Does Rent Control Affect Housing Quality? Evidence from New York City

Beau Bressler
University of California, Davis

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Abstract

This paper studies whether strengthening rent control reduces housing quality, and through what margins. I study this question using New York's 2019 Housing Stability and Tenant Protection Act (HSTPA), which eliminated vacancy decontrol and sharply reduced landlords' ability to recover capital costs through rent increases. Using building-level panel data on housing code violations, tenant complaints, and building permits in a difference-in-differences design, I estimate the effect of HSTPA on maintenance and investment behavior. I find that housing quality declined after HSTPA: hazardous violations increased by 21%, tenant complaints increased by 8%, and violations took 23% longer to resolve. Building permit activity also declined, with the probability of any alteration permit falling by approximately 11%. These findings provide causal evidence that strengthening rent control reduces housing quality through both reduced routine maintenance and reduced capital investment.

JEL Codes: R31, R38, L85

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1 Introduction

Housing affordability has become a central policy challenge of the 21st century (Baum-Snow and Duranton, 2025). One of the most common policy responses is rent control. Economists have long warned that rent control reduces incentives for housing investment and maintenance. The standard theoretical critique, dating to Friedman and Stigler (1946), holds that capping rents below market levels reduces landlord returns and therefore investment incentives. If landlords cannot recover the costs of maintenance and improvements through higher rents, they may reduce upkeep, causing housing to deteriorate. This prediction has been central to the economics profession's longstanding skepticism of rent control.

Despite decades of research on rent control, credible causal evidence on how rent control affects housing quality remains limited. Most existing studies focus on supply-side responses—such as condominium conversions, demolitions, and exit from the rental market—while evidence on the condition of buildings that remain in the rental stock relies largely on cross-sectional comparisons, coarse quality measures, or limited sample sizes. As a result, it remains unclear whether rent regulation primarily induces landlords to exit or instead leads to gradual deterioration among continuing rental properties.

This paper provides causal evidence on this question by exploiting New York's 2019 Housing Stability and Tenant Protection Act (HSTPA), which substantially tightened the city's rent stabilization system. HSTPA made several major changes that dramatically reduced landlords' ability to recover costs and eliminated their option to exit regulation. The law eliminated high-rent vacancy decontrol, which had allowed units to exit stabilization when rents exceeded approximately \$2,774 per month; capped vacancy bonuses at 0–5%, down from approximately 20%; reduced Major Capital Improvement (MCI) rent increases from 6% to 2% of rent annually and made them temporary (30 years) rather than permanent; and capped Individual Apartment Improvement (IAI) increases at \$15,000 over 15 years.[1] The cumulative effect was to substantially reduce the present discounted value of future rent streams in stabilized buildings, creating strong theoretical predictions for reduced investment and maintenance.

Exploiting rich building-level panel data on serious housing code violations, tenant complaints, building permits, and rent-stabilization rates, I study the effect of the HSTPA on housing quality, as experienced by tenants, and investment. My empirical strategy is a difference-in-differences event study design which compares the evolution of outcomes in rent-stabilized to non-stabilized buildings (or alternatively, buildings with high vs. lower shares of

[1] These provisions took effect immediately upon the law's passage on June 14, 2019, though some administrative details were clarified in subsequent DHCR guidance.

rent-stabilized units) before and after HSTPA, while controlling for time-invariant unobservable building characteristics as well as a variety of location and building characteristic-related trends.

I find that HSTPA led to a variety of negative effects on housing quality. First, I find that housing code violations increased. Class C (hazardous) violations—the most serious category, representing immediately hazardous conditions such as lead paint, inadequate heat, pest infestations, and structural defects—increased by 0.21 violations per building per 6-month period following HSTPA. Relative to the pre-HSTPA baseline of 0.96 Class C violations per 6-month period for stabilized buildings, this represents a 21% increase. Furthermore, rent-impairing violations, those which can trigger rent abatement, increased by 14% relative to baseline.[2] I also find that housing violations take longer to resolve. The time to close housing violations increased by 23 days following HSTPA, a 23% increase relative to the baseline of 100 days. This suggests that landlords are slower to address building problems even after violations are issued, consistent with reduced responsiveness to code enforcement.

One might worry that violations are a function of inspections as well as actual conditions. However, I find that HSTPA also led to increases in tenant complaints in rent-stabilized buildings. HPD complaints filed by tenants increased by 0.25 per building per 6-month period, representing an 8% increase relative to the baseline of 3.3 complaints per period. Emergency complaints increased by 7%.

I also find that building permit activity declined. The probability of any alteration permit fell by approximately 11% relative to baseline in rent-stabilized buildings. Because 89% of building-periods have zero permits, I focus on the extensive margin (any permit) rather than counts. The combination of increased violations and complaints with reduced permit activity suggests that HSTPA reduced both routine maintenance and capital investment. Routine maintenance—painting, minor repairs, pest control, prompt response to tenant complaints, and general upkeep—is highly discretionary, does not require permits, and can be adjusted quickly in response to changing incentives. The permit decline indicates that major capital projects also responded, though violations and complaints show larger percentage effects than permits.

These findings contribute to the rent control literature in several ways. First, I provide direct causal evidence on housing quality effects, complementing the existing literature's focus on supply responses. While Diamond, McQuade, and Qian (2019) find that San Francisco's 1994 rent control expansion reduced rental housing supply by 15% through conversions to

[2] Barr (2025) in a blog post documents a similar pattern in Class C violations; see `https://buildingtheskyline.org/hstpa-maintanence/`. On the other hand, NYU Furman Center (2021) argued that HSTPA did not increase violations immediately after its passage; see `https://furmancenter.org/files/Rent_Reform_7_1_A_remediated.pdf`.

condos and redevelopment, and Autor, Palmer, and Pathak (2014) show that Cambridge's 1995 decontrol led to substantial property investment, neither study directly measures the quality of buildings that remain as rental housing. By examining housing code violations, tenant complaints, and resolution times, I capture a dimension of landlord response that is distinct from—and potentially more immediate than—supply adjustments.

Second, I show that landlords reduced both routine maintenance, as reflected by violations and complaints, and capital investment, as reflected by permits. This finding has implications for both theory and policy. Theoretical models that treat "investment" as a single dimension may miss the heterogeneous responses across different types of building work. Policy aimed at maintaining quality in rent-stabilized buildings may need to address both dimensions: enhanced code enforcement for routine maintenance alongside incentives for capital improvements.

The rest of the paper proceeds as follows. Section 2 describes HSTPA and the policy debate surrounding its passage. Section 3 reviews the economics literature on rent control and housing quality, positioning the current study within this literature and clarifying the theoretical predictions. Section 4 describes the data sources and sample construction. Section 5 presents the empirical strategy, including the event study design, fixed effects structure, and identifying assumptions. Section 6 reports the main results on violations, complaints, permits, and major capital improvements, along with robustness checks. Section 7 concludes with a summary of findings and policy implications.

2 Policy Background

2.1 Rent Stabilization in New York City

New York City's rent stabilization system covers approximately one million apartments, primarily in buildings with six or more units built before 1974.[3] Unlike strict rent control—which freezes rents at a fixed level—rent stabilization allows annual rent increases set by the Rent Guidelines Board, typically 1–3% per year for lease renewals. This "second-generation" design, in the terminology of Arnott (1995), was intended to allow landlords to recover operating cost increases while limiting rent growth.

