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Abstract

Several states and localities have begun to implement various forms of pretrial justice reforms aimed at reducing the size of pretrial detainee population. However, empirical investigation of the effect of such reforms on crimes more broadly (other than recidivism) is limited. We analyze the effect of the New Jersey Criminal Justice Reform implemented in 2017 on property crimes. We find that property crime per 100,000 population increased by 22.5% within the first two years. Our findings suggest that reducing the likelihood of pretrial detention for less violent crimes can have substantial impact on behavioral incentives for offenders of such crimes.

Keywords: New Jersey Criminal Justice Reform, property crime, pretrial release, monetary bail, risk based assessment.
JEL Classification Codes: K14, K41, K42.

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1 Introduction

In the U.S., approximately half a million unconvicted individuals are awaiting trial while incarcerated, and the number of such pretrial detainees saw an increase of 433% from 1970 to 2015 (Vera Institute of Justice, 2019). In response, a number of states and localities across the U.S. have begun to reform their pretrial justice systems (see Table 1). While varying in detail, the primary purpose of these reforms is economically rooted. Specifically, the increase in the size of pretrial detainees imposes a great financial burden on our society. In addition, the resource-based pretrial release process that makes up the majority in most jurisdictions in the U.S. favors the haves over the have-nots. As such, one common aspect of these reforms has been to replace the use of money for bail (or "monetary bail") with a statistical risk assessment as a condition of release and to reduce the overall size of pretrial detainee population. Indeed, such reforms are found to be effective in reducing the number of pretrial detainees by anywhere from 16% to 90% (McKinley et al., 2019; Rahman, 2018; Veldman, 2014), as well as flight risk or pretrial recidivism (Barno et al., 2019; Goldkamp and White, 2006).

To the best of our knowledge, however, little is known about the related question of how such reforms affect crimes more broadly—other than recidivism. In this paper, we address this lacuna by examining the effect of pretrial justice reform on property crime. Property crime serves as a natural starting point of investigation given that the rationale for pretrial justice reforms is based on aforementioned economic motives and property crime is known to be well-explained by the economic theory of crime (Kelly, 2000). In addition, property crimes made up more than 85% of all offenses reported to the Federal Bureau of Investigation (FBI) in the Uniform Crime Report in 2018.

Research suggests that there are two channels through which a reform on criminal justice system can affect crime: (1) deterrence and (2) incapacitation (Dobbie et al., 2018; Miles and Cox, 2014). First, deterrence effect refers to the extent to which expected sanction changes the relative net benefit associated with committing a crime. Given that pretrial justice reforms aim to reduce the number of pretrial detainees, incentives for committing a property crime are expected to increase. Second, incapacitation refers to segregating certain individuals from our society. As the composition of pretrial detainees and those released into society changes, property crime is to be affected although the direction is ambiguous. Given these two forces at play, the overall net magnitude of the

Table 1: Examples of Reforms Adopted Across States and Localities.

	Year	Summary of Reform
Washington, D.C.	From 1963	Set out a presumption of unconditional pretrial release and adopted procedural protections.
Kentucky	From 1976	Banned commercial bail bonds and established pretrial services.
New Jersey	2017	Adopted risk-based assessment, set speedy trial limits, and amended constitutional right to bail.
Cook County, IL.	2017	Ability-to-pay determinations before setting bail and use risk assessment instrument.
Santa Clara County, CA.	2019	Adopted community-sponsored release and pretrial risk assessment.

Source: Doyle, Bains, and Hopkins (2019).
Note 1: New Mexico and Maryland also adopted reforms in 2014 and 2017, respectively. However, their reforms do not include using an algorithmic risk assessment (Doyle, Bains, and Hopkins; 2019).
Note 2: Since Washington D.C. and Kentucky conducted multiple series of reform, we list the first year when the reform was adopted.

effect of pretrial justice reform on property crime remains an empirical question.

In our empirical analysis, we exploit the implementation of the New Jersey Criminal Justice Reform (hereinafter "CJR") that went into effect in 2017. Using the county subdivision level data from the FBI for years between 2012 and 2018, we assess the effect of the CJR on property crime using difference-in-differences (DID) method. We find that since the implementation of the CJR, the overall property crime per 100,000 population in New Jersey saw an approximately 22.5% increase when accounting for a set of county subdivision characteristics, year fixed effects, state fixed effects, and state-specific time trend. Each of the three property crime types (burglary, larceny, and vehicle theft) saw an increase as well. Our graphical and estimation results validate the parallel pre-trend assumption and various sensitivity checks confirm that our findings are robust.

This paper is related to a few different streams of research. First, there is a large literature that assesses crime levels in association with various factors of interest, such as unemployment, inequality, wage distribution, education, etc.[1] In particular, this paper is closely related to prior work that analyzed the effectiveness of detaining certain segments

[1]For instance, see the following papers on these topics: unemployment (Cook, Watson, and Parker, 2014; Narayan and Smyth, 2004; Raphael and Winter-Ebmer, 2001), inequality (Choe, 2008; Kelly, 2000), wage distribution (Machin and Meghir, 2004), education (Groot and van den Brink, 2010; Lochner and Moretti, 2004), etc.

of population in changing the levels of crime, which was often studied in the context of detaining or deporting illegal immigrants (Miles and Cox, 2014; Stowell, Messner, Barton, and Raffalovich, 2013). This paper adds to this literature by investigating the impact of detaining certain individuals on property crime in the context of the CJR.

