

Eviction Reduction Policies

Do More Courts Reduce Defaults in LA County?*

Matthew Estes[†] October 22, 2025

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Abstract

Eviction is a leading cause of housing instability and eviction reduction is a policy priority. Yet many strategies to reduce evictions lack empirical footing and the effectiveness of any particular policy is likely to hinge on the mechanisms determining eviction outcomes. Empirical work quantifying how and how much different policies causally effect eviction outcomes is therefore needed. Using data on LA County eviction filings, this paper empirically examines how court-based procedural policies impact eviction. By exploiting an August 2017 change in court policy, this paper quantifies the extent to which changing the number of eviction courts causally effects default evictions via changes in tenant costs. The paper shows that the reform to the number of courts had limited effects on eviction outcomes. Aggregate treatment effect estimates are often insignificant, do not align with expectations, or imply small effect sizes across model specifications. The findings highlight the need for both short-term and long-term strategies to address the eviction and affordable housing crises.

Keywords: eviction, housing, court rules, empirical legal studies

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1 Introduction

The lack of affordable housing is a growing crisis across the United States. By some estimates, “tens of millions of families, across red and blue states, struggle with rent and home prices” (Dougherty, 2024). By U.S. Department of Housing and Urban Development (2024) metrics, more than half of renting households are cost-burdened: they spend more than 30% of their income on housing costs. And according to Pew Research Center (2024) polling, 69% of U.S. adults are now “very concerned” about rising housing costs.¹ Consequently, housing policy is an increasingly pressing priority for elected officials.²

One key consequence of the housing crisis is a growing number of evictions. Because a growing portion of household income is spent on rent, evictions are rising.³ The repercussions for evicted tenants are significant: reduced credit access; reduced earnings; increased hospital visits; lower educational performance; and increased housing instability, including homelessness (Collinson et al., 2024b; Desmond, 2012, 2017; Desmond and Kimbro, 2015; Meyer et al., 2025; Lens et al., 2020). Because eviction is linked to these varied and pernicious social ills, understanding the causes and consequences of eviction is an important goal for researchers. Yet many factors impact eviction outcomes, so understanding which policies deliver temporary versus lasting relief to renters is empirically challenging. Consequently, carefully tailoring eviction and housing policy is an important but practically difficult endeavor.

The focus in this paper is assessing available interventions for effectiveness at decreasing evictions, particularly *default* evictions. Default evictions occur when tenants don’t show up to housing court and landlords win *by default*. Default is not easily reversed and accelerates the process of lock out. Although some level of default is inevitable in any court system, eliminating unnecessary or unfair defaults is a key part of the access-to-justice literature (see, e.g., Greiner and Matthews (2015)). Optimally tailoring the default rate matters for efficiency and welfare reasons. Zero default may strain court resources with little upside if benefits accrue only to tenants with a persistent inability-to-pay (Abramson, 2021). On the other hand, landlords

¹The impact is felt unevenly across demographic groups: for instance, younger renters (18-29) are particularly likely (55%) to say “the availability of affordable housing is a major problem in their local community.” Pew Research Center (2022).

²In California, for example, Governor Newsom has signed into law numerous housing reforms aimed at reducing rent and increasing housing supply (Office of Governor Gavin Newsom, 2024). See also Office of Mayor Karen Bass (2022).

³The most common reason for eviction is non-payment of rent (Desmond, 2017; Los Angeles City Controller’s Office, 2025).

assured default wins may hastily (or improperly) evict tenants with legal or practical grounds for staying in the rental unit.⁴ In equilibrium, eviction protections tend to increase tenant rents, further complicating welfare analysis.⁵

These issues therefore necessitate empirical analysis to understand how tenants, landlords, and courts respond to changes in the housing policy apparatus. This paper begins by mapping the empirical terrain, reviewing legal and economic studies on the effectiveness of various interventions at reducing eviction and defaults in theory and in practice. There are three intervention approaches with prior empirical support: (1) representation-based approaches focused on providing tenants with legal resources at or before eviction court, (2) tenant cost approaches focused on reducing the cost of using or getting to court, and (3) market-based approaches aimed at increasing housing supply and reducing rents.

This paper next empirically studies how part of the eviction machine—the number of eviction courts—impacts default evictions via the tenant cost channel. I quantify how this procedural reform causally effect default evictions over time in Los Angeles County. LA County is the largest local trial court system in the U.S. and is acutely affected by housing and eviction issues.⁶ Using a staggered difference-in-differences (DID) design, I estimate how expanding the number of LA County eviction courts impacted eviction outcomes.

Specifically, the empirical study focuses on a court policy shock in LA County beginning in August 2017 which expanded the number of eviction courts from 8 to 11. The court expansion shocked tenants costs—measured by distance-to-court or commute times—for some but not all tenants. The policy shock resulted in some tenants facing increased costs (longer distance-to-court), some facing decreased costs (shorter distance-to-court), and some unaffected (no change in distance-to-court). Comparing these cohorts, I find mixed results from the reform. Although before-after comparisons show the observed number of defaults increased for those with

⁴If landlords face a legal rule which assures them of a tenant default, they may evict paying tenants for discriminatory or otherwise illegal reasons. *See infra* (discussing composition effects from procedural reforms). Similarly, landlords may be too quick to evict if tenants experience only temporary income shocks. In this case, society may have practical reasons to give tenants who are likely-to-repay a chance to do so before starting the eviction process.

⁵The impact across renters is largely due to an *ex ante* screening problem (Abramson, 2021): landlords don't know with certainty what "type" of tenant you are, so they increase rents to account for the possibility (and added cost) of evicting types that are unable-to-pay rent. In partial equilibrium, policy changes which impact the default probability may affect the total number of evictions in unexpected ways. *See infra* Section 2.

⁶*See, e.g., Lanzas (2024)*: "Los Angeles, the nation's second-largest city, consistently ranks amongst the least affordable housing markets and has one of the highest eviction rates."

increased costs and decreased for those with decreased costs, the DID analysis cannot rule out null effects under many model specifications. Additional robustness checks suggest that, in the aggregate, estimates are either insignificant, do not align with expectations, or are small relative to outcome variation.

The takeaway is that default judgments are persistent and resistant to policy reforms. Additional legal resources (more courts, more judges, more lawyers) or procedural reforms are not guaranteed to meaningfully halt broader legal and socioeconomic trends, at least in the case of eviction default and relatively modest legal reforms. This is part of an emerging set of findings that procedural legal interventions that increase legal resources, reduce litigant costs, or otherwise alter the form of the proceeding may have only limited observed effects in the aggregate.

This paper is also part of a series of works to determine the relative effectiveness of procedural versus structural housing reforms in combating eviction.⁷ The overarching research goal is understanding how different levers—court-based procedural policies and market-based structural reforms—can reduce evictions and housing instability for vulnerable populations. This paper contributes to the procedural half of the broader scholarly agenda to understand how to reduce evictions. But eviction scholars may need to reorient focus away from procedures and toward the structure of rental markets. In other words, legal procedure may simply patch fundamental and long-term problems in housing supply.

The remainder of the paper is structured as follows. [Section 2](#) reviews the empirical and theoretical findings from the law, economics, policy, sociology, and urban planning literatures, focusing on empirical studies of methods to reduce evictions generally and defaults specifically. [Section 3](#) reviews the eviction process and court assignment in LA County, describing the court assignment mechanism which is key to the empirical exercise. Next, in [Section 4](#), I briefly describe the collected data qualitatively and quantitatively. [Section 5](#) is the heart of the paper: I empirically test whether changes in court assignment policy reduced default evictions. Finally, I discuss implications for how policies may or may not reduce evictions in [Section 6](#).

⁷Because tenants often face a large resource disadvantage in eviction cases, procedural reforms aim to “level the playing field” for tenants in cases against better-resourced landlords. By contrast, because non-payment of rent is the primary reason for eviction, structural reforms tend to focus on reducing rent and housing prices by increasing the housing supply. See [Appendix A](#) for further discussion. See also [Teresa et al. \(2025\)](#) describing upstream and downstream eviction prevention policies.

2 Eviction Reduction Strategies

This section briefly reviews the main findings from a diverse, interdisciplinary eviction literature. I begin with the access-to-justice problem posed by default eviction judgments and possible solutions suggested by the literature. I then turn to detailing the emerging economic picture of the costs and benefits of procedural reforms: although strengthening eviction protections for tenants may give them time and/or improve case outcomes, the economic costs of eviction protections in equilibrium are less discussed. Finally, I note the extensive descriptive literature on eviction correlates, which sheds light on how eviction disproportionately affects vulnerable communities.

2.1 The Problem of Default & Possible Solutions

From an access-to-justice perspective, default judgments are a continuing problem across the law. Default judgments occur when plaintiffs obtain judgments against defendants because defendants do not contest the case. Because unrepresented defendants often default at higher rates, “such behavior presents an access to justice problem in that low- and middle-income people are benefiting from—and are in fact disproportionately suffering adverse consequences from—the formal administration of justice.” (Greiner and Matthews, 2015) Defaults are often considered, then, to be a legal harm in themselves (Greiner and Matthews, 2015):

“From a systemic point of view, court actors and judges consider high default rates a public harm. The administration of justice suffers when parties do not meet in an adversarial proceeding in order to resolve claims.”

Understanding the legal and non-legal factors that determine default is important to craft policy solutions. Although other procedural interventions—including mediation and/or technology policy—may merit further empirical study,⁸ I focus here on three legal factors inspired by the literature that partially explain the eviction and default phenomena: (1) lack of resources (esp. legal representation), (2) tenant costs of getting to court, and (3) market structure (esp. rental prices).

⁸For example, it is possible that mediation would “help parties reach better outcomes in eviction cases,” at least for smaller, non-corporate landlords (Bieretz et al., 2020; Armstrong and Ryan Jr, 2024). Another possibility is that technology—and artificial intelligence (AI) in particular—will level the informational playing field in eviction and reduce defaults. *But see Simshaw (2022)* for AI skepticism. The effectiveness of such possibilities may turn on what mechanisms most impact default and eviction outcomes.

2.1.1 Representation-Based Interventions (*Civil Gideon*)

Likely the most common policy suggestion to assist tenants in eviction court is to provide some form of legal representation. Because tenants are often unrepresented and eviction is a summary proceeding, some scholars believe evictions are “patently unfair” or even “violent” (Scherer, 2022, 2023). Empirical scholars, accordingly, have studied whether some forms of legal representation can remedy the disparities between landlord and tenant in eviction proceedings.

Empirical studies have used a variety of methods from different legal settings across the U.S. For example, Greiner et al. (2012,?) study the effects of legal representation interventions on eviction outcomes in a randomized controlled trial (RCT). These studies find mixed results. In Greiner et al. (2012), the authors find:

“no statistically significant evidence that [the offer] of a traditional attorney-client relationship, as compared to a referral to the [lawyer for the day] program, had a large (or any) effect on the likelihood that the occupant would retain possession; on the financial consequences of the dispute; on the judicial involvement in or attention to the litigation cases; or on any other outcome.”

In the other study (Greiner et al., 2013), the authors find that full legal representation following an initial intake and screening process mattered: 34% of treated occupants (who received full legal representation from a legal services staff attorney) versus 62% of control occupants (who received an instructional clinic only) lost possession of their units. Observational studies on the effectiveness of NYC’s access-to-counsel program are similar, finding that tenants are less likely to face adverse eviction judgments, especially those at the highest risk of possessory judgment (Cassidy and Currie, 2023).

Still, there are limits to the effectiveness of legal representation. For example, some eviction defenses may be unlikely to benefit tenants in practice.⁹ And the direct costs can be large: in 2024, NYC spent around \$179.5 million on tenant legal services, including the Universal Access to Counsel (UAC) program.¹⁰

⁹See, e.g., Summers (2020), finding that most represented tenants with likely meritorious warrants of habitability defenses do not win rent abatement.

¹⁰See https://www.nyc.gov/assets/hra/downloads/pdf/services/civiljustice/OCJ_Annual_Report_2024.pdf.

2.1.2 Tenant Costs & Spatial Considerations

Another factor from the literature that may cause defaults are spatial costs (see, e.g., [Clair et al. \(2025\)](#)). As [Prescott \(2017\)](#) puts it, the idea is that:

“[T]he inability to access justice is rooted in something more physical, more mundane: the many and varied costs of getting to and physically using a brick-and-mortar courthouse.”

Put differently, the idea is that factors which increase the costs or effort—longer commutes, for instance—for litigants getting to court tend to increase defaults.

Other studies confirm that the cost of getting to court is important in eviction cases. In Philadelphia, [Hoffman and Strezhnev \(2023\)](#) find that a one hour increase in travel time increases the probability of eviction default by 3.9 to 8.6 percentage points, arguing that distance-to-court causes default evictions. The authors find that this “transit effect” disappeared when courts made virtual accommodations in eviction proceedings during the COVID-19 pandemic.

In LA County, the specter of getting to court looms large. This is especially true for eviction proceedings and for vulnerable renter populations, as argued by tenants-plaintiffs in *Miles v. Wesley*, 801 F.3d 1060 (9th Cir. 2015):

[R]educing the number of courthouses handling unlawful detainer cases disproportionately impacts poor, disabled, and minority residents. ... [B]ecause individuals with disabilities and minorities are disproportionately renters who rely on public transportation, the closure of these court-rooms would have a disparate impact on these communities. [T]he importance of neighborhood court access is heightened in light of the expedited timeline of unlawful detainer actions, the fact that most low-income tenants are not represented by counsel, and the prospect that a default judgment could render a tenant homeless.

Shortly after *Miles*, the LA Superior Court system expanded the number of regional courthouses that process eviction cases. Whether the distance concerns cited in *Miles* continue to exist is studied here: using data before and after a court expansion policy shock, I test whether the number of courts significantly affects default outcomes.¹¹

¹¹Because the policy shock changed the tenant distance-to-court distribution for some but not all tenants, I study whether increased (resp. decreased) tenant costs increase (resp. decrease) default outcomes.

2.1.3 Market Structure, Housing Supply, and Reducing Rents

A final factor that is important but relatively neglected by eviction scholars is the structure of housing markets themselves. Although less discussed by eviction scholars, supply-and-demand arguments suggest that supply-side housing reforms should reduce the number of evictions *ceteris paribus* because they reduce rents (see generally Baum-Snow and Duranton (2025)). Indeed, as shown in Mast (2023), new market-rate construction of even more expensive rental housing improves affordability in the market for low-income housing.¹² The mechanism—called a “migration chain” by Mast (2023)—is simply (albeit provocatively) illustrated by the following graphic:

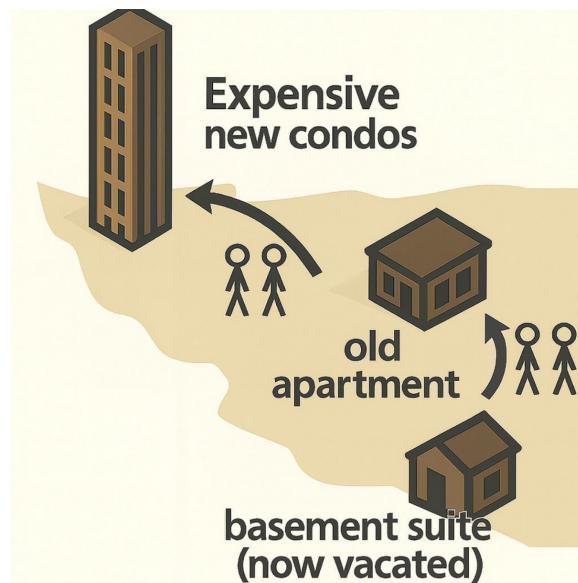


Figure 1. Migration Chains in Housing

Note: The figure is from a post on X by Michael Wiebe (dated July 28, 2025).

The intuition is that constructing new housing, even at the high-end of the market, may have spillover effects (Mast, 2023): “geographically localized shocks are likely to have ripple effects on other parts of the region.” Consequently, failure to construct new housing can impact renters at the lower end of the income distribution disproportionately, with rents increasing the most in the lowest-income zip codes.¹³

¹²See also Bratu et al. (2023), which provides “empirical evidence on how the moving chain mechanism unfolds in a European city”.

¹³Pew Research Ctr., *New Housing Slows Rent Growth Most for Older, More Affordable Units* (Jul. 31, 2025); see also *id.* at Figure 2 (“Rents in Older, Less Expensive Apartments Decreased Most in High-Supply Metropolitan Areas”). The Pew researchers’ explanation for the finding sounds in migration chain reasoning: “When not enough homes are built in high-income neighborhoods, people who would have lived in those neighborhoods can usually afford to move into middle-income

Although there are laws and regulations at all governing levels that affect the supply of housing, one set of rules of growing interest for reform are zoning and land use regulations.¹⁴ Such reforms could reduce evictions: because the proximate cause of an eviction filing is most often non-payment of rent, interventions which reduce rental prices should (on average) reduce the number of evictions holding constant the renter population.¹⁵ Eviction scholars to-date have not empirically tested these possibilities because:

“[A]lthough researchers have established the centrality of zoning policies to a wide array of social problems and conditions, they lack access to national longitudinal zoning and land use data, which would help researchers identify the causal effects of exclusionary zoning.” ([Mleczko and Desmond, 2023](#))

As new data becomes available, researchers will be able to test how housing and eviction outcomes respond to changes in zoning and land use regulations.¹⁶

2.2 The Costs and Benefits of Reform: Equilibrium Rents & Composition Effects

Although well-intended, the welfare effects of access-to-justice interventions can depend on the policy method chosen. In general, [Collinson et al. \(2024a\)](#) offer a model where legal assistance will “delay or prevent eviction,” which is the primary gain accruing to tenants facing eviction.¹⁷ Yet, legal assistance programs generate direct and indirect costs. On the direct side, some interventions may have high direct costs (e.g. right-to-counsel reforms) because they involve costly lawyering. And, on the indirect side, legal assistance to renters tends to raise rents in equilibrium: because landlords face a screening problem when deciding to whom they rent, reducing their ability

neighborhoods, and middle-income residents can usually afford to move into low-income neighborhoods, but residents of low-income neighborhoods have nowhere to turn.” *Id.*

¹⁴ See, e.g., [Klein and Thompson \(2025\)](#). But see [Elmendorf et al. \(2024\)](#), which notes that support for zoning liberalization is “much less stable than support for housing price controls and demand subsidies.”

¹⁵In general equilibrium, the effect could go either way because lower rents tend to induce immigration. See, e.g., [Kim and Lee \(2025\)](#).

¹⁶ See, e.g., [Rollet \(2025\)](#) for a study of urban redevelopment in response to zoning reforms.

¹⁷ Accord [Bell and Parchomovsky \(2004\)](#), stating that: “[I]t is clear that there is considerable value in stable ownership for both tenant and landlord, in particular, because tenants will often develop sentimental value for their leased premises, while landlords will frequently be better suited to extract value (due to specialized knowledge) from their premises than anyone else in the market.”

to quickly re-rent in non-payment situations will tend to increase market rents.¹⁸ Collinson et al. (2024a) finds that the right-to-counsel “cost to tenants due to higher rent prices more than offsets the benefits from better eviction court outcomes.”

