JUDGE PEER EFFECTS IN THE COURTHOUSE

Ozkan Eren
Naci H. Mocan

Judge Peer Effects in the Courthouse
Ozkan Eren and Naci H. Mocan
NBER Working Paper No. 27713
August 2020, Revised April 2023
JEL No. D9,D91,J16,J71,K4,K41

<u>ABSTRACT</u>

We investigate whether consequential decisions made by judges are impacted by observable characteristics of peer judges. We utilize the universe of decisions on juvenile defendants in each courthouse in a Southern state over fifteen years. Leveraging random assignment of cases to judges, and variations in judge peer composition generated by judicial turnover, we show that an increase in the proportion of female peers in the courthouse causes a rise in individual judges' propensity to incarcerate, and an increase in prison time. This effect is driven by female judges. Further analysis suggests that this behavior of female judges is likely due to the sheer exposure to female colleagues.

Ozkan Eren
Department of Economics
University of California, Riverside
Sproul Hall
Riverside, CA 92521
ozkan.eren@ucr.edu

Naci H. Mocan
Department of Economics
Louisiana State University
2439 BEC
Baton Rouge, LA 70803-6306
and NBER
mocan@lsu.edu

1 Introduction

How does the behavior of an individual change as the attributes of his/her peers are altered? This is an important question both for scientific inquiry and for designing strategies to improve performance and productivity. The analysis of peer effects, however, is complicated because in most circumstances people are not assigned to a particular group of peers, nor do they choose their peers randomly. Rather, individuals associate themselves with their peers deliberately. Homophily, the tendency of individuals to choose their peers who share common attributes with them, creates the well-known selection problem, which makes it difficult to identify the influence of the group on an individual who is associated with that group (Manski 1993; Moffitt 2001; Sacerdote 2001). Researchers tackled this issue by exploiting circumstances in which assignment to peer groups is arguably random in a variety of settings, ranging from workplace productivity (Falk and Ichino 2006; Bandiera et al. 2009; Guryan et al. 2009; Mas and Moretti 2009; Arcidiacono et al. 2017) to student outcomes (Hoxby 2000; Arcidiacono and Nicholson 2005; Whitmore 2005; Carrell et al. 2013; Carrell et al. 2018), to business activity and consumer decisions (Field et al. 2016; Bollinger et al. 2020).

While it is important to investigate whether an individual's own behavior, such as own effort and own productivity are influenced by the attributes of that person's peers, it is equally important to analyze whether an individual's judgement on another person, or an individual's decision regarding a matter that impacts the well-being of another person is influenced by the characteristics of the decision-maker's peer group.

In this paper we investigate whether consequential decisions made by judges about defendants in a courtroom are impacted by attributes of these judges' peer group using a unique administrative data from Louisiana covering the universe of conviction records over a fifteen year period. More precisely, we focus on judicial decisions made about juvenile defendants, and we investigate how judges' incarceration and sentencing decisions are impacted by observable characteristics of peer judges in the courthouse, the so-called contextual peer effects. The decision to concentrate on contextual peer effects stems from the fact that observable judge characteristics, such as gender, race and age, are either time-invariant or vary exogenously with time. Thus, they are unlikely to be subject to the reflection problem (Manski 1993) which arises because of challenges in separating the impact of the peer group on the individual from the impact of the individual on the peer group.

Our primary focus is on gender peer effects, although we discuss several other peer characteristic throughout the paper. The underlying conceptual justification that forms the basis of this focus is multifaceted, and has received growing attention with the rise of behavioral and experimental economics. For example, Eckel

and Grossman (1996) report that women are more likely to reciprocally punish unfair behavior in comparison to men. Croson and Buchan (1999) provide evidence for women's higher propensity for reciprocity. It has also been documented that women's decisions are less individually-oriented and more socially-oriented in comparison to those of men (Eckel and Grossman 2008). These findings suggest that if female judges are more likely to consider the offenses of convicted defendants as transgressions towards society, female judges could be more likely to reciprocate on behalf of the society and impose harsher punishment. Such tendencies may amplify (or weaken) in highly gender-skewed environments. Equally important, female behavior is found to be more responsive to context (Eckel and Grosman 1996; Ben-Ner et al. 2004). As explained in detail by Croson and Gneezy (2009), women are more responsive to the environment in which they make decisions, and the responsiveness of women to the context explains some seemingly contradictory results about gender differences in experimental research. Finally, a number of studies have found that men respond much more strongly than women to competition. These differences in attitudes towards competition is one possible reason of why women may be less likely to enter male-dominated fields and underperform in competitive environments (Niederle and Vesterlund 2007; Niederle 2015). Thus, variations in the gender ratio of peers could differentially impact the behaviors of female vs. male judges. A growing body of research also documents gender differences in judicial decisions (Peresie 2005; Boyd et al. 2010; Knepper 2018).

We construct a panel containing the universe of judges serving in each courthouse using annual collections of the Supreme Court of Louisiana which allows us to track judicial movements with exact dates. We merge these judge turnover records, along with information on judge attributes, to administrative juvenile conviction records to analyze judicial spillovers in the courthouse. We estimate the effects of gender (and other) peer composition off of the within-judge variation over time, and exploit movements of peers in and out of courthouse for identification. The composition of judges in a courthouse changes due to departure of judges for such reasons as retirement, resignation and death, and because of arrivals of new judges.[1] Although random assignment of juvenile case files to judges (as discussed in detail below) alleviates concerns about confounding factors, it does not completely eliminate it because judges are not randomly assigned to their peers. Thus, we control for court and year fixed effects and court-specific trends in all specifications. To the extent that time-varying unobservable variables affecting juvenile dispositions are not systematically related to within judge variation in the peer composition, our empirical approach yields credible estimates of gender and other peer effects. We provide several robustness checks and various placebo tests supporting our identifying assumption throughout the paper.

---

[1] There are only a few judges moving from one judicial district to another during their tenure and therefore, identification is obtained from movements of peers in and out of courthouse.

Our results indicate that a 10 percentage point increase in the proportion of female judges (e.g., a change from 1 female and 9 male colleagues to 2 female and 8 male colleagues) increases incarceration decision (extensive margin) by 7 percent and generates an additional 6 days of prison time (intensive margin). Further examination of the data reveals that these effects are driven by female judges. Absent gendered confounders, heterogeneity in the peer effect estimates (lack of a meaningful impact for male judges) also suggests that our results are not a specious reflection of time-varying within-judge unobservable variables. Neither the racial composition of the courthouse, nor the average age of judges or their party affiliation affects judicial decisions, and gender peer effects are not sensitive to the inclusion of other peer characteristics in the model specifications.

We also aim to distinguish contextual peer effects from the impact of peers' outcomes (endogenous peer effects) by controlling for average peer harshness in incarceration and sentencing. Taken together, the results lend support to the argument that it is not the gender-specific leniency/harshness of peers, but rather sheer exposure to female colleagues that impacts judicial decisions. Under this premise, our findings may be consistent with the critical mass hypothesis which postulates that individuals who are in the numerical minority adopt the behavioral norms of those who are in the majority, but that members of the minority group start making decisions that reflect their true tendencies as the proportion of minorities in the group rises. Finally, we provide some back-of-the-envelope calculations by randomly adding one more female judge in one-quarter of all courthouses in Louisiana. The findings from this simulation exercise suggest an additional five juvenile incarcerations and 3,827 days of prison time imposed by female judges per year.

Our work is related to research on peer interactions and social network and their effects on individual outcomes. We provide evidence on the existence of second and higher-order effects in a high-skilled labor market which may be important to assess the overall efficacy of social interactions. This paper also adds to the growing literature on how extraneous influences can sway highly consequential criminal justice decisions of expert decision-makers (Abrams and Yoon 2007; Danziger et al. 2011; Aizer and Doyle 2015; Lim et al. 2015; Chen et al. 2016; Philippe and Ouss 2018; Cohen and Yang 2019; Eren and Mocan 2021; Agan et al. 2021). Our results bolster the view that outside factors unrelated to the merits of the case can influence judicial decisions. A better understanding of the extent of capricious judgement is crucial, given a right to equal and fair treatment under law. Lastly, our findings contribute to work on gender differences in judicial decisions and to the broader literature in preferences and behavior. We extend the investigation of psychological and social differences in gender to a real-life setting where the subjects comprise a uniformly highly educated group of individuals.

The remainder of the paper is organized as follows. Section 2 discusses the institutional settings. Section

3 presents the data. Section 4 describes the econometric methodology. Section 5 presents evidence on random assignment and the variation in peer composition to detect meaningful effects, provides baseline results and the findings from several robustness checks and includes a discussion of mechanisms. Conclusions and welfare implications are provided in Section 6.

2   Institutional Background

The judicial branch in Louisiana includes courts with different jurisdictions, ranging from state supreme court to the courts of appeals, from district courts to juvenile courts. There are currently 42 judicial districts in Louisiana. Twenty-eight of these districts cover one single parish each, and judges handle cases in their respective courthouses in these parishes. There are 10 other judicial districts where each district covers multiple parishes, and judges in these districts rotate between the courthouses of these parishes. Finally, four judicial districts have their separate juvenile courts, where each of these juvenile courts serves one parish.[2]

Judges in district courts and juvenile courts are elected by voters in partisan elections, where the party affiliation of each candidate is shown on the ballot.[3] Candidates for judge positions compete in a primary against other candidates, including those from their own parties, and it is common to have multiple candidates from a given political party to run for the same judge position.[4] District and juvenile court judges serve six-year terms and they are eligible for re-election. Elections are spread throughout the year. For example, elections in 2018 took place in March and November, with the runoff election of the latter being held in December.

Judge turnover, and therefore a change in the composition of judges in a courthouse can be generated not only by elections, but also by the newly created judgeships or by a vacancy. Vacancies can result from a variety of reasons ranging from resignation to death, from suspension to retirement before the end of the term.[5] These vacant positions are filled by special elections within a year after the day the vacancy occurs. The supreme court appoints a judge to the bench until the special election is held.[6]

Cases are randomly assigned to judges unless the juvenile was found guilty in a previous adjudication.[7]

---

[2] These parishes are Caddo, East Baton Rouge, Jefferson and Orleans.

[3] A candidate for the district/juvenile court must satisfy the following criteria to be eligible for judgeship: (i) licensed to practice law in the state for eight years, (ii) domiciled in the respective parish for at least one year, and (iii) be under the age of 70.

[4] This structure is sometimes referred to as the "jungle primary." The candidate who receives the majority vote in the primary is elected. If no candidate receives the majority vote, a runoff election is held between the top two candidates in the general election.

[5] In Louisiana the mandatory retirement age for judges is 70.

[6] This person is ineligible as a candidate to be elected in the special election (Article V, Louisiana Constitution of 1974).

[7] Under the provisions of the Louisiana juvenile justice system, a computer-generated random assignment (open to public)

Such repeat offenders are re-assigned to judges who handled the previous episode of the juvenile. At the adjudication hearing the judge may find the defendant not guilty and dismiss the case if the prosecutor is unable to provide evidence to find the youth delinquent. In this situation the juvenile is considered as not having entered the juvenile justice system, and the case is purged. If the judge finds the defendant guilty, the judge needs to make a disposition decision. Convicted defendants can be assigned by the judge to the custody of the Department of Public Safety and Corrections to be confined in secure placement (i.e., they are incarcerated). Instead of incarceration, the judge can assign a less severe arrangement where the juvenile is removed from his/her home and placed in a residential non-secure placement or treatment facility such as a group or foster home. Similarly, the judge can put the juvenile on probation.

