Racial disparities in law enforcement: The role of in-group bias and electoral pressures[*]

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Abstract

Racial disparities are widespread throughout the U.S. justice system; in arrests and incarceration. These disparities are typically explained by appealing to racial biases among the police and the judiciary. I present a model in which disparities arise between groups in spite of unbiased actions on the part of these authorities. I assume that individuals discount the harm caused by criminal acts by members of their own group. Voters in each county determine the intensity with which legal sanctions are enforced against crimes. There are two groups, with the median voter drawn from the majority. In this model the intensity of law enforcement increases with the size of the minority. When counties are heterogeneous this leads to group disparities at the state level.

The intensity of law enforcement depends on both the level of policing and the strictness of the judiciary. In some states, voters can elect their judges and increase the legal sanction through judicial severity, while in other states judges are appointed. We should therefore expect that the relationship between the size of the minority population and the intensity of policing to be stronger in counties where judges are appointed. Using a county-level panel of arrests between 2000-2014 in the United States, I find that in states with appointed judges the level of policing is increasing with the share of the black population. A 1% higher share of black population leads to a 0.58% increase in the clearance rate of property crimes. I do not find a comparable effect in states with elected judges. This agrees with the predictions of the theoretical model.

1 Introduction

Racial disparities in the administration of justice in the United States is of significant political, policy, and public concern. Approximately 0.85% of the adult population of the U.S is currently incarcerated (Kaeble, 2016). Over 4% of the adult black male population is behind bars compared to 0.7% of the adult white male population. In 2017 approximately 7.5 million people over the age of 18 were arrested with African Americans arrested at twice the rate of their population share (DOJ 2018).

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As early as 1928, Thorsten Sellin questioned whether racial disparities in criminal justice outcomes could solely be attributed to differential involvement in crime or whether discrimination led to inflated perceptions of black criminality. A wide literature has found racial disparities in arrests due to the behavior of institution actors.[1] A common thread throughout this literature is that racial disparities in arrests and sentencing stem from bias on the part of police departments and judges. These observations may suggest that if these biases were eliminated, then any remaining difference across races in arrest rates or incarceration rates would solely reflect differential criminal tendencies among the races.

This paper shows that disparities between (racial) groups in the enforcement of legal sanctions can arise from the preferences of citizens expressed in their electoral behaviour, even in the absence of bias on the part of the police and the judiciary, and with no difference in criminal tendencies between the groups.

In the model presented here, the population is comprised of two identifiable groups, one of which is in the minority in all counties. All individuals are equally likely to take a (criminal) action that causes a negative externality. Voters determine the intensity with which legal sanctions are enforced against crimes within their county. Once the intensity of enforcement is determined by the median voter, it is implemented without discrimination across different population groups within the county. Individuals have an in-group bias which leads them to discount the level of harm that is caused by criminal actions taken by members of their own group. The in-group bias leads the median voter to choose higher levels of enforcement in counties where the size of the minority population is larger. I show that when the population composition of counties is heterogenous across the state, a larger proportion of the minority group faces sanctions even though both groups commit the offence at the same rate.

I assume electoral pressures influence both the intensity of policing and—when judges are elected—the severity of sentencing. These two components of law enforcement are complements in the standard theoretical framework for the analysis of crime and punishment (Becker, 1968), and harsher judges can substitute for more police in providing deterrence. In this framework, I use a variation between different US states in the method of choosing judges to empirically test my assumption of in-group bias.

In the classic Beckerian formulation, a rational individual chooses to commit a crime if the benefits outweigh the costs. The expected penalty is the product of p—the probability of being caught—and f—the punishment faced by the criminal if caught. Becker assumes that fines are

[1] Police are more likely to search, arrest, and book blacks (Antonovics and Knight, 2009; Pierson, Simoiu, Overgoor, Corbett-Davies, Ramachandran, Phillips, and Goel, 2017; Raphael and Rozo, 2019; Goel, Rao, Shroff, et al., 2016) and the racial composition of the police force or the department head affects the racial composition of arrests (Donohue III and Levitt, 2001; Bulman, 2019) especially for crimes that officers can show more discretion. When facing courts, blacks are less likely to receive bail (Didwania, 2018) and conditional on receiving bail, more likely to be assigned monetary bail (Dobbie, Goldin, and Yang, 2018). Prosecutors bring harsher charges against black defendants (Rehavi and Starr, 2014), juries are more likely to convict blacks (Anwar, Bayer, and Hjalmarsson, 2012), more likely to make mistakes when the defendant is black and the victim white (Alesina and La Ferrara, 2014). While judges are more likely to give whites downward departures (Mustard, 2001), and less likely to decide to incarcerate white defendants (Abrams, Bertrand, and Mullainathan, 2012).

a costless transfer between individuals and the government, while the probability of detection is a function of costly policing. He therefore finds that, to implement a given expected penalty, it is optimal to leverage high fines with low levels of detection to minimize the cost of enforcement. However, in reality we do not see maximal fines due to risk aversion of offenders (Polinsky and Shavell, 1979; Kaplow and Shavell, 1994) and concerns for fairness (Polinsky and Shavell, 2000). Further, escalating sanctions are more efficient in multi-period models when offenders differ in their propensity to commit crimes (Polinsky and Rubinfeld, 1991) and if some offenders are harder to catch then others (Bebchuk and Kaplow, 1992).

We do however see variability in the probability of detection between jurisdictions, and variations the strictness of judges; both of which alter the level of punishment ultimately faced by an offender. Police officers show discretion when making arrests and judges use the significant latitude which they possess within the guidelines when sentencing. Both the extent of policing and the strictness of sentencing can be influenced by electoral forces. In many states trial court judges are selected by popular election. In terms of policing, voter preferences can influence county sheriffs, who are elected officials, as well as police chiefs who must answer to elected officials. These electoral pressures play a vital role in determining the structure of local law enforcement.

