The partisan effect hypothesis and efficient crime policy

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Abstract

This paper examines whether Republican governors fight crime more efficiently than Democrats. Empirical evidence on whether Republicans can rightfully credit themselves as 'tough on crime' or as the party of 'law and order' is inconclusive. This paper tackles the question differently: First, it employs a difference-in- differences model, as opposed to standard Fixed Effects models that have previously been used to investigate partisan effects. Second, it examines social efficiency; that is, it goes further than asking a question of effectiveness, but puts costs and benefits into perspective. As such, it estimates how within a state crime variables and, thus, social efficiency change when one party takes over the executive branch from another. It will be argued that the partisan effect hypothesis does not hold with respect to crime. Closer investigation shows that the rate of newly admitted prisoners, as opposed to overall incarceration rates, tends to increase under Republican governors. However, there is no evidence that higher crime rates are a possible cause of this change. This paper concludes that this is an indication of social inefficiency and suggests further avenues for future research.

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I. Introduction

In 2001, 43 per cent less homicides and 34 per cent less violent crimes were committed in the United States, compared to a decade before (FBI, 2016). These developments raise questions about whether police, the economy or politicians can be credited for lower crime rates; or more generally: How can we reduce crime? Policymakers, however, ask a slightly different question: How can we reduce its cost to society? This paper complements the existing literature by analysing whether Republican or Democratic governors fight crime more efficiently. As such, this paper aims to inform policymakers and NGOs about which party is more likely to reduce social inefficiencies with respect to crime. Moreover, it is written for the American parent who wants to reduce the chance of her children becoming victims of neighbourhood crime and the American voter who wants to fact-check whether her tax dollars are well spent.

Crime carries a social cost. In 2007 criminal activity caused over \$15bn in economic losses to victims and caused federal and state governments to spend \$179bn on police protection, the judicial system and corrections (McCollister, et al., 2010). This accounts for more than one cent in every dollar earned in the United States. Criminologists have estimated the overall cost of crime to society to lie at around \$1,800 per capita per year (Miller, et al., 1996). Moreover, crime causes vast externalities not captured by social cost estimates, ranging from reduced trust in institutions to decreasing opportunities and educational attainment (Burdick-Will, 2013). In his inaugural address President Donald Trump hastened to stress the importance his cabinet would give to the social cost associated with "the crime and gangs and drugs that have stolen too many lives and robbed our country of so much unrealized potential" (White House, 2017).

The motivation for this paper stems from the 'stylised fact' that Republicans are 'tough on crime' and previous, partly conflicting, evidence: While the public choice literature suggests that by and large governors behave in a rather unideological manner (Congleton, 2002), there is evidence that Republican executives allocate more resources on policing and prisons (Gerber & Hopkins, 2011). Thereby, Republicans might influence policy outcomes, i.e. reduce criminal activity more successfully than Democrats. While the question about 'toughness' is rather trivial, this paper asks the question: *Do Republican governors fight crime more efficiently than Democrats?*

In this paper I test this hypothesis by way of a difference-in-differences (henceforth: DiD) model. This has previously not been applied to partisan effects analyses. Moreover, in contrast to *(i) effectiveness*, this paper stresses *(ii) social efficiency*. If a society spends more resources on prisons and policing, accompanied only by a minor reduction in crime, we would conclude that such policies might be more effective, but, ultimately, inefficient (Ravallion, 2003). As such, I examine how costly outcomes of governors' actions are to society.

I find little evidence to support the hypothesis of either party being more effective in fighting crime. However, my analysis also shows that under Republicans admissions to prison increase

without an accompanied increase in crime, indicating social inefficiencies. This does confirm the hypothesis that Republicans might want to *appear tough* on crime, more so than Democrats. Using my estimation results I conduct an indicative calculation of the additional social cost of Republicans being in power. This finding complements prior work by offering another dimension to compare Republicans' and Democrats' crime reduction efforts. I will conclude that more work with respect to the precise mechanisms ought to be done.

The remainder of this paper is structured as follows: Section II reviews the literature from the economics of crime and quantitative political science; Section III introduces the approach used throughout this paper; Section IV discusses the findings, in particular, with respect to the question of efficiency. Section V concludes.

II. Literature Review

The economics of crime

Since its inception by Becker (1968) the economics of crime has successfully established itself as a sub-discipline of economics. Two major strands can be identified. Becker's approach, well founded in the neoclassical Chicago tradition, aims to give microeconomic reasoning to the behaviour of criminals. A simple formalisation of this strand is the *rational criminal's* expected utility function $u_i = w_i - \rho d_i$ which depends on the utility w_i from committing a crime minus the disutility from punishment d_i, if caught under probability ρ (Chalfin & McCrary, 2014). This strand views criminal behaviour as individual choice influenced by the consequences faced and seeks to inform policy by aggregating these choices (Machin & Marie, 2014). I term this micro-founded theory the *bottom-up approach*.

On the other hand, the application of sophisticated econometric techniques in analysing the causes of crime has increased strongly in the past decades (Marie, 2013). I will refer to this set of literature as the *top-down approach*. It has grown, in particular, due to the need for explanations for the drastic fall in nearly all crime statistics in the USA over the 1990s (Spelman, 2000). Unlike what is commonly believed, novel policing strategies, stricter gun control laws and the strong economy of the nineties had little influence on the decline in criminal acts. Rather, it was a surge in the number of police, increases in the prison population and legalised abortion that have contributed to falling crime rates (Donohue & Levitt, 2001).

In studying how policy might deter and reduce crime, it becomes apparent how the two identified strands (Paternoster, 2010) in the economics of crime link together: While capital punishment should not deter a rational criminal, given the rarity of executions, an increase in the probability of punishment ρ disincentivises *forward-looking criminals* from committing otherwise attractive crimes (Levitt, 2004). These effects can be tested quantitatively. Politicians can influence both the probability of punishment ρ as well as the severity of punishment d_i. As such, if ideological differences cause parties to act in different ways one can hypothesise that the party in power can be linked to particular outcomes; this is the *partisan effects hypothesis* (Izraeli & Folland, 2007; Reed, 2006).

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The partisan effects hypothesis

Studying policy outcomes as a function of party control is prone to endogeneity. Since voters choose politicians, factors driving voter preferences might bias the estimates. In a simple OLS model of the Republican vote-share on crime $Rvote_{it} = \alpha + \beta crime_{it} + \cdots + \epsilon_{it}$ bias can take the following form:

$$\hat{\beta} = \beta + \partial \frac{\text{cov}(crime_{it}, preferences_{it})}{\text{var}(crime_{it})}$$

where ∂ is the coefficient on the omitted variable $preferences_{it}$. Preferences might well be a function of crime, thus, introducing reverse causality (Ferreira & Gyourko, 2007). As such, there is reason to suggest the explanatory variable (party control) to be correlated with the error term (Pettersson-Lidbom, 2008).

