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

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

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; Jaffrelot 2021). 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 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,

[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. See also Chapter 4 in Jaffrelot (2021).

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 eight and ten 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 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 have important implications for distributive politics in multiethnic developing democracies. 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

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.

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, De La O (2013) in Mexico's Progressa program, and Zucco Jr. (2013) in Brazil's conditional cash transfer 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). In Brazil, conditional cash transfers boosted support for incumbent presidential candidates in the short-term but did not have any long-term impact on political preferences (Zucco Jr. 2013).

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. Imai, King, and Rivera (2020) study two, nonpartisan programmatic policies and conclude that they "have no measurable effect on voter support for incumbents". 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, by-passing 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 (Auerbach 2020; 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 2020, 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

[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).

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; Alesina, Baqir, and Easterly 1999). Access to the benefit or politicians who can secure those benefits for the voter might be conditioned on ethnicity (Marcesse 2018; McClendon 2016). 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; Posner 2005; Dunning and Nilekani 2013; Conroy-Krutz 2013; Burgess et al. 2015; Kramon and Posner 2016; Ejdemyr, Kramon, and Robinson 2018; Auerbach and Thachil 2018; Gulzar, Haas, and Pasquale Forthcoming). 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.

[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.

[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).

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.

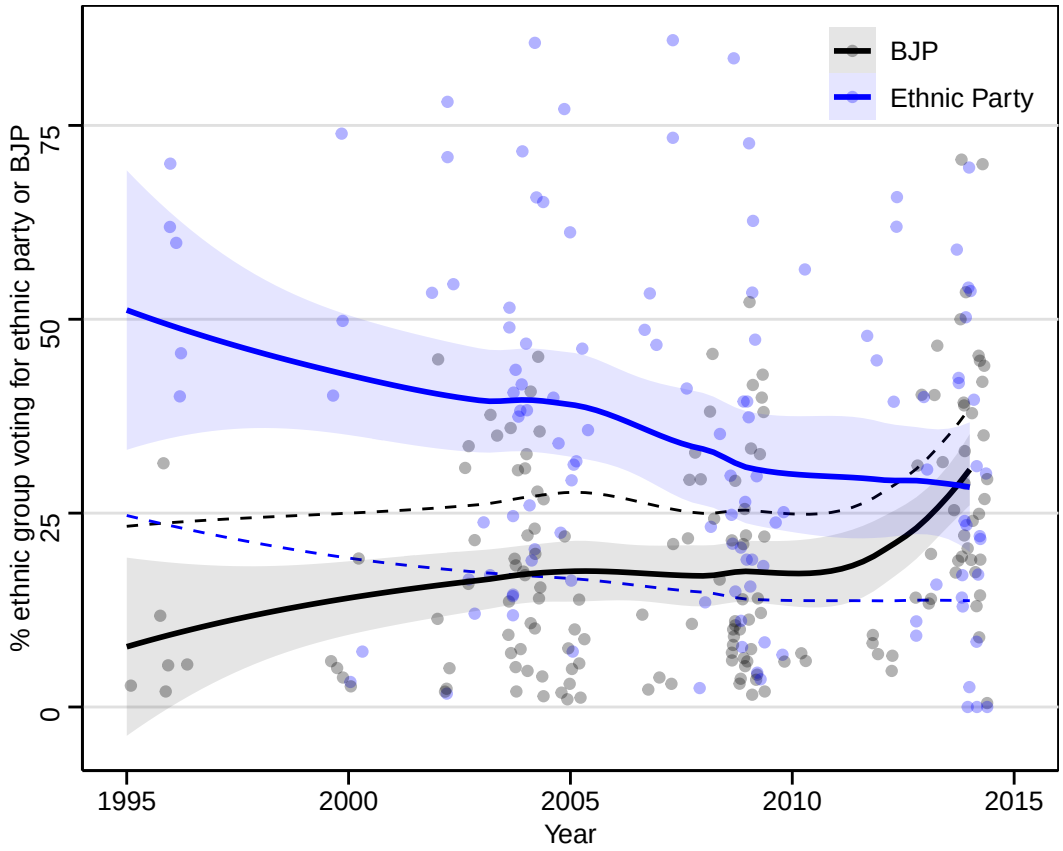
To see this, Figure 1 shows the proportion of an ethnic group voting for *its* ethnic party and the BJP in parliamentary and provincial elections since 1995. The analysis focuses on what Thachil and Teitelbaum (2015) call "narrow ethnic parties" that follow "patronage-based strategies *within* their restricted ethnic cores"(Thachil and Teitelbaum 2015:1394).[7] 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 ($\sigma_i = B$). On the other extreme are voters that support secularism ($\sigma_i = A$). The ruling party, BJP, advocates for Hindu majoritarianism, while the opposition champions secularism.

[7] 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)



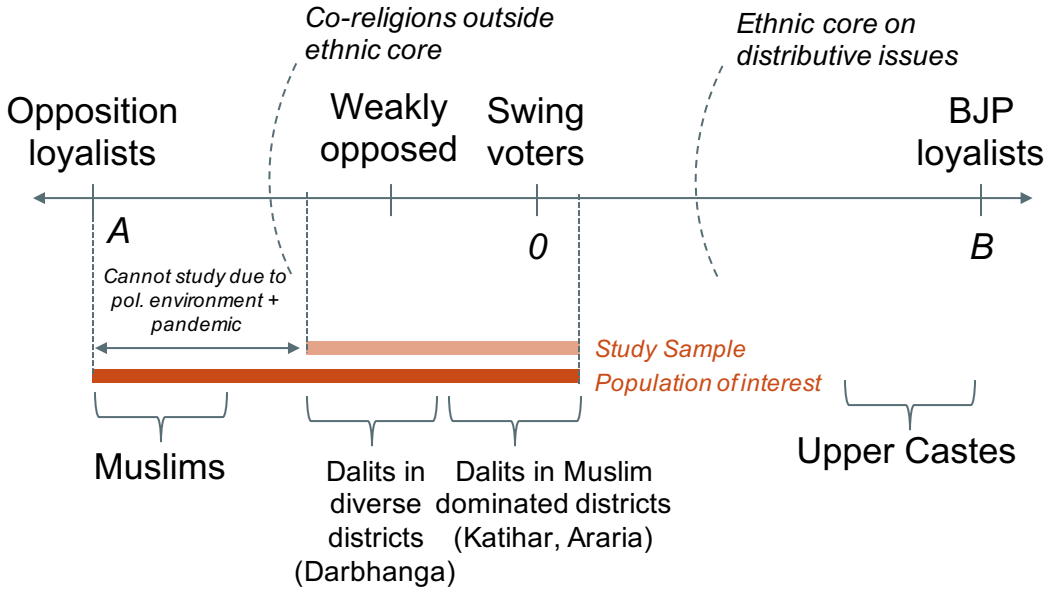
Note: Each point shows the percentage of an ethnic group that voted for its ethnic party or the BJP in an election. The solid trend lines capture over-time variation in groups supporting their ethnic party and the BJP. The dashed lines capture changes in overall party vote share. Data for this analysis 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).

Bihar province's different ethnic groups can be arranged on this latent dimension. High status groups (call them BJP loyalists) are located on one extreme ($\sigma_i = B$), Muslims (call them opposition loyalists) on the other extreme ($\sigma_i = A$), with swing voters in the middle ($\sigma_i = 0$). A constituency's demography determines which group is electorally pivotal.

I focus on lower caste Hindus (Scheduled Castes 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].

[8]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.

Appendix A shows the location of these districts on India's map. 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 (Darbhanga), 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 higher 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. Figure 2 shows the study sample as well as the broader population of interest which could not be studied due to the COVID-19 pandemic, and political conditions in the country.[9]

Appendix B 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,

[9]In December 2019, India's government amended citizenship rules which led to protests, particularly by Muslims, who were discriminated against in the new legislation.

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.[10]

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.

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[11]. 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

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

[11] 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.

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 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[12], interviewing all the households within the bandwidth (ϵ), and p_v proportion of people outside the bandwidth. I picked a

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

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 all the households within a 3% bandwidth, and 10% of households on the list but outside this bandwidth.

The survey company 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 of 63.7%, marginally above our expectations and similar to the rate reported by Manacorda, Miguel, and Vigorito (2011). Appendix L confirms that contact rates are very similar on either side of the cut-point, and there is no statistically significant discontinuous change in the contact rate at the cut-point. 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 2 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)	6.43 (se =1.25)	4.79 (se =2.795)
Density (Right)	7.88 (se =0.98)	5.84 (se =2.98)
Difference	1.45 (se =1.59)	1.05 (se =4.09)
T statistic	0.91 (p =0.36)	0.257 (p =0.797)
Bandwidth (L, R)	0.12, 0.16	0.03, 0.03

Note: 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[13], 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 15 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 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

[13] 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	$\widehat{\tau_{RD}}$	$\widehat{se}$	p	n	$\widehat{\tau_{RD}}$	$\widehat{se}$	p	n
Census	Deprivation Score	0.228	0.148	0.124	297	0.273	0.264	0.301	152
	Female	-0.048	0.060	0.418	293	0.054	0.090	0.549	152
	Age	-0.368	3.286	0.911	295	-1.502	6.078	0.805	152
Survey	Female	0.011	0.095	0.906	305	-0.023	0.195	0.906	152
	Age (binned)	-2.162	3.043	0.477	284	-3.964	5.756	0.491	152
	Education (1-8)	0.300	0.388	0.439	286	0.265	0.547	0.628	152
	Migrant	0.126	0.094	0.180	274	0.154	0.160	0.334	152

Note: The results are obtained in R using the `rdrobust` package. The estimation strategy was pre-registered. The first model (columns 3-6) 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 57 villages.

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

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 ($\widehat{\tau}$).[14] Appendix C reports the regression discontinuity plots for each outcome. The table and figures show that Dalits who were offered a house (henceforth treated subjects) are more likely to agree with the statement, "BJP has done something for [me]". Respondents were given four coins to indicate how much they agree with a statement, and practiced doing this on the ground or table before answering the survey question. They could put no coin (indicating complete disagreement), a few coins, or all four coins (conveying complete agreement). On average, untreated subjects (those left of the cut-point or next-in-line to get the benefit) put 2.25 coins. Those offered a house put an additional 0.6 to 1 coin.

I find 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 explicitly ask whether respondents themselves voted on this consideration. Nearly 20% of untreated subjects 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.

