Equilibrium Effects of Eviction Protections: The Case of Legal Assistance *

Robert Collinson, John Eric Humphries, Stephanie Kestelman, Scott Nelson, Winnie van Dijk & Daniel Waldinger†

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Abstract

"Right-to-counsel" programs provide free legal assistance to tenants facing eviction. While such assistance can delay or prevent eviction, large-scale programs may create costs for tenants through equilibrium rental market responses. Leveraging the partial rollout of New York's program, we find listed rents rose by $29–$38/month. We then develop a framework to evaluate the policy's impact on tenant welfare and quantify it using linked data on evictions, rental listings, and tenant income. After accounting for both direct benefits and insurance value of stronger protections, our estimates imply that equilibrium responses are enough to cause a small net reduction in tenant welfare.

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†Collinson: University of Notre Dame. Humphries: Yale University. Kestelman: Arnold Ventures and Harvard University. Nelson: Chicago Booth. Van Dijk: Yale University. Waldinger: New York University.

1 Introduction

Housing courts across the United States handle high volumes of disputes between landlords and tenants: 5 to 6 percent of renter households have an eviction case filed against them in a typical year, totaling about 2.7 million court cases annually (Gromis et al., 2022). Growing awareness of the prevalence and consequences of evictions has led to increasing policy interest in regulating them, with hundreds of eviction-related bills introduced at the federal and state levels each year (Humphries et al., 2024). Eviction-protection policies are designed to help individual tenants. However, when implemented at a large scale, these interventions may also have welfare-relevant equilibrium effects on the rental housing market. While researchers have begun to empirically examine the benefits of legal assistance for tenants facing eviction, little evidence exists on the equilibrium effects of such policies.

A prominent type of eviction protection policy is "right to counsel" (RTC), which provides free legal assistance to tenants in eviction court. At least seventeen US cities and five states have recently introduced RTC programs (NCCRC, 2024). Legal assistance provided under RTC has been shown to reduce the likelihood of an eviction order and provide tenants continued access to housing as cases take longer to resolve (Ellen et al., 2021; Cassidy and Currie, 2023). The magnitudes of these effects suggest these policies may also lead to equilibrium responses when implemented at scale.

In this paper, we consider whether such equilibrium responses exist and, if so, how they affect tenant welfare. We empirically examine the benefits and costs for tenants from the United States' first RTC program: New York City's Universal Access to Counsel. Beginning in 2017, the program was gradually phased in by ZIP code, providing arguably the best opportunity to date for identifying supply-side responses to an eviction protection policy implemented at scale. To evaluate the market-level responses to RTC, we combine a quasi-experimental research design based on the rollout with multiple linked data sets, including eviction court cases, rental housing listings, credit reports, and tenants' residential histories. We find evidence of rent price increases in ZIP codes where RTC was introduced, as well as evidence consistent with a decrease in the quantity of available rental units. We find no clear evidence that landlords improve maintenance or adjust other margins such as tenant screening.

Guided by this evidence, we develop a model and empirical strategy to quantify the impact of the policy on tenants' welfare. Our approach builds on ideas from the social insurance literature, and weighs our estimates of tenants' costs from RTC due to higher rents against tenants' benefits. These benefits include both the direct transfer value of more favorable court outcomes for evicted tenants and the insurance value from the fact that eviction protections tend to benefit tenants who have recently experienced earnings losses. We measure the insurance value using a panel of linked evictions-earnings data. Combining our empirical results and framework, we estimate that the price increases due to RTC are large enough to more than offset the benefits to tenants, causing a small net reduction in their welfare.[1]

While defendants have a right to a government-provided legal representative in criminal court, no such guarantee exists for civil matters. Large representation asymmetries exist between landlords and tenants: across a large number of jurisdictions, the National Coalition for a Civil Right to Counsel estimates an average representation rate of 4% for tenants and 83% for landlords (NCCRC, 2024).[2] Attorneys may affect a tenant's eviction court outcomes in multiple ways, for example, by negotiating with the landlord on behalf of their client, requesting a continuance or discovery of evidence, or presenting arguments for the defendant in court. Indeed, prior research has found that the RTC program in New York City (hereafter, NYC) increased case duration and reduced the probability of a judgment against the tenant (Ellen et al., 2021; Cassidy and Currie, 2023). Specifically, Cassidy and Currie (2023) find that legal assistance increases case durations by nearly three months and reduces eviction orders by 32 percentage points.

These facts suggest that legal assistance benefits tenants who receive representation in eviction court, but may also generate significant costs for landlords. Further, these costs and benefits need not be symmetric. While longer case durations allow tenants to remain in the unit for additional time while not necessarily paying rent, the extended court process also generates legal, time, and hassle costs for landlords that tenants do not directly value. This asymmetry between landlord costs and tenant benefits is a potentially important feature of legal assistance programs when evaluating their welfare impacts. Using a simple supply and demand model, we argue in Section 2.2 that even for tenants—the intended beneficiaries of RTC—the welfare effects of the program are theoretically ambiguous. It is therefore an empirical question whether tenants are better off as a result of RTC.

In the first part of the paper, we provide quasi-experimental evidence on how the introduction of RTC affected NYC's rental market. We address several empirical challenges related to identification and measurement. Obtaining credible quasi-experimental estimates is difficult when legal representation is provided at scale—which is necessary to detect market-level impacts—since it is often not clear how to choose a reasonable comparison group. Leveraging the geographic rollout of RTC in NYC, our empirical strategy compares outcomes for tenants and rental units close to a border between adjacent ZIP codes that were treated at different times using difference-in-differences designs. An additional challenge for research on rental housing markets is that high-quality microdata on rent prices and other outcomes is difficult

[1]A complete welfare assessment of RTC would also consider impacts on landlords, taxpayers, and other individuals (e.g., children and elderly dependents or neighbors) insofar as impacts on their well-being are not internalized by tenants. We focus on the direct intended beneficiaries of this policy and leave broader impacts to future research.

[2]Most relevant to our setting, in pre-RTC New York City, 1% of tenants had legal representation, compared to 95-99% of landlords (Collinson et al., 2024).

to obtain. However, microdata is essential as it enables researchers to zero in on properties at the lower end of the market, and to observe prices and characteristics of available units at a high frequency. We use data from StreetEasy, a rental listing platform with extensive coverage of the NYC rental market during our sample period, to help address this issue. We show that the StreetEasy data has broad coverage even in lower-income ZIP codes, where rental properties were most likely to be affected by RTC.

We find that the policy led to higher rental prices in the two years after implementation. In our main empirical specifications, we estimate an increase ranging from $6.5 (95% CI: [-6.5,19.4]) to $12.5 (95% CI: [1.3,23.8]) per month in the first year, and ranging from $29.1 (95% CI: [13.3,45.0]) to $38.1 (95% CI: [22.9,53.3]) per month in the second year. The year-two estimates are statistically and economically significant, amounting to a 1.2–1.6 percent price increase. We perform an extensive sensitivity analysis, and in almost all robustness specifications, we can statistically rule out null effects. Using parcel-level data, we provide evidence of small increases in condo conversions, as well as "major alteration" and demolition permits, consistent with a quantity response on the extensive margin contributing to these price changes. Overall, these results provide prima facie evidence of an equilibrium response to the policy. However, they do not by themselves allow us to sign RTC's welfare effect for tenants.

To inform the development of a welfare framework to interpret these equilibrium responses, we first examine whether landlords respond to increased protections in other ways—for example, by screening prospective tenants more aggressively, or by reducing (or increasing) maintenance. While there are limitations to what we can measure in our available data, we use consumer credit reports linked with migration data to test for changes in screening behavior, and data on housing inspections and substantiated housing-code violations to test for changes in unit maintenance. In contrast to the price effects, we find no clear evidence of landlords responding substantially on these screening or maintenance margins. Finally, we note that Cassidy and Currie (2023) finds RTC did not significantly impact the number or composition of eviction cases. Taken together, this evidence motivates us to focus on price and quantity as the first-order supply-side responses to RTC.

Guided by this evidence on equilibrium responses, the second part of the paper quantifies how RTC impacted tenant welfare. A key empirical challenge is that RTC may have shifted both supply and demand for rental housing, similar to a mandated benefit (Summers, 1989). We show that if tenants optimally adjust their behavior in response to the RTC program, our quasi-experimental estimates of RTC's effect on rent prices, combined with estimates of RTC's benefits in eviction court and several other statistics of the data, are sufficient to quantify the tenant welfare effects of marginal changes in RTC policy. The approach is related to the "sufficient statistics" literature on social insurance (Chetty, 2006, 2009), and specifically to recent work on bankruptcy protection (Dávila, 2020), but tailored to the rental housing market and our policy context.

Section 4 presents the model and derives sufficient statistics for welfare analysis. In the model, renters make ex-ante optimal housing choices in the presence of idiosyncratic income risk, and then optimally default after income is realized. RTC benefits tenants in two ways. First, it acts like an in-kind transfer of housing from landlord to tenant by increasing the time a tenant may remain in their unit after defaulting. Second, it reduces the probability of a court judgment against the tenant, which lowers the likelihood of eviction-driven impacts on income and homelessness (Collinson et al., 2024). However, RTC may also shift housing supply. We show that, in this model, the change in rent captures (to first order) the shift in supply that is relevant to tenant welfare. As a result, RTC's tenant welfare impacts depend on a small number of statistics: RTC's effects on equilibrium rent prices; its impact on tenants' outcomes in eviction court; and the insurance value derived from the fact that tenants tend to be evicted when they are in financial distress. This framework has the advantage that it does not require directly measuring tenants' behavioral responses to RTC and the costs the policy generates for landlords, or to explicitly model supply-side behavior and market clearing.

We then quantify the framework's parameters. To estimate the insurance value, we measure how eviction filings co-vary with earnings in a panel of administrative earnings data matched to samples of NYC tenants facing and not facing eviction. Compared to similar tenants not facing eviction, evicted tenants experience larger drops in earnings around filing. Combined with a standard assumption about the functional form of utility, these different earnings distributions over time determine RTC's insurance value. For other inputs to the framework, we use our estimates of RTC's price effect two years after introduction, estimates of court impacts from the literature, and other statistics (e.g., rents and eviction rates by ZIP code) from survey data and administrative court records.

We find that the cost to tenants due to higher rent prices more than offsets the benefits from better eviction court outcomes. Across our main specifications, which vary the richness of tenant heterogeneity and the level of flexibility in estimating rent impacts, we estimate decreases in tenant welfare of \$16.97-\$25.68 per tenant per month. This holds even after accounting for the insurance value from protections, which makes up one third of the benefits to tenants under our baseline assumption about risk aversion. RTC's benefits in eviction court would need to be more than 2.5 times as large, the price effects less than 40 percent as large, or the insurance value more than five times as large as we estimate for us to conclude that tenants were on average better off due to RTC.

There is statistical uncertainty about the magnitude of RTC's tenant welfare effects. In our main specifications, bootstrapped standard errors range from \$7.63 to \$8.05 per tenant per month depending on the rent estimate used and specification of tenant heterogeneity. While we reject zero or positive welfare effects at the 2% level or less for these specifications, 95% confidence intervals contain much smaller (negative) welfare effects than the point estimates. We also explore robustness of our estimates to alternative assumptions about tenant risk aversion and the specification used to estimate the rent effects. While our point estimates of RTC's tenant welfare effect are always negative, 95% confidence intervals contain zero for high levels of tenant risk aversion and for a minority of the smaller and more noisily estimated robustness specifications of the rent effects.

Overall, our findings suggest that RTC imposed significant costs to landlords that were passed on to tenants through higher rent prices. While our exact welfare numbers are subject to statistical and model uncertainty, our analysis suggests that policymakers should take seriously the possibility that tenant protection policies can cause equilibrium responses, such as rent increases, that can more than offset the benefits to tenants.

Related Literature. Our work builds on and contributes to several areas of the economics literature. First, we contribute to a rapidly growing literature on equilibrium responses to tenant-protection policy. Much of this work uses general equilibrium models of the housing market to study medium- or long-run supply responses (Imrohoroglu and Zhao, 2022; Abramson, 2024; Abramson and van Nieuwenburgh, 2024; Corbae et al., 2024). Two recent papers, Coulson et al. (2020) and Humphries et al. (2024), respectively use a search-theoretic framework and a dynamic discrete choice model to study landlord responses to tenant protection policies. Complementing these papers, our estimates of rent price impacts of RTC are, to our knowledge, the first quasi-experimental evidence on the types of responses predicted by many of these structural analyses. We also develop an approach to welfare analysis that does not require a fully-specified model of supply-side behavior. Our estimates of the insurance value of RTC for tenants also relate to Bèzy et al. (2024)'s study of a French insurance program for young renters, Abramson and van Nieuwenburgh (2024)'s analysis of the viability of similar rent-insurance programs in the US, and Favilukis et al. (2023)'s analysis of the insurance value of housing affordability policies.[3]

Second, our work relates to quasi-experimental evaluations of rental housing market regulations. Prior work has studied the impacts of rent control policies on housing misallocation (Glaeser and Luttmer, 2003), the extensive margin of housing supply (Asquith, 2019; Diamond et al., 2019), rent prices and property values (Sims, 2007; Autor et al., 2014), and evictions (Gardner and Asquith, 2024; Geddes and Holz, 2024). Vigdor and Williams (2022) argue that

[3]The eviction process also shares features with the foreclosure process. The consequences of foreclosure are studied in, for example, Diamond et al. (2020), while the labor-market consequences of foreclosure *delay* for homeowners are explored in Herkenhoff and Ohanian (2019). Other related work on rental market equilibrium responses to policy interventions includes Calder-Wang (2021) and Almagro et al. (2024).

habitability standards may negatively impact tenants by making the most affordable units relatively more expensive to rent. Clarke and Gold (2024) study how regulatory changes in contract terms and litigation costs impact rent prices and housing quality in Canada. To our knowledge, we provide the first quasi-experimental estimates of how RTC programs impact market outcomes.

Third, we contribute to a small but growing literature on the consequences of providing expanded access to legal representation and other forms of legal assistance. Ellen et al. (2021) and Cassidy and Currie (2023) use similar court data for NYC and quasi-experimental approaches based on the same RTC rollout to evaluate the impact of legal representation on tenants' court outcomes. We leverage estimates from Cassidy and Currie (2023) in our welfare analysis. A broader literature studies the effects of legal assistance in housing court using randomized controlled trials, finding mixed results. For example, Jarvis et al. (2020) report precise null effects on the likelihood of receiving an eviction order, based on a well-powered randomized controlled trial conducted in California, while Seron et al. (2001), Greiner and Pattanayak (2011), and Greiner et al. (2012) report null effects or modest reductions based on randomized controlled trials in New York City and Massachusetts. Caspi and Rafkin (2025) similarly find null effects outside the period of expanded COVID-era rental assistance.[4] The more pronounced effects in the quasi-experimental studies of New York's RTC may be due to differences in program implementation or due to differences in the population recruited to participate in a randomized controlled trial versus those who take up legal assistance under RTC. In either case, we view the quasi-experimental estimates from Ellen et al. (2021) and Cassidy and Currie (2023) as the most relevant to our analysis. We add to this literature by estimating RTC's impact on market-level outcomes and tenant welfare in NYC.

Fourth, this paper connects to research on how legal protections act as implicit insurance in other parts of the economy, and how such protections can result in equilibrium responses. For example, Gross et al. (2021) finds that more generous consumer bankruptcy protection provides greater insurance against financial risk, but also raises the cost of credit. Agarwal et al. (2015) and Nelson (2025) similarly study the equilibrium responses to pricing regulation for delinquent credit card borrowers, while Mahoney (2015) shows that bankruptcy protection provides implicit health insurance to uninsured, low-asset households. These findings parallel our result that legal counsel protects tenants by partially insuring negative income shocks, but also increases rents. Similarly, a large literature studies how employment protection legislation can provide insurance to incumbent workers but also can have equilibrium effects on the labor market (Lazear, 1990; Hopenhayn and Rogerson, 1993). For instance, studies of the US (e.g., Autor et al., 2007) and Europe (e.g., Bjuggren, 2018; Daruich et al., 2023) show

[4] The impact of legal assistance in other areas of civil law is studied in, e.g., Hoynes et al. (2022) and Cooper et al. (2023).

that employment protections safeguard incumbent jobs, but hinder new job opportunities and lower factor productivity.

Finally, we draw from the longstanding literature in public economics that evaluates the welfare effects of marginal changes in social insurance programs using a sufficient statistics approach (e.g., Baily, 1978; Gruber, 1997; Chetty, 2006, 2009; Kolsrud et al., 2018). Our framework is especially related to Dávila (2020)'s analysis of optimal consumer bankruptcy protection, where borrowers who default on loans play an analogous role in his setting to tenants who default on rent in ours. Relative to this literature, our framework values insurance in the form of a specific good, housing services, rather than a cash transfer. This is similar to, e.g., Hendren and Sprung-Keyser (2020)'s welfare analyses of in-kind transfers, though we value housing services through a revealed-preference approach based on tenants' choices to default on rent, rather than through estimates of housing's long-term effects on tenants' earnings.

2 Policy background, conceptual framework, and data

2.1 Right-to-counsel policies: background

Low-income individuals are frequently involved in civil legal matters: according to a 2022 report by the Legal Services Corporation, 74% of low-income households experienced at least one civil legal problem over the prior year (LSC, 2022). While federal, state, and local governments typically fund some amount of civil legal aid, defendants in civil cases do not have a legal right to advice or representation from a lawyer in court. As a result, legal assistance is not universally publicly provided.[5] Instead, in the typical jurisdiction, only a small fraction of cases receive subsidized legal assistance. Although unsubsidized legal counsel for civil court matters exists, low-income defendants often do not obtain it: survey-based estimates suggest that 92% of civil legal problems had no or inadequate legal assistance (LSC, 2022).

In housing courts, which handle disputes between landlords and tenants, the situation is similar to the aggregate picture for civil matters: tenants in eviction cases are by and large unrepresented, while landlords typically have a legal representative. For example, in NYC's housing courts, 1% of tenants and 95-99% of landlords were represented before 2016, and in Cook County, IL, these numbers are 3% versus 75% (Collinson et al., 2024). Engler (2010)

[5]This situation stands in sharp contrast to criminal legal cases. In *Gideon v. Wainwright* (1963), the Supreme Court ruled that the 6th Amendment requires states to provide attorneys to criminal defendants unable to afford their own legal representation. This case was followed by *Lassiter v. Dept. of Social Services* (1981), in which the Court explicitly ruled that the right to counsel does not extend to civil matters, with the majority opinion arguing that representation is not constitutionally guaranteed when physical liberty is not at stake.

surveys the literature on legal representation in eviction court and finds that, in the studies available at that time, fewer than 20 percent of tenants had legal representation, compared to 80-90 percent of landlords. A more recent survey documents similar statistics across a large number of jurisdictions (NCCRC, 2024).

In recent years, policymakers have implemented right-to-counsel policies that provide free legal assistance to low-income tenants in housing court.[6] New York City was the first city to implement such a policy, beginning in 2017 under Local Law 136. As of 2024, five states, two counties, and seventeen cities have introduced RTC programs, while similar policies were under consideration or being piloted in several cities, including Los Angeles, Boston, and Chicago.[7]

2.2 A conceptual framework for understanding the welfare impacts of right to counsel

This section uses a simple supply and demand framework to argue that the welfare impacts of right to counsel are theoretically ambiguous. Because RTC benefits tenants in eviction court but likely generates costs for landlords, it may shift both the supply of and demand for rental housing in treated neighborhoods. In this regard, programs like RTC share features with mandated benefits such as employer-provided health insurance (Summers, 1989). The impact on equilibrium prices and quantities, as well as on tenant welfare and landlord profits, will depend both on the elasticities of supply and demand for rental housing and on the shifts in the supply and demand curves generated by the policy.

Housing is sold in a competitive market. Before RTC is introduced, the market is described by a supply curve $S(\cdot)$ and a demand curve $D(\cdot)$, yielding an equilibrium price p^* and quantity q^* of housing: $p^* = S(q^*) = D(q^*)$. RTC potentially induces a shift in both the supply of and demand for housing. Let $S'(\cdot)$ and $D'(\cdot)$ denote post-RTC supply and demand, leading to a new equilibrium (p', q'). Given that RTC benefits tenants in housing court and imposes additional costs on landlords through the eviction process, it is plausible that both curves weakly shift upward: $S'(q) \geq S(q)$ and $D'(q) \geq D(q)$ for all q. Economic theory does not tell us the magnitudes of these shifts, nor whether supply or demand shifts by a greater amount.

One possibility is that since the NYC housing stock is approximately fixed in the short run, the supply of housing is perfectly inelastic. Panel A of Figure 1 illustrates this special case. Because quantity is fixed at q^*, prices adjust by the marginal tenant's willingness-to-pay for RTC, regardless of how landlord costs shift. The impact on tenant welfare depends on whether the marginal tenant values protections more or less than inframarginal tenants, and

[6] The right to legal counsel has also been introduced in other areas of civil law. See for example http://civilrighttocounsel.org/map.

[7] For a list of jurisdictions and details on each program, see NCCRC, 2024.

is zero if all tenants value protection equally.

However, it could be that effective housing supply is somewhat elastic in the short run, even if it takes a long time for the physical stock of housing to adjust. Landlords can keep units off the market, or post a higher price and wait longer to find a suitable tenant. The remaining panels of Figure 1 illustrate the other extreme case of perfectly elastic supply. Now the relative magnitudes of landlord costs and tenant benefits from the policy become relevant. If benefits in eviction court have insurance value for tenants because they are provided in high-marginal-utility states of the world, they may be worth more to tenants than the actuarial cost to landlords. Figure 1B illustrates this case. Here, the net welfare impact on tenants is positive because quantity rises to meet demand, but prices increase by a smaller amount than tenants' willingness-to-pay. Figure 1C illustrates the intermediate case in which supply and demand shift by equal amounts. In this case, the policy is neutral in terms of both consumer and producer surplus. Finally, RTC may impose costs on landlords that are not directly transferred to tenants, for example through additional legal fees and the time and hassle costs of a longer court process. Costs to landlords may, therefore, exceed tenants' ex-ante willingness to pay for the benefits of RTC. Panel D of Figure 1 illustrates this case. Tenant welfare falls because the price increase (equal to landlord costs) is greater than tenants' value of the protections.

Section 4 develops an approach to evaluating the impact on tenant welfare which accommodates all of these theoretical possibilities.

2.3 New York City's Universal Access to Counsel policy

NYC's right-to-counsel program, officially named "Universal Access to Counsel" (UAC), was signed into law in August of 2017. The goal was for the city to provide access to legal counsel to all eligible tenants by 2022 (Been et al., 2018). The program was phased in over time on a ZIP code-by-ZIP code basis (see Appendix Figure B.1). The program expanded on a 2016 "Expanded Legal Services" pilot already operating in some ZIP codes at a smaller scale. Following Ellen et al. (2021) and Cassidy and Currie (2023), we do not consider a ZIP code treated until part of the UAC program (see Appendix C.7 for details on the pilot and robustness to the handling of pilot ZIP codes). ZIP codes were included earlier in the rollout based on the availability of other legal service programs, eviction rates, the prevalence of rent-regulated housing, the volume of entries into homeless shelters, and other factors of need (Been et al., 2018). The full formula is not made available to the public. RTC was introduced in three ZIP codes in each of the five boroughs in October 2017; to one ZIP code in each borough in November 2018; and to five additional ZIP codes in December 2019 (Ellen et al., 2021; Office of Civil Justice, 2018, 2019, 2020).[8] The COVID-19 pandemic and subsequent federal eviction moratorium paused the rollout of RTC. In 2022, RTC expanded to all ZIP codes in NYC.

Table 1 shows that treated ZIP codes differ from the rest of NYC in terms of household income, rent prices, eviction rates, and demographics. However, these ZIP codes look quite similar to neighboring ZIPs that were not treated until the full rollout. Thus, while treated ZIP codes are not necessarily comparable to the rest of the city, neighboring ZIP codes may serve as plausible controls. Appendix Table A.2 compares listings posted before the rollout of RTC near the border between Treatment and Control ZIPs. This table shows that these listings are, on average, even more similar in terms of observable characteristics.

During the rollout of RTC, eligibility for free legal counsel was determined by a tenant's ZIP code, the type of eviction case, and their household income. All tenants residing in treated ZIP codes, facing holdover or nonpayment eviction cases, and whose household income was below 200 percent of the federal poverty guideline were eligible for free, full representation. Tenants facing eviction cases would often learn of their eligibility at the courthouse, where signage would inform them of the existence of the program.

Even before the rollout of RTC, NYC had more stringent rental housing regulations than most US cities. About half of rental units were subject to rent control or rent stabilization policies.[9] The presence of rent control or rent stabilization may affect landlords' incentives to evict as well as tenants' ability to find new housing if evicted (Diamond et al., 2019). Tenants in rent-stabilized units in NYC are granted an automatic right to renew their lease. Additionally, landlords may only evict rent-stabilized tenants if the landlord (or a family member) intends to use the unit for personal use, or if the tenant has failed to pay rent or committed another lease violation. A 2019 law further restricted the circumstances under which landlords of rent-stabilized units could evict occupying tenants. These restrictions in NYC may lead to a different composition of eviction cases compared to other cities. The fact that NYC regulates rental housing more than most US cities may have implications for the external validity of our results. Our findings are more likely to extrapolate to other cities with strong rental market regulations or tenant protections.

2.4 Data sources and sample construction

In this section we discuss our data sources and sample construction choices. Further details on the datasets, their coverage, and how they were cleaned are provided in Appendix A.

[8] See Appendix A.1 for the specific ZIP codes and additional details.

[9] Other cities with some form of rent control or stabilization are Washington, D.C., and several cities in California, Maryland, and New Jersey.

2.4.1 Data on rental listings

As mentioned in the introduction, one major challenge in studying the impacts of RTC on rent is that microdata on rent prices are scarce.[10] To address this challenge, we use rental listing data from StreetEasy, a widely used platform for listing rental units in NYC. StreetEasy provided a dataset of geolocated rental listings in NYC between 2007 and 2020. We exclude listings after 2019 due to the COVID-19 pandemic. We drop observations missing geographic coordinates or that map to ZIP codes outside of NYC. The dataset includes the listed rent price, indicators for various unit and building characteristics, and the dates the listing was posted and removed. Appendix A.2 provides additional details on the listings data and our sample construction.[11]

2.4.2 Parcel data, building permits, and habitability inspections and violations

In parts of our analysis we use the Primary Land Use Tax Lot Output (PLUTO) tax lot data. The PLUTO data contains parcel-level shapefiles, property assessment data, and parcel-level characteristics collected from multiple city agencies. Parcels correspond to one or more buildings. We drop parcels with no residential units. We use these linked panel data to identify parcels that were subdivided into condominiums, or that were consolidated.

We merge the parcel-level data with three other data sets. First, we merge in the StreetEasy listings dataset using a spatial join in QGIS. For each listing in StreetEasy, we find the nearest parcel in the 2016 PLUTO shapefile. We use the merged dataset to measure parcel characteristics for the rental listings, as well as the number of rental listings in each parcel and year. We exclude listings that map to buildings completed after 2017 or to parcels with zero residential units in the PLUTO data.

Second, we merge in data on building permits from the Department of City Planning's (DCP) housing database to measure changes in the housing stock. This dataset includes information on new buildings, major alterations, and demolitions, which we merge with the parcel-level panel using filing year and parcel identifier (BBL). We then use the merged dataset to study the impacts of RTC on permits that could be associated with repurposing rental units, such as major alterations or demolitions.

Third, we merge onto the parcel-level panel a dataset of habitability inspections and violations. The Department of Housing Preservation and Development (HPD) orders building or unit inspections in response to complaints made online, via 311, or at Code Enforcement

[10]For example, one of the most commonly used publicly collected data sources on rent is the American Community Survey, but information at the ZIP code level is only available in aggregate and averaged over a five-year window.

[11]One limitation of the data is that listed rents may differ from the final contracted rent. For example, the initial listed rent may be higher than contracted rents for units that are on the market for several weeks (Apartment List, 2024).

Borough Offices. If inspectors find a violation, then a violation notice is issued. We distinguish between violations that are rent-impairing or not, following the New York State Multiple Dwelling Law Section 302 definition (i.e., "a condition within a multiple dwelling which constitutes, or if not promptly corrected will constitute, a fire hazard or a serious threat to the life, health or safety of occupants thereof"). We then use the merged data to study if RTC affected livability, measured as the number of violations. See Appendix A for additional details on the datasets and the cleaning process.

2.4.3 Court records linked to quarterly wage income

We draw on data from Collinson et al. (2024) capturing the earnings history of a sample of low-income individuals in NYC with and without an eviction case. The basis for this data is administrative records of every individual who at some point received SNAP (food stamps), Medicaid, or federal or local cash assistance in NYC from 2004 to 2016. The data capture more than 2 million low-income adults each year. From this benefits data, we use a sample of all recipients that could be linked to an eviction case, which we refer to as the "eviction case sample." As detailed in Collinson et al. (2024), the eviction case sample captures a high proportion of eviction cases and is broadly representative of eviction cases in NYC. The eviction case data also contain details of each case, such as the case outcome, the address of the case, whether the case was brought due to non-payment of rent or other lease violations, the amount of unpaid rent claimed by the landlord, and filing date. The second sample, or the "no eviction sample", is a 15 percent random sample of benefits recipients *not* linked to any eviction case.

Both of these samples are then linked to quarterly earnings records from the New York State Department of Labor (NYSDOL) from 2004-2016. NYSDOL data include quarterly earnings and detailed industry codes (six-digit NAICS) and cover approximately 97 percent of New York State's non-farm employment, but they do not capture private household workers, student workers, the self-employed, or unpaid family workers. We adjust earnings figures with the consumer price index for the New York Metropolitan Area and report earnings values in 2016 dollars.

These samples are well-suited for studying the income risk faced by low-income renters at risk of eviction. We estimate that in a given year, roughly two-thirds of residents and 70 percent of renters in the highest-eviction ZIP codes appear in the historical benefits data. This data also contains address histories and demographic details that are not typically available in quarterly earnings records alone.

We make several sample restrictions to these data for the analysis described in Section 5. First, to capture the most proximate pre-RTC period, we restrict the "eviction case sample" to cases filed from 2011-2015. Second, we limit the eviction case sample to the first calendared non-payment eviction case in private housing (i.e. excluding public housing) that we observe in our data.[12] Third, we drop renters in both samples whose latest pre-filing address is in Staten Island. To ensure there is a similar distribution of calendar year-quarters in the linked earnings records for both samples, we randomly assign placebo eviction dates in "no eviction sample" to match the distribution of actual eviction filing dates in the "eviction case sample." We then organize the earnings panel in quarterly event time relative to the actual or placebo eviction filing date for each individual. Finally, we restrict both the "eviction case sample" and the "no eviction sample" to individuals who are between 18 and 55 at the time of eviction/placebo filing.

2.4.4 NYC Housing and Vacancy Survey

In our welfare analysis, we use statistics from the New York City Housing and Vacancy Survey (NYCHVS), a representative survey of NYC residential housing units conducted triennially by the US Census Bureau. We use the 2017 NYCHVS and limit the sample to renters who are not living in public housing and who live in PUMAs that overlap with RTC-treated ZIP codes, leaving a sample of $3,050$ renters. We use this sample to estimate the joint distribution of rent and income for RTC-treated ZIP codes, which is an input to our welfare quantification.

2.4.5 Infutor migration and Experian credit report data

Finally, to evaluate whether there are changes in migration patterns into RTC-treated ZIP codes (e.g., due to rent increases, tenants valuing the RTC program, or changes in landlord screening), we use Infutor data on in-migrants to NYC neighborhoods linked to individual credit records from Experian. The Infutor data are consumer reference data that provide address histories for a large fraction of US adults and have reasonably good coverage of low-SES individuals (Phillips, 2020). As described in Appendix A, our sample draws from all valid Infutor records of individuals moving to an address in NYC between 2014 and 2019. These records are linked by Experian to anonymized credit report data for these individuals on a semiannual basis.

Credit records are frequently used in the tenant screening process and are available for over 90% of adult US residents (Gibbs et al., 2025). We also merge in 5-year ACS data for the origin Census tract for each sampled individual's move to NYC. Our analysis focuses on the latest credit report data and ACS tract observed prior to the move to NYC, to study how RTC affects migration into treated ZIP codes.

[12] Some eviction filings are never calendared, meaning that the landlord does not pursue the case after the initial filing.

3 Landlord responses to RTC

3.1 Research design

NYC's right-to-counsel program was rolled out by ZIP code over time, prioritizing ZIP codes with high eviction rates and balancing the number of ZIP codes included across boroughs. Due to capacity constraints, only a small number of ZIP codes were added to the program each year, leaving many ZIP codes with similarly high eviction rates that were not included and that may serve as a potential comparison group. Indeed, although ZIP code selection was not random, as can be seen in Table 1, untreated ZIP codes neighboring the treated ones look observably similar to the treated ZIP codes. Despite this observed similarity, one potential concern in comparing treated ZIP codes to their untreated neighboring areas is differences that are not captured in our data. For that reason, we study the program's impacts using a difference-in-differences (DiD) approach, comparing treated areas to untreated areas, since DiD can accommodate unobserved time-invariant differences between treated and untreated locations. DiD recovers the average treatment effect of the partial rollout on treated housing units under the standard assumptions of (i) parallel trends and (ii) no spillovers from treatment onto the control units (i.e., no SUTVA violations).

To mitigate concerns about the plausibility of the parallel trends assumption, we use a calipered DiD design that compares rental units near a ZIP code border before and after one side of the border is treated. One advantage of this approach is that we do not need to assume that the parallel trends assumption holds across a large geography, such as all of NYC, but only for units near a particular ZIP code border.

In our setting, the no-spillovers assumption implies that the treated units do not affect outcomes, such as listed rent prices, in the control group. As discussed in Section 2.2, the RTC policy may shift supply back while shifting demand out in treated neighborhoods. Assuming nearby units in untreated neighborhoods are substitutes, the sign of potential spillovers on rent prices is ambiguous. A decrease in supply in treated neighborhoods could redirect demand toward control neighborhoods (raising prices), while an increase in demand in treated neighborhoods could draw demand away from control neighborhoods (lowering prices).[13] In Section 3.2, we discuss robustness exercises that explicitly account for potential spillovers.

Lastly, one additional concern applies only to our analysis of rental listing data. While

[13]The relative size of either effect in control neighborhoods will depend on how substitutable rental units are in the two neighborhoods, the size of the demand and supply shifts, and whether the treated neighborhoods account for a large share of the close substitutes for control neighborhoods (which is unlikely in our setting given that a small share of NYC ZIP codes were treated during the partial roll-out). For further evidence on substitution across neighborhoods, Appendix Table C.15 finds null or economically small migration responses to RTC in our linked Infutor-Experian data.

our other outcome data are observed for every relevant unit and time period, for the listings data we may be concerned that RTC affects the composition of units that are listed on the StreetEasy platform. Our estimates could be biased if RTC causes differential selection into listing units on the platform across treated and control areas based on unobserved factors correlated with rent prices. While we control for observable unit characteristics, we may inadequately control for, e.g., the unobserved quality of the unit. Below, we test for and do not find evidence of RTC-driven differential selection onto the platform based on the observed characteristics of listed units.

Since we have multiple treated ZIP codes, there are many potential borders on which to run our calipered DiD analysis. Our strategy is to estimate these effects jointly by constructing a stacked dataset, recovering a single estimate that is a weighted average of treatment effects for treated housing units. We build a dataset of ZIP code border pairs $b = (z, z')$, dropping pairs where both sides were never treated or were treated in the same year, and pairs where the treated side was treated in or after 2019 (due to insufficient pre-COVID listings). For borders where one side was treated one year after the other, we exclude listings created after both sides were treated to address potential bias from comparing early- and late-treated units (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). We then subset to listings within 1000 meters of each border pair, excluding border pairs where the nearest listing is more than 300 meters from the border on either side—which in practice excludes borders with large natural barriers such as highways or parks—or where fewer than 50 observations or no pre- or post-period observations exist on either side. Finally, we restrict to Manhattan, Brooklyn, the Bronx, and Queens, dropping Staten Island due to its low rental housing share and few listings near treated and control borders.

We use this stacked dataset to estimate the calipered difference-in-differences regressions. We denote the outcome of interest by Y_{ibt}, where t denotes time relative to treatment, b denotes the border, and i denotes the rental listing.[14] D_{ib} is defined as an indicator that equals 1 if listing i is on the treated side of border b; $Post_{ibt}$ as an indicator equal to 1 if the listing i is posted in relative year t after treatment; ϕ_b as a fixed effect for border b; and X_i as controls for other characteristics of listing i. We estimate the regression equation

$$Y_{ibt} = \phi_b + \beta D_{ib} + \theta_t \text{Post}_{ibt} + \delta_t(\text{Post}_{ibt} \cdot D_{ib}) + X_i'\gamma + U_{ibt}. \tag{1}$$

We also estimate more flexible versions of (1) which allow β and θ to vary across border pairs:

$$Y_{ibt} = \phi_b + D_{ib}'\beta_b + \text{Post}_{ibt}'\theta_{tb} + \delta_t(\text{Post}_{ibt} \cdot D_{ib}) + X_i'\gamma + U_{ibt}. \tag{2}$$

[14] Y_{ibt} includes a b subscript as a small number of observations can contribute to multiple border pairs.

