Addressing Inequity in the Classroom and the Courtroom

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Abstract

The core tenet of American civic society is that accountable, democratic institutions promote social welfare, providing fair opportunities for mobility and protecting against arbitrary punishment. Too often, however, our institutions fall short. Academic research has long recognized that socioeconomic inequity frequently stems from the very civic pillars meant to safeguard our social fabric. From an economic perspective, the costs of inequality in our schools and criminal justice system—two key pillars of the state—are abundantly clear. Poor, Black, and Brown citizens, on average, see worse outcomes from their interactions with schools and the courts. Lower-quality schooling leads to worse career outcomes and social disaffection; harsh judicial punishments similarly jeopardize economic security. While these twin problems have long been recognized, relatively little rigorous empirical work has interrogated the structural features of these institutions that generate unequal outcomes. This dissertation considers the causes of inequities in the education and criminal justice systems, and the efficacy of potential solutions.

Chapter 1. No Spending without Representation: School Boards and the Racial Gap in Education Finance

Chapter 1 examines how school boards shape racial and ethnic inequality in public schools. I provide causal evidence that greater minority representation on school boards translates into greater investment in minority students. Focusing on California school boards, I instrument for minority (specifically, Hispanic) representation using random ballot ordering, and leverage new data from a statewide capital investment program to capture intra-district resource allocations. I show that Hispanic board members invest the marginal dollar in high-Hispanic schools within their districts. High-Hispanic schools also exhibit gains in student achievement, attributable to increased investment alongside decreased teacher churn. I conclude that enhancing minority representation on school boards could help combat racial disparities in education.

Chapter 2. Defense Attorneys and Racial Disparities in Plea Bargaining

Chapter 2 considers the interplay between defense attorney incentives and well-known racial gaps in criminal sentencing. Focusing on indigent defendants, I compare plea bargaining and sentencing outcomes for nonwhite clients assigned public defenders to those instead assigned private attorneys. To deliver causal effects, I use an instrumental variables approach that leverages idiosyncratic variation in public defender assignments, driven by caseload capacities. I show that nonwhite defendants represented by private attorneys plead guilty to charges that carry longer sentences relative to comparable nonwhite defendants represented by public defenders. These effects are not explained by differences in defendant or case characteristics, and I find no comparable effects among white defendants. In other words, prosecutors extract harsher plea deals specifically from minority defendants with private attorneys, suggesting opportunistic discrimination against minority defendants with susceptible defense counsel. Consistent with a straightforward signaling model of plea bargaining, I provide empirical evidence suggesting that public defenders who have extensive interactions with prosecutors improve nonwhite defendants' plea bargaining outcomes. Reallocating public defenders to minority defendants could thus diminish racial bias in plea bargaining and better fulfil nonwhite defendants' constitutional right to effective representation.

Chapter 3. Ignorance is Strength? Information and Incentives in Plea Bargaining

Chapter 3 (joint work with Leora Friedberg) complements Chapter 2, interrogating the efficacy of low-cost information interventions that might help vulnerable criminal defendants secure better criminal justice outcomes. This paper evaluates whether informing defendants about the adverse consequences associated with pleading guilty bridges the information gulf between prosecutors and defendants and thereby reduces plea rates. As a plausibly exogenous shock to defendants' information sets, we study the Supreme Court's

2010 decision in *Padilla v. Kentucky*. *Padilla* requires defense attorneys to advise their noncitizen clients whether or not they will face deportation by accepting a plea offer. We employ a difference-in-differences design to compare citizens and noncitizens before and after the Court's ruling. Our results show that *Padilla*’'s information intervention reduced guilty plea rates among noncitizen cases for which a conviction would risk immediate deportation. Conversely, we find that noncitzen cases that do not risk imminent deportation plead guilty more often following *Padilla*, and were more likely to be incarcerated. We interpret these results as evidence that prosecutors threaten harsher charges for defendants who turn down unfavorable but not catastrophic plea deals. Our findings highlight the limitations of existing and proposed procedural reforms to the plea bargaining process.

JEL Codes: H41,H72,H75,I22,I24, K14,K37,K41,K42
Keywords: School boards, education finance, school spending, student outcomes, plea bargaining, defense attorneys, prosecutors, information, defendant race

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No Spending Without Representation: School Boards and the Racial Gap in Education Finance*

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(Please find the most up-to-date version here.)

Abstract

This paper provides causal evidence that greater minority representation on school boards translates into greater investment in minority students. Focusing on California school boards, I obtain causal effects by instrumenting for minority (specifically, Hispanic) school board representation using the random order in which candidates appear on election ballots. Given the dearth of school-level expenditure data, I introduce detailed records from California's School Facility Program (SFP), a capital investment program for which I observe how school boards allocate the marginal dollar within-district. Instrumental variables estimates show that an additional Hispanic school board member increases SFP-funded investments at high-Hispanic schools within the district by 66 percent, with significantly lower effects at low-Hispanic schools. High-Hispanic schools also exhibit gains in student achievement of 0.08 standard deviations. I attribute this improved performance to increased spending alongside decreased teacher churn: new hiring at high-Hispanic schools decreases by 16 percent. These results provide the first causal evidence that school board politics—and, specifically, school board ethnic composition—shapes education finance policy at the local level. I conclude that enhancing minority representation on school boards could help combat racial disparities in education finance and achievement.

JEL Classifications: H41,H72,H75,I22,I24
Keywords: School boards, education finance, school spending, student outcomes

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1 Introduction

Despite six decades of initiatives to improve racial equity in education finance, nonwhite students continue to face a resource gap relative to their white peers. Nationwide, the average white student benefits from as much as $2,200 in additional spending per year relative to the average nonwhite student (EdBuild 2019); nonwhite students are taught by relatively inexperienced teachers (Lankford, Loeb, and Wyckoff 2002; Scafidi, Sjoquist, and Stinebrickner 2007) and are more likely than whites to attend dilapidated schools (Alexander, Lewis, and Ralph 2014; Filardo, Vincent, and Sullivan 2019). As a result, nonwhite students experience high marginal returns to additional resources: school desegregation benefits these students mostly by exposing them to higher spending (Reber 2010), while higher spending mitigates the consequences of re-segregation (Billings, Deming, and Rockoff 2013). So why do policymakers not spend more on minority students?

It is not for lack of trying, at least at the federal and state levels. But while federal and state redistribution reduces *inter*-district funding gaps (Card and Payne 2002; Papke 2005; Roy 2011), these policies do not reduce *intra*-district achievement gaps (Cascio, Gordon, and Reber 2013; Hyman 2017; Lafortune, Rothstein, and Schanzenbach 2018). That disconnect could arise because locally-elected school boards—rather than legislators—allocate the marginal dollar within districts. School board members may represent voters who do not value investments in high-poverty or minority-majority schools, which limits the political impetus for action and could further marginalize those students. Yet, despite their remit to manage district budgets, there is no rigorous evidence showing how school boards' composition affects intra-district resource allocations.

In this paper, I investigate whether school boards with greater minority representation devote greater resources to minority students. Nonwhite board members may be more cognizant of inequitable spending than their white counterparts, and thus more willing to direct resources towards nonwhite students. Such behavior is consistent with a political model in which a candidate's core constituency comprises voters sharing her race/ethnicity, analogous to Glaeser, Ponzetto, and Shapiro (2005).[1] Conversely, if the median voter decides the election, then board members might adopt similar spending policies, regardless of their own ethnic identities. Greater minority representation could even reduce spending on minority students if ethnic diversity on the school board paralyzes decision-making, as Beach and Jones (2017) observe among city councils. Evidence supporting any of these hypotheses would clarify whether a lack of minority political representation helps drive the racial resource and achievement gaps in education.

To identify the causal effect of school board ethnic composition on intra-district spending outcomes, I need to address two empirical challenges. First, school-level spending data are often restricted to a single district, and there is scant variation in finance policy to identify how boards apportion the marginal dollar.[2] I therefore leverage data from California's School Facility Program (SFP), which since 1998 has provided matching funds for districts that choose to construct new schools or renovate existing ones.[3] Although capital spending accounts for only six percent of the median California district's budget, the program provides an opportunity to track the marginal discretionary dollar within districts, since SFP take-up and allocations depend on school board initiative. Using detailed, school-level data from the SFP, I illustrate how school board ethnic composition affects intra-district spending

1. The Civil Rights-era South highlights an extreme example of this kind of race-based electoral model. Cascio et al. (2013) argue that white-dominated school boards withheld Title I funding from *de facto* segregated Black schools, presumably to appease the (mainly white) electorate. While the authors' inference makes sense given their historical setting, my goal is to rigorously estimate the effect of school board ethnic composition on district resource allocation in a modern context with a diverse electorate.

2. Descriptive analyses of intra-district resource allocation using individual school districts include Jimenez-Castellanos (2010) and Rubinstein et al. (2007). Comparable descriptive studies using data from federal surveys of multiple districts include Heuer and Stullich (2011) and Ejdemyr and Shores (2017). All of these analyses find evidence that intra-district racial gaps in spending are just as prevalent as inter-district racial gaps in spending. However, none of these data sets include the universe of schools or districts (even within a given state); the nationwide data also suffer from inconsistencies in reporting standards that raise concerns about data quality and comparability across survey waves. On the other hand, Lane, Linden, and Stange (2018) use statewide data from Texas, and the authors report substantial within-district gaps in school facility quality and teacher turnover, two themes that feature prominently in this article as well. However, as with Hyman (2017)—who employs comprehensive, statewide data from Michigan (and causal identification)—the lack of recent variation in education finance policy in many states limits the usefulness of most existing data for my purposes.

3. Some of the SFP projects likely also appear in the samples of school facility investments used by Cellini, Ferreira, and Rothstein (2010) and Lafortune and Shönholzer (2018), although my work is, to my knowledge, the first to study the SFP specifically.

patterns.

The second hurdle for causal identification is that school board composition is endogenously determined via elections. Comparing SFP outcomes for districts with high and low minority school board representation will thus confound composition effects with factors that jointly drive the board's investments and election outcomes. For example, districts where minority candidates win school board elections might have low tax bases, which would limit a board's ability to make capital investments and lead to a spurious negative correlation between SFP spending and minority representation.[4]

To overcome this endogeneity concern, I utilize California's "random alphabets," which sort candidates on election ballots and exogenously impart high-ranked candidates with an electoral benefit. Echoing Shi and Singleton (2019), I instrument for Hispanic school board representation with a measure of Hispanic candidates' rankings on recent election ballots.[5] I show that my ballot order instrument is strongly correlated with post-election Hispanic school board representation but uncorrelated with other district characteristics, which bolsters my causal claim.[6] I estimate my instrumental variables model using an original data set that combines California school board election results from 1996-2007 with information on candidate ethnicity, district SFP spending, and student demographic and

4. Such concerns raise doubts about the conclusions in Meier and England (1984) and Fraga et al. (1986), who take up the same topic as this paper, but rely on descriptive statistical techniques. Still, those studies—which show a correlation between Black and Hispanic school board representation, minority teacher hiring, and minority student college-going—provide a fitting backdrop for my work which, as I discuss below, confirms using causal inference that minority representation improves minority student outcomes, and goes one step further by showing suggestive evidence that resource re-allocations underpin these gains.

5. I focus on Hispanic representation because Hispanics easily comprise California's largest ethnic minority group. Furthermore, from a practical perspective, identifying Hispanic school board candidates is easier than identifying, for example, Black school board candidates, as I briefly discuss in Section 3.

6. The ballot order IV has two advantages over the more popular regression discontinuity (RD) approach. First, a RD identifies an effect among close elections that also have ethnically diverse candidate pools; the IV approach still relies on diverse candidate pools, but does not require a narrow election margin. Since competitive school board races are rare—and ethnic minority candidates are even rarer—I argue my estimates have greater external validity. Second, a RD design might pose a challenge given my focus on race, since identification in a RD framework depends on the quasi-randomization of winning and losing candidates across close elections. Vogl (2014) demonstrates that winning minority candidates often increase voter turnout on election day, which undermines the assumption that close elections with different outcomes are comparable. By contrast, the ballot order instrument relies on randomization that occurs prior to election day, but after candidates must enter the race, limiting the potential for gamesmanship.

performance data.

I first consider the relationship between Hispanic school board representation and intra-district investment patters. Two-stage least squares estimates imply that, in the short term, a 20 percentage point increase in Hispanic board representation (equivalent to substituting one Hispanic member for one non-Hispanic member) spurs 35 percent more SFP investment ($146 per student) among schools with relatively high Hispanic enrollment within a district. Spending increases by an insignificant 15 percent ($64 per student) at low-Hispanic schools. I present evidence that this difference is statistically significant—in other words, more-Hispanic school boards devote more resources to more-Hispanic schools.

To be clear, I cannot say exactly how much additional spending Hispanic members direct towards high-Hispanic schools. School boards can pursue all sorts of investments beyond SFP—on technology, textbooks, or teachers, to name a few—and I am limited by the absence of comprehensive school-level spending data. It stands to reason, though, that since Hispanic board members devote more SFP dollars to high-Hispanic schools, they likely do the same for other (unobserved) types of investment. To drive home this point, I consider how Hispanic representation affects teachers, whose salaries comprise a plurality of district budgets. I show that Hispanic board members decrease teacher turnover at high-Hispanic schools, manifested by a 16 percent reduction in new teacher hiring and a 4 percent increase in average tenure. Taken together, the SFP and teacher churn results point to pecuniary and non-pecuniary investments at high-Hispanic schools, and support my overarching claim that minority board members allocate resources to benefit minority students.

Finally, I examine how Hispanic board representation affects student achievement. Again, school boards have myriad policy and spending levers at their disposal. My goal, then, is not to estimate the distinct effect of a particular policy—capital improvements or decreased teacher churn—but rather to estimate the holistic, reduced-form impact of greater minority board representation per se. 2SLS estimates imply that an additional Hispanic board member causes math scores to improve by 0.08 standard deviations at high-Hispanic schools, and by (an insignificant) 0.05 standard deviations at low-Hispanic schools; less precise estimates point to smaller but positive effects on reading scores. In light of my findings above, I argue that these achievement gains stem broadly from increased school board investment in disadvantaged schools.

This paper makes two contributions to the literature. First, I provide new causal evidence that school board composition matters for intra-district spending allocations, which highlights school boards' importance in the education production function. This analysis builds upon a nascent literature which describes how school board members' identities—namely, their partisanship (Macartney and Singleton 2018) and professional backgrounds (Shi and Singleton 2019)—shapes local education policymaking. I also substantiate speculation from Cascio et al. (2013), Gordon (2004), and Hyman (2017) that racial equity in education finance depends on school boards, and that the success of redistributive education policies hinges on how local school boards choose to implement them. Moreover, my findings suggest that schools boards can re-allocate spending towards high-need students and promote student achievement, in contrast with the (less effectual and more costly) Title I program.[7]

Second, this paper speaks to a growing literature which explores how strengthening racial and ethnic minorities' political voices affects the distribution of public resources.[8] Specifically, I show that racial gaps in political representation translate into racial gaps in public resource allocations, a result which is implicit in the literature but can be difficult to establish explicitly since the intended beneficiaries of public dollars often remain unclear. Above all, my findings indicate that improving minority political representation on school boards could help combat persistent racial disparities in education finance and achievement.

7. See, for example, van der Klaauw (2007) and Matsudaira, Hosek, and Walsh (2012).

8. In particular, Ananat and Washington (2009), Beach and Jones (2017), Cascio and Washington (2013), Hopkins and McCabe (2012) stand out.

2 Policy Background

2.1 California School Boards

California has over 950 elected school boards, which makes it an ideal setting to study the relationship between school board composition and intra-district spending outcomes. School boards in the state consist of three, five, or seven community members elected to administer a public school district. Board members' responsibilities include managing a district superintendent, negotiating staff and service contracts, and implementing a district budget (Hochschild 2005). Surveys report that boards spend most of their time on budget matters, allocating spending across different priorities and schools (Grissom 2007).[9]

Most California school board elections occur biennially, with a fraction of seats contested in each cycle. These contests are non-partisan: the ballot lists only candidates' names and self-described occupations. Most districts use an "at-large" election format in which voters district-wide select candidates to fill multiple seats. For example, in an election with two contested seats and five candidates, every voter can choose up to two candidates to support, and the candidates with the highest and second-highest vote shares will both win. All other districts use a "ward" model in which every school board seat corresponds to a constituency within the district, and only candidates who receive a plurality of the vote in their constituencies win. Once elected, school board members serve four-year terms and do not face term limits; they often receive nominal compensation for their time.

2.2 California's School Facility Program (SFP)

Introduced in the Leroy F. Greene School Facilities Act of 1998 (SB 50), the School Facility Program (SFP) provides state financial support for California school districts that choose to undertake capital improvement projects. Four statewide general obligation bonds—issued

9. Modern school boards have little discretion over tax rates, following a nationwide shift towards state-based funding in the 1990s and 2000s. In California, parcel taxes levied directly by school boards account for less than 10 percent of average district revenues; over 75 percent of funding comes from state transfers and property taxes, set by the legislature and county assessors, respectively. Even if school boards lobby counties for additional tax revenue, California's Proposition 13, passed in 1978, prohibits them from substantially raising property tax rates.

in 1998, 2002, 2004, and 2006—provided $35.4 billion in state funding for K-12 school construction and modernization. For most projects, the state contributes between 50 and 60 percent of the total cost; school boards usually issue bonds to pay for their share.

School boards must apply for SFP financing. Eligibility for new construction funds depends on the district's 5-year enrollment projections and its current classroom capacity. The state allots funds for each projected student in excess of the district's current capacity. For instance, a district is eligible for $12,197 per projected elementary school student in excess of capacity. Modernization funds are only available for schools that are at least 25 years old; the SFP provides funds for each student enrolled in the school being modernized ($4,644 per elementary school student, for example). Projects can also receive supplemental funding for, among other goals, relieving chronic overcrowding, installing earthquake safeguards, and acquiring land. Upon its completion, a project undergoes an independent audit, and the district submits an updated expense report to the state. These final expense reports form the basis of my SFP data, which I describe in the next section.

3 Data

In this paper, I evaluate the impact of minority school board representation on intra-district SFP spending patterns and student outcomes. Specifically, since my policy setting is California, I focus on Hispanic school board representation, since Hispanics easily comprise the state's largest ethnic minority group. The core of my data consists of school board election records, matched to information about candidates' and school board members' ethnicities. I link these election and school board composition data to school- and district-level SFP spending; school and district demographics; and student achievement outcomes.

3.1 School Board Elections Data

Data on California school board elections held between 1996 and 2007 come from the California Election Data Archive (CEDA). The CEDA records each candidate's name, vote total, incumbency status, and whether she won the race. I calculate candidates' vote shares as well as the number of contested seats and the number of candidates by election. The first panel of Table 1 describes these 3,738 elections. The median election outcome is nontrivial, with a 5 percentage-point victory margin. But the number of candidates ranges from 1 (i.e., an uncontested race) to 28, with a median of 4, which indicates that election competitiveness varies substantially.

3.2 Hispanic School Board Representation and Hispanic Candidates

For every district-year between 1996 and 2008, I measure Hispanic school board representation using lists of Hispanic school board members provided by the National Association for Latino Elected Officials (NALEO). The annual directories include school board members who confirm to the NALEO that they identify as Latino.[10] I define Hispanic school board representation in a given district-year as the number of Latino school board members (according to the NALEO) divided by the board size.[11]

To infer which school board candidates most likely identify as Hispanic, I use data from two sources. First, I match candidates' names to the NALEO directories for 1996 through 2018. I classify any candidate who appears in the NALEO directory (in any year) as Hispanic. Second, since the NALEO data do not include Hispanic candidates who never won a school board election, I also classify a candidate as Hispanic if more than 50 percent of respondents to the 2000 decennial Census with her surname self-identify as Hispanic.[12] I

10. I use the terms "Hispanic" and "Latino" interchangeably in this paper. As such, I assume that all of the candidates who self-identify as Latino also identify as Hispanic, and vice versa.

11. I assume that all school boards have 5 total members. In a sample of 1,168 district-years, I find that 90 percent of districts have 5 serving school board members while 6.7 percent have 7 members. The sample consists of 100 school districts whose board composition between 1998 and 2018 I researched using archived district web pages. (Due to data quality concerns and the impracticality of scaling this approach to remaining 850-odd California school districts, I do not use this manual search method to find all boards' compositions.) If I find more than 5 Hispanic members listed in the NALEO directory for a given district-year, I assume the board size is 7 since California school boards can only have 3, 5 or 7 members. While not ideal, I show that an alternative "treatment" which does not rely on the imputed board size—whether any Hispanic member holds a school board seat—also has a strong first first stage and yields quantitatively similar results. Since NALEO data from 1998 and 1999 are missing, I calculate Hispanic representation during these years by interpolating data from 1997 and 2000.

12. Unfortunately, neither the Census nor any other data set I am aware of provides a breakdown of ethnicity by first (given) name. This limitation means that, for example, I have no way to comment on the prevalence of Hispanic married women with non-Hispanic married names. By the same token, though, I note that non-Hispanic married women may take Hispanic surnames. The relative sizes of these groups is

find that 84 percent of candidates in the NALEO directory also appear in my list of Census-inferred "Hispanic" surnames, which suggests my name-matching approach does a good job identifying losing candidates who would self-identify as Hispanic.[13] I prefer using both the NALEO and Census records—instead of just Census name matching—since the NALEO directory provides the more accurate snapshot of individuals who self-identify as Hispanic, which is a population of greater interest than just those with Hispanic surnames.[14,15]

The second panel of Table 1 summarizes Hispanic candidates' prevalence and performance in California school board elections. The average California school board election features less than one Hispanic candidate (0.66) and Hispanics comprise only 15.6 percent of all candidates in the average race. Hispanic representation on school boards (measured in the year of the election) is only 11 percent, which amounts to less than one member on the modal, five-seat school board. In the next subsection, I elaborate on the differences between districts where Hispanic candidates do and do not run for school board.

3.3 Student Demographic and Achievement Data

For academic years 1998-1999 and 2015-2016, I obtain student enrollment and demographic data from the Common Core of Data (CCD). The data contain total enrollment by year for every school within a district as well as enrollment by demographic subgroups (Hispanic, white, Black, Asian, and students who are eligible for free or reduced-price lunches [FRL]). The CCD censors school-year-subgroup cells with five or fewer students, which leads

unknown, and so is the direction of the resulting bias.

13. The referenced match results appear in Appendix Table A2.

14. In Appendix Table A3, I show that I obtain similar, albeit quantitatively smaller and less precise, estimates when I use only the Census records to identify Hispanics. The smaller estimates and smaller sample under this approach suggest my NALEO directory approach helps me properly categorize candidates with non-Hispanic surnames who nevertheless identify as Hispanic.

15. Whether a candidate has a Hispanic-sounding surname and whether she self-identifies as Latinx are in fact distinct and deeply personal concepts. In Appendix Table A2, I show that 16 percent of self-identified Latinx school board members have surnames that might not be considered "Hispanic," which attests to the potential divergence between personal identity and family name. However, that 84 percent of officials in the NALEO directory *do* have majority-Hispanic surnames suggests that those candidates with majority-Hispanic surnames strongly correlate to those who self-identify as Latinx. For deeper qualitative and quantitative discussions (respectively) of this issue, see Donella (2017) and Lopez, Gonzales-Barrera, and López (2017). Also see Grofman and Garcia (2015) and Harris (2017) for short explanations of why using the majority ethnicity for a given surname is suitable to identify individuals' ethnic backgrounds.

to missing observations in my final data set.

I also incorporate math and reading (ELA) exam data from grades 2-7 for academic years 1997-1998 through 2011-2012, provided by the California Department of Education (CDE).[16] The data contain the average school- and district-level exam scores by subject-grade; the CDE censors school- and district-subject-grade-year cells with fewer than ten students, which again leads to missing observations in my final data set. In part to minimize the incidence of missing data, I aggregate test scores to obtain composite averages by school- and district-subject-year.[17] I normalize test scores using the statewide mean and standard deviation of district-by-subject scores, so the final units are district-subject-year standard deviations.[18]

In Table 2, I describe my district-level demographic and achievement variables during election years. The first column includes all elections in my sample, which I summarize in Table 1. The remaining columns decompose the sample into those elections with no Hispanic candidates (column 2), those with only Hispanic candidates (column 3), and those with a mix of Hispanic and non-Hispanic candidates (column 4). Comparing the first two columns indicates that districts whose elections feature no Hispanic candidates are positively selected: they have lower poverty rates (as measured by the share of FRL-eligible students) than the full sample as well as student achievement that exceeds the state average by 22-29 percent of a standard deviation. Conversely, districts whose elections feature any Hispanic candidates have higher shares of FRL-eligible students than the whole sample, and test scores there lag the statewide mean by 30-35 percent of a standard deviation. These comparisons demonstrate that Hispanic school board candidacies are negatively correlated with district affluence and student performance.

16. I only consider grades 2-7 in because students begin tracking into different math classes, with different associated math exams, in 8th grade.

17. Beginning in academic year 2002-2003, the data also contain student performance by demographic subgroup. I do not use those data due to large volumes of missing observations and the fact that these outcomes are unavailable for most election years in my sample period. As such, I will not report achievement by Hispanic students per se.

18. I make this choice to ensure that all test score outcomes, no matter the level of aggregation, are comparable. School-level estimates measured in school-subject-year standard deviations show virtually identical results and are available upon request.

3.4 SFP Spending Data

My project-level SFP spending data come from final expense reports that district officials submit to the state. The data contain 7,641 unique project records including the name of the school district and school that secured SFP funds; categorized spending (for example, the amount spent on modernization and the amount spent on earthquake preparedness); and the date when the project received its first funds from the state.[19] I define a project's starting date as the day the state first released funds. For each school- and district-academic year, I sum up total SFP spending on projects started in that year, as well as spending by project type.[20] I inflate all costs to 2016 dollars and normalize spending outcomes at the school- and district-level by total student enrollment in that academic year. To correct for implausibly low enrollment in certain observations, I winsorize school- and district-level SFP spending variables at their ninety-ninth percentiles.[21]

3.5 Comparing SFP Spending by Hispanic School Board Representation

I summarize my district-level SFP outcomes in Table 3. In the first column, I report average SFP spending among all district-academic years between 1999-2000 and 2015-2016. This full sample contains 13,900 district-academic year observations. In the remaining columns, I parse the sample into district-years based on the level of Hispanic school board representation. Interestingly, only 27 percent of district-years have any Hispanic board representation whatsoever, despite the fact that 38 percent of students in the average district are Hispanic.

Moreover, the data exhibit a negative correlation between school board Hispanic representation and district SFP spending per student. Among district-years with no Hispanic school board members, the board invests $365 per student in all SFP construction, while among boards with more than 20 percent Hispanic representation, total SFP spending falls

19. Summary statistics describing the raw data appear in Appendix Table A1.

20. For my purposes, an academic year begins on July 1, which is the beginning of the fiscal year for California school districts. Thus, for example, a project that begins on July 2, 2001 counts towards a district's SFP spending in academic year 2001-2002, while a project that begins on June 30, 2001 counts towards the district's SFP spending in academic year 2000-2001.

21. Alternative winsorization thresholds yield similar findings.

to $300 per student. Similar patterns hold for SFP spending by project type (modernization, new construction, and supplemental). This negative relationship could be causal, or could be a function of the negative correlation between district affluence and Hispanic school board candidacies visible in Table 2. Disentangling the causal effects of Hispanic board representation on SFP take-up and spending from confounding factors—particularly school district size and affluence—will be the primary goal of my research design.

3.6 Final Sample and Panel Data Set Construction

My final sample contains 1,314 competitive school board elections that have both Hispanic and non-Hispanic candidates. I restrict the sample to these ethnically diverse elections so that I can compare elections in which Hispanic and non-Hispanic candidates each had a chance at winning. In so doing, however, I acknowledge that the "treatment" effect I uncover will be local to these ethnically-competitive elections, since districts that hold these elections differ substantially from the universe of California districts (see Table 2).

Around each of these 1,314 elections, I create a panel of data that spans eight years prior to the election through eight years after the election. Each observation in the panel is an election-period, which corresponds to a district-academic year.[22] The panel data set has an unbalanced structure, where periods may be missing for some elections but not others (e.g., for elections prior to 1997, I have no pre-election data). For a given election, all period observations share time-invariant data, such as the share of Hispanic candidates in the race and the number of contested seats. For each period, I also include time-varying district-academic year data, such as SFP spending and student achievement. Together, the election panels have an overlapping "stacked" structure, where a district-academic years can appear in multiple elections' panels. For example, all data from San Jose Unified School District in 1998 will appear in the panels for its 1996 election (as period +2), its 1998 election (as

22. For districts that hold "ward" elections, I treat each individual ward race as a separate election, since there is no way *a priori* to determine which of the multiple races is the most interesting for my purposes. Comparable estimates that drop ward elections altogether yield similar findings, and those results are available upon request.

period 0), and its 2000 election (as period -2).[23] The district-level data set contains 22,595 district-period observations from 489 unique districts.[24]

I construct a similar school-level panel data set using the same time-invariant elections data but with school-year-level SFP and student performance outcomes. That is, rather than matching an election uniquely to a school district, I match every election to potentially many individual schools within the district. This school-level panel contains 168,507 school-period observations from 3,293 unique schools.

4 Research Design

My goal in this paper is to deliver a causal estimate of how greater Hispanic school board representation affects intra-district spending patterns, measured in terms of SFP investment. However, an OLS estimate of the effect of Hispanic representation on SFP spending may be downward-biased since districts with more Hispanic school board candidates are relatively high-poverty and likely have lower tax bases from which to finance capital improvements (see Table 2). In this section, I discuss the instrumental variables (IV) strategy I use to overcome this endogeneity hurdle, the specifications I employ, and the validity of my instrument.

4.1 Randomization on California Ballots

To obtain plausibly exogenous variation in election results and Hispanic representation on school boards, I leverage California's random ordering of candidates on election ballots. On the eighty-second day prior to an election, the Secretary of State's office draws all twenty-six letters from a (figurative) hat that has been "shaken vigorously" (California Election Code §13112). The order in which the official draws those letters creates a random alphabet that counties use to sort candidates on their ballots.[25] The alphabetization process first sorts

23. I diagram this example in Appendix Figure A1.

24. My stacked data structure mirrors that in Cellini et al. (2010) and Shi and Singleton (2019). This setup allows me to explore dynamic election effects (unlike Ferreira and Gyourko [2010]) and means I do not have to take a stand on which post-election period will show the most meaningful election effects (as in Macartney and Singleton [2018]).

25. All school board elections in the same cycle across the state use an identical random alphabet; elections for state officials introduce another layer of randomization across constituencies.

candidates by last name and then first name; the drawing take place after candidates must declare their intentions to run (a deadline which varies by county but falls at least 88 days prior to an election), which precludes endogenous entry into the race.[26]

4.2 Randomized Ballot Order: Candidate-level Variation

Ample evidence suggests the resulting ballot rankings could matter for school board election outcomes, as voters minimize the cost of participating in these low-profile races by arbitrarily picking high-ranked candidates.[27] I verify this ballot order effect on candidates' performance in my setting by first compiling the random alphabets drawn between 1996 and 2007 and combining them with my elections data to determine candidates' rankings on election ballots.[28] To confirm that my alphabetization works, I compare my imputed ballot ordering with actual ballots from several elections. I then construct a binary variable equal to one if a candidate is randomly assigned to one of the top K spots on the ballot, where K is the number of contested seats. I refer to the top K spots as the "top tier" of the ballot. This approach echoes that of Shi and Singleton (2019).[29]

I next evaluate the effect of being assigned a top-tier ballot spot by comparing top-tier candidates to their competitors. For candidate c running in district-election e, I regress her

26. While new candidates cannot join the race in response to the alphabet drawing, it is possible that, upon learning their ballot position, existing candidates alter their campaign strategies. For example, a low-ranked candidate may either put in less effort, convinced he will lose anyways, or redouble his efforts, convinced he can overcome that disadvantage. However, candidates' effort levels are unobservable and, in any event, survey evidence (notably, Hess [2002]) suggests school board campaigns neither garner much public interest nor entail serious campaigning, with most candidates spending less than $\$1,000$. Since high- and low-effort reactions to the ballot drawing are equally plausible *ex ante*, and neither will likely matter much much on the margin, I argue that the net bias is negligible.

27. See, for example, Ho and Imai (2006, 2008), Koppell and Steen (2004), and Meredith and Salant (2013).

28. For all statewide elections since 2004, I use the alphabets provided in press releases published by the California Secretary of State. For on-year elections prior to 2004, I use Ho and Imai's (2008) alphabet table. And for off-year November elections between 1997 and 2003, I use internal memos provided by the Secretary of State's office. Meredith and Salant (2013) use the same data, but also include March elections from 1997-2003. Shi and Singleton (2019) do not include off-year elections from 1997-2003, but their data are otherwise identical.

29. The authors use an indicator equal to one if a former educator is randomly assigned to the first ballot position, since the ballot order literature finds that the top candidate receives the largest electoral benefit. By contrast, I remain relatively agnostic over how exactly the ballot order effect is conferred. For example, voters who want to minimize the effort costs of voting might select the top K names on the ballot, which could benefit any well-placed Hispanic candidate, even one not listed at the very top of the ticket. Note that the two instruments will be identical in elections with a single contested seat.

election outcome Y (either her vote share or an indicator for whether she won the election) on the top-tier ballot indicator as well as district-election fixed effects, θ_e:

$$Y_{ce} = \alpha_0 + \alpha_1 TopTier_{ce} + \theta_e + \epsilon_{ce}. \quad (1)$$

The coefficient of interest, α_1, captures how being assigned to the top tier affects a candidate's performance relative to candidates outside of the top tier in the same election.[30] The identifying assumption is that a candidate's top-tier status is randomly assigned and thus uncorrelated with unobserved determinants of her election performance (represented by ϵ).

Results from this candidate-level analysis appear in the top panel of Table 4. The first and second columns use the full sample of candidates described above (N=6,469); the third and fourth columns use the subsample of Hispanic candidates (N=2,140). For the average candidate, being randomly assigned a top-tier ballot spot increases her vote share by 1.2 percentage points (6.5 percent of the control mean) and her probability of victory by 6.2 percentage points (15 percent of the control mean). The effects are larger, although less precisely estimated, among Hispanic candidates. These coefficients are quantitatively similar to estimates of the "top-of-the-ticket" effect (i.e., the effect of being assigned to the very first ballot position) in the ballot order literature, and confirm that a top-tier ballot assignment has a meaningful effect on candidates' electoral prospects.

Additionally, to provide suggestive evidence that candidate ballot ordering is truly random, I estimate the correlation between a candidate's top-tier status and her political and

30. Including election fixed effects generates a causal estimate of how much better a candidate fared relative to her peers who chose to run the the same race. Leaving the fixed effects out generates a causal estimate of how much better the average top-tier candidate (across all elections) performed relative to the average candidate outside the top tier (across all elections). Since the magnitude of the ballot effect may depend on the number of contested seats, the size of the candidate pool, voter turnout, or any number of election-specific factors, I prefer the fixed-effects approach. I report results from a specification without election fixed effects in Appendix Table A4. I find that top-tier status is closely correlated with incumbency, which I interpret as a byproduct of the fact that, even when board elections are nominally competitive, most incumbents run for re-election, face relatively few challengers, and are thus likely to appear in the top tier. I also find some evidence that top-tier candidates are more likely to be Hispanic. Since my preferred models presented below control for these types of district- and election-based confounding influences, I do not consider these correlations to be a threat to identification.

ethnic identities using Equation 1, with candidate characteristics as outcome variables. The second panel of Table 4 shows these results. Encouragingly, I find that whether a candidate receives a top-tier ballot spot is uncorrelated with her political ideology, her ethnicity, and her incumbency status.[31]

4.3 Randomized Ballot Order: Constructing an Election-level Instrument

The ballot order randomization described above operates at the candidate level, placing some candidates into the top tier and not others. However, my "treatment" of interest—Hispanic school board representation immediately after an election—depends on election-level results. Therefore, to convert my candidate-level variation into election-level variation, I create an indicator equal to one if any Hispanic candidate received a top-tier ballot position in a given election. Intuitively, a Hispanic candidate near the top of the ticket is more likely to win an election than a candidate who is buried in the pack (see Table 4), and her victory could subsequently increase Hispanic representation on the school board.[32]

For the top-tier Hispanic indicator to be a valid instrument, however, it must be uncorrelated with other district and election characteristics that determine both school board composition and district outcomes. That assumption does not follow from randomized ballot ordering at the candidate level. Any candidate's ballot ranking is random from her point of view, since the assignment mechanism is a lottery. But, from the econometrician's perspective, the probability that at least one Hispanic candidate in an election winds up in the top

31. I observe candidate incumbency directly in my elections data. To infer candidates' party affiliations, I match the candidates in my sample to the California Voter Registration Database using candidate name and county of residence. The fuzzy match process leaves many candidates without political parties—either because I cannot uniquely match their names to individuals in the voter roll, or because they have moved or passed away since they ran for school board and have consequently been struck from the voter roll.

32. A potential concern is that the ballot effect is concentrated among top-of-the-ticket candidates, which would limit the relevance of the top-tier effect. Indeed, Meredith and Salant (2013) argue that this is the case. In Appendix Table A6, I compare my preferred first stage specification (see below) to alternatives, one using an indicator for whether a Hispanic appeared at the top-of-the-ticket, the other using the top-tier indicator, but omitting all top-of-the-ticket candidates. I do find that the first stage effect is larger for top-of-the-ticket Hispanic candidates, but that omitting these candidates still yields a reasonably precise, economically meaningful effect on post-election Hispanic board representation. The persistence of the ballot effect among lower-ranked candidates could be tied to race: voters may actively seek out minority candidates on election day, even ones not listed at the very top of the ballot. As such, I continue to use my top-tier instrument, since this variable captures the most information about the ballot order's effect on post-election school board composition.

tier depends upon Hispanics' share of the candidate pool. Elections with larger shares of Hispanic candidates will, on average, have more Hispanic candidates randomized into the top tier of the ballot, and will consequently have larger shares of winning Hispanic candidates (conditional on the number of contested seats). Since Hispanic candidate share is endogenous (see Table 2), the top-tier Hispanic indicator is not excludable on its own. Rather, my top tier IV is *conditionally* exogenous with respect to other determinants of school board composition once I control for the share of Hispanic candidates in the race and the number of contested seats. I include these controls in all specifications except where otherwise noted.[33]

After controlling for Hispanic candidate share and the number of contested seats, I should find no relationship between district i's characteristics χ in the election year t and the presence of a top-tier Hispanic candidate on the ballot. I examine these correlations using the following specification:

$$\chi_{it} = \phi_0 + \phi_1 HispTopTier_{it} + \phi_2 X_{it} + \lambda_i + \gamma_t + \epsilon_{it}, \tag{2}$$

where *HispTopTier* is a binary variable which equals one if a Hispanic candidate appears in the top tier of the ballot. I include election year and district fixed effects (γ and λ, respectively) to control for unobserved factors across school districts and election cycles that determine ballot composition. I cluster my standard errors at the district level to correct for repeated sampling of districts.

The results appear in Table 5.[34] Crucially, I find that whether a Hispanic candidate appears on the ballot is uncorrelated with Hispanic school board representation on the outgoing

33. In the final column of Appendix Table A8, I show that my results are robust to including flexible versions of these covariates—specifically, a quadratic in the share of Hispanic candidates and a fixed effect for the number of seats. In alternative,"event-study" specifications, which I discuss below, I employ election fixed effects, which control for all observable and unobservable election characteristics, as well as any functional forms of the two covariates described here, that could stand in the way of random assignment—not just the (linear) number of seats and (linear) Hispanic candidate share. My results are also robust to this approach.

34. In Appendix Table A7, I consider a range of other election-year outcomes, including SFP spending, SFP take-up, proxies for SFP eligibility, and alternative election outcomes (e.g., whether a Democrat wins the election). None of these outcomes are significantly correlated with whether a top-tier Hispanic candidate appeared on the ballot.

board, which means my instrument is unrelated to pre-election district treatment status. Nor do I find any correlation between whether a top-tier Hispanic candidate appears on a ballot and district observables in the election year, which suggests the instrument is unrelated to these potential confounding variables.[35]

4.4 Estimating the Causal Effect of Hispanic School Board Representation

Using my top-tier Hispanic instrument, I analyze how differences in Hispanic school board representation influence district outcomes over time using a two-stage least squares (2SLS) system. Since the consequences of school board decisions likely take years to play out, I evaluate how the level of Hispanic board representation immediately following an election affects outcomes Y over the next $\tau = 1, ..., T$ periods. In other words, I do *not* estimate the contemporaneous effect of school board composition in period τ on outcomes in period τ—nor do I want to, since those estimates will capture effects of policies enacted by prior school board members.

In the first stage, I compare post-election Hispanic school board representation from districts that did and did not have a top-tier Hispanic candidate on the ballot. In the second stage, I use my first-stage estimate of differences in Hispanic board representation (driven by top-tier Hispanic candidates) to infer the average effect of greater Hispanic school board membership on district outcomes over the T years after the election. Consistent with my goal of identifying within-district effects, I estimate this model using both my school- and district-level data sets; for conciseness, I only describe the school-level model here, but the district-level model operates similarly.[36]

35. One might be concerned that these null results follow entirely from the inclusion of district fixed effects in my specification. In Appendix Table A5, I show that I reach the same conclusion using an alternative specification in which I omit district fixed effects. Though I find a slight correlation between my instrument and Black enrollment, the estimate is both small, imprecisely estimated, and unaccompanied by any effect on other enrollment measures. The largely null results confirm that my identification strategy does not depend on district fixed effects.

36. The primary difference between the school- and district-level specifications is that I use school-level fixed effects in the former and district-level fixed effects in the latter. The other, more subtle distinction concerns the first stage: the district-level model will estimate the first stage specification (which, again, is cross-sectional and does not vary with respect to τ) $T + 9$ times, once for each district-by-period (see below where I note that I include 8 pre-election periods, plus period 0); the school-level model will estimate the first

For each school s in district i, I estimate the following specification:

$$\begin{aligned} HispBoardShare_{it} \times post_{\tau} &= \pi_0 + \pi_1 HispTopTier_{it} \times post_{\tau} + post_{\tau} \\ &+ \rho_{\tau} + \lambda_s + \gamma_t + \psi_a + \pi_2 X_{it\tau} + \epsilon_{1,ista\tau} \\ Y_{ista\tau} &= \beta_0 + \beta_1 HispBoardShare_{it} \times post_{\tau} + post_{\tau} \\ &+ \rho_{\tau} + \lambda_s + \gamma_t + \psi_a + \beta_2 X_{it\tau} + \epsilon_{2,ista\tau}, \end{aligned} \tag{3}$$

where *post* is an indicator for whether the period $\tau > 0$; t denotes an election year; and a denotes an academic year.[37,38] The vector X includes the share of Hispanic candidates and the number of contested seats in the election, which I include to identify my first stage effect π_1, following the discussion above. I index X by τ to allow election covariate effects to vary across periods.

I estimate the model using observations from $\tau = -8, ..., T$. I include pre-election data from periods $\tau \leq 0$ to identify school, election year, period, and academic year fixed effects (λ, γ, ρ, and ψ, respectively). Those fixed effects do not aid with identification—rather, I include them to improve the precision of my estimates.[39] I cluster my standard errors at the district level since treatment statuses are correlated across time within districts.

stage $(T+9) \times s$ times, once for each school-by-period. Note that the district- and school-level specifications yield very similar first stage coefficients, which suggests this re-estimation does not bias my results (compare the first stage estimates given in Tables 7 and 8, for example).

37. Academic years are a function of the election year t and the period relative to the election, τ. For example, if I observe an election in November 2005, the election year is set to 2005, which is the academic year 2006. The first post-period ($\tau = 1$) is academic year 2007, the second ($\tau = 2$) is academic year 2008, and so on.

38. This fixed effects specification with pre-election data is comparable to that of Beach and Jones (2017), Cellini et al. (2010), and Shi and Singleton (2019).

39. In Appendix Table A9, I show that my results are robust to using school-by-election fixed effects or district fixed effects instead; I also show that I obtain significant, though quantitatively smaller results when I exclude school- or district-level fixed effects altogether. In Appendix Table A8, I show what happens when I omit both pre-period data and fixed effects. Quantitatively, the magnitudes and standard errors of my estimates of effects on student achievement and teacher composition change once I exclude both fixed effects and pre-election data; spending data are quite robust. That is to be expected: omitting fixed effects and pre-election data will fail to correct for the fact that I observe district- and school-academic years multiple times, oftentimes as both "treated" and "untreated" observations. Consequently, the post-election data will reflect not only the impact of the focal election, but prior contests as well, which I ought to control for to recover a meaningful treatment effect. Once I include pre-period data, I obtain qualitatively similar results, albeit smaller than in my preferred specification and less precisely estimated.

