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Elliott Ash, Sam Asher, Aditi Bhowmick, Sandeep Bhupatiraju, Daniel L. Chen, Tatanya Devi, Christoph Goessmann, Paul Novosad and Bilal Siddiqi

Toulouse School of Economics

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Elliott Ash, Sam Asher, Aditi Bhowmick,
Sandeep Bhupatiraju, Daniel Chen, Tanaya Devi,
Christoph Goessmann, Paul Novosad, Bilal Siddiqi*

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Abstract

We study judicial in-group bias in Indian criminal courts, collecting data on over 80 million legal case records from 2010–2018. We exploit quasi-random assignment of judges and changes in judge cohorts to examine whether defendant outcomes are affected by being assigned to a judge with a similar religious or gender identity. We estimate tight zero effects of in-group bias. The upper end of our 95% confidence interval rejects effect sizes that are one-fifth of those in most of the prior literature.

JEL codes: J15, J16, K4, O12

1 Introduction

Structural inequalities across groups defined by gender, religion, and ethnicity are seen in almost all societies. Governments often try to remedy these inequalities through

*Author Details: Ash, ETH Zurich: ashe@ethz.ch; Asher, John Hopkins: sasher2@jhu.edu; Bhowmick, Development Data Lab: bhowmick@devdatalab.org; Bhupatiraju: World Bank: sbhupatiraju@worldbank.org; Chen, Toulouse and World Bank: daniel.chen@iast.fr; Devi, Harvard: tdevi@g.harvard.edu; Goessmann, ETH Zurich: christoph.goessmann@gess.ethz.ch; Novosad, Dartmouth College: paul.novosad@dartmouth.edu; Siddiqi, UC Berkeley: bilal.siddiqi@berkeley.edu. We thank Alison Campion, Rebecca Cai, Nikhitha Cheeti, Kritarth Jha, Romina Jafarian, Ornelie Manzambi, Chetana Sabnis, and Jonathan Tan for helpful research assistance. We thank the World Bank Program on Data and Evidence for Justice Reform and the UC Berkeley Center for Effective Global Action for financial support.

policies, such as anti-discrimination statutes or affirmative action, which must then be enforced by the legal system. A challenging problem is that the legal system itself may have unequal representation. It remains an open question whether legal systems in developing countries are effective at pushing back against structural inequality or whether they serve to entrench it (e.g. Aldashev et al., 2012).

This paper examines bias in India's lower courts, asking whether judges deliver more favorable treatment to defendants who match their identities. Judicial bias along gender, religious, or ethnic lines appears to be nearly universal in richer countries, having been identified in a wide range of settings around the world.[1] However, it has not been widely studied in the courts of lower-income countries. In-group bias of this form has been identified in other contexts in India, such as among loan officers (Fisman et al., 2020) and school-teachers (Hanna and Linden, 2012). The judicial setting is of particular interest, given the premise that individuals who are discriminated against in informal settings should receive equal treatment under the law (Sandefur and Siddiqi, 2015).

We focus on the dimensions of gender and religion in India's lower courts, where unequal representation is a recognized issue. Women represent 48% of the Indian population but only 28% of district court judges. Similarly, India's 200 million Muslims represent 14% of the population but only 7% of lower court judges. There is growing evidence that India's Muslims and women do not enjoy equal access to economic or other opportunities (Ito, 2009; Bertrand et al., 2010; Hnatkovska et al., 2012; Hanna and Linden, 2012; Jayachandran, 2015; Borker, 2017; Asher et al., 2020). We examine whether unequal representation in the courts has a direct effect on the judicial outcomes of Muslims and women, in the form of judges delivering better outcomes to criminal defendants who match their gender or religion.

Our analysis draws upon a new dataset of 80 million court records covering 2010–2018 from `https://ecourts.gov.in/`, an online platform documenting the complete set of cases heard in India's district courts.These cases cover the universe of India's 7,000+ district and subordinate trial courts, staffed by over 80,000 judges. We are releasing anonymized data on these cases, opening the door to many new analyses of the judicial process in the world's largest democracy and largest common-law legal system.

We enrich the dataset by classifying judges and defendants to gender and religious

[1] See, for example, Shayo and Zussman (2011), Didwania (2018), Arnold et al. (2017), Abrams et al. (2012), Alesina and La Ferrara (2014),(Anwar et al., 2019) and others below.

(Muslim and non-Muslim) identity groups based on their names. An automated process uses a deep neural network applied to the sequence of characters in names. The distinctive nature of female and Muslim names allows us to classify individuals with over 97% out-of-sample accuracy on both dimensions.[2]

The main research question is whether judges tend to treat defendants differently when they share the same gender or religion. We focus on the subset of cases filed under India's criminal codes ($N = 5.5$ million), where acquittal and conviction rates, as well as judicial delay, are readily interpretable as positive or negative outcomes. We implement two different identification strategies to generate causal estimates of how judge identity affects a defendant's outcome.

First, we exploit the arbitrary rules by which cases are assigned to judges, generating as-good-as-random variation in judge identity. Our preferred specification includes court, charge, and month-year fixed effects. Effectively, we compare the outcomes of two defendants with the same identity classification, charged under the same criminal section, in the same court and in the same month, but who are assigned to judges with different identities.

Second, we exploit judicial turnover events that change the gender and religion balance of judges serving in a district court, exogenously changing the probability that a defendant matches identity with the judge overseeing their case. We use a regression discontinuity specification which measures the difference in judicial outcomes for defendants whose cases are heard immediately before and immediately after a transition that makes the bench more or less similar along identity dimensions.

In both of these specifications, we find a robust null estimate of in-group bias among Indian judges. Judges of different genders do not treat defendants differently according to their gender, nor do judges display favoritism on the basis of religion. This is true both in terms of outcomes (i.e. acquittals and convictions) and in terms of process (i.e. speed of decision). In a subset of specifications, we find a very small in-group gender bias, which is marginally positive and not robust. However, the size of this effect, even in the marginally significant specifications, is an order of magnitude smaller than nearly all prior estimates of in-group bias based on similar identification strategies in the literature.[3] The upper end of our 95% confidence interval rejects a 0.7 percentage

[2]We do not examine bias on the dimensions of income or caste because we do not yet have an algorithm that can classify these dimensions with high accuracy.

[3]The exception is Lim et al. (2016), who find zero effects of in-group gender bias and marginal effects of in-group racial bias among judges in Texas state district courts.

point effect size in the worst case; studies using the same identification strategies in other contexts have routinely found bias effects ranging from 5 to 20 percentage points.

Our analysis largely excludes questions of caste, which remains a major cleavage in Indian society. Caste identity is multidimensional when compared with religious and gender identity, making it more difficult to identify clear ingroups and outgroups. It is also more difficult to classify individuals to caste on the basis of their names. To test ingroup bias among caste, we define the ingroup as the set of defendants who match a judge's last name; this is an imperfect measure because multiple family names may reflect the same caste. Nevertheless, the rate of Type I errors is small: individuals with the same last name are likely to be in the same social group. This is a measure of identity that does predict affinity, for instance in the banking setting (Fisman et al., 2017). We find a small positive ingroup bias; defendants assigned to judges with their same last name are 2 percentage points more likely to be acquitted; the effect remains small in comparison with other bias studies.

Our estimates do not rule out bias in the Indian legal system entirely; we observe only a subset of the legal process and we measure only in-group bias by gender and religion. For example, it is possible that both Muslim and non-Muslim judges discriminate against Muslims (as found for Black defendants in Arnold et al. (2017)). It is also possible that arrests and/or charges disproportionately target Muslims, or that judges exhibit bias based on defendant caste or income. However, the bias that we study has been widely reported in other studies with large effect sizes, and the public discussion of discrimination against Muslims and women in India in many ways parallels discussion of marginalized groups in other countries.

We find no average bias or differential treatment. However, it is possible that bias is activated in certain subsets of cases where judge and defendant identity are activated by external circumstances or features of the case. We examine three special contexts where the literature suggests that ingroup bias may be more likely to be activated. First, we examine cases where the plaintiff and the victim of the crime have different identities; in this case, the judge will have an identity matching either the victim or the plaintiff, but not both (ref). Second, we examine gender bias in the criminal sections categorized as crimes against women, which are mostly sexual assaults and kidnappings. Third, we examine whether ingroup bias on the basis of religion is activated during the month of Ramadan, when religion may become more salient for both Muslims and non-Muslims.

We find no evidence that ingroup bias is activated in cases where the plaintiff and defendant are in different groups; nor do we find evidence of differential behavior of female judges in cases classified as crimes against women. We do find that religious ingroup bias is activated during the month of Ramadan; Muslim defendants assigned to Muslim judges have a 2.0 percentage point higher acquittal rate during the month of Ramadan (p=0.09).[4] The data suggest that this is driven by changed behavior on the part of non-Muslim rather than of Muslim judges, but we cannot make this latter claim conclusively because it is possible that cases being heard during Ramadan have different properties than cases heard at other times of the year. This point estimate remains small compared with much of the prior literature on ingroup bias. The results confirms that district judges have discretion and may apply it in favor of an ingroup if their identity is activated, but in most cases, most of the time, the extent of ingroup bias experienced by defendants in this context is small or zero.

Relative to the prior literature, we make several contributions. First, we demonstrate an absence of bias in an important context with substantial religious and gender cleavages. Second, the sample of our study is an order of magnitude larger than earlier studies, allowing us to measure bias much more precisely than prior work. Third, to our knowledge this is the first large-scale study of judicial bias in a low- or middle-income country and it makes available a dataset which may have substantial utility to future scholars.

These results add to a literature on biased decision-making in the legal system. Most prior work is on the U.S. legal system, where disparities have been documented at many levels.[5] The closest paper to ours is Shayo and Zussman (2011), who analyze the effect

[4]This result is considerably smaller than **?**, who find in Pakistan that conviction rates fall by 14 percentage points during Ramadan, or 1 percentage point for each additional hour of fasting. In their study, nearly all judges and plaintiffs are Muslim, so the effect of identity is playing a different role. Note that we do not exploit differences in daylight hours because there is little variation in the timing of Ramadan across the 8 years in our study.

[5]These include racial disparities in the execution of stop-and-frisk programs (Goel et al., 2016), motor vehicle searches by police troopers (Anwar and Fang, 2006), bail decisions (Arnold et al., 2017), charge decisions (Rehavi and Starr, 2014), and judge sentence decisions (Mustard, 2001; Abrams et al., 2012; Alesina and La Ferrara, 2014; Kastellec, 2013). African-American judges have been found to vote differently from Caucasian-American judges on issues where minorities are disproportionately affected, such as affirmative action, racial harassment, unions, and search and seizure cases (Scherer, 2004; Chew and Kelley, 2008; Kastellec, 2011). In a similar manner, a number of papers have documented the effect of judges' gender in sexual harassment cases (Boyd et al., 2010; Peresie, 2005). A smaller set of papers use information on both the identity of the defendant and the decision-maker. Anwar et al. (2012) look at random variation in the jury pool and find that having a black juror in the pool decreases conviction rates for black defendants. A similar result from Israel is documented by Grossman et al. (2016), who find that the effect of including even one Arab judge on the decision-making panel substantially influences trial outcomes of Arab defendants. Didwania (2018) find in-group bias in that prosecutors charge same-gender defendants with less severe offenses.

of assigning a Jewish versus an Arab judge in Israeli small claims court. They find robust evidence of in-group bias, where Jewish judges favor Jewish defendants (and Arab judges favor Arab defendants) by an average 17–19 percentage-point margin, an effect ten times larger than the upper bound of our confidence interval on either religion or gender bias. Several more studies use one of our two identification strategies to generate point estimates that are directly comparable to ours, and of these only Lim et al. (2016) find a null in-group effect of judge ethnicity or gender.[6]

In the Indian context, there is a growing body of evidence on the legal system, mostly focusing on judicial efficacy and economic performance (Chemin, 2009; Rao, 2019), or on corruption in the Indian Supreme Court (Aney et al., 2017; Poblete-Cazenave, 2020). A recent working paper finds that judges are more prone to deny bail if they were exposed to communal riots in their early childhood (Bharti and Roy, 2020). However, we are aware of no prior large-scale empirical research on unequal legal treatment on either the gender or religion dimension in India, a topic of substantial policy relevance.

Beyond the issue of in-group bias, we add to the growing literature on courts in developing countries. Well-functioning courts are widely considered a central component of effective, inclusive institutions, with judicial equity and rule of law seen as key indicators of a country's institutional quality (Rodrik, 2000; Le, 2004; Rodrik, 2005; Pande and Udry, 2006; Visaria, 2009; Lichand and Soares, 2014; Ponticelli and Alencar, 2016; World Bank, 2017). A handful of important cross-country studies have recovered some broad stylized facts on the causes and consequences of different broad features of legal systems (Djankov et al., 2003; La Porta et al., 2004, 2008). But largely due to a lack of data, there has been a relative paucity of within-country court- or case-level research on the delivery of justice in lower-income settings.

The rest of the paper is organized as follows. After outlining the institutional context (Section 2) and data sources (Section 3), we articulate our empirical approach (Section 4). Section 5 reports the results. Section 6 compares the results to the previous literature and concludes.

[6] Gazal-Ayal and Sulitzeanu-Kenan (2010) find positive in-group bias in bail decisions when Arab and Jewish defendants are randomly assigned to a judge of the same ethnicity. Knepper (2018) and Sloane (2019) leverage random assignment of cases in the U.S. to judges and prosecutors respectively, finding significant in-group bias in trial outcomes. Depew et al. (2017) exploit random assignment of judges to juvenile crimes in Louisiana and find *negative* in-group bias in sentence lengths and likelihood of being placed in custody. It is notable that of all these studies, Lim et al. (2016) has one of the largest sample sizes (N=250,000).

2 Background

2.1 Institutional Context

India's population is characterized by cross-cutting divisions between gender and religion. Women's rights and their status in society are under intense political debate. Muslims in India (14% of the population) have historically had intermediate socioeconomic outcomes worse than upper caste groups but better than lower caste groups. However, they have been protected by few of the policies and reservations targeted to Scheduled Castes and Tribes. In recent decades, many successful political parties have been accused of implicitly or explicitly discriminating against Muslims.

Women constitute 48% of the population, and remain vulnerable to precarious social practices such as female infanticide, child marriage, and dowry deaths despite existing legislation outlawing all of the above. Prior to the pandemic, India accounted for one-third of all child marriages globally (Cousins, 2020). As of 2020, India also accounts for nearly one-third of the 142.6 million missing females in the world (Erken et al., 2020). The unambiguously marginalized status of Indian women and Muslims motivates the exploration of the role of gender and religion in the context of India's criminal justice system in this study.

India's judicial system is organized in a jurisdictional hierarchy that is similar to other common-law systems. There is a Supreme Court, 25 state High Courts, and 672 district courts below them. Beneath the district courts, there are about 7000 subordinate courts. The district courts and subordinate courts collectively constitute India's lower judiciary. These courts represent the preliminary point of entry of almost all criminal cases in India.[7]

These courts are staffed by over 81,000 judges. Due to common law institutions where court rulings serve as binding precedent in future cases, judges in India are important policymakers. Indian judges are arguably even more powerful than their U.S. counterparts because they do not share decision authority with juries, which were banned in 1959. Therefore fair and efficient decision-making by judges is an important issue for governance.

