How Clientelism Closes the Gender Turnout Gap: Theory and Evidence from India

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Abstract

When do women and men participate equally in the electoral process, particularly in developing countries where women often lack the individual resources thought to drive turnout, such as education, income or political knowledge? I argue that besides the traditional, resource-based explanations of women's participation, there is a second path to women's equal electoral participation that depends on clientelist mobilization of household support for female turnout. Where households are supportive of women's participation, they can bridge the resource gap to enable female turnout even in the most unlikely of contexts, namely where women face a lack of resources and high costs to participation in public life. But households will only be supportive were returns to a vote are high. I develop a typology of clientelist regimes that posits that both the *sequencing* of the clientelist exchange as well as the *source* of the clientelist resources determines the value of a vote. Returns to a vote should be highest when clientelist parties rely on post-election resource sharing arrangements that provide selective post-election access to state resources; and lowest when clientelist parties instead bank on privately funded electoral handouts. I test this theory in India, a particularly puzzling case of gender turnout parity. I use the fact that in India, ethnicity is politically salient and clientelist parties purposefully incorporate *some* ethnic groups into their leadership, while excluding others, to send signals about the future distribution of state resources. Members of ethnic groups who have a co-ethnic in any party's leadership will expect the highest returns to a vote, and therefore be most likely to support female turnout. Using a novel panel dataset on the number and types of ethnic groups incorporated into state-level party leadership of all major parties for all state elections in Madhya Pradesh and Uttar Pradesh from 1977 through 2007, I show that a) the number of ethnic groups incorporated went up over time, and that b) a rise in the number of politically incorporated ethnic groups before an election leads to a drop in the gender turnout gap during the election.

1 Introduction

When do women participate equally in the electoral process? Traditional theories of female political participation are fundamentally resource-based. At the micro level, these theories expect a woman's individual-level resource endowments to predict her turnout (see, for example, Schlozman et al., 1994; Brady et al., 1995; Verba et al., 1997; Burns et al., 2001); while at the macro level, they posit that higher levels of economic development (Inglehart and Norris, 2000, 2003) or female labor force participation rates (Iversen and Rosenbluth, 2006, 2010) should correlate with higher aggregate female turnout.

Yet research has shown that the relationship between individual-level resource endowments and political participation is not as straightforward as previously thought in the context of developing democracies. Unlike in industrialized countries, in developing countries the poor tend to vote at higher rates than the rich (Kasara and Suryanarayan, 2015); and women's participation rate does not always rise with increasing resource endowments the way that men's does (Coffé and Bolzendahl, 2011; Gottlieb, 2016). Economic development and female labor force participation rates also only insufficiently explain differences in levels of women's turnout between countries, or change over time, outside of North America and Europe (Christy, 1987, p. 56; Inglehart and Norris, 2003, p. 108).

Why do traditional theories of female political participation struggle to explain women's turnout outside of Europe and North America? Two important structural factors might differ between the countries that were used to build these theories and those countries where the theories hold limited explanatory power: the role of the household unit and the rules of resource distribution. In the absence of well-developed welfare states, families regularly take on important roles in welfare improvement and risk mitigation for individuals (Becker, 1981). This is particularly the case in developing countries, where the state often cannot take care of the young, the old, the sick, or the unemployed, forcing extended families to step in instead (Cox and Fafchamps, 2007; Iversen and Rosenbluth, 2010). This makes households sites of coordination for several individuals, with potentially differing preferences (Becker,

1981; Manser and Brown, 1980). Consequently, many decisions that have traditionally been modeled as individual-level decisions, including the choice to work, to run for office, or to turn out on election day, might be at least partially determined by households in such settings (Chhibber, 2002; Doss, 2013; Jayachandran, 2015; Gottlieb, 2016; Prillaman, 2021; Cheema et al., 2022).

Secondly, while clientelism is not exclusive to developing democracies (e.g., Shefter, 1994; Kitschelt and Wilkinson, 2007), resource distribution strategies in countries at low or medium levels of economic development and/or in relatively young democracies tend to skew toward clientelist distribution over programmatic distribution, driven at least in part by lower bureaucratic capacity and limited democratic competition (Kitschelt and Wilkinson, 2007; Kitschelt, 2007; Kitschelt and Kselman, 2013; Berenschot and Aspinall, 2020; Yıldırım and Kitschelt, 2020). Where votes are exchanged for resources in a quid-pro-quo fashion, it alters voters' incentives to participate compared to settings where distribution is largely programmatic (Stokes, 2005; Nichter, 2008; Gans-Morse et al., 2014; Stokes et al., 2014; Kasara and Suryanarayan, 2015).

Based on these insights, I argue that there is a second path to women's equal political participation, one that relies on *household support* for female turnout incentivized by *high returns to a vote*. This path is possible only where two scope conditions are met: individuals are tightly embedded into household units, and returns to a vote are high. Where households are sites of strong coordination, supportive households can bridge the resource gap for women – by sharing information, transportation, or time-consuming chores, for example – to boost female participation. However, households will only be willing to shoulder those additional costs if returns to a vote are very high. Scholars have argued that most elections in industrialized countries will be low-cost, low-benefit elections, explaining falling turnout rates across most of these countries (Aldrich, 1993). By contrast, clientelism potentially turns votes into assets to bargain with, and effectively limits free-riding options as only those who actively participate will be rewarded. However, not all clientelism provides equally high returns to

a vote. I develop a typology of clientelist regimes that posits that both the *sequencing* of the clientelist exchange as well as the *source* of the clientelist resources determines the value of a vote. Returns to a vote should be highest when clientelist parties rely on post-election resource sharing arrangements that provide selective access to government employment or welfare programs after election results are in; and lowest when clientelist parties instead bank on privately funded electoral handouts disbursed before an election.

I test this theory in India, a particularly puzzling case of gender turnout parity. The gender turnout gap in India closed rapidly over the past 20 years in some of the poorest, but not yet in some of the wealthier states. At the same time, female labor force participation rates have been *falling* in the country. Clientelism has a long tradition in India, but the recipients of clientelist rewards, particularly of post-election access to the state, have changed over time and with spatial variation (Jaffrelot, 2003; Chandra, 2004; Pai, 2004; Witsoe, 2009; Michelutti and Heath, 2013). I use the fact that in India, ethnicity is politically salient and clientelist parties purposefully incorporate *some* ethnic groups into their leadership, while excluding others, to send signals about the future distribution of state resources. Members of ethnic groups who have a co-ethnic in any party's leadership will expect the highest returns to a vote, and therefore be most likely to support female turnout. By contrast, members of ethnic groups that are not represented in any political party's leadership will expect minor clientelist benefits at best, and therefore be unlikely to bridge the resource gap for women. My theory predicts that where more distinct ethnic groups are incorporated into political parties, a larger share of the population will support female turnout, thereby closing the gender turnout gap.

I then rely on a novel panel dataset on the number and types of ethnic groups incorporated into state-level party leadership of all major parties for all state elections in Madhya Pradesh and Uttar Pradesh from 1977 through 2007 to provide evidence in line with my theory. To my knowledge, this is the first panel dataset on state-level party leaders and their ethnic identities in India. I find that the number of incorporated groups indeed went up over time,

but with considerable variation across and within the two states. Using a fixed-effects model, I show that both at the state level as well as at the constituency level a rise in the number of politically incorporated ethnic groups leads to a drop in the gender turnout gap.

This paper makes several important contributions to the literature. First, bringing attention back to the gender turnout gap is normatively important. Voting is arguably "the most common and important act of political participation in any democracy" (Aldrich, 1993, p. 246) as well as "the most fundamental mechanism for holding public officials accountable" (Burns et al., 2001, p. 21). The gender turnout gap is far from closed in many countries around the world and unequal rates of participation among the electorate pose a normative challenge for democracies, what Lijphart (1997) termed "democracy's unresolved dilemma" (p. 1). Second, I contribute to our understanding of clientelism, by developing a typology to analyze the value of clientelist rewards to voters. As such, I join a burgeoning literature that tries to systematically unpack the concept of clientelism and take seriously variation in manifestations and meaning (see, for example, Nichter, 2018; Berenschot and Aspinall, 2020; Aspinall et al., 2022). Third, by taking seriously the importance of the household as a decision-making unit, especially in the context of developing countries, I contribute to a growing literature that stresses the importance of intra-household gender relations in the study of political behavior (Chhibber, 2002; Iversen and Rosenbluth, 2010; Mendelberg et al., 2014; Gottlieb, 2016; Khan, 2017; Prillaman, 2018; Cheema et al., 2022; Prillaman, 2023).

This paper proceeds as follows. Section 2 lays out the theory of clientelist mobilization of household support for female turnout. Section 3 provides background on India, the setting for my empirical investigation. Next, I describe the data on ethnic incorporation and how it was collected. Section 5 shows results of fixed-effects regression analyses that are consistent with the theory, and rules out alternative hypotheses. The paper ends with a conclusion.

2 Theory

Where public goods provision is low, individuals often rely on extended families for welfare improvement, risk mitigation and the production of social status (Cox and Fafchamps, 2007). Because there are few outside options – in the form of public child care, health care, old age pensions, or unemployment benefits – individuals are dependent on the household; as a result, they have to coordinate many of their behaviors, including their political participation, with other family members (Manser and Brown, 1980; Becker, 1981; Iversen and Rosenbluth, 2010).[1]

Members of the same household unit will share resources and responsibilities, as well as any gains from this joint effort (Becker, 1981). However, households are not unitary actors, with different family members having different priorities and everyone bargaining over the outcome (Manser and Brown, 1980). Some members will have more bargaining power than others, determined by their outside options. That is, the more credibly an individual can threaten to leave the household unit, the more bargaining power she will have and, therefore, the more likely she is to have her preferences realized (Manser and Brown, 1980; Agarwal, 1997).[2] A traditional sexual division of labor that places the burden of care and household work on women while bundling income generation with men therefore disadvantages women, leading to systematically slimmer exit options and lower bargaining power for females (Agarwal, 1997). When power is systematically skewed toward senior males, it essentially amounts to patriarchy, "a system of gender inequality [...] supported by institutions that are gendered and therefore working out asymmetrically for men and women" (Staveren and Odebode, 2007, p. 905). To be clear, patriarchy is not limited to developing countries by any means. However, low public goods provision makes individuals in many

[1] By contrast, welfare states make individuals less dependent on the family by providing education, health care, income subsidies, or old age care *outside* the household, therefore reducing other family members' influence and making political participation more likely to be an *individual* decision (Edlund and Pande, 2002; Iversen and Rosenbluth, 2006, 2010).

[2] Two major determinants of outside options are an individual's earning potential as well as remarriage chances (Edlund and Pande, 2002; Iversen and Rosenbluth, 2006). Any legislation that expands public care work or eases the process of (one-sided) divorce for women, therefore, will increase female bargaining power.

developing countries more vulnerable, and therefore more dependent on the household, than in many industrialized countries (Iversen and Rosenbluth, 2010).

Research shows that in societies where individuals are dependent on the household and household bargaining power is gendered, the (often male) head of household[3] is likely to impact a host of female behaviors and outcomes, including health care choices, the decision to work, or educational outcomes (Field et al., 2010; Doss, 2013; Jayachandran, 2015; Bernhardt et al., 2018; Bursztyn et al., 2018; Field et al., 2019). More recently, scholarly attention has turned to the role the household might play in women's political behaviors. As Prillaman (2023) argued, in such settings oftentimes "women's political lives revolve around their household [...] and because of intra-household inequalities and the potential for socially sanctioned and legally tolerated coercion by men, women often concede political authority and representation to the men in their families" (p. 21). Indeed, previous work has found that male household members affect women's public preference expression (Khan, 2017); their attendance of public meetings (Gottlieb, 2016; Prillaman, 2018, 2023); or their decision to run for office (Chhibber, 2002).

Female voter turnout, too, is likely not to be a purely individual decision in such settings. In a field experiment in urban Lahore in Pakistan, Cheema et al. (2022) showed that men's support was essential for female turnout, and that women were more likely to turn out when *men* in their household were targeted with a message on improving women's voter participation. In neighboring India, Prillaman (2023) found that women's voter turnout in rural Madhya Pradesh was nearly at par with men's because the most powerful members of the household – older males – were supportive of women's vote.[4]

[3] For the remainder of this article, whenever I use the phrase "the household," I mean the *head of household*, denoted as the most powerful individual within the household unit. In principle, the head of household need not to be male for the logic of my argument to hold; only to enjoy a disproportionate amount of decision-making power within the household unit. Empirically, however, heads of household are overwhelmingly male (see, for example, Staveren and Odebode, 2007; Farré, 2013; Jayachandran, 2015). In India, for example, only about 13 percent of all households were headed by a woman, according to the 2011 government census. See Brulé and Gaikwad (2021) and Robinson and Gottlieb (2021) for research that shows what impact matrinlineality, and therefore female heads of household, can have on women's political participation.

[4] By contrast, women's non-electoral political participation lagged behind men's because older males were not supportive of women partaking in village council meetings and other activities.

As becomes clear from this discussion, in settings where individuals are tightly embedded into household units, families can either *constrain* or *boost* women's political participation. If the household is opposed to female participation, it can restrict women to activity levels below what their *individual* resource endowments would lead us to expect (Gottlieb, 2016); by contrast, where the household is supportive, it might push women's participation beyond what their individual resources would allow otherwise (Cheema et al., 2022). The question then arises: Under what conditions will households be supportive of women's turnout? Prillaman (2023) argued that households support women's electoral participation "because all-household voting is strategically valuable for those with power in the household" (p 21). And yet, not all households are supportive of women's turnout at all times. Instead, whether or not families support female electoral participation, I argue, depends on the nature of *returns to a vote*.

Canonical turnout models conceive of turnout as a function of costs and benefits. An individual will turn out on election day if and only if the benefits of voting outweigh the costs of doing so (Downs, 1957; Riker and Ordeshook, 1968; Aldrich, 1993). This principle holds true even when turnout is determined at the household level: the head of household will only help shoulder the costs of a female relative's participation if he expects the returns to this additional vote to eclipse the additional costs. Equation 1 summarizes this logic. The head of household forms preferences over each family member i's turnout by considering the costs (right-hand side) and returns (left-hand side) to that person's vote.

$$\underbrace{\underbrace{p_i B_{h,i}}_{\substack{\text{outcome-dependent}\\ \text{benefit}}} + \underbrace{D_{h,i}}_{\substack{\text{outcome-independent}\\ \text{benefit}}}}_{\text{total returns to a vote}} > \underbrace{C_{h,i}}_{\text{costs}} \tag{1}$$

Returns to a vote (the left-hand side of the equation) consist of two parts, one that is outcome-dependent, i.e., contingent on who wins the election, and one that is outcome-independent and therefore unaffected by the election result. The expression $p_i B_{h,i}$ represents

the outcome-*dependent* benefit the household receives for an additional vote, where p_i is the probability that i will cast the decisive vote and $B_{h,i}$ is the benefit accruing to household h if the preferred party wins. Here, $B_{h,i}$ represents the difference between any benefit the household expects in case the preferred party wins, and any benefits the household would receive in case the rival wins; this is usually thought of as the difference in policy positions between the preferred party and the alternative. $B_{h,i}$ should therefore matter most where resource distribution is programmatic and voters can assess parties' policy positions. Outcome-dependent benefits grow as the difference in policy positions between contenders increases. At the same time, the outcome-dependent benefit rises as races become tighter, i.e., as p_i inches closer to 1. This captures the collective action problem inherent in voting: because everyone gets to enjoy the policy benefits provided by the winner after the election anyway, independent of whether they voted themselves, it is rational for the *individual* (or here, household) to not want to shoulder the costs and instead only bag the benefits. After all, if an additional vote will be unlikely to change the outcome of the election – and therefore the outcome-dependent benefit that h can expect – the household has little incentive to incur the costs of an additional vote.