There is also 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, minimizes the party broker's discretion, and directly transfers the benefit to the recipient's bank account. Did treated subjects perceive this as programmatic distribution? On a 0 to 4 scale, where higher values convey greater awareness of programmatic features, the average response among untreated subjects is 2.55. At the cut-point, there is a 0.4 to 0.87 scale unit increase in programmatic awareness. Programmatic awareness is measured using four survey items: whether subjects know of the housing program, whether they know

[14] The table's first column reports the outcome. 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 using 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 using 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	$\overline{Y_{Z=0}}$
BJP has done something for me (0-4)	Pos	0.609	0.299	0.042	348	1.015	0.578	0.079	180	2.255
Some people voted for the BJP because they got a house (0/1)	Pos	0.190	0.090	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	-0.263	0.222	0.237	337	-0.015	0.381	0.969	180	3.057
CYD (Picks BJP, 0-1)	Pos	-0.101	0.074	0.172	300	-0.096	0.156	0.537	150	0.482
Receptive to Modi message (0-1)	Pos	-0.044	0.047	0.347	348	-0.102	0.098	0.297	180	0.940
Likes BJP (0-4)	Pos	0.357	0.222	0.107	316	0.423	0.508	0.404	152	3.156
Like Modi (0-1)	Pos	-0.010	0.031	0.756	352	-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	-0.122	0.226	0.590	324	-0.159	0.347	0.646	180	2.851
Condition of Dalits (-1 to +1)	Pos	-0.227	0.107	0.034	296	0.093	0.201	0.646	152	0.844
Vote for BJP ally (0-1)	Pos	-0.155	0.058	0.008	344	0.035	0.140	0.805	180	0.887
Attend opposition rally (0-1)	Neg	0.178	0.081	0.028	346	0.065	0.167	0.695	180	0.234
BJP defeatable (0-1)	Neg	0.024	0.058	0.683	324	0.002	0.101	0.984	180	0.121

Note: 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 57 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 $\overline{Y_{Z=0}}$).

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 E shows the difference at the cut-point separately for each measure. The direction of these estimates is how we would expect them to be. There is greater statistical uncertainty when using any single measure. Nonetheless, table 10 shows that treated subjects are more likely to know about the housing program and beneficiary list, and less likely to think there is ethnic favoritism or 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 the programmatic features of distribution. Appendix D shows that these results, which are from a pre-registered specification, are robust to alternative specifications with different bandwidth selectors, polynomials, and kernels.

Even so, the program fails to move political preferences. Those offered a house are no more supportive of the BJP than those next in line.

Treated and untreated subjects think the "BJP does something for people like [them]". Untreated subjects, on average, put 3 out of 4 coins in agreement with this statement. There is no increase at the cut-point. The negative coefficient is unstable and statistically insignificant.

The survey includes a semi-behavioral measure in which subjects respond to Prime Minister Modi's election speech in a neighboring province. In this speech, Modi claims the BJP's core philosophy is *sabka saath, sabka vikas, sabka vishwas* (everyone's support, everyone's development, everyone's trust). The subjects are asked 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. Among untreated subjects, the average response is 0.94. 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.

What about perceptions of the BJP? I ask respondents if they "like" the BJP and "trust it will do things for their welfare". On average, untreated subjects 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) though statistically insignificant. When it comes to Modi's speeches, an astounding 98% of untreated subjects 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. Subjects highly approve 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 the BJP government is better, -1 if the Congress government is better, and 0 if both are the same. The average response among untreated subjects is 0.91. The difference at the cut-point is inconsistently and imprecisely estimated. A second question focuses on corruption. Respondents are asked how much they agree with the statement, "BJP is less corrupt, and more reaches the poor [in BJP governments]". The expectation is that treated subjects express greater support for this statement than untreated subjects.[15] Untreated subjects, on average, put 2.85 coins out of 4 coins in support of the statement. Treated subjects agree with this statement at comparable rates. The difference at the cut-point is negative, though statistically indistinguishable from 0. A third question focuses on the respondent's ethnic group's socio-economic condition. Untreated subjects overwhelmingly say their ethnic group'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 inconsistently estimated: negative and statistically significant in one case ($\widehat{\tau} = -0.227$, s.e.$= 0.107$), positive and insignificant in the other specification ($\widehat{\tau} = 0.093$, s.e.$= 0.201$).

Does the housing program generate support for allies of the BJP that are less ethnically antagonistic towards Dalits? Among untreated subjects, 88.7% say they would vote for the Janata Dal United, a BJP ally that runs the provincial government. The difference at the cut-point is not consistently estimated ($\widehat{\tau} = -0.155$, s.e.$= 0.058$ when using the MSE optimal bandwidth, $\widehat{\tau} = 0.035$, s.e.$= 0.140$ when using the 3% bandwidth).

Finally, does receiving an expensive benefit make people less likely to engage in collective action

[15] 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.

against the benefit giving party? Strikingly, only 23.4% of untreated subjects say they would attend an opposition party's election rally. Contrary to my expectation, treated subjects are 6.5 to 18 percentage points *more* likely to attend an opposition rally. This is likely because a cement house increases mobility because subjects can lock their house and protect their material possessions from theft when they travel. This point is developed later in the paper (see Table 6). Subjects also seem to think the ruling party is electorally invincible. Only 12% of untreated subjects think an opposition party or leader can defeat the BJP if elections are held in the next six months. Treated subjects respond in the same way.

Overall then, 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 India's *National Election Studies, 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 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 in local politics? To answer this question, the survey includes a behavioral game in which subjects have to pick between hypothetical local politicians — one a coethnic (Dalit), the other from an out-group (upper caste politician cueing affiliation to the BJP). The subject's pay-off depends on which politician they pick, and the amount of money that politician gives in a dictator game. There are two versions of the game, an *anonymous* version in which subjects are told the politicians don't have any information about them; and a *profiled* version in which the politicians know the subject's name (ethnic identity), age, and occupation while deciding how to split 10 rupees with them. Building on Blum, Hazlett, and Posner (2020)'s design of the game, I use ethnically ambiguous photographs for the local politicians, with the politician's name cueing ethnicity and a saffron *gamcha* (scarf) and *tilak* cueing partisan affiliation.[16] Figure 3 provides an example match-up. The Choose Your Dictator game is informative: it creates a low information environment in which ethnic and party labels can shape perceptions of distributive intent. This allows us to compare the salience of both factors when there are material interests at stake.

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

Strong approval and support for the BJP at the national level does *not* spillover into local politics. Ethnic considerations continue to shape perceptions of politicians' distributive intent. On average, untreated subjects prefer the out-group politician cueing affiliation to the BJP 48% of the time. They prefer the coethnic politician 52% of the time. These probabilities are the same in the anonymous and profiled versions of the game. Treated subjects prefer the out-group politician cueing affiliation to the BJP 49.7% of the time in the anonymous version, and 43.2% of the time in the profiled version of the game. This 6.5 percentage point difference approaches statistical significance ($t = 1.82, p = 0.069$). The difference at the cut-point is consistently negative: approximately 10 percentage points in both specifications but statistically insignificant (see table 5, and the figure in appendix F). In other words, treated and untreated subjects prefer the out-group politician cueing affiliation to the BJP at comparable rates. If anything, treated subjects seem less likely to pick the BJP politician, particularly in the profiled version of the game.

In summary, 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, treated subjects do not support the BJP any more than untreated subjects. Across a range of measures, behavioral and attitudinal, support

Table 5: Choose Your Dictator Game (Picking BJP Politician)

	RD (MSE optimal BW)				RD (BW = 3%)				
Type of CYD Game	$\widehat{\tau}$	SE	p	n	$\widehat{\tau}$	SE	p	n	$\overline{Y_{Z=0}}$
Both Rounds	-0.101	0.074	0.172	300	-0.096	0.156	0.537	150	0.482
Anonymous Round	-0.104	0.110	0.344	282	0.003	0.191	0.989	150	0.479
Profiled Round	-0.096	0.107	0.373	290	-0.195	0.191	0.307	150	0.486

Note: See note to Table 4.

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 a house is offered. Strikingly, BJP's reputation and credibility at the national level does not spillover into local politics, where ethnic labels continue to shape perceptions of distributive intent. Appendix H provides an out-of-sample corroboration of this pattern using electoral data. BJP consistently performs better in parliamentary elections while ethnic parties hold onto their support base in local elections.

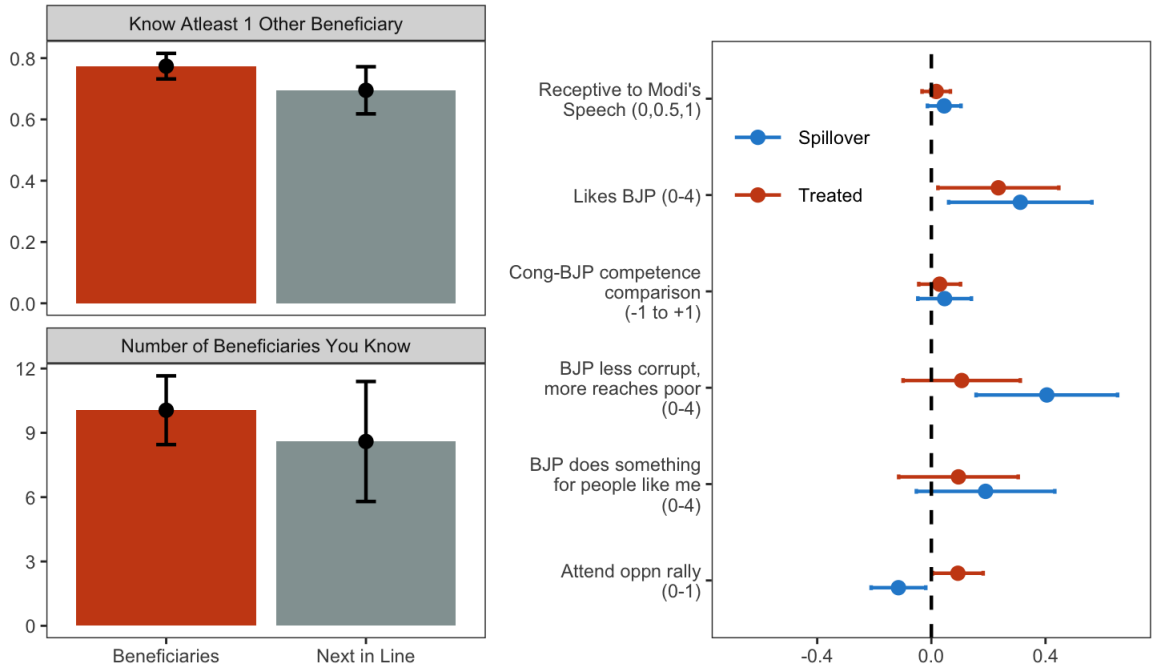
Network Exposure

Why is the BJP so popular among treated and untreated subjects? The evidence here suggests that people might be evaluating the ruling party's performance by observing their social network or local community. Their evaluations are driven by social outcomes, rather than pocketbook considerations. What matters for preference formation is that "many people like me got a house", and not that "I was (or was not) offered a house". Three findings in the data support such an explanation.

First, the housing program is highly visible in the communities we studied, especially the subject's social network which is saturated with the benefit. The left panel in Figure 4 shows that self-reported exposure to the housing program is very high on both sides of the cut-point. 70% ($s.e. = 3.9$) of respondents below the cut-point, and 77% ($s.e. = 2.1$) above the cut point *personally* know at least one other person who has received a house. Treated subjects, on average, know 10 other people who got a house while untreated subjects identify over 8 people. The difference between the groups is not statistically significant so as to merit any conclusion about networking effects because of the program. The figures in

appendix I confirm that respondents around the cut-point report very similar network-level exposure to the program.

