Winning Support by Distributing Houses? Evidence from India

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Abstract

Can an expensive material benefit, delivered programmatically to ethnically opposed voters, 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 typically 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 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, privately funded and provided material benefits can have some impact (Thachil 2014). 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 this 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 beneficiaries were 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.

The study is located in three Muslim dominated districts of Bihar, 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 in these areas, on the other hand, are ethnically cross pressured: when status cleavages are salient they gravitate away from the BJP, but when religious cleavages become salient they tend to side with the Hindu majority.

The identification strategy is a regression discontinuity (RD) design that 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 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.

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

Strikingly, there is very high support for the ruling party across-the-board, and evidence that communities are saturated with the benefit. 70 to 77% respondents *personally* know someone who has got a house, typically between 9 and 16 such people. This points to sociotropic considerations at work. Dalits are forming opinions about the ruling party based on the fact that many people like them got 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 of this, 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 forming 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 pop-

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 broker's 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. No discontinuous change in expectations at the cut-point.

ulous 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 first discuss ethnic dealignments in India, then survey the existing literature, detail my argument and hypotheses, describe the research design, present the main results, and explore alternative explanations.

Why material benefits matter

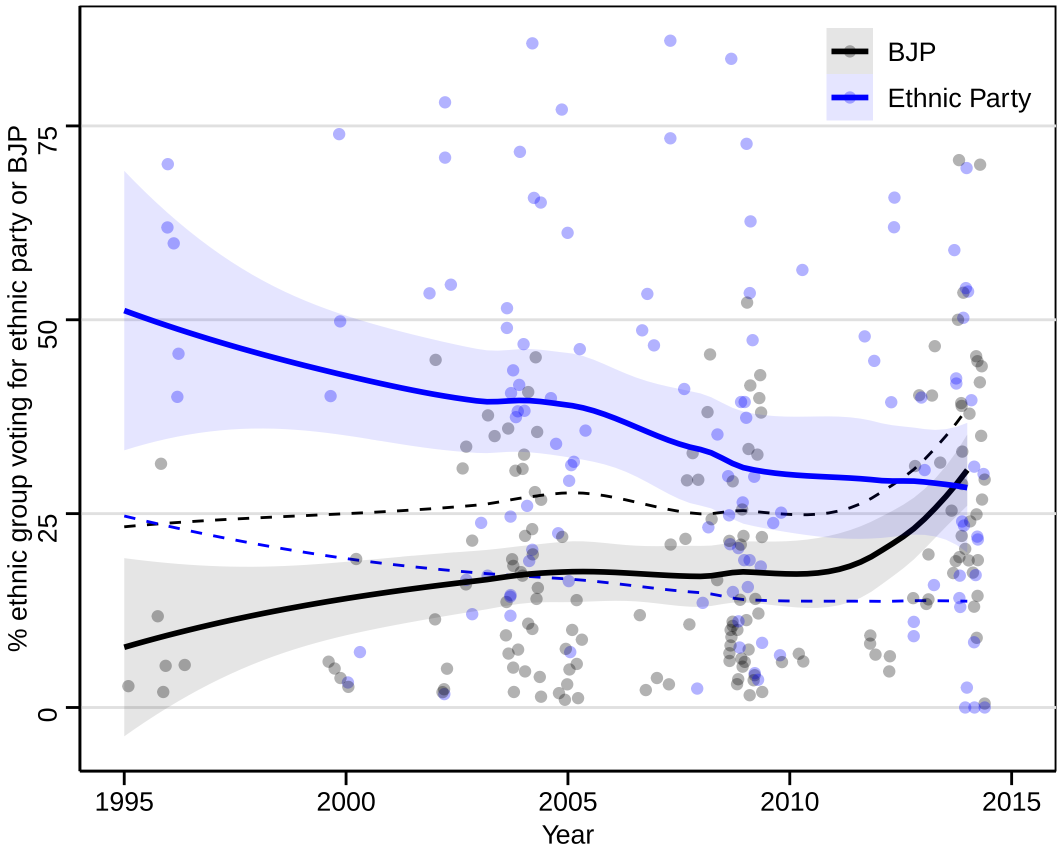
Ethnic dealignments and material benefits

In canonical voting models, material benefits matter because they compensate for ideological distance. It has been shown that the electorally optimal strategy for parties is to target benefits at swing or weakly opposed voters (Lindbeck and Weibull 1987; Dixit and Londregan 1996). This explains why the BJP distributes houses to Dalits in some districts where these communities are pivotal. It does not, however, explain large scale distribution of benefits in a programmatic way to weakly opposed voters and opposition supporters. To understand why, I turn to ethnic voting data in the Indian context.

Figure 1 shows the proportion of an ethnic group voting for *its* ethnic party and the BJP in national and state level elections since 1995. These are, what Thachil and Teitelbaum (2015) call, "narrow ethnic parties" that follow "patronage-based strategies *within* their restricted ethnic cores"(Thachil and Teitelbaum 2015:1394).[2] Two things stand out: first, ethnic parties, even at the height of their electoral relevance, only managed to mobilize a little over half the votes in their community; and second, there is a continuous decline in support for nine well-known ethnic parties between 1995 and 2014, and a commensurate increase in support for the BJP. These stylized facts have some interesting implications. First, contrary

[2] In contrast, "encompassing ethnic parties" mobilize broader identities and are more likely to engage in programmatic distribution. They are not the subject of discussion here.

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).

to what theories of ethnic voting predict, ethnic consolidation is never *complete*. There is always a sizable number of votes up for grabs, incentivizing the entry of other political players. Second, ethnic party decline is not due to erosion in support among peripheral groups but hollowing of the core base. As the figure suggests, BJP seized this opportunity and reached out to new social groups, often using material benefits in exchange for political support.

It is in this context that I study the impact of material benefits. This paper focuses on swing or weakly opposed voters, and shows that their preferences can change even when they are not offered a benefit but "many people like them" (co-ethnics) get that benefit.

Existing literature

In the distributive politics literature any discussion of material benefits starts with the voter's utility function. This conceptualization 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. The idea is that 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, they compensate for ideological disutility. The literature refers to this as *persuasion*. In practice, the distribution of benefits achieves a combination of these things. 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, material 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.