Intuitively, by limiting my first stage to post-election Hispanic board representation, I assume that there are no confounding differences in pre-election Hispanic board share, consistent with Table 5. As such, the first stage coefficient, π_1, delivers the regression-adjusted, cross-sectional difference in post-election Hispanic school board representation between districts that did and did not have a top-tier Hispanic candidate. The 2SLS coefficient of interest, β_1, then captures how the level of Hispanic school board representation causally affects the outcome Y on average over the T years after an election.

4.4.1 Relevance of the First Stage

It remains for me to show that the first stage described in Equation 3 generates meaningful variation in Hispanic school board representation, my treatment of interest. To make this case, I leverage my school-level panel and estimate a representative first stage using my full sample of observations spanning the eight years before and after an election.[40] Table 6 presents these results. The causal parameter in the first row suggests that having a top-tier Hispanic candidate on the ballot raises Hispanic school board representation by 8.6 percentage points (48 percent) immediately after the election.[41]

While I focus exclusively on Hispanic board representation in this paper, given its straightforward interpretation, other measures of Hispanics' presence on a board could also be of interest, and so I highlight alternative first stage outcomes in Table 6 as well. I find that top-tier Hispanic candidates lead to a 10 percentage-point higher probability of any Hispanic member sitting on the board, another treatment that could in itself affect decision-making. This finding helps allay concerns that non-linearities in my preferred treatment—the marginal Hispanic victory may not affect boards with already-high Hispanic representation—sway my results: the binary "any Hispanic" treatment is by definition monotonic, is strongly correlated with my instrument, and will yield a comparable Wald estimator as my preferred

40. This sample composition is identical to the "All Schools" sample described in Table 10. The samples used in Tables 8 and 9 differ due to constraints on post-election timing and missing test score data, respectively, both of which result in slightly different first stage coefficients than the ones presented here.

41. To put this number in perspective, on the modal 5-seat school board, a new Hispanic member will increase Hispanic representation by 20 percentage points.

"Hispanic representation" treatment. Similarly, I find an 8.8 percentage point increase in the probability of a Hispanic majority on the school board, although the F-statistic for this model is low, likely due to the rarity of these majorities. All of these estimates confirm that top-tier Hispanic candidates lead to meaningful increases in Hispanic school board presence.

4.4.2 Estimating the Reduced-Form Effect of Top-tier Hispanic Candidates

I supplement my 2SLS model with a reduced-form framework, which evaluates the effect of a top-tier Hispanic candidate on post-election district outcomes. Admittedly, because this framework removes the "structural" dimension of my 2SLS approach, the magnitudes of the reduced-form coefficients will not be very meaningful in themselves. But, in view of the strong first stage relationship apparent in Table 6, I use the reduced-form model to highlight overall patterns of outcomes (rather than levels) that I attribute to having more Hispanic members on the school board, in whatever capacity.

As with my 2SLS design, my reduced form specification uses a panel approach in which I include pre- and post-election observations and interact the treatment assignment (the top-tier Hispanic candidate indicator, *HispTopTier*) with an indicator for post-election periods:

$$Y_{ista\tau} = \delta_0 + \delta_1 HispTopTier_{it} \times post_\tau + post_\tau + \rho_\tau + \lambda_s + \gamma_t + \psi_a + \delta_2 X_{it\tau} \epsilon_{ista\tau}. \tag{4}$$

I once again include school, election year, period, and academic year fixed effects (λ, γ, ρ, and ψ, respectively), as well as controls for the share of Hispanic candidates and the number of contested seats. The coefficient of interest, δ_1, captures the causal effect of having a Hispanic candidate randomized to the top tier of the ballot on the average outcome Y over the T. periods following the election.

4.5 Estimating the Dynamic Effect of Top-tier Hispanic Candidates

Both my 2SLS design and corresponding reduced form model capture average outcomes over post-election periods. While parsimonious, this approach obscures the dynamic effects of school board ethnic composition on district outcomes. For example, any student performance effects may take time to materialize, in which case the average post-election treatment effect will appear small, even when the treatment effect in the medium term is substantial.

I employ an "event study"-type specification to explore such dynamic effects, as well as to test for the presence of confounding pre-election trends in my outcome variables. As with my reduced-form specification, this event study approach estimates the "top-tier Hispanic" effect, not the impact of Hispanic school board representation per se. As such, I use this framework to qualitatively illustrate how Hispanic school board members' impact on district outcomes evolves over time.

I estimate my dynamic model using the same school-level panel data set as in my 2SLS and reduced form specifications, but now I treat every school-by-election as a separate "event." The specification is a pooled regression, including observations from periods $\tau = -8$ through $\tau = 8$, that isolates period-specific treatment effects:

$$\begin{aligned} Y_{esa\tau} =& \delta_0 + \sum_{j=-8}^{j=8} \Big(\delta_\tau HispanicTopTier_e \times \mathbb{1}(\tau = j) \Big) \\ &+ \kappa HispanicTopTier_e + \rho_\tau + \psi_a + \theta_{e\times s} + \epsilon_{esa\tau}, \end{aligned} \tag{5}$$

where e refers to an election and $e \times s$ refers to a school-by-election. By including school-by-election fixed effects, θ, and a dummy denoting elections with top-tier Hispanic candidates, I measure changes in school outcomes relative to their election-year levels.[42] That is, each event-study coefficient δ_τ reflects the change in Y between the election year (period 0) and period τ attributable to a top-tier Hispanic candidate. Note that because I incorporate school-by-election fixed effects (which control for all observable and unobservable election

42. I do not include school-by-election fixed effects in any other specification because these fixed effects will preclude a 2SLS approach, as the first stage of that specification relies on cross-sectional variation at the election level. Rather, to estimate the effect of Hispanic representation, I prefer to use school fixed effects, as discussed above, and I only introduce (potentially more intuitive) school-by-election effects in this event study (reduced-form) model. Full reduced-form results using school-by-election fixed effects appear in Appendix Table A9, and show precisely-estimated results similar to those I uncover using my preferred specification.

characteristics), I do not have to separately control for the share of Hispanic candidates and number of contested seats in the election. To this extent, the dynamic effects model also functions as a robustness check for my main specification.

5 Impact of Hispanic School Board Representation on SFP Spending

I begin my empirical analysis by establishing how Hispanic representation affects SFP spending along the extensive margin—the total amount of SFP funds the school board invests district-wide. The effect of having more Hispanic school board members on SFP investment is theoretically ambiguous. Beach and Jones' (2017) findings suggest ethnic diversity on a school board may increase discord and reduce total spending; on the other hand, Hispanic board members may take up additional funds to renovate or build more schools to benefit marginalized students that their non-Hispanic counterparts pass over.

5.1 District-level Regression Analysis

Using my top-tier Hispanic instrument, I estimate the causal effect of Hispanic school board membership on district-wide SFP spending. Since most school board members serve four-year terms, I focus on spending during the four years after an election.[43] I include as outcomes SFP spending by project type (modernization, new school construction, or supplemental), as well as total SFP spending. My results appear in Table 7.

Reduced-form coefficients in columns 2-4 reflect the impact of a randomly-assigned top-tier Hispanic candidate on average SFP spending outcomes over the four years after an election. The specification follows Equation 4. Point estimates in the first row reveal that annual SFP modernization spending per student increases by 26-29 percent relative to the post-election mean ($30-34 per student) for districts with a top-tier Hispanic candidate on the ballot. These effects are precisely estimated and robust to including additional election

43. Appendix Figure A2 shows that modernization spending effects largely dissipate by the fifth year after the election. The figure also provides evidence that, in the lead-up to the election, districts with and without top-tier Hispanic candidates do not have systematically different levels of SFP modernization spending. Spikes in modernization spending likely reflect the fact that projects receive state approval in bunches, which tend to occur semi-annually and coincide (particularly in the early 2000s) with new bond issues (see Section 2).

and district demographic controls. I find no appreciable effects on new construction or supplemental SFP spending.[44] While imprecise, point estimates indicate that total SFP spending increases by roughly the same amount as modernization spending.

2SLS estimates, presented in columns 5 and 6 of Table 7, capture the effect of Hispanic representation per se on SFP spending. The IV specification follows Equation 3. Each coefficient captures how a 100 percentage-point increase in Hispanic school board representation immediately after the election would affect the given outcome. But my preferred interpretation considers how a 20 percentage point increase in Hispanic board representation—equivalent to substituting one Hispanic for one non-Hispanic member on the modal board—influences SFP spending. Second-stage coefficients in the first row imply that a 20 percentage-point increase in Hispanic representation causes the board to raise SFP modernization spending by $71 per pupil per year, or 61 percent of the sample mean. Again, point estimates in the fourth row suggest that total SFP spending rises by a similar amount, although these coefficients are imprecisely estimated.

5.2 Robustness of District-level Spending Results

I perform two robustness checks to confirm that the results presented thus far capture causal effects. First, I address the possibility that districts with top-tier Hispanic candidates also have greater SFP eligibility, which would mechanically lead to a positive correlation between SFP spending and my treatment assignment. Reassuringly, reduced-form results in Appendix Table A7 show no correlation between whether a top-tier Hispanic candidate appears on the ballot and various proxies of SFP eligibility criteria.[45]

44. That Hispanic board composition only affects modernization spending is not surprising. In Appendix Table A1, I show that 75 percent of all SFP projects involve a modernization component, which suggests these grants are the most popular investments for school boards to make. Whereas brand new school construction is costly, and requires a suitable location within the district to put a new facility, modernization projects improve existing sites, which might limit their pecuniary and non-pecuniary costs. Furthermore, school boards have little sway over which supplemental grants they can access, since those largely depend on pre-existing district characteristics (for instance, susceptibility to earthquakes). All of these features likely make modernization the most attractive project type for school boards and the one most responsive to school board composition.

45. Specifically, I use district cumulative SFP spending (which captures potential eligibility that has been exhausted); total enrollment (which approximates an enrollment projection); full-time equivalent teachers

Second, I check that my estimator captures just the effect of Hispanic candidates—and not, for example, the effects of incumbent re-elections or elections of Democratic candidates. Based on results in Appendix Table A7, I find no evidence that top-tier Hispanic candidacies are correlated with other election outcomes, which supports my claim that my results only capture Hispanic school board members' influence.

6 How Do Hispanic School Board Members Allocate SFP Funds?

To help explain why Hispanic board members invest more in SFP modernization projects, as I show in the previous section, I analyze how school board ethnic composition affects the distribution of SFP funds within a district. I hypothesize that school board elections follow a "core-constituency" voting model in which a candidate garners support from voters of her own ethnic background. Thus, Hispanic board members might invest the marginal SFP dollar in schools with large shares of Hispanic students in order to cater to nearby Hispanic voters; they take up additional SFP funds in order to pay for those marginal projects. If, on the other hand, school board elections hinge on the median voter, then Hispanic board members may invest the marginal dollar without considering schools' demographics, in which case I would see no correlation between school demographics and SFP allotments. These competing hypotheses have contrasting implications for the role of minority voices on school boards, and the potential for minority officials to reallocate district resources to benefit minority students.

6.1 School-level Regression Analysis

Using my school-level panel data set, I assess how Hispanic officials allocate the marginal dollar within school districts. Specifically, over the four years after an election, I quantify the

(a proxy for the number of classrooms in the district); and facility age. Note that my data on school facility ages are poor: the CDE assigns a default opening date of July 1, 1980 for all schools built prior to that date (67 percent of schools ostensibly opened that day). I therefore include an alternative measure of school age—the share of district schools listed as having opened in 1980—and again find no evidence of a correlation with my top-tier Hispanic indicator. Since the SFP only began in 1998, it is reasonable to assume that most schools with the placeholder opening date are at least 25 years old during my sample period, which means all these schools are eligible for SFP modernization funds.

impact that Hispanic board members have on modernization and total SFP spending among schools that have relatively "high" and "low" shares of Hispanic and impoverished students within a district (which I define below). I describe patterns of intra-district SFP spending by estimating the reduced-form model given in Equation 4, which efficiently conveys which types of school within "treated" districts receive relatively more SFP funds. Likewise, I use the 2SLS model given in Equation 3 to quantify how Hispanic school board representation per se affects spending levels across schools within a district.

I first show that my results from Section 5 translate into comparable effects at the school level by estimating my reduced-form and IV models using the full sample of schools. Every specification includes controls for election characteristics, school demographics, and election year and academic year fixed effects. I also use school enrollment weights to facilitate comparisons in per-pupil spending outcomes across schools. The results appear in the second column of Table 8. Estimates in the first panel show that top-tier Hispanic candidates lead to 30 percent ($51 per student) more SFP modernization spending, and a statistically insignificant 14 percent more total SFP spending ($59 per student), on the average school relative to the post-election mean. IV results in the second panel imply that a 20 percentage-point increase in Hispanic board representation leads to 58 percent ($100 per student) more SFP modernization spending on average across all schools in a district. These findings are similar to the district-level results shown in Table 7.

6.2 Do Hispanic School Board Members Invest in Hispanic Students?

I now consider whether schools boards with more Hispanic members invest in schools within the district that have high Hispanic enrollment, which I interpret as investment intended for Hispanic students. To distinguish "high Hispanic" schools within a district, I first compute the median Hispanic enrollment share among all schools by district-year and then divide schools into two types: those with above-median Hispanic enrollment within a district, and those with below-median Hispanic enrollment within a district.[46] I then separately

46. Schools with exactly-median Hispanic enrollment are implicitly dropped; including those median schools in either the above-median or below-median subsamples has no bearing on my results.

estimate reduced-form and 2SLS spending effects for schools that have high (above-median) and low (below-median) Hispanic enrollment within their districts, using the specifications described above.

The results from those regressions appear in the third and fourth columns of Table 8. Each reduced-form coefficient in the first panel captures how a top-tier Hispanic candidate (in the district-wide election) affects the level of SFP spending per student among schools of the given type. The estimates indicate that students in schools with above-median Hispanic enrollment within a district receive $56 in additional modernization funds (33 percent), whereas schools with below-median Hispanic enrollment receive a marginally significant $36 per student. Similarly, schools with above-median Hispanic enrollment within their district receive $73 more total SFP funds per student (17 percent), while those with below-median Hispanic enrollment receive a statistically insignificant $32 per student.[47]

2SLS coefficients in the second panel of Table 8 underscore the magnitude of this difference. IV estimates imply that a 20 percentage-point increase in Hispanic board representation raises total SFP spending by a marginally significant $146 per student, and SFP modernization spending by $113 per student, among high-Hispanic schools within the district. Imprecise estimates point to considerably smaller increases of $64-$70 per student among schools with relatively low Hispanic enrollment. This comparison indicates that Hispanic voices on the school board substantially increases relative spending on Hispanic students.[48]

47. The data structure precludes a formal test to determine whether these estimates are statistically different. Since each school only appears in one subsample at a time (not by design, but because relative Hispanic enrollment is quite sticky over the sample period), I cannot test for equality using my school fixed effects approach. Still, in Appendix Table A10 I present results from an alternative "pooled" specification to argue the differences in spending across school types are meaningful. That model resembles Equation 4 but includes an indicator for whether a school is high-Hispanic, and interacts that term with my top-tier Hispanic dummy. The coefficient on that interaction term denotes the treatment effect of a top-tier Hispanic candidate among high-Hispanic schools relative to the treatment effect on low-Hispanic schools.

48. See Appendix Figure A5 for event study estimates of SFP spending across high- and low-Hispanic schools. The figure shows no consistent evidence of pre-election trends, although the data are noisy, in part because SFP investments are cyclical, responding to increases in fund availability following new statewide bond approvals (see Section 2 and a similar footnote in Section 5 above). Post-election data point to relatively sustained increases in modernization spending among high-Hispanic schools over the eight years following an election, with especially significant increases in the four years after the election.

6.3 Do Hispanic School Board Members Invest in Disadvantaged Students?

In the remaining columns of Table 8, I consider how the treatment effect varies by school poverty level. I divide schools within a district-year into those with above- and below-median shares of FRL-eligible students, as well as those schools that do and do not qualify for Title I funding. Once again, I separately estimate the reduced-form impact of a top-tier Hispanic candidate, and the 2SLS impact of Hispanic board representation, on SFP spending by school type, and compare the resulting coefficients.

I find that top-tier Hispanic candidates lead to $65 more modernization spending per student among schools with high FRL-eligible student enrollment, compared to $37 more per student on schools with low FRL-eligible enrollment. 2SLS estimates suggest these differences in spending by school poverty level are large. Each Hispanic board member raises total SFP spending by $152 per student at high-FRL schools and $178 at Title I-eligible schools (versus $72 more and $62 *less* spending at low-FRL and non-Title I schools, respectively). Given the strong correlation between Hispanic enrollment and school poverty level, I interpret these results only as a further indication that Hispanic board members direct funds towards marginalized students in general.[49]

6.4 Robustness of School-level Spending Results

In Appendix Table A11, I probe the robustness of these school-level results. First, I consider whether SFP spending across school types differs prior to the election—in other words, whether there are differential pre-election trends at the school level.[50] Importantly, I find that school-level SFP spending over the three years prior to the election is uncorrelated

49. Specifically, 77 percent of high-Hispanic schools are also high-FRL and 85 percent are Title I-eligible. These strong correlations would complicate any cross-type analysis (e.g., the effect on high-FRL/high-Hispanic schools).

50. Note that I employ district-level fixed effects in this specification, not school-level effects (Appendix Table A9 shows that the choice of fixed effects makes little difference for my main findings). I make that choice so I can easily compare coefficients across sub-samples without having to rely on my pooled specification described below Appendix Table A10; p-values from chi-squared tests of equality appear in every third column. I do not use this procedure to test for equality of my main treatment effects because, in that case, the pooled specification provides a more reliable indication of which schools drive my results. By contrast, in this robustness test, I am essentially comparing regression-adjusted, cross-sectional sample means.

with whether a Hispanic candidate will appear in the top tier of the future ballot (which follows from a similar result in the district-level data, shown in Appendix Table A7).

I also check that different school types have similar SFP eligibility. In particular, I want to ensure that an above-median Hispanic schools do not have greater SFP-eligibility than below-median Hispanic schools in treated districts. Using election-year data, I estimate the correlation between the top-tier Hispanic candidate indicator and my proxy measures for SFP eligibility shown in Appendix Table A7. I find no evidence that my treatment assignment indicator is correlated with these measures of SFP eligibility among any school type.[51]

A related concern is that my above- and below-median school types are not comparable across treated and control districts. For example, schools with above-median Hispanic enrollment in the treated group may have systematically higher Hispanic enrollment than above-median Hispanic schools in the control group. Although I find that above-median Hispanic and FRL-eligible schools do have higher Hispanic enrollment in treated districts, these differences amount to at most 1.5 percent of the sample mean. I find no other demographic differences that would bias my estimates. Altogether, these null results indicate that my sample delineations are not themselves driving my results, and support a causal interpretation of my intra-district spending results.

7 Hispanic School Board Representation and Student Achievement

My analysis thus far has established how Hispanic school board members raise district spending on minority students. It remains to be seen whether minority representation on a school board affects students' academic performance, either directly or via SFP spending; these student outcomes are of first-order interest to both policymakers and related research on education finance. As such, in my final analyses, I assess how Hispanic board members affect within-district test scores in the years following an election.

51. I find some evidence that non-Title I schools in treated districts are older than non-Title I schools in untreated districts. But, if anything, that means my point estimate for non-Title I schools is upward-biased (and it is already effectively zero).

7.1 School-level Regression Analysis

I estimate the impact of Hispanic school board representation on school-level math and reading (ELA) scores. I consider a medium-term horizon—the eight years after an election—to allow sufficient time for test score effects to materialize. Reduced-form and 2SLS results appear in Table 9. The reduced-form specification is Equation 4, with additional controls for election characteristics as indicated, while the 2SLS specification is Equation 3 with full election controls.

The first panel indicates that top-tier Hispanic candidates lead to significant gains in math achievement among high-Hispanic schools. Reduced-form results suggest these schools see math scores improve by 0.04 standard deviations on average over the eight years after an election, while low-Hispanic schools improve by an insignificant 0.02 standard deviations.[52] Imprecise point estimates support a similar conclusion for ELA scores. 2SLS coefficients in the second panel imply that a 20 percentage-point increase in Hispanics' share of school board seats improves math test scores by 0.08 standard deviations at high-Hispanic schools over the eight years after an election, and by a statistically insignificant 0.05 standard deviations at low-Hispanic schools. This gap is not large, but it does indicate test score gains are being driven by improvement at high-Hispanic schools.[53]

7.2 School-level Event Study Analysis

Since test score effects appear over time, the estimates in Table 9 likely understate the true effect of Hispanic board representation on student achievement. To document how these student achievement effects play out over time, I use Equation 5 to illustrate the dynamic

52. See Appendix Table A9 for estimates across different types of schools besides high-/low-Hispanic. I find comparable results when I compare high- and low-FRL schools, but less economically and statistically meaningful results when I compare Title I and non-Title I schools. That latter result likely stems from the fact that most schools qualify for Title I in my sample, whereas Hispanic board members target funds towards only those neediest, highest-minority schools within the district. On average, then, Title I schools show no gains.

53. As I note in the previous section, I cannot formally test equality of my estimates due to my fixed effects specification and data structure. But in Appendix Table A10, I show some evidence that the reduced-form gap is significant using a pooled specification in which I interact my top-tier Hispanic indicator with an indicator for whether a school is high-Hispanic.

effect of a top-tier Hispanic candidate on student performance.

Figure 1 visualizes these dynamic test score effects. Importantly, I find no evidence of differences in math scores prior to the election, which once more supports my claim that the estimates deliver causal effects. I do find that ELA scores among high-Hispanic schools are lower in districts with top-tier Hispanic candidates during the year prior to the election, though it is not clear what is causing this pre-election difference. Reassuringly, the effect is exclusive to ELA scores among high-Hispanic schools, and the fact that only one coefficient (out of twenty-four pre-period coefficients tested here) shows significant pre-treatment effects suggests that this significant correlation might be due to chance.

After the election, gains in math scores become apparent within two years. The visual evidence suggests that the eight-year average reduced-form effect of around 5 percent of a standard deviation (see Table 9) masks substantial improvements of up to 20 percent of a standard deviation 6-8 years after the election among high-Hispanic schools. ELA scorse—the problematic pre-election coefficient notwithstanding—show smaller but positive effects of up to 0.10 standard deviations at high-Hispanic schools. That these gains in achievement persist over time suggests that Hispanic board members' influence outlasts their tenure on the school board. Furthermore, the fact that I find test score effects only among high-Hispanic schools mirrors my SFP spending results, and reinforces my conclusion that minority board representation primarily benefits minority students.

Altogether, these results point to an economically meaningful reduced-form effect on student achievement. The effects documented here appear large, yet the post-election mean is -0.17 standard deviations for math and -0.32 standard deviations for ELA scores (see Table 9). The substantial impact on student performance, then, means that an additional Hispanic board member leads to improvement upon a low baseline. Moreover, the coefficients shown here will implicitly capture cohort effects: a student in kindergarten in period 0 will have the most exposure to Hispanic board members' policies, while students in higher grades will have less exposure, and will matriculate out of the sample. Therefore, math scores' upward trajectory reflects not only accumulating gains from district policies but also increasing sample selection in favor of students with long tenures in the district. Given the limited data at my disposal, I do not attempt to separately identify these cohort effects, but I note that they are part and parcel of the Hispanic representation effect that I want to estimate, and support my overall conclusion that minority representation on the school board promotes minority student achievement.

7.3 Exploring the Mechanism

Given my policy context, the mechanism behind these achievement gains is especially difficult to disentangle. My claim throughout this paper has been that Hispanic board members devote greater resources to Hispanic students. I provide evidence to that effect using SFP spending data, but since many policy shifts likely occur at once, and I do not directly observe *which* resources (aside from SFP funds) the school board chooses to expend on high-Hispanic schools, the exact cause for this uptick in achievement will remain elusive.

In light of this limitation, I discuss four straightforward mechanisms that help clarify the roles of the SFP on student performance as well as non-SFP policy shifts prompted by Hispanic school board members. I first discuss the extent to which new facilities themselves improve student test scores. I then present new results showing how Hispanic board members reduce teacher churn among high-Hispanic schools. Finally, I present null results showing no effects on overall district budgeting and no student sorting within, into, or out of the school district. I conclude that capital improvements likely raise test scores, but that Hispanic board members, as hypothesized, pursue myriad investments that benefit high-Hispanic schools.

7.3.1 Mechanism: Does SFP Investment Raise Test Scores?

I first analyze the extent to which the direct and indirect effects of school facility investments explain my findings. To do so, I compare test scores across schools that do and do not begin an SFP modernization project after the focal election. I use my event study framework given in Equation 5, but include only four years of pre-election data, since most SFP projects begin early in the sample period and have few years of pre-construction data.

Event study coefficients, plotted in Appendix Figure A6, indicate that schools with a post-election modernization project exhibit greater gains in student performance those without additional SFP investment. At the same time, schools that did not receive modernization funds show improvement—albeit slight—in math scores post-election. This comparison points suggest that my results partly capture positive returns to new school facilities, but also that achievement increases independent of SFP investment.

7.3.2 Mechanism: How Does Hispanic Board Representation Affect Teachers?

I next analyze how Hispanic board representation affects teachers within the district. Teachers represent the most costly and high-profile educational input, and changes in teacher composition, particularly among high-Hispanic schools, would provide further evidence that increasing Hispanic board representation ushers in new policies. Using anonymous, individual-level teacher data from the CDE, I construct four school-level outcomes: average full-time equivalent (FTE) experience; average FTE tenure in the school district; the number of "new hires" (teachers with ≤ 1 year of tenure in the district); and the share of Hispanic teachers.

Using Equation 5, I examine how teachers' experience and tenure evolve after the election. I find that top-tier Hispanic candidates lead to increases in teacher tenure and experience of up to half a year in the medium term (five years after the election) at high-Hispanic schools, with much smaller and imprecisely estimated gains at low-Hispanic schools.[54] Point estimates in Table 10 indicate that an additional Hispanic board member raises average teacher experience by 0.38 years (2.7 percent of the post-election mean) and average teacher tenure by 0.48 years (4 percent) at high-Hispanic schools. Likewise, I find that these schools have 0.59 (17 percent) fewer newly hired teachers. The data also suggest that, when more-Hispanic school boards do hire new teachers, they tend to hire Hispanics, manifest in a 1.2 percentage-point increase in the share of Hispanic teachers across all schools (6 percent).

These facts support the conclusion that Hispanic board members reduce teacher churn

54. An important caveat is that I find some evidence of differential pre-trends three years prior to the election across all schools; it is unclear what is driving this effect, although I note that no other pre-period coefficients are statistically different from zero.

at high-minority schools and favor hiring Hispanic teachers. That effect likely explains a non-trivial share of the test score effects I observe: per Ronfeldt, Loeb, and Wyckoff (2013), teacher turnover has a large impact on student achievement among high-minority schools. Since underprivileged schools witness especially high turnover, this effect suggests Hispanic board members help alleviate a structural hurdle preventing low-performing schools' advancement. Equally, these findings provide indirect evidence that Hispanic school board members give other pecuniary or non-pecuniary incentives to teachers in these schools to encourage retention—in other words, these results provide further evidence that minority board members invest in minority students.

7.3.3 Mechanism: Do Students Re-sort Within or Between Districts?

I return to my district-level data set to assess potential sorting mechanisms. In-migration could increase test scores if the election or new facilities entice higher-ability students into the district. Appendix Table A12 shows a marginally significant increase in white enrollment share and concomitant decrease in Hispanic enrollment share. The demographic shift is not economically large, amounting to around 1 percent of the sample mean for white enrollment.

I also evaluate the degree of intra-district sorting, which could improve test scores if, for example, high-ability students switch into modernized schools from elsewhere in the district, and positive peer effects improve overall student achievement. As a measure of within-district sorting, I use intra-district dissimilarity indices, a common heuristic to quantify student segregation across schools.[55] Intuitively, if students re-sort within a district—for example, if white students concentrate in renovated schools—then the relevant dissimilarity index would be affected (in my example, the dissimilarity index for white enrollment would increase). Appendix Table A12 shows no evidence of this intra-district sorting response.

55. The general formula for a dissimilarity index is as follows: $\frac{1}{2}\sum_s \mid \frac{z_s}{Z} - \frac{n_s - z_s}{N-Z} \mid$, where s denotes schools within a district, z is the number of students at a school with a given demographic characteristic (e.g., Hispanic students), N is total enrollment in the district, and Z is the total enrollment of type z in the district (e.g., the number of Hispanic students in the district).

7.3.4 Mechanism: Do Hispanic Board Members Reallocate General Funds?

Finally, I analyze how top-tier Hispanic candidates affect districts' non-SFP budget outcomes. It could be that Hispanic candidates manage to raise revenues, potentially by securing other state transfers, or reallocate existing spending across budget priorities, which could improve the return on the marginal dollar.[56] I also estimate changes in student-teacher ratios as a measure of teacher hiring and class sizes. Event study estimates, plotted in Appendix Figure A7, show no relationship between the top-tier Hispanic indicator and subsequent district budget outcomes or student-teacher ratios. In this setting at least, school board ethnic composition does not appear to affect districts' overall budgets.[57]

8 Concluding Discussion

The well-known racial gap in education achievement coexists alongside a racial gap in education finance. State and federal efforts to remedy this imbalance have been unsuccessful at reducing intra-district disparities in white and nonwhite students' performance (Cascio et al. 2013; Lafortune et al. 2018). Researchers have posited that school boards lie at the heart of this discrepancy, but have not previously linked school board composition to intra-district gaps in spending and student achievement.

My analysis confirms that school boards play an integral role in education finance. Using spending data from the SFP, I find that an additional minority (Hispanic) school board member increases spending on school renovations using state transfer grants. In particular, SFP spending on high-Hispanic and high-poverty schools within the district increases by up to 66 percent, which juxtaposes smaller, insignificant changes among low-Hispanic and relatively affluent schools. These findings corroborate indirect evidence, presented in Cascio

56. I use district-wide budget data from the Census of Governments. I include data on total district expenditures and revenues, along with spending on teachers' salaries (which comprises 35 percent—a plurality—of total spending). For each district-year, I divide all three outcomes by total enrollment to obtain spending per pupil, and winsorize at the first and ninety-ninth percentiles.

57. Given the limitations that California school boards face from Proposition 13, it is not surprising that total district revenues do not depend on school board composition. It is possible that in another setting minority representation could affect tax policy and total district spending, but I leave this question for future research.

et al. (2013), Gordon (2004), and Hyman (2017), that school board composition matters for the take-up of state transfers. By extension, the efficacy of "carrot" policies meant to incentivize local school boards to address inequality clearly depends on alignment between state and local politics. Whereas state legislators pass financial reforms and spending bills meant to bridge gaps between rich and poor, white and nonwhite students, this paper suggests that the modal (white) school board member responds to political incentives which discourage investment in marginalized students.

Moreover, these differences in investments translate into differences in student achievement. A combination of SFP spending, implicit incentives to encourage teacher retention, and (likely) other, unobservable extra inputs into high-Hispanic schools result in moderate gains in student test scores. These results re-affirm that spending on traditionally disadvantaged students can maximize the returns to the marginal education dollar.

This article contrasts with prior research on education finance and the racial achievement gap by focusing on how local school board politics can perpetuate unequal outcomes. Building upon the nascent school boards literature, the results in this study link school board politics with changes in intra-district spending patterns and improvement in student outcomes for the first time. More broadly, the education setting provides an apt context to see how minority voices in local government can improve the flows of public resources to minority constituents. These findings provide room for further investigation of the role school boards play in the United States' federalized education system, with a particular emphasis on how local officials shape the practical effects of statewide or even national policies. Overall, my analysis suggests that the success of state and federal redistribution initiatives relies on school boards' cooperation—and that a lack of minority political representation could stymie future top-down initiatives to improve racial equity in education.

Table 1: Summary of California School Board Elections, 1996-2007

	Mean	Median	Std Dev	Min	Max
I. Elections Data					
# Seats	2.15	2	0.84	1	7
# Candidates	4.24	4	2.02	1	28
# Incumbents	1.53	1	0.98	0	5
# Ballots Cast	17,733	6,531	31,934	15	319,754
Election Margin	0.09	0.05	0.11	0.00	0.81
Election Year	2001	2001	3.40	1996	2007
Ward Election?	0.26	0	0.44	0	1
II. Hispanic Candidate Prevalence					
# Hisp Candidates	0.66	0	1.11	0	9
Share Hisp Candidates	0.16	0	0.25	0	1
Any Hisp Candidate?	0.38	0	0.49	0	1
Share Hisp Winners	0.16	0	0.31	0	1

The sample contains 3,738 school board elections held between 1996 and 2007. The election margin reflects the difference in vote shares between the marginal winning candidate and marginal losing candidate.

Table 2: Summary of Election-Year District Demographics and Student Achievement

	(1) All Elections	(2) Elections w/o Hisp	(3) Elections w/ Only Hisp	(4) Elections w/ Both Hisp and non-Hisp
I. Election-Year District Demographics				
Total Enrollment	8,718	7,291	16,396	10,479
	(16,342)	(12,856)	(60,158)	(12,658)
Share White	0.46	0.58	0.10	0.30
	(0.28)	(0.24)	(0.13)	(0.24)
Share Hispanic	0.38	0.26	0.78	0.54
	(0.27)	(0.21)	(0.21)	(0.26)
Share Asian	0.08	0.07	0.07	0.08
	(0.10)	(0.10)	(0.09)	(0.11)
Share Black	0.05	0.04	0.04	0.05
	(0.07)	(0.07)	(0.07)	(0.07)
Share FRL-eligible	0.47	0.39	0.74	0.56
	(0.26)	(0.24)	(0.20)	(0.25)
N:	*3,155*	*1,913*	*92*	*1,150*
II. Election-Year Student Achievement				
Composite Math Score	-0.02	0.22	-0.79	-0.35
	(0.91)	(0.91)	(0.68)	(0.80)
N:	*2,962*	*1,795*	*85*	*1,082*
Composite ELA Score	0.04	0.29	-0.87	-0.30
	(0.97)	(0.94)	(0.69)	(0.88)
N:	*3,251*	*1,968*	*92*	*1,191*

The table describes the mean of demographic characteristics and student achievement outcomes listed in the left-hand column. Standard deviations appear in parentheses. The unit of observation is a district-election. I divide the sample based on the share of Hispanic candidates on the election ballot: column 1 describes all district-elections that I successfully match to enrollment or test score data; column 2 describes elections without any Hispanic candidates; column 3 describes elections with only Hispanic candidates; and column 4 describes elections with both Hispanic and non-Hispanic candidates. Sample sizes vary within column because of missing test score and demographic data, as discussed in the text.

Table 3: Summary of District-year SFP Spending Outcomes, by Hispanic School Board Representation

		Hispanic School Board Representation			
	(1)	(2)	(3)	(4)	(5)
	Full Sample	No Hisp Members	(0%, 20%]	(20%, 60%]	(60%, 100%]
Total SFP Spending per Pupil	352	365	340	297	303
	(1196)	(1244)	(1127)	(962)	(1036)
Modernization Spending per Pupil	99	105	86	83	70
	(382)	(403)	(321)	(322)	(296)
New Constr. Spending per Pupil	44	44	47	41	56
	(226)	(231)	(221)	(182)	(245)
Supplemental SFP Grants per Pupil	190	195	192	164	167
	(699)	(723)	(679)	(571)	(627)
N:	*13,900*	*10,161*	*1,712*	*1,455*	*572*

Each cell provides mean of the outcome variable in the left-hand column by subsample, with standard deviations in parentheses. The full sample in column 1 contains 13,900 district-academic years between 1999-2000 and 2015-2016. The remaining columns describe subsamples, broken down by Hispanic representation on the district's school board in a given year.

Table 4: Relevance and Excludability of Ballot Order: Candidate-level Analysis

	All Candidates		Hispanic Candidates	
	(1) Control Mean	(2) Top-tier Effect	(3) Control Mean	(4) Top-tier Effect
I. Impact of Top-tier Assignment on Candidate Outcomes				
Vote Share	0.185	0.012	0.207	0.021
	(0.116)	(0.002)	(0.129)	(0.007)
Wins?	0.401	0.062	0.404	0.105
	(0.490)	(0.015)	(0.491)	(0.055)
N:	*3,418*	*6,469*	*1,088*	*2,140*
II. Correlation of Top-tier Assignment with Candidate Traits				
Democrat?	0.473	-0.020	0.599	-0.021
	(0.499)	(0.027)	(0.491)	(0.187)
Republican?	0.374	0.014	0.230	0.051
	(0.484)	(0.026)	(0.422)	(0.123)
N:	*1,448*	*2,724*	*421*	*847*
Hispanic?	0.348	-0.007	—	—
	(0.476)	(0.016)		
N:	*3,130*	*5,943*		
Incumbent?	0.315	0.008	0.321	-0.010
	(0.465)	(0.014)	(0.047)	(0.055)
Missing Ethnicity?	0.084	-0.006	—	—
	(0.278)	(0.008)		
Missing Party?	0.576	0.015	0.613	0.013
	(0.494)	(0.013)	(0.487)	(0.052)
N:	*3,418*	*6,469*	*1,088*	*2,140*

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Columns 1 and 3 show the mean of the outcome variable in the left-hand column among candidates who are not assigned a top-tier ballot spot (my control group). Standard deviations appear in parentheses. Columns 2 and 4 present regression estimates of the top tier effect in the relevant sample, following Equation 1. Robust standard errors clustered at the district level appear in parentheses. Sample sizes vary within columns due to missing ethnicity and political party affiliation data, as discussed in the text.

Table 5: Excludability of Top-tier Hispanic IV: Election-level Analysis

	Control Mean (1)	Top-tier Hispanic Effect (2)
Hisp Board Share, Outgoing Board	0.13	0.01
	(0.21)	(0.01)
N:	*486*	*1,314*
Total Enrollment	11448	75
	(12538)	(129)
Share White	0.36	-0.00
	(0.23)	(0.00)
Share Hispanic	0.48	-0.00
	(0.24)	(0.00)
Share Asian	0.08	0.00
	(0.11)	(0.00)
Share Black	0.05	0.00
	(0.06)	(0.00)
Share FRL-eligible	0.51	0.01
	(0.23)	(0.01)
N:	*409*	*1,149*
Composite Math Score	-0.25	-0.03
	(0.80)	(0.04)
N:	*390*	*1,081*
Composite ELA Score	-0.19	-0.01
	(0.86)	(0.04)
N:	*429*	*1,190*

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The data consist of district-election year observations (N=1,314). Column 1 reports the mean of the outcome variable in the left-hand column among district-election years without a top-tier Hispanic candidate (my control group). Standard deviations appear in parentheses. Column 2 presents regression estimates of the top-tier Hispanic effect, following Equation 2, with controls for the share of Hispanic candidates, the number of contested seats in the election, and district fixed effects, as discussed in the text. Robust standard errors clustered at the district level appear in parentheses. Sample sizes vary within columns due to missing enrollment and student performance data, as discussed in the text.

Table 6: Relevance of Top-tier Hispanic IV: Representative First Stage Results Using School-level Panel

		Baseline Model		Addtl. Controls	
	Post-Election Control Mean	Top-tier Hispanic Effect	F-stat	Top-tier Hispanic Effect	F-stat
	(1)	(2)	(3)	(4)	(5)
Hisp Board Share	0.183	0.086***	9.15	0.086***	17.31
	(0.230)	(0.015)		(0.015)	
Any Hisp on Board?	0.509	0.115***	23.95	0.115***	34.78
	(0.500)	(0.032)		(0.032)	
Hisp Majority?	0.119	0.088***	2.62	0.090***	2.31
	(0.324)	(0.026)		(0.026)	
N:	*36,381*	*141,329*		*135,308*	
School FEs		Y	Y	Y	Y
Control for Share Hisp Cand		Y	Y	Y	Y
Control for # Seats		Y	Y	Y	Y
Election/Academic Yr, Period FEs		Y	Y	Y	Y
Other Election Controls		N	N	Y	Y
Demographic Controls		N	N	Y	Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The sample includes school-period observations from pre-election period -8 through post-election period +8. Column 1 reports the mean of the first stage outcome in the left-hand column among observations without a top-tier Hispanic candidate over the 8 periods after an election (my control group). Standard deviations appear in parentheses. Column 2 presents regression estimates of the top-tier Hispanic effect, following the first stage given in Equation 3, with controls for the share of Hispanic candidates, the number of contested seats in the election, as discussed in the text, along with school, election year, academic year, and period fixed effects. Column 4 reports estimates using additional election and demographic controls. Election covariates include the total number of candidates in the race, the number of Hispanic candidates, and an indicator for whether the district holds ward elections. Demographic covariates include the shares of white, Black, Hispanic, Asian, and FRL-eligible students. Sample sizes vary due to missing enrollment data, as discussed in the text. Robust standard errors clustered at the district level appear in parentheses.

Table 7: How Does Hispanic Representation Affect Overall SFP Spending? District-level Analysis

	Post-Election Mean	Reduced-Form			2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Modernization Spending per Pupil	116.0	34.2***	34.2***	29.9***	356.6***	327.3***
	(368.8)	(10.8)	(10.7)	(10.5)	(122.6)	(122.0)
New Construction Spending per Pupil	57.9	-3.0	-2.9	-4.9	-31.2	-53.7
	(239.3)	(7.6)	(7.6)	(7.2)	(77.6)	(77.5)
Supplemental SFP Spending per Pupil	200.6	8.8	8.8	1.9	91.6	20.6
	(619.1)	(16.7)	(16.7)	(16.2)	(172.6)	(173.3)
Total SFP Spending per Pupil	386.6	35.9	36.0	24.9	374.5	272.0
	(1,080.8)	(30.3)	(30.2)	(29.3)	(315.3)	(316.6)
N	*5,009*	*11,590*	*11,590*	*11,584*	*11,590*	*11,584*
District FEs	Y	Y	Y	Y	Y	
Control for Share Hisp Cand	Y	Y	Y	Y	Y	
Control for # Seats	Y	Y	Y	Y	Y	
Election /Academic Yr, Period FEs	Y	Y	Y	Y	Y	
Other Election Controls	N	Y	Y	N	Y	
Demographic Controls	N	N	Y	N	Y	
First Stage F-stat	—	—	—	11.06	12.02	
First Stage Coefficient	—	—	—	0.10	0.09	
				(0.01)	(0.01)	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The sample includes district-period observations from pre-election period -8 through post-election period +4. Column 1 presents the sample mean of the outcome variable in the left-hand column in periods 1-4 after the election. Standard deviations appear in parentheses. Regression results reported in Columns 2-6 use panel data spanning eight years before the election through four years after the election. The specification in columns 2-4 is Equation 4. The specification in columns 5 and 6 is Equation 3. All specifications include election year, academic year, and district fixed effects, as well as controls for the number of contested seats and the share of Hispanic candidates. Additional election controls are described below Table 6. Demographic covariates include the shares of white, Black, Hispanic, Asian, and FRL-eligible students in the district. Robust standard errors are clustered at the district level.

