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Christopher T. Robertson
The University of Arizona
James E. Rogers College of Law

Shima Baradaran Baughman
University of Utah College of Law

Megan S. Wright
Penn State Law & Penn State College of Medicine

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RACE AND CLASS: A RANDOMIZED EXPERIMENT WITH PROSECUTORS

Christopher Robertson*
Shima Baradaran Baughman**
Megan S. Wright***

Disparities in criminal justice outcomes are well known, and prior observational research has shown correlations between the race of defendants and prosecutors' decisions about how to charge and resolve cases. Yet causation is questionable: other factors, including unobserved variation in case facts, may account for some of the disparity. Disparities may also be driven by socio-economic class differences, which are highly correlated with race.

This article presents the first blinded, randomized controlled experiment that tests for race and class effects in prosecutors' charging decisions. Case-vignettes are manipulated between-subjects in five conditions to test effects of defendants' race and class status. In the control condition, race and class are omitted, which allows baseline measures for bias and pilot-testing of a blinding reform. Primary outcome variables included whether the prosecutor charged a felony, whether the prosecutor would pursue a fine or imprisonment, and the amounts thereof. With 467 actual prosecutors participating nationwide, we found that race and class did not have detectable prejudicial effects on prosecutorial decisions. This finding, contrary to the majority of observational studies, suggests that other causes drive known disparities in criminal justice outcomes.

*Professor of Law & Associate Dean for Research and Innovation, University of Arizona James E. Rogers College of Law; 1201 E Speedway, Tucson, AZ 85721; robertson@email.arizona.edu. Thanks to University of Arizona for research funding, as well as Harvard Law School and NYU Law School, where I worked on this project while a visiting professor. For helpful feedback the authors thank Jane Bambauer, Marc Miller, Wes Oliver, Chris Griffin and participants at the 2018 Quantlaw workshop, especially Jim Greiner, Jeff Fagan, John Donohue, and Lee Epstein. Amy Brown was excellent research assistant. We are thankful to all of the prosecutors nationally who participated in this experiment. IRB 69654 (University of Utah).

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***Assistant Professor of Law, Medicine, and Sociology, Penn State Law and Penn State College of Medicine. Thanks to Laureen O'Brien, Ellen Hill, Leann Jones, Danielle Curtin, and Joseph Radochonski for research assistance. Thanks to Roseanna Sommers and Nicholas Sterge for methodological feedback. Thanks to the Yale Law School librarians and faculty, especially Sarah Ryan and Issa Kohler-Hausmann, for consulting on research design in the early stages of this project.

We seek to understand the causes of the persistent and pervasive racial disparities in the American criminal justice system, and in particular whether they are perpetuated by the biases of prosecutors, who have profound power and discretion in this system.¹ One potential mechanism for racial disparities is the underlying socioeconomic disparities that are correlated with race.² Few studies have explored socioeconomic bias in prosecutorial decisions, and there has been no prior randomized, controlled study of prosecutorial decisions to tease apart race and class.

Observational research dominates this field, but causal inference is challenging, due to race and class confounds and by the difficulty of observing and coding the severity of the underlying conduct and the strength of the evidentiary case. It is also challenging to tease apart the underlying causal mechanism in a complex institution with several levels of selection and discretion involving police, prosecutors, defense attorneys, defendants, and judges each making decisions about whether and how to proceed with a case. This article avoids these problems by isolating a few variables and manipulating them systematically.

Of particular interest is the prosecutors' very broad discretion in the initial charging decision, since this discretion may reduce the efficacy of downstream policy reforms such as sentencing guidelines, which have been enacted to reduce disparities in outcomes.³ By collecting hundreds of prosecutorial charging decisions about the same case, this article explores how prosecutors use discretion in the initial charging decision and whether they punish defendants of certain races or socioeconomic classes more than others.

¹ See generally MICHELLE ALEXANDER, *THE NEW JIM CROW: MASS INCARCERATION IN THE AGE OF COLORBLINDNESS* 2 (2012); BRYAN STEVENSON, *JUST MERCY: A STORY OF JUSTICE AND REDEMPTION* 301 (2014); WILLIAM J. STUNTZ, *THE COLLAPSE OF AMERICAN CRIMINAL JUSTICE* 1 (2011); L. Song Richardson, *Systemic Triage: Implicit Racial Bias in the Criminal Courtroom*, 126 *YALE L.J.* 862, 866 (2016); James Forman, Jr. *The Black Poor, Black Elites, and America's prisons*, 32 *CARDOZO L. REV.* 791, 792 (2011); David A. Sklansky, *Cocaine, Race, and Equal Protection*, 47 *STAN. L. REV.* 1283, 1283 (1995).

² See Michele Benedetto Neitz, *Socioeconomic Bias in the Judiciary*, 61 *CLEV. ST. L. REV.* 137, 137 (2013) ("[S]ocioeconomic bias may be more obscure than other forms of bias, but its impact on judicial decision-making processes can create very real harm for disadvantaged populations."); Jay M. Spears, *Voir Dire: Establishing Minimum Standards to Facilitate the Exercise of Peremptory Challenges*, 27 *STAN. L. REV.* 1493 (1975) ("In our heterogeneous society, socially economic factors are especially likely to create powerful prejudices . . ."); Gwen Van Eijk, *Socioeconomic Marginality in Sentencing: The Built-in Bias in Risk Assessment Tools and the Reproduction of Social Inequality*, 19 *PUNISHMENT & SOC'Y* 463, 475 (2016) (noting socioeconomic bias in sentencing).

³ Rachel E. Barkow, *Sentencing Guidelines at the Crossroads of Politics and Expertise*, 160 *U. PA. L. REV.* 1599, 1602 (2012) ("[Sentencing] commissions could and should do more to address the relationship between guidelines and prosecutorial power . . . [b]ecause some amount of prosecutorial discretion is necessary and inevitable."). See also Kate Stith & Karen Dunn, *A Second Chance for Sentencing Reform: Establishing a Sentencing Agency in the Judicial Branch*, 58 *STAN. L. REV.* 217, 221 (2005); Russell D. Covey, *Rules, Standards, Sentencing, and the Nature of Law*, 104 *CAL. L. REV.* 447, 483 (2016).

It is also important to test upstream policy remedies, such as blinding prosecutors to defendant race and class information, which our control-condition allows.⁴ Some prosecutors' offices are beginning to experiment with blinding to reduce biases in or increase perceived legitimacy of prosecutor decisions, but the reform has not been systematically studied.⁵

This article proceeds as follows. Part I provides background on prosecutor decisions and prior research on race and class. Part II describes our research methods. Part III sets out the results. Part IV discusses the strengths and limitations of the study, along with important implications.

I. BACKGROUND

African Americans, or blacks, comprise 38% of all prisoners, though they only constitute 13% of the national population.⁶ Although some aspects of racial bias in America have decreased over the past century, blacks are still imprisoned at almost four times the rate of whites and receive 10% longer prison sentences.⁷ A variety of educational, economic, and cultural factors may contribute to these disparities along with implicit bias from decision makers such as legislators, police, prosecutors, judges, juries, and parole boards.

The prosecutor may be the government official with the most unreviewable power and discretion.⁸ After the police choose to arrest a subject, the first point of contact with a prosecutor is the decision to charge a defendant with a crime. Since the vast majority of cases are resolved short of trial, a second key point of attention is plea bargaining, which is also controlled by prosecutors.⁹

⁴ Sunita Sah et al., *Blinding Prosecutors to Defendants' Race: A Policy Proposal to Reduce Unconscious Bias in the Criminal Justice System*, 1 BEHAV. SCI. & POL'Y 69, 72 (2015). See generally, Jeffrey Fagan, *Legitimacy and criminal justice-introduction*. 6 OHIO ST. J. CRIM. L. 123 (2008).

⁵ Timothy Williams, *Black People Are Charged at a Higher Rate Than Whites. What if Prosecutors Didn't Know Their Race?* NY TIMES, June 9, 2019, <https://www.nytimes.com/2019/06/12/us/prosecutor-race-blind-charging.html>

⁶ *QuickFacts*, U.S. CENSUS BUREAU (Jul. 1, 2017), <https://www.census.gov/quickfacts/fact/table/US/PST045217>; *BOP Statistics: Inmate Race*, FED. BUREAU OF PRISONS (Feb. 24, 2018), https://www.bop.gov/about/statistics/statistics_inmate_race.jsp. In 2016, approximately one in 88 blacks were in prison, compared to one in 565 whites. See John Gramlich, *The Gap Between the Number of Blacks and Whites in Prison is Shrinking*, PEW RESEARCH CTR. (Jan. 12, 2018), <http://www.pewresearch.org/fact-tank/2018/01/12/shrinking-gap-between-number-of-blacks-and-whites-in-prison/>; *American Fact Finder: 2016 Population Estimates*, U.S. CENSUS BUREAU (Jul. 1, 2016), <https://factfinder.census.gov/faces/tableservices/jsf/pages/productview.xhtml?src=bkmk>.

⁷ M. Marit Rehavi & Sonja B. Starr, *Racial Disparities in Federal Criminal Charging and Its Sentencing Consequences*, 122 J. POL. ECON. 1320, 1320 (2014).

⁸ Stephanos Bibas, *Prosecutorial Regulation Versus Prosecutorial Accountability*, 157 U. PA. L. REV. 959, 959 (2009); Erwin Chemerinsky, *Eliminating Discrimination in Administering the Death Penalty: The Need for the Racial Justice Act*. 35 SANTA CLARA L. REV. 35 (1994).

⁹ See generally Sarah Ribstein, *A Question of Costs: Considering Pressure on White-Collar Criminal Defendants*, 58 DUKE L.J. 857, 868 (2009); Wesley MacNeil Oliver & Rishi Batra, *Standards of Legitimacy in Criminal Negotiations*, 20 HARV. NEGOT. L. REV. 61, 67 (2015); George C. Thomas, *Discretion and Criminal Law: The Good, the Bad, and the Mundane*, 109 PENN. ST. L. REV. 1043, 1043 (2005); Brandon K. Crase, *When Doing Justice Isn't Enough: Reimventing the Guidelines for Prosecutorial Discretion*, 20 GEO. J. LEGAL

Indeed, the vast majority of criminal cases are resolved through plea-bargaining.¹⁰ The standard theory is that prosecutors offer and defendants accept plea bargains “in the shadow” of the likely trial outcomes for the given case; such that severity of the agreed punishment tracks the ease of proving guilt and the heinousness of the crime. Bushway, Redlich, and Norris have shown empirical support for this theory, in an experiment involving 378 prosecutors (along with defense attorneys and judges).¹¹ Nonetheless, the researchers also observed substantial variation in outcomes, which could not be explained by the standard model.

If the case facts and evidentiary strength are not completely driving plea bargain outcomes, then what else is? The literature suggests that race and class may be driving outcomes as well.

A. Effects of Race

Prosecutors are human, and a wide range of studies have shown that implicit biases infect such decisions, and race biases are particularly trenchant, even for individuals who have no conscious prejudice.¹² For example, one landmark study found that merely substituting racialized names on resumes had a dramatic effect on an individual's chance of getting a job interview.¹³ Similarly, the media has shown racial bias in its coverage of

ETHICS 475, 475 (2007); Nicole T. Amsler, *Leveling the Playing Field: Applying Federal Corporate Charging Considerations to Individuals*, 66 DUKE L.J. 169, 173 (2016); Josh Bowers, *Legal Guilt, Normative Innocence, and the Equitable Decision Not to Prosecute*, 110 COLUM. L. REV. 1655, 1700 (2010); Peter L. Markowitz, *Prosecutorial Discretion Power at Its Zenith: The Power to Protect Liberty*, 97 B.U. L. REV. 489, 490 (2017); Shima Baradaran Baughman, *Subconstitutional Checks*, 92 NOTRE DAME L. REV. 1071, 1091 (2017); Kate Stith, *The Arc of the Pendulum: Judges, Prosecutors, and the Exercise of Discretion*, 117 YALE L. J. 1420, 1470 (2008); Jeffrey Standen, *Plea Bargaining in the Shadow of the Guidelines*, 81 CALIF. L. REV. 1471, 1472 (1993); Ronald Wright & Marc Miller, *The Screening/ Bargaining Tradeoff*, 55 STAN. L. REV. 29, 33 (2002); Cynthia Kwei Yung Lee, *Prosecutorial Discretion, Substantial Assistance, and the Federal Sentencing Guidelines*, 42 UCLA L. REV. 105, 107 (1994); Michael M. O'Hear, *Plea Bargaining and Procedural Justice*, 42 GEO. L. REV. 407, 420–25 (2008); James Vorenberg, *Decent Restraint of Prosecutorial Power*, 94 HARVARD L. REV. 1521, 1521 (1981); Ronald F. Wright, *Sentencing Commissions as Provocateurs of Prosecutorial Self-Regulation*, 105 COLUM. L. REV. 1010, 1011 (2005); Cynthia Kwei Yung Lee, *Prosecutorial Discretion, Substantial Assistance, and the Federal Sentencing Guidelines*, 42 UCLA L. REV. 105, 109 (1994).

¹⁰ Stephanos Bibas, *Transparency and Participation in Criminal Procedure*, 81 N.Y.U. L. REV. 911, 912, 923 (2006).

¹¹ Shawn D. Bushway, Allison D. Redlich, and Robert J. Norris, *An explicit test of plea bargaining in the “shadow of the trial”*, 52 CRIMINOLOGY 723 (2014).

¹² See Sunita Sah et al., *Blinding Prosecutors to Defendants' Race: A Policy Proposal to Reduce Unconscious Bias in the Criminal Justice System*, 1 BEHAV. SCI. & POL'Y 69, 72 (2015) (reviewing this literature).

