Eviction Reduction Policies

Summary Version (10 pages)

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Eviction is a leading cause of housing instability in California cities. Yet little empirical work quantifies which policies causally effect evictions. Using data on Los Angeles County eviction filings, this paper examines how court assignment policies impact eviction outcomes. By exploiting spatial and time variation in policies, the paper quantifies the extent to which court assignment and court expansion influence eviction.

This summary version of the paper is organized as follows. Section 1 explains the institutional background of the LA County eviction system and estimates the correlation between distance-to-court and eviction default probability. Section 2 reviews the findings of Estes and Nelson (2025): regression discontinuity estimates of the effect of court assignment on default probability and money judgments. Section 3 studies the impact of expanding the number of courthouses in August 2017 on the number of defaults. Finally, Section 4 discusses implications for eviction policy.

1 Background

According to LA County court rules,[1] eviction cases are assigned to courthouses based on a unique[2] spatial mechanism. Table 1 below illustrates the courthouse assignment procedure for the first three zip codes in LA County. In the 90001 zip code, for example, eviction cases are assigned to the Norwalk or Stanley Mosk courthouse depending on which neighborhood the location of the tenant's apartment.

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[1]LASC Local Rule 2.3(a)(2).

[2]Unique because the assignment rule applies ***only*** for unlawful detainer (i.e. eviction) cases. The assignment rule is different for other civil cases (e.g. small claims, unlimited).

Table 1. Zip Code Table for Eviction Cases

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

The spatial assignment mechanism represented by Table 1 is displayed in the LA County map in the left panel of Figure 1. The right panel of Figure 1 shows how the number of eviction courthouses varies over time, which we exploit in Section 3.

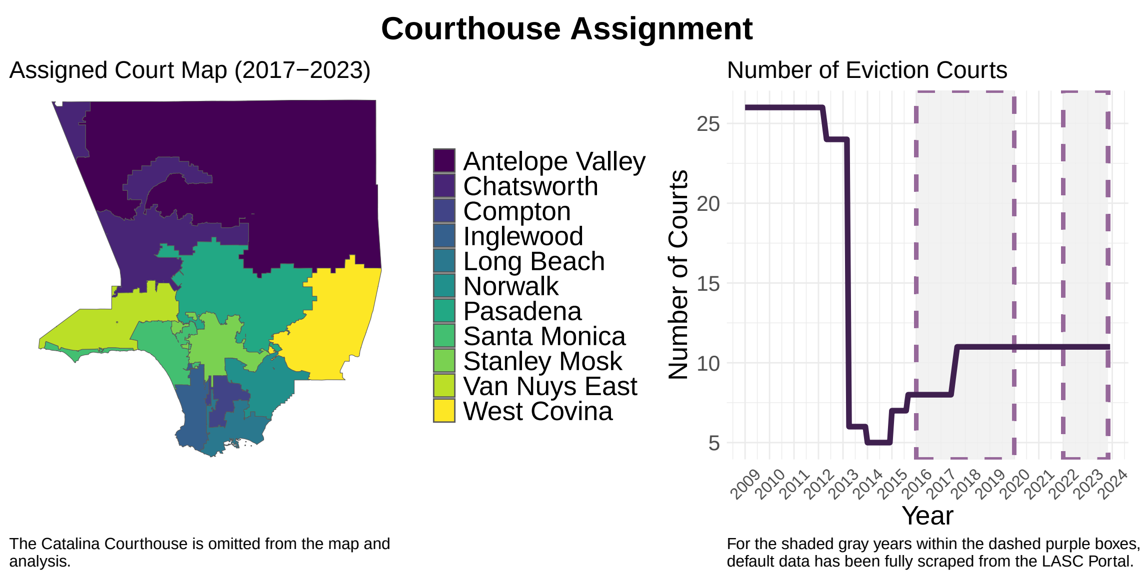


Figure 1. LA County Eviction Court Assignment

Note: The assignment map (Late 2017–2023) is shown in the left panel. The number of courthouse districts expanded in late 2017, which is shown in the right panel. See also Estes and Nelson (2025) and Nelson (2023) for further discussion.

