In-group bias in the Indian judiciary: Evidence from 5 million criminal cases

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Abstract

We study judicial in-group bias in Indian criminal courts, collecting data on over 5 million criminal case records from 2010–2018. We exploit quasi-random assignment of cases to judges to examine whether defendant outcomes are affected by assignment to a judge with a similar identity. We estimate tight zero effects of in-group bias based on shared gender, religion, and last name (a proxy for caste). We do find limited in-group bias in some (but not all) settings where identity is particularly salient, though even here the effect sizes are smaller than those in much of the prior literature.

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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 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.

This paper examines bias in India's courts, asking whether judges deliver more favorable treatment to defendants who match their identities. The literature suggests that judicial bias along gender, religious, or ethnic lines is 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), election workers (Neggers, 2018), and school teachers (Hanna and Linden, 2012). But the judicial setting is of particular interest, given the premise that individuals who are discriminated against in informal settings can find recourse from equal treatment under the law (Sandefur and Siddiqi, 2015).

We focus on the dimensions of gender, religion, and caste, motivated by growing evidence that India's women, Muslims and lower castes do not enjoy equal access to economic or other opportunities (Ito, 2009; Bertrand et al., 2010; Hanna and Linden, 2012; Jayachandran, 2015; Borker, 2017; Asher et al., 2020). In India's lower courts, unequal representation is a recognized issue. Women represent half the population but only 28% of district court judges. Similarly, India's 200 million Muslims represent 14% of the population but only 7% of district court judges.[2] We examine whether unequal representation in the courts has a direct effect on the judicial outcomes of women, Muslims and lower castes, in the form of judges delivering better outcomes to criminal defendants who match their identities.

Our analysis draws upon a new dataset of 5 million criminal court records covering 2010–2018 from *eCourts*, an online government platform containing data on the com-

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

[2] Source: eCourts data, see Table 1. We did not find a statistic for overall representation of Scheduled Castes in the judiciary, but partial evidence suggests they are also underrepresented (Times of India, 2018).

plete set of cases heard in India's district courts.[3] These cases cover the universe of India's 7,000+ district and subordinate trial courts, staffed by over 80,000 judges. We have released an anonymized version of the dataset, opening the door to many new analyses of the judicial process in the world's largest democracy and largest common-law legal system.[4]

We classify judges and defendants to gender and religious (Muslim and non-Muslim) identity groups using a deep neural network applied to the sequence of characters in the names of each judge and defendant. The distinctive nature of female and Muslim names allows us to classify individuals with over 97% out-of-sample accuracy on both dimensions, a significantly higher rate than is achieved by fuzzy matching. Caste identity is complex and hierarchical, making it difficult to identify binary in-groups and out-groups. To measure caste bias, we define an identity match as a case where the defendant's last name matches the judge's last name. This is an imperfect measure because multiple family names may reflect the same caste and certain last names may be used by members of many castes. Nevertheless, for many names, individuals in the same region who share a last name are likely to belong to the same caste.[5]

The research question is whether judges treat defendants differently when they share the same gender, religion, or caste. We focus on the subset of cases filed under India's criminal codes, where acquittal and conviction rates can be interpreted as positive and negative outcomes, respectively. Given the extreme delays in India's judicial system (Trusts, 2019; Rao, 2019), we additionally examine whether in-group judge identity affects the court's speed in reaching a decision.

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. This approach effectively compares 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.[6]

[3]eCourts can be accessed at `https://ecourts.gov.in/`. That site hosts the case records only through a slow search engine that returns unstructured results. The data was not previously available as a structured dataset or API.

[4]The data can be accessed at `https://www.devdatalab.org/judicial-data`. The total dataset – civil and criminal, without filtering – contains 77 million case records. Users of the data are asked to cite this paper.

[5]Using the same last name to classify identity groups has predicted preferential outcomes in previous work, for instance in the banking setting (Fisman et al., 2017).

[6]Results are robust to adding judge fixed effects (which control for variation in the severity of

We find a precise and robust null estimate of in-group bias among Indian judges on all three dimensions. Sharing gender, religion, or last name with a defendant makes a judge no more likely to deliver a positive outcome. This null is seen both in decisions (i.e. acquittals and convictions) and in process (i.e. speed of decision). The confidence intervals rule out effect sizes that are an order of magnitude smaller than nearly all prior estimates of in-group bias based on similar identification strategies in the literature.[7] The upper end of our 95% confidence interval rejects a 0.6 percentage point effect size in the worst case; studies using the same identification strategy in other contexts have routinely found bias effects ranging from 5 to 20 percentage points (see Figure 2).

Notwithstanding a null effect of in-group bias on average, bias could be activated in contexts where judge and defendant identity are more salient. We examine four special contexts that the literature suggests may prime in-group bias (Mullen et al., 1992; Shayo and Zussman, 2011; Anwar et al., 2012; Mehmood and Seror, 2020). First, we examine cases where the defendant and the victim of the crime have different identities. Sharing identity with the victim when the defendant is in an out-group could, by creating an external reference point, activate the judge's sense of opposite identity with the defendant. Second, we examine gender bias in criminal cases categorized as crimes against women, which are mostly sexual assaults and kidnappings. Here, the shared identity of gender is intrinsic to the substance of the case, and may thus be more salient. In both of these subset analyses, we continue to find a null bias.

Third, we examine whether in-group bias on the basis of religion is activated during the month of Ramadan, when religious identity may be more salient. We find suggestive evidence that being assigned to a judge with the same religious identity (i.e. Muslim or non-Muslim) has a 1.5–2.0 percentage point higher acquittal rate *when the case is heard during Ramadan.* The estimate is only marginally statistically significant due to the smaller sample during Ramadan months.[8] This result confirms that district judges have discretion in their decisions and may apply that discretion in favor of an in-group

specific judges), though these are not expected to make a difference under random assignment of cases to judges.

[7]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, notably the statistically highest-powered study in this class before ours.

[8]This point estimate is small when compared with the prior literature. In particular, Mehmood and Seror (2020) find in Pakistan that conviction rates fall by 14 percentage points during Ramadan, or 1 percentage point for each additional hour of fasting. In that study, nearly all judges and defendants are Muslim, so the effect of identity plays a different role than in ours, as we exploit differences between Muslim and non-Muslim judges during Ramadan. 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.

if their identity is activated. But in most cases, and most of the time, the extent of religious and gender in-group bias in acquittal and conviction rates in Indian courts is effectively zero.

Finally, we examine the last name bias when defendants have uncommon last names. In this case, the shared identity with the judge is more narrowly defined, which may magnify the sense of shared identity. Here, we find statistically and economically important signs of pro-in-group bias. The effect remains small in aggregate, because it applies to a narrow subset of defendants who both have uncommon names and are lucky enough to be assigned a judge with the same uncommon name. However, we cannot rule out that judges show bias based on other markers of caste that we do not observe.

Our estimates do not rule out bias on the basis of identity in a general sense. For example, both Muslim and non-Muslim judges could discriminate against Muslims and both male and female judges could provide unfair judgments to women (as found for U.S. Black defendants in Arnold et al. (2018)). There could also be bias higher up the judicial pipeline: arrests and/or charges may disproportionately target Muslims, or charges brought by women may not taken as seriously by the police. Our null estimates are nevertheless notable, given substantial evidence of this kind of bias in other countries, and in other settings in India.

In Section 6, we discuss several potential reasons that bias could be small in our setting, given its apparent ubiquity in other judicial settings and other Indian contexts. At face value, the results suggest that rule-of-law institutions and judicial norms effectively prevent favoritism for in-groups. Other factors that might influence the degree of bias include the extent that the context is adversarial or cooperative, the class distance between judge and defendant, or, as suggested by the rare-last-name result, the overall salience of the shared identity group. The finding that in-group bias emerges only in cases where identity is made salient is also informative for our understanding of prior work, which consistently finds large in-group effects in the judicial domain. The most similar prior studies focus on the United States and Israel. These may be institutional contexts where race, ethnic, or religious identity are particularly salient during the judicial process. The U.S. incarceration system, especially in Southern states, ended up reproducing many aspects of the slave system that preceded it (Alexander, 2010). With this historical legacy, it is perhaps unsurprising to find that defendant race is a highly salient feature of many U.S. criminal cases.

However, another potential contributing factor could be publication bias in the social-science literature on judicial bias, so that contexts *without* in-group bias are not prominently described in completed papers. When we aggregate the effect sizes and standard errors from earlier papers with highly similar empirical designs to ours, we find circumstantial evidence consistent with publication bias. Some studies with null results seem to be missing from the literature, perhaps due to projects being abandoned or failing to make it through the peer review process.

