

Eviction and Crime Incident Reports in Small Communities: Quasi-Experimental Evidence from Boston

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Abstract

Existing evidence suggests that eviction negatively affects tenants, but little is known about its impact on the immediately surrounding area. We estimate the effect of eviction on incident reports in the immediate vicinity. We use a difference-in-differences design that exploits variation in case outcomes across observably similar properties. An eviction results in 2 fewer incident reports per month within 250 meters of a property; the effect persists for two years. We find evidence that treatment effects are driven by landlords' tendency to renovate after an eviction. Our results show that eviction impacts neighborhoods as well as individuals.

Keywords: eviction, crime incident reports, housing, difference-in-differences, building permits, cities

JEL: H00, I30, R23, R38

1. Introduction

Three million evictions are filed in the United States each year (Graetz et al., 2023). The risk of eviction is especially high among certain groups: one in five Black renters faces an eviction filing in a given year vs. one in ten white renters (Graetz et al., 2023). In response to its striking prevalence, a growing literature has studied the outcomes of evicted individuals (An et al., 2021; Collinson et al., 2022; Desmond, 2017; Desmond and Kimbro, 2015; Desmond and Shollenberger, 2015).

While most of the existing literature studies eviction's effects on individuals, there is little evidence of the effects of eviction on the immediately surrounding areas. Many people live in the vicinity of eviction without experiencing it firsthand. The impacts of eviction on residents of these communities who are not themselves evicted could be large, particularly in light of strong evidence on the importance of childhood neighborhoods

for future outcomes (Chetty et al., 2016; Chetty and Hendren, 2018a,b; Chyn, 2018). Policymakers have implemented sweeping measures over recent years to prevent eviction (Liptak and Thrush, 2021; Logan, 2021), placing the issue at the center of debates over social policy. But it is impossible to credibly assess the social cost of eviction without understanding its impacts on the immediately surrounding communities in addition to its impacts on evicted tenants.

This paper seeks to expand our knowledge of eviction's social effects on small communities by asking how an eviction impacts the number of incident reports that occur within 250 meters of the property. Incident reports are distinct from crimes—they are about five times more common and range from being relatively minor (e.g., verbal disputes) to relatively severe (e.g., alleged shootings)—but provide a measure of social disruption in a community.

Empirical research on eviction and its impacts faces two main roadblocks outlined by Collinson et al. (2022). The first is the difficulty of conducting analysis at the individual- or property-level. Eviction case records are often scattered across disjoint public and private organizations and difficult to link to individual- and property-level outcomes. The second main roadblock to empirical eviction research is the endogeneity of eviction. For instance, the neighborhoods where evictions are most often filed within Boston tend to be poorer than neighborhoods where evictions are uncommon. We overcome the first roadblock by obtaining incident report-level data from the Boston Police Department (BPD) and eviction case-level data from MassLandlords, a trade association of landlords in Massachusetts. Armed with records of almost all eviction cases filed in Massachusetts between May 2019 and March 2020 and every BPD incident report between August 2015 and July 2023, we spatially join each eviction record concerning a property in Boston with any incident reports which occurred within 250 meters of the property. We produce a panel that allows us to observe incident report counts near each property for several months before and after case resolution.

To overcome the second roadblock, we use a staggered difference-in-differences research design that exploits variation in eviction case outcomes, taking advantage of our granular data. Our empirical strategy will identify the effect of eviction on incident reports under the assumption of parallel trends among treated and control units. Differences

in incident report trends between properties where evictions were successful and properties where evictions were unsuccessful are insignificant during each of the 12 months prior to eviction. We also do not find evidence of pre-treatment level differences in population density, racial composition, or incident reports between neighborhoods where evictions were successful and neighborhoods where evictions were unsuccessful. Most cases are decided as a result of idiosyncratic procedural or legal errors by landlords and tenants; the median time from case filing to case conclusion is just eleven days. The social context we study and the characteristics of evicted tenants, who tend to be poor, housing insecure, and less educated than individuals not targeted by eviction proceedings (Desmond and Gershenson, 2016), make it very unlikely that defendants would act strategically in ways that would create time-varying bias.¹ Our empirical strategy also compares properties where an eviction case concluded with a landlord victory to properties where an eviction case concluded with a tenant victory. The control group we define is thus more similar to our treatment group on observables—and likely on unobservables—than a control group including properties that were never disputed in eviction cases. Together, these facts suggest that the assumption of parallel trends is plausible.

We find that eviction leads to about two fewer incident reports per month in the 250 meters surrounding a property. These estimates are significantly different from zero for two years after case resolution. Our estimated treatment effects are almost entirely driven by impacts on reports of non-violent incidents. We find suggestive evidence that the main mechanism underlying the treatment effects we estimate is the increased tendency of landlords to renovate after a successful eviction: landlords in our dataset are about one and a half to two times as likely to renovate their properties after a successful case, and our treatment effects do not remain significant for the full two years after eviction when significantly renovated properties are removed from the sample. Renovation likely leads to greater informal policing of the property by landlords and construction workers as well as persistent changes in the characteristics of subsequent tenants.

We proceed by interpreting the magnitude of our estimate. As discussed above, incident reports are distinct from crimes. Thus, estimates of the impacts of interventions

¹In Subsections 2.1.2 and 4.2, we further explain the factors that determine case outcomes and argue that they are orthogonal to trends in potential outcomes.

meant to reduce crime, such as police force expansions, are unlikely to be useful benchmarks for our results. Instead, we use a scaling approach to interpret our magnitude. We find that an eviction leads to 7.8 percent fewer incident reports around a property over the following year, relative to the mean number of incident reports that occurred around treated properties in 2017, two years before the first properties in our sample became treated. This magnitude is large but plausible for two reasons. First, we are not measuring violent crime or property crime but all social disruptions leading to incident reports, which are more common (of Investigation, 2199). Second, eviction made large scale renovations more likely, resulting in permanent changes to the treated properties. When we estimate treatment effects on the subsample of properties that did not experience large scale renovations, we find that an eviction leads to a statistically insignificant three percent decrease in incident reports over the following year.

If neighborhood environments changed differentially over time around treatment and control properties, perhaps due to gentrification, our assumption of parallel trends will be violated. Thus, we use as our main specification a *doubly robust* difference-in-differences design (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). This strategy conditions our analysis on 8 pre-treatment socioeconomic and case-related characteristics of eviction cases. We reweight control properties using inverse propensity scores estimated using these 8 pre-treatment characteristics. Even without reweighting, treatment and control cases are balanced on all observable characteristics except for neighborhood income and reason for filing. Reweighting eliminates significant differences in those characteristics as well, so that there are no significant differences between the two groups on observable characteristics. Our main specification produces very similar results to our unconditional specification.

Our main specification rests on the assumption of parallel trends among treated and control units with the same observed characteristics—a more plausible assumption than the unconditional parallel trends assumption required by traditional difference-in-differences designs. Like our unconditional specification, our main specification finds no evidence of statistically significant pre-treatment differences in trends between treatment and control properties. Even if one allows for nonlinear violations of our parallel trends assumption that worsen over time (Rambachan and Roth, 2023), our treatment effect

estimates remain statistically different from zero during months four through eight after treatment.

We provide additional evidence against the existence of time-varying confounders with a placebo test. We use our doubly robust difference-in-differences design to estimate effects on incident reports in the “donut” area between 250 meters and 400 meters from a property. If our results are merely driven by differential changes in neighborhood environments, we should estimate nonzero treatment effects in the “donut” region. The treatment effects we estimate are indistinguishable from zero in the “donut” region, increasing our confidence that the paths of untreated outcomes are the same in both experimental groups. We also find that our estimated treatment effects do not vary when the radius considered around the property is expanded.

Part of the post-treatment period we study overlaps with the COVID-19 pandemic. If behavior changed differently during the pandemic around treatment and control properties with the same observable characteristics, our assumption of parallel trends could be violated. We provide evidence against this threat to identification by re-estimating treatment effects using our panel dataset, restricting to months that predate April 2020, so that all of our estimates predate the pandemic. We obtain results that are less precise, but similar in magnitude to both our unconditional and main specifications.

These results together imply that any violations of parallel trends that generate bias in our estimates (1) must appear in the post-treatment period and not in the pre-treatment period, (2) must not be driven by the pandemic, (3) must not vary when the radius around each property is expanded, (4) must operate within a 250 meter radius of the property and not in the broader neighborhood environment, and (5) must operate within observably similar groups of properties. We view this as implausible.

These findings are important for two main reasons. First, our findings tell a previously unseen story about communities where eviction occurs. Prior research has documented strong associations at the neighborhood level between eviction and measures of social distress, such as poverty. Our findings imply that eviction could improve the quality of the immediate surroundings, insofar as social disruption leading to incident reports declines, although the overall welfare effects of eviction remain unclear. Second, our findings lead to the testable hypothesis that eviction spreads these social disruptions and

reports across locales. Our data do not allow us to directly answer this question. But its answer will have important implications for eviction policy and may be addressed by future work.

We also contribute to a wide literature that studies the associations between eviction and important determinants of social well-being. Evicted mothers are more likely to be depressed; low-income workers are more likely to lose their jobs after being evicted; and at the height of the pandemic, eviction moratoria limited households' food insecurity and mental stress (An et al., 2021; Desmond and Gershenson, 2016; Desmond and Kimbro, 2015). This paper is also related to a burgeoning literature in economics that seeks to apply quasi-experimental methods to study the effects of eviction. In their study of eviction's impacts on individuals, Collinson et al. (2022) exploit random assignment of eviction cases to judges of varying leniency. They estimate the effects of eviction on outcomes such as consumption of durables and homelessness. This approach finds impacts on social and economic outcomes that are smaller than those estimated by the sociology literature, further underscoring the importance of quasi-experimental evidence in this context. This study is one of relatively few to overcome both the data and identification obstacles to eviction research outlined by Collinson et al. (2022).

There are few papers in the economics literature that address the relationship between eviction and socially disruptive activity. Nuisance ordinances—municipal laws that punish landlords for crimes that occur on their properties—have been studied by Kroeger and La Mattina (2020). They find that nuisance ordinances make eviction filings more common across cities in Ohio. Expanding on this finding, Falcone (2022) argues that evictions increase crime at the municipal level under the assumption that nuisance ordinances affect crime only by making evictions more common. Our research distinguishes itself from these studies by estimating the causal effect of an eviction in its immediate surroundings as opposed to in the city or town in which it occurs.

Section 2 discusses the institutional context of the study. Section 3 discusses in greater depth the data we obtain and the dataset we assemble for our analysis. Section 4.1 explores trends in incident report frequency near properties around the filing and conclusion of eviction cases. Section 4 outlines our empirical strategy. Section 5 provides and discusses results, Section 6 discusses the underlying mechanisms, and Section 7 concludes.

2. Institutional Context

2.1. Eviction in Boston

2.1.1. Legal Landscape

Eviction cases—known formally in Massachusetts as summary process cases—fall under the jurisdiction of the Massachusetts Trial Court. Three sub-departments of the Trial Court hear virtually all summary process cases: the District Court, the Boston Municipal Court, and the Housing Court (MassLegalHelp, 2017)². The vast majority of Boston’s summary process cases are adjudicated in the Housing Court, which has held jurisdiction in the city of Boston since it was established in 1971³.

Four features distinguish the Housing Court from other courts (MassLandlords, 2020b). First, it is led by justices with significant knowledge and experience when it comes to housing-related legal matters, such as summary process cases. Second, it is staffed by housing specialists, employees of the Court with detailed knowledge of Massachusetts housing law who provide information and referrals to resources for landlords and tenants. Third, via the housing specialists, the Court offers a service known as *mediation*, in which cases may be resolved prior to arguments in front of a judge⁴. Mediation is facilitated by a housing specialist, who helps the defendant and plaintiff come to a legally binding agreement and records promises made by both sides with the goal of resolving the dispute before the trial date. Rather than risk an adverse outcome at the hands of a judge, many tenants and landlords prefer to reach mutually agreeable terms of resolution during mediation. If either party violates the terms of the specified mediation agreement, the other may return to the judge in a more favorable legal position. Fourth, either party in a summary process case filed in District Court, Boston Municipal Court or Superior Court

²The Superior Court has jurisdiction to hear evictions exceeding some monetary value at time of filing: \$25,000 prior to January 1, 2020; \$50,000 after (General Court of the Commonwealth of Massachusetts (2020)). The distribution of judgments in our data shows that over 95% of evictions concluded with less than \$25,000 owed; a higher percentage necessarily start with less owed at time of filing, rendering them ineligible for Superior Court filing. Even if some few cases were filed in Superior Court, it seems likely renter advocates would intervene and transfer the case to housing court. We therefore omit Superior Court evictions.

³Since the passage of the most recent Housing Court expansion law in 2017, the Housing Court has jurisdiction over the entire state (MassLandlords, 2017). Across the state, 15 judges preside over cases filed in six divisions: Central, Eastern, Metro South, Northeast, Southeast, and Western.

⁴Since the pandemic, tier one mediation has become a mandatory scheduling step (Housing Court, 2023). During the study timeframe, mediation was optional. Jury trials are presided over by a judge and are treated here as adjudicated by a judge.

has the right to transfer the case to the Housing Court at any time prior to trial. Tenant advocacy groups in Massachusetts recommend that all defendants in summary process cases transfer their cases to the Housing Court (Massachusetts Law Reform Institute, 2022).

2.1.2. *The Eviction Process*

A landlord begins the eviction process by serving their tenant with a *notice to quit*, which serves as written notice of the legal termination of the tenancy on a definite future date. A notice to quit may be served for *nonpayment of rent*, *cause*, or *no fault* at the discretion of the landlord⁵. The notice states an amount of time after which the landlord-tenant agreement will be terminated if no action is taken by the tenant. If the notice to quit is served for nonpayment of rent, the tenant may *cure* the nonpayment of rent, nullifying the notice to quit, by paying the landlord all owed rent with interest and costs within the specified time period⁶.

After the length of time specified by the notice to quit has passed⁷, the tenant's rental agreement has ended. To continue with the eviction, the landlord must serve the tenant with a *summary process summons and complaint* and file this complaint with the court (Devanthy and McDonagh, 2017b). At this point, the eviction case has begun; in every eviction case, the defendant is the tenant and the plaintiff is the landlord. If a landlord fails to follow any of the above protocol—say, by serving a notice to quit that specifies too short a time period, or by failing to prove delivery⁸—their case may be *dismissed*, automatically awarding victory to the tenant (Devanthy and McDonagh, 2017c).

Upon receiving the summons and complaint, the tenant may file a Summary Process Answer form with the court. This is the tenant's opportunity to provide *defenses*, or

⁵Massachusetts law does not in general prohibit “no fault” or “no cause stated” evictions (Devanthy and McDonagh, 2017d). No fault evictions may be used when a landlord anticipates being unable to meet court standards for evidence, for instance, in cases where the premises are being used for a crime but witnesses are afraid to testify and there is inadequate other evidence.

