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The effects of legal representation on tenant outcomes in housing court: Evidence from New York City's Universal Access program

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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 for tenants at higher risk of possessory judgment. Our results support the idea that legal representation in civil procedures can have important positive impacts on the lives of poor people.

1. Introduction

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 Academies 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.[1]

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 (2015-2017) Expanded Legal Services initiative than in other zip codes. They

[1] Collinson and Reed (2019) and Humphries et al. (2019) have been combined as Collinson et al., (2022).

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also document a weak negative correlation between this expansion of free legal services for tenants and eviction rates.[2]

We study the roll-out of New York City's Universal Access to Counsel program (UA), which became law in August 2017, but began operations several months earlier, in February 2017, building on the earlier Expanded Legal Services initiative (NYC Department of Social Services, 2021).[3] 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 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 by using program availability as an instrumental variable in models that also include detailed information about census block groups and housing units, in addition to zip code and borough by month 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 examine heterogeneity in the effects of representation by constructing subsample analyses across higher- and lower-risk cases, as well as according to neighborhood and case characteristics including poverty, rent regulations, and claim amounts.

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 for tenants with the greatest ex ante risk of possessory judgment, suggesting that a program targeting tenants based on observable characteristics could have even larger impact per dollar spent than one with universal ambitions.

The time period we study is not long enough to rule out long-term changes in landlord behavior. For example, a policy change, like UA, that makes it more difficult (and therefore expensive) to evict clients may incentivize landlords to raise rents, screen applicants more strictly, or remove units from the market entirely. Nevertheless, we conduct several tests for landlord strategic behavior and find no evidence of such responses during the early years of UA. More generally, our results support the idea that legal representation in civil procedures can have important positive impacts on the lives of poor people.

The rest of the paper proceeds as follows. Section 2 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 3 provides information about the data on housing court cases, housing, and area-level characteristics. Section 4 describes our empirical methods and Section 5 provides the main results. Section 6 provides a discussion and conclusion.

2. Background

2.1. 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

[2] 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. Their main results compare zip-year changes in average outcomes between zips treated early and late by the Expanded Legal Services program, which was the precursor to Universal Access to Counsel, controlling for lagged changes in outcomes, and zip-level race and poverty measures. We study a larger and more comprehensive program using a within-zip-code 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. (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 causal impact of legal representation.

[3] 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.

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.

2.2. 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 (which together account for only 2.5 percent of cases in our sample).

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 follow a structured and typically straightforward process, the key steps of which are depicted in Fig. 1. 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.[4] 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 the initial petition 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; 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 a settlement, or "stipulation," is often the result of a hasty "conference" between the tenant and the landlord's lawyer in a corridor or a corner in the courthouse, which is then codified by a judge in a formal judgment. These negotiated stipulations are the most common form of judgment in housing court. 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[5] 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.

2.3. 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 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 direct them to designated courtrooms staffed by contracted legal services providers (Been et. al., 2018; Ellen et. al., 2020). Importantly, this first appearance occurs subsequent to a tenant answering the initial petition[6], which means that tenants who never answer and never show up in housing court are unlikely to be represented by UA. As would be expected under this program design–and as we discuss in what follows–UA lawyers can have a notable impact in preventing failures to appear for subsequent court proceedings, but are unlikely to impact the probability of an initial answer to the landlord petition.[7]

Implementation has been phased in by cohorts of target zip codes (Office of Civil Justice. 2018). The first cohort of 10 zip codes

[4] 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.

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

[6] During ELS and the initial transition to the UA program, tenants were provided information about free legal representation at petition answering (which precedes the first court "appearance"), but during program ramp up it became clear that providing lawyers at tenants' initial appearances was a more effective way of connecting clients with attorneys (Office of Civil Justice, 2016; Been et. al., 2018).

[7] UA lawyers–because they typically work for organizations with broad social services experience–may also help connect tenants with other public benefits (such as rental assistance), which can help form the bases of agreements they reach with landlords. However, our data does not include public benefits, so we are unable to provide evidence as to whether legal representation helps tenants on this margin.

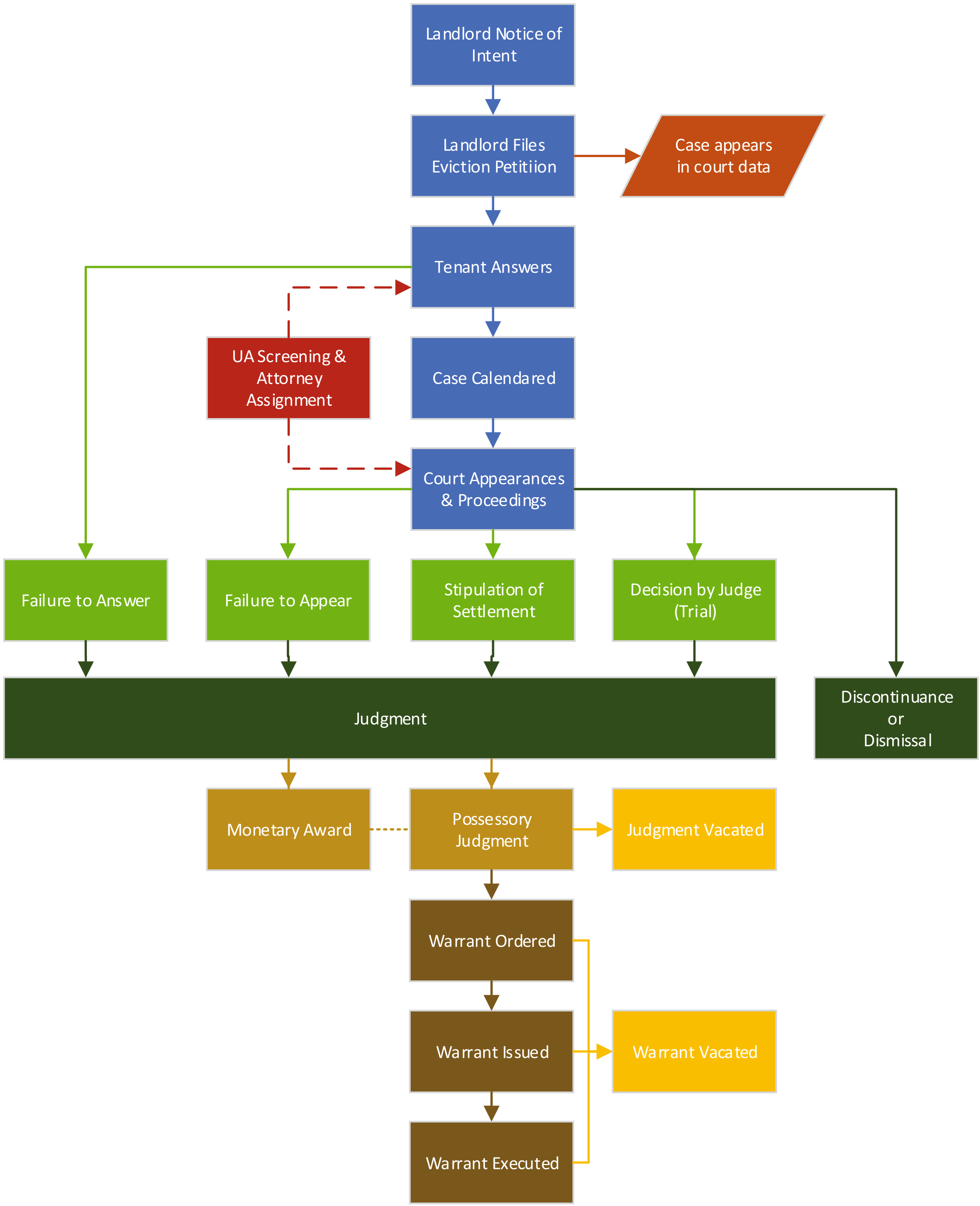

Fig. 1. Housing court process. Note: The figure depicts the key steps for eviction proceedings in New York City.

