Courts and the Policing of Homelessness

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Abstract

Between 2018 and 2024, two federal rulings restricted Western U.S. cities' ability to cite individuals for sleeping or camping in public unless adequate shelter was available. This limitation was widely blamed for contributing to rising visible homelessness. Using novel data on enforcement and homeless encampments from more than 50 cities along with a synthetic difference-in-differences design, we do not find clear evidence that the rulings reduced enforcement or increased visible homelessness in aggregate. Cities responded in varied ways: many did not alter citation behavior, whereas those that did often substituted toward alternative mechanisms such as encampment clearings. When the Supreme Court reversed the restrictions in 2024, citations rose differentially in affected cities, but homelessness did not decline either in level or in rate of growth. These results suggest that federal constraints on local enforcement may have limited and inconsistent effects when cities can adapt along unregulated margins.

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1 Introduction

On a single night in January 2024, more than 270,000 Americans slept outside or in places not meant for human habitation. The unsheltered homeless population has grown by nearly 60% since 2014, far outpacing population growth over the same period. This crisis imposes severe costs: people experiencing homelessness face mortality risk approximately 3.5 times higher than the housed population, and visible homelessness can shape local economic activity, public safety, public health, and urban quality of life (see, e.g., Meyer, Wyse, and Logani 2023; Christopher, Duggan, and Martin 2025).

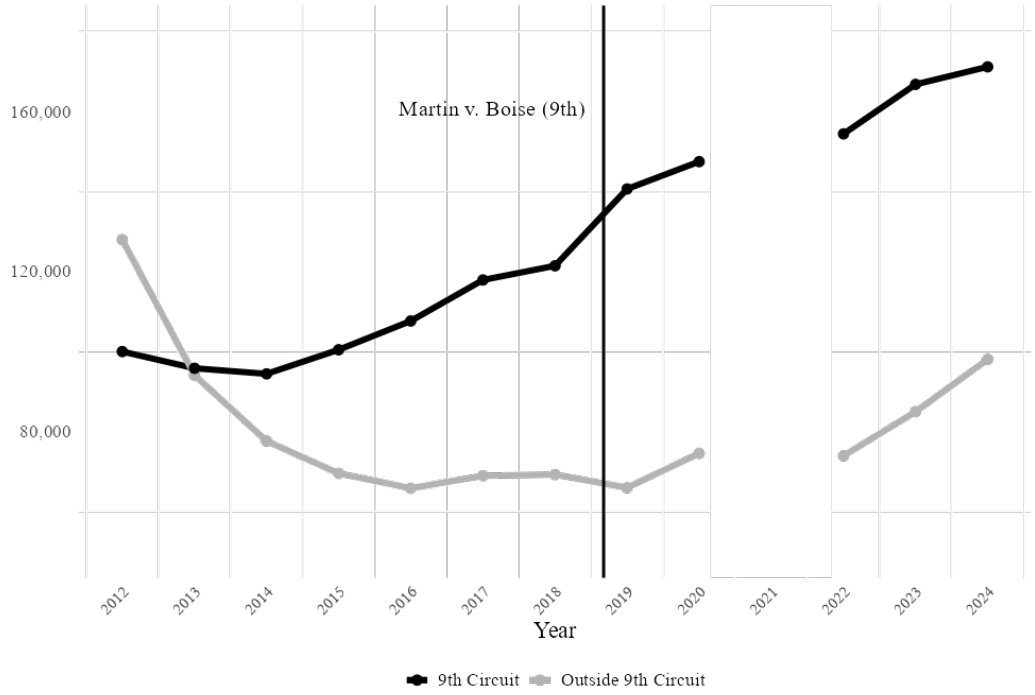
Addressing these social and economic costs falls primarily to local governments. Cities play a central role in homelessness policy: they often contribute significant portions of local shelter funding, and they regulate the use of public space through ordinances that prohibit camping or sleeping outdoors. These ordinances are typically enforced by city police, who may issue citations to individuals sleeping or camping in public, with penalties ranging from civil fines to criminal charges. In 2018, however, the Ninth Circuit Federal Court of Appeals – the federal appellate court covering much of the western United States[1] – ruled in *Martin v. Boise* that cities could not constitutionally enforce these ordinances through criminal citations unless they could also provide adequate emergency shelter space (Martin v. City of Boise 2018). This ruling was strengthened in 2022 by the Ninth Circuit's decision in *Johnson v. Grants Pass*, which extended the prohibition to civil citations, before being ultimately overturned by the U.S. Supreme Court in 2024 (Johnson v. City of Grants Pass 2022; City of Grants Pass v. Johnson 2024).

These court decisions quickly became the focus of intense controversy and were widely blamed for surging visible homelessness across Western U.S. cities. For example, the Mayor of Boise argued that the *Martin* ruling "hamstrung more than 1,600 Western municipalities in their efforts to control homeless encampments in parks and on sidewalks" (La Ganga 2019). This attribution seemed plausible: levels of unsheltered homelessness were indeed much higher in the Ninth Circuit than in the rest of the country during this period, as shown by Figure 1. When *Grants Pass* reached the Supreme Court, dozens of amicus briefs from Western cities echoed similar concerns. The Supreme Court's 2024 decision explicitly cited this reasoning, expressing concern that *Martin* had "shackle[d] the hands of public officials trying to redress the serious societal concern of homelessness... [and wrought] havoc on local governments, residents, and businesses across the American West" (Supreme Court of the United States 2024). Even some legal scholars claimed the decisions "changed the face

[1]Ninth Circuit states include Alaska, Arizona, California, Hawaii, Idaho, Montana, Nevada, Oregon and Washington. See Appendix Figure A1 for an illustrative map.

of homelessness in America..." with the result that "America's homeless increasingly began residing in long-term encampments."(Versteeg, Cope, and Mukherjee 2025a).

Figure 1: Unsheltered Homelessness Count



Note: This figure plots the total count of people experiencing unsheltered homelessness in the Ninth Circuit (black line) and in the rest of the U.S. (gray line), derived from HUD's yearly Point-in-Time (PIT) count. 2021 is omitted because the PIT count was not conducted consistently in 2021.

Despite the prominence of these claims in judicial reasoning and public discourse, no empirical studies have systematically evaluated whether enforcement patterns, shelter provision, or visible homelessness actually changed significantly following the decisions. This paper provides the first comprehensive empirical analysis of how the *Martin v. Boise* and *Grants Pass* court decisions impacted city policies and homelessness-related outcomes.

We examine how the rulings affected two key groups of actors. First, we examine the actions of cities, and ask whether cities (1) reduced citations for camping and sleeping violations following *Martin*, (2) shifted to alternative enforcement mechanisms such as encampment sweeps, or (3) strategically expanded emergency shelter capacity to regain citation authority. Cities are the natural unit of analysis because both enforcement and service provision are typically governed and funded at the municipal level: city police departments enforce anti-camping ordinances, and cities often substantially finance emergency shelters. Second, we examine the actions of people experiencing homelessness (PEH), asking whether reduced

enforcement altered where and how individuals sought shelter or chose to sleep. In particular, we assess whether these potential behavioral responses–such as moving across jurisdictions, remaining outdoors rather than entering shelters, or forming encampments–contributed to increases in visible homelessness in affected cities.

We begin by developing the causal legal framework to clarify the pathways through which the court decisions could influence outcomes. This framework yields predictions which guide our empirical analysis. To test these predictions, our empirical approach exploits the fact that the Ninth Circuit *Martin* (2018) and *Grants Pass* (2022) decisions applied only within the Ninth Circuit, creating sharp differences in legal constraints across otherwise similar cities.[2] As a result, we can compare Ninth Circuit cities subject to these rulings with cities in other circuits that faced no such restrictions.

Consider, for example, Stockton, CA (Ninth Circuit) and Colorado Springs, CO (Tenth Circuit). In January 2018, just before the *Martin* decision, both cities reported similar levels of unsheltered homelessness (79 and 77 individuals per 100,000 residents, respectively). However, Stockton became bound by the Ninth Circuit's *Martin* and later *Grants Pass* decisions, whereas Colorado Springs did not. Following *Martin*, we find that, relative to cities like Colorado Springs, Stockton reduced its enforcement of anti-camping and anti-sleeping ordinances and substituted toward enforcing other ordinances–such as those prohibiting aggressive solicitation or the possession of shopping carts. As a result, the overall level of homelessness-related citations in Stockton remained roughly constant.

A key challenge in this comparison is that homelessness trends can differ substantially across regions. To address large preexisting differences in homelessness trends across circuits, we use the synthetic difference-in-differences (SDID) method (Arkhangelsky et al. 2021). Using this method, we construct a synthetic comparison group with more closely matched pretreatment trends for each outcome variable. We also provide the first empirical application of recent advances in variance estimation for SDID, which flexibly account for heteroskedasticity across time and units (Almeida et al. 2025). In our paper, we extend these methods to allow for multiple treated units and time periods.

We conduct our analysis using novel data that we assembled from more than 140 public records requests. We use the administrative data from these requests to compile detailed new measures of enforcement actions, including citations and encampment sweeps. We also

[2]Note that other circuits had rejected similar Eighth Amendment challenges. See, e.g., *Joel v. City of Orlando*, 232 F.3d 1353 (11th Cir. 2000); *Johnson v. City of Dallas*, 61 F.3d 442 (5th Cir. 1995). In 2019, the Fourth Circuit held that a statutory scheme that criminalizes habitual alcoholism was unconstitutionally vague as applied to a class of alcoholics, *Manning v. Caldwell*, 930 F.3d 264, 280-283 & n.17 (4th Cir. 2019) (agreeing with *Martin*). But the Fourth Circuit had not ruled directly on the application of this doctrine to homeless individuals.

generate new, high-frequency measures of visible homelessness based on citizen-reports of homelessness and encampments. We complement this systematic and comprehensive data creation effort with data from on arrests from the FBI, and on emergency shelter capacity from the department of Housing and Urban Development (HUD).

Our findings challenge the prevailing narrative about the effects of these court decisions. First, we find limited aggregate impacts on both enforcement and visible homelessness. While some cities adjusted their enforcement behavior, the changes were neither dramatic nor uniform across jurisdictions. Moreover, while unsheltered homelessness rose substantially in the Ninth Circuit during this period, our analysis reveals that this trend was well underway before *Martin* was decided. When we compare affected cities to similar unaffected cities,we find no clear change in either the level or growth of unsheltered homelessness that could be attributed to the decision. These null findings challenge claims that *Martin* fundamentally transformed homelessness policy in practice across the American West.

Second, we document pronounced heterogeneity in how cities responded to *Martin* and *Grants Pass*. Rather than uniform compliance or resistance, enforcement practices varied widely: some reduced citations, others increased them, and many made no noticeable adjustment. We exploit this variation to test the deterrence hypothesis, asking whether cities that changed enforcement the most also saw the largest shifts in visible homelessness. We find no systematic relationship, which challenges the premise that citations materially deter unsheltered homelessness.

Third, we show that this heterogeneity reflects, in part, substitution across enforcement margins. Much of cities' enforcement capacity appears to operate outside the specific citation categories constrained by *Martin*. For example, among cities that reduced anti-sleeping and anti-camping citations, some substituted towards to other citation types. We also find evidence that encampment "sweeps," which are administrative operations that forcibly clear camps and often confiscate property, continued and in some cases intensified during the period when citation authority was formally limited.

This pattern is consistent with a policy substitution mechanism: when one enforcement tool is restricted, cities substitute toward others. This aligns with economic intuition and with a broader literature showing that limiting one behavior margin often leads to shifts toward less regulated substitutes rather than overall reductions in activity (see, e.g: Alpert, Powell, and Pacula 2018; Peltzman 1975; Tanaka, Teshima, and Verhoogen 2022).

The heterogeneity also likely reflects different risk tolerance across cities. Since compliance depended on the threat of litigation rather than automatic enforcement, many jurisdictions appear to have delayed or minimized policy changes unless directly challenged. Thus, the practical force of the rulings was uneven, shaped less by the legal constraints themselves

than by local institutional capacity, political priorities, and perceived legal risk.

Finally, and consistent with the limited enforcement response, we do not find strong evidence that cities increased emergency shelter capacity following the rulings. Ninth Circuit jurisdictions did add shelter beds during this period, but the increases were not statistically significant and generally failed to keep pace with growing need. These patterns suggest that the *Martin* constraint was often too weak to compel action, and that even where cities sought to comply, the pathway it envisioned–expanding shelter to restore enforcement–proved difficult to implement in practice.

This paper makes several contributions. First, we provides the first systematic empirical evaluation of the *Martin v. Boise* and *Grants Pass* court decisions that have dominated homelessness policy discourse for several years. Our null findings on visible homelessness challenge widespread claims in judicial opinions, policy debates, and legal scholarship about these rulings' transformative effects.

Second, we contribute to research on measuring and tracking homeless populations. Prior work highlights both the difficulty and importance of obtaining accurate data on unsheltered homelessness. For example, Meyer, Wyse, and Corinth (2023) emphasize the limitations of official counts and propose methodological improvements, while Bhandari (2024) compares 311 report data to HUD counts in Los Angeles and finds that they co-move. O'Flaherty (2019) provides a broader review of the econometric challenges that arise from such measurement issues. In our paper, we assemble new data combining detailed administrative records on enforcement with high-frequency measures of visible homelessness and encampments across over 50 cities. This represents, to our knowledge, the first systematic effort to compile and harmonize this siloed administrative data across jurisdictions.

The most closely related papers are Lebovits and Sullivan (2025) and Versteeg, Cope, and Mukherjee (2025b). Lebovits and Sullivan (2025) use a difference-in-differences design to study changes in homelessness following the adoption of criminalization ordinances, and find no evidence that these laws affect homelessness levels. We extend this literature by exploiting plausibly exogenous variation from Federal and Supreme Court rulings and by assembling new, citation-level enforcement data paired with high-frequency measures of unsheltered homelessness. Versteeg, Cope, and Mukherjee (2025b) provide a legal analysis of the *Martin* and *Grants Pass* decisions, but do not examine their empirical effects. Our paper contributes by providing an empirical analysis, directly testing theoretical claims about how court-imposed constraints shape local enforcement practices and visible homelessness.

More broadly, our findings speak to the limits of federal legal interventions when local governments are responsible for implementation. Prior work has explored this question other domains, such as school desegregation (see, e.g.: Lutz 2011). Our results show that legal

constraints can be partially offset through policy substitution: when one enforcement tool is restricted, cities may shift toward other tools. This mechanism is well understood in economic theory and is consistent with broader evidence on policy substitution discussed above. Together, these patterns suggest that the effectiveness of federal policy depends not only on formal legal rules, but also on local capacity, political incentives, and the ease with which jurisdictions can substitute across instruments.

Finally, methodologically, we advance the application of synthetic difference-in-differences methods in policy evaluation. We implement SDID alongside traditional two-way fixed effects designs, and we provide the first empirical implementation of the variance estimation procedures proposed in Almeida et al. (2025), and we extend this to the setting with multiple treated periods. Recent applications of SDID have examined diverse policy questions including early literacy programs (Spencer 2024) and health insurance coverage (Hackmann et al. 2025). Our application contributes to this growing literature showcasing how SDID can address identification challenges when treated and control regions exhibit diverging pre-treatment trajectories.[3]

Taken together, our findings suggest that the *Martin v. Boise* and *Grants Pass* decisions had limited causal effects on visible homelessness or shelter provision. Although these rulings reshaped the legal and political discourse around homelessness, our evidence points to modest and heterogeneous city responses, characterized by creative adaptation and substitution. Ultimately, the legal constraints imposed by the courts did not fundamentally reshape local homelessness policy in practice, but instead prompted incremental adjustments within existing institutional and political limits.

2 Legal Background and Institutional Setting

2.1 U.S. Homeless Services System

Homelessness services in the United States operate through a complex system of federal, regional, and local entities. The U.S. Department of Housing and Urban Development (HUD) serves as the primary federal agency coordinating homelessness services. HUD defines a person experiencing homelessness (PEH) as someone "who lacks a fixed, regular, and adequate nighttime residence." A PEH can be sheltered or unsheltered. Sheltered homelessness "refers to people who are staying in emergency shelters, transitional housing programs, or

[3]Methodologically, we also join a literature using court rulings as quasi-experimental shocks to identify causal effects. This approach parallels work examining the impact of *Citizens United* on campaign finance and political outcomes (Klumpp, Mialon, and Williams 2016; Slattery, Tazhitdinova, and Robinson 2023), and research on the effects of voting rights decisions (Billings et al. 2024).

safe havens.“ Unsheltered homelessness—the form most visible to the public—refers to those “whose primary nighttime location is a public or private space not designated for, or ordinarily used as, a regular sleeping accommodation for people (for example, the streets, vehicles, or parks)“ (U.S. Department of Housing and Urban Development 2023).

Emergency shelters (ES)–the main type of shelter referenced in the court rulings–provide temporary overnight accommodation.These facilities often operate at capacity and may have restrictions on length of stay.[4] In 2024, ES beds represented 39% of the total shelter beds provided in the United States.[5]

Homelessness services are coordinated through Continuums of Care (CoCs), which are regional or local planning bodies that often correspond to county boundaries. This system encompasses more than 400 CoCs nationwide. CoCs coordinate several types of shelter and housing assistance. The CoC system was established to streamline funding applications and encourage coordination among housing and service providers at a local level, and CoCs are the main vehicle through which homelessness-related data collection occurs. However, the system inadvertently creates potential coordination challenges since CoC boundaries do not always align with city or county jurisdictions.

2.2 Anti-Homelessness Enforcement

For much of U.S. history, being homeless was illegal, deriving from British laws prohibiting vagrancy.[6] Even today, most U.S. cities maintain a complex web of “quality of life” ordinances that regulate the use of public spaces. These ordinances typically prohibit specific behaviors associated with homelessness, including: sleeping or camping in public spaces, sitting or lying on sidewalks during business hours, storing personal property in public areas, urinating or defecating in public, panhandling, and loitering.[7] Cities argue that these

[4]Other housing services include: Transitional housing (TH), which offers longer-term temporary housing, typically up to 24 months, with supportive services. Safe havens provide low-barrier permanent housing for hard-to-serve individuals, particularly those with mental illness. Permanent supportive housing (PSH) combines long-term housing assistance with supportive services for individuals with disabilities. Rapid rehousing (RRH) provides short-term rental assistance and services to help people obtain permanent housing quickly. CoCs also provide non-housing supportive services, such as help with substance abuse treatment services, childcare, and transportation.*See* https://www.hudexchange.info/homelessness-assistance/coc-esg-virtual-binders/coc-eligible-activities/supportive-services/.

[5]Authors’ calculation from the HUD HIC.

[6]See Simon (1992) at 635-641; Goluboff and Sorensen (2018).

[7]These ordinances can vary significantly from town to town. For example, Sacramento’s city code holds that “[i]t is unlawful and a public nuisance for any person to camp, occupy camp facilities, or use camp paraphernalia“ in any public or private property, and considers a violation to be a misdemeanor (a minor criminal offense) (Sacramento City Code § 12.52.030). Meanwhile, Seattle only prohibits camping in public parks (except for places in which it is explicitly permitted) (Seattle City Code § 18.12.250). At the same time, Seattle prohibits sitting or lying down on public sidewalks in the downtown or neighborhood commercial zones, with exceptions for people with medically confirmed disabilities and benches supplied by a public

ordinances are necessary to maintain public health and safety, protect business interests, and preserve public spaces for their intended uses (Livingston 1997).

The enforcement of these ordinances falls primarily to local police departments, who can issue either civil or criminal citations based on the city's ordinances. Civil citations generally result in monetary penalties, while criminal citations can lead to arrest, jail time, and a criminal record. Many cities employ a graduated enforcement approach, beginning with warnings and civil penalties before escalating to criminal charges for repeated violations. Some jurisdictions combine enforcement with outreach, requiring officers to offer services or shelter before issuing citations.

Cities also employ non-citation enforcement mechanisms, primarily encampment "sweeps" or "clearings." These administrative operations forcibly remove camps and often destroy property without issuing formal citations. Sweeps can be conducted under anti-camping or anti-sleeping ordinances, but also under property seizure authority, public health codes, or other administrative regulations. While some sweeps may leverage the threat of citations as an incentive for PEH to comply, the mere risk of having city officials confiscate or destroy belongings–without any citation–is often sufficient to secure compliance. As one Human Rights Watch report notes, "Unlike citations that require a court process...sweeps punish people immediately, on the spot" (Watch 2024). Crucially, because sweeps do not necessarily rely on citations, cities could potentially have continued performing sweeps during the *Martin* restriction period–a nuance largely absent from public discourse, which often portrayed *Martin* as a blanket ban on all enforcement.

2.3 The *Martin* Framework and *Grants Pass*

In theory, the *Martin v. City of Boise* decision in September of 2018 fundamentally changed how cities could enforce anti-camping ordinances by explicitly linking enforcement authority to emergency shelter availability (Martin v. City of Boise 2018). The court ruled that enforcement of anti-camping ordinances through criminal citations was unconstitutional unless cities could provide adequate *emergency* shelter (ES) space. Importantly, the court focused specifically on ES beds, excluding other forms of homeless assistance from consideration. The Ninth Circuit's analysis in *Martin* carefully examined the relationship between enforcement and ES capacity. In Boise's case, the court counted 354 ES beds across three facilities against a homeless population of 867, with 125 being unsheltered.[8]

agency. However, Seattle only allows a person to be cited under this section of the code if the person continues their behavior after having been notified by a police officer that this behavior is in violation of the law (Seattle City Code § 15.48.040).

[8]Note that the court looked beyond simple bed counts, considering factors like religious requirements at shelters and practical accessibility of beds. For instance, the court found that beds at religious facilities

This decision was strengthened in September 2022 by *Johnson v. Grants Pass*, which extended the prohibition to civil citations, and extended protection to include necessary bedding materials (Johnson v. City of Grants Pass 2022). However, in 2024, the Supreme Court overturned these decisions, finding that "the enforcement of generally applicable laws regulating camping on public property does not constitute 'cruel and unusual punishment' prohibited by the Eighth Amendment" (City of Grants Pass v. Johnson 2024). This leaves us with a period from 2018 to 2024 where *Martin* and later *Grants Pass* introduced a significant shift in the legality of enforcing homelessness-related ordinances.

2.3.1 City Challenges in Responding to *Martin*

Cities faced two main challenges in interpreting the *Martin* decision. The first was determining how to compare shelter capacity to homeless population size. Some jurisdictions interpreted the ruling as requiring shelter capacity equal to the total unsheltered population, while others argued that they could enforce ordinances against individuals who refused available shelter.[9] The second was deciding what constituted adequate emergency shelter, with questions around temporary facilities, amenities, and staffing remaining unresolved.[10]

Cities responded in multiple ways, reflecting these ambiguities. Some adopted standardized *Martin*-compliant language in ordinances to clarify enforcement exceptions, others imposed geographic or temporal restrictions to allow partial enforcement while technically complying, and still others conditioned enforcement on individual refusals of available shelter.[11] This diversity of strategies–from textual compliance to substantive operational changes–creates room for the heterogeneous enforcement patterns that we analyze.

requiring participation in religious programming after two to three weeks could not be counted toward available shelter capacity.

