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Political Representation and Judicial Outcomes: Evidence from India

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Abstract

The economic impact of electing members of minority groups into positions of political power is well-established. However, the impact of political representation on broader civil rights and liberties, and particularly access to justice, remains unexplored. This paper employs a close-election regression discontinuity design to explore whether female political representation can explain judicial outcomes in the Indian context, focusing on crimes against women. Despite politicians having no formal influence over the judiciary, I find that the election of a female politician generates a large and statistically significant increase in the likelihood of conviction for crimes against women, relative to the election of a male politician. I do not find similar differences in the likelihood of conviction for gender-neutral crimes, suggesting that female politicians shape judicial outcomes within issue areas that align with gendered spheres of influence and interest. Additional analysis—on whether female politicians cater to gendered preferences in public goods and whether the effect of female representation on the likelihood of conviction varies with local gender bias—points to two potentially important mechanisms. These include a policy channel, whereby female politicians actively attempt to act in women's interests, and an exposure channel, whereby observing female representatives positively informs citizens' views on women's competencies. This study emphasises the importance of political representation in expanding vulnerable groups' access to justice.

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1 Introduction

A well-established literature has highlighted the economic impact of electing members of socially disadvantaged groups into positions of political power, especially for public goods provision (Pande, 2003; Chattopadhyay and Duflo, 2004; Beaman et al., 2007, 2012). However, the impact of political representation on access to justice remains unexplored. While it is often argued that vulnerable groups, particularly women, face major barriers to being heard within the criminal justice system (Jassal, 2024; Sharma et al., 2025; Dutt, 2018; Kadyan & Unnithan, 2019), no study has sought to test whether political representation can alleviate these constraints. This study seeks to address a gap in the literature by estimating the impact of female political representation on judicial case outcomes, focusing on crimes against women in the Indian context.

I employ a close-election regression discontinuity (RD) design to isolate plausibly exogenous variation in Indian electoral constituencies' exposure to male or female politicians. The quasi-experimental RD design circumvents several of the empirical challenges associated with causal estimation of the effect of political representation. As numerous studies have previously argued, narrowly determined election outcomes may plausibly be attributed to idiosyncratic factors rather than systematic constituency characteristics correlated with judicial outcomes, such that the sample conceivably simulates random assignment to male or female political representation (Lee, 2008; Lee and Lemieux, 2010; Dell, 2015; Baskaran et al., 2018; Asher and Novosad, 2017). Balance tests conducted on this study's dataset verify that demographic, economic, and geographic characteristics vary smoothly across the win-loss threshold, indicating that predetermined confounding variables are not driving results. Furthermore, sorting tests confirm that neither male nor female candidates are able to precisely manipulate close election outcomes. Data derived from detailed case records allows me to address the temporal misalignment problem arising from judicial delays and political turnover by linking each case to the politician in office during the end of the trial period.

Baseline RD estimates suggest that constituencies with exposure to female political representatives at the state constituency level exhibit a 5% higher conviction likelihood for cases pertaining to crimes against women, relative to constituencies with exposure to male representatives. I record considerable heterogeneity by crime type, with treatment effects of female (relative to male) politicians estimated at 18% for rape cases and 4% for non-rape sexual harassment cases. I observe these effects despite the fact that the Indian constitution emphasises the separation of state powers, and elected politicians therefore have no formal influence over any aspect of judicial proceedings. Results are robust to a range of specifications, including alternative polynomial forms and bandwidths. For cases related to gender-neutral crimes, including crimes against property and and crimes against religion, I do not identify significantly higher conviction likelihoods in constituencies with female relative to male representation; indeed, for some crimes I estimate a significantly negative treatment effect. This suggests that female politicians shape judicial outcomes within issue areas that align with gendered spheres of interest and influence, rather than a broad-based effect on all judicial outcomes.

To probe mechanisms, I first use an instrumental variables strategy inspired by the close-election RD rationale to test whether female politicians have an impact on public goods provision. Results suggest that, while male and female politicians have similar impacts on the majority of public goods, secondary schools and drinking water are negatively and positively impacted by female politicians (relative to male politicians), respectively. This is consistent with the hypothesis that female politicians cater to gendered preferences in public goods (Chattapadhyay & Duflo, 2004). Second, I exploit constituency-level variation in the female population share to study treatment heterogeneity by ex ante local gender bias. Using a fully interacted local linear RD model, I find that the female treatment effect on conviction likelihood increases with local gender bias. Taken together, I hypothesise that results are consistent with a policy channel, whereby female politicians actively attempt to act in women's interests and improve local attitudes towards women's issues, and an exposure channel, whereby observing female representatives positively informs citizens on women's competencies.

To the best of my knowledge, this is the first paper to examine the effect of politician characteristics on judicial outcomes, either in or outside the Indian context. The bulk of the existing politician characteristic literature has concentrated on economic outcomes, including the effect of female politicians on economic growth (Baskaran et al., 2018), public goods (Chattopadhyay and Duflo, 2004), and child mortality (Bhalotra and Clots-Figueras, 2014). A smaller literature has studied non-economic outcomes, including the roles of Muslim representation on religious riots (Bhalotra et al., 2014) and female representation on parental aspirations (Beaman et al., 2012) and corruption (Pereira and Fernandez-Vazquez, 2022). However, the literature has not, thus far, sought to consider how politicians shape the criminal justice system—-with the notable exception of Iyer et al. (2012), which finds that an increase in mandated political representation for women is associated with a rise in documented crimes against women, driven by a higher reporting rate. I take this research a step further by considering the role of the judiciary in determining final outcomes. Iyer et al. (2012) generate variation in female representation via the introduction of mandated gender quotas on Indian village councils; they may therefore be capturing adjustment effects associated with institutionally imposed reservation policies rather than the pure effect of electorally-determined female leaders.[1] A close-election RD design offers an alternative to this setup. Additionally, my study considers state legislative assemblies, which represents a higher level of governance than village councils. Iyer et al. (2012) hypothesise that the presence of female representatives at the lowest levels of governance (village councils) is more effective in furthering women's interests than at higher levels of governance (district chairpersons); my results suggest that this may not hold true for all contexts.

This study also contributes to a literature challenging the conventional wisdom that judges are sequestered from politics and environmental influence (Maskin and Tirole, 2004). This literature has documented that elected judges cater to their electorate's preferences (Lim, 2013; Gordon and Huber, 2007) and to reputational considerations (Cohen et al., 2015), while non-elected judges' rulings can be influenced by promotion-seeking behaviour (Cohen, 1991; Black and Owens, 2016) and external priming via election campaigns (Chen, 2024). These studies have almost exclusively been applied to US contexts. Moreover, they typically rely on temporal closeness to events of interest, such as elections or promotions, to introduce variation in judges' incentives (Mok, 2022). As Mok (2022) has argued, it is unclear precisely for how long such incentives are pertinent, and these studies fail to identify a baseline reference level for behaviour, i.e., the considered incentives differ in strength rather than direction. Instead, the close-election RD approach employed in this paper identifies a specific discontinuity in judges' exposure to female politicians. Additionally, focusing on a single politician characteristic enables me to explore potential mechanisms of influence.

Finally, this paper contributes to the criminological and legal literature assessing the reasons for low conviction rates for rape and sexual assault cases. This literature has focused on attrition

[1] The village council reservation system is the setting for a large proportion of the studies that have estimated the effects of female political representation in the Indian context, including Beaman et al. (2007, 2012), Chattopadhyay and Duflo (2004), Clots-Figueras (2011, 2012), and Bhalotra and Clots-Figueras (2014).

trajectories of such cases, driven by factors such as rape myths (Gregory and Lees, 1996), victim vulnerabilities (Kelly et al., 2005; Hester, 2015), and evidential issues (Hohl and Stanko, 2015; Lea et al., 2003). Empirical literature has documented gender-based disparities in legal treatment (Jassal, 2024), but studies of the specific factors that disadvantage women during trials have largely relied on qualitative or correlational methods, and, to my knowledge, have not addressed the role of politics beyond studying the effects of specific legislation. Therefore, highlighting the role of politician characteristics allows me to frame a new explanation for regional patterns in rape and sexual assault conviction rates.

In sum, this paper offers new insights into the effects of female political representation on judicial outcomes in the Indian context. The rest of the paper is organised as follows: section 2 describes the context and possible mechanisms, section 3 describes the data, and section 4 the empirical strategy. Section 5 presents results and robustness checks, followed by mechanism tests in section 6. Section 7 discusses limitations and section 8 concludes.