⁶Tenants who rent without a lease agreement do not have the option to cure nonpayment of rent if they have received a separate notice to quit for nonpayment of rent during the last 12 months (Devanthy and McDonagh, 2017b).

⁷If the notice to quit was for nonpayment of rent, the specified length of time must pass without curing of the nonpayment by the tenant.

⁸The time period that must be specified by a notice to quit varies based on, among other things, the reason stated in the notice. Proof of delivery is most likely obtained by hiring a civil process server but can be attempted by the landlord; in either case it may be contested.

legal reasons that the landlord should not evict the tenant, and *counterclaims*, or claims that the tenant has against the landlord⁹ (Devanthery and McDonagh, 2017a).

Once the summons and complaint has been filed with the court and the tenant has had a chance to answer, mediation begins (MassLandlords, 2020a). If mediation does not result in an agreement between the landlord and the tenant, a trial is held in front of a judge (MassLandlords, 2020a). If the tenant does not show up for the trial, the case judgment is listed as a *default* in favor of the landlord; if the landlord does not show up for trial or commits certain procedural or legal errors, the case judgment is listed as a dismissal in favor of the tenant (MassLandlords, 2020a). The landlord may also choose to dismiss the case voluntarily at any point after the entry date.

Assuming both parties are present at the trial, two things may happen. If the judge rules in favor of the tenant, the eviction process is over and was unsuccessful. If the judge rules in favor of the landlord, then the tenant has ten days after the judgment to appeal the case. If there is no successful appeal, the landlord may obtain an *execution for possession* from the court (MassLandlords, 2020a). For the next 90 days, the landlord may hire a law enforcement officer to force a tenant to leave the property with 48 hours' notice (MassLandlords, 2020a). Often, tenants leave of their own accord after a ruling in favor of the landlord or after an execution for possession has been granted.

2.2. Police activity in Boston

Incident reports are much less common than crimes. However, these two types of incidents are related. For broad context, crime is less common in Boston than in many other major US cities. In 2019, its crime rate ranked 80th among America's 100 most populous cities (FBI Uniform Crime Reporting Program, 2019), below cities of comparable size such as Las Vegas, Columbus, and Nashville. Within the city of Boston, incident reports are highest in the poorest neighborhoods and in those with the highest shares of Black and Latino residents. In our dataset, three of Boston's poorest neighborhoods—Roxbury, Dorchester, and Mattapan—account for nearly 40 percent of all incident reports reported

⁹The tenant may also file for *discovery* at this stage. Discovery is the process by which the tenant may request information from their landlord, which the landlord must provide under oath. Tenants often use discovery as a means of postponing a trial: as long as the court receives the request for discovery before mediation begins, the eviction process is paused for two weeks (Devanthery and McDonagh, 2017a).

despite accounting for only about 30 percent of the city’s population (Boston Redevelopment Authority, 2014).

3. Data

3.1. *Evictions Data*

We obtain records of summary process cases filed in Boston with conclusion dates between June 2019 and March 2020 from MassCourts.org, a publicly accessible database of civil cases. We developed a system for manually collecting court docket case histories, using date search fields and case docket numbers to systematically download records for the population of eviction cases in Massachusetts. We then programmatically scrape these downloaded files (MassLandlords, 2020c). Crucially, each record includes the resolution of the case, the last date on the case docket, and the address of the disputed property. Each record also includes details such as the duration of the case (from filing date to latest docket date), whether the tenant had an attorney, and the type of notice to quit that was initially filed by the landlord. We use a paid geocoding service known as Geocodio to obtain latitude and longitude coordinates for each property. For 93 percent of the properties in our sample, the geocoded coordinates lie within the associated property tax parcel.

We restrict our sample in several ways. First, we begin with pre-pandemic Boston cases. Second, we drop all cases for which we cannot determine the cause. Third, we drop cases that were filed due to a foreclosure on the disputed property. Fourth, we drop all cases for which a judgment could not be determined from the case docket. Fifth, we drop all cases resolved through mediation. Sixth, we drop all cases where the defendant is a corporation. Seventh, we drop all cases where the defendant has an attorney. Table 1 outlines how each of these sample restrictions alters the number of observations in our analysis sample. We drop mediated cases because there is no way to know from our dataset whether the renter left or stayed. We drop all cases where the defendant was represented by an attorney because such cases are extremely uncommon, and their circumstances may differ from cases where the defendant does not have legal representation in unobservable ways. In Column (2) of Table 1, we show that even after applying our sample restrictions, our sample includes the vast majority of cases that

resulted in forced move-outs during the time period we study. Thus, while our sample restrictions may not allow us to study a representative sample of cases with incomplete records or cases that were resolved outside of the courts (for instance, through mediation, which is discussed further in Section 2), our sample is well-suited to answering the question posed in this paper about the impacts of successful evictions relative to unsuccessful evictions. The properties disputed in eviction cases in our analysis sample are also very similar on observable characteristics to properties disputed in eviction cases that are not in our analysis sample. We discuss this fact further in 3.5.

3.2. Incident Report Data

We obtain records of every incident to which BPD officers responded from August 2015 to January 2023¹⁰. Each record documents the initial details of an incident to which BPD officers respond and includes the date and location (in latitude and longitude coordinates) of the incident. Each incident report record includes an internal BPD offense code that maps to a written description of the incident. Using these written descriptions, we group offense codes into two exhaustive categories for the purpose of constructing more granular outcome variables: reports of non-violent incidents and reports of violent incidents. Tables A1 and A2 list the 15 most common BPD offense codes in each category.

We stress that the existence of an incident report record does not necessarily imply that a crime was committed. Incident reports are hugely more common than crimes. In 2018, BPD officers responded to 87,184 incidents, nearly five times as many as the total number of crimes reported by BPD in that year under the FBI’s Uniform Crime Reporting (UCR) program (of Investigation, 2199). Incident reports do not necessarily lead to convictions, which require lengthy court processes. Many of the incidents to which officers respond are not criminal offenses, but more mild disruptions such as motor vehicle accidents or suspicious behaviors that officers decide to investigate. An incident report record simply indicates the presence of social disruption to which a BPD officer responded. Thus, this paper estimates the causal effect of eviction on incident reports as opposed to convictions or crimes.

¹⁰BPD’s official terminology is “crime incident report.” We use “incident report” throughout to emphasize that these are unlikely to be crimes.

3.3. Permits Data

We use data on the universe of approved building permits from the Boston Inspectional Services Department. This data contains important information about approved construction permits in the city of Boston. In particular, it includes the total declared value of each permit, the beginning and ending date for each permit, and the address of the property.

3.4. Census Tract Characteristics Data

We obtain census tract-level characteristics of the properties in our sample from Opportunity Insights (Chetty and Hendren, 2018a). This data includes characteristics such as population density, median household income, and poverty rate, each measured at a specific point in time several years before the first evictions in our sample were filed. Figure A1 plots the locations of the properties disputed in eviction cases in our sample, shading Boston's census tracts according to their poverty rates. It shows that the density of eviction filings tends to be higher in poor areas. This result is in line with existing research that finds strong associations between poverty and the prevalence of eviction (Desmond and Gershenson, 2016). Indeed, 63.79 percent of eviction cases won by the plaintiff concern properties in census tracts with poverty rates above 20 percent. This descriptive finding mirrors that of Collinson et al. (2022): they show that 58 percent of evictions in New York and 46 percent of evictions in Cook County occur in census tracts with poverty rates above 20 percent. It is also consistent with a body of work in sociology that finds that eviction is most common in the poorest communities (Desmond, 2017; Desmond and Gershenson, 2016; Desmond and Kimbro, 2015).

3.5. Merged Dataset

To produce the sample used in this analysis, we match each property with all incident reports that occurred within a 250 meter radius—about a three minute walk. This radius is small enough to encompass only the immediate vicinity of a property and large enough to account for noise in the geocoded coordinates of our incident report records. Then, for each property, we count the number of incident reports that occurred within its radius during each month. We are left with a panel dataset of nearby incident report counts at the property-month level.

We first discuss the characteristics of properties in our analysis sample. Column (1), (2), and (3) of Table 2 contain descriptive statistics for the panel dataset. Panel A notes that in the year 2017, there were 350 incident reports within the radius of the average property. Incident report levels during months 12 and six before case conclusion are about 28 and 26 respectively. Panel A also gives pre-treatment levels of reports of violent incidents and reports of non-violent incidents. Across the two categories, incident report counts are relatively similar 12 months and six months before case filing.

Panel B describes socioeconomic characteristics of the census tracts in which evictions occur. On average, properties disputed in our data are located in census tracts that are significantly more diverse, poorer, and denser than Boston as a whole. In the census tract of the average eviction case in our sample, median household income is about \$47,000, compared with \$81,744 in Boston as a whole; population density is about 23,000 people per square mile, compared with 13,977 people per square mile in the city of Boston (Census Bureau, 2020); and 32 percent of people are white, compared with 48.6 percent in Boston as a whole.

Panel C outlines the reasons that evictions in our sample are filed and describes plaintiffs and tenants. About 83 percent of evictions were filed because a tenant did not pay rent. This statistic is consistent with those reported by Collinson et al. (2022), who find that 86 percent of cases in their New York City sample are filed for nonpayment of rent.

Lastly, Panel D describes characteristics of case resolution in our sample. Recall that in our sample, we did not consider cases that were mediated. In 51 percent of the remaining cases, the plaintiff won by default; in 46 percent, the tenant won as a result of a case dismissal. In only about 3 percent of cases did a judge actually hear arguments and decide the case outcome. A more detailed description of the process by which eviction case outcomes are determined can be found in Subsection 2.1.2. The median time between the date a case is filed and its final docket date was 11 days. Panel D also notes that in the average case, there was a money judgment of approximately \$1,500 awarded to the plaintiff. The distribution of this variable is right-skewed. In about half of the cases in our sample, there is no money judgment awarded; in 60 percent of cases, the money judgment is less than \$530 .

Columns (4), (5), and (6) of Table 2 contain descriptive statistics for the full sample of properties. Properties in our analysis sample are extremely similar on observable characteristics to properties in the full sample. In the first row of Columns (1) and (4), we report the total number of incident reports within 250 meters of the average property in 2017 in our analysis sample and the full sample. The gap in incident reports between analysis sample properties and full sample properties is less than one percent of the gap in incident reports between properties located in census tracts with below median and above median poverty rates. Panel B also shows that properties in our analysis sample are located in census tracts that have similar median household incomes, population densities, poverty rates, and shares of white residents. Figure A1 also shows that the spatial distribution of properties is also similar across the two samples; properties in the two groups do not appear to be clustered in distinct areas of Boston.

Table A3 summarizes variation in case filing dates and case outcomes throughout our sample. There are 10 unique case filing months in the sample of cases we consider¹¹.

4. Empirical Strategy

4.1. Trends Around Case Filing

Figure 1 plots trends in incident reports near properties disputed in eviction cases to assess the feasibility of a difference-in-differences analysis. The plot presented at the top of Figure 1 shows that levels of and trends in incident reports prior to the month of case filing appear relatively similar across properties where the eviction case concluded with a landlord victory and properties where the eviction case concluded with a tenant victory. In Figure A3, we test for differences in incident report levels between these two groups at each relative time period prior to case filing and case conclusion. The top graph in Figure A3 reports point estimates of and 95 percent confidence intervals for the difference in incident report levels between the two groups of properties at each filing month-relative time period.

¹¹Due to limitations in our data gathering budget and the fact that this analysis is limited to eviction cases that were filed and concluded prior to the pandemic, we are only able to consider 10 case filing months.

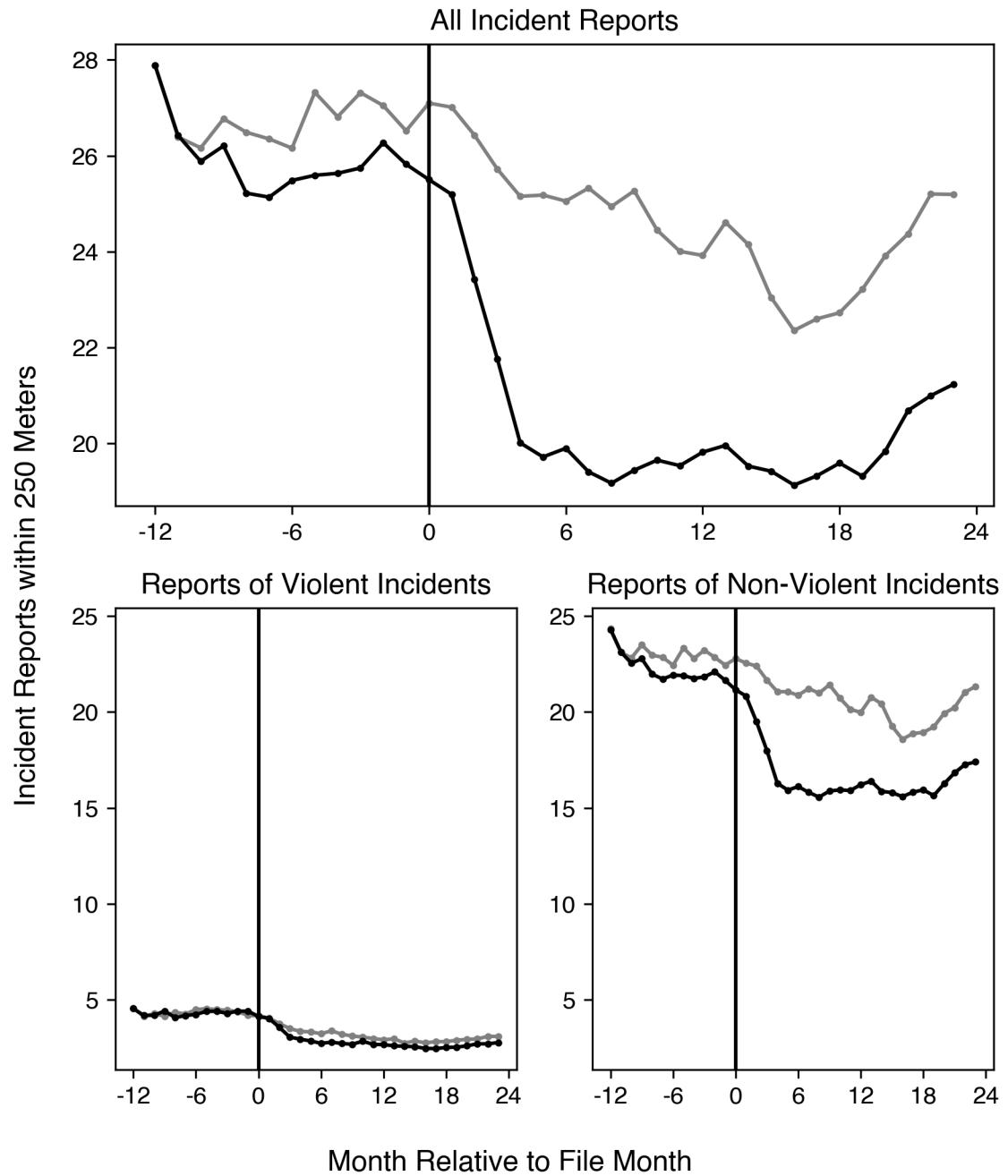


Figure 1: Incident Reports Around Case Filing

Notes: This figure plots the mean number of incident reports that occurred within 250 meters of properties disputed in cases won by the landlord and properties disputed in cases won by the tenant during each month relative to case filing. The mean number of incident reports that occurred within 250 meters of each property is on the y-axis, and the month relative to case filing is on the x-axis.

