NBER WORKING PAPER SERIES

THE EFFECTS OF LEGAL REPRESENTATION ON TENANT OUTCOMES IN HOUSING COURT: EVIDENCE FROM NEW YORK CITY'S UNIVERSAL ACCESS PROGRAM

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Working Paper 29836
http://www.nber.org/papers/w29836

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 2022, Revised July 2022

We would like to thank Matt Desmond and Carl Gershenson at the Eviction Lab at Princeton University for generous guidance about eviction data; Annette Parisi and the staff at the Office of Court Administration of the New York State Unified Court System for providing the core data used in this paper, as well as for facilitating our understanding of it; and seminar participants at UC Berkeley, the University of Nebraska, and the University of Chicago, as well as several anonymous referees, for helpful comments. Any data provided herein does not constitute an official record of the New York State Unified Court System, which does not represent or warrant the accuracy thereof. The opinions, findings, and conclusions expressed in this publication are those of the authors and not those of the New York State Unified Court System, which assumes no liability for its contents or use thereof, nor do they necessarily reflect the views of the National Bureau of Economic Research.

The Effects of Legal Representation on Tenant Outcomes in Housing Court: Evidence from New York City's Universal Access Program
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NBER Working Paper No. 29836
March 2022, Revised July 2022
JEL No. I3,I38,K15,K4

ABSTRACT

Housing is one of the areas where it may be most critical for poor people to have access to legal representation in civil cases. We use the roll-out of New York City's Universal Access to Counsel program (UA) to assess the effects of legal representation on tenant outcomes, using detailed address-level housing court data from 2016 to 2019. The program offers free legal representation in housing court to tenants with income at or below 200 percent of the federal poverty guideline. We find that tenants who gain lawyers are less likely to be subject to possessory judgments, face smaller monetary judgments, are less likely to have eviction warrants issued against them, and are less likely to be evicted. Lawyers have larger effects in poorer places and in those with larger shares of noncitizens. Our results support the idea that legal representation in civil procedures can have important positive impacts on the lives of poor people.

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President Johnson created public defenders for criminal defendants as part of the War on Poverty in 1965. This development followed the 1963 Supreme Court decision (Gideon v. Wainwright 372 U.S. 335 (1963)**)** which established the right of indigent defendants in criminal cases to be represented by counsel at public expense. Yet despite calls for a "Civil Gideon," there is no similar right to representation in U.S. civil cases. The U.S. remains an outlier among wealthy democracies, which otherwise all guarantee access to lawyers in civil suits (Charn, 2013).

Housing is one of the areas where it may be most critical for poor people to have access to legal representation in civil cases. There are about 2.4 million eviction filings and 900,000 formal evictions in the United States annually, which implies that about one in 40 renter households are evicted every year (Eviction Lab, 2018). A 2021 report by the National Academies of Sciences (NAS) characterized the high eviction rate as a "looming crisis"---that is "not only a symptom but also a root cause of poverty" (p. 2). According to the NAS, evictions do more than exacerbate financial difficulties; they also impair health, undermine housing stability, and increase the risk of homelessness (National Academy of Sciences, 2021). Desmond (2017) provides an in-depth look at how evictions disrupt families and lead to a cascade of negative outcomes. Collinson and Reed (2019) provide rigorous empirical evidence that eviction leads to housing instability and homelessness in New York City, as well as to poorer health as reflected in emergency room visits. However, adverse pre-existing trends can also play a role: Humphries et. al. (2019) find that, in Cook County, Illinois, financial strain is more pronounced in the leadup to an eviction than afterwards.

Landlords almost always have legal representation while tenants usually lack it, and this imbalance may generate excessive housing instability from a social perspective. Yet, there is surprisingly little evidence that providing legal representation to tenants improves their outcomes in court. Naïve comparisons between tenants with and without lawyers are likely to be confounded by selection bias; a priori, it is not clear in which direction this bias might operate, as tenants with counsel may be better or worse off than average. As we discuss further below, two small-scale randomized trials of programs providing legal assistance to tenants in housing court produced mixed results. On a larger scale, Ellen et. al. (2020) observe that representation rates rose more in 10 New York City zip codes that were targeted by an early Expanded Legal Services initiative, than in other zip codes. They also document a weak negative correlation between this expansion of free legal services for tenants and eviction rates.[1]

We study the roll-out of New York City's Universal Access to Counsel program (UA), which became law in August 2017.[2] UA provides an offer of free legal representation in housing court to tenants whose income is at or below 200 percent of the federal poverty guideline. This legal assistance is provided by lawyers from non-profit agencies that contract with the city. With UA, New York became the first city in the United States to promise broad legal services to tenants. Since then, other U.S. cities have implemented similar programs, including Newark, NJ; San Francisco, CA; Philadelphia, PA; Santa Monica, CA; and Boulder, CO (Office of Civil Justice, 2020b; Been et al., 2018).

We use detailed address-level housing court records covering 2016 to 2019, the period of initial UA expansion, and examine a broad range of housing court outcomes in addition to

[1] While suggestive, Ellen et. al. (2020) caution that their results are "preliminary" and they do not claim causality, given that their analysis is correlational in nature. They compare zip-year changes in average outcomes between zips treated early and late, controlling for lagged changes in outcomes, and zip-level race and poverty measures. We study a larger and more comprehensive program using a within-zipcode framework (i.e. zip code fixed effects) and leveraging detailed case-level microdata (including case, property, landlord, and Census block group characteristics) in order to control for differences both between and within zip codes that could impact tenant outcomes. In fact, when we estimate analogues of Ellen et. al.'s (2020) specifications for our sample period, we find only a small association between legal service program expansion and representation rates, and essentially no association between program expansion and outcomes in housing court, emphasizing the threat of confounding in observational analysis. Moreover, using the introduction of the program as an instrument for representation, we can get at the more fundamental question of the impact of legal representation.

[2] New York City's Universal Access to Legal Service program (UA) was established by Local Law 136 of 2017, which was passed by the City Council as Intro 214-b and signed by Mayor Bill de Blasio on August 11, 2017 (Office of Civil Justice, 2018). The legislation is codified under the New York City Administrative Code Title 26, Chapter 13: Provision of Legal Services in Eviction Proceedings.

executed evictions. Our identification strategy exploits the gradual roll out of the program—which was introduced in targeted zip codes over a period of several years—to move beyond correlations and isolate the causal effects of legal representation. We estimate models that include detailed information about the Census block group and housing unit in addition to zip code fixed effects, so that causal inferences are made using within-zip code changes in access to legal representation under the UA program rather than comparisons across zip codes. We allow for heterogeneity in the effects of representation by neighborhood characteristics including race/ethnicity, citizenship, and poverty.

We find that increases in legal representation lead to better outcomes for tenants in housing court. Tenants with lawyers are considerably less likely to be subject to possessory judgments, face smaller monetary damages, are less likely to have eviction warrants issued against them, and are ultimately less likely to be evicted. . Legal representation has the largest effects in poorer places and in those with larger shares of noncitizens, suggesting that a program targeting these areas could have even larger impact per dollar spent than one with universal ambitions. More generally, our results support the idea that legal representation in civil procedures can have an important positive impact on the lives of poor people.

The rest of the paper proceeds as follows. Section 1 provides some background about prior research on legal representation in housing court, about the way that New York's housing courts work, and about the roll out of the representation program we study. Section 2 provides information about the data on housing court cases, housing, and area-level characteristics. Section 3 describes our empirical methods and Section 4 provides the main results. Section 5 provides a discussion and conclusion.

1. Background

1a) Prior Research about the Effects of Legal Representation in Housing Court

The UA program did not change the law regarding when and why tenants can be evicted. But it aimed to level the playing field by furnishing tenants with the same access to professional legal representation that landlords typically enjoy. There are many possible benefits of legal representation in housing court. The process is technical and labor-intensive. Proceedings can be fast-paced and intimidating. Identifying persuasive legal defenses and negotiating favorable settlements requires expertise. And parties may be required to make repeated visits to court, running the risk of forfeit for failure to appear at each iteration. Even in a loss, skillful attorneys can buy time and concessions for clients.

Still, it is not obvious that representation will actually be effective in improving tenant outcomes. Having a lawyer does not necessarily address the underlying problems that lead a family to end up in housing court (Humphries et. al., 2019). It is conceivable that having a lawyer may only delay the inevitable, possibly by only a few weeks or months.

Poppe and Rachlinski (2016) provide a detailed review of the literature about the effects of legal representation on the outcomes of civil cases. Most studies are observational. These studies usually find pro-tenant effects of tenant representation, though one study in New Haven did not find any effect. However, as Poppe and Rachlinski (2016) point out, observational studies may be biased due to non-random selection into the use of counsel: Tenants who are more likely to win their cases may be more likely to be represented. For instance, they might live in areas with more lawyers willing to work pro bono, have cases that are more appealing to lawyers, or be better able to afford a lawyer. On the other hand, there may be negative selection if, for example, tenants facing more significant suits are more likely to seek professional representation.

There have been a few randomized trials of legal representation in civil procedures. Greiner and Pattanayak (2012) look at representation for people denied unemployment benefits, and find no effect on the probability of success and a delay in the adjudication of the case. Turning specifically to housing cases, an experiment reported in Seron et al. (2001) involved a comparison of 134 treatment tenants who received access to pro bono lawyers when they arrived at housing court compared to 134 controls who did not. This evaluation found very positive effects of representation for tenants, with represented tenants being about half as likely to be evicted compared to unrepresented tenants. However, these results might not generalize to UA: the sample was small, the program provided pro bono representation from private firms, and the experiment took place more than 20 years ago. Greiner et al. (2013) assess an experiment in a Massachusetts housing court in which treated clients received full representation and control clients only received more limited legal services. This experiment found that full representation helped tenants.

On the whole, these past investigations suggest that representation could lead to better outcomes for clients, but since each program is different, it is important to assess an actual program at scale rather than extrapolating from two small demonstrations.

1b) Housing and Housing Cases in New York City

Housing issues are top of mind in New York City given that 68.1 percent of households rent, compared to 35.9 percent nationwide (NYU Furman Center, 2020). New York's Civil Courts have created special housing courts to deal with conflicts between tenants and landlords. There is one court for each of the five boroughs (i.e., counties) and two additional smaller special courts in Harlem and Red Hook.

Most housing court cases (93 percent) are eviction petitions initiated by landlords, and that is our focus here. Of these, most involve nonpayment of rent (86 percent in fiscal year

2019) (Office of Civil Justice, 2019b). All other types of cases (involving things like violations of the lease or overstaying the end of a lease) are referred to as "holdover" cases. In fiscal year 2019, 209,995 residential eviction petitions were filed and 81,297 eviction warrants were issued (Office of Civil Justice, 2019b). City Marshals executed 20,013 evictions in calendar year 2019 suggesting that only about a quarter of warrants are formally executed (Office of Civil Justice, 2020b).

Eviction cases proceed as follows: First, the landlord must provide the tenant with notice of intent to file a case. In nonpayment cases, tenants must be given written demands for overdue rent 14 days prior to filing Once the case is filed, tenants have 10 days to respond or "answer." Once the answer is received, a trial date is set, usually three to eight days after receipt of the response.[3] Tenants may have to appear multiple times, including for Orders to Show Cause, notices of motions, and other hearings of various sorts. It is common for tenants to forfeit cases either by failing to answer or by failing to appear at what could be one of many mandatory appearances in court. In this event, a landlord can apply for a default judgment including back rent and a warrant of eviction (NYC Housing Court, 2022). Hence, having a lawyer who can repeatedly appear as the tenant's representative is one important way that legal representation could help tenants.

Nonpayment cases automatically end (and any pending warrants are vacated) upon tenant repayment of rent owed. At any stage in the process, tenants may also leave on their own and such outcomes are not observed in the court data. Hence, many observers feel that formal evictions substantially understate the number of moves precipitated by housing court filings, judgments, and warrant issuances (Office of Civil Justice, 2016; NYU Furman Center, 2019;

[3] In holdover cases, predicate notices are more varied and depend on the nature of the case, but entail similar notification periods; calendaring is typically automatic and answering takes place at the hearing.

NYC Housing Court, 2022). In some cases, there is no further action observed in the housing court data beyond the initial filing which may indicate some other resolution of the case.

There are a number of possible outcomes in cases that move forward in court (Office of Civil Justice, 2016; NYU Furman Center, 2019; NYC Housing Court, 2022). First, the parties could come to an agreement before seeing a judge. In the absence of tenant representation, such settlements, or "stipulations," are usually the result of a hasty "conference" between the tenant and the landlord's lawyer in a corridor at the courthouse. These settlements are then codified by a judge in a formal judgment. These negotiated stipulations are the most common form of judgment in housing court. Hence, providing legal representation to tenants in these hallway discussions could potentially have an important impact by, for example, making the tenant less likely to accede to landlord demands without first having a court hearing.

If the case proceeds to a (non-jury) trial[4] there could be a postponement, a dismissal, a discontinuance (i.e. a formal determination that the case will not proceed), or a judgment. Judgments can include monetary awards (e.g. back rent) or the issuance of a warrant of eviction. As a rule of thumb, judgments in cases filed by landlords, including those that reflect settlements, are unfavorable for tenants.

1c) Implementation of the NYC Universal Access to Counsel Program

The law creating UA is administered by the Office of Civil Justice (OCJ) within the NYC Department of Social Services (DSS, also known as the Human Resources Administration; HRA). While the law is sometimes referred to as "right to counsel" (RTC), it does not provide residents a due process right in the constitutional sense of Gideon, but rather imposes *on OCJ* the obligation, subject to appropriation, to provide lawyers in housing court to qualified tenants. The law specifies that UA providers must be not-for-profit legal services organizations. As of

[4] Housing court cases are conducted as summary, or simplified, proceedings.

FY2020, the City held contracts with 15 UA providers (Office of Civil Justice, 2020a). The attorneys and paralegals who work for these providers are more similar to public defenders than to the private lawyers who provided pro bono advice in some previous studies (Been et. al., 2018).

Under the law, all tenants, regardless of income, are entitled to “brief'” legal assistance, consisting of a single individualized consultation with provider staff. Households with income less than 200 percent of the poverty level are entitled to “full,” ongoing, legal representation including: Client consultations; legal advice and research; construction of a defense; preparation and filing of court documents; negotiation with landlords and their attorneys; and representation at hearings, trials, and appeals. These services are to be provided starting no later than a tenant's first scheduled court appearance (NYC Human Resources Administration, 2014).

In practice, services almost always begin at the tenant’s first scheduled court appearance: Court staff screen tenants and refer them to contracted providers, who operate on-site within the courts (Been et. al., 2018; Ellen et. al., 2020). Importantly, this first appearance occurs subsequent to a tenant answering the initial petition, which means that a tenant who never responds and never appears in housing court at all will not be represented by UA. Nevertheless, as we will see, UA can and does help to prevent failure to appear for subsequent court proceedings in the case.

Implementation has been phased in by cohorts of target zip codes (Office of Civil Justice. 2018). The first cohort of 10 zip codes (two in each borough) were grandfathered from UA's predecessor, Expanded Legal Services (ELS), which operated as early as 2016 (we refer to this as the FY16-17 cohort). The City's stated criteria for targeting zip codes included: “shelter entries from the zip code; prevalence of rent-regulated housing; the volume of eviction proceedings; whether the area is already being served through other legal services programs; and other factors of need” (Office of Civil Justice, 2017). But one additional criterion appears to have been the necessity of serving zip codes in all five boroughs.

Following the passage of the UA law in August 2017, the City added cohorts of five zip codes each (generally one per borough) during each succeeding fiscal year[5]for a total of 25 zip codes served (FY16-17, FY18, FY19, and FY20 cohorts) out of 180 by the last year of our data.[6] The original mandate was to serve the whole city by July 2022. However, in part due to the significant changes to housing court processes caused by the COVID-19 pandemic, UA went citywide by June 1, 2021, a year ahead of schedule (Office of Civil Justice, 2021). We exploit the roll out of UA to identify the causal effects of legal representation.

Figure 1 shows a map of New York City zip codes, where the intensity of the blue shading indicates the median income in the Census block group. Target zip codes from the first four UA cohorts are outlined in red.[7] The figure suggests that the zip codes chosen for UA had among the lowest median incomes (lightest shading) in each borough. Figure 2 shows a similar map with the number of housing court cases per 1,000 rental units (averaged over our sample period). The map shows that while some zip codes with the highest caseloads were targeted by UA, others with similarly high rates were not targeted. In what follows, we include zip code fixed effects in all of our models so that the effects of the program are identified by variation within zip code rather than comparisons across zip codes. Consequently, differences between target and non-target zip codes will not drive our estimates of the effects of the program.

[5] The City's fiscal years run from July 1 to June 30 and are named for the calendar years in which they end.
[6] However, as described below, no zips in the FY20 cohort had actually been treated by the end of our sample period in June 2019.
[7] To draw these maps, we augment our geocoded housing court data set, described in Section 2, with shapefiles from the Department of City Planning and the Census Tigerline, and Census crosswalk files between census tracts and zip code tabulation (ZCTA) areas.

Residing in a target zip code was neither necessary nor sufficient for a tenant to be served by the program. Qualifying households in target zip codes were guaranteed representation but could decline it. And as discussed above, tenants who failed to answer a petition at all would not have the opportunity to be offered services. Tenants in non-target zip codes could also be served by UA if sufficient resources were available. In fiscal years 2018 and 2019, the city reported on the number of tenants served in each zip code regardless of whether the zip code was one of the target UA areas. Figure 3 shows that while target zip codes had the greatest number of cases served by UA, there were also relatively high numbers of cases served by UA in adjacent zip codes. We will exploit this source of variation in addition to the roll out. The rate of UA representation per cases filed is shown in Figure A.1.

Two other significant developments have affected evictions in NYC since the UA program was introduced. First, the New York State Housing Stability and Tenant Protection Act (HSTPA) of 2019 made major changes to the state's rent stabilization system and also introduced new provisions designed to protect tenants from eviction (NYS Homes and Community Renewal, 2020). Given this major change in the law governing evictions, we limit our sample to cases filed prior to June 14, 2019, the date the law took effect. The State also made major changes to laws governing eviction in response to the COVID-19 pandemic, but given our cutoff date of cases filed before June 2019, these are not relevant to this study (Office of Civil Justice, 2020b).