Prior to HSTPA, landlords could also increase rents through several additional mechanisms:

[3] Exact counts vary depending on data source and year. The NYC Rent Guidelines Board estimated 966,000 stabilized units in 2018; see `https://rentguidelinesboard.cityofnewyork.us/resources/rent-stabilized-building-lists/`.

- **Vacancy bonuses:** Upon tenant turnover, landlords could increase rents by approximately 20%, reflecting the higher costs associated with unit turnover and the opportunity to reset rents closer to market levels.
- **Major Capital Improvements (MCI):** Landlords who made building-wide improvements—such as new boilers, roofs, elevators, or windows—could recover the cost through permanent rent increases of up to 6% of the legal rent annually, spread across all units in the building.
- **Individual Apartment Improvements (IAI):** Landlords who renovated individual units could increase the legal rent by 1/40th of the improvement cost (effectively a 2.5-year payback period) for units with rent above $2,500, or 1/60th for units below that threshold.
- **High-rent vacancy decontrol:** Units could exit stabilization entirely when the legal rent exceeded approximately $2,774 per month upon vacancy, or when rent exceeded $2,774 and household income exceeded $200,000 for two consecutive years.

These provisions gave landlords multiple avenues to increase rents and, in the case of vacancy decontrol, to eventually exit the regulatory system entirely. The combination created a dynamic system in which landlords had incentives to invest in improvements (recoverable through MCI and IAI increases), to encourage tenant turnover (capturing vacancy bonuses and potentially decontrolling units), and to maintain buildings in marketable condition (to attract higher-income tenants who might trigger decontrol).

2.2 The Housing Stability and Tenant Protection Act (2019)

On June 11, 2019, the New York State Legislature passed the Housing Stability and Tenant Protection Act; Governor Cuomo signed it three days later. The law made the following major changes to rent stabilization:

1. **Eliminated high-rent vacancy decontrol:** Units can no longer exit stabilization regardless of rent level or tenant income. This change effectively closes the main exit option that landlords had previously used to transition buildings to market-rate status.
2. **Capped vacancy bonuses:** Reduced from approximately 20% to a maximum of 0–5%, depending on the time since the last vacancy increase. For units that had not received a vacancy increase in eight or more years, a 5% bonus is permitted; otherwise, no vacancy bonus is allowed.

3. **Reformed MCI increases:** Reduced annual rent increases from 6% to 2% of the legal rent, and made increases temporary (amortized over 30 years) rather than permanent. This change reduces the present value of MCI cost recovery by approximately 50–70%, depending on discount rates and assumptions about future rent growth. Additionally, MCIs are now prohibited for buildings where 35% or fewer of units are rent-stabilized.

4. **Capped IAI increases:** Limited to $15,000 over any 15-year period, with increases made temporary rather than permanent. Previously, there was no cap on total IAI spending, and increases were permanent additions to the legal rent.

5. **Eliminated preferential rent resets:** Landlords can no longer raise preferential rents (voluntarily reduced rents below the legal maximum) to the legal maximum upon vacancy. This change reduces the incentive for landlords to offer preferential rents and may affect tenant retention decisions.

6. **Restricted condo conversions:** Increased the tenant consent threshold for converting rental buildings to condominiums from 15% to 51%, making conversions substantially more difficult.

7. **Limited owner reclaim:** Restricted the number of units an owner can reclaim for personal use to one per building, reducing a pathway landlords had used to remove stabilized units from the rental market.

Taken together, these provisions tightened the rent control system, limited landlords' ability to exit rent stabilization, raise rents in response to capital investment, or convert rental units towards owner-occupation. This paper asks whether these changes led landlords to reduce maintenance and investment in their rental properties.

3 Literature Review

3.1 Theoretical Framework

The standard economic critique of rent control argues that below-market rents reduce landlord returns and therefore weaken investment incentives (e.g. Friedman and Stigler (1946)). When rents are capped, landlords earn less from their buildings, reducing the owner's willingness to incur costs that would maintain or improve the property. Landlords may respond by reducing maintenance and capital improvements, allowing buildings to deteriorate, converting rental units to condos or co-ops, substantially renovating buildings to exit rent regulation, letting buildings deteriorate to the point of abandonment, or reducing services provided to tenants.

Rent control provides clear benefits to tenants through lower rents and greater tenure security. The policy question is whether these protections come at a cost to housing quality. Arnott (1995) argues that "soft" or second-generation rent controls can avoid the worst efficiency costs of hard price freezes. These systems typically allow annual rent increases, provide mechanisms for landlords to recover capital costs, and permit decontrol under certain conditions. By preserving landlord incentives to maintain and invest, soft rent control can theoretically achieve distributional goals without severe quality deterioration. Arnott and Shevyakhova (2014) emphasize that under tenancy rent control, landlords face a commitment problem in maintenance. Because rents are regulated within a tenancy, the present value of rental revenue after lease signing is largely predetermined by the initial rent and regulated rent path. Once a tenant is in place, maintenance expenditures affect future resale or re-letting value but have little effect on contemporaneous rental income. As a result, landlords lack a credible mechanism to commit to maintenance effort, and maintenance is systematically under-provided.

New York's rent stabilization system exemplifies the "soft" design Arnott (1995) describes, with annual increases set by the Rent Guidelines Board, Major Capital Improvement (MCI) cost recovery, and (prior to 2019) vacancy decontrol at high rent levels. The 2019 Housing Stability and Tenant Protection Act substantially tightened New York's rent stabilization system, eliminating vacancy decontrol and limiting landlords' ability to reset rents at tenant turnover. In Arnott and Shevyakhova's framework, these changes weaken landlords' ability to recoup maintenance expenditures through future rents, strengthening the prediction of reduced upkeep in stabilized buildings.

The theoretical prediction for housing quality depends on how sensitive landlord investment is to the marginal return on maintenance spending. If landlords maintain buildings primarily to avoid code violations, maintain tenant relationships, or preserve building value for eventual sale, then reduced rent-recovery provisions may have limited effects on quality. If instead maintenance is tied to the ability to recoup costs through higher rents, then HSTPA should reduce maintenance effort and lead to measurable declines in upkeep.

3.2 Empirical Evidence

The modern empirical literature on rent control has focused primarily on supply effects.[4] Diamond, McQuade, and Qian (2019) study San Francisco's 1994 rent control expansion,

[4]For a comprehensive review of the rent control literature, see Kholodilin (2024).

which extended rent stabilization to small multifamily buildings built before 1980. Using the age discontinuity in coverage as a natural experiment, they find that rent control reduced the rental housing supply by 15% over the subsequent two decades, primarily through conversions to condominiums, demolitions, and redevelopment into new buildings exempt from rent control. Autor, Palmer, and Pathak (2014) study the converse policy change: the end of rent control in Cambridge, Massachusetts, in 1995. They find substantial increases in residential investment following decontrol, with renovations concentrated in previously controlled units. These studies establish that landlords respond to rent control largely through the extensive margin—exiting the rental market—rather than through continued operation at reduced returns. However, they do not directly address what happens to buildings that remain as rentals.

