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The Impact of New Mexico's 2024 Pretrial Rule Reforms (Rules 5-401 and 5-403, NMRA) on Detention Length: A Multi-Method Causal Analysis

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Introduction

New Mexico implemented changes to its pretrial detention procedures in 2024 through Senate Bill 271, titled "Repeat Felony Offender No Bond Hold," which was signed into law by Governor Michelle Lujan Grisham on March 4, 2024. The legislation introduced automatic detention requirements for individuals who commit new felony offenses or enumerated high-risk misdemeanors while already released on pretrial conditions, representing a departure from previous practices that required individualized judicial determinations for each detention decision.

The procedural changes were operationalized through revisions to New Mexico Rules of Criminal Procedure 5-401 and 5-403, which took effect on May 8, 2024. These rule changes introduced a series of procedural modifications related to pretrial detention. Rule 5-401 mandated automatic review hearings for defendants who remained in custody 24 hours after bond conditions were set due to inability to post secured bond or meet release conditions, with hearings required within five days of the initial conditions hearing. The rule also established expedited priority scheduling for detained defendants and required status review hearings every six months for cases where defendants were held for extended periods. Rule 5-403 created procedures for revocation or modification of release conditions, including automatic detention without conditions for defendants who commit new felonies or enumerated misdemeanors while on pretrial release, with mandatory initial hearings within three to five days, depending on detention facility location.

In this report, we evaluate the causal impact of these rule changes on release length of stay (LOS) for individuals with qualifying charges using multiple causal inference methods. We begin by reviewing the characteristics of our sample and then discuss our methodological approach, which combines propensity score matching, interrupted time series, difference-in-differences, and survival analyses to provide triangulating evidence of rule change effects. Our results section presents findings from each methodological approach, including subgroup analyses examining potential differential impacts across demographic groups and case types. We conclude by discussing the limitations and implications of our findings.

Sample Description

We constructed our sample from booking records maintained by the Bernalillo County Metropolitan Detention Center (MDC), spanning from November 8, 2023, through July 31, 2025. To focus the analysis on cases most relevant to the 5-401/5-403 rule change, we restricted the sample to individuals who met specific criteria. Specifically, inclusion in our sample required that individuals have both a qualifying criminal charge (i.e., that they were booked on a felony or enumerated misdemeanor) and an associated case warrant, indicating they had committed new criminal activity while on pretrial release. Qualifying charges included felonies of any degree (F1 through F4) or enumerated misdemeanors as defined by the rule change, encompassing offenses such as battery, aggravated battery, domestic violence-related charges, stalking, violation of protection orders, criminal sexual contact, harassment, DUI, and related offenses[1].

[1] Enumerated misdemeanors included the following New Mexico statutory offenses: battery (NMSA 30-3-4), aggravated battery (NMSA 30-3-5(B)), negligent use of deadly weapon (NMSA 30-7-4), battery against household member (NMSA 30-3-15), aggravated battery against household member (NMSA 30-3-16), stalking (NMSA 30-3A-3), violation of protection order (NMSA 40-13-6), criminal sexual contact (NMSA 30-9-12), harassment (NMSA 30-3A-2), driving under the influence (NMSA 66-8-102), and boating under the influence (NMSA 66-13-3). These offenses represent violent crimes, domestic violence-related charges, sexual offenses, and impaired driving violations that were specifically designated under the legislation as warranting enhanced pretrial detention consideration.

We reduced the sample to a six-month window before and after the effective rule change implementation date of May 8, 2024, spanning the subset of bookings with arrival dates from November 8, 2023, to November 8, 2024. We constructed the pretrial violation flag hierarchically at the case level, identifying any booking episode where an individual had both a qualifying charge (i.e., a felony or enumerated misdemeanor) and a corresponding warrant type (i.e., felony warrant, misdemeanor warrant, or remand/retake status). Our final sample size for our primary analyses, except for the difference-in-differences design, consisted of 6,899 individuals with complete data, 2,985 from the pre-intervention period and 3,914 from the post-intervention period, excluding cases with missing release dates, which primarily occurred in more recent months due to ongoing detention, while the difference-in-differences approach additionally incorporated all other bookings during the same period as a comparison group.

In Table 1, we summarize the characteristics of individuals within our sample. The demographic and offense characteristics of the sample were broadly similar between the pre-rule change and post-rule change periods. Both periods showed a consistent gender distribution with approximately three-quarters male (73.9% pre-rule change vs. 72.2% post-rule change) and approximately one-quarter female (26.1% vs. 27.8%). Racial and ethnic composition were similarly consistent across periods, with Hispanic individuals representing the largest group (45.8% pre-rule change vs. 44.7% post-rule change), followed by White individuals (31.7% vs. 30.8%), and Indigenous individuals (9.8% vs. 10.3%). Black individuals comprised 7.9% of the pre-rule change sample and 7.8% of the post-rule change sample, while Asian individuals represented a small but consistent proportion (0.4% in both periods). The age distribution showed modest changes between periods, with the 25-34 age group increasing slightly from 34.9% to 36.1%, while the 18-24 age group decreased from 12.5% to 10.4%, and the 35-44 age group remained relatively stable (34.3% to 34.9%). Primary charge severity remained largely consistent, with approximately 80% of cases involving felony charges in both periods (79.7% pre-rule change vs. 81.5% post-rule change) and about 20% involving misdemeanor charges (20.3% vs. 18.5%). The proportion of cases involving enumerated misdemeanors was stable at relatively low levels, representing approximately 9% of all cases in both periods (9.5% pre-rule change vs. 8.9% post-rule change).

