Rules, Discretion, and Punishment: Evidence from Mandatory Minimum Repeal*

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Abstract

Sentencing laws shape not only punishment after conviction but also the bargaining process through which criminal cases are resolved. Mandatory minimums do so by raising the defendant's downside risk from trial and giving prosecutors a stronger bargaining position. We study this bargaining channel using detailed administrative court records from Arkansas, where a 2011 reform eliminated mandatory minimum penalties for drug manufacture/sale offenses. Exploiting the timing of the reform, we estimate reductions in charge severity, prison sentence length, and the probability of incarceration, with no detectable increase in subsequent criminal justice involvement or recorded drug-related activity. We then examine how prosecutors adapted after losing this statutory source of bargaining leverage. We find little evidence that prosecutors compensated for this loss by filing additional charges or pursuing more trials. Instead, drug manufacture/sale charges became less likely than co-charges on the same case to be dropped after repeal, while sentencing shifted toward probation. Overall guilty outcomes changed little. These patterns are consistent with lower, more graduated penalties making affected charges more viable to retain through disposition. Thus, repeal reduced punishment without detectable adverse downstream effects, while changing how prosecutors used discretion within cases and how punishment was structured.

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1 Introduction

State and local governments in the United States spent approximately $107 billion on incarceration in 2023 (Congressional Budget Office, 2025). Sentencing laws have large fiscal and social consequences, but they do more than set the punishment imposed after conviction. They also shape the discretionary decisions that come before sentencing. Prosecutors decide which charges to file, keep, and drop, and defendants and their counsel decide whether to bargain or go to trial. The vast majority of criminal cases end in plea agreements rather than trials. A change in the type or severity of punishment can therefore affect not only the final sentence but also the bargaining environment in which cases are resolved.

Mandatory minimums are a good example. A statutory floor raises the punishment a defendant faces if plea negotiations fail, which plausibly strengthens the prosecutor's bargaining position. Removing the floor weakens that threat, but what happens to case outcomes is not mechanical. Prosecutors may try to replace the lost leverage by filing additional charges, offering different charge concessions, or pursuing trials more aggressively. Or the previously severe charge may become easier to carry through to disposition once its statutory consequences are less harsh. Sentencing reform can therefore reduce punishment and change how prosecutors use discretion at the same time, and the net effect on how individual charges resolve is theoretically ambiguous.

We study this bargaining channel in Arkansas, where Act 570 repealed the mandatory minimum sentencing regime for drug manufacture/sale offenses in July 2011. The reform replaced the old statute with post-reform analog statutes that continued to cover related conduct but no longer carried the same mandatory sentencing floors. The setting has two advantages. First, the reform created a sharp break in the statutory environment governing drug cases. Second, Arkansas court records are detailed at the charge level: they include defendant and prosecutor identifiers, charge codes, dispositions, sentences, trial-type indicators, and plea classifications that separate negotiated guilty pleas from other guilty outcomes. We can therefore see how a change in statutory bargaining leverage played out inside individual cases.

The paper asks two linked questions. First, we examine how the reform changed final punishment and subsequent criminal justice involvement. We use a regression discontinuity in time (RDiT) design around the July 2011 implementation date, complemented by a difference-in-differences comparison with non-drug offenses having little overlap with drug cases. Second, we study how prosecutors respond when the old mandatory-minimum threat was removed. We examine standard adjustment margins highlighted in plea-bargaining models using differences in prosecutors' pre-reform exposure to mandatory-minimum cases, then compare affected drug manufacture/sale charges with other charges within the same case before and after repeal. These exercises identify different empirical objects: the RDiT estimates changes at the policy cutoff, while the within-case design identifies changes in relative charge resolution.

The policy results show substantial reductions in punishment. At implementation, maximum charge severity falls by 0.469 points on an eleven-point scale, and imposed prison sentences decline by 4.5 months per drug case, or approximately 12.5 percent of the pre-reform mean. The estimated incarceration probability falls by 2 percentage points (4.4 percent relative to its pre-reform mean). The complementary difference-in-differences estimates have similar magnitudes. We find

no detectable increase in subsequent criminal justice involvement or in police-recorded drug-related incidents or arrests; the RDiT estimate for initiation of a new case within three years is −2.2 percentage points. Large reductions in recorded punishment were thus not accompanied by the increases in these downstream outcomes that a simple deterrence or incapacitation account would predict, or that policymakers may worry about.

To understand how these punishment effects arose, we then examine how case resolution changed after this statutory source of bargaining leverage was removed. Before repeal, only 29.2 percent of charges coded as mandatory-minimum eligible resolved guilty on a case with a prison term at or above the charge's own coded floor. Among multi-charge cases with any guilty outcome, 19.5 percent had the mandatory-minimum charge dropped while another charge resolved guilty. One reason an unusually severe charge might be filed often but not carried through to final disposition is that the threat had bargaining value even when the charge was eventually dropped. We first test whether prosecutors responded to repeal by rebuilding leverage along margins predicted in the literature. We find little evidence of differential increases in charge stacking or charge dropping, reductions in prison exposure, or movement into trial among prosecutors who had been more exposed to mandatory-minimum charges before reform.

Instead, the clearest adjustment is within multi-charge cases, which are very common in our setting: over 70% of drug manufacture/sale cases contain at least one non-drug manufacture/sale charge. After repeal, drug manufacture/sale charges became less likely than their co-charges to be dropped, while formally negotiated guilty resolutions became more common. Probation-only sentences also became more common relative to co-charges, with no significant change in prison. This links the bargaining evidence to the policy effects: affected charges were dropped less often and became more likely to receive probation-only sentences.

The findings fit a simple tension. A severe charge can raise the value of the prosecutor's bargaining position, but carrying it to final disposition can also be costly, whether through litigation resources, institutional constraints, reduced flexibility, or reputation. Repeal reduces the value of the old threat and may lower the cost of retaining the analog charge, so the net change is ambiguous ex ante. In Arkansas, the evidence points less toward compensating charge stacking or trials and more toward retaining the affected charge and assigning it a less carceral sanction. This interpretation is consistent with the contemporaneous Arkansas Working Group's call for "punishments appropriate to the offense" (Arkansas Working Group on Sentencing and Corrections, 2011), specifically for drug charges, and with federal prosecutor and defense-attorney accounts describing mandatory-minimum enhancements as both exceptionally severe and useful in plea negotiations (United States Sentencing Commission, 2011). We cannot observe plea offers or prosecutors' objectives directly, so the charge-retention mechanism is an interpretation consistent with the evidence, not a separately identified parameter.

The paper makes three contributions. First, it evaluates a sentencing reform more broadly than most prior work, following a major change in statutory punishment through incarceration, subsequent court involvement, and recorded drug-related activity. Prior analyses of sentencing reforms focus on final sentencing and conviction outcomes, including the federal Fair Sentencing Act and other reforms (Fischman and Schanzenbach, 2012; Bjerk, 2017a; Craigie and Zapryanova, 2021;

Tuttle, 2026); related evidence documents sentences below coded minimums and the punishment consequences of mandatory-minimum eligibility (Bjerk, 2017b; Cooper, 2026).

Second, we observe which charges are retained or dropped within individual criminal cases, rather than inferring adjustment only from final sentences. Research documents substantial prosecutor heterogeneity and the importance of discretionary charging for case outcomes and disparities (Bjerk, 2005; Naddeo, 2022; Sloan, 2026; Agan et al., 2023; Shaffer, 2023; Shaffer and Harrington, 2026). We study how those discretionary choices respond to a change in statutory bargaining leverage. Our charge-level evidence complements work on plea-bargaining incentives and the consequences of mandatory minimums (Landes, 1971; Bebchuk, 1984; Reinganum, 1988; Abrams, 2011; Silveira, 2017; Naddeo, 2022; Cooper and Yuan, 2026).

Third, the results show how formal legal rules interact with the behavior of the people who implement them. Repeal changed statutory penalties, but observed case outcomes also depended on which charges prosecutors kept pursuing and how those charges were resolved. This links sentencing reform to research showing that drug policy can change enforcement and charging behavior (Soliman, 2022; Jordan, 2026). We do not claim that repealing mandatory minimums always induces charge retention or reduces subsequent involvement. The point is that the consequences of a legal rule depend partly on how discretion is exercised once the rule changes.

2 Background, Data, and Empirical Framework

2.1 Arkansas Act 570 and the Charging Environment

Arkansas Act 570, signed in March 2011 and effective in July 2011, repealed Arkansas Code §5-64-401, which had imposed mandatory minimum sentences for drug manufacture/sale offenses. Before repeal, §5-64-401 attached statutory sentence floors that varied by drug type and quantity. After repeal, prosecutors continued to charge closely related conduct under §5-64-420 and related provisions, but those post-reform analog charges no longer carried the same mandatory sentencing floors. Instead, Act 570 replaced offense-specific drug penalties with a quantity-based classification system that assigned offenses to Arkansas's standard felony classes. Although Act 570 was part of a broader criminal justice reform package, the analysis focuses on the drug-sentencing provisions that eliminated these statutory floors and created the post-reform analog charge structure.

Appendix Tables B.1 and B.2 summarize the statutory severity and core charge categories used in the analysis. The severity ranking shows how much sentencing leverage attached to the affected drug charges before reform. First- and second-degree murder occupy seriousness ranks 10 and 9, respectively, while the most serious mandatory-minimum manufacture, delivery, and possession-with-intent-to-deliver offenses rank 8; other affected drug offenses rank 7, above manslaughter at rank 6. The mandatory-minimum drug charges studied here therefore sat near the top of Arkansas's statutory seriousness scale. The main treated charge universe is old-regime drug manufacture/sale conduct under §5-64-401(a) and §5-64-401(b), together with the post-reform analog statutes that replaced those charges. Simple possession is coded separately because possession cases did not have pre-reform mandatory-minimum exposure and play a different role in the charging-composition analysis.

Figure 1 documents the statutory first stage using the share of drug cases containing each type of charge. Panel A shows that before reform, roughly 40 to 45 percent of drug cases contained an old mandatory-minimum drug manufacture/sale charge, and that this share falls essentially to zero at implementation. Panel B shows the post-reform analog charges: at the same cutoff, a similarly large share of drug cases begins to contain an analog drug manufacture/sale charge. The transition shows that the reform removed the mandatory-minimum sentencing floor without eliminating the underlying drug manufacture/sale charge category from court records. Appendix Figure A.1 shows the same transition in monthly charge counts.

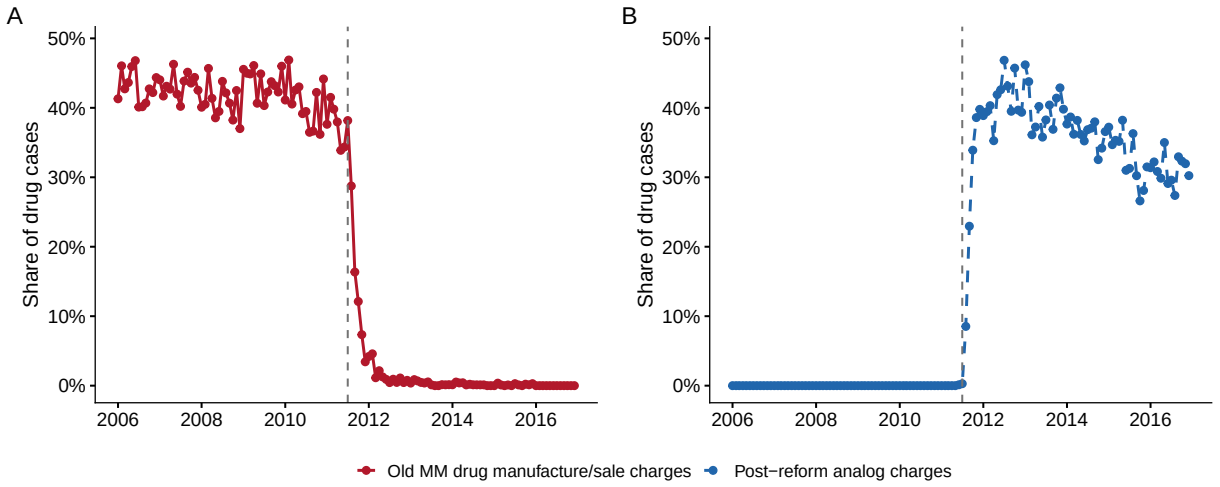


Figure 1: Statutory first stage: share of drug cases with drug manufacture/sale charges

Notes: Panel A plots the monthly share of drug cases containing at least one charge under the old mandatory-minimum drug manufacture/sale statute. Panel B plots the share containing at least one post-reform analog charge. The denominator is all cases with at least one drug charge, by filing month. The dashed vertical line marks the July 2011 implementation of Act 570.

2.2 Conceptual Framework: Sentencing Floors and Bargaining Leverage

Mandatory minimums can affect criminal cases through two channels. First, they impose a sentencing floor after conviction. This direct channel implies that repealing a mandatory minimum should reduce expected punishment for affected charges, unless prosecutors offset the change through other charging or bargaining margins. Second, mandatory minimums impact bargaining before conviction. Plea bargaining occurs in the shadow of trial. A mandatory minimum raises the defendant's expected cost of losing at trial and gives the prosecutor a more severe outside option. Removing the mandatory minimum therefore weakens one source of prosecutorial leverage.

A simple way to organize the prosecutor's incentives is

$$U_p = V(\text{conviction}, \text{punishment}) - C(\text{resources}) - K(\text{carrying a severe charge}), \tag{1}$$

where V summarizes the value of case outcomes, C the resources required to resolve a case, and K other costs associated with carrying a severe charge through disposition. This is an organizing

framework, not a structural model that assigns a particular motive to K. Prosecutors value case resolution, convictions, and punishment, but aggressive charging and trial threats can impose costs. They require resources, may face evidentiary or legal constraints, and may carry institutional or reputation costs with judges, defense counsel, or voters. So a very severe mandatory-minimum charge may be valuable as leverage but also costly to carry through to final disposition. Repeal has two opposing implications: it reduces the leverage attached to the affected charge, but it also lowers the cost of retaining that charge through disposition because the associated punishment is less severe. The effect on observed case outcomes is therefore theoretically ambiguous. Guilty outcomes may fall if defendants become less willing to plead. Sentence lengths may fall if prosecutors cannot replace the statutory threat. Prosecutors may substitute toward other leverage tools, such as charge stacking, charge dropping, or trial threats. Or, if the analog charge becomes more institutionally viable as a final resolving charge, affected charges may be retained more often while punishment becomes less carceral.

Canonical plea-bargaining models typically compress bargaining into a prosecutor offer and a defendant acceptance decision. In these models, mandatory minimum repeal changes the defendant's expected sentence at trial and therefore has implications for conviction rates, sentence lengths, charge concessions, and plea/trial decisions. But criminal cases often contain multiple charges. In multi-charge cases, the reform can also change which charge remains the charge of conviction and what sentence follows from that charge. This charge-resolution margin is central to our setting because the old mandatory-minimum charge could serve as a bargaining threat even when it was later dismissed or replaced by another resolving charge.