Cases where the ruling party narrowly won, can be considered subject to almost randomly assigned political power (Lee & Lemieux, 2010). Employing a regression discontinuity design (henceforth: RDD) of this kind, Petterson-Lidbom (2008) found significant differences in the fiscal policy adopted in Swedish local governments, depending on whether a right-wing or left-wing party holds office. Similarly, Gerber and Hopkins (2011) show that while under Democratic mayors social expenditure on social programmes increase, policing and police pay suffer. They conclude that "there might not be a Republican way to collect the trash, but there is a Republican way to spend on policing" (Gerber & Hopkins, 2011, p. 337).

In an analysis of US governors Leigh (2008) argues that a lack of data makes a state-level RDD unfeasible. Thus, he includes vote-share as a control and investigates reasonably contested races. He concludes that the party in power makes little difference to most policy settings, broadly arguing in favour of the *Median Voter Theorem* (Izraeli & Folland, 2007). While he does not identify significant effects on crime, he finds evidence for increased incarceration rates under Republicans. Little, however, does his method allow for inferences about what happens when a Republican takes over from a Democrat, or vice versa.

This is where the literature on partisan effects stands, and where my paper picks up. In using a difference-in-differences model to investigate changes in political power, I complement both the economics of crime as well as the political science literature. Lastly, I connect them to a major concern of criminologists, that is, the social costs of different crimes and the benefit-to-cost ratios of certain programmes. I will show that while neither party is more effective in reducing crime, Republicans seem to cause social inefficiencies by raising the amount of people admitted to prison, without there being an increase in crime which would merit such actions.

III. Data & Methodology

Data & Sources

This study uses a self-compiled panel dataset, with annual state-level data covering the period 1973-2016.[1] It is built from six sources; with election and governor data from the Inter-University Consortium for Political Science Research (1995), and crime variables from the FBI's Uniform Crime Reporting programme (2016). Data on prisons and police employees have been requested from the Bureau of Justice Statistics (2016) and the FBI Police Employees Masterfile (2017), respectively. The core variables in my dataset are data on a state's governor's party and the following rates, measured per 100.000 residents:

Table 1: Overview of dependent variables

Crime rates	include violent crime, murder, rape, property crime and theft[2]
Incarceration rates	measure the amount of people in prison
Admission rates	capture the number of people newly admitted to prison per year[3]
Police employment rates	measure the number of police officers in a state

Control variables have been chosen in line with the literature (Moody & Marvell, 2010), and the suggested approach of the FBI (2010). They have been retrieved from the US Census Bureau (2016) and Moody's crime database (Moody & Marvell, 2010). Covariates can be broken down into three major groups:

Table 2: Overview of control variables

Laws	e.g. 'zero tolerance', mandatory minimums, gun sale restrictions
Socio-economic factors	e.g. unemployment rate, poverty rate, real income, welfare
Demographics	e.g. divorce rates, percentage of racial minorities and age groups

Albeit comprehensive, this dataset can be improved. First and foremost, it is important to highlight that the crime rates measure crimes as reported to the police. This introduces a *reporting issue* in my data, as not all crimes are reported. Furthermore, district/county level data would allow for a higher number of observations and less aggregation in the underlying data. Last, there are some extreme outliers, where a variable more than doubles within a year, often indicating measurement error. Such clear outliers were excluded from the analysis to prevent bias.[4]

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[1] For summary statistics refer to Appendix 1.

[2] For definitions of these crime categories refer to Appendix 2.

[3] Incarceration and admission rates include federal, state-run and private prisons, as they aim to capture the overall amount of residents in prison or admitted to prison.

[4] For a detailed list of data manipulations refer to Appendix 3.

Baseline model

The analysis begins with a simple economic question: What is the influence of governors on policy outcomes, here: crime rates? In a Fixed Effects model, the impact of a Democrat over a Republican can be estimated, while controlling for unchanging state characteristics. Extended by a number of socio-economic and policy-related covariates, this yields my *Baseline model:*

$$y_{it} = \alpha + \beta D_{it} + \mathbf{X}'_{it}\mathbf{\Omega} + \lambda_i + \theta_t + \epsilon_{it}$$

where y_{it} is the relevant policy outcome, $\mathbf{X}'_{it}$ is a vector of covariates with $\mathbf{\Omega}$ as its corresponding vector of coefficients, and D_{it} is a dummy variable equal to one if a Democrat holds power. State fixed effects λ_i account for time-invariant heterogeneity, i.e. observed and unobserved differences in variables across states. Economic theory suggests that due to their political union, the states are subject to common shocks and trends over time; time fixed effects θ_t are included to account for these (Wooldridge, 2010, p. 487). Serial correlation within a state is controlled for by clustering standard errors on a state-level (Cameron & Miller, 2013).

In the present form, the model is misspecified. It does not counteract the *endogeneity* in political rule, i.e. the potential correlation between the error term and the dependent variable (Ferreira & Gyourko, 2007). This could be due to reverse causality or omitted variables driving the outcome variable. Hence, $\hat{\beta}$ is likely to be biased, and is at best to be interpreted as *association*.

Difference-in-differences model

Previous literature and the baseline model do not say anything about how crime or incarceration actually change when party control changes. Neither do we know whether incarceration rates increase manifold when a Republican takes over, nor whether the opposite occurs under a Democrat. An analysis of how outcomes change within a state once a party takes over from another is missing from the literature, motivating the usage of a difference-in-differences (henceforth: DiD) model. This can isolate the effect of governors by viewing them as treatments that are introduced only for some subsample. For the purpose of my model, I have arranged my data into groups, consisting of two terms of office lasting 4 years each. If the same party is in power twice it is a control group. If there is a change in party it is a treatment group. The data structure is presented in Figure 1 below.

Figure 1: Treatment and control groups
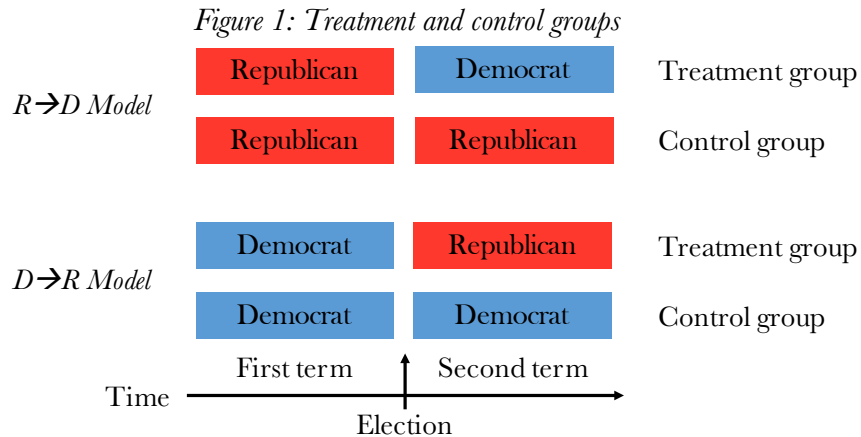
By assuming common trends *(Assumption 1)* between the two groups, the development of the control group informs what would have happened to the treatment group if it were not to receive the treatment. Figure 2 plots the unconditional mean of incarceration rates in states that initially have a Democratic governor and change to a Republican after 4 years. Divergence of the two groups after treatment (election) can be seen. The counterfactual is informed by the evolution of the control group. Assumption 1 implies that the difference between the factual and the counterfactual is the *Average Treatment Effect* – an arguably causal effect (Angrist & Pischke, 2009, p. 231). Note as well that this graph lends further credibility to the common trends assumption, at least for incarceration rates, as these are parallel to each other before treatment occurs.

