Does Eviction Affect Crime? Quasi-Experimental Evidence from Boston

Arjun Shanmugam*

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Abstract

This paper provides quasi-experimental evidence of the effects of eviction on the frequency of crime incidents in the immediate vicinity of a property. Anecdotal evidence suggests that eviction is destructive to its victims, but little is known about its impact on immediately surrounding areas. I use a doubly robust difference-in-differences research design based on the staggered conclusion of eviction cases in Boston, Massachusetts between April 2019 and March 2020. Using properties where eviction was unsuccessful as the counterfactual for properties where eviction was successful, I find negative effects of eviction on crime incidents within 500 meters of a property that persist for three years after case resolution, even conditional on 15 pre-treatment characteristics including income, population density, and race. My point estimate of the average post-treatment effect of eviction on crime incidents has magnitude equal to 3.89 percent of the mean number of crimes per month within 500 meters of a property in 2017. I find strong evidence that the mechanism driving these impacts is the removal of individuals who are crime incident targets, accomplices, and/or perpetrators from the immediate vicinity of the property. I find evidence against several alternative explanations for my results, including that they are driven by a pandemic-related shift in crime trends. Estimates remain significantly different from zero even when the radius considered around each property is adjusted.

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1 Introduction

Eviction is extremely common in the United States relative to other rich countries (OECD, 2021). Faced with compelling non-experimental evidence that it is destructive of victims' lives (Desmond, 2017; Desmond and Gershenson, 2016; Desmond and Kimbro, 2015), policymakers have implemented sweeping measures over recent years to prevent eviction and mitigate its impacts (Liptak and Thrush, 2021; Logan, 2021), placing the issue at the center of debates over social policy. But while much of the eviction literature has studied its effects on individuals, its effects on small communities are unclear. On one hand, researchers have noted that the prevalence of eviction is negatively associated with both economic connectedness, the degree of interaction between low- and high- income people, and social cohesiveness, the degree to which social networks are fragmented into cliques (Weaver, 2023; Chetty et al., 2022). But if victims of eviction are themselves disruptive to the social fabric of their communities, then eviction could actually improve the quality of neighborhoods where it occurs. In order to credibly assess previous policy responses to eviction and inform effective policy responses moving forward, it is crucial to understand eviction's impacts on small communities.

This paper seeks to reinforce our knowledge of eviction's social effects on small communities by estimating the impact of an eviction on the frequency of crime incident responses within 500 meters of the property. 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 obtain in bulk. It is also challenging to link eviction case records to individual- and property-level outcomes. I overcome this barrier by obtaining crime incident-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 since April 2019 and every BPD crime incident response between August 2015 and January 2023, I spatially join each eviction record concerning a property in Boston with any crime incident responses which occurred within 500 meters of the property. I produce a panel that allows me to observe crime incident response counts near each property for several months before and after case resolution.

The second main roadblock to empirical eviction research is the endogeneity of eviction. For instance, partly due to generations of housing policies which sought to systematically exclude African-Americans from home ownership, there is a strong relationship between eviction and race, at the individual- and neighborhood-levels (Rothstein, 2017). Eviction is also correlated with income, population density, and a host of other socioeconomic variables, many of which are correlated with crime incidence. I attempt to address this second barrier in two ways. First, I restrict my study to properties where an eviction case was filed; I use properties where an eviction was filed but unsuccessful as the counterfactual for properties where an eviction was successful. The control group I define is more similar to my treatment group on observables—and likely on unobservables—than a control group including properties which are not disputed in any eviction case. This should reduce the potential for bias in my estimates. Second, taking advantage of my granular outcome data, I use a doubly robust difference-in-differences research design (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021) based on the staggered conclusions of eviction cases, which eliminates bias in my estimates from confounders that are invariant over time[1]. It rests on the assumption of parallel trends among treated and control units with the same observed covariates—a much more plausible assumption than the unconditional parallel trends assumption required by traditional difference-in-difference designs. I include 15 pre-treatment socioeconomic and case-related characteristics in the model. To justify their inclusion as controls, I use them to estimate a logistic regression propensity score model and show that re-weighting counterfactual properties using inverse propensity scores eliminates nearly all significant differences in observable characteristics between treated and counterfactual properties. Time-varying confounders may still bias my estimates, but I argue in subsections 5.3.1 and 5.3.2 that this is unlikely considering the short time frame of my study.

Doubly robust estimates suggest that eviction has negative effects on the number of crime incident responses in the 500 meters surrounding a property. These estimates are significantly different from zero for three years after case resolution. Their magnitudes are between 2 and 5 percent of the number of crime incident responses within 500 meters of the average property in my sample during 2017. I

[1] I initially applied an examiner design to my context, exploiting randomness in assignment of eviction cases to judges of varying leniency towards tenants. However, the sample I have access to is not large enough to answer my research questions using such an empirical strategy.

do not find evidence of pre-treatment differences in crime incident response trends between properties where evictions were successful and properties where evictions were unsuccessful.

Strong evidence suggests that the mechanism driving my results is the movement of individuals associated with crime incident responses outside the immediate vicinity of properties where evictions are successful. Three pieces of evidence support this hypothesis. First, steady increases in the magnitude of treatment effects during the first 90 days after treatment are consistent with the process by which tenants can be removed from dwellings. Second, restricting the outcome variable to crime incidents that evicted tenants are unlikely to commit near their own homes results in small and largely insignificant treatment effects. Third, impacts of eviction are driven by cases which conclude during warm months, during which crime is more common. In subsection 5.3, I provide strong evidence against several other alternative explanations for my results, including a pandemic-related change in crime trends, reverse causality, and improvements in social cohesiveness of neighborhoods post-eviction.

These findings are important for two reasons. First, by suggesting that evicted individuals are more likely to be associated with crime incidents—as perpetrators or victims—they tell a previously unseen story of the characteristics of evicted tenants. Evicted tenants are distressed before eviction, and in many ways, eviction worsens their socioeconomic outcomes (Collinson et al., 2022). But my findings imply that their removal may improve the quality of the neighborhoods they leave behind. Second, they suggest that evictions may encourage the spread of criminal activity across locales.

I contribute to a wide literature which studies the effects of eviction on 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 (Desmond and Gershenson, 2016; Desmond and Kimbro, 2015; An et al., 2021). This paper is also related to a burgeoning literature in economics which seeks to apply quasi-experimental methods to study the effects of eviction. Collinson et al. (2022) exploit random assignment of eviction cases to judges of varying leniency to estimate the effects of eviction on outcomes such as consumption of durables and homelessness. They measure effects on social and economic outcomes which less devastating relative to those estimated by the sociology literature, further underscoring the importance of quasi-experimental evidence in this context.

This paper is one of relatively few in the economics literature to address the relationship between eviction and crime. Kroeger and La Mattina (2020) studies nuisance ordinances—municipal laws which punish landlords for crimes that occur on their properties—and finds that they make eviction filings more common across cities in Ohio. Falcone (2022) expands on this finding, arguing that evictions increase crime at the municipality level under the assumption that nuisance ordinances affect crime only by making evictions more common. My research distinguishes itself from these studies by estimating the causal effect of an eviction on crime 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 I obtain and the dataset I assemble for my analysis. Section 4 outlines my empirical strategy. Section 5 provides and discusses results, and Section 6 concludes.

2 Institutional Context

2.1 Eviction in Massachusetts

2.1.1 Legal Landscape of Eviction

Eviction cases—known formally as summary process cases—fall under the purview of the Massachusetts Trial Court. In particular, three sub-departments of the Trial Court have jurisdiction over summary process cases: the District Court, the Boston Municipal Court, and the Housing Court (MassLegalHelp). Evictions may be filed in any of these three courts. The District Court maintains a large number of locations throughout the state of Massachusetts and Boston Municipal Court operates only within the city limits of Boston.

The vast majority of summary process cases are adjudicated in the Housing Court, established in 1971 with the specific purpose of dealing with housing-related matters (Garrity, 1979). The court has been expanded several times since then; since the passage of the most recent Housing Court expansion law in 2017, every Massachusetts resident has access to a Housing Court (Gee, 2017). Across the state, 15 judges preside over cases filed in six divisions: Central, Eastern, Metro South, Northeast, Southeast, and Western. Figure 1 shows the geographic boundaries of each of these six divisions in Massachusetts.

Four features distinguish the Housing Court from the District Court and Boston Municipal Court (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, it offers a service known as *mediation*, in which cases may be resolved prior to arguments in front of a judge. 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 defeat 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 or Boston Municipal Court has the right to transfer the case to the Housing Court at any time prior to trial. For these reasons, tenant advocacy groups in Massachusetts recommend that defendants transer summary process cases to the Housing Court (Massachusetts Law Reform Institute, 2022).

2.1.2 The Eviction Process in Massachusetts

A landlord in Massachusetts begins the eviction process by serving her tenant with a *notice to quit*, which serves as written notice of her desire to terminate tenancy. A notice to quit is served for *nonpayment of rent*, *cause*, or *no fault*[2] and 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 period[3]

After the length of time specified by the notice to quit has passed[4], the tenant's rental agreement

[2]Massachusetts law does not in general prohibit "no fault" or "no cause" evictions (Devanthéry and McDonagh, 2017d).

