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# The Effects of Leverage on Investments in Maintenance: Evidence from Apartments

Lee Seltzer

## **The Effects of Leverage on Investments in Maintenance: Evidence from Apartments**

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### **Abstract**

This paper studies the sensitivity of investment in apartment building maintenance to building debt levels. I use a novel data set combining housing code violations from forty-five U.S. cities with apartment financing information to show that highly leveraged buildings tend to be less well maintained. I then exploit a natural experiment that effectively increases building leverage for some New York City rent-stabilized buildings, but not others. Following the shock, violations increase for affected buildings relative to unaffected buildings. This change in violations is concentrated among more highly leveraged buildings. The results are consistent with debt-reducing investments in maintenance, with consequences for renter quality of life.

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

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A significant fraction of American households rent rather than own their homes, and this fraction has grown from 27% in 2006 to 31% in 2020.<sup>1</sup> Although owners of multifamily real estate assets invest substantially in maintaining their buildings,<sup>2</sup> tenants frequently complain of poor housing quality. For instance, in New York City alone, there were 360,000 housing maintenance code violations granted for unacceptable housing conditions in 2018, relative to a rental housing stock of 2.2 million units.<sup>3</sup> Moreover, apartment buildings are typically financed with mortgage debt, and there has been little systematic analysis of whether building-level debt is related to the building’s maintenance.

In this paper, I use novel data on housing code violations to consider how a building’s financial position affects the asset’s maintenance, where maintenance is defined as investments made by a building owner to prevent the asset’s quality from depreciating. Insights from the corporate finance literature suggest that a firm’s financial position may affect investment decisions (Myers, 1977). As with corporations, owners of a highly-leveraged building may be unwilling or unable to invest in maintenance. The negative effects of debt on maintenance investment may be particularly acute since to some extent, the renters bear these costs, as is the case with corporate investments benefiting stakeholders such as customers and workers (Titman, 1984). In particular, Maksimovic and Titman (1991) argues that firms with excessive debt spend less on investments that are valuable to stakeholders but yield little short-run cash flows, as the equityholders do not receive the long-term benefits from these investments in the case of default. Much the same can be said of decisions to invest in building maintenance, which have limited immediate financial benefit to the owner but are critical for both the tenant’s quality of life and to prevent the asset’s depreciation.

Given theoretical predictions from the corporate finance literature, I test whether apartment buildings with higher debt levels are less well maintained by combining information on apartment mortgage-financing with novel hand-collected data on housing code violations

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<sup>1</sup>Current Population Survey

<sup>2</sup>A major apartment real estate investment trust Equity Residential’s 2019 10-K showed that 25% of total expenses were on property and maintenance expenses.

<sup>3</sup>Source: Housing code violations data and NYC Rent Guidelines Board 2020 Housing Supply Report.

from 45 US cities to identify instances of poor building maintenance. While Sheen and Bernstein (2016) examined the impact of private equity buyouts of restaurants on health code violations, to my knowledge this is the first finance paper to assemble a multi-city panel of municipal housing code violations. Using these data, I implement building-by-year level regressions of housing code violations on loan-to-value (LTV) ratios at origination. The large panel and high data quality allows for the use of a rich set of controls, including time-varying building, loan, building owner, and lender characteristics as well as both zip-code-by-year and mortgage issue year fixed effects. The analysis shows an economically meaningful sensitivity of code violations to LTV ratios. In particular, a one standard deviation increase in a building's LTV ratio (14.1 percentage points) is associated with a 0.059 increase in the expected number of violations involving the building in a given year, or equivalently 6.2% of the sample mean number of violations.

Of course, LTV ratios are not randomly assigned to buildings and may be correlated with omitted variables that are also related to code violations. For instance, building owners might choose higher debt levels for lower quality buildings, which are likely to have more code violations, because future investment opportunities in these buildings are likely to be limited, reducing concerns about financial distress. As a result, even after controlling for a number of observable time-varying characteristics and both zip-code-by-year and mortgage issue year fixed effects, unobserved heterogeneity could compromise causal interpretation of panel regression estimates.

To address concerns about the endogenous nature of leverage, I exploit a natural experiment stemming from a change to New York State's rent stabilization laws in 2011. One key feature of New York's rent stabilization code is that owners of rent stabilized apartments are allowed to pass on a portion of the cost of apartment unit improvements to their tenants through a rent increase.<sup>4,5</sup> The New York Rent Act of 2011 decreased the amount that

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<sup>4</sup>Building owners are allowed to increase the rent for such an improvement so long as either the improvement is made while the unit is vacant, or the current tenant approves of the improvement. In practice, most such increases occur while the unit is vacant in between leases.

<sup>5</sup>Although the law applies to all of New York State, I specifically examine New York City.

owners could increase monthly rents to recoup improvement costs from one-fortieth of the costs to one-sixtieth. For example, a building owner was limited to raising monthly rent by \$83.33 instead of \$125 for a \$5,000 improvement investment, increasing the length of time needed to recoup the investment by 20 months. At a discount rate of 10%, this change would have reduced the value of the building by \$5,000. In this way, the reduction in the ability to pass through the cost of improvements reduced a building's future cash flows from rental income, thereby decreasing the value of the building.

By lowering building values without affecting building debt levels, the Rent Act shock effectively increased the leverage of affected buildings. Importantly from an identification standpoint, while owners of rent stabilized buildings can pass on the cost of improvements plausibly increasing the building's value to renters, they are unable to pass on basic maintenance costs.<sup>6</sup> As a result, by decreasing the cash flow stream from significant improvements, the law change increased leverage for rent stabilized buildings without directly affecting the incentives to invest in basic maintenance.<sup>7</sup>

Adding to the usefulness of the experiment, the law change only applied 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 use a one-to-one nearest neighbor matching to assign each treated building to the most similar control rent stabilized building with 35 or fewer units. I then estimate generalized difference-in-differences regressions comparing changes in violations after the law passed 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, controlling for building and matched-pair-by-year fixed effects.

Consistent with the hypothesis that increasing leverage leads to a reduction in investment

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<sup>6</sup>Note this feature of the rent stabilization code was true both before and after the Rent Act of 2011.

<sup>7</sup>Significant improvements and basic maintenance comprise distinct investment decisions. In particular, rehabilitation or improvements lead to a sudden increase in the building's value, while basic maintenance merely prevents the asset's decay (Arnott, Davidson, and Pines, 1983), meaning shocks to significant building improvements are plausibly exogenous to basic maintenance spending.

in maintenance, violations per building increase by 3.34 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 alternate 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 and robust evidence that the Rent Act leads to a reduction in building maintenance.

The effect of the shock on building maintenance should be increasing in building leverage prior to the shock, as these buildings were the most sensitive to bankruptcy risk at the time the Rent Act went into effect. To test this hypothesis, I examine the impact of the Rent Act on code violations within different LTV ratio terciles. The results are strongest in the top LTV ratio tercile and absent in the bottom LTV ratio tercile. Additionally, triple-difference regressions show that the number of code violations increased by 3.19 for buildings with more than 35 units in the top LTV ratio tercile relative to other buildings with more than 35 units. This indicates that the effect of the Rent Act on code violations is sensitive to building leverage prior to the shock.

I also conduct several tests to examine alternative stories for the change in code violations. For instance, one may worry that the results may be driven by differences between buildings with more than 35 units and other buildings. However, the results are similar when conducting a test that limits the sample to narrow size bins around the 35-unit cutoff, indicating that the increase in violations is not driven by outliers, but rather by buildings near the cutoff of 35 units.<sup>8</sup> Additionally, it appears unlikely that the results are driven by differences in rents for buildings with more than 35 units and other buildings, as the results are also similar when controlling for the base rent of buildings as of 2010.<sup>9</sup>

It is also possible that some building owners may specialize in operating buildings with

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<sup>8</sup>Difference-in-differences regression results using several size-bin dummies in place of a treatment dummy also indicate the results are not driven by outliers.

<sup>9</sup>Treated buildings are matched to control buildings according to zip code level occupancy rates in the baseline specification, further controlling for differences in rental markets for buildings with more than 35 units and other buildings.

low returns to investment, and the shock may disproportionately affect buildings in these owners' portfolios. To address this possibility, I match each treated building to the most similar control building within the same building owner's portfolio as of 2010 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 may capture changes due to other developments in New York's rental market in 2011. To address this concern, I conduct a placebo test using market-rate (i.e., non rent stabilized) apartment buildings in New York, which are subject to similar market conditions but not to rent stabilization ordinances. The results show no difference in the change in violations for these buildings after 2011, indicating that market trends alone are unable to explain the results.

To summarize, the findings in this paper show that a one standard deviation increase in building leverage is associated with a 0.059 increase in the number of code violations. Moreover, code violations increased by more than three-quarters of a standard deviation for treated buildings relative to control buildings following the Rent Act, and the effect was concentrated in buildings with high leverage. This provides evidence that increased leverage following the regulation change drove the increase in code violations. These findings, combined with those in the panel regressions, are consistent with increases in leverage leading to decreases in building maintenance.

This paper contributes to the literature on the implications of a firm's financing activities for the firm's stakeholders. Financial economists have long understood that high firm leverage can lead to underinvestment (Myers, 1977; Whited, 1992; Lang, Ofek, and Stulz, 1996), and that financial constraints can reduce investment (Fazzari, Hubbard, and Petersen, 1988; Kaplan and Zingales, 1997, 2000; Rauh, 2006; Almeida, Campello, and Weisbach, 2004; Opler and Titman, 1994). Specifically, there is both substantial theoretical work (Titman,



1984; Maksimovic and Titman, 1991) and empirical work (Titman and Wessels, 1988; Cohn and Wardlaw, 2016; Bae, Kang, and Wang, 2011; Benmelech, Bergman, and Seru, 2021) examining the effect of firm financing on stakeholders, such as customers and employees. My paper asks how apartment leverage affects renters, who are a key stakeholder of real estate companies.

As building maintenance is a form of investment needed to preserve an apartment building's quality, my paper is especially related to work showing that poor financing capacity decreases product quality (Matsa, 2011; Kini, Shenoy, and Subramaniam, 2016; Phillips and Sertsios, 2013; Adelino, Lewellen, and McCartney, 2021). My paper examines this question in a setting where the different products being compared (i.e. apartment buildings) vary along the dimension of leverage, but are relatively homogeneous along other dimensions. This unique feature of the market makes the result particularly easy to interpret in terms of the building leverage.

Similarly, this paper relates to an emerging literature empirically examining debt overhang problems in real estate markets. It has been shown that debt overhang problems can reduce maintenance and capital expenditure investments by homeowners (Melzer, 2017; Li, 2016) and can also lead homeowners to reduce their labor and be less productive workers (Bernstein, 2021; Bernstein, McQuade, and Townsend, 2021). These papers examine residential real estate, but commercial real estate owners are fundamentally different from homeowners because they do not typically live in their buildings and may have access to alternate capital sources. This paper shows that even given the unique features of the commercial real estate industry, mortgage debt can still distort investment decisions.

Finally, this paper contributes to the literature in commercial real estate and urban economics. Several papers examine the decision to invest in commercial real estate both in terms of new development (Titman, 1985; Grenadier, 1996; Holland, Ott, and Riddiough, 2000; Reher, 2021; Favilukis and Van Nieuwerburgh, 2021) and investment in maintenance (Chinloy, 1980; Arnott et al., 1983; Pavlov and Blazenko, 2005). However, these models do

not account for a building’s financing structure, which I show is an important determinant of maintenance investment. I also contribute to the urban economics literature on rent regulation laws. Previous findings show rent regulation leads to reduced property values (Autor, Palmer, and Pathak, 2014), increased crime (David, Palmer, and Pathak, 2019), misallocation of housing (Glaeser and Luttmer, 2003; Munch and Svarer, 2002; Favilukis, Mabile, 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 the reduction in housing quality is concentrated in buildings with large mortgages, this paper highlights an interesting setting where leverage exacerbates regulatory frictions.