(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[8] for a total of 25 zip codes served (FY16-17, FY18, FY19, and FY20 cohorts) out of some 180 zip codes by the last year of our data.[9] 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

[8] The City's fiscal years run from July 1 to June 30 and are named for the calendar years in which they end.

[9] However, as described below, no zips in the FY20 cohort had actually been treated by the end of our sample period in June 2019.

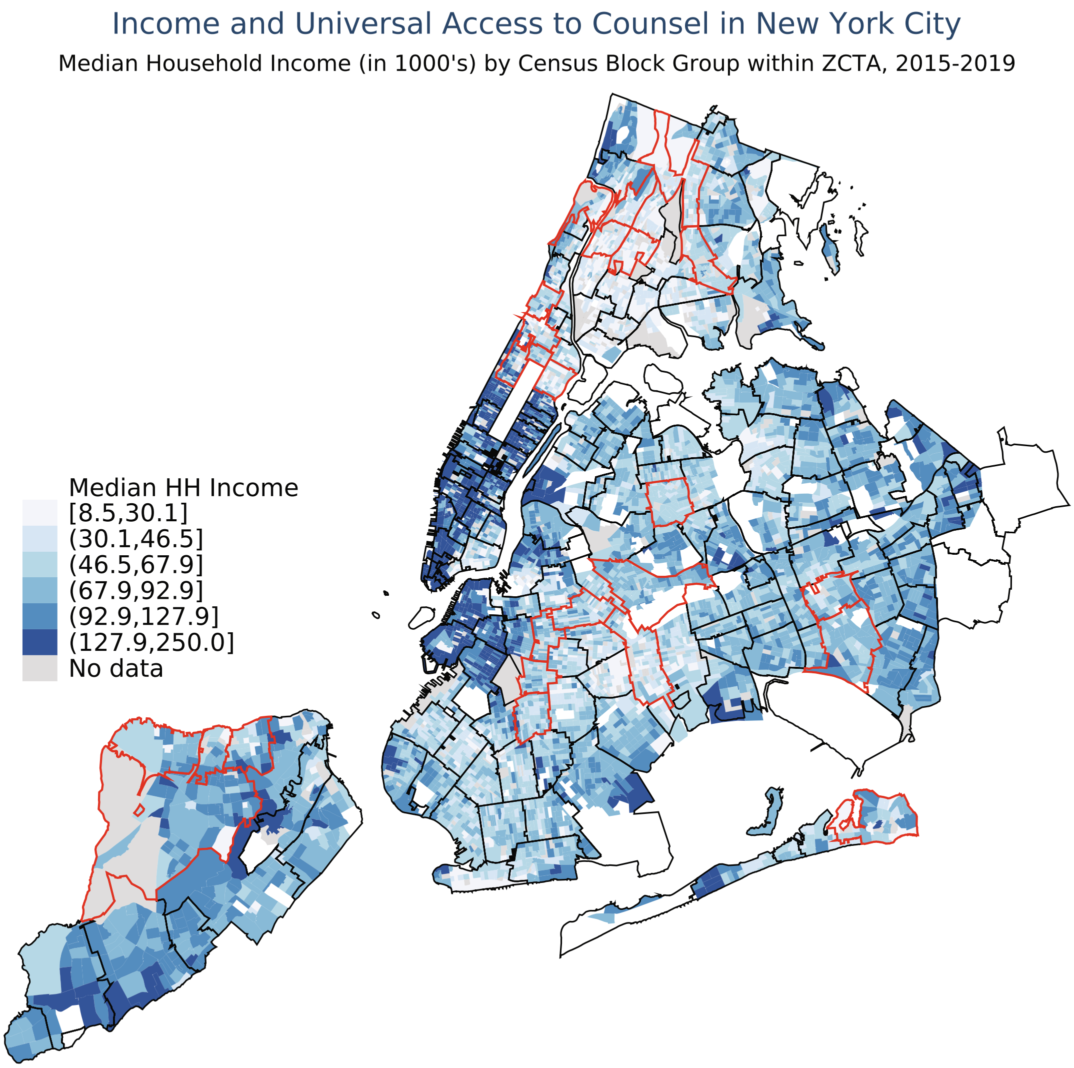

Fig. 2. The figure depicts the census block groups comprising NYC's five boroughs. Black lines delineate zip code tabulation areas. Red lines highlight UA ZCTA's. Limits of shading bins set at 0, 10, 25, 50, 75, 90, and 100 percentiles of CBG median household income, defined within the sample of NYC housing court cases.

(Office of Civil Justice, 2021). We exploit the roll out of UA to identify the causal effects of legal representation.

Fig. 2 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.[10] The figure suggests that the zip codes chosen for UA had among the lowest median incomes (lightest shading) in each borough. Fig. 3 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 eviction 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.

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 (Office of Civil Justice, 2018, 2019b). Fig. 4 shows that while target zip codes had the greatest number of cases served by UA, there were 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 changes are not relevant to this study (Office of Civil Justice, 2020b).

[10] To draw these maps, we augment our geocoded housing court data set, described in Section 3, 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.