Table 1. Descriptive Statistics of Analysis Sample (n = 6,899)

Characteristic	Category	Pre-Policy N (%)	Post-Policy N (%)
Sex	Female	780 (26.1%)	1089 (27.8%)
	Male	2205 (73.9%)	2824 (72.2%)
	Unknown	0 (0.0%)	1 (0%)
Race/Ethnicity	Asian	11 (0.4%)	17 (0.4%)
	Black	237 (7.9%)	306 (7.8%)
	Hispanic	1367 (45.8%)	1749 (44.7%)
	Indigenous	294 (9.8%)	405 (10.3%)
	Other/Unknown	131 (4.4%)	230 (5.9%)
	White	945 (31.7%)	1207 (30.8%)
Age Group	18-24	372 (12.5%)	409 (10.4%)
	25-34	1042 (34.9%)	1413 (36.1%)
	35-44	1023 (34.3%)	1367 (34.9%)
	45-54	406 (13.6%)	523 (13.4%)
	55+	142 (4.8%)	202 (5.2%)
Primary Charge Severity	Felony	2379 (79.7%)	3189 (81.5%)
	Misdemeanor	605 (20.3%)	724 (18.5%)
	Other	1 (0%)	1 (0%)
Enumerated Misdemeanor	No	2700 (90.5%)	3565 (91.1%)
	Yes	285 (9.5%)	349 (8.9%)

Method Overview

We used multiple analytical approaches to evaluate whether the amended 5-401 and 5-403 rules implemented on May 8, 2024, causally impacted the LOS for individuals with inclusionary pretrial violations. We defined LOS as the number of days between an individual's booking and release.

An important methodological consideration was the timing of our analysis relative to data availability. We initially conducted an unpublished version of this analysis in December 2024 but found that many cases booked in late November 2024 had not yet been released, creating missing release date information and a right-censored (i.e., incomplete) post-rule change

sample. To avoid bias from incomplete follow-up that would impact our estimates of the LOS after the rule changes, we restricted our analysis to cases with complete release information. This meant that our effective post-rule change observation window was slightly shorter than six months, but ensured that our LOS calculations were based on actual completed stays rather than censored observations. This conservative approach prevented our results from being artificially inflated by cases that remained in custody at the time of analysis. Our preliminary findings from the analysis in December 2024 suggested that there were no significant changes in the mean or median LOS of the subset of individuals we sampled at that time.

However, one limitation of the December 2024 analysis was that it did not isolate the rule change's causal effect from other factors that might influence LOS. Simply comparing before-and-after averages in LOS across all individuals could be misleading if the types of cases or sample compositions (i.e., the characteristics of those with qualifying charges) changed over time, since these factors may also contribute to LOS. To get around these limitations, in the present report, we aimed to address these different potential sources of bias through four different methodological approaches[2].

- Our first approach used a method called propensity score matching (PSM) to create comparable treatment and control groups within the sample. PSM estimates the probability that each sampled individual would be in the post-rule change period based on observable characteristics like age, sex, race, violent crime classification, charge severity, and warrant status. We then matched cases from before and after the rule changes that had similar propensity scores, creating pairs that were alike but for their exposure to the rule changes. This matching was implemented using nearest-neighbor algorithms to ensure high-quality matches, and we assessed balance (i.e., the degree of similarity between the treatment and control groups) by examining whether socio-demographic and case characteristics were similar between the matched treatment and control groups after this pairing procedure.
- Our second approach used interrupted time series (ITS) analysis to examine how LOS evolved over time. We aggregated LOS data to weekly averages and fitted separate trend lines for the pre-rule change and post-rule change periods, allowing us to detect whether rule change effects emerged immediately or developed over time as the rule changes were implemented.
- Our third approach used a difference-in-differences (DiD) design that compared changes in LOS for our full sample against changes in average LOS for *all other bookings* over the same period. This approach is different from the PSM approach as it accounts for time trends that might impact LOS for all cases equally, such as seasonal patterns. The key assumption of the DiD approach is that pretrial violations and other bookings would have followed similar trends absent the rule change. By comparing the before-and-after difference for the 5-401-403 sample to the before-and-after difference for all other bookings, we can better see whether the rule change resulted in a different slope or degree of LOS relative to other types of bookings.
- Our fourth approach used survival analysis to examine LOS as a time-to-event outcome, treating detention length as the time until release rather than a simple average. This method uses Kaplan-Meier survival curves to visualize the probability of remaining in

[2] Throughout these analyses, we used robust statistical methods, including weighted regression for matched samples, cluster-robust standard errors to account for potential correlation in outcomes, and multiple model specifications to test the sensitivity of our findings. We evaluated statistical significance using conventional p-values while recognizing that policy significance depends on both statistical evidence and the practical magnitude of effects.

custody over time and Cox proportional hazards regression to estimate the relative rate of release between pre- and post-policy periods while controlling for individual characteristics. Survival analysis is particularly valuable because it appropriately handles cases that were still in custody at the end of our observation period (called "censoring"), provides insights into release patterns across the entire detention period rather than just average outcomes, and can detect whether policy effects vary at different points in the detention timeline.