[3]Tenants 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 (Devanthéry and McDonagh, 2017b).

[4]If the notice to quit was for nonpayment of rent, the specified length of time must pass without curing of the

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 (Devanthéry 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 of a time period[5] or by failing to properly deliver a summary process summons and complaint to the tenant[6]—her case may be *dismissed*, automatically awarding victory to the tenant (Devanthéry 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 legal reasons that the landlord should not evict the tenant, and *counterclaims*, or claims that the tenant has against the landlord[7] (Devanthéry 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, 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. After the tenth day, 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

nonpayment by the tenant.

[5]The time period that must be specified by a notice to quit may vary depending on whether it is for nonpayment of rent, for cause, or no fault.

[6]The landlord is required to hire a local law enforcement officer to deliver the tenant with a summary process summons and complaint

[7]The tenant may also file for *discovery* at this stage. Discovery is the process by which the tenant may request information from her 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 halted by two weeks (Devanthéry and McDonagh, 2017a).

a ruling in favor of the landlord or after an execution for possession has been granted.

2.2 Crime in Boston

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), below cities of comparable size such as Las Vegas, Columbus, and Nashville. Within the city of Boston, crime rates are highest in the poorest and most diverse neighborhoods. The median poverty rate across census tracts where crimes were committed between April 2019 and March 2020 is about 23 percent, much higher than Boston's poverty rate as a whole, around 17.6 percent. In my dataset, three of Boston's poorest neighborhoods—Roxbury, Dorchester, and Mattapan (Boston Redevelopment Authority, 2014)—account for nearly 40 percent of all crime incidents reported.

3 Data

3.1 Evictions Data

I obtain records of summary process cases that concluded in Boston between April 2019 and Janury 2023 from MassLandlords, a trade association of landlords in Massachusetts. At the beginning of the pandemic, MassLandlords developed a sophisticated system for manually collecting and programatically scraping court dockets (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, whether the tenant and landlord had attorneys, and the type of notice to quit that was initially filed by the landlord. I use a paid geocoding service known as Geocodio to obtain highly accurate latitude and longitude coordinates for each property.

I restrict my sample in five ways. First, I drop all cases missing a most recent docket date. Second, I drop all cases missing property addresses. Third, I drop cases which concluded during April 2020 or later. Fourth, I drop all cases resolved through mediation. Lastly, I drop all cases for which a judgment could not be scraped. Table 1 outlines how these sample restrictions altered the number of observations in my study. Figure 2 displays the number of evictions in my sample filed during each month.

3.2 Crime Incidents Data

I obtain incident-level records of every crime incident to which BPD officers responded from August 2015 to January 2023. Each record includes the date, latitude and longitude coordinates, and an offense code indicating the type of crime incident that occurred.

3.3 Census Tract Characteristics Data

I obtain time-invariant census tract level characteristics of the properties in my sample from Opportunity Insights (Chetty and Hendren, 2018). This data includes information such as population density, job density, median household income, and poverty rate. Figure 3 plots the locations of the properties disputed in eviction cases in my sample, coloring 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, 64 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.

3.4 Merged Dataset

To produce the sample used in this analysis, I match each property with all crime incidents that occurred within a 500 meter radius. Then, for each property, I count the number of crime incidents that occurred within its radius during each month from August 2015 to January 2023. I am left with a panel dataset of nearby crime incident counts at the property month-level.

Table 2 contains descriptive statistics for the panel dataset. Panel A notes that on average, the total number of crimes that occurred within the radius of a property was slightly lower in 2019 than in 2017. In 2017, the mean number of crimes occuring within 500 meters of a property was about 1,163.

Panel B describes socioeconomic characteristics of the census tracts in which evictions occur. On average, properties disputed in my data are located in census tracts that are significantly less educated, poorer, and denser than Boston as a whole. In the census tract of the average eviction case in my sample, 31 percent of individuals have a bachelor's degree, compared with 52 percent in Boston as a whole; median household income is about $46,400, compared with $81,744 in Boston as a whole; and population density is about 23,450 people per square mile, compared with 13,977 people per square mile in the city of Boston (Census Bureau, 2020).

Panel C outlines the reasons that evictions in my sample are filed. Over 75 percent of evictions were filed because a tenant did not pay rent. Around 10 percent of evictions are filed for cause, while just under 5 percent are filed for no cause. These statistics are 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.

In Panel D, I explore characteristics of defendants and plantiffs. It is extremely rare for tenants to have legal representation in eviction cases; 91 percent of plaintiffs, on the other hand, are represented by an attorney. However, this gap in legal representation is not necessarily driven by socioeconomic differences between plaintiffs and defendants. Massachusetts law requires that plaintiffs which are corporations or limited liability companies are represented by legal counsel (*Sunset Properties LLC v. Valentino*; *Varney Enterprises, Inc. v. WMF, Inc.*), and the vast majority of plaintiffs in my sample are entities, not individual landlords.

Lastly, Panel E describes characteristics of case resolution in my sample. Recall that in my sample, I do not consider cases which are mediated. In the vast majority of remaining cases, the plaintiff wins by default or the tenant wins as a result of a case dismissal. Only about 5 percent of cases are actually heard by a judge. The average amount of time between the time a case is filed and its final docket date is 20 days.

Table 3 expresses variation in latest docket dates and case outcomes throughout my sample. There are 12 unique final docket months in the sample of cases I consider and significant variation in case outcomes within each month.

4 Empirical Strategy

The traditional approach to difference-in-difference estimation when treatment timing is staggered is to use a two-way fixed effects (TWFE) estimator to identify the average treatment effect on the treated (ATT). However, recent advances in econometrics have shown that TWFE estimates can be biased or difficult to interpret when treatment timings are staggered and treatment effects vary over time, as is likely in my setting. As such, I use the staggered difference-in-difference estimator proposed in Callaway and Sant'Anna (2021). It uses the canonical two-period, two-unit difference-in-difference estimator to estimate a single ATT parameter at every time period for each group of units treated at the same time. It then aggregates these estimates to produce summaries of the ATT.

There are three main reasons that the traditional TWFE estimator is inferior to Callaway and Sant'Anna (2021)'s estimator. Goodman-Bacon (2021) shows that the TWFE estimate is a weighted average of many canonical two-period, two-unit difference-in-differences estimates. The first reason that the traditional TWFE estimator is inferior is that when treatment timings are staggered or treatment effects vary over time, the weights in this average may be negative, making estimated treatment effects impossible to interpret intuitively. The second reason that the TWFE estimator is inferior is that some of the two unit, two period difference-in-differences estimates in this average make "forbidden comparisons" between two sets of already treated units. Because these comparisons use already treated units as the counterfactual for other already treated units, they are inconsistent with the spirit of a quasi-experimental approach. The third reason is that the two-way fixed estimator weights the average of two unit, two period difference-in-differences estimates using treatment variances, whereas Callaway and Sant'Anna (2021)'s estimator simply uses the share of treated units. Together, these issues mean than the traditional TWFE estimator may produce severely biased estimates of the parameters I hope to estimate. Callaway and Sant'Anna (2021)'s estimator avoids the limitations of TWFE by individually calculating all possible two unit, two period difference-in-differences estimates which make comparisons between treated and untreated units. For each group of units treated at the same time g, it calculates a two-unit, two-period difference-in-differences estimate at each time t, where the "pre" period is $g-1$, the "post" period is t, the treatment group is all units treated at time g, and the control group is all never-treated units[8].

4.1 Conceptualizing Experiment

In an ideal experimental design, a randomly chosen subset of properties in my sample would be disputed in eviction cases won by the plaintiff, and remaining properties would be disputed in eviction cases won by the defendant. Such random assignment of eviction is impossible. In this subsection, I define my treatment group and control group and argue that, given what I observe, my definition comes as close as possible to this experimental ideal.

I consider any property disputed in an eviction case decided by a judge in favor of the plaintiff or decided by default as a treated property. Each property in the treated group becomes treated during its latest docket month. I consider any property disputed in an eviction case decided by a judge in favor of the defendant or decided by dismissal as a control property. The key difference between treated properties and control properties is that landlords are legally able to remove tenants from treated properties, as I describe in subsection 2.1.2. I note that while I observe case outcomes, I do not have access to data on the portion of treated properties from which tenants physically depart. However discussions with several Boston landlords confirm that in the vast majority cases won by the plaintiff, the defendant ultimately leaves or is removed from the property.

I do not consider properties whose eviction cases were resolved via mediation. I have no way of reliably classifying these properties as part of the treatment or control group, because I have no way to observe whether mediation agreements result in the departure or removal of the tenant.

It is 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 which are not disputed in any eviction case (Robinson and Steil, 2021). My sample supports this claim. In subsection 3.4, I note large differences between the characteristics of properties in my sample and the characteristics of the city of Boston; in column (2) of Table 4, I show that differences in these

[8] I use never-treated units as the control group in my study. Callaway and Sant'Anna (2021)'s framework also allows for the use of not-yet-treated units as the control group. I recommend readers to their paper for more information.

characteristics are much smaller between the treatment and control groups. This claim is likely to hold for unobservable characteristics as well. The control group I define is thus a better counterfactual for the treatment group than a control group including properties which are not disputed in eviction filings. This should reduce the potential for bias in my estimates.