There is an active debate in India around reforming the court system. Problems under discussion include a reputation for corruption (Dev, 2019) as well as a substantial

[7]We define criminal cases as all cases filed either under the Indian Penal Code Act or the Code of Criminal Procedure Act.

backlog of cases (Trusts, 2019). In 2015, Prime Minister Modi attempted to implement a series of reforms giving his administration more control over judge selection through the creation of a National Judicial Appointments Commission. However, the effort to move away from the collegium system of judicial appointment was reversed by the Supreme Court, citing breach of judicial independence.

2.2 Case Assignment To Judges

The procedure of case assignment to judges is important for this study, because our main empirical strategy hinges on exogenous assignment of judges to cases. To better understand the case assignment process, we consulted with several criminal lawyers who practice in India's district courts, senior research fellows at the Vidhi Center for Legal Policy, as well as a number of working court clerks in courts around the country.

Criminal cases are assigned to judges as follows. First, a crime is reported at a particular local police station, where a First Information Report (FIR) is filed. Each police station lies within the territorial jurisdiction of a specific district courthouse, which will receive the case. The case will then be assigned to a judge sitting in that courthouse. If there is just one judge working there, that judge will get the case.

When there are multiple judges, a rules-based process fully determines the judge assignment. Each judge sits in a specific courtroom in a court for several months at a time. A courtroom is assigned for every police station and every charge. For example, at a given police station, every murder charge will go to the same courtroom; a larceny charge might go to a different courtroom, as might a murder charge reported at a different police station. Judges typically spend two to three years in a given court, during which they rotate through several of the courtrooms.[8]

The police station-charge lists thus leave little discretion for charges to be seen by specific judges. Since the timing of the first course appearance is unknown when charges are filed (given judicial delays), even if a defendant or prosecutor had discretion over which police station filed the charges, the rotation of judges between courtrooms would make it difficult to target a specific judge. Finally, the judiciary explicitly condemns the practice of "judge shopping" or "forum shopping", where litigants select particular judges seeking a favorable outcome. One of the earliest cases in which the Indian Supreme

[8]Severe cases (with severity defined by the section or act under which the charge was filed) require judges with higher levels of seniority; thus a case in a given district in some cases may be seen only by a subset of judges in that district.

Court condemned the practice of shopping is the case of *M/s Chetak Construction Ltd. v. Om Prakash & Ors.*, 1998(4) SCC 577, where the Court ruled against a litigant trying select a favorable judge, writing that judge shopping "must be crushed with a heavy hand." This decision has been cited heavily in subsequent judgments.

Finally, it should be noted that in the most recent years (since 2013), some courts have adopted a random assignment lottery mechanism implemented through the eCourts platform, making judge selection very unlikely. The eCourts assignment mechanism was intended to be used throughout the country but in practice it has not been widely adopted to date. In Section 4, we present formal tests of the exogenous assignment of judges to cases in our dataset.

2.3 Prevalence of Plea Bargaining

Since the test of in-group bias reported in the present study is based on trial outcomes of criminal cases, it is important to understand if a large section of criminal cases are settled outside the trial court in the form of plea bargaining.

Plea bargaining was introduced in India in the early 2000s. However, less than 0.5% of all criminal cases pending in India have been disposed through plea bargaining. Administrative data collected since 2015 illustrates negligible usage of plea bargaining in India. In 2015, 4,816 cases out of a total number of 10.5 million criminal cases pending for trial were channeled through plea bargaining. The share of cases disposed through plea bargaining was 0.043 % in 2016, 0.27% in 2017, and 0.16% in 2018. This statistic has never breached 1% over the past 15 years. [9]Given the extremely low prevalence of plea bargaining in Indian trial courts, we are able to rule out selection effects introduced by exclusion of cases that are disposed outside the trial court.

2.4 Recruitment of lower court judges

Following a collegium system, the judges of the Indian lower judiciary are appointed by the governor in consultation with the chief justice of the high court of the specific state. The minimum qualification for recruitment of a district judge is at least seven years of practise as a lawyer at bar, in addition to a written examination and oral interview by a panel of high court judges. This is known as direct recruitment and is the primary

[9] http://www.legalserviceindia.com/legal/article-1784-plea-bargaining-in-\ india-a-ship-with-holes.html, accessed March 2 2021

channel of recruitment, as opposed to indirect recruitment - promotion of judges at subordinate courts in the lower judiciary.

A district judge can be promoted to serve as a high court judge if they have completed a specific number of years at their post. However, high court judges are also recruited from pools of lawyers who have practiced at the high court bar. Lower court judges may be removed from their office by the governor provided the high court collegium agrees.

3 Data

3.1 Case Records

We obtained 81.2 million case records from the Indian eCourts platform – a semi-public system put in place by the Indian government as a "national data warehouse for case data including the orders/judgments for courts across the country."[10] The publicly available information includes the filing, registration, hearing, and decision dates for each case, as well as petitioner and respondent names, the position of the presiding judge, the acts and sections under which the case was filed, and the final decision or disposition.[11]

The database covers India's lower judiciary – all courts including and under the jurisdiction of District and Sessions courts. In this paper, we focus on cases filed either under the Indian Penal Code or the Code of Criminal Procedure for two reasons. First, there is only a single litigant, rather than two, providing a clear definition of identity match between judge and defendant. Second, it is relatively straightforward to identify good and bad outcomes for criminal defendants, and much more difficult to do so for litigants in civil cases. This constraint filters out 69% of the dataset, leaving us with 25.2 million criminal case records. The process through which we arrive at our final analysis dataset of approximately 6 million observations has been illustrated in Figure A2, in the Appendix.

[10] https://ecourts.gov.in/ecourts_home/static/about-us.php, accessed Oct 14 2020

[11] We illustrate such a record in Appendix Figure A1.

3.2 Judge Information

We also obtained data on judges pertaining to all courts in the Indian lower judiciary from the eCourts platform. The data for each judge includes the judge's name, their position or designation, and the start and end date of the judge's appointment to each court.[12]

We joined the case-level data with the judge-level data based on the judge's designation and the initial case filing date. In this process, another 17% of the initial observations are dropped. The remaining dataset where cases are linked to a unique judge consists of 11.0 million cases. Further, we drop all cases where both judge gender and religion could not be deduced. We also drop cases where both defendant gender and religion cannot be inferred from the information available. The analysis dataset for the randomized case assignment experiment approach consists of 8.0 million cases.

For our alternative event study empirical approach, we joined cases to courts based on the court location and decision date associated with each case. The resulting analysis dataset for this approach comprises of 17 million cases – 68% of the initial universe of criminal cases.

3.3 Assigning Religion and Gender Identity

The eCourts platform does not provide demographic metadata on judges and defendants. However, gender and religious identity can be determined quite accurately in India based on individuals' names. We train a machine classifier on a large database of labeled names and then use it to assign these characteristics in the legal data.[13]

We have access to two databases of names with associated demographic labels. First, to classify gender, we use a dataset of 13.7 million names with labeled gender from the Delhi voter rolls. Second, to classify religion, we use a database of 1.4 million names with a religion label for individuals who sat for the National Railway Exam.

Summary tabulations on these datasets are provided in Appendix Table A1. For gender, we observe two categories: female or male. For religion, we observe five categories: Hindu, Muslim, Christian, Buddhist, and Other. Our classifier takes a two-label specification: Muslim or non-Muslim. We do not distinguish between the non-Muslim

[12]See Appendix Figure A3 for a sample page from which we extract the judge data. The data does not include the room in the court to which a judge is assigned.

[13]The existing available name classifiers for gender and religion in India are expensive proprietary solutions, e.g. Namsor (namsor.com).

religion categories because of their small number and because their names are not as distinctive as Muslim names. Each name record is therefore assigned two binary labels: male/female, and Muslim/Non-Muslim.

Before applying the classifier, we pre-process the name strings by transliterating characters from Hindi to Latin, and normalizing capitalization, punctuation and spacing. We then apply a neural net classifier to predict the identity label based on the name string, similar to the approach in Chaturvedi and Chaturvedi (2020). We use a bidirectional Long Short-Term Memory (LSTM) model applied directly to the sequence of name-string characters. LSTMs are a gated recurrent neural network architecture that takes as input sequential data and retains memory of previous inputs as it handles new items in a sequence. LSTMs are particularly useful in understanding text sequences because the meaning of an individual letter or word is often dependent on the context of other letters and words that both precede and follow it. "Bidirectional" means that the classifier reads the sequence both backwards and forwards when trying to assign a label. The ability of the LSTM classifier to understand a text fragment within the context it appears lends this method superiority over a fuzzy string matching method in terms of accuracy. For instance, consider the last names *Khan* and *Khanna*. While the fragment *KHAN* appears in both words, the addition of a single letter *a* following the fragment changes the meaning of the word where it is a distinctly Muslim last name without the letter *a*, and a non-Muslim last name once *a* is added. A standard fuzzy match would ignore the context, that is, sequence of letters that appear before and after the fragment *KHAN*. A counter-example are the names *Fatima* and *Fathimaa* where the additoin of the letters *h* and *a* do not change the meaning of the name in terms of religion. Given these nuances, the LSTM classifier was better suited for the purpose of the present study rather than a simple fuzzy matching function. [14]

We use hold-out test sets within the labeled databases to assess the out-of-sample performance of the LSTM classifiers for gender and religion. The classifiers perform well along the standard metrics, including our preferred metrics which adjust for imbalance in the class shares. We report balanced accuracy, the average accuracy (recall) for each

[14] The neural net architecture is as follows. The model takes as input a sequence of characters and outputs a probability distribution across name classes. The characters are input to an embedding layer, which was initialized randomly rather than using pre-trained weights. The embedded vectors are input to a bidirectional LSTM layer, then to a single dense hidden layer, and finally to the output layer, which uses sigmoid activation to output a probability across the binary classes. To avoid overfitting, we used dropout between layers and used early stopping during training, which ceases network training when validation loss stops improving. To account for the imbalance in the sample, we used class weights during the training.

of the two identity categories, and F1, the harmonic mean of precision and recall.[15] For gender, the balanced accuracy is .975 with F1 = .976. For religion, the balanced accuracy is .98 and the F1 = .99.

We then apply the trained classifier to the eCourts case records. The judge names tend to be complete (first and last name) and often include salutations indicating gender. Our algorithm can classify the names of 96% of the 81,232 judges (22,413 unique names) appearing in the case dataset according to gender (female/male), and 98% according to religion (Muslim/non-Muslim). The information on litigant names is of lower quality, often missing either the first name or last name. We are able to classify 80% of litigants by religion, and 74% by gender. Cases with unclassified labels are dropped from analyses requiring those labels.

To verify the accuracy of the LSTM classification within the new domain of the court records, we conduct a manual verification of random subsets of names classified by gender and religion, stratified across all states. We can confirm an accuracy rate of 97% for both the gender and religion classification based on manual verification. As an additional validation step, we compare the LSTM-classified Muslim defendant share by state to the state-wise Muslim population shares reported in the 2011 Population Census, and find a correlation of 0.88.

3.4 Case Outcome Specification

We define the defendant's outcome (represented by Y below) as a case-level indicator variable that takes the value 1 if the outcome is desirable for the defendant. Our primary specification uses an indicator for defendant acquittal. A secondary specification uses an indicator for any outcome other than conviction. Unfortunately, there are many cases where eCourts does not provide a clear indication of whether the outcome is desirable. For instance, a case outcome may be described in the metadata as "disposed," , with no additional judgment information uploaded for the case. For a case like this, we define the outcome as neither acquitted nor convicted; that is, the positive outcome variable takes the value of 0 when Y=*acquitted*, and the value of 1 when Y=*not convicted*

[15] Balanced accuracy and F1 are preferred as metrics to standard accuracy when the labels to be predicted are not balanced. While gender is roughly balanced in the voter rolls data, religion is heavily imbalanced with Muslims only comprising one-tenth of the sample. Therefore a model could achieve 90% accuracy in predicting religion by guessing non-Muslim. Balanced accuracy addresses this issue by rewarding good accuracy for both classes: we calculate the accuracy for each class and then average, rather than taking the accuracy measure across the whole sample. F1 addresses this issue by rewarding higher precision, which penalizes false positives, and higher recall, which penalizes false negatives.

Table 1: Coding of outcome variables

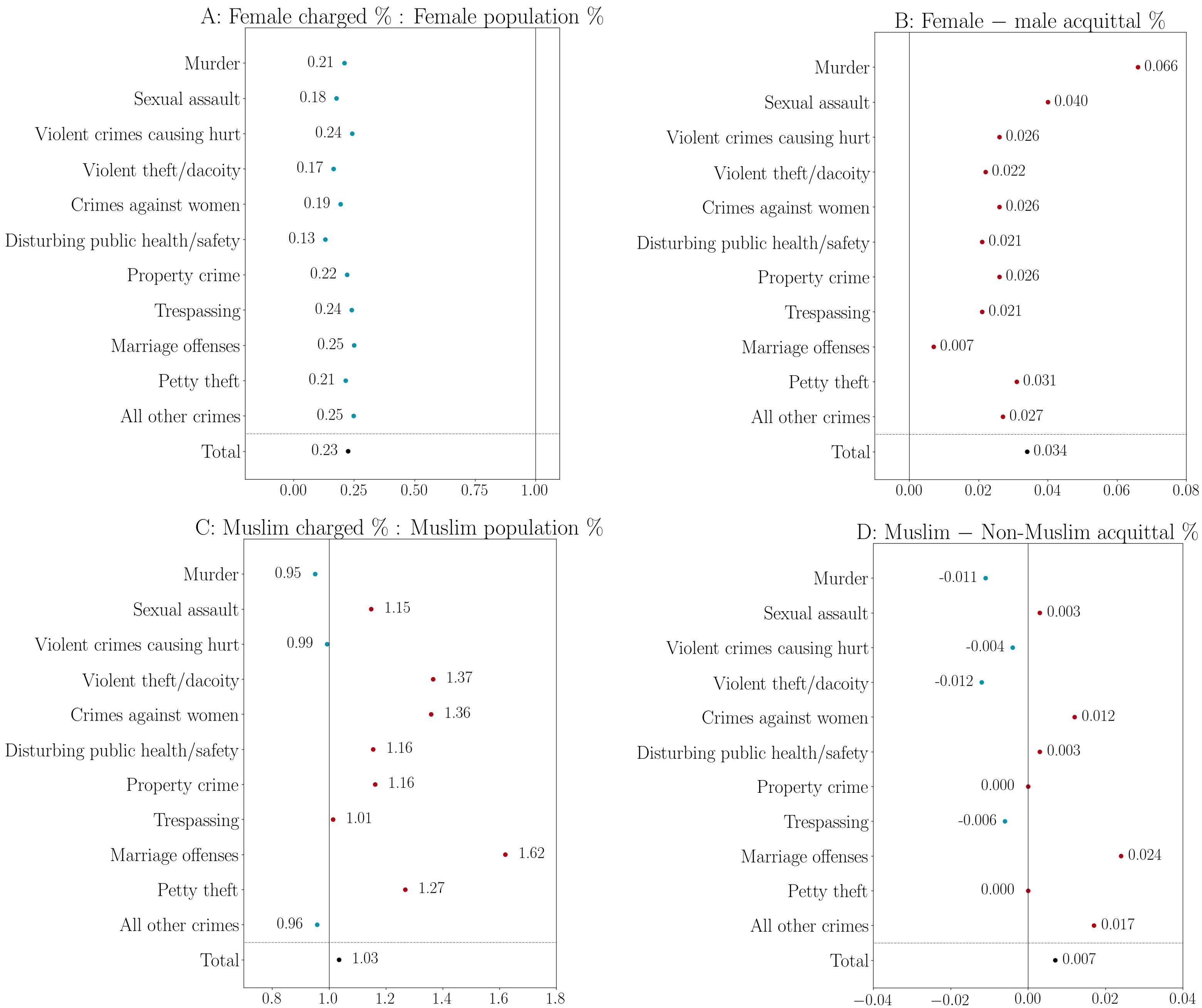
Outcome in e–Courts Data	**1(Decision)**	**1(Acquitted)**	**1(Not Convicted)**
No decision within 6 months	0	0	0
Acquitted	1	1	1
Neither acquitted nor convicted	1	0	1
Convicted	1	0	0

Notes: The outcome variables were coded based on the trial outcome recorded in the disposition variable associated with each case record. Under any of the three trial outcome definitions, a value of 1 always represents a positive outcome.