Direct evidence on rent control's effect on housing quality is more limited and faces methodological weaknesses. Using Housing and Vacancy Survey data, Gyourko and Linneman (1989) document that rent-controlled and stabilized units in New York City exhibit worse physical conditions. However, their analysis relies on cross-sectional comparisons and cannot isolate the causal effect of regulation on maintenance. Using Housing and Vacancy Survey panel data, Moon and Stotsky (1993) estimate a structural model of housing quality transitions in New York City and find that greater rent suppression is associated with faster deterioration. Early (2000) uses American Housing Survey data to examine the relationship between rent control and housing quality, finding that rent-controlled units have lower maintenance expenditures and worse self-reported quality than comparable uncontrolled units. However, this study relies on cross-sectional comparisons and may be confounded by selection. Sims (2007) examines the end of Massachusetts rent control and finds limited effects on housing quality as measured by census variables (such as lacking complete plumbing), but census-based quality measures are coarse and may not capture routine maintenance. Earlier work by Kiefer (1980) and Kutty (1996) examined rent control's effects on maintenance, finding evidence of reduced upkeep, though these studies relied on cross-sectional comparisons or theoretical models rather than causal identification. International evidence from Breidenbach, Eilers, and Fries (2022), who study Germany's 2015 rent brake, finds that the probability a unit is listed in "good condition" fell by approximately 3 percentage points after the regulation took effect. However, their analysis relies on repeated cross-sections of online rental listings with self-reported quality measures, rather than a building-level panel with administrative data. More recently, NYU Furman Center (2021) examined trends in complaints, violations, and permits in the nine months following HSTPA and found no discernible change in housing quality measures. However, the authors note that quality effects may take years to materialize, and their descriptive trend analysis does not employ a causal identification strategy. This paper extends their work with a longer post-period (5.5 years), a rigorous event study design with formal pre-trend tests, and an identification strategy that identifies off within-building quality changes.

A key conceptual distinction underlies the relationship between this prior work and the current study. The supply margin studied by Diamond, McQuade, and Qian (2019) and Autor, Palmer, and Pathak (2014) involves discrete, irreversible decisions: converting a building to condos, demolishing and redeveloping, or exiting the rental market. The quality margin, by contrast, involves continuous, reversible decisions: how much to spend on maintenance this month, how quickly to respond to tenant complaints, whether to repaint hallways or repair leaky faucets. These decisions can be adjusted in real time in response to changing incentives, and they affect tenant welfare without changing the housing stock.

This paper contributes to the literature by providing direct causal evidence on how strengthening rent regulation affects housing quality. A key methodological advantage is a large building-level panel with building fixed effects, which identifies off *within-building* changes in quality over time. Prior work on rent control and quality mostly relied on cross-sectional comparisons (Early, 2000) or repeated cross-sections of different units (Breidenbach, Eilers, and Fries, 2022), which cannot rule out selection: perhaps worse landlords sort into rent control, or stabilized buildings were already on different trajectories. By tracking the same buildings before and after HSTPA, I show that tightening rent control causes *existing buildings* to deteriorate. I also distinguish between routine maintenance (captured by violations and complaints) and major capital investment (captured by permits), finding that HSTPA reduced both. The findings complement Diamond, McQuade, and Qian (2019) by examining a different margin of landlord response: while they find that landlords respond to rent control by exiting the rental market, I find that landlords who remain in the market reduce both routine maintenance and capital investment.

4 Data

4.1 Data Sources and Variables

I assemble administrative data from multiple New York City agencies to measure housing quality, investment, and rent stabilization status at the building level. I link datasets using the Borough-Block-Lot (BBL) identifier, a 10-digit code that uniquely identifies every tax lot in New York City. The BBL system, maintained by the Department of Finance, serves as the common key across city administrative records.

Building characteristics. The Primary Land Use Tax Lot Output (PLUTO) database, published by the Department of City Planning and available through NYC Open Data, provides the universe of tax lots and their characteristics. [5] I use PLUTO to obtain total residential units, year built, building class (walk-up vs. elevator), census tract, and lot area. PLUTO defines the sample frame: I begin with all lots and apply the restrictions described below.

Housing quality outcomes. Data on violations and complaints come from the Department of Housing Preservation and Development (HPD), accessed through NYC Open Data. HPD enforces the New York City Housing Maintenance Code. Tenants file complaints through 311 (phone or online) describing conditions such as lack of heat, water leaks, or pest infestations. HPD inspectors then visit the building to verify reported conditions. If a violation of the Housing Maintenance Code is confirmed, it is classified by severity: Class A (non-hazardous), Class B (hazardous), or Class C (immediately hazardous). Class B violations, which include defective plumbing, inadequate lighting in public areas, broken window guards, and missing smoke detectors, must be corrected within 30 days. Class C violations, which include lead paint hazards, no heat or hot water, pest infestations, and structural defects, must be corrected within 24 hours.

I focus on Class C violations as the primary outcome because they represent the most serious maintenance failures. I also examine Class B (hazardous) violations and rent-impairing violations, a subset for which tenants can deposit rent with the court rather than paying landlords until corrections are made. Violations are closed when either an HPD inspector verifies the condition has been corrected or the landlord submits a certificate of correction. I measure days to close as a proxy for landlord responsiveness.

HPD complaint data provide a complementary measure. Unlike violations, which require inspector verification, complaints represent tenant-reported problems directly, capturing tenant experience without necessarily depending on enforcement intensity.

Investment outcomes. Building permit data come from the Department of Buildings (DOB), also accessed through NYC Open Data. NYC law requires permits for significant construction work: alterations affecting building use or occupancy, structural modifications, and major plumbing or electrical work. I focus on alteration permits (types A1, A2, A3), which capture renovations requiring regulatory approval. I combine data from both DOB's traditional filing system and DOB NOW, the online portal launched in 2016 that now handles the majority of filings.[6]

Routine maintenance (painting, plaster repair, replacing fixtures, minor repairs) does

[5] NYC Open Data is the city's public data portal, available at `https://opendata.cityofnewyork.us/`.

[6] Using only the traditional system would show a spurious decline after 2016 as filings shifted to the online portal.

not require permits. This distinction is central to interpretation: permit activity captures major capital investment, while violations and complaints capture routine maintenance. Because 89% of building-periods have zero alteration permits, I focus on the extensive margin: whether a building has any alteration permit in a 6-month period. This binary measure is more appropriate than counts given the high zero-inflation.

For descriptive analysis of the MCI cost-recovery mechanism, I use Major Capital Improvement application data from the NYCDB project, originally obtained through a FOIL request.[7] MCIs are building-wide improvements (boilers, roofs, elevators) for which stabilized building landlords can seek rent increases.

Treatment variables. I collect data on rent-stabilization status from two sources. First, I collect the list of buildings with any rent-stabilized units from the Division of Housing and Community Renewal (DHCR) registry, published by the NYC Rent Guidelines Board as PDF lists of stabilized buildings (BBLs) by borough.[8] I extract BBLs from these PDFs and match to the PLUTO database. Using 2024 data for a 2019 policy change is a limitation. However, HSTPA eliminated vacancy decontrol, so buildings can no longer exit stabilization. The 2024 registry should therefore capture nearly all buildings that were stabilized at the time of HSTPA's passage.

