Testing Competing Political Expectations of Bail Reform

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Since 2011, several states have implemented statewide bail reforms to reduce the reliance on assigning cash bail to defendants. Since then, political debates have developed around the competing expectations of these reforms. Political Action Committees (PACS) and predominantly Republican state politicians have adopted the view that bail reform increases violent crime and therefore, is a threat to public safety. Reformers and some Democratic state politicians take the position that bail reform does not affect violent crime and that it reduces the pretrial detention rate – those who are awaiting trial in jail. The literature is limited, with the majority focusing on an individual county and often observing alternatives assigned by a judge, but not a policy passed intended to change cash bail assignment behavior. In this study, I test the competing political expectations of bail reform, utilizing multivariate regressions and a Synthetic Control Method on data from 2000-2018. In both approaches, bail reform does not increase violent crime. Pretrial detention rates are more complex. The overall effect of bail reform increases pretrial detention, but the outcomes vary depending on the state. I make the case that implementation and contextual factors drive the effectiveness of bail reform policy goals. An alternative explanation is tested, finding that bail reform passage, instead of actual policy implementation, has a larger and more significant effect on pretrial rates. Additional hypotheses are generated based on these results to better understand judicial behavior in response to bail reform passage.

Keywords: Bail reform, Policy, Pretrial Detention, Violent Crime, Synthetic Control Method

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Introduction

Early positions on cash bail were framed through differing interpretations of the 8th Amendment to the U.S. Constitution. Broader readings implied "excessive bail shall not be required" to include a right to bail that also must not be excessive, while narrower readings claimed that bail was not a right or a policy requirement; it could simply not be excessive when utilized (Verilli, 1982; Bail Reform Act of 1984; United States v. Salerno, 1987). Over time, cash bail reliance increased and was observed to be a driver of incarceration and a contributor to racial disparities (Arnold, Dobbie, & Yang, 2018; Arnold, Dobbie, & Hull, 2022; Donnelly & MacDonald, 2018; Demuth & Steffensmeier, 2004; Free, 2004; Schaefer & Hughes, 2019). In 2014, Michael Brown was shot and killed by police in Ferguson, Missouri, spurring nationwide protests. These protests included wide arrests with high bail that raised public awareness. In 2015, the U.S. Department of Justice released a report on the region, calling out the court's bond procedures and claiming a focus on "maximizing revenue" over the protection of rights or administration of justice (U.S. DOJ, 2015). Protests over bail practices followed (St. Louis Post-Dispatch, 2018), and reforms gained traction as rights groups connected pretrial detention to being a major driver of mass incarceration (Kang-Brown, et al., 2018).

The stated policy goal of cash bail reform is to reduce the pretrial jail population, particularly those in jail because of an inability to pay. Opponents of reform state concern for released defendants committing violent crimes while awaiting trial. This framing has gained traction by PACs and political candidates, leading to the delay and reversal of bail reform policies in California, Illinois, New York, and Utah. The goal of this paper is to test these competing expectations of bail reform and to do so across states with enacted policies. Given the recency of these policy reforms, the literature is limited, with much of it focusing on a single

county. Further, the existing research regularly observes the impact of cash bail or alternatives on outcomes as assigned by a judge, but not an intentional state policy that shifts default reliance on cash bail. The intention is to isolate more generalizable findings compared to county-specific case studies and to generate additional hypotheses for future studies. Examining state-level data through regression analysis and a synthetic control method, bail reform is found to have no effect on violent crime rates. The Synthetic Control Method reveals different pretrial rate outcomes by state, introducing a theory of implementation for these differing results. Finally, when shifting to policy passage date, rather than policy implementation date, an effect is observed before the policy is implemented, suggesting judicial and court actor behavior changes worthy of future study.

Cash Bail Reform Overview

Several states and counties have reformed their cash bail system. This study will focus on states that have enacted statewide legislative reforms that apply to all criminal courts within the state. This allows for a longitudinal analysis with a clear policy intervention. Some prosecutor-driven reforms shift with elections, and even within the same tenure of prosecutor, due to evolving political and crime realities. Individual counties within states without bail reform also present opportunities for analysis, as several researchers have examined. However, the lack of uniformity and availability in data types create challenges for comparison. There is also a higher focus on urban regions in previous research, such as Philadelphia (Ouss & Stevenson, 2022; Stevenson, 2018), Orange County (Barno, et al., 2020), and Harris County (Heaton, 2022).

Table 1 provides an overview of state-level bail reform with the passage and enacted dates, reform process, and type. Washington, DC became the first to enact significant reform.

Table 1. Overview of State-Level Cash Bail Reforms

	Year Passed / Implemented	Reform Process	Bail Reform Type
Implemented:			
District of Columbia	1992 / 1993	State legislation.	Presumption of release without conditions. Judges cannot assign bail with the effect of pretrial detention.
Kentucky	2011 / 2012	State legislation.	Presumption of release with limits on when judge should assign cash bail and least restrictive conditions required.
New Mexico	2014 / 2017	2014 NM Supreme Court ruling/ 2016 Const. Amendment affirming / 2017 enacted.	Constitutional amendment prohibited setting unaffordable bail.
New Jersey	2014 / 2017	State legislation.	Presumption of release with limits on when judge can assign cash bail and least restrictive conditions required.
Nebraska	2017 / 2017	State legislation.	Presumption of release with least restrictive conditions required and ability to pay considered.
Vermont	2018 / 2019	State legislation.	Cash bail eliminated for certain misdemeanors and ability to pay must be considered when assigning bail.
West Virginia	2020 / 2021	State legislation.	Presumption of release with limitations on when a judge should assign cash bail.
Not Implemented:			
New York	2019 / Rolled back 2020	2019 state legislation, 2020 state legislation rolling back, 2021	Initially, cash bail was prohibited for misdemeanors and nonviolent felonies. In 2020, added to the list of crimes that judges can assign bail for, limiting the reform's effect.
California	2018/ Overturned in 2020 / 2021	2018 state legislation, 2020 referendum overturning, 2021 CA Supreme Court ruled system unconstitutional.	2021: Unaffordable bail is unconstitutional. No policy passage to enforce or clarity from upper court enforcing yet.
Illinois	2017 / 2018 2021 / 2023 (Ongoing repeal efforts but planned to go into effect Sept 2023)	2018 state legislation, 2021 state legislation, 2023 IL Supreme Court affirmed	2018: Cash bail cannot be "oppressive" and must consider ability to pay. 2023: Would abolish cash bail with ability for judges to deny release based on flight or safety risk.
Utah	2020 / Repealed in 2021	State legislation.	Presumption of release with limitations on when a judge should assign cash bail.

(Sources: Bail Reform Act of 2017; Huston, 2021; Jorgensen & Smith, 2021; Subramanian & Grawert, 2023).

Their reform led to a decline in pretrial detention reliance and the rare use of cash bail (Pretrial Services Agency, 2020). Now, a pretrial services department makes a recommendation to a judge within 48 hours of a charge. Other conditions are often applied to ensure court appearance outside of cash bail assignment. Since then, ten states have enacted unique versions of bail reform, though none are as expansive as DC. New Mexico's was initiated by a Supreme Court ruling stating the current system was unconstitutional, while all others began due to state legislation. Illinois' and California's Supreme Courts also weighed in after legislation was passed and challenged/overturned. Currently, eight states have a version of bail reform aimed at defaulting to release for certain crimes and requiring consideration of ability to pay when assigning cash bail. These states are – DC, Kentucky, New Mexico, New Jersey, Nebraska, Vermont, West Virginia, and Illinois.[1]

These reforms to cash bail reliance are supported by rights groups and politicians that tend to align more with the Democratic Party and/or progressive politics. For example, the Justice Policy Institute (2012) and the Prison Policy Institute (Sawyer & Wagner, 2023) point to cash bail disproportionately harming low-income and people of color. This aligns with what drove Harris County's changes by the Department of Justice under the Obama Administration (Council of Economic Advisors, 2015; Rosenberg, 2017). The U.S. Commission on Civil Rights, a bi-partisan government research group, but under the Biden Administration, pointed to cash bail as a primary driver for an increase in pre-trial detention over time – specifically a 433% increase from 1970 to 2015 (U.S. Commission on Civil Rights, 2022). Because most people who are held in jail pre-trial are there because they cannot afford to post bail (Leslie & Pope, 2017; Stevenson, 2018), it is believed that reducing or ending cash bail would decrease pre-trial

[1] Illinois has statewide reform in place, but subsequent reform that abolished the full system of cash bail is not currently in place as of the writing of this paper.

detention and result in reducing economic and racial disparities in pre-trial detention. As recently as 2022, The U.S. Commission on Civil Rights recommended that the federal government work with Congress to pass bail-free legislation.

Alternatively, a recent increase in violent crime over the pandemic resulted in Republican state lawmakers blaming bail reforms (Keck, 2022; McCullough, 2021; Ruiz, 2023). The bail bond industry has also claimed that reducing pretrial detention will increase crime rates (Wilson, 2018). Lawmakers and political candidates have adopted the language of "revolving door," connecting lax bail usage to people quickly returning to society and committing additional crimes (Abbott, 2023; Ortt, 2023). Three states have rolled back their reforms (See Table 1). New York and Utah passed legislation the following year, rolling back reforms before they could go into effect. In California, a ballot referendum revoked reforms passed by the state legislature the previous year. Illinois reformed its presumption of release in 2017 and moved to end cash bail entirely in 2020. However, legislation challenging the constitutionality of the reform has stalled its implementation and it continues to experience considerable pushback from state lawmakers (Bishop, 2023). In response to county-level reforms, Texas recently passed stricter requirements for applying cash bail and restricting judges from giving recognizance bonds, citing crime rates and people being released and going on to commit additional crimes (McCullough, 2021). Given these competing framings and recent policy conflicts, it is useful to understand the outcomes across and within states post-reform.

Hypotheses

The research question is: How does cash bail reform impact pre-trial detention and violent crime rates? I present two hypotheses based on advocate and opponent's stated

expectations of bail reform. For each, I present the advocate/opponent claim and support for their expectations.

