

# Transfers and the rise of Hindu nationalism in India

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## Abstract

In democracies with widespread poverty, what is the impact of transfers on electoral behavior and on the incumbent's ability to retain power? This paper provides the first quasi-experimental evidence on this in the Indian context, where a Hindu-nationalist party has been in power for the last decade. I study a national program through which the party, once incumbent, has transferred development funds to villages with a high share of disadvantaged castes. Focusing on India's largest state, I link villages to polling booths, and use a discontinuity design to identify the effects of past and expected transfers on village-level voting in the subsequent elections. I find that expected transfers increased turnout slightly but that neither treatment impacted the share of votes going to the incumbent. The results support findings in other contexts of limited effects of non-discretionary programs on electoral behavior, and they shed light on the recent slide to ethnic nationalism in the world's largest democracy.

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# 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 since included a mix of economic programs with debatable development success as well as steps to cement Hindu ethno-nationalism in the country.<sup>2</sup>

The recent rise of Hindu nationalism in India is significant politically but also operates in an understudied landscape of targeted economic transfers and shifting electoral allegiances. The BJP’s rise to power and, even more strongly, its subsequent reelection have been underpinned by an ability to secure votes from disadvantaged groups, including marginalized caste groups that comprise a large share of the population and that had previously largely voted for other parties. At the same time, as incumbent, the party has rolled out a number of programs transferring funds to some of these poor populations, basing them on specific eligibility criteria and implementing them across the nation.

To what extent did such transfers during incumbency anchor reelection in this context? In the literature on economic benefits and political outcomes, there is consistent evidence globally that highly *discretionary* distribution, such as localized transfers based on an implicit or explicit quid-pro-quo for political support through clientelistic networks, influences voting behavior and generates electoral rewards (Golden and Min, 2013). However, for distribution through (often large) programs with clear eligibility rules, also called *programmatic* transfers,

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<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.

<sup>2</sup>For example, the BJP has revoked (largely Muslim) Kashmir and Jammu’s special status and has introduced Muslim-exclusionary citizenship amendments to the parliament (BBC, 2019), which tie citizenship to religion for the first time in the history of modern India.

the evidence is concentrated on voters in advanced economies (e.g. the literature surveyed in Healy and Malhotra, 2013).<sup>3</sup> We still know too little about whether programmatic transfers in developing countries help shift recipient populations into the incumbent’s base, and the evidence which *is* available is mixed. Earlier studies focusing on cash transfer programs have found increased incumbent support in middle-income countries including Uruguay (Manacorda et al, 2011), Mexico (De La O, 2012), and the Philippines (Labonne, 2013). But, in the context of a low-income country, Blattman et al (2018) find little impact on voting of programmatic policies in Uganda, while earlier findings in the Mexican context are now disputed (Imai et al 2020). Adding to this complexity, in contexts with both discretionary and programmatic distributions, only the former have been found to impact voting behavior (Wantchekon, 2003; Ortega and Penfold-Becerra 2008; Bardhan et al 2022).

The question of how programmatic transfers impact the incumbent’s retention of power takes on added significance when the incumbent is transforming the political landscape through a populist agenda, as it becomes entangled with the question of whether developmental programs contribute to the rise of populism or whether the latter is largely the result of the pull of the political narrative itself, but we know even *less* about this context. The recent empirical literature on populism, surveyed in Guriev and Papaioannou (2022), is almost entirely focused on developments in the USA and Europe, and does not address the interplay between socioeconomic cleavages and populist support in a largely rural and poor, but democratically vibrant, setting like the Indian one. At the same time, a credible empirical approach, along the lines of this literature, is necessary precisely because of the likelihood of confounders under populist incumbents. For example, in India, the BJP paints economic and caste cleavages as secondary to ethnic/religious cleavages, and stresses the importance of a united (Hindu-led) front along the latter lines. This may be effective in attracting into the BJP’s base the same poor and marginalized caste groups that the transfers target.

It is to these research areas that this paper contributes. I offer the first quasi-experimental evidence (using actual election outcomes) on the link between transfers and political support

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<sup>3</sup>In programmatic transfers, even though there may be initial discretion in deciding the eligibility criteria, distribution and exclusion are largely non-discretionary thereafter.

in the Indian case, with a focus on programmatic distribution.

Specifically, I study the effect of rural development transfers that the BJP began distributing nationally in 2018, under a large program titled Pradhan Mantri Adarsh Gram Yojana (PMAGY), on village-level electoral outcomes in the 2019 general elections. The transfers studied are substantive one-time rural development funds targeting villages whose population is at least 50% Scheduled Caste. Scheduled Castes, also known as Dalits and historically subject to “untouchability” discrimination, are among the poorest groups in India while also being electorally significant, at 17% of the population. They were a cornerstone of the BJP’s electoral victory in both 2014 and 2019, making the question of transfers and political allegiances particularly relevant.<sup>4</sup> 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 transfers in Uttar Pradesh, the most populous state with 250 million people.

The program I study targets a critical electoral segment while facilitating a strong research design. First, the its eligibility cutoff is 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>5</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 discontinuity design with heterogeneous treatment effects to identify the impact of two distinct treatments - transfer receipt prior to elections versus eligibility for future transfers - on votes.

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<sup>4</sup>Scheduled Castes voted by 34% for the BJP, up from 24% in 2014 and 12% in 2009; see Section 2.

<sup>5</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 other government programs.

Therefore, the research design not only addresses identification concerns but also makes the paper relevant to both research on retrospective voting (in response to past policies) and on forward looking voting (in response to campaign promises).

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 linked dataset myself of villages - in Uttar Pradesh, and within a sufficiently wide bandwidth of the 50% Scheduled Share cutoff - and their votes.<sup>6</sup> Whereas there is village-level data about village characteristics and transfers, electoral data in India is available only at the polling booth level. Linking 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 link each village to the polling booth(s) in which it voted, in both 2019 *and* 2014.<sup>7</sup> This time-consuming but highly meticulous process is detailed in **Appendix A**, and it produces an end village-booth linked dataset of over 6,300 villages.

To anchor the empirical analysis, I first present a simple model of programmatic transfers and voting behavior, which draws on the canonical framework in Dixit and Londregan (1996). In the model, voters weigh utility from expected transfers by different parties against ideological preferences when voting. Expected transfers by party affect calculations of utility, while past transfer receipt may generate “reciprocal” loyalty for the incumbent and a shift in preferences. Therefore, both can affect the village’s vote share for the incumbent 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 linked dataset in a multi-score sharp regression discontinuity design to estimate these possible treatment effects. The design departs from the assumption of a sin-

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<sup>6</sup>Specifically, I attempt to link the 7,499 villages in Uttar Pradesh within +/- 8% of the cutoff to the booths they voted in. 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>7</sup>Booth assignment changed between elections.

gle 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>8</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. As a secondary outcome, I explore (total) village-level voter turnout. I find that eligibility for future transfers increased turnout by a modest 1.4% while past receipt had no impact.

