

# Transfers and the rise of Hindu nationalism in India

Amal Ahmad\*

*This version: August 2022.*

## Abstract

This paper provides the first quasi-experimental evidence on the extent to which economic benefits programs have contributed to the electoral triumphs of the Hindu-nationalist party (BJP) in India. I study a national program through which the BJP incumbent has transferred development funds to villages with a high share of disadvantaged castes. Focusing on India's largest state, I match villages to polling booths, and use a multiple-score regression discontinuity to identify the effects of past and expected transfers on village-level voting in the 2019 general elections. I find that neither treatment contributed to the BJP's victory among this important swing constituency.

*JEL Classification: D72*

*Keywords: voting behavior, transfers, populism, Hindu nationalism*

---

\*Department of Economics and Centre for Modern Indian Studies, University of Göttingen. Email: amal.ahmad@uni-goettingen.de. Phone: +49 551 39-21703. Address: Waldweg 26, 37073 Göttingen, Germany. I am grateful to Raphael Susewind for sharing his data with me and to Mridhula Mohan for excellent research assistance. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

# 1 Introduction

Around the time that the victory of Donald Trump in the United States was highlighting the surge of right-wing parties in the West, developments of no lesser importance were taking place in India, the world’s largest democracy. The Bharatiya Janata Party (henceforth BJP), a Hindu nationalist party and one of the main political parties in India, rose to parliamentary majority for the first time in the national elections of 2014, securing a sweeping victory which took many observers by surprise (Rukmini, 2019). Under the leadership of the popular Narendra Modi and the banner of dually promoting *Hindutva*<sup>1</sup> and economic development in the country, the BJP secured a second and even stronger win in the subsequent elections of 2019. The party’s rule has included a mix of economic programs with debatable developmental success, as well as steps to cement Hindu ethno-nationalism such as the revocation of (largely Muslim) Kashmir and Jammu’s special status and the introduction of Muslim-exclusionary citizenship amendments to the parliament (BBC, 2019).

There has been significant interest in understanding the BJP’s recent electoral triumph in India, including its ability to secure votes from populations which had previously largely voted for other parties. Perhaps most prominently, this includes what has been termed the party’s “upending” of existing caste-based politics. Within India’s caste system, the largest share of BJP votes had historically come from the upper castes, which constitute 10% of the electorate, while more disenfranchised castes voted heavily for the then-leading Congress Party or caste-based parties (Rukmini, 2019). However, in 2014 and 2019, non-upper caste groups, as well as marginalized groups falling entirely outside of the caste system, turned out for the BJP at unprecedented levels. For example, the Scheduled Castes (also known as Dalits, and historically pejoratively as the “untouchable” caste), who are among the poorest in the country and who constitute 16% of the Indian electorate, voted by 34% for the BJP, up from 24% in 2014 and 12% in 2009 (Kumar and Gupta, 2019; Verma, 2009).

To what extent is the party’s support among key disenfranchised groups the result of

---

<sup>1</sup>The term was first articulated in 1923, by organizational predecessors of the BJP, to refer to the political ideology of Hindu nationalism.

attracting these groups into a grand *Hindutva* narrative, in which caste-based cleavages are secondary to a united Hindu-led front, versus as a result of welfare benefits through transfers and development programs? In the popular media, it is not uncommon to see assertions such as “*the BJP has largely banked on its welfare benefits to the Dalits*” (Kishore, 2022) or “*the party’s dexterous strategy to fortify itself among Dalits and bring them under an overarching umbrella of Hindu consolidation [...] is due to] its ‘social engineering’ playbook [...] the BJP managed to consolidate Dalit voters through welfare schemes*” (Shah 2022). However, it is unclear which kind of evidence such assertions rely on. Within the academic literature, the existing discussion, while potentially illuminating, is suggestive, and the evidence is far from causally identified (e.g. Jaffrelot, 2021; Aiyar, 2019; Jha, 2014).

It is to this debate which this paper contributes, offering the first quasi-experimental evidence at the village level on the effect of transfers to disenfranchised populations on electoral outcomes for the incumbent BJP. Specifically, I study the impact of transfers to Scheduled Caste-majority villages, which Modi’s government began to roll out in 2018 through a large national program, on voting for the BJP in 2019. Taking into account the sheer size of the Indian polity and the data challenges involved in this type of research (described in Section 4), I focus on Uttar Pradesh, India’s most populous state with 250 million people. Given that Uttar Pradesh is the single most important Indian state electorally, and given the large share of Scheduled Castes in the national electorate and their importance to the BJP’s consolidation of power, the findings of this paper are relevant to the larger question of transfers and political allegiances even as they are also local to the state and the constituency.

The national rural development program under study, titled Pradhan Mantri Adarsh Gram Yojana (PMAGY), began to transfer one-time funds to villages having at least 50% Scheduled Caste populations in 2018, and it not only targets a critical electoral segment but also offers the scope for strong research design. First, the program’s eligibility cutoff is both non-manipulable and was used reliably as a sorting instrument: village eligibility was calculated based on the preexisting 2011 Census, and no village below the 50% cutoff

has received a transfer.<sup>2</sup> Second, I use the program’s timing and transparent rollout on the basis of a *second* criterion to sharpen the study of election outcomes. In Uttar Pradesh, a first cohort of villages received transfers in November-December 2018, six months before the state voted in the national elections, while later cohorts received funds after the conclusion of the elections; the first cohort of villages was selected based on having the highest absolute number of Scheduled Caste persons among the eligible villages. This means that villages meeting two thresholds (above 50% share and large population size) received transfers before the election while villages meeting only the first threshold were eligible for transfers after the election. I explain how this allows me to use a multi-score sharp regression discontinuity with heterogeneous treatment effects to identify the impact of two distinct treatments - transfer receipt prior to elections versus eligibility for future transfers - on votes.

Because the empirical research design requires information on voting at the village level and because this is not readily available, I first build a carefully matched dataset myself of villages - in Uttar Pradesh, and within a sufficiently wide bandwidth of the 50% Scheduled Share cutoff - and their votes.<sup>3</sup> Whereas there is village-level data about village characteristics and transfers, electoral data in India is available only at the polling booth level. Matching each village to the polling booth(s) in which it voted is highly challenging, including because village and booth geolocation codes are notoriously inaccurate, and for this reason very few empirical studies explore village-level electoral outcomes in India. To overcome this obstacle, I use a combination of booth names, booth parts descriptions, neighboring village information, and visual map inspections to manually match each village to the polling booth(s) in which it voted, in both 2019 *and* 2014.<sup>4</sup> This time-consuming but highly meticulous process is detailed in **Appendix A**, and it produces matches I am very confident about for 85% of the villages, for an end matched dataset of over 6,300 villages.

---

<sup>2</sup>This is based on the detailed records of which villages received transfers, explained in Section 4 and illustrated in Section 5. Importantly, the 50% SC-share threshold was not used in any other government program.

<sup>3</sup>Specifically, I attempt to generate matches for the 7,499 villages in Uttar Pradesh within +/- 8% of the cutoff. This was to maintain feasibility of this time consuming task, and anticipating that the relevant analytical bandwidth would almost certainly be narrower than 8%.

<sup>4</sup>Booth assignment changed between elections.

To anchor the empirical analysis, I first present a simple theoretical model of transfers and voting behavior, which draws on the canonical framework in Dixit and Londregan (1996). In the model, voters weigh utility from economic benefits against ideological preferences when voting for parties. I show that eligibility for *future* transfers can affect calculations of utility, while *past* transfers can generate loyalty and a shift in ideological preferences; therefore, both can affect the village’s vote share for the incumbent party albeit in conceptually *distinct* ways. I derive expressions for these possible effects, and show how they can be inferred about from unbiased difference-in-means estimators of the vote share for the incumbent in the treated versus control villages.

I then use the matched dataset in a multi-score sharp regression discontinuity design, to estimate these possible treatment effects. The specification uses the fact that villages meeting two thresholds received transfers pre-election while villages meeting only one (specific) threshold were eligible for transfers ex-post, to create *two* sets of treatments. The design departs from the assumption of a single binary treatment variable which other RDDs including other multi-score designs usually adopt, and allows for unbiased estimators in the presence of multiple running variables and heterogeneous treatment effects (Choi and Lee, 2018).<sup>5</sup> For both treatments, the key identification assumption is that bandwidth restrictions generate comparability between treated and control groups. Placebo tests using village characteristics as outcomes support the validity of the research design, as do other falsification exercises.

I find that, for villages in the vicinity of the cutoffs in Uttar Pradesh, neither receipt of the rural development funds pre-election *nor* eligibility for them afterward affected the village’s share of votes going to the BJP in 2019. Treated villages in both cases voted as would be predicted by the counterfactual group of villages that fell just below the relevant threshold(s) and were not eligible for transfers at any point, resulting in coefficients that are very close to zero and with confidence intervals which rule out meaningful magnitudes. In

---

<sup>5</sup>The majority of multi-score designs assume a binary treatment in which meeting either one of the two thresholds results in the (same) treatment effect, or a binary treatment in which meeting *both* thresholds (e.g. longitude and latitude in a spatial RDD) results in the (single) treatment effect. These approaches are explored theoretically in Wong et al (2013) and Keele and Titunik (2015), respectively. As shown in Choi and Lee (2018), both of these approaches are problematic if there are heterogeneous treatment effects from crossing one versus two thresholds.

line with the theoretical model, this implies transfers had no effect on the average voter’s utility function nor on their political loyalties. As a secondary albeit not structurally derived outcome, I explore voter turnout, measured as number of total voters divided by electors in the village.<sup>6</sup> I find that the program did not impact turnout in former recipient villages, and increased turnout by a modest 1.3% in villages eligible for post-election transfers.

Given that, descriptively speaking, the share of votes for the BJP *did* increase in the Uttar Pradesh villages around the cutoff by an average of 9 percentage points between 2014 and 2019, the results show that little, if any, of the BJP triumph among this constituency can be attributed to these treatments. The findings belie the notion that, at least for this constituency, expanding BJP appeal is a result largely of past or future welfare benefits. The appeal of the party among the disenfranchised population under study seems to extend far beyond pure economic expediency, and is possibly anchored heavily in the nationalist narrative articulated by Modi or in other political considerations that shifted affinity for the BJP across the board.

This paper contributes to a number of research areas. First, on the recent rise of ethnic nationalism in the world’s largest democracy, to my knowledge no other paper has provided disaggregated identified evidence on the extent to which economic programs have played a part, despite significant interest in the issue particularly as relates to caste-based voting. In fact, very few empirical papers on the political economy of voting in India use village-level electoral outcomes for exploring the effect of *any* “treatment” on votes, likely due to the sheer difficulty and time-intensive nature of the village-booth matching process.<sup>7</sup> An exception is Hinston and Vaishnav (2021), who study the effect of security crises and nationalist rallies on village-level support for the BJP 2019, also in Uttar Pradesh; however, the authors rely

---

<sup>6</sup>This is due to the well known difficulty of modeling turnout choices, as free riding would often be the logical choice and a Nash equilibrium. More broadly, turnout is not itself the decisive factor in BJP triumph, but rather the share of the BJP vote among those who did vote, hence the focus on this latter variable throughout.

<sup>7</sup>A number of studies use disaggregated *individual*-level outcomes but *not* from actual election votes. For example, Bardhan et al (2022) lead a secret ballot vote as part of the study of the effect of clientelism on votes, while Ray (2021) conducts a survey of individuals prior to the 2019 elections to examine correlates with the proclivity to vote for the BJP. Otherwise, using actual election data, the focus is on voting at least at the Assembly Constituency level (e.g. the poverty-voting correlation study in Kapoor and Ravi, 2021). In Uttar Pradesh, a state with 250 million people and 403 ACs, the average AC would comprise over 600,000 people.

principally on a name-matching algorithm which may not provide the same accuracy as fully manual matching (**Appendix A**).<sup>8</sup>

More broadly, the paper contributes to the literature exploring the drivers of right-wing populism, globally and in the last decade (Guriev and Papaioannou, 2020). Existing studies have been largely West-centered, and have accordingly focused on the changes in advanced economies that may feed economic and social anxieties. This includes the effect on populist support of: trade competition with China in the United States (Autor et al, 2020), the United Kingdom (Colantone and Stanig, 2018), and Germany (Dippel et al, 2018), and of automation of technology (Anelli et al, 2019) and the global financial recession (Antoniades and Calomiris, 2020; Dehdari, 2020) in the United States and Europe. This recent empirical literature does not address the populist shift in the Indian democracy, and the corresponding interplay between socioeconomic cleavages and populist support in a largely rural and poor, but democratically vibrant, context.

In addition, the study offers a South Asia perspective on the effects of transfers on incumbent support in developing country democracies, amongst a literature which is largely focused on other regions. Past studies include the effects on support for the incumbent of a property rights program in rural Mexico (De Janvry et al, 2014; Dower and Pfutze, 2015), rural subsidies in Malawi (Dionne and Horowitz, 2016), anti-poverty programs in Uganda (Blattman et al, 2018) and Brazil (Bursztyn, 2016), and large cash transfer program in Brazil (Frey, 2019), Uruguay (Manacorda et al, 2011), Mexico (De La O, 2012), and the Philippines (Labonne, 2013).

Finally, from a methodological viewpoint, the paper provides an example from political economy on the usefulness of regression discontinuity design with heterogeneous treatment effects, to a literature that has otherwise drawn on education economics applications (Reardon and Robinson, 2012; Choi and Lee, 2018).

---

<sup>8</sup>On a methodological note, both this paper and Hinston and Vaishnav (2021) highlight the need for, and usefulness of, village-level electoral data in India, for addressing a range of political economy questions.

## 2 Political and social context

### 2.1 India’s parliamentary system and the Bharatiya Janata Party

India, home to 900 million electors - one in every four electors in the world - is a parliamentary democracy. Every five years Indian citizens vote by universal suffrage for members of the Lok Sabha, the lower chamber of the Indian Parliament; Uttar Pradesh, India’s most populous state with about 250 million people, is responsible for the election of 80 out of the 543 members of the Lok Sabha.<sup>9</sup> Each member is elected to represent what is called a Parliamentary Constituency (PC), so that Uttar Pradesh is divided into 80 PCs, within which candidates from the different parties compete.<sup>10</sup> The Lok Sabha is not only the most powerful legislative body but its ruling coalition also produces the Prime Minister, who is the real executive authority in India.

The Bharatiya Janata Party (BJP), which arose from a history of Hindu nationalist organizations and tradition (Jaffrelot, 2021), has long been one of the main political parties of India but won a landslide victory in 2014, securing 282 Lok Sabha seats, up from 166 seats in 2009. Under the leadership of Modi as Prime Minister, it was reelected to an even larger majority in 2019, with 303 seats. The win in Uttar Pradesh has been no less impressive: the BJP secured 71 out of Uttar Pradesh’s 80 seats in 2014, an astounding increase from just 10 seats in 2009. Although the party secured somewhat less seats (62) from Uttar Pradesh in 2019, this is due to the system of first-past-the-post victory in Indian elections, and the share of the popular vote in Uttar Pradesh going to the BJP actually rose from 42.3% in 2014 to 51.2% in 2019.