Farris and Holman (2017) find that the political ideology of a sheriff influences the implementation of immigration law. Bulman (2019) finds that white sheriffs are more likely to target low-level crimes committed predominantly by blacks and vice versa for black sheriffs. Recently in Washington State, a large number of county sheriffs—predominantly from rural counties—indicated that they would not enforce a gun control law (I-639) passed by voters at the state level in 2018 (Kaste, 2019). Similarly, political pressures affect municipal police departments. Police hiring is concentrated in mayoral election years (Levitt, 1995), and municipal police departments rely on funding from local governments which in turn respond to the preferences of their constituencies. Thus, local policing is influenced by local politics and the intensity of policing can vary across counties within a state.

On the judicial front, the U.S is one of only three countries that holds elections to select any of its judges. Within the U.S, 30 states elect lower-court judges while judges are appointed in the remaining states. there is significant debate over the most effective method of judicial selection. While proponents of a popularly elected judiciary argue that it increases accountability, a growing literature finds that state judges act strategically as they get closer to re-election by increasing the severity of their sentencing (Huber and Gordon, 2004; Berdejó and Yuchtman, 2013; Gordon and Huber, 2007). Park (2017) finds that this severity is borne by black felons and it is more pronounced in districts where "racial animus" is higher. He finds that appointed judges who face retention elections rather than re-election sentence more consistently throughout their tenure. Thus, there exists a trade-off between judicial accountability and politically motivated sentencing which can have discriminatory effects.

In counties where judges are elected, voters can vary the intensity of the legal sanction by varying the level of policing as well as choosing judges who align with their desired level of severity. Increased policing is costly but since there is no additional cost to elect a harsher judge, the median voter will elect a harsher judge before increasing the level of policing. On the other hand, in counties where judges are appointed the median voter can only influence the intensity of the legal sanction by varying the level of policing. If sanctions do increase with the size of the minority as the model predicts, then it follows that in counties with appointed judges the level of policing should increase more sharply with an increase in the minority than in counties with elected judges.

I test this hypothesis using a 15-year panel on arrests in over 2,400 counties in the U.S. I measure the intensity of policing using the ratio of arrests to the number of reported Part I Index crimes—the clearance rate. These crimes involve two parties and are thus more likely to be reported to the police. Furthermore, the crimes have more standardized definitions across the states which makes them useful when analyzing outcomes across the United States. Part I crimes are split into two categories; violent and property crimes. Violent crimes, which include murder and aggravated assault, have a much higher cost to society per crime than do property crimes. I argue that this makes the demand for policing of violent crimes unlikely to be responsive to the size of the minority. This provides an opportunity to use the violent crime clearance rate as a counterfactual check of increased policing intensity. I find that in counties with appointed judges, a 1% higher black population increases the clearance rate of Part I property crimes by 0.58%. There is no comparable increase in the clearance rate of Part I violent crimes. I do not find a comparable effect in states with elected judges. This agrees with the assertion that the demand for policing does indeed work through electoral channels, and reflects an in-group bias on the part of the majority.

To my knowledge, this paper is the first to explore the consequences of electoral pressures on law enforcement through the dual channels of policing and judicial severity. It is also unique in pointing out that these two modes of enforcement are substitutes and therefore the institutional construction of the decision-making process intimately determines the structure of law enforcement, and its impact on population groups as well as individuals that become embroiled in the system. Finally, it underlines that racial disparities can result from subtle structural characteristics of the political system and biases that work through the system, not only from the obvious discrimination of identifiable individuals.

Section 2 lays out the theoretical model. Section 3 introduces the data and presents the empirical results. Section 4 concludes the paper.

2 The Model

We consider a county with continuous population normalized to size 1. Each individual can choose whether or not to take an action that yields a private benefit x to the perpetrator. The private benefit x is randomly drawn from a distribution $G(x)$ on [0,1] with associated pdf $g(x)$. The action has a known and deterministic social harm of h per unit of population. If an individual decides to take the action, he is caught with probability p, in which case he is assessed a fine f. The probability p is a function of the level of policing. The cost per unit of policing is given by c. All agents are risk neutral which means that an agent will take the action if his private benefit x exceeds the expected sanction pf.

The population of the county is composed of two racial sub-groups—the 'minority' which comprises a fraction $\theta \in [0, \frac{1}{2})$ of the population, and the 'majority' which comprises the remaining fraction $(1-\theta)$ of the population. The private benefit x remains a random variable which is i.i.d for each individual regardless of group identity.
I consider the impact of an in-group bias on the desired level of policing in such a county. Individuals discount the social harm that results from an action taken by a member of their own group. When the action is taken by a member of the individual's own subgroup the harm is evaluated as $(1-\alpha)h$, where $\alpha \in [0, 1]$. A value of 0 for α implies that an individual does not discount this harm at all, while a value of 1 for α implies that an individual completely discounts the harm caused by members of their own group. When a member of the other group takes the action the social harm is evaluated at its value of h.

Consider an agent from the majority group before he draws his value of the action x. His expected utility is calculated as follows. He will take the action if $x > pf$, and obtain an expected benefit $\int_{pf}^{1} xg(x)dx$. He will also endure harm given by $\int_{pf}^{1} hg(x)dx$ from the instances of the action taken by others whose private benefit exceed the expected sanction. However, a fraction $(1-\theta)$ of the perpetrators will be from his own group, so $(1-\theta)\int_{pf}^{1} hg(x)dx$ of this harm will be discounted by a factor of α due to the in-group bias.
Thus, an individual from group j (majority or minority) has the following utility

$$U_{maj} = \int_{pf}^{1} (x - h[1 - \alpha\phi_j])g(x)dx - pc \tag{1}$$

$\phi_j = (1-\theta)$ when j is the majority and θ when j is the minority.