Because there is large variation in how far tenants must travel to court, tenants across the boundary have different court travel "costs." Differences in such travel costs effect the eviction default probability (Hoffman and Strezhnev, 2023). Using

LA Assessor data on the subset of rental apartment buildings, I estimate the average default probability as a function of distance-to-court. The outcome variable is default_{it}, which encodes whether we observe a default eviction for apartment unit i for a given year t. The years are pooled to estimate the conditional expectation $\mathbb{E}[\text{default}_{it}|D_{it} = d]$ for different distance-to-court values d.

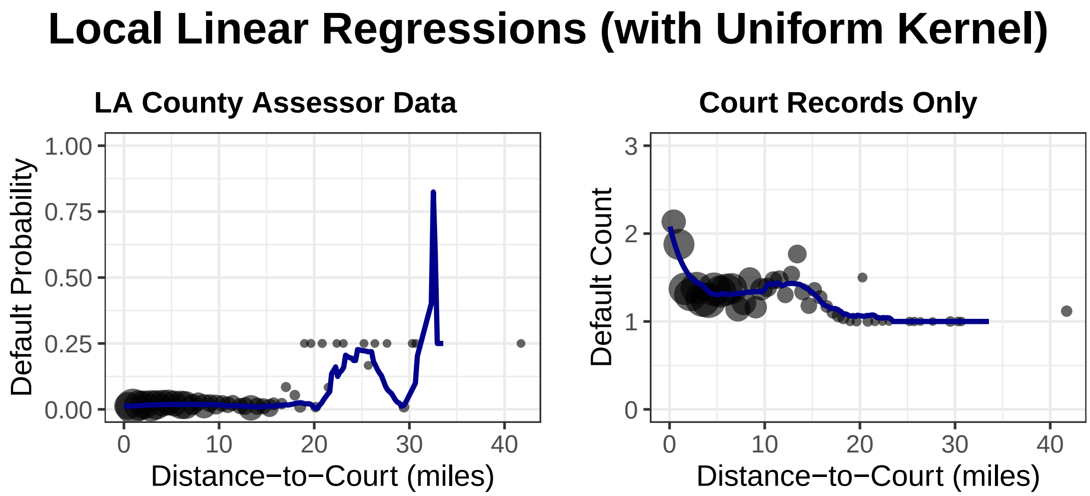


Figure 2. Correlational Relationship: Distance-to-Court and Default Probability

Note: The estimates are local linear regression estimates of the average default probability using a 5km bandwidth and uniform kernel weighting. Equally-spaced binned sample averages are plotted as points in 1km wide bins, with point volumes proportional to the number of observations within the bin.