Our study makes two substantive contributions. First, contrary to most of the existing literature, we demonstrate a notable absence of judicial in-group bias in an important low-income-country context with substantial religious, ethnic, and gender-based cleavages. Because the size of our sample is orders of magnitude larger than nearly all prior studies, we are able to measure this (absence of) bias much more precisely than prior work. We also analyze the universe of criminal cases, heading off most concerns about external validity within the study context. Second, our findings of differential bias effects in certain special cases — when in-group size is small or when the external environment increases the salience of identity — helps shed light on contexts where bias may be more or less likely to occur. In particular, the large and significant bias results for Jewish versus Arab defendants in Israel, and Black versus White defendants in the U.S. (described below), are found in contexts where ethnic identity is salient to the extreme, in-groups are well defined and recognizable, and the external environment is heightened.

More specifically, our substantive results add to the 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.[9] The closest paper to ours is Shayo and Zussman (2011), who analyze the effect 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). Our finding

[9] 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., 2018), 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.

that religious bias is magnified during the month of Ramadan is consistent with their notion of endogenous social identification, though our point estimates on bias are an order of magnitude smaller even under these high-salience conditions.

A handful of other studies use quasi-random designs to estimate in-group biases in a similar fashion to our analysis. While most of these papers report large and statistically significant pro-in-group effects, one paper finds anti-in-group bias.[10] Of the papers we could find, only Lim et al. (2016) find a null in-group effect of judge ethnicity or gender, notably with the largest sample size in this set of papers (N=250,000).

In the Indian legal 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). A recent working paper finds that judges are more prone to deny bail if they had been exposed to communal riots in early childhood (Bharti and Roy, 2020). We are aware of no prior large-scale empirical research on unequal legal treatment 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, 2005; Visaria, 2009; Lichand and Soares, 2014; Ponticelli and Alencar, 2016; 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. Hence, a final key contribution of this paper is the 77 million case dataset that we have posted, which may enable a wide range of future research projects in this domain.

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

[10] 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.

literature and concludes.

2 Background

2.1 Gender and Religion in India

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. Women constitute 48% of the population, and remain vulnerable to social practices such as female infanticide, child marriage, and dowry deaths despite existing legislation outlawing all of the above. India accounts for one third of all child marriages globally (Cousins, 2020) and nearly one third of the 142.6 million missing females in the world (Erken et al., 2020).

Muslims in India (14% of the population) have historically had intermediate socioeconomic outcomes worse than upper caste groups but better than lower caste groups (Sachar Committee Report, 2006). 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. The marginalized statuses of women and Muslims in India motivate our exploration of the role of gender and religion in the context of India's criminal justice system.

2.2 India's Court System

India's judicial system is organized in a jurisdictional hierarchy, 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 (which we study here) collectively constitute India's lower judiciary. These courts represent the point of entry of almost all criminal cases in India.[11]

These courts are staffed by over 80,000 judges. Due to common law institutions where court rulings serve as binding precedent in future cases, judges in India are effectively policymakers. Indian judges are arguably even more powerful than their

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

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 a leading issue for governance.

Lower-court judges in India are appointed by the governor in consultation with the state high court's chief justice. At least seven years of legal practice are required as a minimum qualification. The recruitment process entails a written examination and oral interview by a panel of higher-court judges. Judge tenure is in general well-protected, with removal by the governor only possible with the agreement of the high court. Finally, district judges can be promoted to higher offices in the judiciary after specific numbers of years in their post.

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 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 by creating 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.

2.3 Case Assignment to Judges

The procedure of case assignment to judges is pivotal for this study because our empirical strategy hinges on the 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, and several 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 receives the case. The case is then assigned to a judge sitting in that courthouse. If there is just one judge available to see cases in the courthouse, that judge gets the case.

If there are multiple judges, a rule-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. The police station charge lists leave little room for discretion over which charges are seen by which judges.

Judges typically spend two to three years in a given court, during which they rotate through several of the courtrooms.[12] The timing of the first court appearance is unknown when charges are filed (given judicial delays). Thus, 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 in search of a favorable match. One of the earliest cases in which the Indian Supreme 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 to select a favorable judge, writing that judge shopping "must be crushed with a heavy hand." This decision has been cited heavily in subsequent judgments.[13,14]

In U.S. courts, a large share of criminal cases are disposed through plea bargaining, making appearance in court itself an endogenous outcome. This is not a concern in our context. While plea bargaining was introduced in India in the early 2000s, less than 0.5% of all criminal cases pending in India are disposed through plea bargaining. It is thus unlikely to play a major factor in our analysis.

3 Data

3.1 Case Records

We obtained 77 million case records from the Indian eCourts platform — a semi-public system put in place by the Indian government to host summary data and full text from orders and judgments in courts across the country.[15] The publicly available information includes the filing, registration, hearing, and decision dates for each case, petitioner and

[12] 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 may be eligible to be seen only by a subset of judges in that district.

[13] Since 2013, there has been a random assignment lottery mechanism available through the eCourts platform, but few courts have adopted it to date.

[14] In Section 4, we present formal tests of the exogenous assignment of judges to cases in our dataset.

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

respondent names, the position of the presiding judge, the acts and sections under which the case was filed, and the final decision or disposition.[16]

The database covers India's lower judiciary, consisting of all courts including and under the jurisdiction of District and Sessions courts. This paper focuses 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, which is more difficult in civil cases. This constraint filters out 70% of the dataset, leaving us with 23 million criminal case records (see Appendix Figure A2).

3.2 Judge Information

We also obtained data on judges in 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.[17]

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 10 million cases. From this subset, we drop all bail decisions, which are a narrow share of the data. We then drop cases where we cannot identify both defendant and judge identity (depending on whether we are analyzing religion or gender, see below). Finally, we drop cases in courts where there is only one judge in a given time period. This leaves 5.7 million cases in the religion analysis and 5.3 million in the gender analysis (see Appendix Figure A2).

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.[18]

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

[17]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.

[18]The existing available name classifiers for gender and religion in India are expensive proprietary solutions, e.g. Namsor (namsor.com), and trials with these yielded the same or lower accuracy than

We use two databases of names with associated demographic labels. To classify gender, we use a dataset of 13.7 million names with labeled gender from the Delhi voter rolls. 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 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.

The lists of labeled names from the Delhi voter rolls and National Railway Exam contain some inconsistent formatting and noise which we clean up with a set of pre-processing steps. First, Hindi characters are transliterated to Latin. Second, we normalize capitalization, punctuation, and spacing. Salutations are preserved as they indicate gender.

Taking these pre-processed name strings as inputs, we train a neural net classifier to predict the associated identity label with an approach similar to Chaturvedi and Chaturvedi (2020). We use a bidirectional Long Short-Term Memory (LSTM) model applied directly to the sequence of name-string characters. LSTM uses a gated recurrent neural network architecture that takes as input a sequential data stream and retains a memory of previous inputs while handling new items in the 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 backward and forward when trying to assign a label.[19]

The ability of the LSTM classifier to understand a text fragment within context greatly improves accuracy over standard fuzzy string matching methods. For instance,

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our own classifier.

[19]In more detail, 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.

consider the last names *Khan* and *Khanna*. While the fragment *KHAN* appears in both words, the addition of two letters *na* following the fragment changes the meaning of the word where it is a distinctly Muslim last name without the letters *na*, and a non-Muslim last name once the letters *na* are added. A standard fuzzy match would fail on this example because it ignores the context (that is, the sequence of letters that appear before and after the fragment *KHAN*). A counter-example are the names *Fatima* and *Fathimaa*, where the addition of the letters *h* and *a* do not change the religious classification of the name. Given these nuances, the LSTM classifier is better suited to the objective than a simple fuzzy matching function.

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 on standard metrics, including our preferred metrics that adjust for imbalance in the class shares. We report balanced accuracy, which is the average accuracy (recall) for each of the two identity categories, and F1, the harmonic mean of precision and recall.[20] For gender, the balanced accuracy is .975 with F1 = .976. For religion, the balanced accuracy is .98 and F1 = .99.

The next step is to apply the trained classifier to the eCourts case records. We have plain-text string variables for judge name and defendant name, to which we apply the same pre-processing steps as above (i.e., transliteration and normalization of punctuation/capitalization). We filter out names that are not possible to classify, for example due to emptiness. For defendants, in addition, we drop names that refer to governments or organizations (e.g. "The State of Maharashtra").

For each pre-processed judge name and defendant name, we then apply the trained classifier and form a predicted probability for gender and religion. To improve precision, we further filter names which do not produce a confident classification. Based on inspection of the outputs, we require that the model is at least 65% confident in a predicted gender or religion classification. For predicted probabilities between 0.35 and 0.65, the respective class is left empty. This happens for gender, for example, when the first name is missing and no salutation is included.