2 Context

This section outlines the institutional context in which legal cases are processed and the theoretical mechanisms through which female representation may impact judicial outcomes.

2.1 Institutional background

2.1.1 Legal process

India's judicial hierarchy includes a Supreme Court, 25 High Courts, 672 district courts, and over 7,000 subordinate courts (Ash et al., 2025). This study focuses on the lower judiciary comprising district and subordinate courts.

As Ash et al. have outlined, crimes are first reported to local police stations, each of which falls under the authority of a particular court. Cases are therefore tied to specific geographical jurisdictions, which allows me to match each case to the corresponding constituency and political representative. After an initial investigation, cases are filed with the judiciary by the police. Each case is then assigned to judges following a rules-based procedure, whereby each type of crime from a given police station is assigned to a specific courtroom. Judges rotate through courtrooms following a pre-set schedule. Therefore, there is little room for the manipulation of judge assignment (for example, to more or less lenient judges).

The final court ruling is determined solely by the judge, with no jury or public voting procedures. Although judges are constrained by constitutional provisions under the Indian Penal Code, Ash et al. (2025) find evidence that Indian district judges do have discretion in decisions. Outside the Indian context, a wide literature has shown that judges may depart from impartiality by relying on intuitive reasoning or personal biases (Rachlinski and Wistrich, 2017).

2.1.2 State legislature

I focus on state-level elections, wherein candidates compete within a first-past-the-post system for seats in state legislative assemblies. Each seat corresponds to a single-member constituency. Between 1980 and 2008, women comprised only 5.5% of India's state assemblies (Iyer, 2019).

Asher and Novosad (2017) note that individual members of state legislative assemblies have little direct power over government inputs. Most political decisions fall under the purview of the executive rather than the legislature, who are in session for just 40 days per year on average (Jensenius, 2013). Instead, MLAs serve primarily as intermediaries between the state and its citizens, working on constituent requests and contacting bureaucrats or cabinet ministers on their constituents' behalf. Therefore, these legislators focus on engagement with their constituencies, rather than general political influence over the state. They have have no formal power over or involvement with judicial processes due to the constitutional separation of powers (Pal, 2017).

2.2 Potential effects of politician gender on conviction decisions

This subsection outlines potential mechanisms through which female political representation may affect judicial outcomes for cases involving crimes against women, despite the fact that politicians have no direct hold over judicial processes.

First, exposure to female politicians can inform citizens on women's competencies and effectiveness as leaders, and thereby improve local attitudes towards women (Beaman et al., 2009). If this is the case, citizens may be more likely to take women's accounts seriously, which the legal literature has put forward as a key factor behind low conviction rates for crimes against women (Kelly et al., 2005). Judges may experience this *exposure effect* directly or through socialisation effects, or may be influenced by the altered attitudes of other parties involved in the case, including laywers and witnesses. This channel may also directly impact female victims' own choices. For example, improved self-confidence and a reduced tolerance for gendered injustices may reduce victim withdrawal during the legal process, which has been identified as one of the largest sources of attrition in rape cases (Hohl and Stanko, 2015).

A similar, but distinct, channel may be female politicians actively attempting to improve attitudes towards women's issues, either generally or with a view towards the criminal justice system. Cross-country and correlational studies suggest that female politicians have a higher propensity to pass bills pertinent to women's issues (Schwindt-Bayer, 2006; Kittilson, 2008; Weeks, 2017). Although politicians cannot directly influence the judiciary, they may employ public influence to target gender bias, rape myths, or specific high-profile cases. I call this a *policy effect*. This may affect both judges' and victims' choices during legal proceedings.

Additionally, judges may be motivated by *strategic* considerations. Judges of the lower judiciary are not appointed by local politicians but by the Chief Justice of the state High Court. However, they may still wish to signal alignment with politicians' preferences in order to avoid clashes with local legislators, or in light of politicians' indirect influence on career advancement through reputation effects. For example, Aney et al. (2021) find that Indian Supreme Court judges display strategic behaviour in the hopes of attaining post-retirement governments jobs. In the context of crimes against women this may be harmful if it results in tokenistic convictions or harsher decisions for innocent defendants.

Finally, we may observe a *retaliatory effect*, whereby some citizens might be resentful of female leadership or otherwise respond negatively to a female leader, leading gender bias to rear its head more strongly during legal proceedings. If the effect of this backlash dominates, we would expect a negative effect of female representation on conviction likelihood for cases relating to crimes against women.

Multiple channels may function simultaneously, reinforcing or offsetting each other. I return to this discussion in section 6.

3 Data

To examine whether political representation shapes judicial outcomes, I combine judicial case records with data on state legislative assembly elections. To assess mechanisms and continuity across the win-loss threshold, I further employ satellite-derived geospatial data, information from legally-mandated affidavits disclosing politician characteristics, and data from six censuses and surveys aggregated at the constituency level. Each of these data sources are discussed further below. Due to the large volume of data and the computational intensity of nested matching loops, the Advanced Research Computing (ARC) clusters at Oxford were used for data preprocessing.

3.1 Judicial data

Judicial data is drawn from the Indian eCourts platform as compiled by Ash et al. (2025). This includes the details of 81 million cases handled by India's lower judiciary between 2010 and 2018, covering over 7,000 courts and 80,000 judges. I limit the sample to cases filed under the Indian Penal Code (IPC), since the outcomes of criminal cases can be coded as good or bad for the plaintiff with greater precision than civil case outcomes. By manually mapping IPC section numbers to the crimes they pertain to, I identify cases which relate to specific crime categories. I define the primary case outcome $Convicted$ as an indicator that equals 1 if the case results in conviction or other demonstrable punishment and zero otherwise, following a similar classification strategy to that employed by Ash et al. (2025). Table A1 in the Addendum provides an overview of the classification of dispositions that appear most frequently in the data.

The eCourts description of the case outcome is ambiguous in approximately 19% of cases in my baseline sample. For example, the metadata may describe an outcome as simply "disposed" or "closed" with no further elaboration. In these instances, $Convicted$ is assigned to zero. This may pose an issue if some of these cases resulted in convictions. However, this is only a concern if cases are marked with ambiguous descriptions in a pattern that is systematically related with female political representation. For example, if female political representation leads to clearer documentation of case outcomes (i.e., more transparent reporting of convictions), estimates of the female impact on the conviction likelihood would be biased upwards. Similarly, approximately 7% of cases in my sample are referred or transferred to a higher court; these cases are recognised by the lower courts as worthy of further inspection, but the final outcome is unavailable within my dataset. I return to these concerns in section 4.2.1.

3.2 Election data

Election and candidate data is drawn from the Election Commission of India's Statistical Reports and candidate affidavits as compiled by the Trivedi Center for Political Data (Jensenius and Verniers, 2017) and Prakash et al. (2019), accessed through the Socioeconomic High-resolution Rural-Urban Geographic dataset (SHRUG). I aggregate these data at the election-constituency level, such that each observation represents a specific election within a constituency. I limit the sample to elections in which the top two candidates by vote share were of different genders, i.e., either a male candidate won against a female runner-up, or a female candidate won against a male runner-up. The cleaned dataset comprises 1,910 elections across 1,525 constituencies.

3.3 Merging election and judicial data

I merge election data with judicial data by state names and location names.

The judicial dataset links each case to a specific lower judiciary court, but does not provide location names at a level lower than the district. The election dataset provides constituency names. One district may comprise a single or multiple constituencies depending on the area's population distribution. First, where possible, I employ partial string matching from constituency names to the court names listed in eCourts reports, which often include precise location names as a suffix. Using this method, I am able to match approximately 55.27% of courts to the constituencies they are located in. For the remainder of observations, I allow matching where the constituency name matches the court district name. This method allows me to match an additional 21.48% of all courts, for a total of 76.75% matched courts. Approximately 23.25% of courts remain unmatched to a constituency and are therefore excluded from my sample; however, since matching is determined by administrative labelling conventions and naming overlaps, I do not expect correlation with constituency characteristics.[2]

To both these methods, I add a temporal condition. The appropriate temporal merging condition differs by outcome variable. In aiming to link each case to a single politician, we face a time misalignment problem. Indian courts face severe judicial delays (Mishra, 2023),

[2] I am unable to confirm this formally through balance tests due to the lack of additional parameters or court-level information in the dataset.

implying that the politician in power when the crime is reported may not remain in power when the case is filed in court or when the final decision is made.[3] However, it is worth highlighting that the case outcome is determined by the judge at the end of the trial period; the politician in power during this period is likely more relevant than the politician in power when the case was filed, if different. Therefore, for outcomes relating to the final decision on the case, I employ a condition that requires the date of the case's last update to fall within the elected representative's term in office. The date of last update represents the decision date, or, where cases have not yet come to a decision, the date of the most recent hearing or the date of filing. For outcomes relating to the number of cases filed, I impose a condition that requires the date of filing to fall within the representative's term in office. This is discussed further in section 7.