The impact of the UA rollout is illustrated in Figure 4, which graphs smoothed representation rates by priority cohort for each New York City borough. Bold shading denotes UA treatment, with start dates estimated using the algorithm discussed in Section 2 and more fully described in Appendix A.1.   In brief, a borough-cohort’s UA start date is defined as the month in which the rate of change in the smoothed tenant representation rate is at least one percentage point greater than the prior month's change and the nine-month change in representation for the period beginning with that month is nine percentage points or more. For cohorts with more than one such month, we break ties by choosing the candidate start month with the largest nine-month percent change in representation. Appendix Table A.1 lists the zip codes included in each cohort and Table A.2 gives the empirical UA start dates for each cohort.

We use this procedure in order to zero in on implementation dates that are more precise than just knowing the fiscal years in which zip codes were slated to receive treatment. Notably, identifying empirical implementation dates allow us to measure the effects in zip codes where the program was actually implemented, as it is clear that several target zips were, in fact, not meaningfully treated.

The first panel shows that, in January 2016, the average share of tenants with representation was quite low (about 4.6 percent) across all zip codes in Bronx County. There was a sharp increase in representation in the first cohort to be treated (the Fiscal Year 2016-2017, or FY16-17, cohort) beginning in December 2016, and rising to about 25 percent of cases by April 2017. The next cohort (FY18) shows a sharp jump in representation in January 2018, followed by the FY19 cohort in September 2018. There are no similar increases in representation in the non-target zip codes, or in the FY20 cohort, which had yet to be treated as of June 2019.

The remaining panels show similar patterns for the other four boroughs. A few comments are in order. First, in Manhattan the FY16-17 cohort has a relatively high rate of representation throughout the period, with no sharp change. We treat this cohort as untreated by UA, but given the inclusion of zip code fixed effects would obtain identical results if we labeled it as always treated.

In Kings County (Brooklyn), shown in the second panel, initial rates of representation were higher in the first cohort to be treated (FY16-17), but one can still see a sharp rise from about 20 percent to about 34 percent following the introduction of UA in the second half of 2016. In the FY18 cohort, the representation rate rose from about 10 percent to over 30 percent after the introduction of UA in mid-2017, but there does not seem to have been any implementation in the FY19 cohort. Again, rates of representation are low in the non-target and not-yet-treated (FY20) zip codes.

In New York County (Manhattan, shown in the third panel), rates of representation were already relatively high in the FY16-17 cohort, and rise slowly over time suggesting that UA did not have a dramatic impact in these zip codes. By contrast, in the FY18 and FY19 cohorts, clear pivot points and sharply rising rates of representation are visible. As in the Bronx and Brooklyn, the non-priority zip codes show no increase in representation nor do the not-yet-treated FY20 zip codes. Queens county is also anomalous in that the effect of UA is only apparent in the FY19 cohort, in which rates of representation rise rapidly from about 13 percent to 25 percent beginning in July 2018. Due to this anomaly, we repeat our main estimates excluding Queens as a robustness check. Finally, Richmond County (Staten Island) is smaller than the other boroughs so that the pattern is somewhat bumpy. Nevertheless, there were sharp increases in representation, to around 50 percent in the FY16-17, FY18, and FY19 cohorts, with no change in representation rates in the non-target zip codes.[8]

"Effective" program take-up in target zip codes was considerably higher than these graphs suggest because not all tenants were equally exposed to the program. New York City

[8] Staten Island is has a smaller population than the other boroughs, which explains some of the bumpiness in the graphs. There is no FY20 cohort in Staten Island.

Housing Authority (NYCHA) tenants were not represented by UA.[9] And since the UA offered tenants representation when they arrived in housing court, UA was unlikely to serve cases that had no activity beyond the initial filing or in which the tenant never answered the petition and hence never showed up in housing court. Appendix Figure A.2 repeats Figure 4 excluding these groups of tenants and shows that among those likely to be offered representation, take-up rises after implementation to peaks ranging from a little less than 50 percent in the Bronx to 80 percent in Staten Island.

These graphs show that the program had a much greater impact in some target zip codes than in others, likely due to heterogeneity in housing court personnel and legal services providers across boroughs. Tenants in some zip codes may also have been served by pre-existing city programs or by pro bono private attorneys. However, the combined impact of pre-existing programs was small: For instance, in fiscal year 2013, the budget for tenant legal services was only $6 million compared to $113 in fiscal year 2020 after the implementation of UA (Office of Civil Justice, 2019a). One can also see that, with the exception of Queens, there are no general upward trends in representation in non-target zip codes between 2016 and 2019.

The Office of Civil Justice views the UA program as having been very successful. Evictions carried out by marshals decreased from 28,849 in 2013 to 16,996 in 2019 (Office of Civil Justice, 2020a) while the number of eviction petitions filed decreased from 246,864 in 2013 to 171,539 in 2019. However, as Ellen et al. (2020) point out, evictions had been on a declining trend in both UA and non-UA zip codes since 2011, so the extent to which UA deserves credit

[9] Of the 22,000 households who received full legal assistance from UA in FY2019, just 266 were NYCHA tenants (Office of Civil Justice, 2019b) even though NYCHA is the landlord responsible for the greatest share of eviction filings in the city. When we estimated models separately for NYCHA and non-NYCHA addresses, we found statistically significant effects only in the non-NYCHA units.

for the decline is an open question. It is also of interest to look at a wider array of outcomes and into possible heterogeneity in the effects of the program.

2. Creating the Data Set

Our main source of data is individual Housing Court records from the Civil division of the New York State Unified Court System.[10] These data have full property addresses but no other personally identifying information, and cover all cases filed between 1/1/2016 and 6/14/2019, though we observe the progress of cases through 1/25/2021. Considering only cases filed through 6/14/2019 allows us to abstract from effects of the Housing Stability and Tenant Protection Act and also means that we observe all cases for a minimum of nine months before the COVID-related pause in evictions proceedings that started in March 2020. The median time to first judgment (for cases that receive a judgment) in our main sample is 49 days, and 95 percent of cases with judgments receive them within 199 days so that there is little right censoring of cases in our data.

The unit of observation is the individual case. Each record includes case identifiers (e.g., exact property address, court, filing date), whether the case is active, whether each of the parties have legal representation, and events such as appearances, motions, decisions, and judgments with their associated dates. Information on judgments includes whether a warrant of eviction was ordered, issued, and executed, as well as any monetary amounts awarded.[11] Some of the other variables that we control for include indicators for: type of case (nonpayment or holdover, whether the landlord has a lawyer, whether the landlord is NYCHA, and whether the case has a

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[10] Specifically, housing court data come from the "Customized Statewide Landlord and Tenant (LT) Data Extract," which is derived from the Office of Court Administration's Universal Case Management System for Local Civil Courts (UCMS-LC).

[11] Because these data are maintained for administrative purposes, the raw data requires extensive processing. In particular, the data come in complex nested XML extracts which must be flattened, parsed, and summarized. One challenge is that the number of fields associated with a case varies with the complexity and length of a case. For example, there may be as few as zero and as many as 19 judgments in a case. For most fields, we keep the first and last entry in each field. We also generate count variables (e.g., number of judgments).

“specialty designation” (e.g. a flag indicating that the building is a co-op). We also control for the (log) primary monetary claim against the tenant; counts of respondents and petitioners; court fixed effects (dummies for each county court and the two specialized courts); and borough-by-month fixed effects to flexibly control for idiosyncratic period effects and time trends within each county. After cleaning and standardization, 95 percent of the housing court addresses were successfully geocoded using the NYC Department of City Planning's GBAT desktop application.

The court data is then linked via address to two other data sources. The first is the Department of City Planning’s Primary Land Use Tax Lot Output (PLUTO) database, version 21v1 (February 2021), at the borough-block-lot level. PLUTO is based on administrative records maintained by the Department of City Planning, the NYC Department of Finance (DOF) and other City agencies. The data from PLUTO is used to create detailed controls for the type of property including: year built, assessed total value; lot area; built floor area ratio; number of units; zoning district type (low, medium, or high residential use; other), and land use type (1-2 family homes, multi-family walkup, multi-family elevator, mixed residential and commercial use, other); an indicator for whether it is a single building or part of a complex; an indicator for whether there has been a building alteration; and an indicator for whether the unit is rent-stabilization eligible. In summary, we have very detailed information about the housing unit itself which help to proxy for landlord and tenant characteristics.

We also construct the following landlord-level controls from these PLUTO data, which include a landlord identifier: The number of NYC properties owned by the landlord, the number of NYC buildings owned by the landlord, the number of NYC residential units owned by the landlord, the number of housing court cases the landlord is involved in (during our sample period), housing court cases per number of residential units, and the total assessed value of properties owned.

Second, to impute basic demographic information, the records are linked to the American Community Survey's 2019 Five-Year estimates of census block group characteristics. The models include a vector of census block group characteristics capturing total population, median household income, household poverty rate, total housing units, renter share of housing units, median gross rent, and population shares that are Hispanic, Black, Asian, White, ages 0-17, ages 65+, and female, as well as census tract shares of noncitizens and naturalized citizens.[12] In several analyses, census block groups are characterized using a series of zero-one indicators for whether the block group's majority race/ethnic group is Hispanic, Black, non-Hispanic White, or Asian.

All continuous covariates from the ACS and PLUTO are transformed into a series of indicators for whether the address is in the lowest to highest quartile, *calculated from the distributions within our main sample* (e.g. the "fourth quartile of the CBG poverty rate," refers to the 25 percent of housing court tenants whose CBG poverty rates are the highest in our sample). We also include indicators for missing categorical variables. Note also that, given the nature of these datasets, variables deriving from PLUTO or the ACS are observed at single points in time (2019 for the ACS and 2021 for PLUTO), and so vary within zip code but not over time.[13] Further information about variable definitions appears in Appendix A.2.

Several limitations are imposed on the raw data to refine the sample of cases for analysis. Starting from the sample of all 863,239 housing court cases filed between 1/1/2016 and 6/14/2019, the universe of cases is restricted to landlord-initiated residential eviction petitions, about 89 percent of total filings. Second, the small number of cases where the property in

[12] Block groups are the smallest geographical level available in the published data. New York City has 6,493 block groups, each with an average of 483 households and 1,297 people. Citizenship data is not available at the block group level.
[13] Variables measured in dollars are in 2019 dollars for the ACS and 2021 dollars for PLUTO.

question does not properly geocode are dropped (in the full sample, the geocoding success rate is 95.1 percent). Third, potential duplicate filings are removed from the data.[14] Together, these restrictions leave us with 727,692 cases in the main sample.

We also construct a subsample of cases from the first three UA zip code cohorts to use in a regression discontinuity analysis. This sample focuses on cases that were filed within plus or minus 10 months of the empirical UA ramp-up month for each cohort. A "donut" of plus/minus one month around the empirical start month is excluded to allow for the fuzziness of empirical UA start dates. There are 36,856 cases in this subsample.

2b. Defining the Treatment and Instrumental Variables.

The main explanatory variable of interest is "respondent counsel" a 0/1 indicator for whether a tenant has professional legal representation.[15] The effects of counsel on tenant outcomes are likely to be confounded by selection bias. For example, tenants may only seek representation when they face especially bad outcomes, in which case the raw association between legal representation and adverse outcomes would be negative. Or it may be that the most affluent or savvy tenants retain lawyers, in which case selection bias would operate in the opposite direction.

Given this concern, we use two different instruments for individual tenant representation. The first is the "empirical UA treatment" indicator we describe in Section 1C; this is a 0/1 indicator for whether the UA program is operating in a particular target zip code at the time of the initial case filing. The identifying assumption is that UA affects the probability that a tenant has representation, but has no effect on outcomes other than through that channel. Because all

[14] We keep only the most active filing per address in each two-week period, on the assumption that multiple filings within a two-week span represent administrative or procedural error.

[15] There are two other possibilities for tenant representation status: self-represented litigant (SRL) and no appearance. Both reflect the absence of an attorney.

specifications include zip code fixed effects, identification is not based on a comparison of these zip codes with other zip codes, but on the timing of the introduction of the program within each zip code, as discussed above. The staggered implementation of UA across boroughs and zip code cohorts, such that only certain zip codes were affected during each fiscal year makes it unlikely that the effects of UA could be confounded with those of other policies.[16] Borough-by-month fixed effects are also included in order to absorb the effects of any borough-specific policies, trends, or otherwise confounding shocks.

Because some tenants living outside target zip codes also receive representation under the UA program, we estimate a second set of models using data on the number of households in each zip code that received UA representation during each fiscal year (divided by 1000 for interpretability). These models are estimated using data from fiscal years 2018 and 2019, since these are the only sample years with published DSS information about the number of tenants served in each zip code.

One potential issue is that landlords might change the types of cases that they bring against tenants following the introduction of the program. In this case, the estimated effects of representation in court might reflect changes in the way that cases that go to court are selected. The rich data described above allows us to control for detailed characteristics of the housing units, and to ask whether there are any significant changes in the observable characteristics of the cases that are filed before and after the introduction of the program. We do not find significant differences, which provides evidence in support of the identification assumptions underlying the instrumental variables estimates of the effects of legal representation on tenant outcomes. We

[16] When defining the UA instrument, we use the case's property zip code of record, as entered in the OCA data. For 1.3 percent of the main sample (9,313 cases), the zip code of record is different from the geocoded zip. We rely on the zip code of record on the grounds that this is the information that the Court and Department of Social Services uses to refer tenants to UA providers.

also show that reduced from models demonstrate significant program effects, which is of independent interest for other jurisdictions considering the adoption of programs to expand access to attorneys in civil cases. Finally, we estimate models that include fixed effects for each address—down to the apartment unit number—in which the effects of UA are identified by the subset of approximately 60,000 addresses that had cases filed both before and after UA.

2c. Defining the Outcome Variables

In what follows, we focus on four main tenant outcomes:[17]

Judgment with Possession: a 0/1 indicator for whether the final judgment in a case is possessory, meaning that it grants the landlord the possession of the property. In some cases, a judgment is issued but later vacated. In this case, we code possessory judgment as "0." [18] Possessory judgments are a necessary precursor to the issuance of a warrant of eviction.

Log(Judgment Amount): the natural logarithm of the final monetary amount awarded to a landlord, in real January 2021 dollars adjusted using the monthly Consumer Price Index for all urban consumers and winsorized at the first and ninety-ninth percentiles (with one dollar added to all claims before taking the log so as not to exclude cases with claim amounts of zero).

Warrant Issued: a 0/1 indicator for whether a warrant of eviction is issued in a case, as defined by the presence of a warrant issuance date that is not followed by a warrant vacated date. A judgment must be made before a warrant can be issued.

Warrant Executed: a 0/1 indicator for whether a warrant of eviction is executed, as defined by the presence of a warrant execution date, a warrant returned reason of "executed," or both. We also require that the latest warrant execution date is not followed by a subsequent warrant-

[17] Outcomes generally correspond to pivotal events in the housing court process; we define the presence or absence of these events by whether a date corresponding to the event is recorded in the data. Though the data contain other fields related to these events, we have found the date field to be among the most consistently populated and reliable.
[18] We have also looked at whether any possessory judgment was ever issued in the case (vacated or not) and gotten very similar results.

vacated date. A warrant must be issued before it can be executed. The fraction of cases with executed warrants is substantially less than the fraction of cases with warrants issued, even allowing for the fact that some warrants are vacated. This suggests that many households facing warrants of eviction either settle with their landlords informally or leave "voluntarily" rather than waiting for the marshals to arrive and enforce eviction. [19] Thus, focusing only on evictions that are formally executed may underestimate the impact of housing court proceedings on tenants.

In addition to these main outcome variables, we examine a series of variables that have the potential to shed light on the ways in which tenant representation may affect the main outcomes. These are:

Judgment Type: four 0/1 indicators for the type of judgment including whether the (non-vacated) judgment results from: (1) a stipulation or settlement which was arrived at by the parties and ratified by the judge; (2) a tenant failure to answer the petition (and thereby forfeiture of the case); (3) a tenant failure to appear at subsequent steps in the process (and thereby forfeiture of the case); or (4) a court proceeding (e.g., trial).

Judgment Vacated: a 0/1 indicator for whether the judgment that has been issued is later vacated or overturned, meaning it is no longer in effect. Since judgments are usually bad for tenants, a vacated judgment may represent a tenant victory. We define this outcome only for the subset of cases with a judgment.

Days to Judgment: the number of days between a case filing and the final judgment, if any. Other things equal, a longer case may be advantageous to the tenant.

[19] In New York, the landlord must pay a marshal or sheriff to carry out an eviction or a legal possession. In the former, the marshal takes the resident's possession and puts them in storage. In the latter, the landlord is responsible for storing the resident's possessions. Evidently, both landlords and tenants have incentives to avoid the formal carrying out of the eviction or possession.

Warrant Vacated: a 0/1 indicator for whether a warrant of eviction that was issued was later vacated, in which case it is no longer in effect. A vacated warrant may also represent a victory for a tenant. We define this outcome only for the subset of cases with an ordered warrant.

Days to Warrant Executed: the number of days between the warrant issuance date and the warrant executed date. In general, more days will be better for tenants.

2d. Summary statistics

Table 1 presents some initial summary statistics and a simple difference-in-differences comparison of UA and non-UA zip codes. Columns 1 and 2 show means for calendar year 2016 and fiscal year 2019 for zip codes that were not in the first four UA cohorts. Columns 3 and 4 show means for the same two dates for the UA target zip codes. Columns 5-7 shows the difference-in-difference (DiD), with its standard error and a p-value computed using regressions of the covariate on an indicator for whether UA targeted the zip, the post period (FY2019), and their interaction (the reported coefficient), clustering standard errors by zip code.

The first row shows that while a small number of tenants in non-UA zip codes have representation prior to the program, there is no increase over time in these zip codes. However, in UA target zip codes, representation rates rise from 9.1 percent to 17.3 percent. This increase in representation is statistically significant and consistent with what was reported by Ellen et al. (2020) for the first cohort of treated zip codes through 2017. The second row measures tenant counsel representation rates excluding NYCHA cases, those without any activity beyond the initial filing, and those where the tenant never showed up at housing court. These figures show that the effective counsel take-up rates are considerably higher than those in the first row: They are around 20 percent in non-target zip codes and rise to a mean of 39 percent in the target zips by the end of the sample period. The UA treatment indicator rises from 4.1 percent to 47.8 percent in the UA zip codes. It is less than 100 percent because, as shown in Figure 4, some zip codes earmarked for UA do not seem to have had a meaningful increase in representation. The share of households served by UA rises in both non-UA and UA zip codes, but rises significantly more in the UA zip codes, as expected.