Figure 2: Mean incarceration rates when D➔R
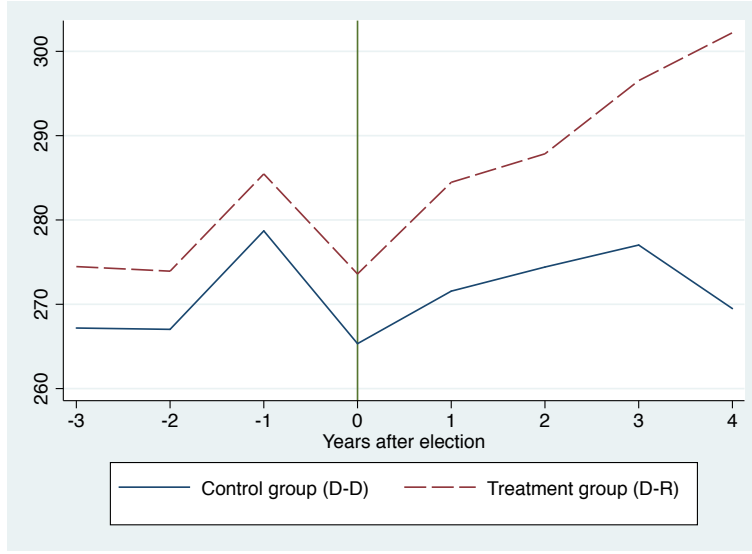
Over and above the common trends assumption two further assumptions are required, with respect to the set-up of my model: On the one hand, parties have similar ideologies across states *(Assumption 2)*, i.e. Republicans in one state are sufficiently similar to Republicans in another state. On the other hand, it requires no other changes to take place around the time of the treatment *(Assumption 3)*. With respect to the latter assumption, there certainly are a number of policies changing at the same time as crime related policies that change as a result of an election. Estimating the aggregate effect of a set of policies resulting from changes in party control, this should not be a major concern in the present case. However, it might well be the case that particular policies' effects outweigh each other, motivating an Instrumental Variables (henceforth: IV) analysis to infer the costs and benefits associated with one specific input. Ultimately, even if all assumptions are satisfied, we would not know *why* governors make a difference, only *if* they make a difference. In other words, I estimate a *reduced form model*.

The specification of the DiD model is as follows:

$$y_{it} = \gamma post_t + \phi(treat * post)_{it} + \mathbf{X}'_{it}\mathbf{\Omega} + \lambda_i + \theta_t + \epsilon_{it}$$

with ϕ as our coefficient of interest, on the interaction between the dummies $treat_i$, indicating whether an 8-year group is among the treated, and $post_t$ which is equal to one in the second

term of every 8-year interval. Again, state fixed effects λ_i allow for observed and unobserved state-specific characteristics that do not change, such as institutions, political tradition and geography. The variable $treat_i$ and the constant α are dropped as they are collinear with state fixed effects. Given the strong decrease in crime rates in the US since the early nineties, it is particularly important to include time dummies θ_t capturing common shocks and trends in the variables across states. Serial correlation within a state is controlled for by clustering standard errors on a group-level (Cameron & Miller, 2013).

The 'treatment' of a Republican (Democrat) governor does not need to be random. As such, the endogeneity in the choice of governor is accounted for. Even though the common trends assumption conceptually allows for *causality*, this model requires careful interpretation and it is, therefore, important to highlights its limitations. We might encounter self-selection of 'tough' Republican candidates in states with higher crime rates, thus, biasing the estimates. Hence, the model rests on the assumption that this is not the case; this is a variant of Assumption 2. Further, an obvious shortcoming is that, in the control group, there are cases where the governor changes, even though party control does not, and some where the same governor adds a second term. It might be that only one type of these groups drives the findings. If any of the proposed assumptions does not hold, DiD fails to produce interpretable estimates, let alone causality.

Given the data structure, and the lack of the literature comparing what actually happens when one party takes over from another, DiD is the most suitable framework for my analysis. It is able to give estimates about how variables evolve compared to their *expected evolution*, i.e. their counterfactual. As such, my chosen method can produce results that are informative about political effectiveness and efficiency.

Causal mechanisms and instruments

If we observe higher crime rates under Republicans, this might be due to a multitude of reasons (Marie, 2010), as outcome variables can be influenced through a set of causal mechanisms. On the one hand, the socio-economic situation of the poor and disenfranchised in a state might change as a result of Republican policies. If these classes are worse off, we can expect crime and, as a result, incarceration rates to rise. On the other hand, more policing and a tougher judicial system might deter crime and lower crime rates (Bell, et al., 2014). Increased policing, however, might raise the amount of criminals jailed, or the amount of crimes reported, without actual crime rates changing. As such, there is scope for measurement error in my data, as discussed above in Section II. These causal links are illustrated in Figure A1 in Appendix 7.

Analysis that goes further than correlation needs to perfectly identify the causal mechanism behind the observed reduced form effects. To this end, the difference-in-differences structure of my data could yield an instrument (Waldinger, 2010), estimating *causality* of individual policies, as opposed to *association*. Using a DiD instrument could show that there is an additional, politically driven, effect on incarceration or crime. An IV analysis has been conducted but relegated to the Appendix, due to a weak instrument in the first stage (Appendix 7).

IV. Results

Baseline model

The baseline model results are reported in Tables A3 and A4 of Appendix 4. Signs and significance of the different outcome variables vary greatly. Moreover, as multiple dependent variables are regressed there is a problem with statistical significance: At a 5% significance level, one regression out of twenty will appear significant by mere fluke in the data. Hence, I adjust significance levels to account for this statistical issue, in line with the Bonferroni adjustment (Sham & Purcell, 2014); the critical p-value when conducting m tests is p/m. I conclude that there is no significant partisan effect on crime. This is in line with Leigh's (2008) analysis.

Difference-in-differences estimates

Specifying the model in which a Democrat takes over from a Republican we find little evidence to support the partisan effects hypothesis. Visual inspection of conditional and unconditional means of these dependent variables reveals insignificant changes to political power. I conclude that none of these estimates is significant at conventional significance levels.