[20] 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.

For judges, names tend to be complete or else include salutations. Of the 81,232 judges (22,413 unique names) appearing in the case dataset, we are able to classify 96% according to gender (female/male) and 98% according to religion (Muslim/non-Muslim). The information on defendant names is of lower quality, mainly due to missing first or last names. Still, we are able to classify 80% of defendants by religion and 74% by gender.[21] 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 manually checked a random sample of names classified by the above process. An annotator manually labeled 100 names by gender and 100 names by religion, with samples stratified across states. This process confirms an accuracy of 97% for both the gender and religion classification in the new domain.[22]

3.4 Defining Case Outcomes

We define the defendant's outcome (represented by Y below) as a case-level indicator variable that takes the value one if the outcome is desirable for the defendant and zero otherwise. Our primary specification uses an indicator for defendant acquittal. A secondary specification uses an indicator for any outcome other than conviction. 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 simply as "disposed," with no additional judgment information uploaded for the case. For cases like these, 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* (Appendix Table A2). 27% of case dispositions can be clearly designated as good or bad, with the remainder ambiguous; we show that our results are robust when we restrict the sample to cases with unambiguous outcomes and that ambiguity is not in itself affected by in-group bias.

Judicial delay is also a major policy issue in India, so getting a decision at all is therefore itself an outcome of interest. We define an outcome indicator for whether a decision is made on a case within six months of the case's filing date.

[21] The proportion of defendants that can be assigned to gender and religion does not vary much by region of India.

[22] As an additional automated validation, we compared the LSTM-classified Muslim defendant share by state to the state-level Muslim population shares from the 2011 Population Census. The correlation is 0.88.

3.5 Summary Statistics on Case Outcomes

Figure 1 presents descriptive statistics of charges and convictions by gender and religious identity of defendants, respectively.[23] These summary measures are descriptive in nature, but are not directly informative of bias in the judicial system because we do not know the share of defendants who commit crimes or are guilty when charged.

Figure 1 Panel A shows that the share of women 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).

Panel C shows that Muslims are over-represented by 3% in the universe of criminal charges. Representation changes substantially depending on the change: relative to their population share, Muslims are 36% more likely to be charged with crimes against women, 37% more likely to be charged with robbery, and 62% more likely to be charged with marriage offenses, but 5% less likely to face charges for murder. Panel D shows that aggregate differences in conviction rates between Muslims and non-Muslims are small.

Table 1 shows descriptive statistics of judges in the analysis sample. About 27% of judges are female and 6.8% of judges are Muslim. On average, Muslim and female judges have similar conviction and decision rates to non-Muslim and male judges. Appendix Figure A4 maps the geographic distribution of our sample of courts, which covers the whole country.

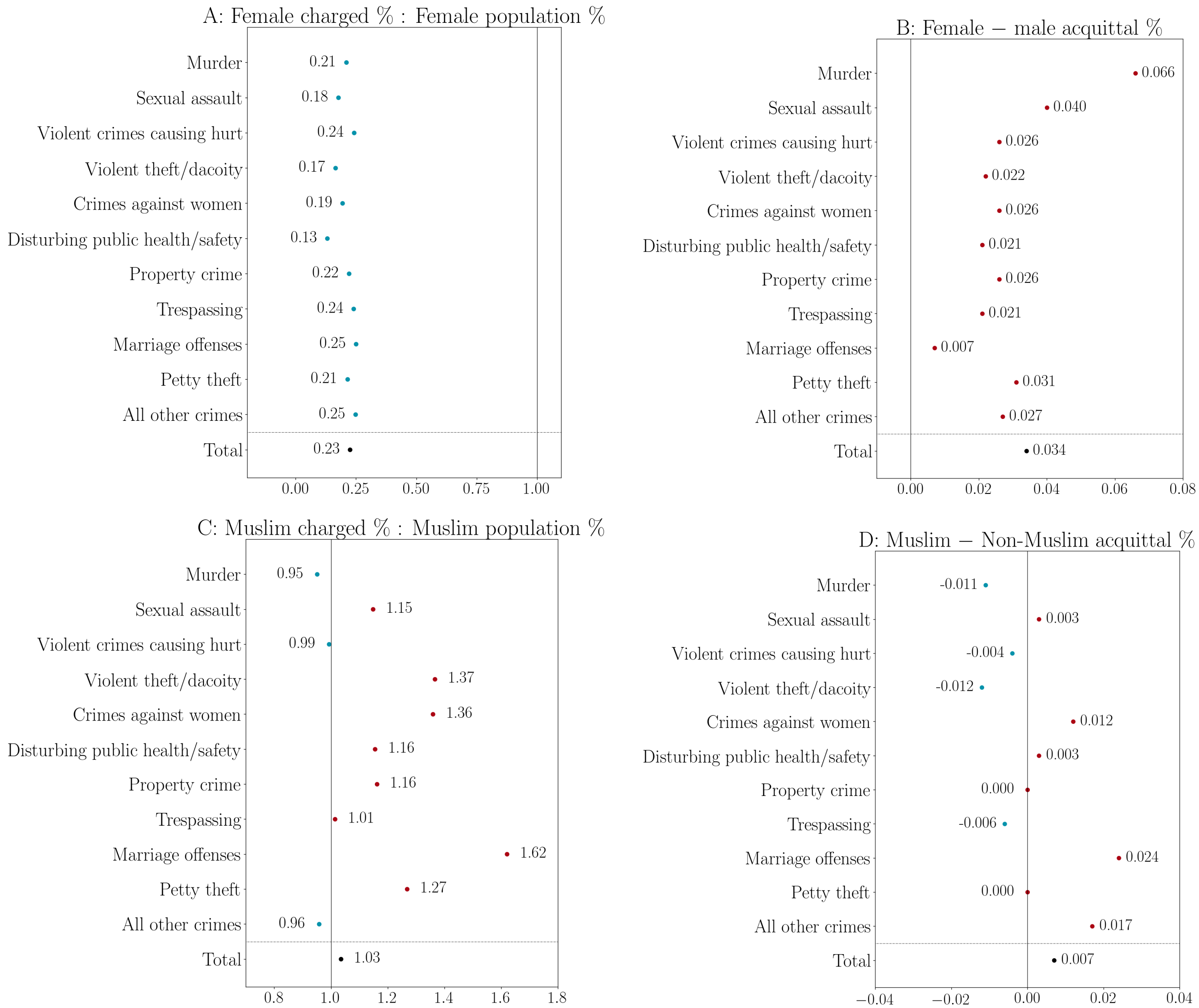
4 Empirical Strategy

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.

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. We formalize our empirical approach in the following subsection. For ease of exposition, we describe the empirical strategy investigating gender bias — the specification and

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

Figure 1: Summary statistics by crime category and defendant identity



Notes: Panel A shows the imbalance in the per capita rate of criminal charges by gender. The share of cases with female defendants is divided by the share of women in the Indian population for each type of criminal charge. Panel C shows the same result for Muslims. Panel B shows the difference between female and male acquittal rates for each type of crime. Panel D shows the same difference between Muslims and non-Muslims. Crimes are ordered by maximal punishment, from most to least severe.

Table 1: Summary statistics, 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 (Days)	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)	0.308	0.302	0.304	0.306	0.309
	(0.002)	(0.004)	(0.002)	(0.007)	(0.002)
Acquitted	0.177	0.181	0.180	0.184	0.177
	(0.002)	(0.003)	(0.002)	(0.006)	(0.002)
Convicted	0.055	0.067	0.049	0.061	0.054
	(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.

considerations for estimating religious identity bias are identical. Specifications used in additional analysis on bias in contexts likely to activate identity are described with the results.[24]

4.1 Random Assignment of Judges to Cases

As with much of the prior empirical literature, judge assignment in district courts is as good as random, conditional on court-time and charge fixed effects, given rules that gives defendants and prosecutors virtually no control over which judge oversees the case (see Section 2). Random assignment of judges to cases addresses the concern that judges with different identities are assigned to different kinds of cases. For example, if Muslim judges could systematically choose to sit in cases with Muslim defendants who had committed less serious crimes, we might mistakenly infer in-group bias even in its absence. Alternately, Muslim defendants and judges are more likely to appear

[24]We also explored an event study specification exploiting case timing and changes in the cohort of judges sitting in each court, but we found that recently changed courts are more likely to see younger cases, violating the assumptions required for the event study analysis.

in regions of the country with more Muslims. If those regions are characterized by different crime distributions (with different acquittal rates), 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. Even if there is bias at other stages of the criminal process (e.g. who gets charged), that would not undermine our identification strategy given the random assignment of judges.