**Figure 1a** shows the share of the national popular vote which went to different parties, including the BJP, in the general elections since 1999. **Figure 1b** shows the corresponding figure for Uttar Pradesh only.<sup>11</sup>

---

<sup>9</sup>For perspective, this makes Uttar Pradesh as central to the Indian parliament as California and New York - combined - are to the United States electoral college.

<sup>10</sup>For this reason, all analysis controls for PC: villages are compared *within* a constituency where they face the same set of candidates, but fall on different sides of the cutoff. See Section 5.

<sup>11</sup>Notably, one of the major competitors for the BJP in Uttar Pradesh is the Bahujan Samaj Party (BSP), a party meant to represent Scheduled Castes and other non-upper caste groups, underscoring the key role and



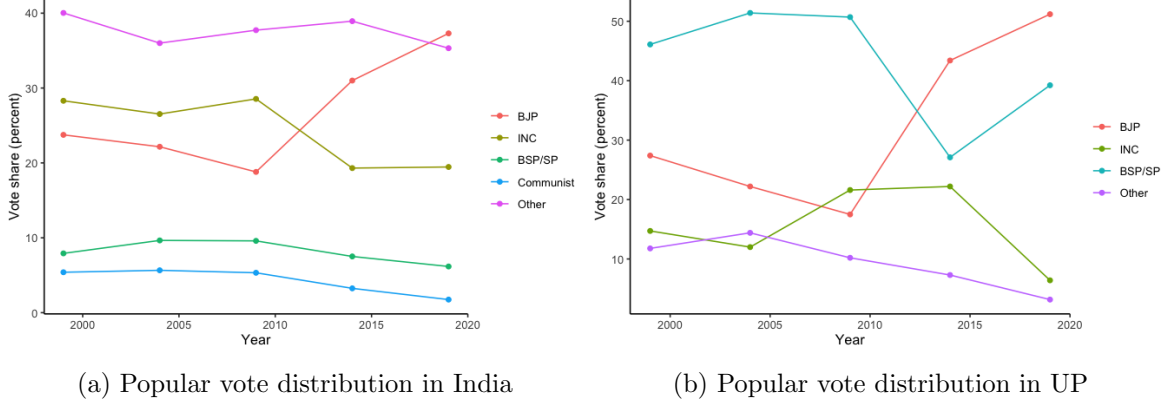


Figure 1: Popular vote in Lok Sabha, 1999-2019

Figure 1 demonstrates the distribution of popular votes to key parties in the general elections of 1999, 2004, 2009, 2014, and 2019, in India as a whole and in Uttar Pradesh. The share of the vote for the BJP is in orange in both panels.

As mentioned above, a key boon for BJP triumph in Uttar Pradesh and in the country as a whole have been the Scheduled Castes. High and rising vote share for the BJP is also apparent in the matched data of Scheduled Caste-majority villages votes in Uttar Pradesh (see the descriptive results in Section 6).

## 2.2 Scheduled Castes and the transfer program

“Scheduled Castes” is an officially designated socioeconomic segment in India consisting of groups that were historically considered outside (and beneath) the Hindu hierarchical caste system, and which was first defined by British colonial authorities in 1935 in light of electoral concerns. The relevant legislation, which defined Scheduled Castes to include groups that the British had loosely referred to as the “Depressed Classes”, came in preparation for the provincial elections of 1937 and in the context greater pressure on colonial authorities to allow for self-rule in India.<sup>12</sup> Post-independence, the Scheduled Caste designation initially continued to apply to Hindu groups only, but was later also extended to Sikh and Buddhist electoral impact of caste politics in the state.

<sup>12</sup>It has been argued that the enfranchisement of these groups, which expanded the size of the electorate significantly, was strategically intended by the British to ensure that the nationalist Congress Party could never rule through a majority and that it be dominated electorally by an alliance of right-wing Hindu princes who were subservient to the British government (Balachandran, 2020).

communities suffering from “untouchability” discrimination.<sup>13</sup>

Today, the sheer size of Scheduled Castes in India (250 million) underlies their electoral significance, with 1 in every 5 Scheduled Caste persons residing in Uttar Pradesh (48 million). **Figure 2a** shows the percent of each Indian state’s population which is Scheduled Caste. **Figure 2b** provides a more granular look into Uttar Pradesh, showing the percent of each of the state’s districts which is Scheduled Caste.

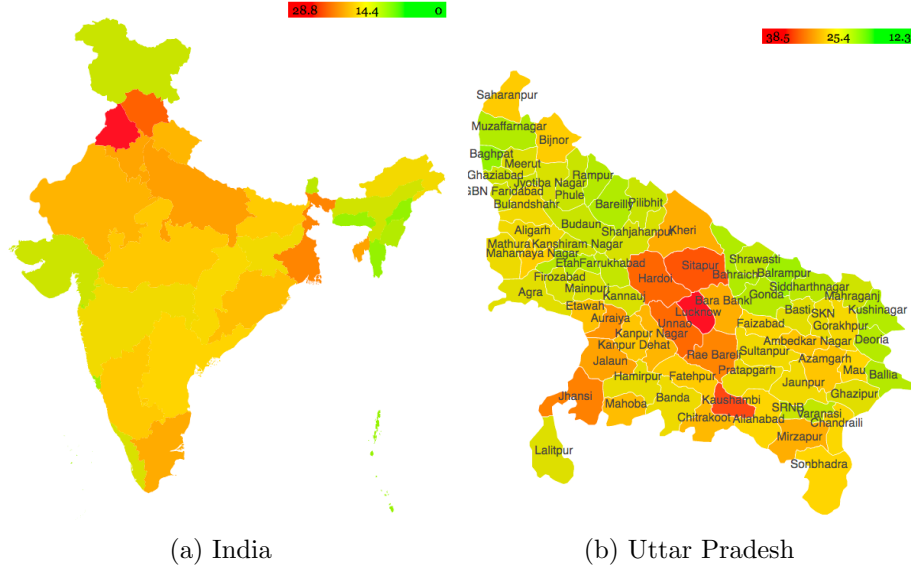


Figure 2: Share of Scheduled Caste population in India and in Uttar Pradesh

Figure 2 shows the percent of the population which is Scheduled Caste in each of India’s 35 states (Panel a) and in each of Uttar Pradesh’s 75 districts (Panel b). Based on 2011 Census.

Despite electoral enfranchisement and reserved public office quotas, Scheduled Castes, the majority of whom are rural and reside in villages, remain among the poorest and most disadvantaged segments of Indian society. On average, Scheduled Castes stand on the lower rungs of wealth (Zacharias and Vakulabharanam, 2011), access to health services (Thapa et al 2021), and health outcomes (Kowal and Afshar, 2015), and are the most likely to be limited to occupations associated with stigma and “untouchability” (Bhattacharjee, 2014).<sup>14</sup>

<sup>13</sup>However, “Scheduled Caste” continues to exclude relevant Muslim subgroups despite evidence that there also exist caste hierarchies and Dalit-type segregation within some Muslim communities in India (Samarendra, 2016; Trivedi et al, 2016). This reflects an official denial that Muslim subgroups also suffer from caste hierarchies. It also means that these groups are excluded from targeted programs such as PMAGY, as the latter determines eligibility based on the official Scheduled Caste designation.

<sup>14</sup>This includes, most prominently, manual removal of human excrement. About 1.3 million Scheduled

It is in this context and arguably in light of electoral concerns that PMAGY was conceptualized, as a program delivering a one-time “gap funding” transfer to each village in India with at least a 50% Scheduled Caste population. Although the program was rolled out by the BJP in 2018, its outlines were first sketched in 2009 by the then-incumbent Congress Party as way to to boost its base among the impoverished Scheduled Caste electorate. Writing in July 2009, the *Times of India* noted that “*For just 1 million Rupees [per village], Congress could carve a political role in Dalit politics worth a fortune, as the ‘Pradhan Mantri Adarsh Gram Yojana’ promises to help consolidate the [Congress alliance] leaders’ hands on its traditional votebank [...] What has Congressmen in glee is the political subtext of the scheme which gives [it] a direct role to cultivate Dalit [votes] at the grassroots*” (Ghildiyal, 2009). Uttar Pradesh in particular was central to this political calculation, as it would have the highest number of eligible villages (ibid), and given that a key opposing party in Uttar Pradesh is one which represents lower-caste groups (see also footnote 11).

The Bharatiya Janata Party implemented the first phase of transfers in November-December 2018, six months before the elections of 2019, with political concerns likely also driving the BJP’s timing and commitment to the program.<sup>15</sup> The selection of villages for this first round was highly standardized: *in every district* the selection started with the village with the highest absolute number of Scheduled Caste persons *among the eligible villages*, descended accordingly, and, in Uttar Pradesh, usually stopped after the tenth village.<sup>16</sup> Note that the homogeneity of program rollout between districts also implies that *district-level* variation (e.g. number of villages that received funds pre-election) cannot be used to measure the effect of transfers on any outcome of interest, confirming the importance of disaggregated village-level analysis.

---

Castes persons in India, mostly women, make their living from this dehumanizing (and officially outlawed) occupation.

<sup>15</sup>Prior to Nov-Dec 2018, 1,000 villages in the country had received funding in a “trial phase” in 2010, but none were in Uttar Pradesh. It is unclear why the Congress party did not roll out the program as intended, and I could not find sources explicitly addressing this issue.

<sup>16</sup>More precisely, of the 75 districts, the selection stopped after the 10th village in 62 districts. In 1 district it stopped after the 11th, in 5 districts after the 9th, in 1 district after the 8th, and in 2 districts after the 7th. The 4 remaining districts had very low numbers of eligible villages and these stopped after the 3rd (3 of them) and 1st (1 of them) village.

A total of 708 villages in Uttar Pradesh received their allotted transfer of about 1 million Rupees in this first (and only pre-election) phase.<sup>17</sup> An additional 1,552 villages received funding between September 2019 and February 2020, and another 3,823 villages afterward by 2022, also all selected strictly by descending absolute number of Scheduled Caste members among the remaining eligible villages per district.

The rest of this paper is an investigation into whether these transfers help explain the BJP’s electoral advantage in 2019 among the target Scheduled Caste communities.

### 3 Theoretical framework

This section presents a simple model of voting behavior in response to received or future transfers, and derives the treatment effects to be estimated empirically.

#### 3.1 Setup

Let there be three types of villages, with village type  $v \in \{a, b, c\}$ , and consider one transfer program,  $p$ , which allocates benefits to villages based on type. Let there be two electoral cycles, one at time  $t_0$  and one at  $t_1$ . Two parties  $R$  and  $L$  compete electorally in both cycles, and both can pledge to transfer  $T_v^k$ , where  $k \in \{R, L\}$ , to villages type of  $v$  if they win.  $T$  can be understood to provide some kind of public good whose consumption everybody in the village benefits from.<sup>18</sup>

Models of transfers and voting behavior (e.g. Dixit and Londregan, 1996; Grossman and Helpman 1996; Bardhan et al, 2022) show that in equilibrium parties make credible promises in that pledged transfers actually materialize if they win, and proceed to calculate what these pledges will be which maximize their chances of electoral victory. However, as the strategic behavior of the party itself is not the focus of this paper, I simply take  $T_v^k$  as exogenously

---

<sup>17</sup>This is the equivalent of about 16,000 USD in 2018 dollars, a nontrivial amount given the prevailing poverty levels and (small) village sizes; the average recipient village had a size of 1,300 people.

<sup>18</sup>Having  $T$  refer to a *per-capita* transfer (distributed equally to constituents in a village of type  $v$ ) does not alter the core results. It simply necessitates that any regression using village-level transfers as the independent variable also control for village size, to generate comparability of transfer per capita. The regressions in Section 6 control for both SC share (through the SC-share centered running variable) and for size of SC population (through the SC-size centered running variable), so they also effectively control for village size.

determined, stylizing it after the PMAGY disbursement structure (see below). This allows me to focus on village voting behavior *in response* to this (given) transfer structure.

As in the literature, I model individuals as voting on the basis of a combination of what they expect to benefit economically from each party and of their ideological preferences, and abstract from the possibility that everybody free rides by not voting.<sup>19</sup>

To see how people weigh their voting options, let all people within a specific village type share a utility function with respect to transfers, so that a transfer  $T$  yields utility for any person  $i$  in village type  $v$  equal to  $U_v(T)$ . Regarding ideology, and as in Dixit and Londregan (1996), let the affinity of person  $i$  in village  $v$  for party  $L$  be  $X_{iv}$ ; this expression allows for individuals' affinities to differ within the same village (type). Therefore, a person with  $X_{iv} > 0$  ( $X_{iv} < 0$ ) ideologically prefers  $L$  ( $R$ ); a person with  $X_{iv} = 0$  is ideologically neutral. Also similar to Dixit and Londregan (1996), I assume that although individuals can be different in their affinities, each village type  $v$  shares a *distribution* of affinities  $\Phi_v$ , where  $\Phi_v(X) \in [0, 1]$  describes a cumulative distribution function. Therefore, the value  $\Phi_v(0)$  is the share of people in village type  $v$  who have affinities to the left of 0 ( $X_{iv} < 0$ ) and thus prefer party  $R$ , so that  $\Phi_a(0) > \Phi_b(0)$  for example means type  $a$  villages lean more heavily toward  $R$  ideologically than  $b$  villages.

Finally, I allow for the possibility that people discount pledges by the non-incumbent by  $\gamma \in [0, 1]$ , imbuing a possible incumbency advantage. I also allow for the possibility of shocks to the affinity  $X$ ; depending on the source, shocks can be  $v$ -specific or general.

Assuming  $R$  was the winner of the  $t_0$  elections, then person  $i$  in  $v$  will vote for  $R$  during the  $t_1$  elections only if they expect to gain more economically from reelecting the incumbent  $R$  (over the opposition  $L$ ), in excess of their affinity for the opposition:

$$U_v(T_v^R) - (1 - \gamma)U_v(T_v^L) > X_{iv} \quad (3.1)$$

---

<sup>19</sup>The latter, while potentially a coherent Nash strategy, would predict a situation in which *nobody* votes, as each person has the incentive to let others incur the cost of voting for the preferred party. This would result in a zero turnout equilibrium, a phenomenon not backed up by the observation that participation rates are high especially in the Indian polity and among poor people (Bardhan, 2008).

where  $U(0) = 0$ ,  $U$  is concave in  $T$ , and where the right hand side can also be subject to a general or  $v$ -specific shock which increases or decreases affinity for  $L$  (see next subsection). Importantly, note that only *future* (post-election) transfers factor into the left hand side in Eq. (3.1). A (non-recurring) past transfer is predetermined, and not a channel through which electing different parties can impact utility; therefore, a person not eligible for *future* transfers would be facing  $T^k = 0$  and only voting based on ideological preference  $X_{iv}$ .

