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 U.S. 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 the same, black (white) juveniles who are randomly assigned to black (white) judges are more likely to get incarcerated (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 of the lab. Explanations for this finding are provided.

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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 investigating 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, Persico et al. 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 2015, Rehavi and Starr 2014, Abrams et. al 2012, Argys and Mocan 2004).

In order to guard against any capricious and discriminatory behavior of the courts towards minorities, some scholars have suggested that racial and gender diversity of the courts could be a mechanism through which different voices and perspectives can be introduced into the judicial decision-making process (Ifill 2000). Specifically, it has been argued that changing the racial and gender composition of judges 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 similar 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 or not 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.[1]

The influential paper of Akerlof and Kranton (2000) highlighted the importance of group identity in economic behavior. Laboratory experiments have shown 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 (Chan and Li 2009). Goette, Huffman and Meier (2012) show that 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 they 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 reported 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, Boyd et al. 2006), other research has found that in-group members may be punished more severely when they defect (Mendoza et al. 2014, Goette et la. 2006). In a different vein, it has been reported that minority donors are less generous towards 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) towards them. Furthermore, it is unclear whether in-group favoritism or prejudice is driven by beliefs or preferences. While preferences refer to predispositions, beliefs are based on expectations of future behavior (see Everett, Faber and Crockett 2015 for a summary). Put differently, any reaction to an in-group

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[1] 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, forthcoming, 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 caseworker-unemployed worker matching by social class (Behncke et al. 2010)

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

As an investigation of the existence of in-group bias in the courts, Shayo and Zussman (2011) analyzed data from Israeli small claims courts where the assignment of a case to an Israeli or Arab judge is random. They found 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 terrorism intensity in the area surrounding the court, preceding the judicial decision. Similarly, Gazal-Ayal and Sulitzeanu-Kenan (2010) found in-group bias in Israeli Arab and Jewish judges' decisions on criminal cases.

The existence and the extent of in-group bias in courts, however, could be culture- and context-specific. The variation in the setting (e.g. criminal vs. civil cases, state vs., federal courts), and the nature of the judicial decision (low-stakes vs. 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 (e.g. Israel), with a given history of ethnic and civil conflict and minority disenfranchisement, may have little validity in some other country, such as the United States.

Evidence is mixed regarding in-group bias in the U.S. courts. Boyd, Epstein and Martin (2010) found that a judge's gender makes a difference in the outcomes of sex discrimination cases. Gruhl, Spohn and Welsh (1981) analyzed convicting and sentencing behavior of male and female judges in felony cases. While they could not find evidence for differences in convicting,

they reported that female judges imposed longer sentences on female defendants.[2] Much of the existing work, however, is based on small samples, or makes inference using data from one city, or a few districts. Consequently, as Schanzenbach (2005) wrote "the literature on judging has not produced consistent findings regarding the impact of a judge's race on his or her decisions."[3]

In this paper we employ the universe of juvenile court decisions in the state of 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 defenders who faced the same judge (who is black or white) allows us to determine the extent of in-group bias.[4]

We find evidence for *negative* racial in-group bias in sentencing. Put differently, all else the same, black (white) juveniles who are randomly assigned to black (white) judges are more likely to get incarcerated (as opposed to being placed on probation), and they receive longer sentences. Although this finding may be surprising at first sight, 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

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[2] In a related paper Glynn and Sen (2015) analyzed whether having a daughter motivates judges to vote more favorably towards women on gender issues.

[3] Anwar, Bayer and Hjalmarsson (2012) analyzed the impact of the racial composition of *juries* in Florida, and found that all-white juries are more likely to convict black defendants.

[4] 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 et al. 2010)

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.[5] If

judges have an in-group bias regarding the well-being of their own race, they are expected to

punish perpetrators who exerted harm on that in-group, who in this case is also someone of that

same group. Second, being a member of an in-group may provide useful inside information

about 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 et al. 2015). This

argument, which is consistent with the model proposed by Cornell and Welch (1996), implies

that judges can assess the subtle clues better and they can extract soft information more

effectively from a member of their in-group, and that harsher treatment of judges targeted to their

own race could be efficient. Even though 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 of an experimental setting, in

naturally occurring 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.

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[5] For example, 56 percent of white victims had white offenders, and 62 percent of black victims had black
offenders (BJS, NCVS 2012-13).

2 Institutional Setting

Intake, the first step in the juvenile justice process, is initiated through a formal complaint filed on a juvenile by a citizen, parent, school administrator, or by the arresting police officer. After a formal complaint or an arrest, a report is submitted to the District Attorney's office for review. The District Attorney (DA) proceeds by either dismissing the case, referring the case to Families in Needs of Services (FINS), proceeding informally by referring the case to a diversion program or entering into an Informal Adjustment Agreement with the juvenile (Ch.C. Art 839), or formally pursing the case by filing a petition with the Clerk of the Court's Office.[6] [7]

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 re-assigned. These cases are excluded from the analyses. That is, we focus on cases where a juvenile interacts with a judge for the first time.

Under the provisions of the Louisiana juvenile justice system, within each court, a random allotment system is implemented by the Clerk's office for all cases filed.[8] The judge who

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[6] Cases referred to FINS typically involve conduct that would not be a crime if committed by an adult. Common examples include truancy, breaking school rules, runaway behavior, and violation of a law for a child under the age of 10. The FINS program works with troubled youth and his/her family to help rectify the issues causing the complaint.