Figure 4: Social Networks are Saturated With the Benefit



Note: The left panel shows the proportion of respondents below and above the cut-point who personally know at least one other program beneficiary, and the average number of beneficiaries people know below and above the cut-point. The right panel plots the coefficients from a fixed effects model along with 95% confidence intervals.

Second, there is observational evidence that knowing someone who has got a house is at least as good a predictor of political preferences as actually getting a house. I estimate a fixed-effects model in which political preferences are predicted by treatment (whether or not someone gets a house) and network exposure (whether or not someone personally knows at least one other program beneficiary). The regression controls for the deprivation score used to rank households. It also includes village fixed effects (λ_j) and therefore only leverages within-village variation.[17]

$$Y_{i,j} = \beta_0 + \beta_1(\text{Benefited}_i) + \beta_2(\text{Exposed}_i) + \gamma(\text{Deprivation Score}_i) + \lambda_j \tag{5}$$

[17] This analysis is exploratory because it was *not* pre-registered. It is also descriptive because it does *not* leverage the regression discontinuity design.

The right panel in Figure 4 plots the coefficients from this model for treatment ($\widehat{\beta_1}$ in orange) and exposure ($\widehat{\beta_2}$ in blue) for six different outcomes. Consistently, exposure to the program and actually benefiting from it are associated with similar changes in political preferences. These associations are in the hypothesized direction, and $\widehat{\beta_2}$ is consistently as large or larger in magnitude than $\widehat{\beta_1}$. These results also fit with other patterns in the data. For example, people who personally know program beneficiaries are *less* likely to attend an opposition party's rally. This is consistent with the original intuition. However, homeowners seem *more* likely to attend an opposition party's public meeting. Again, consistent with the explanation that a cement house increases mobility due to greater physical security.

Finally, subjects across the board believe that the BJP has done something for people like them, and their ethnic group's material condition has improved in the last five years (see Table 4). Taken together, there is evidence social outcomes or sociotropic considerations might be driving political preferences rather than individual outcomes.

Other Explanations

Could there be other explanations for the main result? In this section I evaluate and rule out the role of material and ethnic factors, clientelistic capture or inertia, misattribution, low satisfaction, and anticipation effects at the cut-point.

Material Factors

Could the housing program have no impact on political preferences because it failed to improve the material condition of beneficiaries? Table 6 reports the RD estimates for four outcomes that measure material well being: how physically and economically secure people feel, whether they skip a meal due to financial strains, their monthly household income, and recent debt. I measure physical and economic insecurity by asking subjects how worried they are about their family and material belongings when there is torrential rain or a storm (0-4 coins, increasing in worry). My fieldwork indicates that this captures one of the main psychological benefits of having a cement house for those who previously lived in a mud or bamboo hut. Untreated subjects, on average, put 3.66 coins in response to this question. At the cut-point, treated

subjects put 0.3 to 1 fewer coins. This difference is statistically significant. Treated subjects are also 0.2 to 0.8 scale units more happy than untreated subjects.[18]

Even so, treated subjects experience a temporary economic shock. A typical untreated household reports a monthly income of 6900 rupees. At the cut-point, household income declines by 1600 to 3100 rupees. My fieldwork indicates this is due to temporary unemployment: most families rely on their own labor to build the house, pushing them out of the labor market. In line with this, household debt increases at the cut-point by 2200 to 7000 rupees. Most of this money is borrowed in the informal credit market, namely from family, friends, and moneylenders. Similarly, meal-skipping due to financial constraints increases at the cut-point by 15 to 27 percentage points. These differences are directionally consistent and approach statistical significance at conventional levels ($p < 0.05$).

These findings illustrate the complex economic consequences of the housing program: short-term pain but long-term material improvement. It is unlikely that short-term pain is driving down the impact of the house at the cut-point. This is for several reasons: first, there is strong evidence that treated subjects recognize the BJP has done something for them; second, treated subjects are happier and feel more secure; and third, my fieldwork indicates that people understand that the short-term financial shock is due to their own voluntary actions, not the government per se.

Table 6: 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	386	-1.06	0.38	0.01	180	3.66
Skipped a meal in last 7 days (0-1)	0.15	0.09	0.10	311	0.27	0.19	0.16	152	0.28
Monthly income (Rs)	-1605.05	1067.80	0.13	274	-3105.91	1700.80	0.07	152	6902.84
Recent debt (Rs/binned)	2266.49	1565.74	0.15	305	7029.30	3143.90	0.03	152	5897.16

Note: See note to Table 4.

[18] Happiness is measured using a 1 to 10 visual ladder where higher values indicate greater happiness. On average, untreated subjects report a score of 2.32. There is a 0.2 to 0.8 scale unit increase at the cut-point.

Clientelistic Capture or Inertia

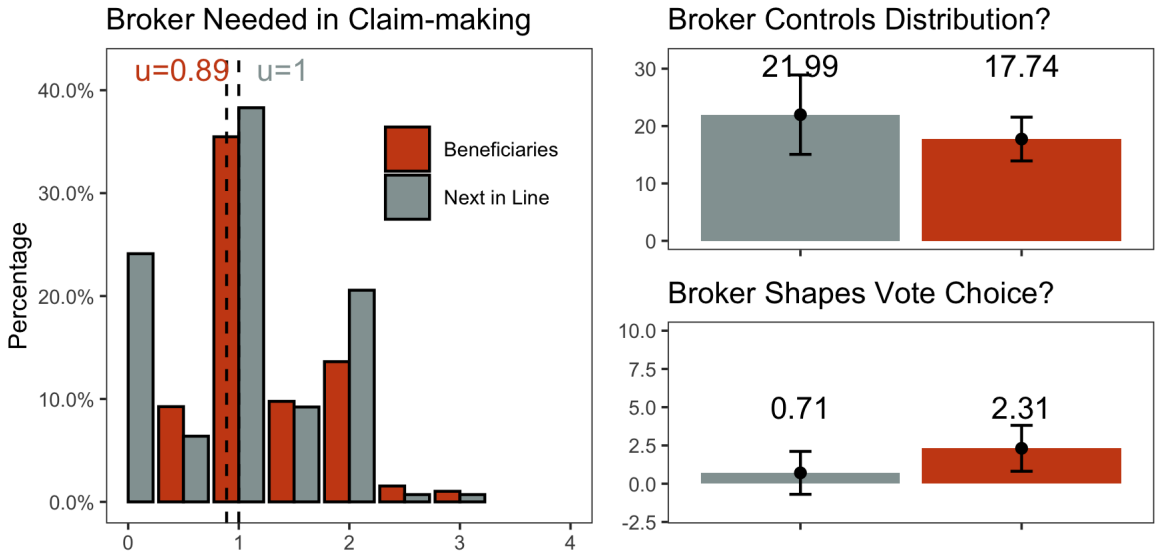
Did brokers hijack credit for the house? For this kind of clientelistic capture, one would expect brokers to play an important role in claim-making (i.e. people need their help to access the programmatic benefit), brokers should get credit for the program, and brokers should have electoral influence.

There is no empirical support for any of these claims. People do not need the local leader's help to benefit from the housing program. I construct a four item index that measures dependence on brokers. The items include whether subjects need the broker's help to register documents to access benefits, procure material or labor to build the house, learn about the program, or whether they think it is important to get along with the broker in order to benefit. The left panel in Figure 5 shows the frequency distribution for the index, separately for treated and untreated subjects. It is immediately apparent that study subjects do not depend much on the broker to access benefits under this program. On average, untreated subjects report needing the broker's help on 1 item. Treated subjects require the broker's help with 0.89 items. There is no statistically significant difference at the cut-point.

Brokers are also not in a position to hijack credit for the program. The survey asks subjects whether the broker (*mukhya*) will benefit from the fact that houses were built in the village. 34% of untreated subjects and 43% of treated subjects believe the broker would electorally benefit (see Table 7). This is a sizable number. To probe this point further, the survey measures perceptions of broker discretion in the distribution of houses. For brokers to get credit for the program, people should think brokers control the distribution of benefits. The top right panel in Figure 5 suggests otherwise: roughly a fifth of respondents (18% of treated subjects and 22% of untreated subjects) believe the local leader can ensure only their supporters get a house. Nearly 80% of people do not believe brokers control the distribution of houses. This is a compelling statistic, particularly given how widespread patronage, discretion, and favoritism are in this context.

Brokers also do not seem to have much influence over vote choice. The survey asks subjects whether they listen to what the local leader says at the time of elections, and vote for whoever the leader says. The bottom right panel in Figure 5 reports the percentage agreeing with this statement: 0.7% of untreated subjects, and 2.3% of treated subjects. In other words, fewer than 3% of people follow the broker while

Figure 5: Dependence on brokers



Note: The left panel shows the frequency distribution for beneficiaries (in orange bars) and those next in line (in gray bars) for an index that measures dependence on brokers for claim-making. The index has four components. Higher values signify greater dependence on the broker for claim-making.The dotted lines show the average response in either sub-group (beneficiaries in orange text, next in line in gray text). The top right panel shows the percentage of respondents in each sub-group that think the broker (*mukhya*) controls the distribution of houses. The bottom right panel shows the percentage of respondents in each sub-group that listen to the broker or local leader while deciding their vote in elections. The group mean estimates are accompanied by 95% confidence intervals constructed using heteroskedasticity-robust standard errors.

Table 7: 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 8: 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)	-0.185	0.436	0.672	299	0.646	0.868	0.457	152	1.617
Contacted by BJP (0-1)	-0.017	0.106	0.873	299	0.046	0.203	0.823	152	0.447

Note: See note to Table 4.

deciding their vote in elections. Cumulatively, the evidence for clientelistic capture is weak.

Could the housing program have no effect on political preferences because of poor credit claiming? This would be a case of clientelistic inertia: brokers are marginalized in the housing program; they are disinclined to publicize the program, contact beneficiaries and monitor them during elections.

There is weak evidence for such a proposition. To begin with, brokers do not have much electoral influence (see bottom right panel in Figure 5). Even if they did, there does not seem to be weak credit claiming for the program. Over seven out of ten subjects credit the ruling party for the housing program. Moreover, treated and untreated subjects are contacted at similar rates by the ruling party (see Table 8). There is no difference in contact rates at the cut-point (see Figure 6). Approximately 45% of untreated subjects are contacted by the BJP before elections. At the cut point, the contact rate does not change. To put this into perspective, on average, untreated respondents are contacted by 1.6 parties. Among those contacted, over 95% report being contacted by the BJP or its organizational affiliates (like the RSS). A lower percentage are contacted by other parties: 82% by Congress, 56% by JDU (BJP ally), and 77% by an opposition ethnic party (RJD). Put simply, the BJP outperforms all other parties in voter contact, and its contacting effort does not vary at the cut-point. There is no evidence of clientelistic inertia, in credit claiming or voter contact.