3.4 Survey and geographic data

To assess demographic and economic characteristics across the win-loss threshold, I obtain data from the 2011 Population Census and the 2013 Economic Census via SHRUG. I also employ small-area consumption estimates as computed by Asher et al. (2021) using data from the 2012 Socio-Economic and Caste Census and the 2011-2012 India Human Development Survey. Using these estimates, the poverty rate is measured by the share of rural households consuming below $2 (Rs 31) per person per day (PPP). To assess the impact of female representation on public goods outcomes, I additionally obtain 1991 and 2001 Population Census data.

To study geographic characteristics across the win-loss threshold, I obtain mean elevation and terrain ruggedness for each constituency location polygon from Asher et al. (2021). Elevation and ruggedness data are sourced from NASA's Shuttle Radar Topography Mission (SRTM) at 1 arc second resolution. The terrain ruggedness index is calculated following Riley et al. (1999). I also use 2001 Population Census data on access to rivers[4] and calculate the share of irrigated land and share of forest area from 2011 Population Census data.

I merge these datasets with election data using Assembly Constituency identifiers.

[3] In my dataset, the average duration of cases involving crimes against women is 472 days from filing to decision; the top quartile of cases last for up to 10 years. This does not include the time between the police receiving a complaint and filing a legal case.

[4] The 2011 census no longer provides separate information on rivers.

4 Empirical Approach

4.1 Regression Discontinuity

The RD strategy exploits discontinuous change in the gender of a constituency's elected representative at the threshold between a female candidate's victory or loss to a male candidate. Constituencies in which female candidates are elected by large margins are likely to differ from constituencies in which they lose by large margins, but narrowly determined election outcomes may plausibly be attributed to idiosyncratic factors rather than systematic constituency characteristics correlated with judicial outcomes (Lee, 2008; Lee and Lemieux, 2010; Dell, 2015). Should we accept this premise, constituencies in which female candidates narrowly lost to male candidates can serve as credible counterfactuals for constituencies in which they narrowly won. I employ regressions of the form:

$$\begin{aligned} Y_{ict} = \ & \beta_0 + \beta_1 Female_{ct} + \beta_2 Female_{ct} \times f(Margin_{ct}) \\ & + \beta_3(1 - Female_{ct}) \times f(Margin_{ct}) + \gamma_t + \eta_s + \varepsilon_{ict} \end{aligned} \tag{1}$$

where Y_{ict} is the case outcome of interest and $Female_{ct}$ is an indicator equal to 1 if the politician representing constituency c at time t is female and 0 otherwise. $f(Margin_{ct})$ is an RD polynomial in the female candidate's margin of victory or loss, estimated separately on each side of the win-loss threshold at zero. $Margin_{ct}$ is defined as the vote share of the male candidate subtracted from the vote share of the female candidate, such that it is negative if the female candidate lost and positive if she won. γ_t and η_s represent year and state fixed effects. Fixed effects are not required for identification, but their inclusion increases the precision of estimates (Angrist and Pischke, 2009). ε_{ict} is clustered at the constituency-election level.

RD estimates are sensitive to the choice of polynomial functional form $f(.)$. While low-order polynomials are associated with a larger asymptotic bias (Hahn et al. 2001), the use of high-order polynomials may lead to overfitting, counterintuitive weighting, and erratic behaviour at the win-loss threshold (Cattaneo and Titiunik, 2022). Following Gelman and Imbens (2019), I employ local linear RD polynomials for the baseline and test robustness to quadratic polynomials.

The choice of bandwidth likewise involves a precision-bias tradeoff. Traditional bandwidth selection rules recommend minimising the mean-squared error of the local linear estimator;

however, the bias in estimators resulting from the use of MSE-optimal bandwidths will be asymptotically non-negligible, and confidence intervals around the estimator will have poor coverage (Armstrong and Kolesár, 2018). To address this, Calonico et al. (2014) propose a robust bias-corrected confidence interval, which subtracts an estimate of the misspecification bias and rescales standard errors to account for additional noise introduced by the bias estimation. For baseline regressions, I employ this robust bias-corrected RD estimator paired with the associated MSE-optimal bandwidth (Calonico et al., 2019). For comparison, I also report results using the coverage error (CE) optimal bandwidth following Calonico et al. (2020) and document robustness to the full range of alternative bandwidths.

All regressions use a triangular kernel such that the weight assigned to observations decreases linearly with distance from the win-loss threshold; this weighting method has an MSE optimality property when paired with the MSE-optimal bandwidth (Cattaneo and Titiunik, 2022).

4.2 Assumptions

4.2.1 Continuity at the discontinuity threshold

The key identifying assumption is that all relevant characteristics besides treatment run smoothly at the discontinuity threshold (Dell, 2015). That is, the conditional regression functions under female wins and losses must be continuous at the win-loss threshold. This allows the specification to treat constituencies barely won by female candidates as similar to those barely lost by them.

To assess this assumption, I study baseline constituency characteristics across the win-loss threshold as shown in columns (1) to (19) of Table 1 using regressions of form (1). For the outcome variables displayed in columns (1)-(4) and (6)-(19), point estimates on $Female$ are statistically insignificant and generally small relative to the sample means. Column (5) exhibits a statistically significant difference in the female population share across the threshold. However, the difference is very small compared to the sample mean. Section 5.1 shows that results remain broadly similar when the female population share is included as a control. It is also worth noting that the Calonico et al. (2014) MSE-optimal bandwidth employed for the column (5) regression is 7.411, which may be insufficiently small to justify the close election premise. By contrast, the bandwidths employed in my main regressions in section 5.1 range from 2.556 to 4.396. Thus, the difference in the female population share across the threshold could feasibly be attributed to this

	Demographic Characteristics						
	Total Population	Population Density	Scheduled Caste Share	Scheduled Tribe Share	Female Share	Female Employment	Male Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Female$	-26,222.846	-0.074	1.253	8.923	0.479	0.034	-0.045
	(19,499.134)	(0.053)	(2.765)	(5.207)	(0.260)*	(0.040)	(0.257)
Mean	236,580.200	0.373	16.749	15.524	48.643	0.250	0.437
Obs	114	149	94	213	160	132	119
Bandwidth	4.992	6.745	3.946	9.819	7.411	5.964	5.395
	Economic Characteristics						
	Primary Schools	Secondary Schools	Health Centres	Health Subcentres	Consumption Per Capita	Poverty Rate	Drainage Presence
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
$Female$	-0.009	0.003	-0.000	-0.001	486.083	0.025	0.080
	(0.098)	(0.011)	(0.004)	0.019	(926.417)	(0.027)	(0.055)
Mean	0.938	0.139	0.033	0.168	18,647.760	0.307	0.107
Obs	233	144	158	152	201	228	205
Bandwidth	10.816	6.558	7.296	6.854	9.560	11.094	9.514
	Geographic Characteristics					Case Outcomes	
	River Water	Land Elevation	Land Ruggedness	Forest Share	Irrigated Land Share	Ambiguous Share	Transferred Share
	(15)	(16)	(17)	(18)	(19)	(20)	(21)
$Female$	-34.082	-84.577	-0.125	-0.017	0.572	0.008	0.035
	(38.146)	(55.254)	(0.997)	(0.042)	(3.965)	(0.047)	(0.026)
Mean	120.188	299.230	6.609	0.090	71.612	0.174	0.067
Obs	114	201	207	201	159	254	235
Bandwidth	5.177	8.690	9.046	7.052	7.351	11.619	10.559

Regressions are of the form described in equation (1) using the Calonico et al. (2014) bandwidth and robust bias-corrected standard errors in parentheses. All regressions include year and state fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1: Balance Tests