For robustness, I also collect data on the share of units that were rent-stabilized in each building as of 2018, which I use as a continuous treatment measure. These data are downloaded from the NYCDB project, and were originally compiled from NYC Department of Finance (DOF) tax bills.[9]

4.2 Sample and Summary Statistics

The analysis sample includes buildings with 6 or more residential units, constructed before 1990, observed from January 2015 through December 2024. I restrict to older, larger buildings to focus on the multifamily rental stock where rent stabilization is prevalent and to ensure common support between stabilized and non-stabilized buildings. I exclude NYCHA public housing and buildings receiving LIHTC, HUD-assisted, or Mitchell-Lama subsidies, which face federal or state affordability requirements distinct from rent stabilization.[10]

Table 1 summarizes the sample construction, showing how the final analysis sample is derived from the universe of NYC properties. The sample begins with all lots in the PLUTO database, then applies restrictions for multifamily buildings (6+ units), valid year

[7] Data from NYCDB; see `https://github.com/nycdb/nycdb`. Original FOIL request by Winnie Shen.

[8] Available at `https://rentguidelinesboard.cityofnewyork.us/resources/rent-stabilized-building-lists/`.

[9] Data originally compiled by John Krauss from DOF tax bills; see `https://github.com/nycdb/nycdb`.

[10] I identify subsidized buildings using the HPD Affordable Building dataset from NYC Open Data.

built, construction before 1990, and available community district information. NYCHA public housing and subsidized housing (LIHTC, HUD-assisted, Mitchell-Lama) are excluded because these buildings face distinct regulatory regimes.

Table 1: Sample Construction

Sample Restriction	Buildings	Dropped
All NYC lots (PLUTO)	858,284	—
Multifamily (≥6 units)	68,040	790,244
Valid year built	67,779	261
Built 1990 or earlier	56,968	10,811
Has community district	56,957	11
Excl. NYCHA & subsidized	53,909	3,048
Final analysis sample	53,909	0

Notes: Table shows the number of buildings remaining after each sample restriction. "Dropped" indicates the number of buildings excluded at each step. NYCHA refers to New York City Housing Authority public housing. Subsidized housing includes LIHTC, HUD-assisted, and Mitchell-Lama buildings.

Table 2 presents summary statistics separately for rent-stabilized and non-stabilized buildings. Stabilized buildings average 32 units compared to 41 for non-stabilized buildings, and both groups were built around 1922–1923 on average. However, stabilized buildings have substantially higher baseline violation rates (0.16 Class C violations per month vs. 0.06) and complaint rates (0.57 vs. 0.17 per month). Figure 1 shows the geographic distribution: rent-stabilized buildings are concentrated in Manhattan, the Bronx, and parts of Brooklyn and Queens, with substantial overlap between stabilized and non-stabilized buildings within neighborhoods.

5 Empirical Strategy

The goal is to estimate the causal effect of HSTPA on building outcomes by comparing rent-stabilized buildings (treated) to non-stabilized buildings (control) before and after June 2019. The challenge is that stabilized and non-stabilized buildings differ systematically. As Table 2 shows, stabilized buildings have nearly three times as many Class C violations per month at baseline (0.16 vs. 0.06) and over three times as many complaints (0.57 vs. 0.17). Simple comparisons would conflate HSTPA effects with pre-existing differences in building quality.

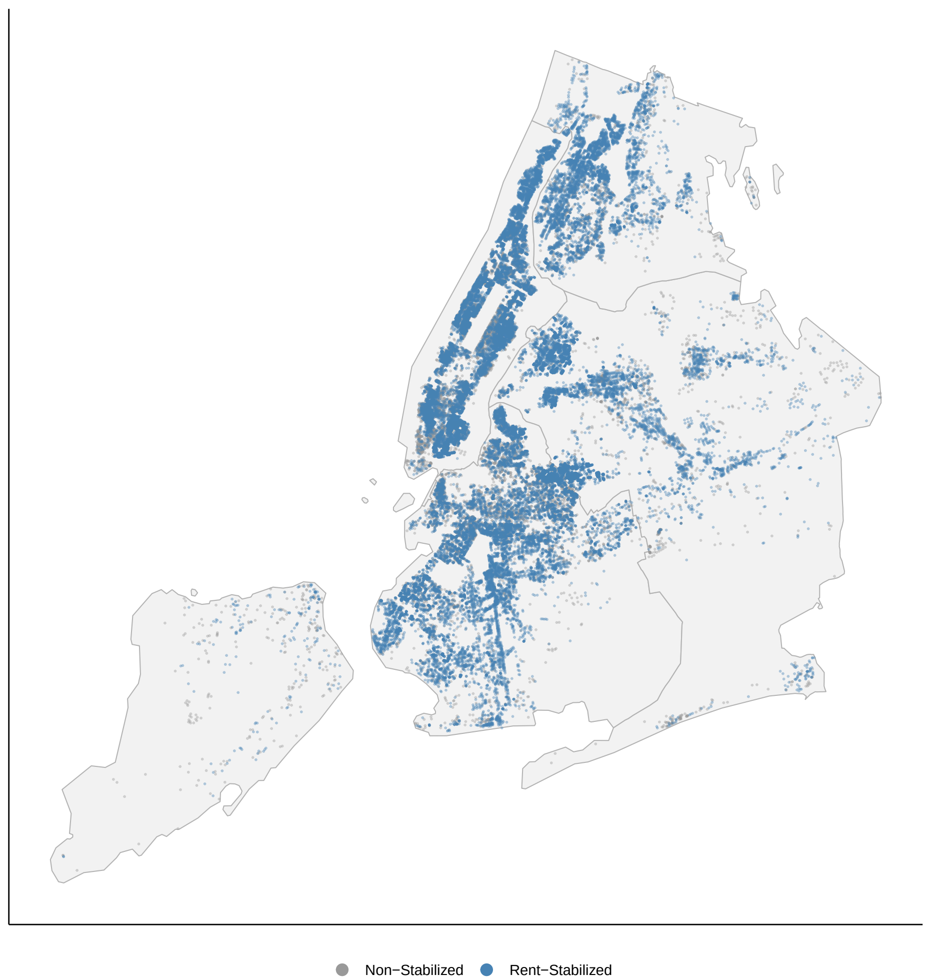

Figure 1: Geographic Distribution of Sample Buildings by Rent Stabilization Status
Notes: Blue points indicate rent-stabilized buildings (36,104); gray points indicate non-stabilized buildings (17,805). Building locations are based on PLUTO lot centroids.

Table 2: Summary Statistics

	Stabilized		Non-Stabilized	
	Mean	SD	Mean	SD
Building Characteristics				
Total Units	31.0	54.9	27.6	90.0
Year Built	1922	19	1922	26
Pre-Period Outcomes (monthly)				
Class C Violations	0.161	0.443	0.055	0.277
Class B Violations	0.468	1.204	0.140	0.660
HPD Complaints	0.549	2.153	0.156	0.980
DOB Alterations	0.044	0.132	0.064	0.183
N Buildings	36,104		17,805	

Notes: Sample consists of 53,909 NYC buildings with six or more residential units built before 1990, excluding NYCHA public housing and buildings receiving LIHTC, HUD, or Mitchell-Lama subsidies. Rent stabilization status is determined from the DHCR registry. Outcome variables are measured at the building-month level during the pre-HSTPA period (January 2015 through May 2019). Class C violations are immediately hazardous conditions; rent-impairing violations allow tenants to deposit rent with the court.