By contrast, $D_{h,i}$ is the outcome-*independent* benefit accruing to household h if i votes. The household will receive this benefit just for i's participation, independent of who wins the election. Importantly, outcome-independent benefits matter most in clientelist settings. Clientelism is the quid-pro-quo exchange of benefits for support, where clientelist parties will only reward those households that actually voted for them, or, at the very least, that turned out and actively participated in the election (Stokes et al., 2014; Nichter, 2008). The collective action problem does not arise to the same degree in clientelist systems as it does in programmatic settings: because access to state resources in not universal – only those who actually participate will be rewarded – it is much harder to free-ride. The closeness of a race, therefore, matters little to none in clientelist settings, since what determines rewards is not just the preferred party winning, but *personally supporting* the preferred party at the

ballot box.

The total returns to a vote – the left-hand side of Equation 1 – need to be weighed against the costs of participation (right-hand side). Only when the benefits outweigh the costs will households be willing to extend support to family members to help them vote. However, the costs C_i of participation are likely gendered. On the one hand, women have fewer of the individual-level resources such as education, political information, or mobility that are usually required to offset the costs of participation (Schlozman et al., 1994; Brady et al., 1995; Burns et al., 2001); accordingly, households will have to provide those resources to women (but not to men who already possess the resources) to enable their turnout (Cheema et al., 2022). At the same time, the social costs of women's participation are often higher than those of men's participation, particularly where the entire family's social status is tied to female behavior (Dube, 1997; Jayachandran, 2015; Bernhardt et al., 2018).

The gendered costs of participation stand in stark contrast to the gender-neutral returns to a vote. An additional vote for the preferred party produces the same rewards for the household, independent of whether a man or a woman casts it (Prillaman, 2023). However, the costs for female participation are, on average, higher than that for male participation. Based on this framework, then, we can make some general predictions about household preferences over female turnout. At low to medium returns to a vote, households will systematically support male turnout but not female turnout, and gender turnout gaps will ensue, both at the household level and in aggregate. However, as returns to a vote go up, *households will switch from opposing to supporting female turnout* even in the face of high social costs for women.

When will returns to a vote be high enough to incentivize household support for female turnout? In programmatic polities, returns to a vote should rise most sharply as races become tighter and/or the policies of the competing parties diverge more strongly (Aldrich, 1993; Teele, 2023). By contrast, in clientelist polities, returns to a vote depend largely on the *type of clientelism* that parties employ. Here, I draw attention to two important dimensions

of clientelist regimes that will heavily affect the *value of clientelist benefits* to voters, namely the sequencing of the exchange and the source of resources.

Clientelist exchanges, in their most simple form, take the shape of a two-player game: a politician offers benefits and demands votes; and a voter offers votes and demands benefits. If this were a back-alley black-market transaction, one party could hand over the goods while the other pushes an envelope with money across the table simultaneously, making sure that everyone gets what they were promised. But the exchange of votes for selective benefits does not happen simultaneously, making clientelist exchanges rife with uncertainty. Whichever party to the deal goes first has to absorb all the risk, because the other has strong incentives to renege on their commitment. If a voter casts her ballot first, the politician has incentives to keep all benefits to herself; after all, the votes are already cast and the voter has no way of taking back her ballot. Conversely, if the politician distributes electoral handouts first, the voter has incentives to just collect benefits from multiple politicians and in the end vote as she pleases anyways (or abstain altogether); after all, the goods are already in her hands. Whoever goes first, therefore, incurs the higher risk, and will want to insure themselves against the other one reneging. In other words, the sequence of the game matters.

We can distinguish two types of clientelist exchanges based on their sequencing: one where the politician moves first, providing *electoral handouts* to the voter before an election in order to elicit votes; and one where the voter moves first, casting a ballot on election day in expectation of *post-election benefits*. The sequencing of the game determines not just the timing, but, I argue, also the type and value of clientelist benefits that will be distributed, and who will be targeted.

Electoral handouts encompass material resources – such as money, alcohol, or consumer goods – that are distributed during an election campaign but, importantly, *before* ballots are cast. The use of electoral handouts forces the party to absorb all the risk: the party moves first and provides goods to the voter, which are difficult to impossible to retrieve once disbursed. Accordingly, the party should want to invest heavily in monitoring and enforcement

capabilities in order to make sure that it gets "its money's worth" on election day. Yet monitoring individual-level vote choice is empirically difficult in most electoral democracies, and enforcement mechanisms are limited once voters received handouts (Nichter, 2008; Hicken and Nathan, 2020).

The alternative to electoral handouts are *post-election benefits*. This encompasses any benefits distributed to voters *after* votes have already been cast. In this case, the voter moves first, casting her ballot on election day; and the party disburses rewards only after being able to observe – or at the very least, deduce – levels of support. Accordingly, all the risk lies with voters. It is the voter who should want to monitor whether the politician follows through on her promise of clientelist benefits. Unlike the vote though, the distribution of clientelist rewards is usually not secret. On the contrary, the party has reason to flaunt its distributive largess and claim credit for any and all resources that reach voters.

There are at least two reasons why post-election benefits will be of higher value to voters than electoral handouts, relating to monitoring and enforcement costs as well as targeting. First, as described above, electoral handouts require the party to move first, meaning the party will have to bear the cost of monitoring and enforcing the clientelist agreement. These costs will have to come out of the party's budget; therefore, there is less left to distribute to potential voters. By contrast, post-election benefits put the onus of monitoring and enforcement on voters, freeing up resources for the party.[5] Assuming a constant budget, then, the party will have more resources to distribute to voters when it banks on post-election resources than on electoral handouts.

Second, post-election benefits can be better targeted and therefore involve less leakage than electoral handouts. Because of the difficulty of monitoring, and the near impossibility of effectively enforcing, vote-buying deals when the ballot is secret, clientelist parties often price in a certain amount of "leakage," knowing full well that only a fraction of those who receive a handout will actually reciprocate (Hicken and Nathan, 2020; Yıldırım and Kitschelt, 2020).

[5] See Hicken and Nathan (2020) on the conditions under which a party might want to forgo monitoring entirely and instead accept leakage in order to maximize its budget's effectiveness.

In practice that means the party will have to distribute handouts to many more voters than it needs to win the election. For example, Aspinall et al. (2022) found that operatives in the Philippines and Indonesia generally estimate that only between one third and two-thirds of targeted individuals will actually vote for them (p. 117). Given a constant budget, therefore, the party has to disburse small amounts to a large number of voters when it relies on electoral handouts. By contrast, post-election resource sharing means that the party can be much more precise in its targeting, disbursing resources only to those individuals (or, more likely, groups or geographic areas) that supported it (Chandra, 2004; Rueda, 2017).[6] A larger budget – because the party incurs no monitoring and enforcement costs – combined with better targeting, therefore, means the party should be handing out larger payoffs per voter when it employs post-election resource sharing strategies than when it relies on electoral handouts.

Besides the sequence of the game, a second dimension of clientelism is predictive of the value of clientelist payoffs for voters, namely the source of the benefits (Berenschot and Aspinall, 2020; Aspinall et al., 2022). Clientelist rewards can stem either from public (i.e., state) sources or from private sources, including a politician's or party's own funds, as well as donations collected from business persons and supporters. Importantly, many of the most sought-after clientelist rewards involve privileged access to the state, including coveted government and party jobs (Chandra, 2004; Oliveros, 2021), access to state-run welfare schemes (Stokes et al., 2014; Aspinall et al., 2022), or police responsiveness (Wilkinson, 2004; Berenschot, 2016).[7]

Scholars have noted that empirically, state resources are mostly deployed in post-election clientelist regimes, while private resources are more likely to be associated with electoral handouts (Berenschot and Aspinall, 2020; Yıldırım and Kitschelt, 2020; Aspinall et al., 2022).

[6] Alternatively, once the party won the election it might opt to simply reward those who participated at all, since turnout is much easier to monitor than vote choice (Nichter, 2008).

[7] The importance of state resources for clientelist exchanges is reflected in the fact that many scholars narrowly define the term "patronage" as referring exclusively to rewards based on privileged access to the state (see, for example, Chandra, 2004; Stokes et al., 2014; Oliveros, 2021).

This is rational for parties, for several reasons. First, a prerequisite for a party distributing state resources is the party having *some* sort of access to the state itself; this usually involves a minimal level of electoral success in the previous election. In fact, promising privileged access to the state *if and only if* the party meets a certain electoral threshold – such as having at least one legislator in parliament or garnering enough votes to be recognized as an official party – eliminates any incentives to shirk on part of voters and aligns the party's and voters' incentives: both now want the party to win in order to gain access to the state (Gallego, 2015).[8] In addition, because of the high value of these state resources, parties generally do not want to incur leakage in those. Instead, they reserve them precisely for post-election rewards so that they can be targeted in the most effective way.

This theory, therefore, predicts that we should see smaller gender turnout gaps in developing countries where clientelist returns to a vote are high. When what's at stake in elections is access to government jobs, work programs, or pension schemes, families are more likely to go above and beyond to enable female turnout.[9] By contrast, where all that the household can get out of an election is a bottle of alcohol or a small amount of cash, only some, but by no means all, households will support women's turnout, and larger gender turnout gaps will ensue.

3 The Case: India

I test this theory in India, a particularly puzzling case of women's turnout. For almost 60 years after Independence, up until 2004, India recorded a relatively stable gender turnout gap, with men turning out at between 8 and 11 percentage-points higher rates than women in national parliamentary elections.[10] Beginning in 2009, the gender gap started narrow-

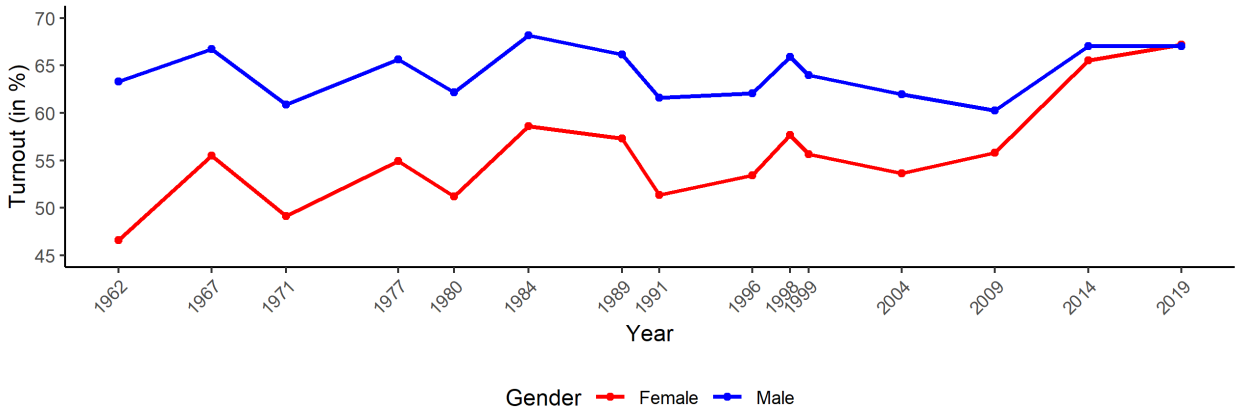
[8] By contrast, post-election benefits stemming from a political actor's private funds still do not require the party to win (as the private coffer is filled anyways, and resources can be distributed whether the party wins or loses), and therefore retain some incentives for voters to shirk.

[9] This effectively decouples women's electoral participation from their individual-level resources. Instead of women's resource endowments, household support should be most predictive of their participation.

[10] India is a parliamentary democracy, electing its national parliament, called *Lok Sabha*, in a massive election effort every 5 years. National elections are also called *General Elections*, as opposed to state-level

ing rapidly, effectively closing in the 2019 elections. Figure 1 shows male (blue) and female (red) turnout in percent for all national elections since 1962.[11] Several aspects of the graph are noteworthy: first, male turnout has stayed remarkably stable over the past 60 years, essentially moving around the 64 percent mark. Second, until 2004, male and female turnout moved basically in unison, just at different levels (with women averaging almost 10 percentage-point lower turnout than men). Third, the closing of the gender turnout gap in India is fundamentally driven by women voting at higher rates, not by male turnout shrinking.

Figure 1: Turnout by Gender in Indian General Elections



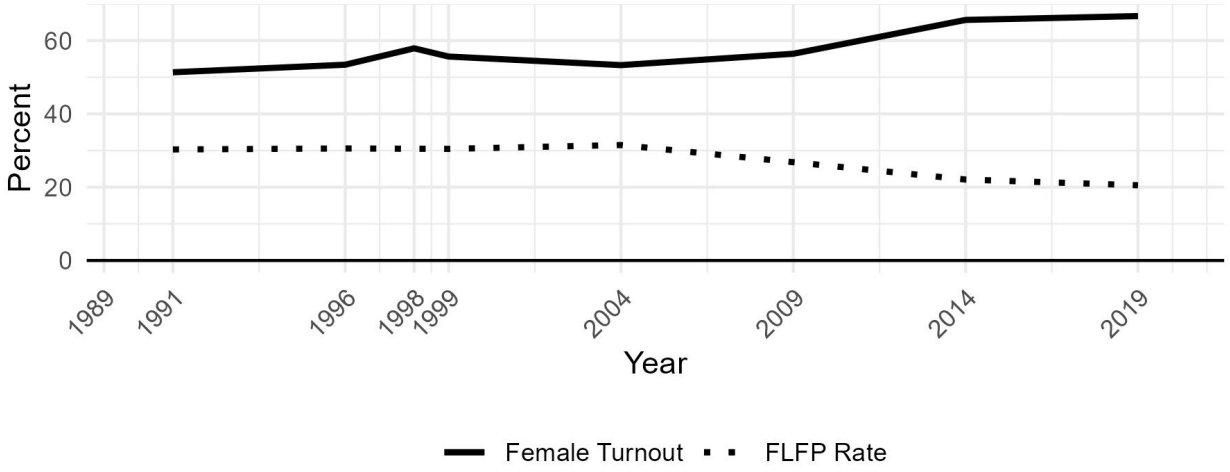
However, national aggregates mask considerable variation across space and time within India. Importantly, some of the least developed states in India, such as Uttar Pradesh and Bihar, reached turnout parity relatively early, while some of India's economic powerhouses, including Maharashtra and Gujarat, still record men voting at higher rates than women. At the same time, women's labor force participation rates have been *falling* over the same period that female turnout has been rising (see Figure 2). Finally, female literacy has been on the rise in India over the past decades, including during the time period when female turnout has been catching up with male turnout, suggesting improvements in education might be

Assembly Elections for state legislatures (institutions that are called *Legislative Assemblies*).

[11] Data on the gender turnout gap is only available starting in 1962, even though India held elections in the 1950s as well.

behind the closing of the gender turnout gap. However, a more fine-grained analysis reveals that female literacy rates have been rising relatively uniformly across all Indian states; yet the gender turnout gap closed early in some states and has remained remarkably stable in others (see Appendix Figure A.1). Taken together, then, traditional theories of women's participation hold limited explanatory power for trends in the gender turnout gap across Indian states.

Figure 2: Female Labor Force Participation and Turnout in India



Instead, I argue that what has changed over the past 20 years is the level of clientelist mobilization which, in turn, has propped up household support for female turnout.

Households play an important role in most women's – indeed, most *individuals'* – lives in India. Because the state only provides limited social welfare programs, families are the most important mechanism for welfare generation and risk mitigation for most individuals in India, particularly in rural areas (Gupta, 2012; Kruks-Wisner, 2018). Accordingly, "[a] whole variety of life choices depend less on individual preference than on the family: what sort of education to pursue, the choice of a profession, the number of children to have, whether the wife should work, and whether to make certain major purchases" (Minault, 1981, p. 3).