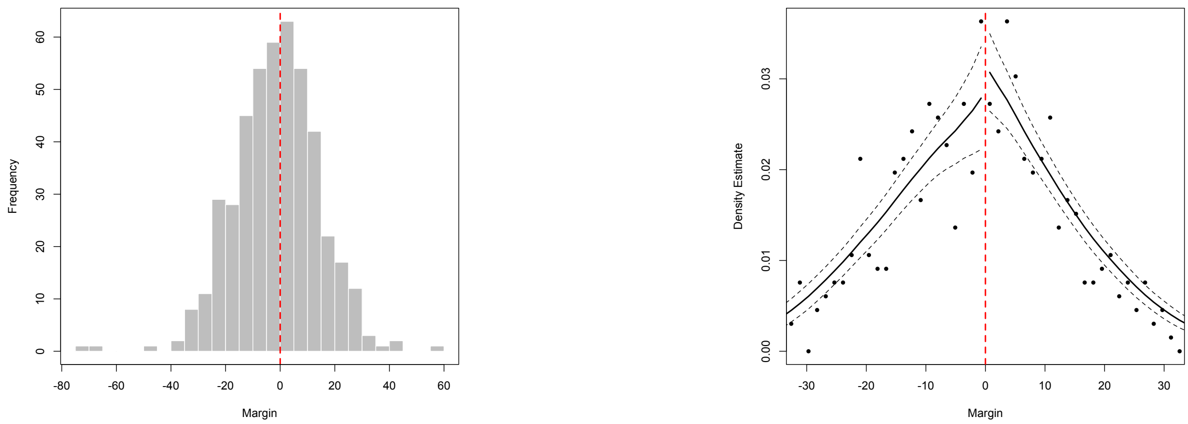
larger bandwidth giving rise to differences in unobservables across the two constituency groups, in ways that are not included in the main regression sample. On balance, it is feasible that, within small bandwidths, differences across the win-loss threshold not attributable to female political representation are minimal.

In light of concerns with the coding of the outcome variable discussed in section 3.1, column (20) of Table 1 tests whether constituencies barely won by a female candidate see a lower share of ambiguous outcomes than constituencies barely lost by a female candidate. Similarly, column (21) tests for balance in the share of cases referred or transferred to a higher court. Reassuringly, in both cases, point estimates on $Female$ are statistically indistinguishable from zero.

4.2.2 Selective sorting

If either men or women systematically win close elections or are able to precisely manipulate vote margins near the win-loss threshold, assignment to treatment cannot be considered quasi-random. For example, Grimmer et al. (2011) find that structurally advantaged candidates, such as those representing the incumbent party, are more likely to win US close elections. Moreover, as pointed out by Makkar (2023), some evidence suggests that Indian constituencies are subject to fraudulent electoral practices such as ballot stuffing, vote buying, or political intimidation (Martin and Picherit, 2019).

To test for sorting, Figure 1 examines the density of the forcing variable $Margin$, the female candidate's margin of victory or loss. Panel (a) displays the distribution of $Margin$ for sample mixed elections following Asher and Novosad (2017); there is no apparent discontinuity in its density at zero. Panel (b) provides a graphical display of the McCrary test for continuity around the treatment threshold (McCrary, 2008). The test fails to reject continuity in $Margin$ at the threshold of zero, suggesting that neither female nor male candidates are able to selectively manipulate close election outcomes.



(a) (b)

Figure 1: Distribution of the Win-Loss Margin
Panel (a) plots a histogram of the win or loss margin for sample elections. Panel (b) plots fitted local polynomial density estimates separately on either side of the zero threshold, using evenly spaced bins to group observations. The McCrary test yields a point estimate of 0.102 and a standard error of 0.176, with a p-value of 0.560.

4.2.3 Compound treatments

Although RD designs are often used to estimate the effect of a politician characteristic on related outcomes, Marshall (2022) suggests that this approach typically results in the identification of compound treatment effects, as opposed to the local average treatment effect (LATE) of the politician characteristic. Given this concern, my RD estimates must be interpreted as the causal effect of electing female politicians, rather than the causal effect of female status.

More specifically, the treatment in my study is defined by being female or not, conditional on a narrow election victory. However, close elections do not enable random assignment of the quality of being female. Intuitively, elected men and women differ on a range of characteristics, due to either self-selection or greater barriers for women during the nomination and election process (Anzia and Berry, 2011); estimates attempting to capture the pure effect of female status may therefore be confounded (Sekhon and Titiunik, 2012). Furthermore, limiting samples to close elections requires conditioning on vote margins that may be partially determined by the gender of the candidates involved. As per Marshall (2022), the RD estimator therefore identifies the LATE of having a female politician in power combined with a differential-weighted LATE of all other candidate characteristics that ensure female candidates remain in close elections with male candidates (compensating differentials). This asymptotic bias is avoidable only under one of two additional assumptions: either that gender does not affect candidate vote shares, or that the considered outcome variables are unaffected by any compensating differentials.

Neither assumption is directly verifiable, but both are likely violated in my context. Table A2 in the Addendum reports results from regressions of form (1) applied to candidate characteristics, including the number of open criminal charges against the candidate, their years of education, age, total assets and liabilities, and membership of the national leading and opposition parties. Although the MSE-optimal bandwidths are often large, likely due to smaller sample sizes, estimates are statistically significant in five out of seven cases and large relative to the sample means in all cases. As such, my results likely capture the weighted effects of both female status and other characteristics which differ between male and female winners of close elections.

Baseline estimates should therefore be interpreted as the effect of a female politician being elected to power, rather than the effect of politician gender alone. In section 5.1, in addition to baseline results, I report regressions that control for the seven candidate characteristics tested in Table A2. This does not wholly address the issues mentioned above, as male and female winners likely still differ in unobservable ways, but estimates may provide a closer indicator of the pure effect of female status.

5 Results

5.1 Crimes against women

Table 2 examines the impact of female political representation on case outcomes for crimes against women. Panel A presents results from a pooled sample comprising all cases involving crimes against women that are explicitly recognised under the Indian Penal Code (IPC)—rape, sexual assault and harassment, dowry deaths, cruelty by husbands or relatives, and causing miscarriage without consent. To capture potential treatment heterogeneity across different types of crimes, Panels B and C present results for samples comprising cases involving only rape charges and non-rape sexual harassment charges, respectively.[5]

	Dependent variable: Convicted						
	Linear Polynomial		Quadratic Polynomial		Additional Controls		
	MSE-optimal bandwidth	CE-optimal bandwidth	MSE-optimal bandwidth	CE-optimal bandwidth	Candidate controls	Female share	Case count
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. All Crimes Against Women							
$Female$	0.050	0.019	0.042	0.018	0.080	0.123	0.059
	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.018)***	(0.000)***	(0.002)***
Mean	0.088	0.098	0.072	0.085	0.085	0.055	0.088
Obs	6,987	5,389	16,880	10,277	8,742	1,516	6,685
Bandwidth	3.466	2.556	7.116	5.025	4.925	0.686	3.481
Panel B. Rape Only							
$Female$	0.183	0.225	0.168	0.187	0.175	0.137	0.198
	(0.018)***	(0.020)***	(0.020)***	(0.021)***	(0.074)**	(0.021)***	(0.017)***
Mean	0.143	0.147	0.123	0.115	0.126	0.148	0.143
Obs	2,427	1,811	5,277	4,391	559	1,457	2,348
Bandwidth	4.396	3.274	8.305	5.929	1.461	3.130	4.264
Panel C. Non-Rape Sexual Harassment Only							
$Female$	0.040	0.023	0.000	0.039	-0.040	0.042	0.034
	(0.003)***	(0.002)***	(0.002)	(0.002)***	(0.003)***	(0.002)***	(0.002)***
Mean	0.066	0.081	0.070	0.086	0.086	0.082	0.082
Obs	4,932	3,377	3,964	3,112	2,838	3,130	3,128
Bandwidth	4.210	3.113	3.285	2.327	2.264	2.588	3.045

Regressions are of the form described in equation (1) with robust bias-corrected standard errors in parentheses. All regressions include year and state fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Close Mixed-Race Elections and Conviction Rate

[5] These two crime categories constitute 83.92% of all cases included in the Panel A sample; the remaining crime categories, such as dowry deaths or forced miscarriage, have small sample sizes and lack sufficient variation for separate regression analysis. For other crimes which may disproportionately impact women, such as kidnappings, the Indian Penal Code does not explicitly differentiate between male and female victims; these crimes are therefore not included in this analysis. Although men may also be victims of rape and sexual harassment, the majority of reported cases in India involve gendered power dynamics, with female victims and male perpetrators (Kalra and Bhugra, 2013).