¹³ Marianne Bertrand & Sendhil Mullainathan, *Are Emily and Greg More Employable than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination*, AM. ECON. REV. 991, 991 (2004).

violent crimes.¹⁴ For instance, a 2018 study found that the media was 95% more likely to attribute white shooters' violent actions to mental illness than black shooters.¹⁵

Scholars have long asserted that minorities, particularly black males, suffer from prosecutorial overcharging.¹⁶ Many researchers specifically cite prosecutors' unchecked decision-making power as a major cause of racial inequality in the criminal justice system.¹⁷ Nonetheless, racial bias can enter the criminal justice system at other junctures.¹⁸

Empirical studies have suggested the presence of racial bias in prosecutorial decisions. A recent study of federal cases by Rehavi and Starr found that blacks receive about 10% longer sentences than whites for comparable crimes, and about half of the gap can be explained by prosecutors' initial charging decisions.¹⁹ Local studies have yielded similar results, including a 1993 study conducted by Berk and Campbell examining crack cocaine charging practices in Los Angeles found that blacks constituted 58% of all federal cocaine arrests, but 83% of all federal cocaine charges.²⁰ Although whites accounted for 3% of all crack cocaine arrests, none were federally prosecuted for crack related offenses over the two-year period examined.²¹ Blacks charged with crack

¹⁴ Scott W. Duxberry et al., *Mental Illness, the Media, and the Moral Politics of Mass Violence: The Role of Race in Mass Shootings Coverage*, J. RES. CRIME & DELINQ. 1, 1 (2018).

¹⁵ *Id.* at 14 (analyzing 433 randomly selected news articles covering 219 mass shootings from 2013 to 2015).

¹⁶ See e.g., Andrew D. Leipold, *Objective Tests and Subjective Bias: Some Problems of Discriminatory Intent in the Criminal Law*, 73 CHI. KENT L. REV. 559, 559 (1998); Abbe Smith, *Can You Be a Good Person and a Good Prosecutor*, 14 GEO. J. LEGAL ETHICS 355, 368–69 (2001); Anthony V. Alfieri, *Community Prosecutors*, 90 CAL. L. REV. 1465, 1466 (2002); Kristin Henning, *Criminalizing Normal Adolescent Behavior in Communities of Color: The Role of Prosecutors*, 98 CORNELL L. REV. 383, 386 (2013)).

¹⁷ James Babikian, *Cleaving the Gordian Knot: Implicit Bias, Selective Prosecution, & Charging Guidelines*, 42 AM. J. CRIM. L. 139, 140 (2015); Angela J. Davis, *Prosecution and Race: The Power and Privilege of Discretion*, 67 FORDHAM L. REV. 13, 18 (1998); Shima Baradaran, *Race, Prediction, and Discretion*, 81 GEO. WASH. L. REV. 157, 172 (2013); Tobin Romero, *Liberal Discovery on Selective Prosecution Claims: Fulfilling the Promise of Equal Justice*, 84 GEO. L.J. 2043, 2051 (1996); K. Babe Howell, *Prosecutorial Discretion and the Duty to Seek Justice in an Overburdened Criminal Justice System*, 27 GEO. J. LEGAL ETHICS 285, 302 (2014).

¹⁸ Andrew Gelman, Jeffrey Fagan, and Alex Kiss, *An Analysis Of The New York City Police Department's "Stop-And-Frisk" Policy In The Context Of Claims Of Racial Bias*. 102 J. AM. STATISTICAL ASSOC. 813 (2007) (finding "that persons of African and Hispanic descent were stopped more frequently than whites, even after controlling for precinct variability and race-specific estimates of crime participation."); Arnold, David, Will Dobbie, and Crystal S. Yang, *Racial Bias in Bail Decisions*. 133 QJ. ECON. 1885 (2018) (finding that judges were biased in bail decisions).

¹⁹ M. Marit Rehavi & Sonja B. Starr, *Racial Disparities in Federal Criminal Charging and Its Sentencing Consequences*, 122 J. POL. ECON. 1320, 1320 (2014); see also Sonja B. Starr & M. Marit Rehavi, *Mandatory Sentencing and Racial Disparity: Assessing the Role of Prosecutors and the Effects of Booker*, 123 Yale L. J. 2 (2013).

²⁰ Richard Berk & Alec Campbell, *Preliminary Data on Race and Crack Charging Practices in Los Angeles*, 6 FED. SENT. R. 36, 38 (1993) (examining the relationship between race and cocaine charges through arrest data obtained from the Los Angeles Sheriff's Department (3,084) and charging data from the District Attorney's Office (8,250), and federal agencies (158) and concluding that African-Americans arrested for cocaine charges are at greater risk of being charged with federal crimes than other offenders).

²¹ *Id.*

cocaine offenses had a greater risk of being charged with federal crimes than all other offenders.²²

Another study examining racial disparities in pretrial diversion among male felons revealed that prosecutors were more likely to grant diversions to white defendants than non-white defendants with similar legal characteristics.²³ The study concluded that prosecutors exhibited a positive preference for diverting white defendants and a negative avoidance of diverting black defendants.²⁴ As a result, black defendants were 42% less likely to receive pretrial diversion than whites.²⁵ Similar results hold for intoxicated driving cases and domestic violence cases.²⁶

Misdemeanor cases also display these effects. A study of shoplifters discovered that blacks were less likely to have their charges dismissed.²⁷ A 2014 observational study on misdemeanor marijuana cases found that prosecutors were less likely to offer blacks reduced charge offers compared to whites.²⁸ However, most of this variation was explained by legal factors, including evidence and arrest circumstances. Nonetheless, black defendants were still more likely to receive custodial offers, after controlling for these factors.²⁹

There are also more comprehensive studies. Stolzenberg and colleagues observe that race may have an effect at multiple points in the criminal justice process, and it may be cumulative. Their metaanalysis of data from 65 populous counties found an effect of a defendant's race at two of eight key decision points: likelihood of incarceration and

²² Richard Berk & Alec Campbell, *Preliminary Data on Race and Crack Charging Practices in Los Angeles*, 6 FED. SENT. R. 36, 38 (1993) (odds of full prosecution were 1.59 times higher for African Americans and 2.54 times higher for Hispanics compared to white defendants).

²³ Traci Schlesinger, *Racial Disparities in Pretrial Diversion: An Analysis of Outcomes Among Men Charged With Felonies and Processed in State Courts*, 3 RACE & JUST. 211, 223 (2013) (controlling for prior record and offense severity, the study found that black, Latino, and Asian/Native American defendants are 28%, 13%, and 31% less likely to receive pretrial diversion than white defendants).

²⁴ *Id.* at 229. See also Michael J. Leiber & Anita N. Blowers, *Race and Misdemeanor Sentencing*, 14 CRIM. JUST. POL'Y R. 464 (2003) (finding evidence of racial bias among defendants charged with misdemeanors; the research indicated that African Americans were 7% more likely than whites to have their case given a classification of priority which put them at a disadvantage).

²⁵ Traci Schlesinger, *Racial Disparities in Pretrial Diversion: An Analysis of Outcomes Among Men Charged With Felonies and Processed in State Courts*, 3 RACE & JUST. 211, 223 (2013).

²⁶ Tana McCoy et al., *An Examination of the Influence of Strength of Evidence Variables in the Prosecution's Decision to Dismiss Driving while Intoxicated Cases*, 37 AM. J. CRIM. JUST. 562, 572 (2012) (finding prosecutorial bias on account of race using data of 2,358 driving while intoxicated cases and controlling for age, sex, criminal history and charge seriousness); Kris Hennings & Lennete Feder, *Criminal Prosecution of Domestic Violence Offenses: An Investigation of Factors Predictive of Court Outcomes*, 32 CRIM. JUST. & BEHAV. 612, 635 (2005) (examining 4,178 defendants arrested for misdemeanor or felony domestic violence offense, finding charging decisions associated with the defendant's race in multivariate models).

²⁷ Kenneth Adams & Charles R. Cutshall, *Refusing to Prosecute Minor Offenses: The Relative Influence of Legal and Extralegal Factors*, 4 JUST. Q. 595, 606 (1987) (a study of 745 shoplifters).

²⁸ Besiki Luka Kutateladze, Nancy R. Andiloro, and Brian D. Johnson. *Opening Pandora's box: How does defendant race influence plea bargaining?* 33 JUSTICE QUARTERLY 398 (2016).

²⁹ *Id.*, at 399.

length of sentence.³⁰ However, a huge cumulative discriminatory effect was evident when all decision points were considered in the totality – yielding a 42% increased chance of a severe sanction for Black defendants, even after controlling for legal factors.³¹

A 2016 meta-analysis assessed empirical findings from 26 studies involving a total of 86,877 criminal cases.³² Minority offenders were 9% more likely to be charged or fully prosecuted than white offenders.³³

To the contrary, a few studies have found null results or even a racial disparity in favor of minorities. For instance, a 2010 study researching the effects of race, age, and gender on prosecutorial decision making found no bias between various race-gender-age characteristics with the exception of one category.³⁴ Several studies examining charging in sexual assault cases have also concluded that the race of the defendant does not have a statistically significant effect on prosecutors' initial charging decisions.³⁵

A 1996 study by Barnes and Kingsnorth found that blacks were more likely than whites and Hispanics to have their drug case dismissed or rejected by prosecutors.³⁶ On the other hand, when sentenced to prison, African Americans and Latinos got longer sentences.³⁷ These dynamics reflect the complications of observational research, where strength of the evidence and other case facts may vary in ways that are confounding with the variables of interest.

A primarily qualitative 2008 study by Wright and Miller compared the racial composition of defendants arrested by police with those formally charged by prosecutors.³⁸

³⁰ Lisa Stolzenberg et al., *Race and Cumulative Discrimination in the Prosecution of Criminal Defendants*, 3 RACE & JUST. 275, 286 (2013).

³¹ *Id.* (decision points include whether release on bail was financial, whether bail was denied, the bail amount, whether the defendant made bail, whether the defendant was held pretrial, whether the case was adjudicated as a felony, whether the defendant received a prison sentence, and the length of the imposed prison sentence).

³² Jawjeong Wu, *Racial/Ethnic Discrimination and Prosecution: A Meta-Analysis*, 43 CRIM. JUST. & BEHAV. 437, 441 (2016).

³³ *Id.* at 447.

³⁴ T.W. Franklin, *The Intersection of Defendants' Race, Gender, and Age in Prosecutorial Decision Making*, 38 J. CRIM. JUST. 185, 190 (2010) (finding that in felony drug cases from 1998, white defendants from thirty to thirty-nine years old were significantly less likely to have their cases dismissed as compared to black defendants between the ages of eighteen and twenty-nine years old (OR: 0.630, SE: 0.145)).

³⁵ Jeffrey W. Spears & Cassia C. Spohn, *The Effect of Evidence Factors and Victim Characteristics on Prosecutors Charging Decisions in Sexual Assault Cases*, 14 JUST. Q. 501, 513 (1997); Dawn Beichner & Cassia Spohn, *Prosecutorial Charging Decisions in Sexual Assault Cases: Examining the Impact of a Specialized Prosecution Unit*, 16 CRIM. JUST. POL'Y R. 461 (2005); Cassia C. Spohn & David Holleran, *Prosecuting Sexual Assault: A Comparison of Charging Decisions in Sexual Assault Cases Involving Strangers, Acquaintances, and Intimate Partners*, 18 JUST. Q. 651, 677 (2001).

³⁶ Carole Wolff Barnes & Rodney Kingsnorth, *Race, Drugs, and Criminal Sentencing: Hidden Effects of the Criminal Law*, 24 J. CRIM. JUST. 39 (1996) (also finding that Caucasians were more likely than African Americans and Latinos to be placed on diversion).

³⁷ *Id.*

³⁸ Marc L. Miller & Ronald F. Wright, *The Black Box*, 94 IOWA L. REV. 125, 157 (2008).

According to the New Orleans data, 12.2% of defendants referred for prosecution by police were white and 85.6% were black, while 12.1% of the defendants formally charged were white and 85.7% were black.³⁹ The study concluded that the racial composition of defendants entering and exiting the “prosecutorial pipeline were virtually identical.”⁴⁰ However, the researchers note that the aggregate numbers “obscure some interesting shifts in the racial composition of defendants for some particular crimes,” as they move through the pipeline from police to prosecution.⁴¹ Prosecutors increased the racial imbalance by at least 2% for aggravated assault, drug, and property crimes, while actually reducing the racial imbalance in nine crime categories.⁴²

Other studies examining the later decision to dismiss have also failed to find evidence of racial bias. A 1986 study conducted by Albonetti examined how race, among other factors, affected prosecutors’ decisions to continue felony prosecution following grand indictment.⁴³ After examining data collected from 4,238 felony cases, Albonetti concluded that race did not produce a statistically significant effect on the probability of continued prosecution.⁴⁴ Around the same time period, Susan Welch conducted a similar study examining the dismissal, conviction, and incarceration rates among 10,000 Hispanic, white, and black male defendants.⁴⁵ Welch found “little difference” in treatment when using multivariate analysis and controlling for legal and extra-legal factors.⁴⁶

However, another study of 33,000 cases filed in Los Angeles County examining the effect of racial bias on both initial charging and dismissal decisions found evidence of racial discrimination at the initial charging stage, but not during the later decision to dismiss.⁴⁷ The authors explained this discrepancy by arguing that dismissals are more

³⁹ *Id.*

⁴⁰ *Id.*

⁴¹ *Id.*, at 157.

⁴² *Id.*

⁴³ Celesta A. Albonetti, *Criminality, Prosecutorial Screening, and Uncertainty: Toward a Theory of Discretionary Decision Making in Felony Case Processing*, 24 *CRIMINOLOGY* 623, 635 (1986).

⁴⁴ *Id.* See also Celesta A. Albonetti, *Prosecutorial Discretion: The Effects of Uncertainty*, 21 *L. & SOC’Y R.* 291, 304 (1987) (a follow up study examining 6,014 felony cases, finding that race was not significant).

