic difference-in-differences regressions comparing trends in code violations for treated buildings (i.e., rent stabilized buildings with over 35 units) relative to control buildings (i.e., rent stabilized buildings with 35 or fewer units) in the top LTV tercile. Regressions are run at the annual frequency. Coefficients to the right of the red-dotted line are for 2011 or later. 2010 is excluded from the regression, so estimates can be interpreted as differences in the change in code violations from 2010 until year j for treated relative to control buildings. The shaded region is the 95% confidence interval. Sample constructed using one-to-one nearest neighbor matching to assign NYC rent stabilized buildings with more than 35 units to those with 35 or fewer units according to average building LTV ratios over the pre-period, most recent transaction prices as of 2010, building ages as of 2010, and indicator variable equal to 1 if a building was owned by an institutional investor in 2010, an indicator variable equal to 1 if the mortgage on a building in 2010 was a refinance, and zip code level occupancy rates as of 2010. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

(a) Number of Violations



(b) Violations per 100 units



(c) Violation Indicator

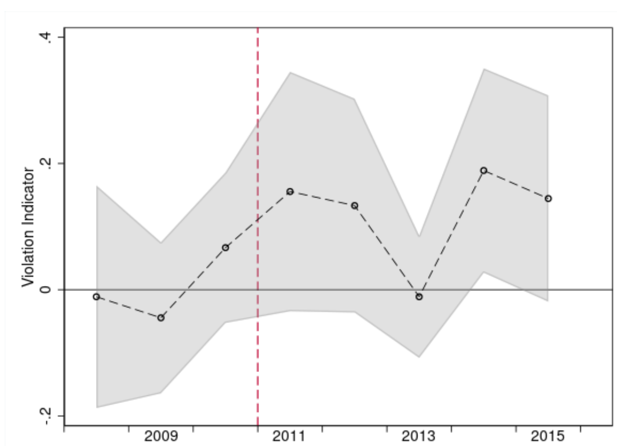


Table IA.1: Cities with Data on Code Violations.

The number of observations is the number of building-year observations observed in each city. The number of observations with a violation is the number of building-year observations with a violation occurring. The number of buildings is number of buildings observed in data. The number of buildings with a violation is number of buildings for which a violation is observed at some point in data. Other cities are those with under 1,000 observations, which includes Baltimore MD, Minneapolis MN, Cincinnati OH, Dallas TX, Tacoma WA, Kansas City MO, Anaheim CA, Greensboro NC, Fort Lauderdale FL, Oklahoma City OK, Cleveland OH, Albuquerque NM, Aurora CO, Milwaukee WI, Nashville TN, Tempe AZ, Greenville SC, Mesa AZ, College Station TX, Gainesville FL, Reno NV, Boston MA, Bakersfield CA, Fayetteville NC, Burbank CA, Santa Rosa CA, El Cajon CA, Hartford CT, New Orleans LA, Detroit MI and Virginia Beach VA. Code violations data are from various municipal governments.

City	No. Obs	No. Obs with Viol	No. Bldgs	No Bldgs w. Viol	Earliest Year	Latest Year
New York	11,522	2,315	2,240	797	2002	2018
Los Angeles	7,883	946	1,343	460	2003	2018
Houston	6,318	323	941	172	2003	2018
Chicago	4,933	655	1,132	328	2006	2018
Austin	2,971	422	487	164	2003	2018
Seattle	2,604	84	472	61	2004	2018
San Francisco	2,215	292	348	133	2003	2018
Philadelphia	2,209	180	317	80	2007	2018
San Diego	1,867	33	326	27	2004	2018
Washington	1,289	253	229	124	2007	2018
Charlotte	1,229	91	220	67	2007	2018
Tucson	1,209	359	205	109	2008	2018
Fort Worth	1,197	738	200	154	2006	2018
Las Vegas	1,107	51	283	33	2012	2018
Other	14,075	2,001	2,893	863	N/A	N/A
Total	62,628	8,743	11,636	3,572	N/A	N/A

Table IA.2: Relationship Between DSCR and Code Violations.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 DSCR_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}.$$

$Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $DSCR_{it-1}$ is the DSCR for building i in year $t-1$. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v and are zip-code-by-year and mortgage issue year fixed effects. LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. Number of violations, number of violations per 100 units, DSCR, transaction prices, building age, number of units per building and interest rates are winsorized at the 1% and 99% levels. Standard errors, clustered at the city level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
DSCR	-0.101***	-0.195**	-0.006**
	(0.024)	(0.078)	(0.003)
Adjusted R^2	0.150	0.143	0.206
Observations	60,399	60,399	60,399
Panel B – Repair Violations			
DSCR	-0.066***	-0.162***	-0.004**
	(0.012)	(0.031)	(0.002)
Adjusted R^2	0.153	0.129	0.143
Observations	53,760	53,760	53,760
FE	Zip-Year	Zip-Year	Zip-Year
	Issue Year	Issue Year	Issue Year
Building Controls	X	X	X
Loan Controls	X	X	X
Building Owner Controls	X	X	X
Lender Controls	X	X	X
S.E. Cluster	City	City	City

Table IA.3: Relationship Between Combined LTV Ratios and Code Violations.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}.$$

$Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. Panel A uses all code violations and Panel B uses only code violations requiring repairs. $LTVratio_{it-1}$ is the combined LTV ratio (calculated using both first and second mortgages) for building i in year $t-1$. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v are zip-code-by-year and mortgage issue year fixed effects. Combined LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. Regression controls the same as in Table 2. Number of violations, number of violations per 100 units, LTV ratios, transaction prices, building age, number of units per building, and interest rates are winsorized at the 1% and 99% levels. Standard errors, clustered at the city level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
LTV Ratio	0.098***	0.251**	0.006***
	(0.019)	(0.103)	(0.002)
Adjusted R^2	0.153	0.145	0.208
Observations	62,628	62,628	62,628
Panel B – Repair Violations			
LTV Ratio	0.054***	0.165**	0.003**
	(0.017)	(0.067)	(0.001)
Adjusted R^2	0.158	0.133	0.146
Observations	55,856	55,856	55,856
FE	Zip-Year	Zip-Year	Zip-Year
	Issue Year	Issue Year	Issue Year
Building Controls	X	X	X
Loan Controls	X	X	X
Building Owner Controls	X	X	X
Lender Controls	X	X	X
S.E. Cluster	City	City	City

Table IA.4: Relationship Between Amortized LTV Ratios and Code Violations.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}.$$

$Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. Panel A uses all code violations and Panel B uses only code violations requiring repairs. $LTVratio_{it-1}$ is the amortized LTV ratio for building i in year $t-1$, where the LTV ratio of the building accounting for amortization is calculated using information provided in the RCA Data. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v are zip-code-by-year and mortgage issue year fixed effects. Amortized LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. Regression controls the same as in Table 2. Number of violations, number of violations per 100 units, amortized LTV ratios, interest rates, number of units per building and building ages are winsorized at the 1% and 99% levels. Standard errors, clustered at the city level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
LTV Ratio	0.055***	0.164***	0.003
	(0.014)	(0.049)	(0.003)
Adjusted R^2	0.152	0.144	0.208
Observations	62,628	62,628	62,628
Panel B – Repair Violations			
LTV Ratio	0.025***	0.095***	0.000
	(0.007)	(0.032)	(0.002)
Adjusted R^2	0.158	0.132	0.145
Observations	55,856	55,856	55,856
FE	Zip-Year	Zip-Year	Zip-Year
	Issue Year	Issue Year	Issue Year
Building Controls	X	X	X
Loan Controls	X	X	X
Building Owner Controls	X	X	X
Lender Controls	X	X	X
S.E. Cluster	City	City	City

Table IA.5: Relationship between LTV Ratios at Issue and Code Violations – Probability Weight by City.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}.$$

$Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. Panel A uses all code violations and Panel B uses only code violations requiring repairs. $LTVratio_{it-1}$ is the LTV ratio at issue for building i in year $t-1$. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v are zip-code-by-year and mortgage issue year fixed effects. LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. Observations are probability weighted by the inverse of the number of observations in each city in running the regressions. Regression controls the same as in Table 2. Number of violations, number of violations per 100 units, LTV ratios, transaction prices, building age, number of units per building and interest rates are winsorized at the 1% and 99% levels. Standard errors, clustered at the city level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
LTV Ratio	0.073***	0.217***	0.009***
	(0.026)	(0.079)	(0.003)
Adjusted R^2	0.213	0.245	0.386
Observations	62,628	62,628	62,628
Panel B – Repair Violations			
LTV Ratio	0.031**	0.100*	0.003***
	(0.014)	(0.057)	(0.001)
Adjusted R^2	0.151	0.122	0.136
Observations	55,856	55,856	55,856
FE	Zip-Year	Zip-Year	Zip-Year
	Issue Year	Issue Year	Issue Year
Building Controls	X	X	X
Loan Controls	X	X	X
Building Owner Controls	X	X	X
Lender Controls	X	X	X
S.E. Cluster	City	City	City

Table IA.6: Relationship between LTV Ratios at Issue and Code Violations – Drop Four Largest Cities.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}.$$

$Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. Panel A uses all code violations and Panel B uses only code violations requiring repairs. $LTVratio_{it-1}$ is the LTV ratio at issue for building i in year $t-1$. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v are zip-code-by-year and mortgage issue year fixed effects. LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. All buildings located in either New York City, Los Angeles, Houston, or Chicago are dropped from the sample. Regression controls are the same as in Table 2. Number of violations, number of violations per 100 units, LTV ratios, transaction prices, building age, number of units per building, and interest rates are winsorized at the 1% and 99% levels. Standard errors, clustered at the city level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
LTV Ratio	0.096***	0.222**	0.010***
	(0.030)	(0.104)	(0.002)
Adjusted R^2	0.124	0.139	0.259
Observations	31,971	31,971	31,971
Panel B – Repair Violations			
LTV Ratio	0.039*	0.121	0.004***
	(0.020)	(0.086)	(0.001)
Adjusted R^2	0.096	0.045	0.081
Observations	25,200	25,200	25,200
FE	Zip-Year Issue Year	Zip-Year Issue Year	Zip-Year Issue Year
Building Controls	X	X	X
Loan Controls	X	X	X
Building Owner Controls	X	X	X
Lender Controls	X	X	X
S.E. Cluster	City	City	City

Table IA.7: Relationship Between LTV Ratios at Issue and Code Violations – Exclude Sample After 2012.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}.$$

$Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. Panel A uses all code violations and Panel B uses only code violations requiring repairs. $LTVratio_{it-1}$ is the LTV ratio for building i in year $t-1$. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v are zip-code-by-year and mortgage issue year fixed effects. LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. Regression controls the same as in Table 2. Number of violations, number of violations per 100 units, LTV ratios, interest rates, number of units per building and building ages are winsorized at the 1% and 99% levels. Standard errors, double-clustered at the city and year levels, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations (1)	Violations/ 100 units (2)	Has Violation (3)
Panel A – All Violations			
LTV Ratio	0.060*	0.165**	0.004
	(0.030)	(0.065)	(0.003)
Adjusted R^2	0.117	0.099	0.220
Observations	21,830	21,830	21,830
Panel B – Repair Violations			
LTV Ratio	0.030***	0.128***	0.004
	(0.010)	(0.040)	(0.003)
Adjusted R^2	0.118	0.090	0.128
Observations	19,673	19,673	19,673
FE	Zip-Year Issue Year	Zip-Year Issue Year	Zip-Year Issue Year
Building Controls	X	X	X
Loan Controls	X	X	X
Building Owner Controls	X	X	X
Lender Controls	X	X	X
S.E. Cluster	City Year	City Year	City Year

Table IA.8: Relationship Between LTV Ratios at Issue and Code Violations – Include Sample from 2013 and Later.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}.$$

$Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. Panel A uses all code violations and Panel B uses only code violations requiring repairs. $LTVratio_{it-1}$ is the LTV ratio for building i in year $t-1$. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v are zip-code-by-year and mortgage issue year fixed effects. LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. Regression controls the same as in Table 2. Number of violations, number of violations per 100 units, LTV ratios, interest rates, number of units per building and building ages are winsorized at the 1% and 99% levels. Standard errors, double-clustered at the city and year levels, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
LTV Ratio	0.126***	0.389**	0.007**
	(0.042)	(0.190)	(0.003)
Adjusted R^2	0.171	0.166	0.204
Observations	40,797	40,797	40,797
Panel B – Repair Violations			
LTV Ratio	0.072**	0.235*	0.002*
	(0.032)	(0.118)	(0.001)
Adjusted R^2	0.178	0.153	0.156
Observations	36,182	36,182	36,182
FE	Zip-Year Issue Year	Zip-Year Issue Year	Zip-Year Issue Year
Building Controls	X	X	X
Loan Controls	X	X	X
Building Owner Controls	X	X	X
Lender Controls	X	X	X
S.E. Cluster	City Year	City Year	City Year

Table IA.9: Relationship Between LTV Ratios at Issue and Code Violations – Double-Cluster by City and Year.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}.$$

$Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. Panel A uses all code violations and Panel B uses only code violations requiring repairs. $LTVratio_{it-1}$ is the LTV ratio at issue for building i in year $t-1$. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v are zip-code-by-year and mortgage issue year fixed effects. LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. Regression controls the same as in Table 2. Number of violations, number of violations per 100 units, LTV ratios, interest rates, number of units per building and building ages are winsorized at the 1% and 99% levels. Standard errors, double-clustered at the city and year levels, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations (1)	Violations/ 100 units (2)	Has Violation (3)
Panel A – All Violations			
LTV Ratio	0.100***	0.294***	0.006**
	(0.013)	(0.087)	(0.002)
Adjusted R^2	0.153	0.145	0.208
Observations	62,628	62,628	62,628
Panel B – Repair Violations			
LTV Ratio	0.057***	0.193***	0.003*
	(0.015)	(0.061)	(0.002)
Adjusted R^2	0.158	0.133	0.146
Observations	55,586	55,586	55,586
FE	Zip-Year Issue Year	Zip-Year Issue Year	Zip-Year Issue Year
Building Controls	X	X	X
Loan Controls	X	X	X
Building Owner Controls	X	X	X
Lender Controls	X	X	X
S.E. Cluster	City Year	City Year	City Year

Table IA.10: Poisson Regression of Code Violations on LTV Ratios at Issue.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}.$$

$Violations_{it}$ is either the number of violations for building i in year t or the number of repair violations for building i in year t. $LTVratio_{it-1}$ is the LTV ratio at issue for building i in year $t-1$. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v are zip-code-by-year and mortgage issue year fixed effects. LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. Regression controls the same as in Table 2. Number of violations, number of violations per 100 units, LTV ratios, interest rates, number of units per building and building ages are winsorized at the 1% and 99% levels. Standard errors, clustered at the city level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations	# Repair Violations
	(1)	(2)
LTV Ratio	0.098***	0.103***
	(0.017)	(0.018)
Psuedo R^2	0.344	0.332
Observations	35,278	22,299
FE	Zip-Year	Zip-Year
	Issue Year	Issue Year
Building Controls	X	X
Loan Controls	X	X
Building Owner Controls	X	X
Lender Controls	X	X
S.E. Cluster	City	City

Table IA.11: Relationship between LTV Ratios at Issue and Code Violations – Define Age using Effective Age.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 LTVratio_{it-1} + X_{it-1}\Gamma + \gamma_{zt} + \kappa_v + \epsilon_{it}.$$

$Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. Panel A uses all code violations and Panel B uses only code violations requiring repairs. $LTVratio_{it-1}$ is the LTV ratio at issue for building i in year $t-1$. X_{it-1} are control variables for building i as of year $t-1$. γ_{zt}, κ_v are zip-code-by-year and mortgage issue year fixed effects. LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. The regression controls are the same as in Table 2, except effective age (defined as the time since the most recent building renovation if available and the building's age otherwise) is used instead of the building's age. Number of violations, number of violations per 100 units, LTV ratios, transaction prices, building age, number of units per building and interest rates are winsorized at the 1% and 99% levels. Standard errors, clustered at the city level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

Variable	# Violations (1)	Violations/ 100 units (2)	Has Violation (3)
Panel A – All Violations			
LTV Ratio	0.101***	0.297***	0.006**
	(0.015)	(0.090)	(0.003)
Adjusted R^2	0.152	0.144	0.208
Observations	62,628	62,628	62,628
Panel B – Repair Violations			
LTV Ratio	0.058***	0.197***	0.003**
	(0.013)	(0.060)	(0.002)
Adjusted R^2	0.158	0.132	0.146
Observations	55,856	55,856	55,856
FE	Zip-Year Issue Year	Zip-Year Issue Year	Zip-Year Issue Year
Building Controls	X	X	X
Loan Controls	X	X	X
Building Owner Controls	X	X	X
Lender Controls	X	X	X
S.E. Cluster	City	City	City

Table IA.12: NYC Summary Statistics – Before Matching.

Summary statistics comparing treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units). Number of violations, number of violations per 100 units, number of repair violations, number of repair violations per 100 units, LTV ratio, building age, and unemployment rate winsorized at the 1% and 99% levels. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are provided by the New York City government.

Variable	Treated		Control		Difference
	Mean	St. Dev	Mean	St. Dev	
Number of Violations	3.136	7.610	0.997	3.560	2.139***
Violations per 100 Units	5.322	14.005	4.640	16.827	0.682
Violation Indicator	0.293	0.455	0.129	0.336	0.164***
LTV Ratio	0.552	0.221	0.598	0.218	-0.046***
Transaction Price (MM)	11.0	17.1	4.500	5.200	6.500***
Building Age	79.633	17.121	91.797	18.399	-12.164***
Mid/High Rise Indicator	0.986	0.118	0.980	0.140	0.006
Public Owner	0.002	0.045	0.000	0.000	0.002
Institutional Owner	0.100	0.301	0.063	0.243	0.037**
Property Capitalization Rate at Origination	0.056	0.016	0.058	0.013	-0.002
Occupancy %	0.883	0.284	0.936	0.179	-0.053

Table IA.13: Summary Statistics – Matched Sample.

Summary statistics comparing treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units). Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age, an indicator variable equal to 1 if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. Number of violations, number of violations per 100 units, number of repair violations, number of repair violations per 100 units, LTV ratio, building age, and unemployment rate winsorized at the 1% and 99% levels. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, and code violations data are provided by the New York City government.

Variable	Treated		Control		Difference
	Mean	St. Dev	Mean	St. Dev	
Number of Violations	3.573	7.904	2.459	5.802	1.114**
Violations per 100 Units	6.669	16.054	9.405	22.534	-2.736*
Violation Indicator	0.342	0.475	0.202	0.402	0.140***
LTV Ratio	0.558	0.203	0.557	0.199	0.001
Transaction Price	5.506	4.619	4.563	4.384	0.942***
Building Age	84.564	8.622	85.476	8.405	-0.912
Mid/High Rise Indicator	0.997	0.057	0.997	0.057	0.000
Public Owner	0.000	0.000	0.000	0.000	0.000
Institutional Owner	0.052	0.223	0.052	0.223	0.000
Property Capitalization Rate at Origination	0.061	0.016	0.059	0.013	0.002
Occupancy %	0.853	0.329	0.903	0.219	-0.050
$N_{control} = N_{treated} = 307$					

Table IA.14: Change in Appraised Values Following the Rent Act of 2011.

This table displays results from the following regression:

$$ApprValperUnit_{it} = \beta_1 Treat_i + \beta_2 After_t + \beta_3 Treat_i \times After_t + FE + \epsilon_{it},$$

where $ApprValperUnit_{it}$ is the appraised value of building i in year t divided by the number of units in building i, $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units and $After_t$ is an indicator variable equal to 1 if year t is 2012. Appraised values per unit winsorized at the 1% and 99% levels. The sample includes all appraisals for all rent stabilized buildings in New York City from 2010 and 2012. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively.

	(1)	(2)	(3)	(4)
$Treat_i \times After_t$	-2772.529***	-2837.047***	-2919.856***	-991.047**
	(365.696)	(368.024)	(361.011)	(435.299)
$Treat_i$	-49488.358***	-26232.860***		
	(830.084)	(906.135)		
$After_t$	8264.906***			
	(290.541)			
FE		Year	Year	
			Building	Building
				Zip-Year
S.E. Cluster	Building	Building	Building	Building
Adjusted R^2	0.092	0.439	0.947	0.952
Observations	39,344	39,344	39,304	39,294