The second graph in Figure A3 repeats this exercise, estimating and testing differences in incident report levels between the two groups at time relative to case conclusion rather than filing. At no period prior to case conclusion or case filing are incident report levels in the two groups statistically different from each other.

Taken together, Figure 1 and Figure A3 provide strong evidence that the dynamics of incident reports surrounding properties in the two groups are similar¹². They also suggest that local individuals cannot anticipate case outcomes in ways that are correlated with criminal behavior. To the extent that observed incident report levels are informative about unobserved characteristics of properties, they also suggest limited scope for unobservable differences between the two groups.

The top plot of Figure 1 also shows a large divergence in incident report levels immediately after case conclusion. After this point, incident reports fall sharply around the properties disputed in cases won by the plaintiff and gradually around the properties disputed in cases won by the defendant. The relative sizes of these drops are striking. In the first group, incident reports fall 14.66 percent during the first three months after case conclusion; in the second, they fall by 6.29 percent. For each of the following 21 months, incident reports remain at least 12.19 percent fewer in the first group than the latter.

We next discuss levels of and trends in reports of violent and non-violent incidents, shown in the remaining plots of Figure 1. In each plot, levels and trends are very similar between both groups of properties in the pre-treatment periods. The number of reports of violent incidents drops slightly more around properties disputed in eviction cases won by the landlord than properties disputed in eviction cases won by the tenant. The number of reports of non-violent incidents remains relatively steady before and after case filing around properties where the tenant won the eviction case; around properties where the landlord won the eviction case, there is a striking decrease in the number of reports of non-violent incidents almost immediately after case filing.

In the analysis that follows, we focus on all incident reports as our main outcome. The appearance of parallel trends between our two groups of properties makes a difference-in-differences design a natural choice for identifying the causal effect of eviction on the frequency of incident reports. The fact that we are unable to reject the presence of level differences in incident report counts between the two groups during any of the pre-case filing or pre-case conclusion time periods also suggests limited scope for potential time-

¹²We also plot incident report trends around case conclusion in Figure A2. Trends are nearly identical to those displayed in Figure 1. This is unsurprising given that half of eviction cases in our sample last 11 days or less.

varying confounders. We investigate the validity of parallel trends more thoroughly in Subsection 5.2.

4.2. Conceptualizing the Experiment

Our estimand of interest is the causal effect of eviction on the number of incident reports that occur in the immediate vicinity of the property. We view eviction-driven changes in a property’s characteristics or surroundings as part of the treatment we study. In an ideal experimental design, a randomly chosen subset of properties in our sample would be disputed in eviction cases won by the plaintiff, and the remaining properties would be disputed in eviction cases won by the defendant. Such random assignment of eviction is impossible because non-random judicial processes decide case outcomes. In this subsection, we define our treatment group and control group and show that, given what we observe, our definitions approximate the experimental ideal.

We define a treated property to be any property disputed in an eviction case decided by a judge in favor of the plaintiff; this includes cases decided by default in favor of the plaintiff. Each property in the treatment group becomes treated during the month of case filing. We define a control property to be any property disputed in an eviction case decided by a judge in favor of the defendant; this includes cases decided by dismissal. The key difference between treated properties and control properties is that landlords are able to remove tenants from treated properties, as we describe in Subsection 2.1.2. We note that while we observe case outcomes, court records do not indicate whether tenants physically depart from properties after losing eviction cases. However, a decade of anecdotally gathered experience prior to this work supports the inference that in virtually all cases won by the plaintiff, the defendant ultimately leaves or is removed from the property.

It is also important to note that properties in both the treatment and control groups are disputed in eviction cases. Properties disputed in an eviction case won by the plaintiff are more similar on observable characteristics to properties disputed in an eviction case won by the defendant than to properties that are not disputed in any eviction case (Robinson and Steil, 2020). Our sample supports this claim. In Subsection 3.5, we note large differences between the characteristics of properties in our sample and the characteristics of the city of Boston; in Column (2) of Table 3, we show that differences in these

characteristics are much smaller between the treatment and control groups in our study. Differences in unobservable characteristics are also likely to be smaller between treatment and control properties than between properties disputed in eviction cases and properties in Boston as a whole. The control group we define is thus a better counterfactual for the treatment group than a control group including properties that are not disputed in eviction filings. This should reduce the potential for bias in our estimates.

4.3. Setup

We denote a particular time period by t where t indexes months. For a particular unit i , let $D_{i,t}$ equal 1 if unit i is treated during month t and zero otherwise. Denote property i 's case filing month as $G_i = g$. Let $C_i = 1$ if property i is a control property and 0 otherwise. If $C_i = 1$ and $G_i = g$, property i is in the control group and an eviction case disputing property i was filed during month g . If $C_i = 0$ and $G_i = g$, then property i is a treated property and an eviction case disputing property i was filed during month g . Define $D_i(g) = 1$ if $C_i = 0$ and $G_i = g$. Let $\Delta Y_{i,g-1,t}$ equal the change in property i 's incident report counts between months t and $g - 1$. Let $Y_{i,t}(0)$ denote the untreated potential outcome for property i at time t .

4.4. Unconditional Estimates of the ATT

The following is an unconditional estimator for $ATT(g, t)$, the average treatment effect during month t for a treated property disputed in an eviction case filed during month g .

$$\hat{ATT}_{unc}(g, t) = \frac{\sum_i \Delta Y_{i,g-1,t} \mathbf{1}\{G_i = g, C_i = 0\}}{\sum_i \mathbf{1}\{G_i = g, C_i = 0\}} - \frac{\sum_i \Delta Y_{i,g-1,t} \mathbf{1}\{G_i = g, C_i = 1\}}{\sum_i \mathbf{1}\{G_i = g, C_i = 1\}} \quad (1)$$

This unconditional estimator is equal to the gap in incident report counts between the treatment and control groups during month t minus the gap in incident report counts between the treatment and control groups during month $g - 1$. It is the canonical two unit, two period difference-in-differences estimate of the treatment effect of eviction during month t for properties disputed in eviction cases filed during month g . This unconditional estimator simply formalizes the event study estimates presented in Section 4.1. It allows us to easily aggregate and report the unconditional difference-in-differences estimates of

the treatment effect of eviction presented visually in Figure 1. The above estimator will identify $ATT(g, t)$ under the following assumption.

Assumption 1 (Parallel Trends Assumption). *For all g, t , with $t \geq g$,*

$$E[Y_t(0) - Y_{t-1}(0)|G = g, C = 0] = E[Y_t(0) - Y_{t-1}(0)|C = 1].^{13}$$

Assumption 1 states that in the absence of any treatment, for each case filing month g , the path of outcomes for treated properties disputed in eviction cases filed during g would have been parallel to the path of outcomes for control properties.

4.5. Doubly Robust Estimates of the ATT

The above unconditional parallel trends assumption will not hold if eviction case outcomes and trends in incident report counts are related to their socioeconomic surroundings. Our second estimator uses covariates to construct a counterfactual for the observed path of outcomes in the treatment group using the doubly robust difference-in-differences estimator proposed by Sant'Anna and Zhao (2020).

First, we collect all 8 pre-treatment covariates from Table 3 in a vector X_i . We next estimate $\hat{p}_g(X_i)$, a logit regression propensity score model for the probability of being in cohort g . We calculate a weight $\hat{w}_i(X_i) = \frac{\hat{p}_g(X_i)}{1 - \hat{p}_g(X_i)}$ for each property. We define normalized weights $\hat{w}_i^*(X_i) = \frac{\hat{w}_i(X_i)}{\sum_i \hat{w}_i(X_i) C_i}$ that sum to one across control properties.

Next, using only control properties, we regress $\Delta Y_{i,g-1,t}$ on X_i . Using the estimated coefficients $\hat{\beta}_{g-1,t}^X$, we define $\Delta \hat{\mu}_{g-1,t}(X_i) = \hat{\beta}_{g-1,t}^X X_i$. This means that $\Delta \hat{\mu}_{g-1,t}(X_i)$ is the predicted counterfactual change in property i 's incident report counts between months t and $g - 1$.

The doubly robust estimator for $ATT(g, t)$ is as follows.

$$\hat{ATT}_{DR,X}(g, t) = \frac{1}{N} \sum_i \left[\left(\frac{D_i(g)}{\bar{D}_i(g)} - \frac{\hat{w}_i^*(X_i) C_i}{\bar{C}_i} \right) (\Delta Y_{i,g-1,t} - \Delta \hat{\mu}_{g-1,t}(X_i)) \right] \quad (2)$$

Note that $\bar{D}_i(g)$ and \bar{C}_i are sample averages.

The estimator in Equation (3) conditions the event study evidence presented visually in Section 4.1 on observable characteristics. It estimates a two unit, two period difference-

¹³A “no anticipation” assumption is implicit in the notation we use. See Callaway and Sant'Anna (2021) for more information.

in-differences estimate of the treatment effect of eviction that reweights the control group's differences using a propensity score model ($\hat{w}_i(X_i)$) and estimates the counterfactual change in outcomes for treated properties using an outcome regression model ($\hat{\mu}_{g-1,t}(X_i)$). As long as one of these two models is correctly specified, the doubly robust estimator will identify $ATT(g, t)$ under the following assumption:

Assumption 2 (*Parallel Trends Assumption*). *For all g, t , with $t \geq g$,*

$$E[Y_t(0) - Y_{t-1}(0)|G = g, C = 0, X] = E[Y_t(0) - Y_{t-1}(0)|C = 1, X].$$

This assumption requires parallel trends only among units with the same covariates, unlike the unconditional parallel trends assumption required by the estimator defined in Equation (2).

The estimator in Equation (3) produces estimates of $ATT(g, t)$ for each cohort g at each time period t . As such, following Callaway and Sant'Anna (2021), we aggregate our estimates of these $ATT(g, t)$ parameters in two ways. First, we aggregate them according to time since treatment, weighting by the size of the treated cohorts. For each $\hat{ATT}_{DR,X}(g, t)$, time since treatment e is equal to $t - g$. We produce 95 percent confidence intervals around these estimates $\hat{ATT}_{DR,X}(e)$ following the bootstrap procedure described in Callaway and Sant'Anna (2021). Following a similar procedure, we also aggregate all estimates $\hat{ATT}_{DR,X}(g, t)$ with $t > g$. These are post-treatment estimates of the effect of eviction.

5. Results and Discussion

5.1. Doubly Robust Estimates of the ATT

Figure 2 presents doubly robust estimates of event-time aggregated treatment effects. Effects are reported on the y-axis, month relative to treatment is reported on the x-axis, and dotted lines represent 95 percent confidence intervals. Our identifying assumption of parallel trends between groups with the same pre-treatment characteristics appears satisfied; treatment effects at event times -12 through -1 are extremely close to and statistically indistinguishable from 0. We further support the validity of this assumption in Section 5.2.

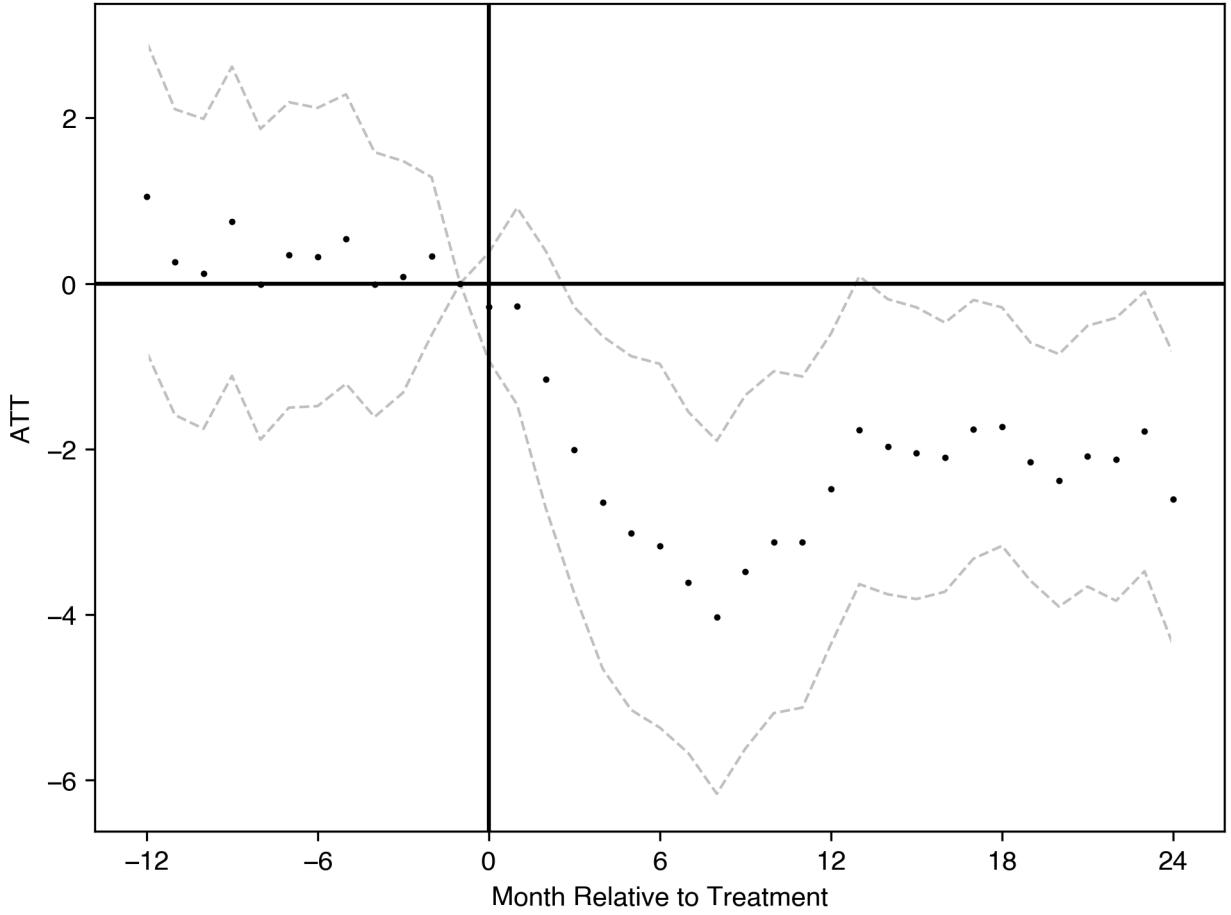


Figure 2: Treatment Effects of Eviction on Incident Reports

Notes: This figure plots doubly robust estimates of treatment effects aggregated by event-time, estimated on the entire sample. Treatment effects on incident report counts are on the y-axis. Treatment-relative month is reported on the x-axis. Black dots represent point estimates and grey dotted lines represent 95 percent confidence intervals. Treatment effects are estimated and aggregated following Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021).

