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PROCEDURAL BARRIERS TO POLITICAL CANDIDACY: GENDER, DISCOURAGEMENT AND CANDIDATE PERSISTENCE*

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A procedural rule in Indian elections requires candidates to pay a nominal deposit, which is forfeited on obtaining fewer than one-sixth of the votes. Forfeiture diminishes female recontesting by 13.3 percentage points in the following election (relative effect size of 68%). We find no such effects for men. These effects are persistent and discourage female candidates in the long term as well. States with more regressive gender norms exhibit stronger findings, with female forfeiters also being 11.8 percentage points less likely to win the following election. We enhance our analysis by conducting a survey experiment with representative voters.

JEL codes: J16, D72

The under-representation of women in leadership roles is a widespread problem that affects almost every society and domain of life.[1] One area where this phenomenon has profound consequences is the political arena. Female under-representation in politics is pervasive the world over, with only 24% of parliamentarians being female across all legislative assemblies as of 2019, ranging from 16% in the Pacific to 30% in the Americas.[2] Not only does this occurrence have considerable fairness repercussions, but also dramatic efficiency implications, on account of the considerable waste of talents and because female political participation has been linked to greater government spending, health care and aggregate productivity (Chattopadhyay and Duflo, 2004; Hsieh *et al.*, 2019). In this paper, we shed light on this critical issue by analysing results from state elections in India between 1977 and 2019. Our contribution is two-fold. We show that a commonly adopted procedural barrier to candidacy has the perverse effect of dissuading women who fail to pass the hurdle from competing again, but does not deter men. Additionally, we provide evidence that regressive gender norms play an important role in driving this result.

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[1] In 2019 only one in four parliamentarians worldwide were women, while only 6.6% of women held CEO positions in the corporate domain; as of 2015, women represented no more than 24% of all people working in the news media (see UN Women, 2020); from 1901 to 2021, the Nobel Prize has been awarded to 943 individuals and only 58 of these were women (see https://www.nobelprize.org/prizes/lists/all-nobel-prizes/).

[2] See the statistics compiled by the United Nations on women's leadership and political representation; UN Women (2021).

We focus on a procedural condition restricting candidates' eligibility in Indian elections. Candidates must pay a monetary deposit to compete: those who receive less than one-sixth (or 16.7%) of the total number of votes forfeit the deposit. The rationale for this institution is to discourage non-serious candidates from contesting and clogging the ballot paper.[3] However, losing the deposit can also deter forfeiters from standing again for election in the future, particularly if, as is the case in Indian society, forfeiture is commonly stigmatised, being regarded as a humiliating defeat and an extreme loss of face (see Section 1). Exploiting the above-induced regression discontinuity (RD) design, we analyse the effect of falling just below, as opposed to just above, the forfeiture threshold on the decision to compete again in the future. We observe that deposit forfeiture has a substantial and persistent deterrence effect on female candidates over time. Women who fall just below the threshold are 13.3 percentage points (pps) less likely to compete in the next election and 8.2 pps less likely to make it into the top-3 positions compared to those who just pass the threshold. Notably, these results are long term: forfeiting the deposit discourages women from competing again over the next 20 years (i.e., four electoral cycles), with the penalty rate being 14.4 pps.[4] Both short- and long-term effects are also pronounced in relative terms, with over 60% relative to the mean among candidates just above the threshold. On the contrary, we consistently find null effects for men. This is our main finding: women, contrary to men, are less likely to put themselves forward again after a clear defeat.

Elections constitute an intricate process, where the dynamic interplay among candidates, voters and parties decisively shapes the outcome. The success of a candidate is influenced by their abilities and efforts, yet it ultimately hinges on the choices made by voters, which are, in turn, mediated by party nominations. Moreover, candidates, parties and voters are all embedded in a given culture, permeated by specific stereotypes (Bordalo *et al.*, 2019), and subject to social pressure. Indeed, we show that our main results are concentrated in states where the gender ratio is tilted in favour of men. When considering these states, where gender norms are more regressive, we find that forfeiting the deposit diminishes women's likelihood to stand again for election four years later by 18.6 pps. It also decreases their probability of appearing in the top-3 positions by 21.9 pps and of winning the next election by 11.8 pps. Considering the long term, the effect on female candidates' persistence extends over four electoral cycles, with a remarkable 25.1-pp penalty rate. Additionally, women who barely lose the deposit are 12.3 pps less likely to either win the election or be the runner-up over the following 20 years. Conversely, in states where the gender ratio is more even, the effect disappears.[5] These results point towards a cultural element to women's reaction to failure[6] and represent the second contribution of our study.

To probe further the nexus between candidates, parties and voters, we focus on candidates who run again for office after forfeiture. Conditional on contesting again in the next election, women who forfeit the deposit are substantially more likely to run with a different party and receive a lower vote share compared to women who retain it. On the contrary, past forfeiture has no effect on male candidates' party affiliation nor on the votes they garner four years later.[7] Since women, unlike men, are very likely to move to a different party, it is hard to know

[3] This is a very common procedural restriction, adopted by roughly half of all the world's democratic countries. See Section 1 for a description of the institutional background.

[4] We thank an anonymous referee for suggesting analysing candidates' behaviour over a longer time span.

[5] We also use an alternative measure and find similar results. See Section 5.1.1 for more details.

[6] This observation is consistent with experimental results reported by Booth *et al.* (2019). See also Gneezy *et al.* (2009) on gender differences in competition in a patriarchal and in a matrilineal society.

[7] We note that these results are conditional on recontesting, which may introduce selection concerns. See Section 5 for a discussion.

whether voters punish female as opposed to male forfeiters. To get a better understanding of this phenomenon, and bearing in mind that our main results originate from states with regressive gender norms, we augment the above analysis by designing and conducting a survey experiment with a representative sample of voters from the state of Uttar Pradesh. Uttar Pradesh is the most populous Indian state, characterised by a gender ratio heavily tilted in favour of men, and ranked among the worst states in terms of gender equality. We find that losing the deposit is, indeed, considered shameful by respondents, a result that lends legitimacy to the rich anecdotal evidence documenting this phenomenon. However, respondents are more lenient towards women in this dimension; they also rate a female candidate who forfeited the deposit higher than a male forfeiter and are more encouraging with the former than with the latter when asked whether they should run for office again, though male respondents exhibit less leniency than women. Two caveats are in order. First, one must always be cautious when interpreting survey results as a certain degree of social desirability bias is inevitable.[8] Second, we can only conduct the survey on the current voter population, but we know that attitudes towards women in India, generally and in politics, have marginally improved over the past 40 years (see, e.g., Kadam, 2012; Fadia, 2014), although plenty remains to be done.

In this manuscript, we connect with and contribute to two bodies of research: empirical studies on electoral competition, and the literature on competition and gender. The former have predominantly focused on the upper end of the vote-share distribution, i.e., winners and runners-up (e.g., Anagol and Fujiwara, 2016 and Folke *et al.*, 2016). Wasserman (2021) considered US state legislators, reporting no gender difference in candidate persistence. Wasserman (2023) analysed local Californian elections, showing that, following a defeat, women are less likely than men to run again within the next four years.[9] Bernhard and de Benedictis-Kessner (2021) studied both state and local legislators, finding no gender difference. They analysed the same local elections as Wasserman (2023), finding similar results when restricting to runs within four years, but showing that the gender difference disappears when they expand the time span. Thomsen (2018) examined US House elections, also finding a null result.[10] Our paper contributes to these scholarly works in two ways. In terms of results, ours is the first study that documents a long-term gendered effect of failure. Additionally, we provide evidence of the cultural origin of such an effect. In terms of approach, differently from this literature, we focus on distant losers, i.e., candidates who are far from the winning vote share. To the best of our knowledge, ours is the first study of this type.[11] We believe that this approach has three merits. First, the lower vote-share range is precisely where we find most female candidates, especially in societies where gender inequality is more pervasive. If forfeiting the deposit deters women from running again, this rule indirectly denies them the opportunity to gain experience and, possibly, contend for office in the future, thus perversely increasing the gender imbalance. Indeed, Thomsen and King

[8] See Section 6 for a detailed description of the design showing how we minimised this potential issue.

[9] This difference depends on the specific setting: the gap disappears in contexts where women are well represented (i.e., school boards) or among experienced candidates (see Wasserman, 2023).

[10] See also Brown *et al.* (2025), who documented gender differences in the political career progression of US candidates.

[11] A notable exception is Pons and Tricaud (2018), who used a fuzzy RD design around the 12.5% threshold in French parliamentary and local elections. Candidates ranked first and second in the first round automatically qualify for the runoff election, while candidates ranked third pass to the second round only if they are voted by more than 12.5% of registered citizens. Thus, although it looks at a lower threshold than we do, differently from us it does not consider candidates ranked worse than third. See also Baskaran and Hessami (2022) for a non-RD approach. They studied local council elections in a German state between 2001 and 2016; using a fixed-effect analysis, they reported gender differences in recontesting.

(2020) showed that the most significant factor contributing to the shortage of female candidates is the lack of women progressing through the pipeline. Second, if the gendered asymmetric effect of barely losing the election wanes with experience, as Wasserman (2023) reported, by focusing on the top two or three candidates we run the risk of overlooking interesting phenomena. Third, it allows us to cleanly identify the effect of failure, disentangling it from incumbency effects. While an incumbency advantage is well documented for the United States (see, e.g., Gelman and King, 1990; Cox and Katz, 1996; Lee, 2008; Ferreira and Gyourko, 2009), Linden (2004, unpublished) and Uppal (2009) reported evidence of an incumbency disadvantage among Indian candidates,[12] and Bhalotra *et al.* (2018) showed that Indian female incumbents are more likely to recontest than male incumbents.

Our main findings are consistent with some of the most recent findings in the literature on gender and competition. Buser and Yuan (2019) analysed the Dutch Math Olympic, showing that, unlike males, female secondary school students who fail to qualify for the second round are less likely to compete again the following year.[13] Experimental results show that the gender gap in competitiveness arises early in life (already in kindergarten) and remains stable thereafter for most participants (Sutter and Glätzle-Rützler, 2015). Various papers find evidence in support of a social and cultural origin of this difference (e.g., Gneezy *et al.*, 2009; Booth and Nolen, 2012; Booth *et al.*, 2019). Indeed, consistently with such an explanation, several studies document that, all else equal, women are evaluated worse than men in politics (Beaman *et al.*, 2009; Branton *et al.*, 2018; Rheault *et al.*, 2019), business (Elsesser and Lever, 2011) and academia (Boring, 2017; Mengel *et al.*, 2019; Sarsons *et al.*, 2021) and even evaluate themselves more harshly in a male-typed task (Exley and Kessler, 2022). Similarly, Egan *et al.* (2022) reported evidence of a double standard in punishing misconduct in the financial advisory industry, while Biasi and Sarsons (2022) showed that female school teachers are paid lower salaries in flexible wage settings, potentially because of a gender gap in wage negotiations.[14] We add to this literature by providing one of the first pieces of evidence of how seemingly innocuous procedural barriers to political candidacy can have deleterious effects, perpetuating gender gaps in the important and policy-relevant domain of electoral competition.

The rest of the paper is organised as follows. Section 1 provides details on the institutional background of the Indian electoral setup, while Section 2 describes our data. Section 3 outlines our empirical methodology and provides some preliminary graphical evidence, as well as the validity of our empirical design. Section 4 presents the main findings and Section 5 explores some potential mechanisms and studies long-term political outcomes. Section 6 reports results from our field survey. Section 7 concludes.

[12] See also Klašnja and Titiunik (2017), who reported a similar pattern in other developing countries.

[13] Similarly, Kang *et al.* (2024) reported a gender disparity in the likelihood to retake a college entrance exam and Ellison and Swanson (2023) documented a stronger discouragement effect among girls in high school math. Different experiments report similar findings in the lab. Gill and Prowse (2014) documented that women react to losing differently than men: while losing per se has a negative impact on women's future productivity, this is only true for men if the stakes are big enough. Women respond to luck more than men, with their response accounting for more than half of the gender performance gap. Shastry *et al.* (2020) also found that men and women react differently to noisy feedback, generating gender differences in tournament entry. See also Buser (2016), who showed that, following a loss in a tournament, women lower their future performance, while men pick more challenging targets.

[14] See also Sarsons (2019), who provided evidence of differential treatment of female and male surgeons. Physicians are more optimistic about male surgeons following a positive outcome and more pessimistic about female surgeons following a negative one.

