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

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NBER Working Paper No. 29836
March 2022, Revised March 2022
JEL No. I3,I38,K15,K4

## ABSTRACT

Housing is one of the areas where it may be most critical for poor people to have access to legal representation in civil cases. We study the roll-out of New York City's Universal Access to Counsel program (UA), using detailed address-level housing court data from 2016 to 2019. The program, which became law in August 2017, 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 access to lawyers are less likely to be subject to possessory judgments, face smaller monetary judgments, and are less likely to have eviction warrants issued against them. Lawyers have larger effects in poorer places and in those with larger shares of non-citizens. UA also reduces executed evictions in these locations. Our results support the idea that legal representation in civil procedures can have an important positive impact on the lives of poor people.

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For more than 50 years, the right to legal representation in criminal cases has been part of the U.S. anti-poverty strategy. 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) which established the right of indigent defendants in criminal cases to be represented by counsel at public expense. Yet despite calls for a "Civil Gideon," there is no similar right to representation in U.S. civil cases. The U.S. remains an outlier among wealthy democracies, which otherwise all guarantee access to lawyers in civil suits (Charn, 2013).

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

Landlords almost always have legal representation while tenants usually lack it, and this imbalance may generate excessive housing instability from a social perspective. Yet, there is surprisingly little evidence that providing legal representation to tenants improves their outcomes in court. Naïve comparisons between tenants with and without lawyers are likely to be confounded by selection bias; a priori, it is not clear in which direction this bias might operate, as tenants with counsel may be better or worse off than average. As we discuss further below, three 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) show that there was a noticeable rise in representation rates in 10 New York City zip codes that were targeted by an Expanded Legal Services initiative in the mid-2010's, and that this rise was not observed in other similar zip codes. They also find a weak negative correlation between increased representation and eviction rates in the 10 zip codes.

We study the roll-out of New York City's Universal Access to Counsel program (UA), which became law in August 2017.[1] 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 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).

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[1] 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.

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 identify its causal effects. We estimate models that include detailed information about the Census block group and housing unit in addition to zip code fixed effects, so that causal inferences are made using within-zip code changes in access to legal representation under the UA program rather than comparisons across zip codes. We allow for heterogeneity in the effects of representation by neighborhood characteristics including racial/ethnic composition, median rent, and the proportion of non-citizens.

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, less likely to have eviction warrants issued against them, and face smaller monetary judgments (i.e. less back rent owed). We also find suggestive evidence that lawyers reduced the probability of executed evictions. However, these benefits are not distributed evenly. Lawyers have markedly larger effects in relatively poorer places and in those with larger shares of non-citizens and non-white residents. In particular, we find large and statistically significant reductions in executed evictions in Census tracts with above-median proportions of non-citizens and in Census block groups with above-median poverty rates. These results suggest that a program targeting these areas could have even larger impact per dollar spent than one with universal ambitions. More generally, our results support the idea that legal representation in civil procedures can have an important positive impact on the lives of poor people.

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

## 1.Background

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

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. Even when they lose, skillful attorneys can buy time and concessions for tenants.

However, doubts have also been expressed about the wisdom of requiring lawyers for both sides: Charn (2013) cites a Judge Friendly who said that "Within the limits of professional propriety, causing delay and sowing confusion not only are [the lawyer's] right but may be his duty." In Price v. Turner 691 S.E.2d 470 n.2 (2010), the Supreme Court noted that in some cases appointing a lawyer for the defendant could make the outcome "less fair overall" by delaying payments to an injured party or by advantaging one poor person in a struggle against another slightly less poor person.

In the law and economics literature, lawyers have been cast as "agents of the devil in a prisoner's dilemma game" (Ashenfelter et al. 2013). The argument is that the outcome may not be very different when both parties have lawyers than when neither party has a lawyer.

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However, the legal costs are undoubtedly higher in the first case. From this point of view, the way to improve outcomes in housing court would not necessarily be to provide lawyers to tenants but to provide an alternative process that bypasses the court system entirely.

All of this discussion assumes that representation is actually effective in improving tenant outcomes, but this is not obvious. 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.

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 few randomized trials of legal representation in civil procedures. Greiner and Pattanayak (2012) look at representation for people denied unemployment benefits, and find no effect on the probability of success and a delay in the adjudication of the case. Turning specifically to housing cases, an experiment reported in Seron et al. (2001) involved a comparison of 134 treatment tenants who received access to pro bono lawyers when they arrived at housing court compared to 134 controls who did not. This evaluation found very positive

effects of representation for tenants, with represented tenants being about half as likely to be evicted compared to unrepresented tenants. However, these results might not generalize to UA: the sample was small, the program provided pro bono representation from private firms, and the experiment took place more than 20 years ago.

Greiner et al. (2013) assess an experiment in a Massachusetts housing court in which treated clients received full representation and control clients only received more limited legal services. This experiment found that full representation helped tenants. One possible takeaway is that the form of the intervention relative to the "control" matters.

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


1b) Housing and Housing Cases in New York City

Housing issues are top of mind in New York City given that 68.1 percent of households rent, compared to 35.9 percent nationwide (NYU Furman Center, 2020). New York's Civil Courts have created special housing courts to deal with conflicts between tenants and landlords. There is one court for each of the five boroughs (i.e., counties) and two additional smaller special courts in Harlem and Red Hook. Most housing court cases (93 percent) are eviction petitions initiated by landlords, and that is our focus here. Of these, most involve nonpayment of rent (86 percent in fiscal year 2019) (Office of Civil Justice, 2019b). All other types of cases (involving things like violations of the lease or overstaying the end of a lease) are referred to as "holdover" cases. In fiscal year 2019, 209,995 residential eviction petitions were filed and 81,297 eviction warrants were issued (Office of Civil Justice, 2019b). City Marshals executed 20,013 evictions

in calendar year 2019 suggesting that only about a quarter of warrants are formally executed (Office of Civil Justice, 2020b).

Eviction cases proceed as follows: First, the landlord must provide the tenant with notice of intent to file a case. Once the case is filed, the tenant has 10 days to respond. When the response is received, a trial date is set, usually three to eight days after receipt of the response.[2] Nonpayment cases automatically end (and any pending warrants are vacated) upon tenant repayment of rent owed. It is common for tenants to forfeit cases by failing to respond or failing to appear in court. In this event, a landlord can apply for a default judgment including back rent and a warrant of eviction. At any stage in the process, tenants may 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).[3]

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, perhaps as a result of a hasty "conference" in a corridor. Such settlements, or "stipulations" are then typically codified by a judge in a formal judgment; indeed, these negotiated stipulations are the most common form of judgment in housing court. If the case proceeds to a (non-jury) trial there could be an postponement, a dismissal, a discontinuance (i.e. a formal determination that the case will not proceed), or a

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[2] This sequence is for nonpayment cases. For other landlord-initiated cases, the calendaring is automatic and tenant responses usually take place at the court appearance.

[3] Whether due to "informal" eviction or otherwise, what is true of many cases is that initial filing is both the first and last step; these cases are eventually disposed of administratively without any further action on the part of the landlord or tenant.

judgment.  Judgments can include monetary awards (e.g. back rent) or the issuance of a warrant of eviction.  As a rule of thumb, judgments, including those that reflect settlements, in cases filed by landlords are unfavorable for tenants.

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

The law creating UA, also known as "right to counsel" (RTC), is administered by the Office of Civil Justice, within the NYC Department of Social Services (DSS).[4]  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[5], all tenants, regardless of income, are entitled to "brief" legal assistance, consisting of a single individualized consultation.  Households with income less than 200 percent of the poverty threshold are entitled to "full," ongoing, legal representation including:  client consultations; legal advice and research; constructing a defense; preparation and filing of court documents; negotiating 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 usually begin at the tenant's first court appearance:  court staff screen tenants on-site and immediately refer them to contracted providers (Been et. al., 2018; Ellen et. al., 2020).

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[4] DSS is alternatively known as the Human Resources Administration (HRA). OCJ was established by law (City Council Intro 736-A) in June 2015 and manages City-funded civil legal services for low-income and otherwise vulnerable New Yorkers (Office of Civil Justice, 2016).
[5] UA is codified as: NYC Administrative Code Title 26, Chapter 13: Provision of Legal Services in Eviction Proceedings.

Implementation has been phased in by cohorts of target zip codes (Office of Civil Justice. 2018). The first cohort of 10 zip codes 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) (Ellen et. al., 2020). These zip codes were selected in part because they had high rates of housing court utilization and in part due to other considerations such as the necessity of serving zip codes in all five boroughs and the availability of non-profit providers.[6] 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[7]---for a total of 25 zip codes (FY16-17, FY18, FY19, and FY20 cohorts) by the end of our sample period---with the mandate of serving the whole city by July 2022. However, in part due to the significant changes to housing court processes caused by the COVID-19 pandemic, OCJ achieved citywide implementation by June 1, 2021, a year ahead of schedule (Office of Civil Justice, 2021). We exploit this gradual roll out to identify the causal effects of legal representation.

Figure 1 shows a map of New York City zip codes, where the intensity of the blue shading indicates the median income in the Census block group. Target zip codes from the first four UA cohorts are outlined in red.[8] The figure suggests that the zip codes chosen to be included in the UA program generally had among the lowest median incomes in each borough. Figure 2 shows a similar figure with the number of housing court cases per 1000 rental units (averaged over our sample period). The map shows that while some of zip codes with the

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[6] 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).

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

[8] For drawing these maps, we augment our geocoded housing court data set, described in Section 2, with shapefiles from the Department of City Planning and the Census Tigerline, and Census crosswalk files between census tracts and zip code tabulation (ZCTA) areas.

highest caseloads were targeted by UA, others with similarly high rates were not targeted. In what follows, we include zip code fixed effects in all 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. Tenants in non-target zip codes could also be served by UA if sufficient resources were available.[9] In fiscal years 2018 and 2019, the city reported on the number of tenants served in each zip code regardless of whether the zip code was one of the target UA areas. Figure 3 shows that while target zip codes had the greatest number of cases served by UA, there were also relatively high numbers of cases served by UA in adjacent zip codes. We exploit this source of variation in addition to the roll out.[10]

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 several provisions designed to protect tenants from eviction.[11] Given this major change in the law governing evictions, we limit our sample to cases filed prior to June 14, 2019, the date the

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[9] Screening and intake for UA occurs primary at housing court itself, not in the community (Been et. al., 2018; Ellen et. al., 2020).

[10] The rate of UA representation per cases filed is shown in Figure A.1.

