The Impact of Right to Counsel on Rental Markets *

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Abstract

Right-to-counsel programs provide tenants in eviction court with free legal assistance. Seventeen cities and four states have recently introduced such programs. These programs can improve tenant outcomes in housing court but, when implemented at scale, may also involve cost to tenants due to equilibrium responses in the rental housing market. We empirically study this trade-off leveraging the ZIP-by-ZIP rollout of New York City's right-to-counsel program. Our analysis brings together data sets on eviction court cases, rental housing units, and individual-level earnings. We find robust evidence of price increases in ZIP codes subject to the policy, suggesting landlords passed costs on to renters through higher rental prices. We then develop a framework to evaluate the impact of expanded legal representation on tenant welfare, incorporating the trade-off between higher housing consumption after default and increased rent prices. We quantify these welfare effects using quasi-experimentally identified parameters. Despite the insurance value from increased consumption of housing after a tenant defaults, in most of our specifications the estimated price increases are large enough to generate a moderate net reduction in tenant welfare.

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1 Introduction

Housing courts across the United States handle high volumes of disputes between landlords and tenants: every year, 5-6% of renter households have an eviction case filed against them, amounting to an estimated 3 million court cases (Gromis et al., 2022). The eviction court process exhibits a stark asymmetry in terms of legal representation. Most tenants are unrepresented, while landlords typically have a legal representative. For example, in New York City's housing courts, 1% of tenants and 95-99% of landlords were represented before 2016, and in Cook County the corresponding numbers are 3% and 75% (Collinson et al., 2022).

Since 2017, four states and seventeen cities have aimed to redress this asymmetry by introducing or piloting large-scale subsidized legal aid programs under the label "right to counsel" ("RTC" henceforth). RTC programs provide free legal assistance to tenants who have a case filed against them in eviction court. Proponents of these policies argue they are beneficial to tenants because they yield more advantageous settlement agreements and lower eviction rates, which can have downstream benefits for tenants in terms of avoiding homelessness and improving labor market, health, and credit outcomes (Collinson et al., 2022). Opponents argue that these programs involve costs – not just directly to local governments, tax payers and landlords, but also to the intended beneficiaries (low-income tenants) by putting upward pressure on rent prices or inducing responses by landlords on other margins.

In this paper, we empirically examine the benefits and costs of RTC to low-income tenants and provide a framework to quantify its net effect on their welfare.[1] We bring together multiple datasets – on eviction court cases, rental housing units, and individual-level earnings – to provide quasi-experimental evidence on the impacts of the first RTC program, which was implemented in New York City in 2017. The program was rolled out on a ZIP-by-ZIP basis, providing arguably the best opportunity to date to identify the causal impact of representation on court outcomes on the one hand, and landlord responses to the policy implemented at scale on the other – both of which are important ingredients in determining the policy's net impact on tenant welfare.

Based on the empirical evidence, we argue that the policy had a sizable impact on court outcomes, implying potential benefits to tenants, as well as non-negligible expected costs to landlords of renting to a tenant at risk of default. We also find impacts on listed rents, suggesting that the benefits accruing to tenants are not without associated costs to that same population. However, it is not clear how these benefits and costs impact tenants' welfare, as this depends on how tenants value the increased protection. Guided by the empirical evidence, we develop a framework to formalize and quantify the impact of the policy on tenants' welfare. In our framework, tenants benefit from increased housing consumption due to RTC protections after they default on their rent, but pay a cost in terms of higher rent prices when they do not default. We quantify the welfare consequences of these two effects, taking

[1] We abstract away from costs and benefits to landlords, local government, and tax payers. For example, we do not consider the direct cost of lawyers' services, nor do we consider benefits to the public from homelessness spells prevented by legal aid. In future work, we aim to incorporate these factors since they are important for efficiency considerations. They are not needed to assess whether RTC can increase low-income tenants' welfare, a necessary condition if the aim of the policy is to make this group better off by redistributing resources towards them.

account of both the direct transfer value of the additional housing consumption, and the insurance value associated with evicted tenants' higher marginal utilities of income; the latter is estimated using administrative earnings data linked with eviction records, together with a range of assumptions about risk aversion. Combining our empirical results with our framework suggests that RTC is moderately welfare-reducing for low-income tenants.

Our analysis proceeds in five steps. First, we consider the impact of legal representation on individual tenants' court outcomes. Attorneys may affect tenants' outcomes in multiple ways. They may engage in negotiation with the landlord on behalf of their clients, obtaining more advantageous settlements or averting an eviction order. They may also request a continuance or discovery of evidence, or they may present arguments for the defendant in court. These actions can have a direct effect on whether an eviction order is given by the judge and the height of a money order awarded, but they can also add to the length of the court procedure. Hence, we consider as outcomes the final court decision, the money judgment, and the duration of the case. Using the universe of New York landlord-tenant court data since 2016 and a research design based on the ZIP-by-ZIP rollout of RTC (as in Ellen et al., 2021; Cassidy and Currie, 2023), we estimate that representation leads to substantial delays in the court process and notable reductions in eviction judgments and money judgments. At the same time, we do not find evidence that RTC changed filing rates or the composition of cases filed throughout the period that we are able to study. Taken together, these results imply substantial potential benefits to tenants who appear in court, in the form of both delay of a potential eviction, more advantageous settlements, and a lower probability of receiving an eviction order.

Second, we argue that landlords who operate in the ZIP codes subject to the policy faced an increase in the expected cost of renting out a unit due to the increase in legal representation, and that this increase is large enough to potentially be passed on to renters in the form of higher rent prices. To make this argument, we consider our estimates of the impact of legal representation on individual court outcomes. Returning to the court data to estimate eviction rates, we provide a conservative estimate of an increase in expected cost of \$16–\$20 per month for the average lease. We argue that this estimate is conservative because it only considers direct costs, and does not include the potentially exacerbating impacts of moral hazard and adverse selection on landlord revenues.

Third, we turn to landlord responses on several margins of adjustment: prices for newly listed units, listing duration (as a proxy for additional screening), and the extensive margin of supply. We address several important empirical challenges. First, most existing evaluations of legal representation in eviction court to date have not been able to evaluate such a policy at scale, making it impossible to empirically study landlord responses to a larger-scale roll-out. The scale of the NYC program helps address this issue. Second, obtaining credible quasi-experimental estimates is difficult when legal representation is provided universally all at once, since it is not clear how to choose a reasonable comparison group. Leveraging the ZIP-by-ZIP rollout of RTC, we compare outcomes for tenants and rental units in zip codes treated at different times using "difference-in-difference" and "difference-in-discontinuity" designs.[2] Third, research on rental housing markets has generally faced challenges due

[2]The phase-in of RTC in most other locations is not done on a location-by-location basis, with the notable exception of Connecticut. We present preliminary estimates based on the Connecticut rollout in Appendix C.

to the fact that high-quality microdata on rent prices is difficult to obtain. However, microdata is essential as it enables researchers to zero in on properties at the lower end of the market. We use data from three rental listing platforms – StreetEasy, Altos Research, and Craigslist – to partially address this issue. The use of multiple data sets increases coverage, both across geographies and rental type and quality, and provides some robustness against data quality issues that may occur in any one data set. Finally, it is difficult to obtain data on rent prices and supply precisely for the types of properties that are most likely to be affected by the RTC policy – those catering to the lowest-income renters – since parts of this market are characterized by informality. While we do not completely address this issue, the use of data from several platforms, covering different areas of the rental market, is likely an improvement over any one data set in isolation. Since the impacts of the policy are likely to be larger for units occupied by renters at higher risk, and these are less likely to be captured in any rent price data set, we believe our estimates of the impact of RTC on listed rent prices may be viewed as helpful lower bounds.

We find that rental prices increase in the two years after the policy is implemented. Across datasets and methodologies, we estimate an increase of 18–50 dollars per month in the first year, and 35–96 dollars per month in the second year. These results are valuable in their own right, as arguments around trade-offs are predicated on landlord responses along at least one margin, and our results provide prima facie evidence of such responses. Additionally, they are a key ingredient in evaluating the impact of RTC on tenant welfare, as we argue next.

Fourth, having documented the existence of a potential trade-off in tenant welfare, we develop a framework to formalize the sources of costs and benefits to tenants and evaluate the impact of RTC on tenant welfare. We use an envelope theorem argument to show how RTC's effects on rent, together with several other readily available statistics of the data, are sufficient to quantify the welfare effects of marginal changes in RTC policy. The approach is similar to that in the literature on optimal unemployment insurance or bankruptcy protection, but tailored to the rental housing market and our policy context. In the model, renters make ex-ante housing choices in the presence of income uncertainty, and then optimally default after income is realized. RTC acts like an in-kind transfer of housing from landlord to tenant by increasing the amount of time a tenant may remain in their unit after defaulting. If renters make optimal housing and default decisions, the welfare impact of RTC depends primarily on its effects on equilibrium rental prices and on the length of the eviction process, as well as its "insurance value" against income shocks. Thus, our estimates from the previous sections are also useful for evaluating RTC's welfare impacts.

Fifth, we implement our welfare framework empirically. We pair our estimates of RTC's effects on rent prices and eviction outcomes with data on how eviction filings co-vary with income, drawing on panel data on earnings from the New York State Department of Labor's Unemployment Insurance data system that we match to samples of New York City tenants facing and not facing eviction. These income data confirm that evicted tenants have lower earnings realizations in the quarters around their eviction filing than tenants not facing eviction, illustrating the potential insurance value of RTC's protections. When we quantify this insurance value in our welfare framework, however, we find it is modest relative to the costs tenants face through higher rent. Overall, we estimate that RTC decreases

tenant welfare by the equivalent of roughly $300 per year, per tenant.

Our analysis provides an empirical evaluation of RTC on tenant welfare for New York City, an important population is its own right, and arguably representative of some of the other major cities in the country that are implementing or considering this policy. Hence we believe that our empirical findings are relevant for the currently-developing policy agenda. The city's rollout represents arguably the best available opportunity to date to obtain quasi-experimental estimates of a key statistic in our framework that has not been considered in previous research on the New York City RTC program: the impact of the policy on rent prices. Our estimates of the response of listed rent prices are also broadly in line with preliminary supplementary evidence from a similar rollout in Connecticut, which are provided in Appendix C, suggesting broader applicability of our framework and empirical approach.

Our work builds on and contributes to several areas of the economics literature. First, we contribute to a small but growing literature on the consequences of providing expanded access to legal representation and other forms of legal aid. Our analysis of the effect of legal representation on court outcomes replicates and expands on Ellen et al. (2021) and Cassidy and Currie (2023), who use similar court data for New York and research designs based on the same program roll-out. We find quantitatively similar impacts on a core set of court outcomes, and present additional analysis on the effect of the policy on landlords' filing behavior. Several other studies consider the consequences of legal representation on eviction court outcomes.[3] Seron et al. (2001) conducted an RCT in New York City's Housing Court and found positive impacts on tenant outcomes (higher representation rate, decrease in eviction warrant issuance). Greiner and Pattanayak (2011) and Greiner et al. (2012) conducted similar experiments in Massachusetts. Jarvis et al. (2020) report on the randomized portion of the Sgt. Shriver pilot project in three California jurisdictions: Kern, San Diego, and Los Angeles. Their findings suggest the impact of legal representation is modest on eviction judgments, but there are notable impacts on money judgments and about a two-week increase in the duration of the case. The findings from RCTs need to be interpreted with care, as selection into the study samples may be of first-order importance. We consider the quasi-experimental evidence from New York to be more relevant to the present study, as these impacts are estimated for the population that ends up in fact receiving legal aid under expanded availability of legal assistance. Relative to the existing literature, we provide the first empirical evidence on landlords' responses to this policy when it is rolled out at scale, and argue these responses are a key factor in determining its overall effect on tenant welfare.

Second, our work relates to quasi-experimental evaluations of rental housing market regulations. Sims (2007) and Autor et al. (2014) study the impact of rent decontrol on prices and property values in Massachusetts and Cambridge MA, respectively, and discuss the welfare impacts of the trade-off between affordability, and greater unit and neighborhood quality. Diamond et al. (2019) examine the consequences of rent control in San Francisco, and similarly quantify both the costs and benefits to tenants of this type policy. We closely follow their approach in considering landlords' responses on the extensive margin of supply. Vigdor and Williams (2022) study the impact of lead paint regulations on housing affordability, and argue that habitability standards may negatively impact tenants by making

[3]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).

the most affordable units relatively more expensive to rent. Unlike these papers, we consider a policy that provides free legal assistance, which we argue leads to an in-kind transfer from landlord to tenant through the effect of legal assistance on the amount of time a tenant in default is able to stay in their home. This feature of the policy may increase the cost of supplying housing to lower income tenants, and specifically those at risk of default. The welfare impact of such a policy depends on several empirical factors, such as take-up of legal services, consumption differences across evicted and non-evicted states, and rent price responses, which we quantify in our framework.

Third, 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). Our framework is especially related to Dávila (2020)'s analysis of optimal consumer bankruptcy as a form of social insurance, where borrowers in default on loans play a similar role in his setting as tenants in default on rent do 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 et al., 2019). One limitation of the sufficient statistics approach is that it is informative about marginal or local policy changes, but not about non-local counterfactual policy changes. One way to address this limitation is to consider fully specified structural models in the style of Abramson (2023), Corbae et al. (2022), and Imrohoroglu and Zhao (2022), or in the search-and-matching model of Coulson et al. (2020). In contrast to these papers, we focus on tenant welfare, and we provide direct quasi-experimental evidence of the type of offsetting rent increases predicted in such models of rental market equilibria.

Finally, our work relates to and draws from a growing set of papers, spanning multiple academic disciplines, empirically examining the prevalence of eviction in the U.S. and its effects on individual- and household-level outcomes of renters. Collinson et al. (2022) is especially relevant, as this work focuses specifically on New York City, which is also the focus of our analysis. A broader literature in sociology has documented the prevalence of eviction around the country (see, e.g. Desmond and Kimbro, 2015; Gromis et al., 2022), and has pointed to the potentially detrimental effects of eviction on tenants' well-being (Desmond, 2012, 2016). The eviction process shares features with the foreclosure process. The consequences of foreclosure are studied in, for example, Diamond et al. (2019), while the labor-market consequences of foreclosure *delay* for homeowners are explored in Herkenhoff and Ohanian (2019). Their evidence on the importance of delay in the foreclosure process suggests the framework we develop may be helpful for assessing welfare impacts in other contexts.

This paper is organized as follows. Section 2 describes the RTC policy in New York City, as well as our data. Section 3 estimates the impact of access to legal counsel on tenant outcomes in housing court. Section 4 estimates the impact of RTC on rental prices. Section 5 develops our tenant welfare framework and provides estimates of the impact of RTC on tenant welfare. Section 6 concludes.

2 Right-to-counsel programs, the New York City rental market, and data

2.1 Right-to-counsel policies: background

While the federal government, as well as state and local governments, have traditionally funded some amount of legal aid in civil matters, 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 typically made universally available at no or subsidized cost.[4] In practice, low-income defendants in civil court matters often do not obtain legal counsel. An influential report by the Legal Services Corporation (*"Documenting the Justice Gap in America: The Current Unmet Civil Legal Needs of Low-Income Americans"*, 2005) estimated that 71% of low-income households needed civil legal services over the past year, and 86% of civil legal problems had no or inadequate legal assistance. In the specific case of housing courts, which handle disputes between landlords and tenants, the situation is similar to the aggregate picture: low-income tenants who are defendants in eviction cases are by and large unrepresented, while landlords typically have a legal representative. For example, in New York City's housing courts, 1% of tenants and 95-99% of landlords were represented before 2016, and in Cook County these numbers are 3% versus 75% (Collinson et al., 2022).[5]

In recent years, advocates for a civil right to legal counsel (sometimes referred to as "access to justice") have been successful in persuading policymakers to implement universal provision of legal aid to low-income tenants in housing court.[6] New York City was the first city to implement a universal right to counsel under Local Law 136 of 2017. Since then, four states and seventeen cities have rolled out or are currently rolling out RTC programs.[7] Similar policies are under consideration in Los Angeles, Boston, and Chicago.

2.2 Universal Access to Counsel in NYC

Universal Access to Counsel was signed into legislation in August of 2017, with the goal that the city would provide these services to all eligible tenants by 2022 (Been et al., 2018). The program was phased in over time on a ZIP-by-ZIP basis (see Appendix Figure B.7). ZIP codes were included earlier in the rollout based on availability of other legal service programs, eviction rate, prevalence of rent-regulated housing, volume of entries into homeless shelters, and other factors of need (Been et al.,

[4] This situation stands in contrast to criminal 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. In *Lassiter v Dept. of Social Services* (1981), the Court ruled that this "right to counsel" does not extend to civil matters, with the majority opinion arguing that representation is not guaranteed when physical liberty is not at stake.

[5] Engler (2010) surveys the literature on legal representation in eviction court and finds that, in those studies, 0–20 percent of tenants had legal representation, compared to 80–90 percent of landlords.

[6] As well as in some other areas of civil law, see for example http://civilrighttocounsel.org/map.

[7] States: Connecticut, Washington, Maryland, and Minnesota. Cities: Baltimore, Boulder, Cleveland, Denver, Detroit, Jersey City, Kansas City (MO), Louisville, Minneapolis, Newark, New Orleans, Oakland, Philadelphia, San Francisco, Seattle, St. Louis, Toledo.