Appendix Table B.3 summarizes the response margins and corresponding empirical tests. The standard margins are those emphasized by plea-bargaining models: conviction and trial rates, sentence lengths, charge stacking, and charge dropping. The Arkansas records also allow us to observe an additional institutional margin inside multi-charge cases: which charge becomes the vehicle of conviction and how that guilty outcome is formally recorded.

Because mandatory-minimum repeal can affect several margins at once, the empirical analysis proceeds in two parts. The policy analysis asks whether the reform changed average case outcomes: charge severity, prison sentence length, incarceration, probation, and subsequent criminal justice involvement. This part speaks to the primary policy concern raised by weakening punishment: whether lower incarceration is accompanied by increased subsequent criminal justice involvement or recorded drug activity. The prosecutor analysis asks how cases were resolved after the statutory threat was removed. In particular, we distinguish changes in the rate of guilty outcomes from changes in the form of charge resolution. If mandatory minimums primarily increased convictions by raising defendants' trial risk, repeal should reduce guilty rates or increase trial rates. If mandatory minimums instead operated partly as bargaining leverage, repeal may leave guilty rates relatively stable while changing which charges are retained or dropped, how sentences are structured, and how guilty pleas are formally recorded.

Mandatory Minimums as Bargaining Leverage

The pre-reform data also show that a guilty disposition on a charge coded as mandatory-minimum eligible did not necessarily correspond to a recorded prison term at that charge's coded statutory floor. Figure 2 shows how these charges resolved. Overall, 29.2 percent resolved guilty on a case whose longest recorded prison-type term met or exceeded the charge's own coded statutory floor, while 36.1 percent resolved guilty without such a term. The latter group divides almost evenly between charges on cases with no prison-type term recorded (18.8 percent) and charges on cases whose longest recorded prison term fell below the charge's own coded floor (17.3 percent). Among the latter, the median recorded prison term is 42 months against a median coded floor of 120 months.[1]

This figure should not be interpreted as evidence that courts imposed sentences inconsistent with governing law. The court records do not report drug weight, so statutory floors are assigned from the subsection of the filed charge, and the data do not allow us to determine whether a plea or charge amendment changed the legally applicable tier without a corresponding change in the recorded charge code. Nor can we distinguish the institutional basis for probation, suspended sentences, or shorter prison terms. We therefore interpret the 36.1 percent as showing that conviction on a charge coded as mandatory-minimum eligible often did not result in a recorded prison term at the coded threshold. Together with the frequency with which such charges were nolle prossed (dropped) while another charge on the same case resolved guilty, this pattern is consistent with the filed charge carrying bargaining relevance even when its coded sentencing floor was not reflected in the recorded final sentence.

In multi-charge cases, the mandatory-minimum charge was sometimes nolle prossed while another charge proceeded to a guilty conviction. Among pre-reform cases carrying a mandatory-minimum charge with any guilty conviction, 19.5 percent followed this pattern. This is consistent with charge bargaining in which the mandatory-minimum charge may have served as leverage and then been traded away. We interpret this as circumstantial evidence rather than direct observation of plea negotiations.

2.3 Data

The analysis uses administrative court records from the Arkansas Administrative Office of the Courts covering 2006–2016. The raw data are charge-level.[2] For the policy analysis, we aggregate

[1] Among charges on cases with no prison-type term recorded, 60 percent record probation, 33 percent a suspended imposition of sentence, and 7 percent no sentence record. Among charges on cases with a recorded prison term below their own coded floor, 62 percent have a recorded term of at least 36 months. Appendix Figure A.2 plots the distribution of recorded prison terms for the three most common statutory-floor categories. Sentences bunch at the coded floor in the 3- and 10-year categories, whereas recorded terms for charges carrying a 40-year coded floor fall almost entirely below that threshold. Appendix Figure A.3 shows the distribution of prison terms in a matched sample of these cases: after repeal, the share with no prison-type term rises from 21 to 29 percent, the mean term among cases with a prison-type term falls from 132 to 101 months, and the share of those terms at exactly ten years falls from 28 to 20 percent.

[2] The court records do not report the underlying quantity of drugs associated with each charge. We therefore cannot exploit the statutory quantity thresholds using a weight-based regression discontinuity or independently reconstruct sentencing exposure from continuous drug weight. Instead, we observe the statutory charges actually filed and use their charge codes to identify pre-reform mandatory-minimum-eligible offenses and the corresponding post-reform

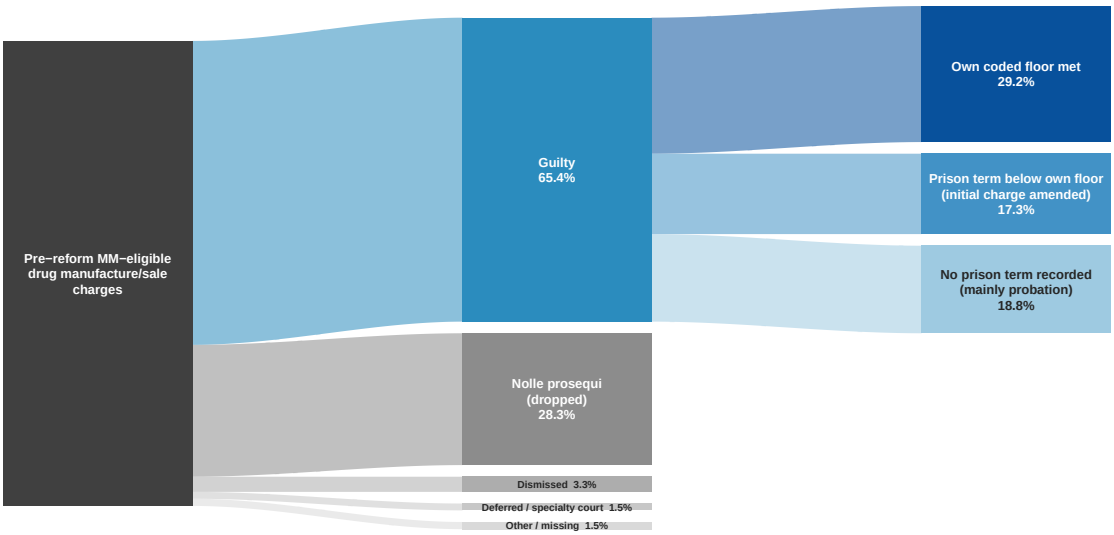


Figure 2: Disposition of pre-reform mandatory-minimum-eligible drug manufacture/sale charges

Notes: The sample includes pre-reform charges coded as mandatory-minimum eligible under §5-64-401(a), 2006–2010. Ribbon widths are proportional to the share of charges. The first split shows how each eligible charge was disposed; the second split divides guilty charges by whether the longest prison-type term recorded on the case met the charge's own coded statutory floor. Each eligible charge is assigned the statutory floor associated with its own filed statutory subsection under former §5-64-401(a). Sentence length is measured as the longest prison-type term recorded anywhere on the case. "Own coded floor met" denotes a guilty eligible charge for which this case-level prison term meets or exceeds the charge's coded statutory floor. "Prison term below own floor" denotes a guilty eligible charge on a case with a recorded prison-type term shorter than that floor. "No prison-type term recorded" includes guilty eligible charges on cases recording probation, suspended imposition of sentence, or no sentence record. Nolle prosequi, dismissal, deferred or specialty-court transfer, and other dispositions are measured from the eligible charge itself.

charges to the case, or case-defendant, level. This level provides more interpretable sentencing and recidivism outcomes. For the prosecutorial discretion analysis, we use charge-level variation to study plea and disposition margins directly.

The data include defendant characteristics, charges, case dispositions, sentencing outcomes, and defendant and prosecutor identifiers. Defendant identifiers allow us to measure subsequent criminal justice involvement. Prosecutor identifiers allow us to measure exposure to the old mandatory minimum regime and persistent differences in prosecutorial severity. The records contain both final charge dispositions and a separate plea-classification field indicating formally negotiated guilty outcomes.

Appendix Table B.4 reports summary statistics for cases containing at least one drug charge. Several raw differences preview the main findings. Average prison months (including zero sentences and with the cap used in the main analysis) fall from 35.87 to 26.25, while average maximum charge severity declines from 8.76 to 7.91. By contrast, the average number of charges per case rises modestly, from 2.68 to 3.06. These descriptive comparisons are consistent with reduced statutory punishment following Act 570, but they also combine the effects of the reform with other changes occurring over time. The causal analyses that follow isolate the discontinuous effect of the policy itself.

2.4 Empirical Objects and Estimands

The empirical analysis studies three objects. First, the statutory first stage asks whether old mandatory minimum charges disappeared and post-reform analog charges replaced them. This is a descriptive validation of the legal transition, which is highlighted in Figure 1.

Second, the policy analysis estimates the effect of Act 570 on drug cases at the implementation cutoff. The primary estimand is the discontinuous change in outcomes for cases initiated after the July 2011 implementation date, relative to the smooth counterfactual trend that would have prevailed absent the reform. These estimates capture the immediate effect of being processed under the new statutory regime. Complementary difference-in-differences estimates compare drug cases to low-overlap non-drug cases over a broader pre/post window; those estimates are used as validity checks and as a measure of the average post-reform change relative to comparison groups.

Third, the prosecutor analysis estimates bargaining responses to the removal of mandatory-minimum leverage. The within-case estimand asks whether affected drug manufacture/sale charges changed how they resolved relative to co-charges on the same case, including nolle prosequi, guilty disposition, negotiated guilty, and sentencing type. The prosecutor-level estimand asks whether negotiated guilty pleas increased more among prosecutors with higher pre-reform exposure to MM-eligible drug cases. These estimates do not measure the direct sentencing effect of Act 570. Instead, they measure whether charge resolution and formal plea resolution changed in the cases and prosecutors most exposed to the old mandatory-minimum bargaining environment.

analog charges. The filed charge is nevertheless central to our bargaining analysis because it determines the statutory sentencing threat reflected in the court record. The absence of drug quantities prevents us from separately studying charging behavior or case outcomes around individual quantity thresholds.

3 Effects of Sentencing Reform

3.1 Charging and Sentencing

To estimate the effects of mandatory minimum repeal on charging and sentencing outcomes, we employ a regression discontinuity in time design. Identification relies on the assumption that, absent Act 570, the outcomes of interest would have evolved smoothly through the policy implementation date. Under this assumption, a discontinuous change at the July 2011 cutoff identifies the causal effect of being processed under the new legal regime. This assumption is plausible in the Arkansas setting because Act 570 took effect on a single date and immediately altered the statutory sentencing framework governing drug offenses. Although the magnitude of the statutory change varied by offense type and prior mandatory-minimum eligibility, all drug cases initiated after July 2011 were processed under the new legal regime. Figure 1 documents the corresponding first stage: charges under the old mandatory-minimum statute collapse at implementation and are replaced by post-reform analog charges, while the share of drug cases carrying a drug manufacture/sale charge remains of similar magnitude on either side of the cutoff. The discontinuity therefore reflects a sharp change in the statutory regime governing a continuing class of drug charges, rather than the disappearance of that charge category from the court system.

The baseline local linear specification is

$$y_{it} = \alpha + \tau \text{Post}_t + \beta_1 r_t + \beta_2 (\text{Post}_t \times r_t) + \varepsilon_{it}, \tag{2}$$

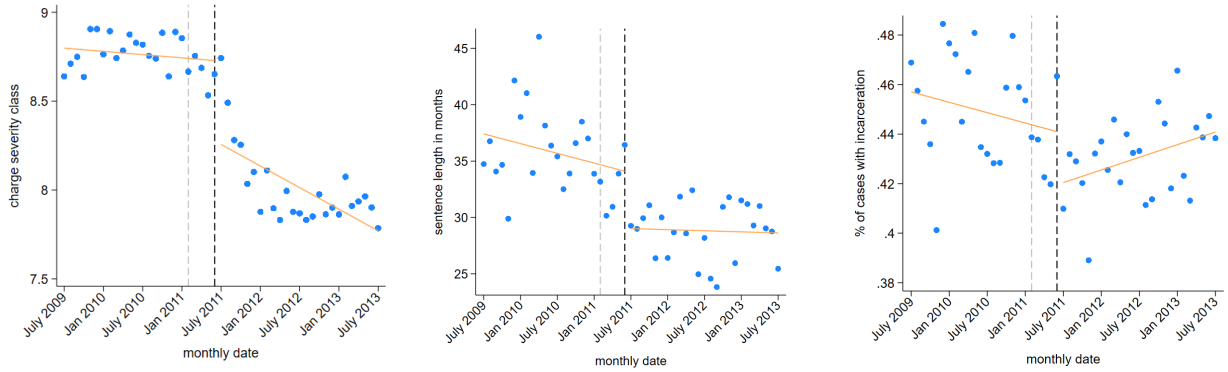
where y_{it} is the outcome for case i initiated in month t, Post_t equals one for cases initiated after the July 2011 implementation date, and r_t is the month of case initiation centered at the implementation cutoff. The coefficient of interest is τ, the discontinuous change in the outcome at the cutoff. The interaction term allows the underlying time trend to differ on either side of the cutoff. The baseline estimates use a 24-month bandwidth, a 48-month bias bandwidth for robust inference, a local linear specification, a uniform kernel, and standard errors clustered by month.

Figure 3 plots the local linear fits underlying the preferred 24-month bandwidth specification for the sample of cases containing at least one drug charge. Corresponding regression estimates are presented in Appendix Table B.5. The most immediate consequence of Act 570 should be a reduction in statutory charge severity, because the reform reassigned many drug offenses to lower felony classifications. Consistent with this prediction, the maximum charge severity within a case declined by 0.469 points from a pre-policy mean of 8.7 on the 11-point severity scale.

The reduction in charge severity was accompanied by lower punishment severity. Prison sentence length fell by approximately 4.5 months, and the probability of incarceration declined by 2 percentage points. Effects are statistically significant under both conventional and robust bias-corrected inference (Calonico et al., 2014), though the decline in incarceration is smaller and less precisely estimated, as it is significant at the 5 percent level under conventional inference but only at the 10 percent level under robust bias-corrected inference.

We also examine whether the effects of Act 570 differ between cases with manufacturing/sale charges and those with possession charges. Because mandatory minimum eligibility was concentrated among manufacturing/sale charges prior to the reform, we would expect the policy effects

Figure 3: Discontinuities in charge severity and punishment at reform



(a) Max charge severity **(b)** Prison sentence length **(c)** Pr(Incarceration)

Notes: Each panel presents a regression discontinuity estimate using a 24-month bandwidth. The gray dashed line marks the signing of Act 570, and the black dashed line marks the July 2011 implementation date used as the discontinuity cutoff. Solid orange lines show separate linear fits estimated on either side of the cutoff.

to be largest for these cases. Appendix Table B.6 replicates Appendix Table B.5 separately for the two case types. Consistent with this prediction, sentence lengths decline considerably more for manufacture/sale cases than for possession cases: possession cases show an insignificant decrease of 2.2 months, compared with a 7.6-month decrease for manufacture/sale cases. Manufacture/sale cases also experience a larger decline in charge severity.[3]

Robustness Checks

The main estimates are robust to bandwidth and kernel choice and are not replicated at alternative implementation dates. Appendix Table B.7 shows similar effects across the MSE-optimal, 12-month, and 36-month bandwidths, though incarceration is less precisely estimated at some bandwidths. Appendix Table B.8 shows comparable results under triangular and Epanechnikov kernels. Appendix Table B.9 reports placebo tests at false implementation dates three years before and after Act 570. The placebo estimates are substantially smaller than those at the true cutoff and do not reproduce the joint pattern of the main outcomes.

