

# Judges, Juveniles, and In-Group Bias

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## Abstract

We investigate the existence of in-group bias (preferential treatment of one's own group) in court decisions. Using the universe of juvenile-court cases in a US state between 1996 and 2012 and exploiting random assignment of juvenile defendants to judges, we find evidence for negative racial in-group bias in judicial decisions. All else being equal, black (white) juveniles who are randomly assigned to black (white) judges are more likely to be placed in custody, as opposed to being placed on probation, and they receive longer sentences. Although observed in experimental settings, this is the first empirical evidence of negative in-group bias based on a randomization design outside the lab. We provide explanations for this finding.

## 1. Introduction

This paper makes contributions to two areas of investigation: the analysis of differential treatment of minorities in the justice system and the investigation of how group membership (group identity) impacts individual behavior. Over the last few decades, a large body of research has emerged that investigates unequal treatment of minorities by law-enforcement authorities and in the judicial system. Prominent examples of this line of inquiry include research on whether racial and ethnic minorities are treated differently by the police (Grogger and Ridgeway 2006; Knowles, Persico, and Todd 2001) and the extent to which the probability of a guilty verdict and the severity of a sentence are related to the attributes of the defendants, judges, and jurors. The overarching question is whether the judicial process is unbiased (Alesina and La Ferrara 2014; Rehavi and Starr 2014; Abrams, Bertrand, and Mullainathan 2012; Argys and Mocan 2004).

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To guard against capricious and discriminatory behavior of the courts toward minorities, some scholars have suggested that racial and gender diversity in the courts could be a mechanism through which different voices and perspectives can be introduced into the judicial decision-making process (Ifill 2000). It has been argued that changing the racial and gender composition of the judicial branch may impact the substantive outcomes of cases pertaining to minority groups because minority judges bring their own perspectives to the bench. The implicit assumption of this argument is that the outcome of the case would be impacted if a judge shares identity attributes with the defendant or the plaintiff.

Yet judges are bound by institutional constraints, and they are expected to apply the law blindly, in an unbiased manner. Thus, whether in-group bias (preferential treatment of the members of one's own group) exists in judicial decision making is an important question with implications beyond the criminal justice system.<sup>1</sup>

Akerlof and Kranton (2000) highlights the importance of group identity in economic behavior. Laboratory experiments show that when subjects are matched with others who are labeled as members of an in-group, they are more likely to reward those in-group members for good behavior (Chen and Li 2009). Goette, Huffman, and Meier (2012) show that the in-group effect is stronger when in-group members have social ties (as opposed to just having been labeled as belonging to the same group), and the authors argue for the existence of heightened empathy toward group members.

On the other hand, evidence is less clear regarding punishment of in-group members. While some papers report experimental evidence that people impose weaker punishment on in-group members when those in-group members violate a social norm of cooperation (Mussweiler and Ockenfels 2013; Bernhard, Fischbacher, and Fehr 2006), other research finds that in-group members may be punished more severely when they defect (Mendoza, Lane, and Amodio 2014; Goette, Huffman, and Meier 2006). In a different vein, it has been reported that minority donors are less generous toward minority solicitors who seek to raise funds for charity (see List and Price [2009] and the literature they cite).

Thus, the outcome of in-group matching is unclear when members of the in-group visibly violate a social norm or if there is mistrust (founded or unfounded) toward them. Furthermore, differential treatment of in-group members may be driven by expectations about their future behavior (see Everett, Faber, and Crockett [2015] for a summary). Put differently, any reaction to an in-group member's actions (especially bad behavior) may represent opinions about that member's behavior in the future. This point is particularly relevant in analyses that seek to

<sup>1</sup> For example, the impact on test scores and other student outcomes of teacher-student matching by race or gender has received attention (Fairlie, Hoffman, and Oreopoulos 2014; Hoffman and Oreopoulos 2009; Dee 2005), although it is difficult to determine whether the effects are due to in-group bias or role modeling. Another example of in-group bias in a different domain is the investigation of the impact of matching caseworkers and unemployed workers by social class (Behncke, Frölich, and Lechner 2010).

explain the behavior of judges in reaction to the delinquent behavior of individuals who can be classified as members their group, defined by race or ethnicity.

As an investigation of the existence of in-group bias in the courts, Shayo and Zussman (2011) analyze data from Israeli small-claims courts where the assignment of a case to an Israeli or Arab judge is random. They find evidence that a claim was more likely to be accepted if assigned to a judge who was of the same ethnicity as the plaintiff and that this effect was associated with the intensity of terrorism in the area surrounding the court preceding the judicial decision. Similarly, Gazal-Ayal and Sulitzeanu-Kenan (2010) find in-group bias in Israeli Arab and Jewish judges' decisions on criminal cases. Grossman et al. (2016) report that while the outcomes of appeals for Jewish defendants were independent of the ethnic composition of the panel of judges in the appellate court, Arab defendants received more lenient punishment when the panel included at least one Arab judge.

The existence and the extent of in-group bias in courts, however, could be culture and context specific. The variation in the setting (for example, criminal versus civil cases, state versus federal courts) and the nature of the judicial decision (low stakes versus high stakes) could matter, along with the historical background in which the judicial system operates. This suggests that the in-group bias reported in a particular country (for example, Israel), with a given history of ethnic and civil conflict and minority disenfranchisement, may have little validity in another country, such as the United States.

Evidence is mixed regarding in-group bias in the US courts. Boyd, Epstein, and Martin (2010) find that a judge's gender makes a difference in the outcomes of sex-discrimination cases. Gruhl, Spohn, and Welsh (1981) analyze convicting and sentencing behavior of male and female judges in felony cases. While they could not find evidence for differences in convicting, they report that female judges imposed longer sentences on female defendants.<sup>2</sup> Schanzenbach (2005) finds that judicial background affects sentencing disparities but not in ways that can be easily reconciled with a particular theory. He shows that having a greater proportion of female judges on the bench was associated with a lower disparity between the sexes for the sentencing of serious crimes, which could be because of the paternalistic biases of male judges or female judges' unfavorable treatment of female defendants. Lim, Silveira, and Snyder (2016) analyze data from Texas district courts and exploit randomized case assignments within counties. They find some evidence of the impact of the race and gender of the judge, although the estimated magnitudes are small.<sup>3</sup> Much of the existing work, however, is based on small samples or makes inferences using data from one city or a few districts. Consequently, as Schanzenbach (2005, p. 59) finds, "[T]he literature on judging

<sup>2</sup> A related paper, Glynn and Sen (2015), analyzes whether having a daughter motivates a judge to cast votes more favorable to women on gender issues.

<sup>3</sup> Lim, Silveira, and Snyder (2016, pp. 25, 27) find that "African-American judges show some favoritism (relative to non-Hispanic white judges) for minority defendants" and that "[t]he results in [their] Table 5 also indicate that female judges assign longer sentences to female defendants in violent offense cases."

has not produced consistent findings regarding the impact of a judge's race on his or her decisions."<sup>4</sup>

In this paper we employ the universe of juvenile-court decisions in Louisiana between 1996 and 2012. Exploiting the fact that cases are randomly assigned to judges, and making use of the racial diversity of judges, we investigate the extent of racial in-group bias in juvenile courts. Our identification strategy relies on a stream of juvenile defenders who are apprehended in the same parish (county) for the same offense and are randomly assigned to a given judge in a particular jurisdiction. Variation in treatment severity between observationally identical black and white defendants who faced the same judge (who is black or white) allows us to determine the extent of in-group bias.<sup>5</sup>

We find evidence for negative racial in-group bias in sentencing. All else being equal, black (white) juveniles who are randomly assigned to black (white) judges are more likely to be placed in custody, as opposed to being placed on probation, and they receive longer sentences. Although this finding may be surprising initially, as described above, it is consistent with recent experimental evidence indicating that members of the in-group are treated more harshly when they violate a social norm. There are a number of reasons for unfavorable treatment of juveniles by judges who are members of the same racial group. First, given that there is substantial race matching between crime victims and their perpetrators, a juvenile offender is likely to have caused harm (physical or property) to someone of the same race.<sup>6</sup> If judges have an in-group bias regarding the well-being of members of their own race, they are expected to punish perpetrators who harm that in-group, who in this case is someone of that same group. We present suggestive evidence that this could be a mechanism in our particular case. Second, being a member of an in-group may provide useful inside information about the future behavior of another member of the group. For example, in India, having an in-group match (based on religion and caste) between a loan officer and the borrower is shown to increase the performance of the loan, presumably because the loan officer can evaluate better the soft information that can be extracted from an in-group member (Fisman, Paravisini, and Vig 2017). This argument, which is consistent with the model proposed in Cornell and Welch (1996), implies that judges can assess the subtle clues better and can extract soft information more effectively from a member of their in-group and that harsher treatment by judges targeted at members of their own race could be efficient. Third, judges could be overcorrecting. That is, it could be that judges impose stiffer sentences on defendants who are of the same race to avoid creating an impression of being lenient

<sup>4</sup> Anwar, Bayer, and Hjalmarsson (2012) analyze the impact of the racial composition of juries in Florida and find that all-white juries are more likely to convict black defendants.