Equation (2) allows border pairs to differ in their pre-RTC difference in rents and other outcomes across the border (β_b), and in their control group time trend (θ_{tb}). We refer to specifications based on this equation as "flex."

Our estimates of δ_t are weighted averages of treatment effects for treated units across border pairs in relative period t. Estimates are relative to the year prior to treatment. We cluster standard errors at the border level to account for potential spatial correlation, with 57 clusters in total.

3.2 Impact on listed rent prices

Table 2 reports RTC's listed rental price impacts 1 and 2 years after its introduction. In the first year after treatment, we estimate an increase of 12.51 dollars (95% CI: [1.3,23.8]) using the calipered DiD specification, and a smaller and statistically insignificant increase of 6.47 (95% CI: [-6.5,19.4]) dollars using the flex specification. In the second year, we estimate larger rent increases of 38.11 (95% CI: [22.9,53.3]) and 29.13 (95% CI: [13.3,45.0]) dollars using the same two specifications, which are 1.2 to 1.6 percent of the pre-period mean ($2,440). In Figure 3, we find no evidence of differential pre-trends in posted rent prices for treatment and control neighborhoods, though our pre-period standard errors are somewhat large, which we discuss more below.

Several factors may explain the larger second-year estimates. First, landlords may not immediately internalize program costs, and instead learn about them over time. Second, housing supply is more elastic over longer horizons, and repurposing rental units requires planning. Third, tenants may take time to learn about the value of the policy, which would initially mute demand increases.

The results above describe a single effect (δ_t) for each year post-treatment, which may mask underlying heterogeneity. Figure 4 plots how the year 2 estimates vary by the baseline eviction filing rate in the treated ZIP (averaged over 2011-2015). To do this, we estimate an alternative version of equation (2) which allows the effect to vary by ZIP code border pair. On average, the estimated impact on prices is larger for border pairs where the treated ZIP code had higher baseline eviction rates. Although the difference is not statistically significant (see Appendix Figure C.6), it is consistent with larger increases in rent occurring in places where landlords and tenants were likely more affected by the policy.

Robustness. Our results are robust to a wide range of alternative specification choices and sample restrictions.

Appendix Figure C.5 shows that the impacts we estimate are similar to what we would expect based on the time series of unadjusted mean rental prices for listings near the treatment/control ZIP borders. Appendix Table C.1 shows the estimated impacts on rent are similar when considering log rent. The results suggest a 0.2-0.4 percent increase in rent in year 1, which increases to a 1.2 to 1.5 percent increase in year 2.

To address concerns about spillovers, Appendix Table C.6 includes regression specifications that exclude listings closest to the ZIP border. The specifications labeled "one-sided 250m donut" and "one-sided 500m donut" exclude control observations within 250 meters and 500 meters of the ZIP border, respectively. The specifications labeled "two-sided 250m donut" and "two-sided 500m donut" do the same for control *and* treated units. Compared to the main specifications, point estimates are generally larger in year 1 and smaller in year 2. The flex specifications are also more noisily estimated because there are fewer observations and border pairs when we drop the observations closest to the ZIP border. Nonetheless, most estimates from the donut specifications remain statistically significant in year 2. Since the sign of the spillovers is theoretically ambiguous (see footnote 13 above), we cannot tell whether the donut reduces or introduces bias. These specifications drop nearby control units to mitigate concerns about spillovers, but rely on more distant, potentially less comparable units as the remaining controls. Still, it is reassuring that we obtain similar estimates using these alternative sample criteria.

Our results are also robust to our choice of bandwidth for the caliper. Appendix Table C.2 shows results when reducing the bandwidth to 500 meters or increasing it to 1500 or 2000 meters. The results are always positive and broadly similar, although point estimates are somewhat smaller for the first year when using the 500 meter bandwidth.

Results are robust to other sample restrictions. Appendix Table C.3 compares estimates from our analysis sample to estimates from (i) the full sample (i.e., including apartments with central AC, gyms, doorpersons, or pools), (ii) our analysis sample restricted to units that are likely not rent-stabilized,[15] (iii) our analysis sample restricted to parcels where the pre-RTC annual ratio of eviction filings to number of units in the parcel is at least .05, and (iv) our analysis sample dropping units listed one month before or after the policy went into effect, motivated by potential anticipation effects or time taken to scale up.[16] When using the full sample, estimates are somewhat smaller in year 2, as we would expect if units with amenities such as pools, gyms, and doorpersons are less likely to be affected by the policy change. Estimates are somewhat larger when excluding apartments likely to have rent control, though standard errors are larger (likely due to the smaller sample). Results are also similar when restricting to parcels with high baseline eviction rates or when dropping the month before and after the start of the policy.

[15] We impute rent stabilization status according to the NYC Rent Guidelines Board. Rent stabilized listings are those in buildings built before 1974, with at least 5 units and with rent below certain thresholds, according to the NYC Rent Guidelines Board.

[16] It is difficult to fully rule out anticipation, as both treated and neighboring ZIP codes may have anticipated that they could be selected and changed how they priced rental units. Such anticipation effects would likely result in smaller estimated impacts on rental prices than the true effects.

Estimates are largely robust when considering alternative control groups. Appendix Table C.4 shows specifications that (i) exclude ZIP codes treated in the following year from the control group or (ii) exclude all ZIP codes that are eventually treated before 2022. Results are not sensitive to these restrictions.

To test whether RTC changed the characteristics of units listed on StreetEasy, we estimate the impact of RTC on the hedonic component of rent prices. First, we regress prices on covariates from our StreetEasy and PLUTO data in the pre-period.[17] Using the coefficients from the hedonic regression, we calculate predicted rent for all listings in our analysis sample. We then run our DiD specifications on predicted rents. As shown in Appendix Table C.5, we find no evidence that treatment differentially changed selection into listing on the platform. The predicted rent estimates in Table C.5 are small and statistically indistinguishable from zero. Moreover, our estimated impacts on rent from Table 2 fall outside the 95% confidence intervals for three of four main results, with the exception only narrowly inside the interval. These results suggest that differential trends in listing quality on the platform do not drive our findings. We also show that our estimates are similar when including a larger set of hedonic controls (cf. Appendix Table C.7).

Appendix Section C.3 provides additional robustness to which border pairs are included in our analysis. If we restrict our sample to border pairs with a ZIP code treated in 2017 and a control ZIP treated in 2019 or later (and therefore identical samples in year 1 and year 2) our results are largely unchanged. A related concern could be that certain border pairs have more observations in later years. Our results are also largely unchanged if we reweight our data to account for potential compositional changes in the sample over time.

Lastly, to assess robustness of our findings to potential violations of parallel trends, we implement sensitivity analyses from Rambachan and Roth (2023) (cf. Appendix Section C.6). Specifically, we report confidence regions as a function of the bound $\bar{M}$ that scales allowable post-period trend changes by the largest adjacent pre-period change. For our calipered DiD specification, the 90% confidence intervals exclude zero when $\bar{M} \leq 0.7$ in year 1 and $\bar{M} \leq 0.5$ in year 2. Although these thresholds are relatively low, they are driven primarily by large standard errors rather than by large deviations from parallel trends in the pre-period coefficients. Overall, the results highlight that we should interpret our DiD estimates with some caution, as imprecise pre-period estimates limit our ability to reject that trends were parallel in the pre-periods (and may therefore have been parallel in the post-period absent treatment).

Appendix Figure C.11 summarizes the quantitative relevance of our robustness exercises

[17] We use data from 2016 to predict rent prices using a regression of listed rent on building year and building year squared, unit square footage, number of bedrooms, number of bathrooms, and indicators for whether the parcel has an elevator, whether the parcel has a garage, whether the parcel is mixed use, and whether the unit has laundry, dishwasher, or central air conditioning.

by plotting the point estimate and 95% confidence interval obtained from each year 2 rent effect specification. A few patterns are apparent. First, our main rent effect point estimates ($29-$38) are in the middle of the range obtained across all specifications ($20-$50). Second, several alternative specifications have wider confidence intervals, often because they further limit the sample of listings used in estimation. Third, all of the point estimates are positive, and a large majority are statistically significant at the 5 percent level. Overall, while the finding that RTC increased rent prices is qualitatively quite robust, the magnitudes of the estimated rent effects do depend considerably on specification choice.

3.3 Quantity responses of rental housing supply

Section 4 develops a framework in which our price effect estimates are sufficient for quantifying the tenant welfare costs of the RTC policy. Though we emphasize that measuring quantity responses is not needed for the welfare analysis, estimates of RTC's quantity effects shed light on one possible mechanism driving RTC's price effects: a contraction in supply.[18] As discussed in Section 2.2, RTC's impact on the market-clearing quantity is ambiguous given that the program likely increased benefits for tenants (increasing demand) and increased costs for landlords (decreasing supply).

We focus on the margins of quantity we believe we can measure most accurately: condo conversions and building permits that could indicate the re-purposing of existing rental units (Diamond et al., 2019).[19] We measure quantity outcomes during 2015-2019 using PLUTO data and publicly available building permit data. For all outcomes, we first compute counts at the parcel level and then rescale them by the number of units in the parcel.

We estimate specifications in which an observation is a parcel instead of a rental listing. As in our prior notation, Y_{ibt} is the outcome of interest, D_{ib} is an indicator that equals 1 if parcel i is on the treated side of border b, $Post_{ibt}$ is an indicator that equals 1 in relative year t after treatment, and ϕ_{ib} is a parcel-by-border fixed effect:

$$Y_{ibt} = \phi_{ib} + \theta_t \text{Post}_{ibt} + \delta_t(\text{Post}_{ibt} \cdot D_{ib}) + \eta_{ibt} \, . \tag{3}$$

Table 3 reports estimates for year 1 in the top panel and year 2 in the bottom panel. The first column reports results for the stock of condos divided by the number of units in the building. We find a small but statistically significant ($p < 0.01$) increase in the number of condominiums, representing a 2-3 percent increase in the number of condos per unit by the

[18] Section 4 describes in further detail how quantity responses fit into our welfare framework and why the price estimates are sufficient for RTC's tenant welfare costs.

[19] As discussed in Appendix C.4, some quantity measures, such as vacancy durations and the number of available rental units, are difficult to measure accurately, especially at high frequency with geographic granularity.

second year.[20]

The last three columns of Table 3 report how RTC affects large construction permits. Again using equation (3), we consider whether the parcel had any permit for major alterations, new construction, or demolitions. We find evidence of an increase in permits for major alterations and new construction in year 1, and evidence of an increase in permits for new construction and demolition in year 2. These permits are, in general, uncommon, with 0.5 percent of parcels having a major alteration in the pre-period. In the first year, permits for major alterations increase by 0.07-0.1 percentage points, a 14-18.5 percent increase. We similarly find small but proportionally large increases in new construction permits in both years, and in demolition permits in the second year.

Overall, we find suggestive evidence that RTC modestly reduced rental housing supply along the margins we can best measure. If true, it suggests that landlord costs from RTC were at least as large as the estimated price increase.[21] Though we do not estimate RTC's costs to landlords, there are several potential sources that could generate cost increases on the order of the estimated \$29-\$38 rent increases. These include additional default by tenants, legal fees, and time and hassle costs for the landlord (cf. Appendix Section D.6).

3.4 Other margins of adjustment

The introduction of RTC could lead landlords to adjust on other margins, with implications for the welfare framework we develop below. We consider three such margins: maintenance, new tenant screening, and filing eviction court cases.

First, RTC could change landlords' willingness to maintain unit quality. Since it is difficult to obtain data on maintenance activities, we use public data on housing inspections and substantiated housing-code violations. We find that inspections and violations increased in treated ZIP codes in the second year after RTC was introduced (see Appendix Table C.16), consistent with a decline in unit quality. However, inspections and violations could also increase if tenants became more likely to report issues, which makes interpreting these results more challenging.[22] We further show that there was no compositional shift toward minor violations, weighing against the reporting explanation (see Appendix Section C.9 for details). Overall, we find no clear evidence that RTC *increased* unit quality; if anything, the results

[20] Appendix C.1 provides additional evidence of parallel pre-trends for the non-price outcomes considered in this section. The pre-trends in these figures largely support the parallel trend assumptions. One exception is that we find evidence of diverging pre-trends in condo conversions one year (but not 2 years) prior to RTC implementation.

[21] This is the case if RTC is a small enough shift in costs that demand and supply can be treated as locally linear.

[22] Note that if we had instead found decreases in inspections and violations, this would indicate an improvement in unit quality even if tenant willingness to report a complaint about any given issue also increased.

point to a decline. In developing our welfare framework, we abstain from including a channel for habitability changes, which is helpful for tractability. A decline in quality missed by this approach would imply that the tenant welfare impact of RTC is more negative than our analysis suggests.

Second, RTC could reduce landlords' willingness to approve rental applications from tenants with higher perceived eviction risk. We investigate such screening responses in the merged Infutor-Experian data described in subsection 2.4.5. Using difference-in-differences specifications analogous to equation (1), we test for changes in the composition of in-migrants to RTC-treated ZIP codes along dimensions such as credit score, indebtedness, and prior-census-tract characteristics. We find little evidence of a substantial change in tenant composition (cf. Appendix C.8 and Table C.15). Granted, RTC could affect tenant composition through tenants' (voluntary) sorting across neighborhoods as well as through screening. If this sorting were to offset the effects of increased screening, our analysis of tenant composition would understate the screening response to RTC. This screening would present an additional cost to (some) tenants not accounted for in our welfare framework.

Third, the decision to file an eviction case could adjust. If RTC makes landlords more hesitant to file, tenants would additionally benefit from being more likely to receive forbearance and avoid the costs of the eviction process when they default. Cassidy and Currie (2023) do not find significant changes in the number or composition of eviction cases due to RTC, perhaps reflecting a shortage of alternative options for NYC landlords at the point when they typically file evictions. Based on this evidence, we develop our welfare framework assuming that RTC does not change how landlords make case filing decisions.

4 Model of RTC's impacts on tenant welfare

The prior section provides evidence that while RTC benefits tenants in eviction court, the program also likely raised rent prices. Without further structure, it is difficult to determine if the benefits from RTC outweigh the higher costs of housing. This section provides a framework to evaluate whether RTC is welfare-improving for tenants. We derive an expression for the change in welfare due to a marginal increase in tenant protections that depends on a small number of empirical quantities.[23] Our model and welfare formula are closely related to the analysis in Dávila (2020) of optimal bankruptcy exemptions, but adapted to match features of the rental housing market and policy environment.

Section 4.1 presents the model. Section 4.2 derives an expression for the marginal change in tenant welfare due to increased tenant protections and shows how this impact can be expressed in terms of observable quantities. Section 4.3 explains the intuition behind our

[23]See, e.g., Chetty (2009) for a review of this "sufficient statistics" approach.

approach and discusses its advantages and limitations. Section 5 then estimates RTC's tenant welfare impact using eviction court records linked to administrative and survey data.

4.1 Model setup

We begin with a set of ex-ante identical renters. A renter chooses an amount of housing h before learning their uncertain income $y \sim F(\cdot)$. Their preferences over housing and numeraire (c) consumption are

$$U(c, h) = u(c) + v(h),$$

where $u(\cdot)$ and $v(\cdot)$ are increasing, concave, and differentiable functions with $\lim_{c\to 0} u'(c) = \infty$.[24]

A rental contract specifies a per-period rent $R(h, \tau)$ in exchange for quantity of housing h, with $\tau \in [0, 1)$ denoting the strength of eviction protections.[25] After choosing a housing quantity, the renter's income y is realized for the duration of their lease and they choose the fraction of the lease term $f \in [0, 1]$ during which to pay rent. If they do not default ($f = 1$), they consume the value h of their rental housing for the full term of their lease, and $y - R(h, \tau)$ of the numeraire. If the renter defaults ($f < 1$), they may remain in the unit for the share f of the lease term during which they pay rent, plus the additional time τ required to complete the eviction process. The renter therefore enjoys housing utility $v(h)$ for $\min\{f + \tau, 1\}$ periods, and their outside option $v(0)$ for the remainder of the lease. We refer to tenants as "evicted" if they have an eviction case *filed* against them, and as receiving an "eviction order" if the judge grants an order for possession to the landlord (i.e. makes a judgment against the tenant). An evicted renter also incurs a utility cost C from having an eviction case filed against them; this cost does not depend on the policy.[26]

In line with the evidence in Cassidy and Currie (2023), eviction protections are modeled as benefiting tenants through two channels. First, they affect the amount of time τ a tenant may remain in their unit after defaulting. Second, they impact the probability $p_j(\tau)$ of the case ending in an eviction order, which may have an additional impact on a tenant's income and housing circumstances (Collinson et al., 2024).[27] We model the impact of an eviction order as an additional drop y_j in income and an increase h_j in time homeless. This allows legal representation to directly reduce the costs of the eviction process by lowering the likelihood

[24]We assume utility is separable in housing and non-housing consumption for tractability, though this assumption is common in the literature. For example, see Iacoviello (2011).

[25]In what follows, we suppress dependence of R and other variables on h and τ where convenient.

[26]For example, C may include the stigma from having an eviction case on the tenant's record.

[27]We do not incorporate lower money judgments into the analysis. Though RTC reduced money judgments for represented tenants, recovery rates on money judgments are quite low.

of an unfavorable court outcome. We assume tenants' housing and default choices correctly anticipate the potential costs from the eviction process given the policy environment.[28]

We now characterize tenants' optimal default and housing choices.

Default Choice: Given h, R, and income realization y, the default choice $f(y;h,\tau)$ solves

$$\begin{aligned}\max_{f\in[0,1]} & u(y-fR)+\min\{\tau+f,1\}[v(h)-v(0)]+v(0) \\ & -\mathbb{1}\{f<1\}[C+\underbrace{p_j(\tau)h_j[v(h)-v(0)]+p_j(\tau)(u(y-fR)-u(y-fR-y_j))}_{\text{Impact of an eviction order on time housed and income}}].\end{aligned} \tag{4}$$

The first line of equation (4) reflects the tenant's housing and numeraire consumption in the absence of the direct costs of the eviction process; the second line includes the fixed cost of the eviction case C and the probabilistic drop in housing and numeraire consumption due to an eviction order.

Because utility from the numeraire $u(\cdot)$ is concave while utility from time in the unit is linear in the amount paid up to $f=1-\tau$, the optimal rent share $f(y;h,\tau)$ is weakly increasing in income y. There are three possible cases for the tenant's solution to equation (4).

1. **Full Default:** $f=0$ and the tenant receives utility

$$u(y)+(\tau-p_jh_j)[v(h)-v(0)]+v(0)-C-p_j[u(y)-u(y-y_j)]\,.$$

A necessary condition for $f=0$ to be optimal is $[p_j(\tau)u'(y-y_j)+(1-p_j(\tau))u'(y)]R(h,\tau)\geq v(h)-v(0)$. This means the renter would rather spend additional income on numeraire consumption rather than on additional time in the unit. Let $y_0(h,\tau)$ be the highest income realization at which full default is optimal.

2. **Partial Default:** $f\in(0,1-\tau)$ and the tenant receives utility

$$p_ju(y-fR-y_j)+(1-p_j)u(y-fR)+(\tau+f-p_jh_j)[v(h)-v(0)]+v(0)-C\,.$$

A necessary condition for optimal partial default is that the tenant is indifferent between spending money on rent and the numeraire:[29]

$$[p_ju'(y-fR-y_j)+(1-p_j)u'(y-fR)]\;R(h,\tau)\;=\;v(h)-v(0)\,. \tag{5}$$

[28]We write p_j as a function of τ for notational convenience. One may think of both variables as depending monotonically on a one-dimensional policy instrument that varies the strength of protections.

[29]We assume the costs of the eviction process are large enough so that there is no income realization for which it is optimal to pay exactly $f=1-\tau$ and then default.

Note that the tenant's expected marginal utility of numeraire consumption is a weighted average over court outcomes due to the effect of an eviction order on income. In contrast, the marginal utility of additional housing consumption does not depend on the outcome in court. Let $\hat{y}(h,\tau)$ be the income realization below which partial default is optimal.

3. **No default:** $f = 1$ and the tenant receives utility $u(y-R(h,\tau))+v(h)$. The indifference condition for a renter with cutoff income $\hat{y}(h,\tau)$ is

$$\begin{aligned} u(\hat{y}-R)+v(h) = & p_j u(\hat{y}-f(\hat{y};h,\tau)R-y_j)+(1-p_j)u(\hat{y}-f(\hat{y};h,\tau)R) \\ & +(f(\hat{y};h,\tau)+\tau-p_j h_j)[v(h)-v(0)]+v(0)-C\,. \end{aligned}$$

Housing Choice: Anticipating their uncertain income and optimal default choice, a renter chooses housing quantity h to maximize their expected utility. The value of this solution is

$$\begin{aligned} W(\tau) = & \max_h \quad v(0) \\ & + \int_0^{y_0(h,\tau)} \Big[p_j(\tau)u(y-y_j)+(1-p_j(\tau))u(y)+(\tau-p_j(\tau)h_j)[v(h)-v(0)]-C\Big]dF(y) \\ & + \int_{y_0(h,\tau)}^{\hat{y}(h,\tau)} \Big[p_j(\tau)u(y-f(y;h,\tau)R(h,\tau)-y_j)+(1-p_j(\tau))u(y-f(y;h,\tau)R(h,\tau)) \\ & +(\tau+f(y;h,\tau)-p_j(\tau)h_j)[v(h)-v(0)]-C\Big]dF(y) \\ & + \int_{\hat{y}(h,\tau)}^{\bar{y}} \Big[u(y-R(h,\tau))+v(h)-v(0)\Big]dF(y)\,. \end{aligned} \tag{6}$$

The first integral term captures tenant welfare when it is optimal to fully default on rent; the second term corresponds to partial default; and the third term to income ranges where the tenant pays the full rent. A renter trades off the value of additional housing consumption while in the unit against the cost of lower numeraire consumption after paying a higher rent and a higher likelihood of default and eviction.

Eviction protections improve court outcomes and create a gap between the time the tenant pays rent and the time they can spend in the unit. The next section derives an expression for how these features of the right-to-counsel program impact tenant welfare.

4.2 Effects of RTC on tenant welfare

Proposition 1. *The tenant welfare impact of a marginal increase in tenant protections* τ *is given by*

$$\begin{aligned}\frac{dW(\tau)}{d\tau} &= F(\hat{y})\left([v(h)-v(0)]\left(1-\frac{dp_j}{d\tau}h_j\right)-\frac{dp_j}{d\tau}\mathbb{E}_{y<\hat{y}}[\Delta_{y_j}u(c)]\right)\\ &-\frac{dR}{d\tau}\mathbb{E}_y[fu'(c)]\,, \end{aligned} \tag{7}$$

where $F(\hat{y})$ *is the share of renters who face an eviction case,* $\frac{dR}{d\tau}$ *is the rate at which equilibrium rents change due to the policy,* $\frac{dp_j}{d\tau}$ *is the rate of change in the probability of an eviction order,*

$$\mathbb{E}_{y<\hat{y}}[\Delta_{y_j}u(c)] = \frac{1}{F(\hat{y})}\int_0^{\hat{y}}[u(y-f(y;h,\tau)R(h,\tau))-u(y-f(y;h,\tau)R(h,\tau)-y_j)]dF(y)$$

is the mean numeraire utility loss due to income lost by tenants who face an eviction order, and

$$\begin{aligned}\mathbb{E}_y[fu'(c)] = &\int_{y_0}^{\hat{y}} f(y;h,\tau)[p_j(\tau)u'(y-f(y;h,\tau)R(h,\tau)-y_j)+(1-p_j(\tau))u'(y-f(y;h,\tau)R(h,\tau))]dF(y)\\ &+\int_{\hat{y}}^{\bar{y}} u'(y-R(h,\tau))dF(y)\end{aligned}$$

is the expected marginal utility of numeraire consumption, weighted by the share of rent paid.

See Appendix D.1 for a proof. Proposition 1 provides an intuitive expression for the trade-off involved in tenant protections such as right to counsel. Increased protections allow evicted tenants more time in the unit and yield more favorable outcomes in court. Both forces increase tenants' expected housing and numeraire consumption when they default on rent. However, this is weighed against equilibrium rent increases due to the policy, the cost of which depends on the covariance between payment rates and the marginal utility of numeraire consumption. It is worth noting that equation (7) does *not* depend directly on renters' behavioral responses to the policy. If renters choose to rent more expensive apartments and default more often in response to marginally stronger protections, these responses only impact tenant welfare through their equilibrium effect on rents.

Similarly, RTC's impacts on tenant welfare only depend on supply-side market structure through equilibrium rents. This allows us to make statements about tenant welfare that are valid under many assumptions about supply-side and tenant behavior. In particular, $\frac{dR}{d\tau}$ reflects any pricing power held by owners, as well as the elasticities of supply and demand for rental housing. Further, it reflects any additional costs to landlords generated by behavioral responses from tenants, including strategic default. Equation (7) clarifies that while these behavioral responses and equilibrium adjustments can have important welfare implications, they only affect tenant welfare (to first order) through their effect on market prices. Of course, these responses may have separate, first-order impacts on landlord profits, and hence total welfare.

Empirical analogue. Next, we derive an alternative expression that ties equation (7) to measurable quantities. This expression will allow us to quantify the welfare change given an assumption about the degree of tenants' risk aversion over numeraire consumption.

Several objects in equation (7) are straightforward to estimate. From eviction court records and survey data on renters, we can construct the share of renters facing an eviction case $F(\hat{y})$. We can also measure the amount of tenant arrears, and hence f, from eviction claim amounts. We can use estimates of the impacts of RTC on rent prices, eviction case durations, and court outcomes to recover $\frac{dR}{d\tau}$ and $\frac{dp_j}{d\tau}$. Finally, we measure y_j and h_j using estimates of the causal effects of an order for possession on tenant income and homelessness in Collinson et al. (2024). Section 5.1 describes these steps and the required assumptions.

The more difficult quantities to measure are the values of housing $v(h) - v(0)$ and the marginal utilities of numeraire consumption $u'(\cdot)$ for renters in different states of the world. We proceed by leveraging the optimal default condition in equation (5) and an assumption about risk aversion over numeraire consumption, i.e. the shape of $u(\cdot)$. In addition, from here on we assume all tenants pay a nonzero share of the rent ($f > 0$), reflecting requirements such as paying the first month of rent on lease signing or renewal.

To simplify notation, define

$$\bar{u}'(c) \equiv \begin{cases} p_j u'(y - fR - y_j) + (1 - p_j)u'(y - fR), & \text{if } y < \hat{y} \\ u'(y - R), & \text{otherwise} \end{cases} \tag{8}$$

as a tenant's expected marginal utility of consumption. It will also be useful to rewrite the difference in consumption utility due to an eviction order as

$$u(y - fR) - u(y - fR - y_j) = \theta y_j \bar{u}'(c) \qquad \theta \equiv \frac{u(y - fR) - u(y - fR - y_j)}{y_j \bar{u}'(c)}.$$

If $u(\cdot)$ is linear (tenants are risk-neutral), $\theta = 1$. If not, θ may differ from 1 depending on the curvature of $u(\cdot)$ and the values of y_j and $p_j(\tau)$. We can now write

$$\frac{\frac{dW(\tau)}{d\tau}}{v(h) - v(0)} = F(\hat{y}) \left[1 - \frac{dp_j}{d\tau}\left(h_j + y_j \mathbb{E}_{y<\hat{y}}\left[\theta \, \frac{\bar{u}'(c)}{v(h) - v(0)}\right]\right)\right] - \frac{dR}{d\tau}\int_y f(y)\left[\frac{\bar{u}'(c)}{v(h) - v(0)}\right] dF(y) \,, \tag{9}$$

where the terms involving the marginal rate of substitution between numeraire consumption and time in the housing unit are highlighted as the remaining unknown quantities. The first highlighted term depends on renters in default, while the second depends both on tenants who partially default, and on tenants who pay the full rent. Rearranging equation (5), for partial defaulters

$$\frac{1}{R(h,\tau)} = \frac{\bar{u}'(c)}{v(h)-v(0)} \qquad y \in [y_0, \hat{y}) .$$

If all evicted tenants pay some rent, the first highlighted term in equation (9) is simply $\frac{\bar{\theta}}{R}$, where $\bar{\theta} \equiv \mathbb{E}_{y<\hat{y}}[\theta]$ is the expected value of θ among evicted tenants. The remaining challenge is calculating the MRS for tenants who are not evicted. If utility from housing $v(\cdot)$ does not depend on the income realization y and $u(\cdot)$ is known, we can compare the marginal utilities of numeraire consumption of a given non-evicted tenant with realized income y to the average marginal utility of an evicted tenant, and obtain the ratio $\frac{\bar{u}'(c)}{v(h)-v(0)}$ for the non-evicted tenant:

$$\begin{aligned}\frac{\bar{u}'(c)}{v(h)-v(0)} &= \frac{\bar{u}'(c)}{\mathbb{E}[\bar{u}'(c) \mid y<\hat{y}]}\frac{\mathbb{E}[\bar{u}'(c) \mid y<\hat{y}]}{v(h)-v(0)} \\ &= \frac{\bar{u}'(c)}{\mathbb{E}[\bar{u}'(c) \mid y<\hat{y}]}\frac{1}{R} .\end{aligned}$$

After further manipulation of equation (9) described in Appendix Section D.1, we obtain

$$\frac{dW(\tau)}{d\tau} = \frac{F(\hat{y})\left[R - \frac{dp_j}{d\tau}\left(h_j R + y_j \bar{\theta}\right)\right] - \frac{dR}{d\tau}\left[\bar{f}_E F(\hat{y}) + (1-F(\hat{y}))\Omega\right]}{F(\hat{y}) + (1-F(\hat{y}))\Omega}\mathbb{E}[\bar{u}'(c)] , \tag{10}$$

where $\bar{f}_E$ is the average share of rent paid by evicted tenants and $\Omega \equiv \frac{\mathbb{E}[\bar{u}'(c)|y \geq \hat{y}]}{\mathbb{E}[\bar{u}'(c)|y<\hat{y}]}$ is the ratio of expected marginal utilities for evicted and non-evicted tenants.

Equation (10) provides another perspective on the benefits and costs of eviction protection for tenants. The first term in the numerator is the marginal benefit of stronger protections. Each year, a fraction $F(\hat{y})$ of tenants are evicted, and benefit from the additional time they can spend in their house as well as a lower probability of an eviction order against them (and of the associated losses in numeraire and housing consumption). The second term is the marginal cost of stronger protections, and is proportional to the change in rent $\frac{dR}{d\tau}$ due to the policy, but multiplied by an additional term with two components. If all tenants paid the full rent and had the same marginal utility of numeraire consumption, this term would be one. However, this term is generally less than one for two reasons. First, evicted tenants do not pay the full rent; they are shielded from higher rent prices to the extent that they default. This is captured in the term $\bar{f}_E F(\hat{y})$. Second, because tenants who default tend to have lower income realizations than tenants who do not default, the ratio Ω may also differ from one. This term captures the insurance value from RTC. To the extent that non-evicted tenants enjoy higher numeraire consumption than evicted tenants, their utility cost from the rent increase is small relative to the value to an evicted tenant of spending more time in the unit and avoiding negative impacts from eviction. The denominator corrects for the insurance value of RTC's protections, and is simply 1 if tenants have the same value of consumption in the evicted and non-evicted states.

Tenant Rationality. It is worth emphasizing that equation (10) relies on the assumption that tenants are rational and optimally adjust their housing and default choices to RTC. Optimal default plays a particularly important role in the empirical quantification of the welfare formula, because the rent R pins down tenants' marginal rate of substitution between housing and numeraire consumption when they are evicted. Since paying additional rent translates (by assumption) into additional time in the unit, evicted tenants must value additional time housed at $v(h) - v(0) = R \times \mathbb{E}[\bar{u}'(c) \mid y < \hat{y}]$. In contrast, the role of tenant optimization over housing h is more limited; it allows the welfare analysis to ignore behavioral responses in h due to the envelope theorem, but is not otherwise used in the empirical implementation.

Of course, there are many reasons why tenants may not behave optimally either at baseline or after RTC is introduced. Tenants may not fully understand the eviction process, the costs of homelessness and other consequences of an eviction order, or the benefits a lawyer would afford them. They may also have been uninformed about the existence of the RTC program entirely. Anecdotal evidence suggests that some tenants only learned they were eligible for free legal counsel after they arrived at the courthouse.[30] To the extent that these biases are present, tenants may value RTC's benefits, particularly reduced homelessness, differently than we estimate.[31] However, as we discuss in Section 5.2, even for our highest (least negative) estimate of RTC's tenant welfare impact, tenants' true valuation of time housed would need to be more than twice as large as our estimates in order for the program to have increased tenant welfare.

To address the specific possibility that tenants were unaware of the RTC program, we also consider a version of the model in which tenants are unaware of improved court outcomes and (incorrectly) reoptimize against only the change in rents when making their housing and default choices (cf. Appendix Section D.1.1). In this alternative model, Proposition 2 characterizes the theoretical bias that arises relative to Proposition 1. We also derive a

[30]Lack of awareness could partly explain incomplete take-up of legal counsel under RTC, despite the considerable estimated benefits from having a lawyer. Limited availability of RTC lawyers due to funding constraints also appears to have contributed. Note that our baseline model does not rely on *take-up* of legal counsel being optimal; it is simply treated as exogenous. However, we do assume tenants optimally respond to the likelihood they will receive legal counsel and the associated court benefits.

[31]An alternative approach would be to value time housed through estimates of other consequences of homelessness. For example, Collinson et al. (2025) find that eviction leads children to have worse subsequent academic outcomes, including lower high school graduation rates, which their parents may internalize. We are not aware of a comprehensive enumeration of such costs, or estimates of their economic value.

bound on the amount that RTC's benefits for tenant welfare could be *understated* in our empirical implementation. This maximum downward bias is quantitatively small, suggesting that our qualitative conclusions are robust to tenants being unaware of the RTC program.

4.3 Intuition for and discussion of approach

Proposition 1 provides an expression for the marginal welfare impact of an increase in tenant protections. The specific formula reflects the institutional details of the RTC program and eviction process, but the idea behind the approach is more general. This section develops that intuition using the supply and demand framework introduced in Section 2.2, and then discusses the advantages and limitations of the approach.

Section 2.2 argued that the welfare impacts of a program like RTC are ambiguous because the policy can shift both rental housing supply and demand. In that simple supply and demand framework, one approach to quantifying the impacts on tenants would be to estimate the demand curves $D(\cdot)$, $D'(\cdot)$, and use them as well as the prices and quantities pre- and post-RTC $(p^*, q^*), (p', q')$ to calculate consumer surplus before and after the policy was introduced:

$$CS^* \equiv \int_0^{q^*} [D(q) - p^*]dq \qquad CS' \equiv \int_0^{q'} [D'(q) - p']dq\,.$$

This approach is illustrated in panel A of Figure 2. One could take the analogous approach to estimate the impacts on producer surplus after estimating the supply curves $S(\cdot)$ and $S'(\cdot)$.

We take an alternative approach to welfare analysis by observing that the change in consumer surplus can be rewritten as follows:

$$\begin{aligned} CS' - CS^* &= \int_0^{q'} [D'(q) - p']dq - \int_0^{q'} [D(q) - p^*]dq - \int_{q'}^{q^*} [D(q) - p^*]dq \\ &= \int_0^{q'} [(D'(q) - D(q)) - (p' - p^*)]dq - \int_{q'}^{q^*} [D(q) - p^*]dq\,. \end{aligned}$$

Simplifying, and assuming (for exposition) a constant shift in demand ($D'(q) - D(q) = \omega \quad \forall q$),

$$\Delta CS = q'(\Delta D - \Delta P) - \int_{q'}^{q^*} [D(q) - p^*]dq\,. \tag{11}$$

Equation (11) decomposes tenants' ex-ante willingness-to-pay for the policy into two parts. The first part is the difference between the mean shift in demand and the change in rent prices up to the post-RTC quantity. These two pieces are denoted by (respectively) the green and brown parallelograms in panel B of Figure 2. The second term is the welfare change due to the change (here, a reduction) in equilibrium quantity, denoted by the green-shaded triangle. This triangle is small and, to first order, can be ignored because the change in behavior comes from tenants who value those units of housing at exactly the pre-RTC price. This is the envelope theorem logic underlying the derivations in the previous section. Conditional on the change in prices and demand shift caused by the policy, tenant reoptimization in terms of housing quantity has no first-order welfare impact. As a result, measuring tenants' ex-ante willingness-to-pay for the policy and the change in rent prices suffices to estimate the impact of RTC on tenant welfare. Proposition 1 extends this reasoning to incorporate the possibility of strategic default by tenants as well as institutional features of the eviction process and RTC program.