It remains to specify how village type is linked to transfers. Stylizing the model after PMAGY disbursements, let the rollout of  $p$  have been announced only after the conclusion of  $t_0$ . In the leadup to  $t_1$ , let it be that (i)  $a$  villages already received a (non-recurring) transfer, (ii)  $b$  villages are eligible to receive transfers after the elections, and (iii)  $c$  villages are ineligible for any transfers at any point. This means:

- In the lead up to  $t_0$ ,  $T_v^k = 0$  for all  $v \in \{a, b, c\}$  and  $k \in \{R, L\}$ .
- In the lead up to  $t_1$ ,  $T_v^k = 0$  for all  $v \in \{a, c\}$  and  $k \in \{R, L\}$ , while  $T_b^R$  and  $T_b^L$  can differ.<sup>20</sup>

### 3.2 Impact of transfers on incumbent vote share

First, to model the impact of *future* transfers on voting behavior, consider the relevant  $b$  villages. Following Eq. (3.1), a person  $i$  in those villages will vote for  $R$  if

$$U_b(T_b^R) - (1 - \gamma)U_b(T_b^L) > X_{ib} \quad (3.2)$$

Denote the cutoff ideological preference which equals the left hand side of Eq. (3.2) as  $X_b^*$ , so that  $X_b^* \equiv U_b(T_b^R) - (1 - \gamma)U_b(T_b^L) \gtrless 0$ . Any individual with  $X_{ib} < X_b^*$  will vote for  $R$ , so that the vote share for  $R$  in  $b$  villages will be

$$\Phi_b(X_b^*) = \Phi_b\left(U_b(T_b^R) - (1 - \gamma)U_b(T_b^L)\right) \quad (3.3)$$

---

<sup>20</sup>Since PMAGY is funded at the national level and given the very low rate of tax payments in villages, I do not assume that one group has to receive negative transfers (taxes) to fund another.

By contrast, and also in line with Eq. (3.1), an individual  $i$  in a  $c$  village faces  $T = 0$  and so will vote  $R$  only if

$$0 > X_{ic} \quad (3.4)$$

Similarly, denote the cutoff as  $X_c^* \equiv 0$ , so that the vote share for  $R$  in  $c$  villages is

$$\Phi_c(X_c^*) = \Phi_c(0) \quad (3.5)$$

Comparing the vote share for  $R$  between  $b$  and  $c$  villages, we obtain

$$\begin{aligned} \Delta_b &= \Phi_b(X_b^*) - \Phi_c(X_c^*) \\ &= \Phi_b(X_b^*) - \Phi_c(0) \end{aligned} \quad (3.6)$$

The net difference  $\Delta_b$  is composed of two parts: (i) the difference between the distribution function  $\Phi$ , reflecting difference baseline preferences for the parties, and (ii) the possible electoral advantage to the incumbent from future benefits, which pushes the cutoff point for voting for  $R$  out by  $X_b^*$ . For this reason, it would be difficult to disentangle the meaning of  $\Delta_b$ . For example, it might be that  $\Delta_b > 0$  not because transfers generate more economic-opportunism voting for  $R$  ( $X_b^* > 0$ ) but because  $b$  villages are already more  $R$  aligned ideologically ( $\Phi_b(x) > \Phi_c(x)$ ). Conversely, if  $b$  villages are *less*  $R$  aligned, then  $\Delta_b$  would be pushed downward.

If, however, very similar villages are compared with the exception of their transfer status, then all distinction between baseline ideological preferences would be neutralized. In this case, it would be possible to write  $\Phi_b = \Phi_c = \Phi$ , so that we obtain:

$$\begin{aligned} \Delta_b &= \Phi(X_b^*) - \Phi(X_c^*) \\ &= \Phi(X_b^*) - \Phi(0) \end{aligned} \quad (3.7)$$

In turn, this allows us to obtain more clear conclusions about the meaning of  $\Delta_b$ . Since  $\Phi$  is a cumulative distribution function and therefore non-decreasing in  $X$ ,  $\Delta_b > 0$  would imply  $X_b^* > 0$ , i.e. that benefits generate an electoral advantage for  $R$  in  $b$  type villages (more

people now fall to the left of the cutoff in these villages). By contrast,  $\Delta_b < 0$  would imply that benefits generate an electoral advantage for  $L$ . Finally,  $\Delta_b = 0$  would imply anticipation of transfers has no effect on electoral behavior, and that people in  $b$ , just like those in  $c$ , also vote based on ideological preference ( $X_b^* = 0$ ).

Given that  $X_b^* \equiv U_b(T_b^R) - (1 - \gamma)U_b(T_b^L)$ , it is possible to see that  $X_b^* \rightarrow 0$  if the incumbency advantage is low and (i)  $T_b^R$  and  $T_b^L$  are very similar, or (ii)  $U'$  is very small in the region of the transfers, so that even large differences do not translate into meaningful utility differences. These correspond, respectively, to a situation where (i) people believe the same benefits will be continued regardless of who wins, or (ii) people derive little utility from adjusting their vote according to welfare benefits, perhaps due to ideological rigidity or other reasons.<sup>21</sup>

Next, to explore the effect of *past transfer receipt* on voting behavior, consider  $a$  villages. Past transfers do not affect future utility calculations but it is possible that they cement loyalty for  $R$ . This can be represented as a negative shock to the affinity for party  $L$  in these villages, which I denote by subtracting  $\mathcal{R}_a > 0$  from the right hand side of Eq. (3.1). Therefore, individual  $i$  in  $a$  will vote for the incumbent  $R$  if

$$0 > X_{ia} - \mathcal{R}_a \quad ; \quad \mathcal{R}_a \geq 0 \quad (3.8)$$

Denoting the cutoff ideological preference by  $X_a^*$ , we now obtain  $X_a^* \equiv \mathcal{R}_a \geq 0$ . The vote share for  $R$  in  $a$  villages will therefore be

$$\Phi_a(X_a^*) = \Phi_a(\mathcal{R}_a) \quad (3.9)$$

Comparing the vote share for  $R$  between  $a$  and  $c$  villages, we obtain  $\Delta_a = \Phi_a(X_a^*) - \Phi_c(X_c^*) =$

---

<sup>21</sup>These issues are explored further in Section 6.



$\Phi_a(X_a^*) - \Phi_c(0)$  where, once more, baseline similarity among villages would allow us to write

$$\begin{aligned}\Delta_a &= \Phi(X_a^*) - \Phi(X_c^*) \\ &= \Phi(\mathcal{R}_a) - \Phi(0)\end{aligned}\tag{3.10}$$

Given that  $\Phi$  is nondecreasing, then  $\Delta_a > 0$  would imply  $\mathcal{R}_a > 0$ . In contrast,  $\Delta_a = 0$  would imply  $\mathcal{R}_a = 0$  so that no such “loyalty” effect is created among past recipients from transfers under  $R$ ’s incumbency.

As a final comment, consider a shock  $\mathcal{R} \gtrless 0$  prior to  $t_1$  elections to *all* villages regardless of type. Rewrite Eq. (3.1) as  $U_v(T_v^R) - (1 - \gamma)U_v(T_v^L) > X_{iv} - \mathcal{R} - \mathcal{R}_a$ , where  $\mathcal{R}_a = 0$  for  $v \in \{b, c\}$  by definition and  $\mathcal{R}_a \geq 0$  for  $a$ .  $\mathcal{R} > 0$  would be a general shock which increases loyalty for the incumbent  $R$  while  $\mathcal{R} < 0$  would be a shock that increases loyalty for the opposition  $L$ , in all village types.

Naturally, a common shock would not impact difference-of-means estimates between village types.<sup>22</sup> However, where this common shock would show is in comparisons of the vote share between  $t_1$  and  $t_0$  *within* each village type  $v$ . Denote this change between election cycles as  $\tilde{\Delta}_v$ . Then we obtain (and recalling  $T = 0$  for all villages in  $t_0$ ):

$$\begin{aligned}\tilde{\Delta}_v &= \Phi_{v,t_1} - \Phi_{v,t_0} \\ &= \Phi_v(X_v^* + \mathcal{R}) - \Phi_v(0)\end{aligned}\tag{3.11}$$

where  $X_v^*$  is the cutoff excluding the common shock. Suppose there are no effects of future nor past transfers so that  $X_b^* = X_a^* = 0$ , and we know that by definition  $X_c^* = 0$ , but that there is a common shock to all villages  $\mathcal{R}$ . Further, suppose an estimator can generate baseline similarity among village types, so that  $\Phi_v = \Phi$ . Then, by Eq. (3.11), we would see the *same* shift in vote share  $\tilde{\Delta} = \Phi(\mathcal{R}) - \Phi(0)$  within all village types between  $t_1$  and  $t_0$ . Given that  $\Phi$  is non-decreasing, then  $\tilde{\Delta} > 0$  would imply  $R$  is now more popular everywhere,

---

<sup>22</sup>To see this clearly, let  $X_v^*$  be the cutoff *excluding* the common shock. Therefore, as before,  $X_a^* = \mathcal{R}_a$ ,  $X_b^* = U_b(T_b^R) - (1 - \gamma)U_b(T_b^L)$ , and  $X_c^* = 0$ . Eq. (3.7) would become  $\Delta_b = \Phi(X_b^* + \mathcal{R}) - \Phi(0 + \mathcal{R})$  so that if  $\Phi$  is approximated linearly, the shock cancels out. Similarly, Eq. (3.10) would become  $\Delta_a = \Phi(X_a^* + \mathcal{R}) - \Phi(0 + \mathcal{R})$  so that the shock cancels out in a linear approximation.

whereas  $\tilde{\Delta} < 0$  would imply  $L$  is now more popular everywhere, for reasons unrelated to past or future transfers from  $p$ .

The RD design in this paper is an effort to measure the differences of means in Eqs. (3.7) and (3.10), to make inferences about the effect of future and past transfers on voting behavior, respectively. However, with data on voting outcomes from two cycles, inferences can also be made about a general change in affinity for the incumbent.

To do this, data is needed on transfer status and electoral outcomes at the village level, so as to construct  $\Phi_v(X_v^*)$ .

## 4 Data

Data on PMAGY transfers to villages is obtained from the “Funds Released” and “Villages Covered” reports on the [PMAGY portal](#) run by the Department of Social Justice and Empowerment. The reports record the name and unique six-digit Census code of each village which received funding, the phase/time it received the funding, and the (standardized) amount for that transfer cohort. Since the PMAGY reports use the unique six-digit 2011 Census codes to identify villages, matching to the 2011 Census to obtain information on each village’s characteristics is straightforward.<sup>23</sup>

From the PMAGY reports, I extract the list of Uttar Pradesh villages which received transfers as well as the timing of the transfers. Due to the regression discontinuity design and time constraints posed by matching, I focus on recipient villages with a maximum of 58% Scheduled Caste population, keeping in mind that the analytical bandwidth on that front will likely be narrower. I use the 2011 Census data to identify all other eligible ( $\geq 50\%$ ) as well as ineligible ( $< 50\%$ ) villages in Uttar Pradesh within the sufficiently wide 8% bandwidth of the cutoff.<sup>24</sup> The result is the set of all 7,499 villages within the bandwidth in Uttar Pradesh,

---

<sup>23</sup>For each village as well as town in India, the 2011 Census provides information on the following, among others: the village/town’s state, district, and subdistrict; total population, Scheduled Caste population, and Scheduled Tribe population; and number of men, women, minors, literate residents, and working residents. The PMAGY reports use the total population and Scheduled Caste population of each village as recorded by the 2011 Census, to calculate eligibility via the 50% threshold criteria.

<sup>24</sup>I also condition on a population of at least 500 people, since PMAGY was only rolled out for villages above this size, and the regression discontinuity is along the Scheduled Caste percent dimension. This also makes

with markers for their PMAGY eligibility and transfer status.

Data on electoral outcomes for the 2019 election is obtained at the most disaggregated level (polling booth) from the [website](#) of the Chief Electoral Officer of Uttar Pradesh. Information is provided on the electorate, turnout, and votes-by-party numbers for each of the approximately 160,000 polling booths across 403 “Assembly Consistencies” (ACs) in the state.<sup>25</sup> Each polling booth has a booth number, which together with its AC number constitutes a unique combination; for example, polling booth Number 390 in AC 71 identifies a unique location. Each booth’s name is also written out, and the name is frequently related to the village(s) it serves. Close to half of the raw data is in English, while the rest is a mix of Hindi and Kruti Dev code; I use Python code to translate the latter two into English. I also match each polling booth to its Parliamentary Constituency (PC) by using [Maps of India](#) to link ACs to PCs. Electoral outcomes by booth for the 2014 election are similarly available from the CEO website, as well as in compressed English format through the repository of Susewind (2014).

To match each village to the booth(s) in which it voted, I proceed in two steps. First, after classifying each village by its district in Uttar Pradesh and doing the same with all the polling booths, I attempt to match each village to a polling booth in 2014 in the same district by polling booth name. This process is complicated by the presence of many villages with the same or similar names within the same district, compounded by naming errors from the translation of Hindi names into English, as well as the fact that some smaller villages vote in booths named after (and primarily intended for) larger neighboring villages. To overcome these issues, I use a mix of the following resources: the “booth parts” component provided in the webscraped Susewind (2014) list; the six digit unique code identifier of each village because codes are typically very close for neighboring villages; the sequence of polling

---

sense from a logistical standpoint, as the majority of very small villages do not have polling booths dedicated primarily to them and so either cannot be matched or the matched booths will not reflect predominantly voting in that village; see below.

<sup>25</sup>In Uttar Pradesh in 2019, the average polling booth serviced about 900 individuals (electors), and approximately every 400 booths were classified into an AC. Every couple of ACs (usually 3 to 6) comprise a Parliamentary Constituency which shares the same candidates across all parties. The 80 members of the Lok Sabha elected from Uttar Pradesh are the winners of the 80 Uttar Pradesh PCs.

booths in each AC because booths are also often listed in order of geographical proximity; a comparison of village population with booth electorate; and Google Map confirmation of village distribution. This time intensive process but which generates the highest possible accuracy is explained in more detail in **Appendix A**.

Second, I use the above matching of villages to where they voted in 2014, to match them to where they voted in 2019. This is because, while from 2014 to 2019 many booths were split into two or (less frequently) merged, resulting in a change of the booth number identifiers, booths remained within the same AC and for the most part listed within a similar sequencing order.<sup>26</sup> I then double check the accuracy of the 2019 matches using the same auxiliary resources mentioned above, with details also outlined in the Appendix. Overall, the process generates matches for over 6,300 villages - an 85% matching success rate - with resulting information on the electoral outcomes in the booth(s) in which each village voted in 2014 and 2019.<sup>27</sup>

Finally, although I was able to match most villages to polling booths, not all matches are equally *useful* for the empirical analysis. Small villages were often voting together or with larger villages in the same booth. For example, a village of size 600 may be voting in a booth where the total electorate (as indicated from the booth information) is 1,500 people, due to the inclusion of other villages as well. In this case, even though I am certain this is where the village voted, the voting outcomes at the booth level are *not* indicative of voting preferences in that specific village. By contrast, booths dedicated to one village, or where one village dominates very clearly by size, are informative about voting preferences in that village. From my matching efforts, I was able to observe that polling booths dedicated primarily or only to one village (as inferred from “booth constituency” listings) had a booth electorate (*not*

---

<sup>26</sup>For example a booth with a specific name in AC 71 may have been numbered Booth 352 in 2014, but numbered Booth 370 (in the same AC) in 2019. Another booth may have been numbered Booth 80 in 2014 but then split into Booth 88 and Booth 89 in 2019.

