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

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Abstract

"Right-to-counsel" programs provide free legal assistance to tenants in eviction court. Legal assistance can delay or prevent eviction. However, large-scale legal assistance programs can also generate costs for tenants due to equilibrium rental market responses. In this paper, we study how right to counsel impacts rental markets when implemented at scale, and quantify the policy's impact on tenant welfare. Leveraging the geographic rollout of New York City's program, we find listed rent prices rose by $22-$38/month within two years of policy implementation, with larger increases in areas with higher baseline eviction rates. We do not find evidence that landlords adjusted on other margins, such as tenant screening or improvements to habitability. Guided by these results, we develop a framework to evaluate the policy's welfare implications for tenants, incorporating the trade-off between protection from eviction and higher rent prices. We quantify the parameters of our framework using linked data on eviction court cases, rental housing listings, and tenant earnings trajectories. Despite the direct benefits and insurance value of stronger eviction protections, the estimated price increases are large enough to generate a small net reduction in ex-ante tenant welfare.

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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, but when implemented at a large scale, these interventions may have additional welfare-relevant equilibrium effects due to demand and supply responses in the rental housing market. While researchers have begun to examine the benefits of assisting tenants facing eviction, little evidence exists on the equilibrium effects of such policies.

A leading tenant 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).[1] 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). This evidence suggests these policies may also lead to equilibrium responses when implemented at scale.

In this paper, 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 to identify supply-side responses to an eviction protection policy implemented at large scale. We combine a quasi-experimental research design based on the rollout with multiple linked data sets—eviction court cases, rental housing properties and listings, credit reports, and residential histories—to evaluate the market responses to RTC. We find evidence of rent price increases in ZIP codes where RTC was introduced, as well as evidence consistent with decreases in quantity. We do not find evidence that landlords adjust on other margins, such as tenant screening or improvements to habitability.

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 additional 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

[1] Given that median tenant representation rates in these jurisdictions was 2% before RTC (NCCRC, 2024), there is substantial scope for these programs to increase legal assistance.

our empirical results and framework, we estimate that the price increases due to RTC are large enough to more than offset the benefits, causing a net reduction in ex-ante tenant welfare.

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, reduced the probability of a judgment against the tenant, and lowered money judgments for represented tenants (Ellen et al., 2021; Cassidy and Currie, 2023). Specifically, Cassidy and Currie (2023) find that providing legal assistance results in a 32 percentage point reduction in eviction orders, and increases case lengths by nearly three additional months.

These facts suggest that legal assistance benefits tenants who receive representation in eviction court, but may also generate significant costs for landlords.[3] Further, these costs and benefits need not be symmetric. For a tenant, a longer case duration means they can remain in their unit longer while not necessarily paying rent. This acts like an in-kind transfer of housing from the landlord to the tenant. For a landlord, RTC may also generate legal, time, and hassle costs that the tenant does not directly value. This separation 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 our 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

[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).

[3]A back-of-the-envelope calculation, detailed in Appendix D, suggests landlord costs increased by at least \$14-18 per tenant per month. We view this as a conservative lower bound.

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 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 rent prices increased in the two years after the policy was implemented. Across a range of empirical specifications, we estimate an increase of 6-17 dollars per month in the first year, and 22-38 dollars per month in the second year. The two-year increases are statistically and economically significant, amounting to a 0.9-1.6 percent price increase. We estimate larger price increases in ZIP codes with higher baseline eviction rates. 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 a supply-side response to the policy.

Landlords could also respond to increased protections in other ways – for example, by screening prospective tenants more aggressively, or by reducing (or increasing) maintenance. Such responses could further impact RTC's costs and benefits for tenants, as well as its incidence on different types of renters. While we do not observe landlord screening practices directly, we test for screening responses by examining whether the credit scores of tenants moving into treated ZIP codes systematically changed in a separate linked dataset of individual credit and address histories. We estimate precise zero effects on credit-report measures, consistent with limited screening responses. We also find null or slightly positive effects on formal habitability complaints, a proxy for poor unit quality. 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 our focus on price and quantity as the first-order supply-side responses to RTC.

In the second part of the paper, we quantify how RTC impacted tenant welfare in light of the market response to RTC. 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 use an envelope theorem argument to show that 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 marginal 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 an eviction judgment, which generates positive 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 the shift in supply that is relevant to tenant welfare. As a result, RTC's 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 approach to welfare analysis has several advantages from an empirical standpoint: it is not necessary to measure tenants' behavioral responses to RTC or the costs the policy generated for landlords, or to explicitly model supply-side behavior or market clearing. However, an important limitation of this approach is that we cannot conduct out-of-sample policy counterfactuals.

We then quantify the framework. 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. Evicted tenants have lower earnings realizations in the quarters around their eviction filing than tenants not facing eviction who had similar pre-period earnings. Combined with an assumption about risk aversion, these different earnings distributions over time determine RTC's insurance value. For other inputs to the framework, we use our most conservative estimate of RTC's price effect 2 years after introduction, together with 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. Our preferred specification estimates that RTC decreases tenant welfare by the equivalent of nearly \$12 per month per tenant household. This is true despite insurance value from protections, which adds 30 percent to the court benefits under our baseline assumption about risk aversion. RTC's benefits in eviction court would need to be more than twice as large, the price effects less than half as large, or the insurance value more than five times as large as we estimate for us to conclude that tenants were on average ex-ante better off due to RTC.

Overall, our findings suggest that RTC generated significant costs to landlords, some of which 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. We find broadly similar effects on court outcomes and listed rent prices studying the recent RTC rollout in Connecticut, which are provided in Appendix F, suggesting equilibrium responses may also be important in other contexts.

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 eviction-prevention 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.[4]

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 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

[4]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).

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.[5] 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 (2024) 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 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, 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. Similar to Finkelstein

[5]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).

et al. (2019)'s welfare analysis of in-kind transfers in healthcare, we value the RTC-induced transfer of housing services by estimating how it relaxes tenant budget constraints across states of the world with higher and lower marginal utilities of income.

2 Policy background, conceptual framework, and data

2.1 Right-to-counsel policies: background

Low-income households 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.[6] 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) surveys the literature on legal representation in eviction court and finds that, in the studies available at that time, 0-20 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.[7] New York City was the first city to implement such a policy, beginning in 2017 under Local Law 136. Since then, five states, two counties, and seventeen cities have introduced RTC programs (NCCRC, 2024). As of 2024,

[6]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.

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

similar policies are under consideration or are being piloted in several cities, including Los Angeles, Boston, and Chicago.

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. We emphasize that 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 simply on whether the marginal tenant values protections more or less than inframarginal tenants, and 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 do not change. Figure 1C illustrates the intermediate case in which supply and demand shift by equal amounts – this would be the case if, for example, the policy required a direct transfer from landlords to tenants. 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.

Rather than trying to directly estimate housing supply and demand curves as well as the shifts induced by RTC, which jointly determine the welfare impact of the policy, Section 4 develops an alternative approach to evaluating the impact on tenant welfare which accommodates all of these possibilities.

2.3 New York City's Universal Access to Counsel policy

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 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 look different 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.

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

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

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 tenants could evict them. 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 has 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.

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.

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

[10]For example, one of the most commonly used publicly collected data sources on rent is the American Community Survey, but at the ZIP code level and is only available in aggregate and averaged over a five-year window. In addition, it includes long-term renters, whose rents may not reflect current market prices.

2.4.2 Parcel data, building permits, and habitability complaints

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 to 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 data set 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 habitability complaints data to the parcel-level panel dataset. The Department of Housing Preservation and Development (HPD) records complaints made by the public online, via 311, or at Code Enforcement Borough Offices. 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." We then use the merged data to study if RTC affected complaints that could be consistent with livability. 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 historical records on benefits receipt of SNAP (food stamps), Medicaid, and cash assistance (including federal and local sources) capturing more than 2 million unique 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 contains 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 10 percent random sample of benefits recipients *not* linked to an 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. NSYDOL 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 CPI for New York City 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 (age, gender, race, etc.) 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, 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.

We supplement these data with 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 living in RTC-treated ZIP codes who are not in public housing, 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; this distribution is an input to our welfare quantification.

2.4.4 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 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 living in NYC between 2014 and 2020. 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., 2023). 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 data available prior to the move to NYC, to study how RTC affects migration into and out of treated ZIP codes.

We make the following sample restrictions. First, we exclude observations where both the previous and subsequent addresses fall within a relevant border region (i.e., ZIP codes that are either treated by RTC or adjacent to RTC-treated ZIP codes). We drop observations in ZIP codes for which the ACS data on median income is missing. We describe additional sample restrictions in Section 3.1.

3 Landlord responses to RTC

3.1 Research design

NYC's right-to-counsel program was rolled out by ZIP code over time, where ZIP codes were not chosen randomly.[11] This feature suggests studying the program's impacts using a difference-in-difference (DiD) approach, comparing treated areas to untreated areas, since DiD can accommodate unobserved time-invariant differences between treated and untreated locations. DiD recovers the 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).

However, it is unlikely that the parallel trends assumption holds across all ZIP codes in New York. For example, some neighborhoods may be gentrifying, while others are not. 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. Another advantage of this approach is that we can continue to forgo the assumption that units on each side of the border have the same unobservable time-invariant characteristics.

[11] The policymakers who designed the program selected equal numbers of ZIP codes from each borough, prioritizing those with high eviction rates. Since a minority of ZIP codes were treated, many untreated ZIP codes had similarly high eviction rates. Indeed, as can be seen in Table 1, untreated ZIP codes neighboring the treated ones look observably similar to the treated ZIP codes.

In our setting, the no-spillovers assumption implies that the treated units do not affect outcomes, such as rent prices, in the control group. As discussed in Section 2.2, the RTC policy likely shifts supply back and demand out in treated neighborhoods. Assuming nearby units in untreated neighborhoods are substitutes, the sign of potential spillovers on rent prices is ambiguous. Any decrease in supply in the treated neighborhoods could increase demand in control neighborhoods, while any increase in demand in treated neighborhoods could decrease demand in control neighborhoods.[12] To address concerns about spillovers, we include regression specifications that exclude control units within 250 meters of the ZIP border. We additionally consider specifications that increase the caliper, including additional units further from the border, which are likely less substitutable.

Finally, while many of our outcome variables are observed for every relevant unit and time period, this is not true for the listings data that we use for our rent price analysis. Listed rent prices are observed only when a unit is listed on the StreetEasy platform. Therefore, an additional concern for our rent price analysis is that RTC may affect *which* units are listed. 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, it is possible that we inadequately control for, e.g., unobserved quality of the unit. We test for RTC-driven differential selection onto the platform based on the observed characteristics of listed units in Section 3.2.

Since we have multiple treated ZIP codes, there are many potential borders between ZIP codes on which to run our calipered DiD analysis. Our strategy is to estimate these effects jointly and recover a single estimate, which, under the assumptions discussed above, will be a weighted average of treatment effects for treated housing units. To implement this strategy, we construct a stacked dataset. First, we build a dataset of ZIP code borders, where each pair of adjacent ZIP codes corresponds to a border $b = (z, z')$. Second, we determine when each side of the border was treated using the RTC rollout dates from Ellen et al. (2021). We drop border pairs where z and z' were never treated or were both treated in the same year. We also exclude border pairs where the treated side was treated in or after 2019, because we do not have enough listings created between the rollout date and the COVID-19 pandemic. In borders where one side was treated one year after the other (2017 and 2018, or 2018 and 2019), we exclude all listings created after both sides were treated, which addresses potential bias from comparing the late-treated ZIP listings to the early-treated ones (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021).[13]

[12]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 of a partial roll-out).

[13]For example, consider the ZIP codes mapped in Appendix Figure B.2. The border pair (11216, 11221) is excluded from the analysis sample as they are treated the same year, but the border pair (11216, 11206) is

Next, we subset our stacked dataset to the rental units within a certain distance of each relevant border pair. Our preferred specification sets a caliper of 1000 meters. We also exclude border pairs where the minimum distance from the border to the closest listing is greater than 300 meters on either side of the border. Manual inspection confirms that this approach excludes all ZIP code borders with large natural barriers, e.g., train tracks, cemeteries, highways, or large parks. 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.

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. Using this notation, the regression equation we estimate is

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

We also estimate a more flexible version of (1) which allows β and θ to vary across border pairs:

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

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}). 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.

3.2 Impact on listed rent prices

We first use calipered DiD regressions described above to estimate the impacts of right to counsel on listed rent prices one and two years after its introduction.

Table 2 reports the results. In the first year after treatment, we estimate that treatment raised rents by 6.5 to 17.5 dollars, depending on the specification. For the baseline calipered DiD, we estimate an increase of 12.5 dollars (p.val < 0.05) and, with the 250m donut, this increases to 17.5 (p.val < 0.01). Using our more flexible DiD specification, the estimates

included. Similarly, we exclude the second year of data for border pair (11385, 11207), because both sides of the border were treated by 2019, but include both post years for border pair (11221, 11207).

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

are smaller, less precisely estimated, and not statistically significant, with values of 6.5 dollars for the calipered DiD and 8.0 dollars with the 250m donut. In the second year, the results are larger, with increases in rent ranging from 22 to 38 dollars, which are 0.9 to 1.6 percent increases relative to the pre-period mean of $2,362. All four estimates are statistically significant at the 0.05 or 0.01 level. Estimates are six to nine dollars larger in the baseline calipered DiD specification than in the flexible DiD specification. Figure 3 plots the estimates for both year 1 and 2, as well as for years -1 and -2. We find no evidence of differential pre-trends in posted rent prices for treatment and control neighborhoods.

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. We plot the year 2 estimates on the y-axis, with the treated groups' baseline eviction filing rates on the x-axis. We then fit a line through the points, weighting by the number of observations in each border-specific estimate. We see that, on average, the estimated impact on prices is larger for border pairs where the treated ZIP code had higher baseline eviction rates. This is consistent with larger increases in rent in places where landlords and tenants were likely more affected by the policy.

Robustness. Our rent results are robust to our choice of specification and sample restrictions. Appendix Table C.1 shows the estimated impacts on rent are similar when considering log rent. The results suggest a half a percentage point increase in rent in year 1, which increases to a one or one-and-a-half percentage point increase in year 2.

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. Estimates are somewhat more precise for larger bandwidths and less precise for the 500 meter bandwidth.

Appendix Table C.3 shows that results are robust to the choice of sample. The table compares estimates from our analysis sample to estimates from (i) the full sample (i.e., including apartments with central AC, gyms, door persons, 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, to account for potential anticipation effects or time taken to scale up.