This approach has several advantages from an empirical standpoint. First, it does not require measuring tenants' behavioral responses to the policy. This could be quite challenging in our context, especially since data on tenant default are rarely available. The framework is also robust to other tenant margins of adjustment, which may be difficult to measure. Second, the framework does not require a fully specified supply-side model. The price-elasticity of supply, and even the model of conduct, only matter to tenants through rent prices. The formula is valid under competitive pricing, landlords having market power, and other models of conduct that may be difficult to distinguish empirically (Calder-Wang and Kim, 2024). A third advantage is that the framework yields a set of parameters required for welfare analysis that can be estimated using the variation provided by the policy change we study. The contribution of each parameter to the overall welfare calculation is relatively transparent, making it easy to assess how our measurement choices drive the results.

Of course, these advantages come with limitations. The marginal approach to welfare analysis allows us to analyze changes along a specific policy path. Out-of-sample counterfactuals would require a structural model of the rental housing market, which is beyond the scope of this paper. The model also does not allow us to quantify the impact on landlord profits without additional assumptions. Finally, the model of default and eviction abstracts away from non-payment dynamics within a tenancy and landlords' forbearance of some default. This is primarily due to lack of detailed ledger data of the kind used in Humphries et al. (2024), which is rarely available to researchers.[32]

Perhaps most importantly, for the welfare framework to be valid, we still need to specify

[32]Our model also abstracts away from RTC's partial geographic rollout during our sample period by considering a market with a common level of tenant protections. This raises the question of how, in our application, price differentials can be sustained for otherwise similar units over and above tenants' willingness-to-pay for RTC's additional protections. In practice, preference heterogeneity over unit location and type may lead units to be imperfectly substitutable across treatment and control ZIP codes. Though the model does not explicitly allow for this heterogeneity, the logic of our sufficient statistics approach would go through even if units were horizontally differentiated.

and measure all of the margins of adjustment for *landlords* that matter to tenants.[33] As discussed in Section 3.4, landlords could respond to stronger tenant protections by changing their screening or eviction practices, or by adjusting investment in maintenance. Such changes would have first-order impacts on tenant welfare and, if present, would need to be incorporated into the welfare framework. Given the evidence from Appendix Sections C.8 and C.9, we focus on price, where there is strong evidence of a response, rather than on these other margins where we do not find conclusive evidence of adjustments.

5 Empirical implementation and results

In this section, we develop an empirical implementation of Equation (10) to quantify the tenant welfare effects of RTC. Section 5.1 derives the empirical expression we use to quantify RTC's welfare impact and explains how we estimate the relevant empirical quantities; Section 5.2 presents our results; and Section 5.3 discusses some conceptual issues related to measurement and interpretation. We find RTC is welfare-decreasing for tenants under a wide range of plausible empirical inputs to our framework, though our confidence intervals are fairly wide.

5.1 Empirical Implementation

To take equation (10) to data, it will be useful to break down the components we seek to quantify empirically. We can rewrite equation (10) as

$$\Delta W = (\text{Court Benefits}) \times (\text{Insurance Multiplier}) - (\text{Rent Effect}) \times (\text{Correction for Default}), \tag{12}$$

where

$$\begin{aligned} \text{Court Benefits} &= F(\hat{y}) \times \left[R - \frac{dp_j}{d\tau} \left(h_j R + y_j \bar{\theta} \right) \right] \\ \text{Insurance Multiplier} &= \left[F(\hat{y}) + (1 - F(\hat{y}))\, \Omega \right]^{-1} \\ \text{Rent Effect} &= \frac{dR}{d\tau} \\ \text{Correction for Default} &= \frac{\bar{f}_E F(\hat{y}) + (1 - F(\hat{y}))\, \Omega}{F(\hat{y}) + (1 - F(\hat{y}))\, \Omega} . \end{aligned}$$

We now explain how we estimate each component. For reference, Table 4 lists each statistic that enters our welfare calculation, the source or underlying data, and the point estimate used in our baseline "No Heterogeneity" specification.

[33]Note that this is also required for other approaches to welfare analysis, including structural approaches.

Rent Effect. For the Rent Effect term, we use the year 2 price effect estimates reported in Column (2) of Table 2. These are \$38.11 from the "DiD (calipered)" specification and \$29.13 from the "DiD (calipered, flex)" specification.

Insurance Multiplier. Ex-ante, the value of the court benefits from RTC (in terms of *evicted* tenants' willingness-to-pay for them) is multiplied by a scaling factor capturing the additional value that arises because evictions tend to occur in high marginal utility states of the world. We refer to RTC's "Insurance Value" as (Court Benefits) × ((Insurance Multiplier) - 1). The insurance multiplier depends on the share of tenants facing eviction ($F(\hat{y})$) and the ratio of tenants' expected marginal utilities of consumption when they do not and do face eviction (Ω). To calculate $F(\hat{y})$, we divide eviction case filing counts in RTC-treated ZIP codes from public OCA court data by the number of renter tenants living in the same geographies based on the 5-year American Community Survey (ACS). This ratio is 12.8% during 2011-2015 for ZIP codes where RTC was introduced in 2017.

The task of estimating $\Omega = \frac{\mathbb{E}[\bar{u}'(c)|y \geq \hat{y}]}{\mathbb{E}[\bar{u}'(c)|y < \hat{y}]}$ is more involved. Our approach first makes an assumption about the functional form of utility over numeraire consumption $u(\cdot)$ and then uses differences in earnings changes between evicted and non-evicted tenants to estimate their consumption levels by eviction status.

We begin with (and later relax) an assumption that tenant income realizations are perfectly correlated with eviction status, similar to much of the optimal-UI literature's treatment of unemployment status (Gruber, 1997; Chetty, 2009; Kroft and Notowidigdo, 2016). Under this assumption, there is a single earnings realization in each of the evicted ($e = 1$) and non-evicted ($e = 0$) states. After using expression (8) to re-expand $\bar{u}'(c)$ in the denominator of Ω, we can write this ratio as

$$\Omega = \frac{\mathbb{E}[u'(y - R) \mid e = 0]}{\mathbb{E}[p_j u'(y - fR - y_j) + (1 - p_j)u'(y - fR) \mid e = 1]} \,. \tag{13}$$

We use our linked earnings and eviction data together with the NYCHVS data, both described in Section 2.4.3, to estimate earnings differences for evicted and non-evicted tenants. We estimate average earnings among tenants in our NYCHVS sample covering RTC-treated ZIP codes, and we use our panel of earnings and eviction filings to estimate earnings changes around eviction. Figure 5 presents our estimates of quarterly earnings dynamics relative to eviction or, for non-evicted tenants, relative to a randomly assigned placebo eviction date. While both evicted and non-evicted tenants' average earnings initially follow similar trends, evicted tenants' average quarterly earnings fall by about \$2,000 per quarter below those of non-evicted tenants in the quarters surrounding their eviction.[34]

[34] Appendix Figure D.1 shows that these average differences mask considerable heterogeneity in income

Motivated by these patterns, our welfare analysis uses estimated tenant earnings averaged over the quarter of, quarter before, and two quarters after eviction filing. These quarterly averages are \$9,897 for evicted tenants, and \$11,829 for non-evicted tenants (using their randomly assigned placebo eviction date).[35]

Other terms in this ratio of marginal utilities include rent R, the share f of rent that is paid by tenants who face eviction, the causal effect y_j of an eviction order on earnings, and the share of tenants p_j who receive an eviction order after filing. Rent R is estimated as the average in the NYCHVS sample described in Section 2.4.3.[36] To estimate f, we divide the average claim amount at filing in eviction court records by the rent, and assume the balance was accrued in the year prior to filing. This statistic (denoted $\bar{f}_E$) is 76.7%. The probability of an eviction order is calibrated to the mean for unrepresented tenants in Cassidy and Currie (2023), 64.3%. Finally, we assume the drop y_j in numeraire consumption due to an eviction order is Collinson et al. (2024)'s estimate of the cumulative earnings effect of an eviction order among New York City tenants over the two years following eviction, which is \$3,200. We assume tenants are hand-to-mouth, consuming their income minus the rent they pay.

Both the denominator and numerator of expression (13) require an assumption about the shape of utility from numeraire consumption $u(\cdot)$, as well as how our earnings measures translate into consumption. We assume $u(\cdot)$ exhibits constant relative risk aversion (CRRA) with a floor for non-housing consumption, representing other social insurance available for tenants with very low earnings (Deshpande, 2016). Our preferred specification assumes a risk aversion parameter of $\gamma = 2$ and an annual consumption floor of \$4,000. We assess our estimates' sensitivity to higher and lower values of these parameters. An implication of Proposition 1 is that we can leave preferences over housing consumption unspecified.

Correction for Default. This term adjusts the welfare cost of RTC's effect on contract

changes for both evicted and non-evicted tenants. While on average evicted tenants have substantially greater declines in earnings, we observe a large share of positive and negative income changes for both groups. Our extension in Appendix D.2 to allow for tenant heterogeneity incorporates this earnings dispersion conditional on eviction status.

[35] By averaging quarterly earnings over a one-year period, we in effect assume tenants can smooth consumption within the year but not across years. In reality, some tenants may be subject to higher-frequency income shocks. We investigated whether within-year variation in earnings could lead our annual income measure to understate RTC's insurance value. While quarterly earnings are more dispersed than the four-quarter average, the distributions are not very different between evicted and non-evicted tenants. As a result, our estimates of RTC's insurance value would be similar if we assumed each tenant's marginal utility of consumption depended only on a given quarter's earnings—for example, in the specification with full heterogeneity and using the "DiD (calipered, flex)" rent effect estimate, RTC's tenant welfare impacts range from -\$16.68 to -\$18.57 depending on which quarter is used, compared to -\$16.97 based on the four-quarter average.

[36] In some cases, the rent a tenant pays may differ from the listed contract rent because the tenant has a rental housing voucher. We believe these cases are likely to have a modest impact on our welfare estimate because they would lead us to overstate the magnitude of both the costs and benefits of RTC, and because fewer than 10 percent of renters in RTC ZIP codes had a housing voucher during our sample period.

rents for the fact that evicted tenants do not pay the full rent, and that they can do so when their marginal utility of consumption is relatively high. The adjustment depends on statistics already estimated: the eviction rate ($F(\hat{y})$), the share of rent paid by evicted tenants ($\bar{f}_E$), and the ratio of marginal utilities (Ω). Since most tenants are not evicted and most evicted tenants paid the majority of the past year's rent, this statistic is close to one: 0.957 in our baseline analysis with no tenant heterogeneity.

Court Benefits. We first rewrite the court benefits term as follows:

$$\text{Court Benefits} \;=\; F(\hat{y}) \times \frac{dl}{dRTC} \times \left[\frac{d\tau}{dl}R - \frac{dp_j}{dl}\left(h_j R + y_j\bar{\theta}\right)\right],$$

where $\frac{dl}{dRTC}$ is the impact of RTC on tenant representation, and $\frac{d\tau}{dl}$ and $\frac{dp_j}{dl}$ are the impacts of representation on (respectively) case duration and the probability of an eviction order. This formulation expresses the court benefits in terms of parameters estimated in prior studies.[37] For the court impacts of RTC, we use the IV point estimates from Cassidy and Currie (2023), which imply that legal representation increases mean case duration by 85 days and reduces the probability of an eviction order by 32.1 percentage points. For the first-stage effect of the program on legal representation, we use the estimate of 12.4 percentage points from Table 3 in Cassidy and Currie (2023).

Finally, using estimates from Collinson et al. (2024), we calibrate the causal effects of an eviction order on tenant income and homelessness, which we model as reducing both numeraire and housing consumption. As for our estimates of the expected marginal utility ratio Ω, we assume the estimated cumulative earnings reduction among NYC tenants ($y_j = \$3,200$) equals the drop in numeraire consumption due to an eviction order, and the cumulative increase in years in a homeless shelter ($h_j = 0.07$) equals the reduction in time housed.

For the remaining parameters, the eviction rate $F(\hat{y})$ and rent R are calculated as described earlier. The term $\bar{\theta}$, which corrects for the curvature of utility when valuing the income loss from an eviction order, is calculated using the same utility function and state-dependent consumption values as the insurance multiplier. It is 0.955 in our baseline no-heterogeneity specification.

Adding Heterogeneity. In Appendix D.2, we extend our empirical welfare framework to allow for heterogeneity across tenants in terms of earnings levels, rents, earnings risk, and nonpayment. Specifically, to allow for richer income heterogeneity, we group tenants

[37] This version also reflects a change of variables from a marginal change in expected case duration (τ) to a marginal change in the probability of tenant representation (l). The unconditional change in the mean case duration among evicted tenants is $1 \times \frac{d\tau}{dRTC} = \frac{dl}{dRTC}\frac{d\tau}{dl}$, and change in the likelihood of an eviction order is $\frac{dp_j}{dRTC} = \frac{dp_j}{d\tau}\frac{d\tau}{dRTC} = \frac{dp_j}{dl}\frac{dl}{dRTC}$.

according to their baseline earnings decile, and then construct decile-specific distributions of earnings realizations and rent nonpayment rates for tenants who do and do not face eviction. We also allow rent levels and eviction rates to vary with each tenant's ZIP code and baseline earnings, and for the baseline earnings distribution to vary across ZIP codes.

This extension is especially valuable for understanding the insurance value of RTC. For example, the insurance value could be largest for tenants who start out at the bottom of the earnings distribution if the earnings losses associated with eviction translate into large changes in the marginal utility of consumption. Furthermore, if incomes, rents, and eviction rates are correlated across ZIP codes, this could affect the court benefits in ways that covary with the insurance value. Allowing this heterogeneity is also helpful for probing the robustness of our findings to our modeling assumptions. For example, in assessing robustness to different levels of relative risk aversion (γ), it is valuable to quantify not just the average income of evicted and non-evicted tenants, but also the full income distribution for both groups.

Statistical Uncertainty. Our bootstrap procedure for inference is described in Appendix D.3. This procedure accounts for sampling uncertainty in all empirical inputs to our welfare framework from Table 4 except for the eviction rate, which is estimated using the full population of New York City eviction filings, and the calibrated parameters γ and c.

5.2 Results

Table 5 reports estimated welfare effects corresponding to our main rent effect specifications. The first column ("No Heterogeneity") assumes an ex-ante representative tenant, while the second column allows for tenant heterogeneity. We estimate that RTC's effects on tenant welfare are negative, with welfare losses of -\$25.68 and -\$17.09 on average per tenant per month in the no-heterogeneity case, and -\$25.57 and -\$16.97 with tenant heterogeneity. In each column, the larger negative welfare estimate corresponds to the larger point estimate of RTC's rent effect. We find in practice that allowing for heterogeneity does not have a large quantitative impact on our main estimates, a point we explore more below.

The bootstrapped standard errors reported next to each point estimate show that our welfare estimates are subject to a fair amount of statistical uncertainty. The standard errors range from \$7.63 to \$8.05 per tenant per month across specifications in Table 5. We reject zero or positive welfare effects at the 2 percent level or less in all of our main specifications. Nonetheless, our confidence intervals are fairly wide, and we cannot always rule out negative welfare effects that are economically small. Using the rent effect estimate from the "DiD (calipered, flex)" specification, symmetric 95% confidence intervals are $[-\$32.87, -\$1.30]$ and $[-\$32.60, -\$1.34]$ under No Heterogeneity and Full Heterogeneity, respectively. Overall, while there remains uncertainty about the magnitude of RTC's welfare impact, we view these estimates as indicating that RTC likely reduced tenant welfare on average.

Figure 6 decomposes the tenant welfare impact of RTC in our baseline no-heterogeneity specification into three terms based on equation (12): (1) court benefits; (2) insurance value; and (3) the cost of higher rents (the rent effect multiplied by the correction for default). We estimate that the first term is \$7.48 per month, to which the insurance value adds \$3.31. Thus, even using the "DiD (calipered, flex)" price effect point estimate (\$29.13), the cost of RTC due to higher rent prices is more than twice as large as the benefits in court, including insurance value. The decomposition remains similar when we allow for tenant heterogeneity, but with a slightly lower value of court benefits (\$7.21) and higher insurance value (\$3.70), leading to similar net welfare effect estimates.

This decomposition allows us to assess how much larger the estimated non-rent components of our welfare calculation would need to be in order to reverse the finding that RTC negatively impacted tenant welfare. For the smaller rent effect point estimate, the court benefits term would need to be more than 2.5 times as large as we estimate. By similar reasoning, the insurance value itself would need to be 270 percent of the court benefits, instead of our estimated 44-51 percent, to make RTC welfare-neutral. For the larger rent effect estimate, the insurance value would need to be more than three times as large as the risk-neutral benefits.

As discussed earlier, one reason why the court benefits could be understated is if tenants do not make rational default decisions, perhaps because they misunderstand the implications of eviction and the RTC program. If the optimal default condition understates tenants' marginal willingness-to-pay for housing in the evicted state of the world, their true willingness-to-pay would need to be nearly three times the contract rent to reverse the sign of our welfare estimate. It is also possible that tenants do not anticipate some of the most acute potential consequences of eviction, such as becoming homeless. The value of avoiding time in a shelter due to eviction would need to be 19 times as valuable as the rent to reverse the overall sign.

This decomposition also illustrates which components of welfare contribute the most uncertainty to our overall estimates. In the baseline no heterogeneity specification, court benefits are relatively precisely estimated, with a standard error of \$1.03 and 95% confidence interval of [\$5.46 , \$9.51]. There is more uncertainty regarding the insurance value, which has a standard error of \$1.79 and a 95% confidence interval [-\$0.19 , \$6.81]. The estimate of the insurance value is somewhat more precise when we allow for tenant heterogeneity, with 95% confidence interval [\$0.83 , \$6.57]; nonetheless, we cannot statistically rule out a modest insurance value of protections, or a substantially higher value.[38] However, in terms of the

[38] Most of the statistical uncertainty in the insurance value is due to the NYCHVS, which we use to construct the baseline distribution of income among renters in RTC ZIP codes. As discussed in Appendix Section D.2, the linked income panel is reweighted to match this distribution, generating additional variation in income realizations conditional on eviction status. When we allow for heterogeneity in income and income risk conditional on eviction status, this ends up averaging out some of the variation induced by the reweighting, leading to a lower variance in the insurance value estimate despite it depending on more input parameters.

total welfare impact, most of the uncertainty comes from the rent effect estimates themselves, which have standard errors of about $8.

Intuition for magnitudes. One of the benefits of a sufficient statistics approach is that the mapping from input parameters to the resulting welfare estimate is relatively transparent. To provide further intuition for the magnitudes in Figure 6, we can trace these welfare components back to the inputs in Table 4 for our baseline no-heterogeneity specification. We discuss two of the most instructive components here, and describe the full calculation for all components in Appendix D.5.

As a first example, the largest component of the Court Benefits term is the value of additional time in the unit after defaulting. Each tenant has a 12.8% chance of facing an eviction case each year; in 12.4% of those cases, the tenant will have a lawyer *because of* the RTC program; and if represented, they can expect to remain in their unit for an additional 85 days (2.79 months), which they value at the rent of $1,527 per month due to optimal default. The expected benefit from longer case durations is therefore $.128 \times .124 \times 2.79 \times \$1,527 = \$67.73$, or $5.64 per month. The rest of the Court Benefits term reflects the tenant's greater housing and numeraire consumption (0.84 months and $3,200, respectively) in the 32.1% of newly-represented cases where the lawyer prevents an eviction order. Following similar calculations, the expected value of these additional benefits is $1.84 per month, yielding $\$5.64 + \$1.84 = \$7.48$ in total court benefits.

As a second example, the Insurance Multiplier is largely determined by our income estimates for evicted and non-evicted individuals, which are (respectively) $39,588 and $47,318 per year. Tenants are assumed hand-to-mouth, and consume their income after paying rent (contract rents are $18,327 per year). Non-evicted tenants pay the full rent by assumption, so their annual consumption is $y - R = \$28,991$. Two additional factors impact the consumption levels of evicted tenants. First, evicted tenants default on 23.3% (2.80 months, valued at $4,269) of their annual rent, consuming that amount instead. Second, evicted tenants face an additional income drop of $3,200 in the 64.3% of cases which receive a judgment against the tenant. Together, this leads to annual consumption levels of $25,530 or $22,330 for evicted tenants depending on their case outcome. The marginal utility of consumption is $u'(c) = c^{-\gamma}$, with $\gamma = 2$ in our preferred specification. The ratio of expected marginal utilities is then 0.648. After accounting for the baseline eviction rate of 12.8% (as detailed in Appendix D.5), this ratio of marginal utilities implies the insurance value of RTC is 44% of the Court Benefits term, or $3.31 per month.

Robustness. We next explore how our modeling assumptions and measurement choices influence our welfare estimates. Table 6 reports estimated welfare effects under a range of assumptions about the shape of $u(\cdot)$, which governs preferences over numeraire consumption, and the consumption floor c. Each row corresponds to either a consumption floor or a relative risk aversion parameter, and each column corresponds to a rent regression specification. Table 6 shows results for the heterogeneous-tenant specification, while Appendix Table D.1 shows analogous results for the no-heterogeneity case.[39] In all cases, we estimate that RTC's effects on tenant welfare are negative. The degree of tenant risk aversion and the consumption floor each has a moderate impact; for example, in the second column of Table 6, the tenant welfare point estimate ranges from -\$19.15 if $\gamma = 1$ to -\$14.12 if $\gamma = 5$, and from -\$15.73 if $c = \$2,000$ to -\$18.13 if $c = \$8,000$. Since we do not estimate $u(\cdot)$ or the consumption floor, it is reassuring that our results are not qualitatively sensitive to a reasonable range of assumptions about them.[40]

As in Table 5, confidence intervals in Table 6 generally exclude zero, though only with 90% coverage under more extreme assumptions about risk aversion or the consumption floor. Higher values of risk aversion lead to slightly more statistical uncertainty, as the same income differences are translated into larger differences in marginal utilities of income. This effect is more pronounced for the no-heterogeneity specifications reported in Appendix Table D.1, where the point estimates are also more sensitive to high degrees of risk aversion.

Because RTC's tenant welfare impact depends almost one-for-one on its rental price effect, the magnitude of our welfare estimate depends on the rent effect estimate used. Appendix Figure D.2 compares welfare point estimates and confidence intervals obtained using all of the year 2 rent effect specifications reported in the paper (which are themselves compared in Appendix Figure C.11). All tenant welfare point estimates are negative, and most specifications reject zero or positive effects at the 5% level. However, some rent effect specifications with smaller point estimates and larger standard errors have 95% confidence intervals which contain zero and some positive values. The welfare point estimate based on the DiD, flex rent effect estimate (-\$17) is still on the higher end compared to all specifications reported in the figure, which range from -\$40 to -\$10.

[39]The no-heterogeneity specifications are unaffected by the choice of consumption floor c, as the representative tenant never has income realizations as low as the values we consider. Estimates are somewhat more sensitive, however, to the risk aversion parameter for high values of γ than in the heterogeneous tenant specifications. This is because in the heterogeneous tenant specification, the distributions of income realizations for evicted and non-evicted tenants are actually more different *on average* than they are in the left tails (which receive more weight as γ increases). For the no-heterogeneity specification, in contrast, the insurance value is determined only by the mean income realization in each state.

[40]We also investigated whether our results are sensitive to accounting for the income tax schedule or other non-wage sources of income such as SNAP and TANF. Results indicate that we are likely overstating RTC's insurance value, as the tax system and other parts of the social safety net partially insure the income losses associated with eviction.

5.3 Interpretation of results

This section discusses the interpretation of our reduced-form estimates and tenant welfare calculations. The gradual rollout provides a basis for the identification of local rental market impacts of RTC. We interpret our reduced-form estimates as representing the impact of RTC as it was implemented throughout our study period, i.e., as the impact of the initial partial rollout. Our welfare calculation, therefore, also represents an assessment of the partial program. Policymakers may also be interested in the longer-term impacts of a full (citywide) rollout. The long-run price and welfare impacts of the full rollout may differ from our short-run estimates for several reasons.

There are several reasons to expect RTC's effect on long-run welfare to be more negative. One reason is that supply and demand may take time to adjust. On the supply side, margins like condo conversion and reduced investment could take longer to fully realize than the two-year horizon over which we estimate price effects. On the demand side, tenants may learn over time about the availability or value of RTC. The take-up estimates in Ellen et al. (2021) and Cassidy and Currie (2023) suggest substantial room for an increase in take-up of representation, which would intensify the impact of the program. Short-run price increases may therefore not fully reflect longer-run changes in housing demand or supply due to RTC.

A citywide rollout, instead of the partial rollout we study, could also raise equilibrium prices further due to strategic interactions among landlords. If, other things equal, it is optimal for one landlord to raise prices when landlords elsewhere in the city do, then the rent increases in treated ZIP codes should be larger when RTC is extended to the rest of the city, which competes with them for tenants.

Lastly, it is possible that the platform from which we obtain listings data does not cover the properties with the highest eviction risk. This is plausible if such properties operate more informally. Theoretically, properties with a higher risk of eviction are expected to see larger impacts of RTC, consistent with the data (see Figure 4). Hence, due to data limitations, we could be underestimating the true effects of RTC.

However, there are also arguments that the long-run, full rollout of RTC could be more advantageous for tenants than we estimate. A citywide rollout could lead to lower price increases if our results are driven by adverse selection into treated ZIP codes. Though we do not find evidence that RTC increased the riskiness of renters moving into treated ZIP codes (see Section 3.3), we cannot rule out that *unobservably* higher-eviction-risk tenants selected into those neighborhoods due to their higher demand for RTC. Rent price increases would then reflect the riskier tenant pool landlords faced in treated ZIP codes during the rollout period; a citywide rollout would limit this channel because tenants with high eviction risk would enjoy RTC's protections anywhere in the city.

It is also possible that some of the benefits from RTC take time to realize. If, anticipating that tenants are likely to be represented in eviction court, landlords begin investing in maintenance, this could lead to improved housing quality and reduce the number of habitability violations in the long run (in contrast to the null or positive effects we estimate in the short run). Of course, since maintenance is costly, this force could also further increase rent prices. At scale, a sustained RTC program could also benefit tenants in the long run if the presence of tenant advocates pressures the court system to provide more transparent, tenant-friendly services.

Another open question is whether our results generalize beyond New York City's RTC program. To explore this question further, we analyze the recent RTC rollout in Connecticut in Appendix E. We find qualitatively similar effects on court outcomes and listed rent prices, suggesting that equilibrium responses to tenant protection policies may matter in other contexts as well.

A final consideration in interpreting our analysis is that a full welfare assessment of RTC would also consider impacts on landlords, taxpayers, and other individuals. In principle, landlords could be better off due to RTC if a large shift in housing demand due to the value of legal assistance (which landlords do not directly pay for) is priced into rents. However, given our evidence that housing quantity decreased, landlords may have been made worse off due to the policy, as discussed in Section 2.2. Impacts on the government budget, and hence taxpayers, are also ambiguous. On one hand, paying for tenants' legal counsel is expensive.[41] On the other hand, there may be public cost savings from fewer evictions if they reduce the frequency of costly events such as emergency room visits or homeless shelter stays. Finally, RTC could impact other individuals, such as children or elderly cohabitants, who are affected by eviction but whose welfare may not be fully internalized by landlords and tenants themselves. Our framework does not account for such externalities. While prior research has studied the impact of eviction on children (Cassidy et al., 2025; Collinson et al., 2025), we do not know the extent to which these effects are internalized. We leave quantifying these additional impacts of RTC for future research.

6 Conclusion

As attention to the prevalence of eviction has grown, so too has the array of policy proposals to protect tenants facing eviction. Prominent among these policies is "right to counsel." As these legal assistance programs expand, understanding their potential benefits and costs is crucial. We study the largest-to-date implementation of RTC, leveraging its ZIP code by ZIP code rollout in NYC to provide the first quasi-experimental evidence of RTC's effects on

[41] NYC budgeted $158 million for the Universal Access to Counsel program in FY 2025 (Salant et al., 2025).

the broader rental housing market. We estimate that RTC caused rent increases for tenants. Adapting tools from the sufficient statistics literature to our setting, we use our estimates to quantify RTC's impact on tenant welfare. Under a range of plausible assumptions, the estimated RTC-induced rent increases are large enough to offset the welfare gains from increased legal protection, leading to a moderate decrease in ex-ante tenant welfare. Though the estimated magnitudes are subject to considerable statistical uncertainty, we reject zero or positive tenant welfare effects at conventional levels in our main specifications.

The decrease in ex-ante tenant welfare is consistent with RTC generating substantial costs for landlords that are not symmetrically valued by tenants. Policymakers may wish to take seriously the possibility that stronger tenant protections could generate costs for renters through rental market responses, which should be weighed against the benefits of those policies. This may be particularly true of interventions in the legal process of eviction, which generate costs to landlords and benefits to tenants that are not necessarily symmetric. Of course, our findings apply only to one program implemented in a specific context (NYC), and are subject to statistical and modeling uncertainty. We do find suggestive evidence of similar rental market responses in Connecticut's more recent rollout. But the effects of similar regulations may be different in other rental markets which have different housing market conditions and levels of tenant protections.

This paper leaves many questions open for future research. In addition to quantifying costs and benefits to landlords and taxpayers, our analysis does not allow us to conduct out-of-sample policy counterfactuals. The impacts of alternative policy instruments and design of optimal policy are important directions for future work.

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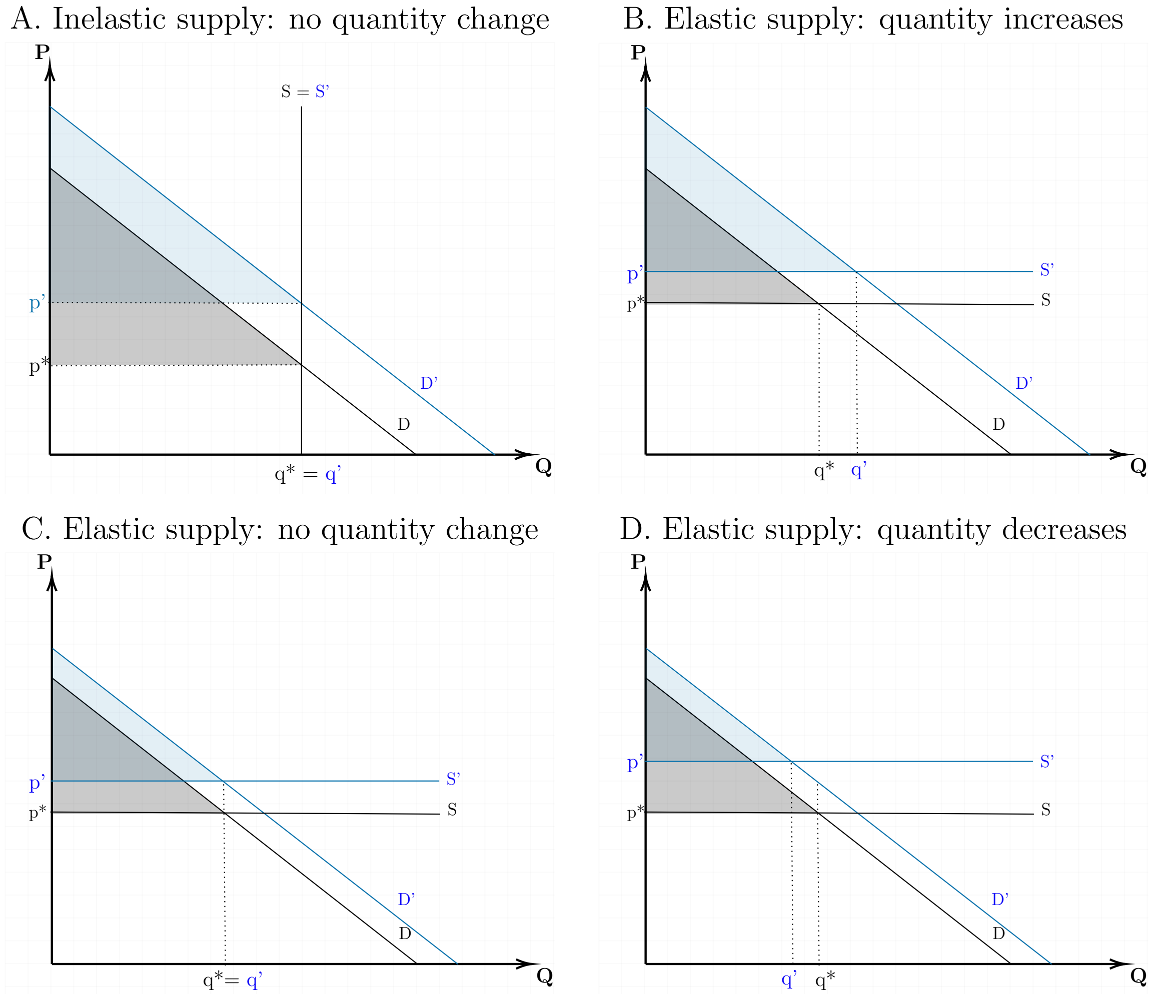
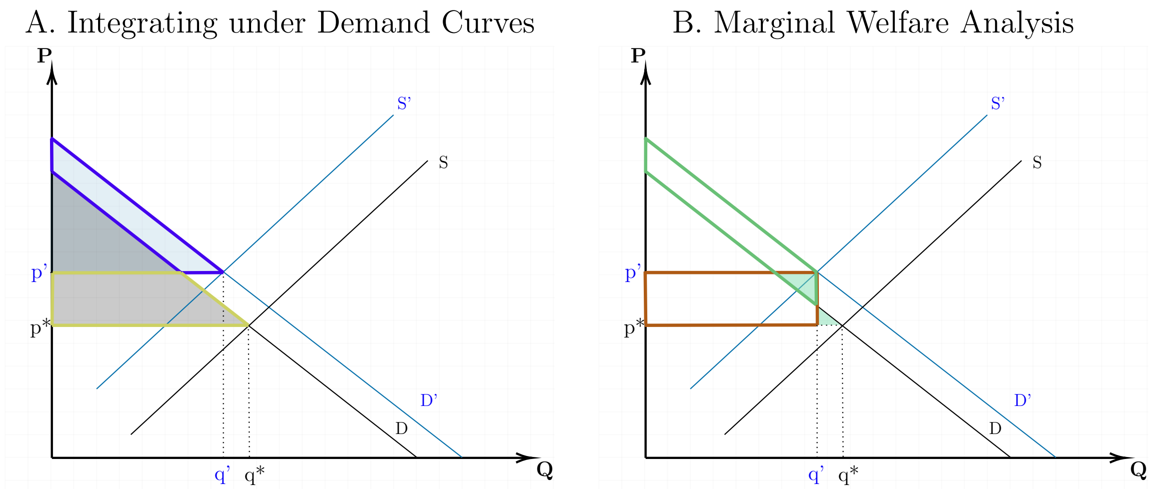
Figures and Tables

Figure 1: Welfare, Price, and Quantity Effects of RTC



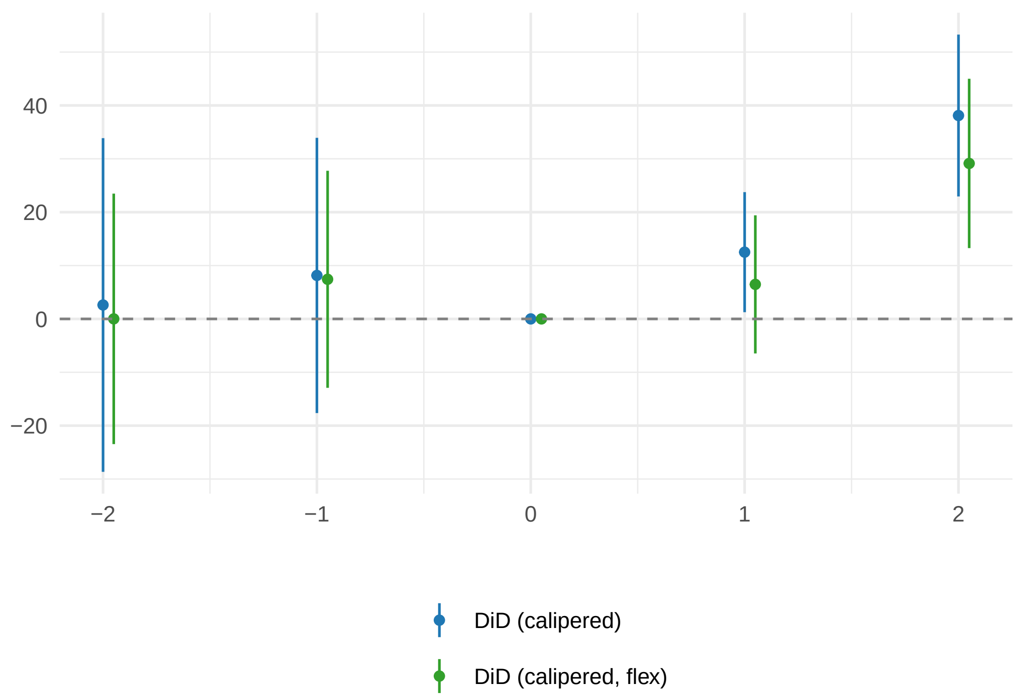
Note: This figure shows hypothetical changes in consumer welfare, price, and quantity, depending on the elasticity of supply and the size of the demand and supply shifts. The grey regions denote consumer surplus prior to the policy; the blue regions denote consumer surplus after. The graphs are discussed in Section 2.2.