In addition, there are papers that specifically assess the outcomes of pretrial detention. Dobbie, Goldin, and Yang (2018) use the detention tendencies of quasi-randomly assigned bail judges to estimate their effects on outcomes such as probability of conviction, pretrial flight, recidivism, etc. In addition, there are studies that examine the effects of data-driven risk assessments that are increasingly being adopted to improve judges' pretrial release decisions. For example, Kleinberg et al. (2018) analyze and compare welfare gains from predictions derived from machine learning approach. Our paper adds to this literature by documenting the realized changes in property crimes following the implementation of a state-wide pretrial justice reform that requires the use of statistical risk assessment as a condition of pretrial release.

The rest of the paper is organized as follows. Section 2 discusses the institutional background of the CJR. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 presents the main results and Section 6 presents additional analyses. Section 7 concludes.

2 Institutional Background

Based on the principle of "innocent until proven guilty" and a constitutional right to a speedy trial, the New Jersey State Assembly passed two legislative measures associated with the CJR in 2014.[2] The two measures, 1) S946/A1910 and 2) SCR128, went into effect on January 1, 2017.

The first legislative measure, S946/A1910, moves New Jersey's pretrial release process from a largely money-based one to a risk-based one and sets speedy trial limits with respect to pretrial detention (American Civil Liberties Union, 2014). Prior to the reform, defendants who were unable to post monetary bail were held in jail even if they posed little risk of danger or flight. Under the CJR, when a defendant is charged on a warrant, the judge uses the Public Safety Assessment (PSA) that considers nine factors to predict

[2] For more background information, refer to the Criminal Justice Reform Information Center on the New Jersey Courts website (https://www.njcourts.gov/courts/criminal/reform.html).

his or her likelihood of failure to appear in court and risk of committing a new crime (New Jersey Courts, 2018).[3] The defendant is classified as low, moderate, or high risk, and the judge makes a pretrial release decision along with conditions of release, also considering arguments from the prosecutor and defense attorney.[4] If a defendant is detained pretrial, the CJR imposes that s/he is subject to speedy trial limits.[5]

The second legislative measure, SCR128, places a constitutional amendment to authorize pretrial detention of a person in criminal case under certain circumstances (American Civil Liberties Union, 2014). Previously, the New Jersey State Constitution required the court to grant bail to a defendant in a criminal case before trial. As a result, courts often ended up setting excessive bail amounts to prevent those accused of committing serious offenses from being released. However, this did not prevent those who could afford to pay from being released. Therefore, proposed amendment to change the constitutional right to bail was submitted on a ballot and was passed with a 61.8% consent in the general election (Department of State New Jersey Division of Elections, 2014). Article I, Paragraph 11 of the New Jersey Constitution now further stipulates that pretrial release of an offender may be denied if the court finds that no conditions of pretrial release would ensure his appearance in court when required, safety of others, or prevention of obstructing the criminal justice process.

3 Data

The data used in this paper comes from two sources. First, we use logged number of various types of offenses reported to law enforcement per 100,000 population from the FBI's Uniform Crime Reports (UCR) at the county subdivision level. This is annually reported data and we use data up to the most recent years currently available, from 2012 to 2018.[6] We examine both the aggregate property crime rate and the rate of each

[3]The nine risk factors are: age at current arrest, current violent offense, pending charge at the time of arrest, prior disorderly persons conviction, prior indictable conviction, prior violent conviction, prior failure to appear pretrial in past 2 years, prior failure to appear pretrial older than 2 years, and prior sentence to incarceration.

[4]The judge can still set a monetary bail, but not for the purpose of keeping the defendant in jail.

[5]There are three distinctive limits under the CJR: 1) an indictment must be returned/unsealed within 90 days; 2) a trial must start within 180 days after indictment; and 3) there is an overall limit of 2 years from detention to trial (Rabner and Grant, 2019).

[6]The UCR data is constructed based on reports submitted by police or state agencies to the FBI. Therefore, despite efforts to improve completeness and consistency of the data, there are some limitations

individual property crime type (i.e., burglary, larceny, and vehicle theft).

Second, consistent with prior literature, we collect the following characteristics for each county subdivision for each year from the American Community Survey of the U.S. Census Bureau: share of black, share of households receiving Supplemental Nutrition Assistance Program (SNAP), share of households with female head, share of young population aged between 15 and 24, and unemployment rate. We match the data from American Community Survey to the UCR data using state-place federal standard geographic codes (FIPS codes). This gives us 486 and 2,642 units of county subdivisions in New Jersey and the Northeast region excluding New Jersey (i.e. Connecticut, Delaware, Maine, Massachusetts, Maryland, New Hampshire, New York, Rhode Island, and Vermont; Hobbs (2009)) for each of the years from 2012 to 2018, respectively. Because the classification of the Northeast region may vary across sources, we also use an alternative definition of the Northeast region that excludes Maryland and Delaware—because these two states are sometimes categorized as the South (e.g., U.S. Census)—and check whether results still hold in Section 6.1.