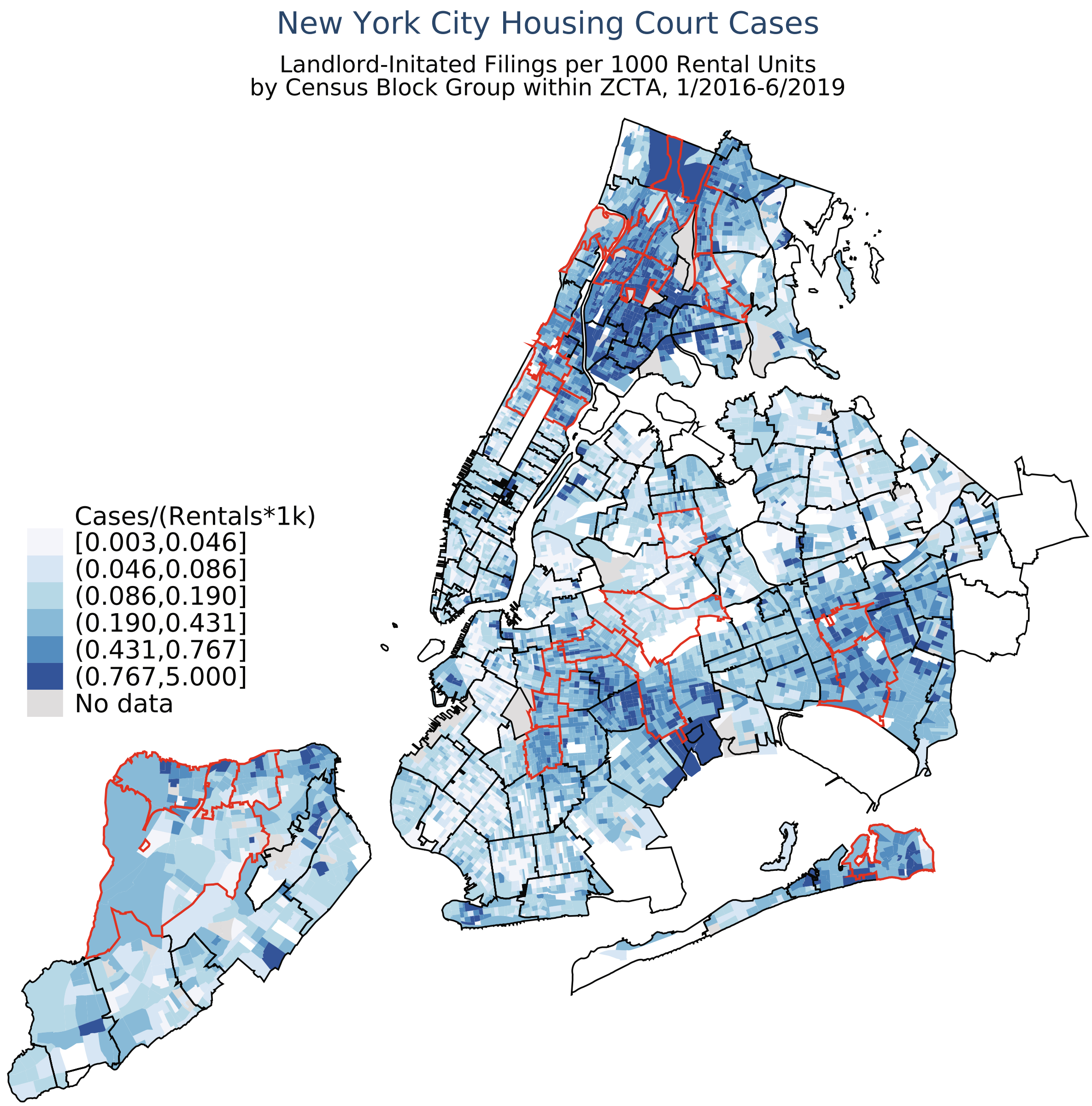

Fig. 3. The figure depicts the census block groups comprising NYC's five boroughs. Black lines delineate zip code tabulation areas. Red lines highlight UA ZCTA's. Limits of shading bins are 0, 10, 25, 50, 75, 90, and 100 percentiles of housing court case counts (specifically, landlord-initiated filings).

The impact of the UA rollout is illustrated in Fig. 5, 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 described in Appendix A.2. 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.[11] Table 1 lists the zip codes included in each cohort, as well as empirical UA start dates and approximate official start dates (NYC Department of Social Services, 2021).

Our primary instrument is an indicator equal to one if the case filing date is at or after the empirical start month of UA in a borough and zip code. This measure helps us to zero in on when and where the program was *actually implemented*. The uptake of lawyers occurred at different times within cohorts, and, several target zip codes were not meaningfully treated. A natural alternative would be to use "official" program start dates. However, the Department of Social Services (2021) cautions that the dates they provided us "should be interpreted as rough indicators of when expansions were initiated – not as precise implementation dates."[12] Nevertheless, we use these semi-official cohort start dates as a robustness check and find they confirm our main results.

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

[11] As discussed in Appendix A.2, our procedure is conceptually and operatively similar to the Card et al. (2008) "fixed point" algorithm for identifying tipping points. Table 1 also shows start dates calculated using their algorithm.

[12] This document is available upon request from the NYC Department of Social Services. Public reports published by Department of Social Services and Office of Civil Justice only describe target start fiscal years for each zip code cohort.

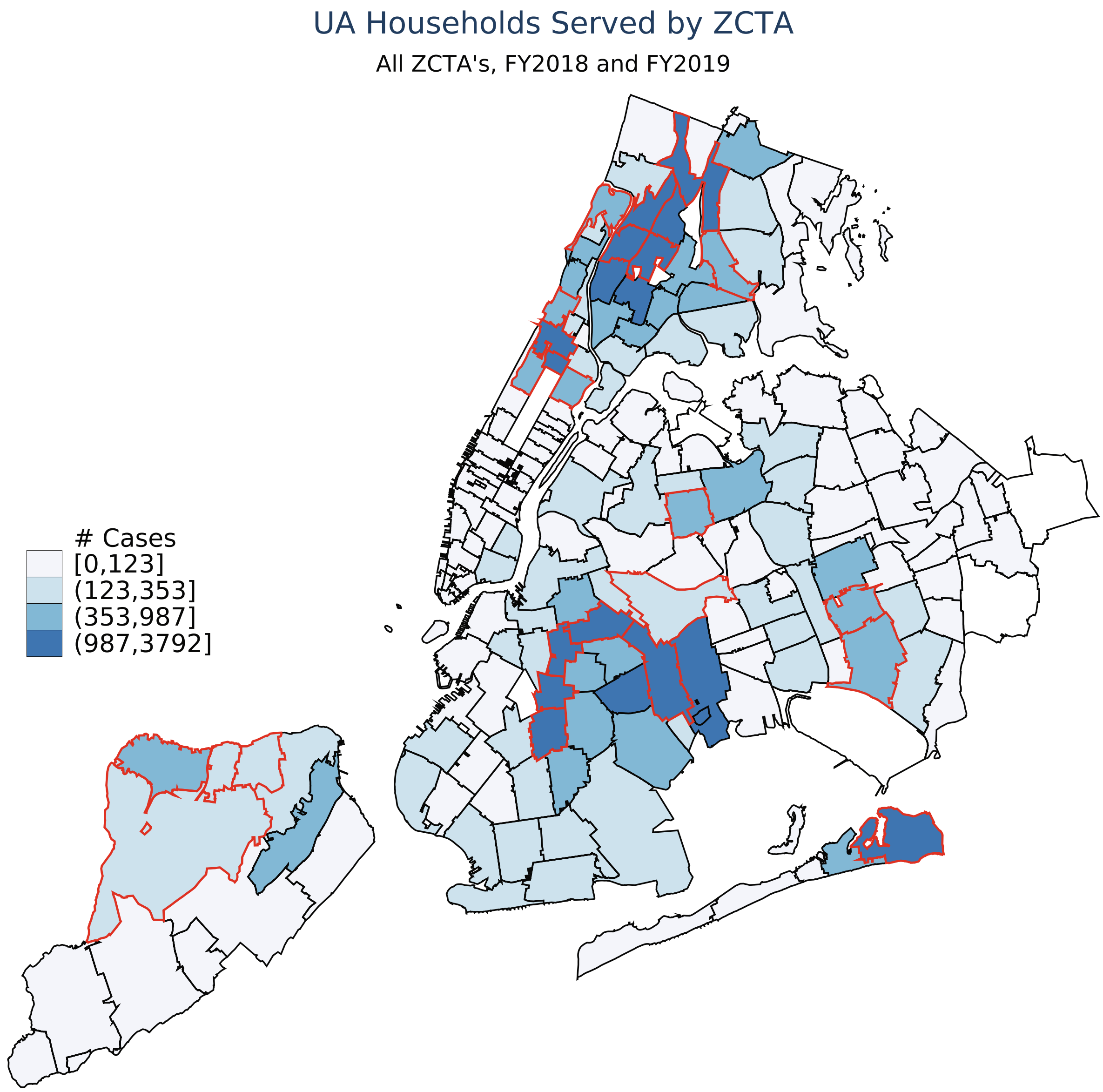

Fig. 4. The figure depicts the zip codes comprising NYC's five boroughs. Black lines delineate zip code tabulation areas. Red lines highlight UA ZCTA's. Limits of shading bins are 0, 50, 75, 90, and 100 percentiles of UA household count from NYC DSS annual reports.

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.

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), the FY16-17 cohort has a relatively high rate of representation throughout the period, with no sharp change, suggesting that UA did not have a dramatic impact in these zip codes. We treat this cohort as untreated by UA, but given the inclusion of zip code fixed effects we would obtain identical results if we labeled it as always treated. 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 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 in population than the other boroughs, so that the patterns are 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.[13]

[13] There is no FY20 cohort in Staten Island.