To explore whether rule effects varied across different population segments, we also conducted subgroup analyses stratified by demographic and case characteristics. We performed separate PSM analyses for different subgroups defined by race (Black vs. non-Black, Hispanic vs. non-Hispanic, White vs. non-White), sex (male vs. female), crime type (violent vs. non-violent), warrant status (felony warrant vs. no felony warrant), and enumerated crime designation (enumerated vs. non-enumerated cases). This stratification approach tests whether the rule changes differentially impacted LOS across various demographic groups and case types, allowing us to identify whether certain populations experienced different treatment effects from the procedural changes implemented in Rules 5-401 and 5-403

Results

Unadjusted Mean and Median LOS

We first conducted an unadjusted comparison of LOS between the pre-rule change period (i.e., individuals booked between November 8, 2023, and May 7, 2024) and the post-rule change period (i.e., individuals booked between May 8, 2024, and November 8, 2024) for individuals with qualifying charges. Per the analysis results, we found that the mean LOS increased modestly from 34.5 ± 72.4 days before the rule change to 36.6 ± 69.9 days after the rule change ($t(6373.3) = 1.22$, $p = 0.223$, Cohen's $d = 0.029$), representing a negligible and statistically non-significant effect. The median LOS remained stable at 8 days in both periods, though the interquartile range expanded slightly from 4-22 days to 4-28 days, and the Mann-Whitney U test confirmed no statistically significant difference in medians between periods ($W = 6388702$, $p = 0.081$, $r = 0.021$). Levene's test revealed no significant difference in variances between the two periods ($F(1,7149) = 1.56$, $p = 0.212$), indicating similar variability in LOS during both the pre-rule change and post-rule change periods.

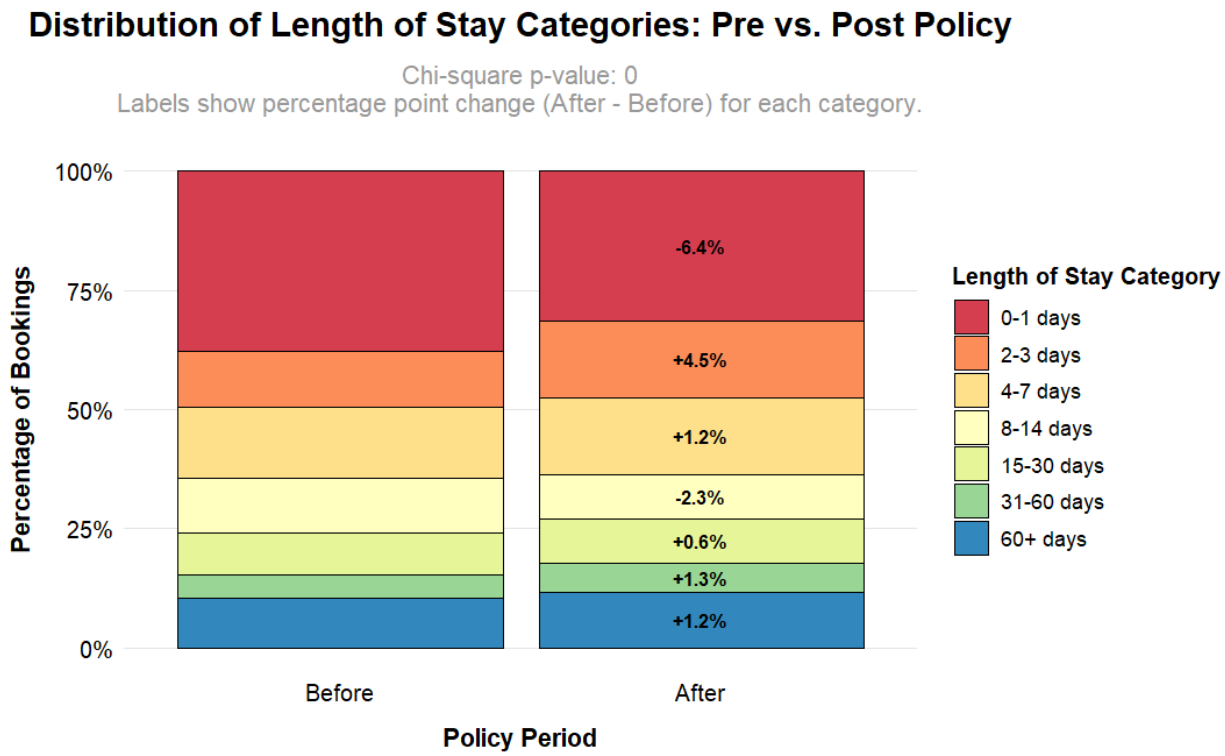
Table 2. Unadjusted LOS Comparison: Pre-Post Rule Changes

Measure	Before Rule Change	After Rule Change	Test Statistic	P-Value	Effect Size
Sample Size (N)	3021	4130	--	--	--
Mean ± SD	34.5 ± 72.4	36.6 ± 69.9	t(6373.3) = 1.22	0.223	d = 0.029
Median (IQR)	8 (4-22)	8 (4-28)	W = 6388702	0.081	r = 0.021
Range (Min-Max)	0-573	0-441	--	--	--

Note: SD = Standard Deviation; IQR = Interquartile Range (25th-75th percentile); Cohen's d: small = 0.2, medium = 0.5, large = 0.8; Mann-Whitney r: small = 0.1, medium = 0.3, large = 0.5; LOS measured in days from arrival to release.

In Figure 1, we present a stacked bar chart that shows the distribution of LOS categories, revealing nuanced shifts in booking duration patterns following the rule change, with a statistically significant overall change in the categorical distribution (chi-square p-value = 0.00). The largest change occurred in the shortest LOS category (0-1 days), which decreased by 6.4 percentage points from the pre-rule change to post-rule change period, suggesting fewer individuals were being released within the first day of booking. Conversely, the 2-3 day category experienced the largest increase at 4.5 percentage points, indicating a potential shift toward slightly longer but still brief detention periods. The middle-range categories showed mixed patterns, with the 8-14 day category decreasing by 2.3 percentage points while the 4-7 day and 15-30 day categories increased by 1.2 and 0.6 percentage points, respectively. The longer-term detention categories (31-60 days and 60+ days) both showed modest increases of 1.3 and 1.2 percentage points, respectively, suggesting a slight shift toward extended detention periods across the distribution. These categorical changes indicate that while the overall mean length of stay showed no significant change, the underlying distribution experienced some moderate restructuring, with fewer immediate releases and a more dispersed pattern of detention durations across multiple time categories.