Table 2. Testing Expectations of Bail Reform

Opponent Expectation for Bail Reform:	Advocate Expectation for Bail Reform:
Hypothesis 1: Reforming cash bail increases violent crime.	Hypothesis 2: Reforming cash bail reduces pre-trial detention.

Support for Hypothesis 1

Researchers have studied the association of non-bail release and re-arrest to measure cost to society and likelihood of committing additional crimes. When looking at a high-risk group, as determined by risk assessment tools used by judges, 8% of these defendants had a chance of rearrest for a new violent crime within six months (Mayson, 2018). Sardar (2018) found alternatives to cash bail did not reduce rearrest rates. After Kentucky's statewide bail reform in 2018, there was a 1% to 2% increase in rearrest rates of those released before trial, yet the author notes this may be due to natural trend shifts over time (Stevenson, 2017). New York saw an increase in crime after bail reform was enacted. Though this has not been academically studied to understand bail reform's influence, it was claimed to be the major driver of rising crime including violent crime, by the NYPD (2020) and the City Journal (Lehman, 2022). Many of the claims about crime and bail reform are featured in opinion editorials. For example, an attorney with the U.S. Commission on Civil Rights featured in the National Review (Kirsanow, 2022). It appears that increases in violent crime are driving an association with bail reform.

Support for Hypothesis 2

Recent academic research finds that pretrial detention at the federal (Didwania, 2020) and county level (Dobbie, Goldin, & Yang, 2018; Leslie & Pope, 2017; Stevenson, 2018) increases probability of conviction (Dobbie et al., 2018; Gius, 2018), most frequently due to an increase in

guilty pleas (Dobbie et al., 2018). Reform groups point to this harm and others as the basis for ending cash bail (ACLU, 2017; Justice Policy Institute, 2012). Because most people who are held in jail pre-trial are there because they cannot afford to post bail (Leslie & Pope, 2017; Stevenson, 2018), it is assumed that ending cash bail would lead to a decrease in pre-trial detention, and in turn reduce these observed harms. However, research observing this decrease through bail reform is also limited. The federal presumption of release shifted to a presumption of detention for certain crimes in 1984 and has been followed by an increase in federal pretrial detention (Austin, 2017). Harris County's reform to default release for non-violent misdemeanors, resulted in a release in pretrial release rates and an overall reduction in pretrial detention rates (Heaton, 2022). In a report to the governor and legislature, New Jersey's court director observed a reduction in the overall jail population following reforms (Grant, 2019). In addition, Kentucky's bail reform was followed by a decrease in pretrial detention but was temporary and rose again to pre-reform rates by 2016 (Stevenson, 2019).

Data and Methodology

I begin with a set of multivariate regressions, and then expand upon these initial findings by utilizing a Synthetic Control Method for four states that had bail reform implemented at the time of the data cutoff (2018). The unit of analysis for all variables is U.S. states. The main dataset is provided by the Vera Institute's Incarceration Trends Dataset, which observes county-level jail population statistics through 2018. Though data is available as early as 1970 for some variables, the pretrial rate is unavailable for many states until later. Further, the earliest bail reform studied was passed in 2011. Including 1980s and 1990s crime data obscures synthetic

matching as there were strong increases and declines in crime during this period.[2] Therefore, the observation period is 2000 to 2018. The dataset includes most U.S. states with a few exemptions. Though Washington DC was the first to implement expansive bail reform, it was removed from this study due to the inability to create a synthetic DC for comparison.[3] Its crime and pretrial rates are unlike any other grouping of states. Connecticut, Delaware, Hawaii, Rhode Island, and Vermont were also excluded due to insufficient pretrial rate and covariate data. Variables from the Vera dataset are transformed into state-level aggregates over each year observed. Treated states are coded at the year of the policy implementation, and all subsequent years. They include Illinois (2018), Kentucky (2013 – implemented toward the end of 2012 so coded as 2013), Nebraska (2017), New Hampshire (2018), New Jersey (2017), and New Mexico (2017).

The independent variable, **Bail Reform**, is defined as a state that has implemented a statewide policy to reduce reliance on cash bail that is currently implemented. For some states, this may include using a pretrial risk tool, however, utilization of a risk tool alone is not considered bail reform as it simply provides information on defendants to a judge with discretion but does not restrict or shift the assignment of cash bail. The first outcome variable is **Violent Crime** rate (per 100,000 residents) which comes from the FBI's Uniform Crime Reporting program at the state level by year.[4] The second outcome variable is **Pretrial Rate**. This comes from the Vera Institute's dataset and measures pretrial detention (those not convicted of a crime)

[2] There is not a clear explanation for these crime trends in the literature and therefore is difficult to appropriately control for without attributing the significant decrease across the nation to any independent variable put into a regression over this period.

[3] Washington DC's crime rates and pretrial rates did not result in an interpretable match, often selecting only Florida due to its somewhat comparable crime rates. No combination of states, regardless of the pre-treatment lags or covariates selected were close enough to pre-treatment DC to create a comparison. A future study could study DC with county or city matches for the synthetic control.

[4] Though well known, it is necessary to state that national crime tracking data is flawed with variation in local and state reporting. Many aggregate findings concerning crime need to be interpreted with caution.

by population aged 15 to 64.[5] Calculating rate in this manner is more accurate due to the extremely low likelihood of jail incarceration below 15 or above 64 and the greatly varying proportion of these age groups across counties (Bloom, Canning, & Sevilla, 2001; Vera Codebook, 2020).

A series of control variables are included that consider state-level environmental and criminal justice system variations that may account for changes in the outcome variables. **Private Jails** are operationalized as the number of counties with privately run jails, which also comes from the Vera dataset, and is included to capture potential impact of these contracts based on observed impacts of facility privatization.[6] **Death Penalty** policy is included as a dummy variable to capture any deterrent effect the death penalty may have on violent crime, while it also works as a proxy for more punitive statewide criminal codes. **Judicial Election** is included as a dummy variable and is identified by how a judge maintains their position. This variable was coded based on information from the National Center for State Courts. Partisan, nonpartisan, and retention elections are treated as elections to understand if and how public opinion and scrutiny may influence judicial behavior. **Private Bail Ban** is included as a dummy if a state bans the private bail industry. This is included as a control as it is another state-level policy that could directly impact the incentives and use of cash bail (Heaton, Mayson, & Stevenson, 2017; Lahm, 2023). **State Party** is included as a -1, 0, and 1 variable depending on whether that state's legislature and governor are under full Democratic control (-1), Mixed partisan control (0), or Republican control (1). This was coded based on historical information from the National

[5] Both violent crime rate and pretrial rate is per 100,000 state residents.
[6] Jail privatization is included based on research that finds privatization of facilities increases recidivism (Duwe & Clark, 2013; Spivak & Sharp, 2018). In addition, private jail companies often have capacity requirements and have been observed by reform groups and in opinion editorials to lead to higher numbers of people in jails (Claitor & Larsen, 2015).

Conference of State Legislatures and the approach was borrowed from a study that coded Congressional-level partisanship in this manner (Stobb, Miller, & Kennedy, 2022). **Poverty Rate** from the U.S. Census Bureau is included due to its argued correlation with crime over time (Fajnsylber, et al., 2002; Hsieh, Pugh, 1993; Imran, et al., 2018). Finally, states are coded as being in the **South** or not due to the South's unique crime and policy trends.

Table 3. Variables

Variable Name	Definition	Source
Bail Reform Independent Variable	A state that has implemented a statewide policy to reduce reliance on cash bail that is currently implemented. Coded: 0, 1	Extensive author research, including reading through state legislation and session databases. Primarily confirmed from: Bail Reform Act of 2017; Huston, 2021; Jorgensen & Smith, 2021; Subramanian & Grawert, 2023
Violent Crime Dependent Variable #1	Rate per 100,000 residents at the state level by year.	FBI's Uniform Crime Reporting program.
Pretrial Rate Dependent Variable #2	Rate of county age 15 to 64 held pretrial, aggregated out to the state level by year.	Vera Institute, Incarceration Trends.
Private Jail	Number of counties with privately run jails per state and year.	Vera Institute, Incarceration Trends.
Death Penalty	Whether a state has the death penalty in effect by year. Coded: 0,1	Death Penalty Information Center
Judicial Election	How a judge maintains their position. Includes: partisan, nonpartisan, and retention elections. Coded: 0,1	National Center for State Courts
Private Bond Ban	If a state bans private the bail industry from operating within the state. Coded: 0,1	Cornell Law Schools' Legal Information Institute and Prison Policy Institute.
State Party	Whether a state is under full Democratic control (coded -1), Mixed partisan control (0), or full Republican control (1). Full control by party considers both the legislature and governor.	National Conference of State Legislatures
Poverty Rate	Poverty rate per 100,000 residents that lives under the nationally defined poverty level by state and year.	U.S. Census Bureau, American Community Survey
South	Dummy variable if a state is in the South (1) or not (0).	U.S. Census Bureau. South Region

OLS Results

The multivariate OLS model reveals (see Table 4) that reforming cash bail does not result in an increase in violent crime. Instead, cash bail reform has an insignificant effect on violent crime and the direction is negative, undermining support for Hypothesis 1. The impact of bail reform on the pretrial jail rate is positive and significant ($p < 0.01$). This is also in defiance of Hypothesis 2 expectations. Violent crime does have a significant positive effect on the pretrial rate, which suggests that it is not only non-violent and low-level crimes driving up the pretrial rate. This would follow the intention of bail reform which typically focuses on lower-level felonies and misdemeanor charges. Yet the positive significant effect on the overall pretrial rate means these policies are not reaching their stated goals. These outcomes may be due to a methodological issue of relying on OLS regression for time series data. These confounding initial results warrant a closer look at the individual states that have implemented bail reform. Four states implemented bail reform before 2018 – Kentucky, New Jersey, New Mexico, and Nebraska. In the next section, a Synthetic Control Method (SCM) is used to create a counterfactual for each state to understand if and how bail reform altered violent crime and pretrial rate.

