Winning Support by Distributing Houses? Evidence from India

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Abstract

Can an expensive material benefit, delivered programmatically to voters outside the ruling party's ethnic core, win support for the benefit-giving party, and undercut the distributive salience of ethnicity? The literature says that material benefits can compensate for ethnic or ideological disutility, and that socioeconomic targeting can weaken beliefs about co-ethnic politicians being more likely to deliver benefits to the voter. I find that a large-scale, rural housing program in India generates support for the benefit-giving party among ethnically opposed voters and even those that do not receive the benefit. Beneficiaries feel gratitude, while non-beneficiaries report that many people like them have benefited from the program. There is no impact on the distributive salience of ethnicity. Beneficiaries recognize that the ruling party has done something for them, and are aware of the programmatic features of distribution. Yet, ethnic considerations predominantly shape distributive beliefs about politicians in a behavioral game. This finding has implications for ethnically diverse, developing democracies where programmatic competition is seen as an antidote to ethnic politics. Even an expensive benefit like a house, delivered programmatically, does little to reduce the distributive salience of ethnicity.

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Introduction

Parties sometimes distribute benefits to ethnically opposed voters with immediate and long term objectives. The immediate calculation is that a benefit can compensate for the voter's ethnic or ideological disutility, and help win their vote in an upcoming election (Lindbeck and Weibull 1987; Dixit and Londregan 1996; Stokes 2005). Typically, the assumption is that people should personally benefit for preferences to change (Bardhan et al. 2020; Heath and Tillin 2018). Parties can also have slightly long term considerations, such as building their reputation and clarifying their distributive intent to swing and weakly opposed voters. Where ethnic divisions are salient, we can think of swing or weakly opposed voters as those outside the party's ethnic core. These voters, for a variety of reasons, think that the party will not benefit them (Alesina, Baqir, and Easterly 1999; Alesina and LaFerrara 2005; Chandra 2004; Dunning and Nilekani 2013; Auerbach and Thachil 2018; Gulzar, Haas, and Pasquale Forthcoming; Kramon and Posner 2016; Posner 2005; Habyarimana et al. 2009; Miguel and Gugerty 2005; Chandrasekhar, Kinnan, and Larreguy 2018). Merely canvassing them can even backfire, and strengthen ethnic considerations (Arriola et al. 2020). However, material benefits delivered through party cadres and brokers can have some impact (Thachil 2014; Gadjanova 2021). Can an expensive material benefit, delivered programmatically, change preferences and weaken the distributive salience of ethnicity?

I study the impact of a large-scale rural housing program in India. The program provides land and money (approximately $2000) to the poorest families in the country to construct a two-room cement house. Recipients also get money for a toilet, a cooking gas connection, and a zero balance bank account. The typical receiving household lives in a *kutcha* (mud or bamboo) hut, and reports a monthly income of about $95. The benefit is about 21 times the household's monthly income.

The study focuses on low-caste Hindus (henceforth Dalits) who are outside the ruling party's ethnic core. Between April 2015 and December 2019, India's government built 8.8 million houses for the rural poor through this program. Of these, roughly 2.4 million houses went to Dalits, 2.1 million to tribals, and 0.98 million to religious minorities (principally, Muslims).[1] In effect, 62% of houses went to individuals from ethnic groups traditionally supportive of opposition parties and outside the ruling party's ethnic core. This distributive outreach by the ruling party coincides with the decline of ethnic parties in India, and the emergence of a hegemonic party seeking to expand its geographic footprint and build an oversized electoral coalition following Magaloni (2006)'s logic.

I focus on India's Bihar province, specifically three districts where Dalits are swing voters or weakly opposed to the ruling party. Conventionally, Muslims have strong ethnic reasons to oppose the current ruling party because of its Hindu majoritarian ideology and politically motivated violence against minorities (Wilkinson 2004; Nellis, Weaver, and Rosenzweig 2016). Dalits, on the other hand, are ethnically cross-pressured. As a subaltern group, they are opposed to the ruling party's elite ideology and do not benefit as much from its economic and social policies.[2] Thus, when status cleavages are salient, Dalits gravitate away from the ruling party. However, when religious cleavages are salient, Dalits are mobilized as Hindus by the ruling party. Historically, religious appeals have been on emotive issues and promise intangible benefits to the Hindu majority. Caste or status appeals overwhelmingly focus on distributive issues (Gupta 2005; Jaffrelot 2003; Jaffrelot and Kumar 2009). This is because ethnic quotas distribute resources and opportunities along status cleavages, pitting status group against each another for preferential access or a greater share of the pie (Lieberman and Singh 2012). The three districts that we study capture this variation in Dalits' ethnic position.

To identify the effect of an expensive material benefit, I employ a regression discontinuity design. The RD leverages an arbitrary cut-off separating those offered a house from those next in line to receive an offer. The estimand is the difference at the cut-point, or the effect of being offered a house. My research team interviewed 530 Dalit households. These

[1]Source: India's Ministry of Rural Development website on December 7, 2019

[2]Chapter 2 in Thachil (2014) explains how the ruling party's ideology, position on key issues and spending priorities have an elite bias and do not appeal to subaltern groups.

households were picked from the beneficiary list. The study was pre-registered with the Open Science Foundation.

I find that those offered a house (henceforth beneficiaries or treated subjects) were more likely to say the ruling party (BJP) has done something for them, more likely to think that some people voted for the BJP because they got a house, and displayed greater awareness about the programmatic features of distribution. Despite this, and contrary to expectation, I detect no difference at the cut-point for a variety of outcomes measuring support for the BJP. This includes how much respondents "like" the BJP, how receptive they are to its election message, and perceive its distributive intent, corruption record, competence and electoral invincibility.

There is very high support for the ruling party across the board, and evidence that communities are saturated with the benefit. 70 to 77% of respondents *personally* know someone who has received a house, typically between 9 and 16 such people. This points to sociotropic considerations at work: people might be evaluating the performance of the government based on social outcomes, more than from their own, pocket-book vantage point. In essence, Dalits formed opinions about the ruling party based on the fact that many people like them have received a house. I am able to rule out a range of explanations such as low satisfaction with the program, misattribution, clientelistic capture or inadequate credit-claiming by brokers, anticipation effects at the cut-point, overriding ethnic factors, and short term financial strain associated with homebuilding. For a discussion, see table 1.

Importantly, the program does not reduce the distributive salience of ethnicity. The survey includes a behavioral game, the Choose Your Dictator (CYD) game, in which participants have to pick between two hypothetical local politicians, one a co-ethnic, another from an out-group who cues affiliation to the BJP. The CYD game creates a low information environment in which ethnic and party labels can shape perceptions of distributive intent. Despite the BJP's high popularity at the national level, fewer than half the participants pick the BJP-cueing politician. There is a reversion to ethnic considerations while form-

Table 1: Evaluating Explanations

Explanation	Evaluation
Sociotropic considerations	Most plausible because of high network exposure to the program, and support for statements like "BJP has done something for people like me" and "condition of Dalits has improved in the last 5 years".
Short-term material shock	Unlikely because beneficiaries are highly satisfied with the program, recognize the long term benefits of a *pucca* house, and credit the BJP with doing something for them. The loss of income, lower consumption, and greater debt are down to voluntary choices, not the program.
Clientelistic capture or inertia	Unlikely because brokers do not play an indispensable role in claim-making, people do not think they control distribution of the benefit, brokers have little influence over vote choice in national elections, and BJP out-performed other parties in voter contact, with no difference in contact rates to the left and right of the cut-point.
Ethnic prejudice	Not very likely because there is weak prejudice against Muslims (37 paisa to 63 paisa in a dictator game involving 10 rupees).
Low satisfaction or misattribution	Unlikely because beneficiaries are very satisfied with the house, did not have trouble getting money from the program, and under 20% report paying harassment bribes or facilitation fees. Misattribution also seems unlikely because over 70% respondents know the program is run by the Modi government.
Anticipation effects	Unlikely because the information and awareness needed to form such expectations does not exist. Only 21% of Dalits to the left of the cut-point think they will get a house in the next few months. These expectations are not correlated with proximity to the cut-point.

ing opinions about politicians' distributive intent. Moreover, those offered a house pick the BJP-cueing politician at comparable rates to those who have not benefited from the program.

These findings contribute to the literature in numerous ways. First, in ethnically diverse, developing democracies, programmatic competition is seen as an antidote to ethnic politics. I show that an expensive benefit, delivered programmatically and recognized as such by beneficiaries, does not "undo" the distributive salience of identity. Ethnic preferences appear rather entrenched despite some programmatic shifts in the polity. Second, I leverage qualitative information about the housing program for empirical identification using a regression discontinuity design. This is one of the few studies that spots a naturally occurring discontinuity, and collects original data around the cut-point using a principled, pre-registered design. Finally, I study a new anti-poverty program in the world's most populous democracy that has funded 8.8 million houses. An evaluation of this program provides valuable lessons for developing countries with similar programs that promote homeownership or seek to reduce housing deprivation.

In what follows, I survey the existing literature, then describe the political context of the study, detail my argument and hypotheses, describe the research design, present the results, and explore the implications of my findings.

Why Material Benefits Matter

Existing Literature

To understand the electoral importance of material benefits, we must start with the voter's utility function. This combines elements of Downs (1957)'s spatial competition model and Riker and Ordeshook (1986)'s rational choice framework. Typically, voter i's utility from voting for party P depends on three things: the ideological distance between i and party P, i.e. $(\sigma_i - \sigma_P)^2$ where σ_P is the party's ideal point; the expected benefit $b \in \{0, b\}$ if party P comes to power, and the costs of voting $c \in (0, 1)$.

$$U_i(b_i, \sigma_i, \sigma_P) = -(\sigma_i - \sigma_P)^2 + b_i - c_i \tag{1}$$

Lindbeck and Weibull (1987) and Dixit and Londregan (1996) show that the optimal strategy for parties is to target benefits at swing voters. Stokes (2005) shows that it makes electoral sense to target benefits at weakly opposed voters. The assumption here is that a benefit, b_i, can compensate for part or all of the disutility arising from ideological differences.

The empirical evidence on this is far from conclusive. Many studies show that government programs, and spending more generally, increases support for the incumbent. For example, Levitt and Snyder Jr. (1997) in US congressional races, Nazareno, Stokes, and Brusco (2006) in Argentina's unemployment benefits program, Chen (2008, 2013) in Florida's disaster relief, Pop-Eleches and Pop-Eleches (2009) in Romania where poor families got coupons to buy computers, Manacorda, Miguel, and Vigorito (2011) in Uruguay's conditional cash transfer scheme, and De La O (2013) in Mexico's Progressa program. When these benefits reach party supporters, they compensate for the costs of voting (c_i) and incentivize turning out to vote. The literature often refers to this as *mobilization*. In contrast, when benefits reach swing voters or weakly opposed voters, they compensate for ideological disutility. The literature refers to this as *persuasion*. In practice, material benefits mobilize *and* persuade voters; and as Hidalgo and Nichter (2016) point out, can be used to "import outsiders" into the electorate as well. In Florida, disaster relief increased turnout among incumbent party supporters and decreased turnout among opposition voters (Chen 2013). In Mexico, *Progressa* increased turnout and support for the incumbent party but did not reduce support for the opposition (De La O 2013). In Romania, both mobilization and persuasion effects were observed. Incumbent party supporters turned out in larger numbers, and opposition voters switched support in favor of the incumbent party (Pop-Eleches and Pop-Eleches 2009).

More recently, studies have shown that voter preferences changed as a result of spending promises (prospect of benefiting), not their actual implementation (receipt of benefits)

(Elinder, Jordahl, and Poutvaara 2015). In Uruguay, beneficiaries rewarded the incumbent even after they stopped receiving benefits. Manacorda, Miguel, and Vigorito (2011) argue this is because rational but poorly informed voters form opinions about politicians and their distributive intent based on their experiences (i.e. whether or not they benefited from a program). These opinions persist, and continue to shape political preferences. In some contexts, incumbents are rewarded for doing nothing because state inaction produces material benefits for voters. As Holland (2015, 2016) argues, politicians in Santiago, Bogota, and Lima intentionally show "leniency towards violations of the law" to benefit squatters and street vendors. This sort of "forbearance" is politically motivated: weak enforcement is implicitly or explicitly contingent on electoral support. Finally, work in this area also looks at the impact of housing programs. Recent work in India and Brazil shows that receiving a house increases civic engagement, leads to greater isolation from ethnic networks, and potentially spurs self-reliance and pro-market beliefs (see Barnhardt, Field, and Pande (2015); Kumar (2021*b*,*a*); Bueno, Nunes, and Zucco Jr. (2017)).

However, benefits do not always win votes. There is puzzling evidence that voters in rural India do not reward road building (Goyal 2019; Bardhan et al. 2020). This is the case even when high quality roads are built, voters attribute road building to the incumbent, and road building takes place close to an election. Wilkinson (2007) corroborates this point, giving the example of two performing governments that subsequently lost elections. Similarly, Kadt and Lieberman (2017) find that in southern African democracies, infrastructural investments in basic services are associated with a decrease in support for the incumbent party.

An emerging argument is that benefits that are distributed programmatically, bypassing brokers and party agents, may not win votes. This is because intermediaries, or *naya netas* (new leaders) as Krishna (2007) describes them, play a vital role in the political process: governments need them to implement policies (Mookherjee and Nath 2021) and provide public goods (Baldwin 2019, 2013), citizens need them to make claims with the state (Kruks-Wisner 2018), and parties use them to mobilize votes in elections.[3] These local leaders fight for public goods, have credibility and influence in the neighborhood, which they use to shape political preferences (Auerbach 2016; Baldwin 2013). When these intermediaries are excluded from the distributive process, there may be less leakage and favoritism but also weaker credit claiming and voter monitoring. Brokers are not incentivized to expend effort to deliver the vote. As a result, material benefits may not win votes at all, or only when the broker is aligned with the governing party.

I focus on another factor that mediates the relationship between material benefits and vote choice: ethnicity. We know that ethnic considerations compete with and are intertwined with material benefits.[4] In the standard voting model, if we treat ethnic differences as the principle ideological dimension, material benefits b_i can compensate for ethnic disutility, $(\sigma_i - \sigma_P)^2$. This captures the idea that voters from group j have ethnic reasons to not vote for party P but some benefit b can compensate for that. An example of this would be "religious welfare" persuading poor (or subaltern) voters to vote for an elite party (Thachil 2014).[5] In rural Ghana, Ichino and Nathan (2013) find that some "voters are less likely to vote for the party of their own ethnic group, and more likely to support a party associated with another group, when the local ethnic geography favors the other group". This happens because voters expect politicians from the other group to deliver non-excludable benefits to the community. Similarly, Gadjanova (2021) shows that incumbents in Uganda, Kenya, and Ghana "campaign on their ability to offer various types of material benefits and local public goods (in the form of patronage or "pork")" when wooing voters outside their ethnic core.