2018). The full formula is not made available to the public. RTC was rolled out in October 2017 to ZIP codes 10457, 10467, and 10468 in the Bronx; 11216, 11221, and 11225 in Brooklyn; 10025, 10026, and 10027 in Manhattan; 11373, 11433, and 11434 in Queens, and 10302, 10303, and 10314 in Staten Island (Office of Civil Justice, 2018; Ellen et al., 2021).[8] In November 2018, ZIP codes 10462 (Bronx), 11226 (Brooklyn), 10031 (Manhattan), 11385 (Queens) and 10310 (Staten Island) were added to the list (Office of Civil Justice, 2019). In December 2019, five additional ZIP codes rolled out RTC: 10453 in the Bronx, 11207 in Brooklyn, 10029 and 10034 in Manhattan, and 11691 in Queens (Office of Civil Justice, 2020). The Covid-19 pandemic and subsequent federal eviction moratorium paused the rollout of RTC. In 2022, RTC expanded to all ZIP codes in the City of New York.

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

2.3 Institutional details on the NYC rental market

Work in progress.

2.4 Data sources and sample construction

In this section we briefly discuss our data sources and sample construction choices. Further details on the data, its coverage, and how it was cleaned are provided in Appendix A.

2.4.1 Court records

We use court records provided by the New York City Office of Court Administration (OCA). The data includes all eviction cases filed in civil courts in NYC between January 1st 2016 and December 30, 2020.

2.4.2 Data on rental listings

As mentioned in the introduction, one major challenge in studying the impacts of policy on rent is that data on rental prices is scarce, especially when the data needs to be at a low level of geography and at relatively high frequency. 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 this is only available in aggregate and averaged over a five year window. In addition, it includes long-term renters, whose

[8] 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 had been implemented in 2016-17.

rents may not reflect current market prices. To address these challenges we use three novel sources of rental listing data, each of which we discuss below.

Table 1 summarizes the three data sets of rental listings we use in our analysis: StreetEasy, Altos Research, and Craigslist. For each data set we report the characteristics of listings for the full sample and for the sample we use for analysis. We report the proportion of cases in each of the five Boroughs, average rent, average number of bedrooms, and average building age. We discuss each data source below. See Appendix A for more details on the data and sample construction.

StreetEasy StreetEasy provided a dataset of geolocated rental listings in New York City between 2007 and 2020. We drop observations missing geographic coordinates or that map to ZIP codes outside of NYC. The data includes some characteristics of the unit, indicators for various unit and building characteristics, the listed rent price, the day it was listed, and the day the listing was removed.

StreetEasy's listings are concentrated in Brooklyn, Manhattan, and Queens. While StreetEasy has more listings in higher income neighborhoods, they have substantial coverage in lower income neighborhoods (based on ACS estimates of the median household income in the ZIP), including the treated and control ZIPs used in our analysis. StreetEasy has the lowest average rent of the three data sets, and a slightly smaller average number of bedrooms.

Altos Research Data on rental listings were purchased from Altos Research, which sells weekly snapshots on rental listings in the United States. We standardized and geocoded listings in New York City as described in Appendix A. The Altos Research data contains more listings than the other two data sets, including listings in most ZIP codes in New York city, with better coverage of ZIP codes and has relatively more listings in Manhattan and the Bronx than the Street Easy data. The average rent in the Altos Research data is $300 more per month than in the StreetEasy data, though similar to the average rent in the Craigslist data described below.

The Altos Research data set is a weekly cross section, so we link listings over time to construct a panel data set. This provides us a measure of how long a property is listed, but less precisely than with the StreetEasy data set. Given we see repeated weekly snapshots, we also see price changes in some listings that are posted for multiple weeks. We use the final posted rental price, but results are robust to using the first listed price, and we find no evidence that the policy affected how often price changes occurred in listings that span more than one week.

Craigslist We use Craigslist data scraped for Besbris et al. (2021) and graciously shared with us by the authors. Many listings in the Craigslist data do not map to a specific address, with a large share mapping to ZIP centroids or intersections. Given our analysis uses distance to the ZIP code boundary we focus on the listings with complete address information (10.7% of listings). If we additionally include listings that map to intersections between two streets (49.9% of the data) or results are qualitatively similar but less precise. We additionally drop listings without a posted rental price. The Craigslist data does not include information about how long the Craigslist posting was listed.

The Craigslist data has slightly better coverage in ZIP codes in the middle-income neighborhoods compared to StreetEasy, but does not notably differ in coverage of low-rent units. The Craigslist listings that include complete addresses are heavily concentrated in Brooklyn. We again have coverage in the ZIPs we use as treatment and control, though the overall number of listings used in our analysis is notably smaller and weighted more heavily towards one specific Borough.

Primary Land Use Tax Lot Output (PLUTO) In parts of our analysis we use the Primary Land Use Tax Lot Output (PLUTO) tax lot data. The PLUTO data contains parcel level shape-files and details provided by multiple city agencies on each parcel. We use these data to construct a parcel-level data set on the number of residential units, the number of condos, the number of newly apportioned lots, and assessment values. We drop parcels that do not include any residential units. See Appendix A for additional details on the data and the cleaning process.

We merge the StreetEasy listings dataset to the parcel-level data using a spatial join in QGIS. For each listing in StreetEasy, we find the nearest parcel in the 2016 MapPLUTO shape-file. We use the merged dataset to calculate 1) the assessed value per unit in each listing's building and 2) the number of listings per building.

2.4.3 Court records linked to labor earnings

We draw on data from Collinson et al. (2022) featuring the labor market history of a sample of low-income individuals in NYC with and without an eviction case. One sample consists of all benefits recipients (SNAP, Medicaid, and TANF) that could be linked to eviction court records. See Collinson et al. (2022) for data details. The second sample is a 10% random sample of historical benefits recipients (without an eviction court record). Both of these samples were linked to quarterly earnings records from the 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.

3 Impacts of legal representation on court case composition and court outcomes

This section uses the staggered roll-out of RTC across ZIP codes to study how legal representation affects case outcomes. This analysis replicates and extends work by Ellen et al. (2021) and Cassidy and Currie (2023). Ellen et al. (2021) study the early years of RTC in New York. Cassidy and Currie (2023) use an IV design using the ZIP-level rollout of RTC as an instrument for having legal representation, which we closely follow.

3.1 Court outcomes

3.1.1 Research design

For evaluating the impact of representation on case outcomes in eviction courts, we use two-stage least squares where we instrument having legal representation, "Counsel", with the case being in a ZIP where RTC has been rolled out at the time of filing Z. Specifically we run:

$$\text{Counsel}_i = \pi + \rho Z_{RTC,i} + X_i'\theta + \lambda_{t,b} + d_z + U_i \tag{1}$$

$$Y_i = \alpha + \beta \text{Counsel}_i + X_i'\gamma + \delta_{t,b} + D_z + V_i \tag{2}$$

where Y_i is a court outcome, "Counsel$_i$" is an indicator for having a lawyer, $\delta_{t,b}$ and $\theta_{t,b}$ are time-by-borough fixed effects, and D_z and d_Z are ZIP-code fixed effects. As the official start date of the program does not directly correspond with the time when legal aid began being offered, we follow Cassidy and Currie (2023) for determining when the policy when into effect in each ZIP code.

3.1.2 Results

Effect on take-up of representation (first stage) Table 2 reports estimates resulting from the first-stage regression (Equation (1)). Cases in ZIP codes that are subject to the policy see an increase in the likelihood of representation of about 16 percentage points, relative to a baseline mean of 8%. This represents a 200% increase, yet clearly this is still far removed from full take-up. One likely contributing factor is that legal aid organizations are not allowed to solicit clients, and as such cannot actively reach out to tenants with cases filed in court. Instead, tenants often learn about their right to legal assistance if and when they arrive at the court house.

IV estimates Table 3 reports our IV and OLS estimates of the impact of legal council on case outcomes. The results suggest that legal council impacts court outcomes on multiple dimensions for those who take up legal assistance due to the program. The IV estimates show reductions in eviction orders (-0.28, off a baseline of 0.65), reductions in log judgment amounts (-2.03 off a baseline of 4.51) and an increase in case duration (80 days off a baseline of 76 days). All results are statistically significant at the 0.01 level. These results suggest two findings. First, the tenants who get legal council due to the program benefit with much longer cases, lower judgment amounts, and reduced probability of an eviction order. Second, landlords face substantial increases in costs from the policy, with case duration increasing by 80 days, a period in which the tenant typically does not pay rent. The cases are also substantially less likely to end in an eviction order, and result in lower judgment amounts.

Case composition An assumption necessary to interpret our 2SLS estimates as capturing the causal effect of legal representation on case outcomes is that the rollout or RTC does not change the composition of cases filed in treated ZIP codes. Ellen et al. (2021) and Cassidy and Currie (2023)

both provide evidence that this is not the case.[9]

4 Landlord responses to RTC

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

In this section we provide a back-of-the-envelope estimate of the direct costs of the policy to landlords. Our cost estimate is conservative because we do not include the potentially exacerbating 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%. In Section 3.1 we documented a 15 percentage point increase in representation as a result of the policy. Since we observed no change in filing rates as a result of the policy, we therefore assume (for the purpose of this calculation) that landlords face a 15 percentage point increase in evictions where the tenant is represented. We also assume that the eviction filing risk on the average lease equals that of the ZIP code. 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 $.15 \times .15 \times 3 \times \$2,200 = \$148.5$, 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 direct data on legal fees, we assume that fees are proportional to the duration of the case. Since case duration roughly doubles as a result of representation, we thus assume that fees roughly double relative to the fees for a typical case without representation. This is likely an underestimate, as the interaction with the tenant's attorney is likely more labor intensive and therefore generates 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 $193.5–$238.5, or $16.1–$19.9 per month on a yearly lease.

As noted above, these estimates are conservative, since they abstract away from moral hazard and adverse selection, which may contribute to reduced landlord income, all else equal. Furthermore, we have not included the lower settlements documented in Section 3.1 and increased search / screening costs.

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.

[9] Ellen et al. (2021) argue that "there is little indication that the rollout of UAC increased the share of tenants responding to eviction filings" while Cassidy and Currie (2023) aregue that "We find that there is no effect on case filings (suggesting that landlords are not filing additional cases)."

4.2 Research design

The staggered roll out of Universal Access to Counsel described in Section 2.2 suggests it may be possible to study impacts using a ZIP code level difference in difference design (DiD). Yet, neighborhood characteristics differ substantially across ZIP codes in New York. For example, some ZIP codes may be gentrifying while others may be in decline. More broadly, different parts of the city may have different trends in their rental markets. These differences suggest that the parallel trends assumption may be violated with comparing treated ZIP codes in New York to all untreated ZIP codes. An alternative approach would be to use a border discontinuity design (BDD) along ZIP code borders. BDD requires continuity of unobservables at the boundary (Cattaneo et al., 2019; Butts, 2021). This assumption is likely violated in New York City, where some ZIP code borders overlap with natural boundaries (e.g. parks, busy roads, train tracks, cemeteries) or with other administrative boundaries, such as school districts and borough lines. Given the limitations of both standard DiD and standard BDD, we instead use a difference-in-discontinuity (diff-in-disc) design with multiple borders and staggered treatment timing. The diff-in-disc specification first subsets the sample to units near a treatment boundary, which helps address concerns that the parallel trends assumption does not hold. Next, it differences the discontinuities from before and after treatment, which can address unobserved changes at the ZIP code's border.

Denote Y_{it} is the outcome of interest, D_i is an indicator that equals 1 if the listing is on the treated side of the border, $Post_t$ equals 1 in the year(s) after treatment, $f(\text{distance}_i^b, D_i)$ is a flexible function of listing distance to the border and treatment status, ϕ_b is a constant specific to a given ZIP code border and X_{it} are controls. Using this notation, we can write the diff-in-disc regression equation as:

$$Y_{it} = \alpha + \beta D_i + \theta \text{Post}_t + \delta(\text{Post}_t \cdot D_i) + f(\text{distance}_i^b, D_i) + \phi_b + X_{it}'\gamma + U_i, \tag{3}$$

where this regression additionally restricts the data to observations within 1000 meters of a treatment boundary. We estimate a number of difference-in-discontinuities specifications based on equation 3, where we allow for different degrees of flexibility in the distance gradient and/or control for school district fixed effects.

Difference-in-discontinuity: Our baseline specification estimates equation 3 with a linear gradient that differs on the treated and control side of the boundary: $f(\cdot) = \zeta \text{distance}_i^b + \omega D_i \text{distance}_i^b$.

Difference-in-discontinuity without distance controls Our "no dist" specification assumes no distance gradient $f(\cdot) = 0$. This is then equivalent to running a within-border difference-in-differences estimator with a bandwidth cutoff around the border.

Difference-in-discontinuity with flexible gradient: Our "flex" difference-in-discontinuities specification parameterizes $f(\cdot)$ as a linear border-specific gradient on each side of the border: $f_b(\cdot) = \zeta_b \text{distance}_i^b + \omega_b D_i \text{distance}_i^b$.

Diff-in-discontinuity with school district fixed effects: This specification replicates equation 3 with a linear gradient, but controls for school district fixed effects λ_s. Differences in elementary school quality are partialled out in border pairs where the two sides of the border are not in the same school district.

Difference-in-discontinuity with polynomial of geographic coordinates: Following Gelman and Imbens (2019) and Dell et al. (2018), we also estimate a version of equation 3 that controls for a polynomial of latitude and longitude instead of distance:

$$Y_{it} = \alpha + \beta D_i + \theta \text{Post}_t + \delta(\text{Post}_t \cdot D_i) + g(lat_i, lon_i) + + \phi_b + \lambda_s + X'_{it}\gamma + U_i \tag{4}$$

where $g(\cdot) = lat + lon + lat^2 + lon^2 + lat \times lon$

We estimate these regressions using a stacked dataset, following Cengiz et al. (2019) and Deshpande and Li (2019). First, we construct a dataset of ZIP code borders, where each border is identified by the ZIP code pair on either side $b = (z, z')$ (e.g., the border between 11206 and 11211 has one identifier). Then, we calculate the perpendicular distance between every rental listing in ZIP code z and each of z's borders. We drop border pairs where z and z' were never treated or treated in the same year. In borders where one side was treated one year after the other (2017 and 2018, or 2018 and 2019), we drop all listings created after both sides were treated, which addresses potential bias from comparing the late-treated ZIP listings to the early-treated ones (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). Our main specification excludes border pairs where the treated side was treated in 2019, since we don't have enough listings created after the policy rollout and before Covid-19 pandemic. We also drop border pairs where smoothness around the border fails, thus violating a key RDD assumption. We drop border pairs where the minimum distance to the border on either side is greater than 300 meters, which ends up capturing all ZIP code borders where we have 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.

4.3 Impact on listed rent prices

Using the three data sets on listing prices discussed in Section 2.4, we run the diff-in-disc regressions above to estimate the impacts of right to counsel on rent prices one and two years after its introduction. Figure 1 plots the results by data set for the five specifications discussed above. Across the StreetEasy, Altos Research, and Craigslist data sets we find evidence of increased rent in new listings. For StreetEasy (Panel A), we estimate one-year effects between 16 and 18 dollars all of which are statistically significant at the five percent level, except for the "Diff-in-Disc (flex)" specification, which is statistically significant at the ten percent level. The estimates for the second year are larger although somewhat less precise due to the smaller number of observations. The second year estimates range from 19 to 36 dollars and are all statistically significant at the five percent level other than the specification that controls for school district fixed effects.

The Altos Research estimates shown in Panel B are qualitatively similar to the StreetEasy results, but larger. The first year results are all in the 40-50 dollar range and are all statistically significant at the five percent level. The results for the second year are on average larger but again less precise with estimates from the 45-95 dollars. All year 2 results are statistically significant at the ten percent level, and the flex and polynomial specifications are statistically significant at the five percent level.

The Craigslist estimates shown in Panel C are also qualitatively similar to StreetEasy results, though they are larger and closer to the Altos Research results. In both periods the results suggest increases in rent prices between 50 and 100 dollars across specifications. In the first year, three of the four specifications are statistically significant at the ten percent level. In the second year three of the specifications are statistically significant at the 5 percent level and all four are significant at the 10 percent level.

Robustness and heterogeneity

The results presented above are robust to our choice of bandwidth and sample. Figure 4 shows results when reducing the bandwidth to 500 meters or increasing it to 1500 meters or 2000 meters. Across data sets, results are largely unchanged when changing bandwidth. For StreetEasy and Altos Research, point estimates are always positive and broadly similar, although pint estimates are somewhat smaller for the first year when using the 500 meter bandwidth. Precision largely stays the same or improves when increasing bandwidth, while estimates are less precise when using the 500 meter bandwidth. For Craigslist, two of the five specifications have negative point estimates when using the 500 meter bandwidth, though these are not statistically significant. More generally, the Craigslist data set is much smaller and restricting bandwidth to 500 meters results in many borders with few observations. For the same reasons, precision improves when increasing bandwidth.