(Table A2). About 40% of case dispositions can be clearly designated as good or bad, while 60% are ambiguous; we show that our results are robust even when we restrict the sample to cases with unambiguous outcomes.

In about 40% of cases, the judge presiding over the initial case filing does not reach a decision; a decision is reached by a future judge or else the case remains undecided. Our analysis is focused entirely on the first judge to see the case; because decision deferral may be endogenous, we cannot treat the assignment of the second judge as random. Judicial delay is also a major policy issue in India; getting a decision at all is therefore an outcome of interest in and of itself. We define a variable *decision* as an indicator for whether the first judge to preside over the case reaches a decision on the case within six months of the case's filing date. We discuss our treatment of cases that pass to other judges in Section 4.

Figure 1: Summary statistics by crime category and defendant identity



Notes: Panels A & C show the ratio of share of accused Muslim or female over the population share of Muslims or females respectively, for each crime category. Panels B & D show the difference in mean conviction rates between defendant groups within crime categories

3.5 Summary Statistics on Case Outcomes

Figure 1 presents descriptive statistics of charges and convictions by gender and religious identity of defendants, respectively.[16] These summary measures are descriptive in nature, and are not directly informative of bias in the judicial system because we do not know the share of Muslim and female defendants who commit crimes, or who are in fact guilty upon being charged with crimes.

Figure 1 Panel A shows that the share of females charged under all crime categories is substantially lower than their population share. Men are three to five times more likely to be charged with crimes under any classification. Panel B shows that the conviction rate varies by crime, but overall it is about 1 percentage point lower for women (the "Total" category, at the bottom). Crimes are ordered by maximal punishment, from most to least severe.

Panel C shows that Muslims are disproportionately represented in the universe of criminal charges for most offenses. In particular, they are 34% more likely to be charged with crimes against women, 23% more likely to be charged with robbery, and 62% more likely to be charged with marriage offenses. Muslims are less likely to face charges for murder. In Panel D, we see no aggregate differences for Muslims in conviction rates, although these vary across crime types. Conditional on being charged, Muslim defendants are substantially more likely to be convicted than non-Muslims with robbery, property crime, and theft, but less likely to be convicted of obscenity, murder, or crimes against women.

Table 2 shows descriptive statistics of judges in the analysis sample. About 28% of judges are female, and 7.5% of judges are Muslim. On average, Muslim and female judges have similar conviction and decision rates to non-Muslim and male judges.

4 Empirical Strategy

Our objective is to estimate whether defendants experience different outcomes depending on the identity of the judge presiding over their case. To estimate a causal effect of judge identity, we need to effectively control for any factors other than defendant identity that could affect both judge identity and the case outcome. For instance, if female judges see less severe cases on average, and less severe cases have different conviction rates, we do not want to attribute that difference to a female judge effect. Similarly,

[16] The corresponding point estimates are reported in Appendix Tables A3 and A4.

Table 2: Outcome probability, by judge identity

		Judge gender		Judge religion	
	(1)	(2)	(3)	(4)	(5)
	Total	Female	Male	Muslim	Non-Muslim
Female judge	0.270	—	0.000	0.257	0.267
	(0.002)		(0.000)	(0.010)	(0.003)
Muslim judge	0.068	0.066	0.069	—	0.000
	(0.001)	(0.003)	(0.002)		(0.000)
Tenure length	520.765	532.378	524.671	528.661	524.180
	(2.501)	(5.128)	(2.995)	(10.226)	(2.607)
Decisions					
Decision (given first filing)	0.629	0.614	0.633	0.642	0.629
	(0.002)	(0.004)	(0.002)	(0.007)	(0.002)
Acquitted	0.302	0.318	0.302	0.316	0.302
	(0.002)	(0.003)	(0.002)	(0.007)	(0.002)
Convicted	0.061	0.071	0.056	0.066	0.060
	(0.001)	(0.002)	(0.001)	(0.004)	(0.001)
N	33,332	8,085	22,802	2,024	30,252

Notes: Coefficients represent means for each variable in the sample, collapsed to the judge level. Standard errors have been reported in parentheses.

Muslim defendants and judges may be more predominant in parts of the country with different base conviction rates.

We use two empirical strategies to isolate the causal effect of judge identity. First, we rely on the exogenous assignment of judges to cases, which produces as-good-as-random assignment of defendants to judges, conditional on charge and district. Second, we use a regression discontinuity design to exploit changes in the staffing of judges sitting in a given court, which creates exogenous changes in the likelihood of judge-defendant identity matches. These different identification strategies also have largely different samples, because random assignment is most relevant in large courts, while staffing changes are most likely to substantially affect the identity composition of small courts.

We formalize each approach in the following subsections. For ease of exposition, we describe the empirical strategy investigating gender bias; the specification and considerations for estimating religious identity bias are identical. Specifications used in additional analysis on contexts likely to activate identity are described with the results.

4.1 Random Assignment of Judges to Cases

As with much of the prior empirical literature, judge assignment in district courts is not strictly random, but follows a set of rules that gives defendants and prosecutors virtually no control over which judge oversees the case. As described in Section 2, a case is assigned to a room in a court (and thus the judge in that room) when charges are filed, based on the police station and charge type, giving prosecutors and defendants little control over the judge's identity. From a defendant's perspective, the judge assignment is as good as random; for simplicity and consistency with the prior literature, we describe the approach as random assignment below, and we follow a standard empirical strategy used by other papers using similar types of judge assignment to estimate judicial bias (Shayo and Zussman, 2011).

Random assignment of judges to cases is empirically important because of the concern that judges could treat defendants differently not because of their identity, but because of other case characteristics that are correlated with judge identity. For example, if Muslim judges could systematically choose to sit in cases with Muslim defendants who had committed less serious crimes, we might see in-group differences, but they would be due to differences in the underlying cases of Muslim defendants matched to Muslim judges, rather than due to bias. Alternately, Muslim defendants and judges may be more likely to appear in some parts of the country than others; of those regions are characterized by different crime distributions, we might again mistakenly attribute those differences to in-group bias.

Our ideal experiment would take two defendants identical in all ways, charged with identical crimes in the same police station on the same date, and then assign them to judges with different identities. In practice, the Indian court system runs this experiment whenever a defendant is charged in a jurisdiction with multiple judges of different identities on the bench.

We use a canonical regression approach to test for the effect of judge identity on case outcomes, as used by Shayo and Zussman's (2011) analysis of judicial in-group bias in Israel. We model outcome $Y_{i,s,c,t}$ (e.g. 1=acquitted) for case i with charge s, filed in court c at time t as:

$$\begin{aligned} Y_{i,s,c,t} = \beta_1 \text{judge\_male}_{i,s,c,t} + \beta_2 \text{def\_male}_{i,s,c,t} + \\ \beta_3 \text{judge\_male}_{i,s,c,t} * \text{def\_male}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta \chi_{i,s,c,t} + \epsilon_{i,s,c,t} \end{aligned} \tag{1}$$

$$\begin{aligned} Y_{i,s,c,t} = \beta_1 \text{judge\_nonMuslim}_{i,s,c,t} + \beta_2 \text{def\_nonMuslim}_{i,s,c,t} + \\ \beta_3 \text{judge\_nonMuslim}_{i,s,c,t} * \text{def\_nonMuslim}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta \chi_{i,s,c,t} + \epsilon_{i,s,c,t} \end{aligned} \tag{2}$$

where judge_male and judge_nonMuslim are binary variables that indicate whether a judge is male or non-Muslim, respectively. Similarly, def_male and def_nonMuslim indicate the defendant's identity. $\phi_{c,t}$ is a court-month or court-year fixed effect, and ζ_s is an act and section fixed effect. $\chi_{i,s,c,t}$ includes controls for defendant religion, judge religion, and an interaction term of judge gender and defendant religion in the gender analysis. In the religion analysis, $\chi_{i,s,c,t}$ represents controls for defendant gender, judge gender, and an interaction term of judge religion and defendant gender.

The charge section fixed effect ensures that we are comparing defendants charged with similar crimes. The court-time fixed effect ensures that we are comparing defendants who are being charged in the same court at the same time. Our primary specification uses a court-month fixed effect; a secondary specification uses a court-year fixed effect. The court-year fixed effect allows a much larger sample, at some potential bias. Judges on the bench may not hear new cases in some months because they are tied up with previous cases or away from work; it is unlikely that prosecutors or defendants can time their filings to match these absences, nor do we find evidence of disproportionate identity matching in balance tests of either specification below. Court-time periods with no variation in judge identity are retained to increase precision of fixed effects and controls but they do not affect the coefficients of interest. We drop court-time periods where only one judge appears, though they may appear in the regression discontinuity setup.

There are three causal effects of interest. β_1 describes the causal effect on a female defendant of having a male judge assigned to her case, rather than a female judge. $\beta_1 + \beta_3$ describes the causal effect on a *male* defendant of having a male judge assigned to his case. The difference between these effects (or β_3) is the own-gender bias — it tells us whether individuals receive better outcomes when a judge matching their gender identity is randomly assigned to their case. Appendix Table A5 presents a visual summary of the meanings of these coefficients in a difference-in-differences setup. Since all three causal effects are of interest, we report all three coefficients in the regression tables. The coefficient meanings are analogous in Equation 2. Standard errors are clustered at the judge level, since judge assignment is the level of randomization.

4.2 Balance Tests

To test the validity of the random assignment of cases to judges, we run the following empirical balance test in the analysis sample:

$$\text{judge\_female}_{i,s,c,t} = \beta_1 \text{def\_Muslim}_{i,s,c,t} + \beta_2 \text{def\_female}_{i,s,c,t} + \gamma\phi_{c,t} + \zeta_s + \delta\chi_{i,s,c,t} + \epsilon_{i,s,c,t} \tag{3}$$

$$\text{judge\_Muslim}_{i,s,c,t} = \eta + \gamma_1 \text{def\_Muslim}_{i,s,c,t} + \gamma_2 \text{def\_female}_{i,s,c,t} + \gamma\phi_{c,t} + \zeta_s + \delta\chi_{i,s,c,t} + \epsilon_{i,s,c,t}, \tag{4}$$

Variables are defined as above. Our causal identification strategy relies on β_1 and γ_1 to be equal to zero.

The result is shown in Table 3. Male and female defendants are effectively equally likely to be assigned to female judges, and similarly, Muslim and non-Muslim defendants are equally likely to be assigned to Muslim judges. We do find that Muslim defendants are 0.12 percentage points more likely to have their cases heard by female judges. This difference is economically small but it is statistically significant in part due to the very large sample. Of the eight prior studies we found that exploit random judge assignment, none of them are statistically powered to rule out an effect of this size in their balance tests, and all report point estimates larger in magnitude than 0.12 percentage points. Nevertheless, to ensure that this small difference in assignment to female judges does not influence our result on Muslims, we control for judge and defendant gender in the

Table 3: Balance test for assignment of judge identity

	(1) Female judge	(2) Female judge	(3) Muslim judge	(4) Muslim judge
Female defendant	-0.000	-0.000	0.001	0.001
	(0.001)	(0.001)	(0.000)	(0.000)
Muslim defendant	0.001	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	5155404	5168610	5240281	5253483
Fixed Effect	Court-month	Court-year	Court-month	Court-year

Standard errors in parentheses

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

Notes: This table reports results from a formal test of random assignment of judges to cases in the study sample. For specification details, see Equations 3 and 4. Columns 1–2 report the likelihood of being assigned to a female judge relative to a male judge using court-month, and court-year fixed effects. Columns 3–4 report the likelihood of being assigned to a Muslim judge relative to a non-Muslim judge using court-month, and court-year fixed effects. Charge section fixed effects have been used across all columns reported. Heteroskedasticity robust standard errors are reported below point estimates.

religion regressions (and for judge and defendant religion in the gender regressions).

Overall, the balance test indicates that defendants of a given identity do not face different odds of encountering judges with the same identity. This null supports the essential assumption underlying causal identification of judge identity on case outcomes.

4.3 Regression discontinuity approach using court transitions

The sample for the randomized assignment design requires courts with many judges to which a defendant can plausibly by assigned. In this section, we describe a complementary identification strategy that focuses on courts with a smaller number of judges on the bench. We exploit our high-frequency outcome data, along with discrete changes in the set of judges working in a given court, to provide additional evidence on the the same questions of in-group bias.

We define a court transition as any instance when a judge begins or ends their tenure in a court. For each court transition, we calculate the change in the shares of female and Muslim judges before and after the transition. We then examine whether the outcomes of defendants with specific social identities change following the transition.

Table 4: Types of Judge Transitions

Event	Description
Pro-defendant transition	Share of judges belonging to defendant's identity increases by ≥ 50 percentage points in the court
Against defendant transition	Share of judges belonging to defendant's identity decreases by ≥ 50 percentage points in the court
Composition neutral transition	Share of judges belonging to defendant's identity remains unchanged by a transition
Dropped from sample	Share of judges belonging to defendant's identity changes by 1–49 percentage points

We analyze three types of court transitions, defined respective to an identity group (female or Muslim), which are listed in Table 4. We define a *pro-defendant* court transition as a transition that results in a court whose judge composition is at least 50 percentage points more similar to the defendant's identity than it was before the transition. For example, in a court with two judges, if one male judge is replaced by a female judge, we describe this as a favorable transition for female defendants. An *against-defendant* transition is the reverse; replacing a male judge by a female judge in the court above would be an against-defendant transition for male defendants.. A *composition-neutral* transition is a judge entry or exit that has zero effect on the balance of the court for the identity in question, such as when a male judge is replaced by another male judge. Judge transitions that result in a non-zero but less than 50 percentage point change in the identity makeup of the court are dropped from the sample. For example, in a court with ten judges, moving from four female judges to five female judges would not be included as an analyzed transition. This approach maximizes statistical power by focusing on transition which have a large effect on the likelihood of a defendant-judge identity match.[17]

We use a regression discontinuity specification to examine whether defendants experience different kinds of outcomes after each type of judge transition. We use time in days as the running variable and the court transition as the event date. Our local linear regression includes cases decided within a given number of days of the transition date (the bandwidth) and controls for the running variable on either side of the threshold. The treatment is having the case heard in the post-transition period for each event.