In the post-treatment period, the estimated magnitudes of our treatment effects increase steadily from month one to month three. Estimates become negative and significantly different from zero during month three after treatment, and remain so for the next 21 months. Treatment effects become larger in magnitude until month eight, at which point they decrease in magnitude until month 13. They then remain relatively steady for the remainder of the two years immediately after treatment.

Our point estimate of the average monthly post-treatment effect of eviction is -2.28 incident reports with a standard error of about 0.82. The point estimate is 0.65 percent of the total number of incident reports that occurred within 250 meters of the mean property in 2017. For the purposes of comparison, we also estimate unconditional event-

study treatment effects of eviction in Figure A4.

We obtain -2.46, with a standard error of 0.84, as our estimate of the unconditional average monthly post-treatment effect of eviction; treatment effect dynamics and magnitudes are similar to those presented in Figure 2. The fact that our unconditional and doubly robust specifications produce such similar results suggests limited scope for time varying confounders. In Figure A5, we also estimate doubly robust treatment effects of eviction using the latest docket month instead of the filing month as the treatment date; treatment effect dynamics and magnitudes are similar to Figure 2.

To understand the drivers of these results, we estimate doubly robust event-study treatment effects on reports of violent and non-violent incidents in Figure 3. We include the same controls used in Figure 2, replacing pre-treatment counts of all incident reports with pre-treatment counts of reports of violent incidents (in the plot on the left) and reports of non-violent incidents (in the plot on the right). Both of the plots in Figure 3 show insignificant pre-trends. While treatment effects in the post-treatment period are significantly different from zero in both plots, treatment effects on reports of non-violent incidents are far larger than treatment effects on reports of violent incidents.

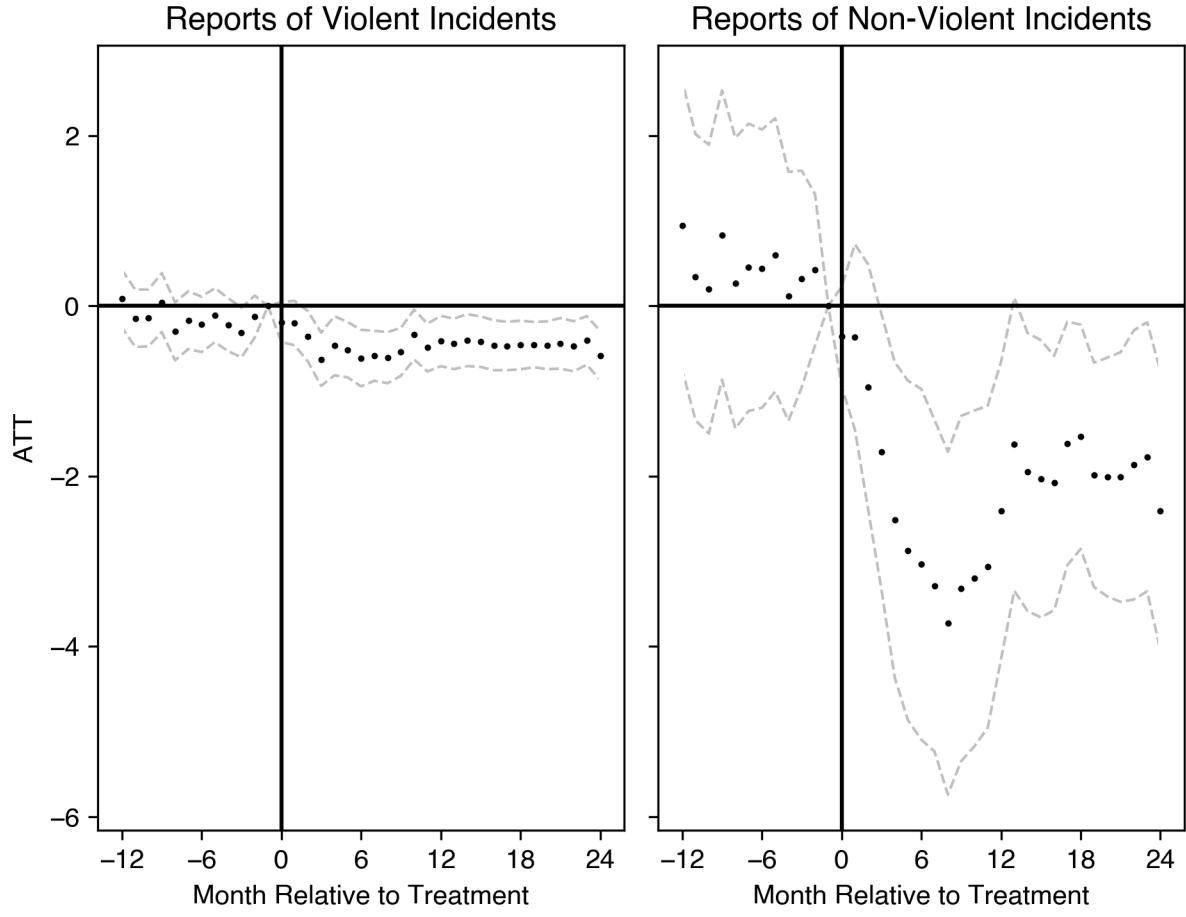


Figure 3: Treatment Effects of Eviction on Reports of Violent and Non-Violent Incidents

Notes: This figure plots doubly robust estimates of treatment effects aggregated by event-time, estimated on the entire sample, using two separate outcome variables. The plot on the left shows estimated treatment effects on reports of violent incidents; the plot on the right shows estimated treatment effects on reports of non-violent incidents. Treatment effects on incident reports are on the y-axis. Treatment-relative month is reported on the x-axis. Black dots represent point estimates and grey dotted lines represent 95 percent confidence intervals. Treatment effects are estimated and aggregated following Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021).

Lastly, we assess whether there are heterogeneous treatment effects across subgroups of properties in our sample. For each subsample we examine, we produce unconditional estimates of the effect of eviction on incident reports. We take the additional step of aggregating the estimated post-treatment effects for easy comparison of treatment effects across subsamples. Figure 4 reports the results of this process. It plots the point estimate of the average post-treatment ATT, along with a 95 percent confidence interval, for each of the eight subsamples described on the y-axis. Treatment effects are largest and most significant in subsamples of properties located in less socially distressed areas. In

particular, they are largest in areas with the lowest poverty rates, the highest median incomes, and the highest shares of white residents. One explanation for this result is that the social disruptions removed by eviction are relatively more noticeable in less distressed areas.

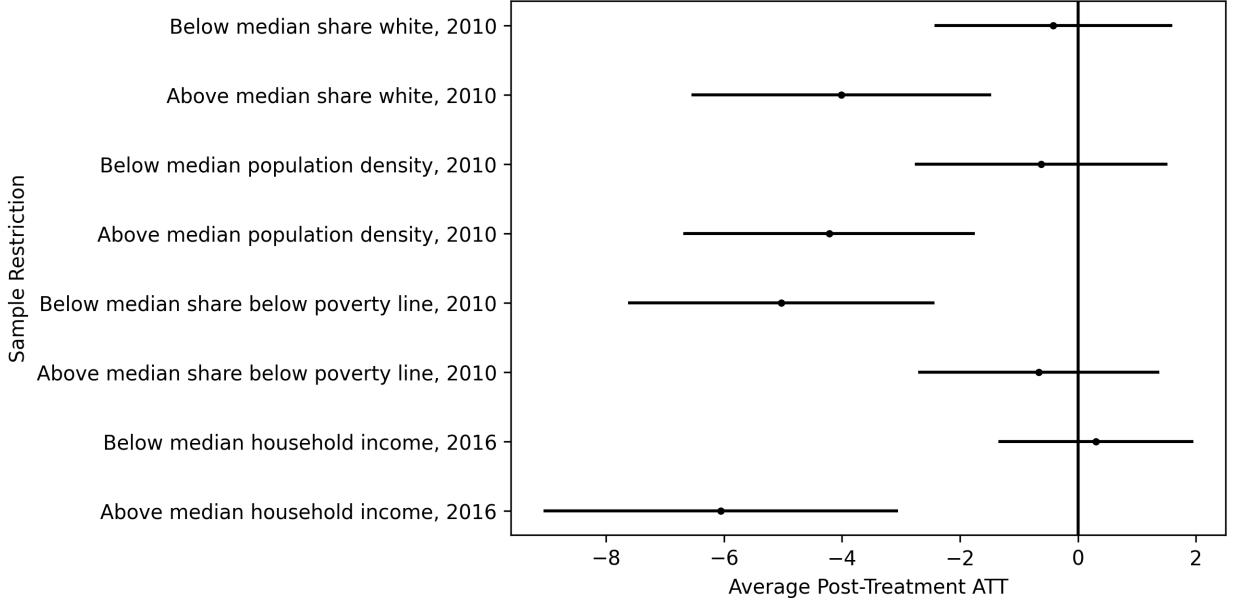


Figure 4: Heterogeneous Treatment Effects

Notes: This figure displays aggregated doubly robust post-treatment estimates of the effect of eviction across different subsets of our data. The y-axis indicates the different splits on which we produce estimates. Treatment effects on incident reports are on the x-axis. Each black dot represents a point estimate of the post-treatment effects of eviction on a different sample; horizontal black lines indicate 95 percent confidence intervals. Treatment effects are estimated and aggregated following Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021).

5.2. Validating the Design

Our estimates may be biased if the socioeconomic characteristics of properties' surroundings change differently after eviction around treated properties than control properties with the same covariates. Qualitative evidence suggests that such changes in socioeconomic characteristics are unlikely for two reasons. First, significant change in the socioeconomic character of neighborhoods often takes many years — far longer than the two years post-treatment during which we present estimates of treatment effects. Second, as discussed in Section 3.5, differences in the socioeconomic characteristics of properties' surroundings are larger between our sample and the city of Boston as a whole than they are between our treatment group and our control group. These differences be-

come even smaller after reweighting during doubly robust estimation. It is unlikely that treatment and control properties that are observably similar on socioeconomic characteristics proceed to exhibit drastically different trends in these socioeconomic characteristics immediately after eviction, particularly within such a short time frame. In the remainder of Subsection 5.2, we provide empirical evidence against the presence of time-varying confounders that may bias our estimates.

5.2.1. *Treatment and Control Properties are Similar*

Underlying our design is the assumption of parallel trends in the untreated potential outcomes of the treatment and control units with the same values of X_i . There are three reasons to believe that this assumption holds.

First, the treatment and control groups are composed of relatively similar kinds of properties. We explore pre-treatment differences in the observable characteristics of these two groups in Table 3. Each cell in Column (2) reports the coefficient from a univariate regression of one covariate on a treatment indicator. Column (3) reports the p-values associated with each of these coefficients. Panel A of Table 3 shows that we cannot reject the null hypotheses of equivalent incident report counts in 2017, equivalent incident report counts in the twelfth month prior to case filing, or equivalent incident report counts in the sixth month prior to case filings. Treatment and control properties are also located in census tracts with similar shares of white individuals and similar population densities.

Second, we can eliminate imbalance between the treatment and control groups using reweighting. Column (3) of Table 3 shows statistically significant pre-treatment imbalance in census tract median household income, census tract poverty rate, and the share of cases filed for nonpayment between the treatment and control groups. In each row of column (4), we regress one covariate on an indicator for plaintiff victory and $\hat{w}_i(X_i)$, the previously estimated propensity score (Austin, 2011), and report the coefficient on the indicator for plaintiff victory. Each cell of column (5) reports the p-values from a hypothesis test that the coefficient on a single pre-treatment characteristic is equal to 0. Column (5) shows that conditioning on covariates makes all pre-treatment differences between the treatment and control groups insignificant.

Third, we do not find evidence of the presence of differential trends in the observed

outcomes of the treatment and control groups in the pre-treatment period. Figure 2 shows that the 95 percent confidence intervals around each of our pre-treatment event study estimates include zero. Of course, while pre-treatment trends may be informative about the counterfactual post-treatment difference in trends, recent econometric advances have highlighted a key limitation of the sort of pre-testing performed in Figure 2: these tests are likely to suffer from low power (Roth, 2022). As another way of validating our results, we show that we can rule out a null effect of our treatment even if we allow specific forms of nonlinear parallel trends violations. We fit a line through our pre-treatment event study estimates and obtain a slope of -0.05. We then suppose that any parallel trends violations may get worse by up to 0.01 units each month (one fifth of the slope of the line fitted through our pre-treatment event study estimates). This hypothetical bias is nonlinear and increasing with time. We next plot confidence intervals for the identified treatment effects in Figure A6 (Rambachan and Roth, 2023). We can still reject null treatment effects of eviction on incident reports from months four through nine after case filing.

5.2.2. *Placebo Tests*

We perform two placebo tests to provide evidence against the existence of time-varying confounders. We first use our doubly robust difference-in-differences design to estimate effects on incident reports in the area within a wider radius of 300 meters around each property. This area is approximately 50 percent larger than the area within the 250 meter radius considered in our main analysis. If our results are driven by broader neighborhood trends, expanding the radius around each property should result in larger estimated treatment effects.

Second, we estimate effects on incident reports in “donut” regions between 250 meters and 300 meters, between 250 meters and 350 meters, and between 250 meters and 400 meters from a property. Eviction is unlikely to affect the frequency of incident reports at large distance from the disputed property. Given the size of typical urban neighborhoods, changes in a property’s larger-scale neighborhood environment are extremely likely to influence the frequency of incident reports both in the immediate area around a property and in the “donut” area. If our results are merely driven by differential changes in

neighborhood environments, we should estimate nonzero treatment effects outside the immediate radius of a property but within its neighborhood.

The validity of the above exercises as placebo tests relies on the assumption that the treatment status of a property does not affect incident reports in the “donut” area that surrounds it. This assumption will be violated if individuals tend to move into the “donut” area surrounding their homes after being evicted. To understand why this assumption is necessary for the validity of our placebo tests, consider an individual who is evicted from a property and then moves into a unit in the “donut” area surrounding the property. It would be impossible to disentangle their impact on incident reports in the “donut” area from the impact of time-varying confounders that might bias our estimates. We argue that it is implausible that individuals tend to move into the “donut” area surrounding a property after being evicted. The longest radius we consider in our placebo tests is 400 meters, about a five minute walk from the property and well within the much wider area where travel is feasible on foot. Desmond and Shollenberger (2015) find that “renters who experienced a forced move relocate to poorer and higher-crime neighborhoods than those who move under less-demanding circumstances,” suggesting that evicted individuals move to different neighborhoods altogether.