Figure 2: Measuring the Change in Consumer Surplus From a Mandated Benefit



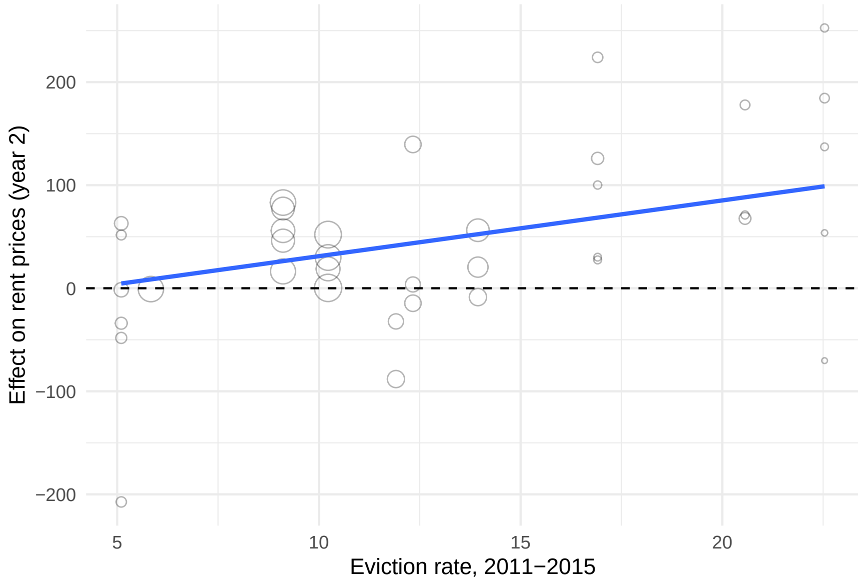
Note: This figure illustrates how we measure change in consumer surplus as discussed in Section 4.3.

Figure 3: Impact of RTC on Rent Prices



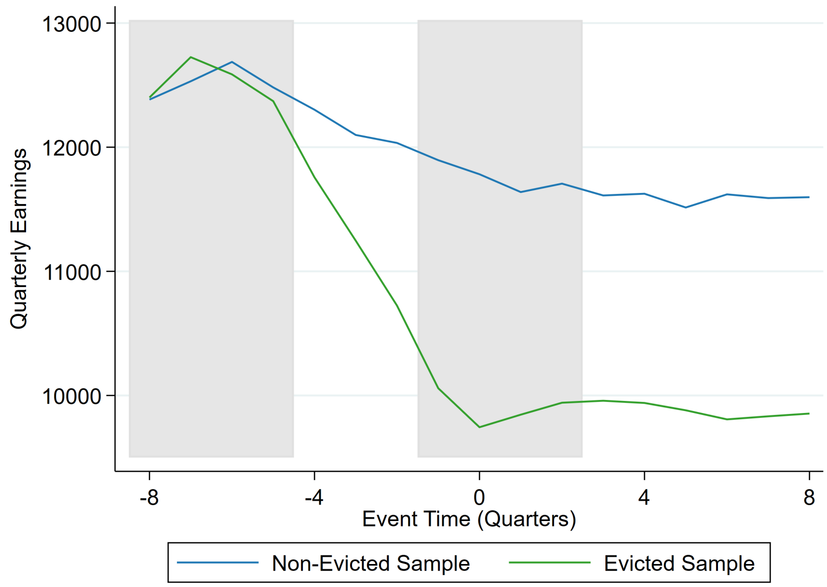
Note: This figure plots estimates of the impact of RTC on listed rent prices (δ_t in equations 1 and 2) using StreetEasy data. The period 1 coefficients compare listings created 0 to 365 days after the rollout date to those created 0 to 365 days prior to the rollout in that border pair. The period 2 coefficients compare listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year -1" compares listings created 365-730 days prior to the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year -2" compares listings created 730-1095 days prior to the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. Section 2.4 and Appendix Section A describe the data in greater detail. Section 3.1 describes the calipered difference-in-differences approach and sample restrictions in greater detail. All specifications use a bandwidth of 1000 meters and control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. Dots correspond to point estimates and bars to 95% confidence intervals. Standard errors are clustered at the border-pair level, with 57 clusters in total.

Figure 4: Rent effects by eviction filing rate



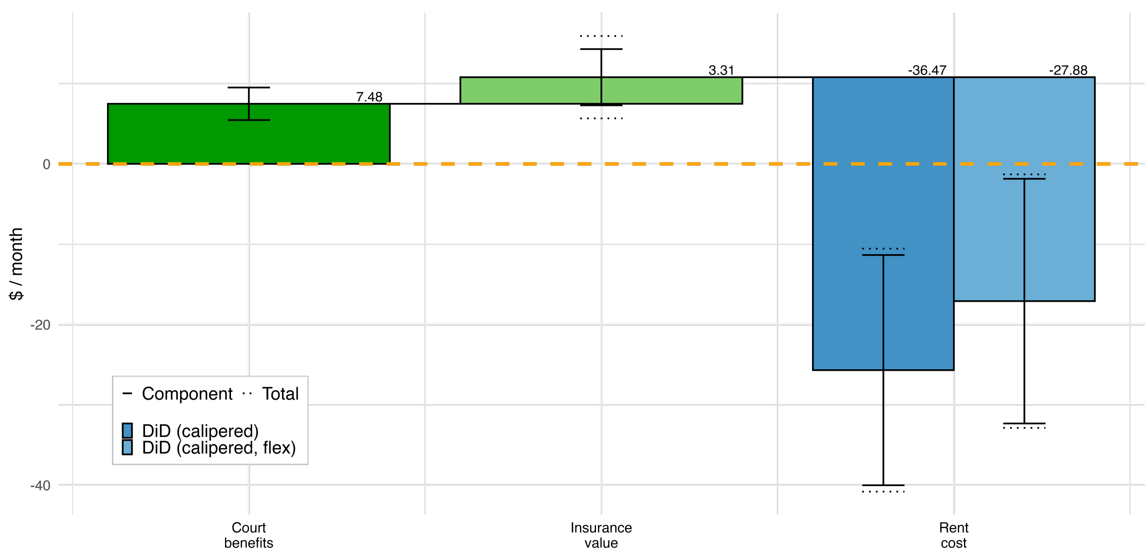
Note: This figure plots border-specific estimates of the impact of RTC on listed rent prices (δ_{bt} in the analogue of equation 2) against the eviction filing rate in the treated ZIP code. The coefficients correspond to the year 2 "calipered, flex" difference-in-differences specification with a bandwidth of 1000 meters. Dot size corresponds to the number of rental listings in that border pair. The blue line plots the linear fit between price effects and eviction rates using a weighted linear regression. The estimated coefficients compare StreetEasy listings created between 365 and 730 days after the rollout date to those created 0 to 365 days prior to the RTC rollout in that border pair. Section 2.4 and Appendix Section A describe the data in greater detail. See Appendix Figure C.6 for estimates split by baseline eviction rates. Point estimates are higher for above-median ZIP codes, although the difference is not statistically significant. Section 3.1 describes the calipered difference-in-differences approach and sample restrictions in greater detail. All specifications use a bandwidth of 1000 meters and control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. We calculate the number of evictions between 2010 and 2015 for each parcel using data from Collinson et al. (2024). We then compute ZIP code-level eviction rates as the ratio of eviction filings to total number of renters in the 2011-2015 ACS.

Figure 5: Earnings Dynamics for Evicted and Non-evicted Renters



Note: This figure shows average quarterly earnings dynamics in the NYSDOL earnings data for the 2011-2015 eviction case sample and no-eviction sample described in Section 2.4.3. Event time zero corresponds to the quarter of eviction filing for the eviction case sample, and to a randomly assigned placebo eviction filing date for the no-eviction sample; the latter are assigned so that the distribution of (real and placebo) eviction dates is the same in both samples. The two shaded regions highlight the quarters of earnings data that are used to calculate ex-ante earnings and ex-post earnings used in the welfare analysis of Section 4. The levels of both earnings series are normalized so that average ex-ante earnings in the NYSDOL data match the average earnings in our NYCHVS sample; the NYSDOL average ex-ante earnings are $1,495 lower for the non-evicted sample and $1,592 lower for the evicted sample.

Figure 6: Welfare Decomposition



Note: This figure decomposes the welfare impact of RTC from the No Heterogeneity specifications reported in Table 5 into three components: (1) the benefits from improved court outcomes, (2) the additional "insurance value" from improved court outcomes due to the fact that eviction occurs in high marginal utility states of the world, and (3) the cost due to higher rent prices. Section 5.1 defines each component and describes how each is estimated. This calculation assumes relative risk aversion of $\gamma = 2$ and an annual consumption floor of \$4,000. The solid error bars represent 95% confidence intervals for each component. The dotted lines show 95% confidence intervals for the cumulative sum of components as components are added when moving from left to right.

Table 1: Characteristics of Treatment and Control Units

	Treated ZIP codes		Never-treated neighbors		All other ZIP codes	
Median household income (1000s, 2019 ACS)	56.82	(14.6)	58.49	(20.6)	93.69	(43.29)
Share renters (2019 ACS)	78.07	(12.19)	70.18	(19.29)	59.5	(23.43)
Median rent (2019 ACS)	1381.69	(141.44)	1388.95	(244.37)	1877.7	(622.54)
Hispanic share (2019 ACS)	34.21	(20.97)	34.34	(21.39)	19.47	(15.38)
Black share (2019 ACS)	40.49	(23.78)	36.53	(25.57)	11.2	(17.55)
White share (2019 ACS)	28.06	(19.98)	31.12	(20.74)	55.32	(24.11)
Share non-citizens (2019 ACS)	16.92	(6.45)	16.06	(6.78)	14.3	(6.71)
Eviction rate (2016)	11.46	(4.86)	11.02	(5.08)	5.2	(4.54)
Nonpayment share of evictions (2016)	81.77	(13.02)	78.92	(18.4)	71.86	(22.06)

Note: This table uses ZIP code (ZCTA) level data from the 2015-2019 5-year American Community Survey, and data from the universe of eviction filings from the Office of Court Administration from 2010-2015. The table reports (equally weighted) means and standard deviations for various ZIP code level characteristics for three different sets of ZIP codes. "Treated ZIP codes" are those that are ever treated before 2020. "Never-treated neighbors" are ZIP codes adjacent to treated ZIPs that are never treated (through 2019, when our analysis ends). "All other ZIP codes" are the remaining untreated and non-adjacent ZIP codes in New York City. Appendix Table A.2 provides an additional comparison of listing characteristics, building characteristics, and neighborhood characteristics for listings near the border between treated and control ZIP codes. Appendix Table A.3 provides additional evidence on the representativeness of the StreetEasy listings.

Table 2: Impact of RTC rollout on listed rents

	(1) Year 1 effect	(2) Year 2 effect
DiD (calipered)	12.51**	38.11***
	(5.74)	(7.74)
DiD (calipered, flex)	6.47	29.13***
	(6.60)	(8.09)
Observations	179578	120229
Pre-period mean ($/month)	2361.70	2439.79

Note: This table reports estimates of the impact of RTC on listed rent prices (δ_t in equations 1 and 2) using StreetEasy data. "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days of the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. Section 2.4 and Appendix Section A describe the data in greater detail. Section 3.1 describes the calipered difference-in-differences approach in greater detail. All specifications use a bandwidth of 1000 meters. The StreetEasy sample only includes "no amenities" listings, i.e. those without central AC, a gym, doorman, or pool. We control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. Standard errors are clustered at the border-pair level, with 57 clusters in total. We drop border pairs where the minimum distance to the border on either side is greater than 300m. We also drop border pairs if there are fewer than 50 observations on either side or if there are no observations in the pre or post-periods. *p<0.1; **p<0.05; ***p<0.01

Table 3: Extensive margin: condos and large permits

	(1) No. condos per unit	Permits (2) Major alteration	(3) New construction	(4) Demolition
Panel A. Year 1				
10,000 · DiD (calipered, parcel FE)	1.51**	7.42**	3.76**	1.59
	(0.68)	(3.38)	(1.68)	(1.61)
Observations	1473365	1465406	1465406	1465406
10,000 · Pre-period mean	79.98	52.14	3.14	4.80
Panel B. Year 2				
10,000 · DiD (calipered, parcel FE)	2.29***	1.95	3.26*	7.18***
	(0.83)	(4.07)	(1.96)	(2.33)
Observations	1473365	1465406	1465406	1465406
10,000 · Pre-period mean	79.98	52.14	3.14	4.80

Note: This table reports estimates of the impact of RTC on condominium conversions and building permits (δ_t in equation 3). All outcome variables were multiplied by 10,000 for readability. We restrict our analysis to parcels that include housing within 1000m of the ZIP code border. The dependent variable in column (1) is the number of condominiums divided by the number of residential units, i.e., the condominium share of each building's housing stock. The dependent variable in column (2) is the number of major alteration permits in that parcel and year. The dependent variable in column (3) is the number of new building permits in that parcel and year. The dependent variable in column (4) is the number of demolition permits in that parcel and year. "Year 1" and "Year 2" are estimated jointly. Since we include all parcels in these regressions, we can include parcel-by-border pair and year-relative-to-treatment fixed effects. Standard errors are clustered at the border-pair level. We drop border pairs where the minimum distance to the border on either side is greater than 300m. We also drop border pairs if there are fewer than 50 observations on either side or if there are no observations in the pre or post-periods. *p<0.1; **p<0.05; ***p<0.01

Table 4: Inputs to Welfare Calculation

Statistic	Notation	Estimate	Source(s)
Eviction Rate	α	12.8%	ACS, public OCA
Rent (annual)	R	\$18,327	NYCHVS
Share of Rent Paid if Evicted	$\bar{f}_E$	76.7%	NYCHVS, OCA
Baseline Probability of an Eviction Order	p_j	64.3%	Cassidy & Currie (2023)
Effect of RTC on			
Rent (annual)	$\frac{dR}{dRTC}$	\$350 - \$457	Section 3.2 / Table 2
Representation	$\frac{d\ell}{dRTC}$	12.4%	Cassidy & Currie (2023)
Effect of Representation on			
Case Duration	$\frac{d\tau}{d\ell}$	85.0 days (0.233 years)	Cassidy & Currie (2023)
Pr(eviction order)	$\frac{dp_j}{d\ell}$	-32.1%	Cassidy & Currie (2023)
Effect of an Eviction Order on			
Income Loss	y_j	$\$3,200$	Collinson et al. (2024)
Years Homeless	h_j	0.070	Collinson et al. (2024)
Income if Not Evicted	$y \mid e = 0$	\$47,318	NYCHVS, NYSDOL, OCA
Evicted	$y \mid e = 1$	\$39,588	NYCHVS, NYSDOL, OCA
Curvature Term	$\bar{\theta}$	0.955	Calculated
Ratio of $u'(c)$	$\Omega = \frac{\mathbb{E}[u'(c)\mid e=0]}{\mathbb{E}[u'(c)\mid e=1]}$	0.648	Calculated
Coef. of Relative Risk Aversion	γ	2	Assumed
Consumption Floor	c	\$4,000/yr	Assumed

Note: Inputs into the tenant welfare calculation for baseline "No Heterogeneity" specifications. Section 5.1 describes how each input is calculated. The second column provides the notation used in Section 5.1 to denote each input; the third column denotes the estimate(s); and the fourth column describes the data source or research paper from which the value was obtained.

Table 5: Impact of RTC on Tenant Welfare

	No Heterogeneity		Full Heterogeneity	
Rent Specification	Estimate (s.e.)	p-value	Estimate (s.e.)	p-value
DiD (calipered)	-25.68 (7.72)	0.001	-25.57 (7.63)	0.000
DiD (calipered, flex)	-17.09 (8.05)	0.017	-16.97 (7.97)	0.016

Note: The welfare estimates use the estimated Year-2 rent effects reported in Table 2. All specifications assume an annual consumption floor of \$4,000 and a coefficient of relative risk aversion of 2. No Heterogeneity assumes a single ex-ante representative tenant and a single income realization for (separately) evicted and non-evicted individuals. Full Heterogeneity allows heterogeneity by ex-ante income and geography as well as heterogeneous income realizations conditional on eviction status. Standard errors are calculated via bootstrap. p-values test the one-sided null of weakly positive welfare effects.

Table 6: Impact of RTC on Tenant Welfare (Alternative Parameters, Tenant Heterogeneity)

	DiD (calipered)	DiD (calipered, flex)
Panel A: Relative Risk Aversion		
$\gamma = 1$	-27.81 (7.49)	-19.15 (7.91)
$\gamma = 2$	-25.57 (7.63)	-16.97 (7.97)
$\gamma = 3$	-24.09 (7.75)	-15.53 (8.06)
$\gamma = 5$	-22.65 (7.90)	-14.12 (8.17)
Panel B: Consumption Floor		
$c = \$2{,}000$	-24.30 (7.76)	-15.73 (8.03)
$c = \$4{,}000$	-25.57 (7.63)	-16.97 (7.97)
$c = \$8{,}000$	-26.76 (7.55)	-18.13 (7.95)

Note: Estimates and standard errors for the tenant welfare impact of RTC. Each row varies one parameter from the baseline value: the coefficient of relative risk aversion in Panel A, or the annual consumption floor in Panel B. Baseline values are $\gamma = 2$ and $c = \$4{,}000$. All specifications allow for heterogeneity in terms of income, rents, income risk, and nonpayment. Standard errors are calculated via bootstrap.

ONLINE APPENDIX

A Additional details on data construction

A.1 Details on the policy rollout

NYC's right-to-counsel program, officially named "Universal Access to Counsel," was signed into law in August of 2017. The goal was for the city to provide access to legal counsel to all eligible tenants by 2022 (Been et al., 2018). The program was phased in over time on a ZIP code-by-ZIP code basis (see Appendix Figure B.1). ZIP codes were included earlier in the rollout based on the availability of other legal service programs, eviction rates, the prevalence of rent-regulated housing, the volume of entries into homeless shelters, and other factors of need (Been et al., 2018). The full formula is not made available to the public. RTC was rolled out to 3 ZIP codes in each of the 5 boroughs in October 2017; to one ZIP code in each borough in November 2018; and to five additional ZIP codes in December 2019 (Ellen et al., 2021; Office of Civil Justice, 2018, 2019, 2020).[42] Out of the 15 ZIP codes treated in 2017, 10 were already part of the Expanded Legal Services program through which the City provided legal representation in eviction cases for individuals with household incomes at or below 200 percent of the federal poverty line. This ELS program was implemented in 2016-17.[43] The COVID-19 pandemic and subsequent federal eviction moratorium paused the rollout of RTC. In 2022, RTC expanded to all ZIP codes in NYC.

A.2 Data on rental listings

Source and coverage. StreetEasy provided a dataset of georeferenced rental listings in New York City between 2007 and 2020. We drop observations that are missing geographic coordinates or that map to a ZIP code different than the one in the data. The clean StreetEasy dataset contains listings in most New York City ZIP codes (Figure A.1A). Figure A.1C compares the number of listings in each ZIP code with the number of renters according to the 2015-2019 ACS. We see that ZIP codes in the bottom decile of median household income have a large number of renters, but the rental units either have low turnover or are not listed on StreetEasy. Some ZIP codes have more StreetEasy listings than there are renters, but those tend to be in the higher income ZIP codes, which are not included in the RTC rollout.

[42]The specific ZIP codes are: 10457, 10467, 10468, 11216, 11221, 11225, 10025, 10026, 10027, 11373, 11433, 11434, 10302, 10303, and 10314 in October 2017 (Office of Civil Justice, 2018; Ellen et al., 2021); 10462, 11226, 10031, 11385, and 10310 in November 2018 (Office of Civil Justice, 2019); and 10453, 11207, 10029, 10034, and 11691 in December 2019 (Office of Civil Justice, 2020).

[43]See Appendix C.7 for additional details on the ELS program and robustness to how we treat affected ZIP codes in our analysis.

Cleaning and construction of analysis sample. We further process the StreetEasy data to construct our main analysis dataset. We impute missing values for unit characteristics. We create an indicator for whether the listing is missing year of construction, and impute missing values as 1936. We assume that, if information is missing for having a gym, a doorman, central AC, washer/dryer in unit, or a pool, then the listing does not have the corresponding amenity. Note that, if one amenity is missing, all of them are. We create an indicator that equals 1 when the amenities are missing. We top code number of bedrooms and bathrooms to 4. We then drop the top 1% of listings by price and days listed ($12,312 and 500 days, respectively). We then map the StreetEasy listings to the PLUTO dataset, described in Section A.3. We restrict our sample to listings that map to a PLUTO parcel with at least one residential unit. We also exclude listings in buildings built after 2017.

Figure A.1B maps the analysis sample to the ZIP codes where RTC was rolled out and their neighbors. Most of our listings are in Brooklyn, Manhattan and Queens, following the pattern seen in Figure A.1A. Panel D shows that we have a good amount of coverage around the borders across the sample. Table A.1 summarizes the full and analysis samples.

We measure "rent" as the price posted for a given listing on the platform. "Days listed" is the number of days between the date the listing was created and the date the listing expired on the website or was removed.

Table A.2 compares listing, building, and neighborhood characteristics in the treatment and control ZIP codes in the years before the RTC rollout (2014-2017). Columns 1 and 2 show the characteristics for all listings and the listings without amenities. Columns 3 and 4 show characteristics for listings in the treatment and control ZIP codes. Columns 5 and 6 show characteristics for those within one kilometer of the border for treatment and control ZIPs. Comparing the fifth and sixth columns, we see that prices and unit characteristics (bedrooms, bathrooms, building age) are similar before RTC rolled out. There are still some modest differences between treatment and control means. For example, listings on the treated side of the border are in buildings with slightly more eviction cases per unit, though the difference is not statistically significant. The demographic characteristics of the block group (from the American Community Survey) are also similar. However, the listings on the treated side of the border are in block groups with somewhat lower household income and median rents. Comparing our sample near the border (columns 5 and 6) to all listings in the treated and control ZIPs (columns 3 and 4), we see that treatment and control groups are modestly more similar when zooming in on those near the border. For example, the average year built for the buildings differs by 1.4 years near the border, but by 2.9 years when comparing all units in the ZIP code. The differences are also modestly smaller for several other measures, such as the average listing's rental price or median household income. While not required for our research design, the observable characteristics are quite similar on opposite sides of the treatment/control borders. Focusing on units near the border further makes characteristics more similar.

Table A.3 benchmarks listings in StreetEasy against assessed properties from PLUTO and survey responses in the ACS. For each characteristic, we compute ZIP code-level average number of units, average year built, median rent given number of bedrooms, and average number of bedrooms. We then report the mean, 25th, 50th, and 75th percentiles across ZIP codes to provide details on the distribution of characteristics. We calculate ZIP-code level aggregates for each sample to make the comparison to the American Community Survey, which is at the ZIP code (ZCTA) level. The use of ZIP-code aggregates is also why the numbers differ from other summary statistics provided elsewhere in the paper. The first four columns show characteristics for: the full StreetEasy sample, restricted to Treatment/Control ZIPs, restricted to being near the borders, and restricted to listings that do not include amenities. Columns 5-7 show building characteristics from PLUTO, restricted to properties with residential units. Columns 8-9 show estimates from the 2016 5-year ACS for all of NYC and for the treated and control ZIPs. Given the aggregate nature of the 5-year ACS data, we cannot restrict to units near the treatment/control borders. A limitation of the ACS is that we can only measure rents for the stock of renters, rather than new listings.

For building characteristics, our final StreetEasy sample has more units per building than PLUTO. This difference is likely due to single-unit dwellings that are not rentals, and the listings data will naturally have more listings in large buildings than in small ones. Compared to the ACS, the final StreetEasy sample has, on average, higher mean units per building but a lower median. Similarly, the average year built for StreetEasy falls between the PLUTO and ACS data. Median rents are a bit higher in our final StreetEasy sample than in the ACS units in the treatment and control ZIP codes. For Studio and one-bedroom apartments, StreetEasy rents are approximately 34% higher, with the gap widening for larger apartments. This may be driven in part by higher rents for new listings compared to those for long-term tenants.

Table A.1: Summary Statistics

	Full	Analysis
Years	2013-2020	2014-2019
Average rent	3215.9	2384.6
	(2492.6)	(805.3)
% Brooklyn	34.8	60.6
% Bronx	1.9	5.8
% Manhattan	50.9	26.1
% Queens	12.3	7.5
% Staten Island	0.1	0.0
Avg. no. bedrooms	1.5	1.9
	(1.1)	(1.1)
Avg. year built	1943.0	1927.1
Avg. no. units in building	121.0	26.4
	(544.2)	(51.3)
% rent stabilized	23.4	47.3
Avg. assessed value per unit	93347.8	24092.8
Number of listings	1593163	381731

Note: This table summarizes the StreetEasy rental listings dataset used in our analysis. Numbers in parentheses are standard deviations. Section 2.4 and Appendix A describe the data in greater detail.

Table A.2: Comparing treatment and control neighborhoods before RTC

	Full sample		Analysis sample		Within 1 km of border	
	Full	No Amenities	Treatment	Control	Treatment	Control
Price (USD)	3271.69	2706.47	2297.05	2319.92	2340.79	2358.51
	(2651.02)	(1701.70)	(768.39)	(822.01)	(812.37)	(822.99)
Bedrooms	1.51	1.59	1.89	1.83	1.89	1.85
	(1.04)	(1.04)	(1.05)	(1.02)	(1.06)	(1.04)
Bathrooms	1.16	1.09	1.11	1.10	1.12	1.10
	(0.47)	(0.39)	(0.38)	(0.37)	(0.39)	(0.37)
Year built	1940.70	1925.05	1927.18	1930.06	1926.16	1927.56
	(38.08)	(26.91)	(26.16)	(28.66)	(26.30)	(27.29)
Rent control	0.32	0.43	0.50	0.50	0.50	0.51
	(0.46)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Condo	0.13	0.04	0.02	0.03	0.02	0.03
	(0.33)	(0.19)	(0.14)	(0.17)	(0.14)	(0.16)
Cases per parcel,	1.61	1.84	3.69	3.02	3.67	3.12
rescaled by no. units	(3.90)	(2.88)	(3.93)	(3.64)	(3.94)	(3.59)
Median HH income (1000s USD)	90.37	79.32	51.76	56.42	52.01	55.27
	(45.52)	(39.63)	(17.76)	(27.49)	(19.23)	(27.70)
Median rent (USD)	1892.94	1694.48	1290.23	1392.69	1291.57	1369.08
	(655.04)	(521.85)	(248.79)	(357.24)	(263.24)	(356.23)
Renter share of households	77.26	77.63	83.15	80.16	83.06	81.79
	(18.38)	(18.08)	(14.16)	(17.31)	(13.98)	(16.14)

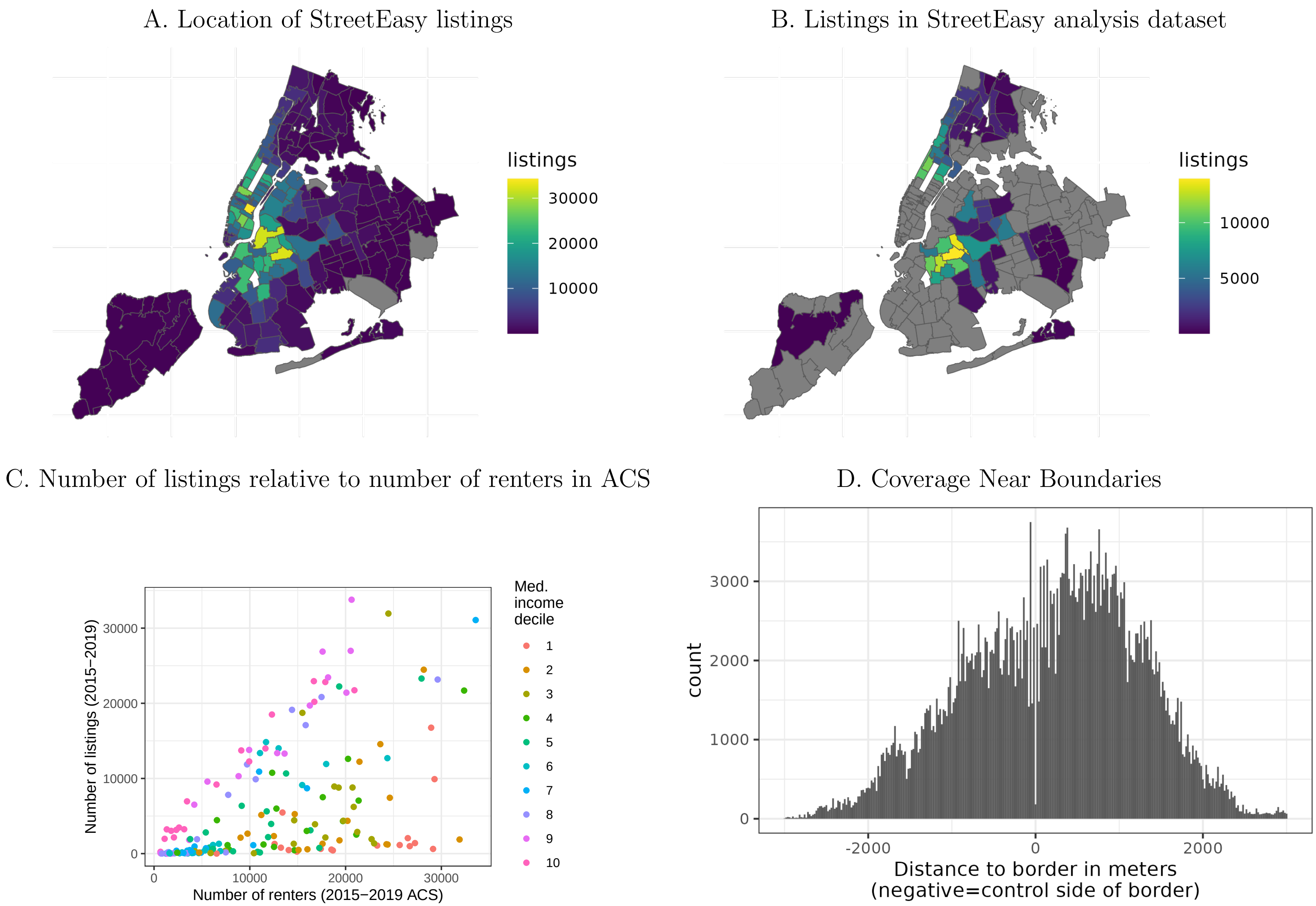
Note: This table summarizes unit, building and neighborhood characteristics for StreetEasy listings between August 2014 and August 2017. "Full sample" corresponds to all listings in New York City, i.e., the raw data. "Full sample, No Amenities" subsets to listings without central AC and in buildings without a gym, doorman, or pool. The "analysis sample" corresponds to all listings in ZIP codes where RTC was rolled out in 2017 or 2018 ("Treatment") and in neighboring ZIP codes ("Control"). The last two columns restrict the analysis sample to listings within 1km of a ZIP code border. Median household income, median rent, and renter share of households are measured using the 2016 5-year ACS, at 2010 Census Block Group level.

Table A.3: Comparison of characteristics across data sets and samples

variable	StreetEasy (Full)	StreetEasy (Treat/Control ZIPs)	StreetEasy (Borders)	StreetEasy (Borders, No Amen.)	PLUTO	PLUTO (Treat/Control ZIPs)	PLUTO (Borders)	ACS	ACS (Treat/Control ZIPs)
Avg. Units									
Mean	113.2	70.3	66.9	57.2	35.4	9.9	8.4	33.7	35.9
25th Percentile	19.3	22.2	23.3	20.9	2.5	3.9	2.5	14.8	24.5
Median	44.6	43.3	41.5	34.2	4.9	6.5	4.6	36.2	40.1
75th Percentile	109.1	77.0	72.6	65.0	19.6	12.8	11.2	47.1	46.6
Avg. Year Built									
Mean	1945	1936	1938	1932	1937	1930	1930	1958	1954
25th Percentile	1934	1929	1931	1928	1923	1921	1921	1951	1949
Median	1941	1936	1936	1932	1935	1928	1932	1956	1953
75th Percentile	1953	1941	1945	1937	1944	1938	1939	1962	1957
Median Rent (Studio)									
Mean	1,873.6	1,544.2	1,562.7	1,527.7				1,373.5	1,134.3
25th Percentile	1,400.0	1,372.5	1,381.2	1,356.2				1,025.5	982.5
Median	1,550.0	1,550.0	1,500.0	1,500.0				1,194.0	1,132.0
75th Percentile	2,000.0	1,672.5	1,695.0	1,650.0				1,573.0	1,255.5
Median Rent (1 Bdr)									
Mean	2,051.5	1,794.3	1,814.5	1,753.1				1,573.9	1,311.6
25th Percentile	1,550.0	1,550.0	1,597.5	1,550.0				1,209.2	1,186.0
Median	1,722.0	1,750.0	1,750.0	1,700.0				1,356.0	1,308.0
75th Percentile	2,400.0	1,921.0	1,972.5	1,899.5				1,702.8	1,394.5
Median Rent (2 Bdr)									
Mean	2,596.2	2,193.0	2,208.4	2,129.3				1,691.1	1,429.5
25th Percentile	1,900.0	1,886.0	1,962.5	1,924.5				1,375.2	1,247.5
Median	2,150.0	2,100.0	2,150.0	2,100.0				1,548.0	1,416.0
75th Percentile	2,823.8	2,350.0	2,363.5	2,297.5				1,786.8	1,592.5
Median Rent (3 Bdr)									
Mean	3,300.4	2,738.3	2,740.8	2,640.9				1,846.2	1,647.2
25th Percentile	2,300.0	2,300.0	2,300.0	2,275.0				1,551.0	1,399.0
Median	2,550.0	2,550.0	2,600.0	2,500.0				1,797.0	1,696.0
75th Percentile	3,499.5	2,875.0	2,872.8	2,843.5				2,070.5	1,834.0
Median Rent (4 Bdr)									
Mean	4,297.5	3,336.9	3,369.8	3,274.0				1,887.1	1,800.5
25th Percentile	2,700.0	2,583.5	2,612.5	2,612.5				1,553.8	1,479.5
Median	3,200.0	3,100.0	3,200.0	3,147.5				1,893.0	1,853.5
75th Percentile	4,500.0	3,625.0	3,798.0	3,678.4				2,168.8	2,116.2
Avg. Bedrooms									
Mean	1.7	1.7	1.7	1.7				1.8	1.8
25th Percentile	1.4	1.5	1.5	1.5				1.6	1.7
Median	1.7	1.7	1.7	1.7				1.8	1.8
75th Percentile	2.0	1.9	2.0	1.9				2.0	1.9

Note: This table compares the StreetEasy data to other data sets. We compute the ZIP code-level mean number of units, mean year built, median rent given number of bedrooms, and mean number of bedrooms. We then report the mean, 25th, 50th, and 75th percentiles of each statistic across ZIP codes. The first four columns show characteristics for the full StreetEasy sample, restricted to Treatment/Control ZIPs, restricted to being near the borders, and restricted to listings that do not include amenities. Columns 5–7 show building characteristics from PLUTO, restricted to properties with residences. Columns 8–9 show estimates from the 2016 5-year ACS for all of NYC and for the treated and control ZIPs.

Figure A.1: StreetEasy Coverage



Note: This figure shows the coverage of the StreetEasy data. Panel A maps the number of StreetEasy listings by ZIP code from 2015 to 2019. Panel B maps the number of "no amenities" StreetEasy listings by ZIP code for treated ZIP codes and their neighbors. Panel C compares the number of listings in each ZIP code to the number of renters in the 2015-2019 5-Year American Community Survey (ACS). Panel D shows a histogram of distance to the border for all border pairs in our analysis sample. Section 2.4 and Appendix Section A describe the data in greater detail.

A.3 Parcel characteristics

Source and coverage. The Primary Land Use Tax Lot Output (PLUTO) tax lot data combines datasets maintained by the Department of City Planning (DCP), Department of Finance (DOF), Department of Citywide Administrative Services (DCAS), and Landmarks Preservation Commission (LPC). We use these data, including MapPLUTO, to construct a parcel-level dataset for the entire City of New York.