Figure 1. Distribution of LOS Pre-Post Rule Change



However, as discussed previously, this unadjusted pre-post analysis is limited because it does not account for potential confounding factors such as changes in case composition, defendant characteristics, or other temporal trends that may have influenced LOS independent of the rule change implementation. For these reasons, we now turn to the discussion of our primary methodological approaches that statistically account for these factors.

Propensity Score Matching

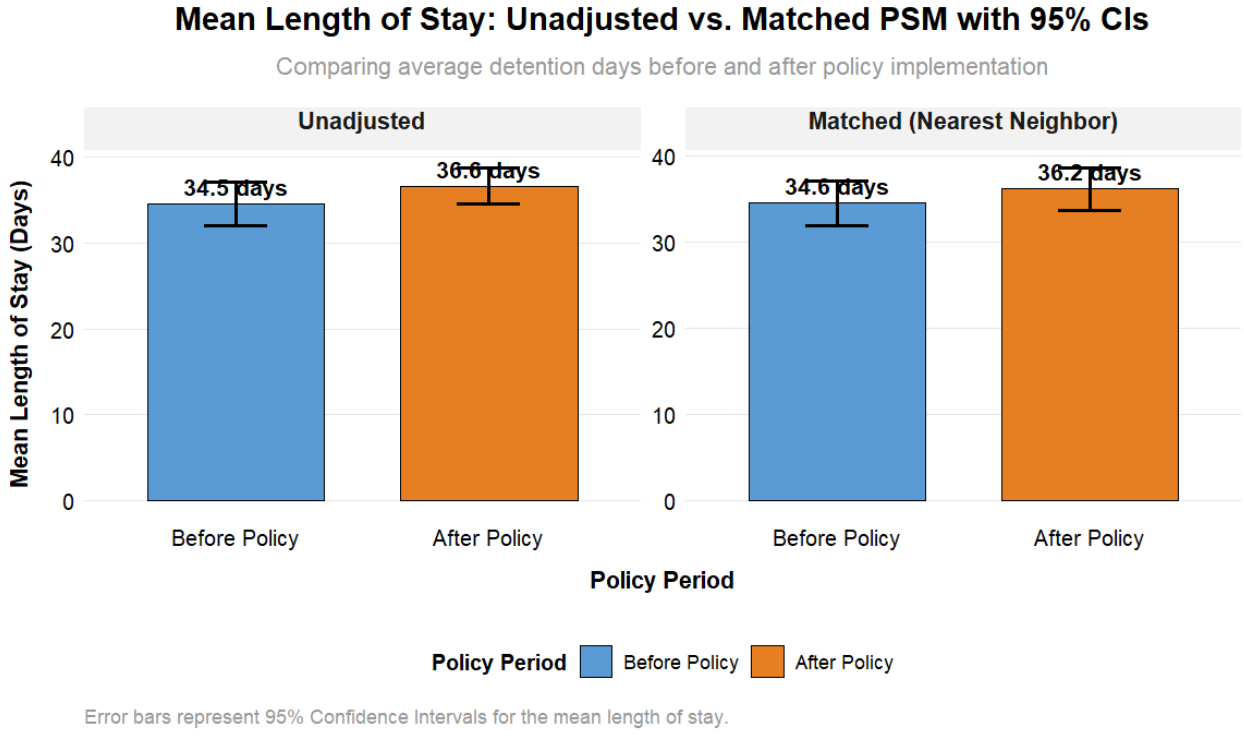
To address the limitations of the unadjusted pre-post comparison, we used PSM to create a more empirically defensible basis for understanding whether the rule change specifically caused changes in LOS. This approach identified cases from before and after the rule changes that were similar across key characteristics, including individual demographics (e.g., age, gender, race), case characteristics (e.g., charge type, whether booking charge was violent or not), and warrant status, and then compared outcomes only among these matched pairs. The nearest-neighbor matching procedure successfully created 2,925 matched pairs from the eligible sample. The matching process dramatically improved balance (i.e., similarity) between the comparison groups, with post-matching standardized mean differences falling below 0.10 for most variables.

The PSM analysis revealed a consistent pattern with the unadjusted comparison, showing virtually no meaningful difference in the estimated rule change effect on mean LOS. Among the balanced matched pairs, the average LOS increased from 34.6 days before the rule changes to 36.2 days after implementation, representing an increase of 1.6 days or approximately 4.6%. This effect was similar to the unadjusted analysis, which showed an increase of 2.1 days from 34.5 to 36.6 days, suggesting that differences in case characteristics and defendant demographics between the pre- and post-implementation periods were not

substantial confounders in this analysis. The matched sample analysis provides greater confidence that the observed modest increase in length of stay reflects the actual causal impact of the rule change, as the propensity score matching process effectively controlled for observable factors that could have otherwise biased the estimated treatment effect. The consistency between the unadjusted and matched estimates indicates that selection bias was minimal in the original comparison, and the negligible change in detention duration following policy implementation represents a genuine lack of substantive policy impact rather than an artifact of compositional differences between time periods.

Although the matched estimate did not reach conventional levels of statistical significance, the PSM approach offers greater confidence in the direction and magnitude of the effect by controlling for selection bias that may have influenced the raw pre-post comparison. The matched analysis suggests the rule changes produced a mild, yet not statistically significant, reduction in pretrial detention length for similar cases.