Table 2: Summary Statistics.

	New Jersey	Northeast region	Diff
Panel A: Property Crime			
log(Property crime per 100,000)	7.134 (0.908)	6.965 (1.440)	0.169**
log(Burglary per 100,000)	5.383 (1.200)	5.061 (1.611)	0.322**
log(Larceny per 100,000)	6.812 (0.968)	6.671 (1.525)	0.140**
log(Vehicle theft per 100,000)	3.342 (1.774)	2.990 (1.934)	0.352**
Panel B: Other Characteristics			
Share of black	7.874 (12.225)	5.138 (10.816)	2.735
Share of households receiving SNAP	5.314 (5.763)	10.442 (8.223)	-5.127
Share of households with female head	0.164 (0.088)	0.175 (0.089)	-0.011***
Share of young (between age 15 and 24)	12.034 (3.551)	13.011 (6.601)	-0.976
Unemployment rate	8.609 (3.454)	7.629 (3.308)	0.980*

Data Source: UCR and American Community Survey of the U.S. Census Bureau.
Note: Numbers in parentheses are standard deviations. Asterisks ***/**/* denote $p<0.01$, $p<0.05$, $p<0.1$, respectively.

Table 2 provides summary statistics. In Panel A, we compare property crime rate statistics in New Jersey to those in the Northeast region prior to the implementation of

such as potential under-reporting of crimes.

the CJR in 2017. New Jersey tends to have slightly higher crime rates than the Northeast region. However, note that the observed differences are not necessarily problematic, as long as they do not change systematically during our sample period. We will investigate this issue in depth in Section 5. In Panel B, we further compare demographic characteristics of New Jersey to those of the Northeast region at the county subdivision level. Except for the share of households with female head and unemployment rate, the differences in the share of black, share of household receiving SNAP, and share of young between New Jersey and the Northeast region are found to be statistically insignificant.

4 Empirical Strategy

To identify the effects of the CJR, we compare the changes in crime rates in New Jersey to the changes in the other states in the Northeast region before and after the reform using the following DID specification:

$$log(Crime)_{ist} = \alpha + \beta \cdot NJ_i \cdot Post_t + X_{ist}\gamma + \delta_s + \rho_t + \epsilon_{ist}. \tag{1}$$

Throughout this paper, we measure $log(Crime)_{ist}$ using the logged number of each type of offenses per 100,000 population as reported in the UCR. The subscripts i, s, and t capture county subdivision, state, and year, respectively. NJ_i is a binary indicator which equals 1 if county subdivision i is located in New Jersey, and 0 otherwise. $Post_t$ equals 1 for years 2017-2018, and 0 otherwise. X_{ist} is a vector of demographic characteristics of county subdivision i located in state s at time t, such as the share of black, share of households receiving SNAP, share of households with female head, share of young, and unemployment rate. δ_s is a state-fixed effect which controls for unobserved time-invariant heterogeneity within the state. ρ_t is a year fixed effect that controls for common trends within each year. Lastly, ϵ_{ist} is an idiosyncratic shock. To account for serial correlation within a state, we cluster standard errors at the state level. In our baseline analysis, we use the aforementioned states in the Northeast region that are geographically proximate to New Jersey as the control group.

The key identifying assumption in DID to recover the causal impacts of the CJR (i.e., β in equation (1)) is that the outcomes of interest between New Jersey and the control states in the Northeast region are parallel in the absence of the reform. To test the validity of this assumption, we first graphically examine whether the trends of the outcome variables are indeed parallel before the reform period. In addition, we formally test the parallel trend assumption by estimating the following equation using the data during the pre-reform period (i.e., before 2017).

$$log(Crime)_{ist} = \alpha + \beta \cdot NJ_i \cdot t + X_{ist}\gamma + \delta_s + \rho_t + \epsilon_{ist}. \tag{2}$$