<sup>27</sup>The matching success rate is closer to 90% when taking into account that some villages could not be matched due to the absence of polling booth information in two ACs. Particularly, there is no information on the polling booths in ACs 264 and 265 in the district of Allahabad, due to technical error from the CEO Uttar Pradesh website. The majority of other villages which could not be matched are the smallest villages which do not show up as either part of the booth name nor booth part description, or villages with very similar names that are also very close neighbors geographically.

turnout) in 2019 which was usually somewhere between 60% to 90% of the village population, with some deviations in both directions.<sup>28</sup> Therefore, in the analytical exercises, I use villages where the electorate of the matched booth is between 0.5 and 1.0 of the village population size, to ensure that the booth largely reflects the preferences of the village in question.

This narrows the number of villages in the dataset with informative booth matches for the 2019 election to 5,039. The number of villages with informative matches in both 2014 and 2019 is slightly lower, at 4,837.

## 5 Research design

As shown in Section 3, (i) a simple difference of means in outcomes between treated and untreated villages would not isolate the effect of transfers on outcomes of interest ( $\Phi$  is different between village types), and (ii) transfers can impact voting behavior in two distinct ways, depending on timing of receipt ( $\Delta_b \neq \Delta_a$ ).

To overcome the selection problem, I use the arbitrary cutoffs of the program in a regression discontinuity design (RDD). Intuitively, the idea is that villages just above and below the cutoffs are similar with the exception of their treatment status. To accommodate the possibility of two distinct treatments, I use a *multi-score* sharp RDD which allows for heterogeneous treatment effects. In this section, I detail and assess the research design.

### 5.1 Reduced form RDD

**Figure 3** illustrates clearly that the rollout of transfers was informed by the 50% Scheduled Caste share rule but not entirely determined by it. Therefore, it is not possible to run a sharp RDD on the 50% cutoff to gauge the effect of either pre or post-election transfers on voting behavior. Similarly, it is not possible to use a fuzzy RDD where eligibility (crossing the 50% threshold) instruments for either treatment, precisely because eligibility can affect

---

<sup>28</sup>For example, a 2019 booth which I matched to be servicing only *or* primarily a village whose size was 1,300 (in 2011), would typically have an “electorate” figure between 800 to 1200.

voting behavior in two conceptually distinct ways, violating the validity of the instrument.<sup>29</sup>

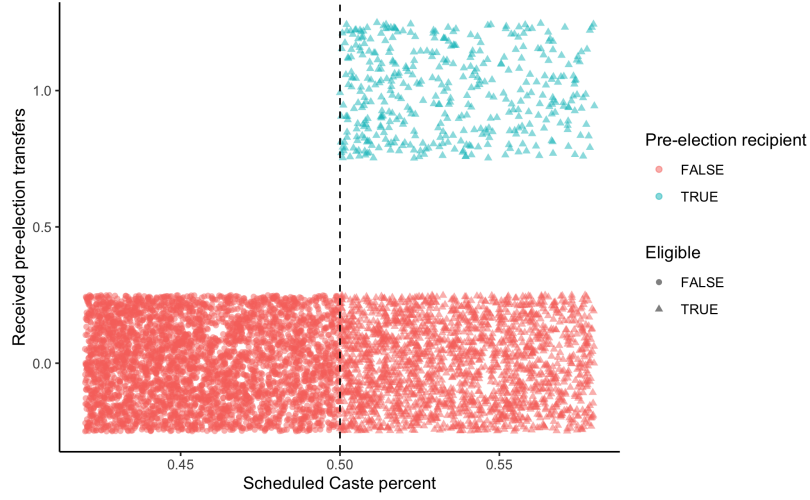


Figure 3: Single discontinuity

Figure 3 plots the percent of village population which is Scheduled Caste on the  $x$ -axis and whether or not the village had received transfers prior to the election on the  $y$ -axis. Each observation is a village. Blue observations received pre-election transfers, while triangle observations are those meeting the eligibility threshold.

Nonetheless, it is possible to use the 50% threshold to identify the effect of *general eligibility* for the program on outcomes of interest, without distinguishing between the two possible channels. Let eligibility for each village  $i$  be  $Z_s \in \{0, 1\}$ , where  $s$  reflects Scheduled Caste ( $SC$ ) share.  $Z_s = 1$  if  $SC_i \geq 0.5$  and 0 otherwise. The “reduced form” sharp RDD is:

$$Y_i = \theta_0 + f(SC_{i,s} - 0.5) + \theta_1 Z_i + \theta_2 PC_i + e_i \quad (5.1)$$

where  $f$  is a function of the centered running variable, potentially with an interaction with  $Z$ , and the above is run on a bandwidth which optimizes the bias-variance tradeoff. I consider only linear and quadratic forms, to avoid bias from overfitting by higher order polynomials (Gelman and Imbens, 2019). Given that a valid RDD does not need controls (Lee and

<sup>29</sup>For example, suppose the treatment of interest is pre-election transfers. Then even within a bandwidth which guarantees baseline similarity among all villages, the instrument can impact outcomes not only through variation created for the treated group but also through variation created for part of the “control” group (the remaining eligible villages, i.e. future recipients). Simply excluding this group from the counterfactual is not a good solution either, as it would result in a biased estimator (Choi and Lee, 2018).

Lemieux, 2010), I control only for parliamentary constituency ( $PC_i$ ) to ensure comparison of villages facing the same candidates from each party.

The main outcome of interest  $Y$  is the share of the village's votes which went to the BJP in 2019. However, as a secondary outcome, I also explore the effect of eligibility on the village's turnout, calculated as total votes divided by its electorate. As long as the cutoff is not used in any other government program - which holds - then  $\theta_1$  identifies the (local) effect of barely crossing the eligibility cutoff on  $Y_i$ :

$$\theta_1 = \lim_{SC \rightarrow 0.5^+} E[Y|SC = 0.5] - \lim_{SC \rightarrow 0.5^-} E[Y|SC = 0.5] \quad (5.2)$$

where  $\theta_1$  is a mix of the effects of eligibility for future transfers and receipt of prior transfers.

## 5.2 Multi-score RDD

Next, I use a multi-score RDD with heterogeneous treatment effects, to separately estimate  $\Delta_b$  in Eq. (3.7) and  $\Delta_a$  in Eq. (3.10).

To do this, I use the key fact that pre-election transfer receipt was a deterministic function of a *combination* of the share and absolute number of Scheduled Caste persons. **Figure 4** plots the share of Scheduled Castes in the village on the  $x$ -axis, and the *size* of Scheduled Caste population in excess of relevant cutoff for the district on the  $y$ -axis.<sup>30</sup> It illustrates that, when both scores are taken into account, the discontinuities becomes 2-dimensional and *sharp*, i.e. it is possible to determine treatment status from the value of the scores.

As first applied in Reardon and Robinson (2012) and explored theoretically in Choi and Lee (2018), when two thresholds together produce sharp discontinuities, it is possible to perform a multi-score RD regression allowing for heterogeneous treatment effects as follows. First, for each village and letting  $s$  denote Scheduled Caste share and  $p$  denote Scheduled Caste population, let there be two scores:

---

<sup>30</sup>The program did not specify a cutoff cardinally, such as minimum size of 600 SC persons, but ordinally, by stopping after (most often) the 10th largest-SC (eligible) village in the district. Therefore, for each village  $i$  the  $y$ -axis is calculated as  $SC_i - SC_{min,dist}$ , where  $SC_{min,dist}$  is the number of Scheduled Caste persons in that last picked (usually 10th) village. The figure shows that villages which were larger than this but had less than 50% Scheduled Caste share did not receive transfers (upper left quadrant).

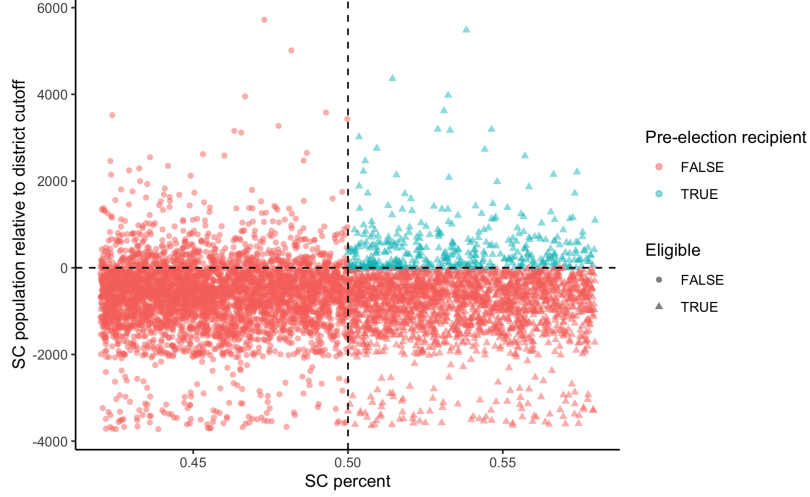


Figure 4: Multi-score sharp discontinuities

Figure 4 plots the share of Scheduled Castes on the  $x$ -axis and the size of the Scheduled Caste population relative to the district cutoff on the  $y$ -axis. Each observation is a village. Blue observations received pre-election transfers, while triangle observations are those meeting the eligibility threshold.

- $Z_s \in \{0, 1\}$  where  $Z_s = 1$  if  $SC_s \geq 0.5$ , and 0 otherwise
- $Z_p \in \{0, 1\}$  where  $Z_p = 1$  if  $SC_p \geq c$ , and 0 otherwise.<sup>31</sup>

This generates four possible score combinations, matching the quadrants in **Figure 4**. The following multi-score sharp RDD can disentangle the treatment effects:

$$Y_i = \beta_0 + f\left((SC_{i,s} - 0.5), (SC_{i,p} - c)\right) + \beta_s Z_{i,s} + \beta_p Z_{i,p} + \beta_r R_i + \beta_{pc} PC_i + e_i \quad (5.3)$$

where  $f$  is a function of the centered running variables,  $R_i = Z_{i,s} * Z_{i,p}$  and therefore equals 1 for pre-election recipients and 0 otherwise, and the specification is run on a bandwidth around both cutoffs.<sup>32</sup>

$\beta_s$  and  $\beta_r$  in Eq. (5.3) are the causal estimators of interest. In **Appendix B** I show

<sup>31</sup>Here,  $c$  is the population cutoff for that district, as explained in footnote 31.

<sup>32</sup>Bandwidth selection is difficult to derive formally in this case; Choi and Lee (2018) recommend starting from a sensible cutoff combination and then checking the robustness of the results to other cutoffs. In Section 6, I use a 5% bandwidth on each side of the share cutoff, as this is the bandwidth which optimizes the bias-variance tradeoff in the reduced form regression. For SC population, it does not appear that restricting observations on this dimension is necessary for generating baseline similarity *once share is restricted*; I explain further below. I check that results are robust to changing the bandwidth combinations in the different directions.



formally that the estimators can be expressed as:<sup>33</sup>

$$\beta_s = \lim_{SC_s \rightarrow 0.5^+, SC_p \rightarrow c^-} E[Y|\mathbf{S}] - \lim_{SC_s \rightarrow 0.5^-, SC_p \rightarrow c^-} E[Y|\mathbf{S}] \quad (5.4)$$

$$\begin{aligned} \beta_r = & \left( \lim_{SC_s \rightarrow 0.5^+, SC_p \rightarrow c^+} E[Y|\mathbf{S}] - \lim_{SC_s \rightarrow 0.5^+, SC_p \rightarrow c^-} E[Y|\mathbf{S}] \right) \\ & - \left( \lim_{SC_s \rightarrow 0.5^-, SC_p \rightarrow c^+} E[Y|\mathbf{S}] - \lim_{SC_s \rightarrow 0.5^-, SC_p \rightarrow c^-} E[Y|\mathbf{S}] \right) \end{aligned} \quad (5.5)$$

where  $\mathbf{S}$  references the value of the running variables at the cutoffs.

To see the relationship between these estimators and the theoretical model, let  $Y$  be vote share, and focus first on the formula for  $\beta_s$  in Eq. (5.4). It expresses the difference in vote share from just crossing the 50% eligibility threshold while being just below the size cutoff (i.e.  $b$  type villages, eligible for future transfers), versus just being under the eligibility threshold and also the size cutoff (i.e. “small”  $c$  type villages, ineligible for any transfers). Therefore, it is a direct comparison of  $\Phi_b(X_b^*)$  with  $\Phi_c(X_c^*)$ , with the bandwidth restriction generating  $\Phi_b = \Phi_c = \Phi$ . In turn,  $\Phi(X_b^*) - \Phi(X_c^*)$  is exactly the definition of  $\Delta_b$  in Eq. (3.7), so that  $\beta_s$  is the local estimator of  $\Delta_b$ :

$$E[\Delta_b|S] = \beta_s \quad (5.6)$$

Second, examine the formula for  $\beta_r$  in Eq. (5.5). The expression comprises the difference in vote share from moving just above the size cutoff among villages that are just barely eligible (first parenthesis, equivalent conceptually to  $\Phi_a(X_a^*) - \Phi_b(X_b^*)$  around the cutoff), minus any effect of moving just above the size cutoff among villages that are barely ineligible (second parenthesis). Since in the model there is no reason for just barely crossing the *size* threshold to affect vote share, I assume the second parenthesis is zero; this also renders  $\beta_p = 0$  in the

---

<sup>33</sup>The expected value would also be conditional on all other covariates  $\mathbf{X}$  included in the regression, here the  $f$  function and the parliamentary constituency.

regression of Eq. (5.3).<sup>34</sup> Therefore,  $\beta_r$  can be written as:

$$\begin{aligned}
\beta_r &= E[\Phi_a(X_a^*) - \Phi_b(X_b^*)|S] \\
&= E[\Phi_a(X_a^*) - \Phi_b(X_b^*) - \Phi_c(X_c^*) + \Phi_c(X_c^*)|S] \\
&\approx E[\Phi_a(X_a^*) - \Phi_c(X_c^*)|S] - E[\Phi_b(X_b^*) - \Phi_c(X_c^*)|S] \\
&= E[\Phi(X_a^*) - \Phi(X_c^*)|S] - E[\Phi(X_b^*) - \Phi(X_c^*)|S] \\
&= E[\Delta_a|S] - E[\Delta_b|S]
\end{aligned} \tag{5.7}$$

where the bandwidth restriction generates  $\Phi_a = \Phi_b = \Phi_c = \Phi$ . Combining Eqs. (5.6) and (5.7), we obtain:

$$E[\Delta_a|S] = \beta_s + \beta_r \tag{5.8}$$

In other words,  $\beta_s$  directly compares villages eligible for future transfers with control villages. Meanwhile,  $\beta_r$  compares villages that received pre-election transfers not with the control group but *with villages eligible for future transfers*; the intuition is that this is the “added” effect of transfer receipt, above and beyond the effect of (only) eligibility. Therefore, the *sum*  $\beta_s + \beta_r$  compares pre-election recipients to control villages.