Other studies find that procedural changes may backfire rather than improve access-to-justice in different ways. Consider, for example, Niblett and Yoon (2017), which studies outcomes from a Canadian policy shock which increased the recoverable amount in small claims cases. The change was meaningful because the informal small claims court was less expensive than Ontario Superior Court. The authors found that plaintiff suits did not increase significantly, but there were *compositional* effects. Specifically, they found that lower dollar claims (those less than the previous cap \$10,000) decreased following the jurisdictional change (see Figure 2). The authors conclude that access-to-justice efforts can have unintended regressive effects, find that evidence suggesting court congestion is driving the regressive effect is mixed, and note that the optimal structure of access-to-justice interventions may depend on the court design perspective.¹⁹

To fully understand the welfare effects of eviction reduction policies, a necessary ingredient is credible evidence for how tenants, landlords, and markets react. As noted by Collinson et al. (2024a), quasi-experimental evidence of legal assistance interventions is difficult because “it is often not clear how to choose a reasonable comparison group.” One possible strategy, Collinson et al. (2024a) note, is leveraging a difference-in-differences design for nearby zip codes treated at different times. This paper similarly uses a difference-in-differences strategy by exploiting quasi-experimental variation stemming from changes in distance-to-court due to a court policy shock.

2.3 The Correlates of Eviction

Finally, this paper also contributes to the correlational research on factors affecting eviction outcomes (including default), such as individual tenant and landlords characteristics. Studies in this vein look at many characteristics correlated with eviction, including: demographic and income correlates of eviction (Desmond, 2017, 2012; Desmond and Kimbro, 2015), including race (Summers and Steil, 2025) and gender

¹⁸Landlords could also respond by increasing screening efforts or reducing maintenance costs, but Collinson et al. (2024a) find little evidence for this.

¹⁹According to Niblett and Yoon (2017): “While economists and legal scholars have looked at the optimal structure of courts from the perspective of appeals, error correction, and *ex ante* rule setting, few have looked at the question of the optimal structure of courts from the perspective of *ex post* dispute resolution for the purposes of access to justice.”

(Buhler and MacLean, 2024); neighborhood and spatial correlates with eviction (Lens et al., 2020); and other case characteristics related to eviction and default (Larson, 2006), including urban-rural considerations (Armstrong and Ryan Jr, 2024). Below, I detail descriptive findings, including whether observable demographic factors are balanced across courthouses and whether they change sharply or not near the 2017 reform.

3 Eviction Process & Institutional Background

This section gives a brief overview of the typical eviction case in LA County. First, I describe the standard timeline for eviction cases, as summarized in Estes and Nelson (2025). Next, I explain how eviction cases are assigned to courthouses and how the number of eviction courts evolved over time. It is the change in eviction court assignment policy over time which is the key quasi-experimental variation I utilize for causal inference.

3.1 Eviction Process

The eviction process begins when a landlord serves an eviction notice on a tenant, most of which are for non-payment of rent.²⁰ Eviction notices give tenants a few days to “cure” their lease breach by paying unpaid rent to the landlord,²¹ although some landlords may permit informal arrangements for repaying rental arrears (such as negotiated payment plans outside of formal court proceedings). If the tenant does not cure non-payment, the landlord may then initiate an eviction by filing an eviction (unlawful detainer) lawsuit.²²

After being notified of an eviction proceeding initiated against them,²³ tenants must file an Answer²⁴ with the court.²⁵ If tenants do not appear to file an Answer

²⁰In LA City, over 96% of the notices in 2023 were for non-payment of rent (Los Angeles City Controller’s Office, 2025).

²¹The amount of unpaid rent and number of eviction notices vary at the zip-code and building levels. Lens et al. (2020) discuss spatial autocorrelation in eviction variables.

²²In the LA Superior Court system the filing costs \$240-\$385. See Los Angeles Superior Court, *Civil Fee Schedule* (2024) (nos. 11 and 14).

²³Tenants are considered notified after being served the Summons and Complaint forms.

²⁴In LA County, tenants pre-2025 had 5 days to file the Answer. The five days do not include weekends or holidays. Additionally, tenants may have longer to respond if they are improperly served. But see Assembly Bill 2347, which as of Jan 2025 gives tenants 10 days to respond.

²⁵The tenant is not supposed to mail the Answer, as they will default if the Answer doesn’t arrive. The official self-help page for California Courts strongly recommends against mailing the Answer, and instead says you should show up to file the Answer at the relevant court. See [https:](https://)

by the court-mandated deadline, landlords may petition the court to enter a default judgment against the tenant. Otherwise, after an Answer is filed, the court sets a trial date. Tenants who do not appear at trial will also receive a default judgment, but if they appear there is a judgment on the merits. The judgment typically awards landlords past due rent if they win at trial.

Following the court-issued judgment, the landlord may enforce the judgment by obtaining a writ of execution. The writ gives the sheriff permission to lock the tenant out of the premises. After obtaining the writ, the sheriff will serve the tenant a Notice to Vacate, which gives the tenant some number of days to move out.²⁶ After receipt of the Notice to Vacate, the sheriff will change the locks, forcing the tenant out of the residence. The typical process is represented graphically below in Figure 2.

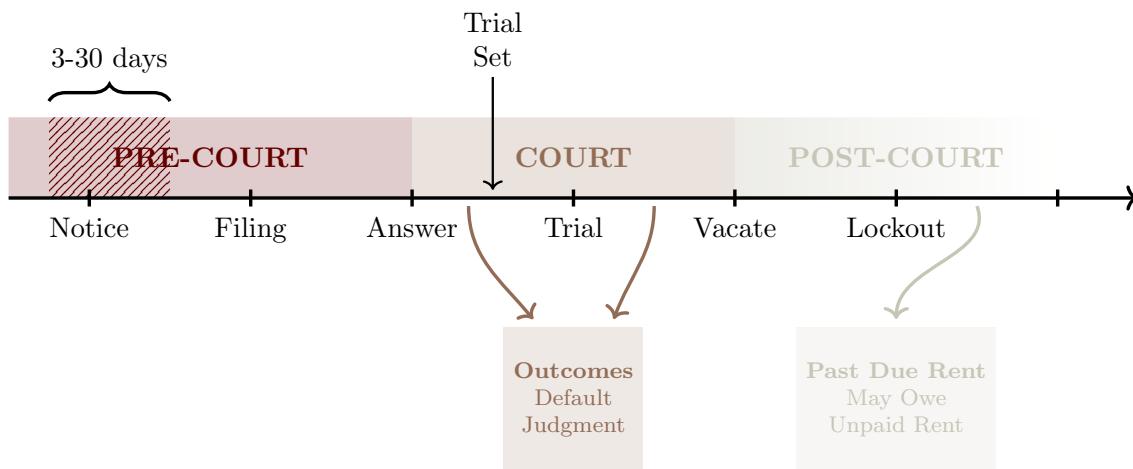


Figure 2. Eviction Timeline

Note: This figure is reproduced from Estes and Nelson (2025).

This eviction timeline here may not reflect the exact process for every eviction case, but it gives a rough picture of the steps and time pressures tenants face once an eviction notice has been served.²⁷ With the timeline of typical events specified, I now turn to explaining the court assignment procedure.

²⁶[//selfhelp.courts.ca.gov/eviction-tenant/respond-file](http://selfhelp.courts.ca.gov/eviction-tenant/respond-file). Filing fees vary over time, but in LA County the 2024 fee in eviction actions where the contested amount of rent is less than \$12,500 is \$225. See Los Angeles Superior Court, *Civil Fee Schedule* (2024) (no. 15). But note that there are fee waiver applications available.

²⁷In LA County, tenants formally have five days to move out after receipt of the Notice to Vacate.

²⁷The eviction timeline outlined above omits, for instance, any special motions that may arise. Eviction moratoria and other COVID-19 protections also altered the 2020–2022 process, e.g., giving tenants more time or limiting the landlord's ability to force the tenant to vacate.

3.2 LA County Eviction Courts: Assignment Rule

According to LA County court rules,²⁸ eviction cases are assigned to courthouses based on a unique spatial mechanism. Under the rules, eviction cases:

“must be filed in the courthouse serving the location and proper United States Postal Service zip code of the property in dispute using the Zip Code Table for [Eviction] cases.”

Since late 2017, the eleven courthouses where eviction cases are filed include: Chatsworth, Compton, Governor George Deukmejian (Long Beach), Inglewood, Michael Antonovich Antelope Valley, Norwalk, Pasadena, Santa Monica, Stanley Mosk, Van Nuys East, and West Covina. [Table 1](#) below illustrates how the courthouse assignment procedure works for the first few zip codes in Los Angeles County.

Table 1. Zip Code Table for Eviction Cases

Zip Code	City/Neighborhood	Modifier	Courthouse
90001	FLORENCE		Stanley Mosk
90001	HUNTINGTON PARK		Norwalk
90001	LOS ANGELES		Stanley Mosk
90002	FLORENCE		Compton
90002	LOS ANGELES		Compton
90002	LYNWOOD		Norwalk
90002	WATTS		Compton
90003	LOS ANGELES	North of Manchester	Stanley Mosk
90003	LOS ANGELES	South of Manchester	Compton
90004	LOS ANGELES		Stanley Mosk

Generally, the city-zip code pairs completely determine the assigned courthouse. However, in some zip codes the assignment is further determined relative to a particular street. For example, in [Table 1](#) above, eviction cases arising in the 90003 zip code are assigned to the Stanley Mosk or Compton Courthouse if the tenant’s address is north or south of Manchester Avenue, respectively.

The full assignment map for LA County is shown in the left panel of [Figure 3](#). The court districts differ in the land area they cover, but all courts—except for the Stanley Mosk courthouse in downtown Los Angeles—hear roughly the same number of cases each year. This map has been in effect since 2017, but prior to 2017 the

²⁸LASC Local Rule 2.3(a)(2)

number of courthouse districts fluctuated due to local budget constraints.²⁹ This is shown in the right panel of Figure 3.

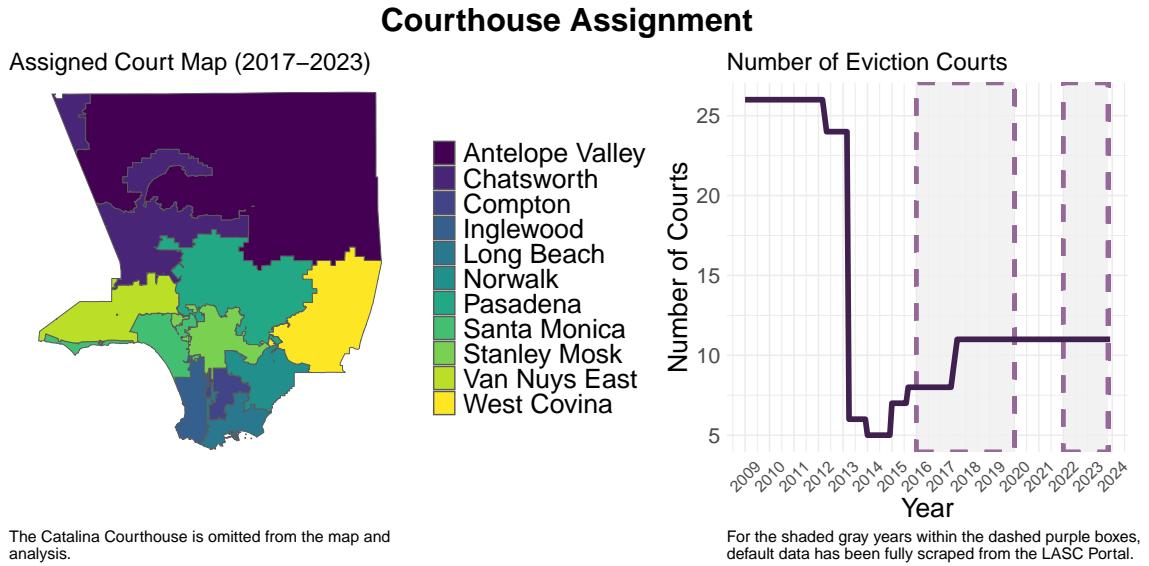


Figure 3. LA County Eviction Court Assignment

Note: The assignment map (Late 2017–2025) is shown in the left panel. The number of courthouse districts expanded in late 2017, which is shown in the right panel. *See also* Estes & Nelson (2025) for further discussion.

In the causal analysis of Section 5, I leverage the expansion of the number of courthouses which began in August 2017. Because the addition of new courthouses changed the distance-to-court for many tenants, I use quasi-experimental variation from the 2017 change in court procedure to isolate the causal effect of increasing or decreasing the distance-to-court. Companion work (Estes and Nelson, 2025) utilizes the other source of variation from the court assignment policy: the spatial nature of the assignment mechanism, as shown in the left panel of Figure 3.

4 Data Collection & Descriptive Evidence

This section briefly describes the data collection process, including information collected from primary sources (docket records), linked datasets (e.g. assessor data, Census variables), and imputation methods (e.g. racial characteristics). Next, I describe the general characteristics of the data, including looking at rough demographic

²⁹In particular, the 2008 financial crisis resulted in a dramatic reduction in the number of eviction courts. The number of courthouses that hear eviction cases has ranged between a low of five courthouses (2014) and a high of twenty-six courthouses (2009–2011). *See* Estes and Nelson (2025); Nelson (2023).

balance across courthouses. Finally, this section estimates shocks to observed variables and discusses regression evidence of the relationship between distance-to-court and eviction outcomes.

4.1 LA Default Eviction Records Data

The data I use here includes tens of thousands of individual docket records in eviction cases scraped from the LA Superior Courts from 2016-2025.³⁰ The docket records contained both structured and unstructured information, including the following fields at the top of the record:

```
Case Number: [10 character case number]  
Filing Courthouse: [court]  
Filing Date: [date]  
Case Type: [Unlawful Detainer Residential]  
Status: [Default]  
Plaintiff: [landlord name]  
Defendant: [tenant name]  
Attorney for Plaintiff: [attorney name]  
Attorney for Defendant: [attorney name]
```

From the individual-level text files, I used regular expressions (regex) to extract this information for each individual record, along with the following variables from the unstructured “register” of case actions: address information, monetary awards, judge, and other case timing information. Particularly important is the address information, which includes the full address (i.e. street number, street, city, state, zip code) for the majority of cases across years.³¹

³⁰Unless otherwise noted, 2020-2022 is excluded from all analysis and discussion because of local, state, and national eviction moratoria.

³¹See Estes and Nelson (2025) for further details on geocoding addresses.

Using the individual-level case address information, I geocode the address to get location information for each docket record³² which allows encoding³³ how many default evictions are observed at each address i at time t :

$$\text{default}_{it} = \text{number of default evictions at address } i \text{ in month } t$$

In what follows, I generally focus only on buildings that have at least one observed default eviction (or an observed eviction notice) unless otherwise noted.

4.2 Mapping the Data & Descriptive Findings

Using the records data, I then link each case to further information. Estes and Nelson (2025) linked data to building assessment data from the LA County Assessor. Here, I supplement the data further in two ways. First, I included several Census variables at the census block group level, including: median gross rent (in dollars) for renter-occupied housing units, median household income (in dollars), and demographic variables (e.g. gender, race, total census block group population).³⁴ Second, I used Bayesian imputation software from the R packages `wru` (Khanna et al., 2024) and `gender` (Mullen, 2021) along with tenant names to impute race and gender for each observed default eviction. The `wru` software, for instance, works to “predict individual race/ethnicity using surname, first name, middle name, geolocation, and other attributes” by “utiliz[ing] Bayes’ Rule (with optional measurement error correction) to compute the posterior probability of each racial category for any given individual” (Khanna et al., 2024).

Figure 4 below details the final dataset in spatial and descriptive terms. In the top left panel of Figure 4, I plot the geocoded defaults across LA County (2016–2025). The map underneath the defaulting address points is the 2017–2025 court assignment map, with eviction court district shaded in the same colors as the observed defaults but transparently. The top right table of Figure 4 shows the average distance-to-court (in miles) for cases by observed filing courthouse under two court assignment policies. For example, for cases actually filed in Compton courthouse pre-reform, the average distance-to-court was 8.93 miles; by contrast, the average distance-to-court post-

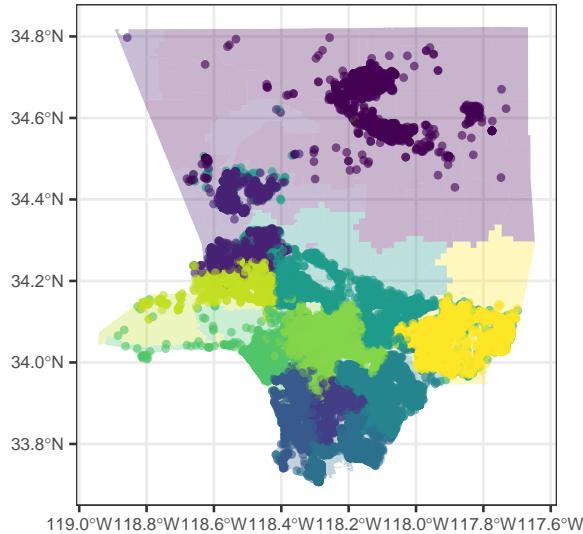
³²This can also be linked to LA County Assessor data. The Assessor data includes building-level covariates, including, e.g., the age of the building.

³³See Estes and Nelson (2025) for more details on data collection and linking, including information on the case availability, FOIA data on the total number of evictions in a courthouse each month, geocoding information, and linking to LA County assessment data.

³⁴See also Cassidy and Currie (2023) for Census variables included in their study.

reform would be 3.47 miles. Whether the change in the distance-to-court distribution induced by the 2017 court expansion reform meaningfully impacted default evictions is studied below in Section 5. Note that in some cases, the decrease in average distance-to-court was large whereas in other cases it is modest (or, in the Stanley Mosk case, a slight increase in the average distance-to-court).