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:

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

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

where judgeMale and judgeNonMuslim are binary variables that indicate whether a judge is male or non-Muslim, respectively. Similarly, defMale and defNonMuslim 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, while 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 the precision of fixed effects and controls, but they do not directly affect the coefficients of interest. We drop court-time periods where only one judge appears. We also test a specification with judge fixed effects, which controls for the average acquittal behavior of each individual judge.[25] Standard errors are clustered at the judge level, since judge assignment is the level of randomization.

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 (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. Since all three causal effects are of interest, we report coefficients for each in the regression tables. The coefficient meanings are analogous in Equation 2.

In 50% of cases, a case stays in the courts long enough such that the judge making a decision on the case is different from the judge to whom the case was randomly assigned. In these cases, we continue to use the randomly assigned initial judge as the relevant actor. We find virtually identical effects if we limit the sample to cases decided by the initially assigned judge, but this is not our primary specification because a rapid decision is itself an outcome. Further, even if the filing judge does not make the final ruling on a case, they can make key decisions on the case process that influence the decision, such as allowing witnesses, admitting evidence, and determining the schedule on which the case is resolved.

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:

$$\begin{aligned} \text{judgeFemale}_{i,s,c,t} = \beta_1 \text{defFemale}_{i,s,c,t} + \beta_2 \text{defMuslim}_{i,s,c,t} + \gamma \phi_{c,t} + \zeta_s \\ + \delta \chi_{i,s,c,t} + \epsilon_{i,s,c,t} \end{aligned} \tag{3}$$

[25]This specification is included for completeness, but is unnecessary for identification (as are the judge and defendant demographic controls) if judges are indeed effectively assigned randomly.

Table 2: 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.

$$\begin{aligned}\text{judgeMuslim}_{i,s,c,t} = \eta + \gamma_1 \text{defMuslim}_{i,s,c,t} + \gamma_2 \text{defFemale}_{i,s,c,t} + \gamma \phi_{c,t} + \zeta_s \\ + \delta \chi_{i,s,c,t} + \epsilon_{i,s,c,t},\end{aligned} \tag{4}$$

with variables defined as above. The coefficients of interest are β_1 and γ_1, which respectively tell us whether female judges are more likely to adjudicate cases with female defendants, and whether Muslim judges are more likely to adjudicate cases with Muslim defendants.

Balance estimates are shown in Table 2. Male and female defendants are equally likely to be assigned to female judges. Similarly, Muslim and non-Muslim defendants are equally likely to be assigned to Muslim judges. These balance tests provide support for our identification assumption of exogenous judge assignment.

5 Results

5.1 Effect of assignment to judge types

The first two rows of Table 3 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 fewer acquittals than female judges. The point estimate on this effect is nearly identical for both male and female defendants across all specifications. The coefficient stability is what we would expect if judge assignment is indeed as good as random. The own-gender bias estimate is a tight zero; the effect estimates rule out even a very small in-group bias effect of 0.6 percentage points with 95% confidence.[26]

Table 3 Panel B shows the effect of filing judge gender on an indicator for case resolution within six months of being filed. Cases assigned to male judges are resolved slightly more quickly, but this difference is unaffected by defendant gender; the in-group bias effect is again a precise zero. In short, we do not find substantial gender bias on any dimension.

Table 4 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 estimate on in-group bias is never higher than 0.2 percentage points and the estimates rule out an own-religion bias of 0.6 percentage points with 95% confidence.[27] Religious in-group bias is also absent in the speed of

[26] Appendix Table A5 shows bias effects on conviction rates; the estimates again are a tight zero. Appendix Table A6 shows estimates when we exclude closed cases for which we are unable to determine the outcome. We prefer the specification in Table 3, because the inability to determine an outcome is itself an outcome. We also find no effect of gender or religious match on whether the outcome is clearly coded as acquittal or conviction (Appendix Table A7).

[27] Appendix Tables A8 and A9 show results on conviction rates, and on acquittals with ambiguous results dropped. While we find marginally significant bias effects (in the in-group direction) in a handful of specifications, the majority are statistically insignificant, and the point estimate on the bias term is never higher than 0.5 percentage points. Appendix Table A10 shows there is no effect of in-group bias on an indicator for an ambiguous case outcome.

Table 3: 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	0.001	0.001	0.000	0.002	0.002	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Reference group mean	0.176	0.177	0.177	0.176	0.177	0.177
Observations	5223433	5129780	5128269	5236865	5143294	5141492
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***	0.021***	—
	(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	-0.001	-0.001	-0.002	-0.001	-0.001	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Reference group mean	0.283	0.283	0.283	0.283	0.282	0.282
Observations	4367368	4286787	4285368	4380105	4299591	4297894
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.
Charge section fixed effects have been used across all columns reported.
Specification: $Y_{i,s,c,t} = \beta_1 \text{judgeMale}_{i,s,c,t} + \beta_2 \text{defMale}_{i,s,c,t} + \beta_3 \text{judgeMale}_{i,s,c,t} * \text{defMale}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta\chi_{i,s,c,t} + \epsilon_{i,s,c,t}$

Table 4: 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	0.008	—	0.007	0.006	—
	(0.004)	(0.005)		(0.004)	(0.005)	
Non-Muslim judge on non-Muslim defendant	0.007**	0.007*	—	0.007**	0.006	—
	(0.003)	(0.004)		(0.003)	(0.004)	
Difference = Own religion bias	-0.001	0.000	0.002	-0.001	0.000	0.002
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Reference group mean	0.18	0.184	0.184	0.181	0.184	0.184
Observations	5655320	5214531	5213019	5668388	5228040	5226225
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	0.008	—	0.009	0.005	—
	(0.006)	(0.008)		(0.006)	(0.008)	
Non-Muslim judge on non-Muslim defendant	0.005	0.002	—	0.003	-0.001	—
	(0.006)	(0.007)		(0.005)	(0.007)	
Difference = Own religion bias	-0.005	-0.006	0.002	-0.006	-0.006	0.003
	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.003)
Reference group mean	0.291	0.287	0.287	0.29	0.287	0.287
Observations	4732429	4360514	4359090	4744851	4373321	4371607
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.
Charge section fixed effects have been used across all columns reported.
Specification: $Y_{i,s,c,t} = \beta_1 \text{judgeNonMuslim}_{i,s,c,t} + \beta_2 \text{defNonMuslim}_{i,s,c,t} + \beta_3 \text{judgeNonMuslim}_{i,s,c,t} * \text{defNonMuslim}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta\chi_{i,s,c,t} + \epsilon_{i,s,c,t}$

judicial decisions, nor is there any evidence that Muslim and non-Muslim judges have different rates of resolving cases (Table 4).

5.2 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. This section examines 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 these cases, when the defendant and judge are mismatched, the judge and victim will share the same gender or religious identity.[28] The identity match or 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 is identified and can be matched to an identity group, we interact all of the terms in the standard in-group bias estimation (Equation 3) with an indicator for whether the victim and defendant have an identity mismatch.

Columns 1 and 2 of Table 5 show the results for gender and religious bias respectively. For legibility, we show only the coefficients on the in-group bias term (e.g. judgeMale * defMale for gender), and its interaction with the victim/defendant identity mismatch indicator. There is no evidence that in-group bias is larger when victim and defendant have different identities. Standard errors are larger due to the smaller sample and interaction specification, but the in-group bias effect is measured under 1 percentage point in both cases.[29]

We next examine whether male and female judges rule differently on cases classified in the criminal code as crimes against women, where judge and defendant gender identities may be particularly salient. These are largely evenly split between sexual assaults and kidnappings.[30] We use the standard bias specification but limit the sample to the set of cases classified as crimes against women (Appendix Table A11). Male judges are

[28] 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.

[29] The sample is smaller because cases in eCourts often do not code information about the crime victim.

[30] 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 similar for both the assault and kidnapping subsets of the data.

Table 5: 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	Yes	Yes	Yes
Bias	Gender	Religion	Gender
Sample	All	All	All

Standard errors in parentheses

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

Notes: This table reports results from a test of in-group bias in cases where gender or religious identity are salient, i.e. when the defendant and victim belong to opposing identities (Columns 1 and 2), or when the charge is classified as a crime against women (Column 3). Charge section fixed effects have been used across all reported columns.

