Transfers and the rise of Hindu nationalism in India

Amal Ahmad*

Abstract

In democracies with widespread poverty, what is the impact of programmatic transfers on voting and on incumbent power? This paper provides the first village-level quasi-experimental evidence on this for India, in the context of the Hindu-nationalist party in power. First, I provide a novel method for linking Indian villages to polling booths and for obtaining village-level electoral data. Second, focusing on a program which transfers development funds to villages with a high share of disadvantaged castes, I use a discontinuity design to identify the effects of both past and promised transfers on voting in India's largest state. Promised transfers increase village turnout slightly but neither treatment impact the villages' vote share for the Hindu-nationalist incumbent, which is high across the board. The results suggest that political competition limits the impact of programmatic transfers on voting behavior, and they shed light on the recent slide to ethnic nationalism in the world's largest democracy.

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*Development Economics Group, Wageningen University. Email: amal.ahmad@wur.nl. I am grateful to Raphael Susewind for sharing his data with me and to Mridhula Mohan for excellent research assistance. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

1 Introduction

Around the time that the victory of Donald Trump in the US 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 country and 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 that took many observers by surprise (Rukmini, 2019). Under the leadership of the popular Narendra Modi and the banner of dually promoting *Hindutva*[1] and economic development, 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.[2]

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 its subsequent reelection have been underpinned by an ability to secure votes from disadvantaged groups, including marginalized caste groups which comprise a large share of the population and which 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 these transfers on specific eligibility criteria and implementing them across the nation.

To what extent did transfers to disadvantaged groups during incumbency anchor reelection in this context? In the literature on economic benefits and political outcomes, there is consistent evidence that *discretionary* transfers, such as through clientelistic networks or

[1]The term was first articulated in 1923, by organizational predecessors of the BJP, to refer to the political ideology of Hindu nationalism.

[2]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.

selective partisan aid, influence voting behavior and generate electoral rewards (Golden and Min, 2013; Mahadevan and Shenoy, 2023). However, for distribution through programs following clear allocation rules, also called *programmatic* transfers, the evidence is concentrated on voters in advanced economies.[3] The evidence on developing country democracies exists for a limited number of countries. Earlier studies found increased incumbent support for cash transfer programs in Uruguay (Manacorda et al, 2011) 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, and in India Zimmerman (2021) finds that the length of exposure to an employment guarantee program mediates its electoral impact. Adding to this complexity, in contexts with both discretionary and programmatic distributions, only the former have been found to impact voting behavior (Wantechkon, 2003; Ortega and Penfold-Becerra 2008; Bardhan et al 2022).

The question of how programmatic transfers impact incumbent power becomes particularly important when the incumbent is transforming the political landscape through a populist agenda, as it becomes informative about 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. The recent empirical literature on populism, surveyed in Guriev and Papaioannou (2022), focuses on the USA and Europe, and does not address 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 because of confounders under populist incumbents. For example, the BJP stresses the importance of a united (Hindu-led) front along ethnic/religious lines, and paints economic and caste cleavages as secondary to this consideration. This narrative may be effective in attracting into the BJP's base the same poor and marginalized caste groups which programmatic transfers may target.

It is to these research areas that this paper contributes. It is the first to provide evi-

[3]See the survey in Healy and Malhotra (2013).

dence on this for India using realized electoral variation at the most micro level possible of the Indian village,[4] and in the context of the Hindu-nationalist incumbent power. It also addresses both retrospective voting, i.e. in response to past policies, and forward looking voting, in response to campaign promises. While some papers have studied transfers and actual election outcomes in India, they have done so at higher levels of aggregation, and they have focused on responses to past distribution and in pre-BJP-incumbency elections.[5] [6]

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.[7] 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 eligibility cutoff is non-manipulable and was used reliably as a sorting instrument: village eligibility was calculated based on the preexisting 2011 Census, and

[4]It is not possible in any way to observe *actual* election outcomes at the individual voter level. Papers on individuals, such as Bardhan et al (2022) and Ray (2021), rely on self-reported answers on electoral support.

[5]In studying the electoral impact of NREGA implementation, Zimmerman (2021) links polling booths to district-level variation in program rollout. It also studies the 2009 elections, prior to the BJP's victory. Closer to - but not quite the same as - this paper's approach is the study of aid to water-distressed regions in West Bengal in Mahadevan and Shinoy (2023), who study variation at the level of *panchayat*, i.e. collection of usually up to 20 villages. The paper studies the impact of this evidently discretionary distribution on voting by panchayats in the national elections of 2014.

[6]Beyond India, research using realized voting outcomes to compare past versus promised transfers remains limited (Elinder et al, 2015).

[7]Scheduled Castes voted by 34% for the BJP, up from 24% in 2014 and 12% in 2009; see Section 2.

no village below the 50% cutoff has received a transfer.[8] 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 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.

Because the empirical design requires information on voting at the village level and because this is not readily available, I first build a carefully linked dataset of villages - in Uttar Pradesh, and within a sufficiently wide bandwidth of the 50% Scheduled Share cutoff - and their votes.[9] Electoral data in India is available only at the polling booth level, and 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. 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. This time-consuming but meticulous process is detailed in **Appendix B** and produces a village-booth linked dataset of over 6,300 villages.

To anchor the empirical analysis, I first provide a model of programmatic transfers and

[8]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.

[9]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%.

voting behavior, detailed in **Appendix A**, 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. Expected transfers by party affect calculations of utility, while past 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 distinct ways. I derive expressions for these possible effects, and use them to derive the relevant estimators (for impacts on vote share) and to explore mechanisms.

I then use the linked dataset in a multi-score sharp regression discontinuity design to estimate these treatment effects. The design departs from the assumption of a single binary treatment variable which other RDDs including other multi-score designs usually adopt, and allows for unbiased estimators in the presence of multiple running variables and heterogeneous treatment effects (Choi and Lee, 2018).[10] 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 village-level voting turnout. I find that eligibility for future transfers increased turnout by a modest 1.4% while past receipt had no impact.

[10]Most 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 Tituinik (2015), respectively. Choi and Lee (2018) show that both are problematic if there are heterogeneous effects from crossing one versus two thresholds.

What accounts for the limited effect of these transfers on incumbent support? After showing that 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.[11] In line with the theoretical model, eligible beneficiaries will have limited electoral response to future transfers when they expect the competing party to match incumbent promises. 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, and in Uttar Pradesh the main BJP competitor has a history of Scheduled Caste advocacy (Kumar, 1999). Voters are therefore likely to expect the BJP's competitors to also commit to this program if elected, in which case only *other* factors which actually differentiate the BJP from other parties (e.g. the ethnocentric narrative or the populist discourse by Modi) would impact vote share. For past recipients, the results suggest transfers generated limited feelings of obligation or reciprocity, feelings which are 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 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

[11]Specifically, 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 nor to the Congress party, i.e. to the "wrong" party.

and the evidence is descriptive (e.g. Jaffrelot, 2021; Aiyar, 2019; Jha, 2017).[12]

By offering the first quasi-experimental evidence on transfers and the BJP's reelection, 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 programmatic benefits. The BJP vote share did increase in the treated villages by an average of 9 percentage points between 2014 and 2019, but it did so equally for the control group, suggesting that little of this triumph owes to these treatments.[13]

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-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. An exception is Hintson and Vaishnav (2021), who study the effect of security crises and nationalist rallies on village-level support for the BJP in 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 B**). More broadly, village-level analysis helps overcome the limitations of using either disaggregated but self-reported measures of electoral support, or actual election outcomes at readily available but higher levels of aggregation, which can limit identification. For India, the readily available political unit at which to aggregate and analyze polling booth data is the Assembly Constituency (e.g. Kapoor and Ravi, 2021), which has about 600,000 people each in Uttar Pradesh, whereas my method disaggregates by 1,300 people (village size), expanding the scope significantly for identification using election outcomes.

[12]An important contribution is Thachil (2014). However, it studies BJP appeal to poor voters prior to the BJP's incumbency, therefore in the absence of the programmatic policies which incumbency allows for.

[13]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.

2 Political and social context

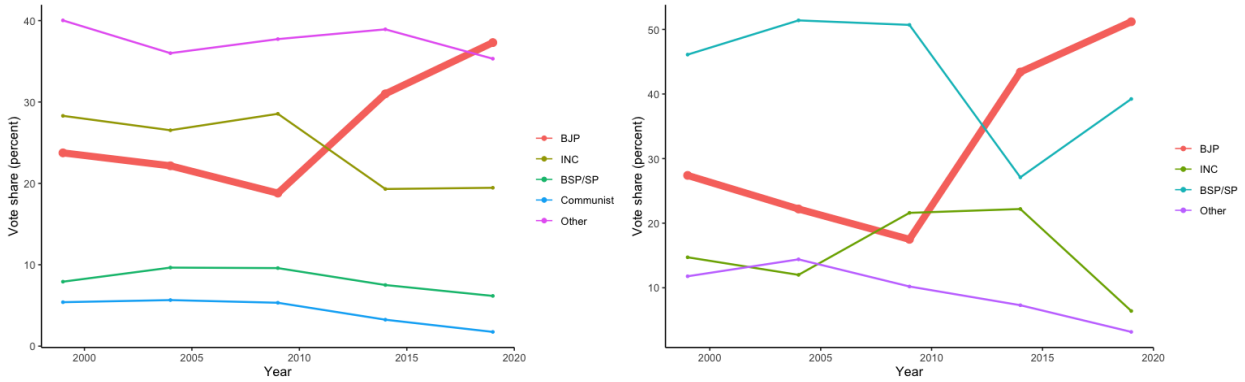
2.1 India's parliamentary system and the Bharatiya Janata Party

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. 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. 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.