<sup>5</sup> Random assignment of judges has been used recently as an identification strategy to determine the impact of sentence severity (based on judges' incarceration tendencies) on recidivism and high-school completion (Aizer and Doyle 2015; Di Tella and Schargrodsky 2013; Green and Winik 2010).

<sup>6</sup> For example, 56 percent of white victims had white offenders, and 62 percent of black victims had black offenders (data for 2012–13 from Bureau of Justice Statistics, National Crime Victimization Survey (NCVS) Series [<http://www.icpsr.umich.edu/icpsrweb/NACJD/series/951>]).

toward them. Of course, it cannot be determined whether judges assign longer than optimal sentences to same-race defendants or whether they assign shorter than optimal sentences to other-race defendants. Furthermore, as is the case in most studies of discriminatory behavior, we cannot determine the particular reason(s) for the emergence of this result. It is nevertheless important to highlight that this is the first paper, that we know of, to provide evidence on negative in-group bias outside an experimental setting, in naturally generated data.

Section 2 describes the institutional setting of the juvenile-court system. Section 3 describes the data used in the study and provides the details of the random assignment of judges. Section 4 presents the details of the empirical implementation, Section 5 discusses the results, and Section 6 concludes.

## 2. Institutional Setting

Intake, the first step in the juvenile justice process, is initiated through a formal complaint filed against a juvenile by a citizen, a parent, a school administrator, or the arresting police officer. After a formal complaint or an arrest, a report is submitted to the district attorney's (DA's) office for review. The DA proceeds by dismissing the case, referring it to Families in Need of Services (FINS), proceeding informally by referring it to a diversion program or entering into an informal adjustment agreement with the juvenile (La. Code Child. Ann. art. 839), or formally pursuing the case by filing a petition with the Clerk of the Court's office.<sup>7,8</sup>

Cases that are pursued by the DA, through the filing of a petition, are brought before a judge at a petition hearing. The judge at the petition hearing is randomly assigned to the case unless the youth has been found guilty in a previous episode in the juvenile justice system. In such instances, the judge who handled the juvenile's initial case is assigned to the new one. These cases are excluded from our analyses. That is, we focus on cases in which a juvenile interacts with a judge for the first time.

Under the provisions of the Louisiana juvenile justice system, in each court the clerk's office implements a random allotment system for all cases filed (La. Dist. Ct. R., chap. 14, app. 14.0A).<sup>9</sup> The judge who is randomly assigned to the case continues to steward all hearings (petition, adjudication, and disposition). Prior to the petition hearing, the court appoints an attorney to discuss the case and to

<sup>7</sup> Cases referred to Families in Need of Services (FINS) typically involve conduct that would not be a crime if committed by an adult. Common examples include truancy, breaking school rules, running away, and violation of a law for a child under the age of 10. The FINS program works with the troubled youth and his or her family to help rectify the issues underlying the complaint.

<sup>8</sup> Cases are assigned to diversion programs designed to discourage juveniles from committing additional offenses. Youths who successfully complete the diversion program have their charges dismissed. Like being assigned to a diversion program, entering into an informal adjustment agreement (IAA) is not considered adjudication. The agreement outlines the terms and conditions of the youth's supervision. A youth who violates the terms of an IAA is referred back to the district attorney (DA), and the case proceeds to an adjudication hearing. If the terms of the agreement are met by the youth, then the case may be closed (La. Code Child. Ann. art. 841).

<sup>9</sup> This is similar to the process in Cook County, Illinois; see Abrams, Bertrand, and Mullainathan (2012) for a similar discussion.

offer advice to the youth. The youth, however, may proceed without the assistance of a lawyer if the parents agree. At the petition hearing, the youth may admit to the offense, deny the offense, or enter a response of *nolo contendere* (La. Code Child. Ann. art. 856). If the youth admits to the offense or pleads no contest, the court may render an adjudication (delinquency verdict) at that time. If the youth denies the allegations in the petition, the court sets the matter for an adjudication hearing that will occur at a later date (La. Code Child. Ann. art. 856).

For juveniles who deny the allegations of the petition, the timing between the petition hearing and the adjudication hearing can vary. Depending on the alleged crime and whether the juvenile is being held in custody, the length between the hearings could be as long as 90 days, and the court may extend the period even further for “good cause” (La. Code Child. Ann. art. 877).<sup>10</sup>

At the adjudication hearing, the sequence of events follows standard procedures, in which both parties are able to present evidence and examine and cross-examine witnesses (La. Code Child. Ann. art. 878–81). The hearing is held before the court without a jury. Thus, the judge has the sole responsibility for declaring whether the evidence warrants an adjudication that the child is delinquent. Youths not found to be delinquent are released, and charges are dropped. In this situation, the case does not enter the Office of Juvenile Justice (OJJ) database. If the judge finds the youth to be delinquent, the judge then sets a disposition hearing for a later date.<sup>11</sup>

At the disposition hearing, the judge determines the type and the duration of the sentence to be rendered to the delinquent youth. Disposed youths are either placed in custody or released to the supervision of a parent or guardian under probation, with specific rules and regulations of the probation that must be met. In the most severe type of custody, the youth is assigned to the supervision of the Department of Public Safety and Corrections to be confined in secure placement. In a less severe custody arrangement, delinquent youths are removed from their home and placed in a nonsecure residential or treatment facility such as a group home.<sup>12</sup>

It is important to note that even those who are not placed in custody (that is, those who receive probation) are assigned a sentence length by the judge. In

<sup>10</sup> In preparation for the adjudication hearing, there is a discovery process that follows the order of the Louisiana Criminal Court. The youth may receive an exam by a physician, optometrist, audiologist, psychologist, or psychiatrist. This exam cannot occur until 5 days after the clerk of the court notifies all parties of the examination order. Findings of the examination are to be submitted within 30 days of the ordered (La. Code Child. Ann. art. 867). Before adjudication, the DA and defense attorney prepare for the case by gathering evidence, including statements from witnesses.

<sup>11</sup> About 80 percent of adjudicated cases result in a guilty plea or guilty verdict. The remaining 20 percent are dismissed, or defendants are found not guilty (for 2002, 2004, and 2005, see Louisiana Juvenile Justice and Delinquency Prevention Advisory Board 2008, 2010, 2011). The files of this latter group are purged from the system, and no information about these cases is available.

<sup>12</sup> One other noncustody option available to the judge is to enter into a deferred disposition agreement rather than follow a formal disposition. Under a deferred disposition agreement, the youth must comply with the conditions set by the court (that is, counseling, curfews, school attendance).

other words, once found guilty, each juvenile receives a sentence length regardless of sentence type (whether placed in custody or on probation).

The judge determines the disposition according to the guidelines set by the Louisiana children's code. The code requires that crimes of first-degree murder, second-degree murder, aggravated or first-degree rape, and aggravated kidnapping receive a disposition of secure custody until the age of 21 years without the benefit of parole, probation, suspension of imposition or execution of sentence, or modification of sentence (La. Code Child. Ann. art. 897.1).<sup>13</sup> We exclude these cases from the analysis. For other offenses, the children's code provides only general guidelines (see La. Code Child. Ann. art. 901).<sup>14</sup> Thus, the judge has discretion in both sentence type and length. In this paper we investigate the two decisions made by judges at the disposition stage: whether the juvenile should be placed in custody (as opposed to being placed on probation) and the length of the sentence.

### 3. Random Assignment and the Data

#### 3.1. *Random Assignment of Judges*

In Section 2 we highlighted that the defendants in the Louisiana juvenile justice system are randomly assigned to judges. One obstacle in our analysis, however, is that we observe only the juveniles who are delinquent. That is, the OJJ data contain records only of those who were determined by a judge to be guilty, those who admitted to the charges in the petition hearing, and those who pleaded no contest in the petition hearing. This is potentially problematic because the judge who was randomly assigned to the case is the same judge who presided over all hearings. Put differently, the judge who makes the determination of guilt or innocence also determines the length of the sentence and whether the juvenile is placed in custody. Thus, our sample is likely contaminated with nonrandom selection of juvenile defendants to the extent that judges' decisions at the adjudication hearings are correlated with their decisions at disposition. For example, if judges discriminate against a certain group of juveniles (for example, minorities) during the adjudication process by being more or less lenient toward that group, then racial differences in sentence type and sentence length may be driven by differences in the types of juveniles who are filtered out during the adjudication process.