Table 8: How Does Hispanic Representation Affect Intra-District SFP Spending? School-level Analysis

		School-level Regression Estimates						
			By Share Hispanic		By Share FRL		By Title I	
	Post-Election Mean	All Schools	Above Median	Below Median	Above Median	Below Median	Title I Eligible	Not Title I Eligible
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
I. Reduced-form Estimates of Top-tier Hispanic Effect								
Modernization Spending per Pupil	172.2	50.8***	56.3***	35.6*	65.4***	37.4**	57.0***	21.3
	(674.2)	(16.4)	(17.1)	(18.2)	(18.7)	(16.6)	(18.0)	(22.6)
Total SFP Spending per Pupil	420.0	59.5*	72.8**	32.3	77.2	35.8	91.8**	-22.2
	(1,458.6)	(34.9)	(36.0)	(38.6)	(47.6)	(36.9)	(39.8)	(44.5)
II. 2SLS Estimates of Hispanic Representation Effect								
Modernization Spending per Pupil	172.2	500.2***	563.6***	352.2*	644.8***	373.5**	552.4**	244.1
	(674.2)	(187.2)	(205.9)	(190.0)	(225.2)	(175.7)	(189.9)	(287.4)
Total SFP Spending per Pupil	420.0	586.0*	728.5*	320.6	761.3**	357.8	890.2**	-308.3
	(1,458.6)	(351.4)	(384.0)	(376.6)	(388.7)	(359.6)	(393.0)	(547.9)
N	*43,446*	*92,601*	*43,610*	*43,635*	*43,510*	*43,510*	*64,827*	*27,768*
First Stage Coefficients	—	0.10	0.10	0.10	0.10	0.10	0.10	0.08
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
First Stage F-stat		19.03	17.70	16.93	16.16	19.05	20.52	6.91
Election/Academic Yr, Period FEs	—	Y	Y	Y	Y	Y	Y	Y
School FEs	—	Y	Y	Y	Y	Y	Y	Y
Control for Share Hisp Cand	—	Y	Y	Y	Y	Y	Y	Y
Control for # Seats	—	Y	Y	Y	Y	Y	Y	Y
Other Election Controls	—	Y	Y	Y	Y	Y	Y	Y
School Demographic Controls	—	Y	Y	Y	Y	Y	Y	Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The sample includes school-period observations from pre-election period -8 through post-election period +4. The first column shows the mean of the two outcome variables in the left-hand column over the four post-election periods. Estimates in the remaining columns cell comes from separate OLS and 2SLS regressions. The reduced-form specification is Equation 4 and the 2SLS specification is Equation 3. All specifications include election and academic year fixed effects, school fixed effects, and additional election and demographic controls as described below Table 6. All specifications use school enrollment weights. Sample sizes vary across columns due to missing enrollment data and exclusion of schools with exactly median enrollment by category. Robust standard errors are clustered at the district level.

Table 9: How Does Hispanic Representation Affect Intra-District Achievement? School-level Analysis

	Post-Election Mean	All Schools	By Share Hispanic: Above Median	By Share Hispanic: Below Median
	(1)	(2)	(3)	(4)
I. Reduced-Form Estimates of Top-tier Hispanic Effect				
Composite Math Score	-0.170	0.030*	0.041**	0.026
	(1.080)	(0.017)	(0.020)	(0.020)
N	*70,260*	*110,613*	*55,460*	*49,476*
Composite ELA Score	-0.318	0.022*	0.028*	0.022
	(1.049)	(0.012)	(0.015)	(0.015)
N	*70,256*	*110,607*	*55,453*	*49,477*
II. 2SLS Estimates of Hispanic Representation Effect				
Composite Math Score	-0.170	0.311*	0.429**	0.263
	(1.080)	(0.165)	(0.197)	(0.194)
N	*70,260*	*110,613*	*55,460*	*49,476*
Composite ELA Score	-0.318	0.229*	0.288*	0.216
	(1.049)	(0.127)	(0.160)	(0.154)
N	*70,256*	*110,607*	*55,453*	*49,477*
Election/Academic Yr, Period FEs		Y	Y	Y
School FEs		Y	Y	Y
Control for Share Hisp Cand		Y	Y	Y
Control for # Seats		Y	Y	Y
Other Election Controls		Y	Y	Y
School Demographic Controls		Y	Y	Y
ELA First Stage Coeff.		0.10	0.10	0.10
		(0.02)	(0.02)	(0.02)
ELA First Stage F-stat		15.14	10.47	15.56
Math First Stage Coeff.		0.10	0.10	0.10
		(0.02)	(0.02)	(0.02)
Math First Stage F-stat		15.04	10.49	15.56

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The sample includes school-period observations from pre-election period -8 through post-election period +8. All test score outcomes are measured in district-subject-year level standard deviations. The first column shows the mean of the two outcome variables in the left-hand column over the eight post-election periods. Estimates in the remaining cells comes from separate OLS and 2SLS regressions given by Equations 3 and 4. Additional controls are described below Table 6. All specifications use school enrollment weights. Sample sizes vary across columns due to missing enrollment and test score data, and due to the exclusion of schools with exactly median Hispanic enrollment, as discussed in the text. Robust standard errors are clustered at the district level.

Table 10: How Does Hispanic Representation Affect Teachers? School-level Analysis

		All Schools		High-Hispanic Schools		Low-Hispanic Schools	
	Post-Election Mean (1)	Reduced-Form (2)	2SLS Effect (3)	Reduced-Form (4)	2SLS Effect (5)	Reduced-Form (6)	2SLS Effect (7)
Mean FTE Experience	13.99	0.14**	1.59**	0.16**	1.88**	0.12**	1.38*
	(4.03)	(0.06)	(0.70)	(0.07)	(0.87)	(0.06)	(0.71)
Mean FTE Tenure	11.85	0.18***	2.07***	0.20***	2.38***	0.15**	1.84**
	(3.85)	(0.06)	(0.70)	(0.07)	(0.84)	(0.06)	(0.71)
# New Hires	3.41	-0.22**	-2.75**	-0.25**	-2.96**	-0.16	-1.87
	(4.65)	(0.11)	(1.29)	(0.12)	(1.39)	(0.11)	(1.26)
Share FTE Hisp	0.190	0.005**	0.061***	0.003	0.038*	0.005**	0.063**
	(0.176)	(0.002)	(0.022)	(0.002)	(0.023)	(0.002)	(0.024)
N:	*91,508*	*135,309*		*63,726*		*63,848*	
School FEs		Y	Y	Y	Y	Y	Y
Control for Share Hisp Cand		Y	Y	Y	Y	Y	Y
Control for # Seats		Y	Y	Y	Y	Y	Y
Election/Academic Yr, Period FEs		Y	Y	Y	Y	Y	Y
Other Election Controls		Y	Y	Y	Y	Y	Y
Demographic Controls		Y	Y	Y	Y	Y	Y
First Stage F-stat		—	16.42	—	14.36	—	16.25
First Stage Coefficient		—	0.085	—	0.084	—	0.084
			(0.017)		(0.017)		(0.018)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The sample includes school-period observations from pre-election period -8 through post-election period +8. Column 1 reports the mean of the first stage outcome in the left-hand column among observations without a top-tier Hispanic candidate (my control group). Standard deviations appear in parentheses. The remaining columns report regression coefficients using Equations 4 (reduced-form) and 3 (2SLS). Additional covariates are described in Table 6. Sample sizes do not add up across columns due to the exclusion of schools with exactly median Hispanic enrollment. Robust standard errors clustered at the district level appear in parentheses.

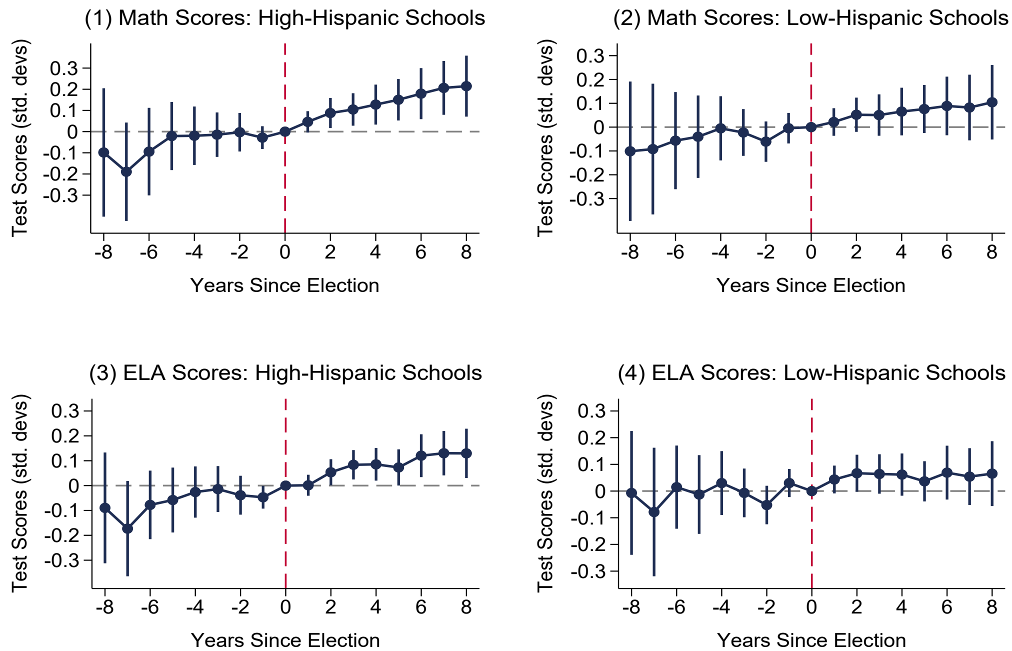

Figure 1: I measure all test score outcomes in district-subject-year-level standard deviation units. The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. Election-period sample sizes vary by panel due to missing test score and enrollment data as well as exclusion of schools with median Hispanic enrollment: N=56,295 for panel 1; N=50,141 for panel 2; N=56,288 for panel 3; and N=50,182 for panel 4.

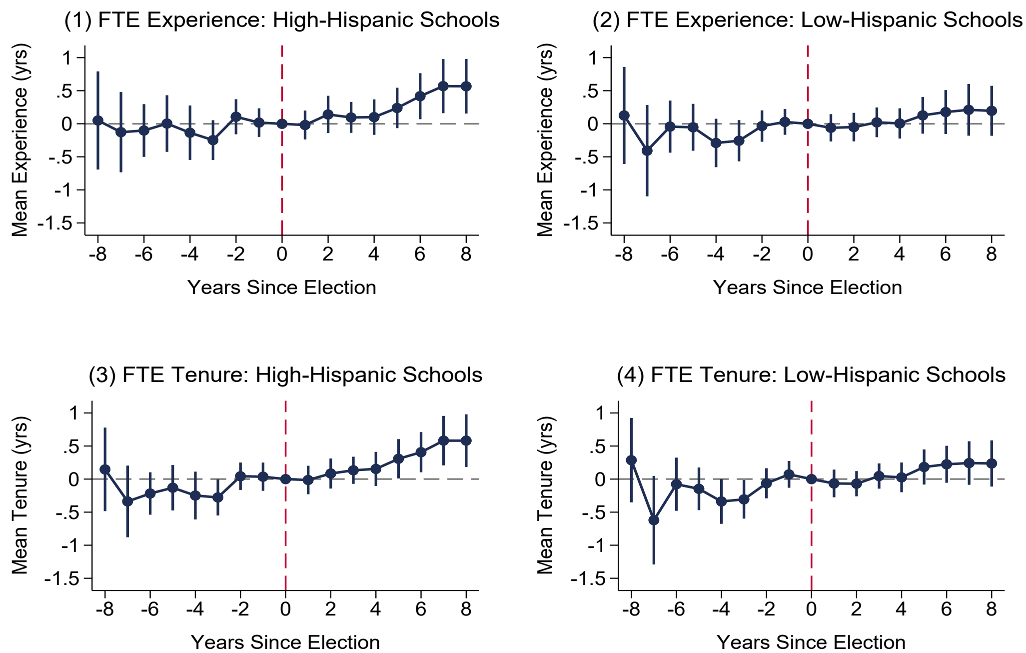

Figure 2: The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. The sample contains N=64,726 school-period observations in panels 1 and 3, and N=63,848 school-period observations in panels 2 and 4.

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9 Appendix

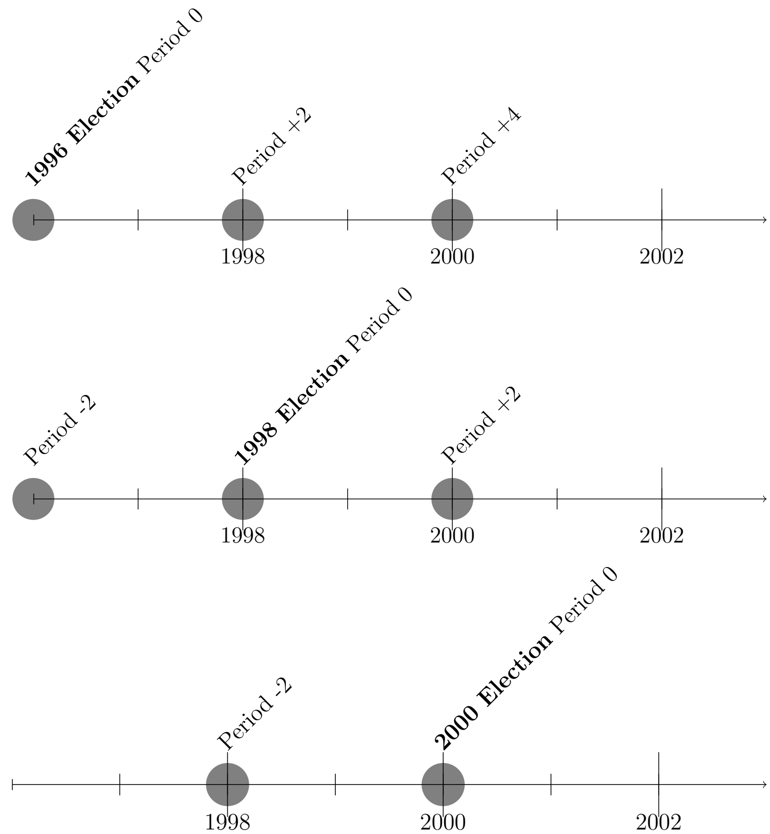


Figure A1: The diagram describes the overlapping panel structure I construct. Data from 1998 appear alternately as post-1996 election period 2 data; the 1998 election period 0 data; and pre-2000 election period -2 data.

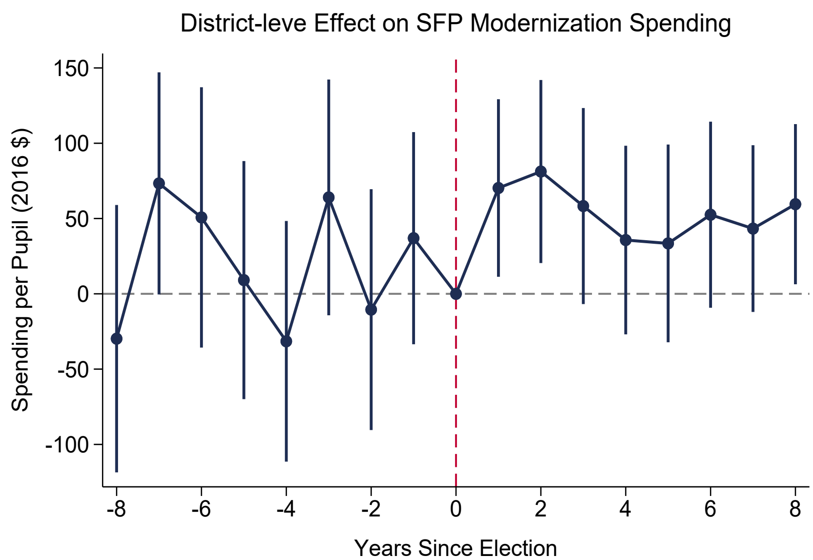
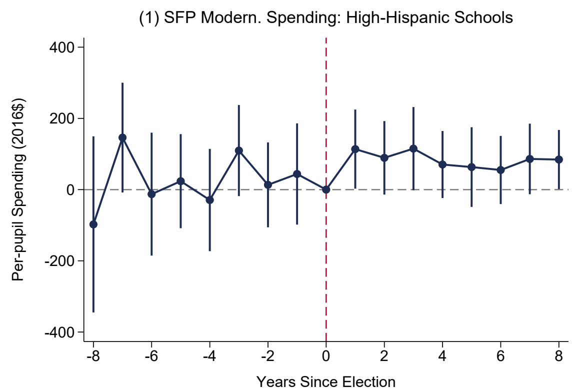


Figure A2: The figure plots event-study estimates of the top-tier Hispanic treatment effect on district-wide SFP modernization spending per pupil by year relative to the election. The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. The sample includes 16,601 district-by-election-period observations.



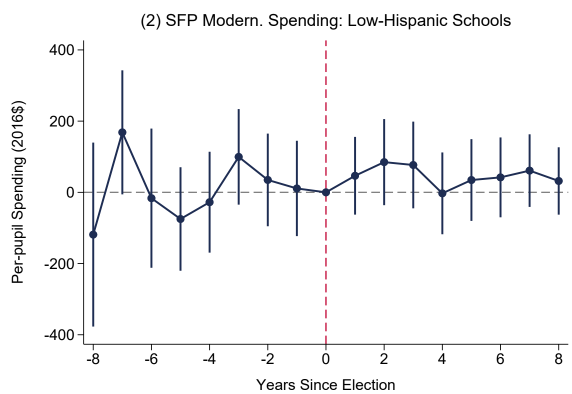
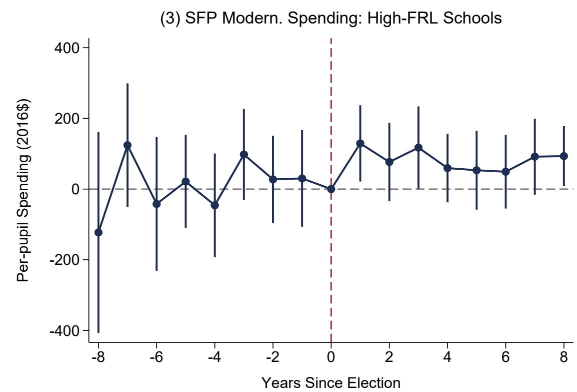


Figure A3: The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. The sample size is N=43,737 for both panels.



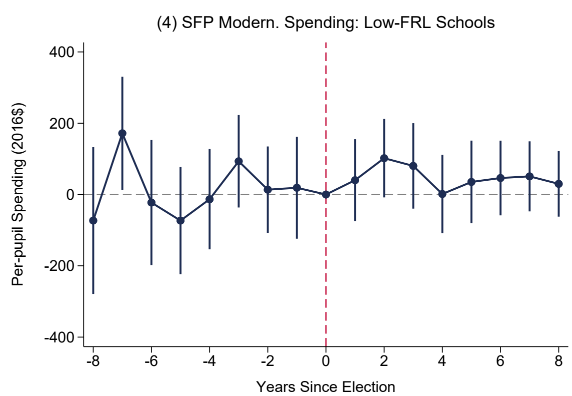


Figure A4: The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level.

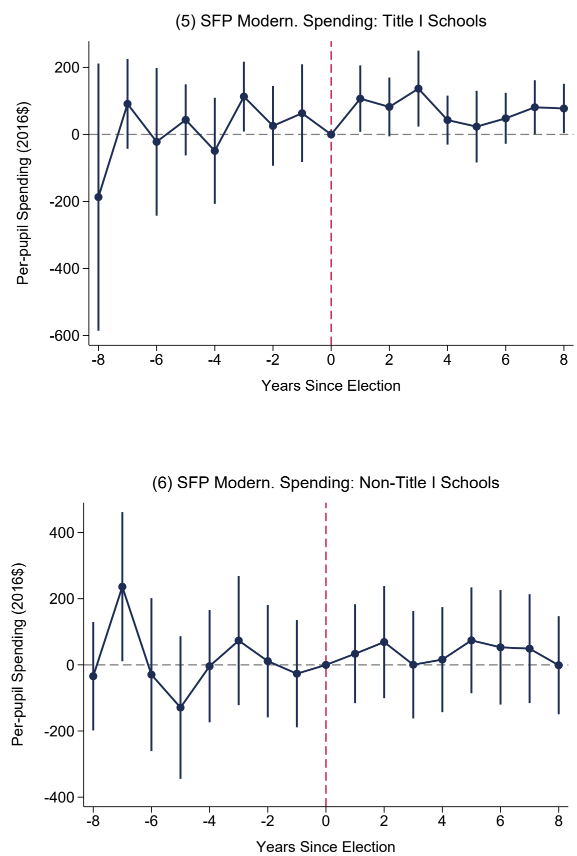


Figure A5: The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level.

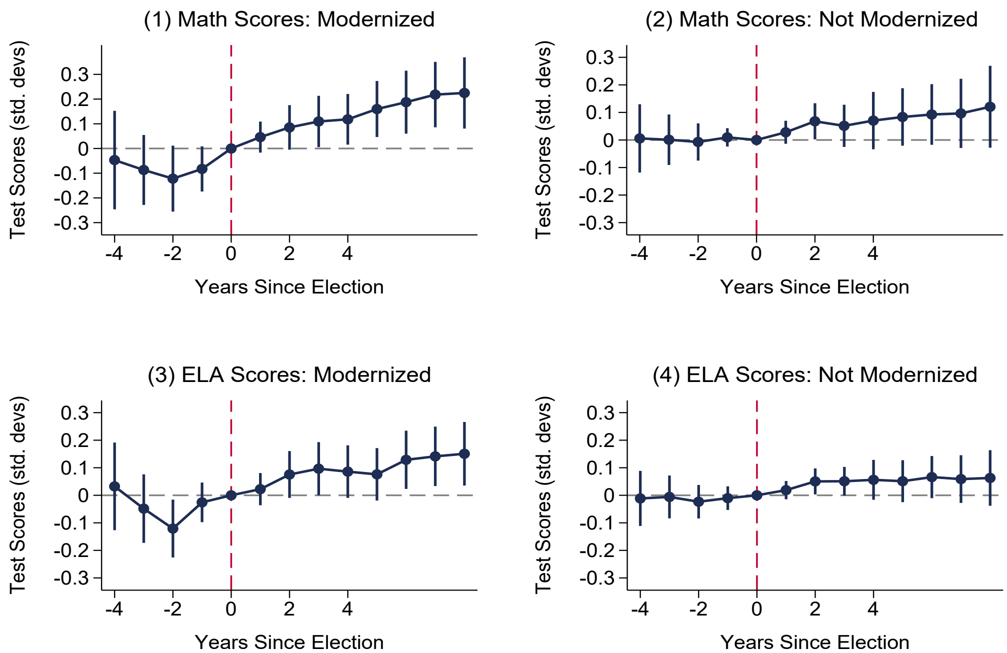


Figure A6: The figure shows event study plots depicting the estimated effect of a top-tier Hispanic candidate on test scores by year relative to the election. The sample is broken down into schools that did and did not initiate an SFP modernization project after the given election. The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. Sample sizes vary due to missing test score data, as discussed in the text: N=45,379 for panel 1; N=56,384 for panel 2, N=45,376 for pane1 3, and N=56,382 for panel 4.

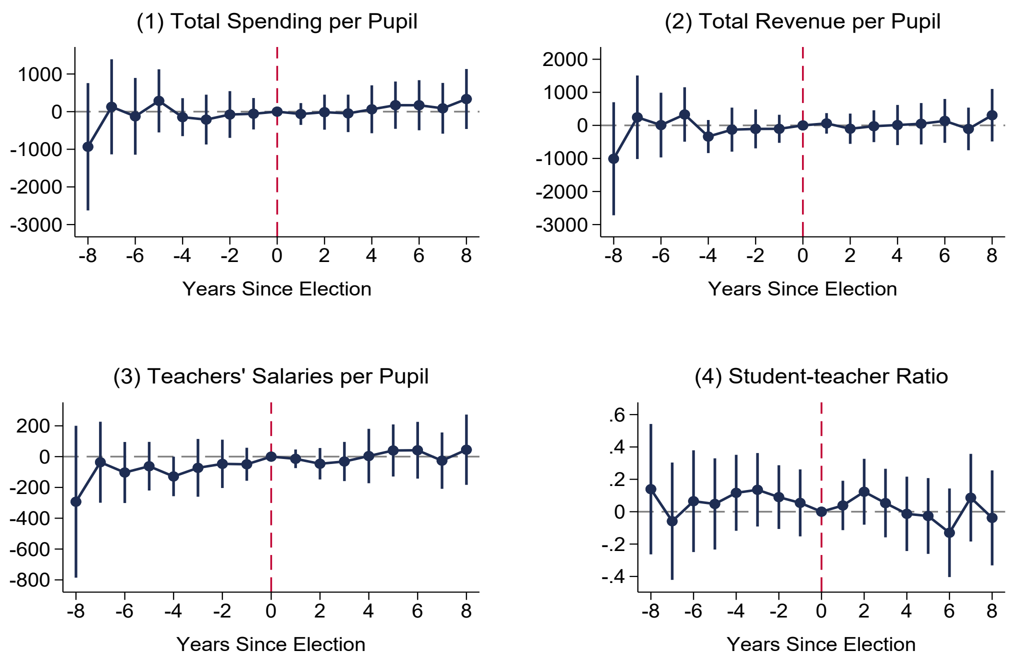


Figure A7: The figure shows event study plots depicting the estimated effect of a top-tier Hispanic candidate on non-SFP budget outcomes by year relative to the election. The data come from the Census of Governments. The specification is Equation 5 with district-level, rather than school-level, data. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. Sample sizes vary due to missing data, as discussed in the test: N=17,414 for panels 1-3 and N=15,963 for panel 4.

Table A1: Summary of School Facility Program (SFP) Projects

	N	Mean	Median	Std Dev	Min	Max
I. All Projects						
Project Involves New Construction?	7641	0.27	0	0.44	0	1
Project Involves Modernization?	7641	0.75	1	0.43	0	1
Year Construction Began	7641	2005	2004	4.18	1999	2017
Total Funds ('000s)	7641	4212	2381	6427	4.6	128470
Funds from State ('000s)	7641	2131	1648	3910	3.7	73102
Funds from District ('000s)	7641	1480	621	2941	0	64235
Modernization Spending ('000s)	7641	1299	786	1876	0	18823
New Construction Spending ('000s)	7641	587	0	1890	0	34142
Supplemental SFP Grants ('000s)	7641	2326	1031	4541	0	97104
II. Projects w/ Enrollment Data						
Total Funds per Pupil	7412	5093	3699	7887	5.1	219253
School Enrollment	7412	956	700	738	0	5213
School Share FRL	7412	0.52	0.53	0.30	0	1
School Share White	7412	0.33	0.27	0.28	0	0.99
School Share Hispanic	7412	0.46	0.43	0.30	0	1
School Share Other Minority	7412	0.20	0.15	0.18	0	1

The table reports summary data from all 7641 SFP projects begun by 2017. All SFP data come from California's Bond Accountability program. Note that a project can have both modernization and new construction components. All enrollment data come from the Common Core of Data. There are 229 new construction projects that I cannot match to enrollment data because the OPSC only assigns temporary identifying information. I report all costs in 2016 dollars.

Table A2: Summary of Name Matching in California School Board Races

	All Candidates	All Hispanic Candidates	NALEO Match	Hispanic Surname	NALEO and Hispanic Surname
N	16112	2494	1233	2302	1041
Avg Name Hisp Share	0.16	0.84	0.80	0.89	0.90
	(0.32)	(0.20)	(0.27)	(0.08)	(0.06)

The elections sample is identical to the one used in Table 1 and contains 16112 candidates across 3738 elections. The first panel describes the candidate-level data I obtain from matching my list of candidates to the list of most common Census surnames by race and to the NALEO directory of Latino officials. Standard deviations appear in parentheses. "Name Hispanic Share" refers to the share of Census respondents with a given surname who self-identify as Hispanic. Note that because some surnames do not appear in the Census' list of common surnames, sample sizes vary: I match 14510 total candidates and 1176 NALEO-listed candidates to the Census list.

Table A3: Reduced-form Results Using Only Census Data to Identify Hispanics

	(1) NALEO + Census Name Matching	(2) Only Census Name Matching
I. First Stage		
Hisp. Board Share	0.081***	0.094***
	(0.017)	(0.019)
	63,726	*62,216*
II. Reduced-Form Estimates Among High-Hispanic Schools		
Modernization Spending per Pupil	56.3***	39.9***
	(17.1)	(18.0)
	43,610	*42,688*
Total SFP Spending per Pupil	72.8**	50.7
	(36.0)	(38.0)
	43,610	*42,688*
Composite Math Scores	0.041**	0.028
	(0.020)	(0.019)
	55,460	*54,187*
Composite ELA Scores	0.028*	0.022
	(0.015)	(0.015)
	55,453	*54,181*
FTE Experience	0.154**	0.138*
	(0.070)	(0.083)
	63,726	*62,216*
FTE Tenure	0.197***	0.165**
	(0.064)	(0.071)
	63,726	*62,216*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The specification is Equation 4 with controls for the share of Hispanic candidates, the number of contested seats, and school-level fixed effects, as well as additional election and demographic covariates, which appear below Table 6. Sample sizes appear in italics. The first column presents point estimates that appear in the main text of the paper and use my preferred definition of Hispanic candidates. The second column identifies Hispanic candidates using only Census name-matching. Robust standard errors clustered at the district level appear in parentheses. Sample sizes vary within columns because of missing enrollment and student performance data. Note that the first stage outcome in both columns is measured using NALEO data, whereas only the first column uses NALEO data to identify candidate ethnicities.

Table A4: Relevance and Excludability of Ballot Order, without Fixed Effects

	All Candidates		Hispanic Candidates	
	(1) Control Mean	(2) Top-tier Effect	(3) Control Mean	(4) Top-tier Effect
I. Impact of Top-tier Assignment on Candidate Performance				
Vote Share	0.185	0.039***	0.207	0.034***
	(0.116)	(0.003)	(0.129)	(0.006)
Wins?	0.401	0.152***	0.404	0.176***
	(0.490)	(0.014)	(0.491)	(0.022)
N:	*3418*	*6469*	*1088*	*2140*
II. Correlation of Top-tier Assignment with Candidate Traits				
Democrat?	0.473	-0.013	0.599	0.000
	(0.499)	(0.019)	(0.491)	(0.034)
Republican?	0.374	0.018	0.230	0.037
	(0.484)	(0.019)	(0.422)	(0.029)
N:	*1448*	*2724*	*421*	*847*
Hispanic?	0.348	0.026*	—	—
	(0.476)	(0.013)		
N:	*3130*	*5943*		
Incumbent?	0.315	0.085***	0.321	0.088***
	(0.465)	(0.013)	(0.047)	(0.021)
Missing Ethnicity?	0.084	-0.006	—	—
	(0.278)	(0.007)		
Missing Party?	0.576	0.005	0.613	-0.018
	(0.494)	(0.012)	(0.487)	(0.021)
N:	*3418*	*6469*	*1088*	*2140*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The table is identical to Table 4, except that the specification used does *not* include election fixed effects. Otherwise, the specification is Equation 1, including controls for the share of Hispanic candidates and the number of contested seats in the election. Robust standard errors are clustered at the district level.

Table A5: Excludability of Top-tier Hispanic IV, without Fixed Effects

	(1) Control Mean	(2) Top-tier Hispanic Effect
Hisp Board Share, Outgoing Board	0.13	0.01
	(0.21)	(0.01)
N:	*486*	*1,314*
Total Enrollment	11448	-714
	(12538)	(734)
Share White	0.36	-0.01
	(0.23)	(0.02)
Share Hispanic	0.48	0.00
	(0.24)	(0.02)
Share Asian	0.08	0.00
	(0.11)	(0.01)
Share Black	0.05	0.01*
	(0.06)	(0.00)
Share FRL-eligible	0.51	0.02
	(0.23)	(0.02)
N:	*409*	*1,149*
Math Composite Scores	-0.25	-0.02
	(0.80)	(0.05)
N:	*333*	*1,081*
ELA Composite Scores	-0.19	-0.03
	(0.86)	(0.05)
N:	*333*	*1,081*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The table is identical to Table 5, except that the specification does not include any year or district fixed effects. Otherwise, the specification is Equation 2 with controls for the share of Hispanic candidates and the number of contested seats in the election. Robust standard errors are clustered at the district level.

Table A6: Exploring Sensitivity of First Stage to "Top-of-the-ticket" Candidates

	Top-tier Effect	"Top-of-the-Ticket" Effect	Top-tier Effect, Excl. Top-of-the-Ticket
Hisp Board Share	0.086***	0.097***	0.047**
	(0.015)	(0.015)	(0.023)
N:	*141,329*	*141,329*	*87,483*
School FEs	Y	Y	Y
Control for Share Hisp Cand	Y	Y	Y
Control for # Seats	Y	Y	Y
Election/Academic Yr, Period FEs	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The data consist of school-period observations. The specification follows the first stage of Equation 3, with modifications as indicated in column headers. Column 2 is identical to the second column in Table 6. Column 3 uses as an instrument an indicator for whether a Hispanic candidate appeared in the first ballot position (the "top of the ticket"). Column 4 uses the top-tier Hispanic indicator, but excludes top-of-the-ticket Hispanic candidates (that is, the sample only includes elections with no top-tier Hispanic candidates and those with a top-tier Hispanic candidate who was not in the first ballot position). Robust standard errors clustered at the district level appear in parentheses.

Table A7: Correlation of Hispanic Top-Tier Indicator with SFP Outcomes, Election Characteristics

	(1) Control Mean	(2) Hisp Top tier Effect
I. Election Year SFP Outcomes		
Total SFP Spending per Pupil	348	114
	(988)	(111)
Modernization Spending per Pupil	140	2
	(424)	(41)
New Constr. Spending per Pupil	49	31
	(216)	(30)
Supplemental SFP Grants per Pupil	155	66
	(498)	(62)
Share of Students Treated	0.39	0.02
	(0.38)	(0.03)
N:	*392*	*1097*
II. Election Year SFP Eligibility Proxies		
Cum. SFP Spending per Pupil	2162	160
	(2933)	(311)
N:	*392*	*1097*
Total Enrollment	11448	75
	(12538)	(129)
# FTE Teachers	524	3
	(576)	(6)
Student-FTE Teacher Ratio	21.2	0.09
	(2.35)	(0.12)
Avg. Age of Schools	21.62	0.06
	(7.38)	(0.18)
Share of Schools Opened 1980	0.80	0.01
	(0.24)	(0.01)
N:	*409*	*1149*
III. Election Candidate Composition, Results		
Top-Tier Democrat?	0.58	0.02
	(0.46)	(0.06)
	486	*1314*
Top-Tier Incumbent?	0.29	0.05
	(0.45)	(0.05)
	486	*1314*
Share Missing Ethnicity	0.08	0.01
	(0.13)	(0.01)
	486	*1314*
# Democrat Wins	0.39	0.05
	(0.63)	(0.06)
	486	*1314*
# Incumbent Wins	1.12	-0.04
	(0.90)	(0.08)
	486	*1314*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The specification is Equation 2 with controls for the share of Hispanic candidates, the number of contested seats, and district-level fixed effects. Robust standard errors clustered at the district level appear in parentheses. Sample sizes vary within columns because of missing enrollment and student performance data.

Table A8: Robustness of Main Results to Alternative Regression Specifications, Reduced-form Estimates

	(1) No FEs Post-election Obs	(2) +Elec Yr FEs	(3) +Academic Yr FEs	(4) +Period FEs	(5) +Period 0 Obs	(6) + Periods -1 thru -8 Obs	(7) +School FEs	(8) + Elec Covars	(9) Preferred Model	(10) + Flexible Elec Covars
I. Student Achievement at High-Hispanic Schools										
ELA Composite Score	0.01	0.02	0.02	0.02	0.02	0.05**	0.03*	0.04**	0.03*	0.03*
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
N	34,416	34,416	34,416	34,416	38,324	55,453	55,453	55,453	55,453	55,453
Math Composite Score	-0.04	-0.06	-0.06	-0.06	-0.06	-0.06	0.05**	0.05**	0.04**	0.04**
	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)
N	35,248	35,248	35,248	35,248	39,164	56,295	56,295	56,295	55,460	55,460
II. SFP Spending at High-Hispanic Schools										
Total SFP Spending per Pupil (School-level)	66.18*	73.45**	72.64**	72.61**	74.93**	69.13*	66.59*	73.19**	72.75**	78.24**
	(38.96)	(36.11)	(35.95)	(35.83)	(36.02)	(35.67)	(34.93)	(35.78)	(35.97)	(36.50)
Modernization Spending per Pupil (School-level)	55.34***	49.30***	48.98***	49.25***	53.11***	56.19***	57.52***	58.22***	56.28***	57.52***
	(18.98)	(18.68)	(18.65)	(18.69)	(18.48)	(16.25)	(17.38)	(16.98)	(17.14)	(17.38)
N	20,481	20,481	20,481	20,481	24,822	43,737	43,737	43,737	43,610	43,610
III. FTE Tenure and Experience at High-Hispanic Schools										
FTE Mean Experience	0.20	0.18	0.19	0.19	0.19	0.19	0.18**	0.19**	0.18**	0.18**
	(0.20)	(0.20)	(0.20)	(0.20)	(0.20)	(0.20)	(0.07)	(0.07)	(0.07)	(0.07)
FTE Mean Tenure	0.02	0.05	0.06	0.06	0.08	0.14	0.22***	0.24***	0.22***	0.22***
	(0.14)	(0.13)	(0.13)	(0.13)	(0.12)	(0.12)	(0.07)	(0.07)	(0.06)	(0.07)
N	40,354	40,354	40,354	40,354	44,320	59,553	59,553	59,553	58,669	58,669

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Each cell comes from a separate regression. The sample consist of school-period observations. All samples include only high-Hispanic schools (the subsample of greatest interest in this paper). The base specification in column 1 regresses the outcome on the top-tier Hispanic indicator and controls for the share of Hispanic candidates and number of contested seats. The sample only includes post-election outcomes. Subsequent columns add additional controls and pre-election observations as indicated. Column 9 reports results that appear in the main paper, with full demographic and election controls, which are described below Table 6. Note that sample sizes decline across columns 8 and 9 because of missing demographic variables. Column 10 presents a specification based on the preferred model which adds a quadratic term in the share of Hispanic candidates and uses a fixed effect for the number of contested seats (instead of a linear control as in the remaining columns). Standard errors in parentheses are clustered at the district level.

Table A9: Specification Robustness: Comparing Fixed Effect Choice

	School-level Regression Estimates						
		By Share Hispanic		By Share FRL		By Title I	
	All Schools	Above Median	Below Median	Above Median	Below Median	Title I Eligible	Not Title I Eligible
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I. School-by-Election Fixed Effects							
Modernization Spending per Pupil	67.4**	79.4***	44.7	93.2***	53.3*	78.2***	31.4
	(26.2)	(28.7)	(32.5)	(31.6)	(29.3)	(27.6)	(44.8)
Total SFP Spending per Pupil	76.2	92.2	39.1	97.3	58.6	106.4*	-10.9
	(57.6)	(65.9)	(70.0)	(66.3)	(67.2)	(62.9)	(90.8)
N	*92,607*	*43,610*	*43,635*	*43,510*	*43,510*	*64,827*	*27,768*
Math Composite Score	0.072**	0.092**	0.079	0.099**	0.053	0.016	0.015
	(0.036)	(0.043)	(0.051)	(0.049)	(0.045)	(0.036)	(0.067)
N	*110,610*	*55,460*	*49,476*	*57,488*	*46,805*	*83,118*	*27,484*
ELA Composite Score	0.052**	0.071**	0.055	0.076**	0.037	0.011	0.027
	(0.027)	(0.034)	(0.038)	(0.036)	(0.035)	(0.030)	(0.056)
N	*110,607*	*55,453*	*49,477*	*57,484*	*46,805*	*83,112*	*27,484*
Mean FTE Experience	0.292**	0.384**	0.249*	0.449***	0.231*	0.241	0.213
	(0.122)	(0.161)	(0.132)	(0.174)	(0.131)	(0.153)	(0.170)
Mean FTE Tenure	0.350***	0.439***	0.310**	0.504***	0.271**	0.319**	0.193
	(0.125)	(0.157)	(0.137)	(0.170)	(0.134)	(0.160)	(0.159)
N	*135,309*	*63,726*	*63,848*	*63,592*	*63,592*	*95,827*	*37,143*
II. District Fixed Effects							
Modernization Spending per Pupil	50.8***	62.3***	38.4**	66.1***	41.1**	64.0***	23.9
	(16.0)	(16.9)	(18.5)	(18.9)	(16.8)	(18.0)	(23.4)
Total SFP Spending per Pupil	58.9*	82.2**	37.7	83.0**	41.1	97.8**	-20.7
	(34.3)	(35.9)	(39.4)	(38.0)	(37.6)	(40.3)	(45.8)
N	*92,607*	*43,610*	*43,635*	*43,510*	*43,510*	*64,827*	*27,768*
Math Composite Score	0.030*	0.041**	0.026	0.038*	0.020	0.006	0.02
	(0.017)	(0.019)	(0.020)	(0.021)	(0.019)	(0.016)	(0.026)
N	*110,610*	*55,460*	*49,476*	*57,488*	*46,805*	*83,118*	*27,484*
ELA Composite Score	0.022*	0.028*	0.022	0.025	0.019	0.003	0.033
	(0.012)	(0.015)	(0.015)	(0.015)	(0.015)	(0.013)	(0.022)
N	*110,607*	*55,453*	*49,477*	*57,484*	*46,805*	*83,112*	*27,484*
Mean FTE Experience	0.143**	0.182**	0.134**	0.177**	0.126*	0.109	0.131*
	(0.063)	(0.080)	(0.067)	(0.077)	(0.068)	(0.072)	(0.075)
Mean FTE Tenure	0.183***	0.223***	0.172***	0.226***	0.160**	0.152**	0.136**
	(0.061)	(0.074)	(0.066)	(0.074)	(0.065)	(0.074)	(0.068)
N	*135,309*	*63,726*	*63,848*	*63,592*	*63,592*	*95,827*	*37,143*
III. Neither District Nor School Effects							
Modernization Spending per Pupil	42.2***	54.9***	32.0***	57.0***	34.8***	55.6***	11.0
	(15.9)	(10.3)	(11.1)	(10.4)	(11.0)	(8.4)	(14.9)
Total SFP Spending per Pupil	61.0*	85.0***	46.6**	86.6***	49.1**	100.0***	-14.9
	(35.0)	(22.3)	(22.2)	(21.5)	(22.9)	(18.1)	(28.7)
N	*92,607*	*43,610*	*43,635*	*43,510*	*43,510*	*64,827*	*27,768*
Math Composite Score	0.060**	0.080***	0.044***	0.077***	0.034***	0.046***	0.002
	(0.027)	(0.010)	(0.011)	(0.010)	(0.012)	(0.008)	(0.016)
N	*110,610*	*55,460*	*49,476*	*57,488*	*46,805*	*83,118*	*27,484*
ELA Composite Score	0.052**	0.056***	0.048***	0.063***	0.033***	0.040***	0.015
	(0.025)	(0.009)	(0.011)	(0.009)	(0.011)	(0.007)	(0.016)
N	*110,607*	*55,453*	*49,477*	*57,484*	*46,805*	*83,112*	*27,484*
Mean FTE Experience	0.093	0.075**	0.140***	0.125***	0.063**	0.039	0.188***
	(0.121)	(0.031)	(0.032)	(0.031)	(0.032)	(0.026)	(0.041)
Mean FTE Tenure	0.103	0.101***	0.150***	0.147***	0.078**	0.044*	0.157***
	(0.106)	(0.029)	(0.030)	(0.029)	(0.030)	(0.024)	(0.040)
N	*135,309*	*63,726*	*63,848*	*63,592*	*63,592*	*95,827*	*37,143*
Election and Academic Yr FEs	Y	Y	Y	Y	Y	Y	Y
Control for Share Hisp Cand	Y	Y	Y	Y	Y	Y	Y
Control for # Seats	Y	Y	Y	Y	Y	Y	Y
School Demographic Controls	Y	Y	Y	Y	Y	Y	Y
Other Election Controls	Y	Y	Y	Y	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The sample includes school-period observations. Each cell comes from a separate reduced-form regression, following Equation 4, with modifications as indicated in the panel headers. Note that the first panel—using school-by-election effects—does not include any election-level covariates, which are collinear with the fixed effects. See the footnote below Table 6 for descriptions of the election and demographic controls used. All specifications use school enrollment weights. Sample sizes vary within columns due to missing enrollment data and exclusion of schools with exactly median enrollment by category. Robust standard errors are clustered at the district level.

Table A10: Are the Treatment Effects Across Schools Statistically Different?