Women are particularly disadvantaged in terms of bargaining power within these household units for several reasons. First, and most importantly, patrilineal, patri-virilocal tra-

ditions mean that upon marriage, women leave their natal home (and village) and become members of their husband's household (Das, 1976; Srinivas, 1977; Dube, 1997). Women, particularly newly married women, are 'outsiders' to these households (and often the village), curtailing their networks and in effect limiting their exit options (Jacobson and Wadley, 1977; Prillaman, 2023).[12] A strong sexual division of labor further means that women are often confined to household chores and unpaid – yet essential – work, such as tending to the family farm or cattle. The lack of an own income, though, further restricts their exit options and lowers their bargaining power (Deshpande and Kabeer, 2021). Finally, because girls are expected to leave upon marriage but boys usually stay with their parents and take care of them in old age, many parents in India exhibit strong son preferences as a rational response to financial insecurity and the lack of public old age provisions (Jayachandran, 2015; Rosenblum, 2017; Kaul, 2018; Bhalotra et al., 2020; Brulé, 2020). Accordingly, household members with more bargaining power affect a host of behaviors for women in India.

Perhaps unsurprisingly, then, women in India are much more likely to participate in *any* political activity when they have their household's support to do so and, conversely, much less likely to partake when their households show opposition (Datta et al., 1998; Chhibber, 2002; Goyal, 2020; Prillaman, 2023).[13] Prillaman (2023) posited that female turnout in Madhya Pradesh was high – much higher than any other form of political activity – because "[t]he coercive unitary household enforces women's voting and restricts women's non-electoral participation" (p. 10). My own research in Uttar Pradesh, one of India's least developed states, found that household support is much more predictive of women's electoral participation than their own resource endowments. In a survey conducted around the 2022 state elections, I found that women's turnout was high and at par with men's, even though they enjoyed

[12] By contrast, matrilineality, which is practiced by many tribal groups in the Northeast, as well as some caste groups in South India and some Muslim groups, generally leads to women enjoying higher bargaining power and therefore fewer restrictions on their conduct (see, for example, Dube, 1997; Brulé and Gaikwad, 2021).

[13] Although the literature also has many examples of women who defy family expectations and persevere in their political activity in the face of coercion and even physical violence (see, for example Joshi, 1998; Brulé, 2020).

low levels of education, income, and political knowledge.[14] By contrast, for the male head of household, individually held resources were much higher and, importantly, associated with turnout. Overall, more than 93 percent of households coordinated strongly on turnout, with either both respondents turning out (92 percent) or neither (1.5 percent).

But when and why would households be supportive of female turnout today, closing the gender turnout gap that persisted for decades? The answer, I argue, lies in changes to clientelist returns to a vote. What has changed over the past decades, I posit, is the extent to which different groups within the population have gained clientelist access to the state through post-election resource sharing arrangements with parties. Groups that have such access receive the highest returns to a vote, and therefore have strong incentives to support female turnout. Consequently, the higher the share of the population that has clientelist access to the state, the smaller the gender turnout gap will be.

Clientelism has been prevalent across many parts of India right since Independence, and continues to this day (Varshney, 2000; Chandra, 2004; Wilkinson, 2007, 2014; Bussell, 2019). Clientelism in India has also taken many shapes, spanning the full spectrum of clientelist goods and exchange practices. On the one end of the spectrum, politicians sometimes distribute electoral handouts, such as cash or liquor, in the run-up to an election (Kerbart and Sivakumar, 2005; Björkman, 2014; Chauchard, 2018); on the other, they rely on promises of future privileged access to state-sponsored goods and services after the election (Chandra, 2004; Witsoe, 2011b).[15] As elsewhere, clientelist parties in India may employ a mix of these different strategies at any given point in time, and target different types of voters with different benefits. Here, I am interested in the clientelist arrangement that is most valuable to households, and therefore should make it most likely that families will support female

[14] Less than 30 percent of women worked, less than half had ever received formal schooling, and only about 20 percent knew the name of their representative in the state assembly. This stands in stark contrast to male heads of household: more than 95 percent worked, almost 70 percent were literate and close to three-quarters knew the name of their representative. See Appendix Figure A.2 for more details.

[15] As elsewhere, outright vote-buying seems to be largely ineffective at swaying an election, leading scholars to reinterpret the presence of such handouts as serving functions other than "vote buying" (Björkman, 2014; Chauchard, 2018).

turnout: post-election resource sharing arrangements that provide access to the state.

State resources that clientelist parties make available in exchange for political support may include government jobs (Chandra, 2004), access to food subsidies, pensions, and other welfare programs (Heath and Tillin, 2018), documentation required to assert one's rights vis-a-vis the state in the form of land titles or caste certificates (Berenschot, 2010, 2016; Corbridge et al., 2012), police responsiveness (Wilkinson, 2004; Michelutti and Heath, 2013; Berenschot, 2016), or licenses and permits to run businesses (Chandra, 2015), among many others. These resources are valuable, often even essential for people's survival, particularly in rural areas (Gupta, 2012; Kruks-Wisner, 2018; Auerbach and Kruks-Wisner, 2020).

In India, parties send signals about who, exactly, will gain privileged state access after an election through the ethnic composition of their leadership. Parties target voters from certain ethnic groups by incorporating co-ethnics into leadership positions; co-ethnics of the political leaders will have the highest expectation of gaining state resources after an election (Chandra, 2004, 2009).[16] Even if the party does not win, it might still be able to extract concessions – in the form of state resources – from the winner by offering to extend support for a coalition government, or by threatening to ally with a rival next time (Chandra, 2004, 2009). Indeed, there is ample empirical evidence of co-ethnic favoritism by party leaders in the distribution of state jobs, government contracts, police protection, or development investments in India (see, for example, Palshikar, 1994; Pai and Singh, 1997; Pai, 2004; Michelutti, 2007; Witsoe, 2009, 2011a; Gundimeda, 2013; Michelutti and Heath, 2013). By the same token, once a rival ethnic groups gains control of the government, groups often *lose* their privileged access to the state, with stark economic consequences for group members (Witsoe, 2009, 2011a; Michelutti and Heath, 2013; Palshikar and Deshpande, 2020). In addition to the material benefits at stake, the descriptive representation provided by co-ethnics in party leadership positions may bestow psychological benefits to members

[16] The identity of party leaders matters more than the identity of individual legislators in this context, since Indian parties are highly centralized and control over resources is exercised by small teams of strategic decision-makers, usually at the state level (Chandra, 2000, 2004; Chandra and García-Ponce, 2019).

of incorporated ethnic groups, particularly for historically subaltern groups (Pai and Singh, 1997; Chandra, 2004; Michelutti, 2007; Witsoe, 2011b; Chauchard, 2014; Ahuja, 2019).

Clientelist parties in India have targeted groups along a number of ethnic dimensions, including caste, religion, or tribe (Chandra, 2004). The number and types of ethnic groups incorporated into the leadership of mainstream political parties has expanded over the years, with considerable temporal and spatial variation. Immediately post-independence, the leadership of most political parties in India was dominated by a small elite of upper-caste Hindus, particularly Brahmins, but also other locally dominant castes such as Rajputs (Chandra, 2004; Jaffrelot, 2003; Witsoe, 2009). Over time, more ethnic groups were incorporated into political parties, particularly when their socioeconomic status rose and they were locally numerous.[17] However, the timing and extent of the incorporation of new groups varied across states. Maharashtra in the West and Tamil Nadu in the South, for example, were the sites of early and important social empowerment movements for subaltern groups. Accordingly, members of the Scheduled Castes, or Dalits,[18] as well as members of the Other Backward Classes (OBCs)[19] were incorporated into the leadership of important political parties in these states earlier than across North Indian states (Zelliot, 1970; Jaffrelot, 2003, p. 153-166; Ahuja, 2019).

Ethnic groups that are represented in the leadership of clientelist parties, then, have the

[17] That was the case, for example, for Dalits, who were meaningfully incorporated into mainstream political parties once constitutionally mandated affirmative action led to the emergence of an educated, high-income elite (Chandra, 2004), or for Yadavs, who were politically incorporated once the Green Revolution improved the economic standing of this traditionally farming caste group (Jaffrelot, 2003; Witsoe, 2016).

[18] Members of the Scheduled Castes, also called Dalits, have historically been severely marginalized. Thought to be outside the traditional caste hierarchy, Dalits were treated as "unclean" and polluting to caste Hindus, and consequently pushed into frowned-upon and unsafe professions, such as scavenging and the disposal of animal cadavers, and spatial segregation within villages and towns (see, for example, Pande, 2003; Chandra, 2004; Dube, 2005). Dalits comprise about 16 percent of the total population in India. Scheduled Castes are listed in special schedules of the Indian constitution, and enjoy special protections and affirmative action, including political reservations in national and state parliaments, government jobs, and educational institutions.

[19] OBCs describes members of groups that are not upper-caste, but still members of the "clean" castes in the traditional Indian caste system. They rank between the upper castes and middle castes on the one hand, and the Scheduled Castes (or Dalits) on the other. There are no exact numbers, but estimates put their share of the population at around 50 percent. While the official term is Other Backward Classes, membership is actually defined for caste groups, not based on income. Accordingly, groups belonging to the OBCs may vary widely on socioeconomic indicators and the social status they have traditionally held.

highest stakes in elections: they stand to gain or lose access to the state. According to my theory, there should be a straightforward relationship between the number of groups that are incorporated into clientelist parties' leadership and the gender turnout gap. Each group that has a co-ethnic in a party leadership position will receive credible signals about post-election resource sharing by that party. Incorporated groups, therefore, should expect high returns to a vote, and consequently be most likely to support female turnout. Conversely, groups that are *not* incorporated into any party ("unincorporated voters") have no party with co-ethnics in leadership in the running. No party can make a credible commitment to share resources after the election to these groups, and voters will likely only expect minor clientelist rewards in exchange for their vote. These groups should therefore be less likely to support female turnout beyond the participation rates that women's individual resources allow. Because women, on average, have lower resource endowments, gender turnout gaps will ensue.[20] Therefore, the higher the number of groups that are incorporated into ethnic clientelist parties, the higher the share of the population that supports female turnout and, ultimately, the smaller the gender turnout gap. This leads to my first testable hypothesis:
Hypothesis 1: The gender turnout gap will be smaller where more ethnic groups are incorporated into clientelist parties.

However, whenever several distinct ethnic groups share in a party's leadership, dispute about the relative size of the pie is possible, if not inevitable. For example, only one individual can become the head of government; accordingly, only one group's members can receive the material and psychological benefits that go along with that. Which group gets which share in the spoils might therefore not be clear to voters before an election. Uncertainty should be particularly pronounced for traditionally subaltern groups, who might fear that traditional status hierarchies will replicate within parties, giving traditionally dominant groups the lion's share of resources and leaving historically marginalized groups with a smaller share (Chandra

[20] To be clear, I am not arguing that unincorporated groups have any incentives to *constrain* women's voting either. But because returns to a vote are low for these groups, they have little incentive to bridge the resource gap for women to enable their participation beyond levels predicted by individual-level resources.

and García-Ponce, 2019). Consequently, parties that form broad ethnic coalitions might be sending weaker signals than those who draw their leadership from a single ethnic group or ethnic category. I term *narrow parties* those that rely on a single ethnic group or ethnic subcategory; while *broad parties* are those that diversify their leadership both within a given year and/or over time. Parties have potentially countervailing incentives when deciding how narrow or broad a coalition to form: they want to be as broad as necessary to win elections; but as narrow as possible to maximize rent extraction (Chandra, 2004). This leads me to the second testable hypothesis:

Hypothesis 2: The gender turnout gap will be smaller where more narrow parties compete.

4 Data

In order to test my theory, I assembled a novel panel data set that codes the number of ethnic groups targeted for high-return clientelism at the constituency level for all Assembly elections between 1977 and 2007 in two major North Indian states, namely Uttar Pradesh and Madhya Pradesh. To do this, I identified all state-level decision-makers for all major parties and determined their salient ethnic identity before each election, based on contemporaneous newspaper articles. I then checked which parties ran in which constituencies in any given election year to calculate the total number of targeted ethnic groups in each constituency for each election. To my knowledge, this is the first data set with longitudinal data on parties' state leadership across all major parties in more than one state.[21] I complement this dataset with official turnout and demographic and economic data assembled from administrative sources.

The full dataset contains information for 6482 constituency-year observations; 4412 for

[21] Chandra (2004) traced the composition of the BSP's leadership over time across several states; while Chandra and García-Ponce (2019) coded state-level leaders for all parties and states for the state elections held between 1977 and 1980. I build on this work by coding both leadership positions and ethnicity for all major parties, similar to Chandra and García-Ponce (2019), while tracing cross-party changes over time in multiple states like Chandra (2004) did.

Uttar Pradesh and 2070 for Madhya Pradesh.[22] However, because constituencies were delimitated in 2008, I have to perform the analysis separately for elections held between 1977 and 2007, and those held from 2008 onwards. Furthermore, potentially relevant covariates are only available at the constituency level for the years 1991 through 2007. I therefore perform all analyses on 3 distinct datasets: one spanning the years 1977 through 2007 containing all dependent and independent variables (N=5227); one spanning the years 2008 through 2017 containing all dependent and independent variables (N=1025); and one spanning the years 1991 through 2007 that includes, besides the relevant dependent and independent variables, a number of covariates (N=2695).

4.1 Data on Incorporated Ethnic Groups

Information on the leadership structure and ethnic composition of parties is hand-coded, based mostly on research in newspaper archives of English-language Indian dailies, primarily *The Times of India*, *The Hindustan Times*, and *The Hindu*, and, where available, on secondary literature on parties.

My theory relies on being able to identify first, parties' leadership; and second the salient ethnic groups that party leaders belong to. I follow Chandra and García-Ponce (2019)'s lead in coding the state-level leadership of parties as well as their ethnic identities. State-level leadership here means the small team of individuals who take strategic decisions for a party, including on the nomination of candidates, electoral alliances, and government formation. I focus on this leadership team because, empirically, they are also the ones with the most say over the distribution of state resources (Chandra, 2004; Wilkinson, 2007; Witsoe, 2009). For each state election year, I tried to identify all strategic decision makers based on historical newspaper articles as well as secondary sources, where available. These key leaders usually include the holder of the highest office in the party's state unit (which is

[22] Between 1977 and 2017, Uttar Pradesh held 11 elections, while Madhya Pradesh held 9. Each state called several snap elections at various points during this time period, leading to the different number of elections held.

most often the president, but for some parties is the general secretary of the state unit); the official candidate for the post of Chief Minister, if the party has named one; and any sitting Chief Minister, if the party has one. These are formal power holders: their formal position grants them influence on important strategic decisions for the party. But key leaders can also include individuals whose formal positions in and of themselves do not grant exuberant power, but who nevertheless influence key decisions within the state-level party ahead of elections. These informal power holders may wield just as much influence as official office holders, and might just as visibly be associated with a party. I identify them by scouring archives for reports on those who factually took the major strategic decisions for the party in any given election year, independent of their formal position.[23]

After identifying all state leaders, I coded each decision maker's salient ethnic identities for each year, where *salient* refers to identities that were discussed – implicitly or explicitly – in newspaper articles at the time of the election. Explicit mentions of a salient ethnic identity include any direct references to the leader's identity in newspaper articles preceding the election, such as the leader belonging "to X community" or the leader being "an X." An example of an explicit mention would be the *Times of India* calling Charan Singh "the most eminent Jat" in Indian politics.[24] Implicit mentions include those where a leader's ethnic identity is obvious from their name *and* the identity group is being discussed in the context of a party and/or election. For example, Mulayam Singh Yadav belongs to the Yadav caste, which is obvious based on his name; newspapers will therefore be less likely to identify him as "a Yadav" or "a member of the Yadav caste," since it is implied in the Indian context. However, not every time a name contains a caste identifier that identity

[23] For example, in the Madhya Pradesh state unit of the Bharatiya Janata Party (BJP) in the 1993, two former Chief Ministers, Sunderlal Patwa and Kailash Chandra Joshi, were important strategic decision makers: both heavily affected the selection of candidates, for example, with Joshi trying to get as many as 90 of his supporters nominated; and they each tried to have a supporter elected as state unit president (with Patwa coming out on top). They were also constantly mentioned in newspaper articles and tipped as potential candidates for the post of Chief Minister, should the party win. However, neither held the office of state unit president; and neither was put forth as an official candidate for Chief Minister. Formally, then, Patwa and Joshi held no power; but informally, they affected many important strategic decisions for the Madhya Pradesh BJP.