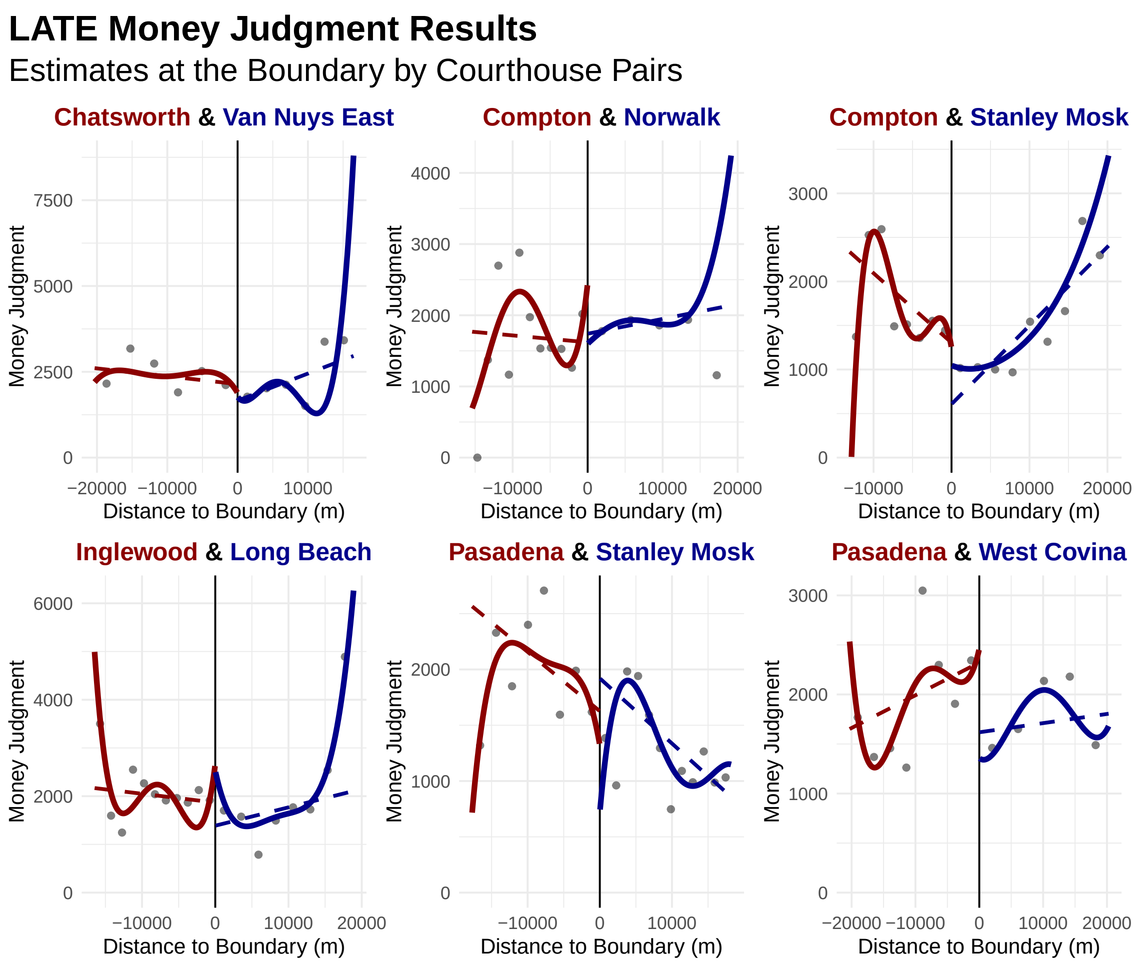
In Figure 2, I plot non-parametric estimates of the reduced-form relationship (correlation) between distance to court and default probability. The estimates show that the average default probability is small for cases close to court (i.e. less than 30km). For distances above 30km, the average default probability rises, although note that there are fewer observations at longer distances.

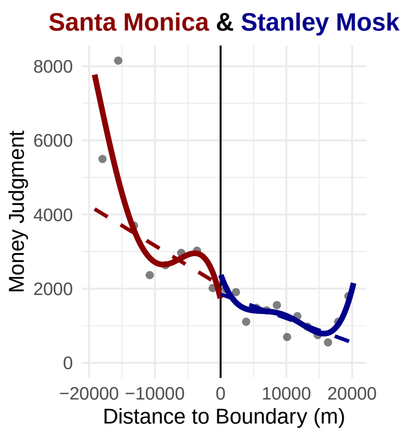
2 Court Assignment Policy: RDD Estimates

The first empirical study focuses on eviction court assignment policy in LA County. Estes and Nelson (2025) use the spatial mechanism (Table 1) within a regression discontinuity design to compare eviction defaults near the boundary of courthouse districts. The intuition is that this comparison is quasi-experimental because nearby renters have "similar" observable and unobservable characteristics. Estes and Nelson

(2025) estimates the causal effect of court assignment on default probability is between 0.7–23.1 percentage points for seven courthouse pairs. After conditioning on distance-to-court and comparing cases near the boundary, Estes and Nelson (2025) find insignificant differences in defaults between cases assigned to courthouse A versus cases assigned to courthouse B. In other words, the data is consistent with the mechanism determining default probability being distance-to-court.

Here, I expand the regression discontinuity analysis to include new data on another outcome variable: default money judgments. Using data on defaulting tenants only, I estimate the effect of courthouse assignment on the monetary judgments defaulting tenants owe landlords. The results are shown in Figure 3. The robust confidence intervals suggest the magnitude (absolute value) of the local average treatment effect (LATE) is around 1-2 months rent for all seven courthouse pairs.