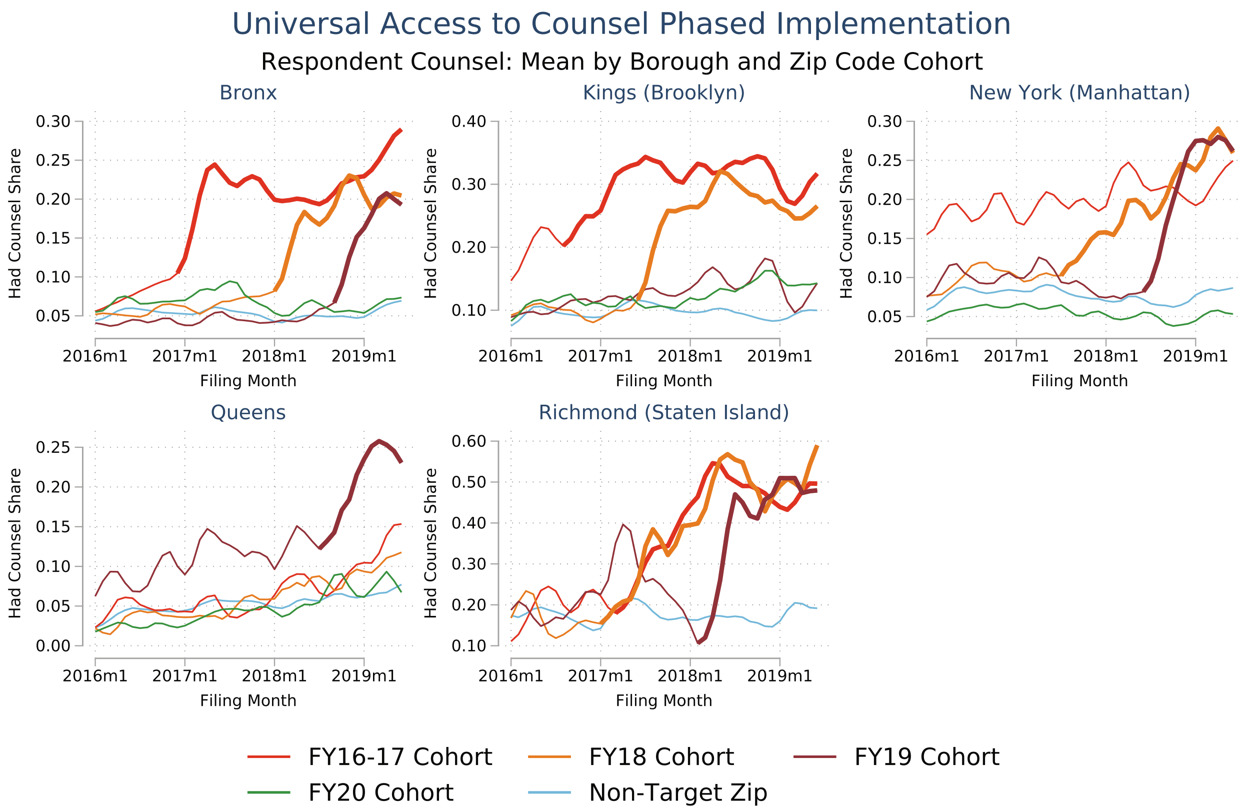

Fig. 5. Bold indicates empirical UA treatment. Monthly respondent counsel means are smoothed using local mean regression with a bandwith of one month.

Table 1
Universal access to counsel start dates by borough and zip code cohort.

	Zip Codes (1)	Empirical (2)	DSS (3)	CMR (4)
Bronx				
FY16-17	10457, 10467	2016m12	2017m2	2017m1
FY18	10468	2018m1	2017m7	2018m3
FY19	10462	2018m9	2018m10	2018m7
FY20	10453		2019m12	
Kings (Brooklyn)				
FY16-17	11216, 11221	2016m8	2017m2	2016m11
FY18	11225	2017m6	2017m7	2017m4
FY19	11226		2018m10	
FY20	11207		2019m12	
New York (Manhattan)				
FY16-17	10026, 10027		2017m2	
FY18	10025	2017m7	2017m7	2018m9
FY19	10031	2018m6	2018m10	2018m5
FY20	10029, 10034		2019m12	
Queens				
FY16-17	11433, 11434		2017m2	2018m11
FY18	11373		2017m7	
FY19	11385	2018m7	2018m10	2018m9
FY20	11691		2019m12	
Richmond (Staten Island)				
FY16-17	10302, 10303	2017m3	2017m2	2017m1
FY18	10314	2017m1	2017m7	2016m11
FY19	10310	2018m2	2018m10	2018m4
FY20				

Dates are in YEARmMONTH format. Blank cells indicate that no UA start date is identified by a given method in a given borough-cohort. (Recall that our sample period ends in 2019m6, so neither the Empirical nor the CMR algorithm can identify start dates for the FY20 cohort.) Rows index boroughs and UA zip code cohorts. Column 1 lists the zip codes in each borough-cohort. Column 2 gives the empirical UA start date, which is estimated using the algorithm described in Appendix A.2 and which is the main instrument in this paper. Column 3 gives the approximate official UA start date, as reported by the NYC Department of Social Services. Column 4 gives the empirical UA start date estimated by an application of the Card, Mas, Rothstein (2008) tipping point algorithm. UA was signed into law 2017m8.

"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 Housing Authority (NYCHA) tenants were not represented by UA.[14] 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 failed to answer the petition and never showed up in housing court. Appendix Figure A.2 repeats Fig. 5 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 million 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 lawyers deserve credit 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 representation.

3. Creating the data set

3.1. Sources, sample, and covariates

Our main source of data is individual Housing Court records from the Civil division of the New York State Unified Court System.[15] 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.[16] 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 "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 main regression 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.[17] In some 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

[14] 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.

[15] 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).

[16] 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).

[17] 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.

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.

Variables derived from PLUTO or the ACS are observed at single points in time (2019 for the ACS and 2021 for PLUTO).[18] However, the PLUTO and ACS variables are defined at a smaller level of geography than the zip code, and cases can be drawn from different block groups or tax lots within the zip code at different times. Hence, the mean PLUTO and ACS characteristics of cases can change within a zip code over time. To the extent that the PLUTO and ACS characteristics of tenant's census block groups proxy for the characteristics of tenants, including these controls helps us to control for within-zip-code changes over time in the type of tenants seeing cases filed against them within the zip code. Further information about variable definitions appears in Appendix A.3.

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 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.[19] Together, these restrictions leave us with 727,703 cases in the main sample.[20]

3.2. 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.[21] 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 cases, creating a situation where the raw association between legal representation and 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 detail in Appendix A.2: 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 specifications include zip code fixed effects, identification is not based on a comparison of UA 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 year, makes it unlikely that the effects of UA could be confounded with those of other policies.[22] Borough-by-month fixed effects are also included in order to absorb the effects of any borough-specific policies, trends, or otherwise confounding shocks.

The major threat to identification is time-varying unobservables coincident with UA start dates. While we cannot rule out all such confounders, we believe this threat is likely to be small. There is no mention of potentially coincident changes to policy or housing markets in the program's official history (see Office of Civil Justice, 2016, 2017, 2018, 2019a,b, 2020a,b, 2021). Below, we provide evidence in support of balance and the maintained parallel trends assumption.

For robustness, as well as additional insights, we also conduct a second instrumental variables exercise using UA intensity rather than the 0/1 UA variable. UA intensity is measured as the number of households in each zip code that received UA representation during each fiscal year (divided by 1000 for interpretability). This measure not only captures the relative importance of UA among target zips, but it also allows us to consider the impact of UA on tenants living outside target zip codes, who are also eligible to receive representation under the UA program if resources allow. 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 census block groups of cases, 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 (see Table 2, discussed below), which provides evidence in support of the identification assumptions underlying the instrumental variables estimates. We also show that reduced-form models demonstrate significant program effects. Finally, we estimate an alternative specification of the instrumental variables models that includes fixed effects for each address down to the apartment unit number. In these models, the effects of UA are identified by the subset of approximately 44,000 addresses that had cases filed both before and after UA.[23]

3.3. Defining the outcome variables

In what follows, we focus on four main tenant outcomes that correspond to pivotal events in the housing court process:[24]

- **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."[25] Possessory judgments are a necessary precursor to the issuance of a warrant of eviction.