Table IA.15: Change in Code Violations After the Rent Act of 2011 by DSCR.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age, an indicator variable equal to 1 if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. All covariates are taken as of 2010. Panel A displays results examining buildings in the bottom-tercile of DSCR, Panel B displays results examining buildings in the middle-tercile of DSCR and Panel C displays results examining buildings in the top-tercile of LTV ratios. DSCR terciles are assigned based on the DSCR of buildings prior to 2011. Number of violations, number of repair violations, number of violations per 100 units and number of repair violations per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Violation type	All Violations			Repair Violations		
Variable	# Violations	Violations/ 100 units	Has Violation	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Bottom DSCR Tercile, N=180						
$Treat_i \times After_t$	9.060***	21.662**	0.205*	6.200**	14.197**	0.150
	(2.854)	(7.811)	(0.113)	(2.087)	(5.846)	(0.111)
Adjusted R^2	0.533	0.453	0.366	0.503	0.402	0.512
Panel B – Mid DSCR Tercile, N=54						
$Treat_i \times After_t$	-0.433	-1.130	g -0.083	-0.550	-1.477	-0.050
	(0.272)	(0.685)	(0.126)	(0.473)	(1.202)	(0.106)
Adjusted R^2	0.271	0.276	0.250	0.111	0.113	0.268
Panel C – Top DSCR Tercile, N=90						
$Treat_i \times After_t$	4.740	15.165	-0.010	3.130	8.874	-0.010
	(4.634)	(14.825)	(0.010)	(3.060)	(8.675)	(0.010)
Adjusted R^2	0.436	0.379	0.618	0.425	0.399	0.618
FE	Building	Building	Building	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year	Pair-Year	Pair-Year	Pair-Year
S.E Cluster	Building	Building	Building	Building	Building	Building

Table IA.16: Change in Code Violations After the Rent Act of 2011 by Combined LTV Ratio.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age, an indicator variable equal to 1 if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. All covariates are taken as of 2010. Panel A displays results examining buildings in the bottom-tercile of combined LTV ratios, Panel B displays results examining buildings in the middle-tercile of combined LTV ratios and Panel C displays results examining buildings in the top-tercile of combined LTV ratios. Combined LTV ratio terciles are assigned based on the combined LTV ratio of buildings prior to 2011. Number of violations, number of repair violations, number of violations per 100 units and number of repair violations per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Violation type	All Violations			Repair Violations		
Variable	# Violations	Violations/ 100 units	Has Violation	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Bottom CLTV Tercile, N=1,620						
$Treat_i \times After_t$	1.208	-0.434	0.066	1.045	0.883	0.045
	(1.370)	(4.160)	(0.043)	(0.794)	(2.347)	(0.053)
Adjusted R^2	0.478	0.451	0.565	0.453	0.433	0.528
Panel B – Mid CLTV Tercile, N=1,314						
$Treat_i \times After_t$	3.740***	7.732***	0.031	2.442***	5.530***	0.095***
	(0.940)	(2.335)	(0.041)	(0.597)	(1.527)	(0.035)
Adjusted R^2	0.465	0.417	0.649	0.431	0.410	0.627
Panel C – Top CLTV Tercile, N=1,458						
$Treat_i \times After_t$	4.504***	10.361***	0.099**	2.986***	6.822***	0.096**
	(1.360)	(3.739)	(0.042)	(0.953)	(2.570)	(0.044)
Adjusted R^2	0.501	0.460	0.638	0.480	0.443	0.600
FE	Building	Building	Building	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year	Pair-Year	Pair-Year	Pair-Year
S.E Cluster	Building	Building	Building	Building	Building	Building

Table IA.17: Change in Code Violations After the Rent Act of 2011 by Amortized LTV Ratio.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building age, an indicator variable equal to 1 if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. All covariates are taken as of 2010. Panel A displays results examining buildings in the bottom-tercile of amortized LTV ratios, Panel B displays results examining buildings in the middle-tercile of amortized LTV ratios and Panel C displays results examining buildings in the top-tercile of amortized LTV ratios. Amortized LTV ratio terciles are assigned based on the amortized LTV ratio of buildings prior to 2011. Number of violations, number of repair violations, number of violations per 100 units and number of repair violations per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Violation type	All Violations			Repair Violations		
Variable	# Violations	Violations/ 100 units	Has Violation	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Bottom Amortized LTV Tercile, N=1,566						
$Treat_i \times After_t$	2.126	1.974	0.066	1.605**	2.398	0.071
	(1.314)	(3.869)	(0.042)	(0.782)	(2.220)	(0.047)
Adjusted R^2	0.461	0.421	0.556	0.448	0.419	0.521
Panel B – Mid Amortized LTV Tercile, N=1,494						
$Treat_i \times After_t$	5.173***	11.060***	0.074*	3.233***	7.355***	0.128***
	(1.097)	(2.758)	(0.042)	(0.690)	(1.754)	(0.038)
Adjusted R^2	0.459	0.379	0.627	0.433	0.371	0.593
Panel C – Top Amortized LTV Tercile, N=1,548						
$Treat_i \times After_t$	5.448***	12.983***	0.090	3.587***	8.753***	0.094
	(1.443)	(4.214)	(0.060)	(1.051)	(3.062)	(0.058)
Adjusted R^2	0.536	0.511	0.667	0.518	0.495	0.619
FE	Building Pair-Year	Building Pair-Year	Building Pair-Year	Building Pair-Year	Building Pair-Year	Building Pair-Year
S.E Cluster	Building	Building	Building	Building	Building	Building

Table IA.18: Impact of Rent Act on Code Violations – Match on Effective Age.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, building effective age, an indicator variable equal to 1 if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. Panel A displays results using all code violations and Panel B displays results using only code violations requiring repairs. Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, code violations and property deeds data are provided by the New York City government.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
$Treat_i \times After_t$	2.893***	4.668**	0.031
	(0.732)	(2.267)	(0.026)
Adjusted R^2	0.502	0.462	0.646
Observations	6,066	6,066	6,066
Panel B – Repair Violations			
$Treat_i \times After_t$	2.070***	3.977***	0.056**
	(0.430)	(1.313)	(0.028)
Adjusted R^2	0.488	0.465	0.599
Observations	6,066	6,066	6,066
FE	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year
S.E Cluster	Building	Building	Building

Table IA.19: Impact of Rent Act on Code Violations – Poisson Regression.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t or the number of repair violations for building i in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, an indicator variable equal to 1 if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. Panel A displays results using all code violations and Panel B displays results using only code violations requiring repairs. Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics, code violations and property deeds data are provided by the New York City government.