I address this with building fixed effects and five sets of group-by-period fixed effects. Building fixed effects absorb all time-invariant characteristics, so identification comes from within-building changes over time. The group-by-period fixed effects control for differential trends along five dimensions: (1) tract × period absorbs neighborhood-specific shocks like local enforcement campaigns or gentrification; (2) size × period accounts for the possibility that larger buildings have different violation trajectories than smaller ones; (3) decade × period allows buildings of different vintages to age differently; (4) building type × period lets walk-ups and elevator buildings follow separate trends; and (5) baseline outcome quintile × period controls for mean reversion—buildings with high baseline violations may trend differently than buildings starting from low levels.

I estimate:

$$Y_{it} = \alpha_i + \sum_{k \neq -1} \beta_k \cdot \mathbf{1}[t = k] \cdot \text{Stabilized}_i + \gamma^{\text{tract}}_{c(i),t} + \gamma^{\text{size}}_{s(i),t} + \gamma^{\text{decade}}_{d(i),t} + \gamma^{\text{type}}_{b(i),t} + \gamma^{\text{baseline}}_{q(i),t} + \varepsilon_{it} \quad (1)$$

where Y_{it} is the outcome for building i in period t, α_i are building fixed effects, and the γ terms are the five sets of group-by-period fixed effects (indexed by tract c, size bin s, decade d, building type b, and baseline quintile q). The coefficients β_k capture the differential change in outcomes for stabilized buildings in period k relative to the reference period ($k = -1$, January–May 2019). Standard errors are clustered at the building level.

I aggregate time into 6-month bins rather than months for three reasons. First, violations and complaints are relatively rare at the building level, so monthly data would be noisy. Second, 6-month bins smooth over seasonal patterns (e.g., heating complaints concentrate in winter). Third, HSTPA passed in mid-June 2019, so the first post-period bin (July–December 2019) captures immediate effects cleanly.

The key assumption is that, conditional on the fixed effects, outcomes in stabilized and non-stabilized buildings would have evolved similarly absent HSTPA. The event study provides an indirect test: if pre-period coefficients (β_k for $k < 0$) are close to zero, the groups were on parallel trajectories before HSTPA. As I show in Section 6, the full set of fixed effects is essential—specifications omitting baseline outcome quintile × period fixed effects exhibit significant pre-trends, while the full specification achieves clean pre-trends across all outcomes.

6 Results

6.1 Main Results

Violations. Housing code violations provide a direct measure of building conditions, with Class C violations representing immediately hazardous conditions. Figure 2 presents the event study. The pre-period coefficients are close to zero and statistically insignificant. Violations increase after HSTPA, with coefficients becoming positive and significant starting in the first post-period (July–December 2019). The effect persists and grows over time.

Table 3 reports the pooled difference-in-differences estimates. Class C violations increased by 0.207 per building per 6-month period, a 22% increase relative to the pre-HSTPA baseline of 0.96 for stabilized buildings.[11] Class B violations increased by 0.187 (7% of baseline). Rent-impairing violations, which allow tenants to deposit rent with the court until corrections are made, increased by 0.051 (14% of baseline). The time to close violations increased by 22.6 days (23% of baseline), indicating that landlords are slower to address building problems even after violations are issued.

Complaints. Figure 3 presents the event study for tenant complaints. Complaints represent tenant-reported problems rather than inspector-identified violations, providing a complementary measure of building quality that does not depend on enforcement intensity.

[11] For comparison, Early (2000) found that rent-controlled units had approximately 10% lower maintenance expenditures than comparable uncontrolled units using American Housing Survey data.

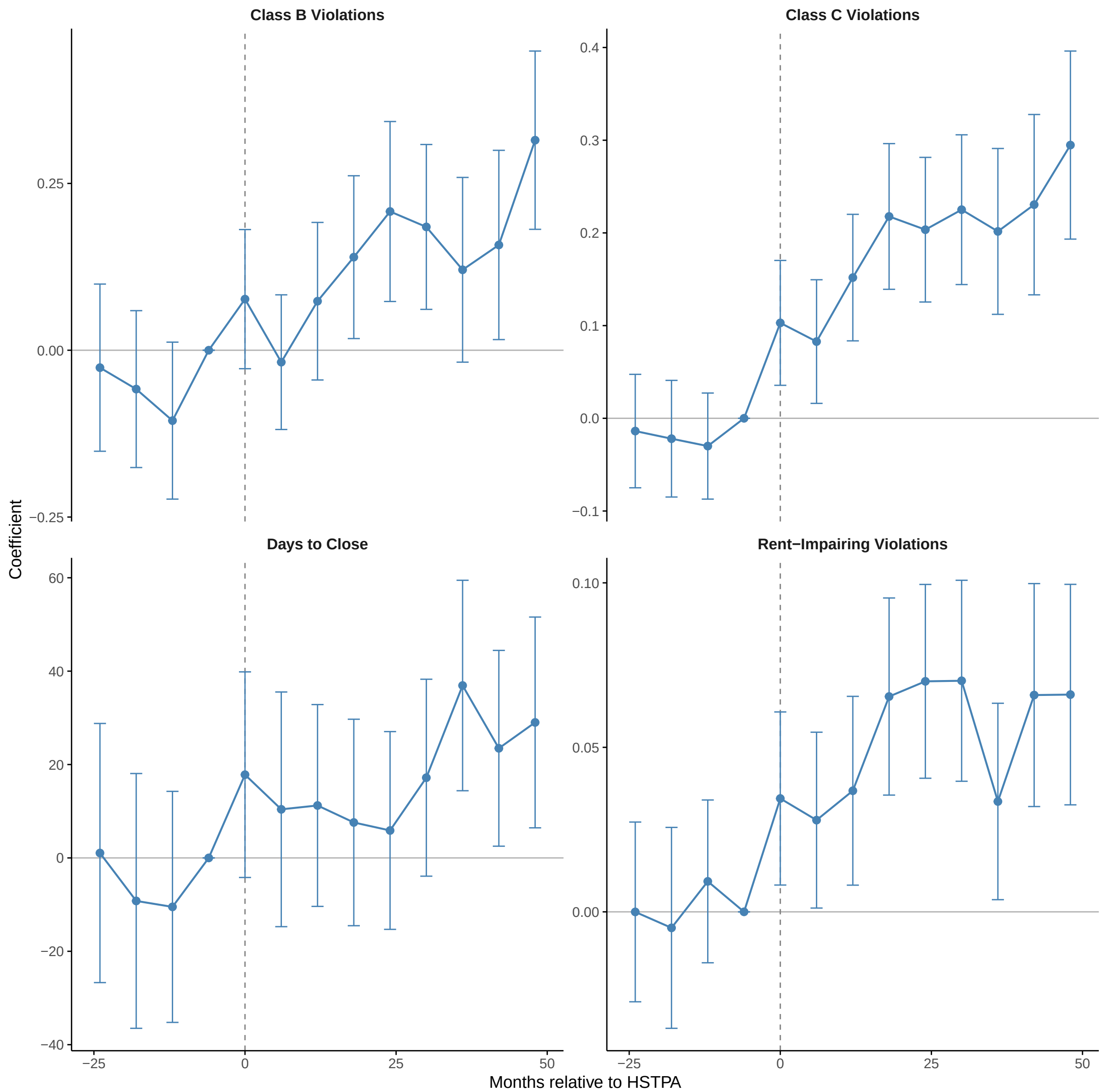

Figure 2: Event Study: Effect of HSTPA on Housing Code Violations

Notes: Sample consists of 53,909 NYC buildings with six or more residential units built before 1990, excluding NYCHA public housing and buildings receiving LIHTC, HUD, or Mitchell-Lama subsidies. Figure plots coefficients β_k on interactions between an indicator for rent-stabilized buildings and 6-month period indicators, relative to January–May 2019 (period $k = -1$). Specification includes building fixed effects plus tract×period, size-bin×period, decade×period, building-type×period, and baseline-quintile×period fixed effects. Vertical dashed line indicates HSTPA passage (June 2019). Error bars show 95 percent confidence intervals. Standard errors are clustered at the building level. $N = 700{,}817$ building-period observations.