[24] *The Times of India*, February 6, 1969, "BKD well placed in West U.P."

is automatically salient; I only count the identity as salient if newspapers *also* discuss the caste group explicitly. For example, if newspapers identify "the Yadavs" as an important electorate for Mulayam Singh Yadav's party, then I will consider the Yadav identity salient even if he himself is not identified as "a Yadav" explicitly.[25]

Because of the sheer number of parties contesting elections in some years, and the very limited vote share many of them attain, I limit my analysis to all viable parties. I define a viable party as one that came at least second in at least one constituency in a given state. I code all those parties as not targeting any ethnic groups for whom I was unable to identify a distinct state-level leader and/or whose leader's ethnic identity was not salient at the time (i.e., not discussed, explicitly or implicitly, in news media at the time); these parties, therefore, are considered not to be sending strong signals about clientelist resource distribution to any specific group. Since both state-level leadership as well as the salience and classification of ethnic identities can change over time, the coding for the same party, or even the same individual, might vary between elections. In total, I coded a total of 216 year-party observations for their leadership, and 194 individual politicians for their ethnic identity. Appendix 6 provides a more detailed discussion of the coding, including the rules I followed for identifying state-level leaders and their ethnic identities, as well as examples of coded parties and leaders.

4.2 Other Data

Most election-related data are collected from official election reports published by the Election Commission of India (ECI). I scraped election reports to collect the number of registered electors by gender as well as the number of voters who turned out by gender to calculate male and female turnout as well as the gender turnout gap. Some electoral results, including vote shares for each candidate/party and measures of electoral competition, come from the Trivedi Centre for Political Data's Lok Dhaba database (Agarwal et al., 2021), which in

[25] *The Times of India*, January 8, 1991, "BJP striding ahead in U.P."

turn was collected from ECI reports. These data are published at the level of the Assembly Constituency.

Most covariates that might be important for the analysis, including variables related to female labor force participation (FLFP), female literacy, and economic development, come from the decennial Indian census. I rely on village level data published by the census office, and aggregate it up to the level of the Assembly Constituency using village-to-constituency assignment keys that were created by Sam Asher and Paul Novosad at the Development Data Lab as part of the Socioeconomic High-Resolution Rural-Urban Geographic Dataset on India (SHRUG) (Asher et al., 2019, 2021).[26]

4.3 Variables and Measurement

Dependent Variable. My main dependent variable is the **gender turnout gap**, which is the difference between male and female turnout rates (male turnout - female turnout), in percentage points.

Independent Variables. To test the hypotheses put forth above, I construct two distinct independent variables. First, to test Hypothesis 1, I calculate the **number of targeted ethnic groups** at the constituency level. Targeting happens at two levels: first, at the state level, parties determine the ethnic composition of their state-level leadership; this is constant across the entire state for any given election. Second, at the constituency level, parties decide whether to run or not. Many smaller parties lack the resources and candidates to run in all constituencies of a state; even larger parties may decide against running in certain constituencies, either because they do not find viable candidates or because they enter into electoral alliances that leave certain constituencies to the alliance partner. The number of targeted ethnic groups, therefore, varies by constituency in any given state election; as well as over time.

To calculate the number of targeted ethnic groups at the constituency level, I first identify,

[26] I would like to thank Paul Novosad who generously shared these linking keys with me for the purpose of this research.

for each state-election year, the distinct ethnic groups represented in each party's leadership. Then, for each constituency, I check which parties ran in said constituency in each election year. Based on the number of ethnic groups incorporated into each party and the specific parties running in a constituency, I then calculate the number of all distinct groups incorporated into all parties that compete in said constituency.[27]

Second, to test Hypothesis 2, I identify **narrow parties**, that is, those parties where the entire leadership belongs to a single ethnic group or category. For the most part, that includes parties that are so small or so centralized that only a single individual takes all strategic decisions for the party; but it also applies to those parties with several leaders that all belong to the same group.

Covariates. The literature proposes several predictors of the gender turnout gap that any complete model should take into account. The first is **economic development**, where higher economic development should correlate with smaller gender turnout gaps. I use three different measures of economic development, based on census data, in my analysis: first, the share of the population living in areas classified as **urban** by the census; second, the share of **men working in agriculture**; and third, the share of male workers who are only **marginal workers**, i.e., who worked less than 6 out of the past 12 months.[28] Another predictor of the gender turnout gap found in the literature is female labor force participation (FLFP). I collected data on the **FLFP rate**, or the share of women who are engaged in any kind of work for cash or kind. The third predictor of the gender turnout gap based on the literature is female education. I use **female literacy** as a measure of a minimal level of education that might make women more likely to participate politically. The Indian census counts as literate anyone aged 7 or older who can read and write in any language, independent of

[27] In an ideal world, I would like to measure not just the number of ethnic groups that are incorporated, but their proportion in the population. However, the Indian census (as well as representative surveys) do not publish fine-grained information on the strength of ethnic groups besides the constitutionally protected categories of Dalits and Adivasis. Even data on the size of religious groups is available only at the district level, not the village level. Other caste data is not available at all. (The last time the Indian census collected detailed caste information was in 1931.)

[28] The perhaps most straightforward measure of economic development, GDP per capita, is only available at the state level, and not at the constituency level.

formal education.[29]

4.4 Descriptives

My dataset contains information for all constituencies across state-level elections between 1977 and 2017 in Uttar Pradesh and Madhya Pradesh, two major neighboring states in North and Central India. At the time of the last census in 2011, Uttar Pradesh had a population of almost 200 million, while Madhya Pradesh was home to more than 72 million people. Together, they account for about 22.5 percent of India's total population. Each state elects their *Legislative Assembly* (state assembly) every five years; Uttar Pradesh's state legislature has 403 seats, while Madhya Pradesh's has 230.

These two states are similar in many respects, making them well-suited for a comparison. For one, they are similar in regard to socioeconomic development, in that they are both among the poorest and least developed states in the country. Madhya Pradesh and Uttar Pradesh are roughly comparable in terms of State Gross Domestic Product (GSDP) per capita, measured in INR, as well as female literacy rates over the period of my analysis. However, Madhya Pradesh has consistently outperformed Uttar Pradesh in terms of female labor force participation rates, with Madhya Pradesh's FLFP rates ranging about 10 percentage points higher than Uttar Pradesh's (see Appendix Figure A.3). In addition, the ethnic makeup of both states is comparable in that both have similar shares of traditional elite and historically subaltern groups.[30] Finally, recent research has shown that in both states, household support can indeed decouple female turnout from individual-level resource endowments and close the gender turnout gap (Prillaman, 2023; Roscher, 2023).

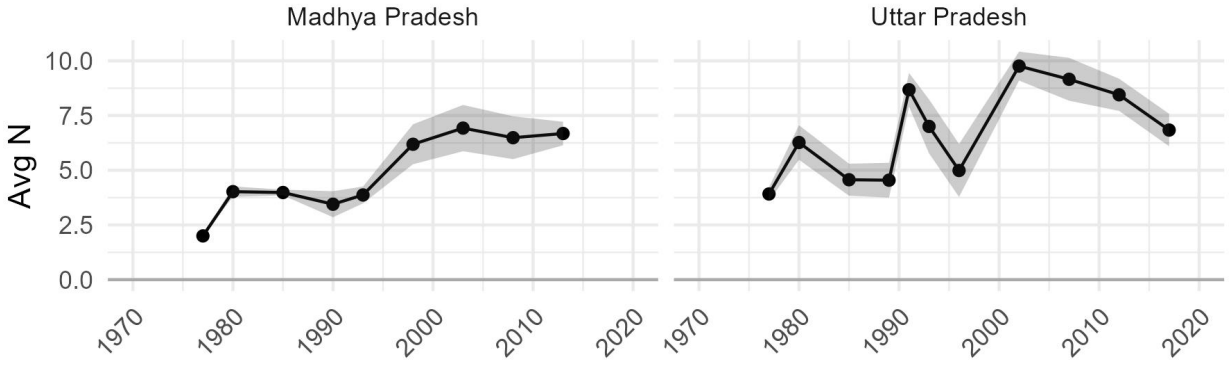
Importantly for my analysis, however, these two states differ in one important point, namely the timing of the incorporation of different ethnic groups (see Figure 3). Uttar

[29] See Appendix for further details.

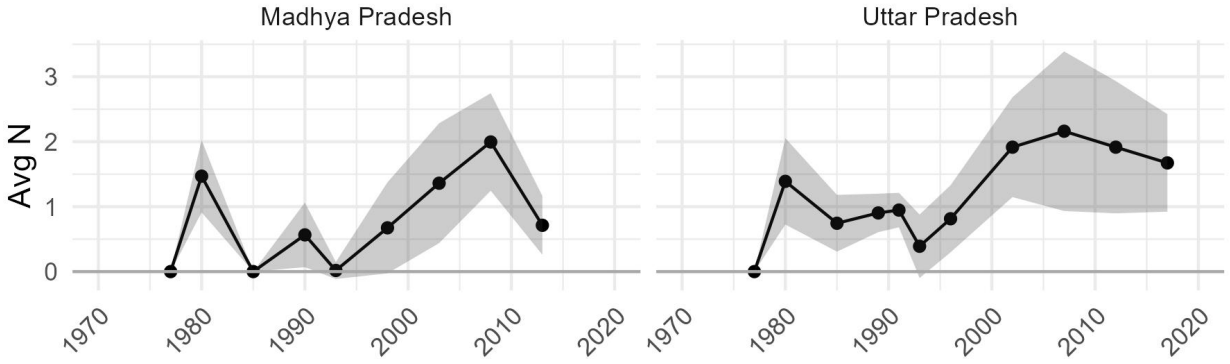
[30] Based on the 2012 India Human Development Survey, about 18 percent of the population in both states identifies as belonging to the forward castes, or upper castes. About 40 percent of the population in Madhya Pradesh and a little over 38 percent in Uttar Pradesh are members of the Other Backward Classes. According to the last census, around 16 percent of Madhya Pradesh's population and 20 percent of Uttar Pradesh's population belong to the Scheduled Castes.

Figure 3: Average Number of Ethnic Groups Targeted, Average Number of Narrow Parties, and Average Gender Turnout Gap at the Constituency Level, by State

(a) Number of Ethnic Groups Targeted



(b) Number of Narrow Parties



(c) Gender Turnout Gap

Note: Shaded areas represent one standard deviation.

Pradesh has seen more ethnic groups incorporated earlier, with subaltern groups being incorporated as early as the 1980s. This increased political incorporation has come with a proliferation of small, *narrow parties* that mainly incorporate one ethnic group or category. By contrast, Madhya Pradesh has a relatively stable two-party system, and both parties were dominated much longer by traditional elites before starting to incorporate more subaltern groups only in the 2000s. Visual inspection of Figure 3 suggests that, in line with my theory, Uttar Pradesh has on average witnessed higher ethnic incorporation, more narrow parties, and smaller gender turnout gaps than Madhya Pradesh.

5 Analysis

To test the hypotheses, I estimate the following model

$$\Delta Y_{it} = \alpha + \beta \Delta X_{it} + \gamma \Delta Z_{it} + ACFE + \epsilon, \tag{2}$$

where ΔY_{it} is the election-on-election change in the gender turnout gap in unit i at time t; ΔX_{it} is the election-on-election change in the independent variable of interest in unit i at time t; ΔZ_{it} is change in a vector of covariates; and FE are unit-level fixed effects. Note that for all independent variables related to ethnic parties, X_i is actually measured *before* the election, that is, before Y_{it} is determined and measured. I therefore do not lag these independent variables further, as they already precede the dependent variable. The sequencing provides confidence that it is indeed clientelist targeting of ethnic groups that drives the gender turnout gap and not vice versa. By looking at election-on-election change instead of levels, I show that the dependent variable does not just correlate in expected ways with the independent variable, but move in the expected direction based on changes in the independent variable.

5.1 Results

Hypothesis 1 states that the gender turnout gap will drop as the number of incorporated groups grows. To test this hypothesis, I regress the election-on-election change in the gender turnout gap on change in the number of distinct groups targeted by parties competing in any given constituency. Table 1 presents results for fixed-effects regressions. I estimate 3 different models: Column (1) shows results for a simple fixed-effects model for the full panel data, spanning all elections from 1977 through 2007; column (2) restricts the analysis to the years for which I have covariates at the constituency level, i.e., 1991 through 2007; and column (3) adds AC-level covariates to the analysis for the limited panel (1991-2007). All models include AC-level fixed effects, meaning coefficients represent the within-constituency change in the gender turnout gap associated with a change in the independent variable. Throughout, the coefficient on my independent variable of interest – change in the number of ethnic groups incorporated – is negative and statistically significant at the .01-level. In the model with covariates (column (3)), an increase in the number of incorporated groups by 1 from one election to the next is associated with a drop of close to .4 percentage points in the gender turnout gap in a constituency. These findings are compatible with Hypothesis 1: An increase in the number of distinct ethnic groups targeted by clientelist parties *before elections* is associated with a drop in the gender turnout gap in the following election at the constituency level.

Economic development figures into the model in predictable ways. In the full model (column (3)), a 1-percentage-point increase in the share of the population classified as urban – the proxy for economic development – correlates with an increase in the gender turnout gap of close to .2 percentage points; the results are statistically significant at the .1-level. In other words, the better developed, i.e. more urban, a constituency is, the *larger* the difference between male and female participation rates. This is in line with the theory: clientelist mobilization should be more important to the poor and, therefore, the rural population since the poor are more dependent on the state and should therefore be more receptive

Table 1: FE Regression Results, Targeted Ethnic Groups

	Dependent variable:		
	Δ Gender Turnout Gap		
	1977-2007	1991-2007	1991-2007
	(1)	(2)	(3)
Δ N Targeted Groups	−0.333***	−0.320**	−0.403**
	(0.100)	(0.139)	(0.160)
Δ Share Urban			0.170*
			(0.094)
Δ Gender Gap LFP			0.472***
			(0.149)
Δ Gender Gap Literacy			0.159
			(0.293)
Constituency FEs	✓	✓	✓
Observations	4,532	1,958	1,498

Note: *p<0.1; **p<0.05; ***p<0.01
Clustered robust standard errors in parentheses.

to promises of future preferential access to state resources (Stokes et al., 2014). At the same time, state programs for rural populations in India are more common and visible than those in urban areas, leading rural populations to have higher aspirations for state access (Auerbach and Kruks-Wisner, 2020).