## I. Data

### A. *Code Violations Data*

I identify instances of insufficient maintenance of multifamily buildings using municipal code violations. In the United States, building owners are legally required to provide minimum standards of living for their tenants, which is referred to as implied “warrant of habitability.”<sup>10</sup> Tenants can complain to the city when they feel the building owner has breached their warrant of habitability, in which case the city inspects to evaluate whether the complaint is valid. Examples of problems leading to complaints include infrastructure in need of repair, issues with plumbing, or infestation with rodents. If the city determines the building owner has failed to provide their tenant with adequate living conditions, they serve the owner of the apartment building with a code violation, in which case 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 very severe. For instance, when building owners fail to make

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<sup>10</sup>For the most part, cities create and enforce these standards.

repairs sufficiently quickly in New York City, the government will sometimes make the repair on their behalf and then bill the building owner afterward.<sup>11</sup> The billed repair carries the same weight as a tax lien, and sometimes leads to foreclosure.<sup>12</sup> There are also potential additional costs to violations, such as 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.<sup>13</sup> 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.

In some cases, the cities provide the text associated with the violation. When examining individual violations, it is clear in some cases that the building owner needs to invest in maintenance to remedy the problem. Examples of these types of violations include:

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

“Deteriorated or missing caulking seal around plumbing piping or electrical conduit under all kitchen and bathroom sinks” – Los Angeles, CA

On the other hand, some violations do not necessarily indicate a need for the building owner to invest in maintenance, such as:

“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<sup>14</sup>

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<sup>11</sup><https://www1.nyc.gov/site/hpd/services-and-information/emergency-repair-program-erp.page>

<sup>12</sup><https://www1.nyc.gov/site/finance/taxes/property-in-rem-forclosure.page>

<sup>13</sup>Cities are selected based on their representation in the Real Capital Analytics mortgage data, which is described in Internet Appendix B.

<sup>14</sup>Text shown as it appears in the violation, preserving typos by code enforcement officials.

“Illegal use - hosting concerts on residential parcel” – Nashville, TN

Sometimes, it is ambiguous whether the building owner needs to invest significantly to correct the violation, or where it is unclear whether the tenant or building owner caused the problem.

“Abate the nuisance consisting of roaches in the entire apartment” – New York,  
NY

“See Inspector Comments” – Chicago, IL

To account for these differences, I parse through all violations for which text is provided and identify all violations which indicate the need for the building owner to make repairs. The intuition behind this approach is that if the building owner is ordered to make repairs this is indicative of insufficient maintenance investment.<sup>15</sup> I detail how violations are classified into these two categories in Internet Appendix A.

Several other data sets are used in this paper. They are described in Internet Appendix B, along with details on the sample construction.

## *B. Summary Statistics*

Table 1 provides information on the code violations data set. 14.6% of total building-year observations incur at least one code violation. 33.1% of buildings incur at least one code violation at some point in the sample period. Data are displayed visually in Figure 1, where the size of each dot is proportional to the number of observations in that Metropolitan Statistical Area (MSA). Points on this map are darker shades of blue if the city has more violations per number of observations. This map makes it clear that there is substantial variation in the likelihood of incurring a code violation across cities in the United States. This could be due either to differences in code enforcement across the US, or differences

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<sup>15</sup>I exclude those violations which indicate need to make large-scale investments to focus on basic maintenance. However, results are similar if I include those large-scale investments.

in the quality of the building stock across the US. I will control for these differences using high-dimensional fixed effects and controlling for building attributes related to its quality.

Summary statistics for the data set employed in this paper are displayed in Table 2. The average number of violations incurred per building in each year is 0.957 and the average number of violations per 100 units for each building in a given year is 2.51. On average, each building incurs 0.463 violations requiring a repair per year, which is about half the amount for all violations. 58.6% of the sample was held in a commercial mortgage backed security (CMBS) and 60.5% was issued by a government lender.

## **II. The relationship between LTV ratios and code violations**

In this section, I test whether more highly-leveraged 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. I residualize LTV ratios for mortgages between 0 and 1 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...). Afterward, I calculate the average residualized number of violations, number of violations per 100 units and probability of incurring a violation within each of these bins. I then graph the resulting binscatter plots, along with the corresponding linear regression lines and confidence intervals, in Figure 2. Note that the size of each point in the plot is proportional to the number of observations in each bin.

No matter which measure of code violations is used, code violations are increasing in LTV ratios after controlling for zip code level time trends. These figures illustrate a clear tendency for highly-leveraged buildings to incur more code violations, which is consistent with highly-leveraged buildings being less well maintained. To consider maintenance more

clearly, Figure IA.1 displays plots using only repair violations. Similarly, there is a positive relationship between violations needing a repair and LTV ratios, which is consistent with maintenance investment driving the relationship between LTV ratios and code violations.

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

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

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  $t - 1$ .<sup>16</sup> I use the LTV ratio at origination as my main measure of building leverage as this provides a proxy for the debt burden faced by the borrower in terms of the size of their debt payments over the life of the mortgage, which is an important channel through which leverage impacts incentives to invest.

$X_{it-1}$  is a vector of building, loan, lender, and borrower controls.<sup>17</sup> Building controls include the building transaction price in millions, the building's age, an indicator variable equal to 1 if the building is either a mid-rise or a high-rise and the number of units in the building.<sup>18</sup> Borrower-level controls include an indicator variable equal to 1 if a building is owned by a public company, an indicator variable equal to 1 if a building is owned by an institutional investor, an indicator variable equal to 1 if a building is owned by a joint venture and an indicator variable equal to 1 if there is a pre-existing relationship between the borrower and loan originator. Lender-controls include an indicator variable equal to 1 if a loan is held by a CMBS lender and an indicator variable equal to 1 if the loan was made by a government lender. Loan-level controls include the loan interest rate, an indicator variable

<sup>16</sup>LTV ratios are standardized by subtracting the sample mean and dividing by the sample standard deviation.

<sup>17</sup>Controls were selected based on a cross-sectional analysis of the determinants of apartment building leverage in Internet Appendix C.

<sup>18</sup>RCA defines mid or high-rise buildings as buildings that have four floors or more.

equal to 1 if a loan is a refinance of a pre-existing loan, an indicator variable equal to 1 if the mortgage is fixed-rate, the mortgage time to maturity, and the mortgage debt-service coverage ratio.  $\gamma_{zt}, \kappa_v$  are zip-code-by-year and mortgage issue year fixed effects. All regressions are clustered at the city-level.

The regression coefficient  $\beta_1$  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 leverage. If more highly-leveraged buildings incur more code violations, I expect  $\beta_1$  to be positive.

Table 3 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 predicts 0.059 more violations per year, or 6.2% of the sample mean. This is a substantial increase in the amount of code violations. Older buildings incur more code violations. Lastly, higher maturity mortgages incur fewer code 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 0.290 violations per 100 units, or 11.5% of the sample mean. Column (3) displays results using the violation indicator, which shows that a one standard deviation increase in building leverage 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 increases in leverage predict increases in code violations. I next examine whether increases in leverage are associated with violations requiring a repair (i.e. new maintenance investment). Results are displayed in Table 4. While the estimate on the violation indicator is not statistically different from zero, the estimates in the other specifications are positive and statistically significant, providing further evidence that the

results are driven by less investment in highly-leveraged buildings.

Several robustness checks are presented in the Internet Appendix. To better control for the quality of a building, Table IA.1 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. 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.2. Table IA.3 also shows results excluding the five most widely represented cities (New York, Los Angeles, Houston, Chicago and Seattle) from the sample. One concern about the LTV ratio in the data is it only includes first mortgages, so it may underestimate the leverage that buildings with second mortgages have. To address this concern, Table IA.4 includes results using a combined LTV ratio using information on second mortgages, and the results are similar. Additionally, Table IA.5 displays regression results using an imputed market LTV ratio in all specifications. In all of these tests, the results are consistent with the conclusions from the baseline specification that higher building leverage is related to more code violations.

### **III. Identification - 2011 NYC Rent Law**

#### *A. Determinants of Building Leverage*

Endogeneity can bias the estimates from Equation 1 since leverage is not randomly assigned to buildings. To more clearly illustrate why this is the case, Figure 3 displays maps of New York City showing average LTV ratios and capitalization rates (which is the measure used to capture the rate of return in commercial real estate) 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 leverage 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. Owners of apartments in zip codes with higher capitalization rates may choose to borrow more because these apartments tend to be less profitable and therefore have lower returns to maintenance investment.<sup>19,20</sup>

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

### *B. 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 building is rent stabilized, the building owner must abide by the guidelines from the New York City Rent Guidelines Board in setting their rent. These guidelines are updated annually according to the conditions of the real estate market.<sup>21</sup> 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, referred to as Individual Apartment Improvements (IAIs). 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.<sup>22</sup> The city enforces this restriction by requiring building owners to submit DHCR Form RN-79b as

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<sup>19</sup>This has particularly important implications in my paper as it has been argued that building maintenance is strongly related to neighborhood characteristics (Pavlov and Blazenko, 2005).

<sup>20</sup>A more formal analysis of the determinants of building leverage is provided in greater depth in Internet Appendix C.

<sup>21</sup>This is distinct from rent control, which has largely been phased out of New York City after rent stabilization was adopted in 1969 and currently only applies to 22,000 units.

<sup>22</sup>The only exception to this rule is if ordinary repairs are a necessary part of completing an approved IAI.

documentation of the service provided as well as the tenant's consent of the rent increase.<sup>23,24</sup>

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.<sup>25</sup>

An example using a \$5,000 bathroom renovation is displayed in Figure 4. 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.<sup>26</sup> This law change therefore decreased a building's future cash flows conditional on investing in IAIs for buildings with more than 35 units, but not those with 35 or fewer.

To reiterate, a key aspect of the rent stabilization law is that the types of 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, building owners may use operating cash flows obtained by increasing

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<sup>23</sup>Examples of IAIs include replacing equipment such as a stove, renovating the bathroom or replacing the carpeting.

<sup>24</sup>Information on New York rent stabilization and IAIs can be found at <https://rentguidelinesboard.cityofnewyork.us/>

<sup>25</sup>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. Aside from these provisions, there were minimal changes introduced by the law change. For reference see the full text of the law here: <https://rentguidelinesboard.cityofnewyork.us/wp-content/uploads/2019/10/rentact2011.pdf> and other details including examples of individual apartment improvements at [https://hcr.ny.gov/system/files/documents/2020/02/operational-bulletin-2016-1\\_0.pdf](https://hcr.ny.gov/system/files/documents/2020/02/operational-bulletin-2016-1_0.pdf)

<sup>26</sup>If you value a building using discounted cash flows and use a discount rate of 10%, this reduced the value of the building by \$5,000.

rent after making building improvements to finance repairs. Therefore, the law was a shock to a building’s future cash flows, which decreased the building’s value, thereby increasing the building’s leverage. Through this channel, the Rent Act decreased the ability for building owners to make repairs, which is plausibly exogenous to incentives for building owners to make basic maintenance investments.

This law change is also empirically useful in that it only impacted rent stabilized buildings with more than 35 units, allowing me to designate a control group of rent stabilized buildings with 35 or fewer units. Therefore, it is possible to observe cross-sectional variation in changes to leverage from the law within the relatively homogeneous pool of rent stabilized buildings in New York City.<sup>27</sup>

### *C. 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, to those with 35 or fewer units, before and after 2011 by running a difference-in-differences regression.<sup>28</sup> The control group is composed exclusively of rent stabilized buildings not impacted by the Rent Act.<sup>29</sup>

To control for differences between rent stabilized buildings with more than 35 units and those with 35 or fewer units, I conduct a one-to-one nearest neighbor matching procedure with replacement. This approach matches each treated building with the control building

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<sup>27</sup>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/>.

<sup>28</sup>I use a difference-in-differences regression instead of a regression discontinuity 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. However, I include 2 tests with a similar intuition to a regression discontinuity design where I (1) include a version of the test where I instead use subsamples of buildings with relatively similar sizes to the cutoff of 35 units and (2) run the difference-in-differences regression placing treated buildings into size bins and examining the effect of the law on treated buildings that have different numbers of units. The results of both of these tests indicate the findings are not driven by buildings far from the cutoff.