Columns (1) and (2) present the estimation of the baseline RD specification using the MSE-optimal bandwidth and coverage error (CE) optimal bandwidth, respectively, paired in both cases with a linear RD polynomial. These columns estimate that exposure to a female politician increases the likelihood of conviction by 5% and 1.9% for the pooled sample, 18.3% and 22.5% for rape cases, and 4% and 2.3% for sexual harassment cases. Although I am unable to reject that the two Panel B estimates are statistically identical, the CE-optimal bandwidth estimates for Panels A and C are significantly lower than the MSE-optimal estimates, likely because the CE-optimal bandwidth is chosen to minimise coverage error of the interval estimator, rather than the MSE of the point estimator (Calonico et al., 2020). However, in all cases, the $Female$ coefficients are statistically significant at the 1% level, indicating compelling evidence for a positive effect on the likelihood of conviction.

As a robustness test, columns (3) and (4) display results from specifications employing quadratic RD polynomials. For Panels A and B, the optimal bandwidths selected exceed the prudent range for close-election RD designs, but documented treatment effects remain broadly similar to the baseline, with coefficients that are positive, statistically significant at the 1% level, and comparable in magnitude to their column (1) and (2) counterparts. The Panel C estimate under the MSE-optimal bandwidth, however, bears a statistically insignificant magnitude of zero. This is likely a result of imprecise estimation under the relatively flexible specification. However, the estimate provided under the CE-optimal bandwidth is a statistically significant 3.9%. Overall, results from quadratic specifications broadly support baseline results.

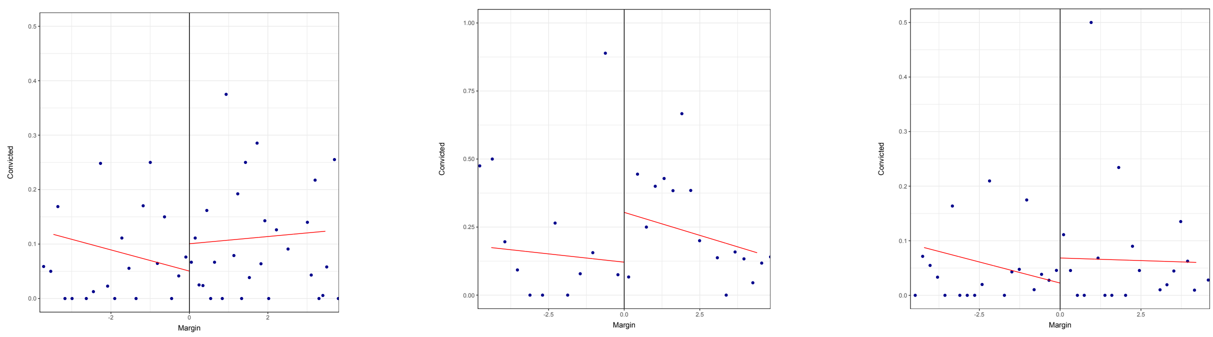
In light of concerns regarding compound treatments discussed in section 4.2.3, column (5) reports results from regressions that control for seven additional candidate characteristics, employing the baseline MSE-optimal and linear polynomial pairing. The Panel B estimate is statistically indistinguishable from the baseline result in column (2) and the Panel A estimate increases in magnitude to 8%. However, the Panel C coefficient turns significantly negative and predicts that female political representation reduces the likelihood of conviction for sexual harassment cases by 4%. This may imply that the treatment effect of pure female status displays more heterogeneity than the combined treatment effect estimated under other specifications, with compensating differentials masking the negative impact of female status; alternatively, these results may be a result of overfitting under a less parsimonious specification. The latter is more theoretically plausible, particularly as there is no obvious a priori reason why the identified compensating differentials should exert varied effects on rape and sexual harassment cases.

Similarly, columns (6) and (7) report results from regressions that control for the female population share and the number of cases filed during the female politician's term, respectively. The latter addresses the concern that results may be driven by increased reporting of crimes against women under a female politician (Iyer et al., 2012). Results remain similar to the baseline except in the case of column (6) under Panel A, for which the estimate increases to 12.3%. Again, this may be a result of overfitting in the presence of an additional degree of freedom or may suggest that the female population share suppresses the observed positive impact of female representation in the baseline specification. If the latter is the case, baseline estimates may be interpreted as an underestimate, rather than overestimate, of the true effect.

Overall, results suggest the presence of a significantly positive $Female$ effect on the likelihood of conviction for crimes against women. Under all specifications, results indicate significant treatment heterogeneity by the nature of the crime, with effects most pronounced for rape. Although the data does not include additional case characteristics that might be used to investigate this heterogeneity further, the difference in the treatment effect for rape and non-rape sexual harassment cases might suggest some relation to the severity of the crime. However, this is not directly testable without further case information.[6]

Figure 2 displays graphical evidence of discontinuous change in $Convicted$ at the win-loss threshold. Local averages of the outcome variable $Convicted$ are plotted along the forcing variable $Margin$. Consistent with the results reported in Table 2, the linear fit lines on either side of the win-loss threshold present clear discontinuous breaks.

[6]Although not presented here due to space constraints, I also identify heterogeneous treatment effects by judge gender and by temporal proximity to the election of the male or female politician. First, results from regressions of form (1) run separately on subsamples comprising cases heard by male judges and by female judges suggest that *both* genders are significantly more likely to rule in favour of conviction for crimes against women under a female politician; however, the estimated effect is larger in magnitude for cases seen by female judges. That said, these results should be interpreted with caution as the judge gender variable is missing for over 60% of observations. Second, splitting the sample into ventiles of the number of days between the date of election of the politician and the date of the final case update, treatment effects appear to be most pronounced in the middle of the politician's term and weakest at the beginning and end.



(a) All Crimes Against Women

(b) Rape Only

(c) Non-Rape Sexual Harassment Only

Figure 2: Graphical Representation of Discontinuities
Dots represent local sample means of the outcome variable *Convicted* calculated within evenly-spaced bins of the forcing variable *Margin*. Local linear regressions of the form given in equation (1) are fitted separately on each side of the win-loss threshold. The sample is restricted to observations within the MSE-optimal bandwidth.

5.2 Gender-neutral crimes

To test whether the results reported in Table 2 are specific to crimes against women, I repeat regressions of form (1) for samples of gender-neutral crimes, following Iyer et al. (2012). Results are reported in Table 3.

Under columns (1)-(4), results are highly sensitive to the choice of bandwidth selection algorithm, suggesting that estimates are unstable and subject to excessively high sampling variation. For murder cases, the MSE-optimal bandwidth under Panel A suggests a significantly positive treatment effect of 6.8% while the CE-optimal bandwidth under Panel B suggests a significantly negative treatment effect of 5%. Similarly, under columns (2)-(3), the two coefficients bear opposing signs. The estimates displayed in columns (2)-(4) are statistically significant under one bandwidth and statistically insignificant under the other. This variation, observed despite the MSE- and CE-optimal bandwidths differing by less than 1%, renders it difficult to claim that female representation has any robust effect on outcomes for these crimes.