2.1 Median Voter

I examine the choice of sanctions when the level of policing is determined by the median voter. This is relevant because sheriffs are elected in the majority of U.S counties and Municipal Police Departments report to local governments and thus, must be responsive to voters. We assume that the penalty f is exogenous to the county since penal codes are determined at the state level and are consistent across counties within a state. The median voter, who is always a member of the majority group, chooses the level of policing (probability of detection) p. He has the same utility as any member of the majority, which is given by (1). He only cares about discounting the harm that results from actions taken by member's of his own group and thus maximizes the following expression.

$$\begin{aligned} U_{MV} &= U_{maj} \\ &= \int_{pf}^{1} (x - h[1 - \alpha(1-\theta)])g(x)dx - pc \end{aligned} \tag{2}$$

The median voter's optimal level of policing is found by taking the derivative of (2) with respect to p and setting it equal to 0.

$$\frac{\partial U}{\partial p} = -\big[pf - h[1-\alpha(1-\theta)]\big]\ g(pf)f - c = 0 \tag{3}$$

When policing is costless ($c = 0$), and there is no in-group bias ($\alpha = 0$), we recover the traditional result that the expected sanction is equal to the harm caused (Becker, 1968; Polinsky and Shavell, 1984).

$$pf = h$$

If $c > 0$, then the the optimal level of policing is correspondingly lower.

When there is an in-group bias the median voter sees less harm in the action. It is still optimal to choose the level of policing such that the expected sanction equal to the harm caused. However, since the perception of harm is now lower it leads the median voter to lower the expected sanction accordingly.

$$pf = h[1-\alpha(1-\theta)] \tag{4}$$

Proposition 1 *(a) For $\alpha > 0$, the expected sanction is increasing in θ; the size of the minority. (b) The expected sanction is decreasing in α; the in-group bias.*

Proof *By assumption, α and h are strictly positive. Parts (a) and (b) then follow readily from (4) by taking derivatives with respect to θ and α respectively.*

(a)

$$\frac{\partial pf}{\partial \theta} = \alpha h > 0 \tag{5}$$

(b) *Taking the derivative of the expected sanction with respect to α gives us*

$$\frac{\partial pf}{\partial \alpha} = -h[(1-\theta)] < 0 \tag{6}$$

Note that part (a) is only relevant for $\theta \in [0, \frac{1}{2})$. If θ crosses a half, the median voter is drawn from the other group which now becomes the majority. The relevant size of the minority then becomes $(1-\theta)$. The intuition for part (b) is that as α increases, individuals discount the harm caused by their group members to a greater extent. As the harm is perceived to be smaller it optimally attracts a reduced sanction.

2.2 A condition for increasing arrests

As the intensity of policing increases, arrests could increase or decrease. How it behaves depends on the elasticity of crime with respect to the intensity of policing. In the empirical literature, the elasticity of property crimes is estimated to be much smaller than unity with respect to policing. Estimates usually range from -0.15 to -0.35 (Evans and Owens, 2007; Chalfin and McCrary, 2018, 2017; Levitt, 1998). If the level of crime decreases by less than the increase in the amount of policing,

then the number of arrests should increase with policing. While less crimes are committed, the number of people arrested increases.
I derive the basic condition for the number of arrests to be increasing in the probability of detection. The total number of arrests, A, can be expressed as the product of the probability of getting caught taking an action and the number of people who chose to commit the action.

$$A = p(1 - G[pf])$$

This implies that the number of arrests is increasing in p when

$$\frac{\partial A}{\partial p} = 1 - G[pf] - pfg(pf) > 0$$
$$\frac{g(pf)}{1 - G[pf]} < \frac{1}{pf} \tag{7}$$

2.3 State-wide Disparities

One effect of in-group bias on policing, as described above, is that it can lead to aggregate disparities in arrests or incarcerations between groups at the state level even though members of both groups are policed at the same rate within each county. Specifically, suppose that the same group constitutes the minority in every county and that condition (7) holds.

Proposition 2 *If θ_i, the proportion of the minority in county i, is heterogenous across counties, then the proportion of the minority among those who are arrested will be larger than its proportion in the state's population.*

Proof *Consider a state with two equally sized counties, Y and Z, with average minority population size μ. Let county Y have a larger minority population than county Z such that*

$$\theta_Y = \mu + \epsilon > \theta_Z = \mu - \epsilon$$

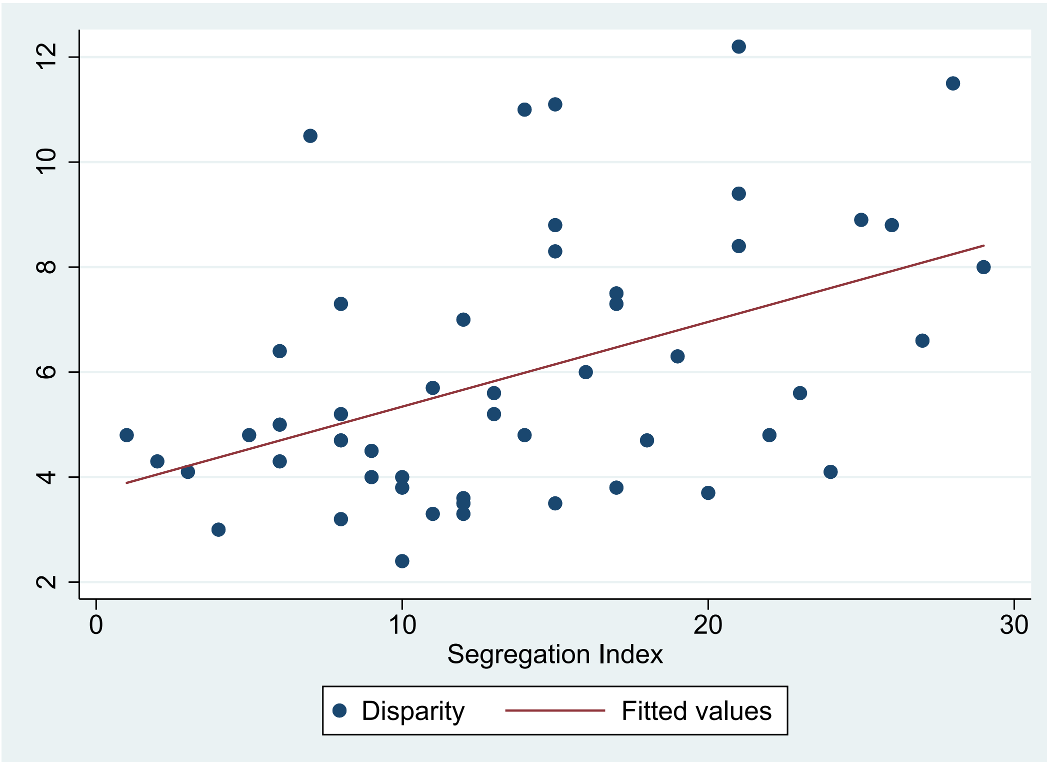
It follows from Proposition 1 that the intensity of policing in county Y, $P(\theta_Y)$, is higher than the intensity of policing in county Z, $P(\theta_Z)$.
I assume that (7) holds and so, the total number of arrests is higher in county Y than in county Z, i.e. $A_Y > A_Z$. We can therefore calculate the proportion of minority arrests to total arrests across the counties:

$$\frac{A_Y(\mu + \epsilon) + A_Z(\mu - \epsilon)}{A_Y + A_Z} = \frac{A_Y\mu + A_Y\epsilon + A_Z\mu - A_Z\epsilon}{A_Y + A_Z} = \mu + \frac{(A_Y - A_Z)\epsilon}{A_Y + A_Z} \tag{8}$$

which is greater than μ since $A_Y > A_Z$. Thus, minorities are over-represented in the total number of arrests, even though they are policed at the same level as the majority within each county.

Note that if the minority population size is equal across the counties, $\epsilon = 0$, then the proportion of minority arrests to total arrests is same as the proportion of the minority to the total population.

While I do not formally test this proposition in the empirical section, I provide some suggestive evidence that between county differences in population composition drive the observed aggregate disparities. The graph below plots the racial disparity in incarceration between blacks and whites against the state segregation index. There is a positive correlation between state-wide segregation and the racial disparities in incarceration.



Notes: Racial disparity data created by "The Sentencing Project" from BOJ 2016 justice data. State Segregation index created by the Michigan Population Study Center using data from ACS between 2005-2009.

2.4 Elected vs. Non-elected Judges

In the model above, the expected sanction is made up of two parts, p—the probability of being caught–and f–the fine for the action. However, the probability of detection can be decomposed further into two probabilities; that of being caught, say p_1, and that of being sentenced or assessed the fine, p_2. Thus second probability relates to the latitude the judge has in the judicial process. An individual may be more or less likely to be given a sentence, and that sentence may be a greater or lesser fraction of the maximal penalty depending on the discretion of the judge. p_2 represents the expected degree to which the sanction is moderated, conditional on the individual being brought to court. We continue to assume that p_2 is independent of the group affiliation of the offender.
In this section I denote the expected sanction as $p_1 p_2 f$. As discussed, the median voter can influence p_1 through his vote. If in addition, a judge can be elected, then he can also influence p_2 by casting

his vote for a judge of the appropriate severity. Thus the median voter's problem is to choose the levels of p_1 and p_2 which maximizes the following utility function:

$$\int_{p_1 p_2 f}^{1} (x - h[1 - \alpha(1-\theta)])g(x)dx - p_1 c \tag{9}$$

I assume that potential judges lie on a continuum of severity from lenient to severe. For a given infraction, a lenient judge typically enforces a smaller portion of the full punishment than a harsh judge. When a judge is appointed to her position, on expectation she is of average severity.

In judicial elections, each constituent must vote for a judge and there is no additional cost to vote for a more severe judge. Thus, in counties where the judge can be elected, the median voter has two instruments through which he can increase the expected sanction; policing and judicial severity. From Proposition 1 we know that the expected sanction is increasing in the size of the minority. Since policing is costly but choosing a harsher judge is not, it follows that the median voter will increase the expected sanction when the minority population is larger by first increasing judicial severity, and only use additional policing when this does not suffice. However, in counties where judges are appointed, the severity of the judge is exogenously given to the voters, On expectation an appointed judge is of 'average severity'. In such counties, an increase in expected sanction can only be affected by increasing the intensity of policing.

Proposition 3 *The intensity of policing will increase more sharply with θ in counties with appointed judges than in counties with elected judges.*

In the empirical section, I exploit this institutional difference between states to test whether counties with higher black populations are policed more intensely in states with appointed judges.

2.5 Simple Extensions and Alternative Representations

2.5.1 Out-group Animosity

The literature on sentencing disparities (see footnote 1) often finds that white defendants are treated advantageously and the shorter sentences that they receive are a result of downward departures from sentencing guidelines (Mustard 2001, Butcher, Park, and Piehl 2017). In contrast, McConnell and Rasul (2018) find evidence of an explicit out-group bias in federal sentencing by exploiting the increased salience of ethnicity in the aftermath of 9/11. Disparities between Hispanic and white defendants increased not by treating whites advantageously but rather specifically treating Hispanic defendants more harshly. Similarly, Alesina and La Ferrara (2014) find that more mistakes are made in capital cases when the defendant is a minority and the victim is white. Given that the aim of a jury is to make a correct judgement and that the mistakes are made only for cross-racial crimes, the findings suggest that some of the disparities can be explained explicitly by out-group animosity towards black defendants rather than preferential treatments for whites.

In this paper I modelled an in-group bias as a discount on the harm caused by members from the same group expressed as the term $\alpha(1-\theta)h$, with $\alpha > 0$. Similarly we can model out-group animosity as a mark-up on the harm caused by members of the other group expressed as the term $\beta\theta h$, with $\beta > 0$. In the presence of out-group animosity so that the median voter's utility is given by:

$$U_{maj}^{out} = \int_{pf}^{1} (x - h[1 + \beta\theta])g(x)dx - pc \tag{10}$$

Under costless policing, this is maximized by the median voter's choice of p when $pf = h[1 + \beta\theta]$ Taking the derivative of the sanction with respect to the size of the minority gives us

$$\frac{\partial pf}{\partial \theta} = \beta h > 0 \tag{11}$$

Thus, the results do not change, the intensity of the legal sanction is still increasing in the size of the minority. Intuitively the net effect of an in-group bias and out-group animosity is the same, for the median voter it still reflects the perception of a relatively lower level of harm for actions taken by those from the same group. However, it is important to note that compared to the case in which there is no bias in either direction, an in-group bias results in the level of expected sanction being less than the actual harm while out-group animosity leads to the expected sanction being set above the level of actual harm.