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

Figure 3. LA County Money Judgment RDD

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

3 Court Expansion Policy: DID Estimates

In the second empirical study, which is the focus of the JMP, I examine how changes in the number of eviction courts over time impacts the number of evictions. As shown in Figure 4, the number of courts hearing eviction actions increased over time from seven courthouses (2015) to eight courthouses (2016–Early 2017) to 11 courthouses (Late 2017–2023). This expansion resulted in many—though not all—tenants experiencing a decrease in their distance-to-court. Because distance-to-court is a tenant cost, defaults should, on average, decrease (resp. increase) where tenant distance-to-court decreased (resp. increased).

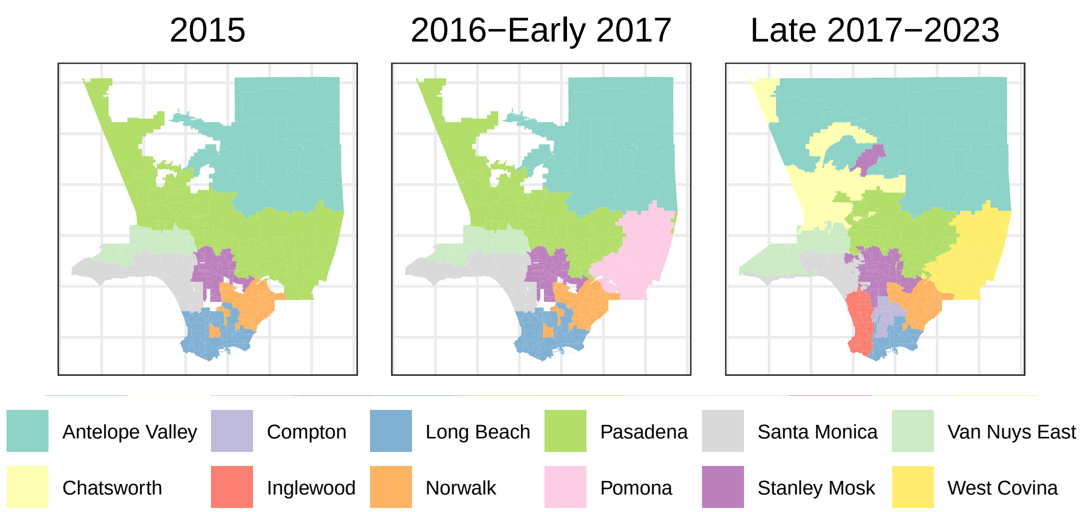


Figure 4. LA County Courthouse Expansion

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

I apply the difference-in-differences (DID) strategy to the court expansion policy. This approach assumes that units with changes in distance-to-court would've experienced "parallel" outcomes to units with no distance-to-court changes. I consider three treatment cohorts: the control units (zero change in distance-to-court), the increase-treated units (positive change in distance-to-court), and the decrease-treated units (negative change in distance-to-court). The treatment effect of interest is the average treatment effect on the treated (ATT).[3]

I estimate how expanding the number of eviction courts in August 2017 impacted the number of default evictions. The dynamic DID estimates of the ATT are shown

[3] The increase-treated ATT is, for example, the average difference in number of defaults between the increase-treated units versus control units.

in Figure 5. The left panel of Figure 5 compares the increase-treated units—i.e. addresses that experienced an increase in distance-to-court—with control units as the comparison group. This method uses control units to impute counterfactual outcomes for the increase-treated units in the counterfactual world where increase-treated units do not experience an increase in distance-to-court. Similarly, the right panel of Figure 5 compares decrease-treated units with the control cohort.