[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 NYC guidelines.

When using the full sample, estimates are somewhat smaller, as we would expect if we believe units with amenities such as pools, gyms, and door persons 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), resulting in one estimate being statistically significant at the 0.1 rather than 0.05 level. 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.

Appendix Table C.4 shows that estimates are largely robust when considering alternative control groups. The table 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.

Another threat to our design would be if treatment changed the characteristics of units listed on StreetEasy. To test this, 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.[16] 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. None of the estimates are statistically significant, and the point estimates are small and negative, which would work against the positive increases in rent we find above.

Lastly, Appendix 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 reweigh our data to account for potential compositional changes in the sample over time.

3.3 Impact on other margins of adjustment

The previous section found that landlords respond to RTC by raising rents. Landlords could also pursue other margins of adjustment, such as seeking alternative uses for their properties or screening potential tenants more carefully. In this section we consider these other margins.

[16] 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.

3.3.1 Quantity responses of rental housing supply

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). Some quantity measures, including vacancy durations and the number of available rental units, are difficult to measure in our setting, especially at high frequency with geographic granularity. In this section, we focus on the margins of quantity we can measure more accurately: condo conversions and building permits that could indicate the re-purposing of existing rental units. We emphasize that quantity responses are not necessary inputs to our welfare analysis; our estimated rent changes are sufficient for understanding the welfare effects of any related changes in quantity. Nonetheless, quantity responses illustrate one of the mechanisms driving the estimated price increases.

We measure quantity outcomes in 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. Our sample spans 2015-2019. We estimate border-level calipered difference-in-differences specifications, restricting the sample to parcels that include housing within 1000m of the ZIP code border. 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:

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

where i now indexes parcels rather than listings.

The top panel of Table 3 reports estimates for year 1, while the bottom panel reports estimates for year 2. The first column reports results for the number of condos per unit. We find a small but statistically significant increase in the number of condominiums, suggesting that some units may be moved out of the rental market. While the size of the changes are relatively small, they represent a 2-3 percent increase in condo units by the second year and are statistically significant at the 0.01 level.

The last three columns of Table 3 report how RTC affects large construction permits that would be consistent with repurposing rental units for other uses. 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 two. 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 demolition permits in the second year.

Overall, we find some evidence that RTC modestly reduced rental housing supply along the margins we can best measure.

3.3.2 Impacts on tenant composition

RTC may also change the composition of tenants in treated neighborhoods. In particular, as a response to RTC, property owners may adjust their screening behaviors in an attempt to rent to tenants who are at lower risk of falling behind on rent. Any screening responses would be important to account for in our welfare framework, and would also shape our understanding of supply responses to eviction-prevention policy more broadly, as stricter screening would suggest supply responses may be borne most by the ex-ante riskiest tenants. While screening responses are difficult to measure, we proxy for changes in screening by testing for changes in the characteristics of newly moved-in tenants.

We use the merged Infutor-Experian data introduced in subsection 2.4.4 to analyze whether RTC affects the characteristics of in-migrants to RTC-treated ZIP codes relative to neighboring ZIP codes, in a calipered difference-in-differences specification analogous to equation (1) with various in-migrant characteristics as the dependent variable.

We report results in Table 5. In column (1), we can reject that in-migrants' credit scores increase by more than 3.3 points or decrease by more than 8.7 points (on a scale from 300 to 850) in response to RTC with 95% confidence. Columns (3) through (5) likewise show precisely estimated zeros for three credit record variables that, based on conversations with the tenant screening industry, we understand are frequently used in tenant screening. In column (2), we use the Infutor migration data and the 5-year ACS to test whether in-migrants migrate from census tracts with higher median household incomes in response to RTC, and we likewise find no effect. All estimated effects in Table 5 are relatively precise zeroes, and if anything, the coefficient signs are generally opposite of what would be predicted by tighter landlord screening in response to RTC.[17]

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

[17] 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.

that screening does not appear to intensify with the introduction of RTC is consistent with landlords already using the information typically available to them to the best of their ability.

Changes to tenant composition could also reflect demand-driven changes in tenant sorting in response to RTC. Overall, the evidence in this subsection suggests that both the screening response and the sorting response to RTC are limited. In Section 5.3, we discuss the knife-edge case where sorting and screening responses offset each other to generate these null results on observable tenant composition, but still lead to an unobservably higher-eviction risk tenant pool in treated ZIP codes.

3.3.3 Impacts on upkeep and habitability

RTC could also change landlords' willingness to maintain units. On one hand, if renting becomes less profitable overall, RTC could cause landlords to reduce investment and defer maintenance. 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, could bias welfare estimates.

Table 4 tests this hypothesis using the parcel-level DiD specification from equation (3). The first column reports results for the number of habitability violations per unit, and the second column reports results for the number of rent-impairing habitability violations per unit. Overall, we find some evidence that habitability violations increase, with small statistically significant coefficients on rent-impairing violations in the first year, and statistically significant or marginally significant results for both outcomes in the second year. Taken at face value, these results suggest that treatment reduced habitability. However, an alternative mechanism could be that treated tenants were more likely to report habitability violations, which could imply improvements in tenant welfare. Overall, we interpret these results as suggestive though inconclusive evidence that habitability may have decreased. Since we do not account for this channel in our welfare analysis below, our estimated welfare impacts may be biased upwards.[18]

[18] Appendix C.1 provides additional evidence of parallel pre-trends for the various outcomes considered in this section. The pre-trends in these figures largely support the parallel trend assumptions. One exception is that we find a reduction in condos in -1 (but not -2).

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 raises 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.[19] 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. Renters make an ex-ante choice of how much housing to consume anticipating income risk and the option to default; they then choose whether and when to default after income uncertainty is resolved. RTC increases the amount of time a renter may remain in their unit after defaulting and reduces the probability of a negative court outcome. Section 4.2 derives an expression for the marginal change in tenant welfare due to increased tenant protections. If renters make optimal housing and default choices, RTC's welfare impact depends on three key quantities: the change in rent prices, the value of additional time in the unit and improved court outcomes for evicted renters, and the insurance value of improved court outcomes. We then show that this impact can be expressed in terms of observable quantities – most importantly, the eviction filing rate, the impact of RTC on rent prices, and the difference in income changes for evicted and non-evicted renters – and a risk aversion parameter. Section 4.3 explains the intuition behind our approach and discusses its advantages and limitations. Section 5 then implements the formula using eviction court records linked to administrative and survey data.

4.1 Model setup

We consider a set of ex-ante identical renter households. 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 $\lim_{c\to 0} u'(c) = \infty$.[20]

A rental contract specifies a per-period rent $R(h,\tau)$ in exchange for quantity of housing h,

[19] See Chetty (2009) for a review of this "sufficient statistics" approach.

[20] 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).

with τ denoting the strength of eviction protections. In what follows, we suppress dependence of R and other variables on h and τ where convenient. After choosing a housing quantity, the renter's income y is realized and they choose the share of months of rent $f \in [0,1]$ to pay. 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 f months they pay rent, plus the additional time τ required to complete the eviction process. An evicted renter also incurs a utility cost C from having an eviction case filed against them, which does not depend on the policy.[21] 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.

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 $p_e(\tau)$ of a negative eviction judgment, which may have an additional impact on a tenant's income and housing circumstances (Collinson et al., 2024).[22] We model the impact of a negative judgment as an additional drop y_e in income and an increase h_e in time homeless. This allows legal representation to directly reduce the costs of the eviction process by reducing the likelihood of a negative outcome in court. We assume tenants' housing and default choices correctly anticipate the potential costs from the eviction process given the policy environment.[23]

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

$$\begin{aligned} \max_{f\in[0,1]} & u(y - fR) + \min\{\tau + f, 1\}[v(h) - v(0)] + v(0) \\ & - 1\{f < 1\}[C + \underbrace{p_e(\tau)h_e[v(h) - v(0)] + p_e(\tau)(u(y - fR) - u(y - fR - y_e))}_{\text{Impact of negative judgment on time housed and income}}]. \end{aligned} \tag{4}$$

The first line in 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 a negative judgment.

Because utility from the numeraire $u(\cdot)$ is concave while utility from time spent in the unit is linear in the amount paid up to $f = 1 - \tau$, the share of rent paid $f(y; h, \tau)$ will be weakly

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

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

[23]We write p_e 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.

monotonically increasing in income y. There are three possible cases for the household's solution to equation (4).

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

$$u(y) + (\tau - p_e h_e) v(h) + v(0) - C - p_e [u(y) - u(y - y_e)] \, .$$

A necessary condition for this to be true is $[p_e(\tau) u'(y - y_e) + (1 - p_e(\tau)) u'(y)] R(h, \tau) \geq v(h) - v(0)$. This means the renter would rather spend rent on numeraire consumption 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:** $f \in (0, 1 - \tau)$[24] and the household receives utility

$$p_e u(y - fR - y_e) + (1 - p_e) u(y - fR) + (\tau + f - p_e h_e)[v(h) - v(0)] + v(0) - C \, .$$

A necessary condition for optimal partial default is that the household is indifferent between spending money on rent and the numeraire:

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

Note that the tenant's expected marginal utility of numeraire consumption is a weighted average over court outcomes due to the effect of a negative judgment 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 highest income realization for which partial default is optimal.

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

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

Housing Choice: Anticipating their uncertain income and optimal default choice, a renter

[24] 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. Relaxing this assumption would not impact the welfare formula, but it would affect our empirical implementation.

chooses housing quantity h to maximize their expected utility. The value of this solution is

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

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 household 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

This section derives an expression for the impact of increased tenant protections on tenant welfare, and then derives an empirical analogue whose components we estimate in Section 5.1.

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

$$
\begin{aligned}
\frac{dW(\tau)}{d\tau} \;&=\; F(\hat{y}) \left([v(h) - v(0)] \left(1 - \frac{dp_e}{d\tau} h_e\right) - \frac{dp_e}{d\tau} \mathbb{E}_{y<\hat{y}}[\Delta_{y_e} u(c)] \right) \\
& - \frac{dR}{d\tau} \mathbb{E}_y[f u'(c)] \, , \qquad (7)
\end{aligned}
$$

where $F(\hat{y})$ is the share of renters who are evicted, $\frac{dR}{d\tau}$ is the rate at which equilibrium rents change due to the policy, $\frac{dp_e}{d\tau}$ is the rate of reduction in the probability of a negative judgment,

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

is the mean numeraire utility loss due to income lost by tenants who receive a negative judgment, and

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

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

See Appendix E 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 quantities that are directly measurable. This expression will allow us to quantify the welfare change given an assumption about the degree of households' risk aversion over numeraire consumption.

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

The more difficult quantities to measure are the values of housing $v(h) - v(0)$ and the marginal utilities of numeraire consumption $u'(c)$ 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

$$u'(c) \equiv p_e u'(y - fR - y_e) + (1 - p_e)u'(y - fR) \tag{8}$$

as the expected marginal utility of consumption for a tenant facing eviction. It will also be useful to rewrite the difference in consumption utility due to an eviction judgment as

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

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_e and $p_e(\tau)$. We can now write

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

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 households who partially default, and on households who pay the full rent. Rearranging equation (4), for partial defaulters

$$\frac{1}{R(h, \tau)} = \frac{p_e u'(y - fR - y_e) + (1 - p_e)u'(y - fR)}{v(h) - v(0)} \qquad y \in [y_0, \hat{y}] \, .$$

If all evicted tenants pay some rent, the first highlighted term in equation (9) is simply $\frac{1}{R}$.The remaining challenge is calculating the MRS for households 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 household with realized income y to the average MU of an evicted household, and obtain the ratio $\frac{u'(c)}{v(h)-v(0)}$ for the non-evicted household:

$$\begin{aligned}
\underbrace{\frac{u'(y-R)}{v(h)-v(0)}}_{\text{MRS for non-evicted household}} &= \frac{u'(y-R)}{\mathbb{E}[u'(c) \mid y \leq \hat{y}]} \frac{\mathbb{E}[u'(c) \mid y \leq \hat{y}]}{v(h)-v(0)} \\
&= \frac{u'(y-R)}{\mathbb{E}[u'(c) \mid y \leq \hat{y}]} \frac{1}{R}\,.
\end{aligned}$$

This allows us to rewrite equation (9) as

$$\frac{dW(\tau)}{d\tau} \propto F(\hat{y}) \left[1 - \frac{dp_e}{d\tau}\left(h_e + \frac{y_e}{R}\bar{\theta}\right)\right] - \frac{dR}{d\tau}\frac{1}{R}\left[\bar{f}_E F(\hat{y}) + (1-F(\hat{y}))\frac{\mathbb{E}[u'(y-R) \mid y > \hat{y}]}{\mathbb{E}[u'(c) \mid y < \hat{y}]}\right] \tag{10}$$

where $\bar{f}_E$ is the average share of rent paid by evicted tenants, and $\bar{\theta} \equiv E_{y<\hat{y}}[\theta]$ is the mean value of θ among evicted tenants.

This formula provides another perspective on the benefits and costs of eviction protection for tenants. This first term 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 judgment against them (and of the associated losses in numeraire and housing consumption). The second term – the marginal cost of stronger protections – is proportional to the change in rent $\frac{dR}{d\tau}\frac{1}{R}$ 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 households who default tend to have lower income realizations than households who do not default, the ratio $\frac{\mathbb{E}[u'(y-R)|y>\hat{y}]}{\mathbb{E}[u'(c)|y<\hat{y}]}$ 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.

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 its 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 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.

Perhaps most importantly, for the welfare framework to be valid, we still need to specify and measure all of the margins of adjustment for landlords that matter to tenants.[25] In addition to adjusting rent prices, landlords could in principle respond to stronger tenant protections by adjusting 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 Section 3, we focus on price – where there is strong evidence of a response – rather than these other margins, where we do not find evidence of adjustments.

5 Empirical implementation and results

In this section, we develop an empirical implementation of expression (10) to quantify the welfare effects of RTC. Section 5.1 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.

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

5.1 Quantification of each component

Expression (10) characterizes the welfare effects of RTC for a set of ex-ante identical renters. To take this expression to the data, we allow heterogeneity across renters in terms of ex-ante earnings Υ and geography g. Appendix E.2 presents an expanded version of expression (10) introducing this heterogeneity. We then quantify expression (10) as follows.