1. Institutional Background

Candidate eligibility requirements are standard throughout all democratic countries. Some of these are fairly universal, such as restrictions linked to age and citizenship. Others are meant to prevent the proliferation of non-serious and farcical candidates who impose a cost on society both directly, through administrative costs and indirectly, by increasing confusion and potential mistakes, thus distorting the representative character of elections. Such conditions typically impose some procedural restrictions that take one of two forms. Candidates are either required to pay a monetary deposit with the risk of losing it, if they fail to obtain a minimum number of votes, or to prove that they have the support of a sufficient portion of the population. Blais *et al.* (2018) conducted a comparative study of electoral laws considering a group of 63 democratic countries[15]—18 from North and South America, 8 from Asia, 6 from Oceania, 8 from Africa and 23 from Europe. According to this study, democracies are almost equally divided between those that require a deposit (32) and those that do not (31).[16] Patterns are quite manifest. Every former British colony, with the exception of Guyana and Namibia, follows Britain's example and requires a deposit. Among the rest, the vast majority (76%) do not impose a deposit (notable examples being France, Portugal, Spain and all former Spanish and Portuguese colonies). All of the countries with plurality or other majority electoral formulae and single-member districts, with the exception of France, require a deposit, while countries with proportional representation tend not to. As a former British colony adopting plurality voting, India has required a deposit since its independence, both to stand for election in the Indian Parliament and to run for a seat in one of the State Legislative Assemblies.

The Republic of India is composed of twenty-eight states and eight union territories. Indian citizens directly elect two legislative bodies: the lower house of the Parliament of India, called Lok Sabha, and the State Legislative Assemblies called Vidhan Sabha. This paper focuses on the latter. Except for five union territories that are under the direct control of the Union Government of India, each of the twenty-eight states and the three remaining union territories is characterised by a State Legislative Assembly. This represents the lower house in six states, where an upper house is also present, while it is the sole legislative body everywhere else. Each member of the Legislative Assembly (MLA) is directly elected by plurality voting in single-member constituencies for a five-year term. Section 34 of the Representation of the People Act, 1951, introduced the requirement of an electoral deposit, which is forfeited if a candidate fails to receive at least one-sixth of the total votes cast. Up until 1996, a general candidate was required to pay a deposit of INR 250 in order to stand for election in Vidhan Sabha; the deposit was half that amount, INR 125, for members of Scheduled Castes (SCs) and Scheduled Tribes (STs).[17] The real value of the deposit decreased over time, while the number of candidates increased, and in 1996 the Representation of the People (Amendment) Act raised the deposit to INR 5,000 for general candidates and INR 2,500 for SC/ST members. In 2009 the deposit was increased

[15] The countries in this group are chosen according to the annual reports of Freedom House on the state of democratic rights and civil liberties in all countries of the world, with the exclusion of the United States and Switzerland. These two countries have been dropped from the study because, unlike other federations, they rely heavily on state and canton laws even in electoral matters (Blais *et al.*, 2018).

[16] To be precise, when considering electoral deposits, Blais *et al.* (2018) dropped Japan from the analysis because of data unavailability and, thus, reported an equal split between thirty-one countries that require the deposit and thirty-one that do not. However, Japan introduced the deposit in 1952, and it has been a requirement since (see Harada and Smith, 2014).

[17] The Scheduled Castes and Scheduled Tribes are officially designated groups of people recognised in the Indian constitution. They comprise 16.6% and 8.6%, respectively, of India's population, according to the 2011 census.

once more to the current amount of INR 10,000 for general candidates and INR 5,000 for SC/ST members.[18]

Forfeiting the deposit is universally regarded in Indian society as a humiliating defeat to be ashamed of.[19] The link between electoral deposit forfeiture and failure is so radicated in Indian culture that the Tamil locution *deposit gaali*, which indicates the loss of the deposit (literally meaning 'deposit gone'), is commonly used as a metaphor to indicate a debacle or an extreme loss of face. Indian media pay plenty of attention to this phenomenon and local news regularly report the names of deposit forfeiters, thus exposing them to the public. Forfeiters are generally mocked and typically ridiculed by their opponents on account of their performance. As we will see in Section 6, results from our survey provide additional evidence of this widespread sentiment, showing that losing the deposit is indeed regarded as shameful by respondents.

2. Data

We employ constituency-level electoral data for State Assembly elections between 1977 and 2019 compiled by the Indian Election Commission (ECI, 2020). We focus on the eighteen most populous states in India that cover around 95% of the total Indian population.[20] These data provide information on two levels: the assembly constituency (AC) and the candidate. The former contains information on the characteristics of the constituency, such as the total number of votes cast and of candidates who filed their nomination papers, but information on candidates is limited to the winner and the runner-up. However, the candidate-level data contain details on every candidate who appeared on the ballot paper in a given constituency-election year pair. This contains the name of the candidate, their gender, party affiliation and the total number of votes they received, regardless of their final ranking or vote share. This latter dataset constitutes our main resource since we need information on all candidates at the lower end of the vote-share distribution, i.e., among losers. From 2004, the Election Commission of India (ECI) also collected mandatory information from all nominating candidates regarding their assets, criminal cases and education qualifications. Since these are available only for a small sample of our data, we mostly use these only to perform balancing analyses.

The Indian electoral landscape changed in 2008, when constituency boundaries were re-drawn. As a result, we divide the sample into two parts—pre-delimitation (1977–2007) and post-delimitation (2008–19)—while defining our variables of interest that link candidates across election years. However, the estimation sample pools these two samples together to maximise statistical power.

As the candidates do not have a unique identifier, we perform matching based on candidate names to identify contestants across elections.[21] This is a non-trivial exercise as Indian names are

[18] Section 4.3 below considers how these changes in the deposit regime might impact our findings and their interpretation.

[19] See, for instance, how the *Times of India* described the electoral performance of the Chief Minister of Delhi, and leader of the Aam Aadmi Party (AAP), Arvind Kejriwal in 2020. 'Arvind Kejriwal suffered a humiliating defeat in Varanasi by forfeiting his security deposit' (TOI, 2020). Another example comes from Prime Minister Narendra Modi, who in 2019 mocked the main opposition party (Congress) and its 'dream' of coming to power, asking the electorate to 'punish Congress in such a way and also their allies that they should not be able to save their deposits' (NDTV, 2019).

[20] We have the following states in our sample: Andhra Pradesh, Assam, Bihar, Chhattisgarh, Gujarat, Haryana, Jharkhand, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh, Uttarakhand, West Bengal.

[21] We drop all candidates with a vote share below 1% to simplify the name matching process. In any event, these are likely to be frivolous candidates and unlikely to ever make it into the bandwidth of interest around the threshold.

often long and spelling errors across years can persist. Along with performing matching within the same AC, we also match candidates across different ACs.[22] In the case of the latter, to limit false matches, we restrict our search to ACs that are within 40 km from each other. We use various techniques such as matching based on the first/last name, using the initials, changing the order of first and last names, and identifying matches based on the difference between string lengths of names and so on.

These steps reduce the number of false mismatches for each candidate; however, each candidate may still have several names matched in election $t + 1$. To ensure that we have correct candidate names being matched, we augment the above by manually checking the matched names and removing any false matches. This further improves the accuracy of our matching procedure. Indeed, our matching algorithm performs remarkably well with a high match rate. To probe this, we randomly pick 500 matched candidates across states and election years and re-perform the matching exercise completely by hand. The algorithm can match 472 of these candidates correctly, amounting to a success rate of roughly 94%. Similarly, to probe the match across different ACs, we randomly pick fifty candidates and establish their identities by going through newspaper records by hand. We can find records for forty of these candidates and the algorithm correctly matches thirty-four of these across election years.

We end up with a sample of 114,433 candidates, out of which 20,705 (or around 18%) stood for election again after five years.[23] Of these, 1,022 (4.94% of all recontesting candidates) ran for a seat in the Assembly from a different AC. Online Appendix Table A.1 presents some constituency-level descriptive statistics at five-year intervals from 1977 to 2007.[24] The average number of male candidates in a constituency has varied between 6 and 11, while for females it was 0.16 in 1977, but has risen since, though it still remains below 1. Indeed, as Online Appendix Table A.1 shows, the average proportion of female candidates in 1977 across constituencies is only 3%, rising to 8.8% in 2007. This highlights the severe under-representation of Indian women in the political landscape, providing empirical support for the motivating arguments presented in the introduction. In terms of forfeiture, proportion of male candidates who forfeit their deposit was around 60% in 1977–82 and has hovered between this and 72%. On the other hand, females forfeit at slightly lower rates, with 58% forfeiting in 1977–82, although this rose to 71% in 2003–7. These numbers signify that the vast majority of candidates who contest state elections in India actually end up forfeiting their monetary deposit, yet there has been no previous evaluation of empirically studying the political careers and pathways of these candidates. This is particularly important for inexperienced and unconnected candidates who might require participation in a few elections to understand the dynamics of the political landscape and to improve their political fortunes. In this paper, we bridge this gap in the literature and investigate how candidates at the lower end of the vote-share distribution fare while facing electoral losses.

[22] Previous research (e.g., Anagol and Fujiwara, 2016) restricts the matching exercise to candidates contesting from the same AC. However, in the Indian setup it is likely that candidates can recontest from neighbouring areas to increase their chances, since ACs are small and rules regarding geographic limitations on contesting from local constituencies are lax.

[23] This sample is restricted to candidates whose vote share lies between 1% and 45%. The former restriction is due to the name matching algorithm we ran, as mentioned in footnote 21, while the latter restriction is just for expositional purposes for the RD plots in Figure 1. We repeat our main analysis, removing the upper limit, and our baseline findings are not impacted, since the optimal bandwidth in Table 2 is less than 6 points.

[24] We restrict these statistics to 2007 since constituency boundaries remained fixed during this time period.

3. Methodology and Graphical Analysis

3.1. *Estimation Strategy*

In this section we outline our main estimation methodology and present some preliminary graphical evidence for our baseline findings. As discussed above, the main goal of the paper is to study the effect of forfeiting the monetary deposit in election t on candidacy in election $t+1$. All candidates who receive less than one-sixth of the vote share in their constituency forfeit the deposit. This induces a sharp RD design and one can study candidates around the threshold to causally answer our research question. RD designs have become the foremost methodology in the causal inference toolkit and, arguably, provide the closest simulation of a randomised experiment in non-experimental settings (Cattaneo and Titiunik, 2022).

Let s_{ict} be the RD running variable defined as the vote share of candidate i in constituency c in election t minus 1/6. For ease of exposition, we invert this variable so that positive values indicate those who lose the deposit (treated), while negative values represent those who retain the deposit (control). We non-parametrically estimate the treatment effect (τ) given by the expression

$$\tau = \lim_{s_{ict}\downarrow 0} \mathbb{E}[y_{ict}|s_{ict}] - \lim_{s_{ict}\uparrow 0} \mathbb{E}[y_{ict}|s_{ict}], \tag{1}$$

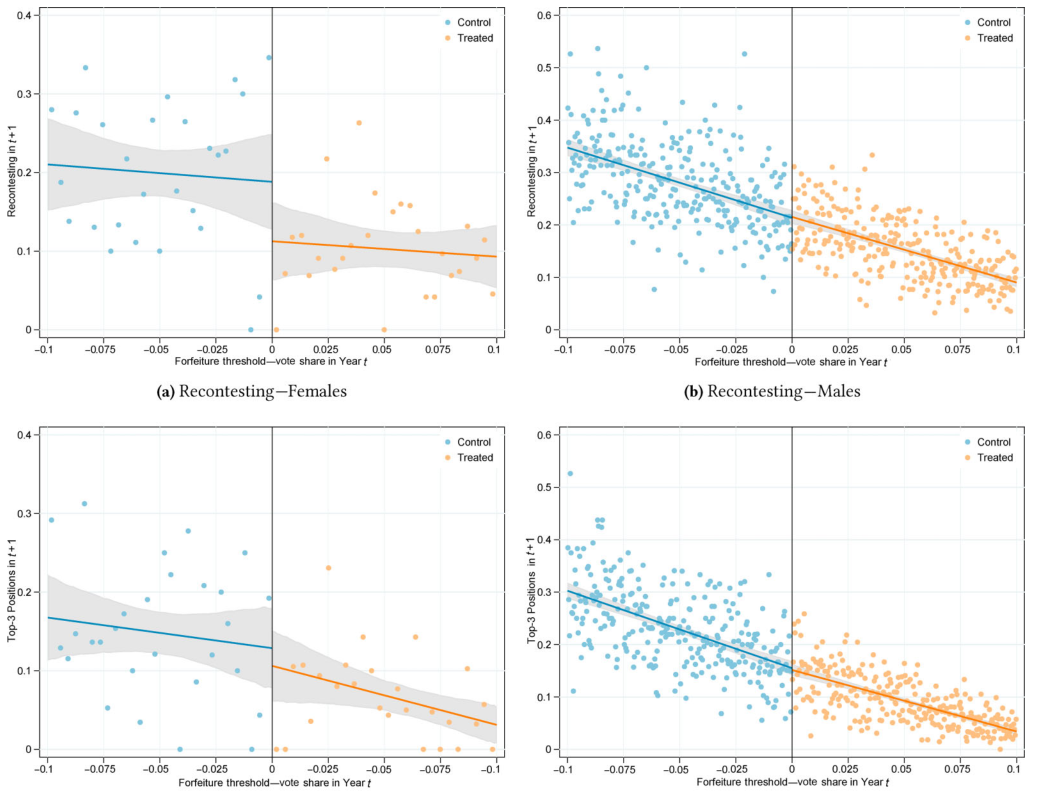
where y_{ict} is our outcome variable of interest, for example, recontesting, vote share or appearing in the top-3 positions in the elections in $t+1$. We implement (1) separately for male and female candidates. Under the assumption that the above conditional expectations are continuous in s_{ict}, the first term will converge to the expected outcome for a deposit forfeiter with the same vote share as a deposit retainer. Similarly, the second term converges to the expected outcome of a deposit retainer with the same vote share as the forfeiter. The latter term thus provides us with an adequate counterfactual for the treated units on the right-hand side of the threshold. The difference between the two terms in (1) thus allows us to estimate the treatment effect of forfeiting the monetary deposit.