Results also show that in the full model (column (3)), a 1-percentage-point rise in the gender gap in LFP is also associated with a .4-percentage-point increase in the gender turnout gap. This is in line with traditional theories of female political participation that posit higher female employment leads to higher turnout among women; conversely, we would expect a higher gender gap in employment to correlate with larger gender gaps in political participation. The fact that the coefficient on targeted groups is still statistically significant suggests that clientelist mobilization can be an additional driver of female turnout, particularly when women's employment is low overall, as is the case in India. Literacy rates, by contrast, are not associated with the gender turnout gap in my model.

Next, Hypothesis 2 posits that the gender turnout gap will decline as the number of

Table 2: FE Regression Results, Narrow Parties

	Dependent variable:		
	Δ Gender Turnout Gap		
	1977-2007	1991-2007	1991-2007
	(1)	(2)	(3)
Δ N Narrow Parties	−0.743***	−1.152***	−0.970***
	(0.146)	(0.246)	(0.271)
Δ Share Urban			0.109
			(0.098)
Δ Gender Gap LFP			0.557***
			(0.129)
Δ Gender Gap Literacy			0.171
			(0.275)
Constituency FEs	✓	✓	✓
Observations	4,532	1,958	1,498

Note: *p<0.1; **p<0.05; ***p<0.01
Clustered robust standard errors in parentheses.

narrow parties competing increases, that is, those parties that draw their entire leadership from a single ethnic group or ethnic category. As before, I estimate a fixed-effects model that regresses change in the gender turnout gap on change in the independent variable, i.e., change in the number of narrow parties. Table 2 shows results, for the simple model on the full panel from 1977 to 2007 (column (1)), the simple model on the truncated panel from 1991 to 2007 (column (2)), and the full model on the truncated panel (column (3)). Growth in the number of narrow parties indeed is associated with a shrinking of the gender turnout gap across all models; the coefficient is substantially large and statistically significant at the .01-level. In the full model that includes covariates (column (3)), an increase by 1 in the number of narrow parties means a decrease in the gender turnout gap of more than 1 percentage point. An election-on-election rise in the number of narrow parties, in which the distribution of resources and status improvements should be most straightforward and that therefore provide the highest returns to a vote, leads to a closing of the gender turnout gap, in line with Hypothesis 2.

As before, the gender gap in labor force participation is positively correlated with the gender turnout gap: a 1-percentage-point increase in the difference between male and female employment is associated with a rise in the gender turnout gap of about half a percentage point. Neither economic development nor literacy rates, however, are statistically significantly correlated with participation rates in this model.

As expected, both the number of ethnic groups incorporated and the number of narrow parties affects the gender turnout gap by improving female turnout relatively more than male turnout.

5.2 Alternative Explanations

I consider two alternative explanations for the closing of the gender turnout gap proposed by recent research on India, namely reservations for women in local government; and male labor migration. Reservations for women in institutions of local governance have shown to dramatically increase women's political participation in government at the town and ward level (see, for example, Chattopadhyay and Duflo, 2004; Beaman et al., 2009; Deininger et al., 2011; Parthasarathy et al., 2019; Brulé, 2020). There is also evidence to suggest that higher female representation in local councils, and particularly in the position of council president, might entice women to vote in higher numbers in local elections (Brulé, 2020; Goyal, 2023). Goyal (2023) posits that the introduction of quotas at the local level has had a bottom-up mobilization effect on women that spilled over into state and national elections over time in Delhi. While I cannot test this directly with my data[31] I argue that there are two reasons why quotas *cannot* convincingly explain the within-state and across state *patterns* of rising female turnout across states and time in my data. First, because quotas are randomly assigned within states, they cannot explain *within-state* regional variation in the timing and extent of the closing of the gender turnout gap that are empirically evident. For example,

[31] Quotas are assigned at the village or ward level. My data, by contrast, is at the constituency level. Each constituency contain hundreds of villages and/or wards. In as much as quotas are assigned quasi-randomly every cycle, each constituency should, in expectation, have the same number of reserved and unreserved seats.

Appendix Figure A.4 visualizes spatial patterns in the closing of the gender turnout gap in Uttar Pradesh, where the gap started closing much earlier, and diminished much more rapidly, in the eastern part of the state than in the western part. Yet as many seats were reserved in eastern Uttar Pradesh as they were in western Uttar Pradesh. Second, quotas cannot explain *across-state* variation in the decline in the gender turnout gap. Reservations were made mandatory across all of India in 1993. As Brulé (2020) documents, most states took action immediately, holding the first elections under the new rules within two to three years (p. 135). Yet, despite having introduced local-level quotas at nearly the same time and to the exact same extent, these states registered very different trajectories in the gender turnout gap in state elections (see Appendix Figure A.5).

Finally, I consider male-only labor migration. Prior research has found that migration might affect turnout in India: because migrants often do not transfer their voter registration to their new location, they would need to travel to their original home locality to vote; not all migrants can or want to do that (Carswell and Neve, 2014; Kumar and Banerjee, 2017; Gaikwad and Nellis, 2021). Because semi-permanent labor migration in India is mostly male – with young men migrating for work, while their wives and children stay in their home village or town (Tumbe, 2018) – this is likely to particularly depress men's voter participation compared to women's. If women, in the absence of men, take over some of the tasks traditionally performed by male household members, such as agricultural work (Pattnaik et al., 2018) or approaching bureaucrats in order to access government schemes (Kumar, 2021), they might also "step in" as voters, to replace the male vote that the household is (temporarily) missing but that still might be required to access state benefits in a highly clientelist setting.

I test this using district-level data on the absence (or surplus) of married men relative to married women. The census collects data on all *currently married* individuals – i.e., those who currently have a living spouse and are not separated or divorced – present in a certain location at the time of the census survey. Any difference in the number of married women

and married men suggests a temporary absence of one spouse. Because women are more likely to stay back in origin communities while men are the ones migrating for employment (Tumbe, 2018), I use the share of of missing (or excess) married men relative to the number of married women to capture temporary migration patterns. The data is at the 1991-district level, and available from 1991 through 2011. As Table 3 shows, an increase in the share of male outmigration in a district actually corresponds to a *rise* in the gender turnout gap (columns (1) and (2)). That means that as more men leave the district for temporary work in other places, the gender gap in turnout becomes larger, i.e., men's advantage in turnout grows. This relationship is, in fact, driven by a reduction in female turnout when men leave: a 1-percentage-point election-on-election increase in the share of married men migrating away is associated with a reduction in female turnout of about 2.4 percentage points in the simple model (column (3)) and 1.9 percentage points in the model with covariates (column (4)). By contrast, when looking at only male turnout, I find no statistically significant correlation. Male labor migration, then, does not seem to be driving the closing of the gender turnout gap.[32]

5.3 Robustness Checks

I perform a number of robustness checks to increase confidence in my analysis and conclusions. First, because I used Uttar Pradesh as an in-depth case study while developing my theory, I perform all tests on the pure out-of-sample data as well, i.e., for Madhya Pradesh alone.[33] Results are qualitatively unchanged: coefficients are statistically significant and substantially large throughout these specifications as well (Appendix Tables A.2 and A.3).

Second, I ensure that results are not driven by the choice of covariates, the time frame under investigation, or the level of analysis. Using alternative covariate specifications at the

[32] This finding makes sense given that research suggests that while the *absolute number* of labor migrants has increased in India over the past two decades, the *relative share* of labor migrants provided by different regions in India has been relatively stable since the early 20th century (Tumbe, 2018).

[33] Uttar Pradesh is where much of my field research happened, and where I conducted the main household survey; it is also the state that most heavily informed my theory, and the first state for which I coded party leadership.

Table 3: FE Regression Results, District-Level Migration

	Dependent variable:					
	Δ Gender Turnout Gap		Δ Female Turnout		Δ Male Turnout	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Male Outmigration	1.724***	1.019**	−2.439***	−1.906**	−0.714	−0.887
	(0.494)	(0.514)	(0.840)	(0.830)	(0.834)	(0.857)
Δ Share Urban		0.002		0.0005		0.002***
		(0.002)		(0.002)		(0.001)
Δ Gender Gap LFP		0.003*		−0.003		0.0003
		(0.002)		(0.002)		(0.002)
Δ Gender Gap Literacy		0.008***		−0.005**		0.003
		(0.002)		(0.002)		(0.002)
District FEs	✓	✓	✓	✓	✓	✓
Observations	298	298	298	298	298	298
R^2	0.029	0.105	0.037	0.057	0.003	0.011
Adjusted R^2	-0.353	-0.265	-0.343	-0.333	-0.391	-0.399
F Statistic	6.459**	6.182***	8.117***	3.202**	0.578	0.564

Note: *p<0.1; **p<0.05; ***p<0.01
Clustered robust standard errors in parentheses.

constituency does not change results. Substituting the share of marginal workers among all male workers as a measure of economic development, or using female labor force participation rates and female literacy instead of the gender gap in employment and literacy does not alter the main findings: the coefficient of interest is still negative, statistically significant and substantially large (Appendix Tables A.4 and A.5).

To ensure that results are not driven by the time frame under investigation, I also run all tests using a district-level panel, which spans all elections from 1981 through 2011 and provides more census-based covariates. This panel therefore slightly expands the time frame under investigation. It also includes more measures of economic performance. I run two distinct models, one using share of the population classified as urban and another using the share of the population that is engaged in non-farm work – i.e., that works neither as agricultural laborers nor as cultivators – to proxy for the level of economic development in a district. The results, shown in Appendix Tables A.6 and A.7, are similar to the main

analysis. The coefficient of interest is still signed as expected and statistically significant across all specifications. The district-level analysis reveals an interesting finding with regard to economic development though: while a higher share of the district population being classified as urban corresponds to smaller gender turnout gaps, a higher share of non-farm employment *independent of urban status* actually is correlated with *higher* gender turnout gaps. This makes sense in the context of my theory: highly urbanized localities are less likely to see high levels of clientelism (Wilkinson, 2007). My own work, as well as the IHDS and other surveys, also show that women's individual-level resource endowments are higher in highly urban settings than in more rural settings. In urban localities, therefore, turnout should more likely be an individual-level decision, and the gender gap in resource endowments should be smaller, leading to smaller gender gaps in turnout. By contrast, in rural localities, where clientelism is likely to be more prevalent, turnout is more likely to be a household-level decision, with households potentially bridging the resource gap for women. However, higher economic development makes households less dependent on the state, and therefore should make them less likely to support female turnout, leading to *larger* gender turnout gaps where economic development is higher.

Finally, I use data at the state level for all elections from 1977 through 2017, expanding the time frame under consideration even further. Even at the state level, and using State Gross Domestic Product as an alternative measure of economic development, the relationship between the number of ethnic groups incorporated and the gender turnout gap holds. Appendix Table A.8 shows that results are qualitatively unchanged, with an increase in the number of incorporated groups leading to a decrease in the gender turnout gap within states.

6 Conclusion

When do women participate equally in the electoral process? Many developing countries register high female turnout rates, at par with or above those of men, despite low levels

of economic development, low female labor force participation rates, and large gender disparities in individual-level resources. While the traditional literature on women's political participation is essentially resource-based, I argue that there is a second path to closing the gender turnout gap: one that relies on household support for female turnout, driven by high returns to a vote. I argue that even in the absence of individual resources – such as income, education or political knowledge – women can participate at equal rates to men, under two conditions: when individuals are highly socially and economically dependent on the household, so as to force household-level coordination; and when returns to a vote are high. Where households play an important role in indiviudals' lives as sites of welfare improvement, risk mitigation, and social status generation, they can affect a host of behaviors traditionally modeled as individual-level decisions. Supportive households can bridge the resource gap for women and enable female political participation even in the absence of individual-level resources and in the face of high social costs to women's participation in public life. But only when returns to a vote are very high will households be supportive of women voting. By contrast, where returns to a vote are low and the costs of women's participation high, households are likely to restrict female turnout.

But when will returns to a vote be high enough to generate household support? Clientelism, I argue, can produce these high returns to an individual vote, although not all clientelist settings are equally conducive to it. I develop a typology that classifies clientelist strategies by the value it provides for potential voters, based on the sequencing of the clientelist exchange and the source of the clientelist resources. I posit that returns to a vote should be highest where a) parties share resources only *after* an election as opposed to handing out goodies before election day; and b) the resources at stake are state resources, in the form of government employment or welfare programs, as opposed to private funds.

I then provide evidence in line with this theory from India, a "hard case" for gender turnout parity, where some of the poorest, but not the wealthiest, states closed the gender turnout gap, while female labor force participation rates have been falling. In India, ethnic

clientelism is prevalent, and those ethnic groups that have a co-ethnic in party leadership are most likely to get access to state resources after an election. Incorporated ethnic groups, therefore, have the highest returns to a vote and should be most likely to support female turnout. I assembled a novel panel dataset that codes the number and types of ethnic groups incorporated into political parties in two large North Indian states between 1977 and 2007. I find that the number of incorporated ethnic groups has gone up over time, but with considerable temporal variation. Using fixed-effect regressions, I show that both at the state and the constituency level, an increase in the number of ethnic groups incorporated before and election leads to a drop in the gender turnout gap.

My work therefore identifies a second path to closing the gender turnout gap that does not depend on women's resources, or aggregate levels of development and female labor force participation rates. Women can, if they have their household's support, participate even in the absence of resources traditionally associated with turnout. However, this raises important questions about women's agency in voting (see, for example, Prillaman, 2023; Roscher and Verma, 2024). If households support female turnout instrumentally, because the value of a vote is high, does that mean that women are mere puppets in the electoral process, used by men to amplify their voices? Or, conversely, do women exercise agency over their vote once they reach the polling station, given that the ballot is secret? In related work, I investigate women's agency in vote choice using original surveys and survey experiments. My research suggests that women are far from mere mouthpieces of their households, and that, contrary to expectations based on the literature, young women in particular are more likely to express agency over their own vote choice. This might hint at a type of socialization effect for young women who came of age at a time when women and men voted at equal rates. But more research is needed to understand if, and to what extent, the closing of the gender turnout gap in India has led to an actual, substantial political empowerment of previously excluded citizens.

The idea that women depend on their households for turnout, and that not all women

might exercise full agency over their vote, also raises questions about the consequences of these increases in household-supported turnout. Will political parties be responsive to women and their often distinctive political preferences (Inglehart and Norris, 2003; Chattopadhyay and Duflo, 2004; Khan, 2020; Gottlieb et al., 2018, see, for example,)? In Europe and North America, women's increasing electoral participation changed the scope of government spending and public employment, for example (Lott and Kenny, 1999; Iversen and Rosenbluth, 2006). In developing countries, women's increased representation in local government, and the subsequent rise in women's participation in local council meetings, has been shown to improve the provision of public goods that female constituents care about (Chattopadhyay and Duflo, 2004). However, if clientelist parties target the household – specifically the male head of household – with clientelist rewards for female turnout, women's particular demands on the state might not be met (Prillaman, 2023). If women subordinate their own preferences under the household's (Khan, 2021) or, taken to the extreme, are coerced to vote along with the household (Prillaman, 2023), parties might not even learn about women's preferences to begin with. Further research is needed to better understand the political consequences of household-supported female turnout.