Ethnic Prejudice

Could the housing program have no effect on political preferences because ethnic considerations, like prejudice towards Muslims, are more salient in vote giving? The field sites have a sizable Muslim population, and Dalits are often mobilized through anti-Muslim appeals. To check whether this is the case, I

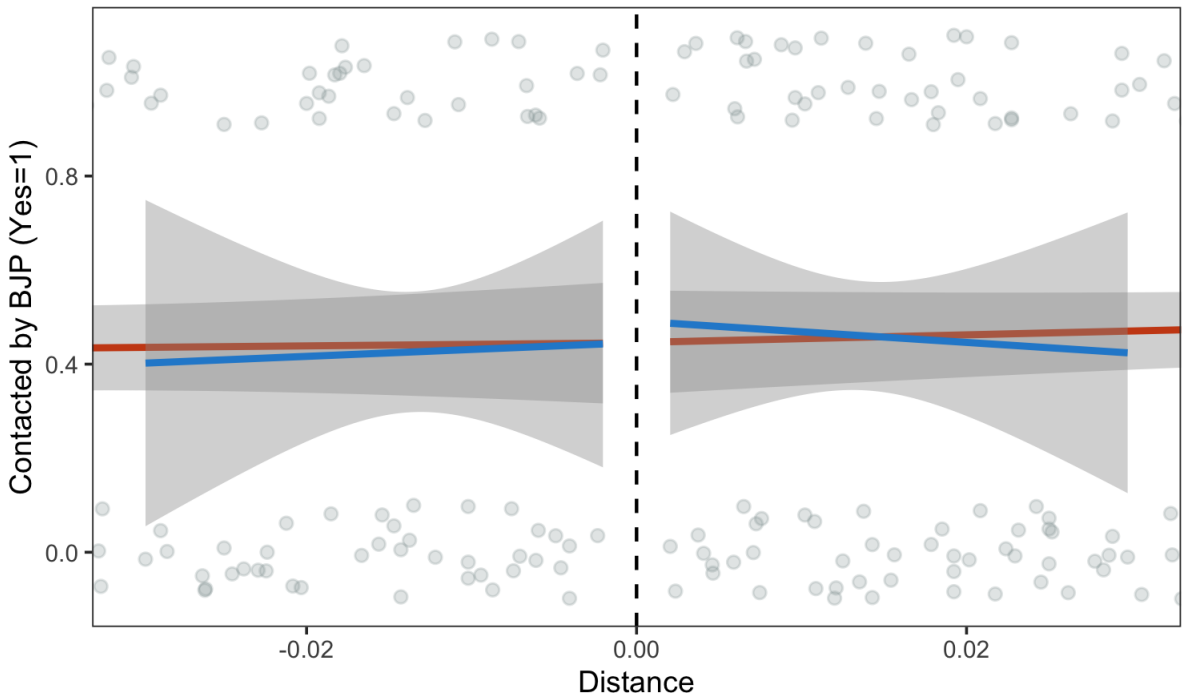


Figure 6: *Note:* The figure shows a regression discontinuity plot where the outcome is whether the BJP canvassed the respondent (Yes= 1, No= 0). The figure zooms-in on data around the cut-point ($\pm 3\%$), and shows estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth ($\pm 3\%$) and triangular kernel (in blue). 95% confidence intervals are depicted in gray.

measure prejudice against Muslims using the dictator game. Subjects play three rounds of the game, each round with 10 rupees. In the first round they play with an ethnically ambiguous recipient. In the next two rounds, they play with ethnically identifiable recipients. The survey enumerator shares the recipient's name, age, and occupation in addition to their photograph. The recipient's name cues religious identity, as does their photograph (Muslim recipients wear a skull cap). The order in which subjects play with a Hindu and Muslim receiver is randomized.

On average, subjects give an anonymous receiver 4.38 rupees, keeping 5.62 rupees for themselves. When the receiver is ethnically identifiable, subjects give 4.13 rupees to the Hindu receiver, and 3.75 rupees to the Muslim receiver. There is discrimination against Muslims: $\mu_{\text{Hindu-Muslim}} = 0.37$ 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 J, I compare the difference in money given to Hindu and Muslim receivers. Untreated subjects on average give the Hindu receiver 0.67 rupee more

than a Muslim receiver. Among treated subjects the average penalty is 0.27 rupees. There is no statistically significant difference in the penalty at the cut-point (see top panel in figure 18 in appendix J).

Clearly, Dalits are prejudiced against Muslims. The ethnic penalty observed in the behavioral game is not very large (2.7% to 6.7% of the total budget available to subjects). Even so, I cannot rule out the role of religion in vote giving. It is, however, unlikely to overwhelm other determinants of political behavior and preferences.

Low Satisfaction or Misattribution

The housing program may have no impact if public satisfaction with it is low, or people misattribute credit. The data enable us to rule out both explanations. 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.

There is also no misattribution of credit. This would be a problem if treated subjects did not know who ran the housing program, or incorrectly attributed it to some other party. In both situations, treated subjects would not reward the BJP for the program and we would observe no difference in support for the party at the cut-point. Table 9 shows that 71% of treated subjects and 78% of untreated subjects correctly attribute the housing program to the BJP government. An additional 15% say it is jointly run by the national government and state government (credit sharing). Only 2 to 3% credit the state government alone, and 5 to 10% don't know who runs the program. The distribution of responses is very similar to the left and right of the cut-point.

Anticipation Effects

Finally, a feature of the research design can also explain why people to the left and right of the cut-point have similar political preferences. If subjects next in line for the benefit are aware that they are imminently going to get the benefit, they may respond to survey questions factoring this information. This will inflate estimates just below the cut-point, and drive down the difference at the cut-point.

Table 9: Credit for the Housing Program

	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

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.

The survey includes a variety of measures to detect this possibility. For subjects to anticipate benefiting, they must know of the list according to which houses are distributed; they should know they are on that list; they should know their rank on that list (knowing ones position relative to the cut-point is necessary to form such expectations); and they should expect to get a house in the next few months.

There is no evidence in support of these assumptions. Only 9% of untreated subjects and 17% of treated subjects know of the beneficiary list (see right side panel in figure 19).[19] An even smaller percentage of subjects on either side of the cut-point think they are on the list. 14% of treated subjects and 6% of untreated subjects believe they are on the beneficiary list. A very small proportion know their rank on the beneficiary list. 1% of treated subjects and 2% of untreated subjects claim to know their rank on the list. Evidently, there isn't sufficient programmatic knowledge to develop expectations about getting a house in the imminent future.

The survey also explicitly measures such expectations. The left panel in Figure 19 shows that a relatively small proportion of untreated subjects expect to get a house in the next few months, and that these expectations are not correlated with proximity to the cut-point. Among untreated subjects ($Z_i = 0$), 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.[20] Furthermore, expectations about getting a house are not correlated with distance from the

[19] There is a statistically significant difference in programmatic awareness of this kind. For more, see Table 10.

[20] 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 ($Z_i = 0$).

cut-point. Subjects 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 in a variety of RD specifications. Overall then, it is unlikely that expectations about getting a house are driving down differences at the cut-point.

Discussion

The findings in this paper have important implications for distributive politics in multiethnic developing democracies.

In several countries, parties make cross-ethnic appeals by distributing material benefits (Gadjanova 2021; Arriola et al. 2020; Thachil 2014; Ichino and Nathan 2013). This paper shows, contrary to Imai, King, and Rivera (2020), that rule-based, non-contingent, direct transfers can generate support for a party among people outside its ethnic core. Among beneficiaries, there is recognition that the BJP has done something for them and support for gratitude voting. Crucially, saturating communities with a private (excludable) benefit can generate support even among those that do not get the benefit. The paper suggests that voters evaluate the incumbent by observing social outcomes, not just personal gain.

Even so, the pay-off from programmatic distribution is limited (Zucco Jr. 2013). I find that the housing program does *not* weaken the distributive salience of ethnicity in low information environments. The logic of ethnic voting, that an in-group politician or ethnic party is more likely to benefit the voter, remains largely intact. The BJP's distributive reputation built at the national level because of programmatic targeting does not spillover into local politics. Ethnicity can continue to be an obstacle for democratic accountability (Adida et al. 2017). It also complicates any transition to programmatic politics. In any country, programmatic challengers are likely to emerge at the local level. However, if ethnic voting is more entrenched at this level, new entrants are disincentivized from making programmatic appeals, and competing on programmatic platforms.

On the positive side, programmatic distribution does seem to improve last-mile delivery. The paper finds that beneficiaries are highly satisfied with the program, there are fewer reports of bribing, and more

"deserving" people get the benefit (less mistargeting). Most importantly, beneficiaries know about the programmatic features of distribution. The case for rule-based, non-contingent, direct transfers is also helped by the limited potential for credit hijacking. Parties do not need engage non-state organizations or other intermediaries to deliver benefits in areas where opposition brokers control distribution (Bueno 2018). This paper shows that parties can successfully claim credit for a programmatic benefit, minimize the possibility of misattribution or credit hijacking, and mobilize beneficiaries through brokers even though they are marginalized in the distributive process. Party brokers do not make any extra effort to contact beneficiaries but they are also not saboteurs that strategically exclude beneficiaries from the canvassing effort.

All this opens several paths 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 an incumbent can obtain more support without actually distributing any benefits? What is that threshold and can it generate resource savings for budget constrained politicians? How does a benefit's value, visibility, distribution, and credit claiming determine that threshold? These are interesting questions for future work in this area. On the voter side, why is there a reversion to ethnic considerations in local politics, even when people positively evaluate a party at the national level? Does the persistence of ethnicity and clientelism disincentivize the use of one-off programmatic distribution? Do institutional features, like multiple tiers of government, contain reputational gains from programmatic distribution and preserve the role of brokers and ascriptive identities?