Variable	# Violations	# Repair Violations
	(1)	(2)
$Treat_i \times After_t$	0.932***	1.068***
	(0.217)	(0.269)
FE	Building	Building
	Pair-Year	Pair-Year
S.E Cluster	Building	Building
Psuedo R^2	0.675	0.603
Observations	1,114	922

Table IA.20: Change in Code Violations After Rent Act of 2011 – All Buildings Registered with HPD.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_t + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_t are building and year fixed effects. Panel A displays results using all code violations and Panel B displays results using only code violations requiring repairs. Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Code violations data are provided by the New York City government and New York apartments data are provided by the New York Department of Housing Preservation and Development.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
$Treat_i \times After_t$	4.623***	0.812	0.068***
	(0.146)	(0.521)	(0.005)
Adjusted R^2	0.409	0.305	0.386
Observations	189,603	189,603	189,603
Panel B – Repair Violations			
$Treat_i \times After_t$	2.864***	1.773***	0.116***
	(0.091)	(0.323)	(0.005)
Adjusted R^2	0.397	0.288	0.369
Observations	189,603	189,603	189,603
FE	Building	Building	Building
	Year	Year	Year
S.E Cluster	Building	Building	Building

Table IA.21: Change in Code Violations After Rent Act of 2011 – Alternate Time Windows.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to LTV, most recent transaction price, an indicator variable equal to 1 if a building was owned by an institutional investor, and zip code level occupancy rates as covariates. Panel A displays results using a time window of 2007-2014, Panel B displays results using a time window of 2006-2015, Panel C displays results using a time window of 2006-2016, Panel D displays results using a time window of 2007-2016 and Panel E displays results using a time window of 2009-2012. Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Violation Type	All Violations			Repair Violations		
Variable	# Violations	Violations/ 100 units	Has Violation	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2007-2014, N=4,848						
$Treat_i \times After_t$	3.649***	7.720***	0.082***	2.548***	5.796***	0.100***
	(0.790)	(2.442)	(0.029)	(0.529)	(1.631)	(0.028)
Adjusted R^2	0.448	0.417	0.606	0.432	0.413	0.553
Panel B: 2006-2015, N=4,300	2.805***	4.194	0.069*	1.956***	3.305*	0.088**
	(0.967)	(3.209)	(0.041)	(0.577)	(1.853)	(0.042)
Adjusted R^2	0.497	0.499	0.612	0.475	0.482	0.572
Panel C: 2006-2016, N=4,928						
$Treat_i \times After_t$	2.946***	4.408	0.067**	2.053***	3.511**	0.088**
	(0.890)	(2.904)	(0.033)	(0.536)	(1.706)	(0.039)
Adjusted R^2	0.496	0.496	0.643	0.478	0.483	0.607
Panel D: 2007-2016, N=6,720						
$Treat_i \times After_t$	3.250***	5.662***	0.041	2.202***	4.318***	0.076***
	(0.714)	(2.156)	(0.027)	(0.453)	(1.375)	(0.028)
Adjusted R^2	0.509	0.492	0.648	0.493	0.488	0.629
Panel E: 2009-2012, N=3,376						
$Treat_i \times After_t$	2.835***	8.182***	0.045	2.114***	6.600***	0.072*
	(0.731)	(2.870)	(0.043)	(0.543)	(2.333)	(0.043)
Adjusted R^2	0.388	0.309	0.584	0.360	0.295	0.519
FE	Building	Building	Building	Building	Building	Building
Pair-Year	Pair-Year	Pair-Year	Pair-Year	Pair-Year	Pair-Year	
S.E. Cluster	Building	Building	Building	Building	Building	Building