Insurance Term. We start with the ratio of marginal utilities in expression (10). After introducing heterogeneity in Υ and g, and after using expression (8) to re-expand $u'(c)$ in the denominator, we can write this ratio as

$$\frac{\mathbb{E}[u'(y-R) \mid e=0, \Upsilon, g]}{\mathbb{E}[p_e u'(y-fR-y_e)+(1-p_e)u'(y-fR) \mid e=1, \Upsilon, g]}. \tag{12}$$

Quantifying this ratio requires estimating a joint distribution of rents R, mid-lease income realizations y, the share f of rent paid by evicted tenants, and eviction filings e. A challenge in estimating this joint distribution is that to our knowledge, there is no dataset in which these four variables are simultaneously observed. We first assume the distribution $G(R, y, f, e \mid g, \Upsilon)$ is the same for all tenants conditional on g and Υ at the time of lease signing, prior to the realization of future earnings and eviction outcomes. We then estimate this joint distribution by estimating several conditional distributions across three different datasets. For the datasets in which we do not observe both g and Υ, we condition on the one we observe and assume independence with respect to the other.

We start with our linked earnings and evictions data supplemented with statistics from the NYCHVS. These datasets are described in Section 2.4.3. We use the NYCHVS to estimate the distribution of rent R conditional on ZIP code g and earnings Υ at the time of lease signing (i.e., $G_1(R \mid \Upsilon, g)$). In practice, we estimate a single rent price for each (Υ, g) as the observed mean and assume these rents are independent of earnings dynamics after conditioning on Υ and g. This is consistent with all tenants facing the same conditional earnings risk and therefore being unable to select into R based on expected future earnings.

We then use the linked administrative data on earnings and eviction filings to estimate the distribution of mid-lease earnings y conditional on Υ and eviction filings e (i.e., $G_2(y \mid \Upsilon, e)$). We are not able to condition this distribution on g,[26] so we assume $G_2(y \mid \Upsilon, e) = G_2(y \mid \Upsilon, g, e)$; in other words, we assume earnings dynamics conditional on ex-ante earnings are the same across RTC-treated ZIP codes. Finally, we use claim amounts from the linked court records and rents from the NYCHVS to estimate the share of rent paid by ultimately evicted tenants, conditional on ex-ante and mid-lease earnings (i.e., $G_3(f \mid \Upsilon, e, y)$). We likewise

[26]While we observe earnings dynamic linked with recent address for the evicted sample, we do not reliably observe detailed geography for the non-evicted sample.

assume f is single-valued at its sample mean and is conditionally independent of g. Finally, we estimate eviction rates $F(\hat{y}) = G_4(e \mid g)$ as an eviction rate in each RTC-treated ZIP code, using public OCA court data on eviction filing counts divided by counts of renter households in the 5-year American Community Survey (ACS). It is not possible in these data to estimate how eviction rates vary by ex-ante earnings, so we assume $G_4(e \mid g) = G_4(e \mid \Upsilon, g)$. All of these quantities are estimated on pre-RTC data. Appendix E.2 provides details on our procedure and measurement choices.

The conditional distribution $G_2(y \mid \Upsilon, e)$ of mid-lease earnings describes how changes in earnings correlate with eviction, the key moment in the data that determines the insurance value of eviction protection. Figure 5 presents quarterly earnings dynamics for evicted and non-evicted tenants. While non-evicted tenants' average earnings are broadly stable over time, evicted tenants' average quarterly earnings fall by over \$2,000 between 6 quarters before eviction and the quarter when eviction is filed. In other words, eviction follows a nontrivial earnings loss on average. However, before the onset of this average earnings loss for evicted tenants, evicted and non-evicted tenants' earnings follow similar trends conditional on ex-ante earnings, consistent with our assumption that they face the same conditional distribution of earnings risk ex-ante.

Appendix Figure E.1 shows that these average differences mask considerable heterogeneity in income changes for both evicted and non-evicted tenants. While on average evicted tenants have substantially lower mid-lease earnings, we observe a large share of positive and negative income changes for both groups. The conditional distributions of mid-lease earnings $G_2(y \mid \Upsilon, e)$ that we estimate capture this wide dispersion in earnings changes.

Both the denominator and numerator of expression (12) require an assumption about the shape of utility from numeraire consumption $u(\cdot)$, as well as how our earnings measures translate into consumption. Our baseline specifications assume hand-to-mouth households that consume their income y minus the rent they pay. We assume a floor to non-housing consumption, representing other social insurance available for tenants with very low earnings (Deshpande, 2016), set to \$4,000 annually in our preferred specification. We assess our estimates' sensitivity to higher and lower values. We assume $u(\cdot)$ exhibits constant relative risk aversion (CRRA) and explore robustness to a range of risk aversion parameters. Recall that, by the envelope theorem argument in Proposition 1, we can leave preferences over housing consumption unspecified.

Other Terms. Other terms to be estimated or calibrated in expression (10) include the effects of RTC on rents, legal representation, and court outcomes. First, for rent prices, we use the year 2 price effect estimates reported in Column (2) of Table 2; our baseline specification uses the smallest point estimate, from the "flexible 250m donut" specification, of \$21.92. Second, for court outcomes, we use the "main" 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 a negative judgment by 32.1 percentage points. Third, 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, we calibrate the causal effects of an eviction judgment on tenant income and homelessness – which we model as impacting both numeraire and housing consumption – from external estimates in Collinson et al. (2024). We assume the estimated cumulative earnings reduction among NYC tenants ($y_e = \$3,200$) equals the drop in numeraire consumption due to an eviction judgment, and the cumulative increase in years in a homeless shelter ($h_e = 0.07$) equals the reduction in time housed. The next section assesses the sensitivity of our results to each of the above estimates.

We are unable to allow most of our quasi-experimental estimates (other than rent price effects) to depend on ex-ante earnings and geography. Estimates of heterogeneous effects at this level of detail are not available from Collinson et al. (2024) or Cassidy and Currie (2023). Heterogeneity in these effects could 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 judgment. We view credible estimates of these heterogeneous effects as a valuable direction for future work.

5.2 Results

Table 6 reports estimated welfare effects under a range of assumptions about the shape of $u(\cdot)$, which governs preferences over numeraire consumption, and its argument c. Each row corresponds to a minimum consumption level, and each column to a relative risk aversion parameter. Panel A assumes evicted renters consume the amount they do not pay ($(1-f)R$) as numeraire consumption, whereas Panel B assumes they consume their income minus the full rent R. In all cases, we estimate that RTC's effects on tenant welfare – or $\frac{dW(\tau)}{d\tau}$ in equation (10) – is negative, between -\$11.52 and -\$13.33 per month. Since we do not estimate $u(\cdot)$, it is reassuring that our results are not qualitatively sensitive to a reasonable range of assumptions about it. Under what we consider to be our baseline specification in Panel A, with risk aversion (γ) set to 2 and the non-housing consumption floor set to \$4,000 annually, RTC reduces tenant welfare by the equivalent of \$11.95 per month. Thus, the benefits in eviction court do not seem to outweigh the welfare cost of higher rents, even accounting for the insurance value of additional protection.

Figure 6 decomposes the tenant welfare impact of RTC under our baseline assumptions into three terms: (1) the ex-ante value of improved court outcomes if tenants were risk-neutral; (2) the insurance value of improved court outcomes because tenants are risk-averse; and (3) the ex-ante cost of higher rents. We estimate that the first term is \$7.06 per month, to which the insurance value adds \$2.08. Even for our smallest price effect point estimate (\$21.92), the cost of RTC due to higher rent prices is more than twice as large as the benefits in court, including insurance value.[27] The insurance value itself would need to be twice as large as (199 percent of) the risk-neutral court benefits, instead of our estimated 30 percent, to make RTC welfare-neutral. Similarly, the court benefits would need to be 131 percent larger. The rest of the section explores how our specification choices and modeling assumptions influence the welfare calculations through these three components.

We first consider the insurance value term. The net welfare impact is somewhat sensitive to the assumed risk aversion parameter and consumption floor because evicted tenants experience worse income realizations than ex-ante similar renters who are not evicted. A lower assumed consumption floor and a higher assumed degree of risk aversion usually generate smaller welfare losses. Appendix Table E.1 illustrates the contribution of this insurance channel to the welfare calculation by reporting the proportional change in the value of improved court outcomes relative to the cost of a \$1 increase in rents. A value of 1 implies that renters value consumption equally (on average) in the evicted and non-evicted states; a higher value means consumption is more valuable in the evicted state. In Panel A, the insurance channel adds between 12 and 37 percent to the value of RTC's benefits in court, and 30 percent at our preferred estimate. In Panel B, where evicted tenants pay as much rent as non-evicted tenants, the insurance value is even higher – between 19 and 59 percent – because tenants are not partially insured against income risk by the ability to default on rent.

Appendix Table E.2 explores sensitivity of our welfare conclusions to two additional inputs that are central to the welfare calculation: RTC's impacts on rent prices and on court outcomes. While our price effect estimates are qualitatively similar across a wide range of specifications, they do differ quantitatively and are subject to statistical uncertainty. The rows of Table E.2 repeat the welfare calculation using alternative rent effect estimates, which will impact the rent cost term in the welfare decomposition. Similarly, our IV estimates of the impact of RTC on legal representation may be lower than the long-run impact. This could occur if more tenants become aware of the program and challenges in the initial implementation, such as staffing issues, are addressed. The columns of Table E.2 vary RTC's impact on representation, holding fixed the causal effects of representation on court outcomes. This will proportionally change the court benefits and insurance value terms in the welfare decomposition. We assume risk aversion of $\gamma = 2$ and an annual consumption floor of \$4,000 in all reported estimates.

The results demonstrate that the impact of RTC on rent prices would need to be

[27] The rent cost is slightly lower than the rent price effect because of partial payment by tenants who default.

considerably lower, or the court benefits considerably larger, than we estimate to reverse the sign of our welfare calculation. If take-up increased by 25 percentage points, tenants are still worse off by -$2.67 per month using our lowest point estimate of the effect on rent prices. The next two rows repeat the welfare calculation using the year 2 point estimates from the first two specifications reported in Column (2) of Table 2. The final row of Table E.2 allows the rent effect to vary by the ZIP code's baseline eviction case rate; we fit a line to the border-pair specific year 2 rent effect estimates reported in Figure 4. This further increases in magnitude the negative welfare impact to -$33.20. Price increases are especially high in the ZIP codes where eviction is most common, which are also in lower-income neighborhoods. The third column of the table assumes RTC eventually has a 50 percentage point impact on take-up, twice as high as the 25pp effect in the second column, and four times as high as the point estimate used in the first column. This dramatically increases the welfare effects, which become small or positive. Overall, this analysis suggests that we would need to have underestimated the court benefits by more than half in order for RTC to be welfare-neutral for tenants.

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 rent prices to be more pronounced in the long run, which would lead our long-run welfare assessment 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 much 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 (2022) 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 if 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, and the data also bears this out (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 impacted the types 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 complaints 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. A different way in which a large-scale, sustained RTC program could benefit tenants in the long run, is if the presence of tenants advocates leads to pressure on the court system and judges to provide more transparent, tenant-friendly services.

A final consideration in interpreting our analysis is that a full welfare assessment of RTC would also consider impacts on landlords and taxpayers. 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.[28] Impacts on the government budget, and hence taxpayers, are also ambiguous. On one hand, paying for tenants' legal counsel is expensive. On the other hand, there may be cost savings from fewer evictions if they reduce the frequency of costly events such as emergency room visits or homeless shelter stays. We leave quantifying landlord and government budget impacts of RTC for future research.

[28]This would be true if RTC increased expected costs equally for all rental units of a given type. However, if it increased costs more for marginal units than for inframarginal units, it is possible for landlords to on average benefit from RTC even when quantity falls.

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 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.

The decrease in ex-ante tenant welfare is consistent with RTC generating substantial costs for landlords that are not directly 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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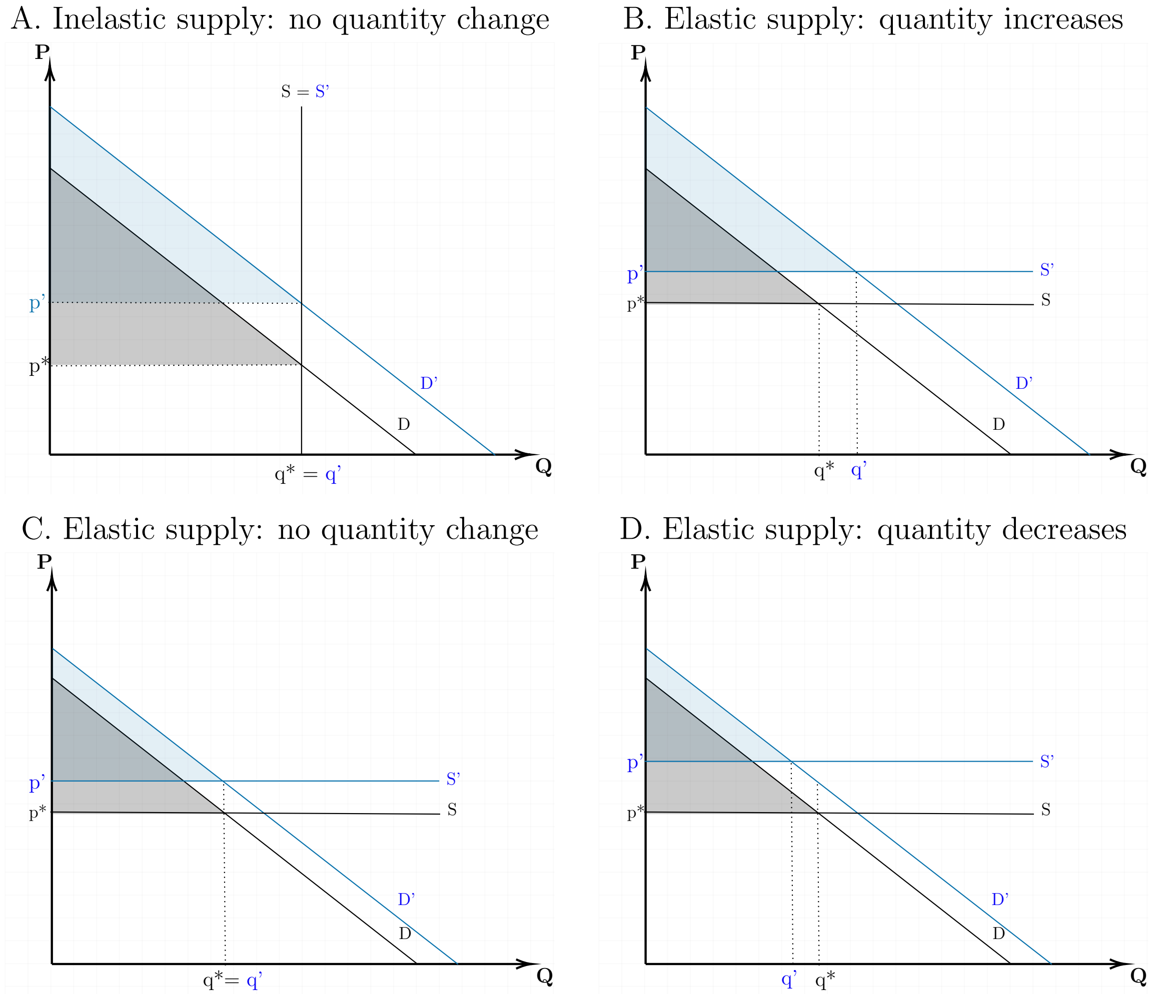
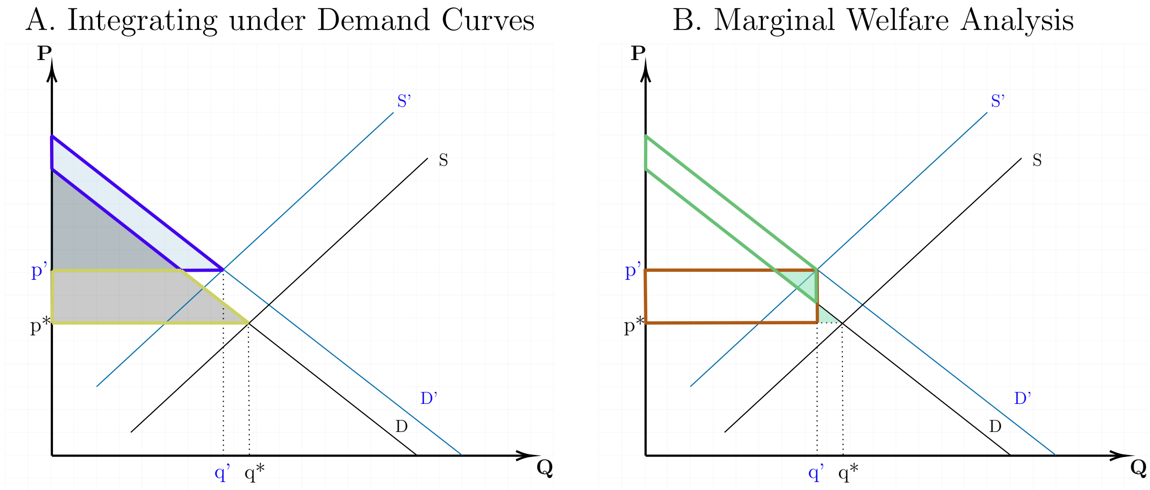
Figures and Tables