[17]Results are similar if we use different thresholds for positive or negative transitions. Using a lower threshold results in a larger sample but a smaller first stage effect of the transition on the likelihood of a defendant-judge identity match. A 50% threshold maximizes power to detect an in-group bias effect.

We set the baseline bandwidth at 25 days, but the estimates are not sensitive to varying the bandwidth. The sample is limited to courts and dates where the justices on the bench have been in position for at least the same number of weeks as the specification bandwidth before and after the transition. This ensures that each case appears only once in the sample — either before a judge transition or after.

The outcome $Y_{i,s,c,t}$ is a binary variable indicating a positive outcome for the defendant in case i, court c, time t, charged under section s. The estimating equation is given by:

$$Y_{i,s,c,t} = \gamma_1 \text{pro\_post}_{c,t} + \gamma_2 \text{against\_post}_{c,t} + \gamma_3 \text{neutral\_post}_{c,t} + \chi_{i,s,c,t} + \epsilon_{i,s,c,t}, \quad (5)$$

where pro_post is a post-event indicator for a pro-defendant transition, against_post for an against-defendant transition, and neutral_post for a composition-neutral transition. $\chi_{i,s,c,t}$ includes all of the linear trends in the forcing variable (date relative to transition), court-time fixed effects, and charge section fixed effects. Standard errors are clustered by transition events. This regression effectively stacks three standard regression discontinuity estimations, estimating a treatment effect for each type of transition. We could also estimate each of the three regression discontinuity specifications separately, but pooling allows us to estimate one set of fixed effects and clusters.

If there is in-group judicial bias, we expect γ_1 (post-event effect for pro-defendant transitions) to be positive and γ_2 (against-defendant transitions) to be negative. We don't expect any effect on average for γ_3.

Identification of these causal parameters comes from the standard assumptions of regression discontinuity designs. Appendix Figure A6 shows that the distribution of cases is flat around positive, neutral, and negative transition events for both men and women, supporting the assumption of no manipulation of case timing around the transition date. This test is analogous to the McCrary test (McCrary, 2008). As further support for absence of manipulation, Appendix Figure A7 shows that there is no variation in the average charge severity of cases seen just before and just after these transitions.

5 Results

5.1 Effect of assignment to judge types

The first two rows of Table 5 Panel A present the impact, for female and male defendants respectively, of being randomly assigned to a male judge; these are β_1 and $\beta_1 + \beta_3$ in Equation 1. The third row shows the difference between these two coefficients (β_3), which is the own-gender bias. The outcome variable is an indicator for defendant acquittal. Columns 1–3 show results using court-month fixed effects, while Columns 4–6 use court-year fixed effects. Within each set of three columns, the second column adds additional demographic controls, while the third column adds judge fixed effects.

Male judges consistently deliver more acquittals than female judges. The point estimate on this effect is nearly identical for male and defendants across all specifications. We interpret this as a null effect. [18]

Panel B shows the effect of filing judge gender on a binary variable indicating whether a case has been decided in our sample period at all. We find no evidence that assignment to a female judge results in any difference in time to resolution for either male or female defendants. In short, we find that while male judges are somewhat more lenient on average in terms of lower acquittal rates, we do not find substantial gender bias in any dimension.

[18]Appendix Table A7 shows estimates when we exclude closed cases for which we are unable to determine the outcome. While we find marginally significant bias effects (in the expected direction), the point estimate on the bias term is never higher than 0.7pp.

Table 5: Impact of assignment to a male judge on defendant outcomes

Outcome variable: Acquittal rate						
	(1)	(2)	(3)	(4)	(5)	(6)
Male judge on female defendant	-0.008***	-0.007**	—	-0.007***	-0.007**	—
	(0.003)	(0.003)		(0.003)	(0.003)	
Male judge on male defendant	-0.006***	-0.006**	—	-0.006***	-0.005**	—
	(0.002)	(0.003)		(0.002)	(0.003)	
Difference = Own gender bias	.001	.001	0	.002	.001	0
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Reference group mean	0.175	0.176	0.176	0.175	0.176	0.176
Observations	5250907	5156887	5155378	5264320	5170380	5168583
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Outcome variable: Decision within six months of filing						
	(1)	(2)	(3)	(4)	(5)	(6)
Male judge on female defendant	0.023***	0.022***	—	0.022***	0.020***	—
	(0.004)	(0.004)		(0.004)	(0.004)	
Male judge on male defendant	0.022***	0.021***	—	0.021***	0.020***	—
	(0.003)	(0.004)		(0.003)	(0.004)	
Difference = Own gender bias	-.001	-.001	-.002	-.001	-.001	-.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Reference group mean	0.285	0.284	0.284	0.284	0.284	0.284
Observations	4382525	4301724	4300307	4395262	4314529	4312834
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Notes: Standard errors in parentheses. $*\ p < 0.10$, $**\ p < 0.05$, $***\ p < 0.01$.
Reference group: Female judges, female defendants.
Charge section fixed effects have been used across all columns reported.
Specification: $Y_{i,c,t} = \beta_1 judge\_male_{i,c,t} + \beta_2 def\_male_{i,c,t} + \beta_3 judge\_male_{i,c,t} * def\_male_{i,c,t} + \phi_{c,t} + \delta\chi_{i,c,t} + \epsilon$

Table 6: Impact of assignment to a non-Muslim judge on defendant outcomes

	Outcome variable: Acquittal rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Muslim judge on Muslim defendant	0.008**	.007	—	0.007*	.006	—
	(0.004)	(0.005)		(0.004)	(0.005)	
Non-Muslim judge on non-Muslim defendant	0.007**	0.007*	—	0.007**	.006	—
	(0.003)	(0.004)		(0.003)	(0.004)	
Difference = Own religion bias	-.001	0	.002	-.001	0	.002
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Reference group mean	0.18	0.183	0.183	0.18	0.183	0.183
Observations	5684426	5241649	5240140	5697480	5255137	5253328
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

	Outcome variable: Decision within six months of filing					
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Muslim judge on Muslim defendant	0.010*	.008	—	.009	.005	—
	(0.006)	(0.008)		(0.006)	(0.007)	
Non-Muslim judge on non-Muslim defendant	.005	.002	—	.003	-.001	—
	(0.006)	(0.007)		(0.005)	(0.007)	
Difference = Own religion bias	-.006	-.006	.002	-.006	-.006	.003
	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.003)
Reference group mean	0.292	0.289	0.289	0.292	0.288	0.288
Observations	4748758	4375562	4374140	4761183	4388369	4386657
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Notes: Standard errors in parentheses. $*\ p < 0.10$, $**\ p < 0.05$, $***\ p < 0.01$.
Reference group: Muslim judges, Muslim defendants.
Charge section fixed effects have been used across all columns reported.
Specification: $Y_{i,c,t} = \beta_1 judge\_nonmuslim_{i,c,t} + \beta_2 def\_nonmuslim_{i,c,t} + \beta_3 judge\_nonmuslim_{i,c,t} * def\_nonmuslim_{i,c,t} + \phi_{c,t} + \delta\chi_{i,c,t} + \epsilon$

Table 6 presents analogous results for Muslim and non-Muslim defendants randomly assigned to Muslim and non-Muslim judges; all panels and columns have the same interpretation as the prior table. The effect of judge religion on the acquittal rate is again a precise zero. The point estimates on any form of bias are never higher than 0.6pp. The estimates rule out an own-religion bias of of 1.0–1.5pp with 95% confidence.

Panel B shows that Muslim judges are 1.2 to 2.7 percentage points more likely to each a decision on a case. This effect holds equally for Muslim and non-Muslim defendants; the own-religion bias estimate is a precise null. These results are robust to alternate specifications.[19]

5.1.1 Ingroup Bias on the Basis of Caste or Cultural Similarity

Ideally, we would like to run similar tests where judges are considered to be in the same social group as the defendant if they are in the same caste. This is more difficult for three reasons. First, caste is multidimensional, individuals in the same broad caste category (*varna*) may not be in the same subcaste (jati); identity similarity on the basis of caste is this continuous rather than discrete. Second, individual names do not identify caste as precisely as they identify Islamic religion or gender identity. We have thus far not been able to develop a reliable correspondence between names and caste; training data for such a correspondence are also scarce. Third, there are very few judges in the most identifiable caste categories, which are Scheduled Castes and Scheduled Tribes.

To measure bias on the basis of caste, we follow (**?**) and define individuals as being in the same cultural group if they share a last name. Last names are effective identifiers of caste for many social groups, but they are more numerous than castes. Thus, two individuals with the same last name are likely to be in the same social group, but two individuals in the same social group will often have different last names. The measure is also a combination of caste and religious group similarity; for instance the names Kaur and Singh (two of the most common names in the data, are likely indicators of Sikhism but are not very informative of socioeconomic status). Our measure thus underestimates the degree of affinity; as such, it will underestimate the true extent of ingroup bias.

We use Equation 6 to determine whether judges deliver more favorable outcomes to

[19] Appendix Table A11 reports analogous regressions with conviction as the outcome. Appendix Table A12 shows estimates that exclude ambiguous case outcomes. While we find marginally significant bias effects (in the expected direction) in a handful of specifications, the majority are statistically insignificant and the point estimate on the bias term is never higher than 1pp.

Table 7: Impact of assignment to a judge with the same last name on defendant outcomes

	(1) acquitted	(2) acquitted	(3) acquitted	(4) acquitted
Same last name	0.013**	0.014**	0.019*	0.025***
	(0.006)	(0.006)	(0.011)	(0.009)
Observations	2239516	2237502	2258437	2256242
Fixed Effect	Court-month	Court-month	Court-year	Court-year
Judge Fixed Effect	No	Yes	No	Yes

Notes: Standard errors in parentheses. $*\ p < 0.10$, $**\ p < 0.05$, $***\ p < 0.01$.

defendants who share their last name:

$$Y_{i,s,c,t} = \beta_1 \text{same\_last\_name}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \nu_i + \delta\chi_{i,s,c,t} + \epsilon_{i,s,c,t} \tag{6}$$

Subscripts i, s, c, t, court-time ($\phi_{c,t}$) and act/section (ζ_s) fixed effects are defined as above. We include an additional fixed effect for the defendant's last name group (ν) to control for the possibility that individuals from some social groups are more or less likely to be acquitted or to appear as judges. The vector $\delta\chi_{i,s,c,t}$ includes defendant and judge gender and religion.

Table 7 shows the results. Across specifications, we find a 1.3–2.5 percentage point increase in the acquittal rate when judges are given a case where the defendant's last name matches their own. The effect is robust to looser definitions of last names (for example, Patil and Patel). There are several factors that differentiate this result from the null effects of gender and religious similarity above. First, the social group defined by last names is much more narrow than the social group defined by all people of the same gender or religion. The family name similarity could also capture similarity in economic status.

The effect size remains small compared with the majority of studies on judicial bias, but is similar in magnitude to Fisman et al. (2017), who find that loan officers in India provide 6.5% more loans to borrowers with matching last names.

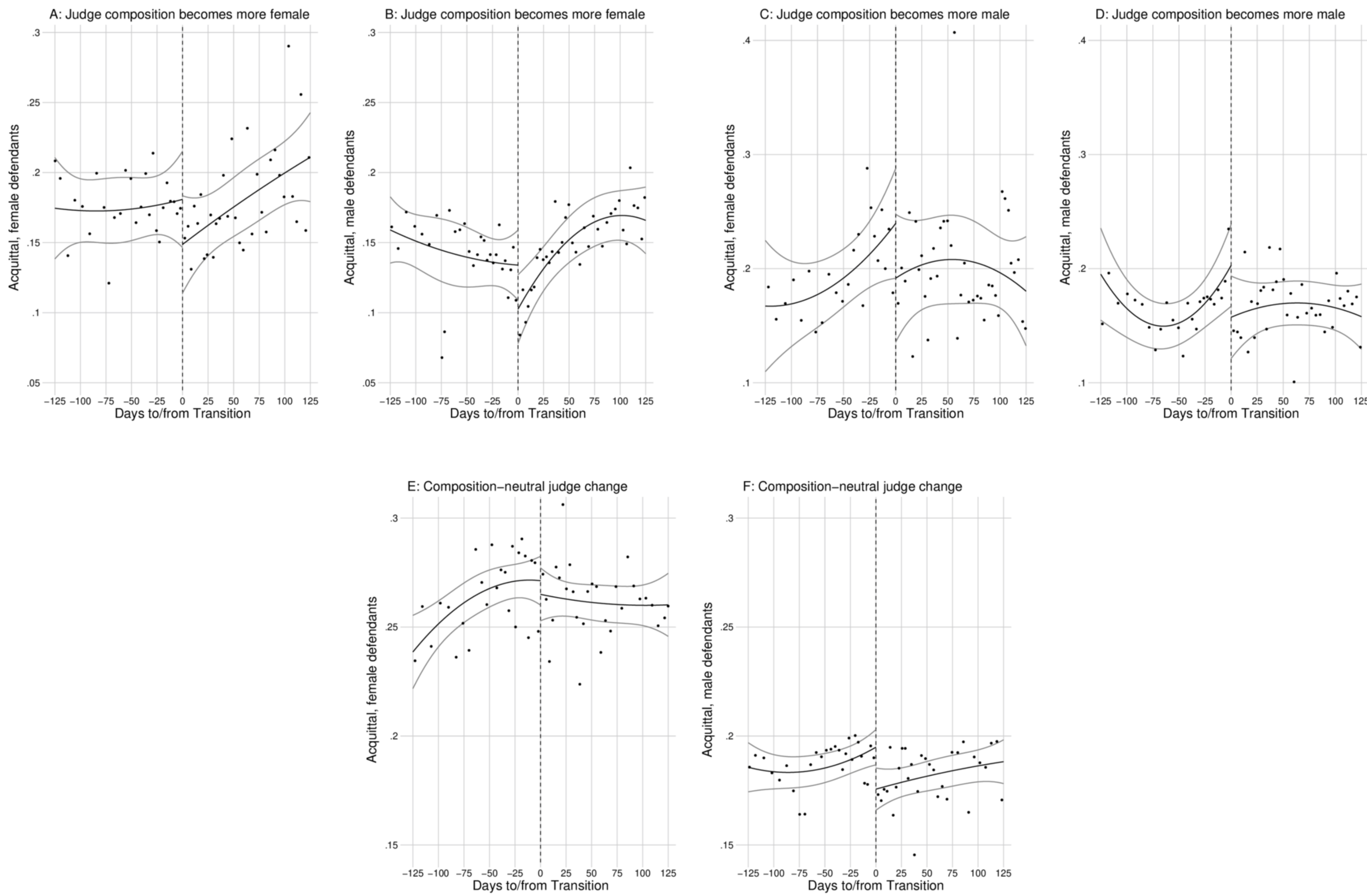
5.2 Effect of changes in court composition

In this section, we estimate judicial bias by exploiting exogenous changes in courtroom staffing. We use a regression discontinuity specification (Equation 5) to test whether defendant outcomes change immediately after the composition of judges in the court becomes more or less similar to the defendant in terms of identity.