Figure 2. Mean LOS Pre-Post Implementation of 5-401/403 Rule Changes (PSM)



Temporal Patterns and Interrupted Time Series (ITS) Analysis

To further explore the rule change's impact, we conducted an interrupted time series (ITS) analysis that tracked LOS patterns over time rather than simply comparing before-and-after averages. For this approach, we aggregated bookings into weekly intervals and examined both the immediate level change and shifts in trend following rule implementation. By observing patterns across the full study period, the use of ITS can help us better parse whether the rule change produced an immediate effect, a gradual change in trajectory, or both.

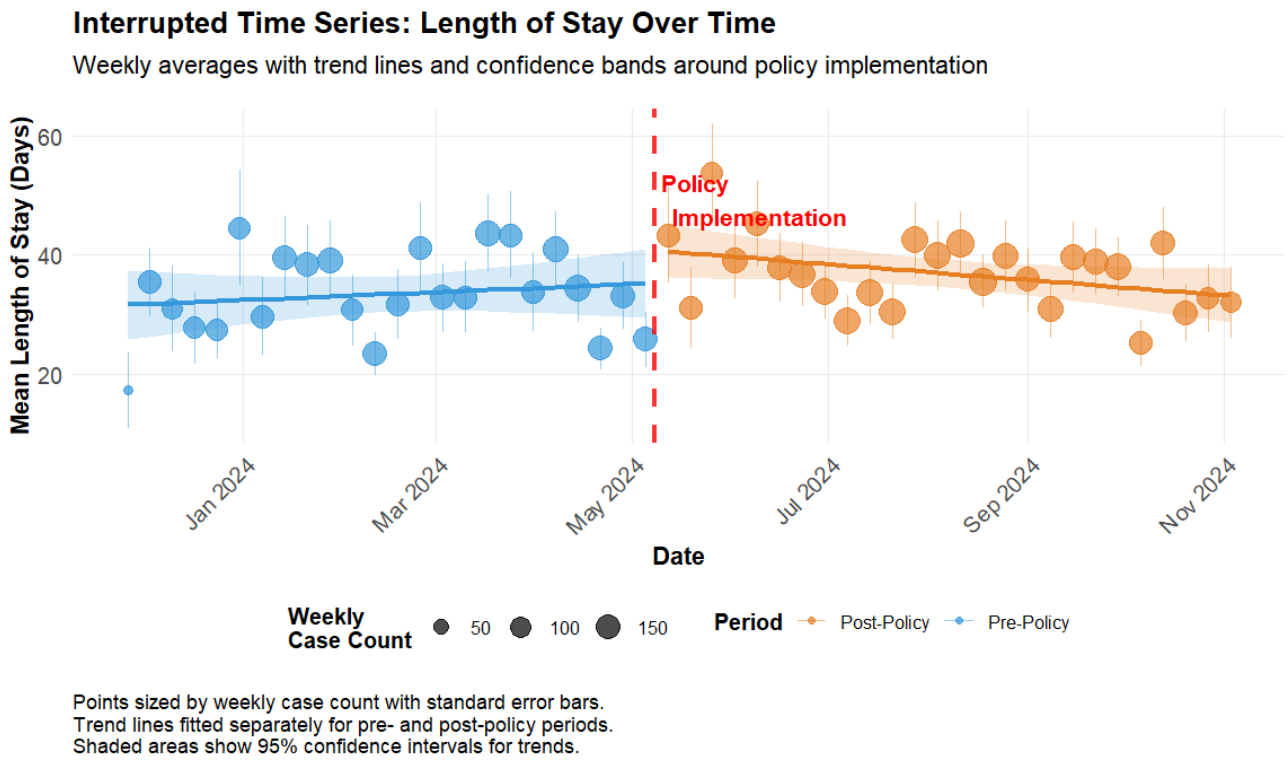
The ITS visualization reveals subtle patterns before and after rule implementation that largely confirm the findings from our unadjusted and matched analyses. Before implementation in May 2024, length of stay showed considerable weekly variation with averages ranging

approximately from 25 to 45 days, though the fitted trend line indicates a slightly increasing trajectory throughout the pre-implementation period. This pre-implementation pattern provides a baseline against which to evaluate post-implementation changes,. The confidence intervals around the pre-implementation trend suggest moderate uncertainty in the weekly estimates, reflecting both sampling variation and genuine changes in case composition over time.

Following rule implementation, the ITS plot shows a modest shift in both level and trajectory that aligns with the negligible effects observed in our comparative analyses. Post-implementation weekly LOS averages began around 40-42 days but showed a gentle downward trend, declining gradually to approximately 30-35 days for bookings in October and early November 2024. While this represents a declining trajectory compared to the stable pre-implementation pattern, the magnitude of change remains modest, and the confidence intervals around both trend lines overlap substantially, suggesting that any policy effect was minimal. The post-implementation period demonstrates similar variability to the pre-implementation period, indicating that the rule change did not dramatically alter the underlying variation in weekly detention patterns.

The ITS analysis provides context about change over time that complements our PSM analysis and reinforces the conclusion of minimal policy impact. While the PSM analysis estimated a 1.6-day increase among matched cases, the time series reveals that this finding reflects a genuine lack of substantial policy effect rather than masking more complex temporal dynamics. The modest downward trend observed in the post-implementation period, while statistically detectable, represents a gradual decline that may reflect broader systemic changes or natural variation rather than a direct causal effect of the rule implementation.