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Appendices

Shikhar Singh

September 10, 2022

Contents

A Location of Field Sites 46

B Dalit Vote in Bihar 47

C Regression Discontinuity Plots 48

D Specification Curves 50

E Programmatic Awareness 53

F Distributive Salience of Ethnicity 54

G Attempts at sorting 55

H Reputational Spillovers: National vs. Local 56

I Exposure to the scheme 57

J Prejudice against Muslims 58

K Anticipation Effects 59

L Contact Rates 60

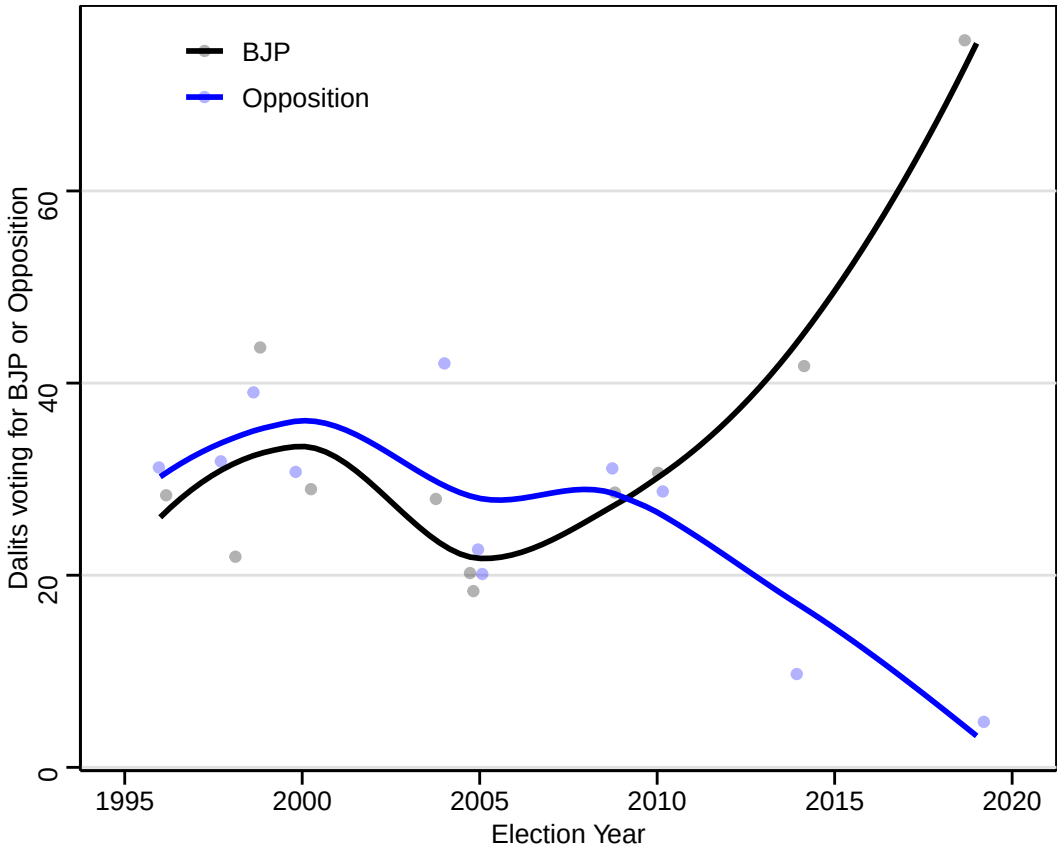
A Location of Field Sites

Figure 7: Location of Field Sites

Note: The area shaded in gray shows Bihar province on India's map. The data collection sites (from left to right: Darbhanga, Araria, Katihar) are shown as orange dots within Bihar province. The three orange dots outside Bihar province are the sites where I conducted fieldwork, developed theoretical intuitions, piloted the survey questionnaire, and trained enumerators. These sites are in Bahraich, Barabanki and Sitapur districts of neighboring Uttar Pradesh. This training set is very similar to the test set (data collection sites) in its geography, economic structure, socioeconomic development and ethnic composition. The map is made with the `sf` package in R, using shape files from `DataMeet`. The shape files are freely available here: http://projects.datameet.org/maps/.

B Dalit Vote in Bihar

Figure 8: 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).

C Regression Discontinuity Plots

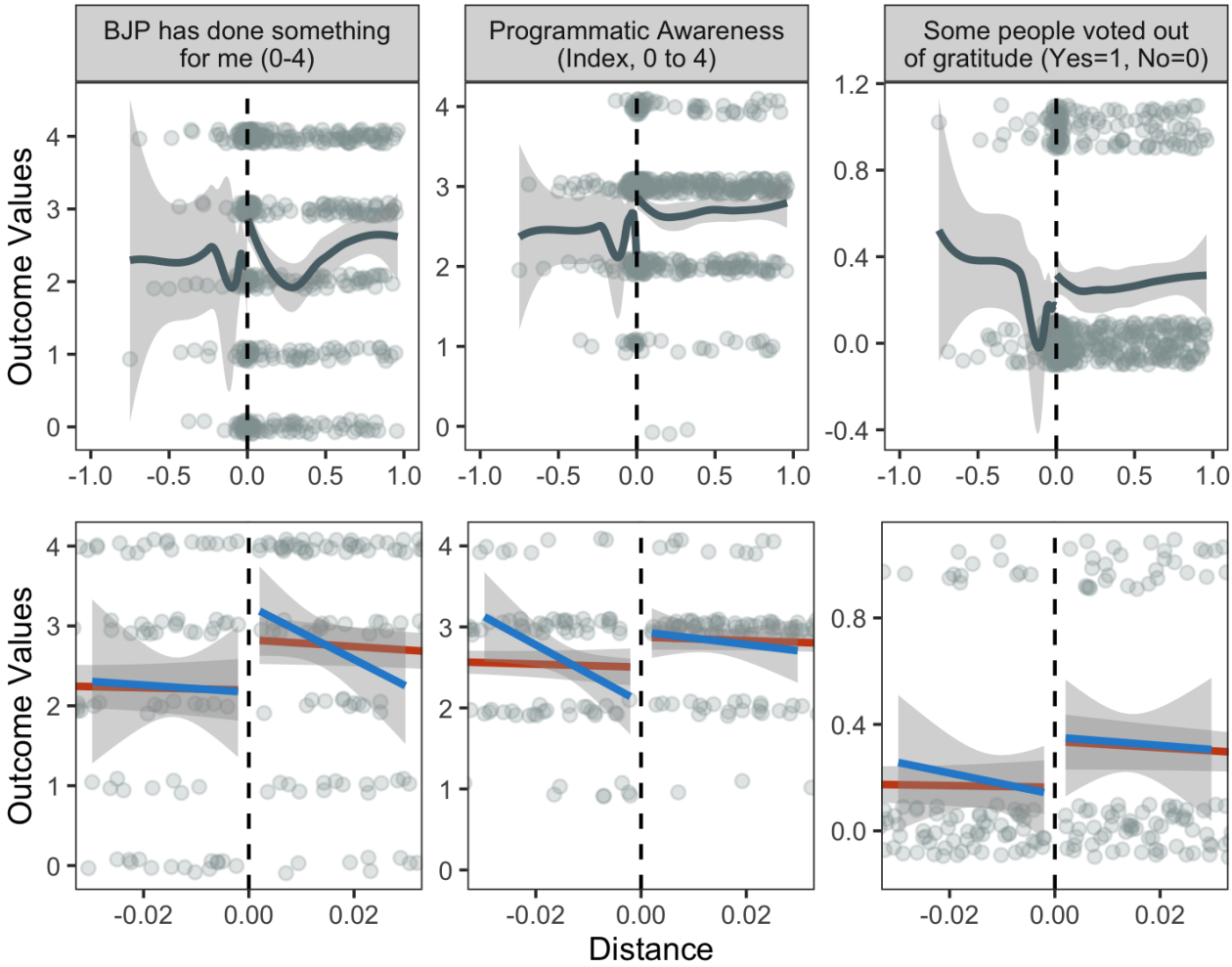


Figure 9: The top panel shows outcomes at different values of the forcing variable. We show the conditional means using a LOESS, with 95% confidence intervals in gray. The bottom panel zooms-in on data around the cut-point ($\pm 3\%$), and shows estimates at the cut-point using two pre-registered specifications: 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 ($\pm 3\%$) and triangular kernel (in blue). 95% confidence intervals constructed using heteroskedasticity-robust standard errors are depicted in gray.

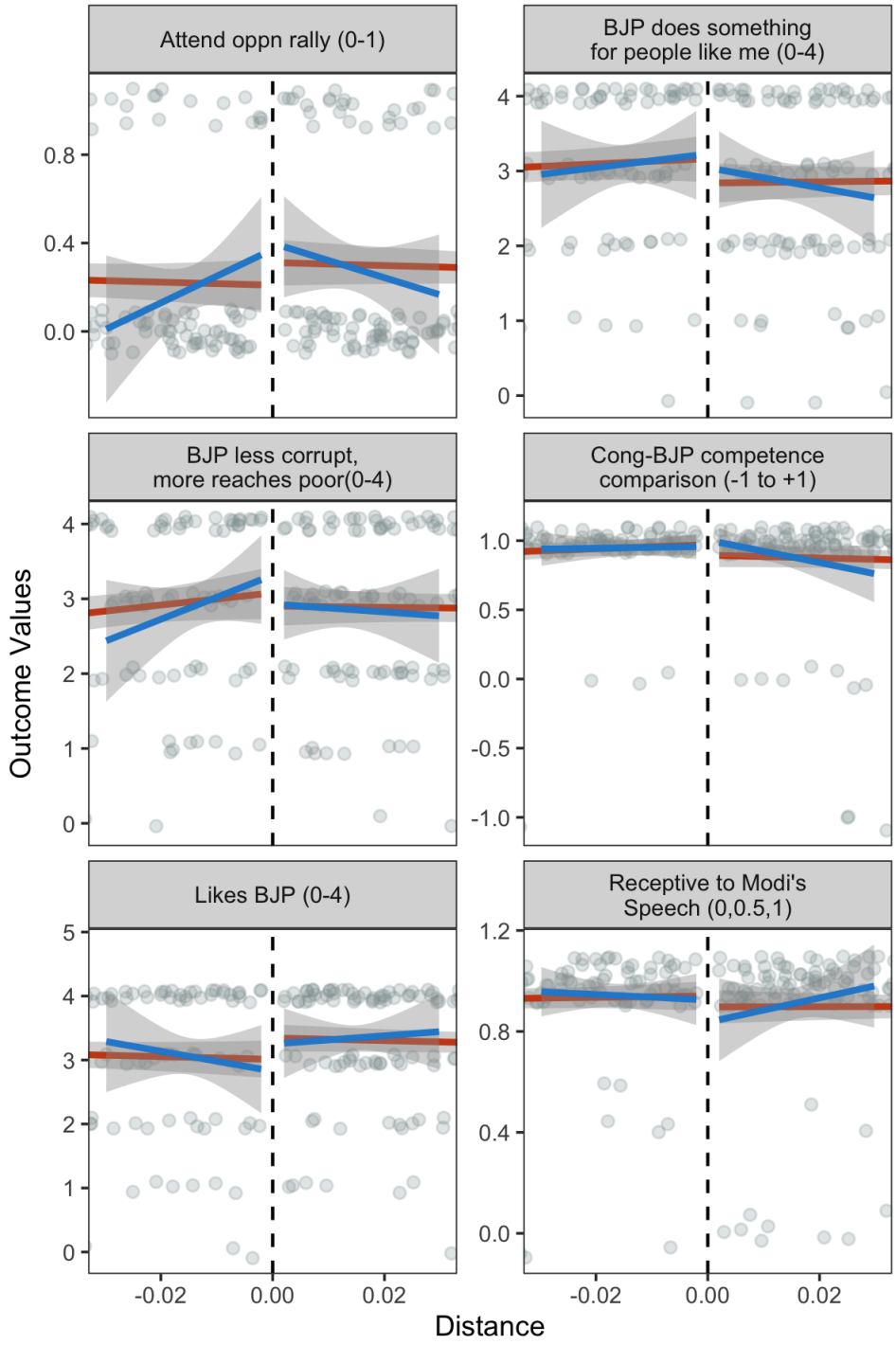


Figure 10: The figure shows outcomes at different values of the forcing variable. We zoom-in on data around the cut-point ($\pm 3\%$), and show estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth ($\pm 3\%$) and triangular kernel (in blue). 95% confidence intervals constructed using heteroskedasticity-robust standard errors are depicted in gray.

