Abstract

Three Essays on Distributive Politics in India
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This dissertation presents three essays on distributive politics in India:

- Governments distribute a variety of benefits to win votes. Why do some benefits have greater electoral impact than others? This paper provides descriptive evidence that a $10 cooking gas cylinder and $2000 house have comparable electoral impact in India. This motivates a typology in which distributive decisions can be organized on two dimensions: the cost of a benefit, and how it is distributed. Politicians face two key trade-offs: first, given a finite budget, they can widely distribute a cheap benefit or give an expensive benefit to fewer voters; and second, they can either distribute the benefit through brokers or as a rule-based, non-contingent, direct transfer. Clientelism skews distribution in favor of party loyalists but provides effective credit claiming. Programmatic distribution provides better targeting but worse credit claiming. Using data from India's National Election Studies, I show that there is political targeting of the cooking gas cylinder but not the house. Cooking gas cylinder recipients are also more likely to be contacted by the ruling party broker before elections but not house recipients. The evidence suggests that party elites pursue a mixed strategy of distribution: relying on brokers to deliver cheap benefits and government programs to deliver expensive benefits. Brokers make up for the value difference in benefits through effective canvassing.

- 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.

- Governments in developing countries spend a considerable amount of money distributing material benefits to their citizens. Some of these benefits are distributed through brokers, others as rule-based, non-contingent, direct transfers. Governments are less likely to adopt programmatic distribution if voters do not prioritize efficient implementation, namely less leakage and more accurate targeting. Since rule-based, non-contingent, direct transfers can end up benefiting out-partisans and ethnic out-groups, supporters of the ruling party should not punish their party for benefiting non-supporters. To assess whether voter behavior incentivizes programmatic distribution, I conduct two pre-registered studies in India: an online survey experiment and a telephone-based survey experiment fielded in 12 languages. Indian voters reward good distributive performance, but are more focused on outcomes than efficient implementation. They place a modest premium on distributive efficiency. Strikingly, ruling party supporters do not punish their party for benefiting ethnic out-groups. These findings suggest there are strong incentives for politicians to deliver benefits, though not entirely as rule-based, non-contingent direct transfers.

Three Essays on Distributive Politics in India

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Introduction

Governments in developing democracies distribute wide-ranging benefits to a broad sweep of citizens. In India, for instance, the government runs nearly 300 schemes that benefit 950 million people and in spending terms account for 3% of the gross domestic product in the last year (Economist 2022*a*). The benefits range from homes to home appliances. In this labyrinth of welfare provisioning, citizens have contrasting experiences while claiming benefits from the state. They get a free cooking gas cylinder when a party broker takes them to a public sector company's office, fills out forms for them, and navigates a bureaucracy that is perennially ready to reject applications on minor errors. Then there are surprises. A government officer knocks on the citizen's door to let them know that they are eligible for cash assistance to build a house. The money is directly transferred to their bank account, in tranches, of course, after a geotagged photograph confirming progress in construction is uploaded to a mobile application. The party broker is a bystander, ready to help if the citizen needs them but not in control of the money or who gets it. Across the developing world, politicians rely on different distributive strategies or delivery mechanisms to reach their citizens. Why do these politicians use brokers to distribute some benefits, and directly transfer other benefits using publicized rules and without conditions or quid pro quo?

The Argument

The three essays in this dissertation identify and evaluate the strategic considerations that shape politicians' distributive decisions, and the electoral consequences of those decisions. The central choice confronting politicians is whether they should distribute

benefits through clientelistic networks and intermediaries, or in a programmatic way. I establish this contrast by focusing on two, large welfare programs in India: an economically targeted, rural housing program that disproportionately benefits people who are ethnically opposed to the ruling party, eliminates the broker's role in identifying beneficiaries, and directly transfers money to the recipients' bank account; and a scheme that delivers free cooking gas cylinders through party agents and brokers.