Appendix A

Additional Figures and Tables

Table A.1: States and Election Years

State	Years
Madhya Pradesh	1977, 1980, 1985, 1990, 1993, 1998, 2003, 2008, 2013
Uttar Pradesh	1977, 1980, 1985, 1989, 1991, 1993, 1996, 2002, 2007, 2012, 2017

Figure A.1: Female Literacy Rates and the Gender Turnout Gap, by State

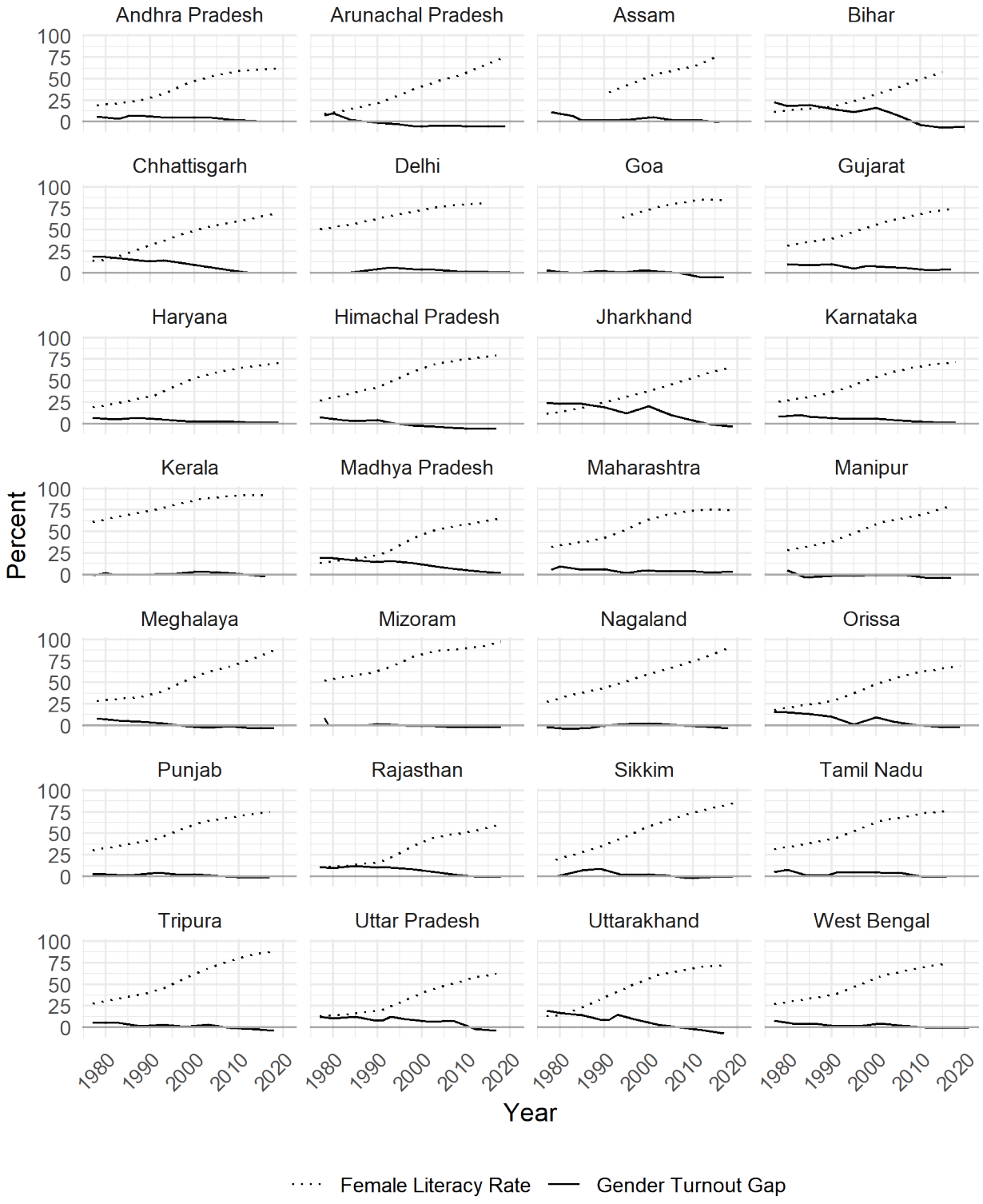


Figure A.2: Individual-Level Resource Endowments and Turnout, by Gender

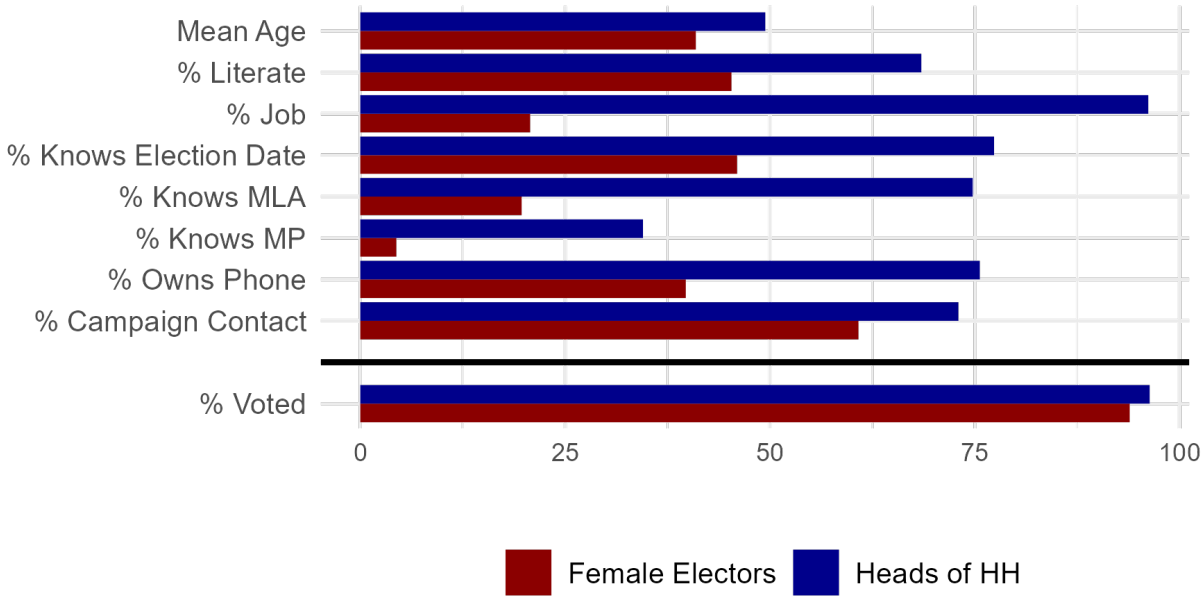
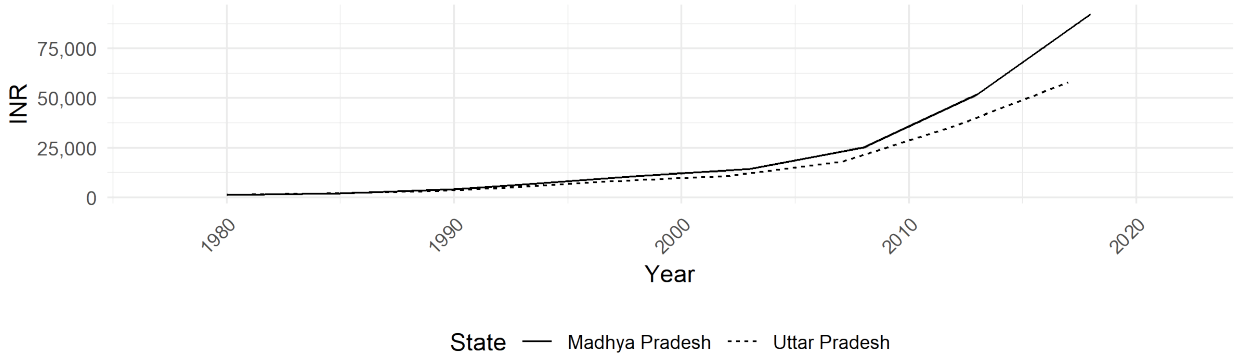
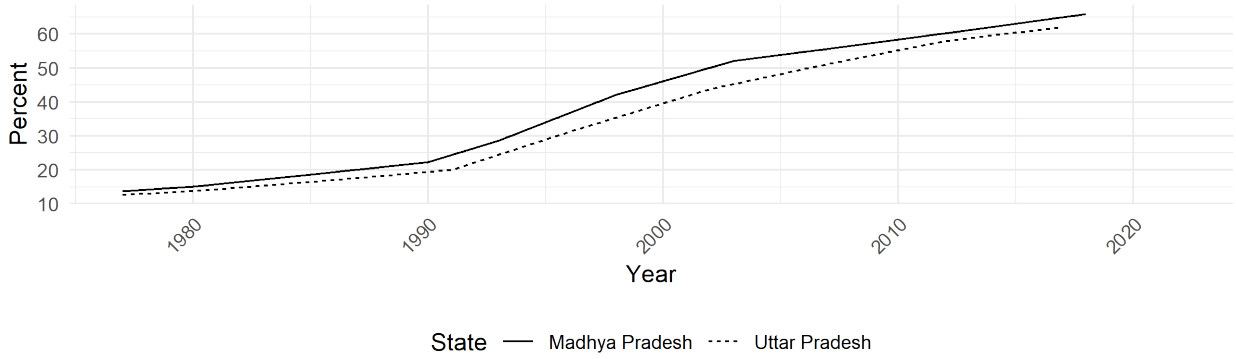


Figure A.3: Development, Literacy and FLFP Across States

(a) GSDP Per Capita



(b) Female Literacy Rates



(c) Female Labor Force Participation Rates

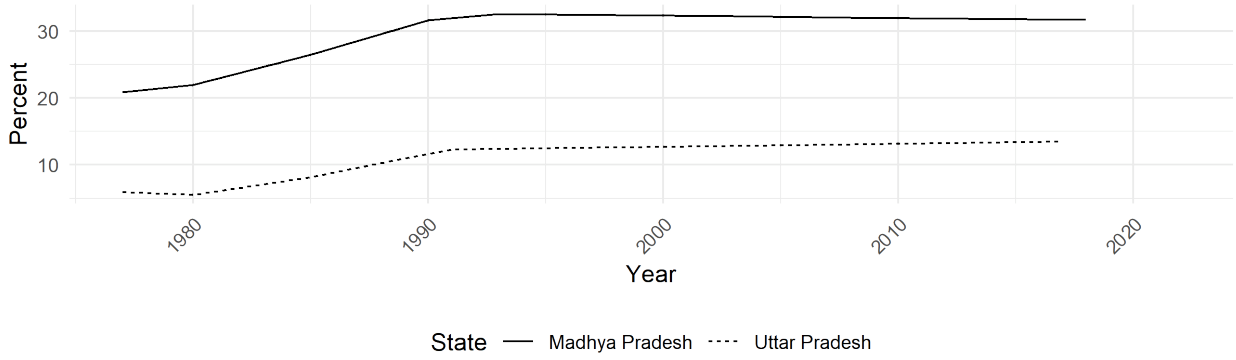


Table A.2: FE Regression Results Incorporated Groups, Excluding UP

	Dependent variable:		
	Δ Gender Turnout Gap		
	(1)	(2)	(3)
Δ N Groups Incorporated	−1.260***	−0.475***	−1.271***
	(0.121)	(0.140)	(0.297)
Δ Share Urban			0.044
			(0.089)
Δ Gender Gap LFP			0.030
			(0.173)
Δ Gender Gap Literacy			0.553*
			(0.299)
AC FEs	✓	✓	✓
Observations	1,380	513	342
R^2	0.109	0.033	0.240
Adjusted R^2	-0.070	-0.452	-0.552
F Statistic	139.959***	11.563***	13.178***

Note: *p<0.1; **p<0.05; ***p<0.01
Clustered robust standard errors in parentheses.

Table A.3: FE Regression Results Narrow Parties, Excluding UP

	Dependent variable:		
	Δ Gender Turnout Gap		
	(1)	(2)	(3)
Lagged Δ N Narrow Parties	−2.629***	−1.015***	−0.933**
	(0.232)	(0.297)	(0.383)
Δ Share Urban			0.232***
			(0.074)
Δ Gender Gap LFP			−0.240
			(0.195)
Δ Gender Gap Literacy			0.713**
			(0.316)
AC FEs	✓	✓	✓
Observations	1,380	513	342
R^2	0.141	0.028	0.165
Adjusted R^2	-0.031	-0.459	-0.704
F Statistic	188.589***	9.856***	8.270***

Note: *p<0.1; **p<0.05; ***p<0.01
Clustered robust standard errors in parentheses.

Table A.4: FE Regression Results Incorporated Groups, Alternative Specifications

	Dependent variable:	
	Δ Gender Turnout Gap	
	(1)	(2)
Δ N Groups Incorporated	−0.311**	−0.457***
	(0.158)	(0.143)
Δ Share Marginal Workers	−0.371**	
	(0.152)	
Δ Gender Gap LFP	0.218*	
	(0.131)	
Δ Gender Gap Literacy	−0.349	
	(0.259)	
Δ Share Urban		0.099
		(0.107)
Δ Female LFP		−0.235*
		(0.123)
Δ Female Literacy		−0.073
		(0.140)
Constituency FEs	✓	✓
Observations	1,498	1,498
R^2	0.032	0.025
Adjusted R^2	-0.401	-0.412
F Statistic	8.593***	6.536***
Note:		*p<0.1; **p<0.05; ***p<0.01
		Clustered robust standard errors in parentheses.

Table A.5: FE Regression Results Narrow Parties, Alternative Specifications

	Dependent variable:	
	Δ Gender Turnout Gap	
	(1)	(2)
Δ N Narrow Parties	−0.570***	−0.866***
	(0.205)	(0.194)
Δ Share Marginal Workers	−0.451***	
	(0.125)	
Δ Gender Gap LFP	0.215*	
	(0.130)	
Δ Gender Gap Literacy	−0.297	
	(0.247)	
Δ Share Urban		0.134
		(0.101)
Δ Female LFP		−0.259**
		(0.116)
Δ Female Literacy		−0.201*
		(0.113)
Constituency FEs	✓	✓
Observations	1,498	1,498
R^2	0.031	0.022
Adjusted R^2	-0.403	-0.416
F Statistic	8.240***	5.836***
Note:		*p<0.1; **p<0.05; ***p<0.01
		Clustered robust standard errors in parentheses.

Table A.6: FE Regression Results Incorporated Groups, District Level

	Dependent variable:		
	Δ Gender Turnout Gap		
	(1)	(2)	(3)
Δ Avg N Groups Incorporated	−0.413**	−0.386**	−0.331**
	(0.162)	(0.162)	(0.163)
Δ Share Urban		−2.486**	
		(0.991)	
Δ Share Non-Farm Employment			1.551***
			(0.427)
Δ Gender Gap LFP		−0.462	0.562
		(0.383)	(0.540)
Δ Gender Gap Literacy		1.492**	0.331
		(0.727)	(0.730)
District FEs	✓	✓	✓
Observations	543	543	543
R^2	0.025	0.032	0.042
Adjusted R^2	-0.152	-0.151	-0.139
F Statistic	11.589***	3.743***	4.972***

Note: *p<0.1; **p<0.05; ***p<0.01
Clustered robust standard errors in parentheses.

Table A.7: FE Regression Results Narrow Parties, District Level

	Dependent variable:		
	Δ Gender Turnout Gap		
	(1)	(2)	(3)
Δ Avg N Narrow Parties	−1.538***	−1.493***	−1.270***
	(0.270)	(0.290)	(0.324)
Δ Share Urban		−2.210**	
		(0.959)	
Δ Share Non-Farm Employment			1.106**
			(0.491)
Δ Gender Gap LFP		−0.076	0.590
		(0.460)	(0.536)
Δ Gender Gap Literacy		1.086	0.212
		(0.695)	(0.711)
District FEs	✓	✓	✓
Observations	543	543	543
R^2	0.042	0.046	0.049
Adjusted R^2	-0.131	-0.134	-0.130
F Statistic	20.305***	5.512***	5.927***

Note: *p<0.1; **p<0.05; ***p<0.01
Clustered robust standard errors in parentheses.