What accounts for the limited effect of these transfers on incumbent support? Ethnographic data would help confirm the mechanisms but I offer some pointers. After explaining why the results are unlikely to be driven by information frictions or transfer-specific limitations, I discuss the underlying preferences and expectations of voters that would generate the limited electoral impact.<sup>9</sup> In line with the theoretical model, eligible beneficiaries will have limited electoral response to future transfers when they are ideologically rigid *or* when they expect the competing party to match incumbent promises. The extent of ideological rigidity cannot be ascertained but, as I explain, competition for the Scheduled Caste vote in India is fierce due to this group’s electoral importance and the view that it is a “swing” group,

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<sup>8</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.

<sup>9</sup>More precisely, I argue that the transfers are not “too small” and that villagers likely knew about them due to extensive PR efforts by the BJP. I also show that the results are not being driven by villagers inaccurately attributing the program to the local politicians and therefore potentially the “wrong” party.

and in Uttar Pradesh the main BJP competitor has a history of Scheduled Caste advocacy (Kumar, 1999). It is therefore likely that voters would expect the BJP’s competitors to also commit to this program if they were elected, so that there is no difference in relative expected economic expediency from promised transfers; only *other* factors which actually differentiate the BJP from other parties would impact vote share. Meanwhile, for past recipients, the results suggest transfers generated limited feelings of obligation or reciprocity, feelings which are probably heightened by, and most relevant in, the context of discretionary and clientelistic transfers (e.g. Finan and Schechter, 2012).

In addition to contributing to the general research areas cited above, the paper contributes to ongoing debates within India and by India scholars on the recent developments in the country. There is significant popular and academic interest *particularly* on the effect of transfers on caste-based voting and on the BJP’s upending of existing caste-based politics, combined with lack of credible evidence on the topic. It is common to see assertions in major media outlets 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 [...] through welfare schemes*” (Shah 2022), but it is unclear which evidence such assertions rely on. Amongst academics, the existing discussion, while potentially illuminating, is suggestive and the evidence is descriptive (e.g. Jaffrelot, 2021; Aiyar, 2019; Jha, 2014).

By offering identified evidence on this topic for the first time, the analysis belies the widespread notion in these circles that (at least for this constituency) expanding BJP appeal is a result largely of past or future welfare benefits. The BJP vote share per village *did* increase in the set of treated villages studied by an average of 9 percentage points between 2014 and 2019, but it did so equally for the control group, suggesting that little if any of this triumph can be attributed to these treatments.<sup>10</sup>

Methodologically, the paper makes headway by distinguishing voting outcomes in India by village and with a high level of accuracy. Likely due to the sheer difficulty of the village-

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<sup>10</sup>The impact of other program-related treatments beyond transfer receipt and eligibility cannot be ruled out, such as for example increased BJP appeal due to feeling targeted as a social group; see Section 7.

booth linking process, there are very few (almost no) papers which use village-level votes in India as either outcome or treatment variable in any context.<sup>11</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, but the authors rely principally on a name-matching algorithm which does not provide the same accuracy as the fully manual process I undertake (**Appendix A**). The use of disaggregated voting data allows the analysis to transcend the limitations of using either disaggregated but self-reported outcomes, which may not reflect electoral support accurately, or voting data at readily available but high levels of aggregation, which present limits to identification. These limitations appear not only in the Indian context but also frequently in the wider literature on electoral behavior. On a secondary methodological note, 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).

The paper proceeds as follows. Section 2 provides background regarding the rise of the BJP, the electoral significance of the Scheduled Castes, and the transfer program under study. Section 3 presents a simple model of voting behavior in response to received or future programmatic transfers and derives the treatment effects to be estimated empirically. Section 4 describes the construction of the dataset and the key process of linking villages to polling booths. Section 5 outlines the regression discontinuity design and assesses its validity for identification. Section 6 presents the results and robustness checks. Section 7 discusses the results and mechanisms, before the last section concludes.

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<sup>11</sup>Some studies use disaggregated but elicited or self-reported data on electoral support (e.g. Bardhan et al, 2022; Ray, 2021). Otherwise, using election data, the focus is on votes at least at the Assembly Constituency level (e.g. Kapoor and Ravi, 2021). In Uttar Pradesh, the average AC comprises 600,000 people.



## 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>12</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>13</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.

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<sup>12</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>13</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.

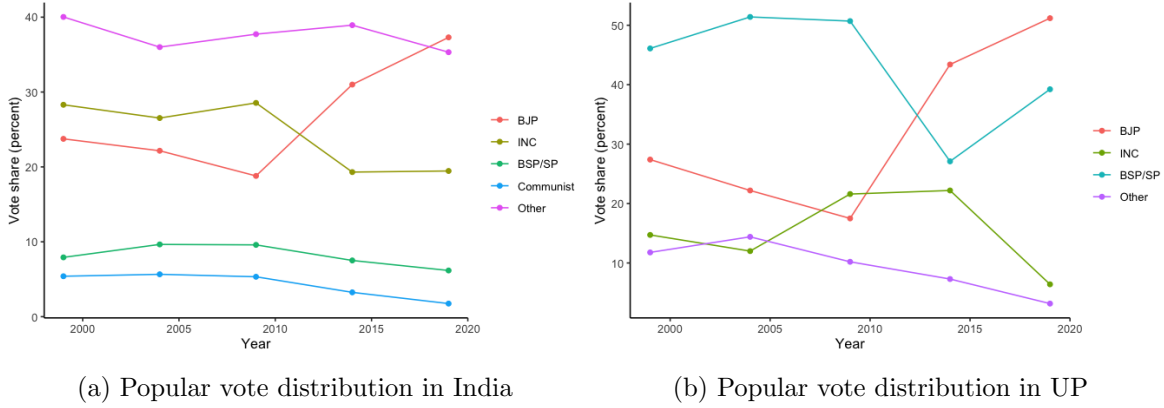


Figure 1: Popular vote in Lok Sabha, 1999-2019

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.

A key boon for BJP triumph in Uttar Pradesh and in the country as a whole have been the Scheduled Castes. 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. The Scheduled Castes in particular voted by 34% for the BJP, up from 24% in 2014 and 12% in 2009 (Kumar and Gupta, 2019; Verma, 2009). High and rising vote share for the BJP is also apparent in the linked 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 defined Scheduled Castes to include groups that the British had loosely referred to as the “Depressed Classes” and it came in preparation



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>16</sup> Over half of all Scheduled Caste households are landless and work as waged labor instead of on their own land, higher than for any other socioeconomic segment (Socioeconomic and Caste Census, 2011).

It is in this context and arguably in light of electoral concerns that PMAGY was conceptualized, as a program delivering a one-time 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 this large and impoverished 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.

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 party’s timing and commitment to the program.<sup>17</sup> The program was structured to deliver a transfer of about 1 million Rupees to each eligible village in the country, in addition to a small

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<sup>16</sup>This includes, most prominently, manual removal of human excrement. About 1.3 million Scheduled Castes persons in India, mostly women, make their living from this dehumanizing (and officially outlawed) occupation.

<sup>17</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.

sum for administrative funds, to help Scheduled Caste-majority villages meet key needs.<sup>18</sup> Each recipient village would have a few target activities identified for which financing gaps existed and which the funds would help fill, centering around needs such as clean drinking water supply and sanitation drainage systems.<sup>19</sup>

The selection of villages for the first round of funding 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>20</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.