Table 3: Difference-in-differences estimates for R➔D

	(1)	(2)	(3)	(4)	(5)
Crime rates:	Violent	Murder	Rape	Property	Theft
DiD estimator	8.387	0.348	-2.129*	65.80	71.34
	(18.63)	(0.289)	(1.105)	(86.09)	(53.58)
N	307	307	307	307	307
R²	0.735	0.310	0.636	0.479	0.402
No. of groups	61	61	61	61	61

Robust standard errors in parentheses; clustered on group level
*** p<0.01, ** p<0.05, * p<0.1
Covariates omitted for simplicity

I have undertaken the same estimations for the *D➔R* case (Appendix 5), which also report insignificant effects. This analysis confirms the findings of the baseline model, that, indeed, the partisan effects hypothesis with respect to crime reduction does not hold. However, investigating the more immediate policy tools incarceration, admission and police employment rates, I find reasonable evidence to support the partisan effects hypothesis with respect to prison admissions. In specifying this model, I drop most control variables from $\mathbf{X}'_{it}$ as incarceration closely tracks crime. Violent crime correlates highly with all other crime measures and is, thus, included as the only crime control.

Table 4: Difference-in-differences estimates for R→D

Rates:	(1) Incarceration	(2) Admission	(3) Police
DiD estimator	4.579	-19.38**	3.384
	(6.974)	(8.947)	(5.021)
N	846	839	1,173
R²	0.494	0.229	0.477
No. of groups	119	119	149

Robust standard errors in parentheses; clustered on group level
*** p<0.01, ** p<0.05, * p<0.1
Covariates omitted for simplicity

Table 4 implies that when Democrats take over from Republicans every year roughly 20 less people are admitted to prison per 100,000 residents. Excluding outliers, this number changes to about 10, without changing significance (Table A9). Estimating the same equation using the natural logarithm of the admissions rate, I find a 7.4% lower admissions rate compared to if a Republican continued to stay in office (Table A7).

Two unexpected findings arise. First, there is a discrepancy between incarceration and admission rates. Possible reasons for this are measurement errors in either of these variables or that more short-term prisoners are being incarcerated.

Secondly, and more importantly, closer investigation of the admission rate data shows that there is a rise in the control group as opposed to the treatment group. As such, the coefficient $\hat{\phi}$ is driven by an increase in the control group. If we are willing to assume that the common trends assumption holds, this suggests that Democrats reverse a trend in admission rates upon taking up office from Republicans. As there is no increase in crime which would merit such actions, this gives rise to the hypothesis that Republicans aim to appear 'tough'.

Figure 3: Visual inspection of common trends in admission rates

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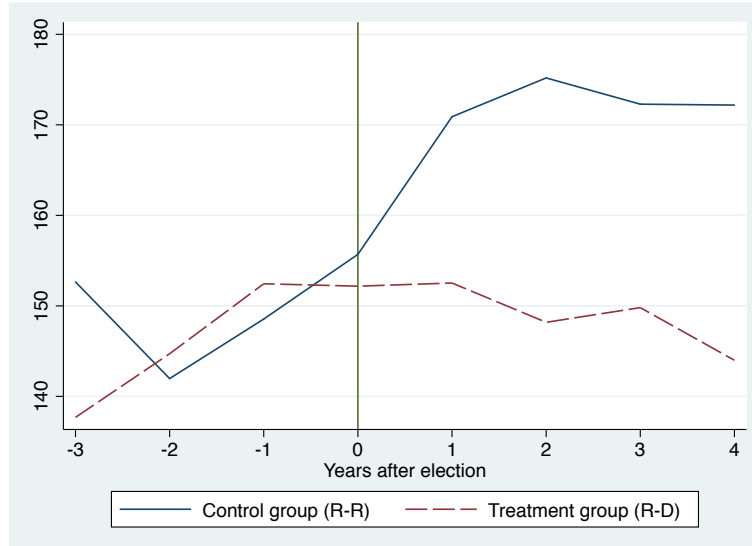
Crucially, this conclusion depends on the viability of the common trends assumption. The unconditional plot of means is generally used as an argument for or against common trends (Angrist & Pischke, 2009). Here, it shows a stronger difference the further we move on in time. First and last years should be considered with caution, because of lagged effects and election year effects, respectively. After dropping irregular observations, the unconditional mean of the control group levels down slightly, but the effect does not change substantially. These issues are addressed more closely in the section on robustness checks. Using this DiD model to investigate the efficiency of particular policies, changing as a result of party control, an IV estimation is proposed, the analysis of which can be found in Appendix 7.

Efficiency and beyond

The results in this section have shown the effect of Republican governorships on the rate of admissions to prison. However, there remains an unanswered question: What does this tell us about efficiency?

In assessing efficiency we "should focus on the ratio of benefits to costs and not just focus on the benefit side of the ledger" (Jens, 2010). Looking only at the 'benefit side' we might err to conclude that there is not much credit to give to either Democrats or Republicans with respect to fighting crime, as both are equally effective – or ineffective. However, this is mistaken. Given crime rates, if Democrats follow Republicans, admissions to prison do not increase as they would under Republicans. Under Republicans punishment increases without an accompanied increase in crime, indicating inefficient policy.[5] The systematically increasing amount of people admitted to prison under Republican governorship indicates costs to society.

The estimated partisan difference comes down to roughly 10-20 more admissions per 100,000 residents, which, at mean characteristics, equals about a 5-10% increase. This is supported by an analysis using the natural logarithm of admissions rates (Table A7). Costs to the justice system vary according to crime, ranging from about $500 for theft to over $500,000 for murders (Aos, 2003). As Republicans raise admissions without an accompanied increase in incarceration, I hypothesise that this is due to 'small crimes' with lower incarceration length being more severely prosecuted. Hence, the per crime cost lies in the range of $500-$1,500 (Aos, 2003). This adds up to $10,000-$30,000 in additional yearly costs to the justice system per 100,000 residents. If these extra resources were to be spent in, say, an aftercare treatment programme, the multiplicative benefits would add up $160,000 in uncommitted crimes (McCollister, et al., 2010).

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[5] Note that it might be that these additional admissions deter crimes that would otherwise have been committed. As a reduced form specification my model cannot estimate this.

Robustness checks

Bias can occur in the estimates or the standard errors. If crimes within a state are non-randomly misreported to the police, this introduces measurement error, resulting in bias and bigger standard errors. Furthermore, using cluster-robust standard errors only yields consistent estimates as the number of clusters approaches infinity. If the number of clusters falls below 50, standard errors get too small, and bootstrapping is advised (Cameron, et al., 2008). Columns 2-5 of Table A8 report the changes in standard errors from using state clusters as opposed to group clusters, as well as bootstrapped standard errors. The results do not change between state and group level clustering, turn insignificant, however, when using the bootrapping method. Thus, the results seem at least moderately robust to using different standard errors.

Moreover, specific subgroups could drive the results. Countering the worry that the found treatment effect is driven by swing states only raises doubts on the generalisability of my findings over all states. Columns 9-10 of Table A9 report that my finding is not robust to separating the sample into overly Republican and overly Democratic states, using historical averages. One problem of these robustness checks might be that the sample size decreases. Otherwise the model is robust to a number of modifications (Table A9).