In general, tenant outcomes improve in both non-UA and UA zip codes, but the improvements are significantly larger in UA zip codes. For example, the fraction of possessory judgments falls from 0.437 to 0.405 in the control zip codes, but falls from 0.432 to 0.366 in the treated zip codes. The DiD's are statistically significantly different than zero for judgment with possession, log judgment amount, and warrant issued.

In terms of possible mechanisms for the effects of tenant representation, Table 1 suggests that the UA zip codes see fewer cases with a judgment due to a settlement, and fewer cases that are forfeited by a tenant failure to appear. There is no significant DiD for tenant failure to answer, i.e. to ever show up in court at all. This makes sense, because, as discussed above, tenants who never appear cannot be represented by UA. The number of days between a case filing and a judgment is also significantly longer in the UA zip codes after program implementation.

The remaining panels of Table 1 focus on the covariates we have defined using the housing court data, the ACS, and PLUTO. In addition to showing means for these variables, the table demonstrates that out of the 53 variables considered, there is only one significant change in the types of cases filed in the target UA zip codes relative to the non-UA zip codes before and after the program was introduced. We see a slight increase in the probability that a case was filed in Manhattan. Notably, there is no change in the amount of the landlord's primary claim against the tenants, and no change in the value of the unit or the likelihood that the unit is rent stabilized. Hence, there is little evidence that landlords changed their propensity to file cases in response to the UA program, at least in the period we study.

3. Empirical Methods

In order to measure the causal effects of tenant representation, we estimate several different sets of models. The ordinary least squares (OLS) model takes the form:

(1) $Y_i = \beta_0 + \beta_1 R_i + \beta_2 HC_i + \beta_3 PLUTO_{a(i)} + \beta_4 ACS_{b(i)} + zip_i + borough_i{*}month_i + \varepsilon_i$,

where Y is a housing court outcome, i indexes the case, a indexes the address, and b indexes the Census block group. R is an indicator equal to one if the tenant has legal representation. HC is the vector of housing court variables shown in Panel B of Table 1, ACS is the vector of census block group (or tract) characteristics shown in Panel C of Table 1, and PLUTO is the vector of tax lot variables shown in Panel D of Table 1. The model also includes fixed effects for each zip code (zip), as well as indicators for each month and year (month) (e.g. December 2018) which are interacted with indicators for each court borough (borough). The ε_i denotes the error term.

We also estimate a model that includes address-unit-specific fixed effects (indexed with subscript i because address units vary at the case level):

(2) $Y_i = \beta_0 + \beta_1 R_i + \beta_2 HC_i + \beta_3 PLUTO_{a(i)} + \beta_4 ACS_{b(i)} + + borough_i{*}month_i + address_i + \varepsilon_i$,

OLS models as well as reduced forms are presented for reference but our focus is on the instrumental variables versions of Equations (1) and (2), which replace R_i with $Rhat_i$, where $Rhat_{it}$ is the predicted value of R from the first-stage equations:

(3) $R_i = \alpha_0 + \alpha_1 UA_i + \alpha_2 HC_i + \alpha_3 PLUTO_{a(i)} + \alpha_4 ACS_{b(i)} + zip_i + borough_i{*}month_i + \varpi_i$,

(4) $R_i = \alpha_0 + \alpha_1 UA_i + \alpha_2 HC_i + \alpha_3 PLUTO_{a(i)} + \alpha_4 ACS_{b(i)} + borough_i{*}month_i + address_i + \varpi_i$,

where UA is an indicator equal to one if UA has been rolled out in the zip code and zero otherwise. Alternatively, when the UA intensity instrument is used, UA is the number of households served in a zip and fiscal year, divided by 1000. All of these models cluster standard errors at the zip code level.

Lastly, regression discontinuity event study graphs are presented which focus on the ten months before and after the ramp up of UA in the 20 zip codes that formed part of the first three treated cohorts. The running variable is the number of calendar months since the UA start date. The discontinuity instrument is an indicator for whether the filing date is after the date of the UA ramp up. The design is a fuzzy regression discontinuity because exposure to UA increases the probability of attorney representation but does not guarantee it. Since the exact date when the increased probability of representation occurs is somewhat blurry, a "donut" of cases that were filed within a month on either side of the UA start date are omitted.

For these models the estimating equation, again estimated by TSLS, is a straightforward modification of Equation (1):

$$(5)\quad Y_i = \gamma_0 + \gamma_1 m_i + \gamma_2 m_i{*}R_i + \gamma_3 R_i + \gamma_4 HC_i + \gamma_5 PLUTO_{a(i)} + \gamma_6 ACS_{b(i)} + zip_i + borough_i + \omega_i,$$

where m is the running variable, i.e., the number of months relative to the UA start date for each borough-cohort. The instrument for R is UA as before, and $m_i{*}R_i$ is instrumented with $m_i{*}UA$. We do not include the borough-by-month fixed effects in the RD specification due to collinearity with the running variable. This RD design is a stringent test of our general identification assumptions since the sample is much smaller, including only 36,856 cases.

4. Estimated Effects of Legal Representation

Table 2 presents the main results: the within-zip code estimates of Equation (1) that rely on the staggered timing of the UA roll-out. The first column shows OLS estimates which indicate that representation is associated with significantly lower probabilities of possessory judgment, warrant issuance, and warrant execution (i.e., eviction). However, as discussed above, these estimates could reflect biases due to selection into representation.

The two-stage least squares (TSLS) estimates shown in Column 2 address this concern. The first row shows the first stage effect of the ramp up of the UA program, which is to increase the probability of tenant representation by 12.4 percentage points (pp), which is larger than the crude DiD comparison shown in Table 1. The remaining estimates suggest that representation has very large effects on the affected cases, reducing the probability of a possessory judgment by 32.1pp, the log judgment amount by 2.126, the probability of a warrant being issued by 32.3pp, and the probability that the warrant is executed by 8.4pp.

TSLS estimates larger than OLS in absolute value suggests that the OLS estimates are biased such that the people who get representation are those most likely to have negative outcomes. One can see this bias most clearly in the positive OLS coefficient on the log judgment amount, which, if it was causal, would imply that legal representation actually worsened tenant outcomes. It may also be the case that (as discussed below), allowing for heterogeneous treatment effects, those tenants who receive representation only because of the UA program (the compliers) receive larger benefits relative to tenants who would always have had representation or those who would never have representation.

Columns 3 and 4 show that the estimates are very similar when the models are re-estimated using the set of addresses that had more than one filing and including a fixed effect for the exact address. In these models, the effects are identified using only the approximately 57,000 addresses that have at least one case filing before UA implementation and one case filing after UA implementation. The effect on evictions is slightly reduced in this sample to 5.8 pp, and is now significant at only the 90 percent level of confidence. The other outcome estimates are somewhat larger than in the full sample. Thus, even if all of the time-invariant characteristics of the units themselves (and implicitly of the type of people who rent them) are held constant, there are large effects of representation on those who gain representation because of UA.

For reference, the reduced form estimates corresponding to Columns 2 and 4 are shown in Columns 1 and 2 of Appendix Table A.3. These estimates show the average effect of the introduction of the UA program on the outcomes of all of the housing court cases in the zip code. They show a reduction in the probability of possessory judgment of 4.0pp, a reduction of log judgment amounts by 0.263, a reduction in the probability that a warrant is issued of 4.0pp, and a reduction of 1.0pp in the probability of eviction. Scaling these estimates using the estimated first stage effect of UA on representation produces estimates very similar to those in Table 2.

The contrast between the OLS and the TSLS estimates begs the question of who the "compliers" are, that is, who are the households that are moved from no representation to representation by the implementation of the UA program? An analysis of the estimated mean characteristics of compliers and non-compliers is shown in Appendix Table A.4. For most observables, estimated differences are statistically significant but small in magnitude, which suggests that the typical complier is not much different than the typical tenant in housing court. However, there are a few larger contrasts that suggest that compliers come from less valuable and less dense places, in terms of the assessed value of the lot ($6.29 million for compliers vs. $11.17 for non-compliers); building-to-lot area ratio (2.92 vs. 3.43); number of units owned by the landlord (29,024 vs. 43,508); and 1-2 family homes (9.6 percent vs. 4.4 percent). Compliers are also less likely to reside in rent-stabilization-eligible housing (8.0 percent vs. 13.1 percent) and face somewhat lower primary claim amounts (6.43 vs. 6.92 in logs). One possible explanation for these patterns is the political constraint that UA access be equalized across boroughs so that people in cheaper, less dense boroughs like Staten Island are relatively more likely to have access. Of course, unobservables may still play a role, and compliers may be, in some sense, negatively selected: For example, they might have a high propensity to miss scheduled court appearances, as discussed further below.

In order to interpret the magnitude of the estimated effects in Table 2, one would ideally like to know what would have happened to the compliers in the absence of the program. One possible baseline for comparison is provided by cases filed in 2016 in which there was at least some housing court activity. Means for this set of cases are shown in the last column of Table A.5. Relative to these means, the estimates in Column 2 of Table 2 suggest that tenant representation via the UA program reduced the probability of a possessory judgment by 51.5 percent, reduced the log award amount by 81.5 percent, reduced the probability of a warrant being issued by 61.3 percent, and reduced the probability of a warrant being executed by 77.8 percent. Of course, if people who obtain representation through the UA program would have had worse outcomes than the average tenant in housing court in the absence of the program, as we argued above, then these implied percent changes should be taken as upper bounds on the possible effects of legal representation in the full sample.

The TSLS findings in Table 2 are very robust. Appendix Table A.6 repeats the main analysis excluding cases from Queens, since these data suggest that the UA program was not strongly implemented there and that there may have been rising trends in representation in non-UA Queens zip codes. The estimates are essentially identical to the main results.

Table A.7 repeats the Table 2 analysis excluding NYCHA cases and those without any activity by either petitioner (landlord) or respondent (tenant) following initial filing. These exclusions make the first stage larger, indicating that exposure to UA increases the probability of legal representation by 17.3 pp (Column 2). However, the estimated effects of tenant legal representation on housing court outcomes are little changed.

Table A.8 shows several alternative specifications: Column 1 shows that in models estimated without controls, the effects are negative but imprecisely estimated. Adding the housing court, ACS, and PLUTO controls (Column 2) produces estimates similar to those shown in Table 2. Adding zip code fixed effects but omitting the borough*month fixed effects produces estimates that are qualitatively similar but larger in absolute value than those shown in Table 2. Columns 4 to 7 shows estimates from only the last two years of our data, fiscal years 2018 and 2019. These are the years when the "UA intensity" instrument is available and these estimates are presented in order to facilitate comparison to the results estimated using that instrument that are discussed below. The main estimates (Column 6) and those with address fixed effects (Column 7) are quite similar to those shown in Table 2.

Table A.9 explores the sensitivity of our estimates to various approaches to correcting standard errors in two-way fixed effects models with staggered treatment. For ease of implementation, this table focuses on reduced form results and collapses the data to a zip-month panel. A comparison of Columns 1 and 2 shows that collapsing the data has no impact on our estimates. The remaining columns show that our estimates are robust to implementing the corrections suggested by Borusyak, Jaravel and Spiess (2022), Callaway and Sant'Anna (2021), Chaisemartin and D'Haultfoeuille (2020), and Sun and Abraham (2021).

Finally, it is worth considering the effect of potential misclassification of tenant representation status. Conversations with housing court officials suggest that misclassification is unlikely to be random. Rather, there may be tenants who are not initially recorded as receiving representation under the UA program, but who later receive it. In this case, we would be understating the extent to which the UA program increased representation, and therefore overstating the extent to which representation affected outcomes. The worst case scenario is that we could be missing up to 45 percent of cases.[20] Inflating the case counts for each zip-month in Table A.9 by this figure yields a first stage coefficient on respondent counsel of 0.225, approximately double our initial first stage estimate. Applying this first stage estimate to the reduced form coefficients in Table A.9 suggests UA effects that are about half of those reported in our main specification. These lower bound estimates of the effects suggest a reduction of 30.5 percent in possessory jugements, 50.2 percent in judgment amounts, 33.0 percent in warrants being executed, and 44.8 percent in warrants being executed.

Table 3 returns to our main specification and explores heterogeneity in the instrumental variables estimates. This analysis speaks to the important question of who is most affected by the UA program—although compliers may not be very different than non-compliers on average, the program may still have had much larger effects on some types of participants.

For example, it might be the case that some tenants are particularly vulnerable to eviction, perhaps because they are afraid to appear in court. Panel (1) splits cases according to whether the percentage of noncitizens living in a tenant's Census tract is above or below the in-sample median. The estimated effects are all larger in tracts with higher percentages of noncitizens. In particular, the effect on evictions is almost twice as large in this subsample compared to areas with more citizen families.

Panel (2) splits cases by whether their Census block groups have a Hispanic majority, a Black majority, a non-Hispanic White majority, or an Asian majority. Not every block group is included because a few have no clear majority group (though given a high degree of residential

[20] This estimate is based on a discussion in a report by the Office of Civil Justice (2019) which reports the total number of tenants city-wide who received full legal representation from UA. This number is almost the same as what we see in our data for the number with any type of legal representation. So the extent of the undercount depends on what one assumes about the extent of non-UA legal services. If we assume that the non-UA representation rate continued at the 2016 level, then we arrive at the 45 percent undercount. But it seems reasonable to assume that UA substituted for some types of representation that tenants were receiving earlier.

segregation, the categorization is essentially identical if we use pluralities). While the effects on possessory judgments, judgment amounts, and warrants issued are similar in the majority Hispanic, Black, and white neighborhoods, the effect on evictions is largest in majority Hispanic neighborhoods and perhaps in majority Asian neighborhoods. The point estimates for cases filed in majority Asian neighborhoods suggest that the program may have had particularly large effects in these locations, but since this subset of cases is quite small, the effects are imprecisely estimated compared to those in other neighborhoods.

Panel (3) of Table 3 splits the sample by indicators of neighborhood poverty. The first set of estimates divide the sample by whether the median rent (imputed using the median rent in the Census block group) is above or below the sample median. The results suggest that the estimated effects are particularly large for the lowest-rent neighborhoods. The second analysis splits the sample by whether a tenant's CBG is above or below the in-sample median of CBG poverty rates. These estimates show that, while representation has an effect in both relatively poor and less poor places, the biggest impacts of UA representation are in the places with above-median poverty rates, particularly when it comes to executed evictions, which are reduced by a statistically significant 12.6 pp.

Table 4 shows estimates for additional outcomes that shed light on how representation affects tenant outcomes. As before, we focus on Columns 2 and 4 which present the IV results. In Panel A, the IV estimates show that conditional on having a judgment, the judgment is more likely to have been vacated if the tenant has legal counsel. This result suggest that lawyers continue to fight for their clients even after adverse rulings, and that in many cases, they are successful in having these rulings overturned. However, once a warrant has been ordered, UA has no effect on the probability that it is later vacated.

Panel B of Table 4 shows the effects of representation on the type of judgment that is reached, which in turn reflects grounds for the judgment. The instrumental variables estimates suggest that representation reduces the probability that the judgment reflects a stipulation/settlement (the most common basis for a judgment) by a large margin (-22.6 pp; Column 2). Tenant representation also reduces the probability that a judgment is reached because the tenant failed to appear in court at some point after answering the initial petition (-7.8 pp; Column 2). As expected, there is no impact of legal representation on tenants failing to answer the initial petition (since the point of access for UA is a tenant's first appearance in housing court).

Panel C of Table 4 shows that the number of days from a case filing until a judgment is entered increases by almost three months. Even in a losing case, buying time may be valuable to a tenant, increasing residential stability by smoothing transitions. In the specification with address fixed effects, there is also a significant effect on the number of days until a warrant is executed after it is issued. The point estimate on the main specification also suggests an increase in the time between warrant issuance and execution, though it is not precisely estimated.

Table 5 shows estimates using the alternative "UA intensity" instrument---that is, the number of households in a zip code that received representation through the UA program in a given fiscal year divided by 1,000. This instrument allows us to take account of the fact that some households outside of the designated UA zip codes were also served by the program. The first stage and reduced form results corresponding to these models are shown in Table A.3. The Table 5 estimates follow the same qualitative pattern as those in Table 2.[21] The point estimates are larger, but this is not surprising given that the intensity instrument uses the continuous

[21] The last two columns of Table A.7 show estimates without covariates (Column 8) and with covariates but without zip code or borough*month fixed effects (Column 9). .

variation in representation rates rather than a 0/1 indicator. For example, there is an estimated reduction in evictions of 9.1 pp (significant at the 90 percent level of confidence) compared to 8.4 pp in Table 2.

The key results of the regression discontinuity exercise are shown in Figure 5.[22] The RD sample of cases—consisting of cases filed within +/-10 months UA start for the treated zip codes in the first three UA cohorts—is much smaller than the full sample. Nevertheless, Figure 5 shows clear evidence of UA-coincident jumps in the probability that respondents have representation, as well as discontinuous declines in the probability of judgment with possession, the probability of a warrant issuance, and the log judgment amount. The probability that a warrant is executed appears to be on a more steeply declining path following UA implementation, but there is no sharp break. Appendix Figure A.3 shows that the density of cases is fairly smooth through the UA implementation "jump" point.

Appendix Figure A.4 shows that there is little change in salient characteristics of cases filed in housing court around the time of UA introduction; in particular, primary claim amounts and the share of cases that involve nonpayment are quite similar in the pre- and post-UA periods. There is a slight but not statistically significant short-term rise in landlord cases per unit, but this returns to the long-term trend by 10 months out. In short, there is little evidence that UA implementation changed the composition of the housing court caseload, at least over the timeframe we examine here. Hence, the regression discontinuity framework supports the identifying assumption that the pool of housing court cases remained similar before and after the implementation of UA.

A final question we investigate is whether the relief offered by the UA program merely "postpones the inevitable." For example, it is possible that landlords who lose a case in housing

[22] The corresponding regression estimates appear in Table A.11.

court immediately launch another case against the tenant, and that they are ultimately successful. Our ability to investigate long-term outcomes for individual addresses is limited by the relatively brief time period between the beginning of the sample period and the onset of the COVID-19 pandemic. However, in Table A.10 we follow unit-level addresses and ask whether UA had an impact on the number of cases filed at the address in the 15 months after the initial case filing, or on the ultimate outcomes observed at that address cumulatively across any case filed in the 15-month follow-up period.. We find that there is no effect on case filings (suggesting that landlords are not filing additional cases) and that the estimated outcomes are quite similar to those in Table 2.