[9]For example, San Francisco argued that enforcement was permissible if individuals were offered shelter and then refused, but a district court found inadequate evidence that such offers were consistently made (Coal. on Homelessness v. City and Cty. of San Francisco, 647 F.Supp.3d 806, 816 (N.D. Cal. Dec. 23, 2022).

[10]While *Martin* excluded facilities requiring religious participation, other questions remained. One California district court held that Chico's attempt to place temporary shelter on an airport tarmac lacking walls and electricity was clearly insufficient. Warren v. City of Chico, No. 221CV00640MCEDMC, 2021 WL 2894648, at *4 (E.D. Cal. July 8, 2021). Another required Los Angeles shelters to provide nursing staff, medical testing, and on-site security in order to be considered adequate (See: L.A. Alliance for Hum. Rts. v. Los Angeles, No. LA CV 20-02291-DOC-KES, 2020 WL 2512811, at *3–4 (C.D. Cal. May 15, 2020).

[11]For example, Sacramento modified ordinances to allow sleeping outside City Hall only at night, while Portland prohibited camping near schools during daytime hours but permitted it at night (Sacramento City Code § 12.52.030; Portland City Code § 14A.50.020). San Rafael created maps identifying specific areas where camping remained lawful, though courts later found the density restrictions unworkable. Santa Barbara attempted to ban sleeping everywhere except "downtown areas," but courts struck this down as creating "a near-blanket ban on sitting or lying in public except for a few hours in the middle of the night," requiring "homeless people to remain standing for nineteen hours a day" (Boring v. Murillo, No. LACV21-07305-DOC(KES), 2022 WL 14740244, at *6 (C.D. Cal. Aug. 11, 2022)).

3 Theoretical Framework and Empirical Predictions

A central premise in the public and judicial debate over *Martin* (2018), *Grants Pass* (9th, 2022), and the Supreme Court's reversal in *Grants Pass* (SC, 2024) is a causal story about how court doctrine reshapes city policy and, in turn, visible homelessness. Justice Gorsuch's majority opinion endorsed the view that the Ninth Circuit decisions "inadvertently contributed" to large increases in unsheltered homelessness, emphasizing citations as essential "tools in the policy toolbox" that can "encourage [individuals] to accept other alternatives like shelters, drug treatment programs, and mental-health facilities."

Local governments echoed this account in amicus briefs supporting reversal. A coalition of California cities argued that many unhoused individuals refuse offers of shelter and services and that constraints on law enforcement "clearly contribut[ed]" to a health and safety crisis.[12] Governor Newsom similarly described lower courts as having expanded *Martin*'s premise into "a far broader set of proscriptions on government action."[13] Even San Francisco, while emphasizing a services-first approach, claimed that constraints on enforcement coincided with high rejection rates of shelter offers.[14] Legal scholarship and policy commentary amplified similar narratives, including claims that the decisions catalyzed sanctioned encampments and discouraged shelter uptake, and possibly induced migration toward jurisdictions where enforcement was perceived as more constrained.(Versteeg, Cope, and Mukherjee 2025a)

Across these sources, the common causal argument is that the court decisions changed city enforcement behavior and that these changes in enforcement led to changed in behavior from people experiencing homelessness (PEH) outcomes, leading to increased visible unsheltered homelessness. This section clarifies the empirically testable links implicit in that template and motivates the core predictions we evaluate.

3.1 From Court Decisions to Municipal Actions

Cities can respond to appellate doctrine along several margins, and treating "enforcement" as a bundle is critical for interpreting the popular narrative and its empirical implications.

Targeted margin: sleep/camp citations. A natural mechanism is deterrence-through-litigation: when appellate courts signal that enforcement is constitutionally risky, risk-averse jurisdictions may reduce that enforcement to avoid injunctions, damages, or monitoring. In

[12] Brief for Thirteen California Cities Supporting Petitioner, City of Grants Pass v. Johnson, No. 23–175 (U.S. filed Mar. 4, 2024).

[13] Brief for California Governor Gavin Newsom as Amicus Curiae Supporting Petitioner, City of Grants Pass v. Johnson, No. 23–175 (U.S. filed Sep. 22, 2023).

[14] Brief for the City and County of San Francisco Supporting Petitioner, City of Grants Pass v. Johnson, No. 23–175 (U.S. filed Sep. 25, 2023).

this case, that would mean reducing or eliminating use of sleep/camp citations in cities without adequate emergency shelter space. At the same time, *Martin* and *Grants Pass (9th)* were operationally difficult to apply (e.g., what constitutes "adequate" shelter), leaving room for heterogeneous compliance. Some jurisdictions may meaningfully pull back; others may narrow ordinances, re-label conduct, or continue enforcement under the expectation (often correct) that it will survive challenge.

Substitution margins: other citations and sweeps. Cities rarely have a single enforcement lever. When sleep/camp citations become more legally contested, jurisdictions may substitute toward (or away from) other citations disproportionately applied to PEH (e.g., panhandling, public urination) and toward encampment clearings ("sweeps") conducted through administrative, public-works, or public-health authority. Whether sweeps move with citations is an empirical question: citations may be complements to sweeps (supporting clearance operations) or substitutes (sweeps expand when citations are constrained). The dominant narrative often foregrounds sleep/camp citations and implicitly treats "enforcement" as moving in one direction, but substitution implies that court doctrine can reallocate enforcement across margins rather than reduce it overall.

Complementary margin: shelter capacity. Court doctrine can also affect incentives around shelter provision. Under *Martin*'s framework, citation authority is constrained by emergency shelter availability, creating an incentive for jurisdictions that prioritize enforcement capacity to expand emergency shelter supply. However, shelter expansion depends on fiscal capacity and political will, and it may respond to rising homelessness levels independent of court doctrine (a demand-driven rather than strategy-driven response). This makes shelter both a potential municipal response margin and a key constraint shaping the feasibility and consequences of enforcement changes.

Finally, as we emphasized throughout, municipal responses are likely to vary across cities. Jurisdictions differ in baseline shelter capacity, reliance on citation-based enforcement versus sweeps, fiscal capacity, political constraints, and exposure to litigation risk (e.g., through the presence of active legal advocacy organizations). These differences imply heterogeneous responses even under the same precedent and motivate an empirical approach that allows for city-specific adaptation rather than assuming a single enforcement path.

3.2 From Municipal Actions to Visible Homelessness

The prevailing narrative's downstream mechanism is threat-based: if citations (and related enforcement) raise the cost of sleeping outside, PEH should be induced to accept shelter, relocate away from visible public spaces, or exit the jurisdiction, reducing visible unshel-

tered homelessness. In this view, restricting citations after *Martin* and *Grants Pass (9th)* should increase visible homelessness, while restoring citation authority after *Grants Pass (SC)* should reduce it, potentially reinforced by shelter availability and other enforcement actions.

Several features of this link are empirically ambiguous. Deterrence requires that threatened punishments meaningfully alter behavior, yet the marginal deterrent effect of financial penalties may be limited when individuals face substantial daily costs and risks of sleeping outdoors. Many refusals of shelter reflect mismatch between needs and available options (e.g., safety concerns, pet restrictions, couple separation) rather than insufficient enforcement pressure. Furthermore, claims that enforcement re-allocates homelessness across jurisdictions rely on substantial mobility among PEH, which may be limited in practice. For example, the California Statewide Study of People Experiencing Homelessness reports that 90% of participants were originally from California and 75% were last housed in the same county where surveyed.(Kushel, Margot et al. 2023) Finally, structural drivers including especially housing costs and affordability may dominate medium-run trends in visible homelessness, implying that court-induced changes in municipal tools could have only marginal effects on observed outcomes.[15]

Testable implications. These mechanisms generate three empirical predictions that guide the remainder of the paper:

1. **City actions: citations and substitution.** The popular narrative predicts broad declines in sleep/camp citations after *Martin* and *Grants Pass (9th)*, and increases after *Grants Pass (SC)*. A substitution framework predicts heterogeneous citation responses and potentially offsetting changes in other enforcement margins (including sweeps).

2. **City actions: shelter provision.** If adequate emergency shelter is a prerequisite for citation-based enforcement, restrictive court rulings may induce shelter expansion in some jurisdictions. However, shelter supply may evolve primarily in response to homelessness pressures and resources rather than court rulings.

3. **PEH actions: visible homelessness.** If citations are a strong deterrent, visible homelessness should rise when citations fall and decline when citations rise. If deterrence is weak or displacement tools substitute, visible homelessness may not track citation changes, and effects may vary with shelter capacity and local constraints.

[15]See, e.g.: Quigley and Raphael (2001), Heston (2023); Zillow Research (2018); Colburn and Aldern (2022), Love and Loh (2023), and Kushel, Margot et al. (2023).

The sections that follow take these predictions to newly assembled cross-city administrative data on municipal responses (citations, sweeps, shelter capacity) and PEH outcomes (visible homelessness) and evaluate whether the causal narrative that influenced *Grants Pass (SC)* is supported by systematic evidence.

4 Data and Measurement

4.1 Data Sources

One of the greatest challenges to empirical research on homelessness is the lack of quality data. As a result, one substantial contribution of our paper is that we create novel administrative datasets tracking both citations and visible homelessness across the United States, based on over 140 public records requests. We list our data sources in Table 1, and describe them in greater detail in the following section. Our integrated data approach provides a uniquely detailed, multi-dimensional view of how U.S. cities manage street homelessness. By linking enforcement records, public perception indicators, and shelter capacity data, we can better understand how policy interventions intersect with visible homelessness and the resources made available to those seeking shelter.

4.1.1 Enforcement

Agency-Level Citation Records. Through more than 70 public records requests to police departments and court systems, we compiled records of citations issued under municipal ordinances related to sitting, lying, sleeping or camping in public (anti-sleeping or anti-camping ordinances). We successfully collected these records for 33 cities (19 in the Ninth Circuit and 14 outside), largely covering the period of 2016-2025, though some cities start later or end earlier.[16] To our knowledge, this is the first time that such data has been collected at such a scale and used comparatively.[17]

[16]For the full list of cities, see Appendix Figure B1. For the full list of response rates, see Appendix C.

[17]The process of obtaining these records revealed significant challenges in the state of municipal record-keeping for enforcement data. In our public records submissions, we requested records of all civil and criminal citations issued by date and ordinance code, for as many years as possible. The quality and availability of responsive records varied dramatically across jurisdictions. Many departments indicated they had no digitally recorded data available to share, while others could only provide very recent subsets of data covering periods as short as one to two years. Several jurisdictions maintained records that lacked adequate details to distinguish between different types of violations or failed to include essential information. Particularly problematic was the inconsistent categorization of enforcement actions: some departments did not distinguish between criminal and civil citations in their databases, while others failed to differentiate between arrests and citations issued. The general state of these records, and the fact that they are not easily accessible even to the departments that maintain them in many cities, underscores the broader challenges facing empirical research on homelessness enforcement. This poor data infrastructure may itself reflect the

Table 1: Data Sources and Coverage

Data Source	Description	Geographic Coverage	Time Period	Key Features
Panel A: Enforcement Data				
Agency-Level Citation Records	Civil and criminal citations under anti-sleeping/camping ordinances from police departments and courts	33 cities (19 Ninth Circuit, 14 other)	2016–2025 (varies by city)	Hand-coded and harmonized across jurisdictions; includes timestamp and offense type.
FBI NIBRS Arrest Data	Arrests for vagrancy, loitering, curfew, and disorderly conduct	2,493 police agencies nationwide	2016–2025	Standardized but over-inclusive categories; arrests are rare events
Encampment Sweep Records	Day-level records of encampment clearing operations	Los Angeles, Seattle, Oakland, San Francisco, CalTrans (CA)	Varies by jurisdiction	Includes date, time, location, and estimated encampment size
Panel B: Homelessness Measures				
HUD Point-in-Time (PIT) Count	Annual volunteer count of unsheltered homeless population	All Continuums of Care (CoCs) nationwide	2012–2024 (no 2021 count)	Conducted last week of January; limited temporal resolution
311 Report Records	Resident-reported homelessness concerns including encampments, individuals, drug use	36 cities (15 Ninth Circuit, 21 other)	2016–2025 (varies by city)	Includes timestamp, location, complaint type;
Panel C: Shelter Resources				
HUD Housing Inventory Count (HIC)	Annual count of shelter beds by category	All CoCs nationwide	2012–2024	Focus on emergency shelters for main analysis; merged with population data

Notes: Complete city lists available in Appendix Figure B1. Data obtained through 140+ public records requests and city open data portals.

To ensure consistency across jurisdictions, we hand-coded all ordinance violations in our dataset, carefully reviewing municipal codes and citation descriptions to identify those that correspond to the types of conduct addressed in *Martin* and Grants Pass, an endeavor informed by city litigation, conversations with police officers, and review of ordinance descriptions. A table of this coding can be found in Appendix D. Wherever possible, we restrict our focus to citations alone. However, in some cities we are unable to separate arrests from citations, and therefore include them both. Following this harmonization process, we compiled monthly counts of relevant citations and arrests for each city, creating a balanced panel dataset that allows us to track enforcement patterns across jurisdictions.

Arrest Data. We additionally conduct analysis using arrest data from the FBI National Incident-Based Reporting System (NIBRS). Specifically, we collect arrest records for vagrancy, loitering, curfew, and disorderly conduct offenses. The NIBRS data is helpful in that it provides a standardized measure of criminal incidents across reporting jurisdictions, providing a balanced panel of 2,493 police agencies across the country. However, the arrest categories are overly broad, and an arrest represents a relatively rare and extreme outcome. Given this, our main results focus on citations.

Encampment Sweep Records. Through public records requests, we collected detailed day-level records of encampment sweeps in Los Angeles, Seattle, Oakland, San Francisco, and for lands (such as the areas around highways and underpasses) under the jurisdiction of the California Department of Transportation (CalTrans).[18] These records were obtained from municipal departments responsible for coordinating street cleanups and policing of public spaces. Each sweep entry includes information on the date and time of the sweep, the location of the encampment, and, where available, the estimated size of the encampment cleared. These data provide valuable descriptive information about enforcement behavior.

4.1.2 Visible Homelessness

Measuring unsheltered homelessness also presents significant methodological challenges that complicate research and policy evaluation. We start with the existing data collected by HUD, and then bring new data and new metric definitions to help address this challenge.

Point in Time (PIT) Count. The primary source of data on unsheltered homelessness is the Department of Housing and Urban Development (HUD)'s "Point in Time" or "PIT" count. The PIT is conducted annually at the CoC level by volunteers on a very early

ad hoc nature of homelessness policy implementation at the municipal level, where enforcement practices often evolve without systematic documentation or evaluation.

[18]The Seattle data is complemented by records collected by journalist Guy Oron. GitHub, "guyoron," https://github.com/guyoron.

morning in the last week of January. The PIT is useful for broad trends but poorly suited to evaluating changes around specific events or rulings.[19]

311 Call Records. To remediate the shortcomings of the PIT for empirical analysis, we construct an alternative measure of visible homelessness via 311 report records. We collected 311 report data through more than 70 public records requests and city open data portals.

311 systems are municipal hotlines that allow residents to report non-emergency issues of concern to the city. Residents can call the city hotline directly (often by dialing 311) or in many cities, can also submit a report through an app or online portal. Cities increasingly rely on 311 reports as a key data source for tracking unsheltered homelessness and encampments. Each 311 record includes a timestamp and a call classification or complaint type, and many include coordinate-level geographic information. We currently have 311 report records from 36 cities (15 in the Ninth Circuit and 21 outside.[20] These data are available from 2016-2025 for most cities in our sample, though some cities start later or end earlier. These reports provide a timely and geographically disaggregated indicator of visible homelessness. In addition, these reports also capture the public's response to homelessness in real time, which is not readily observed through other official channels.

We group 311 reports into harmonized homelessness-related categories across cities. These include encampment reports, general homelessness reports (reports about individual homeless individuals outside encampments, e.g., panhandling), needles and drug-use-related reports, and illegal dumping reports. For our main results, we focus on calls in the encampment and general homelessness categories, as these are most directly related to the concerns around visible unsheltered homelessness. We do not find any meaningfully different results when we substitute or add in the other categories.

Using these categories, we construct our two main outcome measures. First, we compute what percent of reports is related to homelessness in each month in each city. While the total number of calls provides baseline information on system usage, the rate measure helps control for changes in overall 311 reporting trends (e.g., if reporting increases due to system improvements). Second, we create a measure of "unique" encampment regions by month. We identify "unique" encampments by clustering calls within a specific distance (primarily 100m, approximately one city block) and assigning them to the same "encampment region"

[19]Because the PIT is collected only once per year, it offers very few data points for assessing *Martin v. Boise* and cannot capture the effects of *Grants Pass (SC)*, as the most recent national count from January 2024 predates the Supreme Court decision. Reliance on volunteers for data collection on a single night raises accuracy concerns, especially given the difficulty of finding and correctly identifying homeless individuals after dark (see, e.g., O'Flaherty 2019). If jurisdictions with larger homeless populations underinvest in count resources, this could systematically bias counts downward in precisely the areas of greatest interest. Furthermore, the PIT was not conducted in 2021 due to the COVID-19 pandemic.

[20]For the full list of cities, see Appendix Figure B1. For the full list of response rates, see Appendix C.

of concern.[21] This measure of "unique" encampment reports is less sensitive to reporting intensity variations across time and neighborhoods.

To validate these measures, we correlate January 311 report data from each year with the corresponding annual PIT counts of unsheltered homelessness. Across our sample cities, we observe correlations of 0.51 for total calls, 0.16 for the rate measure, and 0.78 for unique encampment regions using the 100m threshold. This lends us additional confidence that 311 reports are a valuable proxy for counts of the population of PEH.

Our analysis builds on work from other researchers innovating in the use of 311 calls to measure visible homelessness, though past analyses have generally focused on one city alone.[22]

Using 311 data introduces new methodological challenges. Reports may reflect both the actual presence of encampments and resident propensity to report them, which may vary systematically across neighborhoods based on factors like demographic composition or attitudes toward PEH. We attempt to address these concerns by focusing not on the total count of reports, but instead on the "unique" encampments metric, designed to partially account for these reporting biases.

4.1.3 Shelter Beds and Capacity

HUD Housing Inventory Count (HIC). We complement the enforcement and homelessness measures with a panel dataset on shelter capacity derived from HUD's Housing Inventory Count (HIC). The HIC is conducted annually by local Continuums of Care (CoCs) and reports the number of beds available for individuals experiencing homelessness in each ju-

[21] This clustering approach specifically addresses two measurement concerns. The first is a concern about potential reporting responses to a change in enforcement: if residents perceive less enforcement activity, they might reduce their reporting frequency for a given encampment, potentially biasing our time series. The second concern is about potential disparities in reporting, e.g., perhaps more affluent neighborhoods may be more likely to call and generate multiple reports about the same encampment, while less affluent areas might produce fewer reports per encampment. By clustering and counting unique encampment regions rather than individual reports, we mitigate these issues since a single report is sufficient to identify an encampment's presence. Appendix Figure A2 gives an example of what the 311 clustering looks like in San Francisco.

[22] For example, Corinth and Finley (2020) evaluate 311 report records as a method for mapping unsheltered homelessness across New York City. They demonstrate that the geographic distribution of homelessness-related reports (both encampment complaints and reports requesting assistance for homeless individuals) closely aligns with official PIT count data across boroughs. Through multiple analyses, they address potential reporting bias concerns, showing that the 311 report patterns are not merely reflections of general 311 usage or driven by the locations of non-homeless reporters (as evidenced by consistent patterns across different times of day and days of the week when population distributions vary). These findings suggest that 311 report data can serve as a reasonable proxy for tracking the spatial distribution of unsheltered homelessness within urban areas. Similarly, in his PhD dissertation, Bhandari (2024) finds that, in Los Angeles, 311 encampment complaints made within 60 days of the PIT count strongly correlate with PIT count data across census tracts. Specifically, he finds that one complaint is associated with approximately three additional tents/makeshift shelters in the PIT count.

risdiction. We compile data for the period 2012–2024. In our main analysis, we focus on emergency shelters since these were the facilities at issue in the *Martin* and *Grants Pass* decisions.

4.2 Summary Statistics

Table 2 presents summary statistics comparing jurisdictions within the Ninth Circuit Court's region to those outside it in the year prior to the *Martin* decision. These statistics paint a stark picture of the differences between Ninth Circuit jurisdictions and the rest of the country, even before *Martin*. We present statistics at the CoC level (which is the level available for HUD data) or city level, depending on data availability.

Table 2: Summary Statistics by Ninth Circuit Status (2018)

	Ninth	Outside
Panel A. CoC-level HUD data		
Census Population (#)	968,576 (1,331,354)	832,068 (1,165,791)
ES Beds (#)	941 (1,520)	873 (4,372)
ES Capacity (%)	42.0 (20.8)	75.1 (22.8)
Unsheltered Homeless Pop. (#)	1,840 (4,682)	223 (451)
Observations (*CoCs*)	66	301
Panel B. City-level data (311 calls, citations)		
Unique Encampments (100m, # /Month)	345 (420)	49 (86)
Observations (*Cities*)	9	12
Citations (Anti-Sleeping, Camping, #/Month)	47 (123)	41 (75)
Observations (*Cities*)	18	13
Panel C. Agency-level arrest data		
Arrests (Curfew, Loitering, Vagrancy, # /Month)	14 (47)	5 (13)
Observations (*CoCs*)	41	269

Note: This table reports means for each outcome with standard deviations in parentheses. The CoC-level HUD data shows means across CoCs in January 2018 from HUD's HIC and PIT. The city-level data and the arrest data show means across regions and months for the period of August 2017 to August 2018, one full year prior to the Martin decision. The arrest data is aggregated from the agency level to the CoC level for comparability with other metrics.

The data reveal substantial baseline differences across regions. Ninth Circuit regions have larger populations on average than regions outside the Ninth Circuit, along with somewhat higher numbers of emergency shelter (ES) beds. Emergency shelters in both regions lack

needed capacity, however, the shelter systems in the Ninth Circuit would only be able to house 42% of those seeking shelter, while systems in the rest of the country could house an average of 75%. This difference reflects the dramatically higher levels of unsheltered homelessness in Ninth Circuit jurisdictions: approximately eight times higher than in other regions. Unsheltered homelessness measured through monthly, per-capita 311 encampment reports tells a similar story, with nearly six times as many reports in the Ninth Circuit compared to regions outside it.