Synthetic Control Methodology

After estimating an OLS regression for both outcome variables, I shift to analyzing the effects of bail reform on individual states. I begin with a series of t-tests to understand a simple before and after bail reform effect.[7] I then implement a Synthetic Control Method to better understand the counter-factual of a lack of bail reform for specific states. This approach helps to overcome the endogeneity inherent in studying crime rates and addresses time-variant trends.

[7] Two-tail t-tests are calculated on a window of years based on how long the bail reform has been implemented as of 2018. Therefore, 5 years before implemented is observed for a policy that has been implemented for 5 years.

Table 4. 2000 – 2018, Bail Reform Implementation Impact on Violent Crime and Pretrial Rate

	Dependent variable:
	Violent Crime
Bail Reform	−53.004
	(40.742)
Death Penalty	0.370
	(12.298)
Judicial Election	58.506***
	(16.025)
Priv Bond Ban	−1.255
	(14.809)
South	65.512***
	(12.948)
Private Jail	7.008**
	(3.542)
State Party	−10.403
	(7.290)
Poverty	1.478
	(1.833)
Pop 15 to 64	0.00001***
	(0.00000)
Constant	269.463***
	(23.631)
Observations	855
R^2	0.132
Adjusted R^2	0.122
Residual Std. Error	147.859 (df = 845)
F Statistic	14.244*** (df = 9; 845)
Note:	*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:
	Pretrial Rate
Bail Reform	77.866***
	(22.949)
Violent Crime	0.052***
	(0.019)
Judicial Election	−12.550
	(9.045)
Priv Bond Ban	−15.669*
	(8.310)
South	67.786***
	(7.148)
Private Jail	29.939***
	(1.996)
State Party	13.854***
	(4.015)
Poverty	13.227***
	(1.031)
Pop 15 to 64	−0.00000***
	(0.00000)
Constant	23.914*
	(14.005)
Observations	855
R^2	0.577
Adjusted R^2	0.573
Residual Std. Error	83.204 (df = 845)
F Statistic	128.184*** (df = 9; 845)
Note:	*p<0.1; **p<0.05; ***p<0.01

Four states are analyzed – Kentucky, New Jersey, New Mexico, and Nebraska – based on the cut-off point of the dataset being 2018. States are matched based on pre-policy implementation outcomes, as well as covariates that are important to understanding the crime and jail context of states. The covariates are male jail rate, female jail rate, Black jail rate, white jail rate, Latino/a jail rate, jail admission rate, jail rate, poverty, and proportion of population 15 to 64. The covariates not discussed previously, all come from the Vera Institute's Jail Incarceration Trends dataset. The approach creates a synthetic match that is closely like the state of interest before the

intervention not just in the outcome variable but also in other areas of system makeup that may influence and interact with the outcome variable.

The Synthetic Control Method (SCM henceforth) utilizes pre-treatment lags of the outcome variable along with pre-treatment covariates that create a synthetic version of the state from different weights of non-treated states.[8] For example, a synthetic Nebraska is calculated for violent crime from Alabama (0.033), Georgia (0.169), Idaho (0.15), Iowa (0.12), Mississippi (0.122), New Hampshire (0.009), Oregon (0.075), South Dakota (0.024), and Utah (0.148). This combination of states creates the closest match to pre-treatment crime rates and covariates and then reveals the post-treatment outcome of this "synthetic Nebraska" to allow us to understand what Nebraska's crime rates would have been without bail reform. Abadie (2021) and Abadie et al. (2010) stress the importance of including covariates that are contextually important to the outcome variable, while Lu (2021) prefers to rely on pre-treatment outcome lags for each year included. After estimating models for treated states with both approaches, the outcome did not change. Therefore, I instituted a combination of both approaches where yearly outcome lags and averaged covariates are incorporated. This produces the highest level of fit based on the RMSPE[9] indicator and the results remain consistent. For each state studied, a unique SCM was estimated for both outcome variables that removed any states that implemented bail reform during the same or prior period. States that contribute to the SCM can only be untreated and are only matched on data before the intervention (Abadie, 2021). As discussed above, I begin with the year 2000. This avoids the debated secular decrease in crime that started in the 1990s, which pre-dates the bail reform considered in this SCM, while also including the highest number of useful pre-treatment lags as predictors, which work better to control for unobserved confounders

[9] Root Mean Squared Percentage Error is calculated on pre-treatment data as a measure of synthetic state fit.

(Abadie, et al., 2010). First, a model is estimated for each of the four states for the violent crime outcome. Next, the same approach is used for estimating four SCM models for the pretrial rate outcome using the same covariates and lags of pretrial rate instead of violent crime.[10] T-tests are used to understand the overall effect size and significance. These are calculated by comparing the synthetic state post-reform to the actual state post-reform.

The benefit of utilizing SCM for this study is its transparency in showing which states contribute to the synthetic control, it reduces researcher bias in selecting specific states where the outcome may already be known, and it increases the precision of the control, reducing potential confounders (Cunningham, 2021). It is a recommended method for studying state policy interventions where sufficient pre-treatment longitudinal data is available and where a smaller number of units have experienced the policy change (Abadie, 2021).

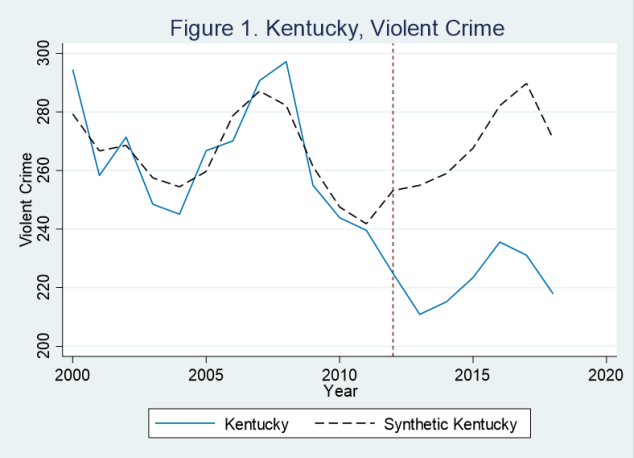
Synthetic Control Results

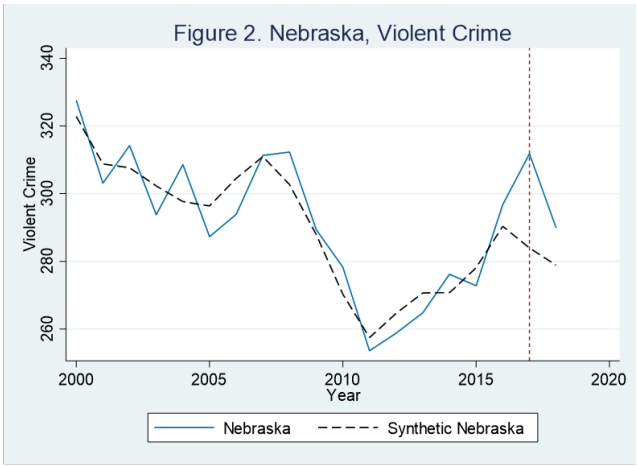
Violent Crime

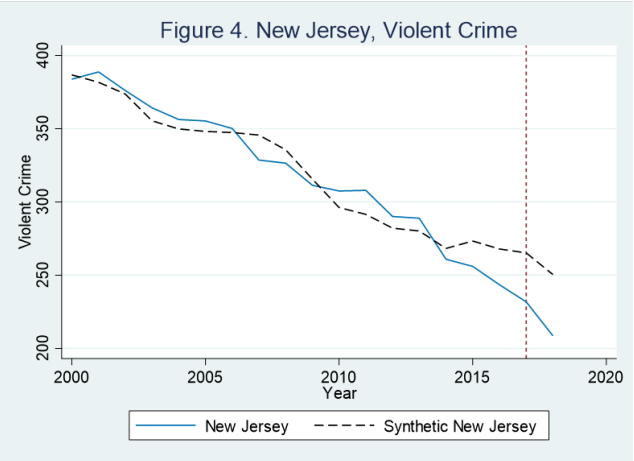
Initial t-tests calculated on violent crime before and after bail reform show that Kentucky and New Jersey saw a net reduction in violent crime, with only Kentucky's being significant. In contrast, Nebraska and New Mexico saw an increase in violent crime, with only New Mexico's being significant. [11] These first-step results suggest different violent crime realities between the four states, and the SCM reveals a clearer picture of bail reform effects given pre-treatment outcome and covariate trends. In confirmation of the OLS results and t-tests, bail reform does not have a consistent or clear impact on violent crime in a state (see Figures 1 to 4). The first issue is

[10] See Appendix A for state weights contributing to synthetic states.
[11] Two-tail t-test results on before and after violent crime rates:
Kentucky *M*= 258.6/222.37, t=2.89, p=0.016
Nebraska *M*= 284.8/300.85, t=-0.99, p=0.43
New Mexico *M*= 678.3/810.7, t = -3.447, p=0.07
New Jersey *M*= 187.8/169.4, t = 0.854, p=0.48

Synthetic Control Method Results for Bail Reform Impact on Violent Crime







that violent crime varies widely from state to state and in some cases the Synthetic Control Method is unable to create a synthetic version of the state that matches closely enough to the reformed state before policy implementation. The Root Mean Squared Percentage Error (RMSPE) is one way to understand the goodness of fit for SCM (Abadie, et al., 2015). New Mexico's violent crime volatility during the pre-bail reform years is not suited for meaningful results by SCM, with a high RMPSE of 42, yet the other states remain interpretable.[12] In New Mexico (Figure 3) violent crime increases after bail reform implementation, but we can see that

[12] RMPSE results for violent crime:
Nebraska = 6.97, Kentucky = 11.7, New Jersey = 11.17, New Mexico = 42.