Last, DiD assumptions are tested: The common trends assumption leads the prudent researcher to include linear group-specific time trends (Column 7, Table A9). While the coefficient becomes smaller, sign and significance remain unchanged. Moreover, there must not be a treatment effect before the treatment. Hence, I conduct a *placebo experiment*. To this end, I change the timing of the treatment and estimate a staggered DiD, with effects for all eight years, as opposed to one DiD estimator (Angrist & Pischke, 2009, p. 237):

$$y_{it} = \sum_{\tau=-3,\tau\neq0}^{4} \gamma_\tau + \sum_{\tau=-3,\tau\neq0}^{4} \phi_\tau\, treat_{it} + \mathbf{X}'_{it}\mathbf{\Omega} + \lambda_i + \theta_t + \epsilon_{it}$$

Figure 4 shows that the estimated pre-treatment coefficients $\sum_{\tau=-3}^{-1}\hat{\phi}_\tau$ are insignificant. This confirms that, indeed, there is no treatment effect before the actual treatment.

Figure 4: Plot of coefficients from the Placebo experiment

Coefficients of Placebo experiment with 95% Confidence Intervals
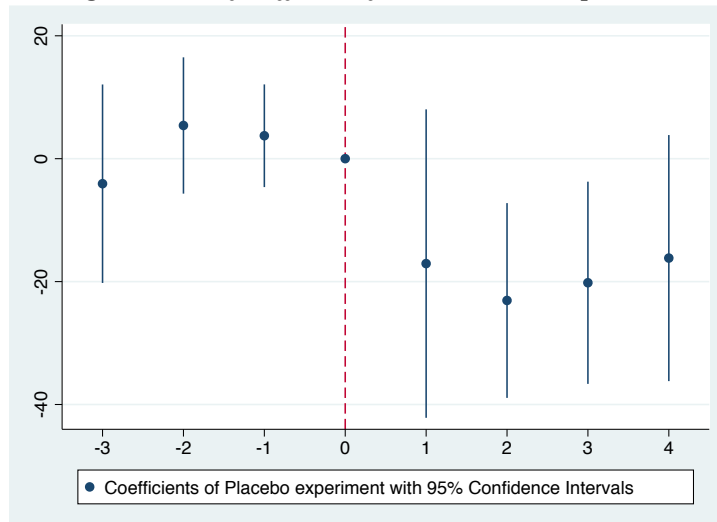
V. Conclusion and Limitations

My paper complements the economics of crime by using a DiD strategy to estimate partisan effects with respect to crime. In contrast to Fixed Effects models, a DiD specification more accurately describes the effect on variables when party control changes, thus, potentially yielding differing conclusions. In line with Leigh (2008), my paper finds little evidence to support the partisan effects hypothesis in crime reduction. However, it does identify an effect on prison admissions. They tend to rise under Republican governors, while under Democratic governors they do not.

Three lessons follow. First, it is likely that Democrats, by not admitting as many new prisoners as Republicans do, save the taxpayer a few dollars per year. Second, a possible conclusion, and testable hypothesis, is that for Democrats it might suffice to stop an increase in incarceration or keep levels stable, when taking over from a preceding Republican governor. Republicans, however, raise admissions to prison to maintain their status as 'tough on crime', thus, decreasing efficiency. The hypothesis resulting from my findings is that Republicans want to 'appear tougher'. In economic language, they might maximise different social objective functions. Third, in line with prior work, criminal activity is not overly responsive to the political environment. As such, it is rather policies than politics, which crime depends on.

It is important to highlight the limitations of my analysis. The DiD model rests on assumptions that, if not fulfilled, invalidate the analysis. Should tough governors, for example, self-select into running for office in crime-ridden states, we encounter selection bias. Likewise, if there is evidence not to support the common trends assumption, this would be a further problem.

Having estimated a reduced form effect, I have attempted further IV estimation, but was unable to identify a variable for which party control was a strong instrument. This is an area for improvement. Future research could disentangle the actual tools governors employ, using the methodology put forward in this paper. Moreover, data on more specific crime types could affirm the potential conclusion that short-term incarceration increases. Since the race of prisoners was not recorded prior to 2000, these data were not included in my analysis. As these data become more available, partisan effects with respect to racial discrimination in the justice system is a further area which this model can be extended to. Last, more granular geographical entities can be studied, exploiting the natural experiment setting of border regions.

In conclusion, the estimated partisan effect on crime outcomes lays the groundwork for more nuanced investigations of political treatment effects. These, in turn, can inform policymakers about the efficiency of their programmes. In this paper, I have highlighted and quantified the potential social inefficiency of a Republican partisan effect on admissions to prison. Ultimately, however, efficiency might not be the major component of politicians' – and voters' – objective functions. Often, it might be easier to stick to 'tough phrases' such as the Republican credo: *"Criminals behind bars cannot harm the public"* (On the Issues, 2012).

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Appendix 1: Data

Table A1: Summary Statistics

Variable	Obs.	Mean	Std. Dev	Std. Dev. (within)	Min	Max
Democrat Governor	2,573	0.56	0.50	0.48	0	1
Democrat vote margin	1,865	6.42	22.84	20.04	-58.4	100
Democrat vote share	1,865	51.92	11.78	10.35	0	100
Republican vote share	1,865	45.51	11.82	10.41	0	79.2
Violent crime rate*	2,617	374.46	231.56	154.85	9.5	1244.3
Murder rate*	2,617	6.10	3.62	1.96	0.2	20.3
Rape rate*	2,617	27.89	15.18	12.41	0.8	102.2
Property crime rate*	2,617	3632.37	1381.25	1098.47	573.1	7996
Theft rate*	2,617	2367.34	895.11	725.15	293.3	5106.1
Incarceration rate*	1,876	301.12	160.83	118.75	20.85	885.56
Admissions rate*	1,868	154.34	86.64	69.78	19.68	599.57
Police employee rate*	2,672	237.57	102.72	90.38	7.22	823.74
Total admissions to prison	1,876	18717.77	26988.51	13767.82	136	173942
Total number of police	2,672	13461.13	18508.10	9431.68	138	127049
Unemployment rate[†]	1,774	5.93	2.01	1.68	2	18
Arrests per violent crime[†]	1,156	0.69	0.58	0.54	0	12.20
Ban on semiautomatics[†]	2,273	0.06	0.24	0.15	0	1
Mandatory waiting law[†]	2,273	0.28	0.45	0.24	0	1
Gun permit law[†]	2,273	0.01	0.09	0.07	0	1
Percentage aged 15-19[†]	1,724	0.08	0.01	0.01	0.06	0.11
Percentage aged 35-44[†]	1,724	0.14	0.02	0.02	0.09	0.20
Percentage aged 55-64[†]	1,724	0.09	0.01	0.01	0.05	0.12
Percent in metro areas[†]	1,774	66.20	21.56	2.84	14.3	100
Percentage black males[†]	1,674	0.04	0.04	0.00	0.00	0.15
Percent white males[†]	1,674	0.36	0.05	0.01	0.14	0.43
Welfare expenditure p.c. in thousands $, lagged *t-15*[†]	1,574	1.17	1.32	0.56	0.04	6.96
Real income p.c. in thousands $[†]	1,774	4.45	0.95	0.72	2.18	8.10
Three strikes law dummy[†]	2,273	0.11	0.32	0.29	0	1
Divorce rate[†]	1,906	4.49	2.52	1.37	0.35	29.05
Percent female headed households[†]	1,032	11.94	3.24	2.37	5.80	24.19
Effective abortion rate[†]	637	0.37	0.47	0.42	0	3.26
Crack cocaine index[†]	1,278	0.86	1.25	1.10	0	7.78
Percentage of executions carried out[†]	2,070	0.00	0.01	0.01	0	0.11

*Rates are calculated per 100,000 residents. [†]Variables as reported in by Moody & Marvell (2010).