To circumvent this issue, we focus on juveniles who pleaded guilty or no contest to the charges filed against them. These individuals bypassed the adjudication hearing without being exposed to a judge at that stage. In these cases,

<sup>13</sup> Similarly, cases of armed robbery committed by youths who are 14 and older at the commission of the offense are also required to receive a disposition of secure custody without the benefit of parole, probation, suspension of imposition or execution of sentence, or modification of sentence.

<sup>14</sup> In general, the code suggests that "the court shall not remove a child from the custody of his parents unless his welfare or the safety and protection of the public cannot, in the opinion of the court, be adequately safeguarded without such removal." The code also holds that "the court should impose the least restrictive disposition . . . which the court finds is consistent with the circumstances of the case, the needs of the child, and the best interest of society" (La. Code Child. Ann. art. 901).

the sole responsibility of the judge is to determine the disposition outcome for the juvenile by setting the disposition type (custody or probation) and length. In the data we cannot distinguish between the juveniles who were adjudicated delinquent through a hearing, those who pleaded guilty, and those who did not contest the charges. Consequently, in an effort to ensure that we focus only on defendants who were not seen by a judge before the disposition hearing (who were not found to be delinquent by a judge), we consider only those individuals for whom the date of the petition hearing and the date of adjudication are the same. As described earlier, juveniles who enter a plea of not guilty in the petition hearing have a separate adjudication hearing at a later date. Thus, if the petition and adjudication dates are the same, this implies that a judge has not made a decision regarding guilt; instead, the juvenile has pleaded guilty or no contest to the charge filed. Put differently, the defendants who admitted to or did not contest the charges received an adjudication of delinquency without an adjudication hearing. These defendants went straight to the disposition stage, in which a judge decided on the type and the length of disposition. This is the core sample of juveniles that we analyze.

### 3.2. Data

The data for this study come from the Louisiana Department of Public Safety and Corrections's Youth Services OJJ and include the universe of case records from 1996 to 2012 in which juveniles were found to be delinquent.<sup>15</sup> Therefore, the data set does not contain information about individuals who were not found to be delinquent. Nor does it contain information about formal complaints and arrests that were not pursued through a petition by the DA (that is, juveniles in diversion programs or FINS and dropped cases).

For each observed record of a case, we have information about both the juvenile defendant and the case. The data on the defendant include race, gender, and age; parish (county) of residence; parish of offense; exact statute offense committed; and a unique individual identifier. The data on the case include the dates of the petition, adjudication, and disposition hearings; the judge's disposition type (custody versus probation) and the disposition length; the court in which the hearings were held; and the name of the judge. By using the names of the judges, we can gather information about them, including race.

We consider two outcomes of interest: the type of disposition rendered to the youth (custody confinement or probation) and the disposition length. To circumvent any potential confounding that may arise from multiple offenses and/or the criminal history of the juvenile, we limit our attention to first-time delinquents ages 10–17 who are black or white and who were convicted for only one

<sup>15</sup> Findings of delinquency were adjudicated through a formal adjudication hearing or admission to or a response of no contest to the allegations in the petition.



Table 1  
Summary Statistics for Delinquent Juveniles

	All ( <i>N</i> = 12,800)		Same Petition and Adjudication Dates ( <i>N</i> = 5,142)		Different Petition and Adjudication Dates ( <i>N</i> = 7,658)	
	Black (1)	White (2)	Black (3)	White (4)	Black (5)	White (6)
Disposition length (days)	605.48 (441.43)	578.81 (390.46)	652.08 (460.12)	561.53 (354.62)	578.51 (427.97)	601.64 (432.43)
Custody	.34 (.47)	.30 (.46)	.23 (.42)	.21 (.41)	.40 (.49)	.42 (.49)
Female juvenile	.23 (.42)	.24 (.43)	.25 (.43)	.25 (.43)	.22 (.42)	.23 (.42)
Juvenile's age (years)	15.22 (1.45)	15.41 (1.40)	15.16 (1.52)	15.41 (1.39)	15.26 (1.41)	15.40 (1.40)
Felony offense	.44 (.50)	.46 (.50)	.41 (.49)	.42 (.49)	.47 (.50)	.50 (.50)
<i>N</i>	10,583	2,217	3,880	1,262	6,703	955

**Source.** Data are from the Louisiana Department of Public Safety and Corrections' Youth Services Office of Juvenile Justice, 1996–2012.

**Note.** Standard deviations are in parentheses. Custody equals one if a juvenile receives a disposition that requires his or her initial placement to be in a secure custody or nonsecure custody facility and zero if he or she is placed on probation.

statute offense.<sup>16</sup> We drop cases in which the offense is first- or second-degree murder, aggravated or first-degree rape, aggravated kidnapping, or armed robbery since the judge is not supposed to deviate from the sentencing guidelines.<sup>17</sup> Because the central element of the analysis is to exploit the random assignment of the race of the judge in a court, we drop courts that do not have at least one black judge and one white judge. Having imposed these sample restrictions, we end up with a sample of 12,800 case records (juveniles) from a total of 105 judges.<sup>18</sup>

Table 1 presents descriptive statistics for adjudicated juveniles in the data. As described in Section 3.1, the subsample of juveniles who have the same petition and adjudication dates is plausibly free of sample-selection issues. The group of juveniles who have different petition and adjudication dates is a selected sample, because its members have been in front of the same judge sequentially. Therefore, for this group of defendants the adjudication decision of the judge may be impacted by the first-stage decision (decision on the petition) made by the same judge. The weighted average of the sample of all juveniles suggests that around

<sup>16</sup>The vast majority of cases consist of charges of only one statute offense. In the data 90.4 percent of cases consist of one offense, 9.5 percent of cases consist of two offenses, and .1 percent of cases consist of three or more offenses. It is worth noting that for children accused of committing a delinquent act, the juvenile justice system in Louisiana has jurisdiction over cases of youths ages 10–17. However, if the juvenile is 17 at the time of the offense, the case is moved to adult court (La. Code Child. Ann. arts. 801, 804).

<sup>17</sup>This amounts to only 243 cases being dropped.

<sup>18</sup>The main results, described in Section 5, are similar to those obtained from the sample that includes courts with no variation in the race of the judge.

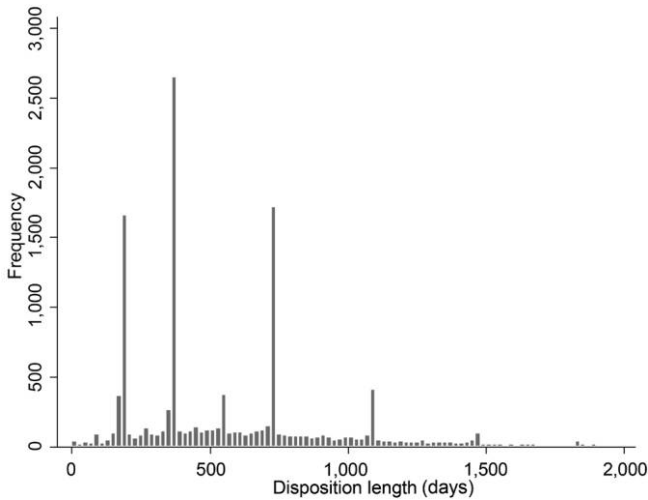


Figure 1. Distribution of disposition length

34 percent of the juveniles are placed in custody (that is, placed in secure or non-secure custody).<sup>19</sup> The sample contains approximately three times more males than females and almost five times as many black youth as white youth. The average age for first-time delinquency is around 15 years, and crimes that are considered felonies account for approximately 44 percent of all offenses.

Figure 1 displays the distribution of the disposition lengths rendered by judges. There is considerable bunching around half-year thresholds. Fifty percent of the disposition lengths are within 2 days of a half-year multiple ( $\frac{1}{2}$  year, 1 year,  $1\frac{1}{2}$  years, and so on). The spikes in disposition length are driven by judges commonly choosing disposition lengths at half-year multiples and not from mandatory sentencing guidelines. It should be noted, once again, that there is no mandatory sentencing, and judges exercise considerable discretion in sentencing. Figure 2 shows that for the four most frequent juvenile offenses in the data (possession, manufacturing, and distribution of drugs; ungovernability; theft of goods; and simple burglary), we observe a similar bunching at half-year intervals. However, within each offense there is significant variation in disposition length. For example, the average disposition length for possessing, manufacturing, and distributing drugs is 606 days, with a standard deviation of 423 days.

Of the 12,800 cases in the data (the sample of all juveniles in Table 1), 5,142 have the same petition and adjudication dates. It is interesting to note that although we do not observe substantial differences across disposition lengths and custody rates between juveniles with the same petition and adjudication dates and

<sup>19</sup> This is higher than the national average incarceration rate (for example, 25 percent in 2011) among all adjudicated cases (Hockenberry and Puzanhera 2014) and the average incarceration rate (23 percent) reported in Aizer and Doyle (2015) in Cook County, Illinois, from 1991 to 2006.

those with different dates, the felony offense rate is higher for the latter group. This is consistent with the supposition that youths facing more serious allegations are more likely to go to trial (adjudication hearing), while those charged with less serious crimes are more likely to plead guilty or no contest at a petition hearing.