Figure 2. Mean LOS Pre-Post Implementation of 5-401/403 Rule Changes (ITS)



Difference-in-Differences

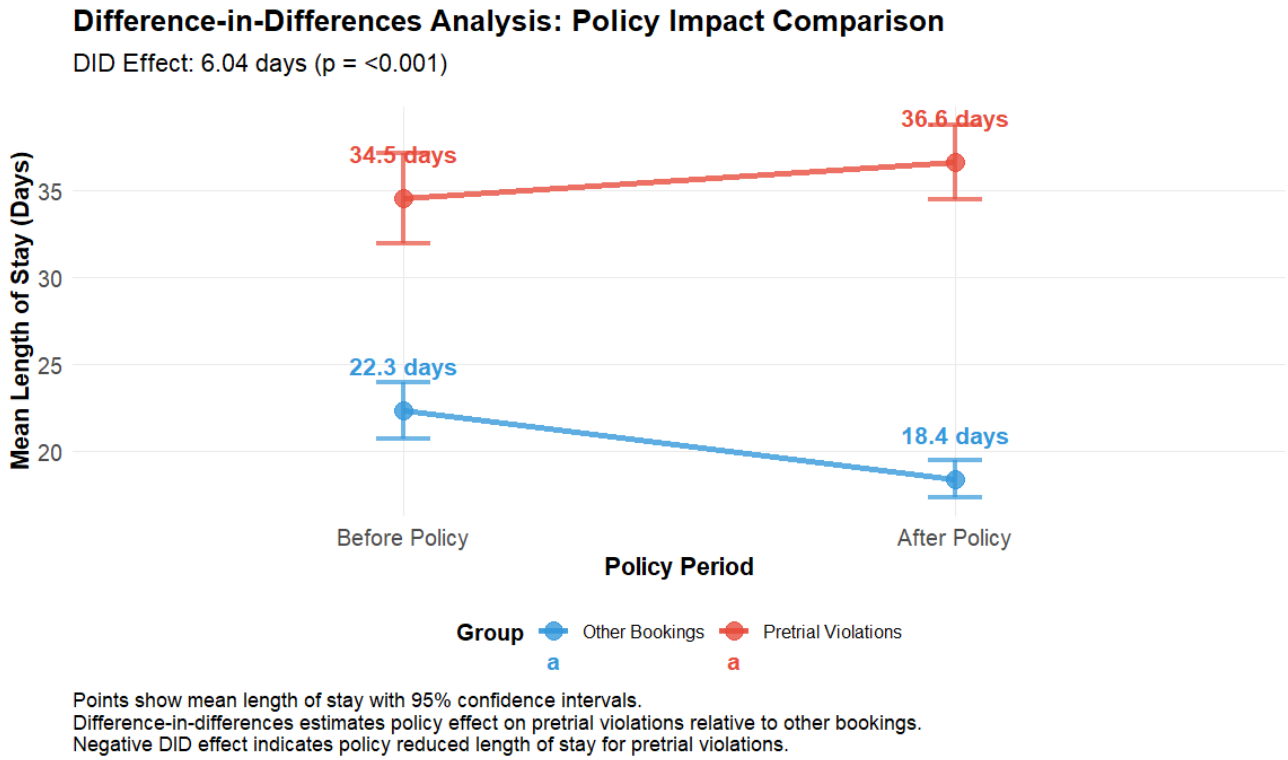
To address potential concerns about broader institutional changes affecting detention patterns (e.g., volume of bookings in any given month), we used a difference-in-differences (DID) approach that compared the subset of qualifying 5-401/5-403 cases to all other bookings over the same period. This method uses the LOS for all other bookings as a comparison group to control for time-varying factors that might affect detention length facility-wide, such as seasonal patterns, staffing changes, or other policy modifications. The DID approach estimates the rule change's specific impact by calculating whether qualifying pretrial violation cases experienced different degrees of change in their LOS compared to other bookings.

More concretely, the DID approach evaluates the average LOS for both groups before and after rule implementation, then calculates the "difference-in-differences," the extent to which the change in the average LOS for 5-401 and 5-403 eligible bookings before and after the rule change differs from the change in the LOS for all other bookings. This approach helps isolate the rule change's effect from other factors that might influence detention duration across all case types during the study period.

The DiD analysis reveals different patterns between qualifying cases and other bookings during the study period. Before rule implementation, qualifying cases averaged 34.5 days LOS compared to 22.3 days for all other bookings, indicating that individuals with qualifying charges experienced longer detention periods prior to the policy change. Following the rule change, the average LOS of pretrial violation cases increased to 36.6 days while the average LOS for other bookings decreased to 18.4 days, representing changes of +2.1 days and -3.9 days, respectively. This indicates that individuals with qualifying pretrial violation charges

experienced an additional 6.04 days of detention compared to other types of bookings ($p < 0.001$) and suggests that while detention lengths decreased facility-wide for other case types, pretrial violation cases experienced a marginal increase in detention duration relative to the other booking types.

Figure 3. Mean LOS Pre-Post Implementation of 5-401/403 Rule Changes (DiD)