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), citizens need them to make claims with the state (Kruks-Wisner 2018), and parties use them to mobilize votes in elections. These local leaders fight for public goods, have credibility and influence in the neighborhood, which they use to shape political preferences (Auerbach 2016). 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.[3] 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" leading to Dalits and tribals voting for the BJP (Thachil 2014).[4] We 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 (Alesina, Baqir, and Easterly 1999; Alesina and LaFerrara 2005). Access to the benefit might be conditioned on ethnicity (Marcesse 2018). Ethnicity can shape how people process information about politicians' 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,

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

[4]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.

and Pasquale Forthcoming; Kramon and Posner 2016; Posner 2005). Ethnic networks can also facilitate political accountability and provide enforcement mechanisms (Habyarimana et al. 2009; Miguel and Gugerty 2005; Chandrasekhar, Kinnan, and Larreguy 2018).[5] In these ways, ethnicity can moderate or mediate the impact of a material benefit on political preferences.

Expectations in the Indian case

To better understand when material benefits affect political preferences, we study a housing program in three districts of Bihar: Araria (which is 43% Muslim), Katihar (44.5% Muslim), and Darbhanga (22.4% Muslim)[6]. I focus on Dalits, who are ethnically cross-pressured, and electorally pivotal to varying degrees. In Araria and Katihar where Muslims are numerous, Dalits tend to be swing voters. The BJP needs to consolidate Hindu votes in order to win, and it makes electoral sense to distribute benefits to Dalits. In Darbhanga, Muslims are not as numerous, and voters are split along caste lines. There is a history of caste antagonism and violence, and the BJP champions the interests of high status groups. Here, Dalits are not as pivotal, and primarily opposition voters or weakly opposed voters.

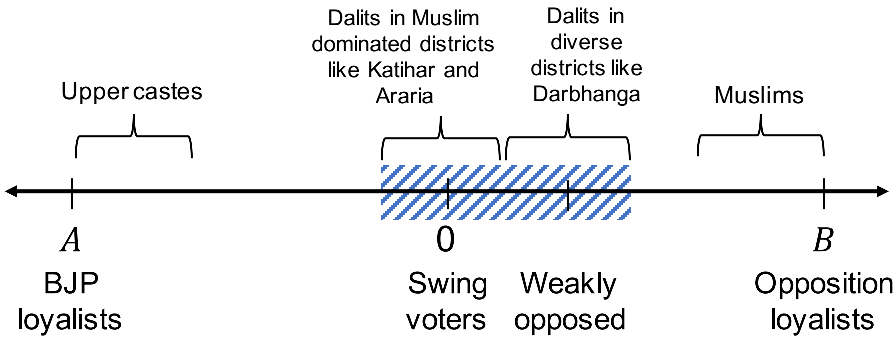
To explain all this, figure 2 arranges ethnic groups in ideological space, ranging from support for majoritarian or Hindu nationalist ideas to support for secularism. BJP loyalists (high status groups) populate one extreme of this dimension ($\sigma_i = A$), opposition loyalists (Muslims) the other extreme ($\sigma_i = B$), with swing voters in the middle ($\sigma_i = 0$). In Araria and Katihar, Muslims are so numerous that Dalits occupy center stage. In ethnically diverse Darbhanga, Dalits can be considered weakly opposed to the BJP. The shaded area represents the set of voters we study in this paper.

The general expectation is that a high value benefit like a house, b_i, will more than

[5]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).

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

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.

compensate for the ideological disutility to Dalits from supporting BJP. Accordingly, I hypothesize:

H_1 Dalits who are offered a house will evaluate the BJP more positively, and be less likely to engage in costly collective action against it, compared to those next in line to be offered a house.

The intuition behind this claim is that benefits not only decide votes but also how a voter thinks about the party, and how motivated they are support the opposing party or join costly collective action against the party giving the benefit. To this end, I measure perceptions about the BJP's distributive intent (agreement with statements like "I have benefited from the BJP government" and "people like me will benefit from a BJP government"), support for its distributive message (reacting to Prime Minister Modi's speech), competence relative to the previous Congress government, perceptions about corruption and inclusive development, likability of the party and its leader. I also measure voter transferability - does getting a house from the BJP increase the chances of voting for a BJP ally? As far as opposition to the BJP is concerned, I ask respondents if they would attend an opposition party's election rally, and whether they think any opposition party or leader can defeat the BJP if elections were held in the next six months. Finally, I also ask respondents if they think some people voted for the BJP because they got a house. In other words, did a feeling of gratitude or indebtedness drive political preferences? My pre-analysis plan describes each of these outcomes, the associated survey measures, and 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.

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. 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[7]. 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.

[7]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.

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 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 Dalits in Muslim dominated areas vote on religious grounds. 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 learnt 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, by then 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. My pre-analysis plan gives a step-by-step description of the implementation process based on bureaucrat interviews 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 with granular information could not ex-post manipulate the ranking. Once again, my pre-analysis plan documents reasons for the plausibility of the design, along with qualitative evidence, and where possible, empirical tests.

With this in mind, let the substantive quantity of interest be:

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

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 were offered a house from those who were not, I define the estimand as 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 individuals 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 everyone within a pre-registered bandwidth around the cut-point, and draw a random sample of people outside that bandwidth. The decision involved 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 were, of course, subject to budgetary constraints.

Following Manacorda, Miguel, and Vigorito (2011), I picked a bandwidth of 1.5% for Muslims and 3% for Dalits. Their study in Uruguay picked a bandwidth of 2%. I use a more conservative bandwidth for Muslims, but a larger bandwidth for Dalits since there are fewer of them in the beneficiary list.

I picked n_v and p_v by calculating the cost of conducting a survey in n_v villages[8], interviewing everyone 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 maximized the number of subjects within the bandwidth. For Dalits, this yielded the following rule: visit 60 villages, interview everyone within the bandwidth ($\pm3\%$), and 10% of people outside the bandwidth.

My field team informed me that non-contact in such surveys is typically 40%. To prepare for this eventuality, I identified a replacement sample before going into the field. I oversampled outside the bandwidth (1.5 times p_v), and picked people 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 survey team was able to interview 530 of them. This put the contact rate at 63.7%, marginally above but broadly in line with our expectations. More details about the sampling strategy are available in the pre-analysis plan and supporting documents.

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

[8]Removing villages that did not have (for Dalits) 3% treated and untreated subjects, and arranging them in descending order of untreated subjects.

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[9], 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 6 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

[9]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.

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 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 balance on the three census variables, and four background characteristics collected in the survey (gender, age (binned), education, migrant status).