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



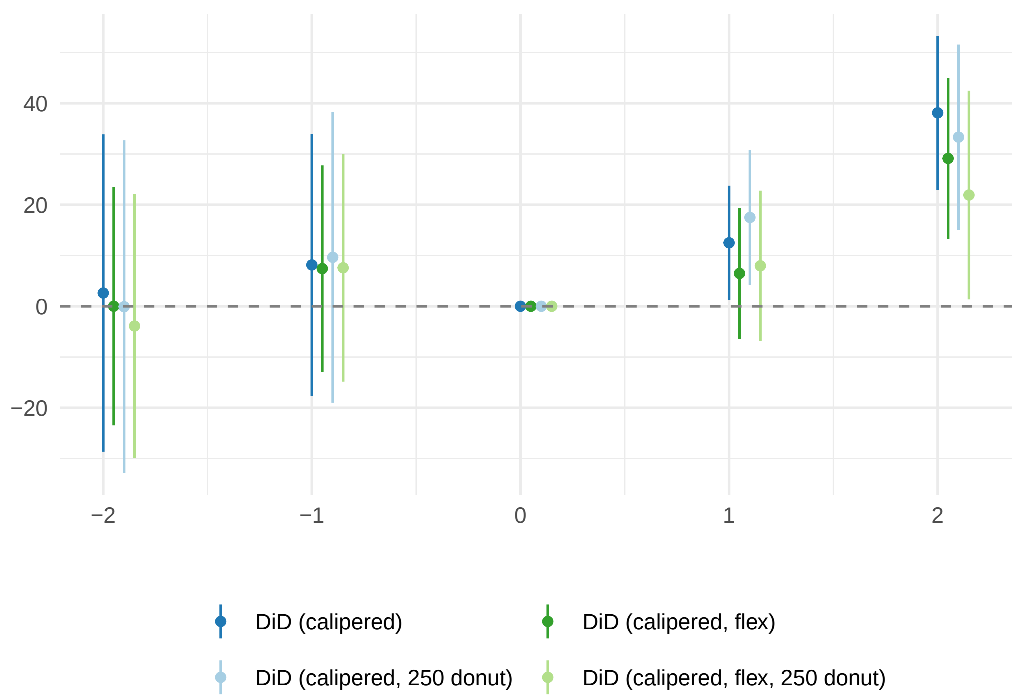
Note: This figure shows hypothetical changes in consumer welfare, price, and quantity, depending on the elasticity of supply and the size of the demand and supply shifts.

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



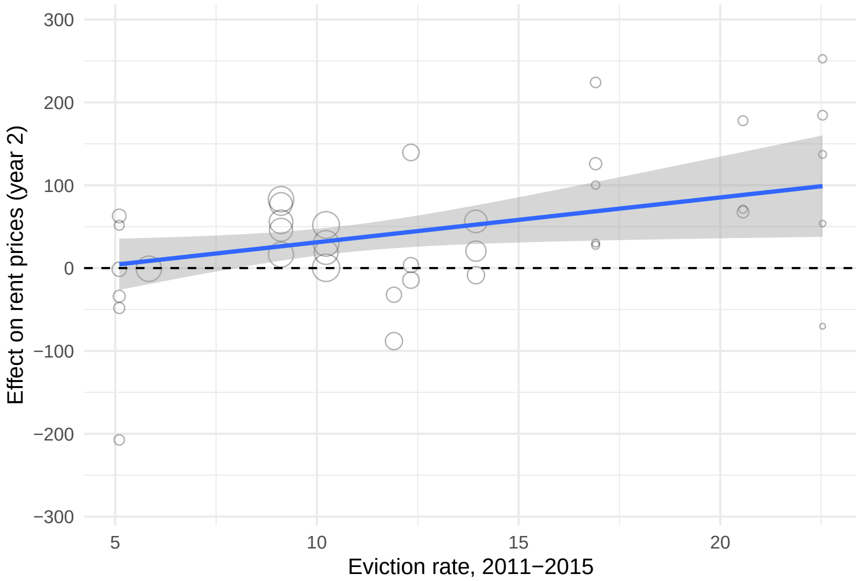
Note: This figure provides intuition on how we measure change in consumer surplus as discussed in Section 4.3.

Figure 3: Impact of RTC on Rent Prices



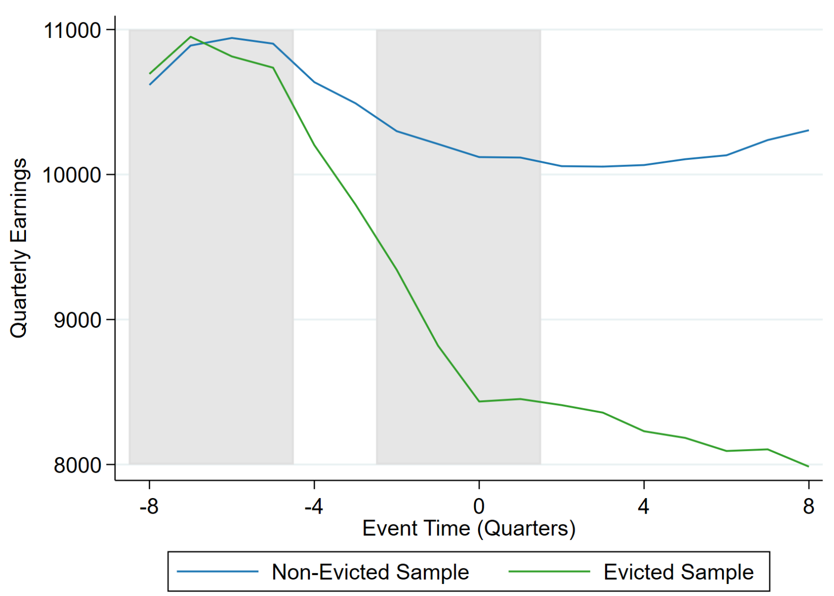
Note: This figure estimates the impact of RTC on listed rent prices (δ_t in equations 1 and 2) using StreetEasy data. "Year 1" compares listings created within 365 days of the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created 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. Section 2.4 and Appendix Section A describe the data in greater detail. Section 3.1 describes the difference-in-discontinuities approach in greater detail. All specifications use a bandwidth of 1000 meters. The bars correspond to the 95% confidence interval. The StreetEasy sample only includes "no amenities" listings, i.e. those without central AC and in buildings without 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 to account for potential spatial correlation. 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.

Figure 4: Rent effects by eviction filing rate



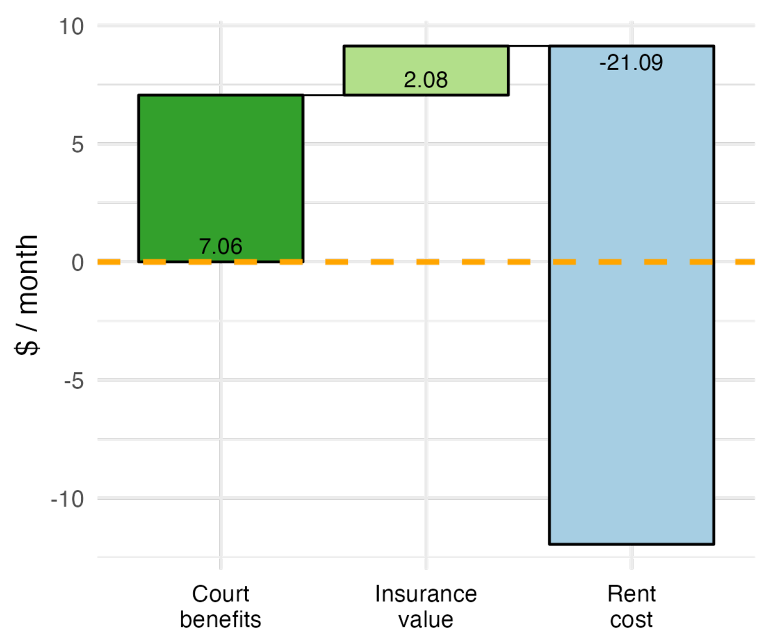
Note: This figure plots border-specific treatment estimates of the impact of RTC on listed rent prices (δ_{bt} in equation 1), given eviction filing rate on treated side. The coefficients correspond to the "calipered, flex" difference-in-discontinuities specification with a bandwidth of 1000 meters. 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 LOESS regression. We use StreetEasy data and "Year 2" listings to estimate the plotted coefficients. "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. The StreetEasy sample only includes "no amenities" listings, i.e. those without central AC and in buildings without 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 to account for potential spatial correlation. 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.

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



Note: This figure shows average quarterly earnings 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 data 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 mid-lease earnings used in the welfare analysis of Section 4.

Figure 6: Welfare Decomposition



Note: This figure decomposes the welfare impact of RTC into three components: (1) the benefits from improved court outcomes if tenants were risk-neutral, (2) the additional "insurance value" from improved court outcomes, and (3) the cost due to higher rent prices. This calculation assumes relative risk aversion of $\gamma = 2$, an annual consumption floor of $\$4,000$, and our preferred price effect point estimate of \$21.92.

Table 1: Characteristics of Treatment and Control Units

	Treated ZIP codes		Never-treated neighbors		All other ZIP codes	
No. households	72454.1	(21486.12)	57469.6	(25252.94)	27657.54	(25733.14)
Median household income	56823.1	(14598.31)	58494.5	(20601.07)	93691.58	(43294.27)
Share renters	78.1	(12.19)	70.2	(19.29)	59.50	(23.43)
Median rent	1381.7	(141.44)	1389.0	(244.37)	1877.70	(622.54)
Hispanic share	34.2	(20.97)	34.3	(21.22)	19.47	(15.38)
Black share	40.5	(23.78)	37.0	(25.66)	11.20	(17.55)
White share	28.1	(19.98)	30.9	(20.66)	55.32	(24.11)
Share non-citizens	16.9	(6.45)	16.0	(6.74)	14.30	(6.71)
Eviction rate	12.5	(4.81)	11.7	(4.88)	5.35	(4.06)
Nonpayment share of evictions	84.3	(11.20)	81.8	(16.18)	75.65	(18.14)

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 who are ever treated before 2020. "Never-treated neighbors" are ZIP codes adjacent to treated neighborhoods 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.