[11] These provisions included: capping security deposits at one month's rent; allowing a five-day grace period for rent to be defined as ``late''; requiring landlords to notify tenants by certified mail when rent is late; allowing judges to stay evictions for up to a year if a tenant can demonstrate genuine inability to find a comparable apartment in the same neighborhood and would experience extreme hardship from a move (e.g., children in school); increasing penalties for retaliatory evictions; requiring 14-day advance notice for nonpayment cases (up from three days); and immediate voiding of nonpayment proceedings upon payment of past-due rent (NYU Furman Center, 2021; Office of the New York State Attorney General, undated; NYS Homes and Community Renewal, 2020).

law took effect. The State also made major changes to laws governing eviction in response to the COVID-19 pandemic, but given our cutoff date of cases filed before June 2019, these are not relevant.[12]

The impact of the UA rollout is illustrated in Figures 4a-4e, which graph smoothed representation rates by priority cohort for each New York City borough. Bold shading denotes UA treatment, with start dates estimated by the algorithm we discuss in Section 2 and fully describe in Appendix A.1. Appendix Table A.1 lists the zip codes included in each cohort and Table A.2 gives the empirical UA start dates for each cohort.

Figure 4a 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. One sees a sharp increase in representation in the first cohort to be treated (the Fiscal Year 2016-2017, or FY16-17, cohort) beginning in December 2016, and rising to about 25 percent of cases by April 2017. The next cohort (FY18) shows a sharp jump in representation in January 2018, followed by the FY19 cohort in September 2018. There are no similar increases in representation in the non-target zip codes, or in the FY20 cohort, which had yet to be treated as of June 2019.

In Kings County (Brooklyn), shown in Figure 4b, 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

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[12] On March 16, 2020, NYC Housing Court halted all new and pending eviction cases (except for ``essential'' cases involving illegal lockouts and emergency repairs). Given our sample cutoff of June 14, 2019, we observe all cases for a minimum of nine month pre-COVID. In April 2020, the Court resumed settlement conferences for pending eviction cases in which both parties were represented by counsel. In August 2020, the Court allowed pre-pandemic eviction warrants to begin moving forward for all cases, including those without counsel. However, in so doing, OCJ and the Court expanded access to legal services beyond the initial UA plan, requiring landlord's motion papers to include information about UA. In addition, UA was expanded temporarily to all tenants facing active evictions, including those outside priority zip codes and above the income limits (Office of Civil Justice, 2020b).

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 Figure 4c), rates of representation were already relatively high in the FY16-17 cohort, and rise slowly over time suggesting that UA did not have a dramatic impact in these zip codes.[13] 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.

In Queens County (Figure 4d) the pattern is different. Most zip codes show low but slowly rising rates of representation, and 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 in Richmond County (Staten Island, Figure 4e) one can see that initial rates of representation in January 2016 were relatively high, at between 10 and 20 percent. 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.[14]

These figures 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

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[13] Since we do not observe a significant change in representation rates in the Manhattan FY16-17 cohort, we cannot be sure whether high representation rates are due to a pre-2016 introduction of UA-style legal services or is instead attributable other (confounding) factors, so we categorize these zip codes as untreated for the duration of our sample. This is inconsequential for our analysis, since the inclusion of zip code fixed effects in all of our main analyses mean that neither always-treated nor never-treated zip codes contribute to the identification of causal effects.
[14] Staten Island is also considerably smaller in population than the other boroughs, so the relatively small sample size explains some of the bumpiness in the graphs. There is no FY20 cohort in Staten Island.

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

The sharp increases in representation in zip codes targeted by the UA program compared to stable patterns in non-target zip codes or zip codes that had yet to be treated provide the basis for the identification strategy.  For each borough and zip code cohort, the start of the program is defined as the first day of the first month to begin an abrupt, large, and sustained increase in tenant representation rates. The algorithm for identifying these turning points is described in Appendix A.1.  In brief, a borough-cohort's UA start date is defined as the month in which the rate of change in the smoothed tenant representation rate is at least one percentage point greater than the prior month's change and the nine-month change in representation for the period beginning with that month is nine percentage points or more.  For cohorts with more than one such month, we break ties by choosing the candidate start month with the largest nine-month percent change in representation.

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

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

## 2. Creating the Data Set

Our main source of data are 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 other), for

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[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).

whether the landlord has a lawyer, whether the landlord is the New York City Housing Authority (NYCHA)[17], whether the case has a "specialty designation" (e.g. a flag indicating that the building is a co-op); counts of respondents and petitioners; court fixed effects (dummies for each county court and the two specialized courts); and time effects (citywide half-year fixed effects, month-of-the-year fixed effects, and linear time trends by zip code).

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; and an indicator for whether there has been a building alteration. We also construct the following landlord-level controls: number of NYC properties, number of NYC buildings, number of NYC residential units, housing court cases, housing court cases per number of residential units, and total assessed value of properties owned.

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[17] One major gap in the program's early years is that it did not serve NYCHA tenants, even in priority zip codes. Of the 32k households assisted with eviction proceedings in FY2019, just 266 were NYCHA tenants (Office of Civil Justice, 2019b). This omission is notable because NYCHA is, by a wide margin, the landlord responsible for the greatest share of eviction filings in the city. This situation began to change in FY2020, when UA commenced on-site legal services to seniors (age 62+) facing NYCHA administrative termination of tenancy proceedings (Office of Civil Justice, 2020a). When we estimated models separately for NYCHA and non-NYCHA addresses, we found that there were only statistically significant effects in the non-NYCHA units.

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.[18] The models include a vector of census block group characteristics capturing total population, median household income, household poverty rate, total housing units, renter share of housing units, median gross rent, and population shares that are Hispanic, Black, Asian, White, ages 0--17, ages 65+, and female, as well as census tract shares of non-citizens and naturalized citizens.[19] In several analyses, census block groups are characterized using a series of zero-one indicators for whether the block group's majority race/ethnic group is Hispanic, Black, non-Hispanic White, or Asian.

All continuous covariates from the ACS and PLUTO are transformed into a series of indicators for whether the address is in the lowest to highest quartile, *calculated from the distributions within our main sample* (e.g. the "fourth quartile of the CBG poverty rate," refers to the 25 percent of housing court tenants whose CBG poverty rates are the highest in our sample).[20] Note also that, given the nature of these datasets, variables deriving from PLUTO or the ACS are observed at single points in time (2019 for the ACS and 2021 for PLUTO), and so vary within zip code but not over time.[21] Further information about variable definitions appears in Appendix A.2.

Several limitations are imposed on the raw data to refine the sample of cases for analysis. Starting from the sample of all 863,239 housing court cases filed between 1/1/2016 and

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[18] 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.
[19] Citizenship data is not available at the block group level.
[20] The reason for transforming some continuous variables into categorical variables using quartiles of the distribution is that we wish to retain cases for which some of these variables are missing. By using categories, we can add a fifth category for "missing." We similarly add a "missing" category to all ACS and PLUTO covariates that are inherently categorical.
[21] Correspondingly, variables measured in dollars are in 2019 dollars for the ACS and 2021 dollars for PLUTO.

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

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

2b. Defining the Treatment and Instrumental Variables.

The main explanatory variable of interest is "respondent counsel" a 0/1 indicator for whether a tenant has professional legal representation.[23] The effects of counsel on tenant outcomes are likely to be confounded by selection bias. For example, tenants may only seek representation when they face especially bad outcomes, in which case the raw association between legal representation and adverse outcomes would be negative. Or it may be that the most affluent or savvy tenants retain lawyers, in which case selection bias would operate in the opposite direction.

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[22] Specifically, for each property address (down to the apartment unit level) and each 14-day period, we keep only the filing with the most case activity (defined as the sum of decisions, judgments, appearances, motions, and other non-filing events). In other words, 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.

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

Given this concern, we use two different instruments for individual tenant representation. The first is the "empirical UA treatment" indicator we describe in Section 1C; this is a 0/1 indicator for whether the UA program is operating in a particular target zip code at the time of the initial case filing. The identifying assumption is that UA affects the probability that a tenant has representation, but has no effect on outcomes other than through that channel. Because zip code fixed effects in all of our specifications, identification is not based on a comparison of these zip codes with other zip codes, but on the timing of the introduction of the program within each zip code, as discussed above. The staggered implementation of UA across boroughs and zip code cohorts, such that only certain zip codes were affected during each fiscal year makes it unlikely that the effects of UA could be confounded with those of other policies.[24] Zip code specific linear time trends are also included in order to absorb the effects of any smoothly trending variables within zip codes.

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

One potential concern is that landlords might change the types of cases that they bring against tenants following the introduction of the program. In this case, estimated effects of representation in court might actually reflect changes in the way that cases that go to court are

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

selected. The rich data described above allows us to control for detailed characteristics of the housing units. We also estimate models that include fixed effects for each address (down to the apartment unit number), in which the effects of UA are identified by the subset of approximately 60,000 addresses that had cases filed both before and after UA.

2c. Defining the Outcome Variables

In what follows, we focus on four main tenant outcomes:[25]

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

*Log(Judgment Amount)*: the natural logarithm of the final monetary amount awarded to a landlord (in real January 2021 dollars adjusted using the monthly Consumer Price Index for all urban consumers).[27]

*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

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[25] Outcomes generally correspond to pivotal events in the housing court process; we define the presence or absence of these events by whether a date corresponding to the event is recorded in the data. Though the data contain other fields related to these events, we have found the date field to be among the most consistently populated and reliable.
[26] We have also looked at whether any possessory judgment was ever issued in the case (vacated or not) and gotten very similar results.
[27] Before taking the log, we winsorize judgment amounts at the first and ninety-ninth percentiles, assign cases with no judgment amount or no judgment are an amount of zero, and then add one to all judgment amounts.

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.

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*: three 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 or failure to appear (and thereby forfeiture of the case); or (3) a court proceeding (e.g., trial).

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

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

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

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

*2d. Summary statistics*

Table 1 presents some initial summary statistics and a simple difference-in-differences comparison of UA and non-UA zip codes. Columns 1 and 2 show means for calendar year 2016 and fiscal year 2019 for zip codes that were not in the first four UA cohorts. Columns 3 and 4 shows means for the same two dates for the UA zip codes. Columns 5-7 shows the difference in difference (DiD), with its standard error and a p-value computed using bivariate regressions of the covariate on an indicator for whether UA had been introduced in the zip code and clustering standard errors by zip code.[28]

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 zip codes representation rates rise from 9.1 percent to 17.3 percent; the DiD is statistically significant and consistent with what was reported by Ellen et al. (2020) for the first cohort of treated zip codes. 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 Figures 4a-4e, some zip codes earmarked for UA do not seem to have had a meaningful increase in representation. The share of households served by UA rises in both non-UA and UA zip codes, but rises by 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, though not for warrant executed. It is worth noting though that the fraction of cases with executed warrants is substantially less than

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[28] Throughout the paper, we denote statistical significance as follows: *p<0.05, **p<0.01. We do not flag results that are significant at only the 10 percent level of confidence.

the fraction of cases with warrants issued, even allowing for the fact that some warrants are vacated. This suggests that most households facing a warrant of eviction either settle with the landlord informally or leave "voluntarily" rather than waiting for the marshals to arrive and enforce the eviction. Thus, focusing only on evictions that are formally executed may substantially underestimate the impact of housing court proceedings on tenants.