Table A.8: FE Regression Results, State Level

	Dependent variable:	
	Δ Gender Turnout Gap	
	(1)	(2)
Δ N Groups Incorporated	−0.229***	−0.593***
	(0.080)	(0.128)
Δ GSDP		−8.593
		(8.763)
Δ Gender Gap LFP		−0.128
		(0.118)
Δ Gender Gap Literacy		−0.103
		(0.791)
State FEs	✓	✓
Observations	18	16
R^2	0.025	0.374
Adjusted R^2	-0.105	0.061
F Statistic	0.385	1.495

Note: *p<0.1; **p<0.05; ***p<0.01
Clustered robust standard errors in parentheses.

Figure A.4: Regional Patters in the Gender Turnout Gap in Uttar Pradesh

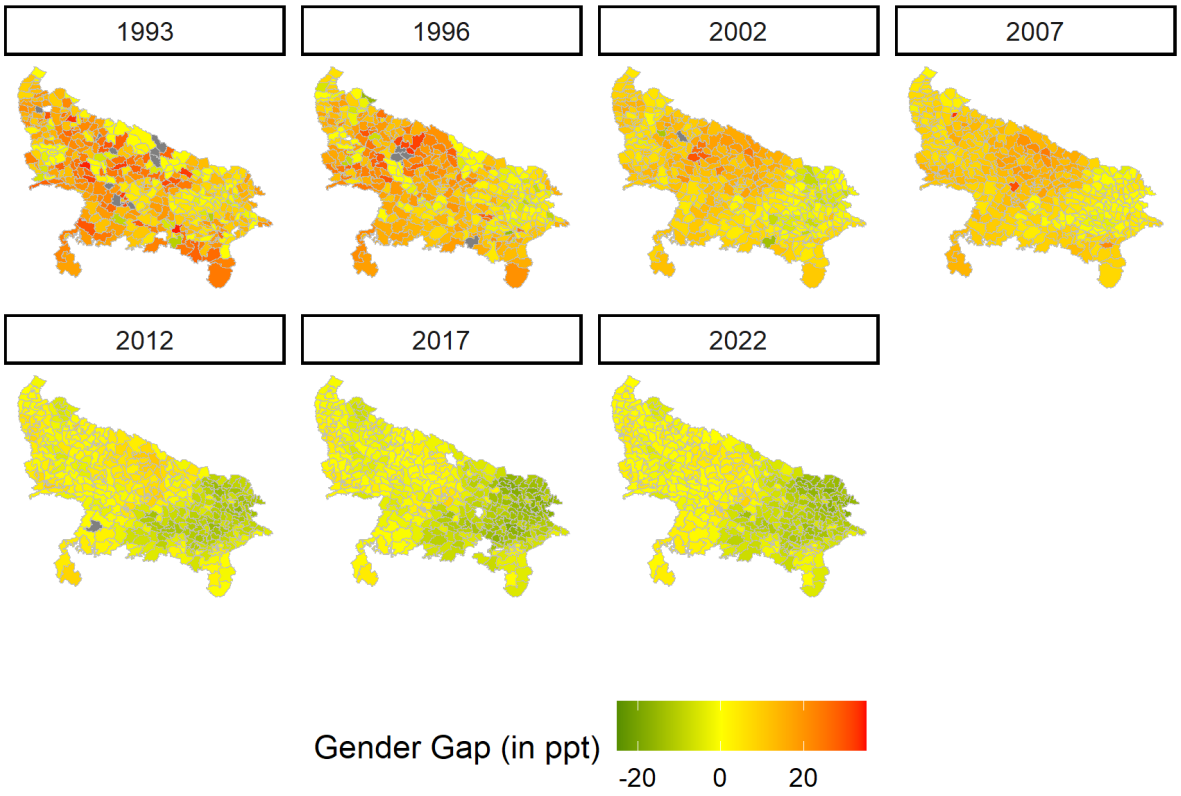
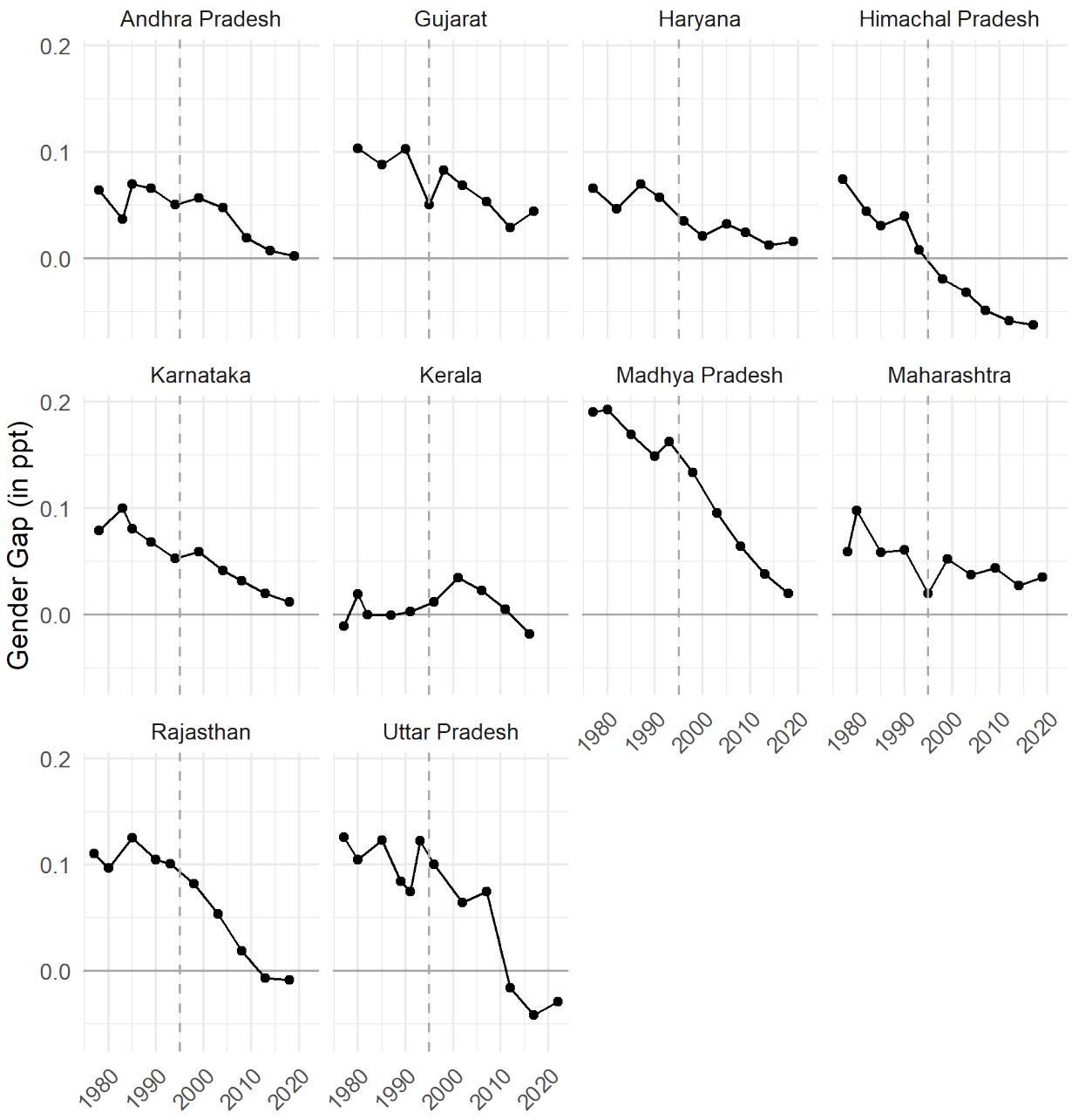


Figure A.5: Variation in the Gender Turnout Gap Across Early Adopters



Appendix B

Coding State-Level Party Leadership

I collected data on the state-level leadership for parties contesting all state elections between 1977 and 2017, with the goal of determining how voters at the time might have perceived the state-level leadership of each party. To that end, I built on Chandra and García-Ponce (2019)'s coding of state-level party leadership, and added the criteria of "visibility."

Like Chandra and García-Ponce (2019), for each party in each election year, I tried to identify the key leaders – i.e. those individuals who take strategic decisions for the party at the state level. To that end, I first broadly read secondary sources about the party and newspaper articles about the party from around the time of the election[34] to determine the pool of "potential leaders," i.e. those names that were mentioned frequently and might be associated with power within the party. Like Chandra and García-Ponce (2019), I then used contemporary newspaper articles to determine if an individual *actually* was involved in strategic decisions for the party, where strategic decisions include candidate nomination, campaign strategy, and pre-electoral alliances. That excludes highly visible politicians whose positions do not actually give them decision-making power, such as party speakers or those who are in the public eye merely because they are related to prominent politicians without holding power themselves. It also excludes holders of formal positions of power in highly centralized parties, where power is concentrated in the hands of a single leader and other positions exert little to no influence on party strategy. Using contemporary news sources to determine who was a decision-maker at the time of the election instead of relying on ex-post scholarly work about the party avoids the biases associated with hindsight: just because an individual *ended up* being important in a party does not mean they were at all times more powerful than others who were active in the party at the same time. Contemporary news sources provide a better picture of who took which decisions within a party ahead of each

[34] My main sources were the *Times of India* and the *Hindustan Times* from 1977 through 2000, and the *Times of India* and *The Hindu* from 2000 through 2017.

election. A good example of this would be Amar Singh of the Samajwadi Party (SP): after joining the SP in 1996, he became one of eight national party secretaries, a position that in and of itself does not guarantee strategic decision-making in the highly centralized party structure of the SP. It was only through his close association with party leader Mulayam Singh Yadav that Amar Singh gained *actual* decision-making power within the SP, eventually becoming the party's second-in-command in 2004; his official title, however, had not changed during his tenure (Verma, 2004). Yet before the 1996 elections, for example, there were no indications that Amar Singh was involved in any strategic decisions for the SP (he only joined the party 4 months before the 1996 elections).

Because I care most about how voters would have *perceived* parties, I additionally check for all strategic leaders' "visibility," i.e. whether they are frequently mentioned in newspaper articles in the six months leading up to the elections. A highly influential decision-maker who remains in the shadows, after all, should not have any effect on how voters relate to a party. I therefore exclude leaders from the coding who were holders of formal or informal power, but who had fewer than 3 newspaper mentions in the 6 months ahead of an election.

I tried to code the leadership of all parties that contested a state election and came in at least second in at least one of the state's constituencies. The logic here is that such a party would have been a realistic contender for at least one seat in the state, therefore potentially exerting influence on turnout in that constituency. However, following the above coding criteria meant that I was unable to identity strategic decision-makers for several of the smaller parties I set out to code, either because there was no secondary literature on these parties that would have shed light on the leadership structure, or because there was little to no reporting about these parties in news media in the six months before the election, or – more often than not – both. For example, in the 1977 Uttar Pradesh elections, 14 different parties contested; four of them won at least 1 seat (the Janata Party, the Congress, the CPI and the CPM); and a fifth, the Muslim League, came in second in 1 of the 425 constituencies of the state. I therefore attempted to code the state-level leadership of all

five parties. However, while there are many secondary sources about the U.P. Janata Party and the Congress at the time, there are none about the CPI, CPM or Muslim League state units. An extensive reading of newspaper articles revealed that Bhika Lal was the state secretary of the CPI at the time, and indeed involved in naming CPI candidates for the state.[35] However, I was unable to identify any further state-level leaders for the party. In addition, I was unable to pinpoint *any* state-level leaders for both the CPM and the Muslim League in Uttar Pradesh in 1977.

However, given that the purpose of my coding is to identify the state-level leadership of parties *as voters would have perceived* said state-level leadership at the time, I would argue that not being able to identify state-level leaders also provides information. If it was hard or impossible for me to determine who the state-level leader of a small party was through extensive archival searches, I would argue that this party's leadership might have not been well-known at the time among the general public either. Consequently, the composition of the state-level leadership would have likely had little effect on voters' behavior at election time. (Instead, it seems reasonable that while specific candidates of these parties might have mattered in their respective pockets of influence, the party leadership would not have mattered enough to send strong signals to the electorate.) If a party has no hits in the newspaper archives, or newspaper articles about the party mention no state-level leaders by name, voters likely would not have had a strong association that party either.

My coding allows for several types of leaders to be included in the state-level leadership of a party: a) those who hold formal positions of power within the state-level party organization; b) leaders who hold state-level government position; c) national leaders of the party who hail from the state and are still involved in decision-making at the state level; and d) informal power holders who may not fill an official post in the party structure but still exert influence over the party's strategic decisions. The first category, state-level party office holders, usually includes the highest position in the party hierarchy at the state level. This position is

[35] *The Times of India*, February 12, 1977, "Ten CPI candidates in UP named".

termed "state president" for most parties, but is designated "Pradesh Congress Committee president" for the Congress party and "state secretary" for the CPI and CPM.[36]

The second type of leader that might be included, holders of elected offices, always includes outgoing Chief Ministers at the time of the election. Any outgoing Chief Minister will have considerable influence on the distribution of party tickets, campaign strategy or pre-electoral alliances. If the party was in the opposition before the election, it may or may not have powerful decision-makers who hold elected office. Elected official from an opposition party that might be powerful decision-makers at the state level include any leader of the opposition in the state assembly or, less frequently, the leader of the party's legislative group. For example, Pramod Tiwari was the leader of the Congress Legislature Party in Uttar Pradesh for more than 20 years and played an important role as a strategic decision-maker for four assembly elections.[37]

The third type of leaders, national leaders, usually involves either ministers at the national level or party office holders at the national level who come from the state (and often used to hold power at the state level) before moving on to the national level. These leaders "stay connected" to their home states and keep exerting influence at the state level ahead of elections, by getting tickets for their loyalists or being included in strategic committees, for instance. They might even be tipped as potential Chief Ministerial candidates. An example of this type of leader would be Murli Manohar Joshi who was deeply involved in the Uttar Pradesh state party's affairs even after he became national general secretary and later national president of the BJP (Jaffrelot, 2003). Another example would be members of the Nehru-Gandhi family, who originally hail from Uttar Pradesh and have exerted major influence on many of the Congress party's state election campaigns in Uttar Pradesh. Indira Gandhi, her sons Sanjay Gandhi and Rajiv Gandhi as well as her grandson Rahul Gandhi have all ran for parliament from constituencies in Uttar Pradesh in the past. (The Congress

[36] However, newspaper articles might also refer to these office holders more generically as "state unit leaders" or "state unit chiefs."

[37] *The Times of India*, August 30, 2012, "With Khatri, Congress tries to rise above caste".

state unit in 1980 had actually unanimously elected Sanjay Gandhi as its leader, effectively crowning him Chief Minister, but was thwarted by then Prime Minister Indira Gandhi's opposition.[38])

The last type of leaders that might be included in my coding are informal power holders, i.e. those who do not officially occupy the highest party office in the state nor an important elected office, but who nevertheless influence strategic party decisions. This mostly includes trusted aides of leaders of highly centralized parties. Formal offices within the party structure of parties such as the BSP or even the SP do not say much about the actual decision-making power an individual enjoys. Instead, proximity to the near-all-powerful main leader (Mayawati for the BSP, and Mulayam Singh Yadav for the SP) is usually a better gauge for an individual's influence on a party's strategic decision. Besides Amar Singh, who became Mulayam Singh Yadav's second-in-command within the SP[39], other examples of leaders who exert influence through their proximity to the party president include BSP leader "Mayawati's right hand man Naseemuddin Siddiqui" in the 2000s.[40] In Tamil Nadu, Sasikala Natarajan, close friend to AIADMK leader Jayalalithaa, exerted powerful influence within the party for a while (Ananth, 2006).