Table IA.22: Change in Code Violations After Rent Act of 2011 – No Matching.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + Controls + \gamma_i + \kappa_t + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i are building fixed effects. κ_t are year fixed effects. Controls are all taken as of 2010 Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Violation Type	All Violations			Repair Violations		
Variable	# Violations	Violations/ 100 units	Has Violation	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – No Controls						
$Treat_i \times After_t$	3.075***	4.914***	0.052***	1.917***	3.193***	0.077***
	(0.458)	(1.247)	(0.016)	(0.285)	(0.768)	(0.016)
Adjusted R^2	0.499	0.431	0.654	0.482	0.423	0.618
Obs	7,209	7,209	7,209	7,209	7,209	7,209
Panel B – With Controls						
$Treat_i \times After_t$	2.959***	5.044***	0.034*	1.862***	3.394***	0.057***
	(0.525)	(1.450)	(0.018)	(0.328)	(0.889)	(0.019)
$Price_i \times After_t$	-0.067***	-0.179***	-0.002***	-0.041***	-0.109***	-0.002***
	(0.009)	(0.023)	(0.000)	(0.006)	(0.015)	(0.000)
$InstOwner_i \times After_t$	1.092	3.144	0.055	0.840	2.215	0.049
	(1.019)	(2.609)	(0.034)	(0.669)	(1.682)	(0.036)
$Age_i \times After_t$	-0.029**	-0.056*	-0.002***	-0.018**	-0.027	-0.002***
	(0.012)	(0.030)	(0.001)	(0.008)	(0.017)	(0.001)
$ZipOccupancy_i \times After_t$	-2.225	-4.196	-0.117**	-1.622*	-3.249	-0.129**
	(1.501)	(3.815)	(0.051)	(0.958)	(2.353)	(0.054)
Adjusted R^2	0.510	0.445	0.670	0.494	0.438	0.630
Obs	6,291	6,291	6,291	6,291	6,291	6,291
FE	Building	Building	Building	Building	Building	Building
	Year	Year	Year	Year	Year	Year
SE	Building	Building	Building	Building	Building	Building

Table IA.23: Change in Code Violations After Rent Act of 2011 – Cluster at Zip Code Level.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to average building LTV ratios over the pre-period, most recent transaction prices as of 2010, building ages as of 2010, an indicator variable equal to 1 if a building was owned by an institutional investor in 2010, an indicator variable equal to 1 if the mortgage on a building in 2010 was a refinance and zip code level occupancy rates as of 2010 as covariates. Panel A displays results using all code violations and Panel B displays results using only code violations requiring repairs. Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Standard errors are clustered at the zip code level. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
$Treat_i \times After_t$	3.764***	7.708***	0.074***
	(0.756)	(2.212)	(0.028)
Adjusted R^2	0.480	0.446	0.620
Obs	5,526	5,526	5,526
Panel B – Repair Violations			
$Treat_i \times After_t$	2.541***	5.600***	0.093***
	(0.507)	(1.473)	(0.028)
Adjusted R^2	0.464	0.440	0.580
Obs	5,526	5,526	5,526
FE	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year
S.E Cluster	Building	Building	Building

Table IA.24: Change in Code Violations After Rent Act of 2011 – Double-Cluster at Building and Year Levels.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, and $After_t$ is an indicator variable equal to 1 if year t is 2011 or later. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to average building LTV ratios over the pre-period, most recent transaction prices as of 2010, building ages as of 2010, an indicator variable equal to 1 if a building was owned by an institutional investor in 2010, an indicator variable equal to 1 if the mortgage on a building in 2010 was a refinance and zip code level occupancy rates as of 2010 as covariates. Panel A displays results using all code violations and Panel B displays results using only code violations requiring repairs. Number of violations, number of violations requiring repairs, number of violations per 100 units and number of violations requiring repairs per 100 units are winsorized at the 1% and 99% levels. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Standard errors are double-clustered at the building and year levels. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
$Treat_i \times After_t$	3.764***	7.708***	0.074*
	(0.635)	(1.779)	(0.035)
Adjusted R^2	0.480	0.446	0.620
Obs	5,526	5,526	5,526
Panel B – Repair Violations			
$Treat_i \times After_t$	2.541***	5.600***	0.093***
	(0.391)	(1.209)	(0.024)
Adjusted R^2	0.464	0.440	0.580
Obs	5,526	5,526	5,526
FE	Building Pair-Year	Building Pair-Year	Building Pair-Year
S.E Cluster	Building Year	Building Year	Building Year

Table IA.25: Triple-Difference – Impact of Rent Act on Violations for Top LTV Tercile Buildings Relative to Bottom LTV Tercile Buildings.

This table displays results from the following regression:

$$Violations_{it} = \beta_1 Treat_i \times After_t + \beta_2 TopLTV_i \times After_t + \beta_3 Treat_i \times TopLTV_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},$$

where $Violations_{it}$ is either the number of violations for building i in year t, the number of violations per 100 units for building i in year t or an indicator variable equal to 1 if building i incurs a code violation in year t. $Treat_i$ is an indicator variable equal to 1 if building i has more than 35 units, $After_t$ is an indicator variable equal to 1 if year t is 2011 or later, and $TopLTV_i$ is an indicator variable equal to 1 if building i is in the top tercile of LTV ratios and 0 if it is in the bottom tercile. γ_i, κ_{pt} are building and matched-pair-by-year fixed effects. Sample constructed using one-to-one nearest neighbor matching of treated buildings (i.e., rent stabilized buildings with over 35 units) to control buildings (i.e., rent stabilized buildings with 35 or fewer units) according to average building LTV ratios over the pre-period, most recent transaction prices as of 2010, building ages as of 2010, an indicator variable equal to 1 if a building was owned by an institutional investor in 2010, an indicator variable equal to 1 if the mortgage on a building in 2010 was a refinance and zip code level occupancy rates as of 2010 as covariates. Panel A displays results examining all code violations, Panel B displays results examining code violations requiring repairs. LTV ratio terciles are assigned based on the LTV ratio of buildings prior to 2011. We include the top and bottom terciles of LTV ratios in the test sample. Number of violations, number of repair violations, number of violations per 100 units and number of repair violations per 100 units are winsorized at the 1% and 99% levels. Standard errors, clustered at the building level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics and code violations data are provided by the New York City government.