4.2 Setup

I denote a particular time period by t where $t = 1, ..., 90$ indexes months August 2015 through January 2023. For a particular unit i, let $D_{i,t}$ be a binary variable equal to 1 if unit i is treated during month t and zero otherwise. Denote property i's latest docket month as $G_i = g \in \{35, ..., 46\}$[9], so that property i becomes treated during month g. Let $C_i = 1$ if property i is never treated and 0 otherwise. If $C_i = 1$, property i is in the control group. If $G_i = g$ and $C_i = 0$, then property i is a treated property and a member of cohort g, whose members become treated during month g. Define $D_i(g) = 1$ if property i is a member of cohort g and 0 otherwise. Furthermore, denote $Y_{i,t}$ as property i's crime incident count during month t. Let $\Delta Y_{i,g-1,t}$ equal the change in property i's crime incident counts between months t and $g - 1$.

4.3 Unconditional Estimates of the ATT

The following is an unconditional estimator for $ATT(g, t)$, the average treatment affect during month t for cohort g.

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

The above estimator will identify $ATT(g, t)$ under the following assumptions:

Assumption 1a *(Staggered Treatment Adoption Assumption). Let $D_{it} = 1$ if unit i has been treated by month t. Then, for $t = 1, ..., 89$, $D_{i,t} = 1 \implies D_{i,t+1} = 1$.*

Assumption 1a requires that treated units remain treated for the remainder of the sample period

[9] $35, ..., 46$ are the indices corresponding to April 2019, ..., March 2020. As noted in subsection 3.1, I restrict my sample to cases with latest docket dates between April 2019 and March 2020.

once they become treated. Since an evicted individual may not return as a tenant to the property from which she was evicted, treated properties in my context remain treated upon becoming treated, and this assumption is likely satisfied.

Assumption 1b *(Parallel Trends Assumption). For all* $g = 35, ..., 46$, $t = 2, ..., 90$, *with* $t \geq g$, $E[Y_t(0) - Y_{t-1}(0)|G = g, C = 0] = E[Y_t(0) - Y_{t-1}(0)|C = 1]$.[10]

Assumption 1b states that in the absence of any treatment, the path of untreated potential outcomes for each cohort g would have been parallel to the path of untreated potential outcomes for control properties.

4.4 D.R. Estimates of the ATT

The above unconditional parallel trends assumption may not hold, as eviction case outcomes and trends in crime incident counts may be related to their socioeconomic surroundings. My next strategy uses covariates to construct a more credible 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, I collect the 15 pre-treatment covariates from panels A through D of 2 in a vector X_i. I next estimate $\hat{p}_g(X_i)$, a logit regression propensity score model for the probability of being in cohort g. I assign a weight $\hat{w}_i(X_i) = \frac{\hat{p}_g(X_i)}{1-\hat{p}_g(X_i)}$ to each never-treated property i; $\hat{w}_i(X_i) = 1$ for treated properties. I define normalized weights $\hat{w}_i^*(X_i) = \frac{\hat{w}_i(X_i)}{\sum_i \hat{w}_i(X_i)}$.

Next, using only never-treated properties, I regress $\Delta Y_{i,g-1,t}$ on X_i, weighting by $\hat{w}_i^*(X_i)$. Using the estimated coefficients $\hat{\beta}^X_{g-1,t}$, I define $\Delta\hat{\mu}_{g-1,t}(X_i) = \hat{\beta}^X_{g-1,t} X_i$. This means that $\Delta\hat{\mu}_{g-1,t}(X_i)$ is the predicted change in property i's crime incident counts between months t and $g-1$.

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

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

[10]A "no anticipation" assumption is implicit in the notation I use. See Callaway and Sant'Anna (2021) for more information.

Note that $\bar{D_i(g)}$ and $\bar{C_i}$ are sample averages.

The estimator defined in equation 2 simultaneously models the counterfactual change in *observed* outcomes for untreated properties ($\hat{w}_i(X_i)$) and the *predicted* change in outcomes for untreated properties ($\hat{\mu}_{g-1,t}(X_i)$). As long as one of these two models is correctly specified, the doubly robust estimator will identify $ATT(g,t)$ under the following two assumptions:

Assumption 2a *(Staggered Treatment Adoption Assumption).* This assumption is identical to assumption 1a.

Assumption 2b *(Conditional Parallel Trends Assumption). For all $g = 35, ..., 46$, $t = 2, ..., 90$, 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. This is a much weaker assumption than the unconditional parallel trends assumption required by the estimator defined in equation 1.

To provide support for assumption 2b, I show that controlling for the empirically chosen covariates significantly improves balance between the treatment and control groups. Columns (3) and (4) of Table 4 show significant pre-treatment imbalance in covariates between the treatment and control groups. Each cell in column (3) reports the coefficient from a univariate regression of one covariate on a treatment indicator. Column (4) reports the p-values associated with each of these coefficients; many are significant at all conventional levels. In each row of column (5), I regress one covariate on an indicator for plaintiff victory and $\hat{w}_i(X_i)$, the previously estimated propensity scores (Austin, 2011), and report the coefficient on the indicator for plaintiff victory. Each cell of column (6) reports the p-values from a hypothesis test that the coefficient on a single pre-treatment characteristic is equal to 0. Column (6) shows that conditioning on pre-treatment covariates makes almost all pre-treatment differences in covariates insignificant.

4.5 Aggregating Treatment Effects

The estimator in equation (2) produces estimates of $ATT(g,t)$ for each cohort g at each time period t. As such, following Callaway and Sant'Anna (2021), I aggregate my estimates of these $ATT(g,t)$ parameters in two ways. First, I aggregate them according to time since treatment, weighting by the size of the treated cohorts. For each $ATT(g,t)$, time since treatment e is equal to $t-g$. I produce 95 percent confidence intervals around my estimates of $ATT(e)$ across different event times following the bootstrap procedure described in Callaway and Sant'Anna (2021).

Following a simlar procedure, I also aggregate all $ATT(g,t)$ parameters with $t>g$—post-treatment estimates of the effects of eviction. I report the standard errors of these estimates to facilitate easy comparison across my results.

5 Results and Potential Mechanisms

5.1 Doubly Robust Estimates of the ATT

Figure 4 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. Estimates become negative and significantly different from zero during month two after treatment, and remain so for the next 34 months. My identifying assumption of parallel trends between groups with the same pre-treatment characteristics appears satisfied; treatment effects at event times -5 through -1 are extremely close to 0 and are not significantly different from 0[11]. Crime incident counts in the vicinity of treated properties and the vicinity of untreated properties only begin to change relative to each other after treatment occurs.

My point estimate of the average post-treatment effect of eviction is -3.77 crimes with a standard error of about 1.0. The point estimate is 3.89 percent of the mean number of crimes per month that occurred within 500 meters of a property in 2017. It is 1.92 percent of the mean change in crimes that occurred within 500 meters of a property between 2017 and 2019.

I next assess whether there are hetereogeneous treatment effects across subgroups of properties in my sample. For each subsample I examine, I produce doubly robust estimates using the same 15 covariates listed in panels A through D of Figure 2. I take the additional step of aggregating the

[11] Confidence intervals around my pre-treatment estimates of the ATT are wide and include values between 2 and -2. The fact that pre-trends are not significantly different from zero does not rule out violations of assumption 2b. I am currently implementing a Python version of the HonestDiD R package. Once finished, I hope to apply the recommendations of Rambachan and Roth (2023) to more credibly assess the validity of the parallel trends assumption in my setting.

estimated post-treatment effects for easy comparison of treatment effects across subsamples. Figure 5 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 six subsamples described on the y-axis. While half of the average post-treatment ATTs are significantly different from zero, they do not appear to be significantly different from each other. In other words, I do not estimate significant differences in treatment effects across subsamples.

One explanation for the lack of significant differences in treatment effects across subsamples with such ostensibly different socioeconomic characteristics is that treatment and control properties alike are largely located in socioeconomically distressed areas. Consider, for instance, the bottom two coefficients plotted in Figure 5. It may be that differences between below median poverty and above median poverty properties in my sample are too small to drive differences in treatment effects between these two groups. It is plausible that a comparison of treatment effects estimated on my sample to those estimated on a sample of evictions in a wealthier or less diverse area would show reveal larger differences.

5.2 Mechanisms

In this section, I argue that the mechanism for the treatment effects I observe is the removal of individuals who are targets of, accessories to, and/or culprits of crime incidents from the immediate vicinity of the property. Three pieces of evidence support this hypothesis.

First, the steady increase in estimated treatment effects during the first three months after treatment is consistent with the timeline by which tenants are removed from properties. During months 0, 1, 2, and 3 relative to treatment, the magnitude of treatment effects steadily increases. This steady increase in estimated treatment effects during the first three months after treatment is consistent with the fact that an execution for possession remains valid for 90 days after its issuance and suggests that progressively more tenants are removed from units during the first three months after treatment. In contrast, treatment effect magnitudes at event times 4 and greater do not follow any clear pattern.