5. Discussion and Conclusions

Though detailed, the housing court records have several shortcomings. The most obvious is the redaction of personally identifying information, which limits our ability to observe respondent characteristics and to follow respondents after they disappear from the housing court records.[23] A second limitation is the relatively short time period that the program was in effect before the seismic upheaval in New York City housing markets and housing court caused by the COVID-19 pandemic and subsequent ban on evictions. The consequences of these disruptions are still playing out: While the program is now theoretically available to all low-income renters, a backlog of eviction cases is hitting the housing courts all at once, leaving short-staffed non-profit legal services contractors struggling to keep pace with demand (Zaveri, 2022). Hence, it is unlikely that additional years of data would be useful for evaluating the roll out of this program.

Still, we have used the available data to estimate a wide variety of models which rely on different identifying assumptions. These include reduced form models of the effects of the UA

[23] We tried and failed to get access to data with personal identifiers. There appears to have been a change in court policy given that this housing court records spanning earlier years has been made available in the past to other researchers with personal identifiers included.

program; instrumental variables models intended to identify the effects of legal representation itself using first a roll-out instrument, and then a UA-intensity instrument based on the number of cases with representation in each zip code; and a fuzzy-RD model examining the impact of UA within affected zip codes. These models produce remarkably consistent estimates showing that UA increased the probability of tenant legal representation, and that legal representation greatly improved outcomes among tenants who received services due to the program.

In particular, we find large reductions in: the probability that there is a judgment with possession (between 30.5 and 51.5 percent), log judgment amount (between 50.2 and 81.5 percent),[24] in the probability of eviction warrant issuance (between 33 and 61.3 percent), and in the probability of ultimately being evicted (between 44.8 and 77.8 percent). The high end estimates are upper bounds on the possible effects of the program if people who obtain representation through the UA program would have had worse outcomes than the average tenant in housing court in the absence of the program or if the extent of participation in the UA program is under-stated in administrative records. But lower bound estimates still suggest very substantial effects. We also find that the program had larger effects in the poorest neighborhoods and in those with large shares of noncitizens.

Of course, an open question is whether giving tenants representation will, in the longer term, reduce the supply of affordable apartments or make landlords more reluctant to rent to some types of tenants. However, at least in the relatively short time frame we study, we find little evidence of changes in the characteristics of cases filed before and after the introduction of UA. In particular, there is no evidence that post-UA cases are drawn from areas with higher

[24] Estimating in levels, we find a 69 percent reduction in judgment amounts relative to the mean judgment amount for cases filed in 2016 in which there was at least some housing court activity, including cases with zero judgment amount. The coefficient from our main IV specification (equivalent to the results in Column 2) of Table 2 is -$877 (significant at the one percent level) relative to a mean of $1,277.

median rents, as one might expect if UA caused rents to rise, or that the back-rent amounts in question have changed.[25] Moreover, this program does not change the law regarding whether and when a tenant can be evicted. Rather, it levels the playing field so that both tenants and landlords have access to counsel.

In terms of cost, the Department of Social Services FY2021 budget for tenant legal programs was $136 million and 42,000 households were served by UA, implying a cost of about $3,200 per household (Office of Civil Justice, 2021). Our estimates suggest that these households experienced substantial benefits through both reductions in judgments and reductions in the costs associated with forced relocation, as detailed by the National Academy of Sciences (2021). In sum, our findings contribute to a small but growing literature showing that legal representation can substantially improve the lives of poor families at modest cost (Hoynes et al, 2022, Cooper et al. 2022).

However, there is always room for improvement. One issue is that many tenants continue to forfeit cases by failing to answer petitions. Because the main point of entry to UA is at housing court, the program is limited in its ability to help those unable or unwilling to show up. Moreover, our estimates show considerable heterogeneity in the effectiveness of the program, depending on the characteristics of the neighborhoods and tenants served: Targeting resources to areas with more noncitizens and poorer households would likely produce a greater impact on tenant outcomes per dollar expended. Whether such targeting is politically palatable remains to be seen.

[25] Some theoretical models suggest large increases in rents as a result of access to counsel programs (e.g. Abramson, 2022).

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Table 1A: Summary Statistics

	Sample Means Category				UA Change (Diff.-in-Diff.)		
	Non Pre 1/16-12/16 (1)	Non Post 7/18-6/19 (2)	UA Pre 1/16-12/16 (3)	UA Post 7/18-6/19 (4)	Coef (5)	SE (6)	P-value (7)
A. Treatment, Instruments, and Outcomes (NYC Housing Court)							
Respondent Counsel	0.069	0.072	0.091	0.173	0.079	0.016	0.000**
Respondent Counsel Take-Up[1]	0.201	0.197	0.217	0.385	0.172	0.032	0.000**
Empirical UA Treatment (IV)	0.000	0.000	0.041	0.478	0.437	0.110	0.000**
UA Households Served/1000 (IV)	0.000	0.275	0.000	0.886	0.611	0.138	0.000**
Judgment with Possession	0.437	0.405	0.432	0.366	-0.035	0.008	0.000**
Log Judgment Amount	1.796	1.697	1.848	1.516	-0.232	0.054	0.000**
Warrant Issued	0.367	0.335	0.376	0.313	-0.031	0.007	0.000**
Warrant Executed	0.079	0.056	0.076	0.048	-0.006	0.005	0.227
Judgment Vacated (Cond. on Judgment)	0.110	0.125	0.142	0.152	-0.005	0.007	0.459
Warrant Vacated (Cond. on Warrant)	0.066	0.068	0.084	0.076	-0.011	0.004	0.007**
Judgment Failure to Answer	0.139	0.128	0.141	0.127	-0.003	0.004	0.480
Judgment: Failure to Appear	0.066	0.066	0.069	0.060	-0.009	0.004	0.012*
Judgment: Stip/Settle	0.245	0.227	0.242	0.198	-0.025	0.006	0.000**
Judgment: Court Proceeding	0.006	0.006	0.005	0.005	-0.000	0.001	0.850
Days to Judgment Entered	67.561	64.793	67.186	71.189	6.771	2.701	0.013*
Days to Warrant Executed	206.933	170.088	209.955	183.592	10.483	5.623	0.064
B. NYC Housing Court							
Petitioner Counsel	0.977	0.980	0.977	0.980	-0.000	0.002	0.982
Nonpayment	0.863	0.861	0.883	0.882	0.001	0.005	0.803
Bronx	0.349	0.337	0.390	0.372	-0.006	0.012	0.626
Kings (Brooklyn)	0.284	0.303	0.242	0.234	-0.027	0.015	0.067
New York (Manhattan)	0.192	0.186	0.226	0.248	0.028	0.014	0.048*
Queens	0.157	0.154	0.117	0.118	0.004	0.006	0.506
Richmond (Staten Island)	0.019	0.021	0.025	0.027	0.001	0.004	0.877
Court: Harlem	0.024	0.022	0.010	0.013	0.005	0.003	0.109
Court: Redhook	0.007	0.005	0.000	0.000	0.001	0.001	0.318
Filed Month (1==Jan 2016)	6.6	36.1	6.5	36.1	0.1	0.1	0.2
Respondent Count == 1	0.707	0.716	0.732	0.743	0.002	0.008	0.749
Petitioner Count == 1	0.988	0.988	0.991	0.990	-0.001	0.001	0.516
NYCHA	0.199	0.242	0.122	0.161	-0.004	0.017	0.799
Specialty Designation	0.046	0.029	0.058	0.022	-0.019	0.019	0.320
Log (Real 2021$) Primary Claim	6.748	6.797	6.926	6.960	-0.014	0.052	0.781
Observations	153,582	135,405	66,666	58,554			

Columns 1–4 give sample means by UA zip code group and period. The UA group consists of cases located in zip codes belonging to the first four UA cohorts. The Non group are non-pilot cohorts. The Pre period consists of cases filed from Jan. 2016 to Dec. 2016. The Post period consists of cases filed from July 2018 to June 2019. Columns 4–7 report the difference-in-difference coefficients, standard errors, and p-values from regressions of each row-enumerated characteristic on indicators for UA zip, post period, and their interaction (the reported coefficient), using the subsample of cases summarized in Columns 1–4 and clustering standard errors at the zip code level. * $p < 0.05$, ** $p < 0.01$

[1] Respondent counsel "take-up" is meant to give a measure of the impact of Universal Access among those who the program reaches: that is, the calculation excludes NYCHA, cases with no activity beyond initial filing, and cases where tenants never appear at court, as tenants must show up at housing court to access the program and NYCHA tenants where not initially prioritized for services.

Table 1B: Summary Statistics

	Sample Means Category				UA Change (Diff.-in-Diff.)		
	Non Pre 1/16-12/16 (1)	Non Post 7/18-6/19 (2)	UA Pre 1/16-12/16 (3)	UA Post 7/18-6/19 (4)	Coef (5)	SE (6)	P-value (7)
C. US Census American Community Survey							
CBG Population/1000	1.745	1.767	1.652	1.660	-0.014	0.013	0.291
CBG HH Median Income/1000 (in 2019$)	49.596	48.123	46.883	45.331	-0.079	0.635	0.901
CBG Poverty Pct.	0.280	0.291	0.275	0.287	0.001	0.004	0.837
CBG Hispanic Pct.	0.404	0.401	0.444	0.441	0.000	0.007	0.989
CBG Black Pct.	0.327	0.338	0.378	0.387	-0.002	0.006	0.765
CBG Asian Pct.	0.080	0.079	0.045	0.044	0.001	0.002	0.455
CBG White Pct.	0.164	0.158	0.109	0.103	-0.001	0.004	0.857
CBG 0-17 Years Pct.	0.228	0.230	0.231	0.234	0.001	0.002	0.571
CBG 65+ Years Pct.	0.133	0.134	0.121	0.122	0.001	0.001	0.481
CBG Female Pct.	0.540	0.543	0.540	0.543	0.000	0.001	0.759
CBG Total Housing Units/1000	0.732	0.742	0.661	0.663	-0.007	0.006	0.224
CBG Rental Units Pct.	0.852	0.856	0.882	0.884	-0.001	0.003	0.663
CBG Median Gross Rent/1000 (in 2019$)	1.240	1.201	1.237	1.193	-0.005	0.013	0.700
CT Naturalized Pct.	0.192	0.190	0.184	0.179	-0.003	0.002	0.101
CT Noncitizen Pct.	0.162	0.157	0.173	0.166	-0.002	0.002	0.280
D. NYC DCP PLUTO							
Zone Dist.: Res. Low Density	0.226	0.227	0.142	0.153	0.010	0.009	0.290
Zone Dist.: Res. Medium Density	0.608	0.611	0.688	0.689	-0.001	0.012	0.948
Zone Dist.: Res. High Density	0.096	0.091	0.140	0.130	-0.005	0.006	0.362
Zone Dist.: Other	0.064	0.062	0.025	0.024	0.001	0.003	0.851
Land Use: 1-2 Family	0.055	0.058	0.042	0.044	-0.000	0.002	0.831
Land Use: Multi-Family Walkup	0.252	0.230	0.272	0.251	0.000	0.007	0.974
Land Use: Multi-Family Elevator	0.449	0.470	0.488	0.508	-0.001	0.011	0.931
Land Use: Mixed Res.-Comm.	0.230	0.226	0.188	0.188	0.005	0.006	0.414
Land Use: Other	0.008	0.008	0.005	0.006	0.001	0.001	0.270
Num. Buildings == 1	0.358	0.386	0.271	0.318	0.019	0.013	0.141
Residential Units	323.7	331.9	365.1	386.9	13.61	19.47	0.49
Year Built	1940.9	1944.2	1916.4	1918.8	-0.8	2.2	0.7
Building Altered == 1	0.687	0.696	0.645	0.646	-0.008	0.010	0.392
Lot Area/1000000	0.162	0.172	0.184	0.190	-0.004	0.012	0.736
Building-to-Lot Area Ratio	3.384	3.305	3.405	3.300	-0.026	0.040	0.510
Lot Assessed Value/1000000 (in 2021$)	10.584	10.733	9.854	10.401	0.398	0.675	0.556
Rent Stabilization Eligible	0.128	0.127	0.110	0.118	0.010	0.009	0.263
Landlord Properties	249.3	301.8	163.6	230.3	14.20	19.05	0.46
Landlord Buildings	637.4	770.7	415.7	586.6	37.58	48.70	0.44
Landlord Units	41,574.7	50,291.8	27,178.8	38,350.9	2,455.03	3,183.56	0.44
Landlord Assessed Value	1,201.1	1,452.8	784.7	1,106.4	70.03	91.80	0.45
Landlord Cases	31,134.6	37,684.6	20,336.2	28,718.1	1,831.95	2,387.89	0.44
Landlord Cases Per Units	0.788	0.786	0.880	0.885	0.007	0.008	0.364
Observations	153,582	135,405	66,666	58,554			

Columns 1–4 give sample means by UA zip code group and period. The UA group consists of cases located in zip codes belonging to the first four UA cohorts. The Non group are non-pilot cohorts. The Pre period consists of cases filed from Jan. 2016 to Dec. 2016. The Post period consists of cases filed from July 2018 to June 2019. Columns 4–7 report the difference-in-difference coefficients, standard errors, and p-values from regressions of each row-enumerated characteristic on indicators for UA zip, post period, and their interaction (the reported coefficient), using the subsample of cases summarized in Columns 1–4 and clustering standard errors at the zip code level. * $p < 0.05$, ** $p < 0.01$

Table 2: Main Results: Respondent Counsel and Housing Court Outcomes

	Main		Address FE	
	OLS	UA IV	OLS	UA IV
	(1)	(2)	(3)	(4)
Respondent Counsel (First Stage)		0.124**		0.118**
		(0.006)		(0.008)
Judgment with Possession	-0.073**	-0.321**	-0.056**	-0.334**
	(0.006)	(0.041)	(0.008)	(0.054)
Log Judgment Amount	0.157**	-2.126**	-0.075	-2.519**
	(0.038)	(0.284)	(0.048)	(0.405)
Warrant Issued	-0.068**	-0.323**	-0.050**	-0.340**
	(0.006)	(0.037)	(0.009)	(0.056)
Warrant Executed	-0.027**	-0.084**	-0.007**	-0.058
	(0.002)	(0.021)	(0.003)	(0.034)
Observations	727,692	727,692	456,788	456,788
First-Stage F Stat	.	495.93	.	240.05
Covariates	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes
Borough × Month FE	Yes	Yes	Yes	Yes
Address FE	No	No	Yes	Yes

Outcomes are listed in rows. Analytical specifications are indexed by column. All results are for the main sample. Unit of observation is a housing court case. Each cell in Columns 1–4 reports the coefficient on respondent (tenant) counsel from a separate regression of the row-enumerated outcome on the covariates and fixed effects summarized at the bottom of the table. Columns 1 and 3 report the ordinary least squares linear associations between outcomes and tenant counsel. Columns 2 and 4 report two-stage least squares instrumental variable results for tenant counsel, using an indicator for empirical UA treatment (i.e., program rollout) as the instrument (equal to one if UA is operating in a case's zip code at the time of filing). Supercolumns group specifications by the major fixed effects included. Columns 1 and 2 control for zip and court borough by month fixed effects, while Columns 3 and 4 additionally control for address fixed effects. First row reports first-stage results with tenant (respondent) counsel as the dependent variable. Standard errors clustered by zip code are given in parentheses. * $p < 0.05$, ** $p < 0.01$

Table 3: Heterogeneity Analysis: IV Results

	Respondent Counsel . (1)	Judgment with Possession (2)	Log Judgment Amount (3)	Warrant Issued . (4)	Warrant Executed . (5)
(1) Citizenship					
CT Noncitizen Pct. Above In-Sample Median					
Yes	0.125**	-0.371**	-2.377**	-0.363**	-0.108**
	(0.006)	(0.040)	(0.251)	(0.040)	(0.027)
	362,738				
	[432.59]				
No	0.124**	-0.299**	-1.928**	-0.299**	-0.055*
	(0.012)	(0.064)	(0.453)	(0.057)	(0.022)
	364,944				
	[111.13]				
(2) Race					
CBG Hispanic Majority	0.123**	-0.316**	-2.300**	-0.336**	-0.101**
	(0.006)	(0.054)	(0.388)	(0.044)	(0.027)
	292,455				
	[390.87]				
CBG Black Majority	0.128**	-0.310**	-2.118**	-0.282**	-0.056
	(0.011)	(0.057)	(0.304)	(0.068)	(0.045)
	211,142				
	[137.59]				
CBG White Majority	0.119**	-0.357**	-2.191**	-0.357**	-0.057
	(0.016)	(0.079)	(0.581)	(0.090)	(0.066)
	72,078				
	[57.42]				
CBG Asian Majority	0.129**	-0.522	0.308	-0.598	-0.237*
	(0.027)	(0.416)	(1.558)	(0.510)	(0.104)
	14,018				
	[22.35]				
(3) Poverty					
CBG Gross Rent Above In-Sample Median					
Yes	0.123**	-0.321**	-1.989**	-0.292**	-0.071*
	(0.007)	(0.042)	(0.282)	(0.042)	(0.033)
	356,809				
	[321.55]				
No	0.122**	-0.397**	-2.504**	-0.400**	-0.084**
	(0.009)	(0.056)	(0.467)	(0.048)	(0.019)
	370,874				
	[190.12]				
CBG Poverty Pct. Above In-Sample Median					
Yes	0.127**	-0.341**	-2.420**	-0.346**	-0.126**
	(0.007)	(0.073)	(0.361)	(0.057)	(0.016)
	364,174				
	[363.53]				
No	0.120**	-0.330**	-1.957**	-0.310**	-0.036
	(0.006)	(0.052)	(0.372)	(0.052)	(0.028)
	363,511				
	[390.66]				

Outcomes are listed in columns. Rows index the characteristics and levels defining the subsamples among which the heterogeneity analysis is conducted.Each cell reports the coefficient on tenant counsel from a separate 2SLS instrumental variable regression of the column-enumerated outcome, with empirical UA treatment as the instrument and using the main specification (corresponding to Column 2 in Table 2) for the subsample defined by the row.First column reports first-stage results with tenant (respondent) counsel as the dependent variable. Unit of observation is a housing court case. Standard errors clustered by zip code are given in parentheses. Number of observations and first-stage F-statistic (in brackets) reported below SE's in Column 1. Covariates for UA cohort subsample include linear half-year controls rather than fixed effects due to collinearity with the instrument within cohort. * $p < 0.05$, ** $p < 0.01$