Despite this, our sample of citations data shows that prior to *Martin*, Ninth Circuit cities in our sample issued only slightly more anti-sleeping and anti-camping citations than other cities. Given the substantially higher levels of unsheltered homelessness in Ninth Circuit regions, this suggests that pre-*Martin* enforcement occurred at a lower rate per person experiencing unsheltered homelessness in the Ninth Circuit than elsewhere. In the arrest data, we see that, as expected, arrests for related offenses are relatively rarer than citations.

These patterns provide important context for understanding regional variations in homelessness governance and the potential impacts of court decisions on local policy approaches.

5 Empirical Strategy

Our empirical approach exploits the quasi-experimental setting created by the *Martin v. Boise* (2018) and *Grants Pass* (2022, 2024) decisions, which applied only to jurisdictions within the Ninth Circuit. The circuit boundaries create a sharp discontinuity in legal exposure for otherwise similar jurisdictions facing comparable homelessness trends, economic conditions, and policy environments. Given this structure, we compare outcomes between cities inside and outside the Ninth Circuit's jurisdiction, focusing specifically on cities in CoCs where emergency shelters were recorded as being at or near capacity when the *Martin* case was decided.

We estimate two complementary quantities of interest. First, we measure the average treatment effect across all Ninth Circuit jurisdictions affected by each ruling, which provides a broad assessment of the decisions' aggregate impact. Second, recognizing that local policy responses likely varied substantially, we estimate city-specific treatment effects that capture heterogeneous impacts across different jurisdictions. This dual approach allows us to evaluate both the average effect of the rulings and the distribution of responses across cities.

5.1 Conceptual Baseline: Difference-in-Differences Framework

A natural starting point for this analysis is a standard difference-in-differences (DID) framework, which compares changes in outcomes between treated (Ninth Circuit) and control (Outside Ninth Circuit) cities before and after the rulings. We could implement this via a two-way fixed effects (TWFE) event-study specification as in equation (1):

$$\text{Outcome}_{it} = \sum_{\kappa \neq 0} \tau_\kappa \mathbb{1}(\text{month}_t = \kappa) + \lambda_i + \gamma_t + \epsilon_{it} \tag{1}$$

where i indexes cities (or CoCs) and t indexes months (or years). The coefficients τ_κ capture the dynamic treatment effect at event time κ relative to the reference period, while λ_i and γ_t denote city and time fixed effects, respectively. The key identifying assumption for this approach is that trends in the Ninth Circuit and in the rest of the country would have continued evolving similarly in the absence of the court rulings ("parallel trends" assumption).

Although DID offers a transparent baseline and a useful diagnostic for testing pre-trends, we find substantial pre-existing differences in homelessness levels and trajectories across regions which make this assumption difficult to justify in practice. Visual inspection of the event-study coefficients confirms that Ninth and non-Ninth cities often exhibit diverging pre-trends for key outcomes. As a result, our main analysis adopts a more flexible estimator that better adjusts for such baseline differences.

5.2 Preferred Specification: Synthetic Difference-in-Differences

To construct a more credible counterfactual, we employ the Synthetic Difference-in-Differences (SDID) approach introduced by Arkhangelsky et al. (2021). SDID combines the intuitive DID design with the flexible weighting of Synthetic Control, creating a weighted control group that closely matches the pre-treatment trends of treated units.

Formally, for a balanced outcome panel Y of dimension $N \times T$, we partition the data into N_0 control and N_1 treated units, and T_0 pre-treatment and T_1 post-treatment periods. We first estimate weights $\hat{\omega}^{SDID}$ such that pre-ruling trends from comparison units match the pre-ruling trends in the Ninth Circuit as closely as possible: $\sum_i^{N_0} \hat{\omega}^{SDID} Y_{it} \approx \sum_i^{N_1} Y_{it}$, where $\sum_i^{N_1} Y_{it}$ represents the unweighted average of outcomes for cities within the Ninth Circuit's jurisdiction. Then, in SDID (unlike in the related Synthetic Control method), we also estimate time weights λ_t^{SDID} to balance pre- and post-ruling time periods (e.g., find pre-treatment periods that match post-treatment periods most closely among the control units).

SDID uses these weights to estimate the average treatment effect on the treated (ATT) as a weighted double difference. Operationally, we use these weights in a basic TWFE regression to estimate our treatment effect τ^{SDID}:

$$(\tau^{SDID}, \hat{\alpha}, \hat{\beta}) = \arg\min_{\tau,\alpha,\beta} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \alpha_i - \beta_t - \tau Sweeps_{it})^2 \hat{\omega}_i^{SDID} \hat{\lambda}_t^{SDID} \right\}.$$

To estimate standard errors, we implement the conditional variance estimator proposed in Almeida et al. (2025), which flexibly accounts for heteroskedasticity across units and time periods, and which has superior power properties relative to other variance estimators in this setting. Our paper provides the first empirical application of this variance estimation method. We futher extend the proposed method to allow for multiple treated periods using a block-resampling strategy.

Compared to TWFE, the SDID estimator produces a counterfactual that more closely follows the pre-treatment trajectory of treated cities, reducing bias from non-parallel trends. Compared to the standard Synthetic Control (SC) approach, SDID accommodates constant level differences between treated and control units and uses more of the available time variation. Nevertheless, the limited donor pool of cities in our context means that even these advanced methods cannot always reproduce perfect pre-treatment fit. We therefore present raw trend plots alongside our point estimates for each of our main outcomes to help us interpret and understand potential trend deviations. While no estimator can fully eliminate the limitations of the observational data in our setting, we argue that SDID offers the most credible and transparent framework—and that seeking perfect identification should not preclude informative analysis.

5.3 Treatment Timing

For each of these strategies, we need to identify the "treatment" period, which begins when each ruling took effect. We split our results into three main analysis periods corresponding to the three court decisions. The first decision was filed on September 4, 2018. For this decision, when we have data available at monthly (or more frequent) intervals, we use August 2018 as the reference month (-1). We use as long of a pre-treatment window as is available based on the data source, and we end analysis at February 2020, excluding the first few months of the COVID pandemic, where there were large changes in both actual outcomes and data reporting practices.

Some data are only available at annual intervals (collected each January). Given the inherent limitations with yearly data, when analyzing results for these outcomes we only use

a single pre-post-*Martin* window. Choosing which year to set as the reference period when the *Martin* decision came out between rulings is a bit tricky. Since the ruling came down in September 2018, it would be unreasonable to expect substantial changes in homelessness-related outcomes just a few months later. Most of what we observe in January 2019 likely reflects the pre-treatment policy environment. Using the 2018 reference likely overstates any treatment effect, as it attributes changes that occurred between January and September 2018 to the *Martin* decision. Conversely, using the 2019 reference likely understates the effect, as it excludes potential changes that may have occurred between September and December 2018. In our main results, we present estimates using both the 2018 and 2019 reference periods. We slightly prefer the 2019 reference period, since cities would have needed time to respond and adjust policies or enforcement guidance. Nevertheless, the true effect likely lies between the two estimates, and our confidence increases if they yield similar results.

The second decision was filed on September 28, 2022. When we have data available at the monthly level, we set September 2022 as the reference month. The pre-period for this case starts in June 2020 and we end our analysis in January 2024. Finally, the Supreme Court decision was filed on June 28, 2024. In the monthly data, we set June 2024 as the reference month and our window of analysis for this case runs from January 2023 to June 2025.

One important nuance is that based on commentary/legal scholarship(?), it is reasonable to assume that the outcome of the 2018 *Martin* was unexpected, so we would not expect any anticipation effects. In contrast, once the Supreme Court granted a writ of certiorari, agreeing to hear the *Grants Pass* case in January 2024, many observers expected the Supreme Court to overturn the Ninth Circuit's *Grants Pass* decision. As a result, we might expect some anticipation effects in the period from cert (January) to ruling (June). For consistency with the other cases, we keep the ruling month as the reference month, but when discussing results we will comment on the possibility of anticipation effects, noting that these move in the expected direction.

5.4 Sample Construction and Limitations

Following the reasoning employed in *Martin*, the restrictions imposed by the 2018 and 2022 decisions should only apply to cities that do not have adequate shelter capacity. While, as described earlier, cities and lower courts puzzled over how to precisely apply this formula, the most literal form of it is straightforward to investigate. Following the calculations used

in the court cases, we define emergency shelter (ES) capacity as:

$$\text{ES Capacity}_i = \frac{\text{Total ES Beds}_i}{\text{Total Unsheltered}_i + \text{Total Sheltered in ES}_i} * 100\%$$

We can read this as: What percent of people who need shelter (unsheltered or currently in ES) does the ES system have beds for? In our main results, where possible, we restrict to regions that do not have capacity to house everyone (ES capacity $\leq 100\%$) in 2018. We use the 2018 levels in order to maintain a consistent sample, and we separately look at shelter capacity itself as an outcome variable of interest.

Still following the reasoning from *Martin*, we note that an emergency shelter system need not reach full capacity to be effectively full. Practical constraints may render nominally available beds unsuitable due to, for example: religious requirements or restrictions or insufficient accommodations for individuals with disabilities. There could also be nominal availability that does not translate to practical accessibility (e.g., vacant family unit beds that cannot accommodate single adults). As a result, where possible, we also provide versions of our main results using the full sample of all regions at all levels of capacity, though we note that more than 60% of all CoCs are at- or over-capacity.[23]

Our final sample integrates the enforcement, visible homelessness, and shelter capacity data at the city-year level for the 2016–2024 period. For variables that are available at the day or hour level, we aggregate up to the monthly level to smooth day-to-day variation. To account for varying city sizes and different levels of administrative or public reporting, we present all of our main results as per-capita rates.[24] These re-scalings help ensure that measures from larger cities, or from cities with higher absolute numbers of reports or levels of enforcement do not disproportionately influence the results.

6 Results

This section presents our main empirical findings on the impact of the three court decisions described above. First, we examine the actions of cities, and ask whether cities (1) reduced

[23]For our more limited city-by-city datasets, all cities are above 84% capacity, and all but one are above 90% capacity (the vast majority of this sample is over 100% capacity). As a result, we do not exclude any cities using this filter, as it is reasonable to assume that they would all be conceptually bound by the ruling were they in the Ninth Circuit.

[24]For example, we scale the enforcement measures by the fixed 2018 homeless population in each region (with the non-fixed yearly interpolated homeless population in each region in the appendix). The 311 report data is similarly scaled by population, and we also present results for homelessness-related 311 calls as a fraction of the total report volume in each city (i.e., the sum of all 311 calls, including those not related to homelessness).

citations for camping and sleeping violations following *Martin*, (2) shifted to alternative enforcement mechanisms such as encampment sweeps, or (3) strategically expanded emergency shelter capacity to regain citation authority. Second, we examine the actions of people experiencing homelessness (PEH), asking whether reduced enforcement altered where and how individuals sought shelter or chose to sleep, measured as changed in levels of reported visible homelessness.

6.1 Changes in Enforcement Behavior

Recall from our theoretical framework that we expected to see a differential *decrease* in sleeping & camping citations following the restrictive Ninth Circuit *Martin* (9th, 2018) and *Grants Pass* (9th, 2022) rulings, followed by a reversal after the Supreme Court *Grants Pass* (SC, 2024) decision. To investigate this, we present raw trends and then compute treatment effect estimates using synthetic difference-in-differences (SDID) estimates.

6.1.1 Cities: Sleeping & Camping Citations, Descriptive

Figure 2 plots the average number of anti-sleeping and anti-camping citations issued per 1,000 PEH in cities within the Ninth Circuit (black line) and cities outside the Ninth Circuit (gray line).[25][26]. Three vertical lines indicate the timing of the *Martin*, *Grants Pass* (9th), and *Grants Pass* (SC) decisions. We also shade the first year of the COVID-19 pandemic, from March 2020 to March 2021.[27] As a reminder, these are the types of citations that were targeted by the restrictive court rulings.

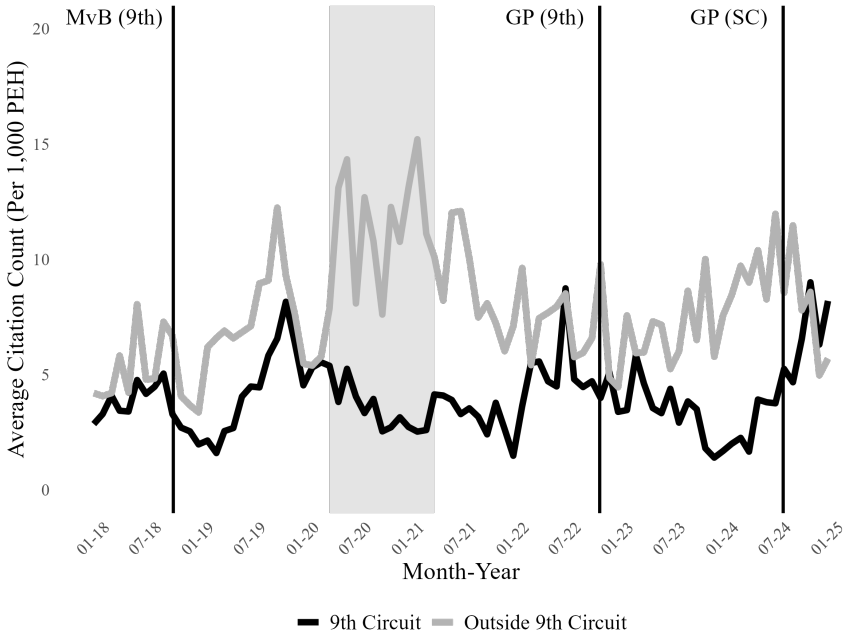
If shelter capacity were binding in practice, we would expect a sharp decline in citations in the Ninth Circuit during the restricted period. Yet, as the figure shows, cities in the Ninth Circuit maintained a relatively consistent level of citations (per 1,000 PEH) throughout the period, even though their shelters were at or near capacity. To quantify this stability, the

[25]We scale by the average level of PEH from 2016-2018 level for a few reasons. First, the PIT count only occurs once a year in January, and we do not want to introduce artificial jumps when we change measures in January. Second, the 2021 unsheltered PIT count was not conducted in most jurisdictions, which would leave a gap. Third, scaling by a fixed number allows us to look at comparable and appropriately scaled changes in enforcement alone without mixing in changes in the relevant PEH population. However, we realize that this is somewhat unsatisfying, and so to complement our main measure, we present results using an alternative scaling where we interpolate the PIT count to the monthly level in between years. Results using this scaling appear quite similar, if somewhat smaller, and are presented in Appendix Figure A3.

[26]In the descriptive results, all cities are weighted equally, which we interpret as answering the question of what happened to the average city in each region. We obtain similar results if we instead weight by total population or total homeless population (see, e.g., Appendix Figure A4), which we interpret as answering the question of what happened to the average person or PEH in each region

[27]A full detailing of the trends of each city in our complete (unbalanced) panel can be found in Appendix Figure B2.

Figure 2: Average Count of Anti-Sleeping and Anti-Camping Citations per 1,000 PEH



Note: This figure plots the mean count of anti-sleeping and anti-camping citations across cities in the Ninth Circuit and in the rest of the country, scaled by the average unsheltered homeless population of that city from 2016-2018. Each city is weighted equally.

variance in citations per capita in the Ninth Circuit was approximately 84 citations per 1,000 PEH, whereas it was 256 citations per 1,000 PEH in the rest of the country.

Walking through this figure chronologically: We see that immediately following *Martin* (first restrictive decision), the average number of citations fell somewhat in the Ninth Circuit. However, the gray line shows that citations outside of the Ninth Circuit also experienced a similar (possibly seasonal) drop around the same time. Furthermore, citations in the Ninth Circuit returned to, and then exceeded, pre-decision levels within a year. This pattern is surprising and suggests that this first restrictive ruling was not particularly restrictive in practice.

Moving into the shaded gray portion of the graph, we see that the the COVID-19 pandemic produced a similar-sized decline in enforcement in the Ninth Circuit, while citations remained high in the rest of the country.

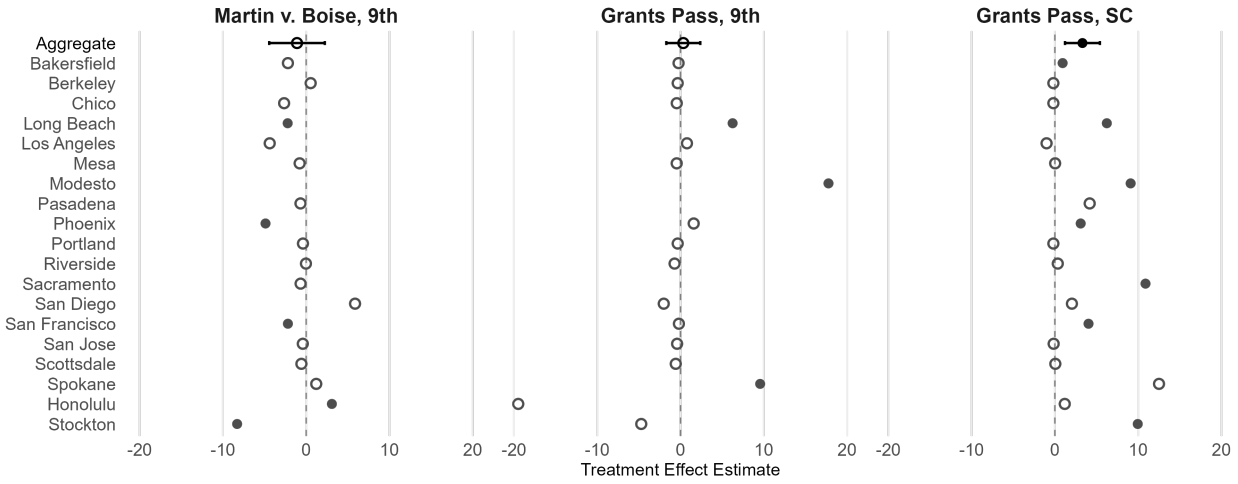
After the first year of the pandemic and until *Grants Pass* (9th, second restrictive decision), the rest of the country gradually decreased their use of citations, while cities in the Ninth Circuit, in anything, increased their use of citations. Following *Grants Pass* (9th), Ninth Circuit cities maintained a similar level of citations. About a year later, it appears the Ninth Circuit cities decreased their use of citations somewhat, especially relative to cities outside of the Ninth Circuit. Again, this is not as immediate or as substantial of a decrease as we might have expected based on the public narrative about these restrictive rulings. Finally, as we describe in the empirical section, the Supreme Court's decision to grant certiorari in *City of Grants Pass v. Johnson* in January 2024 plausibly altered cities' expectations about the durability of the Ninth Circuit's *Martin*/*Grants Pass* constraint. Consistent with the possibility that some cities anticipated a reversal, we observe an increase in public-camping citations beginning around January 2024, with citations rising further after the Supreme Court's June 2024 decision, which removed the Ninth Circuit constraint. This is the only change that follows the expected direction.

We want to emphasize two main takeaways: First, the level of citations remained surprisingly consistent in the Ninth Circuit across this entire period. Second, the average of cities outside of the Ninth Circuit exhibited rather different trends than the Ninth Circuit, making it difficult to extract credible treatment effect estimates from the raw plots alone.

6.1.2 Cities: Sleeping & Camping Citations, Synthetic Difference in Differences

To move toward better causal inference, we employ our synthetic difference-in-differences (SDID) strategy, which, as described earlier, divides this time period into three windows. In each window, we construct a synthetic control group whose pre-trends match the pre-trends in the Ninth Circuit as closely as possible. Figure 3 plots point estimates for the aggregate

Figure 3: Treatment Effect Estimates: Anti-Sleeping and Anti-Camping Citations per 1,000 PEH



Note: These figures display the SDID point estimates by city following the Martin and GP (9th) & (SC) decisions. Solid circles indicate that the estimated coefficient is statistically significant at the 95% level.

impact on the Ninth Circuit and for each Ninth Circuit city in our sample. Note that solid circles indicate that the result is statistically significant at the 95% level. Table 3 provides the SDID point estimates for enforcement outcomes, and Appendix Figure A5 presents the associated SDID plots.

In the first panel (*Martin v. Boise, 9th*), the first row shows that we estimate a small decrease in citations following the first restrictive decision (-1.10 citations per 1,000 PEH, from a baseline of 3.76). However, this estimate is quite noisy (SE: 1.70). If we look at the city treatment effects in the rest of the rows, we see that this large aggregate variance can be attributed to heterogeneous responses across cities. Most cities have treatment effects centered at 0, some decrease their use of citations, and a few even increase their use of citations.

In the second panel (*Grants Pass, 9th*), we see that the aggregate effect following the second restrictive decision is even smaller (+0.33 citations per 1,000 PEH, SE: 1.04). Again, we see that there was substantial heterogeneity in city responses, with most exhibiting no measurable change, three cities substantially increasing their use of citations and some decreasing citations.

In the last panel, after *Grants Pass (SC)*, we measure a large and statistically significant aggregate increase of 3.31 citations per-1,000 PEH (SE: 1.07), representing more than a 70% increase relative to the baseline mean of 4.59 citations per-1,000 PEH. This comes from

Table 3: Impact of Court Decisions — Enforcement (SDID)

	Martin v. Boise (9th)	Grants Pass (9th)	Grants Pass (SC)
Sleeping Camping Citations /1,000 PEH			
τ	-1.10	0.33	3.31***
(SE)	(1.70)	(1.04)	(1.07)
$\bar{Y}$	3.76	4.26	4.59
N	837	1,312	768
Other Homeless-Related Citations /1,000 PEH			
τ	1.10	-1.22	0.47
(SE)	(0.91)	(0.87)	(0.56)
$\bar{Y}$	2.01	1.73	0.70
N	810	1,271	744
Relevant Arrests /1,000 PEH			
τ	0.20	-0.03	
(SE)	(0.45)	(0.44)	
$\bar{Y}$	2.58	1.11	
N	8,300	7,138	

Note: This table displays the aggregate SDID point estimates, comparing the change in sleeping & camping citations, other homelessness-related citations, and homelessness-related arrests (vagrancy, loitering, curfew) in the Ninth Circuit following each of the three court cases on the Ninth Circuit compared to a synthetic control of cities in the rest of the country. The unit of analysis is a month-city. All outcomes are scaled per 1,000 PEH, where the count of PEH is the fixed average from 2016-2018.

the fact that more than half of the cities in the sample increased their citations while the other half did not experience a measurable change. Notably, this is the only treatment effect that moves in the direction predicted by the popular hypothesis. It is somewhat puzzling that cities responded to the removal of constraints when they had not measurably adjusted behavior in response to the imposition of similar constraints. We interpret this pattern as consistent with the Supreme Court functioning as a rhetorical licenser: its decisions can provide political cover or legitimacy for taking actions that local leaders may have been reluctant to pursue independently.

The aggregate citation effects help establish the broad pattern: no strong citation response to the restrictive court cases, and an increase in response to the overturning Supreme Court case. However, the heterogeneity highlighted in this figure is the critical finding. Despite popular portrayals of the decision as having "hamstrung" or "shackled" local governments, the actual pattern was far from uniform. The ruling introduced uncertainty rather than uniformity, and its doctrinal ambiguity produced highly uneven effects across jurisdictions.