Panel A of Figure 5 shows that results are robust to the choice of sample in the StreetEasy data. The figure compares four specifications run on our main sample to (i) the full sample, (ii) our sample but restricted to units that are likely not rent stabalized, (iii) our main sample but restricted to buildings where the pre-RTC annual eviction filing rate is at least five percent of the number of units in the building, and (iv) our main sample restricted to buildings where the pre-RTC annual eviction filing rat is at least one percent of the number of units in the building. When using the full sample, estimates are somewhat smaller. This is what 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. For the three other alternative samples, estimates are similar, but somewhat less precise, which we interpret as likely coming from changes in sample size.

Figure B.3 shows the estimated impacts on rent are also present when considering log rent. The StreetEasy 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 two. For Altos Research, the results are around one percent in the first year and 1.75 to three percent in the second year. For Craigslist, the estimates vary around four percent both years. Log rent estimates are somewhat less precise than our main specification.

Panel A of Appendix Figure B.4 shows that estimates are largely robust when considering alternative control groups. In particular, we use ZIP codes that are eventually treated as control groups in early years. For example, if two adjacent ZIP codes are treated one year apart, then the ZIP code that is treated one year later serves as a control in the Year one estimates (but not the Year 2 estimates). This figures shows specifications which (i) only use never treated ZIPs or ZIP codes not treated until two years into the future, (ii) only never treated ZIPs, and for comparison (iii) only *ever* treated ZIPs. The first two specifications produce estimates that are similar to our main specification though they are somewhat larger and, in some cases, more precise. The final specification produces statistically insignificant estimates with negative point estimates, which suggests that there may be some anticipation of the policy change.

4.4 Impact on other margins of adjustment

Above we focused on changes in prices for listed apartments, but landlords could pursue other margins of adjustment, such as increased screening or seeking alternative use of their properties. Alternatively, the introduction could also affect how landlords post their units, potentially affecting if they appear in our three data sources. The three data sets on rental listings are somewhat limited in their ability to capture these other margins of adjustment, but we consider two here. First, for StreetEasy we are able to measure how many days a listing is posted, while for Altos Research we can measure the number weeks for which a listing appears. Listings could be posted longer due to increased screening by landlords, but they could also be filled sooner if tenants value the additional legal protection offered by treated units. Second, landlords may choose not to list their units at all, or choose to list them in alternative ways such that they do not appear in our data. To test for suggestive evidence of this, we map listings to the PLUTO data on assessed property values and construct a measure of pre-rollout log assessed value per unit. This allows us to test if the composition of rental units changed based on our proxy for their value.

Figure 2 provides estimates similar to Figure 1, but for days listed in StreetEasy and weeks listed in Altos Research. The results are all small and almost all statistically insignificant. For StreetEasy, estimates are positive but less than one day in the first year, and increase to around one day in the second year, though none of the first year results are statistically significant and only two of the five specifications are statistically significant at the ten percent level in the second year. For the Altos Research data, estimates are close to zero in the first year and statistically insignificant. In the second year estimates are negative, suggesting a 0.3 week reduction in listing length, though they again are not statistically significant.

Figure 3 plots the log assessed value per rental unit of listed properties for StreetEasy, measured in 2016. The estimates are positive and around 1 to 2 percent in the first year, but all five estimates are statistically insignificant. In the second year, two of the specifications are positive and two are negative, but all five are small and statistically insignificant. Overall, we find no evidence that the properties appearing in our data are shifting due to the policy (either through landlords choosing not to list properties, or landlords renting properties in a way that they do not appear in our three data

sets).

Appendix Figures B.1 and B.2 show robustness to bandwidth selection and Figure 5 shows robustness to sample selection. Overall we consistently find small and largely statistically insignificant results.

4.5 Impacts on extensive margin of rental housing supply

Building on our prior analysis, we additionally consider the impacts of the UAC rollout on measures of extensive margin of rental housing supply. Similar to Diamond et al. (2019), we consider the following outcomes at the building level, albeit for a shorter time span:

- Number of StreetEasy listings,
- Condo conversions, and
- Building permits

These outcomes are constructed through a combination of PLUTO data, StreetEasy data, and publicly available data on permits. We measure these outcomes at the building level, and rescale them by the number of units in the building reported in the PLUTO data. We estimate border-level difference-in-differences specifications, where we restrict our analysis to parcels within 1000m of the ZIP code border. Building on our prior notation, Y_{it} is the outcome of interest, D_i is an indicator that equals 1 if the parcel is on the treated side of the border, $Post_i$ equals 1 in the years after treatment, and η_i^b is a parcel-by-border fixed effect:

$$Y_{it} = \alpha + \theta \text{Post}_i + \delta(\text{Post}_i \cdot D_i) + \phi_b + X_i'\gamma + U_i \tag{5}$$

Since we include all parcels in these regressions, we are able to include parcel fixed effects and therefore omit distance to the border.

Appendix Figure B.6 shows these results. Overall we find no evidence that the overall number or proportion of units listed on the platform changed or that there were increases in condo conversions. We find evidence that there may have been a small reduction in the number of building permits and alteration permits per unit (though no effect on new construction or demolition permits). This last result would be consistent with reduced investment in treated units by landlords.

Overall, all three of the data sets suggest that RTC increased rental prices in the treated ZIP codes in the two years after the introduction of the program. These results are robust to the choice of specification, the bandwidth, the sample selection, and which ZIP codes are used as controls. We also find no evidence that the sample of units on the platform, or the length of time units were listed notably changed.[10]

[10]While this paper focuses on New York, many other locations are considering RTC policies or are already in the process of implementing them. In Appendix C we replicate the analysis for Connecticut's RTC program. This program is just starting and there are few treated ZIP codes, but we find positive point estimates on the impact on posted rent in the first six months using Altos Research data, though these results are statistically insignificant, which may be expected given the

5 Impacts on tenant welfare

The prior two sections provide evidence supporting the idea that tenants in court benefit from the legal representation provided by RTC, but that the program also raises rental prices. Without further structure it is difficult to weigh if the benefits of RTC when facing eviction outweigh the higher costs of housing when paying rent, leaving ambiguous the impacts of the policy on tenants. This section provides a welfare framework to evaluate whether RTC is welfare improving for tenants. We follow the "sufficient statistics" literature in deriving an expression for the change in welfare due to a marginal increase in tenant protections that depends on a small number of empirical quantities.[11] Our model and welfare formula are similar in spirit to the analysis in Dávila (2020) of optimal bankruptcy exemptions, but adapted to match features of the rental housing market and policy environment. Section 5.1 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. Right-to-counsel increases the amount of time a renter may remain in their unit after defaulting.

Section 5.2 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 two key quantities: the value of additional time in the unit for evicted renters, and the cost of higher rents paid by tenants who do not default. We then show that this impact can be expressed in terms of observable quantities – specifically, the eviction filing rate, the impact of RTC on rental prices, and the difference in income changes for evicted and non-evicted renters – and a risk aversion parameter.

Section 5.3 implements the formula. We take estimates of RTC's rental price effects from Section 4.3 and RTC's effects on case duration from Section 3. The remaining parts of the formula are quantified using linked eviction court filings and New York state income records used in Collinson et al. (2022). We find RTC is welfare decreasing for tenants, under a wide range of plausible values of risk aversion.

As with other studies in the sufficient statistics vein, a key advantage of our approach is that it allows us to make policy-relevant welfare statements based on a small number of empirical quantities that we can identify in a transparent way. However, our approach has several limitations. First, it only allows us to make local welfare statements; we cannot determine the globally optimal level of RTC or perform other policy counterfactuals unrelated to the policy variation in our data. Doing so would require a fully specified structural model of the rental housing market, which is beyond the scope of this paper.[12] Second, we cannot make statements about the welfare impact on landlords (producer surplus) without additional (strong) assumptions regarding market structure on the supply side. Given that RTC likely (at least weakly) reduced producer surplus, the estimated welfare change for tenants is a plausible upper bound on the total welfare impact. Section 5.4 discusses other reasons why our quantification is a likely upper bound on the long-run welfare impacts on tenants from a

smaller number of ZIP codes and shorter time horizon.

[11] See Chetty (2009) for a review of this approach.

[12] See Abramson (2021), Corbae et al. (2022), and Imrohoroglu and Zhao (2022) for structural equilibrium models of the rental housing market.

city-wide rollout.

5.1 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 $\lim_{c\to 0} u'(c) = \infty$.[13]

A rental contract specifies a per-period rent $R(h, m)$ in exchange for quantity of housing h, with m denoting the strength of eviction protections. In line with the evidence in Section 3.1, eviction protections are modeled as affecting the amount of time a tenant may remain in their unit after defaulting. 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 $y - R(h, m)$ 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 m required to complete the eviction process. An evicted renter also incurs a utility cost C.[14] The renter therefore enjoys housing utility $v(h)$ for $\max\{f + m, 1\}$ periods, and their outside option $v(0)$ for the remainder of the lease.

The rest of the section first characterizes the optimal default and housing choices, and then derives a formula for the change in tenant welfare due to a marginal increase in m.

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

$$\max_{f\in[0,1]} u(y - fR(h, m)) + \min\{m + f, 1\}v(h) + (1 - \min\{m + f, 1\})v(0) - 1\{f < 1\}C\,. \tag{6}$$

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 $f = 1 - m$, the share of rent paid $f(y; h, m)$ will be weakly monotonically increasing in income y. There are three possible cases for the household's solution to Equation 6.

1. **Full Default:** $f = 0$ and the household receives utility $u(y) + mv(h) + (1 - m)v(0) - C$. A necessary condition for this to be true is $u'(y)R(h, m) \geq v(h) - v(0)$. This means the renter would rather spend rent on numeraire consumption than on additional time in the unit. Let $y_0(h, m)$ be the highest income realization at which full default is optimal.
2. **Partial Default:** $f \in (0, 1 - m)$ and the household receives utility $u(y - fR(h, m)) + (m + f)v(h) + (1 - m - f)v(0) - C$.[15] A necessary condition for optimal partial default is that the

[13] 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).

[14] In addition to being empirically plausible, without an eviction cost, all renters would strategically default on the last m periods of rent.

[15] We assume C is large enough so that there is no income realization for which it is optimal to pay exactly $f = 1 -$

household is indifferent between spending money on rent and the numeraire:

$$u'(y - fR(h,m))R(h,m) \;=\; v(h) - v(0)\,. \tag{7}$$

Let $\hat{y}(h,m)$ be the highest income realization for which partial default is optimal.

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

$$u(\hat{y} - R(h,m)) + v(h) = u(\hat{y} - f(\hat{y};h,m)R(h,m)) + (f(\hat{y};h,m) + m)[v(h) - v(0)] + v(0) - C\,.$$

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

$$\begin{aligned} W(m) = \max_h \;\; & v(0) \;+ \int_0^{y_0(h,m)} (u(y) + m[v(h) - v(0)] - C)\, dF(y) \\ & + \int_{y_0(h,m)}^{\hat{y}(h,m)} (u(y - f(y;h,m)R(h,m)) + (m + f(y;h,m))[v(h) - v(0)] - C)\, dF(y) \\ & + \int_{\hat{y}(h,m)}^{\bar{y}} (u(y - R(h,m)) + v(h) - v(0))\, dF(y)\,. \end{aligned} \tag{8}$$

The first line captures welfare when it is optimal to fully default on rent; the second line corresponds to partial default; and the third line 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 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 this gap impacts tenant welfare.

5.2 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 5.3.

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

$$\frac{dW(m)}{dm} \;=\; [v(h) - v(0)]F(\hat{y}) - \frac{dR}{dm}\mathbb{E}_y[fu'(c)]\;, \tag{9}$$

where $F(\hat{y})$ is the share of renters who are evicted, $\frac{dR}{dm}$ is the rate at which equilibrium rents change due to the policy, and $\mathbb{E}_y[fu'(c)] = \int f(y;h,m)u'(y - f(y;h,m)R(h,m))dF(y)$ is the expected marginal utility of numeraire consumption, weighted by the share of rent paid.

m and then default. Relaxing this assumption would not impact the welfare formula, but it would affect our empirical implementation.

See Appendix D 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. 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 9 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 they 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 on tenant welfare that are valid under many assumptions about supply-side and tenant behavior. In particular, $\frac{dR}{dm}$ 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 9 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 9 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 9 are already measurable. 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. Finally, we can use our estimates of the impacts of RTC on rental prices and eviction case durations to recover $\frac{dR}{dm}$. Section 5.3 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 7 and an assumption about risk aversion over numeraire consumption, i.e. the shape of $u(\cdot)$.

First, Equation 9 can be rewritten as follows:

$$\frac{\frac{dW(m)}{dm}}{v(h) - v(0)} = F(\hat{y}) - \frac{dR}{dm} \int_y f(y) \left[\frac{u'\left(y - f(y)R\right)}{v(h) - v(0)} \right] dF(y) \ , \tag{10}$$

where the marginal rate of substitution between numeraire consumption and time in the housing unit is highlighted as the remaining unknown quantity. First note that households who fully default make no contribution to the integral term, since they pay no rent. The remaining terms depend on households who partially default, and households who pay the full rent. Rearranging Equation 6, for

partial defaulters

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

The remaining challenge is calculating the MRS for households who are not evicted. If utility from housing $v(\cdot)$ is not state-dependent (does not depend on the income y realization) and $u(\cdot)$ is known, then 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:

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

This allows us to rewrite Equation 10 as

$$\frac{dW(m)}{dm} \;\propto\; F(\hat{y}) - \frac{dR}{dm}\frac{1}{R}\left[\bar{f}_E F(\hat{y}) + (1 - F(\hat{y}))\frac{\mathbb{E}[u'(y-R) \mid y > \hat{y}]}{\mathbb{E}[u'(y-fR) \mid y < \hat{y}]}\right] \tag{11}$$

where $\hat{f}_E$ is the average share of rent paid 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. The second term – the marginal cost of stronger protections – is proportional to the change in rent $\frac{dR}{dm}\frac{1}{R}$ 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. In that case, one could sign the welfare change due to increased protections simply by comparing the annual share of evicted renters to the rate at which rents (proportionally) increase. 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 rental 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 $\frac{\mathbb{E}[u'(y-R)|y>\hat{y}]}{\mathbb{E}[u'(y-fR)|y<\hat{y}]}$ may also differ from one. One can think of this term as the "insurance value" of 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.

5.3 Empirical implementation and results

Our empirical implementation of Expression 11 uses three main inputs: RTC's effects on rents; RTC's effects on case duration; and information on how eviction filings vary with income. The first two inputs come from our estimates in Sections 3 and 4. For the third, we use panel data on earnings for a sample of low-income individuals in NYC with and without an eviction filing described in 2.4, we

refer to these samples as the "courts" and "non-courts" samples respectively.

The New York State earnings data are further detailed in Collinson et al. (2022). We use these earnings data to estimate the ratio of expected marginal utilities in Expression 11, which we can write as

$$\frac{\mathbb{E}[u'(y-R) \mid \text{not evicted}]}{\mathbb{E}[u'(y-fR) \mid \text{evicted}]}. \tag{12}$$

For the denominator, we use realized earnings in the quarters around eviction filing for tenants facing eviction. The expression also requires an estimate of each tenant's rent. While actual rent is unobserved in the data, we suppose rent is increasing in annual earnings from the year prior to eviction filing, corresponding to income at the time the evictee's lease was likely signed. We estimate these rent levels in the 5-year ACS as a function of the year-prior average earnings observed in the earnings data. For the numerator, we conduct an analogous exercise using the joint distribution of prior earnings and realized earnings, where the timing of these two earnings observations are set for each tenant using placebo dates drawn to match the eviction-filing date distribution in the evicted-tenant data.

Both the denominator and numerator suppose a floor to non-housing consumption, representing other social insurance available for tenants with very low earnings realizations (Deshpande, 2016). In our baseline estimates we set this floor to $4,000 annually, and Table 4 shows our welfare conclusions are not sensitive to a wide range of plausible consumption floors. We suppose CRRA preferences over non-housing consumption 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.

For the remainder of Expression 11, we use estimates of the pre-RTC eviction filing rate in RTC-treated ZIP codes, weighted by observations in our estimation sample for RTC's rent effects ($F(\hat{y}) =$ 12.1% filing probability); estimates of the share of rent unpaid by each tenant facing eviction (f, and its average $\bar{f}_E$, taken from the unpaid rent claim amounts in the eviction filing data); and our baseline estimate of $\frac{dR}{dm}$. This last term is a ratio of our estimated rent effect (dR) in Section 4, which in our preferred specification is $67.49 per month or $809.88 per year, and our estimate of how RTC allows tenants facing eviction to remain in their apartment for longer (dm), which we take to be 3 months.

Results are reported in Table 4. For a wide range of plausible consumption floors (reported across rows of the table) and coefficients of relative risk aversion (reported across columns of the table) we estimate that RTC's effects on tenant welfare – or $\frac{dW(m)}{dm}$ in Expression 11 – are negative. For our baseline specification 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 $25.87 per month. This amount is only modestly sensitive to risk aversion and the consumption floor because evicted tenants experience only slightly worse income realizations than ex-ante similar renters who are not evicted. As a result, the "insurance value" from allowing tenants more time in the unit when the marginal utility of consumption is high is small.