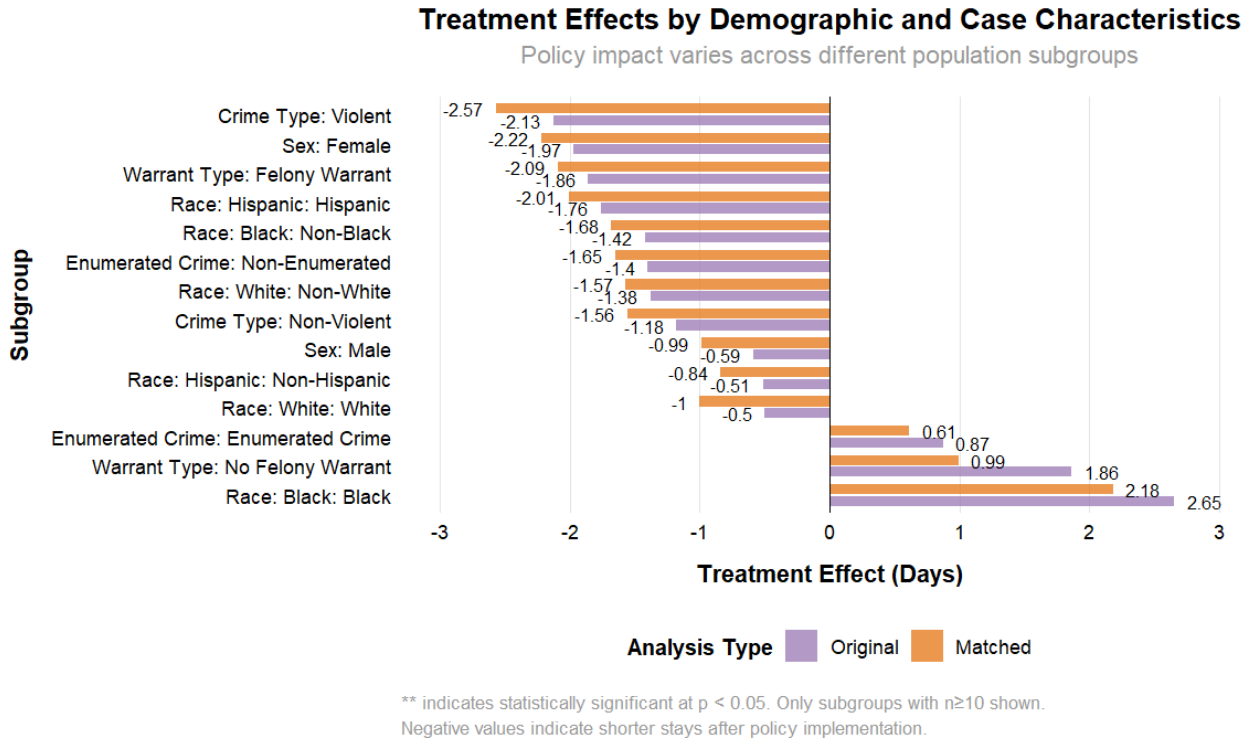
Evaluation of Effects Across Different Subgroups

To explore whether rule change impacts varied across different population segments, we conducted subgroup analyses by demographic characteristics (e.g., sex, race/ethnicity) and case characteristics (e.g., booking charge type, warrant status). This approach estimates treatment effects separately for each subgroup using both the original unmatched sample and the PSM matched sample.

This analysis revealed variation in estimated effects across different population segments, though importantly, none of the individual subgroup effects reached conventional levels of statistical significance ($p < 0.05$). The largest negative effects were observed among individuals with violent crime charges, showing an estimated LOS reduction of 2.57 days in the original sample and 2.13 days in the matched analysis. Females also had larger reductions in LOS (1.97 days original, 2.22 days matched) compared to males (0.99 days original, 0.59 days matched). Conversely, Black individuals showed positive treatment effects, indicating increased LOS following rule implementation (2.18 days original, 2.65 days matched), contrasting with reductions observed among non-Black individuals (1.68 days original, 1.42 days matched). Effects also varied by warrant type, with individuals having felony warrants experiencing larger reductions (2.09 days original, 1.86 days matched) compared to those without felony warrants, who showed small increases in LOS (0.99 days original, 1.86 days matched).

Having noted this, the lack of statistical significance means that we cannot adequately parse whether these subgroup differences, to the extent they were observed, represent actual differences in the rule change's causal impact or random noise. The subgroup analysis highlights that average effects may mask meaningful variation across population segments, but the absence of statistically significant findings suggests caution in concluding that the rule change had substantially different impacts across different groups.

Figure 4. Treatment Effect Estimates within Demographic Groups



** indicates statistically significant at p < 0.05. Only subgroups with n≥10 shown.
Negative values indicate shorter stays after policy implementation.