Table 3: Effect of HSTPA on HPD Violations

Dependent Variable:	outcome				
	Class C	Class B	Rent-Impairing	Days to Close	Any Alteration
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Stabilized × Post	0.2065***	0.1872***	0.0512***	22.61***	-0.0156***
	(0.0230)	(0.0409)	(0.0079)	(6.919)	(0.0018)
Baseline mean	0.96	2.81	0.37	99.59	0.11
Max $\|t\|$ pre-trend	1.03	1.76	0.73	0.83	1.11
Buildings	53,909	53,909	53,909	29,318	53,909
Fit statistics					
Observations	700,817	700,817	700,817	129,367	700,817

Clustered (bbl) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Sample consists of NYC buildings with six or more residential units built before 1990, excluding NYCHA public housing and buildings receiving LIHTC, HUD, or Mitchell-Lama subsidies. Table reports pooled difference-in-differences coefficients on the interaction of Post (after June 2019) and Stabilized. Specification includes building fixed effects plus tract×period, size-bin×period, decade×period, building-type×period, and baseline-quintile×period fixed effects. "Baseline mean" is the pre-period mean for stabilized buildings. "Max $|t|$ pre-trend" is the maximum absolute t-statistic among pre-period coefficients in the corresponding event study. Standard errors clustered at the building level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4 shows that HPD complaints increased by 0.251 per building per 6-month period (8% of baseline). Emergency complaints, which represent more urgent conditions, increased by 0.250 (7% of baseline). These findings address the concern that violation results might reflect changes in enforcement intensity: complaints are filed by tenants regardless of inspection activity.

Permits. Figure 4 presents the event study for building permits. Because 89% of building-periods have zero alteration permits, I focus on the extensive margin: whether a building has any alteration permit in a 6-month period. The probability of any alteration permit fell by approximately 1.3 percentage points following HSTPA, representing about an 11% decline relative to the baseline probability of 11%.

The permit decline is smaller in percentage terms than the violation and complaint increases, but still economically meaningful. While violations increased by 21% and complaints by 8%, permits fell by approximately 11%. This pattern suggests that HSTPA reduced both routine maintenance (violations, complaints) and capital investment (permits).

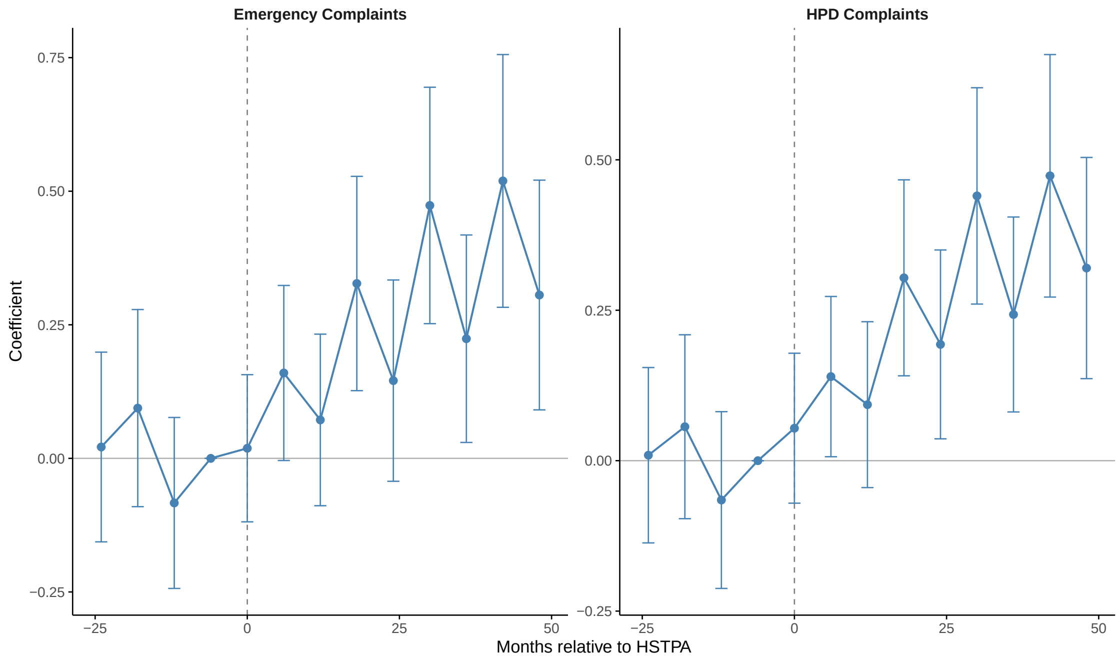

Figure 3: Event Study: Effect of HSTPA on HPD Complaints

Notes: Sample consists of 53,909 NYC buildings with six or more residential units built before 1990, excluding NYCHA public housing and buildings receiving LIHTC, HUD, or Mitchell-Lama subsidies. Figure plots coefficients on interactions between an indicator for rent-stabilized buildings and 6-month period indicators, relative to January–May 2019. Specification includes building fixed effects plus tract×period, size-bin×period, decade×period, building-type×period, and baseline-quintile×period fixed effects. Vertical dashed line indicates HSTPA passage (June 2019). Error bars show 95 percent confidence intervals. Standard errors are clustered at the building level.

6.2 MCI Applications

Figure 5 shows the time series of MCI applications. MCI filings collapsed immediately upon HSTPA's passage: MCI filings fell from an average of 118 applications per month before HSTPA to 23 per month after, an 80 percent decline.

The June 2019 spike (313 applications, versus 118 monthly average) reflects anticipation: landlords who were planning MCI projects rushed to file before HSTPA reduced the return on such investments. The immediate collapse in July 2019 and subsequent months demonstrates that landlords respond quickly to changes in MCI economics.

As noted above, this is descriptive evidence only—there is no control group of non-stabilized MCI applications because MCIs are a feature of rent stabilization. However, the timing is striking. The collapse occurs exactly at HSTPA passage, not gradually over time. This pattern is more consistent with a causal effect of HSTPA than with confounding trends.