One potential concern is that the reform may have generated anticipatory responses. Act 570 was announced four months before its official implementation, providing an opportunity for defendants, prosecutors, or other actors to adjust their behavior in advance. In the discontinuity figures, we mark the timing of the announcement and find no evidence of a sharp response at that date. We nevertheless systematically test for anticipation by repeating our main discontinuity analysis in two ways: first, excluding the four months immediately preceding implementation, and second, using a four-month donut that excludes observations in the four months before and after implementation. Appendix Table B.10 reports these estimates. The results are similar to our baseline estimates: the main effects remain statistically significant, while the estimated declines in charge severity and prison sentence length become larger in magnitude.

[3]Note that this analysis omits cases with both manufacture/sale and possession charges, which helps explain why incarceration effects move towards zero.

Although the RDiT design provides the primary source of identification, changes in case composition or other contemporaneous shifts in Arkansas courts could complicate interpretation. We therefore estimate a complementary difference-in-differences design that compares drug cases to Arkansas offense categories not directly affected by the mandatory minimum repeal. The key empirical requirement is that the comparison group exhibits similar pre-reform trends. A second consideration is contamination: offense categories that frequently co-occur with drug charges may be indirectly affected by the reform because prosecutors can substitute across charges within a case. Appendix Table B.11 reports pre-reform overlap rates for candidate control groups. Fraud and procedural offenses have the lowest overlap rates, each below 1.2 percent.

The DiD specification is

$$y_{ict} = \alpha + \kappa \text{DrugCase}_i + \tau(\text{DrugCase}_i \times \text{Post}_t) + X_i'\gamma + \delta_c + \lambda_t + \varepsilon_{ict}, \tag{3}$$

where y_{ict} is an outcome for case i in county c and year t, DrugCase_i identifies cases containing a drug charge, Post_t indicates the post-reform period, X_i includes defendant age, age squared, and sex, and δ_c and λ_t are county and year fixed effects. The coefficient of interest is τ, the differential pre-to-post change for drug cases relative to the comparison group. Cases that contain both a drug charge and a control-group charge are excluded to avoid direct contamination. The post indicator is absorbed by year fixed effects and is therefore omitted from Equation (3).

Appendix Figure A.4 presents event-study estimates comparing drug cases with the preferred fraud and procedural control groups for charge severity, prison sentence length, and incarceration probability. For both charge severity and sentence length, the estimated coefficients remain close to zero throughout the pre-reform period and decline sharply following implementation, providing support for the parallel trends assumption. Furthermore, these decreases persist across the entire time period, showing the policy was not simply a short-term shock to case outcomes. Pre-policy trends for incarceration are noisier, but the figure nonetheless shows a decrease in the trend at the timing of the policy. Appendix Table B.12 reports the corresponding DiD estimates across all outcomes, which are consistent with the event studies and the RDiT results. The average highest charge declined by approximately 1.1 felony classifications, and prison sentence length fell by 9.2 months, roughly 10 percent of the pre-policy average.

The RDiT and DiD designs produce qualitatively similar conclusions, but the DiD estimates are generally larger. This difference is consistent with dynamic adjustment visible in the RD plots and event-study estimates. Rather than adjusting immediately to a new equilibrium, several outcomes continued to change during the months after implementation before stabilizing. As a result, the RDiT estimates capture the immediate discontinuity at implementation, while the DiD estimates reflect the average change over a broader post-reform period.

3.2 Downstream Consequences

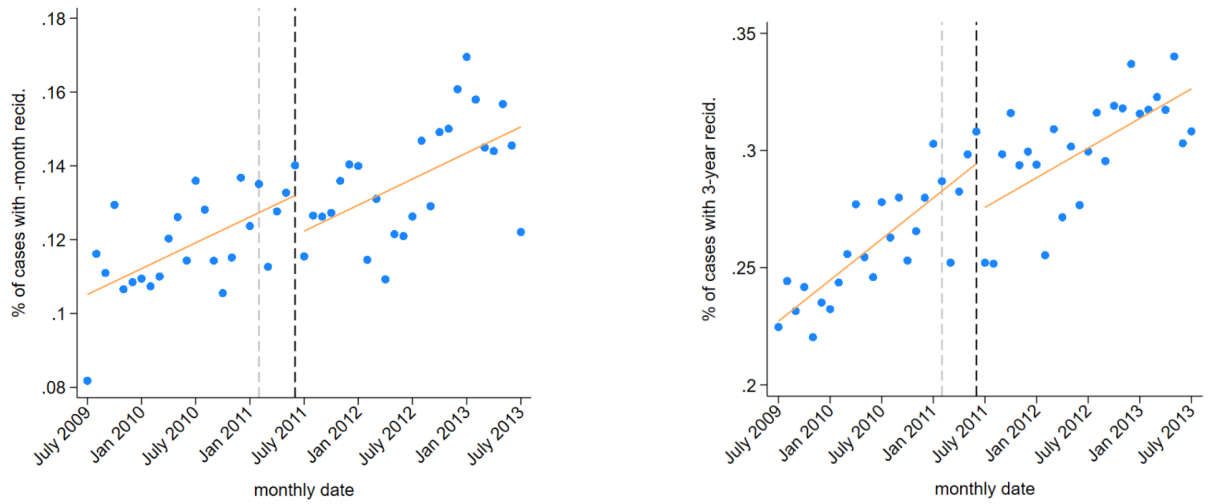
Subsequent Criminal Justice Involvement

We next examine whether the changes in charging and sentencing induced by Act 570 translated into changes in subsequent criminal justice involvement. One concern surrounding reforms that reduce

statutory penalties is that they may increase recidivism by reducing deterrence or incapacitation. We measure recidivism as the initiation of a new criminal case within six months, one year, two years, and three years following the filing date of the original case. Because our measure captures the initiation of a subsequent criminal case, rather than criminal offending directly, these estimates should be interpreted as evidence about subsequent criminal justice involvement rather than crime itself.

Figure 4 illustrates the main regression discontinuity estimates for six-month and three-year recidivism, and Appendix Table B.13 reports all four horizons. The estimates are negative at the reported horizons, with no systematic increase as the follow-up period lengthens. The estimated three-year effect is a decline of 2.2 percentage points, and estimates at shorter horizons are also negative. These findings give no indication that the reduction in statutory penalties increased subsequent criminal justice involvement on the outcomes and horizons measured. They do not, however, rule out changes in underlying drug activity that may not translate into the initiation of a subsequent criminal case, which we explore next.

Figure 4: Subsequent criminal case initiation around the reform cutoff (recidivism)



(a) 6-month recidivism **(b)** 3-year recidivism

Notes: Each panel presents a regression discontinuity estimate using a 24-month bandwidth, a local linear specification, and a uniform kernel; recidivism is the initiation of a new criminal case within the stated horizon. The estimated discontinuities are −0.9 percentage points at six months (robust $p = 0.086$) and −2.2 percentage points at three years (robust $p = 0.028$). Estimates for all four horizons, with conventional and robust p-values, are reported in Appendix Table B.13. Standard errors are clustered by month.

Drug-Related Incidents

A related concern with reducing mandatory sentencing floors is that lower expected punishment could increase drug-related offending. We examine this possibility using agency-month NIBRS data on recorded drug incidents per 100,000 residents. Appendix Table B.14 reports both regression-discontinuity-in-time estimates and complementary difference-in-differences estimates using avail-

able bordering states as controls.[4] Across specifications, we find no statistically significant evidence of an increase in recorded drug incidents following Act 570. Estimates for drug incidents resulting in at least one arrest are similarly small and statistically insignificant.

3.3 Heterogeneity by Previously Recorded Criminal History

The effects of punishment may differ according to defendants' previous contact with the criminal justice system. We divide cases into those with no previously recorded court cases and those with at least one prior recorded case, using defendant identifiers in the administrative records. This is recorded history rather than an exhaustive lifetime criminal record: the available panel begins in 2006. We estimate subgroup RDiTs and difference-in-discontinuities specifications that directly test whether the policy discontinuity differs between groups. The latter estimates the discontinuity for defendants with any prior recorded case minus the discontinuity for those with none.

Appendix Tables B.15 and B.16 report the subgroup RDiT estimates, while Appendix Tables B.17 and B.18 report formal tests of heterogeneity. Defendants without prior recorded cases have larger estimated declines in prison months (-6.265 versus -3.098) and incarceration probability (-2.5 versus -1.2 percentage points), but these differences are not statistically distinguishable from zero. The reduction in charge severity is instead significantly larger for defendants with prior records, by approximately 0.17 points.

The most pronounced heterogeneity is in subsequent case initiation. Three-year recidivism falls by 5.0 percentage points for defendants without prior recorded cases, compared with an imprecisely estimated increase of 2.0 percentage points for those with prior cases. The directly estimated difference is 7.0 percentage points (standard error 2.2 percentage points). Differences are statistically significant at six months, one year, two years, and three years.

The longer available post-2006 history for defendants observed later raises a measurement concern. We therefore also classify prior cases using fixed three-year and five-year lookback windows. Appendix Table B.19 shows that the heterogeneity in recidivism is robust to these alternative definitions. With a three-year lookback, the differences remain significant at one, two, and three years, while the six-month estimate is smaller and insignificant. The five-year definition yields significant differences at all four horizons. The three-year specification is particularly informative near the July 2011 cutoff because the entire lookback is observable throughout the main 24-month estimation window. The five-year lookback is not fully observable for earlier cases in that window and is therefore best viewed as additional sensitivity evidence.

One possible interpretation is that reducing incarceration exposure has greater benefits for defendants with limited prior criminal justice involvement, for whom imprisonment may disrupt employment, social ties, or other protective factors. Another possibility is diminishing marginal deterrence from already substantial punishment. The evidence does not distinguish these mechanisms. We therefore treat this heterogeneity as an additional policy result, not as direct evidence for a particular channel.

[4] NIBRS coverage was insufficient for Louisiana, Missouri, and Mississippi, so the comparison states are Oklahoma, Tennessee, and Texas.

3.4 Fiscal Implications

The estimated 4.495-month reduction in imposed prison sentences is potentially important for corrections spending. To convey its scale, we construct an illustrative expenditure equivalent using approximately 10,300 drug cases initiated annually in Arkansas and the Arkansas Department of Correction's FY2011 average prison operating cost of $59.70 per inmate-day, or about $1,816 per month (Arkansas Department of Correction, 2015).

Let s be the assumed fraction of a month removed from the imposed sentence that translates into an actual month of prison time avoided. The accounting calculation for one annual cohort is therefore

$$10{,}300 \times 4.495 \times s \times \$1{,}816 \simeq \$84 \text{ million} \times s.$$

With a comparable cohort entering each year, this expression is also the approximate steady-state annual flow of the expenditure equivalent, after the effects of the sentence reductions have worked through the prison population. It is not an estimate of the immediate first-year budgetary effect. Appendix Table B.20 reports expenditure equivalents ranging from 7% to 21% of FY2011 care and custody spending under the assumed pass-through rates.

The illustration should not be interpreted as a welfare calculation. Average operating costs need not equal the marginal resources saved from lower prison utilization, and the exercise does not value effects on victims, defendants, families, labor supply, prosecution costs, or supervision. Its purpose is to make the scale of a reduction in imposed prison exposure transparent, while keeping the fiscal extrapolation separate from the causal estimates.

3.5 Interpretation of the Policy Effects

The policy analysis shows that Act 570 reduced the severity of punishment for Arkansas drug defendants. Among cases with at least one drug charge, the reform lowered maximum charge severity, prison sentence length, and the probability of incarceration. At the same time, we find no evidence that these reductions increased subsequent criminal justice involvement or recorded drug-related activity. The reform cut prison exposure for the population directly affected by the statutory change, yet we do not detect the increase in either outcome that large adverse deterrence or incapacitation effects would produce.

Although our findings speak to the potential benefits and costs of Act 570, they do not provide a comprehensive estimate of net welfare effects. A full cost-benefit calculation would require valuing victimization, drug-market activity, defendant and family welfare, labor-market outcomes, probation and prison costs, and the costs of prosecution and supervision. The fiscal calculation in Section 3.4 illustrates the scale of the estimated reduction in imposed sentences, not realized savings. The absence of detectable adverse downstream effects is consistent with some slack in punishment at the pre-reform margin: additional months of the old sentences may have provided little incremental deterrence or incapacitation, and reduced incarceration may also have avoided criminogenic disruption. Our design does not identify a punishment threshold below which deterrence would increase, nor can it isolate these channels.

The largest reductions in punishment occurred among manufacture/sale cases, the group with

near-universal mandatory minimum eligibility before reform. However, these estimates do not distinguish the direct effects of reducing statutory penalties from changes in how prosecutors charge and bargain. We therefore turn to prosecutor-level analyses to examine how charging and bargaining decisions changed after the reform removed this source of statutory leverage.

4 Prosecutorial Charging and Bargaining

This section examines how prosecutors responded to the change in the bargaining environment created by Act 570. Standard models of prosecutorial bargaining predict several possible margins of adjustment: prosecutors might file more charges to recreate lost leverage, dismiss charges more readily as part of negotiation, offer shorter sentences in exchange for a guilty plea, or shift along the plea/trial margin itself. We test each of these standard margins directly in Section 4.1.

The main analysis then turns to the internal structure of multi-charge cases. Because most drug cases carry multiple charges, we can examine how the specific charge affected by repeal resolves relative to other charges on the same case, and whether that resolution changed after reform. Section 4.2 develops this within-case evidence, which is the paper's central evidence on prosecutorial adaptation. Section 4.3 then turns to the plea-form margin specifically and uses prosecutor-exposure and judge-robustness checks to ask whether it moves for reasons connected to the old mandatory-minimum regime.

The central empirical distinction throughout is between the rate of guilty outcomes and the form those outcomes take. If mandatory minimums primarily increased prosecutors' ability to obtain convictions, repeal should reduce guilty conviction rates or increase trial rates. If mandatory minimums instead operated partly as bargaining leverage, repeal may leave guilty rates relatively stable while changing how guilty outcomes are reached and resolved, including which charge remains the charge of conviction, whether other charges are nolle prossed, and what sentence follows. We do not observe plea offers or negotiation transcripts, so nothing below speaks directly to the content of any negotiation. However, the data do record whether individual charges were formally negotiated, so we can say something about the role mandatory minimums played in plea bargaining.