[7] Cases are assigned to diversion programs which are designed to discourage juveniles from committing additional offenses. Youth that successfully complete the diversion program have their charges dismissed. Like being assigned to a diversion program, an IAA is not considered adjudication. The agreement outlines the terms and conditions of the youth's supervision. Youth that violate the terms of their IAA are referred back to the 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 (Ch.C. Art. 841).

[8] (Rules for Louisiana District Courts, Chapter 14, Appendix 14.0A, various years). This is similar to the process in Cook County, Illinois; see Abrams et al. (2012) for a similar discussion.

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 offer advice to the youth. The youth, however, may proceed without the assistance of a lawyer if agreed upon by the parents. At the petition hearing the youth may admit to the offense, deny the offense, or enter a response of nolo contendere (no contest) (CHC 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 of the petition, the court shall set the matter for an adjudication hearing that will occur at a later date (CHC 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" (CHC 877).[9]

At the adjudication hearing, the sequence of events follow standard procedures, in which both parties are able present evidence and examine and cross-examine witnesses (CHC 878-881). The hearing is held before the court without a jury. Thus, the judge has the sole responsibility of declaring whether the evidence warrants an adjudication that the child is delinquent. Youth not found to be delinquent are released and charges of the case are dropped. In this situation, the case does not enter the OJJ data base. If the judge finds the youth to be delinquent, the judge then sets a disposition (i.e. sentencing) hearing for a later date.

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[9] 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 shall not occur until five days after the clerk of the court notifies all parties of the examination order. Findings of the examinations are to be submitted within 30 days from the time it is ordered (CHC 867). During the pre-adjudication time period, the DA and defense attorney prepare for the case gathering evidence, including witnesses.

At the disposition hearing, the judge determines the type and the duration of the sentence rendered to the delinquent youth. Disposed youth are either incarcerated or released to the supervision of a parent or guardian under probation. The most severe type of incarceration is when the youth is assigned to the custody of the Department of Public Safety and Corrections to be confined in secure placement. Less severe incarceration consists of the delinquent youth being removed from their home and being placed in a non-secure residential placement or treatment facility, like a group home. Non-incarcerated dispositions consist of the delinquent youth being placed on formal probation for a specified period of time with rules and regulations that must be met.[10]

The judge determines the disposition by 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 twenty-one years without the benefit of parole, probation, suspension of imposition or execution of sentence, or modification of sentence (CHC 897.1).[11] We exclude these cases from the analysis. For other offenses, the children's code provides only general guidelines (see CHC 901).[12] Thus, the judge has discretion on both the sentence type and the sentence length.

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[10] One other non-custody 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 child must comply with the conditions set by the court (i.e., counseling, curfews, school attendance).

[11] Similarly, armed robbery committed by youth 14 years of age and older at the commission of the offense are also required to receive a disposition of secure custody without benefit of parole, probation, suspension of imposition or execution of sentence, or modification of sentence.

[12] In general, the code suggests, "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." And 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" (CHC 901).

In this paper we investigate the two decisions made by judges at the disposition stage: whether the juvenile should be incarcerated (as opposed to being placed on probation), and the length of the sentence.

3 Random Assignment of Judges 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 only observe the juveniles who are delinquent. That is, the OJJ data contain records only of those who are determined by a judge to be guilty, those who admitted to the charges in the petition hearing, or those who answered "no contest" in the petition hearing. This is potentially problematic because the judge who is randomly assigned to the case is the same judge who presided all hearings. Put differently, the judge who makes the determination of guilt/innocence also determines the length of the sentence and whether the juvenile is incarcerated. As such, our sample is likely contaminated with non-random 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 (e.g. minorities) during the adjudication process by being more or less lenient towards that group, then racial differences in sentence type and sentence length may be driven by differences between the types of juveniles who are filtered out during the adjudication process.

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

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judge is to determine the disposition outcome for the juvenile by setting the disposition type (custody or probation) and the disposition length. In the data, we cannot distinguish between the juveniles who were actually adjudicated delinquent through a hearing, the ones who pleaded guilty, and the ones who did not contest the charge. Consequently, in an effort to ensure that we focus only on those 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 will have a separate adjudication hearing at a later date. Thus, if the petition and disposition dates are the same, this implies that the judge has not made a guilty/not guilty decision; 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 charge, received an adjudication of delinquency without an adjudication hearing. These defendants went straight to the disposition stage, where the judge decided on the type and the length of disposition. This is the core sample of juveniles that will be analyzed in the paper.

3.1 Data

The data for this study come from the Louisiana Department of Public Safety and Corrections, Youth Services, Office of Juvenile Justice (OJJ) and include the universe of case records from 1996 to 2012 in which juveniles were found to be delinquent.[13] Therefore, the data set does not contain information of individuals who were not found to be delinquent. Nor does it contain information on formal complaints and arrests that were not pursued through a petition by the DA (i.e. diversion programs, FINS, and dropped cases).

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[13] Adjudicated through a formal adjudication hearing or admission to or a response of no contest to the allegations in the petition.

For each observed case record, we have information on both the juvenile defendant and the case itself. The defendant data include information on race, gender and age of the juvenile, parish (country) of residence, parish of offense committed, the exact statute offense committed, and a unique individual identifier. The case data include information on the dates of the petition hearing, adjudication hearing, and disposition hearing, the judge's disposition type (custody versus probation) and the disposition length, the court in which the hearings are held and the name of the judge. By using the names of the judges, we can gather information on them: the most critical one, for the purposes of this paper, being the race of the judge.