⁴⁵ Susan Welch et al., *Sentencing: The Influence of Alternative Measures of Prior Record*, 22 *CRIMINOLOGY* 257, 261 (1984) (controlling for seriousness of offense, prior record, employment status, type of attorney, and whether anyone was injured during the crime and concluding that there was little difference in dismissal, conviction, and incarceration among Hispanic (28%), white (26%), and black (27%) male defendants).

⁴⁶ *Id.* See also Ilene Bernstein et al., *Societal Reaction To Deviants: The Case of Criminal Defendants*, 42 *AM. SOC. REV.* 743, 752 (1977) (race had no significant effect on dismissal and adjournment, and only a small effect on sentencing); Moheb Ghali & Meda Chesney-Lind, *Gender Bias and the Criminal Justice System: An Empirical Investigation*, 70 *SOC. SCI. REV.* 164 (1986) (race was not a significant predictor of case dismissal (OR: 1.103)); Martha A. Myers & John Hagan, *Private the Public Trouble: Prosecutors and the Allocation of Court Resources*, 26 *SOC. PROBLEMS* 439, 447 (1979) (finding that defendants race did not impact prosecutorial discretion but race of the victim did have a significant effect on the decision).

⁴⁷ Cassia Sophn et al., *The Impact of the Ethnicity and Gender of Defendants on the Decision to Reject or Dismiss Felony Charges*, 25 *CRIMINOLOGY* 175, 187 (1987)(At the rejection stage, prosecutors failed to

visible than rejections and thus “[a]s the process becomes more visible, norms against racial discrimination in the process may become more pronounced.”⁴⁸

Meta-analyses by Free in 2001 and 2005 argued that even though some studies support a nondiscrimination thesis, many were invalid due to flaws in research methods and design.⁴⁹ For example, of the seventeen nondiscrimination studies examined in Free’s 2001 meta-analysis, approximately three-fourths of the studies combined racial categories, over half failed to control for evidentiary strength, a third used aggregate data from different offenses, and a fourth limited analysis to a single presentencing decision.⁵⁰ Free concluded that although these deficiencies significantly minimize the likelihood of detecting racial disparities, some racial differences still emerged.⁵¹

While there are a few outliers, most of the empirical studies on race and prosecutorial decision making have concluded that racial bias exists, particularly in the initial charging decision. Nonetheless, almost all of these studies are observational, and often lack independent and granular measures of the key drivers of appropriate discretion, underlying crime severity and evidentiary strength. The studies often also lack granular information about other defense characteristics, such as income or wealth, which are known mechanisms by which criminal defendants can secure excellent legal representation and better outcomes. Finally, because race and class are significantly correlated, these studies can rarely untangle their causal effects, even if class were carefully measured.

B. *Effects of Socioeconomic Class*

Researchers have also examined the effect of socioeconomic or class bias on the criminal justice system. Minorities account for almost 80% of the poor in America,⁵² and approximately 80% of people charged with a crime are poor.⁵³

There are several causal mechanisms that may drive class disparities in criminal justice outcomes. First there may be differences in criminality or in the enforcement / detection of criminality. Studies show that criminal involvement among nonwhites is

prosecute 59% of Anglos, 40% of black and 37% of Hispanic defendants ($p < 0.01$); Prosecutors dismissed 34% of the charges against Anglos, 36% against blacks, and 34% against Hispanics).

⁴⁸ *Id.* See also Eric P. Baumer et al., *The Role of Victim Characteristics in the Disposition of Murder Cases*, 17 JUST. Q. 281 (2000) (random sample of 2,000 murder cases; found that race did not have a significant effect during initial screening, but cases involving non-white defendants were more likely to be carried forward (less likely to be dismissed)).

⁴⁹ Marvin D. Free, Jr., *Racial Bias and the American Criminal Justice System: Race and Presentencing Revisited*, 10 CRITICAL CRIMINOLOGY 195, 200 (2001); Marvin D. Free, *Prosecutorial Decision Making and Minority Group-Threat Theory*, 18 CRIM. JUST. STUD. 7, 24 (2005).

⁵⁰ Marvin D. Free, Jr., *Racial Bias and the American Criminal Justice System: Race and Presentencing Revisited*, 10 CRITICAL CRIMINOLOGY 195, 219 (2001).

⁵¹ *Id.* at 220.

⁵² *Who is the Poor?*, INST. FOR RESEARCH ON POVERTY, <https://www.irp.wisc.edu/resources/who-is-poor/> (last visited Jul. 31, 2018).

⁵³ Paul D. Butler, *Poor People Lose: Gideon and the Critique of Rights*, 122 YALE L.J. 2176, 2181 (2013).

directly correlated with social class indicators such as personal income, education, unemployment, and welfare use.⁵⁴ For example, prisoners have a median annual income of \$19,185 prior to imprisonment, which is 41% less than individuals who have never been incarcerated.⁵⁵ Poor communities, particularly poor black neighborhoods, also have a much greater police presence as measured by the number of police stops and arrests.⁵⁶

Second, focusing on prosecutorial decisions, on standard economic theories about deterrence of crime, one might expect prosecutors to utilize different punitive strategies for wealthier defendants, in contrast to poorer defendants.⁵⁷ Since incarceration is a costly sanction for the state, there will generally be a preference to use fines where feasible.⁵⁸ For poorer defendants with no wealth to pay such fines, prosecutors must shift to incarceration. Dynamically targeting such sanctions may be a rational strategy for prosecutors, even if it creates a bias towards incarceration for poorer persons.

Third, there are also institutional drivers of disparate outcomes on the basis of social class, including access to representation. While the Supreme Court in *Gideon v. Wainwright* sought to decrease socioeconomic disparity within the judicial system by providing all criminal defendants with an attorney regardless of their ability to pay, criminal prosecutions against the poor increased from 43% in 1962 to 80% by 1992.⁵⁹ Although the poor now have a constitutional right to an attorney, many still decline to have a public defender appointed to their case. Research conducted in Michigan found that 95% of people facing misdemeanors waived the right to an attorney and plead guilty because they could not afford the \$240 charge for a public defender.⁶⁰ As such, socioeconomic factors have impacted a defendant's access to adequate representation and seem to have other criminal justice impacts.⁶¹ Prosecutors may predict that higher class individuals may be more likely to have private representation that resists severe sanctions, creating more work for prosecutors.

⁵⁴ R. Gregory Dunaway et al., *The Myth of Social Class and Crime Revisited: An Examination of Class and Adult Criminality*, 38 CRIMINOLOGY 589 (2000) (concluding that social class was related to criminal involvement for nonwhites).

⁵⁵ Bernadette Rabuy & Daniel Kopf, *Prisons of Poverty: Uncovering the Pre-Incarceration Incomes of the Imprisoned*, PRISON POL'Y INITIATIVE (Jul. 9 2015), <https://www.prisonpolicy.org/reports/income.html>.

⁵⁶ Paul D. Butler, *Poor People Lose: Gideon and the Critique of Rights*, 122 YALE L.J. 2176, 2183 (2013).

⁵⁷ See generally Gary S. Becker, *Crime and Punishment: An Economic Approach*, THE ECONOMIC DIMENSIONS OF CRIME, pp. 13-68 (1968).

⁵⁸ Mitchell Polinsky and Steven Shavell. *The optimal use of fines and Imprisonment*. 24 J. PUBLIC ECON. 89 (1984).

⁵⁹ William J. Stuntz, *The Uneasy Relationship Between Criminal Procedure and Criminal Justice*, 107 YALE L.J. 1, 7 n.7 (1997).

⁶⁰ Nat'l Legal Aid & Defender Ass'n, *A Race to the Bottom: Speed & Savings over Due Process* 32 (2008).

⁶¹ See e.g., Stewart J. D'Alessio & Lisa Stolzenberg, *Socioeconomic Status and the Sentencing of the Traditional Offender*, 21 J. CRIM. JUST. 61 (1993) (finding significant inverse relationship between socioeconomic status and length of sentence for the crimes of manslaughter and the possession of narcotics).

Finally, prosecutors may suffer from out-group bias, viewing higher-class individuals as more like themselves and lower-class individuals as unlike themselves. It may be easier to excuse conduct by those from fellow members of the professional class.⁶²

Socioeconomic bias has manifested at other points in the criminal justice process, including convictions and sentencing.⁶³ Research indicates that socioeconomic discrimination is especially prevalent at bail hearings, for example.⁶⁴

A study of domestic violence crimes found that socioeconomic status affected prosecutorial discretion, as prosecutors were more likely to drop a case if the defendant was from a higher socioeconomic status, as defined by an imputed income based on his home address.⁶⁵ Data was obtained from a sample of 4,178 defendants arrested for a misdemeanor or felony domestic violence offense in Shelby County, Tennessee. From this data, researchers discovered that prosecutors took 86% of cases in which the defendant had an income of less than \$20,694, but only 74% of cases with defendants from a higher income.

However, another study researching the socioeconomic status of 175 defendants on death row found no significant disparities in charging and sentencing outcomes among defendants of different socioeconomic backgrounds.⁶⁶ Another study found even more mixed results, concluding that earlier case processing decision points (filing charges and full prosecution) resulted in more favorable dispositions for defendants of lower socioeconomic status while later decision points (convictions and sentencing) favored

⁶² See generally, Bastian Schiller, Thomas Baumgartner, and Daria Knoc, *Intergroup Bias in Third-Party Punishment Stems from both Ingroup Favoritism and Outgroup Discrimination*, 35 EVOLUTION AND HUMAN BEHAVIOR 169 (2014).

⁶³ Freda Adler, *Socioeconomic Factors Influencing Jury Verdicts*, 3 N.Y.U. REV. L. & SOC. CHANGE 1, 10 (1973) (finding that high discrepancies in occupational status between juror and defendant lead to higher conviction rates); Sonja B. Starr, *Evidence-Based Sentencing and the Scientific Rationalization of Discrimination*, 66 STAN. L. REV. 803 (2014); Terance D. Miethe & Charles A. Moore, *Socioeconomic Disparities Under Determinate Sentencing Systems: A Comparison of Preguideline and Postguideline Practices in Minnesota*, 23 CRIMINOLOGY 337, 358 (1985) (noting that socioeconomic attributes of the offender influenced sentencing decisions indirectly through presentence decisions and case attributes).

⁶⁴ See Lucy Nicholson, *Not in it for Justice*, HUMAN RIGHTS WATCH (Apr. 11 2017), <https://www.hrw.org/report/2017/04/11/not-it-justice/how-californias-pretrial-detention-and-bail-system-unfairly> (observing that, in California, over 63% of prisoners in county jail are serving time because they cannot pay bail); See also SHIMA BARADARAN BAUGHMAN, THE BAIL BOOK: A COMPREHENSIVE LOOK AT BAIL IN AMERICA'S CRIMINAL JUSTICE SYSTEM 159 (2018) (noting that, in New York, five in six detained defendants had bail set with impossible financial conditions).

⁶⁵ Kris Henning & Lynette Feder, *Criminal Prosecution of Domestic Violence Offenses: An Investigation of Factors Predictive of Court Outcomes*, 32 CRIM. JUST. & BEHAV. 612, 631 (2005).

⁶⁶ David C. Baldus et al., *Arbitrariness and Discrimination in the Administration of the Death Penalty: A Legal and Empirical Analysis of the Nebraska Experience*, 81 NEB. L. REV. 486, 665 (2002) (Socioeconomic status was measured in terms of the defendant's occupation; the death sentencing rate for low SES defendants was 14% compared to a 32% rate for mid-range SES defendants.)

defendants from a higher socioeconomic status.⁶⁷ As a result, the conclusions are mixed as far as whether socioeconomic bias exists in criminal justice decision making.

Although it seems clear that poverty is related to criminal justice outcomes, the few existing studies of socioeconomic bias are not sufficient to provide any conclusive determination of whether prosecutors suffer from subconscious bias due to class, nor teased out its relation to race bias. None of these studies have studied prosecutorial charging decisions broadly, and none have used randomized controlled trials. We are particularly interested in how race and class interact. Some have suggested that narrowing the economic divide between races may be instrumental to reducing implicit racial biases, and also useful to reduce institutional racism.⁶⁸ These prior studies leave an important research gap filled by this study.

II. METHODOLOGY

This study summarizes the results of a randomized controlled experiment in which real prosecutors were asked to review realistic but hypothetical cases and make charging decisions. The case-vignettes were manipulated between-subjects in five conditions, a 2x2 factorial + control design, to test the effects of defendants' race, white versus black, and defendants' class status, working-class versus college-educated.

A. Sample Description

No comprehensive database of U.S. prosecutors is available for research purposes, and our initial discussions with prosecutor organizations suggested that research collaboration would be unlikely. In order to construct a sample of prosecutors for this study, we targeted one or two states (depending on population) in each of the nine United States Census regional divisions. Research assistants collected non-federal prosecutor email addresses from prosecutor office websites and state bar associations. After these sources were exhausted, state FOIA requests were also used to obtain prosecutor names and contact information from all states that had accessible records. Information was obtained for at least one respondent from over half of the 50 states.

Prosecutors were then emailed an invitation to participate in the study, with a link to the study and an offer of a \$5 Amazon gift card. Respondents were told that, "the purpose of this research study is to understand how prosecutors make decisions." The protocol was approved by the University of Utah Institutional Review Board. We contacted 4,484 prosecutors, and 542 completed the study for a 12.09% response rate. A sample size of 467 responses was obtained after eliminating responses that were incomplete or were completed in fewer than 10 minutes (suggesting poor engagement with the

⁶⁷ John D. Wooldredge & Amy Thistlethwaite, *Bilevel Disparities in Court Dispositions for Intimate Assault*, 42 CRIMINOLOGY 417 (2004).