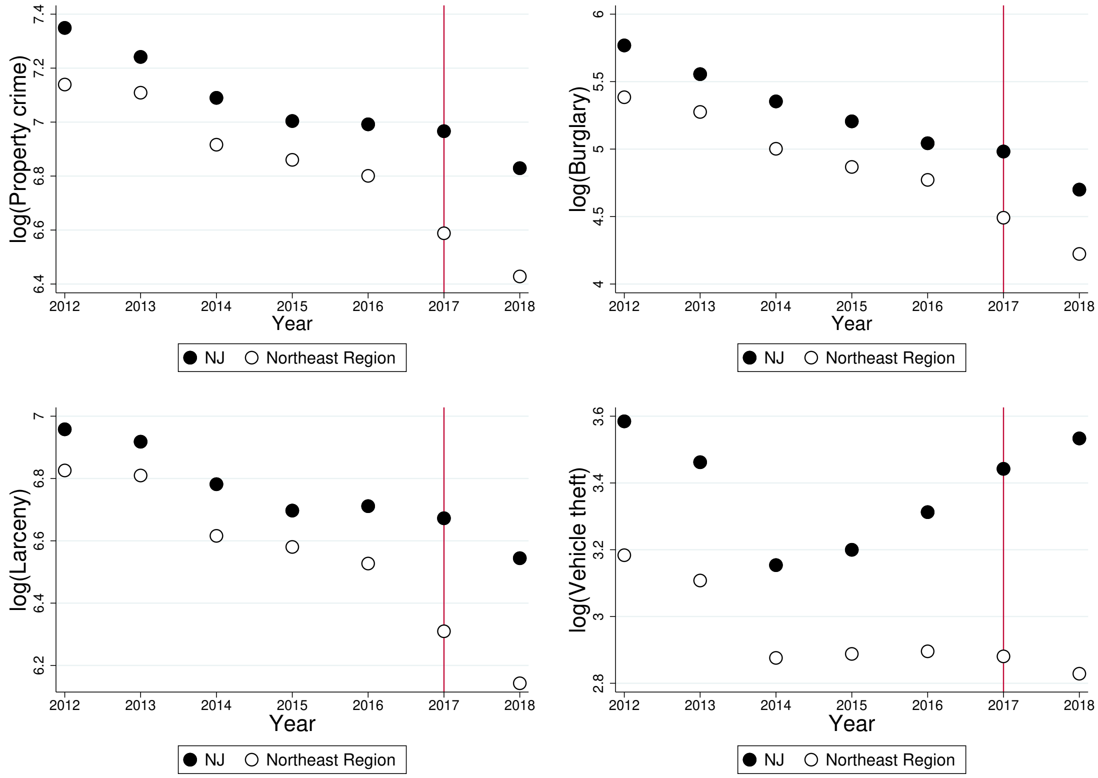
The goal is to see whether the coefficient β in equation (2) is statistically insignificant from zero. If β is statistically insignificant from zero, then this further provides evidence that the parallel trend assumption is not violated.

We also conduct various robustness checks and additional analyses. First, we conduct our analyses using two alternative control groups: 1) the Northeast region excluding Delaware and Maryland; and 2) all other 49 U.S. states. Second, following Abadie et al. (2010), we construct synthetic control states such that they are most comparable to New Jersey before the CJR in terms of various crime rates and other control variables. Then we compare how the trends diverged after the CJR between New Jersey and these synthetic control states. Third, following an event study approach, we estimate coefficients on time relative to the implementation of the CJR and track their trajectories. Fourth, we explore whether the effects of the CJR on crime rates were heterogeneous across county subdivisions within New Jersey given their poverty levels. Lastly, we also estimate and compare the impact of the CJR on violent crimes at the aggregate level.

5 Results

To begin, we check the parallel trend assumption by illustrating the trends of logarithmic property crime rates in New Jersey and the control group (i.e., other states in the Northeast region) in Figure 1. The units of all crime rates in the y-axis are in terms of logged crime per 100,000 population. Although crime rates were slightly higher in New Jersey relative to the control group, the figures show that there was a comparable parallel path before the implementation of the CJR in 2017, which is marked by a vertical red line. The trends of vehicle theft seems to show a slight divergence from 2016, but the gap between the rest of property crime rates in New Jersey and those in control group only widens from 2017. In addition to the graphical illustrations, we further check the parallel trend as-

Figure 1: Trends of Property Crime Rate: NJ vs. Northeast Region.



Notes: Vertical lines mark the implementation timing of the CJR. All variables are measured as the logged crime per 100,000 population.

sumption by estimating equation (2) for years before the CJR's implementation. Results are reported in Table 3. None of the coefficients for NJ $\times$ t are statistically significant at standard levels, suggesting that the parallel trend assumption is not violated.

Based on these checks conducted on the key identifying assumption, we now estimate the main equation (1) using OLS. The key variable of interest is $NJ \times Post$, which measures the extent to which property crime rate is affected by the implementation of the CJR. The results are reported in Table 4. In Panel A, we report the effects on the overall property crime rate, followed by results in Panel B on each of the three property crime types. In column (1), we do not include any set of controls or fixed effects. Given such specification, the impact of the CJR is estimated to be statistically insignificant. In column (2), we include the aforementioned set of control variables and year fixed effects. We find that overall property crime rate, as well as the rates of burglary, larceny, and

Table 3: Testing the Parallel Trend Assumption before 2017.

Dep. variable:	Property Crime	Burglary	Larceny	Vehicle Theft
NJ $\times$ t	0.001	-0.010	0.011	-0.002
	(0.017)	(0.013)	(0.019)	(0.024)
Observations	11,921	11,926	11,924	11,925
R-squared	0.11	0.17	0.09	0.18
Controls	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
State Fixed Effects	Y	Y	Y	Y

Note: All dependent variables are measured as the logged crime per 100,000 population. For control variables, we use share of black, share of households receiving SNAP, share of households with female head, share of young, and unemployment rate. Standard errors in parentheses are corrected for heteroskedasticity and clustered at the state level. Asterisks ***/**/* denote p<0.01, p<0.05, p<0.1, respectively.

vehicle theft, all show statistically significant increases. Such results are still robust, after including state fixed effects in column (3), although the magnitude of effects tend to become slightly smaller. In column (4), we include state-specific time trend to further minimize the omitted variable bias. That is, we address a potential concern that our estimates are just picking up some state-specific time trend that arises from something other than the CJR. Relative to the other states in the Northeast region, we still find consistent positive effects of the CJR on property crime rates. New Jersey experienced a 22.5% increase in overall property crime per 100,000 population accounting for a set of county subdivision characteristics, year fixed effects, state fixed effects, and state-specific time trend. Larceny, which had the smallest magnitude of impact, still increased by approximately 19.9% with the same specification.