One can complicate this further by thinking of material benefits in ethnic terms. Co-ethnics can value similar public goods or have the same preference ordering for policies (Lieberman and McClendon 2012; Baldwin and Huber 2010; Alesina and LaFerrara 2005;

[3]For example, Harding and Michelitch (2019) show that trust in and contact with traditional authorities (intermediaries) strengthens partisanship in Africa.

[4]For a comprehensive survey of this literature, see Kalin and Sambanis (2018).

[5]Material and ethnic considerations can be in competition if receiving benefits "trigger[s] a common cross-ethnic ingroup identity" (Thachil 2017:908), as is the case for urban migrants.

Alesina, Baqir, and Easterly 1999). Access to the benefit might be conditioned on ethnicity (Marcesse 2018). Ethnicity can shape how people process information and evaluate performance (Adida et al. 2017). These things can amplify or mute the impact of a benefit. People can expect ethnic favoritism in the distribution of benefits and opportunities (Chandra 2004; Dunning and Nilekani 2013; Auerbach and Thachil 2018; Gulzar, Haas, and Pasquale Forthcoming; Kramon and Posner 2016; Posner 2005). Ethnic networks can influence the cost of distribution, particularly when they provide monitoring and enforcement mechanisms (Habyarimana et al. 2009; Miguel and Gugerty 2005; Chandrasekhar, Kinnan, and Larreguy 2018). They also shape norms, and the cost of participation in political processes (Anoll 2018).[6] In summary, ethnicity can moderate or mediate the impact of material benefits on political preferences through a variety of mechanisms.

Cross-Ethnic Appeals and Material Benefits

This paper focuses on the role of material benefits when a party appeals to voters outside its ethnic core. Can material benefits win support for the party? They are unlikely to if voters outside the party's ethnic core have their own party that champions their interests and gives them preferential access to resources and opportunities. Material benefits can win support if there is no such challenger. This is precisely what happened in India with the decline of ethnic parties. The ruling party, BJP, sensed an opportunity, and targeted benefits at voters outside its ethnic core.

To see this, Figure 1 shows the proportion of an ethnic group voting for *its* ethnic party and the BJP in parliamentary and provincial elections since 1995. The analysis focuses on what Thachil and Teitelbaum (2015) call "narrow ethnic parties" that follow "patronage-based strategies *within* their restricted ethnic cores"(Thachil and Teitelbaum 2015:1394).[7]

[6] The ethnicity literature identifies other motivations that are not directly related to material benefits. For example, expressive benefits from the act of voting for a co-ethnic, anticipated or actual status benefits, expression of prejudice or altruism (see Haynie (2001)'s survey of the literature in Chapter 5, "Race and Peer Evaluations of African American Legislators", pp.93).

[7] In contrast, "encompassing ethnic parties" mobilize broader identities and are more likely to engage in

The figure clearly shows that ethnic parties, even at the height of their electoral relevance, only managed to mobilize a little over half the votes in their ethnic group. Moreover, there is a continuous decline in support for nine well-known ethnic parties between 1995 and 2014. This decline is not due to erosion in support among peripheral groups but hollowing of the core base. In the same period, there is a commensurate increase in support for the BJP. Clearly, the BJP increasingly appealed to voters outside its ethnic core as ethnic parties declined. Did material benefits play an important role in this outreach? And if these benefits were delivered programmatically, did that weaken the distributive salience of ethnicity?

Expectations in the Study Context

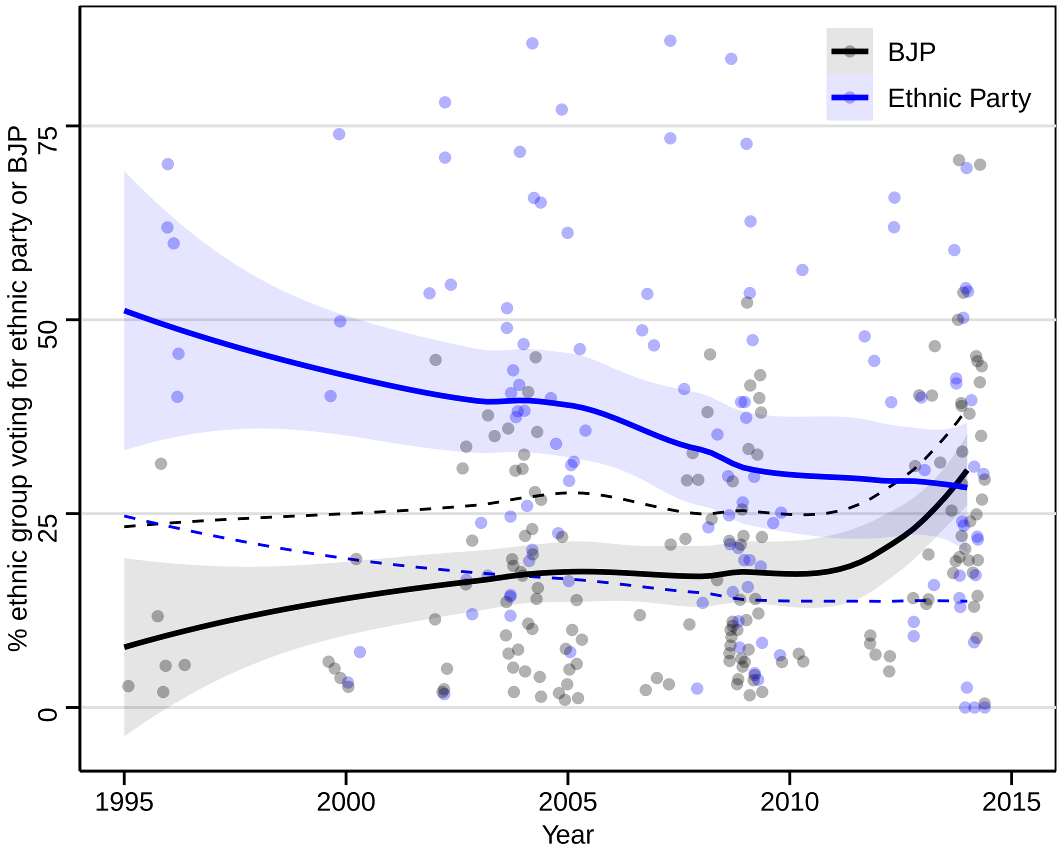
To evaluate these questions, I turn to India's Bihar province and focus on a large-scale, rural housing program. Bihar's politics is fractured along caste and religious lines like elsewhere in India. Figure 2 arranges voters on a majoritarian-secular ideology dimension. On one end of the spectrum are voters that support Hindu majoritarianism ($\sigma_i = A$). On the other extreme are voters that support secularism ($\sigma_i = B$). The ruling party, BJP, advocates for Hindu majoritarianism, while the opposition champions secularism. Bihar's ethnic groups can be arranged on this dimension. High status groups (call them BJP loyalists) are located on one extreme ($\sigma_i = A$), Muslims (call them opposition loyalists) on the other extreme ($\sigma_i = B$), with swing voters in the middle ($\sigma_i = 0$). A district's demography determines which group is electorally pivotal.

I focus on low-caste Hindus (or Dalits), who are ethnically cross-pressured, and electorally pivotal to varying degrees. The study focuses on three, theoretically interesting districts of Bihar province: Araria (which is 43% Muslim), Katihar (44.5% Muslim), and Darbhanga (22.4% Muslim)[8]. In the first two districts (Araria and Katihar), Muslims are numerous and Dalits tend to be swing voters. The BJP needs Dalit votes to win an election and it makes electoral sense to distribute benefits to this group. In the third district (Darb-

programmatic distribution. They are not the subject of discussion here.

[8]Source: India's Census, 2011, District Handbooks

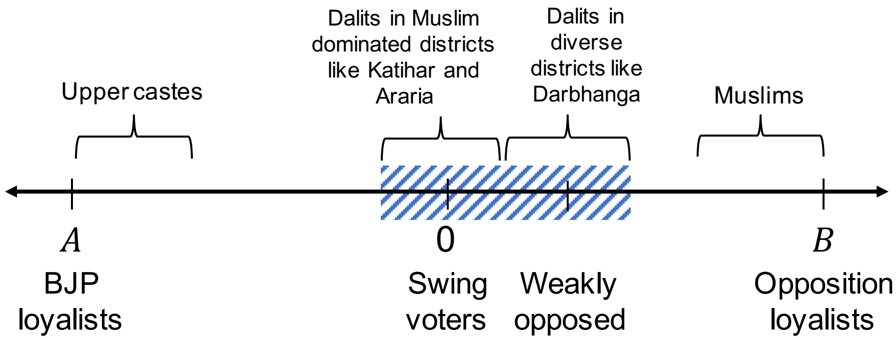
Figure 1: Ethnic Dealignments (1995-2014)



Note: Each point shows the percentage of an ethnic group that voted for its ethnic party or the BJP in an election. The solid trend lines capture over-time variation in groups supporting their ethnic party and the BJP. The dashed lines capture changes in overall party vote share. Data for this analysis is from the CSDS Lokniti Post Poll Surveys conducted for various state and national elections between 1995 and 2014. The analysis focuses on 8 states (UP, Bihar, Chhattisgarh, Jharkhand, MP, Karnataka, Maharashtra and Haryana) and 10 parties (BSP, SP, RLD, RJD, LJP, INLD, JDS, NCP, JMM and BJP).

hanga), Muslims are not as numerous, and cleavages within the Hindu group are politically salient. There is a history of caste antagonism and violence. BJP champions the interests of high status groups, which are its ethnic core, and depends less on Dalit votes. Dalits have historically supported opposition parties. To summarize, in the first two districts, Muslims are so numerous that Dalits occupy center stage, in a third, ethnically diverse district, Dalits are weakly opposed to the BJP. The shaded area in Figure 2 represents the set of voters we study in this paper.

Figure 2: Dalits as swing and weakly opposed voters



Note: Dalits in Muslim dominated districts of Katihar and Araria are swing voters, while those in ethnically diverse districts like Darbhanga are weakly opposed to the BJP.

Appendix A shows that parties have historically competed neck and neck for the Dalit vote in Bihar province. However, the BJP has gained an upper hand in recent years. This trend coincides with the nationwide decline of ethnic parties shown in Figure 1. Could material benefits, delivered programmatically, have contributed to this realignment?

For this to be the case, the valuable benefit should increase support for the BJP, and reduce the distributive salience of ethnicity. This leads to two primary hypotheses, and several empirical measures associated with each.

H1 Dalits who are offered a house should be more supportive of the benefit giving party (BJP) and be less likely to engage in costly collective action against it, compared to those next in line to be offered a house.

Empirically, this hypothesis is evaluated in several steps. First, do beneficiaries recognize that the ruling party has done something for them? I get at this by measuring agreement with statements like "I have benefited from the BJP government" and "people like me will benefit from a BJP government". Second, does this translate into support for the ruling party? I employ a variety of survey measures to get at this. For example, likability of the party (BJP) and its leader (Narendra Modi), performance evaluations (e.g.: competence relative to the previous government, perceptions of corruption and development work done), support for its distributive message, and reaction to the party leader's election speech. Third, I evaluate whether receiving an expensive material benefits shapes attitudes towards the opposition and political competition more generally. I do so by including survey questions that measure likelihood of attending an opposition party's election rally, vote transferability (likelihood of voting for a party allied with the BJP), and perceptions of electoral invincibility (can any other party or leader defeat the BJP if elections are held in the next six months). Finally, I get at an important mechanism that could be driving support for the benefit giving party: gratitude. I ask study participants if some people voted for the BJP in the last parliamentary election because they got a house. In other words, did a feeling of gratitude or indebtedness drive political preferences?

H2 Dalits who are offered a house should have weaker ethnic preferences on distributive issues, compared to those next in line to be offered a house.

I evaluate this hypothesis using a behavioral game, the Choose Your Dictator (CYD) game. Study participants have to pick one of two hypothetical local politicians: a caste co-ethnic (subaltern leader), and a non-coethnic cueing affiliation to the benefit giving party (BJP). Since distributive politics is highly politicized along this status cleavage, the idea is to measure ethnic preferences with distributive implications in a low information environment.[9]

My pre-analysis plan describes each of these outcomes, the associated survey measures, and outcome-level hypotheses.

Alternative Explanations

As prior work suggests, there are many reasons why the offer of a material benefit does not affect political preferences. I identify some of the most likely substantive explanations in our context.

1. **Low satisfaction:** Beneficiaries may not reward the BJP despite being offered a house if the promise is not credible or satisfaction with the program is low.

[9] Chandra (2004) would consider these the ideal theoretical conditions for ethnic voting in patronage democracies.

2. **Misattribution:** If beneficiaries incorrectly attribute the program to the state government, not the national government, there may be no difference at the cut-point because beneficiaries do not credit the BJP for the program.

3. **Clientelistic capture or inertia:** When a benefit is distributed through clientelistic channels, brokers can take credit for it. When this happens, beneficiaries reward the broker with an eye to future benefits. This means the party distributing the benefit only wins support when *their* broker is distributing the benefit. The opposition party's broker can "hijack" credit for the benefit, particularly if the ruling party cannot channel resources through non-state organizations (Bueno 2018). A different kind of problem emerges when brokers are not involved in the distribution process: they may not expend effort to inform voters about the government's achievements, persuade beneficiaries to vote for the party, and turnout the vote[10]. For the clientelistic capture story to hold, two things must be true: (i) people should need the local leader's help to benefit from the program; and (ii) the local leader should get most of the credit for the program. For there to be clientelistic inertia, brokers should exert influence over political preferences, and control the supply of information to voters. This would typically imply low levels of awareness about the benefit, and high levels of misattribution.

4. **Sociotropic considerations:** If the next in line form preferences based not on their *own* treatment status but how much of their social network is treated, there may be no difference at the cut-point. The idea here is that Dalits who have not been offered a house support the BJP because many people like them were offered a house. In closely-knit village communities beneficiaries and non-beneficiaries have similar exposure to the program. If sociotropic considerations drive preferences, there may be little difference between preferences of beneficiaries and non-beneficiaries. I get at this by measuring exposure to the program: how many people someone personally knows that have got

[10]We know that brokers engage in persuasion and mobilization because they have ideologically heterogeneous networks (Stokes et al. 2013). In the Indian context, Sircar and Chauchard (2017) finds that clientelistic networks are multi-ethnic too.

a *pucca* house? If both beneficiaries and non-beneficiaries know many such people, and there is no statistically significant difference in exposure to the program at the cut-point, this type of explanation might be plausible.

5. **Anticipation effects:** A regression discontinuity estimates the difference at the cut-point. A technical reason for a null result can be anticipation effects: the next in line very close to the cut-point know they are imminently going to be offered a house, and adjust their preferences in anticipation of receiving the benefit. This sort of thing is only possible if someone knows their position relative to the cut-point, and explicitly articulate an expectation that they are about to benefit from the program.