4.1 Standard Prosecutorial Response Margins

The conceptual framework outlined in Section 2.2 suggests several ways prosecutors could respond to the removal of mandatory-minimum leverage. One possibility is charge stacking: prosecutors may file more charges per case to recreate bargaining leverage after the statutory floor is removed. A second possibility is charge dropping: prosecutors may continue to file severe charges but dismiss them more often as part of plea negotiations. A third possibility is sentence concessions: if prosecutors have less leverage after repeal, negotiated guilty pleas may carry shorter prison sentences than otherwise similar straight guilty pleas. A fourth possibility is a shift in the trial margin itself: if mandatory minimums raised the defendant's expected cost of losing at trial, repeal should make trial comparatively more attractive, particularly for prosecutors most exposed to the old regime.

To test these standard response margins directly, we use a prosecutor-exposure design. Let Z_p denote prosecutor p's pre-reform share of drug cases that were mandatory-minimum eligible, stan-

dardized to mean zero and standard deviation one. We estimate exposure-gradient specifications of the form

$$Y_{dct} = \alpha_p + \mu_{ct} + \beta\,(\text{Post}_t \times Z_p) + X_d'\gamma + \varepsilon_{dct}, \tag{4}$$

where d indexes cases, p indexes prosecutors, c indexes counties, and t indexes years. The prosecutor fixed effects (α_p) absorb permanent differences across prosecutors, while county-by-year fixed effects (μ_{ct}) absorb annual shocks common to prosecutors practicing in the same county, including changes in court practice and recording. Identification of the interaction comes from differences in pre-reform exposure across prosecutors after residualizing prosecutor and county-by-year fixed effects and the other controls. Prosecutors need not move between counties to contribute identifying variation: 89% of observations belong to county-year cells with at least two prosecutors. The coefficient β measures whether prosecutors more exposed to the old mandatory-minimum regime changed the outcome differentially after repeal. Appendix Table B.21 describes the prosecutors and cases in the high- and low-exposure halves of the sample.

Figure 5 reports the results from four tests, and Appendix Table B.22 reports the underlying estimates. The first panel examines charge stacking, using the number of charges filed on the case as the outcome. The estimate is close to zero, providing little evidence that more-exposed prosecutors increased the number of charges filed after repeal. The second panel examines charge dropping, using the share of charges on a case that were nolle prossed, among multi-charge cases with at least one guilty conviction. The estimate is small and statistically insignificant, so there is little sign that the response ran mainly through more dismissals. The third panel examines the plea/trial margin using an indicator for a bench or jury trial (the trial rate). The preferred estimate is small and insignificant, so we find no differential move into or away from trial among the most exposed prosecutors. The fourth panel uses prison months, with zeros for charges that receive no prison sentence. The estimate is positive but imprecise, and we see no evidence that more-exposed prosecutors cut prison months.

These standard margins show little evidence of charge stacking, overall charge dropping, differential reductions in prison months, or movement into trial, and the nulls hold under alternative constructions of Z_p (Appendix Table B.23). That does not mean prosecutors failed to adapt. Adaptation may instead show up in how affected charges resolve within cases, which we turn to next.

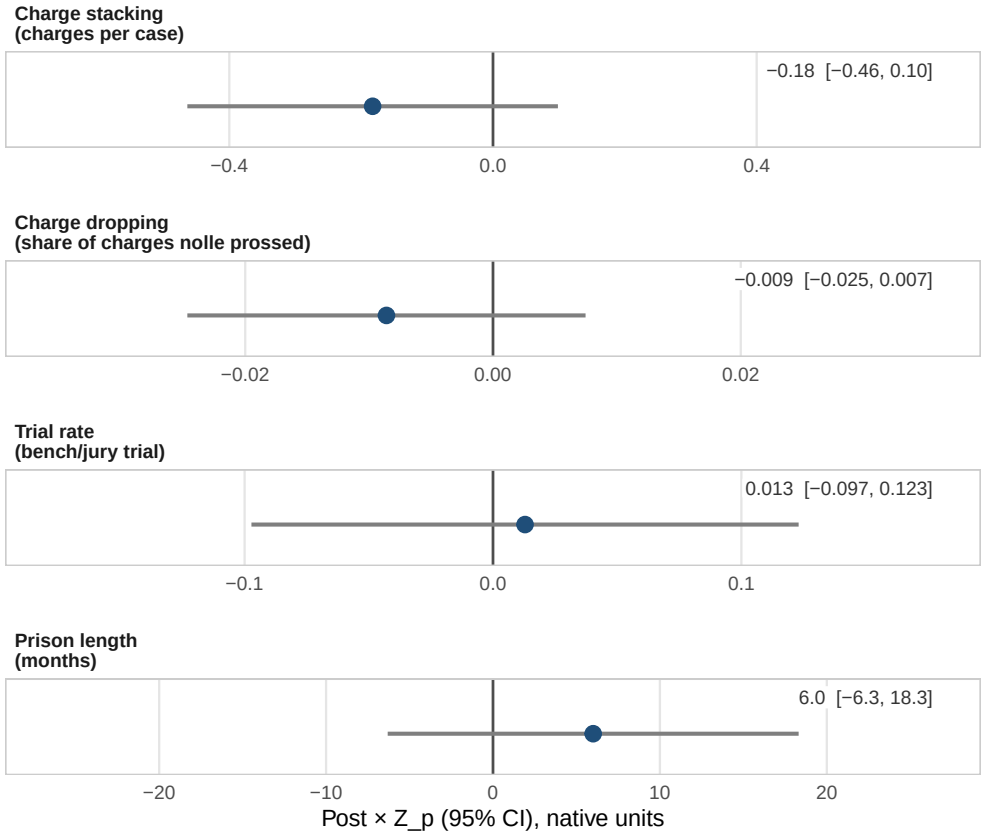


Figure 5: Standard prosecutorial responses by pre-reform mandatory-minimum exposure

Notes: Each panel reports the coefficient on Post × Z_p and its 95% confidence interval from a separate regression. Z_p is the standardized pre-reform share of a prosecutor's drug cases that were mandatory-minimum eligible. The coefficients measure differential post-reform changes among prosecutors with greater pre-reform exposure. Outcomes are reported in their native units. Charge stacking measures the number of charges per case in the balanced-prosecutor sample. Charge dropping measures the share of charges nolle prossed among multi-charge cases with at least one guilty conviction. Prison length measures months sentenced among drug manufacture/sale charges with a guilty disposition, including zeros for charges without prison sentences. Trial rate indicates whether a case involved a bench or jury trial. All specifications include prosecutor and county-by-year fixed effects, with standard errors clustered by prosecutor. Full regression results are reported in Appendix Table B.22.

4.2 Within-Case Charge Resolution

The prosecutor-exposure design above compares prosecutors with different pre-reform exposure to mandatory-minimum leverage. We complement it with a charge-level comparison within the same case, which asks a narrower and more direct question: within a case that includes both a drug manufacture/sale charge and at least one other charge, does the drug manufacture/sale charge resolve differently from its co-charges, and did that difference change after repeal?[5] Because the comparison is within the same case, it is less vulnerable to broad changes in court recording practice or prosecutor- and county-level composition.

We restrict attention to mixed cases that contain both a drug manufacture/sale charge and at least one other charge, and estimate

$$Y_{jd} = \alpha_d + \eta\,\text{DrugSaleManuCharge}_{jd} + \beta\left(\text{Post}_d \times \text{DrugSaleManuCharge}_{jd}\right) + \varepsilon_{jd}, \tag{5}$$

where j indexes charges, d indexes cases, and Y_{jd} is one of several charge-level outcomes: an indicator for nolle prosequi or guilty disposition (final dispositions), negotiated guilty plea (the plea field), or any prison sentence, any probation sentence, or a probation sentence with no prison component (sentencing, unconditional on disposition); the appendix also reports sentencing outcomes conditional on a guilty disposition. α_d is a case fixed effect, $\text{DrugSaleManuCharge}_{jd}$ indicates a drug manufacture/sale charge, and Post_d equals one for cases initiated after July 2011; the post indicator is absorbed by the case fixed effect. The coefficient η is the drug manufacture/sale charge premium in pre-reform cases, and β is the change in that premium after reform. The case fixed effect absorbs all county-by-year main effects because charges on the same case share a county and filing year. Standard errors are clustered by prosecutor. Our preferred estimation sample is restricted to charges with an identified prosecutor among those observed before and after reform, so that the within-case and prosecutor-exposure analyses use the same underlying prosecutor population. We show the same within-case outcomes for all mixed-charge cases as a robustness check.

Drug manufacture/sale charges already resolved differently from their co-charges before reform, so the main estimates are changes in these differentials, not differences emerging from zero. Appendix Table B.24 reports the drug manufacture/sale charge differential separately across pre- and post-reform windows. Before reform, drug manufacture/sale charges were more likely than co-charges to resolve negotiated guilty or guilty overall, and less likely to be nolle prossed or dismissed. Conditional on a guilty disposition, they were also more likely to receive probation and somewhat less likely to receive prison.

Figure 6 plots the main within-case estimates, and Appendix Table B.25 reports them in full. Panel A uses final dispositions. After reform, the nolle prosequi differential falls by 5.4 percentage points: relative to co-charges on the same case, drug manufacture/sale charges became less likely to be dropped. The guilty-disposition estimate is positive (3.2 percentage points) but not statistically significant. Panel B uses the plea field: the negotiated-guilty premium rises by 6.3 percentage points, consistent with the plea-form evidence presented in Section 4.3. The Panel A estimates

[5] The within-case design does not rely on an unusual case structure. Among all drug manufacture/sale cases, 68.6 percent before reform and 71.7 percent after reform contain at least one non-drug manufacture/sale co-charge; in the balanced-prosecutor estimation sample, the corresponding shares are 83.9 and 77.8 percent (Appendix Figure A.5).

do not use the plea field at all, so they indicate that the post-reform shift reflects changes in charge resolution, not only a change in how existing guilty outcomes are labeled. Panel C reports sentencing outcomes, unconditional on disposition. The premium for any prison sentence changes little, while the premium for any probation sentence rises by 4.9 percentage points and the premium for probation without prison rises by 6.4 percentage points.

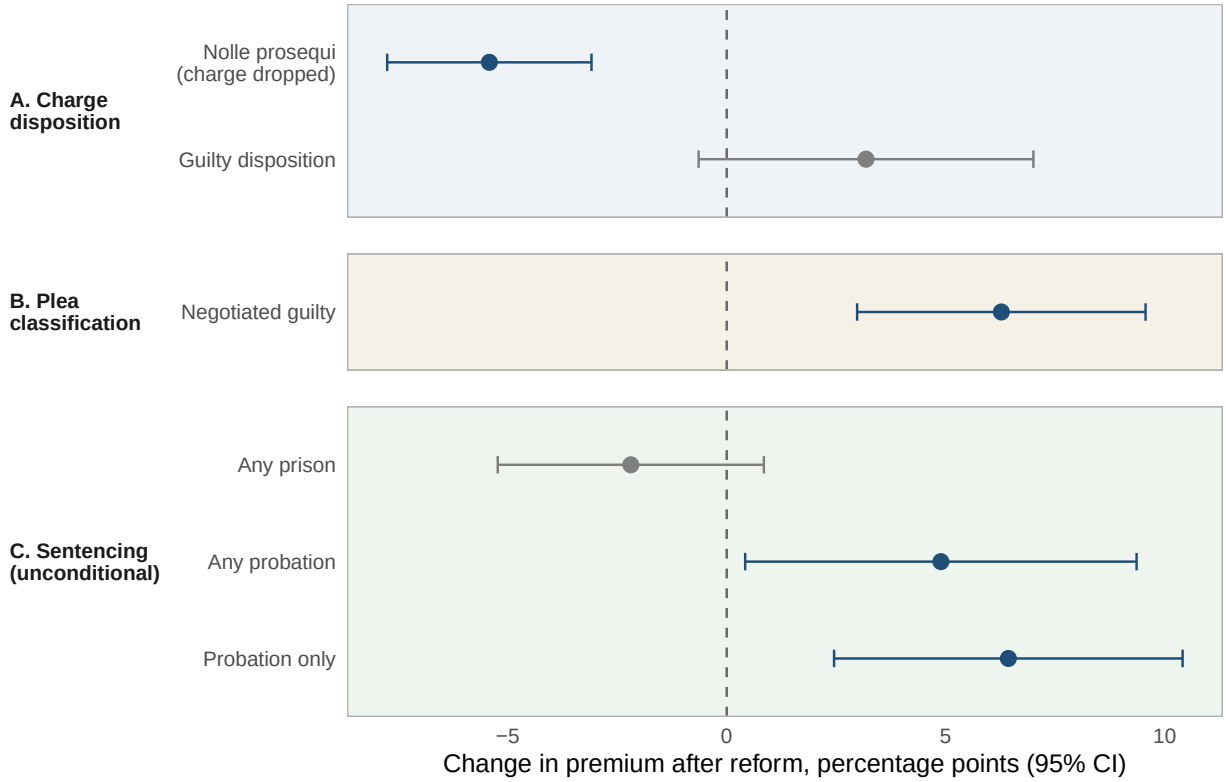


Figure 6: Changes in charge resolution and sentencing relative to co-charges on the same case

Notes: Each point reports the coefficient on Post × drug manufacture/sale charge from a separate charge-level regression, measuring the post-reform change in the drug manufacture/sale charge premium relative to co-charges on the same case. Bars indicate 95% confidence intervals; blue estimates have intervals excluding zero. The sample consists of charges on mixed cases among balanced-panel prosecutors, 2006–2016, containing at least one drug manufacture/sale charge and one other charge (N = 37,299). Panel A uses final charge dispositions, Panel B plea classifications, and Panel C sentencing outcomes unconditional on disposition. All specifications include case fixed effects and a drug manufacture/sale charge indicator, with standard errors clustered by prosecutor. Full regression results, including pre-reform means, are reported in Appendix Table B.25.

Appendix Table B.26 restricts the sentencing outcomes to charges with a guilty disposition and separates prison and probation into mutually exclusive categories, since Arkansas sentences can combine both. Conditional on conviction, the premium for any prison sentence falls by 2.3 percentage points and the premium for any probation sentence rises by 4.7 percentage points; probation without prison rises by 6.9 percentage points, prison with probation decreases (2.2 percentage points), and prison without probation is essentially unchanged. Appendix Table B.27 re-estimates the three panels on all mixed cases, which adds the charges with no identified prosecutor: the negotiated-guilty estimate is larger (9.6 percentage points), the nolle prosequi estimate has the

same sign at about half the size (2.5 percentage points), and the probation premium rises (3.7 percentage points for any probation and 3.8 for probation only), with no detectable change in the prison premium. Across these tables, drug manufacture/sale charges were less likely to be nolle prossed and more likely to end in probation than co-charges on the same case after reform.[6]

One interpretation of this pattern is that the pre-reform mandatory-minimum charge was sufficiently severe that prosecutors valued it as a bargaining tool but were less willing to carry it through to final disposition. Act 570 replaced the old floors with more graduated penalties, potentially making the analog charge more viable as the charge retained through disposition. This interpretation is consistent with the contemporaneous rationale for Arkansas's reform: the state Working Group recommended separating drug offenses and setting "punishments appropriate to the offense" (Arkansas Working Group on Sentencing and Corrections, 2011, p. 7). It is also consistent with qualitative evidence from other mandatory-minimum settings. In interviews conducted by the U.S. Sentencing Commission, federal prosecutors in one district described mandatory-minimum enhancements as a "hammer for the worst offenders" but otherwise too harsh for low-level drug offenders; defense attorneys in the same district reported that the possibility of seeking the enhancement was often used to negotiate a guilty plea (United States Sentencing Commission, 2011, p. 113). These accounts provide institutional support for the proposed mechanism, although our data do not allow us to observe prosecutors' motives directly.