Table 2: Impact of RTC rollout on listed rents

	(1) Year 1 effect	(2) Year 2 effect
DiD (calipered)	12.509**	38.110***
	(5.739)	(7.736)
DiD (calipered, flex)	6.472	29.131***
	(6.602)	(8.094)
DiD (calipered, 250m donut)	17.497***	33.318***
	(6.771)	(9.308)
DiD (calipered, flex, 250m donut)	7.978	21.915**
	(7.553)	(10.489)
Observations	179578	120229
Pre-period mean	2361.702	2439.793

Note: This table estimates the impact of RTC on listed rent prices (δ_t in equations 1 and 2) using StreetEasy data. "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. 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. 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

		Permits		
	(1)	(2)	(3)	(4)
	No. condos per unit	Major alteration	New construction	Demolition
Panel A. Year 1				
DiD (calipered, parcel FE)	0.00015**	0.00074**	0.00038**	0.00016
	(0.00007)	(0.00034)	(0.00017)	(0.00016)
DiD (calipered, 250m donut, parcel FE)	0.00020***	0.00096**	0.00038**	0.00018
	(0.00007)	(0.00039)	(0.00019)	(0.00018)
Panel B. Year 2				
DiD (calipered, parcel FE)	0.00023***	0.00020	0.00033*	0.00072***
	(0.00008)	(0.00041)	(0.00020)	(0.00023)
DiD (calipered, 250m donut, parcel FE)	0.00028***	0.00040	0.00040*	0.00085***
	(0.00008)	(0.00038)	(0.00021)	(0.00025)
Observations	1473365	1465406	1465406	1465406
Pre-period mean	0.00800	0.00521	0.00031	0.00048

Note: This table estimates the impact of RTC on condominium conversions and building permits (δ_t in equation 3). 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. 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: Habitability: Reports of Violations

	(1)	(2)
	No. of HPD violations	No. of Rent-impairing HPD violations
Panel A. Year 1		
DiD (calipered, parcel FE)	0.04510	0.00850*
	(0.05809)	(0.00465)
DiD (calipered, 250m donut, parcel FE)	0.04131	0.00980**
	(0.05899)	(0.00475)
Panel B. Year 2		
DiD (calipered, parcel FE)	0.15700**	0.01237**
	(0.07696)	(0.00617)
DiD (calipered, 250m donut, parcel FE)	0.13327*	0.01066*
	(0.07851)	(0.00573)
Observations	1465406	1465406
Pre-period mean	2.07177	0.13961

Note: This table estimates the impact of RTC on reports of habitability violations (δ_t in equation 3). We restrict our analysis to parcels that include housing within 1000m of the ZIP code border. The dependent variable in column (1) is an indicator for whether the building had any violations recorded by the Department of Housing Preservation and Development (HPD). Column (2) replicates column (1) for 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." We then use the merged data to study if RTC affected complaints that could be consistent with livability. The dependent variables in columns (3) and (4) are, respectively, the number of violations and the number of rent-impairing violations recorded in that parcel. 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. 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 5: Tenant Screening: Evidence from Infutor & Experian

	(1) Credit Score	(2) Median Income (Previous Tract)	(3) Total Monthly Payment	(4) Number of Public Records	(5) Revolving Balance
Panel A. Year 1					
DiD (calipered)	-2.39	-421.35	48.97	0.00021	-190.07
	(3.20)	(607.38)	(98.19)	(0.00567)	(448.52)
DiD (calipered, 250m donut)	-3.37	116.61	91.38	0.00453	-104.34
	(4.42)	(844.03)	(124.80)	(0.00547)	(512.16)
Observations	20545	20545	20545	20545	20545
Pre-period mean	688.65	78100.89	635.97	0.02314	4891.13
Panel B. Year 2					
DiD (calipered)	-1.38	1164.50	60.02	-0.00656	365.69
	(4.73)	(1438.39)	(94.06)	(0.00557)	(364.43)
DiD (calipered, 250m donut)	-3.28	240.78	68.70	-0.00282	388.55
	(5.54)	(1838.25)	(149.30)	(0.00449)	(540.91)
Observations	17175	17175	17175	17175	17175
Pre-period mean	690.24	78801.84	633.01	0.02236	4664.76

Note: This table estimates the impact of RTC on tenant characteristics (δ_t in equation 1). We use Infutor data on all in-migrants to treated or control ZIP-border areas in New York City between January 2014 and December 2019, linked to Experian credit report data. We drop observations where previous and subsequent address are both in a relevant border region (either in a zip code or bordering a zip code with RTC rollout). 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 score for a given move. The median income of the previous tract is obtained from the five-year ACS (2015-2019) dataset. The vector of controls X_i from equation 1 is left empty for these specifications. 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

Table 6: Impact of RTC on Tenant Welfare

	Coef. of Relative Risk Aversion $\gamma = 1$	$\gamma = 2$	$\gamma = 3$	$\gamma = 5$
Panel A. Main Estimates				
Min c of \$2000	-12.40	-11.60	-11.52	-11.62
Min c of \$4000	-12.87	-11.95	-11.75	-11.81
Min c of \$8000	-13.33	-12.52	-12.17	-12.07
Panel B. Evicted Tenants Pay Full Rent				
Min c of \$2000	-12.44	-11.40	-11.13	-10.95
Min c of \$4000	-12.96	-11.81	-11.47	-11.28
Min c of \$8000	-13.50	-12.37	-11.83	-11.50

Note: Table reports estimated welfare impacts on tenants in \$/month. All numbers use the following inputs unless otherwise specified: the default consumption floor c is \$4,000; the rent effect is the "DiD (calipered, flex, 250m donut)" Year-2 estimate reported in column (2) of Table 2; RTC has a 25pp effect on counsel take-up; and tenants' consumption includes the amount they default on rent. Panel A varies the assumed consumption floor across rows. Panel B varies the rent effect estimate. Panel C varies RTC's impact on counsel take-up. Panel D also varies the consumption floor, but assumes that evicted tenants pay the full rent instead of consuming the amount they default. See Section 4.2 for details on the welfare formula and construction of inputs.

ONLINE APPENDIX

A Additional details on data construction

A.1 Details on the policy roll-out.

NYC's right-to-counsel program, officially named "Universal Access to Counsel", was signed into legislation 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).[29] 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. 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 different 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 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.

[29]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).

Cleaning and construction of analysis sample. We further process the StreetEasy data to construct our main analysis dataset. We restrict our sample to listings in Manhattan, Brooklyn, Bronx and Queens. We then impute missing values for unit characteristics. We create an indicator for whether 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.

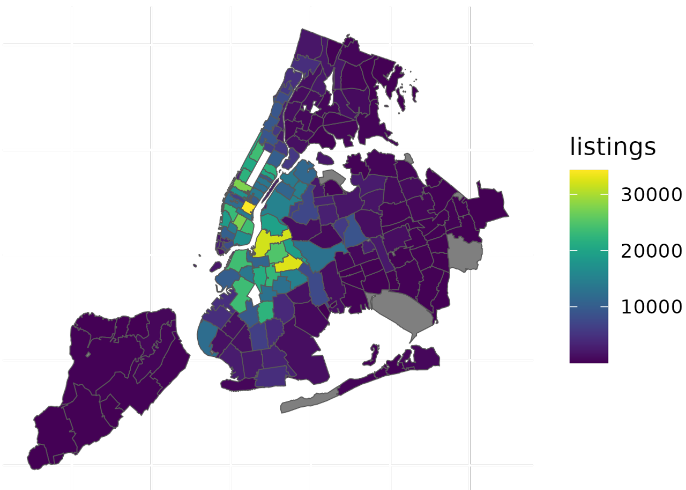
Table A.1: Summary Statistics

	Full	Analysis
Years	2013-2020	2014-2019
Average rent	3215.9	2386.7
	(2492.6)	(805.2)
% Brooklyn	34.8	60.6
% Bronx	1.9	5.8
% Manhattan	50.9	26.3
% Queens	12.3	7.4
% Staten Island	0.1	0.0
Avg. no. bedrooms	1.5	1.9
	(1.1)	(1.1)
Avg. building age	39.2	26.4
Avg. no. units in building	121.0	26.3
	(544.2)	(51.0)
% rent stabilized	23.4	47.3
Avg. assessed value per unit	93347.8	24192.1
Number of listings	1593163	390105

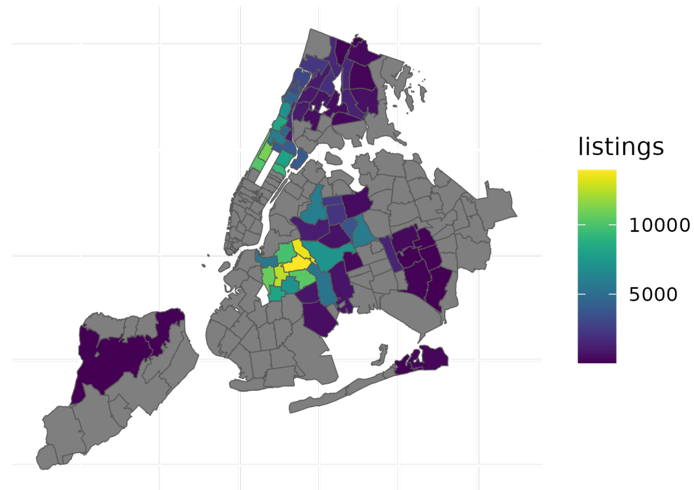
Notes: This table summarizes the StreetEasy rental listings dataset used in our analysis. Section 2.4 and Appendix Section A describe the data in greater detail.

Figure A.1: StreetEasy Coverage

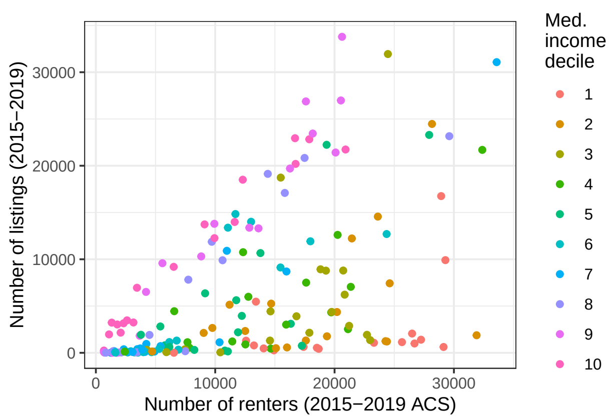
A. Location of StreetEasy listings



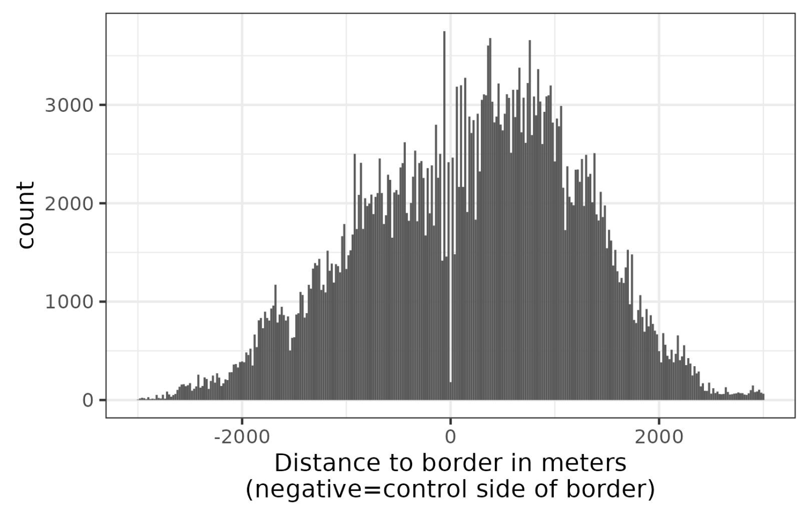
B. Listings in StreetEasy analysis dataset



C. Number of listings relative to number of renters in ACS



D. Coverage Near Boundaries



Note: This figure shows the coverage of the StreetEasy data. Panel A maps StreetEasy listings from 2015 to 2019. Panel B maps the number of "no amenities" StreetEasy listings by ZIP code, for the 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's (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 Complaints

Source and coverage. Our complaints dataset comes from the Department of Housing Preservation and Development (HPD), who records complaints 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).

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

Source and coverage. We rely on consumer reference data from Infutor Data Solutions which covers 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, as well as 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.

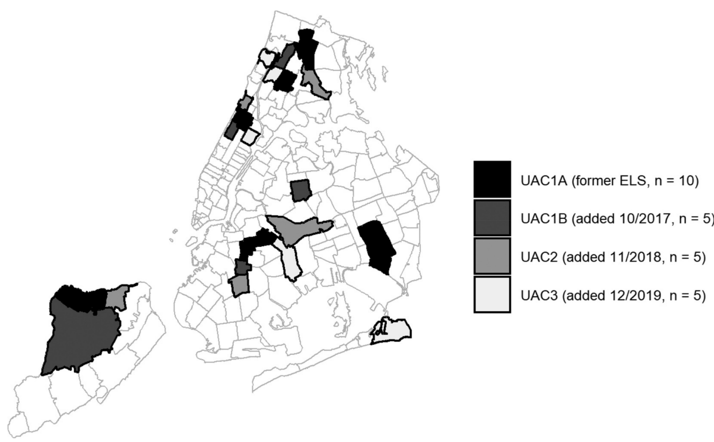
Cleaning and construction of analysis sample. We use a cleaning approach that mirrors Phillips (2020). We drop records associated with Post Office Boxes, records for individuals who are recorded as deceased before the sample period, and records where Infutor assigns the same individual to multiple address with identical "effective dates." To construct the analytical sample, we restrict to individuals living in New York City at some point between January 2014-and December 2020. For the purpose of linking to credit records, we send to Experian the person-address pairs for all NYC addresses during the sample period, which comprises 16.7M person-address pairs.

We receive from Experian a semiannual 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, and otherwise enhance the quality of, the Infutor data (Diamond et al., 2020; Blattner and Nelson, 2024). 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. Our analysis then 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). We also link each individual to 2015-2019 5-year ACS data on the Census tract that the individual moved from, which we use to study additional characteristics of moves into treated and control ZIPs.