In terms of possible mechanisms for the effects of tenant representation, Table 1 suggests that the UA zip codes see fewer cases with a judgment due to a settlement, and fewer cases that are forfeited by a tenant failure to appear. The number of days between a case filing and a judgment is also longer. Taken together, this suggests that the DiD reduction in the probability of judgment may be driven by the effects of lawyers at the early stages of proceedings; there does not appear to be an impact on cases that make it to the trial stage.

The remaining panels of Table 1 focus on the covariates we have defined using the housing court data, the ACS, and PLUTO. In addition to showing means for these variables, the table demonstrates that there are no significant changes 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. Hence, there is little evidence that landlords changed their propensity to file cases in response to the UA program, at least in the period we study.

## 3. Empirical Methods

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

$$(1) \ Y_{iabt} = \beta_0 + \beta_1 R_{iabt} + \beta_2 HC_{iabt} + \beta_3 PLUTO_a + \beta_4 ACS_b + Month_t + HY_t + Zip_b*Time_t$$
$$+ \ \varepsilon_{iabt},$$

where Y is a housing court outcome, i indexes the case, a indexes the address, b indexes the Census block group, and t indicates the half year in which the case was filed. R is an indicator equal to one if the tenant has legal representation. HC is the vector of housing court variables shown in Panel B of Table 1, ACS is the vector of census block group (or tract) characteristics shown in Panel C of Table 1, and PLUTO is the vector of variables shown in Panel D of Table 1. The model also includes fixed effects for each zip code (Zip), which are interacted with a zip code specific linear time trend (Time), as well as dummy variables for calendar month of case filing and half-year (HY) fixed effects.[29] The $\varepsilon_{iabt}$ denotes the error term. We also estimate a model that includes address-specific fixed effects:

$$(2) \ Y_{iabt} = \beta_0 + \beta_1 R_{iabt} + \beta_2 HC_{iabt} + \beta_3 PLUTO_a + \beta_4 ACS_b + Month_t + HY_t + Zip_b*Time_t +$$
$$Address_{iabt} + \varepsilon_{iabt},$$

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

$$(3) \ R_{iabt} = \alpha_0 + \alpha_1 UA_{iabt} + \alpha_2 HC_{iabt} + \alpha_3 PLUTO_a + \alpha_4 ACS_b + Month_t + HY_t +$$
$$Zip_b*Time_t + \varpi_{iazt},$$

$$(4) \ R_{iabt} = \alpha_0 + \alpha_1 UA_{iabt} + \alpha_2 HC_{iabt} + \alpha_3 PLUTO_a + \alpha_4 ACS_b + Month_t + HY_t +$$
$$Zip_b*Time_t + Address_{iabt} + \varpi_{iazt},$$

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[29] Half-years are included because confounding policy changes could occur either at the calendar years or city fiscal year level (where fiscal years run July 1-June 30).

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

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

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

$$(5)\ Y_{iabt} = \gamma_0 + \gamma_1 m_{iabt} + \gamma_2 m_{iabt}{*}R_{ibt} + \gamma_3 R_{iabt} + \gamma_4 HC_{iabt} + \gamma_5 PLUTO_a + \gamma_6 ACS_b + Month_t +$$
$$HY_t + Zip_b + \omega_{iazt},$$

where m is the running variable, i.e., the number of months relative to the UA start date for each borough-cohort. The instrument for R is UA as before, and $m_{ibt}{*}R_{ibt}$ is instrumented with $m_{ibt}{*}UA$. This RD design is a stringent test of our general identification assumptions since the sample is much smaller, including only 36,856 cases.

## 4. Estimated Effects of Legal Representation

Table 2 shows within-zip code estimates of Equation (1) that rely on the staggered timing of the UA roll-out. The first column shows OLS estimates which indicate that representation is associated with a significantly lower probability of a possessory judgment, having a warrant issued, or having a warrant executed. However, as discussed above, these estimates could reflect the effects of 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 eight percentage points (pp), or about 88 percent relative to the full sample mean. The remaining estimates suggest that representation has very large effects on the affected cases, reducing the probability of a possessory judgment by 38.5 pp, the log judgment amount by 2.226, and the probability of a warrant being issued by 38.1 pp. The point estimate for executed evictions is also large---a reduction of 6.7 pp---but imprecise, in part because formal evictions are relatively rare.

The fact that the TSLS estimates are much larger than the OLS suggests that the cases that receive representation because of the UA program (the compliers) receive large benefits relative to tenants who would always have had representation (or those who would never have representation). Comparing the OLS and TSLS estimates also 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 coefficient on the log judgment amount, which, if it was causal, would imply that legal representation actually worsens tenant outcomes.

Columns (3) and (4) shows that the estimates are very similar when the models are re-estimated using the set of addresses that had more than one filing and include a fixed effect for

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the exact address.  In these models, the effects are identified using only the approximately 57,000 addresses that have at least one case filing before UA implementation and one case filing after UA implementation.  The outcome estimates are somewhat larger than in the full sample.  For example, the effect on executed evictions is a reduction of 8.9 pp which is now statistically significant at the 10 percent level. Thus, even if all of the time-invariant characteristics of the units themselves (and implicitly the type of people who rent them) are controlled, there are large effects of representation on those who gain representation because of UA.

For reference, the reduced form estimates corresponding to columns (2) and (4) are shown in columns (1) and (2) of Appendix Table A.3.   These estimates show the average effect of the introduction of the UA program on the outcomes of all of the housing court cases in the zip code.  These estimates are statistically significant but, of course more modest since the TSLS estimates are scaled by the first stage.  They show a reduction in the probability of possessory judgment of 3.1 pp, a reduction of log judgment amounts by 0.177, and a reduction in the probability that a warrant is issued of 3.0 pp.[30]

The contrast between the OLS and the TSLS findings 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 presented in Appendix Table A.4.  For most observables, estimated differences are statistically significant but small in magnitude, which suggests that the typical complier is not much different than the typical tenant in housing court. However, there are a few larger contrasts that suggest 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

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[30] Recall that TSLS coefficients give the estimated impact of a 1 unit, or 100 percent change in the probability of representation, but, as the first-stage makes clear, UA does not ensure that a tenant will have a lawyer.

for non-compliers); building-to-lot area ratio (2.92 vs. 3.43); landlord units (29,024 vs. 43,508); and 1--2 family homes (9.6 percent vs. 4.4 percent). One possible explanation is the political constraint that UA be relatively equalized across boroughs. Of course, unobservables may also play a role, and compliers may be, in some sense, negatively selected. For example, they might have a high propensity to miss a scheduled hearing, as discussed further below.

In order to interpret the magnitude of the effects, one would ideally like to know what would have happened to the compliers in the absence of the program. One possible baseline is provided by cases file in 2016 in which there was at least some housing court activity. Means for this set of cases are shown in the last column of Table A.8. Relative to these means, the estimates in column 2 of Table 2 suggest that tenant representation via the UA program reduced the probability of a possessory judgment by 62 percent, reduced the award amount by 85 percent, reduced the probability of a warrant being issued by 72 percent, and reduced the probability of a warrant being executed by 62 percent (though this last estimate is not statistically significant).

The TSLS findings in Table 2 are very robust. Appendix Table A.5 shows estimates 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.

Table A.6 shows estimates omitting zip code fixed effects and zip-specific time trends (Column 1), and estimates including zip code fixed effects but omitting the time trends (Column 2). Columns (3) to (5) shows estimates from only the last two years of our data, fiscal years 2018 and 2019. All of these estimates are similar to those shown in Table 2. Of particular interest, Column 2 shows that the estimated effect on executed evictions is a highly significant

reduction of 7.6 pp in the absence of zip-specific linear time trends (which eat up much of the identifying variation in the data).

Table 3 explores heterogeneity in the instrumental variables estimates. Panel (1) splits cases according to whether the percentage of non-citizens living in a tenant's Census tract is above or below the in-sample median. The estimated effects are all larger in tracts with higher percentages of non-citizen. There is also a statistically significant reduction in the probability that a warrant is executed in this subsample.

Panel (2) splits Census block groups by whether they have a Hispanic majority, a Black majority, a non-Hispanic white majority, or an Asian majority. Not every block group is included because a few have no clear majority group (though given residential segregation, the categorization is essentially identical if we use pluralities). The largest first stage effects are in Hispanic majority block groups, followed by those with Black majorities, and those with Asian majorities. UA is not estimated to have any effect on the probability of representation in majority non-Hispanic white block groups. The effects of representation on outcomes is also estimated to have the largest effects in the majority Hispanic block groups, followed by the majority Black block groups. The effects for Asian-majority block groups are imprecisely estimated, which is not surprising since there are relatively few cases in these block groups. However, we still find a significant negative effect of representation on the probability that a warrant is executed, which may be consistent with the UA program having larger effects in heavily immigrant neighborhoods.

Panel (3) of Table 3 splits the sample by indicators of poverty. The first set of estimates divide the sample by whether the median rent (imputed using the median rent in the Census block group) is above or below the sample median. The results suggest that the estimated effects

are particularly large for the lower-rent neighborhoods. The second analysis splits the sample by whether a tenant's CBG is above or below the in-sample median of CBG poverty rates. These estimates show that while representation has an effect in both relatively poor and less poor places, the biggest impacts of UA representation are in the places with above-median poverty rates. In these zip codes, representation reduces executed evictions by a statistically significant 14.7 pp.[31]

Table 4 shows estimates for some additional outcomes that shed light on how representation affects tenant outcomes. Panel A shows that conditional on having a judgment, the judgment is much more likely to have been vacated if the tenant has legal counsel. Similarly, when a warrant has been ordered, it is more likely to be vacated if the tenant has representation. These results suggest that lawyers continue to fight for their clients even after adverse rulings, and that in many cases, they are successful in having these rulings overturned.