In addition, the judge has to assign a sentence length to each convicted juvenile. This is true for those who are incarcerated and placed in secure custody, but it is also true for those who are placed on probation or in non-secure custody. That is, each convicted juvenile is assigned a sentence length regardless of the type of custody in which they are placed. Judges are responsible for weighing the severity of the offense committed and the prior offense history of the juvenile.[8] There are no mandatory sentencing guidelines and judges exercise considerable discretion in sentencing. Additional institutional details can be found in Eren and Mocan (2021).

3   Data

The data are obtained from two sources. Louisiana Department of Public Safety and Corrections, Youth Services, Office of Juvenile Justice (OJJ) provides the universe of case files from 1998 to 2012. Each case file contains information on the juvenile and the case, including the gender, race and the age of the juvenile, the statute offense committed, the date the juvenile was sentenced, sentence type (secure custody, probation, etc.), sentence length, the courthouse in which the disposition was held and the identifier of the judge.

The second source is the annual collections of the Supreme Court of Louisiana, the so-called *The Guide to Louisiana Courts*.[9] These annual periodicals feature statewide list of judges along with information on courthouse served and judicial turnovers. Specifically, the annual reports allow us to track movements into and out of courthouses (e.g., retirement and resignation) with exact dates. Using the Guide to Louisiana

---

is implemented in each court by the Clerk's office for all case files (Rules for Louisiana District Courts, Chapter 14, Appendix 14.0A, various years).

[8]In general, the judge will impose the least restrictive disposition consistent with the circumstances of the case, the health and safety of the child, and the best interest of the society (Louisiana Children's Code CHC 683).

[9]These annual collections are available at http://www.lasc.org/press_room/annual_reports/default.asp.

Although the universe of case files from OJJ dates back to 1996, we limit our attention to 1998 and onwards because annual collections are available online since 1998.

Courts, we construct a panel containing the universe of judges serving in each courthouse. We supplement this data by gathering information on judge attributes.[10]

We impose the following restrictions on our research sample. First, to be able construct the gender peer measure, we use only those courthouses that have at least two judges in a given year. Second, we exclude judges from the benchmark model if they had handled fewer than 25 case files in a courthouse over 1998-2012. Finally, in order to avoid potential confounding effects that may arise from multiple offenses and/or criminal history of the juvenile, we limit our attention to first-time delinquents ages 10 through 17 who were convicted for only one statute offense. Recall that repeat offenders are assigned to the same judge who has handled the original case. This attribute of the juvenile justice system compromises the critical condition of random assignment of defendants to judges. Thus, we exclude repeat offenders from the analysis, and focus on delinquents who had their first interaction with the juvenile justice system.[11] We end up with a total of 20,244 juvenile case files handled by 138 judges in 59 courthouses.[12]

In order to better capture the severity of punishment, we study both the extensive and intensive margins of judicial dispositions. We measure extensive margin by defining an indicator variable for whether the juvenile offender had been incarcerated following a conviction. We use the sentence length set by the judge to analyze the intensive margin. Sentences for juveniles who were not incarcerated are coded as zeros for this outcome, although we also present the results by replacing the assigned sentence length for those who were put on probation or held in non-secure custody. Doing so has no appreciable impact on the results (discussed in Section 5.2).

Table 1 presents the descriptive statistics. As shown in Panel A, the incarceration rate is about 14 percent while the average sentence length is around 81 days. Black (62 percent) and white (36 percent) juveniles comprise approximately 98 percent of all offenders. About 24 percent of the sample is female. Age at first conviction is almost 15, and 37 percent of juveniles are convicted of a felony crime.[13] Panel B of Table 1 reveals that 24 percent of judges are female and that the mean age of judges when they made incarceration decisions is 53. There are on average 6 judges in a courthouse in a year.

---

[10]We collect information on judges from variety of sources including online searches and Louisiana District Judges Association Periodicals (1956-2000).

[11]To avoid the influence of the outliers, we drop observations with assigned sentence lengths shorter than 80 days (bottom 1 percent of the sentence length distribution) and longer than four years.

[12]Fifty-three of these are district courthouses, four are juvenile courts, and two of them are city courts. The locations of city courts are generally different from district courts. Cases handled in city courts typically involve juvenile and petty offenses, ordinance and traffic violations. In an overwhelming majority of city courts a single judge is in charge of adjudicating cases.

[13]Ungovernable (11 percent), simple battery (8 percent) and simple burglary (8 percent) are the most common offense types in the data.

4   Empirical Methodology

To evaluate the impact of judicial gender composition of the courthouse on the decisions made by judges, we estimate the following equation

$$D_{ijct} = \beta_0 + \beta_1 F_{\square j,ct} + X'_{ijct}\beta_2 + \lambda_j + \theta_t + \theta_c + \theta_c t + \epsilon_{ijct} \tag{1}$$

where D_{ijct} is the disposition of juvenile defendant i set by judge j in courthouse c at time t. This variable takes the value of one if juvenile had been incarcerated (placed in secure custody) following his/her conviction. If D_{ijct} is zero, this indicates that he/she was not incarcerated, but instead was placed on probation or held in non-secure custody. Alternatively, D_{ijct} stands for the sentence length set by judges, including zeros for all non-incarcerated juvenile offenders. $F_{\square j,ct}$ is the proportion of female judges in the courthouse c, excluding judge j, at the beginning of year t. X'_{ijct} is a vector of observed juvenile characteristics (i.e., gender, race, age and its square and offense type), λ_j, θ_t and, θ_c stand for judge, year and court fixed effects, respectively, $\theta_c t$ are court-specific trends to control for linearly trending unobserved court characteristics and ϵ_{ijct} is the error term. We use the inverse hyperbolic sine (IHS) transformation of the sentence length because the empirical distribution of this outcome measure is right-skewed and has a large number of zero-valued observations. Standard errors are clustered at the judge level.

Several comments are warranted about the gender composition measure. First, we use the universe of judges in the state to construct $F_{\square j,ct}$, which allows us to measure the peer composition variable accurately. More specifically, in the creation of the peer measure we utilize all judges, even those who do not enter the analysis sample.[14] Second, recall that there are 10 judicial districts where each district covers multiple parishes, and judges in these districts rotate between the courthouses of different parishes. In these cases, ignoring judge mobility within the judicial district (i.e., ignoring the movement of judges between courthouses of different parishes) would prevent us from fully capturing the interactions between these judges. Thus, for these 10 judicial districts we calculate $F_{\square j,ct}$ at the district level, using all courthouses with which each judge is affiliated, although we also show that the results are insensitive to the inclusion/exclusion of these districts. Finally, using judge gender composition of the courthouse from the beginning-of-year may raise concerns for two related reasons, both of which may bias gender peer effect estimates towards zero. First,

---

[14]For example, assume that a district courthouse consists of four judges: A, B, C and D. Assume further that only three of these judges (A, B, and C) deal with juvenile cases, while the fourth judge (D) in the same courthouse takes on only adult crimes. This fourth judge is included to calculate the proportion of female peers faced by A, B, and C. It is important, however, to note that the overwhelming majority oj judges in district courthouses (77 percent of all judge-by-year observations in our analysis sample) handled juvenile cases. Consequently, gender peer effect estimates from equation (1) are similar when we construct the peer measure by ignoring judges who have not handled juvenile cases in that year (see Section 5.4 for these additional results).

the peer composition measure described in equation (1) may not fully reflect the exposure of judges to their peers in the courthouse because it does not take into account possible judge turnover occurring throughout the year. Second, a change in judge behavior in response to a change in peer gender composition may take some time to manifest itself. Therefore, we also estimate variants of equation (1) by: (i) constructing gender peer measure at the year-by-quarter level, and (ii) including its lagged values.

We control for judge fixed effects in all specifications to circumvent any potential bias from selection into peer groups. Thus, we estimate the effect of peer gender composition faced by a particular judge off of the variation in that composition over time. By relying only on within-judge variation, identification is obtained from the movements of peers in and out of courthouse. Judges, in principle, can switch peer groups if they relocate from one district to another. For example, a judge can resign from his post in a courthouse and can subsequently be elected to a position in another courthouse. This type of mobility, however, is extremely limited and comprises only about 2 percent of the sample. In conjunction with this, specifications that control for court-by-judge fixed effects provide estimates of gender peer effects that are almost identical to those presented in the paper. These results are available upon request.

Although random assignment of case files to judges mitigates concerns over omitted variable bias, it does not completely eliminate it because judges are not randomly assigned to their peers. It is conceivable that changes in the proportion of female judges are correlated with unobserved factors that also affect judicial decisions. For example, assume that state-wide awareness rises with regard to issues related to women's well-being (e.g., Me Too movement). Such awareness may motivate voters to elect female judges as opposed to male judges, and the same awareness may compel judges to be harsher against male defendants. In this case, a positive relationship may emerge between sentence severity and the proportion of female peers, but this would be an artifact of the change in the underlying state-wide sentiment towards females. To account for such confounders, we control for court and year fixed effects, as well as court-specific trends in all specifications.

The key identifying assumption underlying this framework is that time-varying unobservable variables affecting the outcome are not systematically related to within judge variation in the gender composition. To the extent that this assumption holds, the coefficient estimate β_1 can be interpreted as the causal impact of gender peer effects on judicial decisions in the courthouse. Note that our identification strategy does not rule out all potential confounding factors. For example, consider a violent crime in a particular parish before a judicial election, in which the perpetrator is a man and the victim is a woman. This event can lead to the election of a female judge and also to harsher sentences imposed by all judges in that parish. In this scenario, the correlation between the behavioral response of judges and the change in gender peer

composition is not causal, but it is driven by the event preceding the election in that location. This sort of a (court-specific) shock calls for conditioning on court-by-year fixed effects. Doing so, however, would result in almost no variation to exploit for identification.

We provide a number of robustness analyses which indicate that such location-and-year specific shocks are not the drivers of our results. For example, we show that the results are not sensitive to controlling for the local (parish-level) juvenile and adult crime rates and violence against police in the parish of the courthouse. Similarly, the results are robust to the omission of observations around major judicial elections, limiting the analysis sample to a short time horizon (1998-2008) which reduces the influence of judge turnover resulting from major election cycles, and to the exclusion of sex crimes. Finally, as discussed below, we show that gender peer effects are solely driven by female judges. Absent gendered confounders, the lack of a meaningful impact for male judges suggest that our results are not an artifact of spurious time-varying factors.