### 5.3 Design assessment

To assess the research design, I first explore differences between, and discontinuities in, predetermined covariates around the threshold(s). **Table 1** reports the simple difference of means in key characteristics between pre-election recipients and all other villages in Uttar Pradesh, first for all villages, then by decreasing bandwidth around the 50% share cutoff, and finally by a narrow Scheduled Caste share *and* population size bandwidth. The characteristics considered are share of Scheduled Tribes (a distinct marginalized socioeconomic segment), percent of population which is literate, which works, and which is involved in “marginal”

---

<sup>34</sup>I set  $\beta_p = 0$  in Section 6 to generate full comparability with the theoretical model, so that all ineligible villages are part of the intercept. In Section 7 I check that including the size cutoff in the regressions does not alter results.

work, all as reported by the 2011 Census.<sup>35</sup>

Table 1: Difference between pre-election recipients and other villages in UP

Variable	All	+/-20 SC%	+/-5 SC%	Dual bandwidth
ST share	-0.01***	0.00	0.00	0.00
Literacy	-0.02***	-0.02***	-0.01***	-0.02**
Working population	0.01***	0.00	0.00	0.00
Marginal work population	0.01***	0.00	0.01	0.00
Observations	76,348	22,635	4,532	2,058

Table 1 compares key characteristics of the pre election recipient villages to all other villages, in Uttar Pradesh. The columns report the simple difference of means, first for all villages and subsequently for villages within the specified bandwidth of the share cutoff. The last column includes villages within +/-5% of the share cutoff and +/-600 SC persons of the size cutoff. Note: \*p<0.1; \*\*p<0.05; \*\*\*p<0.01.

When *all* villages in Uttar Pradesh are considered, it is clear that pre-election recipients are different: they have a lower share of Scheduled Tribes, lower literacy rates, and greater involvement in marginal work. However, these differences diminish with narrowing the bandwidth around the 50% eligibility cutoff; only a small difference in literacy rates remains in the 5% bandwidth. The last column additionally adds a bandwidth around population size: it includes only villages within 5% of the Scheduled Caste share cutoff *and* within 600 persons of the district-level Scheduled Caste population cutoff.<sup>36</sup> This additional restriction does not seem to add further baseline similarity among villages, while cutting the number of observations by more than half. This suggests that, once *share* is taken into account, the *absolute number* of Scheduled Caste persons makes little difference to key village characteristics.

**Table 2** follows a similar approach, comparing villages eligible for future (post-election) funding with all other villages in Uttar Pradesh. Once more, the key result is that restricting the bandwidth around the share appears to be sufficient for generating baseline similarity in observables among villages.<sup>37</sup>

<sup>35</sup>Marginal work is defined as as employment under six months per year. The Census does not report other important potential confounders such as average income level at the village level, but these are likely highly correlated with the reported variables.

<sup>36</sup>This was chosen as it reduces the sample size by not much more than half.

<sup>37</sup>The difference in literacy, while statistically significant at the ten percent level, is economically negligible, at less than 0.1%.

Table 2: Difference between villages eligible for future funding and other villages in UP

Variable	All	+/-20 SC%	+/-5 SC%	Dual bandwidth
ST share	-0.01***	0.00**	0.00	0.00
Literacy	-0.01***	-0.02***	0.00*	0.00
Working population	0.01***	0.00	0.01	0.00
Marginal work population	0.01***	0.00	0.00	0.00
Observations	76,348	22,635	4,532	2,058

Table 2 compares key characteristics of the villages eligible for future funds to all other villages, in Uttar Pradesh. The columns report the simple difference of means, first for all villages and subsequently for villages within the specified bandwidth of the share cutoff. The last column includes villages within +/-5% of the share cutoff and +/-600 SC persons of the size cutoff. Note: \*p<0.1; \*\*p<0.05; \*\*\*p<0.01.

To formally check that there are no discontinuities in these variables around the cutoff(s), I use the RDD specifications in Sections 5.1 and 5.2 but with these village characteristics as outcomes. In **Table 3**, Column (1) shows the estimate for  $\theta_1$  from the reduced form RDD in Eq. (5.1). The remaining columns show the estimates for  $\beta_s$  and  $\beta_r$  from the multi-score RDD in Eq. (5.3), when restricting the bandwidth around the Scheduled Caste share and when adding a Scheduled Caste population size restriction.<sup>38</sup> None of the specifications predict a jump in these variables.

Finally, **Figure 5** shows continuity in both Scheduled Caste share and absolute size running variables, with no sign of sorting around the cutoffs to indicate manipulation. This is unsurprising, as both thresholds are calculated based on pre-existing 2011 Census counts. More formally, a test following McCrary (2008) fails to reject the null hypothesis of continuous density around the threshold, for both running variables.

<sup>38</sup>Note that the number of observations falls here, relative to the tables simply comparing means. This is because the exact regression specification involves parliamentary constituency, so it is necessary to use observations with useful booth matches. This also generates full comparability with the results in Section 6, as these are the villages on whom the main analysis is run.

Table 3: Testing for discontinuities in village characteristics

Coefficient	<i>Reduced form</i>		<i>Multi-score</i>		
	$\pm 5\%$	$\pm 5\%$		Dual bandwidth	
	$\theta_1$	$\beta_s$	$\beta_r$	$\beta_s$	$\beta_r$
ST share	0.0008 (0.0006)	0.0005 (0.002)	0.0015 (0.0013)	0.001 (0.002)	0.001 (0.002)
Literacy	-0.003 (0.006)	-0.002 (0.006)	-0.005 (0.006)	-0.0001 (0.006)	-0.002 (0.008)
Working population	0.01 (0.008)	0.01 (0.01)	-0.007 (0.008)	0.01 (0.01)	-0.017 (0.012)
Marginal work population	0.0002 (0.008)	-0.001 (0.008)	0.005 (0.008)	0.0001 (0.01)	0.002 (0.01)
Observations	3,034	3,034	3,034	1,498	1,498

Table 3 presents the results of Eq. (5.1) (Column 1) and of Eq. (5.3) (Col 2-5) with village characteristics as outcomes, and with PC-clustered robust standard errors in parenthesis. For the multi-score RDD, the first two columns use villages within a 5% bandwidth of the eligibility cutoff, while the last two additionally restrict Scheduled Caste population size to be within 600 of the relevant cutoff.

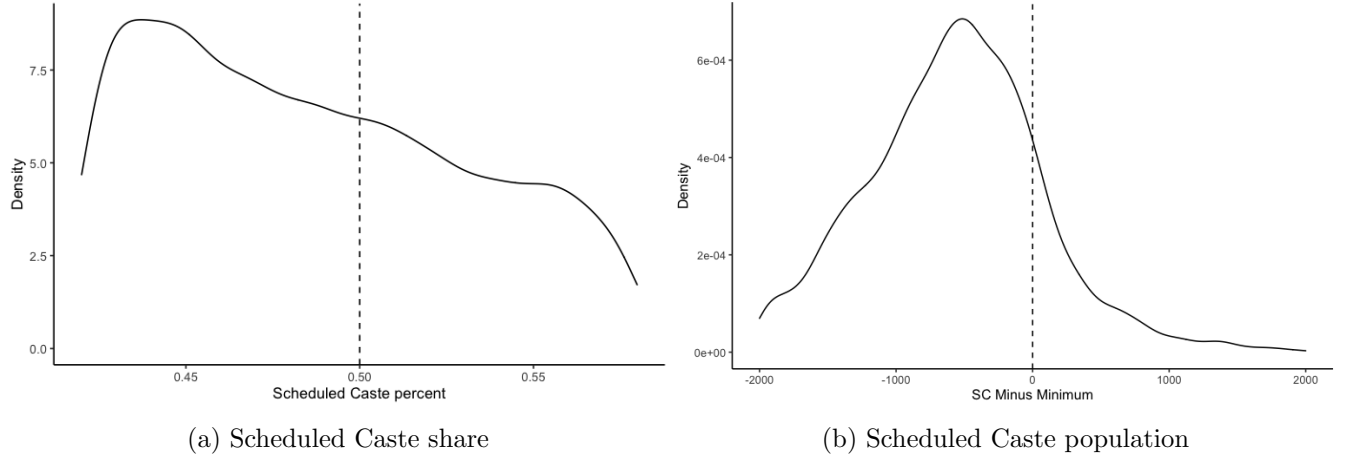


Figure 5: No manipulation of thresholds

Figure 5 plots the density of both running variables. In Panel (a), a vertical line indicates the common cutoff of 0.50 for Scheduled Caste share, and demonstrates no sign of sorting. In Panel (b), Scheduled Caste population for each village is reported net of the population minimum in the village's district, so that 0 is the common cutoff. Similarly, there is no sign of sorting around the threshold.

## 6 Results

### 6.1 Descriptive results

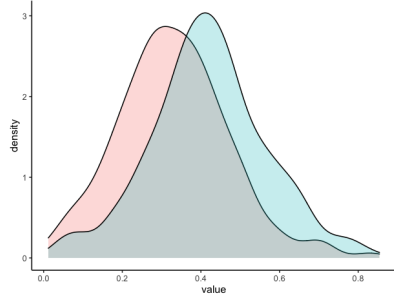
First, matching villages to polling booths allows me to describe and plot voting patterns at the village level and by village characteristics.

Specifically, for the UP villages with 42%-58% Scheduled Caste share which I was able to generate useful booth matches for ( $N = 4837$ ), there is a clear shift in favor of the BJP. With information on each village's votes in 2014 *and* 2019, I am able to calculate a mean *change* in vote share for the BJP at the village level of 9.2 pct points - from 35.4% to 44.6% of the total village vote - representing a 26% increase. Whereas votes for the BJP in Uttar Pradesh *in general* increased by 8.9 pct points from 2014 to 2019 (see Section 2), the initial vote share in these Scheduled Caste-heavy villages was lower (35.4% versus 42.3%), reflecting an even more resounding triumph for the BJP with this constituency in 2019. By contrast, turnout largely remained the same (average increase of 0.5 pct points) in this set of matched villages.

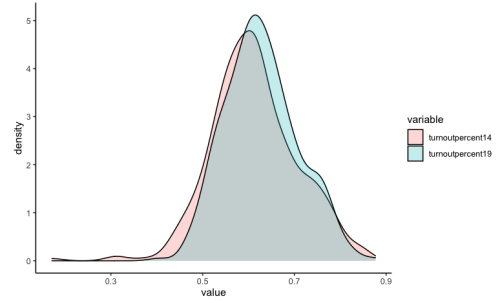
This implies that Schedule-Caste heavy villages in Uttar Pradesh *shifted* votes from other parties to the BJP between 2014 and 2019, and to a significant extent.

To anticipate the analytical results, **Figure 6** illustrates the distribution of vote shares for the BJP (as well as total turnout) for the matched villages ( $N = 4837$ ), in 2014 and 2019, grouping villages by transfer receipt status. It plots only villages which received transfers pre-election in the first row, only villages eligible for post-election transfers in the second row, and only ineligible villages in the third row.

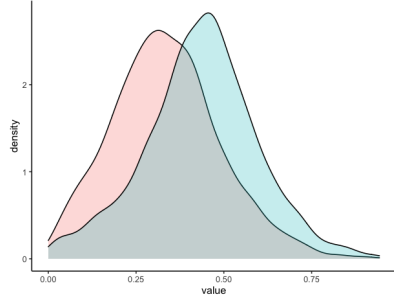
As shown, there is no discernible difference in votes for the BJP between these groups, neither in terms of voting in 2019 *nor* in terms of the shift between 2014 and 2019. Turnout density appears slightly higher for 2019 in villages that can anticipate future transfers. The next subsections confirm these results analytically, by employing the RD designs of Section 5.



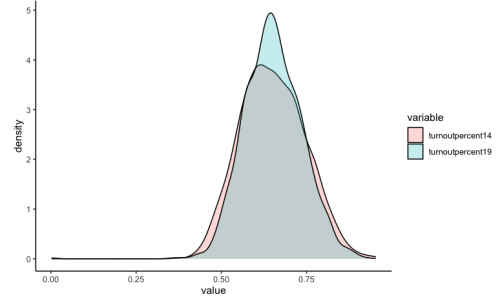
(a) Share of votes for BJP, pre-election recipients



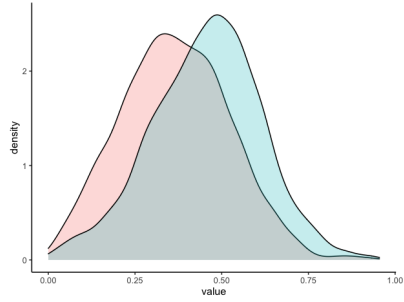
(b) Turnout, pre-election recipients



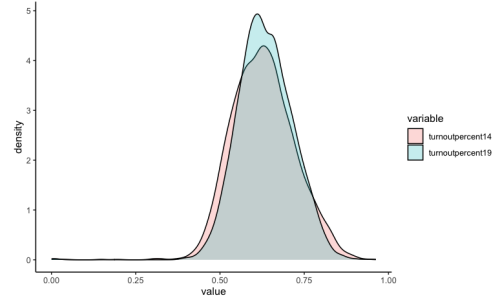
(c) Share of votes for BJP, future recipients



(d) Turnout, future recipients



(e) Share of votes for BJP, ineligible villages



(f) Turnout, ineligible villages

Figure 6: BJP triumph, by transfer status

For UP villages within an 8% bandwidth of the cutoff whom I matched to their polling booths, Figure 6 plots density of vote share for the BJP and turnout in pre-election recipients (Panels a and b), villages eligible for post-election funding (Panels c and d); and ineligible villages (Panels e and f).

## 6.2 Main results

Beginning with the reduced form RDD specification in Eq. (5.1), the results are displayed in **Table 4** and illustrated in **Figure 7**. In all tables, the standard errors used are robust and clustered by PC, and the 95% confidence interval is noted below the coefficient.

Table 4: Reduced form RDD

	<i>Dependent variable:</i>			
	Vote share for BJP		Turnout	
	(1)	(2)	(3)	(4)
Eligible	−0.003 (−0.024, 0.017)	0.005 (−0.026, 0.037)	0.011** (0.001, 0.021)	0.015* (−0.001, 0.031)
Functional form	Linear	Quadratic	Linear	Quadratic
PC controls	Yes	Yes	Yes	Yes
Observations	3,034	3,034	3,034	3,034
R <sup>2</sup>	0.218	0.219	0.359	0.360
Adjusted R <sup>2</sup>	0.198	0.198	0.343	0.343
Residual Std. Error	0.143 (df = 2957)	0.143 (df = 2955)	0.067 (df = 2957)	0.067 (df = 2955)

Note:

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table 4 reports the results of Eq. (5.1), with interactions to allow for differential slopes. The MSE-optimal bandwidth (45-55%) observations with useful booth matches are  $N = 3,034$ . In Col. (1) and (2), the dependent variable is vote share for the BJP; in Cols. (3) and (4), it is voter turnout.