We consider two outcomes of interest: (i) the type of disposition rendered to the youth (custody confinement or probation), and (ii) the disposition length. In order to circumvent any potential confounding that may arise from multiple offenses and/or prior criminal history of the juvenile, we limit our attention to the first time delinquents ages 10 to 17 who are either black or white and who were convicted for only one statute offense.[14] We drop all cases in which the offense is first degree murder, 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.[15] Because the central element of the analysis is to exploit the random assignment of the race of the judge within a court, we drop all courts that do not have at least one black judge

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[14] The vast majority of cases consist of charges of only one statute offense. In the data 90.4% of cases consist of one offense, 9.5% of cases consist of two offenses, and 0.1% of cases consist of 3 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 youth aged 10 through 17. However, if the juvenile is age 17 at the time of offense, the case is moved to adult court (CHC 801; 804).

[15] This only amounts to 243 cases being dropped.

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.[16]

Columns (1)-(6) of Table 1 present descriptive statistics for adjudicated juveniles in the data. The first two columns pertain to all juveniles. Columns (3) and (4) display information on juveniles who have the same petition and adjudication dates. As described in the previous section, this sub-sample of juveniles is plausibly free of sample selection issues. Columns (5) and (6) pertain to juveniles who have different petition and adjudication dates. This means that this particular group is a selected sample, because they have been in front of the same judge sequentially. Therefore 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 columns (1) and (2) suggest that around 33 percent of the juveniles are incarcerated (i.e. placed under secure or non-secure custody).[17] 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. In fact, we observe that 50% of the disposition lengths are within 2 days of a half-year multiple (half-year, one-year, one and a half-year, etc.). 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,

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[16] The main results, described in section 5 below, are similar to those obtained from the sample that includes courts with no variation in the race of the judge.

[17] This is higher than the national average incarceration rate (e.g., 25 percent in 2011) among all adjudicated cases (Hockenberry and Puzzanchera 2014) and the average incarceration rate (23 percent) reported in Aizer and Doyle (2013) in Cook County, Illinois from 1991 to 2006.

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; ungovernable; 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 of *possession, manufacturing, and distribution of drugs* is 606 days, with a standard deviation of 423 days.

Of the 12,800 cases in the data (displayed in columns 1 and 2 of Table 1) 5,142 have common petition and adjudication dates. Columns (3) and (4) of Table 1 present the descriptive statistics for these juveniles; and columns (5) and (6) of Table 1 present the descriptive statistics for juveniles with different petition and adjudication dates. It is interesting to note that although we don't observe substantial differences across disposition lengths and incarceration rates between the two subsamples, the felony offense rate is higher for youth who have different petition and adjudication dates. This is consistent with the supposition that youth facing more serious allegations are more likely to go to trial (adjudication hearing), while youth of less serious allegations are more likely to admit or not contest the charge.

From a total of 105 judges that are in the effective sample, 76 percent are white (80 of 105 judges) and 30 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 the Judge Assignment

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 run a regression of a black judge indicator on juvenile and case characteristics, controlling for any variables on which the randomization was conditioned (e.g. court-by-year fixed effects). More specifically, we estimate

$$BJ_{ijc} = \pi_0 + \pi_1 x_i + \lambda_c + \varepsilon_{ijc} \qquad (1)$$

where BJ_{ijc} is an indicator that takes the value one if judge j in court-by-year cell c, who is faced by juvenile defendant i, is black; and zero otherwise. 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, i.e. ages 15-17, and felony offense). λ_c is the set of court-by-year fixed effects and ε_{ijc} is an unobserved component.

Columns (1)-(3) of Table 2 display the results from the randomization tests using data from all delinquent youth. Columns (4)-(6) present the results pertaining to delinquent youth with the same petition and adjudication dates; and columns (7)-(9) show the results for the sample of youth who have different petition and adjudication dates. Each point estimate and the corresponding standard error (in parenthesis) pertain to a separate OLS regression. Standard errors are clustered at the court level. Columns (1), (4), and (7) include court-by-year fixed effects. To further test the sensitivity of randomization tests we add in fixed effects for the parish the offense was committed and the parish of residence of the juvenile (displayed in

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columns (2), (5), and (8)) and fixed effects for the offense (displayed in columns (3), (6), and (9)).[18]

For the sample of juveniles who have the same petition and adjudication date, the coefficient estimates in Table 2 (columns 4, 5 and 6) are all very small in magnitude and none of them is statistically different from zero. 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 (columns 7, 8, and 9) does not produce consistently insignificant point estimates. Rather, both gender and felony offenses are both statistically significant indicators of the judge's race, suggesting that the sample of juveniles with different petition and adjudication dates may in fact be effected by the judge's decision in the adjudication hearing. 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 and that focusing on youth 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 The Impact of Racial in-Group Bias

In order to isolate any potential racial disparity in disposition outcomes by the race of judges we estimate the following regression model

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

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 incarceration), and disposition

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[18] We observe 165 different offenses in the data.

length (log of the sentence length). BJ_{ijc} is an indicator that takes the value of one if judge j who

was assigned to youth i in court-by-year cell c is black. Similarly, BY_{ijc} takes the value of one if

youth i is black. λ_c is the set of court-by-year fixed effects, θ_j stands for judge fixed effect, and

μ_{ijc} is an unobserved error term.

Under random assignment, the coefficient estimate of α_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 (sentence length), α_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, α_1 is the average change in

the outcome for white juveniles, relative to blacks, from having been assigned to a white judge.