Table 4: Additional Results: Respondent Counsel and Housing Court Outcomes

	Main		Address FE	
	OLS (1)	UA IV (2)	OLS (3)	UA IV (4)
A. Judgment Procedures				
Judgment Vacated (Cond. on Judgment)	0.186**	0.087*	0.219**	0.156**
	(0.009)	(0.034)	(0.008)	(0.048)
Warrant Vacated (Cond. on Warrant)	0.123**	-0.013	0.141**	-0.008
	(0.007)	(0.019)	(0.006)	(0.033)
B. Judgment Type				
Judgment Failure to Answer	-0.055**	-0.016	-0.020**	0.030
	(0.002)	(0.033)	(0.004)	(0.042)
Judgment: Failure to Appear	-0.041**	-0.078**	-0.019**	-0.103**
	(0.003)	(0.023)	(0.002)	(0.031)
Judgment: Stip/Settle	0.041**	-0.226**	0.012	-0.255**
	(0.006)	(0.030)	(0.006)	(0.046)
Judgment: Court Proceeding	0.012**	0.006	0.009**	0.012
	(0.001)	(0.005)	(0.001)	(0.007)
C. Length of Case				
Days to Judgment Entered	69.291**	85.000**	45.728**	76.867**
	(2.186)	(8.074)	(1.897)	(7.934)
Days to Warrant Executed	96.423**	50.672	64.063**	113.252*
	(2.453)	(32.427)	(12.016)	(44.769)

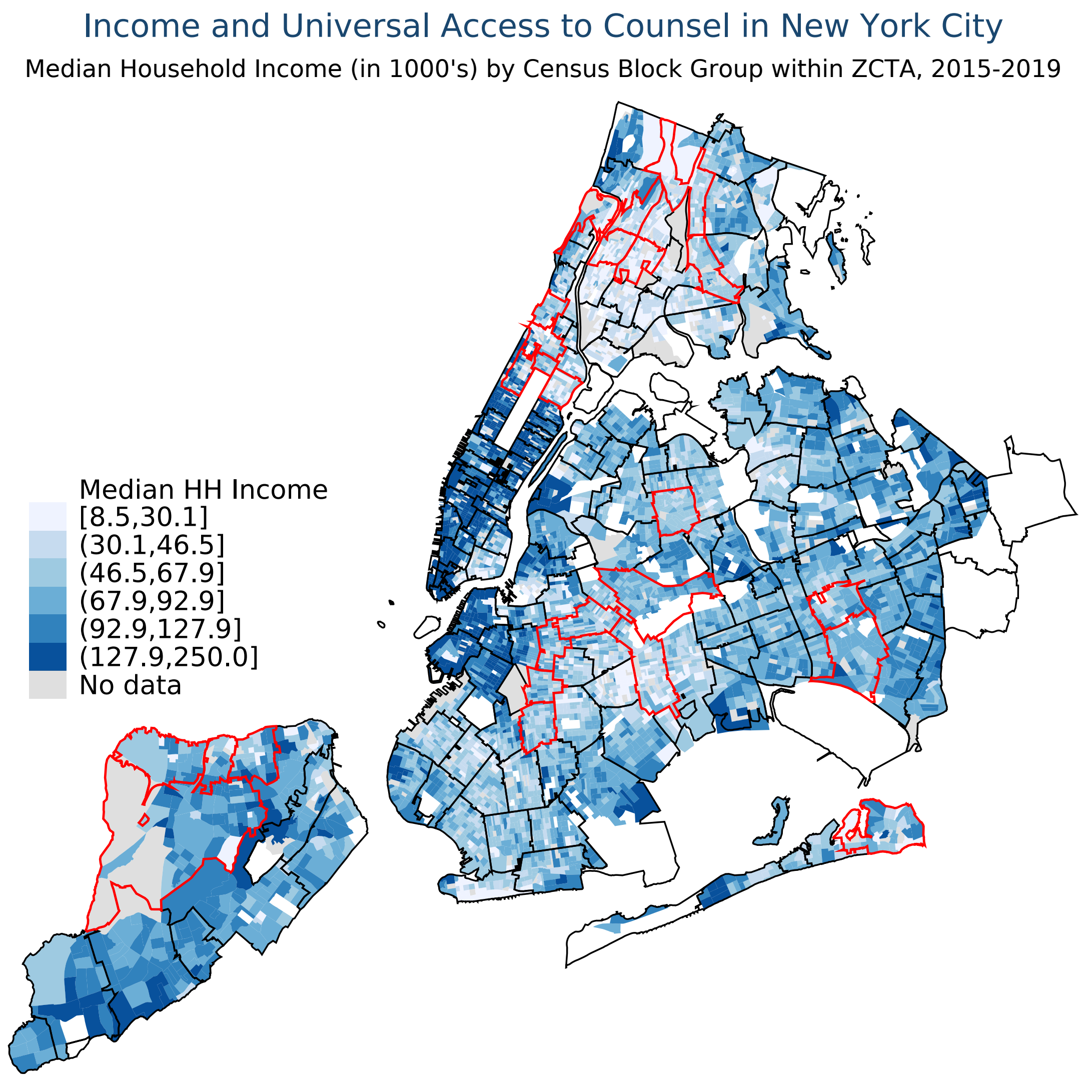
Outcomes are listed in rows. Analytical specifications are indexed by column. All results are for the main sample. Unit of observation is a housing court case. Each cell in Columns 1–4 reports the coefficient on tenant counsel from a separate regression of the row-enumerated outcome on the covariates and fixed effects summarized at the bottom of the table. Columns 1 and 3 report the ordinary least squares linear assoications between outcomes and tenant counsel. Columns 2 and 4 report two-stage least squares instrumental variable results for tenant counsel, using an indicator for empirical UA treatment (i.e., program rollout) as the instrument (equal to one if UA is operating in a case's zip code at the time of filing). Supercolumns group specifications by the major fixed effects included. Columns 1 and 2 control for zip by linear time fixed effects, while Columns 3 and 4 additionally control for address fixed effects. First row reports first-stage results with tenant (respondent) counsel as the dependent variable. Standard errors clustered by zip code are given in parentheses. * $p < 0.05$, ** $p < 0.01$

Table 5: UA Intensity IV Results: UA Share by Zip-Fiscal-Year

	Main (1)	Address FE (2)
Respondent Counsel	0.158**	0.157**
	(0.037)	(0.027)
Judgment with Possession	-0.539**	-0.449**
	(0.125)	(0.117)
Log Judgment Amount	-3.125**	-2.854**
	(0.703)	(0.744)
Warrant Issued	-0.506**	-0.439**
	(0.127)	(0.122)
Warrant Executed	-0.091	-0.053
	(0.048)	(0.065)
Observations	403,483	202,409
First-Stage F-Stat	18.27	34.78
Covariates	Yes	Yes
Zip FE	Yes	Yes
Borough $\times$ Month FE	Yes	Yes
Address FE	No	Yes

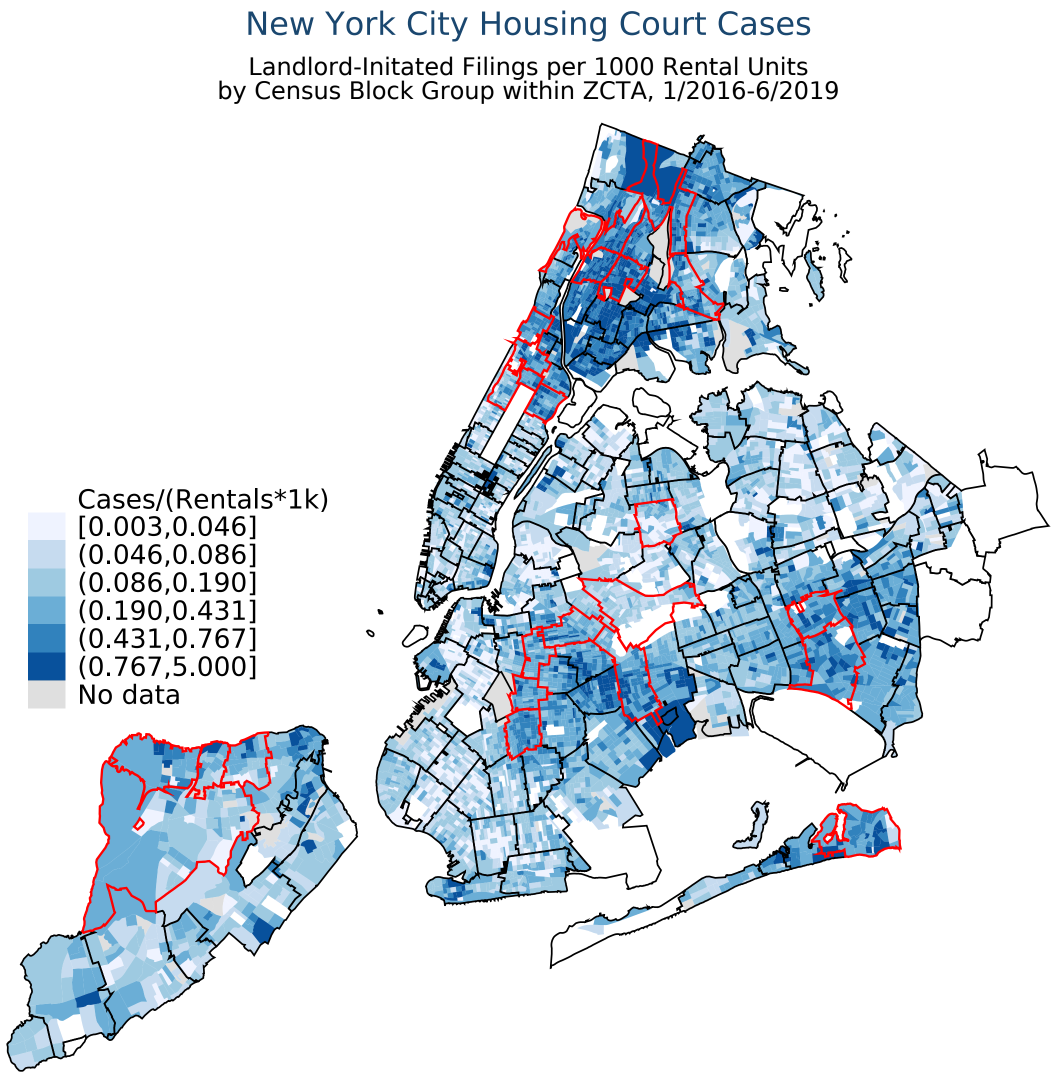
Outcomes are listed in rows. Analytical specifications are indexed by column. Sample is subsample of main sample cases filed in City Fiscal Years 2018 and 2019.Unit of observation is a housing court case. Each cell in Columns 1 and 2 reports the coefficient on tenant counsel from a separate instrumental variable regression of the row-enumerated outcome on the covariates and fixed effects summarized at the bottom of the table, using as the instrument the number of UA households served by zip-fiscal-year (divided by 1000). The first row reports first-stage results with tenant (respondent) counsel as the dependent variable. Standard errors clustered by zip code are given in parentheses. * $p < 0.05$, ** $p < 0.01$

Figure 1



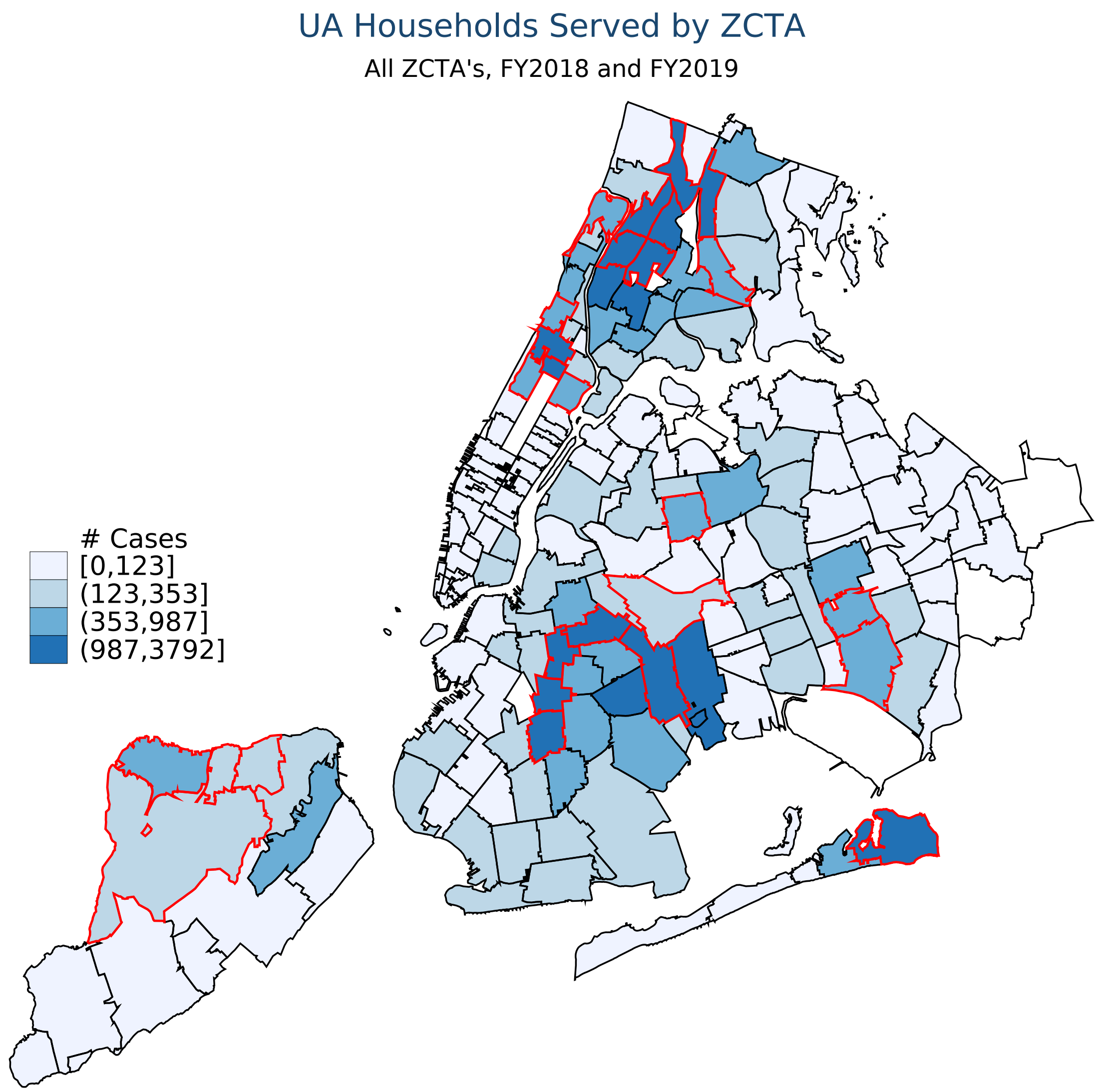
The figure depicts the census block groups comprising NYC's five boroughs.
Black lines delineate zip code tabulation areas. Red lines highlight UA ZCTA's.
Limits of shading bins set at 0, 10, 25, 50, 75, 90, 100 percentiles of CBG median household income, defined within the sample of NYC Housing Court cases.

Figure 2



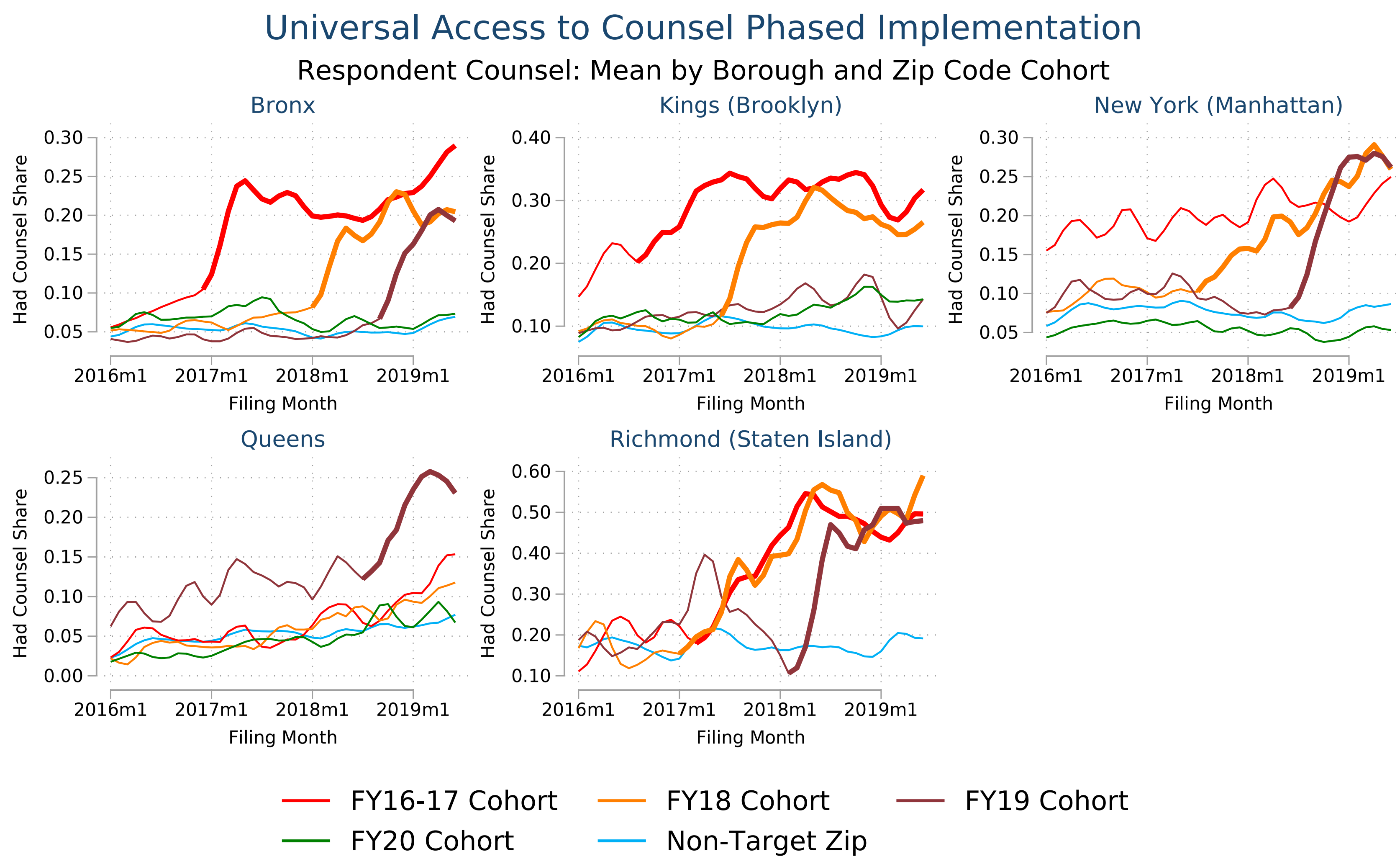
The figure depicits the census block groups comprising NYC's five boroughs.
Black lines delineate zip code tabulation areas. Red lines highlight UA ZCTA's.
Limits of shading bins are 0, 10, 25, 50, 75, 90 100 percentiles of housing court case counts
(specifically, landlord-initiated filings).