Default Evictions by Filing Court



Average Distance-to-Court (miles)

Court	Pre Aug 2017	Post Aug 2017
Antelope Valley	5.24	5.14
Chatsworth	23.10	7.66
Compton	8.93	3.47
Inglewood	9.74	4.61
Long Beach	4.53	3.70
Norwalk	5.66	5.07
Pasadena	9.76	8.80
Pomona	8.32	6.18
Santa Monica	6.53	4.66
Stanley Mosk	3.65	3.70
Van Nuys East	6.66	5.48
West Covina	8.07	6.19

Summary Statistics by Court

Court	White Prob.	Black Prob.	Hispanic Prob.	Female Prob.	Income	Rent	% Represent
Antelope Valley	0.31	0.16	0.43	0.50	50193.49	1249.88	3.67
Chatsworth	0.27	0.10	0.51	0.42	83420.30	1914.55	3.41
Compton	0.20	0.14	0.56	0.49	52832.82	1320.31	4.47
Inglewood	0.30	0.17	0.40	0.41	68648.13	1598.15	3.84
Long Beach	0.27	0.13	0.49	0.44	58281.11	1412.77	1.95
Norwalk	0.14	0.06	0.72	0.44	62482.92	1441.87	2.70
Pasadena	0.26	0.09	0.48	0.39	71084.02	1760.67	4.17
Pomona	0.16	0.06	0.65	0.45	67653.91	1587.16	1.69
Santa Monica	0.35	0.15	0.35	0.39	80849.42	1938.76	3.21
Stanley Mosk	0.26	0.14	0.44	0.38	54355.54	1527.56	4.24
Van Nuys East	0.27	0.11	0.51	0.39	68333.16	1809.77	2.72
West Covina	0.17	0.07	0.62	0.45	66931.51	1623.42	3.09

Figure 4. Observed Defaults & Descriptive Findings

Note: The top-left map shows where default evictions occur geographically in LA County (2016–2025) as colored points, with the Late 2017–2025 court assignment map plotted beneath it. The top-right table shows the average distance-to-court by observed filing courthouse under the 2016–Aug 2017 and Aug 2017–2025 courthouse assignment policies. The bottom table shows summary averages by observed filing court for all available default records using: imputed race data (first three columns), geo-linked Census data (next three columns), and representation data from the docket records (last column).

The bottom table of [Figure 4](#) shows averages in several covariates by courthouse. For example, the average imputed white probability (following the imputation procedure described above) is 31% for observed eviction cases filed in Antelope Valley courthouse. Average incomes across the filing courthouses range from \$50,193–\$83,420 approximately. Average rents are positively correlated with the median income and range from \$1250–\$1939. The percentage of observed cases with tenant legal representation is around 1-5% across filing courts. The observed data is consistent with the standard empirical portrait of eviction: tenants are most often minority renters with below median incomes who lack legal representation at eviction proceedings.

Finally, I also use the data to estimate the general relationship between distance-to-court and default outcomes using binned averages and non-parametric regressions. In [Appendix C](#) (and [Figure C4](#), in particular), I estimate the default probability as a function of distance-to-court pooling across years. The estimated relationship shows that the default probability is low but roughly increasing as distance-to-court increases (at least up to a certain distance).

5 Court Expansion Study

This section uses the LA County eviction data and changes in court assignment policy over time. Specifically, I estimate the impact of expanding the number of eviction courts in the second half of 2017. Because this expansion changed the distance-to-court for some but not all locations, I model defaults as a function of the change in distance-to-court. I show that units with an increased distance-to-court following the court expansion policy had higher average defaults post-reform relative to control units (no change in distance-to-court) and units with a decreased distance-to-court had lower defaults. But I caution against causally interpreting naive before-after comparisons.

Instead, I describe a counterfactual framework—the staggered difference-in-differences (DID) design—to assess causality. The DID framework is popular in empirical law and economics studies ($\approx 40\%$ of 2016–2024 Law and Economics NBER working papers per [Goldschmidt-Pinkham \(2024\)](#)) and the intuition underlying it is briefly explained. Using this framework, I find that DID estimates imply directional treatment effects consistent with the distance-as-tenant-cost model, but only for units with large changes in distance-to-court. The effect is subject to model specification uncertainty and aggregate estimates are not always aligned with expectations under the tenant

cost theory.

5.1 Expanding the Number of Eviction Courthouses & Key Expectations

Over time, LA County has expanded the number of eviction courthouses.³⁵ In 2015, seven eviction courthouses—plotted in the left-most map of Figure 5—heard eviction cases in LA County.³⁶ But in September 2015, the LA County Superior Court system increased the number of eviction courts from seven to eight, splitting off part of the Pasadena eviction court district for coverage by the Pomona courthouse. This is shown in the middle map of Figure 5. Then, beginning in August 2017, LA County expanded the number of courthouses more substantially, adding 3 new courthouses and sending Pomona court cases to the West Covina courthouse.³⁷

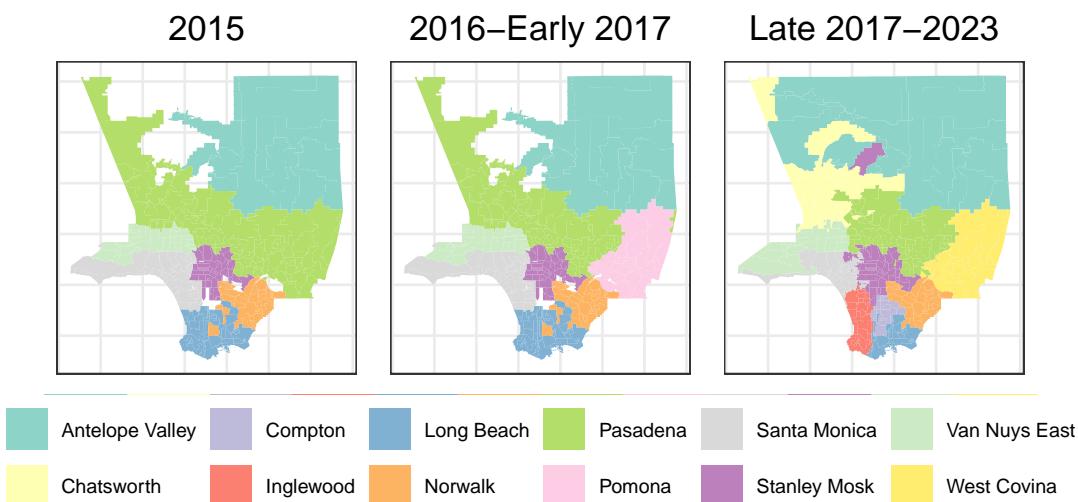


Figure 5. LA County Courthouse Expansion

Note: All maps omit the Catalina courthouse, where there are few evictions and no observed defaults.

I utilize all available data collected (from 2016 onwards) to study the effect of the 2017 policy change in court assignment on tenants. Because the court assignment

³⁵See *supra* Figure 3 (right panel).

³⁶Note that for 2015 and 2016, due to eviction data unavailability we cannot know with certainty the contours of the eviction map in the northern-most parts of LA County or in some of the south-eastern regions. Only zip codes with uniquely-assigned courts and no potentially mis-filed cases under the then-operating eviction assignment rule are shown in the left and middle maps. See also Estes and Nelson (2025) discussion of misfiling, map creation, and the assignment rule.

³⁷Formally, this is a staggered rollout: the change from Pomona to West Covina happened in August 2017. The addition of the Chatsworth, Compton, and Inglewood courthouses occurred in September 2017. Remaining changes to the assignment rule were effective Oct 10, 2017.

policy change induced changes in distance-to-court, I test whether default evictions responded to changes consistent with the tenant cost theory of defaults. Recall that on this theory, defaults are caused by the myriad costs of getting to court, with distance-to-court being a large obstacle in LA County specifically.³⁸ We therefore expect that tenants with increased costs—such as higher difficulty in physically getting to court—should experience higher rates of default. Conversely, lowering costs by assigning tenants to closer courthouses should tend to decrease defaults.

Accordingly, I begin by splitting tenants into three cohorts.³⁹ The first group I call the Increase-Treated group: tenants in this group experienced an increase in distance-to-court because of the 2017 court expansion policy shock, which represents an increase in the tenant cost of getting to court. The second group is the Decrease-Treated group faced decreased “costs” post-August 2017, meaning they experienced a decrease in distance-to-court following the court expansion. Finally, the last group is the Control group, which did not undergo a change in court assignment and therefore experienced the same distance-to-court before and after the 2017 reform. The cohort definitions and expectations under the tenant cost theory are summarized below in [Table 2](#).

Cohort	Definition	Tenant Cost Expectation
Control	Distance Before = Distance After	
Increased	Distance After > Distance Before	Defaults Up ↑
Decreased	Distance After < Distance Before	Defaults Down ↓

Table 2. Cohorts Definitions and Expectations

The changes from the August 2017 policy shock on distance-to-court are also summarized in [Figure 6](#). In the left panel of [Figure 6](#), I show the histogram of the

³⁸See *supra* (discussing *Miles v. Wesley*).

³⁹To formalize the cohorts, define the following variable:

$$\Delta_i = D_{i,Post-Expansion} - D_{i,Pre-Expansion}$$

where $D_{i,Post-Expansion}$ (resp. $D_{i,Pre-Expansion}$) is the distance from unit i to the assigned courthouse after (resp. before) the 2017 courthouse expansion policy. The cohorts are defined as: Increase-Treated ($\Delta_i > 0$)), Decrease-Treated ($\Delta_i < 0$), and Control ($\Delta_i = 0$)).

changed distances across all of LA County. The large black bar (aprx. 23,000 addresses) are Controls: they did not experience any change in distance-to-court from the 2017 court expansion. The dark blue bars plotted to the left (less than zero) are Decrease-Treated units: tenants living at these addresses were closer to court because of the 2017 court expansion. Although fewer in number than the Control group, the Decrease-Treated cohort outnumbers the Increase-Treated cohort: units with an increased distance-to-court following the 2017 court expansion are shown in dark red bars on the histogram.

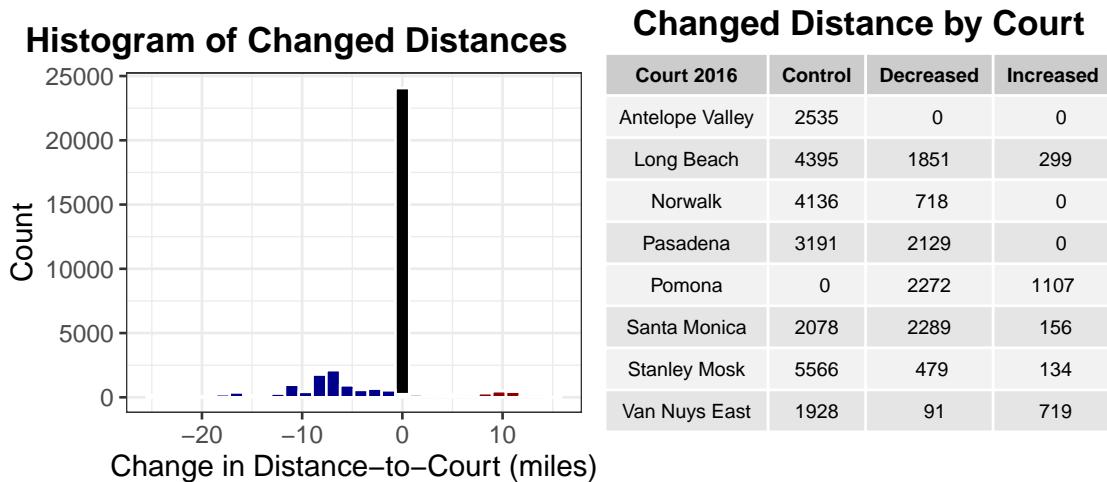


Figure 6. Changes in Distance-to-Court from 2017 Court Expansion Policy

Note: The left panel shows a histogram of changes in distance-to-court following the 2017 Court Expansion: controls are shown in black, decrease-treated units in blue, and increase-treated units in red. The right panel table shows the cohort counts by 2016 assigned courts.

The number of addresses within each cohort are broken up by assigned 2016 court in the right panel of Figure 6. Four of the 2016 court districts had some units in all three cohorts (Long Beach, Santa Monica, Stanley Mosk, Van Nuys East), two were only in the Control or Decrease-Treated cohorts (Norwalk & Pasadena), one had no Control units (Pomona), and one had only Control units (Antelope Valley).

To confirm that the 2017 Court Expansion policy shocked tenant costs but not other important eviction determinants, I measure twelve variables across eviction records before and after the policy shock in Figure 7. Fitting separate LOESS regressions to before and after the 2017 reform, I investigate whether there are significant discontinuities in important covariates other than the tenant cost of getting to court. If a significant difference in, e.g., legal representation rates around the policy treat-

ment timing were discovered, researchers may suspect that change is related to potential changes in outcomes-of-interest rather than the change in the policy treatment. [Figure 7](#) shows that tenant costs—measured by either distance-to-court or commute-to-court—significantly and immediately changed at the policy reform timing, which is indicated by the vertical dashed line. By contrast, other socioeconomic variables (predicted race, predicted gender, imputed income, imputed rent), legal representation variables (plaintiff and defendant representation rates), and procedural motions are not significantly different at the 2017 policy shock date.

Shocks/Discontinuities in Measured Variables?

Separate LOESS Fits Split at 2017 Reform

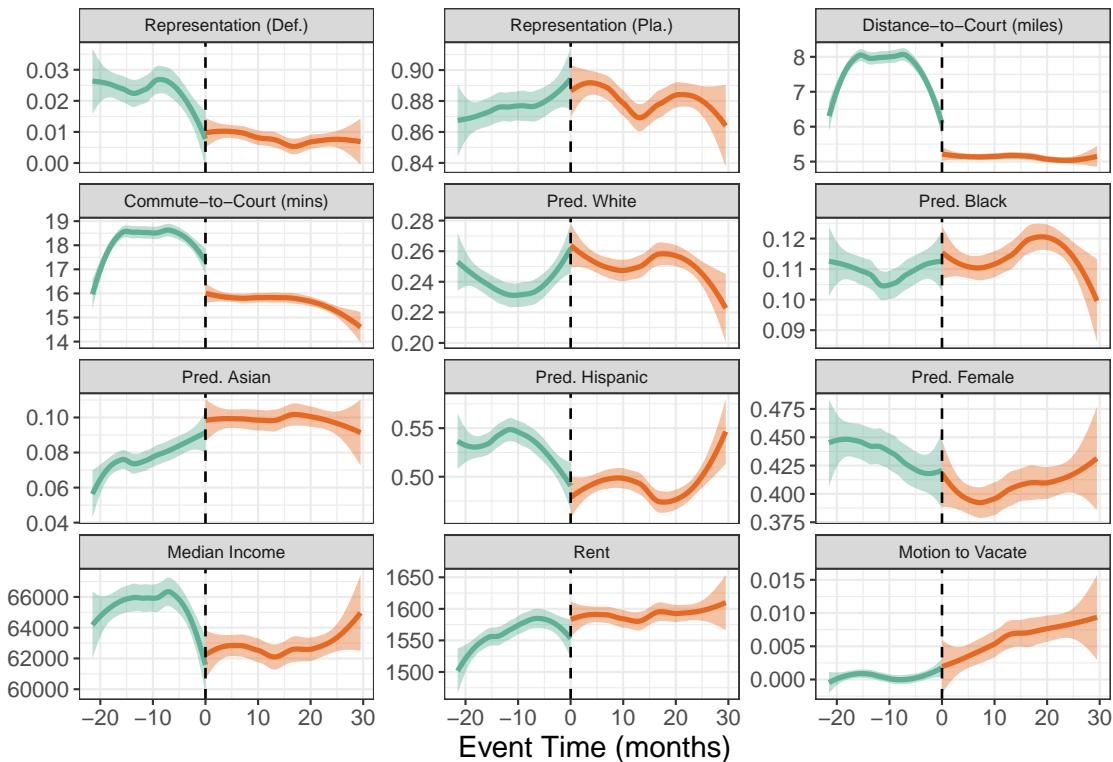


Figure 7. Checking for Discontinuous Shocks near Aug 2017

Note: Each covariate panel estimates the average among all observed eviction records relative to the 2017 court expansion policy. The x -axis is in event time to account for the staggered policy treatment. Separate LOESS regressions are fit on each side of the treatment using data from 2016–2020 only.

Using the defined cohorts, I next turn to plotting the average number of default evictions over time for each cohort. In [Figure 8](#), I plot estimates of the average number of defaults for the cohorts in different colors for each year with collected

and available data. The average number of default evictions for the Increase-Treated cohort is shown in green, the average for the Decrease-Treated cohort is shown in orange, and the average for Controls is shown in black. The court expansion policy shock is shown as the vertical dashed line in August 2017.

Average Defaults by Cohort

Increased, Control, and Decreased – Piecewise Constant Fits

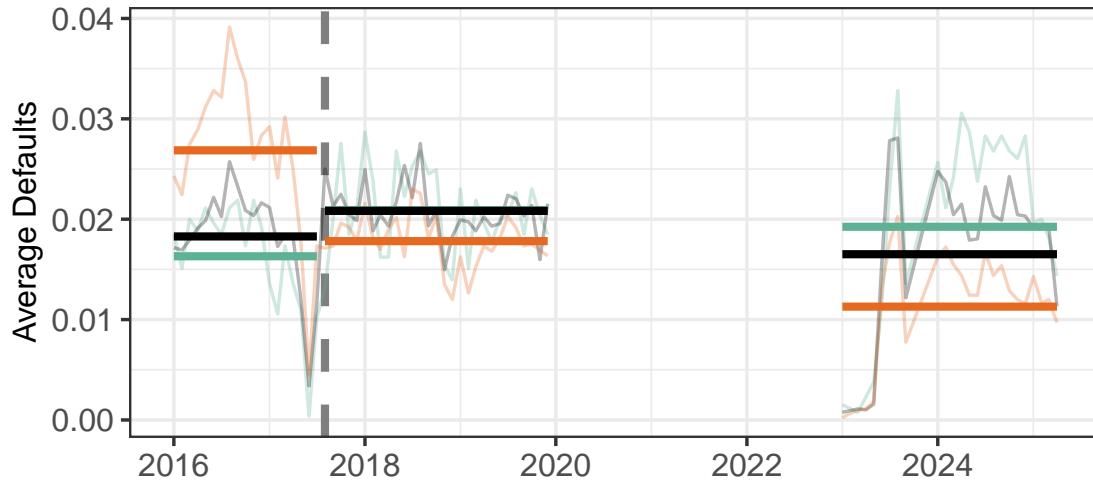


Figure 8. Cohort Averages: Piecewise Fits

Note: The plot shows the average number of default evictions for the three cohorts: Increase-Treated (red), Decrease-Treated (blue), and Controls (black). The raw averages are plotted transparently for years with available data. The piecewise constant fits are overlaid in the same colors. The dashed vertical line is plotted at the beginning of the policy shock (August 2017). Note that this plot shows averages for cases labeled with “default” status only, not all observed evictions.

Following this shock, the estimated average for the Increase-Treated cohort is higher than the Decrease-Treated cohort and about the same as the Control cohort, suggesting such tenants experienced the highest average defaults post-reform. The Increase-Treated group average post-reform is also higher than the pre-reform average for the Increase-Treated cohort. This observation is consistent with the tenant cost theory of default, since an increase in distance-to-court is an increased litigation cost which is expected, on average, to increase defaults. By contrast, the Decrease-Treated cohort saw the average defaults fall following the August 2017 policy change, which is also consistent with the tenant cost theory: lower distance-to-court is expected to lower defaults. But note that, for instance, average defaults were decreasing for

the Decrease-Treated cohort pre-August 2017, so we cannot attribute causality on the basis of lower average defaults alone. Moreover, the control group also saw an increase in average defaults even though it experienced no change in distance-to-court costs. Therefore, in order to assess causality, we need to use a causal design (a counterfactual model) that models counterfactual outcomes for our cohorts.