⁶⁸ See e.g., Jeffrey, Fagan, Ellen Slaughter, and Eliot Hartstone. *Blind justice? The Impact of Race on the Juvenile Justice Process*, 33 CRIME & DELINQUENCY 224, 224 (1987) ("the narrowing of social gaps may also reduce disparate perceptions of minorities in the juvenile justice system, and restore their population balance in delinquent populations").

materials) or greater than 100 minutes (suggesting that they were interrupted and thus distracted).

The profile of the jurisdiction and prosecutor characteristics of study respondents is shown in Table 1. Nearly a quarter, 23%, of respondents were lead prosecutors, and 79% worked in the felony division. The length of time respondents served as prosecutors ranged from less than a year to 45 years with a median length of service of 10 years. Respondents ranged in age from 27 to 78, with a median age of 45.⁶⁹

Approximately 65% of the sample were men. Although there does not appear to be a canonical study of the demographics of state prosecutors nationwide, our sample may under-represent men, who may in fact constitute 83% of elected state prosecutors.⁷⁰

Ninety-six percent of respondents were non-Hispanic, 90% were white, 4% were black, almost 4% were “other,” and the remainder were Native American or Asian. Again, there appears to be no canonical report of prosecutor demographics, but our sample is similar to that reported in one recent study of state prosecutors (where 88.42% were white, 2.58% were black, and 1.68% were Hispanic/Latino).⁷¹

Just under 8% of respondents were in a jurisdiction of over 2 million, about 11% in a jurisdiction of between 1-2 million, 10% in a jurisdiction of 500,000-1 million, 28% in a jurisdiction of 100,000-500,000, and 43% in a jurisdiction of less than 100,000. In comparison, national data on prosecutors suggests that our sample over-represents prosecutors working in highly-populous jurisdictions (with 18% of our sample in jurisdictions over 1M people, compared to 1.8% of prosecutors nationwide in 2007).⁷² Similarly, our sample underrepresents prosecutors in less populous regions (with 43% in jurisdictions under 100,000 compared to 59.6% nationwide).⁷³

Due to differences in data availability and response rates, the sample primarily consists of prosecutors from the Mountain (23%), East North Central (21%), South Atlantic (16%), Pacific (12%), West North Central (11%), and East South Central (9%) regions.

B. *Materials and Manipulations*

After conducting a pilot experiment with a Salt Lake City prosecutors office and collaborating with experienced prosecutors on design, a case-vignette was constructed

⁶⁹ See Bushway et al., *supra* note 11 at 735 (finding an average age of 45.03 years for prosecutors in their online research population).

⁷⁰ See Justice for All? A Project of the Reflective Democracy Campaign, (2014) <https://wholeads.us/justice/wp-content/themes/phase2/pdf/key-findings.pdf>. See also Bushway et al., *supra* note 11 at 735 (finding 68.25% of prosecutors in their online research population to be male).

⁷¹ Authors calculators from Justice for All? A Project of the Reflective Democracy Campaign, (2014), available at <https://wholeads.us/justice/wp-content/themes/phase2/data/JusticeforAllDataSummer2015.zip>. See also Bushway et al., *supra* note 11 at 735 (finding 8.73% of prosecutors in their online research population to be non-white).

⁷² Steven W. Perry and Duren Banks, *Prosecutors in State Courts, 2007 - Statistical Tables NCJ-234211*, U.S. Department of Justice Office of Justice Programs Bureau of Justice Statistics ,1, December, 2011.

⁷³ *Id.*

using two police reports describing an arrest that the officers coded for disorderly conduct. These 750-word, two-page reports were realistically designed to include police department logos and a standardized form.

The specific vignette in the study is purposely designed to allow a prosecutor to charge substantial major crimes, minor crimes, or no crimes at all. It involves an assault, or potential aggravated assault, depending on how the prosecutor views the situation and individual. It is designed to allow for maximum discretion of the prosecutor.

In the vignette, a slightly intoxicated man is found in a subway station yelling obscenities, asking people for money, and brandishing a knife. He is frustrated that no one will give him money. At one point, he is angered that a certain woman does not give him money after repeated requests and grabs her arm. He does not threaten her specifically but does “dangle a knife at his side by his other arm.” The police then arrive and arrest him, and he reports having just broken up with his girlfriend. Prosecutors were also provided with an abbreviated 623-word two-page statutory code and sentencing guidelines based on the laws of a real state, defining these crimes and specifying the punishment ranges for each. The study was designed to take participants about 15 minutes to complete.

The case-vignettes were manipulated between-subjects in five conditions, a 2x2 factorial + control design to test effects of defendants’ race, white versus black, and defendants’ social class, higher-status accountant versus working-class fast food worker. In the control condition, race and class are redacted altogether in order to assess differences from the baseline. Moreover, a control allows us to pilot-test a potential blinding reform to be used in the field, in which real prosecutors would make charging decisions without knowledge of these extraneous factors of race and class.

We selected a seemingly race-neutral name, Michael Johnson, for the defendant in all conditions. Prior research on racial bias in employment decisions used racialized names as proxies for race.⁷⁴ The criminal charging domain is distinctive because police reports actually have specific blanks for defendant race, for use in identifying a suspect.

The overt race field was manipulated to be black, white, or completely omitted (as if there was no field at all). Each of the officer’s narratives also mentioned the defendant’s race through statements like “...two citizens approached me to report an unstable, white male of moderate height and build...” and “...upon entering the [train] station I saw a white male of medium build and height...”. Respondents had a total of four opportunities to be exposed to the defendant’s race in the non-blinded conditions, as there were two police officer reports for this same incident. In the control condition, race references were seamlessly omitted from the reports. Black-line redaction was not used in control conditions to avoid drawing attention to the missing information.

⁷⁴ Marianne Bertrand & Sendhil Mullainathan, *Are Emily and Greg More Employable than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination*, AM. ECON. REV. 991, 991 (2004).

Prior research from the U.S. Census identified occupations with high and low prestige.⁷⁵ Two commonplace occupations were selected, an accountant with an average prestige score of 5.7 and a fast food worker with an average score of 2.8. Police reports do not typically include a field for the defendant's socioeconomic class, so this variable was only manipulated in the police officer's narrative. In the non-blinded conditions, the first officer's report included this fact in the very first line stating "on Friday evening, March 12, 2013, Michael Johnson, a twenty-nine-year-old fast food worker, was placed under arrest...".

Respondents were first asked which charges they would apply, with ten choices ranging from no charges to aggravated assault. The options were: No Charges, Deferred Prosecution, Disorderly Conduct, Loitering, Public Nuisance, Criminal Nuisance, Harassment, Endangerment, Assault, and Aggravated Assault. Respondents then indicated if they would press multiple charges.

Next, respondents were asked to "indicate which confinement term and/or monetary penalty, if any, you would most likely seek in a plea deal with the suspect (i.e., the term and/or penalty that would ultimately satisfy your pursuit of justice). In answering this question, you may refer to the sentencing guidelines if you wish." Respondents had separate blanks for confinement and monetary penalty, and were also able to make notes as to suspension of these penalties, which were later coded. Some respondents provided ranges, so minimum and maximum values were recorded. Some respondents also provided additional comments.

III. RESULTS

Random assignment of 467 prosecutor participants yielded about 90 respondents in the control condition and in each race/class cell. For pooled analyses exploiting our factorial design, we had about 180 respondents per race and per class.

As shown in Table 1, the prosecutors responded to the case by charging felonies 15.85% of the time. They demanded monetary penalties 41.50% of the time, and confinement 27.05% of the time. The average monetary penalty was \$242.75 (\$746.78 excluding zeros) and the average minimum days of confinement was 21.40 (80.17 excluding zeros).

[INSERT TABLE 1 APPROXIMATELY HERE]

A. Overall Prosecutorial Severity

Our primary outcome variable measures the overall severity of the charges and punishment that prosecutors recommended. Although charging decisions can be extremely complex, this index variable allows us to avoid multiple-testing problems by focusing on a single key hypothesis test, and also accounts for tradeoffs between outcomes (e.g., prosecutors who might substitute a harsh monetary penalty for an even

⁷⁵ TOM W. SMITH & JAESOK SON, MEASURING OCCUPATIONAL PRESTIGE ON THE 2012 GENERAL SOCIAL SURVEY 12, 16 (2014), *available at* <http://gss.norc.org/Documents/reports/methodological-reports/MR122%20Occupational%20Prestige.pdf>.

harsher confinement term, which would not be captured if each outcome were considered separately).

[INSERT FIGURE 1 APPROXIMATELY HERE]

[INSERT TABLE 2 APPROXIMATELY HERE]

This severity of punishment variable incorporates whether prosecutors decided to charge the defendant, and what monetary penalty and confinement term, if any, was recommended. This variable consists of eight levels, in ascending order of severity: no charges/deferred prosecution (16% of respondents); charges with a suspended sentence (37%); charges with a monetary penalty in the bottom third of monetary penalties (7%); charges with a monetary penalty in the middle third (8%); charges with a monetary penalty in the top third (5%); charges with a confinement recommendation in the lowest third of confinement terms (9%); charges with a confinement recommendation in the middle third (9%); and charges with a confinement recommendation in the top third (9%). Prosecutors may have recommended both a monetary penalty and a term of confinement, and in these cases, their recommendation was coded based on their confinement response, as the more severe penalty.

A regression model assessed which, if any, factors affected the severity of punishment prosecutors recommended. See Table 2 (reporting linear regression; ordered logit models were substantially the same). We see substantial internal validity: prosecutors who recommended felony charges were 1.2 levels of severity greater than prosecutors who did not ($p=.000$), and for every additional charge recommended, prosecutors increased the level of recommended punishment severity by .24 ($p=.000$).

Figure 1 displays our primary findings on race and class. None of the experimental conditions (or pooled analysis for race or class, discussed below) were statistically significant in any of the models.

The confidence intervals around our coefficients provide a sense of the study's statistical power, as they rule out substantial effects as being inconsistent with our data. For example, for Black defendants in our pooled analysis, our point estimate of -0.11 for severity suggests that they may receive slightly less severe treatment than White defendants. At our sample we cannot rule out the possibility of very small positive effects, but with the upwards bound of our confidence interval reaching 0.53 (a 10.9% increase), we can rule out larger effects as being inconsistent with our observed data.

In our covariates, we also found some incidental effects. In the complete model, New England prosecutors were 1.28 levels of severity below Mountain region prosecutors ($p=.029$), prosecutors who work in misdemeanor or other nonfelony divisions were half a level of severity below felony prosecutors ($p=.088$), and for every year of age, prosecutors decreased their level of recommended punishment severity by .03 ($p=.047$).

B. Number of Charges

For robustness, to determine whether race or class biases may be expressed in more granular ways, we examined a range of individual outcome variables, first being the number of charges prosecutors would bring against the hypothetical defendant. Almost all (97%) of prosecutors filed at least one charge. The mean number of charges was 3.16 [CI 2.98, 3.33], and excluding the 15 prosecutors who declined to bring charges, the mean number of charges was 3.26 [CI 3.09, 3.44]. The number of charges ranged from 1-11 (the maximum number of charges possible was 16). The most common number of charges were two (26%), three (20%), and four (15%).

[INSERT TABLE 3 APPROXIMATELY HERE]

A linear regression was conducted to assess whether other factors, such as jurisdiction and prosecutor characteristics, were important to the number of charges that prosecutors chose to file. See Table 3. In the first model, containing only the experimental conditions, regression results show that the black accountant experimental condition is different from the control condition with marginal statistical significance; that is, prosecutors recommend about one-half more charges for the defendant who is a black accountant ($p=.094$). When jurisdiction characteristics are added, and in the complete model, none of the experimental conditions are statistically different from the control.

C. Felony Charging

For additional robustness, we separately examined whether a prosecutor would charge the defendant with a felony versus a misdemeanor (or no charge at all), and whether this would differ by experimental condition. Only 16% of prosecutors opted to charge the defendant with aggravated assault (the clear felony available), whereas 84% opted for charges that were a misdemeanor only (or could be either a felony or a misdemeanor). In a simple bivariate test, there is no statistically significant relationship between the experimental conditions and the decision to charge a felony.

[INSERT TABLE 4 APPROXIMATELY HERE]

A binary logistic regression was conducted to assess whether other factors were important to the prosecutors' decision to charge a felony or not. See Table 4. Only one of the experimental conditions is (marginally) statistically different from the control condition: when the defendant was a white accountant, he was less likely to be charged with a felony (odds ratio=.45; $p=.067$) compared to the control condition. This finding could suggest a potential existence of class bias, in that white individuals of higher socioeconomic status received less harsh treatment than the individuals with unidentified race.

The next model added jurisdiction-specific variables such as the size of respondents' geographic region, office size, and jurisdiction size. The white accountant condition remains marginally statistically different from the control condition (odds ratio=.43; $p=.065$). Adding prosecutor characteristics, such as age, race, ethnicity, sex, length of job experience, and type of prosecutor, does not change the outcome (odds ratio=.40,

$p=.06$), and the black fast food condition also becomes marginally significant (odds ratio=.44, $p=.09$). The final model included information regarding whether the prosecutor recommended a monetary penalty and confinement, and the number of total charges. Regression results show the same marginal effects. We also see substitution effects: prosecutors who recommend a monetary penalty are less likely to charge a felony (odds ratio=.44, $p=.015$), and some complements, in that prosecutors who recommend confinement are more likely to charge a felony (odds ratio=2.52, $p=.005$). Unsurprisingly, prosecutors who recommend more charges are more likely to charge a felony (odds ratio=1.41, $p=.000$).

D. Monetary Penalties

For further robustness, the study separately investigated whether a prosecutor would recommend a monetary penalty, and if so, the dollar amount of the penalty, and whether this would differ by experimental condition. About 41% of prosecutors opted to recommend a monetary penalty, whereas 59% opted for no monetary penalty. The mean monetary penalty recommended, including all prosecutors who recommended no monetary penalty, was \$242.75 [CI \$191.90, \$293.60], or when excluding prosecutors who did not recommend a monetary penalty, the mean recommended amount was \$640.25 [CI \$530.77, \$749.73]. Of those prosecutors that recommended a monetary penalty, the amount recommended ranged from \$10 to \$5,000, and the most common recommended amount was \$500 (55/160 prosecutors recommended) with common recommendations of \$100, \$200, \$250, \$750, and \$1,000.