Table A2: Means of key variables across states

State	Incarceration	Admissions	Violent crime	Police employees
Alabama	455.87	174.89	434.18	235.39
Alaska	330.86	301.57	475.80	214.25
Arizona	424.39	174.64	487.34	280.37
Arkansas	369.00	196.31	373.21	199.60
California	340.27	257.76	642.07	265.88
Colorado	294.19	137.62	383.21	274.17
Connecticut	274.94	142.60	283.71	225.08
Delaware	385.61	204.73	478.76	273.44
Florida	398.02	193.07	737.75	318.53
Georgia	421.98	190.49	456.47	325.87
Hawaii	222.34	106.57	213.00	266.68
Idaho	307.67	178.78	206.80	213.47
Illinois	277.30	191.22	633.82	221.32
Indiana	292.74	170.55	318.43	205.36
Iowa	203.90	124.16	197.87	182.20
Kansas	258.16	143.78	319.21	257.10
Kentucky	314.11	204.69	264.74	188.71
Louisiana	593.35	252.86	594.07	308.48
Maine	119.36	58.48	114.21	166.68
Maryland	348.31	158.15	677.62	292.37
Massachusetts	135.37	45.69	444.21	261.77
Michigan	374.70	116.08	567.37	231.10
Minnesota	114.96	84.50	225.28	185.33
Mississippi	470.84	195.70	291.56	197.86
Missouri	369.79	207.90	478.06	253.88
Montana	253.41	136.84	194.18	210.64
Nebraska	182.90	91.46	243.67	202.20
Nevada	433.49	209.73	601.28	339.68
New Hampshire	148.04	74.75	112.86	183.16
New Jersey	253.17	128.85	404.01	362.35
New Mexico	236.53	146.45	580.15	245.47
New York	282.03	121.15	731.08	339.48
North Carolina	313.23	167.65	442.02	244.76
North Dakota	123.69	96.43	81.93	176.85
Ohio	335.99	177.90	345.69	213.72
Oklahoma	494.90	190.89	395.27	222.42
Oregon	261.20	129.35	353.28	212.86
Pennsylvania	252.19	91.50	326.61	200.73
Rhode Island	158.84	114.25	276.05	245.17
South Carolina	437.89	181.27	617.52	223.99
South Dakota	278.07	184.94	152.69	181.76
Tennessee	307.43	158.49	497.44	265.05
Texas	497.74	238.06	479.65	255.49
Utah	175.55	97.32	220.39	196.33
Vermont	168.98	180.78	104.61	162.69
Virginia	354.26	138.58	289.55	224.25
Washington	208.69	140.14	338.54	188.78
West Virginia	193.39	92.51	184.90	165.41
Wisconsin	256.47	104.47	183.91	250.25
Wyoming	282.17	117.20	221.61	294.85
US average	330.79	181.38	476.55	N/A

*Rates are calculated per 100,000 residents.

Appendix 2: Crime definitions

Definitions are taken from the FBI's UCR programme's Offense Definition Factsheet (2010):

Violent crimes are defined in the UCR Program as those offenses which involve force or threat of force. Violent crime is composed of four offenses: murder and nonnegligent manslaughter, forcible rape, robbery, and aggravated assault.

Murder and nonnegligent manslaughter: the willful (nonnegligent) killing of one human being by another. Deaths caused by negligence, attempts to kill, assaults to kill, suicides, and accidental deaths are excluded. The program classifies justifiable homicides separately and limits the definition to: (1) the killing of a felon by a law enforcement officer in the line of duty; or (2) the killing of a felon, during the commission of a felony, by a private citizen.

Rape: (a) before 2013: The carnal knowledge of a female forcibly and against her will. Rapes by force and attempts or assaults to rape, regardless of the age of the victim, are included. Statutory offenses (no force used—victim under age of consent) are excluded; *(b) after 2013:* penetration, no matter how slight, of the vagina or anus with any body part or object, or oral penetration by a sex organ of another person, without the consent of the victim. Attempts or assaults to commit rape are also included; however, statutory rape and incest are excluded.

Property crime includes the offenses of burglary, larceny-theft, motor vehicle theft, and arson. The object of the theft-type offenses is the taking of money or property, but there is no force or threat of force against the victims.

Larceny-theft: The unlawful taking, carrying, leading, or riding away of property from the possession or constructive possession of another. Examples are thefts of bicycles, motor vehicle parts and accessories, shoplifting, pocketpicking, or the stealing of any property or article that is not taken by force and violence or by fraud. Attempted larcenies are included. Embezzlement, confidence games, forgery, check fraud, etc., are excluded.

Appendix 3: Data manipulations

24 observations with independent governors were dropped: Alaska 1991-1994, Connecticut 1991-1994, Maine 1975-1978, Maine 1995-2002 and Minnesota 1999-2002.

4 observations, where there was an unforeseen change of governor during the term and no governor of either party was in office for more than 70% of the year, were dropped: Arkansas 1996, Minnesota 1963, Rhode Island 2011, Vermont 1991.

Extreme outliers, mentioned in the section on robustness checks, were dropped, as follows: Rhode Island 1995-2002 and Connecticut 1995-2002.

No data interpolation was required.

Appendix 4: Baseline results

Table A3: Baseline results for crime variables

Variables	(1) violent	(2) murder	(4) rape	(5) property	(6) theft
Democrat governor	27.77**	0.0359	-0.577	9.279	25.06
	(11.55)	(0.230)	(0.993)	(70.84)	(44.71)
Democrat vote margin	-0.307	0.000682	-0.0106	-0.349	-0.651
	(0.308)	(0.00490)	(0.0305)	(1.170)	(0.736)
N	592	592	592	592	592
R^2	0.598	0.290	0.521	0.448	0.445
No. of groups	50	50	50	50	50

Robust standard errors in parentheses, clustered at state level
*** p<0.01, ** p<0.05, * p<0.1
Covariates omitted for simplicity

Table A4: Baseline results for incarceration, admission and police employee rates

Variables	(1) prison	(2) adm	(3) policerate
Democrat governor	-4.530	-0.503	6.025
	(7.731)	(7.087)	(5.041)
Democrat vote margin	-0.210	-0.0717	-0.228*
	(0.198)	(0.154)	(0.119)
Violent crime rate	0.0647	0.125***	0.0991***
	(0.0495)	(0.0358)	(0.0246)
N	1,052	1,052	1,863
R^2	0.796	0.627	0.850
No. of groups	50	50	50