5   Results

5.1   Random Assignment and Variation in the Gender Peer Composition

We first examine the validity of random assignment of case files to judges. A typical test for this, in our context, is to run a series of regressions where the proportion of female peers in the courthouse is regressed on juvenile and case characteristics, while controlling for court and year fixed effects and court-specific trends. These results are reported in Appendix Table A1. Each cell represents a separate regression. The point estimates are all small in magnitude and none of them is statistically significant. We also run a single regression where we condition on all juvenile and case characteristics. The p-value for joint significance is 0.47 (reported in the last row of Appendix Table A1).[15]

Because our identification hinges on within-judge variation in the proportion of female peers over time, variation in this variable needs to be confirmed. Table 2 presents information related to variation in the proportion of female peers faced by each judge in each year. The mean and standard deviation of the proportion of female peers in the courthouse are 0.20 and 0.32, respectively. The standard deviation of the change in a judge's gender peer composition from one year to the next is 0.07, corresponding to around 23 percent of the overall variation. A simple variance decomposition exercise (sum of squares) shows that within-judge variation accounts for around 7 percent of the total (within and between judge) variation in the proportion of female peers. Furthermore, the share of judge-year observations experiencing any judge

---

[15]We also experimented with similar randomization tests by dropping trends and controlling for court-by-year fixed effects. The point estimates from these alternative specifications yield the same conclusion about randomization.

turnover in courthouses is 17 percent. Thus, there is non-trivial variation in the composition of judges in courthouses to detect meaningful gender peer effects.

Figure 1 provides additional information about the extent of the variation in the proportion of female peers. The figure displays the distribution of the residuals from a regression where the proportion of female peers is regressed on judge, year, and court fixed effects, court-specific trends and defendant characteristics. Consistent with Table 2, non-negligible identifying variation is displayed in Figure 1, where the standard deviation of the residuals is 0.083. Appendix Figure A1 presents this information by judge gender.

Finally, Figure 2 presents the proportion of female peers, averaged across male and female judges over the years, and we do not observe any compelling evidence for a secular trend.[16] Appendix Figures A2 (a)-(c) plots several other measures related to variation in the gender peer composition.

5.2   Baseline Results and the Threat of Selection Bias

Baseline estimates of the impact of gender composition in the courthouse on judicial decisions are presented in Table 3. Panel A reports the results of the models where the dependent variable is the incarceration decision of judges. The outcome in Panel B is sentence length (including zeros for non-incarcerated juveniles) and we use the IHS transformation. Column (1) reports the results by controlling for judge, year, and court fixed effects and court-specific trends. Column (2) adds juvenile characteristics and offense fixed effects. The results in Panel A indicate a statistically significant point estimate of about 0.10, which implies that a 10 percentage point increase in the proportion of female judges (e.g., a change from being exposed to 1 female and 9 male colleagues in the courthouse to being exposed to 2 female and 8 male colleagues) leads to approximately 1.0 percentage point increase in the probability of incarceration decision made by judges. Taking the incarceration rate of 0.137 from Table 1 as our benchmark, this estimated impact indicates an average increase of 7.3 percent. Panel B shows that an analogous change in share of female judges generates an increase in prison time by about 7 percent (about 6 days relative to sample mean).[17] Appendix Table A2 presents the results by replacing zeros with assigned sentence length for those juveniles who were

---

[16]We observe an increase in the average proportion of female peers for male judges beginning in 2009. This increase is attributable to the election that took place in November 2008. Excluding judicial decisions made after 2008 did not alter the conclusions. These results are reported in Section 5.4.

[17]To put the estimates in perspective we compare our findings to those of previous work that examined the impact of factors unrelated to the merits of the case on judicial decisions. For example, Butcher et al. (2017) show that a one standard deviation change in judge stringency increases the probability of incarceration by 53 and 18 percent for female and male offenders, respectively. Philippe and Ouss (2018) find that facing a jury trial during a period after media coverage of crime incidents increases sentence lengths for juveniles by 7 percent. Finally, Didwania (2022) shows that defendants who are charged by same-gender prosecutors receive sentences that are 8 percent shorter compared to defendants charged by opposite-gender prosecutors.

Analyses of decisions that are influenced by gender exposure include Battaglini et al. (2020) who find that a one-standard deviation increase in a U.S. appellate court judge's exposure to female colleagues on judicial panels leads to a four percentage point increase in the probability that the judge hires a female clerk three years later.

not incarcerated. The estimated effects using this alternative measure of judicial disposition are virtually identical to those reported in Table 3.

As noted in Section 2, if the judge acquits the defendant, there is no sentencing decision to be made. In this case, the case file is purged and the juvenile defendant is treated as if he/she had no contact with the juvenile justice system. This means that the data contain only those individuals who are convicted. We argue that this particular limitation is not likely to be a serious concern in the interpretation of our results for several reasons. First, aggregate annual statistics from the reports of the Louisiana Supreme Court indicate that only around 10 percent of all adjudicated cases were dismissed by juvenile court judges (Louisiana Juvenile Justice and Delinquency Prevention Advisory Board Annual Reports 2005-2010).[18]

Second, we exploit an important feature of institutional structure of the juvenile courts in Louisiana to shed further light on potential sample selection bias. Specifically, prior to the adjudication hearing in which the judge makes a decision on guilt-vs-innocence, a petition hearing takes place. At this petition hearing the district attorney charges the juvenile with a crime. If the defendant pleads not guilty, the case goes to trial, which takes place at a later date. In this case, the date of the adjudication hearing is later than the date of the petition hearing. If, on the other hand, the petition and adjudication dates are the same, this means that the judge has not made a guilty/not guilty decision; instead, the juvenile has pleaded guilty or no contest to the charge filed (Depew et al. 2017). Given that the verdict on guilt vs. innocence is not a relevant margin on judicial dispositions for those who have plead guilty to the charge filed, analyzing only the plead guilty sample may provide valuable insights on the extent of bias. Although we lose more than 60 percent of the original sample, the point estimates from exercise, as shown in the last column of Table 3, are almost identical to our baseline results.

Finally, existing evidence shows that the likelihood of being adjudicated delinquent is far greater for serious crimes (Puzzanchera and Hockenberry 2015). For example, in all U.S. courts with juvenile jurisdiction, 64 percent of petitioned robbery cases were adjudicated while this number is 53 percent for public order offenses. This means that guilty vs. innocence margin is arguably less of a concern for juveniles convicted of serious crimes, thereby detecting a non-trivial statistically significant effect on such offenses may further alleviate concerns on sample selection bias. To this end, we examined gender peer effect by severity of crimes (felony and non-felony crimes). The point estimates on the share of female judges in incarceration regressions are 0.124 (s.e=0.088) and 0.058 (s.e=0.043) for felony and non-felony offenses, respectively. These same effects on sentence length are 0.941 (s.e=0.643) and 0.388 (s.e=0.282).

---

[18]The dismissal rate by juvenile court judges is about 12 percent among case files in which the juvenile pleads not guilty.

5.3 Other Observable Peer Characteristics and Judicial Decisions

In this section, we examine other observable peer characteristics that change exogenously and thus unlikely to be subject to the reflection problem: race, age and party affiliation. Apart from providing further insights on social interactions in the courthouse, extending the analysis beyond gender allows us to test whether judges were responding to some other peer characteristics that were systematically correlated with gender. For example, female judges may more likely be black or white, and it could be the race of the peers rather than their gender that may be the driver of observed peer effects in Table 3.

Table 4 reports these results. As shown in the first column, the coefficient of black peers is not significantly different from zero in the incarceration regression and adding the proportion of black peers has no impact on the coefficient of female peers. Similarly, Column (2) displays that the point estimate of the proportion of Democrat judges in the courthouse is small and insignificant. We also do not find any impact of the average age of peers on the incarceration decision made by judges, and conditioning on average peer age in the model does not alter the coefficient of the proportion of female peers (Column 3). Finally, as shown in Column 4, including peers' race, party affiliation and age at the same time to the model has no appreciable impact on the results either. Columns (5) to (8) report the peer effect coefficient estimates from the same exercise by replacing the outcome of interest with sentence length. Once again, other observable peer characteristics do not appear to influence judicial dispositions and gender peer effect is not sensitive to controlling for these peer measures. We further examined the association between average experience of peers (i.e., total years of experience since law school) and judicial dispositions and did not detect any meaningful relationship (available upon request).

5.4 Robustness Checks

We implement several sensitivity checks to examine the robustness of our results. These additional estimations are reported in Appendix Table A3. We first present the effects from a specification where the measure of gender peer composition ($F_{\Box j,ct}$) is constructed at the year-by-quarter level (Column 1).[19] In this model, the proportion of female peers of each judge in a given courthouse is allowed to vary from quarter-to-quarter. The coefficient estimates from this exercise are almost identical to those obtained from the baseline model of Table 3. Second, the turmoil during and after hurricanes Katrina and Rita may have impacted peer group composition and judicial decisions. To investigate this hypothesis, we exclude parishes that are known to be most affected from these natural disasters.[20] As shown in Column (2), doing so provides larger effects

---

[19]We control for quarter fixed effects in this specification.

[20]These parishes are Jefferson, Lafourche, Orleans, Plaquemines, St. Bernard, St. Tammany, and Terrebonne.

for both incarceration and sentence length. Third, recall that we calculate gender composition at the judicial district level in jurisdictions where judges rotate between different parish courthouses. Dropping these parishes reduces the sample size almost by half and lowers the precision, but it does not alter the point estimates (Column 3).[21]

Fourth, recall that in the main analysis we exclude judges if they handled fewer than 25 case files in a given courthouse over the course of the analysis period. Including all judges with any number of dispositions or restricting the sample to judges who handled at least 150 cases produce almost identical results. (Columns 4 and 5, respectively). Fifth, adding first-time juvenile offenders with multiple convictions back to the sample reveals that the results are not very sensitive to this sample restriction either (Column 6).

Sixth, we replace court trends with judge-specific linear trends under the identifying assumption that unobservable variables related to judicial outcomes do not deviate from an individual judge's trend when within-judge variation in the gender composition deviates from trend. The estimated effects, reported in Column 7, are very similar in magnitude to those presented in Table 3.

Seventh, recall that we constructed the peer composition measure using the universe of judges in the district courthouses. Our results remain similar when we define the peer measure only using female judges who handled juvenile cases (Column 8). This is not surprising because, as noted, an overwhelming majority of the judges in these district courthouses handled juvenile cases (77 percent of all judge-by-year observations in the analysis sample). Finally, we drop all judicial decisions made after 2008 to circumvent concerns related to an increase in the trend of average proportion of female peers for male judges (Figure 2). This sample restriction further alleviates potential confounders related to election cycles and long-time horizon because there is only one major election over the period 1998-2008. Doing so does not largely alter our baseline findings (Column 9).

Could the results be driven by local shocks in certain years that may have changed the sensibility of judges and voters? For example, if there is a brutal sex crime with a female victim before an election, voters may end up electing female judges, and judges may become harsher in their decisions because of this crime. To address this concern, we added to the model property crimes and violent crimes committed by juveniles in the parish of the courthouse, property crimes and violent crimes committed by adults, the number of police officers killed or assaulted in the parish, along with other parish attributes such as log of population, fraction of the county population with a high school degree or less and unemployment rate.[22] The results,

---

[21] Pooling all parishes and running fully interacted models produce similar results. The coefficient estimates for incarceration are 0.093 (s.e.=0.059) and 0.013 (s.e.=0.100) on the proportion of female judges and its interaction with rotating districts, respectively. The same coefficient estimates for sentence length are 0.682 (s.e.=0.412) and 0.078 (s.e.=0.678).

[22] The data on local crime come from FBI's Uniform Crime Reporting (UCR) program. UCR provides annual county level property and violent crime statistics for both juveniles and adults, as well as information about officers who were feloniously

reported in Column (1) of Appendix Table A4, show that controlling for the extent of local crime, violence against police and local economic conditions has no impact on the estimated coefficients of the proportion of female peers.