Following recommendations in Cattaneo *et al.* (2019), we estimate the conditional expectation of y_{ict} on s_{ict} using local polynomial regressions. These methods require three key choices: (1) choosing a neighbourhood around the threshold, called the bandwidth (h); (2) choosing the degree of the local polynomial in this neighbourhood and (3) choosing the kernel specifying the weighting scheme of observations within h. Recent work has established the suitability of using local linear regression for estimation in RD designs (Gelman and Imbens, 2019), while we employ a triangular kernel for the third decision, following Cattaneo *et al.* (2019).[25] We use methods developed by Calonico *et al.* (2014) for choosing the optimal bandwidth, but we provide extensive evidence probing the robustness of our results to different choices of bandwidth.

3.2. *Graphical Evidence*

We now present some preliminary graphical evidence as a precursor to our non-parametrically estimated main results in Section 4 below. While such preliminary plots are standard in the empirical RD literature, valid estimates of treatment effects can only be obtained by applying non-parametric methods around the threshold, which can account for various identification and inference concerns like bandwidth selection, appropriate SEs and p-values, and the choice of

[25] Results remain robust if we employ the Epanechnikov kernel instead.



(a) Recontesting—Females

(b) Recontesting—Males

(c) Top-3 Positions—Females

(d) Top-3 Positions—Males

Fig. 1. *(a),(b) Recontesting and (c),(d) Top-3 Positions in* $t + 1$ *around the Forfeiture Threshold.*
Notes: The above figure presents graphical evidence from our regression discontinuity design around the forfeiture threshold. We use the optimal procedures outlined in Cattaneo *et al.* (2019) for binning the raw data. The solid lines plot a global linear polynomial on the unbinned data on each side of the discontinuity. The shaded area depicts the 95% confidence interval for the polynomial smooths.

polynomial functions. Cattaneo *et al.* (2019) provided an excellent primer on the importance and implementation of estimating RD designs using non-parametric methods.

Figure 1 optimally bins up the raw data using methods outlined in Cattaneo *et al.* (2019) rather than using arbitrary, predetermined bin sizes. We focus on the vote-share distribution around the threshold and plot these bins within twice the optimal bandwidth, calculated based on Calonico *et al.* (2014). The solid lines implement a linear polynomial on the unbinned data on either side of the discontinuity. The running variable is normalised to zero at the threshold by subtracting 1/6, the forfeiture threshold, from the candidates' vote share. We also invert this so that candidates on the right-hand side are treated as having lost their monetary deposit.

Figure 1(a) plots the recontesting probabilities in election $t + 1$ for female candidates. We see a sizeable discontinuity at the threshold, signifying that women who barely lose the deposit are around 9 pps less likely to recontest in the following year. Figure 1(b) uncovers no discontinuity for men with the polynomial fits being almost perfectly smooth around the threshold. Of course, the scatter for men is much more populated than for women due to the massive under-representation of women in the political arena in India. Similarly, we plot the likelihood of appearing in the top-3 positions in $t+1$ for females and males in Figure 1(c) and (d), respectively. The data for females are a little more sparse here compared to recontesting and it is difficult to discern a discontinuity. For males, we again see no discontinuity for appearing in top-3 positions, although the scatter is more even around the threshold.

In Online Appendix Figure A.1 we do not restrict the data to a neighbourhood of the threshold, but instead plot a global polynomial on the raw data. The discontinuity in the recontesting rates in $t+1$ persists for females and we continue to see null effects for male candidates. For the top-3 positions, the discontinuity for females is slightly more muted compared to Figure 1 and we also see a slight gap at the threshold for men. In Section 4 below, we non-parametrically estimate these RD effects using optimal econometric procedures, which will help establish whether the findings from the graphical treatment above hold. The procedures undertaken there will test the robustness of our findings to various choices of bandwidths and polynomial fit around the threshold, and also consider placebo thresholds.

3.3. *Validity of the RD Design*

The early evidence in Section 3.2 implies the possible existence of a substantial effect of deposit forfeiture on depressing recontesting rates in the subsequent election. However, in RD settings it is crucial to adequately probe the validity of the design in properly uncovering causal effects of the specific treatment. Following Lee (2008), a popular approach is to consider outcomes in election $t-1$ and check for the lack of discontinuity in conditional means around the threshold. This can provide indirect evidence about the continuity of the conditional expectation functions defined in (1), which is a crucial assumption for identification. To perform this analysis, we repeated the matching exercise outlined in Section 2 by matching candidates in election t to those in $t-1$. Online Appendix Table A.2 performs the non-parametric analysis undertaken in Section 4 below for the lagged outcomes of recontesting and appearing in top-3 positions. We uncover no discontinuity for either variable in the previous election, neither for males nor for females.

In other words, the expectations of the lagged outcomes are smooth around the threshold, and therefore our setup passes an important validity test.

However, recent work by Canay and Kamat (2018) has shown that validity checks using the mean of covariates or outcomes at $t-1$ may incorrectly lead researchers to conclude in favour of continuity around the threshold if features of the distribution of these covariates other than the mean are actually discontinuous. They designed a permutation-based test that essentially compares the empirical cumulative density function of the covariates on either side of the threshold. Their method selects a neighbourhood q around the cut-off or RD threshold and tests the hypothesis of continuity in the distribution of the covariates/lagged outcomes at the cut-off. We implement their test in Table 1. Panel A uses candidate-level lagged outcomes for recontesting, appearing in the top-3 positions and whether or not the candidate was affiliated with a major party.[26] For all these outcomes, for both female and male candidates, we estimate a large p-value, and thus cannot reject the null hypothesis of continuity of their distributions around the cut-off. Next, using the affidavit data, we employ information on candidate characteristics outside the political arena, such as their age, wealth, education and number of criminal cases filed against

[26] A major political party is defined as one that has been in the top three parties by total seat count at least once, along with winning at least 10% of the total seats in their respective state legislature.

Table 1. *RD Validity Tests.*

	(1)	(2)	(3)	(4)
	Females		Males	
Panel A: candidate-level variables				
	N_q	p-value	N_q	p-value
Recontest, $t-1$	40	1	255	.463
Top-3 positions, $t-1$	40	1	250	1
Top-2 positions, $t-1$	40	.355	248	.709
Major party, $t-1$	41	1	266	.371
Wealth, t	15	.550	60	.905
Education, t	18	.136	73	.080
No. of criminal cases, t	19	.482	75	.989
Candidate age, t	13	.532	58	.343
Panel B: constituency-level variables				
	N_q	p-value	N_q	p-value
Female turnout, $t-1$	30	.607	221	.148
Total turnout, $t-1$	30	.659	221	.429
Total females contested, $t-1$	34	.537	245	.644
Number of primary schools, $t-1$	24	.822	139	.595
Number of colleges, $t-1$	24	.568	139	.613
Literate total population, $t-1$	24	.179	139	.415
Total rural population, $t-1$	24	.110	139	.825
Number of households, $t-1$	24	.352	139	.910
Panel C: running variable s_{ict}				
	N_h	p-value	N_h	p-value
Cattaneo *et al.* (2020)	196	.8125	2,407	.7095

Notes: Panels A and B employ the methods developed in Canay and Kamat (2018). In both panels, N_q represents the number of observations in the optimally selected window, q, around the RD threshold, while columns (2) and (4) report the p-value for the test. In panel C, we use the density test developed by Cattaneo *et al.* (2020) and N_h represents the number of observations within the optimally selected bandwidth.

them; these data are available only post-2004, and therefore we have a substantially smaller sample size. Again, for all covariates, we fail to reject the null hypothesis of continuity. Panel B performs a similar exercise for constituency-level covariates, reaching similar conclusions.[27] Here the smaller sample sizes shown in Table 1 are a feature of the method devised by Canay and Kamat (2018), as their approach assumes a smaller number of effective observations around the threshold for valid inference. Indeed, in their own re-implementation of Lee (2008) their test selects neighbourhoods around the threshold that are ten times smaller than those given by the optimal bandwidth based on Calonico *et al.* (2014). The difference between the effective number of observations in our analysis here and the main analysis in Section 4 below is also of a similar magnitude.

Another popular way to probe the validity of RD designs is through measuring a discontinuity in the density of the running variable at the threshold. If individuals have control over choosing which side of the threshold they can lie on, then this can invalidate the RD setup, often termed as a manipulated design (Gerard *et al.*, 2020). In essence this can be thought of as introducing a selection bias in the setup, and hence standard RD tools can fail to deliver a causal estimate. McCrary (2008) provided one of the most popular ways of evaluating the above concern in RD

[27] Constituency-level variables on the number of primary schools, colleges, total population, rural population and number of households are sourced from Central Statistics Organisation and Programme Implementation (2013) compiled by Asher *et al.* (2021).

Table 2. *Electoral Outcomes in $t+1$: By Gender.*

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
		$p = 1$			$p = 2$	$p = 1$
		h	$1/2 \times h$	$2 \times h$	h_2	h (ctrls)
Panel A: females						
Recontesting, $t+1$	$h = 0.053$	-0.133^{**}	-0.167^{**}	-0.092^{**}	-0.147^{**}	-0.156^{***}
	$N_h = 695$	(0.055)	(0.077)	(0.040)	(0.061)	(0.057)
	Ctrl = 0.197	[0.024]	[0.004]	[0.011]	[0.031]	[0.009]
Top-3 positions, $t+1$	$h = 0.044$	-0.082^{*}	-0.125^{*}	-0.043	-0.100^{*}	-0.102^{**}
	$N_h = 574$	(0.049)	(0.068)	(0.038)	(0.059)	(0.052)
	Ctrl = 0.132	[0.100]	[0.011]	[0.128]	[0.091]	[0.048]
Top-2 positions, $t+1$	$h = 0.038$	-0.066^{*}	-0.064	-0.030	-0.081^{*}	-0.070^{*}
	$N_h = 498$	(0.036)	(0.050)	(0.031)	(0.043)	(0.037)
	Ctrl = 0.085	[0.062]	[0.178]	[0.083]	[0.073]	[0.057]
Winning, $t+1$	$h = 0.037$	-0.056^{*}	-0.081	-0.016	-0.074^{*}	-0.063^{*}
	$N_h = 485$	(0.034)	(0.050)	(0.025)	(0.043)	(0.037)
	Ctrl = 0.051	[0.100]	[0.194]	[0.212]	[0.072]	[0.077]
Panel B: males						
Recontesting, $t+1$	$h = 0.053$	0.011	0.016	0.003	0.017	0.009
	$N_h = 14{,}535$	(0.015)	(0.022)	(0.011)	(0.019)	(0.015)
	Ctrl = 0.237	[0.416]	[0.446]	[0.580]	[0.311]	[0.465]
Top-3 positions, $t+1$	$h = 0.078$	-0.001	0.014	-0.011	0.020	-0.001
	$N_h = 22{,}069$	(0.011)	(0.016)	(0.008)	(0.019)	(0.011)
	Ctrl = 0.194	[0.920]	[0.382]	[0.816]	[0.227]	[0.919]
Top-2 positions, $t+1$	$h = 0.071$	-0.000	0.007	-0.008	0.007	-0.000
	$N_h = 19{,}963$	(0.009)	(0.014)	(0.007)	(0.013)	(0.010)
	Ctrl = 0.129	[0.805]	[0.597]	[0.843]	[0.587]	[0.836]
Winning, $t+1$	$h = 0.074$	-0.009	-0.002	-0.009^{**}	-0.004	-0.009
	$N_h = 20{,}799$	(0.007)	(0.010)	(0.005)	(0.010)	(0.007)
	Ctrl = 0.066	[0.284]	[0.698]	[0.184]	[0.799]	[0.274]

Notes: ***, **, * Significance at the 1%, 5% and 10% levels, respectively. Conventional SEs are reported in parentheses, while the bias-corrected robust p-values are reported in square brackets. Here p represents the degree of the polynomial used, while h represents the optimal bandwidth, and N_h represents the number of effective observations within the bandwidth. Ctrl signifies the mean of the dependent variable among the control group, i.e., close deposit retainers. We employ the estimation methods outlined in Cattaneo *et al.* (2019).

designs, but used binned data for estimation of density functions on both sides of the cutoff, which can be problematic at times. A recent density test by Cattaneo *et al.* (2020) improves on these shortcomings and delivers better power and size properties as well. The first row in panel C of Table 1 provides an implementation of this test for both males and females where the null hypothesis is the continuity of density at the RD threshold. We estimate large p-values, and hence fail to reject the null.