Of the 105 judges who are in the effective sample, 76 percent are white (80 of 105 judges), and the rest are black. Thirty percent (31 of 105) are female. In the analysis we rely on judge fixed effects to control for judge-specific time-invariant unobservables that may impact judges' sentencing tendencies.

#### 4. Empirical Implementation

##### 4.1. Randomness of Assignment to Judges

To formally investigate the extent to which juvenile defendants are in fact randomly assigned to judges, we test whether characteristics of the juvenile or the case are correlated with the race of the assigned judge. We estimate a regression of an indicator for black judge on juvenile and case characteristics, controlling for variables on which the randomization was conditioned (for example, court-by-year fixed effects). We estimate

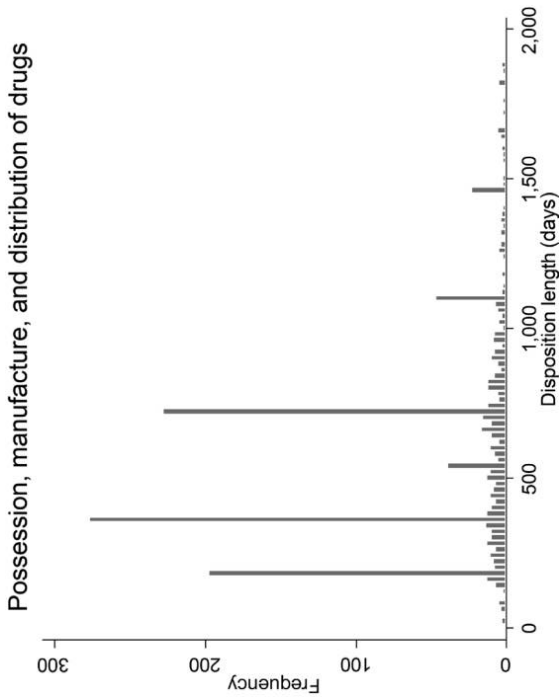
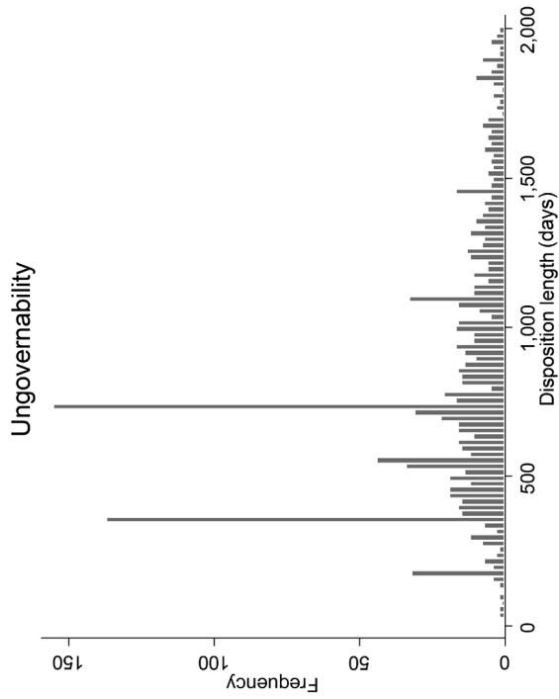
$$BJ_{jc} = \pi_0 + \pi_1 x_i + \lambda_c + \varepsilon_{jic}, \quad (1)$$

where  $BJ_{jc}$  is an indicator that takes a value of one if judge  $j$  in court-by-year cell  $c$  who is faced by juvenile defendant  $i$  is black and zero otherwise. The term  $x_i$  is an indicator representing a particular attribute of the juvenile or the case. Four specifications are estimated, where each specification includes a different attribute (an indicator for black defendant, female defendant, above median age [that is, ages 15–17], and felony offense). The term  $\lambda_c$  is the set of court-by-year fixed effects, and  $\varepsilon_{jic}$  is an unobserved component. Standard errors of these regressions are clustered by courtroom because randomization takes place at the courtroom level.

Table 2 displays the results from the randomization tests using data from all delinquent youth and for delinquent youth with the same and with different petition and adjudication dates. Each point estimate and the corresponding standard error pertains to a separate ordinary least squares regression. Columns 1, 4, and 7 include only court-by-year fixed effects. To further test the sensitivity of randomization tests, we add fixed effects for the parish where the offense was committed and the parish of residence of the juvenile and fixed effects for the offense.<sup>20</sup>

For the sample of juveniles who have the same petition and adjudication dates, the coefficient estimates in Table 2 are all very small in magnitude, and none is statistically different from 0. Adding parish-of-residence fixed effects, parish-of-offense-committed fixed effects, and offense fixed effects does not significantly alter the estimates. It is worth noting that the sample of juveniles who have different petition and adjudication dates does not produce consistently insignificant

<sup>20</sup> We observe 165 different types of offenses in the data.



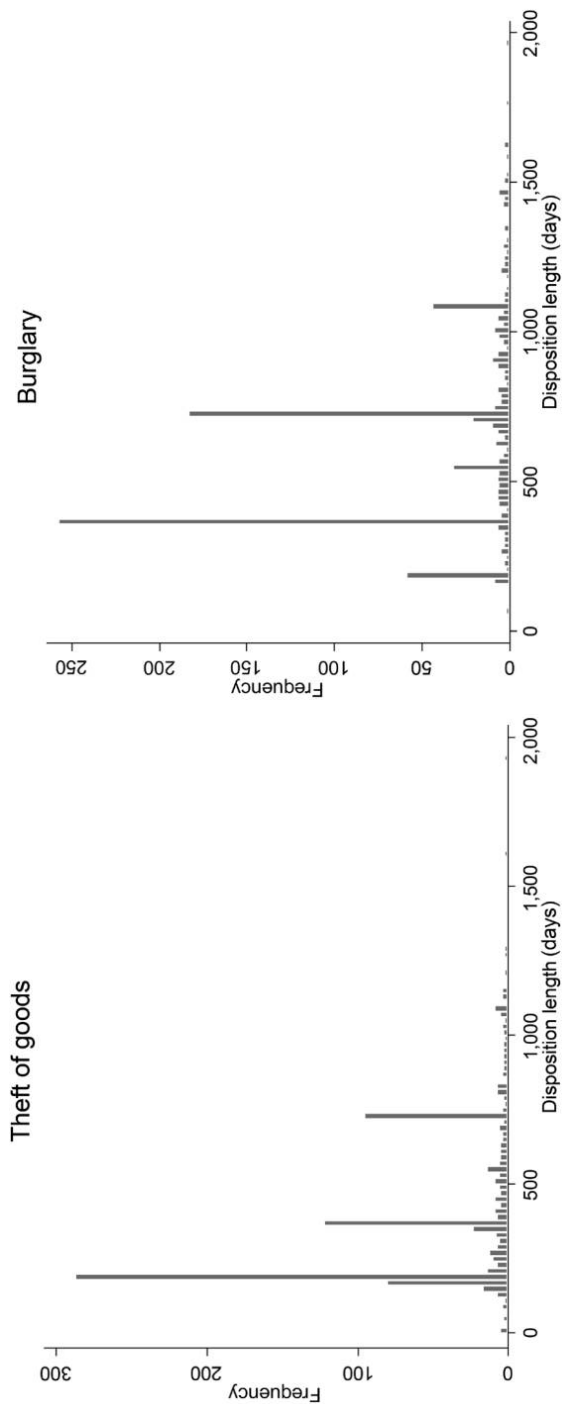


Figure 2. Distribution of disposition lengths by offense type

Table 2  
The Effect of Juveniles' Characteristics on Black Judges' Assignment

	All Juveniles (N = 12,800)			Same Petition and Adjudication Dates (N = 5,142)		Different Petition and Adjudication Dates (N = 7,658)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black juvenile	.010 (.021)	.013 (.020)	.015 (.017)	.004 (.029)	.017 (.024)	.009 (.022)	.017 (.026)	.014 (.023)	.023 (.023)
Female juvenile	.016 <sup>+</sup> (.008)	.017 <sup>+</sup> (.009)	.011 (.011)	-.003 (.006)	-.001 (.007)	-.007 (.008)	.030* (.014)	.031* (.014)	.021 (.017)
Age 15 and older	.007 (.006)	.005 (.006)	.009 (.007)	.007 (.005)	.003 (.005)	.002 (.007)	.006 (.010)	.005 (.011)	.008 (.013)
Felony offense	-.014 (.014)	-.015 (.014)		.002 (.015)	-.001 (.016)		-.029 <sup>+</sup> (.015)	-.028 <sup>+</sup> (.016)	
Parish-of-offense fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Parish-of-residence fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Offense fixed effects	No	No	Yes	No	No	Yes	No	No	Yes

Note. All regressions include court-year fixed effects. Offense fixed effects represent 165 offense categories. Standard errors clustered at the court level are in parentheses.