Separately, I also code any chief ministerial candidate(s) a party might be projecting ahead of an election, independent of whether or not that person holds any party or elected office at the time. The post of Chief Minister is the highest and therefore, arguably, most valuable post a winning party will be able to occupy. If a party commits to a candidate for this office before the election, it sends a strong message to the electorate, and arguably that projected chief ministerial candidate will likely be most associated with a party in the minds of voters. In the vast majority of cases, the projected chief ministerial candidate was also an important decision-maker at the state level for the party. The only exception so far might have been Uma Bharti, who was projected as the Uttar Pradesh BJP's chief ministerial

[38] *The Times of India*, June 7, 1980, "PM rejects UP plea for Sanjay".
[39] *The Times of India*, August 10, 2002, "Oppn pledges to take on govt unitedly"
[40] *The Times of India*, December 14, 2011, "A busy day for parties".

candidate in 2012, despite hailing from neighboring Madhya Pradesh and never having held a position in the UP state unit nor having been elected from a UP constituency before. It is unclear from newspaper articles how much strategic decision-making power she actually had ahead of the election.

I also code the Chief Ministers any given party put up in the past. Since the Chief Minister is the highest office at the state level, any history of incorporation of groups at that level likely will affect voters' evaluation of a party for a while into the future. For each party in each year, I checked how many Chief Ministers that party had in the previous two election cycles. India's parliamentary system allows for the head of the state government to change without new elections being held. In fact, it is not uncommon for a party to have more than one Chief Minister over the course of a single term. Independent of the duration for which they held office, I coded all individuals who a) held the office during any of the previous two terms *and* b) are still part of the party at the time of the election. The reason this second condition is necessary is the fluidity of parties and party affiliations in some Indian states during the time of my analysis: a leader might have been Chief Minister under one party's banner, but then later defected to another party. It seems implausible that voters would still associate that leader with the old party, even if the leader is now actively campaigning for the new party, usually using their status as former CM to give this campaign more weight. Take, for example, the 1977 elections in Uttar Pradesh. A total of 6 different CMs had served in the previous 2 terms under 3 different parties: Chandra Bhanu Gupta (Congress), Charan Singh (BKD), Tribhuvan Narain Singh (Congress (O)), Kamalapati Tripathi (Congress) and Hemwati Nandan Bahuguna (Congress) in the 1960-1974 term; and Bahuguna and Narayan Dutt Tiwari (Congress) during the 1974-1977 term. By the time the 1977 elections were approaching, three of these former CMs, C.B. Gupta, Charan Singh and H.N. Bahuguna, were associated with the newly formed Janata Party; and three with the Congress, T.N. Singh, Tripathi and Tiwari. I therefore coded C. B. Gupta, Charan Singh and H.N. Bahuguna as former CMs for the Janata Party; and the other three

as former CMs for the Congress. Note that coding for past Chief Ministers does not require these individuals to still be important state leaders in the given election; it merely requires that these individuals have not switched party alliances and would therefore be more likely to be associated with a different party by now. For example, in 1977, T.N. Singh is coded as a former CM for the Congress, even though he was not active in Uttar Pradesh state politics anymore but actually governor of West Bengal by that time. In the extreme case, I even code former CMs who had died by the time of an election (but had served during the previous two terms) as a party could still use their image for campaigning and voters would still likely associate that leader with the party, even after their death. That was the case for Vir Bahadur Singh, who served as Congress CM for two years in the 1985-1989 term, but passed away before the 1989 elections were held. Still, he was invoked frequently in newspaper articles, and since he never changed parties since holding the office, I still coded him as a past CM for the Congress in 1989 and 1991.

Coding Ethnic Identity

For each leader I identified for each party, I attempted to code their most salient ethnic identity/identities. I did this in two steps: first, I coded the *nominal* ethnic identity of leaders, where I was able to determine it; and then I checked for salience at the time of each election. To determine a leader's nominal identity/identities, I consulted scholarly work on the leader and the party, as well as searched broadly in the archives. For Uttar Pradesh in particular, Jaffrelot (2003) was a great help in identifying *which* ethnic identity categories each leader might belong to. For leaders who have since passed away, obituaries and articles that reviews their impact on state politics after their death also often are good sources of information.

After determining nominal ethnic identity categories, where possible, I coded whether said categories were salient for the leader at the time of each election. I coded salience purely based on contemporary newspaper articles. Specifically, I looked for the earliest

explicit or implicit mention of each leader's potentially relevant ethnic identity/identities in newspaper articles. If a leader's identity was discussed in a newspaper article *before* a given election, I coded that identity as being salient for that election. Conversely, if there were no mentions of a leader's identity before that election, I did code the identity as not being salient.

Explicit mentions of a leader's identity are straightforward classifications of a leader as belonging to "x community" in the text of a newspaper. For example, Janata leader Charan Singh was identified as a "Jat leader" explicitly in articles dating back to the 1960s. As early as 1969, the *Times of India* wrote: "Mr. Charan Singh is the most eminent Jat on the country's political stage;"[41] and five years later, he was identified as "Mr Singh who is himself a Jat" in the *Hindustan Times*.[42]

Implicit mentions, by contrast, are less obvious at first glance. In India, ethnic identity can often be inferred from a person's name (and sometimes even is *part of the name*). For example, former Uttar Pradesh Chief Minister Narayan Dutt Tiwari would easily be identifiable as Brahmin to any Uttar Pradesh resident, based on his name alone. Yet neither the *Times of India* nor the *Hindustan Times* explicitly called Tiwari "Brahmin" before the 1977 elections, the first election in my dataset. In fact, the earliest mention of Tiwari's Brahmin caste identity in the archives is from 1979,[43] two years after the election. Yet his Congress party was regularly associated with Brahmin dominance in the media even before that. For example, in 1970, the *Times of India* reported that the post-split Congress party headed by Indira Gandhi, herself a Brahmin, was led mostly by Brahmins in Uttar Pradesh: "the party organisation in the districts—-to the extent it exists at all-—is Brahmin-dominated."[44] His predecessor Kamalapati Tripathi's reign at the helm of the UP Congress was described as "Mr. Tripathi's Brahmin leadership,"[45] and newspapers stated that "the

[41] *The Times of India*, February 6, 1969, "BKD well placed in West U.P."
[42] *The Hindustan Times*, February 15, 1974, "Charan's Jat armour dented".
[43] *The Times of India*, December 25, 1979, "N. D. Tiwari has edge over rivals in Nainital".
[44] *The Times of India*, December 12, 1970, "U.P. Election Arithmetic".
[45] *The Times of India*, August 22, 1970, "Mrs. Gandhi's Problem in U.P.: The Compulsions of Caste Arithmetic."

caste composition of the cabinet, the parliamentary board and the UPCC executive [was] heavily weighted in favour of Brahmins" in 1975.[46] While Tiwari's caste identity was not *explicitly* mentioned in the news media at the time, I argue, it was *implicitly* referred to by discussing Brahmin dominance in the Congress party during Tiwari's time as a prominent leader, while most readers of the *Times of India* or *Hindustan Times* would have been easily able to infer Tiwari's Brahmin identity from his name as well. I therefore code Tiwari's Brahmin identity as salient ahead of the 1977 elections (and all following elections).

Implicit mentions become even more tricky when a leader's caste identity is part of their name. For example, most Reddys in Andhra Pradesh carry "Reddy" in their name. No matter how salient the identity is for a leader, then, journalists would likely never find it necessary to identify Chenna Reddy as "a Reddy," the way that Charan Singh was identified as "a Jat." At the same time, that does not mean that the Reddy identity should *automatically* be assumed to be salient for a politician, just because they carry the caste name "Reddy" in their name. As a rule, I coded the ethnic identity of those leaders who carry the caste name in their name as salient only if a) they are juxtaposed to other politicians who are explicitly identified by their caste and/or b) the caste group is discussed explicitly in the context of their party. For example, while Congress leaders in Andhra Pradesh belonging to the Reddy caste are not usually explicitly identified as "Reddys," if they are juxtaposed with a leader identified as belonging to another caste, such as Kamma – a caste identity that cannot always be inferred from name the same way as the Reddy identity – I code the Reddy identity as salient as well. The reason is that if the Kamma leader's caste is specifically identified, the only identity that it could be juxtaposed with, then, is the Reddy identity of the leader which is presumed common knowledge, given that it is contained in the name. Similarly, if "Reddys" as a group are discussed in the context of a Reddy leader's party, that also is a sign that the Reddy leader's identity would be salient (and presumed common knowledge from the name). Take, as an example, an article from 1970 that never

[46] *The Times of India*, April 26, 1975, "Dissidence in U.P. Congress: Caste pressure groups."

explicitly identifies N. Sanjiva Reddy as belonging to the Reddy caste. But the article says:

> The grand alliance that Mr. Sanjiva Reddy is hoping to forge with Professor Ranga, a life-long rival, does represent something of a threat. An alliance between Professor Ranga's following among Kamma caste-men and the Reddys may be dangerous for the new Congress[47]

This article does both: it juxtaposes Sanjiva Reddy with a leader specifically identified as Kamma; and it references "Reddys" as a group as his traditional followers. In this case, I would code Sanjiva Reddy's caste identity as being salient. Similarly, when newspapers talk about "the Reddy-dominated Pradesh Congress" ahead of an election, I will code leaders' Reddy identity as salient.[48]

What then counts as a non-salient ethnic identity? I coded all those identities as not salient where a) I could not identify any state-level leaders of a party (and consequently, no identity for those leaders); b) I could not determine the ethnic identity of state-level leaders of a party *at all*, despite extensive searches in the secondary literature and newspaper archives; and/or c) I found no explicit or implicit mentions of the leader's identity in newspaper articles dated prior to the election. This last point requires that the leader's identity was neither discussed directly, nor was the leader's identity category ever discussed by a newspaper articles in the context of the leader's party. An example of the first case would be the Uttar Pradesh CPM in 1977: because I was unable to determine the name of *any* state-level leader for the party ahead of the 1977 elections, I was also unable to code any ethnic identity for the leadership.[49] An example of the second case, where the leader is known but their ethnic identity is not, includes Bhika Lal, state secretary of the CPI ahead of the 1977 Uttar Pradesh elections. While I was able to determine that Bhika Lal not only held a formal office within the party structure but also took strategic decisions for the party, there were no mentions of his caste or other identities in any articles in the archive. An example of the third case,

[47] *The Times of India*, August 1, 1970, "New phase in Telengana".
[48] *The Times of India*, December 14, 1984, "Rao shaky in AP seat".
[49] The CPM won 1 out of 425 seats in Uttar Pradesh in 1977, and came in second in 1 more constituency.

where the leader is known and so is their identity (for example from secondary sources or obituaries) but the identity is not salient, is Puchalapalli Sundarayya, state secretary of the CPM in Andhra Pradesh in 1983 (and previously prominent national leader of the CPM). Based on Harrison (1956), his full name is Puchalapalli Sundar Rama Reddy, and he was the only prominent Reddy among the Communists in the state in the 1950s (p. 384). Yet no newspaper articles from the years when he was coded as a state-level leader for the Andhra Pradesh CPM either juxtapose him with someone whose caste identity is identified explicitly; nor reference his caste group, the Reddys, more generally in the context of his party, be it in terms of leadership, target group or otherwise. The fact that he even changed his name to make his caste affiliation less obvious further supports the claim that his caste identity was not salient at the time. I argue that for all conditions that I code as non-salient identities, the average voter would have likely not had any information about the level of a party's incorporation of co-ethnic elites, either because there was no mentionable state-level party organization, or because the identity of the leader was unknown, or because the party/leader specifically cultivated an image that did not rely on ethnic identity.

Covariates

I use three main covariates in the analysis that the literature suggests should affect gender turnout gaps. The first is **economic development**, where higher economic development should correlate with smaller gender turnout gaps. One of the most straightforward measures of economic development, **GDP per capita**, is only available at the state level. At the Assembly Constituency level, I therefore use two measures found in the census to proxy for economic development. One is the share of the population living in areas classified as **urban** by the census. The census classifies areas as urban based on three criteria: a locality needs to have at least 5,000 inhabitants, a density of 400 people per square kilometer or more, and at least 75 percent of the male working population engaged in non-farm work to be considered urban. Urban areas, on average, record more of the markers commonly

associated with development, including higher levels of education, better health outcomes, higher incomes and more household assets.[50]

A second, related measure of economic development is the share of **men working in agriculture**. The classification as "urban" requires that at least three-quarter of men work outside of agriculture. Yet even within those localities classified as rural, there is variation in the share of the population that makes their living from farming. A stronger reliance on agriculture signals fewer outside options, indicating lower levels of overall development. I therefore also collected information on the percentage of all male (main) workers that is engaged in agriculture.[51]

A third measure that proxies for economic development is the share of male workers who are only **marginal workers**. "Marginal workers" according to the Indian census are those who worked for less than 6 out of the past 12 months, while "main workers" are those who worked for more than 6 out of the past 12 months. Marginal workers include many individuals who are engaged in seasonal work, such as harvesting or sowing; or those on short-term assignments in construction or other manual labor. Marginal workers are therefore more precarious than main workers. Especially among men, marginal work is often not a choice, but a necessity, for casual informal workers who make ends meet by going from gig to gig. The share of male workers who are marginal workers, therefore, captures a dimension of economic development that relates to the quality of employment, where a higher share of male workers in marginal work indicates that employment conditions are more precarious in a locality.[52]

Another predictor of the gender turnout gap found in the literature is female labor force participation (FLFP). I collected data on the **FLFP rate**, or the share of women who are engaged in any kind of work for cash or kind. Where possible, I collected data disaggregated

[50] See, for example, The Indian Express, October 14, 2021, "Household-level asset ownership: just 3% in India, Delhi on top"; Mint, September 14, 2017, "How much of India is actually urban?".

[51] In census nomenclature, that includes both "cultivators," who take decisions themselves and have a personal stake in the farming activity – for example because they own or lease the land – as well as "agricultural laborers," who merely work for cash or kind and have no personal stake in the activity.

[52] The NSS adopted a procedure of measuring LFP that allows for calculating the share of marginal workers.

by main or marginal work status. However, what matters might not be absolute levels of FLFP, but work rates relative to male labor force participation: if unemployment is high in general, FLFP rates will necessarily go down. However, if male employment is considered a baseline that is dependent on levels of economic development, then the difference between male and female work rates indicates the extent of women's economic agency. I therefore also calculate the **gender gap in work rates**.[53]

The third predictor of the gender turnout gap based on the literature is female education. I use **female literacy** as a measure of a minimal level of education that might make women more likely to participate politically. Indian elections are meant to be accessible to illiterate voters as well, with parties using symbols and colors together with party names, and candidate photos being printed alongside candidate names on ballot papers. Before casting a ballot on election day, voters are required to sign their name with an election official; however, those who cannot sign their own name are allowed to substitute a thumb print in ink instead. Yet not being able to sign their own name could, in theory, be a deterrent for women on election day; and gaining enough literacy to be able to write their name in the list might make women feel empowered enough to turn out in greater numbers. In addition, besides making the election process more accessible, basic literacy should aid women in gaining and processing political information. I calculate literacy rates based on the number of literate women provided by the census. The Indian census counts as literate anyone aged 7 or older who can read and write in any language, independent of formal education.[54] However, in a setting with low educational requirements for participation, maybe what matters are not overall levels of female education, but women's education *relative* to that of men as a measure of the value parents place on daughters' education relative to sons'. Where women's literacy rates approach those of men, women's empowerment should be higher and gender turnout gaps should be smaller. I therefore also calculate the **gender gap in literacy**.

[53] The NSS contains data on women's LFP as well.
[54] The NSS adopted a procedure to measure literacy that is comparable to the Indian census.

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