Robustness. Table 5 explores sensitivity of our welfare conclusions to two additional inputs to the calculation: RTC's impact on rental prices and on court outcomes. While our price effect estimates

are qualitatively similar across a wide range of specifications and multiple rental listing datasets, they do differ quantitatively. The columns in Table 5 repeat the welfare calculation using the mean point estimate from our preferred specifications across the three datasets, and for each dataset separately.

Across rows, we assess robustness to the amount of protection offered by RTC. Our baseline calculation assumes that RTC's primary effect operates through longer case durations, but the estimates in Table 3 also show significant effects on the eventual case outcome. A tenant who avoids an eviction order may enjoy greater protection than that implied by case duration alone. Note, however, that this may be dampened by the fact that many tenants who avoid eviction are named in future eviction cases and pay much of the rent while they remain in the unit. Perhaps more importantly, our baseline calculation assumes that price increases reflect perfect take-up of RTC; in reality, 22-25% of tenants in treated ZIP codes received representation.[16] These complexities motivate exploring how different amounts of protection from RTC—i.e. the additional (rent-free) days in the unit—impact our results.

RTC's welfare impact remains negative for most values of additional time in unit and samples in Table 5. Using the Combined price effect estimate, RTC would have to give the average evicted tenant nearly 180 additional days in their unit for tenants to benefit from the policy – double the estimated effect of legal representation on case duration. The results in the Altos Research and Craigslist columns are similar. In contrast, the estimated rent effect in the StreetEasy sample is small enough to generate a small welfare gain for tenants if RTC provides 90 extra days in the unit. Due to low take-up in treated ZIP codes, we believe 90 days is likely an overestimate of the impact per eviction case. If the gain is 60 days, the welfare impacts become negative in the StreetEasy sample.[17]

Overall, these results help quantify and compare the welfare benefits to tenants in eviction court to the costs of higher rents borne by all renters. While preliminary, our estimates suggest negative welfare impacts for tenants in most of our specifications. These impacts are not sensitive to the choice of consumption floor or risk aversion parameters, but are sensitive to the estimated increase in rental prices and RTC protection. We believe many of our implementation choices push us to overstate renters' welfare gains, and that the estimated welfare losses in our baseline calculations are, if anything, conservative. Regardless, the welfare calculations illustrate that the direct benefits to renters through RTC's stronger protections are largely (if not more than) offset by the cost of higher rents.

5.4 Partial versus full roll-out

This section discusses several reasons why RTC's effects may differ in a full (i.e., citywide) implementation as opposed to a partial, ZIP-by-ZIP rollout. There are two broad classes of reasons that we consider: temporal and spatial.

Temporally, a partial roll-out that builds up to a citywide implementation, as in New York's RTC

[16]The impact on listed rental prices should reflect landlords' beliefs about RTC take-up going forward, which could have been above take-up rates immediately after the roll-out. In our view, those believed take-up rates should be well below one, making our baseline calculations conservative.

[17]See Appendix Table B.1 in Appendix B.3 for additional robustness based on the standard errors for our estimates of the impact of RTC's impact on rent prices.

program, may induce learning about RTC's effects on the parts of both landlords and tenants. Landlords may update their beliefs about RTC-induced costs and may intensify their response, especially if there are short-run frictions to adjusting rents. Tenants may learn more about the availability of RTC, which may increase take-up and any behavioral responses through their housing and default choices. For these reasons, the longer-run effects of RTC in a citywide implementation may feature more pronounced rent-increase effects than those we estimate based on New York's partial rollout.

Spatial reasons are more nuanced. On the one hand, the estimated rent increases in our various border discontinuity strategies may be attenuated by competition between landlords on treated and untreated sides of a zip border: as prices are strategic complements, treated landlords may not fully pass-through their RTC-related costs to tenants when they compete against untreated landlords on the other side of the border. A citywide roll-out (i.e, on both sides of any zip border) would then relax this competitive pressure and may lead to greater rent increases than we estimate. On the other hand, a component of the estimated rent increase may be due to adverse selection that arises in a partial roll-out: when only the treated side of a zip border increases its price, adverse selection (i.e., a correlation between default risk and willingness to pay) may lead to higher-cost tenants applying to units on treated sides of the border, further exacerbating any RTC-related rent increase. The net effect of these two depends on the extent of price competition and the severity of adverse selection and is in general ambiguous (Mahoney and Weyl, 2017). Thus an interesting question for further understanding RTC's effects on rent increases is the extent to which these rent increases arise through adverse selection, which will vary with the extent of spatial competition between landlords in a partial rather than full roll-out, or through other channels such as tenant moral hazard that do not.

6 Conclusion

As attention has grown around the prevalence of eviction, so too has the array of policy proposals to protect tenants facing eviction. Prominent among these policies is right-to-counsel, recently introduced in at least seventeen cities and four states. As such policies expand, understanding their potential benefits and costs for tenants is crucial. We study the largest-to-date implementation of RTC, in New York City beginning in 2017, leveraging its ZIP-by-ZIP rollout to provide the first quasi-experimental evidence of RTC's effects on the broader rental housing market. While we find, consistent with and extending prior work, that RTC provided meaningful benefits to tenants newly represented by lawyers in eviction proceedings, we also estimate that RTC led to nontrivial rent increases for tenants potentially affected by RTC's protections. Using tools from the sufficient statistics literature adapted for our setting, we illustrate how to quantify the welfare implications of these estimates for tenants. Under a range of plausible assumptions about risk aversion, we estimate that RTC-induced rent increases were sufficiently large as to offset the welfare gains from increased legal protection, leading to a moderate decrease in tenant welfare overall. Interesting questions for future work include the extent to which RTC-induced rent increases are shaped by landlord pricing power, tenant moral hazard, and adverse selection among rental applicants.

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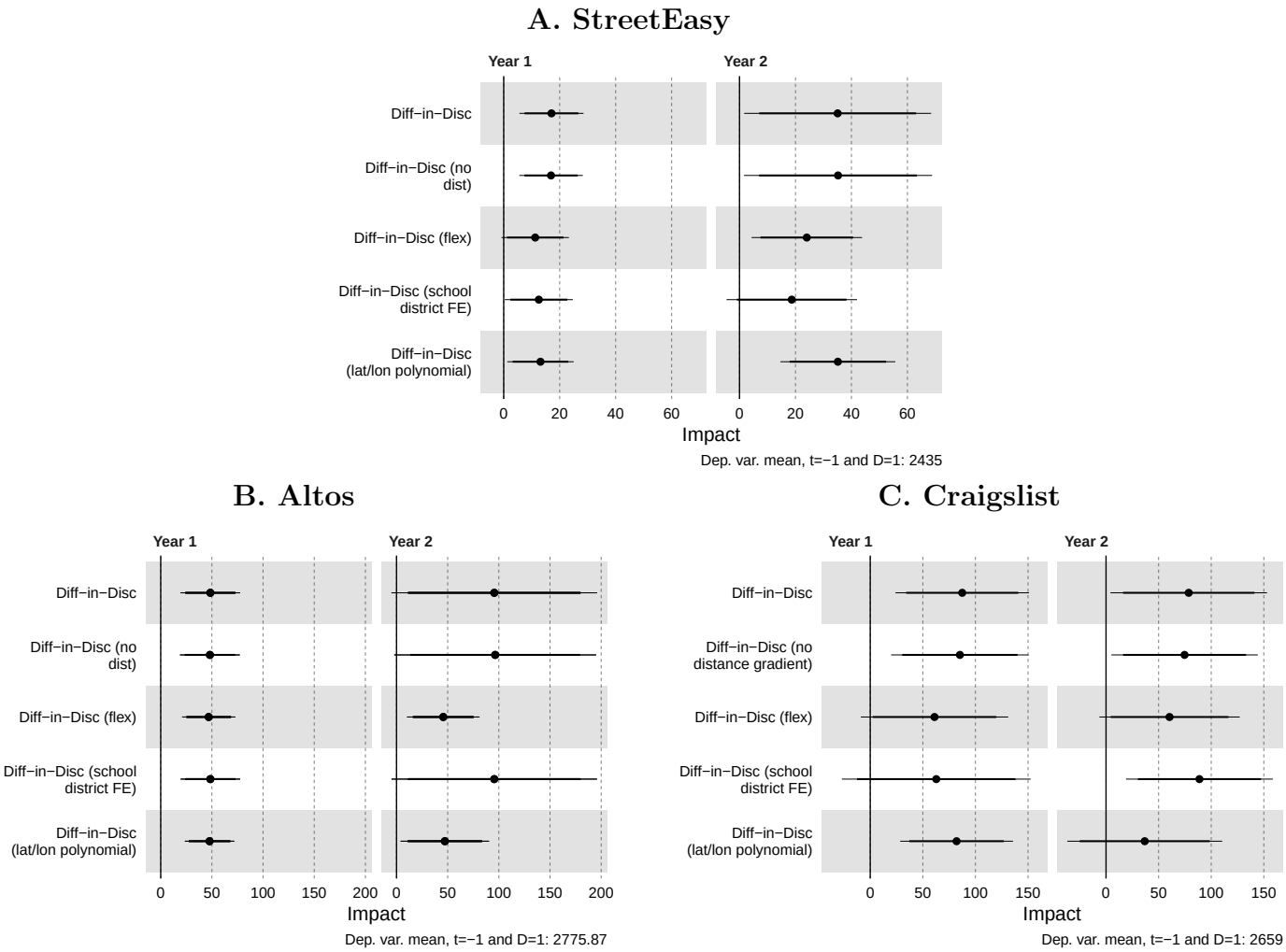
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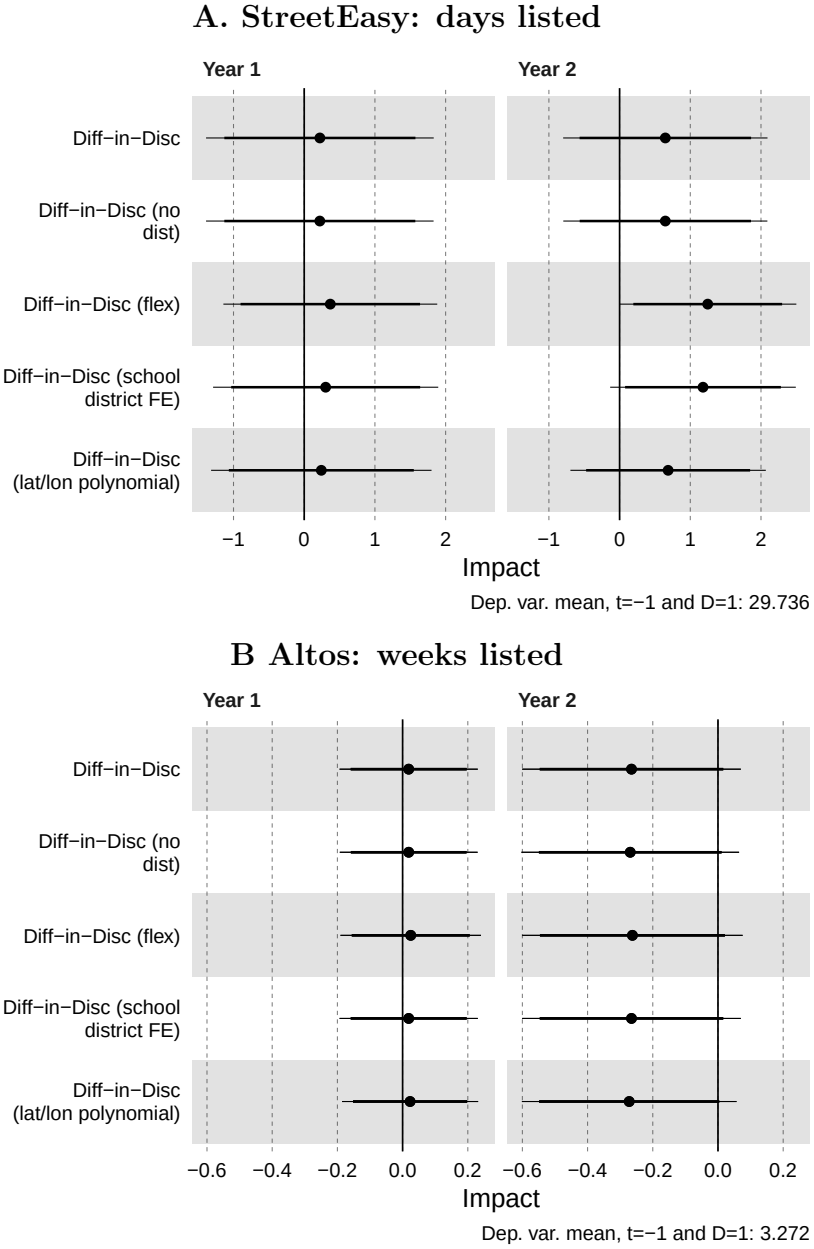
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Figure 1: Impact of RTC rollout on listed rents



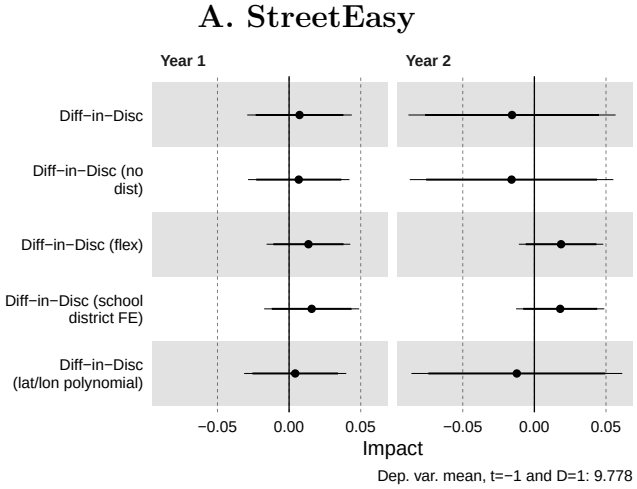
Note: This figure plots the difference-in-discontinuities estimates of the impact of RTC on listed rent prices (δ in equation 3) using StreetEasy data, Altos data, and Craigslist data from Besbris et al. (2021). Each panel provides estimates from a different data set of rental listings."Year 1" compares 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. Section 2.4 and Appendix Section A describe the data in greater detail. Section 4.2 describes the difference-in-discontinuities 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 and in buildings without a gym, doorman or pool. The Altos sample is limited to listings without a gym or a pool, and in the bottom 90th percentile of the asking price distribution. The Craigslist sample is limited to ads with complete address. In the regressions using StreetEasy data, we control for building age, year when 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. In the regressions using Altos data, we control for year when listing was created, an indicator for whether unit characteristics were missing, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. In the regressions using Craigslist data, we control for year when listing was created, an indicator for whether the ad mapped to an intersection, an indicator for whether the ad was missing data on number of bedrooms, an indicator for whether the ad was missing data on number of bathrooms, 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. We include both the 90% confidence interval (thicker line) and the 95% confidence interval (thinner line).

Figure 2: Impact of RTC rollout on time listed



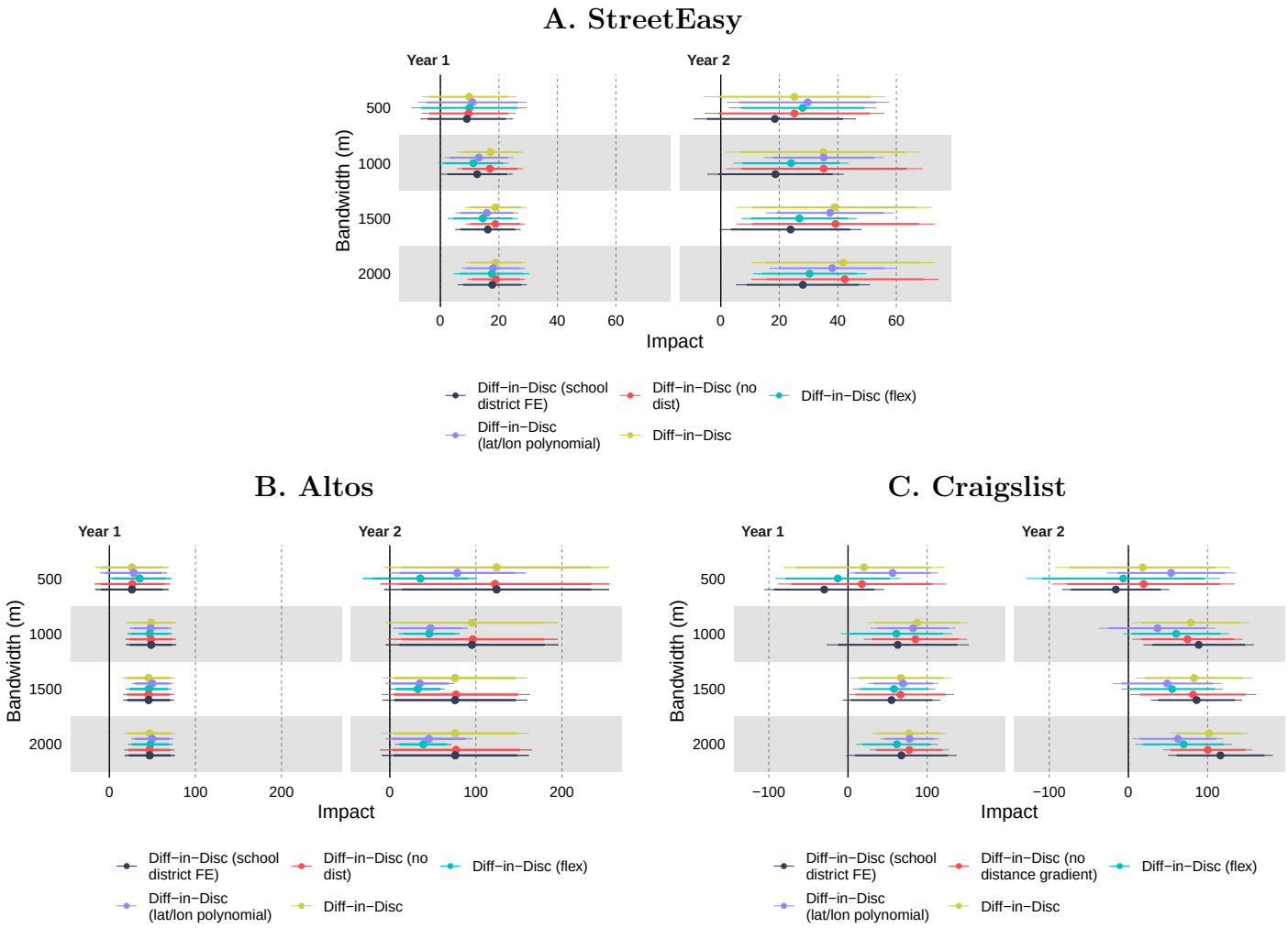
Note: This figure plots the difference-in-discontinuities estimates of the impact of RTC on time listed (δ in equation 3) using StreetEasy data and Altos data. "Year 1" compares listings created within 365 days of the rollout date to those created at most 365 days prior to the RTC rollout in that border 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 4.2 describes the difference-in-discontinuities 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 and in buildings without a gym, doorman or pool. The Altos sample is limited to listings without a gym or a pool, and in the bottom 90th percentile of the asking price distribution. The Craigslist sample is limited to ads with complete address and ads geolocated to the nearest intersection. In the regressions using StreetEasy data, we control for building age, year when 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. In the regressions using Altos data, we control for year when listing was created, an indicator for whether unit characteristics were missing, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. In the regressions using Craigslist data, we control for year when listing was created, an indicator for whether the ad mapped to an intersection, an indicator for whether the ad was missing data on number of bedrooms, an indicator for whether the ad was missing data on number of bathrooms, 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. We include both the 90% confidence interval (thicker line) and the 95% confidence interval (thinner line).