Figure 3



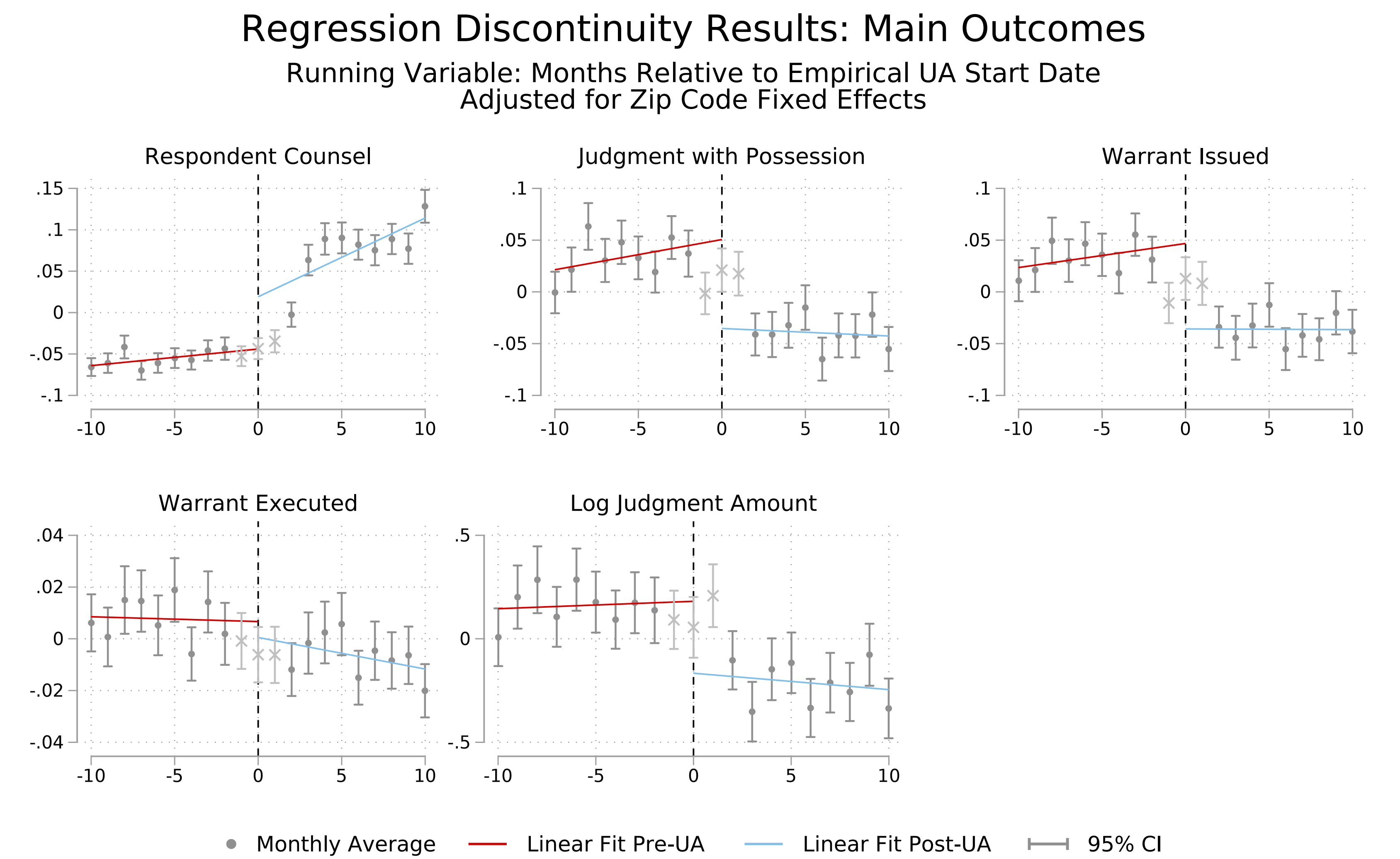
The figure depicts the zip codes comprising NYC's five boroughs.
Black lines delineate zip code tabulation areas. Red lines highlight UA ZCTA's.
Limits of shading bins are 0, 50, 75, 90 100 percentiles of UA household count from NYC DSS annual reports.

Figure 4



Bold indicates empirical UA treatment.
Monthly respondent counsel means are smoothed using local mean regression with a bandwith of one month.

Figure 5



Running variable is months relative to UA empirical start date. Bandwidth months [-10,-2] and [2,10]. Sample consists of subset of main sam
Controls for zip code fixed effects. Excluded donut [-1,-1] plotted for reference with X's.

Appendix

A.1 Universal Access Empirical Start Date Instrument Algorithm

As discussed in the main text, we devise an algorithm to identify empirical UA start dates. Our main instrumental variable is then an indicator for case filing subsequent to empirical program start. For each borough and zip-code cohort:

1. Calculate the share of tenants with counsel in our main sample for each month.

2. Smooth the data by fitting a local mean regression with a bandwidth of one month for each borough-cohort and generate smoothed predicted tenant representation rates for each borough-cohort-month.

3. Identify candidate start months by calculating lagging (t-1 to t) and leading (t to t+1) changes in smoothed representation rates. Candidate start months are then defined as those whose (a) lead-lag differential change is greater than one percentage point, and (b) leading change is positive.

4. If a borough-cohort has several consecutive candidate start months, keep only the first month in the streak as a candidate.

5. Calculate the absolute nine-month leading change in smoothed representation rates (t to t+9).

6. Refine the candidate start list to include only those months beginning a nine-month period with a cumulative increase in tenant representation of at least nine percentage points. (9 percent is the mean tenant representation rate in our sample, so this represents a 100 percent increase relative to the mean.)

7. If more than one candidate start date remains for a borough-cohort, select the month whose relative nine-month leading change (i.e., percent change) is the largest among all the candidates. UA start is the first day of that month.

A.2 Data Appendix

A.2.1 Covariates Definitions

Our covariates, grouped by data source, consist of the following.

1. **Housing Court**

    - Petitioner counsel: a 0–1 indicator for whether the landlord (petitioner) is represented by a lawyer.
    - Nonpayment: a 0–1 indicator for whether the case is a nonpayment case. Most LT cases are nonpayment cases (the omitted category here); all other cases are classified as "holdover," meaning that the purported violation is for something other than nonpayment of rent (e.g., staying past the end of a lease).
    - Court Borough: a categorical variable denoting the county in which the case is filed. New York City has five county housing courts, one for each of its five geographical boroughs: Bronx, Kings (Brooklyn), New York (Manhattan), Queens, and Richmond (Staten Island). There are also two specialized courts that also handle housing cases, which are each assigned to the borough in which they are located: Harlem (New York) and Red Hook (Kings).
    - Specialized Court Indicators: separate indicators for whether the case is filed is in the Harlem or Red Hook courts.
    - Time Effects: our main specifications include (court) borough-by-month fixed effects to flexibly control for secular trends and idiosyncratic shocks.
    - Respondent Count == 1: an indicator for whether there is a single (as opposed to multiple) respondents in a case.
    - Petitioner Count == 1: an indicator for whether there is a single (as opposed to multiple) petitioners in a case.
    - NYCHA: a 0–1 indicator for whether the case involves the New York City Housing Authority (public housing).
    - Specialty designation: a 0–1 indicator for the whether the case is flagged by the courts for having an attribute of interest (e.g., co-ops, condos). Excludes those cases flagged for specialty zips (for reasons of collinearity with our instruments).
    - Log(Primary Claim Amount): the natural logarithm of the total monetary claim by the landlord against the tenant, in real January 2021 dollars adjusted using the monthly Consumer Price Index for all urban consumers and winsorized at the

first and ninety-ninth percentiles (and with one dollar added to all claims before taking the log so as not to exclude cases with claim amounts of zero.)

2. **Census American Community Survey**
   ACS data comes from 2019 Five-Year Estimates. Unless otherwise noted, all ACS variables refer to the characteristics of an address' census block group.

   - A vector of census block group demographic attributes: total population, median household income, household poverty rate, total housing units, renter share of housing units, median gross rent, and population shares that are Hispanic, Black, Asian, White, ages 0–17, ages 65+, and female, as well as census tract shares of non-citizens and naturalized citizens (citizenship data is not available at the block group level). All CBG covariates are transformed into categorical quartiles defined within our sample and appended with a fifth "unknown" category to avoid dropping observations with missing data.
   - In several analyses, we also categorize CBG's with a series of indicators describing the block group's majority ($\geq$0.5 share) race is Hispanic, Black, White, or Asian.
   - All monetary variables from the ACS are in real 2019 dollars.

3. **PLUTO**
   PLUTO data comes from version 21v1 (February 2021). All PLUTO variables describe the characteristics of a housing unit's tax lot or building. Unless otherwise noted, all quartile covariates are defined within-sample. All indicator and categorical variables are appended with an "unknown" category to avoid dropping observations with missing data. All monetary variables from PLUTO are in real 2019 dollars.

   - Zoning district: four categories describing the tax lot's primary zoning classification (low-, medium-, and high-density residential; other (e.g., commercial, manufacturing))
   - Land use: five categories describing the tax lot's land use designation and summarizing its building class (1–2 family; multi-family walkup; multi-family elevator; mixed residential-commercial; other (e.g., commercial)).
   - Single building: a 0–1 indicator for whether a tax lot contains a single building.
   - Residential units: categorical quartiles describing the total number of residential units in a tax lot.

- Year built: three categories describing the year a building completed construction (<1947; 1947–1973; 1974–2021). Buildings with six or more units constructed in the 1947–1973 period are likely to be rent stabilized.
- Building altered: a 0–1 indicator for whether a building was altered in a manner that changed its value after initial construction.
- Lot area: categorical quartiles of the total area of the tax lot, measured in millions of square feet.
- Building-to-lot area ration: categorical quartiles of the total building floor area ratio divided by the tax lot area. Also known as built floor area ratio.
- Property assessed total: categorical quartiles the total assessed value of the tax lot, measured in millions of dollars, as recorded in the Department of Finance's (DOF) FY22 Tentative Assessment Roll.
- Landlord characteristics: categorical quartiles (appended with unknowns) of property owner's number of NYC properties, number of NYC buildings, number of NYC residential units, sum of assessed total value, within-sample housing court cases, and within-sample housing court cases per number of residential units.
- Rent Stabilization Eligible: a 0–1 indicator for whether a housing unit is likely to be rent stabilized, which means that the NYC Rent Guidelines Board sets limits on allowable annual rent increases. Buildings meeting the following criteria are likely to be rent stabilized in NYC: (1) constructed between 1947 and 1973, inclusive, (2) contains six or more units, and (3) is not a co-op, condo, or NYCHA. Note that not every unit in a rent-stabilized building is necessarily rent stabilized, as historically some units became deregulated when rent exceeded certain thresholds. In addition, though less common, newer buildings may be temporarily rent stabilized if they receive 421-a or J-51 tax exemptions. Rent stabilization is the primary form of rent control in NYC (a smaller number of units are "rent controlled") (NYC Rent Guidelines Board, 2022; NYU Furman, 2022).

A.2.2 Additional Outcomes

As a supplement to our main analysis, we additionally provide results for the following outcomes as robustness checks and to gain insights into potential pathways for lawyer effects.

- **Judgment Vacated (Conditional on Judgemnt)**: a 0–1 indicator for whether the last judgment in a case is vacated, entered in error, rejected, or stayed, defined for only the subset of cases with an entered judgment.

- **Judgment Type: Stipulation/Settlement**: a 0–1 indicator for whether the basis of a (non-vacated) judgment is a negotiated stipulation (i.e., settlement) between landlord and tenant.

- **Judgment Type: Failure to Appear**: a 0–1 indicator for whether the basis of a (non-vacated) judgment is a tenant's failure to answer or failure to appear in court.

- **Judgment Type: Court Proceeding**: a 0–1 indicator for whether the basis of a (non-vacated) judgment involves active resolution by a judge, including through hearing, trial, or other rulings.

- **Warrant Vacated (Conditional on Warrant)**: a 0–1 indicator for whether a warrant of eviction is vacated in a case, as defined by the presence of any warrant vacated date, defined only for the subset of cases where a warrant is ordered.

- **Length of Stay: Judgment Entered**: the number of days between filing date and final judgment date.

- **Length of Stay: Warrant Executed**: the number of days between filing date and final warrant execution date.

A Note on Dispositions: Disposition, or whether a case has been officially closed by the court, is not an informative outcome in the housing court data. During the course of our analysis, we found that it is common for cases to remain open but "dormant" for inconsistent and often long (over a year) periods after the involved parties have ceased actively pursuing them. In particular, OCA implemented "mass disposals" of dormant cases on two particular dates during our study period. Per OCA, we believe we are the first to raise this issue in the academic literature. This issue is important because, in the cross-section, it is not clear whether a non-disposed case is right-censored or concluded.

A.3 Complier Characterization

To characterize compliers, we use a procedure similar to that described by Angrist and Pischke (2008), Abadie (2003), Dahl, Kostøl and Mogstad (2014), and Dobbie, Goldin and Yang (2018). There are two steps. First, we estimate the share of the sample that are compliers. Second, we identify their average characteristics.

The complier share is the proportion of tenants whose treatment status depends on the instrument: those tenants who have a lawyer if and only if UA is operating in their zip code. Using potential outcomes notation, $R_i(Z_i = 1) > R_i(Z_i = 0)$. The complier share (CS) can thus be estimated from the Wald first stage (i.e., first stage without covariates):

$$\begin{aligned} CS &= R_i(Z_i = 1) - R_i(Z_i = 0) \\ &= (\widehat{\pi}_0 + \widehat{\pi}_1 \times 1) - (\widehat{\pi}_0 + \widehat{\pi}_1 \times 0) \\ &= \widehat{\pi}_1 \end{aligned}$$

where $\widehat{\pi}_0$ and $\widehat{\pi}_1$ are the intercept and slope coefficients, respectively, from the Wald first stage.

Similarly, always-takers are those who are treated even without UA, $AS = \widehat{\pi}_0$, and never-takers are those who are do not have a lawyer even with UA, $NS = 1 - \widehat{\pi}_0 - \widehat{\pi}_1$. Using this simple linear Wald first stage, we estimate that the complier share is 15.8 percent. Always-takers comprise 7.7 percent of the sample, while never-takers represent 76.5 percent.

While it is impossible to identify individual compilers, describing their average characteristics is a straightforward application of Bayes' rule.

For a binary characteristic, X, the mean is a probability, $E(X) = 1 \cdot Pr(X)$. Letting C be an indicator for complier, and NC for non-complier, what we want to estimate is $E(X|C) = Pr(X = 1|C = 1)$. This expression cannot be evaluated directly, because compliance is based on unobserved counterfactuals. Fortunately, Bayes' Rule allows a reformulation in terms of known quantities $Pr(X = 1|C = 1) = \frac{Pr(X \cap C)}{Pr(C)} = \frac{Pr(C|X)Pr(X)}{Pr(C)}$. All of the quantities in the last expression are estimable the data. $Pr(X)$ is just the mean of X in the full sample. $Pr(C) = \widehat{\pi}_1$ is the complier share of the sample, estimated above. $Pr(C|X) = Pr(C = 1|X = 1)$ is the complier share in the subpopulation with the characteristic of interest, $\widehat{\pi}_1^X$, estimated from the Wald first stage in the subsample with $X = 1$.

Similarly, the non-complier, NC, mean is $E(X = 1|C = 0) = \frac{Pr(X=1 \cap C=0)}{1-Pr(C)} = \frac{Pr(X=1)(1-Pr(C=1|X=1))}{1-Pr(C=1)} = \frac{Pr(X=1)-Pr(X=1)Pr(C=1|X=1)}{1-Pr(C=1)}$.

For continuous characteristics, we partitioning the covariate into discrete deciles, repeat the above algorithm for each decile, and then take a weighted average. We calculate standard errors and perform a formal mean comparison using 200 bootstrap replications.

Table A.1: UA Zip Code Cohorts

	Boroughs & Zip Codes				
Cohort	Bronx	Brooklyn	Manhattan	Queens	Staten Island
FY16–17 Cohort	10457, 10467	11216, 11221	10026, 10027	11433, 11434	10302, 10303
FY18 Cohort	10468	11225	10025	11373	10314
FY19 Cohort	10462	11226	10031	11385	10310
FY20 Cohort	10453	11207	10029, 10034	11691	

Table A.2: Universal Access to Counsel Empirical Start Dates by Borough and Zip Code Cohort

	UA Cohort				
	Non-Target	FY16-17	FY18	FY19	FY20
Court Borough					
Bronx	.	2016m12	2018m1	2018m9	.
Kings (Brooklyn)	.	2016m8	2017m6	.	.
New York (Manhattan)	.	.	2017m7	2018m6	.
Queens	.	.	.	2018m7	.
Richmond (Staten Island)	.	2017m3	2017m1	2018m2	

Start dates in YEARmMONTH format. Blank cells indicate no empirical UA start date. Cohort 0 consists of all zip codes not part of a pilot treatment cohort. UA was officially signed into law 2017m8. Zip code cohorts, as referred to in the column headings, were intended to roll out on a city fiscal year basis. City fiscal years begin in July (m7) and end in June (m6) and are named for the ending calendar year. For example, the 2017 fiscal year ran from 2016m7 to 2017m6.