6. **Ethnic or economic factors:** A very valuable benefit, like a house, may not move preferences if other factors drive preferences. There are two possibilities here: overriding ethnic considerations like prejudice against Muslims; and financial shocks associated with homebuilding. On the ethnicity front, it is possible that, for Dalits in Muslim-dominated areas, their Hindu identity becomes more salient, and their political preferences are driven by religious identity rather than by any receipt of material benefits. Here, ethnic prejudice dominates the voter's mind, not a material benefit. When it comes to financial shocks, there may be community-wide or individual-specific factors exclusively affecting beneficiaries that offset the impact of a house. In my field sites, I can think of three such factors: unemployment, income loss, and increased household debt. When a poor family is offered a house, very often they self-build to save money. This means family members temporarily lose employment, and a source of income. My fieldwork also suggests that families over-spend because their aspirations exceed the money they get from the government. Families borrow money to top-up what they get from the program, and build more than a basic structure. This implies greater household debt compared to those next in line. Cumulatively, we can think of this as a short-term financial shock associated with homebuilding. Purely on pocketbook

considerations then, beneficiaries may not reward the BJP.

Research Design

To evaluate my primary hypothesis, and possible explanations for a null result, I leverage qualitative information about the distribution process. This section details the identification strategy, sampling procedure, pre- and post-data collection design tests, measures and estimation strategy.

Identification

I am interested in the impact of a housing program started by India's BJP government in 2016. This program provides land and money ($\approx$ USD 2000) to the poorest families to construct a two-room cement structure. They also get money for a toilet, a cooking gas connection, and a bank account. Between 2016 and 2019, 8.8 million houses were funded by the government, nearly 62% of those for lower castes, tribals, and religious minorities. It is worth noting that this is not the first instance of government providing housing assistance to the poor. Past governments ran programs like the *Indira Awas Yojana* but fewer houses were built, and there was considerable discretion and favoritism in the distribution of benefits.

Based on interviews with bureaucrats, I learned that the current housing program was designed to minimize discretion, favoritism, and patronage. The government used socioeconomic indicators from the 2011 census to identify the poorest households in the country. It assigned qualifying households a deprivation score using census measures, then ranked the households from most to least deprived by census village and ethnic category (lower caste, tribal, minority, and general). This ranking was sent to the village assembly for corrections like removing dead people, ineligible households, or those who migrated to another area. The village assembly did not know the purpose of the list, it could not add new names to the list, and its decision to remove names was formally recorded as part of the proceedings and subject to an appeals process. After this process was completed, the government announced the housing program. It publicized the beneficiary list (or rankings), and followed that order while offering houses. The pre-analysis plan gives a step-by-step description of the implementation process based on interviews of bureaucrats and government documents.

The identification strategy hinges on the claim that when I started collecting data, an arbitrary cut-point separates the last person offered a house, and the one next in line to be offered a house. The cut-point is plausibly exogenous because: (a) bureaucrats who decided how many houses to build each year lacked fine-grained information on beneficiaries and the incentive to precisely set the cut-point; (b) beneficiaries could not sort, or alter their household's ranking; (c) local politicians who have granular information on beneficiaries and political incentives could not ex-post manipulate the ranking. The pre-analysis plan documents reasons for the plausibility of the design, along with qualitative evidence, and where possible, ex-ante design tests.

I define the substantive quantity of interest as the difference in expected outcomes when Dalits are offered a house and when they are not offered a house. Formally:

$$\mathbb{E}(Y_i|\text{Offered a house}) - \mathbb{E}(Y_i|\text{Not offered a house}) \tag{2}$$

where Y_i is a set of behavioral and attitudinal measures for person i.

Since there are obvious selection issues, and observed and unobserved factors that distinguish those who are offered a house from those who are not, the identified quantity or estimand is the average causal effect of being offered a house *exactly at the cut point*:

$$\mathbb{E}(Y_i(1) - Y_i(0)|Distance_i = 0) \tag{3}$$

Where $Y_i(1)$ describes the treated potential outcome for Dalits at the cut point, and $Y_i(0)$ their untreated potential outcome. $Distance_i$ is the forcing variable, and the cut point is at $Distance_i = 0$. I construct $Distance_i$ as follows:

$$\text{Distance}_i = \frac{(-1) \times (\text{Rank}_i - [\text{Rank}_{\text{last beneficiary j}} + 0.5])}{n_{\text{village}}} \tag{4}$$

As Cattaneo, Idrobo, and Titiunik (2019) show, under certain assumptions the average causal effect at the cut point is identified. The key intuition is that as we get arbitrarily close to the cut-point (in the "immediate neighborhood" of the discontinuity), conditional independence of treatment assignment is more plausible, and individuals are in expectation similar in observed and unobserved ways.

Data

India's government agreed to share beneficiary data for three districts in Bihar: Katihar, Darbhanga, Araria. I received three files from them: (i) an excel sheet with the permanent wait list (PWL) or beneficiary list; (ii) census data, including the deprivation score, used to identify and rank beneficiaries; and (iii) disbursement data for those who have received money for a house.

The sampling strategy was two-fold: interview households within a pre-registered bandwidth around the cut-point, and draw a random sample of people who are on the list but outside that bandwidth. This decision involves three parameters: the bandwidth (ϵ), number of villages to sample (n_v), and proportion of subjects outside the bandwidth to be sampled (p_v). These decisions are, of course, subject to budgetary constraints.

Following Manacorda, Miguel, and Vigorito (2011), I picked a bandwidth of 3% for Dalits. Their study in Uruguay picked a bandwidth of 2%. I use a slightly larger bandwidth since there are fewer households in the beneficiary list.

I picked n_v and p_v by calculating the cost of conducting a survey in n_v villages[11], interviewing all the households within the bandwidth (ϵ), and p_v proportion of people outside the bandwidth. I picked a sampling decision (n_v and p_v) that was within my budget, and

[11]Removing villages that did not have 3% treated and untreated subjects, and arranging them in descending order of untreated subjects.

maximized the number of subjects within the bandwidth. For Dalits, this yielded the following rule: visit 60 villages, interview all the households within a 3% bandwidth, and 10% of households on the list but outside this bandwidth.

The survey team informed me that the non-contact rate is typically 40%. As preparation for this, I identified a replacement sample before going into the field. I oversampled outside the bandwidth (1.5 times p_v), and picked households adjacent to the bandwidth (i.e. just outside the bandwidth but most-proximate to it) as replacements for those within the bandwidth. Ultimately, the sample frame (including replacements) had 832 Dalit households and the team interviewed 530. This yields a contact rate at 63.7%, marginally above our expectations and similar to the rate reported by Manacorda, Miguel, and Vigorito (2011). The sampling strategy, enumeration protocol, and non-contact protocol were pre-registered. The fieldwork followed most of the recommendations in Logan et al. (2020), as they relate to survey design, partner selection, interviewer training, and monitoring and assessing data quality.

Design tests

To empirically validate the regression discontinuity design, I perform a variety of tests discussed in Cattaneo, Idrobo, and Titiunik (2019). This includes the McCrary density test to check for sorting around the cut-point, and balance tests that detect discontinuous changes in covariates at the cut-point. I perform these tests pre-data collection, and post-data collection. The pre-analysis plan reports the design tests for the planned sample ($n = 608$, excluding replacements). Here, I report the results of the McCrary density test and balance tests for the realized sample ($n = 530$, including replacements).

Table reports the density of the forcing variable just below the cut-point and just above the cut-point, along with their uncertainty estimates. The third row in the table reports the difference in densities, and the associated standard error (computed using the jackknife method). The fourth row of the table reports the t statistic and p value from a t-test. A large

p value suggests that the densities to the left and right of the cut-point are not statistically distinguishable, while a small p value suggests the difference is statistically significant. As table 2 confirms, there forcing variable's density on either side of the cut-point is very similar. The fifth row in the table reports the bandwidth used in the McCrary density test, either the MSE optimal bandwidth or a pre-specified bandwidth of 3%. The results are largely the same under both specifications.

Table 2: McCrary Density Tests

	MSE optimal bandwidth	Pre-specified bandwidth
Density (Left)	6.39 (se =1.25)	4.92 (se =2.78)
Density (Right)	7.83 (se =0.97)	5.96 (se =2.97)
Difference	1.44 (se =1.58)	1.03 (se =4.07)
T statistic	0.91 (p =0.36)	0.25 (p =0.79)
Bandwidth (L, R)	0.12, 0.16	0.03, 0.03

The test is performed in R using the `rddensity` package. We use the default settings: a local quadratic approximation (p=2), triangular kernel, and MSE optimal bandwidth. In an alternative specification, the bandwidth is manually set to 3% (h = 0.03).

We know that the McCrary density test is designed to detect sorting around the cut-point. While qualitative knowledge of the housing program rules out this possibility[12], I nonetheless included a survey question about this. I ask respondents if they tried to get a house before their turn. About 35% of respondents attempted (in vain) to get a house before their turn. Figure 13 confirms there is no asymmetry or discontinuous change at the cut-point. Nonetheless, this provides an insight into popular perceptions of the program. Even though houses were distributed in a pre-decided order, people believe there is discretion and it is possible to jump the queue and expedite things. This does not invalidate the design. It is not evidence of sorting. It is, at best, evidence that people attempted sorting but our qualitative knowledge strongly rules out the possibility of actual sorting.

Table 3 reports the results from the balance test. The idea here is to use exactly the

[12]Households were ranked within each village and ethnic community, these rankings were finalized before the launch of the program and did not change subsequently. They are public information, and houses were offered in that order.

Table 3: Balance Tests

		RD (MSE optimal BW)				RD (BW = 3%)			
Source	Covariate	Estimate	$\widehat{se}$	p	n	Estimate	$\widehat{se}$	p	n
Census	Deprivation Score	0.237	0.146	0.106	298	0.273	0.264	0.301	152
	Female	-0.046	0.059	0.434	294	0.054	0.090	0.549	152
	Age	-0.360	3.275	0.912	295	-1.502	6.078	0.805	152
Survey	Female	0.011	0.095	0.909	305	-0.023	0.195	0.906	152
	Age (binned)	-2.000	3.018	0.508	285	-3.964	5.756	0.491	152
	Education (1-8)	0.299	0.387	0.439	286	0.265	0.547	0.628	152
	Migrant	0.125	0.094	0.183	274	0.154	0.160	0.334	152

The results are obtained in R using the `rdrobust` package. The estimation strategy was pre-registered. The first model (columns3-6) reports the bias-corrected robust standard errors and estimates using an MSE optimal bandwidth, triangular weights, and linear specification ($p = 1$). The second model (columns 7-10) reports conventional estimates and standard errors using the pre-registered bandwidth ($h = 0.03$), triangular weights, and $p = 1$. There are 530 households (clusters), spread across 53 villages.

same specification as the outcome analysis but replace the outcome variable with a covariate to see if there is a discontinuous change in its value at the cut-point. In the pre-analysis plan, I check for "balance" on three census variables: age, gender, and the deprivation score (1 to 10). Here, I check for discontinuous changes in the three census variables, and four background characteristics collected in the survey (gender, age (binned), education, migrant status).

I observe no statistically significant discontinuous change at the cut-point in gender composition, age, education, migrant status, and socioeconomic deprivation. Table 3 reports the estimate of the difference at the cut-point, the standard error, associated p value, and effective sample size (n). These results are robust to the use of an MSE-optimal bandwidth and the pre-registered bandwidth of 3%.

Taken together, these design tests give us confidence in the identification strategy used in this paper.

Estimation strategy

The survey questions, coding of variables and RD specification were pre-registered. The primary specification uses a linear regression (first-order polynomial), triangular weights, the MSE optimal or pre-registered bandwidth (3%), and clustered standard errors if more than one member of a household is interviewed. I report the robust, bias-corrected estimate and standard error when using the MSE optimal bandwidth, and the conventional estimate and standard error when using the narrower, pre-registered bandwidth of 3%.

Results

Main outcomes

There are substantial and interesting differences between Dalits who were offered a house, and those next in line to receive a house. Table 4 reports the difference at the cut-point ($\widehat{\tau}$).[13] Appendix B contains regression discontinuity plots for each outcome. The table and figures show that Dalits who were offered a house are more likely to agree with the statement, "BJP has done something for [me]". Respondents were given four coins and practiced putting coins on the ground or table to indicate how much they agree with a statement. They could put no coin (indicating complete disagreement), a few coins, or all four coins (conveying complete agreement). On average, in the "control" group, subjects put 2.25 coins. Those offered a house put an additional 0.6 to 1 coin.

There is also support for the gratitude mechanism. I ask respondents whether some people voted for the BJP because they received a house. Owing to social desirability concerns, I did not ask explicitly whether respondents themselves voted on this consideration. Nearly

[13] The table's first column reports the variable name. The second column indicates the hypothesized direction of the effect. Columns 3 to 6 report the difference at the cut-point, standard error and associated p value under an MSE optimal bandwidth picked by `rdrobust`. These are robust, bias-corrected estimates and standard errors. Columns 7 to 10 report the same statistics when we use the pre-registered bandwidth of 3%. Column 11 reports the mean value of the outcome for subjects to the left of the cut-point (notionally in the "control" group) as a reference point.

Table 4: Primary Outcomes Analysis

		RD (MSE optimal BW)				RD (BW = 3%)				
Outcome	Hyp.	$\widehat{\tau}$	SE	p	n	$\widehat{\tau}$	SE	p	n	$\overline{Y_{\text{Z}=0}}$
BJP has done something for me (0-4)	Pos	0.615	0.295	0.037	348	1.015	0.578	0.079	180	2.255
Some people voted for the BJP because they got a house (0/1)	Pos	0.189	0.089	0.035	299	0.216	0.162	0.184	152	0.199
Programmatic Awareness (0-4 Index)	Pos	0.42	0.18	0.02	295	0.87	0.35	0.01	152	2.55
BJP does something for people like me (0-4)	Pos	-0.212	0.213	0.319	346	-0.015	0.381	0.969	180	3.057
CYD (Picks BJP, 0-1)	Pos	-0.098	0.072	0.177	304	-0.096	0.156	0.537	150	0.482
Receptive to Modi message (0-1)	Pos	-0.040	0.046	0.380	351	-0.102	0.098	0.297	180	0.940
Likes BJP (0-4)	Pos	0.360	0.219	0.101	318	0.423	0.508	0.404	152	3.156
Like Modi (0-1)	Pos	-0.010	0.031	0.741	359	-0.064	0.069	0.353	180	0.979
Cong-BJP competence comparison (-1 to +1)	Pos	-0.093	0.063	0.143	346	0.046	0.073	0.530	180	0.908
BJP less corrupt, more reaches poor (0-4)	Pos	-0.108	0.222	0.626	330	-0.159	0.347	0.646	180	2.851
Condition of Dalits (-1 to +1)	Pos	-0.227	0.107	0.033	297	0.093	0.201	0.646	152	0.844
Vote for BJP ally (0-1)	Pos	-0.149	0.055	0.007	347	0.035	0.140	0.805	180	0.887
Attend opposition rally (0-1)	Neg	0.176	0.079	0.026	348	0.065	0.167	0.695	180	0.234
BJP defeatable (0-1)	Neg	0.026	0.056	0.641	334	0.002	0.101	0.984	180	0.121

These are results from a survey conducted on Dalits in Darbhanga, Araria, and Katihar between January and March, 2020. The estimation strategy was pre-registered. Columns 3-6 report the bias-corrected robust estimates and standard errors using an MSE optimal bandwidth, triangular weights, and linear specification ($p = 1$). Columns 7-10 report conventional estimates and standard errors using the pre-registered bandwidth ($h = 0.03$), triangular weights, and $p = 1$. Responses are clustered at the household level. There are 530 households (clusters), spread across 53 villages. Column 11 reports the mean value of the outcome to the left of the cut-point (i.e. among those who have not been offered a house, hence $\overline{Y_{\text{Z}=0}}$).