I do, however, find significantly negative effects on conviction likelihoods for both bandwidth choices under columns (5)-(7). For defamation cases and 'crimes against religion' (categorised as such by the IPC), the coefficients under the two bandwidth choices are statistically indistinguishable from each other. For theft, the magnitude of the effect estimated under the MSE-optimal bandwidth is relatively small at 0.4%, but significantly larger under the CE-optimal bandwidth. This suggests a negative treatment effect, noting that the CE-optimal bandwidth minimises the coverage error of the interval estimator, but with a small magnitude, commensurate with the

	Dependent variable: Convicted						
	Murder	Trespassing	Forgery	Kidnapping	Defamation	Religious	Theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A. MSE-optimal bandwidth						
$Female$	0.068	0.020	0.010	-0.003	-0.017	-0.121	-0.004
	(0.005)***	(0.003)***	(0.005)	(0.005)	(0.002)***	(0.024)***	(0.002)**
Mean	0.189	0.071	0.025	0.097	0.014	0.061	0.050
Obs	6,826	4,521	3,700	5,905	355	229	5,847
Bandwidth	3.727	1.639	1.663	2.224	4.371	4.154	1.396
	Panel B. CE-optimal bandwidth						
$Female$	-0.050	-0.002	-0.035	-0.028	-0.014	-0.106	-0.047
	(0.005)***	(0.003)	(0.004)***	(0.004)***	(0.003)***	(0.023)***	(0.000)***
Mean	0.251	0.076	0.044	0.075	0.007	0.061	0.050
Obs	3,851	2,948	1,076	5,038	270	196	3,926
Bandwidth	2.759	1.210	1.233	1.645	3.300	3.160	1.029

Regressions are of the form described in equation (1) with robust bias-corrected standard errors in parentheses. All regressions include year and state fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

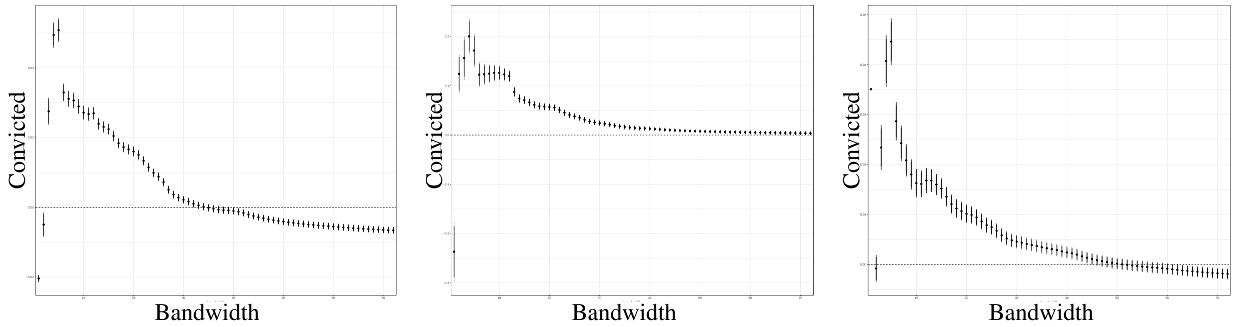
Table 3: Gender-Neutral Crimes

MSE-optimal bandwidth. Taken together, results imply that the influence of female political representation does extend beyond crimes that explicitly impact women more than men; however, we would not expect these negative treatment effects to appear selectively or to differ in magnitude across crimes unless $Female$ effects reflect gendered spheres of influence.

Therefore, results remain consistent with the interpretation that female politicians shape outcomes within issue areas that align with their perceived representational roles, including crimes against women, rather than exerting a broad-based effect on judicial outcomes.

5.3 Bandwidth sensitivity analysis

This subsection considers how results change as the sample is widened to include elections won by larger vote margins. Figure 3 displays estimated treatment coefficients and confidence intervals from regressions of form (1) for the full range of bandwidths, from 0% to 72%. Estimates derived at larger bandwidths cannot be causally interpreted, but their inclusion enables an assessment of the stability of estimates and possible directions of bias from unobserved confounders, thereby supporting the credibility of earlier causal claims.



(a) Crimes Against Women (b) Rape Only (c) Sexual Harassment Only

Figure 3: Robustness to Alternative Bandwidths
The x-axis displays the full range of possible bandwidths within the sample. *Female* coefficients from regressions of the form given in equation (1) are plotted along the y-axis, along with 90% and 95% confidence intervals.

Outside of the smallest bandwidths of 1% and 2%,[7] the $Female$ treatment effect appears stable and remains positive within the range of bandwidths that can be reasonably considered to satisfy the close-election premise. However, it decreases in magnitude as the sample is expanded to include elections won by larger margins, and turns significantly negative at larger bandwidths in the case of subfigures (a) and (c).

This may be symptomatic of constituencies in which women win by large margins differing in unobserved ways from constituencies in which they lose by large margins, as hypothesised in section 4.1. For example, areas with stronger patriarchal norms may be less likely to elect women but may also have more conservative judges who harshly punish crimes against women. Alternatively, the potential impact of stronger partisan dynamics on conviction likelihoods in large-margin constituencies could confound the estimate of the effect of politician gender. If such unobservable factors are driving the observed downward trend in estimated treatment effects, the bandwidth sensitivity plots explicitly justify the use of the close-election RD design.

However, it is also possible that the LATE may diverge from the ATE of female political representation. For example, in safe seats considered to be at less risk of political competition, the politician may have fewer incentives to exert effort to cater to their voter base, regardless of gender. The downward trends observed in Figure 3 may therefore represent differential effort exerted by the elected politician. Alternatively, female politicians' effects on judicial outcomes may be weaker in constituencies where women are commonly elected and their political presence therefore represents a weaker signal.

[7]Estimates produced using the smallest bandwidths must be interpreted with caution. At a bandwidth of 1%, the sample includes cases from only 27, 21, and 25 constituencies for subfigures (a), (b), and (c), respectively; standard errors are therefore likely biased downwards due to insufficient clusters (Cameron and Miller, 2015) and estimates imprecisely estimated due to the small sample size (Angrist and Pischke, 2009).

I am unable to directly disentangle these two potential channels driving the downward trends observed in Figure 3. That said, I find no evidence that female politicians' impact on public goods provision differs between constituencies won by smaller and larger margins; when the win margin is included either as an additional control or as part of an interaction term in the instrumental variable specification from section 6.1 below, which estimates the effect of female political representation on public goods outcomes, it is statistically insignificant. This does not necessarily imply that the same should hold true for female politicians' impact on judicial outcomes, but it provides some limited evidence against the hypothesis that politicians exert less effort in safe seats.

In sum, the bandwidth sensitivity plots support baseline findings at lower bandwidths and highlight the potential influence of either unobserved factors or heterogenous treatment effects at larger bandwidths.

6 Mechanisms

The previous section suggests that female political representation increases the likelihood of conviction for crimes against women at the win-loss threshold. This section investigates which of the channels discussed in section 2.3 might be driving these results.

6.1 Public goods

The exposure channel and policy channel discussed in section 2.3 are both contingent on female politicians' activities while in office. The former requires the politician to engage in activities that signal competence; the latter requires efforts focused around women's issues. To evaluate whether either channel operates in practice, I test whether male and female politicians have differential impacts on public goods provision. If female politicians focus on women's issues, we may expect differences to be more pronounced for goods preferred by women.[8]

Since elections occur at least every five years but data on public goods is accessible only through the Population Census at ten-year intervals, it is challenging to link public good outcomes to specific politicians. Instead, I consider the number of years in a census decade that a

[8]There is some evidence to suggest that policy preferences differ by gender (Alesina and La Ferrara, 2005; Funk and Gathmann, 2015) and the female politicians cater to these preferences (Chattapadhyay and Duflo, 2004).

female politician is in power. To overcome possible endogeneity issues stemming from unobservable differences between constituencies that preferentially elect women or men, I employ an instrumental variables strategy following Bhalotra and Clots-Figueras (2014) and Makkar (2023). Specifically, I use the number of close elections won by a woman between 1987 and 2000 as an instrument for the number of years a female politician was in power between 1991 and 2001,[9] conditional on the number of close elections between a man and woman over the same period. I estimate a two-stage least squares model of the form:

$$Y_c = \alpha_0 + \alpha_1 Female\hat{Y}ears_c + \alpha_2 CloseElections_c + X_c + \eta_s + \varepsilon_c \quad (2)$$

$$FemaleYears_c = \gamma_0 + \gamma_1 CloseFemale_c + \gamma_2 CloseElections_c + X_c + \eta_s + \varepsilon_c \quad (3)$$

where Y_c is the public good outcome of interest for a given constituency measured through 2001 census data, $FemaleYears_c$ is the number of years a woman was in power between 1991 and 2001, $CloseElections_c$ is the number of close elections between a man and a woman between 1987 and 2000, $CloseFemale_c$ is the number of close elections that resulted in a female victory, X_c is a vector of constituency-level controls, and η_s represents state fixed effects.

Identification requires that the number of close elections resulting in a female victory is uncorrelated with the error term in equation (2), or, in other words, that it does not affect public goods outcomes except through the winning politicians' influence, conditional on the total number of close elections and constituency-level controls. This assumption would be satisfied if close elections mimic random assignment to male or female political representation, as previously discussed in section 4. Since optimal bandwidth selection algorithms cannot be employed outside the standard RD framework, I present results for three different close-election thresholds: 1%, 3%, and 5%. Larger thresholds may increase bias due to a greater possibility of non-random election results; smaller thresholds may decrease estimate precision due to fewer elections meeting the threshold condition, thereby limiting the variation available for identification.