B Additional details on stacked dataset construction

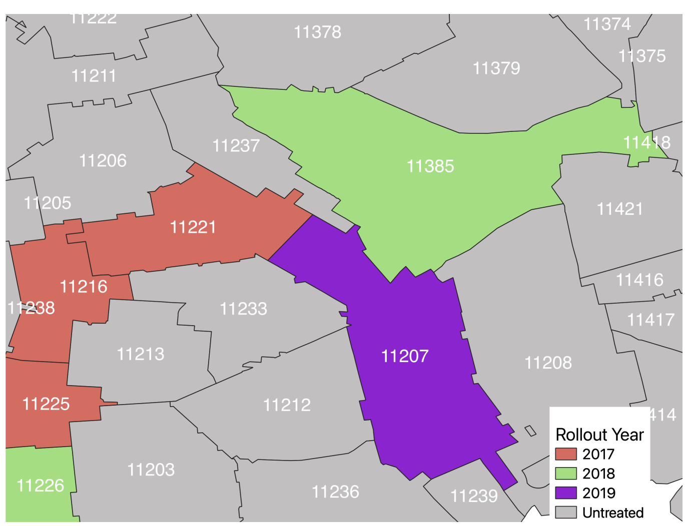
This appendix describes the construction of our analysis dataset. Figure B.1 shows the date when RTC was rolled out in each ZIP code, according to Ellen et al. (2021). Figure zooms into a subset of the ZIP codes in Figure B.1. Figure B.3 contains a decision tree that summarizes the process by which border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



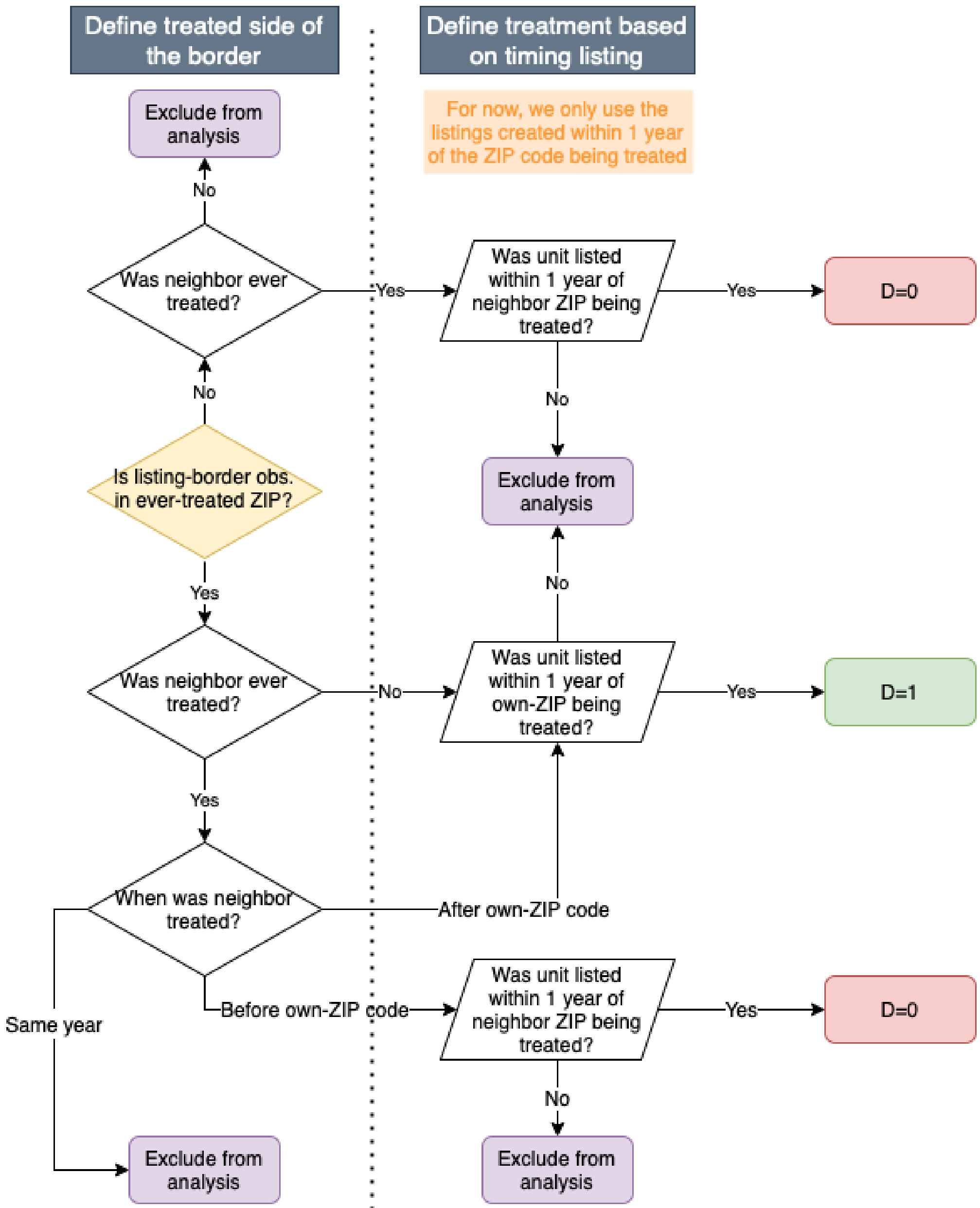
Note: This figure from Ellen et al. (2021) maps the rollout of Universal Access to Counsel in New York City.

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).

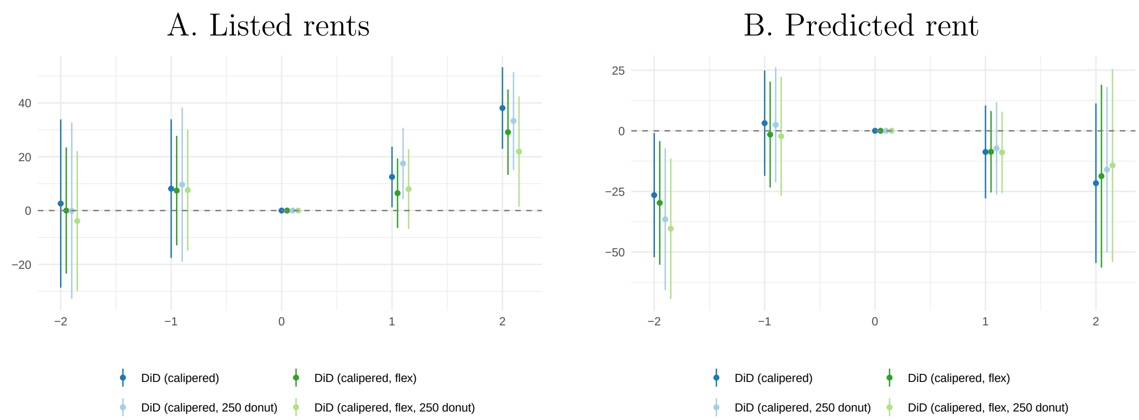
C Additional details on empirical estimates

This section provides additional details and robustness for the analysis of RTC's impact on rent, quantities, and tenant characteristics. 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. 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.

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

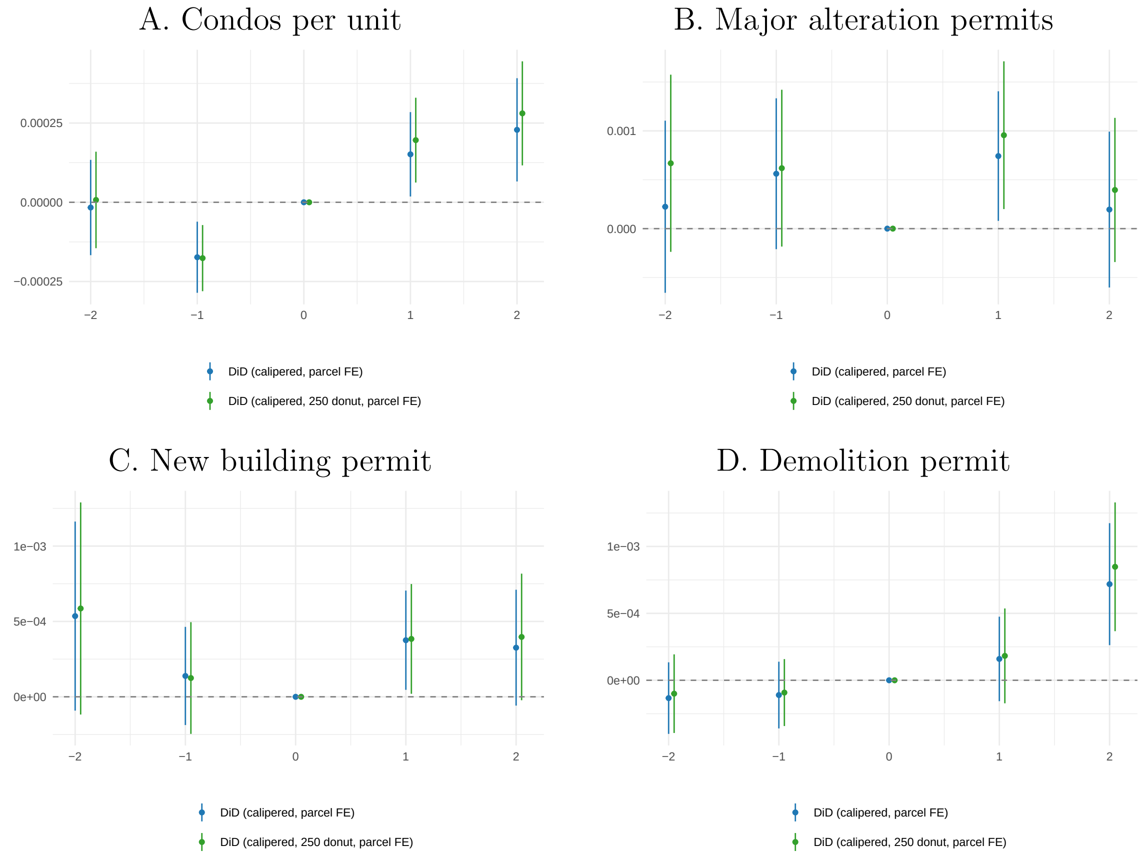
This section plots raw means and event study coefficients that correspond to the tables discussed in Section 3.2.

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



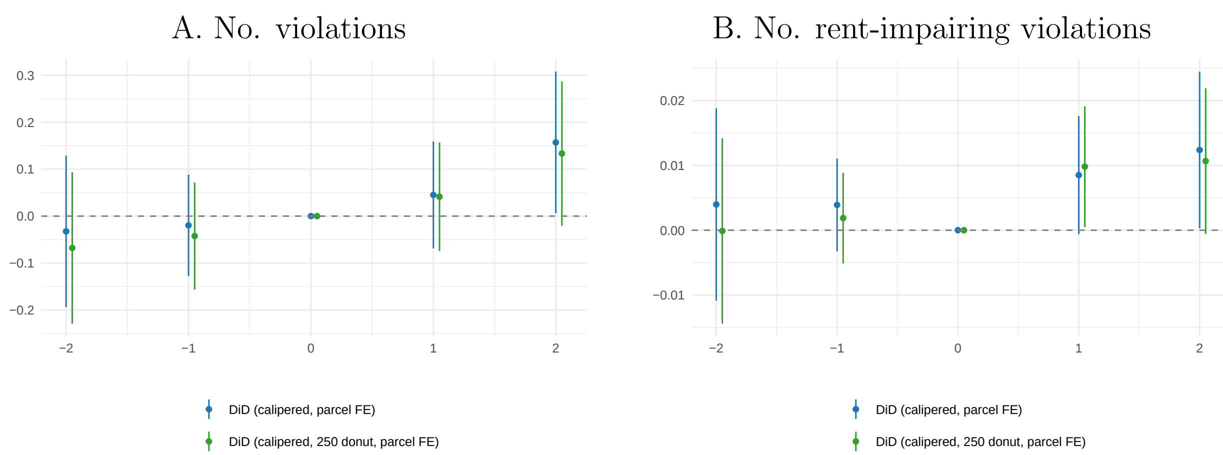
Note: This figure plots event study coefficients corresponding to the estimates in Table 2 (panel A), and Table C.5 (panel B). "Year 1" compares listings created within 365 days of the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings 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. "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. 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.2: Event Studies of Impacts of RTC: Quantity responses



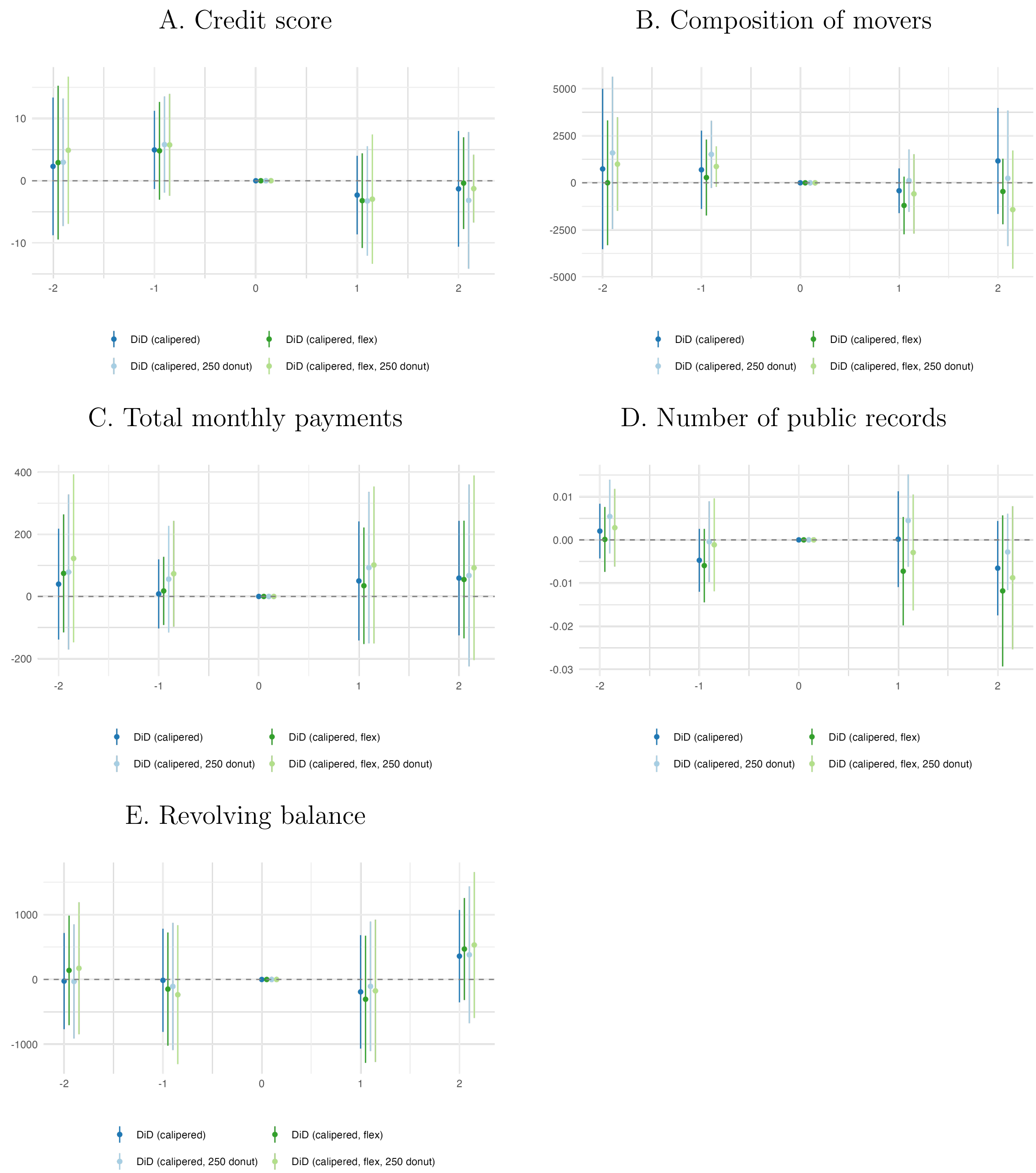
Note: This figure plots event study coefficients corresponding to the estimates in Table 3. We restrict our analysis to parcels that include housing within 1000m of the ZIP code border. The bars correspond to the 95% confidence interval. "Year 1", "Year 2", "Year -1" and "Year -2" are estimated jointly.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

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 4. 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.3: Event Studies of Impacts of RTC: Tenant Characteristics



Note: This figure plots event study coefficients corresponding to the estimates in Table 5. The bars correspond to the 95% confidence interval. 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 January 2016 and December 2019, linked to Experian credit report data. We drop observations where previous and subsequent address are both in a relevant border region (either in a zip code or bordering a zip code with RTC rollout). 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 score for a given move. The median 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 equation 1.