20% of Dalits in the control group agree with the statement that some people voted for the BJP because they received a house. Support for this proposition increases by 19 to 22 percentage points at the cut-point.

I also find a substantial increase in programmatic awareness. This is a key outcome because the housing program identifies beneficiaries using objective indicators of poverty from the census, and minimizes the party broker's discretion. Did beneficiaries, or those next in line, perceive this as programmatic distribution? On a 0 to 4 scale, where higher values convey greater awareness of programmatic features, the average response in the control group is 2.55. At the cut-point, there is a 0.4 to 0.87 scale unit increase in programmatic awareness. I use four survey questions to measure programmatic awareness. I ask respondents whether they know of the housing program, whether they know of a beneficiary list (rank ordering) according to which houses are distributed, and whether they think there is broker discretion and ethnic favoritism in distribution. Appendix D shows the difference at the cut-point separately for each measure. The direction of these estimates is exactly how we would expect them to be. There is greater statistical uncertainty when using any single measure. Nonetheless, table 13 shows that people who are offered a house are (unsurprisingly) more likely to know about the housing program. They are more likely to know about the beneficiary list as the basis for distribution. They are also less likely to think there is ethnic favoritism and broker discretion in the distribution process.

Putting these pieces together, we can say that when a Dalit is offered a house, they recognize the benefit-giving party (BJP) has done something for them, they are more likely to think people voted out of gratitude for the BJP, and are more aware of programmatic features of distribution. Appendix C shows that these results, which are from a pre-registered specification, are robust to alternative specifications with different bandwidth selectors, polynomials, and kernels.

Despite all this, the program fails to move political preferences at the cut-point. Dalits who were offered a house are no more supportive of the BJP than those next in line.

Dalits on both sides of the cut-point think the "BJP does something for people like [them]". In the control group, on average, they put 3 out of 4 coins to express agreement with this statement. There is no increase in support for the statement at the cut-point. The negative coefficient is unstable and statistically insignificant.

I show respondents an election speech of Prime Minister Modi from the neighboring state of Jharkhand. In that speech, Modi claims the BJP's core philosophy is *sabka saath, sabka vikas, sabka vishwas* (everyone's support, everyone's development, everyone's trust). I ask respondents whether Modi seriously wants to take everyone along (coded as 1), whether this is cheap talk (0.5), or whether he is misleading people to get votes (0). Modi's distributive message seems to have a lot of credibility, with an average response of 0.94 in the control group. The difference at the cut-point is not in the hypothesized direction: it is negative, though statistically insignificant. This unexpected finding may reflect a ceiling effect – baseline support for Modi's message is extremely high, leaving little room for any increase.

Next, I ask respondents if they "like" the BJP and "trust it will do things for their welfare". On average, Dalits in the control group put 3.1 coins out of a possible 4 coins. The difference at the cut-point is in the hypothesized direction (increase of 0.36 to 0.42 scale units) but statistically insignificant. When it comes to Modi's speeches, an astounding 98% people in the control group like his speeches, leaving little room for any increase when they are offered a house. Unsurprisingly, the difference at the cut-point is not in the hypothesized direction and is statistically insignificant.

The survey also measures performance evaluations. There is very high approval of the current government. For example, one question asks respondents to compare the current BJP government to the previous Congress government. Responses are coded as +1 if they think the BJP government is better, −1 if the previous Congress government is better, and 0 if both are the same. The average response in the control group is 0.91. The difference at the cut-point flip-flops, and is statistically insignificant in both specifications. On corruption, respondents have to express agreement with the statement, "BJP is less corrupt, and more reaches the poor [in BJP governments]". The expectation is that Dalits who are offered a house express greater support for this statement than those who have not been offered a house.[14] In the control group, on average, respondents put 2.9 coins out of a possible 4 coins in support of the statement. Dalits who were offered a house agree with this statement at comparable rates. The difference at the cut-point is negative, though statistically insignificant. When it comes to their ethnic group's socio-economic condition, Dalits in the control group overwhelmingly say their community's condition has improved in the last five years. The average response in the control group is 0.84, on a -1 to $+1$ scale where higher values imply greater improvement in their material condition. The difference at the cut-point is inconsistent: negative and statistically significant in one case, positive and insignificant in the other.

Does the housing program generate support for parties that are allied with the BJP and less ethnically antagonistic towards Dalits? Nearly 88.7% of respondents in the control group say they would vote for an alliance partner, the Janata Dal United, which runs the provincial government. The difference at the cut-point is not consistently in any direction.

Another set of outcomes focus on whether the housing program makes voters less likely to engage in costly collective action against the benefit-giving party, and strengthens perceptions of its electorally invincible. Strikingly, only 23.4% Dalits in the control group say they would attend an opposition party's election rally. Contrary to expectation, Dalits who are offered a house are 7 to 18 percentage points *more* likely to attend an opposition rally. This is likely because of the economic and physical security that comes with a cement house, which can be locked and material possessions protected from theft. Table 7, later in the paper, develops this point. Dalits also seem to think the ruling party is electorally invincible. Only 12% in the control group think an opposition party or leader can defeat the

[14]This can be for several reasons: exposure to programmatic distribution, or as Klasnja, Lupu, and Tucker (2021) show, voters are less likely to sanction corrupt politician when they receive a benefit from that politician.

BJP if elections are held in the next six months. Dalits offered a house respond in pretty much the same way.

In summary, the ruling party is exceptionally popular among people who were offered a house, and those next in line to get a house. This tallies with the findings of the National Election Studies (NES) 2019. In a newspaper article Ranjan, Singh, and Alam (2019) report that 76% Dalits in the same province voted for the ruling party in the 2019 parliamentary election. 75% of NES respondents were satisfied with the government's performance, and 60% were willing to give the government another chance.

Did the ruling party's national reputation of programmatic efficiency weaken the distributive salience of ethnicity? To get at this, my survey included a behavioral game, the Choose Your Dictator (CYD) game. In this game, participants had to pick between two hypothetical local politicians — one a co-ethnic (Dalit), the other an out-group, upper-caste politician cueing affiliation to the BJP. Figure 3 provides an example match-up. I use ethnically ambiguous photographs for this game, with the politician's name cueing ethnicity and a saffron *gamcha* (scarf) or *tilak* cueing partisan affiliation.[15] Using Blum, Hazlett, and Posner (2020)'s design, there are two rounds of the game, an "anonymous" round in which respondents are told the politician does not have any information about them; and a "profiled" round in which the politician knows the respondent's name (ethnic cue), age, and occupation while deciding how to split 10 rupees with them. The CYD game is informative: it creates a low information environment in which ethnic and party labels can shape perceptions of distributive intent. We can then compare the salience of these factors when there are real stakes for the respondent.

Strong approval and support for the benefit-giving party, BJP, does not spillover into the local political context, where ethnic considerations continue to shape perceptions of distributive intent. On average, Dalits who were not offered a house pick the politician

[15]Every confederate was photographed twice: with and without the orange scarf (partisan cue). For any pair of confederates, A and B, the respondent could be randomly assigned to one of two possible match-ups: $\{A = \text{Dalit}, B = \text{Upper Caste} + \text{BJP}\}$ or $\{A = \text{Upper Caste} + \text{BJP}, B = \text{Dalit}\}$. Respondents only see a confederate's photograph once.

Figure 3: Choose Your Dictator Game, Example Match-Up

Note: In this example match-up respondents are shown two (hypothetical) local politicians. Politician 1 is Kishori Lal Paswan (Age 35), Politician 2 is Giriraj Jha (Age 29). Politician 1's last name (Paswan) cues their ethnicity, or Dalit identity in this case. Politician 2's last name (Jha) cues an upper caste identity, while a saffron *gamcha* (scarf) and *tilak* cues partisan affiliation to the BJP. Respondents have to pick one of the two politicians.

cueing affiliation to the BJP 48% of the time. They prefer the co-ethnic politician 52% of the time. The probability of selecting the BJP politician is comparable in the anonymous and profiled versions of the game. Dalits who were offered a house pick the politician cueing affiliation to the BJP 49.7% of the time in the anonymous round and 43.2% of the time in the profiled version of the game. This 6.5 percentage point difference borders on statistical significance at conventional levels ($t = 1.82$, $p = 0.069$). The difference at the cut-point is consistently negative: approximately 10 percentage points but statistically insignificant (see table 5, and the figure in appendix E). In other words, Dalits who are offered a house are no more likely to pick the BJP politician than those next in line. If anything, they seem less likely to pick the BJP politician, particularly in the profiled version of the game.

In summary, I find that those offered a house recognize that the BJP has done something for them, and they believe that some people voted for the BJP out of gratitude. However, they do not support the BJP any more than those next in line. Across a range of

Table 5: Choose Your Dictator Game

	RD (MSE optimal BW)				RD (BW = 3%)				
Outcome	$\widehat{\tau}$	SE	p	n	$\widehat{\tau}$	SE	p	n	$\overline{Y_{Z=0}}$
Both Rounds	-0.098	0.072	0.177	304	-0.096	0.156	0.537	150	0.482
Anonymous Round	-0.104	0.110	0.343	282	0.003	0.191	0.989	150	0.479
Profiled Round	-0.097	0.106	0.360	291	-0.195	0.191	0.307	150	0.486

measures, support for the BJP is very high among Dalits who were and were not offered a house. In some cases, there is little room for improvement when Dalits are offered a house. Strikingly, the BJP's distributive reputation and credibility at the national level does not travel to the local level, where ethnic labels continue to shape perceptions of distributive intent. Appendix G corroborates this claim using electoral data on the BJP's performance in national and local elections.

A case of socio-tropic voting?

What explains such high levels of support for the BJP among non-beneficiaries? I contend that Dalits who were not offered a house evaluate the government's performance by looking at their social network or local community. They assess the government's performance based on social outcomes, more than from their own, pocket-book vantage point. In other words, what matters to them is that "many people like me got a house", and not that "I was offered a house". I focus on two empirical findings that support this explanation. First, the housing program has considerable impact on the local society, especially the respondent's social network. I find that the respondents' social network is saturated with the benefit. Second, Dalits across the board think the BJP has done something for people like them, and their ethnic group's material condition has improved in the last 5 years (see Table 4).

Table 6 shows that network-level exposure to the housing program is very high on both sides of the cut-point. Between 70 and 77% respondents *personally* know at least one other person who has received a house. When asked how many beneficiaries they know, those

Table 6: Exposure to the housing program

	Know at least 1 other beneficiary		How many people do you know who got a house?	
Z	Percentage	SE	Count	SE
0	70	4	8.91	1.58
1	77	2	16.35	5.31

offered a house identify, on average, 16 people, while those next in line identify 9 people. These differences are not statistically significant to merit any conclusion about networking effects because of the program. The figures in appendix H confirm that respondents around the cut-point report very similar network-level exposure to the program. What this does suggest is that the Dalit community is saturated with the benefit, and even non-beneficiaries know many people who have received a house. This strengthens the case for the sociotropic explanation.

Other explanations

Before concluding, I evaluate alternative explanations for the main result. In this section, I evaluate the role of material and ethnic factors, clientelistic capture or inertia, misattribution, low satisfaction, and anticipation effects at the cut-point.

Material factors

One explanation for this result is that the program failed to improve the material condition of recipients. Table 7 reports the RD estimates for four economic variables: physical and economic insecurity, self-reported meal skipping due to financial strain, monthly household income, and recent debt. For economic insecurity, we ask respondents how worried they are about their family and material belongings when there is torrential rain or a storm (0-4 coins, increasing in worry). This measure captures one of the main psychological benefits of having a cement house for those who previously lived in mud or bamboo huts. Non-beneficiaries put, on average, 3.66 coins. Beneficiaries, on average, put 0.3 to 1 fewer coins in response to this question. This difference at the cut-point is statistically significant.

That said, I find that Dalits who are offered a house experience a short-term economic shock, even if in the long term homeownership improves their material conditions. On average, a Dalit household in the control group earns 6900 rupees per month. The household's income drops by 1600 to 3100 rupees at the cut-point. My fieldwork indicates this is due to temporary unemployment: most families tend to build their houses themselves, relying on their own labor to lower costs. In line with this, I find that household debt increases at the cut-point by 2200 to 7000 rupees (most of this money is borrowed from family, friends, and moneylenders in the informal credit market); and meal-skipping due to financial constraints increases by 14 to 27 percentage points. These differences are directionally consistent and approach statistical significance at conventional levels ($p < 0.05$).

Even though there is some evidence of a short-term financial shock, I do not think this explains why Dalits on either side of the cut-point comparably support the BJP. This is because: (a) there is evidence that Dalits who are offered a house recognize the BJP has done something for them; (b) they are cognizant of the long-term benefits of a house, namely greater physical and economic security; and (c) seem to understand that the short-term financial shock is because of their own voluntary actions, not something the government has done.

Clientelistic capture or inertia

We would expect clientelistic capture when brokers play an important role in claim-making (i.e. people still need the local leader's help to access the programmatic benefit), they get most of the credit for the program such that it generates political capital for them, and they have electoral influence that they can leverage during elections.