The behavioral effect of the CJR on property crime through the deterrence channel is expected to be positive. As it reduces the number of pretrial detainees, it can increase incentives to commit a nonviolent offense. On the other hand, the effect of the CJR on property crime through the incapacitative channel is less clear as the composition of the detained and the released changes. To the extent that pretrial detainees under the CJR's risk-based assessment process are indeed more likely to commit crimes, the CJR's impact on property crime is likely to be negative. However, the impact could potentially be positive if those who are released into the society under the risk-based assessment system have higher risks of committing crimes. Based on previous findings in the literature, it

Table 4: The Effects of the CJR on Property Crime Rate.

	(1)	(2)	(3)	(4)
Panel A: Overall Property Crime				
	log(Property Crime per 100,000 population)			
NJ × Post	0.025	0.402**	0.211***	0.203**
	(0.140)	(0.145)	(0.047)	(0.090)
Observations	17,764	17,746	17,746	17,746
R-squared	0.00	0.09	0.13	0.13
Panel B: Property Crime by Type				
	log(Burglary per 100,000 population)			
NJ × Post	-0.085	0.510**	0.154***	0.195***
	(0.207)	(0.208)	(0.036)	(0.057)
Observations	17,770	17,752	17,752	17,752
R-squared	0.00	0.13	0.19	0.19
	log(Larceny per 100,000 population)			
NJ × Post	0.030	0.393**	0.224***	0.182*
	(0.133)	(0.137)	(0.053)	(0.093)
Observations	17,767	17,749	17,749	17,749
R-squared	0.00	0.08	0.11	0.11
	log(Vehicle theft per 100,000 population)			
NJ × Post	0.482**	0.488*	0.274***	0.254**
	(0.208)	(0.239)	(0.066)	(0.114)
Observations	17,769	17,751	17,751	17,751
R-squared	0.00	0.12	0.19	0.19
Controls		Y	Y	Y
Year Fixed Effects		Y	Y	Y
State Fixed Effects			Y	Y
State-specific time trend				Y

Note: For control variables, we use share of black, share of households receiving SNAP, share of households with female head, share of young, and unemployment rate. Standard errors in parentheses are corrected for heteroskedasticity and clustered at the state level. Asterisks ***/**/* denote p<0.01, p<0.05, p<0.1, respectively.

does not seem to be the case that the increase in crime rate is driven by recidivism of those who are no longer detained pretrial. For example, Dobbie, Goldin, and Yang (2018) find that "initial pretrial release has a small and statistically insignificant effect on post-trial incarceration." Therefore, the magnitude of incapacitative effect on property crime is unlikely to be large, even if it is positive. Although our findings cannot separate the magnitudes of deterrence effect and incapacitative effect, the overall net effect implies that positive effect from the deterrence channel seems to be non-negligible. That is, the extent to which removing pretrial detention for less violent crimes seems to have substantial effects on behavioral incentives for offenders of such crimes.

6 Additional Analyses

6.1 Alternative Control Groups

Table 5: The Effects of the CJR on Property Crime using Alternative Control Groups.

Dep. variable:	Property Crime	Burglary	Larceny	Vehicle Theft
Panel A: Alternative Control Group 1: Northeast Region excluding DE and MD				
NJ × Post	0.216**	0.202***	0.195*	0.269**
	(0.091)	(0.058)	(0.095)	(0.116)
Observations	16,975	16,981	16,978	16,980
R-squared	0.13	0.19	0.11	0.19
Panel B: Alternative Control Group 2: All Other States in the U.S.				
NJ × Post	0.086*	0.085**	0.066	0.302***
	(0.043)	(0.040)	(0.045)	(0.042)
Observations	65,082	65,134	65,162	65,202
R-squared	0.15	0.21	0.12	0.20
Controls	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
State Fixed Effects	Y	Y	Y	Y
State-specific linear time trend	Y	Y	Y	Y

Note: All dependent variables are measured as the logged crime per 100,000 population. In Panel A, the control group includes Connecticut, Maine, Massachusetts, New Hampshire, New York, Rhode Island, and Vermont. For control variables, we use share of black, share of households receiving SNAP, share of households with female head, share of young, and unemployment rate. Standard errors in parentheses are corrected for heteroskedasticity and clustered at the state level. Asterisks ***/**/* denote $p<0.01$, $p<0.05$, $p<0.1$, respectively.