	High/low Hispanic		High/low FRL		Title I Status	
	Top-tier Effect (1)	Interaction Effect (2)	Top-tier Effect (3)	Interaction Effect (4)	Top-tier Effect (5)	Interaction Effect (6)
Total SFP Spending per Pupil	38.53	48.42*	41.50	44.90*	-2.85	97.33*
	(37.22)	(27.58)	(36.65)	(26.91)	(48.86)	(58.72)
Modernization Spending per Pupil	38.91**	27.37**	39.53**	28.09**	27.41	36.26
	(17.56)	(12.22)	(16.94)	(12.96)	(24.00)	(26.66)
N	92,607		92,607		92,595	
Math Composite Score	0.00	0.05**	-0.00	0.06**	-0.06*	0.08***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
N	110,613		110,613		110,602	
ELA Composite Score	0.01	0.03*	0.00	0.04**	-0.04*	0.07***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
N	110,607		110,607		110,596	
FTE Mean Experience	0.09	0.10	0.09	0.12**	0.06	0.07
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.08)
FTE Mean Tenure	0.13**	0.11*	0.13**	0.13**	0.06	0.12
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.09)
N	135,309		135,309		132,970	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The data consist of school-period observations. Each row within grouping (high/low Hispanic, high/low FRL, and Title I status) comes from a single regression. The reduced-form specification is analogous to Equation 4, but includes a fixed effect for whether the school has above-median Hispanic enrollment, above-median FRL enrollment, or Title I status ("high Hispanic") and interacts this dummy with my main treatment variable ($TopTierHisp \times post \times HighHisp$) as well as the "post" indicator. The interaction effects estimates measure the treatment effect on high-Hispanic, high-FRL, and Title I-eligible schools, relative to the ffect on schools that do not fall into these categories, given by the "top-tier effect." Sample sizes vary due to missing test score data and different post-election time frames. All specifications include school, election year, academic year, and period fixed effects, as well as controls for the share of Hispanic candidates and the number of contested seats, as discussed in the text. All specifications also include other election controls and demographic controls, as described below Table 6. Standard errors are clustered at the district level.

Table A11: Pre-election Placebo and Balance Estimates, by School Type

			Share Hispanic			Share FRL			Title I		
	Sample Mean	All Schools	Above Median	Below Median	p-value	Above Median	Below Median	p-value	Title I Eligible	Not Title I Eligible	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Total SFP Spending per Pupil	456.3	32.6	6.3	-15.3	0.704	58.2	-0.4	0.054	-20.4	31.9	0.636
	(1482.5)	(60.4)	(72.6)	(74.3)		(69.1)	(65.7)		(74.5)	(95.2)	
N	25179	25179	11867	11867		11826	11826		17512	7664	
Mdernization Spending per Pupil	448.0	19.0	-0.0	1.1	0.965	17.5	17.5	0.279	-10.2	21.6	0.564
	(1130.6)	(27.4)	(29.7)	(34.3)		(30.3)	(32.4)		(28.9)	(50.2)	
N	27079	27079	11867	11867		11826	11826		17512	7664	
Cum. SFP Spending per Pupil	4695.6	129.8	54.6	-119.9	0.369	127.6	5.71	0.245	29.3	76.8	0.85
	(8538.3)	(167.4)	(175.4)	(198.7)		(171.7)	(173.9)		(188.5)	(221.9)	
N	9788	9788	4342	4342		4330	4330		6500	2704	
Total Enrollment	909.0	-0.2	-3.8	14.6	0.52o	-7.6	-3.2	0.973	7.5	-48.8	0.231
	(636.7)	(5.2)	(21.7)	(17.2)		(18.6)	(8.5)		(21.2)	(36.1)	
N	9206	9206	4342	4342		4330	4330		6500	2704	
Total FTE Teachers	41.58	-0.03	-0.42	0.58	0.404	-0.46	-0.12	0.961	-0.07	-1.94	0.268
	(25.47)	(0.26)	(0.83)	(0.82)		(0.75)	(0.43)		(0.87)	(1.30)	
N	9205	9205	4342	4342		4330	4330		6500	2704	
Student-FTE Ratio	21.22	0.00	0.07	0.05	0.878	0.08	-0.02	0.849	0.16	-0.04	0.411
	(2.82)	(0.14)	(0.15)	(0.14)		(0.12)	(0.16)		(0.12)	(0.21)	
N	9204	9204	4342	4342		4330	4330		6500	2704	
School Age	23.3	0.04	0.06	0.17	0.589	0.11	-0.11	0.383	-0.11	0.56**	0.02
	(7.18)	(0.04)	(0.08)	(0.15)		(0.12)	(0.13)		(0.09)	(0.22)	
N	9230	9230	4342	4342		4330	4330		6500	2704	
School Opened in 1980?	0.889	0.002	0.003	0.004	0.962	0.005	-0.006	0.561	-0.005	0.013	0.234
	(0.315)	(0.002)	(0.004)	(0.005)		(0.004)	(0.005)		(0.005)	(0.012)	
N	9237	9237	4342	4342		4330	4330		6500	2704	
Share Hispanic	0.559	0.003	0.006*	0.002	0.103	0.009***	-0.001	0.015	0.008	0.006	0.594
	(0.276)	(0.003)	(0.003)	(0.003)		(0.003)	(0.004)		(0.003)	(0.008)	
Share White	0.242	-0.004*	-0.006**	-0.003	0.484	-0.005**	-0.003	0.526	-0.001	-0.006	0.454
	(0.230)	(0.002)	(0.003)	(0.003)		(0.002)	(0.003)		(0.002)	(0.006)	
N	9205	9205	4342	4342		4330	4330		6500	2704	
Share FRL	0.589	0.002	0.004	0.002	0.685	-0.001	0.002	0.423	-0.001	0.013	0.372
	(0.285)	(0.004)	(0.006)	(0.005)		(0.004)	(0.004)		(0.007)	(0.013)	
N	9186	9186	4342	4342		4330	4330		6500	2704	
Elec Yr FEs	—	Y	Y	Y		Y	Y		Y	Y	
District FEs	—	Y	Y	Y		Y	Y		Y	Y	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The table presents placebo and robustness results for my school-level SFP analysis. The sample contains school-period observations. Column 1 reports the sample mean of each variable in the election year. The remaining columns report OLS coefficients from separate regressions. The specification employs district and election year fixed effects and controls for the share of Hispanic candidates and the number of contested seats. The first panel uses data from the three periods prior to the election. All other models use data from only the election year. All specifications use school enrollment weights. Sample sizes vary within columns due to missing enrollment data. Robust standard errors are clustered at the district level.

Table A12: Reduced Form Effect on District Demographics, 1-6 and 1-8 Years After Election

	Sample Mean	After 6 Year	After 8 Years
	(1)	(2)	(3)
Total Enrollment	10618	-40.1	-53.1
	(13106)	(64.0)	(61.6)
	7515	*10021*	*12313*
Share White	0.27	0.003	0.003*
	(0.23)	(0.002)	(0.002)
Share Hisp	0.57	-0.002	-0.003*
	(0.27)	(0.002)	(0.001)
	7505	*10010*	*12298*
Share FRL-eligible	0.58	-0.001	-0.000
	(0.25)	(0.003)	(0.003)
	7515	*10010*	*12298*
FRL Dissimilarity Index	0.24	0.002	0.002
	(0.16)	(0.003)	(0.002)
White Dissimilarity Index	0.20	0.001	0.001
	(0.15)	(0.002)	(0.002)
Hispanic Dissimilarity Index	0.19	0.000	0.001
	(0.14)	(0.002)	(0.002)
	7515	*10021*	*12313*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The sample mean consists of data from years 1-6 after the election. The regression specification follows Equation 4 with district, election year, and test year fixed effects, as well as the same election and demographic covariates described below Table 7. Sample sizes appear in italics. Sample sizes vary due to missing data. Robust standard errors are clustered at the district level.

Defense Attorneys and Racial Disparities in Plea Bargaining *

Brett Fischer†

April 23, 2021

(Please find the most up-to-date version here.)

Abstract

This paper provides causal evidence that defense attorneys can mitigate racial disparities during pre-trial bargaining and thus reduce racial inequity in criminal sentencing. Focusing on indigent defendants, I compare plea bargaining and sentencing outcomes for nonwhite clients assigned public defenders to those instead assigned private attorneys. To deliver causal effects, I use an instrumental variables approach that leverages idiosyncratic variation in public defender assignments, driven by caseload capacities. I show that nonwhite defendants represented by private attorneys plead guilty to charges that carry 38 percent longer sentences, and they serve 52 percent longer prison terms, relative to comparable nonwhite defendants represented by public defenders. These effects are not driven by differences in defendant or case characteristics, and I find no comparable effects among white defendants. In other words, prosecutors extract harsher plea deals specifically from minority defendants with private attorneys, suggesting opportunistic discrimination against minority defendants with susceptible defense counsel. Consistent with a straightforward signaling model of plea bargaining, I provide empirical evidence suggesting that public defenders who have extensive interactions with prosecutors improve nonwhite defendants' plea bargaining outcomes. Reallocating public defenders to minority defendants could thus diminish racial bias in plea bargaining and better fulfil nonwhite defendants' constitutional right to effective representation.

JEL Classifications: H10,J15,K14,K41
Keywords: Plea bargaining, defense attorneys, defendant race

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1 Introduction

Racial inequity pervades the criminal justice system, from arrest to sentencing.[1] Existing evidence suggests nonwhite defendants face particularly adverse pre-trial outcomes, as manifested by racial disparities in bail and charging (Arnold, Dobbie and Yang 2018; Rehavi and Starr 2019; Tuttle 2019). Yet, little is known about how a defendant's race impacts his negotiating position during plea bargaining, despite the fact that most criminal cases culminate in guilty pleas in lieu of a trial.[2] Given well-documented racial disparities in criminal sentencing, it stands to reason—but has not been shown definitively—that many minority defendants receive plea offers carrying unnecessarily harsh sentences.[3] The possibility that nonwhite defendants consent, at least on paper, to discriminatory outcomes in the court system underscores the need for empirical evidence about whether—and how—racial bias in the plea bargaining process can be curtailed. Moreover, since even short spells in jail have severe social and economic repercussions (Dobbie, Goldin, and Yang 2018; Mueller-Smith 2015; Rose 2019; White 2019), understanding the scope and causes of racial disparities in plea bargaining provides a basis for addressing racial disparities in society writ large.

This paper presents causal evidence of sizeable racial disparities in plea bargaining. Specifically, I analyze the intersection of defendant race, plea bargaining, and another pillar of the legal system: indigent defense.[4] Up to 70 percent of all defendants—and 80 percent

1. See Abrams, Bertrand, and Mullainathan (2012); Antonovics and Knight (2009); Anwar, Bayer and Hjallmarsson (2012); Fischman and Schanzenbach (2012); and Yang (2015), among others cited below.

2. For instance, in 2015, 95 percent of federal court cases were resolved with (guilty) plea deals (Federal Bureau of Justice Statistics [2015]).

3. Berdejó (2018) provides a descriptive analysis of racial disparities in plea bargaining, but focuses only on guilty defendants and cannot fully control for differences across Black and white defendants that would influence pre-trial outcomes. Implicitly, both Rehavi and Starr (2014) and Tuttle (2019) indicate that nonwhite defendants fare worse in plea bargaining, but neither work provides a complete picture of how defendant race affects prosecutor and defense negotiating positions.

4. Economic researchers usually model plea bargaining as a contest between defendants per se and the prosecution (see, for example, Baker and Mazzetti [2001], Friedman and Wittman [2007], Silveira [2017], Reinganum [1988], and Spier [1992]). This approach makes sense to the extent that defendants and defense attorneys share information and priorities, but might omit information specific to defense attorneys that could help defendants, as well as incentives to collude with the prosecution and resolve cases quickly that could hurt them. Legal and criminal justice scholars have long argued that defense attorneys play a critical role in plea bargaining (for instance, Abrams [2011], Alschuler [2017], Bibas [2004], Bushway and Redlich [2012], and Gilchrist [2015]). But these studies do not provide rigorous empirical evidence to describe how, in practice, attorneys can improve pre-trial outcomes, nor do they discuss how defense attorney incentives

of Black defendants—utilize a court-appointed attorney (Harlow 2000), either a public defender (PD) or private assigned counsel (PAC).[5] The role of these lawyers is to provide a bulwark against plea deals that violate defendants' interests.[6] Yet, as Agan, Freedman, and Owens (2019) stress, attorney and client incentives do not always align; Richardson and Goff (2013) go further, positing that defense attorneys incorporate their own racial biases when evaluating a client's guilt. It thus remains unclear how well court-appointed defense attorneys guard against unfair pre-trial outcomes for minority defendants.

I address this issue empirically by leveraging variation in the type of representation—PD versus PAC—received by indigent defendants. Public defenders differ from private counsel along at least two dimensions that existing research suggests could affect pre-trial negotiations involving minority defendants. First, PDs handle larger caseloads, which could yield harsher plea deals for vulnerable clients since, as Richardson and Goff (2013) and Shulhofer (1984) argue, overburdened lawyers face pressure to collude with prosecutors and avoid bottlenecks in the trial process. On the other hand, PDs frequently engage with prosecutors, which over time could alert them to biased charging patterns and thereby strengthen the defense's hand, as Bibas (2004) and Bushway, Redlich, and Norris (2014) contend. Both of these channels could shape the degree to which PDs use race as a signal of defendant guilt, which Berdejó (2018) contends lies at the heart of racial discrimination in plea bargaining.

While this analysis applies to all attorneys, PDs and PACs exhibit particularly stark differences in these areas. My goal is not just to estimate the PD "treatment" effect per se.

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interact with racial disparities in the judicial system. As I discuss below, this paper bridges these perspectives by documenting how defense attorneys' institution-specific knowledge and well-informed expectations can enhance minority clients' bargaining positions.

5. Public defenders are full-time salaried government employees who serve in county public defender's offices and cannot practice law in private practice. Private assigned counsel are private-sector lawyers who volunteer to be called upon to represent indigent clients in exchange for hourly compensation from the state. I discuss the distinction between these attorneys in more detail in Section 2.

6. For example, the Supreme Court has established that "it is quintessentially the duty of counsel to provide her client with available advice" about any adverse consequences of accepting a plea deal (*Padilla v. Kentucky*, 559 US 356 [2010]), and that "defense counsel has the duty to communicate formal offers from the prosecution to accept a plea on terms and conditions that may be favorable to the accused" (*Missouri v. Frye*, 566 US 134 [2012]). Still, as these two cases attest, at least some lawyers do not live up to the Court's standard.

Rather, I compare defendants on the margin between PD and PAC representation to expose the causes and consequences of racial disparities in pre-trial proceedings and sentencing.[7]

To estimate the causal impact of defense counsel on defendant outcomes, I leverage idiosyncratic variation in attorney assignments consistent with PD caseload capacities. Public defender's offices often defer cases to private counsel when they do not have enough capacity to provide the marginal client with adequate representation.[8] Intuitively, then, some indigent defendants will enter the court system at a time when the PD's office has extra capacity (due to, for example, a lull in crime), which improves their odds of receiving a PD.[9] To operationalize this logic, I instrument for PD representation using the leave-out share of defendants who entered the court system in the same week and also received a PD. Using administrative court records from North Carolina spanning 2013-2018, I show that my instrument has a strong, positive correlation with the probability of PD representation, but is uncorrelated with defendant and case characteristics, which bolsters my causal claim.

Using my IV design, I first estimate how the type of indigent defense counsel affects pre-trial bargaining outcomes—the charges a defendant faces and the sentence imposed.

7. Abrams and Yoon (2007), Anderson and Heaton (2012), Cohen (2012), Iyengar (2007), Roach (2017), and Shem-Tov (2020) all find that PD representation results in lower sentences for defendants. Among the well-identified studies, Anderson and Heaton (2012) consider only murder cases, while Shem-Tov (2020) examines cases with co-defendants. Neither constitutes a very broad subset cases, nor does either paper consider plea bargaining outcomes; both studies also focus on urban jurisdictions (Philadelphia and San Francisco) with typically progressive politics and high-profile, prestigious public defenders' offices. Those factors likely shape their estimated PD treatment effects, and account for the (lack of) racial heterogeneity in their results. Abrams and Yoon (2007) find positive though insignificant correlations between representation by nonwhite (Hispanic) PDs and the probability of entering a guilty plea, but do not comment on potential mechanisms. Finally, while Abrams and Fackler (2019) and Silveira (2017) find a positive correlation between public defender representation and the probability a defendant enters a guilty plea, neither study frames this relationship as causal given endogenous selection into indigent defense.

8. As I discuss in Section 2.2, a public defender can also turn down a case if she has previously represented that defendant and/or has a poor working relationship with the defendant, or if her office already represents another defendant involved in the same case (which poses a conflict of interest). Shem-Tov (2020) utilizes the latter assignment rule to realize quasi-random variation in attorney assignments among cases with multiple defendants. By contrast, my instrumental variables approach allows me to consider a wider range of cases, not just those with multiple defendants.

9. As I discuss in Section 4, one might be concerned that that temporal variation in case composition will drive variation in my instrument and also be correlated with PD capacity. To help address those concerns, my default specification includes court-time fixed effects to isolate idiosyncratic variation in PD capacity. Since I find no correlation between my instrument and case and defendant characteristics—conditional on these fixed effects—I argue that these shocks to PD capacity are as good as randomly assigned.

My IV results suggest that, relative to defendants with PD representation, defendants with private attorneys see 0.50 (80 percent) more prosecutor-imposed charges tacked onto their cases. This effect does not vary by defendant race. But, Black and Hispanic defendants represented by PAC plead guilty to charges carrying 38 percent (about 1.2 months) higher average sentences than white defendants with PAC representation. Moreover, minority defendants receive 52 percent (about 4.7 months) longer prison sentences when they have PAC representation than do white defendants.[10]

I draw two conclusions from these findings. First, PDs discourage "overcharging"—prosecutors' imposition of harsher charges to increase their leverage in pre-trial negotiations (Alschuler 1968; Crespo 2018; Stuntz 2004)—for all defendants, regardless of race. Second, prosecutors and PACs exhibit hallmarks of racial discrimination, echoing Rehavi and Starr (2014), Sloan (2020), and Tuttle (2019). My findings indicate that nonwhite defendants with private counsel accept harsher plea deals not only relative to similar minority defendants with PD representation, but also relative to similar white defendants with private attorneys. These results do not point to racial prejudice among prosecutors, which would lead to uniformly harsher sentencing for minorities, irrespective of defense counsel. Rather, my analysis implies that prosecutors opportunistically pursue guilty pleas on more serious charges when facing susceptible defense attorneys. In this way, I depart from prior work by clarifying the circumstances that allow prosecutors to pursue biased plea deals.

To formalize this intuition, I present a signaling model of plea bargaining with three salient features: an explicit overcharging decision by the prosecutor; defense attorney beliefs about client guilt that factor in race; and defense attorney beliefs about the prosecutor's tendency to overcharge. I empirically probe potential mechanisms suggested by the model

10. As I discuss in Section 6, my preferred specification—which generates the results I report here—controls for observable differences between white and minority defendants, and my results remain robust whether I include or exclude such controls. I also provide suggestive evidence to rule out potential channels. I show that Black and Hispanic defendants face similarly serious charges as white defendants; that minority defendants draw do not draw stricter prosecutors; and that observable differences between PDs and PAC do not explain the racial gap. I conclude that my results signify racial bias, rather than an underlying correlation between a defendant's potential sentence, race, and attorney type.

using my IV framework. I show that PDs with the most prior interactions with a given prosecutor achieve the best outcomes for minority defendants. In this light, the efficacy of plea bargains for nonwhite defendants depends on whether the defense attorney has sufficient insight about the (potentially biased) plea deal to disentangle her client's actual culpability from the prosecutor's bluster.

This paper provides evidence both that racial bias in plea bargaining exists, and that capable defense attorneys can play a constructive role in mitigating it. In this respect, I build on Bushway et al. (2014) and help reconcile the duelling narratives about defendant expectations and institutional pressures on attorneys that divide the plea bargaining literature. Furthermore, I contribute to a growing consensus that pre-trial events profoundly impact defendant outcomes in the courtroom and beyond, particularly for minority defendants.[11] More broadly, this paper brings a deeper empirical understanding of plea bargaining to the economics literature, complementing Silveira (2017), and sheds much-needed light on how attorneys shape defendant outcomes, alongside Abrams and Yoon (2007), Agan et al. (2020), Anderson and Heaton (2012), Roach (2017), and Shem-Tov (2020). I add to these prior results by connecting defendant race, defense attorney capability, and detailed plea bargaining outcomes, an approach that underlines the pernicious consequences of the plea-based judicial model for traditionally marginalized defendants who lack adequate defense counsel. Increasing minority defendants' access to public defenders could thus diminish prosecutorial leverage, combat punitive sentencing, and better fulfill nonwhite defendants' constitutional right to effective legal counsel.[12]

11. See Berdejó 2018; Dobbie et al. 2018; Leslie and Pope 2017; and Stevenson 2018.

12. I am not the first to make this policy recommendation: Bibas (2004) argues that public defenders might reach the most favorable plea deals for defendants because "experienced, specialized repeat players are best able to forecast trial verdicts and sentences, including plea sentences" (2534). My results provide empirical evidence supporting this conclusion and its rationale.

2 Policy Background: Procedure, Indigent Defense, and Plea Bargaining

2.1 North Carolina Criminal Procedure

To assess how defense attorneys, plea bargaining, and defendant race interact, I examine the North Carolina court system. As a relatively large state with a diverse population and expansive judicial system, North Carolina provides an ideal policy setting. Most importantly, the state has dual system for providing indigent defense, relying on both full-time public defenders and contract private attorneys.[13]

The state's criminal process begins after arrest, when the defendant makes an "initial appearance" before a magistrate.[14] At this point, the defendant does not have legal counsel. Within 96 hours (or on the court's next working day, whichever comes first), the defendant makes a "first appearance" before a judge at his county's District Court, who reiterates the charges he faces and advises him of his right to counsel.[15] The judge then assesses his eligibility for a court-appointed attorney (see Subsection 2.2).

After his first appearance, the defendant either has an attorney or has waived his right to counsel. If he faces a misdemeanor charge (relatively minor offenses that result in no more than five months in jail, and often far less than that), the defendant receives a trial date in District Court; the presiding judge adjudicates the case and determines sentencing if she finds the defendant guilty. Prior to the trial, the defendant can plead guilty as part of an arrangement with the prosecutor (see Subsection 2.3). If he faces a felony charge (more serious offenses with potentially long prison terms), his case will be bound over to

13. This feature stands apart from many jurisdictions which rely almost exclusively on either public defenders (e.g., Colorado, Pennsylvania, and Washington, among many others) or private assigned counsel (e.g., Texas).

14. The magistrate informs the defendant of his rights, confirms that probable cause exists for his arrest, and sets conditions for his release from jail prior to his first appearance before a judge.

15. Defendants in felony cases also have the opportunity to request a probable cause hearing, another appearance in District Court at which the prosecution must establish probable cause before the case is bound over to Superior Court. However, in practice, these procedural events rarely happen: even if the District Court judge rules there is not probable cause, the prosecutor can still pursue an indictment, so neither party has an incentive to expend resources on this hearing.

Superior Court once the prosecutor secures a grand jury indictment.[16,17] Most cases then enter the pre-trial phase in which the defense attorney and the assistant district attorney (ADA) prosecuting the case can strike a plea deal.[18] Otherwise, the defendant is tried in Superior Court by a jury of 12, and the presiding judge determines his sentence in the event of a guilty verdict. Appendix Figure 9 visually summarizes the criminal process.

2.2 The North Carolina Indigent Defense System

In North Carolina, any defendant who faces potential jail time has the right to an attorney. Defendants can choose to retain their own counsel, or, if they cannot afford to do so, can petition to be assigned a court-appointed attorney. Court-appointed counsel is not free: if the defendant pleads or is found guilty, he must pay a share of his legal costs. Even so, those fees are usually less than those of a retained attorney.

A defendant usually receives a court-appointed attorney at his first appearance before a District Court judge.[19] At the first appearance, the judge asks the defendant whether he would like to be considered for a court-appointed attorney. If he does, the defendant submits an "affidavit of indigency," which affirms that he cannot afford his own attorney and includes basic financial information. In theory, the judge reviews the affidavit and determines the defendant's eligibility. In practice, North Carolina provides no benchmarks for judges to gauge a defendant's actual need.[20] The entire first appearance—including attorney assignment—lasts only minutes; survey and anecdotal evidence from the North

16. Grand juries consist of 18 jurors. At a grand jury hearing, the prosecutor presents his case to the jurors, who then determine whether there is probable cause to proceed to trial. Neither the defendant nor his attorney is present. At least 12 jurors must vote to indict, but if the prosecutor cannot secure an indictment, he can re-file the case in the future. Grand juries usually convene once a month in each county.

17. Some district courts, particularly in urban counties, will hear low-level felony cases (I- and H-level offenses in the structured sentencing hierarchy).

18. The defendant can opt to have a formal arraignment after his indictment at which a Superior Court judge advises the defendant about the charges he faces, and the defendant can enter a plea. The majority of defendants waive the right to an arraignment and move immediately into the pre-trial phase.

19. Note that a defendant can receive a court-appointed attorney at a later stage in the criminal process as well, and can also decide to retain his own attorney after initially being given a public defender or private assigned counsel. My sample construction, which I discuss in Section 3, aims to minimize the incidence of this sort of endogenous switching in my data.

20. In this respect, North Carolina differs from most jurisdictions, which rigorously screen defendants' financial records to determine their eligibility for indigent defense. As I discuss in Section 4, North Carolina's relatively informal system helps guide my identification strategy.

Carolina Indigent Defense Service suggest judges spend little or no time reviewing defendants' affidavits, and usually appoint counsel to all defendants who ask for it.

All defendants deemed eligible for a court-appointed lawyer have their cases sent to the local public defender's office.[21] Public defenders (or PDs) are full-time indigent defense lawyers whose salaries come from public funds. In large jurisdictions, PD's offices may assign cases to individual attorneys based on their expertise. However, most PD's offices lack sufficient personnel and information about cases to optimize assignments, and instead direct incoming cases to attorneys with relatively low caseloads. A PD can only turn down a case if she faces one of three potential ethical conflicts: (1) she or another PD currently represents a co-defendant in the same case, or a witness involved in the same case; (2) she or another PD has recently represented the same client, or her poor relationship with the client impedes the defense; or (3) her current caseload prevents her from providing an adequate defense for the new client.

When a conflict arises, the PD's office turns the case over to private assigned counsel (PAC), an attorney in private practice who serves as an indigent defender part-time. The specific PAC is selected via a rotation scheme: when a PAC receives a case, she returns to the bottom of the rotation, whereupon another PAC takes her place, receives the next case, goes to the bottom of the rotation, and so on. Private attorneys cannot turn down cases unless they face one of the ethical conflicts listed above.

2.3 Plea Bargaining and Sentencing in North Carolina

Plea bargaining in North Carolina criminal cases takes place anytime after a defendant's first appearance (in misdemeanor cases), or after he has been indicted (in felony cases), up until his trial. Bargaining takes place between the defendant (represented by his attorney) and an ADA; judges can reject plea deals struck between these parties but rarely do so.

In its simplest form, a plea bargain stipulates that a defendant pleads guilty to a given charge in exchange for a lenient sentence, which legal scholars term "sentence bargaining" and

21. 31/100 of counties in North Caroline have public defender's offices; this paper focuses exclusively on cases filed in those counties. See Section 3 for more detail.

generally see as a benign (if not ideal) feature of the criminal justice system.[22] Yet there is almost no formal law pertaining to plea bargains.[23] Consequently, pre-trial negotiations often take the murkier form of "charge bargaining," in which the prosecution and defense haggle over which charges a defendant will plead guilty to and which the DA's office will drop.[24] This process empowers prosecutors and can incentivize them to "overcharge" defendants by adding extra charges to a case or increasing the severity of existing charges.[25] In the end, as Crespo (2018) and Stuntz (2004) argue, the prosecutor's goal is probably not to obtain a guilty plea on the harshest charge he imposes. Rather, he uses these charges to force the defense to negotiate "down" to the prosecutor's preferred sentence. In practice, then, plea bargains involve negotiations over charges, not sentences, and revolve around the dynamic between the prosecuting and defense attorneys.[26]

No matter whether a case resolves via plea or trial, North Carolina uses a structured sentencing system to prescribe minimum and maximum sentences for all offenses. Felony

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22. See, for example, Bibas (2004), who ventures that "clear sentence bargains are preferable to opaque charge bargains" (2534), or Meares (1995), who points out that charge bargaining is especially potent because it takes place at the prosecutor's discretion, whereas judges can water down too-harsh sentences (888).

23. The Supreme Court's majority and dissenting opinions in *Missouri v. Frye* crystallize this fact. In his dissent, Justice Antonin Scalia remonstrates that plea bargains are not "substantive or procedural right[s]" and that defendants have "no entitlement" to a plea bargain. The majority, represented by Justice Anthony Kennedy, disagrees but nonetheless confronts the ambiguous rules governing plea bargaining ("it *appears that* the prosecution has some discretion to cancel a plea agreement to which the defendant has agreed"; emphasis added), and ultimately leaves it up to courtroom actors to self-police improper plea bargaining: "It can be assumed that in most jurisdictions prosecutors and judges are familiar with the boundaries of acceptable plea bargains and sentences." This paper interrogates that assumption using causal analysis.

24. Piehl and Bushway (2007) provide a statistical analysis of charge bargaining, and conclude that it plays a substantial role in observed sentencing outcomes. However, as the authors note, quantifying the extent of charge bargaining is challenging because any case-level data sets will only include plea deals that were implemented, making it difficult to define a counterfactual. As such, much of the evidence concerning overcharging remains anecdotal.

25. Alschuler (1968) coined the terms "horizontal overcharging" and "vertical overcharging," respectively, to describe these behaviors. Crespo (2018) dubs the same phenomena "piling on" and "overreaching." In theory, prosecutors have an ethical obligation to avoid charging a defendant for a crime without probable cause. In practice, as hinted at throughout this section, defendants and defense attorneys have no formal recourse to prevent the prosecutor from charging for offenses he would not be able to prove at trial, aside from calling his bluff and going to trial.

26. This idea does not appear explicitly in economic models of plea bargaining. Rather, economists assume pre-trial negotiations involve only sentence bargaining. As Silveira (2017) rightly notes, the distinction does not matter in a typical plea bargaining model, since harsher charges by definition entail harsher sentences. However, as I discuss in Section 7, the difference *does* matter in a bargaining model in which the defense has prior information about whether the prosecution's offer contains an empty threat to overcharge.

charges fall into classifications I - C, B2, B1, and A, where category A consists of capital offenses. Misdemeanor charges fall into classifications 3, 2, 1, and A1, where A1 misdemeanors are the most serious.[27] Those found guilty face greater minimum punishments for crimes in higher classes and for repeat offenses.[28] Since mandatory minimum sentences limit the potential for finely-tuned sentence bargaining, Bibas (2004) and Stuntz (2004) (among others) argue that structured sentencing increases prosecutor power and promotes charge bargaining. I return to this idea in Section 6, in which I briefly discuss how North Carolina's structured sentencing regime might affect the interpretation of my main results.

3 Data

This study assesses the prevalence of racial disparities in plea bargaining and defense attorneys' role in the pre-trial process. My empirical approach uses detailed administrative court records from North Carolina, provided by the North Carolina Administrative Office of the Courts. The data include all cases with any court proceeding between 2013 and 2018. In the raw data, each observation is a charge. Each charge record contains information about the offense committed, the defendant's name and demographic information, how the defendant pled to the charge, the charge disposition (e.g., dropped by the district attorney, jury trial, etc.), and any sentence associated with the charge. Importantly, given my focus on attorneys, the data also include the name of the defense attorney, and the type of defense counsel (i.e., public defender, private assigned counsel, or retained counsel). I can also distinguish between charges filed at the time of arrest and those filed by prosecutors post-arrest—a crucial piece of information, missing from many comparable data sets, which allows me to comment on the scope of charge bargaining.

27. See https://www.nccourts.gov/assets/documents/publications/Sample-list-2017.pdf for examples of offenses by classification level.

28. See Appendix Figure A1 for the misdemeanor sentencing grid and Appendix Figure A2 for the felony sentencing grid. Note that "A" refers to active sentences (jail/prison time), while "C" and "I" refer to community punishment (probation, community service) and intermediate punishment (probation, with the potential for some jail time and/or a rehabilitation program), respectively.

3.1 Sample Construction

I aggregate charges into cases to construct my final data set. I define a "case" to be all charges filed on a single day and county (i.e., court) for the same defendant.[29] To arrive at my final research sample, I restrict my focus to cases both filed and disposed between January 1, 2013 and December 31, 2018 to avoid left- and right-censoring.[30] I keep only cases served via an arrest (that is, a warrant or magistrate's order). I drop cases missing information about the defense attorney. I remove all cases filed in counties without a public defender's office. I drop cases involving more than twelve charges (the ninety-ninth percentile), since cases with higher charge counts typically result in guilty verdicts and long prison terms. Finally, I drop defendants under age 18 (i.e., minors charged as adults) and I winsorize defendant age at the ninety-ninth percentile to correct for implausibly old defendants.

I make two further sample restrictions to support my research design (see Section 4). First, I do not include observations from Mecklenburg or Wake Counties (which contain the state's largest cities, Charlotte and Raleigh, respectively). I exclude these counties because both have unusually well-staffed and high-profile public defender's offices, as well as atypically large shares of minority defendants and high rates of PD representation among minorities. All these facts suggest the meaning of the PD "treatment" is distinct in these jurisdictions from that of the rest of the state.[31] Second, I restrict my focus to cases which receive a final attorney assignment within five days of filing, to preclude endogenous re-assignment to different counsel.[32]

29. The courts will, from the outset, treat groups of charges like this as one unit, potentially including any other charges relating to the same root crime that were entered on different dates. I take a relatively conservative approach in blocking off cases and only group together charges created for the same defendant on the same day. To be clear, if and when a prosecutor adds additional charges, she does so by amending an existing docket number (case).

30. Cases in the data set filed prior to 2013 either involve historical cases or appeals.

31. In Appendix Table A6, I show that my results are robust to including data from Mecklenburg and Wake. These results are very similar to those generated by my preferred sample. My main findings—pertaining to plea bargaining severity and sentence length—are slightly smaller, although still economically large and precisely estimated. Note that my preferred sample still contains several metropolitan areas, including Asheville, Durham, and Greensboro, so I do not interpret my results as unique to rural counties.

32. Conversations with experts on North Carolina's indigent defense system indicate that most initial attorney assignments occur relatively quickly—within days of the first appearance. I focus on cases assigned final counsel within five days of entry into the system since the data exhibit a natural kink at this point. This

3.2 Constructing Outcome Variables

I create several variables to capture the outcomes of pre-trial bargaining. First, I define a defendant's incarceration sentence as longest jail or prison term imposed, among all charges for which the court finds the defendant guilty. Second, I create a measure of a defendant's "expected sentence" based on the lead charge he faces (that is, the charge with the longest potential incarceration term). Specifically, this measure reflects the leave-out average jail or prison sentence given to all offenders convicted of a given charge.[33] Second, I use the same average jail or prison term measure to define an offender's expected sentence from the charges to which he pleads guilty. I measure all sentence and expected sentence outcomes given the dispersion of sentence lengths.[34] Finally, I determine the number of charges that prosecutors add to a given case and create an indicator for whether prosecutors added any additional charge.

Unfortunately, I do not directly observe information on defendants' prior criminal records. The data do report prior record points—the metric by which North Carolina courts determine minimum sentences (see Section 2)—but only for those who are found or plead guilty.

restriction is non-trivial as it removes almost 75 percent of my potential sample. However, 55 percent of cases assigned a final attorney within a month of filing receive one within five days, confirming that the process generally occurs quickly, absent endogenous re-assignment. Longer lag times between filing/first hearing date and attorney assignment dates indicate a combination of administrative delays on the part of PD's offices and (endogenous) switching from originally-assigned attorneys to new counsel, which I cannot observe. Both of these effects detract from my research design, which relies on quasi-random attorney assignment. In Appendix Table A7, I show that my results are robust to alternative cutoffs within two weeks of case filing, although not to including cases assigned counsel more than three weeks after filing—which, again, makes sense, as initial attorney assignments generally occur relatively quickly and defendants with assignments weeks after the first appearance will likely have sorted to receive their preferred attorney.

33. While North Carolina uses a structured sentencing regime with mandatory sentences, those sentences vary greatly within "grids" delineated by defendants' prior convictions (see Appendix Figures A1 and A2). I can only observe prior conviction history if a defendant pleads or is found guilty. Thus, I cannot determine the actual minimum sentence a defendant would have faced had he pleaded guilty to a given charge—a major issue, given my focus on plea bargaining outcomes. Instead, I use the leave-out average sentence meted out for each charge as a measure of the expected sentence a defendant faces for any charge. This measure will tend to provide an (ordinal) ranking of more- and less-severe cases, as well as a (cardinal) sense of how much incarceration time the average given defendant faces given his charges. In other words, I assume that the marginal defendant can expect the same sentence—and thus has the same demographics, case characteristics, and prior record—as the average defendant convicted of the same offense during my sample period. Although quite coarse, this measure has intuitive appeal, and my large sample will tend to de-emphasize problematic outliers.

34. Comparable results using sentences measured in months show similar patterns as my main findings; see Appendix Table A4.

Instead, I use the data to construct two variables that capture past interactions with the court system: the number of prior appearances (since 2013) a defendant has had, and the number of prior felony convictions (again, since 2013) he has experienced. I argue these outcomes, though limited by the scope of my data, adequately proxy for past criminality.[35]

3.3 Summary of Defendant, Case Characteristics by Defense Attorney Type

My data set contains 59,860 unique criminal cases involving indigent defendants, encompassing 29 out of 100 North Carolina counties. Table 1 describes these data. Almost two-thirds of indigent defendants in my sample receive a public defender. Just over half of defendants with court-appointed counsel are nonwhite (53 percent), and 45 percent face a (serious) felony charge.

Comparing PDs' to PACs' cases, the latter include more nonwhite defendants, slightly younger defendants, more felony charges, and defendants facing charges with higher expected sentences. These trends support the notion that PDs, who have higher workloads (see Section 7), pass more "difficult" cases involving more serious charges to PAC, who may have more time to provide a better defense. That behavior would comport with the ethical considerations PDs face when selecting which defendants to represent. Still, my research design (which I describe in the next section) aims to bridge these differences, which point to endogenous selection.

3.4 Summary of Plea Bargaining Outcomes by Defendant Race/Ethnicity

To help motivate my focus on race and pre-trial outcomes, I detail how the results of plea bargaining vary by defendant race and ethnicity. Table 2 reports these outcomes for all defendants as well as for Hispanic, Black, and white defendants separately. This descriptive analysis echoes Berdejó (2018) who, to my knowledge, provides the only analysis (descriptive or otherwise) of racial heterogeneity in plea bargaining outcomes. My data set—in which I observe charges filed by prosecutors after the defendant enters the court system—represents

35. Among convicted offenders in the data, 18 percent have no prior record, while 80 percent have five or fewer prior points, equivalent to one low-level felony conviction plus either one misdemeanor conviction or one repeat offense. This fact suggests habitual offenders—who would have much higher point totals—do not comprise a large share of my sample.

a key addition to this prior work, which cannot comment on disparities in charge bargaining (see Section 2.3).

Data in the second and third columns of Table 2 suggest that experience worse outcomes from the plea bargaining process. Black and Hispanic defendants actually plead guilty less often than whites (33 percent of the time, whereas whites plead guilty 37 percent of the time), and Hispanics are more likely to see their entire case dismissed (50 percent versus 43 percent for whites).[36] These facts highlight the prevalence of case dismissal in state court systems, and indicate that some minority defendants face charges which the prosecutor either cannot prove or otherwise declines to prosecute, in line with evidence that police over-arrest nonwhite suspects on potentially flimsy grounds (Antonovics and Knight 2009; Kochel, Wilson, and Mastrofski 2011). At the same time, prosecutors appear to engage in greater charge bargaining against Black defendants. District attorneys are five percentage points (33 percent) more likely to add additional charges to cases involving Black defendants, which amounts to 0.75 additional charges imposed on the average Black defendant, against 0.58 additional charges for whites. And when they do plead guilty, Black and Hispanic defendants admit to charges carrying 14-15 percent longer expected sentences; they go on to serve up to 23 percent longer prison sentences, conditional on being incarcerated.

These data points do not in themselves demonstrate a causal relationship between minority status and plea bargaining results. But these facts support a key premise of this paper: that, as in other stages of the criminal process, minority defendants fare worse than whites in pre-trial negotiations. My research design will allow me to reflect on whether these manifest differences in the raw data stem from racial bias per se, as well as to assess the extent to which defense attorneys influence these disparities.

36. The prevalence of dropped cases in North Carolina stands contrasts with the 90 percent conviction rate often reported in the federal judicial system, the policy context for most related research (in particular, Rehavi and Starr [2014] and Tuttle [2019]). This comparison underscores the gulf between the federal and state court systems, and reveals a more nuanced pattern of race-based disparities in criminal prosecutions.

4 Research Design

My goal in this paper is to identify the prevalence of racial disparities in plea bargaining, using variation in indigent defense counsel to uncover these patterns. To obtain causal effects, I need to ensure that the type of defense counsel is randomized. Yet, North Carolina public defenders select which cases to represent, subject to the ethical considerations and capacity constraints I discus in Section 2. My research design focuses on cases deferred or accepted by the PD solely as the result of idiosyncratic variation in capacity over time. This section describes my instrumental variables (IV) approach, the validity of my instrument, and the specifications I employ.

4.1 Constructing a PD Capacity Instrument

North Carolina's county-level public defenders' offices process all indigent defendants, assigning some to private counsel and others to full-time public defenders. This setup introduces potential selection bias. To assess the direction of this bias, I use a single linear probability regression to examine the correlations between defendant and case characteristics and PD assignments, after controlling for county-by-time fixed effects that might predict PD assignment rates (see below). I report the results in the first column of Table 3. Confirming a pattern hinted at in Table 1, I find negative correlations between adverse defendant and case characteristics—felony charges, prior records, and minority status, in particular—and the probability of PD representation. Such endogenous referral of more difficult cases to PAC would create a spurious negative correlation between PD representation, plea bargain severity, and sentence length.

However, as I note in Section 2, public defenders' offices appoint counsel mainly in the event that no public defender has spare caseload capacity to provide an adequate defense to an extra client. Logically, then, while the average defendant experiences non-random assignment, some defendants rest on the margin between receiving a PD and receiving a PAC based on the local public defender's current caseload.

Motivated by this intuition, I construct a measure of this variation in caseload capacities that I use to instrument for the probability a defendant is assigned a public defender. Specifically, for every defendant, I compute the leave-out share of indigent defendants who entered the same county court system on the same week and were assigned a PD. When this "PD capacity" measure is high (i.e., many other defendants that week were assigned a PD), then the PD's office exhibits relatively high capacity, which increases the probability that a given defendant receives a public defender rather than private counsel.[37] Figure 1 illustrates the distribution of PD capacity, which ranges from 0 (i.e., no other defendants received a PD) to 1 (all other defendants received a PD), with a median of 0.74.

However, shocks to PD capacity may be correlated with defendant characteristics and outcomes. For example, overall crime rates—and specifically the incidence of violent crime—increase during summer months, which will simultaneously reduce public defenders' capacities and shift the composition of cases towards relatively serious offenses.[38] To overcome this endogeneity hurdle, I include county-by-year-by-day of week and county-by-month-by-day of week fixed effects in all specifications. These controls ensure that I compare defendants whose cases entered the system on comparable days, months, and years, but happened to appear on different weeks, during which the PD's office had idiosyncratically higher or lower capacity. In other words, the fixed effects obviate concerns about seasonality or other systematic time shocks generating spurious correlations between my PD capacity measure and defendant outcomes.[39]

4.2 Validity: Assessing the Randomness of PD Capacity

For my PD capacity measure to deliver a well-defined local average treatment effect (LATE), it must be related to defendant outcomes only via its effect on the probability

37. I focus on assignments during the same week—as opposed to month, for example—because this level of variation reflects the fact that most (67 percent) of all defendants receive their final attorney assignment within seven days of their case being filed, which suggests most PDs' offices make initial attorney assignments relatively quickly, and thus the relevant capacity measure is a short-term (weekly) one.

38. See Jacob, Lefgren, and Moretti (2007) and McDowall, Loftin, and Pate (2012), among others, for evidence of the relationship between weather/seasonality and crime.

39. In Appendix Table A3, I show that my results are generally robust to alternative county-time fixed effect choices, as well as to the elimination of such fixed effects altogether, although estimated effects on incarceration length lose precision in some specifications. I prefer using the fixed effects described here since these best reflect the logic of my approach, which rests on week-to-week variation in PD capacities.

of PD representation. In particular, if PD capacity is excludable, then I should find no correlation between observable case features and my prospective instrument. I evaluate these correlations using a single OLS model in which I regress PD capacity on defendant and case characteristics, plus the county-time fixed effects discussed above. Those estimates appear in the second column of Table 3. None of these the coefficients is statistically meaningful, and I cannot reject the null hypothesis that all estimates are zero (p-value of 0.497). These null findings stand in stark contrast to those in column 1, and provide some reassurance that cases entering the court system when the local PD has high capacity do not differ from those entering when the PD has low capacity.