[INSERT TABLE 5 APPROXIMATELY HERE.]

A binary logistic regression was conducted to assess whether other factors were important to the prosecutors' decision to recommend *any* monetary penalty or not. See Table 5. Experimental conditions are not statistically different from the control condition in any model.

A linear regression was conducted to assess whether other factors were important to the dollar *amount* that prosecutors chose to recommend. See Table 6. Experimental conditions are not statistically different from the control condition in any model. We did find that more punitive prosecutors recommended more severe punishments, in that prosecutors who sought a term of confinement recommended about \$178 more in monetary penalties as well ($p=.006$).

E. Confinement

For robustness, we separately examined whether a prosecutor would recommend a term of confinement, and if so, the minimum days of confinement, and whether this would differ by experimental condition. About 27% of prosecutors recommended confinement, whereas 73% opted for no confinement. The mean recommended minimum days of confinement, including prosecutors who recommended no confinement, was just over 21 (21.40, CI [15.02, 27.79]), or when excluding prosecutors

who did not recommend confinement, 80.17 days [CI 59.51, 100.83].⁷⁶ Of those prosecutors that recommended confinement, the minimum number of days of confinement recommended ranged from two to 720 days, and the most common recommended amount was 30 days (31/122 prosecutors recommended) with common recommendations of 10, 90, and 180 days.

[INSERT TABLE 7 APPROXIMATELY HERE.]
[INSERT TABLE 8 APPROXIMATELY HERE.]

A binary logistic regression was conducted to assess whether other factors were important to the prosecutors' decision to recommend *any* confinement. See Table 7. The initial model contained only primary independent variables of interest, and the other models added in stages jurisdiction characteristics, prosecutor characteristics, and penalty recommendations, as described earlier. Regression results show that the experimental conditions are not statistically different from the control condition in any model.

A linear regression assessed whether other factors were important to the *minimum number* of days of confinement that prosecutors chose to recommend. See Table 8. Nothing was statistically significant in the basic model. In the complete model, which includes other penalty recommendations, with marginal significance, prosecutors deciding on penalties for defendants who were described as black fast food workers recommended almost 20 more days than for defendants in the control condition, without race and class information given ($p=.059$). Nonwhite prosecutors recommended about 27 fewer days of confinement than white prosecutors ($p=.016$), and Hispanic prosecutors recommended almost 57 *more* days of confinement than non-Hispanic prosecutors ($p=.000$). Finally, unsurprisingly, prosecutors who charged a felony recommended almost 71 more days of confinement ($p=.000$).

There were a few extreme values. For example, one prosecutor recommended 720 days of confinement, and eight prosecutors recommended 365 days of confinement. The 95% of the distribution was 180 days or less, and if that is used as a ceiling, then the none of the experimental conditions are different from the control in any of the models. Prosecutors may have been influenced by race in their likelihood of imposing a severe sentence greater than 180 days. A Fisher Exact Tests yielded $p = .064$ for difference between black fast food workers and white accountant defendant on this ad-hoc analysis. Readers should not make inferences of causality from such ad-hoc testing.

F. Pooled Analyses

We also used a factorial design to maximize the efficiency of our study. Because we generally found null effects in the analyses of experimental cells (race-class pairings, such as Black Accountant), we also pooled across the classes to test race effects with greater power, and pooled across the races to test class effects with greater power.

⁷⁶ If nine extreme values are recoded to a maximum value of 95% percentile of the distribution, or 180 days, when prosecutors recommend confinement, the mean minimum number of days recommended becomes 63.05 [CI 51.15, 74.95].

The overall severity outcomes are shown in Figure 1 and are indistinguishable from the null.

We also conducted pooled analyses for the individual outcomes (e.g., chance of confinement) but do not display regression tables. Instead Figures 2 and 3 show forest plots of the results, with odds ratios for binary outcomes and linear regression coefficients for scalar outcomes (and their 95% confidence intervals). None of the outcomes are distinguishable from the null, and as a whole do not suggest race or class effects.

[INSERT FIGURE 2 HERE]

[INSERT FIGURE 3 HERE]

IV. DISCUSSION

We review the limitations of our methodological approach, and remark on both the severity and variability we observed in charging identical cases. We also explore the relevance of our findings for policy.

A. Limitations

A randomized method has the advantage of allowing causal inference, and the population of real prosecutors reduces the inferential gap. Vignette-based research has become a staple of social science, as it has been shown to be a valid method for predicting real-world behavior.⁷⁷ However, there are important methodological limitations.

Although practicing prosecutors were consulted in order to design realistic vignettes, any particular fact pattern may not be generalizable to the wide range of real-world situations that prosecutors face. This case represented a fairly minor crime, unlike many drug crimes or violent crimes, which can lead to greater penalties. This lack of severity may have also created a floor, with 73% of responding prosecutors opting for no confinement and 59% opting for no monetary penalty. A more severe crime may have revealed more variability among the prosecutors in our sample.

Furthermore, the study's manipulations may not have been perfectly calibrated to the actual real-world salience of race and class. For example, defendant mugshots and racialized names could have created stronger impressions of race. Defendants with high socio-economic status may be able to communicate this fact to the prosecutor in other ways, including through choice of defense counsel and communications by defense counsel. Finally, although this Article sometimes refers to "class" as shorthand, it bears emphasis that the defendant's job is as an imperfect proxy of socioeconomic status.

⁷⁷ JENS HAINMUELLER ET AL., VALIDATING VIGNETTE AND CONJOINT SURVEY EXPERIMENTS AGAINST REAL-WORLD BEHAVIOR 2395–400 (2015); Lisa Wallander, *25 Years of Factorial Surveys in Sociology: A Review*, 38 SOC. SCI. RES. 505, (2009).

By typical experimental research standards this was a relatively large study, and we used a fully-crossed factorial design to maximize potential power (with about 180 respondents assigned to each race and each class). Although our primary analyses broke out each race-class pairing (e.g., black accountant), we also performed regressions entering race and class as dummy variables, which effectively pooled across experimental conditions, exploiting the factorial design. Even here, however, we found no significant effects. Our confidence intervals allow that there may nonetheless be small effects, but there was no consistent trend of insignificant findings of race or class bias, and in some cases the signs were contrary to the hypothesized effect (see e.g., Table 2 Model 3, showing both Black defendants receiving less severe punishment than the White defendants).

It is worth highlighting that our primary regression models used the control condition as the base case (i.e., omitted category), and thus the regression coefficients estimate differences against that counterfactual where class and race are occluded. These coefficients do not directly estimate whether there is disparate treatment of black versus white or high class versus low class individuals. Our alternative specifications (not shown), using white/black contrasts did not yield substantially different results.

Given the generally null results, a mathematical adjustment for multiple testing was not applied.⁷⁸ Nonetheless, readers should be aware that multiple dependent variables and multiple models increases the risk of false positives. Indeed, Tables 2-8 show 28 different specifications testing the race variable, and we found only one instance at the $p=0.05$ level in which whites may have received leniency. We would expect this rate out of mere chance. (Similarly, when we pool the race variable across both class levels, and run the 28 specifications (not shown), we find nothing more than the null hypothesis would predict.)

Additionally, although participants were recruited from a nationwide pool of prosecutors, and were incentivized to participate, this study is not a probability-based sample of American prosecutors. It is particularly worrisome that we had a relatively low response rate of 12.9%. Nonetheless, unlike a simple survey, our experimental hypotheses tests depend on intra-sample comparisons with random-assignment across conditions.

Prosecutors also may have behaved differently in this experimental setting than they do in the real world. Knowing that their responses were being observed, prosecutors may have attempted to give more socially-desirable answers than they otherwise would.⁷⁹ The blinded, randomized, between-subjects design reduces the ability of individuals to

⁷⁸ See Aickin, Mikel & Helen Gensler, *Adjusting for multiple testing when reporting research results: the Bonferroni vs Holm methods*. 86 AM. J. PUB HEALTH 726 (1996).

⁷⁹ See Norbert Schwarz, *Self-Reports: How the Questions Shape the Answers*. 54 AM. PSYCHOL. 93 (1999). See also Ebbe B. Ebbesen and Vladimir J. Konecni, *Decision Making and Information Integration in the Courts: The Setting of Bail*, 32 J. PERSONALITY & SOCIAL PSYCH. 805 (1975) (showing that judges in fact behaved differently than they said they would in survey responses).

provide socially-desirable answers.⁸⁰ Implicit bias generally cannot be switched on and off, or overcome by simply being observed.⁸¹

To be sure, the prosecutors were not simply asked whether they would be more punitive towards blacks. Instead, individuals were asked to decide the specific case presented, which had innumerable variables in its rich vignette with open-ended responses, making it far from obvious to determine what would be the socially desirable response. Even if prosecutors did try to be more lenient on black defendants, to counteract their implicit bias, it would be difficult to achieve the same level of severity imposed by other prosecutors in the white-defendant conditions. If this heuristic is successful, perhaps prosecutors deploy it just as successfully in their real world decisions? Notwithstanding the anonymity of our experiment, perhaps the fact of our observation is more salient here than in the real world. If monitoring of prosecutors improves behavior, this suggests that real-world monitoring interventions may be worthwhile.⁸²

Prosecutors may have discussed the study with other potential respondents in their offices or otherwise, which may have broken our blind. The data indicated evidence of respondents clicking through the survey very quickly as if seeking to preview it, which is typical of online research. Our between-subjects randomized design helps minimize the risk that any individual could determine what factors were manipulated, and we did not reveal our purpose at the end of the study. Our national research population and direct recruitment strategy (rather than, say using an email list serve or a snowball sample) reduces the risk of cross-talk.

Finally, the study cannot unequivocally determine whether respondents failed to notice the race and class of the defendants, or they noticed but did not respond to these factors. In a laboratory setting, prosecutors would have been asked manipulation-check questions to assess their ability to recall the race and class of the defendant in each vignette. This strategy was not used here, due to concerns that doing so would break the blind, and cause respondents to communicate with future respondents about the purpose of the study.

B. Implications

As we have seen, prosecutors have broad discretion and little direction, and they in fact react in highly-variable ways to the same case. This is a domain where one might expect bias to flourish. As L. Song Richardson has recently pointed out, “implicit biases flourish in situations where individuals make decisions quickly and on the basis of limited information.”⁸³

⁸⁰ See Hainmueller et al., *supra* note 77; Wallander *supra* note 77.

⁸¹ See e.g., Justin Levinson, Cai Huajian, & Danielle Young, *Guilty by implicit racial bias: The guilty/not guilty Implicit Association Test*, 8 OHIO ST. J. CRIM. L. 187 (2010) (finding race biases using the implicit attitudes test, and correlating them with behavior).

⁸² See Tanya Kateri Hernandez, *Bias Crimes: Unconscious Racism in the Prosecution of "Racially Motivated Violence"*, 99 YALE LAW J. 845, 855 (1990) (calling for monitoring of prosecutors to reduce unconscious bias).

⁸³ L. Song Richardson, *Systemic Triage: Implicit Racial Bias in the Criminal Courtroom*, 126 YALE L.J. 862, 866 (2016). See also Justin D. Levinson & Danielle Young, *Different shades of bias: Skin tone, implicit*

Oddly enough, however, our data and analyses do not support the proposition that implicit bias arising from prosecutors' awareness of race is the primary driver of the indisputable racial disparities that exist in criminal justice outcomes. Our novel experimental design, which separately manipulated race and class, had the potential to shed light on the interaction of these factors. Yet, this study's null result on the class variable further suggests that underlying economic disparities, which have a strong association with race, are not driving prosecutorial decisions in a simplistic way. Instead, systematic and institutionalized racism and classism may be playing a larger role.

That said, race and class are not simple phenomena. Some have recently challenged whether race can even be studied in the counterfactual model of the randomized experiment and the social science methods that seek to approach that ideal.⁸⁴ Our experiment only tests the prosecutor's simulated behavior after being exposed to certain words in the police reports.

We extend prior work demonstrating why race scholars must dig deep into the mechanisms driving racial disparities. McIntyre and Baradaran demonstrated in the context of pretrial detention that with a naïve regression analysis, one would conclude significant racial discrimination.⁸⁵ However, once one adequately accounts for prior history and forecasts recidivism, race has very little explanatory effect. Similarly, our work shows that while there are observable racial differences in charging between races, the race effects are small to nonexistent when examined closely through a randomized experiment.

We also remind readers that we did find marginally significant race/class effects in some conditions and some specifications, and our statistical power is unable to rule out smaller effects across the board. We only conclude that our research does not support the proposition that implicit race and class biases are a large driver of prosecutorial charging decisions in the sort of case we studied.

Since we cannot rule out the possibility that awareness of race and class have modest effects on charging decisions, there remains a risk of race-biased decisions. From a

racial bias, and judgments of ambiguous evidence, 112 W. VA. L. REV. 307 (2010); Charles Lawrence, *The Id, The Ego, and Equal Protection: Reckoning with Unconscious Racism*, 39 STAN. L. REV. 317, 341 (1987); Sheri Johnson, *Unconscious Racism and the Criminal Law*, 73 CORNELL L. REV. 1016, 1019 (1988).

⁸⁴ Issa Kohler-Hausmann, *Eddie Murphy and the Dangers of Counterfactual Causal Thinking About Detecting Racial Discrimination* (Feb. 1, 2018) (unpublished), available at <http://dx.doi.org/10.2139/ssrn.3050650> (arguing that the counterfactual model is “wrong because it is based on a flawed theory of (1) what the word race references and how it produces effects in the world and (2) what we mean when we say it is bad to make important decisions ‘because of race.’”). See also Anthony V. Alfieri, *Race Prosecutors, Race Defenders*, 98 GEO. LAW. J. 2227, 2229 (2000) (“The actions of prosecutors and defense lawyers reflect and refashion cultural artifacts (caste and color) and social norms (character and community). Acting as sociolegal agents, prosecutors and defenders infuse legal discourse with images and tropes gleaned outside the law, inscribing cultural and social meaning into law.”)