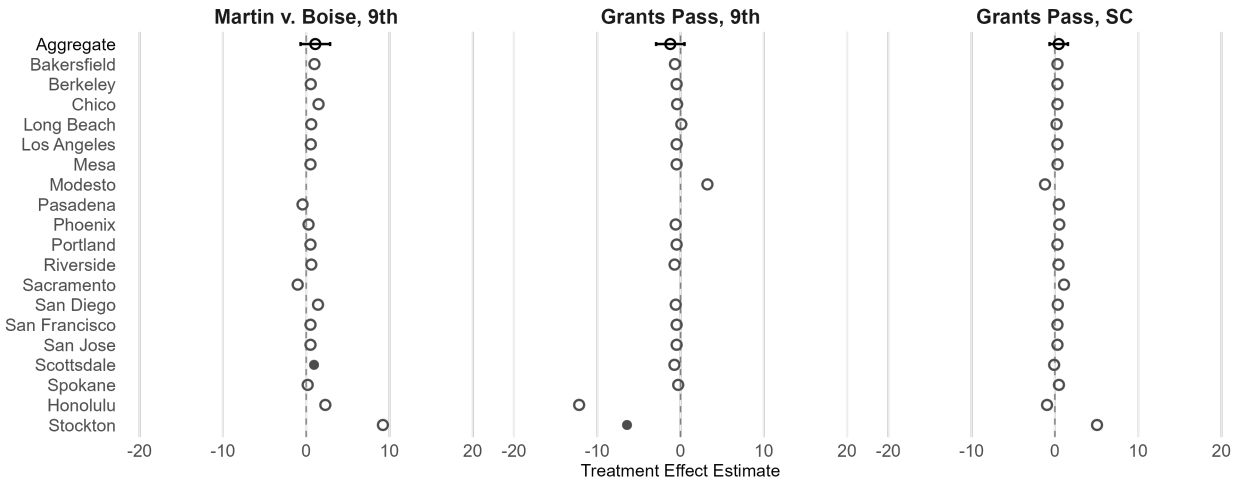
6.1.3 Cities: Other Homelessness-Related Citations

One possible explanation for why we see limited impacts, and heterogeneity across cities, is that cities could have substituted towards other homelessness-related citations that the court cases did not restrict, including, for example, citations for panhandling, public urination, or possession of shopping carts. We repeat the SDID exercise with these other homelessness-related citations ("substitution" citations), and present the results in Figure 4. Appendix Figure A6 presents the SDID plots for the aggregate treatment effect following each court case.

First, it's interesting to see that the aggregate point estimate following *Martin* is 1.10, which (though imprecisely estimated) almost exactly offsets the aggregate decrease in sleeping & camping citations in the same period. Looking at the city-level results, we don't see much movement overall. However, there are a few interesting case studies. For example, in Figure 3, we saw that Stockton, CA was one of the cities that reduced their use of the targeted anti-camping and anti-sleeping citations notably after *Martin* ($\hat{\tau}$: -8.3 citations per 1,000 PEH). However, in Figure 4, we see that Stockton appears to have substituted almost one-for-one with these other citation types ($\hat{\tau}$: 9.23), though the effect on the "substitution" citations is much noisier.

This highlights that these other citations may have indeed served as substitutes in some cities, which underscores the importance of local institutional and interpretive context in understanding the impact of the rulings.

Figure 4: Treatment Effect Estimates: Other Homelessness-Related Citations per 1,000 PEH



Note: These figures display the SDID point estimates by city following the Martin and GP (9th) & (SC) decisions. Solid circles indicate that the estimated coefficient is statistically significant at the 95% level.

6.1.4 Cities: Sweeps

Following our theoretical framework, we next examine the data on sweeps to determine whether citations and sweeps function as complements or substitutes. Our sweep data comes from several Ninth Circuit cities and from CalTrans (the California Department of Transportation, which is responsible for conducting sweeps on lands under its jurisdiction, such as areas around highways and underpasses). Without data from cities outside the Ninth Circuit, we cannot employ the same SDID analysis we used for citations. Nevertheless, the available data reveal two important patterns which we present in Appendix Figure A7.

First, we note that sweeps continued throughout the restricted period in all cities for which we have data, as well as for CalTrans. This indicates that even when citation authority was (nominally) constrained, cities maintained enforcement capacity through sweeps operating under different legal mechanisms.

Second, we observe clear substitution patterns in at least one city. In Los Angeles, citations and sweeps exhibit a correlation of -0.47. As shown in the previous section in Figure 3, Los Angeles appears to have reduced citations following *Martin*. Yet precisely during this period between *Martin* and *Grants Pass (9th)*, the city increased their use of encampment sweeps more than sixfold. The number of sweeps far exceeded their use of citations: Los Angeles conducted more than 3,000 sweeps per month at its 2020 peak, compared to a maximum of only just over 800 sleeping or camping citations in our sample window.

Without a counterfactual, this evidence remains suggestive. Nevertheless, the strong negative correlation and temporal pattern indicate that in at least some cities, sweeps and citations acted as substitutes, allowing cities to retain substantial enforcement authority despite restrictions on citation-based enforcement.

6.1.5 Cities: Arrests

Finally, we perform a similar SDID analysis using related arrest records from the FBI NIBRS to look at the impact of *Martin* and *Grants Pass (9th)* (we cannot observe outcomes post-*Grants Pass SC*). As described earlier, the available arrest categories are overly broad compared to the offense types at issue. Moreover, arrests remain a relatively rare outcome for the types of ordinances at issue. Nevertheless, we include the results in Table 3, and in Appendix Figures A8 and A9 for completeness, comparing homelessness-related offenses (categorized as curfew, loitering, or vagrancy violations) scaled per 1,000 PEH. We find no impact of either case on related arrest categories.

6.1.6 Summary of Enforcement Results

Sleeping & camping citations did not meaningfully decline in aggregate following *Martin* or *Grants Pass (9th)*, and we observe substantial heterogeneity in city responses, including substitution into sweeps or other related citations in some jurisdictions. By contrast, once the Supreme Court lifted the constraint, average citation levels increased sharply. This asymmetric response suggests that the restrictive rulings did not uniformly constrain enforcement, but the Supreme Court decision served as a clearer authorization for renewed enforcement activity.

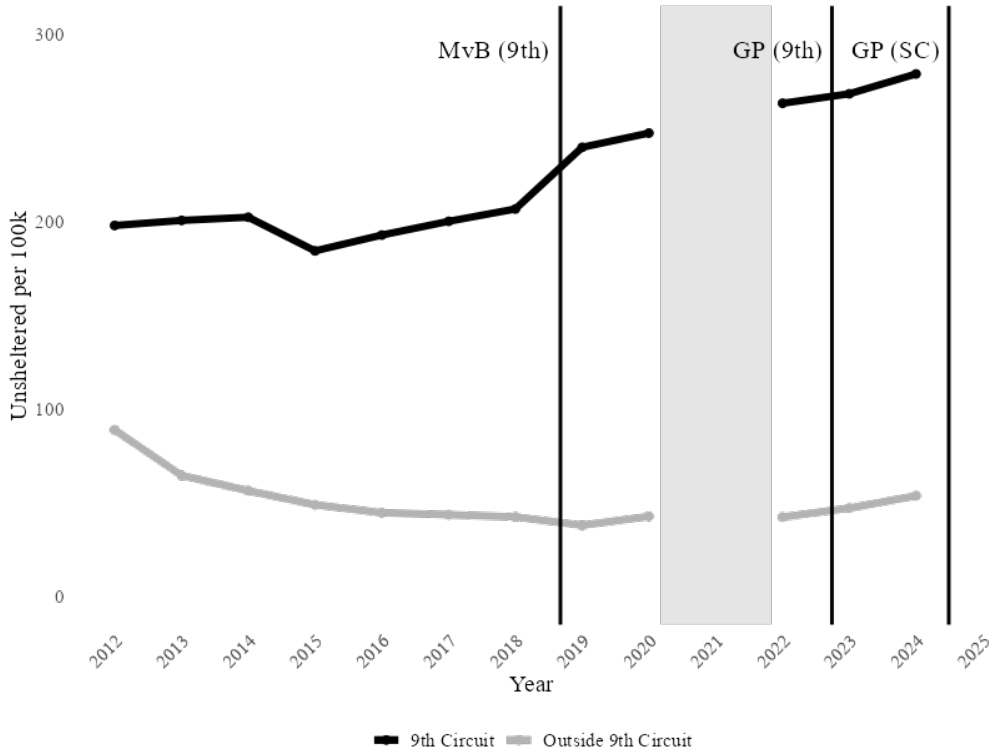
6.2 People Experiencing Homelessness: Changes in Visible Homelessness

We now turn to the analysis of how trends in unsheltered homelessness evolved following the Federal and Supreme Court cases. Remember that following the popular hypothesis, we would have expected unsheltered homelessness to increase following the restrictive decisions (*Martin* and *Grants Pass (9th)*, and then to decrease following the Supreme Court's *Grants Pass (SC)* decision, which released the constraints on enforcement. Following the weak and scattered enforcement responses, we might not expect the court cases to have caused large changes in unsheltered homelessness. Nevertheless, this is a very important outcome and we run our descriptive and SDID analyses using both the yearly Point-in-Time (PIT) count from HUD and our unique encampment region measure derived from 311 data.

6.2.1 Unsheltered Homelessness: Point in Time Count

Figure 5 plots the average level of unsheltered homelessness per 100,000 residents across CoCs in the Ninth Circuit and outside. As described in the empirical section, these results restrict to CoCs that have ES systems that do not have adequate capacity in 2018. However, if we instead use all CoCs, as in Appendix Figure A10, the results are almost identical.

Figure 5: Average Unsheltered Homeless Count per 100k Population



Note: This figure plots the mean unsheltered homeless population per 100,000 residents across CoCs in the Ninth Circuit (black) and in the rest of the country (gray), scaled by the population of that CoC in 2016. Three vertical lines indicate the timing of the Martin, Grants Pass (9th), and Grants Pass(SC) decisions, and we shade the first year of the COVID-19 pandemic, from March 2020 to March 2021. We omit 2021 since most jurisdictions did not conduct a PIT count due to the pandemic. Data is derived from the annual PIT count, and includes all 233 CoCs at or above 100% ES capacity in 2018 (63 in the Ninth Circuit and 170 outside). CoCs are all weighted equally.

Figure 5 shows that per-capita unsheltered homelessness is substantially higher and has been growing faster in the Ninth Circuit over almost the entire data window. This large difference in levels and trend is certainly the driving force behind the popular hypothesis that the court cases increased homelessness. Indeed, unsheltered homelessness in the Ninth Circuit increased more sharply from 2018 to 2019 than in any previous year. In contrast,

regions outside the Ninth Circuit saw a slight decline over the same period, continuing a gradual downward trend from earlier years. The sharp uptick from 2018-2019 within the Ninth Circuit coincides with the *Martin* decision, issued in September 2018. However, because the PIT counts are conducted each January–just three months after the ruling–it is challenging to determine how much of the 2019 increase could be attributed directly to *Martin* as opposed to other developments earlier in 2018.

Given the large differences in both trends and levels across areas, we use SDID to construct a more credible comparison group. Because the PIT data are limited, we do not divide the analysis into three separate post-periods; instead, we examine the entire post-*Martin* period together. Since the data are annual and the court decision occurred late in 2018, we report results using two reference points: January 2018 and January 2019. The true effect likely lies between these values: it would be misleading to attribute the entire change from January 2018 to January 2019 to a decision made in September 2018, but it could also be misleading to ignore any potential impact that occured between September and December.

Table 4: Impact of Court Decisions — Unsheltered Homelessness (HMIS Data, SDID)

	Martin 2018	*Martin* 2019
Unsheltered /100k Pop		
τ	52.04	15.36
(SE)	(37.59)	(27.26)
$\bar{Y}$	197.85	203.08
N	2,796	2,796

Note: This table displays the aggregate SDID point estimates, comparing the change in shelter supply and shelter capacity in the Ninth Circuit following Martin to a synthetic control of CoCs in the rest of the country. The first column, Martin 2018 measures the impact if we set the reference year to 2018. The second column measures the impact if we set the reference year to 2019. The unit of analysis is a CoC-year. All outcomes are scaled per 100,000 residents, where the number of residents is the average population in the CoC from 2016-2018.

Table 4 presents the corresponding point estimates. Using January 2018 as the reference period, we estimate a post-*Martin* increase of 52 unsheltered individuals per 100,000 residents (SE: 38). Using January 2019 as the reference, the estimated increase falls to 15 (SE: 27). Neither estimate is statistically significant. The associated SDID plots are presented in Appendix Figure A11. As discussed earlier, we would expect to have seen larger impacts in this sample of regions that lacked adequate shelter capacity. Instead, when we expand the sample to all CoCs we see almost identical effects on the level of unsheltered homelessness ($\hat{\tau}$: 48, SE: 33). This provides additional evidence that a legal interpretation of *Martin* was not the primary driver of the observed increases in unsheltered homelessness.

Unfortunately, the missing data for 2021 and 2025, combined with the fact that we observe only one point per year, limits the precision of this analysis. To address these limitations, we turn to a higher-frequency dataset based on 311 call records.

6.2.2 Unsheltered Homelessness: 311 Encampment Reports

As a reminder, we use the 311 reports to generate a measure of encampments (unique encampment regions) that counts the number of distinct 100m clusters of reports in a given month, which we interpret as encampment regions of concern to residents. This is our main measure, as it correlates most closely with the PIT (in January), and is less subject to concerns about reporting bias.[28]

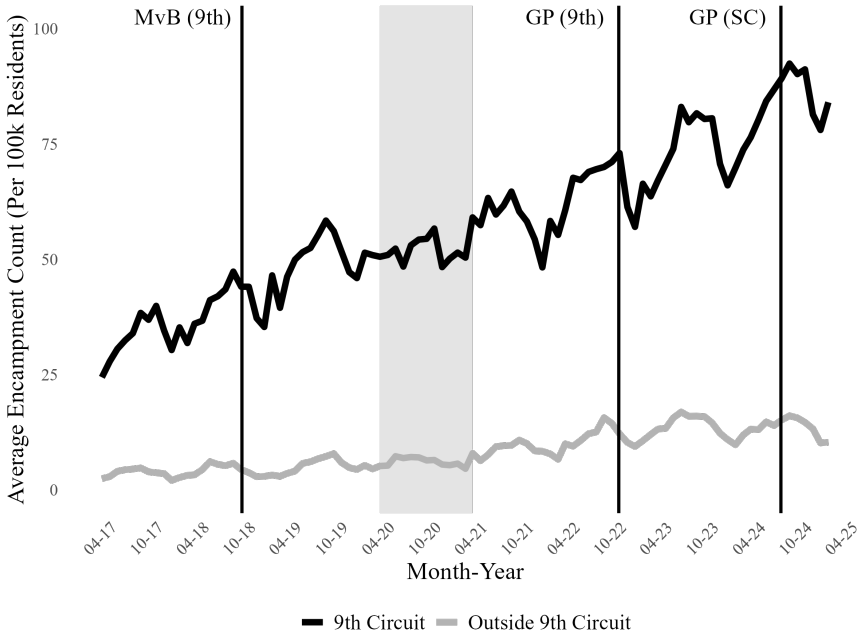
Figure 6 shows the raw average number of unique encampment regions reported by month in Ninth Circuit cities (black line) compared to cities outside of the Ninth Circuit (gray line). Compared to the PIT count, this chart begins later, in April 2017, and extends out to April 2025. Furthermore, since we can see observations within a year, we can observe that encampment reports in both regions exhibit consistent seasonality, with fewer encampments (or reports of encampments) in the winter.

Despite these differences in sample size and time coverage compared to the PIT, several patterns in the 311 data mirror those in the PIT. First, Ninth Circuit cities have more encampments reports on average. Second, encampments in these cities have grown faster over time, beginning at least a year before the *Martin* decision. In fact, when we extend a linear trend from the pre-Martin period through the rest of the time series (as shown in Appendix Figure A12), the post-decision data aligns falls slightly below that projected path–suggesting that unsheltered homelessness continued along a trajectory similar to or lower than what we might have expected even without the *Martin* decision.[29] The large difference in levels and the relatively constant growth rates motivate our use of a first-differenced version of this outcome, which compares changes in the outcome from one period to the next. The first-differenced version of this plot is presented in Figure 7. As expected, the two regions appear much more similar in terms of their period-to-period changes, although the Ninth Circuit exhibits substantially higher variance.

[28] We also present results for our measure based on call rates in Appendix Figure A16, which exhibit similar patterns.

[29] This projection assumes that, absent *Martin*, the pre-Martin trend in Ninth Circuit cities would have continued unchanged. While this linear extrapolation isn't necessary when using a comparison group (as we do in the SDID analysis in the next section), it serves as a useful visual heuristic. Importantly, this assumption is generally harder to test and often less plausible than the assumptions underlying methods that incorporate comparison groups, like SDID. Nonetheless, it helps illustrate that the rate of growth in unsheltered homelessness was already steep before *Martin* and that the observed post-Martin trajectory does not clearly deviate from that trend.

Figure 6: Average Unique Encampment Count per 100k Population



Note: This figure plots the mean count of "unique encampment regions" across cities in the Ninth Circuit and in the rest of the country, scaled by the population of each city in 2018. Cities are all weighted equally.

In both versions of the descriptive plots, the evidence suggests that the growth rate in unsheltered homelessness did not shift markedly after *Martin* or *Grants Pass (9th)* and *(SC)*: it was already increasing rapidly beforehand and continued along a similar trajectory afterward.

To formalize this intuition, we present the SDID results in Figure 8. Table 5 reports the aggregate SDID point estimates for enforcement outcomes, and Appendix Figure A13 displays the corresponding SDID plots. We focus on the first-differenced specification as our main result in Figure 8.[30]

Using the first differenced specification, we have that encampments were increasing by an average of 1.22 per 100,000 residents per month. Following Martin, we estimate an additional 0.30 encampments per 100,000 residents per month (SE: 0.87). However, this estimate is not statistically significant. Examining results city by city, we find a mix of modest increases

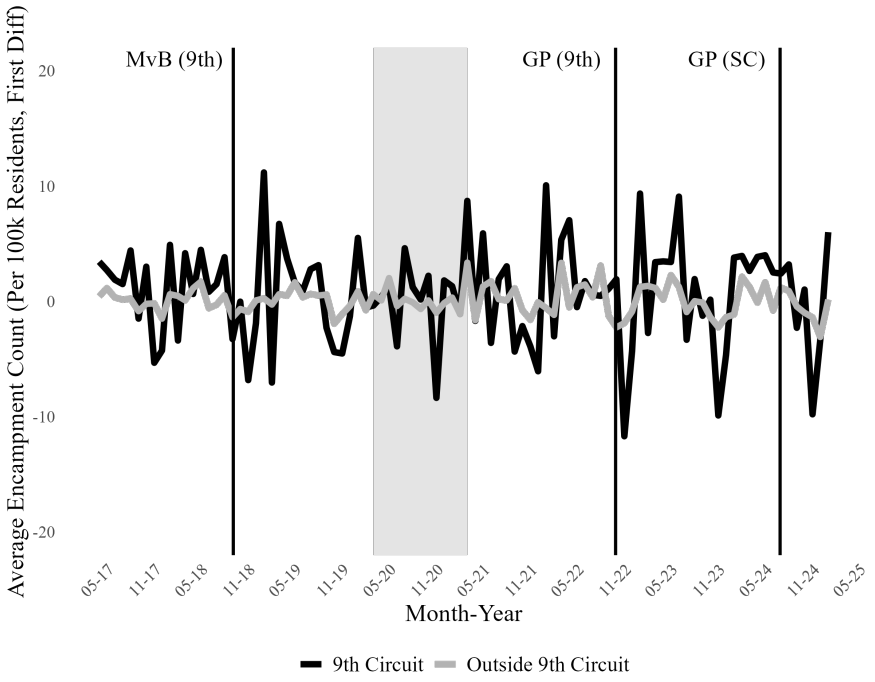
[30]Given the limited donor pool and the substantial pre-existing differences between the Ninth Circuit and the rest of the country, the synthetic control group struggles to reproduce the Ninth Circuit's level & trajectory of encampment reports during this period. In contrast, the first-differenced specification provides a much closer pre-treatment fit. For completeness, we also include point estimates for the level changes in Table 5, and associated estimates in Appendix Figures A14 and A15.

Table 5: Impact of Court Decisions — Unsheltered Homelessness (311 Data, SDID)

	Martin v. Boise (9th)	Grants Pass (9th)	Grants Pass (SC)
Unique Encamp. /100k Pop, First Diff			
τ	0.30	-1.54	0.62
(SE)	(0.87)	(1.49)	(1.43)
$\bar{Y}$	1.22	0.62	1.02
N	567	675	782
Unique Encamp. /100k Pop, Levels			
τ	5.30	2.42	7.09
(SE)	(6.80)	(3.58)	(12.17)
$\bar{Y}$	33.23	49.29	80.26
N	588	700	816
Enc/Hmls % of Calls			
τ	1.36	-0.51	0.13
(SE)	(2.72)	(0.41)	(0.44)
$\bar{Y}$	5.29	7.08	10.94
N	560	792	768

Note: This table displays the aggregate SDID point estimates, comparing the change in unique encampments (100m), first difference of unique encampments, and the % of reports related to encampments or homelessness in the Ninth Circuit following each of the three court cases on the Ninth Circuit compared to a synthetic control of cities in the rest of the country. The unit of analysis is a month-city. All outcomes are scaled per 100,000 residents, where the number of residents is the average population in the city from 2016-2018.

Figure 7: First Difference: Average Unique Encampment Count per 100k Population



Note: This figure plots the mean count of "unique encampment regions" across cities in the Ninth Circuit and in the rest of the country, scaled by the population of each city in 2018.

and decreases.