A total of 708 villages in Uttar Pradesh received their allotted transfer of 1 million Rupees in this first (and only pre-election) phase. 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 programmatic transfers, and derives the treatment effects to be estimated empirically.

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<sup>18</sup>This is \$14,600 in 2018 exchange rates; given village sizes and on per capita terms, this is equivalent to about \$73-88 per family. I put these numbers in further context in section 7.

<sup>19</sup>By covering possible funding gaps from other schemes, the program was envisioned to act as “convergent implementation” of other programs, although only for Scheduled Caste-majority villages.

<sup>20</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.

### 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.

Models of programmatic 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 materialize if they win, and proceed to calculate what these pledges will be which maximize 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 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>21</sup>

To see how people weigh their voting options, let all people within a specific village type share a utility function with respect to the transfers, so that  $T$  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 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

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<sup>21</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).

type  $v$  who have affinities to the left of 0 ( $X_{iv} < 0$ ) and thus prefer party  $R$ ; for example,  $\Phi_a(0) > \Phi_b(0)$  indicates 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:

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

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 would only vote based on ideological preference  $X_{iv}$ .

It remains to specify how village type is linked to the program. 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>22</sup>

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<sup>22</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.

### 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  $X_b^* \equiv U_b(T_b^R) - (1 - \gamma)U_b(T_b^L) \leq 0$ . Any individual with  $X_{ib} < X_b^*$  will vote for  $R$ , so that the vote share for  $R$  in  $b$  villages will be

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

By contrast, 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:

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

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 ( $X_b^* = 0$ ).

Since  $X_b^* \equiv U_b(T_b^R) - (1 - \gamma)U_b(T_b^L)$ , 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, due for example to ideological rigidity.

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

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

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 the program’s transfers under  $R$ ’s incumbency.

As a final comment, consider a general shock  $\mathcal{R} \lesseqgtr 0$  prior to  $t_1$  which impacts *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  $L$ .

Naturally, a common shock would not impact difference-of-means estimates between village types.<sup>23</sup> However, where this common shock would show is in comparisons of the vote share *within* each village type  $v$  between  $t_1$  and  $t_0$ . Denote this change between election

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<sup>23</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.

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, 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>24</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 village-booth linking, 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>25</sup> The result is the set of all 7,499 villages within the bandwidth in Uttar Pradesh, 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>26</sup> Each polling booth has a booth number, which together with its AC number constitutes a unique combination; for example, 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 the raw data is in English, while the rest is a mix of Hindi and Kruti Dev code; Python is used to translate the latter two to 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).

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<sup>24</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>25</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 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 linked or the linked booths will not reflect predominantly voting in that village; see below.

<sup>26</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.

To figure out how each village 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 link 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 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 detailed in **Appendix A**.

Second, I use the above linkage of villages to where they voted in 2014, to link 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>27</sup> I then double check the accuracy of the 2019 linkages using the same auxiliary resources mentioned above, with details also outlined in the Appendix. Overall, the process generates village-specific voting data for over 6,300 villages - an 85% linking success rate - with information on the electoral outcomes of each of those village in both 2014 and 2019.<sup>28</sup>

Finally, although I was able to link most villages to polling booths, not all linkages are equally *useful* for the empirical analysis. Small villages were often voting together or with

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<sup>27</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>28</sup>The linking success rate is closer to 90% when taking into account that some villages could not be linked 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 linked 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.

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 linking 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* turnout) in 2019 which was usually somewhere between 60% to 90% of the village population, with some deviations in both directions.<sup>29</sup> Therefore, in the analytical exercises, I use villages where the electorate of the linked 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 linkages for the 2019 election to 5,039. The number of villages with informative booth linkages 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.

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<sup>29</sup>For example, a 2019 booth which I linked 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.

## 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 voting behavior in two conceptually distinct ways, violating the validity of the instrument.<sup>30</sup>

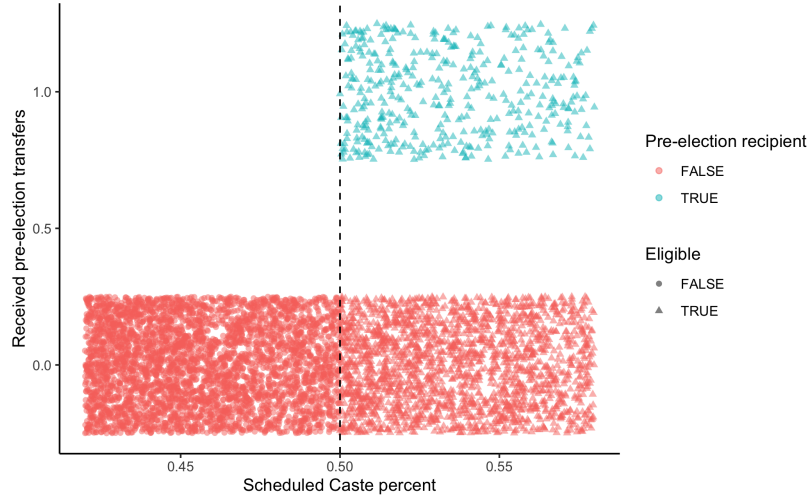


Figure 3: Single discontinuity

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

<sup>30</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).

( $SC$ ) share.  $Z_s = 1$  if  $SC_i \geq 0.5$  and 0 otherwise. The “reduced form” sharp RDD is:

$$Y_i = \theta_0 + f(SC_{i,s} - 0.5) + \theta_1 Z_i + \theta_2 PC_i + \theta_3 D_i + e_i \quad (5.1)$$

where  $f$  is a function of the centered running variable, potentially with an interaction with  $Z$ , and the above is run on a bandwidth optimizing the bias-variance tradeoff. I use a linear form (and check robustness to a quadratic form) to avoid bias from overfitting by higher order polynomials (Gelman and Imbens, 2019). Given that a valid RDD does not need controls (Lee and Lemieux, 2010), I control only for parliamentary constituency ( $PC_i$ ), to ensure comparison of villages facing the same candidates from each party, and for district ( $D_i$ ).

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 albeit not structurally derived outcome, I also explore village turnout, calculated as total votes in the village 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>31</sup> It illustrates

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<sup>31</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



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.

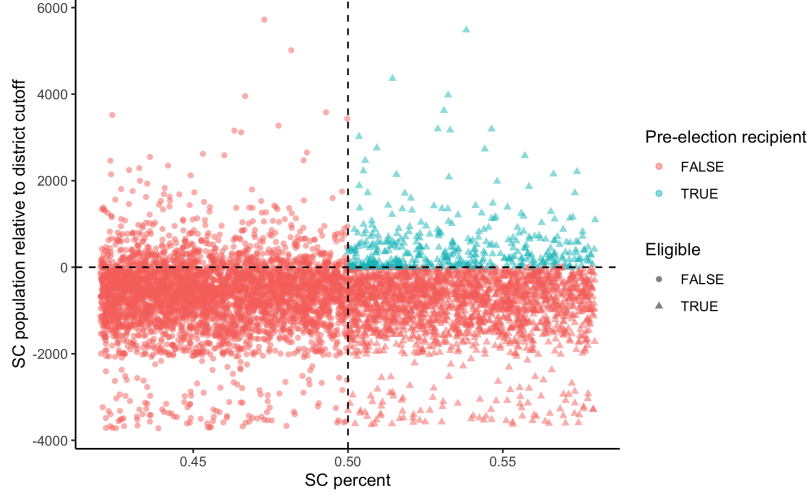


Figure 4: Multi-score sharp discontinuities

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.