[18] Variables measured in dollars are in 2019 dollars for the ACS and 2021 dollars for PLUTO.

[19] 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.

[20] In our main analysis featuring zip code and borough-by-month fixed effects, eleven singleton observations are dropped.

[21] There are two other possibilities for tenant representation status: self-represented litigant (SRL) and no appearance. Both reflect the absence of an attorney. Note also that the housing court data does not distinguish between UA and non-UA (e.g., private) attorneys. However, given the historically very low rates of tenant legal representation in NYC prior to the introduction of publicly funded lawyers (e.g., ELS), it is safe to assume that a majority of tenants who have lawyers during the study period have UA lawyers (Office of Civil Justice, 2016).

[22] 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 courts and DSS use to refer tenants to UA providers.

[23] Address fixed effects do not capture long-run changes in landlord strategic behavior (e.g. increasing rents or more selectively screening tenants), but they do rule out (potentially numerous) address-invariant unobservables, such as the types of tenants who would rent a given unit.

[24] 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.

[25] We have also looked at whether any possessory judgment was ever issued in the case (vacated or not) and gotten very similar results.

Table 2
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**
Log Judgment Amount (Cond. on >0)	8.000	8.067	8.021	8.063	−0.025	0.027	0.359
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$) Primary Claim	6.748	6.797	6.926	6.960	−0.014	0.052	0.781
Observations	153,582	135,405	66,666	58,554			
C. US Census American Community Survey							
CBG Population/1000	1.745	1.767	1.652	1.660	−0.014	0.013	0.291
CBG HH Median Income/1000 (in 2019$)	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

(continued on next page)

Table 2 (*continued*)

	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)
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-target zips. 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 were not initially prioritized for services.
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. * $p < 0.05$, ** $p < 0.01$

- **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-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.[26] 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., a hearing or a 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.
- **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.
- **Log Judgment Amount (Conditional on Judgment Amount > 0)**: the log judgment amount, as above, but defined only for cases with a positive judgment amount.

3.4. Summary statistics

Table 2 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 (the first and last years in our sample period) 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

[26] 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.

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 Fig. 5, some zip codes earmarked for UA do not seem to have had meaningful increases 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 2 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 never show up in court at all. This makes sense, because, as discussed above, tenants who never appear are not 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 2 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 are no changes in landlord primary claim amounts, assessed values, or likelihood that units are rent stabilized.

These impressions–notable changes in tenant representation rates and outcomes in UA zips as compared with non-target zips, paired with no corresponding contrast in other case observables–is further illustrated in Figs. 6 (tenant representation rates and outcomes), 7 (covariates), and A.3 (additional covariates). These graphs depict time trends in UA and non-UA zips. We first estimate linear regressions of each variable of interest on zip code and borough*month fixed effects. We then take the residuals from each regression and recenter them by adding back each variable's respective non-UA zip mean. Finally, we take the monthly mean of each residualized and recentered variable for each zip group and perform local mean regressions on filing month using a bandwidth of one. The expected contrasts between UA and non-UA zips in representation rates and outcomes are obvious, but there are no discernible differences in trends for other tenant and case characteristics.[27] Hence, there is little evidence that landlords changed their case filing practices in response to the UA program, at least during the period we study.

[27] We present these comparisons as overall calendar time trends rather than as a pre-/post-implementation analysis given the gradual roll out of UA across boroughs and zip codes that we document (i.e., there is no single start date in calendar time). In an aggregate zip-code level analysis of UA, Liaw (2021) raises a concern about "pre-trends" in tenant representation; however, that paper uses, but does not motivate, 2018 as the UA start date for the FY16-17 and FY18 cohorts, which is later than the empirical (and official) start dates for most of the zip codes in these cohorts.

4. Empirical methods

As discussed above, our main focus is on instrumental variables models, but OLS models and reduced forms are presented first for reference. The ordinary least squares (OLS) model takes the form:

$$Y_i = \beta_0 + \beta_1 R_i + \boldsymbol{\beta}_2 \mathbf{HC}_i + \boldsymbol{\beta}_3 \mathbf{PLUTO}_{a(i)} + \boldsymbol{\beta}_4 \mathbf{ACS}_{b(i)} + \\ + zip_i + borough_i \times month_i + \varepsilon_i \quad (1)$$

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 2, **PLUTO** is the vector of tax lot variables shown in Panel D of Table 2, and **ACS** is the vector of census block group (or tract) characteristics shown in Panel C of Table 2. 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 denotes the error term.

We also estimate a model that includes address-unit-specific fixed effects[28] (indexed with subscript i because address units vary at the case level):

$$Y_i = \beta_0 + \beta_1 R_i + \boldsymbol{\beta}_2 \mathbf{HC}_i + \boldsymbol{\beta}_3 \mathbf{PLUTO}_{a(i)} + \boldsymbol{\beta}_4 \mathbf{ACS}_{b(i)} + \\ + borough_i \times month_i + address_i + \varepsilon_i \quad (2)$$

The instrumental variables versions of Eqs. (1) and (2), replace R_i with $\widehat{R}_i$, where $\widehat{R}_i$, is the predicted value of R from the first-stage equations:

$$R_i = \alpha_0 + \alpha_1 UA_i + \boldsymbol{\alpha}_2 \mathbf{HC}_i + \boldsymbol{\alpha}_3 \mathbf{PLUTO}_{a(i)} + \boldsymbol{\alpha}_4 \mathbf{ACS}_{b(i)} + \\ + zip_i + borough_i \times month_i + \omega_i \quad (3)$$

$$R_i = \alpha_0 + \alpha_1 UA_i + \boldsymbol{\alpha}_2 \mathbf{HC}_i + \boldsymbol{\alpha}_3 \mathbf{PLUTO}_{a(i)} + \boldsymbol{\alpha}_4 \mathbf{ACS}_{b(i)} + \\ + borough_i \times month_i + address_i + \omega_i \quad (4)$$

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.

In Appendix A.1, we present a supplementary regression discontinuity event study analysis that homes in 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. These estimates are somewhat imprecise but support the qualitative findings from the instrumental variable models.

5. Estimated effects of legal representation

Table 3 presents the main results. The first column shows OLS estimates of Eq. (1) 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). The magnitude of this effect is larger than the crude difference-in-

[28] An alternative strategy, since the PLUTO data includes landlord identifiers, is to use landlord fixed effects. However, landlord fixed effects are collinear with address fixed effects, and address fixed effects are more precise, so we opt for the latter.

Table 3
Main results: respondent counsel and housing court outcomes.

	Main		Address FE	
	OLS (1)	UA IV (2)	OLS (3)	UA IV (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 roll out) 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. * $p < 0.05$, ** $p < 0.01$

differences comparison shown in Table 2. The remaining estimates suggest that representation has very large effects on the affected cases, reducing the probability of a possessory judgment by 32.1 pp, the log judgment amount by 2.126, the probability of a warrant being issued by 32.3 pp, and the probability that the warrant is executed by 8.4 pp.

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 further 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 44,000 addresses that have at least one case filing before UA implementation and one case filing after UA implementation.[29] The effect on evictions is slightly reduced in this sample to 5.8 pp and is now significant only at 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.[30]

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

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.2.[31] 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, unobservable variables 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 3, 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

[29] These 44,000 cases are the core observations contributing to identification in the address FE model. However, the actual sample size reported in Table 3 is a factor of ten larger because, given that we wish to include borough-by-month fixed effects, it is helpful to include any address with a filing in more than one month. Overall, 63 percent of addresses in our sample have only one case filing, 28 percent have two or three filings, and 9 percent have more than three filings.

[30] The results are similar if we use building fixed effects rather than address fixed effects (results not shown). Notably, the building FE model estimates a 7-pp reduction in the probability of eviction, which is statistically significant at the one percent level.

[31] The details of the complier characterization analysis are discussed in Appendix A.4.