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.

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



(a) Popular vote distribution in India

(b) Popular vote distribution in UP

Figure 1: BJP popular vote share 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 bold orange in both panels.

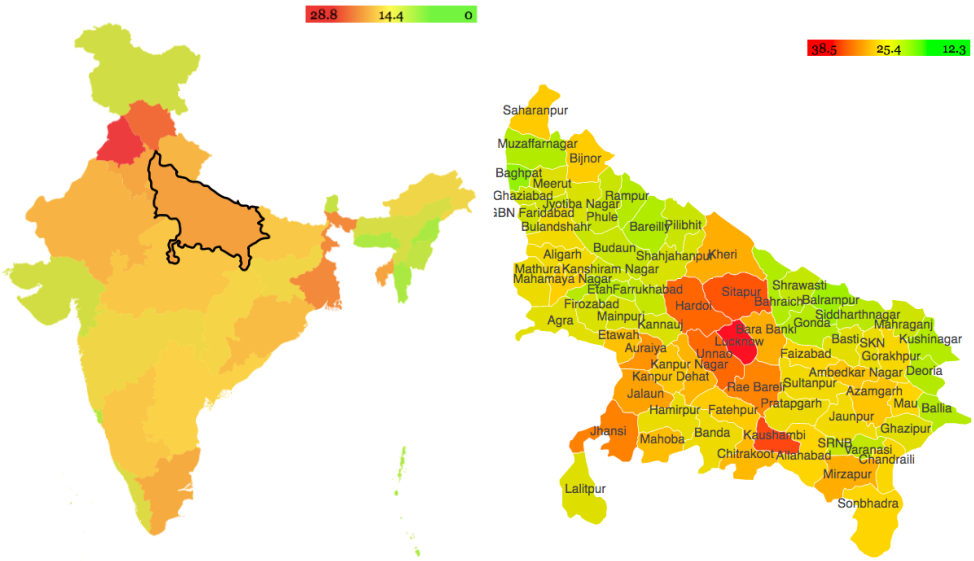
parties (Rukmini, 2019). However, in 2014 and 2019, non-upper caste groups, as well as marginalized groups falling entirely outside of the caste system, turned out for the BJP at unprecedented levels. The Scheduled Castes in particular voted by 34% for the BJP, up from 24% in 2014 and 12% in 2009 (Kumar 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 for the provincial elections of 1937, in the context of greater pressure on colonial authorities to allow for self-rule in India. Post-independence, the Scheduled Caste designation initially

continued to apply to Hindu groups only, but was later also extended to Sikh and Buddhist communities suffering from "untouchability" discrimination.[14]

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



(a) India (b) Uttar Pradesh

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

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

Despite electoral enfranchisement and reserved public office quotas, Scheduled Castes, the majority of whom reside in villages, remain among the poorest and most disadvantaged segments of Indian society. On average, they stand on the lower rungs of wealth (Zacharias and

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

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). Over half of all Scheduled Caste households are landless and work as waged labor instead of on 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 a 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.[15] 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 sum for administrative funds, to help Scheduled Caste-majority villages meet key needs.[16] Each recipient village would have a few target activities identified for

[15]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.

[16]This is $14,600 in 2018 exchange rates; given village sizes and on per capita terms, this is equivalent to

which financing gaps existed and which the funds would help fill, centering around needs such as clean drinking water supply and sanitation drainage systems.[17]

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.[18] 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

In this section I summarize a model of voting behavior in response to received or promised transfers, which I present in detail in **Appendix A**. The model combines probabilistic voting with backward and forward looking voters, and generates testable predictions about the

about $57-70 per family in recipient villages. I put these numbers in further context in section 7.

[17]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.

[18]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.

impact of transfers on village-level vote shares for an incumbent. I use it to structurally derive the estimators in the empirical design (Section 5) and to help interpret the findings.

In the model, an incumbent and opposition party are competing in an election. There is also a program distributing one-time funds to villages based on specific eligibility criteria. Mirroring PMAGY structure, I assume villages belong to one of three types based on eligibility: one is eligible for (and received) the transfer prior to the elections, one is eligible to receive a transfer post-election, and one is ineligible to receive a transfer at any point.

In line with models of programmatic transfers and voting, I assume that individuals decide who to vote for by weighing expected utility from future transfers from each party against relative ideological preference for the parties, and that in equilibrium the parties make credible pledges about future transfers which they follow through on if they win.[19] Following Dixit and Londregan (1996), I assume that while individuals within a village have varying ideological preferences, the distribution of ideological preference (how incumbent-friendly the village is on average) is fixed for villages of a specific type. In addition, for individuals whose village received a pre-election transfer under the incumbent, I allow for past disbursement to generate a positive shock to their relative ideological preference for the incumbent, which can be understood as arising from feelings of reciprocal loyalty.

Drawing on this setup, I calculate mean village-level vote shares for the incumbent in each village type, as well as the difference-in-means in vote shares between village types. The model underscores four key points which inform the empirical design and findings.

First, both prior and promised transfers can impact voting behavior albeit in distinct ways. Villagers who are eligible for future funding will factor this into their expected utility-by-party calculations, while past recipients - and who are not eligible for repeated transfers - will react, if at all, on the basis of (loyalty) shifts to their preference for the incumbent. There

[19]Models with these assumptions include Dixit and Londregan (1996), Grossman and Helpman (1996), and Bardhan et al (2022).

is no a priori reason to expect these potential treatment effects to be similar in magnitude.

Second, for each of the two treatments, comparing the average incumbent vote share in treated villages versus in control villages (the ineligible type) yields a difference of means with two components. In particular, the difference-in-means will include (i) the treatment effect of having received a prior transfer, or of being eligible to receive a future transfer and (ii) the differences in average incumbent preference (affinity) between treated and control types. For example, if future recipients are also more ideologically incumbent-aligned than non-eligible villages, the difference-of-means between the two groups will be higher than (just) the treatment effect of promised transfers.

Third, the model illustrates the conditions under which the treatment effects may or may not materialize. In particular, future funding will increase incumbent vote share only if these voters are not ideologically rigid and if they expect future funding to be greater under the reelected incumbent than under the opposition party. In contrast, if voters expect both parties to commit to the program to similar extents, the relative utility differential and therefore electoral impact of promised funding will be muted. Meanwhile, past funding will only increase vote share among recipients only insofar as receipt generates feelings of loyalty toward the incumbent. And information problems which mute awareness of the program among villagers, as well as a small transfer amount which is insufficient to impact villagers' utilities or preferences, would mute both treatment effects.

Fourth, and aside from these treatment effects, the model considers the possibility of a general shock to incumbent popularity which impacts all villages regardless of type. Naturally, such a common shock would not impact different-of-means in incumbent vote shares between village types. However, its effect would show in comparisons of vote shares *within* each village type between two electoral cycles. Therefore, with information on village-level voting not just in the electoral cycle which divides past and future transfer receipt, but also in the prior cycle, shifts in incumbent popularity across the board can be revealed.

Informed by the theoretical model, I structure the empirical design in Section 5 to estimate the (potentially heterogeneous) effects of past and future receipt on incumbent vote shares separately, and to deal with potentially confounding differences between treated and control villages' political preferences. In addition, with data on voting outcomes from two cycles, I can investigate the presence of a general shift in affinity for the BJP between 2014 and 2019 for the villages under study.

To do this, data is needed on transfer status and electoral outcomes at the village level, so as to construct village-level incumbent vote shares by treatment group.

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.[20] I also use the SHRUG datasets, which use Census codes, to explore further village-level characteristics for balance tests (Section 5).

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

[20]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.

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.[21] 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 160,000 polling booths across 403 "Assembly Consistuencies" (ACs) in the state.[22] 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).

To find 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

[21] 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.

[22] 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.

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 B**.

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.[23] 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.[24]

Finally, although I was able to link most villages to polling booths, not all linkages are equally useful for the empirical analysis. Small villages often vote together or with larger villages in the same booth. For example, a village of size 600 may be voting in a booth where the total electorate (as indicated from the booth information) is 1,500 people, due to the inclusion of other villages as well. In this case, even though I am certain this is where the

[23]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.