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:

- $Z_s \in \{0, 1\}$  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>32</sup>

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

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

than 50% Scheduled Caste share did not receive transfers (upper left quadrant).

<sup>32</sup>Here,  $c$  is the population cutoff for that district, as explained in footnote 31.

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>33</sup>

$\beta_s$  and  $\beta_r$  in Eq. (5.3) are the causal estimators of interest. In **Appendix B** I show formally that the estimators can be expressed as:<sup>34</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

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<sup>33</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.

<sup>34</sup>The expected value would also be conditional on all other covariates  $\mathbf{X}$  included in the regression, here the  $f$  function, parliamentary constituency, and district.

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 regression of Eq. (5.3).<sup>35</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

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<sup>35</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 6.3 I check that including the size cutoff in the regressions does not alter results.

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” work, all as reported by the 2011 Census.<sup>36</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>37</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.

<sup>36</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>37</sup>This was chosen as it reduces the sample size by not much more than half.

**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>38</sup>

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>39</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

<sup>38</sup>The difference in literacy, while statistically significant at the ten percent level, is economically negligible, at less than 0.1%.

<sup>39</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 links. 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 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.

density around the threshold, for both running variables.

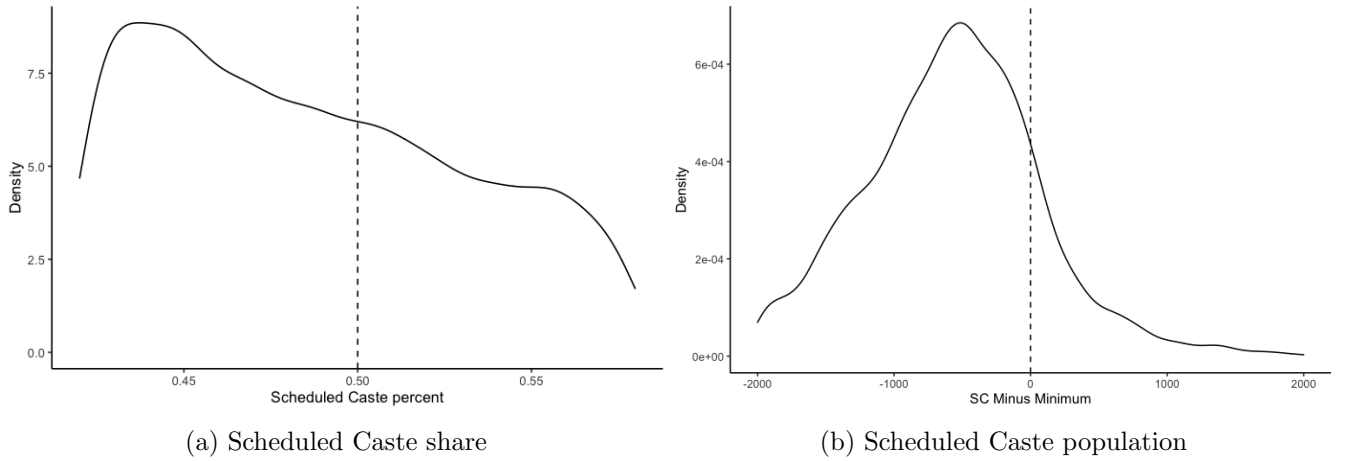


Figure 5: No manipulation of thresholds

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

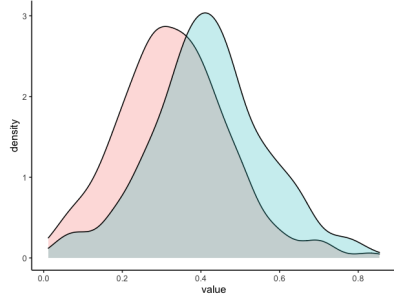
First, linking villages to polling booths allows me to describe and plot voting patterns at the village level and by village characteristics.

Specifically, for the UP villages with 42%-58% Scheduled Caste share which I was able to generate useful booth links 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 villages.

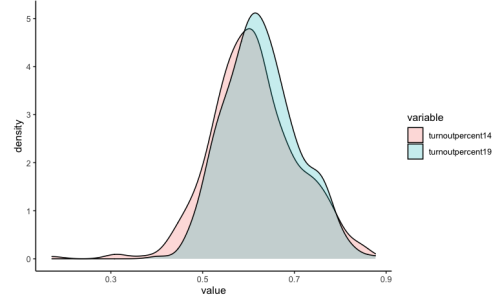
This implies that Schedule-Caste heavy villages in Uttar Pradesh *shifted* votes from other parties to the BJP between 2014 and 2019, and to a significant extent.

To anticipate the analytical results, **Figure 6** illustrates the distribution of vote shares for the BJP (as well as total turnout) for the linked 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.

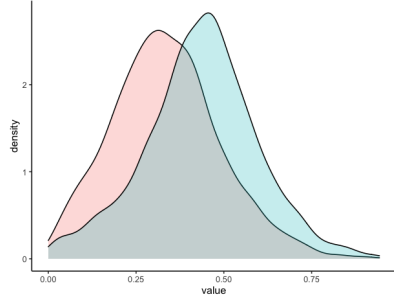
As shown, there is no discernible difference in votes for the BJP between these groups, neither in terms of voting in 2019 *nor* in terms of the shift between 2014 and 2019. Turnout density appears slightly higher for 2019 in villages that can anticipate future transfers. The next subsections confirm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



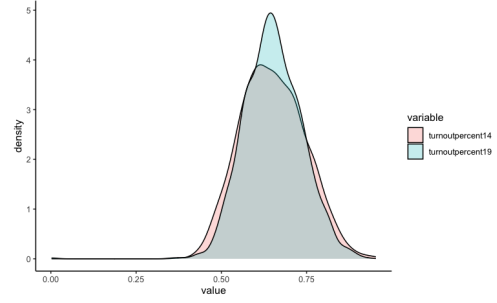
(a) Share of votes for BJP, pre-election recipients



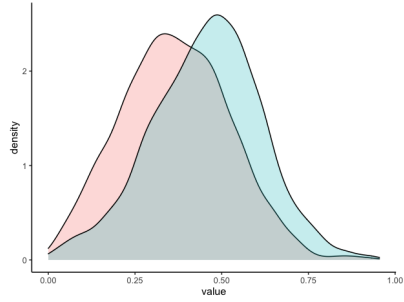
(b) Turnout, pre-election recipients



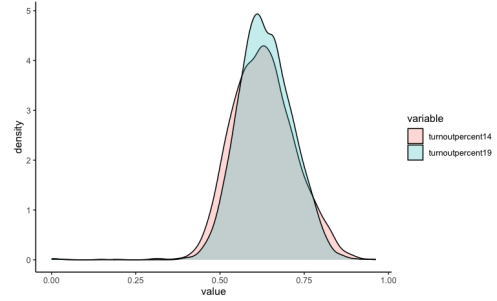
(c) Share of votes for BJP, future recipients



(d) Turnout, future recipients



(e) Share of votes for BJP, ineligible villages



(f) Turnout, ineligible villages

Figure 6: Village outcomes by transfer status

For the villages I linked to 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). Distributions in pink are for 2014 and distributions in blue are for 2019.