Second, restricting my dependent variable to a subset of crime incidents which perpetrators are unlikely to commit near their own homes results in small and insignificant treatment effects. If my hypothesized mechanism is correct—if the effects I observe are the result of the removal of individuals who interact with crime in some capacity—then I should estimate weaker and less significant effects of eviction on crime incidents which perpetrators tend to commit outside the immediate vicinity of their homes. In other words, if soon-to-be evicted tenants commit these forms of crimes, they are likely to do so outside of a 500 meter radius of their homes, so we should not estimate significant treatment effects. I select shoplifting, motor vehicle accidents, and motor vehicle towing as crime incidents that meet the criteria described. To further validate my choice of crime incidents, I produce Table 5. Table 5 compares the poverty rates of the census tracts in which each of the crimes I choose occur to the poverty rates of the census tracts in which the properties in my sample are located. Table 5 shows that these crimes tend to occur in wealthier areas than eviction filings, further validating the use of these crimes as a "placebo" outcome variable.

Figure 6 shows that eviction has small and insignificant treatment effects on shoplifting, motor vehicle accidents, and motor vehicle towing. Point estimates are not uniformly positive or negative. The average post-treatment effect is 0.15 with a standard error of 0.14. The lack of a treatment effect of eviction on crimes which evicted individuals would be unlikely to commit near their own homes provides strong evidence for the mechanism I hypothesize.

Third, treatment effects are driven by evictions that were concluded during warm months. Post-treatment impacts of eviction are uniformly smaller in magnitude during cold months, as shown in Figure 7. A large body of research has found evidence of significant seasonal increases in crime during warmer months (Lauritsen, 2014). The fact that treatment effects are largest during warm months is consistent with the hypothesis that evicted tenants interact with crime to a greater extent than the tenants that replace them.

5.3 Ruling Out Alternative Explanations

5.3.1 Pandemic-Driven Changes in Crime

Another explanation for my results is that the pandemic drove differences in crime trends between treatment and control properties. To address this possibility, I re-estimate treatment effects using a subsample containing only cases which concluded in April 2019 through September 2019. Then, I report estimated treatment effects for the first six months after treatment. Since this sample contains only cases which concluded prior to October 2019, each of these six post-treatment estimates of the ATT is an estimate of pre-pandemic effects. In Figure **??**, I display these first six post-treatment estimates in the plot on the left. In the plot on the right, I display the first six post-treatment estimates shown in Figure 4 for the purpose of comparison. While confidence intervals are wider, point estimates calculated on the subsample are actually larger in magnitude than those estimated on the entire sample, increasing confidence that the results are not driven by pandemic-related shifts in crime trends.

5.3.2 Correlated Changes in Socioeconomic Characteristics

My 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. I argue that such changes in socioeconomic characteristics are unlikely for two reasons. First, significant change in the socioeconomic character of neighborhoods often takes several years—far longer than the three years post-treatment during which I present estimates of treatment effects. Second, as discussed in section 3.4, differences in the socioeconomic characteristics of properties' surroundings are larger between my sample and the city of Boston as a whole than they are between my treatment group and my control group. These differences become even smaller after re-weighting during doubly robust estimation. It is implausible that treatment and control properties which 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.

5.3.3 Improvements in Social Cohesiveness

Another plausible mechanism is that eviction reduces crime by removing individuals who socially destabilize communities in ways that make crime more common without themselves being perpetrators or targets of crime. The idea that there is a relationship between eviction and social stability is not a new one (Semenza et al., 2022). Under this mechanism, community ties are rebuilt once those individuals individuals are removed. The near-immediate appearance of treatment effects is inconsistent with this mechanism; it is unlikely that social ties in neighborhoods are re-strengthened on so short a time frame.

Also, it is highly likely that individuals who socially destabilize communities are culprits or targets of crime as well. Together, these points suggest that eviction does not reduce crime by improving the social environment around a property.

5.3.4 Reverse Causality

In this subsection, I address the threat of reverse causality: that increases in crime lead landlords to file evictions. The lack of a visible pre-trend in Figure 4 provides evidence against this possibility. As a second check, I apply my doubly robust estimator to calculate treatment effects of eviction on crime, but using the month of case filing instead of the latest docket month as the date of treatment. Figure 8 presents the results of this procedure. Pre-filing treatment effects are not significantly different from zero. All pre-filing point estimates of treatment effects are in fact negative, which is inconsistent with the hypothesis that pre-filing crime increases drive landlords to evict tenants.

5.3.5 Robustness

In Figure 9, I show that the significance of my treatment effects is robust to altering the size of the radius that I draw around each property during merge. To produce Figure 9, I re-merge my dataset using the same sample of evictions and the same sample of crimes, but create a smaller radius around each property of 250 meters. I estimate treatment effects of eviction on crime incidents within 250 meters and show event study-aggregated ATTs in Figure 9. Reassuringly, while the magnitudes of the estimated treatment effects are smaller, they are largely significant for three years after treatment.

6 Conclusion

This paper provides the first quasi-experimental evidence on the effect of eviction on crime in the immediate vicinity of a property. Evidence suggests that eviction removes individuals connected to crime from local communities, leading to a sustained decrease in crime incidence. This implies that while eviction may be destructive to its victims, it may also improve the quality of neighborhoods for tenants who are not evicted and non-renters.

Questions about the external validity of these results remain. My study is restricted to the city of Boston. Massachusetts law guarantees tenants many protections that many other states do not. For example, Massachusetts landlords may not charge tenants late fees on rent unless payment is over 30 days delayed; this is twice the minimum period of the next highest state (General Court of the Commonwealth of Massachusetts). In this light, the propensity of evicted tenants in Massachusetts to commit crimes is likely different than in, say, Louisiana, where tenant protections are much weaker. I leave exploration of the effects of eviction on local communities in other states as an area for future research.

7 Tables

Restriction	Observations
Case Filed in Boston	6,856
Non-missing latest docket date	6,072
Non-missing property address	6,071
Case concluded before April 2020	3,352
Case not resolved through mediation	1,701
Succesfully scraped judgment	1,689

Table 1: Sample Construction

Notes: This table shows how the number of eviction cases in my sample changes as sample restrictions are applied. The final row of the "Observations" column gives the number of eviction cases in my final sample.

Panel	Variable	Mean	S.D.	N
Panel A: Pre-treatment Outcomes	Change in Crime Incidents, 2017-2019	-152.24	217.71	1,689
	Total Crime Incidents, 2017	1,164.18	714.31	1,689
Panel B: Census Tract Characteristics	Bachelor's degree, 2010	0.32	0.22	1,689
	Job density, 2013	16,714.10	43,368.36	1,689
	Median household income, 2016	46,250.26	24,935.09	1,689
	Poverty rate, 2010	0.29	0.15	1,689
	Population density, 2010	23,449.59	14,401.77	1,689
	Share white, 2010	0.31	0.27	1,689
Panel C: Case Initiation	Filing for cause	0.11	0.32	1,689
	Filing without cause	0.04	0.20	1,689
	Filing for nonpayment	0.76	0.43	1,689
Panel D: Defendant and Plaintiff Characteristics	Defendant has attorney	0.03	0.18	1,689
	Plaintiff has attorney	0.91	0.29	1,689
	Defendant is entity	0.02	0.15	1,689
	Plaintiff is entity	0.84	0.36	1,689
Panel E: Case Resolution	Case duration	20.10	25.06	1,595
	Judgment by default	0.46	0.50	1,689
	Case dismissed	0.43	0.50	1,689
	Case heard	0.05	0.21	1,689
	Money judgment	1,488.32	3,429.81	1,689

Table 2: Summary Statistics

Notes: This table provides descriptive statistics for the 1,689 eviction cases in my dataset and the properties they dispute.