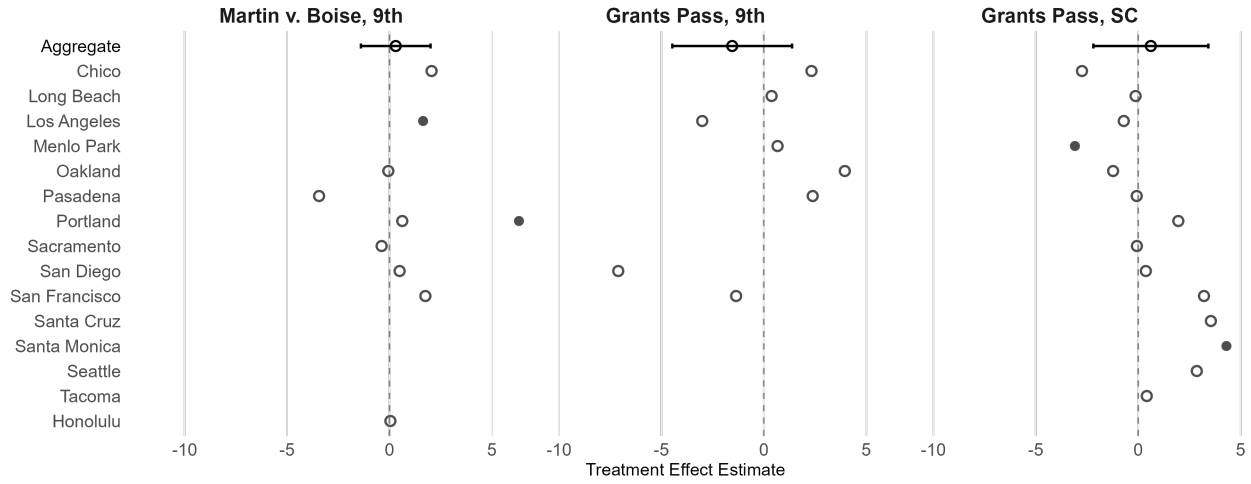
Following *Grants Pass (9th)*, we find a large aggregate decrease of -1.54 (SE: 1.49), driven by large declines in a few cities and increases in others. Finally, after *Grants Pass (SC)*, we observe an aggregate increase of 0.62 (SE: 1.43). Looking city-by-city, we find that after *Grants Pass*(SC), most cities actually experienced an increase in encampment reporting. This pattern runs counter to the popular hypothesis. Under that view, the increase in citations following the Supreme Court decision should have reduced visible homelessness. Instead, we observe the opposite. The finding suggests that substantial increases in enforcement did not lead to corresponding reductions in encampment presence.[31]

6.2.3 Summary of Homelessness Results

The PIT and 311 data paint a consistent picture: while visible homelessness in the Ninth Circuit has increased over the past decade, these increases do not line up clearly with the timing of *Martin*, *Grants Pass (9th)*, or *Grants Pass (SC)*. Instead, both sources suggest

[31] We see similar directional results with call rates (Appendix Figures A16 & A17), and similarly impre.

Figure 8: Treatment Effects: Unique Encampment Count per 100k Population (First Difference)



Note: These figures display the SDID point estimates by city following the Martin and GP (9th) & (SC) decisions. Solid circles indicate that the estimated coefficient is statistically significant at the 95% level.

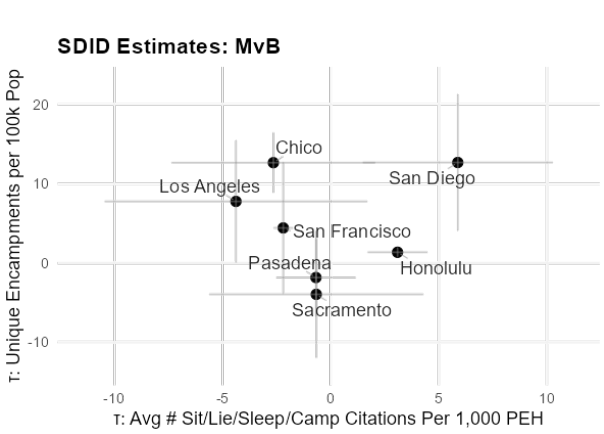
long-standing structural trends.

6.3 Relationship Between Enforcement and Visible Homelessness

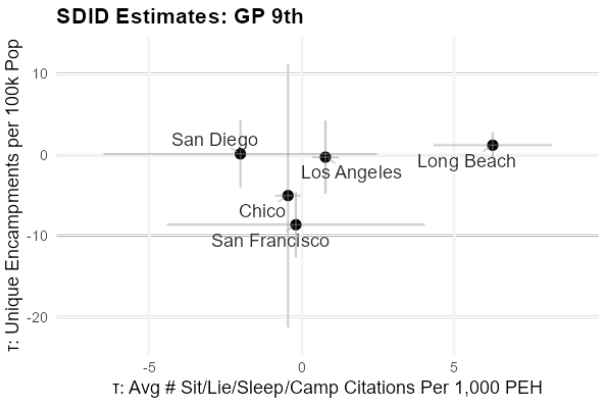
Could enforcement changes be driving a significant part of the growth in unsheltered homelessness? Our findings suggest that this is likely not the case. As shown in the prior sections, enforcement (measured through citations) did not consistently decline after *Martin* or *Grants Pass (9th)*, making it difficult to argue that changes in enforcement explain changes in homelessness. However, there is a chance that the heterogeneity in citation effects could align with the heterogeneity in homelessness effects in a way that could persuade us one way or the other.

In this section, we ask whether cities that changed enforcement practices experienced correspondingly different changes in visible homelessness compared to cities that maintained their pre-decision enforcement levels. To investigate this, Figure 9 plots each city's point estimate for changes in citations (relevant citations per 1,000 PEH, X-axis) against the same city's point estimate for changes in encampment reporting (unique encampments per 100k residents, Y-axis). Each panel presents results following one of the main three court cases. The 95% confidence intervals are displayed along both axes for each city's estimate. We can think of this figure in a simple instrumental variables framework, where the X-axis is plotting

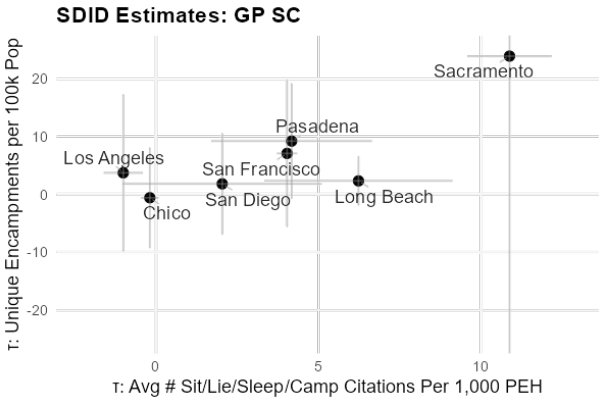
Figure 9: City Level Enforcement and Homelessness Reporting Responses



(a) *Martin*



(b) *GP (9th)*



(c) *GP (SC)*

Note: This figure plots treatment effects on enforcement (citations, y-axis) and unsheltered homelessness (# unique encampments, x-axis) for the set of cities for which we have data for both. The point estimates come from a SDID specification where we have one treated unit (the labeled city) and all control cities. Standard errors are estimated using a unit-placebo estimator.

the first stage effect on enforcement in each city, and the Y-axis is plotting the reduced form effect on homelessness. The standard error associated with these point estimates is quite large, and as a result we want to frame the following discussion as providing helpful suggestive evidence.

Following the popular explanation endorsed by cities and the Supreme Court, we would expect to observe a negative relationship between these variables–that is, cities that reduced enforcement more substantially should have experienced greater increases in encampment reporting, consistent with the theory that citation authority is necessary for managing visible homelessness.

Looking first at the *Martin v. Boise* results in Panel A, we observe no clear pattern. If we put a line of best fit through the points, it would actually have a slope of 0.17. Panel B, examining *Grants Pass (9th)*, similarly reveals no strong pattern for either measure, if anything, showing a positive relationship. Panel C, covering *Grants Pass (SC)*, also shows a plausibly positive relationship, and the simple line of best fit has a slope of 1.67 – though again, we emphasize that this is an imprecise and illustrative exercise.

This city-level analysis reveals that only the weakest correlation between enforcement changes and homelessness outcomes. This pattern does not support the popular theory that citation authority is a crucial tool for managing visible homelessness. The heterogeneity we observe indicates that local institutional factors, political priorities, and enforcement capacity likely play a more significant role in determining outcomes than the formal legal constraints imposed by court decisions.

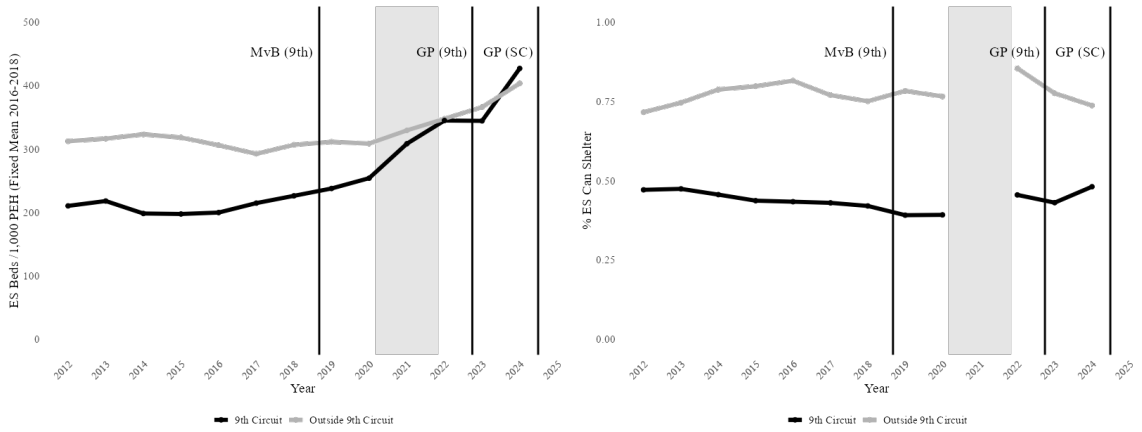
In sum, we do not find compelling evidence that legal constraints on enforcement led to systematic increases in homelessness. Nor do we find clear evidence that expanded enforcement following *Grants Pass (SC)* has, at least in the short term, reduced visible homelessness.

6.4 Cities: Changes in Shelter Provision

Finally, we look at whether the legal incentives led cities (or the CoCs under which they operate) to change their level of shelter provision. Figure 10, Panel A plots the mean count of ES beds, while Panel B plots average shelter capacity (ES beds/[unsheltered + ES sheltered]). Panel A shows that there was a clear relative increase in number of beds in the Ninth Circuit. This pattern is consistent with the theoretical mechanism in which cities facing new legal constraints expand shelter supply strategically to preserve enforcement authority. However, Panel B shows that these increases were insufficient to keep pace with growing need: average ES capacity rose only slightly by the end of the period, implying that additional beds primarily served to offset rising unsheltered populations rather than to

create meaningful excess capacity.

Figure 10: Emergency Shelter: Bed Count and Capacity



(a) Average ES Bed Count

(b) Average ES Capacity

Note: Panel A plots the mean count of ES beds across CoCs in the Ninth Circuit and in the rest of the country, scaled by the average unsheltered homeless population in each CoC from 2016-2018 (/1,000 PEH). Panel B plots mean ES capacity (% of need that ES can house). Data is a balanced panel, derived from the annual PIT count, and includes all 233 CoCs at or above 100% ES capacity in 2018 (63 in the Ninth Circuit and 170 outside).

The SDID estimates in Table 6 and Appendix Figure A18 confirm this pattern: emergency shelter beds saw a relative increase after *Martin*, using either the January 2018 or January 2019 reference periods, although the standard errors are very large. We see similar trends in other (non-ES, e.g., transitional or permanent supportive housing shelter beds).[32] In contrast, the same table shows that ES capacity did not expand meaningfully–and may even have declined slightly–meaning that the growth in homelessness outpaced new shelter provision. When we expand the sample to all CoCs, where we would expect to see a smaller impact if the court case constraint was driving these changes, we see very similar results for the count of ES beds ($\hat{\tau}$: 59, SE: 60) and ES capacity ($\hat{\tau}$: 0.03, SE: 61).

Together, these results are consistent with (but cannot provide conclusive evidence in support of) the idea that cities may have used shelter beds as a strategic response mechanism. Cities appear to have partially adjusted shelter supply in the face of new legal constraints, but not to the extent necessary to restore full enforcement authority. This muted response likely reflects fiscal and political constraints on shelter expansion.

Moreover, the minimal aggregate impact on ES capacity aligns with our broader finding that enforcement practices and visible homelessness changed only modestly after *Martin*.

[32]See Appendix Figures A19 and A20 for the raw trends in non-ES beds and associated SDID plots.

Table 6: Impact of Court Decisions — Shelter Provision (SDID)

		Martin 2018	*Martin* 2019
ES Beds /1,000 PEH			
	τ	47.74	50.44
	(SE)	(53.20)	(47.11)
	$\bar{Y}$	209.11	212.68
	N	3,029	3,029
Other Beds /1,000			
	τ	94.16	76.64
	(SE)	(96.45)	(56.37)
	$\bar{Y}$	473.87	484.29
	N	3,029	3,029
ES Capacity (% Can Shelter)			
	τ	-0.03	0.03
	(SE)	(0.07)	(0.06)
	$\bar{Y}$	0.45	0.44
	N	2,796	2,796

Note: This table displays the aggregate SDID point estimates, comparing the change in shelter supply and shelter capacity in the Ninth Circuit following Martin to a synthetic control of CoCs in the rest of the country. The first column, Martin 2018 measures the impact if we set the reference year to 2018. The second column measures the impact if we set the reference year to 2019. The unit of analysis is a CoC-year. All outcomes are scaled per 1,000 PEH, where the count of PEH is the fixed average from 2016-2018.

Even if some cities sought to increase shelter beds strategically, the scale of these adjustments was too small to shift overall system dynamics dominated by structural housing shortages. In this sense, the shelter results provide an important link between the legal incentives and the broader equilibrium forces: legal constraints appear to have nudged local governments toward marginal policy adjustments rather than fundamentally altering homelessness trajectories.

7 Understanding the Drivers of Unsheltered Homelessness

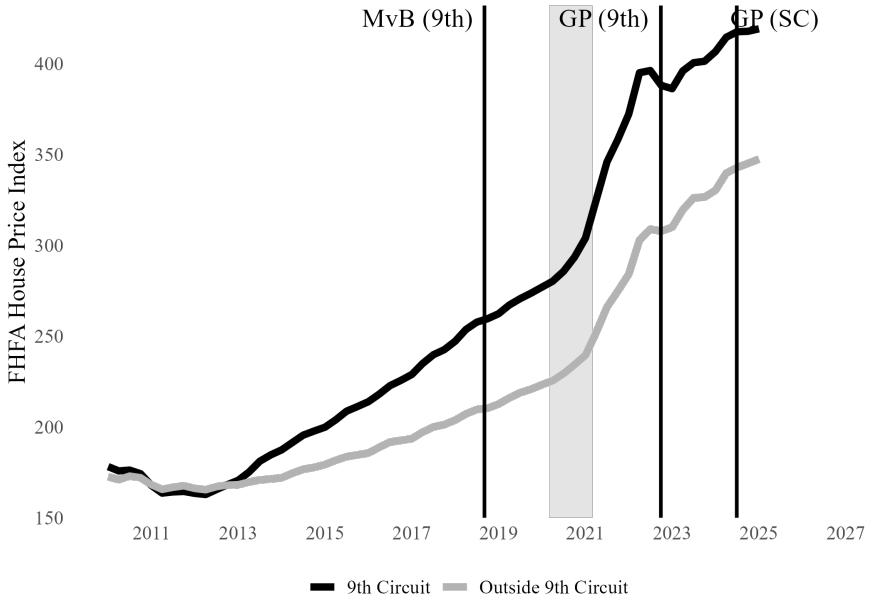
Our results challenge popular narratives attributing rising homelessness to the restrictive Ninth Circuit *Martin v. Boise* and *Grants Pass* decision. While some cities adjusted their enforcement policies in response to the ruling, many cities substituted to other equivalent enforcement strategies, or did not respond at all. Furthermore, we find limited evidence that these legal constraints caused the significant increases in encampments observed across Western cities. This finding raises an important question: If court rulings did not drive the differential increase in homelessness in Ninth Circuit jurisdictions, what factors did? Though a comprehensive causal analysis of the drivers of homelessness exceeds this paper's scope, we discuss housing market dynamics as one plausible mechanism that could explain regional disparities in homelessness trends.

Extensive empirical research shows that trends in homelessness are associated with house prices and rent, which are both generally at higher levels and increasing at faster rates in West Coast cities.[33] Figure 11 displays the evolution of the FHFA house price index in Ninth Circuit versus non-Ninth Circuit regions from 2011-2025. Western cities experienced substantially steeper increases in housing costs, with home prices rising 117% in the Ninth Circuit regions compared to only 80% elsewhere during this period. Meanwhile, the ratio of median rent to median household income (a helpful proxy for rent burden) increased 4% in the Ninth Circuit from 2016 to 2023, compared to only 1% outside of it. In this light, the court decisions may have played a secondary role, shaping enforcement decisions at the margin but not driving the broader trend in visible homelessness.

Restrictive land use regulations may exacerbate these housing market pressures. We investigate this using the Wharton Residential Land Use Regulatory Index. This index is standardized to have a mean of 0 and a standard deviation of 1, with higher values indicating stricter regulation (Gyourko, Hartley, and Krimmel 2021). The average index in the Ninth Circuit is 0.51, compared to -0.07 in the rest of the country, suggesting that more restrictive

[33]See, e.g.: Quigley and Raphael (2001), Heston (2023); Zillow Research (2018); Colburn and Aldern (2022), Love and Loh (2023), and Kushel, Margot et al. (2023).

Figure 11: FHFA House Price Index



Note: This figure plots the average FHFA House Price Index in the Ninth Circuit and outside of the Ninth Circuit). Data is at the state-by-quarter level.

zoning may also contribute to limited housing supply and higher cost of housing the the Ninth Circuit.

8 Conclusion

Our findings reveal three fundamental insights about how constitutional constraints shape local homelessness policy. First, and perhaps most importantly, the pronounced heterogeneity in city responses underscores that landmark court decisions may not–and likely will not – have uniform effects. Rather than the single national story portrayed in public discourse and the Supreme Court's 2024 decision, *Martin* produced a patchwork of local responses shaped by cities' fiscal capacities, institutional structures, and political priorities. Some cities reduced enforcement substantially, others maintained or even increased it, and many adapted incrementally. This variation reveals how high-level legal constraints are mediated by local discretion and capacity.

Second, the limited aggregate impact challenges the dominant narrative that *Martin v. Boise* and related rulings fundamentally reshaped homelessness enforcement in practice.

Despite widespread claims that *Martin* "changed the face of homelessness in America,"[34] we find only modest shifts: cities' enforcement practices changed unevenly through graduated adjustments rather than dramatic reversals, and levels of unsheltered homelessness remained largely unchanged. This makes it difficult to sustain the narrative that set the stage for the Supreme Court's reversal.

Third, our results reveal a critical disjuncture between legal doctrine and enforcement practice. The persistence of encampment "sweeps" despite changes in citation activity suggests that some cities' enforcement capacity may operate largely outside the formal legal channels that *Martin* addressed. Sweeps can be conducted under property seizure authority, public health codes, or administrative regulations that remain available even when citation authority is constrained. This finding exposes a gap between constitutional doctrine focused on criminal penalties and the administrative reality of homelessness governance where property confiscation, public health authority, and informal pressure often matter more than citations.

Taken together, these results suggest that the key drivers of unsheltered homelessness lie beyond the reach of court rulings on criminalization. Future policy efforts must focus on underlying structural conditions such as housing affordability, service provision capacity, and interjurisdictional coordination that shape both the scale of homelessness and the realistic menu of responses available to cities. While data limitations and methodological constraints prevent us from ruling out all possible subtle effects, our analysis represents the most systematic empirical examination of these questions to date. Future research should examine the causal mechanisms linking housing costs to homelessness using plausibly exogenous variation in housing supply constraints, as well as the differential effectiveness of pandemic-era housing stabilization measures across jurisdictions.