Table 4, Figure 5, and Figure A7 report the results of our placebo tests. In the first and second rows of Table 4, we estimate doubly robust treatment effects of eviction on incident reports within 250 meters and 300 meters of a property and then aggregate them across the post-treatment periods. The aggregated treatment effects are statistically indistinguishable from each other. In Figure 5, we increase the area around each property by approximately 50%. The stability of our treatment effects when the area around each property is expanded suggests that our treatment effects are driven by factors that are highly local to the property as opposed to those related to broader neighborhood trends. In Figure A7, we more gradually increment the radius around each property. Within each radius, we estimate the average post-treatment effect of eviction using our unconditional specification. There is no discernible trend in the estimated treatment effects as the radius is expanded. In the third through fifth rows of Table 4, we estimate “donut” treatment effects on incident reports as described above, aggregating treatment effects across all post-treatment time periods. These treatment effects are indistinguishable from zero.

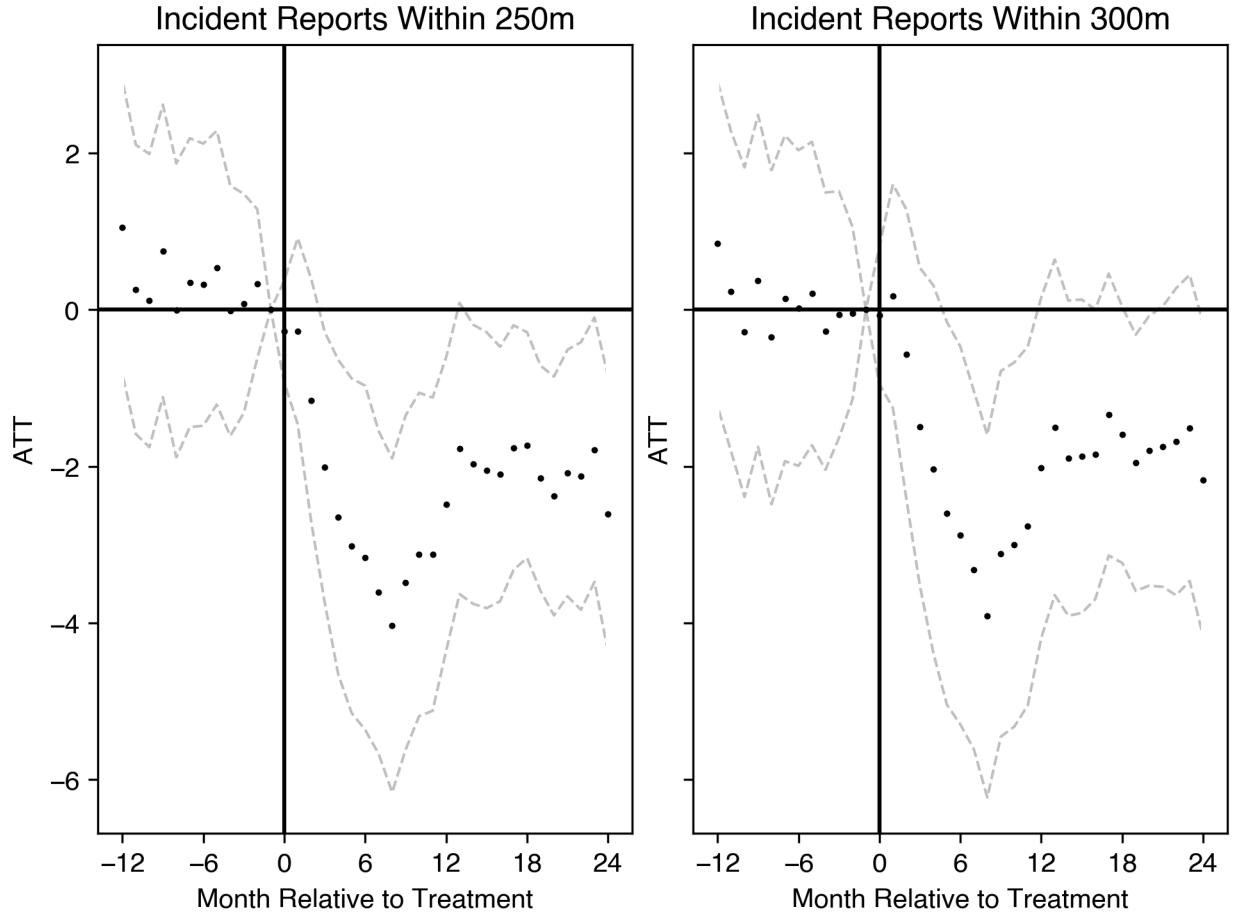


Figure 5: Doubly Robust Event-Study Estimates of ATT, Alternative Radii

Notes: This figure plots doubly robust estimates of treatment effects on incident reports within 250 meters and 300 meters, aggregated by event-time. Treatment effects on incident reports are on the y-axis. Treatment-relative month is reported on the x-axis. Black dots represent point estimates and grey dotted lines represent 95 percent confidence intervals. Treatment effects are estimated and aggregated following Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021).

5.2.3. COVID-19 Pandemic

Much of the post-treatment period we study overlaps with the COVID-19 pandemic. An additional worry is that treatment and control properties would not have followed parallel trends in the absence of treatment during the COVID-19 pandemic. This would lead us to attribute the differential trends around treatment and control properties during the pandemic to the effects of eviction. To evaluate this possibility, we estimate treatment effects as before using our panel dataset, but restrict to months that predate April 2020, so that all of our post-treatment estimates predate the pandemic. In Figure A8, we plot these treatment effects by filing-relative month, dropping months of incident reports for each treated property if that month was during the pandemic. We plot treatment effects

for only the first six months after treatment in this graph as our sample size declines rapidly after this point. Reassuringly, while our estimates are less precise, they are similar in magnitude to our main results and become significant during month three after treatment. Treatment effects during months one through three after treatment display the same steady increase in magnitude that is visible in Figure 2.

5.2.4. *Summary*

As stated in the introduction, the results in this section imply that any violations of parallel trends that generate bias in our estimates (1) must appear in the post-treatment period and not in the pre-treatment period, (2) must not be driven by the pandemic, (3) must not vary when the radius around each property is expanded, (4) must operate within a 250 meter radius of the property and not in the broader neighborhood environment, and (5) must operate within observably similar groups of properties. We view this as implausible.

6. Mechanisms

6.1. *Property Renovations*

We compare the dynamics of treatment effects on incident reports and construction permitting rates in Figure 6. The topmost graph plots the treatment effects of eviction, estimated using our doubly robust specification; this plot is identical to the one presented in Figure 2. Treatment effects remain statistically significant for two years after case filing. In the second graph, at each time period relative to case filing, we plot the share of properties with active construction permits having value above \$5000. We plot this share separately for the treatment group and control group. At month 8 after case filing, the share of properties in the treatment group with at least \$5000 in active construction permits diverges from the share of properties in the control group with at least \$5000 in active construction permits. The gap between the treatment and control group widens, and by month 11 after case filing, properties in the treatment group are nearly twice as likely as properties in the control group to have at least \$5000 in active permits. In the third graph, we plot treatment effects of eviction estimated using our doubly robust specification, dropping from the sample all properties with at least \$5000 in active

construction permits at any point in 2019, 2020, or 2021. Treatment effects steadily increase until month 8 after case filing, at which point they are gradually attenuated, becoming statistically insignificant by month ten. They remain statistically insignificant for the remainder of the two years after case filing. Our point estimate of the average monthly post-treatment effect of eviction in this subsample of properties is -0.98, over 50 percent smaller than the average monthly point estimate from our main specification.

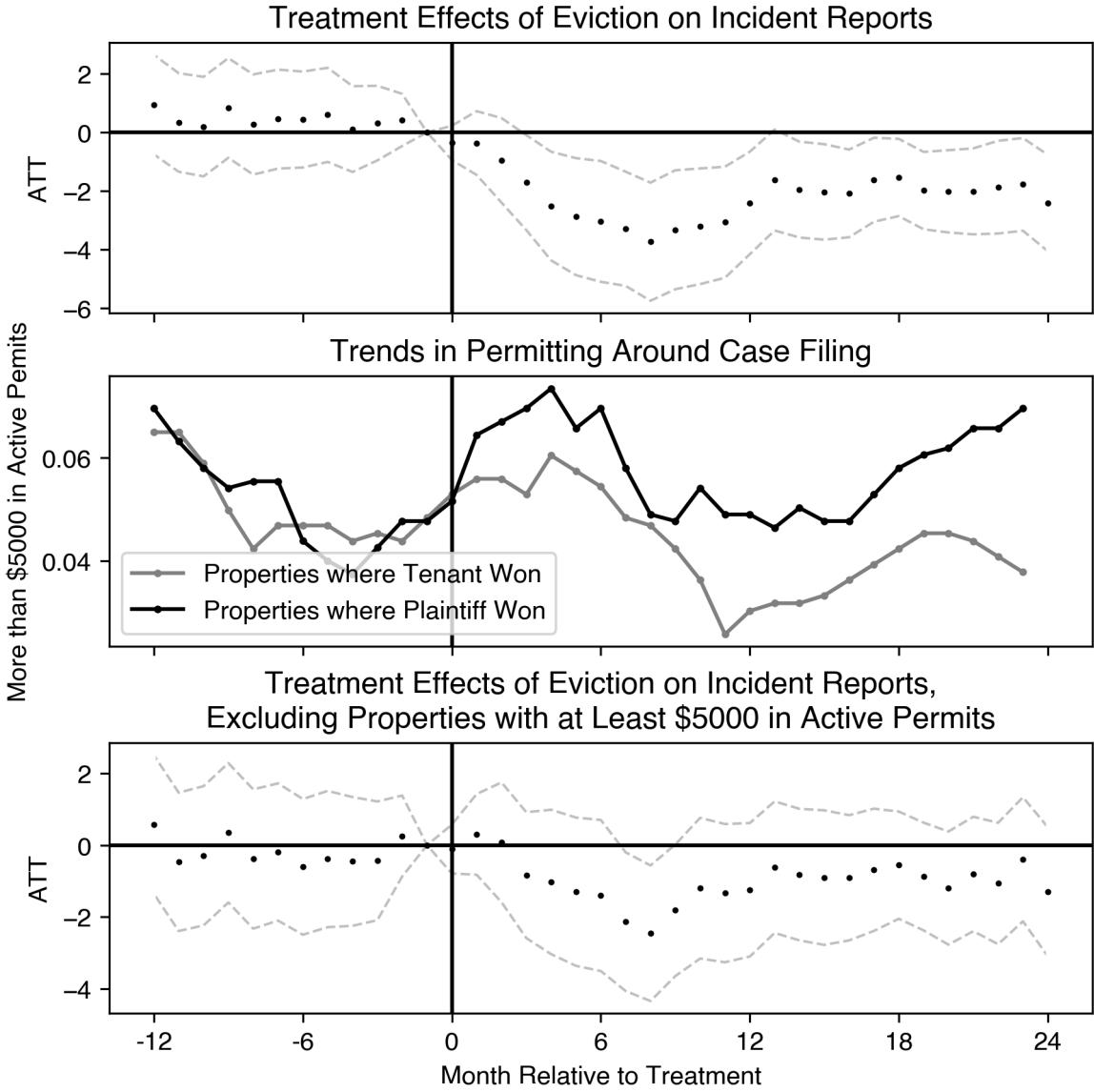


Figure 6: Treatment Effects and Permitting Rates Around Eviction

Notes: This figure plots treatment effects and permitting rates around the time of the filing of eviction cases. The topmost figure plots doubly robust event study estimates of the treatment effect of eviction; it is exactly the plot presented in Figure 2. The second plot displays the share of properties in the treatment group and the control group with active construction permits that have total value greater than or equal to \$5000. The third graph plots doubly event study estimates of the treatment effect of eviction, using the same controls as in the top graph, dropping from the sample all properties which had active construction permits with total value above \$5000 at any time during 2019, 2020, or 2021.

These results are consistent with treatment effects being driven by property-improving renovations. These renovations likely affect incident reports around the property in two ways. First, they likely increase a property's value and the rent it may command, leading to a shift in the types of tenants that occupy the property and contributing to gentrification.

cation in the surrounding area. These changes could lead to reduced incident reports by reducing the frequency of incidents that lead to police calls and by reducing the frequency with which police are called by individuals around the property. Second, construction workers and people associated with the property could appear more following an eviction, thereby creating an informal policing or other deterrent effect.

6.2. Removal of Tenants and Associated Individuals

We are unable to confirm or reject the hypothesis that our treatment effects are driven in part by the removal of tenants and associated individuals. On one hand, figure 2 shows that estimated treatment effects increase steadily during the first three months after treatment. This is in keeping with the timeline by which evicted tenants (and associated individuals) are removed from properties. After an eviction case concludes with a landlord victory, a landlord may obtain an execution for possession from the court. An execution for possession remains valid for 90 days after its issuance. At any time during this 90 day period, the landlord may hire a law enforcement officer to remove the tenant from the disputed property (MassLandlords, 2020a). In contrast, treatment effect magnitudes at event times 4 and greater are neither strictly increasing nor strictly decreasing.

The bottom plot shown in Figure 6 more directly interrogates the behaviors of tenants and associated individuals by removing from the sample properties that underwent large renovations. While the treatment effect of eviction remains significant during months seven through nine, it is not significant during any other month after treatment. Our point estimate of the average post-treatment effect of eviction becomes insignificant. A more direct study of tenants' behavior after eviction is needed to investigate this mechanism.

6.3. Magnitude of Treatment Effects

In interpreting the magnitude of our estimate, we first emphasize again that incident reports are distinct from and more common than crimes. Thus, estimates of the impacts of interventions such as the expansion of police forces on citywide crime may not be useful comparisons for our results (Worrall and Kovandzic, 2010; Evans and Owens, 2007; Zhao et al., 2002; Mello, 2019; Chalfin et al., 2022). Instead, we use a scaling approach to

interpret the magnitude of our treatment effect. We begin with -2.28, our point estimate of the average monthly post-treatment effect of eviction. After multiplying this number by twelve, we find that it is 7.8 percent of the mean number of incident reports that occurred around treated properties in 2017.