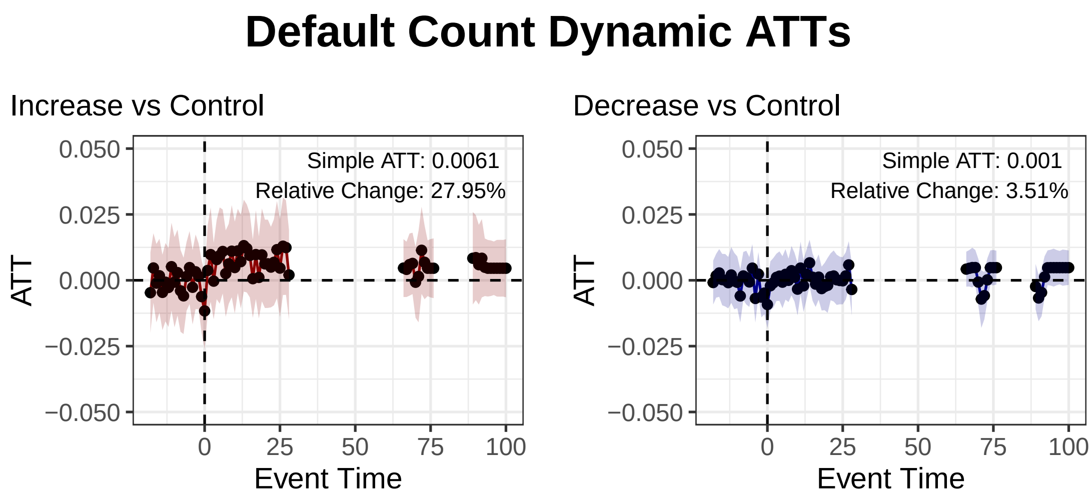


Figure 5. All Comparisons with Control Group

Note: The dynamic ATTs (and SEs) for each period are computed as in Callaway and Sant'Anna (2021). The simple ATT aggregates the ATTs in all post-treatment (vertical dashed line) time periods, whereas the relative change is the simple ATT as a percentage of the pre-treatment average default count for the treated cohort. The left (resp. right) panel compares the increase-treated (resp. decrease-treated) cohort to the control cohort.

The point estimates after the court expansion policy (vertical dashed line) are mostly positive for both comparisons. This means that units with an increased (or decreased) distance-to-court experienced greater default counts on average than units with zero change in distance-to-court following the court expansion policy. However, note that the confidence band includes the zero effect for most time periods.[4]

Note also that the aggregate ATTs are different in magnitude. The estimates imply that average number of defaults across post-treatment periods for increase-treated units is 27.95% larger than the average number of defaults across pre-treatment periods due to the increased distance-to-court treatment. By contrast, the average number of defaults across post-treatment periods for decrease-treated units is only 3.51%

[4]The zero effect is represented by the dashed horizontal line at $ATT = 0$.

larger than the average number of defaults across pre-treatment periods because of the decreased distance-to-court treatment.

Finally, I repeat the exercise using only units that experienced large changes in distance-to-court. In Figure 6, I compare units with above-median increases in distance-to-court (left panel) and above-median decreases in distance-to-court with control units. In this type of comparison, units with large increases in distance-to-court experience a positive aggregate treatment effect (Simple ATT $= 0.0012$) whereas units with large decreases in distance-to-court have a negative aggregate effect (Simple ATT $= -0.00018$). The results for these comparisons better align with the predicted effect but note the increased uncertainty.[5]