The prevalence of sex offenses may be correlated with judge harshness and the gender composition. Dropping from the sample sex offense cases leaves gender peer effect estimates intact (Column 2 of Appendix Table A4). To further analyze whether the results reflect the impact of a local event that took place before an election (which led to the election of female judges and an increase in harshness following the election) we re-estimated the model after deleting judicial decisions that took place during the year after the election of judges (Column 3 of Appendix Table A4), and after dropping observations in the election year and the year after the election (Column 4).[23] The results remained the same.

To investigate whether the results are driven by the decisions of a particular judge, we estimated equation (1) repeatedly, each time removing dispositions handed down by a different judge. Appendix Figures A3 and A4 plot the distribution of the coefficient estimates for the gender peer effect from a total of 138 regressions. The average of the coefficient estimates for incarceration is 0.098 (s.d=0.005), while it is 0.712 (s.d=0.034) for sentence length, indicating that the results are not driven by a particular judge.

We analyzed the timing of the gender peer effects by augmenting the model with the lags and leads of the gender composition measure. Appendix Table A5 presents these results. Variations in the future values of the peer composition should not influence current decisions of judges, and as Columns (1) and (3) reveal, the coefficient estimates of the leads of the proportion of female peers in courthouse are small and never statistically different from zero. This means that future values of the proportion of female peers have no impact on judges' current decisions on incarceration or sentence length. Columns (2) and (4) report the result from the models that investigate the existence of path-dependence in peer effects by further regressing judicial decisions on current, future and lagged values of the gender peer composition measure. Overall, gender peer effects do not appear to exhibit a persistent pattern over time.

Finally, we estimate the models under placebo values of female peers. Specifically, we consider actual values of the proportion of female peers in each courthouse in each year (pertaining to both female and male judges) and randomly assign these values to different years for the same courthouse. We then run equation (1) and obtain the coefficient estimates of the proportion of female peers and repeat this exercise for 1,000 times. Appendix Figures A5 and A6 display the distribution of the coefficient estimates obtained from this exercise. The vertical line depicts the actual point estimates from Column (2) of Table 3. Only

killed or assaulted while performing their duties. Other parish attributes are drawn from the U.S. Bureau of Labor Statistics.

[23]We considered the major elections of (November) 2002 and 2008. Column (3) drops election years of 2002 and 2008. Column (4) additionally drops post-election years (2003 and 2009).

3 of the 1,000 placebo regressions produce effects that are larger than the actual value in Figure A5 (p-value=0.00), and only 18 placebo estimates in Figure A6 are greater than the corresponding estimate in Table 3 (p-value=0.02).

5.5 Unbundling the Impact

In this section we investigate whether variations in the proportion of female peers in the courthouse has a differential effect on the decisions made by male/female judges on male/female defendants by estimating the following equation

$$D_{ijct} = \beta_0 + \beta_1 F_{\square j,ct} + \beta_2 Female\ Defendant * F_{\square j,ct} + \beta_3 Female\ Judge * F_{\square j,ct}$$

$$+ \beta_4 Female\ Defendant * Female\ Judge + \beta_5 Female\ Defendant * Female\ Judge * F_{\square j,ct} \qquad (2)$$

$$+ X'_{ijct}\beta_6 + \lambda_j + \theta_t + \theta_c + \theta_c t + \epsilon_{ijct}$$

where the impact on the outcome of the change in the proportion of female peers in case of male judge-male defendants is β_1. The impact is $(\beta_1 + \beta_2)$ in case of male judge-female defendant pairs. If the judge is female and the defendant is male, the impact of a change in the proportion of female peers is $(\beta_1 + \beta_3)$, and the impact is $(\beta_1 + \beta_2 + \beta_3 + \beta_5)$ for female judge-female defendant pairs.[24]

Table 5 reports the results from this heterogeneity analysis. Estimated β_1 and β_2 are not significantly different from zero (in rows one and two) in either the incarceration or the sentence length regressions. Similarly, as shown at the bottom section of the table, $(\beta_1 + \beta_2)$ is not significantly different from zero in either regression. This means that there is no compelling evidence of the influence of gender peer composition on male judges. More specifically, the incarceration decision and the sentence length set by male judges do not significantly react to a change in the proportion of their female peers in the courthouse, regardless of whether the defendant is male or female.

The gender peer effect on judicial decisions made by female judges on male defendants is $(\beta_1 + \beta_3)$ and both sums are statistically significantly different from zero in Columns (1) and (2) of Table 5 (with p-values of 0.00 and 0.00, respectively). This indicates that an increase in the proportion of female peers in the courthouse makes female judges more likely to incarcerate male defendants and also causes female judges to impose longer sentences on male defendants. The impact of an increase in the proportion of female

---

[24] These specifications control for interactions of juvenile's gender with individual characteristics, offense and judge fixed effects as well as interactions between judge's gender and individual characteristics and offense fixed effects.

peers on female judges' propensity to incarcerate female defendants is 0.173 ($\beta_1 + \beta_2 + \beta_3 + \beta_5$), and it is significantly different from zero with a p-value of 0.00. Finally, our findings indicate that female judges impose longer sentences on female juvenile defendants (Column 2).[25] Absent gendered confounders, the comparative effects on male and female judges may also provide some assurance as to the credibility of our identification strategy.

Appendix Table A6 summarizes these results in the context of a courthouse consisting of 10 judges, two of whom are female. If the gender composition of judges changes so that the courthouse now has 3 female and 7 male judges, this event increases the proportion of female peers for female judges by 0.11. In response, female judges become 2.6 percentage points more likely to incarcerate male defendants and they impose around 22 days longer sentences to males. The same increase in the proportion of female peers triggers an increase in female judges' proclivity to incarcerate female defendants by 1.9 percentage points and sentence lengths received by female juveniles goes up by around 13 days.

To put gender peer effect in perspective, we provide some back-of-the-envelope calculations by randomly adding one more female judge in one-quarter of all courthouses in Louisiana. We repeat this exercise 1,000 times, each time calculating the difference between simulated and actual average gender peer measures. The mean of the differences from 1,000 simulations indicates a 5.5 percentage points increase in the average proportion of female peers. Using this change along with the estimates reported in Table 5 and the number of case files handled by female judges suggests that such an increase in female peers leads to five additional juveniles incarcerated and an additional 3,827 days of prison time imposed by female judges per year.

5.6   Potential Mechanisms

In this section, we explore the underlying mechanisms by discussing two potential avenues. Before proceeding, it bears noting that the analysis described below is suggestive and that gender peer effects in the courthouse can play out through other channels as well. With this proviso, it is noteworthy that female judges handling juvenile cases are harsher than male judges: their incarceration rate is twice as high that of male judges and they impose longer sentences (as displayed in the bottom rows of Appendix Table A6). It follows that an increase in the proportion of female judges would be associated with a rise in average strictness in judicial decisions in the courthouse. If female judges have the inclination to conform to the norms and customary standards of judicial decision-making, they would become harsher in their own judicial

---

[25] We also experiment with our analysis by excluding court-specific trends from the model, controlling for quadratic court-specific trends, clustering the standard errors at the courthouse level and two-way clustering at the courthouse and judge level. Neither the point estimates nor their statistical significance changes in a meaningful way and the results from these additional robustness checks are available upon request.

decisions as the average harshness goes up in their environment. Consequently, the first hypothesis is that female judges adjust their decisions to conform to evolving judicial stringency, generated by an increase in the proportion of female judges. This explanation is consistent with Abrams et al. (2022) which, using data from North Carolina, find that judges adjust to the existing local norms of sentience harshness when they rotate between judicial districts.

The second hypothesis postulates that female judges do not try and adjust to the changing norms of stringency per se. Instead, female judges are influenced by the sheer presence of their female peers. In other words, female judges alter their behavior simply because they are exposed to more female peers. This second channel resembles the "critical mass" hypothesis, which posits that individuals who are in the minority of a group conform to the behavioral norms of the majority. As the share of minorities in the group goes up, members of the minority group start making decisions that reflect their true tendencies. Examples include females on corporate world, in politics, and in science (Kanter 1977, Dahlerup 1988, Etzkowitz et al. 1994). In our context because female judges are in the minority in a courthouse, they may feel pressure to adopt the incarceration and sentencing behavior of their male peers. When the number of female judges in the group (in the courthouse) goes up, the decision-making of female judges would start reflecting their true personal inclinations in incarceration and sentencing.

To examine these proposed explanations, we calculated the incarceration rate and the sentence length at the judge-by-year level. Table 6 displays the means, weighted by total number of cases in each judge-by-year cell. As expected, Column (1) shows that female judges are harsher than their male peers in their judicial dispositions. The incarceration rate of female judges is almost twice that of male judges (0.22 vs. 0.13) and they impose longer prison time. Columns (2) and (3) of Table 6, however, indicate that the averages reported in Column (1) mask a more subtle picture. Specifically, we divided judges into two groups in each year: those judges who made decisions while facing a proportion of female peers which is lower than the average in that year and judges whose female peers' proportion was greater than the average of that year. Column (2) shows that when the proportion of female peers is low (below the sample mean), female judges are similar to male judges in their harshness. On the other hand, as shown in Column (3), the picture is reversed when female judges face a high proportion (above average) of female peers. The incarceration rate of female judges is almost two-and-a-half times higher (34 percent vs. 14 percent) and the average sentence length imposed by female judges is 98 days longer (190 days vs. 92 days) than their male counterparts. Note also that judicial decisions made by male judges do not change appreciably with the proportion of female peers in the courthouse.

We also test these differences more formally by running regressions of individual level juvenile dispositions

on an indicator for female judge and its interaction with a binary variable for whether the proportion of female peers is greater than the average in that year, while controlling for observable juvenile and judge characteristics and court-by-year fixed effects (unit of randomization). The findings from these specifications confirm the descriptive statistics reported in Table 6. For example, the point estimate on female judge is statistically insignificant 0.072 (s.e.=0.051) when the peer proportion is low, while the female judge coefficient estimate is 0.117 (s.e.=0.054) when female judges face a high proportion (above average) of female peers.

The evidence presented thus far is consistent with both hypotheses described above. In an attempt to further differentiate between these two explanations, we calculated average peer harshness in incarceration and sentencing for each judge. We analyzed the extent to which an increase in average peer harshness and an increase in the proportion of female peers are related to a change in judicial decisions. It is important to note that the coefficient estimates on harshness measures should be interpreted with caution because of the reflection problem (Manski 1993): average harshness of a judge's peers is likely to be endogenous because it could be impacted by the behavior of the judge. With this proviso, Table 7 displays the results. Columns (1) and (3) reproduce our baseline results with non-missing information on peer harshness measures.[26] As shown in Column (2), average incarceration rate of peers is not associated with the incarceration propensity of judges. The point estimate is small in magnitude and statistically indistinguishable from zero and the impact of female peers remains about the same in magnitude and significance. The last column of Table 7 reinforces these findings. Our main takeaway remains intact when we limit the analysis to include only female judges (Panel B). Taken together, these findings are consistent with the conjecture that it is the exposure to female peers which has an impact on judicial decisions, and not exposure to the leniency/harshness of peers.[27]

6   Conclusion

There exists a large literature analyzing whether an individual's peers have an impact on that individual's own behavior and subsequent outcomes. There is, however, paucity of research on whether peers influence a person's decisions and judgments regarding a third party. We try to fill this gap in the literature and

---

[26] The calculation of peer harshness in incarceration and sentencing requires the courthouse to have at least two judges who handled juvenile cases. Thus, those judges who are the only ones in a courthouse who handle juvenile cases cannot be included in this particular analysis. Average peer harshness in incarceration and sentence length are calculated in the same manner as the calculation of the proportion of female peers, as described in Section 4.