[24]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.

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.[25] 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 outlined in Section 3, transfers can impact voting behavior in two distinct ways depending on timing of receipt, and a simple difference of means in outcomes between treated and untreated villages would not isolate the treatment effects.

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.

[25] 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.[26]

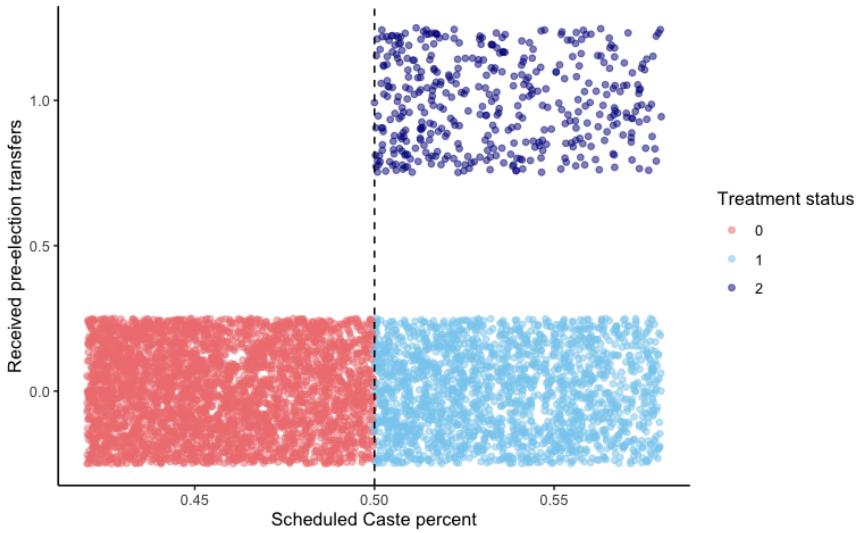


Figure 3: Single discontinuity

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. Dark blue observations received pre-election transfers, light blue observations were eligible for post-election transfers, and red observations are ineligible.

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

[26]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).

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

$$Y_i = \theta_0 + f(SC_{i,s} - 0.5) + \theta_1 Z_i + \theta_2 PC_i + \theta_3 D_i + e_i \tag{1}$$

where f is a function of the centered running variable 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 θ_1 identifies the (local) effect of barely crossing the eligibility cutoff on Y_i:

$$\theta_1 = lim_{SC \to 0.5^+} E[Y|SC = 0.5] - lim_{SC \to 0.5^-} E[Y|SC = 0.5] \tag{2}$$

where θ_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 the effects of prior receipt and of eligibility for future transfers.

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.[27] It illustrates that, when both scores are taken into account, the discontinuities becomes 2-dimensional and sharp , i.e. it is possible to determine treatment status from the value of the scores.

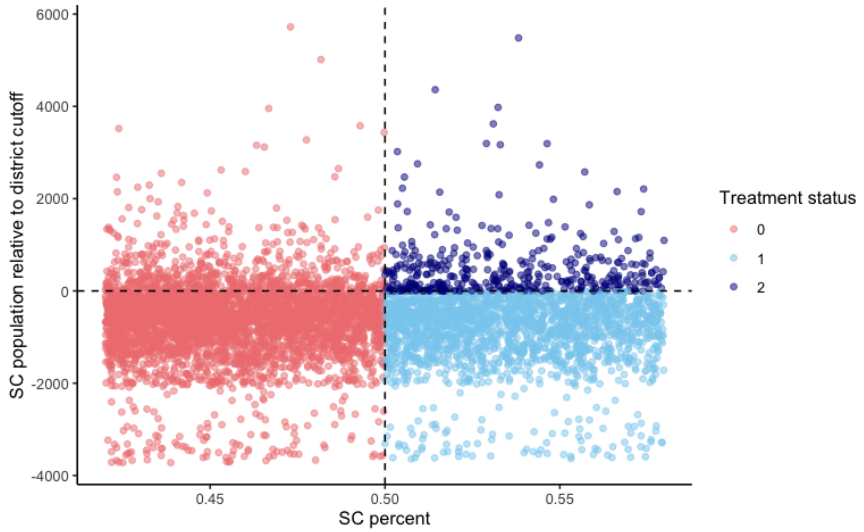


Figure 4: Multi-score sharp discontinuities

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. Dark blue observations received pre-election transfers, light blue observations were eligible for post-election transfers, and red observations are ineligible.

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

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

- $Z_p \in \{0, 1\}$ where $Z_p = 1$ if $SC_p \geq c$, and 0 otherwise.[28]

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

$$Y_i = \beta_0 + f\Big((SC_{i,s} - 0.5), (SC_{i,p} - c)\Big) + \beta_s Z_{i,s} + \beta_p Z_{i,p} + \beta_r R_i + \beta_{pc} PC_i + \beta_d D_i + e_i \quad (3)$$

where f is a function of the centered running variables, $R_i = Z_{i,s} * Z_{i,p}$ and is therefore 1 for pre-election recipients and 0 otherwise, and the specification is run on a bandwidth around both cutoffs.[29]

β_s and β_r in Eq. (3) are the causal estimators of interest. In **Appendix C** I show formally that the estimators can be expressed as:

$$\beta_s = lim_{SC_s \to 0.5^+, SC_p \to c^-} E[Y|\mathbf{S}] - lim_{SC_s \to 0.5^-, SC_p \to c^-} E[Y|\mathbf{S}] \quad (4)$$

$$\beta_r = \Big(lim_{SC_s \to 0.5^+, SC_p \to c^+} E[Y|\mathbf{S}] - lim_{SC_s \to 0.5^+, SC_p \to c^-} E[Y|\mathbf{S}]\Big) - \Big(lim_{SC_s \to 0.5^-, SC_p \to c^+} E[Y|\mathbf{S}] - lim_{SC_s \to 0.5^-, SC_p \to c^-} E[Y|\mathbf{S}]\Big) \quad (5)$$

where **S** references the value of the running variables at the cutoffs.

To understand these estimators, focus first on β_s in Eq. (4), and let Y be the incumbent vote share at the village level. Then β_s expresses the difference in vote share between villages just crossing the 50% eligibility threshold while being just below the size cutoff, i.e. villages eligible for future transfers, and villages under the eligibility as well as size cutoff, i.e. a

[28] Here, c is the population cutoff for that district, as explained in footnote 26.

[29] Bandwidth selection is difficult to derive formally in this case; Choi and Lee (2018) recommend starting from a sensible cutoff combination and checking the robustness of the results to other cutoffs. In Section 6, I use a 5% bandwidth on each side of eligibility, as this 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 below. I check that results are robust to changing the bandwidth combinations in the different directions.

subset of ineligible villages.

Since β_s is the difference-in-means of the outcome between the promised transfers group and a subset of the control group, with the bandwidth restriction generating baseline similarity, it is the local estimator for the impact of eligibility for a future transfer.

Second, examine β_r in Eq. (5). The expression comprises the difference in vote share from moving just above the size cutoff among barely eligible villages (first parenthesis) minus any effect of moving just above the size cutoff among barely ineligible villages (second parenthesis). Since there is no reason for just barely crossing the size threshold among ineligible villages to affect votes, I assume the second parenthesis is zero.[30] Focusing on the first parenthesis then, β_r compares outcomes in villages that received pre-election transfers with outcomes not in the control group but in *villages eligible for future transfers*; intuitively this is the "added" effect of transfer receipt, above and beyond the effect of (only) eligibility.

With β_s being the impact of crossing only the eligibility threshold, and β_r being the impact of additionally crossing the size threshold among the eligible villages, their *sum*, $\beta_s + \beta_r$, is the impact of crossing both thresholds, i.e. of receiving a transfer pre-election.

Importantly, when Y is incumbent vote share, it can be shown that β_s and $\beta_s + \beta_r$ are in fact structurally derived from the theoretical model in Section 3. In **Appendix C**, I show formally that β_s is equivalent to the (localized) impact of eligibility for future transfers on incumbent vote share in the model, and that $\beta_s + \beta_r$ is equivalent to the (localized) impact of receipt of prior transfer receipt in the model. When Y is village turnout instead of incumbent vote share, the model is no longer linked explicitly to the estimators, but it remains econometrically true that β_s is the effect of only crossing the eligibility threshold while β_r is the effect of additionally crossing the size threshold, so that $\beta_r + \beta_r$ is the impact of crossing both thresholds, on the outcome of interest (**Appendix C**).

[30] I show in Section 6.3 that the results are robust to allowing the crossing of the size cutoff (alone) to have an effect on vote share.

5.3 Design assessment

To assess the research design, I first explore differences between, and discontinuities in, predetermined covariates around the threshold(s). **Table 1** reports the simple difference of means in key characteristics between pre-election recipients and all other villages in Uttar Pradesh, first for all villages, then by decreasing bandwidth around the 50% share cutoff, and finally by a narrow Scheduled Caste share and population size bandwidth.