<sup>29</sup>To determine which buildings in my sample were rent stabilized at the time the law passed, I merge the mortgage and code violations data set with a list of buildings with rent stabilized units in New York City as of 2011 posted publicly online after being obtained by a Freedom of Information Law (FOIL) request from <https://github.com/clhenrick/dhcr-rent-stabilized-data>.

that has the shortest Mahalanobis distance calculated along observable characteristics (Mahalanobis, 1936). I choose matching covariates that best control for the most important traits differentiating buildings with more than 35 units from other buildings that could also be correlated with the outcome variables.<sup>30</sup> By utilizing this approach, I ensure that I am comparing each treated building with the most similar control building possible.

I calculate the Mahalanobis distance using an indicator variable for whether the building has more than 35 units as the treatment variable and average building LTV ratio over the pre-period, the most recent property transaction price as of 2010, the building age 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 of a pre-existing mortgage and the zip code level occupancy rate based on rent stabilized buildings in RCA in 2010 as covariates.<sup>31,32</sup> The difference-in-differences regression equation is as follows:

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

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 an indicator variable equal to one if the building has more than 35 units,  $After_t$  is an indicator variable equal to one if the observation is from 2011 or later.  $\gamma_i, \kappa_{pt}$  are building and matched-pair-by-year fixed effects. In reported results, I cluster standard errors at the building-level.<sup>33</sup>  $\beta_1$  can be interpreted

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<sup>30</sup>The purpose of the matching is improve the precision of the estimation. However, difference-in-differences results with no matching are included in the Internet Appendix and are qualitatively similar to those with matching. I also include several tests employing variations on the matching procedure in the Internet Appendix and the results are qualitatively similar.

<sup>31</sup>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.

<sup>32</sup>It has been shown that with two or more continuous variables the Mahalanobis distance is biased. To address this, I use the appropriate bias adjustment in conducting the matching (Abadie and Imbens, 2006).

<sup>33</sup>Results clustered at the zip-code-level are shown in the Internet Appendix.

as the difference in the change in  $Violations_{it}$  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  $\beta_1$  to be positive if increases in leverage 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.<sup>34</sup> 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.

One important identifying assumption is changes in the building value after the Rent Act and basic maintenance spending decisions are only related through changes in leverage. 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 my repair violations measure based on the classifications in Internet Appendix A. For this reason, if the identifying assumption is compromised, I would not expect the results to hold when examining violations requiring repairs.

Similarly, one may be concerned that building owners reduce investment after changes in rent regulation laws due to changes in the returns on these investments. However, building owners are not allowed to increase rent for basic maintenance investment, meaning that the law did not directly affect profitability of the types of investments that are most important for mitigating code violations. I also match on zip code level occupancy rates prior to the shock, which captures turnover of units, as well as the most recent transaction price for the building, capturing the building's value. I also provide a robustness check matching on building rent.

Table IA.6 displays summary statistics for the treated and control groups. Statistics are taken for the samples as of 2010. Differences between the treated and control groups are

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<sup>34</sup>Results using alternative time periods are included in the Internet Appendix.

economically small, indicating that the matching is effective. While in some cases there are statistically significant differences, it is more important that the outcome variables do not change differentially for the treated and control groups before the shock, which I will formally show is the case after discussing the difference-in-differences regression results.

Next, I test whether the law change affected the values of treated buildings by using data on tax assessments for rent stabilized New York City apartment buildings in 2010 and 2012. I then conduct a difference-in-differences regression comparing the changes in the appraised values of treated rent stabilized apartment buildings with control rent stabilized apartment buildings from 2010 to 2012.<sup>35</sup> Results are displayed in Table IA.7. The results show that following the Rent Act, appraisal values per unit decrease for treated buildings relative to control buildings, providing evidence of a decrease in building values after the Rent Act, leading to increasing building leverage.

#### *D. Difference-in-Differences Results*

Table 5 displays difference-in-differences results. Panel A has results using all code violations. Column (1) shows results using the number of violations per building, which show an increase in code violations for treated buildings following the Rent Act. In particular, code violations increase by 3.34 for treated buildings relative to control buildings. 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 6.71 for treated buildings relative to controls after the Rent Act. Column (3) shows results using the violation indicator, where the estimate is statistically insignificant. 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 2.24 for treated

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<sup>35</sup>I use appraised values per unit to control for the size of the building.

buildings relative to controls following the Rent Act. According to column (2), violations requiring a repair per 100 units increases by 4.85 relative to controls following the Rent Act. Finally, column (3) shows the probability of incurring a violation increases by 6.8 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 ties the reduction in code violations following the Rent Act to reductions in building maintenance. 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 and not from building owners investing in fewer IAIs. This provides evidence that the results are unlikely driven by changes in the profitability of maintenance investment following the law change.

As a whole, the results in Table 5 are consistent with affected building owners incurring substantially 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 that require repairs, which is consistent with decreases in maintenance investment due to increasing leverage after the Rent Act.

Next, I formally 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:

$$Outcome = \sum_{j=2008}^{2015} \beta_{1j} [Treat_i \times \mathbb{1}(j = t)] + \gamma_i + \kappa_{pt} + \epsilon_{it}. \quad (3)$$

Each  $\beta_{1j}$  can be interpreted as the difference between the outcome variable in year  $j$  relative to 2007 for New York City rent stabilized buildings larger than 35 units relative to those with 35 units or fewer.  $\beta_{1j}$  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 were evolving 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. Results for violations requiring repairs is shown in Figure IA.2, and the conclusions are similar.

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.

## IV. Additional Tests

The results in Section III.D show that following the passage of the Rent Act of 2011, code violations increase for New York City rent stabilized buildings with more than 35 units, which is consistent with high leverage leading to reductions in building maintenance. In this section, I first show that the increase in violations after the Rent Act is sensitive to building leverage, providing further evidence that the effect is driven by leverage. I then examine several alternative stories that could potentially explain the increase in building leverage.

### *A. Are Results Sensitive to Leverage?*

Because it decreased building values without affecting debt levels, the Rent Act was effectively a shock to building leverage. Generally speaking, shocks to leverage should more negatively affect maintenance investment for buildings that were more highly-leveraged ex ante, as they were closer to bankruptcy at the time of the shock. To evaluate this claim,



I test whether violations increase more for treated buildings with high leverage before the shock relative to other treated buildings

To consider whether the Rent Act disproportionately affects highly-leveraged buildings, I divide the sample into terciles based on the building loan-to-value ratio calculated using the average building LTV ratio across the pre-period (2007-2010). Afterward, I repeat the difference-in-differences analysis in each of these terciles.

Results are displayed in Table 6. Panel A shows the buildings in the bottom tercile of LTV ratios. For all outcome variables examined, 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 larger and more statistically significant than in Panel A; however, the change in both the number of violations and the number of violations per 100 units are smaller in magnitude than the estimates in the main specification.

Panel C displays results for buildings in the top LTV ratio tercile. Column (1) shows that the number of violations per building increases by 5.56 for treated buildings relative to controls after the Rent Act. Column (2) shows the number of violations per 100 units increases by 13.2 for treated buildings relative to control buildings, an increase that is about double that found using the whole matched sample. Column (3) uses the violations indicator, which is statistically indistinguishable from zero. In all three specifications, the results for the top LTV tercile are larger than those observed in Table 5. Moreover, for the number of violations per building and number of violations per 100 units, the estimates are larger than those seen in all other terciles. Columns (4) through (6) display results using violations requiring repairs and the findings are similar. Just as when using all the violations, the increase in violations requiring repairs appears to be largest for buildings in the top LTV tercile.

To more formally compare treatment effects for buildings in the highest LTV ratio tercile

relative to others, I implement the following triple-difference regression:

$$\begin{aligned}
 Violations_{it} = & \beta_1 Treat_i \times After_t + \beta_2 TopLTV_i \times After_t \\
 & + \beta_3 Treat_i \times TopLTV_i \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it},
 \end{aligned} \tag{4}$$

where  $TopLTV_i$  is equal to one if building  $i$  is in the top LTV ratio tercile. To reiterate, the LTV ratio terciles are assigned using the building leverage before the shock.  $\beta_3$  is the additional impact that the Rent Act has on buildings in the top LTV ratio tercile relative to other buildings. This can be interpreted as the sensitivity of the effect of the Rent Act on property maintenance to leverage. The primary assumption necessary to interpret the results is that within the treated and control groups, a building's leverage before the shock is unrelated to the change in code violations after the shock. If this assumption holds, code violations would have evolved similarly for treated buildings that with high leverage and those with low leverage if the Rent Act had never passed.

Results are displayed in Table 7. Panel A displays results using all code violations. Column (1) uses the number of violations as the outcome variable, which increase by 3.19 for top LTV ratio tercile treated buildings relative to other treated buildings after the Rent Act. Column (2) shows the number of code violations per 100 units increase by 9.99 for top LTV ratio treated buildings relative to other treated buildings. In fact, the results show that violations per 100 units do not change at all for treated buildings that do not have high leverage, providing further evidence that high debt levels drove reduced maintenance following the Rent Act. Column (3) shows results using the violation indicator, which did not experience a statistically significant increase.

Panel B displays results using only violations requiring a repair, which are very similar. These results show that while the Rent Act is unimportant for buildings outside the top LTV ratio tercile, it severely affects those in the top LTV ratio tercile. Moreover, the effect is also present for violations requiring repairs. Additionally, note the difference-in-differences

estimate is statistically insignificant in these regression results, indicating that the decrease in code violations is driven by changes in violations for buildings in the top LTV ratio tercile. This is consistent with leverage explaining the change in violations for treated buildings following the Rent Act.

## *B. Examining Alternative Stories*

The subsample and triple-difference results provide evidence that building leverage is the main 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.

### **B.1. Controlling for Differences in Size**

The Rent Act of 2011 only affected buildings with more than 35 units. However, as shown in Section C, a building's size is related to both its LTV ratio and its propensity to incur code violations, so it is possible that 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 that buildings sufficiently close to the cutoff are likely very similar (the rationale is analogous to that of a regression discontinuity design). 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 8, where Panel A includes only buildings with 10-to-60 units, Panel B includes only buildings with 15-to-55 units, Panel C includes only buildings with 20-to-50 units, and Panel D includes buildings with 25-to-45 units.

In all subsamples, there is a positive and statistically significant increase in the number of violations per building and the number of violations per 100 units. This remains true even in the most restrictive bins, making it appear increasingly unlikely that the results are driven by outliers.

Nonetheless, it is not clear that the results within the size bins are driven by the buildings closest to the cutoff. To consider this further, use the following regression on the subsample of buildings with no more than 75 units:<sup>36</sup>

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

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,  $\beta_1$  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 results should not be stronger for the larger buildings in the sample relative to the buildings closer to the cutoff.

Results are displayed in Table 9, 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 55 and 65 units, but the magnitude is smaller than for those between 35 and 45 units. Overall, the magnitude of the estimates are decreasing in the size of each bin, and in fact there is no change in code violations for buildings that have between 65 and 75 units, indicating that the change in code violations is driven by buildings closest to the cutoff of 35 units.

Together, these two tests provide evidence that the results are not 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

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<sup>36</sup>I restrict the sample to buildings with fewer than 75 units in order to exclude the largest buildings from the sample to ensure the results are not contaminated by outliers.

units specified by the law.

## **B.2. Controlling for Differences in Rental Rates**

While after the Rent Act, code violations decreased 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 per-unit rent, it is possible their investment decisions would be more sensitive to the distortions induced by the Rent Act. 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.<sup>37</sup> 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 10, and find similar results to the baseline specification. This provides evidence that the results are not driven by differences in the rental rates of the assets. Moreover, as the sample is limited to buildings with rent increases no greater than 2%, it is clear that the results hold when examining buildings where it is possible to validate the owners comply of rent stabilization ordinances.

## **B.3. Does Building Leverage Proxy for Owner Characteristics?**

It is possible that building owners whose portfolios have larger apartment buildings have other traits making code violations more likely. For instance, financially constrained building management companies may be forced to use high levels of mortgage debt due to insufficient access to other capital markets. Additionally, some building owners may specialize in

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<sup>37</sup>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.

operating buildings with low returns to investment. To control for owner-characteristics, I implement difference-in-differences regressions on a sample matched within building owner.<sup>38</sup> 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 11 and the results are qualitatively similar. Based on these findings, the increase in violations surrounding the Rent Act was likely not driven by owner-level characteristics.