Figure 3: Impact of RTC rollout on log assessed value per unit



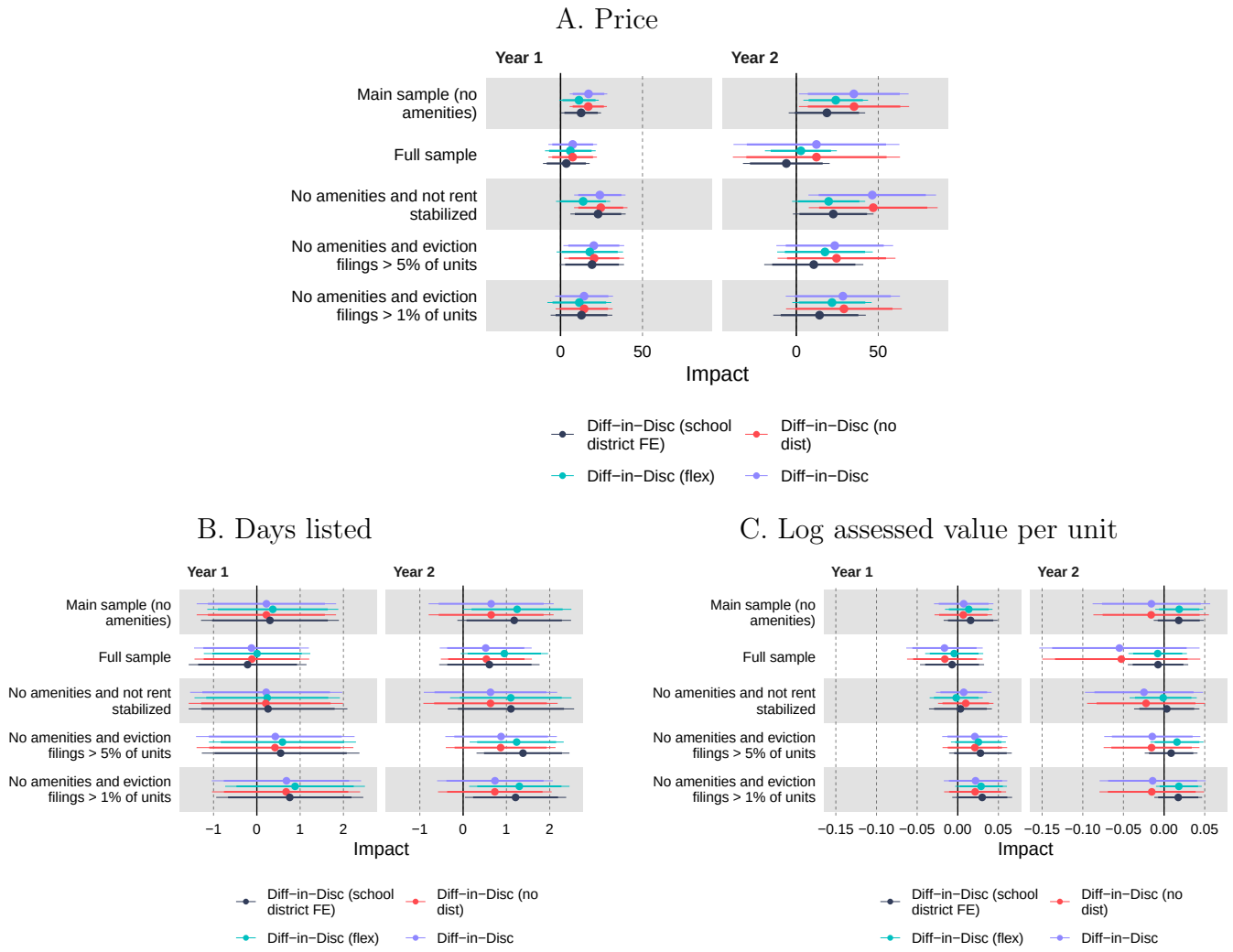
Note: This figure plots the difference-in-discontinuities estimates of the impact of RTC on the log assessed value per unit (δ in equation 3) using StreetEasy data. Each panel provides estimates from a different data set of rental listings. "Year 1" compares listings created within 365 days of the rollout 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. Section 2.4 and Appendix Section A describe the data in greater detail. Section 4.2 describes the difference-in-discontinuities 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 and in buildings without a gym, doorman or pool. In the regressions using StreetEasy data, we control for building age, year when 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. In the regressions using Altos data, we control for year when 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 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. We include both the 90% confidence interval (thicker line) and the 95% confidence interval (thinner line).

Figure 4: Impact of RTC rollout on listed rent: robustness to bandwidth selection



Note: This figure plots the difference-in-discontinuities estimates of the impact of RTC on listed rents for different bandwidths around the border (measured in meters) for the StreetEasy, Altos, and Craigslist data sets. "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 4.2 describes the difference-in-discontinuities approach 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. The Altos sample is limited to listings without a gym or a pool, and in the bottom 90th percentile of the asking price distribution. The Craigslist sample is limited to ads with complete address and ads geolocated to the nearest intersection. In the regressions using StreetEasy data, we control for building age, year when 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. In the regressions using Altos data, we control for year when listing was created, an indicator for whether unit characteristics were missing, bedroom count dummies, bathroom count dummies, and border-pair fixed effects. In the regressions using Craigslist data, we control for year when listing was created, an indicator for whether the ad mapped to an intersection, an indicator for whether the ad was missing data on number of bedrooms, an indicator for whether the ad was missing data on number of bathrooms, 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. We include both the 90% confidence interval (thicker line) and the 95% confidence interval (thinner line).

Figure 5: Impact of RTC rollout: robustness to sample selection in StreetEasy



Note: This figure plots the difference-in-discontinuities estimates of the impact of RTC on prices, days listed and log assessed value per unit for different samples of listings from StreetEasy. "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 4.2 describes the difference-in-discontinuities approach in greater detail. The full sample" includes all StreetEasy listings. The "no amenities" sample excludes listings without central AC and in buildings without a gym, doorman or pool. "Not rent stabilized" listings are those in buildings built before 1974, with at least 5 units and with rent below certain thresholds (see Board (n.d.)). We georeference the StreetEasy data and our parcel-level dataset to determine the average eviction rate in each listing's building between 2010 and 2015. In the "no amenities" samples, we control for building age, year when 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. In the "full" sample, we additionally control for whether the listing has central AC, and whether it is in a building with a gym, a doorman or a pool. 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. We include both the 90% confidence interval (thicker line) and the 95% confidence interval (thinner line).

Table 1: Summary Statistics

	StreetEasy		Altos		Craigslist	
	Full	Analysis	Full	Analysis	Full	Analysis
Years	2006-2020	2016-2019	2013-2021	2016-2019	2017-2022	2017-2019
Average rent	3231.5	2444.1	3439.6	2744.8	2811.0	2726.3
	(3172.1)	(720.4)	(1949.6)	(1363.9)	(1001.1)	(636.3)
% Brooklyn	34.5	53.9	25.9	45.0	65.1	93.4
% Bronx	1.9	6.9	3.0	9.4	3.0	0.5
% Manhattan	51.2	29.8	66.4	37.0	16.5	3.9
% Queens	12.3	9.4	3.9	8.1	12.4	2.2
% Staten Island	0.1	0.0	0.8	0.4	3.0	0.0
Average no. bedrooms	1.6	1.9	1.8	2.1	2.0	2.2
	(1.5)	(1.1)	(1.1)	(1.0)	(1.1)	(1.0)
Average building age	39.1	28.2				
Number of listings	1636465	206231	5032447	606333	86695	28293

Notes: This table summarizes the rental listings datasets used in our analysis: StreetEasy, Altos, and Craigslist from Besbris et al. (2021). Section 2.4 and Appendix Section A describe the data in greater detail. The full datasets include all listings. In the Altos and Craigslist datasets, we drop observations listed for $0 or $1. We also trim the full dataset at the 99th percentile to handle outliers. The StreetEasy analysis sample only includes "no amenities" listings, i.e. those without central AC and in buildings without a gym, doorman or pool. The Altos analysis sample is limited to listings without a gym or a pool, and in the bottom 90th percentile of the asking price distribution. The Craigslist sample is limited to ads with complete address.

Table 2: Impact on representation (first stage)

	$\mathbb{E}[D\|Z=0]$	OLS
Respondent counsel	0.079	0.158^{***}
		(0.010)

Note: This table shows the estimated coefficient from the first stage regression in equation 1. We include controls for the court borough, an indicator for specialized courts, an indicator for public housing cases, an indicator for a specialty designation in the case, an indicator for non-payment cases, indicators for legal representation of both sides, indicators when there is one petitioner and one respondent, the log of the primary claim total (inflation adjusted). From the 5-years ACS, the following controls on zip code level are included: quantiles for population, quantiles for the median household income, quantiles for housing units, quantiles for fraction of renters, quantiles for the median rent, quantiles for the fraction os hispanic, black, white, asian, naturalized citizens and non-US citizens population. Furthermore, there are indicators for a hispanic, black, asian and white majority. All standard errors are clustered by postal code and by court borough by filing month fixed effects.

Table 3: Impact on court outcomes (conditional on filing)

	$\mathbb{E}[Y\|D=0]$	OLS	IV
Eviction order	0.65	-0.101^{***}	-0.283^{***}
		(0.005)	(0.062)
Log judgment amount	4.51	1.204^{***}	-2.028^{***}
		(0.044)	(0.597)
Case duration (days)	75.9	100.042^{***}	80.339^{***}
		(2.048)	(8.678)

Note: This table shows IV results that correspond to the 2SLS regression written out in equations 1 and 2. We additionally include OLS estimates that do not instrument having legal council but rather include it directly as a regressor. Construction of outcomes closely follows Cassidy and Currie (2023). "Eviction order" is an indicator if the case ends in an eviction order. "Log judgment amount" is the natural log of judgment amount winsorized at the 1st and 99th percentiles and with one dollar added to cases with no judgment amount. "Case duration (days)" is the number of days from the filing date to the judgment date. All regressions include controls for the court borough, an indicator for specialized courts, an indicator for public housing cases, an indicator for a specialty designation in the case, an indicator for non-payment cases, indicators for legal representation of both sides, indicators when there is one petitioner and one respondent, the log of the primary claim total (inflation adjusted). From the 5-years ACS, the following controls on zip code level are included: quantiles for population, quantiles for the median household income, quantiles for housing units, quantiles for fraction of renters, quantiles for the median rent, quantiles for the fraction os hispanic, black, white, asian, naturalized citizens and non-US citizens population. Furthermore, there are indicators for a hispanic, black, asian and white majority. All standard errors are clustered by postal code and by court borough by filing month fixed effects.

Table 4: Tenant welfare impacts of RTC

	risk aversion:		
	$\gamma = 1$	$\gamma = 2$	$\gamma = 3$
Consumption floor:			
Min c of \$2,000	-27.06	-26.60	-27.87
Min c of \$4,000	-26.85	-25.87	-26.54
Min c of \$8,000	-26.64	-24.66	-23.84

Notes: Table reports estimated welfare impacts on tenants in \$/month. Calculations based on combined estimate of rent effect of RTC (\$67/month), which is the sample-size weighted average of the estimates from the three listings data sets. Eviction filing rates are a listing-weighted average of pre-2016 ZIP-code level filing rates in the pooled sample.

Table 5: Tenant welfare impacts of RTC: robustness to additional time in unit and effects on rent

	Sample used for rent effects:			
	Combined	StreetEasy	Altos	Craigslist
Additional Time in Unit:				
23 days	-54.1	-20.2	-65.1	-58.0
30 days	-51.0	-17.1	-62.0	-55.1
60 days	-38.7	-4.7	-49.7	-43.4
90 days	-26.4	7.6	-37.4	-31.6
120 days	-14.1	20.0	-25.1	-19.8
180 days	10.5	44.8	-0.5	3.7
Rent Effect Est. ($/mo)	67.5	31.4	79.2	71.2

Notes: Table reports estimated welfare impacts on tenants in $/month. Each column corresponds to a sample of listings. The Rent Effect Estimate is the unweighted weighted mean point estimate from the three preferred specifications in the relevant sample. The "Combined" estimate is then the sample-size weighted average of the estimates from the three data listings data sets. Each row corresponds to an additional time a tenant may spend in their unit after defaulting. 23 days is based on RTC take-up rates among treated eviction cases. Calculations assume relative risk aversion of gamma=2 and a $4,000 annual consumption floor. Eviction filing rates are a listing-weighted average of pre-2016 zip-code level filing rates in the relevant sample. The final row reports the estimated impact of RTC used in each column.

A Additional details on data construction

In this section we briefly discuss our data sources and sample construction choices. Further details are provided in Appendix A.

A.1 Court records

Source and coverage. The data was provided by the NYC Office of Court Administration (OCA). The data includes all eviction cases filed in civil courts in NYC (Bronx County Civil Court, Harlem Community Justice, Kings County Civil Court, New York County Civil Court, Queens County Civil Court, Redhook Community Justice Center, and Richmond County Civil Court) between January 1st, 2016, and December 30, 2020.

Cleaning and construction of analysis sample. **To be written.**

Variable definitions and construction.

- *Eviction order*:
- *Log judgment amount*:
- *Case duration*:

A.2 Data on rental listings

A.2.1 StreetEasy

Source and coverage. StreetEasy provided a dataset of geolocated 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 (Appendix Figure B.8A). Appendix Figure B.8C 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.

Cleaning and construction of analysis sample. We further process the StreetEasy data to construct our main analysis dataset. First, we drop the top .1% of listings by price and days listed ($6395 and 174 days, respectively). We then impute missing values for unit characteristics. We create 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. Finally, we top code number of bedrooms and bathrooms to 4. Finally, we restrict

Appendix Figure B.8B 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 Appendix Figure B.8A. Panel D shows that we have adequate coverage around the borders across the sample.

Variable definitions and construction.

- *Rent*: price included in the rental listing on the platform
- *Days listed*: number of days between the date the listing was created and the date the listing expired on the website or was removed

A.2.2 Craigslist

Source and coverage. John Kuk generously provided Craigslist data used in Besbris et al. (2021). We drop listings without price information, or whose listed price is 0 or 1 dollars. Unfortunately, not every rental listing on Craigslist is mapped to the actual address. Our difference-in-discontinuities approach requires the calculation of distance to the border between ZIP codes. Thus, we only use listings with complete address information (10.7% of listings) and listings that map to the intersection between two streets (49.9% of listings). Figure B.9A maps the listings with complete address or mapped to the nearest intersection. The data in the sample are concentrated in Brooklyn. Figure B.9C compares the number of listings in each ZIP code with number of renters according to the 2015-2019 ACS. Craiglist has slightly better coverage than StreetEasy of the middle deciles of the median household income distribution.