This magnitude is large but plausible for several reasons. First, our outcome variable is not crimes but incident reports, which are much more common than crimes. Incident reports are also likely to be more elastic in response to shocks than crime: they may be influenced by individuals' reporting behaviors and are often reactions to incidents that are not crimes in any sense. Second, the impacts on incident reports that we estimate almost certainly represent displacement; that is, eviction is likely to move social disruption to other parts of the city rather than prevent it. Third, the effects we estimate are driven by impacts on reports of non-violent incidents, many of which are not crimes (e.g., "verbal dispute", see Table A1), that may be more responsive to shocks than violent crime. For this reason, too, it is difficult to benchmark our results with estimates of the impacts of crime interventions, which often focus on violent crime as an outcome. Fourth, as noted in Subsection 6.1, we find suggestive evidence that our treatment effects are largely driven by the fact that properties where evictions are successful are significantly more likely to experience renovations. These renovations are likely to fundamentally change the characteristics of the property and its new inhabitants, plausibly leading to neighborhood change in the surrounding area.

We next repeat this scaling exercise using the treatment effect we estimate on the subsample of properties that did not experience large renovations. In this subsample of our data, we find that the average monthly post-treatment effect of eviction is -0.98. This estimate is not significantly different from zero, and has a standard error of .81. Multiplying the point estimate by twelve, we find that it is about 3 percent of the mean number of incident reports that occurred around treated properties in 2017. This is a nearly 60 percent smaller and statistically insignificant decrease in incident reports over the course of a year than implied by our main specification.

7. Conclusion

This paper provides the first quasi-experimental evidence for the effects of eviction on incident reports in the immediate vicinity of a property. Evidence shows that eviction leads to a decrease in incident reports, driven by the fact that landlords are more likely to renovate after an eviction. This implies that while eviction negatively affects evicted tenants, it may also improve the quality of the immediately surrounding area along certain dimensions. Our results also underscore the importance of research that explores the effects of eviction on communities as opposed to individuals.

The overall social impact of eviction is less clear, but will be influenced primarily by three factors. The first factor is its overall impact on neighborhoods. This paper has shown that eviction can lead to neighborhood change, as measured by social disruption that leads to incident reports. But it is also conceivable that eviction leads to neighborhood change in more damaging ways, perhaps by reducing social cohesion. The second factor is the impact of eviction on surrounding tenants. If eviction leads to broad neighborhood change, it is conceivable that many of these tenants are priced out of their neighborhoods. The change in the outcomes of these tenants must enter any welfare analysis of eviction's impacts. The third factor is the impact of eviction on evicted tenants and their behavior. There is little evidence as to whether eviction impacts tenant behavior. However, it is known that eviction does not address the root causes of housing instability. In a previous analysis of 8,091 summary process cases filed between January 2014 and October 2014, we found that 5 percent of defendants were so named two times; 1 percent of defendants had three or more cases filed against them (MassLandlords, 2016). We leave broader analyses of the welfare impacts of eviction's effects to future work.

8. Tables

Restriction	Observations (1)	Forced Move-Outs (2)
Case concluded in Boston before April 2020	3,356	863
Non-missing case initiating action	3,235	794
Case initiated for reason other than foreclosure	3,229	794
Cases for which disposition could be scraped	3,137	794
Case not resolved through mediation	1,511	794
Defendant is an individual and not an entity	1,475	779
Defendant has no attorney	1,438	776

Table 1: Sample Construction

Notes: This table shows how the number of total cases and forced move-outs in our sample changes as sample restrictions are applied. The final row of Column (1) gives the number of cases in our final sample. Column (2) gives the total number of cases resulting forced move-outs after each sample restriction is applied.

		Analysis Sample (N=1,438)			Full Sample (N=3,356)		
		Mean (1)	S.D. (2)	Median (3)	Mean (4)	S.D. (5)	Median (6)
<i>Panel A: Pre-Treatment Incident Report Levels</i>	All Incident Reports, 2017	350.35	265.38	306.00	350.86	251.60	318.00
	All Incident Reports, Month -12	27.89	20.80	24.00	27.78	19.81	25.00
	All Incident Reports, Month -6	25.80	19.99	22.00	25.59	19.17	22.00
	Reports of Non-Violent Incidents, 2017	307.61	234.01	267.00	307.98	222.25	279.00
	Reports of Non-Violent Incidents, Month -12	24.32	18.04	21.00	24.29	17.33	22.00
	Reports of Non-Violent Incidents, Month -6	22.16	17.13	19.00	21.94	16.33	19.00
	Reports of Violent Incidents, 2017	53.91	41.11	47.50	54.46	38.93	49.00
	Reports of Violent Incidents, Month -12	4.54	4.14	4.00	4.52	3.99	4.00
	Reports of Violent Incidents, Month -6	4.35	3.98	3.00	4.41	4.01	4.00
<i>Panel B: Census Tract Characteristics</i>	Median household income, 2016	47,181.81	25,929.27	40,764.00	46,580.36	25,308.68	40,764.00
	Population density, 2010	23,356.02	14,420.49	20,644.44	23,271.47	13,668.72	20,670.66
	Poverty rate, 2010	0.29	0.15	0.26	0.29	0.15	0.26
	Share white, 2010	0.32	0.27	0.31	0.30	0.27	0.23
<i>Panel C: Case Initiation</i>	Filing for nonpayment	0.83	0.38	1.00	0.75	0.43	1.00
	Case dismissed	0.46	0.50	0.00	0.22	0.41	0.00
	Case duration	17.73	22.93	11.00	23.92	27.93	14.00
	Case heard	0.03	0.16	0.00	0.02	0.15	0.00
	Case mediated	0.00	0.00	0.00	0.49	0.50	0.00
	Judgment by default	0.51	0.50	1.00	0.23	0.42	0.00
	Money judgment	1,561.59	3,263.93	181.12	1,791.55	4,170.59	22.50

Table 2: Summary Statistics

Notes: This table provides descriptive statistics for the properties in our dataset and the eviction cases in which they are disputed.

		<i>Difference in Cases Won by Plaintiff</i>				
		Cases Won by Defendant	Unweighted	<i>p</i>	Weighted	<i>p</i>
		(1)	(2)	(3)	(4)	(5)
<i>Panel A</i>	All Incident Reports, 2017	351.77	-2.64	0.85	-14.31	0.31
	All Incident Reports, Month -12	27.89	-0.01	1.00	-1.08	0.33
	All Incident Reports, Month -6	26.16	-0.67	0.52	-0.92	0.38
<i>Panel B</i>	Median household income, 2016	45,239.43	3,601.55	0.01	-2,611.11	0.07
	Poverty rate, 2010	0.30	-0.03	0.00	-0.01	0.32
	Population density, 2010	23,875.35	-962.36	0.21	-921.73	0.22
	Share white, 2010	0.32	0.00	0.96	-0.02	0.19
<i>Panel C</i>	Filing for nonpayment	0.88	-0.10	0.00	-0.04	0.09

Table 3: Balance Tests

Notes: This table summarizes differences in pre-treatment characteristics between the treatment group and the control group, before and after reweighting on pre-treatment characteristics. Column (1) reports means of each pre-treatment characteristic in the treatment group. To produce the values in Column (2), we separately regress each pre-treatment characteristic on a treatment indicator and report the point estimate of the coefficient. We test the hypotheses that each of these coefficients is equal to 0 and report the corresponding p-values in Column (3). We next regress each pre-treatment characteristic on a treatment indicator and propensity scores estimated using a logistic regression propensity score model; this model includes all pre-treatment characteristics listed in this table. In Column (4), we report the point estimates for the coefficients on the treatment indicators. In Column (5), we test the hypotheses that each of these coefficients are equal to 0 and report the corresponding p-values.

		Treatment Effect (S.E.)	Total Incidents, 2017 (Mean Property)	Treatment Effect as % of Mean
		(1)	(2)	(3)
<i>Incident Reports Less Than</i>	250m away	-2.28 (0.82)	350.35	0.65
	300m away	-1.94 (0.93)	482.41	0.4
<i>Incident Reports Between</i>	250m and 300m away	0.36 (0.32)	132.0	0.27
	250m and 350m away	1.0 (0.66)	284.98	0.35
	250m and 400m away	0.67 (0.75)	449.5	0.15

Table 4: Summary of Treatment Effects

Notes: Table 4 summarizes treatment effects of eviction. In Column (1), we estimate the treatment effect of eviction at various distances from a property. We use the controls listed in Table 3. In Column (2), we calculate the total number of incident reports logged in 2017 at various distances from the mean property. In Column (3), we express the treatment effects estimated in Column (1) as percentages of the means calculated in Column (2).

9. Appendix Figures

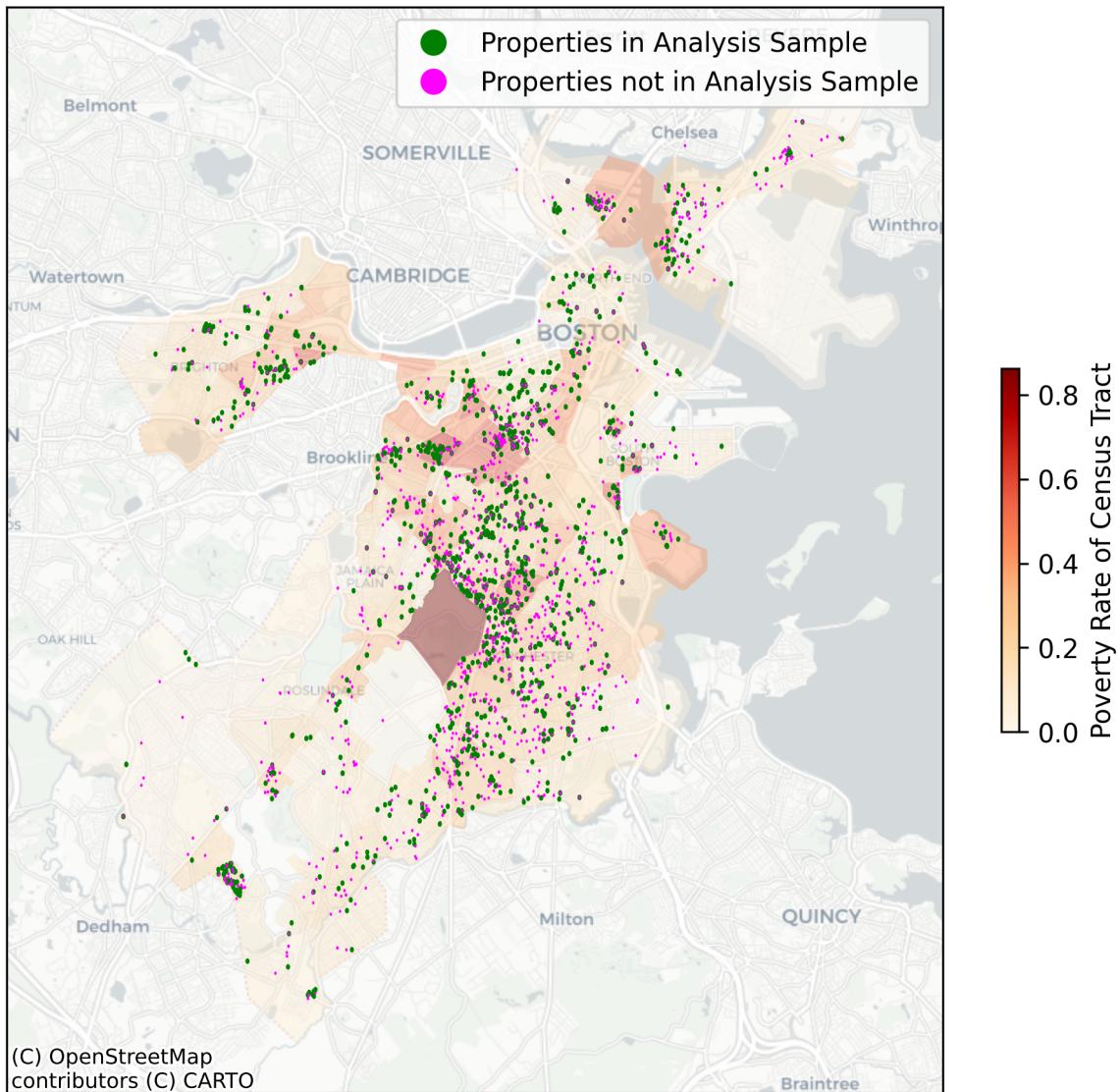


Figure A1: Spatial Incidence of Eviction

Notes: This figure plots the locations of properties disputed in eviction cases in our sample. Census tracts are colored according to poverty rate; darker colors correspond to census tracts with higher rates of poverty. Underlying map data © OpenStreetMap licensable under the Open Data Commons Open Database License (ODbL) and © CARTO.

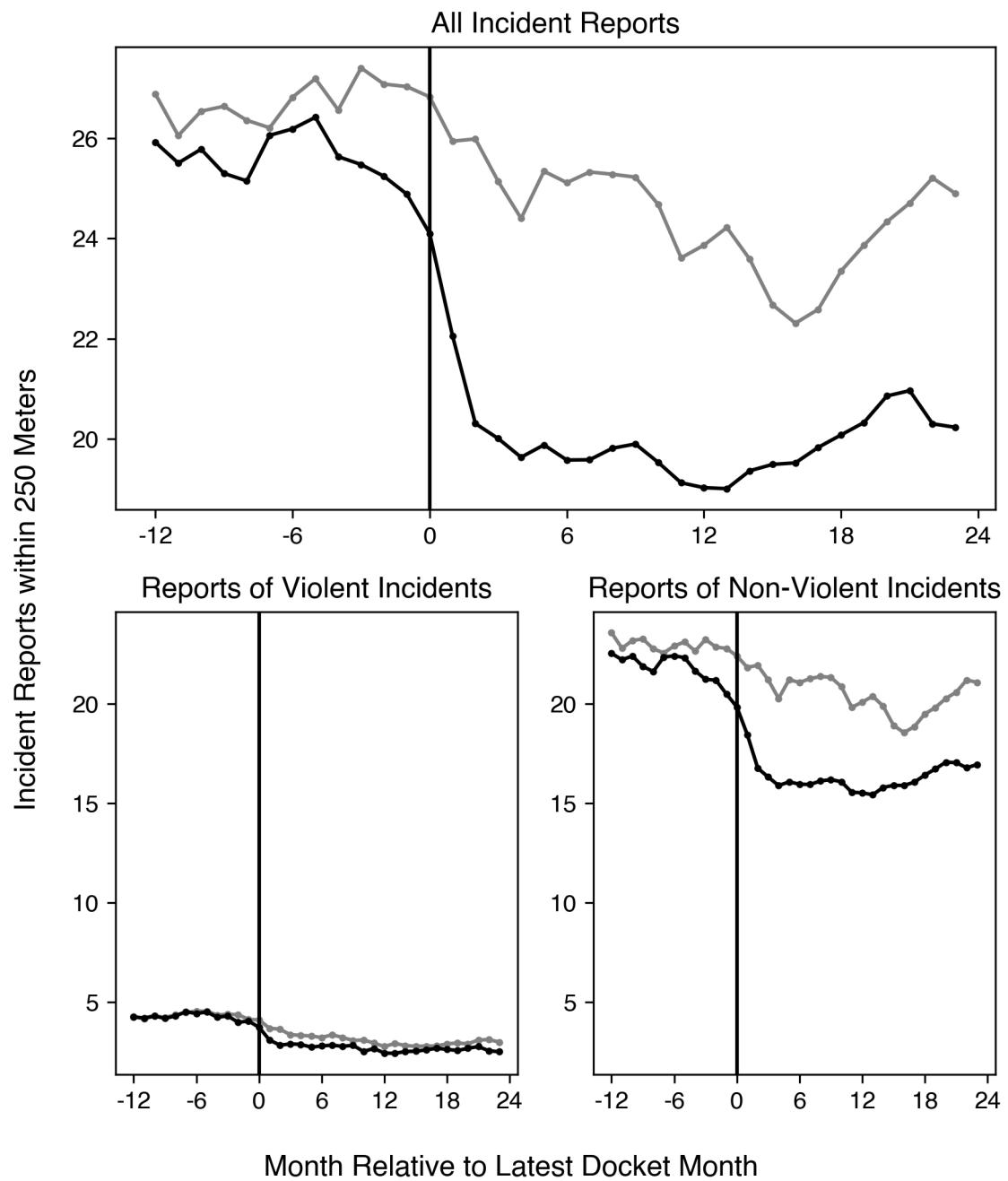


Figure A2: Incident Report Trends Around Case Conclusion

Notes: This figure plots the mean number of incident reports that occurred within 250 meters of properties disputed in cases won by the landlord and properties disputed in cases won by the tenant during each month relative to case conclusion. The mean number of incident reports that occurred within 250 meters of each property is on the y-axis, and the month relative to case filing is on the x-axis.