#### **B.4. Placebo Test on Non Rent Regulated Buildings**

The New York Rent Act of 2011 can only act as a shock to leverage if nothing else occurred in New York City that year affecting large buildings relative to small buildings. Note that if the increase in violations was driven by other market trends in New York City, the same effect should be observed for non rent stabilized buildings with more than 35 units in New York. However, those building owners should not react to the Rent Act. Using this insight, I conduct a placebo test using New York City buildings that were not rent stabilized (i.e. market-rate rental housing) in 2011. If the results are not driven by other New York City rental market trends, we should not observe a change in code violations after 2011.

Results are shown in Table 12. In all specifications, the estimate of  $\beta_1$  is close to zero and is not statistically significant. Since the increase in code violations observed for large buildings relative to small buildings in New York did not also occur in market-rate housing, it appears unlikely that the change in code violations following the Rent Act was due to other market conditions.

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<sup>38</sup>Matching specification is the same as in main tests except the institutional investor indicator is excluded since matching is done within owner.

### *C. Other Robustness Checks*

Rent stabilized buildings with more than 35 units may overall have lower quality than those that those with 35 or fewer units, which may make building owners more sensitive to the reduction in cash 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 in place of a building's age. Results are displayed in Table IA.8, and are qualitatively similar to those in the main specification. This provides further evidence that the results are not driven by differences in quality for rent stabilized buildings exposed to the Rent Act shock and the controls.

The results from Section III uses 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.9. 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.10 displays tests using several different time windows. Table IA.11 displays results on the full unmatched sample. Lastly, Table IA.12 shows results clustering standard errors at the zip-code-level instead of the building-level. In all of these cases, the conclusions are qualitatively similar.

## V. Conclusion

Given the large population of renters in the United States, it is important for academics and policy-makers to understand characteristics of the rental housing market. As financial economists, we are uniquely positioned to understand the implications of apartment financing on apartment renters. The results in this paper make it clear that more highly-leveraged 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 capital structure has important implications for apartment renters. More broadly, this paper highlights an example of an interesting and socially relevant situation where a firm's financing decisions has notable consequences for its customers.

By showing that apartment buildings with higher leverage have lower basic maintenance investment, these results provide evidence that leverage reduces 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.

These results highlight one cost of high levels of apartment debt, while leaving welfare analysis to future work. Nonetheless, the findings in this paper highlight the importance of understanding financing policies of apartment owners for the long-term asset value of buildings as well as the living experience of tenants. By doing so, this paper highlights one cost of high debt that firms should consider when choosing their capital structure.



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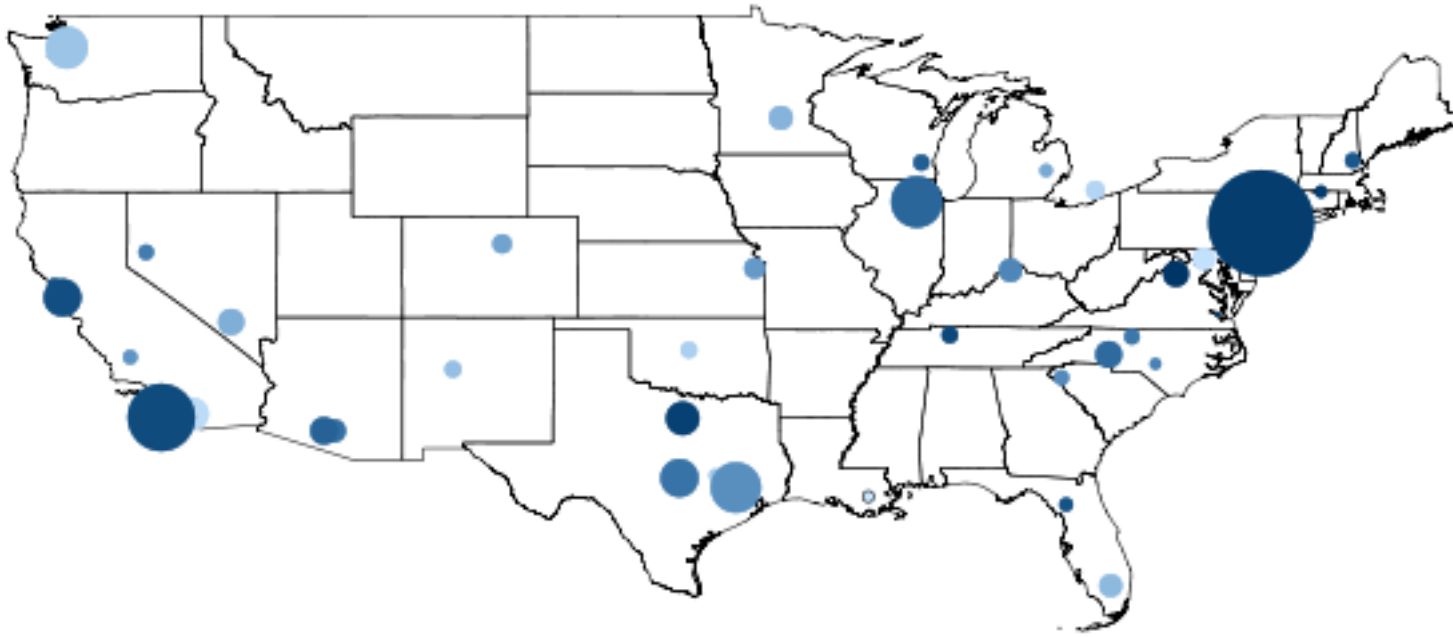
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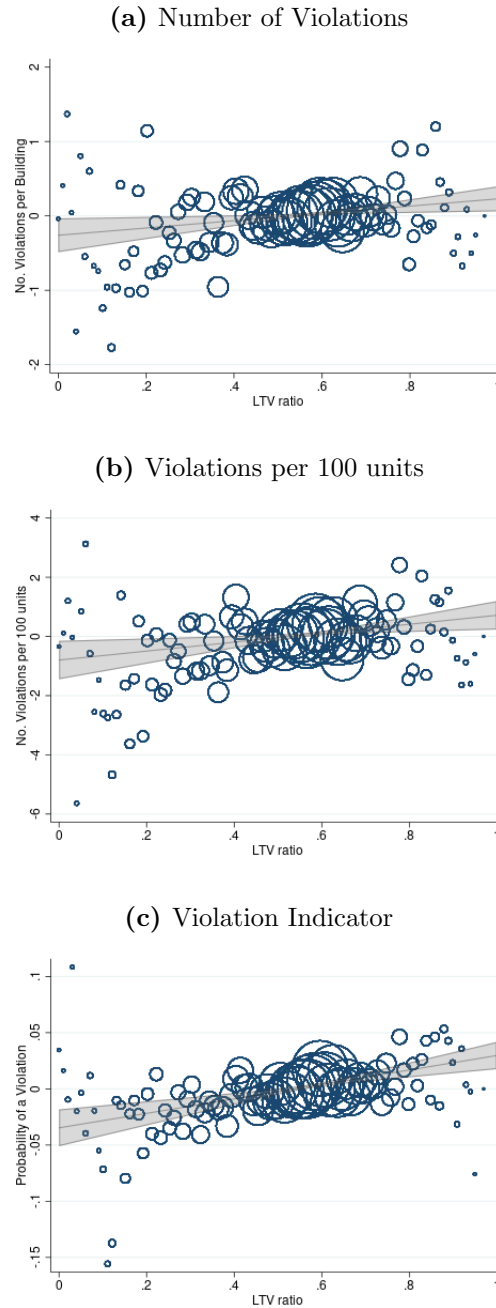
**Figure 1.** Geographic Distribution of Data

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.



**Figure 2.** Correlation Between LTV and Code Violation

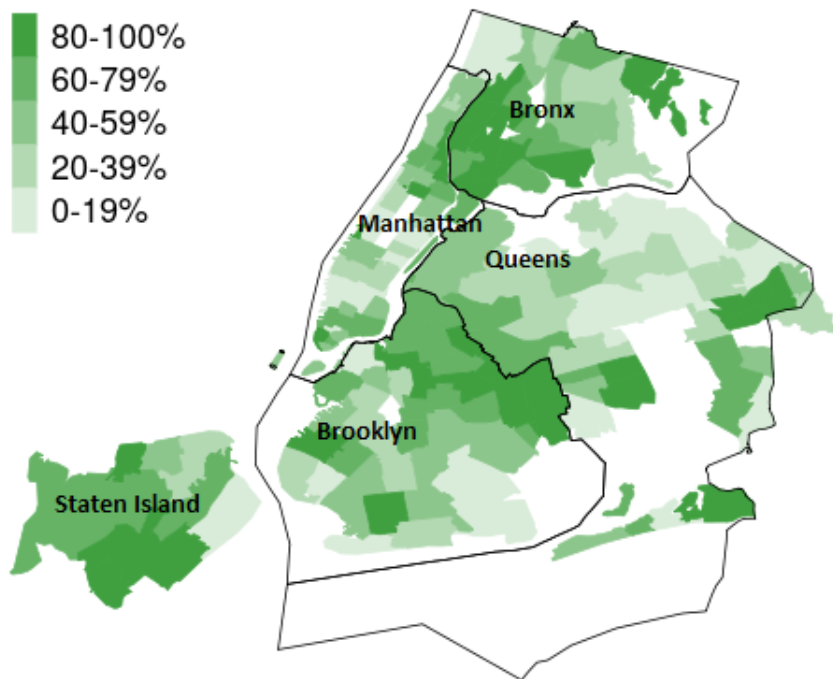
Code violations (measured using the number of code violations in a given year, the number of code violations per 100 units in a given year, or an indicator variable equal to one if a property incurs a code violation in a given year) graphed in 100 LTV ratio percentile bins, where the y-axis shows average 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.



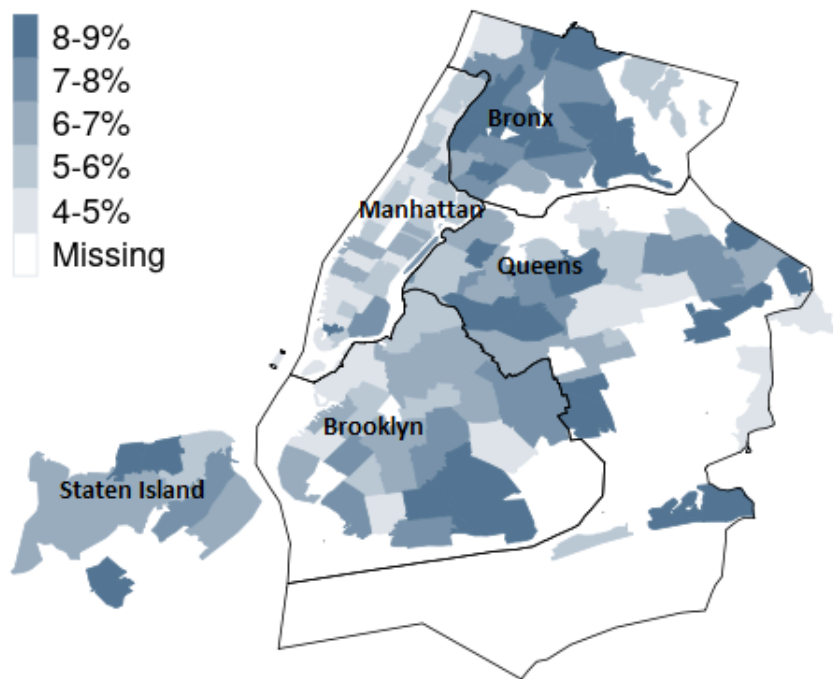
### Figure 3. Within Zip Code Variation in LTV – New York City