⁸⁵ Frank McIntyre and Shima Baradaran, *Race, Prediction & Pretrial Detention*, 10 J. EMP. LEG. STUD. 741 (2013)

Bayesian perspective, that risk is buttressed by a wealth of background research showing racial bias in a range of situations. Thus, notwithstanding the null results, it may still be worthwhile to remove race information from the reports provided to prosecutors, as some have proposed and one district is now trying.⁸⁶ An individual cannot be biased by information that she did not receive. Blinding is valuable both for the sake of preventing bias, but also to maintain trust, confidence, and legitimacy.

Our study's control condition can be viewed as a pilot that proves one part of the feasibility of redacting race and class information from police reports. The study included race in a long list of information that prosecutors could consider when making charging decisions, and asked respondents to mark all of the factors they needed. The vast majority did not include race as a necessary factor, a finding that supports the proposition that its inclusion in police dossiers is gratuitous, at least for the prosecutor's function (although some prosecutors noted that it was important in determining whether to charge a hate crime). To the extent that information technology allows the data to be collected for police identification purposes but redacted for prosecutorial decision making, this study supports the feasibility of such a reform. Additional research is needed to explore whether blinding of prosecutors' decisions may improve perceptions that prosecutors decide fairly.

However, the data includes suggestive trends that prosecutors were more severe in the blinded control condition (i.e., less severe in most of the other conditions), when race and class were occluded. See e.g., Table 2. Notably, the study only had one control condition and it occluded *both* variables, so it is impossible to distinguish which occlusion is driving this potential affect. Furthermore, it is possible that being reminded that the defendant has a job, regardless of whether the particular job has markings of class and privilege, tends to humanize the defendant and mitigate a prosecutor's tendency to pursue jail-time and the felony charges that go with it.

Indeed, for a person who does have a job, incarceration can cause additional collateral consequences compared to those who don't have a job, both in terms of marginal lost hours of work but also given the risk of being fired altogether.⁸⁷ Thus it may seem rational for prosecutors to consider this information, arguably unlike race information. Further research might explore whether prosecutors and police officers should purposefully include such job information to reduce incarceration rates. Doing so may however exacerbate disparities, where the underlying unemployment rate suffers from a racial disparity too.

CONCLUSION

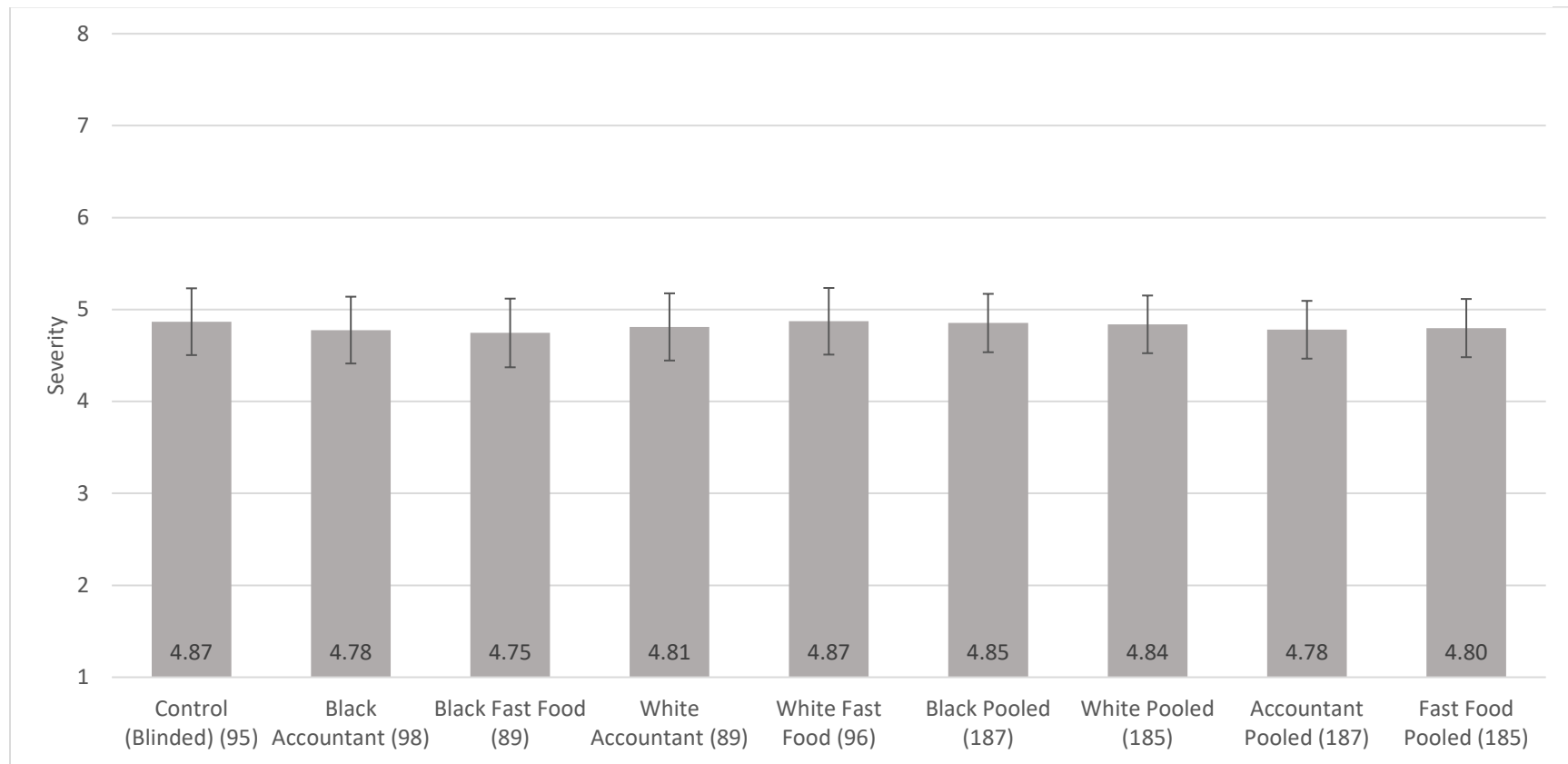
We know that there are huge racial and socioeconomic disparities in criminal justice outcomes, but our research does not suggest that they are primarily due to implicit biases

⁸⁶ See notes 4 & 5 *supra*.

⁸⁷ See Margaret E. Finzen, *Systems of Oppression: The Collateral Consequences of Incarceration and Their Effects on Black Communities*, 12 GEO. J. ON POVERTY L. & POL'Y 299, 317 (2005); John Hagan & Dinovitzer Ronit, *Collateral Consequences of Imprisonment for Children, Communities, and Prisoners*, 26 CRIME & JUST. 121, 137 (1999); ; Will Dobbie et al., *The Effects of Pre-Trial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges*, 108 AM. ECON. REV. 201 (2018)

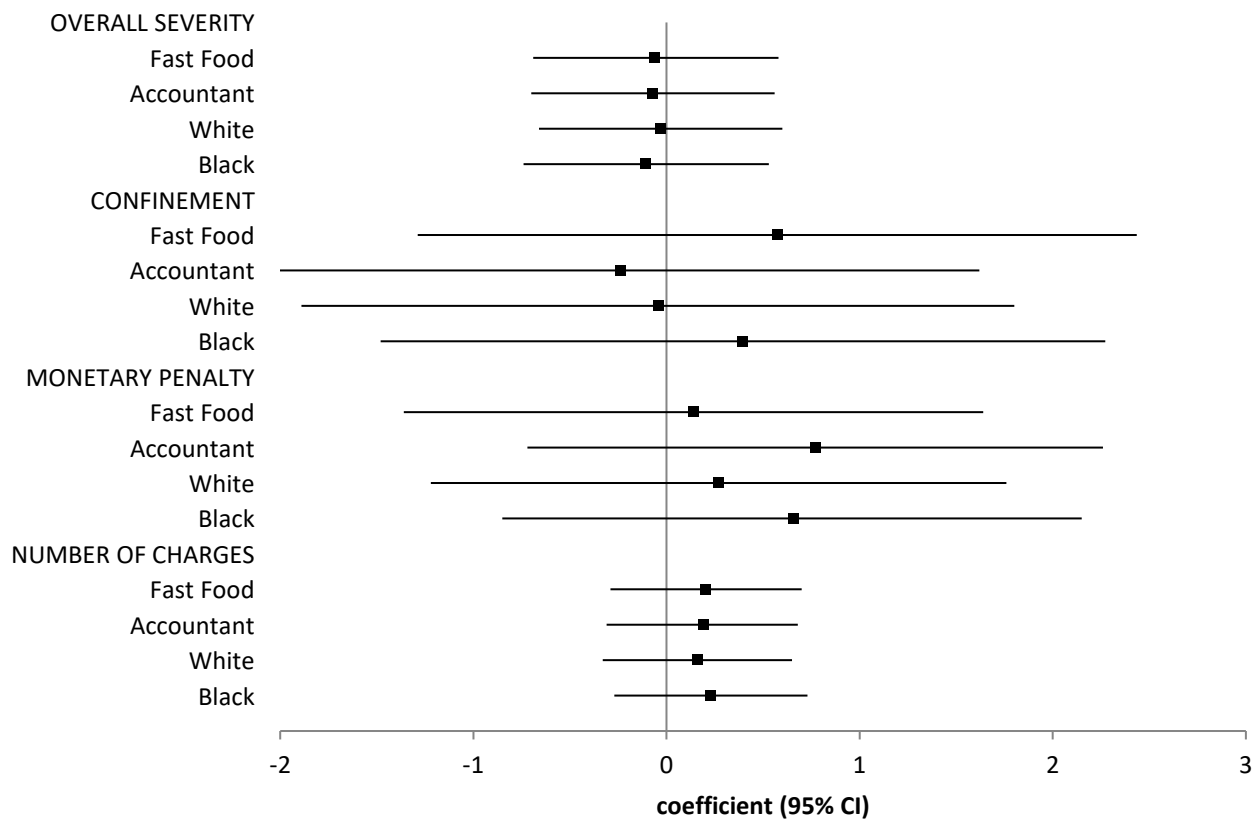
by prosecutors. We have determined that severe and variable prosecutorial decisions occur with minimal oversight or institutional guidance. Policymakers must consider reforms of the criminal justice process and more fundamental and structural solutions to address systemic and institutionalized racism and classism.

Figure 1 – Severity of Prosecutors’ Charging and Punishment Intentions by Experimental Conditions and Factorial Race/Class Variables Pooled (with sample size shown)



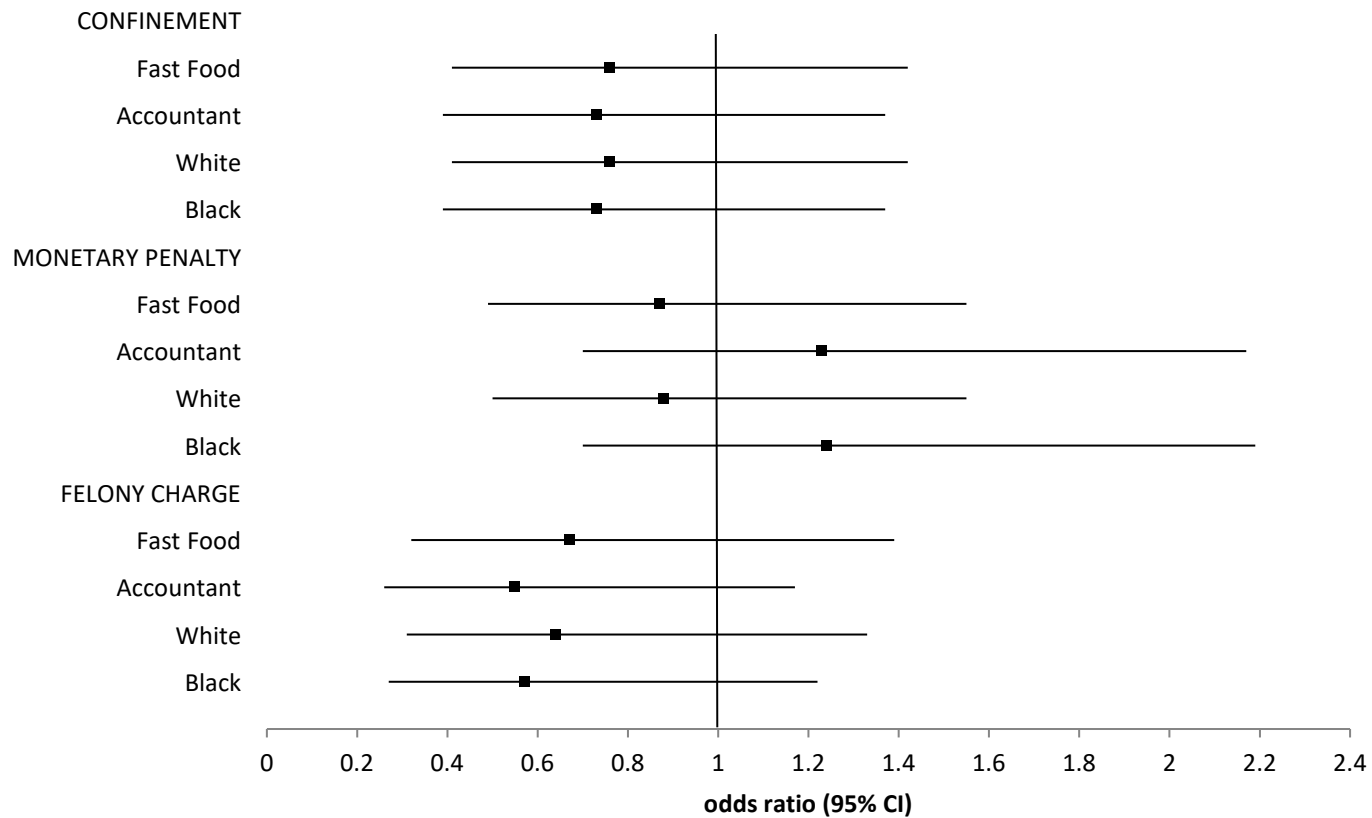
Note: For each experimental condition, the predicted value is shown on an 8-level outcome variable, which indexes whether charges were brought, whether deferred, whether monetary penalty sought and dollar amount thereof (in tertiles), and whether confinement sought and time thereof (in tertiles). Predicted values adjusted for jurisdiction characteristics and prosecutor characteristics as shown in Model 3 below. 95% confidence intervals shown.