As such, 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

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

$$E(Y|BJ = 1, BY = 0) = \alpha_0 + \lambda_c + \tilde{\theta}_{j|BJ=1} \qquad (3b)$$

$$E(Y|BJ = 0, BY = 1) = \alpha_0 + \alpha_2 + \lambda_c + \tilde{\theta}_{j|BJ=0} \qquad (3c)$$

$$E(Y|BJ = 1, BY = 1) = \alpha_0 + \alpha_1 + \alpha_2 + \lambda_c + \tilde{\theta}_{j|BJ=1} . (3d)$$

Equation (3a) displays the expected value of the disposition outcome when both the judge and

the juvenile defendant are white. Equation (3b) shows the expected value of the outcome when

the judge is black but the defendant is white. $\tilde{\theta}_{j|BJ=0}$ in equation (3a) 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 (3b) is

the weighted average 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 $(3b - 3a)$ 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 $(3d - 3c)$ is equal to $(\alpha_1 + \tilde{\theta}_{j|BJ=1} - \tilde{\theta}_{j|BJ=0})$. Therefore, the expected difference in the differences $(3d - 3c) - (3b - 3a)$ is simply, α_1. The important takeaway is that the expected value of α_1, which can be written as $3a + 3d - (3b + 3c)$ is simply,

$$E(Y|BJ = 0, BY = 0) + E(Y|BJ = 1, BY = 1)$$

$$- \big(E(Y|BJ = 1, BY = 0) + E(Y|BJ = 0, BY = 1)\big) \quad (4)$$

Hence, the estimate of α_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). This particular specification we employ to identify the in-group effect is the same specification used by 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 partitioned by race of the juvenile because a judge fixed effect would remove all 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, are important. To see this point more clearly, consider two races, A and B, and two types of judges, low-standard (L) and high-standard (H). Let L and H type judges have different sentencing behaviors. Now suppose race A judges are more likely to be L-type and race B judges

are more likely to be H-type. An analysis of the impact of judge race A on disposition outcome

for a sample of juveniles that consist only of type A will endogenously capture both the effects

from race and the effects from type, differences in L and H. As such, an analysis using

partitioned samples does not allow us to fully disentangle race effects from "type" effects. This

may occur, if courts in jurisdictions where whites are a majority may attract high-standard white

judges, but only low-standard black judges. Similarly, white judges in predominantly black areas

may be of low-standard. Other potential non-random 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.

　　　　We report the standard errors clustered at the judge level for all remaining

analysis (105 clusters). The results of the paper 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 three different specifications that include various controls and fixed

effects. In some specifications we also included the age and gender of the juvenile.

5. Results

5.1 Main Results

　　　Table 3 presents the estimate of α_1 from Equation (2). Panel A displays the results where

the outcome is the logarithm of disposition length. Panel B reports the results from linear

probability models where the dependent variable takes the value of one if the juvenile was

incarcerated, and zero if he/she was placed on probation. In the regressions of Table 3 we

employ the *entire* sample of delinquent juveniles (12,800 observations) even though the results

obtained from this sample may be confounded with selection.   Specifically, in Table 3 we use all

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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 estimate of interest, α_1, measures the impact of same-race assignment (the juvenile being assigned to a judge of the same race). Column (1) of Table 3 reports the results of the model that includes judge fixed effects and court-by-year fixed effects. Column (2) adds parish of residency and parish of offense fixed effects. The model behind column (3) is the same as that of column (2), but it also includes observable characteristics of the juvenile, which include age and its square (at time of admittance of delinquency) and an indicator for gender.

The results in Panel A of Table 3 show that when the entire sample is considered, there is no statistically significant association between same-race juvenile-judge assignment and disposition length. However, the point estimates are positive suggesting that same race matching may be positively correlated with disposition length. Panel B of Table 3 provides a similar result. In this sample of all juvenile defendants, same-race juvenile-judge assignment has no statistically significant impact on the probability of being incarcerated (vs. being placed on probation). 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 non-random selection of juveniles who then moved to the disposition hearing (thus being part or the sample of Table 3), and faced the same judge to receive their sentence on the type and the disposition length. Any biased decision of judges at the adjudication hearing would render unreliable the estimates reported in Table 3 regarding the disposition outcomes.

The sample of juveniles used in the regressions reported in Table 4 helps us avoid the potential sample selection bias. These are the juveniles who have the *same* petition and adjudication dates. *As such, the judge has made one decision on them, rather than two*

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consecutive decisions. Panel A of Table 4 shows that same-race juvenile-judge assignment has a positive and statistically significant effect on sentence length. The most extensive specification, presented column (3), suggests that same-race juvenile-judge assignment increases the initial disposition length rendered at the disposition hearing by 13.9 percent. This corresponds to an 87-day longer sentence length for a juvenile assigned to a judge of the same race, relative to the disposition length of a juvenile assigned to a judge of a different race, even though these defendants have the same attributes (such as age, gender type of offense committed, and the parish of the offense). It is worth noting that the point estimates on disposition length are very stable across the three specifications, ranging from 13.94 percent to 14.62 percent.