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

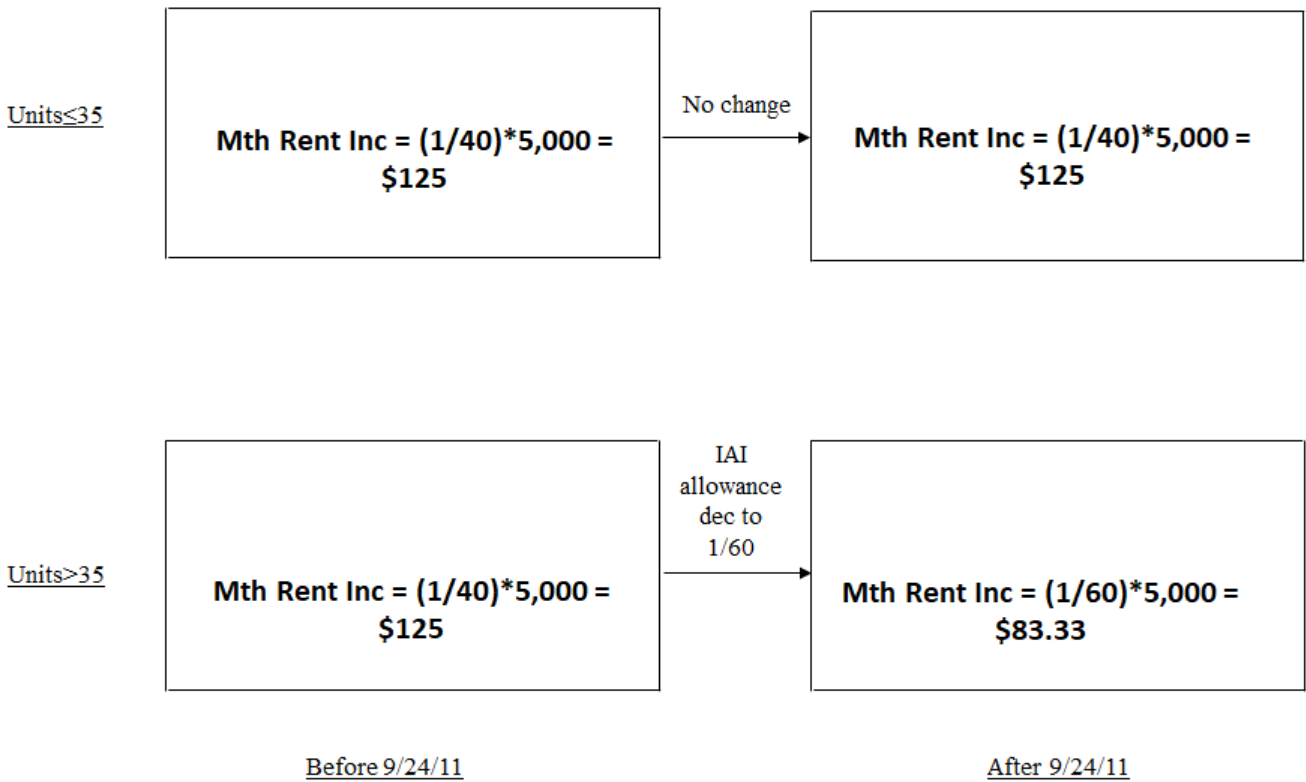


(b) Average Capitalization Rates by NYC Zip Codes





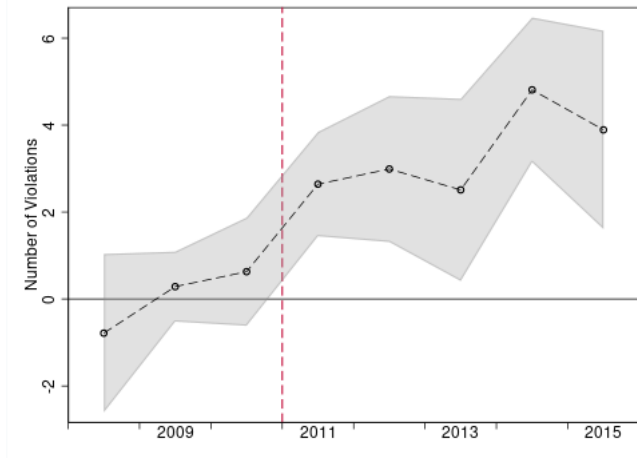
**Figure 4.** Impact of Rent Act of 2011 on \$5,000 bathroom renovation



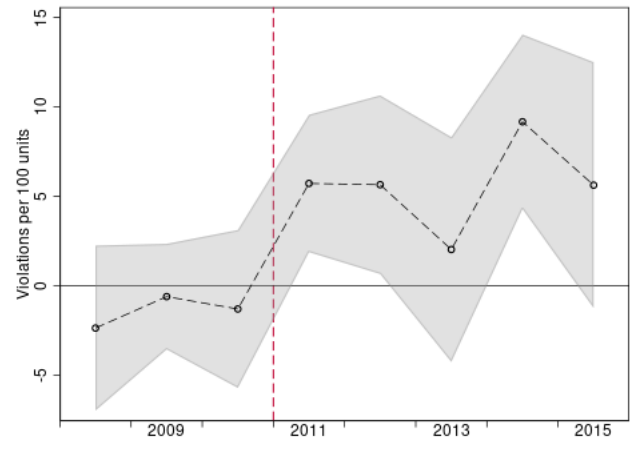
**Figure 5.** Dynamic Difference-in-Differences Results – All Code Violations

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. 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 average building LTV over the pre-period, most recent transaction price as of 2010, building age 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. Property data are sourced from Real Capital Analytics, and code violations data are from various municipal governments.

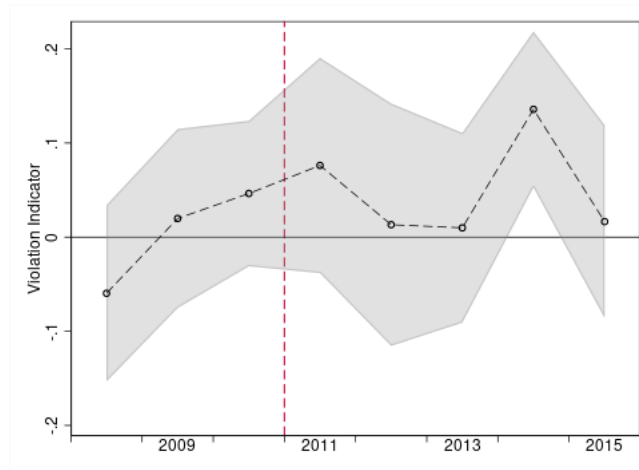
(a) Number of Violations



(b) Violations per 100 units



(c) Violation Indicator



**Table 1:** 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 Albuquerque NM, Aurora CO, Bakersfield CA, Burbank CA, College Station TX, Detroit MI, El Cajon CA, Fayetteville NC, Gainesville FL, Greensboro NC, Greenville SC, Hartford CT, Milwaukee WI, Nashville TN, New Orleans LA, Reno NV, Santa Rosa CA, Tempe AZ 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	38,230	7,998	5,048	1,836	2001	2018
Los Angeles	19,348	2,151	2,335	807	2003	2019
Houston	9,696	525	1,206	237	2003	2018
Chicago	8,999	1,238	1,650	517	2006	2018
Seattle	6,151	248	780	159	2003	2018
San Francisco	5,673	689	664	284	2002	2018
Austin	5,070	722	622	232	2001	2019
San Diego	4,623	56	560	50	2002	2018
Philadelphia	3,567	376	485	151	2007	2019
Washington	2,832	458	421	200	2007	2019
Charlotte	2,436	190	324	118	2007	2019
Fort Worth	2,389	1,300	315	230	2006	2019
Las Vegas	2,275	125	444	68	2012	2018
Tucson	2,149	647	304	172	2008	2018
Fort Lauderdale	2,140	204	226	51	2002	2019
Cincinnati	1,881	213	245	73	2002	2019
Baltimore	1,830	43	259	36	2004	2019
Minneapolis	1,731	3	296	2	2001	2018
Mesa	1,502	545	143	116	2004	2018
Anaheim	1,429	442	166	122	2003	2018
Tacoma	1,420	57	181	38	2004	2019
Dallas	1,369	167	536	141	2013	2015
Kansas City	1,326	76	240	52	2009	2019
Cleveland	1,119	32	181	23	2005	2019
Boston	1,070	310	107	53	2001	2019
Oklahoma City	1,034	71	218	47	2014	2019
Other	12,352	2,165	1,927	760	N/A	N/A
Total	143,641	21,051	19,883	6,575	N/A	N/A

**Table 2: Summary Statistics**

Number of violations, number of violations per 100 units, number of repair violations, number of repair violations per 100 units, imputed LTV ratios, LTV ratios, Combined LTV ratios, interest rates, number of units per building, building ages, Zillow index, debt-service coverage ratios 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	77,640	0.957	4.120	0.000	35.000
Violations per 100 Units	77,640	2.505	12.311	0.000	100.000
Violation Indicator	77,640	0.134	0.341	0.000	1.000
Number of Repair Violations	68,743	0.463	2.434	0.000	22.000
Repair Violations per 100 Units	68,743	1.352	7.428	0.000	63.158
Repair Violation Indicator	68,743	0.069	0.253	0.000	1.000
LTV Ratio	77,640	0.651	0.141	0.051	1.033
Combined LTV Ratio	77,640	0.659	0.151	0.051	1.410
Imputed LTV Ratio	68,137	0.687	0.264	0.046	1.864
Transaction Price	77,640	12.907	17.801	0.688	106.250
Building Age	77,640	49.681	32.456	1.000	120.000
Mid/High Rise Indicator	77,640	0.297	0.457	0.000	1.000
Number of Units in Building	77,640	123.316	131.878	5.000	618.000
Public Owner	77,640	0.008	0.089	0.000	1.000
Institutional Owner	77,640	0.085	0.279	0.000	1.000
Joint Venture	77,640	0.053	0.223	0.000	1.000
Borrower-Originator Relationship	77,640	0.432	0.495	0.000	1.000
CMBS Indicator	77,640	0.586	0.492	0.000	1.000
Loan Held by Government Lender	77,640	0.605	0.489	0.000	1.000
Refinance Indicator	77,640	0.790	0.407	0.000	1.000
Fixed-Rate Indicator	77,640	0.949	0.220	0.000	1.000
Interest Rate	77,640	0.051	0.011	0.025	0.079
Time to Maturity	77,640	7.105	5.422	0.000	30.167
Debt-Service Coverage Ratio	77,640	1.558	0.780	0.000	9.880
Time Since Most Recent Renovation	23,048	10.531	11.666	0.000	127
Property Capitalization Rate at Origination	41,270	0.062	0.015	0.011	0.134
Property Occupancy Rate at Origination	67,160	0.945	0.057	0.440	1.000
Zip Code Zillow Index	69,585	448,804.7	389,023.8	34,400	3,338,500

**Table 3:** Relationship Between LTV Ratios 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 1 if building  $i$  incurs a code violation in year  $t$ .  $LTVratio_{it-1}$  is the LTV ratio for building  $i$  in year  $t - 1$ .  $X_{it-1}$  are control variables for building  $i$  as of year  $t - 1$ .  $\gamma_{zt}, \kappa_v$  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, interest rates and debt-service coverage ratios 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)
LTV Ratio	0.059*** (0.017)	0.290*** (0.106)	0.006** (0.003)
<i>Building Controls</i>			
Transaction Price	-0.005 (0.003)	-0.023** (0.009)	-0.001*** (0.000)
Building Age	0.004*** (0.001)	0.021*** (0.005)	0.000*** (0.000)
Mid/High Rise Indicator	0.442** (0.213)	0.438 (0.613)	0.035*** (0.008)
Number of Units in Building	0.001 (0.001)	-0.003 (0.002)	0.000*** (0.000)
<i>Building Owner Controls</i>			
Public Owner	-0.090 (0.156)	-0.129 (0.344)	-0.013 (0.021)
Institutional Owner	-0.006 (0.048)	0.291* (0.170)	0.007 (0.007)
Joint Venture	0.270 (0.174)	0.899 (0.550)	0.034** (0.016)
Borrower-Originator Relationship	0.042 (0.030)	0.081 (0.064)	0.003 (0.004)
<i>Lender Controls</i>			
CMBS Indicator	-0.083 (0.054)	-0.015 (0.174)	0.001 (0.007)
Loan Held by Government Lender	-0.299*** (0.109)	-0.309 (0.217)	-0.012** (0.005)
<i>Loan Controls</i>			
Interest Rate	5.275 (4.664)	14.299 (11.830)	0.506 (0.322)
Refinance Indicator	-0.142 (0.131)	-0.365 (0.313)	-0.012 (0.009)
Fixed-Rate Indicator	-0.185** (0.072)	0.107 (0.272)	-0.013** (0.006)
Time to Maturity	-0.012*** (0.004)	0.015 (0.018)	-0.002*** (0.000)
Debt-Service Coverage Ratio	-0.029 (0.024)	-0.006 (0.113)	-0.000 (0.003)
FE	Zip-Year Issue Year	Zip-Year Issue Year	Zip-Year Issue Year
S.E. Cluster	City	City	City
Adjusted $R^2$	0.151	0.140	0.201
Obs	77,640	77,640	77,640