Despite our evidence shown in Figure 1 and Table 3, there may still be concerns that other states in the Northeast Region may not serve as most suitable control group to predict counterfactual outcomes in the absence of the CJR. To confirm the robustness of our findings, we repeat our DID analyses using two alternative control groups. In Panel A, we report the results based on an analysis where the control group is defined as the Northeast region excluding Delaware and Maryland. In Panel B, we use all other 49 states in the U.S. besides New Jersey as the control group. We report our estimates that include all controls, year fixed effects, state fixed effects, and state-specific linear time trend using these two alternative control groups in Table 5. We find that except for the statistically insignificant effect on larceny in Panel B, all other effects are still positive and statistically significant at standard levels.

6.2 Synthetic Control Method

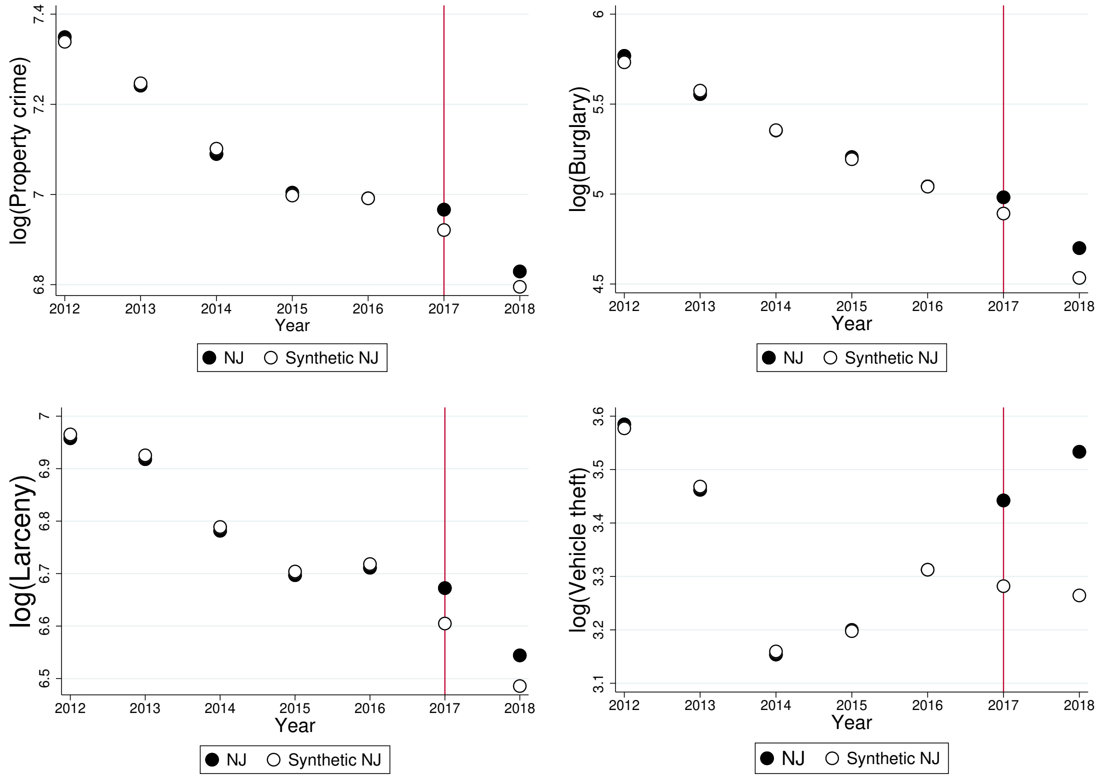
To construct a yet another alternative control group, we follow Abadie et al. (2010). That is, we use crime rates before the CJR implementation and the set of control variables to construct a weighted combination of states in the Northeast region to be used as the control group that most closely fits New Jersey before the reform.[7] By comparing New Jersey to this synthetic control group, we can predict what would have happened to property crime rate in New Jersey had the CJR not been implemented. Figure 2 shows the trends of property crime rates in New Jersey and the synthetic control group. By construction, the levels of property crime rates for New Jersey and synethtic control group are almost identical prior to the reform. Following the reform in 2017, however, all four graphs show that there is a diverging trend as New Jersey faces an increase in property crime rates as compared to the synthetic control group. These results are consistent with our previous findings.

6.3 Year-specific Coefficient Trajectories.

We use the event-study analysis method to examine the evolution of property crime rate for each of the years before and after the implementation of the CJR. This not only helps us to further validate the identification assumption, but also to check the magnitude of

[7] We use the Stata module "synth," following Abaide et al. (2011).

Figure 2: Trends of Property Crime Rates Using Synthetic Control.