Variable	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)
Panel A – All Violations			
$Treat_i \times TopLTV_i \times After_t$	4.179**	13.061**	0.074
	(1.847)	(5.399)	(0.062)
$Treat_i \times After_t$	1.583	0.487	0.046
	(1.300)	(3.942)	(0.042)
$TopLTV_i \times After_t$	0.000	0.000	0.000
	(.)	(.)	(.)
Adjusted R^2	0.507	0.470	0.612
Obs.	3,366	3,366	3,366
Panel B – Repair Violations			
$Treat_i \times TopLTV_i \times After_t$	2.443**	7.333**	0.075
	(1.201)	(3.438)	(0.069)
$Treat_i \times After_t$	1.279*	1.468	0.044
	(0.761)	(2.245)	(0.050)
$TopLTV_i \times After_t$	0.000	0.000	0.000
	(.)	(.)	(.)
Adjusted R^2	0.491	0.459	0.568
Obs.	3,366	3,366	3,366
FE	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year
S.E Cluster	Building	Building	Building

Table IA.26: Cross-Sectional Variation in LTV at Origination.

This table displays results from the following regression:

$$LTVratio_{it} = \beta_1 X_{1,it} + \beta_2 X_{2,it} + \beta_3 X_{3,it} + \beta_4 X_{4,it} + FE + \epsilon_{it},$$

where $LTVratio_{it}$ is the LTV ratio for the mortgage issued on building i in year t, $X_{1,it}$ are building characteristics, $X_{2,it}$ are local zip code level characteristics, $X_{3,it}$ are building-owner characteristics, $X_{4,it}$ are loan characteristics, and fixed effects vary according to specification and are indicated at the bottom of the table. Data are taken at time of mortgage issuance. Age, units, and the time since the most recent renovation are standardized by subtracting the mean and dividing by the standard deviation for all observations. LTV ratios, ages, number of units, transaction price, interest rates and DSCR are winsorized at the 1% and 99% levels. Standard errors, clustered at the city level, are shown in parenthesis. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Property data are sourced from Real Capital Analytics.

Sample	All RCA Data				Code Violations Sample			
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Building Characteristics								
Building Age	0.002*	0.003	0.003	0.008***	0.003	0.003	0.001	0.010***
	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)	(0.004)	(0.005)	(0.003)
Number of Units in Building	0.006***	0.002	0.003**	0.011***	0.010***	0.005*	0.005	0.017***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)
Mid/High Rise Indicator	-0.002	-0.005**	-0.004	-0.004	-0.002	-0.001	-0.000	0.003
	(0.002)	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)
Transaction Price	-0.004	-0.001	-0.003**	-0.011***	-0.011***	-0.007**	-0.007***	-0.018***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.006)
Time Since Renovation				-0.003***				-0.004**
				(0.001)				(0.002)
Local Economic Characteristics								
Zip Code Capitalization Rate	2.096***	1.961***			2.297***	2.350***		
	(0.138)	(0.152)			(0.492)	(0.536)		
Zip Code Occupancy Rate	0.128***	0.074***			0.183***	0.047		
	(0.022)	(0.019)			(0.045)	(0.042)		
Zip Code Zillow Index	-0.029***	-0.013**			-0.024***	-0.005		
	(0.003)	(0.005)			(0.004)	(0.006)		
Owner Characteristics								
Public Owner	-0.014***	-0.021***	-0.017***	-0.035***	-0.011	-0.021*	-0.023	-0.056**
	(0.004)	(0.004)	(0.006)	(0.013)	(0.012)	(0.011)	(0.016)	(0.021)
Institutional Owner	0.002	0.002	0.005	0.011	-0.005	-0.007	0.006	0.014
	(0.005)	(0.005)	(0.004)	(0.007)	(0.019)	(0.015)	(0.009)	(0.014)
Loan Characteristics								
Loan Held by Government Lender	-0.016***	0.002	-0.002	0.036**	-0.005	0.002	-0.004	0.037
	(0.004)	(0.005)	(0.009)	(0.015)	(0.012)	(0.013)	(0.020)	(0.026)
Fixed-Rate Indicator	-0.004	-0.004	-0.013**	-0.028***	0.016	0.009	0.001	-0.039***
	(0.005)	(0.004)	(0.006)	(0.004)	(0.012)	(0.009)	(0.011)	(0.005)
Refinance Indicator	-0.049***	-0.046***	-0.046***	-0.037***	-0.046***	-0.042***	-0.042***	-0.038***
	(0.001)	(0.001)	(0.002)	(0.004)	(0.004)	(0.004)	(0.003)	(0.005)
Time to Maturity	-0.001***	-0.001***	-0.001***	0.000	-0.001	-0.001***	-0.001	-0.000
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Interest Rate	-0.374	0.168	0.488	1.522***	-0.675	-0.463	-0.139	1.578**
	(0.233)	(0.226)	(0.350)	(0.358)	(0.641)	(0.559)	(0.714)	(0.681)
FE	N/A	Zip	Zip-Year	Zip-Year	N/A	Zip	Zip-Year	Zip-Year
	N/A	Year	N/A	N/A	N/A	Year	N/A	N/A
S.E. Cluster	City	City	City	City	City	City	City	City
R^2	0.257	0.423	0.619	0.656	0.255	0.403	0.586	0.643
Observations	39,780	38,277	32,589	7,584	10,320	10,206	10,793	3,584