2.5.2 Voter cares about the fine

Suppose that instead of discounting the harm caused by member's of his own group, an individual dislikes the imposition of the punishment, f, on his own group. Let γ be an altruism weight for the median voter. When members of his group is caught and punished, he suffers a disutility which equates to a portion of the punishment, expressed as γf with $\gamma > 0$. Thus, the median voter chooses the level of p which maximizes the following utility function.

$$U_{maj}^{fine} = \int_{pf}^{1} (x - h)g(x)dx - pc - p[1 - G(pf)](1 - \theta)\gamma f \tag{12}$$

The term $p[1-G(pf)](1-\theta)\gamma f$ denotes the number of arrested individuals who are from the median voter's group multiplied by the disutility the median voter faces when a group member is punished. From (7) we have that the number of arrests is increasing with p. This implies that $\frac{\partial pf}{\partial \theta} > 0$ Intuitively, the median voter perceives the cost of policing as larger when the majority population is larger. The total level of harm perceived by the median voter is unresponsive to the size of the minority but the altruism parameter acts to increase the perceived cost of policing as the size of the majority increases. As the size of the minority population increases, the probability that an individual who is caught and punished is from the majority decreases. Thus, the marginal cost of policing decreases but the marginal benefit of increasing policing at the optimum remains the same. Therefore, as the minority size increases the median voter finds it cost-effective to increase the level of policing.

This intuition also extends to the case in which the size of the fine can be influenced by the median voter. In practice voters within counties do not have direct control over the setting of fines since penal codes are decided at the state level. However, there are some notable recent exceptions such as the legalization of marijuana through referendum in certain states. Marijuana usage is consistently cited to be approximately equal across races (SAMHSA 2017). It is interesting to note that marijuana was legalized first in states that have smaller black population shares such as Washington and Oregon. In the context of the model proposed in this paper, we can consider a state as a single county. In states with higher white populations, the demand for legal sanctions will be lower. As such, as perceptions surrounding marijuana become more favorable (the perceived harm of its use decreases) we would expect states with larger white populations to legalize its usage first.

2.5.3 Differential Crime Rates

Non-homogeneous behaviour between groups is easily accommodated in the model. It extends to situations in which the distribution of potential benefits of taking an action differs between racial groups. Suppose that the distribution from which individuals from the majority group draw their private benefit stochastically dominates that of the minority group. For any given level of expected sanction, majority members constituted a larger proportion of action takers than their population share. In the presence of an in-group bias, the median voter will choose a lower level of policing than in the baseline model where the act is taken at the same rate between groups.
The intuition is as follows. Suppose the median voter has selected an expected sanction which is optimal for a crime which is committed by each group at the same rate. Now suppose that, holding the number of acts constant, an individual from the minority is removed from the group of action-takers and replaced with a member from the majority. The initial sanction is no longer optimal. The median voter discounts the total harm by one more member of the majority group and so, the sanction is too high. The reverse is true if the distribution of private benefits for the minority population stochastically dominates that of the majority population. In this case, we would expect the median voter to demand a higher level of policing compared to the baseline model for the same reasoning above. This agrees with finding that the race of a sheriff affects whether they target crimes predominantly committed by whites or blacks (Bulman, 2019). Since the race of a sheriff is strongly predicted by the racial makeup of the county population, it is likely that electoral pressure and the racial preferences of a county alters sheriff behaviour.

3 Empirical Results

3.1 Data Description

3.1.1 Incarceration Trends Dataset

The Incarceration Trends Dataset is put together from a variety of different sources by the Vera Institute of Justice. It provides data on the reported number of Part I index crimes from the Uniform Crime Reporting Program, which is independently available from the Federal Bureau of Investigation (FBI).[2] The crime numbers are voluntarily reported by law enforcement agencies in the United States but covers approximately 98% of the population. The dataset also contains county by year population estimates which are constructed from the Census Bureau as well as the Center for Disease Control.

3.1.2 Judicial Election Data

I match states to their judicial selection method using data maintained by the National Center for State Courts.[3] The courts of interest are circuit, superior, and county courts which try non-federal criminal cases within states. There are three methods of judicial selection used in the United States:

1. Popular Election
   - Partisan elections: Elections in which the judge's political affiliation appears on the ballot
   - Non-Partisan elections: Elections where the judge's political affiliation does not appear on the ballot
2. Appointment
   - Merit Selection: Nominating commissions evaluate judicial candidates and appoint appropriately qualified judges.
   - Gubernatorial: The Governor of the state appoints judges. In these states there is often a nominating commission that provides a short-list of candidates from which the Governor chooses.
   - Legislative Appointment
3. Legislative Election: A judicial selection method in which the state-legislature votes on a judge's appointment.

If a state uses any appointive method of judicial selection it is coded as a "non-elected" state. I include the two states with legislative elections (South Carolina and Virginia) as "non-elected" states since there does not exist a direct selection channel between the constituents of a county and the judge who serves them. If a state uses any type of election, either partisan or non-partisan, it is coded as a 0 in the non-elected variable. Kansas and Arizona have mixed systems and are coded as such. Approximately half of the judicial districts in Kansas use judicial elections while the other half of districts use a merit selection procedure. In Arizona, counties with populations over 250,000 have appointed judges while the remaining counties elect their judiciary. [4]

[2] There are eight Part I index crimes which can be split into two categories; violent crimes and property crimes. Violent crimes consist of criminal homicide, forcible rape, robbery, and aggravated assault. The property crimes consist of Burglary, Larceny theft, motor vehicle theft, and arson.