5.2 Difference-in-Differences (DID) Design

One such counterfactual design—widespread in economics—is the difference-in-differences (DID) design.⁴⁰ This strategy assumes that counterfactual outcomes for the treated group would have evolved “in parallel” to the control group (the *parallel trends* assumption). The DID strategy accounts for differences in baseline outcomes (pre-treatment differences) while controlling for time factors unrelated to the treatment. The results in this section rely on popular methods for DID designs.

In the standard causal inference setting, the researcher first specifies potential outcomes in two states: the treated and untreated (or control) states. The “fundamental problem of causal inference”⁴¹ is that we cannot observe individual outcomes in both states simultaneously. However, under randomization, estimating the average treatment effect is straightforward: simply take the difference in average outcomes between the treated and control groups.

Unfortunately, not every causal question can be (or is) addressed by running a randomized experiment due to ethical, legal, financial, or other practical constraints. Researchers therefore turn to observational studies and methodologies to isolate causal effects. The most popular observational methodology in economics is the difference-in-differences (DID) approach: more than 30% of applied microeconomics working papers mentioned DID or similar designs in 2024.⁴²

⁴⁰The set-up for the regression discontinuity design (RDD) in [Estes and Nelson \(2025\)](#) is different. Instead of panel data, this method assumes the researcher has access to data on two groups—the treated and control groups—where treatment is determined by an underlying running variable X . This running variable can be many things—age, income, test scores, etc.—but the basic RDD assumes that units above some cutoff C all receive treatment, whereas units below the cutoff C do not receive treatment.

In this setting, the problem of causal inference is that we never observe treated and control units for the same values of the running variable X . This is the more common “sharp” RDD. *But see, e.g., Imbens and Lemieux (2008)* for a discussion of the fuzzy RDD. The standard RDD deals with this failure—a *failure of overlap* in the running variable—by making an assumption about what happens across the cutoff C that determines treatment. Specifically, researchers using this design assume that treated and control average outcomes across the cutoff would have evolved continuously.

⁴¹[Holland \(1986\)](#).

⁴²Figure 5 of [Goldsmith-Pinkham \(2024\)](#).

In the DID design, it is similarly assumed that the researcher has access to data on two groups—the “treated” and “control” groups—over time.⁴³ At some point in time, the treated group receives a treatment, whereas the control group does not. The data available includes an outcome variable from before and after the treatment date for both groups. The goal is to estimate the causal effect of treatment on the treated group—called the average treatment effect on the treated (ATT)—after the treatment date. The “fundamental problem of causal inference” (Holland, 1986) in this setting is that we do not observe untreated outcomes for treated units after the treatment date. The DID method assumes that the average untreated outcome for units that actually receive treatment would have evolved in parallel to the control group average outcome.

In Figure C3, I show how this assumption implies a counterfactual average outcome for the treated group. In this example, the treated group (solid blue line) receives treatment in the year 1950 and the control group (solid red line) never receives treatment. Naively taking differences post-1950 between the treated group average outcome and the control group average outcome would result in an over-estimate of the true treatment effect because the treated and control groups start (and stay) at different baselines in the pre-treatment periods. Indeed, in Figure C3, the treated group average outcome at the beginning of the study period (year 1900) is 5 whereas the control group average outcome is 0.

Instead, the DID method assumes the counterfactual outcome for the treated group would’ve evolved “in parallel” to the control group average. This results in the dashed blue line for the treated group counterfactual. The true average treatment effect on the treated (ATT) is the difference between the observed outcome (solid blue line) and the counterfactual outcome (dashed blue line): it is the average difference in where treated units are versus where they would have been without treatment. This is illustrated in Figure C3 by the double-sided black arrow labeled “Treatment Effect.” Note that it is not a requirement of the methodology that the average treatment effect on the treated be constant in all post-treatment time periods.

5.3 Baseline Results: Applying the DID Strategy

Here, I apply the DID methodology (Callaway and Sant’Anna, 2021) to the court expansion policy. I assume that units with changes in distance-to-court would’ve

⁴³This section details the intuition for simple DID designs, although more complicated (e.g. staggered treatment) designs are possible.

experienced parallel outcomes to units with zero changes in distance-to-court. Recall that I consider three cohorts of units: the Control units (zero change in distance-to-court), the Increase-Treated units (positive change in distance-to-court), and the Decrease-Treated units (negative change in distance-to-court). The treatment effect of interest is the average treatment effect on the treated (ATT), which is the difference in expected or average outcomes from treatment versus control for units that actually receive treatment. Event-study dynamic DID estimates are shown below in [Figure 9](#).

Consider the first set of DID results comparing Increase-Treated units—i.e. units that experienced an increase in distance-to-court—with control units as the comparison group. This means we are using control units to impute counterfactual outcomes for the Increase-Treated units in the counterfactual world where these units do not experience an increase in distance-to-court. In the left panels of [Figure 9](#), I plot the ATT estimates from this comparison, with and without additional covariates.

Default Count Dynamic ATTs

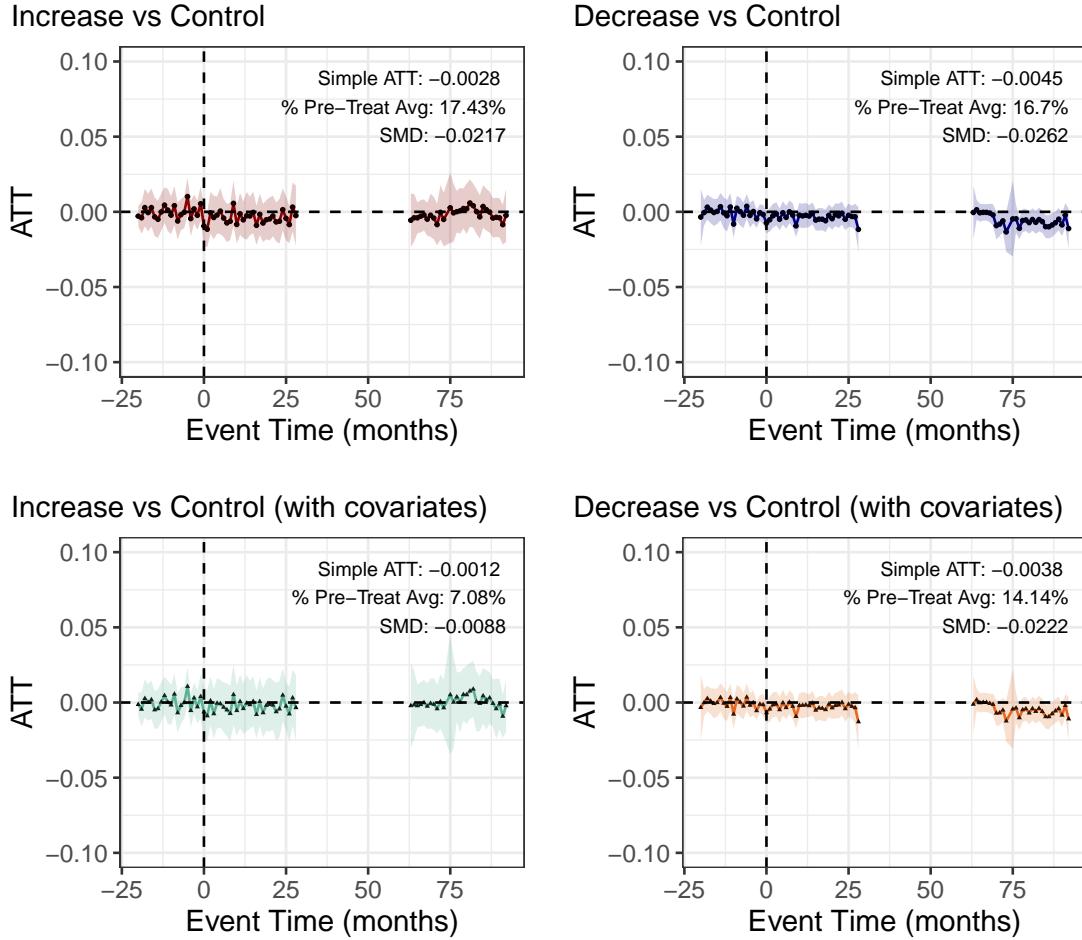


Figure 9. Dynamic DID Estimates: Comparisons with Controls

Note: The panels show estimates of the average treatment effect on the treated for Increase-Treated (left panel) and Decrease-Treated (right panel) units. The Simple ATT aggregates post-treatment ATT estimates as in Callaway and Sant'Anna (2021). The percent pre-treatment average is the ratio of the ATT and the cohort average outcome in the pre-treatment period. The standardized mean differences (SMDs) suggest small effects under standard rule-of-thumb interpretations. The covariate models include gender, race, and income variables.

The point estimates after the court expansion policy (vertical dashed line) are mostly negative and all are insignificant. This means that units with an increased distance-to-court experienced a lower number of defaults on average than units with zero change in distance-to-court following the court expansion policy. However, note that the standard errors include the zero effect for all time periods (dashed horizontal line at 0) and the standardized mean difference (SMD = -0.0217) suggests a small effect size. Finally, note that the aggregate effect (simple ATT = -0.0028) does not align

with the expected direction, although it too is insignificant.

The right panels of [Figure 9](#) repeat this exercise comparing units that are Decrease-Treated with Control units. The post-expansion ATT estimates for the effect on the Decrease-Treated units relative to the Control units are mostly negative and the overall aggregate ATT estimate (a weighted average across post-treatment periods) is negative and significant. The size of the aggregate ATT estimate can be assessed multiple ways: it is only 14-16% of the Decrease cohort average in the pre-treatment period and is small relative to the pre-treatment variation ($SMD = -0.0262$). Note, however, that the estimates here too reflect a high degree of uncertainty: most of the post-treatment ATTs are not statistically significant, although the aggregate effect in the no covariate specification is significant and negative (in accord with tenant cost expectations).

In sum, I find limited evidence that court expansion significantly impacted default evictions for early 2017 units using all Increase-Treated and Decrease-Treated units versus the Control group. Although the aggregate estimate signs sometimes align with expectations and are significant in one model, I assess below the sensitivity of the estimates to model specification and find considerable uncertainty. In addition, the event-study dynamic effects are almost always insignificant and, in many cases for the Increase vs Control comparison, do not align with tenant cost expectations.

These comparisons use all units that experienced an increase or decrease in distance-to-court as a treatment cohort. What about units which experienced “large” changes in distance-to-court? I repeat the exercise comparing treated units (those with a change in distance-to-court) versus control units (no change in distance-to-court) by considering only treated units with above-median changes in distance-to-court. The results for this comparison are shown in [Figure 10](#).

Default Count Dynamic ATTs

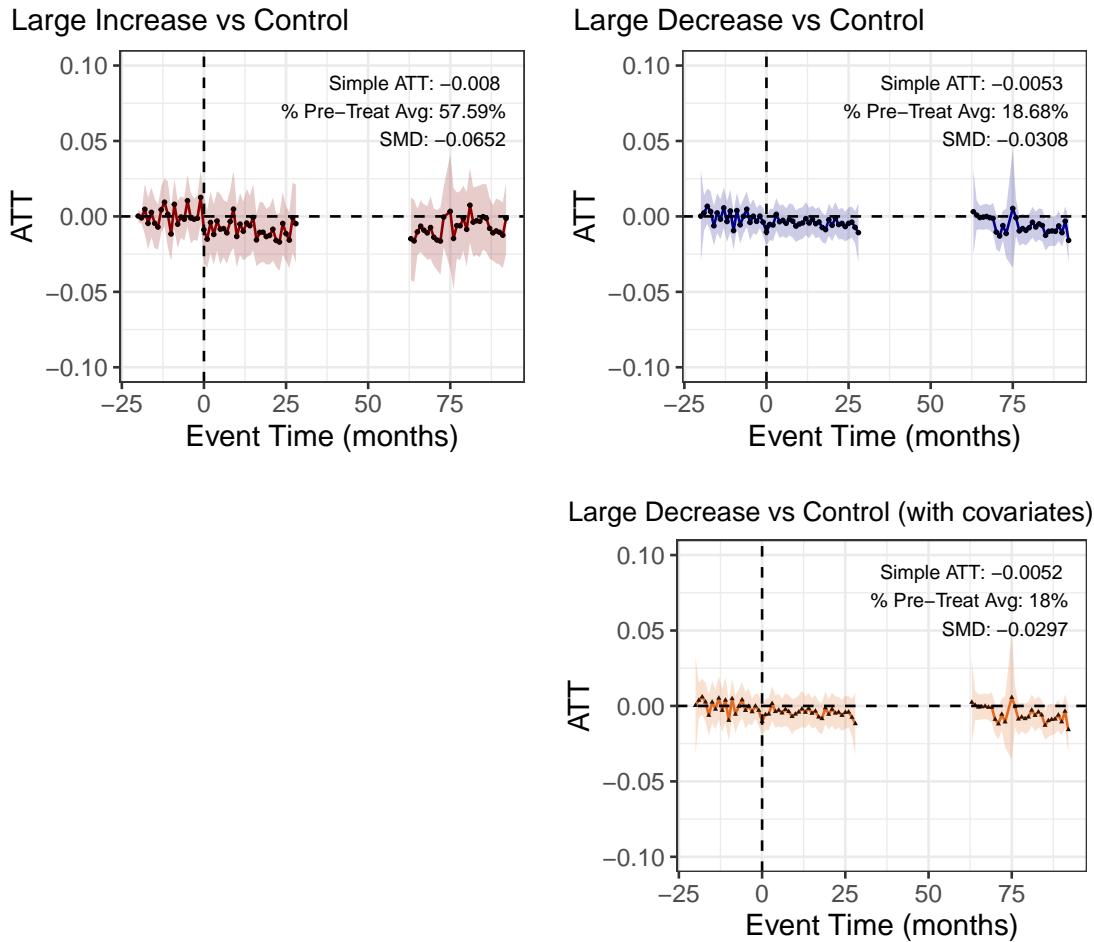


Figure 10. Dynamic DID Estimates: Above-Median Comparisons with Controls

Note: The panels show estimates of the average treatment effect on the treated for Above-Median Increase-Treated (left panel) and Above-Median Decrease-Treated (right panel) units. The Simple ATT aggregates post-treatment ATT estimates as in Callaway and Sant'Anna (2021). The percent pre-treatment average is the ratio of the ATT and the cohort average outcome in the pre-treatment period. The standardized mean differences (SMDs) suggest small effects under standard rule-of-thumb interpretations. The large increase sample size is too small to include the covariate specification, but the large decrease specification includes gender, race, and income variables.

In this case, the results are subject to increased uncertainty because they utilize a smaller sample.⁴⁴ The simple ATT estimate for units with an above-median increase in distance-to-court is negative and implies treatment (the August 2017 policy) decreased defaults. But note that this point estimate is insignificant and, therefore, I cannot rule out a true positive effect for units with a large increase in tenant costs

⁴⁴This is reflected in the larger confidence intervals on the ATT estimates.

(as measured by distance-to-court). By contrast, the aggregate estimate (“simple ATT”) for units with an above-median decrease in distance-to-court is negative and significant (with or without covariates), implying treatment (the August 2017 policy) decreased defaults but the effect size is small ($SMD \approx 0.03$). In other words, there were fewer defaults for tenants with a large changes in distance-to-court but only the above-median decrease model is significant. Note, however, that the post-treatment period ATT point estimates and aggregate estimates (simple ATTs) are, in most periods, not statistically significant.

5.4 Model Specification & Robustness

In this subsection, I discuss several robustness checks. The checks test how the findings change using additional data, measurements, covariates, outcomes, or criteria to define the population of interest.

Tenant Cost Measurement. First, I test whether the results are sensitive to our distance-to-court measure of tenant costs. Rather than using distance-to-court, I measure tenant costs by changes in commute time to court. Because distance-to-court and commute times are highly correlated (0.84), this change in how tenant costs are measured should not make a large difference in the estimates. In [Appendix D](#), I show that the dynamic event-study often remain insignificant, do not always align with expectations under the tenant cost theory, and are small in magnitude (as measured by standardized mean differences). Aggregate estimates are very similar to our baseline distance-to-court estimates: only the Decrease cohort model without covariates aligns with expectations and is significant.

Address Population. Next, I consider alternative ways to define the address population-of-interest. The baseline model uses the set of all observed addresses to form a panel dataset tracking defaults at those addresses over time. There are two potential problems with this: first, the analysis omits many addresses at-risk of eviction which experience zero defaults and are therefore not in the panel dataset; and, second, using all observed addresses may fail to account for possible selection and composition effects if certain addresses appear post-reform with defaults that did not have defaults pre-reform.

To address the first possible problem, I use new data from the LA City Controller office. Because a 2023 change in LA City law required all landlords to submit eviction notices to the City Controller, I can construct a set of addresses “at-risk” of eviction. Using this set of addresses only, I repeat the staggered DID estimation and plot results

in [Figure C5](#) and [Figure C6](#). Pre-trend differences for Increased vs Control cohorts appear smaller, although the same issues remain: insignificant, small effect estimates which (in some cases) do not align with expectations.

To address the second potential selection problem, I also analyze the effects of the reform only considering addresses that appeared in the default eviction records prior to August 2017. In [Figure C9](#) and [Figure C10](#), I find that the uncertainty increases (smaller samples) but the size of the standardized point estimates is slightly larger. Additionally, the direction of the aggregate estimates from this model (“Pre-Aug 2017” below) align with expectations but are insignificant.

Covariates. Next, I test whether covariates make a meaningful difference to the significance of the aggregate ATT estimates. Because not all covariates are available for every address (e.g. not all addresses have predicted race), I test the sensitivity of aggregate ATT estimates to the inclusion of many possible covariate combinations. Specifically, I use the all eviction panel dataset, all possible four covariate combinations, and compute aggregate ATT estimates. Results are shown in [Figure 11](#) below for both the Decreased vs Control and Increased vs Control cohort comparisons. Darker colors (dark red or dark blue) indicate significant aggregate estimates, whereas lighter colors (light red or light blue) indicate insignificant estimates. Positive and negative aggregate estimates are shown in blue and red, respectively.

Specification Curve: Aggregate ATT Estimates

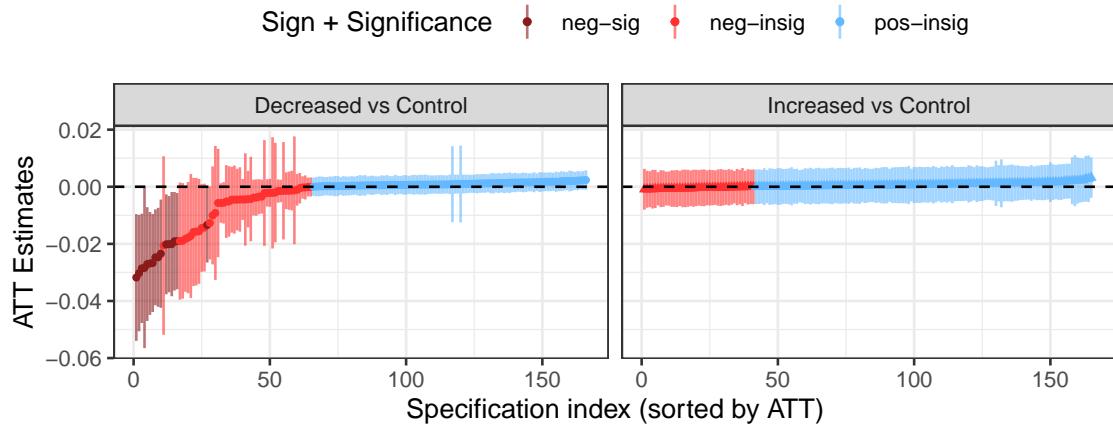


Figure 11. Covariate Specification Curve

Note: Positive and negative aggregate ATT estimates are shown in blue and red, resp. Dark colors (resp. light colors) indicate significant (resp. insignificant) aggregate estimates.