4. Main Results

4.1. *Baseline Findings*

In this section we present our baseline findings. We do this by employing non-parametric local polynomial estimates to calculate treatment effects of losing the monetary deposit for both female and male candidates. We start by considering the entire sample period, i.e., 1977–2019. Table 2, panel A begins with female candidates, with columns (1) to (4) employing a local linear estimator and a triangular kernel. Column (1) reports the optimal bandwidth as per Calonico *et al.* (2014) and covers 695 effective observations for the estimation of the treatment effect. In column (2), we estimate that women who barely lose their deposit are around 13.3 pps less likely to recontest in the subsequent election. Cattaneo *et al.* (2019) pointed out that due to local polynomial estimation and the first step of selecting an optimal bandwidth, standard inference procedures might be invalid. They provided robust bias-corrected confidence intervals and *p*-values that take care of these concerns. In Table 2, these *p*-values are reported in square brackets, with our baseline estimation in column (2) having a bias-corrected *p*-value of .024. Additionally, we also report the control group mean or the baseline recontesting probability among individuals who barely retained the deposit. This provides a relative effect size of the estimate in column (2) of around 68% (0.133/0.196), i.e., the deposit rule substantially depresses treated female candidates' recontesting rates.

Crucially, our set-up allows us to continuously contrast our findings across gender to examine the effect of the deposit rule. In panel B of Table 2, we present our findings for male candidates. The estimated effects are close to zero and statistically insignificant.[28] Moreover, male candidates within the control group are around 4 pps or 20% more likely to recontest in the next election compared to females. In other words, over and above the election result, the deposit rule adds a further layer of deterrence, depressing female candidates' recontesting probabilities. Thus, contrasting results across gender provides a crucial ingredient to our setup, helping us explore whether the deposit rule has an inherent dissuasion effect, regardless of gender, or whether the potential gendered interaction of the social and political setup with the deposit rule may result in the negative findings presented above.

Columns (3) to (5) of Table 2 provide some standard robustness checks for our preferred specification, reported in column (2). In columns (3) and (4) we halve and double the optimal bandwidth, respectively. Since larger bandwidths induce a higher bias, but allow for higher precision, this allows us to study the variability of our findings, depending on the choice of bandwidth. Our point estimate for female candidates is slightly higher in column (3), 16.7 pps, and slightly lower in column (4), 9.2 pps, but remains of similar magnitude as our baseline. Online Appendix Figure A.2(a) presents different point estimates by varying bandwidth between a factor of 0.5 and 2 of the optimal bandwidth in increments of 0.1. We uncover a very stable pattern for the treatment effect for recontesting in $t+1$ as we vary the bandwidth size. Importantly, when it comes to male candidates, we continue to estimate a null effect throughout this range. The point estimates for top-3 positions in Online Appendix Figure A.2(b) are understandably more noisy, but are consistently larger in magnitude for female than male candidates. In column (6), we re-estimate column (2), i.e., the local linear specification with optimal bandwidth, but add a set of controls in an attempt to increase the precision of our findings.[29] We now estimate slightly higher point estimates, with both recontesting and appearing in top-3 positions being statistically significant—the former now at the 1% level and the latter at the 5% level.

Finally, column (5) of Table 2 repeats the analysis with a quadratic polynomial and we recalculate the optimal bandwidth (now represented by h_2) here. The point estimate is again very

[28] We also conduct formal tests for equality of coefficients across gender. For column (1), the *p*-value for the test for recontesting is .032, and for top-2 positions, it is .075, but we fail to reject the equality of coefficients for the other outcomes. However, when we consider the slightly more precise estimates in column (6), the *p*-value for tests of equality of coefficients is .013 for recontesting, .06 for top-3 positions, and .07 for top-2 positions. Overall, some of the lack of significance here is because of statistical imprecision owing to the sheer under-representation of women in politics that leads to very small sample sizes for the female sample.

[29] We are grateful to an anonymous referee for pointing this out. We control for whether the constituency the candidate contested from belonged to the SC/ST category, had elected a female candidate before and whether the candidate contested as an independent or belonged to a major political party.

similar to the baseline, with a forfeiture effect of 14.7 pps on the likelihood to recontest. However, the robust p-value in square brackets is slightly higher than the local linear specifications.

Beyond the recontesting probability, we also examine the likelihood of attaining a top-3 position or top-2 position in elections in period $t + 1$ for candidates at the margin of forfeiture, as well as whether they win the elections. These outcomes are all unconditional on recontesting in $t + 1$ to avoid any bias that may be introduced by who decides to recontest. In other words, these outcomes take the value 1 if the condition is true for the relevant outcome, and 0 for all other candidates irrespective of whether they recontest or not in the next election.

We estimate that female forfeiters are around 8.2 pps less likely to appear among the top-3 candidates in the next election relative to those who just retained the deposit. This amounts again to a relative effect size of over 60%, given the mean of the dependent variable among the control group of close deposit retainers. On the other hand, the estimated effect for gaining a top-2 position or winning falls in magnitude (though not in relative terms) and is statistically significant at the 10% level. This is not surprising since it is difficult to envision candidates on the verge of losing the deposit in period t surging to the very top in period $t + 1$. Instead, one can anticipate a more gradual path as candidates gain experience over multiple elections, eventually targeting the top positions. In other words, forfeiting the deposit can deprive inexperienced female candidates of the chances of this upward trajectory. This is also evident from the work of Anagol and Fujiwara (2016), which shows that even coming second in an election can have large future gains of eventually rising to the top and winning the constituency.[30] Overall, our baseline findings imply that the forfeiture rule may perpetuate gender differences in political representation. We explore this further in Section 5.1 below where we undertake a heterogeneity analysis by the prevalence of regressive gender norms.

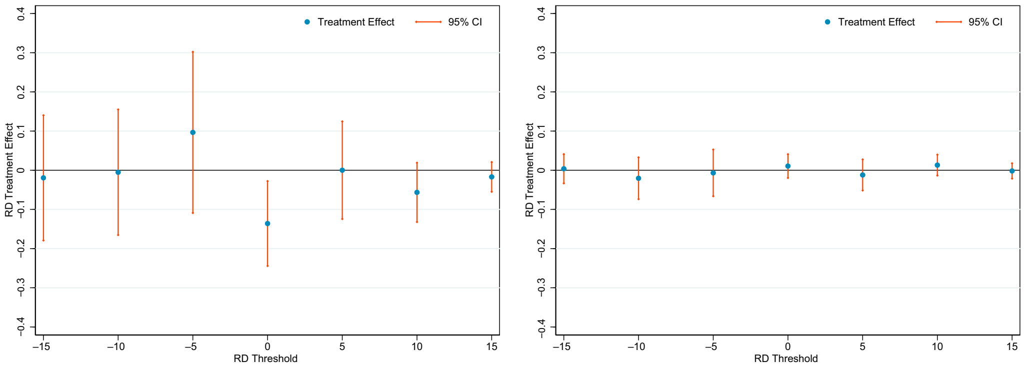
4.2. *A Few Robustness Exercises*

In this subsection, we present a few robustness exercises to probe the baseline findings presented above. While the analysis in Section 4.1 causally estimates the impact of forfeiture on future electoral outcomes within gender, one may be worried that potential differences in political or demographic characteristics of candidates at the threshold, across gender, may partially explain these findings. To explore this concern, following Wasserman (2023), we estimate a parametric RD design on the combined sample of men and women and interact the threshold with various salient measures of a candidate's political background.[31] Online Appendix Table A.3 presents the results from this exercise. Column (1) reproduces the baseline result for the combined sample and yields an estimate of 17.7 pps, which is broadly comparable to the 13.3 pps obtained from the non-parametric approach used in Table 2.[32] From columns (2)–(4), we interact with whether the candidate belongs to a scheduled caste or tribe, whether they were fielded by a major political party and whether they are contesting from a constituency that has had a previous female incumbent, respectively. In all three specifications, our main effect survives even after the addition of these

[30] On the other hand, it could be the case that similar rank-based mechanisms could actually be behind the effect of forfeiture itself. Using rank in the current election as the outcome, which varies between 1 and 35, we estimate a small and statistically insignificant point estimate of -0.119 (robust p-value = .43) for female candidates, allaying concern that rank differentials between close forfeiters and retainers may be partially driving our findings.

[31] We thank two anonymous referees for highlighting the importance of such a robustness exercise.

[32] This also establishes that the 'difference of differences' (i.e., the effect of forfeiture across genders) is also statistically significantly different from zero. This is not surprising since we estimated a precise null effect for male candidates in Table 2 and a strong robust effect for female candidates.



(a) Females **(b)** Males

Fig. 2. *Placebo Discontinuities for (a) Females and (b) Males—I.*

interactions and we continue to conclude that forfeiture of the deposit strongly diminishes the likelihood of recontesting for female candidates.

Ideally, we would have liked to conduct the above analysis with various demographic variables of the candidates as well. However, the Election Commission of India only started collecting these data for elections conducted after 2004, which leaves a very small sample size, particularly for female candidates. Nevertheless, here we present some descriptive statistics for three key variables comparing men and women at the threshold for the post-2004 sample. Male candidates (48 years of age) within the bandwidth are around 3 years older than female candidates (45 years), have slightly higher education (around 0.8 years) and have almost identical wealth compared to women. These minor differences in characteristics are unlikely to have a large enough impact on recontesting rates such that they contribute in a significant manner to explaining our findings. Therefore, to understand which underlying factors may be more likely to explain the phenomenon that we document here, we have to dig a bit deeper to offer some potential mechanisms behind our main effects—an exercise we turn to in Section 5.

Next, in Figure 2 we run our baseline analysis again, but for recontesting in $t+1$ at placebo forfeiture thresholds. We estimate the 'treatment effect' at placebo cut-offs in increments of 5 pps on either side of zero. Panel (a) reproduces our baseline findings at zero, but estimates null effects at all other thresholds on either side. Similarly, for males, the 95% confidence interval contains zero across all seven specifications. This underscores that indeed something salient is happening at the forfeiture cut-off, which is unlikely to be a spurious effect due to some unobservable underlying mechanism. A similar argument can be made for using rounded-off thresholds, such as 10%, 20%, 25% (and so on), which may induce psychological effects akin to our actual cut-off. Online Appendix Figure A.3 repeats the above exercise for such potentially focal thresholds. Again, we uncover null effects for all cut-offs for both genders and find a sizeable, statistically significant effect only for females at the true threshold for deposit loss.

Since our data span a large number of elections across years and states, we explore whether our results can be driven by either some unique election years or a particular state. In Online Appendix Figure A.4(a) we drop one state at a time from our estimation sample and re-estimate the baseline RD effect for both genders. The horizontal axis reports the dropped state. Results are remarkably stable for females at around 15 pps and are consistently close to zero, and statistically insignificant for males. In Online Appendix Figure A.4(b) we repeat the same exercise by dropping one election year at a time.[33] Our estimated RD effect is again strong and statistically significant for females, but delivers a null effect for males.

Finally, we also conducted two other exercises to explore if markers of electoral competitiveness could partially explain our findings as opposed to gender itself. First, we hand-coded candidate names by religion (Muslim and non-Muslim) and repeated our analysis similar to the male-female dichotomy in the main analysis. The null hypothesis here is that if the lack of competitiveness of minority candidates becomes more salient in the face of forfeiture then we should see similar negative findings for Muslim candidates who lose the deposit as we see for females in our main analysis. However, as Online Appendix Table A.4 shows, we find a positive impact of deposit forfeiture for Muslim candidates on the likelihood of future recontesting. Similarly, we find no impact for non-Muslim candidates either, underscoring that the religious affiliation of the candidate does not play a salient role in our setup.