The characteristics considered are share of Scheduled Tribes (a distinct marginalized socioeconomic segment), percent of population which is literate, which works, and which is involved in "marginal" work (defined as employment under six months per year), all as reported by the 2011 Census. I also consider estimates of the following variables in 2011 at the village level, obtained from the SHRUG datasets: annual per capita consumption (in hundreds of Indian Rupees), whether electric power is available in the village for domestic use and for agricultural use, and whether the village has no drainage system.[31] Note that it is not possible to include prior (2014) election outcomes here, precisely because the village-booth matching was performed for a bandwidth and not for all villages in the state, but I check for regression discontinuities in these variables around the bandwidth in Section 6.

[31] Consumption estimates are obtained from the SHRUG SECC dataset while data on rural electrification and drainage are obtained from the SHRUG Population Census.

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
Consumption pc ('00 INR)	1.7	1.4	−1.3	0.08
No drainage (y/n)	−0.02	0.00	0.01	0.04
Power for domestic use (y/n)	0.01	−0.01	−0.02	−0.01
Power for agricultural use (y/n)	0.02	−0.01	−0.04	−0.02
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 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.[32] This additional restriction does not add much baseline similarity among villages, while cutting the number of observations by more than half. This suggests that, once *share* is taken into account, the absolute number of Scheduled Caste persons makes little difference to key village characteristics.

Table 2 follows a similar approach, comparing villages eligible for future (post-election) funding with all other villages in Uttar Pradesh. Once more, restricting the bandwidth around the share is sufficient for generating similarity in observables among villages.

[32]This was chosen as it reduces the sample size by not much more than half.

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	0.00	0.00
Marginal work population (%)	0.01^{***}	0.00	0.00	0.00
Consumption pc ('00 INR)	-1.7^{***}	-1.8^{***}	-1.5	-1.8
No drainage (y/n)	-0.02^{***}	0.00	0.00	0.02
Power for domestic use (y/n)	0.02^{***}	0.00^{*}	0.00	-0.01
Power for agricultural use (y/n)	0.05^{***}	0.02^{***}	0.01	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 θ_1 from the reduced form RDD in Eq. (1). The remaining columns show the estimates for β_s and β_r from the multi-score RDD in Eq. (3), when restricting the bandwidth around the Scheduled Caste share and when adding a Scheduled Caste population size restriction.[33] Almost none of the specifications predict a jump in these variables.

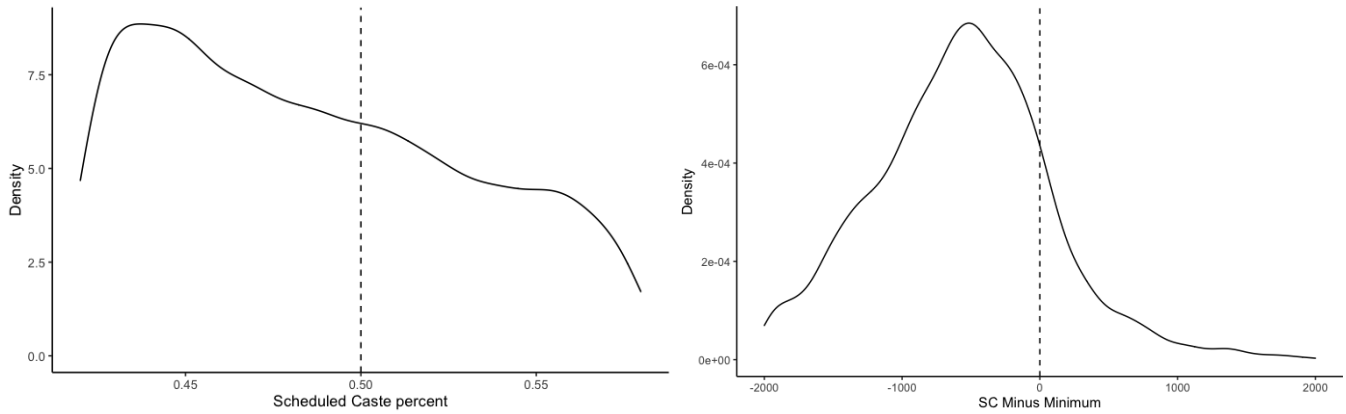
[33]Note that the number of observations falls here, relative to the tables simply comparing means. This is because the exact regression specification involves parliamentary constituency, so it is necessary to use observations with useful booth links. This also generates full comparability with the results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

Table 3: Testing for discontinuities in village characteristics

	Reduced form	*Multi-score*			
	+/-5%	+/-5%		Dual bandwidth	
Coefficient	θ_1	β_s	β_r	β_s	β_r
ST share	0.0005	0.0003	0.001	0.001	0.004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Literacy	−0.003	−0.002	−0.002	−0.002	0.006
	(0.005)	(0.005)	(0.005)	(0.007)	(0.006)
Working population	0.011*	0.012*	−0.006	0.013	−0.012
	(0.006)	(0.006)	(0.006)	(0.009)	(0.008)
Marginal work population	0.001	−0.001	0.008	−0.001	0.006
	(0.007)	(0.007)	(0.007)	(0.010)	(0.009)
Consumption pc	−1.17	−1.52	0.74	−5.18*	2.0
	(2.2)	(2.3)	(1.9)	(2.9)	(2.6)
No drainage	0.049	0.041	0.037	0.042	0.052
	(0.036)	(0.037)	(0.039)	(0.053)	(0.050)
Power for domestic use	−0.010	−0.010	−0.002	−0.022	0.001
	(0.015)	(0.015)	(0.016)	(0.023)	(0.020)
Power for agricultural use	0.019	0.019	0.007	0.008	0.011
	(0.024)	(0.024)	(0.025)	(0.035)	(0.034)
Observations	3,034	3,034	3,034	1,498	1,498

Table 3 presents the results of Eq. (1) (Column 1) and of Eq. (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. Note: *p<0.1; **p<0.05; ***p<0.01.

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



(a) Scheduled Caste share

(b) Scheduled Caste population

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

Specifically, for the Uttar Pradesh villages with 42%-58% Scheduled Caste share which I was able to generate useful booth links for ($N = 4837$), there is a clear shift toward 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 the state in general increased by 8.9 pct points from 2014 to 2019 (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.

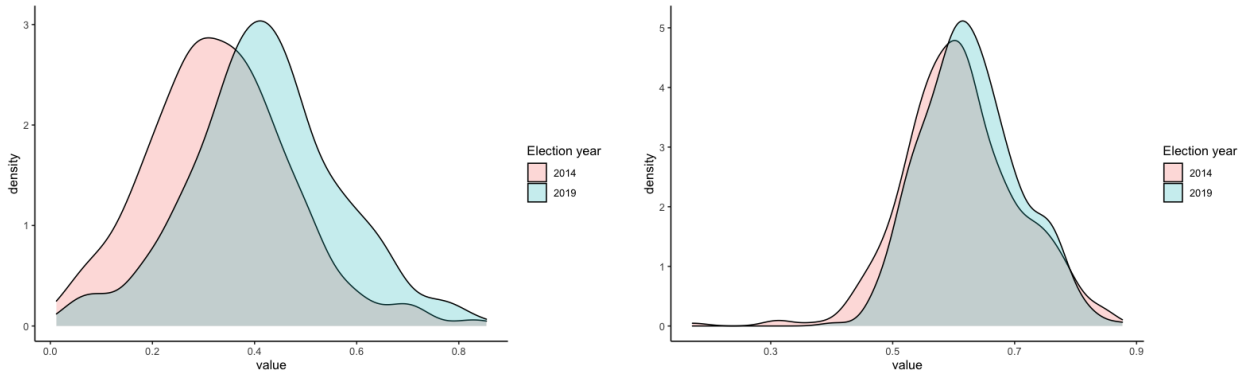
To anticipate the analytical results, **Figure 6** illustrates the distribution of vote shares for the BJP (as well as total turnout) for the linked villages ($N = 4837$), in 2014 and 2019, grouping villages by transfer receipt status. It plots only villages which received transfers pre-election in the 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 anticipate future transfers. The next subsections confirm these results analytically, by employing the RD designs of Section 5.