Table 6: Impact of Ramadan on own religion bias in acquittal rates

	(1) acquitted	(2) Acquitted	(3) Acquitted	(4) Acquitted
Non-muslim judge	0.012**		0.012**	
	(0.006)		(0.006)	
Non-muslim defendant	0.002	0.002	0.003	0.003
	(0.004)	(0.002)	(0.004)	(0.002)
Ramadan	0.125***	0.091***	0.123***	0.091***
	(0.012)	(0.012)	(0.013)	(0.012)
Own religion bias	-0.003	-0.003	-0.002	-0.002
	(0.004)	(0.002)	(0.004)	(0.002)
Own religion bias * Ramadan	0.016	0.015	0.020**	0.018*
	(0.010)	(0.009)	(0.010)	(0.010)
Observations	5211432	6050334	5224676	6062453
Fixed Effect	Court-month	Court-month	Court-year	Court-year
Judge fixed effect	No	Yes	No	Yes
Sample	Full sample	Full sample	Full sample	Full sample

Standard errors in parentheses

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

Notes: This table reports results from a test of in-group religious bias conditional on trial during the months of Ramadan (following Mehmood and Seror (2020)). Charge section fixed effects have been included across all reported columns.

less likely to acquit for these cases than female judges, but show no disproportional harshness for male defendants.

Finally, we examine whether religious in-group bias emerges during the month of Ramadan, when Muslim religious identity may become particularly salient for both Muslims and non-Muslims.[31] We use a binary variable indicating whether the decision day is in the month of Ramadan and we interact it with all the variables in the standard bias specification.[32]

[31] Unlike the sample in Mehmood and Seror (2020), our sample only covers eight years, with Ramadan occurring only in the summer. There is thus no substantial time-series variation in daylight hours that can be exploited.

[32] Note that for this table only, we use the identity of the judge *deciding* on the case, rather than the judge to whom it was assigned initially. Our implicit assumption is that the effect of Ramadan affects the outcome on the day the decision is reached, rather than on the day the case first appeared before

Table 6 reports the results, with and without judge fixed effects, and with either court-month or court-year fixed effects. The coefficient of interest is the interaction of own-religion bias with the Ramadan indicator. There is a consistent 1.5–2.0 percentage point in-group bias effect, though it is of marginal statistical significance, with a p-value under 0.05 in only one of the four specifications. The result suggests that religious in-group bias may be activated when religious identity is particularly salient, but the effect size remains small relative to other studies.[33]

5.3 In-group Bias on the Basis of Caste

We now consider one of the most important social cleavages in India: caste. Ideally, we would like to run an equivalent statistical test, where judge and defendant identity sometimes match on the caste dimension and sometimes do not. An equivalent caste analysis to what we have done for gender and religion is not feasible, however, for three reasons. First, unlike gender and religion, there is no classification for caste along which in- and out-groups can be confidently and universally defined. The two major categories of caste, *varna* (four broad hierarchical categories, although hundreds of millions of Indians are *avarna*, or having no *varna*) and *jati* (approximately 5,000 endogamous communities), are both insufficient in characterizing the affinities that people may feel within the caste system. For example, an upper caste person could identify with another upper caste person despite sharing neither *varna* or *jati*. Likewise, the term *bahujan* is often used to describe the shared identity of marginalized groups such as Scheduled Castes and Other Backwards Castes. Second, individual names do not identify caste as precisely as they identify Islamic religion or gender identity and the caste significance of names can vary across regions. Due to these limitations and to a lack of training data, we have not been able to develop a reliable correspondence between names and specific castes. Third, there are few district judges in the most identifiable caste categories: Scheduled Castes and Scheduled Tribes.

For these reasons, a direct analysis of caste bias in the Indian judiciary is not feasible at this time. Instead, we analyze caste indirectly. Specifically, we follow Fisman et al. (2017) and define individuals as being in the same cultural group if they share a last name. As discussed in that paper and other work, shared last names are a noisy measure

a judge. See Section 4 for more on how we treated cases seen by more than one judge.

[33] Mehmood and Seror (2020) find in Pakistan that conviction rates are 16 percentage points lower during the month of Ramadan. The final section further discusses the size of our estimates compared with other studies.

of caste similarity for many social groups.

The measure is admittedly imperfect. Names are more numerous than castes, so members of the same caste usually have different last names. Further, sharing names can indicate greater affinity and closer social proximity than caste. Last names could signal similar socioeconomic status, for example, or shared religion. When a judge and defendant share a last name, they could even be relatives by blood or marriage. Individuals can also share a last name and be in different castes.

To determine whether judges deliver more favorable outcomes to defendants who share their last name, we estimate

$$Y_{i,s,c,t} = \beta_1 \text{sameLastName}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta\chi_{i,s,c,t} + \epsilon_{i,s,c,t}. \tag{5}$$

where subscripts i, s, c, t are defined as above. The court-time ($\phi_{c,t}$) and act/section (ζ_s) fixed effects, and judge/defendant characteristics $\delta\chi_{i,n,s,c,t}$, are also as above. Further, we include additional fixed effects for judge and defendant last names and control for judge and defendant gender and religion. We limit the sample to individuals with last names that match at least one judge in their district at any time.[34]

The identification assumptions for consistent estimation of $\hat{\beta}_1$ are the same as in the prior section. Under random assignment of judges to cases, whether a judge and defendant's last names match is exogenous within the court-time randomization block. The act/section fixed effects adjust for judge assignment rules based on the seriousness of the crime. Finally, the last-name fixed effects adjust for the possibility that individuals from some social groups are more or less likely to be acquitted, and that judges in different social groups may have different average acquittal rates.

The results for last name bias are reported in Table 7. Columns 1 and 2 report unweighted estimates from Equation 5, comparable to the specifications in the previous sections. The point estimate of in-group bias is a precisely estimated zero.

An issue with the unweighted case-level regressions is that the sample is dominated by social groups with common last names. The results are thus driven by individuals with common last names, like *Kumar* and *Singh*. These are the cases where a defendant-judge last-name match is the least likely to indicate shared caste. Matching on a common name may not indicate much cultural similarity, and the resulting esti-

[34]Without this limitation we have substantially more last name fixed effects in the sample but there is no additional variation in terms of identity match, because the *sameLastName* variable always takes the value 0 for defendants whose last name never appears in the judge list.

Table 7: Effect of assignment to judge with same last name on defendant outcomes

	(1) Acquitted	(2) Acquitted	(3) Acquitted	(4) Acquitted	(5) Acquitted	(6) Acquitted
Same last name	-0.000	-0.001	0.014**	0.012*	0.001	-0.001
	(0.001)	(0.001)	(0.006)	(0.006)	(0.004)	(0.004)
Same name * Rare name					0.032**	0.033**
					(0.015)	(0.015)
Observations	2239384	2237476	2239384	2237476	2239384	2237476
Judge Fixed Effect	No	Yes	No	Yes	No	Yes
Inverse Group Weight	No	No	Yes	Yes	Yes	Yes

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

Notes: This table reports results from a test of the effect of assignment to a judge with the same last name as the defendant on likelihood of acquittal (Equation 5). Court-month fixed effects, charge section fixed effects, and judge and defendant last name fixed effects have been used across all columns reported. Standard errors are clustered by judge.

mates may not capture the experience of smaller caste groups. To address this issue, we estimate an alternate specification where sample weights treat each defendant last name group equally. Formally, we estimate weighted regressions where the weights are computed as the inverse of the number of defendants in the sample with each given last name. These regressions therefore describe variation in bias across *groups*, rather than across individuals.

The weighted regressions are reported in Columns 3 and 4, corresponding to the respective unweighted regressions in Columns 1 and 2. The weighted regressions show that a judge-defendant name match increases the likelihood of acquittal by 1.2–1.4 percentage points (statistically significant). This result suggests caste-based in-group bias driven by groups with less common names. To confirm this more directly, we add a "rare name" interaction with the last name match indicator, where the "rare name" variable takes the value one if the defendant has a name with a below-median count in the data.[35] Columns 5 and 6 report this specification. The uninteracted coefficient shows an absence of bias for common last names, and the interacted coefficient shows a 3.2–3.3 percentage point in-group bias for individuals with uncommon last names.

The effect size among individuals with uncommon names is economically relevant and statistically significant. It represents about a 15% increase in the probability of acquittal. The social proximity signalled by sharing a rare last name, often indicating a

[35]Results are similar whether we use the median across individuals or the median across groups. Out of 2,761,382 defendants with last names that appear at least once in the judge sample, 112,934 have rare names based on the individual median, and 1,376,640 have rare names based on the group median. These effects are robust to looser definitions of last name similarity (for example, treating *Patil* and *Patel* as similar).

a shared caste (Fisman et al., 2017), is associated with judicial in-group bias. Yet this bias is only seen for the relatively narrow social groups demarcated by less common names. By definition, then, the same-name effect is relevant only for a small share of the population. Groups with rare names are mechanically underrepresented in the population, and the likelihood of matching a judge with the same rare name is even smaller. This bias, therefore, may be large in magnitude for some individuals, but will be small in aggregate if it operates only at the level of narrow social groups. Of course, we cannot rule out that judges may be exhibiting in-group bias on the basis of cultural similarity measures that we are not able to observe.