Last Docket Date	Cases Won By Defendant	Cases Won By Plaintiff	Portion of All Cases
All Months	734	955	1.00
2019-04	1	5	0.00
2019-05	1	7	0.00
2019-06	51	24	0.04
2019-07	64	47	0.07
2019-08	68	110	0.11
2019-09	76	101	0.10
2019-10	90	98	0.11
2019-11	62	76	0.08
2019-12	67	75	0.08
2020-01	78	126	0.12
2020-02	84	133	0.13
2020-03	92	153	0.15

Table 3: Case Outcomes and Dates of Conclusion

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

	(1)	(2)	(3)	(4)	(5)	(6)
			Difference in Cases Won by Defendant			
		Cases Won by Plaintiff	Unweighted	p	Weighted	p
Panel A	Total Crime Incidents, 2017	1,151.91	-28.23	0.42	-51.17	0.15
	Change in Crime Incidents, 2017-2019	-155.35	-7.17	0.50	6.18	0.57
Panel B	Bachelor's degree, 2010	0.32	0.00	0.81	-0.01	0.22
	Job density, 2013	16,161.03	-1,272.66	0.55	-775.79	0.72
	Median household income, 2016	47,553.06	2,997.87	0.01	-1,838.01	0.11
	Poverty rate, 2010	0.28	-0.02	0.00	-0.01	0.08
	Population density, 2010	23,320.19	-297.77	0.67	-1,017.97	0.16
	Share white, 2010	0.32	0.01	0.35	-0.01	0.33
Panel C	Filing for cause	0.13	0.04	0.01	-0.00	0.85
	Filing without cause	0.03	-0.02	0.03	-0.00	0.78
	Filing for nonpayment	0.72	-0.11	0.00	-0.04	0.06
Panel D	Defendant has attorney	0.01	-0.05	0.00	-0.00	0.62
	Plaintiff has attorney	0.88	-0.05	0.00	-0.04	0.00
	Defendant is entity	0.02	-0.01	0.24	-0.00	0.85
	Plaintiff is entity	0.81	-0.08	0.00	-0.04	0.03

Table 4: Balance Tests

Notes: This table summarizes differences in pre-treatment characteristics between the treatment group and the control group, before and after re-weighting. Column (2) reports means of each pre-treatment characteristic in the treatment group. Column (3) reports unweighted differences in each pre-treatment characteristic between the treatment group and the control group. To produce the values in column (3), I regress each pre-treatment characteristic on a treatment indicator and report the point estimate of the coefficient. I test the hypothesis that each of these coefficients are equal to 0 and report the corresponding p-values in column (4). In column (5), I regress each pre-treatment characteristic on a treatment indicator and propensity scores estimated using a logistic regression propensity score model that includes all pre-treatment characteristics. In column (6), I test the hypothesis that each of these coefficients are equal to 0 and report the corresponding p-values.

Event	Median Poverty Rate of Census Tract Where Event Occurred	75th Percentile	95th Percentile
Shoplifting	0.16	0.26	0.42
Motor Vehicle Accident	0.18	0.29	0.44
Motor Vehicle Towing	0.18	0.27	0.44
Eviction Filing	0.26	0.39	0.57

Table 5: Poverty Rates Around Select Crimes and Eviction Filings

Notes: This table provides information about levels of poverty in census tracts where shoplifting, motor vehicle accidents, motor vehicle towings, and eviction filings occur.

8 Figures

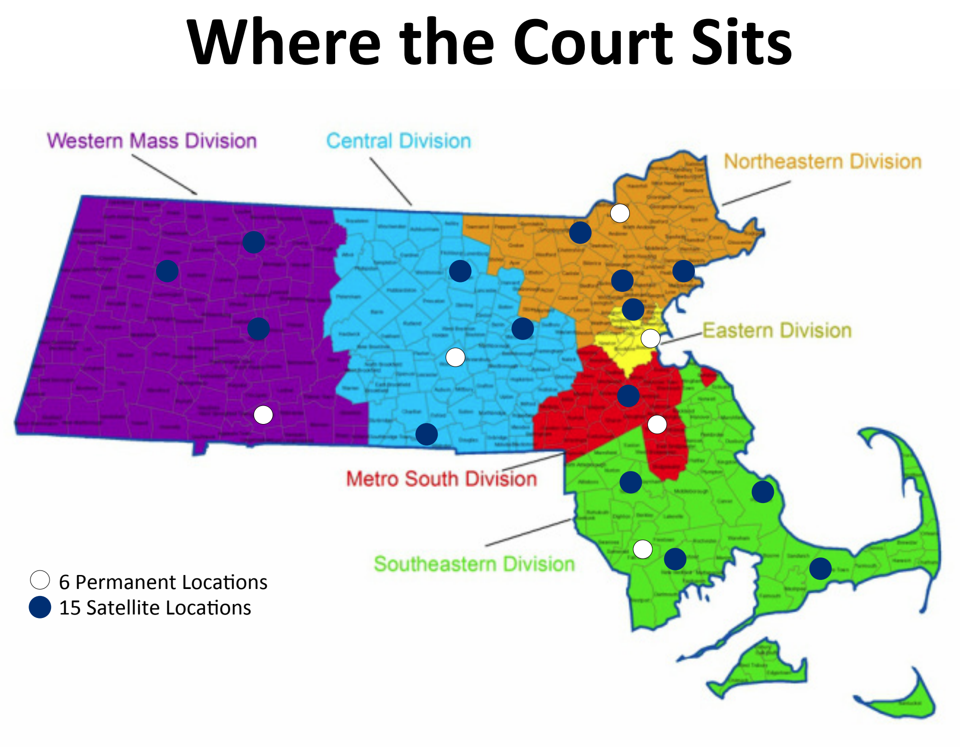

Figure 1: Divisions of the Massachusetts Housing Court (Housing Court 4 All)

Notes: This figure shows the divisions of the Massachusetts Housing Court and the towns in Massachusetts which fall under the jurisdiction of each division.

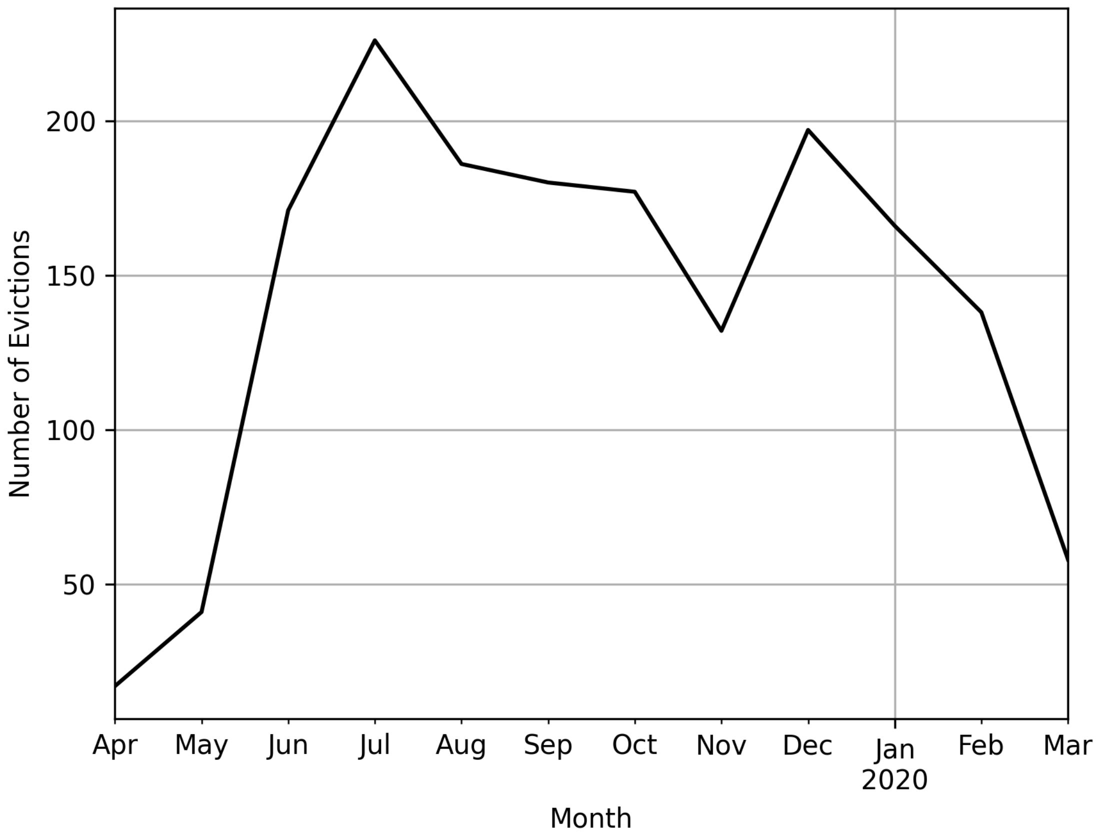

Figure 2: Filings Over Time

Notes: This figure plots the number of eviction cases which are filed during each month in my sample between April 2019 and March 2020.