The MCI collapse is consistent with the permit decline documented above, though the magnitudes differ. MCIs are the *rent-recovery mechanism* for capital improvements—the

Table 4: Effect of HSTPA on HPD Complaints

Dependent Variable:	outcome	
	All Complaints	Emergency
Model:	(1)	(2)
Variables		
Stabilized × Post	0.2512***	0.2415***
	(0.0550)	(0.0630)
Baseline mean	3.29	3.62
Max $\|t\|$ pre-trend	0.87	1.02
Buildings	53,909	53,909
Fit statistics		
Observations	700,817	700,817

Clustered (bbl) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Sample and specification as in Table 3. Standard errors clustered at the building level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

application to DHCR for permission to increase rents to recover improvement costs. Building permits are the *regulatory approval mechanism* for the physical work itself. A landlord can pursue a capital improvement project (requiring a permit) without filing an MCI (foregoing the rent increase). The 81% collapse in MCIs indicates that landlords largely stopped using the rent-recovery mechanism; the 11% decline in permits indicates that they also reduced capital projects, though by a smaller margin.

6.3 Robustness

Fixed effects specifications. A key concern in difference-in-differences designs is that treated and control groups may follow different trends even absent treatment. Table 5 investigates this by estimating specifications with progressively richer fixed effects structures for Class C violations.

All specifications produce positive point estimates, but simpler specifications exhibit substantial pre-trend violations. Only the full specification with baseline quintile × time fixed effects achieves clean pre-trends. This pattern highlights that credible identification requires comparing buildings that share similar baseline violation rates, not just similar observable characteristics.

Treatment intensity. If HSTPA caused the observed decline in maintenance quality,

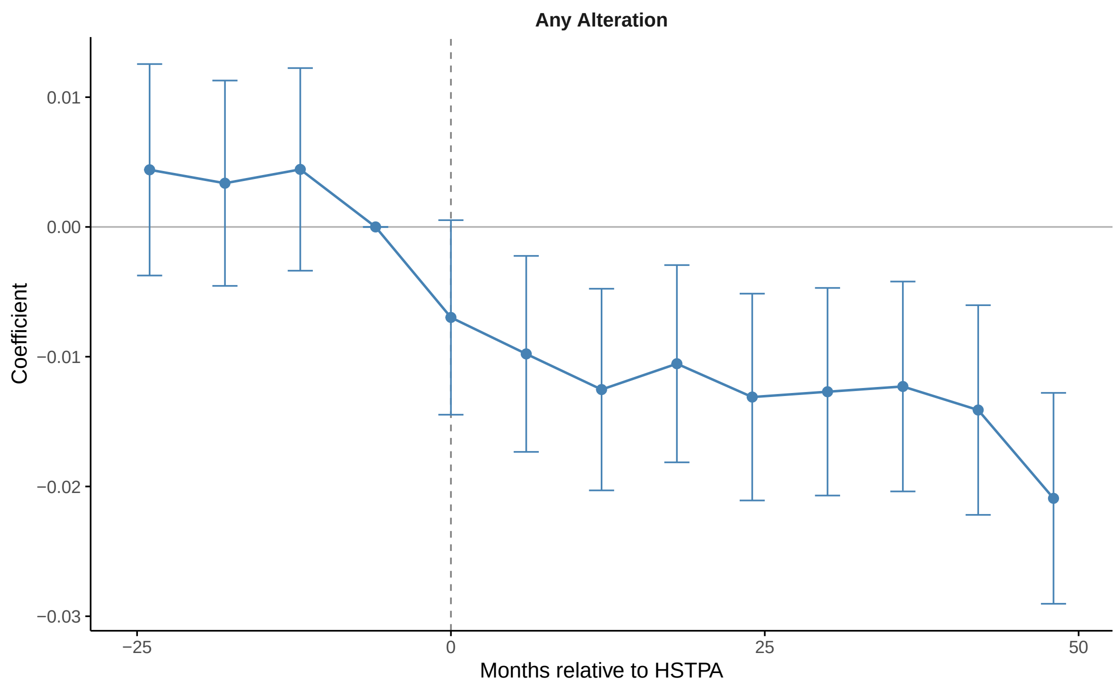

Figure 4: Event Study: Effect of HSTPA on Building Permit Activity (Extensive Margin)
Notes: Sample consists of 53,909 NYC buildings with six or more residential units built before 1990, excluding NYCHA public housing and buildings receiving LIHTC, HUD, or Mitchell-Lama subsidies. Figure plots coefficients on interactions between an indicator for rent-stabilized buildings and 6-month period indicators, relative to January–May 2019. Outcome is a binary indicator for whether the building had any Department of Buildings alteration permit (types A1, A2, A3) in the period. Specification includes building fixed effects plus tract×period, size-bin×period, decade×period, building-type×period, and baseline-quintile×period fixed effects. Vertical dashed line indicates HSTPA passage (June 2019). Error bars show 95 percent confidence intervals. Standard errors are clustered at the building level.

effects should scale with treatment intensity. Figure 7 tests this prediction by comparing the binary treatment (any stabilization) with a continuous measure (share of units stabilized).

Both specifications show flat pre-trends followed by divergence after HSTPA. The continuous specification yields a larger point estimate, consistent with dose-response: fully stabilized buildings experience larger effects than partially stabilized buildings.

7 Conclusion

This paper studies how strengthening rent control affects housing quality. I use New York's 2019 Housing Stability and Tenant Protection Act, which sharply reduced landlords' ability to raise rents and recover capital costs, and compare outcomes in rent-stabilized and non-stabilized buildings. I find that hazardous violations increased by 21%, complaints increased by 8%, and violations took 23% longer to resolve. Building permit activity also declined, with

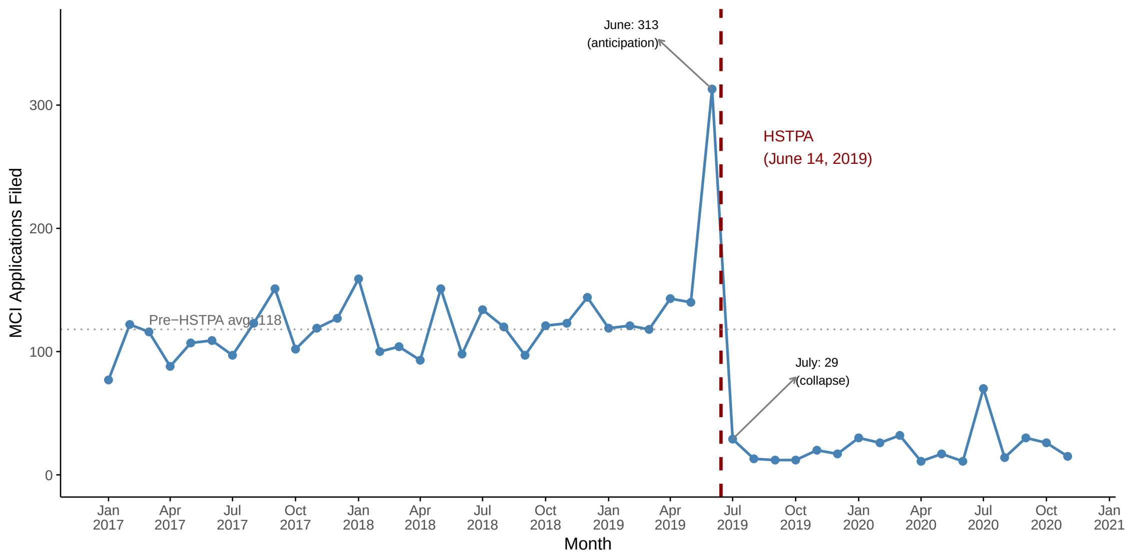

Figure 5: MCI Applications Over Time

Notes: Figure shows monthly Major Capital Improvement (MCI) application filings to the Division of Housing and Community Renewal (DHCR) from January 2017 through November 2020. MCIs are building-wide improvements (boilers, roofs, elevators) for which landlords of rent-stabilized buildings can seek rent increases. Vertical dashed line indicates HSTPA passage (June 14, 2019). Pre-HSTPA monthly average: 118 filings. Post-HSTPA monthly average: 23 filings (81 percent decline). The June 2019 spike (313 filings) reflects anticipation as landlords rushed to file before the law took effect. Data obtained via FOIL request. This analysis is descriptive only; MCIs are available only for rent-stabilized buildings, so there is no valid control group for causal inference.

the probability of any alteration permit falling by approximately 11%. These findings suggest that strengthening rent control reduced both routine maintenance and capital investment.