Cleaning and construction of analysis sample. We drop the top .1% of listings by price ($5000). We then impute missing values for unit characteristics. We do not observe building age or building amenities in the Craiglist data. We create an indicator that equals 1 when the listing is missing information on the number of bedrooms or bathrooms, and assign one bedroom or bathroom when those variables are missing. Finally, we top code number of bedrooms and bathrooms to 4. Finally, we restrict

Appendix Figure B.9B 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 Appendix Figure B.9A. Panel D shows that we have adequate coverage around the borders across the sample.

Variable definitions and construction.

- *Rent*: price included in the rental ad on the platform

A.2.3 Altos

Source and coverage. Altos Research has collected weekly snapshots of rental listings. We standardize listing addresses in New York City and geocode them using ArcGIS Pro. We drop observations that are missing geographic coordinates after the geocoding procedure, or that map to a different ZIP code different than the one in the data. The clean Altos dataset contains listings in most New York City ZIP codes (Figure B.10A). Figure B.10C compares the number of listings in each ZIP code with number of renters according to the 2015-2019 ACS. The Altos dataset has better coverage than StreetEasy in Manhattan and in parts of Queens.

Cleaning and construction of analysis sample. The Altos Research dataset is a weekly cross-section, so we link listings over time to construct a panel dataset. For each unit ID, we calculate number of weeks between date of observation and last date of observation. We then create an identifier based on unit ID, street address, price, and date. Then, for each listing, if the same unit was listed within the past 3 weeks, we determine that the listings in week t and $t-3$ are the same, even if the asking rent changes. We repeat this until we cannot link any more listings. With the linked data, we can calculate the number of weeks that a unit was listed. We generate an indicator for whether price changed during the lifetime of the listing, and determine the initial and final asking rent. We then impute characteristics of the units as follows. First, we correct inconsistencies in the amenities variables. We then impute missing information on amenities with 0. Finally, we impute missing data on bedrooms and bathrooms as 2 and 1, respectively, and top code number of bathrooms and bedrooms at 4. We construct indicators that equal 1 when we have imputed unit amenities.

Variable definitions and construction.

- *Price:* asking rent when listing was last observed, thus most likely capturing the contract rent
- *First price:* asking rent when listing is first observed in the Altos dataset. "First price" and "price" are the same when the price has not changed
- *Weeks listed:* number of weeks between first and last appearance in the data, and 0 if the listing only appears once
- *Price change:* indicator for whether price changed during the lifetime of the listing. It equals 1 if the maximum and minimum price for a given listing ID are different.

A.3 Parcel characteristics

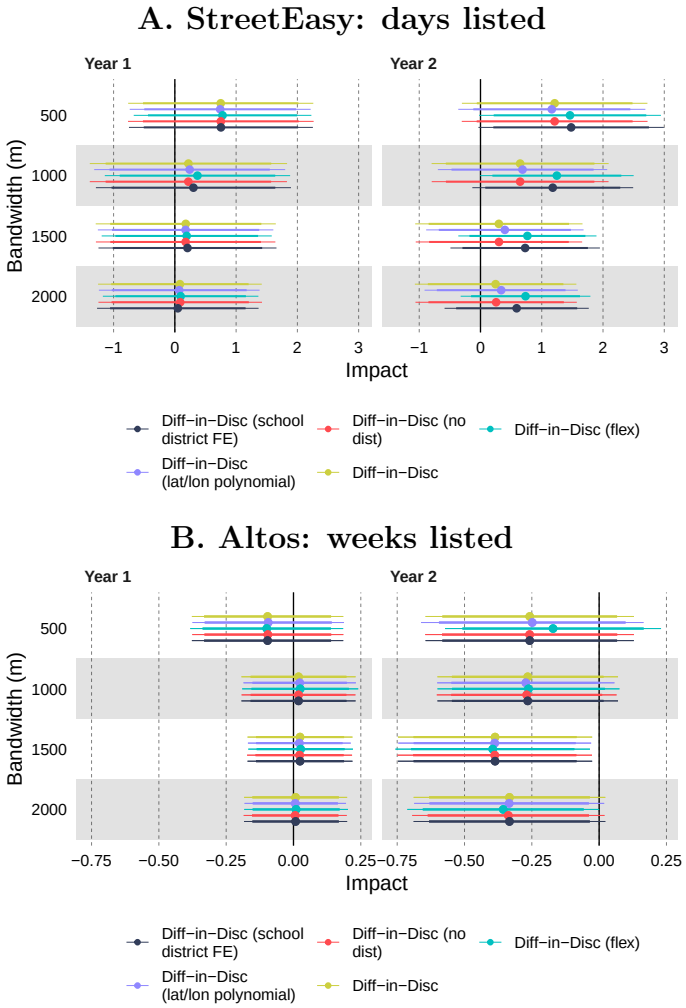
Source and coverage. The Primary Land Use Tax Lot Output (PLUTO) tax lot data combines datasets maintained by the Department of City Planning (DCP), Department of Finance (DOF), Department of Citywide Administrative Services (DCAS), and Landmarks Preservation Commission (LPC). We use these data, including 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. 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. We drop parcels with no residential units, no residential area or that show up in fewer than 4 vintages of the PLUTO data between 2016 and 2020. For each parcel, we calculate the number of evictions between 2010 and 2015 using data from Collinson et al. (2022).

B Additional results

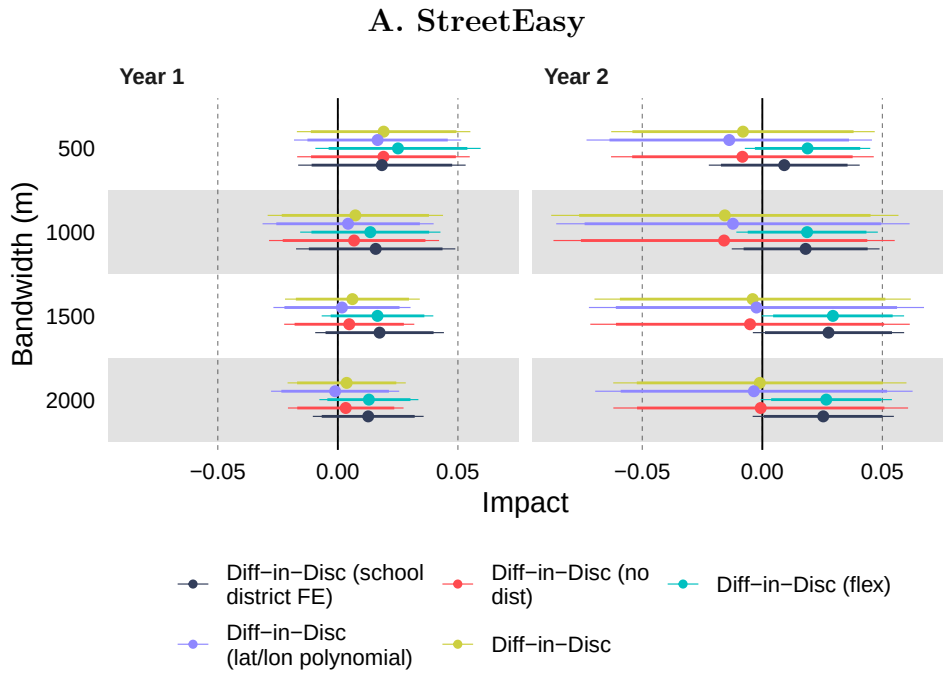
B.1 Listings data: additional robustness and heterogeneity results

Figure B.1: Impact of RTC rollout on length listed: robustness to bandwidth selection



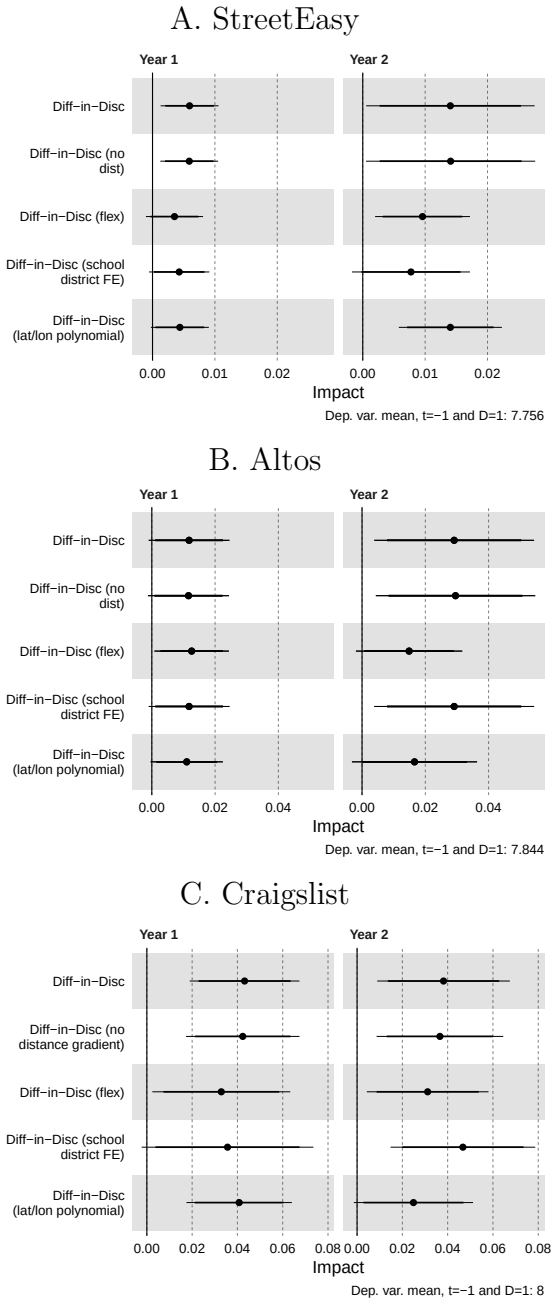
Note: This figure plots the difference-in-discontinuities estimates of the impact of RTC on length of listing for different bandwidths around the border (measured in meters) for the StreetEasy, Altos, and Craigslist data sets. "Year 1" compares listings created within 365 days of the rollout date to 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 4.2 describes the difference-in-discontinuities approach 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. The Altos sample is limited to listings without a gym or a pool, and in the bottom 90th percentile of the asking price distribution. In the regressions using StreetEasy data, we control for building age, year when 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. In the regressions using Altos data, we control for year when 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 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. We include both the 90% confidence interval (thicker line) and the 95% confidence interval (thinner line).

Figure B.2: Impact of RTC rollout on log assessed value per unit: robustness to bandwidth selection



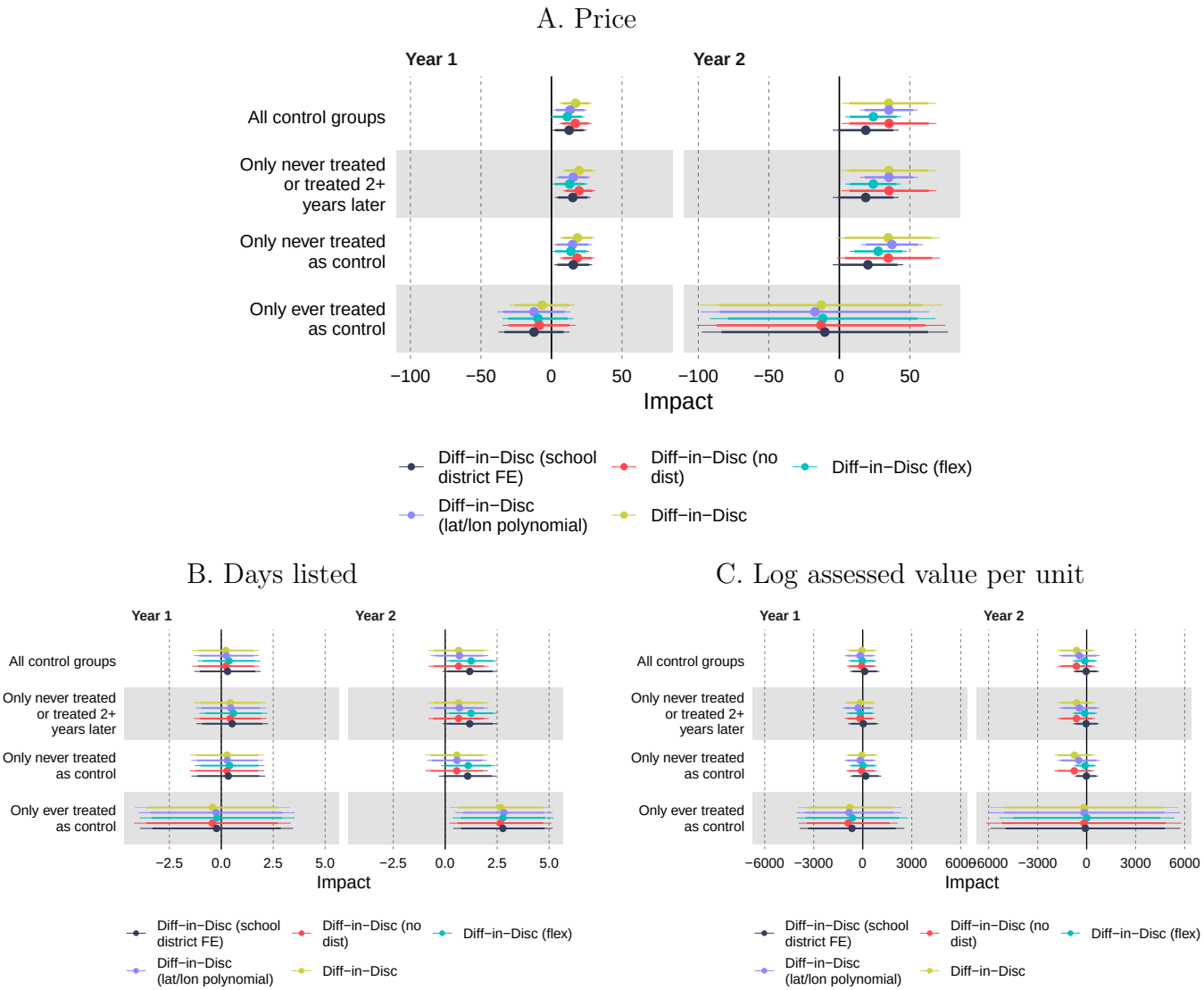
Note: This figure plots the difference-in-discontinuities estimates of the impact of RTC on listed rents for different bandwidths around the border (measured in meters) for the StreetEasy, Altos, and Craigslist data sets. "Year 1" compares listings created within 365 days of the rollout date to those 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 4.2 describes the difference-in-discontinuities approach 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. In the regressions using StreetEasy data, we control for building age, year when 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. We include both the 90% confidence interval (thicker line) and the 95% confidence interval (thinner line).

Figure B.3: Impact of RTC rollout on log rent



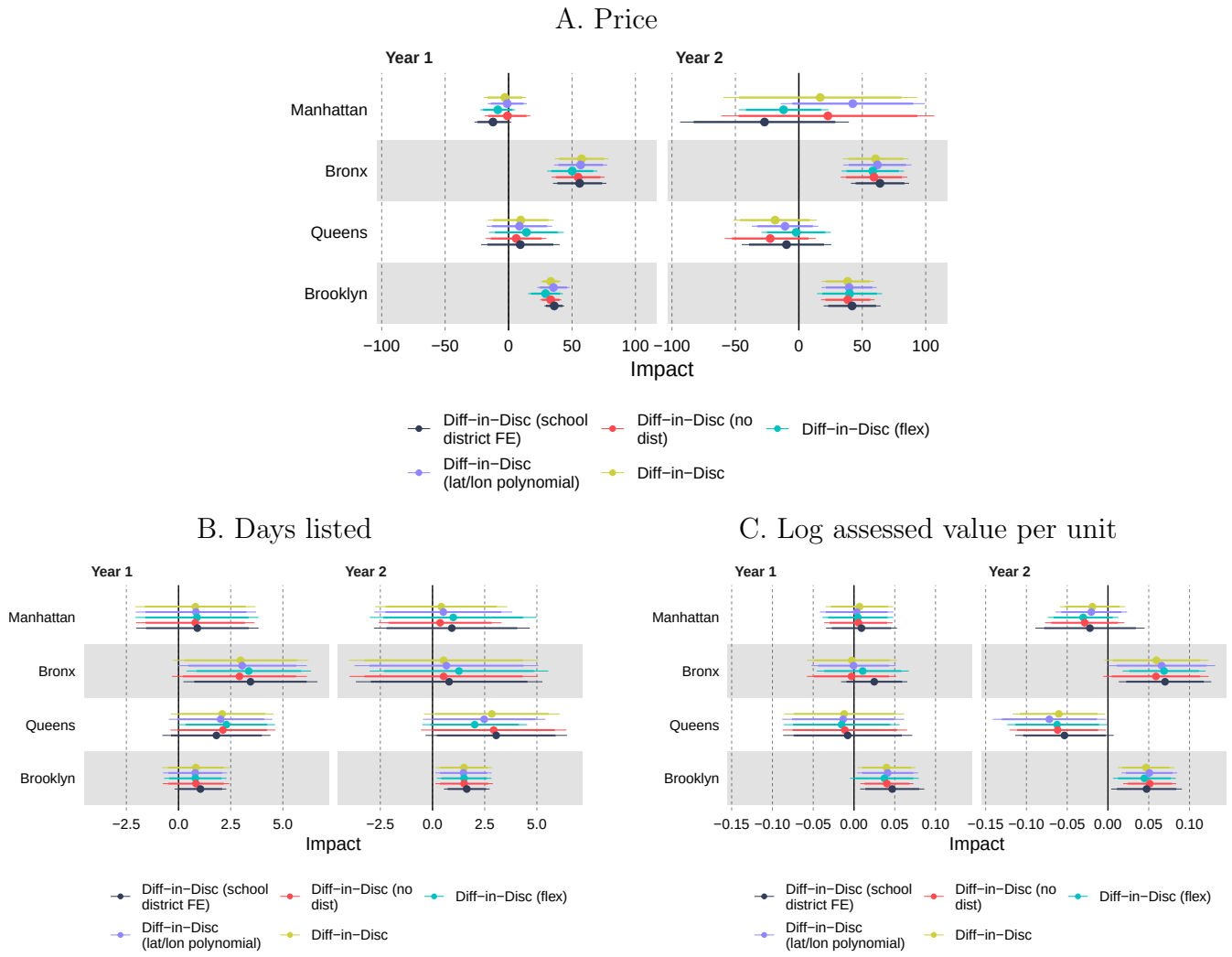
Note: Note: This figure plots the difference-in-discontinuities estimates of the impact of RTC on log prices. "Year 1" compares listings created within 365 days of the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. "Year 2" compares listings created between 365 and 730 days of the rollout date to those created at most 365 days prior to the RTC rollout in that border pair. Section 2.4 and Appendix Section A describe the data in greater detail. Section 4.2 describes the difference-in-discontinuities approach 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. The Altos sample is limited to listings without a gym or a pool, and in the bottom 90th percentile of the asking price distribution. In the regressions using StreetEasy data, we control for building age, year when 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. In the regressions using Altos data, we control for year when 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 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. We include both the 90% confidence interval (thicker line) and the 95% confidence interval (thinner line).