4.3 Plea-Form Evidence and Exposure Gradients

The within-case evidence above shows that affected charges changed how they resolved, using outcomes that do not depend on the plea field. We next ask whether the formal plea category moves in the same direction. This is a distinct test because the court records identify whether a guilty plea was recorded as negotiated. If mandatory minimums operated partly as bargaining leverage, prosecutors most exposed to that leverage before repeal should exhibit larger shifts toward negotiated guilty pleas after repeal.

The descriptive patterns are consistent with such a shift. For drug manufacture/sale charges, formally recorded negotiated guilty pleas rose from 6.5 percent under the old mandatory-minimum regime to 36.5 percent under the post-reform analog regime, while the overall guilty conviction rate was essentially unchanged (64.8 to 65.1 percent; Appendix Figure A.6). The increase was not isolated to one county: among 16 counties with at least 20 cases pre- and post-reform in the balanced panel, 15 show a positive shift toward negotiated guilty pleas.

The key test of this section is not whether negotiated guilty pleas rose in the aggregate, but whether the increase is larger among prosecutors more exposed to the old mandatory-minimum regime after absorbing local time-varying court conditions. We estimate this test using the prosecutor-exposure design in Equation (4). In the preferred specification with county-by-year fixed effects, a one standard deviation increase in a prosecutor's pre-reform mandatory-minimum exposure is associated with a 6.1-percentage-point larger post-reform increase in negotiated guilty pleas, compared with 18.1 percentage points in the baseline specification with common year effects (Appendix

[6] Appendix Tables B.28 and B.29 show robustness to the three comparison groups used elsewhere in the paper – any co-charge, all-but-possession, and possession-only.

Table B.30).[7]

We further test whether this exposure gradient reflects the prosecutor's own pre-reform history rather than the courtroom environment. The estimate is robust to replacing county-by-year fixed effects with judge-by-year fixed effects, to including a judge-side exposure measure with little independent explanatory power, and to a two-way prosecutor/judge fixed-effects decomposition on the connected judge-prosecutor network (Appendix Table B.32).[8]

Finally, negotiated guilty pleas are associated with other features of case processing. They predict longer case duration, a higher probability of probation, and, conditional on prison, a lower probability of a sentence above 120 months (Appendix Table B.33). The plea-form results corroborate that the reform changed formal guilty-plea resolution in the cases and prosecutors most exposed to the old mandatory-minimum regime. The negotiated-guilty field is not the foundation of the mechanism; the main evidence is the within-case change in nolle prosequi, guilty disposition, and sentencing type. The exposure-gradient results show that the formal plea category moves in the same direction, and in the same parts of the system, as the broader charge-resolution response.

5 Conclusion

Arkansas's repeal of drug-offense mandatory minimums shows that the effects of sentencing reform cannot be read off statutory penalties alone. The reform lowered charge severity and imposed prison sentences by approximately 4.5 months per drug case, with a smaller and less precisely estimated decline in the likelihood of incarceration. We find no detectable increase in subsequent criminal justice involvement or recorded drug-related activity; three-year subsequent case initiation instead declines. Those declines are larger among defendants without previously recorded cases, although our data do not isolate the mechanism underlying that heterogeneity.

The main prosecutorial response is inside multi-charge cases. We find little evidence that prosecutors most reliant on the former minimums systematically replaced lost leverage through more charges, greater overall dropping, longer prison sentences, or more trials. Instead, following repeal, affected manufacture/sale charges were less likely to be dropped and more likely to receive probation-only sanctions relative to co-charges on the same case. Formally negotiated guilty classifications also changed, which fits the same story. These patterns are consistent with the possibility that unusually severe charges carried bargaining value before repeal but became more viable to retain when the associated punishment became more graduated.

The observed effects do not establish that mandatory minimums never deter crime, that prosecutors share a particular objective, or that the sentencing provisions of Act 570 can be separated perfectly from its other components. They do show that a meaningful reduction in statutory punishment can coexist with stable or lower measured downstream involvement and with changes in

[7] Appendix Table B.31 applies the same design to alternative offense groups and supports a mandatory-minimum-specific component to the negotiated-plea shift.

[8] We also examined heterogeneity by pre-reform judge-prosecutor familiarity. Simple familiar-pair measures are difficult to interpret because repeated pairings partly reflect concentration before high-volume judges rather than relationship-specific ties. Using a residualized dyadic measure that nets out both prosecutor and judge activity, we find little evidence that the prosecutor-exposure gradient differs by pair familiarity. We therefore use judge-prosecutor information primarily for the robustness checks reported in Appendix Table B.32.

how discretionary actors resolve cases. Evaluating a sentencing reform requires looking at the decisions through which punishment schedules are implemented, not just at the schedules.

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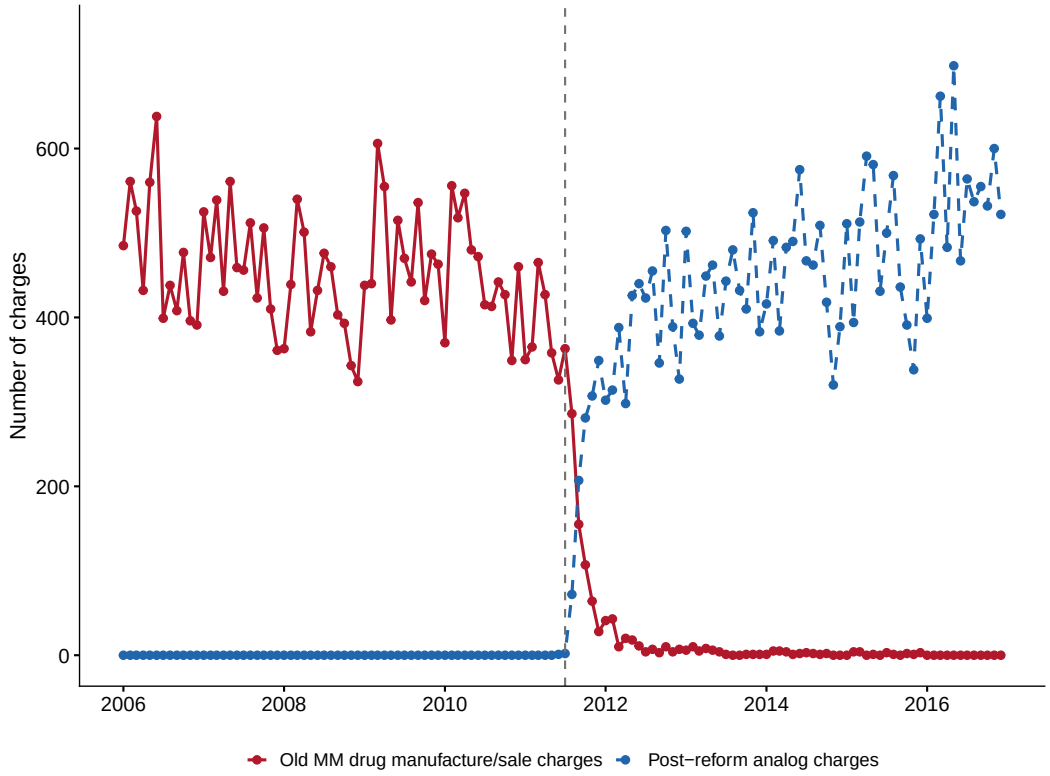
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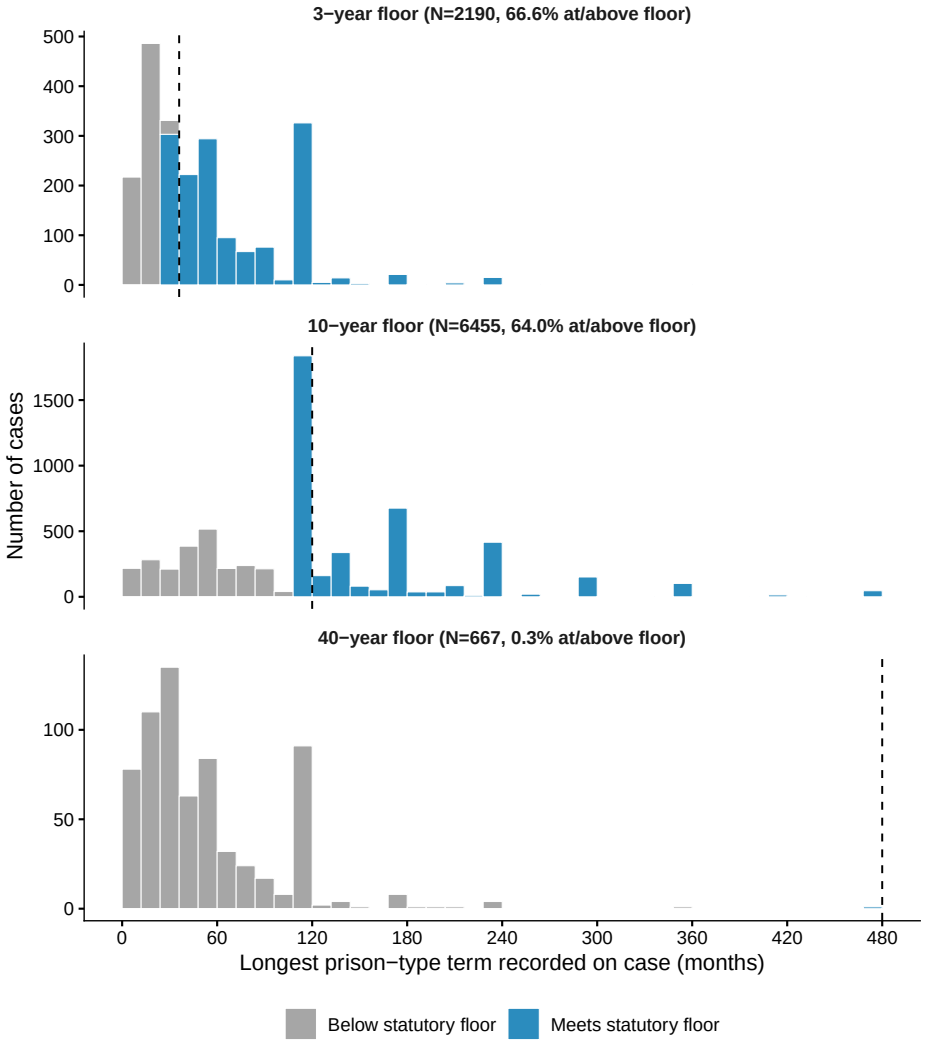
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A Additional Figures



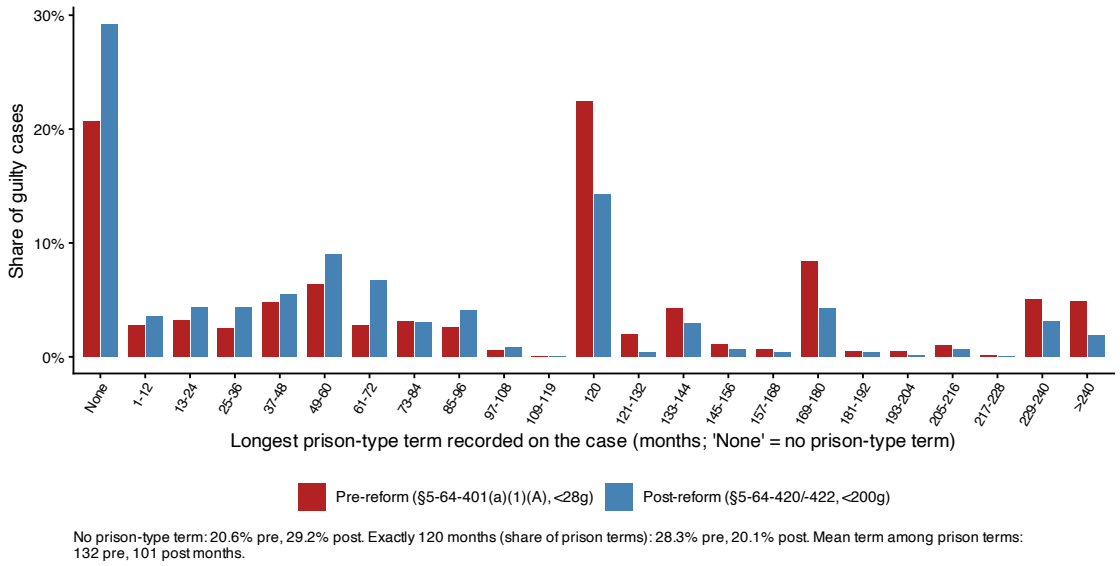
Appendix Figure A.1: Statutory first stage: monthly charge counts

Notes: Monthly number of charges filed under the old mandatory-minimum drug manufacture/sale statute and under the post-reform analog statutes, by filing date. The dashed vertical line marks the July 2011 implementation of Act 570.



Appendix Figure A.2: Recorded prison terms relative to coded mandatory-minimum floors (pre-reform, conditional on positive prison term)

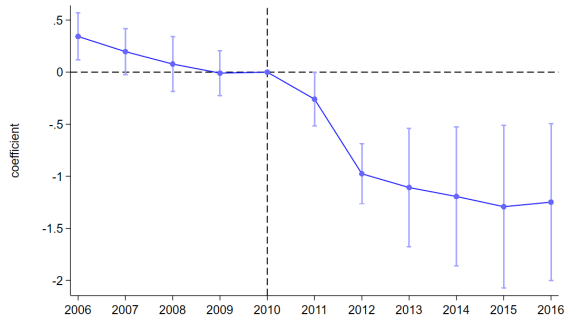
Notes: The sample includes pre-reform §5-64-401(a) cases, 2006–2010, with at least one mandatory-minimum-eligible charge that resolved guilty and a recorded prison-type term. Cases are deduplicated so that each case appears once. Panels are shown for the three statutory-floor categories with at least 150 cases: 3 years, 10 years, and 40 years. The statutory floor is assigned from the eligible charge's filed statutory subsection under former §5-64-401(a). Sentence length is measured as the longest prison-type term recorded anywhere on the case because the court records do not reliably assign sentence terms to individual charges. Dashed vertical lines mark the coded statutory floor for each tier. Bars to the left of the line show cases with recorded prison terms below the coded floor; bars at or to the right show cases meeting or exceeding it. Panel-specific percentages report the share of cases at or above the coded floor. Y-axis scales differ across panels. Unlike Figure 2, which reports shares of all mandatory-minimum-eligible charges, this figure conditions on a guilty disposition and a recorded prison-type term and is constructed at the case level. Accordingly, the percentages reported in the panel headings are conditional shares and are not directly comparable to the unconditional disposition shares in Figure 2. For comparison, among guilty eligible charges with a recorded prison-type term in Figure 2, 63.2 percent meet or exceed their own coded floor.