The effect of same-race juvenile-judge match on the probability of incarceration is reported in Panel B of Table 4. The results across the three specifications show an effect of roughly 4.5 to 5.4 percentage points. This implies that all else the same, if a juvenile defendant is assigned to a judge of the same race, the defendant is nearly 20 percent more likely to receive incarceration (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 5 presents estimates from the models that include various fixed effects. Column (1) reports the effect on disposition length and column (2) presents the effect on incarceration probability. The first row of Table 5 displays estimates that include separate fixed effects for court and judge, instead of controlling for court-by-year fixed effects. The second row adds in court specific linear trends. The third row adds in offense fixed effects to the specification presented in

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Equation 2. The fourth row includes judge-by-year fixed effect and the last row includes court-by-year-by-month fixed effects.

The point estimates reported in Table 5 are very similar to those presented in Table 4, although the precision of the estimates goes down in some instances. The stability of the estimated coefficients across the various specifications support the finding that juveniles who are matched (randomly) with the same race judge receive longer disposition lengths and are more likely to be incarcerated in custody confinement.

As an additional robustness check we consider whether one outlying judge is driving the estimated effects. To do so, we estimate Equation 2 controlling juvenile characteristics, judge fixed effects, and offense fixed effects but, but we drop from the sample all cases handled by one particular judge. We repeat this process 105 times so that each judge is omitted one-at-a-time. We graphically display this analysis in Figures 3 and 4 for the 25 black judges in the sample. As shown in the figures, removal of any one particular 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), indicating 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 research areas: 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 to 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 herself. Group identity is expected to cause individuals to be more generous towards the members of their in-group. 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 claim of the plaintiff is more likely to be accepted if the randomly-assigned judge is of the same ethnicity as the plaintiff (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 et al. 2014, Goette et la. 2006).

In this paper we use the universe of juvenile court decisions in the state of Louisiana between 1996 and 2012. The data include the case records which contain detailed information on the defendants and the charges, as well as the details of adjudication and disposition including the guilty/not guilty verdict, the sentence type and sentence length. Also known is the particular judge who was in charge. 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 sentencing of juvenile defendants. Our identification strategy relies on a stream of juvenile defenders, 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)

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juveniles who are randomly assigned to black (white) judges are 20 percent *more* likely to get incarcerated (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 non-experimental setting.

There could be a number of mechanisms that can generate this result. For example, given that there is substantial race-matching between crime victims and their perpetrators, if judges belong 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 exerted harm on the in-group.[19] Second, being a member of an in-group may provide useful inside information about future behavior of another member of the group. To the extent that judges can better evaluate the difficult-to-observe attributes of the same-race defendants, harsher treatment of judges targeted to their own race could be efficient (Cornell and Welch 1996, Fisman et al. 2015). Along the same lines, judges may have inside information on the impact of longer incarceration on recidivism and labor market outcomes. If judges believe that a stricter punishment of the juvenile would reduce recidivism and would improve the juvenile's future outcomes, in-group bias of judges would generate harsher punishment of the juvenile defendants of the same race.[20]

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[19] 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 (2,397 observations). Although the point estimates across the partitioned samples were not statistically different from one another, the results were informative. If the crime involved a victim, the effect of same race match increased disposition length by 15.8 percent versus 1.8 percent if the crime was victimless and increased incarceration by 6.3 percentage points versus 2.8 percentage points if the crime was victimless. That the estimated effects are larger for juveniles whose crime involved victims (in comparison to those who committed victimless crimes) arguably provides support for this conjecture.

[20] 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 generates lower unemployment rates and higher earnings, possible due to rehabilitation. Similarly, Kling (2006) reported a small positive impact of incarceration on employment. On the other hand, Di Tella and Schargrodsky (2013) found a positive impact of imprisonment on recidivism in Argentina, where the prison conditions are far from ideal.

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 the opposite race are lenient. This could be the case if judges are concerned about creating the impression of being prejudiced towards the defendants who are of the opposite race.

The finding of the paper suggests that the in-group bias detected here could be the result of the beliefs, but not preferences. This is because racial in-group bias driven by preferences would have produced more lenient punishment for juveniles who are of the same race as the judge. On the other hand, as described above, there are potential mechanisms through which judges' beliefs about future behavior of the juvenile can be impacted, which can translate into the negative in-group bias reported in the paper. 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 these juveniles face a judge of their own race.