<sup>+</sup>  $p < .10$ .

\*  $p < .05$ .

point estimates. Rather, both gender and felony offenses are statistically significant indicators of the judge's race, which suggests that the sample of juveniles with different petition and adjudication dates may in fact be affected by judges' decisions in the adjudication hearings. (Recall that this is the sample of juveniles who faced the same judge previously.)

Overall, the results in Table 2 provide additional assurance that the randomization of judges is carried out in practice. The table also indicates that focusing on youths who have the same petition and adjudication dates circumvents biases in the adjudication hearing that may be carried over from the petition hearing.

#### 4.2. Racial In-Group Bias

As a preliminary analysis, we applied the rank-based test of Anwar and Fang (2006). The test was originally used to distinguish between statistical and taste-based discrimination in motor-vehicle searches. The concept, when applied to our framework, involves the comparison of average outcomes (the probability of being placed in custody or the sentence length) of black and white judges across black and white juveniles. Let  $S(b, B)$  stand for average sentence length assigned to black juveniles ( $b$ ) by black judges ( $B$ ), and let  $S(b, W)$  represent the average sentence length assigned to black juveniles ( $b$ ) by white judges ( $W$ ). Similarly,  $S(w, B)$  and  $S(w, W)$  represent the average sentence given to white juveniles by black judges and the average sentence given to white juveniles by white judges, respectively. If  $S(b, B) < S(b, W)$ , this indicates that white judges assign longer sentences to black juveniles in comparison to black judges. If it is also true that  $S(w, B) < S(w, W)$ , this implies that white judges also assign longer sentences to white juveniles. Thus, the preservation of the ranking of average sentence length by the race of the judge may support the idea of no racial discrimination by white judges, and it also provides support for the relative toughness of white judges in comparison to black judges. On the other hand, if it is true that  $S(b, B) < S(b, W)$  and  $S(w, B) > S(w, W)$ , this indicates that either black or white judges are racially biased.

We consider observations from our main sample of interest: juveniles who have the same petition and adjudication dates in Table 1. In the test of Anwar and Fang (2006), it is assumed that white and minority troopers face the same population of white and minority motorists. The authors resample the data to ensure that white and minority officers in the sample are assigned to different trooper stations with equal probability (Anwar and Fang 2006, p. 140). Similarly, in our case systematic differences might emerge because black and white judges could face different populations of black and white juveniles depending on the court on which they serve. We eliminate such systematic differences by regressing log sentence length and the indicator for being placed in custody on court-by-year fixed effects using all 5,142 observations that form the base of the analyses. The mean residuals from these regressions, grouped by the race of the juvenile and the race of judge, are presented in Table 3. Table 3 shows that sentences assigned

Table 3  
Average Custody Rates and Sentence Lengths

	Black Judge (1)	White Judge (2)	p-Value (3)
Sentence length:			
Black juvenile	.079 (.015)	-.085 (.016)	<.001
White juvenile	-.089 (.032)	.011 (.016)	.003
Custody:			
Black juvenile	.011 (.008)	.015 (.009)	.749
White juvenile	-.066 (.020)	-.031 (.012)	.141

Note. Standard errors of the means are in parentheses.

by black judges are longer than those assigned by white judges when the defendants are black (.079 > -.085), and the difference is statistically different from 0 ( $p$ -value < .001). But the reverse is true when the defendants are white; that is, white judges assign longer sentences in comparison to black judges when the defendants are white. This reversal supports the conjecture of racial prejudice of judges. We do not detect such evidence regarding the probability of custody, also displayed in Table 3.

To more formally isolate any potential racial disparity in disposition outcomes by the race of judges, we estimate the following regression model:

$$Y_{ijc} = \alpha_0 + \alpha_1(BJ_{ijc} \times BY_{ijc}) + \alpha_2 BY_{ijc} + \lambda_c + \theta_j + \mu_{ijc}, \tag{2}$$

where  $Y_{ijc}$  is the outcome of interest. We analyze two outcomes for juvenile  $i$  facing judge  $j$  in court-by-year cell  $c$ : disposition type (an indicator for custody) and disposition length (log of the sentence length). The term  $BJ_{ijc}$  is an indicator that takes a value of one if judge  $j$  who was assigned to youth  $i$  in court-by-year cell  $c$  is black. Similarly,  $BY_{ijc}$  takes a value of one if youth  $i$  is black. The term  $\lambda_c$  is the set of court-by-year fixed effects,  $\theta_j$  stands for judge fixed effects, and  $\mu_{ijc}$  is an unobserved error term.

Under random assignment, the coefficient estimate of  $\alpha_1$  provides an unbiased estimate of the effect of same-race assignment (of the judge and juvenile) on the outcome of interest. For example, in the analysis of disposition length,  $\alpha_1$  reflects the average change in the disposition length for black juveniles, relative to white juveniles, that stems from assignment to a black judge as opposed to a white judge. Conversely,  $\alpha_1$  is the average change in the outcome for white juveniles, relative to black juveniles, from having been assigned to a white judge. Thus, this particular identification strategy does not allow us to isolate whether the estimated effect is driven by white judges, black judges, white juveniles, or black juveniles.



To emphasize this point, consider the following:

$$E(Y|BJ = 0, BY = 0) = \alpha_0 + \lambda_c + \tilde{\theta}_{j|BJ=0}, \quad (3)$$

$$E(Y|BJ = 1, BY = 0) = \alpha_0 + \lambda_c + \tilde{\theta}_{j|BJ=1}, \quad (4)$$

$$E(Y|BJ = 0, BY = 1) = \alpha_0 + \alpha_2 + \lambda_c + \tilde{\theta}_{j|BJ=0}, \quad (5)$$

and

$$E(Y|BJ = 1, BY = 1) = \alpha_0 + \alpha_1 + \alpha_2 + \lambda_c + \tilde{\theta}_{j|BJ=1}. \quad (6)$$

Equation (3) displays the expected value of the disposition outcome when both the judge and the juvenile defendant are white. Equation (4) shows the expected value of the outcome when the judge is black but the defendant is white. The term  $\tilde{\theta}_{j|BJ=0}$  in equation (3) is the weighted average judge fixed effect on the outcome  $Y$  from the set of white judges, and  $\tilde{\theta}_{j|BJ=1}$  in equation (4) is the weighted average of judge fixed effect on  $Y$  from the set of black judges, conditional on court-by-year fixed effects.

In this framework, the expected difference between being assigned to a black judge instead of a white judge for a white juvenile (equation [4] – equation [3]) is equal to  $(\tilde{\theta}_{j|BJ=1} - \tilde{\theta}_{j|BJ=0})$ , and the expected difference between being assigned to a black judge instead of a white judge for a black juvenile (equation [6] – equation [5]) is equal to  $(\alpha_1 + \tilde{\theta}_{j|BJ=1} - \tilde{\theta}_{j|BJ=0})$ . Therefore, the expected difference in the differences (equation [6] – equation [5]) – (equation [4] – equation [3]) is  $\alpha_1$ . The important takeaway is that the expected value of  $\alpha_1$ , which can be written as equation (3) + equation (6) – (equation [4] + equation [5]), is

$$\begin{aligned} & E(Y|BJ = 0, BY = 0) + E(Y|BJ = 1, BY = 1) \\ & - [E(Y|BJ = 1, BY = 0) + E(Y|BJ = 0, BY = 1)]. \end{aligned} \quad (7)$$

Hence, the estimate of  $\alpha_1$  is the effect of a youth being disposed by a judge of the same race (averaged between white and black juvenile-judge pairs) relative to a youth being disposed by a judge of a different race (averaged between non-matched-race juvenile-judge pairs). The particular specification we employ to identify the in-group effect is that used in Hoffman and Oreopoulos (2009).