[27] This finding provides support to the critical mass hypothesis, although we do not take it literally. More specifically, we do not attempt to identify the location of a "critical proportion of female peers" where the leniency/harshness of female judges are turned on and off. This is because, the response of female judges could be gradual around a critical mass point, rather than exhibiting a discrete jump. Furthermore, even if such a unique threshold existed, its location could depend on the context (e.g., rural vs. urban courthouses and small vs. large courthouses) and judge attributes (e.g., younger judges vs. older judges).

provide evidence on judicial spillovers in the courthouse using administrative criminal justice records linked with judicial turnover records. To avoid a potential reflection problem, we focus on observable judge peer characteristics such as gender, race and age. We exploit movements of judges in and out of courthouse (e.g., retirement, resignation, and death) in conjunction with random assignment of case files to judges to identify second and higher-order causal effects of social interactions in a high-skilled labor market. The most extensive specifications control for judge, court and year fixed effects as well as court-specific trends, observable juvenile characteristics and local crime and economic conditions. To the extent that time-varying unobservable variables impacting juvenile dispositions are not systematically related to within judge variation in the peer composition, our research design yields credible estimates of the contextual peer effects. We provide several robustness checks and various placebo tests supporting our identifying assumption throughout the paper.

Our results show that an increase in the proportion of female peers faced by each judge generates an increase in the severity of punishment both on the extensive and intensive margins. That is, judges are more likely to incarcerate, and they assign longer prison time when the proportion of their female peers in the courthouse goes up. This result is driven by the reaction of female judges. Neither the racial composition of the courthouse, nor the average age of judges or their party affiliation affects judicial decisions. We find that female judges, on average, are more stringent, indicating that an increase in the proportion of female judges in the courthouse is associated with increased average judicial harshness. We present evidence which suggests that the dominant force behind the increase in strictness of female judges is the sheer exposure to female colleagues, rather than a change in the overall leniency/harshness standards. This finding is consistent with recent experimental evidence demonstrating that women's propensity to make risky choices goes up when they are exogenously exposed to all-women classroom environments as compared to coeducation groups (Both et al. 2014). Similarly, using a controlled experiment, Booth and Nolan (2012) find that girls in all-girls groups or attending all girls-schools are more likely than their coed counterparts to choose a real-stakes gamble. These findings, as those in our paper, indicate the importance of social learning.

The welfare implications of our findings are not straightforward for two reasons. First, it is unclear what the optimal level of punishment is. While we report that female judges are harsher than their male counterparts, this information in-and-of itself does not imply that male judges provide sub-optimal level of punishment or that female judges provide excessive punishment. Therefore, that the stringency of female judges goes up as they are exposed to more female peers may be beneficial or detrimental for social welfare. Second, it is complicated to address all potential aspects of social welfare related to judicial decisions and dispositions regarding juvenile punishment may have context-specific impacts.[28] Nevertheless, our results

---

[28] For example, while Aizer and Doyle (2015) report that the severity of juvenile punishment increases the propensity of further

from a simulation exercise suggest an additional five juvenile incarcerations and 3,827 extra days of prion time imposed on juveniles per year.

criminal activity as an adult (adult recidivism) in Cook County/Chicago, Eren and Mocan (2021) show that incarceration as a juvenile in Louisiana has no impact on the propensity to commit a violent crime, but that it increases the propensity of being convicted for a drug crime as an adult.

References

Abrams, D., R. Galbiati, E. Henry, and A. Philippe (2022). When in Rome... On Local Norms and Sentencing Decisions. *Journal of the European Economic Association 20*(2), 700–738.

Abrams, D. S. and A. H. Yoon (2007). The Luck of the Draw: Using Random Case Assignment to Investigate Attorney Ability. *University of Chicago Law Review 74*(4), 1145–1177.

Agan, A. Y., J. L. Doleac, and A. Harvey (2021). Misdemeanor Prosecution. *NBER Working Paper 28600*.

Aizer, A. and J. J. Doyle Jr. (2015). Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges. *Quarterly Journal of Economics 130*(2), 759–803.

Arcidiacono, P., J. Kinsler, and J. Price (2016). Productivity Spillovers in Team Production: Evidence from Professional Basketball. *Journal of Labor Economics 35*(1), 191–225.

Arcidiacono, P. and S. Nicholson (2005). Peer Effects in Medical School. *Journal of Public Economics 89*(2), 327–350.

Bandiera, O., I. Barankay, and I. Rasul (2010). Social Incentives in the Workplace. *Review of Economic Studies 77*(2), 417–458.

Battaglini, M., J. M. Harris, and E. Patacchini (2020). Professional Interactions and Hiring Decisions: Evidence from the Federal Judiciary. *NBER Working Paper 26726*.

Ben-Ner, A., L. Putterman, F. Kong, and D. Magan (2004). Reciprocity in a Two-Part Dictator Game. *Journal of Economic Behavior and Organization 53*(3), 333–352.

Bollinger, B., J. Burkhardt, and K. T. Gillingham (2020). Peer Effects in Residential Water Conservation: Evidence from Migration. *American Economic Journal: Economic Policy 12*(3), 107–133.

Booth, A., L. Cardona-Sosa, and P. Nolen (2014). Gender Differences in Risk Aversion: Do Single-Sex Environments Affect Their Development? *Journal of Economic Behavior and Organization 99*, 126–154.

Booth, A. L. and P. Nolen (2012). Gender Differences in Risk Behaviour: Does Nurture Matter? *Economic Journal 122*(558), F56–F78.

Boyd, C. L., L. Epstein, and A. D. Martin (2010). Untangling the Causal Effects of Sex on Judging. *American Journal of Political Science 54*(2), 389–411.

Butcher, K. F., K. H. Park, and A. M. Piehl (2017). Comparing Apples to Oranges: Differences in Women's and Men's Incarceration and Sentencing Outcomes. *Journal of Labor Economics 35*(1), 201–234.

Carrell, S. E., M. Hoekstra, and E. Kuka (2018). The Long-Run Effects of Disruptive Peers. *American Economic Review 108*(11), 3377–3415.

Carrell, S. E., B. I. Sacerdote, and J. E. West (2013). From Natural Variation to Optimal Policy? The Importance of Endogenous Peer Group Formation. *Econometrica 81*(3), 855–882.

Chen, D. L., T. J. Moskowitz, and K. Shue (2016). Decision Making Under the Gambler's Fallacy: Evidence from Asylum Judges, Loan Officers, and Baseball Umpires. *Quarterly Journal of Economics 131*(3), 1181–1242.

Cohen, A. K. *Delinquent Boys: The Culture of the Gang.* Glenco, IL: Free Press.

Croson, R. and N. Buchan (1999). Gender and Culture: International Experimental Evidence from Trust Games. *American Economic Review 89*(2), 386–391.

Croson, R. and U. Gneezy (2009). Gender Differences in Preferences. *Journal of Economic Literature 47*(2), 448–474.

Dahlerup, D. (1988). From a Small to a Large Minority: Women in Scandinavian Politics. *Scandinavian Political Studies 11*(4), 275–298.

Danziger, S., J. Levav, and L. Avnaim-Pesso (2011). Extraneous Factors in Judicial Decisions. *Proceedings of the National Academy of Sciences 108*(17), 6889–6892.

Depew, B., O. Eren, and N. Mocan (2017). Judges, Juveniles, and In-Group Bias. *Journal of Law and Economics 62*(2), 209–239.

Didwania, S. H. (2022). Gender Favoritism among Criminal Prosecutors. *Journal of Law and Economics 65*(1), 77–104.

Eckel, C. C. and P. J. Grossman (1996). The Relative Price of Fairness: Gender Differences in a Punishment Game. *Journal of Economic Behavior and Organization 30*(2), 143–158.

Eckel, C. C. and P. J. Grossman (2008). Differences in the Economic Decisions of Men and Women: Experimental Evidence. In *Handbook of Experimental Economics Results, eds. Charles R. Plott and Vernon L. Smith*, Volume 1, Chapter 57, pp. 509–519. Elsevier.

Eren, O. and N. Mocan (2021). Juvenile Punishment, High School Graduation and Adult Crime: Evidence from Idiosyncratic Judge Harshness. *Review of Economics and Statistics 103*, 34–47.

Etzkowitz, H., C. Kemelgor, M. Neuschatz, B. Uzzi, and J. Alonzo (1994). The Paradox of Critical Mass for Women in Science. *Science 266*(5182), 51–54.

Falk, A. and A. Ichino (2006). Clean Evidence on Peer Effects. *Journal of Labor Economics 24*(1), 39–57.

Field, E., S. Jayachandran, R. Pande, and N. Rigol (2016). Friendship at Work: Can Peer Effects Catalyze Female Entrepreneurship? *American Economic Journal: Economic Policy 8*(2), 125–153.

Guryan, J., K. Kroft, and M. J. Notowidigdo (2009). Peer Effects in the Workplace: Evidence from Random Groupings in Professional Golf Tournaments. *American Economic Journal: Applied Economics 1*(4), 34–68.

Hockenberry, S. and C. Puzzanchera (2015). Juvenile Court Statistics 2013. *National Center for Juvenile Justice*.

Hoxby, C. (2000). Peer Effects in the Classroom: Learning from Gender and Race Variation. *NBER Working Paper 7867*.

Kanter, R. M. (1977). Some Effects of Proportions on Group Life: Skewed Sex Ratios and Responses to Token Women. *American Journal of Sociology 82*(5), 965–990.

Knepper, M. (2018). When the Shadow Is the Substance: Judge Gender and the Outcomes of Workplace Sex Discrimination Cases. *Journal of Labor Economics 36*(3), 623–664.

Lim, C. S. H., J. M. Snyder Jr., and D. Strömberg (2015). The Judge, the Politician, and the Press: Newspaper Coverage and Criminal Sentencing across Electoral Systems. *American Economic Journal: Applied Economics 7*(4), 103–135.

Liu, H. H. (2021). Masked Evaluations: The Role of Gender Homophily. *Journal of Legal Studies 50*(2), 303–330.

Louisiana Children's Code (2012). Louisiana State Legislature-Various Chapters.

Louisiana District Judges Association Periodicals (2000). Guide to the Louisiana Judiciary (1956-2000).

Manski, C. F. (1993). Identification of Endogenous Social Effects: The Reflection Problem. *Review of Economic Studies 60*(3), 531–542.

Mas, A. and E. Moretti (2009). Peers at Work. *American Economic Review 99*(1), 112–145.

Moffitt, R. (2001). Policy Interventions, Low-Level Equilibria, and Social Interactions. In *Social Dynamics, eds. Steven N. Durlauf and H. Peyton Young*, pp. 45–82. Cambridge, MA: MIT Press.