Figure 1
Distribution of Disposition Length
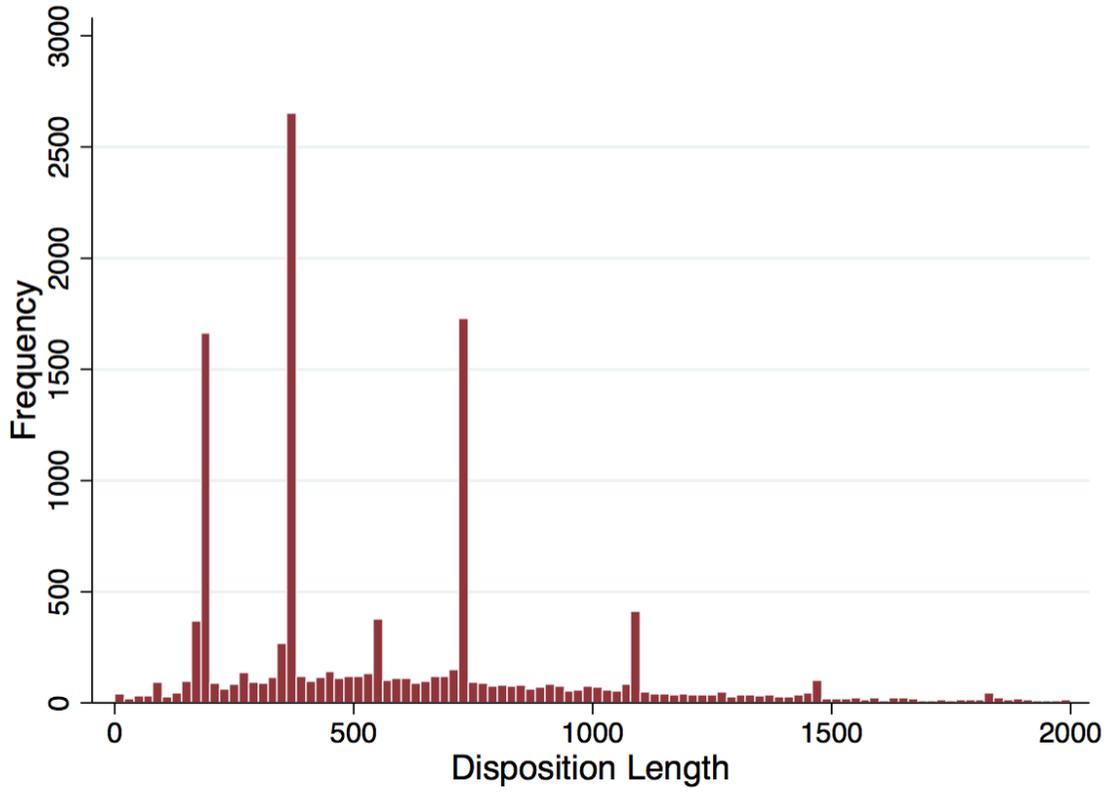
Figure 2
Distribution of Disposition Length by Offence Type
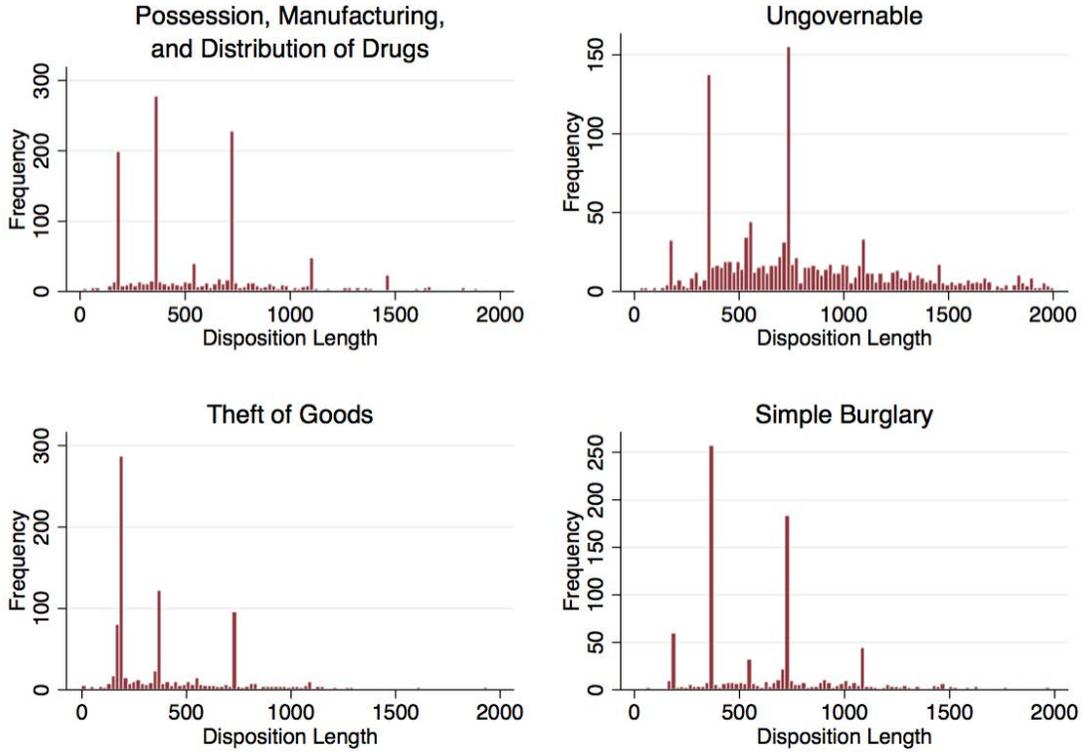
Figure 3
Estimates of Same-race Judge on log Disposition Length
(Excluding one Black Judge at a time)
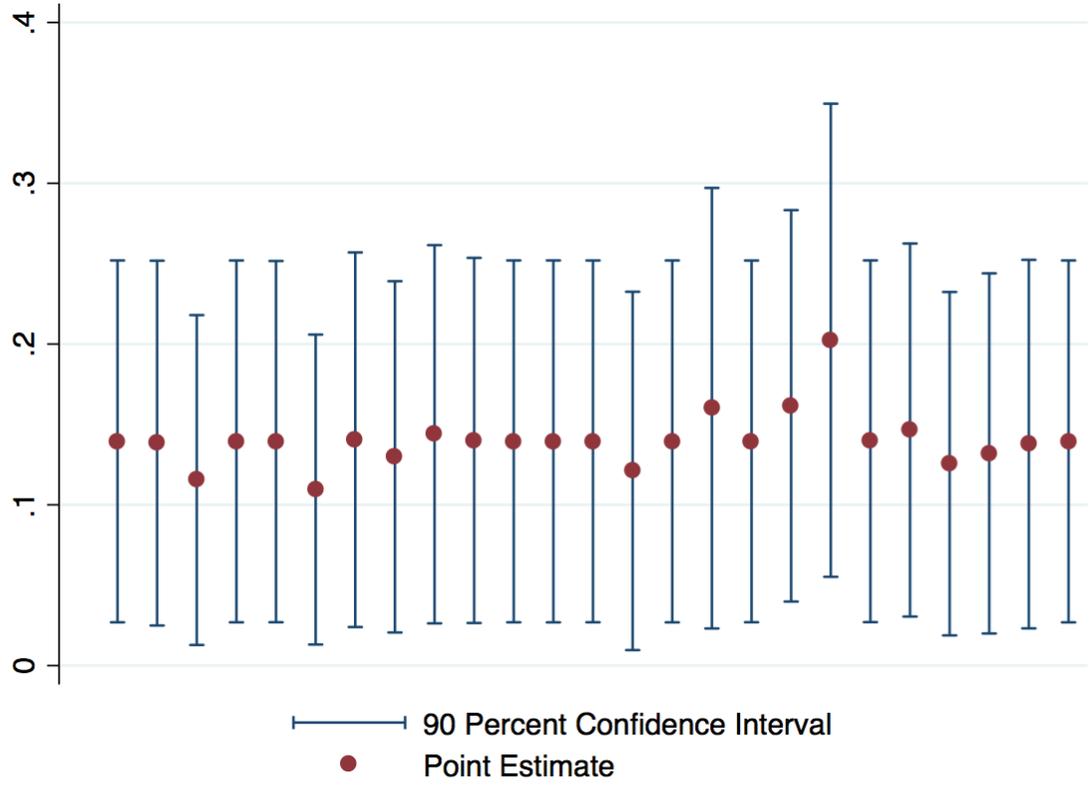
Figure 4
Estimates of Same-race Judge on Incarceration
(Excluding one Black Judge at a time)