Table 3 reports the estimate of the difference at the cut-point ($\widehat{\tau_{RD}}$), the standard error, associated p value, and effective sample size (n). I do not detect any discontinuous shifts in these variables in the survey sample ($n = 530$ households), whether those are census or survey variables, and whether we use an MSE-optimal bandwidth or force a 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, index formation, and RD estimation were pre-registered. The primary specification uses: (a) a first-order polynomial, (b) triangular weights, (c) MSE optimal or pre-registered bandwidth (3% for Dalits), and (d) clustered standard errors if there is outcome data for more than one member of a household. 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 narrow, pre-registered bandwidth of 3%.

Results

Main outcomes

I capture political preferences using a variety of attitudinal and behavioral measures. Table 4 reports the difference at the cut-point ($\hat{\tau}$) for Dalits that have been offered a house, and those next in line. 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.

Focussing on the first three rows, we see 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 asked to put coins on the ground 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

Table 4: Primary Outcomes Analysis

		RD (MSE optimal BW)				RD (BW = 3%)				
Outcome	Hyp.	$\widehat{\tau}$	SE	p	n	$\widehat{\tau}$	SE	p	n	$\overline{Y_{Z=0}}$
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 $\overline{Y_{Z=0}}$).

put 2.25 coins. Those offered a house put an additional 0.6 to 1 coin.

There is also evidence of gratitude voting in elections. 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 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.

Next, I look at programmatic awareness. This is of interest because the housing program identifies beneficiaries using objective indicators of poverty from the census, and minimizes broker discretion. Did beneficiaries, or those next in line, perceive this as programmatic distribution? I use four survey questions to get at this. 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. 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 units increase in programmatic awareness. Appendix A 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. Predictably, 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 also more likely to know about the beneficiary list. Crucially, they seem less likely to think there is ethnic favoritism and broker discretion in the distribution process.

Putting these pieces together, we can say that when someone is offered a house, they recognize the BJP has done something for them, they are more likely to think people voted for the BJP because they got a house, and are more aware of programmatic features. Despite all this, the program fails to move political preferences at the cut-point. In the remaining rows of table 4 we see that Dalits who were offered a house are no more supportive of the

BJP than those next in line.

Dalits on either side 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. Next, 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), this is cheap talk (0.5), or he is misleading people to get votes (0). Modi's distributive message has 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 can be down to a ceiling effect because baseline support for Modi's message is extremely high. I also 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. I ask if they like 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.

Next, I ask respondents to compare BJP and Congress governments. I code responses as $+1$ if someone thinks the BJP government is better, -1 if the previous Congress government was better, and 0 if both are the same. Once again there is very high support for the BJP in the control group with an average response of 0.91. The point estimate of the difference at the cut-point flip-flops, and is statistically insignificant in both cases. On corruption, I ask respondents how much they agree with the statement, "BJP is less corrupt, and more reaches the poor [in BJP governments]". 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. If anything, the difference at the cut-point is negative, though statistically insignificant. On 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 (average response of 0.84, on a -1 to $+1$ scale). The difference at the cut-point is inconsistently estimated: negative and statistically significant in one case, positive and insignificant in the other.

I look at whether support for the BJP spills over to other parties that are allied with it, and less antagonistic towards Dalits and Muslims. Nearly 88.7% Dalits in the control group say they would vote for the Janata Dal (United), a BJP ally that runs the state government. Support for this ally does not change at the cut-point. The difference at the cut-point is negative and significant in one specification, positive and insignificant in the other.

Finally, I check whether the housing program makes it less likely for voters to engage in costly collective action against the BJP, and enhances perceptions of the BJP's electoral invincibility. 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. Dalits also seem to think the BJP is electorally invincible. Only 12% in the control group think an opposition party or leader can defeat the BJP if elections are held within the next six months. Dalits who are offered a house respond in pretty much the same way.

So far I have shown that the BJP is exceptionally popular at the national level. Does this reputation travel to local politics? More specifically, does the housing program generate reputational spillovers? 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 upper caste politician cueing affiliation to the BJP (as an example, see figure 3). I use ethnically ambiguous photographs for this game, with the politician's name cueing ethnicity and a saffron *gamcha* (scarf) or

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.

tilak cueing partisan affiliation.[10] 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 supposedly knows the respondent's name (ethnic cue), age, and occupation while deciding how to split 10 rupees with them. The CYD game is insightful because 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.

[10]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: $\{A = \text{Dalit}, B = \text{Upper Caste + BJP}\}$ or $\{A = \text{Upper Caste + BJP}, B = \text{Dalit}\}$. Respondents only see a confederate's photograph once.

Dalits to the left of the cut-point pick the BJP-cueing politician 48% of the time, roughly at the same rate in the anonymous and profiled rounds. Dalits who are offered a house pick the BJP-cueing politician 49.7% of the time in the anonymous round and 43.2% of the time in the profiled round. This 6.5 percentage point difference borders statistical significance ($t = 1.82$, $p = 0.069$). The difference at the cut-point is consistently negative (approximately 10 percentage points) but statistically insignificant (see table 5). In other words, strong approval and support for the BJP does not spillover into the local context, where ethnic considerations continue to shape perceptions of distributive intent for a majority of Dalits. Moreover, 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.

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

In summary, I find that those offered a house recognize that the BJP has done something for them, and that some people voted for the BJP out of gratitude. However, they do not support the BJP any more than those next in line. Some of this may be down to statistical power but a lot of it is due to ceiling effects. Support for the BJP to the left of the cut-point is very high across a range of measures, leaving little room for increase when Dalits are offered a house. Strikingly, the BJP's national reputation and credibility does not travel to other tiers of government, where ethnic labels still matter.

A case of socio-tropic voting?

What explains such high levels of support for the BJP among non-beneficiaries, potentially wiping out any difference at the cut-point? I contend that the Dalits we interview assess performance and form opinions about distributive intent by looking at their social network or local community. What matters in this preference-forming process is not so much that "I was offered a house" but that "many people like me got a house". For this to be the case, two things should be true: (a) social networks should be saturated with the benefit; (b) Dalits across-the-board should think the BJP has done something for people like them, and that their condition has improved in the last 5 years. In table 4 we find evidence of (b). Here, I focus on social network-level exposure to the housing program.

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

Table 6 shows that exposure is very high on both sides of the cut-point. Between 70 and 77% respondents in the survey *personally* know at least one other beneficiary. When asked how many beneficiaries they know, those offered a house name, on average, 16 people, while those next in line identify 9 people. These differences are not statistically significant to merit any conclusion about networking through the program. The figures in appendix C confirm that respondents just to the left and right of the cut-point report very similar network-level exposure to the program. What this does suggest is that communities are saturated with the benefit, and even non-beneficiaries know many people who have benefited from the program. All this suggests that sociotropic considerations might be driving the political preferences of those next in line.