6 Discussion and Conclusion

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 and physical 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 described only anecdotally.

We make progress in this area by analyzing decisions in over 5 million criminal cases in India, 2010–2018. We estimate robust, tight zero effects of judicial in-group bias along the dimensions of gender, religion, and caste. We do not find gender-based bias even when gender identity is more salient, but we do find religion-based bias in one of two subsamples where religion is more salient (during Ramadan). We also find some in-group bias among social groups with shared uncommon last names. The aggregate effects of the measured biases are small, but there is evidence that bias can be magnified in circumstances which make the dimension of shared (or unshared) identity more salient.

The aggregate null effects are surprising, especially given well-documented gender and religious in-group bias in non-judicial contexts in India. Two relevant examples are Fisman et al. (2017), who find that credit offers and repayment rates rise when loan officers and clients have the same last name, and Neggers (2018), who finds that random assignment of a minority election worker to a polling station has a large pro-minority effect on vote counts at that station. The divergent findings raise the question of how these contexts differ from the judicial setting.

One major difference is the judge's incentive structure. Judges expect little direct economic benefit or cost from seeing members of the out-group punished. That "game" is quite different from the cooperative context in Fisman et al. (2017) (where joint gains are possible through a successful loan), or the adversarial context in Neggers (2018) (where only one party can win an election).

A second relevant feature is the competing relevance of other identity factors. The judicial setting may make salient the class, education, or other status differences between judges and defendants, crowding out broader identity characteristics like religion and gender. In contrast, political competition for resources (as in Neggers (2018)) may magnify the salience of these identities.[36] Consistent with this interpretation, our results on matching last names suggest that in-group bias is stronger under more narrow definitions of the in-group.

An example of both of these dynamics outside of judging is Hanna and Linden (2012), who find no evidence of out-group animus (on the caste dimension) in the case of teachers grading student exams. Like judging, grading is a non-adversarial context, where teachers face flat incentives for how students are assessed. Further, there are impactful class and authority differences between teachers and students, which make differences due to caste less salient. From a theoretical perspective, then, our results echo those from Hanna and Linden (2012).

This discussion highlights the sensitivity of in-group bias to context. Further, it hints at a theoretical grounding for why results on in-group bias vary across different settings. Further empirical research drilling down on these theories will be valuable.

In the judicial setting, our null estimates of in-group bias contrast with findings in other jurisdictions, where researchers have tended to find large effects. To compare our estimates to those in the literature, we collect coefficients and standard errors from the studies of judge in-group bias that are most similar to ours.[37] To make the studies comparable, effect sizes are standardized by dividing each in-group bias effect by the sample standard deviation of the outcome variable. As shown in Figure 2 Panel A, our primary effect sizes on religion and gender are the smallest in the literature. The high

[36]Similarly, Sharan (2020) finds that ethnic quotas in local government only improve public service delivery when lower-status groups occupy multiple positions in the political hierarchy.

[37]We include every study we can find that focused on measuring in-group bias among judges on a race/ethnicity, gender, or religious dimension, that exploited an as-good-as-random judge or jury assignment mechanism for causal identification.

end of our confidence interval is an order of magnitude smaller than nearly all prior studies.

Another notable pattern in the graph is that the confidence intervals (and hence standard errors) grow with the effect sizes. A positive relationship between effect size and standard errors suggests that there could be publication bias in studies of judicial in-group bias, which would also help explain the distinctiveness of our null finding. To show this more directly, Figure 2 Panel B plots the effect size of each of these studies against the standard error of the main estimated effect.[38] With the standard error axis reversed, this is a "funnel plot," a graph used to examine publication bias. In the absence of publication bias or a design-based mechanical relationship between effect size and precision (such as adaptive sampling), study estimates should form a cone that is centered around the true estimate.[39] The set of studies examined here show a highly non-conic and asymmetric shape, where effect magnitude is highly correlated with effect size, such that many of the studies fall just outside the cone boundary defining statistical significance at the 5% level. While other explanations may be considered, the funnel plot is at least consistent with a substantial degree of publication bias.[40]

The rest of the literature aside, our finding of a lack of in-group bias in India's lower courts should be celebrated, not least because it can inform policymakers allocating resources to address the clear and extant social disparities in Indian society. Yet our research does not rule out bias in the criminal justice system as a whole. Notwithstanding our results on acquittals, the legal system could still be biased against marginalized groups due to unequal 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, civil litigation, or via other factors. There could also be absolute bias, where both in- and out-group judges discriminate against out-groups. Based on our evidence, concerns about in-group bias might be

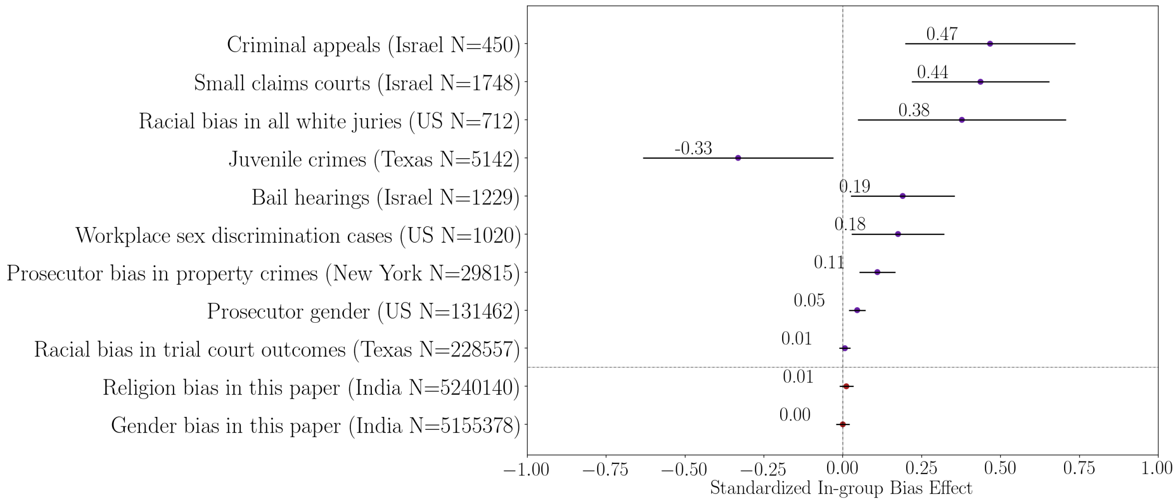
[38]When papers report multiple specifications for the main effect, we use the effect size described most prominently in the text or described by the authors as the "main specification." When papers have multiple outcomes, we use the outcome most similar to the acquittal or conviction rate, as in this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

[39]See Egger et al., 1997; Gerber et al., 2001; Levine et al., 2009; Slavin and Smith, 2009; Kühberger et al., 2014. A cone shape is expected because studies with larger standard errors should produce a wider range of estimates that are symmetric around the true value.

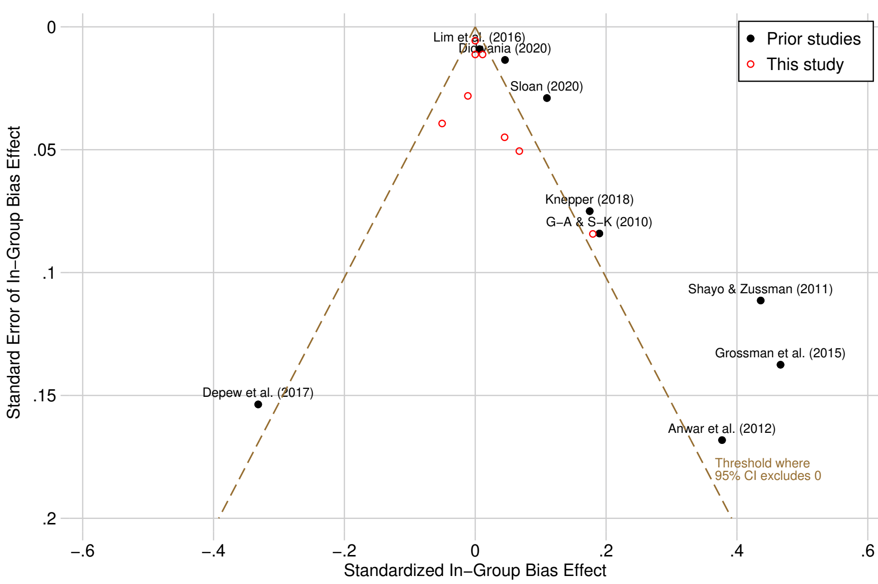
[40]Indeed, since posting this paper, we have heard from more than one researcher who abandoned research on in-group bias when their preliminary results suggested a null result. Other papers that find null or reverse effects of in-group bias tend to spend only a small amount of space an these findings (Arnold et al., 2018; Hanna and Linden, 2012).