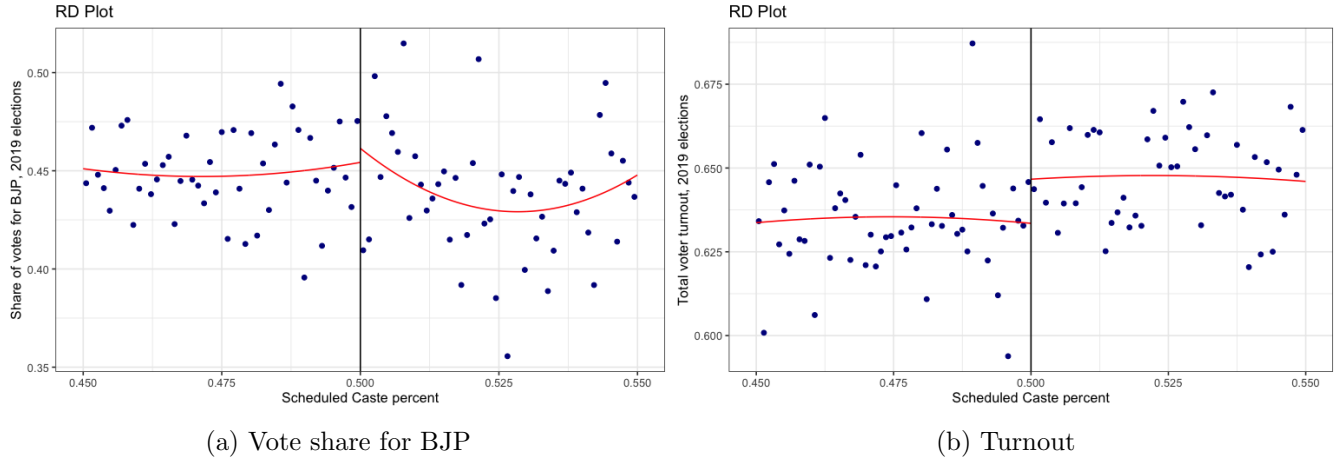


Figure 7: Reduced form RDD

Panel (a) and (b) illustrate the RDD estimates in Table 4, in Cols (2) and (4) respectively.

Regarding the key outcome of interest, the share of the villages' votes which went to the BJP in 2019, the consistent result is that crossing the eligibility threshold has no impact on this variable. Point estimates are close to zero, and at the 95% confidence interval effects greater than 1.7 (3.7) pct points can be ruled out in the linear (quadratic) specification. There does appear to be a modest effect on overall turnout, with villages just above the



eligibility cutoff having 1.1 (1.5) pct points higher turnout than those just below the cutoff, significant at the 5% (10%) level.

Next, to disentangle the effects of treatment by transfer receipt status, **Table 5** presents the results of the multi-score sharp RDD in Eq. (5.3), for share as well as dual share-and-size bandwidth restrictions.<sup>39</sup> The coefficient on “Eligible” is  $\beta_s$  while the coefficient on “Pre-election recipient” is  $\beta_r$ . As explained in Section 5, the former isolates the effect of crossing only the eligibility threshold, while the latter shows the added effect of also crossing the size threshold.

Table 5: Multi-score sharp RDD with heterogeneous treatment effects

	<i>Dependent variable:</i>			
	Vote share for BJP		Turnout	
	(1)	(2)	(3)	(4)
Eligible	−0.003 (−0.024, 0.019)	0.009 (−0.022, 0.040)	0.013** (0.003, 0.024)	0.006 (−0.009, 0.020)
Pre-election recipient	−0.001 (−0.022, 0.020)	−0.001 (−0.029, 0.026)	−0.011** (−0.020, −0.001)	0.001 (−0.011, 0.013)
Bandwidth	Share only	Dual	Share only	Dual
PC controls	Yes	Yes	Yes	Yes
Observations	3,034	1,498	3,034	1,498
R <sup>2</sup>	0.218	0.231	0.395	0.376
Adjusted R <sup>2</sup>	0.197	0.188	0.379	0.341
Residual Std. Error	0.144 (df = 2954)	0.144 (df = 1418)	0.065 (df = 2954)	0.063 (df = 1418)

*Note:*

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table 5 reports the results of Eq. (5.3), with  $\beta_p=0$  to generate complete comparability with the theoretical model, so that all ineligible villages are part of the intercept. I use a quadratic specification for the centered running variables. The restriction on share only (45-55% SC share) generates  $N = 3,034$ , while the restriction which also adds a bandwidth of 600 SC persons from the size cutoff reduces the sample by half, to  $N = 1,498$ .

In Columns 1 and 2, where the outcome is the village’s 2019 vote share for the BJP, the coefficients are very small and close to zero. Neither crossing only the eligibility cutoff nor crossing both cutoffs appears to influence BJP vote share, and effects greater than 4 percentage points can be excluded at the 95% confidence level. We cannot reject the null hypothesis that  $\beta_s = 0$  nor that  $\beta_r = 0$ .

<sup>39</sup>I use the MSE-optimal bandwidth of 5% for the SC-share bandwidth. For simplicity, I do not include interactions for differential slopes, as the multi-score dimension would imply numerous different possible interaction terms (Choi and Lee, 2018).

In Columns 3 and 4, where the outcome is the village’s 2019 turnout, the preferred specification with greater power - which restricts only on Scheduled Caste share - picks up an effect of 1.3 pct points on crossing the eligibility threshold and an almost equal negative effect on crossing both thresholds, and both are significant at the 5% level.<sup>40</sup> Although this outcome is not structurally derived, it remains true econometrically that the effect of future transfers is  $\beta_s$  while the effect of past transfers is  $\beta_s + \beta_r$ .

Note that **Table 5** provides a formal test for the null  $\beta_s = 0$  (and for  $\beta_r = 0$ ), and only informally suggests that  $\beta_s + \beta_r = 0$  in all columns. To formally examine the latter, note that  $\beta_s + \beta_r = 0$  would imply that  $\beta_r = -\beta_s$ . The corresponding restricted version of Eq. (5.3) becomes  $Y_i = \beta_0 + f\left((SC_{i,s} - 0.5), (SC_{i,p} - c)\right) + \tilde{\beta}(Z_{i,s} - D_i) + \beta_{pc}PC_i + e_i$ , where  $\tilde{\beta} = \beta_s = -\beta_r$ .<sup>41</sup> In **Table C1** in **Appendix C**, I replicate each of the columns in Table 5 but with this restriction, by regressing  $Y$  on a composite “*Eligibility*” - “*Pre-election transfer*” variable, and examine whether this restriction results in a significant loss of explanatory power. **Table C2** shows that, for each of the four columns, the null that the restricted model is equally as good as the unrestricted model, i.e. that  $\beta_s + \beta_r = 0$ , cannot be rejected in ANOVA tests. This lends support to the informal observation that this sum is not different from zero in all specifications.

### 6.3 Discussion

First, regarding turnout, the results suggest that eligibility for future transfers increased village turnout by just over 1 percent, while receipt of prior transfers had no impact. This implies the effect picked up in the reduced form RDD (**Table 4**) is driven precisely by the former group. On a methodological note, the presence of this “partial” effect (from crossing only one threshold) supports the value of a multi-score specification.

Regarding the effect on incumbent vote share, the results imply that:

$$\Delta_b = 0 \tag{6.1}$$

---

<sup>40</sup>The significance disappears with the smaller sample size after the dual restriction.

<sup>41</sup>As in the unrestricted model, I set  $\beta_p = 0$ .

$$\Delta_a = 0 \tag{6.2}$$

Recall that  $\Delta_b = 0$  when  $U_b(T_b^R) - (1 - \gamma)U_b(T_b^L) = 0$ , whereas  $\Delta_a = 0$  when  $\mathcal{R}_a = 0$ . Therefore, Eq. (6.2) suggests directly that there is no loyalty effect created among former recipients; in contrast, there are several possible explanations for Eq. (6.1).

As mentioned earlier,  $\Delta_b = 0$  could arise from a low incumbency advantage and that villagers expect future transfers to be equal regardless of who wins ( $T_b^R = T_b^L$ ), possibly due to the clear eligibility criteria of the program, or from villagers being ideologically rigid (causing low  $U'$ ), or a combination of these. As another possibility not incorporated explicitly in the model,  $U'$  may also be very small with respect to public transfers with clear eligibility rules, when there are other clientilistic private benefits on the side which dominate the effect on voting behavior; this pattern has been found to hold in studies which document both types of benefits although it is difficult to know whether it is also relevant here without data on clientilistic benefits.<sup>42</sup> It is also possible that villagers are simply unaware of their eligibility for future transfers, although this is unlikely in this case.<sup>43</sup>

Only more data, possibly through field-level ethnographic work, could determine which channel is responsible for future transfers not impacting voting behavior in the villages under study, but the clear end outcome is that while the BJP triumphed in these villages, neither receipt of nor eligibility for this benefits program created the electoral advantage. Instead,

---

<sup>42</sup>Bardhan et al (2022) study political preferences in West Bengal, India, by using closed ballots (as part of the study, not in actual elections). They find that in the presence of both public good programs with clear eligibility rules and clientilistic private benefits - the latter in which politicians can condition payment on previous electoral support - people's voting preferences only respond to the latter. Earlier results from Benin also suggest that voters respond more strongly to promised private than public goods (Wantchekon, 2003). In the setting of the United States, Levitt and Snyder (1997) find that private benefits generate *less* of a response from voters than public benefits, but Bardhan et al (2022) note this is congruent with private individual-level benefits in the USA being based in strict eligibility rules while public programs such as infrastructure are subject to political discretion.

<sup>43</sup>This can be modelled by adding a simple information parameter which multiplies  $U_b(T_b^R) - (1 - \gamma)U_b(T_b^L)$ , and which is the share of "aware" villagers; in this case  $\Delta_b$  could be null because this information parameter is zero. Although it cannot be definitively excluded, this is an unlikely scenario given the strong presence of the BJP in most villages and what appears to be the party's significant publicity events around PMAGY. The "Gallery" section on the official website shows a sample of over 100 photos taken in PMAGY villages in Uttar Pradesh, mostly holding large sitdowns for residents, but also showing Hindi newspaper media coverage or distributing advertisements for the scheme. Empirically, the effect detected on *turnout* in these villages, albeit modest, supports the interpretation that at least some villagers were aware of the scheme, so that an information parameter would not be zero.

the large shift in BJP vote share in *all* villages (around the cutoff) between 2014 and 2019 corresponds to the situation of a general shock  $\mathcal{R} > 0$  in favor of the BJP, entirely exogenous to these treatments (Section 3). This may represent increased appeal across the board of the Hindu nationalist narrative, of Narendra Modi, or a mix of these and other related factors.

Finally, note that the results identify specifically the (lack of) effect of receipt of and eligibility for program transfers, and *not* all possible effects of the program on voting behavior. For example, it cannot be excluded that PMAGY’s rollout improved the BJP’s standing among *all* Scheduled-Caste heavy communities regardless of their transfer status, through a “dignity” channel: they felt heard and valued, *as a social group*, by the government. There would be no reason for this channel (alone) to impact barely-ineligible villages differently than barely-eligible villages, so its effect would likely not be picked up in a discontinuity design, and would instead form part of the general shock increasing affinity for the BJP across all (similarly Scheduled Caste-heavy) villages.<sup>44</sup> Nevertheless, to the extent that transfer eligibility and receipt are themselves of interest, the results shed skepticism on the widespread notion that such treatments have been key for coopting poorer populations into the BJP’s base, at least for this important constituency.

## 7 Robustness and falsification tests

The following robustness and falsification tests are all presented in **Appendix C**. Regarding functional form, **Table C3** shows that nearly identical results are obtained for the multi-score RDD when allowing  $\beta_p \neq 0$ , so that moving just above the size threshold can have an effect on outcomes.<sup>45</sup> **Table C4** shows that the multi-score RDD also generates nearly identical results with the use of a linear (instead of quadratic) specification.<sup>46</sup>

<sup>44</sup>In fact, in this case the treatment would be a function of SC village share, so a research design which uses very *dissimilar* villages in terms of SC share, for example 20% versus 80%, would be necessary to generate plausible treatment variation. Of course, the problem is that this would also introduce significant selection issues which undercut identification.

<sup>45</sup>The interpretation that  $\Delta_b = \beta_s$  continues to hold. However, as shown in **Appendix B**, then if  $\beta_p \neq 0$ ,  $\Delta_a = \beta_s + \beta_p + \beta_r$ . The columns suggest that this sum is not different from zero in all specifications.

<sup>46</sup>This is similarly established for the reduced form RDD in Table 4 in the main results, which embeds both linear and quadratic specifications.

Regarding bandwidth, for the reduced form and multi-score RDDs **Table C5** and **Table C6** show results using a 7%, 6%, and 4% share bandwidth, as well as using a 5% share bandwidth combined with a population size restriction of  $\pm 700$  and  $\pm 500$  Scheduled Caste persons. The results are very similar to those in Section 6.2, with the bandwidths with more observations predictably generating smaller standard errors.<sup>47</sup>

**Table C7** considers a *change* in outcomes as the relevant outcome, instead of levels. Now, the dependent variables are calculated as, for each village (i) its vote share for the BJP in 2019 minus its vote share of the BJP in 2014, and (ii) its turnout in 2019 minus its turnout in 2014. For exercises involving both 2014 and 2019 electoral data for each village, it is necessary to restrict the sample size slightly, to limit errors from possible changes in booth composition between the years.<sup>48</sup> The key finding that BJP share is not impacted by general program eligibility in the reduced form specification, nor by anticipation of or receipt of funds in the multi-score specification, remains intact. The coefficient on turnout loses some significance ( $p = 0.14$ ) in the reduced form specification possibly due to the loss of observations, but is significant at the 10% level in the multi-score specification.

Importantly, for a falsification exercise, **Table C8** presents the results from using lagged outcomes for the reduced form and multi-score specifications. Specifically, the outcomes now are the village's share of votes for the BJP in 2014, and its turnout in 2014. For the RDDs to be valid, it would be necessary that they do not predict jumps in these past outcomes. Indeed, all coefficients in all specifications are close to zero and insignificant, with confidence intervals ruling out any meaningful effects.<sup>49</sup> Therefore, **Table C8** complements the finding

---

<sup>47</sup>It can be shown that even at the wider 7% bandwidth, there is no statistical difference in village characteristics, along the lines of Section 5.3, and that the placebo and falsification tests generate the expected null effects (omitted).

<sup>48</sup>Although the matches generated ensure the village voted in the right booth and that booth outcomes are informative about village preferences, it is still possible that booth constituencies changed between the years. For example, to a village  $i$  in the sample, another (much smaller) village  $j$  outside the sample (e.g. with Scheduled Caste share 0.30) may have been added to vote in  $i$ 's booth in 2019. This would generate some error in calculating the change between 2019 and 2014 as owing to a change in village  $i$ 's preferences. Although this error cannot be eliminated entirely, I reduce it by including only villages where the number of legitimate electors listed under the booth is at most 20% different between 2014 and 2019. This narrows the number of observations in the MSE-optimal share bandwidth slightly, from  $N = 3,034$  to  $N = 2,841$ . Stronger restrictions result in greater loss of observations and of regression power.