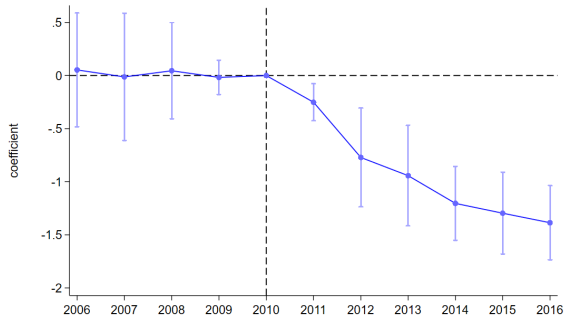
Appendix Figure A.3: Distribution of prison-type terms before and after repeal, matched methamphetamine, cocaine, and heroin cases

Notes: Each bar is the share of guilty cases in the matched sample whose longest recorded prison-type term falls in the listed range (months), in 12-month bins with exactly 120 months shown separately; "None" denotes cases with no prison-type term recorded. The pre-reform sample (filed before July 1, 2011; $N = 8{,}508$) consists of cases with a guilty disposition on a charge under former §5-64-401(a)(1)(A)(i) (Schedule I/II narcotic or methamphetamine under 28 grams, with a 10-year mandatory minimum). The post-reform sample (filed on or after July 1, 2011; $N = 8{,}277$) consists of cases with a guilty disposition on a analog charge under §5-64-420(b)(1)–(3) or §5-64-422(b)(1)–(3) (methamphetamine, cocaine, or heroin under 200 grams). Among cases with a prison-type term, the mean term falls from 132 to 101 months and the share at exactly 120 months falls from 28.3 to 20.1 percent. The prison-type term is the longest recorded anywhere on the case because the records do not reliably assign sentence terms to individual charges, and drug weights are inferred from the filed charge code. The comparison is descriptive and has no control group.

Appendix Figure A.4: Event-study estimates using alternative offense groups



(a) Max charge severity: fraud



(b) Max charge severity: procedural



(c) Prison sentence length: fraud



(d) Prison sentence length: procedural

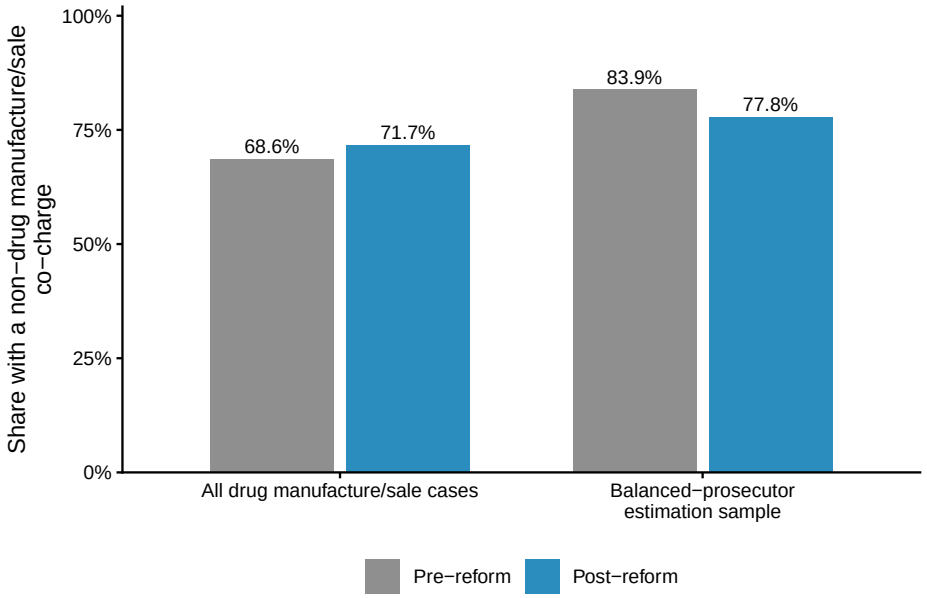


(e) Incarceration: fraud



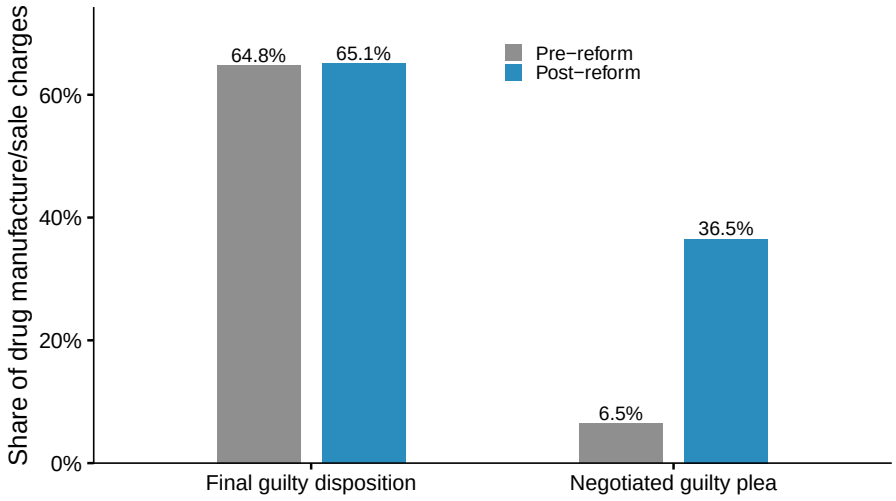
(f) Incarceration: procedural

Notes: This figure considers the policy impacts shown in Figure 3, but now utilizes the difference-in-differences strategy outlined in Equation 3 with corresponding estimates reported in Appendix Table B.12. Each panel presents event-study estimates comparing drug cases with the indicated comparison offense group. Points report the estimated coefficients and vertical bars report confidence intervals. The figures are used to assess whether changes in the main outcomes around Act 570 are specific to drug cases rather than reflecting contemporaneous changes in other criminal cases.



Appendix Figure A.5: Prevalence of mixed drug manufacture/sale cases

Notes: The figure reports the share of drug manufacture/sale cases containing at least one non-drug manufacture/sale co-charge, separately before and after the July 2011 reform. The first pair uses all drug manufacture/sale cases; the second restricts to cases assigned to prosecutors in the balanced panel used in the prosecutor analysis. These mixed cases provide the variation used in the within-case charge-resolution design.



Appendix Figure A.6: Guilty dispositions and negotiated guilty pleas before and after reform

Notes: The sample includes drug manufacture/sale charges, defined as pre-reform §5-64-401(a)/(b) mandatory-minimum drug manufacture/sale charges and post-reform analog drug manufacture/sale charges (§5-64-420 and related statutes). Final guilty disposition is measured from the charge disposition field. Negotiated guilty plea is measured from the charge-level plea field.

B Additional Tables

Appendix Table B.1: Sentencing severity context for Arkansas offenses

Offense category	Seriousness rank	Sentencing range	Mandatory imprisonment / floor
First-degree murder	10	Class Y: 10–40 years or life	Prison required under Class Y rules
Second-degree murder	9	Class A: 6–30 years	Prison required by statute
Aggravated robbery	8	Class Y: 10–40 years or life	Prison required under Class Y rules
Pre-reform MM drug offense: Schedule I/II narcotic or meth manufacture/delivery/PWID, 200–400g	8	20–40 years or life	20-year drug-specific floor
Pre-reform MM drug offense: Schedule I/II narcotic or meth manufacture/delivery/PWID, $\geq$ 400g	8	40 years or life	40-year drug-specific floor
Pre-reform MM drug offense: Schedule I/II narcotic or meth manufacture/delivery/PWID, $<$ 28g	7	10–40 years or life	10-year drug-specific floor
Pre-reform MM drug offense: Schedule I/II narcotic or meth manufacture/delivery/PWID, 28–200g	7	15–40 years or life	15-year drug-specific floor
Manslaughter	6	Class C: 3–10 years	No separate offense-specific floor

Notes: The table compares familiar Arkansas offenses to the pre-reform mandatory-minimum drug offenses studied in the paper. Seriousness ranks refer to Arkansas Sentencing Commission seriousness levels, where higher values indicate more serious offenses. General felony sentencing ranges are defined in Arkansas Code §5-4-401. For Class Y felonies and second-degree murder, imprisonment was required under Arkansas sentencing law. The drug rows summarize former §5-64-401(a)(1), which imposed quantity-specific prison floors for manufacture, delivery, or possession with intent to manufacture or deliver Schedule I/II narcotic drugs or methamphetamine. The comparison offenses are included to illustrate the severity of the statutory bargaining threat, not to equate drug offenses with homicide or violent offenses.

Appendix Table B.2: Statutory transition for core Arkansas drug manufacture/sale offenses

Drug / conduct category	Pre-reform regime	Post-reform analog regime	Main empirical coding
Methamphetamine, cocaine, or heroin possession with purpose to deliver	Former §5-64-401(a): offense-specific sentencing ranges with statutory imprisonment floors that varied by drug type and quantity.	§5-64-420: quantity-based felony classes. Less than 2g is Class C; 2g to less than 10g is Class B; 10g to less than 200g is Class A.	Treated drug manufacture/sale charge
Delivery of methamphetamine or cocaine	Former §5-64-401(a): manufacture, delivery, or possession with intent to deliver charged under the old mandatory-minimum drug statute.	§5-64-422: quantity-based felony classes. Less than 2g is Class C; 2g to less than 10g is Class B; 10g to less than 200g is Class Y.	Treated drug manufacture/sale charge
Manufacture of methamphetamine or cocaine	Former §5-64-401(a): drug manufacture/sale charged under the old mandatory-minimum drug statute.	§5-64-423: post-reform manufacture offense; less than 2g is Class C, with higher classifications for larger quantities or prior manufacture convictions.	Treated drug manufacture/sale charge
Other Schedule I–V delivery or possession with purpose to deliver	Former §5-64-401(a): offense-specific sentencing ranges with statutory imprisonment floors that varied by schedule and quantity.	§5-64-426, §5-64-430, §5-64-434, and §5-64-438: post-reform analog provisions for delivery or possession with purpose to deliver, organized by drug schedule and quantity.	Treated drug manufacture/sale charge
Simple possession	Former §5-64-401(c): simple possession provisions.	§5-64-419: post-reform possession provisions.	Coded separately from treated drug manufacture/sale charges

Notes: This table summarizes the statutory transition relevant to the empirical design. The treated charge universe consists of old-regime manufacture, delivery, and possession-with-intent or purpose-to-deliver charges under former §5-64-401(a), together with post-reform analog statutes. Simple possession is coded separately because it did not have the same pre-reform mandatory-minimum exposure and is used mainly in the charging-composition analysis. Act 570 added post-reform provisions including §5-64-420, §5-64-422, and §5-64-423. Arkansas felony classes carry general sentencing ranges of 10–40 years or life for Class Y, 6–30 years for Class A, 5–20 years for Class B, 3–10 years for Class C, and up to 6 years for Class D.

Appendix Table B.3: Theoretical response margins after mandatory-minimum repeal

Response margin	Prediction or empirical implication after repeal	Test in the paper
Conviction / trial margin	If mandatory minimums increased defendants' willingness to plead guilty by raising trial risk, repeal should reduce guilty conviction rates or increase trial rates.	Compare guilty conviction rates for drug manufacture/sale charges before and after reform; estimate exposure-gradient regressions using bench or jury trial as the outcome.
Prison sentence length	If repeal weakens prosecutorial leverage and prosecutors cannot replace the lost statutory threat, prison exposure may fall for affected cases.	Estimate policy effects on prison months and incarceration; test sentence concessions and exposure-gradient effects on prison sentences among imprisoned drug manufacture/sale guilty-plea cases.
Charge-stacking margin	Prosecutors may replace lost statutory leverage by filing more charges per case.	Estimate exposure-gradient regressions using the number of charges filed on the case as the outcome.
Charge-dropping margin	Prosecutors may continue to file severe charges but dismiss them more often as part of charge bargaining.	Estimate exposure-gradient regressions using the nolle prosequi share among multi-charge guilty-conviction cases.
Charge-resolution / plea-form margin	If mandatory minimums changed bargaining leverage without changing the overall guilty rate, repeal may change which charge becomes the vehicle of conviction and the formal mode through which guilty outcomes are reached.	Study within-case charge resolution, conditional sentencing type, negotiated guilty pleas, and prosecutor exposure gradients.

Notes: The table summarizes the response margins considered in the prosecutor analysis. Standard plea-bargaining models emphasize conviction probabilities, sentence lengths, and plea/trial tradeoffs. The court records additionally allow us to observe charge-level dispositions, sentence types, and whether a guilty plea is formally recorded as negotiated. The plea-form margin does not reveal the sequence of plea offers or counteroffers; it is an observed court-record measure of how guilty outcomes were formally resolved.

Appendix Table B.4: Summary statistics: drug cases

	(1) All Cases	(2) Pre-Reform	(3) Post-Reform
Male	0.730	0.766	0.694
White	0.644	0.591	0.697
Age	33.86	33.05	34.67
Max charge severity (1–11)	8.330	8.761	7.907
Number of charges per case	2.874	2.683	3.062
Any guilty plea	0.787	0.790	0.785
Pr(Incarceration)	0.436	0.452	0.421
Prison months (capped)	31.01	35.87	26.25
Probation months	17.30	16.07	18.51
6-month recidivism	0.137	0.124	0.149
3-year recidivism	0.313	0.271	0.355
Any MM-eligible charge	0.398	0.431	0.366
# of drug cases	113606	56214	57392

Notes: Cases with at least one drug charge, 2006–2016. Charge severity represents the level of the most severe charge and is coded 1–11, with 11 denoting the most severe felony class.

Appendix Table B.5: Policy effects on drug cases

Outcome	Coefficient	p-value (conv.)	p-value (robust)	Eff. N (L/R)
Max charge severity	-0.469***	0.000	0.000	20325 / 17739
Prison months	-4.495***	0.000	0.004	20325 / 17739
Pr(Incarceration)	-0.020**	0.033	0.076	20325 / 17739

Notes: Each row gives a separate RD regression using a different dependent variable; all regressions use the sample of cases with at least one drug charge. Max charge severity refers to the highest offense class within a case, which is on an 11-point scale. All regressions use a 24-month bandwidth and allow for 48 months of bias correction for the robust p-value. All regressions are local linear and use a uniform kernel. Standard errors are clustered by month. Conventional p-values are shown in the third column; bias-corrected robust p-values (Calonico et al., 2014) are shown in the fourth. Effective N reports the number of observations within the MSE-optimal bandwidth to the left and right of the cutoff. Significance stars are based on the conventional p-value.