I do not find much empirical support for any of these supporting conditions. Firstly, local leaders do not play an important role in claim-making. In the survey, I included

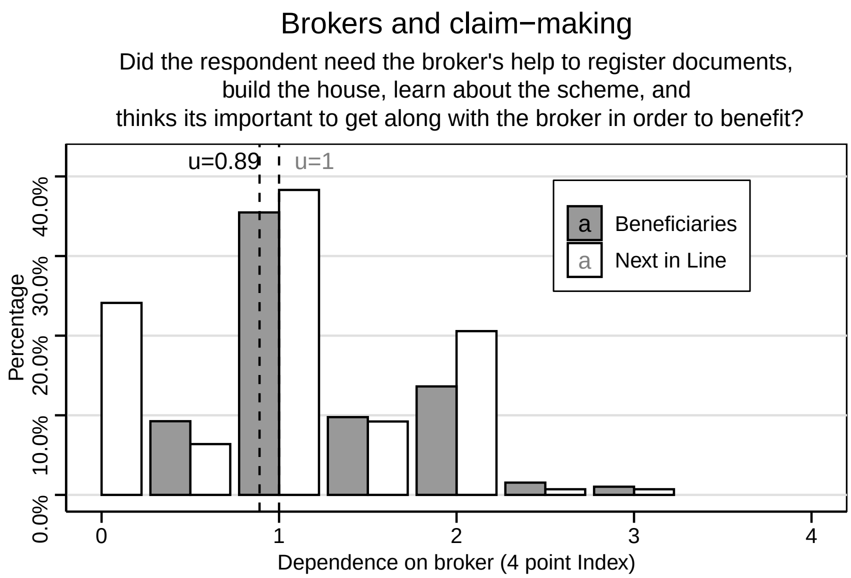
Table 7: Material impact of the housing program

	RD (MSE optimal BW)				RD (BW = 3%)				
Outcome	$\widehat{\tau}$	SE	p	n	$\widehat{\tau}$	SE	p	n	$\overline{Y_{Z=0}}$
Economic insecurity (0-4)	-0.29	0.18	0.10	388	-1.06	0.38	0.01	180	3.66
Skipped a meal in last 7 days (0-1)	0.14	0.09	0.10	313	0.27	0.19	0.16	152	0.28
Monthly income (Rs)	-1605.14	1067.77	0.13	274	-3105.91	1700.80	0.07	152	6902.84
Recent debt (Rs/binned)	2273.17	1552.68	0.14	307	7029.30	3143.90	0.03	152	5897.16

questions about this and created a four-item index: did the respondent need the broker's help to register documents, build the house, learn about the scheme, and thinks it is important to get along with the broker in order to benefit? Figure 4 plots the distribution of responses separately for Dalits to the left of the cut-point ($Z_i = 0$) and right of the cut-point ($Z_i = 1$). What is immediately clear is that Dalits do not depend much on the broker to benefit from this program. On average, in the control group, they report needing the broker's help on 1 item. Dalits to the right of the cut-point need help with 0.89 items. The difference at the cut-point is not statistically significant.

Next, I evaluate if the broker gets political credit for the program. I ask respondents whether the local politician (*mukhya*) will benefit from the fact that houses were built in the village in the next *panchayat* (village level) elections. Table 8 shows that between 34 to 42% believe the broker would electorally benefit from the program. To probe this further, I ask respondents how much discretion they think the broker has in the distribution of houses. For brokers to get any meaningful political credit, people would need to think they control the distribution of benefits. Table 9 suggests otherwise: roughly a fifth of respondents (17% to the right of the cut-point and 22% to the left) believe the local leader can ensure only their supporters get a house. Though not an insignificant number, this is on the low side given how widespread patronage, local discretion, and favoritism are in India.

Figure 4: Dependence on brokers



Note: Dependence on brokers is measured using a four component index. Higher values signify greater dependence on the broker for claim-making. Gray bars show the distribution of responses for Dalits offered a house (beneficiaries, to the right of the cut-point). White bars show the distribution of responses for Dalits next in line (notionally "control" group, to the left of the cut-point). The dotted lines show the average response in either sub-group (beneficiaries in black text, next in line in light gray text).

Table 8: Will the mukhya/local politician benefit from the fact that houses were built in the next panchayat elections?

Z_i	Yes (Percentage)	SE	n
0	34.04	4.00	141
1	42.67	2.51	389

Table 9: Did the mukhya ensure only his supporters got a house? (Yes/No)

Z_i	Yes (Percentage)	SE	n
0	21.99	3.50	141
1	17.74	1.94	389

Finally, I explicitly measure the broker's electoral influence. I ask respondents whether they listen to what the local leader says, and vote for whoever their leader says, at the time of elections. Table 10 reports the percentage saying "yes": 0.7% to the left of the cut-point, and 2.3% to the right. In other words, fewer than 3% of respondents seriously consider the broker or local leader's opinion while deciding who to vote for in national elections. All this suggests that the possibility of clientelistic capture is rather weak.

Table 10: At the time of elections, do you listen to what the local leader says, and vote for whoever they say?

Z_i	Yes (Percentage)	SE	n
0	0.71	0.71	141
1	2.31	0.76	389

For clientelistic inertia, we would expect that brokers have political influence over voters but they are disinclined to publicize the program, contact beneficiaries and monitor them because they are excluded from the distribution process, Again, the evidence for this is not strong. As table 10 shows, very few Dalits take their cue from the broker or local leader, while deciding who to vote for in a national election. A large proportion of respondents correctly attribute the scheme to the BJP central government, suggesting there isn't a credit claiming or publicity problem. When it comes to voter contact, table 11 makes it clear that Dalits who were offered a house are not less likely to be contacted than those who were not offered a house. The difference at the cut-point is not consistently in one direction and is statistically indistinguishable from 0. Figure 5 makes the point more clearly: about 45% of those who were not offered a house are contacted by the BJP before elections, and between 43% and 49% of those who are offered a house are contacted by the BJP. To put this into perspective, on average, respondents are contacted by 1.6 parties. Of those contacted by any party, over 95% report being contacted by the benefit-giving party (BJP) or an organizational affiliate (RSS). By comparison, 82% are contacted by Congress, 56% by a coalition partner of the BJP (the JDU), and 77% by an opposition ethnic party (RJD). In other words, the

Table 11: Contact by parties during elections

	RD (MSE optimal BW)				RD (BW = 3%)				
Outcome	$\widehat{\tau}$	SE	p	n	$\widehat{\tau}$	SE	p	n	$\overline{Y_{Z=0}}$
Contacted by parties (Index, 0-7)	-0.188	0.433	0.663	301	0.646	0.868	0.457	152	1.617
Contacted by BJP (0-1)	-0.017	0.106	0.875	299	0.046	0.203	0.823	152	0.447

BJP outperforms all other parties in voter contact, and its contacting effort does not vary at the cut-point. Taken together, there does not seem to be any evidence of clientelistic inertia.

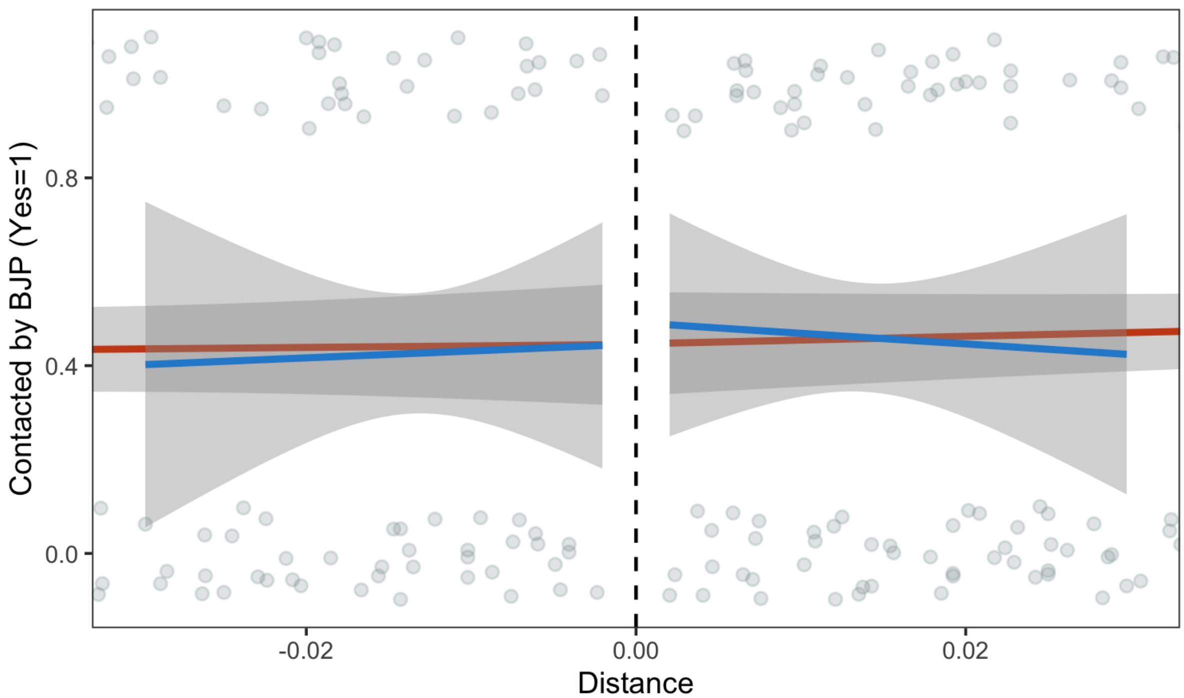


Figure 5: The figure shows a regression discontinuity plot where the outcome is whether the BJP canvassed the respondent (Yes= 1, No= 0). The figure zooms-in on data around the cut-point (±3%), and shows estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth (±3%) and triangular kernel (in blue). 95% confidence intervals are depicted in gray.

Ethnic prejudice

A material benefit may not shape political preferences if an ethnic consideration, like prejudice towards Muslims, is more salient in vote giving. We know that Araria, Darbhanga, and Katihar have a sizable Muslim population, and Dalits can be mobilized on an anti-Muslim platform. To test for this, I measure prejudice against Muslims using the dictator game. Respondents play three rounds of the game, each with 10 rupees. In the first, anonymous round, they see an ethnically ambiguous photograph of a recipient. In the remaining two rounds, the enumerator shares the recipient's name, age, and occupation in addition to the recipient's photograph. The recipient's name cues religious identity, as does their skull cap. I randomize the order in which respondents play with a Hindu and Muslim receiver.

On average, respondents give an anonymous receiver 4.38 rupees, keeping Rs. 5.62 for themselves. They give Rs. 4.13 to a Hindu receiver, and Rs. 3.75 to a Muslim receiver. The discrimination against Muslims is statistically significant: $\mu_{\text{Hindu-Muslim}} = 0.37$ rupees ($t = 2.31, p = 0.02$) when there is a Hindu recipient; and $\mu_{\text{Anon-Muslim}} = 0.63$ rupees ($t = 3.91, p < 0.01$) when there is an ethnically ambiguous recipient. In appendix I, I compare the difference in giving to Hindu and Muslim recipients. Dalits in the control group, on average, give the Hindu recipient 67 paisa more than the Muslim recipient. Among beneficiaries, the average penalty is 27 paisa. There is no statistically significant difference in the penalty at the cut-point (see top panel in figure 16 in appendix I).

All this suggests ethnic prejudice does exist, even among lower caste Hindus who have traditionally not been part of BJP's ethnic core. However, considering its monetary value, the prejudice appears weak. While I cannot rule out its role, it is unlikely to overwhelm other determinants of political behavior and preferences.

Low satisfaction or misattribution

There are two program-specific explanations for no difference at the cut-point: low satisfaction with the program, and misattribution. I am able to rule out low satisfaction: 91% of those that get a house report being satisfied with it, only 13.6% have any difficulty getting money for the house, and under 20% report paying any harassment bribes or facilitation fees in the entire process. Anecdotally, these exceed local expectations and suggest above-average satisfaction with the program.

I can also rule out misattribution. Logically, if Dalits to the right of the cut-point don't know who runs the housing program, or incorrectly attribute it to some other political party, they may not reward the governing party. However, over 70% of them correctly attribute the scheme to the BJP government. An additional 15% say it is jointly run by the national government and state government (credit sharing). Only 2-3% credit the state government alone, and between 5 and 10% don't know who runs the program. The distribution of responses is very similar to the left and right of the cut-point.

	Who runs the housing program?	
	$Z_i = 0$	$Z_i = 1$
Both governments	0.15	0.148
Don't know	0.05	0.106
Modi government (national)	0.78	0.710
Nitish government (state)	0.02	0.035

Table 12: The table reports the proportion of respondents who think the housing program is run by the national government (colloquially, "Modi government"), state government ("Nitish government"), both governments, or don't know who runs it.

Anticipation effects

Finally, a research-design flaw can also explain why Dalits to the left and right of the cut-point have similar political preferences. If respondents next in line are aware that they are imminently going to benefit from a program, they may respond to survey questions factoring this information. This will inflate estimates just below the cut-point, and reduce the difference at the cut-point.

I included a number of survey questions to detect this possibility. For respondents to anticipate receiving benefits, it must be the case that: (i) they know of a list according to which houses are distributed; (ii) they know they are on that list; (iii) they know their rank on that list (this is necessary in order to know ones position relative to the cut-point); and (iv) they expect to get a house in the next few months.

I do not find evidence to corroborate such an explanation. For starters, only 9% of those below the cut-point and 17% above the cut-point know of the beneficiary list (see top panel in figure 17). As figure 17 and table 13 show, people next-in-line ($Z_i = 0$ or left of the cut-point) are less likely to know of the beneficiary list compared to those who are offered a house ($Z_i = 1$ or right of the cut-point). An even smaller proportion of subjects on either side of the cut-point think they are on the list. 14% of beneficiaries and 6% of those next in line believe they are on the list. An extremely small proportion of the sample know their rank on this list. 1% of beneficiaries and 2% of those next in line claim to know their rank on the list. Clearly, there isn't sufficient programmatic knowledge to develop expectations about getting a house in the imminent future.

Nonetheless, I explicitly measure such expectations as well. Figure 17 in Appendix J shows that a relatively small proportion expect to get a house in the next few months, and that these expectations are not correlated with proximity to the cut-point. Among those next in line ($Z_i = 0$), 21% (se= 3.5) think they will get a house in the next few months. For comparison, 20% (se=2.76) of untreated subjects to the right of the cut-point (essentially "never takers") expect to get a house in the next few months.[16] The bottom panel in figure 17 shows that expectations about getting a house are not correlated with distance from the cut-point. Dalits who are far away from the cut-point are just as likely to think they will get a house in the next few months compared to those near the cut-point. This is the case for a variety of specifications. In other words, it seems unlikely that expectations about getting a house are driving down differences at the cut-point.

[16] A caveat here: we are comparing "never takers" on the right side of the cut-point ($Z_i = 1$) to a mix of "compliers" and "never takers" on the left side of the cut-point ($Z_i = 0$).