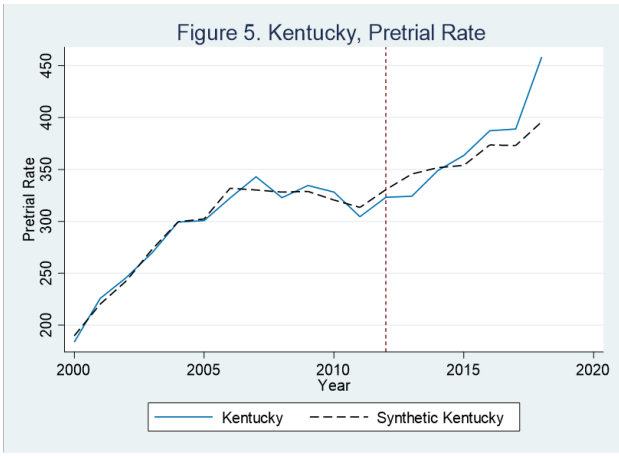
this trend began in 2014 and SCM is unable to create a close enough Synthetic New Mexico for useful interpretation. New Jersey's violent crime decreased after bail reform implementation, but likewise, the trend began in 2014 and the averaged results compared to synthetic New Jersey are significant only at the $p \leq .1$ level.[13] Kentucky's violent crime rate plummeted before the cash bail reform implementation and both increased and decreased again after the policy. However, the net post-bail reform effect is negative ($p<0.001$). Nebraska has the best pre-treatment SCM fit and shows a change in the direction of violent crime, from a sharp increase to a sharp decrease. However, these results are not significant ($p>.1$). SCM results of bail reform on violent crime largely confirms the OLS results which are that things other than bail reform are driving violent crime rate increases. Where there are interpretable, significant effects on crime rate, violent crime decreases rather than increases. Yet these decreases began before the policy was implemented. Perhaps these declines enabled an Overton window for bail reform as a statewide policy, but there is no evidence to support bail reform resulting in increases in violent crime.

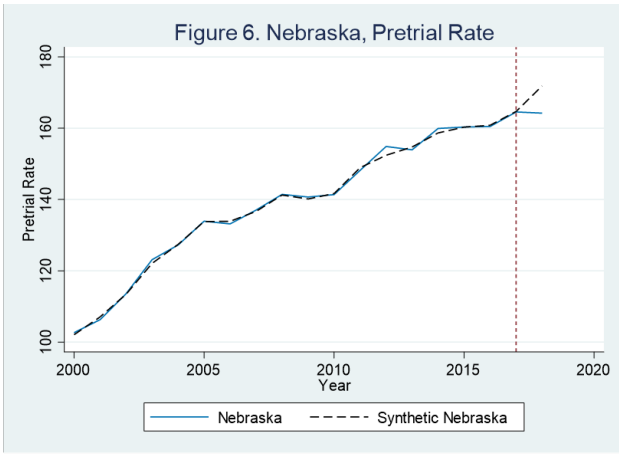
Pretrial Rate

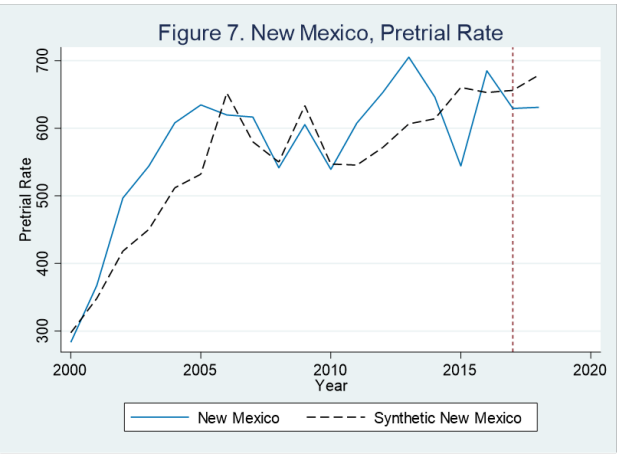
Initial t-tests calculated on pretrial rates before and after bail reform[14] show a more consistent pattern than observed in violent crime. In support of the OLS results, post-bail reform pretrial rates increased in Kentucky ($p<0.05$), Nebraska ($p<0.01$), and New Mexico ($p>0.1$) but New Mexico's difference is not significant. However, New Jersey's pretrial rate decreased ($p=0.1$).[15] Figures 5 to 8 present the SCM results for bail reform's impact on pretrial

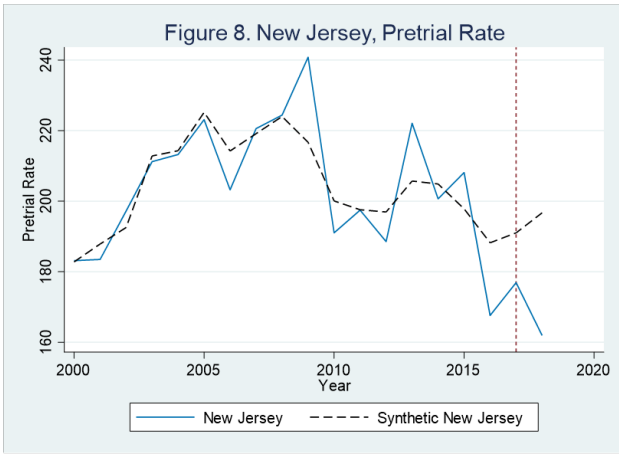
[13] Post-treatment effects on violent crime:
New Jersey p=0.11, Kentucky p=0.00003, Nebraska p=0.226, New Mexico p=0.05.
[14] Here again a balanced two-tail t-test is calculated observing the same number of years pre-bail reform as are available post-reform.
[15] Two-tail t-test results on before and after pretrial rates:
Kentucky *M*=326/378.5, t=-2.685, p=0.02
Nebraska *M*=160.4/164.4, t=-21.68, p=0.002
New Mexico *M*= 614.7/630.2, t = -0.22, p=0.846
New Jersey *M*= 249.75/220.2, t = 2.91, p=0.1

Synthetic Control Method Results for Bail Reform Impact on Pretrial Rate









rate.[16] First, pretrial rate is a better fit for the SCM approach for Kentucky, Nebraska, and even New Jersey despite some sharp increases and declines.[17] The same covariates are included that were included for violent crime, along with pretrial rate across the pre-intervention period. New Mexico again has a high RMPSE and the pre-treatment data does not fit the pretrial trends well, once again rendering the results uninterpretable. These null results are consistent for New Mexico. Kentucky's pretrial rate first flattens after a period of increase and remains lower than synthetic Kentucky for a period, but then it increases again and reaches equivalent rates to its counterfactual, rendering the net effect insignificant.[18] Nebraska shows that bail reform appears

[16] See Appendix B for state weights contributing to synthetic states.
[17] RMPSE results for Pretrial Rate: Nebraska = 0.82, Kentucky = 7.5, New Jersey = 10, New Mexico = 66
[18] Post-treatment effects on pretrial rate:

to stop a trend of consistent increase and instead maintains the rate that would otherwise continue to increase among the grouping of states that make up Synthetic Nebraska, yet once again the net effect of these shifts are not significant. Finally, New Jersey shows a change in a short-term increase to a sharp decrease that otherwise would continue to increase for Synthetic New Jersey with significant results ($p<0.1$). Some initial takeaways are that Kentucky appears to be driving much of the increase in pretrial rate captured in the OLS model. Further, the results vary from bail reform post-periods experiencing increases, decreases, and stalls in the pretrial rates. The commonality is that for each state, the implementation of bail reform is followed by a change in the trajectory of pretrial rate before the intervention. Yet only New Jersey is both significant and an interpretable SCM model, and it declines in conflict with the OLS results, but is a case study level of support for hypothesis 2.

Discussion

Alternative to Hypothesis 1: Explanations for No Effect on Violent Crime Rates

The OLS and SCM results find that bail reform does not have a positive effect on state violent crime rates. It has a net null effect, and in Kentucky and New Jersey, it has a significant negative effect according to the SCM models. This consistent result provides evidence to support rejecting Hypothesis 1. Some explanation and confirmation of this result can be found in previous research. The Vera Institute points out that the majority of those jailed pretrial are charged with nonviolent offenses, where they suggest the risks of individuals committing violent crimes are significantly low (Subramanian, et al., 2015). After having their bail system struck down in federal court (Rosenburg, 2017), Harris County revamped its system to remove the use of cash bail for misdemeanor crimes. Preliminary research on this change observed less crime

New Jersey p=0.09, Kentucky p= 0.9, Nebraska p= 0.39, New Mexico p=0.08.

and less recidivism as a result (Heaton, 2022). In confirmation of this report, Ouss & Stevenson (2019) find that crime rates decreased in New Jersey due to bail reform. A 2013 study that tested different pretrial release types (cash bonds, commercial bonds, and recognizance bonds) found that the type of release does not predict recidivism or misconduct prior to trial (Morris, 2013). Reforms may simply result in low-risk defendants being released, and/or the type of charge may not dictate the likelihood of rearrest or additional crimes while awaiting trial. Given the consistency of null and at times negative effects on violent crime, the remainder of this paper focuses on explanations for the variation in pretrial rate effects to better understand why.

Alternative to Hypothesis 2: Explanations for Increased Pretrial Rate

Despite an intended goal of bail reform, OLS results show a significant increase in pretrial rate and SCM results show variation in outcome depending on the state. A set of scholarship finds that judges tend to be influenced by political pressure and public opinion when making criminal judicial decisions. Berdejo and Yuchtman (2012) found that Washington State judges respond to political pressure by sentencing more harshly. In regions that elect judges, as proximity to an election got closer, punitive sentences increased (Brooks & Raphael, 2002; Hall, 1992 and 1995; Huber & Gordon, 2004). If this punitive impact on judicial behavior extends to bail decisions, it is plausible that reforming cash bail results in judges ignoring bail reforms or utilizing more bond denials to avoid negative political pressure and/or public attention.

The example of Texas passing stricter requirements for applying cash bail and restricting judges from giving recognizance bonds (McCullough, 2021), may further support the logic of this explanation. Both the Governor of Texas and Republicans who pushed and voted for the bill cited crime rates and examples of people being released pre-trial and then going on to harm someone while awaiting trial (McCullough, 2021). This suggests increased political pressure

from politicians who publicly call out judges who release defendants. The desire to avoid this type of negative attention, political pressure, and possible subsequent media attention aligns with the expectation of a shift to more bond denials and/or circumventing the purpose of the reforms. Yet judicial election was not significant in the OLS model. Future work could observe proximity to election in judicial election states, along with election type to better understand this potential dynamic.