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 a null 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 self-build, and rely 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.

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	$\overline{Y_{\mathrm{Z=0}}}$
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

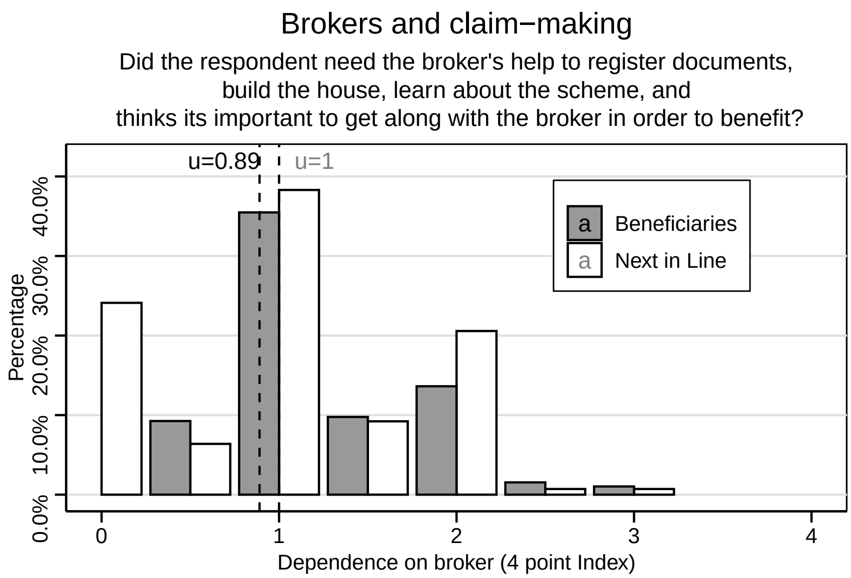
Clientelistic capture or inertia

Earlier I identified certain conditions under which there may be clientelistic capture: brokers should play an important role in claim-making (i.e. people should need the local leader's help to access benefit from the program); brokers should get most of the credit for the program so it generates political capital for them; and brokers should 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 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.

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).

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* 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 should 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 lower side given how widespread patronage, local discretion, and favoritism are.

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, a few things should hold: first, brokers should have political influence over voters; second, because the program excludes them from the distribution process, they should less aggressively publicize the program, contact and monitor voters. Again, the evidence for this is slender. As table 10 shows, very few Dalits take their cue

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	$\overline{Y_{\text{Z=0}}}$
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

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-led central government, suggesting there isn't a credit claiming or publicity problem. When it comes to contacting voters, table 11 makes it clear that Dalits to the left and right of the cut-point are contacted by BJP workers at roughly similar rates. The difference at the cut-point flip-flops under different specifications, and is statistically indistinguishable from 0. Figure 5 makes the point more clearly: about 45% Dalits to the left of the cut-point are contacted by the BJP before elections, and about 48% of them to the right. 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 a BJP or RSS worker. In comparison, only 82% report being contacted by Congress, 56% by BJP's ally JDU, and 77% by the opposition ethnic party RJD. In other words, the BJP outperforms all other parties in voter contact, and its contacting effort does not vary across the cut-point. In effect, there does not seem to be weak credit claiming or contact with voters, which might suggest clientelistic inertia.

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

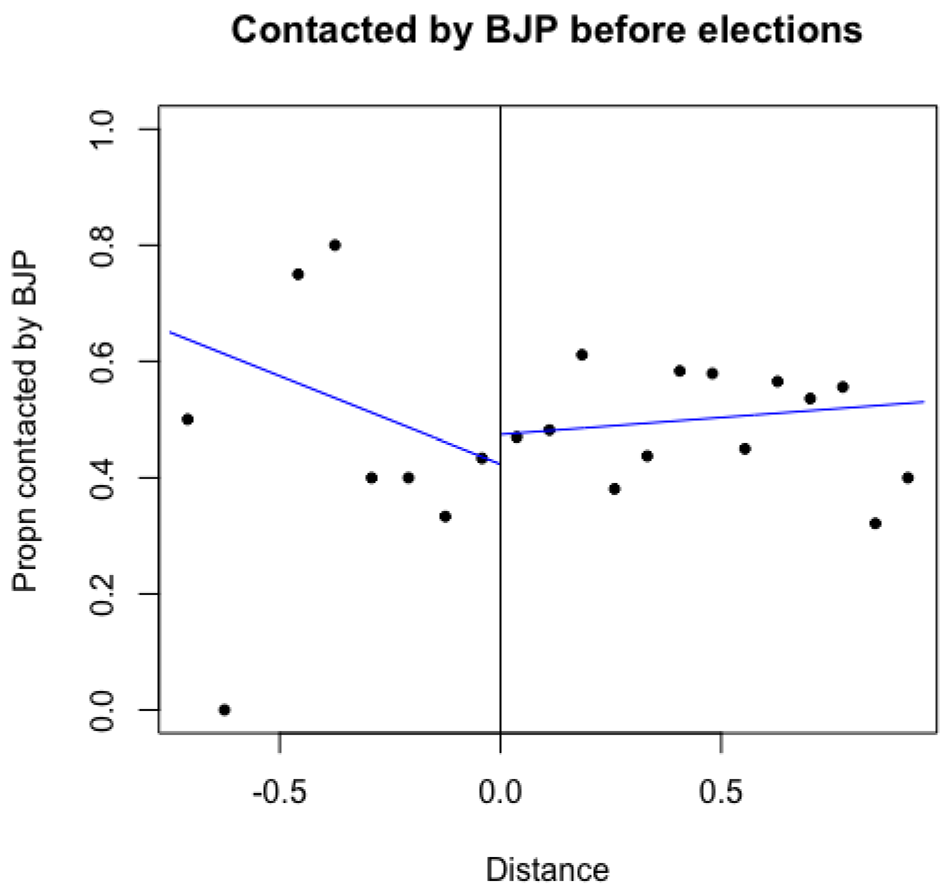


Figure 5: This figure shows the proportion of respondents contacted by the BJP before the national elections in each bin of the forcing variable. It is made using `rdplot`, specifying a first-order polynomial, triangular weights, bin selection through the mimicking variance evenly-spaced method using spacings estimators.

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 D's figure 10, I plot the difference in giving to a Hindu versus Muslim recipient. 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 figure 9 in appendix D).