We first show our results graphically. Figure 2 shows average acquittal rates before and after a judge joins or leaves the staff in a courtroom. The horizontal axis shows the number of days before and after the transition (with negative and positive numbers respectively), while the vertical axis shows the acquittal rate for defendants of a given gender. Panels A and B show the effects of transitions that increase the female judge share (by at least 50%) on female and male defendants respectively. Panels C and D show composition-neutral transitions as a reference or control group.

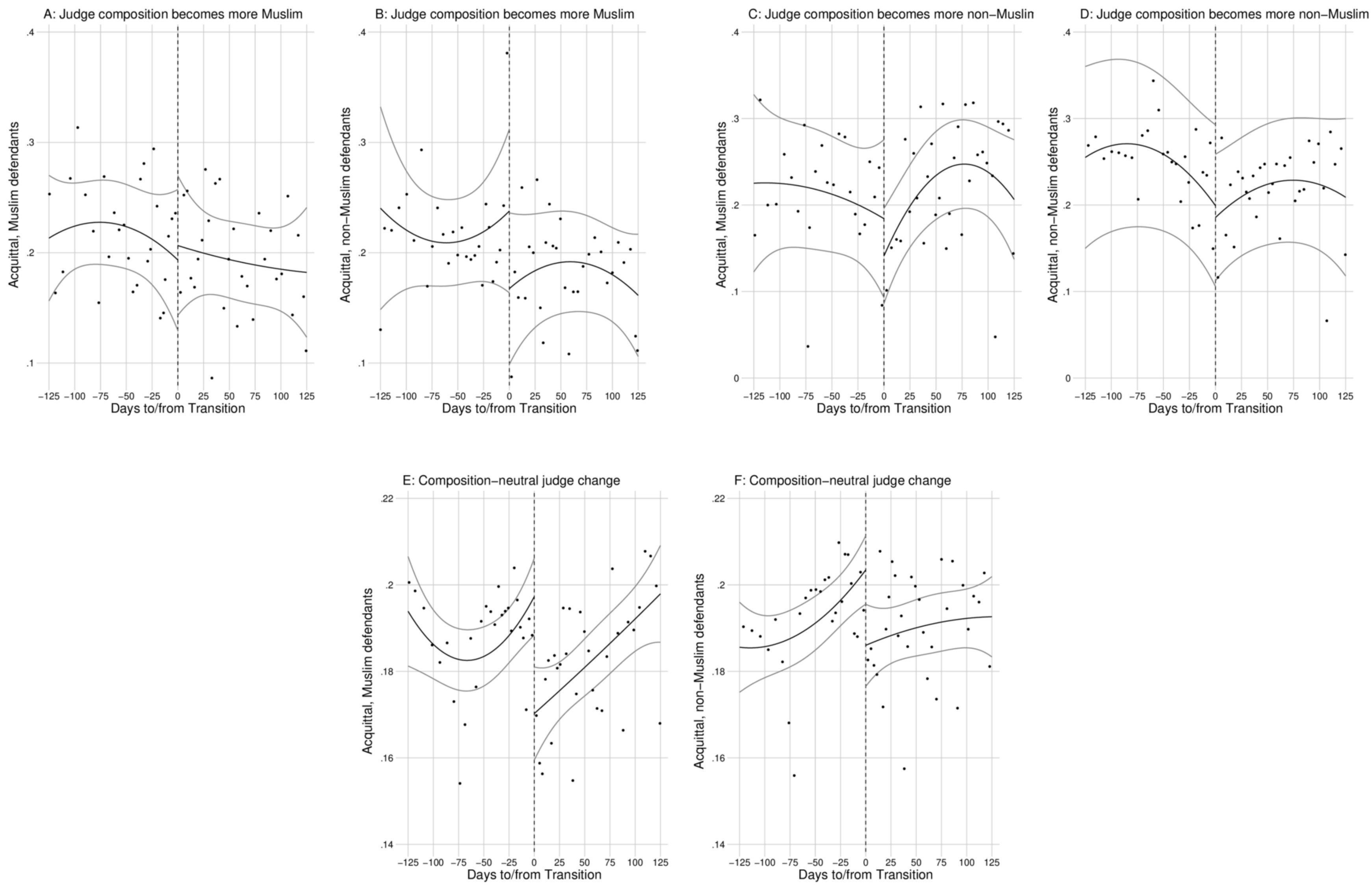
In almost all cases, we see a small decline in the acquittal rate in the weeks immediately after the transition; importantly, this also holds for null transitions. This tells us that courts become briefly more strict in the weeks after a staffing change, even if the gender composition does not change. If judges show bias toward members of their own gender, then we should see a differential break in the acquittal rate when the court becomes more or less matched to the defendant in identity terms. In other words, we should see a relative increase in the acquittal rate when the court becomes more similar to the defendant (Panel A) and the opposite when it becomes less similar (Panel B). In fact, we find that none of the switches that change gender composition have effects that are significantly different from judge changes that are composition-neutral.

Figure 2: Event Discontinuity effect: Transitions that change the likelihood of a same gender judge assignment



Notes: The figure shows acquittal rates of cases decided before and after a transition in the court. Panels A & B show the effect of a transition that increases the likelihood of getting assigned to a female judge. Panels C & D show the effect of a transition that increases the likelihood of getting assigned to a male judge. Panels E & F represent the effect of a transition that leaves the gender composition of the court unchanged.

Figure 3: Event Discontinuity effect: Transitions that change the likelihood of a same religion judge assignment



Notes: The figure shows acquittal rates of cases decided before and after a transition in the court. Panels A & B show the effect of a transition that increases the likelihood of getting assigned to a Muslim judge. Panels C & D show the effect of a transition that increases the likelihood of getting assigned to a non-Muslim judge. Panels E & F represent the effect of a transition that leaves the religion composition of the court unchanged.

Table 8: Impact of judge transitions that affect court composition on acquittal rates

	Gender composition changes		Religion composition changes	
	(1)	(2)	(3)	(4)
	Acquitted	Acquitted	Acquitted	Acquitted
Pro-defendant transition	0.008	0.010	0.018	0.013
	(0.011)	(0.010)	(0.020)	(0.017)
Against-defendant transition	0.004	0.002	-0.009	-0.010
	(0.007)	(0.007)	(0.016)	(0.013)
Constant composition judge transition	-0.014***	-0.015***	-0.016***	-0.015***
	(0.004)	(0.004)	(0.004)	(0.004)
Observations	371959	415857	503774	552088
No. of transitions	2784	4243	4105	5853
Mean Acquittal rate	0.191	0.191	0.192	0.192
Fixed effect	Court-month	Court-year	Court-month	Court-year

Notes: This table presents regression discontinuity estimates from the main estimating equation of the effect of judge transitions that affect court composition on acquittal rates of defendants. Columns 1–2 estimate the impact of a court transition that increases or decreases the share of judges belonging to the defendant's gender, using different fixed effects. Columns 3–4 estimate the analogous impact on acquittal rates for court transitions that change the religion share of judges in the court (see Section 4 for specification details). Sample bandwidth is 25 days before and after the transition. Charge section fixed effects have been used across all columns reported. Heteroskedasticity robust standard errors are reported below point estimates.

Figure 3 shows a similar graph examining whether changing the Muslim / Non-Muslim composition of the judges' bench differentially affects outcomes for Muslims and non-Muslims. There is no evidence of a differential change in acquittal rates based on the religion of the defendant or direction of the religious composition change in the court.

To formally test these hypotheses, we use a single estimation that calculates all these regression discontinuites simultaneously. We pool results from the six graphs into point estimates for three transition types: pro-defendant, against-defendant, and constant-composition. Table 8 reports the results. Constant composition transitions are associated with about a 1 percentage point decline in the acquittal rate; this effect is not driven by judge identity, because the average judge identity on the bench has not changed.

If there is same-identity bias, then the coefficient on pro-defendant transition should be higher than the coefficient on the constant-composition transition, and the against-defendant coefficient should be lower. However, we find no statistically significantly different effect on either of these transitions. The standard errors are slightly larger than in the randomized assignment regressions, because the regression discontinuity approach has less power. We can rule out a 2.5 percentage point bias effect on the gender dimension and a 4 percentage point bias effect on the religion dimension. We find similar results for alternate specifications and outcome variables. [20]

The null results are consistent with the findings on randomized judge assignment; we reject the hypothesis that judges issue more favorable rulings for defendants with the same gender or religious identity. This specification is not only a robustness test; it also shows that the result holds up in smaller courtrooms, whereas the random assignment tests are mostly identified off of larger courtrooms.

5.3 Judicial Bias when Identity is Salient

Our estimates thus far show that judges do not provide substantively better outcomes for own-gender and own-religion defendants, on average. Some of the prior literature suggests that various identities can be made more salient by specific contexts or primes. In this section, examine several circumstances where gender or religious identity may become particularly salient to judges.

We first examine the subset of cases where the victim and defendant have different identities. In this case, when the defendant and judge are mismatched, it implies that the judge and victim share the same gender and religious identity.[21] The identity mismatch between judge and defendant may be particularly salient in this case (Baldus et al., 1997; Baumgartner et al., 2015; ForsterLee et al., 2006). Examining a subset of cases where the victim in the case is identified and can be matched to an identity group, we interact all of the terms in the standard ingroup bias estimation (Equation 3) with an indicator for whether the victim and defendant have an identity mismatch.

Columns 1 and 2 of Table **??** shows the results. In each table, columns 1 and 2 estimate the standard ingroup bias specification, separately for cases where the identity of victim and defendant are matched (Column 1) and unmatched (Column 2). The effect of judge identity is a tight null whether the victim and defendant are matched or unmatched. In Column 3, we interact the victim-defendant mismatch indicator with all the other variables in the base model. The coefficient of interest (in the gender regression) is the interaction between male judge, male defendant, and gender mismatch.

[20] Appendix Figure A8 shows no effect on case delay. Appendix Table A17 there is no bias effect for other outcome definitions, such as the conviction rate.

[21] In the case of religion, 6% of Indians are neither Muslim nor Hindu, so two non-Muslim individuals are highly likely to be in the same broad religious group but in some cases will not be.

Table 9: Differential judge bias effect based on victim of crime

	(1) Acquitted	(2) Acquitted	(3) Acquitted
Ingroup Bias	0.004	0.001	0.000
	(0.003)	(0.005)	(0.002)
Ingroup Bias * Victim Gender Mismatch	-0.006		
	(0.005)		
Ingroup Bias * Victim Religion Mismatch		0.007	
		(0.008)	
Ingroup Bias * Crime Against Women			-0.009
			(0.007)
Observations	1790929	2022473	5149667
Fixed Effect	Court-month	Court-month	Court-month
Judge Fixed Effect bias Bias sample Sample	Yes	Yes	Yes

Notes: Standard errors in parentheses. $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

This tells us whether judges rule differently against the outgroup when the victim is in the ingroup. In both the religion and the gender cases, we find a null effect and can rule out bias effect sizes of 1.5 percentage points on the acquittal rate.

We next examine whether male and female judges rule differently on cases classified in the criminal code as crimes against women, where the judge and defendant gender identities may be particularly salient. These are largely evenly split between sexual assaults and kidnappings.[22] We use the standard ingroup bias specification of Equation 3; Table A16 shows the result. As above, there is no evidence of gender bias in this subset of cases.[23]

Finally, we examine whether religious ingroup bias emerges during the month of

[22]One reason "kidnappings" are so common in the data is that this may be the formal charge filed against a man who elopes with a woman. Results are very similar for both the assault and kidnapping subsets of the data.

[23]Appendix Figure XX shows estimates for similar tests for both gender and religious ingroup bias for all crime types; out of these ten tests, we find an ingroup bias only on the basis of religion in the category of crimes against women. This may suggest that judges rule more favorably for their own social groups in cases involving gender. This is plausibly due to different gender norms among Muslims and non-Muslims (chiefly Hindus). However, we are hesitant to lean too much on this positive coefficient given that it is from one test out of ten, and would not be statistically significant if adjusted for multiple testing.

Ramadan, when Muslim religious identity may become particularly salient for both Muslims and non-Muslims. Because our sample only covers eight years, Ramadan always occurs in the summer and there is not substantial time series variation in daylight hours. We therefore use a simple indicator variable marking days in the month of Ramadan, and interact it with all the variables in the standard bias specification.

6 Conclusion

In providing fair justice, courts in developing countries face a number of special challenges, including cultural mismatch from transplanted legal codes, informal justice-system substitutes, citizen skepticism toward formal courts, insufficient (human) capital investments in the court system, the inability of many individuals to pay for high-quality representation, implicit or explicit bias among members of the judiciary, and corruption (Djankov et al., 2003; La Porta et al., 2008). Yet with a few exceptions (Ponticelli and Alencar, 2016, for example), these characteristics of developing-country courts have been documented only anecdotally.

In this paper, we make available a large-scale dataset for the analysis of court proceedings in India and present evidence of negligible judicial in-group bias in criminal cases. The null estimate of in-group bias presented here contrasts with findings in the previous literature, which has tended to find large effects. Figure 4A compares our point estimates with estimates of bias from the studies most similar to ours that we were able to find.[24] Effect sizes are standardized by dividing each in-group bias effect by the sample standard deviation of the outcome variable. The high end of our confidence interval is an order of magnitude smaller than nearly all prior studies.