<sup>49</sup>Because the RDD does not predict any jump in these lagged outcomes, it can also be shown that including them as controls in the baseline specifications does not alter results (omitted).

in Section 5.3 that villages within the (share) threshold are balanced on covariates, as it shows that this holds also with respect to baseline political preferences.

## 8 Conclusion

In 2019, the Indian polity voted in the largest democratic exercise yet in history and reelected the Hindu-nationalist Bharatiya Janata Party to parliamentary majority by a wide margin. Although the drivers of the BJP’s triumph are certainly multifaceted and heterogeneous across the country, there remains little causally identified evidence even at the local level of what has propelled and sustained the party’s popularity.

This paper offers the first disaggregated quasi-experimental evidence, using actual election outcomes, on the extent to which economic transfers to disenfranchised populations coopted these groups into the BJP’s base in 2019. I focus on villages with a high share of Scheduled Castes - a core “swing” constituency - in India’s largest state. I study the effect on their vote share for the BJP of a national program which, beginning in 2018, targeted them for one-time transfers.

I first provide a simple theoretical model of voting behavior in response to past and future transfers, and use this to derive the treatment effects of interest. I then employ a multi-score regression discontinuity design to estimate these treatment effects empirically. The research design overcomes selection issues through exploiting arbitrary discontinuities in program thresholds, and further disentangles the effect of receipt of past transfers from the effect of eligibility for future transfers. The empirical application is possible because I undertake a process of matching villages to the booths in which they voted, allowing this paper to offer one of the few analyses of electoral outcomes in India using village-level variation.

I find that while the BJP triumphed in the villages under study, it did so to a similar extent in both treated and control villages. The discontinuity design shows that neither past receipt nor eligibility for future transfers created the electoral advantage, while the latter treatment increased voter turnout in the village by a modest amount (just over 1%). In line with the theoretical model, this implies that transfers had no effect on the average voter’s

utility function nor on their political loyalties. Instead, factors exogenous to these treatments, including possibly the appeal of the Hindu-nationalist narrative itself, the popularity of Narendra Modi, or other considerations, shifted affinity for the BJP across the board.

The results shed skepticism on the notion that, for the constituency under study, economic benefits have been key for coopting this largely poor population into the Hindu-nationalist electoral base. Further empirical research can confirm the extent to which other types of transfers, as well as factors rooted in the political narrative itself, have contributed to the recent slide toward ethnic nationalism in the Indian democracy.

## References

- [1] Aiyar Y. (2019) Modi consolidates power: Leveraging welfare politics. *Journal of Democracy*, 30(4):78-88.
- [2] Anelli M, Colantone I and Stanig P. (2019) We were the robots: automation and voting behavior in Western Europe. *IZA Discussion Paper* No. 12485.
- [3] Antoniadou A and Calomiris C. 2020. Mortgage market credit conditions and U.S. Presidential elections. *European Journal of Political Economy* 64:101909.
- [4] Autor D, Born D, Hanson G, and Majlesi K. (2020) Importing political polarization? The electoral consequences of rising trade exposure. *American Economic Review*, 110(10):3139-83.
- [5] Balachandran V. (2020) Jinnah’s role in weakening Indian territorial integrity. *Outlook*, Sep. 24.
- [6] Bardhan P. (2008) Democracy and distributive politics in India. In Shapiro I, Swenson P, and Panayides D (eds). *Divide and Deal*. New York: New York University Press.
- [7] Bardhan P, Mitra S, Mookherjee D, and Nath A. (2022) How do voters respond to welfare vis-a-vis public good programs? An empirical test for clientelism. *Federal Reserve Bank of Minneapolis*, Staff Report No. 605.
- [8] BBC. (2019) “Citizenship Amendment Bill: India’s new ‘anti-Muslim’ law explained.” December 11.
- [9] Bhattacharjee B. (2014) Cleaning human waste: “Manual Scavenging,” caste, and discrimination in India. *Human Rights Watch Report*.
- [10] Blattman C, Emeriau M, and Fiala M. (2018) Do anti-poverty programs sway voters? Experimental evidence from Uganda. *Review of Economics and Statistics*, 100(5):891-905.

- [11] Bursztyn L. (2016) Poverty and the political economy of public Education spending: Evidence from Brazil. *Journal of the European Economic Association*, 14(5):1101–1128.
- [12] Choi JY and Lee MJ. (2018) Regression discontinuity with multiple running variables allowing partial effects. *Political Analysis*, 26:258-274.
- [13] Colanton I and Stanig P. (2018). Global Competition and Brexit. *American Political Science Review*, 112(2):201-18.
- [14] De Janvry A, Gonzales-Navarro M, and Sadeoulet E. (2014) Are land reforms granting complete property rights politically risky? Electoral outcomes of Mexico’s certification program. *Journal of Development Economics*, 110(C):216-225.
- [15] De La O, A. (2012) Do conditional cash transfers affect electoral behavior? Evidence from a randomized experiment in Mexico. *American Journal of Political Science*, 57(1):1-14.
- [16] Dehadri S. (2020) Economic distress and support for radical right parties: Evidence from Sweden. *Comparative Political Studies*, 55(2):191-221.
- [17] Dionne K and Horowitz J. (2016) The political effects of agricultural subsidies in Africa: Evidence from Malawi. *World Development*, 87:215-226.
- [18] Dippel C, Gold R, Heblich S, and Pinto R. (2018) Instrumental variables and causal mechanisms: Unpacking the effect of trade on workers and voters. *NBER Working Paper* No. 23209.
- [19] Dixit A and Londregan S. (1996) The determinants of success of special interests in redistributive politics. *Journal of Politics*, 58(4):1132-55.
- [20] Dower P and Pfutze T. (2015) Vote suppression and insecure property rights. *Journal of Development Economics*, 114(C):1-19.
- [21] Frey A. (2019) Cash transfers, clientelism, and political enfranchisement: Evidence from Brazil. *Journal of Public Economics*, 176(C):1-17.
- [22] Gelman A and Imbens G. (2019) Why higher-order polynomials should not be used in regression discontinuity designs. *Journal of Business & Economic Statistics*, 37(3):447-456.
- [23] Ghildiyal S. (2009) Congress devises low-cost plan to woo Dalits. *Times of India*, July 11.
- [24] Grossman G and Helpman E. (1996) Electoral competition and special interest politics. *Review of Economic Studies*, 63(2):265-286.
- [25] Guriev S and Papaioannou E. The political economy of populism. *Journal of Economic Literature*, forthcoming.
- [26] Hinston J and Vaishnav M. (2021) Who rallies around the flag? Nationalist parties, national security, and the 2019 Indian election. *American Journal of Political Science*, forthcoming.



- [27] Jaffrelot C. (2021) *Modi's India: Hindu Nationalism and the Rise of Ethnic Democracy*. Princeton: Princeton University Press.
- [28] Jha P. (2017) *How the BJP Wins: Inside India's Greatest Election Machine*. New Delhi: Juggernaut.
- [29] Kapoor M and Ravi S. (2021) Poverty, pandemic and elections: Analysis of Bihar assembly elections 2020. *Indian Journal of Human Development*, 15(1):49-61.
- [30] Keele L and Titiunik R. (2015) Geographic boundaries as regression discontinuities. *Political Analysis*, 23:127–155.
- [31] Kishore R. (2022) UP elections: How the BSP lost political plot in coveted UP. *Hindustan Times*, March 11.
- [32] Kowal P and Afshar S. (2015) Health and the Indian caste system. *The Lancet*, 385(9966):415-6.
- [33] Kumar S and Gupta P. (2019) Where did the BJP get its votes from in 2019? *Mint*, June 3.
- [34] Labonne J. (2013) The local electoral impacts of conditional cash transfers: Evidence from a field experiment. *Journal of Development Economics*, 104:73-88.
- [35] Lee D and Lemieux T. (2010) Regression discontinuity designs in economics. *Journal of Economic Literature*, 48(2):281-355.
- [36] Levitt S and Snyder J. (1997) The impact of federal spending on house election outcomes. *The Journal of Political Economy*, 105(1):30-53.
- [37] Manacorda M, Miguel E, and Vigorito A. Government transfers and political support. *American Economic Journal: Applied Economics*, 3(3):1-28.
- [38] McCrary J. (2008) Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, 142(2):698-714.
- [39] Ray S. (2021) Dominant party rule, development and the rise of Hindu nationalism in West Bengal. In Nath S and Bhattacharya D (eds). *Theory, Policy, Practice Development and Discontents in India*. London: Routledge India.
- [40] Reardon S and Robinson J. (2012) Regression discontinuity designs with multiple rating-score variables. *Journal of Research on Educational Effectiveness*, 5(1):83-104.
- [41] Rukmini S. (2019) The BJP's electoral arithmetic. In Vaishnav M, *The BJP in Power: Indian Democracy and Religious Nationalism*. Carnegie Endowment for International Peace.
- [42] Samarendra P. (2016) Religion and scheduled caste status. *Economic and Political Weekly*, 51(31):13-16.
- [43] Shah P. (2022) BJP wins Dalit support with social engineering, double ration. *Times of India*, March 11.

- [44] Susewind R. (2014) *Data on religion and politics in India*, [Github repository](#).
- [45] Thapa R, van Teijlingen E, Regmi P, and Heaslip V. (2021) Caste exclusion and health discrimination in South Asia: A systematic review. *Asia Pacific Journal of Public Health*, 33(8):828-838.
- [46] Trivedi P, Goli S, Kumar F, and Kumar S. (2016) Does untouchability exist among Muslims? Evidence from Uttar Pradesh. *Economic and Political Weekly*, 51(15):32-36.
- [47] Verma R. (2009) Dalit voting patterns. *Economic and Political Weekly*, 44(39).
- [48] Wantchekon L. (2003) Clientelism and voting behavior: Evidence from a field experiment in Benin". *World Politics*, 55(3):399-422.
- [49] Wong V, Steiner P, and Cook T. (2013) Analyzing regression discontinuity designs with multiple assignment variables: a comparative study of four estimation methods. *Journal of Educational and Behavioral Statistics* 38:107–141.
- [50] Zacharias A and Vakulabharanam V. (2011) Caste stratification and wealth inequality in India. *World Development*, 39(10):1820-33.

## APPENDIX

### A Matching villages to polling booths

To try to match each of the 7,499 villages around the cutoff in Uttar Pradesh to the polling booth(s) in which they voted in 2014, I rely principally on the fact that a majority of booths are named in relation to the main village they serve. I make use of the publically available webscraped list in Susewind (2014), which lists the approximately 140,000 polling booths used in the general elections in Uttar Pradesh in 2014, webscraped and Hindi-to-English translated from the raw electoral data PDFs on the website of the Chief Electoral Officer of UP. Crucially, for each polling booth, it has not only booth name, but also a “booth parts” component (in English), which lists the villages or village parts that voted there in 2014, also scraped from the raw electoral roll PDFs.

After classifying all villages and polling booths into districts, I then proceed as follows *within each district*. For each village:

1. I first look for an exact match of the official English village name with the name of a booth or booth parts component.
2. If there are no exact matches, I look for a *rough* booth name or booth parts match. This involves using approximate spellings that can account for frequent Hindi-to-English automatic translation mistakes, such as from a village’s official English Census name *Kheri* to the Hindi-to-English translated name in the booth lists *Khedi*. To assist in guessing spelling deviations, I use the 2014 polling booth lists in [Elections of India](#) (which are not reliable for actual matching but include name variations of some villages).
3. If Step 1 or 2 generate only one matched booth (or multiple sequential booths with the same name but numbered such as *X1*, *X2*, and *X3*), I assign the village to that booth(s). Although straightforward, I also check accuracy of this assignments (see Step 6).

4. Suppose Step 1 or 2 generate matches for village  $v_i$  with several disparate polling booths  $W, Y, Z$ . Then I utilize  $v_i$ 's six digit Census code as follows. Villages with codes very close to each other (such as 125427 and 125429) are usually neighbors geographically, which can be confirmed with Google Maps. Moreover, polling booths are usually numbered with *some* degree of proximity, so that Village 125427 may for example have voted in Booth 106 in AC 95, while Village 125429 may have voted in Booth 150 in AC 95. Therefore, for  $v_i$ , I look for villages with very close six-digit codes (using [Indian Village Directory](#)) and with a “distinctive” name, and which can produce a single name match with a booth. I then search “around” this in the booth list to see which of  $W$   $Y$  or  $Z$  lie in proximity. Suppose this is  $Y$ . I then check that some other booths around  $Y$  match names of other neighbors for  $v_i$ . When this holds, I match  $v_i$  to  $Y$ .<sup>50</sup>
5. Instead, suppose Steps 1 and 2 do not generate any booth matches at first try, due to unpredictable spelling differences between official English village name and booth name (for example, from *Haradi Kalan* to *Hardi Kla*, or from *Shahabad* to *Shavad*). Then I use Step 4 to produce matches for these villages (i.e. using information on code-neighbors), and confirm that no other villages in the district have a similar name to the misspelled English name.
6. I also use Step 4 to generate random checks on the accuracy of Step 3 for villages where I had been able to find a single (exact or rough) name match.
7. I leave unmatched the minority of villages for which: (i) there is a neighboring village with a similar name, so that neighbor-code information cannot be used to identify the right village, or (ii) even a rough approximation of its name does not appear in any booth names or booth parts components (usually because it is quite small and probably included within a larger booth, without all components of the latter enumerated).

Next, to match each village to where it voted in 2019, I rely on both Raphael Susewind's list of webscraped 2019 polling booths in Uttar Pradesh, and on the official list from the

---

<sup>50</sup>After matching, I see that in all cases,  $Y$  electorate size also makes the most sense given  $v_i$ 's population, confirming the accuracy of this method.

website of the state’s Chief Electoral Office. Note that it is precisely because these have less comprehensive information than the webscraped 2014 rolls, that I begin with 2014 matches and work to 2019, and not vice versa.<sup>51</sup> *Within each district* and for every village:

1. If I was able to generate a 2014 booth match, I examine the list of booths in that same AC in 2019, and try to find the corresponding 2019 booth in terms of name and listing order. This is because while booths did often change numbers, merge, or split from the 2014 to 2019 elections, they remained within the same AC, and mostly within a similar sequencing order per AC.
2. If there are any doubts about Step 1, for example if I find the same name booth in the AC but in a very different sequence order, I use step 4 above (code-neighbors) within the AC to identify and confirm the accurate 2019 booth.
3. If I was unable to generate a 2014 match, I use Steps 1-6 above but for 2019 booths, widening my search to all ACs in the village’s district.