[9] I consider the decade 1991-2001 rather than 2001-2011 because the redesignation of constituency boundaries in 2008 raises issues with linking electoral data from before and after 2008.

	Education		Health		Utilities		Transport	
	Primary Schools	Secondary Schools	Health Centres	Health Subcentres	Drinking Water	Power Supply	Railway Facilities	Paved Road
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 5% vote margin								
FemaleYears	-0.005	-0.002	-0.000	-0.002	-0.190	0.000	0.004	0.000
	(0.007)	(0.001)*	(0.000)	(0.002)	(0.187)	(0.000)	(0.015)	(0.000)
First-Stage F-Statistic	44.36	44.36	44.36	44.36	43.82	44.36	13.27	44.94
Panel B: 3% vote margin								
FemaleYears	-0.009	-0.003	-0.000	-0.002	-0.298	0.000	0.016	-0.000
	(0.009)	(0.001)**	(0.000)	(0.002)	(0.231)	(0.000)	(0.021)	(0.000)
First-Stage F-Statistic	26.12	26.12	26.12	26.12	26.13	26.12	6.85	26.19
Panel C: 1% vote margin								
FemaleYears	-0.002	-0.001	0.001	-0.001	0.000	0.000	-0.024	-0.000
	(0.016)	(0.002)	(0.001)	(0.003)	(0.403)	(0.001)	(0.035)	(0.001)
First-Stage F-Statistic	8.84	8.84	8.84	8.84	8.82	8.84	2.84	8.82
Mean	1.029	0.117	0.034	0.123	7.329	0.999	0.635	0.999
Obs	3,084	3,084	3,084	3,084	3,077	3,084	1,019	3,073

Regressions are of the form described in equation (2) with robust standard errors in parentheses. All regressions include state fixed effects and controls for constituency population, female share, Scheduled Tribe share, and Scheduled Caste share. Outcome variables listed in columns (1)-(5) are reported per 1,000 population members, and those in columns (6)-(8) are binary indicators. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: IV Estimates, Public Goods

Results are reported in Table 4. In most cases, estimates are statistically insignificant and very small relative to the sample mean. The sole exception is column (2) at the 5% and 3% vote margins. At these thresholds, point estimates are statistically significant at the 10% and 5% levels, respectively, and suggest that an additional year of female representation elicits decreases of 0.002 and 0.003 secondary schools per 1,000 population members, relative to a year of male representation. This is equivalent to 1.7% and 2.6% per year relative to the sample mean. At the 1% vote margin, the instrument is weak; the column (2) estimate loses statistical significance and decreases in magnitude to 0.001. Although the causal impact of female political representation does not appear to differ significantly to that of male representation for most public goods, the possibility of a non-negligible differential impact in secondary school provision should not be overlooked.

Table 4 considers 2001 *levels* of public goods provision, rather than *changes* between 1991 and 2001 levels. This is due to a high degree of missing data for 1991 constituency-level data, which weakens the first-stage estimates and reduces statistical power. That said, as a further test, Table A3 in the Addendum repeats the 2SLS strategy on changes in public goods levels between 1991 and 2001. Since over 50% of constituency-level population figures are missing for

1991 data, I apply differences in absolute levels of public goods rather than population-adjusted figures. As with Table 4, estimates are generally statistically insignificant and small relative to the sample means. Estimates for secondary schools are no longer statistically significant, but remain negative and large relative to the mean at all vote margins. In contrast to Table 4, Table A3 reports a statistically significant positive effect on drinking water at the 5% vote margin, of a magnitude equivalent to 9.35% of the sample mean. The estimate for drinking water loses significance at the 3% and 1% vote margins, but retains relatively large magnitudes of 2.75% and 9.44%, respectively.[10]

Overall, results from Tables 4 and A3 suggest that female and male political representatives have very similar impacts on the provision of most public goods. However, results share a striking parallel with Chattopadhyay and Duflo (2004), who find that women elected to reserved village council seats in India invest more in drinking water in West Bengal and Rajasthan, and less in education in West Bengal; they also find that drinking water and education are closely tied to women's and men's concerns, respectively, in the relevant regions. Similarly, my results provide tentative evidence of a negative impact of female (relative to male) political representation on secondary school provision and a positive impact on drinking water provision. I observe these effects despite the fact that state-level representatives have relatively little direct control over local public goods (section 2.1.2), as compared to the village council members studied by Chattopadhyay and Duflo (2004).

Findings therefore may be compatible with the hypothesis that female politicians prioritise women's issues, potentially supporting the policy effect. However, results are not conclusive and must be interpreted with caution, as effects observed on public goods may not necessarily extend to legal issues. It is also unclear whether results support the exposure effect. For most public goods, outcomes remain similar to the male politician counterfactual, but it is uncertain how this might impact the public perception of women (Eggers et al., 2018).

In sum, results from studying public goods provision provide tentative support for the policy effect and do not rule out the exposure effect.

[10] Table A3 also predicts a large and statistically significant impact on railway facilities at the 3% vote margin, but I have limited confidence in this estimate due to weak instrument concerns.

6.2 Gender bias

The mechanisms covered in section 2.3 offer nuanced predictions for treatment heterogeneity by gender bias. In constituencies with more pronounced gender bias, there are four possible effects: (1) the election of a female political representative may communicate a stronger signal, precipitating a larger exposure effect, (2) female politicians may exert more effort to address gender-based issues, magnifying policy effects, (3) it may be more costly for judges to alter their behaviour in women's favour, reducing strategic effects, and (4) retaliatory effects may be magnified, and attempts by female politicians to reduce gender bias may therefore be ineffectual.

To test these potential effects, I exploit variation in the constituency-level female population share, defined by the total number of females divided by the total population, which may be distorted by factors such as the local extent of son preference, sex-selective abortions, excess female mortality, and gendered migration (Sen, 2003; Jayachandran, 2023). The female population share therefore serves as a proxy for gender bias (Priyanka, 2020). Following Calonico et al. (2025), I employ a fully interacted local linear RD model:

$$\begin{aligned} Y_{ict} = \delta_0 &+ \delta_1 Female_{ct} + \delta_2 Margin_{ct} + \delta_3 FemaleShare_{ct} \\ &+ \delta_4 Female_{ct} \times Margin_{ct} + \delta_5 Female_{ct} \times FemaleShare_{ct} \\ &+ \delta_6 Margin_{ct} \times FemaleShare_{ct} + \delta_7 Margin_{ct} \times FemaleShare_{ct} \times Female_{ct} \\ &+ \gamma_t + \eta_s + \varepsilon_{ict} \end{aligned} \tag{4}$$

The four effects detailed above are not mutually exclusive. However, if effects (3) or (4) dominate, we would expect $\delta_5 < 0$. If effects (1) or (2) dominate, we would expect $\delta_5 > 0$.

	All Crimes Against Women			Rape Cases Only			Non-Rape Harassment Only		
	MSE-optimal bandwidth	1% bandwidth	5% bandwidth	MSE-optimal bandwidth	1% bandwidth	5% bandwidth	MSE-optimal bandwidth	1% bandwidth	5% bandwidth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Female	1.716	3.687	2.164	4.828	-1.14 $\times 10^{12}$	4.553	1.900	0.530	2.305
	(1.575)**	(8.652)***	(2.824)	(6.252)	(1.55 $\times 10^{12}$)	(6.551)	(1.049)***	(20.550)	(1.822)*
Female $\times$ *FemaleShare*	-0.035	-0.076	-0.042	-0.096	2.43 $\times 10^{10}$	-0.090	-0.039	-0.009	-0.045
	(0.032)*	(0.177)***	(0.057)	(0.128)	(2.88 $\times 10^{10}$)	(0.134)	(0.021)***	(0.420)	(0.038)
Mean	0.079	0.057	0.084	0.133	0.112	0.143	0.060	0.036	0.061
Obs	15,523	1,701	9,898	3,017	472	2,675	9,927	869	5,747
Bandwidth	7.364	1	5	5.387	1	5	8.617	1	5

Regressions are of the form described in equation (4) with robust bias-corrected standard errors in parentheses. All regressions include year and state fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Heterogeneity in Treatment Effect

Results are presented in Table 5. Since the MSE-optimal bandwidth[11] exceeds 5% in all three samples, I also present results for fixed bandwidths of 1% and 5%. In all cases except column (5),[12] the estimate on $Female \times FemaleShare$ is negative and large relative to the sample mean, while the estimate on $Female$ remains positive. Although estimates are statistically insignificant under specifications with smaller sample sizes or outside MSE-optimal bandwidth choices, the magnitude of point estimates still suggest that the effect of female (relative to male) political representation on conviction likelihoods decreases as the female population share increases.