Niederle, M. (2017). Gender. In J. H. Kagel and A. E. Roth (Eds.), *Handbook of Experimental Economics*, pp. 481–562. Princeton University Press.

Niederle, M. and L. Vesterlund (2007). Do Women Shy Away From Competition? Do Men Compete Too Much? *Quarterly Journal of Economics 122*(3), 1067–1101.

Peresie, J. L. (2005). Female Judges Matter: Gender and Collegial Decisionmaking in the Federal Appellate Courts. *Yale Law Journal 114*(7), 1759–1790.

Philippe, A. and A. Ouss (2018). No Hatred or Malice, Fear or Affection: Media and Sentencing. *Journal of Political Economy 126*(5), 2134–2178.

Sacerdote, B. (2001). Peer Effects with Random Assignment: Results for Dartmouth Roommates. *Quarterly Journal of Economics 116*(2), 681–704.

The Supreme Court of Louisiana (2012). Annual Reports (1998-2012).

Whitmore, D. (2005). Resource and Peer Impacts on Girls' Academic Achievement: Evidence from a Randomized Experiment. *American Economic Review 95*(2), 199–203.

Table 1: Summary Statistics for Juveniles and Judges

	Mean	SD
Panel A: Juvenile Characteristics		
Incarceration (Secure Custody)	0.137	0.344
Sentence Length	80.55	241.04
Black	0.621	0.485
White	0.363	0.481
Female	0.236	0.425
Age	14.75	1.45
Committed a Felony	0.369	0.483
Sample Size	20,244	
Panel B: Judge Characteristics		
Female	0.239	0.428
Age at Disposition	53.09	8.63
Average Number of Judges in the Court	5.57	3.32
Number of Judges	138	

NOTES: The statistics above reflect our research sample, which consists of first-time juvenile offenders over the period from 1998 to 2012. The sample is restricted to juveniles whose disposition decisions were made in courts where there were at least two regular judges in the beginning-of-year. Sentence length is coded as zero for juvenile offenders who are not incarcerated.

Table 2: Variation Analysis for Identification

	Value
Variation in Peer Measure	
(Unit of Analysis: Judge-by-Year)	
Mean of the Proportion of Female Peers in the Courthouse	0.204
Standard Deviation of the Proportion of Female Peers in the Courthouse	0.324
Standard Deviation Change of the Proportion of Female Peers in the Courthouse from $t\text{-}1$ to t	0.074
Sum of Squares within Judges of the Proportion of Female Peers in the Courthouse (%)	6.89
Sum of Squares across Judges of the Proportion of Female Peers in the Courthouse (%)	93.11
Share of Judge-Year Observations in Courts with Turnover (%)	17.05

NOTES: The statistics above describe the variation in the proportion of female peers in the courthouse which we exploit in subsequent estimations.

Table 3: Estimates of Judge Peer Effects on Incarceration and Sentence Length Imposed by Judges

	Full Sample		Plead Sample
	Coefficient		
	(Standard Error)		
	(1)	(2)	(3)
Panel A: Incarceration			
Proportion of Female Peers in the Court	0.087*	0.098**	0.096
	(0.050)	(0.048)	(0.084)
Panel B: Sentence Length			
Proportion of Female Peers in the Court	0.632*	0.713**	0.730
	(0.345)	(0.334)	(0.589)
Sample Size	20,244	20,244	7,118
Controls:			
Court and Year Fixed Effects	Yes	Yes	Yes
Court-Specific Trends	Yes	Yes	Yes
Judge Fixed Effects	Yes	Yes	Yes
Juvenile Characteristics	No	Yes	Yes
Offense Fixed Effects	No	Yes	Yes

NOTES: The sample consists of courts where there were at least two judges in the beginning-of-year (1998-2012). Sentence length is coded as zero for juvenile offenders who are not incarcerated. The dependent variable in Panel B is the inverse hyperbolic sine of the sentence length. Standard errors are reported in parentheses and are clustered at the judge level. Juvenile controls include indicators for juvenile's gender and race and juvenile's age and its square. Offense fixed effects include indicators for type of offense a juvenile was convicted for (violent, property, drug-related and other offenses).

*significant at 10%, ** significant at 5%.

Table 4: Estimates of Judge Peer Effects and Other Peer Attributes on Incarceration and Sentence Length Imposed by Judges

	Incarceration				Sentence Length			
	Coefficient (Standard Error)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Proportion of Female Peers in the Court	0.089**	0.098**	0.094**	0.087**	0.645**	0.712**	0.675**	0.628**
	(0.042)	(0.048)	(0.042)	(0.037)	(0.288)	(0.333)	(0.284)	(0.255)
Proportion of Black Peers in the Court	0.046			0.043	0.341			0.312
	(0.074)			(0.076)	(0.512)			(0.527)
Proportion of Democrat Peers in the Court		-0.005		-0.007		-0.029		-0.042
		(0.058)		(0.057)		(0.413)		(0.412)
Average Age of Peers in the Court			-0.001	-0.000			-0.005	-0.003
			(0.002)	(0.002)			(0.012)	(0.012)
Sample Size	20,113				20,113			
Controls:								
Court and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-Specific Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Juvenile Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Offense Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: Sentence length is coded as zero for juvenile offenders who are not incarcerated. The dependent variable in Columns 5-8 is the inverse hyperbolic sine of the sentence length. Standard errors are reported in parentheses and are clustered at the judge level. See notes to Table 3 and the text for further details.

** significant at 5%.

Table 5: Estimates of Judge Peer Effects-Judge and Defendant Gender Interactions

	Incarceration	Sentence Length
	Coefficient (Standard Error)	
	(1)	(2)
Proportion of Female Peers in the Court (β_1)	0.026	0.226
	(0.039)	(0.268)
Proportion of Female Peers in the Court*Juvenile is Female (β_2)	-0.003	-0.106
	(0.034)	(0.256)
Proportion of Female Peers in the Court*Judge is Female (β_3)	0.214***	1.520***
	(0.064)	(0.447)
Proportion of Female Peers in the Court*Juvenile is Female*Judge is Female (β_5)	-0.064	-0.634
	(0.087)	(0.668)
p-value $(\beta_1+\beta_2)$	0.62	0.70
p-value $(\beta_1+\beta_3)$	0.00	0.00
p-value $(\beta_1+\beta_2+\beta_3+\beta_5)$	0.00	0.00
Sample Size	20,244	20,244
Controls:		
Court and Year Fixed Effects	Yes	Yes
Court-Specific Trends	Yes	Yes
Judge Fixed Effects	Yes	Yes
Juvenile Characteristics	Yes	Yes
Offense Fixed Effects	Yes	Yes

NOTES: Sentence length is coded as zero for juvenile offenders who are not incarcerated. The dependent variable in Column 2 is the inverse hyperbolic sine of the sentence length. These specifications control for interactions of juvenile's gender with individual characteristics, offense and judge fixed effects as well as interactions between judge's gender, individual characteristics and offense fixed effects. Standard errors are reported in parentheses and are clustered at the judge level. See notes to Tables 3 and the text for further details.

*** significant at 1%.

Table 6: Distribution of Judge Harshness in Incarceration and Sentence Length by Judge's Gender

	Full Sample	Proportion of Female Peers <=Sample Mean	Proportion of Female Peers>Sample Mean
	(1)	(2)	(3)
Panel A: Female Judges (Judge-by-Year)			
Mean of Judge Harshness in Incarceration	0.220	0.090	0.343
Mean of Judge Harshness in Sentence Length	127.40	61.80	189.50
Panel B: Male Judges (Judge-by-Year)			
Mean of Judge Harshness in Incarceration	0.129	0.122	0.141
Mean of Judge Harshness in Sentence Length	93.08	93.608	92.07

NOTES: The entries represent weighted means, where the weights are based on judges' total number of dispositions in each year. The mean proportion of females that are used in columns (2) and (3) are calculated separately for each year in the sample. See notes to Tables 3 and the text for further details.

Table 7: Estimates of Judge Peer Effects-Controlling for Average Peer Harshness of Judges Handling Juvenile Cases

	Incarceration		Sentence Length	
	Coefficient (Standard Error)			
	(1)	(2)	(3)	(4)
Panel A: Full Sample (N=17,388)				
Proportion of Female Peers in the Court	0.090* (0.053)	0.092* (0.056)	0.655* (0.368)	0.629* (0.380)
Average Judge Peer Harshness in Incarceration		0.032 (0.172)		
Average Judge Peer Harshness in Sentence Length/100				-0.164* (0.097)
Panel B: Female Judge Sample (N=5,356)				
Proportion of Female Peers in the Court	0.392*** (0.043)	0.394*** (0.042)	2.654*** (0.322)	2.662*** (0.321)
Average Judge Peer Harshness in Incarceration		-0.157 (0.261)		
Average Judge Peer Harshness in Sentence Length/100				-0.084 (0.300)
Controls:				
Court and Year Fixed Effects	Yes	Yes	Yes	Yes
Court-Specific Trends	Yes	Yes	Yes	Yes
Judge Fixed Effects	Yes	Yes	Yes	Yes
Juvenile Characteristics	Yes	Yes	Yes	Yes
Offense Fixed Effects	Yes	Yes	Yes	Yes

NOTES: Sentence length is coded as zero for juvenile offenders who are not incarcerated. The dependent variable in Columns 3-4 is the inverse hyperbolic sine of the sentence length. Standard errors are reported in parentheses and are clustered at the judge level. N represents the sample sizes. See the text for further details.

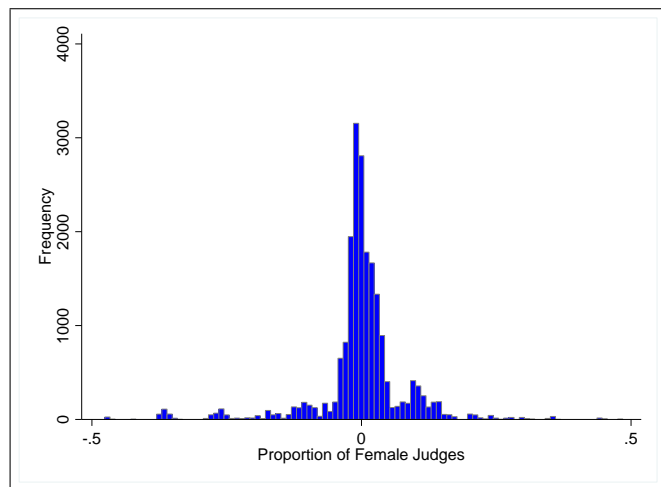*significant at 10%, *** significant at 1%.