**Table 4:** Relationship between LTV Ratios 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 1 if building  $i$  incurs a code violation requiring repairs in year  $t$ .  $LTVratio_{it-1}$  is the LTV ratio for building  $i$  in year  $t - 1$ .  $X_{it-1}$  are control variables for building  $i$  as of year  $t - 1$ .  $\gamma_{zt}, \kappa_v$  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, interest rates and debt-service coverage ratios 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	0.022*** (0.007)	0.156** (0.058)	0.002 (0.002)
<i>Building Controls</i>			
Transaction Price	0.001 (0.002)	-0.008** (0.004)	-0.000 (0.000)
Building Age	0.002*** (0.001)	0.012*** (0.003)	0.000*** (0.000)
Mid/High Rise Indicator	0.268* (0.149)	0.339 (0.433)	0.035*** (0.010)
Number of Units in Building	-0.000 (0.000)	-0.002 (0.001)	0.000 (0.000)
<i>Building Owner Controls</i>			
Public Owner	-0.040 (0.053)	-0.005 (0.115)	-0.001 (0.013)
Institutional Owner	-0.024 (0.017)	0.160* (0.085)	-0.001 (0.005)
Joint Venture	0.194 (0.122)	0.626* (0.363)	0.027* (0.015)
Borrower-Originator Relationship	0.038* (0.019)	0.059 (0.042)	0.008*** (0.001)
<i>Lender Controls</i>			
CMBS Indicator	-0.037 (0.034)	0.020 (0.140)	0.001 (0.005)
Loan Held by Government Lender	-0.130* (0.067)	-0.076 (0.114)	-0.004 (0.004)
<i>Loan Controls</i>			
Interest Rate	2.905 (3.184)	8.129 (7.284)	0.212 (0.241)
Refinance Indicator	-0.110 (0.087)	-0.318 (0.225)	-0.012 (0.009)
Fixed-Rate Indicator	-0.108** (0.053)	0.079 (0.173)	-0.008 (0.005)
Time to Maturity	-0.007*** (0.002)	0.013 (0.010)	-0.001*** (0.000)
Debt-Service Coverage Ratio	-0.027*** (0.009)	-0.042 (0.052)	-0.001 (0.001)
FE	Zip-Year Issue Year	Zip-Year Issue Year	Zip-Year Issue Year
S.E. Cluster	City	City	City
Adjusted $R^2$	0.154	0.129	0.142
Obs	68,743	68,743	68,743

**Table 5:** 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.  $\gamma_i$ ,  $\kappa_{pt}$  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 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 (1)	Violations/ 100 units (2)	Has Violation (3)
<i>Panel A – All Violations</i>			
$Treat_i \times After_t$	3.337*** (0.697)	6.705*** (2.115)	0.049 (0.031)
Adjusted $R^2$	0.478	0.443	0.605
Obs	5,436	5,436	5,436
<i>Panel B – Repair Violations</i>			
$Treat_i \times After_t$	2.238*** (0.430)	4.854*** (1.301)	0.068** (0.032)
Adjusted $R^2$	0.471	0.442	0.572
Obs	5,436	5,436	5,436
FE	Building Pair-Year	Building Pair-Year	Building Pair-Year
S.E Cluster	Building	Building	Building

**Table 6:** 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.  $\gamma_i, \kappa_{pt}$  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 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.

Variable	# Violations (1)	Violations/ 100 units (2)	Has Violation (3)	# Violations (4)	Violations/ 100 units (5)	Has Violation (6)
<i>Panel A – Bottom LTV Tercile, N=1,638</i>						
$Treat_i \times After_t$	1.919 (1.403)	1.325 (4.261)	0.048 (0.050)	1.445* (0.808)	1.925 (2.448)	0.053 (0.058)
Adjusted $R^2$	0.477	0.442	0.579	0.458	0.431	0.545
<i>Panel B – Mid LTV Tercile, N=1,386</i>						
$Treat_i \times After_t$	2.499*** (0.917)	5.074** (2.150)	0.056 (0.044)	1.524*** (0.553)	3.442** (1.323)	0.077* (0.040)
Adjusted $R^2$	0.427	0.409	0.597	0.420	0.434	0.610
<i>Panel C – Top LTV Tercile, N=1,566</i>						
$Treat_i \times After_t$	5.560*** (1.038)	13.210*** (2.998)	0.051 (0.061)	3.636*** (0.706)	8.622*** (2.068)	0.073 (0.057)
Adjusted $R^2$	0.502	0.451	0.606	0.505	0.450	0.556
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 7:** Triple-Difference – Impact of Rent Act on Violations for Top LTV Tercile Buildings Relative to Other 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.  $\gamma_i, \kappa_{pt}$  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. 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)
<i>Panel A – All Violations</i>			
$Treat_i \times After_t$	2.267** (0.893)	3.345 (2.649)	0.046 (0.033)
$HighLTV_i \times After_t$	-2.390 (1.708)	-8.591 (5.702)	-0.011 (0.096)
$HighLTV_i \times Treat_i \times After_t$	3.190** (1.404)	9.991** (4.128)	0.007 (0.071)
Adjusted $R^2$	0.482	0.449	0.604
Obs.	5,436	5,436	5,436
<i>Panel B – Repair Violations</i>			
$Treat_i \times After_t$	1.545*** (0.511)	2.837* (1.517)	0.061 (0.037)
$HighLTV_i \times After_t$	-1.829 (1.167)	-6.009 (3.787)	0.005 (0.095)
$HighLTV_i \times Treat_i \times After_t$	2.057** (0.892)	5.971** (2.637)	0.020 (0.070)
Adjusted $R^2$	0.475	0.447	0.571
Obs.	5,436	5,436	5,436
FE	Building	Building	Building
	Pair-Year	Pair-Year	Pair-Year
S.E Cluster	Building	Building	Building

**Table 8:** 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.  $\gamma_i, \kappa_{pt}$  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 buildings with 10-60 units, Panel B displays results using buildings with 15-55 units, Panel C displays results using 20-50 units and Panel D displays results using 25-45 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 (1)	Violations/ 100 units (2)	Has Violation (3)
<i>Panel A – 10-60 units, N=3,780</i>			
$Treat_i \times After_t$	3.162*** (0.648)	6.918*** (1.796)	0.051* (0.030)
Adjusted $R^2$	0.442	0.415	0.591
<i>Panel B – 15-55 units, N=2,894</i>			
$Treat_i \times After_t$	3.338*** (0.800)	7.088*** (2.168)	0.068** (0.034)
Adjusted $R^2$	0.418	0.399	0.575
<i>Panel C – 20-50 units, N=1,890</i>			
$Treat_i \times After_t$	3.067*** (0.876)	7.095*** (2.494)	0.064 (0.043)
Adjusted $R^2$	0.432	0.413	0.565
<i>Panel D – 25-45 units, N=612</i>			
$Treat_i \times After_t$	2.878** (1.301)	6.717* (3.417)	0.175*** (0.053)
Adjusted $R^2$	0.354	0.343	0.602
FE	Building Pair-Year	Building Pair-Year	Building Pair-Year
S.E. Cluster	Building	Building	Building

**Table 9:** Change in Code Violations After Rent Act of 2011 by Size Bin.

This table displays results from the following regression:

$$Violations_{it} = \beta_1[\mathbb{1}(35 < Units \leq 45)] \times After_t + \beta_2[\mathbb{1}(45 < Units \leq 55)] \times After_t + \beta_3[\mathbb{1}(55 < Units \leq 65)] \times After_t + \beta_4[\mathbb{1}(65 < Units \leq 75)] \times After_t + \gamma_i + \kappa_{pt} + \epsilon_{it}, \quad (6)$$

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.  $\gamma_i, \kappa_{pt}$  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 price 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. 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 (1)	Violations/ 100 units (2)	Has Violation (3)
$\mathbb{1}(35 < Units \leq 45)$	3.398*** (0.898)	8.171*** (2.476)	0.069 (0.046)
$\mathbb{1}(45 < Units \leq 55)$	3.381*** (1.080)	6.326** (2.940)	0.061* (0.036)
$\mathbb{1}(55 < Units \leq 65)$	2.455*** (0.893)	5.823** (2.531)	0.001 (0.036)
$\mathbb{1}(65 < Units \leq 75)$	1.513 (2.220)	1.272 (7.041)	-0.040 (0.077)
FE	Building Pair-Year	Building Pair-Year	Building Pair-Year
S.E. Cluster	Building	Building	Building
Adjusted $R^2$	0.458	0.423	0.596
Obs	4,554	4,554	4,554

**Table 10:** 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.  $\gamma_i, \kappa_{pt}$  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 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 (1)	Violations/ 100 units (2)	Has Violation (3)
<i>Panel A – All Violations</i>			
$Treat_i \times After_t$	4.887*** (1.126)	11.550*** (2.990)	0.044 (0.036)
Adjusted $R^2$	0.556	0.475	0.677
Obs	1,134	1,134	1,134
<i>Panel B – Repair Violations</i>			
$Treat_i \times After_t$	2.857*** (0.721)	6.947*** (1.987)	0.053 (0.038)
Adjusted $R^2$	0.556	0.473	0.647
Obs	1,134	1,134	1,134
FE	Building Pair-Year	Building Pair-Year	Building Pair-Year
S.E Cluster	Building	Building	Building

**Table 11:** 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.  $\gamma_i, \kappa_{pt}$  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 the mortgage on a building in 2010 was a refinance and zip code level occupancy rates as of 2010 as covariates. 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 (1)	Violations/ 100 units (2)	Has Violation (3)
<i>Panel A: All Violations</i>			
$Treat_i \times After_t$	3.581*** (1.217)	7.894** (3.361)	0.027 (0.046)
Adjusted $R^2$	0.551	0.524	0.626
Obs	1,152	1,152	1,152
<i>Panel B: Repair Violations</i>			
$Treat_i \times After_t$	2.392*** (0.778)	5.578** (2.175)	0.070 (0.049)
Adjusted $R^2$	0.516	0.482	0.648
Obs	1,152	1,152	1,152
FE	Building Pair-Year	Building Pair-Year	Building Pair-Year
S.E. Cluster	Building	Building	Building

**Table 12:** 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.  $\gamma_i, \kappa_{pt}$  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 over the pre-period, most recent transaction price as of 2010, building age 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 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 (1)	Violations/ 100 units (2)	Has Violation (3)
<i>Panel A – All Violations</i>			
$Treat_i \times After_t$	1.068 (0.850)	0.936 (2.512)	-0.023 (0.021)
Adjusted $R^2$	0.442	0.388	0.677
Obs	2,070	2,070	2,070
<i>Panel B – Repair Violations</i>			
$Treat_i \times After_t$	0.792 (0.506)	1.351 (1.529)	0.009 (0.027)
Adjusted $R^2$	0.408	0.361	0.665
Obs	2,070	2,070	2,070
FE	Building Pair-Year	Building Pair-Year	Building Pair-Year
S.E Cluster	Building	Building	Building

## Internet Appendix

## Appendix A. Violations Indicating Repairs or Improvements Needed

I collect data on housing code violations for 45 cities.<sup>1</sup> 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 III. 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

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



department that handled the violation is included. If this is the case, I designate violations as relating to repairs as well as possible.

## Appendix B. Other Data and Sample Construction

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 on apartment buildings issued in transactions greater than or equal to \$2.5 million associated with both property sales and mortgage refinancing activity.<sup>2</sup> Data from RCA include building LTV ratios at loan-issuance, transaction prices, loan origination dates, loan interest rates, loan maturity dates, information on second mortgages, building locations, the number of units in a building, building ages, building owners and firm type.<sup>3</sup> The data also include lender and originator characteristics.<sup>4</sup> I drop all buildings labeled as co-ops or condos. I also drop observations where the number of units, zip code or address is missing.