Panel B of Table 4 shows the effects of representation on the type of judgment that is reached, which in turn reflects grounds for the judgment. The instrumental variables estimates suggest that representation reduces the probability that the judgment reflects a settlement (the most common basis for a judgment), which may reflect a diminution in self-represented tenants being strong-armed by landlords' attorneys before appearing before a judge. Tenant representation also reduces the probability that a judgment is reached because the tenant "failed to answer" the petition or "failed to appear" in court. Along the same lines, Panel C of Table 4 shows that the number of days from a case filing until a judgment is entered increases by almost three months. Buying time for tenants, even in a loss, may be valuable to them. There is

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[31] This pattern of results---larger impacts among noncitizens, racial minorities, and lower-income tenants---is not incompatible with the compliers analysis discussed above. The compliers analysis suggested that compliers were not very different on average than non-compliers. The heterogeneity analysis is looking at the marginal effect of treatment within subgroups.

however, no significant effect on the number of days until a warrant is executed after it is issued (though the point estimates are suggestive of case lengthening here as well).

Table 5 shows estimates using the alternative "UA intensity" instrument---that is, the number of households in a zip code that received representation through the UA program in a given fiscal year divided by 1,000.  This instrument allows us to take into account 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.4.  The Table 5 estimates follow the same qualitative pattern as those in Table 2.  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.

The results of the regression discontinuity exercise are shown in Figure 5.[32]  Appendix Figure A.2 shows that the density of cases is fairly smooth through the UA implementation "jump" point.  As discussed above, the RD sample of cases---consisting of cases filed within +/- 10 months UA start for the treated zip codes in the first three UA cohorts---is much smaller than the full sample. Nevertheless, Figure 5 shows clear evidence of UA-coincident jumps in the probability that respondents have representation, as well as discontinuous declines in the probability of judgment with possession, the probability of a warrant issuance, and the log judgment amount. The probability that a warrant is executed appears to be on a declining path following UA implementation, but there is no sharp fall.

Appendix Figure A.3 shows that there was no obvious change in the characteristics of cases filed in housing court. There is also little change in the fraction of cases that involve nonpayment.  There is a slight but not statistically significant short-term rise in landlord cases

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[32] The corresponding regression estimates appear in Table A.7.

per unit, but this returns to the long-term trend by 10 months out.  In short, there is little evidence

that UA implementation changed the composition of the housing court caseload, at least over the

timeframe we examine here. Hence, the regression discontinuity framework supports the

identifying assumption that the pool of housing court cases remained similar before and after the

implementation of UA.


## 5. Discussion and Conclusions

Though detailed, the housing court records have several shortcomings. The most obvious

is the redaction of personally identifying information, which limits our ability to observe

respondent characteristics and to follow respondents after they disappear from the housing court

records.  Still, we have estimated a variety of models which rely on different identifying

assumptions.  All of them suggest that the UA program increased the probability that a tenant

had legal representation and that legal representation greatly improved tenant outcomes in

housing court.

In particular, we find large reductions in: the probability that there is a judgment with

possession (on the order of 62 percent), log judgment amount (on the order to 68 percent),[33] and

in the probability of eviction warrant issuance (on the order of 72 percent). We do not always

find statistically significant effects on warrant executions, but we do find large and statistically

significant effects in relatively poor neighborhoods and in those with large shares of non-

citizens.  In the full sample, the point estimates for evictions are similar in magnitude to those on

the other outcomes, and they are statistically significant in models that do not include zip-code

specific time trends.  Moreover, it important to recognize the reality of informal eviction: some--

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[33] Estimating in levels we find an 89% reduction in judgments when looking at mean judgments including zeros.

-and perhaps many---tenants facing eviction warrants will leave before a marshal shows up at the door, suggesting that the whole process leading up to eviction is of consequence.[34]

Of course, an open question is whether giving tenants more bargaining power 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 timeframe we study, we find little evidence of changes in the characteristics of cases filed before and after the introduction of UA. In particular, there is no evidence that post-UA cases are drawn from areas with higher median rents, as one might expect if UA caused rents to rise.

In terms of costs, 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 show that legal representation can substantially improve the lives of poor families at modest cost (Hoynes et al, 2022, Cooper et al. 2022).

At the same time, our estimates show considerable heterogeneity in the effectiveness of the program, depending on the characteristics of the neighborhoods and tenants served: Targeting resources to areas with high concentrations of non-citizens, racial minorities, and poor households would produce a greater impact on tenant outcomes per dollar expended. Whether such targeting is politically palatable remains to be seen.

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[34] 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.

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| | 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** |
| Empirical UA Treatment (IV) | 0.000 | 0.000 | 0.041 | 0.478 | 0.437 | 0.110 | 0.000** |
| UA Households Served/1000 (IV) | 0.000 | 0.275 | 0.000 | 0.886 | 0.611 | 0.138 | 0.000** |
| Judgment with Possession | 0.437 | 0.405 | 0.432 | 0.366 | -0.035 | 0.008 | 0.000** |
| Log Judgment Amount | 1.796 | 1.697 | 1.848 | 1.516 | -0.232 | 0.054 | 0.000** |
| Warrant Issued | 0.367 | 0.335 | 0.376 | 0.313 | -0.031 | 0.007 | 0.000** |
| Warrant Executed | 0.079 | 0.056 | 0.076 | 0.048 | -0.006 | 0.005 | 0.227 |
| Judgment Vacated (Cond. on Judgment) | 0.110 | 0.125 | 0.142 | 0.152 | -0.005 | 0.007 | 0.459 |
| Warrant Vacated (Cond. on Warrant) | 0.066 | 0.068 | 0.084 | 0.076 | -0.011 | 0.004 | 0.007** |
| Judgment: Stip/Settle | 0.245 | 0.227 | 0.242 | 0.198 | -0.025 | 0.006 | 0.000** |
| Judgment: FTA | 0.187 | 0.173 | 0.186 | 0.163 | -0.009 | 0.005 | 0.049* |
| 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 |
| Court: Bronx | 0.349 | 0.337 | 0.390 | 0.372 | -0.006 | 0.012 | 0.626 |
| Court: Kings | 0.277 | 0.298 | 0.242 | 0.234 | -0.028 | 0.015 | 0.054 |
| Court: Redhook | 0.007 | 0.005 | 0.000 | 0.000 | 0.001 | 0.001 | 0.318 |
| Court: New York | 0.167 | 0.163 | 0.217 | 0.236 | 0.023 | 0.013 | 0.068 |
| Court: Harlem | 0.024 | 0.022 | 0.010 | 0.013 | 0.005 | 0.003 | 0.109 |
| Court: Queens | 0.157 | 0.154 | 0.117 | 0.118 | 0.004 | 0.006 | 0.506 |
| Court: Richmond | 0.019 | 0.021 | 0.025 | 0.027 | 0.001 | 0.004 | 0.877 |
| Filed Half-Year | 2016.5 | 2019.0 | 2016.5 | 2019.0 | 0.0 | 0.0 | 0.3 |
| Filed Month | 6.6 | 6.6 | 6.5 | 6.5 | -0.02 | 0.05 | 0.72 |
| 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 |
| Observations | 153,582 | 135,405 | 66,666 | 58,554 | | | |

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

Table 1B: Summary Statistics

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

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

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

| | Zip × Linear Half-Year | | Address FE | |
| --- | --- | --- | --- | --- |
| | OLS (1) | UA IV (2) | OLS (3) | UA IV (4) |
| Respondent Counsel (First Stage) | | 0.079** (0.007) | | 0.078** (0.008) |
| Judgment with Possession | -0.063** (0.006) | -0.385** (0.062) | -0.046** (0.008) | -0.513** (0.081) |
| Log Judgment Amount | 0.231** (0.037) | -2.226** (0.631) | 0.000 (0.048) | -3.057** (0.778) |
| Warrant Issued | -0.059** (0.006) | -0.381** (0.064) | -0.042** (0.009) | -0.503** (0.080) |
| Warrant Executed | -0.024** (0.002) | -0.067 (0.062) | -0.004 (0.003) | -0.089 (0.053) |
| Observations | 727,692 | 727,692 | 456,788 | 456,788 |
| First-Stage F Stat | . | 128.73 | . | 99.99 |
| Covariates | Yes | Yes | Yes | Yes |
| Zip FE | Yes | Yes | Yes | Yes |
| Zip × Linear Half-Year FE | Yes | Yes | Yes | Yes |
| Address FE | No | No | Yes | Yes |

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

Table 3: Heterogeneity Analysis: IV Results

| | Respondent Counsel . (1) | Judgment with Possession (2) | Log Judgment Amount (3) | Warrant Issued . (4) | Warrant Executed . (5) |
|---|---|---|---|---|---|
| **(1) Citizenship** | | | | | |
| CT Noncitizen Pct. Above In-Sample Median | | | | | |
| Yes | 0.091** (0.005) 362,738 [340.06] | -0.422** (0.068) | -2.089** (0.507) | -0.404** (0.073) | -0.153** (0.059) |
| No | 0.065** (0.013) 364,944 [24.23] | -0.320* (0.139) | -2.616* (1.122) | -0.319* (0.136) | 0.092 (0.062) |
| **(2) Race** | | | | | |
| CBG Hispanic Majority | 0.086** (0.005) 292,455 [308.15] | -0.425** (0.063) | -2.603** (0.798) | -0.409** (0.058) | -0.084 (0.062) |
| CBG Black Majority | 0.076** (0.011) 211,142 [43.51] | -0.307** (0.102) | -1.654 (0.893) | -0.285** (0.094) | -0.063 (0.096) |
| CBG White Majority | 0.031 (0.022) 72,078 [1.99] | -0.244 (0.733) | -4.051 (3.935) | -0.472 (0.598) | -0.224 (0.313) |
| CBG Asian Majority | 0.057** (0.012) 14,018 [23.34] | -0.855 (0.521) | 2.518 (2.008) | -1.362 (0.722) | -0.591* (0.280) |
| **(3) Poverty** | | | | | |
| CBG Gross Rent Above In-Sample Median | | | | | |
| Yes | 0.086** (0.008) 356,809 [121.06] | -0.328** (0.072) | -1.762** (0.634) | -0.294** (0.078) | -0.048 (0.078) |
| No | 0.066** (0.011) 370,874 [34.34] | -0.512** (0.096) | -3.175** (0.896) | -0.583** (0.102) | -0.101 (0.057) |
| CBG Poverty Pct. Above In-Sample Median | | | | | |
| Yes | 0.084** (0.006) 364,174 [174.84] | -0.350** (0.096) | -1.970 (1.042) | -0.372** (0.078) | -0.147** (0.041) |
| No | 0.075** (0.010) 363,511 [58.38] | -0.408** (0.104) | -2.368* (0.957) | -0.383** (0.109) | 0.011 (0.071) |