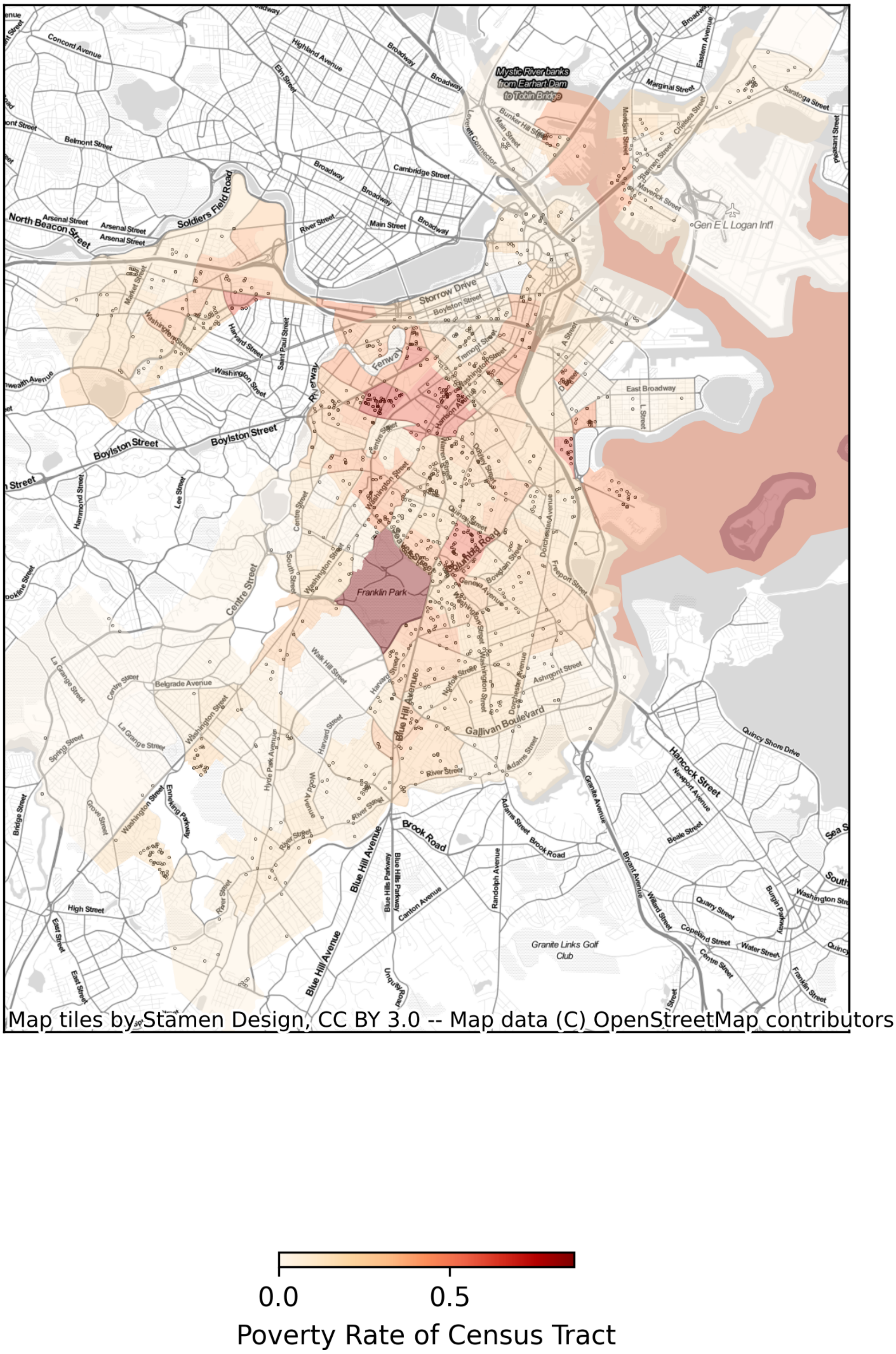

Figure 3: Spatial Incidence of Eviction

Notes: This figure plots the locations of properties disputed in eviction cases in my sample. Census tracts are colored according to poverty rate; darker colors correspond to census tracts with higher rates of poverty.

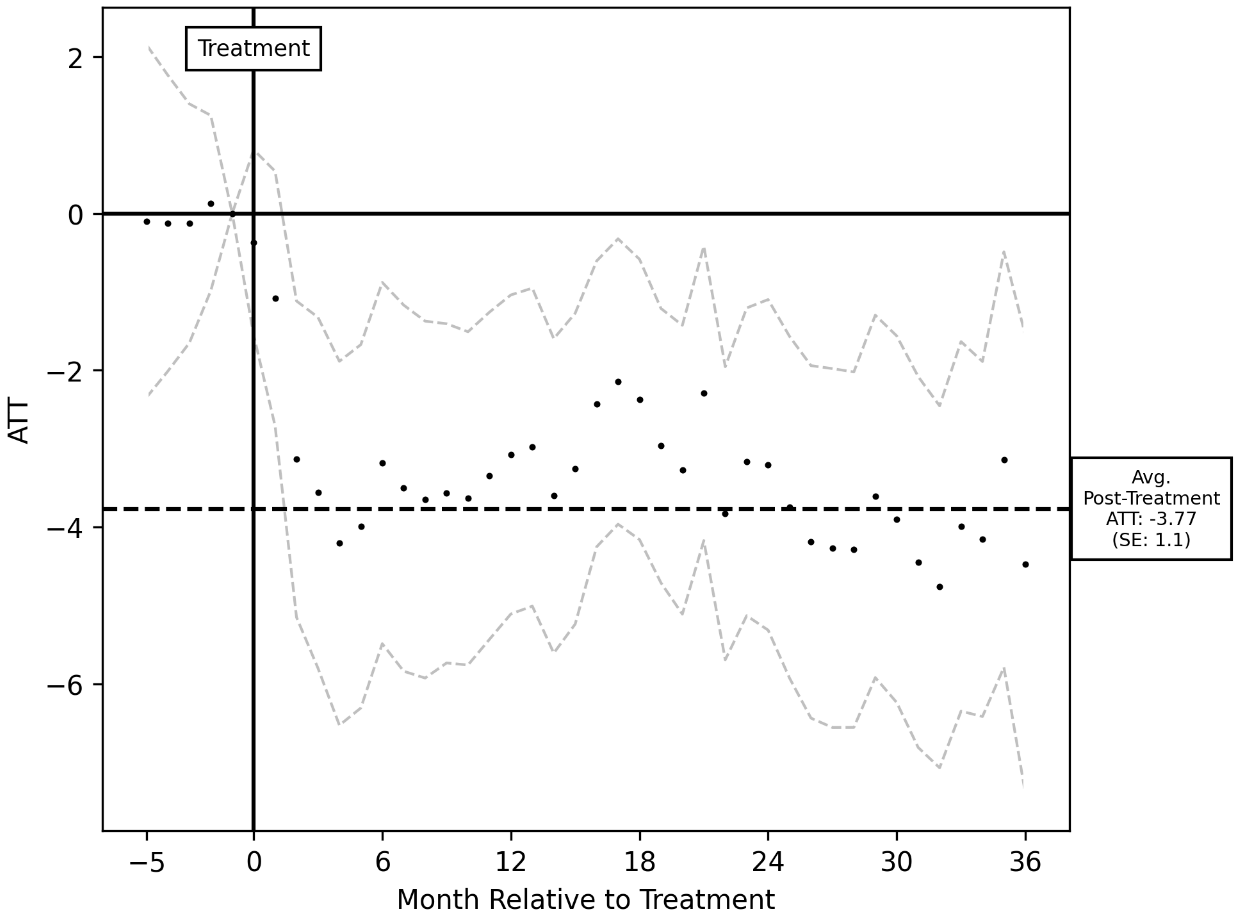

Figure 4: Doubly Robust Event-Study Estimates of ATT

Notes: This figure plots doubly robust estimates of treatment effects aggregated by event-time, estimated on the entire sample. Treatment effects on crime incident responses 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).