4.3 First Stage: PD Capacity and PD Representation

It remains for me to show that this PD capacity measure generates meaningful variation in PD representation, my treatment of interest. I estimate this relationship by regressing an indicator for whether indigent defendant i entering court c at time t receives a public defender (PD) on my leave-out capacity metric (Cap), alongside the county-by-year-by-day and county-by-month-by-day fixed effects (X) described above:

$$PD_{ict} = \alpha_0 + \alpha_1 Cap_{ct} + \alpha_2 X_{ict} + \epsilon_{ict}. \tag{1}$$

I cluster my standard errors at the court-week level, since individuals' treatment statuses will be correlated within court-week (the level at which I define my PD capacity measure). Figure 1 visualizes this first stage relationship, and provides evidence that the probability an individual defendant receives a PD increases alongside PD capacity. I confirm this result in Table 4, which presents formal first-stage regression estimates after controlling for court-time fixed effects. I find that PD capacity strongly predicts whether public defender represents the defendant: a 20 percentage-point increase in the share of defendants assigned a PD increases the probability that a given defendant receives a PD by 4 percentage points (5.4 percent of the sample mean). This relationship is precisely estimated: the first-stage F-statistic is

80.78.

I also must assume that the first stage relationship is monotonic—in other words, that every defendant assigned a public defender during a "low capacity" week would have also received a public defender during a "high capacity" week. While not directly testable, this assumption requires that my first stage coefficients be non-negative among all subsamples of my data. Consistent with the monotonicity assumption, I find similar, uniformly positive first-stage estimates across different subsamples; I present these results in Appendix Table A1.

Finally, to help contextualize the LATE supported by this first stage, I characterize complier defendants in my data set. "Compliers" in this setting are those defendants who receive a PD only by virtue of entering the county court system during a "high capacity" week, and would not have received a PD had they entered during a "low capacity" week. To evaluate the share of compliers and complier characteristics, I focus on defendants for whom PD capacity takes its extreme values—namely, 0 and 1.[40] As such, my full-sample first stage coefficient reveals the share of compliers: 20.7 percent. A further 76.7 percent of defendants are always takers who receive a PD regardless of PD capacity; the remaining 2.6 percent of defendants are never takers who are never assigned a public defender. Interestingly, in Appendix Table A2, I show that compliers' average characteristics closely resemble those of my full sample, which suggests my LATE approximates the average treatment effect (on the treated).

4.4 Second Stage: Estimating the Causal Impact of PD Representation

Using my instrumental variables (IV) framework, I evaluate the causal effect of public defender representation on case outcomes. I do so in part using a reduced form specification, which identifies the intent-to-treat (ITT) effect of PD capacity (Cap) on pre-trial and

40. This approach reflects the logic in Abadie (2003), and echoes the procedure used in research designs relying on harsh/lenient judge tendencies for identification, principally Dahl, Kostøl, and Mogstad (2014) and Dobbie et al. (2018). Those studies compare observations in the first and ninety-ninth percentiles of the instrument which, in my context, is tantamount to comparing defendants entering the county court during weeks with PD capacities of zero and one.

sentencing outcomes, Y, for case i entering the county court system c in time t:

$$Y_{ict} = \rho_0 + \rho_1 Cap_{ct} + \rho_3 X_{ict} + \epsilon_{ict}, \tag{2}$$

where X includes the court-time fixed effects discussed above, as well as baseline case and defendant characteristics in some specifications. The ITT coefficient of interest is ρ_1, which captures the impact of a 100 percentage-point increase in PD capacity on defendant outcomes. The identifying assumption is that, conditional on the court-time fixed effects, PD capacity is uncorrelated with unobserved defendant and case characteristics. Equation 2 has the advantage of cleanly describing the relationship between PD capacity and case outcomes, but does not recover the PD treatment effect per se.

Instead, I estimate the treatment-on-treated (TOT) effect of PD representation using a two-stage least squares (2SLS) system in which I instrument for PD representation using PD capacity, Cap:

$$PD_{ict} = \pi_0 + \pi_1 Cap_{ct} + \pi_3 X_{ict} + \epsilon_{1,ict}$$

$$Y_{ict} = \beta_0 + \beta_1 PD_{ict} + \beta_3 X_{ict} + \epsilon_{2,ict}. \tag{3}$$

The TOT parameter, β_1, captures the effect of PD representation on Y among "complier" defendants—those who receive a PD as a result of entering the county court system during a "high capacity" week. The identifying assumption is that PD representation is as good as randomly assigned, insofar as it is driven by PD capacity, conditional on the court-time fixed effects.

4.5 Estimating the Causal Impact of PD Capacity for Minority Defendants

Beyond simply estimating the PD representation effect, however, I want to comment on heterogeneity in the PD treatment effect across white and nonwhite defendants. Again, my goal is to use quasi-random variation in PD representation to see how having a PD influences the racial gap in pre-trial bargaining outcomes (see Table 2). I do so by employing a pooled reduced form specification in which I interact my PD capacity measure (Cap) with an indicator for whether a defendant is Black or Hispanic ($BlackHisp$), and separately control for whether a defendant is Black or Hispanic. The reduced form specification is

$$Y_{ict} = \gamma_0 + \gamma_1 Cap_{ct} + \gamma_2 Cap_{ct} \times BlackHisp_{ict} + \gamma_3 BlackHisp_{ict} + \gamma_4 X_{ict} + \epsilon_{ict}, \qquad (4)$$

where γ_2 captures the ITT effect on Black and Hispanic defendants, relative to the effect (γ_1) on all other defendants (97 percent of whom are white).[41,42] This specification has the advantage of showing the relative magnitudes of the ITT effect on minority and white defendants.

This ITT model maps into a 2SLS system that isolates the treatment effect of PD representation per se on Black and Hispanic defendants. The specification is:

$$PD_{ict} \times BlackHisp_{ict} = \kappa_0 + \kappa_1 Cap_{ct} \times BlackHisp_{ict} + \kappa_2 BlackHisp_i + \kappa_3 X_{ict} + \epsilon_{1,ict},$$

$$Y_{ict} = \psi_0 + \psi_1 PD_{ict} \times BlackHisp_{ict} + \psi_2 BlackHisp_{ict} + \psi_3 X_{ict} + \epsilon_{2,ict}, \qquad (5)$$

where the 2SLS coefficient of interest, ψ_1, captures the effect of PD representation on Black and Hispanic defendants, relative to the PD representation effect among all other defendants.[43] Unlike Equation 4, Equation 5 does not explicitly estimate an effect among non-Black, non-Hispanic defendants. Rather, I leave it to my reduced-form estimates to convey the relative magnitude of the treatment effect among white defendants.[44] Equation 5 also

41. In Appendix Table A8, I present results from a similar specification in which I interact my instrument with an indicator for whether a candidate is Black (rather than Black or Hispanic). Those estimates show materially similar results.

42. The coefficient γ_2 also lends itself to a slightly different interpretation: the difference in the racial disparity in Y (given by γ_3) driven by PD capacity. Interpreting γ_2 as a kind of difference-in-differences coefficient like this assumes not just that PD capacity is exogenous, but that cases involving Black and Hispanic defendants are comparable to those involving white defendants. That level of randomization is not guaranteed by my research design, and so γ_3 will be very sensitive to my choice of covariates. As such, I do not interpret γ_2 as a DiD estimate to remain transparent about what my research design does and does not claim to accomplish.

43. I also evaluate the race-specific impact of PD representation using Equation 3 run on separate subsamples. This exercise helps me gauge the robustness of my primary 2SLS specification (Equation 5) to an equally reasonable approach. In Appendix Table A9, I show similar results using this subsample approach, although the resulting estimates are a bit less precise.

44. In practice, my results cut clearly by defendant race/ethnicity, and this limitation of the model omits

relies on a similar but distinct first stage as the one I present in Table 4. I report the associated first stage and reduced form estimates in Appendix Table A5. The first stage is precisely estimated, with an F-statistic of 3,364; reduced-form estimates from this interacted-treatment framework show quantitatively similar effects as those estimated using Equation 4. The overlap between estimates from these two approaches attests to the robustness of my results.

5 Do Public Defenders Affect Plea Bargaining and Sentencing Outcomes?

In Section 3, I motivated my discussion of race/ethnicity and plea bargaining by describing several dimensions along which Black and Hispanic defendants experience worse outcomes in pre-trial negotiations relative to their white peers. Now, I motivate my focus on court-appointed counsel by showing that defendants represented by public defenders appear to stake a stronger bargaining position than defendants represented by private attorneys. I do so utilizing the IV framework discussed in Section 4.

I begin by examining public defenders' impact on charging and plea bargaining outcomes. Using my reduced form and 2SLS specifications (Equations 2 and 3), I estimate the effect of PD capacity on key outcomes from the pre-trial bargaining process: pre-trial release conditions (as measured by the required bail amo8nt to be posted); additional charges imposed by the prosecution; whether the defense and prosecution strike any plea deal; and the conditions of that plea deal (whether it involves jail/prison, and the expected incarceration length from a guilty plea).

In the first panel of Table 5, I show that PD representation leads to more favorable bail and charging outcomes for indigent defendants. Reduced-form results in columns 2 and 3 capture the effects of 100 percentage-point increases in PD capacity. I focus on the 2SLS effect of PD representation (in column 5) since these coefficients lend themselves to more straightforward interpretations.

very little information.

Estimates in the first row of Table 5 show that a PD representation leads to a marginally significant 55.7 percent lower bail thresholds. More lenient bail conditions might lead to greater rates of pre-trial release and thereby give defendants more flexibility and time to negotiate a favorable plea bargain, although (again) I caution that this estimate is not precisely estimated.[45] Moreover, indigent defendants face nearly 0.79 fewer prosecutor-imposed charges (95 percent of the mean among defendants assigned a private attorney) and are 22 percentage points (81 percent) less likely to see any charges added to their case. These point estimates suggest that PD representation forestalls prosecutor-imposed charges. And, since additional charges represent a way for prosecutors to extract guilty pleas on tougher charges, both data points suggest that PD representation helps defendants maintain a stronger bargaining position, relative to defendants represented by private counsel.

The second panel of of Table 5 shows comparatively mixed effects of PD representation on case outcomes. Defendants represented by public defenders are no less likely to plead guilty or receive an incarceration sentence relative to defendants assigned private counsel.[46] Economically large but statistically imprecise estimates suggest defendants represented by a public defender plead guilty to charges carrying 27 percent lower sentences. On the other hand, I find an economically and statistically significant effect on sentencing (conditional on incarceration): indigent defendants serve 59 percent shorter incarceration spells than those represented by private attorneys. While sizeable, this magnitude is not implausible. The fact that public defenders preclude additional prosecutor-imposed charges (which mostly carry harsher mandatory minimum sentences than the original lead charge) suggests the effect on

45. Robustness checks in the appendix show more precise effects when I tweak my sample design and specification, which leads me to conclude that this effect is meaningful. Note that I do not observe whether a defendant was actually released pre-trial, although, logically, more lenient bail thresholds will likely encourage defendants to secure a bond and obtain their release. See Dobbie et al. (2018), Leslie and Pope (2017), or Stevenson (2018) for a deeper discussion of how pre-trial incarceration impairs a defendant during plea bargaining.

46. To be fair, the 2SLS point estimates in both cases are economically large, but both coefficients are estimated very imprecisely, and rbustness checks in the appendix confirm that this effect is tenuous at best. As such, I do not draw any meaningful conclusions from these findings. Still, I acknowledge that the increase in guilty pleas, to the extent this effect is "real," could partly drive my results by reducing the average severity of cases resulting in guilty pleas.

sentencing is in some sense mechanical: defendants with PD representation face less serious charges and, given North Carolina's structured sentencing regime, that means they face lower minimum sentences if found guilty. In other words, I interpret this effect on sentence length as a product of charge bargaining, not sentence bargaining. I return to this point in the next section, in which I consider how the PD effect on the severity of plea deals and sentences varies between white and non-white defendants.

6 Is There Evidence of Racial Bias in Plea Bargaining?

In the previous section, I identified economically meaningful, positive effects PD representation on pre-trial charging outcomes among all defendants, as well as mixed but suggestive evidence of similar effects on defendant sentencing. Beyond simply identifying this PD effect, though, my goal is to use this variation in indigent defense counsel to comment on racial and ethnic disparities in plea bargaining. Intuitively, PDs have more experience negotiating with prosecutors, which could improve defense attorneys' ability to negotiate on behalf of minority defendants in particular, consistent with Bibas (2004) and Richardson (2014), since these prior cases will alert them to biased plea terms and curtail their use of race as a signal of their clients' guilt.

In the narrowest sense, the results in this section explore heterogeneity in the PD representation representation effect by defendant race and ethnicity. But I interpret my results more broadly: there should not be any race-specific effects of PD representation among otherwise identical defendants in an unbiased court system. Therefore, the presence of race- or ethnic-specific effects could signify bias on the part of prosecutors and/or defense attorneys, provided defendants of different backgrounds and with different attorney types are comparable.

Below, I estimate how PD representation affects pre-trial outcomes and sentence length for minority (Black and Hispanic) and white defendants. Note that I refer to the non-Black, non-Hispanic subsample as "white" since only 3 percent of this subsample identifies as Asian or "other."[47] I then justify my preferred interpretation of these findings as evidence of racial and ethnic bias.

6.1 Does the PD Representation Effect Differ for Minority Defendants?

To assess any differences in how PD representation affects minority and white defendants, I first estimate the pooled reduced-form regression given by Equation 4. Intuitively, this model captures both the effect of PD capacity for white defendants as well as the effect of PD capacity for Black and Hispanic defendants, relative to the effect on white defendants. In all specifications, I include the court-time fixed effects described in Section 4, as well as case and demographic controls.[48] Estimates in columns 5 and 6 also include controls interacted with an indicator for whether the defendant is Black or Hispanic, which in effect adjusts for observable differences between minority and white defendants.

I report estimates from these models in Table 6. I find no evidence of racial differences in the PD effect on bail amounts or additional prosecutor-imposed charges: the reduced-form interaction estimates are not statistically different from zero. In other words, white and non-white defendants realize similar gains from PD representation along these dimensions. Likewise, I find no evidence that PD representation affects the probability that Black and Hispanic defendants plead guilty or receive an incarceration sentence: the interaction effects are statistically zero and economically small, as are the (non-interacted) baseline estimates.[49]

Interestingly, in the second panel of Table 6, I find no evidence that white defendants' plea outcomes vary at all by the type of indigent defense counsel: reduced-form estimates in

47. In Appendix Table A8, I show that I obtain similar results when I focus exclusively on Black and white defendants.

48. I only report results using covariates here for conciseness. The final column of Appendix Table A3 shows that I obtain similar results when I omit controls altogether. The remaining columns of this table also show that adjusting the fixed effects I include in the model does not materially affect my conclusions.

49. I do note that I find some evidence—particularly in the 2SLS approach I discuss below—that nonwhite defendants with PD representation are up to 5.3 percentage points (16 percent) more likely to enter a plea deal (plead guilty), although this evidence remains statistically imprecise. This effect, such as it is, could suggest that PDs cooperate more with the prosecution and obtain more lenient plea deals as a result. Again, though, given how noisy these findings are, it is difficult to draw firm conclusions; I acknowledge this possibility, though, and claim it lines up with my overall conclusion that PD representation appears to achieve better outcomes for minority defendants, as these plea deals do not accompany an uptick in incarceration rates.

columns 3 and 5 indicate that, relative to white defendants represented by public defenders, whites with private counsel do not plead guilty to substantially more severe charges, nor are they sentenced to different spells in jail or prison, conditional on being incarcerated: both estimates are statistically and economically insignificant, with a virtually zero effect on sentencing.

By contrast, in column 6 (which presents estimates from my preferred specification), I report substantial gains from PD representation among Black and Hispanic defendants: non-white defendants plead guilty to less severe charges (5 percent lower expected sentences for every 20 percentage-point increase in PD capacity) and serve shorter sentences (6 percent shorter sentences per 20 percent increase in PD capacity). Both estimates are significant at the one-percent level.[50]

Strictly speaking, these latter estimates mean that Black and Hispanic defendants with a PD experience better outcomes than white defendants with a PD. But I have established that white defendants experience statistically similar outcomes with PD and PAC representation. As such, my findings support two conclusions. First, Black and Hispanic defendants with PAC representation experience longer sentences and plead guilty to more serious charges during pre-trial negotiations than do equivalent defendants with PD representation. Second, Black and Hispanic defendants with PAC representation fare worse along these same dimensions relative to *white* defendants who also have PAC representation.

To crystallize these results, I present 2SLS coefficients from Equation 5 in final column of Table 6. Each coefficient captures the effect of PD representation among Black and Hispanic defendants, relative to the effect on white defendants. The specification includes full interacted controls. Note that this specification does *not* draw on the reduced-form

50. In Appendix Table A4, I explore reduced-form effects on other outcomes of potential interest, including those related to probation. I find some evidence that Black and Hispanic defendants with PD representation are more likely than white defendants to receive probation, although whites with PD representation may be slightly *less* likely to receive probation than those with PAC representation. The competing signs and imprecision make interpreting these results challenging. I also find similarly imprecise negative effects on court fines and fees imposed on Black and Hispanic defendants relative to white ones. In general, I see these further results as part of the broader pattern I document in Table 6, but I do not draw firm conclusions about these outcomes.

estimates shown in the remainder of the table; appropriate reduced form, first stage, and robustness estimates appear in Appendix Table A5.

Point estimates in column 7 confirm that the PD representation effect on charging outcomes does not vary by race—in other words, these outcomes are, at best, marginally significant.[51] Again, these estimates do *not* mean that PD representation has no effect on Black and Hispanic defendants' charging outcomes; rather, these results, in light of the reduced-form estimates in prior columns, imply that PD representation has a substantial effect on charging outcomes among *all* defendants, and that this effect does not differ by defendant race/ethnicity.

The final two rows of column 7 emphasize that PD representation has an economically significant effect on the severity of Black and Hispanic defendants' plea bargains. Representation by a public defender leads nonwhite defendants to plead guilty to charges carrying 38.4 percent shorter sentences, and to serve 52.2 percent shorter incarceration terms. Both effects are precisely estimated. Altogether, these results paint a picture of public defenders helping Black and Hispanic defendants secure less punitive plea deals and, ultimately, sentences.

6.2 Racial Disparity, or Racial Bias?

The question remains whether these worse plea deals among Black and Hispanic defendants with private counsel signify racial and ethnic bias per se, or simply disparities in case and defendant characteristics that in turn drive differences in plea deal and sentence severity. I argue that these results in fact point to systematic bias, although I remain agnostic over *whose* biases—public defenders', private attorneys', or prosecutors'—drives my results for the time being; I take up that issue in Section 7. I outline my argument below, showing how alternative explanations besides out-and-out bias do not explain my findings.

51. In Appendix Table A5, I show that removing the relatively restrictive set of interacted control variables improves precision on many of these estimates and makes many statistically significant. I infer that these more precise results spuriously capture differences across white and non-white defendants with PD representation, and do not read too much into those findings, although all support my overall conclusions and are consistent with my other results.

6.2.1 Are Minority Defendants Observably Different from White Defendants?

As I note above, my preferred regression results in columns 5, 6, and 7 of Table 6 account for defendant and case characteristics, and I allow those effects to vary for minority defendants. Thus, to the extent that white and non-white defendants differ along observable dimensions—age, gender, the type of charges filed, and recent interactions with the court system—I control for those differences. Moreover, the similarity of my estimates with and without interacted controls (comparing columns 3 and 4 to columns 5 and 6) indicate that observable differences between Black, Hispanic, and white defendants do not drive my results. In fact, if anything, the results with interacted controls are larger than those without.

6.2.2 Do Minority Defendants with PDs Face Less Serious Charges?

It is possible that Black and Hispanic defendants with PD representation simply face less serious charges than similar defendants with PAC representation, as well as whites with PD representation. To extent that charge severity is correlated with the battery of covariates I include in all specifications, I have already addressed this possibility. But, because such a pattern would generate a spurious negative correlation between PD representation and sentencing outcomes among Black and Hispanic defendants, I directly speak to this possibility and control for the "expected" sentence a defendant faces (used and defined earlier in this paper), which proxies for the seriousness of the case against the defendant.

In the second column of Table 7, I present reduced form estimates in which I control for the (log) expected sentence a defendant faces, and interact this variable with an indicator for whether the defendant is Black or Hispanic.[52] Unsurprisingly, controlling for expected sentence length reduces the magnitude and precision of my estimates, since I effectively estimate the difference not only across attorneys and by race/ethnicity, but relative to the mean sentence for a given charge as well. The resulting estimates, then, refer to differences in plea bargain and sentence severity relative to the (race-specific) average sentence imposed on a defendant's lead charge. Even with this exacting standard, I continue to find no meaningful

52. See Section 3.2 for a definition of the expected sentence variable.

PD representation effects among white defendants, and economically large (albeit, imprecise) effects among Black and Hispanic defendants. This persistence reinforces my claim that white and non-white defendants are fundamentally similar, both within and across attorney type.

6.2.3 Do Minority Defendants with PDs Have Shorter Criminal Records?

I next try to determine whether differences in prior criminal records across attorneys and defendant demographics explain my results. For example, public defenders might simply encourage Black defendants with fewer prior convictions to plead guilty, which, under North Carolina's structured sentencing regime, would mean that the average Black defendant faced lower mandatory minimum punishments. Unfortunately, as I discussed in Section 3, I do not observe a defendant's prior record points—which reflect his prior convictions—unless he is found (or pleads) guilty. As such, prior record information is technically endogenous, and will reflect the composition of defendants sentenced, rather than all defendants.

Still, I examine what happens to my estimates when I include these controls, which addresses the possibility—laid out above—that the average Black or Hispanic defendant with a guilty verdict and PD representation has a less serious criminal history than the average white defendant with a guilty verdict and PD representation. Results in columns 3 and 4 of Table 7 show that controlling for defendant prior points (and defendant prior points interacted with race/ethnicity) has no bearing on my results. This null finding again supports my contention that white and non-white defendants in this sample are comparable.

6.2.4 Do PDs Face More Lenient Prosecutors?

Of course, plea deals and sentencing do not just depend on defense counsel—prosecutors must accept the terms of any deal, and their zealousness will likely shape the terms of any pre-trial bargain. As I discuss in Section 7, I hypothesize that prosecutors play a key role in driving my results. But I need to first account for the possibility that Black and Hispanic defendants with PAC representation face tougher prosecutors (and thus have worse outcomes) than Black and Hispanic defendants represented by PDs. In other words, prosecutors are not randomly assigned, and if prosecutor zeal varies systematically by defendant race/ethnicity and type of counsel, that would help explain my results.

To test for such an effect, I augment my reduced-form specification with prosecutor fixed effects, which will control for any observable and unobservable prosecutor characteristics, including harshness, race, or experience. Estimates in column 5 of Table 7 show that including prosecutor fixed effects attenuates my estimates slightly, but leaves a sizeable and significant gap between white and non-white defendants.

6.2.5 The Role of Defense Attorney Characteristics

I next consider the role of attorney characteristics, which might vary across public defenders and private attorneys and explain my findings. In fact, Shem-Tov (2020) argues that the benefits he observes from PD representation stem largely from the fact that public defenders (in the federal court system and San Francisco's district court) have better credentials than the private attorneys drafted in to serve as indigent counsel. Whether the same pattern holds in North Carolina remains unclear: unlike the two institutions Shem-Tov (2020) examines, my study focuses on relatively low-profile PDs' offices in a racially diverse context with a (relatively) low conviction rate. Consequently, the PD treatment effect I estimate differs substantially from the one Shem-Tov (2020) uncovers—which suggests the mediating mechanism might differ as well.

In the sixth column of Table 7, I present results in which I control for defense attorney fixed effects, which in practice will control for any observable and unobservable characteristics that might distinguish PDs from PACs in my sample. Including these controls has little effect on my results.

In fact, unlike Shem-Tov (2020), I observe few differences between PDs and PACs in my data set. I obtain demographic information about a subset attorneys' using a "fuzzy match" with the North Carolina voter roll. The voter roll contains the age, gender, race, and political party affiliation of all voters in North Carolina; the merge process is imperfect, due to the lack of standardized attorney names in the court records and ambiguity surrounding common names in the voter roll. Of the 1,122 defense attorneys in my research sample, I successfully match 261 (23 percent; 134 PDs and 127 PAC) to the voter roll. I describe these attorneys' characteristics in Appendix Table A10. While I find that private attorneys are more likely to be female and slightly younger, the two groups otherwise have very similar demographic makeups. To the extent these characteristics are correlated with attorney quality, that is also similar across attorneys.[53] Overall, I conclude that differences in PD and PAC backgrounds do not explain the racial disparity in plea bargaining.

6.2.6 Is There Evidence of Moral Hazard?

Finally, I investigate the role of moral hazard in my setting. Agan et al. (2020) argue that private assigned counsel (in Texas) perform worse than retained attorneys largely because, in their capacity as contracted indigent defenders, they have an incentive to resolve cases quickly in order to accept more lucrative cases on which they serve as retained counsel. If, for example, private assigned attorneys in my setting put in much less effort towards defending Black clients, that could explain the racial gap I observe in sentencing outcomes.

In the last row of Appendix Table A4, I present reduced-form estimates that indicate public defenders actually resolve cases more quickly than do private attorneys. This finding is consistent with an alternative version of moral hazard from that in Agan et al. (2020): the evidence suggests private attorneys drag out a case in order to collect more fees from the state. Prolonging a case could harm defendants if, for example, defense attorneys ignore generous initial plea offers, or it frustrated prosecutors exact harsher plea terms. Importantly, though, the PD effect on time to case disposition does not vary by defendant race/ethnicity. In other words, PAC resolve cases more slowly than PDs, but do not resolve cases with Black or Hispanic defendants even more slowly than they do with white defendants. That suggests

53. In Appendix Table A11, I re-estimate my preferred reduced-form specification using the subset of cases for which I have defense attorney demographic data. The results appear in columns 1 and 2, and show qualitatively similar results to those reported in Table 6. Although these latter findings are imprecisely estimated and 5-20 percent smaller in magnitude, they show the same basic pattern: PD representation benefits Black and Hispanic but not white defendants. In columns 3 and 4, I show how these results change once I control for attorney demographic characteristics and—crucially—allow those controls for vary by defendant race/ethnicity, which would could help explain my results if PD or PAC characteristics varied by race. Again, my estimates attenuate slightly, but the overall race-specific pattern remains, which, again, means that it is unlikely attorney characteristics per se (aside from type of counsel) explain the racial disparity in plea bargaining and sentencing.

moral hazard might play a role here, in so far as marginal effort directed towards Black and Hispanic defendants may have higher returns than marginal effort directed towards white defendants. Still, to the extent this story plays out, it raises the obvious question of why attorney effort does not matter for whites but does matter—quite a lot—for minorities. On the whole, then, moral hazard offers, at best, a partial explanation for the effects I observe.

7 Explaining Racial Bias with a Model of Plea Bargaining

The results presented thus far indicate that public defenders realize more favorable outcomes in pre-trial bargaining for indigent Black and Hispanic defendants compared to private-sector attorneys. I have argued—and, at this point, take as a given—that I have controlled for any differences across cases involving white and non-white defendants with different indigent counsel. In other words, following my analysis in the previous section, I assume that the racial disparities I observe in plea bargain severity and sentencing do not stem from pre-existing differences across cases, and in fact signify racial or ethnic bias.

My goal in this section is to clarify whose biases drive these effects. Motivated by arguments proposed by legal scholars—namely, Berdejó (2018), Bibas (2004), and Richardson and Goff (2013)—I hypothesize that defense attorneys use defendant race as a proxy for guilt, but that more frequent engagements with tough prosecutors can alert them to unfair charging behavior. I formalize this intuition using a signaling model of plea bargaining and provide empirical support for the model's predictions.

7.1 A Model of Plea Bargaining with Defense Attorneys

Standard economic models of plea bargaining incorporate neither defense attorney characteristics, nor defendant race, and so shed little light on the mechanism that makes PD representation so valuable for minority defendants. Most plea bargaining models assume that a defendant accepts a plea deal because he expects an even worse trial outcome. I accept this premise. But this "shadow trial" paradigm does not, on its own, explain my results: why do minority defendants (but not white defendants) represented by private counsel (but not by PDs) accept harsher plea deals, even when faced with similar potential sentences?

I bridge this gap in the theoretical literature with a straightforward model of plea bargaining featuring two actors—prosecutors and defense attorneys—and asymmetric information. The key insight of the model is that prosecutors exercise substantial sway over case proceedings, and in effect determine the charges (and, thus, potential sentence) that a defendant will face at trial. To reflect this fact, my model gives the prosecutor accurate information about the strength and severity of the case, and I assume that only the prosecutor knows the outcome from a trial with certainty. Consistent with the law literature, I model the defense attorney's problem as a signal extraction process: she does not know her whether her client is guilty or not, or the sentence he will face if found guilty at trial. Rather, she must infer that information from the prosecutor's plea offer and her own prior beliefs about her client's culpability (factoring in his race) and the prosecutor's tendency to overcharge. This paradigm—in which the prosecutor has more information than the defense—departs from most (if not all) existing plea bargaining models, but captures the legal consensus that plea bargaining strengthens prosecutors' hands, and introduces the possibility of defense attorney racial discrimination.

The model functions as a game with two periods. In the first period, the prosecutor makes a plea offer to the defense attorney in which he chooses how much "bluster" (e.g., additional charges, or tougher sentences) to include in his demands. In the second period, the defense attorney receives the offer and must disentangle the share of the plea offer that accurately reflects the facts of the state's case against her client (which predict trial outcomes) from the DA's bluster. If she perceives that the plea deal is "fair"—no more severe than what she infers to be facts of the case—then she accepts. Otherwise, she rejects the plea deal, and the case goes to trial.

7.1.1 Setup and the Prosecutor's Problem

At the beginning of the game, the prosecutor observes f, the facts of the case at hand. He knows with certainty that, if the case goes to trial, the judge or jury will assign sentence

$j(f)$. He also knows that if the case goes to trial, he will have to spend time and energy proving the defendant's guilt, for which he incurs a cost $t_P(f)$. I assume that $j'(f) > 0$ and $t'_P(f) < 0$ (i.e., more damning facts lead to harsher punishments at trial and lower effort costs of prosecution).

The prosecutor wants to maximize the sentence imposed on the defendant. He can do so either by going to trial, with its associated cost, or by persuading the defense attorney to accept a plea deal he proposes. A plea offer $\phi(b, f)$ reflects both the facts of the case f and a degree of "bluster"—extra charges or added prison time that the prosecutor claims he can prove at trial, but are not supported by the facts. I assume $\frac{\partial \phi}{\partial f} > 0$ and $\frac{\partial \phi}{\partial b} > 0$ (i.e., the sentence associated with the plea offer increases with the level of bluster and seriousness of the facts of the case). The prosecutor chooses an optimal level of bluster b^*, and pays a cost $c(b^*, f)$ to do so, where $\frac{\partial c}{\partial f} > 0$ and $\frac{\partial c}{\partial b} > 0$ (i.e., blustering entails effort—filing paperwork, haggling with the defense—and blustering on already-serious cases takes more effort).

If the optimal plea offer (less the costs of blustering) leads to a longer sentence than the trial outcome, then the prosecutor prefers to make the deal. Otherwise, he would rather go to trial. Formally, the prosecutor solves

$$\max\{\phi(b^*, f) - c(b^*, f), j(f) - t_P(f)\}.$$

7.1.2 Stages of the Game and the Defense Attorney's Problem

In the first period of the game, the prosecutor chooses b^* and makes a plea offer, $\phi(b^*, f)$, to the defense attorney. In the second period of the game, the defense attorney either accepts or rejects the plea offer. The defense attorney knows the trial outcome for any set of facts $j(f)$, and faces a cost $t_D(f)$ of representing a client at trial when faced with facts f, where $t'_D(f) > 0$.

The defense attorney's goal is to minimize her client's sentence, but she does not observe f directly. Rather, she must use the prosecutor's plea offer $\phi(b^*, f)$ as a noisy signal of f.

She interprets the plea offer as an implicit function $\widetilde{f}$ of b^*: $\widetilde{f} = s(b^*)$. The defense attorney then predicts the true facts of the case to be $\widehat{f}$, which is some share θ of $\widetilde{f}$:

$$\widehat{f} = \theta(r, n, l) \times \widetilde{f},$$

where $\theta \in [0, 1]$. The weight θ is a function of the defendant's race ($r \in \{white, nonwhite\}$), the number of interactions the defense attorney has ever had with the prosecutor (n), and the defense attorney's current caseload (l). Based on her inferred $\widehat{f}$, and facing a cost $t_D(\widehat{f})$ of going to trial, the defense attorney faces a simple decision rule in the second period over whether to accept or reject the plea deal $\phi(.)$:

$$\begin{cases} Accept & \text{if } \phi(b^*, f) \leq j(\widehat{f}) + t_D(\widehat{f}) \\ Reject & \text{else.} \end{cases}$$

Using backward induction, the prosecutor's optimal level of bluster b^* makes the defense attorney indifferent between going to trial and accepting the deal: $j(\widehat{f}) + t_D(\widehat{f}) = \phi(b^*, f)$.

7.1.3 Model Intuition and Implications

Intuitively, the model assumes that prosecutors exhibit racial bias in so far as defense attorneys fail to recognize that bias due to their own assumptions about the correlation between "fair" sentences and defendant race. In contrast with Tuttle (2019), my model minimizes the role of prosecutorial animus against minority defendants. I make this choice because my data highlight variation in minority defendant outcomes by defense counsel, even within prosecutor (see Table 7). If certain prosecutors really are motivated by racial prejudice, then they would push for harsher plea deals irrespective the type of defense counsel. Instead, I argue that the racial disparities in plea bargaining outcomes I observe result from opportunistic discrimination: prosecutors, in their universal effort to secure tough sentences, take advantage of defense attorney assumptions about minority defendants' guilt. One can easily extend the model to incorporate prosecutorial prejudice—for example, by reducing the costs of trial for minority defendants—without affecting my overall conclusions with regard to defense attorneys.

The predictions of the model hinge on the function $\theta(.)$, which captures the defense attorney's willingness to take the prosecutor's offer at face value. The racial disparities in plea bargaining that I have found in this paper suggest that, for a given (n, l), $\theta(white, n, l) < \theta(nonwhite, n, l)$ for the average private attorney—in other words, at least some private attorneys use race as a proxy for defendant guilt, as Berdejó (2018) suggests.[54]

But the function θ also includes two other salient characteristics that distinguish PDs from private attorneys. A defense attorney's prior dealings with the DA—n—could make her more skeptical of the prosecutor's offer (i.e., $\frac{\partial \theta}{\partial n} < 0$). For example, prior interactions might reveal habitual overcharging behavior, consistent with the logic in Bibas (2004). Conversely, an attorney's caseload l could have the opposite effect (i.e., $\frac{\partial \theta}{\partial l} > 0$) if defense attorneys are more likely to use race as a signal of defendant guilty when their time is scarce, per Richardson and Goff (2013). My results, which suggest PDs exhibit less racial bias on balance, indicate that the (positive) marginal effect of additional interactions with the DA outweighs the (negative) marginal effect of greater caseloads. I provide empirical support for this conclusion below.

7.2 Empirically Evaluating the Model's Implications

My model of plea bargaining suggests that, on balance, PAC realize worse outcomes for minority defendants because they view prosecutors' offers with less skepticism than do PDs. To test the primary implication of the model—namely, that PDs engage more with prosecutors and thus treat their plea offers as potentially biased—I explore heterogeneity in my results by PD characteristics.

I first document how PDs and PAC differ along two margins that feature prominently in the model—caseloads and interactions with individual prosecutors. For a given defendant i,

54. I do not speculate as to why defense attorneys make this assumption, which could result from racial animus, statistical discrimination, or, more subtly, from a prediction that judges and juries are biased against nonwhite defendants.

I define his defense attorney's caseload to be the number of other cases she has been assigned to but has not disposed at the time case i enters the court system. To create a measure of a given attorney's interactions with a given prosecutor, I sum up all cases on which the two served over my sample period, and take the log of this measure. The goal of this approach is not to capture "recent" engagements, but rather to get a identify which DA-defense attorney match-ups occur relatively frequently (bearing in mind that my sample window is a relatively short period in these lawyers' careers), and to see how cases involving these pairs compare to those that involve attorneys who rarely meet.

Figure 2 plots (log) defense attorney caseloads and (log) prior interactions across cases over my PD capacity instrument, as in Figure 1. Both plots show a clear positive trend, which confirms an institutional feature of the court system: PDs serve in more cases than private attorneys, and are thus have much more experience facing off against a given prosecutor. This gap between PDs and PAC, though widely acknowledged in the relevant law literature, has been comparatively overlooked in quantitative assessments of indigent defense.

To evaluate how familiarity with the prosecutor and caseloads affect PDs' performance, I re-estimate my reduced form specification (Equation 4), focusing on different subsamples of PDs: those with above- and below-median levels of engagement with prosecutors, and above- and below-median caseloads. I include defense attorney fixed effects to control for unobserved differences across attorneys that distinguish those with large caseloads and interaction rates. I also control for the (log) total number of cases an attorney disposes over my sample period to ensure I do not pick up any effects of greater experience or benefits from disposing many cases per se on case outcomes. Again, I want to isolate effects driven by high levels of interactions with the DA and caseloads, both of which are strongly correlated with total cases disposed, but are distinct channels.

My results appear in Table 8. Overall, I find that my main findings are driven by public defenders with above-median levels of engagement with prosecutors: Black and Hispanic defendants represented by one of these PDs see 41 percent lower expected sentences from guilty pleas, and 31 percent shorter sentences, conditional on incarceration. The effects stemming from PDs with lower rates of engagement with prosecutors are insignificant by comparison, which indicates that PDs and PAC perform almost identically on average. To some degree, this effect appears driven by high-interaction PDs' relatively worse performance among non-Black, non-Hispanic defendants, although imprecise point estimates limit the conclusions I can draw from these results. Still, if this pattern is "real," it could indicate that PDs who frequently engage with a given DA make a trade-off between securing preferable outcomes for minority clients and accepting slightly worse outcomes for white clients.

By contrast, I find that having a higher caseload appears to worsen PDs' performance among defendants, as evidenced by (weakly) larger effects on non-Black, non-Hispanic defendants. However, higher caseloads do not seem to harm Black and Hispanic defendants on balance, and I find no meaningful differences across PDs with high and low caseloads. These results support the notion that larger caseloads harm attorney performance, but cast doubt on the common assumption in legal research that caseloads overtax PDs and encourage racial discrimination (e.g., Richardson and Goff [2013]).

These results suggest defense attorneys might use race as a signal of defendant culpability in the absence of additional information about the prosecutor's intentions. The results support the implications of the model: that PDs, who have more experience working with (potentially biased, or aggressive) prosecutors have more information with which to bolster their clients' bargaining position in pre-trial negotiations. These findings shed new light both on the importance of defense attorneys' information sets and incentives during the criminal process, but also points to a source of racial disparities in the criminal justice system.

8 Concluding Discussion

Pre-trial bargaining comprises a crucial stage in the criminal justice process, a phase in which a defendant might inadvertently agree to serve unnecessarily punitive sentence. My results indicate that racial discrimination during plea bargaining drives well-known racial disparities in sentencing. Moreover, this paper clarifies what drives this discrimination: racial bias in plea bargaining appears to be opportunistic, rather than the product of out-and-out prejudice. Prosecutors pursue harsher plea deals when facing defense attorneys they rarely engage and thus have the least ability to distinguish the actual strength of the DA's case. That pattern suggests better-informed defense counsel, familiar with prosecutors' tactics, can help forestall unfavorable outcomes for minority defendants.

This insight into how defense counsel functions in the plea bargaining process expands on a nascent empirical understanding of attorneys' role in the criminal process as well as the theoretical literature on plea bargaining. Recent work has come a long way towards clarifying defense attorneys' role in the criminal process, but mine is the first (to my knowledge) to tie together defense attorney capability and perhaps the most salient feature of the criminal justice system: its inequity. Through a straightforward model of plea bargaining in which I abstract from the prosecutor-defendant dynamic that defines existing theoretical frameworks, I provide formal intuition for a fact long recognized in (qualitative) legal scholarship: defense attorneys have complex incentives and information sets, which color how they perceive their clients' culpability and, in turn, defendants' pre-trial bargaining positions. Understanding and addressing racial disparities in the court system hinges on recognizing defense attorneys' critical role in the criminal process, one which they do not always live up to.

The policy implications are stark, yet straightforward. The fact that white defendants experience similar outcomes with either PD or PAC representation suggests the state is leaving money on the table: North Carolina could simply re-allocate public defenders to minority defendants, requiring no additional costs on attorneys, and no worse outcomes from white defendants, all while improving minority defendants' outcomes. In fact, a back-of-the-envelope calculation suggests that, by avoiding having Black and Hispanic defendants serve unnecessary incarceration time, the state could save upwards of $12,395 in incarceration costs per defendant. Moreover, Rose and Shem-Tov (2019) suggest the deterrence effect of incarceration in North Carolina is minimal, which implies the expected costs due to incarceration for future crime is relatively low—about $1,146 on average over 3 years. That means assigning a public defender to the average minority defendant saves the state $11,248 over three years.[55]

Plea bargaining is entrenched in the judicial landscape. To the extent that the plea-based regime exacerbates racial inequity in the courts, those disparities are here to stay. But this paper offers a partial remedy within the confines of this imperfect system: by reallocating defense counsel and allotting more experienced attorneys to minority defendants, indigent defense systems can create net cost savings and properly realize nonwhite defendants' right to legal counsel.

55. Specifically, Rose and Shem-Tov (2019) find that each month of incarceration reduces the probability of committing another crime within three years by 0.00794 percentage points. My reduced form estimate on sentencing in months (given in Appendix Table A4) suggests a Black or Hispanic defendant with a PD serves of -4.709 + 0.765 = -3.944 months in prison. That gives an added probability of future crime of 3.13 percentage points. Since the average incarceration spell in my sample is 11.662 months, one expects a future 0.365 months of incarceration for each minority defendant represented by a PD. I obtain monthly incarceration cost data from the North Carolina Department of Public Safety: https://www.ncdps.gov/adult-corrections/cost-of-corrections. That information suggests one month of incarceration costs the state $37,712.87 / 12 = $3,142.74 per month. Thus, the expected (net) cost savings of re-allocating public defenders are $12,394.96 - ($3,142.74*0.365) = $11,247.86.

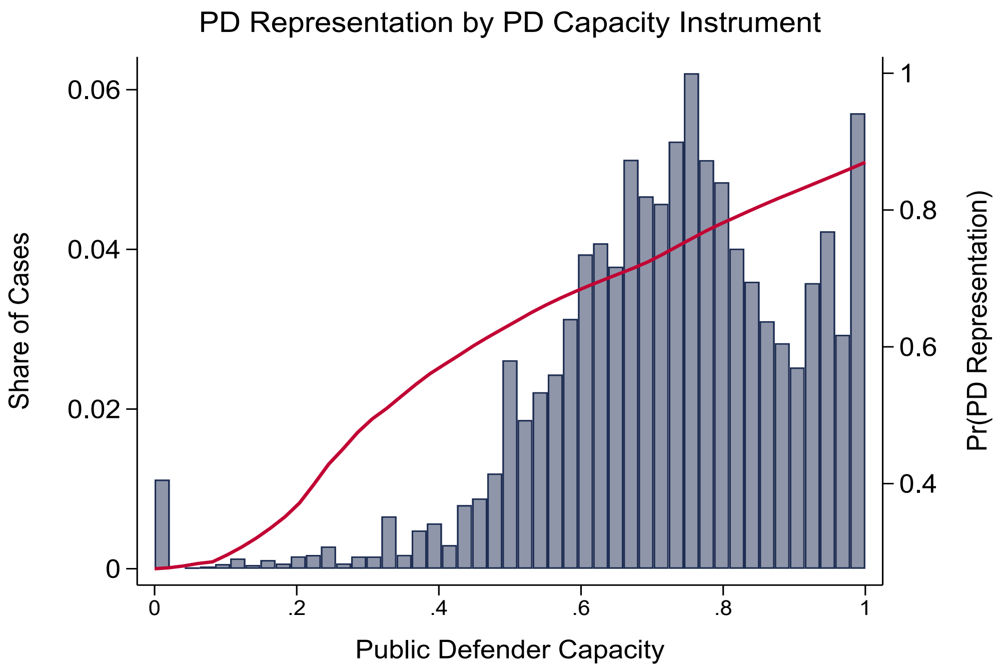

Figure 1: This figure displays the distribution of the leave-out public defender capacity measure in my research sample (N=59,860). I superimpose the fitted share of defendants assigned a PD by the capacity measure.