Cleaning and construction of analysis sample. We use the 2016 MapPLUTO shapefile to define a parcel. We map the parcels in 2015, 2017, 2018, 2019 and 2020 to the 2016 shapefile to construct a panel dataset at the parcel level. Mapping the parcels to a single shapefile allows us to identify condominium conversions or parcel mergers, which respectively translate into creation or merging of parcels in the shapefile. For each parcel and every year of PLUTO data, we calculate the number of condos, number of newly apportioned lots, total number of residential units, total residential area and total assessment value. Each vintage is measured by April of that year, so we use data from vintage t to characterize $t-1$. We drop parcels with no residential units, no residential area or that show up in fewer than 4 vintages of the PLUTO data between 2015 and 2020. For each parcel, we calculate the number of evictions between 2010 and 2015 using data from Collinson et al. (2024).

A.4 Building permits

Source and coverage. New York City's Department of Buildings (DOB) and Department of City Planning (DCP) issue a dataset containing all approved housing construction and demolition jobs filed or completed since January 1, 2010. This dataset includes three job types that add or remove residential units: new buildings, major alterations, and demolitions. DCP conducts preliminary data cleaning, which is described on the Housing Database page on "Bytes of the Big Apple."

Cleaning and construction of analysis sample. We restrict our building permits dataset to permits filed between September 1, 2014 and December 31, 2019. We group permits with the exact same job description, and use the earliest filing date.

A.5 Inspections and violations

Source and coverage. Our inspections and violations data come from the Department of Housing Preservation and Development (HPD). Complaints are made by the public through the 311 Citizen Services Center, Code Enforcement Borough Offices or the internet for conditions which violate the New York City Housing Maintenance Code (HMC) or the New

York State Multiple Dwelling Law (MDL). HPD records the housing inspections made in response to these public complaints. If the inspector finds evidence for a code violation, then a violation notice is issued.

Cleaning and construction of analysis sample. We restrict our complaints dataset to those filed between September 1, 2014 and December 31, 2019. We group complaints with the exact same description, and use the earliest filing date. We use the New York State Multiple Dwelling Law Section 302 definition of a rent-impairing violation, which is "a condition within a multiple dwelling which constitutes, or if not promptly corrected will constitute, a fire hazard or a serious threat to the life, health or safety of occupants thereof."

A.6 Tenant migration and screening

We use consumer reference data from Infutor Data Solutions which cover residential address histories from the mid-1990s to January 2022. These data provide information on individual address histories for adults living in every state in the U.S., including a date of move-in, the precise address, and identifiers such as name, birth-month and birth-year, and social security numbers (where available). These data are derived primarily from phone plan data, deed and property information, subscription services, and, in the 1990s, consumer credit bureaus.

We use a cleaning approach that mirrors Phillips (2020). The Infutor data record sequential address spells for each individual. Moves are constructed by linking each address spell to the next distinct address for the same person; the move date is defined as the start date of residence at the destination address. We drop records associated with Post Office Boxes. In cases where two distinct addresses have the same start date, we retain the address with the later end date; when a given address has two distinct start dates, we retain the record with the earlier start date. Address spells for which the timing of either the origin or destination address is missing are also dropped. We further restrict to addresses that geocode to within 50 feet of a valid NYC parcel, as identified by its Borough-Block-Lot number (BBL).

To construct the analysis sample, we restrict to individuals who move to New York City at some point between November 2014 and December 2019. We sent Experian the person $\times$ NYC-address pairs for all such in-migrants. Experian then returned an anonymized panel of consumer-level credit report data observed in February and August for each matched individual. 52% of movers in the Infutor data match to Experian data, which is comparable to other papers that match Infutor to credit bureau data to remove fragmentary records from the Infutor data (Diamond et al., 2020; Blattner and Nelson, 2024).

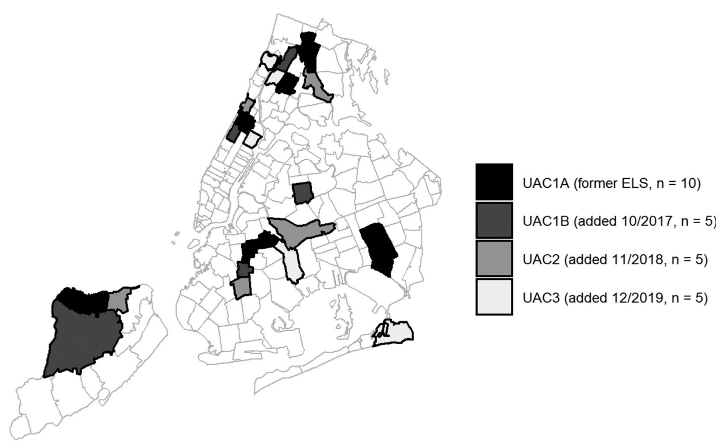
We also link each individual to the 2015-2019 5-year ACS data on the Census tract that the individual moved from in the Infutor address panel, which we use to study additional characteristics of moves into treated and control ZIPs. We drop observations moving from census tracts for which the ACS data on median income is missing.

For our analysis of how RTC affects migration patterns, we subset to individuals with a move into a treated or control ZIP observed in Infutor at any time in our analysis period. To focus on renters, we also restrict to individuals with no mortgage debt. Our analysis uses the Experian data observed most recently *prior* to each individual's move (for example, we use August 2017 data from Experian for an individual with a move to a relevant ZIP code in November 2017).

B Additional details on stacked dataset construction

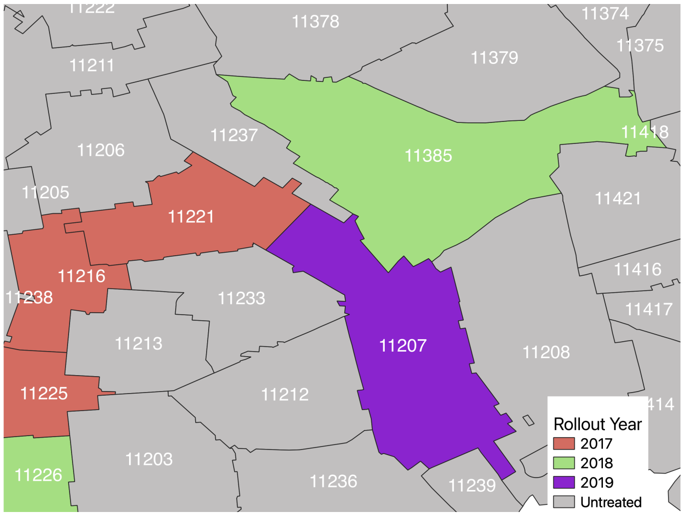
This appendix describes the construction of our analysis dataset. Figure B.1 shows the date when RTC was rolled out in each ZIP code, according to Ellen et al. (2021). Figure B.2 zooms into a subset of the ZIP codes in Figure B.1. Figure B.3 contains a decision tree that summarizes the process by which border pairs are included or excluded from the analysis sample. Section 3.1 describes the dataset construction in detail.

Figure B.1: Rollout of Universal Access to Counsel in New York City



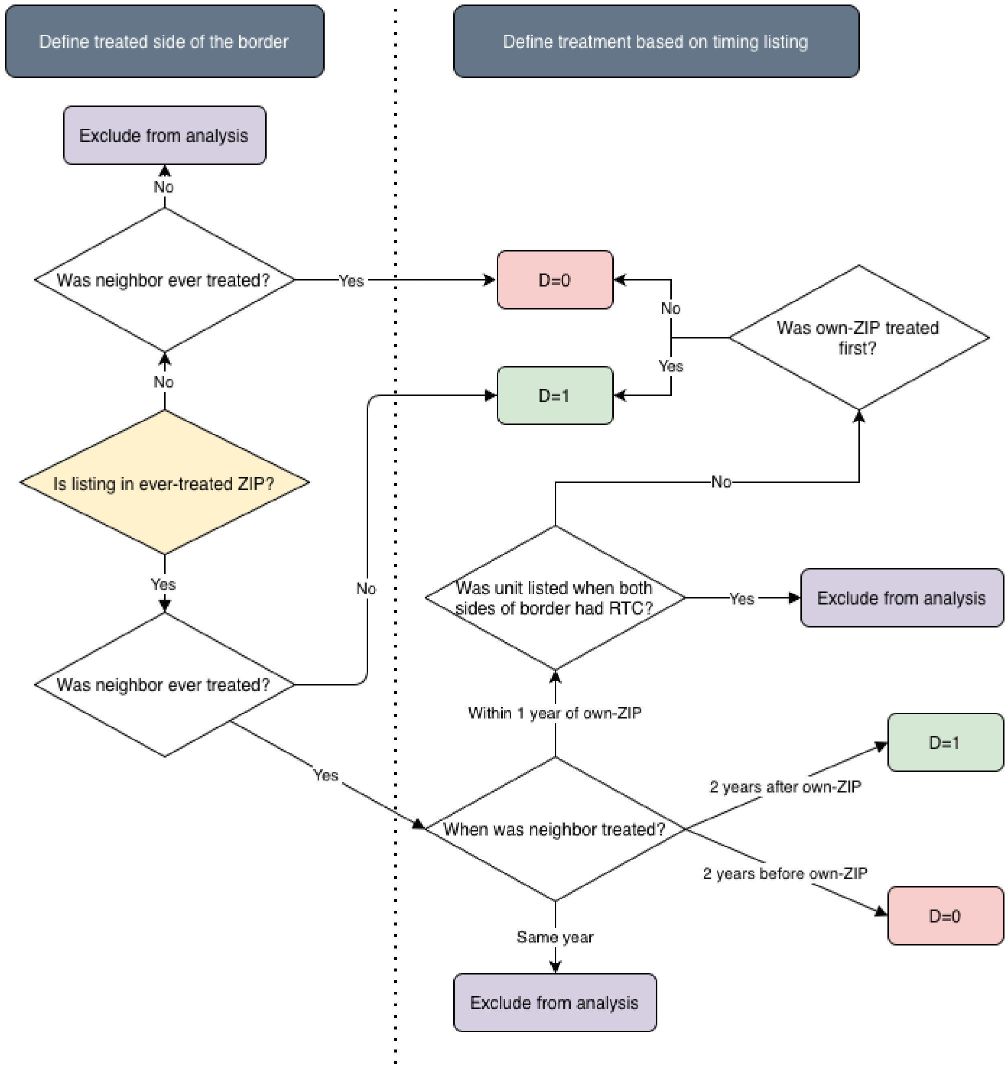
Note: This figure from Ellen et al. (2021) maps the rollout of Universal Access to Counsel in New York City.

Figure B.2: Example of ZIP codes and their borders



Note: This figure maps New York City ZIP codes that rolled out RTC in different years, if ever. Jointly with Appendix Figure B.3, it illustrates which ZIP code border pairs were used in the construction of our stacked dataset.

Figure B.3: Construction of Stacked Dataset



Note: This diagram summarizes the decisions made when selecting ZIP code border pairs for our stacked analysis dataset, described in greater detail in Section 3.1. Appendix Figure B.2 maps ZIP codes treated in different years, or never treated (during our analysis period).

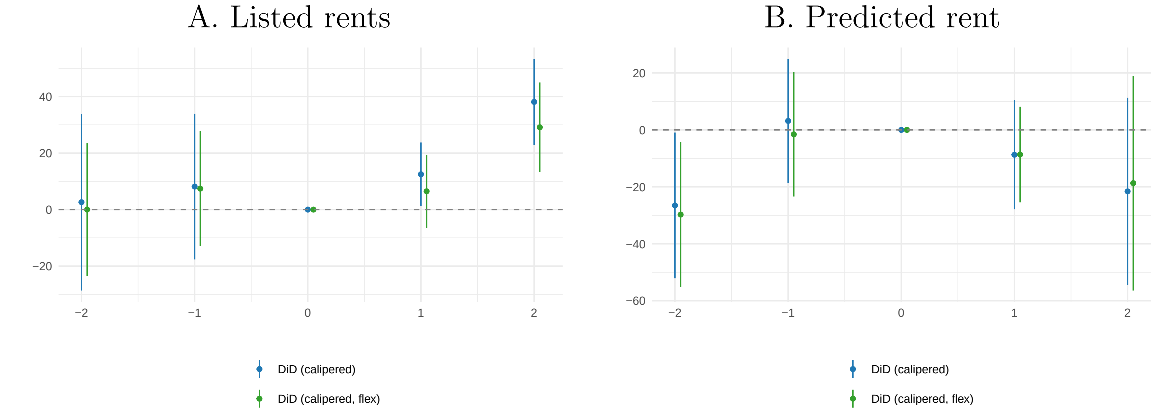
C Additional details on empirical results

This section provides additional details and robustness for the analysis of RTC's impact on rental price, quantity, and habitability, as well as on tenant characteristics.

C.1 Pre-trends across New York City border pairs

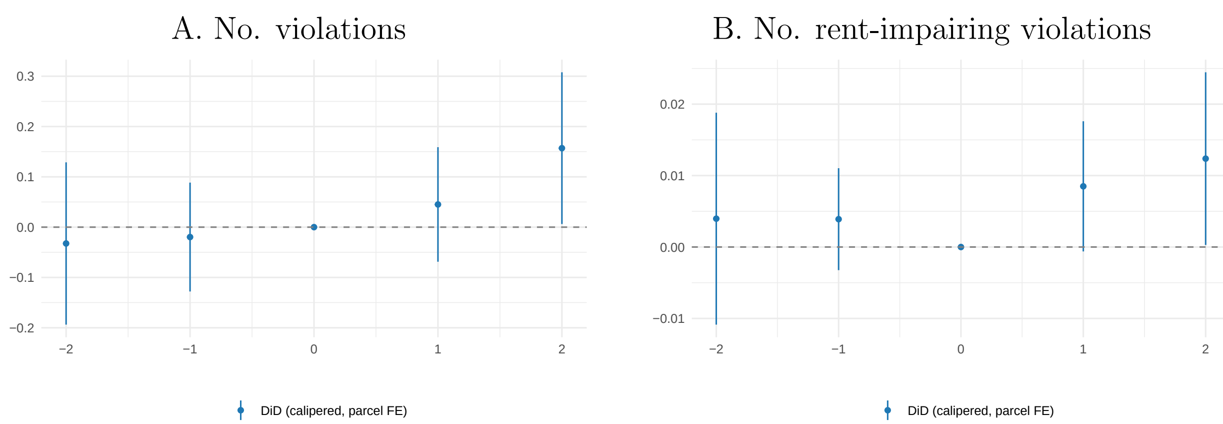
This section plots raw means and event study coefficients that correspond to the tables discussed in Section 3.2.

Figure C.1: Event Studies of Impacts of RTC: Prices and Unit Quality



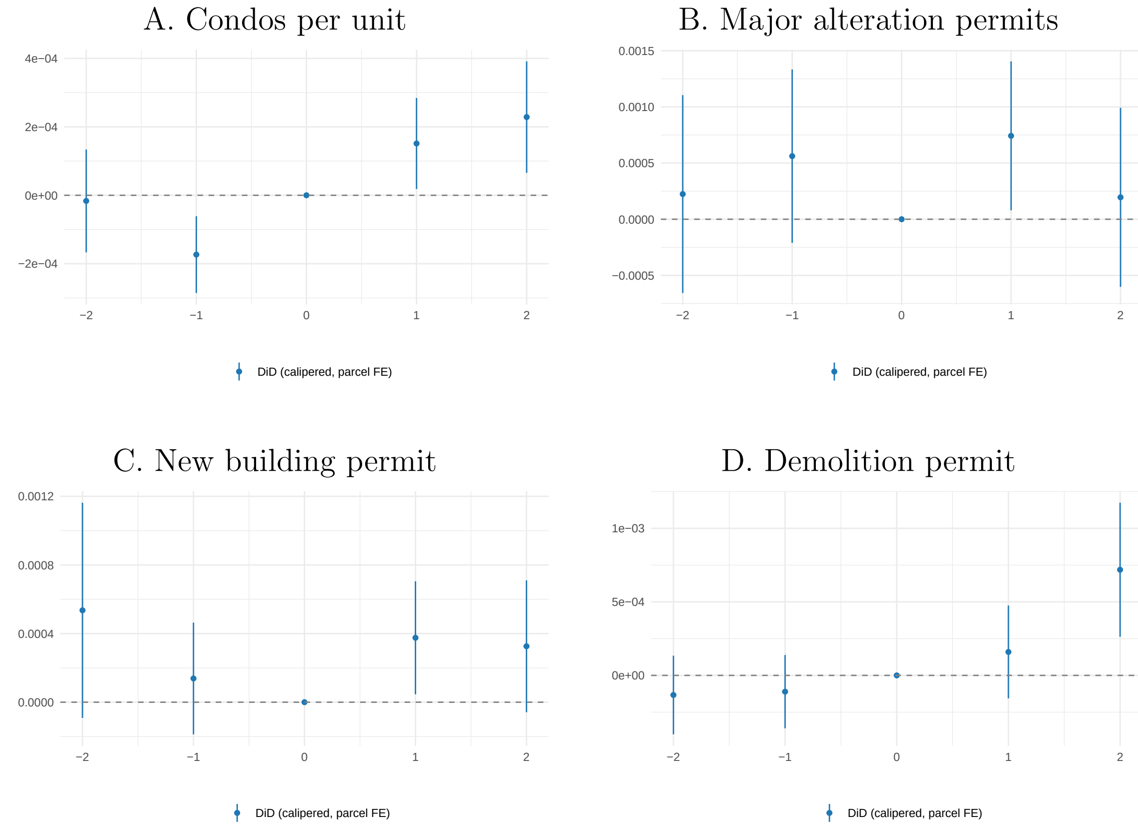
Note: This figure plots event study coefficients corresponding to the estimates in Table 2 (panel A) and Table C.5 (panel B). "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year -1" compares listings created 365-730 days prior to the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year -2" compares listings created 730-1095 days prior to the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. The bars correspond to the 95% confidence interval. Our analysis sample only includes "no amenities" listings, i.e. those without central AC, a gym, doorman, or pool. The specifications in Panel A control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. Panel B controls for the year when the listing was created. Predicted rent is calculated as follows. First, we regress prices in the pre-period on building age and building age squared; an indicator for whether building age was imputed; an indicator for whether the listing was missing data on amenities; an indicator for whether the building has an elevator, a garage, or is mixed use; bedroom count dummies; bathroom count dummies; indicators for whether the unit has a dishwasher, central AC, or laundry; unit square footage; and border-pair fixed effects. Using the coefficients from the hedonic regression, we calculate predicted rent for all listings in our analysis sample, both in the pre- and post-periods.

Figure C.4: Event Studies of Impacts of RTC: Upkeep and Habitability



Note: This figure plots event study coefficients corresponding to the estimates in Table C.16. We restrict our analysis to parcels that include housing within 1000m of the ZIP code border. The bars correspond to the 95% confidence interval. All specifications use a bandwidth of 1000 meters. "Year 1", "Year 2", "Year -1" and "Year -2" are estimated jointly. We include parcel-by-border pair and year-relative-to-treatment fixed effects.

Figure C.2: Event Studies of Impacts of RTC: Quantity responses



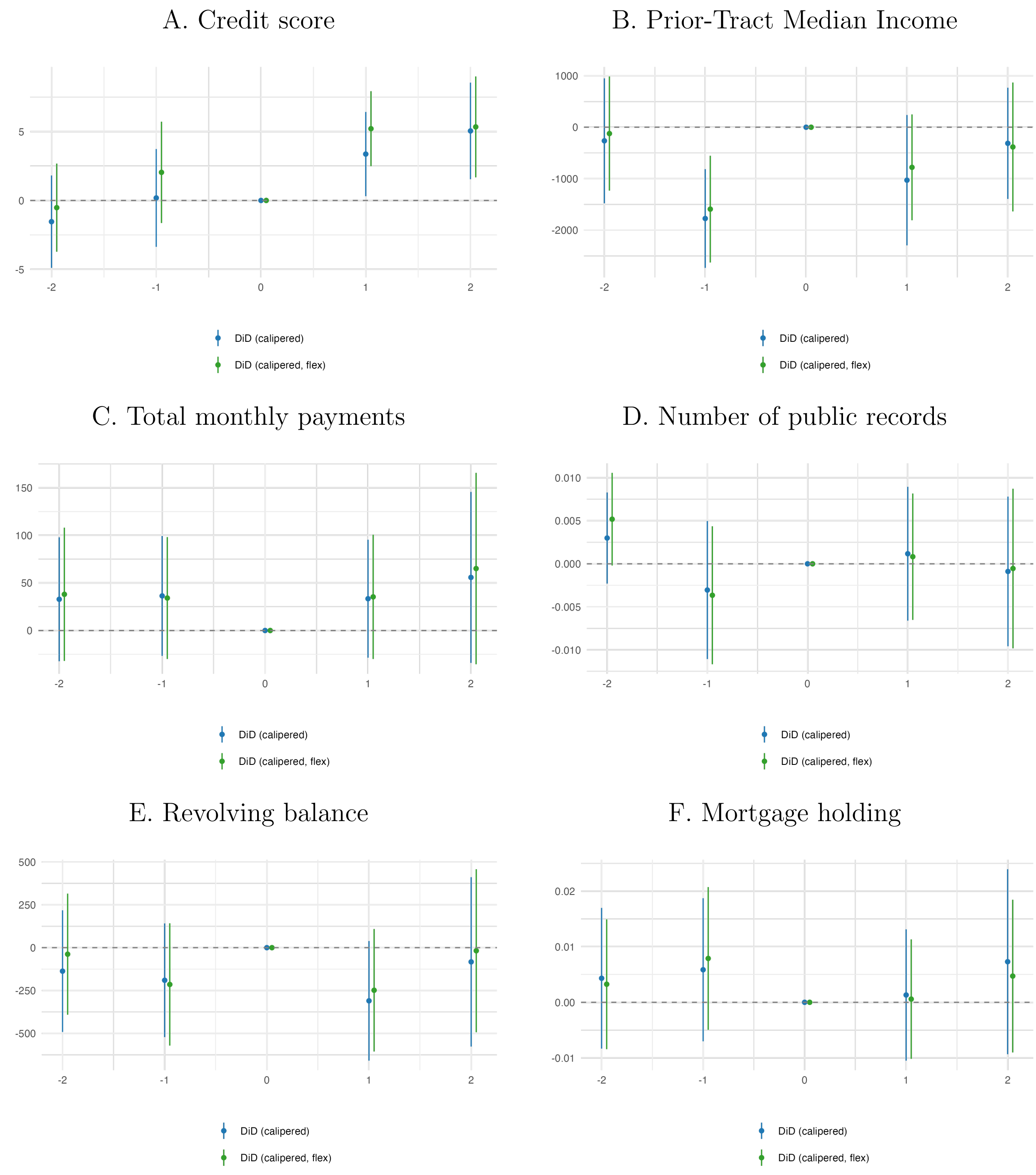
Note: This figure plots event study coefficients corresponding to the estimates in Table 3. We restrict our analysis to parcels that include housing within 1000m of the ZIP code border. The bars correspond to the 95% confidence interval. "Year 1", "Year 2", "Year -1" and "Year -2" are estimated jointly. We include parcel-by-border pair and year-relative-to-treatment fixed effects.

C.2 Additional estimates of impact of RTC

This section provides alternative estimates of RTC's rent price effects. First, Figure C.5 plots the raw means for listings near the borders for treatment and control without adjustment. We see that trends are broadly parallel from year -2 to year 0, and then the treated group's average rent increases more quickly, catching up with and then surpassing the control-group's mean.

Second, we report estimates of how the impacts of RTC on rent vary by baseline eviction filing rates in the ZIP codes prior to the RTC roll-out (average eviction filing rate between 2011 and 2015). Figure C.6 shows estimates conditional on the treated ZIP code being above or below the median eviction rate for our analysis sample of treated and bordering ZIP codes. The left panel shows results for year 1, while the right panel shows results for year 2, both for our main difference-in-difference specifications. In year 2, the estimated impacts on rent are notably higher in the ZIP codes with above-median eviction filing rates. However, we cannot reject the null hypothesis that the estimates are equal, likely because the larger standard

Figure C.3: Event Studies of Migration Effects of RTC: Tenant Characteristics



Note: This figure plots event study coefficients corresponding to the estimates in Table C.15. The bars correspond to 95% confidence intervals. All specifications use a bandwidth of 1000 meters. The figure uses Infutor data on all in-migrants to treated or control ZIP-border areas in New York City between November 2014 and December 2019, linked to Experian credit report data. The Experian data are recorded twice per year in February and August. When merging with the Infutor data, we use the most recent previously recorded credit report for a given move, except in Panel F, where we use the first credit report observed subsequent to a move. The median household income of the previous tract is obtained from the five-year ACS (2015-2019) dataset. This specification does not include any additional controls X_i from equations 1 and 2.

errors associated with splitting the sample make the test less powerful.

Third, we report estimates of the impact of RTC on rents using log-rent as the outcome (Table C.1). As mentioned in Section 2.4, we use listed rental prices rather than contracted rents, which may not be identical. For example, contracted rents may be lower for units on the market for an extended period (Apartment List, 2024). The log rent specification is more robust if our rental prices are all modestly overstated.

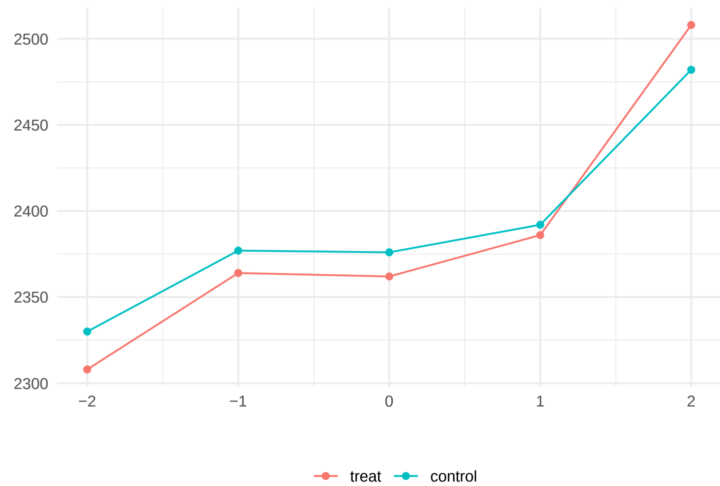
We additionally include robustness to our other data construction and modeling choices. Table C.2 shows that results are similar regardless of the choice of caliper around the border. Table C.3 shows robustness to the construction of our analysis sample. Results are similar or larger when dropping units that likely have rent control, restricting to units with high eviction rates, or when dropping listings one month before or after the policy went into effect to avoid potential anticipation or short-term delays. If we use the full sample, including higher-end units with amenities, the results are smaller and noisier, consistent with such units likely not being affected by the policy. Table C.4 shows that results are robust to our choice of control group (i.e. excluding ZIPs from the control group that are treated the next year, or excluding ZIPs from the control group that are ever treated in our time window). The prior two tables together provide some evidence against landlords changing behaviors in anticipation of the policy as results are very similar when dropping listings the month before the policy or using the never-treated ZIPs as controls, where landlords may have anticipated they were less likely to be treated in the near future. While these are imperfect tests for anticipation, we believe anticipation would likely lead to underestimates of the overall impact on rents. Table C.6 provides further evidence that our estimates are robust to how we specify our DiD regression and our choice of donut around the border. Table C.7 shows that our estimates are very similar when including a larger set of hedonic controls.

Table C.1: Impact of RTC rollout on log listed rents

	(1) Year 1 effect	(2) Year 2 effect
DiD (calipered)	0.004**	0.015***
	(0.002)	(0.003)
DiD (calipered, flex)	0.002	0.012***
	(0.002)	(0.004)
Observations	179578	120229
Pre-period mean	7.723	7.752

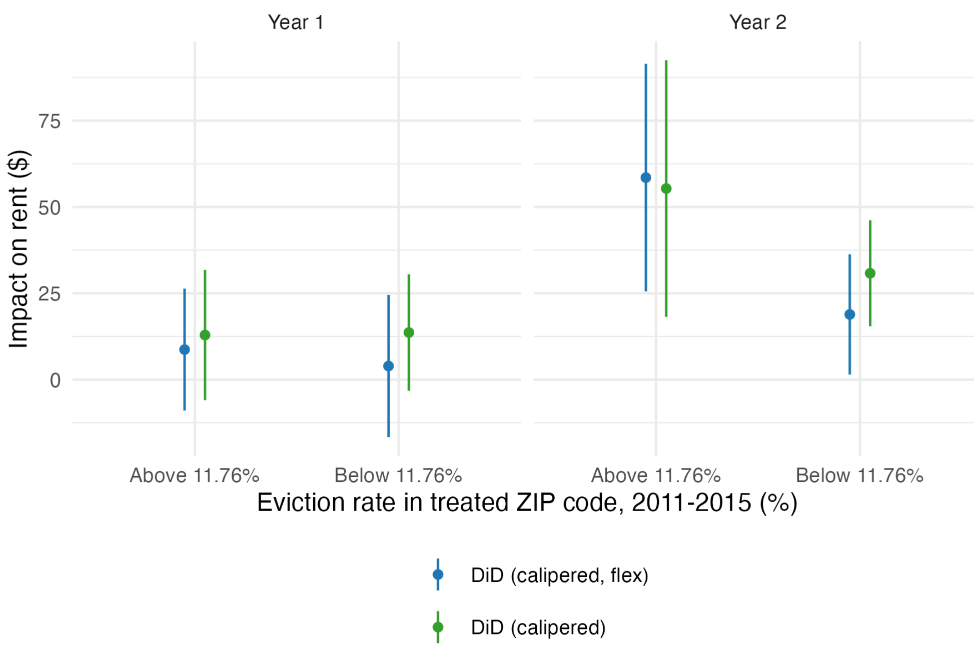
Note: This table reports estimates of the impact of RTC on log listed rent prices. "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. Our analysis sample only includes "no amenities" listings, i.e., those without central AC, a gym, doorman, or pool. We control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. *p<0.1; **p<0.05; ***p<0.01

Figure C.5: Average listing price within 1000m of the ZIP code border



Note: This figure plots average rent prices for StreetEasy listings in our no amenities analysis sample, within 1km of the border. The year 2 coefficients exclude listings in border pairs where one side of the border rolled out RTC in 2017, and the other rolled out in 2018.

Figure C.6: Impact on rent, given treated ZIP eviction filing rate in 2011-2015



Note: This figure reports estimates of the impact of RTC on rents conditional on pre-RTC eviction filing rates. The left panel reports year 1 results split by if the border's treated ZIP code was above or below the median average eviction filing rate in our sample between 2011 and 2015 (11.76%). The right panel reports the same results for year 2. We include the same controls used in our main specifications. Standard errors are clustered at the border-pair level. The line through each dot denotes a 95% confidence interval.

Table C.2: Impact of RTC rollout on listed rents, different caliper around borders

	(1) 500m	(2) 1000m	(3) 1500m	(4) 2000m
Panel A. Year 1				
DiD (calipered)	8.53	12.51**	15.66***	16.21***
	(7.23)	(5.74)	(4.91)	(4.89)
DiD (calipered, flex)	2.77	6.47	9.49*	12.65**
	(6.19)	(6.60)	(5.54)	(5.77)
Observations	87112	179578	243192	276374
Pre-period mean ($/month)	2374.25	2361.70	2335.24	2321.39
Panel B. Year 2				
DiD (calipered)	43.58***	38.11***	44.97***	44.50***
	(8.39)	(7.74)	(8.21)	(8.69)
DiD (calipered, flex)	32.45***	29.13***	36.95***	38.56***
	(5.94)	(8.09)	(7.90)	(8.25)
Observations	60101	120229	160309	180779
Pre-period mean ($/month)	2476.19	2439.79	2415.90	2404.16

Note: This table reports estimates of the impact of RTC on listed rent prices for different calipers. "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. Our analysis sample only includes "no amenities" listings, i.e., those without central AC, a gym, doorman, or pool. We control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. *p<0.1; **p<0.05; ***p<0.01

C.3 Robustness to sample and weights selection

This section estimates our main rent effects using a subset of border pairs and an alternative set of border-pair-by-year weights (Table C.8).

In our main analysis, we exclude listings in border pairs where both sides have been treated. This means that the sample of border pairs changes between the Year 1 and Year 2 estimates. Table C.8 restricts the analysis to a "stable" set of border pairs, i.e., border pairs that are always included in the analysis. These "stable" pairs are those where RTC was rolled out in 2017 on one side, and in 2019 or later on the other. Columns (3) and (4) reweight borders using the number of listings in each border in a given year, relative to the number of listings in the year before RTC was rolled out. This exercise addresses potential concerns that the number of listings in each border-pair changes with the policy, and that changes in the composition may explain our estimated rent effects.

Table C.3: Impact of RTC rollout on listed rents, different samples

	(1) Full sample	(2) Main	(3) Main + no rent control	(4) Main + bldg eviction rate above 5 pct	(5) Main + no anticipation
Panel A. Year 1					
DiD (calipered)	10.45	12.51**	21.28**	11.50*	13.25**
	(7.07)	(5.74)	(9.28)	(6.49)	(5.70)
DiD (calipered, flex)	7.53	6.47	14.19	11.81*	6.82
	(6.61)	(6.60)	(11.50)	(6.16)	(6.01)
Observations	262903	179578	88004	163809	164237
Pre-period mean ($/month)	2503.97	2361.70	2757.79	2425.67	2362.39
Panel B. Year 2					
DiD (calipered)	18.72	38.11***	40.50***	42.43**	38.31***
	(14.54)	(7.74)	(12.30)	(16.99)	(8.18)
DiD (calipered, flex)	15.31	29.13***	33.68***	33.94***	29.74***
	(9.77)	(8.09)	(11.40)	(12.51)	(8.50)
Observations	185085	120229	70967	109263	115679
Pre-period mean ($/month)	2591.73	2439.79	2801.54	2527.29	2437.45

Note: This table reports estimates of the impact of RTC on listed rent prices for different samples. "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. Column (1) estimates the impacts using the full sample (i.e., including apartments with central AC, gyms, door persons, or pools). Column (2) is our main sample, which only includes "no amenities" listings, i.e., those without central AC and in buildings without a gym, doorman or pool. Column (3) builds on (2), and additionally excludes listings in buildings that are likely rent stabilized. To define the sample of units that are likely not rent stabilized, we assume rent stabilized listings are those in buildings built before 1974, with at least 5 units and with rent below certain thresholds, following NYC guidelines. Column (4) restricts the main sample to buildings where the pre-RTC annual eviction filing rate is at least five percent of the number of units in the building. Column (5) replicates column (2), but drops units listed one month before or after the policy went into effect to account for potential anticipation effects or time taken to scale up. We control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. *p<0.1; **p<0.05; ***p<0.01

C.4 Challenges in measuring days listed and number of listings

The nature of our listings data limits our ability to accurately measure the total number of unique listings or the overall duration for which an apartment is listed. There are three major empirical challenges. First, some landlords may use a single listing for multiple similar units in one building, while others may use separate listings for each unit. Second, if a unit goes unfilled, some landlords may choose to sequentially re-post a unit, while others may have one listing posted for a longer period of time. Moreover, if landlords change language or details about this listing, it can be challenging to identify when two sequential postings are for the same unit. Third, landlords may not immediately remove listings once a unit is filled. For example, a landlord may just forget to remove the listing, or they may leave a listing open to collect backup applications while finalizing a lease. Indeed, Figure C.7 shows that there is

Table C.4: Impact of RTC rollout on listed rents, different control groups

	(1) Main	(2) Excl. control treated in t+1	(3) Excl. ever-treated control
Panel A. Year 1			
DiD (calipered)	12.51**	13.45**	11.25*
	(5.74)	(6.07)	(6.39)
DiD (calipered, flex)	6.47	6.00	5.44
	(6.60)	(7.11)	(7.59)
Observations	179578	165090	156425
Pre-period mean	2361.70	2364.27	2358.40
Panel B. Year 2			
DiD (calipered)	38.11***	38.11***	39.98***
	(7.74)	(7.74)	(8.28)
DiD (calipered, flex)	29.13***	29.13***	33.39***
	(8.09)	(8.09)	(7.75)
Observations	120229	120229	110610
Pre-period mean	2439.79	2439.79	2436.67

Note: This table reports estimates of the impact of RTC on listed rent prices using different control groups. Column (1) replicates the estimates from Table 2. Column (2) excludes border-pairs where the control side of the border is treated the year after the treatment side. This does not affect the "Year 2" sample because our sample restrictions already drop border pairs in which a ZIP code was treated in 2018, so the Year 2 samples and estimates are identical in columns (1) and (2). Column (3) excludes all border-pairs where both sides of the border are treated during our study period. "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. Our analysis sample only includes "no amenities" listings, i.e., those without central AC, a gym, doorman, or pool. We control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. *p<0.1; **p<0.05; ***p<0.01

bunching around posts lasting 7, 14, and 30 days, which suggests that listing duration only provides a noisy proxy. In total, the three challenges above make it difficult to use the noisy signals provided by the listings data to accurately measure quantity or duration.[44]

We attempt to mitigate differential listing length behavior by linking sequential listings for the same unit. First, we identify cases where two or more listings share all the same characteristics, including unit number, but one listing ends within 7 days of another listing

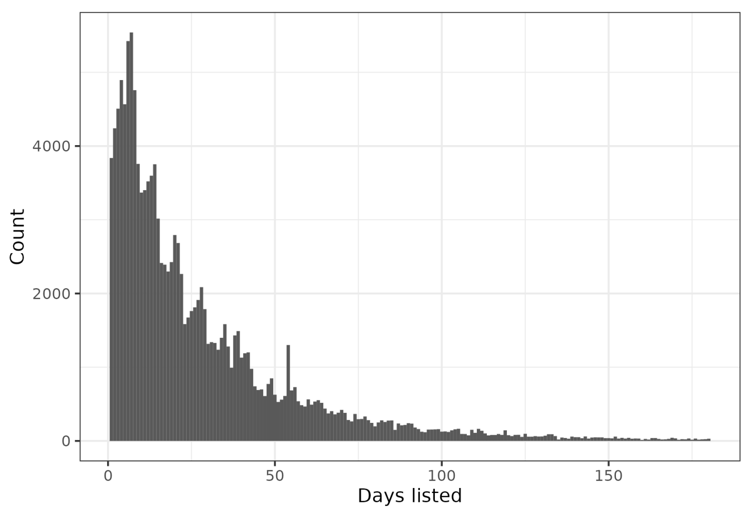
[44]Note that these same issues have a much smaller impact on our rent price estimates given we do not need to know how long the property is listed. Similarly, if we double-count or under-count some of the sequential listings, this would result in us reweighting our sample, not mismeasuring posted rents.