Several other data sources are used for robustness checks. To estimate market LTV ratios for use in robustness checks, I merge the data set with zip-code level house prices from the Zillow Home Values Index (ZHVI). I deflate the LTV ratios from RCA by local changes in house prices since issuance of the mortgage to calculate an imputed LTV ratio:

$$\begin{aligned} ImpLTVratio_{house,t} &= BookLTVratio_{house,issue} * \frac{HomeValues_{zip,issue}}{HomeValues_{zip,t}} \\ &= \frac{Debt_{house,issue}}{HomeValue_{house,issue}} * \frac{HomeValues_{zip,issue}}{HomeValues_{zip,t}}. \end{aligned} \quad (B1)$$

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 using data posted publicly at <https://github.com/clhenrick/dhcr-rent-stabilized-data> that was obtained in a FOIL request.

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<sup>2</sup>To alleviate concerns that this may introduce selection bias, a version of the difference-in-differences analysis using a wider population of New York City buildings is included in the Internet Appendix.

<sup>3</sup>I include in my tests indicator variables equal to 1 if a building is owned by a public company or an institutional investor. If the firm type field is empty, I assume the building was owned by neither a public firm nor an institutional investor, but results are similar if I instead leave these variables empty in those cases.

<sup>4</sup>I refer to the financial institution holding the loan as an asset on their balance sheet as the lender, and the financial institution that actually makes the loan as the originator.

## Appendix C. Drivers of Apartment Leverage

In this section, I examine what drives the apartment leverage 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.

Figure 3 shows that buildings in zip codes with high capitalization rates tend to be more highly leveraged, 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 leverage, I run regressions of apartment LTV ratios at-issuance on hypothesized drivers of leverage:

$$LTVratio_{it} = \beta_1 X_{1,it} + \beta_2 X_{2,it} + \beta_3 X_{3,it} + \beta_4 X_{4,it} + FE + \epsilon_{it}, \quad (C1)$$

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 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 populated for most observations in the sample.  $X_{2,it}$  includes the zip code level capitalization rate, the zip code level occupancy rate and as the 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, the mortgage interest rate and the mortgage debt-service coverage ratio. The fixed effects vary by specification and are specified in the table.

Table IA.13 displays cross-sectional regression results. Column (1) displays results using no fixed effects. As previously noted, older buildings have higher LTV ratios. Larger buildings 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 3b. Similarly, buildings in zip codes with higher home values tend to have lower mortgages. This

provides further evidence that building owners choose lower levels of debt for more promising buildings located in more attractive markets. 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. Owner characteristics are displayed below, where there is no statistically significant relationship between owner characteristics and apartment LTV ratios.

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. However, there is now a positive correlation between property transaction values, indicating that when looking within zip code, buildings that are worth more tend to be more highly leveraged. Additionally, there is now a negative correlation between the mid or high-rise indicator and apartment LTV ratios, indicating that building owners tend to take on less debt for high-rises, perhaps because they tend to be more profitable. Moreover, it is also now apparent that buildings owned by public companies tend to have lower mortgages, which is intuitive since public companies have other sources of capital and may need less debt. 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 leverage, 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 no longer statistically significant. On the other hand, the institutional owner indicator is now statistically significant and positive, indicating that institutional owners tend to use more mortgage debt in financing their properties. Furthermore, the adjusted  $R^2$  of the regression increases from 0.559 in Column (2) to 0.702, 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.<sup>5</sup>

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<sup>5</sup>While there are some differences in the results in this column relative to others, this is largely due to

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. those cities referenced in Table 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 data on code violations are available.

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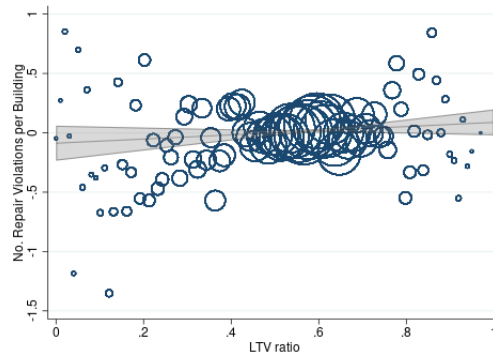
the significant decrease in the sample size when including the time since renovation variable.

## Appendix D. Additional Results

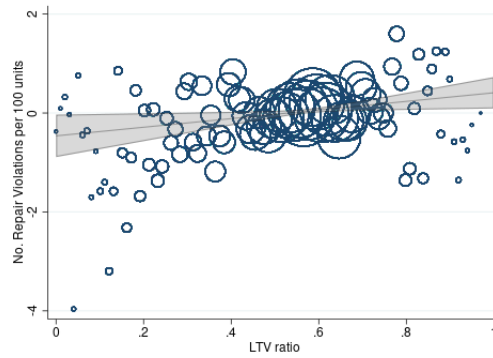
**Figure IA.1.** Correlation Between LTV Ratios and Code Violation Requiring a Repair

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.

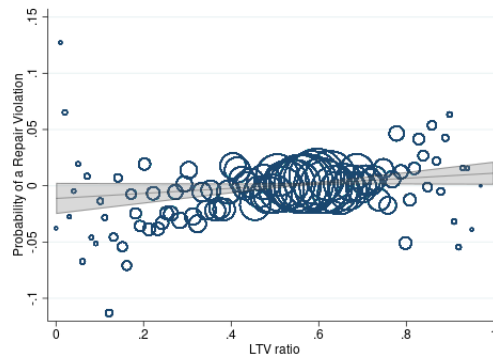
**(a)** Number of Repair Violations



**(b)** Repair Violations per 100 units

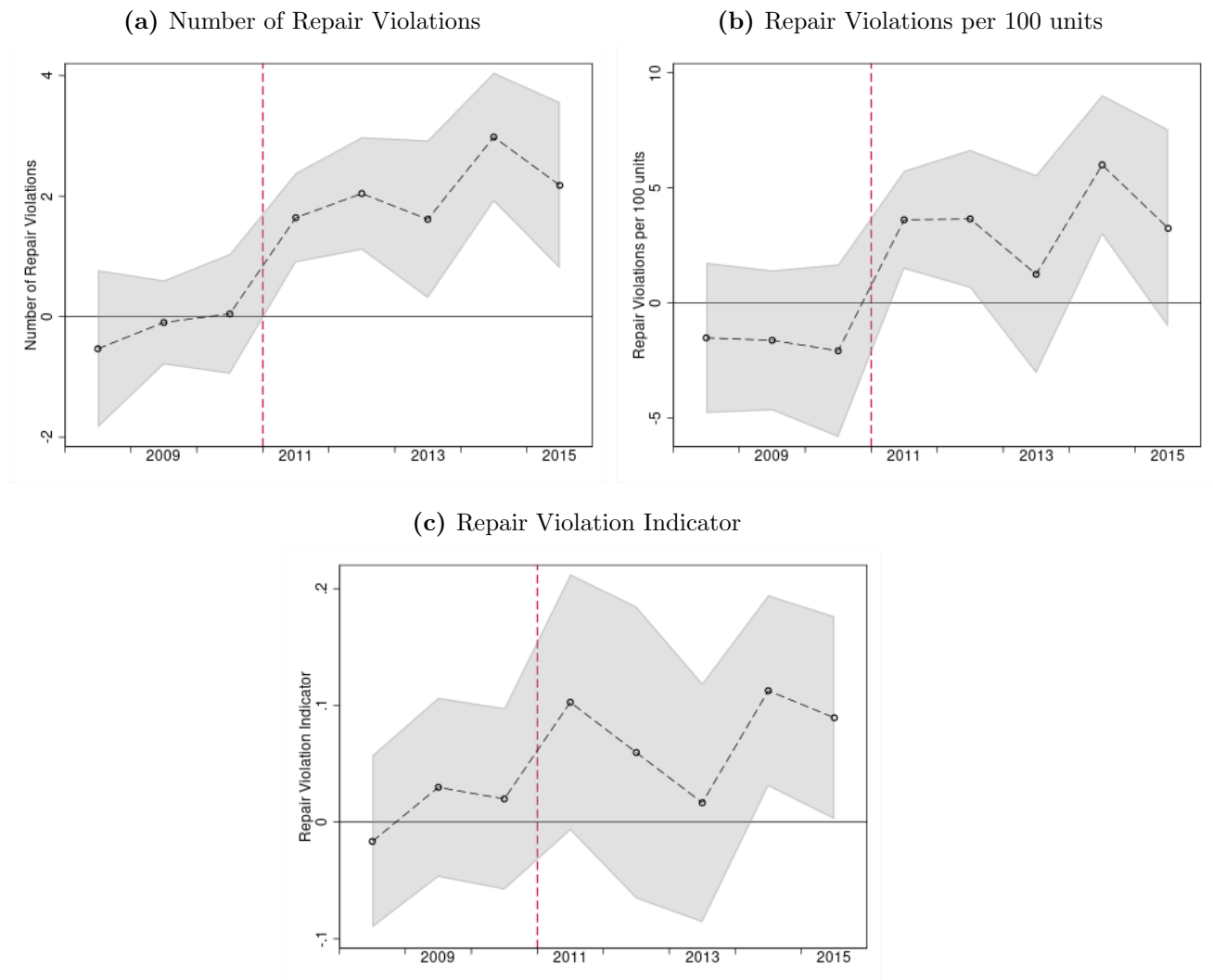


**(c)** Repair Violation Indicator



**Figure IA.2.** 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. 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.





**Table IA.1:** Relationship between LTV Ratios 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 for building  $i$  in year  $t - 1$ .  $X_{it-1}$  are control variables for building  $i$  as of year  $t - 1$ .  $\gamma_{zt}, \kappa_v$  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 3, 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, interest rates and debt-service coverage ratios 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)
<i>Panel A – All Violations</i>			
LTV Ratio	0.063*** (0.016)	0.312*** (0.112)	0.007** (0.003)
Adjusted $R^2$	0.150	0.139	0.201
Obs	77,640	77,640	77,640
<i>Panel B – Repair Violations</i>			
LTV Ratio	0.025*** (0.007)	0.172*** (0.061)	0.003 (0.002)
Adjusted $R^2$	0.154	0.129	0.141
Obs	68,743	68,743	68,743
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.2:** Relationship between LTV Ratios 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 for building  $i$  in year  $t - 1$ .  $X_{it-1}$  are control variables for building  $i$  as of year  $t - 1$ .  $\gamma_{zt}, \kappa_v$  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 3. Number of violations, number of violations per 100 units, LTV ratios, transaction prices, building age, number of units per building, interest rates and debt-service coverage ratios 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)
<i>Panel A – All Violations</i>			
LTV Ratio	0.089** (0.040)	0.428* (0.234)	0.016*** (0.005)
Adjusted $R^2$	0.210	0.222	0.390
Obs	77,640	77,640	77,640
<i>Panel B – Repair Violations</i>			
LTV Ratio	0.020*** (0.007)	0.092* (0.050)	0.004** (0.002)
Adjusted $R^2$	0.147	0.119	0.175
Obs	68,743	68,743	68,743
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.3:** Relationship between LTV Ratios and Code Violations – Drop Five 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 for building  $i$  in year  $t - 1$ .  $X_{it-1}$  are control variables for building  $i$  as of year  $t - 1$ .  $\gamma_{zt}, \kappa_v$  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, Chicago or Seattle are dropped from the sample. Regression controls are the same as in Table 3. Number of violations, number of violations per 100 units, LTV ratios, transaction prices, building age, number of units per building, interest rates and debt-service coverage ratios 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)
<i>Panel A – All Violations</i>			
LTV Ratio	0.083*** (0.030)	0.324** (0.144)	0.012*** (0.003)
Adjusted $R^2$	0.120	0.125	0.246
Obs	37,632	37,632	37,632
<i>Panel B – Repair Violations</i>			
LTV Ratio	0.022** (0.009)	0.114 (0.071)	0.003* (0.001)
Adjusted $R^2$	0.091	0.043	0.079
Obs	31,868	31,868	31,868
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.4:** Relationship Between Combined LTV Ratios and Code Violations.