6.2 Main results

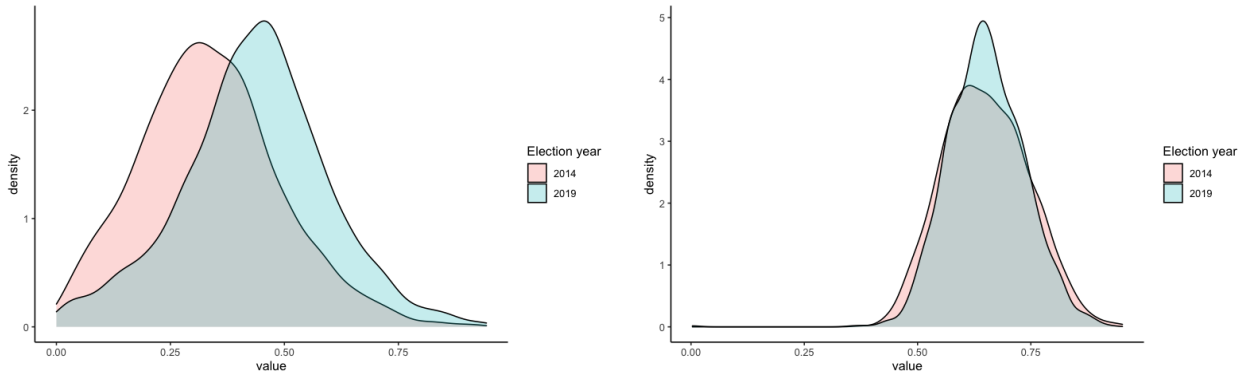
Beginning with the reduced form RDD specification in Eq. (1), the results are displayed in **Table 4** and illustrated in **Figure 7**. In all tables, the standard errors used are robust and clustered by district, and the 95% confidence interval is noted below the coefficient.

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.



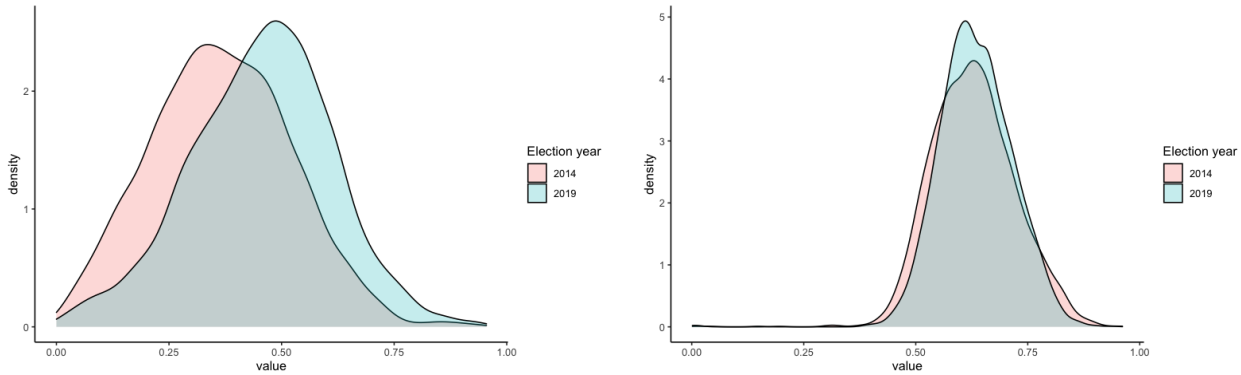
(a) BJP vote share, pre-election recipients

(b) Turnout, pre-election recipients



(c) BJP vote share, future recipients

(d) Turnout, future recipients



(e) BJP vote share, ineligible villages

(f) Turnout, ineligible villages

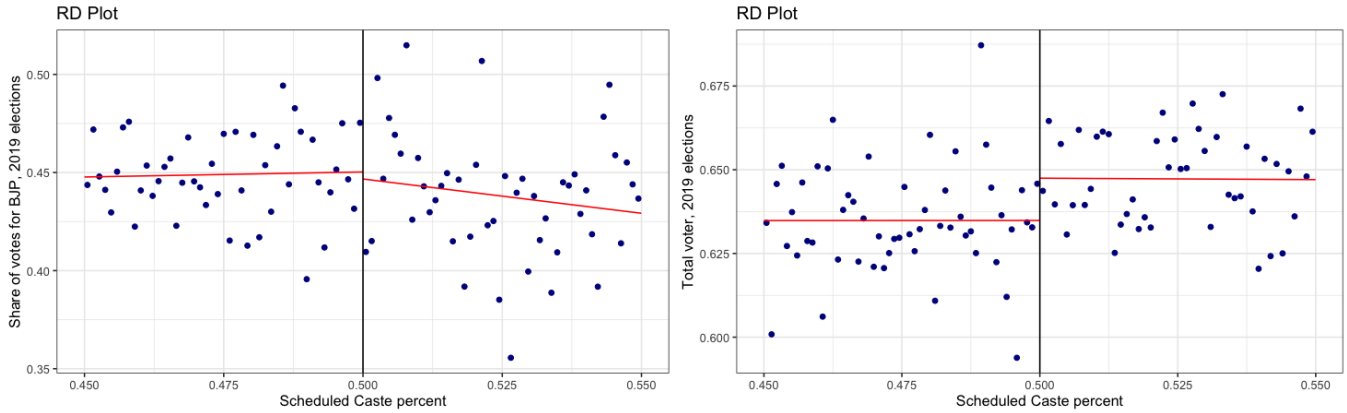
Figure 6: Village outcomes by transfer status

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.

Table 4: Reduced form RDD

	Dependent variable:	
	Vote share for BJP	Turnout
	(1)	(2)
Eligible	−0.0002	0.012**
	(−0.020, 0.020)	(0.002, 0.022)
PC & District controls	Yes	Yes
Observations	3,034	3,034
R^2	0.255	0.402
Adjusted R^2	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. (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.



(a) BJP vote share

(b) Turnout

Figure 7: Reduced form RDD

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

Next, to disentangle the effects of treatment by transfer receipt status, **Table 5** presents the results of the multi-score sharp RDD in Eq. (3), for share as well as dual share-and-size bandwidth restrictions.[34] The coefficient on "Eligible" is β_s while the coefficient on "Pre-election recipient" is β_r. As explained, 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

	Dependent variable:			
	Vote share for BJP		Turnout	
	(1)	(2)	(3)	(4)
Eligible	−0.001	0.016	0.014***	0.005
	(−0.023, 0.020)	(−0.014, 0.046)	(0.004, 0.024)	(−0.009, 0.020)
Pre-election recipient	0.0005	−0.004	−0.010**	0.002
	(−0.020, 0.021)	(−0.031, 0.023)	(−0.020, −0.001)	(−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^2	0.254	0.283	0.432	0.424
Adjusted R^2	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. (3), with β_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

[34] 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.[35]

As outlined in Section 5, our estimators of interest are β_s and $\beta_s + \beta_r$. Note that **Table 5** provides a formal test for the null $\beta_s = 0$ (and for $\beta_r = 0$), and only informally suggests that $\beta_s + \beta_r = 0$ in all columns. To formally examine the latter, note that $\beta_s + \beta_r = 0$ would imply that $\beta_r = -\beta_s$. The corresponding restricted version of Eq. (3) becomes $Y_i = \beta_0 + f\Big((SC_{i,s} - 0.5), (SC_{i,p} - c)\Big) + \tilde{\beta}(Z_{i,s} - D_i) + \beta_{pc}PC_i + e_i$, where $\tilde{\beta} = \beta_s = -\beta_r$.[36] In **Table D1** in **Appendix D**, 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 D2** 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 D**. Regarding functional form, **Tables D3** and **D4** 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 D5** shows that nearly identical results are obtained for the multi-score RDD when allowing

[35] The significance disappears with the smaller sample size after the dual restriction.

[36] As in the unrestricted model, I set $\beta_p = 0$.

$\beta_p \neq 0$, so that moving just above the size threshold can also have an effect on outcomes.[37] Although the results do not rely on controls besides parliamentary constituency and district, **Table D6** demonstrates the robustness of findings to including village-level characteristics as covariates and applying a covariate-adjusted RDD along the lines of Calonico et al (2019).

Regarding bandwidth, for the reduced form and multi-score RDDs **Table D7** and **Table D8** show results using a 7%, 6%, and 4% share bandwidth, as well as using a 5% share bandwith combined with a population size restriction of +/-700 and +/-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 D9 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.[38] 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 D10** presents the results from using lagged

[37] As I show in **Appendix C**, if $\beta_p \neq 0$, β_s still estimates the impact of eligibility for future transfers. However, now the sum $\beta_s + \beta_p + \beta_r$ would measure the impact of prior receipt. The columns suggest that this sum is not different from zero in all specifications.

[38] 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.

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.[39] Therefore, **Table D10** 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).

7 Discussion

7.1 Interpretation of results

On the central outcome of interest, incumbent vote share, transfers had no impact 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 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 this is improbable and that the findings likely reflect villagers' preferences and expectations, and connect the discussion to the model in Section 3.

[39]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).

Before delving into mechanisms, however, it should be noted that the results identify 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 ruled out 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.[40] Nevertheless, to the extent that transfer eligibility and receipt are themselves of interest, the results indicate 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 (**Appendix A**).

Regarding villager awareness, it is doubtful that the results on vote share owe to lack of knowledge, most importantly because it appears the BJP has engaged in heavy publicity efforts around PMAGY in the target villages.[41] On the official website for the program,

[40] 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, e.g. 20% versus 80%, would be necessary to generate treatment variation. Of course, the problem is this introduces selection issues which undercut identification.