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

Ultimately, there are two program-specific explanations for no effect 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 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 16% above the cut-point know of the beneficiary list (see table 14). As figure 11 shows, people to the left of the cut-point are just as aware of the list as those to the right. There is no discontinuous change at the cut-point. Among those that know of the list, 69% to the left of the cut-point and 82% to the right think they are on that list. In other words, an even smaller percentage of the overall population know of a list *and* think they are on it. Of those that know about the list, 23% to the left of the cut-point, and 6% to the right, know their rank. Clearly, there isn't sufficient knowledge to develop expectations about getting a house in the imminent future.

Nonetheless, I explicitly measure such expectations as well. Since everyone who was offered a house does not end up getting a house, we can compare expectations on either side of the cut-point. To the left of the cut-point, 20% expect to get a house in the next few months. There is no discontinuous change in expectations at the cut-point (see figure 12. Roughly a similar proportion of people just below the cut-point ($Z_i = 0$) and above it ($Z_i = 1$) anticipate getting a house in the next few months. This suggests that asymmetric expectations are not driving down the difference in outcomes at the cut-point.

Conclusion

The findings in this paper contribute to the literature in at least three ways. First, 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 political reputations built at the national level because of programmatic distribution do not spillover into the local context, where ethnic considerations continue to shape the voter's perceptions of distributive intent. Even an expensive benefit, delivered programmatically, fails to "undo" the distributive salience of ethnicity. Second, I show that voters do not have to personally benefit from a government program for 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 plays a role in the preferences of Dalit beneficiaries. They recognize that the BJP has done something for them, display greater programmatic awareness, and are more likely to think some people voted for the BJP out of gratitude. For Dalits next in line, 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

August 29, 2021

Contents

A 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	$\overline{Y_{\text{Z=0}}}$
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

B Attempts at sorting

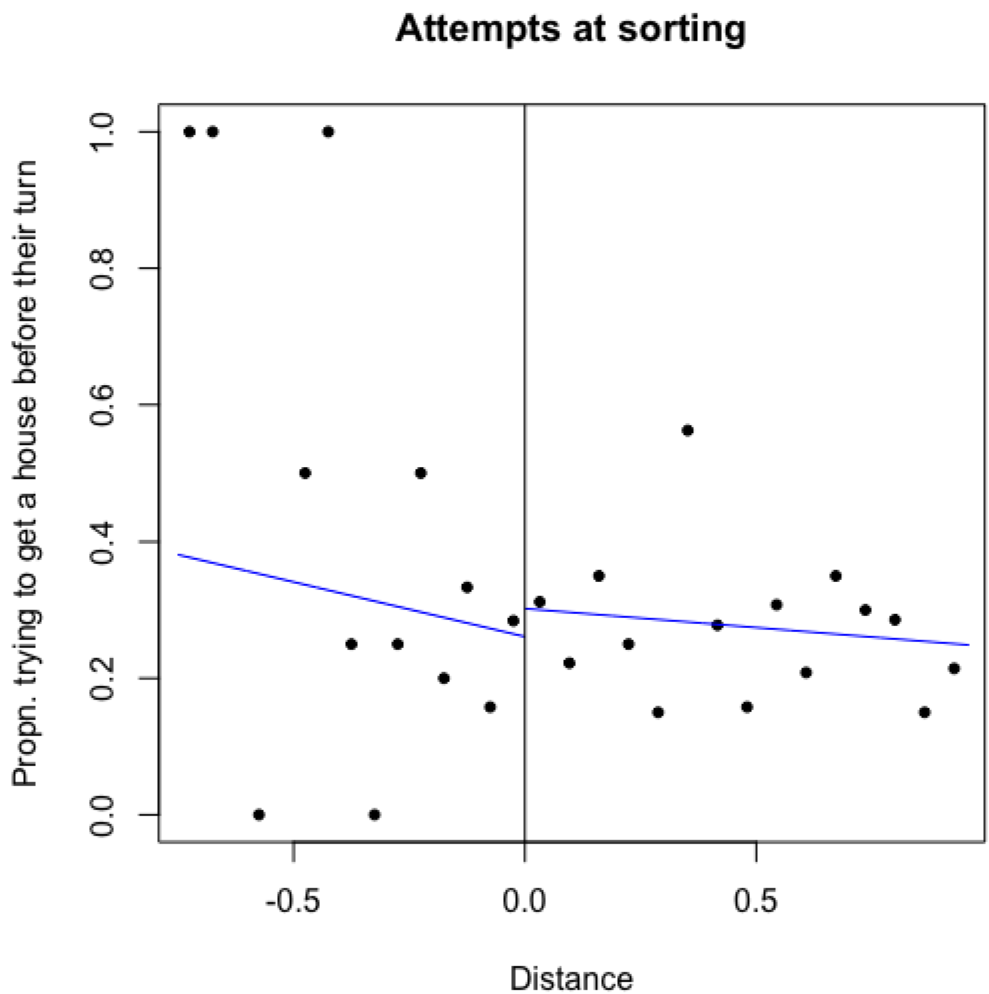

Figure 6: This figure shows the proportion of respondents who claim to have tried getting a house before their turn. It is made using `rdplot`, specifying a first-order polynomial, triangular weights, bin selection through the mimicking variance evenly-spaced method using spacings estimators.