Figure 2 – Forest Plot Summarizing Pooled Analyses of Race and Class Scalar Outcomes



Note: Coefficients are based on regression models including prosecutor and jurisdiction characteristics (as in Model 3 shown below). To depict on common scale, confinement is displayed as 10-day units and monetary penalties are displayed as hundreds of dollars.

Figure 3 – Forest Plot Summarizing Pooled Analyses on Race and Class Binary Outcomes



Note: Odds ratios are based on regression models including prosecutor and jurisdiction characteristics (as in Model 3 shown below).

Table 1 -- Descriptive Statistics

	Percent of Sample or Mean
Recommended Disposition of Case	
Felony Charge	15.85%
Monetary Penalty	41.50%
Average Amount of Monetary Penalty	\$242.75
Confinement	27.05%
Average Minimum Days of Confinement	21.40 days
Jurisdiction Characteristics	
Average Size of Office	31.60 prosecutors
Size of Jurisdiction	
Over 2,000,000 people	7.54%
1,000,000-2,000,000 people	10.56%
500,000-1,000,000 people	10.13%
100,000-500,000 people	28.23%
Less than 100,000 people	43.53%
Region	
New England	4.30%
Middle Atlantic	3.44%
Midwest	21.08%
West North Central	11.18%
South Atlantic	15.70%
East South Central	8.60%
West South Central	1.08%
Mountain	22.80%
Pacific	11.83%
Prosecutor Characteristics	
Average Number of Years as Prosecutor	12.70 years
Head Prosecutor	22.96%
Average Age	46.18 years
Gender	
Male	65.52%
Female	34.48%
Race	
White	90.41%
Black/African American	4.14%
American Indian/Alaska Native	.65%
Asian	.65%
Native Hawaiian/Pacific Islander	.22%
Other	3.92%
Hispanic	
No	96.09%
Mexican/Mexican American/Chicano	1.30%
Puerto Rican	.22%
Cuban	1.09%
Other Spanish/Hispanic/Latino	1.30%

Table 2 – Severity of Punishment Regression, with Blinded Condition as Reference Category

	Model 1: Experimental Conditions	Model 2: Experimental Conditions and Jurisdiction Characteristics	Model 3: Experimental Conditions, Jurisdiction Characteristics, and Prosecutor Characteristics	Model 4: Experimental Conditions, Jurisdiction Characteristics, Prosecutor Characteristics, and Penalty Recommendations
	Coeff [CI]	Coeff [CI]	Coeff [CI]	Coeff [CI]
Experimental Conditions				
Black Accountant	-.01 [-.71, .70]	-.08 [-.80, .63]	-.09 [-.82, .64]	-.08 [-.78, .62]
Black Fast food	-.04 [-.76, .69]	-.11 [-.84, .62]	-.12 [-.87, .63]	-.02 [-.74, .70]
White Accountant	.01 [-.71, .73]	-.05 [-.77, .67]	-.06 [-.79, .68]	.02 [-.68, .73]
White Fast food	.21 [-.50, .92]	.15 [-.56, .86]	.00 [-.72, .73]	-.02 [-.71, .68]
Jurisdiction Characteristics				
Size of Office		.00 [-.01, .01]	.00 [-.01, .01]	.00 [-.00, .01]
Size of Jurisdiction		.14 [-.13, .42]	.13 [-.17, .42]	.17 [-.11, .45]
New England		-1.69 [-2.84, -.53] **	-1.53 [-2.72, -.34] *	-1.28 [-2.42, -.13] *
Middle Atlantic		.17 [-1.19, 1.54]	.48 [-.89, 1.85]	.24 [-1.09, 1.57]
Midwest		-.80 [-1.52, -.08] *	-.59 [-1.34, .16]	-.28 [-1.01, .44]
West North Central		-.21 [-1.08, .66]	-.09 [-1.02, .83]	-.07 [-.96, .82]
South Atlantic		.11 [-.61, .83]	.23 [-.53, 1.00]	-.05 [-.78, .69]
East South Central		-.53 [-1.43, .37]	-.28 [-1.19, .63]	-.11 [-.98, .77]
West South Central		-1.39 [-3.74, .97]	-1.24 [-3.60, 1.12]	-1.00 [-3.26, 1.26]
Pacific		.27 [-.58, 1.12]	.64 [-.26, 1.54]	.67 [-.19, 1.53]
Prosecutor Characteristics				
Time as Prosecutor			.02 [-.02, .05]	.03 [-.01, .06]
Felony Division			-.67 [-1.27, -.07] *	-.50 [-1.08, .08] +
Head Prosecutor			-.28 [-.95, .39]	-.14 [-.78, .51]
Age			-.03 [-.06, .01]	-.03 [-.06, .00] *
Female			-.01 [-.51, .49]	-.06 [-.54, .42]
Not White			-.35 [-1.17, .46]	-.42 [-1.20, .36]
Hispanic			.72 [-.45, 1.88]	.65 [-.47, 1.76]
Penalty Recommendations				
Felony Charge				1.20 [.55, 1.85] ***
Number of Charges				.24 [.12, .37] ***
Constant	3.51 [3.01, 4.01] ***	3.24 [1.92, 4.55] ***	4.87 [2.54, 7.19] ***	3.50 [1.23, 5.77] **
R - squared	.0015	.0464	.0783	.1600
N=	411	408	389	389

Note: +=statistical significance at .10 level, *=statistical significance at .05 level, **=statistical significance at or below .01 level, ***=statistical significance at or below .001 level

Table 3 – Total Number of Charges Regression, with Blinded Condition as Reference Category

	Model 1: Experimental Conditions		Model 2: Experimental Conditions and Jurisdiction Characteristics		Model 3: Experimental Conditions, Jurisdiction Characteristics, and Prosecutor Characteristics		Model 4: Experimental Conditions, Jurisdiction Characteristics, Prosecutor Characteristics, and Penalty Recommendations	
	Coeff [CI]		Coeff [CI]		Coeff [CI]		Coeff [CI]	
Experimental Conditions								
Black Accountant	.47 [-.08, 1.03]	+	.34 [-.20, .89]		.32 [-.25, .88]		.39 [-.16, .94]	
Black Fast food	.32 [-.25, .89]		.12 [-.44, .68]		.13 [-.45, .71]		.21 [-.36, .78]	
White Accountant	.16 [-.41, .73]		.08 [-.47, .64]		.04 [-.53, .62]		.33 [-.23, .89]	
White Fast food	.40 [-.16, .95]		.31 [-.23, .85]		.27 [-.29, .83]		.38 [-.17, .93]	
Jurisdiction Characteristics								
Size of Office			-.00 [-.01, .00]		-.00 [-.01, .00]		-.00 [-.01, .00]	
Size of Jurisdiction			-.00 [-.22, .22]		-.10 [-.33, .13]		-.05 [-.28, .17]	
New England			-.21 [-1.14, .72]		-.16 [-1.13, .81]		.25 [-.68, 1.18]	
Middle Atlantic			1.33 [.32, 2.33]	**	1.4 [.37, 2.43]	**	1.64 [.63, 2.64]	***
Midwest			-.62 [-1.16, -.08]	*	-.61 [1.18, -.03]	*	-.63 [-1.19, -.07]	*
West North Central			.26 [-.39, .92]		.19 [-.52, .90]		-.32 [-.38, 1.01]	
South Atlantic			.70 [.14, 1.27]	*	.82 [.20, 1.43]	**	.76 [.18, 1.35]	*
East South Central			-.17 [-.88, .54]		-.15 [-.88, .58]		.00 [-.69, .70]	
West South Central			.04 [-1.67, 1.76]		.05 [-1.71, 1.81]		.15 [-1.53, 1.82]	
Pacific			.04 [-.61, .68]		.18 [-.52, .88]		.11 [-.57, .78]	
Prosecutor Characteristics								
Time as Prosecutor					-.03 [-.06, -.002]	*	-.03 [-.06, -.00]	*
Felony Division					-.35 [-.81, .11]		-.10 [-.55, .36]	
Head Prosecutor					-.49 [-1.00, .03]	+	-.45 [-.95, .06]	+
Age					.02 [-.00, .05]	+	.02 [-.00, .05]	+
Female					.27 [-.13, .66]		.27 [-.11, .65]	
Not White					.39 [-.26, 1.04]		.37 [-.25, 1.00]	
Hispanic					.39 [-.54, 1.33]		.43 [-.46, 1.32]	
Penalty Recommendations								
Confinement							.50 [.08, .91]	*
Felony Charge							1.17 [.66, 1.69]	***
Monetary Penalty							.24 [-.13, .62]	
Constant	2.88 [2.49, 3.28]	***	3.04 [2.00, 4.08]	***	3.32 [1.47, 5.17]	***	2.40 [.59, 4.21]	**
R - squared	.0077		.0722		.1128		.2045	
N=	467		461		434		411	

Note: +=statistical significance at .10 level, *=statistical significance at .05 level, **=statistical significance at or below .01 level, ***=statistical significance at or below .001 level

Table 4 -- Felony Charge (Yes/No) Regression, with Blinded Condition as Reference Category

	Model 1: Experimental Conditions		Model 2: Experimental Conditions and Jurisdiction Characteristics		Model 3: Experimental Conditions, Jurisdiction Characteristics, and Prosecutor Characteristics		Model 4: Experimental Conditions, Jurisdiction Characteristics, Prosecutor Characteristics, and Penalty Recommendations	
	OR [CI]		OR [CI]		OR [CI]		OR [CI]	
Experimental Conditions								
Black Accountant	.90 [.44, 1.84]		.79 [.36, 1.74]		.71 [.31, 1.67]		.66 [.26, 1.64]	
Black Fast food	.51 [.22, 1.16]		.51 [.21, 1.21]		.44 [.17, 1.14]	+	.36 [.13, 1.03]	+
White Accountant	.45 [.19, 1.06]	+	.43 [.18, 1.05]	+	.40 [.15, 1.04]	+	.37 [.13, 1.03]	+
White Fast food	.92 [.45, 1.89]		.94 [.44, 2.01]		.90 [.40, 2.01]		.81 [.34, 1.94]	
Jurisdiction Characteristics								
Size of Office			1.00 [.99, 1.01]		1.00 [.99, 1.01]		1.00 [.99, 1.01]	
Size of Jurisdiction			.85 [.64, 1.13]		.87 [.63, 1.20]		.92 [.64, 1.32]	
New England			omitted		omitted		omitted	
Middle Atlantic			.27 [.03, 2.24]		.36 [.04, 3.11]		.10 [.01, 1.30]	+
Midwest			.30 [.12, .76]	*	.39 [.15, 1.03]	+	.48 [.16, 1.42]	
West North Central			.56 [.20, 1.54]		.61 [.20, 1.91]		.51 [.14, 1.78]	
South Atlantic			1.70 [.85, 3.42]		1.48 [.67, 3.28]		1.12 [.48, 2.64]	
East South Central			.31 [.08, 1.12]	+	.36 [.09, 1.35]		.43 [.11, 1.70]	
West South Central			omitted		omitted		omitted	
Pacific			.75 [.30, 1.87]		.80 [.29, 2.18]		.65 [.22, 1.94]	
Prosecutor Characteristics								
Time as Prosecutor					.99 [.94, 1.04]		1.00 [.95, 1.06]	
Felony Division					.43 [.17, 1.06]	+	.47 [.18, 1.26]	
Head Prosecutor					.74 [.31, 1.80]		1.06 [.38, 2.94]	
Age					.99 [.95, 1.03]		.97 [.93, 1.01]	
Female					1.02 [.55, 1.88]		.84 [.43, 1.62]	
Not White					1.11 [.43, 2.86]		1.16 [.42, 3.24]	
Hispanic					.75 [.19, 2.92]		.42 [.10, 1.78]	
Penalty Recommendations								
Monetary Penalty							0.44 [.23, .85]	**
Confinement							2.52 [1.31, 4.82]	*
Number of Charges							1.41 [1.20, 1.65]	***
Constant	.25 [.15, .41]	***	.72 [.19, 2.72]		2.30 [.12, 42.57]		.86 [.03, 23.68]	
Pseudo R - squared	.0152		.0818		.0874		.1863	
N=	467		437		410		387	

Note: +=statistical significance at .10 level, *=statistical significance at .05 level, **=statistical significance at or below .01 level, ***=statistical significance at or below .001 level

Table 5 -- Monetary Penalty Recommendation (Yes/No) Regression, with Blinded Condition as Reference Category