[41] Here it should be noted that even if the BJP campaigned about the program at large (for which there is less direct evidence), this could (also) cement a social loyalty shift across the board as noted earlier, but it does not inhibit effects of transfer receipt or eligibility which the paper studies. What matters is that

there are dozens of sample pictures of BJP officials gathering villagers in eligible 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 (Upadhyay and Upadhyay, 2020). As secondary points, note that (i) the effect on vote share is null also among past recipients where zero knowledge is even less plausible than for future recipients, especially amid PR efforts before the elections, and (ii) the effect picked up on turnout suggests that at least *some* future recipients were aware of the transfers.

Another information issue arises if villagers inaccurately credit non-BJP parties, for example through attributing the transfers to the local government's party instead of the federal government's party. Note that this would occur if, along with inaccurate attribution, the majority of villages had non-BJP local governments. In this case 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) and it would even go towards negative as more villages shift votes from the incumbent to competing parties inaccurately receiving credit.

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 D11** shows, the results hold in this subset too.[42]

targeted villages understood they were recipients or eligible.

[42] A distinct possibility is that villagers (only) reward parties in local and *not* national elections. This is unlikely as increasing centralization of politics, and differentiation between national and the regional politics, has characterized the reign of the BJP (Aiyar and Sircar, 2020). Nonetheless, this can be explored in future research with data on local legislative election results and therefore additional with village-booth linking.

Yet another attribution problem would arise if villagers accurately understand this is national distribution but credit another party - not the incumbent - for the program, but this too is unsupported by the data. In the case of PMAGY, although the Congress party first suggested its outlines in 2009, it never implemented the program nor generated any significant lasting publicity around it. **Table D12** demonstrates that neither received nor promised transfers impacted the Congress party's vote shares in the 2019 elections, which were miniscule in Uttar Pradesh villages generally, where the party is unpopular.

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 suggest the amount is not trivial. The median target village has about 1,300 people, so that the transfer (in 2018) was equivalent to \$57-\$70 for a family of 5-6 people; this is close to the estimated average monthly income of target households at that time.[43] This amount is also comparable to other major programmmatic transfers in rural India.[44]

To further explore the role of transfer amount, I consider whether results change by village poverty levels. Intuitively, if transfer size is responsible for generating null results, point estimates may be higher and significant for more impoverished villages as they derive greater utility from the (fixed) transfer amount due to declining marginal utility of transfers. **Table D13** restricts the regressions to villages at or below median consumption per capita,

[43]There is no information on average income by village, but 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.

[44]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.

while **Table D14** restricts them to villages at or below median poverty level. In both cases, the main results are robust, suggesting that transfer size is not key to the attenuated effects.[45]

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?

Focusing fist on future recipients, then as outlined by the theoretical framework, a null treatment effect could arise from villagers expecting future transfers to be equal regardless of who wins, or from villagers being ideologically rigid, or a combination of the two (see **Appendix A**). There is reason to expect that at least the first mechanism - equivalent expectations about persistence of the program - is relevant in this context. As outlined in Section 2, Scheduled Castes are a core constituency and a range of parties have tried to woo them. The Congress Party, historically the BJP's main national competitor, first conceptualized 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, 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 does not demonstrate predictable allegiance to one specific party (Misra, 2020).

For these reasons, competition for Scheduled Caste votes is particularly fierce, and 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

[45]Note that it is not possible to consider heterogeneity by village *size* without introducing selection problems, because size was used in the program receipt criteria. More concretely, *for villages below median size* prior recipients are systematically different from other villages. They came from districts where the ten largest villages (in absolute SC numbers) are relatively small, whereas the other recipients come from districts where the ten largest villages are relatively large. It can be shown that these translate into differences in key village characteristics along the lines of design assessment in Section 5.3, even with bandwidth restrictions.

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, future recipients perceive economic expediency by party to be equivalent, so that transfer promises are not a differentiation point for the incumbent. 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: the treatment effect is null 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.[46]

Note as well that, for recipients, the funds were received only a few months prior to elections, such that although funding had been received, no projects (funded by the program) had yet started. According to the official PMAGY website, tangible projects began underway in summer of 2019 after an assessment of which were most needed in the village, therefore after elections ended. We can rule out that villagers did not reward the incumbent due to disappointment with end outcomes, as end outcomes had not materialized due to an arguably short funding-elections window. Of course, it is possible that at the outset villagers *expected* the transfers to lead nowhere in terms of actual improvements in their livelihood, although this is difficult to reconcile with the BJP's already high popularity and credibility

[46]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.

in these villages. Future research using the upcoming (not yet available) Indian Census can investigate whether the transfers, in combinations with concrete end outcomes at the village level, have impacted the voting behavior of recipients in subsequent elections.

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 village-level election evidence on whether transfers shifted target groups into the BJP's base, focusing on programmatic distribution to disadvantaged castes and on 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 Model

A.1 Setup

Let there be three types of villages, with village type $v \in \{a, b, c\}$, and consider a 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 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 is a 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. 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. 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 India and among poor people (Bardhan, 2008).

To see how people vote, let all individuals 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. Although individuals can be different in their affinities, each village type v shares a *distribution* of affinities Φ_v, where $\Phi_v(X) \in [0, 1]$ describes a cumulative distribution function. Therefore, the value $\Phi_v(0)$ is the share of people in village type v who have affinities to the left of 0 ($X_{iv} < 0$) and thus prefer party R; 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 people to discount pledges by the non-incumbent by $\gamma \in [0, 1]$, imbuing a possible incumbency advantage. I also allow for 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:

$$U_v(T_v^R) - (1 - \gamma)U_v(T_v^L) > X_{iv} \tag{A.1}$$

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 below). Importantly, note that only *future* (post-election) transfers factor into the left hand side in Eq. (A.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.[47]

A.2 Treatment effects

To model the impact of future transfers on voting behavior, consider the relevant b villages. Following Eq. (A.1), a person i in those villages will vote for R if

$$U_b(T_b^R) - (1-\gamma)U_b(T_b^L) > X_{ib} \tag{A.2}$$

Denote the cutoff ideological preference which equals the left hand side of Eq. (A.2) as X_b^*, so that $X_b^* \equiv U_b(T_b^R) - (1-\gamma)U_b(T_b^L) \lesseqgtr 0$. Any individual with $X_{ib} < X_b^*$ will vote for R, so that the vote share for R in b villages will be

$$\Phi_b(X_b^*) = \Phi_b\Big(U_b(T_b^R) - (1-\gamma)U_b(T_b^L)\Big) \tag{A.3}$$

By contrast, an individual i in a c village faces $T = 0$ and so will vote R only if

$$0 > X_{ic} \tag{A.4}$$

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

$$\Phi_c(X_c^*) = \Phi_c(0) \tag{A.5}$$

[47]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.

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

$$\begin{aligned}\Delta_b &= \Phi_b(X_b^*) - \Phi_c(X_c^*) \\ &= \Phi_b(X_b^*) - \Phi_c(0)\end{aligned} \tag{A.6}$$

The net difference Δ_b is composed of the difference between the distribution function Φ, reflecting difference baseline preferences for the parties, and 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 Δ_b. 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:

$$\begin{aligned}\Delta_b &= \Phi(X_b^*) - \Phi(X_c^*) \\ &= \Phi(X_b^*) - \Phi(0)\end{aligned} \tag{A.7}$$

Since Φ 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). $\Delta_b = 0$ 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 $X_b^* \to 0$ 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.

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 $\mathscr{R}_a > 0$ from the right hand side of Eq. (A.1). Therefore, individual i in a will vote for the incumbent R if

$$0 > X_{ia} - \mathscr{R}_a \quad ; \quad \mathscr{R}_a \geq 0 \tag{A.8}$$

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

$$\Phi_a(X_a^*) = \Phi_a(\mathscr{R}_a) \tag{A.9}$$

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

$$\begin{aligned} \Delta_a &= \Phi(X_a^*) - \Phi(X_c^*) \\ &= \Phi(\mathscr{R}_a) - \Phi(0) \end{aligned} \tag{A.10}$$

Given that Φ is nondecreasing, then $\Delta_a > 0$ would imply $\mathscr{R}_a > 0$. In contrast, $\Delta_a = 0$ would imply $\mathscr{R}_a = 0$ so that no such "loyalty" effect is created among past recipients from the program's transfers under R's incumbency.

Finally, a number of issues may drive both treatment effects to zero. One is lack of awareness about the transfers, which can be integrated with a simple information parameter which multiplies $U_b(T_b^R) - (1-\gamma)U_b(T_b^L)$, or $\mathscr{R}_a$, as the share of "aware" villagers. Treatment effects would be zero if the information parameter is zero. Another is 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. This would cause the sign of the effect to

flip. A third is transfers which are too small and render $T_b^R \to 0$; if the same is expected of the competing party, then they exert no impact on voting behavior of future recipients. If the loyalty parameter $\mathscr{R}_a$ is increasing in the transfer amount, the effect of small amounts approaches zero for past recipients as well.