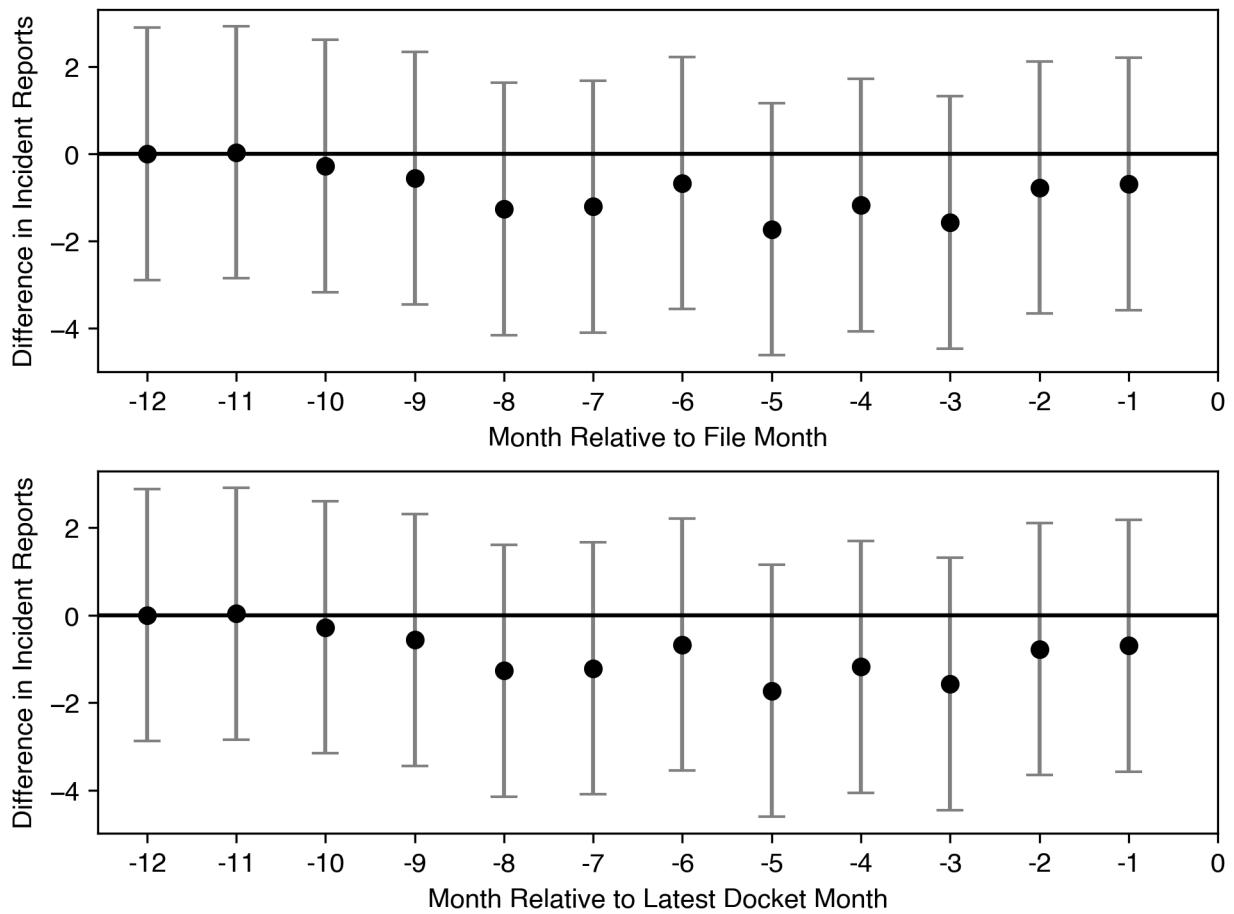


Figure A3: Differences in Incident Report Levels Before Case Filing and Conclusion

Notes: Figure A3 summarizes differences in pre-filing and pre-case conclusion incident report levels between properties disputed in eviction cases won by the plaintiff and properties disputed in eviction cases won by the tenant. Each dot in the first graph plots a point estimate of the difference in incident report levels between the two groups of properties at a single case filing-relative time period. Gray lines plot 95 percent confidence intervals. The second graph replicates the first using case conclusion-relative time instead of case filing-relative time.

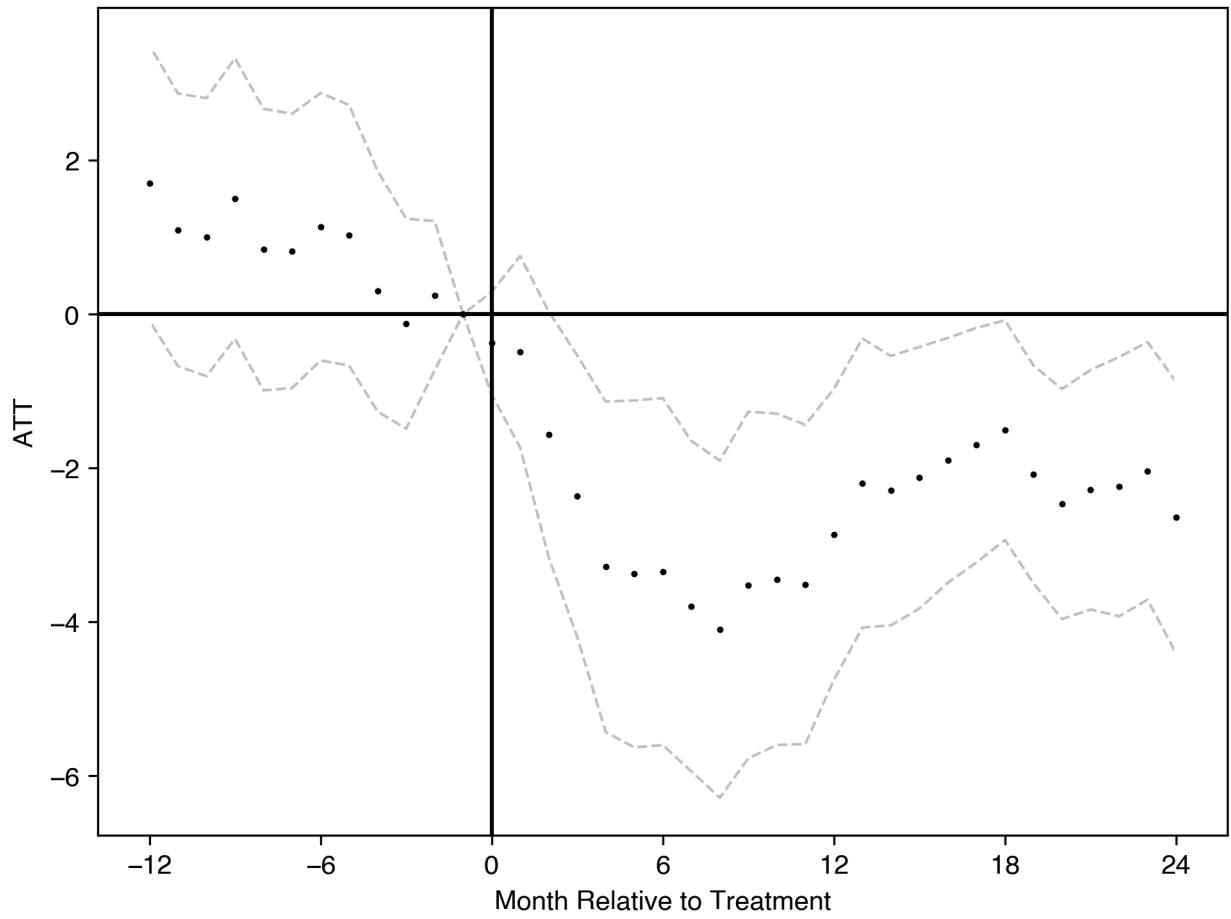


Figure A4: Unconditional Treatment Effects of Eviction on Incident Reports

Notes: This figure plots unconditional estimates of treatment effects aggregated by event-time, estimated on the entire sample. Treatment effects on incident reports are on the y-axis. Treatment-relative month is reported on the x-axis. Black dots represent point estimates and grey dotted lines represent 95 percent confidence intervals. Treatment effects are estimated and aggregated following Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021).

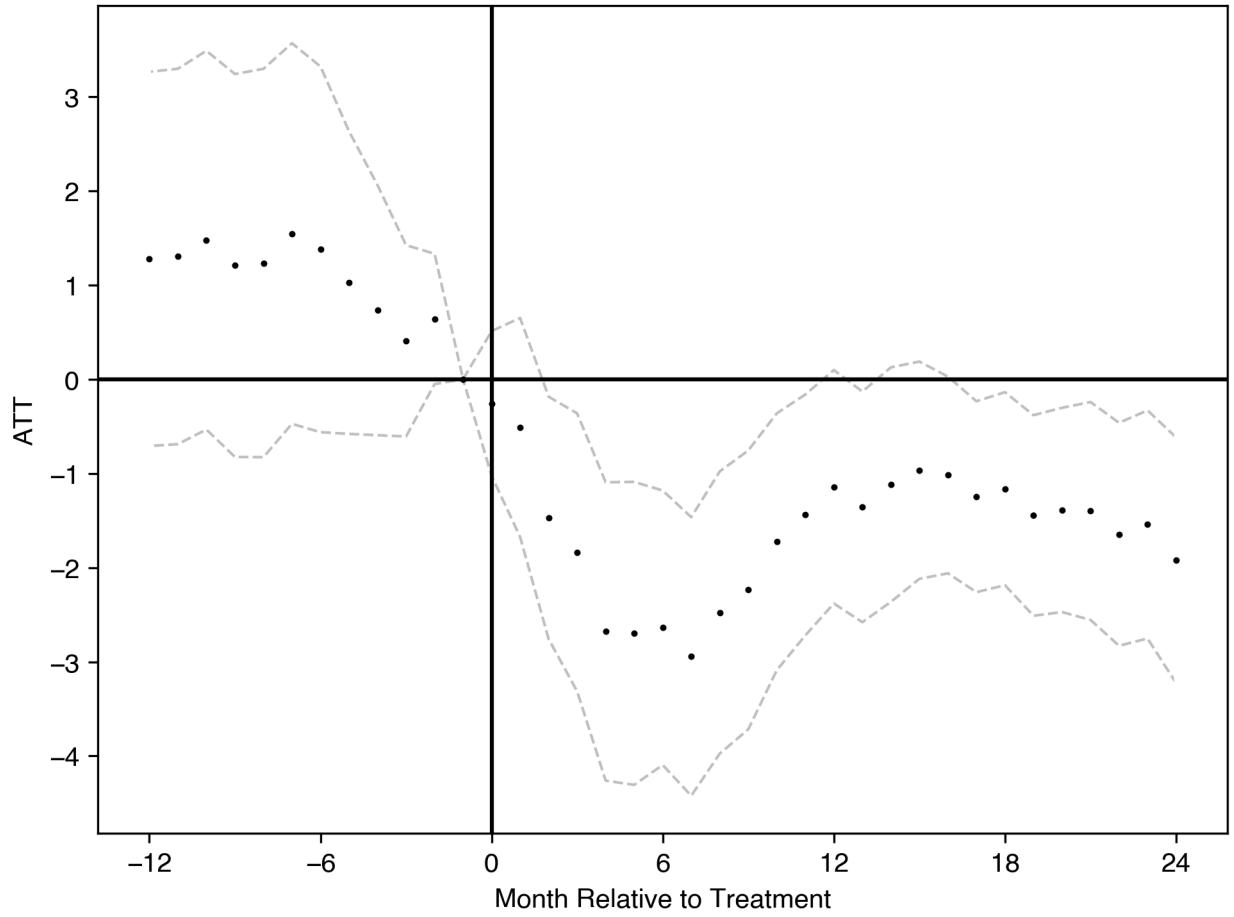


Figure A5: Doubly Robust Event-Study Estimates of ATT Using Latest Docket Month as Treatment Date

Notes: This figure plots doubly robust estimates of treatment effects aggregated by event-time, estimated on the entire sample. Treatment effects on incident reports are on the y-axis. Treatment-relative month is reported on the x-axis. Black dots represent point estimates and grey dotted lines represent 95 percent confidence intervals. Treatment effects are estimated and aggregated following Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021). The estimates in this figure are produced following a procedure identical to the one used in Figure 2, except that the latest docket month is used as the treatment date instead of the file month.