Other empirical research has found instances of no effect on pre-trial detention as well. In Philadelphia, a prosecutor-driven "No-Cash-Bail policy" resulted in a 22% increase in being granted release on a recognizance bond, but no overall impact on pretrial detention rates (Ouss & Stevenson, 2022). The authors find this is due to the increase in recognizance bonds being assigned to people who would have received approximately $500 bonds previously, and therefore would have been able to bail themselves out regardless.

Pretrial Rate & Kentucky

One possibility for different outcomes to bail reform are the variations within the substance of the laws and their implementations. Kentucky's bail reform passed in 2011, which was followed by a notable increase in the pretrial rate (Figure 9). In 2013, risk assessments were implemented to aid judges in making bail decisions. Following this, there was a flattening of the pretrial rate. In 2015, the Administrative Release program began. This program automatically releases defendants charged with certain crimes and avoids going before a judge to make this decision (Spalding, 2021). The program existed throughout Kentucky but was not mandatory until 2017. This is when we observe the sharpest increase in the pretrial rate across the 18 years. The answer to why is at least partially captured by judges ignoring the risk assessment recommendation (Stevenson, 2018; U.S. Commission on Civil Rights, 2021) and because the law allows for discretion concerning the presumptive release component. Judges ignored this release

Table 5. Examination of the Context in Treated States

	Kentucky	Nebraska	New Mexico	New Jersey
Year Passed / Implemented	2011 / 2012	2017 / 2017	2014 / 2017	2014 / 2017
Pretrial Outcome	Increase	Flatten	Flatten (but poor SCM fit)	Decrease
Bail Reform Type	Presumption of release with limits on when judge should assign cash bail and least restrictive conditions required.	Presumption of release with least restrictive conditions required and ability to pay considered.	Constitutional amendment prohibited setting unaffordable bail.	Presumption of release with limits on when judge can assign cash bail and least restrictive conditions required.
Judicial Selection	Nonpartisan election	Gov appointment from committee then retention election	Partisan election then retention election	Gov appointment with state senate approval
Private Bail Industry	Banned	Banned	Not Banned	Not Banned
Number of Private Jails	2000-2018: 0	2000-2018: 0	2000-2018: 3 (Mode)	2000-2018: 0
State Party Control	2000-2016: Mixed; 2017-2018: Rep Trifecta	2000-2018: Rep Gov Control (Unicameral legislature non-partisan)	2000-2002: Mixed, 2003-2010: Dem Trifecta; 2011-2018: Mixed	2000-2001: Rep Trifecta; 2002-2009: Dem Trifecta; 2010-2017: Mixed; 2018: Dem Trifecta
Poverty	Pre-treatment average: 15.5; post-treatment average: 17.7	Pre-treatment average: 10.2; post-treatment average: 11	Pre-treatment average: 18.8; post-treatment average: 18.15	Pre-treatment average: 9.2; post-treatment average: 9
Region	South	Midwest	West	Northeast

presumption, and instead assigned cash bail for low and moderate risk defendants in over two-thirds of cases during this period (Stevenson, 2018). Albright (2019) observed that these overrides occurred more frequently for black defendants in favor of more punitive measures,

than for white defendants at the same assigned risk level. Further, judges in whiter counties were more likely to follow the default recommendation than judges in blacker counties. The judicial discretion incorporated into the law coupled with a lack of pushback for overriding mandatory components of the law appears to drive the increase rather than decrease in pretrial detention, while also failing to overcome racial bias.

Pretrial Rate & Nebraska

Nebraska's pretrial rate stalls after a consistent period of increase. This is only a short-term outcome and updated data is needed to understand any long-term effects as they were not statistically significant. The state law passed in 2017 was similar to Kentucky's in that it shifted to a presumption of release with a requirement to consider defendant's ability to pay and to rely on the least restrictive conditions. The ACLU of Nebraska partnered with two counties to observe practices after the state reform was enacted. They found that judges continued to assign cash bail more than any other option, determined more than 80% of defendants to be dangerous or a flight risk, failed to inquire about ability to pay in 38% of the cases, and advised on rights connected to fines and fees half of the time (Petto, et al., 2022). This is a two-county study from an advocacy group, but it suggests that if these findings apply to the state, judicial behavior in bail hearings has not shifted as intended with the bill. Nebraska did not have a multi-branch approach to crafting and implementing reform, and it appears to be solely guided by the state legislature. It has avoided negative backlash and received little coverage overall. In fact, a bail reform guide from Harvard faculty does not include Nebraska as a state with bail reform (Doyle, Bains & Hopkins, 2019). Nebraska's quiet approach to bail reform appears to have allowed it to avoid backlash effects, but this may still be to the detriment of judicial buy in.

Pretrial Rate & New Jersey

Differently, in New Jersey, pretrial rates declined after the implementation of bail reform, are significant, and experienced the sharpest decline in the years after passage and during statewide training (See Figure 8 & 10). Researchers point to the implementation of New Jersey's bail reform as one of the reasons for the difference in outcome. The reform passed in 2014 but was not implemented until 2017. In the between years, statewide trainings were conducted for "judges, attorneys, court personnel, local officials, and the public to ensure that everyone understood how the new system worked and how release and detention determinations were to be made" (Doyle, Bains & Hopkins, 2019, p.20). The way in which reform was passed is also pointed to as a piece of its success. State legislation ultimately enacted the reforms, but the New Jersey Supreme Court first formed a committee of diverse stakeholders, which included representatives from the three branches along with community members, rights organizations, and private attorneys (Jorgensen & Smith, 2021). Doyle et al point to this inclusive approach to reform, along with public forums and education seminars in every county, as one of the key reasons for success (2019). Indeed, the counter movement observed in Illinois and California have not played out in New Jersey. Further, New Jersey's reform is more comprehensive than Kentucky's. It created types of pretrial monitoring directly connected to the risk level assessment by a risk assessment tool (Jorgensen & Smith, 2021). In addition, data is made public and a commission reviews progress annually. Transparency, oversight, education, and more comprehensive reforms contrast Kentucky's policy. It is noteworthy that in both Kentucky and New Jersey, violent crime decreased after both passage and implementation of bail reform.

Interaction Effects

In addition to these implementation and policy differences, Kentucky and Nebraska both have bans on private bail bond industries operating within the state, while New Mexico and New

Jersey do not (see Table 5). In the OLS model, banning the private bail bond industry had a small, negative effect (only at the $p<0.1$ level) but considering these case studies the opposite is observed for Kentucky vs. New Jersey. Despite much criticism and research suggesting harm of the private bail bond industry, it is possible that in states without it, and without more comprehensive and mandatory bail reform, fewer people are able to be released from jail. In addition, there are political differences where Kentucky is Republican led and New Jersey is Democratic led. I therefore estimate a stepwise OLS model, interacting bail reform with each of the controls (See Appendix C for full stepwise regression).

Number of counties with private jails and violent crime were statistically significant (Appendix C). State party control becomes insignificant when interacted with bail reform as does private bail bond bans. However, violent crime and private jails are significant. In states that have reformed their bail system, as violent crime increases pretrial rate decreases, becoming insignificant. This is interesting given that we know that bail reform does not increase violent crime. It also does not appear to be driving pretrial rates where there is bail reform implementation. Further, in states that have reformed their bail system, as private jails increase, the pretrial rate increases. Privatization continues to matter but is likely better understood studying counties given the few states with both statewide bail reform *and* private jails.

Alternative Explanation for Pretrial Rate

Passage Effect, Rather than Implementation Effect

Policy implementation and state contextual factors may explain the variation within pretrial rate. A possible alternative explanation is that there is a shock happening due to passage and not necessarily implementation, where judges react to the knowledge of reform and any attention it may receive. As noted above, a change in pretrial rate was observed before

implementation in three of the four states, even though the SCM model worked to match during this period. Changes in the policy preferences of principals, whether they are legislatures or appellate courts, affect the decision-making of judges (Baum, 2008; Epstein & Jacobi, 2010; Stobb, Miller, & Kennedy, 2022). If these observations extend to state legislature control and state supreme courts, it is reasonable that judges will adjust their behavior once state legislatures and/or state supreme courts signal the expectation to change. However, setting bail is a unique judicial behavior that is rarely reviewed by appellate courts. Typically, if a defendant believes bail assignment is inappropriate, the defense attorney will file a motion to reconsider before the same judge and bring additional facts before that judge. Further, research has found that judges become more punitive during election seasons (Berdejo & Yuchtman, 2012; Huber & Gordon, 2004; Park, 2017) and that judges are only harmed in publicity around lenience by both Democratic and Republican voters. Therefore, the publicity and later backlash concerning bail reform in some states may create a perverse incentive so that judges can avoid negative attention. Based on the findings above, this effect would be stronger in Republican led states. To consider this explanation, I re-estimate the OLS and SCM models changing the treatment date to the date of policy passage rather than implementation. This will account for changes in judicial behavior due to signaled principal and public preferences at the time of the court decision or the passed legislation, rather than the implementation of the policy.

Looking at the OLS models (See Table 6), we see the effect size on pretrial rate increases when considering passage date rather than implementation date. [19] Judicial election remains insignificant.[20] This could be that judges exhibit strategic behavior even when not facing election

[19] OLS on violent crime was also re-estimated and remained negative and insignificant, suggesting that the attention on bail reform does not increase violent crime behavior.
[20] See Appendix E for full stepwise regression results.