A.3 General shock

Consider a general shock $\mathscr{R} \lesseqgtr 0$ prior to t_1 which impacts *all* villages regardless of type. Rewrite Eq. (A.1) as $U_v(T_v^R) - (1-\gamma)U_v(T_v^L) > X_{iv} - \mathscr{R} - \mathscr{R}_a$, where $\mathscr{R}_a = 0$ for $v \in \{b, c\}$ by definition and $\mathscr{R}_a \geq 0$ for a. $\mathscr{R} > 0$ would be a general shock which increases loyalty for the incumbent R while $\mathscr{R} < 0$ would be a shock that increases loyalty for the opposition L.

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

$$\begin{aligned} \tilde{\Delta}_v &= \Phi_{v,t_1} - \Phi_{v,t_0} \\ &= \Phi_v(X_v^* + \mathscr{R}) - \Phi_v(0) \end{aligned} \tag{A.11}$$

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 $\mathscr{R}$. Further, suppose an estimator can generate baseline similarity among village types, so that $\Phi_v = \Phi$. Then, by Eq. (A.11), we would see the *same* shift in vote share $\tilde{\Delta} = \Phi(\mathscr{R}) - \Phi(0)$ within all village types between t_1 and t_0. Given that Φ 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.

B 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.[48]

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

[48] 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.

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 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.[49] *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

[49]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.

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 Hintson and Vaishnav (2021) for checks on their algorithm - and which by comparison include numerous inconsistencies.[50]

C 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 $\mathbf{S}$ is therefore (net of any other variables that can affect Y):

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

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

$$\begin{aligned} E[Y|\mathbf{S}] = E[Y^{00}|\mathbf{S}] &+ \Big(E[Y^{10}|\mathbf{S}] - E[Y^{00}|\mathbf{S}]\Big)Z_s + \Big(E[Y^{01}|\mathbf{S}] - E[Y^{00}|\mathbf{S}]\Big)Z_p \\ &+ \Big(\big(E[Y^{11}|\mathbf{S}] - E[Y^{10}|\mathbf{S}]\big) - \big(E[Y^{01}|\mathbf{S}] - E[Y^{00}|\mathbf{S}]\big)\Big)Z_s * Z_p \end{aligned} \tag{C.2}$$

[50]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.

Consider the regression form:

$$Y = \beta_0 + \beta_1 Z_s + \beta_2 Z_p + \beta_3 Z_s * Z_p + \epsilon \tag{C.3}$$

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

$$\beta_1 = E[Y^{10}|\mathbf{S}] - E[Y^{00}|\mathbf{S}] \tag{C.4}$$

$$\beta_3 = \big(E[Y^{11}|\mathbf{S}] - E[Y^{10}|\mathbf{S}]\big) - \big(E[Y^{01}|\mathbf{S}] - E[Y^{00}|\mathbf{S}]\big) \tag{C.5}$$

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. (C.4) and (C.5) can be rewritten respectively as:

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

$$\beta_r = \Big(lim_{SC_s \to 0.5^+, SC_p \to c^+} E[Y|\mathbf{S}] - lim_{SC_s \to 0.5^+, SC_p \to c^-} E[Y|\mathbf{S}]\Big) - \Big(lim_{SC_s \to 0.5^-, SC_p \to c^+} E[Y|\mathbf{S}] - lim_{SC_s \to 0.5^-, SC_p \to c^-} E[Y|\mathbf{S}]\Big)$$

Finally, to see the relationship between these estimators and the theoretical model when Y is incumbent vote share, note that β_s in Eq. (4) expresses the difference in vote share from just crossing the eligibility threshold while being just below the size cutoff (i.e. b type

villages in **Appendix A**) versus just being under both thresholds ("small" c type villages). 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 the definition of Δ_b in Eq. (A.7), so that β_s is the local estimator of Δ_b:

$$E[\Delta_b|\mathbf{S}] = \beta_s \tag{C.6}$$

For β_r in Eq. (5), the second parenthesis can be assumed to be zero, as it measures the treatment effect of moving just above the size threshold for ineligible villages. Focusing on the first parenthesis then, this is equivalent to $\Phi_a(X_a^*) - \Phi_b(X_b^*)$ around the cutoff, i.e. a comparison of pre-treatment recipients to future recipients. Hence β_r can be written as:

$$\begin{aligned}
\beta_r &= E[\Phi_a(X_a^*) - \Phi_b(X_b^*)|\mathbf{S}] \\
&= E[\Phi_a(X_a^*) - \Phi_b(X_b^*) - \Phi_c(X_c^*) + \Phi_c(X_c^*)|\mathbf{S}] \\
&\approx E\Big[\Phi_a(X_a^*) - \Phi_c(X_c^*)|\mathbf{S}\Big] - E\Big[\Phi_b(X_b^*) - \Phi_c(X_c^*)|\mathbf{S}\Big] \\
&= E\Big[\Phi(X_a^*) - \Phi(X_c^*)|\mathbf{S}\Big] - E\Big[\Phi(X_b^*) - \Phi(X_c^*)|\mathbf{S}\Big] \\
&= E[\Delta_a|\mathbf{S}] - E[\Delta_b|\mathbf{S}]
\end{aligned} \tag{C.7}$$

where the bandwidth restriction generates $\Phi_a = \Phi_b = \Phi_c = \Phi$. Combining Eqs. (C.6) and (C.7), we obtain:

$$E[\Delta_a|\mathbf{S}] = \beta_s + \beta_r \tag{C.8}$$

D Additional results

Table D1: MRDD with coefficient restriction $\beta_s = -\beta_r$

	Dependent variable:			
	Vote share for BJP		Turnout	
	(1)	(2)	(3)	(4)
I(Eligible - recipient)	−0.001	0.010	0.012***	0.001
	(−0.018, 0.016)	(−0.014, 0.033)	(0.004, 0.020)	(−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^2	0.254	0.283	0.432	0.424
Adjusted R^2	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 D1 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 D2: 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 D2 reports the results of ANOVA tests between the unrestricted MRDD in Table 5 and the restricted MRDD in Table D1, 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 D3: Reduced form with quadratic specification

	Dependent variable:	
	Vote share for BJP	Turnout
	(1)	(2)
Eligible	0.008	0.014*
	(−0.023, 0.040)	(−0.001, 0.030)
PC & District controls	Yes	Yes
Observations	3,034	3,034
R^2	0.255	0.402
Adjusted R^2	0.222	0.376
Residual Std. Error (df = 2904)	0.141	0.065
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table D3 reports the results of the reduced form RDD with a quadratic specification for the centered running variable.

Table D4: MRDD with quadratic specification

	Dependent variable:			
	Vote share for BJP		Turnout	
	(1)	(2)	(3)	(4)
Eligible	0.00001	0.016	0.014***	0.005
	(−0.021, 0.021)	(−0.015, 0.046)	(0.004, 0.024)	(−0.009, 0.019)
Pre-election recipient	−0.0001	−0.005	−0.010**	0.001
	(−0.021, 0.021)	(−0.032, 0.022)	(−0.019, −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^2	0.254	0.283	0.436	0.427
Adjusted R^2	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 D4 reports the results of the multi-score RDD with a quadratic specification for the centered running variables.

Table D5: MRDD with $\beta_p \neq 0$

	Dependent variable:			
	Vote share for BJP		Turnout	
	(1)	(2)	(3)	(4)
Eligible	−0.0004	0.018	0.013**	0.005
	(−0.022, 0.021)	(−0.013, 0.048)	(0.003, 0.024)	(−0.009, 0.020)
Above size	0.012	0.021	−0.007	−0.0004
	(−0.007, 0.031)	(−0.008, 0.051)	(−0.016, 0.002)	(−0.014, 0.013)
Pre-election recipient	−0.007	−0.015	−0.006	0.002
	(−0.031, 0.016)	(−0.045, 0.015)	(−0.016, 0.005)	(−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^2	0.254	0.284	0.432	0.424
Adjusted R^2	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 D5 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 D6: Reduced form with covariates and covariate-adjusted local polynomial design

	Dependent variable:	
	Vote share for BJP	Turnout
	(1)	(2)
Eligible (conventional)	0.006	0.010
	(−0.019 , 0.031)	(−0.002 , 0.023)
Eligible (bias-corrected)	0.003	0.011*
	(−0.022 , 0.028)	(−0.001 , 0.024)
Eligible (robust)	0.003	0.011
	(−0.027, 0.033)	(−0.004 , 0.026)
PC, District, and village characteristics controls	Yes	Yes
Observations	1,744	1,804

Note: *p<0.1; **p<0.05; ***p<0.01

Table D6 reports the results of the reduced form RDD with additional covariates and a covariate-adjusted local polynomial regression along the lines of Calonico et al (2019), using the *rdrobust* command in R. The village-level covariates are literacy rate, percent of the population working, percent of the population working in a main job, and a lagged dependent variable (2014 BJP vote share and 2014 turnout, respectively).