Robust standard errors in parentheses, clustered at state level
*** p<0.01, ** p<0.05, * p<0.1

Appendix 5: Difference-in-differences results

Table A5: D→R

	(1) Violent	(2) Murder	(3) Rape	(4) Property	(5) Theft
Crime rates:					
DiD estimator	1.385	0.197	2.259	172.5	102.3*
	(15.10)	(0.318)	(1.605)	(103.8)	(53.37)
N	368	368	368	368	368
R²	0.619	0.437	0.397	0.532	0.487
No. of groups	67	67	67	67	67

Robust standard errors in parentheses, clustered on group level
*** p<0.01, ** p<0.05, * p<0.1
Covariates omitted for simplicity

Table A6: D→R

	(1) Incarceration	(2) Admission	(3) Police
Rates:			
DiD estimator	4.261	7.678	-3.108
	(6.395)	(8.891)	(3.278)
N	949	945	1,418
R²	0.589	0.290	0.484
No. of groups	140	140	179

Robust standard errors in parentheses, clustered on group level
*** p<0.01, ** p<0.05, * p<0.1
Covariates omitted for simplicity

As rates are measured in levels, not in logs, the identified effect might overstate an increase (decrease) in states with lower (higher) admission rates. The estimation has been undertaken with logged dependent variables, reported in Table A7. Generally using the natural logarithm of the rates as dependent variables results in more singificant findings.

Table A7: Estimation using logs

	(1) Initial	(5) Excluding outliers
Variables		
DiD estimator	-0.139***	-0.0743**
	(0.0526)	(0.0330)
N	820	804
R²	0.365	0.401
No. of groups	119	117

Robust standard errors in parentheses,
clustered on group level
*** p<0.01, ** p<0.05, * p<0.1
Covariates omitted for simplicity

Appendix 6: Robustness checks

Investigations of different cluster types are reported in Columns 2-5 of Table A8.

I have undertaken various changes in the model specification, reported in Columns 2-11 of Table A9. First, I drop outliers. The estimated effect decreases, but remains significant at the 5% level. All further robustness checks are carried out on the this, preferred, model. Including the full set of control variables does not change the outcome (Column 3). Since some covariates in the crime rates model are missing for later years it uses a smaller sample; for the purpose of comparability, the admission rates model for this smaller sample is investigated in Column 4.

Next, there might be a lagged treatment effect. Dropping the first year of each term (Column 5), does not change the results. Potential effects of manipulation during election years have been accounted for by dropping the last year of every 4-year cycle (Column 6). Third, I specify a 'discontinuity approach' looking only at reasonably contested races in Column 8. The standard errors increase, but the results remains significant at the 10% level. Columns 9-11 report the estimation results over samples of overly Democratic states (more than half of the governors from 1970-2010 were Democrats), overly Republican states (vice versa) and swing states (with no more than 60% of governors between 1970-2010 from one party).

The staggered DiD model for the placebo experiment is reported in Table A10.

Table A8: Robustness to different cluster types

Variables	(1) Initial	(2) No outliers	(3) No clusters	(4) State clusters	(5) Bootstrap
DiD estimator	-20.63**	-10.28*	-10.28***	-10.28*	-10.28
	(9.102)	(5.757)	(3.582)	(5.879)	(6.489)
Cluster type	Group level	Group level	NO	State level	Bootstrap
$\mathcal{N}$	820	804	804	804	804
R^2	0.230	0.249	0.249	0.249	0.249
No. of groups	119	117	117	117	117

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Covariates omitted for simplicity

Table A9: Overview of Robustness Checks

Variables	(1) Initial	(2) Excluding outliers	(3) More controls	(4) Small sample	(5) Excluding first year	(6) Excluding last year	(7) Linear time trends	(8) Close election	(9) Overly Democrat	(10) Overly Republican	(11) Swing states
DiD estimator	-20.63**	-10.28*	-16.00**	-15.60*	-14.89**	-10.65	-13.02**	-14.20*	-8.805	-11.17	-14.63**
	(9.102)	(5.757)	(7.123)	(8.172)	(6.859)	(6.522)	(5.811)	(8.071)	(7.018)	(8.531)	(7.325)
N	820	804	455	446	605	603	804	315	487	317	511
R²	0.230	0.249	0.467	0.349	0.279	0.246	0.594	0.327	0.237	0.360	0.287
No. of groups	119	117	82	79	116	106	117	51	71	46	74

Robust standard errors in parentheses, clustered on group level
*** p<0.01, ** p<0.05, * p<0.1
Covariates omitted for simplicity

Table A10: Staggered DiD estimates for Placebo Experiment

$\hat{\gamma}_{-3}$	-9.844
	(7.475)
$\hat{\gamma}_{-2}$	-13.48***
	(3.773)
$\hat{\gamma}_{-1}$	-6.458*
	(3.723)
$\hat{\gamma}_{1}$	17.44
	(13.32)
$\hat{\gamma}_{2}$	24.84***
	(7.505)
$\hat{\gamma}_{3}$	24.33***
	(8.180)
$\hat{\gamma}_{4}$	27.16***
	(8.733)
$\hat{\phi}_{-3}$	-4.071
	(8.154)
$\hat{\phi}_{-2}$	5.403
	(5.600)
$\hat{\phi}_{-1}$	3.735
	(4.218)
$\hat{\phi}_{1}$	-17.07
	(12.67)
$\hat{\phi}_{2}$	-23.07***
	(8.003)
$\hat{\phi}_{3}$	-20.19**
	(8.308)
$\hat{\phi}_{4}$	-16.17
	(10.11)
Population	-4.15e-08
	(2.63e-08)
Violent	0.0800*
	(0.0458)
Constant	99.62***
	(22.56)
$\mathcal{N}$	820
R^2	119
No. of groups	0.234

Robust standard errors in parentheses; clustered on group level
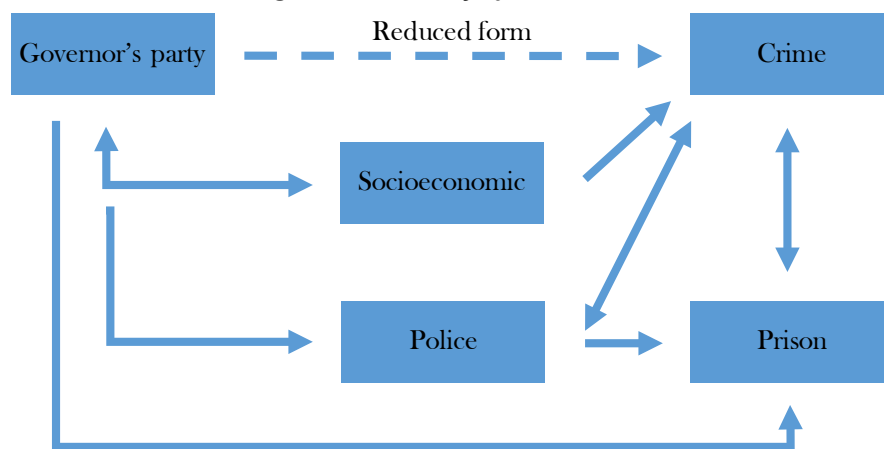*** p<0.01, ** p<0.05, * p<0.1
Time fixed effects omitted for simplicity
The year before treatment, $\tau = 0$, was chosen as the default.
Estimation was carried out using the regression:

$$y_{it} = \sum_{\tau=-3,\tau\neq0}^{4} \gamma_\tau + \sum_{\tau=-3,\tau\neq0}^{4} \phi_\tau \, treat_{it} + \mathbf{X}'_{it}\mathbf{\Omega} + \lambda_i + \theta_t + \epsilon_{it}$$