C.2 Additional estimates of impact of RTC

This section estimates our rent effects using of log-rent as the outcome (Table C.1), as well as under different choice of caliper around the border (Table C.2), constructions of the analysis sample (Table C.3), choice of control groups (Table C.4), choice of donut around boundary (Table C.6), and choice of hedonic controls (Table C.7).

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)
DiD (calipered, 250m donut)	0.006***	0.014***
	(0.002)	(0.003)
DiD (calipered, flex, 250m donut)	0.003	0.010**
	(0.003)	(0.004)
Observations	179578	160280
Pre-period mean	7.723	7.723

Note: This table estimates the impact of RTC on log listed rent prices. "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, 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 an 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.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)	8.530	12.509**	15.661***	16.210***
	(7.225)	(5.739)	(4.909)	(4.888)
DiD (calipered, flex)	2.770	6.472	9.493*	12.651**
	(6.187)	(6.602)	(5.541)	(5.769)
DiD (calipered, 250m donut)	17.435*	17.497***	19.758***	20.069***
	(10.134)	(6.771)	(5.605)	(5.371)
DiD (calipered, flex, 250m donut)	9.971	7.978	10.928*	15.068**
	(8.467)	(7.553)	(6.399)	(6.543)
Observations	87112	179578	243192	276374
Pre-period mean	2374.250	2361.702	2335.236	2321.385
Panel B. Year 2				
DiD (calipered)	43.583***	38.110***	44.970***	44.496***
	(7.225)	(5.739)	(4.909)	(4.888)
DiD (calipered, flex)	32.452***	29.131***	36.951***	38.560***
	(6.187)	(6.602)	(5.541)	(5.769)
DiD (calipered, 250m donut)	34.825**	33.318***	42.546***	42.899***
	(10.134)	(6.771)	(5.605)	(5.371)
DiD (calipered, flex, 250m donut)	20.861*	21.915**	33.280***	36.091***
	(8.467)	(7.553)	(6.399)	(6.543)
Observations	66817	160280	224619	257896
Pre-period mean	2374.250	2361.702	2335.236	2321.385

Note: This table estimates the impact of RTC on listed rent prices for different calipers. "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, 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.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.5 shows that there is

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)	10.451	12.509**	21.284**	11.500*	13.252**
	(7.073)	(5.739)	(9.277)	(6.489)	(5.699)
DiD (calipered, flex)	7.534	6.472	14.188	11.812*	6.820
	(6.610)	(6.602)	(11.503)	(6.164)	(6.008)
DiD (calipered, 250m donut)	22.091***	17.497***	24.675**	18.375*	18.229**
	(8.219)	(6.771)	(11.712)	(9.388)	(7.310)
DiD (calipered, flex, 250m donut)	15.011*	7.978	14.507	15.412**	8.253
	(7.684)	(7.553)	(14.459)	(7.779)	(7.543)
Observations	262903	179578	88004	163809	164237
Pre-period mean	2503.968	2361.702	2757.791	2425.670	2362.390
Panel B. Year 2					
DiD (calipered)	18.721	38.110***	40.495***	42.430**	38.313***
	(14.544)	(7.736)	(12.303)	(16.990)	(8.178)
DiD (calipered, flex)	15.308	29.131***	33.681***	33.936***	29.744***
	(9.768)	(8.094)	(11.398)	(12.513)	(8.497)
DiD (calipered, 250m donut)	23.538	33.318***	31.862**	45.262**	33.118***
	(16.152)	(9.308)	(12.849)	(20.643)	(9.357)
DiD (calipered, flex, 250m donut)	15.341	21.915**	15.288	29.314*	22.211**
	(12.836)	(10.489)	(13.629)	(16.028)	(10.754)
Observations	185085	120229	70967	109263	115679
Pre-period mean	2591.735	2439.793	2801.540	2527.291	2437.447

Note: This table estimates the impact of RTC on listed rent prices for different samples. "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. 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

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.[30]

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

[30]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.4: **Impact of RTC rollout on listed rents, different control groups**

	(1) Main	(2) Excl. ever-treated control	(3) Excl. control treated in t+1
Panel A. Year 1			
DiD (calipered)	12.509**	13.454**	11.254*
	(5.739)	(6.389)	(6.389)
DiD (calipered, flex)	6.472	5.998	5.438
	(6.602)	(7.592)	(7.592)
DiD (calipered, 250m donut)	17.497***	19.055***	15.498**
	(6.771)	(6.994)	(6.994)
DiD (calipered, flex, 250m donut)	7.978	7.728	6.921
	(7.553)	(8.718)	(8.718)
Observations	179578	120229	165090
Pre-period mean	2361.702	2439.793	2364.267
Panel B. Year 2			
DiD (calipered)	38.110***	38.110***	39.980***
	(7.736)	(8.277)	(8.277)
DiD (calipered, flex)	29.131***	29.131***	33.391***
	(8.094)	(7.752)	(7.752)
DiD (calipered, 250m donut)	33.318***	33.318***	33.822***
	(9.308)	(10.380)	(10.380)
DiD (calipered, flex, 250m donut)	21.915**	21.915**	26.771**
	(10.489)	(10.722)	(10.722)
Observations	160280	105786	147956
Pre-period mean	2361.702	2439.793	2364.267

Note: This table estimates the impact of RTC on listed rent prices using different control groups. Column (1) replicates the estimates from Table 2. Column (2) excludes all border-pairs where both sides of the border are treated during our study period. Column (3) excludes border-pairs where the control side of the border is treated the year after the treatment side. "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, 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

characteristics, including unit number, but one listing ends within 7 days of another listing 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 2013-2023. We then repeat the above procedure for listings created up to 14 days after an identical listing, and merges 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 we might estimate impacts on listing behavior that reflect different pre-trends, rather than policy impacts.

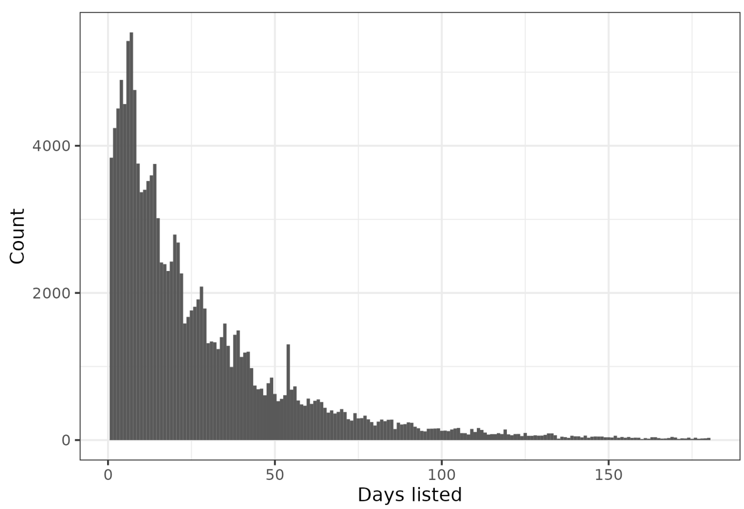
Using the dataset described above, Figure C.6 plots the effects of RTC on listing days

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

	(1) Year 1 effect	(2) Year 2 effect
DiD (calipered)	-8.722	-21.589
	(9.777)	(16.781)
DiD (calipered, flex)	-8.651	-18.702
	(8.568)	(19.237)
DiD (calipered, 250m donut)	-7.238	-16.002
	(9.727)	(17.386)
DiD (calipered, flex, 250m donut)	-8.881	-14.296
	(8.557)	(20.314)
Observations	175120	116877
Pre-period mean	2362.224	2461.521

Note: This table estimates 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-discontinuities 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. 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: Distribution of "days listed" in $t = -1$



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

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

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

	(1) Year 1 effect	(2) Year 2 effect
DiD (calipered)	12.509**	38.110***
	(5.739)	(7.736)
DiD (calipered, flex)	6.472	29.131***
	(6.602)	(8.094)
DiD (calipered, 250m donut)	17.497***	33.318***
	(6.771)	(9.308)
DiD (calipered, flex, 250m donut)	7.978	21.915**
	(7.553)	(10.489)
DiD (calipered, 500m donut)	19.998**	36.994***
	(9.454)	(12.482)
DiD (calipered, flex, 500m donut)	5.569	20.127
	(9.568)	(13.498)
DiD (calipered, two-sided 250m donut)	18.593***	37.881***
	(6.326)	(9.891)
DiD (calipered, flex, two-sided 250m donut)	10.615	29.787***
	(6.871)	(11.183)
DiD (calipered, two-sided 500m donut)	17.218**	30.701**
	(8.562)	(12.518)
DiD (calipered, flex, two-sided 500m donut)	8.997	26.785*
	(8.196)	(14.779)
Observations	179578	88221
Pre-period mean	2361.702	2361.702

Note: This table estimates the impact of RTC on listed rent prices, for specifications using different donut widths. "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, 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

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 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)

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

	(1) Main	(2) More controls
Panel A. Year 1		
DiD (calipered)	12.509**	14.074**
	(5.739)	(6.196)
DiD (calipered, flex)	6.472	8.713
	(6.602)	(6.785)
DiD (calipered, 250m donut)	17.497***	18.812***
	(6.771)	(6.919)
DiD (calipered, flex, 250m donut)	7.978	10.160
	(7.553)	(7.338)
Observations	179578	175120
Pre-period mean	2361.702	2361.702
Panel B. Year 2		
DiD (calipered)	38.110***	37.580***
	(7.736)	(7.413)
DiD (calipered, flex)	29.131***	29.178***
	(8.094)	(8.683)
DiD (calipered, 250m donut)	33.318***	31.292***
	(9.308)	(7.705)
DiD (calipered, flex, 250m donut)	21.915**	19.123*
	(10.489)	(10.369)
Observations	160280	156293
Pre-period mean	2361.702	2361.702

Note: This table estimates 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 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, a gym, doorman, or pool. *p<0.1; **p<0.05; ***p<0.01

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)	21.090***	38.110***	32.976***	42.921***
	(7.682)	(7.736)	(8.175)	(8.891)
DiD (calipered, flex)	9.181	29.131***	14.479	29.438***
	(9.002)	(8.094)	(9.222)	(8.765)
DiD (calipered, 250m donut)	21.897**	33.318***	34.498***	37.697***
	(9.009)	(9.308)	(9.159)	(9.053)
DiD (calipered, flex, 250m donut)	6.363	21.915**	11.723	19.522*
	(10.413)	(10.489)	(10.017)	(10.551)
Observations	112199	120229	109114	116877
Pre-period mean	2446.813	2439.793	2446.813	2439.793

Note: This table estimates 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 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. 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

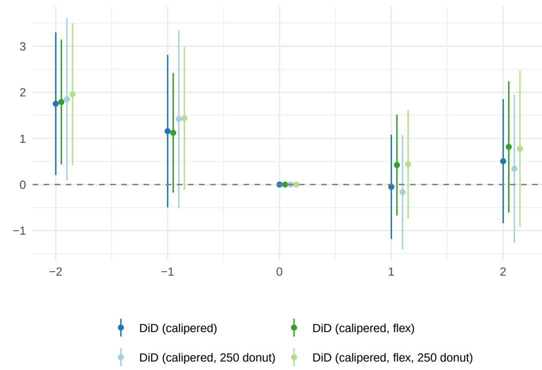
Table C.9: **Impact of RTC rollout on days listed**

	(1)	(2)
	Year 1 effect	Year 2 effect
DiD (calipered, parcel FE)	-0.371	-0.832
	(0.644)	(1.262)
DiD (calipered, 250m donut, parcel FE)	-1.122	-1.507
	(0.765)	(1.455)
Observations	88561	88561
Pre-period mean	21.309	21.309

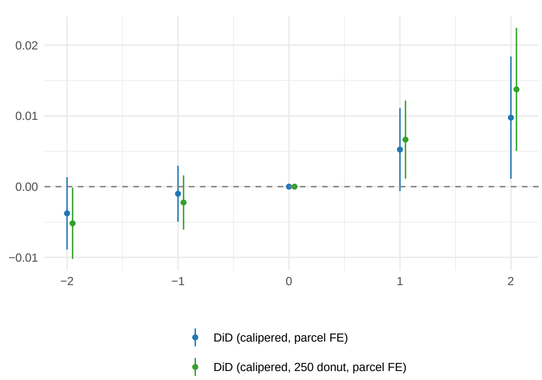
Note: This table estimates the impact of RTC on days listed per residential unit in each parcel. We restrict our analysis to parcels that include housing within 1000m of the ZIP code border. "Year 1" and "Year 2" are estimated jointly. We include parcel-by-border pair and year-relative-to-treatment fixed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

Figure C.6: Event Studies of Impacts of RTC: Number of Listings and Days Listed

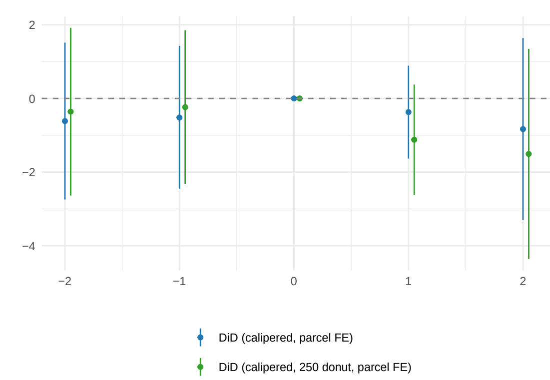
A. Days listed



B. Number of listings per unit



C. Days listed per unit



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. "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.

D Back-of-the-envelope calculation of direct cost to landlords

This section offers a back-of-the-envelope estimate of RTC's direct costs to landlords. We view our estimate as conservative because we do not include the potential impacts of moral hazard and adverse selection on landlord revenues, the contribution of which to landlord cost is difficult to quantify.