C Exposure to the scheme

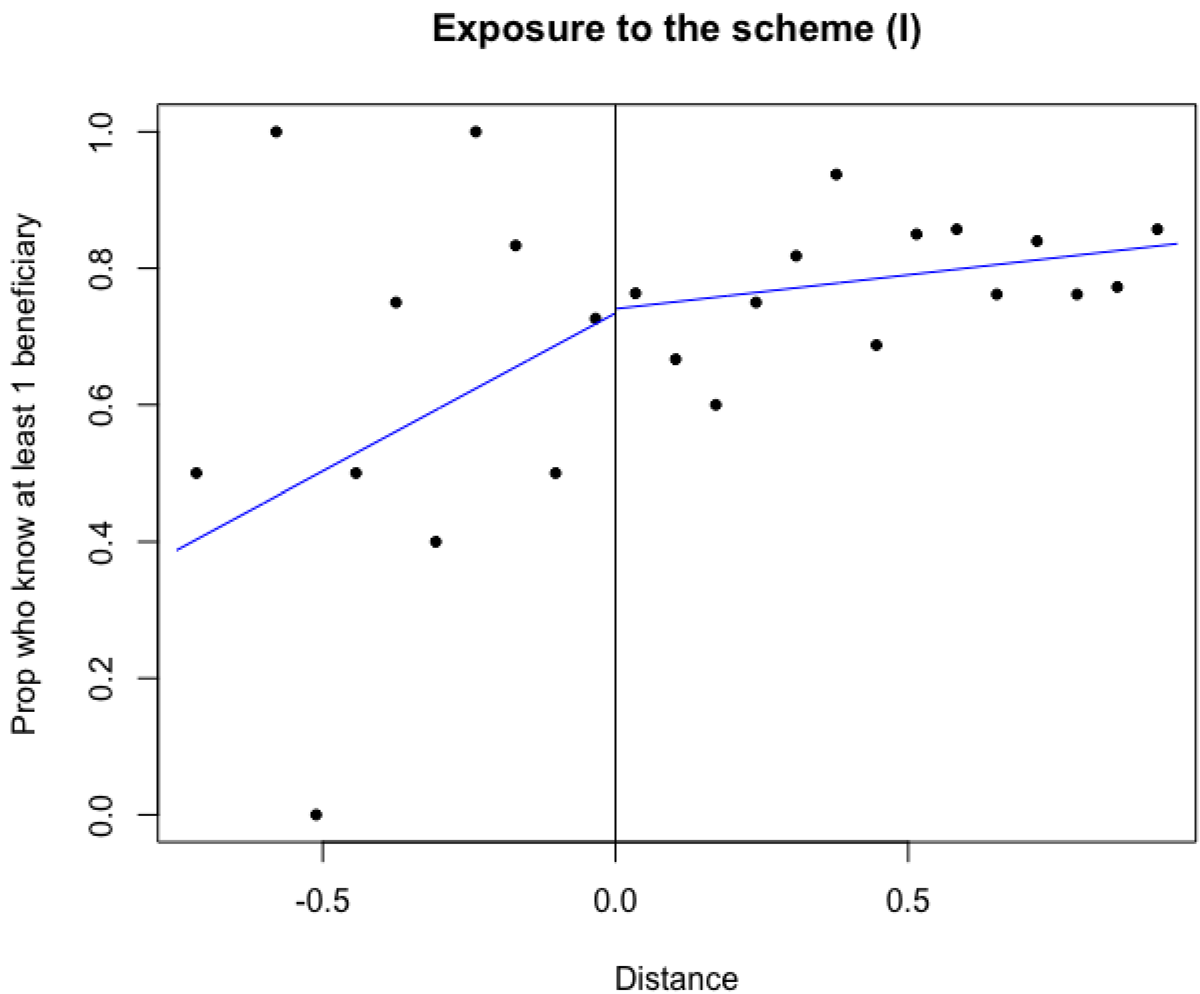

Figure 7: This figure shows the proportion of respondents who personally know at least one other beneficiary in each bin of the forcing variable. It is made using `rdplot`, specifying a first-order polynomial, triangular weights, bin selection through the mimicking variance evenly-spaced method using spacings estimators.

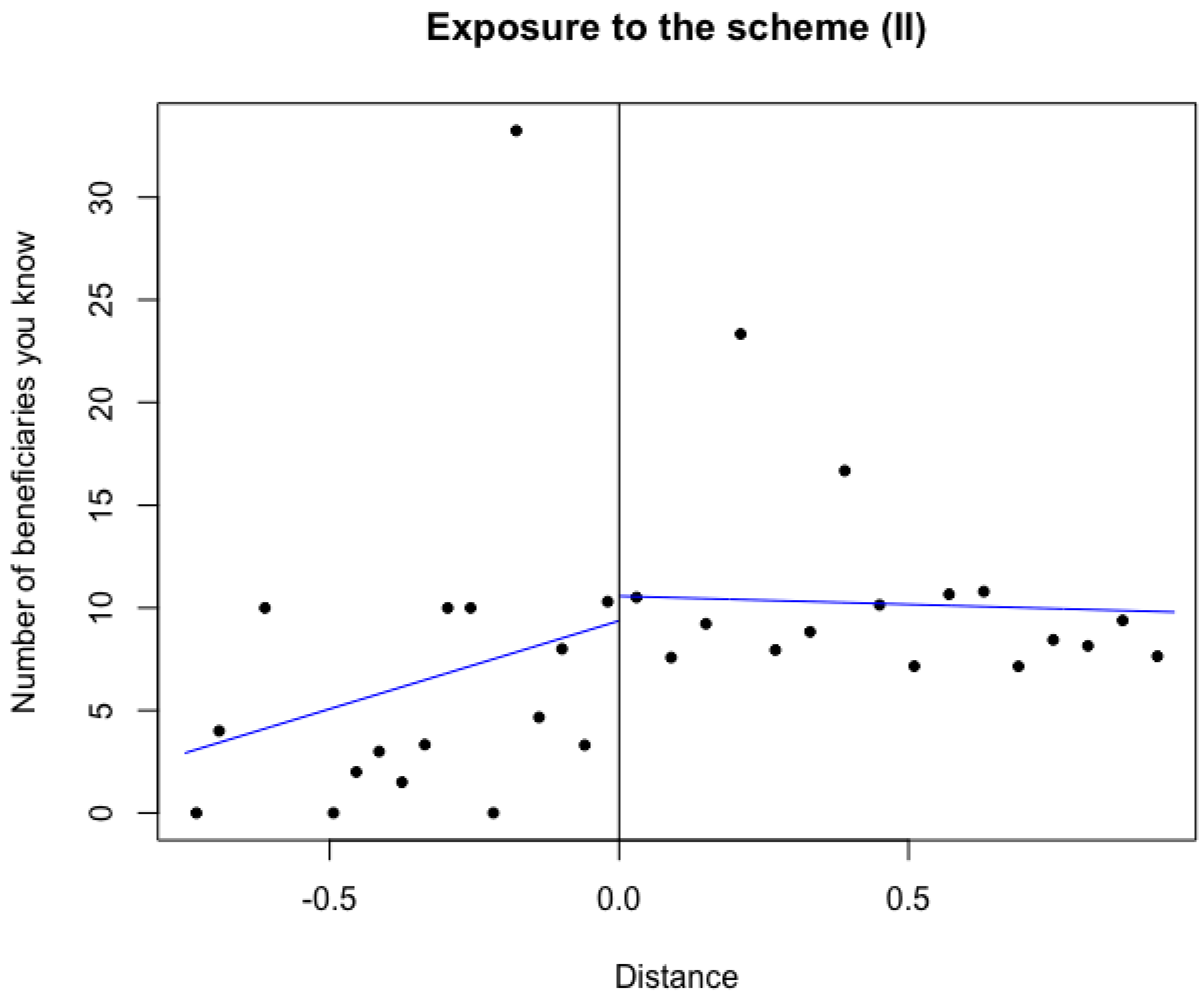

Figure 8: This figure shows, for each bin of the forcing variable, the average number of beneficiaries the respondent personally knows. I recode extreme values since they can distort the results. There are four instances of respondents claiming to know more than 100 beneficiaries. I cap these extreme values at the 99th percentile value on that side of the cut-point. The figure is made using `rdplot`, specifying a first-order polynomial, triangular weights, bin selection through the mimicking variance evenly-spaced method using spacings estimators.