Figure B.4: Sensitivity to Choice of Control ZIP Codes: StreetEasy



Note: This figure plots the difference-in-discontinuities estimates of the impact of RTC on prices, days listed and log assessed value per unit using alternative choices of control ZIP codes. "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 4.2 describes the difference-in-discontinuities approach in greater detail. "All control groups" includes border pairs where the control side was never treated, treated one year later or treated two years later. "Only never treated or treated 2+ years later" excludes the border pairs where the control side of the border is treated the year after the treated side of the border. "Only never treated as control" additionally excludes the border pairs where the control side of the border is treated two years after the treated side of the border. "Only ever treated as control" estimates the impacts of RTC only for those border pairs where the control side is treated 1 or 2 years after the treated side. We drop listings in borders when both sides are treated. We use "no amenities" listings from StreetEasy, i.e. listings without central AC and in buildings without a gym, doorman or pool. We control for building age, year when 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. We include both the 90% confidence interval (thicker line) and the 95% confidence interval (thinner line).

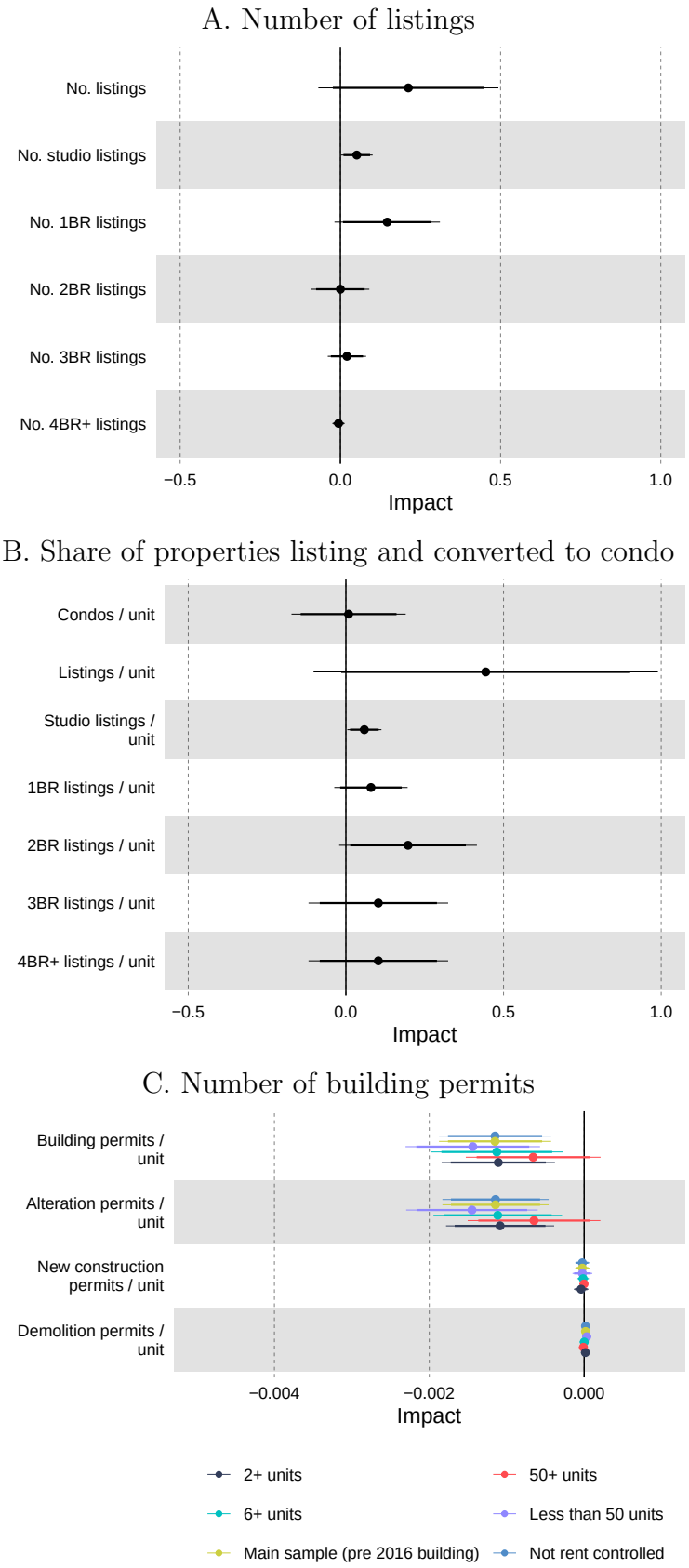
Figure B.5: StreetEasy: Impact of RTC rollout, by borough



Note: This figure plots the difference-in-discontinuities estimates of the impact of RTC on prices, days listed and log assessed value per unit for four out of five New York City boroughs. We exclude Staten Island due to small sample size. "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 4.2 describes the difference-in-discontinuities approach in greater detail. We use "no amenities" listings from StreetEasy, i.e. listings without central AC and in buildings without a gym, doorman or pool. We control for building age, year when 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. We include both the 90% confidence interval (thicker line) and the 95% confidence interval (thinner line).

B.2 Impact of RTC rollout: extensive margin

Figure B.6: StreetEasy: Impact of RTC rollout on extensive margin of housing supply



Note: This figure plots the difference-in-discontinuities estimates of the impact of RTC on the extensive margin of rental housing supply using PLUTO data (see Section 2.4 and Appendix Section A). We control for parcel-by-border and year fixed effects. Standard errors are clustered at the border-pair level to account for potential spatial correlation. We exclude buildings created after 2016 and buildings with zero residential units or residential area. We include both the 90% confidence interval (thicker line) and the 95% confidence interval (thinner line).

B.3 Additional welfare analysis results

Table B.1 explores sensitivity of our welfare conclusions to different datasets on which we estimate the rent effects, and to statistical uncertainty about the magnitude of those rent effects. If we use rent estimates from only the StreetEasy data, we find RTC's welfare effects for tenants are modestly positive, at $8.17 per month. Estimates from either Altos Research or Craigslist data, in contrast, suggest welfare effects are more negative than in our preferred "combined" sample: -$36.86 or -$31.11 per month instead of -$25.87 per month. Across columns of the table, we also explore sensitivity to statistical uncertainty in our rent estimates. As shown in column 1, if the true rent effect of RTC were one standard-error lower than our baseline estimate, we would conclude that RTC has a slight positive effect on tenant welfare.

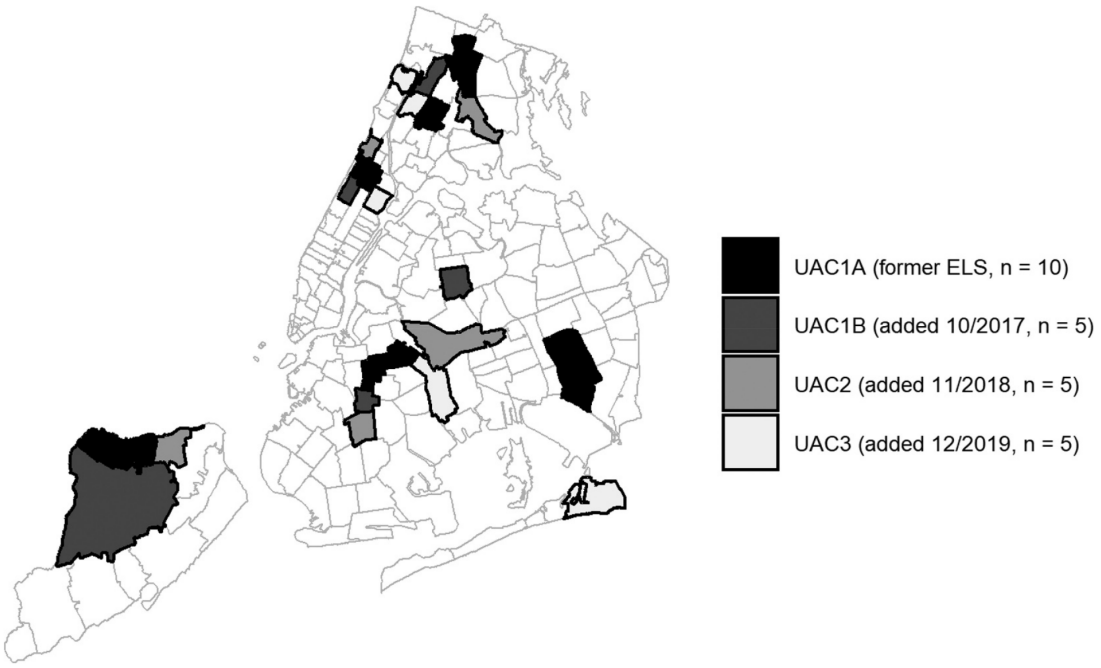
Table B.1: Tenant welfare impacts of RTC: robustness to sample and point estimate uncertainty

	Impacts on rent:		
	-1 S.E.	point estimate	+1 S.E.
Sample used for rent effects:			
Combined	3.21	-25.87	-54.95
StreetEasy	21.96	8.17	-5.62
Altos Research	0.56	-36.86	-74.29
Craigslist	2.51	-31.11	-64.72

Notes: Table reports estimated welfare impacts on tenants in $/month. Calculations assume relative risk aversion of gamma=2 and a $4,000 annual consumption floor. Eviction filing rates are a listing-weighted average of pre-2016 ZIP-code level filing rates in the relevant sample. Note that the combined estimates are an observation-weighted average of the dataset-specific estimates. Some listings may appear in multiple data sets, making it challenging to calculate the standard error of the combined data set. We take the observation-weighted average of the variances, which likely results in a larger standard error for the combined estimate than if the covariances between data sets were known.

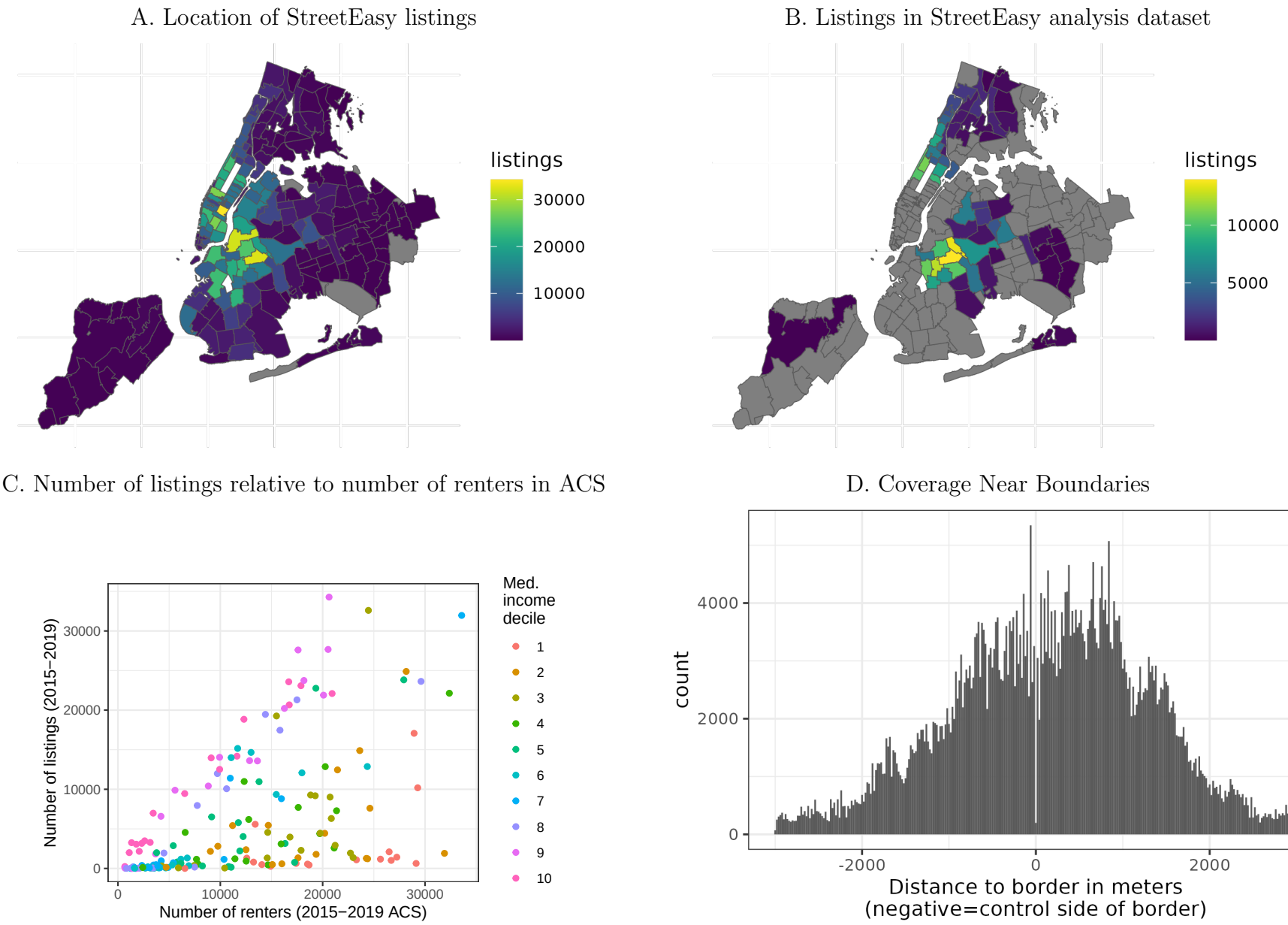
B.4 Details on the treated ZIPs and on coverage in listing data sets

Figure B.7: 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.

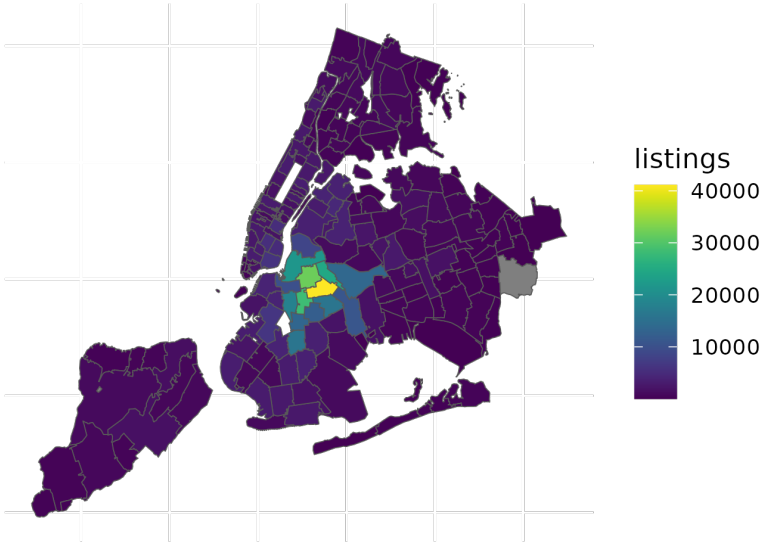
Figure B.8: StreetEasy Coverage



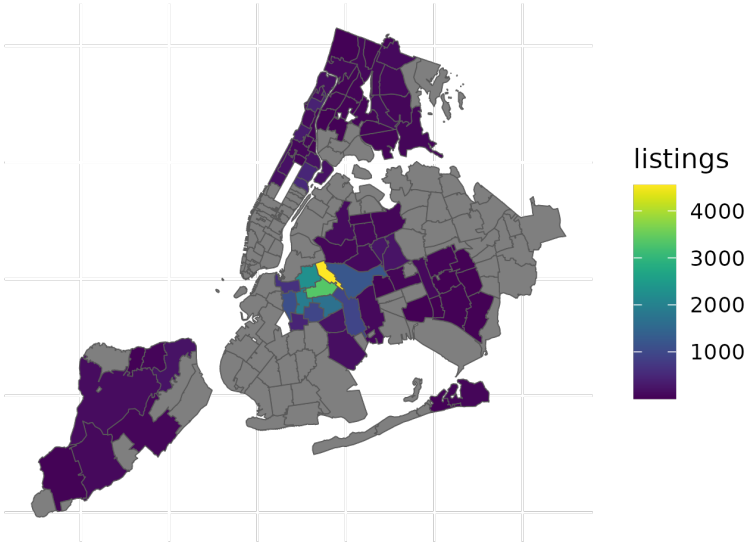
Note: This figure shows the coverage of the StreetEasy data. Panel A maps StreetEasy listings from 2015 to 2019. Panel B maps the number of "no amenities" StreetEasy listings by ZIP code, for the treated ZIP codes and their neighbors. Panel C compares the number of listings in each ZIP code to the number 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.