## 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 district, 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)
Eligible	−0.0002 (−0.020, 0.020)	0.012** (0.002, 0.022)
PC & District controls	Yes	Yes
Observations	3,034	3,034
R <sup>2</sup>	0.255	0.402
Adjusted R <sup>2</sup>	0.222	0.376
Residual Std. Error (df = 2906)	0.141	0.065

*Note:*

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

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

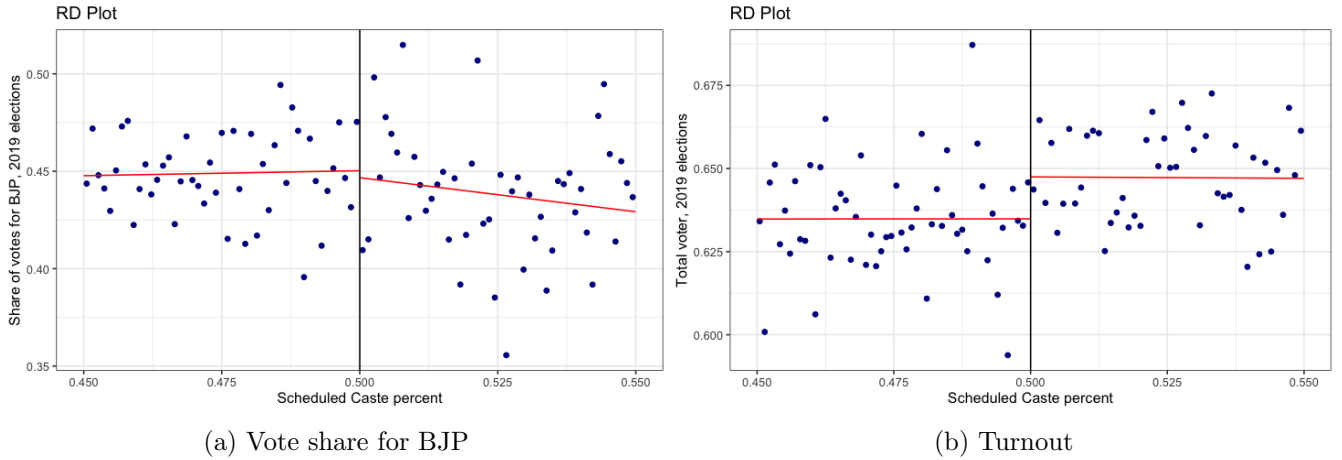


Figure 7: Reduced form RDD

Panels (a) and (b) illustrate the RDD estimates in Table 4.

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. The point estimate is close to zero, and at the 95% confidence interval effects greater than 2.0 pct points can be ruled out. There does appear to be a modest effect on

overall turnout, with villages just above the eligibility cutoff having 1.2 pct points higher turnout than those just below the cutoff, significant at the 5% 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>40</sup> The coefficient on “Eligible” is  $\beta_s$  while the coefficient on “Pre-election recipient” is  $\beta_r$ . As explained above, the former isolates the effect of crossing only the eligibility threshold, and 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	−0.001 (−0.023, 0.020)	0.016 (−0.014, 0.046)	0.014*** (0.004, 0.024)	0.005 (−0.009, 0.020)
Pre-election recipient	0.0005 (−0.020, 0.021)	−0.004 (−0.031, 0.023)	−0.010** (−0.020, −0.001)	0.002 (−0.010, 0.014)
Bandwidth	Share	Dual	Share	Dual
PC & District controls	Yes	Yes	Yes	Yes
Observations	3,034	1,498	3,034	1,498
R <sup>2</sup>	0.254	0.283	0.432	0.424
Adjusted R <sup>2</sup>	0.221	0.216	0.407	0.370
Residual Std. Error	0.141 (df = 2905)	0.142 (df = 1369)	0.064 (df = 2905)	0.061 (df = 1369)

*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 linear 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, and we cannot reject the null hypothesis that  $\beta_s = 0$  nor that  $\beta_r = 0$ . In the preferred specification with a share bandwidth only, effects greater than 2.1 pct points can be excluded for both coefficients at the 95% confidence level (in the specification with half the observation size, the confidence interval is predictably wider). Neither crossing only the eligibility cutoff nor additionally crossing the size cutoff

<sup>40</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).

appears to influence BJP vote share.

In Columns 3 and 4, where the outcome is the village’s 2019 turnout, the preferred specification with greater power picks up an effect of 1.4 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>41</sup> Although this outcome is not structurally derived, it remains true econometrically that  $\beta_s$  is the effect of only crossing the eligibility threshold while  $\beta_r$  is the effect of additionally crossing the size threshold.

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>42</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 Robustness and falsification tests

The following tests are all presented in **Appendix C**. Regarding functional form, **Tables C3** and **C4** show that the results hold for the reduced form as well as multi-score specifications, respectively, when using a quadratic (instead of linear) specification. **Table C5** 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 also have an effect on outcomes.<sup>43</sup>

Regarding bandwidth, for the reduced form and multi-score RDDs **Table C6** and **Table**

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<sup>41</sup>The significance disappears with the smaller sample size after the dual restriction.

<sup>42</sup>As in the unrestricted model, I set  $\beta_p = 0$ .

<sup>43</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.

**C7** 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 narrower confidence intervals.

**Table C8** 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>44</sup> The findings remain intact: 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. Meanwhile, turnout is slightly higher for villages that could expect transfers post-election.

Importantly, for a falsification exercise, **Table C9** presents the results from using lagged outcomes for the reduced form and multi-score specifications. The placebo outcomes are the village's share of votes for the BJP in 2014, and its turnout in 2014; for the RDD to be valid, it would be necessary that it not predict jumps in past outcomes. Indeed, all coefficients in all specifications are close to zero and insignificant, with confidence intervals ruling out meaningful effects.<sup>45</sup> Therefore, **Table C9** complements the finding in Section 5.3 that villages are balanced on covariates, as it shows that this holds also with respect to baseline political preferences (with the share bandwidth restriction being sufficient for this).

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<sup>44</sup>Although the links 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>45</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).

## 7 Discussion

### 7.1 Interpretation of results

On the central outcome of interest, transfers had no impact on the vote share for the incumbent among villages that were eligible for future distribution, nor among villages that had already received them. Instead, the large shift in BJP vote share in *all* villages (around the cutoff) between 2014 and 2019 corresponds to a general shock  $\mathcal{R} > 0$  in favor of the BJP, entirely exogenous to these treatments (Section 3); this may be due to increased appeal across the board of the ethnocentric narrative, of Modi, or a mix of these and other factors. On turnout, eligibility for future transfers increased village turnout slightly while receipt of prior transfers had no impact, so that the effect picked up in the reduced form RDD (Table 4) is driven 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.