Conclusion

These findings contribute to the literature in several ways. I critically evaluate the notion that programmatic distribution blunts the logic of ethnic voting, namely the voter's belief that an in-group politician or ethnic party is more likely to deliver benefits than an out-group politician or non-ethnic party. I find that distributive reputations built at the national level because of programmatic targeting do not spillover into the local context, where ethnic considerations continue to shape voters' perceptions of distributive intent. Even an expensive benefit like a house, delivered programmatically, fails to "undo" the distributive salience of ethnicity. I also show that voters do not have to personally benefit from a government program for electoral preferences to change. Finally, I employ a novel and principled research design. I use fieldwork and interviews to identify a naturally occurring regression discontinuity design, then collect data around the cut-point in a principled way, pre-registering hypotheses, alternative explanations, a sampling strategy, enumeration and non-contact protocol, survey measures, coding of those measures, estimation strategy, and ex-ante design tests.

Substantively, this paper proposes two mechanisms through which material benefits can shape political preferences. Gratitude seems to drive the preferences of Dalits who were offered a house. They recognize that the BJP has done something for them, display greater awareness of the programmatic features of distribution, and are more likely to think some people voted for the BJP out of gratitude. For Dalits next in line for benefits, sociotropic considerations play an important role. Even though they do not personally receive the benefit, their social network is visibly saturated with that benefit. These voters also think their ethnic group's condition has improved in the last five years, and that people like them have benefited under a BJP government. Together, this explains why there is no increase in support for the benefit-giving party at the cut-point *and* very high support for it across the board. For robustness, I evaluate several alternative explanations, ranging from low satisfaction with the program, misattribution of credit, clientelistic capture or inertia, overriding ethnic considerations, short-term financial shocks, and anticipation effects at the cut-point.

Strikingly, the national program's success and reputational dividends for the benefit-giving party do not travel to the local political context. Here, the benefit-giving party is considerably less popular, and ethnic considerations shape perceptions of distributive intent for a majority of respondents. For instance, in the Choose Your Dictator (CYD) game, Dalits on either side of the cut-point pick the co-ethnic politician over half the time; and Dalits who are offered a house are less likely to pick the BJP-cueing politician in the profiled version of the game.

What does all this mean for future research? If governments do not need to deliver benefits to *every* voter in a pivotal group, is there a *saturation threshold* above which a benefit-giving party can obtain further support without distributing benefits? Is that threshold high? Does the benefit's value, visibility, distribution, and credit claiming determine that threshold? Future research can look at these interesting questions. Relatedly, why do voters update their beliefs about the BJP's distributive intent in the national context but revert to ethnic considerations in the local context? Does this create an incentive for parties to adopt a mixed strategy? And do institutional features, like multiple tiers of government, contain reputational spillovers and preserve the role of brokers and ascriptive identities?

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Appendices

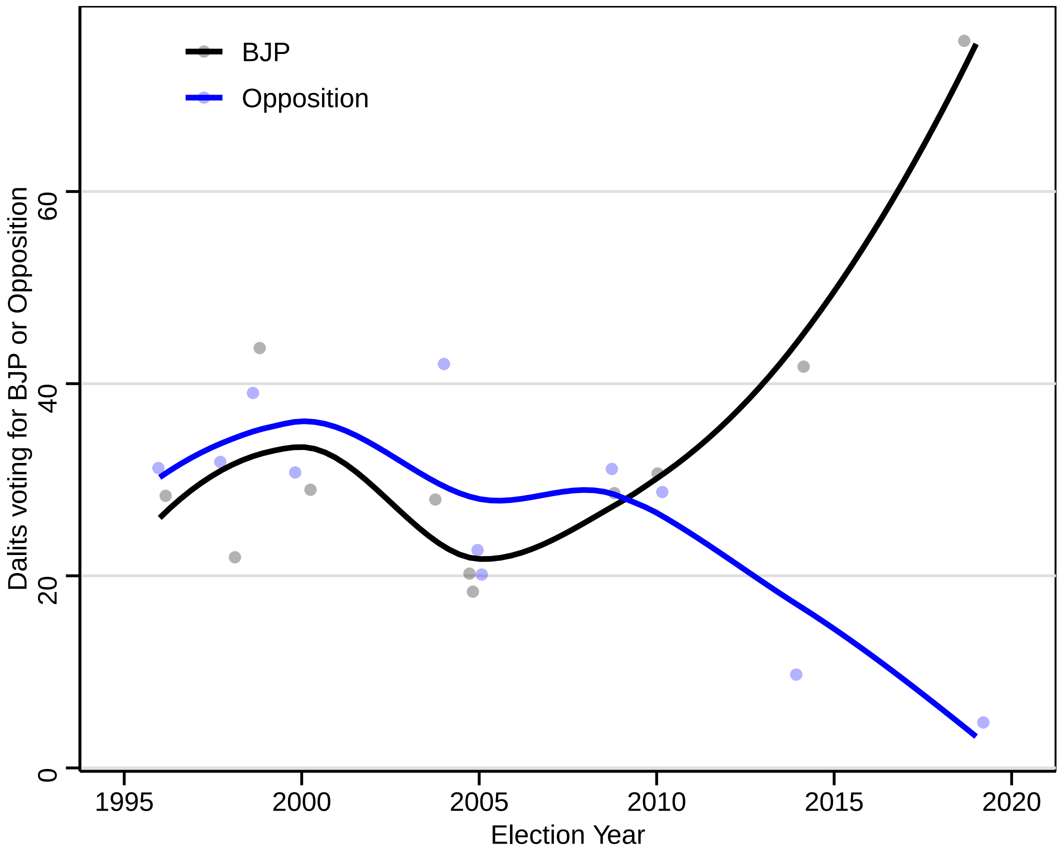
Shikhar Singh

October 26, 2021

Contents

A Dalit Vote in Bihar

Figure 6: Dalit Vote in Bihar (1995-2019)



Note: Each point shows the percentage of Dalits that voted for the BJP (in black) or the opposition alliance (in blue) in an election. The solid trend lines capture over-time variation in group support for a party. Data from post-poll surveys conducted by the Center for the Study of Developing Societies (CSDS), as reported in Kumar (2014) and Ranjan, Singh, and Alam (2019).

B Regression Discontinuity Plots

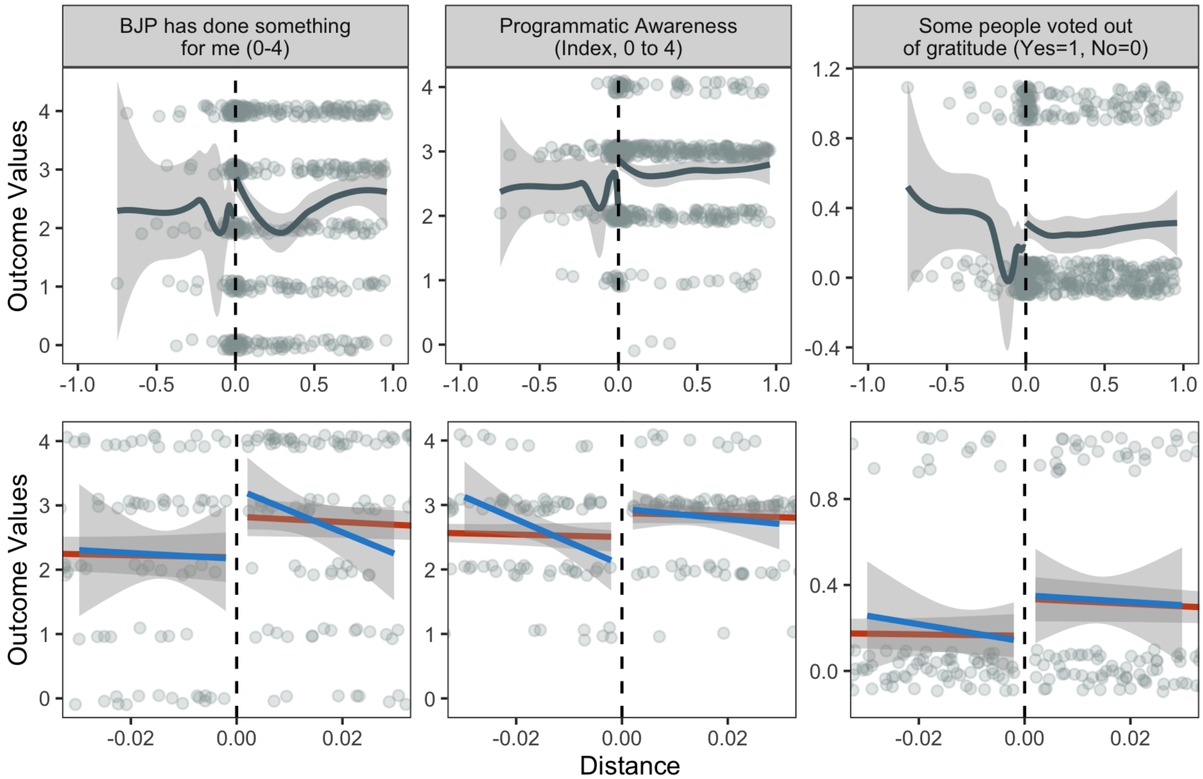


Figure 7: The top panel shows outcomes at different values of the forcing variable. We show the conditional means using a LOESS, with 95% confidence intervals in gray. The bottom panel zooms-in on data around the cut-point (±3%), and shows estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth (±3%) and triangular kernel (in blue). 95% confidence intervals constructed using heteroskedasticity-robust standard errors are depicted in gray.

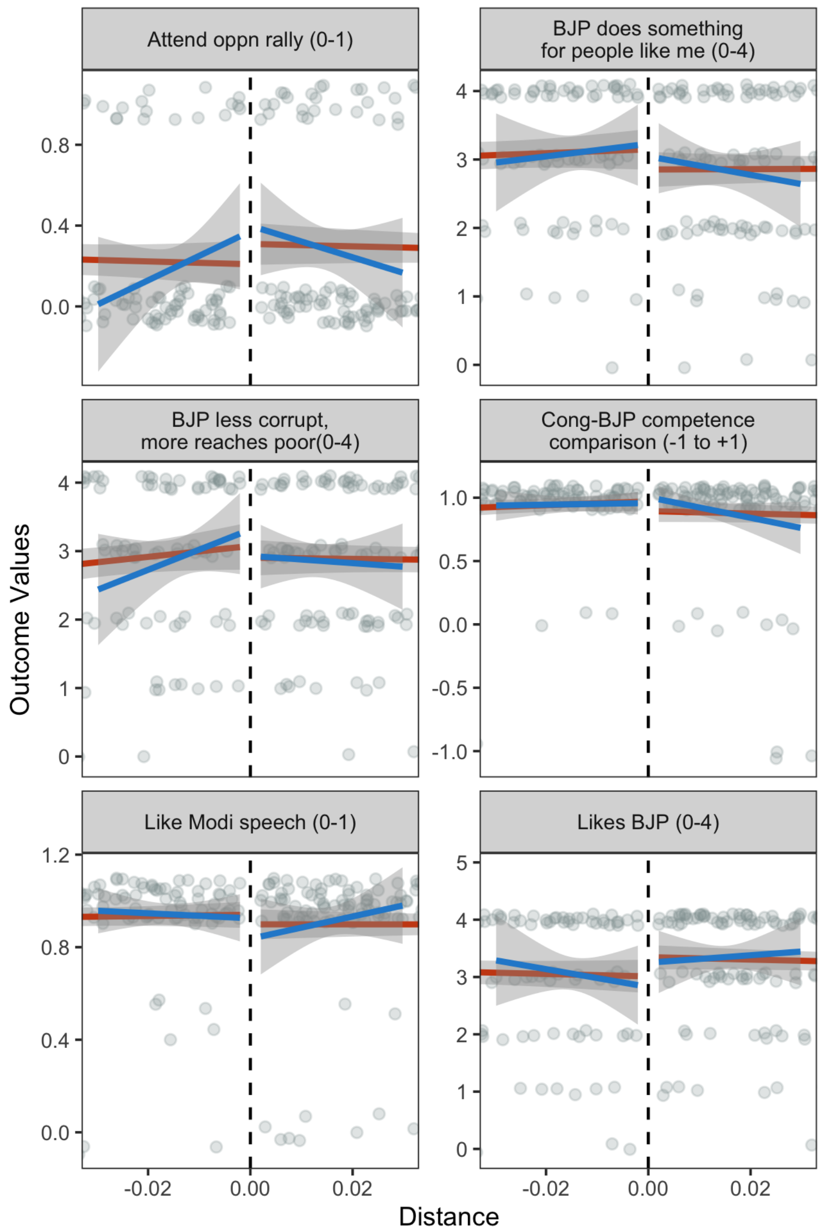


Figure 8: The figure shows outcomes at different values of the forcing variable. We zoom-in on data around the cut-point (±3%), and show estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth (±3%) and triangular kernel (in blue). 95% confidence intervals constructed using heteroskedasticity-robust standard errors are depicted in gray.

C Specification Curves

The figures below report the difference at the cut-point for three outcomes under various specifications. The specifications employ different data-driven bandwidth selection procedures included in `rdrobust` package, polynomial specifications, and kernels (`triangular`, `epanechnikov`, and `uniform`).

The following bandwidth selection procedures are used: manually selected and pre-registered bandwidth of 3%, one common MSE-optimal bandwidth selector (`mserd`), two different MSE-optimal bandwidth selectors (`msetwo`), one common MSE-optimal bandwidth selector for the sum of regression estimates (`msesum`), a selector that picks $min(\text{mserd}, \text{msesum})$, a selector that picks $median(\text{mserd}, \text{msesum}, \text{msetwo})$ for each side of the cut-off separately, one common CER-optimal bandwidth selector (`cerrd`), two different CER-optimal bandwidth selectors (`certwo`), one common CER-optimal bandwidth selector for the sum of regression estimates (`cersum`), a selector that picks $min(\text{cerrd}, \text{cersum})$, and a selector that picks $median(\text{cerrd}, \text{certwo}, \text{cersum})$ for each side of the cut-off separately.