Appendix Table B.6: Comparing manufacture/sale and possession cases

Outcome	Coefficient	p-value (conv.)	p-value (robust)	Eff. N (L/R)
Panel A: Manufacture/Sale Cases				
Charge severity	-0.470***	0.002	0.012	6601 / 3420
Prison months	-7.614**	0.023	0.073	6601 / 3420
Pr(Incarceration)	-0.004	0.859	0.875	6601 / 3420
Panel B: Possession Cases				
Charge severity	-0.360***	0.000	0.000	10918 / 12086
Prison months	-2.174*	0.082	0.074	10918 / 12086
Pr(Incarceration)	-0.015	0.214	0.201	10918 / 12086

Notes: This table replicates the main results for manufacture/sale and possession cases separately. Panel A gives the results for manufacture/sale cases and Panel B for possession cases. Note that cases with both types of charges are omitted in this analysis. All regressions use a uniform kernel and a 24-month bandwidth and allow for 48 months of bias correction for the robust p-value. Standard errors are clustered by month.

Appendix Table B.7: Bandwidth robustness test

Outcome	Optimal	12-month	36-month
Charge severity	-0.616***	-0.162	-0.594***
	(0.084)	(0.100)	(0.089)
	[40.2]	[12]	[36]
Prison months	-4.476***	-3.626**	-3.660***
	(1.579)	(1.709)	(1.280)
	[21.4]	[12]	[36]
Pr(Incarceration)	-0.019*	-0.026**	-0.004
	(0.011)	(0.013)	(0.009)
	[16.2]	[12]	[36]

Notes: This table considers the main discontinuity outcomes across three alternative bandwidth choices: the optimal bandwidth, 12 months, and 36 months. Standard errors are presented in parentheses and the bandwidth value is presented in brackets. All regressions use a uniform kernel and local linear form. Significance stars are based on the conventional p-value.

Appendix Table B.8: Kernel robustness test

Outcome	Uniform	Triangular	Epanechnikov
Charge severity	-0.469***	-0.297***	-0.349***
	(0.105)	(0.108)	(0.107)
Prison months	-4.945***	-3.563**	-3.742***
	(1.371)	(1.456)	(1.444)
Pr(Incarceration)	-0.020**	-0.019*	-0.019*
	(0.009)	(0.011)	(0.010)
N (L/R)	20325 / 17739	19457 / 16879	19457 / 16879

Notes: This table considers the main discontinuity outcomes across triangular and Epanechnikov kernels. Column (1) presents the uniform kernel for comparison; these estimates are similar to Table B.5. Standard errors are presented in parentheses and the bandwidth value is presented in brackets. All regressions use a 24-month bandwidth and local linear form. Significance stars are based on the conventional p-value.

Appendix Table B.9: Placebo test: alternate timings

Outcome	Coefficient	p-value (conv.)	p-value (robust)	Eff. N (L/R)
Panel A: 3 Years Before Act 570				
Charge severity	-0.027	0.638	0.083	20433 / 20922
Prison months	1.362	0.460	0.853	20433 / 20922
Pr(Incarceration)	0.007	0.460	0.939	20433 / 20922
Panel B: 3 Years After Act 570				
Charge severity	-0.036	0.301	0.009	18723 / 24505
Prison months	-1.211	0.425	0.303	18723 / 24505
Pr(Incarceration)	-0.006	0.653	0.533	18723 / 24505

Notes: This regression replicates Table B.5 using alternate timings for the policy change. Panel A considers three years before Act 570, July 2008, and Panel B considers three years after Act 570, July 2014. Each row gives a separate RD regression using a different dependent variable. All regressions use a 24-month bandwidth and allow for 48 months of bias correction for the robust p-value. All regressions are local linear and use a uniform kernel. Standard errors are clustered by month. Significance stars are based on the conventional p-value.

Appendix Table B.10: Robustness to anticipatory effects

Outcome	Coefficient	p-value (conv.)	p-value (robust)	Eff. N (L/R)
Panel A: Excluding Four Months Before Implementation				
Charge severity	-0.569***	0.000	0.000	16925 / 17739
Prison months	-6.837***	0.000	0.003	16925 / 17739
Pr(Incarceration)	-0.028**	0.017	0.113	16925 / 17739
Panel B: Four-Month Donut Around Implementation				
Charge severity	-0.832***	0.000	0.000	16925 / 14953
Prison months	-7.682***	0.000	0.003	16925 / 14953
Pr(Incarceration)	-0.029**	0.038	0.112	16925 / 14953

Notes: This table replicates Table B.5 but omits months with potential anticipatory effects. Panel A omits the four months before the policy, which is the period between when Act 570 was announced and went into effect. Panel B omits four months on either side of the policy. Each row gives a separate RD regression using a different dependent variable. All regressions use a 24-month bandwidth and allow for 48 months of bias correction for the robust p-value. All regressions are local linear and use a uniform kernel. Standard errors are clustered by month. Significance stars are based on the conventional p-value.

Appendix Table B.11: Co-occurrence between drug cases and candidate control groups, pre-reform

Offense Type	Cases Overlapping with Drug	Share of Drug Cases (%)
Property	2,573	4.58
Violent	1,174	2.09
Fraud	661	1.18
Procedural	550	0.98
Total Drug Cases	56214	100.00

Notes: Counts of pre-reform drug cases that also contain at least one charge in the listed category. Percentages are relative to 56,214 pre-reform drug cases.

Appendix Table B.12: Main difference-in-differences estimates

	(1)	(2)	(3)
Charge severity	-1.050***	-0.830***	-1.032***
	(0.201)	(0.0489)	(0.228)
Prison months	-8.956***	-7.020***	-9.083***
	(1.751)	(1.098)	(1.939)
Pr(Incarceration)	-0.0739***	-0.0349***	-0.0783***
	(0.0195)	(0.00899)	(0.0193)
Control group	proc. + fraud	procedural	fraud
Drug cases	107301	109320	108730
Control cases	49562	14634	35402

Notes: Each cell is a separate DiD regression using Equation (3). Controls in all specifications: defendant age, age squared, and sex; county and year fixed effects. Cases overlapping between the drug and control category are excluded. Standard errors clustered by county. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Appendix Table B.13: Policy effects on recidivism

Outcome	Coefficient	p-value (conv.)	p-value (robust)	Eff. N (L/R)
6-month recid.	-0.009	0.143	0.086	20325 / 17739
1-year recid.	-0.005	0.528	0.318	20325 / 17739
2-year recid.	-0.009	0.430	0.299	20325 / 17739
3-year recid.	-0.022**	0.048	0.028	20325 / 17739

Notes: Each row gives a separate RD regression using a different dependent variable. All regressions use a 24-month bandwidth and allow for 48 months of bias correction for the robust p-value. All regressions are local linear and use a uniform kernel. Standard errors are clustered by month. Significance stars are based on the conventional p-value.

Appendix Table B.14: Crime and arrest counts

	(1) Drug Crime per 100k	(2) Drug Arrests per 100k
Panel A: RD in Time		
Discontinuity	-0.652	-2.000
	(2.792)	(2.469)
Pre-policy mean	56.83	50.94
Observations (L/R)	5592/5825	5592/5825
Panel B: Difference in Differences		
DiD	0.725	-2.309
	(3.588)	(3.010)
Pre-policy mean	56.83	50.94
Observations	44820	44820

Notes: This table presents estimates of changes in drug incidents and drug-related enforcement following the policy using agency-month NIBRS data. Panel A uses an RDiT design with a 24-month bandwidth, a uniform kernel, and a local linear specification. Panel B uses a difference-in-differences design comparing Arkansas agencies with agencies in Oklahoma, Tennessee, and Texas. The DiD regressions include agency and month fixed effects, with standard errors clustered at the agency level. Only agencies with NIBRS coverage in each year are included. Significance stars are based on conventional clustered standard errors.

Appendix Table B.15: Effects of Act 570 on sentencing outcomes by previously recorded criminal history

Outcome	Coefficient	p-value (conv.)	p-value (robust)	Eff. N (L/R)
Panel A: No Past Cases				
Charge severity	-0.405***	0.000	0.002	12690 / 10547
Prison months	-6.265***	0.000	0.000	12690 / 10547
Pr(Incarceration)	-0.025**	0.020	0.037	12690 / 10547
Panel B: Any Past Cases				
Charge severity	-0.574***	0.000	0.000	7635 / 7192
Prison months	-3.098	0.309	0.718	7635 / 7192
Pr(Incarceration)	-0.012	0.448	0.673	7635 / 7192

Notes: The table reports subgroup regression-discontinuity-in-time estimates for drug cases, separately for defendants with no previously recorded court cases (Panel A) and at least one previously recorded case (Panel B). Criminal history is constructed from Arkansas court records beginning in 2006. Estimates use a 24-month bandwidth around the July 2011 implementation date and local linear specifications. Conventional and robust bias-corrected p-values are reported separately. Effective sample sizes are shown to the left and right of the cutoff. Statistical significance within a subgroup does not establish that the two subgroup effects differ.

Appendix Table B.16: Effects of Act 570 on subsequent case initiation by previously recorded criminal history

Outcome	Coefficient	p-value (conv.)	p-value (robust)	Eff. N (L/R)
Panel A: No Past Cases				
6-month recid.	-0.026***	0.000	0.000	12690 / 10547
1-year recid.	-0.030***	0.000	0.000	12690 / 10547
3-year recid.	-0.050***	0.000	0.000	12690 / 10547
Panel B: Any Past Cases				
6-month recid.	0.014	0.338	0.407	7635 / 7192
1-year recid.	0.032*	0.061	0.098	7635 / 7192
3-year recid.	0.020	0.321	0.470	7635 / 7192

Notes: The table reports subgroup regression-discontinuity-in-time estimates for the probability of a subsequent court case within the indicated horizon. Panel A includes defendants with no previously recorded cases, and Panel B includes those with at least one. Criminal history is based on court records available from 2006 onward. Estimates use a 24-month bandwidth around July 2011. Conventional and robust bias-corrected p-values are reported separately. These outcomes measure subsequent case initiation, not underlying offending.

Appendix Table B.17: Differences in sentencing discontinuities by previously recorded criminal history

	(1) Max charge severity	(2) Prison months	(3) Pr(Incarceration)
Any prior - No prior	-0.169***	3.167	0.0128
	(0.0587)	(3.272)	(0.0187)
N	38064	38064	38064

Notes: The reported coefficients are differences in the estimated policy discontinuity between defendants with any previously recorded court case and those with none (Any prior minus No prior). Positive coefficients indicate a more positive, or less negative, policy effect among defendants with prior cases. Standard errors are in parentheses. Unlike comparisons of subgroup-specific significance levels, these estimates directly test for heterogeneity in the policy discontinuity.

Appendix Table B.18: Differences in recidivism discontinuities by previously recorded criminal history

	(1) 6-month recid.	(2) 1-year recid.	(3) 2-year recid.	(4) 3-year recid.
Diff. in disc. effect	0.0398**	0.0619***	0.0718***	0.0696***
	(0.0175)	(0.0202)	(0.0252)	(0.0217)
N	38064	38064	38064	38064

Notes: Coefficients report the difference in the policy discontinuity for subsequent case initiation between defendants with any previously recorded case and those with none (Any prior minus No prior). A positive coefficient indicates a larger reduction, or smaller increase, in subsequent case initiation among defendants without prior recorded cases. Standard errors are in parentheses. The comparison concerns the effects of reform, not differences in baseline recidivism rates between the groups.

Appendix Table B.19: Sensitivity of recidivism heterogeneity to fixed criminal-history lookback windows

	6 months	1 year	2 years	3 years
Panel A: Three-year criminal-history lookback				
Any prior − No prior	0.0230	0.0426**	0.0569**	0.0505**
	(0.0173)	(0.0210)	(0.0216)	(0.0218)
Panel B: Five-year criminal-history lookback				
Any prior − No prior	0.0443**	0.0632***	0.0828***	0.0759***
	(0.0201)	(0.0228)	(0.0241)	(0.0230)
Observations	38,064	38,064	38,064	38,064

Notes: Each coefficient is the difference in the estimated policy discontinuity for subsequent case initiation between defendants with any and no previously recorded court cases (Any prior minus No prior). Prior cases are classified using a three-year lookback in Panel A and a five-year lookback in Panel B. Standard errors are in parentheses. $^{**}p < 0.05$, $^{***}p < 0.01$. Arkansas court records begin in 2006. The three-year lookback is fully observable throughout the baseline 24-month bandwidth around July 2011. The five-year lookback is left-censored for earlier observations and is included as supplementary sensitivity evidence.

Appendix Table B.20: Illustrative prison expenditure equivalents of reduced imposed sentences

Assumed sentence-to-time-served pass-through (s)	25%	50%	75%
Steady-state annual expenditure equivalent ($ millions)	21	42	63
As share of FY2011 care and custody spending ($304m)	7%	14%	21%

Notes: A scenario calculation, not an estimate of realized fiscal savings. It applies the estimated average decline in imposed prison months per drug case to approximately 10,300 annual drug cases and values assumed time served avoided at the FY2011 average operating cost. Actual budgetary savings depend on sentence-to-time-served pass-through, the timing of prison-population changes, avoidable marginal costs, and potential changes in probation and other public spending. Source for correctional cost and FY2011 care-and-custody expenditures: Arkansas Department of Correction (2015).

Appendix Table B.21: Prosecutors by pre-reform exposure to the mandatory-minimum regime

	High exposure		Low exposure	
	Pre	Post	Pre	Post
Panel A: Prosecutors				
Prosecutors	65		130	
Mean pre-reform exposure Z_p (share MM-eligible)	0.44		0.03	
Cases per prosecutor (mean / median)	242.7 / 115		121.7 / 25	
Panel B: All counties				
Charges per case (mean)	3.19	3.14	3.84	3.51
Case has drug manufacture/sale charge	35.2%	35.3%	18.3%	35.8%
Any prison-type sentence	51.5%	34.7%	46.4%	41.9%
Any probation	42.1%	40.7%	62.7%	46.5%
Any guilty disposition	84.1%	79.5%	87.2%	80.5%

Notes: The sample is the balanced panel of 195 prosecutors with at least one drug case before and one after July 1, 2011. Cases are case-by-defendant records with at least one drug charge and an identified prosecutor, filed 2006–2016. Exposure Z_p is the prosecutor's pre-reform share of drug cases that were mandatory-minimum eligible, set to zero for prosecutors with fewer than five pre-reform drug cases; "high exposure" denotes Z_p above the median across cases in the panel (0.22). Panel A pools both periods. Panel B describes the cases handled by each group before and after the reform; the court record identifies the prosecutor on only a subset of cases, and that share varies across counties and over time, so changes between periods partly reflect which cases are observed. "Drug manufacture/sale charge" is a former §5-64-401(a)/(b) mandatory-minimum charge or a post-reform analog charge.