Table 6. 2000 – 2018, Bail Reform *Passage* Impact on Pretrial Rate with Interactions

	Dependent variable:			
	Pretrial Rate			
	(1)	(2)	(3)	(4)
Reform Passed	82.085***	-18.651	23.345	33.197*
	(17.604)	(34.935)	(34.799)	(19.496)
Violent Crime	0.048**	0.031	0.046**	0.028
	(0.019)	(0.020)	(0.019)	(0.019)
Jud. Election	-9.814	-10.244	-12.744	-9.151
	(9.028)	(8.976)	(9.137)	(8.879)
Priv Bond Ban	-17.526**	-14.954*	-20.162**	-13.653*
	(8.256)	(8.244)	(8.352)	(8.150)
South	69.948***	74.273***	69.882***	76.797***
	(7.123)	(7.199)	(7.111)	(7.116)
Private Jail	29.721***	29.013***	29.602***	28.184***
	(1.985)	(1.985)	(1.983)	(1.973)
State Party	13.988***	14.811***	13.879***	15.423***
	(3.988)	(3.972)	(3.982)	(3.931)
Poverty	12.841***	12.583***	12.641***	12.163***
	(1.032)	(1.029)	(1.036)	(1.023)
Pop 15 to 64	-0.00000***	-0.00000**	-0.00000***	-0.00000**
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Passed*Violent Crime		0.265***		
		(0.079)		
Passed*Jud. Election			79.653*	
			(40.728)	
Passed*Private Jail				78.453***
				(14.388)
Constant	26.051*	33.401**	32.021**	37.442***
	(13.935)	(14.027)	(14.243)	(13.862)
Observations	855	855	855	855
R^2	0.582	0.588	0.584	0.596
Adjusted R^2	0.578	0.583	0.579	0.592
Residual Std. Error	82.712 (df = 845)	82.222 (df = 844)	82.574 (df = 844)	81.340 (df = 844)
F Statistic	130.836*** (df = 9; 845)	120.270*** (df = 10; 844)	118.529*** (df = 10; 844)	124.730*** (df = 10; 844)

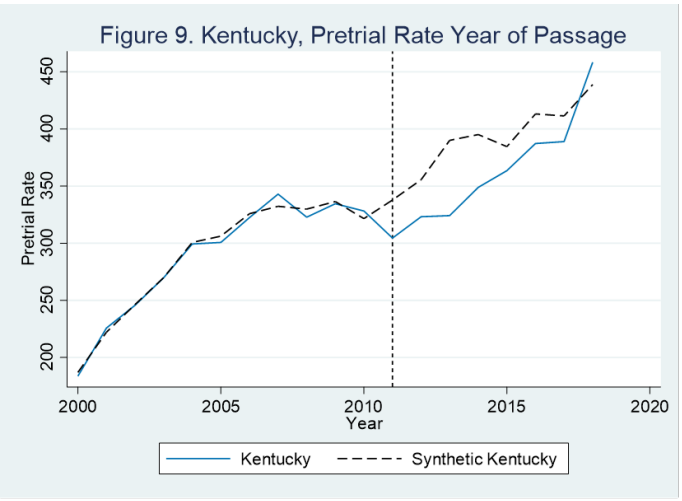
(Gordon & Yntiso, 2022), or that there is no strategic behavior happening among elected judges specifically. However, when judicial election is interacted with passage date it becomes significant ($p<.1$). Future work could examine election season effects on judicial behavior to see how this interaction being picked up at the state level plays out in the behavior of county judges at the district court level. Does Huber and Gordon's (2004) observation that sentence lengths increase with election proximity extend to judicial bail decisions? Further, private jails have a nearly identical effect size from policy passage to policy implementation. When interacting

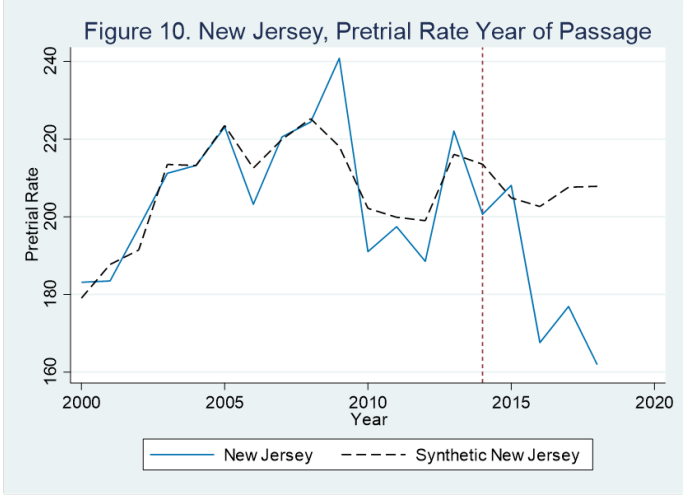
reform passage with private jails there is a significant effect. Future research could better understand the direction of this potentially endogenous relationship. Is the existence of private jails signaling a response to overcrowding and high pretrial rates or if there is a profit motivation playing out that leads to pretrial rate increases?

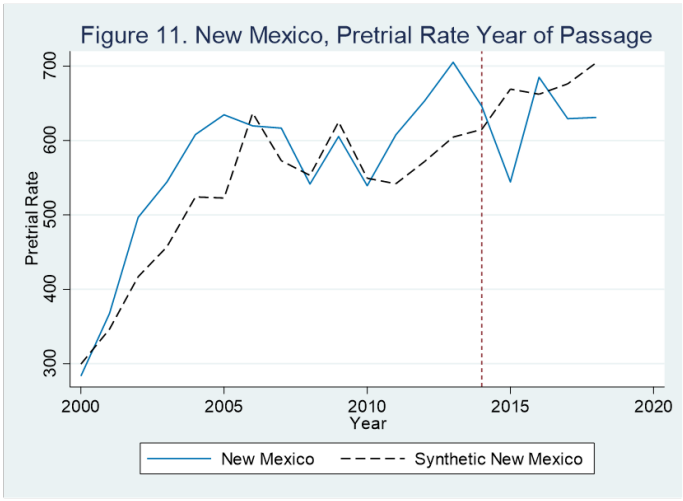
I end this analysis by re-estimating SCMs for Kentucky, New Jersey, and New Mexico with bail reform passage date as the independent variable (See Figures 9-11).[21] Nebraska's law was implemented the same year it was passed so it is not considered. Preliminary t-test results remain consistent.[22] The SCM fit for pre-passage data improves for all three states, but New Mexico remains uninterpretable.[23] The significance for post-passage effects also improves for both New Jersey and Kentucky. [24] New Jersey's pretrial rate has a significant, net decrease after passage, but it first increases seemingly in reaction to passage. The collaboration and education work described above in New Jersey may have mitigated this initial increase, along with the Democratic state party control and the lack of judicial elections. Both would theoretically reduce pressure and attention on judicial bail decisions. Kentucky reacts with a shift in direction from decreasing to increasing pretrial rate. However, when removing 2018, the significance improves ($p<0.5$) and Kentucky's pretrial rates are lower than they would have been without the passage of bail reform. [25] The differing political pressures and contextual factors discussed above are supported by opposite reactions to attention on bail reform in Kentucky vs. New Jersey. Yet even with Kentucky's increase and these factors, Kentucky's pretrial rate was significantly lower

[21] See Appendix C for state weights contributing to synthetic states.
[22] Two-tail t-test results on before and after pretrial rates:
Kentucky *M*=315/362, t=-2.43, p=0.03
New Mexico *M*=622/627, t=-.14, p=0.9
New Jersey *M*= 208/183, t = 1.38, p=0.10
[23] RMPSE results for violent crime: Kentucky = 5, New Jersey = 8.25, New Mexico = 36.
[24] Post-treatment effects on pretrial rate:
New Jersey p=0.03, Kentucky p= 0.1, New Mexico p=0.19.
[25] Post-treatment effects on pretrial rate in Kentucky if 2018 is removed: p=0.05.

Synthetic Control Method Results for Bail Reform *Passage* Impact on Pretrial Rate







than it would have been for 7 years. The effect of passage has a stronger effect on pretrial rate in both the OLS (Table 6) and individual SCM models (Figures 9 -11). Bail reform matters, but the passage alone leads to behavior changes and provides insight into the seemingly disparate outcomes between New Jersey and Kentucky.

Limitations and Next Steps

Limitations of this study include that endogeneity is inherent in the OLS regressions for both violent crime and pretrial rate, along with omitted variable bias. Therefore, regressions are used as a first step, and then the SCM is utilized. However, SCM did not work to create an

interpretable synthetic New Mexico for either outcome. A county approach with a different research design may be useful in understanding states like New Mexico and Washington, DC. Next steps will include a Two-Way Fixed Effects analysis to isolate the state and year changes in case design choices are driving results. Data availability ending in 2018 presents a limitation in interpreting the findings. SCM is limited to four states and those with fewer post-treatment lags should be considered with caution as the results only reflect short-term trends. Both crime and pretrial rates have short-term trends that do not always align with long-term increases or decreases. The Vera Institute plans to make an updated version of this dataset available in the future. Further, bail reform is operationalized as a dichotomous value. Given the above discussion, implementation, and type of bail reform likely matter. Measuring bail reform types in terms of more conservative to more expansive reforms likely plays a role in understanding variations in outcomes. An additional concern for proponents of bail reform is the racial disproportionality within local jails (Arnold, Dobbie & Yang, 2018; Shaefer & Hughes, 2019) and the belief that bail reform can reduce it (Justice Policy Institute, 2012). Considering jail rate by race in future studies is important to understanding how both passage and implementation impact racial disparities.

Newly Generated Hypotheses

This study revealed interactions between bail reform with private jails and judicial elections that generate initial hypotheses and warrant further study. As detailed in the above discussion, pretrial rate significantly increases with private jails (See Table 2). Additional work is needed to understand the direction of this potentially endogenous relationship. Is the existence of private jails signaling a response to overcrowding and high pretrial rates or is a profit motivation playing out that leads to pretrial rate increases? If it is the latter, understanding how

the profit motivation may be interacting with judicial or prosecutor behavior is important to understanding bail reform and privatization effects more broadly. Judicial election is not significant to pretrial rates but when interacted with reform passage it becomes significant. Connecting this initial result with the judicial behavior literature and more precise measures of judicial selection type and election proximity will provide useful insights for both understanding bail reform passage effects and consistency of the judicial behavior scholarship at county court levels. Therefore, the hypotheses for future work are:

- *Judges will assign bail more punitively as proximity to election increases, and this effect will be greater with recent bail reform.*
- *Increases in private jail contracts will increase pretrial rates, and this effect will be greater with bail reform.*

Conclusion

The results of this study indicate that bail reform does not increase violent crime. This is confirmed both through OLS models and a Synthetic Control Method of individual states. This confirms previous research finding many things impact crime rates, including much that is unaccounted for. Bail reform in its current implementations has not been a strong enough policy to influence overall crime rates statistically. However, in states with significant results through the SCM models, violent crime rate decreased after bail reform implementation.