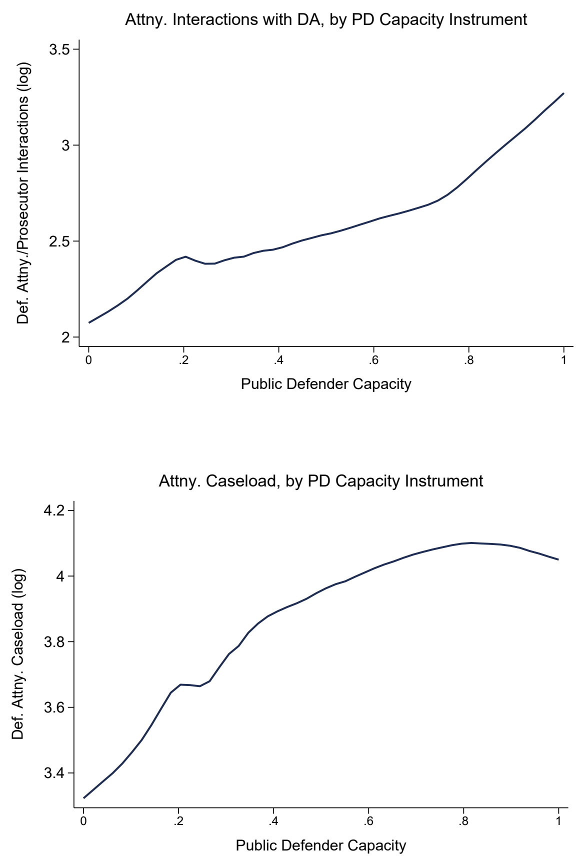

Figure 2: Each figure plots the relationship between the outcome variable—defense attorney interactions with the district attorney (DA) and caseload at the time of case entry—by the PD capacity instrument. See the text for a description of how I define these variables. The sample size is N=59,860 for both panels.

Table 1: Summary of Defendant and Case Characteristics, by Attorney Type

		Type of Defense Counsel	
	All Cases	Public Defender	Private Assigned
	(1)	(2)	(3)
I. Defendant Demographics			
White	0.47	0.49	0.41
	(0.50)	(0.50)	(0.49)
Black	0.47	0.45	0.51
	(0.50)	(0.50)	(0.50)
Hispanic	0.03	0.03	0.03
	(0.17)	(0.17)	(0.16)
Female	0.25	0.24	0.26
	(0.43)	(0.43)	(0.44)
Age	34.12	34.62	32.89
	(11.38)	(11.43)	(11.16)
N:	*59,860*	*44,072*	*15,619*
II. Defendant Prior Record			
Num. Prior Appearances	2.42	2.34	2.67
	(4.03)	(4.06)	(3.92)
Num. Prior Felony Convictions	0.64	0.61	0.75
	(1.00)	(0.96)	(1.01)
N:	*59,860*	*44,072*	*15,619*
III. Case Characteristics			
Any Felony Charge	0.45	0.43	0.49
	(0.50)	(0.49)	(0.50)
Any Drug Charge	0.24	0.23	0.25
	(0.43)	(0.42)	(0.43)
Any Violent Charge	0.49	0.49	0.49
	(0.50)	(0.50)	(0.50)
Any Property Charge	0.44	0.44	0.43
	(0.50)	(0.50)	(0.50)
Any Traffic Charge	0.11	0.11	0.10
	(0.30)	(0.31)	(0.30)
Num. Charges	2.25	2.20	2.37
	(1.73)	(1.67)	(1.88)
Expected Sent. (log)	0.43	0.41	0.47
	(0.89)	(0.86)	(0.95)
N:	*59,860*	*44,072*	*15,619*

Each cell presents the mean of the variable in the left-hand column. Standard deviations appear in parentheses. Columns 2 and 3 break down cases by the type of indigent defense counsel at case disposition. All data come from the North Carolina Administrative Office of the Courts. Sample sizes do not add up within row because 50 indigent defendants initially given court-appointed counsel go on to waive the right to counsel by the time their cases are disposed, while 119 retain private counsel. "Expected Sent." refers to the log of the leave-out mean sentence among all other offenders incarcerated on the most severe charge a defendant faces.

Table 2: Summary of Plea Bargaining Outcomes, by Defendant Race/Ethnicity

		Defendant Race/Ethnicity		
	All Cases (1)	White (2)	Black (3)	Hispanic (4)
Bail Amount (log)	8.40	8.28	8.51	8.46
	(2.51)	(2.30)	(2.71)	(2.65)
N:	*51,269*	*24,500*	*23,594*	*1,673*
Plead Guilty?	0.35	0.37	0.33	0.33
	(0.48)	(0.48)	(0.47)	(0.47)
Case Dropped?	0.43	0.43	0.43	0.50
	(0.50)	(0.50)	(0.50)	(0.50)
Any Added Charge?	0.16	0.13	0.18	0.11
	(0.36)	(0.34)	(0.39)	(0.31)
# Added Charges	0.67	0.58	0.75	0.55
	(1.62)	(1.51)	(1.70)	(1.67)
N:	*59,860*	*28,522*	*27,645*	*1,945*
Exp. Sent. of Guilty Plea (log)	0.73	0.66	0.80	0.81
	(1.03)	(0.95)	(1.09)	(1.09)
N:	*20,909*	*10,661*	*9,037*	*638*
Sent. Length (log)	1.46	1.34	1.56	1.41
	(1.17)	(1.09)	(1.22)	(1.22)
N:	*14,172*	*6,726*	*6,638*	*439*

Each cell presents the mean of the variable in the left-hand column. Standard deviations appear in parentheses. Columns 2 and 3 break down cases by defendant race. "Exp. Sent. of Guilty Plea" refers to the (log) expected sentence a defendant will be given for the most serious charge to which he pleads guilty. See the text or Table 1 for more detail on how I define expected sentences. Sample sizes vary within columns because not all cases result in a guilty plea or incarceration.

Table 3: Validity of PD Capacity Instrument

	Outcome Variable	
	Represented by PD (1)	PD Capacity (2)
Black/Hispanic	-0.02950***	0.00041
	(0.00394)	(0.00103)
Female	-0.04451***	0.00044
	(0.00454)	(0.00110)
Age	0.00195***	0.00002
	(0.00016)	(0.00004)
Num. Charges	-0.01026***	-0.00017
	(0.00137)	(0.00037)
Any Felony Charge	-0.01182**	-0.00056
	(0.00511)	(0.00122)
Any Traffic Charge	0.03732***	0.00051
	(0.00626)	(0.00171)
Any Drug Charge	0.01497***	-0.00197
	(0.00550)	(0.00147)
Any Property Charge	0.03474***	0.00127
	(0.00478)	(0.00116)
Any Violent Charge	0.01243***	0.00125
	(0.00470)	(0.00112)
Exp. Sent. (log)	0.00046	-0.00010
	(0.00229)	(0.00063)
Num. Prior Appearances	-0.00346***	-0.00006
	(0.00052)	(0.00012)
Num. Prior Felony Convictions	-0.01242***	0.00018
	(0.00244)	(0.00055)
Joint F-stat	40.50	0.99
(p-value)	(0.000)	(0.458)
N:	*59,860*	*59,860*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Each column presents OLS estimates from a single regression. Column 1 reports the correlations between the listed case and defendant characteristics and an indicator for whether the defendant is assigned a public defender. Column 2 uses a similar specification, but uses my leave-out PD capacity measure as the outcome variable. Both regressions include court-year-day and court-month day fixed effects, as discussed in the text. Robust standard errors clustered at the court-week level appear in parentheses. The F-statistic and associated p-value at the bottom of each column comes from a test of the joint significance of the coefficients listed in the rows above, again using standard errors clustered at the court-week level.

Table 4: Relevance of PD Capacity Instrument

	Sample Mean	PD Capacity Effect	
	(1)	(2)	(3)
Represented by PD?	0.736	0.210***	0.207***
	(0.441)	(0.024)	(0.024)
N:	*59,860*	*59,860*	*59,860*
F-statistic		80.78	41.38
Case, Demographic Controls		N	Y
County-time FEs		Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Column 1 of reports the share of defendants in my research sample receiving a public defender (PD), with its standard deviation in parentheses. Column 2 reports the effect of my leave-out PD capacity measure on the probability a defendant receives a PD, estimated using Equation 1 with court-time fixed effects and no additional controls. Column 3 reports estimates using a similar model, but with additional case and defendant demographic controls included. Robust standard errors clustered at the court-week level appear in parentheses.

Table 5: PD Representation Effect on Plea Bargaining and Sentencing Outcomes, All Defendants

	PAC Mean	Reduced-Form		First Stage	2SLS
	(1)	(2)	(3)	(4)	(5)
I. Bail and Charging Outcomes					
Bail Amount (log)	9.094	-0.171**	-0.110*	0.209***	-0.557*
	(2.135)	(0.081)	(0.059)	(0.024)	(0.299)
N:	*12,940*	*48,879*	*48,879*	*48,879*	*48,879*
# Added Charges	0.843	-0.201***	-0.163***	0.207***	-0.787**
	(1.825)	(0.064)	(0.052)	(0.024)	(0.262)
Any Added Charge?	0.266	-0.053***	-0.045***	0.207***	-0.216***
	(0.442)	(0.016)	(0.014)	(0.024)	(0.069)
N:	*15,619*	*59,860*	*59,860*	*59,860*	*59,860*
II. Plea and Sentencing Outcomes					
Any Guilty Plea?	0.338	0.017	0.021	0.207***	0.102
	(0.473)	(0.017)	(0.016)	(0.024)	(0.077)
Incarcerated?	0.243	0.012	0.013	0.207***	0.064
	(0.429)	(0.016)	(0.015)	(0.024)	(0.072)
N:	*15,619*	*59,860*	*59,860*	*59,860*	*59,860*
Exp. Sent. of Guilty Plea (log)	0.814	-0.054	-0.066	0.207***	-0.274
	(1.104)	(0.068)	(0.064)	(0.027)	(0.266)
N:	*5,271*	*20,909*	*20,909*	*20,909*	*20,909*
Sentence Length (log)	1.633	-0.137	-0.164**	0.355***	-0.593**
	(1.213)	(0.099)	(0.082)	(0.035)	(0.297)
N:	*3,789*	*14,172*	*14,172*	*14,172*	*14,172*
Case, Demographic Controls		N	Y	Y	Y
County-time FEs		Y	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Column 1 reports the mean of the outcome variable given in the left-hand column among cases assigned a private attorney (PAC). Standard deviations appear in parentheses. Columns 2 and 3 present reduced-form estimates of the effect on case outcomes, following Equation 2. Column 4 notes the first stage relationship between early docket filing and PD representation. Column 5 reports the 2SLS effect of PD representation, following Equation 3. Robust standard errors clustered at the court-week level appear in parentheses. Case and demographic controls include indicators for whether the defendant is Black or Hispanic, and whether the defendant is female; linear controls for defendant age and the number of charges originally filed in the case; indicators for whether the case involves traffic, drug, property, and violent charges; and linear controls for the number of prior appearances in court and felony convictions I observe for the defendant.

Table 6: Are There Racial Disparities in Plea Bargaining? Isolating PD Effect by Race/Ethnicity

			Pooled Reduced-form Using Race/Ethnicity Interaction				
			Standard Covariates		Interacted Covariates		Interacted 2SLS
	PAC Mean	PD Capacity	PD Capacity	Capacity×Black/Hisp	PD Capacity	Capacity×Black/Hisp	PD × Black/Hisp
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I. Bail and Charging Outcomes							
Bail Amount (log)	9.094	-0.111*	-0.095	-0.032	-0.100	-0.025	-0.146
	(2.135)	(0.059)	(0.068)	(0.082)	(0.068)	(0.080)	(0.116)
N:	*12,831*	*48,838*	*48,838*		*48,838*		*48,838*
# Charges Added	0.843	-0.163***	-0.172***	0.021	-0.178***	-0.015	-0.132
	(1.825)	(0.052)	(0.058)	(0.067)	(0.058)	(0.010)	(0.099)
Any Charge Added?	0.266	-0.045***	-0.045***	-0.000	-0.046***	0.003	-0.043*
	(0.442)	(0.014)	(0.016)	(0.019)	(0.016)	(0.018)	(0.026)
N:	*15,619*	*59,860*	*59,860*		*59,860*		*59,860*
II. Plea and Sentencing Outcomes							
Any Guilty Plea?	0.338	0.021	0.005	0.036	0.008	0.027	0.053*
	(0.473)	(0.016)	(0.018)	(0.024)	(0.019)	(0.021)	(0.030)
Incarcerated?	0.243	0.013	0.014	-0.002	0.013	0.001	0.014
	(0.429)	(0.015)	(0.017)	(0.019)	(0.017)	(0.020)	(0.028)
N:	*15,619*	*59,860*	*59,860*		*59,860*		*59,860*
Exp. Sent. of Guilty Plea (log)	0.814	-0.066	0.008	-0.168*	0.049	-0.253***	-0.384***
	(1.104)	(0.072)	(0.071)	(0.094)	(0.092)	(0.065)	(0.140)
N:	*5,271*	*20,909*	*20,909*		*20,909*		*20,909*
Sentence Length (log)	1.633	-0.164**	-0.036	-0.253**	-0.001	-0.308***	-0.522***
	(1.212)	(0.082)	(0.096)	(0.110)	(0.095)	(0.107)	(0.156)
N:	*3,789*	*14,172*	*14,172*		*14,172*		*14,172*
Case, Demographic Controls		Y	Y	Y	Y	Y	Y
Controls Interacted w/ Race		N	N	N	Y	Y	Y
County-Time FEs		Y	Y	Y	Y	Y	Y
First-stage F-statistic		–	–	–	–	–	3,392
First-stage Coefficient		–	–	–	–	–	0.600***
							(0.018)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Column 1 reports the mean of the outcome variable given in the left-hand column among defendants represented by private counsel. Standard deviations appear in parentheses. Column 2 reports the reduced-form effect of the PD capacity measure among all defendants, estimated using Equation 2. For each row, coefficients in columns 3 and 4 comes from a single regression model, as do the coefficients in columns 5 and 6. Columns 3 and 4 present reduced-form estimates of the early docket effect by defendant race, using the pooled specification given by Equation 4. Columns 5 and 6 also come from Equation 4, but include covariates interacted with an indicator for whether the defendant is Black or Hispanic. Column 7 reports 2SLS coefficients, following the interacted 2SLS specification given in Equation 5. The interacted reduced form estimates and other robustness tests corresponding to this model appear in Appendix Table A5. Robust standard errors clustered at the court-week level appear in parentheses. Case and demographic controls are described below Table 5. Sample sizes vary within column because not all defendants receive bail, plead guilty, or are sentenced to incarceration.

Table 7: Ruling Out Potential Explanations for the PD Effect on Minority Defendants

	Preferred Model (1)	Exp. Sent. Control (2)	Prior Points Control (3)	Prior Points FE (4)	Prosecutor FE (5)	Def. Attny FE (6)
Exp. Sent. of Guilty Plea (log)						
PD Capacity	0.049	-0.021	0.084	0.078	0.053	0.081
	(0.072)	(0.040)	(0.081)	(0.079)	(0.073)	(0.076)
Capacity × Black/Hisp	-0.253***	-0.064	-0.349***	-0.335***	-0.205**	-0.284***
	(0.092)	(0.051)	(0.099)	(0.098)	(0.093)	(0.097)
N:	*20,909*	*20,909*	*17,422*	*17,422*	*20,888*	*20,868*
Sentence Length (log)						
PD Capacity	-0.001	-0.022	0.039	0.042	0.028	0.015
	(0.095)	(0.082)	(0.086)	(0.081)	(0.085)	(0.097)
Capacity × Black/Hisp	-0.308***	-0.178*	-0.353***	-0.335***	-0.208**	-0.232**
	(0.107)	(0.093)	(0.095)	(0.092)	(0.098)	(0.110)
N:	*14,172*	*14,172*	*13,840*	*13,840*	*14,162*	*14,144*
Case, Demographic Controls	Y	Y	Y	Y	Y	Y
Controls Interacted w/ Race	Y	Y	Y	Y	Y	Y
County-time FEs	Y	Y	Y	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

For each outcome variable, estimates in each column come from a single regression, following Equation 4. Column 1 reports identical estimates to those that appear in columns 5 and 6 of Table 6. The remaining columns augment the model used in column 1. Column 2 adds in a linear control for the (log) expected sentence; column 3 adds in a linear control for a defendant's prior record points; column 3 adds a fixed effect for a defendant's prior record points; column 5 adds in prosecutor (assistant DA) fixed effects; and column 6 adds in defense attorney fixed effects. I interact standard covariates (i.e., not those listed in column headers) with an indicator for whether a defendant is Black or Hispanic. Those case and demographic controls are described below Table 5. Robust standard errors clustered at the court-week level appear in parentheses. Sample sizes vary across columns due to missing defendant prior record data (which is only available for those found guilty) and missing prosecutor and defense attorney names. Sample sizes vary within column because not all defendants who plead guilty are incarcerated.

Table 8: Mechanism Behind PD Effect: The Roles of DA Interactions and Caseloads

		By DA Interactions		By Caseload	
	Full Sample	Above-Median	Below-Median	Above-Median	Below-Median
	(1)	(2)	(3)	(4)	(5)
Exp. Sent. of Guilty Plea (log)					
PD Capacity	0.078	0.192*	-0.010	0.060	0.244*
	(0.077)	(0.109)	(0.098)	(0.086)	(0.135)
PD Capacity × Black/Hisp	-0.285***	-0.431***	-0.207	-0.328***	-0.377**
	(0.100)	(0.134)	(0.128)	(0.110)	(0.164)
N	*20,667*	*12,393*	*13,362*	*16,258*	*9,497*
Sentence Length (log)					
PD Capacity	0.008	0.140	-0.074	-0.089	0.213
	(0.099)	(0.133)	(0.123)	(0.114)	(0.161)
PD Capacity × Black/Hisp	-0.210*	-0.313**	-0.042	-0.137	-0.209
	(0.112)	(0.150)	(0.142)	(0.126)	(0.177)
N	*13,998*	*8,651*	*9,038*	*10,938*	*6,751*
Case, Demographic Controls	Y	Y	Y	Y	Y
County-time FEs	Y	Y	Y	Y	Y
Def. Attny. FE	Y	Y	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

For each outcome variable, estimates in each column come from a single regression, following Equation 4. Each specification includes controls for defendant and case characteristics interacted with an indicator for whether the defendant is Black or Hispanic; all specifications also include a linear control in the log of total cases the given defense attorney disposes over my sample period (interacted with defendant race/ethnicity again) and defense attorney fixed effects. Column 1 uses data from my full sample. Column 2 restricts the sample of public defenders to those with above-median levels of interactions with the prosecutor assigned to the case. Column 3 focuses on PDs with below-median interactions with the DA. Likewise, columns 4 and 5 break down the sample by whether a PD has an above- or below-median caseload at the time of case entry. See the text for descriptions of how I define these variables. Robust standard errors clustered at the court-week level appear in parentheses.

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9 Appendix

MISDEMEANOR PUNISHMENT CHART

<table>
<tr><th rowspan="3">CLASS</th><th colspan="4">PRIOR CONVICTION LEVEL</th></tr>
<tr><th>I</th><th colspan="2">II</th><th>III</th></tr>
<tr><th>No Prior Convictions</th><th colspan="2">One to Four Prior Convictions</th><th>Five or More Prior Convictions</th></tr>
<tr><td>A1</td><td>C/I/A
1 - 60 days</td><td colspan="2">C/I/A
1 - 75 days</td><td>C/I/A
1 - 150 days</td></tr>
<tr><td>1</td><td>C
1 - 45 days</td><td colspan="2">C/I/A
1 - 45 days</td><td>C/I/A
1 - 120 days</td></tr>
<tr><td>2</td><td>C
1 - 30 days</td><td colspan="2">C/I
1 - 45 days</td><td>C/I/A
1 - 60 days</td></tr>
<tr><td rowspan="2">3</td><td rowspan="2">C
Fine Only*
1 - 10 days</td><td>One to Three Prior Convictions</td><td>Four Prior Convictions</td><td rowspan="2">C/I/A
1 - 20 days</td></tr>
<tr><td>C
Fine Only*
1 - 15 days</td><td>C/I
1 - 15 days</td></tr>
</table>

Figure A1: This table shows North Carolina's structured sentencing "grid" for misdemeanor offenses. Each cell identifies the minimum sentence a defendant faces if found guilty of a felony offense in the class given on the left-hand axis. The severity of the minimum sentence depends on the defendant's prior criminal record. "Active" punishment refers to incarceration; "community" punishment refers to probation; and "intermediate" punishment typically resembles parole, may may include involuntary confinement in, for example, a rehab program.

FELONY PUNISHMENT CHART

PRIOR RECORD LEVEL

OFFENSE CLASS	I 0-1 Pt	II 2-5 Pts	III 6-9 Pts	IV 10-13 Pts	V 14-17 Pts	VI 18+ Pts
A	Death or Life Without Parole Defendant Under 18 at Time of Offense: Life With or Without Parole					
B1	A *240 - 300* **192 - 240** *144 - 192*	A *276 - 345* **221 - 276** *166 - 221*	A *317 -397* **254 - 317** *190 - 254*	A *365 - 456* **292 - 365** *219 - 292*	A *Life Without Parole* **336 - 420** *252 - 336*	A *Life Without Parole* **386 - 483** *290 - 386*
B2	A *157 - 196* **125 - 157** *94 - 125*	A *180 - 225* **144 - 180** *108 - 144*	A *207 - 258* **165 - 207** *124 - 165*	A *238 - 297* **190 - 238** *143 - 190*	A *273 - 342* **219 - 273** *164 - 219*	A *314 - 393* **251 - 314** *189 - 251*
C	A *73 – 92* **58 - 73** *44 - 58*	A *83 - 104* **67 - 83** *50 - 67*	A *96 - 120* **77 - 96** *58 - 77*	A *110 - 138* **88 - 110** *66 - 88*	A *127 - 159* **101 - 127** *76 - 101*	A *146 - 182* **117 - 146** *87 - 117*
D	A *64 - 80* **51 - 64** *38 - 51*	A *73 - 92* **59 - 73** *44 - 59*	A *84 - 105* **67 - 84** *51 - 67*	A *97 - 121* **78 - 97** *58 - 78*	A *111 - 139* **89 - 111** *67 - 89*	A *128 - 160* **103 - 128** *77 - 103*
E	I/A *25 - 31* **20 - 25** *15 - 20*	I/A *29 - 36* **23 - 29** *17 - 23*	A *33 - 41* **26 - 33** *20 - 26*	A *38 - 48* **30 - 38** *23 - 30*	A *44 - 55* **35 - 44** *26 - 35*	A *50 - 63* **40 - 50** *30 - 40*
F	I/A *16 - 20* **13 - 16** *10 - 13*	I/A *19 - 23* **15 - 19** *11 - 15*	I/A *21 - 27* **17 - 21** *13 - 17*	A *25 - 31* **20 - 25** *15 - 20*	A *28 - 36* **23 - 28** *17 - 23*	A *33 - 41* **26 - 33** *20 - 26*
G	I/A *13 - 16* **10 - 13** *8 - 10*	I/A *14 - 18* **12 - 14** *9 - 12*	I/A *17 - 21* **13 - 17** *10 - 13*	I/A *19 - 24* **15 - 19** *11 - 15*	A *22 - 27* **17 - 22** *13 - 17*	A *25 - 31* **20 - 25** *15 - 20*
H	C/I/A *6 - 8* **5 - 6** *4 - 5*	I/A *8 - 10* **6 - 8** *4 - 6*	I/A *10 - 12* **8 - 10** *6 - 8*	I/A *11 - 14* **9 - 11** *7 - 9*	I/A *15 - 19* **12 - 15** *9 - 12*	A *20 - 25* **16 - 20** *12 - 16*
I	C *6 - 8* **4 - 6** *3 - 4*	C/I *6 - 8* **4 - 6** *3 - 4*	I *6 - 8* **5 - 6** *4 - 5*	I/A *8 - 10* **6 - 8** *4 - 6*	I/A *9 - 11* **7 - 9** *5 - 7*	I/A *10 - 12* **8 - 10** *6 - 8*

DISPOSITION
Aggravated Range
PRESUMPTIVE RANGE
Mitigated Range

A – Active Punishment I – Intermediate Punishment C – Community Punishment
Numbers shown are in months and represent the range of minimum sentences

Figure A2: This table shows North Carolina's structured sentencing "grid" for felony offenses. Each cell identifies the minimum sentence a defendant faces if found guilty of a felony offense in the class given on the left-hand axis. The severity of the minimum sentence depends on whether the judge finds that the offender deserves an aggravated or mitigated sentence, as well as the defendant's prior criminal record. "Active" punishment refers to incarceration; "community" punishment refers to probation; and "intermediate" punishment typically resembles parole, may may include involuntary confinement in, for example, a rehab program.

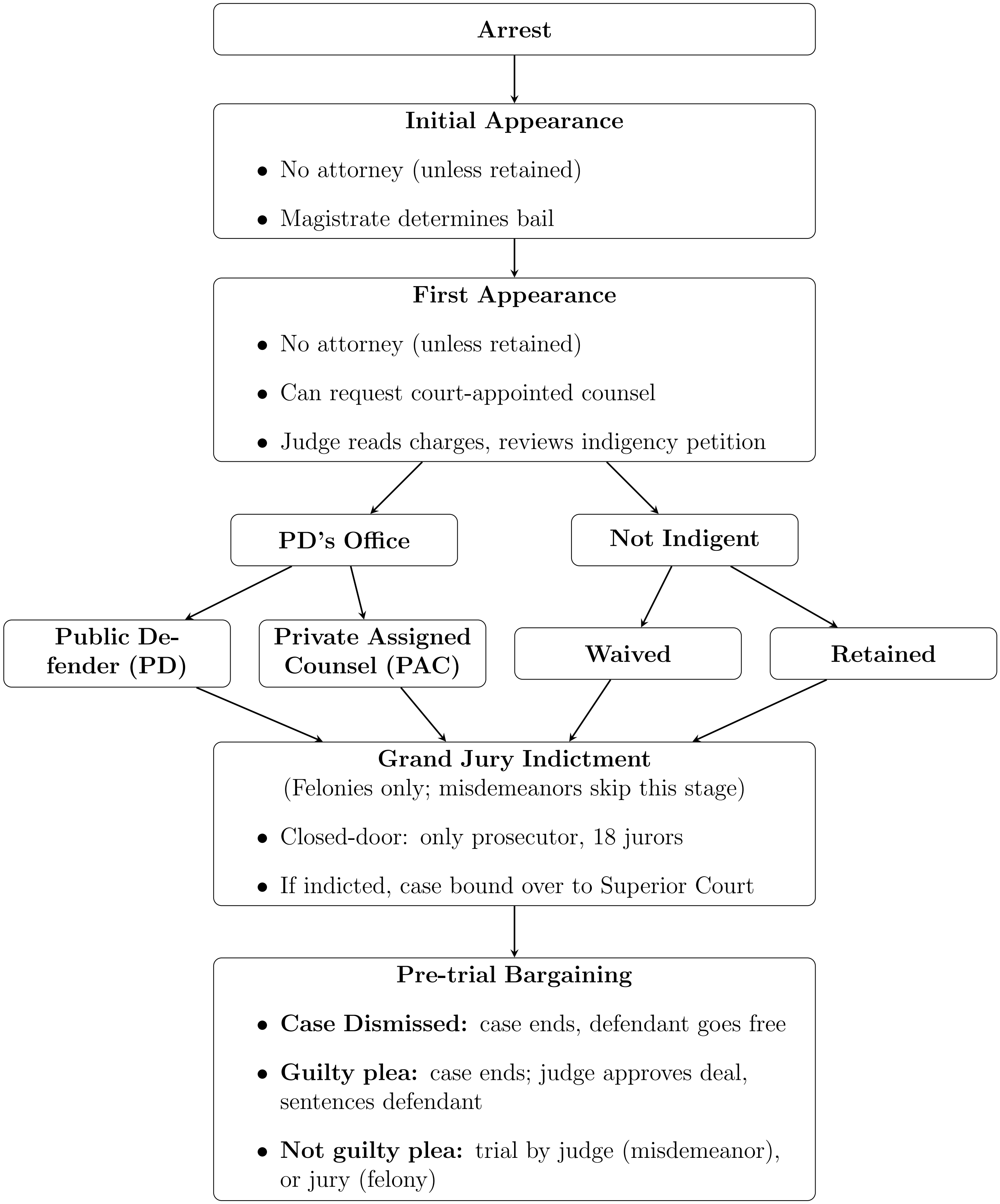

Figure A3: The flow chart depicts the criminal process for North Carolina defendants.

Table A1: First Stage Estimates Across Subsamples

	I. Case Characteristics					
	Felony	Misd.	Drug	Traffic	Property	Violent
PD Capacity	0.163***	0.221***	0.244***	0.149**	0.215***	0.248***
	(0.031)	(0.028)	(0.043)	(0.069)	(0.032)	(0.032)
N	26,880	43,046	14,189	6,488	26,290	29,241

	II. Defendant Characteristics							
					Prior App.?		Prior Conv.?	
	Black	White	Female	Male	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PD Capacity	0.297***	0.162***	0.340***	0.222***	0.194***	0.288***	0.176***	0.256***
	(0.032)	(0.032)	(0.047)	(0.025)	(0.029)	(0.032)	(0.032)	(0.029)
N	27,645	28,522	14,122	45,728	36,928	22,932	24,979	34,881

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Each cell comes from a separate first-stage regression, following Equation 1. The specification includes only the court-time fixed effects discussed in Section 4, and no additional covariates. Each column header describes the subsample on which I estimate the first stage coefficient. Note that the case characteristic categories are not mutually exclusive. "Prior App." indicates whether a defendant has had a prior appearance in court within my sample period; "Prior Conv." indicates whether a defendant has had a prior felony conviction within my sample period. Robust standard errors clustered at the court-week level appear in parentheses.

Table A2: Describing Defendants on the Margin of PD Representation (Compliers)

	Likelihood Ratio (1)	Full Sample Mean (2)	Complier Mean (3)
White	1.023	0.476	0.488
Black	0.996	0.462	0.460
Hispanic	1.117	0.032	0.036
Female	1.020	0.236	0.241
Male	1.013	0.764	0.774
Felony	0.951	0.449	0.427
Drug Charge	0.990	0.237	0.235
Property Charge	1.007	0.439	0.442
Violent Charge	1.015	0.488	0.496
Traffic Charge	1.008	0.108	0.109
Any prior Appearance	0.975	0.617	0.602
Any Prior Conviction	0.956	0.417	0.399

Column 1 shows the likelihood ratio for the given outcome variable, which is equal to the ratio of the fitted first stage estimates for that subsample (e.g., the subsample of white defendants for the first row) to the ratio of the first stage in the whole sample. First stage coefficients by subsample appear in Appendix Table A1. Column 2 reports the full-sample mean of the given outcome. Column 3 reports the complier mean, equal to the full sample mean times scaled by the likelihood ratio.

Table A3: Robustness of Reduced-form Interaction Estimates to Alternative Specifications

	No FEs (1)	County FEs (2)	County-Year FEs (3)	County-year, County-month FEs (4)	County-year-day, County-month FEs (5)	Preferred Model (6)	No Addtl. Controls (7)
Bail Amount (log)							
PD Capacity	0.106*	-0.156**	-0.119*	-0.119*	-0.114*	-0.100	-0.070
	(0.060)	(0.064)	(0.071)	(0.071)	(0.067)	(0.068)	(0.091)
Capacity × Black/Hisp	-0.322***	-0.055	0.007	0.007	-0.019	-0.025	-0.217**
	(0.085)	(0.079)	(0.081)	(0.081)	(0.080)	(0.080)	(0.110)
N	48,838	48,838	48,838	48,838	48,838	48,838	48,838
# Charges Added							
PD Capacity	0.193***	-0.161***	-0.176***	-0.176***	-0.190***	-0.178***	-0.167**
	(0.050)	(0.058)	(0.062)	(0.062)	(0.058)	(0.058)	(0.070)
Capacity × Black/Hisp	-0.152**	0.052	0.091	0.091	0.061	0.030	-0.073
	(0.069)	(0.067)	(0.066)	(0.066)	(0.066)	(0.066)	(0.083)
Any Charge Added?							
PD Capacity	0.069***	-0.039**	-0.044***	-0.044***	-0.051***	-0.046***	-0.045**
	(0.015)	(0.016)	(0.017)	(0.017)	(0.016)	(0.016)	(0.018)
Capacity × Black/Hisp	-0.059***	0.008	0.015	0.015	0.010	0.003	-0.016
	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.021)
Any Guilty Plea?							
PD Capacity	-0.009	0.008	0.031	0.031	0.007	0.008	0.003
	(0.015)	(0.017)	(0.020)	(0.020)	(0.018)	(0.019)	(0.020)
Capacity × Black/Hisp	0.074***	0.016	0.026	0.026	0.024	0.027	0.031
	(0.020)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.023)
Incarcerated?							
PD Capacity	-0.075***	0.013	0.002	0.002	0.008	0.013	0.012
	(0.014)	(0.016)	(0.018)	(0.018)	(0.017)	(0.017)	(0.018)
Capacity × Black/Hisp	0.001	-0.014	0.000	0.000	-0.002	0.001	0.002
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)
N	59,860	59,860	59,860	59,860	59,860	59,860	59,860
Exp. Sent of Guilty Plea (log)							
PD Capacity	-0.064	0.031	0.055	0.055	0.057	0.049	0.044
	(0.054)	(0.065)	(0.075)	(0.075)	(0.068)	(0.072)	(0.076)
Capacity × Black/Hisp	-0.263***	-0.230***	-0.219**	-0.219**	-0.231***	-0.253***	-0.213**
	(0.081)	(0.083)	(0.089)	(0.089)	(0.087)	(0.092)	(0.097)
N	20,909	20,909	20,909	20,909	20,909	20,909	20,909
Sent. Length (log)							
PD Capacity	0.168**	-0.134*	-0.027	-0.027	-0.117	-0.001	0.056
	(0.074)	(0.080)	(0.097)	(0.097)	(0.087)	(0.095)	(0.112)
Capacity × Black/Hisp	-0.292***	-0.133	-0.125	-0.125	-0.178*	-0.308***	-0.361***
	(0.100)	(0.097)	(0.104)	(0.104)	(0.102)	(0.107)	(0.125)
N	14,172	14,172	14,172	14,172	14,172	14,172	14,172
Case, Demographic Controls	Y	Y	Y	Y	Y	Y	N
Controls Interacted w/ Race	Y	Y	Y	Y	Y	Y	–

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Cells within a single column for a given outcome come from a single regression. The specification is Equation 4, with fixed effects added or omitted as indicated. All specifications in columns 1-6 include case and demographic controls, interacted with an indicator for Black and Hispanic defendants. Those controls appear below Table 5. Column 1 includes no additional fixed effects. Column 2 adds court-level fixed effects to this model. Column 3 includes court-year fixed effects. Column 4 adds court-month fixed effects. Column 5 includes court-year-day of week and court-month fixed effects. The preferred model in column 6 includes court-year-day of week and court-month-day of week fixed effects. Column 7 reports estimates using the preferred set of court-year-day and court-month-day fixed effects, but omits any additional case and demographic controls besides an indicator for whether a defendant is Black or Hispanic. Results in column 6 appear in columns 5 and 6 of Table 6. Robust standard errors clustered at the court-week level appear in parentheses. Sample sizes vary within column because not all defendants receive bail, plead guilty, or are sentenced to incarceration.

Table A4: Exploring Further Outcomes, by Defendant Race/Ethnicity

	Sample Mean	PD Capacity	Capacity×Black/Hisp
	(1)	(2)	(3)
Sentence Length (months)	11.662	0.765	-4.709**
	(26.420)	(2.303)	(2.384)
N:	*3,789*		*13,659*
Exp. Sent. of Guilty Plea (months)	4.330	-0.159	-1.135
	(11.121)	(0.765)	(0.980)
N:	*5,271*		*20,524*
Probation?	0.160	-0.030*	0.049***
	(0.367)	(0.016)	(0.018)
N:	*15,619*		*59,625*
Probation Length (log)	1.671	-0.132	0.119
	(0.735)	(0.081)	(0.098)
N:	*2,503*		*9,755*
Fines and Fees ($)	96.653	72.712	-126.101
	(1,568.487)	(74.280)	(86.850)
N:	*15,619*		*59,625*
Time to Disposition (days)	194.247	-32.331***	-2.402
	(217.108)	(8.720)	(9.300)
N:	*15,609*		*59,604*
Case, Demographic Controls		N	Y
County-time FEs		Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Column 1 reports the mean of the outcome variable given in the left-hand column among defendants represented by private counsel. Standard deviations appear in parentheses. For each row, coefficients in columns 2 and 3 come from a single regression model following the pooled specification given by Equation 4. Robust standard errors clustered at the court-week level appear in parentheses. Case and demographic controls are described below Table 5. Sample sizes vary within column because not all defendants receive probation, are found guilty, or plead guilty. Sixteen observations have erroneously-coded disposition dates that occur prior to the case filing date and are omitted from the "time to disposition" analysis.

Table A5: Reduced-form, 2SLS Estimates Using Interacted Treatment (Based on Equation 5)

	Reduced Form			First Stage	2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bail Amount (log)	-0.261***	-0.148**	-0.088	0.600***	-0.425***	-0.242**	-0.146
	(0.097)	(0.070)	(0.070)	(0.020)	(0.158)	(0.114)	(0.116)
N	48,838	48,838	48,838	48,838	48,838	48,838	48,838
# Charges Added	-0.175**	-0.103*	-0.079	0.600***	-0.286**	-0.169*	-0.132
	(0.075)	(0.059)	(0.059)	(0.018)	(0.123)	(0.098)	(0.099)
Any Charge Added?	-0.043**	-0.031**	-0.026*	0.600***	-0.071**	-0.051**	-0.043*
	(0.018)	(0.015)	(0.015)	(0.018)	(0.030)	(0.025)	(0.026)
Any Guilty Plea?	0.033*	0.035**	0.032*	0.600***	0.054*	0.057**	0.053*
	(0.019)	(0.018)	(0.018)	(0.018)	(0.031)	(0.029)	(0.030)
Incarcerated?	0.009	0.007	0.008	0.600***	0.014	0.011	0.014
	(0.018)	(0.017)	(0.017)	(0.018)	(0.029)	(0.027)	(0.028)
N	59,860	59,860	59,860	59,860	59,860	59,860	59,860
Exp. Sent. of Guilty Plea (log)	-0.185**	-0.216***	-0.222***	0.578***	-0.314**	-0.369***	-0.384***
	(0.086)	(0.081)	(0.081)	(0.031)	(0.145)	(0.138)	(0.140)
N	20,909	20,909	20,909	20,909	20,909	20,909	20,909
Sent. Length (log)	-0.325***	-0.292***	-0.309***	0.592***	-0.545***	-0.492***	-0.522***
	(0.110)	(0.092)	(0.092)	(0.037)	(0.185)	(0.156)	(0.156)
N	14,172	14,172	14,172	14,172	14,172	14,172	14,172
Case, Demographic Controls	N	Y	Y	Y	N	Y	Y
Controls Interacted w/ Race	–	N	Y	Y	–	N	Y
County-time FEs	Y	Y	Y	Y	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Each cell comes from a separate regression. The 2SLS specification is given by Equation 5. The reduced form specification is not listed, but is the reduced form analogue of Equation 5: the model regresses the outcome variable on PD capacity, interacted with whether a defendant is Black or Hispanic, with a separate control for whether a defendant is Black or Hispanic. Columns 1 and 5 report estimates from regressions without any controls besides court-time fixed effects and an indicator for whether a defendant is Black or Hispanic. Columns 2 and 4 include additional covariates—described below Table 5—and columns 3 and 6 include those same covariates interacted with the Black/Hispanic defendant indicator. First stage F-statistics are all in excess of 1,000. Robust standard errors clustered at the court-week level appear in parentheses. Sample sizes vary within column because not all defendants receive bail, plead guilty, or are sentenced to incarceration.

Table A6: Reduced-form and 2SLS Estimates, Including Mecklenburg, Wake Counties

	Reduced Form Estimates				2SLS Estimates	
	Preferred Sample		+ Wake, Mecklenburg		Pref. Sample	+ Wake, Meck.
	PD Capacity (1)	Capacity × Black/Hisp (2)	PD Capacity (3)	Capacity × Black/Hisp (4)	PD × Black/Hisp (5)	PD × Black/Hisp (6)
Bail Amount (log)	-0.100	-0.025	-0.097	-0.093	-0.146	-0.223***
	(0.068)	(0.080)	(0.059)	(0.059)	(0.116)	(0.073)
N	48,838	48,838	89,196	89,196	48,838	89,196
# Charges Added	-0.178***	0.030	-0.213***	0.041	-0.132	-0.133*
	(0.058)	(0.066)	(0.051)	(0.054)	(0.099)	(0.069)
Any Charge Added?	-0.046***	0.003	-0.052***	-0.007	-0.043*	-0.057***
	(0.016)	(0.018)	(0.014)	(0.015)	(0.026)	(0.019)
Any Guilty Plea?	0.008	0.027	0.029*	-0.017	0.053*	0.002
	(0.019)	(0.021)	(0.017)	(0.018)	(0.030)	(0.022)
Incarcerated?	0.013	0.001	0.017	-0.020	0.014	-0.014
	(0.017)	(0.020)	(0.016)	(0.016)	(0.028)	(0.020)
N	59,860	59,860	101,564	101,564	59,860	101,564
Exp. Sent of Guilty Plea (log)	0.049	-0.253***	-0.032	-0.123*	-0.384***	-0.208**
	(0.072)	(0.092)	(0.062)	(0.069)	(0.140)	(0.085)
N	20,909	20,909	34,808	34,808	20,909	34,808
Sent. Length (log)	-0.001	-0.308***	-0.024	-0.178**	-0.522***	-0.282**
	(0.095)	(0.107)	(0.083)	(0.088)	(0.156)	(0.112)
N	14,172	14,172	23,391	23,391	14,172	23,391
Case, Demographic Controls	Y	Y	Y	Y	Y	Y
Controls Interacted w/ Race	Y	Y	Y	Y	Y	Y
County-time FEs	Y	Y	Y	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

For each outcome variable in the left-hand column, results in columns 1 and 2 come from a single reduced form model, following Equation 4. These estimates appear in Table 6, columns 5 and 6. Columns 3 and 4 come from a similar model that includes data from Wake and Mecklenburg counties. Column 5 reports a 2SLS estimate following Equation 5 and is identical to column 7 of Table 6. Column 8 reports results from a similar specification but includes data on Wake and Mecklenburg counties. All specifications include case and demographic controls interacted with defendant race, as well as court-time fixed effects, as discussed in the text. Robust standard errors clustered at the court-week level appear in parentheses. Sample sizes vary within column because not all defendants receive bail, plead guilty, or are sentenced to incarceration.