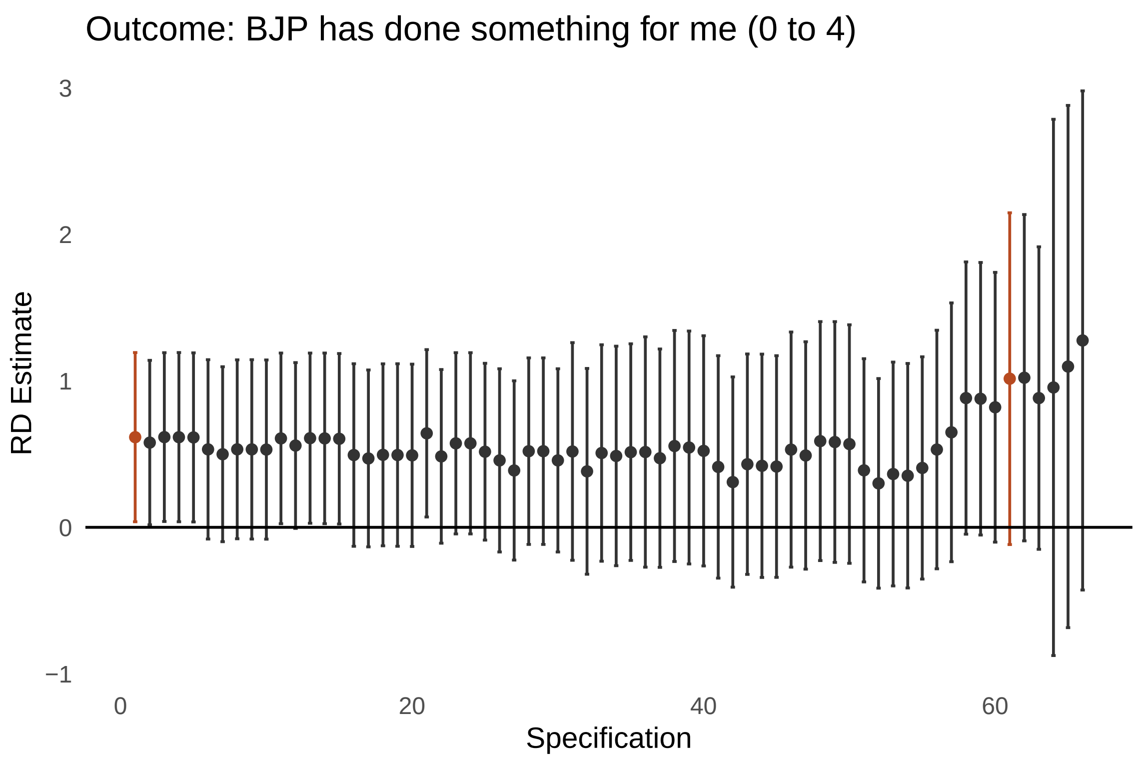


Figure 9: The figure reports the difference at the cut-point and 95% confidence intervals produced by `rdrobust` under different specifications, with the pre-registered specification in red.

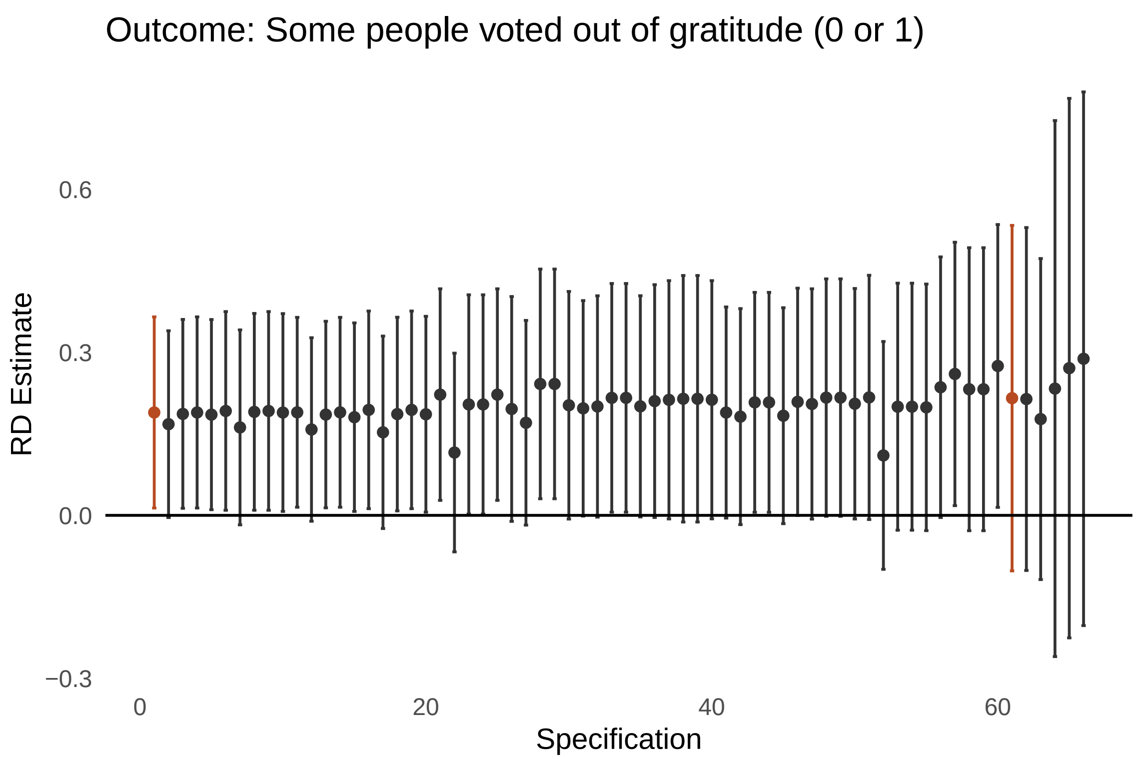


Figure 10: The figure reports the difference at the cut-point and 95% confidence intervals produced by `rdrobust` under different specifications, with the pre-registered specification in red.

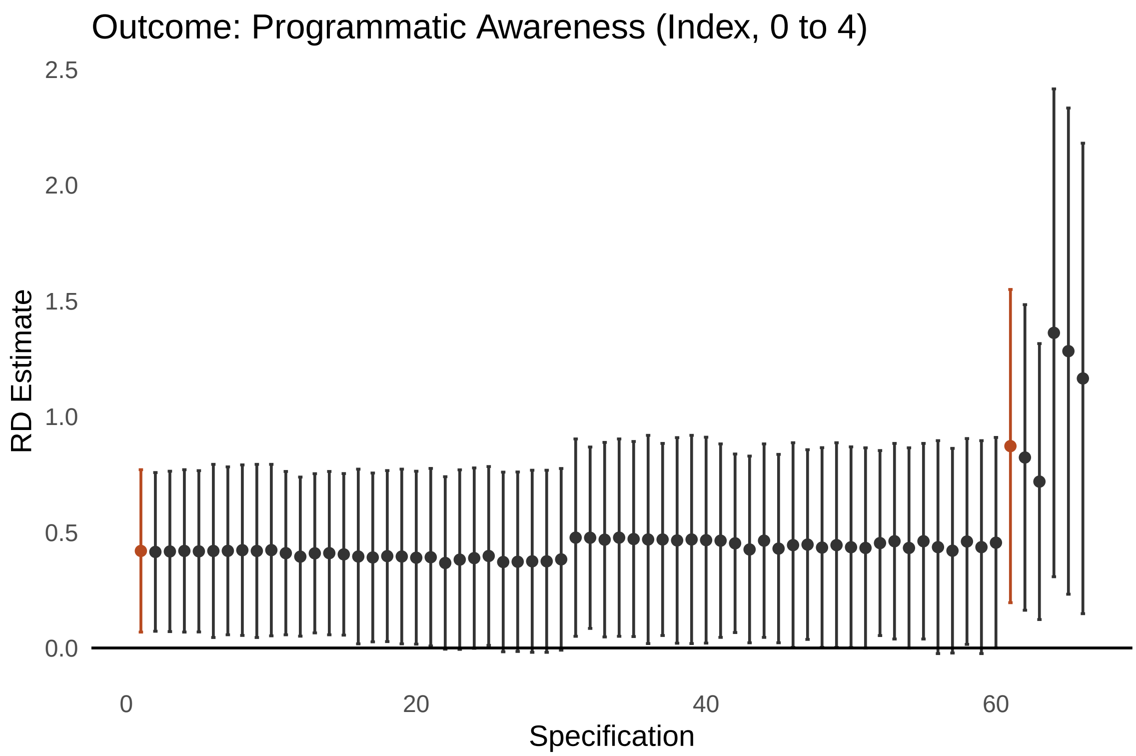


Figure 11: The figure reports the difference at the cut-point and 95% confidence intervals produced by `rdrobust` under different specifications, with the pre-registered specification in red.

D Programmatic Awareness

Table 13: Programmatic Awareness (Index and Components)

		RD (MSE optimal BW)				RD (BW = 3%)				
Outcome	Hyp.	$\widehat{\tau}$	SE	p	n	$\widehat{\tau}$	SE	p	n	$\overline{Y_{\mathrm{Z=0}}}$
Programmatic Awareness (Index)	Pos	0.42	0.18	0.02	295	0.87	0.35	0.01	152	2.55
Know of Program (0-1)	Pos	0.17	0.08	0.03	326	0.21	0.17	0.23	152	0.71
Know of Beneficiary List (0-1)	Pos	0.08	0.07	0.25	330	0.27	0.15	0.07	152	0.09
Ethnic Favoritism (0-1)	Neg	-0.05	0.05	0.24	298	-0.13	0.11	0.21	152	0.03
Broker Discretion Matters (0-1)	Neg	-0.11	0.10	0.27	289	-0.26	0.18	0.14	152	0.22

E Distributive Salience of Ethnicity

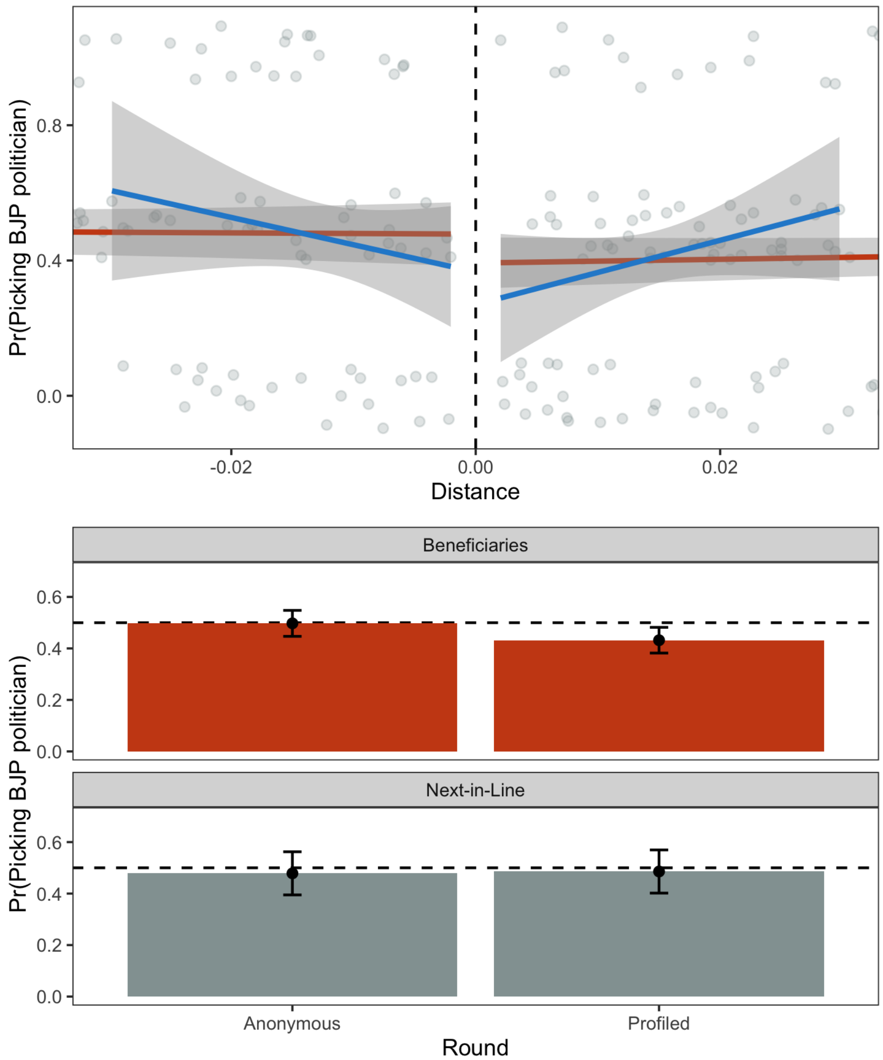


Figure 12: The top panel shows a regression discontinuity plot where the outcome is the probability of picking the BJP cueing politician in the Choose Your Dictator Game. The panel zooms-in on data around the cut-point (±3%), and shows estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth (±3%) and triangular kernel (in blue). 95% confidence intervals constructed using heteroskedasticity-robust standard errors are depicted in gray. The bottom panel reports the probability of picking the BJP cueing politician separately for beneficiaries and those next in line in the anonymous and profiled versions of the behavioral game. The plot includes 95% confidence intervals constructed using heteroskedasticity-robust standard errors.

F Attempts at sorting

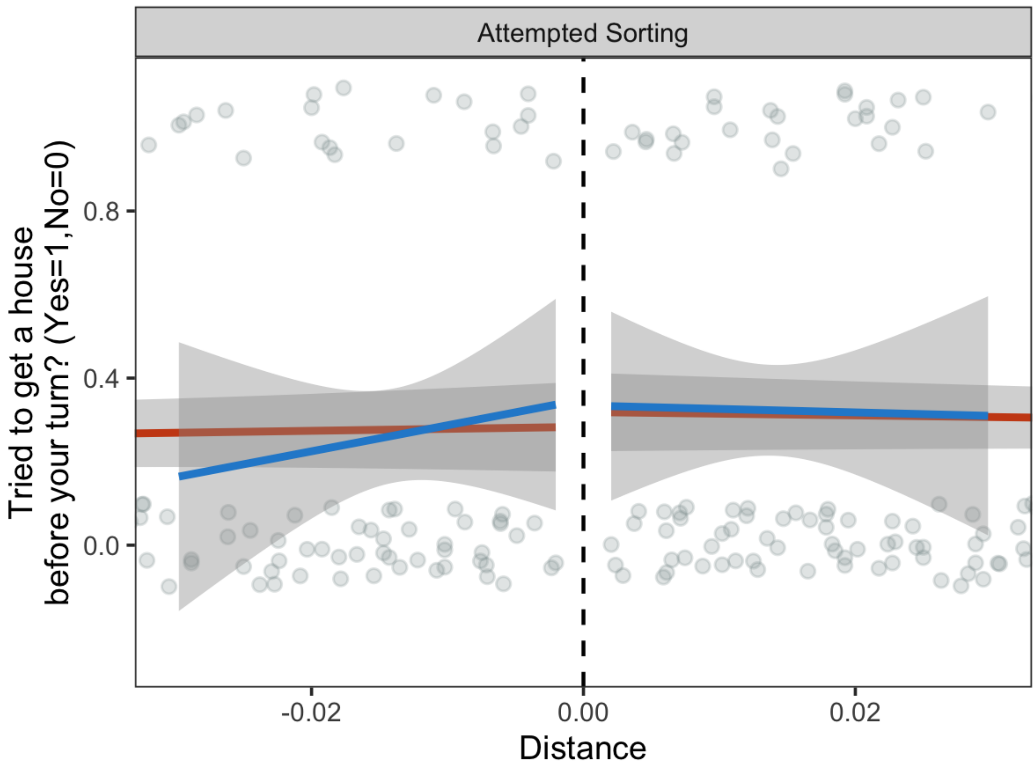


Figure 13: The figure shows a regression discontinuity plot where the outcome is whether the respondent attempted to get a house before their turn. The panel zooms-in on data around the cut-point (±3%), and shows estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth (±3%) and triangular kernel (in blue). 95% confidence intervals constructed using heteroskedasticity-robust standard errors are depicted in gray.