[3] This data is available at http://www.judicialselection.us/

[4] While technically California uses judicial elections for its superior court judges, in practice most judges are appointed by the governor (Dubois, 1985; NCSC, n.d.; Trevor, Graumlich, Mercado, Perez-Sangimino, Phillips, Ricca, and Solomon, 2017). Dubois finds that between 83-90% of superior court judges in California were initially appointed by the governor. The judges then face an election at the next gubernatorial election, but they are overwhelming confirmed by voters (in

3.1.3 Unemployment Data

I create a county by year panel of unemployment data to serve as an economic control. I use the annualized yearly unemployment data that is compiled from the "Local Area Unemployment Statistics Program" run the Bureau of Labor Statistics.

3.1.4 Policing/Arrest Data

I create a county by year panel of arrests using individual UCR arrest report files from 2000-2015. The reports provide annualized arrest data by crime type at the agency level. I aggregate arrests made for Part 1 crimes by Municipal police departments and sheriff departments to create a measure of total arrests made by local law enforcement in a county. This data is then matched to the population estimate data and the Part I index crime data from the Incarceration Trends dataset, the judicial selection method data from the NCSC, and the unemployment data from the BLS. This procedure results in data on 2449 counties across 47 states.

3.2 Summary Statistics

Table 1 provides summary statistics for the property crime clearance rates, property crime rates, and Arrest Rate per 1000 residents aged between 15 to 64. There do not seem to be systematic differences in crime rates or clearance rates between counties with elected judges and counties with appointed judges.

Counties in states with elected judges have higher shares of black populations—approximately 8% compared to 10% in counties with elected judges. The property crime rate across judicial selection methods is approximately equal—39 crimes per thousand residents in counties with elected judges vs. 38 per thousand in states with appointed judges. The Property crime clearance rates are approximately equal at 13% and 13.5% for elected counties and appointed counties, respectively. The property crime arrest rate is approximately 4.8 arrests per thousand in both types of counties. The violent crime rate per thousand is also balanced across judicial selection methods at 4.6 crimes per thousand and 4.7 crimes per thousand in elected counties and appointed counties. The violent crime clearance rate is slightly higher in states with elected judges at 48% compared to 45%. However, the violent crime arrest rate is approximately 1.8 arrests per thousand for both types of counties. On average, counties with elected judges are larger and have higher levels of unemployment.

3.3 Econometric Framework

Combining Proposition 1 and Proposition 3 from the theory section, we expect the level of policing to be increasing with the black share of population more sharply in counties with appointed judges than in counties with elected judges. I test this by measuring the effect of the share of black

99.4% of cases). For this reason, I have coded California as a state with judicial appointments rather than elections. If California is instead coded as a state with judicial elections, the results do not change measurably.

Table 1: Summary Statistics

Summary Statistics

	Elected	Non Elected	Total
Share Black	0.097204	0.0795147	0.0924904
	(0.1468)	(0.1392093)	(0.1449924)
Property Crime Rate	39.07397	37.89835	38.76395
	(20.65027)	(21.47347)	(20.8766)
Property Crime Clearance Rate	0.1304738	0.1355045	0.1318007
	(0.0991537)	(0.0957705)	(0.0982963)
Property Crime Arrest Rate	4.787749	4.871926	4.809949
	(3.51764)	(4.403717)	(3.771695)
Violent Crime Rate	4.587538	4.725111	4.623971
	(3.546779)	(5.820812)	(4.268934)
Violent Crime Clearance Rate	0.4796194	0.4468784	0.4709482
	(4.14696)	(0.3432405)	(3.560053)
Violent Crime Arrest Rate	1.864332	1.818548	1.852206
	(5.399048)	(2.640773)	(4.824562)
Total Population	103602.1	80400.21	97419.61
	(357464)	(163809.2)	(317782.1)
Unemployment Rate	6.763126	5.61	6.4558
	(2.814522)	(2.47348)	(2.775007)

population on the rate of property crime clearances. The property crime clearance rate is the ratio of arrests to the number of reported Part 1 property crimes.[5] Formally I test

$$\begin{aligned} \text{PCCR} &= \alpha_i + \alpha_t + \beta_1 NonElected_{i,t} * ShareBlack_{i,t} \\ &\quad + \beta_2 Nonelected_{i,t} + \beta_3 ShareBlack_{i,t} + \beta X_{i,t} + \epsilon_{i,t} \end{aligned}$$

where i corresponds to the county, j the state, t the year. PCCR is the property crime clearance rate, Nonelected is a dummy indicator for whether the state has appointed judges, ShareBlack is the percentage of the population that is black and NonElected*ShareBlack is an interaction term between the two. α_i captures county fixed effects and α_t captures time fixed effects. Note that during the period of study no counties changed their judicial selection method from appointed to elected or vice-versa. Thus, in the estimation the coefficient on NonElected is captured by county

[5] See Kennedy (2009) for a discussion on why clearance rates are a good measure of policing intensity.

fixed effects. I include county by year controls for the size of the population, the unemployment rate, and the violent crime rate.

3.4 Results

3.4.1 Baseline Property Crime Clearance Results

The baseline results are presented in Table 2. Column (1) and column (2) present the results for the entire sample without and with controls, respectively. In column (1) the coefficient on the interaction term between Non-Elected and Share Black is positive and significant. It indicates that a 1% increase in the share of black population increases the property clearance rate by 0.6% in counties with appointed judges compared to counties with elected judges. Adding controls increases the magnitude and significance level slightly. I do not include the property crime rate as a control since the dependent variable includes the total number of reported crimes within the county and I control for population.

Columns (3) and (4) present the results for the sample restricted to counties with appointed judges without and with controls, respectively. In Column (3) the coefficient of interest on Share Black is positive and significant. It indicates that a 1% increase in the share of black population increases the property clearance rate by 0.58%. Column (4) reports that adding controls which do not change the results.

Columns (5) and (6) present the results for the sample restricted to counties with elected judges, without and with controls, respectively. In Column (5) the coefficient of interest on Share Black is positive but not significant. Column (6) reports that the coefficient remains positive and insignificant when adding controls.