Table D7: Reduced form with different bandwidths

	PANEL A. Dependent variable: Vote share BJP				
	(1)	(2)	(3)	(4)	(5)
Eligible	0.008	0.006	0.003	0.009	0.021
	(−0.009, 0.025)	(−0.012, 0.025)	(−0.020, 0.026)	(−0.017, 0.036)	(−0.009, 0.051)
Observations	4,372	3,727	2,420	1,704	1,264
R^2	0.234	0.237	0.256	0.274	0.301
Adjusted R^2	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)
	PANEL B. Dependent variable: Turnout				
	(1)	(2)	(3)	(4)	(5)
Eligible	0.010**	0.012**	0.011*	0.008	−0.013
	(0.002, 0.019)	(0.003, 0.021)	(−0.00001, 0.023)	(−0.004, 0.021)	(−0.011, 0.020)
Observations	4,372	3,727	2,420	1,704	1,264
R^2	0.389	0.397	0.412	0.423	0.414
Adjusted R^2	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<0.1; **p<0.05; ***p<0.01

Table D7 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 D8: MRDD with different bandwidths

	PANEL A. Dependent variable: Vote share BJP				
	(1)	(2)	(3)	(4)	(5)
Eligible	0.008	0.005	0.003	0.009	0.020
	(−0.010, 0.026)	(−0.014, 0.025)	(−0.021, 0.027)	(−0.019, 0.038)	(−0.013, 0.053)
Pre-election recipient	0.001	0.002	0.001	−0.001	−0.002
	(−0.016, 0.019)	(−0.017, 0.021)	(−0.021, 0.024)	(−0.026, 0.025)	(−0.031, 0.027)
Observations	4,372	3,727	2,420	1,704	1,264
R^2	0.237	0.237	0.256	0.273	0.304
Adjusted R^2	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)
	PANEL B. Dependent variable: Turnout				
	(1)	(2)	(3)	(4)	(5)
Eligible	0.012***	0.014***	0.014**	0.008	0.007
	(0.003, 0.020)	(0.004, 0.023)	(0.002, 0.026)	(−0.005, 0.022)	(−0.011, 0.023)
Pre-election recipient	−0.005	−0.007*	−0.012**	0.001	−0.002
	(−0.013, 0.003)	(−0.015, 0.001)	(−0.022, −0.002)	(−0.010, 0.012)	(−0.015, 0.010)
Observations	4,372	3,727	2,420	1,704	1,264
R^2	0.421	0.432	0.444	0.443	0.426
Adjusted R^2	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<0.1; **p<0.05; ***p<0.01

Table D8 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 D9: Specifications with change between 2014 and 2019 as outcome

	PANEL A. Reduced form RDD	
Outcome	Change in vote share BJP	Change in turnout
	(1)	(2)
Eligible	0.007	0.008*
	(−0.008, 0.023)	(−0.001, 0.017)
Observations	2,841	2,841
R^2	0.343	0.178
Adjusted R^2	0.313	0.140
Residual Std. Error (df = 2713)	0.106	0.057
	PANEL B. Multi-score RDD	
Outcome	Change in vote share BJP	Change in turnout
	(1)	(2)
Eligible	0.006	0.008*
	(−0.010, 0.022)	(−0.002, 0.018)
Pre-election recipient	0.005	−0.002
	(−0.010, 0.020)	(−0.012, 0.008)
Observations	2,841	2,841
R^2	0.344	0.186
Adjusted R^2	0.313	0.186
Residual Std. Error (df = 2712)	0.106	0.057
Note:		*p<0.1; **p<0.05; ***p<0.01

Table D9 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 D10: Specifications with 2014 outcomes (placebo)

	PANEL A. Reduced form RDD	
Outcome	Past vote share BJP	Past turnout
	(1)	(2)
Eligible	−0.009	0.004
	(−0.029, 0.010)	(−0.007, 0.015)
Observations	2,841	2,841
R^2	0.272	0.428
Adjusted R^2	0.238	0.402
Residual Std. Error (df = 2713)	0.135	0.073
	PANEL B. Multi-score RDD	
Outcome	Past vote share BJP	Past turnout
	(1)	(2)
Eligible	−0.009	0.006
	(−0.030, 0.012)	(−0.005, 0.016)
Pre-election recipient	−0.005	−0.007
	(−0.025, 0.016)	(−0.020, 0.006)
Observations	2,841	2,841
R^2	0.272	0.475
Adjusted R^2	0.238	0.450
Residual Std. Error (df = 2712)	0.135	0.070
Note:		*p<0.1; **p<0.05; ***p<0.01

Table D10 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 D11: Subset of villages where BJP was in power locally

	PANEL A. Reduced form RDD	
Outcome	Vote share for BJP	Turnout
	(1)	(2)
Eligible	0.008	0.008
	(−0.014, 0.030)	(−0.003, 0.019)
Observations	2,379	2,379
R^2	0.245	0.394
Adjusted R^2	0.205	0.362
Residual Std. Error (df = 2258)	0.140	0.065
	PANEL B. Multi-score RDD	
Outcome	Vote share for BJP	Turnout
	(1)	(2)
Eligible	0.004	0.011*
	(−0.019, 0.028)	(−0.001, 0.023)
Pre-election recipient	0.008	−0.014***
	(−0.015, 0.030)	(−0.025, −0.004)
Observations	2,379	2,379
R^2	0.244	0.427
Adjusted R^2	0.204	0.397
Residual Std. Error (df = 2257)	0.140	0.063
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table D11 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.

Table D12: Specifications with Congress share as outcome

	Dependent variable:	
	INC Vote share, Reduced	INC Vote share, MRDD
	(1)	(2)
Eligible	−0.001	−0.001
	(−0.010, 0.008)	(−0.010, 0.009)
Pre-election recipient		−0.001
		(−0.010, 0.007)
PC & District controls	Yes	Yes
Observations	3,034	3,034
R^2	0.792	0.792
Adjusted R^2	0.783	0.783
Residual Std. Error (df = 2906/5)	0.061	0.061
Note:		*p<0.1; **p<0.05; ***p<0.01

Table D12 reports the results of the reduced form RDD and MRDD where the dependent variable is the vote share of the Indian Congress Party in the 2019 elections.

Table D13: Subset of villages at or below median consumption per capita

	PANEL A. Reduced form RDD	
Outcome	Vote share BJP	Turnout
	(1)	(2)
Eligible	−0.010	0.009
	(−0.039, 0.018)	(−0.006, 0.023)
Observations	1,462	1,462
R^2	0.332	0.369
Adjusted R^2	0.276	0.317
Residual Std. Error (df = 1349)	0.139	0.064
	PANEL B. Multi-score RDD	
Outcome	Vote share BJP	Turnout
	(1)	(2)
Eligible	−0.010	0.010
	(−0.040, 0.019)	(−0.004, 0.024)
Pre-election recipient	−0.007	−0.010
	(−0.036, 0.022)	(−0.023, 0.003)
Observations	1,462	1,462
R^2	0.330	0.398
Adjusted R^2	0.274	0.348
Residual Std. Error (df = 1348)	0.139	0.063
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table D13 reports the results of the reduced form RDD (Panel A) and the multi-score RDD (Panel B) when observations are restricted to villages in the 5% bandwidth at or below median per capita consumption (15,380 INR per annum) as estimated by SHRUG data. Note that $N = 1,462$ is slightly less than half of the observations used in the main regressions ($N = 3,034$) because a few villages are missing observations for per capita consumption.

Table D14: Subset of villages at or below median poverty rate

	PANEL A. Reduced form RDD	
Outcome	Vote share BJP	Turnout
	(1)	(2)
Eligible	0.005	0.014*
	(−0.025, 0.034)	(−0.001, 0.029)
Observations	1,462	1,462
R^2	0.276	0.453
Adjusted R^2	0.212	0.404
Residual Std. Error (df = 1341)	0.140	0.068
	PANEL B. Multi-score RDD	
Outcome	Vote share BJP	Turnout
	(1)	(2)
Eligible	0.002	0.018**
	(−0.029, 0.032)	(0.002, 0.034)
Pre-election recipient	0.014	−0.013*
	(−0.016, 0.044)	(−0.028, 0.001)
Observations	1,462	1,462
R^2	0.278	0.467
Adjusted R^2	0.212	0.429
Residual Std. Error (df = 1340)	0.140	0.066
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table D14 reports the results of the reduced form RDD (Panel A) and the multi-score RDD (Panel B) when observations are restricted to villages in the 5% bandwidth at or below median poverty rate in the sample (36% of the population living on 31 or less INR per day), as estimated by SHRUG data. Note that $N = 1,462$ is slightly less than half of the observations used in the main regressions ($N = 3,034$) because a few villages are missing observations for poverty rate.