Appendix Table B.22: Tests of standard prosecutorial response margins

	Charge Stacking (1)	Charge Dropping (2)	Prison Length (3)	Trial Margin (4)
Post × Z_p	−0.1826	−0.0086	6.002	0.0129
	(0.1434)	(0.0082)	(6.281)	(0.0562)
Prosecutor FEs	✓	✓	✓	✓
County × Year FEs	✓	✓	✓	✓
Pre-reform mean	4.026	0.182	64.00	0.043
Observations	27,669	20,345	9,437	27,669
R^2	0.091	0.321	0.172	0.485

Notes: Each column reports a separate regression testing a standard prosecutorial response margin. In all four columns, Z_p is the standardized pre-reform share of a prosecutor's drug cases that were mandatory-minimum eligible. Column (1) uses the full balanced-panel drug-case sample and measures charge stacking as the number of charges filed on the case. Column (2) restricts to cases with at least two charges and at least one guilty conviction; the outcome is the share of charges on the case that were nolle prossed. Column (3) is a charge-level regression restricted to drug manufacture/sale charges with a guilty disposition (charge disposition field, not the plea field, so the sample includes trial convictions as well as guilty and nolo contendere pleas); the outcome is prison sentence length in months. Column (4) uses an indicator for whether the case involved a bench or jury trial, based on the trial-type field. All specifications include prosecutor fixed effects and county-by-year fixed effects. Standard errors are clustered by prosecutor.

Appendix Table B.23: Sensitivity of Appendix Table B.22 to alternative definitions of Z_p

Exposure measure Z_p	Corr. with paper Z_p	Charge stacking (1)	Charge dropping (2)	Prison length (3)	Trial margin (4)
(a) Share of drug cases with an old-MM charge (paper)	1.00	−0.175	−0.0091	6.00	0.026
		(0.142)	(0.0079)	(6.28)	(0.070)
(b) Share of drug cases with an old-MM charge dropped	0.70	−0.121	−0.0074	4.60	0.038
		(0.155)	(0.0071)	(6.46)	(0.062)
(c) Share of old-MM charges dropped	−0.02	−0.119	−0.0130	7.97	0.045
		(0.176)	(0.0094)	(8.50)	(0.070)
(d) Share of drug charges that are old-MM charges	0.96	−0.069	−0.0081	4.67	0.044
		(0.131)	(0.0075)	(5.83)	(0.071)

Notes: Each row re-estimates the four adjustment margins with Z_p redefined from pre-reform cases (standardized within the estimation sample. Each model includes prosecutor and county-by-year fixed effects, and standard errors are clustered by prosecutor. Prosecutors with fewer than five pre-reform drug cases get $Z_p = 0$, as in the paper. "Dropped" = nolle prosequi or dismissed. Row (a) differs slightly from the main table because Z_p is standardized within this sample.

Appendix Table B.24: Drug manufacture/sale charge resolution premium by period

	2006–2008	2006–2009	2010	Jul 2011–2016
Panel A: all charges				
Negotiated Guilty	0.1006***	0.0776***	0.0404*	0.1338***
	(0.0131)	(0.0110)	(0.0225)	(0.0119)
Guilty Disposition	0.1515***	0.1363***	0.2211***	0.1844***
	(0.0211)	(0.0174)	(0.0326)	(0.0173)
Nolle Prosequi	−0.0837***	−0.0708***	−0.1102***	−0.1327***
	(0.0093)	(0.0133)	(0.0299)	(0.0099)
Dismissed	−0.0444**	−0.0429**	−0.0701***	−0.0324***
	(0.0203)	(0.0170)	(0.0226)	(0.0082)
Observations	6,246	8,190	1,178	27,472
Panel B: charges with guilty disposition				
Any Prison	−0.0231*	−0.0171	−0.0162	−0.0427***
	(0.0116)	(0.0104)	(0.0337)	(0.0082)
Any Probation	0.0834***	0.0804***	0.0883***	0.1316***
	(0.0181)	(0.0173)	(0.0327)	(0.0202)
Observations	3,653	4,768	584	13,317

Notes: Each cell reports the coefficient on the drug manufacture/sale charge indicator from a separate regression estimated within the listed period only, with case and county fixed effects (no year trend, since a trend cannot be fit within the single year 2010). Panel A: all charges on mixed cases among balanced-panel prosecutors. Panel B: restricted to charges with a guilty disposition. This table reports the level of the drug manufacture/sale charge premium directly in each period, rather than an event-study coefficient relative to a single reference year. The change reported in Appendix Table B.25 and Appendix Table B.26 is the difference between the post-reform and pre-reform premium. The single year 2010 rests on few charges and is noisy. Standard errors clustered by prosecutor. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Appendix Table B.25: Within-case charge resolution and sentencing: drug manufacture/sale charges relative to co-charges

	A. Charge disposition		B. Plea	C. Sentencing (unconditional)		
	Nolle Prosequi (1)	Guilty Disposition (2)	Negotiated Guilty (3)	Any Prison (4)	Any Probation (5)	Probation Only (6)
Post × Drug manufacture/sale charge	−0.0542***	0.0318	0.0627***	−0.0219	0.0489**	0.0643***
	(0.0119)	(0.0195)	(0.0168)	(0.0155)	(0.0228)	(0.0203)
Case FEs	✓	✓	✓	✓	✓	✓
Pre-reform mean (drug manufacture/sale charges)	0.266	0.701	0.346	0.374	0.305	0.263
Observations	37,299	37,299	37,299	37,299	37,299	37,299

Notes: Sample: charges on mixed cases among balanced-panel prosecutors, 2006–2016, containing both a drug manufacture/sale charge and at least one other charge. Each column is a separate regression of a charge-level indicator; all outcomes are unconditional on disposition. Panel A uses final dispositions, Panel B the plea field, and Panel C sentences. Case fixed effects included (which subsume county-by-year main effects). Every specification also includes the drug manufacture/sale charge main effect (the pre-reform premium), which is not reported. The reported coefficient is the interaction of post-reform status and drug manufacture/sale charge, i.e., the change in the drug manufacture/sale charge premium after reform. Standard errors clustered by prosecutor. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Appendix Table B.26: Within-case sentencing conditional on a guilty disposition

	Headline outcomes		Decomposition		
	Any Prison (1)	Any Probation (2)	Prison Only (3)	Prison + Probation (4)	Probation Only (5)
Post × Drug manufacture/sale charge	−0.0225*	0.0473**	−0.0008	−0.0217***	0.0690***
	(0.0121)	(0.0230)	(0.0137)	(0.0078)	(0.0175)
Case FEs	✓	✓	✓	✓	✓
Pre-reform mean (drug manufacture/sale charges)	0.534	0.432	0.472	0.062	0.370
Observations	18,860	18,860	18,860	18,860	18,860
R^2	0.687	0.692	0.689	0.549	0.689

Notes: Sample: charges on mixed cases among balanced-panel prosecutors, 2006–2016, restricted to charges with a guilty disposition. Prison-Only, Prison + Probation, and Probation-Only are mutually exclusive indicators (prison with no probation component; prison and probation; probation with no prison component), since Arkansas sentences can combine both. Case fixed effects included (which subsume county-by-year main effects). Every specification also includes the drug manufacture/sale charge main effect (the pre-reform premium), which is not reported. The reported coefficient is the interaction of post-reform status and drug manufacture/sale charge, i.e., the change in the drug manufacture/sale charge premium after reform. Standard errors clustered by prosecutor. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Appendix Table B.27: Within-case charge resolution and sentencing on all mixed cases, without requiring an identified prosecutor

	A. Charge disposition		B. Plea	C. Sentencing (unconditional)		
	Nolle Prosequi	Guilty Disposition	Negotiated Guilty	Any Prison	Any Probation	Probation Only
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Drug manufacture/sale charge	−0.0245**	0.0145	0.0956***	−0.0003	0.0366***	0.0375***
	(0.0115)	(0.0156)	(0.0224)	(0.0101)	(0.0108)	(0.0106)
Case FEs	✓	✓	✓	✓	✓	✓
County-specific premium	✓	✓	✓	✓	✓	✓
Pre-reform mean (drug manufacture/sale charges)	0.302	0.634	0.072	0.333	0.206	0.185
Observations	121,519	121,519	121,519	121,519	121,519	121,519

Notes: Same outcomes as Appendix Table B.25, all unconditional on disposition. Sample: every case containing both a drug manufacture/sale charge and at least one other charge, 2006–2016 (121,519 charges), whether or not the prosecutor is identified. Case fixed effects, the drug manufacture/sale charge main effect, and county-by-drug-manufacture/sale-charge fixed effects (which let the pre-reform premium differ by county) are included. Standard errors clustered by county. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Appendix Table B.28: Within-case test: change in the drug manufacture/sale charge premium in negotiated guilty, by comparison group

	Any Co-charge	All but Possession	Possession-only
	(1)	(2)	(3)
Post × Drug manufacture/sale charge	0.0627***	0.0614***	0.0388**
	(0.0168)	(0.0181)	(0.0188)
Case FEs	✓	✓	✓
Observations	37,299	33,592	6,870
R^2	0.579	0.588	0.656

Notes: Outcome: indicator for negotiated guilty plea. Sample: charges on mixed cases among balanced-panel prosecutors, 2006–2016. Column (1): cases containing both a drug manufacture/sale charge and at least one other charge, with all non-drug manufacture/sale co-charges as the comparison group. Column (2): cases containing both a drug manufacture/sale charge and a non-possession, non-drug manufacture/sale charge, with the comparison restricted to non-possession co-charges. Column (3): cases containing both a drug manufacture/sale charge and a possession-type charge, with the comparison restricted to those two charge types. Possession is defined as §5-64-401(c) pre-reform or §5-64-419(b) post-reform. Case fixed effects are included; they subsume county-by-year main effects. Every specification also includes the drug manufacture/sale charge main effect (the pre-reform premium), which is not reported; the reported coefficient is the change in the premium after reform. Standard errors are clustered by prosecutor.

Appendix Table B.29: Within-case charge resolution by comparison group: guilty disposition, nolle, dismissed, prison, probation

	Any Co-charge	All-but-Possession	Possession-only
Guilty Disposition	0.0318	0.0275	0.0450*
	(0.0195)	(0.0229)	(0.0247)
Nolle Prosequi	−0.0542***	−0.0531***	−0.0760***
	(0.0119)	(0.0120)	(0.0223)
Dismissed	0.0165	0.0190	0.0155***
	(0.0161)	(0.0192)	(0.0054)
Any Prison	−0.0219	−0.0217	0.0139
	(0.0155)	(0.0157)	(0.0171)
Any Probation	0.0489**	0.0426*	0.0365**
	(0.0228)	(0.0232)	(0.0153)
Case FEs	✓	✓	✓
Observations	37,299	33,592	6,870

Notes: Outcome as row header, comparison group as column header; see Table B.28 for the definition of each comparison group. Sample: charges on mixed cases among balanced-panel prosecutors, 2006–2016. Case and county-by-year fixed effects included in every column. Every specification also includes the drug manufacture/sale charge main effect (the pre-reform premium), which is not reported; each coefficient is the change in the premium after reform. Standard errors clustered by prosecutor. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Appendix Table B.30: Dose-response DiD: negotiated guilty plea rate

	Negotiated Guilty (any)	
	(1) Baseline	(2) Preferred
post × Z_p	0.1812***	0.0613**
	(0.0313)	(0.0244)
Prosecutor FEs	✓	✓
Year FEs	✓	
County FEs	✓	✓
County × Year FEs		✓
Pre-reform mean	0.550	0.550
Observations	27,669	27,669
R^2	0.287	0.304

Notes: Outcome: indicator for any negotiated guilty plea on the case. Z_p is each prosecutor's pre-reform share of drug cases that were MM-eligible, standardized. Column (1) includes prosecutor, year, and county fixed effects. Column (2) is the preferred specification with prosecutor fixed effects and county-by-year effects. Controls include defendant race, gender, age, case type, and number of charges. Standard errors clustered by prosecutor. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Appendix Table B.31: Falsification: dose-response DiD on drug vs. alternative offense groups

	Drug manufacture/sale	Fraud/Procedural
	(1)	(2)
Post × Z_p	0.061**	0.044
	(0.024)	(0.030)
Prosecutor FE	Yes	Yes
County × year FE	Yes	Yes
Observations	27,669	11,993
R^2	0.304	0.254

Notes: Outcome: indicator for any negotiated guilty plea on the case. Z_p is each prosecutor's pre-reform share of drug cases that were MM-eligible, standardized. Drug manufacture/sale columns use affected drug manufacture/sale cases. Fraud/procedural column restrict to pure non-drug cases to avoid contamination from charge co-occurrence. All specifications include prosecutor fixed effects, county-by-year effects, and controls for defendant race, gender, age, case type, and number of charges. Standard errors clustered by prosecutor.

Appendix Table B.32: Robustness of the exposure-gradient result to judge controls

	Preferred	Judge × Year	Judge × Year + County × Year†	Two-Way FE
	(1)	(2)	(3)	(4)
Post × Z_p	0.0613**	0.0673**	0.0420**	0.2104***
	(0.0244)	(0.0262)	(0.0211)	(0.0395)
Prosecutor FEs	✓	✓	✓	✓
County × Year FEs	✓		✓	
Judge × Year FEs		✓	✓	
Judge FEs				✓
Year FEs				✓
Observations	27,669	27,665	27,665	25,451
R^2	0.304	0.334	0.359	0.297

Notes: Outcome: indicator for any negotiated guilty plea on the case. Z_p is each prosecutor's pre-reform share of drug cases that were MM-eligible, standardized. Column (1) is the preferred specification from Table B.30. Column (2) replaces county-by-year fixed effects with judge-by-year fixed effects. Column (3) includes both judge-by-year and county-by-year fixed effects; †the fixed-effects demeaning algorithm does not converge reliably in this specification, so this estimate is reported for completeness but should be treated as inconclusive. Column (4) is a two-way fixed-effects specification with static prosecutor, judge, and year fixed effects, estimated on the largest connected component of the prosecutor–judge bipartite network (150 of 184 prosecutors, 64 of 90 judges). Controls include defendant race, gender, age, case type, and number of charges. Standard errors clustered by prosecutor. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Appendix Table B.33: Validity tests: does negotiated guilty predict observable case differences?

	Log Duration (1)	Co-charge Nolle (2)	Any Probation (3)	Prison > 120 mo. (4)
Negotiated Guilty (any)	0.1084*	−0.0282	0.2541***	−0.0324**
	(0.0550)	(0.0484)	(0.0469)	(0.0163)
Prosecutor FEs	✓	✓	✓	✓
County FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Mean of dep. var. (post-reform)	5.855	0.522	0.440	0.044
Observations	21,058	18,149	21,187	8,301
R^2	0.201	0.233	0.131	0.109

Notes: Each column is a separate regression of the listed outcome on an indicator for any negotiated guilty plea on the case. Sample: post-reform drug manufacture/sale cases, balanced-panel prosecutors. Log duration and co-charge nolle prosequi specifications include county and year fixed effects; the probation and prison > 120 months specifications add prosecutor fixed effects. Column (4) is conditional on a positive prison sentence. Controls include defendant race, gender, age, number of charges, and case type. Standard errors clustered by prosecutor. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.