3.4.2 Violent Crime Clearance Rates as a Counterfactual

Part I violent crimes have a significantly higher cost to society than Part I property crimes. The Rand Cost of Crime Calculator (Heaton, 2010) estimates the cost per crimes as follows:

1. Violent Crimes: Murder –$8,649,216, Rape– $217,866, Robbery– $67,277, Aggravated Assault–$87,238
2. Property Crimes: Burglary – $13,096, Larceny – $2,139, Motor Vehicle Theft –$9,079, Arson –$13,196

In the theory section the median voter chose a level of policing that depended on the level of harm caused by the action:

$$pf = h(1 - \alpha(1 - \theta))$$

It follows that for actions with a sufficiently high level of harm the expected sanction should be maximal, and unresponsive to changes in θ. Robbery, the least costly violent crime, has a cost of over 5 times that of arson or burglary–the most costly property crimes. The differential costs to society of violent crimes presents an opportunity to use their clearance rates as a counterfactual to the property crime clearance rates. Therefore, I also test the following specification, where VCCR is the violent crime clearance rate.[6]

$$\begin{aligned}\text{VCCR} &= \alpha_i + \alpha_t + \beta_1 NonElected_{i,t} * ShareBlack_{i,t} \\ &\quad + \beta_2 Nonelected_{i,t} + \beta_3 ShareBlack_{i,t} + \beta X_{i,t} + \epsilon_{i,t}\end{aligned}$$

If for some reason counties with appointed judges and larger black populations have more efficient police departments for some reason orthogonal to race, then we would expect the violent crime clearance rates to also be higher in these counties. However, if the violent crime clearance rates are unaffected by race it suggests that the property crime results found in the specifications above are driven by the theoretical predictions

3.4.3 Baseline Violent Crime Clearance Results

Next I run the specifications using the violent crime clearance rates. The results are presented in Table 3. Column (1) and column (2) present the results for the entire sample without and with controls, respectively. In column (1) the coefficient of interest on Share Black is positive but not significant. Adding controls in column (2) does not change the sign or significance of the results.
Columns (3) and (4) present the results for the sample restricted to counties with appointed judges without and with controls, respectively. In Column (3) the coefficient of interest on Share Black is positive but not significant. Column (4) reports that adding controls does not change the sign or the magnitude of the results.
Columns (5) and (6) present the results for the sample restricted to counties with elected judges, without and with controls, respectively. In Column (5) the coefficient of interest on Share Black is positive but not significant. Column (6) reports that the coefficient remains positive and insignificant when adding controls.

3.4.4 Property Crime Clearance Results with VCCR as control

A potential concern for using the violent crime rate as a counterfactual is that fewer counties report complete data for violent crimes than property crimes. Only 2409 of the original 2449 counties report which could be a result of selection bias. In the next specifications I restrict the samples to counties which report both property crime and violent crime arrest data. I also rule out that the higher property crime clearance rates in counties with larger black populations is driven by more efficient policing overall in the county by controlling for the violent crime clearance rate. Although it is unlikely that the judicial selection method systematically affects whether or not arrest data is reported by a county, controlling for violent crime clearance rates also restricts the sample to counties which report arrest data for both property and clearance crimes.

[6]The remaining components are as described in Section (3.3).

For the property crime clearance rate regressions, this reduces the number of observations from 9305 to 7869 for counties with appointed judges, and reduces the number of observations from 22983 to 21276 for counties with elected judges. The percentage loss in observations is comparable between the two selection methods. I present the following specifications with the controls for population size and the unemployment rate in Table 4.

Controlling for the violence crime clearance rate within a county reduces the estimates modestly Column (1) reports the results on the full sample. The magnitude of the coefficient on the interaction term between Non-Elected and share black decreases from 0.602 to 0.562. Column (2) presents the results for the sample of counties with appointed judges. The magnitude of the coefficient on Share Black decreases more measurably in this sub-sample from 0.581 to 0.437. However, the magnitude is still economically significant, implying that a 1% higher black population increases the property crime clearance rate by 0.44%. The estimate remains significant at the 1% level. Column (3) presents the results on the sample of counties with elected judges. The coefficient of interest remains positive but insignificant just as in the baseline specification. Across all three samples— the full sample, non-elected sample, and elected sample— the coefficient on the violent crime clearance rate is positive and significant at the 1% level. This suggests that counties which clear a higher percentage of violent crimes also clear a higher percentage of property crimes.

3.4.5 Violent Crime Clearance Results with PCCR as control

In the following specifications I use the violent crime clearance rate as the dependent variable and control for the property crime clearance rate within a county. I present the results with the controls for population size and the unemployment rate in Table 5.

Controlling for the property crime clearance rate within a county reduces the magnitude of the estimates on Share Black across all specifications but the coefficient remains insignificant.The coefficient of interest remains positive but insignificant just as in the baseline specification. Across all three samples— the full sample, non-elected sample, and elected sample— the coefficient on the property crime clearance rate is positive and significant at the 1% level. The magnitude is large ranging from 0.605 in counties with appointed judges to 0.899 to counties with elected judges. Counties with high property clearance rates also have high violent crime clearance rates.

4 Conclusion

Racial disparities are ubiquitous throughout every stage of the US justice system. While biases on the part of law enforcement and the judiciary contribute to these disparities and should be eliminated to promote equitable treatment across races, broader structural determinants must also be considered. In this paper I propose that individuals have in-group biases when it comes to the enforcement of the law. They discount the harm caused by members of their own group and thus, demand lower intensities of law enforcement when their (racial) group comprises a larger portion of the population. In the United States there are political channels through which voters can influence the level of law enforcement and this leads to counties with large minority populations being policed more intensely. If a state is composed of counties with heterogeneous population compositions, such that the size of minority populations varies between counties, then differential intensities of policing across these counties can lead to racial disparities even in the absence of bias in legal institutions. The model extends naturally to alternative assumptions of voter preferences for legal sanction, as well as potential applications to more general settings such as the provision of public goods.