Second, we repeat our baseline analysis for recontesting by SC/ST and general constituencies for both men and women; Online Appendix Table A.5 presents these findings. We find a 13-pp reduction in recontesting for women among general seats, as well as a 13-pp reduction for SC/ST candidates, albeit the latter is imprecisely estimated due to a much smaller sample size for these reserved constituencies. If SC/ST candidates had much more trouble raising funding then we would have expected a higher point estimate for this subsample. A possible explanation for this reduction could be that the reservation ameliorates the discouragement effect on female candidates, although empirically establishing this is hard based on the available sources of variation.[34]

4.3. *The Nature of the Deposit: Some Interpretation Concerns*

In this section we scrutinize two potential concerns that can plague the interpretation of our main results. First, as documented earlier, the amount of the deposit varied over the years and this can have consequences in terms of heterogeneity in the treatment effect across time. Second, as the deposit is monetary, it may potentially induce a mechanical effect owing to harsher financial constraints for women in India (Heath and Tan, 2020). While this is not particularly problematic in terms of interpretation, in Section 4.3.2 we show that it is unlikely to be the driving force. The small monetary amount of the deposit is a nominal fee at best and, therefore, other mechanisms need to be explored, which we do in Section 5.

4.3.1. *The deposit over time*

Indian lawmakers have changed the amount of the electoral deposit twice since its introduction. These changes determine three distinct deposit regimes within our sample period: (1) 1977–91, when the deposit was set at INR 250; (2) 1996–2007, when it was multiplied by a factor of 20 and raised to INR 5,000; (3) 2009–14, when it doubled, increasing to INR 10,000. In terms of

[33] This plot has some gaps as state elections follow a staggered schedule and might not fall every year. In addition, on account of the delimitation in 2004, we lose data points as the pre-delimitation sample size ends in 2004 and the post-delimitation starts in 2007. As mentioned earlier, in our main sample we pool all election years together to increase statistical power.

[34] It could also be the case that, given the smaller monetary deposit the SC/ST candidates have to pay, the effect of forfeiture may end up being more muted. However, as we argue in the next section, it seems unlikely that the monetary amount of the deposit is a key lever since it amounts to a fairly nominal share of their average candidate-level campaign expenditure.

Table 3. *Heterogeneity Analysis for Candidacy in $t+1$ by Security Deposit Changes.*

	(1)	(2)	(3)	(4)	(5)	(6)
	1977–91		1996–2007		2009–14	
Female recontesting, $t+1$	$h=0.083$	-0.145	$h=0.055$	-0.145	$h=0.048$	-0.182^{*}
	$N_h=344$	(0.080)	$N_h=143$	(0.095)	$N_h=204$	(0.099)
	Ctrl = 0.215	[0.113]	Ctrl = 0.112	[0.104]	Ctrl = 0.195	[0.066]
Male recontesting, $t+1$	$h=0.072$	0.010	$h=0.054$	-0.014	$h=0.047$	0.008
	$N_h=10{,}648$	(0.018)	$N_h=2{,}511$	(0.028)	$N_h=2{,}031$	(0.041)
	Ctrl = 0.257	[0.447]	Ctrl = 0.124	[0.583]	Ctrl = 0.275	[0.778]

Notes: * Significance at the 10% level. Conventional SEs are reported in parentheses and bias-corrected robust p-values are reported in square brackets. Here h represents the optimal bandwidth, and N_h represents the number of effective observations within the bandwidth. Ctrl signifies the mean of the dependent variable among the control group, i.e., close deposit retainers. We employ the estimation methods outlined in Cattaneo *et al.* (2019).

purchasing power parity at current prices this is equivalent to \$77 in 1977 and \$41 in 1991, at the start and end of the first regime, respectively; \$596 in 1996 and \$425 in 2007, at the start and end of the second regime; \$748 and \$544 in 2009 and 2014, at the start and end of the third regime. We take advantage of these changes to probe our baseline results, bearing in mind that, as we are splitting the sample into smaller subsamples, statistical power may become a concern.

Table 3 reports separate estimates, by gender, for the three subsamples. We start by dropping all t and $t+1$ election pairs whenever they lie in different deposit regimes. Excluding these transition year elections, for all three regimes, we estimate negative point estimates for female candidacy in $t+1$. Effects persist across regimes with comparable sizes: 14.5 pps in the first, 14.5 pps in the second and 18.2 pps in the third. Nonetheless, only the effect in the third regime is statistically significant at the 10% level. Estimates for male candidates, on the contrary, remain persistently close to zero across all three regimes. Overall, the deterrence effect uncovered in Section 4.1 holds across all three deposit regimes and is similar in size as well. The lack of responsiveness of the strength of our findings to the amount of the deposit suggests that the size of the deposit is not a salient mechanism behind the deterrence effect on female candidates. This would especially be true if the deposit is a nominal expense for the candidates. In the next subsection, we provide some institutional background and back-of-the-envelope calculations to probe whether this is a likely conjecture.

4.3.2. *The monetary nature of the deposit: a purely nominal fee?*

It is well documented that women in India have limited financial autonomy (Khalil and Mookerjee, 2019; Heath and Tan, 2020). This raises the question of whether the gender difference we observe is purely a mechanical effect of the existence of a monetary barrier. We argue that this is unlikely to be a plausible explanation, given the substantial sums of money required to mount a reasonable election campaign in Indian elections.[35] Although campaign finance laws put theoretical limits on the total expenditure a candidate can incur, it has been typically argued that these ceilings are too low and that the actual expenditures are substantially higher (CMS, 2019). Nevertheless, even if we work with these theoretical limits, the deposit fee was a mere 0.5% of these limits pre-1996, 0.7% between 1997 and 2003 (as the ceilings were raised) and fell again to 0.4% towards the

[35] Although the candidates we focus on are at the lower end of the vote-share distribution, they are still able to garner around 16% of the vote share, amounting to roughly 20,000 votes on average. This is no mean feat and would require an adequately run election campaign.

end of our sample period.[36] These figures clearly show that the deposit is only a nominal fee in comparison to candidates' election expenditures.

Furthermore, these theoretical limits are almost certainly a lower bound on actual campaign expenditure. It is difficult to have an accurate measure of exact campaign expenditure due to measurement issues and lack of data. There is some survey evidence that in the 2017 Uttar Pradesh State election the average expenditure per voter amounted to INR 700 (PTI, 2017). In our data, candidates at the margin of the forfeiture threshold garner around 20,000 votes on average; even if we halve the cost per voter, this would amount to around 7 million INR. The deposit fee is only 0.14% of this amount. One concern can be that campaign expenditure is skewed towards the top two candidates and candidates in our sample are actually spending much less. While we do not have data for the entire distribution of candidates across states and years, the Election Commission of Tamil Nadu released handwritten scans of election expenditure data for the 2021 election. We hand-collected these data, and the average expenditure of candidates that lie within the optimal bandwidth around the one-sixth vote- share threshold is around 1.4 million INR. In addition, illegal funding ('black money') on top of the admissible amounts is widespread and the issue, raised since the 1960s, is very well documented (see Gowda and Sridharan, 2012). Moreover, corruption is widespread and there are allegations of candidates directly buying votes by handing out cash or in-kind payout to voters. Survey evidence from 2007 to 2008 concluded that between 20% (in Jharkhand) to 94% (Andhra Pradesh) of low-income voters were offered money by electoral candidates, across all states, varying from 500 to 5,000 INR per vote (CMS, 2019).[37]

Overall, we conclude that the election deposit is incontrovertibly a nominal fee, which represents just the tip of the iceberg of a typical candidate's expenditure. In light of this, the null effect we observe among males is the most rational response one could expect following the forfeiture of the deposit. This raises the question: why, instead, do we observe a substantial, significant and persistent negative effect among women who forfeit the deposit?

5. Mechanisms and Long-Term Effects

5.1. *Heterogeneity Analysis*

So far, we have established a strong and persistent negative effect of losing the electoral deposit on females' future candidacy, but null effects for males. Although the RD design and, more generally, the causal inference toolkit are instrumental in estimating first-degree causal effects, it is difficult to empirically study the mechanisms that drive these effects with non-experimental data. The literature across identification strategies resorts to providing some suggestive evidence of potential mechanisms by conducting heterogeneity analysis along interesting pre-existing dimensions. In this section, we conduct such subsample analysis across margins of potential interest.

[36] The Representation of the People Act of 1951, Section 77(3), sets limits on the amount a candidate can spend on election campaigns, with the intent of levelling the playing field. Candidates who exceed these limits can theoretically be disqualified and, if elected, their election is annulled. In 1996 the limit for Vidhan Sabha, which had been kept for a long time at the artificially low amount of INR 50,000, was raised to INR 150,000 for most states. It was further increased to INR 700,000 in 1997 and to INR 1,000,000 in 2003. After becoming INR 1,600,000 in 2011, it was increased to INR 2,800,000 and, finally, to INR 3,060,000 in 2020.

[37] The actual expenditure on a given electoral campaign could be even higher, given the role that political parties play in the Indian electoral landscape. Parties habitually spend considerable amounts on candidates above and beyond the ceiling imposed by the law (see Gowda and Sridharan, 2012).

5.1.1. *By gender norms*

In light of the across-gender differential in our estimates, one natural explanation points to India's cultural setup, which is characterised by substantially regressive gender norms (Jayachandran, 2015). Women face barriers to decision-making autonomy and mobility even within households (Khalil and Mookerjee, 2019), as well as in the labour market (Field *et al.*, 2010; Klasen and Pieters, 2015), in addition to early marriage and childbearing being very common. Thus, it is reasonable to expect that our findings can, partly, be driven by areas with strongly regressive gender norms. Previous research has employed the sex ratio as a proxy for gender norms across regions in India (Vogl, 2013; Jayachandran and Pande, 2017; Bhalotra *et al.*, 2018).

In panel A of Table 4, we start by splitting our sample around the median sex ratio, defined as the share of the population that is female in the state the constituency is located in. There is substantial variation across this dimension, with the northern state of Haryana having around 879 women per 1,000 men, while the southern state of Kerala has 1,084 women per 1,000 men. The estimated effect is concentrated in states with below-median sex ratio, around −19 pps, despite a slightly higher likelihood of recontesting in the control group. This amounts to a relative effect size of around 90%.[38] On the other hand, we estimate a statistically insignificant reduction of 4.9 pps for women in more progressive states; owing to the small sample sizes, these estimates are not statistically significantly different.

Similarly, women in these below-median sex ratio states are close to 22 pps less likely to appear in the top-3 positions in the next elections; in contrast, in above-median states, the estimate is close to zero and statistically insignificant (p-value for equality of coefficients = .062). Finally, we also uncover a statistically significant reduction in the likelihood of winning as well with a point estimate of −11.8 compared to virtually zero for above-median states.[39] This indeed underscores that forfeiture does reduce female political representation in subsequent years, particularly in states where this matters the most. Our findings also corroborate previous work by Booth *et al.* (2019) documenting that gender norms play a key role in explaining the gender gap in competitiveness.

While the RD approach ensures that close forfeiters and retainers are on average similar, this is unlikely to hold for those who choose to recontest in the subsequent election. Therefore, as mentioned in Section 4.2, the results in the preceding paragraph are from specifications that do not condition on recontesting. However, this also implies that we cannot directly separate the pure effect of forfeiture on appearing in the top-3/top-2 positions or winning from the effect stemming purely from close-forfeiters not recontesting. By considering the direction of bias that may be induced by selection into candidacy, we can sharpen the interpretation of the above findings. Since candidates are likely to take their probability of winning into account when deciding whether to recontest, they are likely positively selected in terms of political ambition, future viability and other electoral-performance-enhancing attributes. This selection is also likely to be stronger for those who forfeited and chose to recontest than those who retained, as the former received a negative signal about their future chances due to forfeiture.[40] Therefore, the negative effect of forfeiture on, for example, winning is likely to be even larger than the 11.8 pps reported in Table 4 for those who chose to recontest. Similar conclusions would hold for appearing in

[38] We also split the sample between the bottom quartile and the top three quartiles of the sex ratio to further probe this angle. We uncover an even higher point estimate for women, but no statistically discernible effect for men.

[39] Although we fail to reject the hypothesis of the equality of these coefficients statistically.

[40] A crucial point to note here is that the forfeiture threshold in our setting is close to 17%, and thus these candidates are by no means lemons, and have a fair chance of being competitive in subsequent electoral rounds.