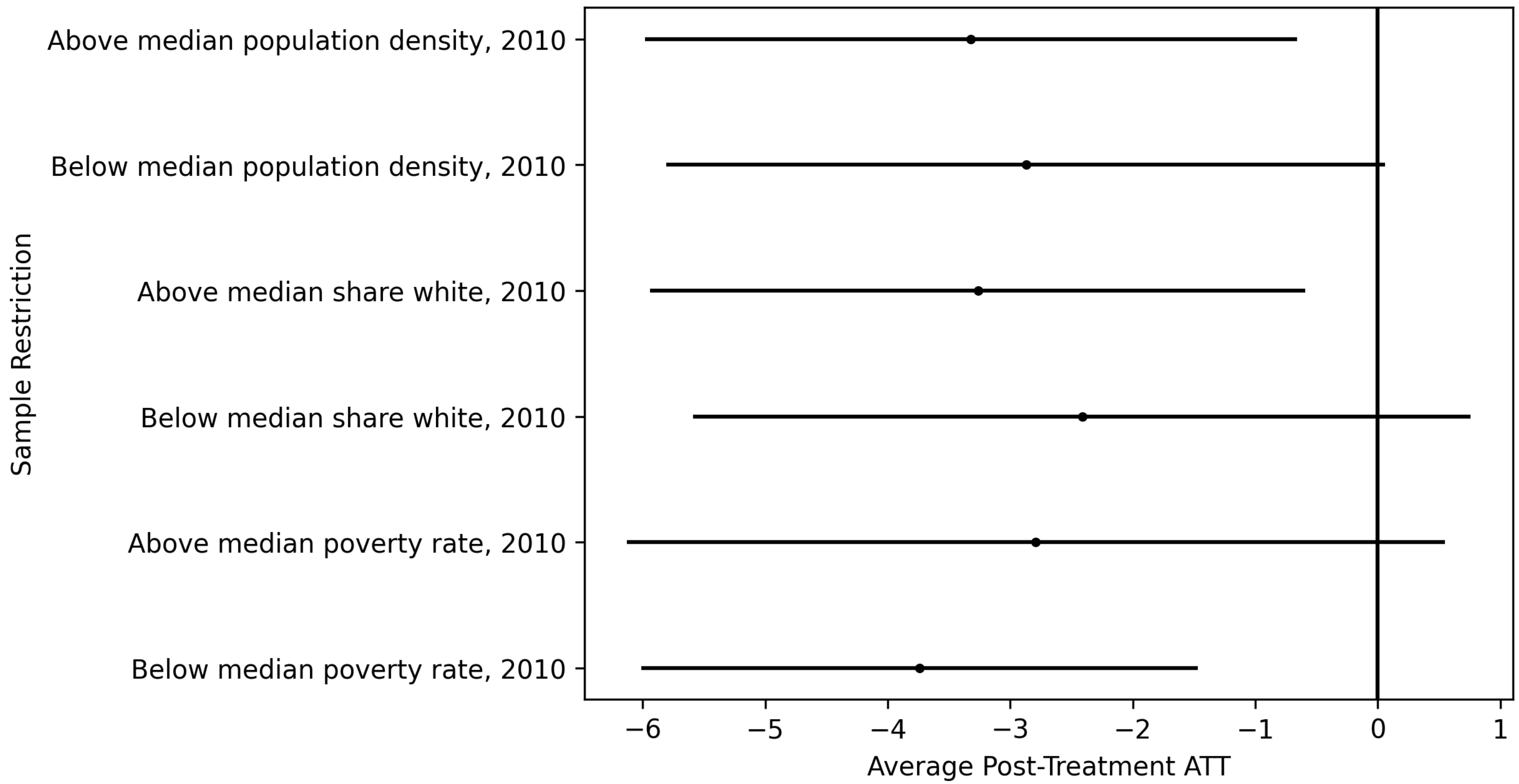

Figure 5: Heterogeneous Treatment Effects

Notes: This figure displays aggregated doubly robust post-treatment estimates of the effect of eviction across different subsets of my data. The y-axis indicates the different splits on which I produce estimates. Treatment effects on crime incident responses are on the x-axis. Each black dot represents a point estimate of the post-treatment effects of eviction on a different sample; 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).

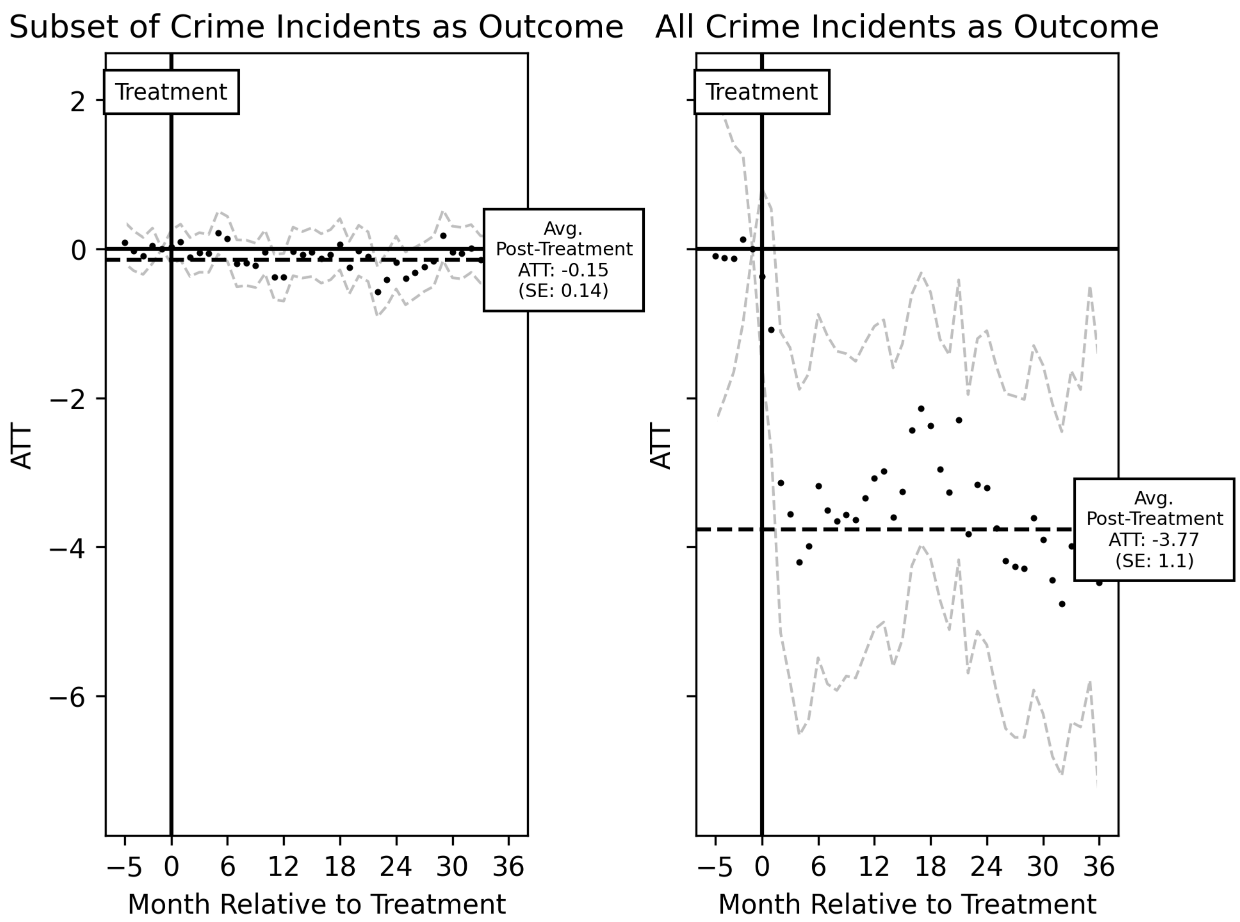

Figure 6: Doubly Robust Event-Study Estimates of the Effect of Eviction on Shoplifting, Motor Vehicle Accidents, and Motor Vehicle Towing

Notes: This figure plots doubly robust estimates of treatment effects on a subset of crime incident responses and on all crime incident responses, aggregated by event-time. Treatment effects on crime incident responses are on the y-axis. Treatment-relative month is reported on the x-axis. Treatment effects are estimated using the entire sample of properties. 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 plot on the left displays estimates of the effects of eviction on the subset of crimes, while the plot on the right displays the same results as Figure 4 for the purpose of comparison.

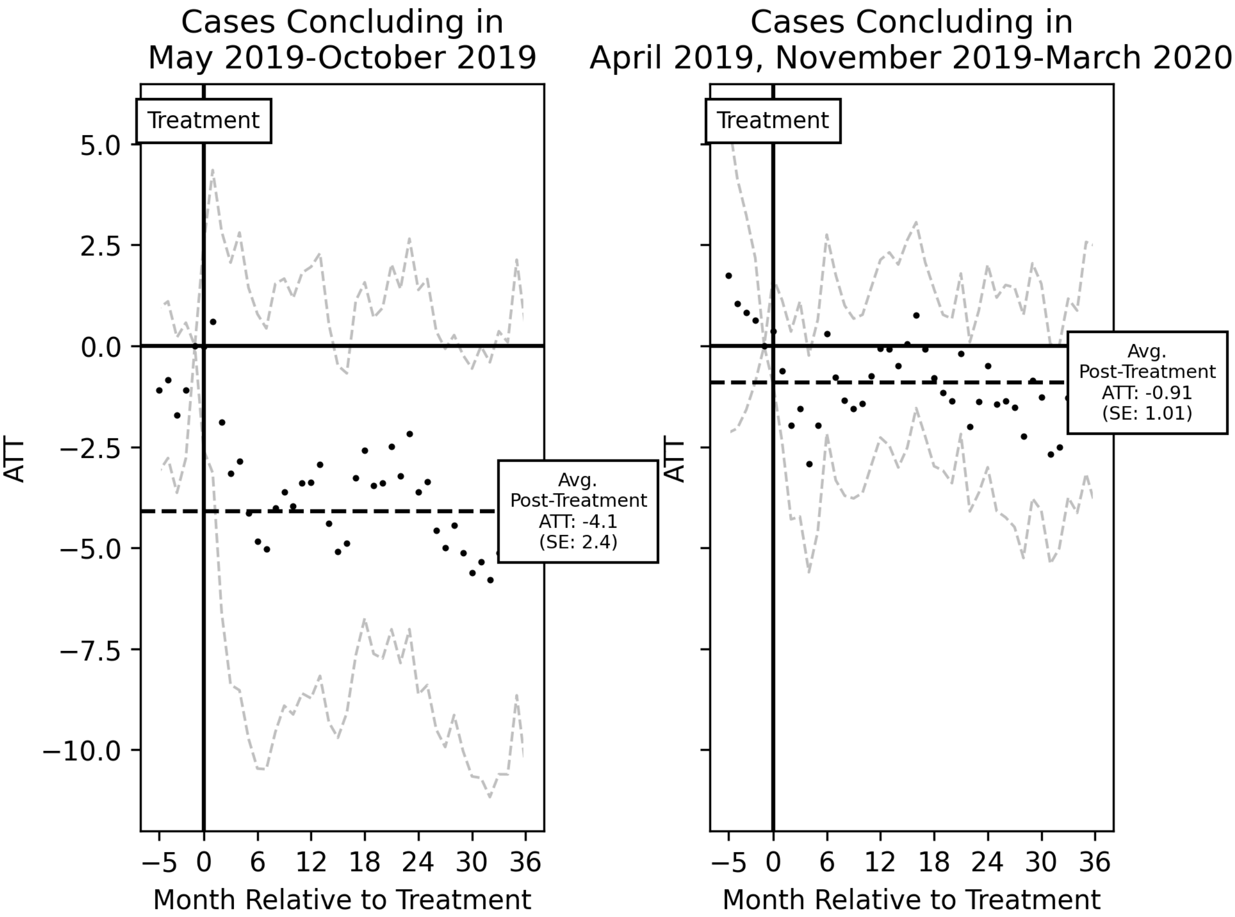

Figure 7: Doubly Robust Event-Study Estimates of ATT, Separately by Season

Notes: This figure plots doubly robust estimates of treatment effects aggregated by event-time, estimated separately on a subsample of cases concluding in warm months and a subsample of cases concluding in cold months. Treatment effects on crime incident responses 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).