It is worth clarifying that black and white juveniles are pooled together to estimate causal effects. We cannot control for judge fixed effects if the analysis is based on samples that are partitioned by the race of the juvenile because a judge fixed effect would remove the variation from the regressor of interest: the race of the judge. Even though judges are randomly assigned to cases, the inclusion of controls for the race of the judge in a pooled analysis, or more generally judge fixed effects, is important. To see this point more clearly, consider two races, A and B, and two types of judges, lenient (L) and strict (S). Let L- and S-type judges

Table 4  
The Impact of Race Matching on the Propensity  
to Plead Not Guilty

	(1)	(2)	(3)	(4)
Same Race	.0035 (.0221)	.0069 (.0224)	.0070 (.0224)	.0106 (.0221)
Parish fixed effects	No	Yes	Yes	Yes
Juvenile controls	No	Yes	Yes	Yes
Offense fixed effects	No	No	No	Yes

**Note.** The dependent variable equals one if the defendant pleaded not guilty and thus has different petition and adjudication dates. Parish fixed effects are for the parishes of residence and offense. Juvenile controls include a dummy for gender and linear and quadratic terms of age. All regressions include judge and court-year fixed effects. Offense fixed effects represent 165 offense categories. Standard errors clustered at the judge level are in parentheses.  $N = 12,800$ .

have different sentencing behaviors. Now suppose that race A judges are more likely to be L-type judges and race B judges are more likely to be S-type judges. An analysis of the impact of a judge of race A on the disposition outcome for a sample of juveniles that consists only of type A will endogenously capture the effects from both race and type, differences in L and S. Thus, an analysis that uses partitioned samples does not allow us to fully disentangle race effects from type effects. This may occur, for example, if courts in jurisdictions where whites are a majority may attract strict white judges but only lenient black judges. Similarly, white judges in predominantly black areas may be of the lenient type. Other potential nonrandom selection of judges by race to courts would cause similar issues. Hence, the inclusion of judge fixed effects allows us to adjust for such potential type effects.

Although judges are assigned randomly to defendants, it could be that the defendants (or their lawyers) might decide on their plea strategy on the basis of the race of the judge. For example, if a black (white) defendant realizes that he will face a black (white) judge, he might decide to plead not guilty, hoping for lenient treatment from the same-race judge. Because we can infer the defendants who pleaded not guilty (those who have different dates for their petition and adjudication hearings), we can test if the decision to plead not guilty was impacted by race matching between the defendant and the judge. We estimated the specification in equation (2) by employing as the dependent variable a dummy indicating the decision not to plead guilty (having different dates for the petition and adjudication hearings). The estimated coefficient of same-race assignment ( $\alpha_1$ ) is displayed in Table 4 for various specifications. The results reveal that race matching between the judge and the defendant has no impact on the propensity of a guilty plea. In all specifications, the estimated coefficient of the same-race indicator is close to 0,

with standard errors that are two to six times larger than the coefficients.<sup>21</sup> This result provides evidence that the propensity of a defendant to plead not guilty is not impacted by the match of his or her race to the race of the judge.

In addition, strategic self-selection of juvenile defendants based on the race of the judge is unlikely given the specifics of Louisiana's juvenile justice system. As detailed in Celeste and Puritz (2001), the majority of juveniles do not consult with attorneys about their cases, and most public defenders do not meet with their clients prior to court. Furthermore, there is evidence of a high incidence of waiver of counsel (Celeste and Puritz 2001, p. 59), large caseloads for public defenders (Celeste and Puritz 2001, p. 62), and poor counseling in general (Celeste and Puritz 2001, p. 65). Thus, the lack of appropriate counseling makes it unlikely that defendants would consider the identity of the judges in their defense strategy.

## 5. Results

### 5.1. Main Results

Table 5 presents the estimate of  $\alpha_1$  from equation (2) for various specifications. These regressions and the analyses in the rest of the paper use the sample of juveniles who have the same petition and adjudication dates. As such, the judge has made one decision rather than two consecutive decisions. Thus, using this sample of juveniles helps us avoid the potential sample-selection bias.

Table 5 displays the results for which the outcome is the logarithm of disposition length and the results from linear probability models in which the dependent variable takes a value of one if the juvenile was taken into custody and zero if he or she was placed on probation. We report the standard errors clustered at the judge level for all remaining analyses (105 clusters). Our results remain intact if we instead cluster the standard errors at the court or parish level. To evaluate the sensitivity of the estimates, we present the main results of equation (2) using four specifications that include various controls and fixed effects. In some specifications we also include the age and gender of the juvenile.

The estimate of interest,  $\alpha_1$ , measures the impact of same-race assignment. Table 5 shows that same-race juvenile-judge assignment has a positive and statistically significant effect on sentence length. The most extensive specification, presented in column 4, suggests that same-race juvenile-judge assignment increases the initial disposition length rendered at the disposition hearing by 13.6 percent. This corresponds to a sentence that is 85 days longer for a juvenile assigned to a judge of the same race, relative to a juvenile assigned to a judge of a different race, even though the defendants have the same attributes (such as age, gender, type of offense, and parish of the offense). It is worth noting that the point estimates

<sup>21</sup> It should be recognized, however, that the sample we analyze consists of defendants who are found guilty and therefore recorded in the OJJ system. Juveniles who are found not guilty are not recorded in the OJJ system. This group is about 20 percent of all cases for which a petition is filed by the DA.

Table 5  
The Impact of Race Matching on Disposition Length  
and Probability of Custody

	(1)	(2)	(3)	(4)
Log Disposition Length	.1402* (.0613)	.1462* (.0693)	.1394* (.0686)	.1359* (.0630)
Custody	.0543* (.0240)	.0473+ (.0243)	.0447+ (.0246)	.0431+ (.0259)
Parish fixed effects	No	Yes	Yes	Yes
Juvenile controls	No	No	Yes	Yes
Offense fixed effects	No	No	No	Yes

**Note.** Parish fixed effects are for the parishes of residence and offense. Juvenile controls include a dummy for gender and linear and quadratic terms of age. Offense fixed effects represent 165 offense categories. All regressions include judge and court-year fixed effects. Standard errors clustered at the judge level are in parentheses.  $N = 5,142$ .

+  $p < .10$ .

\*  $p < .05$ .

on disposition length are very stable across the three specifications, ranging from 13.59 to 14.62 percent.<sup>22</sup>

The effect of same-race juvenile-judge matching on the probability of custody is also reported in Table 5. The results across the four specifications show an effect of roughly 4.3–5.4 percentage points. This implies that all else being equal, if a juvenile defendant is assigned to a judge of the same race, the defendant is nearly 20 percent more likely to be placed in custody (instead of being placed on probation).

### 5.2. Robustness Checks

We implement a number of sensitivity analyses to examine the robustness of our results. Table 6 presents estimates from the models that include various fixed effects.

The point estimates reported in Table 6 are very similar to those presented in Table 5, although the precision of the estimates is reduced in some instances. The stability of the estimated coefficients across the various specifications supports the finding that juveniles who are matched (randomly) with a judge of the same race receive longer disposition lengths and are more likely to be in custody.

<sup>22</sup>Regressions that employ the full sample of delinquent juveniles (12,800 observations) are likely to be confounded by selection. This sample contains all juveniles who went through a disposition hearing, regardless of whether they pleaded guilty or not guilty in the petition hearing (see the discussion on potential sample selection in Section 3). The results from this sample, which are not reported in the interest of space, show no statistically significant association between same-race juvenile-judge assignment and disposition length. These estimates, however, may not reflect the causal impact of race matching. This is because, as discussed earlier, the decision of judges at the adjudication hearing may have generated nonrandom selection of juveniles who then moved to the disposition hearing (thus being part of the full sample of 12,800 juveniles) and faced the same judges to receive their sentences. Any biased decision of judges at the adjudication hearing would render unreliable the estimates obtained from this sample.

Table 6  
The Impact of Race Matching: Alternative Specifications

	Log Disposition Length (1)	Custody (2)
Court and year fixed effects	.1255+ (.0641)	.0443+ (.0257)
Court, year, and court-trend fixed effects	.1226+ (.0625)	.0492* (.0246)
Court-year and offense fixed effects	.1441* (.0593)	.0537* (.0258)
Judge-year fixed effects	.1195* (.0584)	.0489* (.0245)
Court-year-month fixed effects	.1477+ (.0797)	.0312 (.0319)

**Note.** Each estimate is from a separate regression. Offense fixed effects represent 165 offense categories. Standard errors clustered at the judge level are in parentheses.  $N = 5,142$ .

+  $p < .10$ .

\*  $p < .05$ .

It is possible that black juveniles commit a particular type of crime more frequently than whites and that white juveniles could be more likely to engage in another type of crime. If black judges are stricter against the first type of crime and if white judges are stricter against the second type, then our results would incorrectly imply that judges assign harsher sentences against defendants of their own race when in fact the results are driven by matching of the judges' attitude and offense mix by race. To investigate the validity of this conjecture, we used the OJJ categories and classified crimes into three groups: crimes against persons, crimes against property, and crimes against society.<sup>23</sup> While both white and black defendants were equally likely to commit a crime against society in our sample (46 percent and 47 percent, respectively), 18 percent of black juveniles committed a crime against a person, whereas the rate is 14 percent for white defendants, and the difference is statistically different from 0. There is a statistically significant 4-percentage-point difference in white and black juveniles in their propensity to commit a crime against property (39 percent for whites and 35 percent for blacks). To control for the potential impact of this offense mix, we reestimated the models, including judge-by-offense-type fixed effects. This is a demanding specification that wipes out much of the variation. Nevertheless, the estimated coef-

<sup>23</sup> Crimes against property include such offenses as theft, illegal possession of stolen items, burglary, criminal trespassing, robbery, bank fraud, and motor-vehicle theft. Crimes against persons include assault, battery, rape, incest, stalking, cyberbullying, public intimidation, and kidnapping. Crimes against society include offenses such as reckless operation of a vehicle; illegally carrying a weapon; violation of city or parish ordinance; possession, manufacture, or distribution of drugs; and ungovernability. Note that even though robbery is a violent crime because it involves a weapon, force, or threat of force, it is categorized as a crime against persons because something valuable is taken from a person.