Table 4. *Heterogeneity Analysis for Electoral Outcomes in $t+1$—Regressive Gender Norms.*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Females				Males			
	Below median		Above median		Below median		Above median	
Panel A: sex ratio								
Recontesting, $t+1$	$h = 0.058$	-0.186^{**}	$h = 0.048$	-0.049	$h = 0.089$	-0.001	$h = 0.051$	0.009
	$N_h = 437$	(0.080)	$N_h = 276$	(0.073)	$N_h = 12{,}776$	(0.017)	$N_h = 7{,}162$	(0.020)
	Ctrl = 0.208	[0.022]	Ctrl = 0.181	[0.679]	Ctrl = 0.273	[0.864]	Ctrl = 0.217	[0.691]
Top-3 positions, $t+1$	$h = 0.038$	-0.219^{**}	$h = 0.044$	0.016	$h = 0.062$	0.010	$h = 0.076$	-0.004
	$N_h = 279$	(0.089)	$N_h = 256$	(0.063)	$N_h = 8{,}463$	(0.019)	$N_h = 10{,}867$	(0.015)
	Ctrl = 0.134	[0.013]	Ctrl = 0.130	[0.647]	Ctrl = 0.197	[0.461]	Ctrl = 0.183	[0.771]
Top-2 positions, $t+1$	$h = 0.036$	-0.155^{**}	$h = 0.044$	0.005	$h = 0.071$	-0.001	$h = 0.059$	0.003
	$N_h = 259$	(0.071)	$N_h = 257$	(0.040)	$N_h = 9{,}778$	(0.014)	$N_h = 8{,}253$	(0.014)
	Ctrl = 0.092	[0.028]	Ctrl = 0.089	[0.864]	Ctrl = 0.140	[0.804]	Ctrl = 0.114	[0.996]
Winning, $t+1$	$h = 0.039$	-0.118^{*}	$h = 0.044$	0.010	$h = 0.085$	-0.015	$h = 0.071$	-0.004
	$N_h = 291$	(0.063)	$N_h = 254$	(0.029)	$N_h = 12{,}044$	(0.009)	$N_h = 10{,}134$	(0.009)
	Ctrl = 0.061	[0.067]	Ctrl = 0.050	[0.759]	Ctrl = 0.076	[0.211]	Ctrl = 0.058	[0.643]
Panel B: females rejected								
Recontesting, $t+1$	$h = 0.073$	-0.128	$h = 0.081$	-0.150	$h = 0.086$	0.020	$h = 0.053$	0.012
	$N_h = 329$	(0.080)	$N_h = 186$	(0.100)	$N_h = 9{,}582$	(0.019)	$N_h = 3{,}031$	(0.030)
	Ctrl = 0.215	[0.154]	Ctrl = 0.165	[0.145]	Ctrl = 0.240	[0.274]	Ctrl = 0.195	[0.669]
Top-3 positions, $t+1$	$h = 0.044$	-0.106	$h = 0.078$	-0.082	$h = 0.082$	0.006	$h = 0.070$	0.017
	$N_h = 186$	(0.065)	$N_h = 183$	(0.093)	$N_h = 9{,}011$	(0.017)	$N_h = 4{,}103$	(0.023)
	Ctrl = 0.138	[0.114]	Ctrl = 0.099	[0.316]	Ctrl = 0.184	[0.534]	Ctrl = 0.153	[0.464]
Top-2 positions, $t+1$	$h = 0.041$	-0.053	$h = 0.068$	-0.133	$h = 0.065$	0.012	$h = 0.064$	0.017
	$N_h = 175$	(0.034)	$N_h = 161$	(0.084)	$N_h = 6{,}907$	(0.016)	$N_h = 3{,}718$	(0.020)
	Ctrl = 0.077	[0.118]	Ctrl = 0.103	[0.114]	Ctrl = 0.118	[0.390]	Ctrl = 0.104	[0.454]
Winning positions, $t+1$	$h = 0.034$	-0.037	$h = 0.060$	-0.108	$h = 0.053$	0.004	$h = 0.065$	0.013
	$N_h = 135$	(0.030)	$N_h = 141$	(0.080)	$N_h = 5{,}665$	(0.012)	$N_h = 3{,}852$	(0.015)
	Ctrl = 0.009	[0.205]	Ctrl = 0.082	[0.203]	Ctrl = 0.057	[0.666]	Ctrl = 0.049	[0.410]

Notes: **, * Significance at the 5% and 10% levels, respectively. Conventional SEs are reported in parentheses and the bias-corrected robust p-values are reported in square brackets. Here p represents the degree of the polynomial used, h represents the optimal bandwidth and N_h represents the number of effective observations within the bandwidth. Ctrl signifies the mean of the dependent variable among the control group, i.e., close deposit retainers. We employ the estimation methods outlined in Cattaneo *et al.* (2019).

the top-3 or top-2 positions as well. Finally, we also implement the approach of Anagol and Fujiwara (2016) to provide empirical bounds on these findings, which is an extension of the widely used method by Lee (2009). This requires three estimable entities (for details, see Anagol and Fujiwara, 2016): the recontesting probability of close-retainers; the RD effect on recontesting and on the outcome for which we want bounds, e.g., winning; and the unobservable counterfactual likelihood of winning for those close-forfeiters that did not recontest. We calculate the upper bound for winning by plugging in zero for this latter term and the lower bound by replacing it with the winning probability of close-retainers who recontested. The bounds thus calculated are [−0.21, −0.54] for winning, [−0.34, −0.72] for top-2 positions and [−0.59, −1.09] for top-3 positions.[41]

In panel B of Table 4, we explore a different way of measuring regressive gender norms. Our data allow us to observe the total number of candidates who submitted their nomination papers to the local election committee, as well as the number of candidates whose nomination papers were rejected by election officials. These rejections can arise from incorrect filling of forms or other violations of the electoral protocol. However, this is ultimately the local officer's subjective decision and one can expect that places with more regressive gender norms, i.e., those more prone to discrimination against women, might see higher rates of rejected applications among female candidates. This measure also allows us to construct a proxy of gender norms at the much finer level of an electoral constituency rather than the state, which can help capture variation in gender norms within states. Although the findings are less precise, constituencies with above-median rejection rates show point estimates of 15 pps, equivalent to around a 91% reduction in recontesting for the treated group compared to the control. The point estimate is also negative for the below-median sample, but the relative effect is only 59%. This evidence supports the findings from panel A of Table 4, that is, regions where regressive gender norms are more ingrained are associated with lower female candidate persistence. This highlights the role that culture and social pressure play in women's decision to run again for election. Once again, we uncover estimated effects close to zero for male candidates across specifications.

5.1.2. *By existence of female role models*

A broad literature documents the potentially beneficial effect of role models in fostering female participation across various fields. Previous studies have shown the existence of such effects in arenas as diverse as college major choice (Porter and Serra, 2020), participation in village council elections (Beaman *et al.*, 2009), entrepreneurship and business activity (Field *et al.*, 2016), among others. Panel A of Table 5 explores the existence of such a channel for our findings by splitting the sample into whether or not the candidate's constituency has ever had a female incumbent. Columns (2) and (4) report negative effects of −10 percentage points (significant at the 10% level) and −23 percentage points (significant at the 5% level), respectively. In relative terms these translate to effect sizes of 71% and 87%. These findings do not support the existence of a role-model effect; instead, the presence of a female incumbent is associated with an even larger and statistically significant negative effect on the likelihood of recontesting.

In panel B, we explore whether women candidates at the margin of the threshold are more likely to be encouraged by the presence of better-performing female peers in the same constituency.

[41] It is important to note that these bounds are large because of the extremely stringent assumptions we make regarding the unobservable likelihood of winning for close-forfeiters who do not recontest, particularly in calculating the upper bound. For instance, if we assume that these individuals have a winning likelihood equal to the midpoint between 0 and the winning probability of close retainers, then the bounds will be mechanically tighter.

Table 5. *Heterogeneity Analysis for Electoral Outcomes in $t+1$—Role-Model Effects.*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Females				Males			
	No		Yes		No		Yes	
Panel A: female incumbents								
Recontesting, $t+1$	$h=0.071$	-0.099^{*}	$h=0.045$	-0.230^{**}	$h=0.052$	-0.003	$h=0.075$	0.043
	$N_h=549$	(0.059)	$N_h=242$	(0.100)	$N_h=10{,}863$	(0.018)	$N_h=5{,}047$	(0.026)
	Ctrl = 0.138	[0.091]	Ctrl = 0.263	[0.024]	Ctrl = 0.244	[0.991]	Ctrl = 0.224	[0.114]
Top-3 positions, $t+1$	$h=0.064$	-0.023	$h=0.037$	-0.202^{**}	$h=0.070$	-0.008	$h=0.056$	0.037
	$N_h=487$	(0.053)	$N_h=198$	(0.088)	$N_h=14{,}803$	(0.014)	$N_h=3{,}640$	(0.027)
	Ctrl = 0.083	[0.640]	Ctrl = 0.184	[0.028]	Ctrl = 0.199	[0.735]	Ctrl = 0.156	[0.179]
Top-2 positions, $t+1$	$h=0.044$	-0.013	$h=0.036$	-0.178^{**}	$h=0.060$	-0.004	$h=0.069$	0.021
	$N_h=341$	(0.022)	$N_h=190$	(0.088)	$N_h=12{,}730$	(0.012)	$N_h=4{,}593$	(0.020)
	Ctrl = 0.033	[0.510]	Ctrl = 0.155	[0.049]	Ctrl = 0.131	[0.954]	Ctrl = 0.106	[0.386]
Winning, $t+1$	$h=0.044$	0.000	$h=0.037$	-0.159^{*}	$h=0.067$	-0.014	$h=0.053$	0.016
	$N_h=339$	(0.018)	$N_h=197$	(0.084)	$N_h=14{,}299$	(0.008)	$N_h=3{,}471$	(0.015)
	Ctrl = 0.014	[0.895]	Ctrl = 0.103	[0.072]	Ctrl = 0.071	[0.167]	Ctrl = 0.045	[0.328]
Panel B: females above								
Recontesting, $t+1$	$h=0.055$	-0.121^{**}	$h=0.059$	-0.181	$h=0.063$	0.010	$h=0.056$	-0.012
	$N_h=608$	(0.059)	$N_h=119$	(0.153)	$N_h=15{,}766$	(0.015)	$N_h=1{,}452$	(0.049)
	Ctrl = 0.181	[0.045]	Ctrl = 0.306	[0.312]	Ctrl = 0.243	[0.455]	Ctrl = 0.233	[0.854]
Top-3 positions, $t+1$	$h=0.044$	-0.082	$h=0.050$	-0.057	$h=0.054$	0.013	$h=0.053$	-0.062
	$N_h=485$	(0.055)	$N_h=100$	(0.122)	$N_h=13{,}437$	(0.014)	$N_h=1{,}368$	(0.047)
	Ctrl = 0.178	[0.123]	Ctrl = 0.300	[0.802]	Ctrl = 0.238	[0.274]	Ctrl = 0.232	[0.199]
Top-2 positions, $t+1$	$h=0.038$	-0.067^{*}	$h=0.049$	-0.039	$h=0.076$	0.006	$h=0.072$	-0.072^{**}
	$N_h=423$	(0.037)	$N_h=98$	(0.118)	$N_h=19{,}483$	(0.010)	$N_h=1{,}911$	(0.034)
	Ctrl = 0.178	[0.067]	Ctrl = 0.298	[0.878]	Ctrl = 0.248	[0.387]	Ctrl = 0.237	[0.031]
Winning, $t+1$	$h=0.039$	-0.049	$h=0.046$	-0.045	$h=0.069$	-0.005	$h=0.049$	-0.063^{**}
	$N_h=428$	(0.035)	$N_h=91$	(0.100)	$N_h=17{,}318$	(0.007)	$N_h=1{,}252$	(0.031)
	Ctrl = 0.177	[0.150]	Ctrl = 0.297	[0.797]	Ctrl = 0.246	[0.703]	Ctrl = 0.223	[0.032]

Notes: **, * Significance at the 5% and 10% levels, respectively. Conventional SEs are reported in parentheses and the bias-corrected robust p-values are reported in square brackets. Here p represents the degree of the polynomial used, h represents the optimal bandwidth and N_h represents the number of effective observations within the bandwidth. Ctrl signifies the mean of the dependent variable among the control group, i.e., close deposit retainers. We employ the estimation methods outlined in Cattaneo *et al.* (2019).