Figure 1: Distribution of Proportion of Female Peers-Residualized

NOTES: The residuals are obtained from a regression of proportion of female peers on judge, year and court fixed effects, court-specific trends and defendant characteristics.
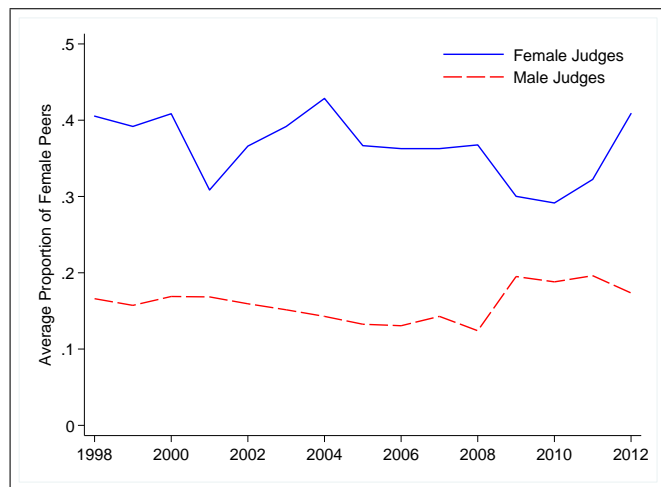
Figure 2: Average Proportion of Female Peers by Judge's Gender

NOTES: The analysis sample consists of courthouses where there were at least two judges in the beginning-of-year (1998 to 2012).

Table A1: Randomization Tests for Judge Peer Effects

Dependent Variable: (Proportion of Female Peers in the Courthouse)	Coefficient (Standard Error)
Female	0.003
	(0.003)
White	0.000
	(0.002)
Juvenile Age	-0.000
	(0.001)
Offense Types:	
Violent	-0.007
	(0.005)
Property	-0.002
	(0.004)
Drug	0.000
	(0.003)
Felony	-0.004
	(0.004)
Joint Significance (*p-value*)	0.47
Sample Size	20,244

NOTES: Standard errors are clustered at the court level. The sample consists of courts where there were at least two judges in the beginning-of-year (1998-2012). Each cell represents a separate regression of the proportion of female peers on juvenile characteristics and offense types. Randomization regressions control for court and year fixed effects and court-specific trends. See the text for further details.

Table A2: Estimates of Judge Peer Effects on Assigned Sentence Length by Judges

	Full Sample	
	Coefficient	
	(Standard Error)	
	(1)	(2)
Assigned Sentence Length		
Proportion of Female Peers in the Court	62.530*	67.802**
	(36.708)	(33.708)
Mean Outcome	507.16	
Sample Size	20,244	20,244
Controls:		
Court and Year Fixed Effects	Yes	Yes
Court-Specific Trends	Yes	Yes
Judge Fixed Effects	Yes	Yes
Juvenile Characteristics	No	Yes
Offense Fixed Effects	No	Yes

NOTES: The dependent variable is the assigned sentence length, irrespective of the type of custody in which juveniles are placed. Standard errors are reported in parentheses and are clustered at the judge level.
*significant at 10%, ** significant at 5%.

Table A3: Robustness Checks-Judge Peer Effects

	Alternative Peer Measure Using Quarterly Variation	Drop Hurricane Katrina/Rita Regions	Judicial Districts Serving Single Parishes (Courthouses)	All Judges (Case Files Judge Handled>0)	Case Files Judge Handled>=150 in the Same Court
			Coefficient (Standard Error)		
	(1)	(2)	(3)	(4)	(5)
Panel A: Incarceration					
Proportion of Female Peers in the Court	0.098**	0.160***	0.095	0.094**	0.096*
	(0.050)	(0.049)	(0.063)	(0.045)	(0.050)
Mean of Outcome	0.137	0.122	0.168	0.137	0.141
Panel B: Sentence Length					
Proportion of Female Peers in the Court	0.702**	1.134***	0.696	0.680**	0.699**
	(0.349)	(0.345)	(0.434)	(0.313)	(0.343)
Mean of Outcome	80.58	77.08	96.67	80.53	82.12
Sample Size	20,216	15,212	12,755	20,647	18,020
Controls:					
Court and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Court-Specific Trends	Yes	Yes	Yes	Yes	Yes
Judge Fixed Effects	Yes	Yes	Yes	Yes	Yes
Juvenile Characteristics	Yes	Yes	Yes	Yes	Yes
Offense Fixed Effects	Yes	Yes	Yes	Yes	Yes

Table A3 cont.

	Juvenile Offenders with Multiple Convictions Added	Control Judge-Specific Trends	Alternative Peer Measure-Using Judges Dealing with Juv. Cases	Case Disposition Year<=2008
	Coefficient (Standard Error)			
	(6)	(7)	(8)	(9)
Panel A: Incarceration				
Proportion of Female Peers in the Court	0.108**	0.096*	0.126***	0.137**
	(0.052)	(0.055)	(0.049)	(0.069)
Mean of Outcome	0.148	0.137	0.137	0.150
Panel B: Sentence Length				
Proportion of Female Peers in the Court	0.782**	0.707*	0.904***	0.994**
	(0.372)	(0.380)	(0.334)	(0.467)
Mean of Outcome	91.05	80.55	80.55	88.71
Sample Size	23,015	20,244	20,244	15,114
Controls:				
Court and Year Fixed Effects	Yes	Yes	Yes	Yes
Court-Specific Trends	Yes	Yes	Yes	Yes
Judge Fixed Effects	Yes	Yes	Yes	Yes
Juvenile Characteristics	Yes	Yes	Yes	Yes
Offense Fixed Effects	Yes	Yes	Yes	Yes

NOTES: Sentence length is coded as zero for juvenile offenders who are not incarcerated. The dependent variable in Panel B is the inverse hyperbolic sine of the sentence length. Standard errors are reported in parentheses and are clustered at the judge level. Peer measure in Column 1 is constructed using quarterly variation in the proportion of female peers. Column 2 excludes parishes which were affected from the Hurricane Katrina/Rita (Orleans, Jefferson, St. Tammany, Plaquemines, St. Bernard, Lafourche, and Terrebonne). Column 3 excludes judicial districts presiding over multiple parishes (courthouses). Column 4 extends the effective sample to include juveniles without imposing any restrictions on the number of dispositions made by judges, while Column 5 limits the effective sample to include juveniles whose dispositions were made by judges with at least 150 cases in the same courthouse over the period from 1998 to 2012. Column 6 includes first-time offenders who were convicted for more than one statute offenses. Column 7 replaces court trends with judge-specific linear trends, while Column 8 constructs the peer measure using judges dealing with juvenile cases. The last columns excludes dispositions made after 2008.

*significant at 10%, ** significant at 5%, *** significant at 1%.

Table A4: Robustness Checks cont.-Judge Peer Effects

	Include Parish Level Crime/Other Controls	Drop Sex Offenses	Drop Years After the Major Elections	Drop Major Election Years and the Years After Elections
		Coefficient (Standard Error)		
	(1)	(2)	(3)	(4)
Panel A: Incarceration				
Proportion of Female Peers in the Court	0.111***	0.100**	0.099*	0.085**
	(0.042)	(0.045)	(0.052)	(0.042)
Total Juvenile Violent Crimes in the Parish/100	-0.008			
	(0.022)			
Total Juvenile Property Crimes in the Parish/100	0.020***			
	(0.004)			
Total Officers Killed/Assaulted in the Parish/100	-0.003			
	(0.003)			
Mean of Outcome	0.137	0.133	0.139	0.137
Panel B: Sentence Length				
Proportion of Female Peers in the Court	0.793***	0.730**	0.723**	0.603**
	(0.293)	(0.311)	(0.362)	(0.290)
Total Juvenile Violent Crimes in the Parish/100	-0.082			
	(0.149)			
Total Juvenile Property Crimes in the Parish/100	0.151***			
	(0.034)			
Total Officers Killed/Assaulted in the Parish/100	-0.017			
	(0.024)			
Mean of Outcome	80.55	76.06	82.99	83.34
Mean of Juvenile Property Crimes	111.56			
Mean of Officers Killed/Assaulted	45.46			
Sample Size	20,244	19,561	17,197	14,491
Controls:				
Court and Year Fixed Effects	Yes	Yes	Yes	Yes
Court-Specific Trends	Yes	Yes	Yes	Yes
Judge Fixed Effects	Yes	Yes	Yes	Yes
Juvenile Characteristics	Yes	Yes	Yes	Yes
Offense Fixed Effects	Yes	Yes	Yes	Yes

NOTES: Sentence length is coded as zero for juvenile offenders who are not incarcerated. The dependent variable in Panel B is the inverse hyperbolic sine of the sentence length. Standard errors are reported in parentheses and are clustered at the judge level. Column 1 includes the following parish level controls: number of adult property and violent crimes, number of juvenile property and violent crimes, number of officers feloniously killed or assaulted, log of county population, unemployment rate and fraction of the county population with a high school degree or less. Column 2 drops sex crime convictions. Column 3 drops judicial decisions that took place during the year of the major elections (2002 and 2008), while the last column drops observations in the election year and the year after the elections (2002, 2003, 2008 and 2009).

*significant at 10%, ** significant at 5%, *** significant at 1%.

Table A5: Timing of Judge Peer Effects and Falsification Test

	Incarceration		Sentence Length	
	Coefficient			
	(Standard Error)			
	(1)	(2)	(3)	(4)
Proportion of Female Peers in the Court	0.117**	0.112**	0.848**	0.734**
	(0.055)	(0.051)	(0.389)	(0.356)
Proportion of Female Peers in the Court *(t+1)*	0.017	-0.024	0.108	-0.172
	(0.023)	(0.028)	(0.163)	(0.199)
Proportion of Female Peers in the Court *(t-1)*		-0.007		0.056
		(0.044)		(0.307)
Mean of Outcome	0.140	0.137	82.60	80.35
Sample Size	19,049	16,988	19,049	16,988
Controls:				
Court and Year Fixed Effects	Yes	Yes	Yes	Yes
Court-Specific Trends	Yes	Yes	Yes	Yes
Judge Fixed Effects	Yes	Yes	Yes	Yes
Juvenile Characteristics	Yes	Yes	Yes	Yes
Offense Fixed Effects	Yes	Yes	Yes	Yes

NOTES: Sentence length is coded as zero for juvenile offenders who are not incarcerated. The dependent variable in Columns 3 and 4 is the inverse hyperbolic sine of the sentence length. Standard errors are reported in parentheses and are clustered at the judge level. See Table 3 and the text for further details.

** significant at 5%.

Table A6: Simulation Exercise-The Impact of Replacing One Male Judge with One Female Judge in a Courthouse of Two Female and Eight Male Judges (10 Judges in Total)

Judge-Juvenile Defendant:	The Impact on Incarceration	The Impact on Sentence Length
	(1)	(2)
Female Judge-Female Juvenile Defendant (N=1,555)	1.9 pp. (9.3%)	12.6 days (11.1%)
Female Judge-Male Juvenile Defendant (N=5,068)	2.6 pp. (12.7%)	21.7 days (19.2%)
Male Judge-Female Juvenile Defendant (N=3,230)	insignificant	insignificant
Male Judge-Male Juvenile Defendant (N=10,391)	insignificant	insignificant
Female Judge's Incarceration Rate/ Average Sentencing	0.205	113.22
Male Judge's Incarceration Rate/Average Sentencing	0.104	64.67

NOTES: The percentage values in parantheses represent changes relative to the average judge-gender specific dispositions. N is the sample sizes. See Table 5 and the text for further details.