90 Percent Confidence Interval

Point Estimate
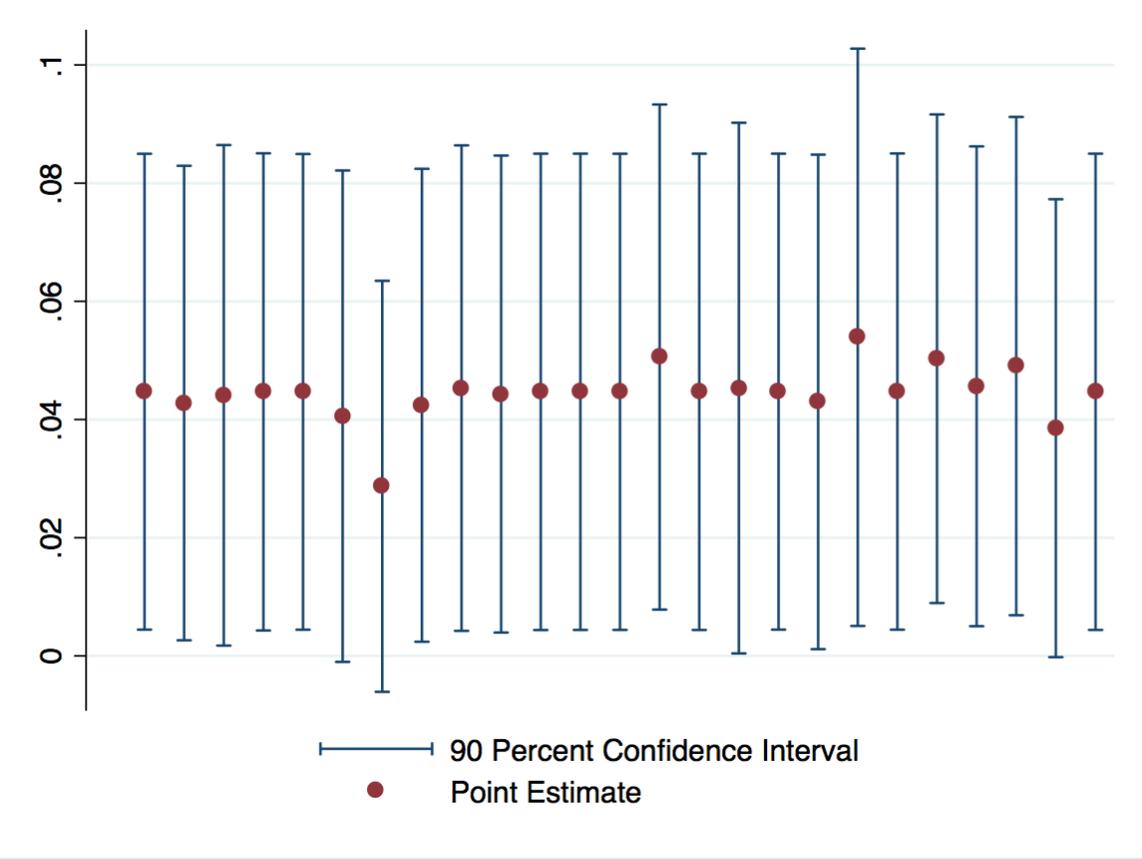
Table 1

Summary Statistics of Delinquent Juveniles

	All Juveniles (N=12,800)		Juveniles with the Same Petition and Adjudication Dates (N=5,142)		Juveniles with Different Petition and Adjudication Dates (N=7,658)	
	Black (1)	White (2)	Black (3)	White (4)	Black (5)	White (6)
Disposition length	605.48	578.81	652.08	652.08	578.51	601.64
	(441.43)	(390.46)	(460.12)	(354.62)	(427.97)	(432.43)
Incarceration	0.34	0.30	0.23	0.21	0.40	0.42
	(0.47)	(0.46)	(0.42)	(0.41)	(0.49)	(0.49)
Female juvenile	0.23	0.24	0.25	0.25	0.22	0.23
	(0.42)	(0.43)	(0.43)	(0.43)	(0.42)	(0.42)
Juvenile's age in years	15.22	15.41	15.16	15.41	15.26	15.40
	(1.45)	(1.40)	(1.52)	(1.39)	(1.41)	(1.40)
Felony Offense	0.44	0.46	0.41	0.42	0.47	0.50
	(0.50)	(0.50)	(0.49)	(0.49)	(0.50)	(0.50)
N	10,583	2,217	3,880	1,262	6,703	955

Standard deviations are in parentheses. Data are from the Louisiana Office of Juvenile Justice (1996-2012). Incarceration takes the value of one if a juvenile receives a disposition that requires his or her initial placement to be in a secure custody or non-secure custody facility, and zero if he/she is placed on probation.

Table 2
Tests for Random Assignment of Judges: Estimated Effect of Juvenile Characteristics on Black Judge Assignment
(Dependent Variable = 1 if the Judge is Black)

	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	0.010	0.013	0.015	0.004	0.017	0.009	0.017	0.014	0.023
	(0.021)	(0.020)	(0.017)	(0.029)	(0.024)	(0.022)	(0.026)	(0.023)	(0.023)
Female Juvenile	0.016*	0.017*	0.011	-0.003	-0.001	-0.007	0.030**	0.031**	0.021
	(0.008)	(0.009)	(0.011)	(0.006)	(0.007)	(0.008)	(0.014)	(0.014)	(0.017)
Age 15 or Older	0.007	0.005	0.009	0.007	0.003	0.002	0.006	0.005	0.008
	(0.006)	(0.006)	(0.007)	(0.005)	(0.005)	(0.007)	(0.010)	(0.011)	(0.013)
Felony Offense	-0.014	-0.015		0.002	-0.001		-0.029*	-0.028*	
	(0.014)	(0.014)		(0.015)	(0.016)		(0.015)	(0.016)	
Court-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parish of Crime	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Parish of Residence	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Offense FE	No	No	Yes	No	No	Yes	No	No	Yes

Each point estimate and the corresponding standard error (in parenthesis) is estimated from a separate OLS regression. Standard errors are clustered on the court level. *: significant at 0.10, **: significant at 0.05, ***: significant at 0.01 level.