Table C.5: Impact of RTC rollout on predicted rent

	(1) Year 1 effect	(2) Year 2 effect
DiD (calipered)	-8.72	-21.59
	(9.78)	(16.78)
DiD (calipered, flex)	-8.65	-18.70
	(8.57)	(19.24)
Observations	175120	116877
Pre-period mean ($/month)	2362.22	2461.52

Note: This table reports estimates of the impact of RTC on unit quality, proxied by predicted prices in StreetEasy data. We construct "predicted prices" as follows. We first use the 2016 data to run a hedonic regression of rent prices on building year and building year squared, unit square-footage, number of bedrooms, number of bathrooms, and indicators for whether the building has an elevator, whether the building has a garage, whether the building is mixed use, and whether the unit has laundry, dishwasher, or central air conditioning. We then estimate predicted rent for all listings in our analysis sample. Finally, we estimate our calipered difference-in-differences regressions using the predicted rent, without controlling for listing or building characteristics. "Year 1" compares listings created within 365 days of the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days of the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. Our analysis sample only includes "no amenities" listings, i.e., those without central AC and in buildings without a gym, doorman or pool. *p<0.1; **p<0.05; ***p<0.01

Figure C.7: Distribution of "days listed" in $t = -1$



Note: This figure plots days listed for listings in StreetEasy the year before the rollout of RTC.

being created. We link these listings, and assign the earliest listing date and the last end date. This procedure identifies 28,529 sequential listings across all of NYC from 2013-2020. We then repeat the above procedure for listings created up to 14 days after an identical listing, and merge another 4,912 sequential listings. Our procedure still likely understates the number of days a given unit is listed, and overstates the number of listings per building. If the prevalence of nearly sequential listings varies across ZIP codes and over time, then

Table C.6: Impact of RTC on listed rents, robustness to choice of donut around boundary

	(1) Year 1 effect	(2) Year 2 effect
DiD (calipered)	12.51**	38.11***
	(5.74)	(7.74)
DiD (calipered, flex)	6.47	29.13***
	(6.60)	(8.09)
DiD (calipered, 250m donut)	17.50***	33.32***
	(6.77)	(9.31)
DiD (calipered, flex, 250m donut)	7.98	21.92**
	(7.55)	(10.49)
DiD (calipered, 500m donut)	20.00**	36.99***
	(9.45)	(12.48)
DiD (calipered, flex, 500m donut)	5.57	20.13
	(9.57)	(13.50)
DiD (calipered, two-sided 250m donut)	18.59***	37.88***
	(6.33)	(9.89)
DiD (calipered, flex, two-sided 250m donut)	10.62	29.79***
	(6.87)	(11.18)
DiD (calipered, two-sided 500m donut)	17.22**	30.70**
	(8.56)	(12.52)
DiD (calipered, flex, two-sided 500m donut)	9.00	26.78*
	(8.20)	(14.78)

Note: This table reports estimates of the impact of RTC on listed rent prices for specifications using different donut widths. A "one-sided" donut omits listings that are within a specified distance of the treatment border from control ZIPs only; a "two-sided" donut omits listings within the specified distance in both treatment and control ZIPs. "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. Our analysis sample only includes "no amenities" listings, i.e., those without central AC, a gym, doorman, or pool. We control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. *p<0.1; **p<0.05; ***p<0.01

we might estimate impacts on listing behavior that reflect different pre-trends, rather than policy impacts.

Using the dataset described above, Figure C.8 plots the effects of RTC on listing days and number of units. Panel A plots the δ_t coefficients from equations (1) and (2), where the outcome is number of days listed. Panel B plots the estimates for the number of listings per unit from the parcel-level regressions. Individually, panels A and B suggest a modest decrease in days listed and an increase in the number of listings per parcel. However, these panels exhibit pre-trends in opposite directions, suggesting the existence of pre-RTC differences in landlord listing behavior. Panel C plots our preferred measure of vacancy length: total listing-days per parcel unit. This measure aggregates overall listings in each parcel and

Table C.7: Impact of RTC rollout on listed rents, additional hedonic controls

	(1) Main	(2) More controls
Panel A. Year 1		
DiD (calipered)	12.51**	14.07**
	(5.74)	(6.20)
DiD (calipered, flex)	6.47	8.71
	(6.60)	(6.79)
Observations	179578	175120
Pre-period mean ($/month)	2361.70	2361.70
Panel B. Year 2		
DiD (calipered)	38.11***	37.58***
	(7.74)	(7.41)
DiD (calipered, flex)	29.13***	29.18***
	(8.09)	(8.68)
Observations	120229	116877
Pre-period mean ($/month)	2439.79	2439.79

Note: This table reports estimates of the impact of RTC on listed rent prices, for different sets of controls X_i. In column (1), we control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. Column (2) additionally controls for building age squared, whether the building has an elevator, whether the building is mixed use, whether the building has a garage, and average square footage per unit. "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. Our analysis sample only includes "no amenities" listings, i.e. those without central AC, a gym, doorman, or pool. *p<0.1; **p<0.05; ***p<0.01

year. This measure captures building-level search time, whether a landlord creates multiple short-term listings for a single unit or one long listing for multiple units. Here we find no pre-trends and no impacts of RTC (with small, negative, and statistically insignificant impacts on days listed). This suggests the modest upward trend in the number of listings (panel B) and the downward trend in days listed (panel A) may be coming from changes in landlord listing behavior on the platform, rather than changes in vacancy length or the number of listings. Appendix Table C.9 reports the year 1 and year 2 estimates from panel C, which combines the number of listings and how long each listing is posted. Overall, we do not find any statistically significant impact on the total days listed per unit at the parcel level.

Table C.8: Impact of RTC rollout on listed rents, stable sample

	(1)	(2)	(3)	(4)
	Stable sample		Reweighted stable sample	
	Year 1	Year 2	Year 1	Year 2
DiD (calipered)	21.09***	38.11***	32.98***	42.92***
	(7.68)	(7.74)	(8.18)	(8.89)
DiD (calipered, flex)	9.18	29.13***	14.48	29.44***
	(9.00)	(8.09)	(9.22)	(8.77)
Observations	112199	120229	109114	116877
Pre-period mean ($/month)	2446.81	2439.79	2446.81	2439.79

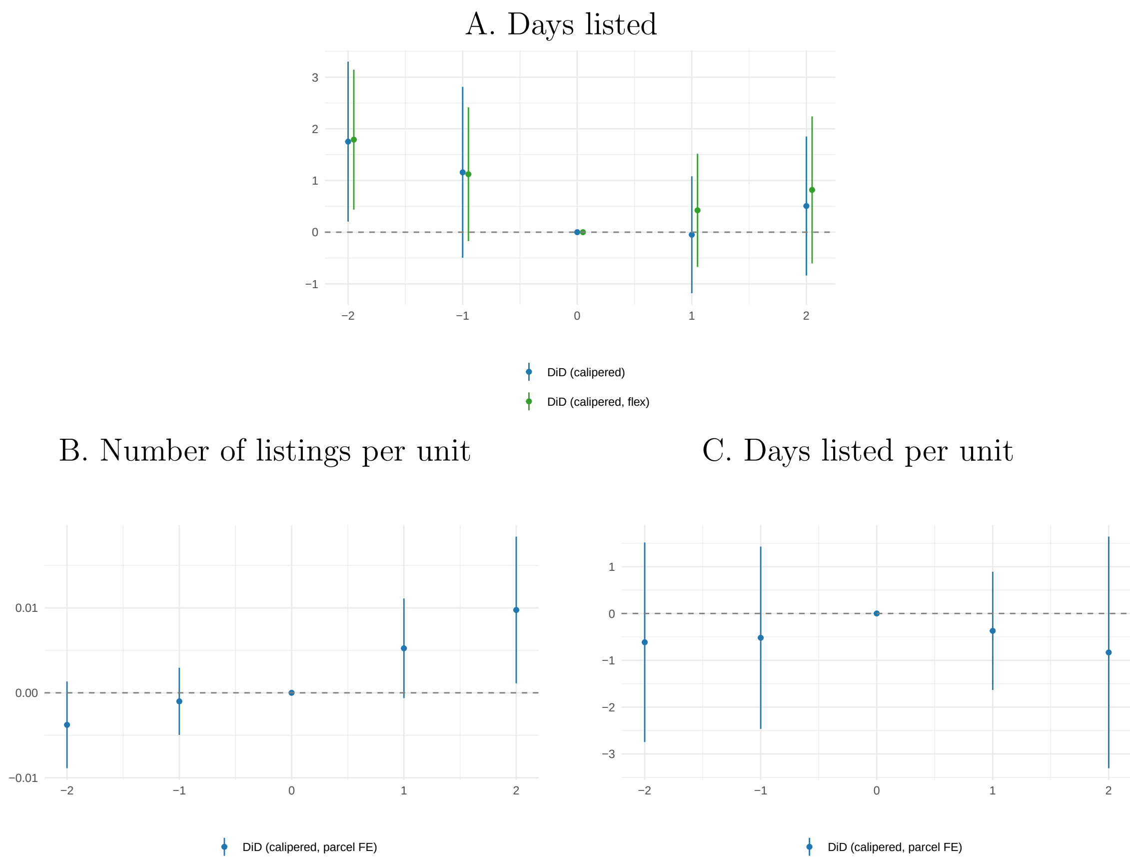
Note: This table reports estimates of the impact of RTC on listed rent prices. The analysis is restricted to border pairs where the treated side is treated in 2017, and the control side is never treated or treated after 2019 ("stable sample"). "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. In columns (3) and (4), observations are weighted by the ratio of observations in that year, relative to the number of observations in the year prior to RTC implementation. Our analysis sample only includes "no amenities" listings, i.e. those without central AC, a gym, doorman, or pool. We control for building age, the year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. *p<0.1; **p<0.05; ***p<0.01

Table C.9: Impact of RTC rollout on days listed per residential unit

	(1)	(2)
	Year 1 effect	Year 2 effect
DiD (calipered, parcel FE)	-0.37	-0.83
	(0.64)	(1.26)
Observations	88561	88561
Pre-period mean	21.31	21.31

Note: This table reports estimates of the impact of RTC on days listed per residential unit in each parcel. We restrict our analysis to parcels that include housing within 1000m of the ZIP code border. "Year 1" and "Year 2" are estimated jointly. We include parcel-by-border pair and year-relative-to-treatment fixed effects. *p<0.1; **p<0.05; ***p<0.01

Figure C.8: Event Studies of Impacts of RTC: Number of Listings and Days Listed



Note: Panel A replicates the analysis in Table 2 using days listed as the outcome. Panels B and C estimate our parcel-level DiD specifications for the number of listings per unit and total days listed per unit. Our analysis sample in Panel A only includes “no amenities” listings, i.e., those without central AC, a gym, doorman, or pool. We control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. Panels B and C implement our parcel-level specifications. We restrict our analysis to parcels that include housing within 1000m of the ZIP code border. The coefficients for years 1, 2, -1, and -2 are estimated jointly. We include parcel-by-border pair and year-relative-to-treatment fixed effects.

C.5 Clustering and effective sample sizes

A common concern when using difference-in-differences with clustered standard errors is that the total number of clusters can be small (Conley and Taber, 2011). In our analysis, we have 57 border-pair clusters in total. Here we consider how our clustering affects the effective sample size using tools from the literature on design effects (Kish, 1965; Moulton, 1986).

Given that we can estimate the variance of our coefficient of interest both with and without clustered standard errors, we can directly estimate the empirical design effect, which is calculated as the ratio:

$$DE = \frac{Var_{CR}(\hat{\delta})}{Var_{HC}(\hat{\delta})},$$

where $\hat{\delta}$ is our estimate of the parameter of interest, $Var_{CR}(\hat{\delta})$ is the variance of our coefficient of interest when using cluster-robust standard errors, and $Var_{HC}(\hat{\delta})$ is the variance when using i.i.d. heteroskedastic-robust standard errors. We can then calculate the empirical effective sample size:

$$N_{eff} = \frac{N}{DE}$$

Table C.10 reports $\hat{\delta}$, the variance of $\hat{\delta}$ with and without clustering, the sample size, the empirical design effect, and the effective sample size. We estimate the design effect to be between 1.83 and 2.26, implying that clustering in our analysis approximately halves our sample size; however, the effective sample size is still over 57,000 for each specification.

Table C.10: Calculating Effective Sample Sizes

Spec	estimate	$Var_{CR}(\hat{\delta})$	$Var_{HC}(\hat{\delta})$	N	DE	N_{eff}
Main - Year 1 effect						
DiD (calipered)	12.51	32.94	17.95	179578	1.83	97881.11
DiD (calipered, flex)	6.47	43.59	19.26	179578	2.26	79350.27
Main - Year 2 effect						
DiD (calipered)	38.11	59.85	31.83	120229	1.88	63934.07
DiD (calipered, flex)	29.13	65.51	31.51	120229	2.08	57828.82

Note: This table reports our DiD estimates on the impact of RTC on rent for our two main specifications for year 1 (top panel) and year 2 (bottom panel), and their effective sample size (Kish, 1965; Moulton, 1986). Column one reports our estimates. Columns two and three report the variance when clustered "CR" and with standard i.i.d. robust standard errors "HC." Column 4 reports the number of observations N. Column five reports the empirical design effect DE (the ratio of the variances). Column six reports the empirical effective sample size ($N_{eff} = N/DE$).

C.6 Sensitivity to Violations of Parallel Trends

This appendix describes and then implements sensitivity analysis for our differences-in-differences (DiD) estimation from Rambachan and Roth (2023) (RR).

Methodology

Following their notation, let δ_t denote the average treatment effect in relative year t from a two-way fixed effects (TWFE) regression. The standard parallel-trends assumption requires that all pre-treatment δ_t are zero, which we can test using the pre-period estimates $\hat{\delta}_{-1}$ and $\hat{\delta}_{-2}$. RR replace the exact parallel-trends assumption with a *bounded departure* restriction and characterize the identified set for post-treatment effects that is consistent with (i) the observed pre-treatment estimates and (ii) the chosen bound. We adopt RR's recommended "relative-magnitude" restriction, which limits the size of post-treatment trend deviations by a multiple of the largest adjacent-period deviation observed in the pre-period.

Notation and restriction. Let $\Delta\delta_t \equiv \delta_t - \delta_{t-1}$ denote the adjacent-period change in the relative-time path. Define the pre-period volatility benchmark

$$B_{\text{pre}} \;\equiv\; \max_{s\leq 0} |\, \Delta\delta_s \,| \,.$$

The relative-magnitude restriction requires that for each post period $t > 0$,

$$|\, \Delta\delta_t \,| \;\leq\; \bar{M} \cdot B_{\text{pre}} \;, \tag{14}$$

where $\bar{M} \geq 0$ is a sensitivity parameter (which we vary below). When $\bar{M} = 0$, this collapses to the strict parallel-trends assumption. Larger $\bar{M}$ allows proportionally larger post-treatment deviations (relative to the worst adjacent-period change already seen in the pre-period).

Identified sets and inference. The restriction (14) and the observed (noisy) pre-period estimates imply a set of post-treatment paths $\{\delta_t : t > 0\}$ that are consistent with the data and the bound. RR shows how to compute (i) pointwise identified sets for each post-treatment δ_t and (ii) valid confidence regions for these sets that account for sampling error in the event-study estimates. The bounds can be obtained by solving constrained least-squares/projection problems that impose (14) and treat the estimated relative-time coefficients (and their covariance) as inputs. We present the resulting confidence intervals for the identified sets as functions of $\bar{M}$ below.

Results from Rambachan and Roth (2023) Sensitivity Analysis

Rambachan and Roth (2023) takes as input the estimates from a pooled two-way fixed effects model that jointly estimates the period-specific difference-in-differences estimates for the years before and after treatment. This differs from our main specification, which estimates pairwise difference-in-differences. We restrict the sample to border-pairs where RTC is implemented in 2017 on one side, and 2019 or later on the other side of the border ("stable sample"). This sample restriction ensures that our TWFE estimates are not affected by common issues associated with staggered adoption (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). Table C.8 estimates the impact of RTC on rents for this subset of border pairs.[45] Table C.11 reports estimates from the two-way fixed effects regression for rental prices and log rent. These estimates are very similar to our main estimates for year 2, though the two-way fixed effects estimates for year 1 are modestly larger. For example, the "DiD (calipered)" specification in period 1 has a treatment effect of 12.51 in our main specification and 11.25 in the stable sample, while it is 21.45 in the TWFE specification. The year 2 estimates for the "DiD (calipered)" specification differ by 13 cents. In both specifications, all pre-period estimates are statistically insignificant. Note that the standard errors for both pre-periods are large. The fact that these consecutive pre-period estimates both have large standard errors has implications for our confidence intervals, which we discuss further below.

Figure C.9 presents RR confidence intervals for our rental price estimates across different values of $\bar{M}$, ranging from 0 to 1.5. The intervals are displayed at both 90% (thick lines) and 95% (thin lines) confidence levels, with $\bar{M} = 0$ shown in blue and all other values in red. The top panels display results from the calipered DiD specification for years 1 and 2, while the bottom panel shows our more flexible calipered DiD specification (see Section 3.1). For the top row, the 90% confidence intervals exclude zero for $\bar{M} \leq 0.7$ in year 1 and $\bar{M} \leq 0.5$ in year 2. For the 95% confidence intervals, these thresholds are 0.5 and 0.4, respectively. In the more flexible DiD specification, year 1 is not significant for any choice of $\bar{M}$. For year 2, the confidence intervals exclude zero for $\bar{M} \leq 0.5$ at 90% confidence and $\bar{M} \leq 0.4$ at 95% confidence. These maximum values of $\bar{M}$ for which the 95% confidence intervals exclude zero are similar to those of some recent papers (Flynn et al., 2024; Briones and Turner, 2025; Korovkin et al., 2025), larger than some (Tandel et al., 2025), and smaller than several (e.g., Colmer et al. (2024) reports breakdown values greater than 1, Deisenroth et al. (2024) reports a breakdown value of 2, Jones and Pineda-Torres (2024) reports a breakdown value of 1.25, and Cabral et al. (2025) reports breakdown values ranging from 0.6 to 2 for their statistically significant outcomes).

Taken at face value, these results suggest that our findings are not robust to pre-trend

[45] We estimate pairwise difference-in-differences to allow for more flexibility in loadings for the fixed effects and covariates.

Table C.11: The impacts of RTC rollout on listed rents using a two-way fixed effects specification

	DiD (calipered, TWFE)	DiD (calipered, flex, TWFE)
Price		
Period -2	-2.159	-6.055
	(17.355)	(14.907)
Period -1	5.655	2.288
	(18.164)	(14.244)
Period 1	21.450***	10.107
	(7.624)	(9.090)
Period 2	37.978***	27.720***
	(7.659)	(8.352)
N	266,128	266,128
Log Rent		
Period -2	-0.003	-0.004
	(0.008)	(0.007)
Period -1	0.002	0.000
	(0.006)	(0.005)
Period 1	0.009***	0.005
	(0.002)	(0.003)
Period 2	0.015***	0.011***
	(0.003)	(0.004)
N	266,128	266,128

Note: This table reports difference-in-difference estimates for periods -2 to 2 (with period 0 as the baseline pre-period), estimated using a two-way fixed effects model. The sample and controls are identical to those used in our main analysis.

violations. However, we believe it is illuminating to consider what drives these results. Our deviations from parallel trends in the pre-period are small compared to the size of our year 1 and year 2 estimates. For example, in our calipered DiD estimates, the deviations are -2.1 in year -2 and 5.6 in year -1, while the year 1 estimate is 21.5 and the year 2 estimate is 38.0. Consequently, much of what drives the width of the RR confidence intervals is the imprecision in the pre-period estimates. This is particularly true because B_{pre} above is the difference between the year -1 and year -2 estimates, both of which have large standard errors.

To illustrate this point, we repeat the RR exercise after dropping year -2, which has a small point estimate but the largest standard errors of any year, from our analysis. Doing so results in B_{pre} being calculated from year -1 and the base year. One limitation of this approach is that the method only allows us to construct confidence sets and confidence intervals for year 1, not year 2. Figure C.10 replicates Figure C.9 after dropping year -2 from the two-way fixed effects regression. With this approach, we find tighter confidence intervals. For year 1, the 90% confidence intervals exclude zero for $\bar{M} \leq 1$ for the calipered DiD and $\bar{M} \leq 1.1$ for the more flexible specification. The 95% confidence intervals exclude zero for 0.8 and 0.9, respectively.[46]

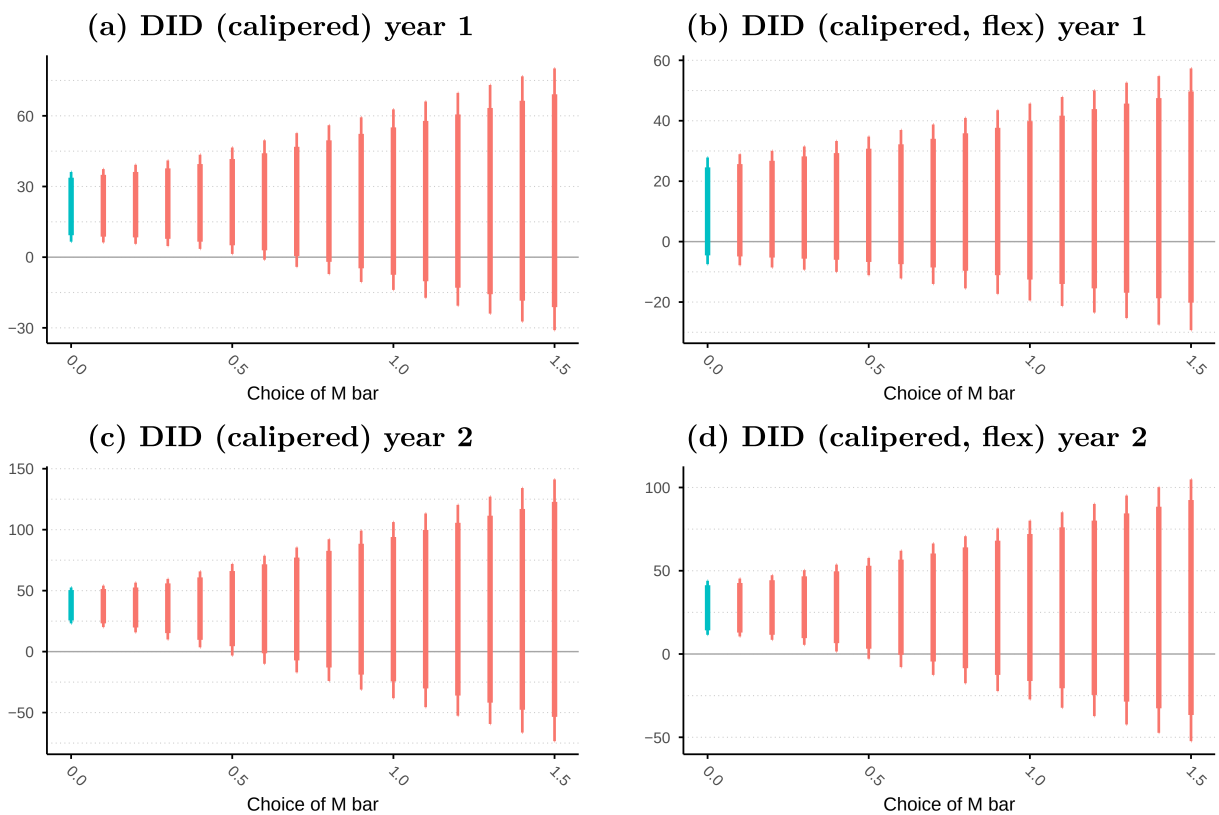
Overall, this robustness exercise suggests that we should be cautious in interpreting our DiD estimates, often due to imprecision in the pre-period estimates. While our confidence intervals include zero for choices of $\bar{M} < 1$ in the full specification, this is driven in part by the imprecision rather than by large deviations from parallel trends in the pre-period. More generally, it suggests that we should not interpret the typically small pre-period deviations from parallel trends as strong evidence that trends are parallel in the pre-period, given the large standard errors.

C.7 Robustness to the handling of pilot programs

As discussed in Appendix A.1, out of the 15 ZIP codes treated in 2017, 10 were already part of the Expanded Legal Services program through which the City provided legal representation in eviction cases for individuals with household incomes at or below 200 percent of the federal poverty line. This ELS program was implemented in 2016-17, though with lower rates of funding, which restricted the number of households that could be served by the pilot. Funding in 2017 was approximately three times higher than during the pilot (Office of Civil Justice, 2016, 2017, 2018). We do not include the pilot program as part of "treatment" in our analysis,

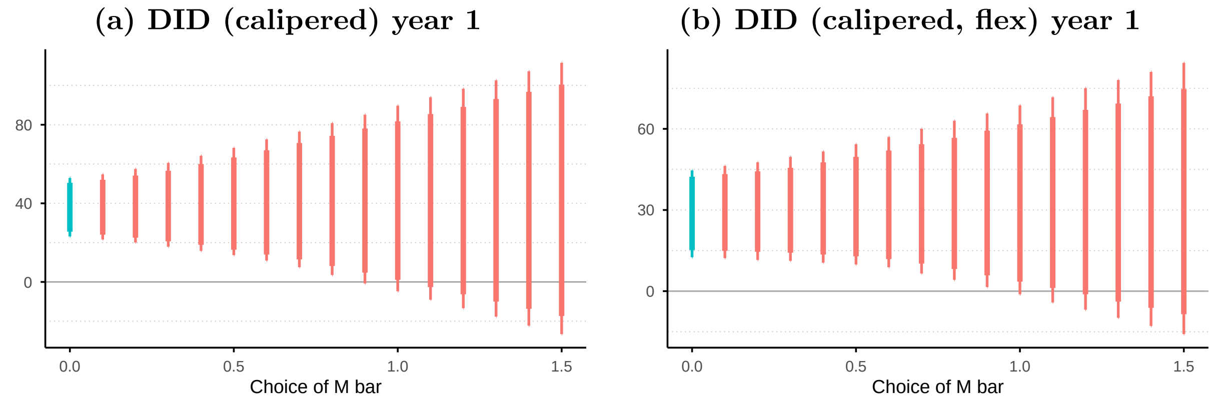
[46]While we focus on bounded departure restrictions, results for smoothness class restrictions also suggest that the results are somewhat robust. When allowing for linear deviations from parallel trends, the 95% confidence intervals exclude zero. This remains true when allowing for more flexible deviations (with $M \leq 38.9$ for the DiD and $M \leq 5.6$ for the more flexible specification).

Figure C.9: Sensitivity analysis to parallel-trends violations



Note: This figure displays the results of applying the bounding methods of Rambachan and Roth (2023) to equations 1 and 2, estimated using TWFE and StreetEasy data. The sample is restricted to StreetEasy listings in border-pairs where one side of the border is treated in 2017, and the other side is treated in 2019 or later. The StreetEasy sample only includes "no amenities" listings, i.e. those without central AC and in buildings without a gym, doorman or pool. "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. We control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects.

Figure C.10: Sensitivity analysis to parallel-trends violations (dropping year -2)



Note: This figure replicates Figure C.9 after dropping year −2 from the two-way fixed effects regression. The sample is restricted to StreetEasy listings in border-pairs where one side of the border is treated in 2017, and the other side is treated in 2019 or later. The StreetEasy sample only includes "no amenities" listings, i.e. those without central AC and in buildings without a gym, doorman or pool. "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. We control for building age, year when the listing was created, an indicator for whether building age was imputed, an indicator for whether the listing was missing data on amenities, bedroom count dummies, bathroom count dummies, and border-pair fixed effects.

though we provide robustness to this choice below.

Table C.12 estimates the impact of the RTC rollout in 2017 for the "stable sample" (column 1), and then separately for border-pairs where the treated side was not in the ELS pilot (column 2) and where the treated side was in the pilot (column 3).[47] In year 1, the overall estimates are similar to the estimates based only on the pilot. The estimates that exclude the pilot are smaller and much noisier, which may be expected given we are using a much smaller sample (and fewer border pairs). While the point estimates for year one differ between columns 2 and 3 of Table C.12, we cannot reject that they are equal (the p-values for the test of equality are: 0.37 and 0.30 for the rows of Panel A). In year two, the point estimates are large and positive in all three specifications. The estimates that exclude the pilot are still somewhat smaller (ranging from \$9 to \$24) and less precisely estimated, but again we cannot reject equivalence between the pilot and non-pilot ZIPs (smallest p-value: 0.39).

We additionally check the robustness of our choice of rollout date in Table C.13. Column 2 in Table C.13 assumes that RTC rolled out in pilot ZIP codes when the pilot was announced (i.e., February 2016). To estimate these results, we re-construct the analysis dataset and include different ZIP code borders relative to our main analysis sample, following the approach described in Appendix A. There are more border pairs in column 2 than in column 1, since many of the ZIP codes included in the rollout in 2017 neighbor pilot ZIP codes (Figure B.2)

[47]We focus on the stable sample in year 1 so that we are working with a consistent set of borders across year 1 and year 2 for this comparison.

and the approach to constructing the sample, described in Figure B.3, excludes those border pairs from our analysis sample. Given column 2 adds additional borders, as a robustness check, Column 3 excludes border pairs where one side of the border is included in the pilot and the other side rolls out RTC in 2017. Both Columns 2 and 3 show that the estimates are much smaller (and somewhat noisier) when we assume the pilot ZIPs were treated in 2016, consistent with little treatment occurring in 2016. We have replicated this exercise assuming the pilot rolled out in July 2016, and results are similar.

As an alternative robustness check, Column 4 of Table C.13 estimates the impact of RTC assuming that RTC rolled out on the empirical rollout date calculated by Cassidy and Currie (2023). This approach empirically determines when treatment was implemented based on when legal representation increases. To use empirical rollout dates, we again reconstruct the analysis dataset, which then includes different ZIP code borders relative to our main analysis sample, since treatment years and status change. Comparing the estimates in column 4 to our main estimates (column 1), the estimates in column 4 are similar or slightly larger, though with larger standard errors (potentially due to the smaller sample). In year 2, the estimates in column 4 are larger than our main estimates, with similar levels of statistical significance but, again, larger standard errors.

Finally, we also consider a second small pilot initiative, the Anti-Harassment and Tenant Protection pilot. This pilot was introduced at the same time as ELS, but in a different set of ZIP codes. The pilot "provided resources for tenant outreach and pre-litigation services with the goal of preventing eviction and displacement. In addition to full representation and brief legal assistance for Housing Court and administrative proceedings, AHTP legal services providers offer[ed] community education, landlord/tenant mediation, and counsel on cooperative tenant actions and building-wide lawsuits." Table C.14 compares our main estimates to estimates that exclude the AHTP ZIP codes. Excluding these ZIP codes does not notably change our estimates.

C.8 Impacts on tenant composition

In addition to affecting rental prices, RTC could also change tenant composition. On the supply side, landlords could become less willing to approve rental applications from tenants with higher perceived default risk, such as tenants with low income or low credit scores. Some prior work, largely on consumer credit markets, suggests that supply contractions for higher-default-risk consumers can be a primary response to similar regulation (Han et al., 2018; Cuesta and Sepúlveda, 2021; Cherry, 2025; Nelson, 2025; Matcham, 2025; Chakrabarti et al., 2025). On the demand side, renters' (voluntary) sorting into specific neighborhoods could also adjust, in principle with either higher- or lower-default-risk tenants preferring

Table C.12: **Impact on rent, pilot vs. non-pilot ZIP codes treated in 2017**

	(1) Stable sample	(2) Exclude pilot	(3) Pilot only
Panel A. Year 1			
DiD (calipered)	21.09***	8.97	28.28***
	(7.68)	(20.04)	(7.97)
DiD (calipered, flex)	9.18	-6.16	16.91**
	(9.00)	(20.68)	(8.45)
Observations	112199	34754	77445
Pre-period mean ($/month)	2446.81	2419.03	2457.56
Panel B. Year 2			
DiD (calipered)	38.11***	23.62*	36.62***
	(7.74)	(13.76)	(9.81)
DiD (calipered, flex)	29.13***	14.23	30.36***
	(8.09)	(20.83)	(10.50)
Observations	120229	35518	84711
Pre-period mean ($/month)	2439.79	2419.03	2447.74

Note: This table shows estimates of the impact of RTC on rents in the stable sample. The top panel shows results for year 1 and the bottom panel shows results for year 2. Column (1) shows estimates on the whole stable sample; column (2) shows estimates that exclude border pairs where the treated side was in the ELS pilot; column (3) restricts to border pairs where the treated ZIP code was in the ELS pilot.

Table C.13: **Impact on rent, robustness to choice of treatment date**

	(1) Original	(2) Pilot treated Feb 2016	(3) + no extra border	(4) Empirical UA rollout date (CC)
Panel A. Year 1				
DiD (calipered)	12.51**	-2.71	2.93	14.98
	(5.74)	(8.61)	(8.74)	(10.50)
DiD (calipered, flex)	6.47	1.04	3.60	9.65
	(6.60)	(7.98)	(8.64)	(11.58)
Observations	179578	177951	162811	97259
Pre-period mean ($/month)	2361.70	2341.59	2332.20	2344.22
Panel B. Year 2				
DiD (calipered)	38.11***	10.68	10.68	49.67***
	(7.74)	(13.21)	(13.21)	(15.37)
DiD (calipered, flex)	29.13***	12.34	12.34	35.50**
	(8.09)	(12.92)	(12.92)	(14.94)
Observations	120229	108368	108368	61056
Pre-period mean ($/month)	2439.79	2415.21	2415.21	2360.84

Note: Column 2 assumes all pilot ZIPs were treated in February 2016. Column 3 excludes border pairs where both sides are treated in the same year, even if the empirical rollout date is different. This additional restriction does not affect the "Year 2" sample because our sample restrictions already drop border pairs in which a non-pilot ZIP code was treated in 2017, so the Year 2 samples and estimates are identical in columns (2) and (3). Column (4) conducts a different robustness check, assuming that RTC rolled out on the emirical rollout dates calculated by Cassidy and Currie (2023).

RTC-treated ZIP codes.[48]

Despite these potentially compounding or offsetting supply and demand effects, we argue our analysis of tenant composition is still valuable. In a sense we make more precise below, "large" increases in tenant income or credit score on new leases would likely reflect stricter landlord screening, rather than a demand response to RTC. If we found such effects, this screening behavior would have implications for our welfare framework and for our understanding of which tenants bear the costs of RTC.

With this goal in mind, we analyze changes in tenant composition using our merged Infutor-Experian data, which show address histories and a panel of credit report data for likely-renter in-migrants to NYC from 2014 to 2019. These Infutor-Experian data are further described in subsection 2.4.5 and Appendix A.6.