There are two channels through which voters can increase legal sanctions. The first is by increasing the probability that an offender is caught—i.e. the level of policing—or by increasing the probability that an offender faces a punishment for their crime conditional on being caught—i.e. the harshness of the judge. I argue that it is relatively less expensive to increase the level of the legal sanction through the judiciary than it is through increased policing. However, not all voters have the same policy instruments available to them. Only 30 states utilize judicial elections for lower-court judges. In the remaining states, voters can only influence legal sanctions through policing. Thus, in the presence of an in-group bias, the level of policing should increase more sharply with the minority population size in counties where judges are appointed than in counties where judges are elected.

I take this theoretical prediction to a 15-year panel of county-level arrests and, exploiting variation in the judicial selection method between states, find evidence that counties with appointed judges increase their intensity of policing as the minority population size increases while counties with elected judges do not. In states with appointed judges, a 1% higher black population corresponds to a 0.58% increase in the clearance rate for property crimes —the ratio of arrests to reported crimes. There is no comparable effect in states which use judicial elections to select their judges. I rule out that the property crime results are driven by factors unrelated to race that make policing in counties with appointed judges by showing that there is no comparable change in the violent crime clearance rate among these counties. Violent crimes have much higher social costs than property crimes and I argue that this leads the demand for sanctions on violent crimes to be unresponsive to the minority population size. Thus, if counties with larger black populations in appointed states were simply more efficient for a reason orthogonal to race, then we would expect the violent crime clearance rate to also be higher in counties with larger black populations. The absence of this effect suggests that the proposed in-group bias is driving the results.

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Table 2: Property Crime Clearance Rates

	Full Sample		Non-Elected		Elected	
	(1)	(2)	(3)	(4)	(5)	(6)
	No Controls	With Controls	No Controls	With Controls	No Controls	With Controls
Share Black	0.078	0.048	0.581***	0.582***	0.102	0.051
	(0.081)	(0.088)	(0.172)	(0.171)	(0.080)	(0.092)
Non-Elected*Share Black	0.602**	0.634**				
	(0.219)	(0.216)				
Violent Crime Rate	No	Yes	No	Yes	No	Yes
Log Population	No	Yes	No	Yes	No	Yes
Unemployment Rate	No	Yes	No	Yes	No	Yes
Constant	0.110***	-0.216	0.086***	0.168	0.120***	-0.366
	(0.007)	(0.222)	(0.013)	(0.278)	(0.008)	(0.284)
Observations	32,198	32,194	9,305	9,305	22,893	22,889
R-squared	0.616	0.616	0.663	0.664	0.597	0.599
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the ratio of property crime arrests to property crime reports. Column (1) and Column (2) contains the full sample of observations Columns 3-6 are run on sub-samples of the observations. Column (3) and Column (4) run the specification only on states with non-elected judges. (5) and Column (6) run the specification only on states with elected judges. Standard errors are based on multi-way clustering at the state and year level. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Violent Crime Clearance Rates

	Full Sample		Non-Elected		Elected	
	(1)	(2)	(3)	(4)	(5)	(6)
	No Controls	With Controls	No Controls	With Controls	No Controls	With Controls
Share Black	0.125	0.072	0.443	0.515	0.143	0.050
	(0.269)	(0.284)	(0.363)	(0.369)	(0.273)	(0.298)
Non-Elected*Share Black	0.389	0.483				
	(0.326)	(0.351)				
Property Crime Rate	No	Yes	No	Yes	No	Yes
Log Population	No	Yes	No	Yes	No	Yes
Unemployment Rate	No	Yes	No	Yes	No	Yes
Constant	0.375***	0.467	0.391***	0.475	0.369***	0.448
	(0.022)	(0.505)	(0.029)	(0.949)	(0.028)	(0.594)
Observations	29,361	29,357	8,540	8,540	20,821	20,817
R-squared	0.574	0.577	0.569	0.572	0.571	0.574
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the ratio of violent crime arrests to violent crime reports. Column (1) and Column (2) contains the full sample of observations Columns 3-6 are run on sub-samples of the observations. Column (3) and Column (4) run the specification only on states with non-elected judges. (5) and Column (6) run the specification only on states with elected judges. Standard errors are based on multi-way clustering at the state and year level. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Property Crime Clearance Rates: PCCR

	Full Sample (1) With Controls	Non-Elected (2) With Controls	Elected (3) With Controls
Share Black	0.004	0.437***	0.020
	(0.096)	(0.132)	(0.099)
Non-Elected*Share Black	0.562***		
	(0.189)		
Violent Crime Clearance Rate	0.099***	0.073***	0.108***
	(0.009)	(0.011)	(0.009)
Log Population	Yes	Yes	Yes
Unemployment Rate	Yes	Yes	Yes
Constant	-0.252	0.056	-0.364
	(0.187)	(0.253)	(0.226)
Observations	29,145	7,869	21,276
R-squared	0.672	0.701	0.660
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: The dependent variable is the ratio of property crime arrests to property crime reports. Column (1) contains the full sample of observations Columns (2) and (3) are run on sub-samples of the observations. Column (3) runs the specification only on states with non-elected judges. Column (3) run the specification only on states with elected judges. Standard errors are based on multi-way clustering at the state and year level. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Violent Crime Clearance Rates: VCCR

	Full Sample (1) With Controls	Non -Elected (2) With Controls	Elected (3) With Controls
Share Black	0.101	0.138	0.092
	(0.259)	(0.269)	(0.265)
Non-Elected*Share Black	-0.085		
	(0.234)		
Property Crime Clearance Rate	0.821***	0.605***	0.899***
	(0.081)	(0.101)	(0.090)
Log Population	Yes	Yes	Yes
Unemployment Rate	Yes	Yes	Yes
Constant	0.338	0.119	0.444
	(0.442)	(0.876)	(0.480)
Observations	29,145	7,869	21,276
R-squared	0.606	0.526	0.631
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: The dependent variable is the ratio of violent crime arrests to violent crime reports. Column (1) contains the full sample of observations Columns (2) and (3) are run on sub-samples of the observations. Column (3) runs the specification only on states with non-elected judges. Column (3) run the specification only on states with elected judges. Standard errors are based on multi-way clustering at the state and year level. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$