Figure 2: Comparison with judicial bias estimates in other contexts

A. Coefficient Plot



B. Standardized Errors vs. Effect Sizes



Notes: This figure shows point estimates of in-group bias from other studies in the relevant literature. From the top, the coefficients of in-group bias (Panel A) correspond to Grossman et al. (2016), Shayo and Zussman (2011), Anwar et al. (2012), Depew et al. (2017), Gazal-Ayal and Sulitzeanu-Kenan (2010), Knepper (2018), Sloane (2019), Didwania (2018), Lim et al. (2016), and the main estimates from the present study respectively. Panel B plots reported bias effects (Y axis) against effect standard errors. All effect sizes are standardized (dividing outcome variables by their standard deviation) to allow comparison across studies. From each table in this paper, we chose the specification with court-month and judge fixed effects. For contexts magnifying bias, we show the average effect for the group facing magnified bias. For example, for the Ramadan analysis, we show the sum of the bias coefficient and the bias * Ramadan coefficient, which describes religious in-group bias in the month of Ramadan. The only statistically significant estimate is the inverse group size weighted interaction between same name and rare last name (Table 7 Column 6); note the unweighted regression (which weights each case equally) found a zero estimate.

better directed to other parts of the justice pipeline than judge acquittal decisions.

These points highlight that our results are a first step. More research is sorely needed to create an empirical basis for understanding the entire judicial process in India and in other developing countries. The expansion of publicly available datasets on judicial systems worldwide would be a first step in making this possible.

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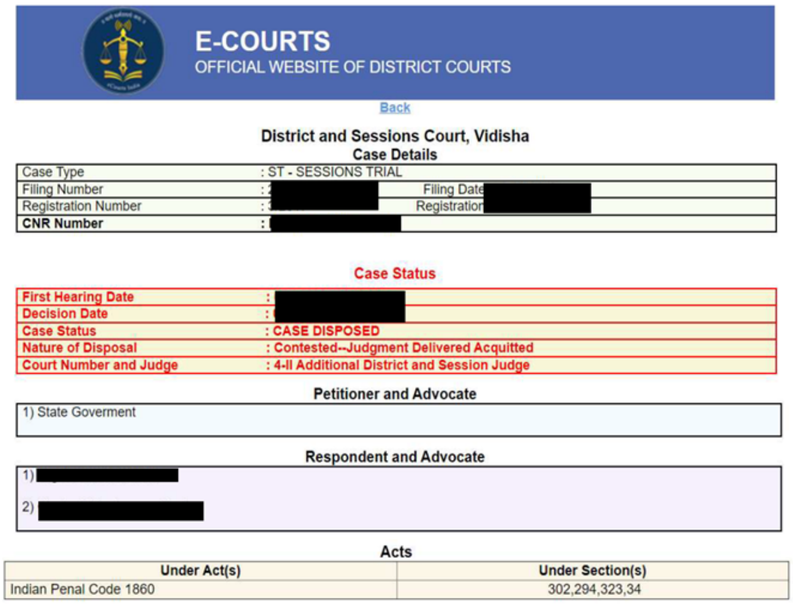
A Appendix

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	
CNR Number	: [redacted]		

Case Status

First Hearing Date	: [redacted]
Decision Date	:
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]

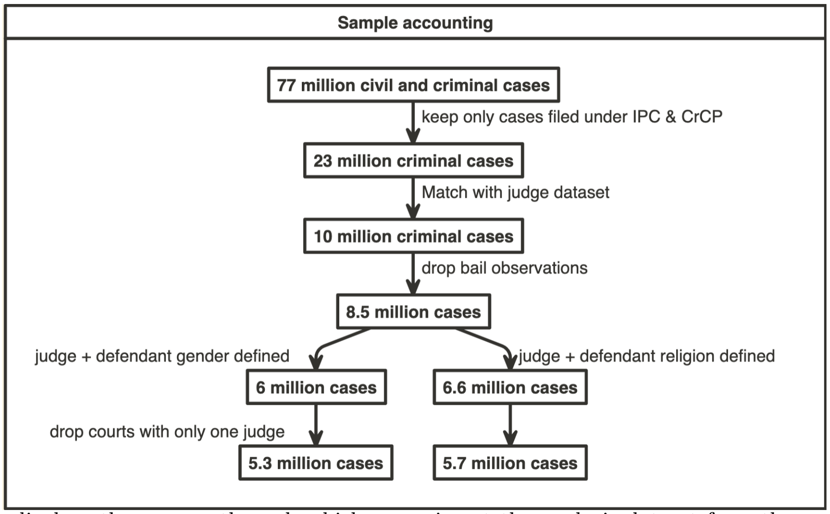
2) [redacted]

Acts

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

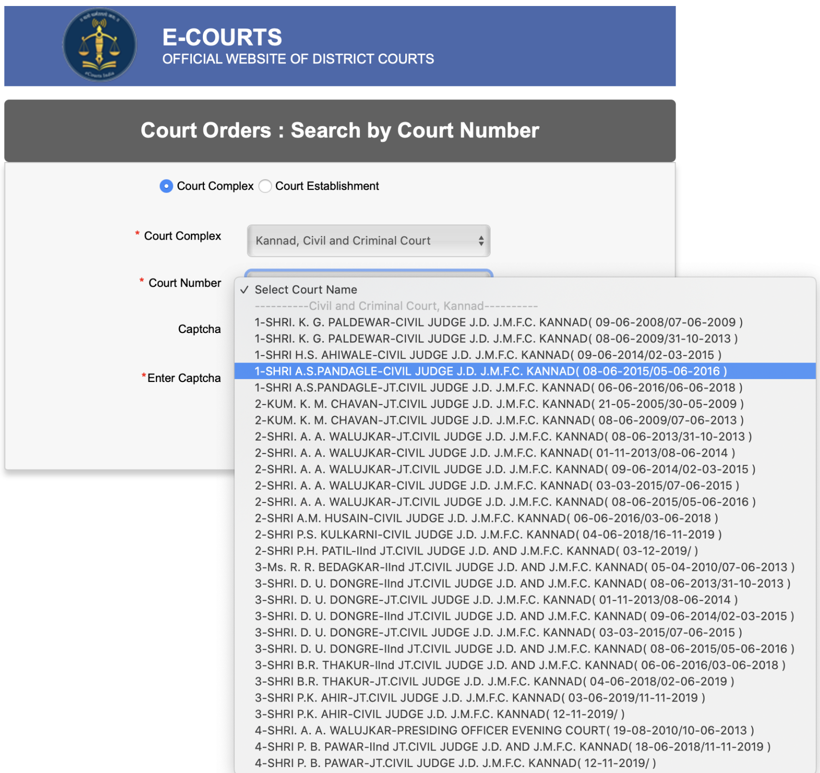
Notes: The figure displays an anonymized version of a sample court record from https://ecourts.gov.in/ for the District and Sessions Court of Vidisha. The 'Petitioner and Advocate' and 'Respondent and Advocate' sections contain the litigant names that we use for assigning gender and religion. The 'Acts' 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



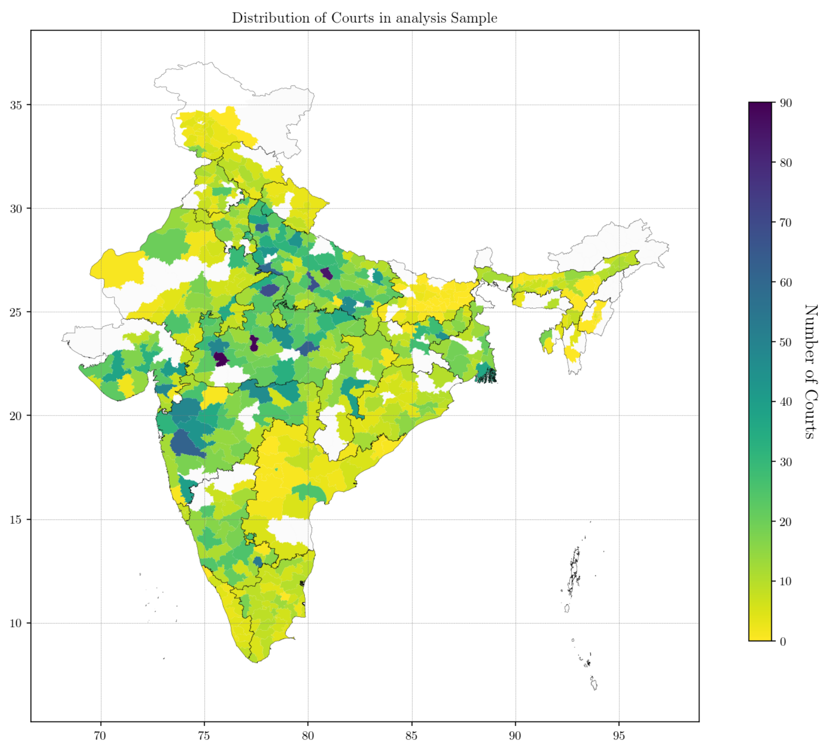
Notes: The figure displays the process through which we arrive at the analysis dataset from the parent dataset of 77 million legal case records. After restricting the sample to criminal cases, matching these criminal cases with our judge dataset, and dropping bail observations, 8.5 million case records remain. We can then assign the gender of the judge and defendant using our machine classifier for 6 million cases, and 6.6 million for religion. Finally, cases are dropped if they are seen in a court where only one judge is observed in a given month. This leaves 5.7 million cases in the religion analysis and 5.3 million in the gender analysis.