Through this process, I am able to generate matches for about 6,300 villages, across the two election cycles. This manual matching, while highly time intensive, yields the highest possible accuracy, given the notoriously inaccurate village pincodes (so that matching based on geolocation is highly flawed) and the frequency of villages with similar names and wide range of translation spelling mistakes (so that matching based on a name algorithm is also flawed). Moreover, as it uses the unique 6-digit village codes, it also provides more accurate results than manual matching relying on auxiliary websites such as [Village Atlas](#) or [OneFiveNine](#), which by comparison include numerous inconsistencies.<sup>52</sup>

---

<sup>51</sup>The webscraped list is acquired directly through email correspondence with the author. However, unlike the 2014 list, this one does not have comprehensive “booth parts” coverage, and there are many translation mistakes in terms of booth names (and parts). Therefore, I double check booth names using the official CEO booth lists - which have more accurate translated names but do not have a booth parts component.

<sup>52</sup>For example, suppose two villages  $v_1$  and  $v_2$  have very similar names in the same district, and  $v_1$  falls within the 8 percent bandwidth. It is not uncommon, under the page for  $v_1$  information, to find a Google Map of  $v_2$  instead, so that matching by “neighbor” this way would result in exactly the wrong polling booth.

## B Deriving the multi-score RDD estimators

The outlines of this exposition are drawn from Choi and Lee (2018). Let  $Y^{ij}$  denote the outcome of interest when  $Z_s = i$  and  $Z_p = j$ . Then  $Y^{11}$  is the outcome of villages that received pre-election transfers,  $Y^{10}$  of villages eligible for future transfers,  $Y^{01}$  of ineligible villages that cross the size threshold, and  $Y^{00}$  of ineligible villages that do not cross the size threshold.

The general equation for expected  $Y$  in a neighborhood of the cutoffs  $S$  is therefore (net of any other variables that can affect  $Y$ ):

$$E[Y|S] = E[Y^{00}|S](1-Z_s)(1-Z_p) + E[Y^{10}|S]Z_s(1-Z_p) + E[Y^{01}|S](1-Z_s)Z_p + E[Y^{11}|S]Z_s*Z_p \quad (\text{B.1})$$

Rewriting this so that  $Z_s$ ,  $Z_p$ , and  $Z_s * Z_p$  appear separately, we obtain

$$\begin{aligned} E[Y|S] = & E[Y^{00}|S] + \left( E[Y^{10}|S] - E[Y^{00}|S] \right) Z_s + \left( E[Y^{01}|S] - E[Y^{00}|S] \right) Z_p \\ & + \left( (E[Y^{11}|S] - E[Y^{10}|S]) - (E[Y^{01}|S] - E[Y^{00}|S]) \right) Z_s * Z_p \end{aligned} \quad (\text{B.2})$$

Consider the regression form:

$$Y = \beta_0 + \beta_1 Z_s + \beta_2 Z_p + \beta_3 Z_s * Z_p + \epsilon \quad (\text{B.3})$$

where other variables that can affect  $Y$  are abstracted from, and observations are at the village level. Then it is clear that

$$\beta_1 = E[Y^{10}|S] - E[Y^{00}|S] \quad (\text{B.4})$$

$$\beta_3 = (E[Y^{11}|S] - E[Y^{10}|S]) - (E[Y^{01}|S] - E[Y^{00}|S]) \quad (\text{B.5})$$

Since a regression discontinuity calculates jumps in the limit (as the cutoffs are approached), it is possible to write the above expressions more explicitly. Each observation

approaches  $SC_s = 0.5$  (share cutoff) from the right hand side when  $i = 1$  and from the left hand side otherwise. And each observation approaches  $SC_p = c$  (size cutoff) from the right hand side when  $j = 1$  and from the left hand side otherwise. Therefore, Eqs. (B.4) and (B.5) can be rewritten respectively as:

$$\beta_s = \lim_{SC_s \rightarrow 0.5^+, SC_p \rightarrow c^-} E[Y|\mathbf{S}] - \lim_{SC_s \rightarrow 0.5^-, SC_p \rightarrow c^-} E[Y|\mathbf{S}]$$

$$\begin{aligned} \beta_r = & \left( \lim_{SC_s \rightarrow 0.5^+, SC_p \rightarrow c^+} E[Y|\mathbf{S}] - \lim_{SC_s \rightarrow 0.5^+, SC_p \rightarrow c^-} E[Y|\mathbf{S}] \right) \\ & - \left( \lim_{SC_s \rightarrow 0.5^-, SC_p \rightarrow c^+} E[Y|\mathbf{S}] - \lim_{SC_s \rightarrow 0.5^-, SC_p \rightarrow c^-} E[Y|\mathbf{S}] \right) \end{aligned}$$

## C Additional results

Table C1: MRDD with coefficient restriction  $\beta_s = -\beta_r$

	<i>Dependent variable:</i>			
	Vote share for BJP		Turnout	
	(1)	(2)	(3)	(4)
I(Eligible - recipient)	-0.001 (-0.018, 0.016)	0.005 (-0.019, 0.029)	0.012*** (0.004, 0.020)	0.002 (-0.008, 0.013)
Bandwidth	Share only	Dual	Share only	Dual
PC controls	Yes	Yes	Yes	Yes
Observations	3,034	1,498	3,034	1,498
R <sup>2</sup>	0.218	0.231	0.395	0.375
Adjusted R <sup>2</sup>	0.197	0.189	0.379	0.341
Residual Std. Error	0.144 (df = 2955)	0.144 (df = 1419)	0.065 (df = 2955)	0.063 (df = 1419)

*Note:*

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table C1 reports the results of restricting  $\beta_s = -\beta_r$  in the multiscore specification; quadratic form for the running variables is used. In Columns (1) and (2), the dependent variable is vote share for the BJP; in Columns (3) and (4), it is voter turnout.

Table C2: ANOVA of restricted versus unrestricted model

Statistic	Column 1	Column 2	Column 3	Column 4
F	0.072	0.175	0.199	0.6269
Pr(>F)	0.788	0.6755	0.6555	0.4286

Table C2 reports the results of ANOVA tests between the unrestricted MRDD in Table 5 and the restricted MRDD in Table C1, for each of their four columns.  $Pr(> F)$  is the probability of obtaining the given F-statistic would occur if we are unable to reject the null that the restricted model is as good as the unrestricted model.

Table C3: MRDD with  $\beta_p \neq 0$ 

	<i>Dependent variable:</i>			
	Vote share for BJP		Turnout	
	(1)	(2)	(3)	(4)
Eligible	−0.002 (−0.024, 0.020)	0.011 (−0.021, 0.042)	0.013** (0.002, 0.023)	0.005 (−0.009, 0.019)
Above size	0.010 (−0.010, 0.029)	0.018 (−0.012, 0.049)	−0.007 (−0.017, 0.002)	−0.006 (−0.020, 0.008)
Pre-election recipient	−0.007 (−0.031, 0.016)	−0.010 (−0.040, 0.020)	−0.006 (−0.017, 0.005)	0.004 (−0.010, 0.017)
Bandwidth	Share only	Dual	Share only	Dual
PC controls	Yes	Yes	Yes	Yes
Observations	3,034	1,498	3,034	1,498
R <sup>2</sup>	0.218	0.232	0.395	0.376
Adjusted R <sup>2</sup>	0.197	0.189	0.379	0.341
Residual Std. Error	0.144 (df = 2953)	0.144 (df = 1417)	0.065 (df = 2953)	0.063 (df = 1417)

*Note:*

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table C3 reports the results of Eq. (5.3), with  $\beta_p \neq 0$ , so that the control group is only the set of ineligible villages falling below the size threshold. I use a quadratic specification for the centered running variables.



Table C4: MRDD with linear specification

	<i>Dependent variable:</i>			
	Vote share for BJP		Turnout	
	(1)	(2)	(3)	(4)
Eligible	−0.004 (−0.026, 0.017)	0.010 (−0.021, 0.040)	0.013** (0.003, 0.024)	0.006 (−0.009, 0.020)
Pre-election recipient	−0.0003 (−0.021, 0.021)	−0.001 (−0.029, 0.026)	−0.011** (−0.020, −0.001)	0.002 (−0.010, 0.014)
Bandwidth	Share only	Dual	Share only	Dual
PC controls	Yes	Yes	Yes	Yes
Observations	3,034	1,498	3,034	1,498
R <sup>2</sup>	0.217	0.231	0.390	0.373
Adjusted R <sup>2</sup>	0.197	0.190	0.374	0.339
Residual Std. Error	0.144 (df = 2956)	0.144 (df = 1420)	0.065 (df = 2956)	0.063 (df = 1420)

Note:

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table C4 reports the results of the multi-score RDD with a linear specification for the centered running variables.

Table C5: Reduced form with different bandwidths

<i>PANEL A. Dependent variable: Vote share BJP</i>					
	(1)	(2)	(3)	(4)	(5)
Eligible	−0.001 (−0.028, 0.025)	−0.006 (−0.035, 0.023)	−0.002 (−0.037, 0.034)	0.001 (−0.041, 0.042)	0.023 (−0.023, 0.069)
Observations	4,372	3,727	2,420	1,704	1,264
R <sup>2</sup>	0.206	0.208	0.221	0.226	0.249
Adjusted R <sup>2</sup>	0.191	0.191	0.195	0.189	0.200
Residual Std. Error	0.144 (df = 4293)	0.143 (df = 3648)	0.143 (df = 2341)	0.144 (df = 1625)	0.142 (df = 1185)
<i>PANEL B. Dependent variable: Turnout</i>					
	(1)	(2)	(3)	(4)	(5)
Eligible	0.013* (−0.0002, 0.026)	0.013* (−0.001, 0.028)	0.014 (−0.003, 0.032)	0.018* (−0.002, 0.038)	0.017 (−0.007, 0.041)
Observations	4,372	3,727	2,420	1,704	1,264
R <sup>2</sup>	0.349	0.357	0.367	0.374	0.357
Adjusted R <sup>2</sup>	0.337	0.344	0.345	0.343	0.315
Residual Std. Error	0.068 (df = 4293)	0.068 (df = 3648)	0.067 (df = 2341)	0.064 (df = 1625)	0.064 (df = 1185)

Note:

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table C5 reports the results of the reduced form RDD using different bandwidths, first where the dependent variable is BJP share (Panel A) and then where the dependent variable is turnout (Panel B). In both panels, Columns 1, 2, and 3 use a 7%, 6%, and 4% bandwidth around the 50% SC share cutoff, respectively. Columns 4 and 5 combine a 5% bandwidth around the share cutoff with a +/-500 and 700 SC persons bandwidth around the size cutoff, respectively.

Table C6: MRDD with different bandwidths

<i>PANEL A. Dependent variable: Vote share BJP</i>					
	(1)	(2)	(3)	(4)	(5)
Eligible	0.005 (−0.013, 0.024)	0.003 (−0.017, 0.023)	0.001 (−0.023, 0.025)	0.005 (−0.025, 0.034)	0.015 (−0.018, 0.049)
Pre-election recipient	−0.0001 (−0.018, 0.018)	0.0004 (−0.019, 0.019)	0.0005 (−0.022, 0.023)	0.001 (−0.025, 0.027)	−0.003 (−0.032, 0.027)
Observations	4,372	3,727	2,420	1,704	1,264
R <sup>2</sup>	0.205	0.207	0.221	0.225	0.248
Adjusted R <sup>2</sup>	0.191	0.190	0.194	0.187	0.198
Residual Std. Error	0.144 (df = 4292)	0.144 (df = 3647)	0.143 (df = 2340)	0.145 (df = 1624)	0.142 (df = 1184)
<i>PANEL B. Dependent variable: Turnout</i>					
	(1)	(2)	(3)	(4)	(5)
Eligible	0.011*** (0.003, 0.020)	0.013*** (0.004, 0.023)	0.014** (0.002, 0.026)	0.009 (−0.005, 0.022)	0.005 (−0.011, 0.021)
Pre-election recipient	−0.006 (−0.014, 0.002)	−0.008* (−0.016, 0.001)	−0.013** (−0.023, −0.003)	0.001 (−0.011, 0.012)	−0.0004 (−0.013, 0.013)
Observations	4,372	3,727	2,420	1,704	1,264
R <sup>2</sup>	0.381	0.392	0.399	0.391	0.370
Adjusted R <sup>2</sup>	0.370	0.379	0.379	0.362	0.328
Residual Std. Error	0.066 (df = 4292)	0.066 (df = 3647)	0.066 (df = 2340)	0.063 (df = 1624)	0.063 (df = 1184)

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

Table C6 reports the results of the multi-score RDD using different bandwidths, first where the dependent variable is BJP share (Panel A) and then where the dependent variable is turnout (Panel B). In both panels, Columns 1, 2, and 3 use a 7%, 6%, and 4% bandwidth around the 50% SC share cutoff, respectively. Columns 4 and 5 combine a 5% bandwidth around the share cutoff with a +/-500 and 700 SC persons bandwidth around the size cutoff, respectively.

Table C7: Specifications with change between 2014 and 2019 as outcome

<i>PANEL A. Reduced form RDD</i>		
<i>Outcome</i>	Change in vote share BJP	Change in turnout
	(1)	(2)
Eligible	0.012 (−0.013, 0.036)	0.010 (−0.004, 0.024)
Observations	2,841	2,841
R <sup>2</sup>	0.298	0.138
Adjusted R <sup>2</sup>	0.278	0.113
Residual Std. Error (df = 2762)	0.109	0.058
<i>PANEL B. Multi-score RDD</i>		
<i>Outcome</i>	Change in vote share BJP	Change in turnout
	(1)	(2)
Eligible	0.007 (−0.009, 0.024)	0.008* (−0.001, 0.018)
Before2	0.005 (−0.010, 0.021)	−0.003 (−0.013, 0.006)
Observations	2,841	2,841
R <sup>2</sup>	0.298	0.151
Adjusted R <sup>2</sup>	0.278	0.127
Residual Std. Error (df = 2761)	0.109	0.057

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

Table C7 reports the results of the reduced form RDD (Panel A) and the multi-score RDD (Panel B) when the outcome is a difference variable. “Change in vote share BJP” is the village’s vote share for the BJP in 2019 minus its vote share for the BJP in 2014, and similarly for “Change in turnout”.

Table C8: Specifications with 2014 outcomes (placebo)

<i>PANEL A. Reduced form RDD</i>		
<i>Outcome</i>	Past vote share BJP	Past turnout
	(1)	(2)
Eligible	−0.014 (−0.046, 0.017)	0.005 (−0.011, 0.021)
Observations	2,841	2,841
R <sup>2</sup>	0.222	0.399
Adjusted R <sup>2</sup>	0.200	0.382
Residual Std. Error (df = 2762)	0.138	0.074
<i>PANEL B. Multi-score RDD</i>		
<i>Outcome</i>	Past vote share BJP	Past turnout
	(1)	(2)
Eligible	−0.011 (−0.033, 0.010)	0.005 (−0.006, 0.016)
Pre-election recipient	−0.006 (−0.027, 0.015)	−0.006 (−0.018, 0.006)
Observations	2,841	2,841
R <sup>2</sup>	0.222	0.458
Adjusted R <sup>2</sup>	0.200	0.443
Residual Std. Error (df = 2761)	0.138	0.070

*Note:*

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table C8 reports the results of the reduced form RDD (Panel A) and the multi-score RDD (Panel B), when the outcome is the village's BJP vote share in 2014 (Column 1) or its turnout in 2014 (Column 2).