Table A7: Robustness of Reduced-form Estimates to Alternative Attny. Assignment Cutoffs

	Time Between Filing Date to Final Attorney Assignment Date						
	≤ 3 Days	≤ 5 Days	≤ 7 Days	≤ 14 Days	≤ 21 Days	≤ 28 Days	≤ 35 Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bail Amount (log)							
PD Capacity	-0.033	-0.100	-0.113*	-0.129**	-0.126**	-0.128***	-0.132***
	(0.087)	(0.068)	(0.061)	(0.054)	(0.051)	(0.048)	(0.046)
Capacity × Black/Hisp	-0.089	-0.025	-0.056	-0.010	0.000	0.005	0.028
	(0.105)	(0.080)	(0.072)	(0.064)	(0.060)	(0.056)	(0.054)
N	30,361	48,838	59,428	71,195	78,766	86,557	90,690
# Charges Added							
PD Capacity	-0.198***	-0.178***	-0.165***	-0.144***	-0.144***	-0.133***	-0.135***
	(0.071)	(0.058)	(0.054)	(0.048)	(0.045)	(0.041)	(0.039)
Capacity × Black/Hisp	0.004	0.030	0.049	0.069	0.041	0.048	0.046
	(0.083)	(0.066)	(0.060)	(0.054)	(0.050)	(0.046)	(0.044)
Any Charge Added?							
PD Capacity	-0.037*	-0.046***	-0.046***	-0.044***	-0.045***	-0.044***	-0.042***
	(0.020)	(0.016)	(0.015)	(0.013)	(0.012)	(0.011)	(0.011)
Capacity × Black/Hisp	0.010	0.003	0.013	0.022	0.021	0.022*	0.019
	(0.023)	(0.018)	(0.016)	(0.015)	(0.014)	(0.013)	(0.012)
Any Guilty Plea?							
PD Capacity	0.011	0.008	0.016	0.023	0.022	0.019	0.021*
	(0.024)	(0.019)	(0.017)	(0.015)	(0.014)	(0.013)	(0.013)
Capacity × Black/Hisp	0.014	0.027	0.026	0.003	-0.001	-0.001	0.001
	(0.028)	(0.021)	(0.019)	(0.017)	(0.016)	(0.015)	(0.014)
Incarcerated?							
PD Capacity	0.006	0.013	0.018	0.014	0.013	0.011	0.012
	(0.022)	(0.017)	(0.016)	(0.014)	(0.013)	(0.012)	(0.011)
Capacity × Black/Hisp	0.010	0.001	0.012	0.010	0.006	0.002	-0.001
	(0.026)	(0.020)	(0.018)	(0.015)	(0.014)	(0.013)	(0.013)
N	37,460	59,860	72,674	87,504	97,320	107,513	113,309
Exp. Sent of Guilty Plea (log)							
PD Capacity	0.092	0.049	0.065	0.016	-0.000	0.014	0.005
	(0.095)	(0.072)	(0.067)	(0.061)	(0.058)	(0.053)	(0.052)
Capacity × Black/Hisp	-0.258**	-0.253***	-0.209**	-0.105	-0.076	-0.083	-0.078
	(0.124)	(0.092)	(0.084)	(0.075)	(0.071)	(0.066)	(0.064)
N	13,078	20,909	25,677	30,620	33,888	37,168	38,836
Sent. Length (log)							
PD Capacity	0.043	-0.001	-0.044	-0.084	-0.097	-0.112*	-0.112*
	(0.129)	(0.095)	(0.084)	(0.074)	(0.068)	(0.065)	(0.063)
Capacity × Black/Hisp	-0.343**	-0.308***	-0.213**	-0.122	-0.127	-0.095	-0.089
	(0.148)	(0.107)	(0.094)	(0.085)	(0.079)	(0.076)	(0.075)
N	8,649	14,172	17,590	20,818	22,764	24,442	25,217
Case, Demographic Controls	Y	Y	Y	Y	Y	Y	Y
Controls Interacted w/ Race	Y	Y	Y	Y	Y	Y	Y
County-time FEs	Y	Y	Y	Y	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Cells within a single column for a given outcome come from a single regression. The specification is Equation 4, with court-time fixed effects added as discussed in the text. All specifications in columns 1-6 include case and demographic controls, interacted with an indicator for Black and Hispanic defendants. Those controls appear below Table 5. Sample construction varies by column as I adjust to include cases that took different amounts of time to . My preferred sample appears in column 2; these estimates are identical to those in columns 5 and 6 of Table 6. Robust standard errors clustered at the court-week level appear in parentheses. Sample sizes vary within column because not all defendants receive bail, plead guilty, or are sentenced to incarceration.

Table A8: Reduced-form Interaction Effects, Focusing on Black and White Defendants

	Preferred Specification		Interaction w/ Black Only		Only White and Black Defendants	
	PD Capacity (1)	Capacity × Black/Hisp (2)	PD Capacity (3)	Capacity × Black (4)	PD Capacity (5)	Capacity × Black (6)
Bail Amount (log)	-0.100	-0.025	-0.061	-0.116**	-0.085	-0.056
	(0.068)	(0.080)	(0.062)	(0.051)	(0.070)	(0.083)
N	48,586	48,586	48,586	48,586	45,509	45,509
# Charges Added	-0.178***	0.030	-0.140***	-0.053	-0.153**	0.019
	(0.058)	(0.066)	(0.054)	(0.037)	(0.061)	(0.069)
Any Charge Added?	-0.046***	0.003	-0.043***	-0.005	-0.043**	-0.004
	(0.016)	(0.018)	(0.014)	(0.010)	(0.017)	(0.019)
Any Guilty Plea?	0.008	0.027	0.022	-0.003	0.004	0.037
	(0.019)	(0.021)	(0.017)	(0.012)	(0.020)	(0.023)
Incarcerated?	0.013	0.001	0.020	-0.016	0.004	0.009
	(0.017)	(0.020)	(0.016)	(0.011)	(0.018)	(0.021)
N	59,625	59,625	59,625	59,625	55,916	55,916
Exp. Sent of Guilty Plea (log)	0.049	-0.253***	-0.005	-0.144***	0.039	-0.239**
	(0.072)	(0.092)	(0.066)	(0.053)	(0.074)	(0.096)
N	20,524	20,524	20,524	20,524	19,276	19,276
Sent. Length (log)	-0.001	-0.308***	-0.080	-0.166**	0.018	-0.330***
	(0.095)	(0.107)	(0.089)	(0.071)	(0.098)	(0.111)
N	13,659	13,659	13,659	13,659	12,833	12,833
Case, Demographic Controls	Y	Y	Y	Y	Y	Y
Controls Interacted w/ Race	Y	Y	Y	Y	Y	Y
County-time FEs	Y	Y	Y	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Each cell presents a reduced-form coefficient from a regression following Equation 4. For each outcome variable, coefficients in columns 1 and 2, 3 and 4, and 5 and 6 come from a single specification. Columns 1 and 2 are identical to columns 5 and 6 in Table 6. The specification I use in columns 3 and 4 interacts my PD capacity instrument with an indicator for whether the defendant is Black (not Black or Hispanic, as in the preferred model). The final two columns present estimates from a sample that includes only white and Black defendants. All specifications include case and demographic controls interacted with defendant race, as well as court-time fixed effects, as disucssed in the test. Robust standard errors clustered at the court-week level appear in parentheses. Sample sizes vary within column because not all defendants receive bail, plead guilty, or are sentenced to incarceration.

Table A9: Evaluating PD Effect Separately by Defendant Race

	Black Defendants				White Defendants			
	PAC Mean (1)	Reduced Form (2)	First Stage (3)	2SLS (4)	PAC Mean (5)	Reduced Form (6)	First Stage (7)	2SLS (8)
Exp. Sent. of	0.919	-0.191*	0.312***	-0.612	0.694	0.021	0.174***	0.118
Guilty Plea (log)	(1.18)	(0.115)	(0.054)	(0.373)	(0.995)	(0.085)	(0.050)	(0.489)
N:	*2,464*	*9,037*	*9,037*	*9,037*	*2,473*	*10,661*	*10,661*	*10,661*
Sentence Length (log)	1.765	-0.344***	0.344***	-0.998***	1.451	-0.036	0.258***	-0.140
	(1.265)	(0.125)	(0.063)	(0.387)	(1.128)	(0.117)	(0.064)	(0.452)
N:	*1,978*	*6,638*	*6,638*	*6,638*	*1,587*	*6,726*	*6,726*	*6,726*
Case, Demographic Controls		Y	Y	Y	—	Y	Y	Y
County-Time FEs		Y	Y	Y	—	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Columns 1 and 5 present sample means of the outcome variables in the left-hand column among Black and white defendants, respectively, who are represented by private assigned counsel. Standard deviations appear in parentheses. Each of the remaining cells comes from a distinct regression run on the given subsample (Black defendants only for columns 2-4, white defendants only for columns 6-8). Columns 2 and 6 report reduced-form estimates following Equation 2; columns 3 and 7 report first-stage estimates following Equation 1; and columns 4 and 8 report 2SLS coefficients following Equation 3. Robust standard errors clustered at the court-week level appear in parentheses. Case and demographic controls are described below Table 5. Sample sizes vary within column since not all defendants are incarcerated.

Table A10: Summary of Defense Attorney Characteristics, by Type of Attorney

	(1) Public Defender	(2) Private Counsel
Matched to Voter Roll?	0.16	0.40
	(0.37)	(0.49)
N:	*807*	*315*
Black?	0.16	0.16
	(0.37)	(0.37)
White?	0.73	0.77
	(0.44)	(0.42)
Democrat?	0.44	0.50
	(0.50)	(0.50)
Female?	0.25	0.50
	(0.43)	(0.50)
Age	49.04	46.27
	(13.80)	(13.61)
N:	*134*	*127*

Each cell provides the mean of the left-hand defense attorney characteristic, with standard deviations in parentheses. Defense attorney demographic information comes from a "fuzzy match" if defense attorney names to the North Carolina voter roll. As the first row indicates, of 1,122 public defenders and private assigned counselors in my research sample, I successfully match 261, or 24 percent, to voter registration information. Defense attorney age is captured in 2018.

Table A11: Explicitly Controlling for Defense Attorney Characteristics

	Matched Subsample		+Def Attny Controls	
	PD Capacity (1)	Capacity×Black (2)	PD Capacity (3)	Capacity×Black (4)
Exp. Sent. of Guilty Plea (log)	0.002	-0.240	-0.038	-0.178
	(0.142)	(0.173)	(0.142)	(0.173)
N:	*7,852*		*7,852*	
Sentence Length (log)	-0.034	-0.184	-0.055	-0.155
	(0.198)	(0.211)	(0.199)	(0.212)
N:	*5,504*		*5,504*	
Case, Demographic Controls	Y	Y	Y	Y
Def. Attny. Controls	N	N	Y	Y
Controls Interacted w/ Race	Y	Y	Y	Y
County-time FEs	Y	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Within each row, estimates in columns 1 and 2 come from a single regression, as do estimates in columns 3 and 4. The specification is Equation 4, with controls for case and demographic characteristics, interacted with an indicator for whether the defendant is Black or Hispanic. Both estimation samples include only cases for which I successfully matched the defense attorney to the North Carolina Voter roll. In columns 3 and 4, I include controls for defense attorney demographics that I obtain from the voter roll. These variables include indicators for defense attorney race (Black or white); age (in years, as of 2018); an indicator for whether the attorney is registered as a Democrat; and an indicator for whether the defense attorney is female. Robust standard errors clustered at the court-week level appear in parentheses.

Ignorance is Strength?

Information and Incentives in Plea Bargaining*

Brett Fischer and Leora Friedberg†

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(Please find the most up-to-date version here.)

Abstract

Up to 95 percent of criminal cases result in negotiated guilty pleas, which legal scholars often attribute to a disparity between well-informed prosecutors and poorly-informed defendants. This paper evaluates whether informing defendants about the adverse consequences associated with pleading guilty bridges this information gulf and reduces plea bargaining rates. As a plausibly exogenous shock to defendants' information sets, we study the Supreme Court's 2010 decision in *Padilla v. Kentucky*. *Padilla* requires defense attorneys to advise their noncitizen clients whether or not they will face deportation by accepting a plea offer. We employ a difference-in-differences design to compare citizens and noncitizens before and after the Court's ruling. Our results show that *Padilla*'s information intervention reduced guilty plea rates by 17 percent among noncitizen cases for which a conviction would risk immediate deportation. Conversely, we find that noncitzen cases that do not risk imminent deportation plead guilty 14 percent *more often* following *Padilla*, and were 9.7 percent more likely to be incarcerated. We interpret these results as evidence that prosecutors threaten harsher charges for defendants who turn down unfavorable but not catastrophic plea deals. Our findings highlight the limitations of existing and proposed procedural reforms to the plea bargaining process.

JEL Classifications: K14,K37,K41,K42
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*The authors declare that they have no relevant or material financial interests that relate to the research described in this paper. We take responsibility for any mistakes herein.

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1 Introduction

The vast majority of criminal cases culminate in negotiated guilty pleas, a status quo which observers argue empowers prosecutors seeking harsh sentences and exacerbates socioeconomic disparities in the criminal justice system (Berdejó 2018; Fischer 2021; Silveira 2017).[1] In particular, legal scholars contend that prosecutors commonly threaten to pursue harsh charges if defendants reject their plea offers, undermining the Sixth Amendment right to trial (Crespo 2018; Stuntz 2004); these "coercive" bargaining tactics have drawn fire for perpetuating mass incarceration.[2] Reform proposals abound. These plans often try to shift bargaining power away from prosecutors by ensuring defendants receive accurate signals of probable trial outcomes and the ramifications of accepting a plea deal.[3] For example, some states allow judges to weigh in on the merits of plea offers (Batra 2015; Turner 2006), while others require prosecutors to share evidence during pre-trial bargaining (Grunwald 2017).

Despite the intuitive appeal of these policies, it is theoretically ambiguous whether clarifying the costs and benefits of a plea deal actually helps defendants. The canonical model of plea bargaining—as in Abrams (2011), Bushway, Redlich, and Norris (2014), Reinganum (1988), and Silveira (2017)—implies that well-informed defendants are more likely to reject punitive plea offers from the prosecutor. In this light, explicating the consequences of plea deals to defendants might improve their outcomes. However, expansive prosecutorial powers and the complexity of plea negotiations mean that additional information may simply confuse defendants (Bibas 2011) and thereby reinforce prosecutors' leverage. The stakes are high: avoidable guilty pleas generate substantial economic costs (Agan and Starr 2018; Dobbie, Goldin, and Yang 2018; Mueller-Smith 2015; Rose 2019).

1. Around 95 percent of federal cases result in guilty pleas (Federal Bureau of Justice Statistics 2015). Patchier data suggest state courts have lower plea rates than the federal courts, but still see a large majority of cases resolved via guilty pleas.

2. See, among many others, "A Plea for Change" (2014), Weiser (2016), Will (2020), and Yoffe (2017).

3. To be sure, other policy initiatives take a very different tack. For instance, the Philadelphia district attorney's office has eliminated cash bail for minor offenses and has pursued plea deals involving non-incarceration punishments. Still, since neither policymakers nor researchers have coalesced around a panacea to address the side effects of the plea-based criminal justice system, we believe it is of first-order importance to understand the impact of information-based interventions, which are already on the books in many court systems.

Yet, there is little rigorous empirical evidence to inform this debate.[4] Any research strategy must overcome two empirical challenges. First, any data source must track defendants from arrest to sentencing; using sentencing information alone introduces selection bias, since cases resulting in conviction differ from those resulting in dismissal. Second, the quality and quantity of information a defendant receives about a plea offer is ordinarily both unobservable and endogenous—a function of the severity of the charges against her, and of her ability to pay for competent counsel, both of which are likely correlated with case outcomes.

To address this endogeneity concern, we leverage a natural experiment in the provision of information during plea bargaining: the Supreme Court's 2010 ruling in *Padilla v. Kentucky* (559 US 356), an unanticipated judicial decision that specified an information intervention for some defendants, but not others. *Padilla* stipulated that defense attorneys must advise noncitizen defendants whether or not they will risk deportation if they accept a given plea deal. *Padilla*'s rationale echoes that of the legislation described above: that well-informed defendants, aware of the advantages and disadvantages of taking a plea offer, can optimize their decision over whether to plead guilty.

Padilla spurred visible changes in criminal procedure for noncitizens that likely increased their awareness of the immigration consequences of pleading guilty. Attorneys now have to ascertain defendants' immigration status and, ideally, "craft a conviction and sentence that reduce the likelihood of deportation" (*Padilla* 559 US 373). Additionally, pursuant to *Padilla*, judges now caution defendants that they might experience immigration consequences by admitting their guilt; when signing plea agreements, defendants specifically acknowledge that pleading guilty could initiate separate immigration proceedings.[5] Crucially, *Padilla* did

4. The notable exception is Grunwald (2017), who effectively conducts a time-series analysis of recent open-file discovery reforms (whereby prosecutors reveal their cases to the defense pre-trial) in Texas and North Carolina. Using aggregate data, the author finds suggestive evidence that these policies led to more trials, at least temporarily, although he concludes that, overall, they have little effect on trial rates or disposition times. Given the author's time series approach, though, it remains unclear how trial rates would have evolved in the absence of these open-file policies.

5. As we discuss in Section 2, neither of these warnings needs to specify what consequences a noncitizen might encounter, and are given even to those who will face no consequences whatsoever.

not change any underlying immigration law, nor did it affect how courts process citizens.[6]

We employ a difference-in-differences (DiD) design to evaluate *Padilla*'s causal impact on noncitizen guilty plea rates. Our DiD framework compares citizen and noncitizen defendants charged before and after *Padilla* came into force. We argue that the parallel trends assumption holds in our context, and we provide suggestive evidence that no asymmetric shocks to noncitizen defendant characteristics bias our results. To estimate our DiD specification, we construct a detailed dataset that includes the universe of charges filed in Harris County, Texas (which contains the city of Houston). Sixteen percent of Harris County defendants are noncitizens, one of the highest rates in the country. Our data include charge, disposition, and sentencing information, alongside defendant characteristics, including citizenship.[7]

Difference-in-differences estimates reveal that, on average, *Padilla*'s information intervention led to *worse* outcomes for noncitizens. We find that *Padilla* decreased case dismissal rates by 3.9 percentage points (24 percent), and increased guilty plea rates by 2 percentage points (3.5 percent), among defendants whose cases involve charges with immigration consequences. These cases took 66 fewer days to dispose (17 percent), consistent with the explanation that noncitizens quickly accepted unfavorable plea offers from the prosecutor.[8]

To better understand these counterintuitive results, we consider heterogeneity in the *Padilla* effect by the type of crime committed. Generally, a first-time conviction for a misdemeanor offense does not provide grounds for deportation; however, many felony convictions do trigger immediate deportation.[9] These circumstances, in turn, lead to very different infor-

6. Subsequent Supreme Court decisions in 2012, drawing on *Padilla*, clarified that defense attorneys have a responsibility to accurately convey the details of plea offers to defendants. We discuss this point in Section 2. But our sample period ends in 2012, and so our results will not be biased by these later rulings.

7. Most comparable data sets do not include information about defendant citizenship. However, all we can observe is whether or not a defendant is a citizen of the United States; we cannot distinguish, for example, permanent residents from undocumented immigrants.

8. We argue that our results stem entirely from *Padilla*. To make this case, we provide evidence of zero effects among two groups that should be unaffected by *Padilla*: citizens charged with any crime, and noncitizens charged with crimes that do not have immigration consequences. Our DiD specification shows that the post-*Padilla* effect on citizen defendants is negligible. Moreover, complementary analyses using the sample of cases that have no immigration consequences reveal statistically and economically insignificant effects, for both citizens and noncitizens.

9. In Section 2, we discuss this point in detail. Only a conviction for "aggravated felonies"—crimes such as drug distribution and aggravated assault—trigger immediate deportation. The majority of misdemeanor

mation interventions under *Padilla*: some defendants will be told, unambiguously, that they face deportation upon conviction; others will receive a more nuanced warning that conviction will likely not have immediate consequences, but could have long-term ramifications.

In fact, we find that *Padilla* actually benefited defendants who face the greatest risk of deportation if convicted. DiD estimates indicate that, among noncitizen felony defendants, *Padilla* decreased guilty plea rates by up to 2.8 percentage points (7 percent) and increased case disposition times by 58 days (11 percent). These defendants appear to strike higher-quality plea deals: their incarceration rate decreased by 3.21 percentage points (4.4 percent).[10] These findings suggest that clarifying the consequences of pleading guilty benefits defendants who are most susceptible to catastrophic outcomes.

On the other hand, *Padilla* appears to have disadvantaged noncitizens who would have received less clear-cut advice from their attorneys. Post-*Padilla*, noncitizen misdemeanor defendants are 5.5 percentage points (8 percent) more likely to plead guilty and 6.8 percentage points (39 percent) less likely to have their cases dismissed. We also find that these defendants are 5.6 percentage points (8.6 percent) more likely to be incarcerated.

It is worth emphasizing that noncitizens charged with misdemeanor offenses do not face any short-term judicial or immigration consequences beyond those faced by comparable citizens, but could, in some circumstances, face long-term repercussions. As such, our results are consistent with the notion that ambiguity empowers prosecutors: the data suggest that prosecutors implicitly or explicitly threaten to pursue charges with more serious immigration ramifications should the defendant reject a plea offer, using the specter of deportation to extract harsher plea terms. Of course, we cannot observe the negotiation process. However,

offenses—such as theft or simple assault—technically have immigration consequences but fall under the umbrella of "crimes involving moral turpitude" (CIMTs). Conviction of a CIMT does not provide grounds for deportation unless the defendant has a recent prior CIMT conviction.

10. To probe the robustness of these felony results, we perform a similar analysis among felony cases filed in Bexar County, Texas, a much smaller jurisdiction containing the city of San Antonio. The Bexar County courts handle far fewer noncitizen defendants than their counterparts in Harris County, which reduces the precision of our estimates. Still, we uncover suggestive evidence that *Padilla* also reduced guilty plea rates and increased disposition times among felony cases in Bexar County. These results help allay concerns about external validity, as well as concerns about bias from unrelated policy changes in Harris County.

we note that misdemeanor defendants with prior convictions (for whom another guilty plea would prompt deportation) have lower guilty plea and incarceration rates than defendants without prior convictions. These patterns in the data suggest that prosecutors leverage defendants the possibility (but not inevitability) of deportation to pursue tougher plea deals. Put differently, complexity of the plea bargaining process and immigration law seems to make prosecutors' threats more credible This conclusion underscores the sway prosecutors hold during plea bargaining, and highlights the unintended consequences of existing and proposed policies meant to better inform defendants and strengthen their bargaining positions.

Indeed, we believe our analysis speaks to broader policy debates concerning the efficacy of plea bargaining. Citizen defendants routinely confront the prospect of catastrophic outcomes not unlike deportation, including life sentences, thousands of dollars in fines, and the loss of parental rights. Our results suggest that informing defendants about such dire consequences contained (or not) in a plea deal is an incomplete remedy for the ills of plea bargaining. Procedural fixes—including those prescribed by the Supreme Court and many state legislatures—do assist defendants facing the most severe plea and trial outcomes, but incentivize defendants facing relatively minor charges to plead guilty. Instead, policy efforts to reform plea bargaining should incentivize defendants to wait for favorable plea offers or case dismissal, including by removing cash bail (Leslie and Pope 2017; Stevenson 2018) and discouraging punitive charging practices (Agan, Doleac, and Harvey 2021).

Beyond its policy implications, our analysis contributes to three strands of research. First, by showing how defendants respond to information about adverse plea outcomes, we advance the empirical literature on the determinants and efficacy of plea bargains (Abrams 2011; Berdejó 2018; Bushway and Redlich 2011; Bushway et al. 2014; Fischer 2021; Silveira 2017; Yang 2016). Second, our findings demonstrate how the ubiquity of negotiated guilty pleas might generate high conviction rates for relatively minor crimes. This finding helps explain the prevalence of misdemeanor guilty pleas, and helps contextualize a budding literature that explores the causes and consequences of misdemeanor convictions (Agan et al. 2021; Mueller-Smith and Schnepel 2021; Natapoff 2018; Stevenson and Mayson 2018). Finally, our paper provides a striking example of how deportation (dis-)incentivizes noncitizens, complementing existing evidence of aversion to immigration enforcement in American communities (Alsan and Yang 2018; Bellows 2019; Watson 2014).

2 Policy Background

2.1 The Immigration Consequences of Criminal Convictions

For noncitizen residents of the United States, encounters with the criminal justice system can have profound immigration consequences, including removal from the country and revocation of any future right to seek asylum or apply for residence in the US. Broadly speaking, federal law sanctions immigrants for two types of offenses: (1) aggravated felonies (AFs) and (2) crimes involving moral turpitude (CIMTs).[11] These categories do not have clear analogues in ordinary criminal law. Even more confusingly, the crimes that fall into these categories can also vary across states (depending on the exact language of state criminal statutes) and across US appellate circuits (depending on case law brought in those jurisdictions). Bear in mind that being *charged* with either an AF or a CIMT does not in itself pose any immigration consequences for those legally in the United States. Only a conviction for one of these offenses—via a guilty plea, a bench ruling, or a jury trial—can prompt deportation proceedings. We summarize the salient features of AFs and CIMTs below.

1. **Aggravated Felonies (AFs)**: Convictions for AFs pose the more severe threat for noncitizens, and often provide grounds for immediate deportation. Contrary to their name, these offenses need neither be aggravated offenses nor felonies. For example, possession of a small quantity of marijuana is a misdemeanor in many states—where it is a crime at all—but was until recently considered an aggravated felony for the purposes of immigration law.[12] Some offenses, including controlled substance possession and

11. Immigration lawyers oftern refer to several more categories of problematic offenses, including crimes of violence, firearms offenses, and crimes involving controlled substances. These types of offenses mostly fall under the broad AF or CIMT umbrellas, however.

12. That policy has since been revised; possession of up to 30g of marijuana for personal use is no longer

distribution charges, immediately qualify as AFs. Others, particularly burglary, only count as AFs if the court imposes a sentence of at least one year, including any combination of incarceration time and probation/suspended sentencing. Other common AFs include violent offenses such as murder and rape, any crimes involving firearms, as well as aggravated assault.[13]

2. **Crimes Involving Moral Turpitude (CIMTs):** Unlike AFs, convictions for CIMTs do not usually entail immediate immigration consequences. A defendant convicted of a CIMT can only be deported if she has previously been convicted of another CIMT, or if her current CIMT conviction results in an sentence of at least one year probation, incarceration, or a combination of the two. The phrase "crime involving moral turpitude" has no statutory definition, and so the offenses that fall into this category have been identified almost exclusively via case law. Generally, a CIMT involves both conduct that is "inherently base, vile, or depraved, contrary to the rules of morality" (*Matter of Danesh* 19 IN Dec. 669, 670 [BIA 1988]) and a degree of willfulness—an offender must intentionally commit an immoral act. By this logic, drink-driving (DUI/DWI) does *not* count as a CIMT, since the offense does not stem from an evil intent. On the other hand, theft, fraud, and sexual assault all qualify as CIMTs. (Note that simple assault does *not* count as a CIMT.) Many CIMTs become AFs if a defendant receives a sentence of a year or more, or if the charges are enhanced (e.g., while "assault" on its own is not a CIMT, "assault with a deadly weapon" is).

Given the nuances involved in determining whether a conviction poses a deportation risk to a defendant, many criminal defense attorneys seek outside assistance from immigration specialists, or recommend that their noncitizen clients do so. Public defenders' offices and private practices often develop reference guides to enumerate ways for their attorneys to avoid

considered an AF.

13. Aggravated assault only qualifies as an AF if the court imposes a sentence of one year or more. Oftentimes, this is a moot point as state laws—including the Texas criminal code, the relevant standard for our study—impose mandatory minimum sentences that exceed one year.

immigration consequences for noncitizen clients, particularly in the aftermath of *Padilla v. Kentucky*, which we discuss below.

2.2 How *Padilla v. Kentucky* Impacted Criminal Procedure for Noncitizens

The Supreme Court's decision in *Padilla v. Kentucky* (559 US 356 [2010]) represents a landmark acknowledgement of both the centrality of plea bargaining within the criminal justice system, as well as the unique challenges noncitizen defendants face when weighing plea offers. The Court's decision provided redress to Jose Padilla, a lawful permanent resident arrested on marijuana possession and transportation charges. The defendant pled guilty to these controlled substances offenses—which qualify as aggravated felonies (see the previous subection), and thus his conviction subjected him to deportation. The Supreme Court agreed with Mr. Padilla that his defense attorney's (incorrect) affirmation that pleading guilty would not result in immigration consequences led him to accept a plea deal he otherwise would have declined. The Court ruled that the deportation consequences of Mr. Padilla's guilty plea were easy to establish, that the defense attorney should have done so, and that he should have advised his client accordingly.

In practice, *Padilla* obligates defense attorneys to alert noncitizen defendants that they may face immigration consequences after accepting a prosecutor's plea offer. The Supreme Court stipulated that when immigration law is "succinct, clear, and explicit" that deportation will result from pleading guilty to a particular offense (*Padilla* 559 US 368), an attorney must inform his client that she will likely be deported if she accepts a plea deal. If the law is unclear or if the attorney is unsure of the probability of deportation, he must still "advise a noncitizen client that pending criminal charges may carry a risk of adverse immigration consequences" (*Padilla* 559 US 369). To this end, post-*Padilla*, judges often caution defendants who attempt to plead guilty that they should consider any immigration consequences that might ensue. Plea deals themselves include a boilerplate statement to the effect that the defendant might face immigration consequences by pleading guilty. These forms require defendants to sign or initial next to this statement. In other words, *Padilla* generated a set of concrete interventions in the plea bargaining process that likely influenced noncitizen defendants' information set.[14]

Moreover, as the Supreme Court itself noted, informing noncitizen defendants of impending immigration consequences alters their incentive structure during pre-trial bargaining. Armed with the knowledge that a current plea offer would lead to deportation, defendants and their attorneys can "avoid[ ] a conviction for an offense that automatically triggers the removal consequence" (*Padilla* 559 US 373); likewise, defendants may "plead guilty to an offense that does not mandate [deportation]," potentially in exchange for the prosecutor dropping charges that do carry immigration consequences (*Padilla* 559 US 373).

Of course, legal scholars have cast doubt on *Padilla*'s real-world utility for defendants. For instance, Brown (2010) contends that, irrespective of the vagaries of immigration law, criminal law leaves defense attorneys with little room to actually improve the quality plea deals, for either citizens or noncitizens; Vázquez (2011) takes issue with the vagueness of the Court's information mandate, and argues that the nebulous (and potentially erroneous) caution of unspecified immigration consequences could even hurt defendants. In other words, from an empirical perspective, it is unclear *ex ante* whether *Padilla*'s information intervention benefited defendants, as the Supreme Court hoped it would, left them unaffected, or undermined their efforts to secure advantageous plea deals.

2.3 Harris County Institutional Features and Contemporaneous Policy Changes

Our policy setting is Harris County, Texas, which contains the city of Houston. A large, urban jurisdiction, Harris County provides an ideal setting for our study, since the court system processes an unusually high volume of (otherwise relatively rare) noncitizen defendants.

14. Many legal scholars read *Padilla* as the first in a series of recent cases that have tried to square the Sixth Amendment right to trial with the prevalence of plea bargains, which effectively preclude trials. For our purposes, we think of*Padilla* as case exclusively pertaining to noncitizens, and we provide empirical evidence suggesting the decision had no bearing on citizen defendants' outcomes. Still, we acknowledge subsequent cases—that, as we discuss in the next section, fall outside our tim eframe in this study—which applied themes from *Padilla* to citizen defendants. In *Missouri v. Frye* (566 US 134 [2012]), the Court ruled that attorneys must relay to defendants any plea offers made by the prosecution. And in *Lafler v. Cooper* (566 U.S. 156 [2012]), the Court decided that defendants can seek redress when their attorneys advise them to reject favorable plea offers.

More to the point, Harris County courts record defendant citizenship status—and, to our knowledge, it is the largest court system to do so.

Harris County criminal procedure echoes that of other jurisdictions in Texas and around the country. At the time of arrest, the police recommend charges against the defendants; the district attorney's (DA's) office then elects whether or not to prosecute those charges.[15] Defendants charged with (relatively minor) misdemeanor offenses have their cases entered into the Harris County Criminal Court at Law; those accused of felonies proceed through the Harris County District Court. Within either system, defendants are randomly assigned to a courtroom using a computerized random number generator. A single judge presides over each courtroom and, typically, each courtroom will also have its own team of assistant district attorneys (prosecutors). Neither prosecutors nor judges rotate very frequently; Mueller-Smith (2018) notes that prosecutors remain generally in the same courtroom for a whole calendar year, while judges only enter and exit as the result of quadrennial elections.

In early 2010, coincident with the *Padilla* ruling, the county created a public defender's office. Although its mandate has recently expanded, the public defender mostly represents the tiny share of indigent clients accused of capital offenses (e.g., murder). The vast majority of indigent defendants rely on private attorneys appointed by the court.

More problematic for our study are two large policy shifts in the Harris County DA's office that between 2009 and 2013. On January 1, 2009, District Attorney Patricia Lykos took office. Early in her tenure, the new district attorney pursued harsher penalties for undocumented immigrants entering the court system.[16] Additionally, beginning January 1, 2010, the DA's office stopped prosecuting drug possession charges involving "trace" amounts of controlled substances (less than 0.1 grams). When DA Lykos lost her bid for re-election in 2012, her successor made a point of reimposing prosecutions for trace drug offenses. This dy-

15. As in most jurisdictions, Harris County makes use of pre-trial detention and bail systems. At the time of arrest, defendants appear before a magistrate who sets conditions for pre-trial release. Unfortunately, we do not observe any information about the bail hearings, or bail conditions imposed, on the defendant. We do note, however, that *Padilla* should not have had any bearing on these proceedings.

16. The new DA also had a history of increasing the county's cooperation with Immigration and Customs Enforcement (ICE), which as a result had a high profile in the Harris County Jail by 2009.

namic policy environment obviously had a direct bearing on noncitizen defendants in Harris County, and poses a significant challenge as we try to estimate the causal impact of *Padilla* per se, rather than these unrelated changes in the DA's office. In the next section, we discuss how we construct our research sample in a way that, we believe, minimizes the potential bias from these confounding policy shifts.

3 Data

Our analysis leverages administrative data from the Harris County district clerk's office. These data contain the universe of criminal cases filed in the county between 1995 and 2020. Each entry in the raw data is a charge. Each charge record contains information about the offense committed, the defendant's name and demographic information, the charge disposition (e.g., dropped by the district attorney, guilty plea, etc.), and any sentence associated with the charge. We aggregate charges into cases to construct our final data set. We define a "case" to include all charges filed on a single day and courtroom for the same defendant.[17]

3.1 Sample Construction

The data do not include information about defendant Hispanicity. As such, we identify Hispanic defendants using a combination of two data sources. First, we try to locate defendants in the Texas Computerized Criminal History databse (CCH). The CCH contains charge records from across the state comparable to those we obtain from Harris County, but the CCH includes a field indicating whether a defendant is Hispanic. However, because counties submit their charge data to the CCH voluntarily, its records are incomplete. As such, we supplement the CCH data with a list of surnames for which the majority of 2010 Census respondents self-identified as Hispanic. We define a defendant to be Hispanic if her name and date of birth match to any Hispanic defendant in the CCH, or if her surname matches to the Census list.

17. The courts treat groups of charges like this as one unit, potentially including any other charges relating to the same root crime that were entered on different dates. We take a relatively conservative approach in blocking off cases and only group together charges created for the same defendant on the same day.

Importantly, the data include an indicator for whether or not a defendant is a citizen of the United States. Very few jurisdictions record this information, which is crucial to our research design. However, the citizenship indicator is very coarse. We do not know the specifics of any noncitizen defendant's immigration status—for example, we cannot tell apart lawful permanent residents from undocumented immigrants.

Our final research sample focuses on cases filed between January 1, 2009, and June 30, 2012, which overlaps with the tenure of an atypical district attorney in Harris County, as we discussed in Section 2. Recall that the new DA pursued harsher charging and sentencing of undocumented immigrants who appeared in the court system. We observe this policy change as a discontinuous jump in noncitizen guilty verdicts and sentences beginning in early 2009; citizens' outcomes remain flat. As we discuss in the next section, this asymmetric shift in outcomes would undermine our research design, and so we omit cases filed prior to 2009. We similarly remove cases filed after the second quarter of 2012 to avoid capturing the effects of the 2012 DA election, in which the incumbent lost and the incoming DA introduced a distinct policy agenda.

Additionally, we drop all cases that involve possession of less than one gram of a controlled substance—the smallest unit present in the charge data. We do so because both the prevalence and conviction rates of these charges decline in January 2010, when the DA's office stopped prosecuting "trace" possession offences. This policy shift appears to have disproportionately affected citizens, which, again, poses a threat to our identification approach.

Finally, to ensure that cases involving citizen and noncitizen defendants are homogeneous and comparable, we only include cases for which the original charges (i.e., those filed by the police) carry immigration consequences, either CIMTs or aggravated felonies (see Section 2). Logically, the Supreme Court's decision in *Padilla*—our "treatment"—should have had no bearing on cases whose charges do not foreshadow immigration consequences. Indeed, in Section 6, we present a placebo analysis using cases without CIMTs or aggravated felony charges, and we report consistent null effects.

3.2 Summary Data

Table 1 summarizes the 149,872 cases in our final sample. A large majority—84 percent—of cases involve citizen defendants. Only 25 percent of defendants are white; a plurality of citizen defendants are Black (48.5 percent), while a majority of noncitizen defendants are Hispanic (53.4 percent). Citizen and noncitizen defendants have similar ages, on average, and are equally likely to be female (28.6 percent). Unsurprisingly, given the deportation risks associated with arrest, noncitizen defendants have fewer than half the number of prior appearances in the Harris County justice system as do citizen defendants (0.92 prior interactions, versus 2.12 prior interactions for citizens). Differences in case characteristics while less stark, suggest citizen defendants are more likely to face drug charges (39.4 percent, versus 28.3 percent for noncitizens). Only around 40 percent of cases involve felony (relatively severe) charges; the remaining 59 percent are misdemeanor case, which carry maximum sentences of at most one year.

For most of this paper, we focus on four results of pre-trial bargaining: whether a defendant entered a guilty plea; whether she saw her case dismissed (dropped); how long it took for her case to be disposed; and whether she was incarcerated. Table 2 describes these case outcomes, by defendant citizenship and time of entry into the court system. The data underscore the punitiveness of the Harris County criminal justice system between 2009 and 2012. Almost 55 percent of defendants plead guilty, although that rate is lower (around 48 percent) among noncitizens. Only 17.6 percent of cases are dismissed (22-25 percent for noncitizens). Two-thirds of defendants receive an incarceration sentence. Above all, these facts underline the stakes involved in pre-trial bargaining in our setting.

4 Empirical Design

Our goal in this paper is to evaluate whether informing defendants about the consequences of entering a plea deal yields more favorable pre-trial outcomes for them. In general, the information sets held by the actors involved in pre-trial bargaining—the defendant, defense attorney, and prosecutor—are unobservable. Moreover, even if we could observe metrics of attorney and defendant information, those would likely be correlated with other determinants of case outcomes, such as attorney quality and (actual) defendant guilt.

Our research design leverages a natural shock to defendant information sets during pretrial bargaining. Specifically, we estimate the causal effect of *Padilla v. Kentucky*'s provision of information to noncitizen defendants. Our research design relies on the fact that *Padilla* only affected noncitizen defendant, leaving citizen defendants untreated. As such, we use a difference-in-differences (DiD) framework to compare (treated) noncitizens with (untreated) citizens whose cases entered the Harris County court system between 2009 and 2012, just before and after the Court's ruling on March 31, 2010.

4.1 Difference-in-Differences Framework

We estimate a straightforward DiD specification that captures the impact of having a case filed post-*Padilla* (that is, on or after April 1, 2010) on noncitizen defendant outcomes:

$$Y_{it} = \alpha_0 + \beta noncitizen_i \times post_t + \alpha_1 noncitizen_i + \alpha_2 post_t + \gamma_x X_{it} + \epsilon_{it}, \qquad (1)$$

where Y_{it} denotes an outcome for defendant i (e.g., whether she pleads guilty) who entered the court system on date t; "noncitizen" is an indicator equal to one if defendant i is not a citizen; "post" is an indicator equal to one if i entered the court system after March 31, 2010 (the date *Padilla* was handed down); X is a vector of baseline case and defendant characteristics; and ϵ is the error term. In some specifications, we also interact the vector X with the *noncitizen* indicator, thus allowing for defendant and case characteristics to have separate effects among cases concerning citizen and noncitizen defendants. The coefficient of interest, β, estimates the DiD effect of *Padilla* on noncitizens—that is, the difference in outcomes between noncitizens' cases filed pre-*Padilla* and noncitizens' cases filed post-*Padilla*, relative to the analogous change among cases involving citizen defendants.[18] Since

18. In 2013, the Supreme Court ruled in *Chaidez v. United States* (568 US 342) that *Padilla* does not apply retroactively to cases filed prior to March 2010. As such, we do not believe Harris County courts would have reopened cases filed in the year before *Padilla*—or, in other words, that our assumption that the "post"

cases filed in the same year and same courtroom will likely share a judge and lead prosecutor, we cluster our standard errors at the courtroom-year level.

4.2 Validity of the DiD Approach

For β to deliver a causal effect, we must assume that, had the *Padilla* ruling not occurred, noncitizen and citizen defendant outcomes would have evolved at the same rate after April 2010. Of course, we cannot test this assumption directly. However, we can test its "parallel trends" implication, that citizen and noncitizen outcomes should exhibit similar rates of change prior to *Padilla*. To evaluate this assumption empirically, we create a categorical "month" variable that denotes each month between January 2009 (the beginning of our sample period) and February 2010 (the month before *Padilla* was handed down).[19] We then estimate a version of Equation 1, substituting our categorical month time variable for the "post" indicator:

$$Y_{it} = \delta_0 + \omega noncitizen_i \times month_t + \delta_1 noncitizen_i + \delta_2 month_t + \nu_{it}, \tag{2}$$

where i, t, and "noncitizen" denote defendants, dates of case filings, and noncitizens, respectively. The parameter of interest, ω, captures the pre-*Padilla* difference in time trends in outcome Y between citizens and noncitizens.

We estimate Equation 2 for our four main outcome variables—whether a defendant pleads guilty; whether her case is dismissed; how long it takes for her case to be disposed; and whether she is incarcerated. The key results appear in first row of Table 3. Overall, we find nearly-zero and statistically insignificant differences in trends for citizens and noncitizens pre-*Padilla*, which reinforces our causal claim.[20]

period begins in April 2010 is reasonable. We do note, though, that β will likely be biased towards zero because cases filed prior to *Padilla* could still be "treated" by the decision if they are pending as of April 1, 2010. Since case disposition time is endogenous, there is no clear way ex ante to remove this source of bias. We thus take the transparent approach of using *Padilla*'s ruling date to delineate the pre- and post-periods, with the recognition that we will tend to understate *Padilla*'s true effect.

19. We omit March 2010 from our pre-period in this specific context because most cases filed in this month will not have nearly enough time to be disposed before the Supreme Court's ruling in *Padilla*. Including March would thus point to "'pre-trends" that are actually part of our treatment effect.

20. Figure 1 provides visual evidence that the parallel trends assumption holds, as we note in the next

Still, beyond the parallel trends assumption, we must also rule out potential confounding variation that could bias our results. For example, if noncitizens systematically faced more severe charges post-*Padilla*, that would lead us to overstate the decision's negative impact. This possibility is nontrivial in our setting. As we mentioned in Sections 2 and 3, the Harris County DA's office introduced an unrelated policy in January 2010 that resulted in fewer prosecutions of drug possession cases. We have already restricted our sample to exclude those cases most likely impacted by this policy. But we still want to rule out potential spillover effects—for instance, if police used the time they would have spent on trace drug arrests investigating crimes involving illegal immigrants.

To address this concern, we use Equation 1 to estimate the relative changes in defendant and case characteristics among citizens and noncitizens post-*Padilla*. We test for DiD effects among the covariates we summarize in Table 1. Those results appear in the first two panels of Table 4. Overall, we find no evidence of differential changes in defendant characteristics, nor in the probability that a defendant faces a felony charge. However, we do find some evidence of a shift in the types of offenses in cases involving noncitizens. In particular, post-*Padilla*, noncitizen defendants may be slightly less likely to be charged with property offenses; while only marginally significant, the point estimate (3.8 percentage points) is relatively large (6.7 percent of the mean). Instead, noncitizens appear to have higher rates of drug and traffic offenses (which increase by 2.3 and 1.1 percentage points, respectively). We suspect these shifts stem from the DA's drug policy, which may have had a halo effect on citizens but not on noncitizens, who include undocumented immigrants aggressively prosecuted by the DA.[21]

While this pattern concerns us, we note that the type of offense a case contains does not strongly predict any of our outcome variables. To emphasize this point, and to help allay

section.

21. A more prosaic explanation is that police had a greater incentive to keep pursuing low-level drug arrests among noncitizens, but not citizens. While the DA's office stopped prosecuting trace possession charges, it continued prosecuting (misdemeanor) drug paraphernalia charges. Anecdotal evidence suggests that although police could continue to arrest suspects caught with trace amounts of drugs and recommend a paraphernalia charge, few bothered, since the cost of filing paperwork outweighed the potential punishment for the defendant (at most, a $500 ticket). However, if the police believed a trace-possession suspect to be an undocumented noncitizen, that might have provided an extra incentive to go through with the arrest.

concerns that shifting charge composition might drive out results, we use the same covariates shown in Panels I and II to construct linear predictions of our main outcome variables. Encouragingly, our predicted outcome means resemble our actual outcome means (compare the first column of Table 4, Panel III to first column of Table 2), and, more importantly, we find no evidence of differential changes in these predicted measures. These null results suggest that, while the types of crimes noncitizens face may be changing, this effect has little bearing on their actual outcomes. Furthermore, in robustness checks, we allow for the effect of case type to vary by a defendant's citizenship status, which should account for any bias stemming from the observed changes in case composition. Most of our key results hold up to these more stringent controls, although we acknowledge that their magnitudes tend to be lower and less precise than those from our preferred specification. We describe these results, as well as our primary findings, in the next section.