D 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(mserd, msesum), a selector that picks $median$(mserd, msesum, 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(cerrd, cersum), and a selector that picks $median$(cerrd, certwo, cersum) for each side of the cut-off separately.

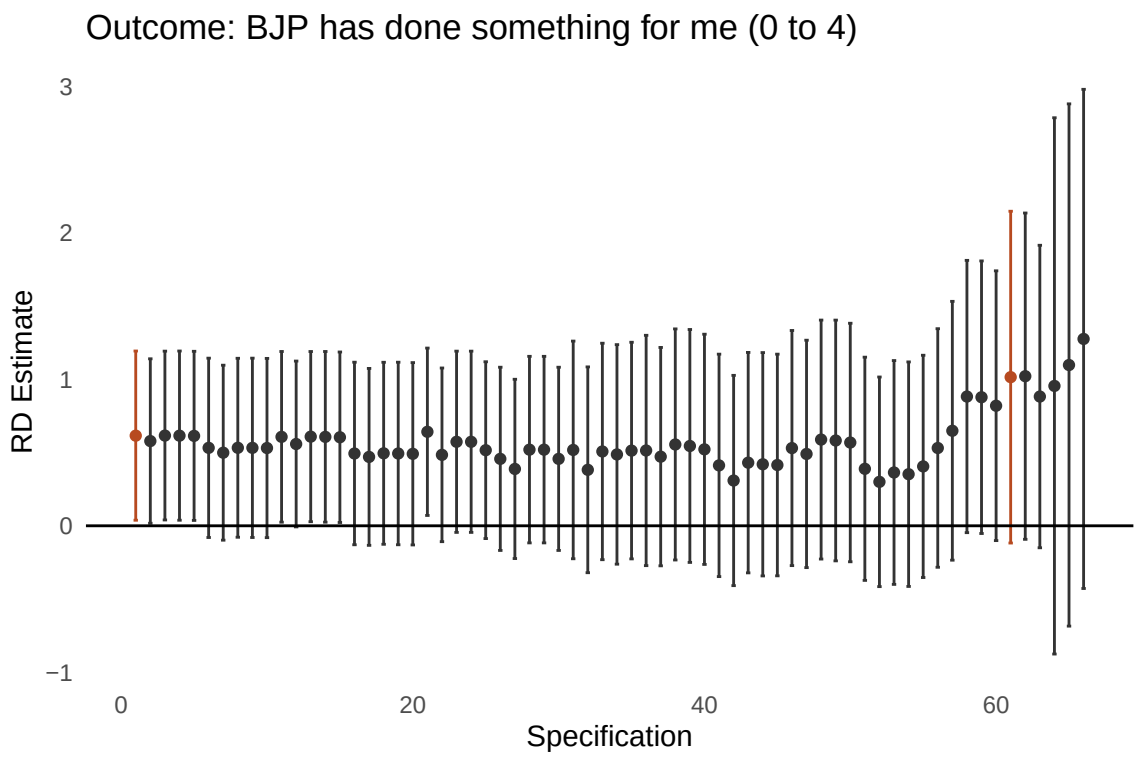


Figure 11: The figure reports the difference at the cut-point and 95% confidence intervals produced by `rdrobust` under different specifications, with the pre-registered specification in red.

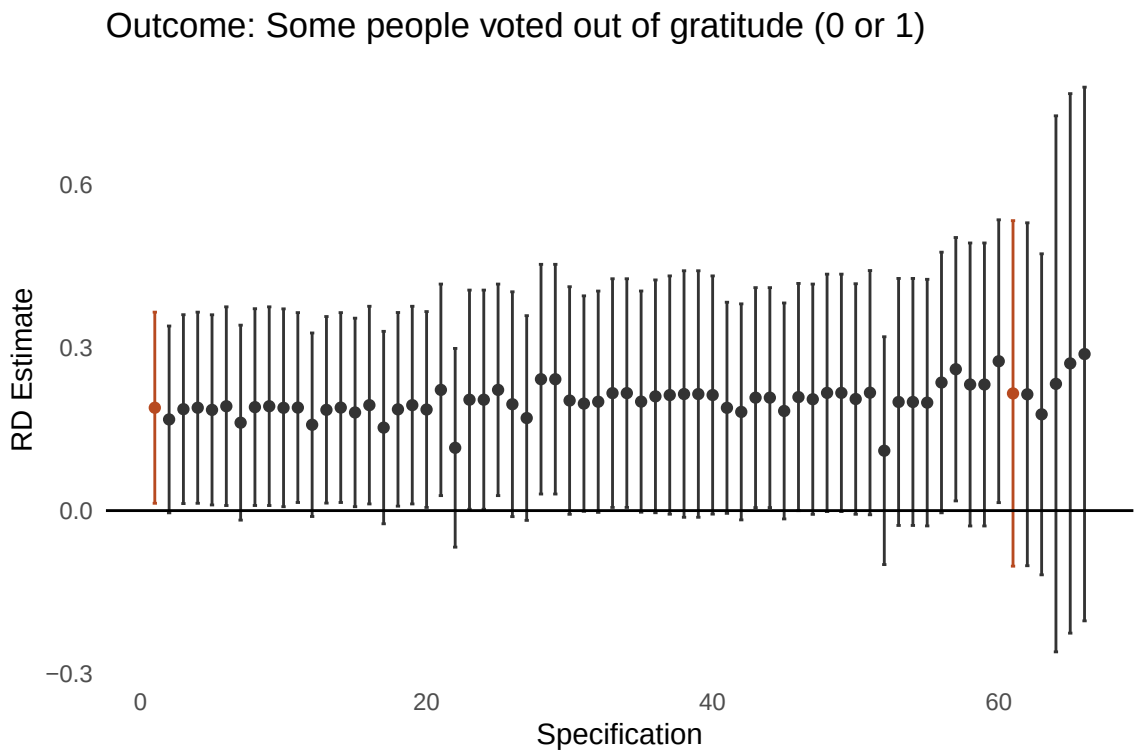


Figure 12: The figure reports the difference at the cut-point and 95% confidence intervals produced by `rdrobust` under different specifications, with the pre-registered specification in red.

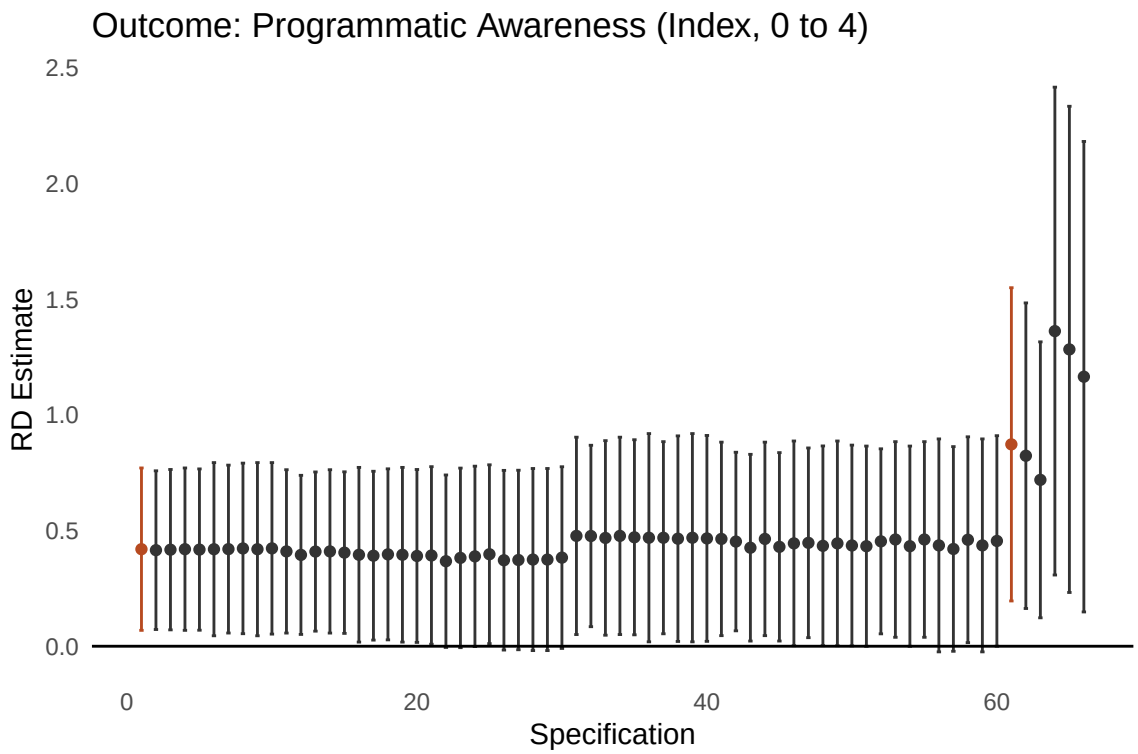


Figure 13: The figure reports the difference at the cut-point and 95% confidence intervals produced by `rdrobust` under different specifications, with the pre-registered specification in red.

E Programmatic Awareness

Table 10: Programmatic Awareness (Index and Components)

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

Note: See note to Table 4.