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

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

| | Zip × Linear Half-Year | | Address FE | |
|---|---|---|---|---|
| | OLS (1) | UA IV (2) | OLS (3) | UA IV (4) |
| **A. Judgment Procedures** | | | | |
| Judgment Vacated (Cond. on Judgment) | 0.187** | 0.328** | 0.223** | 0.544** |
| | (0.009) | (0.077) | (0.008) | (0.145) |
| Warrant Vacated (Cond. on Warrant) | 0.126** | 0.156** | 0.145** | 0.181** |
| | (0.007) | (0.036) | (0.006) | (0.062) |
| | | | | |
| **B. Judgment Type** | | | | |
| Judgment: Stip/Settle | 0.048** | -0.179** | 0.020** | -0.279** |
| | (0.005) | (0.046) | (0.006) | (0.085) |
| Judgment: FTA | -0.122** | -0.206** | -0.074** | -0.242** |
| | (0.002) | (0.036) | (0.004) | (0.039) |
| Judgment: Court Proceeding | 0.012** | 0.009 | 0.009** | 0.014 |
| | (0.001) | (0.012) | (0.001) | (0.019) |
| | | | | |
| **C. Length of Case** | | | | |
| Days to Judgment Entered | 69.721** | 87.639** | 45.319** | 69.188 |
| | (2.235) | (28.404) | (1.875) | (39.172) |
| Days to Warrant Executed | 97.273** | 96.813 | 72.477** | 81.435 |
| | (2.448) | (70.160) | (12.165) | (64.980) |

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

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

|  | Zip FE (1) | Address FE (2) |
|---|---|---|
| Respondent Counsel | 0.145** (0.040) | 0.146** (0.028) |
| Judgment with Possession | -0.619** (0.128) | -0.633** (0.140) |
| Log Judgment Amount | -3.722** (0.765) | -3.588** (0.814) |
| Warrant Issued | -0.553** (0.137) | -0.627** (0.162) |
| Warrant Executed | -0.068 (0.102) | -0.043 (0.076) |
| Observations | 403,483 | 202,409 |
| First-Stage F-Stat | 13.46 | 27.55 |
| Covariates | Yes | Yes |
| Zip FE | Yes | Yes |
| Zip × Linear Half-Year FE | No | No |
| Address FE | No | Yes |

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

Figure 1

## Income and Universal Access to Counsel in New York City

Median Household Income (in 1000's) by Census Block Group within ZCTA, 2015-2019
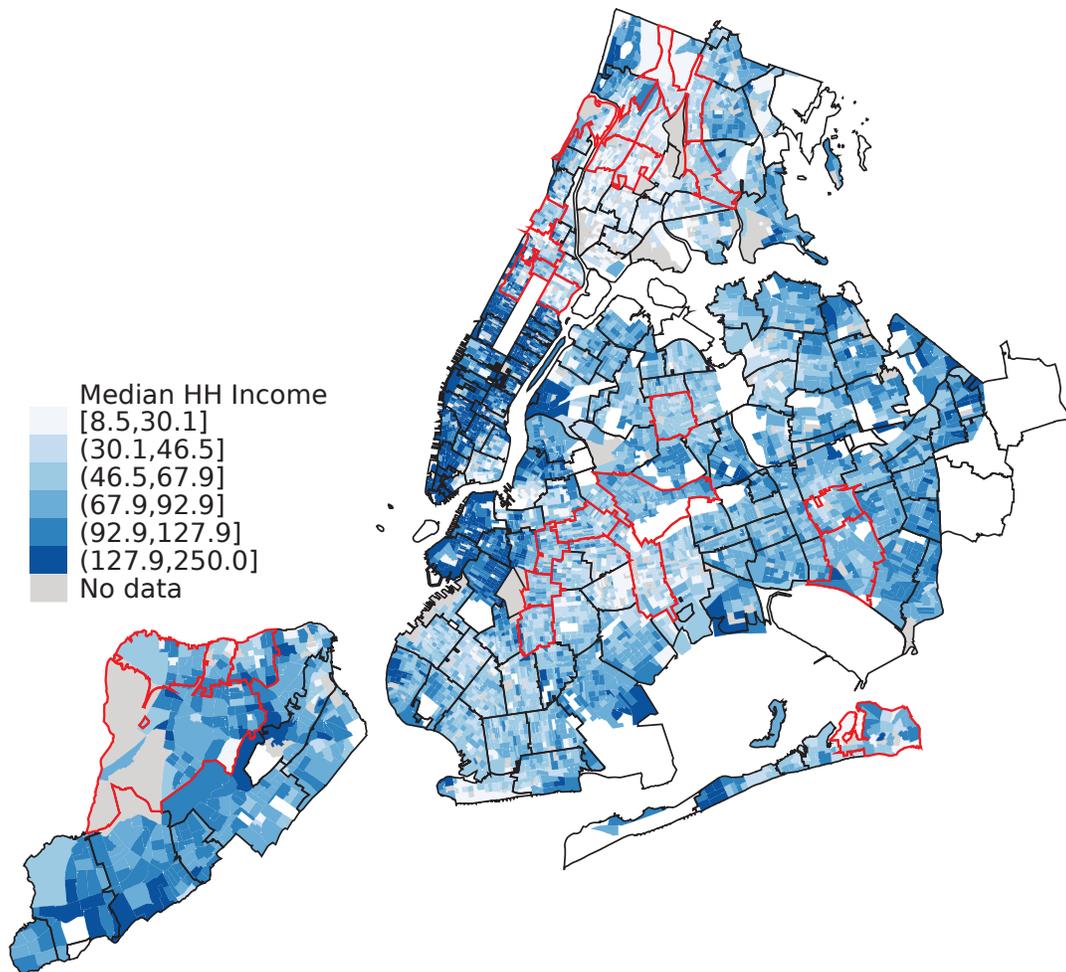
Median HH Income
[8.5,30.1]
(30.1,46.5]
(46.5,67.9]
(67.9,92.9]
(92.9,127.9]
(127.9,250.0]
No data

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

Figure 2

## New York City Housing Court Cases

Landlord-Initated Filings per 1000 Rental Units
by Census Block Group within ZCTA, 1/2016-6/2019
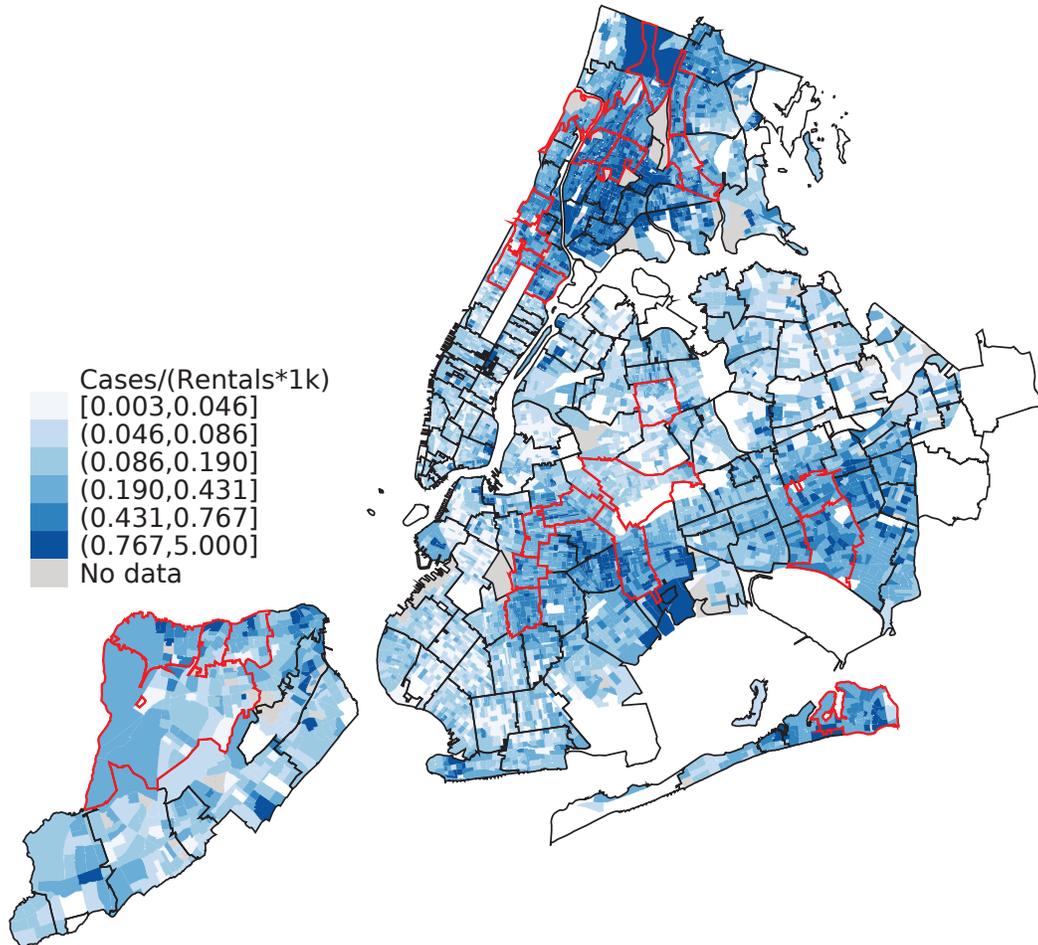


Cases/(Rentals*1k)
[0.003,0.046]
(0.046,0.086]
(0.086,0.190]
(0.190,0.431]
(0.431,0.767]
(0.767,5.000]
No data

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

Figure 3

## UA Households Served by ZCTA

### All ZCTA's, FY2018 and FY2019



# Cases
[0,123]
(123,353]
(353,987]
(987,3792]

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

## Universal Access to Counsel Phased Implementation: Bronx

### Respondent Counsel: Mean by Borough and Zip Code Cohort



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

**Universal Access to Counsel Phased Implementation: Kings (Brooklyn)**

Respondent Counsel: Mean by Borough and Zip Code Cohort

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

Universal Access to Counsel Phased Implementation: New York (Manhattan)