We begin by observing that the eviction filing rate in treated ZIP codes is about 15%. Cassidy and Currie (2023) estimate a 12.4 percentage point increase in representation due to the policy. Cassidy and Currie (2023) observed no change in filing rates, so we assume the filing rates continue to be 15% in covered areas. Since the effect of representation is to increase the duration of the court process by approximately three months, the missed rent component of the expected cost increase on any given lease is now $.15 \times .124 \times 3 \times \$2,200 = \$122.76$, where we have assumed that the average monthly rent is $2,200.

This figure does not include the additional legal fees associated with a more protracted court process. Since we do not have data on how legal fees change with the introduction of RTC, we assume they are proportional to the duration of the case. Case duration roughly doubles as a result of representation, so we assume that fees also roughly double. This is likely an underestimate, as the interaction with the tenant's attorney is likely more labor intensive, generating a larger increase in billable hours than the duration of the court case would suggest. Anecdotal evidence suggests that attorney fees for a typical eviction case are between $2,000 and $4,000. Combining the increase in legal fees with the increase in missed rent, we obtain an estimated per-lease increase in the expected cost of representation of $167.8–$212.8, or $14.0–$17.7 per month on a yearly lease.

As noted above, these estimates are conservative because they omit moral hazard and adverse selection. They also exclude the lower settlements documented in Cassidy and Currie (2023), and any added screening costs the landlord may incur. Despite being conservative, this estimate is non-negligible, and we believe it provides a useful lower bound that informs our discussion of landlord responses in the remainder of the paper.

E Additional details on welfare analysis

E.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. Define

$$u'(c(y)) \equiv p_e u'(y - fR - y_e) + (1 - p_e)u'(y - fR)$$

as the expected marginal utility of income for a household in default with realized income y.

The solution to the housing choice problem in Equation 6 satisfies the first-order condition

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

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 $v(h) - v(0) = Ru'(c(y))$ in the partial default region, the housing FOC simplifies to

$$0 \quad = \quad (\tau - p_e h_e)v'(h)F(y_0) \tag{13}$$

$$+ \int_{y_0}^{\hat{y}} \left[(\tau + f - p_e h_e)v'(h) - f\frac{dR}{dh} u'(c(y)) \right] dF(y) + \int_{\hat{y}}^{\bar{y}} \left(v'(h) - \frac{dR}{dh} u'(y - R) \right) dF(y). \tag{14}$$

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

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

$$\begin{aligned}
= & \frac{dh}{d\tau} \underbrace{\left((\tau - p_e h_e)v'(h)F(y_0) + \int_{y_0}^{\hat{y}} \left[(\tau + f - p_e h_e)v'(h) - f\frac{dR}{dh} u'(c(y)) \right] dF(y) + \int_{\hat{y}}^{\bar{y}} \left[v'(h) - \frac{dR}{dh} u'(y - R) \right] dF(y) \right)}_{= 0 \text{ by optimal housing choice (Eq. 13)}} \\
& + \int_{y_0}^{\hat{y}} \underbrace{\left[\frac{df}{d\tau} + \frac{df}{dh}\frac{dh}{d\tau} \right] (v(h) - v(0) - Ru'(c(y)))}_{= 0 \text{ by optimal default (Eq. 5)}} \\
& \qquad + \left[\left(1 - \frac{dp_e}{d\tau} h_e \right) [v(h) - v(0)] + \frac{dp_e}{d\tau}[u(y - fR - y_e) - u(y - fR)] - \frac{dR}{d\tau} f u'(c(y)) \right] dF(y) \\
& + \left(1 - \frac{dp_e}{d\tau} h_e \right) [v(h) - v(0)]F(y_0) - \frac{dp_e}{d\tau} \int_0^{y_0} [u(y) - u(y - y_e)] dF(y) - \frac{dR}{d\tau} \int_{\hat{y}}^{\bar{y}} u'(c) dF(y)
\end{aligned}$$

$$\implies \frac{dW(\tau)}{d\tau} = F(\hat{y})\left[\left(1-\frac{dp_e}{d\tau}\right)[v(h)-v(0)] - \frac{dp_e}{d\tau}\mathbb{E}_{y<\hat{y}}[u(y-fR)-u(y-fR-y_e)]\right] - \frac{dR}{d\tau}\mathbb{E}_y[fu'(c)] \ . \tag{15}$$

Note that we did not consider the eviction cost C in this calculation. This can be ignored because of optimal default by the marginal defaulting tenant. This completes the proof.

Equivalent Variation. Equation 10 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. To do so, we define the marginal equivalent variation from an increase in tenant protections as

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

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.[31] From optimal default, we know

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

so that

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

E.2 Empirical implementation

This appendix section provides further detail on the empirical implementation of Expression 10 in the main text. We generalize Expression 10 to include heterogeneity across renters in terms of ex-ante earnings Υ and geography g as follows:

$$\frac{dW_{\Upsilon g}(\tau)}{d\tau} \propto F_{\Upsilon g}(\hat{y}_{\Upsilon g})\left[1-\frac{dp_e}{d\tau}\left(h_e + \frac{y_e}{R_{\Upsilon g}}\bar{\theta}_{\Upsilon g}\right)\right] - \frac{dR_{\Upsilon g}}{d\tau}\frac{1}{R_{\Upsilon g}}\left[\bar{f}_{E,\Upsilon g}F_{\Upsilon g}(\hat{y}_{\Upsilon g}) + (1-F_{\Upsilon g}(\hat{y}_{\Upsilon g}))\frac{\mathbb{E}[u'(y-R_{\Upsilon g}) \mid y>\hat{y}_{\Upsilon g},\Upsilon,g]}{\mathbb{E}[u'(c_{\Upsilon g}) \mid y<\hat{y}_{\Upsilon g},\Upsilon,g]}\right]$$

Following the exposition in section 5.1, a first step is to use the NYCHVS to estimate the joint distribution of rent and ex-ante earnings (i.e., earnings at the time of lease signing) in each RTC-treated ZIP code. We assume that the labor earnings reported in the NYCHVS are representative of average ex-ante earnings at each rent level; the earnings trajectories for

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

non-evicted tenants shown in Figure 5 are broadly supportive of this assumption.[32] We then define deciles of ex-ante earnings on our entire NYCHVS sample (as described in Section 2.4.3). Next, we estimate a geography-specific average rent for each decile: we model rent as a linear function of ex-ante earnings and geography fixed effects, estimate this relationship via OLS, and use average fitted values as a smoothed measure of rent in each decile-by-geography pair. Throughout, we use NYCHVS-provided survey weights.

The finest level of geography observed in the NYCHVS is a sub-borough area (SBA), which typically comprises several (and up to 15) zip codes. When a treated ZIP code is in a unique SBA, we assume statistics from that SBA are representative of that ZIP code. When a treated ZIP is in multiple SBAs, we allocate from SBAs to the ZIP proportionally to the weighted number of NYCHVS observations in each SBA. We then calculate the share of tenants from each ZIP who are in each earnings decile, together with an average rent paid in that ZIP and decile. This is our desired joint distribution of rents and ex-ante earnings.

We then use the linked data on earnings and evictions to calculate, for each decile of ex-ante earnings as defined in the NYCHVS, the deciles of ex-post (mid-lease) earnings. These two sets of deciles imply 100 grid points on which we flexibly estimate the distribution of earnings changes. We compute ex-post earnings deciles separately for tenants who are and are not evicted.

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-a-vis our finding about the insurance value and welfare benefits of RTC, we treat terminal zeros in the evicted sample as true zeros, while dropping terminal zeros in the non-evicted sample, so that our insurance value estimates are plausibly an upper bound on the true insurance value of RTC.

Ex-ante earnings are defined using an average in quarters -8 through -5 relative to eviction filing, and ex-post (mid-lease) earnings are defined using an average in quarters -2 through 1 relative to eviction filing. As described in Section 2.4.3, 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.

We assume that earnings changes and eviction outcomes in the administrative data are independent of rent after conditioning on ex-ante earnings. Together with the NYCHVS data, these conditional distributions for evicted and non-evicted tenants then yield the joint distribution needed to quantify the ratio in Expression 12.

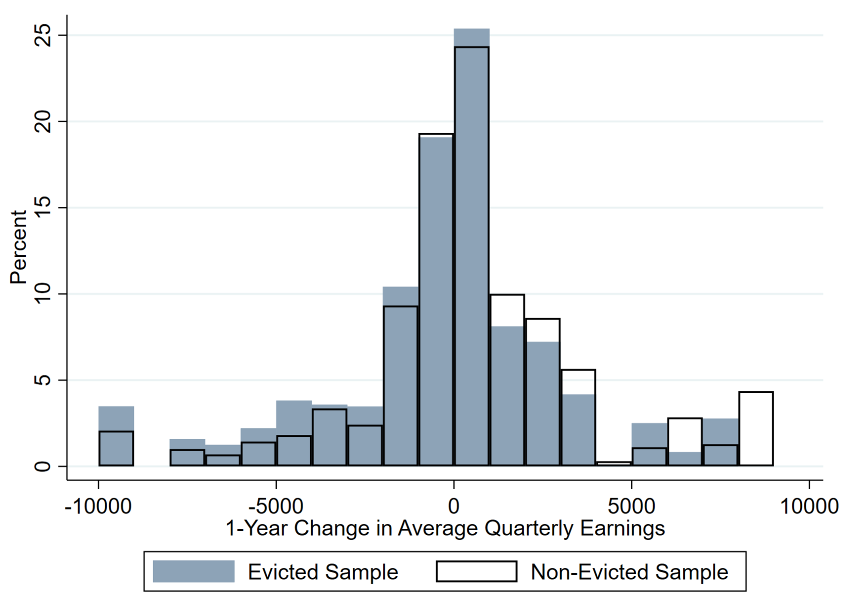
[32] As shown in Figure 5, earnings for non-evicted tenants are on average close to constant over time, and the NYCHVS data are broadly representative of non-evicted tenants.

We also use the court records in these linked administrative data to estimate the share of rent paid by evicted tenants for each ex-ante earnings decile and each ex-post earnings decile. Specifically, we use court data on the average amount of unpaid rent claimed by landlords at the time of eviction filing, together with average rent at each ex-ante earnings level, and an assumption that evictions on average occur at the culmination of a 12-month lease – a conservative assumption vis-a-vis our conclusion that the insurance value of RTC is somewhat modest.

E.3 Additional welfare results

This section includes evidence on the distribution of earnings changes for evicted and non-evicted tenants (Figure E.1), our estimates of the insurance value multiplier of RTC (Table E.1) and robustness of our welfare estimates to using different rent effect estimates from Table 2 (Table E.2).

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



Notes: 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 5.1 of the text. Ex-ante earnings are defined using an average in quarters -8 through -5 relative to eviction filing, and ex-post (mid-lease) earnings are defined using an average in quarters -2 through 1 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.

Table E.1: Insurance Value Multiplier of RTC

	Coef. of Relative Risk Aversion			
	$\gamma = 1$	$\gamma = 2$	$\gamma = 3$	$\gamma = 5$
Panel A. Main Estimates				
Min c of \$2000	1.24	1.35	1.37	1.36
Min c of \$4000	1.18	1.30	1.33	1.33
Min c of \$8000	1.12	1.23	1.28	1.29
Panel B. Evicted Tenants Pay Full Rent				
Min c of \$2000	1.35	1.51	1.55	1.59
Min c of \$4000	1.27	1.45	1.50	1.54
Min c of \$8000	1.19	1.36	1.45	1.50

Notes: Table reports the marginal rate of substitution between consumption in non-evicted and evicted states under different modeling assumptions given the correlation between eviction cases and income changes. A multiplier of 1 implies RTC has no "insurance value".

Table E.2: Welfare Impacts of RTC (\$/month): Robustness to Estimated Impacts

	Effect of RTC on Representation		
	16pp	25pp	50pp
Calipered, Flex, 250m Donut	-11.95	-2.67	15.75
Calipered	-27.54	-18.25	0.17
Calipered, Flex	-18.90	-9.61	8.81
Calipered, Flex, by Zip	-33.20	-23.92	-5.50

Notes: Table reports estimated welfare impacts on tenants in \$/month given different impacts of RTC on rental prices and legal representation. Each row uses a different estimated Year-2 rent effect of RTC. The first three rows use estimates from column (2) of Table 2. The final row uses linear fit of the border-pair specific price effects as a function of the treated ZIP code's baseline eviction rate (pre-rollout). The columns assume different impacts of RTC on legal representation, holding fixed the benefits in court for represented tenants in terms of longer case duration and fewer possession judgments. The first column uses our estimated take-up impact from the court analysis in Cassidy and Currie (2023). The second and third columns assume higher take-up impacts. Calculations assume a consumption floor of \$4,000, a coefficient of relative risk aversion of $\gamma = 2$, and that evicted tenants consume what they default on rent.

F 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). Then 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 rents increased \$26-50 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 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 In 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.[33] There was also a switch in ZIP codes in Bridgeport in an effort to increase case referrals.

F.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 starting in 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

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

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

$$R_i = \alpha_0 + \alpha_1 RTC_i + \alpha_2 X_i + ZIP_i + city_i \times month_i \times year_i + e_i \tag{16}$$

$$Y_i = \beta_0 + \beta_1 R_i + \beta_2 X_i + ZIP_i + city_i \times month_i \times year_i + \epsilon_i, \tag{17}$$

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 F.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 if 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).

F.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 deviation is that StreetEasy data we use on rental data in NYC does not include CT listings. Instead, we use data from Altos Research, a commercial data vendor on 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 F.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 to 50 dollars in year 1, and between 31 and 88 dollars in year 2, depending on the specification. Our estimates for year 2 are noisier and largely not statistically significant, potentially due to the smaller sample.

Table F.1: The 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

Notes: 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 respondent counsel. Columns (2) and (5) report two-stage least squares instrumental variable results for respondent 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 defendant 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 tenant (respondent) counsel as the dependent variable. *p<0.1; **p<0.05; ***p<0.01

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

	(1)	(2)
	Year 1 effect	Year 2 effect
DiD (calipered)	39.253***	55.928
	(12.534)	(37.482)
DiD (calipered, flex)	26.111*	31.483
	(14.079)	(31.309)
DiD (calipered, 250m donut)	50.157***	88.026*
	(17.474)	(48.832)
DiD (calipered, flex, 250m donut)	39.097*	51.812
	(22.025)	(39.590)
Observations	38439	14937
Pre-period mean	1511.777	1626.837

Note: This table estimates the impact of Connecticut's Right to Counsel on listed rent prices (δ_t in equation 1) using Altos data. Each panel provides estimates from a different data set of rental listings. "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. 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 90th percentile 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