This table displays results from the following regression:

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

$Violations_{it}$  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.  $CLTVratio_{it-1}$  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  $t - 1$ .  $\gamma_{zt}, \kappa_v$  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 3. Number of violations, number of violations per 100 units, combined 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 (1)	Violations/ 100 units (2)	Has Violation (3)
<i>Panel A – All Violations</i>			
Combined LTV Ratio	0.063*** (0.020)	0.242** (0.104)	0.006*** (0.002)
Adjusted $R^2$	0.151	0.140	0.201
Obs	77,640	77,640	77,640
<i>Panel B – Repair Violations</i>			
Combined LTV Ratio	0.024** (0.011)	0.126** (0.057)	0.002 (0.002)
Adjusted $R^2$	0.154	0.129	0.142
Obs	68,743	68,743	68,743
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.5:** Relationship Between Imputed 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 imputed LTV ratio for building  $i$  in year  $t - 1$ , where imputed LTV is calculated by deflating the LTV ratio at-issuance by changes in the local Zillow index (see Equation B1 for calculation).  $X_{it-1}$  are control variables for building  $i$  as of year  $t - 1$ .  $\gamma_{zt}, \kappa_v$  are zip-code-by-year and mortgage issue year fixed effects. Imputed LTV ratios are standardized by subtracting the mean and dividing by the standard deviation. Regression controls the same as in Table 3. Number of violations, number of violations per 100 units, imputed 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)
<u>Panel A – All Violations</u>			
Imputed LTV Ratio	0.073** (0.031)	0.253* (0.145)	0.010*** (0.002)
Adjusted $R^2$	0.133	0.119	0.205
Obs	68,137	68,137	68,137
<u>Panel B – Repair Violations</u>			
Imputed LTV Ratio	0.024** (0.011)	0.154 (0.092)	0.003*** (0.001)
Adjusted $R^2$	0.134	0.104	0.125
Obs	59,677	59,677	59,677
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.6:** 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 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. 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	3.685	8.117	2.083	5.600	1.602***
Violations per 100 units	6.944	16.490	8.215	21.779	-1.271
Violation Indicator	0.341	0.475	0.182	0.387	0.159***
Repair Number of Violations	2.291	5.141	1.550	4.182	0.741*
Repair Violations per 100 units	4.299	10.409	5.925	16.129	-1.626
Repair Violation Indicator	0.291	0.455	0.169	0.375	0.122***
LTV Ratio	0.568	0.205	0.568	0.197	0.000
Building Age	85.093	7.307	85.821	7.079	-0.728
Transaction Price	5.455	4.664	4.445	4.503	1.000***
Mid/High Rise Indicator	0.990	0.099	0.960	0.196	0.030**
Refinance Indicator	0.103	0.304	0.103	0.304	0.000
Institutional Owner	0.053	0.224	0.053	0.224	0.000
Occupancy Rate	0.975	0.019	0.974	0.014	0.001
$N_{control} = N_{treated} = 302$					

**Table IA.7:** 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)
$Treat_i \times After_t$	-2,740.281*** (367.147)	-2,806.037*** (369.502)	-2,889.245*** (362.390)	-933.188** (436.654)
$Treat_i$	-49,610.862*** (834.606)	-26,089.911*** (908.298)		
$After_t$	8,255.756*** (290.839)			
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
Obs	39,256	39,256	39,216	39,206

**Table IA.8:** 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.  $\gamma_i, \kappa_{pt}$  are building and matched-pair-by-year fixed effects. 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 effective ages (defined as the time since a building’s most recent renovation when available and the building’s age otherwise) 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. 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 (1)	Violations/ 100 units (2)	Has Violation (3)
<i>Panel A – All Violations</i>			
$Treat_i \times After_t$	2.436*** (0.739)	3.439 (2.307)	0.009 (0.027)
Adjusted $R^2$	0.482	0.439	0.622
Obs	5,850	5,850	5,850
<i>Panel B – Repair Violations</i>			
$Treat_i \times After_t$	1.747*** (0.427)	3.206** (1.331)	0.033 (0.029)
Adjusted $R^2$	0.464	0.438	0.594
Obs	5,850	5,850	5,850
FE	Building Pair-Year	Building Pair-Year	Building Pair-Year
S.E Cluster	Building	Building	Building



**Table IA.9:** 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.  $\gamma_i, \kappa_t$  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 (1)	Violations/ 100 units (2)	Has Violation (3)
<i>Panel A – All Violations</i>			
$Treat_i \times After_t$	4.743*** (0.147)	0.955* (0.524)	0.074*** (0.005)
Adjusted $R^2$	0.411	0.305	0.391
Obs	193,860	193,860	189,189
<i>Panel B – Repair Violations</i>			
$Treat_i \times After_t$	2.932*** (0.091)	1.861*** (0.325)	0.120*** (0.005)
Adjusted $R^2$	0.398	0.288	0.373
Obs	193,860	193,860	189,189
FE	Building Year	Building Year	Building Year
S.E Cluster	Building	Building	Building

**Table IA.10:** 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.  $\gamma_i, \kappa_{pt}$  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 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.

Variable	# Violations	Violations/ 100 units	Has Violation	# Violations	Violations/ 100 units	Has Violation
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: 2007-2014, N=4,944</i>						
$Treat_i \times After_t$	3.262*** (0.716)	6.821*** (2.192)	0.058* (0.031)	2.245*** (0.452)	4.991*** (1.390)	0.066** (0.032)
Adjusted $R^2$	0.447	0.402	0.595	0.440	0.404	0.543
<i>Panel B: 2006-2015, N=4,540</i>						
$Treat_i \times After_t$	2.991*** (0.925)	5.405* (2.998)	0.082** (0.038)	2.021*** (0.552)	3.925** (1.772)	0.102*** (0.039)
Adjusted $R^2$	0.481	0.485	0.602	0.464	0.465	0.566
<i>Panel C: 2006-2016, N=4,730</i>						
$Treat_i \times After_t$	2.908*** (0.912)	5.180* (2.888)	0.078** (0.034)	2.016*** (0.527)	3.931** (1.683)	0.098** (0.040)
Adjusted $R^2$	0.489	0.497	0.626	0.479	0.480	0.601
<i>Panel D: 2007-2016, N=5,700</i>						
$Treat_i \times After_t$	2.825*** (0.755)	5.059** (2.289)	0.031 (0.029)	1.927*** (0.452)	3.950*** (1.384)	0.056 (0.035)
Adjusted $R^2$	0.520	0.502	0.636	0.511	0.498	0.614
<i>Panel E: 2009-2012, N=3,456</i>						
$Treat_i \times After_t$	2.333*** (0.627)	6.066*** (2.119)	0.023 (0.034)	1.628*** (0.440)	4.564*** (1.617)	0.058* (0.035)
Adjusted $R^2$	0.395	0.327	0.584	0.376	0.312	0.535
FE	Building	Building	Building	Building	Building	Building
Pair-Year	Pair-Year	Pair-Year	Pair-Year	Pair-Year	Pair-Year	Pair-Year
S.E. Cluster	Building	Building	Building	Building	Building	Building

**Table IA.11:** Change in Code Violations After Rent Act of 2011 – No Matching.

This table displays results from the following regression:

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

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.  $\gamma_i$  are building fixed effects.  $\kappa_t$  are year fixed effects. Panel A displays results with all violations and Panel B displays results using only 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 (1)	Violations/ 100 units (2)	Has Violation (3)
<i>Panel A – All Violations</i>			
$Treat_i \times After_t$	2.788*** (0.422)	4.360*** (1.132)	0.048*** (0.015)
Adjusted $R^2$	0.506	0.441	0.651
Obs	7,785	7,785	7,785
<i>Panel B – Repair Violations</i>			
$Treat_i \times After_t$	1.715*** (0.260)	2.840*** (0.700)	0.068*** (0.016)
Adjusted $R^2$	0.490	0.433	0.616
Obs	7,785	7,785	7,785
FE	Building Year	Building Year	Building Year
SE	Building	Building	Building

**Table IA.12:** 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.  $\gamma_i, \kappa_{pt}$  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 (1)	Violations/ 100 units (2)	Has Violation (3)
<i>Panel A – All Violations</i>			
$Treat_i \times After_t$	3.337*** (0.633)	6.705*** (1.839)	0.049 (0.031)
Adjusted $R^2$	0.478	0.443	0.605
Obs	5,436	5,436	5,436
<i>Panel B – Repair Violations</i>			
$Treat_i \times After_t$	2.238*** (0.381)	4.854*** (1.097)	0.068** (0.031)
Adjusted $R^2$	0.471	0.442	0.572
Obs	5,436	5,436	5,436
FE	Building Pair-Year	Building Pair-Year	Building Pair-Year
S.E Cluster	Building	Building	Building

**Table IA.13:** 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 debt-service coverage ratios 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)
<i>Building Characteristics</i>								
Building Age	0.003*** (0.001)	0.004*** (0.001)	0.004** (0.002)	0.007*** (0.002)	0.002* (0.001)	0.003 (0.003)	0.002 (0.003)	0.007*** (0.003)
Number of Units in Building	0.005*** (0.001)	0.000 (0.001)	0.001 (0.002)	0.005** (0.003)	0.005 (0.003)	-0.003 (0.005)	-0.003 (0.006)	0.004 (0.005)
Mid/High Rise Indicator	-0.003 (0.002)	-0.007*** (0.002)	-0.005** (0.002)	-0.011*** (0.002)	-0.007 (0.004)	-0.004 (0.004)	-0.004 (0.004)	-0.008** (0.003)
Transaction Price	0.001 (0.001)	0.003*** (0.001)	0.001 (0.002)	-0.003* (0.002)	0.000 (0.002)	0.005* (0.003)	0.003 (0.004)	-0.002 (0.003)
Time Since Renovation				-0.003*** (0.001)				-0.002** (0.001)
<i>Local Economic Characteristics</i>								
Zip Code Capitalization Rate	2.894*** (0.116)	2.575*** (0.106)			2.943*** (0.361)	2.546*** (0.281)		
Zip Code Occupancy Rate	0.074*** (0.020)	0.041*** (0.015)			0.129** (0.059)	0.060 (0.036)		
Zip Code Zillow Index	-0.024*** (0.002)	-0.014*** (0.002)			-0.021*** (0.003)	-0.011*** (0.003)		
<i>Owner Characteristics</i>								
Public Owner	-0.005 (0.004)	-0.009*** (0.003)	-0.006 (0.005)	-0.019** (0.009)	0.011 (0.020)	0.008 (0.018)	0.003 (0.021)	-0.024 (0.022)
Institutional Owner	0.002 (0.003)	0.004 (0.003)	0.007*** (0.003)	0.010** (0.004)	-0.002 (0.014)	0.000 (0.011)	0.006 (0.006)	0.010 (0.009)
<i>Loan Characteristics</i>								
Loan Held by Government Lender	-0.014*** (0.003)	0.004 (0.004)	0.002 (0.005)	0.023*** (0.009)	-0.011 (0.010)	0.004 (0.010)	0.000 (0.012)	0.024* (0.014)
Fixed-Rate Indicator	-0.014*** (0.003)	-0.013*** (0.003)	-0.024*** (0.003)	-0.016*** (0.003)	-0.005 (0.009)	-0.010 (0.007)	-0.018*** (0.006)	-0.026*** (0.005)
Refinance Indicator	-0.038*** (0.001)	-0.035*** (0.001)	-0.036*** (0.001)	-0.032*** (0.002)	-0.035*** (0.003)	-0.032*** (0.003)	-0.032*** (0.003)	-0.030*** (0.004)
Time to Maturity	-0.002*** (0.000)	-0.002*** (0.000)	-0.001*** (0.000)	-0.000 (0.000)	-0.002*** (0.000)	-0.002*** (0.000)	-0.001** (0.001)	-0.001 (0.000)
Interest Rate	-2.248*** (0.130)	-1.723*** (0.153)	-1.402*** (0.197)	-0.854*** (0.326)	-2.139*** (0.381)	-1.872*** (0.433)	-1.674*** (0.455)	-0.396 (0.689)
Debt-Service Coverage Ratio	-0.102*** (0.004)	-0.096*** (0.003)	-0.093*** (0.003)	-0.088*** (0.005)	-0.095*** (0.008)	-0.092*** (0.007)	-0.092*** (0.007)	-0.092*** (0.007)
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
Adjusted $R^2$	0.456	0.559	0.702	0.715	0.464	0.560	0.687	0.728
Obs	54,180	52,715	47,163	11,845	13,079	12,977	13,957	4,687