Respondent Counsel: Mean by Borough and Zip Code Cohort

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

## Figure 4D

### Universal Access to Counsel Phased Implementation: Queens
Respondent Counsel: Mean by Borough and Zip Code Cohort



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

## Universal Access to Counsel Phased Implementation: Richmond (Staten Island)

Respondent Counsel: Mean by Borough and Zip Code Cohort



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

Figure 5

Regression Discontinuity Results: Main Outcomes

Running Variable: Months Relative to Empirical UA Start Date
Adjusted for Zip Code Fixed Effects
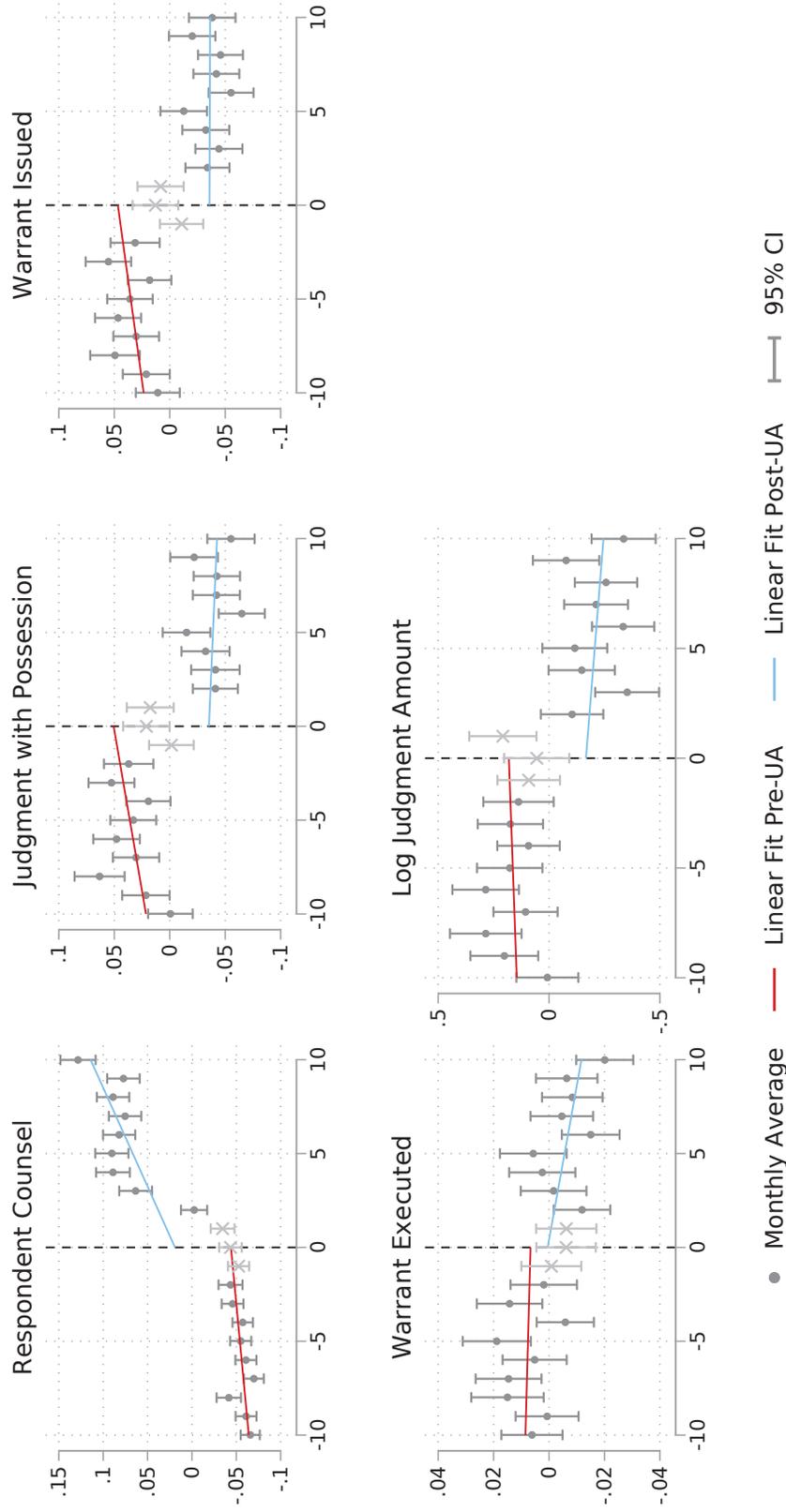
Running variable is months relative to UA empirical start date. Bandwidth months [-10,-2] and [2,10].
Sample consists of subset of main sample cases from first three UA zip code cohorts.
Controls for zip code fixed effects. Excluded donut [-1,-1] plotted for reference with X's.

# A.1 Universal Access Empirical Start Date Instrument Algorithm

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

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

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

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

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

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

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

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

## A.2 Data Appendix

### A.2.1 Covariates Definitions

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

1. **Housing Court**

   - Petitioner counsel: a 0–1 indicator for whether the landlord (petitioner) is represented by a lawyer.

   - Nonpayment: a 0–1 indicator for whether the case is a nonpayment case. Most LT cases are nonpayment cases (the omitted category here); all other cases are classified as "holdover," meaning that the purported violation is for something other than nonpayment of rent (e.g., staying past the end of a lease).

   - Court: a categorical variable describing where the case was filed. There are seven housing court locations in NYC: one of each county (borough)—Bronx, Kings (Brooklyn), New York (Manhattan), Queens, Richmond (Staten Island)—and two specialized courts, in Harlem (New York) and Red Hook (Kings). In several analyses, we simplify the analysis to consider geographical "court boroughs," only, by incorporating Harlem and Red Hook in their respective geographic boroughs.

   - Filing date: semi-annual fixed effects (dummies for each half year, January-June and July-December, from 2016–2019, to capture both calendar year and NYC fiscal year effects; month of-the-year fixed effects (to capture seasonal trends). NYC fiscal years begin in July and end in June and are named for their ending years.

   - Respondent Count == 1: an indicator for whether there is a single (as opposed to multiple) respondents in a case.

   - Petitioner Count == 1: an indicator for whether there is a single (as opposed to multiple) petitioners in a case.

   - NYCHA: a 0–1 indicator for whether the case involves the New York City Housing Authority (public housing).

   - Specialty designation: a 0–1 indicator for the whether the case is flagged by the courts for having an attribute of interest (e.g., co-ops, condos). Excludes those cases flagged for specialty zips (for reasons of collinearity with our instruments).

2. **Census American Community Survey**

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

- A vector of census block group demographic attributes: total population, median household income, household poverty rate, total housing units, renter share of housing units, median gross rent, and population shares that are Hispanic, Black, Asian, White, ages 0–17, ages 65+, and female, as well as census tract shares of non-citizens and naturalized citizens (citizenship data is not available at the block group level). All CBG covariates are transformed into categorical quartiles defined within our sample and appended with a fifth "unknown" category to avoid dropping observations with missing data.

- In several analyses, we also categorize CBG's with a series of indicators describing the block group's majority ($\geq$0.5 share) race is Hispanic, Black, White, or Asian.

- All monetary variables from the ACS are in real 2019 dollars.

3. **PLUTO**

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

- Zoning district: four categories describing the tax lot's primary zoning classification (low-, medium-, and high-density residential; other (e.g., commercial, manufacturing) )

- Land use: five categories describing the tax lot's land use designation and summarizing its building class (1–2 family; multi-family walkup; multi-family elevator; mixed residential-commercial; other (e.g., commercial)).

- Single building: a 0–1 indicator for whether a tax lot contains a single building.

- Residential units: categorical quartiles describing the total number of residential units in a tax lot.

- Year built: three categories describing the year a building completed construction ($\leq$1949; 1950–1989; 1990–2021)

- Building altered: a 0–1 indicator for whether a building was altered in a manner that changed its value after initial construction.

- Lot area: categorical quartiles of the total area of the tax lot, measured in millions of square feet.

- Building-to-lot area ration: categorical quartiles of the total building floor area ratio divided by the tax lot area. Also known as built floor area ratio.

- Property assessed total: categorical quartiles the total assessed value of the tax lot, measured in millions of dollars, as recorded in the Department of Finance's (DOF) FY22 Tentative Assessment Roll.

- Landlord characteristics: categorical quartiles (appended with unknowns) of property owner's number of NYC properties, number of NYC buildings, number of NYC residential units, sum of assessed total value, within-sample housing court cases, and within-sample housing court cases per number of residential units.

## A.2.2 Additional Outcomes

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

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

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

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

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

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

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

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

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

## A.3 Complier Characterization

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

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

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

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

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

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

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

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

For continuous characteristics, we partitioning the covariate into discrete deciles, repeat the above algorithm for each decile, and then take a weighted average. We calculate standard

errors and perform a formal mean comparison using 200 bootstrap replications.

# A.4 References

**Abadie, Alberto.** 2003. "Semiparametric Instrumental Variable Estimation of Treatment Response Models." *Journal of Econometrics*, 113(2): 231–263.

**Angrist, Joshua D., and Jorn-Steffen Pischke.** 2008. *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton University Press.

**Dahl, Gordon B., Andreas Ravndal Kostøl, and Magne Mogstad.** 2014. "Family Welfare Cultures." *The Quarterly Journal of Economics*, 129(4): 1711–1752.

**Dobbie, Will, Jacob Goldin, and Crystal S. Yang.** 2018. "The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges." *American Economic Review*, 108(2): 201–40.

Table A.1: UA Zip Code Cohorts

| Cohort | Zip Codes |
| --- | --- |
| FY16–17 Cohort | 10457, 10467, 11216, 11221, 10026, 10027, 11433, 11434, 10302, 10303 |
| FY18 Cohort | 10468, 10025, 10314, 11225, 11373 |
| FY19 Cohort | 10462, 11226, 10031, 11385, 10310 |
| FY20 Cohort | 10453, 11207, 10029, 10034, 11691 |

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

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

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

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

| | UA Indicator | | UA Intensity | |
| --- | --- | --- | --- | --- |
| | Zip FE (1) | Addr FE (2) | Zip FE (3) | Addr FE (4) |
| Respondent Counsel | 0.079** (0.007) | 0.078** (0.008) | 0.145** (0.040) | 0.146** (0.028) |
| Judgment with Possession | -0.031** (0.005) | -0.040** (0.008) | -0.090** (0.023) | -0.092** (0.026) |
| Log Judgment Amount | -0.177** (0.049) | -0.238** (0.065) | -0.542** (0.099) | -0.523** (0.124) |
| Warrant Issued | -0.030** (0.005) | -0.039** (0.008) | -0.080** (0.024) | -0.091** (0.029) |
| Warrant Executed | -0.005 (0.005) | -0.007 (0.004) | -0.010 (0.014) | -0.006 (0.011) |
| Observations | 727,692 | 456,788 | 403,483 | 202,409 |
| Covariates | Yes | Yes | Yes | Yes |
| Zip FE | Yes | Yes | Yes | Yes |
| Zip × Linear Half-Year FE | Yes | Yes | No | No |
| Address FE | No | Yes | No | Yes |