The most straightforward interpretation of these findings is that, unlike judges analyzed in the other papers, India's district court judges do not exhibit in-group bias along the pertinent identity margins. Our research is consistent with judges taking their role seriously and working hard to provide justice on fair terms to all litigants, or with judicial institutions that constrain discretion and protect defendants from biased decision-making. It is also possible that the social distance between (normally) upper class judges and (normally) lower class criminal litigants may mitigate a sense of shared identity between judges and litigants. Yet it is also consistent with corruption that is

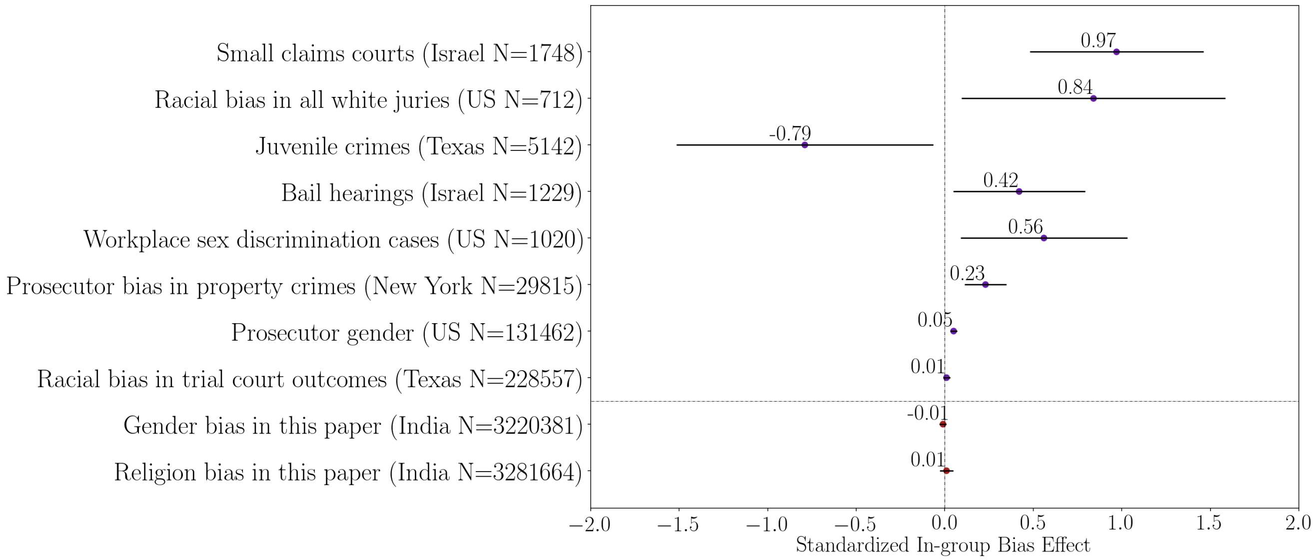
[24]We included every study we could find that focused on measuring in-group bias among judges on a race/ethnicity, gender, or religious dimension, using either random assignment to judges or rotations in judge cohorts as an identification strategy.

Table 10: Impact of Ramadan on own religion bias in acquittal rates

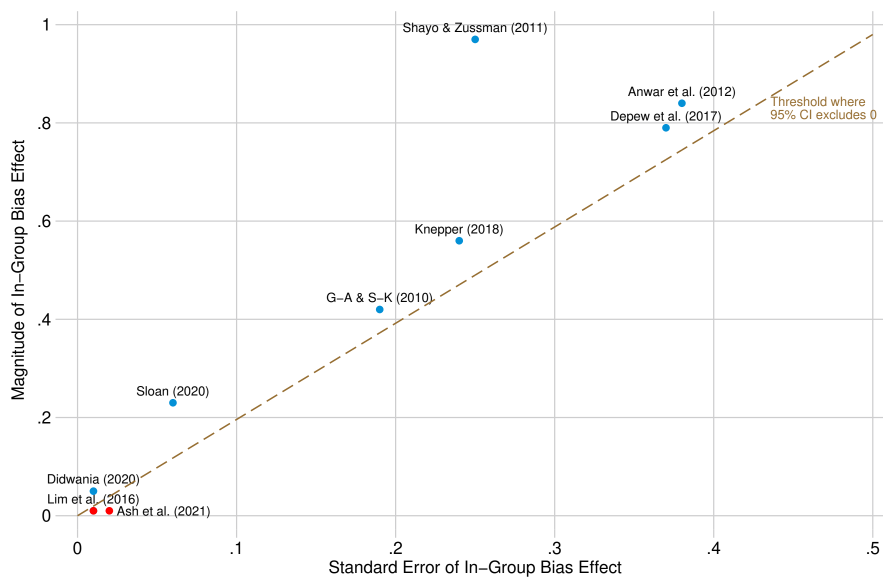
	(1) acquitted	(2) acquitted	(3) acquitted
Non-muslim judge	0.012**	0.012**	0.000
	(0.006)	(0.006)	(.)
Non-muslim defendant	0.002	0.003	0.003
	(0.004)	(0.004)	(0.002)
Ramadan	0.125***	0.123***	0.091***
	(0.012)	(0.013)	(0.012)
Own religion bias	-0.003	-0.002	-0.002
	(0.004)	(0.004)	(0.002)
Own religion bias X Ramadan	0.016	0.020**	0.018*
	(0.010)	(0.010)	(0.010)
Observations	5211432	5224676	6062453
Fixed Effect	Court-month	Court-year	Court-year
Judge fixed effect	No	No	Yes
Sample	Full sample	Full sample	Full sample

Figure 4: Comparison with judicial bias estimates in other contexts

A. Coefficient Plot



B. Standardized Errors vs. Effect Sizes



Notes: This figure reports point estimates of in-group bias from other studies in the relevant literature. From top to bottom, the coefficients of in-group bias (Panel A) correspond to Shayo and Zussman (2011), Anwar et al. (2012), Depew et al. (2017), Knepper (2018), Gazal-Ayal and Sulitzeanu-Kenan (2010), Sloane (2019), Didwania (2018), Lim et al. (2016), and the present study respectively. Panel B plots reported effect magnitudes (Y axis) against effect standard errors. All effect sizes are standardized (dividing outcome variables by their standard deviation) to allow comparison across studies.

blind to religious and gender identity. Rich as they are, our data do not allow us to differentiate between these very different mechanisms; exploring these possibilities is an important area for future work.

Another interpretation of why our India results stand out from the literature is that there could be publication bias in studies of judicial in-group bias. Focusing on the subset of bias studies that use one of our two identification strategies, Figure 4B plots the effect size of each study against the standard error of the main estimated effect.[25] In the absence of publication bias or a design-based mechanical correlation (such as adaptive sampling), the standard error should not be correlated with the effect size (Gerber et al., 2001; Levine et al., 2009; Slavin and Smith, 2009; Kühberger et al., 2014). In fact, a regression of effect size on standard error is highly significant ($\hat{\beta} = 2.25, p = 0.001$), which may suggest that studies finding a lack of bias are less likely to be published.

It is worth emphasizing again that we have not ruled out bias in the Indian criminal justice system as a whole. We have focused on two kinds of bias which have been widely documented in other countries and we have focused on the singular contributions of judges to criminal-justice outcomes. The legal system could still be biased against Muslims and women overall, through geographic distribution of policing, discrimination in investigations, police/prosecutor decisions to file cases, the severity of charges applied, the severity of penalties imposed, the appeals process, and others. It is also possible that bias takes a more subtle form, such as discrimination conditional on the interaction between defendant, victim, and type of crime. More research, and in particular more data, are needed to study the entire justice process in India and other developing countries.

[25] When papers report multiple specifications for the main effect, we used the effect size described most prominently in the text or described by the authors as the "main specification." When papers had multiple outcomes, we used the outcome most similar to the acquittal or conviction rate, as in this study. If these were unavailable, we used the outcome most prominently described in the paper's abstract and introduction.

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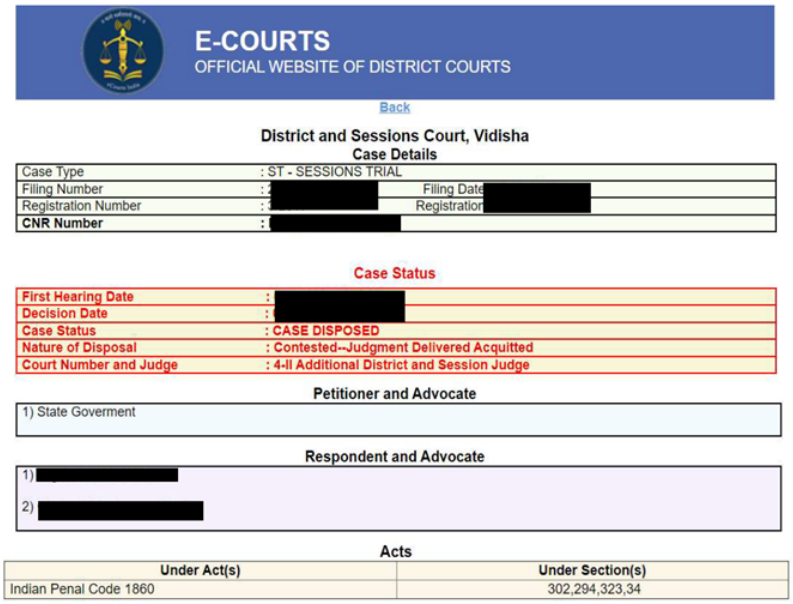
A Appendix

Figure A1: India eCourts Case Record Sample



E-COURTS
OFFICIAL WEBSITE OF DISTRICT COURTS

Back

District and Sessions Court, Vidisha
Case Details

Case Type	: ST - SESSIONS TRIAL		
Filing Number	: [redacted]	Filing Date [redacted]	
Registration Number	: [redacted]	Registration [redacted]	
CNR Number	: [redacted]		

Case Status

First Hearing Date	: [redacted]
Decision Date	: [redacted]
Case Status	: CASE DISPOSED
Nature of Disposal	: Contested--Judgment Delivered Acquitted
Court Number and Judge	: 4-II Additional District and Session Judge

Petitioner and Advocate

1) State Goverment

Respondent and Advocate

1) [redacted]

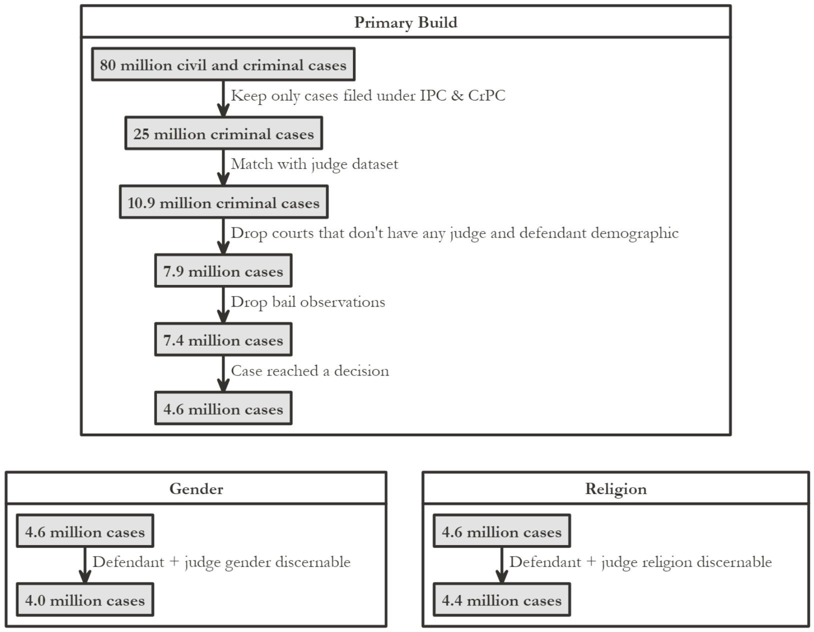
2) [redacted]

Acts

Under Act(s)	Under Section(s)
Indian Penal Code 1860	302,294,323,34

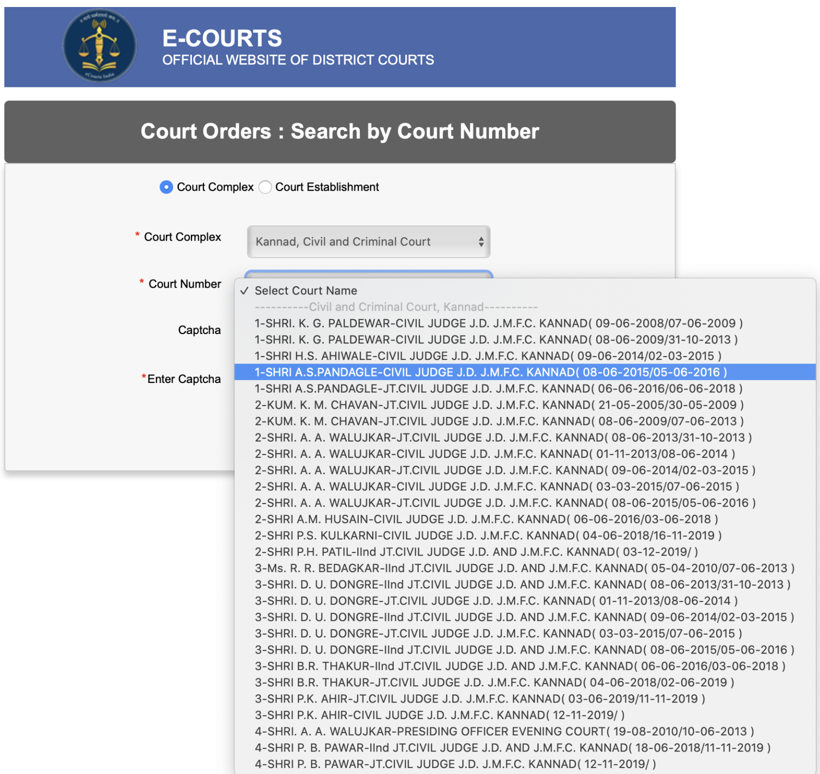
Notes: The figure displays an anonymized version of a sample court record from https://ecourts.gov.in/ for the District and Sessions Court of Vidisha. The record is comprising 'Case Details' variables, as well as information on the 'Case Status' – for instance the disposition information. The 'Petitioner and Advocate' and 'Respondent and Advocate' sections contain the litigant names that we use for assigning gender and religion. The 'Acts' section contains the data that allows us to discriminate between civil and criminal cases. We use the 'Under Section(s)' column to infer the corresponding crime categories.

Figure A2: Sample accounting



Notes: The figure displays the process through which we arrive at the analysis dataset from the parent dataset of 80 million legal case records.

Figure A3: India eCourts Sample Judge Information inside the Search Engine



Notes: Sample view of the eCourts court order search engine. We scraped the judge information implicitly given in the 'Court Number' drop-down list of the search mask on – in this case – `https://services.ecourts.gov.in/ecourtindia_v4_bilingual/cases/s_order.php?state=D&state_cd=1&dist_cd=19` to obtain judge names and tenures.

Table A1: Summary of Name Classifier Training Datasets

Panel A: Delhi voter rolls names		
Gender	Instances	Percentage
Female	6,138,337	44.8%
Male	7,556,138	55.2%
Total	13,694,475	100.0%

Panel B: National Railway exam names		
Religion	Instances	Percentage
Buddhist	1,910	0.1%
Christian	11,194	0.8%
Hindu	1,174,076	84.8%
Muslim	163,861	11.8%
NA	33,882	2.4%
Total	1,384,923	100.0%

Notes: Panels A & B of this table show the distribution of identities in the underlying training datasets of the gender and religion LSTM name classification models respectively.

Table A2: Outcome variables mapped to dispositions

	Mapping of Dispositions to Outcomes		
Disposition name	Acquitted	Convicted	Decision
258 crpc [acquitted]	X		X
Acquitted	X		X
Allowed	X		X
Committed			X
Compromise			X
Convicted		X	X
Decided			X
Dismissed			X
Disposed			X
Fine			X
Judgement			X
Other			X
Plead guilty		X	X
Prison		X	X
Referred to lok adalat			X
Reject			X
Remanded			X
Transferred			X
Withdrawn			X
Missing			

Notes: This table illustrates the classification of the disposition types of the sample case records into three outcome variables. If a case has a disposition at all, the indicator variable *Decision* equals 1, and 0 otherwise. Conditional on having a disposition, if the disposition is clearly acquitted, the outcome variable *Acquitted* takes the value 1, and 0 otherwise. The outcome variable for conviction has been coded analogously.