Appendix 7: Discussion of IV estimation

*Figure A1: Summary of causal links*

Finding a causal link between a change in governor and another crime-related variable would require the instrumented variable to be the sole channel through which the executive branch can have an impact on the outcome variable. Hence, as discussed above, using crime as the outcome variable in the reduced form with governor as instrument is problematic, because it is likely that unobservables, driven by party control, have an impact on crime (Levitt, 1997). However, the composite function for determinants of incarceration is considerably smaller than for crime. Incarceration, it can be argued, is a function of crime rates and the probability of detection (proxied by the number of policemen). The increase in police caused by a change in governor can, therefore, be used for a two stage analysis. It is unlikely that there is a further impact of a change in party on incarceration, i.e. $\mathrm{cov}(party, \epsilon) \neq 0$. This is the *exclusion restriction* and is illustrated in Figure A2, showing no causal link from governor's party through unobservables.

Figure A2: Discussion of exclusion restriction with incarceration/admission rates as dependent variable
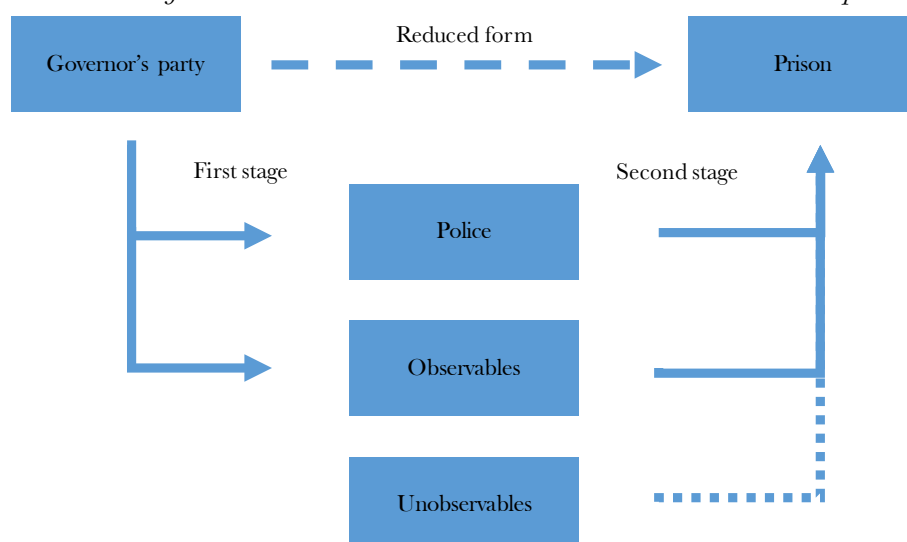
In the case where the outcome variable is crime, it is likely that other factors are driven by the instrument (party control), illustrated as the dotted line in Figure A3.

Figure A3: Discussion of exclusion restriction with crime as dependent variable
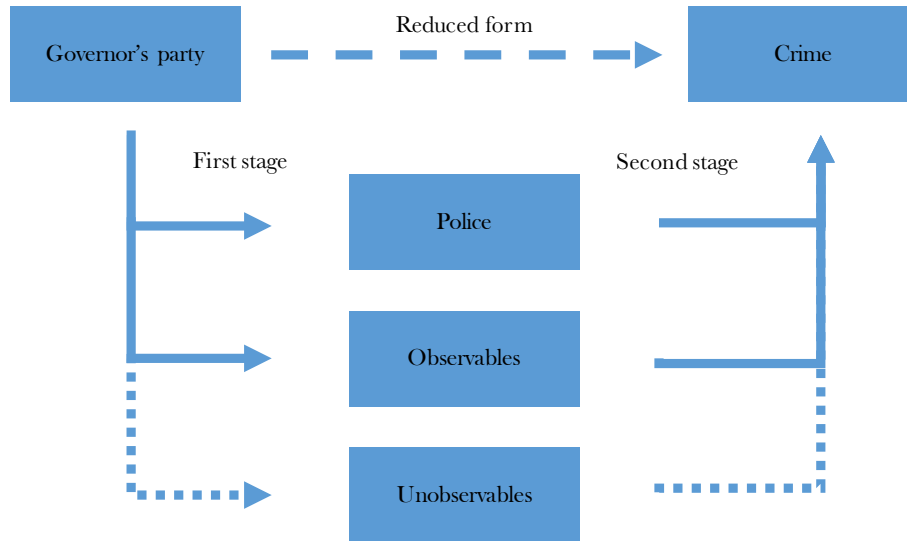
The following regression is biased, due to e.g. reverse causality:

$$admissions_{it} = \chi policerate + \mathbf{X}'_{it}\mathbf{\Omega} + \lambda_i + \theta_t + \epsilon_{it}$$

Having, in theory, explained the applicability of my DiD framework for a two stage model, I proceed to estimating the *first stage*:

$$policerate_{it} = \gamma post_t + \phi(treat * post)_{it} + \omega violent_{it} + \lambda_i + \theta_t + \zeta_{it}$$

IV estimation requires $\text{cov}(instrument, instrumented) = \text{cov}(party, police) \neq 0$ to hold. Rule of thumb for strong instruments: $F > 10$. However, I do not find reasonable evidence for a strong instrument, with an F-statistic of $F = t^2 = 1.84^2 = 3.39$ on $\hat{\phi}$.

With a strong instrument I could proceed to estimating the *second stage*:

$$admissions_{it} = \chi^{IV} \widehat{policerate}_{it} + \zeta violent_{it} + \lambda_i + \theta_t + \eta_{it}$$

Under the assumptions of IV, the coefficients of this model would eliminate the bias of the initial regression of admissions on the police employment rate. This is because a change in political control induces a movement in the police-rate. This exogenous increase is captured by the second stage. Note that the reduced form of this model is the DiD specification in the main section of this paper.

The estimated coefficient (standard error) [p-value] on police-rate $\hat{\chi}^{IV}$ in the two-stage model is: -3.318 (2.031) [0.102]; compared to -.066 (.139) [0.635] in Fixed Effects model. Admissions to prison react more negatively to changes in the number of policemen, than in the Fixed Effects model with biased estimates. However, since the first stage result does not identify a strong instrument, this conclusion must not be made, as IV estimation is not admissible.