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

Table A.4A: Complier Characteristics

| | Compliers (1) | Non-Compliers (2) | Difference (3) |
|---|---|---|---|
| Nonpayment | 0.822 (0.000) | 0.878 (0.000) | -0.056** [-12.912] |
| Bronx | 0.332 (0.000) | 0.361 (0.000) | -0.029** [-7.691] |
| Kings | 0.320 (0.000) | 0.265 (0.000) | 0.055** [9.419] |
| New York | 0.141 (0.000) | 0.215 (0.000) | -0.075** [-10.295] |
| Queens | 0.126 (0.000) | 0.147 (0.000) | -0.021 [-1.358] |
| Richmond | 0.032 (0.000) | 0.020 (0.000) | 0.012** [8.589] |
| NYCHA | 0.101 (0.000) | 0.211 (0.000) | -0.110** [-19.933] |
| CBG HH Median Income/1000 | 48.45 (0.17) | 48.03 (0.01) | 0.42 [1.00] |
| CBG Poverty Pct. | 0.282 (0.000) | 0.284 (0.000) | -0.002 [-0.917] |
| CBG Hispanic Pct. | 0.394 (0.000) | 0.420 (0.000) | -0.026** [-9.110] |
| CBG Black Pct. | 0.373 (0.000) | 0.342 (0.000) | 0.032** [8.670] |
| CBG Asian Pct. | 0.069 (0.000) | 0.069 (0.000) | 0.001 [0.365] |
| CBG White Pct. | 0.154 (0.000) | 0.143 (0.000) | 0.011** [3.645] |
| CBG 0-17 Years Pct. | 0.231 (0.000) | 0.230 (0.000) | 0.001 [1.228] |
| CBG 65+ Years Pct. | 0.125 (0.000) | 0.131 (0.000) | -0.006** [-6.897] |
| CBG Female Pct. | 0.538 (0.000) | 0.541 (0.000) | -0.003** [-3.523] |

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

Table A.4B: Complier Characteristics

|  | Compliers (1) | Non-Compliers (2) | Difference (3) |
|---|---|---|---|
| CBG Rental Units Pct. | 0.828 | 0.868 | -0.040** |
|  | (0.000) | (0.000) | [-16.644] |
| CBG Median Gross Rent | 1.32 | 1.20 | 0.12** |
|  | (0.00) | (0.00) | [14.54] |
| CT Naturalized Pct. | 0.176 | 0.190 | -0.014** |
|  | (0.000) | (0.000) | [-10.526] |
| CT Noncitizen Pct. | 0.150 | 0.165 | -0.015** |
|  | (0.000) | (0.000) | [-15.316] |
| Zone Dist.: Res. Low Density | 0.287 | 0.186 | 0.101** |
|  | (0.000) | (0.000) | [14.275] |
| Zone Dist.: Res. Medium Density | 0.625 | 0.637 | -0.011* |
|  | (0.000) | (0.000) | [-2.508] |
| Zone Dist.: Res. High Density | 0.086 | 0.110 | -0.024** |
|  | (0.000) | (0.000) | [-8.664] |
| Land Use: 1-2 Family | 0.096 | 0.044 | 0.052** |
|  | (0.000) | (0.000) | [16.554] |
| Land Use: Multi-Family Walkup | 0.270 | 0.244 | 0.026** |
|  | (0.000) | (0.000) | [6.107] |
| Land Use: Multi-Family Elevator | 0.391 | 0.485 | -0.094** |
|  | (0.000) | (0.000) | [-16.129] |
| Land Use: Mixed Res.-Comm. | 0.199 | 0.219 | -0.020** |
|  | (0.000) | (0.000) | [-4.284] |
| Filed Month | 6.169 | 6.102 | 0.067 |
|  | (0.001) | (0.000) | [1.754] |
| Filed Year | 2017.378 | 2017.197 | 0.181** |
|  | (0.001) | (0.000) | [7.369] |
| Building-to-Lot Area Ratio | 2.92 | 3.43 | -0.52** |
|  | (0.00) | (0.00) | [-23.72] |
| Lot Assessed Value/1000000 | 6.29 | 11.17 | -4.88** |
|  | (0.11) | (0.00) | [-14.26] |
| Landlord Units | 29023.76 | 43507.92 | -1.4e+04** |
|  | (6.1e+05) | (30375.50) | [-18.13] |
| Landlord Cases Per Units | 0.79 | 0.83 | -0.04** |
|  | (0.00) | (0.00) | [-6.71] |

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

Table A.5: Main Results: Respondent Counsel and Housing Court Outcomes, Excluding Queens

| | Zip × Linear Half-Year | | Address FE | |
|---|---|---|---|---|
| | OLS (1) | UA IV (2) | OLS (3) | UA IV (4) |
| Respondent Counsel (First Stage) | | 0.080** (0.007) | | 0.078** (0.008) |
| Judgment with Possession | -0.076** (0.006) | -0.352** (0.058) | -0.055** (0.008) | -0.496** (0.083) |
| Log Judgment Amount | 0.192** (0.039) | -2.043** (0.613) | -0.042 (0.046) | -3.080** (0.786) |
| Warrant Issued | -0.073** (0.005) | -0.351** (0.062) | -0.051** (0.008) | -0.491** (0.083) |
| Warrant Executed | -0.027** (0.002) | -0.055 (0.063) | -0.006* (0.003) | -0.075 (0.052) |
| Observations | 623,050 | 623,050 | 402,075 | 402,075 |
| First-Stage F Stat | . | 125.99 | . | 98.23 |
| Covariates | Yes | Yes | Yes | Yes |
| Zip FE | Yes | Yes | Yes | Yes |
| Zip × Linear Half-Year FE | Yes | Yes | Yes | Yes |
| Address FE | No | No | Yes | Yes |

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

Table A.6: IV Results: Additional Specifications

| | Empirical UA Treatment (Main IV) | | | | | Intensity IV |
|---|---|---|---|---|---|---|
| | All Years | | FY18–19 | | | FY18–19 |
| | No FE (1) | Zip FE (2) | No FE (3) | Zip FE (4) | Addr. FE (5) | No FE (6) |
| Respondent Counsel | 0.139** (0.007) | 0.120** (0.006) | 0.147** (0.009) | 0.108** (0.006) | 0.093** (0.010) | 0.109** (0.010) |
| Judgment with Possession | -0.339** (0.034) | -0.391** (0.038) | -0.349** (0.041) | -0.495** (0.094) | -0.623** (0.100) | -0.351** (0.039) |
| Log Judgment Amount | -1.726** (0.223) | -2.402** (0.298) | -1.797** (0.232) | -2.731** (0.342) | -3.160** (0.818) | -2.165** (0.342) |
| Warrant Issued | -0.337** (0.041) | -0.391** (0.039) | -0.332** (0.048) | -0.507** (0.090) | -0.688** (0.076) | -0.333** (0.052) |
| Warrant Executed | -0.059** (0.014) | -0.076** (0.022) | -0.064** (0.012) | -0.031 (0.059) | 0.011 (0.097) | -0.069** (0.019) |
| Observations | 727,703 | 727,692 | 403,495 | 403,483 | 202,409 | 403,495 |
| First-Stage F-Stat | 401.01 | 370.78 | 262.68 | 308.06 | 82.01 | 131.28 |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes |
| Zip FE | No | Yes | No | Yes | Yes | No |
| Zip × Linear Half-Year FE | No | No | No | No | No | No |
| Address FE | No | No | No | No | Yes | No |

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

Table A.7: Regression Discontinuity Results: Months Since UA Start

|  | (1) | (2) | (3) |
|---|---|---|---|
| Respondent Counsel | 0.066** | 0.063** | 0.058* |
|  | (0.012) | (0.013) | (0.020) |
| Judgment with Possession | -1.295* | -1.285 | -1.161 |
|  | (0.576) | (0.637) | (0.596) |
| Log Judgment Amount | -5.580 | -5.174 | -4.751 |
|  | (3.204) | (3.559) | (3.406) |
| Warrant Issued | -1.250* | -1.222 | -1.103 |
|  | (0.535) | (0.570) | (0.528) |
| Warrant Executed | -0.103 | -0.101 | -0.070 |
|  | (0.100) | (0.110) | (0.106) |
| Observations | 36,856 | 36,855 | 36,855 |
| First-Stage F-Stat | 31.93 | 25.57 | 23.78 |
| Bandwidth | [-10,10] | [-10,10] | [-10,10] |
| Donut | [-1,1] | [-1,1] | [-1,1] |
| Covariates | No | Zip FE | Non-Time |
| Polynomial | Linear | Linear | Linear |
| Diff. Slopes | Yes | Yes | Yes |
| Running Var. | Month | Month | Month |

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

Table A.8: Outcome Means

| | Main Sample | UA Zips | Addr FE Sample | RD Sample | Action Cases | Action 2016, No RC |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Respondent Counsel | 0.090 | 0.132 | 0.404 | 0.163 | 0.125 | 0.000 |
| Empirical UA Treatment (IV) | 0.085 | 0.279 | 0.200 | 0.560 | 0.087 | 0.012 |
| UA Households Served/1000 (IV) | 0.230 | 0.435 | 0.351 | 0.586 | 0.234 | 0.000 |
| Judgment with Possession | 0.415 | 0.400 | 0.393 | 0.407 | 0.576 | 0.623 |
| Log Judgment Amount | 1.728 | 1.693 | 2.027 | 1.780 | 2.397 | 2.610 |
| Warrant Issued | 0.349 | 0.343 | 0.353 | 0.359 | 0.484 | 0.527 |
| Warrant Executed | 0.070 | 0.065 | 0.058 | 0.071 | 0.097 | 0.108 |
| Judgment Vacated (Cond. on Judgment) | 0.126 | 0.148 | 0.258 | 0.166 | 0.126 | 0.103 |
| Warrant Vacated (Cond. on Warrant) | 0.072 | 0.081 | 0.155 | 0.094 | 0.099 | 0.087 |
| Judgment: Stip/Settle | 0.230 | 0.220 | 0.239 | 0.225 | 0.319 | 0.340 |
| Judgment: FTA | 0.179 | 0.175 | 0.144 | 0.177 | 0.249 | 0.277 |
| Judgment: Court Proceeding | 0.006 | 0.005 | 0.009 | 0.004 | 0.008 | 0.006 |
| Days to Judgment Entered | 68.6 | 70.6 | 85.4 | 70.1 | 68.6 | 59.8 |
| Days to Warrant Executed | 195.2 | 199.8 | 215.2 | 199.1 | 195.2 | 194.9 |
| Observations | 727,703 | 220,383 | 56,673 | 85,680 | 524,650 | 142,829 |

Rows index treatment, instruments, and outcomes. Columns define samples of interest. Each cell give the the mean for the row-indexed variable in the column-indexed sample. Column 1 is the main (full) sample. Column 2 is UA treatment (pilot) zip codes only. Column 3 is the address fixed effects sample; specifically, it is the subset of cases contributing to identification of respondent counsel effects in the address FE sample. Column 4 is the regression discontinuity sample. Column 5 is the subsample of main sample cases with activity beyond initial filing. Column 6 further refines Column 6 by including only cases with activity from 2016 among tenants without a lawyer.