The specification curves suggest that the significance and sign of the Decreased vs

Control aggregate estimates are sensitive to the covariate specification. By contrast, the Increased vs Control estimates are all insignificant, although the direction of the effect is sensitive to model specification. In general, the specification curve estimates show that aggregate ATT estimates are sensitive to model specification, generally insignificant, and therefore cannot be sign-identified. In both the Decreased vs Control and Increased vs Control cases, the majority of the aggregate estimates across specifications are positive and insignificant.

All Observed Outcomes. I also examine whether the reform had effects when we use all eviction outcomes, rather than only outcomes marked with “default” status. Using all observable evictions as the outcome-of-interest, I repeat the modeling exercise as in our baseline model and re-estimate the event-study dynamic plots in [Appendix F](#). Aggregate estimates are all insignificant but the sign of the aggregate ATTs in the no covariate models do align with the tenant cost theory expectations.

Relaxing Parallel Trends. What about relaxing the key parallel trends assumption underlying the DID analysis? Given that the parallel trends-based estimates are already insignificant, relaxing the assumption is unlikely to yield sign-identification. Nevertheless, I show in [Appendix E](#) that a Manski-style partial identification approach—adapted to our setting—may be employed ([Manski and Pepper, 2018](#); [Manski, 2007](#)). The method therein allows for parallel trend violations provided counterfactual cohort averages remain “sufficiently close” to observed cohort averages. How close? Within some multiple of the maximum deviation between cohorts before their treatment paths diverged. Applying this partial identification strategy does not yield sign-informative bounds for most post-Aug 2017 time periods.

Summary. Finally, I plot the aggregate estimates from each of the previously models discussed models. In [Figure 12](#), aggregate ATT estimates from different model specifications are shown for both Increase vs Control (green estimates) and Decrease vs Control (orange estimates) cohort comparisons. Additional models with covariates are included where sample size permits, which are shown with triangle (rather than circle) estimates.

The models show that, in most cases, aggregate ATT estimates are insignificant, do not align with tenant cost expectations, or both. Additionally, all estimates imply small effect sizes (SMD less than 0.20). A few models with restrictions on the time period of study (e.g. “pre-2020” panels only) are also estimated to account for possibility of post-moratoria changes in eviction practice affecting estimates. This does not make a noticeable difference on the aggregate ATTs shown below.

Aggregate Estimates with 95% Confidence Intervals

Increase and Decrease vs Control Comparisons (with covariates = triangles)

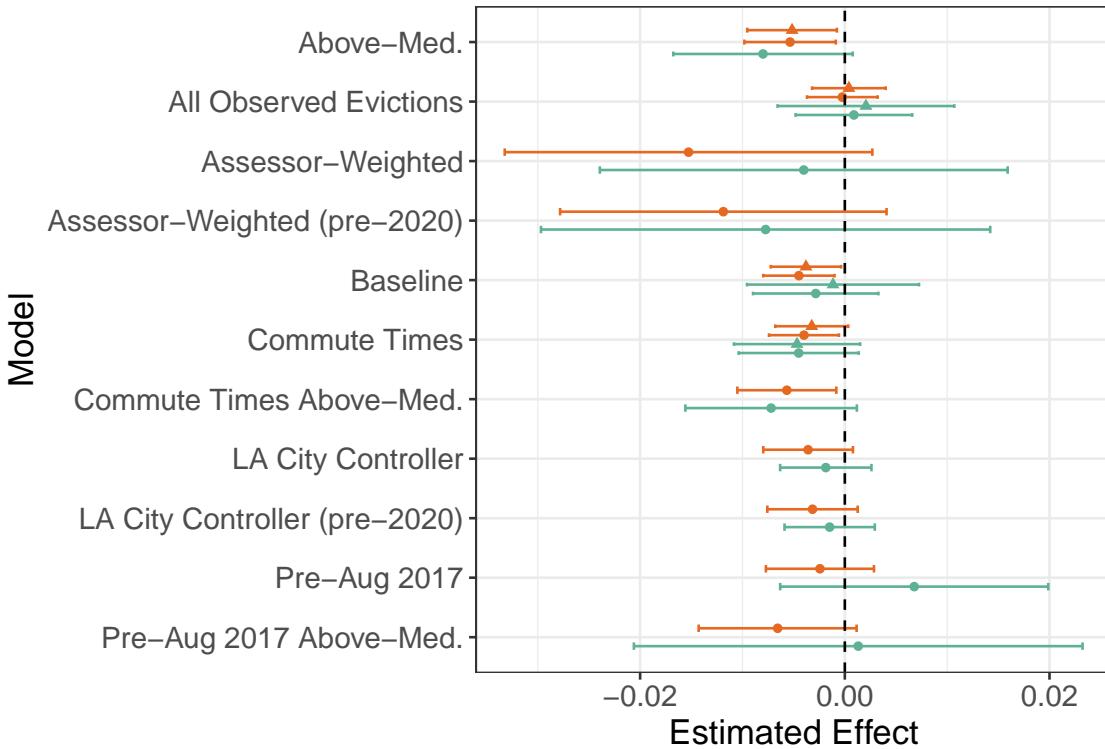


Figure 12. Model Summary: Aggregate DID Estimates

Note: Model names listed on the y -axis are explained above and further elaborated in the appendices. Green estimates are Increase vs Control comparisons and orange estimates are Decrease vs Control comparisons. In models with sufficient sample size, covariate specifications including gender, race, and income variables are also estimated (results shown with triangles).

6 Discussion & Policy Implications

What lessons to draw from the empirical exercises highlighted in Section 5? Because of uncertainty in the estimates (stemming from relatively small treated cohorts), I first caution against over-generalization. The point estimates are somewhat consistent with the tenant cost theory, but we cannot rule out zero effects in most post-treatment time periods or in the aggregate. If legally meaningful effects are limited only to a subset of the population (e.g. those with the highest distance-to-court values), then a larger dataset would be needed to discern the magnitude and direction of the effect.

On the other hand, the results suggest that expanding access via increasing the number of operating courts is not a panacea, at least not if expansion is a modest

three court increase in a metropolitan region as large as Los Angeles County. However, the results do not imply that distance-to-court is unimportant or that larger reforms would not find large effects: the results show that this particular 2017 reform (from 8 to 11 courts) did not significantly reduce defaults. In some filing courthouses, the average change in the distance-to-court may have been insufficient (see [Figure 4](#)) to have meaningful effects.

This study accords with much of the procedural intervention literature discussed in [Section 2](#), showing that relatively limited interventions are often of limited efficacy.⁴⁵ Larger changes to the eviction system may prove meaningful, but work remains to find cost-effective measures to optimize the legal details of the eviction system. I turn now to discussing some possibilities, including additional work focused on LA County and future research possibilities.

6.1 LA County Regression Discontinuity Results ([Estes and Nelson, 2025](#))

One possible strategy is explored in [Estes and Nelson \(2025\)](#), where we examine the boundaries of the spatial map in LA County to test whether how the map is drawn creates meaningful differences in tenant cost structures. The idea is that, unlike the Fedex or Amazon delivery system, legal assignment mechanisms are not designed, tested, and regularly optimized. Although the designers of legal rules and institutions may intend to improve welfare (or at least do no harm), the nature of the legal enterprise—especially judicial decision-making—is such that rules often need to be fashioned without empirical evidence and are, therefore, non-optimal.⁴⁶ Identifying areas for improvement with empirical study can assist legal and policy decision-makers to better fashion alternative rules, especially when it comes to the important task of simulating counterfactual policies (e.g. redrawing maps).

This companion paper studies the spatial aspect the case assignment mechanism and suggests regions where alternative maps may be most effective. Empirical evidence therein suggests statistically significant differences in default probabilities near the boundaries of specific LA County eviction court districts. Using spatial variation stemming from the discontinuous assignment of cases to eviction courthouses,

⁴⁵Some interventions, moreover, may backfire. *See supra* discussion of regressive effects of expanding small claims amounts.

⁴⁶As noted by [Goldsmith and Vermeule \(2002\)](#): “One reason (but not the only one) why legal scholars sometimes trade accuracy for relevance and timeliness is their close connections to governmental institutions, especially courts, that have to make decisions in the short term under conditions of empirical uncertainty.”

the paper estimates the effects of eviction procedures on tenant outcomes locally at courthouse boundaries.⁴⁷ The main contribution of the paper is to show that, at the boundary of several eviction courthouse districts, there are significant differences in the default probability for tenants on one side of the boundary versus the other.⁴⁸

Specifically, we test whether units located nearby experience differences in default probability due to assignment to one court versus another. Formally, this type of study is a spatial regression discontinuity design, which takes advantage a spatial policy that creates borders between two (or more) regions. A local causal effect is estimated by comparing cases along a policy border that receive different treatments. The intuition is that cases located nearby—but across some quasi-random policy boundary—have “similar” observed and unobserved characteristics. Under this assumption, differences between nearby units are therefore attributable to the difference in the spatial “treatment” variable.

To illustrate graphically the types of comparisons the method makes to obtain causal estimates, consider, for example, the rental units near the Compton and Norwalk boundary in LA County. In [Figure 13](#), I plot the Compton and Norwalk court districts in red and blue, resp. Addresses with at least one default eviction that are within 5km of the Compton-Norwalk boundary (yellow boundary) are plotted below.

⁴⁷ Cf. [Collinson et al. \(2024a\)](#), which uses spatial comparisons from nearby zip codes.

⁴⁸ I also show that the gap obtains also for another outcome variable: money judgments. See [Figure B2](#) (estimating how large courthouse assignment effects are in monetary terms for defaulting tenants near courthouse boundaries). Because defaulting tenants are liable for unpaid rent, courthouse assignment can effect the average monetary judgments owed landlords near the courthouse boundary. The results shown below in [Figure B2](#) provide robust estimates of the LATE in the bottom-right table for each courthouse pair, along with robust CIs. The CIs restrain the magnitude of the effects to around 1-2 months rent. Because these comparisons only use addresses with observed default evictions, they are robust to issues stemming from assessor-imputed outcomes for unobserved units.

Buildings with Default Evictions (2017–2023)

Comparing Compton and Norwalk evictions within 5 km buffer

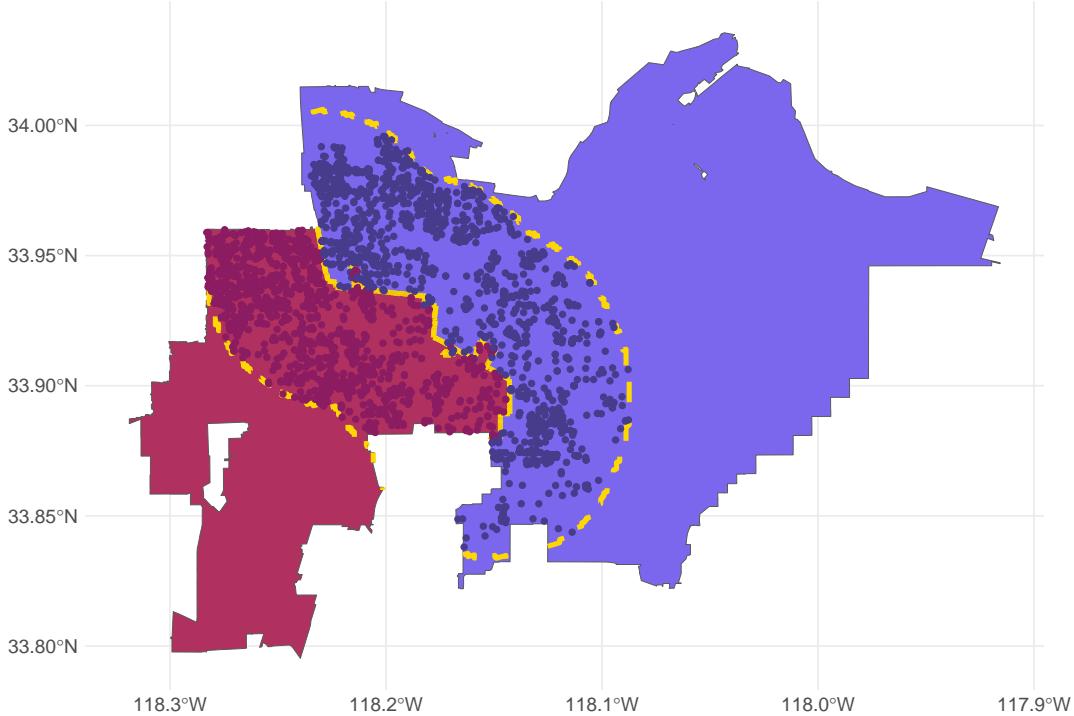


Figure 13. Compton & Norwalk Eviction Districts

Note: Defaults within 5km of the courthouse boundary (solid-yellow line) are shown as red (Compton courthouse) and blue (Norwalk courthouse) points.

We compare only units near the boundary because they are expected to be similar the closer we get to the boundary. Under the assumption that units near the boundary are, on average, “sufficiently similar,” the differences in the probability of default eviction are attributable to being assigned to the Compton courthouse rather than the Norwalk courthouse. In this example, being assigned to the Compton courthouse rather than the Norwalk courthouse increases the probability of default eviction by 8.81⁴⁹ percentage points. See Appendix B for results for seven different courthouse pairs.⁵⁰ In some cases, there is no significant difference at the courthouse boundary, whereas in other cases there are large, significant effects. In those regions where large local effects appear persistent, “re-optimizing” the court assignment map may help reduce default evictions. In the long-term, adaptive court assignment policies—with

⁴⁹The robust confidence interval for this courthouse boundary is (7.05, 10.56) percentage points. These estimates are obtained from using standard robust RDD tools. See Estes and Nelson (2025) for further details.

⁵⁰Reproducing results from Estes and Nelson (2025).

periodic assessment, analysis, and adjustment (if warranted)—may help alleviate the access-to-justice problem.

6.2 Other Research: Transportation & Structural Reforms

Another possibility is investment in transportation infrastructure. Although I am unaware of comprehensive panel datasets on changes to the transportation network in LA County, future work might look at how transportation and commuting costs—as opposed to distance-as-cost—impacts tenants. These variables are all likely to be highly correlated, but additional survey research on tenant commuting patterns could be helpful. Potential avenues include studying how tenants get to court, how far their jobs are from courts, and what methods of transportation they use in a sprawling urban city like Los Angeles.⁵¹

Finally, I conclude by observing that further research into long-term strategies to reduce evictions is needed. Trying to solve the eviction problem only after tenants receive an eviction notice may be too late. The “disease” of high rent is left untreated if a broader strategy to reform housing markets—especially to increase housing supply—is not undertaken. Fortunately, advocates, voters, and policymakers seem increasingly willing to embrace an abundance agenda in housing. In time, researchers will have new data to monitor the impact of such reforms on rental markets. As policy changes unfold, eviction researchers should monitor how eviction outcomes respond to structural housing reform.

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⁵¹Potentially the best way to get at this possibility would be to use cell-tracking data to pinpoint exact travel distances or times to court from tenant last location (e.g. job).

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APPENDICES

A Appendix A: Types of Reforms

Appendix A includes a table summarizing two approaches to combat evictions: procedural and structural reforms. Procedural reforms tend to focus on making the eviction process “fair” for tenants. Because tenants often face a large resource disadvantage in eviction cases, procedural reforms aim to “level the playing field” for tenants in cases against better-resourced landlords. Procedural policy reforms might include, for example, allowing longer tenant response times, permitting remote court attendance, reducing filing fees, and providing publicly-funded tenant counsel.⁵² Procedural proposals prioritize helping tenants *after* they’ve received an eviction notice.⁵³

Structural reforms, by contrast, aim to address the underlying causes of eviction. Because non-payment of rent is the primary reason for eviction, structural reforms tend to focus on reducing rent and housing prices. Because of the large impact of housing supply on prices,⁵⁴ the goal is usually (but not always) to increase the available stock of rental housing. Structural reforms may include, for example, subsidizing the building of affordable housing units, building public housing, increasing housing vouchers, or changing income thresholds to receive rental assistance. Structural proposals prioritize helping tenants *before* they’ve received an eviction notice.

⁵²The last proposal is sometimes referred to as “civil *Gideon*”, a reference to *Gideon v. Wainwright* (1963), the landmark Supreme Court decision requiring states to provide defense counsel to indigent criminal defendants.

⁵³See, e.g., D. James Greiner, Cassandra Wolos Pattanayak, and Jonathan Philip Hennessy, *How Effective Are Limited Legal Assistance Programs? A Randomized Experiment in a Massachusetts Housing Court* (2012); Mike Cassidy and Janet Currie, *The Effects of Legal Representation on Tenant Outcomes in Housing Court: Evidence from New York City’s Universal Access Program*, 222 J. PUBLIC ECONOMICS 104844 (2023).

⁵⁴See Nathaniel Baum-Snow and Gilles Duranton, *Housing Supply and Housing Affordability*, NBER Working Paper (2025) (discussing extensive literature on housing supply and prices).