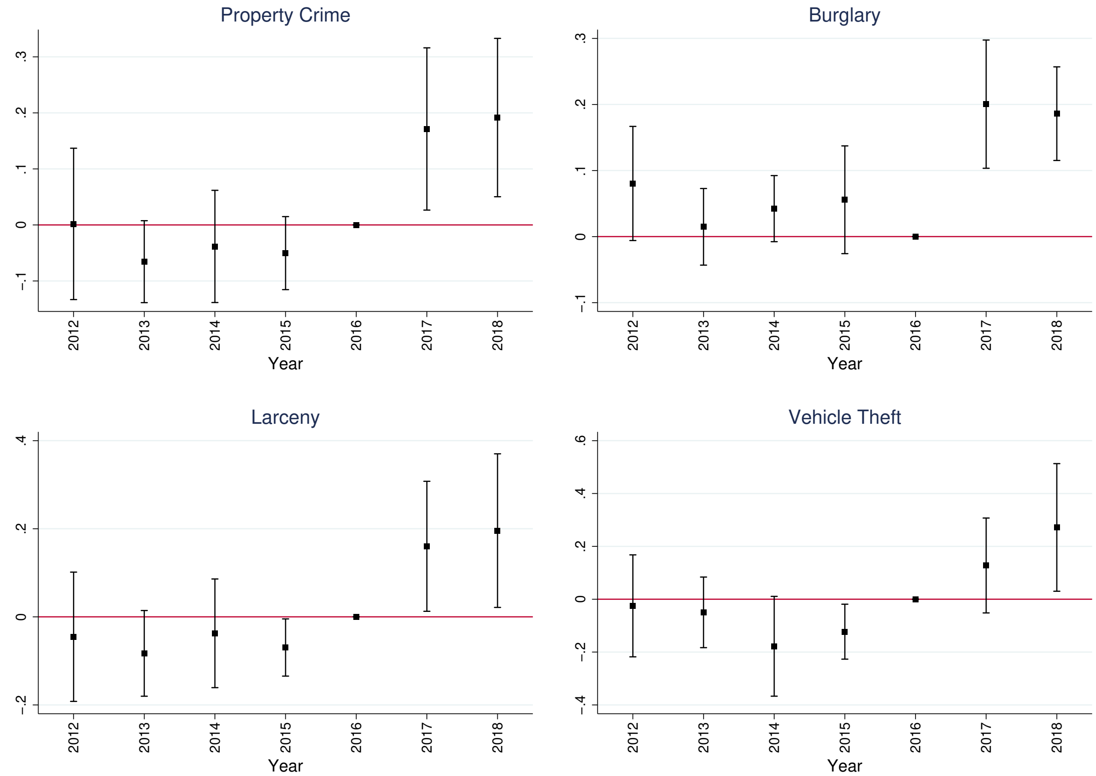
Notes: Vertical lines mark the implementation timing of the CJR in New Jersey. All variables are measured as the logged crime per 100,000 population. To calculate state weights in synthetic New Jersey, we used each type of property crime rates, share of black, share of households receiving SNAP, share of households with female head, share of young, and unemployment rate prior to the reform.

the effects of the CJR across years. For this aim, we estimate the following:

$$(3)\quad log(Crime)_{ist} = \alpha + \sum_{k \in \{-5,-4,-3,-2,0,1\}} \beta_k NJ_i \cdot I(t = 2017 + k) + X_{ist}\gamma + \delta_s + \rho_t + \epsilon_{ist}.$$

The coefficients of interest are the β_k's, which are the coefficients on time indicators relative to the CJR. For instance, $k = -2$ captures the impact on crime rate two years before the CJR. One year before the CJR, or year 2016 equivalently, is the omitted category. We show the estimated β_k's for each property crime type, along with the 95% confidence intervals in Figure 3. This allows us to visually assess how crime rate outcomes appear to have changed sharply around the reform. Almost all coefficients for the years

Figure 3: Year-specific Coefficient Trajectories



Notes: The points in each figure represent the estimated effects of event time (i.e., the β_k's from equation (3)), along with bars representing the 95% confidence intervals. One year before the CJR (i.e., year 2016) is the omitted category.

prior to the CJR are insignificantly different from zero; if anything, it appears that vehicle theft and larceny have slightly decreased two years before the CJR. Once the CJR went into effect, we find that the rates of the overall property crime, burglary, larceny, and vehicle theft all increased. In addition, we find that the magnitude of the increase in the year of the CJR's implementation (i.e., 2017) was very similar to that in year 2018, except for vehicle theft which showed a further increase in 2018.

6.4 Heterogeneous Effects

We now analyze heterogeneous effects of the CJR across the different county subdivisions in New Jersey. In particular, we investigate whether the increase in property crimes

Table 6: Heterogeneous Effects of the CJR by SNAP Share.

	(1)	(2)	(3)
A. Overall Property Crime			
	log(Property Crime per 100,000 population)		
NJ × Post	0.312*	0.141**	0.132
	(0.157)	(0.062)	(0.095)
NJ × Post × SNAP	0.013***	0.010**	0.010**
	(0.004)	(0.004)	(0.004)
Observations	17,764	17,746	17,746
R-squared	0.09	0.13	0.13
B. Property Crime by Types			
	log(Burglary per 100,000 population)		
NJ × Post	0.382	0.079	0.120
	(0.236)	(0.064)	(0.068)
NJ × Post × SNAP	0.019***	0.011*	0.011*
	(0.005)	(0.005)	(0.005)
Observations	17,752	17,752	17,752
R-squared	0.13	0.19	0.19
	log(Larceny per 100,000 population)		
NJ × Post	0.295*	0.140*	0.098
	(0.146)	(0.067)	(0.098)
NJ × Post × SNAP	0.014***	0.012**	0.012**
	(0.004)	(0.005)	(0.005)
Observations	17,749	17,749	17,749
R-squared	0.08	0.11	0.11
	log(Vehicle theft per 100,000 population)		
NJ × Post	0.375	0.255***	0.235*
	(0.249)	(0.064)	(0.107)
NJ × Post × SNAP	0.016**	0.002	0.002
	(0.005)	(0.007)	(0.007)
Observations	17,751	17,751	17,751
R-squared	0.12	0.19	0.19
Controls	Y	Y	Y
Year Fixed Effects	Y	Y	Y
State Fixed Effects		Y	Y
State-specific time trend			Y