Results therefore appear to support either explanation (1) or (2), suggesting that strategic effects and retaliatory effects may not be of first-order importance. This leaves room for a dominant exposure effect or policy effect. In the latter case, results also suggest that politicians are not constrained by socio-institutional barriers to the extent posited in explanation (4).

7 Caveats and Extensions

This section discusses important limitations and possible extensions to this study.

First, my main specification can only consider constituencies in which women are nominated for political roles and are able to win majority or near-majority vote shares. This may indicate fewer barriers against women in these constituencies, compared to constituencies in which there are no close elections between men and women. I attempt to address this concern by expanding the sample in section 5.3 and considering how the treatment effect differs with gender bias in section 6.2. However, potential differences between the LATE and ATE remain a concern and may limit external validity. That said, the results do provide initial indications of dimensions which might foster heterogenous treatment effects, including the win margin as a proxy measure for political competition or barriers against electing females. Thus, encouraging female political representation may be more important for judicial outcomes in areas facing higher political competition or areas where female leaders are uncommon.

Second, although all primary specifications employ bandwidths that fall within the standard range for close-election RD studies, the quasi-random assumption is not explicitly verifiable. As

[11] Calonico et al. (2025) establish an adapted method of MSE-optimal bandwidth selection and bias-corrected inference for estimating heterogenous treatment effects, but no paper has yet studied how to adapt CE-optimal bandwidth selection for this purpose. Therefore, the CE-optimal bandwidth is not employed in this analysis.

[12] Column (5) displays imprecise and implausibly large point estimates, likely due to the small number of observations and clusters.

mentioned previously, the analysis in section 5.3 provides some support for the close-election premise, but residual concerns about possible confounding effects may remain. However, reassuringly, the bandwidth sensitivity plots suggest that estimated treatment effects decrease as the bandwidth is increased, implying that confounding influences are likely to result in underestimation, rather than overestimation, of the true treatment effect at the win-loss threshold.

Third, the RD design and data structure restrict the analysis to contemporaneous effects of politician gender, preventing examination of dynamic or lagged impacts of female political representation from prior terms. As discussed in section 3.3, baseline regressions employ data that links each case to the politician in power during the date of last case update, in most instances the end of the trial period. Future research may wish to consider how final case outcomes are conditioned by each of the politicians in power during the case cycle,[13] the cumulative effects of sustained female political representation on judicial outcomes, and whether the positive effects of female political representation on the likelihood of conviction decisions for crimes against women last once the politician's term is over.

Finally, I am unable to explicitly disentangle the specific mechanisms driving the treatment effects identified in this study, beyond pointing to the broad possibility of a dominant policy or exposure effect. Addressing this question may require more detailed information on case or judge characteristics or politicians' engagement with gender-based issues, so as to exploit direct variation in judge and politician incentives. I view these as avenues for future research.

8 Conclusion

This paper presents novel evidence of the impact of female political representation on judicial outcomes for crimes against women. Using a close-election regression discontinuity design, I find that constituencies that narrowly elect female over male political representatives exhibit a higher likelihood of conviction for these crimes. I do not find similar differences in conviction likelihoods for gender-neutral crimes, suggesting that female politicians shape judicial outcomes

[13] Although not reported due to space constraints, I run an additional set of RD regressions using data that links each case to the politician in power when the case was filed, to test whether female representation impacts the likelihood that a case receives a hearing within 6 months of being filed. Due to India's severe judicial delays, this is also an outcome of interest. Rape cases are significantly more likely to receive a hearing within 6 months, but non-rape sexual harassment cases are significantly less likely to receive one, hinting that more severe cases might be heard at the expense of less severe ones. These dynamics may serve as an area for future research.

in areas that align with gendered spheres of influence and interest. Additional analysis—on whether female politicians cater to gendered preferences in public goods and whether the effect of female political representation on conviction decisions for crimes against women varies with gender bias—points toward two potentially important mechanisms. These include a policy channel, whereby female politicians actively attempt to act in women's interests, and an exposure channel, whereby observing female representatives positively informs citizens' views on women's competencies.

My results contribute to three strands of the extant literature. First, by showing that female political representation can impact conviction decisions, my findings highlight the role of politician characteristics in shaping judicial outcomes, a discussion previously neglected in the empirical literature. Second, they emphasise that the judiciary is not immune to political and environmental influence. While the literature covering the Indian context has previously highlighted the role of strategic motivations, this study highlights new mechanisms through which judges may be influenced by the political environment. Third, my results support a previously unexplored explanation for regional patterns in conviction rates for crimes against women, with implications for judicial reform policies.

This study therefore emphasises the importance of political representation in expanding vulnerable groups' access to justice. Future research may wish to consider how politician characteristics impact other civil rights, or the dynamic effects of female political representation and mechanisms through which such representation shapes judicial outcomes.

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Addendum

Disposition Name	Convicted
Abated	
Absconded	
Acquitted	
Allowed	
Appeal Accepted	
Award	
Cancelled	
Closed	
Committed	X
Compounded	
Compromise	
Confession	

Disposition Name	Convicted
Convicted	X
Decided	
Dismissed	
Disposed	
Died	
Execution	X
Fine	
Judgement	
Missing [Pending]	
Other	
Plead Guilty	X
Prison	X

Disposition Name	Convicted
Procedural	
Probation	
Quash	
Referred	
Reject	
Remanded	
Settled	
Sine Die	
Stayed	
Transferred	
Untrace	
Withdrawn	

Table A1: Mapping Dispositions to Judicial Outcomes

	Candidate Characteristics						
	Criminal	Education	Age	Assets	Liabilities	BJP	INC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$female$	-0.387	2.325	-7.769	-1712.677	-686.364	0.469	-0.131
	(0.540)	(0.935)**	(2.908)***	(673.658)**	(280.450)**	(0.109)***	(0.125)
Mean	0.828	11.588	48.607	749.468	220.277	0.309	0.326
Obs	87	68	140	105	99	123	175
Bandwidth	4.344	3.541	7.672	5.636	5.286	6.664	9.742

Regressions are of the form described in equation (1) using the Calonico et al. (2014) MSE-optimal bandwidth and robust bias-corrected standard errors in parentheses. All regressions include state fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Candidate Characteristics

	Education		Health		Utilities		Transport	
	Primary Schools	Secondary Schools	Health Centres	Health Subcentres	Drinking Water	Power Supply	Railway Facilities	Paved Road
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: 5% vote margin				
FemaleYears	-0.497	-0.025	0.166	-0.225	53.090	-0.000	0.021	-0.001
	(0.792)	(0.282)	(0.114)	(0.313)	(22.860)**	(0.000)	(0.016)	(0.002)
First-Stage F-Statistic	42.05	32.29	36.22	37.25	23.82	42.88	9.68	42.05
				Panel B: 3% vote margin				
FemaleYears	-1.303	-0.436	-0.126	-0.471	15.650	-0.000	0.036	-0.000
	(0.994)	(0.351)	(0.141)	(0.388)	(28.050)	(0.001)	(0.020)*	(0.002)
First-Stage F-Statistic	24.19	18.78	21.50	22.14	15.06	24.87	5.77	24.70
				Panel C: 1% vote margin				
FemaleYears	-1.814	-0.564	-0.011	-0.087	53.630	0.000	-0.003	0.000
	(1.688)	(0.592)	(0.225)	(0.664)	(45.800)	(0.002)	(0.031)	(0.004)
First-Stage F-Statistic	8.67	7.14	8.49	7.84	6.35	8.62	2.53	8.52
Mean	34.518	-1.414	0.132	6.669	568.1	0.002	0.042	0.009
Obs	2,982	2,599	2,520	2,638	1,989	2,995	840	2,952

Regressions are of the form described in equation (2) with robust standard errors in parentheses. All regressions include state fixed effects and controls for constituency population, female share, Scheduled Tribe share, and Scheduled Caste share. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: IV Estimates, Public Goods