Table 3

The Impact of Juvenile Defendant-Judge Race Matching on log Disposition Length and Incarceration (all Juveniles)

	(1)	(2)	(3)
Panel A			
Log Disposition Length			
Same Race (Judge-Juvenile)	0.0963	0.1008	0.0950
	(0.0601)	(0.0628)	(0.0591)
Panel B			
Incarceration			
Same Race (Judge-Juvenile)	0.0154	0.0183	0.0165
	(0.0242)	(0.0248)	(0.0248)
Judge FE	Yes	Yes	Yes
Court-Year FE	Yes	Yes	Yes
Parish FE	No	Yes	Yes
Juvenile Controls	No	No	Yes
N	12,800	12,800	12,800

FE stands for fixed-effects. Juvenile Controls: age indicators, gender, county of residence, county where the crime was committed. Standard errors, clustered on the judge, are reported in parentheses.

Table 4

The Impact of Juvenile Defendant-Judge Race Matching on
log Disposition Length and Incarceration
(Juveniles who have the same Petition Hearing and Adjudication Dates)

	(1)	(2)	(3)
Panel A			
Log Disposition Length			
Same Race (Judge-Juvenile)	0.1402**	0.1462**	0.1394**
	(0.0613)	(0.0693)	(0.0686)
Panel B			
Incarceration			
Same Race (Judge-Juvenile)	0.0543**	0.0473*	0.0447*
	(0.0240)	(0.0243)	(0.0246)
Judge FE	Yes	Yes	Yes
Court-Year FE	Yes	Yes	Yes
Parish FE	No	Yes	Yes
Juvenile Controls	No	No	Yes
N	5,142	5,142	5,142

FE stands for fixed-effects. Juvenile Controls: age indicators, gender, county of residence, county where the crime was committed. Standard errors, clustered on the judge, are reported in parentheses. *: significant at 0.10, **: significant at 0.05, ***: significant at 0.01 level.

Table 5
The Impact of Juvenile Defendant-Judge Race Matching
using Alternative Specifications

	Log Disposition Length	Incarceration
	(1)	(2)
Court FE and Year FE	0.1255*	0.0443*
	(0.0641)	(0.0257)
Court FE, Year FE, Court-Trend	0.1226*	0.0492**
	(0.0625)	(0.0246)
Court-Year FE and Offense FE	0.1441**	0.0537**
	(0.0593)	(0.0258)
Judge-Year FE	0.1195**	0.0489**
	(0.0584)	(0.0245)
Court-Year-Month FE	0.1477*	0.0312
	(0.0797)	(0.0319)

FE stands for fixed-effects. Each estimate is from a separate regression. Each set of estimates is from the base sample of 5,142 observations. Standard errors, clustered on the judge, are reported in parentheses*: significant at 0.10, **: significant at 0.05, ***: significant at 0.01 level.

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