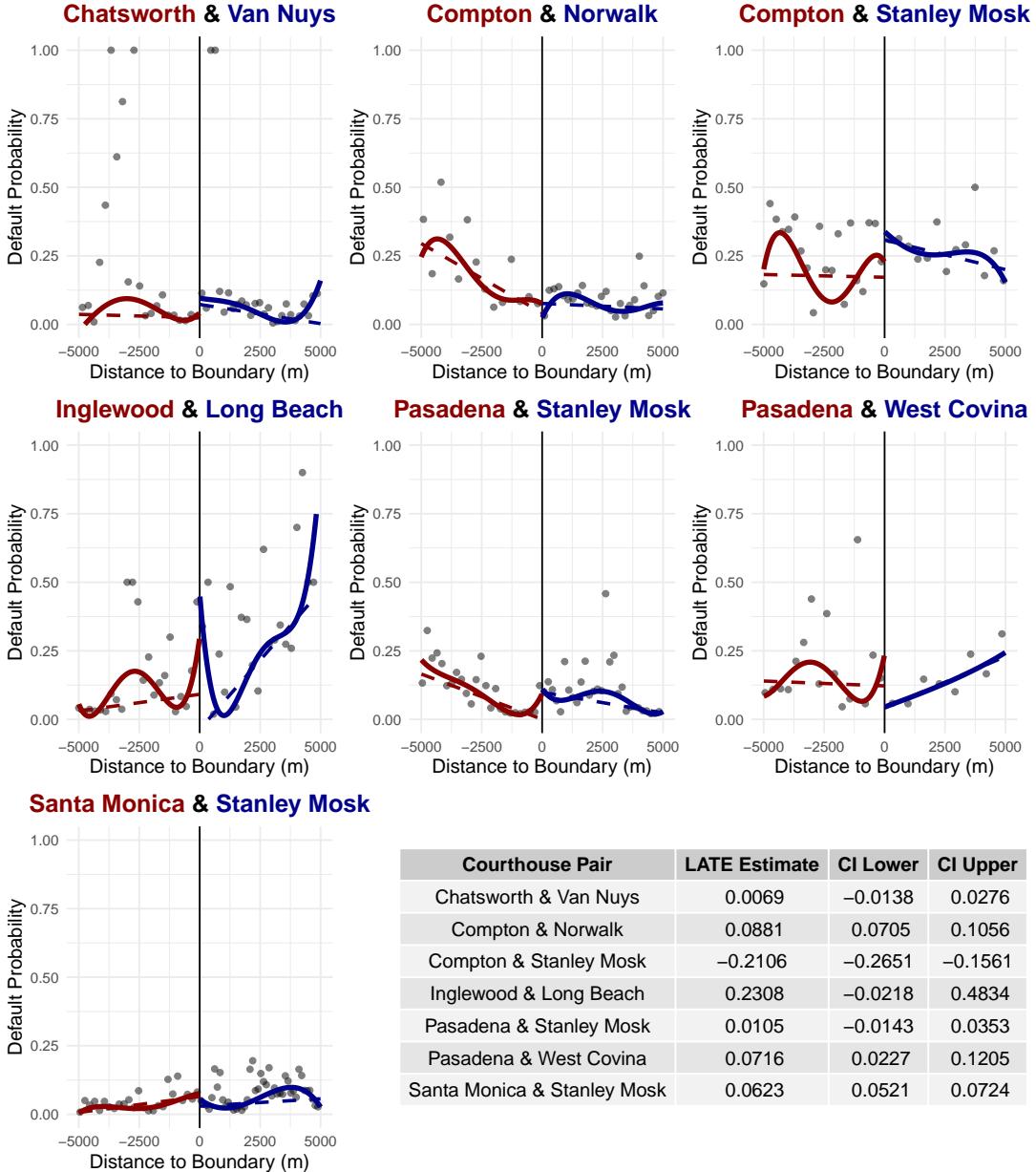
Characteristic	Procedural Reforms	Structural Reforms
Primary Goal	Fair process	Eliminate root causes
Policy Focus	Alter legal proceedings	Reduce rent burden and increase affordability
Intervention Timing	After eviction notice	Before eviction notice
Mechanisms	Legal aid, court procedures, fee reductions	Supply-side housing policy, subsidies, vouchers
Typical Examples	Longer response times, remote hearings, public counsel (<i>Civil Gideon</i>)	Public housing, expand vouchers, eligibility thresholds

Appendix Table A1. Comparison of Procedural and Structural Eviction Reforms

B Appendix B: RDD Study Plots (Estes & Nelson 2025)

LATE Results by Courthouse Pairs

Estimates at the Boundary for Robust Bandwidth

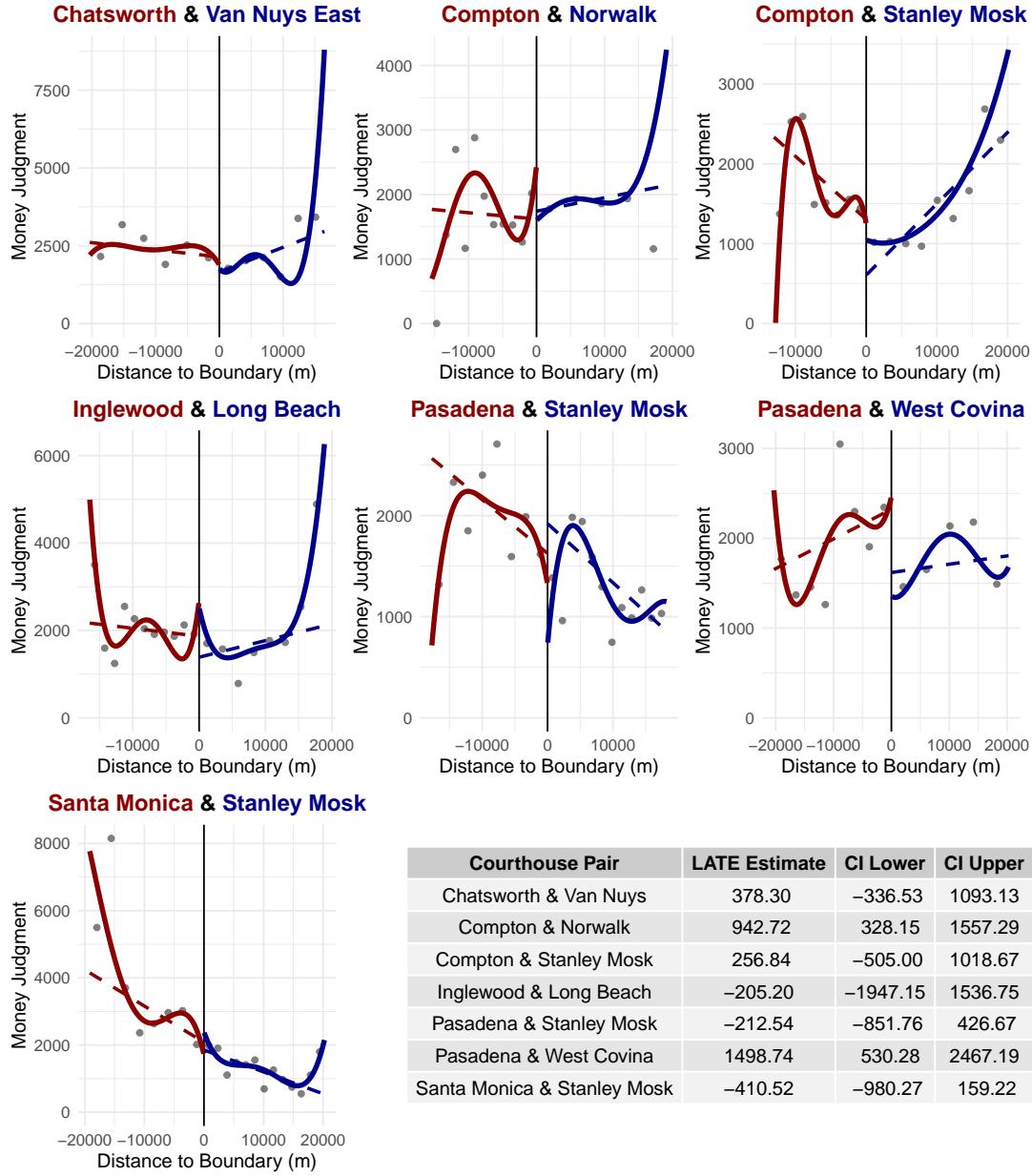


Appendix Figure B1. LA County Default Eviction Probability

Note: The LATE estimates on default probability ($\hat{\tau}_C$) at each courthouse pair boundary use the optimal bandwidth selection procedure in the `rdrobust` package (Calonico et al., 2023). The global quartic polynomial (solid line) and global linear (dashed line) fits are plotted for each courthouse separately. The gray points are evenly-spaced binned means using the `rdplot()` function. The table reports robust point estimates (with robust CIs) for each courthouse pair.

LATE Money Judgment Results

Estimates at the Boundary by Courthouse Pairs



Courthouse Pair	LATE Estimate	CI Lower	CI Upper
Chatsworth & Van Nuys	378.30	-336.53	1093.13
Compton & Norwalk	942.72	328.15	1557.29
Compton & Stanley Mosk	256.84	-505.00	1018.67
Inglewood & Long Beach	-205.20	-1947.15	1536.75
Pasadena & Stanley Mosk	-212.54	-851.76	426.67
Pasadena & West Covina	1498.74	530.28	2467.19
Santa Monica & Stanley Mosk	-410.52	-980.27	159.22

Appendix Figure B2. LA County Money Judgments

Note: The LATE estimates on money judgment amounts ($\hat{\tau}_{C,m}$) at each courthouse pair boundary use the optimal bandwidth selection procedure in the `rdrobust` package (Calonico et al., 2023). The global quartic polynomial (solid line) and global linear (dashed line) fits are plotted for each courthouse separately. The gray points are evenly-spaced binned means using the `rdplot()` function. The table reports robust point estimates (with robust CIs) for each courthouse pair.

C Appendix C: Additional Results

Appendix C includes additional results plots, focusing mostly on court expansion DID estimates.

C.1 Regression Evidence: Distance-to-Court and Default Relationship

These plots are discussed briefly in text above. They show reduced form or regression relationships between distance-to-court and default evictions using assessor data and court records.

First, I use data on all rental units from the LA County Assessor dataset⁵⁵ to encode whether each unit at rental addresses has an observed default in a given year.⁵⁶ In Figure C1, I use this data to estimate the default probability as a function of distance-to-court pooling across years. The average default probabilities are shown in 5-mile distance-to-court intervals. Next, using non-parametric local linear regressions, I also plot estimated curves in Figure C2. In the left panel of Figure C2, the estimated relationship shows that the default probability is low but roughly increasing as distance-to-court increases (at least up to a certain distance). Finally, I estimate the relationship between the number of observed defaults and distance-to-court for addresses observed in the court docket records only. This exercise gets at the spatial concentration of defaults across LA County and is shown in the right panel of Figure C2: the number of observed default evictions is a complicated non-linear function of distance-to-court.⁵⁷

Because there are some disadvantages to using either the Assessor data or court record counts only, I also repeat the reduced-form estimation in Appendix C using newly available on the near universe of eviction notices in LA City. Using a new legal reporting requirement of eviction notices to the LA City Controller's Office,⁵⁸ I am able to define the population of at-risk tenant addresses as those appearing in the

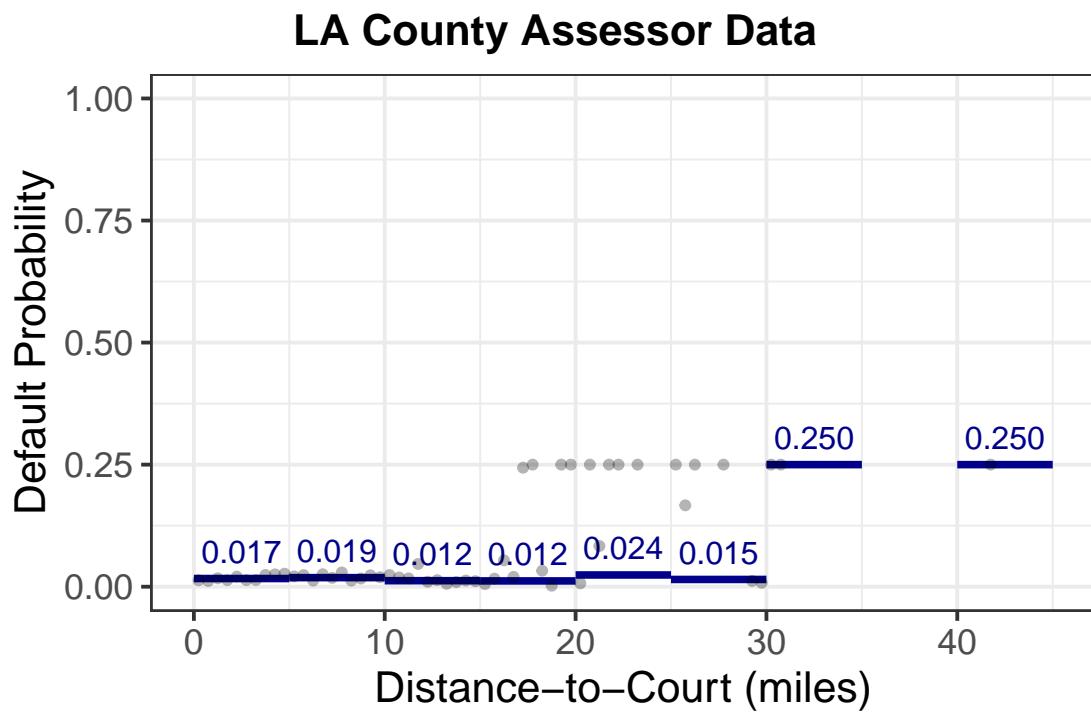
⁵⁵See also Matthew Estes & Kyle Nelson, *Justice Divided, Justice Denied? The Effects of Court Rules on Eviction Outcomes in Los Angeles County* (2025) (unpublished manuscript).

⁵⁶For example, if unit A at address i in year t has a default but units B and C do not, then unit A is encoded as a 1 whereas B and C are encoded as zeroes.

⁵⁷The number of defaults is highest at the smallest distances, largely due to a high number of evictions in downtown LA relatively close to the Stanley Mosk courthouse. The estimated curve then decreases between 0–5 miles, before increasing from around 5–13 miles, at which point it decreases and levels off at 1 default. This is because most addresses at the highest distance-to-court values have only a few observed defaults.

⁵⁸Failure to report the eviction notice to the City Controller constitutes an affirmative defense to the eviction.

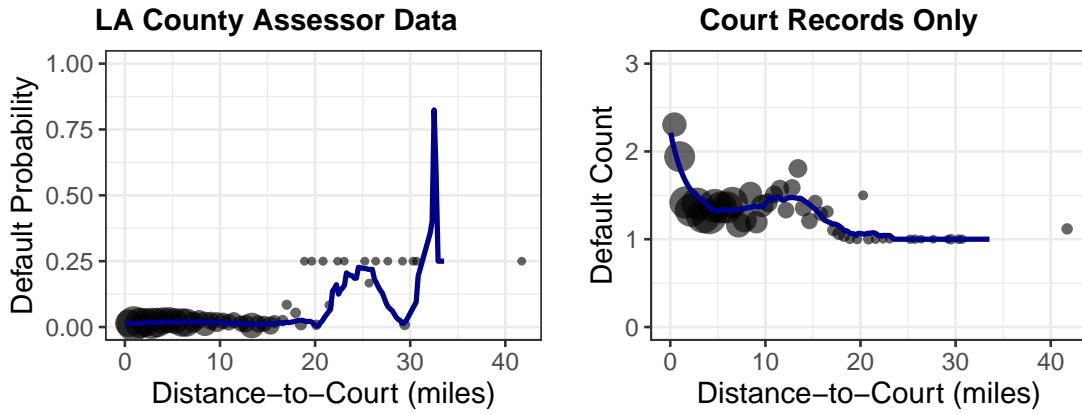
City Controller eviction notice data.



Appendix Figure C1. Reduced Form (LA County Assessor): 5-mile Binned Averages

Note: The blue lines are binned averages (every 5 miles) and the transparent points show default probabilities every 0.5 miles.

Local Linear Regressions (with Uniform Kernel)



Appendix Figure C2. Reduced Form Relationships: Default Probability & Default Counts

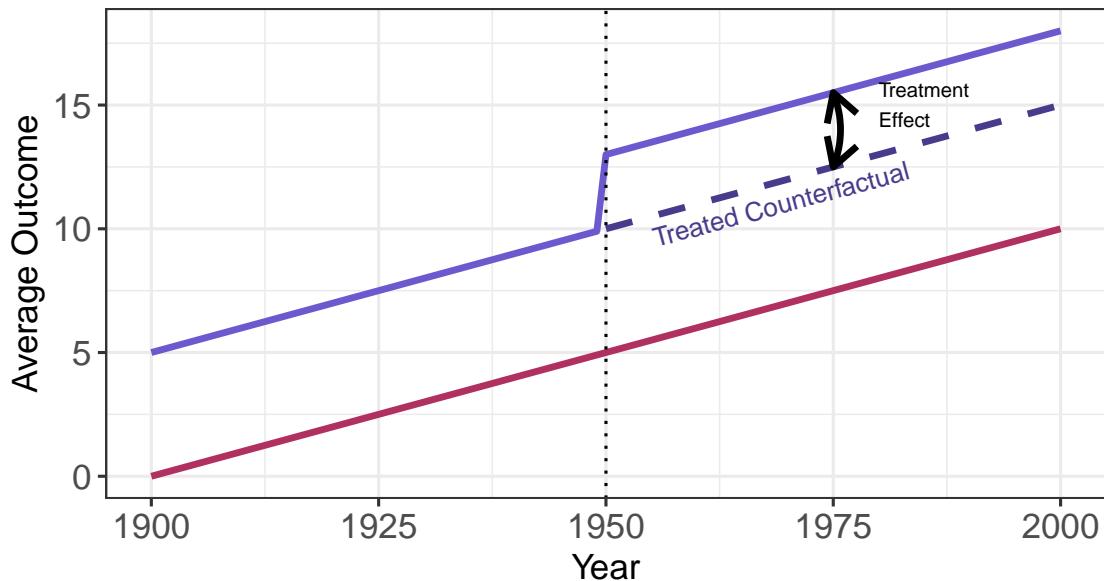
Note: The blue curves are local linear regression estimates (with a uniform kernel and bandwidth 5km) of the relationship between distance-to-court and default probability (left panel) or default counts (right panel).

C.2 DID Design Illustration

This subsection illustrates the DID counterfactual intuition. The plot below illustrates the average treatment effect on the treated estimation (ATT), where the ATT is the difference between the observed treatment group average outcome and the counterfactual outcome. The counterfactual outcome is assumed parallel to the control group outcomes and is shown below as a dashed blue line.

Difference-in-Differences (DID)

Treated and Control groups



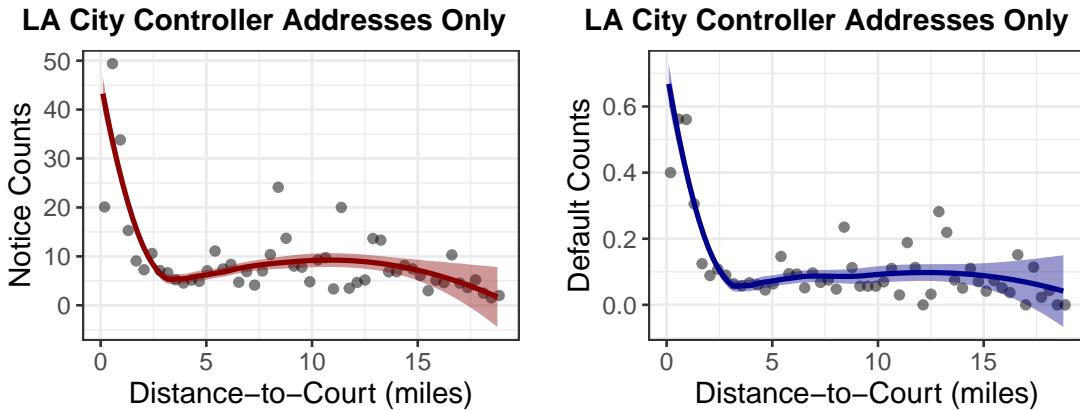
Appendix Figure C3. Difference-in-Differences (DID) Method

Note: Average outcomes for the treated and control groups are shown in blue and red, respectively. The dotted vertical line (at 1950) is the treatment date for the Treated group. The dashed blue line is the counterfactual for Treated units: this is the average outcome Treated units would have experienced if they were untreated. The average treatment effect on the treated (ATT) is the difference between the solid and dashed blue lines.