	Model 1: Experimental Conditions	Model 2: Experimental Conditions and Jurisdiction Characteristics	Model 3: Experimental Conditions, Jurisdiction Characteristics, and Prosecutor Characteristics	Model 4: Experimental Conditions, Jurisdiction Characteristics, Prosecutor Characteristics, and Penalty Recommendations
	OR [CI]	OR [CI]	OR [CI]	OR [CI]
Experimental Conditions				
Black Accountant	1.37 [.77, 2.48]	1.48 [.80, 2.75]	1.35 [.71, 2.57]	1.45 [.75, 2.82]
Black Fast food	1.39 [.76, 2.53]	1.33 [.71, 2.52]	1.13 [.58, 2.21]	1.15 [.58, 2.32]
White Accountant	1.20 [.66, 2.20]	1.19 [.63, 2.23]	1.12 [.58, 2.18]	1.15 [.58, 2.28]
White Fast food	.81 [.44, 1.50]	* .78 [.42, 1.46]	.70 [.36, 1.35]	.74 [.37, 1.45]
Jurisdiction Characteristics				
Size of Office		1.00 [.99, 1.00]	1.00 [.99, 1.01]	1.00 [.99, 1.00]
Size of Jurisdiction		1.12 [.88, 1.44]	1.18 [.90, 1.54]	1.13 [.85, 1.50]
New England		.07 [.01, .54]	* .06 [.01, .50]	** .06 [.01, .50]
Middle Atlantic		.58 [.18, 1.84]	.57 [.17, 1.89]	.41 [.12, 1.44]
Midwest		.86 [.47, 1.58]	.81 [.42, 1.55]	.83 [.42, 1.64]
West North Central		.70 [.33, 1.46]	.62 [.28, 1.40]	.52 [.23, 1.21]
South Atlantic		1.22 [.66, 2.27]	1.14 [.58, 2.23]	1.15 [.57, 2.31]
East South Central		1.29 [.60, 2.77]	1.26 [.56, 2.80]	1.31 [.57, 2.97]
West South Central		omitted	omitted	omitted
Pacific		.99 [.48, 2.04]	.90 [.41, 1.97]	.79 [.35, 1.79]
Prosecutor Characteristics				
Time as Prosecutor			1.01 [.98, 1.05]	1.01 [.98, 1.05]
Felony Division			0.59 [.34, 1.02]	+.59 [.33, 1.05]
Head Prosecutor			.95 [.52, 1.73]	1.01 [.55, 1.87]
Age			.99 [.96, 1.02]	.99 [.96, 1.02]
Female			.75 [.47, 1.17]	.71 [.44, 1.13]
Not White			1.91 [.91, 4.00]	+.203 [.96, 4.32]
Hispanic			.33 [.10, 1.14]	+.27 [.08, .94]
Penalty Recommendations				
Confinement				1.89 [1.15, 3.09]
Felony Charge				.44 [.23, .85]
Number of Charges				1.09 [.96, 1.23]
Constant	.63 [.41, .95]	* .47 [.14, 1.53]	1.01 [.12, 8.37]	.95 [.10, 8.69]
Pseudo R - squared	.0073	.0440	.0640	.0896
N=	441	432	409	406

Note: +=statistical significance at .10 level, *=statistical significance at .05 level, **=statistical significance at or below .01 level, ***=statistical significance at or below .001 level

Table 6 -- Monetary Penalty Recommendation (Amount) Regression, with Blinded Condition as Reference Category

	Model 1: Experimental Conditions	Model 2: Experimental Conditions and Jurisdiction Characteristics	Model 3: Experimental Conditions, Jurisdiction Characteristics, and Prosecutor Characteristics	Model 4: Experimental Conditions, Jurisdiction Characteristics, Prosecutor Characteristics, and Penalty Recommendations
	Coeff [CI]	Coeff [CI]	Coeff [CI]	Coeff [CI]
Experimental Conditions				
Black Accountant	34.62 [-124.53, 193.77]	60.47 [-101.11, 222.06]	41.50 [-128.77, 211.77]	30.37 [-139.29, 200.02]
Black Fast food	87.07 [-75.41, 249.54]	93.89 [-71.53, 259.31]	91.84 [-83.39, 267.06]	83.16 [-91.07, 257.38]
White Accountant	100.61 [-60.86, 262.09]	107.35 [-55.21, 269.90]	116.47 [-56.10, 289.05]	108.30 [-63.42, 280.02]
White Fast food	-57.93 [-217.98, 102.13]	-48.44 [-209.13, 112.24]	-57.56 [-228.30, 113.19]	-62.49 [-231.72, 106.74]
Jurisdiction Characteristics				
Size of Office		-.85 [-2.56, .86]	-.65 [-2.46, 1.16]	-.64 [-2.44, 1.16]
Size of Jurisdiction		1.99 [-60.83, 64.80]	6.92 [-61.77, 75.61]	-.16 [-68.52, 68.20]
New England		-343.46 [-605.62, -81.30] **	-280.52 [-561.29, -.26] *	-248.76 [-529.14, 31.62] +
Middle Atlantic		-262.82 [-554.42, 28.78] +	-246.68 [-552.00, 58.63]	-305.05 [-612.55, 2.46] +
Midwest		-161.84 [-324.19, .51] +	-137.21 [-312.49, 38.07]	-115.47 [-291.45, 60.52]
West North Central		-244.60 [-440.16, -49.04] *	-212.44 [-428.90, 4.02] +	-230.95 [-446.43, -15.47] *
South Atlantic		-17.38 [-180.78, 146.02]	4.04 [-174.97, 183.05]	-.39 [-179.82, 179.04]
East South Central		-142.51 [-345.28, 60.26]	-145.45 [-358.17, 67.26]	-121.86 [-334.01, 90.30]
West South Central		91.68 [-391.00, 574.36]	97.62 [-407.88, 603.13]	139.50 [-362.51, 641.50]
Pacific		-64.80 [-258.57, 128.98]	-36.82 [-248.54, 174.90]	-54.09 [-264.93, 156.75]
Prosecutor Characteristics				
Time as Prosecutor			-6.98 [-16.03, 2.07]	-6.43 [-15.47, 2.60]
Felony Division			12.07 [-128.61, 152.75]	37.27 [-103.37, 177.92]
Head Prosecutor			34.27 [-122.48, 191.02]	46.46 [-109.57, 202.50]
Age			1.30 [-6.22, 8.82]	.94 [-6.56, 8.44]
Female			-36.60 [-152.85, 79.65]	-43.37 [-158.81, 72.08]
Not White			2.90 [-188.53, 194.34]	4.05 [-185.98, 194.09]
Hispanic			-132.76 [-407.80, 142.28]	-168.04 [-441.27, 105.20]
Penalty Recommendations				
Confinement				178.06 [50.99, 305.13] **
Felony Charge				-81.45 [-240.97, 78.06]
Number of Charges				19.94 [-10.42, 50.30]
Constant	210.83 [97.00, 324.67] ***	328.11 [29.45, 626.76] *	316.82 [-231.12, 864.76]	243.15 [-310.45, 796.76]
R - squared	.0119	.0492	.0651	.0894
N=	422	418	398	397

Note: +=statistical significance at .10 level, *=statistical significance at .05 level, **=statistical significance at or below .01 level, ***=statistical significance at or below .001 level

Table 7-- Confinement Recommendation (Yes/No) Regression, with Blinded Condition as Reference Category

	Model 1: Experimental Conditions	Model 2: Experimental Conditions and Jurisdiction Characteristics	Model 3: Experimental Conditions, Jurisdiction Characteristics, and Prosecutor Characteristics	Model 4: Experimental Conditions, Jurisdiction Characteristics, Prosecutor Characteristics, and Penalty Recommendations
	OR [CI]	OR [CI]	OR [CI]	OR [CI]
Experimental Conditions				
Black Accountant	.74 [.38, 1.43]	.66 [.33, 1.31]	.68 [.33, 1.40]	.63 [.29, 1.37]
Black Fast food	.94 [.49, 1.80]	.84 [.43, 1.67]	.79 [.38, 1.65]	.87 [.40, 1.88]
White Accountant	.97 [.50, 1.87]	.84 [.43, 1.66]	.79 [.39, 1.62]	.83 [.39, 1.78]
White Fast food	.94 [.50, 1.79]	.88 [.45, 1.70]	.74 [.36, 1.50]	.83 [.39, 1.75]
Jurisdiction Characteristics				
Size of Office		1.10 [1.00, 1.01]	1.00 [.99, 1.01]	1.00 [.99, 1.01]
Size of Jurisdiction		1.19 [.91, 1.57]	1.15 [.85, 1.56]	1.15 [.84, 1.59]
New England		.12 [.02, .96]	.15 [.02, 1.25]	.22 [.03, 1.85]
Middle Atlantic		1.30 [.43, 3.94]	1.94 [.60, 6.26]	2.05 [.59, 7.16]
Midwest		.65 [.34, 1.24]	.84 [.41, 1.72]	.97 [.45, 2.10]
West North Central		1.32 [.63, 2.80]	1.60 [.69, 3.71]	1.54 [.63, 3.77]
South Atlantic		.81 [.41, 1.59]	.99 [.47, 2.10]	.75 [.34, 1.66]
East South Central		.36 [.13, .96]	.45 [.16, 1.26]	.49 [.17, 1.38]
West South Central		omitted	omitted	omitted
Pacific		1.04 [.49, 2.19]	1.56 [.69, 3.53]	1.74 [.75, 4.07]
Prosecutor Characteristics				
Time as Prosecutor			1.00 [.96, 1.04]	1.00 [.96, 1.05]
Felony Division			.42 [.21, .83]	.51 [.25, 1.03]
Head Prosecutor			.85 [.45, 1.60]	.85 [.43, 1.69]
Age			1.00 [.97, 1.03]	1.00 [.96, 1.03]
Female			1.10 [.67, 1.81]	1.15 [.69, 1.93]
Not White			.67 [.29, 1.56]	.57 [.24, 1.38]
Hispanic			2.33 [.82, 6.59]	2.63 [.89, 7.80]
Penalty Recommendations				
Monetary Penalty				1.90 [1.16, 3.11]
Felony Charge				2.28 [1.20, 4.36]
Number of Charges				1.17 [1.02, 1.33]
Constant	.41 [.26, .64]	***	.26 [.07, .94]	*
Pseudo R - squared	.0019		.0365	
N=	451	443	417	406

Note: +=statistical significance at .10 level, *=statistical significance at .05 level, **=statistical significance at or below .01 level, ***=statistical significance at or below .001 level

Table 8-- Confinement Recommendation (Minimum Days) Regression, with Blinded Condition as Reference Category

	Model 1: Experimental Conditions	Model 2: Experimental Conditions and Jurisdiction Characteristics	Model 3: Experimental Conditions, Jurisdiction Characteristics, and Prosecutor Characteristics	Model 4: Experimental Conditions, Jurisdiction Characteristics, Prosecutor Characteristics, and Penalty Recommendations
	Coeff [CI]	Coeff [CI]	Coeff [CI]	Coeff [CI]
Experimental Conditions				
Black Accountant	-1.63 [-21.53, 18.27]	-2.38 [-22.80, 18.04]	.40 [-20.97, 21.76]	8.73 [-10.94, 28.39]
Black Fast food	2.84 [-17.58, 23.25]	3.50 [-17.44, 24.43]	8.02 [-14.07, 30.12]	19.51 [-.78, 39.80] +
White Accountant	-9.21 [-29.69, 11.27]	-8.85 [-29.58, 11.88]	-5.17 [-26.80, 16.46]	6.74 [-13.22, 26.70]
White Fast food	3.48 [-16.47, 23.44]	2.87 [-17.35, 23.08]	3.90 [-17.24, 25.03]	10.59 [-8.97, 30.16]
Jurisdiction Characteristics				
Size of Office		.04 [-.18, .26]	.01 [-.22, .24]	0.01 [-.20, .22]
Size of Jurisdiction		-3.48 [-11.55, 4.59]	-4.80 [-13.53, 3.94]	-3.17 [-11.17, 4.82]
New England		-20.40 [-54.32, 13.52]	-16.21 [-51.92, 19.50]	-6.44 [-39.49, 26.62]
Middle Atlantic		6.62 [-32.27, 45.52]	13.20 [-26.92, 53.32]	18.57 [-19.53, 56.67]
Midwest		-4.07 [-24.18, 16.05]	2.13 [-19.43, 23.69]	5.96 [-14.21, 26.13]
West North Central		1.68 [-23.33, 26.69]	5.15 [-21.64, 31.95]	10.22 [-14.54, 34.98]
South Atlantic		6.89 [-14.03, 27.81]	11.48 [-11.31, 34.27]	5.89 [-15.07, 26.85]
East South Central		-8.79 [-34.85, 17.28]	-3.38 [-30.41, 23.65]	5.33 [-19.56, 30.22]
West South Central		-17.52 [-87.14, 52.10]	-15.64 [-86.96, 55.67]	2.06 [-63.14, 67.26]
Pacific		4.51 [-19.33, 28.36]	12.08 [-13.87, 38.02]	13.31 [-10.63, 37.25]
Prosecutor Characteristics				
Time as Prosecutor			-.13 [-1.25, .98]	.03 [-.99, 1.06]
Felony Division			-10.94 [-28.27, 6.40]	-4.54 [-20.67, 11.58]
Head Prosecutor			-5.91 [-25.04, 13.23]	-.34 [-18.29, 17.61]
Age			-.33 [-1.26, .60]	-.29 [-1.15, .56]
Female			.04 [-14.81, 14.88]	.27 [-13.41, 13.95]
Not White			-27.16 [-51.11, -3.22] *	-27.20 [-49.30, -5.10] *
Hispanic			56.13 [21.88, 90.39] ***	56.77 [25.44, 88.09] ***
Penalty Recommendations				
Monetary Penalty				-9.96 [-23.19, 3.27]
Felony Charge				70.94 [52.26, 89.62] ***
Number of Charges				.54 [-3.01, 4.09]
Constant	22.21 [8.02, 36.40] **	35.34 [-3.06, 73.75] +	65.00 [-3.54, 133.54] +	25.15 [-39.73, 90.03]
R - squared	.0043	.0235	.0637	.2085
N=	442	439	414	403

Note: +=statistical significance at .10 level, *=statistical significance at .05 level, **=statistical significance at or below .01 level, ***=statistical significance at or below .001 level