Note: For control variables, we use share of black, share of households receiving SNAP, share of households with female head, share of young, and unemployment rate. Standard errors in parentheses are corrected for heteroskedasticity and clustered at the state level. Asterisks ***/**/* denote p<0.01, p<0.05, p<0.1, respectively.

was particularly larger for poorer areas, which have higher shares of households receiving SNAP. We present the estimation results in Table 6, which shows that the increase in the overall property crime rate, as well as burglary and larceny, were particularly larger in county subdivisions with higher SNAP shares. Alternatively, we tried using the share of households below poverty level instead of SNAP share and find estimates to be mostly consistent. Overall, our findings in Table 6 suggest that when the CJR reduces the expected sanction on less violent crimes by releasing offenders of such crimes pretrial, there may be more marginal offenders who now commit property crimes in poorer neighborhoods.

6.5 Effects on Violent Crime

Table 7: The Effects of the CJR on Violent Crime.

	(1)	(2)	(3)	(4)
Dep. variable:	log(Violent Crime per 100,000 population)			
NJ $\times$ Post	-0.421**	-0.319	-0.179***	-0.001
	(0.123)	(0.199)	(0.032)	(0.046)
Observations	17,762	17,744	17,744	17,744
R-squared	0.00	0.17	0.22	0.22
Controls		Y	Y	Y
Year Fixed Effects		Y	Y	Y
State Fixed Effects			Y	Y
State-specific time trend				Y

Note: For control variables, we use share of black, share of households receiving SNAP, share of households with female head, share of young, and unemployment rate.. Standard errors in parentheses are corrected for heteroskedasticity and clustered at the state level. Asterisks ***/**/* denote $p<0.01$, $p<0.05$, $p<0.1$, respectively.

Instead of property crimes, we alternatively analyze the effects of the CJR on violent crimes for a comparison. Given that each of the violent crime types (e.g. murder, rape, etc.) tends to have very small number of occurrences at the county subdivision level, we focus on the overall violent crime rate. Unlike property crime, violent crime tends to have less clear economic motives. For example, Kelly (2000) finds that "Property crime is well explained by the economic theory of crime, while violent crime is better explained by strain and social disorganization theories" (p.530). However, if we just focus on deterrence and incapacitative effects on violent crimes, the effects are expected to be negative to the extent that the risk-based assessment system under the CJR successfully detains offenders of violent crime. We check whether this is the case in Table 7 by estimating equation (1) with violent crime rate as the dependent variable. The results show that effects are all estimated to be negative across specifications, although it is insignificant when controls, year fixed effects, state fixed effects, and state-specific time trend are included in the specification.

7 Conclusion

In this paper, we investigate the impact of pretrial justice reform on property crime using the implementation of the New Jersey Criminal Justice Reform in 2017. We find that the overall property crime rate, as well as the rate of each property crime type (i.e., burglary, larceny, and vehicle theft), all increased within the first two years of the reform's implementation. This implies that the extent to which the CJR affects behavioral incentive to commit a property crime by reducing the expected sanctions upon arrest is non-negligible. We conduct various sensitivity checks and additional analyses to provide robustness of our findings.

Whereas our findings provide one of the first pieces of evidence on how pretrial justice reforms can affect property crime, there are some caveats as well as possible extensions to consider. First, as our analysis is currently limited to the state of New Jersey, the effects may not be readily generalized to other states due to potential heterogeneity. Although there were reforms adopted by other localities and states as can be seen in Table 1, observations from other state-level reforms during recent periods are lacking. As more states adopt similar reforms, it would be interesting to explore effects on a broader scale in the future by exploiting the variations in timing of the implementations of such reforms across different states. Second, we do not have individual-specific information on crime. With additional micro-level data and modelling, it would be interesting to further investigate the separate magnitudes of deterrence and incapacitation effects, as well as the recidivism of those released pretrial versus and arrests by first-time offenders. Third, as the CJR was implemented relatively recently, we focused on its short term consequences for the two-year post-period using all the data currently available. As effects could dynamically evolve across time, one could further include more years as data becomes available to assess mid to long term consequences. Finally, it is possible that the increase in property crime rate is affected by increased efforts in monitoring and policing. Nevertheless, we believe this is less likely to be the driving force following the analogous logic as in Kang (2017). If this were the case, then it is highly likely that we would have observed increases in all types of crime rates. However, we only find consistent increases in property crimes but not in violent crimes.

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