Table 4
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	0.158	0.130	0.163	0.125	0.000
Empirical UA Treatment (IV)	0.085	0.397	0.533	0.560	0.087	0.012
UA Households Served/1000 (IV)	0.230	0.479	0.520	0.586	0.234	0.000
Judgment with Possession	0.415	0.397	0.405	0.407	0.576	0.623
Log Judgment Amount	1.728	1.628	1.960	1.780	2.397	2.610
Warrant Issued	0.349	0.347	0.368	0.359	0.484	0.527
Warrant Executed	0.070	0.068	0.047	0.071	0.097	0.108
Judgment Vacated (Cond. on Judgment)	0.126	0.152	0.181	0.166	0.126	0.103
Warrant Vacated (Cond. on Warrant)	0.072	0.083	0.102	0.094	0.099	0.087
Log Judgment Amount (Cond. on >0)	8.039	8.092	8.034	8.075	8.039	7.968
Judgment Failure to Answer	0.134	0.136	0.157	0.138	0.186	0.207
Judgment: Failure to Appear	0.067	0.066	0.060	0.067	0.092	0.095
Judgment: Stip/Settle	0.230	0.215	0.217	0.225	0.319	0.340
Judgment: Court Proceeding	0.006	0.005	0.003	0.004	0.008	0.006
Days to Judgment Entered	68.6	72.7	64.3	70.1	68.6	59.8
Days to Warrant Executed	195.2	198.2	189.1	199.1	195.2	194.9
Observations	727,703	155,163	43,691	85,680	524,650	142,829

Rows index treatment, instruments, and outcomes. Columns define samples of interest. Each cell gives the mean for the row-indexed variable in the column-indexed sample. Column 1 is the main (full) sample. Column 2 is the subsample of main sample cases from UA target zip codes in the first three UA cohorts only. Column 3 is the address fixed effects sample. Specifically, it refers to the core subset of cases contributing to identification of respondent counsel effects in the address FE sample: that is, addresses with case filings both pre- and post-UA. Note that the actual sample size in the address fixed effects analysis is a factor of ten larger because, given the inclusion of borough-by-month fixed effects, any address with a filing in more than one month also contributes to estimation. Column 4 is the regression discontinuity sample. Column 5 is the subsample of main sample cases with activity beyond initial filing. Column 6 refines Column 5 by further limiting the sample to filings from 2016 where the tenant did not have a lawyer.

Table 5
Official start date IV robustness.

	Main			Address FE		
	Empirical (1)	DSS (2)	CMR (2008) (3)	Empirical (4)	DSS (5)	CMR (2008) (6)
Respondent Counsel (First Stage)	0.124**	0.100**	0.122**	0.118**	0.095**	0.114**
	(0.006)	(0.012)	(0.006)	(0.008)	(0.012)	(0.008)
Judgment with Possession	−0.321**	−0.325**	−0.336**	−0.334**	−0.334**	−0.341**
	(0.041)	(0.052)	(0.037)	(0.054)	(0.074)	(0.048)
Log Judgment Amount	−2.126**	−2.188**	−2.122**	−2.519**	−2.633**	−2.418**
	(0.284)	(0.318)	(0.294)	(0.405)	(0.458)	(0.423)
Warrant Issued	−0.323**	−0.314**	−0.325**	−0.340**	−0.317**	−0.340**
	(0.037)	(0.045)	(0.035)	(0.056)	(0.077)	(0.051)
Warrant Executed	−0.084**	−0.072**	−0.086**	−0.058	−0.078**	−0.064
	(0.021)	(0.019)	(0.017)	(0.034)	(0.030)	(0.033)
Observations	727,692	727,692	727,692	456,788	456,788	456,788
First-Stage F Stat	495.93	69.43	451.85	240.05	60.88	205.66
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes	Yes	Yes
Borough × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Address FE	No	No	No	Yes	Yes	Yes

This table repeats the main instrumental variable analysis from Table 3 to assess the robustness of the UA instrument to different start dates. The first three columns assess the main covariate specification, while the last three columns assess the address fixed effect specification. Columns 1 and 4, labeled Empirical, use our preferred UA empirical treatment instrument and repeat Columns 2 and 4, respectively, from Table 3. Columns 2 and 5 use the approximate official program start dates at the zip code cohort level reported by the NYC Department of Social Services (DSS) to define the instrument (NYC Department of Social Services, 2021). Columns 3 and 6 apply the Card, Mas, Rothstein (2008) tipping point algorithm to identify the program start dates by borough and zip code cohort. Outcomes are listed in rows. All results are for the main sample. Unit of observation is a housing court case. 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. Standard errors clustered by zip code are given in parentheses. * $p < 0.05$, ** $p < 0.01$

there was at least some housing court activity. Means for this set of cases are shown in the last column of Table 4. Relative to these means, the estimates in Column 2 of Table 3 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 3 are very robust. Table 5 repeats the IV analysis from Table 3 using alternative UA start dates as instruments.[32] The first three columns correspond to our main covariate specifications; the last three columns include address fixed effects. Columns 1 and 4 repeat Columns 2 and 4 from Table 3, respectively, and use our preferred empirical start dates. Columns 3 and 5 use the

[32] The start dates are enumerated in Table 1.

Table 6
Heterogeneity analysis: IV results.

	Respondent Counsel . (1)	Judgment with Possession (2)	Log Judgment Amount (3)	Warrant Issued . (4)	Warrant Executed . (5)
Judgment Risk Above In-Sample Median					
Yes	0.085**	−0.610**	−4.257**	−0.569**	−0.188**
	(0.007)	(0.090)	(0.756)	(0.084)	(0.070)
	181,916				
	[143.02]				
No	0.144**	−0.217**	−0.771*	−0.240**	−0.015
	(0.011)	(0.047)	(0.309)	(0.046)	(0.015)
	181,922				
	[173.50]				
Difference in Means	−0.059++	−0.393++	−3.485++	−0.329++	−0.173+
	{0.0000}	{0.0001}	{0.0000}	{0.0006}	{0.0151}

Unit of observation is a housing court case. Outcomes are listed in columns. Rows index the characteristics and levels defining the subsamples among which the heterogeneity analysis is conducted. Each cell in a characteristic-level row reports the coefficient on tenant counsel from a separate TSLS instrumental variable regression of the column-enumerated outcome on main specification controls (corresponding to Column 2 in Table 3) and empirical UA treatment as the instrument, for the subsample defined by the characteristic-level row. 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. First column reports first-stage results with tenant (respondent) counsel as the dependent variable. Difference in Means row gives the difference in coefficients for the binary contrast provided. Judgment risk is the ex ante probability that a tenant will receive a possessory judgment, estimated from a linear regression of possessory judgment on main covariates (e.g., Column 1 in Table 3) on a 50 percent training sample. P-values for the differences in means reported in braces below the point estimates. Stars attached to coefficients indicate statistical significance with respect to zero, $^{*}p < 0.05$, $^{**}p < 0.01$; crosses attached to differences in means reflect statistical significance of differences in coefficients between subgroups, $+p < 0.05$, $++p < 0.01$.

official DSS cohort start dates (NYC Department of Social Services, 2021). Columns 4 and 6 use the Card et al. (2008) tipping point algorithm. The estimates for the alternative instruments closely align with our main results.

Appendix Table A.3 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.4 repeats the Table 3 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.5 shows several alternative specifications. The Wald estimates in Column 1 show that even in models without any controls (where the instrument may not be valid), the effects of representation are qualitatively similar. Since the exclusion restrictions underlying our instrument are valid only within zip codes, a minimal specification is the one in Column 2 which includes zip code fixed effects but no other controls. This specification yields estimates that are qualitatively similar but larger in absolute value than those shown in Table 3. Combining both zip code fixed effects and other covariates but omitting the borough*month fixed effects (Column 3), produces estimates slightly larger than the zip-FE-only specification. Columns 4 to 7 show estimates from only the last two years of the data, fiscal years 2018 and 2019. These are the years when the "UA intensity" instrument is available. These estimates are presented in order to facilitate comparison to the results estimated using that instrument, discussed below. The main estimates (Column 6) and those with address fixed effects (Column 7) are quite similar to those shown in Table 3.