[34] Versteeg, Cope, and Mukherjee (2025a)

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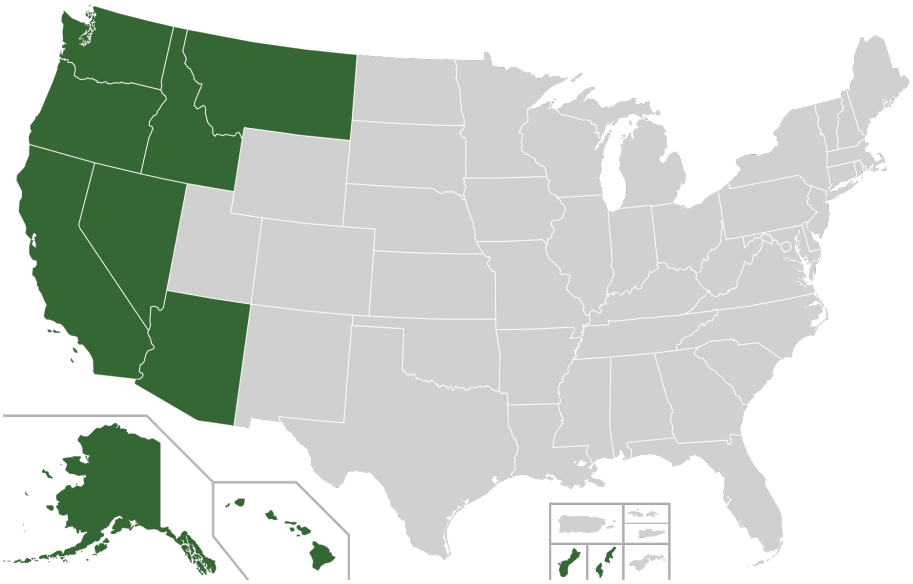
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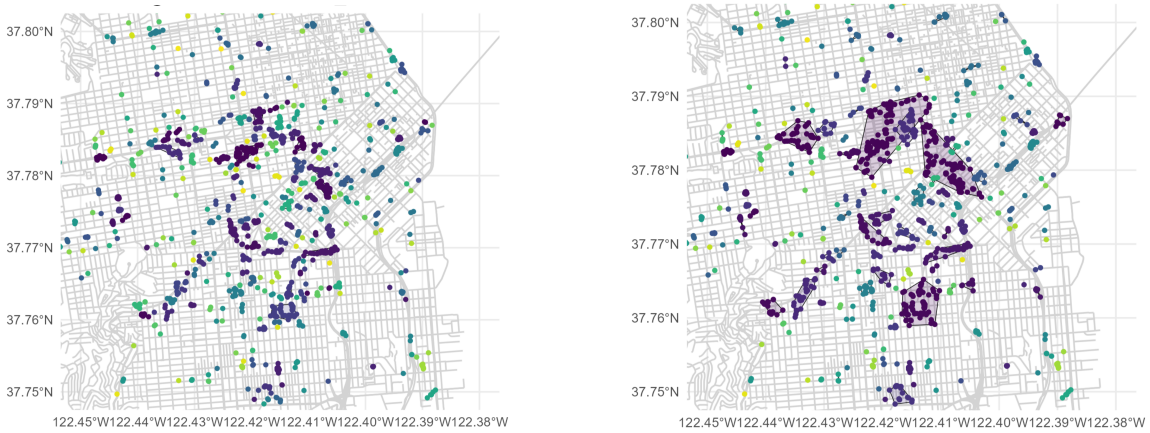
A Appendix Figures

Figure A1: Ninth Circuit Federal Court of Appeals Jurisdiction



Note: *Ninth Circuit states are highlighted in green: Alaska, Arizona, California, Hawaii, Idaho, Montana, Nevada, Oregon and Washington. Source: Wikipedia.*

Figure A2: Encampment Locations: Clustering 311 Calls

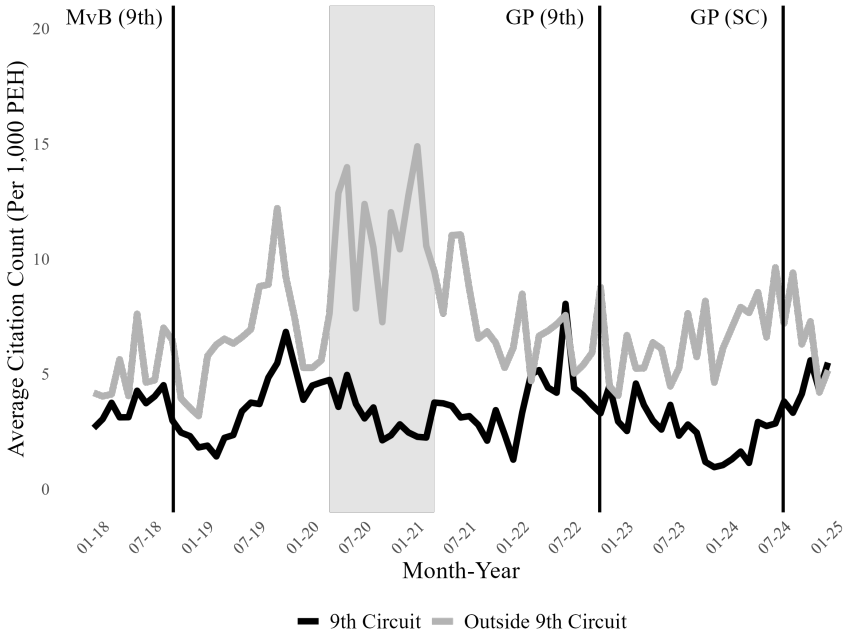


(a) 100m Clusters

(b) 150m Clusters

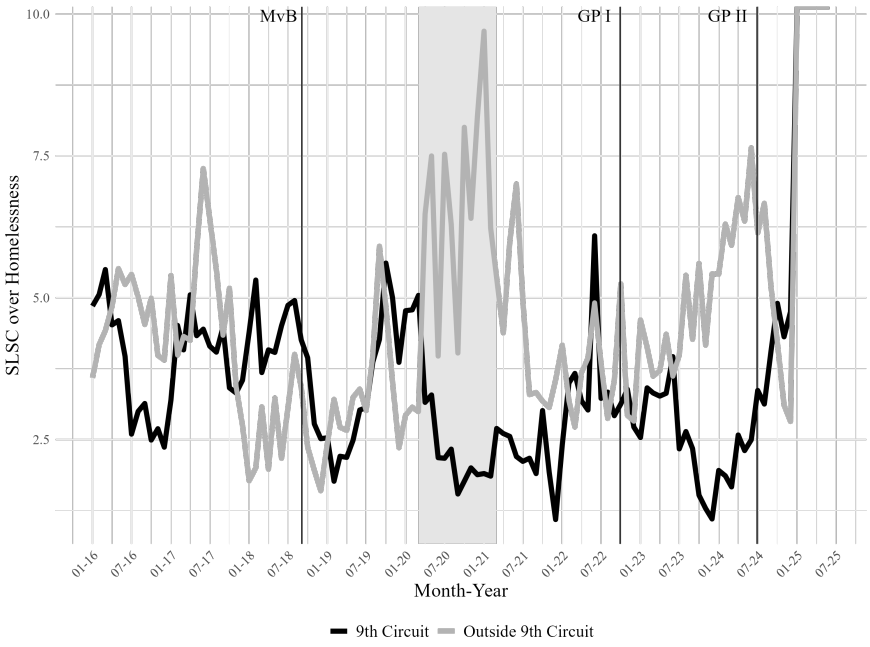
Note: These figures illustrate clustered unique encampment regions, using a clustering distance of 100 meters (our preferred measure) or 150 meters. This example is drawn from 311 reports made in January 2023 for San Francisco.

Figure A3: Average Count of Anti-Sleeping and Anti-Camping Citations per 1,000 PEH (Interpolated)



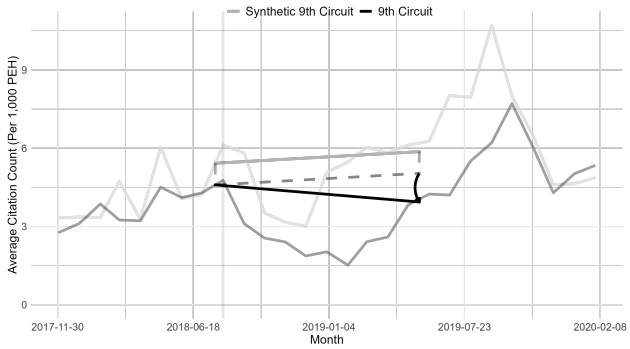
Note: This figure plots the mean count of anti-sleeping and anti-camping citations across cities in the Ninth Circuit and in the rest of the country, scaled by the total homeless population in each CoC and period. Since the total homeless count is only available in January of each year, we use a linear interpolation to approximate monthly values. Each city is weighted equally.

Figure A4: Average Count of Anti-Sleeping and Anti-Camping Citations per 1,000 PEH, Population Weighted

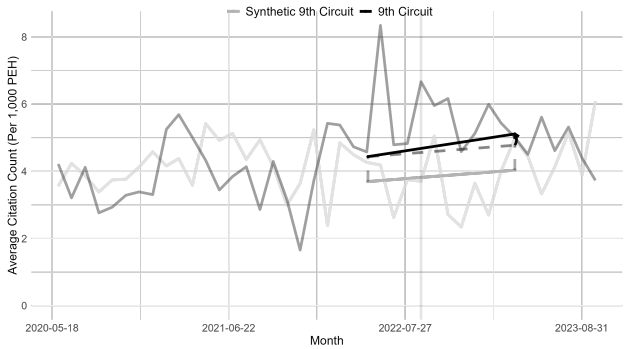


Note: This figure plots the mean count of anti-sleeping and anti-camping citations across cities in the Ninth Circuit and in the rest of the country. The averages are weighted by the homeless population in each city: (total citations) / (sum average PEH from 2016-2018).

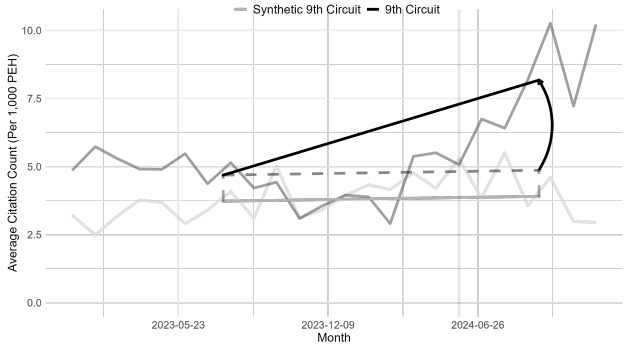
Figure A5: SDID Plots: Anti-Sleeping and Anti-Camping Citations per 1,000 PEH



(a) *Martin.* $\hat{\tau}$: −1.10, SE: 1.70



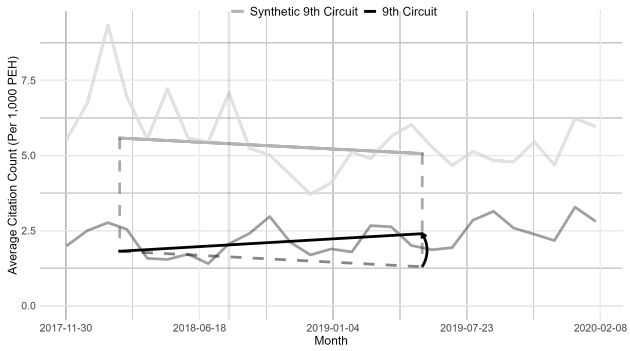
(b) *GP (9th).* $\hat{\tau}$: 0.33, SE: 1.04



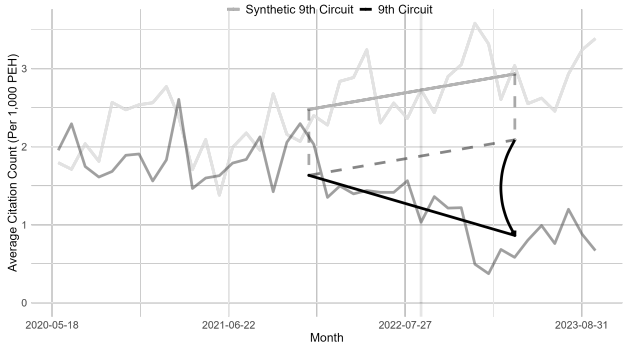
(c) *GP (SC).* $\hat{\tau}$: 3.31**, SE: 1.07

Note: These figures display the SDID estimate plots for the Martin and GP (9th) & (SC) decisions. The black arrow indicates the size of the treatment effect.

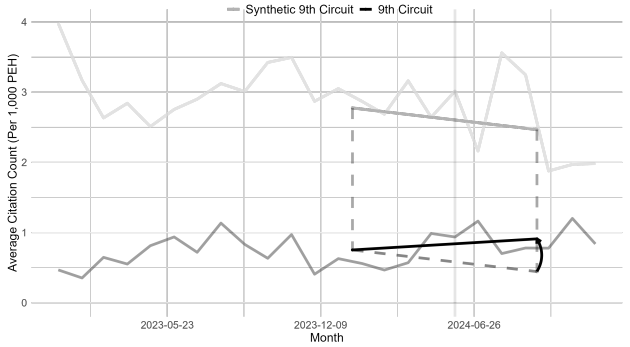
Figure A6: SDID Plots: Other Homelessness-Related Citations per 1,000 PEH



(a) *Martin.* $\hat{\tau}$: −1.10, SE: 0.91



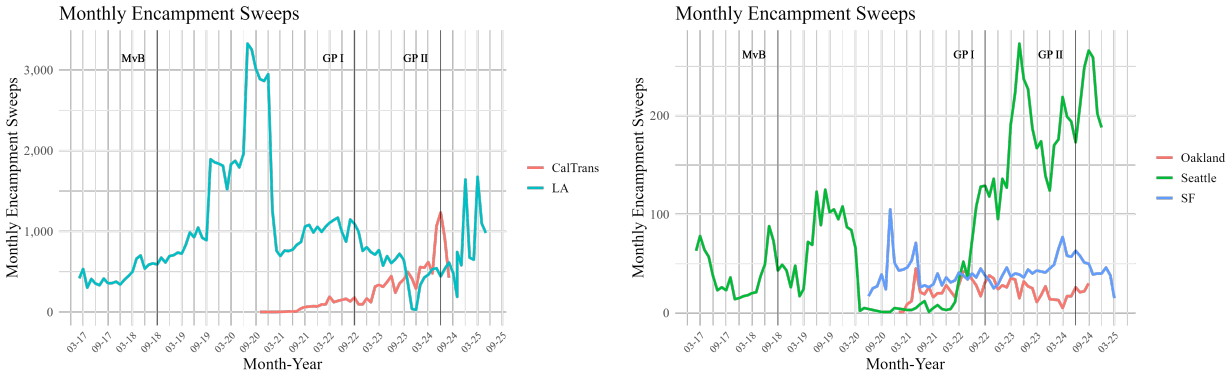
(b) *GP (9th).* $\hat{\tau}$: −1.22, SE: 0.87



(c) *GP (SC).* $\hat{\tau}$: 0.47, SE: 0.56

Note: Other homelessness-related citations include panhandling, public urination/defecation, and possession of shopping carts. These figures display the SDID estimate plots for the Martin and GP (9th) & (SC) decisions. The black arrow indicates the size of the treatment effect.

Figure A7: Encampment Sweeps

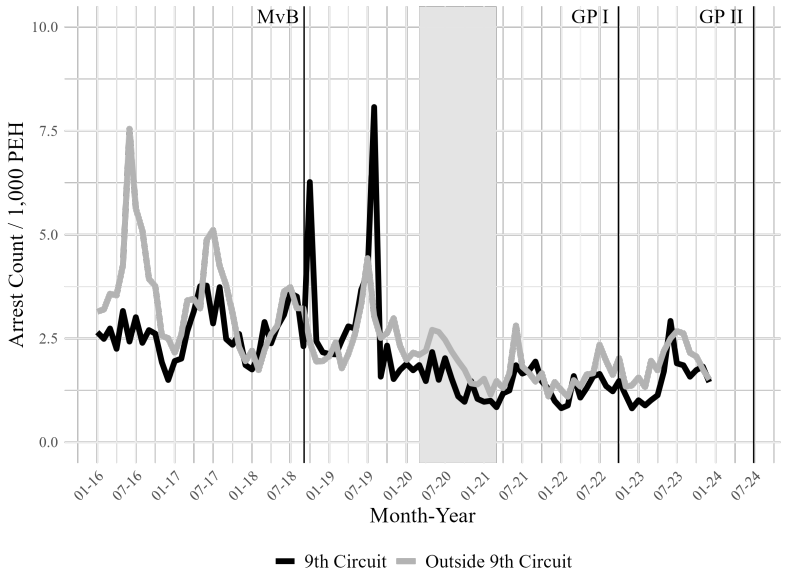


(a) LA, CalTrans

(b) Seattle

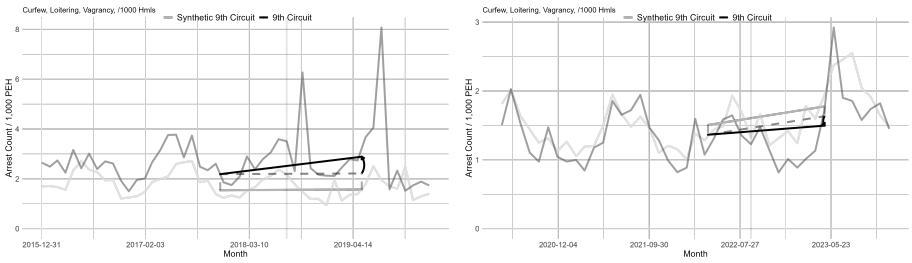
Note: These figures plot the total number of encampment "sweeps" or "clearings" in each region.

Figure A8: Average Count of Curfew, Loitering, Vagrancy Arrests per 1,000 PEH



Note: This figure plots the mean count arrests related to curfew, loitering, or vagrancy offenses in CoCs in the Ninth Circuit and in the rest of the country, scaled by the average unsheltered homeless population in that CoC from 2016-2018. Data is a balanced panel, derived from the FBI NIBRS, and represents 2,493 agencies in 166 CoCs (36 in the Ninth Circuit and 130 outside). Each CoC is weighted equally.

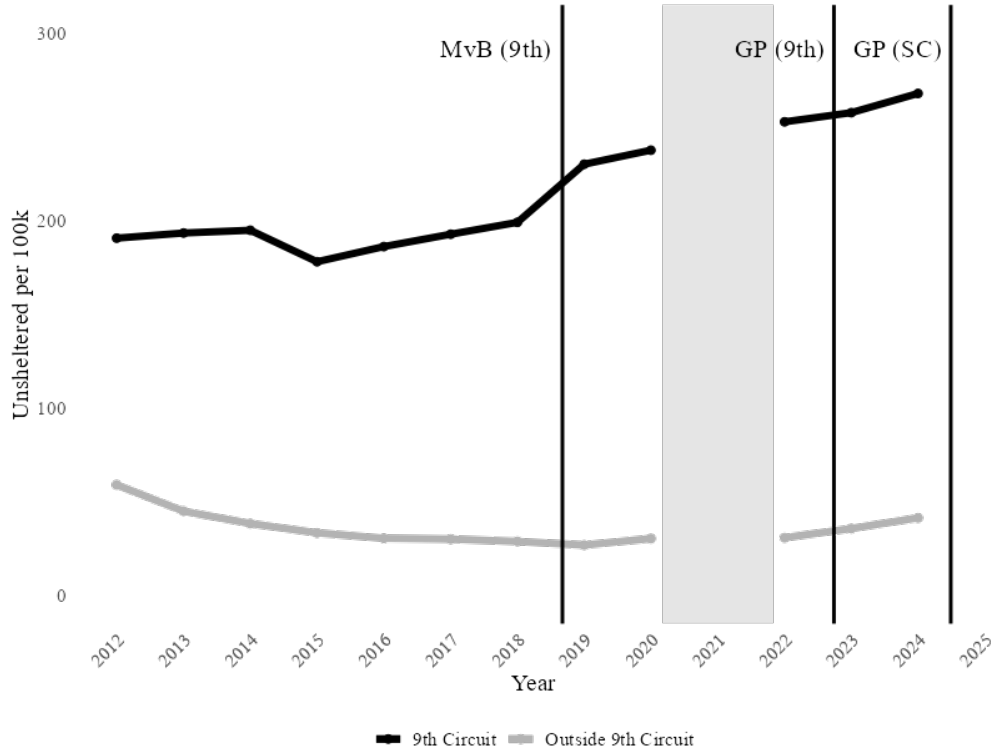
Figure A9: SDID Plots: Count of Relevant Arrests per 1,000 PEH



(a) *Martin* (b) *GP (9th)*

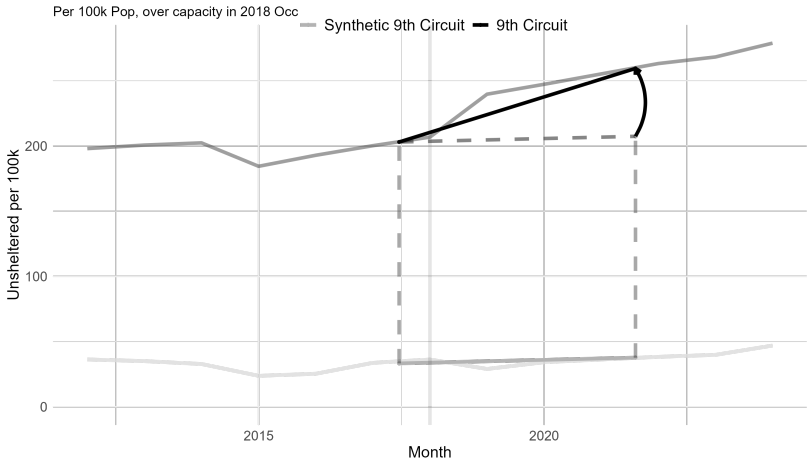
Note: These figures display the SDID estimate plots for the Martin and GP (9th) decisions. The black arrow indicates the size of the treatment effect. Standard errors are provided in Table 3.

Figure A10: Average Unsheltered Homeless Count per 100k Population: No Capacity Constraint

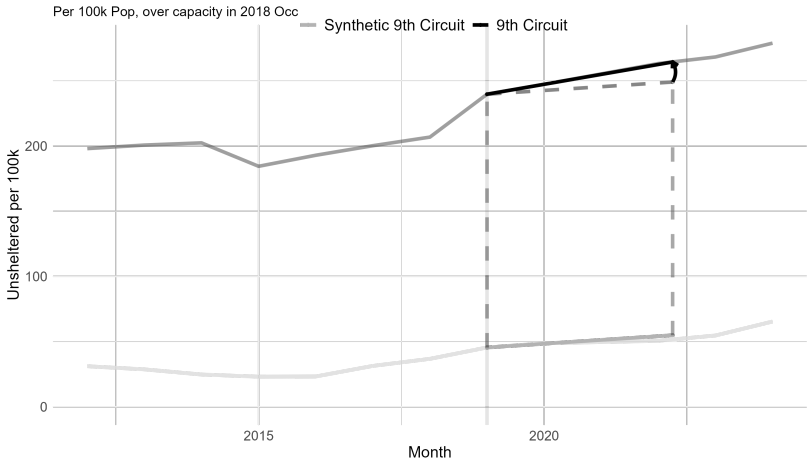


Note: This figure plots the mean unsheltered homeless population per 100,000 residents across CoCs in the Ninth Circuit (black) and in the rest of the country (gray), scaled by the population of that CoC in 2016. Three vertical lines indicate the timing of the Martin, Grants Pass (9th), and Grants Pass(SC) decisions, and we shade the first year of the COVID-19 pandemic, from March 2020 to March 2021. We omit 2021 since most jurisdictions did not conduct a PIT count due to the pandemic. Data is derived from the annual PIT count, and includes all CoCs with no capacity constraint.

Figure A11: SDID Plots: Unsheltered Homelessness (PIT Count) per Population



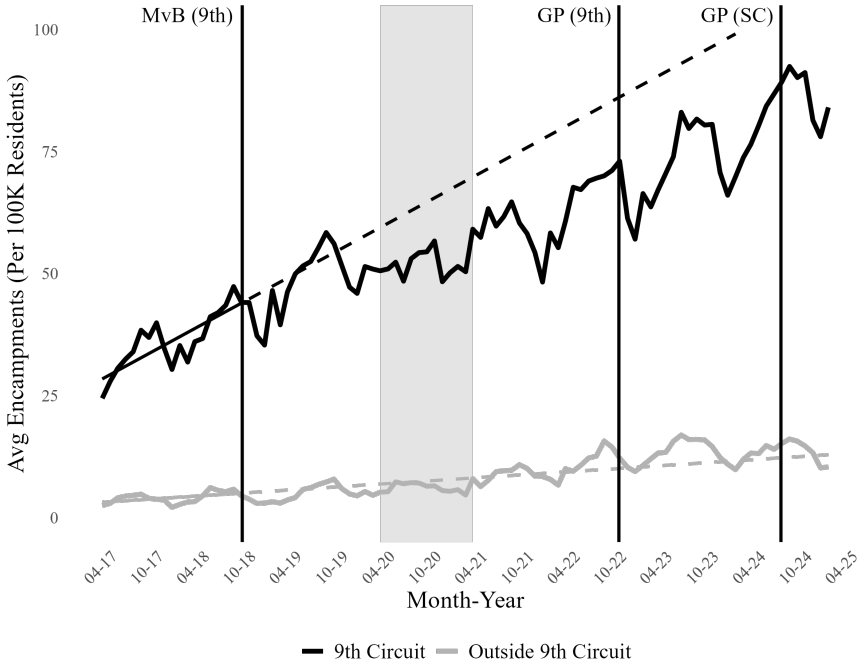
(a) *Martin* 2018 Reference. $\hat{\tau}$: 52.04, SE: 37.59



(b) *Martin* 2019 Reference. $\hat{\tau}$: 15.36, SE: 27.26

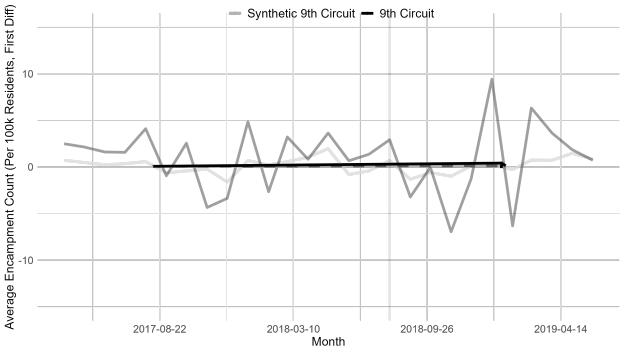
Note: These figures display the SDID estimate plots for the Martin decision on the average level of unsheltered homelessness (measured by the yearly PIT count), using a January 2018 reference period (Panel A) and a January 2019 reference period (Panel B). The black arrow indicates the size of the treatment effect. Standard errors are provided in Table 4.