D Prejudice against Muslims

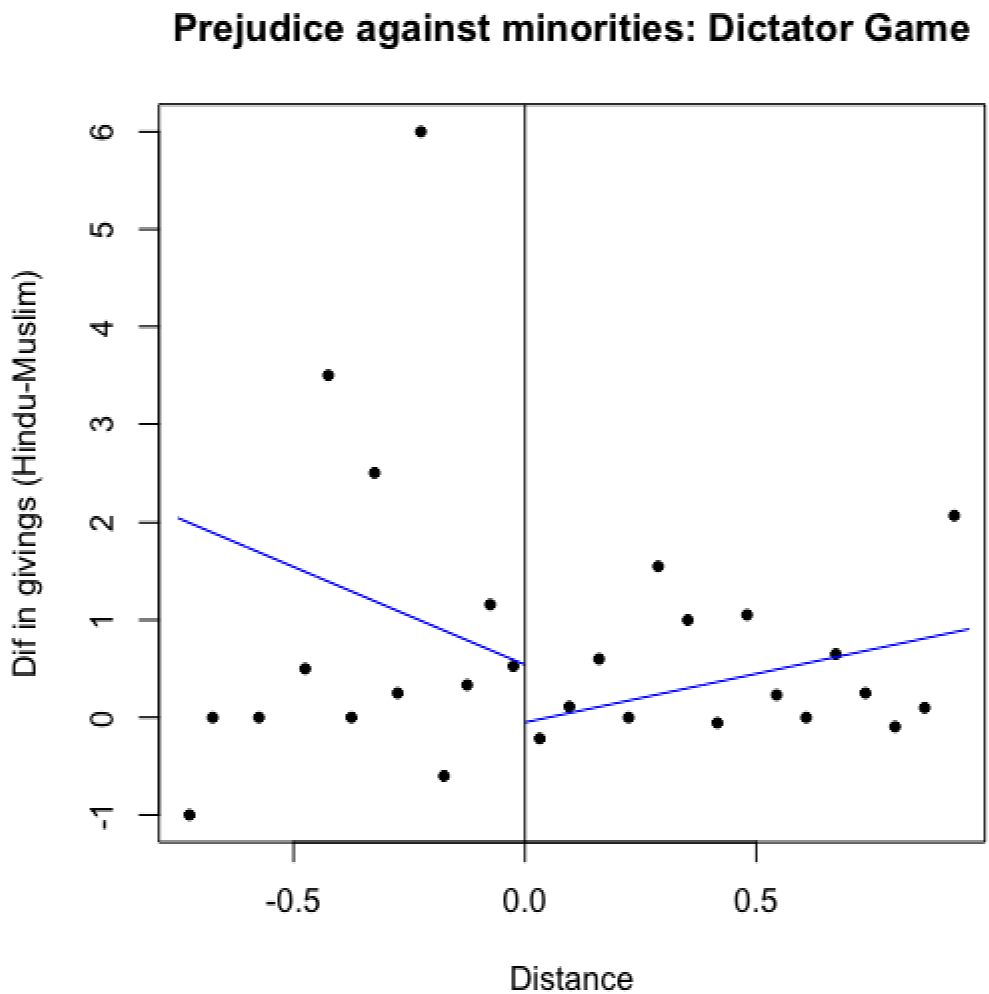

Figure 9: This figure shows the difference in givings (Hindu minus Muslim) for respondents in each bin of the forcing variable. It is made using `rdplot`, specifying a first-order polynomial, triangular weights, bin selection through the mimicking variance evenly-spaced method using spacings estimators.

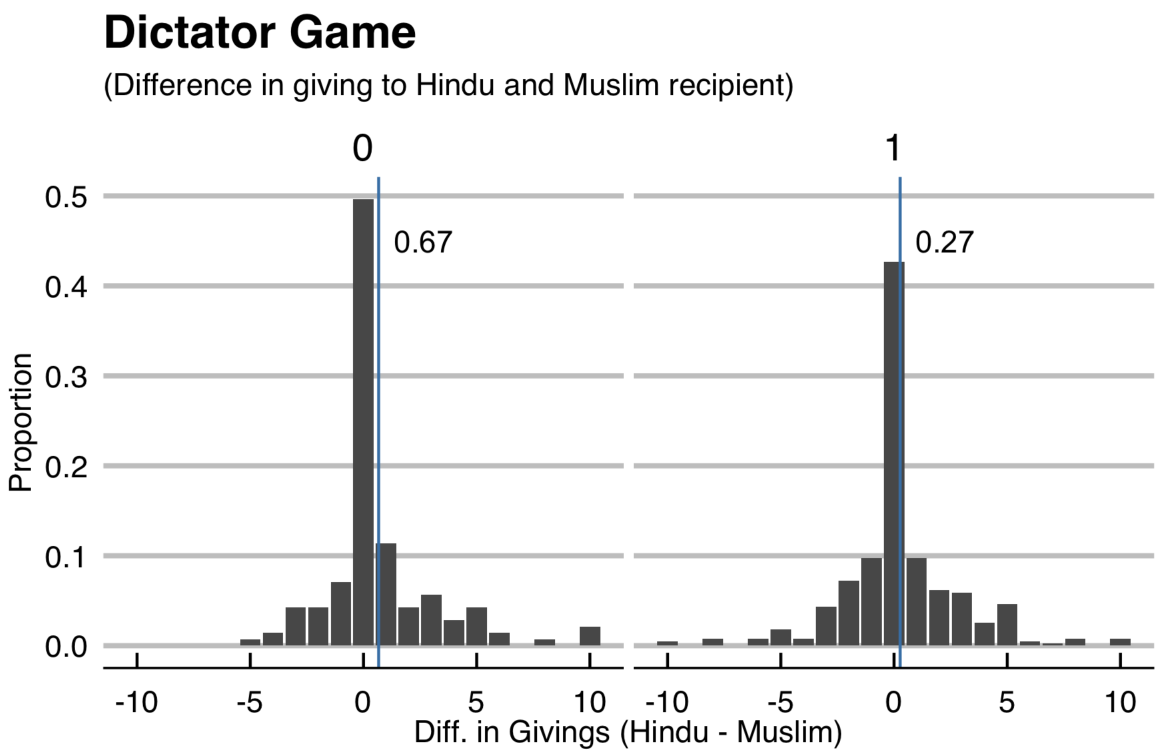

Figure 10: This figure shows frequency distribution for the difference in givings (Hindu recipient minus Muslim recipient) for Dalits respondents below the cut-point ($Z_i = 0$), and above the cut-point ($Z_i = 1$).