We split the sample by whether there is a female candidate with a higher vote share than the focal woman at the cut-off. In column (2), when there are no female candidates above the treated candidate, we uncover a smaller point estimate of −0.12 for recontesting in the next election compared to −0.18 in column (4) for the sample that has other female candidates with higher vote shares in the same constituency. The corresponding relative effects are 67% when no female candidates are above and 59% when they are, reflecting a difference in magnitude across the two subsamples. The above analyses highlight that female candidates at the forfeiture margin, i.e., at a salient point among the distribution of election losers, are not necessarily swayed by the presence of better-performing female candidates. It is important to note that, in columns (2) and (4), both deposit forfeiters and retainers lie within the sample restriction in play, making the comparison for heterogeneity analysis valid.

5.2. *Electoral Outcomes over the Long Term*

So far, we have documented a strong, robust impact of forfeiting the deposit on discouraging female candidates from recontesting in the next election cycle, an effect which is particularly strong in gender-regressive states. However, it is crucial to determine whether this effect persists over multiple election cycles or merely reflects a short-term response to an electoral setback. In other words, if after a defeat male candidates simply recontest sooner than women then the above-documented difference may be trivial from a policy perspective.

To explore this issue in our context, we extend our main sample to four subsequent electoral runs, which can amount to a maximum of 20 years after we observe a candidate's forfeiture. Table 6 presents the results from this exercise, where outcome variables are defined based on whether a candidate recontests, appears in a top-2 or top-3 position or wins an election anytime within the next 20 years. For the full sample in column (1) of panel A, we estimate a 14.4-percentage-point reduction in recontesting for female candidates who forfeit, while panel B again finds a null effect for men. This result establishes that deposit forfeiture permanently, or at least over a very long time horizon, differentially discourages female candidates from recontesting again.[42] These findings are robust to a quadratic polynomial for the non-parametric RD estimator, as shown in column (2).

Next, we repeat the above exercise, but across the main lever that explains our findings: regressive gender norms. In column (3), we see that women who contest from states with a below-median sex ratio are 25 pps less likely to ever return on the ballot paper (p-value $< .01$), and 17 (p-value $< .05$) and 12.3 (p-value $< .10$) pps less likely to appear in the top-3 or top-2 candidates, respectively. We also estimate a 7.5-pp reduction in their likelihood of winning, but this is imprecisely estimated. In other words, those who barely retain the deposit, and thus end up recontesting do increase their chances of rising through the political ranks as they gain experience over their political careers. This makes sense since the forfeiture threshold is not at a trivial level of vote share: candidates who can garner over 16% of the vote share are not fringe candidates and it may be optimal for them to persist in their political careers over the long term. The analysis in this section thus helps establish that this procedural rule does indeed contribute to female political under-representation by permanently discouraging aspiring women candidates from the political arena.

[42] The average age of a female candidate in the bandwidth is around 45 years, implying that, at least until age 65, we do not observe these women recontesting, which, arguably, amounts to a permanent discouragement in their political aspirations.

Table 6. *Outcome Measures over Four Electoral Cycles: By Gender.*

		(1)	(2)		(3)	(4)		(5)	(6)
Dependent variable	Full sample			Below sex ratio			Above sex ratio		
		$p=1$	$p=2$		$p=1$	$p=2$		$p=1$	$p=2$
Panel A: females									
Ever contest	$h=0.054$	−0.144**	−0.163**	$h=0.051$	−0.251***	−0.288***	$h=0.064$	−0.058	−0.063
	$N_h=706$	(0.063)	(0.075)	$N_h=371$	(0.092)	(0.108)	$N_h=335$	(0.084)	(0.096)
	Ctrl = 0.229	[0.028]	[0.048]	Ctrl = 0.241	[0.006]	[0.009]	Ctrl = 0.215	[0.568]	[0.620]
Ever top 3	$h=0.052$	−0.052	−0.063	$h=0.043$	−0.173**	−0.236**	$h=0.067$	0.016	0.064
	$N_h=666$	(0.055)	(0.066)	$N_h=313$	(0.086)	(0.104)	$N_h=377$	(0.074)	(0.106)
	Ctrl = 0.152	[0.341]	[0.331]	Ctrl = 0.149	[0.039]	[0.018]	Ctrl = 0.158	[0.756]	[0.470]
Ever top 2	$h=0.045$	−0.044	−0.055	$h=0.042$	−0.123*	−0.145*	$h=0.053$	0.031	0.040
	$N_h=581$	(0.049)	(0.059)	$N_h=305$	(0.069)	(0.083)	$N_h=300$	(0.073)	(0.088)
	Ctrl = 0.110	[0.339]	[0.345]	Ctrl = 0.107	[0.074]	[0.065]	Ctrl = 0.115	[0.686]	[0.639]
Ever won	$h=0.041$	−0.047	−0.073*	$h=0.052$	−0.075	−0.140*	$h=0.043$	0.013	0.008
	$N_h=543$	(0.036)	(0.046)	$N_h=374$	(0.059)	(0.084)	$N_h=253$	(0.030)	(0.036)
	Ctrl = 0.070	[0.181]	[0.090]	Ctrl = 0.090	[0.230]	[0.080]	Ctrl = 0.054	[0.815]	[0.960]
Panel B: males									
Ever contest	$h=0.050$	0.009	0.012	$h=0.054$	0.007	0.009	$h=0.050$	0.011	0.013
	$N_h=13{,}490$	(0.017)	(0.020)	$N_h=7{,}305$	(0.024)	(0.027)	$N_h=6{,}985$	(0.023)	(0.027)
	Ctrl = 0.294	[0.509]	[0.506]	Ctrl = 0.327	[0.618]	[0.613]	Ctrl = 0.264	[0.664]	[0.599]
Ever top 3	$h=0.074$	−0.000	0.025	$h=0.065$	0.002	0.025	$h=0.091$	0.008	0.020
	$N_h=20{,}818$	(0.013)	(0.021)	$N_h=8{,}807$	(0.020)	(0.029)	$N_h=13{,}672$	(0.015)	(0.026)
	Ctrl = 0.236	[0.858]	[0.176]	Ctrl = 0.252	[0.692]	[0.332]	Ctrl = 0.226	[0.609]	[0.371]
Ever top 2	$h=0.067$	0.003	0.011	$h=0.072$	0.003	0.020	$h=0.056$	0.004	0.001
	$N_h=18{,}738$	(0.011)	(0.015)	$N_h=9{,}972$	(0.016)	(0.022)	$N_h=7{,}842$	(0.016)	(0.019)
	Ctrl = 0.159	[0.640]	[0.427]	Ctrl = 0.179	[0.637]	[0.313]	Ctrl = 0.136	[0.942]	[0.882]
Ever won	$h=0.074$	−0.008	−0.003	$h=0.061$	−0.006	0.001	$h=0.064$	−0.005	0.002
	$N_h=20{,}784$	(0.007)	(0.010)	$N_h=8{,}280$	(0.013)	(0.017)	$N_h=8{,}978$	(0.010)	(0.015)
	Ctrl = 0.090	[0.519]	[0.854]	Ctrl = 0.112	[0.927]	[0.870]	Ctrl = 0.067	[0.589]	[0.770]

Notes: ***, **, * Significance at the 1%, 5% and 10% levels, respectively. Conventional SEs are reported in parentheses and the bias-corrected robust p-values are reported in square brackets. Here p represents the degree of the polynomial used, h represents the optimal bandwidth and N_h represents the number of effective observations within the bandwidth. Ctrl signifies the mean of the dependent variable among the control group, i.e., close deposit retainers. We employ the estimation methods outlined in Cattaneo *et al.* (2019).

Table 7. *Outcome Measures Conditional on Recontesting Candidates in $t+1$: By Gender.*

	(1)	(2)	(3)	(4)	(5)
Dependent variable		$p=1$			$p=2$
		h	$1/2 \times h$	$2 \times h$	h
Panel A: females					
Vote share, $t+1$	$h=0.028$	−0.168	−0.454**	−0.036	−0.283**
	$N_h=51$	(0.110)	(0.224)	(0.087)	(0.122)
	Ctrl = 0.252	[0.036]	[0.015]	[0.012]	[0.016]
Different Party, $t+1$	$h=0.060$	0.403	0.198	0.320**	0.368
	$N_h=125$	(0.248)	(0.439)	(0.149)	(0.380)
	Ctrl = 0.391	[0.177]	[0.221]	[0.112]	[0.422]
Panel B: males					
Vote share, $t+1$	$h=0.058$	−0.003	−0.021	−0.003	−0.002
	$N_h=3{,}435$	(0.012)	(0.024)	(0.012)	(0.014)
	Ctrl = 0.241	[0.866]	[0.169]	[0.798]	[0.948]
Different Party, $t+1$	$h=0.073$	0.012	0.018	0.009	0.023
	$N_h=4{,}319$	(0.034)	(0.054)	(0.026)	(0.051)
	Ctrl = 0.550	[0.739]	[0.819]	[0.902]	[0.713]

Notes: ** Significance at the 5% level. Conventional SEs are reported in parentheses and the bias-corrected robust p-values are reported in square brackets. Here p represents the degree of the polynomial used, h represents the optimal bandwidth and N_h represents the number of effective observations within the bandwidth. Ctrl signifies the mean of the dependent variable among the control group, i.e., close deposit retainers. We employ the estimation methods outlined in Cattaneo *et al.* (2019).

5.3. *Other Electoral Outcomes*

In Table 7, we report results for outcomes that can only be observed in $t+1$, i.e., conditional on the candidate recontesting. As Anagol and Fujiwara (2016) pointed out, this can potentially induce a bias in the estimation, since the RD only randomises the treatment of deposit forfeiture and not the distribution of candidates who do choose to recontest in $t+1$. For instance, it could be the case that extremely driven candidates on both sides of the threshold recontest at similar rates, or it could be that this varies by treatment status. Therefore, we interpret the findings in Table 7 with caution. Focusing on women, panel A estimates a 16.8-pp reduction in the vote share in election $t+1$, compared to t, among treated candidates, with a robust p-value of .036. More importantly, point estimates for men are close to zero and vary between 0.2 to 2 pps across specifications. Because of the substantially smaller sample size of recontesting women, our estimates in panel A are particularly noisy.

To further understand the mechanism behind our main findings, we next turn to the role of political parties in candidacy.[43] We measure whether candidates are likely to be nominated by a different party in election $t+1$. The *Different Party* variable in Table 7 takes value one if candidates are nominated by a different party, i.e., either change party or move from independent status to party or vice versa. We estimate a huge effect size among female candidates: a 20–40-pp increase in changing party affiliation; we uncover no such effects for males. Because of the small

[43] Indian political parties play a paramount role in the electoral process. While the nomination of candidates to party electoral tickets varies from party to party, and across space and time, the influence of senior party members in the process is well recognised (Sircar, 2018). Generally, a candidate has to convince party leadership that they have sufficient financial resources to mount a successful campaign; their electability, using previous vote share as a measure, as well as educational background and lack of criminal history, can also play a key role (Sircar, 2018). However, it is also recognised that no one general rule summarising the candidate nomination process can adequately measure the heterogeneity on the ground.

sample size, the point estimate is insignificant for the optimal bandwidth, but is statistically significant when we double the bandwidth (column (4)). It is worth noting that most candidates in election t within the bandwidth around the threshold were affiliated with a political party: only 14 out of 127 candidates contested as independents.

5.4. *Exploring Potential Mechanisms: The Role of Culture*

In light of the insights gained from the heterogeneity analysis and the additional findings discussed above, we now turn our attention to exploring the potential mechanisms that underlie the main results, and their dynamic interplay. Although the heterogeneity analysis above may not provide direct causal evidence for culture as the main lever driving our findings, the exercise is nevertheless helpful in ascertaining where the response to forfeiture is most salient. Given the absence of an observed effect in states with more progressive gender norms, it becomes evident that culture plays a crucial role in shaping women's persistence. This finding is consistent with results from studies reporting that society influences gender differences in competitiveness. See, for instance, Gneezy *et al.* (2009), who compared a patrilineal and a matrilineal society and reported that the gender gap is reversed in the latter. Using the same comparison, Andersen *et al.* (2013) observed that girls become less competitive around puberty in the patrilineal society.[44] Indeed, Booth and Nolen (2012) found that girls from same-sex schools are as competitive as boys. Similarly, in an experiment with Israeli children Gneezy and Rustichini (2004) found that competition improves the performance for boys, but not for girls, while Dreber *et al.* (2011) and Khachatryan *et al.* (2015), using the same task as Gneezy and Rustichini (2004), reported no difference among Swedish children and Armenian children, respectively (see List *et al.*, 2023 for a review of this literature).