Figure A4: Distribution of courts across districts, for both analysis samples

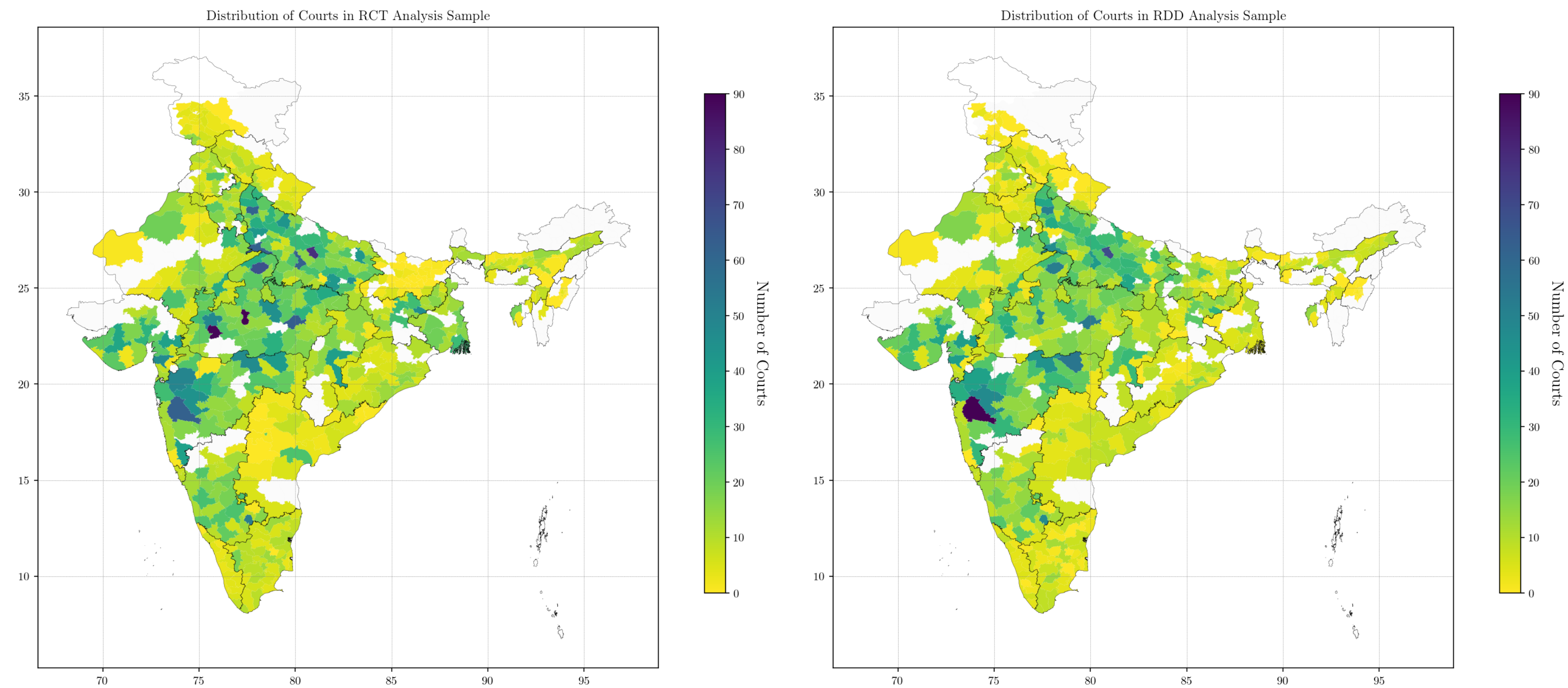


Table A3: Summary of charges, by gender of defendant

	(1) Female share	(2) Female share/ population share/	(3) Female acquittal rate	(4) Male acquittal rate	(5) Difference (3) - (4)	(6) Number of cases
Murder	0.101	0.210	0.249	0.183	0.066	1,129,000
Sexual assault	0.085	0.177	0.275	0.235	0.040	254,928
Violent crimes causing hurt	0.116	0.242	0.213	0.187	0.026	1,846,000
Violent theft/dacoity	0.079	0.165	0.170	0.148	0.022	252,046
Crimes against women	0.093	0.194	0.274	0.248	0.026	725,388
Disturbed pub. health/tranquility	0.063	0.131	0.096	0.075	0.021	1,852,000
Property Crime	0.106	0.221	0.184	0.158	0.026	2,558,000
Trespass	0.115	0.240	0.223	0.202	0.021	339,045
Marriage offenses	0.120	0.250	0.271	0.264	0.007	326,214
Petty theft	0.103	0.215	0.180	0.149	0.031	946,890
Other crimes	0.119	0.248	0.204	0.177	0.027	9,008,000
Total	0.108	0.225	0.201	0.167	0.034	17,170,000

Notes: Column 1 of this table reports the share of female defendants for each crime category. Column 2 reports the ratio of the female share for each crime and the female population share in India. Column 3 reports the conviction rate for females accused of each crime category. Column 4 reports the analogous conviction rates for males. Column 5 reports the difference in female and male conviction rates for each crime category. Column 6 reports the total number of case records in each crime category.

Table A4: Summary of charges, by religion of defendant

	(1) Muslim share	(2) Muslim share/ population share/	(3) Muslim acquittal rate	(4) Non-Muslim acquittal rate	(5) Difference (3) - (4)	(6) Number of cases
Murder	0.135	0.951	0.182	0.193	-0.011	1,204,000
Sexual assault	0.163	1.148	0.241	0.238	0.003	271,622
Violent crimes causing hurt	0.141	0.993	0.187	0.191	-0.004	1,980,000
Violent theft/dacoity	0.194	1.366	0.140	0.152	-0.012	271,901
Crimes against women	0.193	1.359	0.260	0.248	0.012	771,555
Disturbed pub. health/tranquility	0.164	1.155	0.078	0.075	0.003	2,002,000
Property Crime	0.165	1.162	0.161	0.161	0.000	2,711,000
Trespass	0.144	1.014	0.200	0.206	-0.006	362,459
Marriage offenses	0.230	1.620	0.285	0.261	0.024	344,708
Petty theft	0.180	1.268	0.153	0.153	0.000	1,003,000
Other crimes	0.136	0.958	0.195	0.178	0.017	9,556,000
Total	0.147	1.035	0.177	0.170	0.007	18,280,000

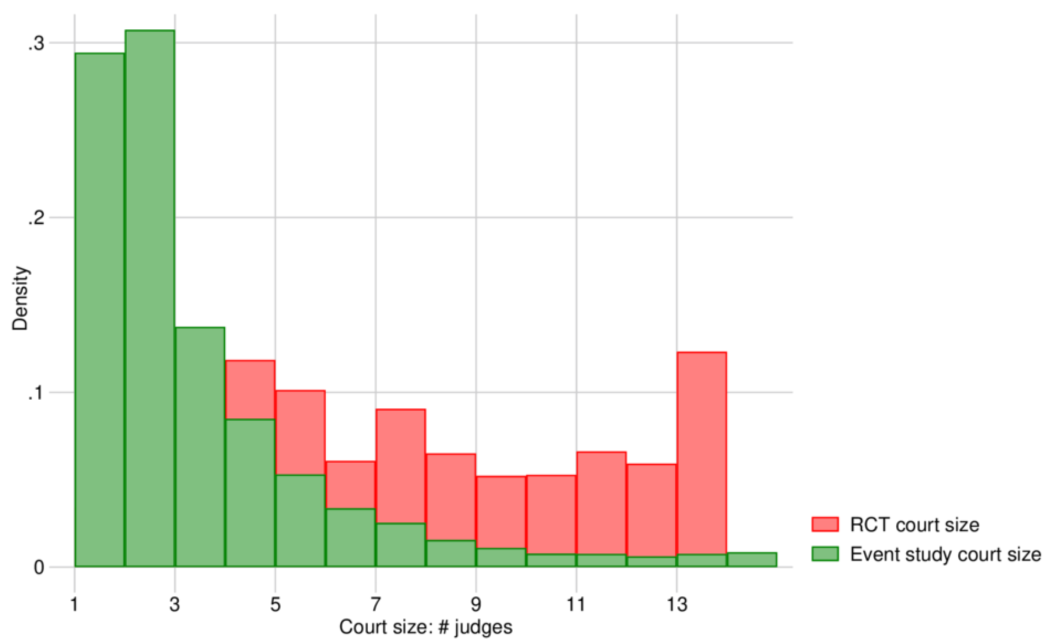
Notes: Column 1 of this table reports the share of Muslim defendants for each crime category. Column 2 reports the ratio of the Muslim share for each crime and the Muslim population share in India. Column 3 reports the conviction rate for Muslims accused of each crime category. Column 4 reports the analogous conviction rates for non-Muslims. Column 5 reports the difference in Muslim and non-Muslim conviction rates for each crime category. Column 6 reports the total number of case records in each crime category.

Table A5: Summary of Coefficients for Case-Assignment Analysis

	Female Defendant	**Male Defendant**	**Defendant Difference Female - Male**
Female Judge	$\beta_1 + \beta_2 + \beta_3$	β_1	$\beta_2 + \beta_3$
Male Judge	β_2	–	β_2
Judge Difference Female - Male	$\beta_1 + \beta_3$	β_1	**Diff-in-Diffs:** β_3

Notes: This table delineates the meaning of each coefficient that appears in Equation 3. The coefficients have analogous meanings in the religion bias analysis specification — Equation 4.

Figure A5: Distribution of court size in the analysis samples



Note: The median courts in the RCT and event study samples have 5 judges and 2 judges respectively

Table A6: Impact of assignment to a male judge on non-conviction

	Outcome variable: Not convicted					
	(1)	(2)	(3)	(4)	(5)	(6)
Male judge on female defendant	0.003**	.002	—	0.003*	.002	—
	(0.002)	(0.002)		(0.002)	(0.002)	
Male judge on male defendant	0.003**	.002	—	0.003*	.002	—
	(0.002)	(0.002)		(0.002)	(0.002)	
Difference = Own gender bias	0	0	.001	0	0	.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Reference group mean	0.947	0.947	0.947	0.947	0.947	0.947
Observations	5250907	5156887	5155378	5264320	5170380	5168583
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Table A7: Impact of assignment to a male judge on acquittal rates, dropping ambiguous outcomes

	Outcome variable: Acquittal rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Male judge on female defendant	-.003	-.008	—	-.005	-0.009*	—
	(0.005)	(0.005)		(0.004)	(0.005)	
Male judge on male defendant	0	-.005	—	-.001	-.006	—
	(0.004)	(0.005)		(0.004)	(0.004)	
Difference = Own gender bias	.002	.003	.004	.003	.003	.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Reference group mean	0.663	0.664	0.664	0.666	0.666	0.666
Observations	1182938	1162073	1159551	1203939	1183155	1180587
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Table A8: Impact of assignment to a male judge on acquittal rates, keeping years with the highest sample share (2014–2018)

	Outcome variable: Acquittal rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Male judge on female defendant	-0.009***	-0.009***	—	-0.009***	-0.008***	—
	(0.003)	(0.003)		(0.002)	(0.003)	
Male judge on male defendant	-0.007***	-0.007***	—	-0.007***	-0.006**	—
	(0.002)	(0.003)		(0.002)	(0.003)	
Difference = Own gender bias	.002	.002	0	.002	.002	0
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Reference group mean	0.156	0.157	0.157	0.157	0.158	0.158
Observations	4838005	4752974	4751550	4846724	4761759	4760079
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Table A9: Impact of assignment to a male judge on whether the disposition is ambiguous

	Outcome variable: Ambiguous outcome					
	(1)	(2)	(3)	(4)	(5)	(6)
Male judge on female defendant	0.011***	0.008**	—	0.010***	0.007**	—
	(0.003)	(0.004)		(0.003)	(0.004)	
Male judge on male defendant	0.011***	0.008**	—	0.010***	0.007**	—
	(0.003)	(0.003)		(0.003)	(0.003)	
Difference = Own gender bias	0	0	.002	0	0	.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Reference group mean	0.737	0.736	0.736	0.737	0.735	0.735
Observations	5250907	5156887	5155378	5264320	5170380	5168583
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Table A10: Impact of assignment to a male judge on whether the first assigned judge was the deciding judge

	Outcome variable: Filing judge reached decision					
	(1)	(2)	(3)	(4)	(5)	(6)
Male judge on female defendant	.003	.006	—	.002	.004	—
	(0.006)	(0.007)		(0.007)	(0.008)	
Male judge on male defendant	-.001	.002	—	-.003	-.001	—
	(0.006)	(0.007)		(0.006)	(0.007)	
Difference = Own gender bias	-.004	-.004	-.002	-0.005*	-0.005*	-.003
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Reference group mean	0.511	0.511	0.511	0.51	0.511	0.511
Observations	5250907	5156887	5155378	5264320	5170380	5168583
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Table A11: Impact of assignment to a non-Muslim judge on non-conviction

	Outcome variable: Not convicted					
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Muslim judge on Muslim defendant	.002	-.003	—	.001	-0.004*	—
	(0.003)	(0.003)		(0.003)	(0.003)	
Non-Muslim judge on non-Muslim defendant	.005	.001	—	.005	.001	—
	(0.003)	(0.003)		(0.004)	(0.003)	
Difference = Own religion bias	0.003*	0.004**	0.003*	0.004*	0.005**	0.003**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Reference group mean	0.936	0.937	0.937	0.937	0.938	0.938
Observations	5684426	5241649	5240140	5697480	5255137	5253328
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Table A12: Impact of assignment to a non-Muslim judge on acquittal rates, dropping ambiguous outcomes

	Outcome variable: Acquittal rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Muslim judge on Muslim defendant	.01	0	—	.007	-.006	—
	(0.006)	(0.008)		(0.006)	(0.007)	
Non-Muslim judge on non-Muslim defendant	0.010*	.002	—	.009	-.002	—
	(0.006)	(0.007)		(0.006)	(0.007)	
Difference = Own religion bias	0	.002	-.002	.002	.004	0
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)
Reference group mean	0.676	0.682	0.682	0.677	0.683	0.683
Observations	1285585	1187003	1184449	1306413	1208242	1205650
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Table A13: Impact of assignment to a non-Muslim judge on acquittal rates, keeping years with the highest sample share (2014–2018)

	Outcome variable: Acquittal rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Muslim judge on Muslim defendant	0.008**	.008	—	0.008**	.007	—
	(0.004)	(0.005)		(0.004)	(0.005)	
Non-Muslim judge on non-Muslim defendant	0.008**	0.008*	—	0.008***	0.007*	—
	(0.003)	(0.004)		(0.003)	(0.004)	
Difference = Own religion bias	0	0	.001	0	.001	.001
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Reference group mean	0.156	0.159	0.159	0.156	0.159	0.159
Observations	5237310	4829586	4828147	5245681	4838328	4836629
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Table A14: Impact of assignment to a non-Muslim judge on whether the disposition is ambiguous