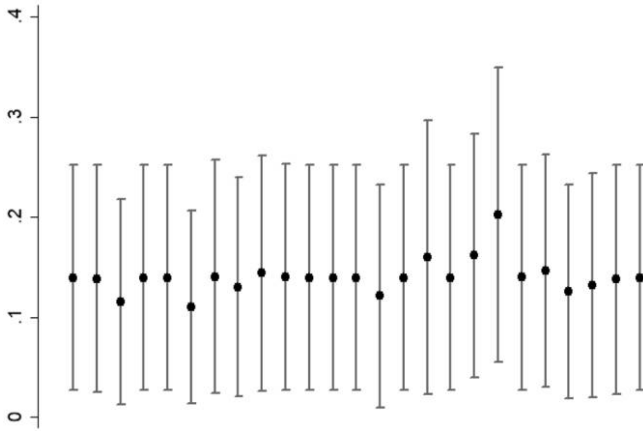


Figure 3. Impact of race matching on log disposition length (with 90 percent confidence intervals).

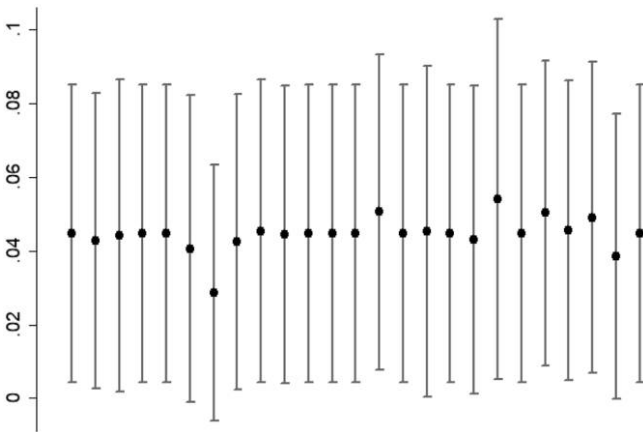


Figure 4. Impact of race matching on probability of custody (with 90 percent confidence intervals).

ficient of the same-race dummy was .098 (SE = .062) in the disposition-length regression, and it was .055 (SE = .028) in the custody regression, which indicates that the results are not an artifact of the difference in the offense mix by race.

As an additional robustness check, we considered whether one outlying judge was driving the estimated effects. To do so, we estimated equation (2), controlling for juvenile characteristics, judge fixed effects, and offense fixed effects, but we dropped from the sample all cases handled by one particular judge. We repeated this process 105 times so that each judge was omitted one at a time. We display

this analysis in Figures 3 and 4 for the 25 black judges in the sample. As shown, removal of any one black judge from the analysis does not alter the impact of same-race matching on either the disposition length (Figure 3) or the disposition type (Figure 4), which indicates that the results are not driven by a particular black judge. Repeating the same exercise for the 80 white judges provided the same conclusion: the results are not driven by the behavior of a particular white judge.

## 6. Discussion and Conclusion

This paper aims to contribute to two areas of research: the investigation of unequal treatment of minorities in the justice system and the analysis of in-group bias (preferential treatment of one's own group). In the former area, it is well documented that there exist disparities between whites and minorities and between males and females in the application of the law. In response, some scholars have argued for generating racial and gender diversity in the courts to introduce the perspectives of minorities and females in the decision-making process of judges (Ifill 2000). This argument is related to our latter area of investigation: in-group bias, or preferential treatment of the group with which one identifies. Group identity is expected to cause individuals to be more generous toward the members of their in-groups. Thus, creating racial, ethnic, and gender diversity in the courts is assumed to counteract the inherent in-group bias that emerges because of the homogeneity of judges. There is experimental evidence to support the hypothesis of preferential treatment of members of the in-group (Chen and Li 2009; Goette, Huffman, and Meier 2012), and in-group bias is reported in Israeli courts, where a plaintiff's claim is more likely to be accepted if the randomly assigned judge is of the same ethnicity (Shayo and Zussman 2011). On the other hand, it has also been reported that in-group members are punished more severely when they violate a social norm (Mendoza, Lane, and Amodio 2014; Goette, Huffman, and Meier 2006).

In this paper we use the universe of juvenile-court decisions in Louisiana between 1996 and 2012. The data include the case records containing detailed information about the defendants, the charges, and details of adjudication and disposition, including the verdict (guilty or not guilty), the sentence type and length, and the identity of the judge. A key aspect of our analysis is the fact that defendants are randomly assigned to judges.

We investigate the existence of racial in-group bias in the sentencing of juvenile defendants. Our identification strategy relies on a stream of juvenile defendants who are apprehended in the same parish (county) for the same offense and randomly assigned to a given judge in a particular jurisdiction. We find evidence for negative racial in-group bias in sentencing. Black (white) juveniles who are randomly assigned to black (white) judges are 20 percent more likely to be placed in custody, as opposed to being placed on probation, and they receive sentences

that are about 14 percent longer. This negative in-group bias is the first result reported in a nonexperimental setting.

We cannot determine the specific avenue through which this result is generated, and there could be a number of potential mechanisms. For example, given that there is substantial race matching between crime victims and perpetrators, if a judge belongs to the same in-group as the perpetrator, it is likely that the victim is a member of the in-group as well. Thus, judges may be expected to punish perpetrators who cause harm to the in-group. We test this conjecture by implementing our analysis for two subsets of juveniles in our effective sample: crimes with victims (2,733 observations) and victimless crimes (2,409 observations). The results are provided in Table A1. Although the point estimates across the partitioned samples are not statistically different, the results are informative. If the crime involved a victim, the effect of race matching increases the disposition length by 15.3 percent (column 4). On the other hand, if the crime was victimless, race matching increases sentence length by about 2.2 percent (column 8). The impact of race matching is also larger in the probability-of-custody regression when the crime involves a victim. That the estimated effects are larger for juveniles whose crime involves victims (than for those who commit victimless crimes) arguably provides support for this conjecture.

In an attempt to increase the statistical power in this last set of regressions, we pooled the data from two subsamples and estimated the model with an interaction term between the same-race dummy and the dummy indicating that the crime involved a victim along with the main effects and other controls. The results, reported in Table A2, are consistent with those reported in Table A1, although the estimated coefficients of the same-race dummy and its interaction with the crime-with-victim dummy are not significant at conventional levels.

To investigate this point in more detail, we obtained parish population data by race from the Surveillance, Epidemiology, and End Results Program of the National Institutes of Health.<sup>24</sup> In our sample (weighted by the number of juvenile cases in a parish), on average .461 of the population is black, with a standard deviation of .195. Using these data and the race of the judge, we calculated the share of the population that is the same race as the judge in the parish where the crime was committed.<sup>25</sup> Following our main empirical strategy, for crimes that involved victims, we interacted the indicator for same-race match with a third-degree polynomial in the share of the population that is the same race as the judge. Because the marginal effect of the same-race assignment now depends on the share of the population that is the same race as the judge, we display our results graphically. Figure A1 shows that an increase in the proportion of the population that is of the same race as the judge (and the juvenile) is associated with an increase in sentence length. This result indicates that if the crime involves a victim and if

<sup>24</sup> For All States Combined and Louisiana (adjusted) population files, see National Cancer Institute, Surveillance, Epidemiology, and End Results Program, Download U.S. Population Data—1969–2015 (<https://seer.cancer.gov/popdata/download.html>).

<sup>25</sup> Hearings are held where the crime was committed, not in the parish of residence.



the defendant, the judge, and a large proportion of the local population (from which the victim was drawn) belong to the same race, then the judge imposes an even stiffer sentence on the same-race defendant. We do not find an impact of the same-race local population when we analyze the probability of custody (Figure A2). Consistent with the conjecture entertained here, the variation in the racial composition of local population has no impact on judicial outcomes if the crime is victimless (Figures A3 and A4).