Pretrial rate was a more complex outcome, with a net positive effect but different results depending on the state examined. Analyzing contextual factors qualitatively, interacting variables, and re-estimating models based on when policies were passed, revealed a story of implementation differences and a stronger effect from passage than reform implementation. The signal and attention from passing bail reform, rather than the implementation of reform policies

on their own, results in an initial response that adjusts differently depending on the state. However, both Kentucky and New Jersey's pretrial rate was lower than it would have been without reform for a period. By the eighth year, Kentucky's pretrial rates were back to what they would have been without reform. Takeaways for policymakers are that bail reform does not pose a public safety threat but on its own is unlikely to reduce pretrial detention rates long-term without stakeholder and public buy-in.

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Appendix A: State Weights for Violent Crime Synthetic Control Method

State Weights for Kentucky

State	Unit Weight
Alabama	0.033
Alaska	0
Arizona	0
Arkansas	0
California	0
Colorado	0
Connecticut	-
Delaware	-
District of Columbia	-
Florida	0
Georgia	0.097
Hawaii	-
Idaho	0
Illinois	0
Indiana	0
Iowa	0.12
Kansas	0
Kentucky	-
Louisiana	0.214
Maine	0.094
Maryland	0
Massachusetts	0
Michigan	0
Minnesota	0
Mississippi	0
Missouri	0
Montana	0
Nebraska	0.144
Nevada	0
New Hampshire	0
New Jersey	0
New Mexico	0
New York	0
North Carolina	0
North Dakota	0
Ohio	0
Oklahoma	0
Oregon	0
Pennsylvania	0
Rhode Island	-
South Carolina	0
South Dakota	0.053
Tennessee	0
Texas	0
Utah	0.002
Vermont	-
Virginia	0
Washington	0
West Virginia	0
Wisconsin	0.193
Wyoming	0.17

State Weights for New Jersey

State	Unit Weight
Alabama	0
Alaska	0
Arizona	0
Arkansas	0
California	0
Colorado	0
Connecticut	-
Delaware	-
District of Columbia	-
Florida	0.131
Georgia	0
Hawaii	-
Idaho	0
Illinois	0
Indiana	0
Iowa	0
Kansas	0
Kentucky	-
Louisiana	0
Maine	0
Maryland	0.138
Massachusetts	0.003
Michigan	0.474
Minnesota	0
Mississippi	0
Missouri	0
Montana	0
Nebraska	-
Nevada	0
New Hampshire	0.121
New Jersey	-
New Mexico	-
New York	0.044
North Carolina	0
North Dakota	0
Ohio	0
Oklahoma	0
Oregon	0
Pennsylvania	0
Rhode Island	-
South Carolina	0.088
South Dakota	0
Tennessee	0
Texas	0
Utah	0
Vermont	-
Virginia	0
Washington	0
West Virginia	0
Wisconsin	0
Wyoming	0

State Weights for New Mexico

State	Unit Weight
Alabama	0
Alaska	0.377
Arizona	0
Arkansas	0
California	0
Colorado	0
Connecticut	-
Delaware	-
District of Columbia	-
Florida	0
Georgia	0
Hawaii	-
Idaho	0
Illinois	-
Indiana	0
Iowa	0
Kansas	0
Kentucky	-
Louisiana	0
Maine	0
Maryland	0.275
Massachusetts	0
Michigan	0
Minnesota	0
Mississippi	0
Missouri	0
Montana	0
Nebraska	-
Nevada	0
New Hampshire	0
New Jersey	-
New Mexico	-
New York	0
North Carolina	0
North Dakota	0
Ohio	0
Oklahoma	0
Oregon	0
Pennsylvania	0
Rhode Island	-
South Carolina	0.083
South Dakota	0
Tennessee	0.265
Texas	0
Utah	0
Vermont	-
Virginia	0
Washington	0
West Virginia	0
Wisconsin	0
Wyoming	0

State Weights for Nebraska

State	Unit Weight
Alabama	0.033
Alaska	0
Arizona	0
Arkansas	0
California	0
Colorado	0
Connecticut	-
Delaware	-
District of Columbia	-
Florida	0
Georgia	0.169
Hawaii	-
Idaho	0.15
Illinois	-
Indiana	0
Iowa	0.12
Kansas	0
Kentucky	-
Louisiana	0.15
Maine	0
Maryland	0
Massachusetts	0
Michigan	0
Minnesota	0
Mississippi	0.122
Missouri	0
Montana	0
Nebraska	-
Nevada	0
New Hampshire	0.009
New Jersey	-
New Mexico	-
New York	0
North Carolina	0
North Dakota	0
Ohio	0
Oklahoma	0
Oregon	0.075
Pennsylvania	0
Rhode Island	-
South Carolina	0
South Dakota	0.024
Tennessee	0
Texas	0
Utah	0.148
Vermont	-
Virginia	0
Washington	0
West Virginia	0
Wisconsin	0
Wyoming	0

Appendix B: State Weights for Pretrial Synthetic Control Method

State Weights for Kentucky	
State	**Unit Weight**
Alabama	0
Alaska	0
Arizona	0.036
Arkansas	0.062
California	0
Colorado	0
Connecticut	-
Delaware	-
District of Columbia	-
Florida	0
Georgia	0.03
Hawaii	-
Idaho	0
Illinois	0.04
Indiana	0
Iowa	0
Kansas	0
Kentucky	-
Louisiana	0.096
Maine	0
Maryland	0
Massachusetts	0.245
Michigan	0
Minnesota	0
Mississippi	0
Missouri	0
Montana	0
Nebraska	0
Nevada	0
New Hampshire	0
New Jersey	0
New Mexico	0.075
New York	0
North Carolina	0
North Dakota	0
Ohio	0
Oklahoma	0
Oregon	0
Pennsylvania	0
Rhode Island	-
South Carolina	0
South Dakota	0
Tennessee	0
Texas	0.065
Utah	0.031
Vermont	-
Virginia	0
Washington	0
West Virginia	0.021
Wisconsin	0
Wyoming	0

State Weights for New Jersey	
State	**Unit Weight**
Alabama	0
Alaska	0
Arizona	0.187
Arkansas	0
California	0
Colorado	0
Connecticut	-
Delaware	-
District of Columbia	-
Florida	0
Georgia	0
Hawaii	-
Idaho	0
Illinois	0
Indiana	0
Iowa	0
Kansas	0
Kentucky	-
Louisiana	0
Maine	0
Maryland	0.565
Massachusetts	0
Michigan	0
Minnesota	0.248
Mississippi	0
Missouri	0
Montana	0
Nebraska	-
Nevada	0
New Hampshire	0
New Jersey	-
New Mexico	-
New York	0
North Carolina	0
North Dakota	0
Ohio	0
Oklahoma	0
Oregon	0
Pennsylvania	0
Rhode Island	-
South Carolina	0
South Dakota	0
Tennessee	0
Texas	0
Utah	0
Vermont	-
Virginia	0
Washington	0
West Virginia	0
Wisconsin	0
Wyoming	0

State Weights for New Mexico	
State	**Unit Weight**
Alabama	0
Alaska	0
Arizona	0
Arkansas	0
California	0
Colorado	0
Connecticut	-
Delaware	-
District of Columbia	-
Florida	0
Georgia	0
Hawaii	-
Idaho	0
Illinois	-
Indiana	0
Iowa	0
Kansas	0
Kentucky	-
Louisiana	0.047
Maine	0
Maryland	0
Massachusetts	0
Michigan	0
Minnesota	0
Mississippi	0
Missouri	0
Montana	0
Nebraska	-
Nevada	0
New Hampshire	0
New Jersey	-
New Mexico	-
New York	0
North Carolina	0
North Dakota	0
Ohio	0
Oklahoma	0
Oregon	0
Pennsylvania	0
Rhode Island	-
South Carolina	0
South Dakota	0
Tennessee	0
Texas	0.953
Utah	0
Vermont	-
Virginia	0
Washington	0
West Virginia	0
Wisconsin	0
Wyoming	0

State Weights for Nebraska	
State	**Unit Weight**
Alabama	0
Alaska	0
Arizona	0.018
Arkansas	0
California	0
Colorado	0.013
Connecticut	-
Delaware	-
District of Columbia	-
Florida	0
Georgia	0
Hawaii	-
Idaho	0
Illinois	-
Indiana	0
Iowa	0.26
Kansas	0.107
Kentucky	-
Louisiana	0
Maine	0
Maryland	0
Massachusetts	0
Michigan	0.085
Minnesota	0.042
Mississippi	0
Missouri	0
Montana	0
Nebraska	-
Nevada	0.011
New Hampshire	0
New Jersey	-
New Mexico	-
New York	0.09
North Carolina	0
North Dakota	0.038
Ohio	0.24
Oklahoma	0
Oregon	0
Pennsylvania	0
Rhode Island	-
South Carolina	0
South Dakota	0.011
Tennessee	0
Texas	0
Utah	0
Vermont	-
Virginia	0
Washington	0
West Virginia	0
Wisconsin	0.084
Wyoming	0