Table A.6 explores the sensitivity of our estimates to various approaches to correcting known biases in standard 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 et al. (2022); Callaway and Sant'Anna (2021); De 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.[33] Inflating the case counts for each zip-month in Table A.1 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.6 suggests UA effects that are about half of those reported in our main specification. These lower bound estimates of the effects suggest reductions of 28.5 percent in possessory judgments, 44.7 percent in log judgment amounts, 33.7 percent in warrants being issued, and 41.1 percent in warrants being executed.

Another important question is who is most affected by legal representation. 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, lack legal literacy, or assume they will lose.

One summary way to classify cases is by *ex ante* risk of possessory judgment, which is arguably the most comprehensive measure of an adverse outcome for a tenant. In Table 6, we construct predicted judgment risk by regressing possessory judgment on the covariates and fixed effects from our main specification

[33] This estimate is based on a discussion in a report by theOffice of Civil Justice (2019a,b) which enumerates 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. Hence, 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.

Table 7
Additional results: respondent counsel and housing court outcomes.

	Main		Address FE	
	OLS	UA IV	OLS	UA IV
	(1)	(2)	(3)	(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)
Log Judgment Amount (Cond. on > 0)	0.332**	0.224**	0.242**	0.069
	(0.009)	(0.065)	(0.014)	(0.110)
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 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 roll out) 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.

(Table 3, Column 1), using a random 50 percent sample. We then use the fitted values from this model to predict possessory judgment using the other half of the cases (the hold-out sample). We then split the hold-out-sample cases into two subsamples according to whether the predicted risk of possessory judgment is above or below the in-sample median and estimate TSLS regressions in each sample for each outcome (i.e., repeating the specification of Table 3, Column 2). Differences in means and the associated p-values are reported in the bottom two rows of Table 6.

The results are striking. While UA has a larger effect on the probability of representation for low-risk cases (14.4 pp vs. 8.5 pp in the high-risk group), high-risk tenants experience improvements in outcomes that are several times larger. Compared to low-risk tenants, high-risk tenants with legal representation experience 39.3 pp larger reductions in the probability of possessory judgment, 32.9 pp larger reductions in the probability of warrant issuance, and 17.3 pp larger reductions in the probability of eviction; their reduction in judgment amounts is also 3.5 units greater on a log scale.[34]

Appendix Tables A.7A-A.7C conduct additional heterogeneity analyses. These tables show that legal representation has larger effects in poorer places (9 pp greater reduction in the probability of eviction in higher-poverty neighborhoods) and among less rent-protected tenants (9 pp greater reduction in the probability of eviction in units ineligible for rent stabilization[35]), as well as in cases that feature nonpayment (versus holdover), larger primary claim amounts, and landlords with greater propensities to file eviction petitions (i.e., more cases per units). There is also suggestive evidence that noncitizens and Hispanics benefit more than average, but these differences are not statistically significant.

Table 7 shows examines additional outcomes that shed light on the mechanisms through which representation affects tenant outcomes. As before, we focus the discussion 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 suggests 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. In addition, the coefficients on log judgment amount conditional on having a positive judgment amount, are positive (though not statistically significant in the address fixed effects specification). These observations indicate that lawyer effects operate primarily through the preventing judgments (the extensive margin) rather than by reducing the severity of judgments.

Panel B of Table 7 shows the effects of representation on the type of judgment that is reached, which in turn reflects grounds for the judgment. The instrumental variable 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). This suggests that having professional legal representation in negotiations with landlords and their lawyers is an effective means of preventing tenants from merely conceding to landlords' preferred terms of settlement in an initial hallway conference. 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

[34] This high-risk/low-risk contrast is also apparent in the address fixed effects model (results not shown), though the eviction point estimate becomes imprecise.

[35] The finding that the effects of legal representation are larger among tenants less likely to reside in rent-regulated housing suggests that the benefits of a UA-like program may generalize quite well to places with less robust tenant protections than New York.

Table 8
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 × 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. * $p < 0.05$, ** $p < 0.01$

(since the point of access for UA is a tenant's first appearance in housing court).

Panel C of Table 7 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 8 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.1. The Table 8 estimates follow the same qualitative pattern as those in Table 3.[36] The point estimates are larger, but this is not surprising given that the intensity instrument uses the continuous 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 3.

However, these results do not imply that spillovers to neighboring non-target zips are quantitatively important. Appendix Table A.8 repeats the main IV specification from Table 3, Column 2, but splits the sample into UA zips in the first three cohorts (Column 1) and zips adjacent to these UA zips (Column 2). The empirical UA instrument is redefined so that a zip code is considered treated at the date UA begins in the first zip with which it is geographically contiguous (including itself). Non-adjacent (i.e., never-treated) zips are included as controls in both subsamples. The estimates from the UA zips in Column 1 are similar to those in the main analysis. In the adjacent zips, the first stage is essentially nonexistent, making it plain that the main results are driven by the effect of lawyers on tenant outcomes in target zips.

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 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 9 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 3. We take this as evidence that if, as a worst case, UA merely delays the inevitable, this postponement is of sufficient duration (i.e., more than a year) to have a meaningful impact on tenant housing stability in the medium term, especially since adverse effects of eviction on tenants seem to be concentrated in relatively short time periods pre- and post-eviction (Collinson et. al., 2022). Nevertheless, without longer-term data, we are unable to say whether the tenant gains we observe abate over time, or whether landlords

Table 9
Address-level 15-month outcomes.

	Main	
	OLS (1)	UA IV (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 cumulative 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 roll out) 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.* $p < 0.05$, ** $p < 0.01$

[36] The last two columns of Table A.5 show estimates without covariates (Column 8) and with zip code fixed effects only (Column 9).

adopt alternative strategies (e.g., increasing rents) to compensate for a diminished ability to evict.

Appendix A.1 discusses a supplementary regression discontinuity event study analysis, the main results of which are presented in Figures A.4-A.6. This exercise focuses on cases filed within +/-10 months UA start for the treated zip codes in the first three UA cohorts. Abrupt increases in representation rates and coincident decreases in adverse outcomes are evident; at the same time, case filings and other case characteristics remain smooth around program start, providing additional support for the main results and their identifying assumptions.

6. Discussion and conclusions

Though detailed, the housing court records have several shortcomings. The most obvious is the redaction of personally identifiable information, which limits our ability to observe respondent characteristics and to follow respondents after they disappear from the housing court records.[37] A second limitation is that we do not directly observe the quality or the content of the services lawyers provide. While we are able to measure some channels through which lawyers have the notable impacts, future work will be necessary to understand the precise contributions lawyers make and explore whether these contributions can be delivered through alternative, potentially cheaper, means (e.g., paralegals or court processes). A third 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 effects of 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 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 28.5 and 51.5 percent), log judgment amount (between 44.7 and 81.5 percent),[38] in the probability of eviction warrant issuance (between 33.7 and 61.3 percent), and in the probability of ultimately being evicted (between 41.1 and 77.8 percent). The high-end estimates are upper bounds on the possible effects of the program if people who obtain