C.3 LA City Controller Data Only Results

To address potential concerns with using either assessor-based addresses as the population of rental addresses or only addresses with at least one observed default, I consider here using new data from the LA City Controller’s Office. A rule change in 2023 mandated that all landlords in LA City record eviction notices with the City Controller’s Office. The data includes address information for each eviction notice, allowing me to identify the universe of apartment buildings at-risk of eviction or default eviction. Using only these addresses in LA City, I reproduce the main-text results: reduced-form relationships and dynamic DID event-study type plots.

LA City Controller Averages (2023–2025)

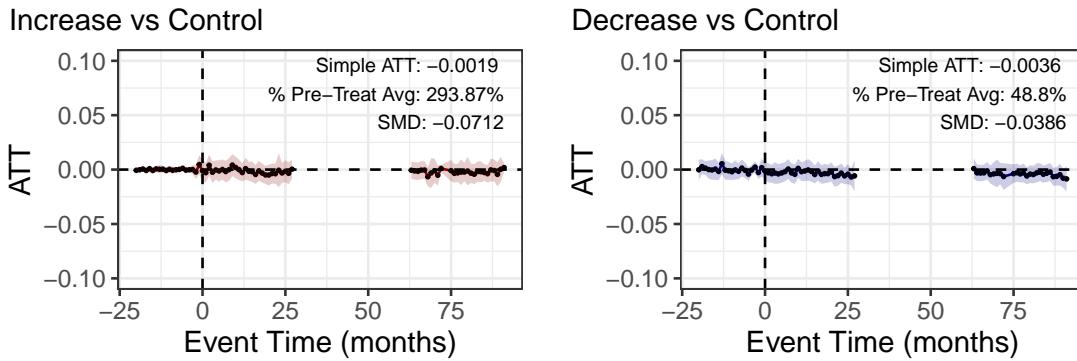


Appendix Figure C4. Reduced Form (LA City Controller Data)

Note: The left panel shows the smoothed LOESS estimates of the average number of notice counts as a function of distance-to-court. The right panel shows the smoothed LOESS estimates of the average number of defaults as a function of distance-to-court.

Default Count Dynamic ATTs

LA City Controller Addresses Only (2016–2025)

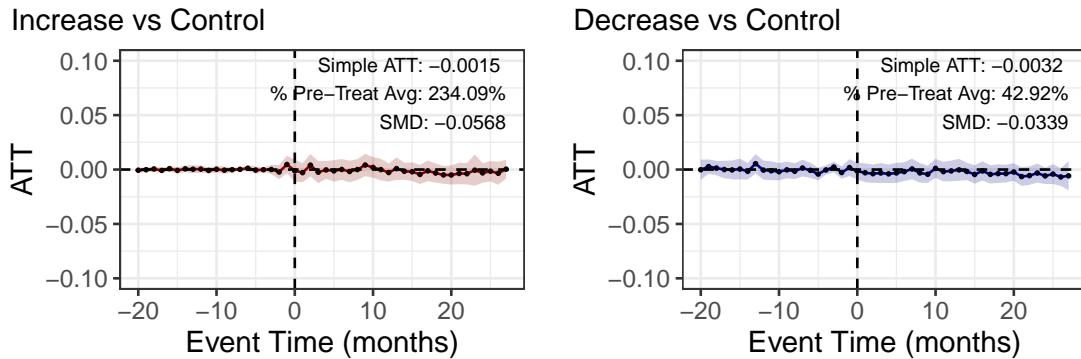


Appendix Figure C5. Dynamic DID Estimates (LA City Controller Addresses Only)

Note: The panels show estimates of the average treatment effect on the treated for Increase-Treated (left panel) and Decrease-Treated (right panel) units. The Simple ATT aggregates post-treatment ATT estimates as in Callaway and Sant'Anna (2021). The percent pre-treatment average is the ratio of the ATT and the cohort average outcome in the pre-treatment period. The standardized mean differences (SMDs) suggest small effects under standard rule-of-thumb interpretations.

Default Count Dynamic ATTs

LA City Controller Addresses Only (pre–2020)



Appendix Figure C6. Dynamic DID Estimates Pre-2020 (LA City Controller Addresses Only)

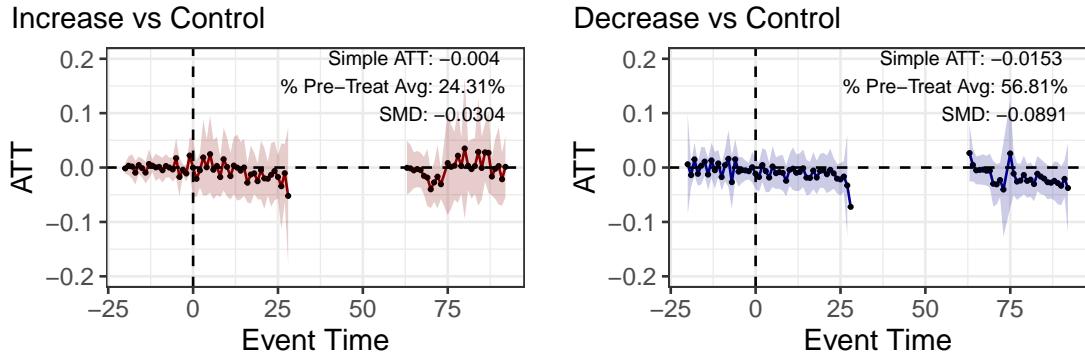
Note: The panels show estimates of the average treatment effect on the treated for Increase-Treated (left panel) and Decrease-Treated (right panel) units. The Simple ATT aggregates post-treatment ATT estimates as in Callaway and Sant’Anna (2021). The percent pre-treatment average is the ratio of the ATT and the cohort average outcome in the pre-treatment period. The standardized mean differences (SMDs) suggest small effects under standard rule-of-thumb interpretations.

C.4 Assessor Unit-Weighted Results

I also include results with outcomes weighted by the number of apartment units. Using the LA Assessor data, I match addresses in the eviction dataset to assessment data on how many units there are in each building. The exercise is complicated from a data perspective, as some rental units may be empty or matches to assessment data may be subject to assessor recording or matching error. But the DID-style estimates allow us to estimate how the court expansion affected tenants in all units living at the addresses in the eviction dataset. The weighted estimates are produced below for all years and for the pre-2020 years separately. The results are subject to very large uncertainty (much larger SEs) and insignificant in both the point estimates and the aggregate. Moreover, the direction of the effect does not align with expectations for the Increase versus Control comparisons.

Default Count Dynamic ATTs

Assessor Unit–Weighted

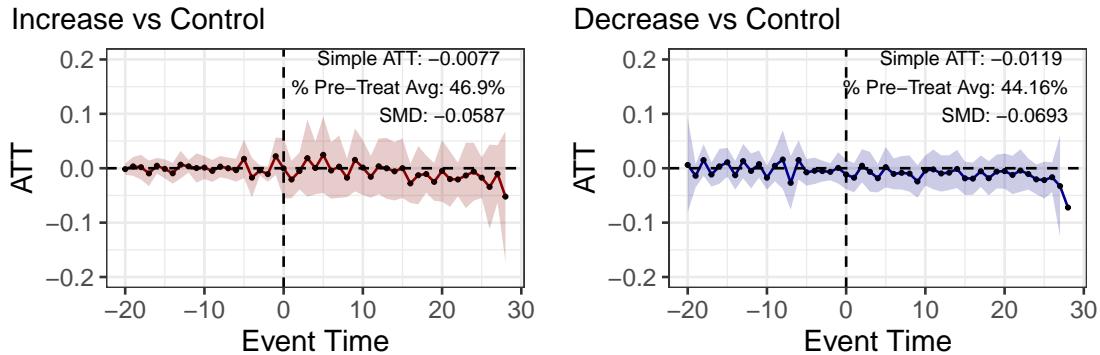


Appendix Figure C7. Dynamic DID Estimates (Assessor Unit-Weighted)

Note: The panels show estimates of the average treatment effect on the treated for Increase-Treated (left panel) and Decrease-Treated (right panel) units. The Simple ATT aggregates post-treatment ATT estimates as in Callaway and Sant'Anna (2021). The percent pre-treatment average is the ratio of the ATT and the cohort average outcome in the pre-treatment period. The standardized mean differences (SMDs) suggest small effects under standard rule-of-thumb interpretations.

Default Count Dynamic ATTs

Assessor Unit–Weighted (pre–2020)



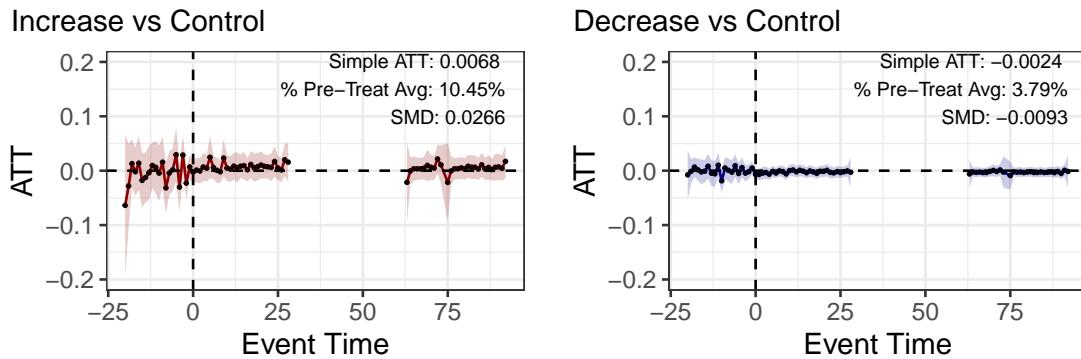
Appendix Figure C8. Dynamic DID Estimates Pre-2020 (Assessor Unit-Weighted)

Note: The panels show estimates of the average treatment effect on the treated for Increase-Treated (left panel) and Decrease-Treated (right panel) units. The Simple ATT aggregates post-treatment ATT estimates as in Callaway and Sant'Anna (2021). The percent pre-treatment average is the ratio of the ATT and the cohort average outcome in the pre-treatment period. The standardized mean differences (SMDs) suggest small effects under standard rule-of-thumb interpretations.

C.5 Pre-Aug 2017 Buildings Only Results

Finally, to address potential bias from including all buildings with observed evictions in the main analysis, I include results for only those buildings with observed default evictions pre-Aug 2017. The results for the Increase-Treated and Large Increase-Treated versus Control group are both insignificant; moreover, the Large Increase ATTs are mostly negative, which does not align with the predicted effect: those buildings with an increase in distance-to-court are expected to have increased defaults. Conversely, the Decrease-Treated and Large Decrease-Treated comparisons with the Control group are significant, but neither effect aligns with expectations: the point estimates and aggregate simple ATTs are all positive. The results reinforce the conclusions in the main body of the paper: either there is large uncertainty (Increase comparisons) or the results do not align with expectations (Decrease comparisons).

Default Count Dynamic ATTs Pre-Aug 2017 Buildings Only

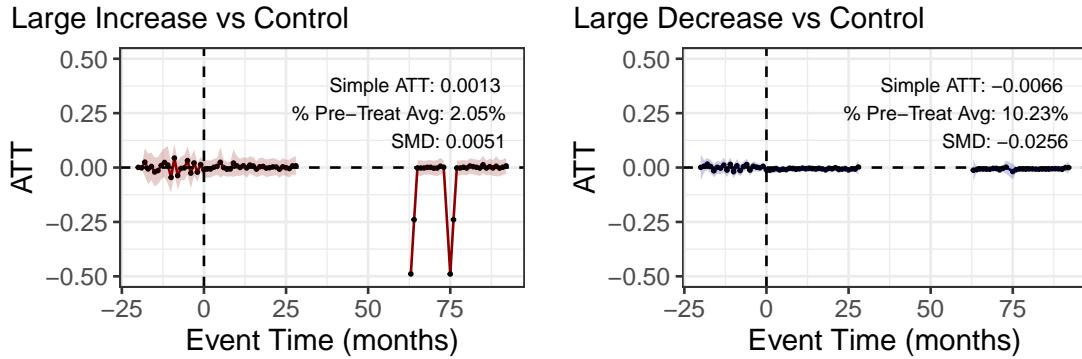


Appendix Figure C9. Dynamic DID Estimates (pre-Aug 2017 Buildings Only)

Note: The panels show estimates of the average treatment effect on the treated for Increase-Treated (left panel) and Decrease-Treated (right panel) units. The Simple ATT aggregates post-treatment ATT estimates as in Callaway and Sant'Anna (2021). The percent pre-treatment average is the ratio of the ATT and the cohort average outcome in the pre-treatment period. The standardized mean differences (SMDs) suggest small effects under standard rule-of-thumb interpretations.

Default Count Dynamic ATTs

Pre–Aug 2017 Buildings Only

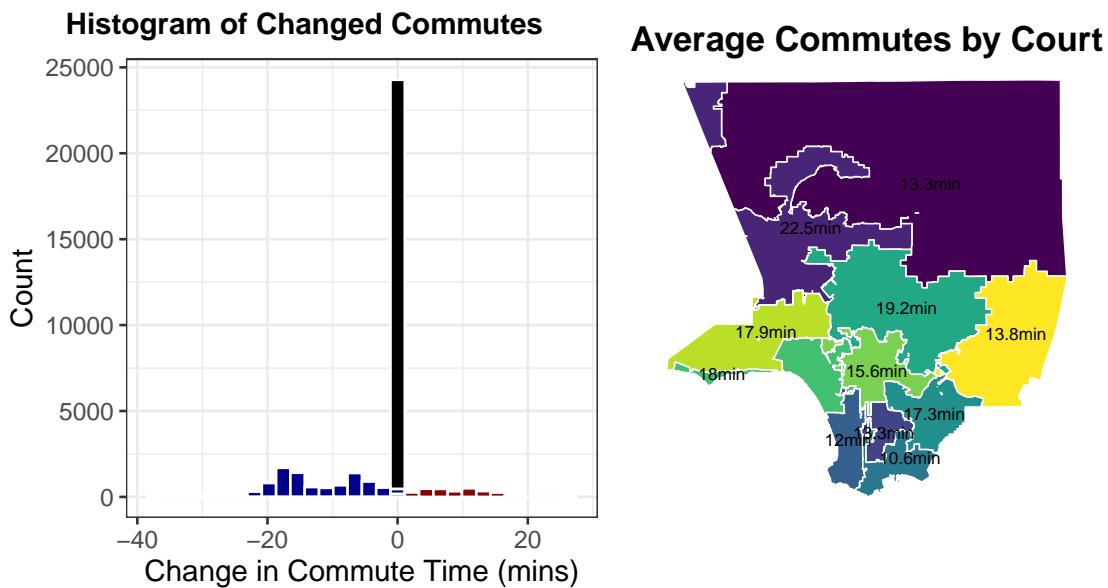


Appendix Figure C10. Dynamic DID Estimates: Above-Median Comparisons (pre-Aug 2017 Buildings Only)

Note: The panels show estimates of the average treatment effect on the treated for Increase-Treated (left panel) and Decrease-Treated (right panel) units. The Simple ATT aggregates post-treatment ATT estimates as in Callaway and Sant'Anna (2021). The percent pre-treatment average is the ratio of the ATT and the cohort average outcome in the pre-treatment period. The standardized mean differences (SMDs) suggest small effects under standard rule-of-thumb interpretations.

D Appendix D: Commute Times

Appendix D includes additional findings using commute times rather than distance-to-court as the treatment variable of interest. Commute times were calculated for each address using the Mapbox API.



Correlation: Distance-to-Court and Commute Time

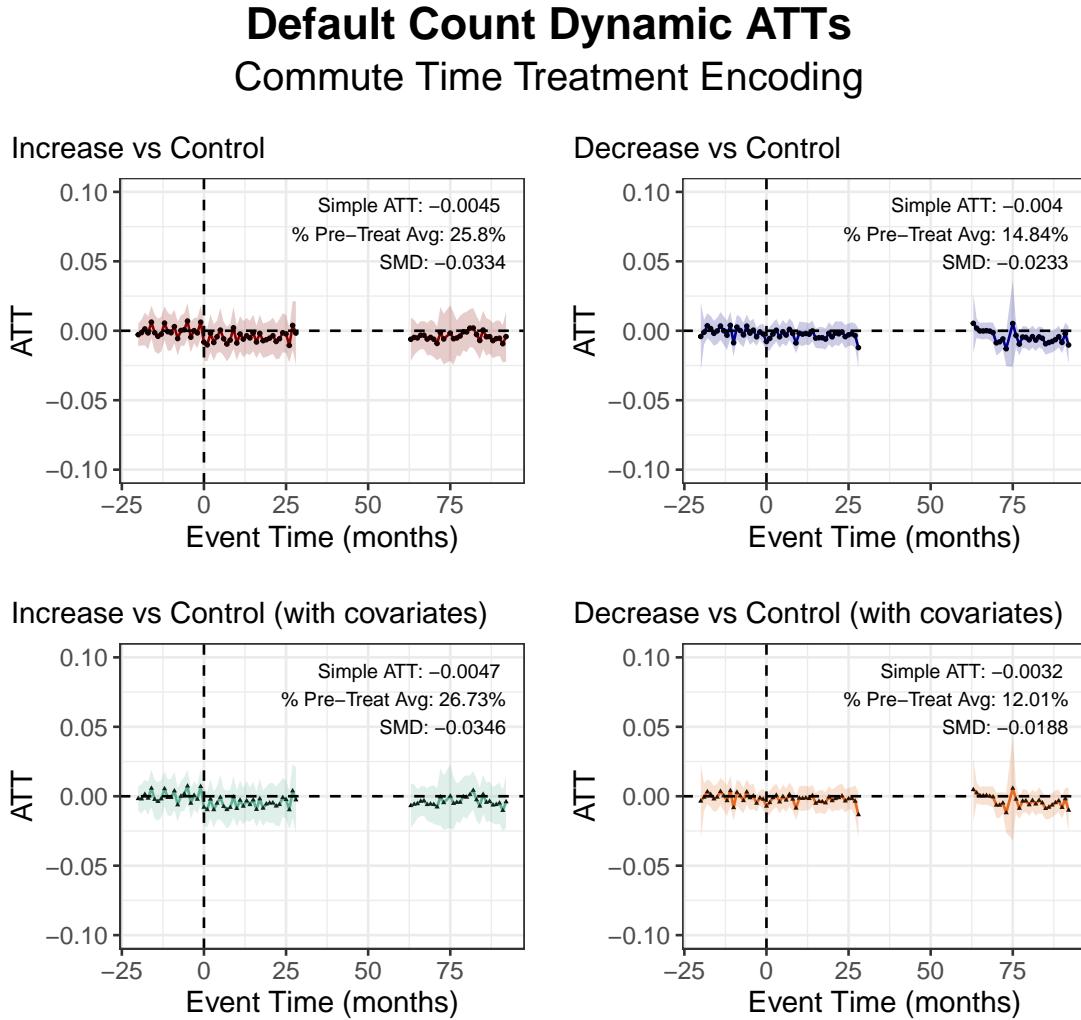
Overall Correlation = 0.848

Filing Courthouse	Correlation	N	Filing Courthouse	Correlation	N
Antelope Valley	0.965	3809	Pasadena	0.918	6677
Chatsworth	0.947	1715	Pomona	0.948	2082
Compton	0.871	1926	Santa Monica	0.925	4452
Inglewood	0.953	2229	Stanley Mosk	0.934	11080
Long Beach	0.914	7342	Van Nuys East	0.938	3327
Norwalk	0.963	5771	West Covina	0.765	2739

Appendix Figure D1

Note: The top-left panel shows the histogram of changes in commute times following the policy shock. The top-right panel maps the average commutes by court district under the post-reform assignment map. The bottom table shows the correlation between distance-to-court and commute times for each observed filing courthouse (overall corr. ≈ 0.848).