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.

Figure A4: Distribution of courts across districts in the analysis sample



Notes: This figure shows the geographical distribution of the trial courts in our sample. Black lines delineate states, and within those the unit of observation for this graphical illustration are districts. Districts marked in white have no courts in our analysis.

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

	Mapped Outcome(s)		
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 raw dispositions into our three outcome variables. In the table, no entry corresponds to the default value 0, and X denotes that the corresponding outcome value is set to 1. 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.

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 to the female population share in India. Column 3 reports the acquittal rate for females accused of each crime category. Column 4 reports the analogous acquittal rates for males. Column 5 reports the difference in female and male acquittal rates for each crime category. Column 6 reports the total number of case records in each crime category. The total number of cases in this table is larger than the 6 million cases mentioned in A1 as we also include cases records in the statistics where only the defendant gender is defined, even if the judge gender is unknown.

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 to the Muslim population share in India. Column 3 reports the acquittal rate for Muslims accused of each crime category. Column 4 reports the analogous acquittal rates for non-Muslims. Column 5 reports the difference in Muslim and non-Muslim acquittal rates for each crime category. Column 6 reports the total number of case records in each crime category. The total number of cases in this table is larger than the 6.6 million cases mentioned in A1 as we also include cases records in the statistics where only the defendant religion is defined, even if the judge religion is unknown.

Table A5: 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*	0.002	—	0.003*	0.002	—
	(0.002)	(0.002)		(0.002)	(0.002)	
Male judge on male defendant	0.002	0.001	—	0.002	0.001	—
	(0.002)	(0.001)		(0.002)	(0.002)	
Difference = Own gender bias	-0.001	-0.001	0.001	-0.001	-0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Reference group mean	0.952	0.952	0.952	0.952	0.952	0.952
Observations	5223433	5129780	5128269	5236865	5143294	5141492
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.
Charge section fixed effects have been used across all columns reported.
Specification: $Y_{i,s,c,t} = \beta_1 \text{judgeMale}_{i,s,c,t} + \beta_2 \text{defMale}_{i,s,c,t} + \beta_3 \text{judgeMale}_{i,s,c,t} * \text{defMale}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta \chi_{i,s,c,t} + \epsilon_{i,s,c,t}$
The table shows estimates of in-group gender bias. The setup is identical to Table 3, but the outcome variable is an indicator for non-conviction instead of for acquittal.

Table A6: 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	-0.003	-0.009	—	-0.005	-0.010*	—
	(0.005)	(0.006)		(0.004)	(0.005)	
Male judge on male defendant	-0.001	-0.007	—	-0.002	-0.007	—
	(0.004)	(0.005)		(0.004)	(0.005)	
Difference = Own gender bias	0.002	0.002	0.004	0.003	0.003	0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Reference group mean	0.676	0.677	0.677	0.679	0.679	0.679
Observations	1155224	1134736	1132174	1176466	1156052	1153438
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.
Charge section fixed effects have been used across all columns reported.
Specification: $Y_{i,s,c,t} = \beta_1 \text{judgeMale}_{i,s,c,t} + \beta_2 \text{defMale}_{i,s,c,t} + \beta_3 \text{judgeMale}_{i,s,c,t} * \text{defMale}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta\chi_{i,s,c,t} + \epsilon_{i,s,c,t}$
The table shows estimates of in-group gender bias. The setup is identical to Table 3, but with ambiguous outcomes dropped.

Table A7: 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	0.000	0.000	0.002	0.000	0.000	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

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: $Y_{i,s,c,t} = \beta_1 \text{judgeMale}_{i,s,c,t} + \beta_2 \text{defMale}_{i,s,c,t} + \beta_3 \text{judgeMale}_{i,s,c,t} * \text{defMale}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta \chi_{i,s,c,t} + \epsilon_{i,s,c,t}$
The table validates the primary in-group gender bias test by reporting whether cases are differentially recorded with ambiguous outcomes when the judge and defendant match identity. The setup is identical to Table 3, but the outcome variable is an indicator for an ambiguous case outcome.

Table A8: 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	0.003	-0.002	—	0.001	-0.004	—
	(0.002)	(0.003)		(0.002)	(0.002)	
Non-Muslim judge on non-Muslim defendant	0.005	0.001	—	0.004	0.000	—
	(0.003)	(0.003)		(0.004)	(0.003)	
Difference = Own religion bias	0.002	0.003	0.002	0.003	0.004	0.002
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Reference group mean	0.941	0.942	0.942	0.941	0.942	0.942
Observations	5655320	5214531	5213019	5668388	5228040	5226225
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: $Y_{i,s,c,t} = \beta_1 \text{judgeNonMuslim}_{i,s,c,t} + \beta_2 \text{defNonMuslim}_{i,s,c,t} + \beta_3 \text{judgeNonMuslim}_{i,s,c,t} * \text{defNonMuslim}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta \chi_{i,s,c,t} + \epsilon_{i,s,c,t}$
The table shows estimates of in-group religious bias. The setup is identical to Table 4, but the outcome variable is an indicator for non-conviction instead of for acquittal.

Table A9: 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	0.010	-0.001	—	0.007	-0.007	—
	(0.007)	(0.008)		(0.006)	(0.008)	
Non-Muslim judge on non-Muslim defendant	0.010	0.001	—	0.008	-0.004	—
	(0.006)	(0.007)		(0.006)	(0.007)	
Difference = Own religion bias	0.000	0.001	-0.002	0.002	0.004	0.000
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)
Reference group mean	0.688	0.694	0.694	0.689	0.696	0.696
Observations	1256206	1159640	1157045	1277307	1181128	1178485
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: $Y_{i,s,c,t} = \beta_1 \text{judgeNonMuslim}_{i,s,c,t} + \beta_2 \text{defNonMuslim}_{i,s,c,t} + \beta_3 \text{judgeNonMuslim}_{i,s,c,t} * \text{defNonMuslim}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta \chi_{i,s,c,t} + \epsilon_{i,s,c,t}$
The table shows estimates of in-group religious bias. The setup is identical to Table 4, but with ambiguous outcomes dropped.

Table A10: 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	-0.006	-0.013	—	-0.006	-0.013	—
	(0.005)	(0.006)		(0.005)	(0.006)	
Non-Muslim judge on non-Muslim defendant	-0.002	-0.008	—	-0.002	-0.008	—
	(0.005)	(0.006)		(0.005)	(0.006)	
Difference = Own religion bias	0.004	0.005	0.001	0.005	0.005	0.001
	(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

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: $Y_{i,s,c,t} = \beta_1 \text{judgeNonMuslim}_{i,s,c,t} + \beta_2 \text{defNonMuslim}_{i,s,c,t} + \beta_3 \text{judgeNonMuslim}_{i,s,c,t} * \text{defNonMuslim}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta \chi_{i,s,c,t} + \epsilon_{i,s,c,t}$
The table validates the primary in-group religious bias test by reporting whether cases are differentially recorded with ambiguous outcomes when the judge and defendant match identity. The setup is identical to Table 4, but the outcome variable is an indicator for an ambiguous case outcome.

Table A11: 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)	(0.010)		(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	-0.001	-0.003	-0.004	0.004	0.002	0.003
	(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

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: $Y_{i,s,c,t} = \beta_1 \text{judgeMale}_{i,s,c,t} + \beta_2 \text{defMale}_{i,s,c,t} + \beta_3 \text{judgeMale}_{i,s,c,t} * \text{defMale}_{i,s,c,t} + \phi_{c,t} + \zeta_s + \delta \chi_{i,s,c,t} + \epsilon_{i,s,c,t}$