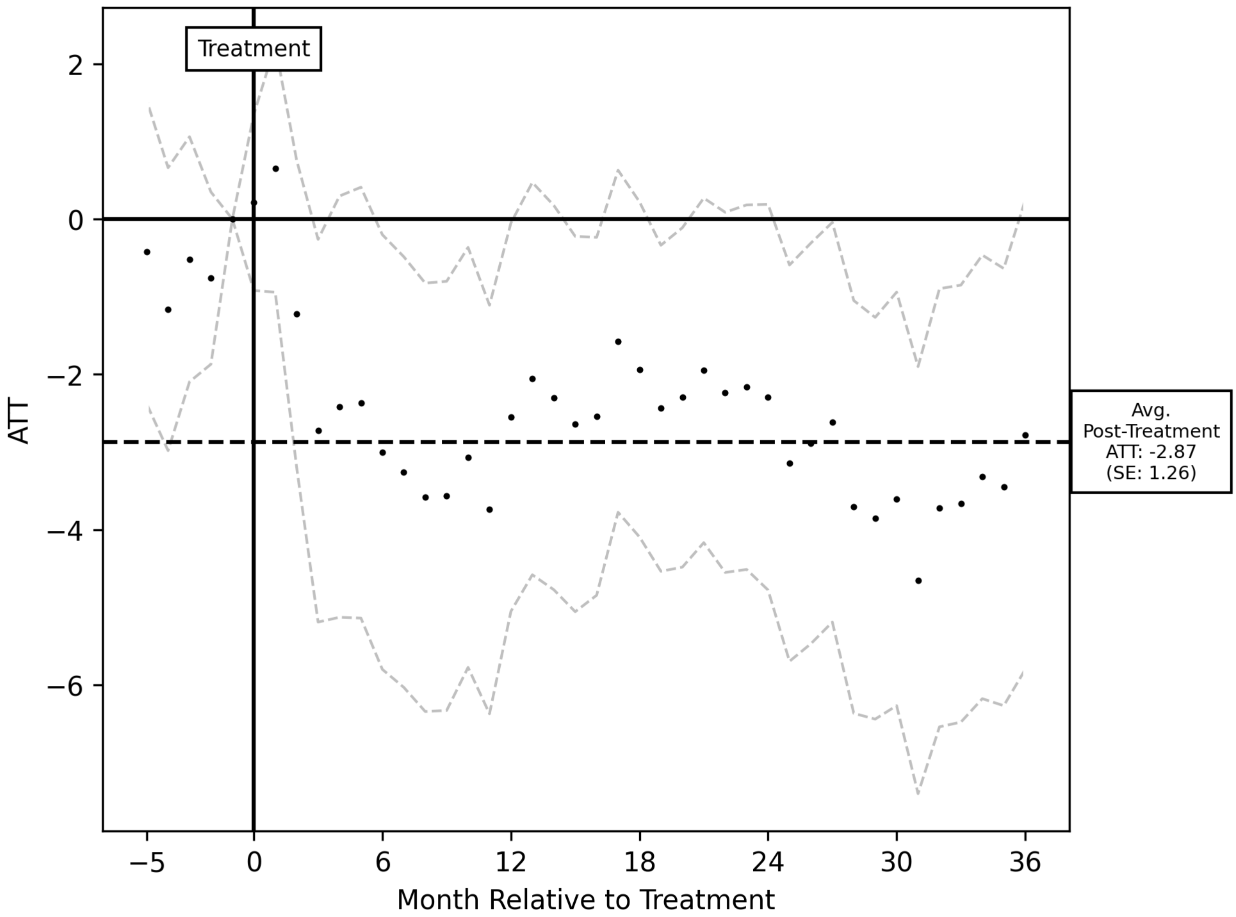

Figure 8: Doubly Robust Event-Study Estimates of ATT Using File Month as Treatment Date

Notes: This figure plots doubly robust estimates of treatment effects aggregated by event-time, estimated on the entire sample and using file month as the date of treatment. Treatment effects on crime incident responses 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).

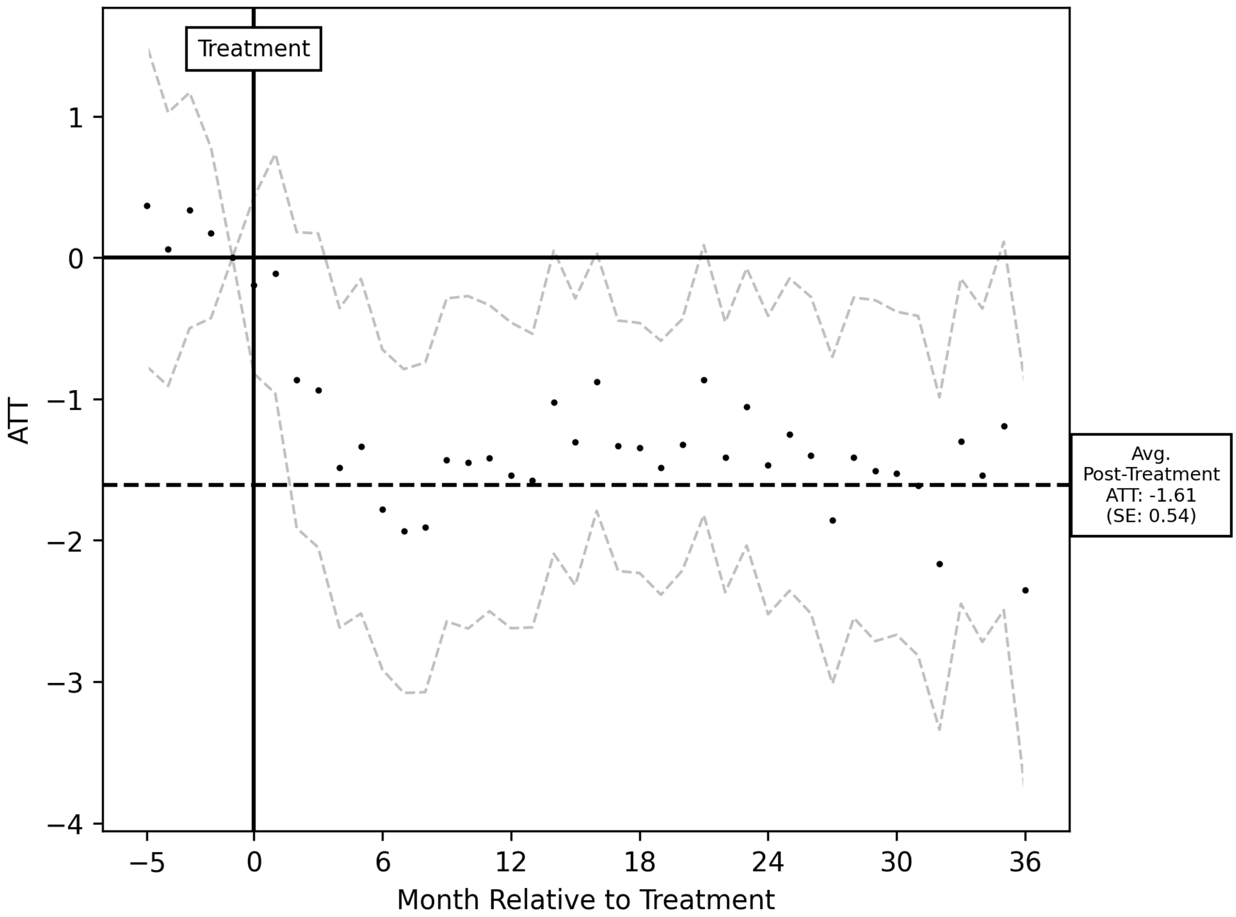

Figure 9: Doubly Robust Event-Study Estimates of ATT, Alternative Radius

Notes: This figure plots doubly robust estimates of treatment effects aggregated by event-time, estimated on the entire sample using 250 meters as the radius around each property instead of 500 meters. Treatment effects on crime incident responses 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).

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