Another potential mechanism behind our main results is that being a member of an in-group may provide useful inside information about the future behavior of another member of the group. To the extent that judges can better evaluate the difficult-to-observe attributes of same-race defendants, harsher treatment by judges targeted to their own race could be efficient (Cornell and Welch 1996; Fisman, Paravisini, and Vig 2017). Along the same lines, judges may have inside information about the impact of longer sentence lengths on recidivism and labor-market outcomes. If judges believe that stricter punishment of juveniles will reduce recidivism and improve these juveniles' future outcomes, in-group bias of judges would generate harsher punishment of the juvenile defendants of the same race.<sup>26</sup> A similar but more nuanced explanation is the possibility that the punishment meted out by judges to defendants of their own race is optimal but that sentences assigned to defendants of another race are lenient. This could be the case if judges are concerned about creating the impression of being prejudiced toward defendants who are of another race. This might be especially important if judges are concerned that such an impression could impact their reelection chances or their careers more generally.<sup>27</sup> Even though the specific reason for the negative in-group bias cannot be identified, our results underline the handicap faced by juvenile defendants in courts when they face judges of their own race.

<sup>26</sup> Note that the impact of punishment on future outcomes is still debated. For example, Landersø (2015) uses register data from Denmark and finds that longer incarceration spells generate lower unemployment rates and higher earnings, possibly because of rehabilitation. Similarly, Kling (2006) reports a small positive impact of incarceration on employment. On the other hand, Di Tella and Schargrodsky (2013) find a positive impact of imprisonment on recidivism in Argentina, where the prison conditions are far from ideal.

<sup>27</sup> We thank the editor for this insight.

## Appendix

### Tests of the Impact of Judge-Defendant-Victim Race Matching

**Table A1**  
The Impact of Race Matching on Disposition Length and Custody by Crime Type

	Crime with Victim (N = 2,733)				Victimless Crime (N = 2,409)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Disposition Length	.1838 (.1138)	.1568 (.1250)	.1583 (.1175)	.1526 (.1018)	.0300 (.0535)	.0264 (.0689)	.0183 (.0700)	.0219 (.0639)
Custody	.0680+ (.0367)	.0654 (.0415)	.0629 (.0406)	.0490 (.0488)	.0305 (.0251)	.0322 (.0269)	.0321 (.0256)	.0427 (.0287)
Parish fixed effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Juvenile controls	No	No	Yes	Yes	No	No	Yes	Yes
Offense fixed effects	No	No	No	Yes	No	No	No	Yes

**Note.** Parish fixed effects are for the parishes of residence and offense. Juvenile controls include a dummy for gender and linear and quadratic terms of age. Offense fixed effects represent 165 offense categories. All regressions include judge and court-year fixed effects. Standard errors clustered at the judge level are in parentheses.

<sup>+</sup>  $p < .10$ .

**Table A2**  
The Impact of Race Matching on Disposition Length and Custody: Pooled Data

	(1)	(2)	(3)	(4)
Log Disposition Length:				
Same Race	.1193 (.0718)	.1334 (.0830)	.1298 (.0855)	.1208 (.0799)
Same Race × Crime with Victim	.0453 (.1023)	.0309 (.1117)	.0262 (.1099)	.0292 (.1063)
Custody:				
Same Race	.0339 (.0307)	.0249 (.0318)	.0246 (.0318)	.0265 (.0338)
Same Race × Crime with Victim	.0375 (.0389)	.0415 (.0412)	.0375 (.0416)	.0306 (.0486)
Parish fixed effects	No	Yes	Yes	Yes
Juvenile controls	No	No	Yes	Yes
Offense fixed effects	No	No	No	Yes

**Note.** Parish fixed effects are for the parishes of residence and offense. Juvenile controls include a dummy for gender and quadratic terms of age. Offense fixed effects represents 165 offense categories. All models include indicators for black juvenile and crime with victim and interactions of the indicator for crime with victim with black judge and with black juvenile indicators. All regressions include judge and court-year fixed effects. Standard errors clustered at the judge level are in parentheses. N = 5,142.

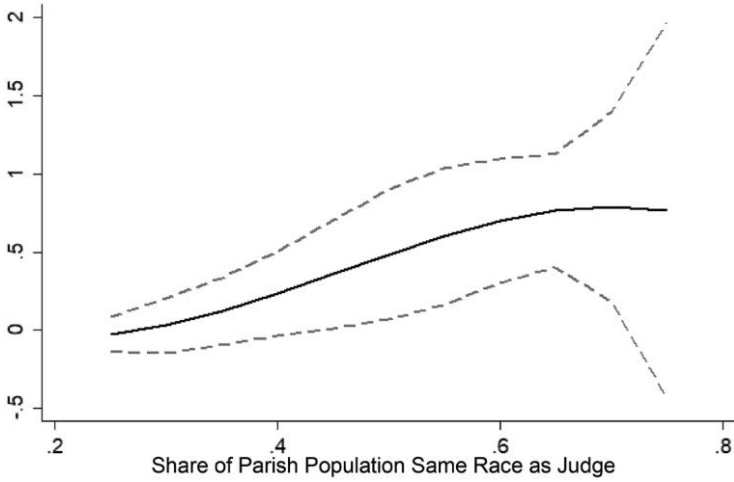


Figure A1. Log of sentence length: crimes with victims (with 95 percent confidence intervals)

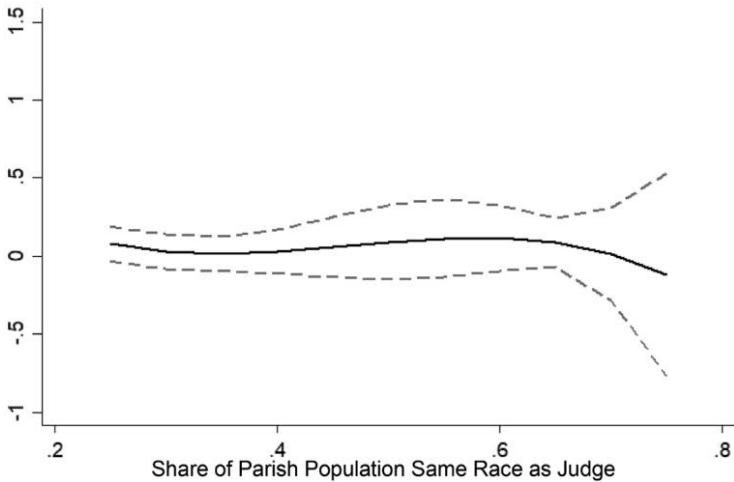


Figure A2. Incarceration: crimes with victims (with 95 percent confidence intervals)

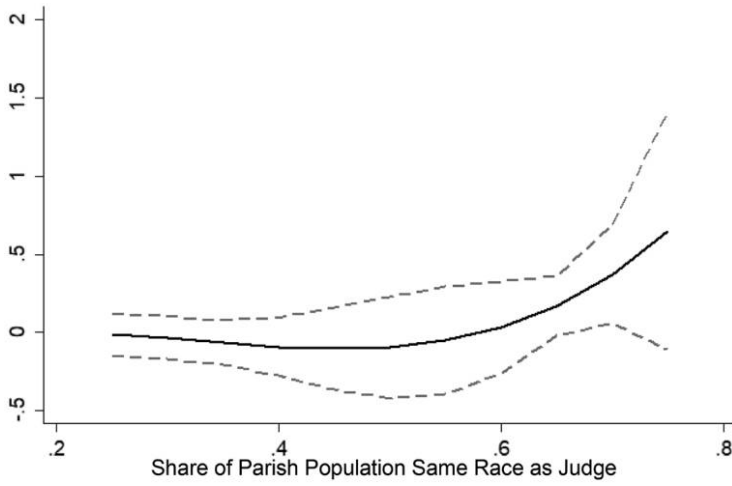


Figure A3. Log of sentence length: crimes without victims (with 95 percent confidence intervals).

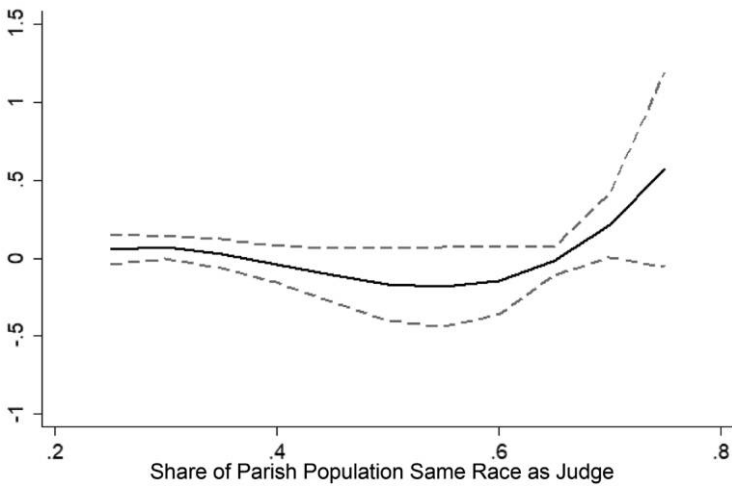


Figure A4. Incarceration: crimes without victims (with 95 percent confidence intervals)

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