F Distributive Salience of Ethnicity

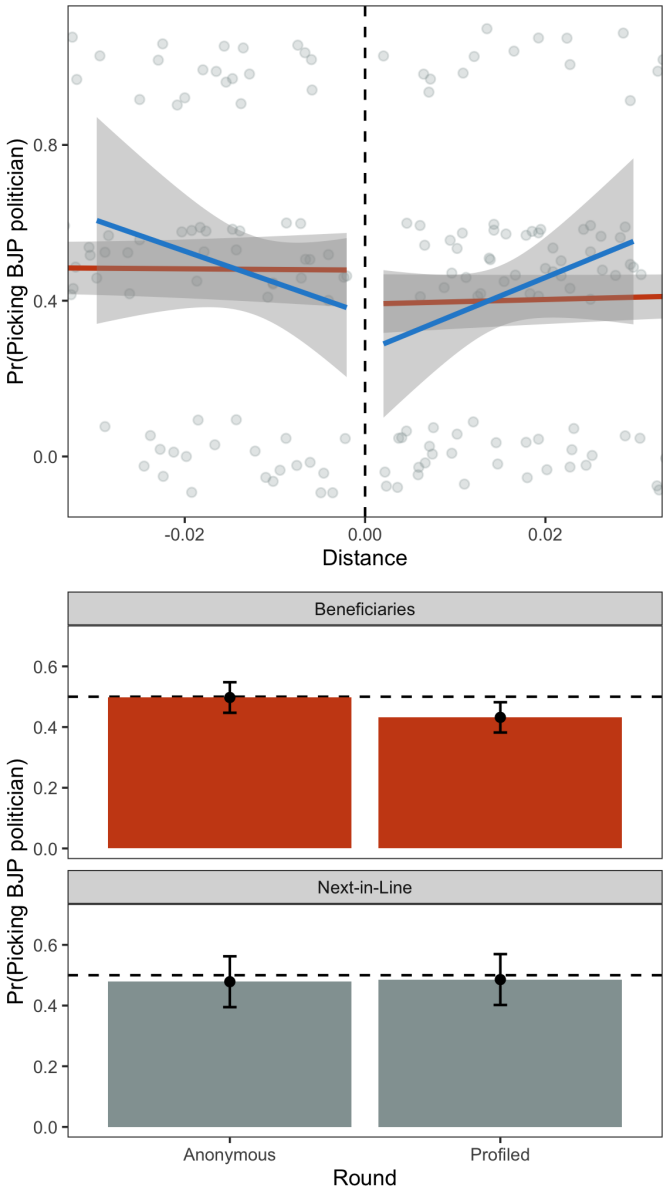


Figure 14: The top panel shows a regression discontinuity plot where the outcome is the probability of picking the BJP cueing politician in the Choose Your Dictator Game. The panel zooms-in on data around the cut-point ($\pm 3\%$), and shows estimates at the cut-point using two pre-registered specifications: 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 ($\pm 3\%$) 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.

G Attempts at sorting

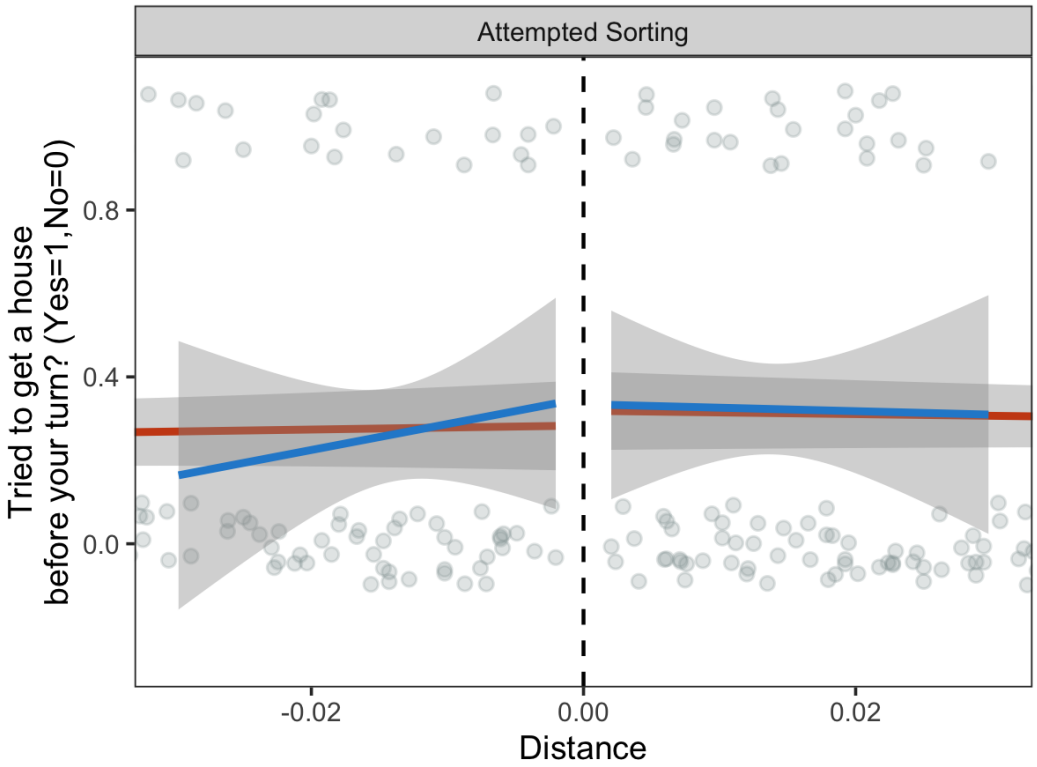


Figure 15: The figure shows a regression discontinuity plot where the outcome is whether the respondent attempted to get a house before their turn. The panel zooms-in on data around the cut-point ($\pm 3\%$), and shows estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth ($\pm 3\%$) and triangular kernel (in blue). 95% confidence intervals constructed using heteroskedasticity-robust standard errors are depicted in gray.

H Reputational Spillovers: National vs. Local

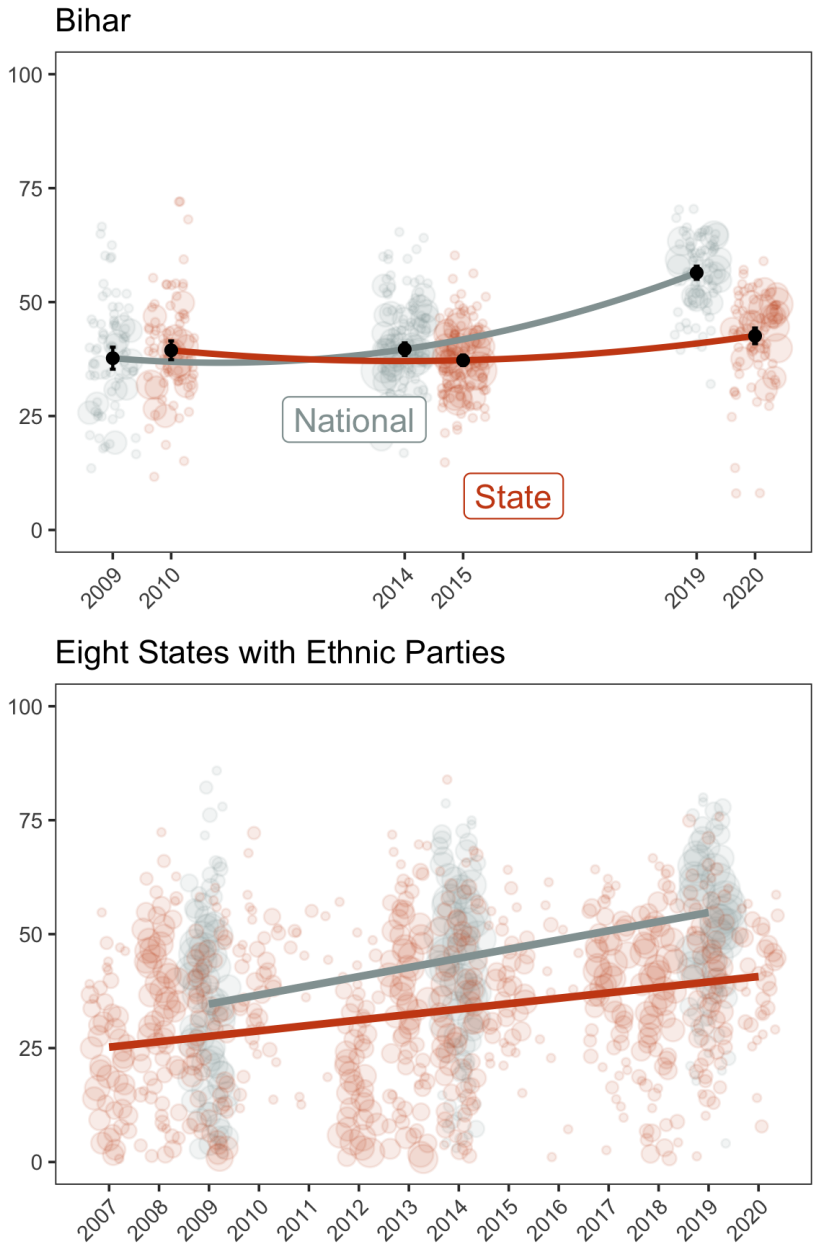


Figure 16: The figure plots the benefit-giving party's vote share at the state assembly constituency level in national elections (gray) and state elections (orange). The top panel presents this information for Bihar province. The bottom panel presents this information for eight Indian states with ethnic 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).

I Exposure to the scheme

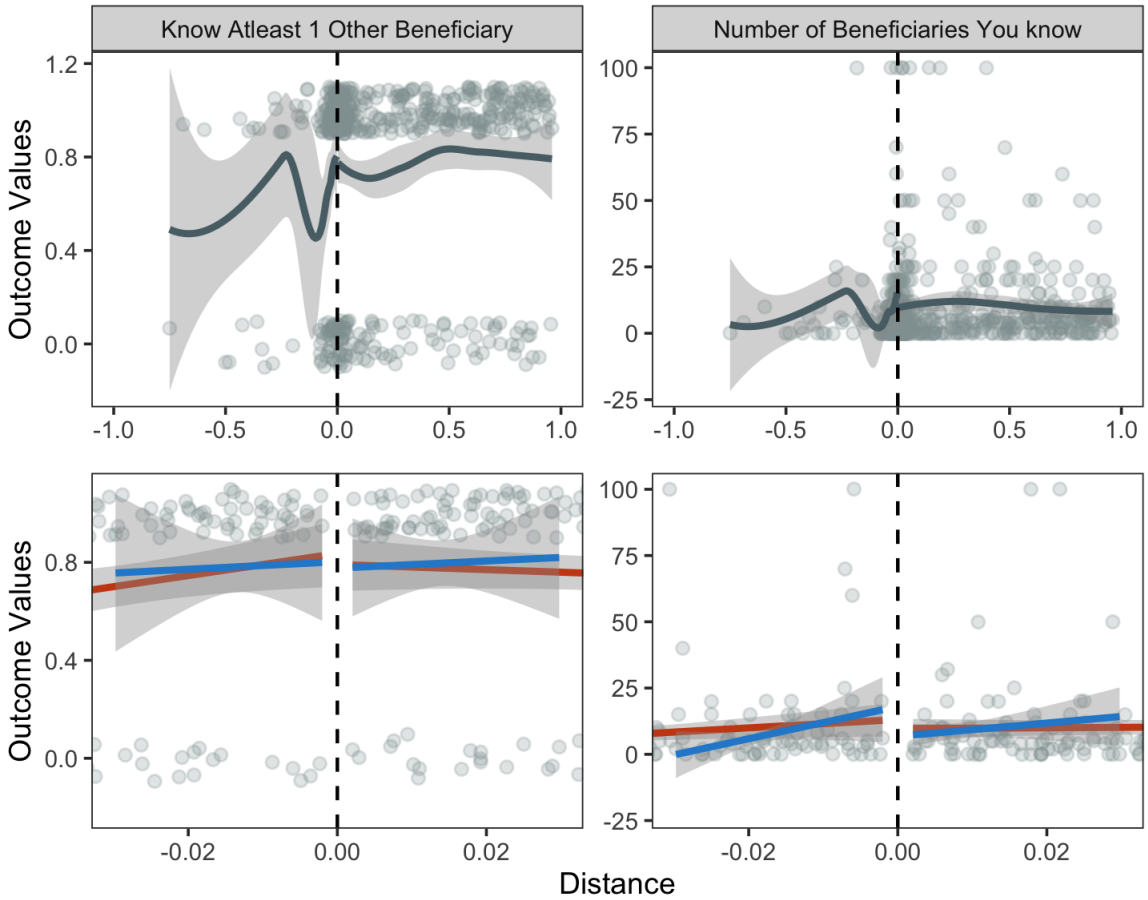


Figure 17: The top panel shows outcomes at different values of the forcing variable. We show the conditional means using a LOESS, with 95% confidence intervals in gray. The bottom panel zooms-in on data around the cut-point ($\pm 3\%$), and shows estimates at the cut-point using two pre-registered specifications: 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 ($\pm 3\%$) 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.