Next, I explore whether the limited effects on vote share simply reflect program-specific limitations or frictions. I argue that this is improbable and that the findings likely reflect villagers’ preferences and expectations, and connect the discussion to the model in Section 3.

Before delving into mechanisms, however, it should be noted 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>46</sup> Nevertheless, to the extent that transfer eligibility and receipt are themselves of

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<sup>46</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.

interest, the results show that these did not coopt the constituency into the incumbent’s base.

## 7.2 Mechanisms

### 7.2.1 Information frictions and transfer size

There are three ways in which the treatments may generate limited effects on incumbent vote share, even if villagers’ preferences and expectations were amenable to being influenced by programmatic transfers. This would happen if the villagers did not even know about the transfers, if they knew but inaccurately attributed them to other parties, or if the transfers were too negligible in size to have any effect on behavior.<sup>47</sup>

In terms of awareness of the program, there is reason to doubt that the null effects on vote share are a result of lack of knowledge. Most importantly, it appears the BJP has been engaged in heavy publicity efforts around PMAGY. On the official website for the program, there are dozens of sample pictures of BJP officials gathering villagers in target villages in Uttar Pradesh to discuss the program’s intended initiatives in the village, of advertisements about the program in local newspapers, and of villagers being handed information leaflets about it. Such efforts are consistent with the fact that under Modi’s leadership the BJP has been exceptionally savvy in political PR and in connecting with voters at the grassroots.<sup>48</sup> As secondary points, (i) the effect on vote share is similarly null among past recipients, where zero knowledge about the treatment is even less plausible than for future recipients especially amid PR efforts six months before the elections, and (ii) the effect picked up on turnout, albeit small, suggests that at least *some* future recipients were aware of the transfers.

Another information issue arises if villagers inaccurately give credit to the other parties, for example through attributing the transfers to the local government’s party instead of the

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<sup>47</sup>These can be connected to the theoretical model as follows. Lack of awareness about the transfers can be integrated with a simple information parameter which multiplies  $U_b(T_b^R) - (1 - \gamma)U_b(T_b^L)$ , or  $\mathcal{R}_a$ , as the share of “aware” villagers. These expressions, which measure transfer effects, would be zero if the information parameter is zero. Attributions to the wrong party, i.e. conflating  $T_b^R$  with  $T_a^R$  for future recipients and generating a loyalty shock in favor for the opposition for past recipients, would cause the sign of the effect to flip, as explained below. Transfers that are too small would render  $T_b^R \rightarrow 0$ ; if the same is expected of the competing party, then they exert no impact on voting behavior of future recipients; if the loyalty parameter  $\mathcal{R}_a$  is increasing in the transfer amount, the effect of small amounts approaches zero for past recipients as well.

<sup>48</sup>Remarkably for a national leader, Narendra Modi himself has a weekly radio show in which he discusses social issues and where Indian citizens can have direct access to him, see Upadhyay and Upadhyay (2020).

federal government’s party. Note that this would occur if, along with inaccurate attribution, the majority of villages had non-BJP local governments, so that the point estimate, which is a weighted average of effects across villages, is pushed downward until it reaches almost zero (the results in the paper); the point estimate would go towards the *negative* as more villages shift votes from the incumbent to competing parties receiving credit for the transfers. But in Uttar Pradesh in particular - one of the most BJP-aligned states in the country - the majority of local government positions are also BJP held, so that the impact of such credit-transference would be small.

To show that the results are not driven by rewards to non-BJP local parties, I run the RDD specifications on the subset of villages where the BJP also held the local legislative seat (around three-fourths of the total sample). If inaccurate attribution were driving results, we would expect positive effects on incumbent support at least in this subset, but as **Table C10** shows, the results hold in this subset too.<sup>49</sup>

Distinct from information frictions is the possibility that villagers knew about the program and attributed it to the BJP, but the transfers were negligible so that the prior would be no effect on incumbent support. It is subjective what makes a transfer amount “sizeable”, but two points of reference can help: household income (even though the transfer was not a direct income supplement like cash programs), and other programmatic transfers in rural India. Both frames of reference suggest the amount is not trivial. The average target village has about 1,000 people, so that the transfer (in 2018) was equivalent to \$73-\$88 for a family of 5-6 people; it can be estimated that this is higher than the average monthly income of target households at that time.<sup>50</sup> This amount is also comparable to other major programmatic

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<sup>49</sup>A related but distinct possibility is that villagers not only inaccurately think of the program as a “local” one, but also only reward parties accordingly in local (legislative) elections, without having their preferences translate to national choices. Although it is unclear why this kind of compartmentalization would hold, I plan to generate data on local legislative election results (which requires additional village-booth linking) as an upcoming extension for the analysis.

<sup>50</sup>The transfer equivalence figure is obtained by using the 2018 exchange rate of 0.0146 USD per Rupee, for 1 million Rupees, dividing by average village size and multiplying by household size. To compare to household incomes, there is no information on average income by village, but I use the fact that a few years prior to the program the average rural farming household in Uttar Pradesh had a monthly income of about \$72 (Times of India, 2017), among the lowest in the nation. Given that Scheduled Caste families are particularly impoverished, this is a ceiling of the average household monthly income in target villages.

transfers in rural India.<sup>51</sup>

### 7.2.2 Underlying preferences and expectations

If the program was publicized and substantive in size, and given that credit attribution issues are not driving the results, why would future or past recipients have limited response in terms of their votes for the incumbent?

In particular, regarding the effect on incumbent vote share, the results imply that:

$$\Delta_b = 0 \tag{7.1}$$

$$\Delta_a = 0 \tag{7.2}$$

Recall that, for future recipients,  $\Delta_b = 0$  when  $U_b(T_b^R) - (1 - \gamma)U_b(T_b^L) = 0$ . Substantive transfers imply  $T > 0$  while low information frictions imply information problems do not nullify or reverse the decision making process (see footnote 48). In this case, and as mentioned earlier,  $\Delta_b = 0$  could arise from villagers expecting future transfers to be equal regardless of who wins ( $T_b^R = T_b^L$ ), or from villagers being ideologically rigid (causing low  $U'$ ), or a combination of the two.

Further ethnographic data would help pin this down further, but there is reason to expect that at least the first mechanism - equivalent expectations about persistence of the program - is relevant in this context. As explained in Section 2, Scheduled Castes are a core electoral constituency and a range of parties have tried to woo them. The Congress Party, historically the BJP's main national competitor, was the one to first conceptualize the idea (despite never implementing it) precisely due to electoral concerns. Meanwhile, the main party which competes with the BJP for Uttar Pradesh Lok Sabha seats, called BSP, is one which identifies with and has historically represented and advocated for marginalized caste groups (Kumar,

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<sup>51</sup>For example, a key flagship scheme of the Ministry of Agriculture and Farmers' Welfare, called PM-KISAN, transfers the equivalent of \$87 per year per farmer (in 2018 USD) to eligible farmers in India. A plan which supports maternal care, PM Matritva Vandana Yojana, distributes the same amount to pregnant and lactating women. A third program which supports rural entrepreneurs including women, called Standup India, distributed an average of \$28 per loan in loans between 2016 and 2020.



1999). Adding to the competitive drive to capture Scheduled Caste votes is the fact that the weight of this group’s vote has shifted over time and is not a stable or predictable allegiance for one specific party (Misra, 2020).