Beyond the direct effect of culture and social norms on women's decision to stand again for election, their persistence can be further depressed through parties and voters. Bhalotra *et al.* (2018) documented that male-dominated parties actively impede women's advancement in political careers. Their study revealed that constituencies with a female incumbent actually experience a lower number of new female political candidates in subsequent years, a phenomenon they attributed to male backlash from party leadership. If indeed parties hinder the candidacy of women following a woman's electoral success, it would not be surprising to observe the same attitude following electoral failure.[45] Moreover, if voters are similarly inclined and tend to stigmatise female failure, it may actually be rational for parties to be reluctant to field a woman who forfeited the deposit again. This would align with our findings, indicating that, conditional on running again, treated women are much more likely to be nominated by a different party and secure a lower share of the vote. Two caveats must be kept in mind. First, we have no way to verify whether women change party spontaneously or whether parties deny them another

[44] Related studies also comparing these two types of societies confirm the influence of culture on gender differences in pro-sociality (Andersen *et al.*, 2008; Gong *et al.*, 2015), risk preferences and stereotypes (Pondorfer *et al.*, 2017), willingness to assume a position of power (Banerjee *et al.*, 2015) and negotiations (Andersen *et al.*, 2018).

[45] Evidence suggests that political party competition in India tends to marginalise women from electoral participation (e.g., Rai, 2011; Ravi and Sandhu, 2014; Franceschet *et al.*, 2018; Rai and Spary, 2018; Prodip, 2021). Dominance and patriarchal attitudes within parties often contribute to the exclusion of women from the political process (Rai, 2011). Despite rhetorical commitments, parties consistently fail to allocate an adequate number of seats to women in elections (Kishwar, 1996). In addition to the Indian context, consider Casas-Arce and Saiz (2015) as well, who presented evidence indicating that the implementation of female quotas in Spanish municipal elections led to improved electoral outcomes for parties primarily impacted by these quotas. This suggests that party leaders were not optimising their electoral performance prior to the introduction of quotas.

chance. Second, we recall that these results must be taken cautiously because the distribution of candidates who decide to recontest is not randomised.

Given the intricate dynamics between candidates, parties and voters, we currently lack sufficient evidence to take a definitive stance on this matter. To study voters' attitudes in states characterised by regressive gender norms, in the next section we report the results of a survey experiment that we designed and conducted with a representative sample from Uttar Pradesh, one of the states that fall behind in terms of gender equality.

6. Survey Study

One shortcoming of studies using aggregated electoral data is that they cannot shed light on the motivations behind voters' choices. A major driving force behind our estimates can fundamentally operate at the individual candidate level either through channels explaining female disinclination to compete or more serious incorporation of negative feedback in future decision-making. Although we cannot provide direct evidence for the motivations of individual candidates we can attempt to study the role that the electorate plays in explaining our findings. To this end, we designed and conducted our own survey with a representative sample of the population of Uttar Pradesh.[46] With more than 200 million inhabitants, Uttar Pradesh is the most populous state in India. It is typically ranked among the worst Indian states in terms of gender equality. For instance, based on the latest data, in 2019 the sex ratio at birth in Uttar Pradesh was 878 girls per 1,000 boys (fifth lowest), 38% of women reported facing domestic abuse (fifth highest), female age at marriage was only 18.5 years (fifth lowest) and the female labour force participation rate was only 9.4% (second lowest; Ayog, 2020).[47] The survey took approximately 20 minutes to answer, and we collected a sample of 500 respondents (242 women and 258 men).[48]

The survey presents respondents with a hypothetical situation: an individual, who in 2016 took part in the last state election in the respondent's constituency, is considering whether to recontest in the next election. The respondent is told that this hypothetical person ranked fourth in 2016, forfeiting the deposit. This exercise hinges on a between-subject design based on two different versions of the survey. Half of the sample was randomly assigned one version, while the remaining half received the other. The only difference between the two versions is the name of the candidate. In one the candidate is called Manoj, a typical male name, in the other Neha, a typical female name. Their forfeiture status and their first name is the only information provided to the respondents, who are asked to answer a few questions. First, based on the candidate's past performance, how do they rank the candidate on a scale from 1 to 10? Second, based on their past performance, do they think the candidate should stand for election the next time? Third, how ashamed should the candidate feel for having forfeited the deposit?

Let us start by looking at whether respondents think that the candidate should feel ashamed for having forfeited the deposit. Answers confirm that forfeiting the deposit is indeed considered shameful. On a scale from 1 (no shame) to 10 (extreme shame) the average answer is above 5 for

[46] The complete survey is provided in the Online Appendix. It was conducted in July–August 2021 and was administered over the phone due to COVID-19 restrictions.

[47] In our heterogeneity analysis, we employ sex ratio for the year 2001, given that the non-experimental data we employ in the RD analysis ranges from 1970 to 2019.

[48] We also repeat the analysis only for Uttar Pradesh, which of course substantially reduces the sample size, and uncover an RD estimate of -0.297 percentage points (robust p-value $= .029$) for female candidates, which is over twice as high as our baseline estimate of -0.133 in Table 2. This aligns with our expectations, given that Uttar Pradesh is one of the most regressive states in terms of gender norms, as discussed above.

Table 8. *Survey Results Eliciting Voter Preferences over Forfeited Candidates.*

	(1)	(2)	(3)
Panel A: recontest in next election? (Yes/No)			
Forfeited candidate female	0.142***	0.127***	0.185***
	(0.037)	(0.039)	(0.060)
Respondent male		0.006	0.067
		(0.050)	(0.066)
Candidate female × Respondent male			−0.115
			(0.095)
Panel B: rate the candidate (1–10) std.			
Forfeited candidate female	0.184**	0.159*	0.239*
	(0.088)	(0.091)	(0.139)
Respondent male		−0.007	0.078
		(0.082)	(0.138)
Candidate female × Respondent male			−0.158
			(0.205)
Panel C: candidate be ashamed? (1–10) Std.			
Forfeited candidate female	−0.182**	−0.147	−0.204
	(0.085)	(0.092)	(0.128)
Respondent male		0.028	−0.032
		(0.075)	(0.130)
Candidate female × Respondent male			0.112
			(0.176)

Notes: ***, **, * Significance at the 1%, 5% and 10% levels, respectively. Here $N = 500$. Treated arm refers to the survey with a female forfeited candidate, while control refers to the survey with a male forfeited candidate. Outcomes in panels B and C are standardised by the mean and SD of the control group. The raw variables are scales between 1–10, with 1 signifying very low rating/no shame. Column (1) presents an unconditional regression. Column (2) adds the following respondent-level controls: age, is Hindu, belongs to a general caste, lives in an urban area, has college education, is employed, is married, voted in the last federal or state elections, is below the poverty line. Column (3) adds an interaction between the respondent's gender and the treated dummy. SEs are clustered at the location level.

both male (6.13) and female (5.64) candidates. Online Appendix Figure A.5 helps us dig a bit deeper. It splits the sample between voters and non-voters and plots the kernel density estimates among the former and the latter, respectively. As we can see from panel b, non-voters treat the male and female candidates in a similar way, although there is more density around intermediate values for the female and more extreme values (both low and high) for the male. However, among voters (panel a) we recognise a very interesting pattern. While answers about the level of shame the female candidate should feel are essentially the same as among non-voters, the distribution is skewed to the right when it comes to the male candidate. This means that voters are more demanding towards male candidates and believe that losing the deposit if you are a man is indeed something to be very ashamed of.

Let us now move on to the question of whether the candidate should recontest in the next election. As we can see from Table 8, panel A, results are indeed consistent with what we have seen so far. Respondents are 14% more encouraging towards the female candidate when we do not control for observable characteristics (column (1)); this number remains virtually the same at 13% when we add controls (column (2)). But, if we also add the interaction between the respondent's and the candidate's gender (column (3)), it goes up to 18.5%. All three coefficients are strongly significant, with p-values below .01. Moreover, we can see from column (3) that male respondents are less encouraging towards the female candidate (−11.5%), although this is a noisy, insignificant estimate.

Next, we look at how respondents rate the two candidates on a scale from 1 (not suitable) to 10 (excellent). We first standardise this scale by the sample mean and SD of the respondents who received the Manoj survey, i.e., our 'control' condition. In line with previous results, we can see from Table 8, panel B that the female candidate is rated higher than the male. When we do not control for respondents' characteristics (column (1)), Neha receives a score 0.18 SDs higher than Manoj (p-value below .05). If we introduce controls (column (2)), the estimated coefficient is slightly lower (0.16) and significant at the 10% level. If we add the interaction between respondent's and candidate's gender (column (3)), it goes up to 0.24 SDs, significant at the 10% level. We also see that male respondents are relatively less favourable to females, −0.16 SDs, though this result is insignificant.

Finally, from the first column in Table 8, panel C, we see that, for the question of shame, the female candidate is treated more leniently than the male, with an estimated score 0.182 SDs below her counterpart, and this is significant at the 5% level.[49] Controlling for the observable characteristics of respondents (column (2)), that number decreases slightly (−0.147), but is no longer significant. Interestingly, the respondent's gender does not seem to matter. However, if, on top of these controls, we also add an interaction between the respondent's gender and the treatment dummy (column (3)), we see that male respondents are actually relatively less lenient towards female candidates (0.112), although this coefficient is not statistically significant.

All in all, these results paint quite a positive picture. They support the idea that losing the deposit is indeed regarded as shameful, thus supporting our conjecture that forfeiture is stigmatised in Indian society. However, on average, respondents think that men should be more ashamed and this is especially true among voters, which is the population that mostly interests us. Secondly, respondents are more favourable towards the female candidate, rating her higher than the male, and more encouraging when it comes to recontesting. These are all interesting results, especially coming from a state that is a very poor performer when it comes to gender equality. Two caveats should be borne in mind. First, we must be cautious when interpreting survey responses as they may be subject to social desirability bias.[50] However, note that we tried to minimise this potential issue by adopting a between-subject design and by using the name of the hypothetical candidate, thus pairing down the centrality of gender. Second, our survey necessarily depicts society's sentiment today, limiting our ability to explore the influence of changing mores over time.

7. Conclusion

We demonstrate that seemingly innocuous procedural barriers designed to discourage non-serious candidates from the ballot paper can have unintended consequences by perpetuating gender gaps in political candidacy. We study a particular eligibility requirement in Indian elections demanding candidates to submit a nominal monetary deposit, which is forfeited if the candidate fails to garner more than one-sixth of the vote share in their constituency. Forfeiture is universally regarded as a humiliating defeat in Indian society. The induced RD design allows us to cleanly identify the underlying effect. We show that female candidates who forfeit the deposit are over 60% less likely to recontest and appear in the top-3 positions in the next election, relative to candidates

[49] The shame index is again standardised using the sample mean and SD of the control condition.

[50] Respondents may tend to conceal preferences that are considered socially undesirable (see Maccoby and Maccoby, 1954; Edwards, 1957).

who retain the deposit. Remarkably, these effects endure over the long term, lasting over twenty years (four electoral cycles), i.e., virtually the whole political life of the average candidate. On the contrary, we consistently estimate null effects for men.

We show that the roots of our findings lie in cultural factors. Indeed, our results are driven by states where the gender ratio is tilted in favour of men. Effects in these states are even larger, both in the short and long runs. We also find that treated women are substantially and significantly less likely to win the election four years later. We enhance this analysis by conducting a survey experiment with a representative sample of the population of one of the states that fare worse in terms of gender parity. Results from the survey confirm that voters consider forfeiture shameful, but respondents seem to be more lenient towards women than men.

Female under-representation in political offices is a concern for most governments and societies. We document that one major hindrance contributing to this gap is the differential impact of procedural barriers to candidacy across gender. Our findings fit with the vast literature that has documented gender gaps in competitiveness across various domains (see the review in the introduction). However, there is also evidence that gender imbalance can be mitigated by information interventions highlighting the costs of dropping out (Kessel *et al.*, 2021). Interventions aimed at mitigating such gender gaps can arguably be even more effective in electoral settings, where agents are competing to be policy-makers with potential long-term personal and social payoffs. Previous research has shown that a cognisance of intergenerational returns in the decision to compete can help reduce the gender gap in competitiveness (Cassar *et al.*, 2016). Our research thus highlights an important domain of future policy space focused on reforming and revisiting procedural barriers to political candidacy as a channel for increasing female political representation. For instance, one alternative is to replace the conditionally recoverable deposit by a uniformly applicable, non-refundable fee. Such a fee will still continue to dissuade non-serious candidates from clogging the ballot paper, the original intent of the deposit regime, without creating artificial categories of success/failure. Such policies can also have wider relevance as over thirty democracies around the world implement similar deposit forfeiture rules. Our future research aims to evaluate whether such protocols also have relevance in Western democratic societies.

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Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package

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