Because the Infutor data may over-represent some demographic groups (Agostini et al., 2025), we reweight the Infutor–Experian sample so that its demographic composition matches that of renters in the 2015–2019 five-year American Community Survey who moved within the past year to a comparable set of NYC neighborhoods (PUMAs that are at least 50%

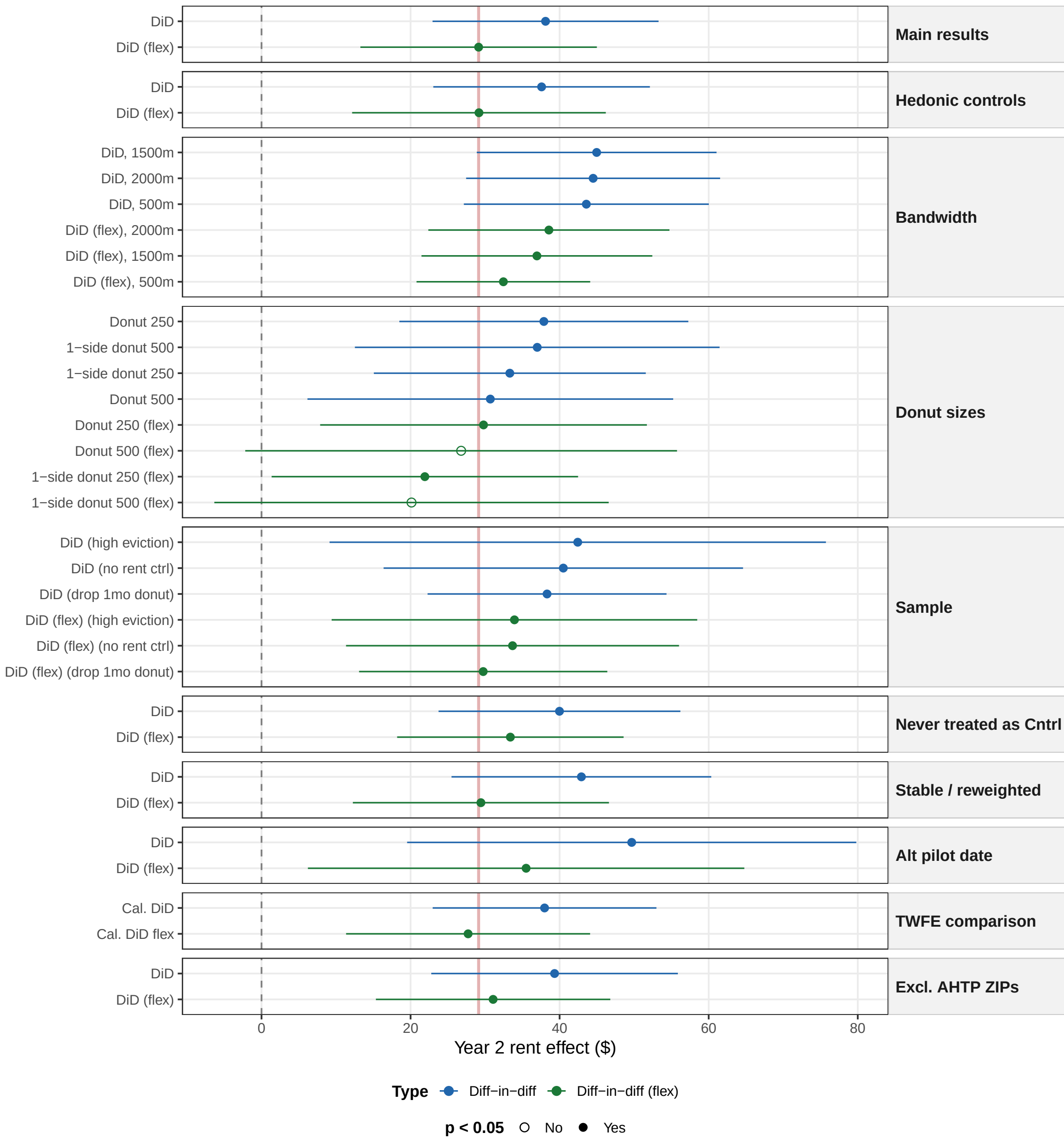
[48]If poorer renters are more price-elastic, RTC-induced rent increases could shift the composition of tenants in RTC-treated ZIP codes toward wealthier or higher-income renters, who may have correspondingly lower default risk. On the other hand, poorer renters and those with greater default risk may particularly value RTC's increased protections and therefore prefer to live in RTC-treated ZIP codes.

Table C.14: Impact on rent, exclude ZIP borders where one side had the Anti-Harassment and Tenant Protection program

	(1) All ZIPs	(2) Exclude AHTP ZIPs
Panel A. Year 1		
DiD (calipered)	12.51**	13.20**
	(5.74)	(6.12)
DiD (calipered, flex)	6.47	5.69
	(6.60)	(7.35)
Observations	179578	151519
Pre-period mean ($/month)	2361.70	2340.23
Panel B. Year 2		
DiD (calipered)	38.11***	39.32***
	(7.74)	(8.44)
DiD (calipered, flex)	29.13***	31.07***
	(8.09)	(8.02)
Observations	120229	91846
Pre-period mean ($/month)	2439.79	2425.51

Note: In 2016, another program was piloted to prevent eviction cases. Column 2 excludes the border pairs where either side was a part of the Anti-Harassment and Tenant Protection pilot.

Figure C.11: Rent effect estimates across specifications



Note: This figure compares point estimates and confidence intervals for RTC's Year 2 rent effect obtained across the robustness specifications reported in this section. Each row corresponds to one specification; for each, the dot denotes the point estimate and the line a symmetric 95% confidence interval. Dots are hollow if the confidence interval contains zero, otherwise solid. The vertical red line marks the point estimate from the baseline DiD (flex) specification reported in column 2 of Table 2. Rows are grouped by the type of robustness specification. "Hedonic controls" correspond to estimates from panel B of Table C.7; "Bandwidth" to panel B of Table C.2; "Donut sizes" to column 2 of Table C.6; "Sample" to panel B of Table C.3; "Never treated as Cntrl" to column 3, panel B of Table C.4; "Stable/reweighted" to column 4 of Table C.8; "Alt pilot date" to columns 2 and 4, panel B of Table C.13; "TWFE comparison" to the first panel of Table C.11; and "Excl. AHTP ZIPs" to column 2, panel B of Table C.14.

covered by ZIP codes in our analysis sample). We define weights based on race/ethnicity and gender, dividing ACS-implied migration inflows by Infutor-implied migration inflows during our analysis period within each cell. We impute race and ethnicity in Infutor using Bayesian-Improved Surname Geocoding at the tract level (Elliott et al., 2009; Consumer

Financial Protection Bureau, 2014; Diamond et al., 2019). The resulting weights range from 1.721 for white males to 0.501 for Black females.

We then test for changes to tenant composition using a calipered difference-in-differences specification analogous to equation (1), with various in-migrant characteristics as the dependent variable.[49] We report results in Appendix Table C.15. In column (1), we find that in-migrants' credit scores in RTC-treated areas increase by about 3 points on average in the first year after treatment and by about 5 points in the second year, a modest effect relative to a baseline standard deviation of 101-102 points. In columns (2)-(4), we generally do not find significant effects of RTC on three credit record variables that, based on conversations with the tenant screening industry (Humphries et al., 2024), we understand are frequently used in tenant screening: total monthly debt payments, public records such as prior court judgments for unpaid debts, and revolving debt balance (principally credit card balances). In column (5), we find no significant effect on our closest available proxy for in-migrant income, the ACS median household income of an in-migrant's prior Census tract in Infutor, though movers are known to be not representative of their source geographies (Gabriel and Schmitz, 1995; Dahl, 2002; Wozniak, 2010).[50]

These null or economically small results are consistent with limited responses to RTC in both landlord screening and tenant sorting. To the extent that tenant credit scores did increase in column (1), it is difficult to say conclusively whether this is the result of screening or sorting (or both). However, prior work suggests that these responses are small enough that sorting rather than screening may play the most important role. In particular, if we assume tenants in NYC spend 31.7% of their income on rent (New York City, 2025) and assume income is correlated with credit score to the extent documented in Albanesi et al. (2017), then our estimates of RTC's rent effects from section 3.2 imply credit score changes of about the size we see in column (1), even without any screening response.

Because we do not detect clear evidence of a substantial effect on tenant screening, we do not include screening as an additional cost to tenants in our tenant welfare framework.[51]

[49]Besides the Infutor data cleaning steps described in Appendix A.6, we also impose the sample restrictions discussed in Section 3.1 for comparability with the rent analysis.

[50]Column (6) of the table presents a separate analysis of whether in-migrants have a mortgage on their credit report after moving to NYC – a proxy for being an owner-occupier rather than renter. In this column only, the outcome is measured in the first credit report observed after the relevant move (as opposed to the most recent credit report observed prior to the move). We do not find a statistically significant effect of RTC on this margin. A positive sign, as we find (insignificantly), is consistent with the evidence on increased condo conversions discussed in section 3.3. All other columns of Table C.15 are conditioned on non-mortgage-holding, in order to focus on a likely-renter population, as discussed in Appendix A.6.

[51]One potential explanation for the lack of a screening response is that it is difficult for landlords to identify ex-ante risky tenants, at least among the set of tenants whom they already typically approve. Prior evidence suggests that credit report data and credit scores have limited predictive power for loan default among the most economically disadvantaged consumers (Blattner and Nelson, 2024), and that tenant non-payment is poorly predicted from the application information of approved applicants (Humphries et al., 2024). The fact that screening does not appear to intensify substantially with the introduction of RTC is consistent with

Nevertheless, the results in Table C.15 are also consistent with more substantial changes to both screening and sorting that happen to largely offset each other. If the screening responses to RTC were instead nontrivial, this would suggest an additional cost to RTC borne by (some) tenants that is not accounted for in our welfare framework.[52]

Table C.15: Migration Evidence from Infutor & Experian

	(1) Credit Score	(2) Total Monthly Payment	(3) 1000 · Number of Public Records	(4) Revolving Balance	(5) Median Income previous tract	(6) Has Mortgage
Panel A. Year 1						
DiD (calipered)	3.37**	33.39	1.17	-310.19*	-1029.20	0.00
	(1.56)	(31.61)	(3.97)	(178.10)	(646.60)	(0.01)
DiD (calipered, flex)	5.21***	35.32	0.83	-248.46	-779.71	0.00
	(1.39)	(33.29)	(3.75)	(182.32)	(524.81)	(0.01)
Observations	77734	77734	77734	77734	77734	88101
Pre-period mean	673.76	331.57	35.76	4714.76	71838.54	0.15
Pre-period s.d.	101.01	569.64	209.06	10903.7	36776.86	0.36
Panel B. Year 2						
DiD (calipered)	5.05***	55.75	-0.89	-82.85	-312.79	0.01
	(1.79)	(45.81)	(4.44)	(252.16)	(551.49)	(0.01)
DiD (calipered, flex)	5.34***	65.06	-0.55	-17.96	-383.69	0.00
	(1.87)	(51.28)	(4.73)	(242.67)	(639.32)	(0.01)
Observations	50827	50827	50827	50827	50827	55515
Pre-period mean	673.89	328.86	31.08	4643.37	73425.26	0.15
Pre-period s.d.	102.2	634.89	178.88	12321.05	38987.19	0.36

Note: This table estimates the impact of RTC on tenant characteristics (δ_t in equations 1 and 2). We use Infutor data on all in-migrants to treated or control ZIP-border areas in New York City between November 2014 and December 2019, linked to Experian credit report data. The Experian data are recorded twice per year in February and August. When merging with the Infutor data, we use the most recent previously recorded credit report for a given move. The median income of the previous tract is obtained from the 2015-2019 five-year ACS. The vector of controls X_i from equation 1 is left empty for these specifications. All columns except column (6) are restricted to in-migrants with no mortgage on their credit report when moving to NYC. Regressions are weighted to match the demographics of renters in the ACS who recently moved to in-sample geographies, as described in Appendix C.8. All specifications use a bandwidth of 1000 meters. Standard errors are clustered at the border-pair level. We drop border pairs where the minimum distance to the border on either side is greater than 300m. We also drop border pairs if there are fewer than 50 in-migrant observations on either side or if there are no observations in the pre or post-periods. *p<0.1; **p<0.05; ***p<0.01

landlords already using the information typically available to them to the best of their ability.

[52] As an alternative approach to measuring screening responses, we could consider how long units are listed on the StreetEasy platform. Longer listing durations could reflect stricter tenant screening practices, which increase the time it takes landlords to fill a vacant unit. However, as we discuss in Appendix C.4, this approach has several challenges.

C.9 Impacts on upkeep and habitability

RTC could also change landlords' willingness to maintain units and buildings. On one hand, if renting becomes less profitable overall, RTC could cause landlords to reduce investment and defer maintenance (Seltzer, 2024). On the other hand, certain habitability issues are valid reasons for a tenant to withhold rent; if legal representation helps tenants to mount a valid defense in eviction court, landlords may be incentivized to perform *more* maintenance to ensure they can evict a nonpaying tenant. A response in either direction could impact the livability or quality of the housing; this in turn would directly affect the welfare of tenants, which, if not accounted for, would bias welfare estimates.

Table C.16 tests this hypothesis using the parcel-level DiD specification from equation (3) and public data on housing inspections and substantiated housing-code violations from NYC's Department of Housing Preservation and Development (or HPD, further described in Appendix A). HPD schedules inspections in response to tenant complaints, and issues violation notices when the inspection identifies a housing-code violation. The first column reports results for the number of inspections, and the second column reports results for the number of violation notices issued to each parcel. Columns (3) and (4) separate rent-impairing and non-rent-impairing violations. Overall, we find some evidence that habitability violations increase, with a statistically significant increase in rent-impairing violations in the first year (estimates for other outcomes are positive but statistically insignificant), and larger and statistically significant increases for all outcomes in the second year.

Taken at face value, these results suggest that treatment reduced habitability. However, an alternative mechanism could be that RTC made tenants more likely to make habitability complaints, which would in turn lead to more inspections and violation notices. Landlords must resolve the cause of the complaint in order to close out the violation notice; if greater reporting drives the increase in violation notices, then the increase in violations could reflect constant or improved unit quality rather than a decline. To investigate this possibility, we examine inspections and violations relative to pre-period means, which are reported in brackets in Table C.16. If RTC made tenants more willing to report, then for a given set of underlying habitability issues we would expect to see increases concentrated among less severe ones, which may be less likely to be substantiated upon inspection. In year 2, the relative increase in inspections is smaller than that in violation notices, inconsistent with increased reporting of less severe issues. Moreover, relative increases in rent-impairing and non-rent-impairing violations are nearly identical, similarly providing no indication that less severe (non-rent-impairing) issues are driving the increase.

Overall, although it is difficult to distinguish changes in reporting from changes in underlying habitability, we believe the evidence weighs against *improvements* in habitability due to RTC. Since we do not account for this channel in our welfare analysis, our estimated welfare impacts would understate RTC's costs to tenants to the extent that habitability in fact declined.

Table C.16: Housing Code Inspections and Violations

	(1) Inspections	(2) Violation notices	(3) RI violation notices	(4) Non-RI violation notices
Panel A. Year 1				
1000 · DiD (calipered, parcel FE)	45.06	66.73	8.26*	58.47
	(58.21)	(57.88)	(4.67)	(53.88)
	[0.022]	[0.035]	[0.059]	[0.033]
Observations	1465406	1465406	1465406	1465406
1000 · Pre-period mean	2070.81	1887.69	139.57	1748.12
Panel B. Year 2				
1000 · DiD (calipered, parcel FE)	159.29**	167.60**	12.28**	155.33**
	(78.04)	(78.06)	(6.21)	(73.16)
	[0.077]	[0.089]	[0.088]	[0.089]
Observations	1465406	1465406	1465406	1465406
1000 · Pre-period mean	2070.81	1887.69	139.57	1748.12

Note: This table estimates the impact of RTC on habitability inspections and violations (δ_t in equation 3). All outcome variables were multiplied by 1000 for readability. We restrict our analysis to parcels that include housing within 1000m of the ZIP code border. The dependent variable in column (1) is the number of inspections for violations of New York City Housing Maintenance Code (HMC) or the New York State Multiple Dwelling Law (MDL) recorded by the Department of Housing Preservation and Development (HPD). Column (2) replicates column (1) but for violations rather than inspections. Columns (3) and (4) split violations into rent-impairing (RI) and non-rent-impairing violations. We use the New York State Multiple Dwelling Law Section 302 definition of a rent-impairing violation, which is "a condition within a multiple dwelling which constitutes, or if not promptly corrected will constitute, a fire hazard or a serious threat to the life, health or safety of occupants thereof." Section 2.4 and Appendix Section A describe the data in greater detail. "Year 1" and "Year 2" are estimated jointly. Since we include all parcels in these regressions, we can include parcel-by-border pair and year-relative-to-treatment fixed effects. Standard errors are clustered at the border-pair level and are reported in parentheses. The relative impacts (the treatment effect divided by the pre-period mean) are reported in brackets. We drop border pairs where the minimum distance to the border on either side is greater than 300m. We also drop border pairs if there are fewer than 50 observations on either side or if there are no observations in the pre or post-periods. *p<0.1; **p<0.05; ***p<0.01

D Additional details on welfare analysis

D.1 Model derivations

Proof of Proposition 1. For notational convenience, we suppress mutual dependence of the endogenous variables R, h, and f, and their dependence on the policy parameter τ and realized income y. The solution to the housing choice problem in equation (6) satisfies the first-order condition

$$
\begin{aligned}
[h] \quad 0 \quad = \quad & (\tau - p_j h_j) v'(h) F(y_0(h,\tau)) \\
& + \int_{y_0}^{\hat{y}} \left((\tau + f - p_j h_j) v'(h) + \frac{df}{dh}[v(h) - v(0)] - \left[R\frac{df}{dh} + \frac{dR}{dh} f \right] \bar{u}'(c) \right) dF(y) \\
& + \int_{\hat{y}}^{\bar{y}} \left(v'(h) - \frac{dR}{dh} \bar{u}'(c) \right) dF(y) \,.
\end{aligned}
$$

Since tenants default optimally, we can ignore the contribution of the changes in the limits of integration $y_0, \hat{y}$ due to changes in h. Further, using the optimal default condition $v(h) - v(0) = R\bar{u}'(c)$ in the partial default region, the housing FOC simplifies to

$$
\begin{aligned}
0 \quad = \quad & (\tau - p_j h_j) v'(h) F(y_0) \\
& + \int_{y_0}^{\hat{y}} \left[(\tau + f - p_j h_j) v'(h) - f \frac{dR}{dh} \bar{u}'(c) \right] dF(y) + \int_{\hat{y}}^{\bar{y}} \left(v'(h) - \frac{dR}{dh} \bar{u}'(c) \right) dF(y) \,.
\end{aligned} \tag{15}
$$

For the welfare impact of increasing protections, differentiating with respect to τ we obtain

$$
\begin{aligned}
\frac{dW}{d\tau} = & \int_0^{y_0} \left[(\tau - p_j h_j) \frac{dh}{d\tau} v'(h) + \left(1 - \frac{dp_j}{d\tau} h_j \right) [v(h) - v(0)] - \frac{dp_j}{d\tau} [u(y) - u(y - y_j)] \right] dF(y) \\
& + \int_{y_0}^{\hat{y}} [- \left(R \left[\frac{df}{d\tau} + \frac{df}{dh} \frac{dh}{d\tau} \right] + f \left[\frac{dR}{d\tau} + \frac{dR}{dh} \frac{dh}{d\tau} \right] \right) \bar{u}'(c) + \frac{dp_j}{d\tau} [u(y - fR - y_j) - u(y - fR)] \\
& \qquad + \left(1 + \frac{df}{d\tau} + \frac{df}{dh} \frac{dh}{d\tau} - \frac{dp_j}{d\tau} h_j \right) [v(h) - v(0)] + (f + \tau - p_j h_j) \frac{dh}{d\tau} v'(h)] \; dF(y) \\
& + \int_{\hat{y}}^{\bar{y}} \left[\frac{dh}{d\tau} v'(h) - \left(\frac{dR}{dh} \frac{dh}{d\tau} + \frac{dR}{d\tau} \right) \bar{u}'(c) \right] dF(y)
\end{aligned}
$$

$$
\begin{aligned}
= & \frac{dh}{d\tau} \underbrace{\left((\tau - p_j h_j) v'(h) F(y_0) + \int_{y_0}^{\hat{y}} \left[(\tau + f - p_j h_j) v'(h) - f \frac{dR}{dh} \bar{u}'(c) \right] dF(y) + \int_{\hat{y}}^{\bar{y}} \left[v'(h) - \frac{dR}{dh} \bar{u}'(c) \right] dF(y) \right)}_{= 0 \text{ by optimal housing choice (Eq. 15)}} \\
& + \int_{y_0}^{\hat{y}} \underbrace{\left[\frac{df}{d\tau} + \frac{df}{dh} \frac{dh}{d\tau} \right] (v(h) - v(0) - R\bar{u}'(c))}_{= 0 \text{ by optimal default (Eq. 5)}} \\
& \qquad + \left[\left(1 - \frac{dp_j}{d\tau} h_j \right) [v(h) - v(0)] + \frac{dp_j}{d\tau} [u(y - fR - y_j) - u(y - fR)] - \frac{dR}{d\tau} f \bar{u}'(c) \right] dF(y) \\
& + \left(1 - \frac{dp_j}{d\tau} h_j \right) [v(h) - v(0)] F(y_0) - \frac{dp_j}{d\tau} \int_0^{y_0} [u(y) - u(y - y_j)] dF(y) - \frac{dR}{d\tau} \int_{\hat{y}}^{\bar{y}} \bar{u}'(c) dF(y)
\end{aligned}
$$

$$
\begin{aligned}
\implies \frac{dW(\tau)}{d\tau} \quad = \quad & F(\hat{y}) \left[\left(1 - \frac{dp_j}{d\tau} h_j \right) [v(h) - v(0)] - \frac{dp_j}{d\tau} \mathbb{E}_{y < \hat{y}} [u(y - fR) - u(y - fR - y_j)] \right] \\
& - \frac{dR}{d\tau} \mathbb{E}_y [f \bar{u}'(c)] \ .
\end{aligned} \tag{16}
$$

Note that while the eviction cost C is absent from this calculation, it is implicitly accounted for because of optimal default by the marginal defaulting tenant. This completes the proof.

Equivalent Variation. Equation (9) provides an expression for $\frac{\frac{dW(\tau)}{d\tau}}{v(h)-v(0)}$. Our implementation requires aggregating across renters with different incomes, and we would also like to convert the welfare impact of RTC to a dollar value. We therefore derive an expression for tenant's equivalent variation, which is given in equation (10).

We first use the relationship $\mathbb{E}_{y\geq\hat{y}}\left[\frac{\bar{u}'(c)}{v(h)-v(0)}\right] = \frac{\mathbb{E}[\bar{u}'(c)|y\geq\hat{y}]}{\mathbb{E}[\bar{u}'(c)|y<\hat{y}]}\frac{1}{R}$ to rewrite equation (9) as

$$\frac{\frac{dW(\tau)}{d\tau}}{v(h)-v(0)} = F(\hat{y})\left[1-\frac{dp_j}{d\tau}\left(h_j+\frac{y_j}{R}\bar{\theta}\right)\right] - \frac{dR}{d\tau}\frac{1}{R}\left[\bar{f}_E F(\hat{y}) + (1-F(\hat{y}))\frac{\mathbb{E}[\bar{u}'(c)\mid y\geq\hat{y}]}{\mathbb{E}[\bar{u}'(c)\mid y<\hat{y}]}\right],$$

where $\bar{\theta}\equiv\mathbb{E}_{y<\hat{y}}[\theta]$ is the expected value of $\theta\equiv\frac{u(y-fR)-u(y-fR-y_j)}{y_j\bar{u}'(c)}$ if evicted. We then define the marginal equivalent variation from an increase in tenant protections as

$$\frac{\frac{dW(\tau)}{d\tau}}{\mathbb{E}[\bar{u}'(c)]} = \frac{\frac{dW(\tau)}{d\tau}}{v(h)-v(0)}\frac{v(h)-v(0)}{\mathbb{E}[\bar{u}'(c)]}.$$

The left-hand side of this expression is the value of tenant protections relative to a marginal increase in numeraire consumption in all states of the world.[53] From optimal default, we know

$$\frac{\mathbb{E}[\bar{u}'(c)]}{v(h)-v(0)} = \frac{1}{R}\left[F(\hat{y}) + (1-F(\hat{y}))\frac{\mathbb{E}[\bar{u}'(c)\mid y\geq\hat{y}]}{\mathbb{E}[\bar{u}'(c)\mid y<\hat{y}]}\right],$$

so that

$$\frac{\frac{dW(\tau)}{d\tau}}{\mathbb{E}[\bar{u}'(c)]} = \frac{\frac{dW(\tau)}{d\tau}}{v(h)-v(0)}\cdot\frac{R}{F(\hat{y}) + (1-F(\hat{y}))\frac{\mathbb{E}[\bar{u}'(c)\mid y\geq\hat{y}]}{\mathbb{E}[\bar{u}'(c)\mid y<\hat{y}]}}.$$

This last relationship implies equation (10).

D.1.1 What if Tenants are not Aware of RTC?

Our welfare analysis assumes that tenants are perfectly informed about RTC and its benefits and optimally respond to the RTC program in terms of their housing and default choices. This may be a strong assumption; anecdotally, tenants in some cases learned about RTC when they arrived at the courthouse to contest their eviction case. We therefore analyze an alternative version of the theoretical model in which tenants behave rationally, but are unaware of the RTC program. Specifically, we assume that tenants understand that the rental price schedule $R(\cdot;\tau)$ has changed, but they don't understand why. They act as though

[53] Due to optimal housing choice and default, $\mathbb{E}[\bar{u}'(c)]$ also equals the expected value of a marginal increase in income across all states of the world.

tenant protections remain at the pre-RTC expected case duration τ_0 and the probability of an eviction order $p_j(\tau_0)$.

This alternative model will affect both the theoretical formula for RTC's tenant welfare impact, and our strategy for quantifying it empirically. We go through each set of implications, and then derive an upper bound on the amount by which our welfare estimates could be *understated* (i.e., too negative). If tenants are in fact unaware of the program, the largest possible downward bias in our welfare estimates is modest—9 cents per month in the baseline no heterogeneity specification. Our qualitative conclusion that RTC likely harmed tenants is therefore robust to tenants being unaware of the program.

Implications for Theoretical Welfare Formula. In this model, tenants will respond to the rental price changes induced by RTC, but not the additional protections themselves. Because of this, tenants reoptimize solving the wrong problem, and so this reoptimization has a first-order welfare impact as the level of protection τ moves away from the pre-RTC level τ_0. Our analysis therefore has to separately keep track of the problem tenants are solving, and the function that actually determines their welfare. At the default stage, given housing choice h and their actual rent $R(h;\tau)$, a tenant solves the following problem:

$$\begin{aligned} \max_{f\in[0,1]} u(y-fR) &+ \min\{\tau_0+f,1\}[v(h)-v(0)]+v(0) \\ &- 1\{f<1\}[C+\underbrace{p_j(\tau_0)h_j[v(h)-v(0)]+p_j(\tau_0)(u(y-fR)-u(y-fR-y_j))}_{\text{Perceived impact of an eviction order on time housed and income}}]\,. \end{aligned} \tag{17}$$

Denote the solution to this problem $f(y;\tau_0,h,R(h;\tau))$, where we now distinguish the direct impact of τ on a tenant's decision through (i) perceived protections τ (which in this case, the tenant misperceives as τ_0) and (ii) the impact on rental prices R. The relevant conditions are for the upper and lower bounds $\hat{y}(\tau_0,h,R(h;\tau))$ and $y_0(\tau_0,h,R(h;\tau))$ on income realizations such that the tenant defaults an interior amount, and for optimal interior default:

- y_0 satisfies

$$[p_j(\tau_0)u'(y_0-y_j)+(1-p_j(\tau_0))u'(y_0)]R(h;\tau)=v(h)-v(0)\,.$$

- $\hat{y}$ satisfies

$$\begin{aligned} u(\hat{y}-R)+v(h)-v(0) &= p_j(\tau_0)u(\hat{y}-f(\hat{y})R-y_j) \\ &\quad + (1-p_j(\tau_0))u(\hat{y}-f(\hat{y})R) \\ &\quad + (\tau_0+f(\hat{y})-p_j(\tau_0)h_j)[v(h)-v(0)]-C \end{aligned}$$

- For $y \in (y_0, \hat{y})$,

$$[p_j(\tau_0)u'(y - f(y)R - y_j) + (1 - p_j(\tau_0))u'(y - f(y)R)]R(h;\tau) = v(h) - v(0)\,.$$

In the above and below, we will sometimes suppress arguments of $f(\cdot)$, $y_0(\cdot)$, and $\hat{y}$. Given the solution to the optimal default problem given h and $R(h;\tau)$, the household makes their housing choice to solve

$$\begin{aligned}
\tilde{W}(\tau;\tau_0) = \max_h \quad & v(0) \\
&+ \int_0^{y_0(\tau_0,h,R(h;\tau))} \Big[p_j(\tau_0)u(y - y_j) + (1 - p_j(\tau_0))u(y) + (\tau_0 - p_j(\tau_0)h_j)[v(h) - v(0)] - C\Big] dF(y) \\
&+ \int_{y_0(\tau_0,h,R(h;\tau))}^{\hat{y}(\tau_0,h,R(h;\tau))} \Big[p_j(\tau_0)u(y - f(y)R(h,\tau) - y_j) + (1 - p_j(\tau_0))u(y - f(y)R(h,\tau)) \\
&+ (\tau_0 + f(y) - p_j(\tau_0)h_j)[v(h) - v(0)] - C\Big] dF(y) \\
&+ \int_{\hat{y}(\tau_0,h,R(h;\tau))}^{\bar{y}} \Big[u(y - R(h,\tau)) + v(h) - v(0)\Big] dF(y)\,, \qquad (18)
\end{aligned}$$

where we denote the value of the solution as $\tilde{W}$ to emphasize that this is the tenants' *perceived* value of the optimum, not the true value. The tenant's housing choice satisfies the following first-order condition:

$$\begin{aligned}
[h] \quad 0 \quad = \quad & (\tau_0 - p_j(\tau_0)h_j)v'(h)F(y_0(\tau_0, h, R(h;\tau))) \\
&+ \int_{y_0}^{\hat{y}} \left((\tau_0 + f - p_j(\tau_0)h_j)v'(h) + \frac{df}{dh}[v(h) - v(0)] - \left[R\frac{df}{dh} + \frac{dR}{dh}f\right] \tilde{u}'(c(y))\right) dF(y) \\
&+ \int_{\hat{y}}^{\bar{y}} \left(v'(h) - \frac{dR}{dh}\bar{u}'(c)\right) dF(y)\,,
\end{aligned}$$

where $\tilde{u}'(c(y)) \equiv p_j(\tau_0)u'(y - f(y)R - y_j) + (1 - p_j(\tau_0))u'(y - f(y)R)$ is defined as the tenant's perceived expected marginal utility of income at an income realization y where they partially default. By optimal interior default, the housing FOC simplifies to

$$\begin{aligned}
0 \quad = \quad & (\tau_0 - p_j(\tau_0)h_j)v'(h)F(y_0) \qquad (19) \\
&+ \int_{y_0}^{\hat{y}} \left[(\tau_0 + f - p_j(\tau_0)h_j)v'(h) - f\frac{dR}{dh}\tilde{u}'(c(y))\right] dF(y) + \int_{\hat{y}}^{\bar{y}} \left(v'(h) - \frac{dR}{dh}\bar{u}'(c)\right) dF(y)\,.
\end{aligned}$$

Denote the solution to equation (19) by $h(\tau_0; R(\cdot;\tau))$. This FOC is analogous to equation (15) in the Appendix, but with τ_0 and $p_j(\tau_0)$ at their pre-RTC values. However, *actual* tenant welfare $W(\tau, \tau_0)$ is given by

$$
\begin{aligned}
W(\tau,\tau_0) = & \int_0^{y_0(\tau_0, h(\tau_0; R(h;\tau)), R(h;\tau))} [p_j(\tau)u(y-y_j) + (1-p_j(\tau))u(y) + (\tau - p_j(\tau)h_j)[v(h)-v(0)] - C]dF(y) \\
& + \int_{y_0(\tau_0, h(\tau_0; R(h;\tau)), R(h;\tau))}^{\hat{y}(\tau_0, h(\tau_0; R(h;\tau)), R(h;\tau))} [p_j(\tau)u(y - f(y;\tau_0, h, R(h;\tau))R(h,\tau) - y_j) \\
& + (1-p_j(\tau))u(y - f(y;\tau_0, h, R(h;\tau))R(h,\tau)) \\
& + (\tau + f(y;\tau_0, h, R(h;\tau)) - p_j(\tau)h_j)[v(h)-v(0)] - C]dF(y) \\
& + \int_{\hat{y}(\tau_0, h(\tau_0; R(h;\tau)), R(h;\tau))}^{\bar{y}} [u(y - R(h,\tau)) + v(h) - v(0)]dF(y) \ + v(0)\,.
\end{aligned}
\tag{20}
$$

Proposition 2 derives an analogous expression to Proposition 1 for this model.

Proposition 2. *The marginal tenant welfare impact of increasing protections from level τ when tenants perceive protections τ_0 is*

$$
\begin{aligned}
\frac{dW(\tau,\tau_0)}{d\tau} \ = \ & F(\hat{y})\left[\left(1 - \frac{dp_j}{d\tau}h_j\right)[v(h)-v(0)] - \frac{dp_j}{d\tau}\mathbb{E}_{y<\hat{y}}[u(y-fR) - u(y-fR-y_j)]\right] \\
& - \frac{dR}{d\tau}\mathbb{E}_y[f\bar{u}'(c)] + \Delta_h + \Delta_f + \Delta_{\hat{y}}\,,
\end{aligned}
\tag{21}
$$

where

$$
\begin{aligned}
\Delta_h = & \frac{dR}{d\tau}\frac{dh}{dR}[F(y_0)(\tau - p_j(\tau)h_j)v'(h) \\
& + \int_{y_0}^{\hat{y}} \left[(\tau + f - p_j(\tau)h_j)v'(h) - f\frac{dR}{dh}\bar{u}'(c)\right] dF(y)\,, \\
& + \int_{\hat{y}}^{\bar{y}} [v'(h) - \frac{dR}{dh}\bar{u}'(c)]dF(y)] \\
\Delta_f = & \frac{dR}{d\tau}\int_{y_0}^{\hat{y}} \left[\frac{df}{dh}\frac{dh}{dR} + \frac{df}{dR}\right](v(h) - v(0) - R\bar{u}'(c))\,dF(y) \\
\Delta_{\hat{y}} = & \frac{dR}{d\tau}\frac{dF}{d\hat{y}}\left[\frac{d\hat{y}}{dh}\frac{dh}{dR} + \frac{d\hat{y}}{dR}\right] \times \\
& (p_j(\tau)u(\hat{y} - f(\hat{y})R - y_j) + (1-p_j(\tau))u(\hat{y} - f(\hat{y})R) + (\tau + f(\hat{y}) - p_j(\tau)h_j)[v(h)-v(0)] - C \\
& - [u(\hat{y} - R) + v(h) - v(0)]\,)\,.
\end{aligned}
$$

Proposition 2 is proved by taking the derivative of W with respect to τ as in the proof of Proposition 1, and collecting the additional terms that do not cancel out due to suboptimal behavior. Equation (21) provides a characterization of how the theoretical welfare formula changes when tenants are unaware of RTC. Terms Δ_h, Δ_f, and $\Delta_{\hat{y}}$ would all be zero if tenants optimized correctly. Δ_h reflects the welfare impact of misoptimization due to housing choice; Δ_f for default choice; and $\Delta_{\hat{y}}$ for the cutoff of when a tenant begins to default. Each term has a straightforward interpretation as the product of two components: (1) the rate at which the endogenous variable (h, f, or $\hat{y}$) adjusts in response to the policy change (through the change in rents only), and (2) the degree to which the FOC does not hold at the tenant's chosen values because they have misoptimized. The larger each component, the larger the discrepancy between the welfare impact of RTC when tenants are vs. are not aware of the policy change.

These expressions clarify that if tenants' behavioral responses are small in magnitude, or if their first-order conditions are close to holding, the discrepancy in our welfare formulas will be small. (In the extreme case where tenants do not even perceive the change in rents, these terms are zero and the theoretical formula for RTC's welfare impact is unchanged.) In addition, the second component of each term is near zero locally to τ_0; it is only as the policy moves far away from tenants' perceptions that the misoptimization becomes first order.

It is also true, as a general property of optimization, that the *integral* of the sum of the three terms over any interval $[\tau_0, \tau]$ must be weakly negative; otherwise, a fully informed tenant could improve on her choice. This means that Proposition 1 provides a theoretical upper bound on RTC's tenant welfare impact when integrated over the path of any discrete policy change in this alternative model.