5 Results: How *Padilla*'s Information Treatment Impacted Noncitizens

We turn now to presenting our difference-in-differences (DiD) estimates of *Padilla*'s impact on defendant plea bargaining outcomes. The information requirements imposed by the Supreme Court could have a substantial effect on the quality of plea bargains struck. Noncitizen defendants may have eschewed deals that would result in their deportation, potentially striking better deals or waiting so long that the prosecutor elected to drop his case. On the other hand, noncitizens may have had a hard time interpreting cautions about unspecified "immigration consequences," which might have further empowered prosecutors to impose harsh plea terms. We explore these possibilities empirically, first among all cases, then among felony cases for which conviction would likely to lead to deportation, and finally among misdemeanor cases for which conviction would probably not lead to immediate deportation. Throughout, we discuss what our findings show about the contours of prosecutors' powers during pre-trial bargaining, and the potential value of information to criminal defendants.

5.1 *Padilla*'s Average Effect on Noncitizens

We first present results from our full sample, which includes all criminal cases involving charges with immigration consequences, filed between 2009 and 2012, and excluding cases involving the smallest amounts of drug possession. Estimates from our DiD model appear in Table 5. The first column presents the pre-*Padilla* mean of each outcome variable among citizens (our control group). Column 2 presents DiD estimates from a specification without any controls; column 3 reports the results once we add case and defendant controls; column 4 reports the results once we add courtroom-year fixed effects; and column 5 reports results once we add case and defendant controls that we allow to vary by defendant citizenship.

Our headline results concern how *Padilla* impacted defendant guilty plea rates. Estimates from the first row of Table 5 show that, following *Padilla*, the guilty plea rate among noncitizens *increased* by 2-2.3 percentage points (3.5-4 percent of the control mean). This effect appears to stem from a reduction in case dismissals, which decline by 3.4-3.9 percentage points (21-24 percent).[22] Consistent with these findings, we also observe that cases involving noncitizens were disposed more quickly, 64-67 days sooner on average (16.2-17 percent). While faster disposition may seem like a good thing, quicker case resolutions suggests less effort on the part of attorneys and acceptance of earlier, likely less favorable, plea deals from the prosecutor. Indeed, we find suggestive evidence that defendants suffer immediate judicial consequences: point estimates indicate that incarceration rates among noncitizens increase by 1.9-2.2 percentage points post-*Padilla*, although we acknowledge that these estimates are imprecise without our full battery of controls.

For transparency, we also provide visual evidence in support of our regression results. Figure 1 plots average case outcomes by filing month during our sample period, for citizens and noncitizens. We include best-fit lines pre- and post-*Padilla* to help capture trends in the

22. The remaining cases that were not dismissed but also did not result in traditional guilty pleas had a "deferred adjudication" verdict. This outcome, unique to Texas, typically involves a guilty plea (though not always; it could result from a bench trial) in exchange for a kind of probation under which the defendant receives no criminal record. We return to this point in Section 5.2 below. For the time being, though, we note that deferred adjudication is a less-preferred outcome to dismissal, and functionally the same as a guilty plea for most of our purposes.

data and visualize the lack of differential pre-trends. Recall that cases filed in the months just prior to *Padilla* might still be "treated" as these cases likely remained pending in the court system as of April 2010.

These visual results reinforce two critical points. First, we note that, across all four outcomes, cases involving citizen defendants see no appreciable changes in their outcomes. Unreported regression coefficients from our DiD specification also bear this out. In other words, our DiD estimates capture changes in noncitizens' outcomes, and not changes in citizens' outcomes, consistent with our claim that the results we present stem entirely from *Padilla*, which only affected noncitizens. Second, we argue that our visual representation of the data confirms our regression results. In fact, Figure 1 points to discontinuous changes in outcomes—particularly dismissal, disposition time, and the rates—just after *Padilla* was handed down. This pattern lends added assurance that the Court's decision underpins our results. At the same time, we acknowledge that visual evidence for an uptick in guilty plea rates is less than compelling, although the months with the highest plea rates among noncitizens all appear after the *Padilla* ruling.

Our findings thus far support the conclusion that *Padilla* caused defendants to more readily accept plea offers from the prosecution. The question remains how informing defendants that they could face immigration consequences from pleading guilty somehow incentivizes higher plea rates. At stake is the value of the information during pre-trial bargaining. If, as Vázquez (2011) suggests, the vague *Padilla* warning simply confused defendants, we must conclude that information has limited utility for criminal defendants. On the other hand, if the average results mask heterogeneity in defendants' responses to the *Padilla* warning, that could leave room for policy interventions targeting specific defendants most able to utilize that knowledge.

Put differently, as we emphasize in Section 2, *Padilla* entailed different interventions for different defendants and cases. The most ambiguous information would likely have been presented to noncitizens whose charges included first-time CIMT offenses, which would not lead to deportation upon conviction, but *could* provide grounds for removal in the future, should the defendant re-offend. Conversely, defendants charged with aggravated felonies should have received an unambiguous statement that conviction would lead to deportation. Which of these pieces of information drives our results could shed light on how defendants cope with the complexities of plea bargaining and criminal law.

5.2 *Padilla*'s Effect on High-Risk Cases

To interrogate this point in detail, we consider how *Padilla* impacted defendants charged with offenses most likely to lead to deportation—that is, those charged with potential aggravated felonies. Defense attorneys representing these cases should inform their clients that pleading guilty will definitely result in deportation.

We first present visual evidence of changes in outcomes for felony cases. Figure 2 shows an uptick in case dismissal rates, at least temporarily, and slight decrease in guilty plea rates, among cases involving noncitizens. More convincingly, the raw data indicate that noncitizen defendants' disposition times increase markedly after *Padilla*, while their incarceration rates decrease sharply.

Regression estimates confirm these visual results. Table 6 reports marginally significant reductions in guilty plea rates—of up to 2.8 percentage points (7 percent)—among noncitizen defendants charged with offenses that qualify as aggravated felonies. We find no effects on dismissal rates. These cases take longer to resolve, up to 58 days more (11 percent). And we find up to a 3.1 percentage-point decrease in the probability of incarceration (4.5 percent), although we caution that this point estimate is sensitive to controlling separately for case and defendant characteristics among noncitizen defendants.

To contextualize these results, we break our sample down by type of offense. The decrease in incarceration rates that we observe (such as it is) suggests avoiding incarceration is a priority. That goal would primarily benefit defendants charged with felony property crimes, chiefly burglary, which only qualifies as an aggravated felony with a long enough sentence. The first two panels of Figure 3 compare the incarceration rate for cases involving (felony) property offenses with those involving all other offenses. We find that cases involving property offenses drive our findings.

The bottom panels of Figure 3 show effects on an outcome we have not explored so far, but which has particular significance for noncitizens facing potential aggravated felonies: deferred adjudication. This form of probation does not count as a sentence for immigration purposes, and so avoids a aggravated felony designation for property convictions. We find that *Padilla* increased deferred adjudication rates among noncitizen felony defendants charged with property crimes, but not those charged with other crimes, for whom their charges automatically qualify as aggravated felonies, irrespective of the sentence.

We interpret these findings as evidence that informing defendants about the adverse consequences of pleading guilty can incentivize them and their attorneys to seek better plea terms, at least in some circumstances. Indeed, our results highlight how limited the defense's toolkit appears to be in pre-trial bargaining: in the best case, defendants can avoid jail, but not conviction. For the majority of felony offenses—which will invariably lead to deportation following a conviction—the only reprieve would be dismissal, and defendants remain unable to secure that outcome.

5.3 *Padilla*'s Effect on Low-Risk Cases

We next consider *Padilla*'s effect on cases for which a noncitizen defendant might get less clear-cut input from her attorney: crimes involving moral turpitude (CIMTs). Visual evidence in Figure 4 shows noticeable drops in case dismissal rates and disposition times among noncitizens charged with CIMTs post-*Padilla.* Incarceration rates appear to increase discontinuously, while guilty plea rates rise, at least in a difference-in-differences sense. Regression estimates in Table 7 quantify these effects. We find that *Padilla* increased guilty plea rates among noncitizens charged with CIMTs by 3.8-5.5 percentage points (5.6-8.1 percent) and decreased case dismissal rates by 132-153 days (42-49 percent). Perhaps as a consequence, incarceration rates increase by 4-5.6 percentage points (6.1-8.6 percent).

We believe these results stem from defense attorneys informing their clients that they will *not* face deportation as a consequence of pleading guilty. The narrative we have in mind assumes that a prosecutor's first plea offer comes soon after a case is filed, and extends unfavorable terms to the defendant. Yet, however harsh the deal is in terms of incarceration, defendants interpret the lack of immediate immigration consequences as a sign of the plea deal's quality. Prosecutors might encourage this assumption by threatening charges at trial that would lead to deportation. For example, a prosecutor might announce that a defendant charged with theft (a CIMT) could face charges of burglary (a potential aggravated felony) if she decided to go to trial. We cannot observe whether prosecutors make such threats. However, the fact that noncitizens' cases resolve so much more quickly post-*Padilla*, at the same time as plea rates go up, strongly suggests these defendants are pleading guilty faster and more often than they otherwise would have.

Our story hinges on the idea that prosecutors have leverage over noncitizens facing charges that do not portend imminent deportation, because they can promise worse consequences at trial. To informally test this idea, we consider how *Padilla* affected noncitizens who have prior convictions in the court system—in other words, those who likely *would* face deportation should they receive another conviction. We do so using a triple-difference specification in which we interact our DiD interaction term ($noncitizen \times post$) with an indicator for whether a defendant has a prior conviction in the Harris County court system, alongside appropriate triple-difference interaction covariates. Our specification also includes full interaction of our case and defendant demographic controls with both an indicator for whether a defendant is a noncitizen and an indicator for wheether the defendant has a prior conviction. Triple-difference estimates appear in Table 8. We find that having a prior conviction essentially negates the impact of *Padilla*, decreasing the likelihood that a noncitizen defendant pleads guilty and increasing the probability the she sees her case dismissed. In other words, prosecutors' leverage appears to be inversely related to the probability that a defendant faces adverse consequences from a conviction.

6 Robustness

In the previous section, we reported results suggesting that *Padilla*'s information intervention benefited defendants for whom a conviction would prompt immediate deportation, while additional information hurt defendants who did not face immediate deportation. We interpreted these results as evidence that greater information affected the degree of leverage which prosecutors have over defendants. In this section, we explore alternative explanations for our results.

6.1 Complementary Analysis of Bexar County, TX

Our analysis thus far has focused exclusively on Harris County, TX, which has the advantage of a large sample of clearly-identified noncitizen defendants. However, focusing on only one jurisdiction leaves open the possibility that our results capture a reaction unique to our specific setting. It could be that because Harris County processes so many noncitizen defendants, defense and prosecuting attorneys there reacted much differently to *Padilla* than did their counterparts in other jurisdictions with less experience handling such cases. Equally, we wish to dispel concerns that our results stem from contemporaneous policy changes in Harris County.

To achieve both these goals, we replicate our analysis using data on felony cases filed in Bexar County, TX (home to the city of San Antonio). Data from the Bexar County District Court provide much of the same information as our main Harris County data, but omit defendant citizenship. We use the Texas CCH database (see Section 3) to determine Bexar defendants' citizenship status. Of 155,046 cases filed between 2005 and 2020, we successfully match 108,558 to the CCH database using name and date of birth. Of those cases, only 2,944 involve noncitizen defendants. This exercise attests to the unique advantages of the Harris County setting for our purposes. Due to the already-small number of noncitizens, we include all charges in our analysis. We also acknowledge up front that our analyses of Bexar County data will be far noisier than our main results, although, as we will show, the findings are qualitatively similar.

We re-estimate Equation 1 using our Bexar County dataset. Results appear in Appendix Table A1. The first two columns present results using the sample of cases filed between 2009 and 2012, to echo our main analysis. Though imprecise, point estimate reveal that *Padilla* had the exact impact on noncitizen felony defendants in Bexar County as it did in Harris County: we report a decrease in guilty plea rates, an increase in case dismissal rates, an increase in time to disposition, and a decrease in the probability of incarceration. When we expand our sample to include all cases filed between 2005 and 2015 (the last year during which Bexar's longtime Republican DA served), we uncover comparable effects on all outcomes except time to disposition, for which point estimates attenuate drastically.

Taken together, these findings suggest that our results do not stem from coincident changes going on in Harris County around the time *Padilla* was handed down. Specifically, the fact that we can replicate our findings in Bexar suggests that the Harris County DA election—and subsequent changes in prosecutions—do not account for our results. Of course, we cannot rule out the possibility that statewide changes in Texas around this time explain our findings. Still, since we do not know of any such policy shifts that would have primarily impacted noncitizens, we discount this possibility.

6.2 Placebo Test: Effects Among Cases Without Immigration Consequences

Our preferred research sample contains only cases involving charges that carry potential immigration consequences, either CIMTs or aggravated felonies. We justified this approach by arguing that we should only find treatment effects among these cases, and not among cases that have no bearing on defendants' immigration standing. But, as we attempt to show that our results derive entirely from *Padilla* and not from other sources of variation, we revisit these no-risk cases and re-estimate our DiD specification on this subsample.

Regression results appear in Appendix Table A2. Encouragingly, we find null effects across all outcomes and specifications, consistent with our argument that our main analysis exclusively captures the effects of *Padilla*.

7 Conclusion

Plea bargaining inherently involves asymmetric information. Increasing popular attention on prosecutorial strong-arm negotiation tactics and biases suggests this asymmetry primarily impairs defendants. To help address the information disparity between prosecutors and defendants, courts and legislatures have pursued myriad reforms meant to level the playing field, equipping defendants with more details about the prosecutor's case and the consequences of pleading guilty. We provide causal evidence that these interventions can have unintended consequences.

For defendants most at risk of significant repercussions from conviction, we find that information is valuable. Knowing that a guilty plea entails relatively severe consequences provides them an incentive to negotiate a better offer or a dismissal; in effect, these defendants learn they have nothing to lose, and can wait out the prosecutor.

But for other defendants who face potential but not inevitable adverse consequences, we find the opposite effect. Better information about their complicated status seems to open opens the door for (possibly empty) prosecutorial threats. These defendants—our of confusion or risk aversion—become more likely to plead guilty, on less favorable terms than they otherwise would have. Thus, existing and proposed policies meant to better inform defendants and increase their bargaining power may, in some circumstances, actually do the opposite. We recommend future reform efforts focus on initiatives that discourage harsh prosecutorial tactics.

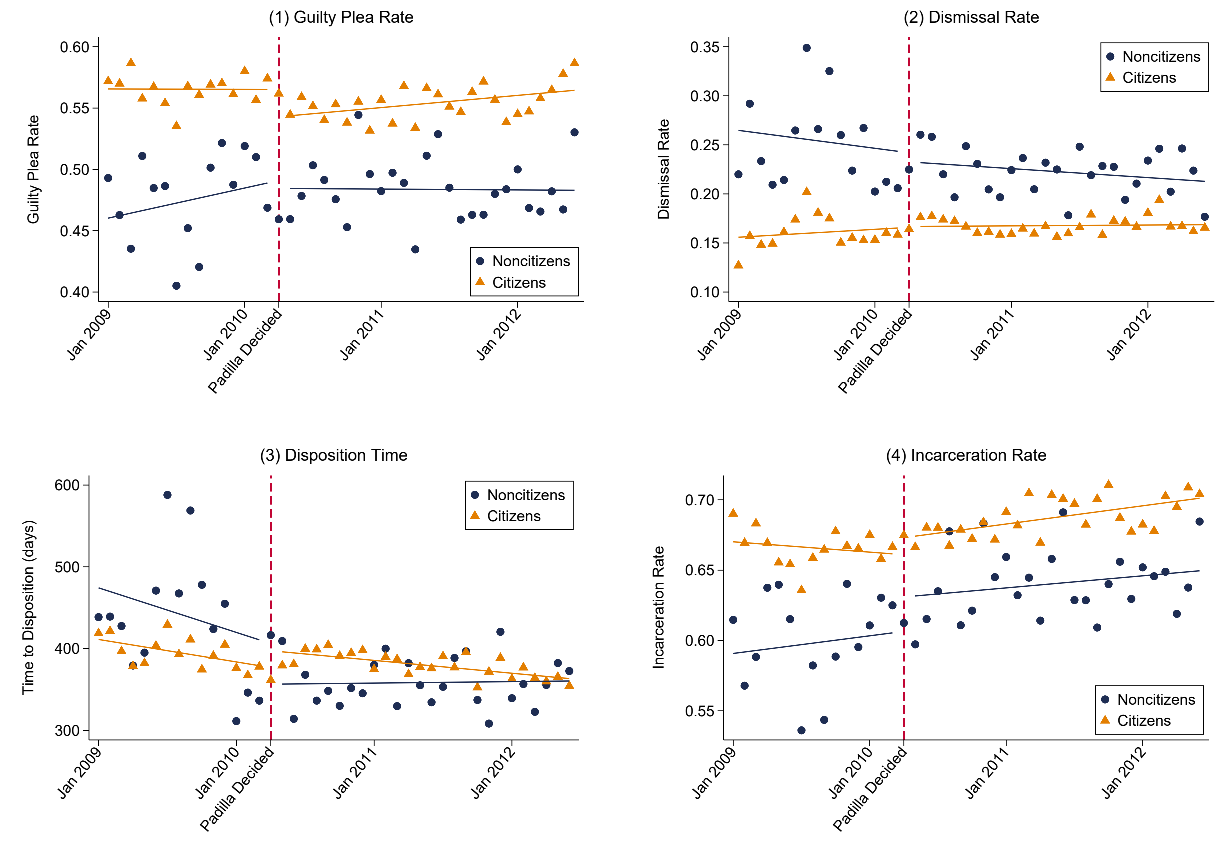

Figure 1: **Outcomes for All Cases:**
Each panel shows a nonparametric plot of the mean of the titular outcome variable, by defendant citizenship and month of case filing. Lines of best fit are estimated separately for cases filed before and after the *Padilla* ruling. The sample size is N=142,872 for all panels.

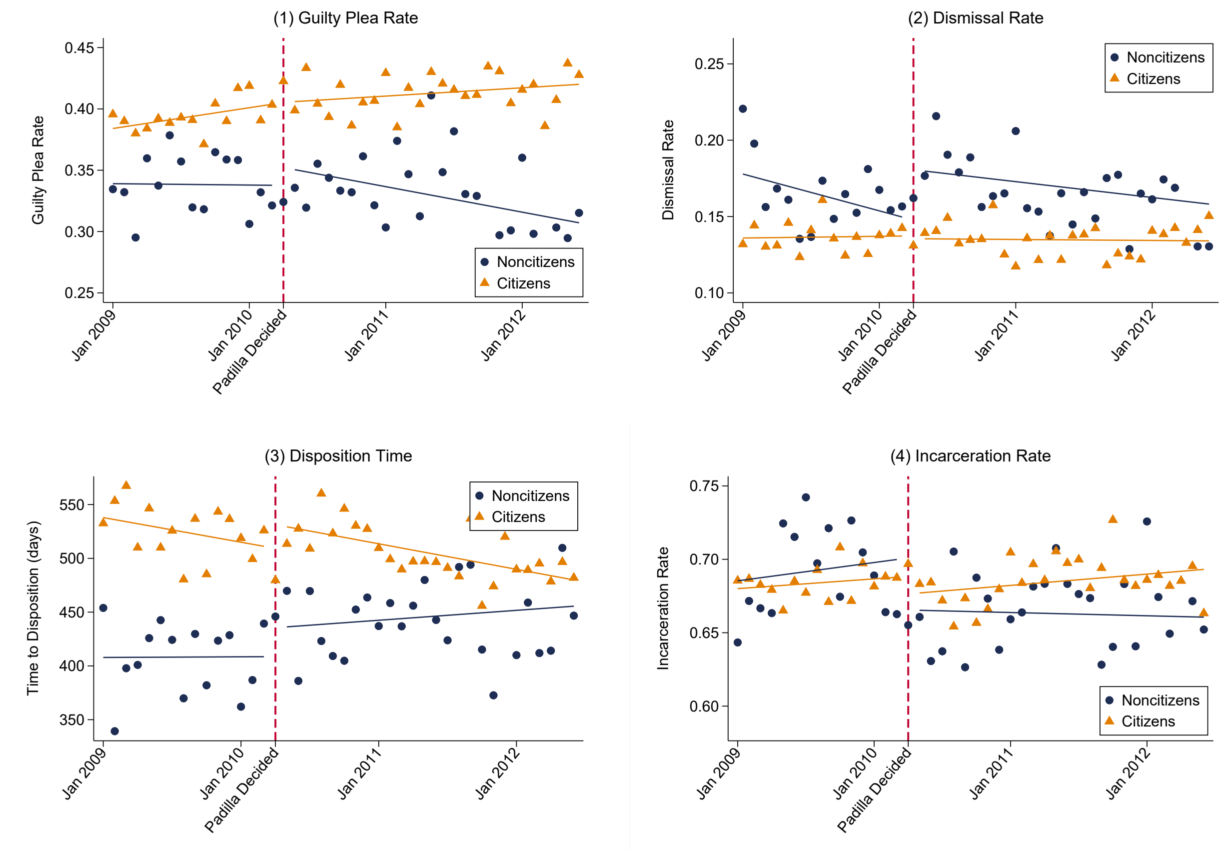

Figure 2: **Outcomes for High-Risk Felony Cases:**
Each panel shows a nonparametric plot of the mean of the titular outcome variable, by defendant citizenship and month of case filing. Lines of best fit are estimated separately for cases filed before and after the *Padilla* ruling. The sample size is N=60,960 for all panels.

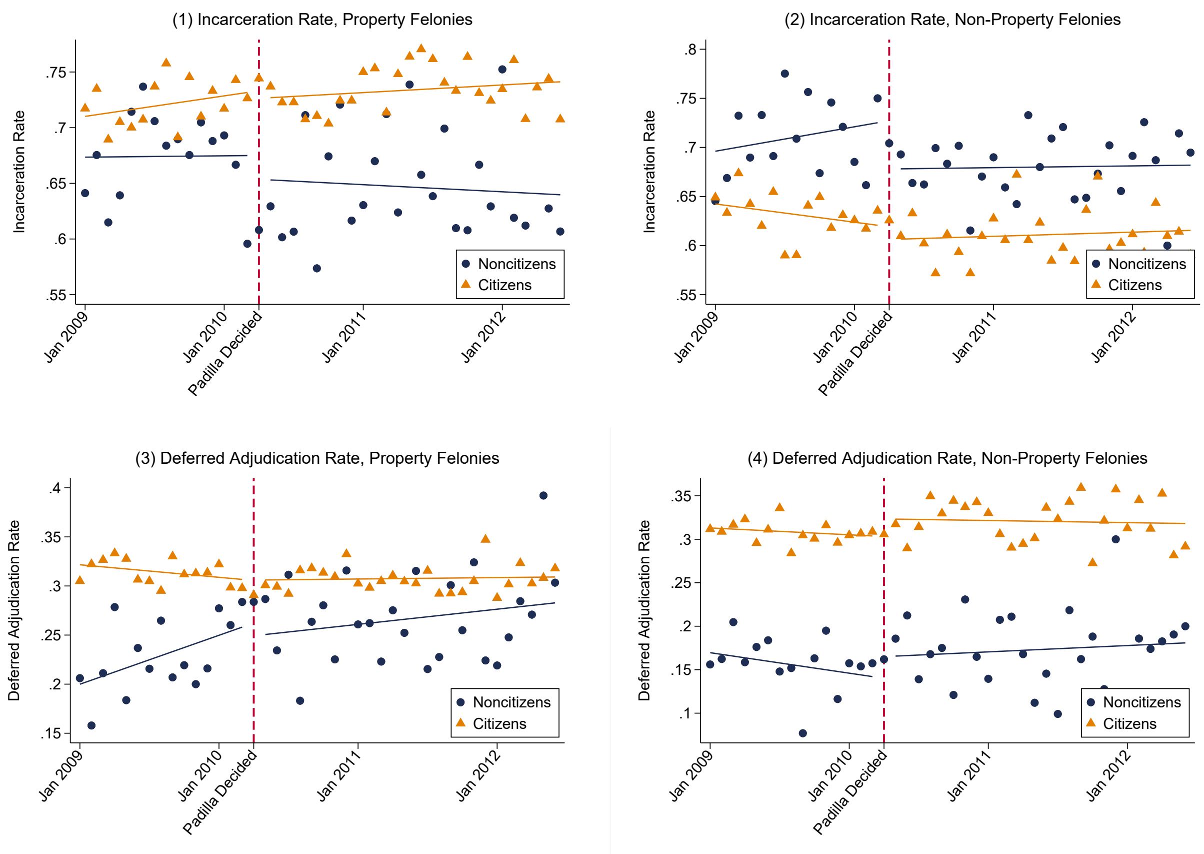

Figure 3: **Outcomes for Felony Property Cases:**
Each panel shows a nonparametric plot of the mean of the titular outcome variable and subsample, by defendant citizenship and month of case filing. Panels 1 and 2 display incarceration rates among property and all other (non-property) cases, respecitvely. Panels 3 and 4 show deferred adjudication rates among property and other (non-property) cases, respectively. Lines of best fit are estimated separately for cases filed before and after the *Padilla* ruling. The sample size is N=35,301 for all panels.

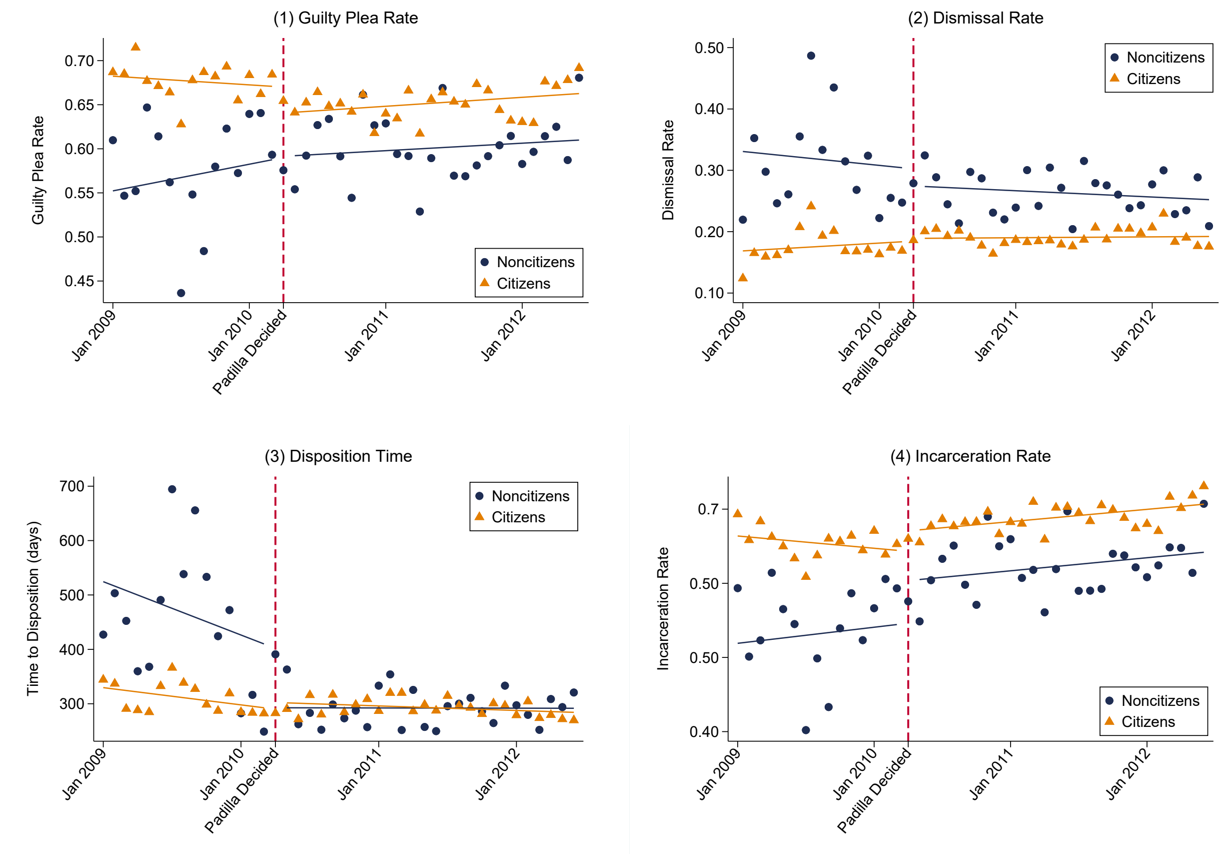

Figure 4: **Outcomes for Low-Risk Misdemeanor Defendants:**
Each panel shows a nonparametric plot of mean of the titular outcome variable, by defendant citizenship and month of case filing. Lines of best fit are estimated separately for cases filed before and after the *Padilla* ruling. The sample size is N=88,906 for all panels.

Table 1: Summary of Defendant, Case Characteristics by Citizenship

	All (1)	Noncitizens (2)	Citizens (3)
I. Defendant Characteristics			
White	0.259	0.212	0.268
	(0.438)	(0.409)	(0.443)
Black	0.441	0.216	0.485
	(0.497)	(0.412)	(0.500)
Hispanic	0.286	0.534	0.237
	(0.452)	(0.499)	(0.426)
Female	0.286	0.286	0.286
	(0.452)	(0.452)	(0.452)
Age	29.93	30.92	29.73
	(10.88)	(10.78)	(10.89)
# Prior Appearances	1.92	0.92	2.12
	(2.74)	(1.77)	(2.85)
II. Case Characteristics			
Felony	0.407	0.425	0.403
	(0.491)	(0.494)	(0.491)
Property	0.554	0.581	0.549
	(0.497)	(0.493)	(0.498)
Drug	0.376	0.283	0.394
	(0.484)	(0.450)	(0.489)
Violent	0.137	0.156	0.133
	(0.343)	(0.363)	(0.339)
Traffic	0.053	0.054	0.053
	(0.225)	(0.226)	(0.225)
N:	*149,872*	*24,583*	*125,289*

The table summarizes the defendant and case characteristics listed in the left-hand column. Column 1 shows the mean among all cases. Columns 2 and 3 report means among noncitizens and citizens, respectively. Standard deviations appear in parentheses.

Table 2: Summary of Outcomes by Citizenship, Pre- and Post-*Padilla*

		Noncitizens		Citizens	
	All (1)	Pre-*Padilla* (2)	Post-*Padilla* (3)	Pre-*Padilla* (4)	Post-*Padilla* (5)
Guilty Plea	0.546	0.474	0.483	0.565	0.554
	(0.498)	(0.499)	(0.500)	(0.496)	(0.497)
Case Dismissed	0.176	0.254	0.223	0.161	0.168
	(0.381)	(0.436)	(0.416)	(0.367)	(0.374)
Time to Disposition (days)	387	443	361	395	379
	(612)	(768)	(568)	(657)	(567)
Incarceration	0.670	0.598	0.639	0.666	0.687
	(0.470)	(0.490)	(0.480)	(0.472)	(0.464)
N:	*149,872*	*10,008*	*14,575*	*48.249*	*77,040*

The table summarizes the plea bargaining outcomes listed in the left-hand column. Column 1 shows the mean among all cases. Columns 2 and 3 report means among noncitizens whose cases were filed before and after the *Padilla* ruling, respectively. Columns 4 and 5 report means among citizens whose cases were filed before and after the *Padilla* ruling, respectively. Standard deviations appear in parentheses.

Table 3: Parallel Trends—Pre-*Padilla* Differences in Time Trends

	Outcome Variable			
	Guilty Plea (1)	Dismissal (2)	Time to Disp. (3)	Incarceration (4)
Month × Noncit	0.0021	-0.0001	2.499	0.0001
	(0.0015)	(0.0013)	(2.448)	(0.0014)
Month (1 - 14)	-0.0001	0.0011**	-1.748	-0.0007
	(0.0014)	(0.0005)	(1.324)	(0.0007)
Noncitizen	-0.1057***	0.0999***	42.985	-0.0727***
	(0.0105)	(0.0119)	(28.084)	(0.0144)
Constant	0.5661***	0.1539***	408.974***	0.6705***
	(0.0280)	(0.0057)	(21.453)	(0.0068)
N:	*50,931*	*50,931*	*50,931*	*50,931*

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Each column reports estimates from a regression, following Equation 2 without additional controls. The sample includes all cases filed between January 2009 and February 2010, which yields 14 unique months. Robust standard errors clustered at the courtroom-year level appear in parentheses.

Table 4: Baseline Balance—*Padilla* Effect on Defendant, Case Characteristics

	Pre-*Padilla* Mean (1)	Post × Noncit Effect (2)
I. Defendant Characteristics		
White	0.258	0.007
	(0.438)	(0.006)
Black	0.440	0.007
	(0.496)	(0.012)
Hispanic	0.290	-0.021
	(0.454)	(0.016)
Female	0.287	-0.012
	(0.452)	(0.013)
Age	29.8	-0.2
	(10.7)	(0.2)
# Prior Appearances	1.80	-0.03
	(2.59)	(0.06)
II. Case Characteristics		
Felony	0.397	0.005
	(0.489)	(0.009)
Property	0.561	-0.038*
	(0.496)	(0.019)
Drug	0.370	0.023*
	(0.482)	(0.013)
Violent	0.135	0.004
	(0.342)	(0.009)
Traffic	0.050	0.011***
	(0.218)	(0.003)
III. Predicted Outcomes		
Predicted Guilty Plea	0.542	-0.004
	(0.211)	(0.004)
Predicted Dismissal	0.1792	0.0003
	(0.0810)	(0.0011)
Predicted Disposition Time	387	-0.299
	(198)	(2.723)
Predicted Incarceration	0.664	-0.002
	(0.153)	(0.002)
N:	*58,257*	*149,872*

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Column 1 reports the mean of the variable in the left-hand column among all defendants prior to the *Padilla* ruling. Standard deviations appear in parentheses. Each cell in column 2 comes from a separate difference-in-differences regression. The specification is Equation 1 without additional controls. See the text for details on how we construct our predicted outcomes in panel III. Robust standard errors clustered at the courtroom-year level appear in parentheses.

Table 5: How Did *Padilla* Affect Noncitizen Cases? Full-sample Results

	Pre-*Padilla* Control Mean	Post-*Padilla* × Noncitizen Effects			
	(1)	(2)	(3)	(4)	(5)
Guilty Plea	0.565	0.020**	0.023**	0.023**	0.020***
	(0.496)	(0.008)	(0.009)	(0.009)	(0.007)
Case Dismissal	0.161	-0.039***	-0.039***	-0.039***	-0.034***
	(0.367)	(0.011)	(0.010)	(0.010)	(0.006)
Time to Disposition	395	-66***	-66***	-67***	-64***
	(656)	(23)	(23)	(23)	(13)
Incarceration	0.666	0.020	0.022*	0.023*	0.019***
	(0.472)	(0.012)	(0.013)	(0.013)	(0.006)
N:	*48,249*	*149,872*	*149,872*	*149,872*	*149,872*
	Defendant Controls	N	Y	Y	Y
	Case Controls	N	Y	Y	Y
	Courtroom-Year FEs	N	N	Y	Y
	Interacted Controls	N	N	N	Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Column 1 reports the mean of the outcome variable in the left-hand column among citizens prior to the *Padilla* ruling. Standard deviations appear in parentheses. Each of the remaining cells comes from a separate difference-in-differences regression. The specification is Equation 1. Defendant controls include indicators for whether a defendant is white, Black, or Hispanic, and whether the defendant is female; the defendant's age; and the number of prior appearances the defendant has had in the Harris County court system. Case controls include indicators for whether a case contains a felony offense, as well as indicators for whether the case includes property, drug, violent, and traffic offenses. Interacted controls interact all of these covariates with an indicator for whether the defendant is a noncitizen. Robust standard errors clustered at the courtroom-year level appear in parentheses.

Table 6: How Did *Padilla* Affect High-Risk Cases? Felony Results

	Pre-*Padilla* Control Mean	Post-*Padilla* × Noncitizen Effects			
	(1)	(2)	(3)	(4)	(5)
Guilty Plea	0.394	-0.028**	-0.020*	-0.022**	-0.013
	(0.489)	(0.011)	(0.010)	(0.010)	(0.010)
Case Dismissal	0.137	0.007	0.004	0.005	0.001
	(0.343)	(0.008)	(0.008)	(0.008)	(0.008)
Time to Disposition	525	58***	52***	50***	32**
	(696)	(15)	(14)	(15)	(14)
Incarceration	0.684	-0.031***	-0.022**	-0.023**	-0.010
	(0.465)	(0.010)	(0.010)	(0.010)	(0.009)
N:	*18,985*	*60,960*	*60,960*	*60,960*	*60,960*
Defendant Controls		N	Y	Y	Y
Case Controls		N	Y	Y	Y
Courtroom-Year FEs		N	N	Y	Y
Interacted Controls		N	N	N	Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The table reports summary statistics and difference-in-differences estimates for felony cases. Column 1 reports the mean of the outcome variable in the left-hand column among citizens prior to the *Padilla* ruling who are charged with misdemeanor offenses. Standard deviations appear in parentheses. Each of the remaining cells comes from a separate difference-in-differences regression. The specification is Equation 1. Defendant and case controls, as well as interacted controls, are the same as those described beneath Table 5. Robust standard errors clustered at the courtroom-year level appear in parentheses.

Table 7: How Did *Padilla* Affect Low-Risk Cases? Misdemeanor Results

	Pre-*Padilla* Control Mean	Post-*Padilla* × Noncitizen Effects			
	(1)	(2)	(3)	(4)	(5)
Guilty Plea	0.677	0.055***	0.049***	0.049***	0.038***
	(0.468)	(0.010)	(0.009)	(0.009)	(0.008)
Case Dismissal	0.176	-0.068***	-0.064***	-0.064***	-0.052***
	(0.381)	(0.009)	(0.008)	(0.008)	(0.007)
Time to Disposition	311	-153***	-146***	-145***	-132***
	(615)	(16)	(16)	(16)	(14)
Incarceration	0.654	0.056***	0.052***	0.052***	0.040***
	(0.476)	(0.009)	(0.009)	(0.008)	(0.008)
N:	*29,264*	*88,906*	*88,906*	*88,906*	*88,906*
	Defendant Controls	N	Y	Y	Y
	Case Controls	N	Y	Y	Y
	Courtroom-Year FEs	N	N	Y	Y
	Interacted Controls	N	N	N	Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The table reports summary statistics and difference-in-differences estimates for misdemeanor cases. Column 1 reports the mean of the outcome variable in the left-hand column among citizens prior to the *Padilla* ruling who are charged with misdemeanor offenses. Standard deviations appear in parentheses. Each of the remaining cells comes from a separate difference-in-differences regression. The specification is Equation 1. Defendant and case controls, as well as interacted controls, are the same as those described beneath Table 5. Robust standard errors clustered at the courtroom-year level appear in parentheses.

Table 8: How Did *Padilla* Affect Re-offenders? Triple-difference Results

	Outcome Variable			
	Guilty Plea (1)	Dismissal (2)	Time to Disp. (3)	Incarceration (4)
Noncit × Post	0.0565***	-0.0891***	-211***	0.0376***
	(0.0130)	(0.0103)	(21)	(0.0128)
Noncit × Post × Prior	-0.0478***	0.0819***	209***	-0.0192
	(0.0171)	(0.0156)	(28)	(0.0191)
N:	*88,906*	*88,906*	*88,906*	*88,906*

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The table reports triple-difference regression results, using a specification analagous to Equation 1, but including a "prior" conviction dummy and corresponding covariates. Each column comes from a separate regression. All specifications include the case and defendant controls described beneath Table 5, interacted separately with an indicator for defendant citizenship and an indicator for whether a defendant hasa prior conviction. Robust standard errors clustered at the courtroom-year level appear in parentheses.

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8 Appendix

Table A1: How Did *Padilla* Affect Felony Cases in Bexar County, TX?

	Pre-*Padilla* Control Mean	Post-*Padilla* × Noncitizen Effects			
	(1)	(2)	(3)	(4)	(5)
Guilty Plea	0.163	-0.030	-0.030	-0.025	-0.022
	(0.369)	(0.027)	(0.027)	(0.017)	(0.017)
Case Dismissal	0.427	0.059*	0.057	0.054**	0.049**
	(0.495)	(0.036)	(0.035)	(0.023)	(0.022)
Time to Disposition	315	93**	91**	23	19
	(686)	(47)	(46)	(30)	(30)
Incarceration	0.419	-0.073**	-0.070**	-0.071***	-0.064***
	(0.493)	(0.036)	(0.035)	(0.023)	(0.022)
N:	*9,149*	*28,453*	*28,453*	*69,359*	*69,359*
	Sample Period	2009-2012		2005-2015	
	Defendant Controls	N	Y	N	Y
	Case Controls	N	Y	N	Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The table reports summary statistics and regression analysis results using data from Bexar County. Column 1 reports the mean of the outcome variable in the left-hand column among citizens prior to the *Padilla* ruling offenses. Standard deviations appear in parentheses. Each of the remaining cells comes from a separate difference-in-differences regression. The specification is Equation 1. Defendant and case controls include indicators for whether a defendant is white, Black, Hispanic, and female, and whether a case includes property or violent charges. Standard errors appear in parentheses.

Table A2: Placebo Results: *Padilla*'s Effect on Cases Without Immigration Consequences

	Pre-*Padilla* Control Mean	Post-*Padilla* × Noncitizen Effects			
	(1)	(2)	(3)	(4)	(5)
I. All Cases					
Guilty Plea	0.607	-0.003	-0.001	0.001	0.007
	(0.488)	(0.006)	(0.006)	(0.006)	(0.006)
Case Dismissal	0.243	0.007	0.001	0.000	-0.003
	(0.429)	(0.006)	(0.006)	(0.006)	(0.006)
Time to Disposition	286	-6	-10	-9	-10
	(518)	(8)	(8)	(7)	(7)
Incarceration	0.665	-0.010	-0.005	-0.004	0.002
	(0.472)	(0.007)	(0.006)	(0.006)	(0.006)
N:	*37,609*	*139,146*	*139,146*	*139,146*	*139,146*
II. Felony Cases					
Guilty Plea	0.454	-0.015	-0.008	-0.010	0.012
	(0.498)	(0.014)	(0.013)	(0.013)	(0.013)
Case Dismissal	0.222	0.021	0.017	0.014	0.007
	(0.416)	(0.016)	(0.016)	(0.016)	(0.016)
Time to Disposition	455	12	8	1	-16
	(640)	(21)	(20)	(19)	(18)
Incarceration	0.641	-0.006	-0.002	0.000	0.012
	(0.498)	(0.018)	(0.017)	(0.017)	(0.017)
N:	*5,787*	*20,383*	*20,383*	*20,383*	*20,383*
III. Misdemeanor Cases					
Guilty Plea	0.645	-0.003	-0.001	-0.001	0.005
	(0.481)	(0.007)	(0.006)	(0.006)	(0.006)
Case Dismissal	0.246	0.005	-0.002	-0.003	-0.005
	(0.431)	(0.006)	(0.006)	(0.006)	(0.006)
Time to Disposition	255	-8	-11	-11	-10
	(486)	(7)	(8)	(8)	(8)
Incarceration	0.670	-0.012*	-0.005	-0.004	-0.001
	(0.470)	(0.007)	(0.007)	(0.007)	(0.007)
N:	*31,822*	*118,763*	*118,763*	*118,763*	*118,763*
	Defendant Controls	N	Y	Y	Y
	Case Controls	N	Y	Y	Y
	Courtroom-Year FEs	N	N	Y	Y
	Interacted Controls	N	N	N	Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel I includes the sample of all cases for which conviction would not have any immigration ramifications for noncitizens. Panel II includes the subset of these cases that involve felony charges. Panel III includes the subset that involve misdemeanor charges. Column 1 reports the mean of the outcome variable in the left-hand column among citizens prior to the *Padilla* ruling. Standard deviations appear in parentheses. Each of the remaining cells comes from a separate difference-in-differences regression. The specification is Equation 1. Defendant, case, and interacted controls are described below Table 5. Robust standard errors clustered at the courtroom-year level appear in parentheses.