For these reasons, competition for Scheduled Caste votes is particularly fierce. In such a context, it would not be surprising that voters in eligible villages expect the BJP’s political competitors to also commit to this program if they were elected. In fact, canonical models like Dixit and Londregan (1996) show that competing parties’ optimal strategies regarding core swing constituencies would be to offer equally generous distribution promises and to follow through on them. In this case and with low incumbency credibility advantage, economic expediency by party for future recipients would be equivalent, so that  $\Delta_b = 0$ . Only *other* factors which actually differentiate the BJP from other parties in the voters’ eyes (e.g. the ethnocentric narrative, Modi, or other factors) would impact incumbent vote share.

With regard to past receipt of transfers and assuming no major information frictions, the mechanism is straightforward:  $\Delta_a = 0$  when  $\mathcal{R}_a = 0$ , i.e. when receipt does not generate feelings of obligation or reciprocal loyalty for the incumbent. Although the concept of “vote buying” based on reciprocal loyalty has been explored in the literature, it has been usually discussed in the context of clientelistic benefits targeted and delivered personally (e.g. Finan and Schechter, 2012). If loyalty feelings arise precisely from being targeted with a high level of personalization and discretion, but not from being the recipient of a program with clear eligibility rules, we would see limited impacts in the latter setting. Of course, feelings of loyalty and subsequent electoral reward may result from feeling targeted as a social group (Scheduled Caste heavy villages in general), but as explained earlier this would not be picked up in this regression discontinuity design.

## 8 Conclusion

In 2019, the Indian polity voted in the largest democratic exercise in history and reelected the Hindu-nationalist incumbent (BJP) to parliamentary majority by a wide margin. This paper offers the first disaggregated evidence on whether transfers shifted target groups into

the BJP's base, focusing on programmatic distribution targeting villages with a high share of disadvantaged castes, and on its implementation in India's largest state.

I first provide a model of voting behavior in response to past and future programmatic 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 linking 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 incumbent 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 slightly (1.4%). Instead, factors exogenous to these treatments shifted affinity for the incumbent across the board. I explain why the context supports the interpretation that villagers eligible for future transfers likely expected them to be supported by competing parties as well, while past recipients felt little obligation or reciprocal loyalty from non-discretionary distributions, causing both promised and received transfers to have limited electoral impact.

The results contribute to our understanding of the effect of programmatic transfers on political outcomes in developing countries and on what helps keep incumbents, including populists, in power. In the Indian context, the findings shed skepticism on the notion that economic benefits have necessarily been key for coopting poor populations into the Hindu-nationalist electoral base, at least for the constituency under study. More research can investigate the extent to which the findings apply for other constituencies and in other settings, and explore other potential drivers of ethnic nationalism in the Indian democracy.

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## APPENDIX

### A Linking villages to polling booths

To try to link 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 village-booth linking but include name variations of some villages).
3. If Step 1 or 2 generate only one possible 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 possible links 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 link  $v_i$  to  $Y$ .<sup>52</sup>
5. Instead, suppose Steps 1 and 2 do not generate any possible booth link 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 links 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 unlinked 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 link 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

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<sup>52</sup>After linking, 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 village-booth links and work to 2019, and not vice versa.<sup>53</sup> *Within each district* and for every village:

1. If I was able to generate a 2014 booth link, 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 booth link, 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 link about 6,300 villages to where they voted across the two election cycles. This manual linking process, while highly time intensive, yields the highest possible accuracy, given the notoriously inaccurate village pincodes (so that linking based on geolocation is highly flawed) and the frequency of villages with similar names and wide range of translation spelling mistakes (so that using a name-matching algorithm is also flawed). Moreover, as it uses the unique 6-digit village codes, it also provides more accurate results than manual efforts relying on auxiliary websites such as [Village Atlas](#) or [OneFiveNine](#) - which are used by Hinston and Vaishnav (2021) for checks on their algorithm - and which by comparison include numerous inconsistencies.<sup>54</sup>

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<sup>53</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>54</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 (is the village of interest). It is not uncommon, under the page for  $v_1$  information, to find a Google Map of  $v_2$  instead, so that using information on “neighbors” to generate a booth link 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)	0.010 (-0.014, 0.033)	0.012*** (0.004, 0.020)	0.001 (-0.009, 0.012)
Bandwidth	Share	Dual	Share	Dual
PC & District controls	Yes	Yes	Yes	Yes
Observations	3,034	1,498	3,034	1,498
R <sup>2</sup>	0.254	0.283	0.432	0.424
Adjusted R <sup>2</sup>	0.221	0.217	0.407	0.371
Residual Std. Error	0.141 (df = 2906)	0.142 (df = 1370)	0.064 (df = 2906)	0.061 (df = 1370)

*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; linear 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	0.0047	0.3959	0.3786	0.9394
Pr(>F)	0.9456	0.5293	0.5384	0.3326

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 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: Reduced form with quadratic specification

	<i>Dependent variable:</i>	
	Vote share for BJP (1)	Turnout (2)
Eligible	0.008 (−0.023, 0.040)	0.014* (−0.001, 0.030)
PC & District controls	Yes	Yes
Observations	3,034	3,034
R <sup>2</sup>	0.255	0.402
Adjusted R <sup>2</sup>	0.222	0.376
Residual Std. Error (df = 2904)	0.141	0.065

*Note:*

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

Table C3 reports the results of the reduced form RDD with a quadratic specification for the centered running variable.

Table C4: MRDD with quadratic specification

	<i>Dependent variable:</i>			
	Vote share for BJP		Turnout	
	(1)	(2)	(3)	(4)
Eligible	0.00001 (−0.021, 0.021)	0.016 (−0.015, 0.046)	0.014*** (0.004, 0.024)	0.005 (−0.009, 0.019)
Pre-election recipient	−0.0001 (−0.021, 0.021)	−0.005 (−0.032, 0.022)	−0.010** (−0.019, −0.001)	0.001 (−0.011, 0.013)
Bandwidth	Share	Dual	Share	Dual
PC & District controls	Yes	Yes	Yes	Yes
Observations	3,034	1,498	3,034	1,498
R <sup>2</sup>	0.254	0.283	0.436	0.427
Adjusted R <sup>2</sup>	0.221	0.215	0.411	0.372
Residual Std. Error	0.141 (df = 2903)	0.142 (df = 1367)	0.064 (df = 2903)	0.061 (df = 1367)

*Note:*

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

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

Table C5: MRDD with  $\beta_p \neq 0$ 

	<i>Dependent variable:</i>			
	Vote share for BJP		Turnout	
	(1)	(2)	(3)	(4)
Eligible	−0.0004 (−0.022, 0.021)	0.018 (−0.013, 0.048)	0.013** (0.003, 0.024)	0.005 (−0.009, 0.020)
Above size	0.012 (−0.007, 0.031)	0.021 (−0.008, 0.051)	−0.007 (−0.016, 0.002)	−0.0004 (−0.014, 0.013)
Pre-election recipient	−0.007 (−0.031, 0.016)	−0.015 (−0.045, 0.015)	−0.006 (−0.016, 0.005)	0.002 (−0.011, 0.016)
Bandwidth	Share	Dual	Share	Dual
PC & District controls	Yes	Yes	Yes	Yes
Observations	3,034	1,498	3,034	1,498
R <sup>2</sup>	0.254	0.284	0.432	0.424
Adjusted R <sup>2</sup>	0.221	0.217	0.407	0.370
Residual Std. Error	0.141 (df = 2904)	0.142 (df = 1368)	0.064 (df = 2904)	0.061 (df = 1368)

*Note:*

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

Table C5 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 linear specification for the centered running variables.