Appendix C:Stepwise Regression for Pretrial Rate Outcome with Interactions

	Dependent variable: **Pretrial Rate**							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bail Reform	77.866***	-41.098	21.295	115.636***	82.175***	45.784*	81.795***	8.577
	(22.949)	(43.310)	(48.801)	(37.770)	(29.841)	(24.829)	(25.290)	(71.002)
Viol Crime	0.052***	0.040**	0.051***	0.049**	0.051***	0.041**	0.052***	0.052***
	(0.019)	(0.020)	(0.019)	(0.019)	(0.020)	(0.020)	(0.019)	(0.019)
Jud Elec	-12.550	-12.497	-13.931	-11.253	-12.409	-12.014	-12.402	-13.060
	(9.045)	(8.995)	(9.103)	(9.101)	(9.072)	(8.995)	(9.059)	(9.059)
Priv Bond Ban	-15.669*	-14.055*	-17.223**	-13.788	-15.439*	-13.574	-15.457*	-16.379**
	(8.310)	(8.279)	(8.390)	(8.441)	(8.377)	(8.287)	(8.334)	(8.338)
South	67.786***	70.800***	67.666***	68.718***	67.985***	70.723***	67.891***	67.779***
	(7.148)	(7.169)	(7.145)	(7.184)	(7.206)	(7.163)	(7.157)	(7.148)
Priv Jail	29.939***	29.405***	29.930***	29.782***	29.917***	29.358***	29.930***	29.910***
	(1.996)	(1.992)	(1.996)	(2.000)	(2.000)	(1.993)	(1.998)	(1.997)
State Party	13.854***	14.430***	13.702***	14.169***	13.875***	14.517***	14.012***	13.949***
	(4.015)	(3.997)	(4.015)	(4.021)	(4.018)	(3.997)	(4.039)	(4.016)
Poverty	13.227***	13.047***	13.107***	13.263***	13.243***	13.004***	13.211***	13.065***
	(1.031)	(1.027)	(1.035)	(1.031)	(1.034)	(1.028)	(1.033)	(1.043)
Pop 15 to 64	-0.00000***	-0.00000***	-0.00000***	-0.00000***	-0.00000***	-0.00000***	-0.00000***	-0.00000***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Bail Ref * Viol Crime		0.359***						
		(0.111)						
Bail Ref *Jud Elec			73.091					
			(55.652)					
Bail Ref * Priv Bond Ban				-60.556				
				(48.106)				
Bail Ref *South					-10.472			
					(46.321)			
Bail Ref *Priv Jail						71.171***		
						(21.717)		
Bail Ref *State Party							-14.147	
							(38.171)	
Bail Ref *Poverty								4.921
								(4.772)
Constant	23.914*	28.665**	27.009*	22.658	23.683*	28.529**	23.981*	26.451*
	(14.005)	(14.005)	(14.196)	(14.035)	(14.050)	(13.996)	(14.013)	(14.219)
Observations	855	855	855	855	855	855	855	855
R^2	0.577	0.582	0.578	0.578	0.577	0.583	0.577	0.578
Adjusted R^2	0.573	0.577	0.573	0.573	0.572	0.578	0.572	0.573
Residual Std. Error	83.204 (df = 845)	82.743 (df = 844)	83.169 (df = 844)	83.175 (df = 844)	83.251 (df = 844)	82.729 (df = 844)	83.247 (df = 844)	83.201 (df = 844)
F Statistic	128.184*** (df = 9; 845)	117.700*** (df = 10; 844)	115.637*** (df = 10; 844)	115.604*** (df = 10; 844)	115.241*** (df = 10; 844)	117.769*** (df = 10; 844)	115.261*** (df = 10; 844)	115.481*** (df = 10; 844)
Note:								*p<0.1; **p<0.05; ***p<0.01

Appendix D: State Weights for Pretrial Synthetic Control Method – Passage Date Approach

State Weights for Kentucky	
State	**Unit Weight**
Alabama	0
Alaska	0
Arizona	0
Arkansas	0
California	0
Colorado	0
Connecticut	-
Delaware	-
District of Columbia	-
Florida	0
Georgia	0.049
Hawaii	-
Idaho	0
Illinois	0.233
Indiana	0
Iowa	0
Kansas	0
Kentucky	-
Louisiana	0.014
Maine	0
Maryland	0
Massachusetts	0
Michigan	0
Minnesota	0
Mississippi	0.086
Missouri	0
Montana	0
Nebraska	0
Nevada	0
New Hampshire	0
New Jersey	0
New Mexico	0.138
New York	0
North Carolina	0
North Dakota	0.178
Ohio	0
Oklahoma	0
Oregon	0
Pennsylvania	0
Rhode Island	0
South Carolina	0
South Dakota	0
Tennessee	0
Texas	0.066
Utah	0
Vermont	-
Virginia	0.237
Washington	0
West Virginia	0
Wisconsin	0
Wyoming	0

State Weights for New Jersey	
State	**Unit Weight**
Alabama	0
Alaska	0.179
Arizona	0.195
Arkansas	0
California	0
Colorado	0
Connecticut	-
Delaware	-
District of Columbia	-
Florida	0
Georgia	0
Hawaii	-
Idaho	0
Illinois	0
Indiana	0
Iowa	0
Kansas	0
Kentucky	-
Louisiana	0
Maine	0
Maryland	0.464
Massachusetts	0
Michigan	0
Minnesota	0
Mississippi	0
Missouri	0
Montana	0
Nebraska	-
Nevada	0
New Hampshire	0
New Jersey	-
New Mexico	-
New York	0
North Carolina	0
North Dakota	0.089
Ohio	0
Oklahoma	0
Oregon	0
Pennsylvania	0
Rhode Island	-
South Carolina	0
South Dakota	0
Tennessee	0
Texas	0
Utah	0
Vermont	-
Virginia	0
Washington	0
West Virginia	0
Wisconsin	0
Wyoming	0.073

State Weights for New Mexico	
State	**Unit Weight**
Alabama	0
Alaska	0
Arizona	0
Arkansas	0
California	0
Colorado	0
Connecticut	-
Delaware	-
District of Columbia	-
Florida	0
Georgia	0
Hawaii	-
Idaho	0
Illinois	-
Indiana	0
Iowa	0
Kansas	0
Kentucky	-
Louisiana	0.163
Maine	0
Maryland	0
Massachusetts	0
Michigan	0
Minnesota	0
Mississippi	0
Missouri	0
Montana	0
Nebraska	-
Nevada	0
New Hampshire	0
New Jersey	-
New Mexico	-
New York	0
North Carolina	0
North Dakota	0
Ohio	0
Oklahoma	0
Oregon	0
Pennsylvania	0
Rhode Island	-
South Carolina	0
South Dakota	0
Tennessee	0
Texas	0.837
Utah	0
Vermont	-
Virginia	0
Washington	0
West Virginia	0
Wisconsin	0
Wyoming	0

Appendix E: Stepwise Regression for Pretrial Rate Outcome with Interactions – Bail Reform Passage Date

	Dependent variable: Pretrial Rate							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bail Ref Passed	82.085***	-18.651	23.345	112.260***	93.135***	33.197*	84.585***	-62.534
	(17.604)	(34.935)	(34.799)	(24.808)	(21.361)	(19.496)	(18.588)	(59.742)
Viol Crime	0.048**	0.031	0.046**	0.043**	0.045**	0.028	0.048**	0.046**
	(0.019)	(0.020)	(0.019)	(0.019)	(0.020)	(0.019)	(0.019)	(0.019)
Jud Election	-9.814	-10.244	-12.744	-7.477	-8.975	-9.151	-9.611	-11.531
	(9.028)	(8.976)	(9.137)	(9.119)	(9.075)	(8.879)	(9.045)	(9.025)
Priv Bond Ban	-17.526**	-14.954*	-20.162**	-14.112*	-16.206*	-13.653*	-17.222**	-19.297**
	(8.256)	(8.244)	(8.352)	(8.481)	(8.383)	(8.150)	(8.292)	(8.260)
South	69.948***	74.273***	69.882***	71.983***	71.117***	76.797***	70.124***	70.482***
	(7.123)	(7.199)	(7.111)	(7.212)	(7.238)	(7.116)	(7.139)	(7.103)
Private Jail	29.721***	29.013***	29.602***	29.428***	29.608***	28.184***	29.707***	29.417***
	(1.985)	(1.985)	(1.983)	(1.990)	(1.990)	(1.973)	(1.987)	(1.983)
State Party	13.988***	14.811***	13.879***	14.497***	14.116***	15.423***	14.178***	14.366***
	(3.988)	(3.972)	(3.982)	(3.994)	(3.991)	(3.931)	(4.016)	(3.978)
Poverty	12.841***	12.583***	12.641***	12.835***	12.887***	12.163***	12.819***	12.357***
	(1.032)	(1.029)	(1.036)	(1.031)	(1.034)	(1.023)	(1.034)	(1.046)
Pop 15 to 64	-0.00000***	-0.00000**	-0.00000***	-0.00000***	-0.00000***	-0.00000**	-0.00000***	-0.00000***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Passed* Viol Crime		0.265***						
		(0.079)						
Passed*Jud Elec			79.653*					
			(40.728)					
Passed*Priv Bond Ban				-62.137*				
				(36.039)				
Passed*South					-34.513			
					(37.784)			
Passed*Priv Jail						78.453***		
						(14.388)		
Passed*State Party							-15.293	
							(36.358)	
Passed*Party								10.109**
								(3.992)
Constant	26.051*	33.401**	32.021**	24.775*	25.290*	37.442***	26.164*	33.876**
	(13.935)	(14.027)	(14.243)	(13.939)	(13.961)	(13.862)	(13.945)	(14.230)
Observations	855	855	855	855	855	855	855	855
R^2	0.582	0.588	0.584	0.584	0.583	0.596	0.582	0.585
Adjusted R^2	0.578	0.583	0.579	0.579	0.578	0.592	0.577	0.580
Residual Std. Error	82.712 (df = 845)	82.222 (df = 844)	82.574 (df = 844)	82.615 (df = 844)	82.720 (df = 844)	81.340 (df = 844)	82.752 (df = 844)	82.448 (df = 844)
F Statistic	130.836*** (df = 9; 845)	120.270*** (df = 10; 844)	118.529*** (df = 10; 844)	118.325*** (df = 10; 844)	117.813*** (df = 10; 844)	124.730*** (df = 10; 844)	117.656*** (df = 10; 844)	119.148*** (df = 10; 844)
Note:								*p<0.1; **p<0.05; ***p<0.01