Table A.3: First Stage and Reduced Form Results: Universal Access to Counsel and Housing Court Outcomes

	UA Indicator		UA Intensity	
	Main (1)	Addr FE (2)	Main (3)	Addr FE (4)
Respondent Counsel	0.124**	0.118**	0.158**	0.157**
	(0.006)	(0.008)	(0.037)	(0.027)
Judgment with Possession	-0.040**	-0.039**	-0.085**	-0.070**
	(0.006)	(0.008)	(0.022)	(0.023)
Log Judgment Amount	-0.263**	-0.297**	-0.494**	-0.447**
	(0.042)	(0.060)	(0.094)	(0.123)
Warrant Issued	-0.040**	-0.040**	-0.080**	-0.069**
	(0.005)	(0.008)	(0.021)	(0.024)
Warrant Executed	-0.010**	-0.007	-0.014	-0.008
	(0.003)	(0.004)	(0.009)	(0.011)
Observations	727,692	456,788	403,483	202,409
Covariates	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes
Borough × Month FE	Yes	Yes	No	No
Address FE	No	Yes	No	Yes

Outcomes are listed in rows. Analytical specifications are indexed by column. All results are for the main sample. Unit of observation is a housing court case. Each cell in Columns 1 and 2 reports the coefficient on an indicator for empirical UA treatment (i.e., the instrument in the main IV results) from a separate regression of the row-enumerated outcome on the covariates and fixed effects summarized at the bottom of the table. Columns 3 and 4 report analogous results for the UA intensity instrument (UA households served by zip-year, divided by 1000). Standard errors clustered by zip code are given in parentheses. * $p < 0.05$, ** $p < 0.01$

Table A.4A: Complier Characteristics

	C (1)	NC (2)	Diff (3)
Nonpayment	0.822	0.878	-0.056**
	(0.000)	(0.000)	[-12.265]
Log Primary Claim	6.432	6.922	-0.491**
	(0.001)	(0.000)	[-13.123]
Bronx	0.332	0.361	-0.029**
	(0.000)	(0.000)	[-8.406]
Kings (Brooklyn)	0.320	0.265	0.055**
	(0.000)	(0.000)	[10.760]
New York (Manhattan)	0.141	0.215	-0.075**
	(0.000)	(0.000)	[-12.408]
Queens	0.126	0.147	-0.021
	(0.000)	(0.000)	[-1.343]
Richmond (Staten Island)	0.032	0.020	0.012**
	(0.000)	(0.000)	[9.103]
NYCHA	0.101	0.211	-0.110**
	(0.000)	(0.000)	[-20.594]
CT Naturalized Pct.	0.176	0.190	-0.014**
	(0.000)	(0.000)	[-10.787]
CT Noncitizen Pct.	0.150	0.165	-0.015**
	(0.000)	(0.000)	[-15.291]
CBG Hispanic Pct.	0.394	0.420	-0.026**
	(0.000)	(0.000)	[-9.156]
CBG Black Pct.	0.373	0.342	0.032**
	(0.000)	(0.000)	[8.790]
CBG Asian Pct.	0.069	0.069	0.001
	(0.000)	(0.000)	[0.392]
CBG White Pct.	0.154	0.143	0.011**
	(0.000)	(0.000)	[3.567]
CBG 0-17 Years Pct.	0.231	0.230	0.001
	(0.000)	(0.000)	[1.289]
CBG 65+ Years Pct.	0.125	0.131	-0.006**
	(0.000)	(0.000)	[-6.880]
CBG Female Pct.	0.538	0.541	-0.003**
	(0.000)	(0.000)	[-3.592]

This table summarizes the average observable characteristics of tenants who are compliers with the empirical universal access to counsel instrument (an indicator equal to one if UA is operating in a tenant's borough and zip code cohort at the time of case filing.) Compliers are tenants whose legal representation is affected by the instrument: that is, those who have lawyers when UA is operating, but not otherwise. Non-compliers are always- and never-takers.Columns 1 and 2 give the complier and non-complier means, respectively, for the row-enumerated characteristics. Standard errors, computed from 200 bootstrap replications are in parentheses. Column 3 gives the differences in means, with test statistics in brackets. The algorithm for estimating these means is described in Appendix A.3.
* $p < 0.05$, ** $p < 0.01$

Table A.4B: Complier Characteristics

	C (1)	NC (2)	Diff (3)
CBG HH Median Income/1000 (in 2019$)	48.45	48.03	0.42
	(0.14)	(0.01)	[1.08]
CBG Poverty Pct.	0.282	0.284	-0.002
	(0.000)	(0.000)	[-0.957]
CBG Rental Units Pct.	0.828	0.868	-0.040**
	(0.000)	(0.000)	[-15.257]
CBG Median Gross Rent/1000 (in 2019$)	1.32	1.20	0.12**
	(0.00)	(0.00)	[16.38]
Rent Stabilization Eligible	0.080	0.131	-0.051**
	(0.000)	(0.000)	[-14.600]
Specialty Designation	0.030	0.042	-0.011**
	(0.000)	(0.000)	[-3.868]
Zone Dist.: Res. Low Density	0.287	0.186	0.101**
	(0.000)	(0.000)	[13.508]
Zone Dist.: Res. Medium Density	0.625	0.637	-0.011*
	(0.000)	(0.000)	[-2.431]
Zone Dist.: Res. High Density	0.086	0.110	-0.024**
	(0.000)	(0.000)	[-9.363]
Land Use: 1-2 Family	0.096	0.044	0.052**
	(0.000)	(0.000)	[16.285]
Land Use: Multi-Family Walkup	0.270	0.244	0.026**
	(0.000)	(0.000)	[6.138]
Land Use: Multi-Family Elevator	0.391	0.485	-0.094**
	(0.000)	(0.000)	[-16.421]
Land Use: Mixed Res.-Comm.	0.199	0.219	-0.020**
	(0.000)	(0.000)	[-4.291]
Building-to-Lot Area Ratio	2.92	3.43	-0.52**
	(0.00)	(0.00)	[-24.39]
Lot Assessed Value/1000000 (in 2021$)	6.29	11.17	-4.88**
	(0.11)	(0.00)	[-14.46]
Landlord Units	29023.76	43507.92	-1.4e+04**
	(8.3e+05)	(30987.85)	[-15.64]
Landlord Cases Per Units	0.79	0.83	-0.04**
	(0.00)	(0.00)	[-6.61]
Filed Month	6.169	6.102	0.067
	(0.001)	(0.000)	[1.839]
Filed Year	2017.378	2017.197	0.181**
	(0.001)	(0.000)	[7.757]

This table summarizes the average observable characteristics of tenants who are compliers with the empirical universal access to counsel instrument (an indicator equal to one if UA is operating in a tenant's borough and zip code cohort at the time of case filing.) Compliers are tenants whose legal representation is affected by the instrument: that is, those who have lawyers when UA is operating, but not otherwise. Non-compliers are always- and never-takers.Columns 1 and 2 give the complier and non-complier means, respectively, for the row-enumerated characteristics. Standard errors, computed from 200 bootstrap replications are in parentheses. Column 3 gives the differences in means, with test statistics in brackets. The algorithm for estimating these means is described in Appendix A.3.

* $p < 0.05$, ** $p < 0.01$

Table A.5: Outcome Means

	All Cases				With Activity Only	
	Main Sample (1)	UA Zips (2)	Addr FE Sample (3)	RD Sample (4)	All Years (5)	2016 w/o Counsel (6)
Respondent Counsel	0.090	0.132	0.404	0.163	0.125	0.000
Empirical UA Treatment (IV)	0.085	0.279	0.200	0.560	0.087	0.012
UA Households Served/1000 (IV)	0.230	0.435	0.351	0.586	0.234	0.000
Judgment with Possession	0.415	0.400	0.393	0.407	0.576	0.623
Log Judgment Amount	1.728	1.693	2.027	1.780	2.397	2.610
Warrant Issued	0.349	0.343	0.353	0.359	0.484	0.527
Warrant Executed	0.070	0.065	0.058	0.071	0.097	0.108
Judgment Vacated (Cond. on Judgment)	0.126	0.148	0.258	0.166	0.126	0.103
Warrant Vacated (Cond. on Warrant)	0.072	0.081	0.155	0.094	0.099	0.087
Judgment: Stip/Settle	0.230	0.220	0.239	0.225	0.319	0.340
Judgment: FTA	0.179	0.175	0.144	0.177	0.249	0.277
Judgment: Court Proceeding	0.006	0.005	0.009	0.004	0.008	0.006
Days to Judgment Entered	68.6	70.6	85.4	70.1	68.6	59.8
Days to Warrant Executed	195.2	199.8	215.2	199.1	195.2	194.9
Observations	727,703	220,383	56,673	85,680	524,650	142,829

Rows index treatment, instruments, and outcomes. Columns define samples of interest. Each cell gives the the mean for the row-indexed variable in the column-indexed sample. Column 1 is the main (full) sample. Column 2 is UA treatment (pilot) zip codes only. Column 3 is the address fixed effects sample; specifically, it is the subset of cases contributing to identification of respondent counsel effects in the address FE sample. Column 4 is the regression discontinuity sample. Column 5 is the subsample of main sample cases with activity beyond initial filing. Column 6 further refines Column 5 by further limiting the sample to filings from 2016 where the tenant did not have a lawyer.

Table A.6: Main Results: Respondent Counsel and Housing Court Outcomes, Excluding Queens

	Main		Address FE	
	OLS (1)	UA IV (2)	OLS (3)	UA IV (4)
Respondent Counsel (First Stage)		0.125**		0.119**
		(0.006)		(0.008)
Judgment with Possession	-0.086**	-0.314**	-0.064**	-0.333**
	(0.006)	(0.041)	(0.008)	(0.055)
Log Judgment Amount	0.116**	-2.081**	-0.119*	-2.512**
	(0.039)	(0.291)	(0.047)	(0.406)
Warrant Issued	-0.082**	-0.316**	-0.059**	-0.340**
	(0.005)	(0.037)	(0.008)	(0.057)
Warrant Executed	-0.031**	-0.077**	-0.009**	-0.057
	(0.002)	(0.020)	(0.003)	(0.034)
Observations	623,050	623,050	402,075	402,075
First-Stage F Stat	.	515.52	.	242.82
Covariates	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes
Borough × Month FE	Yes	Yes	Yes	Yes
Address FE	No	No	Yes	Yes

Outcomes are listed in rows. Analytical specifications are indexed by column. All results are for the main sample, excluding cases from Queens. Unit of observation is a housing court case. Each cell in Columns 1–4 reports the coefficient on respondent (tenant) counsel from a separate regression of the row-enumerated outcome on the covariates and fixed effects summarized at the bottom of the table. Columns 1 and 3 report the ordinary least squares linear associations between outcomes and tenant counsel. Columns 2 and 4 report two-stage least squares instrumental variable results for tenant counsel, using an indicator for empirical UA treatment (i.e., program rollout) as the instrument (equal to one if UA is operating in a case's zip code at the time of filing). Supercolumns group specifications by the major fixed effects included. Columns 1 and 2 control for zip and court borough by month fixed effects, while Columns 3 and 4 additionally control for address fixed effects. First row reports first-stage results with tenant (respondent) counsel as the dependent variable. Standard errors clustered by zip code are given in parentheses. * $p < 0.05$, ** $p < 0.01$

Table A.7: Main Results: Respondent Counsel and Housing Court Outcomes, Excluding NYCHA and Cases without Activity beyond Initial Filing

	Main		Address FE	
	OLS (1)	UA IV (2)	OLS (3)	UA IV (4)
Respondent Counsel (First Stage)		0.173**		0.175**
		(0.008)		(0.009)
Judgment with Possession	-0.204**	-0.387**	-0.159**	-0.426**
	(0.007)	(0.041)	(0.010)	(0.050)
Log Judgment Amount	-0.413**	-2.401**	-0.546**	-2.858**
	(0.039)	(0.302)	(0.060)	(0.507)
Warrant Issued	-0.191**	-0.370**	-0.149**	-0.410**
	(0.007)	(0.040)	(0.011)	(0.052)
Warrant Executed	-0.049**	-0.086**	-0.024**	-0.099**
	(0.002)	(0.025)	(0.003)	(0.035)
Observations	431,038	431,038	218,780	218,780
First-Stage F Stat	.	527.59	.	355.67
Covariates	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes
Borough × Month FE	Yes	Yes	Yes	Yes
Address FE	No	No	Yes	Yes

Outcomes are listed in rows. Analytical specifications are indexed by column. All results are for the main sample, excluding NYCHA cases and those with no activity beyond initial filing. Unit of observation is a housing court case. Each cell in Columns 1–4 reports the coefficient on respondent (tenant) counsel from a separate regression of the row-enumerated outcome on the covariates and fixed effects summarized at the bottom of the table. Columns 1 and 3 report the ordinary least squares linear associations between outcomes and tenant counsel. Columns 2 and 4 report two-stage least squares instrumental variable results for tenant counsel, using an indicator for empirical UA treatment (i.e., program rollout) as the instrument (equal to one if UA is operating in a case's zip code at the time of filing). Supercolumns group specifications by the major fixed effects included. Columns 1 and 2 control for zip and court borough by month fixed effects, while Columns 3 and 4 additionally control for address fixed effects. First row reports first-stage results with tenant (respondent) counsel as the dependent variable. Standard errors clustered by zip code are given in parentheses. * $p < 0.05$, ** $p < 0.01$

Table A.8: IV Results: Additional Specifications

	Empirical UA Treatment (Main IV)							Intensity IV	
	All Years			FY18–19				FY18–19	
	No Covs (1)	No FE (2)	Zip FE (3)	No Covs (4)	No FE (5)	Main (6)	Addr. FE (7)	No Covs (8)	No FE (9)
Respondent Counsel	0.158**	0.147**	0.127**	0.162**	0.154**	0.107**	0.095**	0.083**	0.108**
	(0.015)	(0.009)	(0.006)	(0.016)	(0.012)	(0.006)	(0.009)	(0.012)	(0.009)
Judgment with Possession	-0.235**	-0.350**	-0.590**	-0.221*	-0.312**	-0.437**	-0.327**	-0.208*	-0.334**
	(0.081)	(0.037)	(0.040)	(0.085)	(0.039)	(0.081)	(0.108)	(0.103)	(0.039)
Log Judgment Amount	-0.758	-1.614**	-3.255**	-0.809	-1.470**	-2.535**	-2.641**	1.736	-2.084**
	(0.532)	(0.236)	(0.320)	(0.570)	(0.247)	(0.423)	(0.433)	(0.920)	(0.315)
Warrant Issued	-0.114	-0.353**	-0.555**	-0.083	-0.307**	-0.439**	-0.270**	0.081	-0.301**
	(0.077)	(0.035)	(0.040)	(0.088)	(0.039)	(0.070)	(0.089)	(0.120)	(0.049)
Warrant Executed	-0.040	-0.077**	-0.186**	-0.026	-0.055**	-0.117*	0.165*	-0.171*	-0.104**
	(0.032)	(0.019)	(0.014)	(0.032)	(0.020)	(0.052)	(0.080)	(0.083)	(0.036)
Observations	727,703	727,703	727,692	403,495	403,495	403,483	202,409	403,495	403,495
First-Stage F-Stat		257.09	459.37		175.60	314.51	109.72	48.10	144.22
Covariates	No	Yes	Yes	No	Yes	Yes	Yes	No	Yes
Zip FE	No	No	Yes	No	No	Yes	Yes	No	No
Borough × Month FE	No	No	No	No	No	No	No	No	No
Address FE	No	No	No	No	No	No	Yes	No	No

Outcomes are listed in rows. Analytical specifications are indexed by column. Unit of observation is a housing court case. Each cell reports the coefficient on tenant counsel from a separate instrumental variable regression of the row-enumerated outcome on the covariates and fixed effects summarized at the bottom of the table. Top supercolumns group specifications by the instrument used. Colums 1–5 use the main instrument: an indicator for empirical UA treatment (equal to one if UA is operating in a case's borough and zip code at the time of filing). Column 6 uses the UA intensity instrument (UA households served by zip-year, divided by 1000). Second-level supercolumns group specifications by the years included. Columns 1 and 2 include all years (i.e., the main sample), while Columns 3–6 limit the analysis to the subsample of cases filed in City Fiscal Years 2018 and 2019, (for comparability with the UA intensity instrument). The first row reports first-stage results with tenant (respondent) counsel as the dependent variable. Standard errors clustered by zip code are given in parentheses. * $p < 0.05$, ** $p < 0.01$

Table A.9: Alternative Difference-in-Differences Estimators for Two-Way Fixed Effects with Staggered Treatment: Zip-Month Panel Results

	Full	Zip-Month Panel				
	OLS	OLS	BJS	CS	CD	SA
	(1)	(2)	(3)	(4)	(5)	(6)
Respondent Counsel	0.122**	0.122**	0.124**	0.117**	0.129**	0.123**
	(0.007)	(0.007)	(0.002)	(0.013)	(0.008)	(0.003)
Judgment with Possession	-0.045**	-0.045**	-0.046**	-0.043**	-0.040**	-0.045**
	(0.006)	(0.006)	(0.003)	(0.010)	(0.010)	(0.003)
Log Judgment Amount	-0.301**	-0.303**	-0.300**	-0.236**	-0.259**	-0.297**
	(0.041)	(0.042)	(0.039)	(0.056)	(0.065)	(0.040)
Warrant Issued	-0.041**	-0.042**	-0.041**	-0.041**	-0.037**	-0.040**
	(0.005)	(0.006)	(0.003)	(0.010)	(0.010)	(0.003)
Warrant Executed	-0.007*	-0.008**	-0.010**	-0.014**	-0.010**	-0.010**
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Obs.	727,692	7,483	7,496	7,460	7,496	7,483
Zip FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	No	No

This table assesses the robustness of our first-stage and reduced form results to recently proposed alternatives estimators of average treatment effects on the treated for two-way fixed effects models, where treatment is a function of cohort and time indicators and adoption is staggered across time. Outcomes are listed in rows. Estimators are indexed by column. Each cell represents a distinct estimate of the overall static ATT. Column 1 gives the full sample OLS TWFE estimate, where the unit of observation is an individual housing court case (pooled cross section), as in the main analysis. Columns 2–6 are estimated in a panel collapsed at the zip-month level, with results weighted by the number of observations in each cell. Column 2 reproduces the standard OLS TWFE estimate for the zip-month panel. Column 3 gives the Borusyak, Jaravel, Spiess (2022) estimator. Column 4 gives the Callaway and Sant'Anna (2021) estimator. Column 5 gives the de Chaisemartin and D'Haultfoeuille (2020) estimator. Column 6 gives the Sun and Abraham (2021) estimator. For simplicity, no time- or group-varying covariates are included in any model. For treated zips, pre-treatment outcomes are set to pre-treatment mean for all pre-treatment months. For the CD (2020) estimator, reported number of observations is the total observations in zip-month panel, since the manner in which this estimator counts observations used in estimation is not comparable with the other estimators (Stata reports that 3.6 million outcome and treatment first differences are used in the estimation). Standard errors clustered by zip code are given in parentheses. * $p < 0.05$, ** $p < 0.01$