Figure A12: Average Unique Encampment Count per 100k Population, with Linear Projection

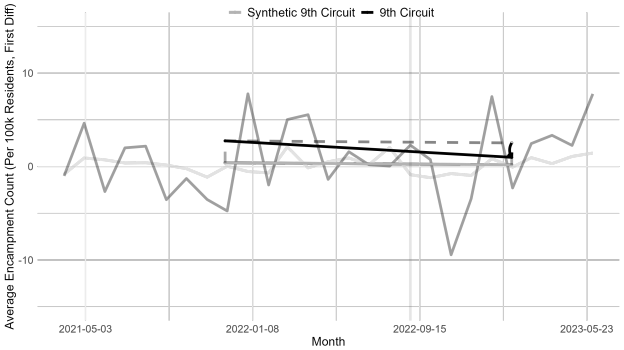


Note: This figure plots the mean count of "unique encampment regions" across cities in the Ninth Circuit and in the rest of the country, scaled by the population of each city in 2018. The dashed line is fit on the pre-2018 data and projected forward through the entire period using the same slope. Data is derived from 311 reports, and uses a balanced panel of cities from January 2018 to January 2025, including: Austin, Baton Rouge, Chattanooga, Chico, Dallas, Kansas City, Los Angeles, Minneapolis, New York City, Oakland, Orlando, Pasadena, Pittsburgh, Portland, Raleigh, San Diego, San Francisco, St. Louis.

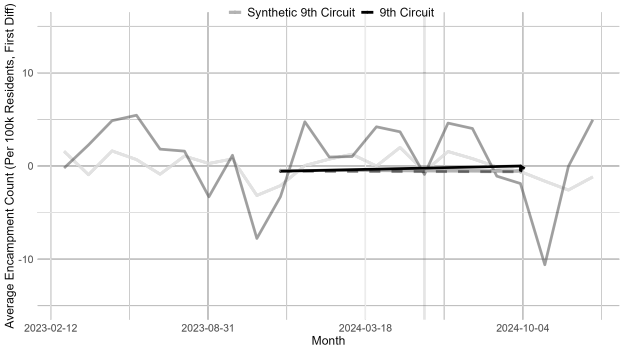
Figure A13: SDID Plots: Unique Encampment Count per 100k Population: First Difference



(a) *Martin.* $\hat{\tau}$: 0.30, SE: 0.87



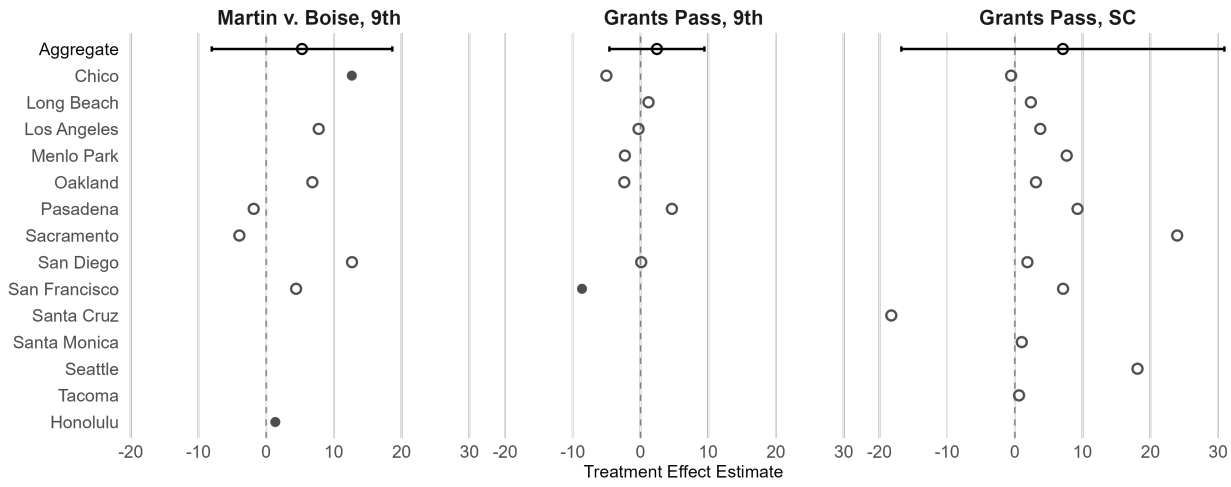
(b) *GP (9th).* $\hat{\tau}$: -1.54, SE: 1.49



(c) *GP (SC).* $\hat{\tau}$: 0.62, SE: 1.43

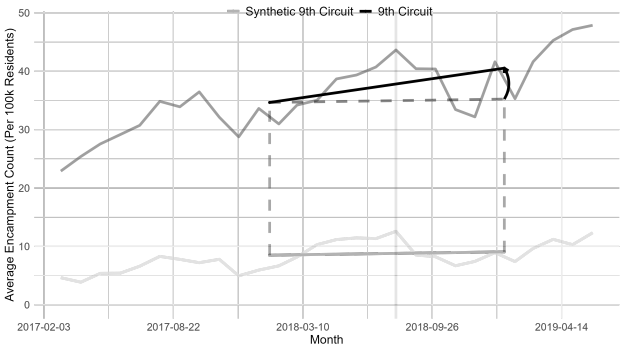
Note: These figures display the SDID estimate plots for the Martin and GP (9th) decisions. The black arrow indicates the size of the treatment effect. Standard errors are provided in Table 5.

Figure A14: Treatment Effects: Unique Encampment Count per 100k Population: Levels

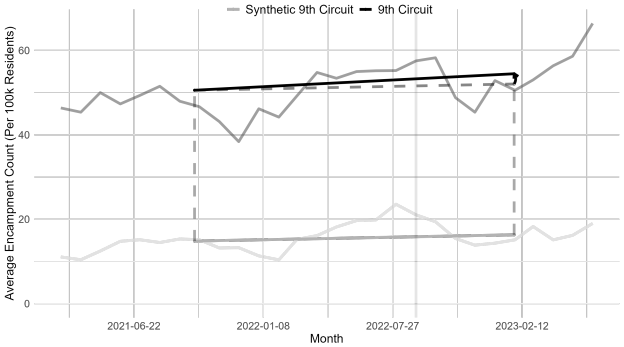


Note: These figures display the SDID point estimates for the impact on the count of unique encampment regions (100m, levels), on aggregate and by city following the Martin and GP (9th) & (SC) decisions. Solid circles indicate that the estimated coefficient is statistically significant at the 95% level. Numeric aggregate estimates are provided in Table 5.

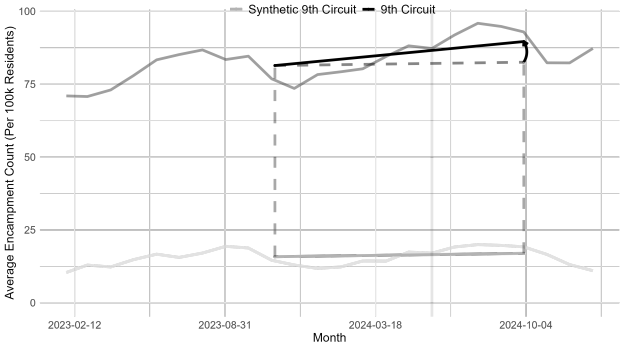
Figure A15: SDID Plots: Unique Encampment Count per 100k Population



(a) *Martin.* $\hat{\tau}$: 5.30, SE: 6.80



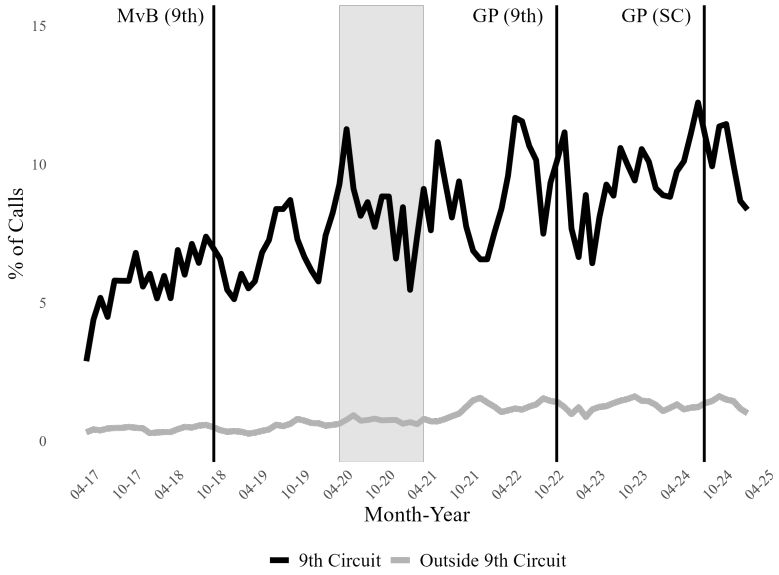
(b) *GP (9th).* $\hat{\tau}$: 2.42, SE: 3.58



(c) *GP (SC).* $\hat{\tau}$: 7.09, SE: 12.17

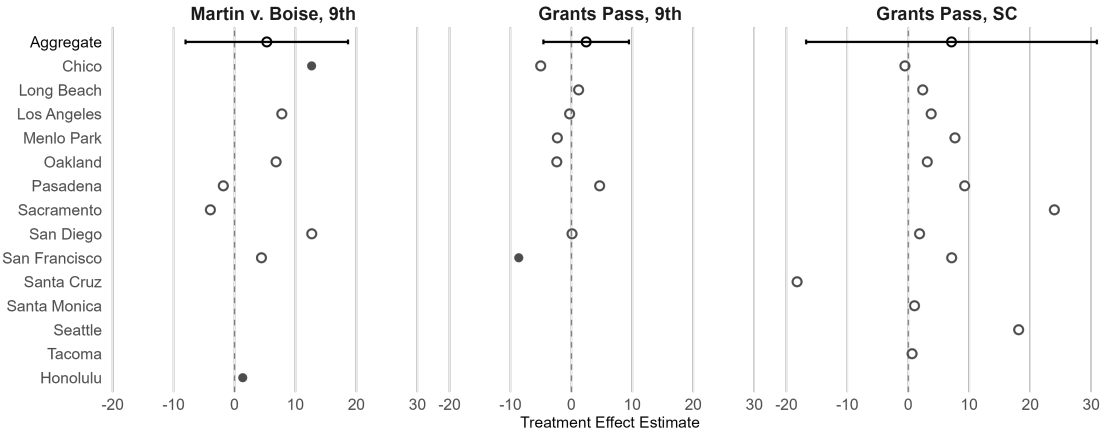
Note: These figures display the SDID estimate plots for the Martin and GP (9th) decisions. The black arrow indicates the size of the treatment effect. Standard errors are provided in Table 5.

Figure A16: Average % Homelessness-Related Reports



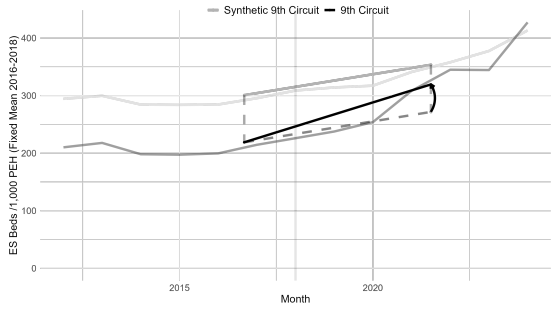
Note: This figure plots the average % of calls related to homelessness (# calls related to homelessness / # total calls) across cities in the Ninth Circuit and in the rest of the country. Data is derived from 311 reports, and uses a balanced panel of cities from January 2018 to January 2025.

Figure A17: Treatment Effects: Average % Homelessness-Related Reports

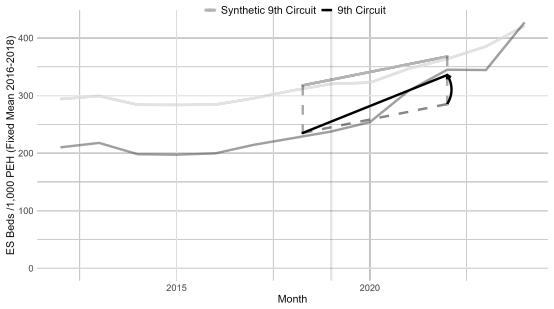


Note: These figures display the SDID point estimates and 95% CIs for the impact on the % of 311 reports related to homelessness or homeless encampments, on aggregate and by city following the Martin and GP (9th) & (SC) decisions. Numeric aggregate estimates are provided in Table 5.

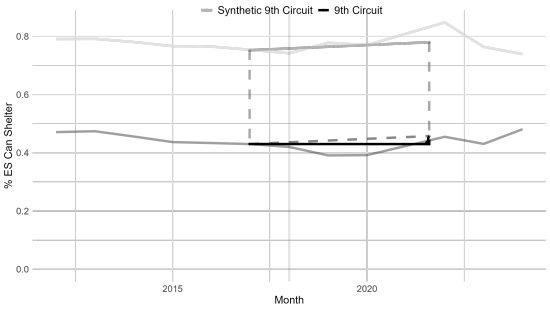
Figure A18: SDID Plots: Emergency Shelter Beds and Capacity



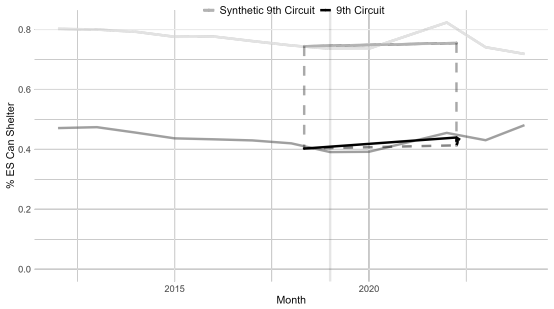
(a) ES Beds, *Martin* 2018 Reference. $\hat{\tau}$: 47.74, SE: 53.20



(b) ES Beds, *Martin* 2019 Reference. $\hat{\tau}$50.44 =, SE: 47.11



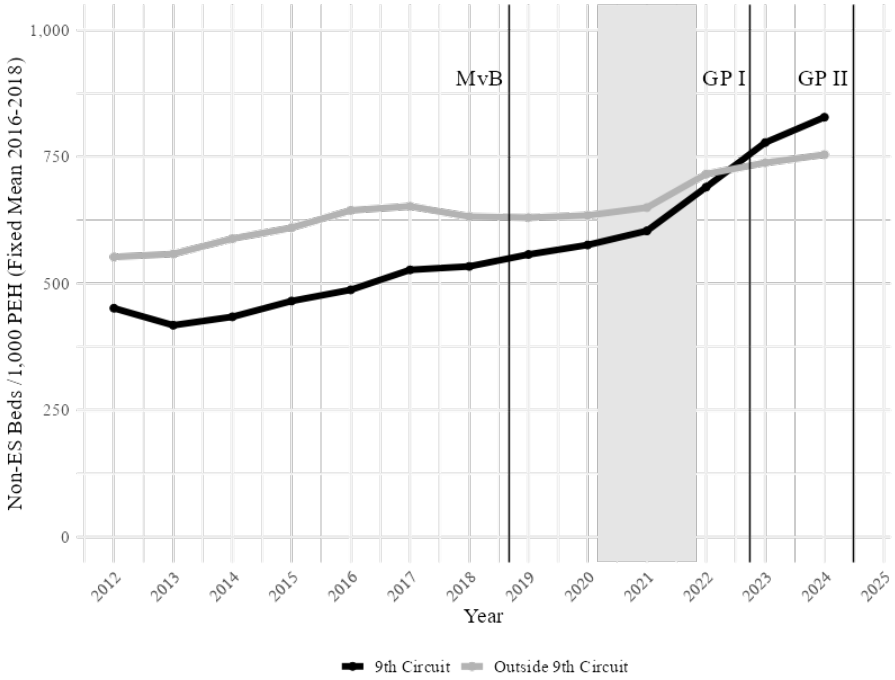
(c) ES Capacity, *Martin* 2018 Reference. $\hat{\tau}$: −0.03, SE: 0.07



(d) ES Capacity, *Martin* 2019 Reference. $\hat{\tau}$: 0.03, SE: 0.06

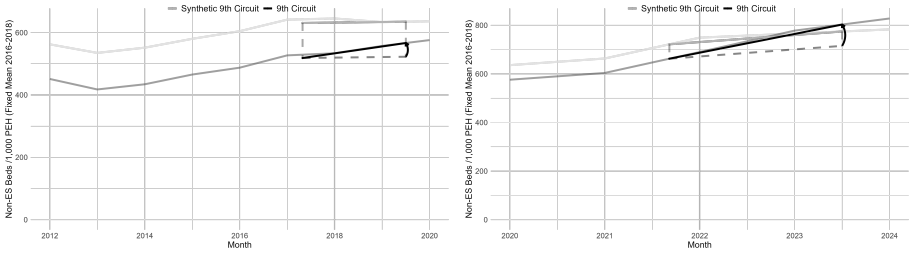
Note: These figures display the SDID estimate plots for the Martin decision on the average number of ES beds (Panels A & B) and average ES capacity (Panels C & D), using a January 2018 reference period (column 1) and a January 2019 reference period (column 2). The black arrow indicates the size of the treatment effect. Standard errors are provided in Table 6.

Figure A19: Average Non-Emergency Shelter Bed Count per 100k Population



Note: This figure plots the mean count of Non-ES beds across CoCs in the Ninth Circuit and in the rest of the country, scaled by the average unsheltered homeless population in that CoC from 2016-2018. Data is a balanced panel, derived from the annual PIT count, and includes all 233 CoCs at or above 100% ES capacity in 2018 (63 in the Ninth Circuit and 170 outside).

Figure A20: SDID Plots: Non-Emergency Shelter Bed Count per 100k Population

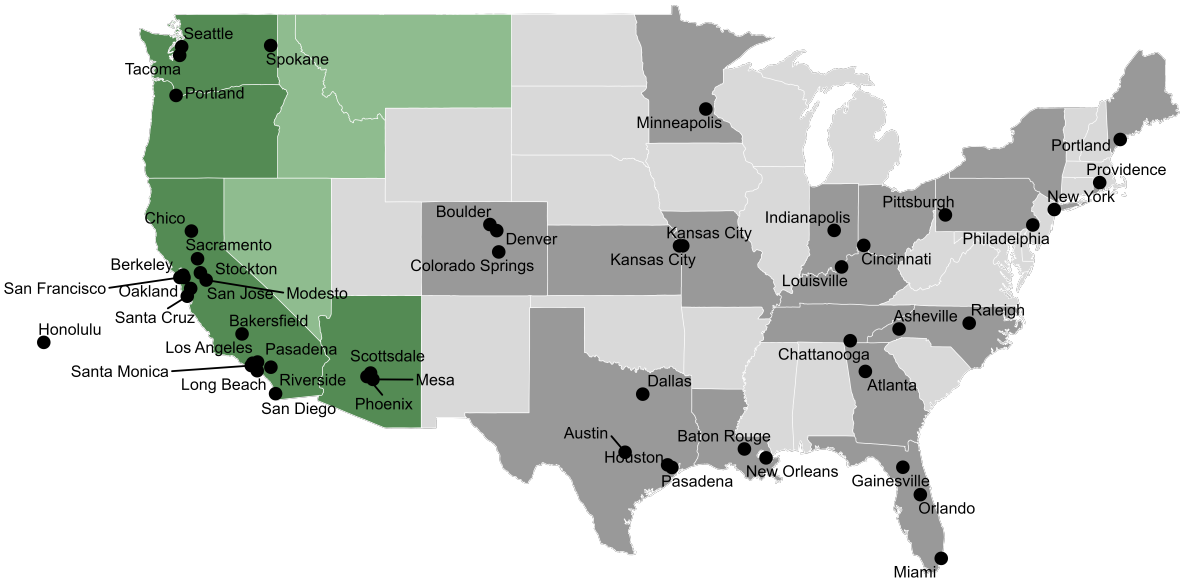


(a) *Martin* (b) *GP (9th)*

Note: These figures display the SDID estimate plots for the Martin and GP (9th) decisions. The black arrow indicates the size of the treatment effect. Standard errors are provided in Table 6. Data is a balanced panel, derived from the annual PIT count, and includes all 233 CoCs at or above 100% ES capacity in 2018 (63 in the Ninth Circuit and 170 outside).