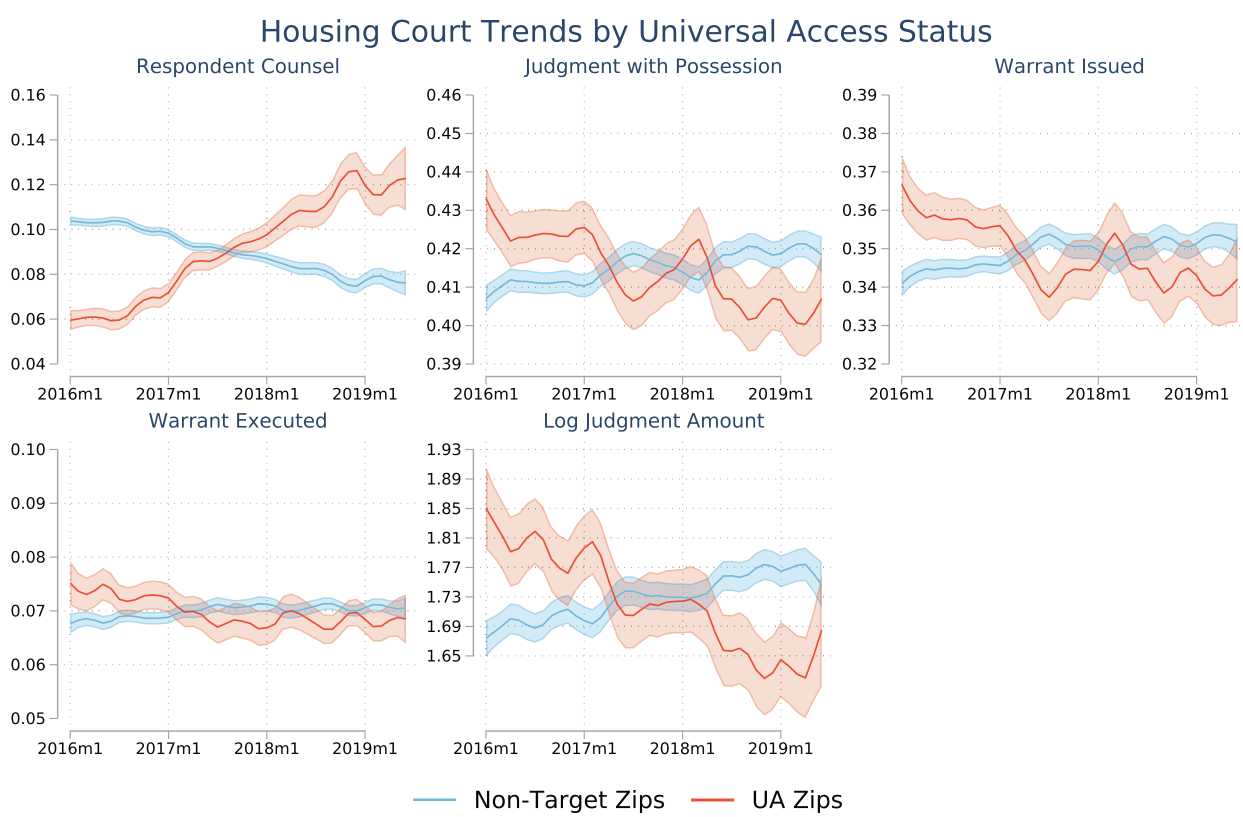

Fig. 6. Local mean regressions residualizing for zip code and borough*month fixed effects. X-axes give filing month; Y-axes give residualized mean, recentered by adding back in non-target zip mean. Shaded areas are 95 percent confidence intervals.

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

[38] 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 3 is -$877 (significant at the one percent level) relative to a mean of $1,277.

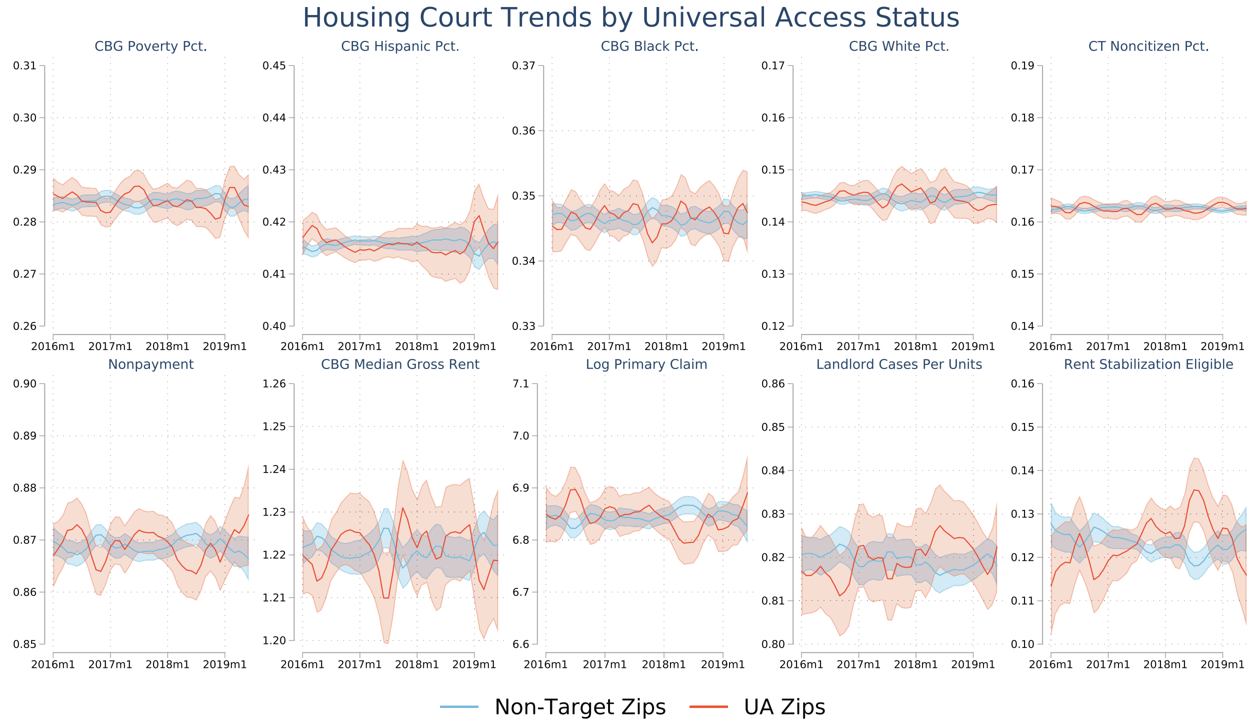

Fig. 7. Local mean regressions residualizing for zip code and borough*month fixed effects. X-axes give filing month; Y-axes give residualized mean, recentered by adding back in non-target zip mean. Shaded areas are 95 percent confidence intervals.

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 understated in administrative records. But lower bound estimates still suggest very substantial effects. We also find that legal representation has larger effects among tenants with the greatest ex ante risks of adverse outcomes in housing court.

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 (see Table 2 and Figs. 6, 7, and A.3). In particular, there is no evidence that post-UA cases are drawn from areas with higher median rents, as one might expect if UA caused rents to rise, or that the back-rent amounts in question have changed.[39] We cannot rule out the possibility that landlords will change their behavior in the longer run, which is a key question for future work. However, providing legal representation for tenants is a relatively light-touch invention into housing markets. UA does not impose rent control, housing regulations, or new taxes, nor does it 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.

The benefits of averted evictions extend beyond housing court. As documented by Collinson et al., (2022), evictions have myriad adverse effects on evictees' housing stability; likelihood of homelessness; earnings and employment; financial wellbeing; and physical and mental health. These consequences are concentrated in the year or two following an eviction, and moreover, the onset of tenant distress is evident in many domains prior to eviction. Thus, even in a worse-case scenario for legal representation–a delaying of the inevitable–it would seem that the benefits of buying of time for tenants to smooth their challenges is potentially considerable.

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: targeting resources to tenants whose observable characteristics suggest greater likelihoods of adverse outcomes would likely produce a greater impact on tenant outcomes per dollar spent. Whether such targeting is politically palatable remains to be seen.

Data availability

The authors do not have permission to share data.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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

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Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jpubeco.2023.104844.

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