J Prejudice against Muslims

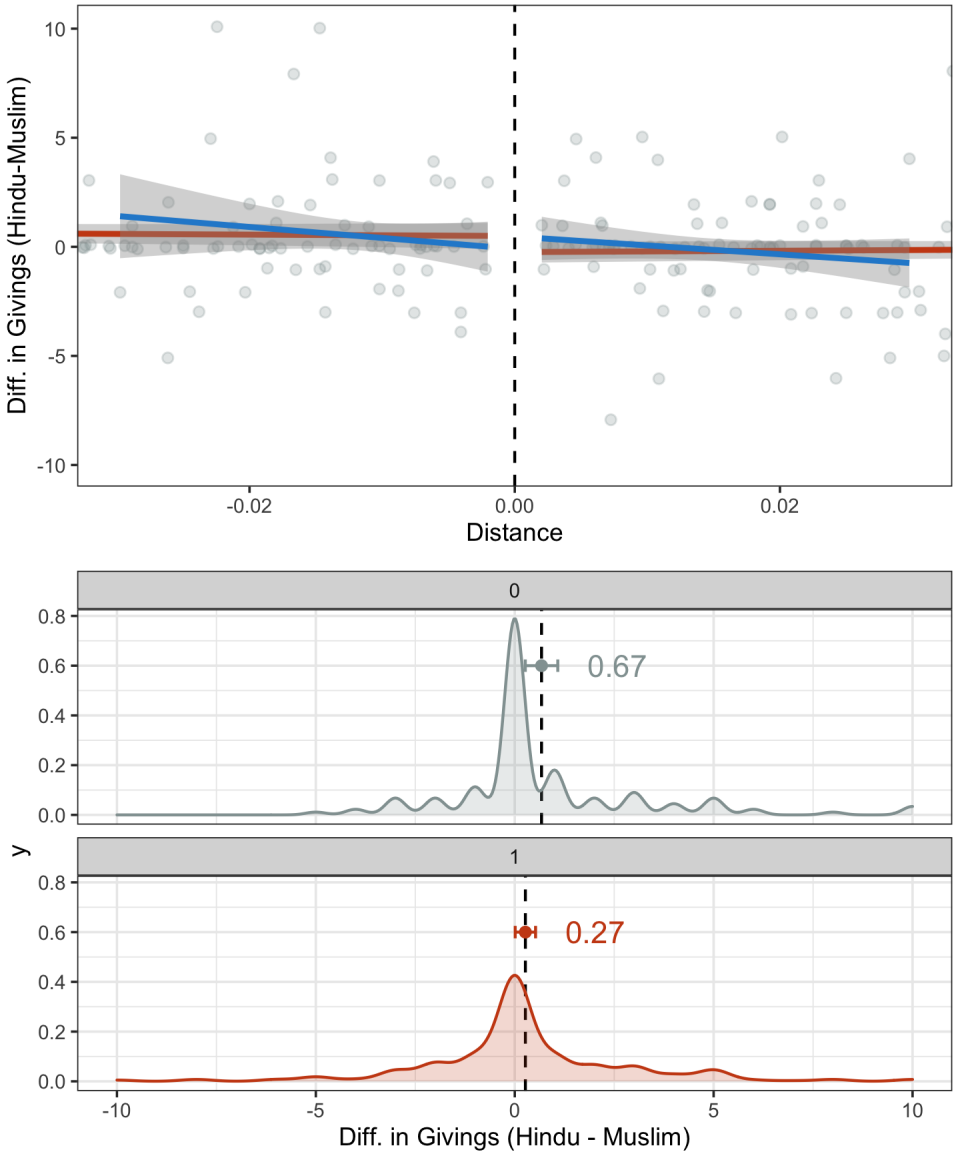


Figure 18: The top panel shows a regression discontinuity plot where the outcome is the difference in giving to a Hindu and Muslim recipient in a Dictator Game. The panel zooms-in on data around the cut-point ($\pm 3\%$), and shows estimates at the cut-point using two pre-registered specifications: a 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 ($\pm 3\%$) 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.

K Anticipation Effects

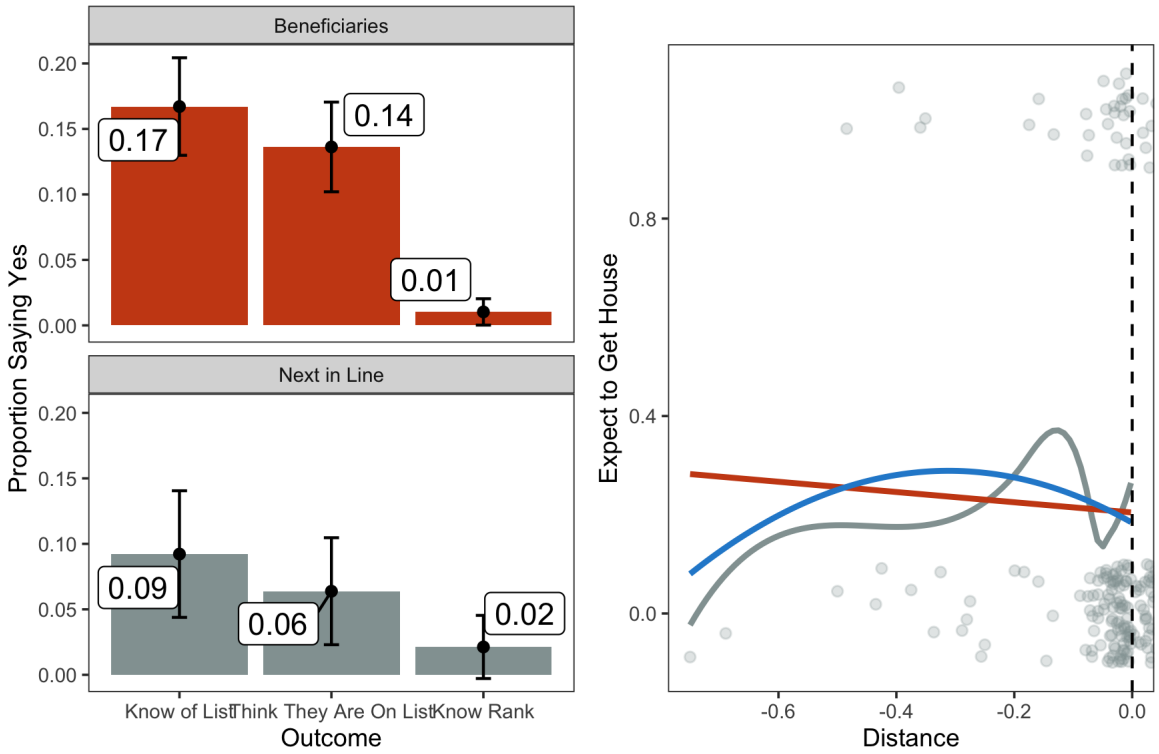


Figure 19: The right panel shows the proportion of respondents that know of a beneficiary list according to which houses were distributed, think that they are on this list, and know their rank on the list. The bar chart includes point estimates and 95% confidence intervals constructed using heteroskedasticity-robust standard errors. The left 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.

L Contact Rates

Table 11: Difference in Contact Rates at the Cut-Point

		RD (MSE optimal BW)				RD (BW = 3%)			
Outcome	Hyp.	$\widehat{\tau}$	SE	p	n	$\widehat{\tau}$	SE	p	n
Contacted (0=No,1=Yes)	No Diff	0.086	0.084	0.304	469	0.176	0.158	0.267	220

Note: See note to Table 4.

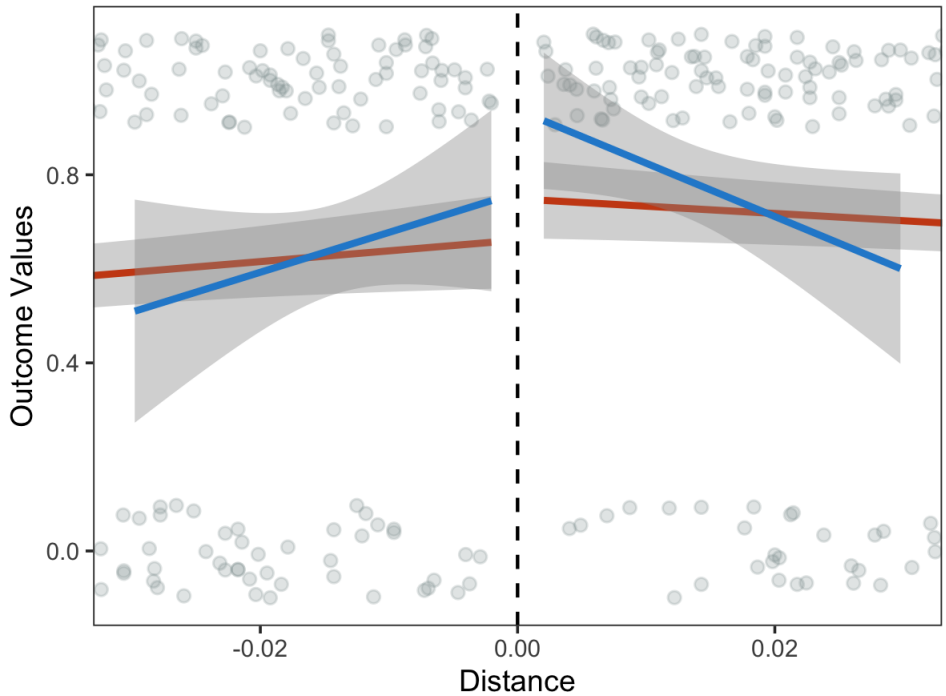


Figure 20: The figure shows a regression discontinuity plot where the outcome is whether the research team interviews (or contacts) a person in the sample frame ($0 =$ no, $1 =$yes). The panel zooms-in on data around the cut-point ($\pm 3\%$), and shows estimates at the cut-point using two pre-registered 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 ($\pm 3\%$) and triangular kernel (in blue). 95% confidence intervals constructed using heteroskedasticity-robust standard errors are depicted in gray.