DID results are shown below. As in the other cases, the results are not significant, do not align with expectations as to the sign of the effect (e.g. Decrease-Treated and Large Decrease-Treated units have positive aggregate simple ATTs), or both. Note as well the small standardized mean differences (SMDs) in each panel.

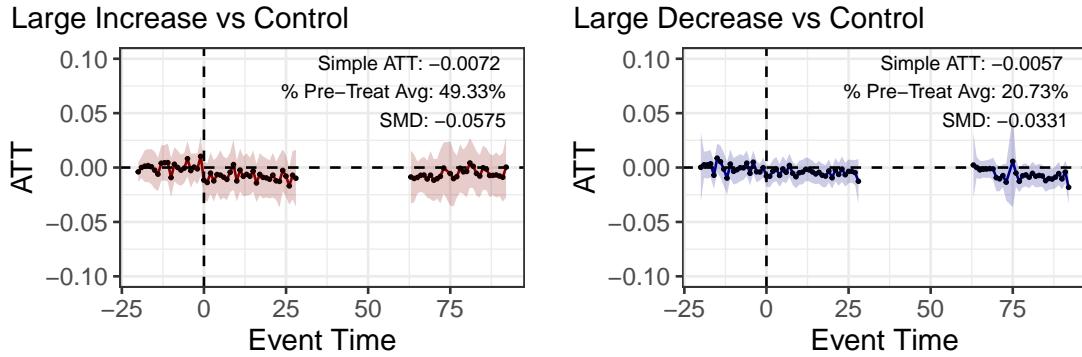


Appendix Figure D2. Dynamic DID Estimates: Comparisons with Controls (Commute Times)

Note: The panels show estimates of the average treatment effect on the treated for Increase-Treated (left panel) and Decrease-Treated (right panel) units. The Simple ATT aggregates post-treatment ATT estimates as in Callaway and Sant'Anna (2021). The percent pre-treatment average is the ratio of the ATT and the cohort average outcome in the pre-treatment period. The standardized mean differences (SMDs) suggest small effects under standard rule-of-thumb interpretations. The covariate models include gender, race, and income variables.

Default Count Dynamic ATTs

Commute Time Treatment Encoding



Appendix Figure D3. Dynamic DID Estimates: Above-Median Comparisons with Controls (Commute Times)

Note: The panels show estimates of the average treatment effect on the treated for Large Increase-Treated (left panel) and Large Decrease-Treated (right panel) units. The Simple ATT aggregates post-treatment ATT estimates as in Callaway and Sant'Anna (2021). The percent pre-treatment average is the ratio of the ATT and the cohort average outcome in the pre-treatment period. The standardized mean differences (SMDs) suggest small effects under standard rule-of-thumb interpretations.

E Appendix E: Relaxing Parallel Trends

Appendix E includes additional results which weaken the key identification assumption. Meta-analyses of existing DID research suggests violations of parallel trends is common and requires validation, sensitivity analysis, or relaxation.⁵⁹ Although there are a few ways to relax the parallel trends assumption, I examine here a Manski-style partial identification approach following Manski & Pepper (2018)⁶⁰ and Estes & Clark (2025).⁶¹ This approach easily accommodates a form of sensitivity analysis to a tuning parameter, which I discuss below.

To begin, I specify the treatment effect estimand of interest: the *average treatment effect on the treated* (“ATT”) for units in two treatment cohorts $\vec{k} = (k_1, k_2, \dots)$ and $\vec{k}' = (k'_1, k'_2, \dots)$. This notation for treatment \vec{k} says that k_1 is the treatment level received at time 1, k_2 the treatment level at time 2, etc. The average treatment effect in time t on units treated with \vec{k} :

$$ATT(t, \vec{k}, \vec{k}') = \mathbb{E}[Y_{it}(\vec{k}) - Y_{it}(\vec{k}') | \vec{K}_i = \vec{k}]$$

This is the average treatment effect from switching to treatment \vec{k}' for units on treatment \vec{k} at time t . The identification problem is that $Y_{it}(\vec{k}')$ is unobservable for units with observed treatment $\vec{K}_i = \vec{k}$.

For two distinct treatment histories \vec{k} and \vec{k}' , let the first time period the treatments diverge (i.e. do not agree) be denoted $T_{div} = \text{argmin}_t(k_t \neq k'_t)$. I use information from the pre-divergence periods (i.e. time periods before T_{div}) and the following assumption to impute counterfactual outcomes.

Assumption 1. [Average Bounded Deviation] Let $T_{div} = \arg\min_t(k_t \neq k'_t)$ for distinct treatments \vec{k} and \vec{k}' . Then the following holds for each i , $t \geq T_{div}$, any (\vec{k}, \vec{k}') pair, and some $C \in \mathbb{R}_+$:

$$\left| \mathbb{E}[Y_{it}(\vec{k}') | \vec{K}_i = \vec{k}] - \mathbb{E}[Y_{it}(\vec{k}') | \vec{K}_i = \vec{k}'] \right| \leq C \cdot \max_{t < T_{div}} \left| \mathbb{E}[Y_{it} | \vec{K}_i = \vec{k}] - \mathbb{E}[Y_{it} | \vec{K}_i = \vec{k}'] \right|$$

where $C = C(t, \vec{k}, \vec{k}')$ can depend on the time period and both treatments.

⁵⁹ See Albert Chiu, Xingchen Lan, Ziyi Liu, and Yiqing Xu, *Causal Panel Analysis under Parallel Trends: Lessons from a Large Reanalysis Study*, AMERICAN POLITICAL SCIENCE REVIEW (2025).

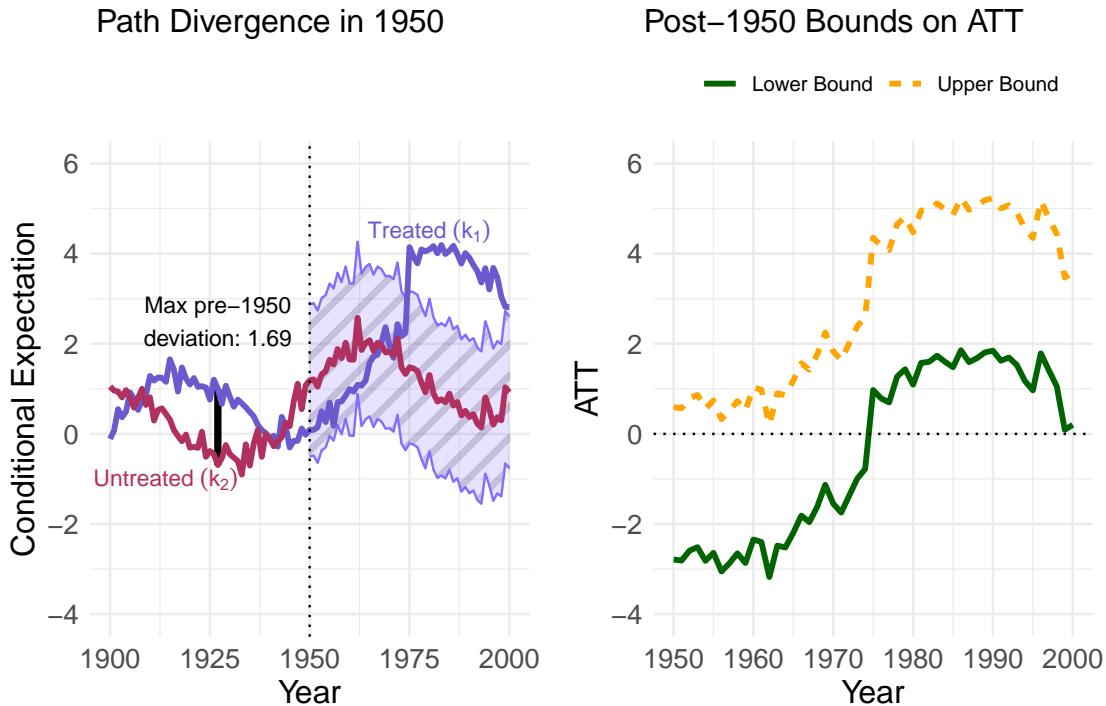
⁶⁰ Charles F. Manski and John V. Pepper, *How Do Right-to-Carry Laws Affect Crime Rates? Coping with Ambiguity Using Bounded-Variation Assumptions*, 100 REV. ECONOMICS & STATISTICS 232 (2018).

⁶¹ Matthew Estes and Ransi Clark, *Courting the Academy: The Judicial Role in Popularizing Legal Scholarship*, Working Paper (2025).

This assumption says the counterfactual average outcome from treating with \vec{k}' instead of \vec{k} for units with observed treatment $\vec{K}_i = \vec{k}$ is “near” the observed outcome for units with treatment \vec{k}' . Specifically, the counterfactual average for the \vec{k} -treated group is within C times the maximum absolute difference in means for units on the two treatments before they diverge in time.

The method is illustrated graphically below in [Figure E1](#). In the left panel, the two lines are average outcomes for Treated (blue) and Untreated (red) units. The black-dotted vertical line is the path divergence time period, which in this example is 1950. The blue-striped region is the counterfactual region for blue Treated units: this is where counterfactual outcomes for Treated units would be if they had been Untreated. The maximum pre-divergence deviation is 1.69 and is shown by the black vertical line around 1925. In the right panel, the y -axis is the average treatment effect on the treated (ATT), with a black-dotted horizontal line plotted for zero effect. The upper and lower bounds on the ATT for each post-1950 time are plotted as dashed-yellow and solid-green lines, resp.

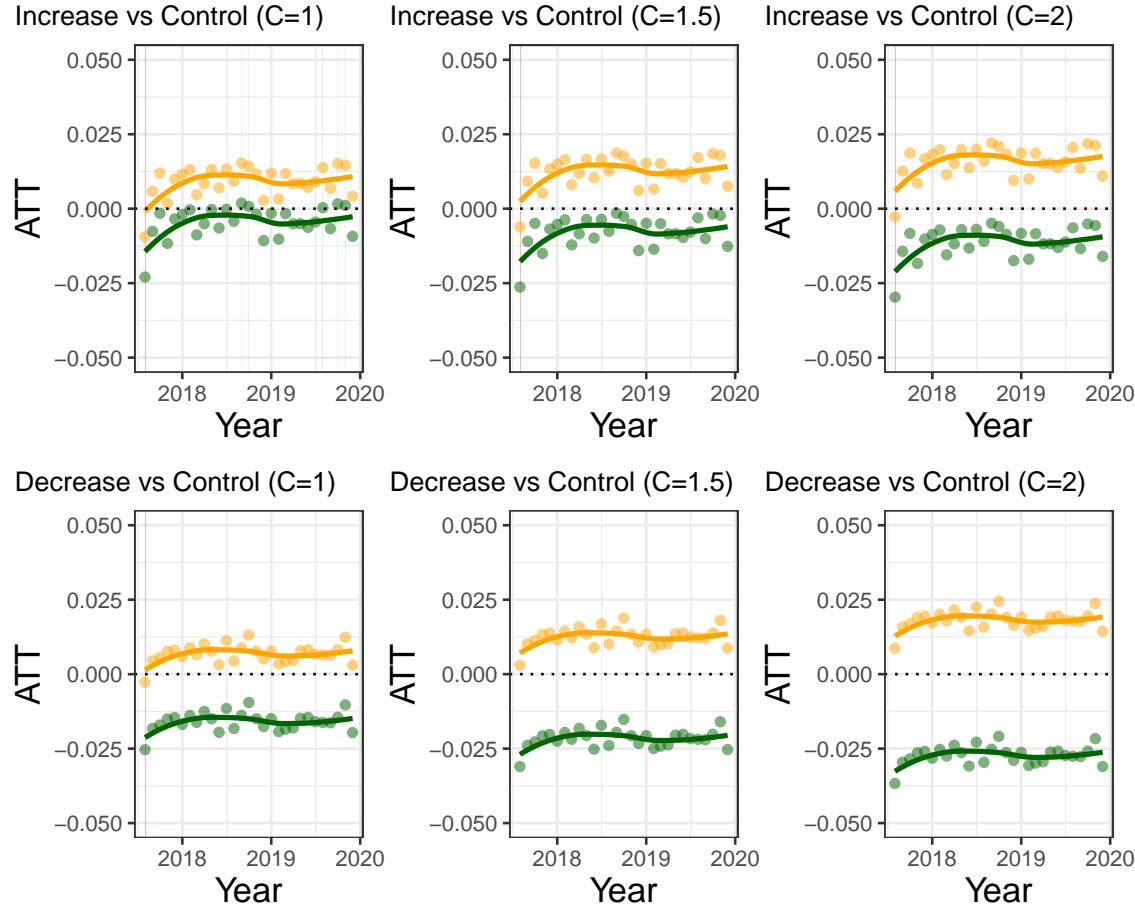
Bounded Deviation Identification (Fixed C=1.0)



Appendix Figure E1. Partial Identification in the Bounded Deviations Approach
Note: The figure is reproduced from Estes & Clark (2025).

The upper and lower bounds implied by this exercise for three different C values are shown in [Figure E2](#). The top row shows estimated ATT bounds comparing the Increase-Treated and Control units, whereas the bottom row shows estimated ATT bounds comparing the Decrease-Treated and Control units.

Comparing Treated Cohorts with Control Upper and Lower Bounds post-Aug 2017



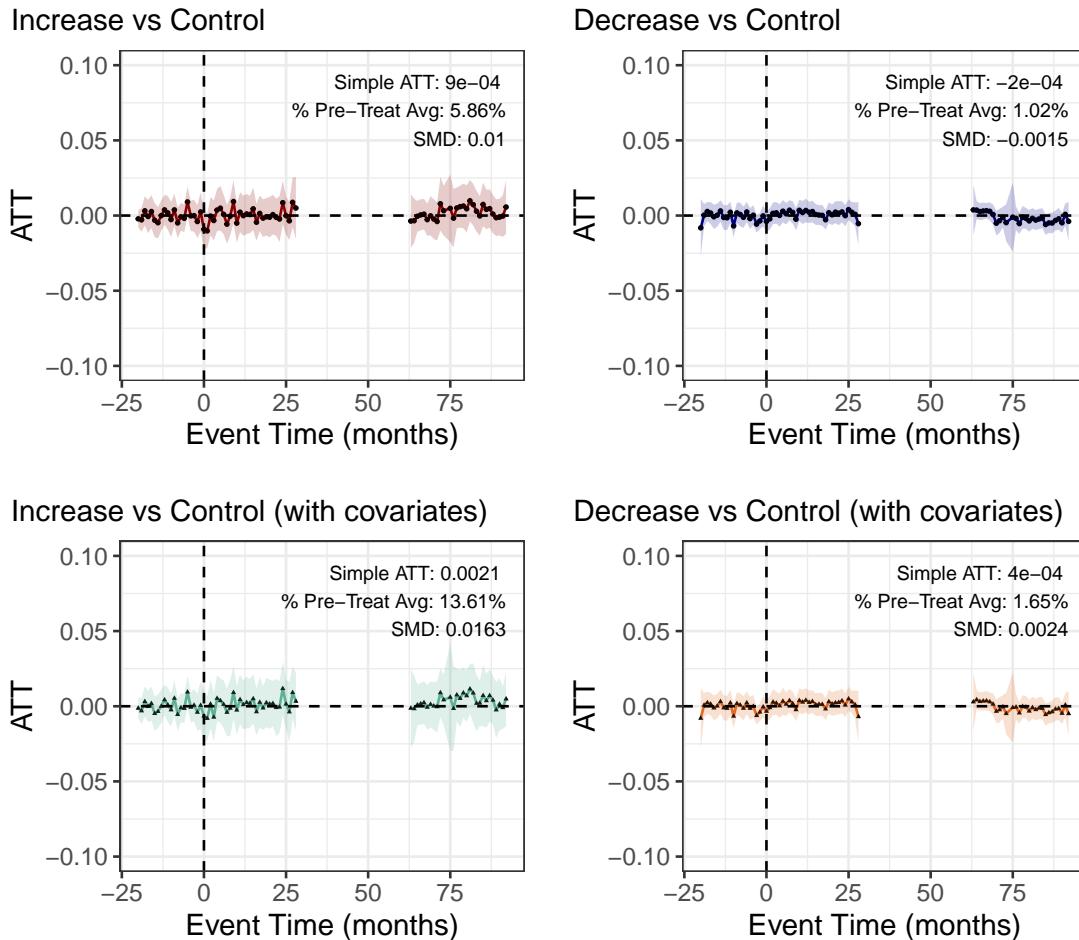
Appendix Figure E2. Relaxing Parallel Trends: Bounded Deviations

Note: The upper (yellow) and lower (green) bounds on the ATT in each post-divergence time are shown from Aug. 2017–Dec. 2020. The points show estimates in each time period, and the solid lines are smoothed fits of those point estimates. The maximum deviations in the pre-divergence period are approximately 0.00674 (Increase vs Control) and 0.0114 (Decrease vs Control).

F Appendix F: All Observed Evictions

Appendix F includes DID results using any observed eviction as the outcome-of-interest. Results from the baseline specification—with and without the same covariates—are shown below.

All Observed Residential Evictions Dynamic ATTs



Appendix Figure F1. Dynamic DID Estimates: All Observed Outcomes

Note: The panels show estimates of the average treatment effect on the treated for Large Increase-Treated (left panel) and Large Decrease-Treated (right panel) units. The Simple ATT aggregates post-treatment ATT estimates as in Callaway and Sant'Anna (2021). The percent pre-treatment average is the ratio of the ATT and the cohort average outcome in the pre-treatment period. The standardized mean differences (SMDs) suggest small effects under standard rule-of-thumb interpretations. The covariate models include gender, race, and income variables.