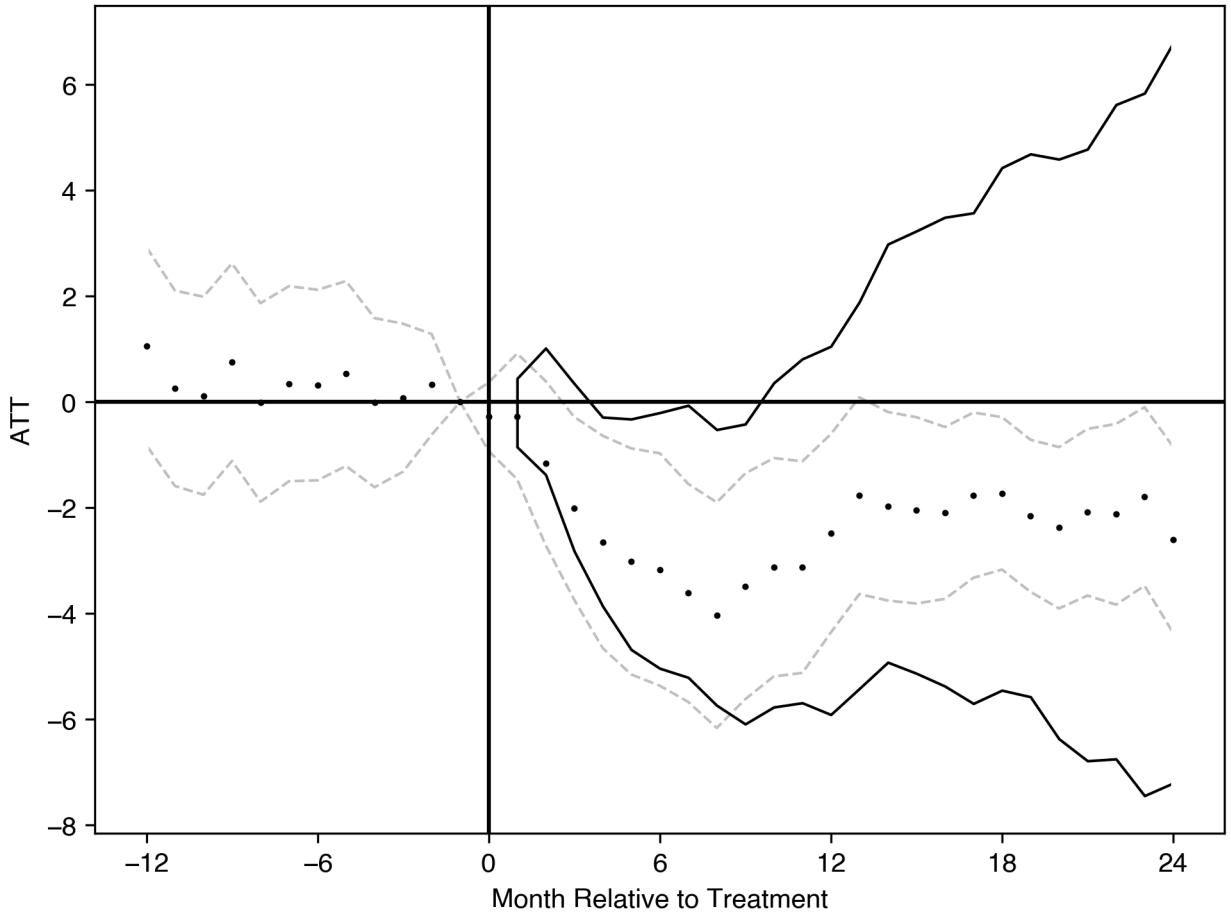


Figure A6: Doubly Robust Event-Study Estimates of ATT Under Parallel Trends Violations

Notes: This figure plots doubly robust estimates of treatment effects aggregated by event-time, estimated on the entire sample. Treatment effects on incident reports are on the y-axis. Treatment-relative month is reported on the x-axis. Black dots represent point estimates and grey dotted lines represent 95 percent confidence intervals. Treatment effects are estimated and aggregated following Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021). Solid black lines give the set of identified ATTs under the assumption that parallel trends violations may get worse in either direction by as much as 0.1 units per month.

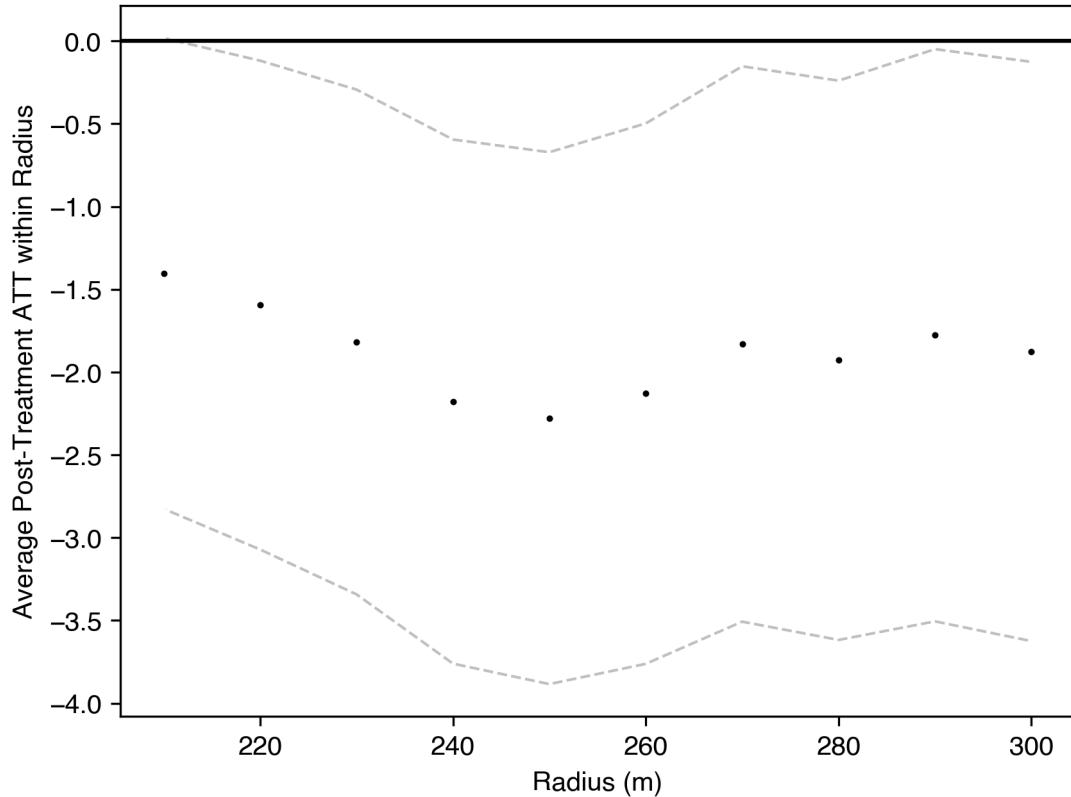


Figure A7: Average Post-Treatment ATT By Radius Around Property

Notes: This figure plots doubly robust estimates of average post-treatment effects of eviction within various distances from the property. Each dot represents a single estimate of the average post-treatment effect of eviction. Treatment effects on incident reports are on the y-axis. Treatment-relative month is reported on the x-axis. Black dots represent point estimates and grey dotted lines represent 95 percent confidence intervals. Treatment effects are estimated and aggregated following Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021).

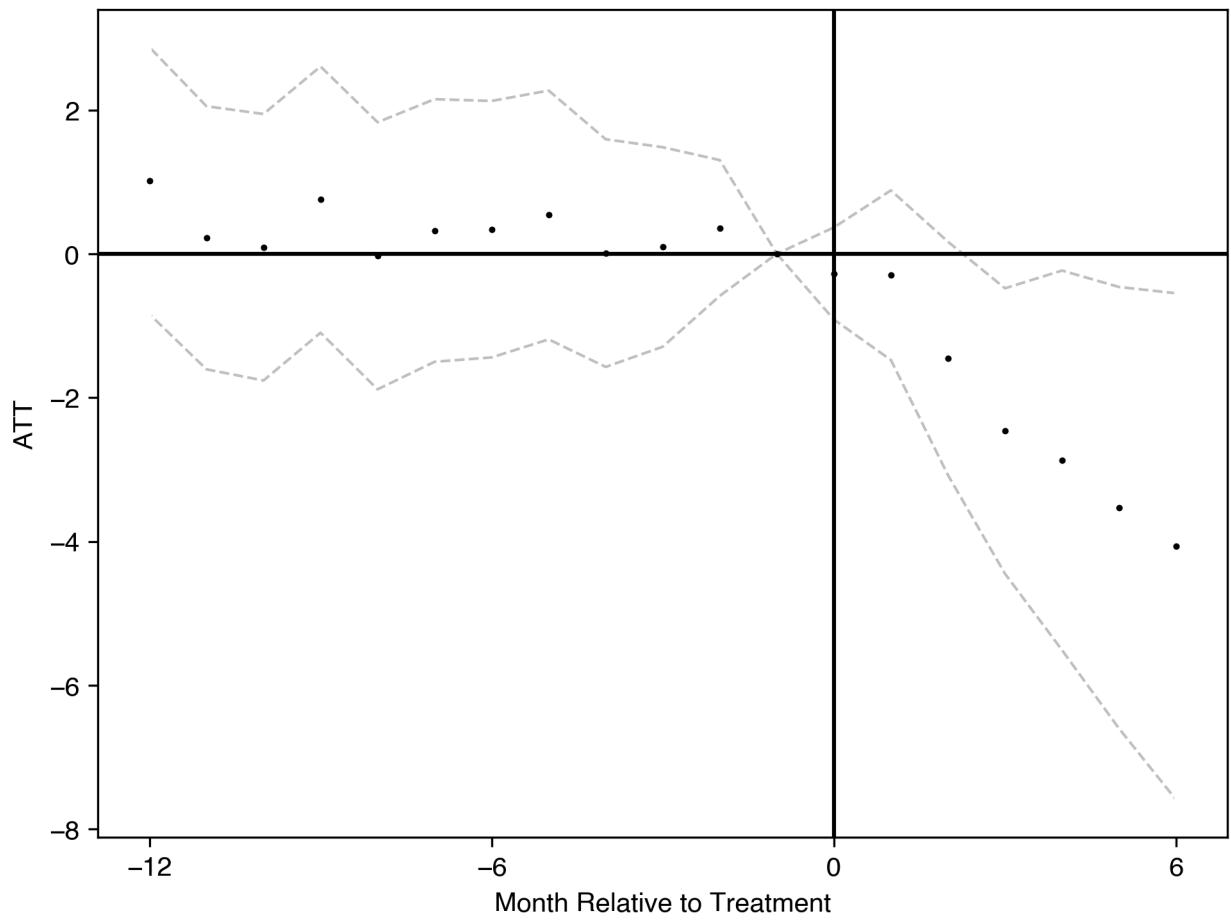


Figure A8: Doubly Robust Event-Study Estimates of ATT, Restricting Post-Treatment Period to Predate the Pandemic

Notes: This figure plots doubly robust estimates of treatment effects aggregated by event-time, estimated on the entire sample of cases, restricting to months prior to April 2020. Treatment effects on incident reports are on the y-axis. Treatment-relative month is reported on the x-axis. Black dots represent point estimates and grey dotted lines represent 95 percent confidence intervals. Treatment effects are estimated and aggregated following Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021).

10. Appendix Tables

Offense Code	Description (1)	Share of All Incidents
		(2)
3115	INVESTIGATE PERSON	0.07
3831	M/V - LEAVING SCENE - PROPERTY DAMAGE	0.05
3006	SICK/INJURED/MEDICAL - PERSON	0.05
1402	VANDALISM	0.05
3114	INVESTIGATE PROPERTY	0.04
3410	TOWED MOTOR VEHICLE	0.04
3301	VERBAL DISPUTE	0.04
3201	PROPERTY - LOST	0.03
613	LARCENY SHOPLIFTING	0.03
614	LARCENY THEFT FROM MV - NON-ACCESSORY	0.03
617	LARCENY THEFT FROM BUILDING	0.03
2647	THREATS TO DO BODILY HARM	0.03
3802	M/V ACCIDENT - PROPERTY DAMAGE	0.02
619	LARCENY ALL OTHERS	0.02
3502	MISSING PERSON - LOCATED	0.02

Table A1: Most Common Non-Violent Incidents

Notes: This table lists the 15 most common types of non-violent incidents that occur within 250 meters of properties in our sample between 2015 and 2023. Column (1) provides a description of the type of incident. Column (2) gives the frequency of each type of incident as a share of the total number of incidents that occurred within 250 meters of properties in our sample between 2015 and 2023.

Offense Code	Description (1)	Share of All Incidents
		(2)
802	ASSAULT SIMPLE - BATTERY	0.03
2647	THREATS TO DO BODILY HARM	0.03
801	ASSAULT - SIMPLE	0.02
423	ASSAULT - AGGRAVATED	0.01
301	ROBBERY - STREET	0.01
413	ASSAULT - AGGRAVATED - BATTERY	0.01
2629	HARASSMENT	0.01
2670	CRIMINAL HARASSMENT	0.01
3830	M/V - LEAVING SCENE - PERSONAL INJURY	0.00
2403	DISTURBING THE PEACE	0.00
361	ROBBERY - OTHER	0.00
311	ROBBERY - COMMERCIAL	0.00
3170	INTIMIDATING WITNESS	0.00
2401	AFFRAY	0.00
111	MURDER, NON-NEGLIGENT MANSLAUGHTER	0.00

Table A2: Most Common Violent Incidents

Notes: This table lists the 15 most common types of violent incidents that occur within 250 meters of properties in our sample between 2015 and 2023. Column (1) provides a description of the type of incident. Column (2) gives the frequency of each type of incident as a share of the total number of incidents that occurred within 250 meters of properties in our sample between 2015 and 2023.

	Cases Won By Defendant (1)	Cases Won By Plaintiff (2)	Portion of All Cases (3)
All Months	662	776	1.00
2019-06	45	13	0.04
2019-07	61	40	0.07
2019-08	66	84	0.10
2019-09	68	89	0.11
2019-10	81	80	0.11
2019-11	56	63	0.08
2019-12	63	63	0.09
2020-01	76	124	0.14
2020-02	74	127	0.14
2020-03	72	93	0.11

Table A3: Case Outcomes and Dates of Conclusion

Notes: This table summarizes variation in dates of case conclusion and case outcomes in our analysis sample.

11. CRediT authorship contribution statement

Arjun Shanmugam: Conceptualization, Methodology, Software, Validation, Formal Analysis, Writing - Original Draft, Writing - Review & Editing, analysis, Visualization.

Douglas Quattrochi: Software, Validation, Data Curation, Writing - Review & Editing.

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13. Data statement

Source code is available for inspection at github, url by request to Arjun Shanmugam. Data is available at MassLandlords by request to hello@masslandlords.net. Our code base contains detailed Readme files adequate for reproduction.

The underlying source dataset is public at MassCourts.org. It is however not downloadable from that domain in a research-ready way. Our research-ready dataset was produced by manually downloading case dockets over a period of several years. Massachusetts law makes it clear that names and addresses at MassCourts.org are not protected, but case law and pending legislation make it unclear whether our concentrated list of names and addresses suddenly would become protected if transmitted to a third-party. For this reason, out of an abundance of caution, we are limiting distribution to researchers with research intent. All reasonable applications will be accepted. We will facilitate transmission of addresses for reproduction.

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