Implications for Welfare Estimates. Lack of tenant awareness will also affect our empirical implementation of the welfare framework through the tenant's default condition, which we use to determine tenants' willingness-to-pay for additional housing consumption. In the fully rational model, the optimal default condition is

$$\frac{1}{R} = \frac{\bar{u}'(c)}{v(h) - v(0)} = \frac{p_j(\tau)u'(y - fR - y_j) + (1 - p_j(\tau))u'(y - fR)}{v(h) - v(0)}.$$

However, if tenants believe that their probability of an eviction order remains at pre-RTC levels, their default condition will instead satisfy

$$\begin{aligned}\frac{1}{R} &= \frac{p_j(\tau_0)u'(y - fR - y_j) + (1 - p_j(\tau_0))u'(y - fR)}{v(h) - v(0)} \\ &> \frac{p_j(\tau)u'(y - fR - y_j) + (1 - p_j(\tau))u'(y - fR)}{v(h) - v(0)},\end{aligned}$$

where the last inequality follows from $p_j(\tau_0) > p_j(\tau)$ and $u'(y - fR - y_j) > u'(y - fR)$.[54]

[54] In our empirical implementation of the welfare framework, we evaluate p_j at its pre-RTC value when estimating the insurance value of protections. This is conservative in the sense that we likely overestimate

The economic implication is that if tenants are unaware of RTC, their value of additional time housed is *greater* than the rent R because they are overly pessimistic about their outcomes in eviction court and hence their expected marginal utility of numeraire consumption. Our welfare estimates would then understate the value, in terms of additional numeraire consumption, of the additional housing consumption that RTC entails. We may therefore be understating the benefits from RTC, and hence its overall welfare impact for tenants.

To summarize, there are two components to the bias of the welfare analysis in our rational model if tenants are in fact unaware of RTC. First, we know Proposition 1 (under which tenants optimize) is an upper bound to the welfare benefits of RTC if tenants instead fail to optimize due to incorrect beliefs. Second, in our empirical implementation, we undervalue the additional time housed under RTC, as just discussed. While it is difficult to estimate the magnitude of the first component, we are able to quantify the second component, and hence to bound the extent to which we understate the welfare benefits (or overstate the costs) of RTC. The rest of this section applies this reasoning to show that *if* our welfare estimates are biased downward, the lower bound implies the magnitude of the bias is quantitatively small. As a result, even if tenants are unaware of the RTC program, that alone would not change the qualitative conclusion that RTC likely reduced tenant welfare.

We proceed as follows. From the unaware tenant's default condition, we can rewrite a tenant's *true* MRS between c and h as

$$\frac{\bar{u}'(c,\tau)}{v(h)-v(0)} = \frac{\bar{u}'(c,\tau)}{\bar{u}'(c,\tau_0)}\frac{\bar{u}'(c,\tau_0)}{v(h)-v(0)} = \frac{\bar{u}'(c,\tau)}{\bar{u}'(c,\tau_0)}\frac{1}{R} = \frac{\Delta}{R},$$

where

$$\Delta \equiv \frac{p_j(\tau)u'(y-fR-y_j)+(1-p_j(\tau))u'(y-fR)}{p_j(\tau_0)u'(y-fR-y_j)+(1-p_j(\tau_0))u'(y-fR)}.$$

Then, repeating the derivations in the *Equivalent Variation* subsection of Section D.1 above, we obtain an alternate version of equation (10):

$$\frac{dW(\tau)}{d\tau} = \frac{F(\hat{y})\left[\frac{R}{\Delta} - \frac{dp_j}{d\tau}\left(h_j\frac{R}{\Delta}+y_j\bar{\theta}\right)\right] - \frac{dR}{d\tau}\left[\bar{f}_E F(\hat{y}) + (1-F(\hat{y}))\Omega\right]}{F(\hat{y})+(1-F(\hat{y}))\Omega}\mathbb{E}[\bar{u}'(c)]\,. \quad (22)$$

In other words, the value of additional housing consumption from RTC is scaled up by a factor of $\frac{1}{\Delta}$ (which is greater than 1). Equation (10) is otherwise unchanged.

Using this expression, we can now calculate an upper bound on RTC's actual tenant welfare impact if tenants are unaware of the program. We simply calculate Δ and recompute welfare as described in Section 5. As in our baseline calculation, RTC increased tenant

RTC's insurance value. The willingness-to-pay for additional time housed is still R for evicted tenants if they default optimally.

representation by 12.4 percentage points in treated ZIP codes; representation reduces the probability of an eviction order by 32.1%; and the probability of an eviction order without representation is 64.3% (Cassidy and Currie, 2023). We therefore use $p_j(\tau_0) = 0.643$ and $p_j(\tau) = 0.643 - 0.124 \times 0.321 = 0.603$. In the no-heterogeneity specification, this yields the following values of Δ and resulting bias as a function of tenant risk aversion γ:

γ	Δ	Bias in W
1	0.995	−\$0.04
2	0.990	−\$0.09
3	0.985	−\$0.16
4	0.981	−\$0.25
5	0.976	−\$0.36

Our estimates could overstate RTC's welfare cost to tenants by at most \$0.04-\$0.36, depending on tenant risk aversion, if tenants are unaware of the program.

D.2 Tenant Heterogeneity

This appendix section extends our empirical welfare framework to allow for richer heterogeneity across tenants. It is useful to first introduce two terms. We use "ex-ante income" to refer to a tenant's income prior to the realization of eviction-relevant income shocks. Conceptually, ex-ante income is the income level at which a tenant's (ex-ante) choice of housing consumption is assumed to be optimal. We define "ex-post income" as a tenant's realized income later in their lease, at which point rent default is decided.

Our extended welfare framework allows for heterogeneity in terms of ex-ante income as well as the distributions of ex-post income, rent, and nonpayment. Throughout, we use subscripts g to refer to a tenant's ZIP code of residence, which is an important dimension of heterogeneity in our empirical implementation, and i to refer to their ex-ante income. The generalized version of equation (10) is:

$$\frac{dW_{gi}(\tau)}{d\tau} = \frac{F_{gi}(\hat{y}_{gi})\left[R_{gi} - \frac{dp_j}{d\tau}\left(h_j R_{gi} + y_j \bar{\theta}_{gi}\right)\right] - \frac{dR}{d\tau}\left[\bar{f}_{E,gi} F_{gi}(\hat{y}_{gi}) + (1 - F_{gi}(\hat{y}_{gi}))\Omega_{gi}\right]}{F_{gi}(\hat{y}_{gi}) + (1 - F_{gi}(\hat{y}_{gi}))\Omega_{gi}} \mathbb{E}_i[u'(c_{gi}(\tilde{y}))] \tag{23}$$

with

$$\begin{aligned}
\bar{\theta}_{gi} &= \mathbb{E}_i\left[\frac{u(\tilde{y}-f_{gi}(\tilde{y})R_{gi})-u(\tilde{y}-f_{gi}(\tilde{y})R_{gi}-y_j)}{y_j u'(c_{gi}(\tilde{y}))} \;\mid\; \tilde{y}<\hat{y}_{gi}\right] \\
\Omega_{gi} &= \frac{\mathbb{E}_i[u'(c_{gi}(\tilde{y}))\mid \tilde{y}\geq \hat{y}_{gi}]}{\mathbb{E}_i[u'(c_{gi}(\tilde{y}))\mid \tilde{y}<\hat{y}_{gi}]} \\
u'(c_{gi}(\tilde{y})) &= \begin{cases} p_j u'(\tilde{y}-f_{gi}(\tilde{y})R_{gi}-y_j)+(1-p_j)u'(\tilde{y}-f_{gi}(\tilde{y})R_{gi}) & \text{if } \tilde{y}<\hat{y}_{gi} \\ u'(\tilde{y}-R_{gi}) & \text{otherwise} \end{cases} \\
\bar{f}_{E,gi} &= \mathbb{E}_i[f_{gi}(\tilde{y})\mid \tilde{y}<\hat{y}_{gi}]\,.
\end{aligned}$$

Equation (23) highlights that tenants are treated as ex-ante identical conditional on geography (ZIP code) and ex-ante income, and shows which dimensions of heterogeneity we allow to vary at which level. In particular, we allow the distribution of ex-ante income and rents to vary by ZIP code, and for the distributions of income realizations and nonpayment to depend on ex-ante income. We discuss these choices, which are driven by data availability, further below. After calculating $\frac{dW_{gi}(\tau)}{d\tau}$ for each ex-ante income bin i and ZIP code g, we take the population-weighted mean to obtain the average tenant welfare impacts reported in the paper for the full-heterogeneity case.

The empirical implementation of equation (23) largely follows section 5.1 with several additional steps. First, to characterize the distribution of ex-ante income, we use the NYCHVS sample described in section 2.4.4 to calculate deciles of renter earnings in RTC-treated ZIPs.[55] This approach assumes that earnings in the NYCHVS are representative of *ex-ante* earnings levels. We then estimate the share of renters in each ZIP code belonging to each ex-ante income decile. To calculate rents R_{gi}, we estimate an average rent in each g, i pair: we model rent as a function of ex-ante earnings and geography fixed effects, estimate this relationship via OLS, and use the predicted values as a smoothed estimate. We also allow the eviction rate to vary by ZIP code, and calculate it analogously to the no-heterogeneity case. Throughout, we use NYCHVS-provided survey weights.

We then construct decile-specific distributions of earnings realizations and rent nonpayment rates for tenants who do and do not face eviction. Specifically, we use our linked data on earnings and evictions to calculate, for each ex-ante decile, the deciles of ex-post earnings for evicted and non-evicted tenants. Ex-ante earnings are computed in the linked data using an average of quarters -8 through -5 relative to eviction filing. The non-evicted sample has event time defined relative to placebo eviction dates, which are randomly generated to

[55]The finest level of geography observed in the NYCHVS is a PUMA, which typically comprises several (and up to 15) ZIP codes. When a treated ZIP code is in a unique PUMA, we assume statistics from that PUMA are representative of that ZIP code. When a treated ZIP is in multiple PUMAs, we allocate from PUMAs to the ZIP proportionally to the weighted number (using survey weights) of NYCHVS observations in each PUMA.

match the distribution of eviction dates in the evicted sample. These two sets of deciles, together with the ten ex-ante deciles, imply $2 \times 10 \times 10$ grid points on which we can flexibly characterize tenant earnings dynamics. Note that these specifications allow for much richer income heterogeneity than the binary realizations we consider in the no-heterogeneity case. We assume that earnings changes and eviction outcomes in the administrative data are independent of rent after conditioning on ex-ante earnings, consistent with the evidence in Garin et al. (2025) that earnings dynamics for lower-income individuals generally not varying by neighborhood. We also allow the share of rent paid, $f_{gi}(\tilde{y})$, to vary across these 10×10 decile pairs in each ZIP code for evicted tenants.

One limitation of our earnings data is that it only covers wages earned in the state of New York. This makes it impossible to distinguish non-employment from attrition due to moving out of state. To address this, we construct an indicator for a string of terminal zeros (consecutive quarters of zero earnings through the end of our sample). To be conservative vis-à-vis our finding about the insurance value and welfare benefits of RTC, we treat terminal zeros in the evicted sample as true zeros when measuring ex-post income, while dropping terminal zeros in the non-evicted sample, thus treating them as randomly censored by out-migration from New York state. We use this approach in our representative-tenant specifications as well as when we allow for greater tenant heterogeneity. This approach makes our insurance value estimates plausibly an upper bound on the true insurance value of RTC.

We are unable to allow for heterogeneity in most quasi-experimental estimates that are inputs to equation (23). Estimates of heterogeneous effects at this level of detail are not available from Collinson et al. (2024) or Cassidy and Currie (2023). We also do not include heterogeneity in $\frac{dR}{d\tau}$, though the evidence in Figure 4 suggests that doing so would likely increase the welfare costs of RTC, as rent increases due to RTC were greater in the (typically lower-income) ZIP codes with higher eviction rates. Heterogeneity in the other quasi-experimental inputs to equation (23) could also impact our welfare calculations, particularly on the benefits side – for example, if tenants seeing the largest improvements in court outcomes due to representation also would have the greatest impacts of an eviction order. We view credible estimates of these heterogeneous effects as a valuable direction for future work.

D.3 Standard Errors

We compute standard errors for the welfare estimates via bootstrap. The inputs to the welfare calculation come from a variety of sources, and in some cases, we do not have access to the underlying data that generated the statistics. For statistics taken from other papers, we appeal to asymptotic normality and assume each estimate is independently normally distributed. For statistics we generated, we bootstrap each statistic by appropriately resampling the underlying data and recomputing the statistics generated from that dataset. Then, for bootstrapped draws of all inputs, we recompute the welfare estimate in equation (10). The rest of this section describes our resampling procedure for each statistic.

The first set of statistics are causal estimates taken from other papers. RTC's impact on the likelihood of legal representation, and the impacts of representation on case durations and the probability of a judgment against the tenant, are taken from Cassidy and Currie (2023). Estimates of the impact of an eviction order on subsequent income and homelessness are taken from Collinson et al. (2024). Appealing to asymptotic normality of OLS and IV estimators, we resample each estimate independently from a normal distribution with mean equal to the point estimate and standard deviation equal to the reported standard error. The values are reported in Table 4 in the main text.

The second category of statistics are ones where we have access to the underlying data.

- We sample each rent effect estimate by drawing from a normal distribution with mean equal to the point estimate and standard deviation equal to the asymptotic standard error.

- We use the NYCHVS to calculate the average income and rent in RTC ZIP codes. In the no-heterogeneity specifications, these are simply the means across all renters. For the specifications with income and geographic heterogeneity, we construct income deciles and calculate mean rent and income within each decile. Rent is also allowed to vary by ZIP code. Because the NYCHVS uses a complex sampling methodology, resampling observations with replacement from the survey would not give a correct estimate of the sampling variation. Instead, we estimate the covariance matrix of the relevant statistics using the replicate weights provided by the NYCHVS.[56] Then we jointly sample the statistics from a normal distribution with the mean and covariance matrix of the statistics from the replicate draws.

- From the linked eviction and quarterly earnings data, we resample households with replacement overall (no heterogeneity) or within each ex-ante income decile (full heterogeneity). In the latter specifications, the income decile cutoffs come from bootstrap draw k of the NYCHVS statistics. We then compute the appropriate mean income realization—either across all households or within each realization decile for each ex-ante income decile—in the evicted and non-evicted bootstrapped samples.

We treat eviction filing rates as not subject to statistical uncertainty, because we observe the population of eviction cases and renters. Since we independently resample different

[56] The U.S. Census Bureau, which conducts the NYCHVS, describes the replicate weights methodology and appropriate construction of standard errors here: https://www.nyc.gov/assets/hpd/downloads/pdfs/about/2023-nychvs-guide-to-estimating-variances.pdf.

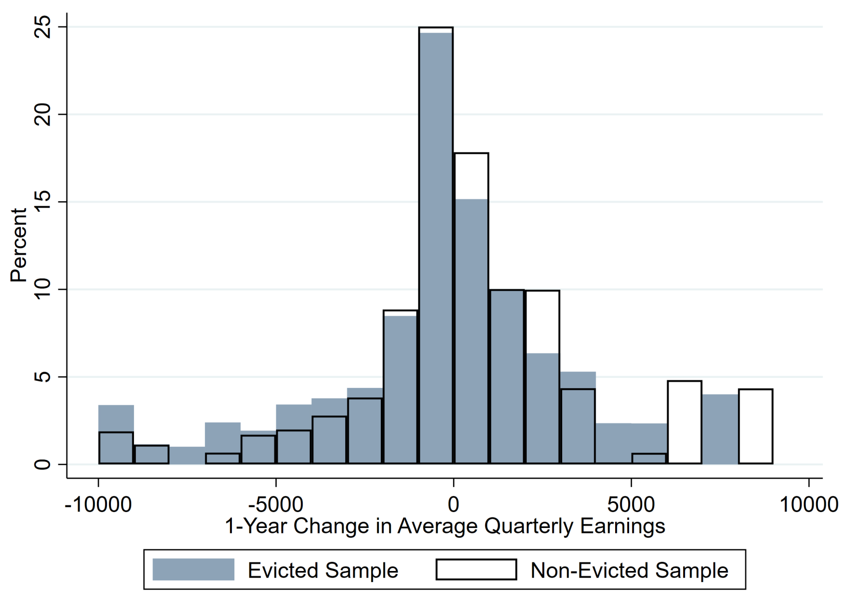
datasets, our resampling procedure treats statistics as independent if they are generated from different datasets, but allows dependence if they come from (or depend on) the same underlying data.

We take 500 bootstrap draws of each statistic. Where relevant, we use the same bootstrap draw of the underlying data to generate the k-th bootstrapped statistic across different specifications. Reported 95% confidence intervals are centered at our point estimate and extend 1.96 standard deviations in either direction. The p-values reported in Table 5 are for one-sided tests of the null hypothesis that RTC had nonnegative welfare effects against the alternative of negative effects.

D.4 Additional welfare results

This section includes evidence on the distribution of earnings changes for evicted and non-evicted tenants (Figure D.1) and robustness of our welfare estimates to alternative utility parameters under the no-heterogeneity specification (Table D.1).

Figure D.1: 1-Year Earnings Changes for Evicted and Non-Evicted Tenants



Note: This figure shows 1-year changes in earnings from ex-ante to ex-post, separately for the evicted and non-evicted samples described in section 2.4.3 of the text. Ex-ante earnings are defined using an average in quarters -8 through -5 relative to eviction filing, and ex-post earnings are defined using an average in quarters -1 through 2 relative to eviction filing. The non-evicted sample has event time defined relative to placebo eviction dates, which are randomly generated to match the distribution of eviction dates in the evicted sample.

D.5 Step-by-Step Example of Welfare Calculation

To develop intuition for how we arrive at our welfare estimates, it is useful to revisit the various components of expression 12 in Section 5.1. This section does so in a step-by-step

Table D.1: Impact of RTC on Tenant Welfare (Alternative Parameters, No Tenant Heterogeneity)

	DiD (calipered)	DiD (calipered, flex)
Panel A: Relative Risk Aversion		
$\gamma = 1$	-27.71 (7.51)	-19.05 (7.92)
$\gamma = 2$	-25.68 (7.72)	-17.09 (8.05)
$\gamma = 3$	-23.32 (8.20)	-14.80 (8.44)
$\gamma = 5$	-17.60 (10.04)	-9.26 (10.04)
Panel B: Consumption Floor		
$c = \$2{,}000$	-25.68 (7.72)	-17.09 (8.05)
$c = \$4{,}000$	-25.68 (7.72)	-17.09 (8.05)
$c = \$8{,}000$	-25.68 (7.72)	-17.09 (8.05)

Note: Estimates and standard errors for the tenant welfare impact of RTC. Each row varies one parameter from the baseline value: the coefficient of relative risk aversion in Panel A, or the annual consumption floor in Panel B. Baseline values are $\gamma = 2$ and $c = \$4{,}000$. All specifications are for an ex-ante representative tenant. Estimates are insensitive to the level of the consumption floor because the representative tenant's earnings are never low enough for the floor to be binding.

way, focusing on the estimates corresponding to our "DiD (calipered, flex)" rent specification with no tenant heterogeneity, which corresponds to an estimated welfare loss of just over $17 per month due to RTC.

The "cost side" of the formula is relatively straightforward to understand; it is the product of the Rent Effect and the Correction for Default. As discussed in the previous section, the correction for default is close to 1 (0.957), and so the cost side of the formula using the rent effect estimate from the DiD (calipered, flex) specification is $\$29.13 \times 0.957 = \$27.88$.

The "benefit side" of the formula is the product of the Court Benefits and Insurance Value for the average renter in ZIP codes which received RTC. The Court Benefits include the value of additional time in the unit after defaulting, and the avoided losses in housing and numeraire consumption in the event of an eviction order. The value of the time in unit after defaulting is the product of the eviction rate (12.8%), the impact of RTC on representation (12.4%), and the increase in case duration for represented tenants (85 days, or 2.79 months). This additional time is valued at the contract rent, or $1,527 per month, due to optimal default. The benefit from longer case durations is then

$$.128 \times .124 \times 2.79 \times \$1,527 = \$67.73\,,$$

or $5.64 per month. The benefits from increased housing and numeraire consumption occur in the 32.1% of eviction cases in which tenants avoid an eviction order because they are represented. In these cases, tenants enjoy 0.070 years (0.84 months) of additional housing consumption through avoided homelessness, also valued at $1,527 per month, as well as

\$3,200 of additional consumption through avoided income loss. The term $\bar{\theta}$, which accounts for the curvature of utility when valuing the avoided income loss, scales the \$3,200 but is close to 1. The value of the income and consumption drops are then

$$.128 \times .124 \times 0.321 \times (0.84 \times \$1,527 + \$3,200 \times 0.955) = \$\,22.10,$$

or \$1.84 per month. Together, the Court Benefits total \$7.48 per month for the average tenant.

The court benefits are then scaled by the insurance multiplier, $[F(\hat{y}) + (1 - F(\hat{y}))\,\Omega\,]^{-1}$. The ratio Ω depends on our income measures for evicted and non-evicted individuals; how we translate those into consumption; and an assumed functional form for utility. For the no-heterogeneity specifications, we estimate annual incomes of \$47,318 for non-evicted tenants, and \$39,588 for evicted tenants. Non-evicted tenants pay the full rent (by assumption), so their annual consumption is $y - R = \$28,991$. Evicted tenants have a lower income realization—\$39,588—but also do not pay the full rent; they default on 23.3% of the annual rent, or $\$1,527 \times 2.80 = \$4,269$. As a result, evicted tenants' annual consumption is \$25,530 if they do not receive an eviction order, and \$22,330 if they do. The marginal utility of consumption is $u'(c) = c^{-\gamma}$, with $\gamma = 2$ in our preferred specification.[57] The ratio of expected marginal utilities is then

$$\Omega = \frac{28,991^{-2}}{0.357 \times 25,530^{-2} + 0.643 \times 22,330^{-2}} = 0.648\,,$$

using the baseline probability of an eviction order p_j (without RTC) of 64.3%. This yields an Insurance Multiplier of 1.44. The ex-ante value of the court benefits, including the insurance value of protections, is therefore $\$7.48 \times 1.44 = \$10.79$ per month.

Putting the four terms together, we obtain

$$\begin{aligned}\Delta W &= (\text{Court Benefits}) \times (\text{Insurance Multiplier}) - (\text{Rent Effect}) \times (\text{Correction for Default}) \\ &= \$7.48 \times 1.44 - \$29.13 \times 0.957 \\ &= -\$17.09\,,\end{aligned}$$

which is the corresponding "No Heterogeneity" point estimate in Table 5.

[57]The consumption floor is not binding for any income realization in the no-heterogeneity specification, but it is for some realizations in specifications with full income heterogeneity.

D.6 Sources of Landlord Costs

The estimated reduction in rental housing supply suggests that landlord costs from RTC were at least as large as the estimated price increase. Though we do not attempt to directly measure the additional costs to landlords generated by the RTC program, there are several potential sources that could generate cost increases equal to or greater than the estimated \$29-\$38 rent increase per tenant per month. To begin with, if tenants continue defaulting during their eviction proceedings, the court delays due to RTC would cost landlords about \$5.50-\$6 per tenant month in lost rental revenue (cf. the *Intuition for Magnitudes* subsection of Section 5.2). Longer legal proceedings likely also raised landlords' legal fees. Though it is difficult to obtain price quotes for private eviction lawyers, anecdotes suggest that lawyers typically charge \$150-\$400 per hour, which could easily amount to an additional \$2,000-\$4,000 per case, or up to \$5.29 per tenant month.[58] Added to this would be any time or hassle costs for the landlord due to a more protracted eviction process. Finally, tenants' behavioral responses could also generate costs—for example, through additional default in response to stronger eviction protections, or causing additional damage to the unit while occupying it (as Campbell et al. (2011) emphasize in the context of mortgage foreclosure). As our model highlights, these responses have second-order welfare effects for tenants, but first-order effects on landlord costs.

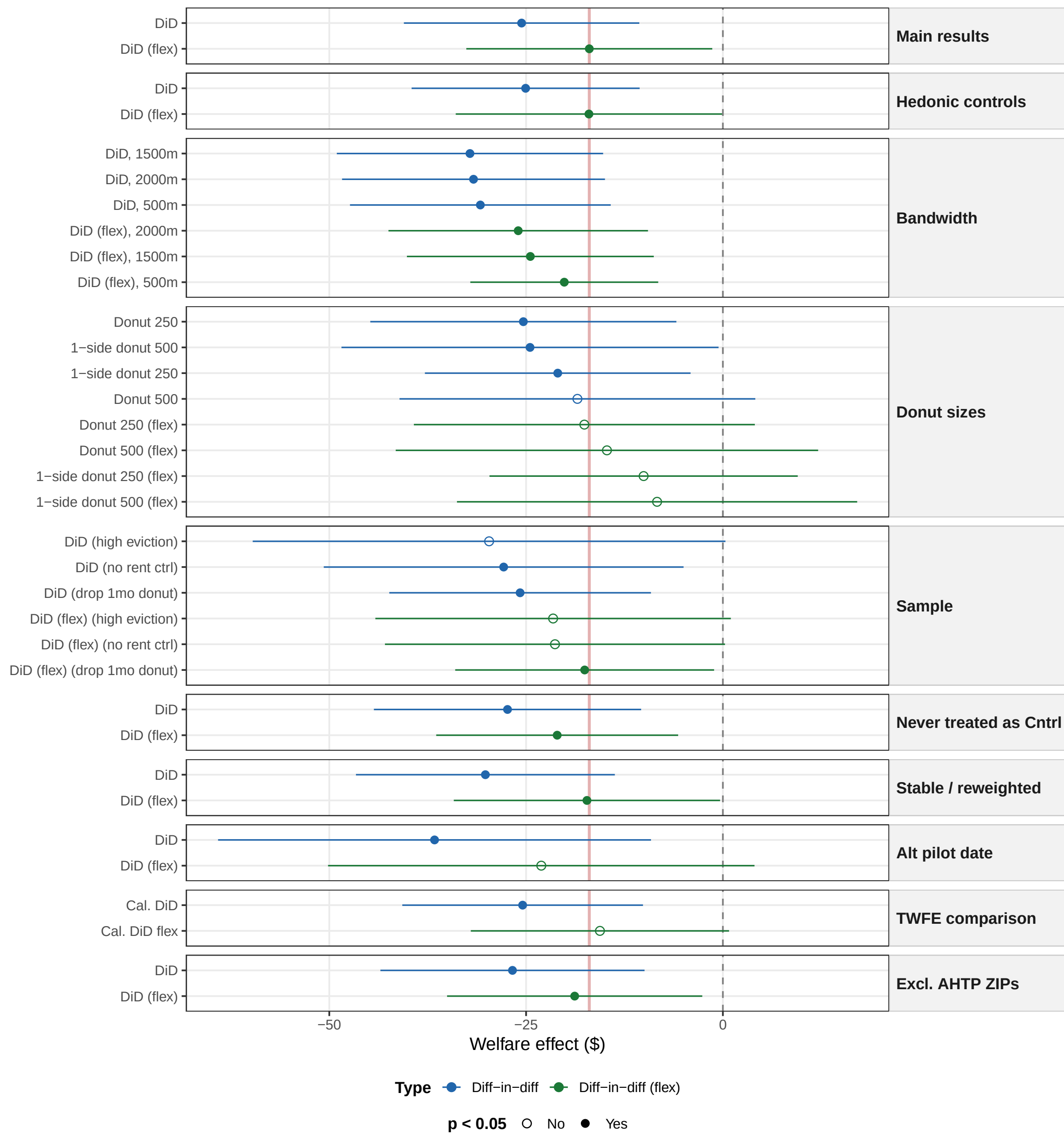
E Right to Counsel in Connecticut

This section replicates our empirical evidence on RTC in New York City for Connecticut, which has been rolling out legal representation in select ZIP codes since 2022. First, we estimate the impact on court outcomes, closely following Cassidy and Currie (2023). We replicate our calipered border DiD design to estimate the impacts on listed rents. Similar to the estimates from Cassidy and Currie (2023) for NYC, we find that providing legal counsel results in fewer judgments for possession and longer cases. Second, similar to our analysis for NYC, we estimate that RTC increased rent prices. We estimate that posted monthly rents increased \$26-40 in year 1. Our estimates for year 2 are noisier and largely not statistically significant, likely due to the smaller sample.

Background Since 2000, landlords in Connecticut (henceforth, CT) filed roughly 20,000 eviction cases yearly, half of which occurred in five of the state's largest cities. A report by the CT Data Collaborative showed that only 7% of tenants are represented by a lawyer in eviction court annually (CTDC, 2022). Although the COVID-19 temporary renter protections

[58] On the tenant side, the mean tenant lawyer compensation per case was roughly \$3,200 in 2021 (Cassidy and Currie, 2023).

Figure D.2: Tenant welfare estimates across rent effect specifications



Note: This figure compares point estimates and standard errors of RTC's tenant welfare impact obtained using alternative estimates of the program's effect on rent prices. The specifications are otherwise analogous to the "Full Heterogeneity" specifications reported in column 2 of Table 5, which assume a minimum consumption level of $c = \$4,000$ and a coefficient of relative risk aversion of $\gamma = 2$. Each row corresponds to one rent effect specification in Figure C.11; for each, the dot denotes the tenant welfare effect point estimate and the line a symmetric 95% confidence interval. Dots are hollow if the confidence interval contains zero, otherwise solid. The vertical red line marks the welfare point estimate using the rent effect estimate from the baseline DiD (flex) specification reported in column 2 of Table 2. Rows are grouped by the type of robustness specification. "Hedonic controls" correspond to estimates from panel B of Table C.7; "Bandwidth" to panel B of Table C.2; "Donut sizes" to column 2 of Table C.6; "Sample" to panel B of Table C.3; "Never treated as Cntrl" to column 3, panel B of Table C.4; "Stable/reweighted" to column 4 of Table C.8; "Alt pilot date" to columns 2 and 4, panel B of Table C.13; "TWFE comparison" to the first panel of Table C.11; and "Excl. AHTP ZIPs" to column 2, panel B of Table C.14.

led to a 50% decrease in the rates of removal orders, "the disparity in outcomes between unrepresented and represented renters grew substantially, with those who were unrepresented

125% more likely to have removal orders issued" (CTDC, 2022). Anticipating an increase in eviction cases after the pandemic, Connecticut created an eviction right-to-counsel program in 2022 (State of Connecticut, 2021).

The program The Connecticut Right to Counsel Program (RTC) provides free legal representation to income-eligible tenants facing eviction or the loss of a housing subsidy. The Connecticut Bar Foundation administers the program and legal services providers deliver legal representation to eligible tenants (Stout, 2022). The program had a gradual rollout to cover ZIP codes across several Connecticut cities throughout 2022 and 2023.

A tenant is eligible if they live in an active ZIP code and if either their household income is at or below 80% of the state median income or if they receive specific types of public assistance, such as TANF, SNAP, Medicaid, or federal housing vouchers (State of Connecticut, 2021).

Program rollout On January 31, 2022, the program started its two-year rollout by offering legal representation in selected ZIP codes across several cities. At launch, 14 ZIP codes were prioritized for implementation based on high eviction filing rates and availability of trained attorneys. ZIP codes in Hartford were removed from the program due to capacity limitations on June 13, 2022 and reincluded on September 25, 2023.[59] There was also a switch in ZIP codes in Bridgeport in an effort to increase case referrals.

E.1 Impacts of right to counsel on court outcomes in Connecticut

This section estimates the impacts of counsel on eviction court outcomes in Connecticut. We closely follow the specification in Cassidy and Currie (2023) (CC) to compare these estimates to their estimates for New York City.

We use state eviction court records from August 1, 2021 through January 1st, 2024. To determine if the case was in a ZIP code with RTC and if the policy was yet active in that ZIP code at the time of filing, we rely on the ZIP code of the address listed in the case combined with the date of filing.

We report estimates from three models, all closely following Cassidy and Currie (2023), with only small changes to fit the CT setting. We estimate TSLS regressions of the form

$$R_i = \alpha_0 + \alpha_1 RTC_i + \alpha_2 X_i + ZIP_i + city_i \times month_i \times year_i + e_i \tag{24}$$

$$Y_i = \beta_0 + \beta_1 R_i + \beta_2 X_i + ZIP_i + city_i \times month_i \times year_i + \epsilon_i, \tag{25}$$

[59] Hartford has the highest filing rate in the state and saw a 20% increase in eviction filings in 2022 compared to 2019.

where R_i is an indicator of the defendant having legal representation, RTC_i is an indicator for the case being filed in a ZIP code where RTC is active, X_i is a vector of covariates, ZIP_i is a ZIP code fixed effect and $city_i \times month_i \times year_i$ is a city-by-month-year fixed effect. We additionally report results that replace the ZIP code-fixed effects with address-fixed effects. We additionally report OLS estimates and reduced-form estimates for comparison.

Table E.1 reports our main findings. We estimate that RTC increased the probability of a defendant having a lawyer by 8.5 to 8.8 percentage points, providing a strong first stage for our IV estimates. We then look at IV estimates for whether the case ends in a judgment for possession (i.e., an eviction order) and the number of days the case took to receive a disposition. We estimate that defendants who received counsel via RTC were around 23 to 39 percentage points less likely to have their case end in a judgment for possession (p-value < 0.05 for our main results). Lastly, we find that when defendants received counsel via RTC, the length of cases increased substantially, with IV estimates of 34-50 days (p-value < 0.01 for our main results).

E.2 Impacts of right to counsel on rents in Connecticut

In this section we estimate the impacts of RTC on posted rent prices in Connecticut. This analysis mirrors the analysis for NYC as closely as possible. The main difference is that because the StreetEasy data we use on rental listings in NYC does not include CT listings, we instead use data from Altos Research, a commercial data vendor of rental listings. Using these data and the information on when ZIP codes were treated, we then estimate the impacts of RTC on rent using the same research design as in Section 3.1 of the paper. We restrict our analysis to border pairs where the treated ZIP code was treated in 2022.

Table E.2 shows the estimates on rent prices using the same specifications as in our NYC analysis in the main paper. The point estimates on price suggest an increase in rent of between 26 and 40 dollars in year 1, and between 31 and 56 dollars in year 2, depending on the specification. Our estimates for year 2 are noisier and not statistically significant, potentially due to the smaller sample.

Table E.1: Impacts of defendant legal counsel from RTC on court outcomes

	Main			Address FE		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	RF	OLS	IV	RF
Defendant Counsel (First Stage)		0.088***			0.085***	
		(0.010)			(0.013)	
		[48,321]			[40,795]	
Judgment with Possession	-0.250***	-0.230**	-0.020**	-0.276***	-0.390	-0.033
	(0.009)	(0.097)	(0.008)	(0.018)	(0.265)	(0.021)
	[48,321]	[48,321]	[48,321]	[40,795]	[40,795]	[40,795]
Days to Disposition (Wins.)	37.892***	49.960***	4.380***	40.698***	34.635	2.929
	(1.856)	(16.139)	(1.509)	(3.774)	(26.221)	(2.163)
	[48,311]	[48,311]	[48,311]	[40,786]	[40,786]	[40,786]
First-Stage F Stat		75.44			45.32	
Zip FE	Yes	Yes	Yes	No	No	No
Address FE	No	No	No	Yes	Yes	Yes

Note: All results are for the main sample (filing date between August 1, 2021 and January 1, 2024). Columns (1) and (4) report the OLS linear associations between outcomes and tenant (defendant) counsel. Columns (2) and (5) report two-stage least squares instrumental variable results for defendant counsel, using an indicator for empirical RTC treatment as the instrument (equal to one if RTC is operating in a case's ZIP code at the time of filing). We also present our Reduced Form estimates in columns (3) and (6). Covariates and fixed effects are summarized at the bottom of the table. Columns (1)-(3) control for city-month-year of filing and ZIP code fixed effects, while columns (4)-(6) control for city-month-year of filing and address fixed effects. Standard errors clustered by ZIP code are given in parentheses. Observation counts are in brackets. Following Cassidy and Currie (2023), regressions control for indicators for whether the plaintiff had an attorney, whether the case is a nonpayment case, whether there is a single defendant in a case, and whether there is a single plaintiff in a case. Additionally, from the 5-year ACS, the following controls on ZIP code level are included: quantiles for population, quantiles for fraction of renters, quantiles for fraction female, and quantiles for naturalized citizens. First row reports first-stage results with defendant counsel as the dependent variable. *p<0.1; **p<0.05; ***p<0.01

Table E.2: Impact of CT right to counsel on posted rents

	(1)	(2)
	Year 1 effect	Year 2 effect
DiD (calipered)	39.25***	55.93
	(12.53)	(37.48)
DiD (calipered, flex)	26.11*	31.48
	(14.08)	(31.31)
Observations	38439	14937
Pre-period mean ($/month)	1511.78	1626.84

Note: This table reports estimates of the impact of Connecticut's Right to Counsel on listed rent prices (δ_t in equations 1 and 2) using Altos data. "Year 1" compares listings created 0 to 365 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days after the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. We construct our stacked analysis dataset as described in Section 3.1. We exclude ZIP code border-pairs where one side is treated within one year of the other. The Altos sample is limited to listings without a gym or a pool, and in the bottom 90% of the asking price distribution. We control for year when the listing was created, an indicator for whether unit characteristics were missing, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. Standard errors are clustered at the border-pair level. We drop border pairs where the minimum distance to the border on either side is greater than 300m. We also drop border pairs if there are fewer than 50 observations on either side, or if there are no observations in the pre or post periods. *p<0.1; **p<0.05; ***p<0.01