## UA Households Share by ZCTA

All ZCTA's, FY2018 and FY2019
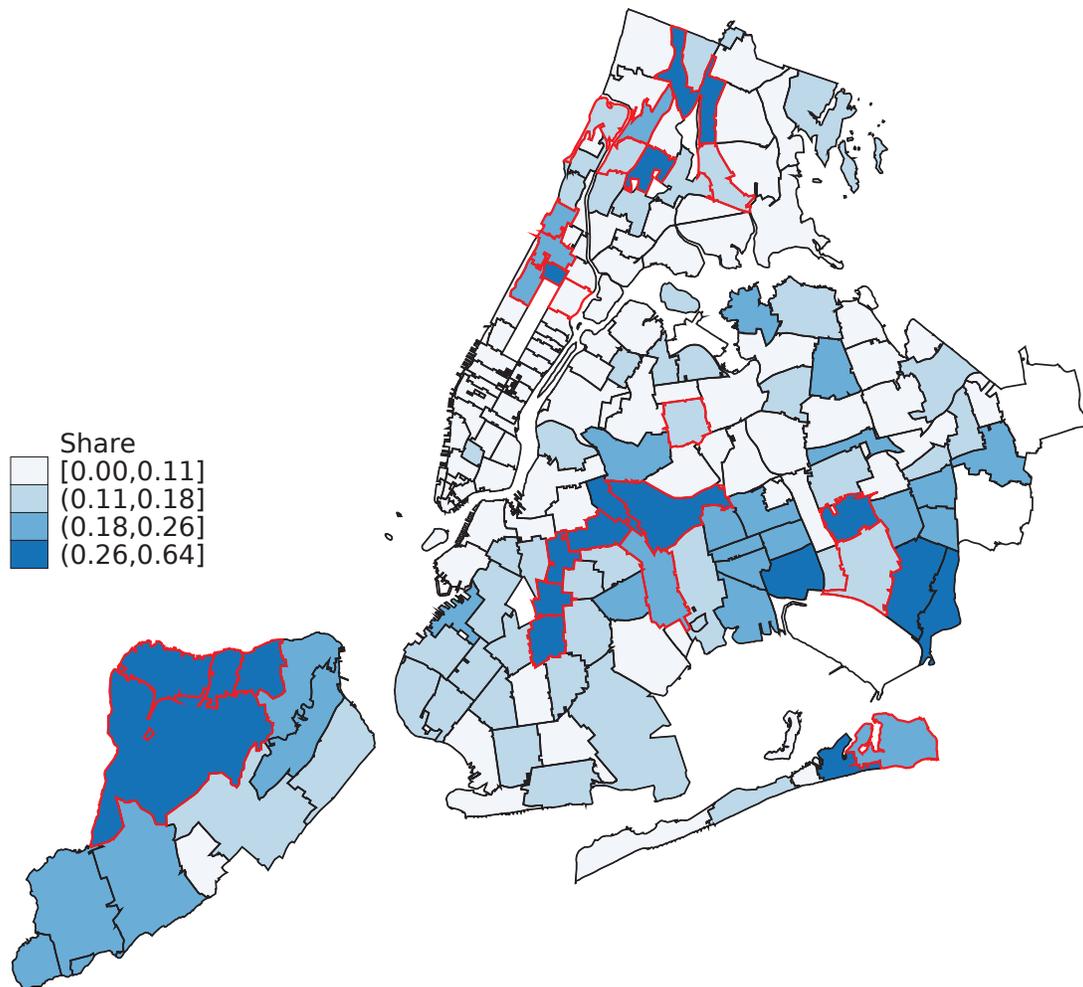


The figure depicits the zip codes comprising NYC's five boroughs.
Black lines delineate zip code tabulation areas. Red lines highlight UA ZCTA's.
Limits of shading bins are 0, 50, 75, 90 100 percentiles of UA household count from NYC DSS annual reports, divided by total housing court filings by

Regression Discontinuity: Density of Running Variable
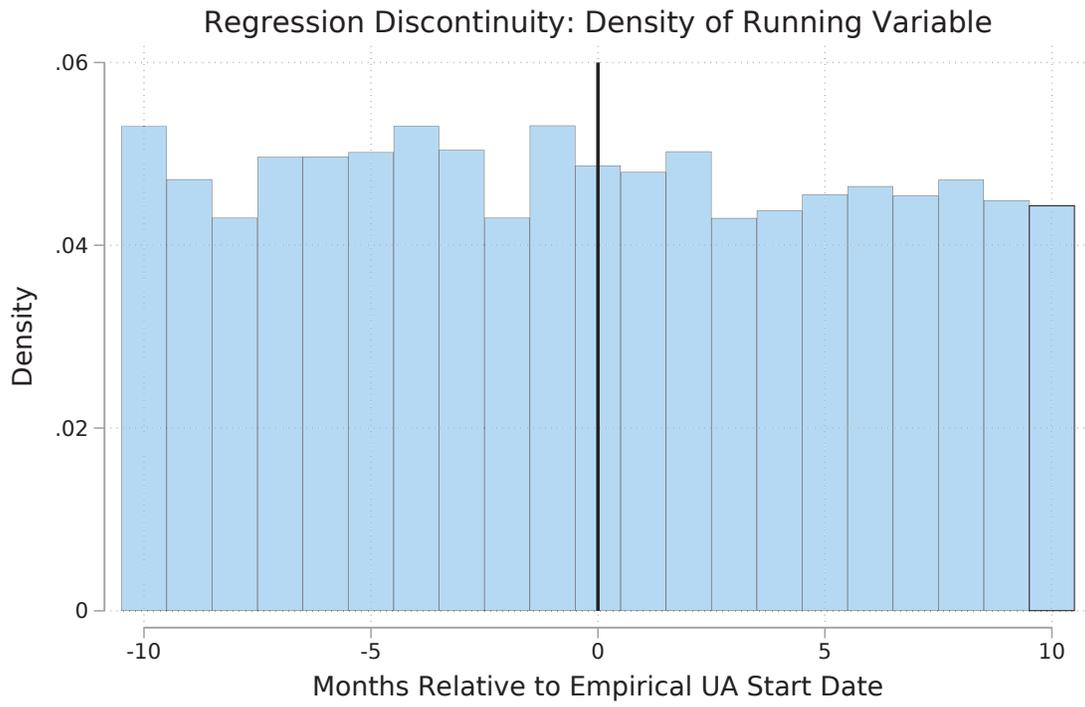
Figure A.3

# Regression Discontinuity: Covariates

## Running Variable: Months Relative to Empirical UA Start Date
## Adjusted for Zip Code Fixed Effects
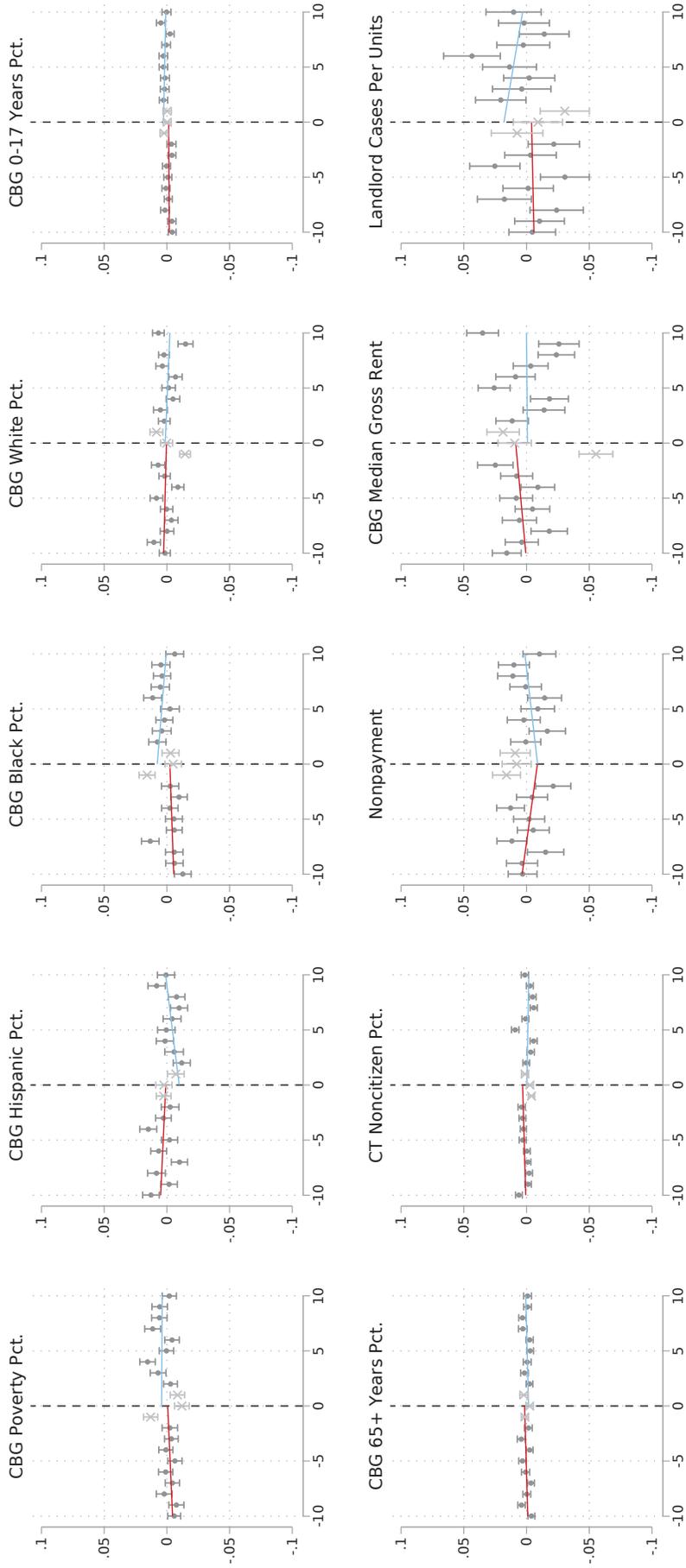


Monthly Average          Linear Fit Pre-UA          Linear Fit Post-UA          95% CI

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