These results provide causal evidence on a longstanding prediction in the rent control literature. A natural next step is to explore heterogeneity in these effects: which buildings, neighborhoods, or landlord types saw the largest quality declines.

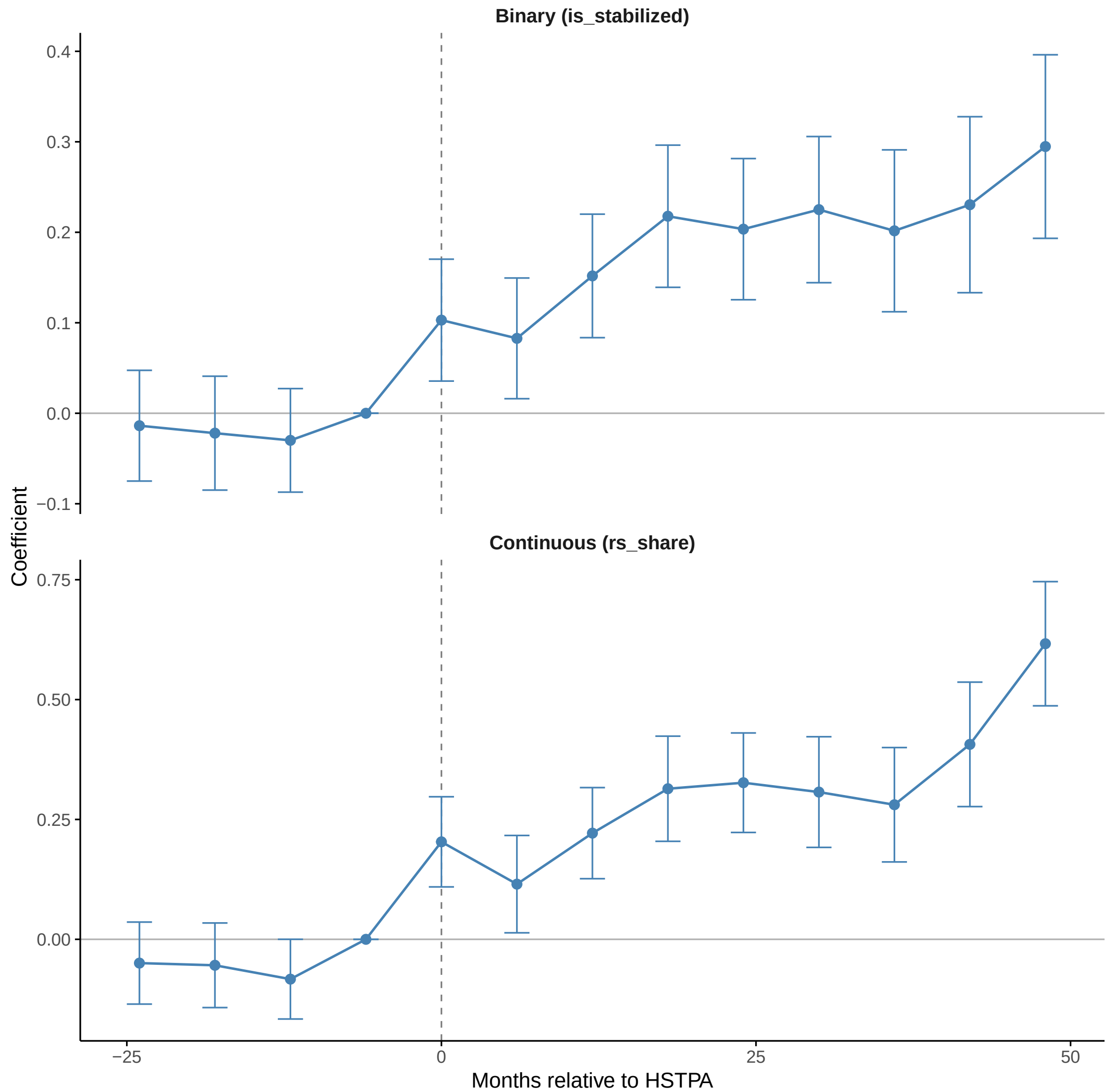

Figure 6: Binary vs. Continuous Treatment

Notes: Sample consists of 53,909 NYC buildings with six or more residential units built before 1990. Top panel uses a binary indicator for rent-stabilized buildings from the DHCR registry. Bottom panel uses continuous treatment intensity, defined as the share of units that are rent-stabilized (ranging from 0 to 1), constructed from 2018 property tax bill data. Both specifications include building fixed effects plus tract×period, size-bin×period, decade×period, building-type×period, and baseline-quintile×period fixed effects. Error bars show 95 percent confidence intervals. Standard errors are clustered at the building level.

Table 5: Robustness to Alternative Fixed Effects Specifications

Dependent Variable:	outcome					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Post × Stabilized	0.6870***	0.3012***	0.2189***	0.2048***	0.1973***	0.2065***
	(0.0161)	(0.0221)	(0.0216)	(0.0220)	(0.0230)	(0.0230)
Building	Yes	Yes	Yes	Yes	Yes	Yes
Tract × Time		Yes	Yes	Yes	Yes	Yes
Size Bin × Time			Yes	Yes	Yes	Yes
Decade Built × Time				Yes	Yes	Yes
Building Class × Time					Yes	Yes
Baseline Outcome × Time						Yes
Buildings	53,909	53,909	53,909	53,909	53,909	53,909
Pre-trend max$\lvert t \rvert$	23.04	7.710	6.360	6.180	5.260	1.030
Fit statistics						
Observations	700,817	700,817	700,817	700,817	700,817	700,817

Clustered (Building) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Sample consists of 53,909 NYC buildings with six or more residential units built before 1990, excluding NYCHA public housing and buildings receiving LIHTC, HUD, or Mitchell-Lama subsidies. Table reports pooled post-period coefficients from difference-in-differences regressions of Class C violations on Post × Stabilized, where Post equals one for periods after June 2019. Columns differ in fixed effects structure, building progressively from building fixed effects only (column 1) to the full matching specification (column 6). "Pre-trend max $\lvert t \rvert$" is the maximum absolute t-statistic among pre-period coefficients from the corresponding event study. Standard errors (in parentheses) are clustered at the building level.

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