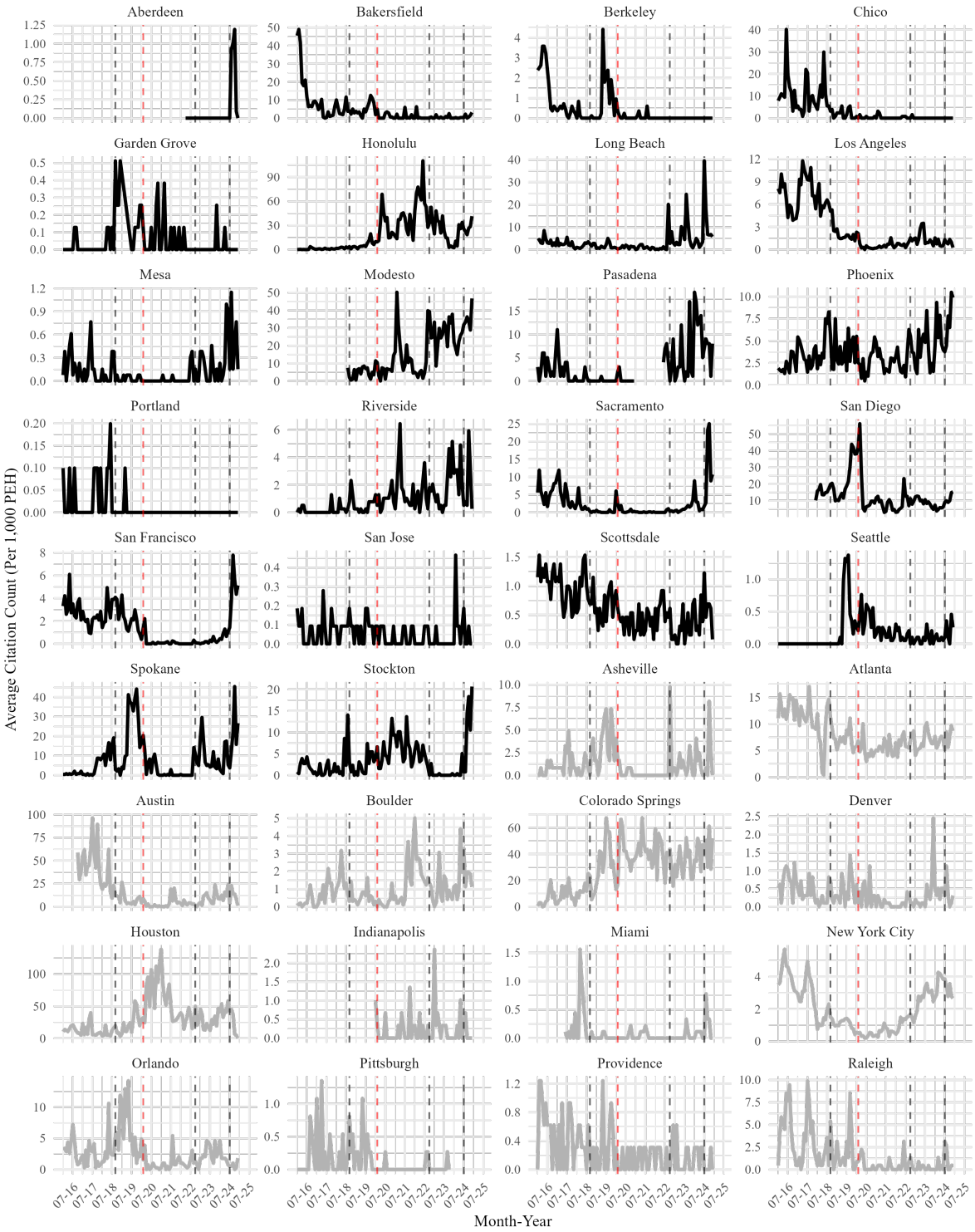
B City Data

Figure B1: Ninth Circuit Federal Court of Appeals Jurisdiction



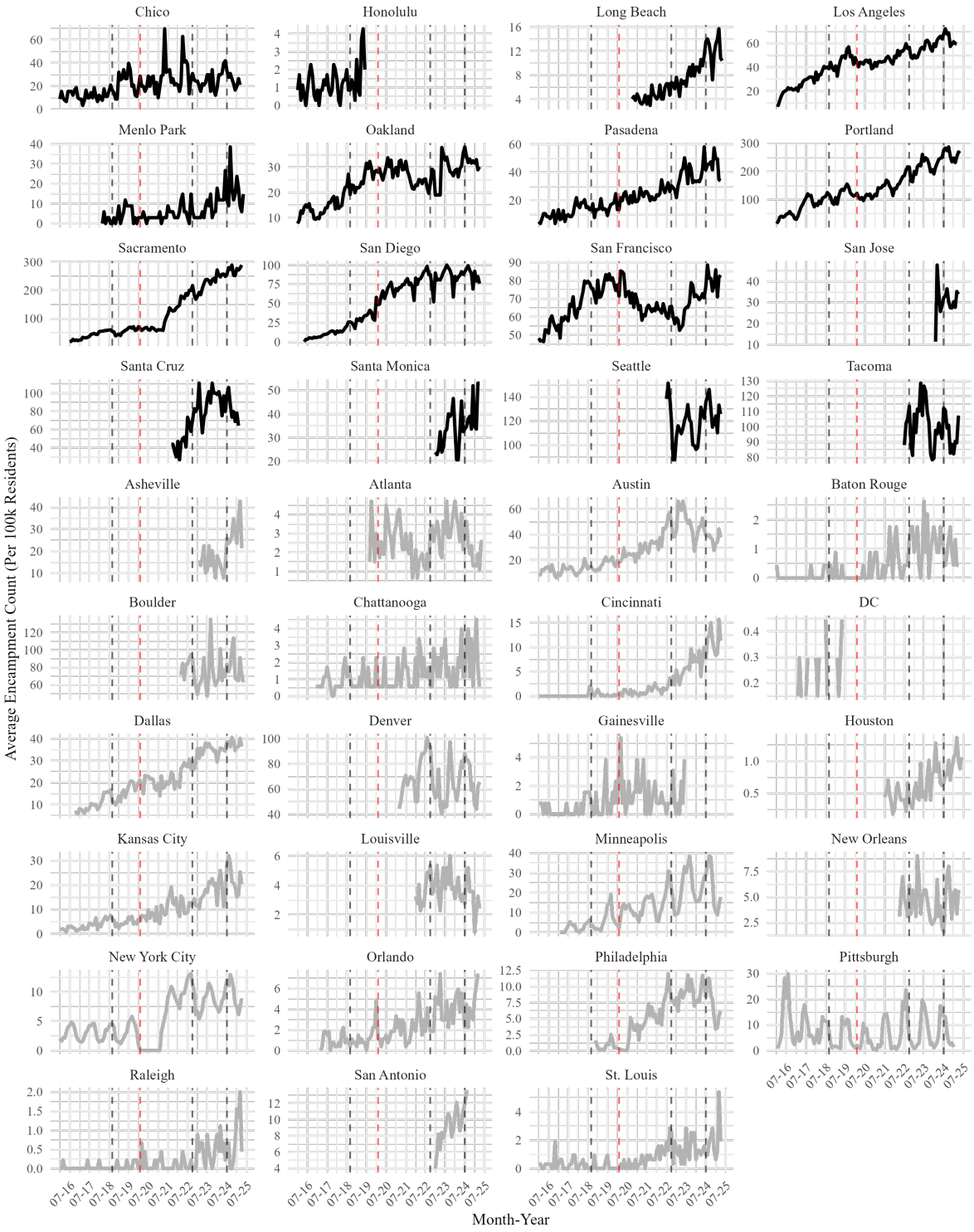
Note: This map highlights cities for which we have either citation or 311 data. Cities with citation data include: Asheville, Atlanta, Austin, Bakersfield, Berkeley, Boulder, Chico, Colorado Springs, Denver, Honolulu, Houston, Indianapolis, Long Beach, Los Angeles, Mesa, Miami, Modesto, New York City, Orlando, Pasadena, Phoenix, Pittsburgh, Portland, Providence, Raleigh, Riverside, Sacramento, San Diego, San Francisco, San Jose, Scottsdale, Spokane, Stockton. Cities with 311 data include: Asheville, Atlanta, Austin, Baton Rouge, Boulder, Chattanooga, Chico, Cincinnati, Dallas, Denver, Gainesville, Honolulu, Houston, Kansas City, Long Beach, Los Angeles, Louisville, Menlo Park, Minneapolis, New Orleans, New York City, Oakland, Orlando, Pasadena, Philadelphia, Pittsburgh, Portland, Raleigh, Sacramento, San Diego, San Francisco, Santa Cruz, Santa Monica, Seattle, St. Louis, Tacoma.

Figure B2: Monthly Anti-Sleeping and Anti-Camping Citations per 1,000 PEH



Note: This figure plots the count of anti-sleeping and anti-camping citations across cities in the Ninth Circuit and in the rest of the country, scaled by the average unsheltered homeless population from 2016-2018.

Figure B3: Monthly Average Unique Encampment Count per 100k Population



Note: This figure plots the count of "unique encampment regions" across cities in the Ninth Circuit and in the rest of the country, scaled by the population of each city in 2018.

C PRR Response Rates

Table C1: Citations Data Public Records Responses by City and Court Decision

City	State	9th Cir.	Full Panel	MvB (9th)	Grants Pass (9th)	Grants Pass (SC)
Aberdeen	WA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Albuquerque	NM	0	Did not receive	Did not receive	Did not receive	Did not receive
Anaheim	CA	1	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Anchorage	AK	1	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Asheville	NC	0	In sample	In sample	In sample	In sample
Atlanta	GA	0	In sample	In sample	In sample	In sample
Austin	TX	0	In sample	In sample	In sample	In sample
Bakersfield	CA	1	In sample	In sample	In sample	In sample
Baltimore	MD	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Baton Rouge	LA	0	Did not receive	Did not receive	Did not receive	Did not receive
Berkeley	CA	1	In sample	In sample	In sample	In sample
Birmingham	AL	0	No, rejected	No, rejected	No, rejected	No, rejected
Boise	ID	1	No, rejected	No, rejected	No, rejected	No, rejected
Boston	MA	0	No, rejected	No, rejected	No, rejected	No, rejected
Boulder	CO	0	In sample	In sample	In sample	In sample
Charlotte	NC	0	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Chattanooga	TN	0	No, rejected	No, rejected	No, rejected	No, rejected
Chicago	IL	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Chico	CA	1	In sample	In sample	In sample	In sample
Cincinnati	OH	0	No, rejected	No, rejected	No, rejected	No, rejected
Colorado Springs	CO	0	In sample	In sample	In sample	In sample
Dallas	TX	0	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Denver	CO	0	In sample	In sample	In sample	In sample
Detroit	MI	0	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Filmore	CA	1	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Fresno	CA	1	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Gainesville	FL	0	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Garden Grove	CA	1	No, data received but incomplete	No, data received but incomplete	In sample	In sample
Honolulu	HI	1	In sample	In sample	In sample	In sample
Houston	TX	0	In sample	In sample	In sample	In sample
Irvine	CA	1	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Indianapolis	IN	0	No, data received but incomplete	No, data received but incomplete	In sample	In sample
Jersey City	NJ	0	No, did not receive	No, did not receive	No, did not receive	No, did not receive

Table continued on next page.

City	State	9th Cir.	Full Panel	MvB (9th)	Grants Pass (9th)	Grants Pass (SC)
Kansas City	MO	0	No, rejected	No, rejected	No, rejected	No, rejected
Las Vegas	NV	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Long Beach	CA	1	In sample	In sample	In sample	In sample
Los Angeles	CA	1	In sample	In sample	In sample	In sample
Louisville	KY	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Menlo Park	CA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Mesa	AZ	1	In sample	In sample	In sample	In sample
Miami	FL	0	In sample	In sample	In sample	In sample
Minneapolis	MN	0	Rejected	Rejected	Rejected	Rejected
Modesto	CA	1	No, data received but incomplete	No, data received but incomplete	In sample	In sample
Murrieta	CA	1	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Nashville	TN	0	No, rejected	No, rejected	No, rejected	No, rejected
New Orleans	LA	0	No, rejected	No, rejected	No, rejected	No, rejected
New York City	NY	0	In sample	In sample	In sample	In sample
Oakland	CA	1	No, rejected	No, rejected	No, rejected	No, rejected
Orlando	FL	0	In sample	In sample	In sample	In sample
Pasadena	CA	1	No, data received but incomplete	In sample	No, data received but incomplete	In sample
Philadelphia	PA	0	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Phoenix	AZ	1	In sample	In sample	In sample	In sample
Pittsburgh	PA	0	No, data received but incomplete	In sample	In sample	No, data received but incomplete
Portland	OR	1	In sample	In sample	In sample	In sample
Providence	RI	0	In sample	In sample	In sample	In sample
Raleigh	NC	0	In sample	In sample	In sample	In sample
Riverside	CA	1	In sample	In sample	In sample	In sample
Sacramento	CA	1	In sample	In sample	In sample	In sample
San Diego	CA	1	In sample	In sample	In sample	In sample
San Francisco	CA	1	In sample	In sample	In sample	In sample
San Jose	CA	1	In sample	In sample	In sample	In sample
Santa Cruz	CA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Santa Monica	CA	1	No, rejected	No, rejected	No, rejected	No, rejected
Scottsdale	AZ	1	In sample	In sample	In sample	In sample
Seattle	WA	1	No, data received but incomplete	No, data received but incomplete	In sample	No, data received but incomplete
Spokane	WA	1	In sample	In sample	In sample	In sample
St. Louis	MO	0	No, rejected	No, rejected	No, rejected	No, rejected
Stockton	CA	1	In sample	In sample	In sample	In sample
Tacoma	WA	1	No, did not receive	No, did not receive	No, did not receive	No, did not receive

Table continued on next page.

City	State	9th Cir.	Full Panel	MvB (9th)	Grants Pass (9th)	Grants Pass (SC)
Thousand Oaks	CA	1	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Tucson	AZ	1	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Tulsa	OK	0	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Washington	DC	0	No, did not receive	No, did not receive	No, did not receive	No, did not receive

Note: "Incomplete" data indicates that the dataset was missing relevant variables or years, or that report labels changed definitions over time in ways that could not be reconciled.

Table C2: 311 Data Public Records Responses by City and Court Decision

City	State	9th Cir.	Full Panel	MvB (9th)	Grants Pass (9th)	Grants Pass (SC)
Aberdeen	WA	1	Does not appear to have 311	Does not appear to have 311	Does not appear to have 311	Does not appear to have 311
Albuquerque	NM	0	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Anaheim	CA	1	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Anchorage	AK	1	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Asheville	NC	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	In sample
Atlanta	GA	0	No, data received but incomplete	No, data received but incomplete	In sample	In sample
Austin	TX	0	In sample	In sample	In sample	In sample
Bakersfield	CA	1	Did not receive	Did not receive	Did not receive	Did not receive
Baltimore	MD	0	No, did not receive	No, did not receive	No, did not receive	No, did not receive
Baton Rouge	LA	0	In sample	In sample	In sample	In sample
Berkeley	CA	1	In sample	In sample	In sample	In sample
Birmingham	AL	0	No, rejected	No, rejected	No, rejected	No, rejected
Boise	ID	1	Did not receive	Did not receive	Did not receive	Did not receive
Boston	MA	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Boulder	CO	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	In sample
Charlotte	NC	0	Did not receive	Did not receive	Did not receive	Did not receive
Chattanooga	TN	0	In sample	In sample	In sample	In sample
Chicago	IL	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Chico	CA	1	In sample	In sample	In sample	In sample
Cincinnati	OH	0	No, data received but incomplete	No, data received but incomplete	In sample	In sample
Colorado Springs	CO	0	Did not receive	Did not receive	Did not receive	Did not receive
Dallas	TX	0	In sample	In sample	In sample	In sample
Denver	CO	0	No, data received but incomplete	No, data received but incomplete	In sample	In sample
Detroit	MI	0	Did not receive	Did not receive	Did not receive	Did not receive
Filmore	CA	1	Does not appear to have 311	Does not appear to have 311	Does not appear to have 311	Does not appear to have 311
Fresno	CA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Gainesville	FL	0	No, data received but incomplete	In sample	In sample	No, only received limited data (up to 2023)
Garden Grove	CA	1	Did not receive	Did not receive	Did not receive	Did not receive
Honolulu	HI	1	No, data received but incomplete	In sample	No, data received but incomplete	No, data received but incomplete
Houston	TX	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	In sample
Irvine	CA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Indianapolis	IN	0	Did not receive	Did not receive	Did not receive	Did not receive
Jersey City	NJ	0	No, rejected	No, rejected	No, rejected	No, rejected

Table continued on next page.

City	State	9th Cir.	Full Panel	MvB (9th)	Grants Pass (9th)	Grants Pass (SC)
Kansas City	MO	0	In sample	In sample	In sample	In sample
Las Vegas	NV	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Long Beach	CA	1	No, data received but incomplete	No, data received but incomplete	In sample	In sample
Los Angeles	CA	1	In sample	In sample	In sample	In sample
Louisville	KY	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	In sample
Menlo Park	CA	1	No, data received but incomplete	No, data received but incomplete	In sample	In sample
Mesa	AZ	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Miami	FL	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Minneapolis	MN	0	In sample	In sample	In sample	In sample
Modesto	CA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Murrieta	CA	1	No, rejected	No, rejected	No, rejected	No, rejected
Nashville	TN	0	No, rejected	No, rejected	No, rejected	No, rejected
New Orleans	LA	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	In sample
New York City	NY	0	In sample	In sample	In sample	In sample
Oakland	CA	1	In sample	In sample	In sample	In sample
Orlando	FL	0	In sample	In sample	In sample	In sample
Pasadena	CA	1	In sample	In sample	In sample	In sample
Philadelphia	PA	0	No, data received but incomplete	No, data received but incomplete	In sample	In sample
Phoenix	AZ	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Pittsburgh	PA	0	In sample	In sample	In sample	In sample
Portland	OR	1	In sample	In sample	In sample	In sample
Providence	RI	0	Did not receive	Did not receive	Did not receive	Did not receive
Raleigh	NC	0	In sample	In sample	In sample	In sample
Riverside	CA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Sacramento	CA	1	In sample	In sample	In sample	In sample
San Diego	CA	1	In sample	In sample	In sample	In sample
San Francisco	CA	1	In sample	In sample	In sample	In sample
San Jose	CA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Santa Cruz	CA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	In sample
Santa Monica	CA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	In sample
Scottsdale	AZ	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Seattle	WA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	In sample
Spokane	WA	1	Did not receive	Did not receive	Did not receive	Did not receive
St. Louis	MO	0	In sample	In sample	In sample	In sample
Stockton	CA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Tacoma	WA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	In sample

Table continued on next page.

City	State	9th Cir.	Full Panel	MvB (9th)	Grants Pass (9th)	Grants Pass (SC)
Thousand Oaks	CA	1	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Tucson	AZ	1	Did not receive	Did not receive	Did not receive	Did not receive
Tulsa	OK	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete
Washington	DC	0	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete	No, data received but incomplete

Note: "Incomplete" data indicates that the dataset was missing relevant variables or years, or that report labels changed definitions over time in ways that could not be reconciled.

D Ordinance Coding

Note: Cities either categorized citations by municipal code, a description of the violation, or both. Where a city only provided one, the other was inferred from the municipal code. Asterisks (*) indicate entries where this inference was made.

Table D1: Ordinance Harmonization for Citation Measure

City	Ordinance Codes	Violation Description in Data
Aberdeen, WA	12.46*, 36-63	Camping on City Street/Public rights-of way; Camp Public Place
Asheville, NC	11-16*, 16-2, 11-16, 12-51	Sleep in public place; Obstruct pedestrian sidewalk; Trespass or sleep in park; Camping violations
Atlanta, GA	106-81*, 106-12, 106-1	Physical obstruction; Urban camping; Loitering around railroad tracks
Austin, TX	9-4-14*, 8-1-11, 8-1-12, 14-9-1	Sitting or lying on sidewalk; Park curfew violation; Camping in public place; Obstruction in DACC area
Bakersfield, CA	9.20.020(B), 9.70.010(A), 9.70.010(B), 647(E)	Lodging or sleeping in public area or vehicle; Camp on public property; Sleep outdoors on public property; Lodge without owner consent
Berkeley, CA	6.32.020, 13.36.010, 6.20.260, 13.36.015, 13.36.020	Park curfew; Sit/lie prohibition; Overnight camping in marina; Accessibility on sidewalks; Obstruction of entrances
Boulder, CO	8-3-21*, 5-6-10	Tents and nets prohibited; Camping/lodging on public property without consent
Chico, CA	9.20.030, 12R.04.340, 9.44.015, 9.50.030(B), 12R.04.370, 9.50.030(C), 9.20.050, 12.18.430, 9.44.018	Camping unlawful; Public nuisance; Sit-lie; Waterway camping; Park closure; Storage in waterways; Obstruct entrance; Other CMC violations
Colorado Springs, CO	9.6.111*, 9.9.404, 9.6.110, 9.2.104	Camping within public stream; Camping restrictions in parks; Camping on public property; Obstructing passage or assembly
Denver, CO	38-86	Obstruction of streets or public passageways*

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City	Ordinance Codes	Violation Description in Data
Honolulu, HI	RO 13-15, 13-16, 13-20, 13-21, 13-18	Prohibits sit/lie on sidewalks; Nuisances and obstructions on sidewalks; Illegal lodging; Time restrictions for sidewalk use
Houston, TX	§43.352*, §43.361, §21-62, §40.362, §32.28	Sit/lie on sidewalk during restricted hours; Place obstruction; Encamp in public after warning; Obstruct sidewalk; Camping in park or public place
Long Beach, CA	LBMC16.16.010, 647(E)PC, LBMC9.42.110	Camping in parks/beaches; Lodging without consent; Sleeping/camping in restricted areas
Los Angeles, CA	41.18, 56.11	Prohibits sitting, lying, or sleeping on sidewalks; Storage of property in public areas
Miami, FL	54-2(C), 37-16, 37-3, 37-4	Obstruction of sidewalk; Encampments on public property; Sleeping in streets/sidewalks; Living or sleeping in vehicles
New York, NY	56RCNY1-04P, 751.04, 1-03(A)(3), 16-122(B), 24RCNY6-04	Unauthorized camping; Unauthorized park presence; Street obstruction; Mobile dwelling use; Obstruction of benches/sidewalks; Loitering; Camping without permit
Orlando, FL	§43.52, §43.88	Camping prohibited; Sitting/lying on downtown sidewalks prohibited
Phoenix, AZ	23-9_1M, 23-8B_1M, 23-4801_1M, 23-30A_1M, 23-8A_1M	Obstructing public areas; Loitering in closed park; Lying or sitting on right-of-way; Camping on city property; Loitering; Camping in prohibited location
Pittsburgh, PA	473.07(a), 419.01, 541.04	Park hours restrictions; Obstruction of streets or sidewalks
Riverside, CA	9.04.600, 9.11.030, 9.04.400, 9.11.060	Camping on sidewalks*; Obstruction sit/lie; Unauthorized encampments
Sacramento, CA	12.52.030(A), 12.72.090, 12.74.030(A), 8.140.030(A), 12.24.100	Unlawful camping; Loitering in park; Sit/lie restrictions; Camping near infrastructure; Sidewalk obstruction

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City	Ordinance Codes	Violation Description in Data
San Diego, CA	54.0110, 647(E), 86.0137(F), 63.20.11, 63.0102B12	Unauthorized encroachments; Lodging without consent; Vehicle habitation; Sleeping on beach; Camping on park/beach
San Francisco, CA	Police Code Sec. 168*, Penal Code 647, MC 22, MC 23	Civil sidewalks; Lodging without permission; Obstructions on streets; Obstructing public thoroughfare
Scottsdale, AZ	13-2917A1*, 20-31B12, 19-21B, 19-4, 20-132C4	Public nuisance—obstruction; Park closed; Camping without permit; Obstructing right of way; Camping in flood areas
Seattle, WA	12A.12.015, 18.12.279(A), 18.12.070(A), 15.46.010	Pedestrian interference; Trespass in parks; Entering restricted areas; Failure to remove obstruction or debris
Spokane, WA	12.02.1010, 12.02.1010.A3.C, 12.02.1010.A3.A, 12.02.1003.A.1	Unlawful camping on public property; Camping near parks/schools; Camping under viaducts; Waterway obstruction; Camping near shelters
Stockton, CA	12.56.020, PC647e, MC9.64.030, MC9.64.150	Park hours; Lodging without permission; Sidewalk obstruction; Impeding sidewalk access