Table C6: Reduced form with different bandwidths

<i>PANEL A. Dependent variable: Vote share BJP</i>					
	(1)	(2)	(3)	(4)	(5)
Eligible	0.008 (-0.009, 0.025)	0.006 (-0.012, 0.025)	0.003 (-0.020, 0.026)	0.009 (-0.017, 0.036)	0.021 (-0.009, 0.051)
Observations	4,372	3,727	2,420	1,704	1,264
R <sup>2</sup>	0.234	0.237	0.256	0.274	0.301
Adjusted R <sup>2</sup>	0.211	0.210	0.215	0.215	0.222
Residual Std. Error	0.142 (df = 4243)	0.142 (df = 3598)	0.141 (df = 2292)	0.142 (df = 1576)	0.140 (df = 1136)
<i>PANEL B. Dependent variable: Turnout</i>					
	(1)	(2)	(3)	(4)	(5)
Eligible	0.010** (0.002, 0.019)	0.012** (0.003, 0.021)	0.011* (-0.00001, 0.023)	0.008 (-0.004, 0.021)	-0.013 (-0.011, 0.020)
Observations	4,372	3,727	2,420	1,704	1,264
R <sup>2</sup>	0.389	0.397	0.412	0.423	0.414
Adjusted R <sup>2</sup>	0.370	0.376	0.379	0.376	0.349
Residual Std. Error	0.066 (df = 4243)	0.066 (df = 3598)	0.066 (df = 2292)	0.063 (df = 1576)	0.062 (df = 1136)

*Note:*

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

Table C6 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 C7: MRDD with different bandwidths

<i>PANEL A. Dependent variable: Vote share BJP</i>					
	(1)	(2)	(3)	(4)	(5)
Eligible	0.008 (−0.010, 0.026)	0.005 (−0.014, 0.025)	0.003 (−0.021, 0.027)	0.009 (−0.019, 0.038)	0.020 (−0.013, 0.053)
Pre-election recipient	0.001 (−0.016, 0.019)	0.002 (−0.017, 0.021)	0.001 (−0.021, 0.024)	−0.001 (−0.026, 0.025)	−0.002 (−0.031, 0.027)
Observations	4,372	3,727	2,420	1,704	1,264
R <sup>2</sup>	0.237	0.237	0.256	0.273	0.304
Adjusted R <sup>2</sup>	0.211	0.209	0.214	0.213	0.224
Residual Std. Error	0.142 (df = 4240)	0.142 (df = 3595)	0.141 (df = 2289)	0.142 (df = 1573)	0.140 (df = 1133)
<i>PANEL B. Dependent variable: Turnout</i>					
	(1)	(2)	(3)	(4)	(5)
Eligible	0.012*** (0.003, 0.020)	0.014*** (0.004, 0.023)	0.014** (0.002, 0.026)	0.008 (−0.005, 0.022)	0.007 (−0.011, 0.023)
Pre-election recipient	−0.005 (−0.013, 0.003)	−0.007* (−0.015, 0.001)	−0.012** (−0.022, −0.002)	0.001 (−0.010, 0.012)	−0.002 (−0.015, 0.010)
Observations	4,372	3,727	2,420	1,704	1,264
R <sup>2</sup>	0.421	0.432	0.444	0.443	0.426
Adjusted R <sup>2</sup>	0.403	0.411	0.412	0.397	0.360
Residual Std. Error	0.065 (df = 4240)	0.064 (df = 3595)	0.064 (df = 2289)	0.062 (df = 1573)	0.062 (df = 1133)

*Note:*

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

Table C7 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 C8: 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	0.007 (−0.008, 0.023)	0.008* (−0.001, 0.017)
Observations	2,841	2,841
R <sup>2</sup>	0.343	0.178
Adjusted R <sup>2</sup>	0.313	0.140
Residual Std. Error (df = 2713)	0.106	0.057
<i>PANEL B. Multi-score RDD</i>		
<i>Outcome</i>	Change in vote share BJP	Change in turnout
	(1)	(2)
Eligible	0.006 (−0.010, 0.022)	0.008* (−0.002, 0.018)
Pre-election recipient	0.005 (−0.010, 0.020)	−0.002 (−0.012, 0.008)
Observations	2,841	2,841
R <sup>2</sup>	0.344	0.186
Adjusted R <sup>2</sup>	0.313	0.186
Residual Std. Error (df = 2712)	0.106	0.057

*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 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 C9: Specifications with 2014 outcomes (placebo)

<i>PANEL A. Reduced form RDD</i>		
<i>Outcome</i>	Past vote share BJP	Past turnout
	(1)	(2)
Eligible	−0.009 (−0.029, 0.010)	0.004 (−0.007, 0.015)
Observations	2,841	2,841
R <sup>2</sup>	0.272	0.428
Adjusted R <sup>2</sup>	0.238	0.402
Residual Std. Error (df = 2713)	0.135	0.073
<i>PANEL B. Multi-score RDD</i>		
<i>Outcome</i>	Past vote share BJP	Past turnout
	(1)	(2)
Eligible	−0.009 (−0.030, 0.012)	0.006 (−0.005, 0.016)
Pre-election recipient	−0.005 (−0.025, 0.016)	−0.007 (−0.020, 0.006)
Observations	2,841	2,841
R <sup>2</sup>	0.272	0.475
Adjusted R <sup>2</sup>	0.238	0.450
Residual Std. Error (df = 2712)	0.135	0.070

*Note:*

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

Table C9 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).



Table C10: Subset of villages where BJP was in power locally

<i>PANEL A. Reduced form RDD</i>		
<i>Outcome</i>	Vote share for BJP	Turnout
	(1)	(2)
Eligible	0.008 (−0.014, 0.030)	0.008 (−0.003, 0.019)
Observations	2,379	2,379
R <sup>2</sup>	0.245	0.394
Adjusted R <sup>2</sup>	0.205	0.362
Residual Std. Error (df = 2258)	0.140	0.065
<i>PANEL B. Multi-score RDD</i>		
<i>Outcome</i>	Vote share for BJP	Turnout
	(1)	(2)
Eligible	0.004 (−0.019, 0.028)	0.011* (−0.001, 0.023)
Pre-election recipient	0.008 (−0.015, 0.030)	−0.014*** (−0.025, −0.004)
Observations	2,379	2,379
R <sup>2</sup>	0.244	0.427
Adjusted R <sup>2</sup>	0.204	0.397
Residual Std. Error (df = 2257)	0.140	0.063

*Note:*

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

Table C10 reports the results of the reduced form RDD (Panel A) and the multi-score RDD (Panel B) for the subset of villages in districts where the BJP was in power in the Member's Legislative Assembly.