Table A.10: Address-Level 15-Month Outcomes

	Main	
	OLS	UA IV
	(1)	(2)
Case Filed	-0.116**	0.034
	(0.004)	(0.068)
Judgment with Possession	-0.098**	-0.233**
	(0.006)	(0.034)
Log Judgment Amount	0.070	-1.551**
	(0.045)	(0.277)
Warrant Issued	-0.095**	-0.196**
	(0.006)	(0.039)
Warrant Executed	-0.038**	-0.086**
	(0.003)	(0.025)
Observations	637,981	637,981
First-Stage F Stat	.	257.84
Covariates	Yes	Yes
Zip FE	Yes	Yes
Borough × Month FE	Yes	Yes
Address FE	No	No

This table repeats the main analysis for address-level outcomes. The unit of observation remains an individual housing court case. However, dependent variables measure the culmative outcome at the apartment-unit level address in the 15 months following the date of case filing, regardless of whether the outcome took place in a separate case. Outcomes are listed in rows. Analytical specifications are indexed by column. All results are for the subset of main sample cases filed through December 2018 (to allow for 15 months follow-up pre-COVID). Each cell reports the coefficient on respondent (tenant) counsel from a separate regression of the row-enumerated outcome on the covariates and fixed effects summarized at the bottom of the table. Column 1 reports the ordinary least squares linear associations between outcomes and tenant counsel. Column 2 reports two-stage least squares instrumental variable results for tenant counsel, using an indicator for empirical UA treatment (i.e., program rollout) as the instrument (equal to one if UA is operating in a case's zip code at the time of filing). Supercolumns group specifications by the major fixed effects included. First row reports first-stage results with tenant (respondent) counsel as the dependent variable. Standard errors clustered by zip code are given in parentheses. * $p < 0.05$, ** $p < 0.01$

Table A.11: Regression Discontinuity Results: Months Since UA Start

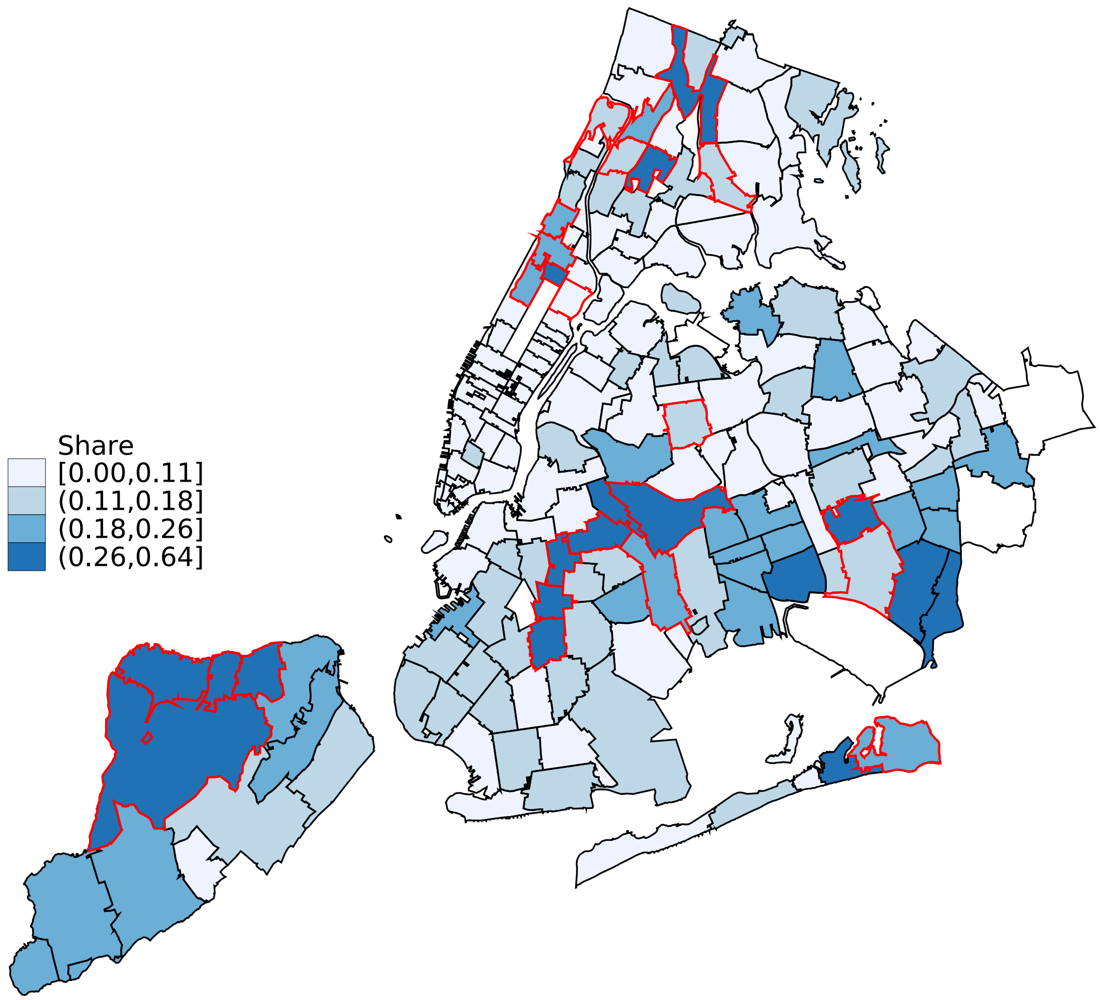
	(1)	(2)	(3)
Respondent Counsel	0.066**	0.063**	0.064**
	(0.012)	(0.013)	(0.014)
Judgment with Possession	-1.295*	-1.285	-1.300
	(0.576)	(0.637)	(0.620)
Log Judgment Amount	-5.580	-5.174	-5.639
	(3.204)	(3.559)	(3.606)
Warrant Issued	-1.250*	-1.222	-1.236*
	(0.535)	(0.570)	(0.553)
Warrant Executed	-0.103	-0.101	-0.111
	(0.100)	(0.110)	(0.111)
Observations	36,856	36,855	36,855
First-Stage F-Stat	31.93	25.57	22.79
Bandwidth	[-10,10]	[-10,10]	[-10,10]
Donut	[-1,1]	[-1,1]	[-1,1]
Covariates	No	Zip FE	Non-Time
Polynomial	Linear	Linear	Linear
Diff. Slopes	Yes	Yes	Yes
Running Var.	Month	Month	Month

This table summarizes fuzzy regression discontinuity results for the relationship between tenant counsel and housing court outcomes. The running variable is months since empirical Universal Access zip code start month at time of case filing. Outcomes are listed in rows. Analytical specifications are indexed by column, with features summarized at the bottom of the table. All results are for the RD subsample of cases from the first three UA zip cohorts with +/-10 months of UA start in each zip. As in the main IV analysis, the instrument that defines threshold-crossing is empirical UA treatment, an indicator equal to one if UA is in operation in a given case's zip code at the date of filing. All specification allow for separate slopes of the local linear regressions on each side of the threshold and exclude a donut of +/-1 month around UA start. Unit of observation is a housing court case. Each cell in the first row reports first-stage OLS results for the UA instrument with tenant (respondent) counsel as the dependent variable. Each cell in all following rows reports the coefficient on tenant counsel from a separate regression of the row-enumerated outcome.Standard errors clustered by zip code are given in parentheses. * $p < 0.05$, ** $p < 0.01$

Figure A.1

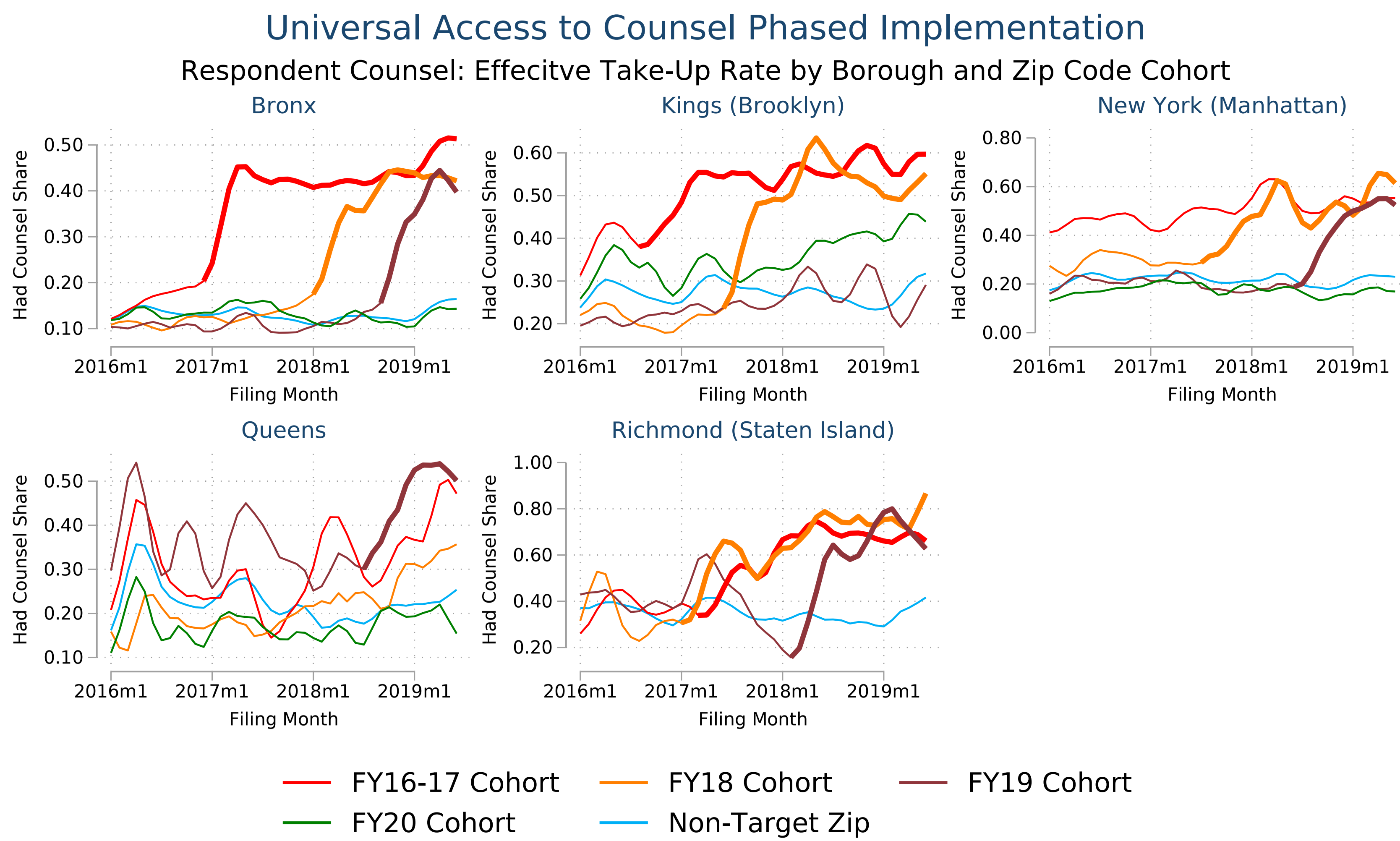
UA Households Share by ZCTA

All ZCTA's, FY2018 and FY2019



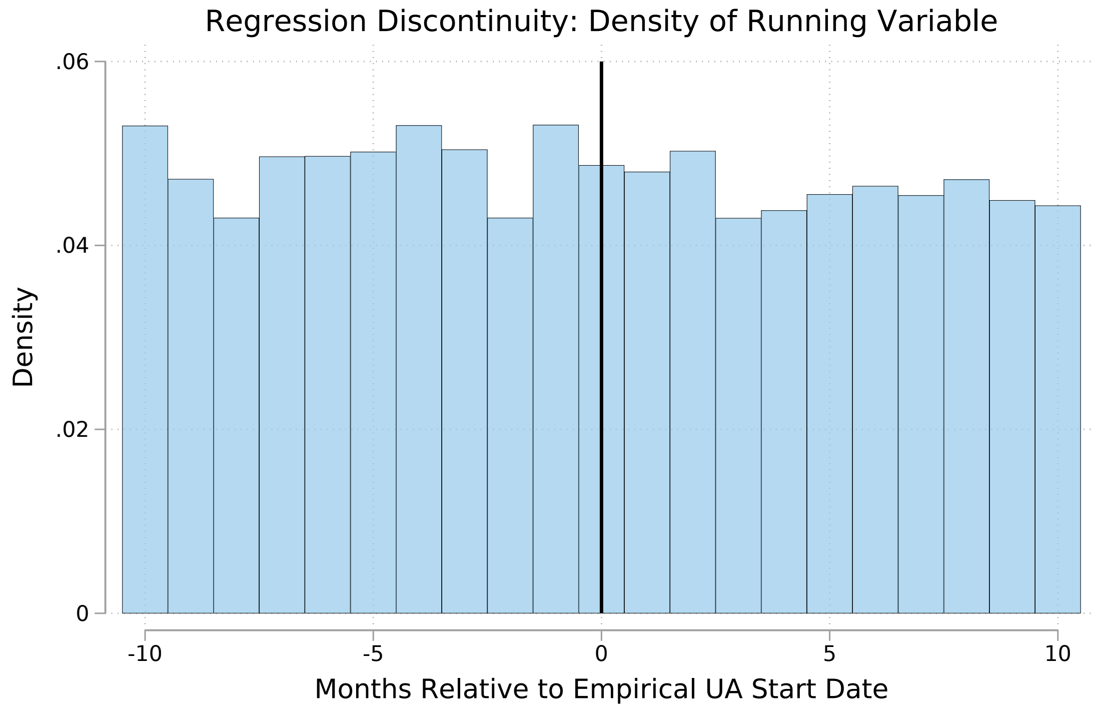
The figure depicits the zip codes comprising NYC's five boroughs.
Black lines delineate zip code tabulation areas. Red lines highlight UA ZCTA's.
Limits of shading bins are 0, 50, 75, 90 100 percentiles of UA household count from NYC DSS annual reports, divided by total housing court filings by

Figure A.2



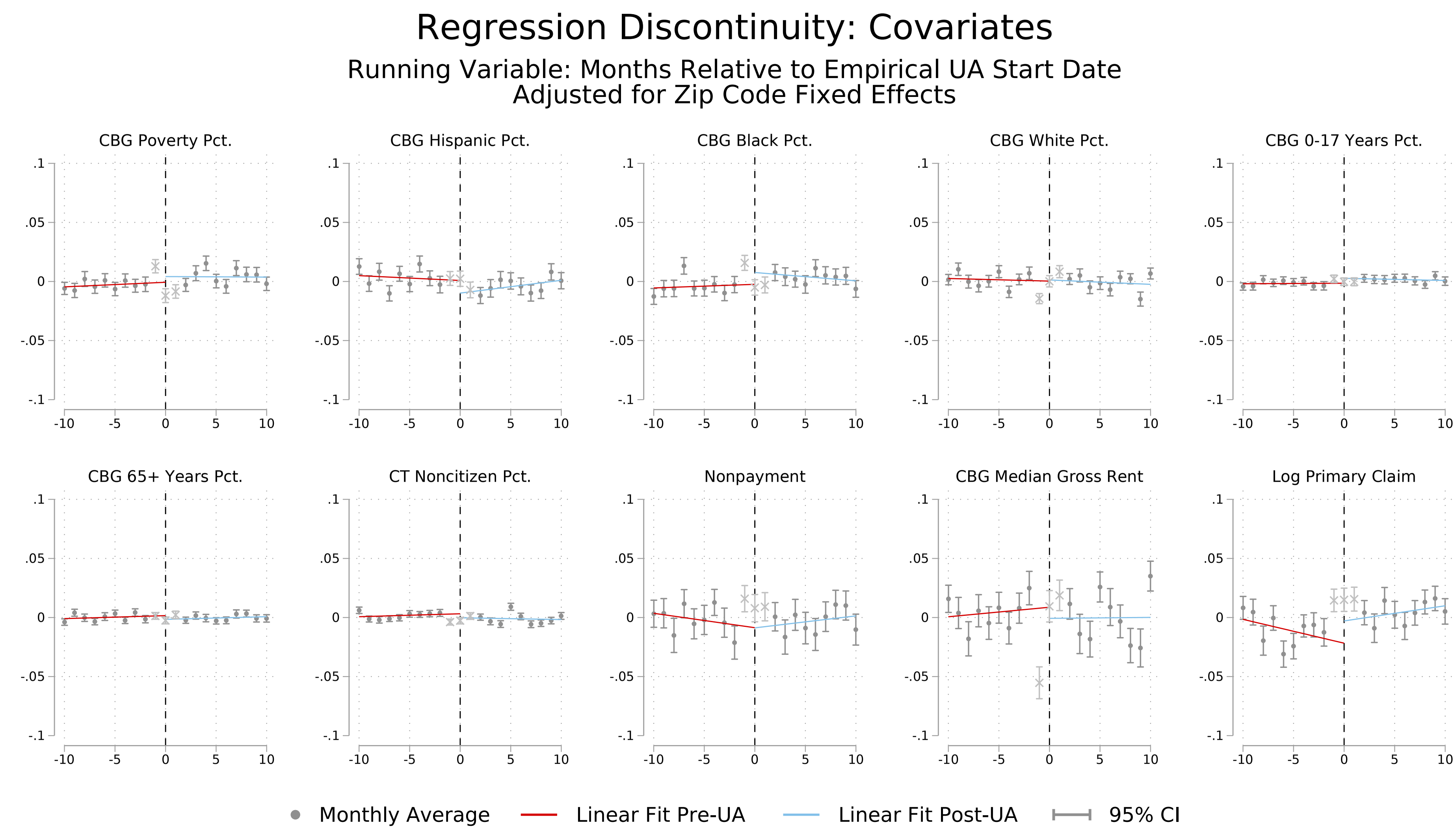
Bold indicates empirical UA treatment.
Monthly respondent counsel means are smoothed using local mean regression with a bandwith of one month.
Excludes NYCHA cases, cases with no activity beyond initial filing, and cases where the respondent never appeared in housing court.

Figure A.3



Regression Discontinuity: Density of Running Variable
.06
.04
.02
0
Density
-10
-5
0
5
10
Months Relative to Empirical UA Start Date

Figure A.4



Running variable is months relative to UA empirical start date. Bandwidth months [-10,-2] and [2,10]. Sample consists of subset of main sample cases from first three UA zip code cohorts. Controls for zip code fixed effects. Excluded donut [-1,-1] plotted for reference with X's. All residuals plotted on same scale for comparability; log primary claim first divided by 10.