	Outcome variable: Ambiguous outcome					
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Muslim judge on Muslim defendant	-.006	-0.013**	—	-.006	-0.013**	—
	(0.005)	(0.006)		(0.005)	(0.006)	
Non-Muslim judge on non-Muslim defendant	-.002	-.008	—	-.002	-.008	—
	(0.005)	(0.006)		(0.005)	(0.006)	
Difference = Own religion bias	.004	.005	.001	.005	.005	.001
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)
Reference group mean	0.735	0.732	0.732	0.734	0.732	0.732
Observations	5684426	5241649	5240140	5697480	5255137	5253328
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Table A15: Impact of assignment to a non-Muslim judge on whether the first assigned judge was the deciding judge

	Outcome variable: Filing judge reached decision					
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Muslim judge on Muslim defendant	.005	.001	—	-.003	-.006	—
	(0.011)	(0.012)		(0.012)	(0.013)	
Non-Muslim judge on non-Muslim defendant	.006	.002	—	.003	.001	—
	(0.009)	(0.01)		(0.009)	(0.01)	
Difference = Own religion bias	.001	.001	.004	.006	.007	0.005*
	(0.007)	(0.007)	(0.003)	(0.008)	(0.008)	(0.003)
Reference group mean	0.513	0.512	0.513	0.513	0.512	0.513
Observations	5684426	5241649	5240140	5697480	5255137	5253328
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Table A16: Impact of assignment to a male judge when the offence was a crime against women

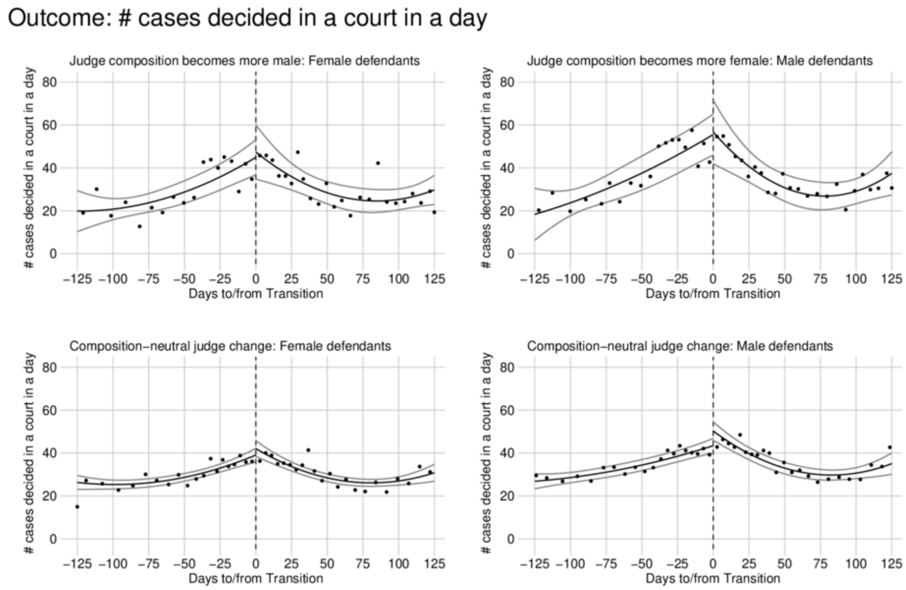
	Outcome variable: Acquittal rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Male judge on female defendant	-0.027***	-0.018*	—	-0.029***	-0.020**	—
	(0.009)	(0.01)		(0.008)	(0.009)	
Male judge on male defendant	-0.028***	-0.021***	—	-0.025***	-0.018***	—
	(0.005)	(0.007)		(0.005)	(0.007)	
Difference = Own gender bias	-.001	-.003	-.004	.004	.002	.003
	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)
Reference group mean	0.27	0.27	0.27	0.276	0.276	0.277
Observations	261459	258216	255314	280236	276998	273964
Demographic controls	No	Yes	Yes	No	Yes	Yes
Judge fixed effect	No	No	Yes	No	No	Yes
Fixed Effect	Court-month	Court-month	Court-month	Court-year	Court-year	Court-year

Standard errors in parentheses

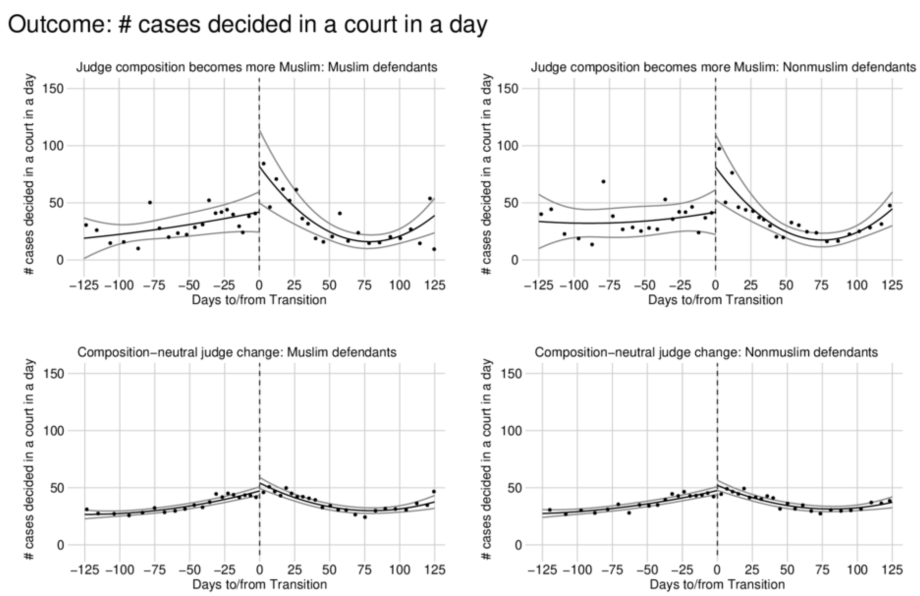
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A6: Distribution of Cases Is Even at Transition Events

Panel A. Gender



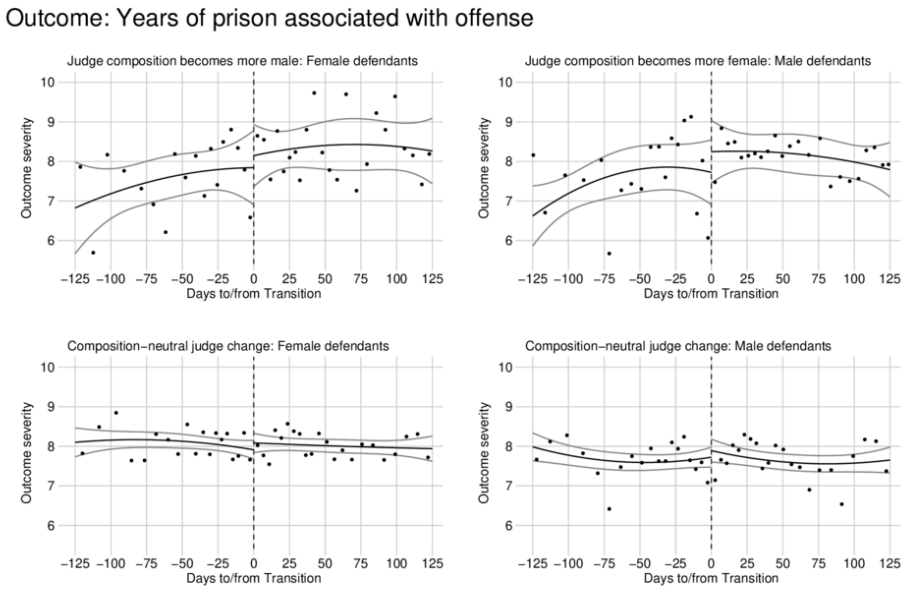
Panel B. Religion



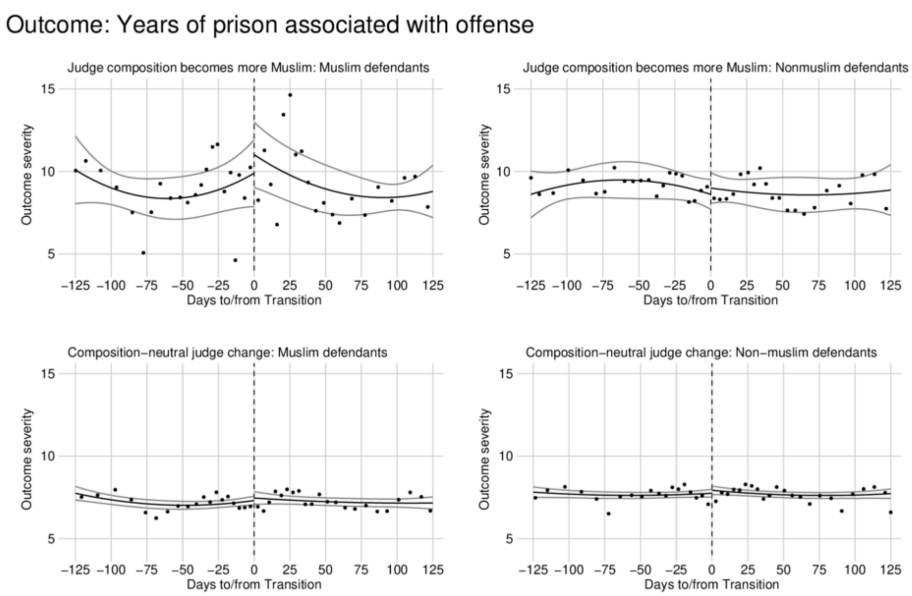
Notes: Panels A and B illustrate that the number of cases decided in a court does not vary when the composition of judges in a court becomes more female or more Muslim respectively.

Figure A7: Judge Transition Events do not Affect Charge Severity

Panel A. Gender



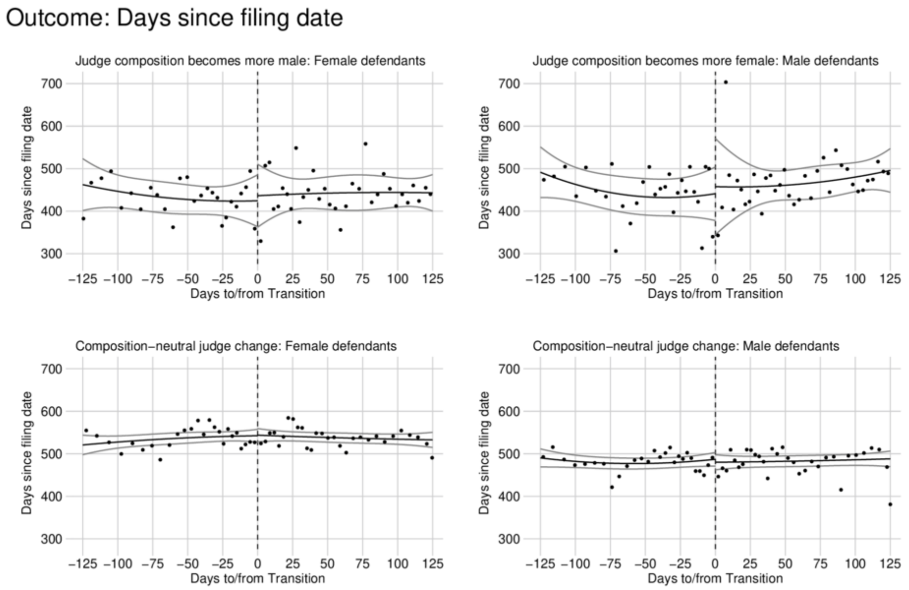
Panel B. Religion



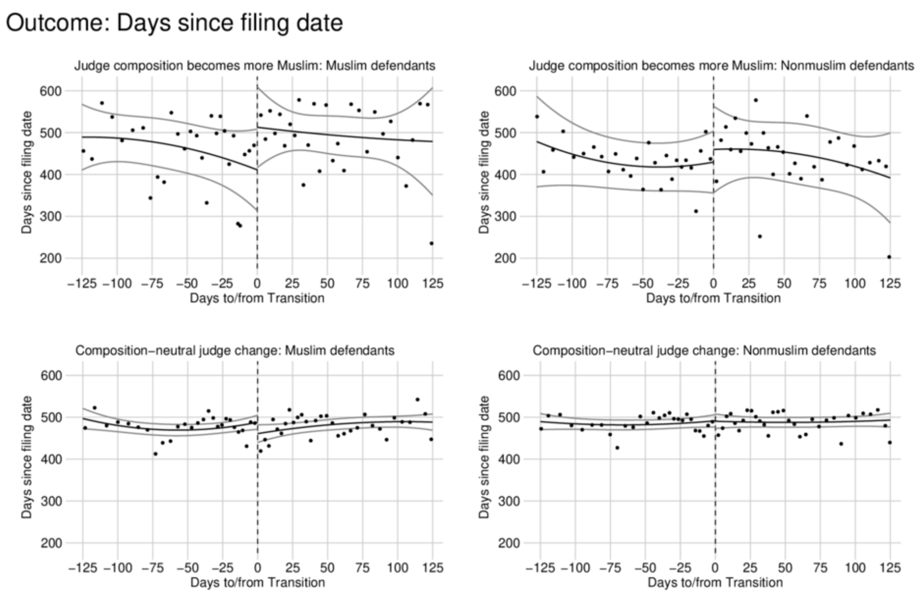
Notes: Panels A and B illustrate that the severity of cases decided, as measured by years of punishment associated with the charge of each case, does not vary when the composition of judges in a court becomes more female or more Muslim respectively.

Figure A8: Effect of Judge Composition Change on Case Delay

Panel A. Gender



Panel B. Religion



Notes: Panels A and B illustrate that the time it takes for a case to reach completion does not vary when the composition of judges in a court becomes more female or more Muslim respectively.

Table A17: Impact of judge transitions that affect court composition on non conviction rates

	Gender composition changes		Religion composition changes	
	(1)	(2)	(3)	(4)
	not convicted	not convicted	not convicted	not convicted
Pro-defendant transition	-0.004	-0.002	-0.009	-0.004
	(0.005)	(0.005)	(0.006)	(0.006)
Against-defendant transition	-0.005	-0.000	-0.002	0.001
	(0.006)	(0.005)	(0.007)	(0.007)
Constant composition judge transition	0.002	-0.000	0.003	0.003
	(0.003)	(0.003)	(0.002)	(0.002)
Observations	371959	415857	503774	552088
No. of transitions	2784	4243	4105	5853
Mean non-convicted	0.919	0.919	0.919	0.919
Fixed effect	Court-month	Court-year	Court-month	Court-year

Notes: This table presents regression discontinuity estimates from the main estimating equation of the effect of judge transitions that affect court composition on non-conviction rates of defendants. Columns 1–2 estimate the impact of a court transition that increases or decreases the share of judges belonging to the defendant's gender, using different fixed effects. Columns 3–4 estimate the analogous impact on acquittal rates for court transitions that change the religion share of judges in the court (see Section 4 for specification details). Sample bandwidth is 25 days before and after the transition. Charge section fixed effects have been used across all columns reported. Heteroskedasticity robust standard errors are reported below point estimates.