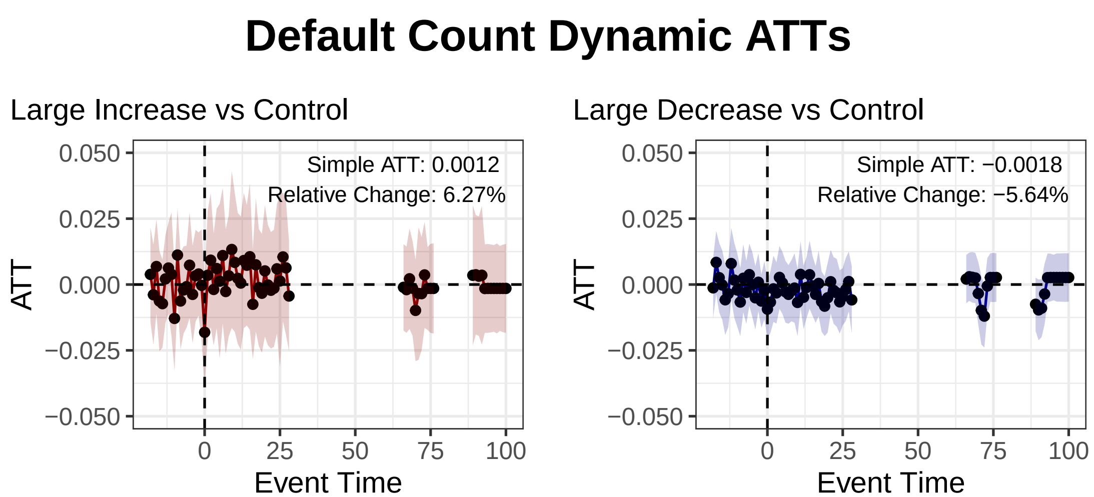


Figure 6. Above-Median Comparisons with Control Group

Note: The dynamic ATTs (and SEs) for each period are computed as in Callaway and Sant'Anna (2021). The simple ATT aggregates the ATTs in all post-treatment (vertical dashed line) time periods, whereas the relative change is the simple ATT as a percentage of the pre-treatment average default count for the treated cohort. The left (resp. right) panel compares the above-median increase-treated (resp. above-median decrease-treated) cohort to the control cohort.

In sum, I find limited evidence that court expansion significantly impacted the default eviction probability for 2016–Early 2017 units. Although the point estimates align with the predicted effect in the large increase and large decrease comparisons, there remains considerable uncertainty and the magnitude of the estimated aggregate effect is not large in relative or absolute terms.

[5]In other words, observe that the confidence bands are larger because fewer observations are being used.

4 Discussion

This paper offers new empirical evidence on how court policies affecting tenant court access shape eviction outcomes in LA County. The court assignment and court expansion analyses highlight the role that "access to justice" plays in determining tenant eviction outcomes. Reduced-form evidence suggests that tenants who face a high distance-to-court are less likely to appear and contest an eviction, whereas closer tenants are more likely to appear. But not all procedural reforms appear effective: the August 2017 court expansion did not significantly increase (resp. decrease) defaults for tenants with an increased (resp. decreased) distance-to-court.

The research designs help isolate the causal effects of differences in court access on tenant outcomes. While the magnitude of the effects varies across contexts, the results illustrate possible avenues for reform. Policy choices that reduce the burden of getting to court—whether by redrawing the assignment map, opening more courthouses to eviction cases, allowing virtual appearance options, or some combination—may improve outcomes in some cases but not others. In follow-up research, the findings may be used to study the aggregate effects of such counterfactual policies on tenant defaults.

More broadly, current eviction work is focused mostly on court-based procedural reforms to make the eviction process fairer for tenants. Because tenants often face a large resource disadvantage in eviction cases, procedural reforms aim to "level the playing field" for tenants in cases against better-resourced landlords.[6] Greiner et al. (2012, 2013) and Cassidy and Currie (2023), for example, exemplify this literature by examining how legal representation affects tenant outcomes in housing court.

Future research, by contrast, may seek to study the effectiveness of structural reforms at addressing the underlying causes of eviction. Because non-payment of rent is the primary reason for eviction, such reforms would tend to focus on reducing rent and housing prices. Because of the large impact of housing supply on prices, the goal of these reforms is (largely) to increase the available stock of rental housing.[7] In the future, researchers may examine how, e.g., zoning and land use regulation impact

[6]Procedural policy reforms might include, for example, allowing longer tenant response times, permitting remote court attendance, reducing filing fees, and providing publicly-funded tenant counsel. The last proposal is sometimes referred to as "civil *Gideon*", a reference to *Gideon v. Wainwright* (1963), the landmark Supreme Court decision requiring states to provide defense counsel to indigent criminal defendants.

[7]Structural reforms may include, e.g.: subsidizing the building of affordable housing units, building public housing, increasing housing vouchers, or changing income thresholds to receive rental assistance.

eviction outcomes.

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