G Reputational Spillovers: National vs. Local

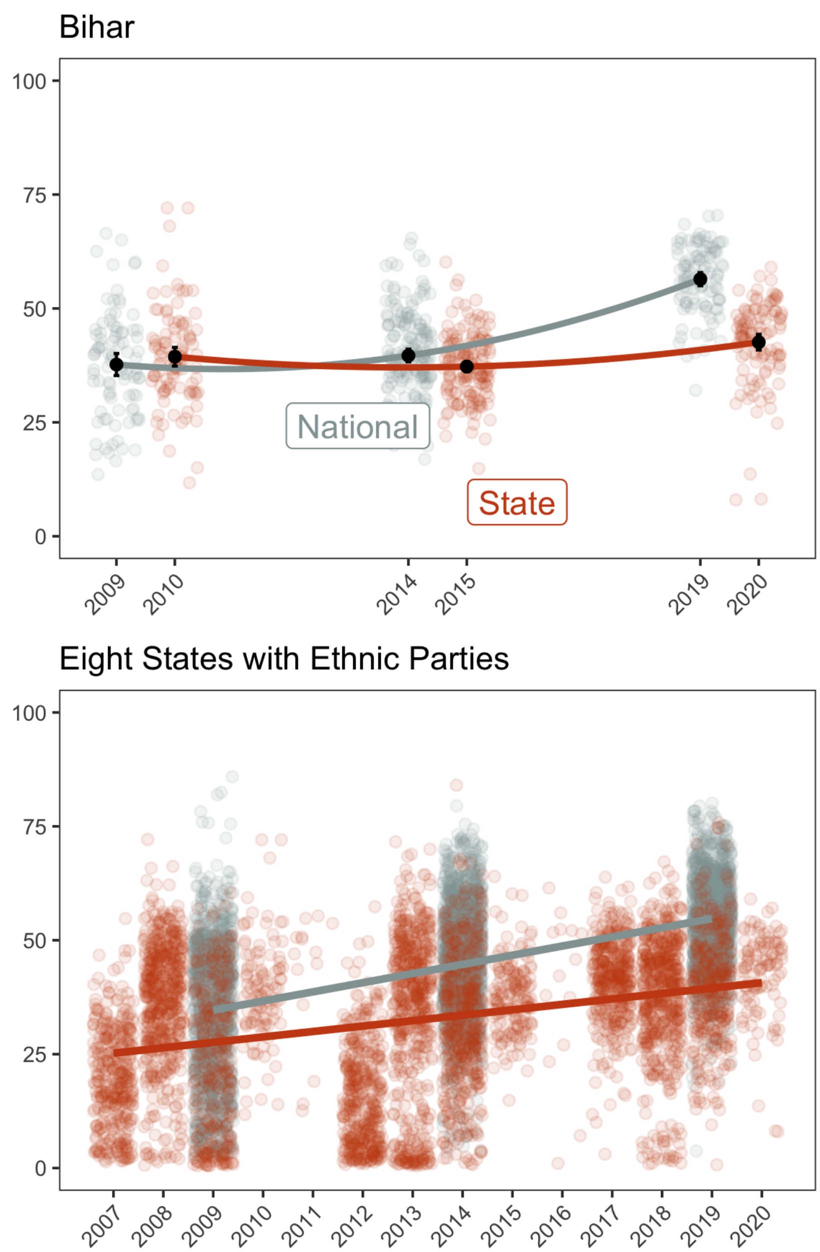


Figure 14: The figure plots the benefit-giving party's vote share at the state assembly constituency level in national elections (gray) and state elections (orange). The top panel presents this information for Bihar province. The bottom panel presents this information for eight Indian states with ethnic parties (see figure 1). The benefit-giving party, BJP, consistently underperforms in state elections compared to national elections. The vote share gap between national and state elections increases over time, in Bihar province and elsewhere. Data for the figure is obtained from India's Election Commission, and Agarwal et al. (2021).

H Exposure to the scheme

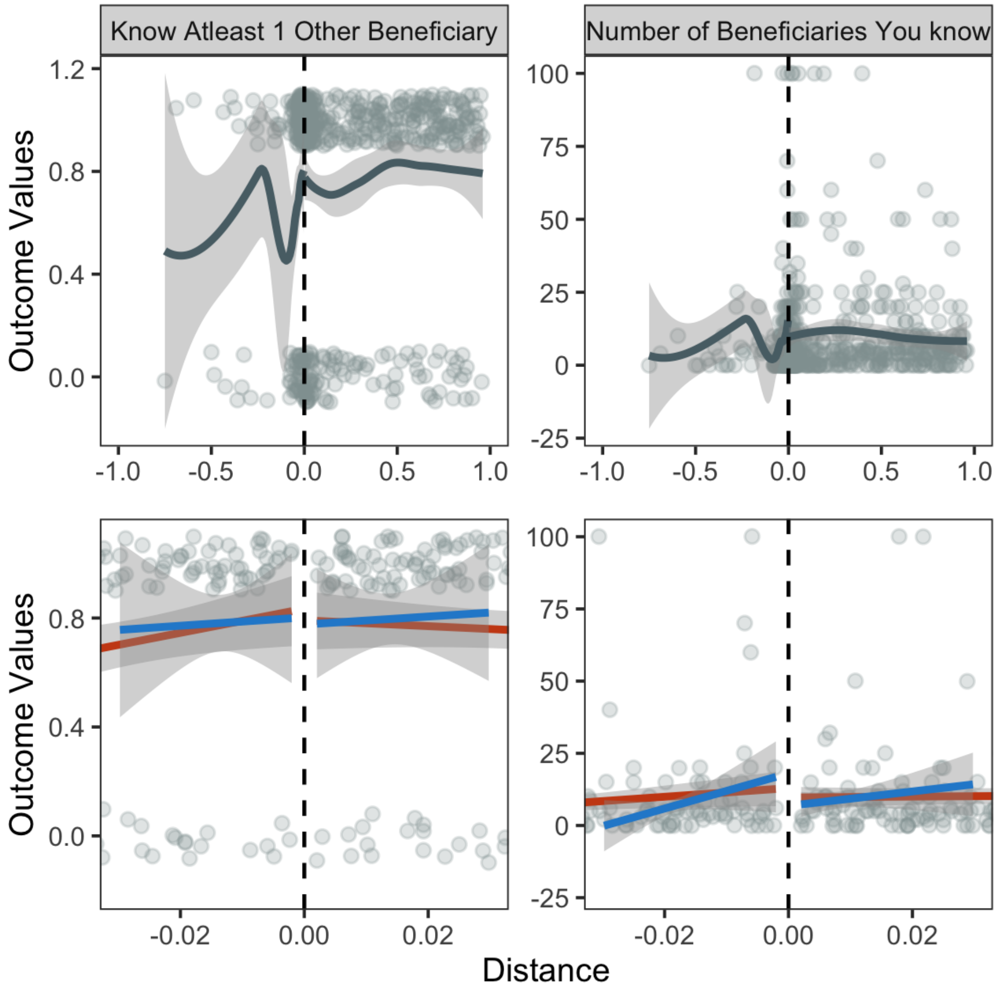


Figure 15: The top panel shows outcomes at different values of the forcing variable. We show the conditional means using a LOESS, with 95% confidence intervals in gray. The bottom panel zooms-in on data around the cut-point (±3%), and shows estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth (±3%) and triangular kernel (in blue). 95% confidence intervals constructed using heteroskedasticity-robust standard errors are depicted in gray. The outcome in the first column is whether the respondent personally knows at least one other program beneficiary (Yes= 1, No= 0). The outcome in the second column is a count of the number of other program beneficiaries known to the respondent. I recode extreme values since they can distort the results. There are four instances of respondents claiming to know more than 100 beneficiaries. I cap these extreme values at the 99th percentile value on that side of the cut-point.

I Prejudice against Muslims

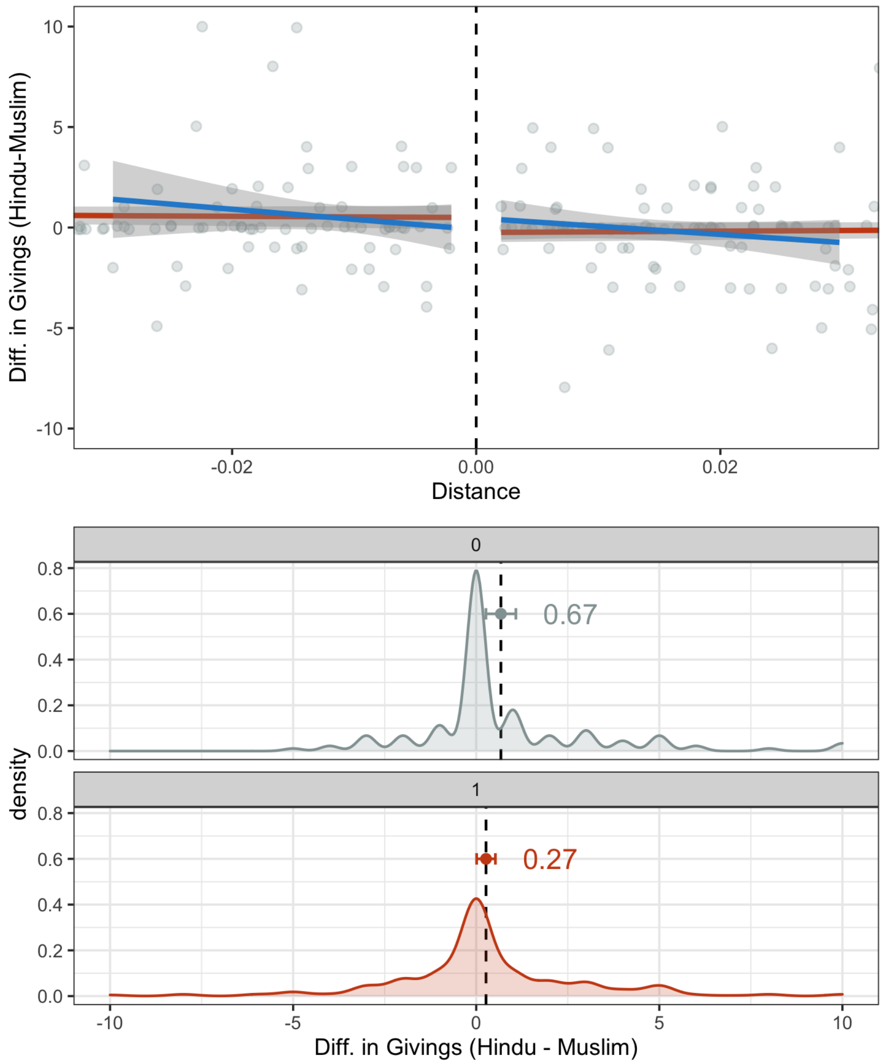


Figure 16: The top panel shows a regression discontinuity plot where the outcome is the difference in giving to a Hindu and Muslim recipient in a Dictator Game. The panel zooms-in on data around the cut-point (±3%), and shows estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth (±3%) and triangular kernel (in blue). 95% confidence intervals constructed using heteroskedasticity-robust standard errors are depicted in gray. The bottom panel shows density plots for the outcome, separately for respondents to the left of the cut-point ($Z_i = 0$) in gray and to the right of the cut-point ($Z_i = 1$) in orange. The dot and dotted line show the average difference in giving, accompanied by a 95% confidence interval of that estimate.

J Anticipation Effects

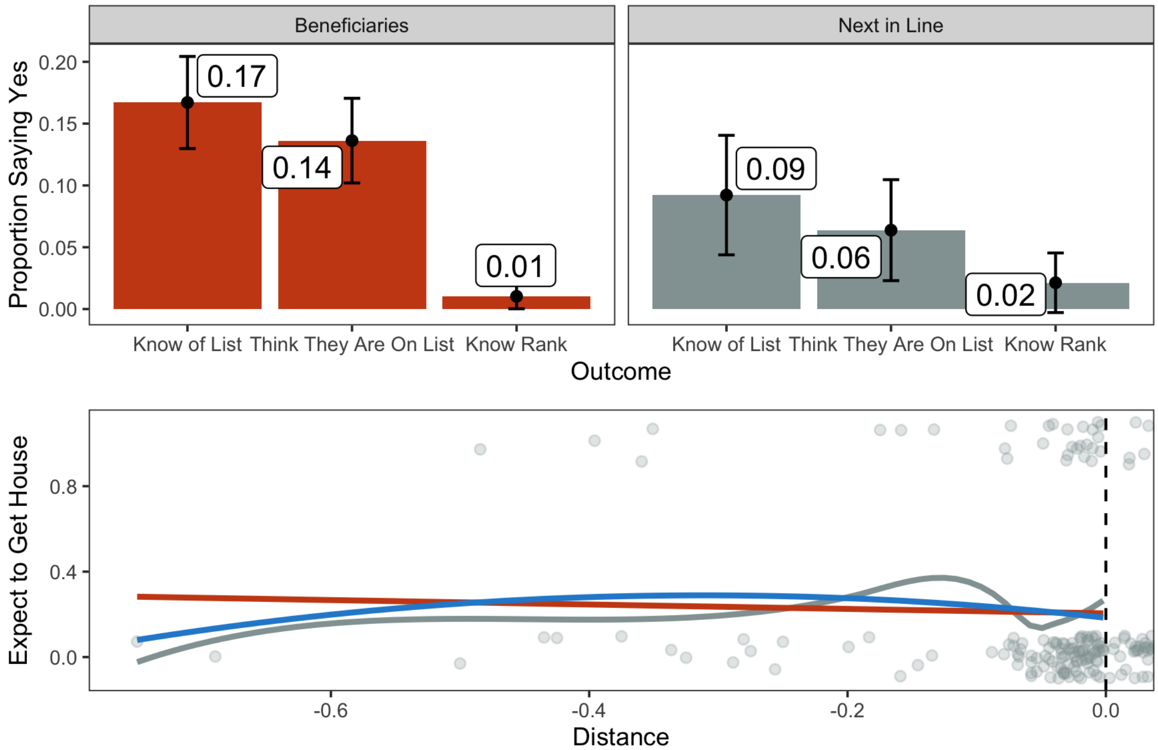


Figure 17: The top panel shows the proportion of respondents that know of a beneficiary list according to which houses were distributed, think that they are on this list, and know their rank on the list. The bar chart includes point estimates and 95% confidence intervals constructed using heteroskedasticity-robust standard errors. The bottom panel plots expectations about getting a house in the near future at different values of the forcing variable. We overlay three summary statistics: a LOESS regression in gray, a linear regression in orange, and quadratic regression specification in blue.