E Anticipation Effects

Table 14: Do you know of a list according to which houses were distributed?

Z_i	Yes (Percentage)	SE	n
0	9.22	2.45	141
1	16.71	1.89	389

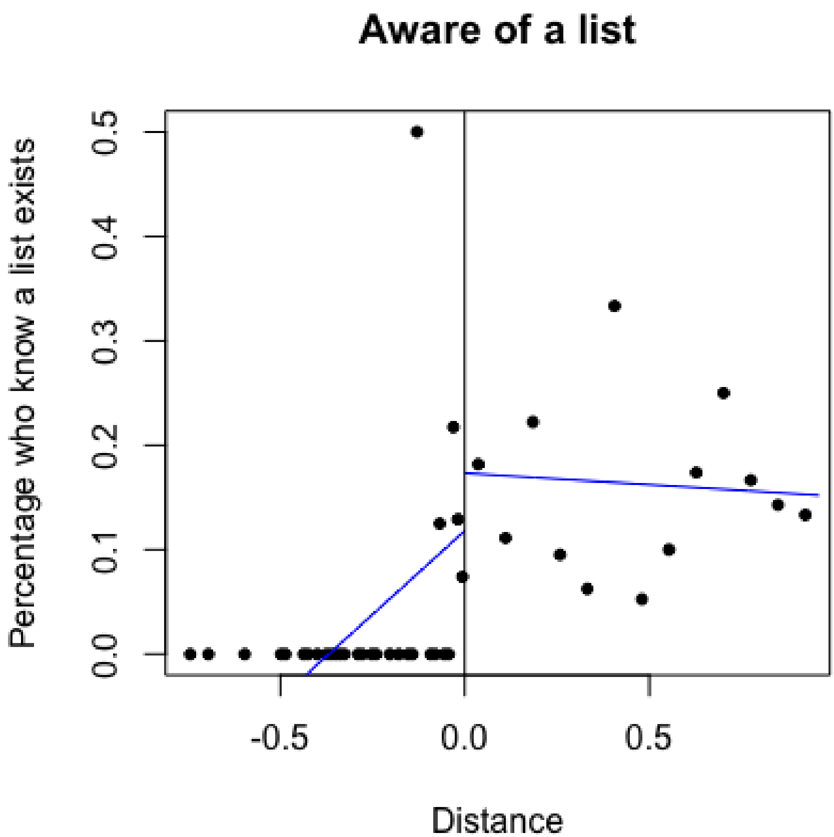

Figure 11: This figure shows the proportion of respondents in each bin of the forcing variable who know about a list according to which houses were distributed. It is made using `rdplot`, specifying a first-order polynomial, triangular weights, bin selection through the mimicking variance evenly-spaced method using spacings estimators.

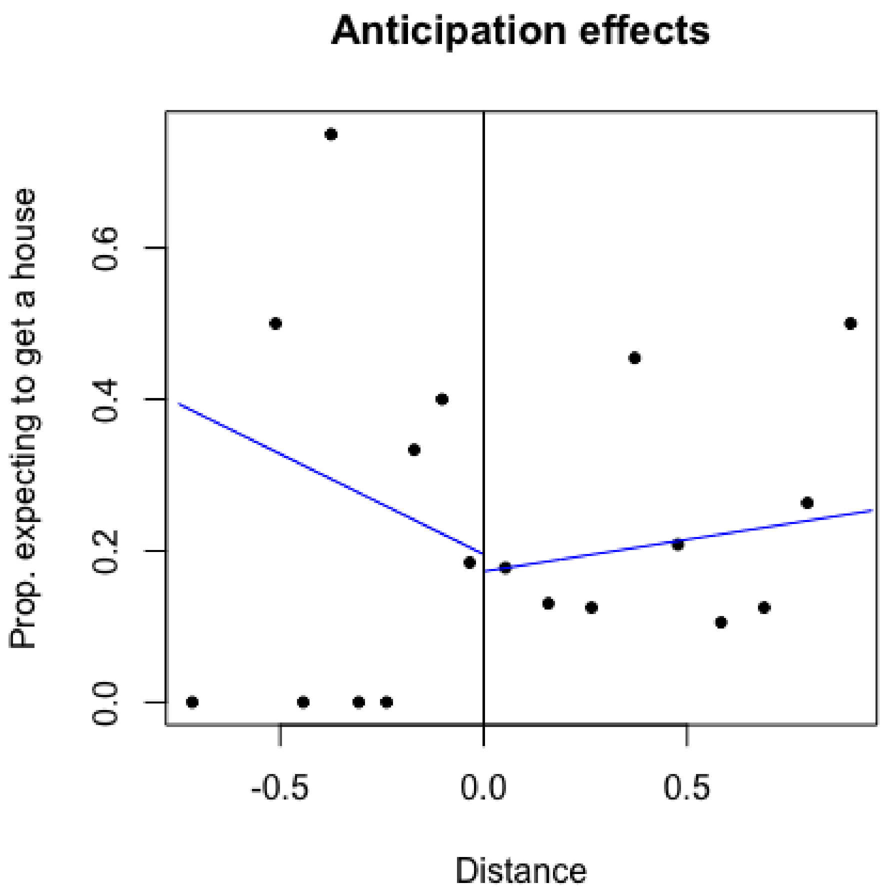

Figure 12: This figure shows the proportion of respondents in each bin of the forcing variable that expect to get a house in the next few months. It is made using `rdplot`, specifying a first-order polynomial, triangular weights, bin selection through the mimicking variance evenly-spaced method using spacings estimators.