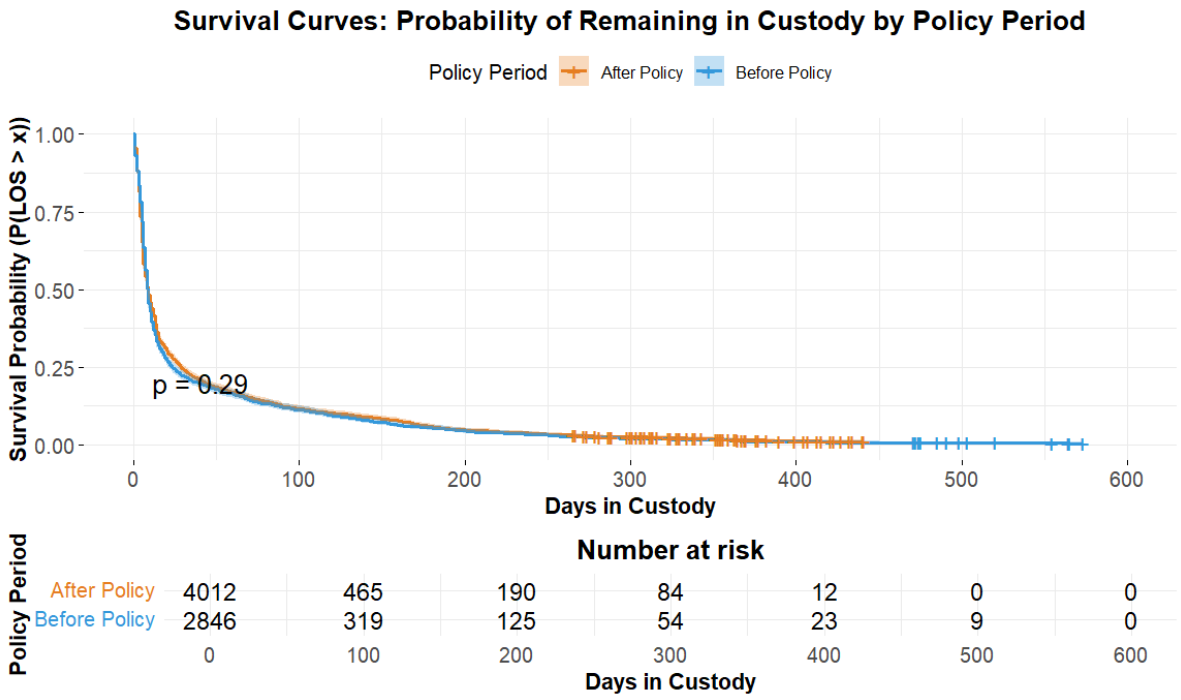
Survival Analysis

To complement our other approaches, we conducted survival analysis using Kaplan-Meier curves and Cox proportional hazards regression. This approach treats LOS as a time-to-event outcome, which means that it can appropriately handle administratively censored cases (i.e., those still in custody at the end of our observation period; i.e., cases without release dates as of August 1, 2025). The survival analysis estimated hazard ratios representing the relative rate of release between pre- and post-policy periods, with hazard ratios greater than 1.0 indicating faster release rates (i.e., shorter LOS) following the rule change.

The survival analysis revealed patterns consistent with the other analytical approaches, showing minimal policy impact on detention duration. The Cox proportional hazards model, which controlled for age, charge severity, and gender, estimated a hazard ratio of 0.974 for the treatment effect (95% CI: 0.928-1.022, p = 0.286), indicating that individuals in the post-policy period had a 2.6% lower release rate compared to the pre-policy period. This translates to slightly longer detention times following policy implementation, though the effect was not statistically significant. Among the control variables, charge severity emerged as the strongest predictor of release timing (HR = 0.825, p < 0.001), with higher severity charges associated with substantially longer detention periods, while male gender was also associated with slower release rates (HR = 0.831, p < 0.001).

The Kaplan-Meier survival curves provided visual confirmation of the minimal policy impact, showing nearly overlapping survival functions for the pre- and post-policy periods (log-rank test p = 0.29). Both periods demonstrated similar patterns of rapid initial releases, with approximately 50% of individuals released within the first few days, followed by more gradual release rates for longer-term detentions. The survival curves remained closely aligned throughout the observation period, with confidence intervals substantially overlapping, reinforcing the conclusion that the rule change did not produce meaningful changes in the timing of releases. The near-identical survival patterns across policy periods provide confirmatory evidence that the rule change did not alter the fundamental dynamics of detention duration for individuals with pretrial violation charges.

Figure x. Survival Curves Showing Probability of Remaining in Custody by Period



Discussion and Policy Implications

In this report, we evaluated the impact of New Mexico's May 8, 2024, pretrial rule changes (Rules 5-401 and 5-403) on length of stay for individuals with qualifying charges at the Bernalillo County MDC. Rules 5-401 and 5-403 established new procedural requirements for pretrial release hearings and revocation processes, including mandatory timelines for hearings, automatic review mechanisms for defendants unable to post bond, and enhanced procedural protections during revocation proceedings. Using multiple analytical approaches, we found no evidence that these procedural changes significantly changed detention length for the target population. The unadjusted analysis revealed a modest but statistically non-significant increase in mean LOS from 34.5 days to 36.6 days (an increase of 2.1 days, p = 0.223), with no change in median LOS between periods. Our propensity score matching analysis, which provides a more credible causal estimate by balancing observable characteristics between treatment and comparison groups, confirmed this pattern with matched cases experiencing an average increase of 1.6 days in LOS after the rule changes. The interrupted time series analysis showed relatively stable patterns both before and after implementation, with only modest changes in trajectory that fell within expected variation. Most notably, the difference-in-differences

approach revealed that pretrial violation cases experienced a statistically significant 6.04-day increase in detention relative to other bookings ($p < 0.001$), indicating that while detention lengths decreased facility-wide for other case types during the study period, individuals with 401-403 qualifying charges were not subject to this trend. The survival analysis corroborated these findings, showing no statistically significant changes in release rates following policy implementation (HR = 0.974, $p = 0.286$). Subgroup analyses revealed variation in estimated effects across demographic and case characteristics, though none reached statistical significance.

These findings should be understood within the context of broader trends documented in our May 2025 population review (Guerin & Ferguson, 2025). In that report, we noted that the MDC population increased 30.3% from 2019 through 2024, while national jail populations decreased 15.3% during the same period. This growth in the MDC has been driven primarily by increased bookings for felony charges and felony warrants, and a shift in population composition toward higher-severity cases. The absence of substantively large increases in mean or median LOS following 401-403 rule changes, coupled with the fact that 5-401-5-403 eligible cases only comprised 35.4% of total bookings, suggests that the rule change alone was insufficient to meaningfully alter MDC-wide detention patterns.

However, it is important to note that the analysis in this report has limitations that represent potential frontiers for future work. First, we lack information on unobserved system variables such as judicial and case processing backlogs, hearing scheduling patterns, and court capacity constraints that could independently influence detention length regardless of rule changes. For example, we did not have data on hearing frequency or timing, case processing timelines, or implementation fidelity measures such as judicial compliance with new procedures that would help isolate the specific effects of Rules 5-401 and 5-403 from other factors. Second, our analysis was limited to Bernalillo County and may not generalize to other jurisdictions or districts within New Mexico, where the same rule changes could have different impacts depending on local practices, resources, and implementation approaches. Future research incorporating system-level measures, examining implementation fidelity, and assessing longer-term follow-up periods could provide insights into whether these patterns reflect temporary implementation challenges or indicate that additional systemic reforms may be needed to achieve meaningful reductions in detention length.

In sum, this report provides preliminary evidence that the May 2024 Rules 5-401 and 5-403 did not produce significant changes in detention length for individuals with qualifying charges during the initial six-month implementation period.