Appendix:

Panel A: Female Judges
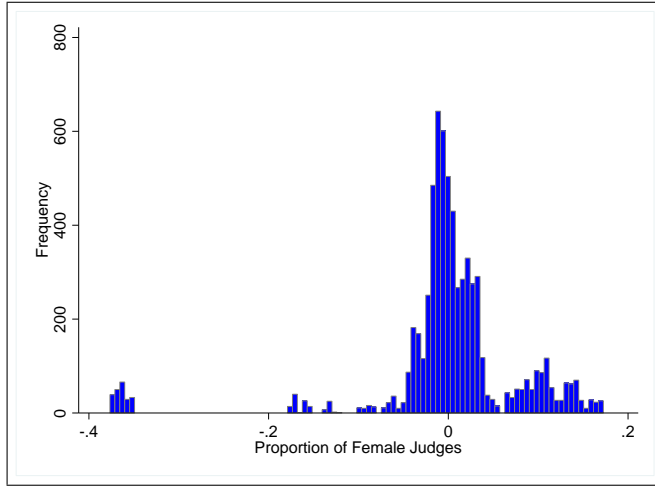
Panel B: Male Judges
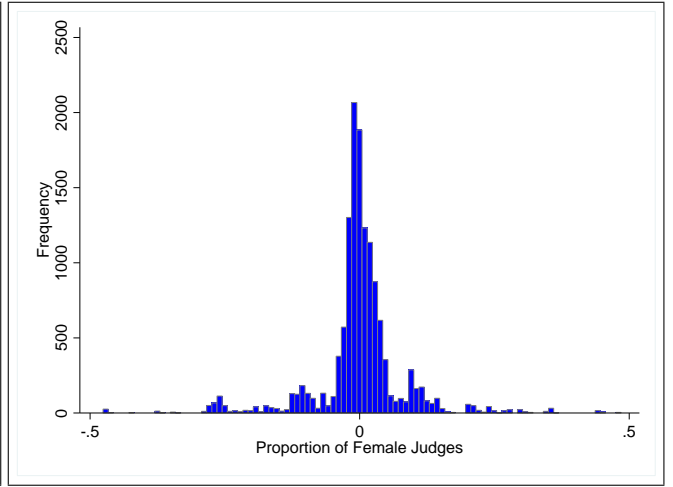
Figure A1: Distribution of Proportion of Female Peers-by Judge's Gender-Residualized

NOTES: The residuals are obtained from a regression of proportion of female peers on judge, year and court fixed effects, court-specific trends and juvenile characteristics.
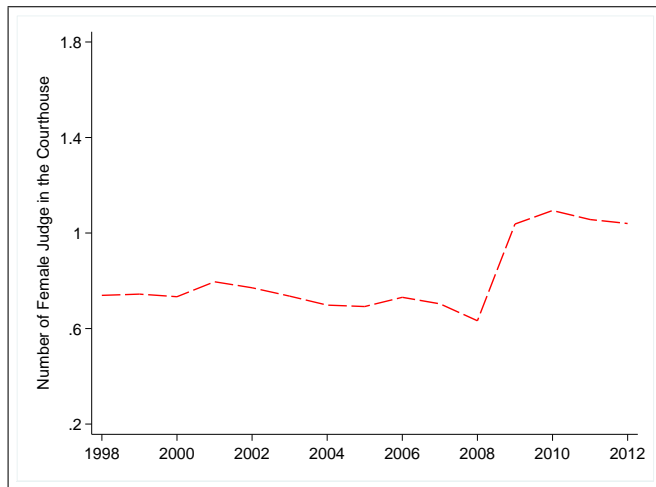
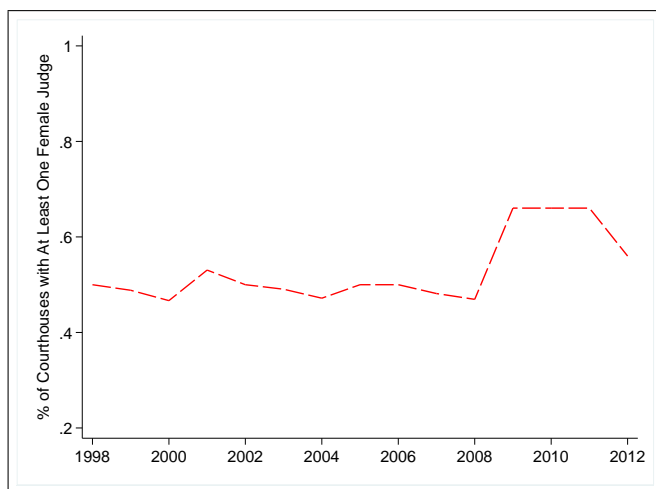Figure A2(a): Number of Female Judges in the Courthouse

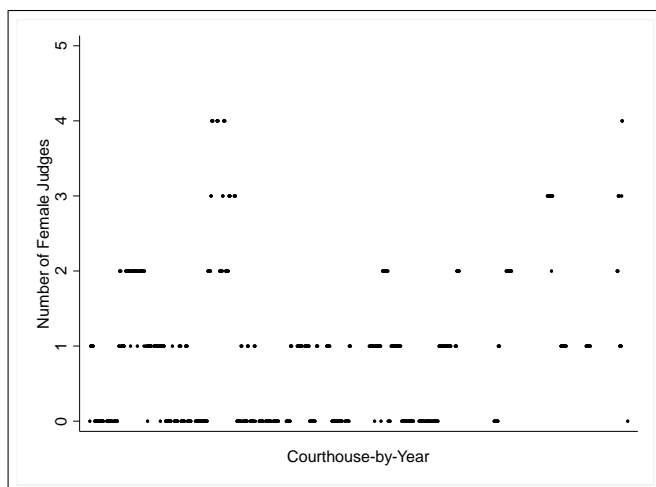Figure A2(b): Fraction of Courthouses with at Least One Female Judge

Figure A2(c): Number of Female Judges-Courthouse-by-Year

NOTES: The analysis sample consists of courthouses where there were at least two judges in the beginning-of-year (1998 to 2012).
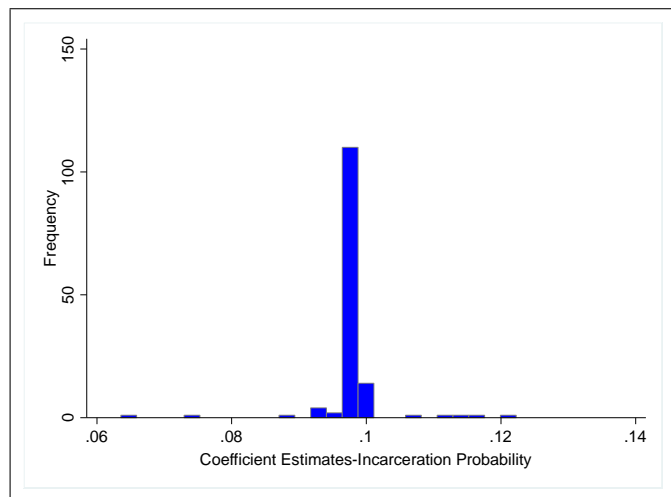
Figure A3: Distribution of Leave-One Judge Out Estimates of the Gender Peer Effects in the Courthouse on Incarceration

NOTES: The distribution of the coefficient estimates of the proportion of female peers in the courthouse on incarceration decision set by the judges. The baseline specification is estimated repeatedly, each time removing dispositions set by a different judge. There are 138 judges in the effective sample.
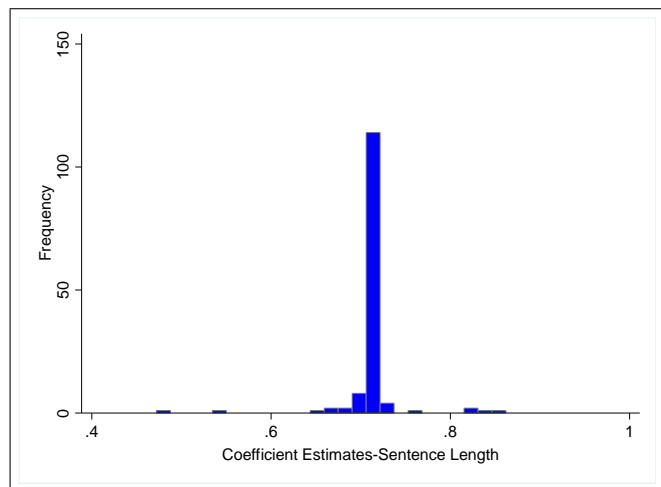
Figure A4: Distribution of Leave-One Judge Out Estimates of the Gender Peer Effects in the Courthouse on Sentence Length

NOTES: The distribution of the coefficient estimates of the proportion of female peers in the courthouse on sentence length set by the judges. The baseline specification is estimated repeatedly, each time removing dispositions set by a different judge. There are 138 judges in the effective sample.
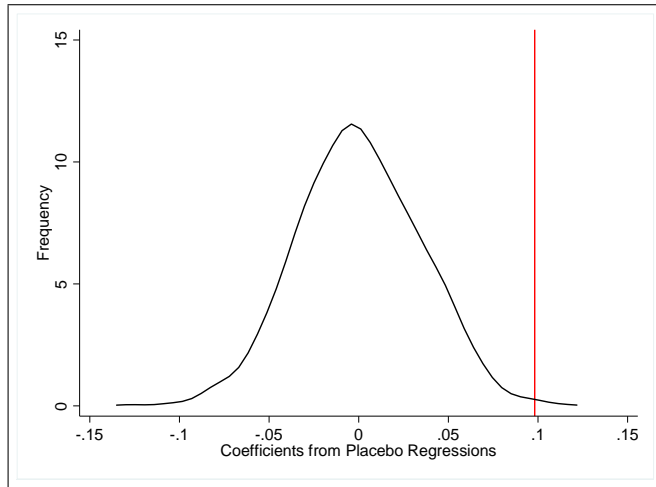
Figure A5: Placebo Coefficients of the Proportion of Female Peers in Incarceration Regression

NOTES: The figure displays the distribution of placebo coefficients of the proportion of female peers, where the proportions of female peers of a courthouse are randomly assigned to different years of the same courthouse. The vertical line represents the actual point estimate reported in Column 2 of Table 3.
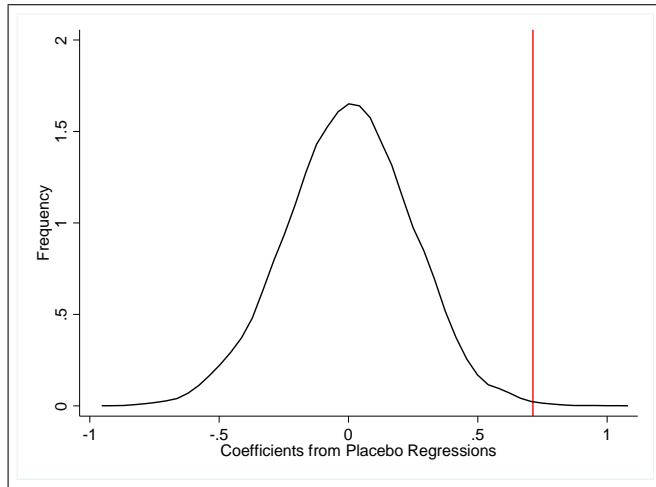
Figure A6: Placebo Coefficients of the Proportion of Female Peers in Sentence Length Regression

NOTES: The figure displays the distribution of placebo coefficients of the proportion of female peers, where the proportions of female peers of a courthouse are randomly assigned to different years of the same courthouse. The vertical line represents the actual point estimate reported in Column 2 of Table 3.