Figure B.9: Craigslist Coverage – Complete Addresses only

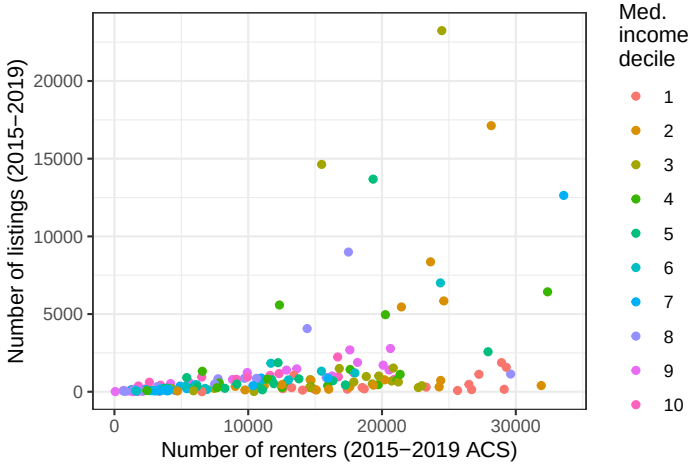
A. Location of Craigslist ads



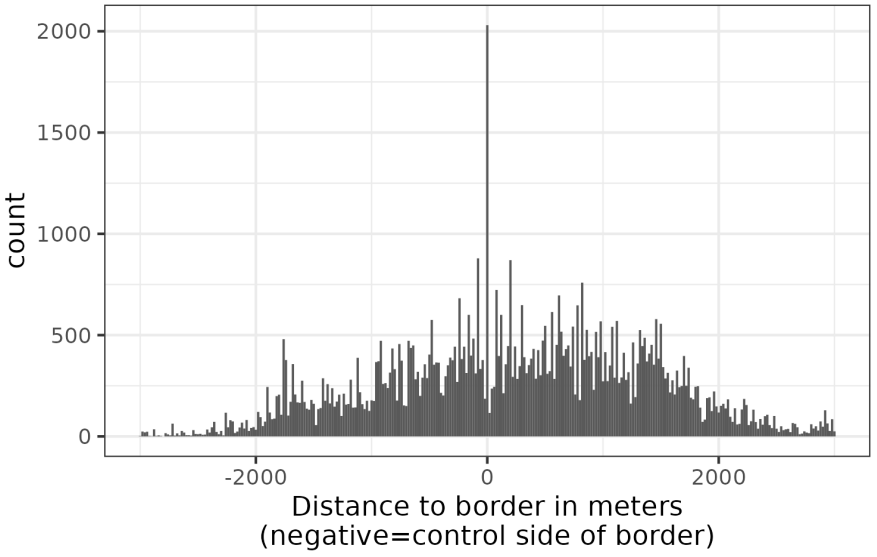
B. Ads in Craigslist analysis dataset



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

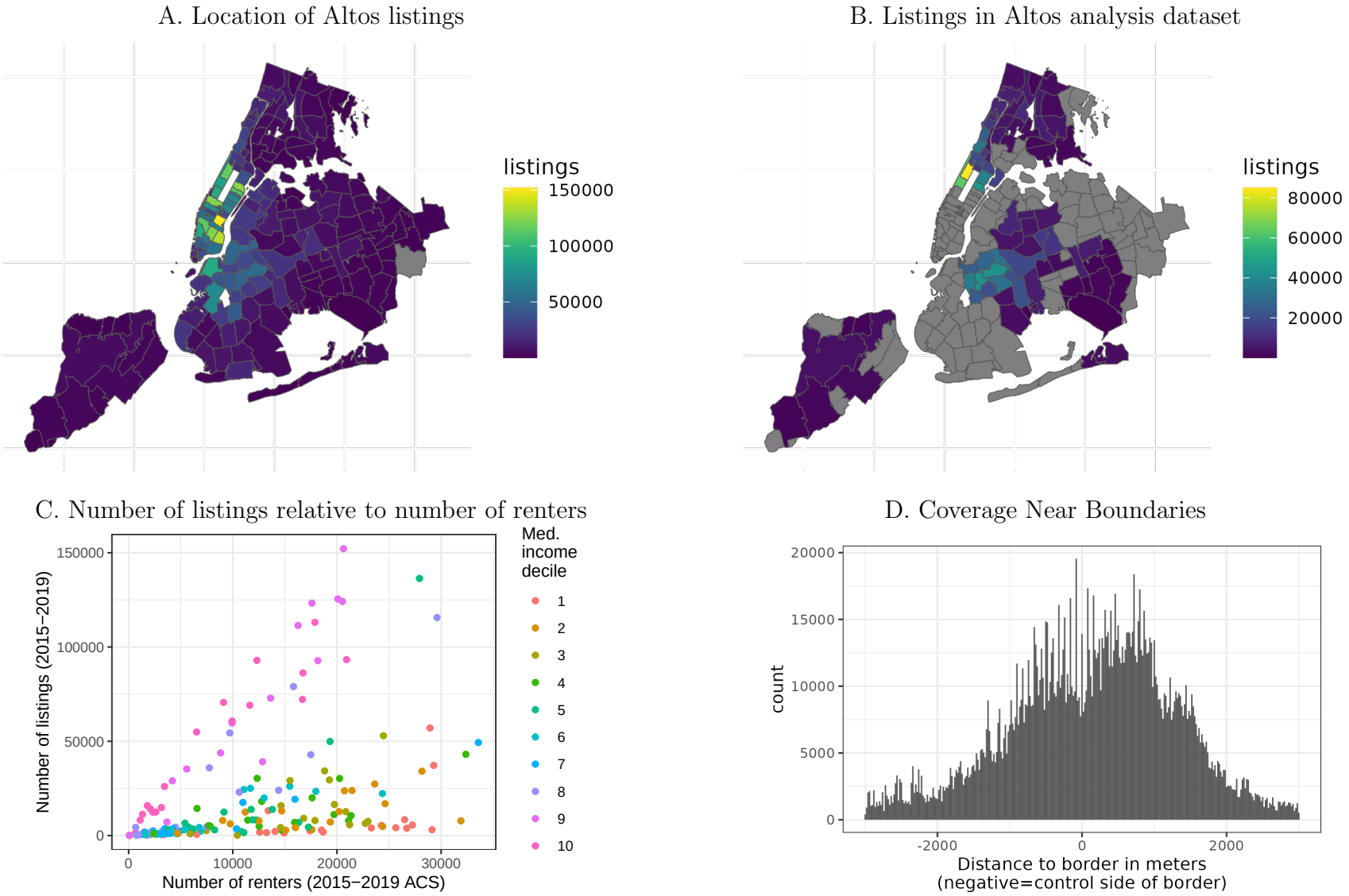


D. Coverage Near Boundaries



Note: This figure shows the coverage of our Craigslist data. Panel A maps Craigslist ads from 2015 to 2019. Panel B maps the number of Craigslist ads with complete address by ZIP code, for the treated ZIP codes and their neighbors. Panel C compares the number of ads 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.

Figure B.10: Altos Coverage



Note: This figure shows the coverage of our Altos analysis dataset. Panel A maps Altos listings from 2015 to 2019. Panel B maps the number of "no amenity" Altos listings by ZIP code, for the treated ZIP codes and their neighbors. Panel C compares the number of ads 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.

C Right to Council in Connecticut

Background Since 2000, landlords in Connecticut filed roughly 20,000 eviction cases every year, half of which took place in five of the state's largest cities. A report by the CT Data Collaborative (2022) shows that legal representation in eviction proceedings nearly doubles the likelihood of avoiding an order for possession in the state, but only 7% of tenants are represented by a lawyer in eviction court annually. The report finds the rates of removal orders more than halved during the COVID-19 pandemic as a result of temporary renter protections, "the disparity in outcomes between unrepresented and represented renters grew substantially, with those who were unrepresented 125% more likely to have removal orders issued" (CT Data Collaborative and Connecticut Fair Housing Center, 2022). In addition, the expiration of the COVID-era tenant protections lead to a surge in the number of cases, raising the eviction filing rate to pre-pandemic levels (CT Data Collaborative and Connecticut Fair Housing Center, 2022). Anticipating an increase in the state's housing crisis, the Connecticut General Assembly and Governor Ned Lamont created an eviction right to counsel program in mid-2021 to prevent worsening outcomes for households already burdened by the economic hardships of the pandemic (State of Connecticut, 2021).

The program The Connecticut Right to Counsel Program (CT-RTC) provides free legal representation to income-eligible tenants facing eviction or the loss of a housing subsidy. The program is administered by the Connecticut Bar Foundation and five legal services providers[18] are responsible for the delivery of legal representation to eligible tenants (Stout, 2022). The program was designed to be phased-in by zip codes across several cities throughout 2022 and 2023.

Tenant eligibility A tenant meets the income criteria if her household income is at or below 80% of the state median income adjusted for family size at the time of the request for representation or if she receives 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. Table C.1 shows the launch dates by each zip code in 2022. At launch, 14 zip codes were prioritized for implementation based on high eviction filing rates and availability of trained attorneys. However, three zip codes in Hartford were removed from the program due to capacity limitations after three months of operation (the city has the highest filing rate in the state and saw a 20% increase in eviction filings in 2022 compared to 2019). There was also a switch in zip codes in Bridgeport in an effort to gain increased case referrals. Later in 2022, the program was able to hire and train additional attorneys and the additional operational capacity afforded the addition of four new zip codes as seen in Table C.1.

[18] Providers in 2022 included Connecticut Legal Services, Connecticut Veterans Legal Center, Greater Hartford Legal Aid, New Haven Legal Assistance Association, Inc., and Statewide Legal Services. Each provider was assigned to serve a zip code in Connecticut, except for Statewide Legal Services and Connecticut Veterans Legal Center, who operate statewide.

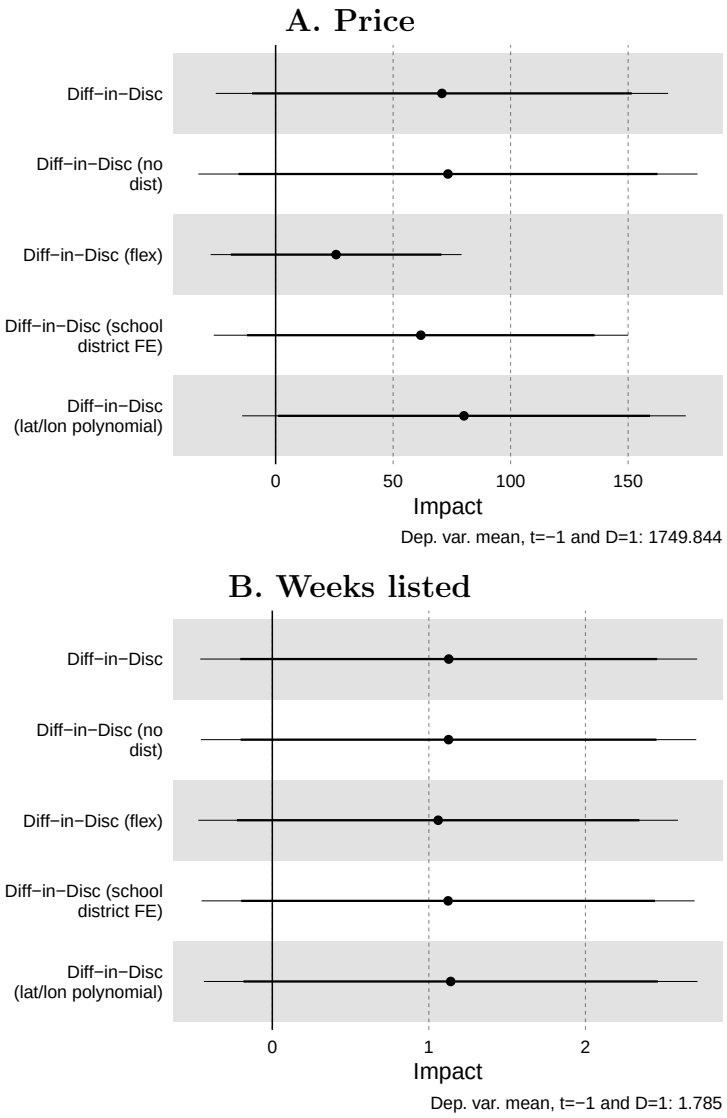
The impacts of right to council in Connecticut Figure C.1 shows the estimates on rent prices using the same specifications as in our NYC analysis in the main paper using Altos Research data. The top panel shows estimated impacts on rental prices, while the bottom panel shows estimated impacts on the number of weeks a unit was listed. All results are short-term, covering approximately the first six months after the start of the programs in each ZIP, and we have relatively few treated ZIPs. The point estimates on price suggest an increase in rent of between 25 and 60 dollar depending on the specification, though none of the estimates are statistically significant. For weeks listed, the point estimates suggest an increase of just over one week, though again estimates are not statistically significant.

Table C.1: Launch dates of zip codes in 2022

Zip code	City	Launch Date	Zip Code Status Notes
06110	Bridgeport	2022-01-31	Removed on June 13, 2022. Switched to 06605 to obtain more case referrals.
06605	Bridgeport	2022-06-13	Active
06239	Danielson	2022-01-31	Active
06105	Hartford	2022-01-31	Active
06106	Hartford	2022-01-31	Removed due to capacity limitations on April 28, 2022.
06112	Hartford	2022-01-31	Removed due to capacity limitations on April 28, 2022.
06114	Hartford	2022-01-31	Removed due to capacity limitations on April 28, 2022.
06120	Hartford	2022-01-31	Active
06457	Middletown	2023-06-12	Active
06051	New Britain	2022-07-19	Active
06511	New Haven	2022-01-31	Active
06513	New Haven	2022-01-31	Active
06519	New Haven	2022-01-31	Active
06320	New London	2022-03-24	Active
06260	Putnam	2022-01-31	Active
06902	Stamford	2022-11-22	Active
06702	Waterbury	2022-11-22	Active
06710	Waterbury	2022-01-31	Active
06516	West Haven	2022-01-31	Active. Only subsidy terminations.
06226	Willimantic	2022-01-31	Active

Notes: Data provided by the Connecticut Bar Foundation on June 21, 2023.

Figure C.1: Impact of CT right to council, 6 months, Altos



Note: This figure plots the difference-in-discontinuities estimates of the impact of right to council roll out on listed rent prices in Connecticut using Altos data. The sample restrictions and regressions follow those used for the evaluation of the UAC rollout in New York. The sample 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 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 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. We include both the 90% confidence interval (thinner line) and the 95% confidence interval (thicker line).

D Model derivations

Proof of Proposition 1. The solution to the housing choice problem in Equation 8 satisfies the first-order condition

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

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

$$
yeahmv'(h)F(y_0) + \int_{y_0}^{\hat{y}} \left[(m+f)v'(h) - f\frac{dR}{dh}u'(c) \right] dF(y) + \int_{\hat{y}}^{\bar{y}} \left(v'(h) - \frac{dR}{dh}u'(c) \right) dF(y) = 0 \,. \tag{13}
$$

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

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

$$
\begin{aligned}
= & \frac{dh}{dm} \underbrace{\left(mv'(h)F(y_0) + \int_{y_0}^{\hat{y}} \left[(m+f)v'(h) - f\frac{dR}{dh}u'(c) \right] dF(y) + \int_{\hat{y}}^{\bar{y}} \left[v'(h) - \frac{dR}{dh}u'(c) \right] dF(y) \right)}_{=0 \text{ by optimal housing choice (Eq. 7)}} \\
& + \int_{y_0}^{\hat{y}} \left(\underbrace{\left[\frac{df}{dm} + \frac{df}{dh}\frac{dh}{dm} \right] (v(h)-v(0)-Ru'(c))}_{=0 \text{ by optimal default (Eq. 6)}} + \left[v(h)-v(0) - \frac{dR}{dm}fu'(c) \right] \right) dF(y) \\
& + [v(h)-v(0)]F(y_0) - \frac{dR}{dm}\int_{\hat{y}}^{\bar{y}} u'(c)dF(y)
\end{aligned}
$$

$$
\implies \frac{dW(m)}{dm} \;=\; [v(h)-v(0)]F(\hat{y}) - \frac{dR}{dm}\mathbb{E}_y[fu'(c)] \;. \tag{14}
$$

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.