Puzzlingly, I find that the $10 cooking gas cylinder has as much political impact as the $2000 house. Why is this the case? The first essay uses this empirical result to bring into focus politicians' distributive choices. The existing literature claims that brokers are a double-edged sword: their local embeddedness helps the party claim credit for benefits and mobilize support, but their involvement leads to rent-seeking, ethnic biases, and partisan targeting of benefits. The paper develops a typology of distributive decisions that draws on this insight and shows that politicians have an incentive to mix distributive strategies. They employ brokers to distribute cheap benefits and government programs to give out expensive ones. This strategy optimally engages brokers, using them to claim credit for cheap benefits that, on their own, may not have as much electoral impact. The strategy excludes brokers from distributing expensive benefits and thus reduces mistargeting and rent-seeking by brokers. The politicians' hope is that high-value rewards will elicit gratitude from voters, even without the intervention of brokers. These distributive choices have interesting electoral consequences. When brokers are excluded from the distribution process, they make no additional effort to canvass beneficiaries before an election. When benefits are distributed through brokers, they are more likely to canvass beneficiaries. In competitive elections, canvassing plays an important role in converting latent goodwill from benefits into votes for a party. The paper identifies this asymmetry in credit claiming as the reason for why a cheap clientelistic benefit can have as much electoral impact as an expensive programmatic benefit.

What impact does programmatic distribution have? Do voters, as politicians hope, reward the distribution of expensive benefits that do not involve brokers? The second essay evaluates the impact of the housing program. It leverages qualitative information about the program's implementation for empirical identification using a regression discontinuity design. The data comes from an original, face-to-face survey, fielded in 57 villages in three districts of Bihar province covering 530 households. It shows that people who were offered a house under the program recognize that the ruling party has done something for them. They are more likely to think some people voted for the ruling party out of gratitude. They are also more aware of the programmatic, non-discriminatory features of distribution, compared to people who were not offered a house. Across the board, there is very high support for the ruling party on national issues, and evidence that communities are saturated with the benefit. The paper considers a variety of explanations for the ruling party's popularity, especially among people who have not benefited from the program. It concludes that people seem to evaluate the ruling party's performance based on social outcomes more than pocket book considerations.

In multiethnic countries, discretionary distribution often results in ethnic favoritism (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), which then motivates ethnic voting (Chandra 2004). Does programmatic distribution blunt the logic of ethnic voting? Existing work suggests that people support co-ethnic politicians or ethnic parties because they expect more benefits from them (Chandra 2004). The housing program undercuts this logic in two ways: a party that does not champion the voters' material interests gives them an expensive benefit, and makes explicit the non-ethnic, economic basis for giving that benefit. Did this succeed in weakening ethnic preferences? I find that in a behavioral game, ethnic considerations still heavily shape

beliefs about politicians' distributive intent. The housing program fails to reduce the distributive salience of ethnicity.

Why then should politicians distribute benefits through rule-based, non-contingent, direct transfers? The third essay identifies conditions under which politicians would pursue programmatic distribution. Empirical evidence from the first two essays suggests that programmatic distribution reduces leakages, improves last-mile delivery, and minimizes broker discretion which is responsible for partisan or ethnic-based targeting of benefits. When publicized, objective rules determine who gets the benefit, people who support an opposition party or those outside the ruling party's ethnic core can also end up benefiting from a welfare program. For this type of distributive strategy to be electorally viable voters should recognize and reward good performance, they should care about efficient implementation (i.e. reducing leakages and discretion), supporters of the ruling party should not punish their party for benefiting out-partisans and ethnic out-groups, and those voters should ideally reward the ruling party for channeling benefits to them.

Using survey experiments that randomly vary information about distributive policies and outcomes, I show that people reward good performance and punish bad distributive outcomes. They value efficient implementation but place a modest premium on it. Supporters of the ruling party do not punish it for benefiting out-partisans and ethnic out-groups. People outside the ruling party's ethnic core do not additionally reward it for channeling benefits to them. Overall, there are strong incentives for parties to deliver benefits but with only modest rewards for efficient implementation, not entirely as rule-based, non-contingent, direct transfers.

The coexistence of clientelism, ethnic voting, and programmatic distribution then is down to three things: parties' continued dependence on brokers for credit claiming, ideological dissemination, and risk diversification; voters' modest reward for efficient implementation of policies; and the stubborn persistence of expectations

of ethnic favoritism. There is reason to cheer: voters, even in an ethnically divided and polarized polity, recognize and reward good distributive outcomes, creating strong incentives for politicians to deliver. The evidence in support of these claims comes from a variety of sources, and research designs. Table 1.1 summarizes the type of evidence — research design and data source — for each major finding.

Table 1.1: Research Design and Data Sources

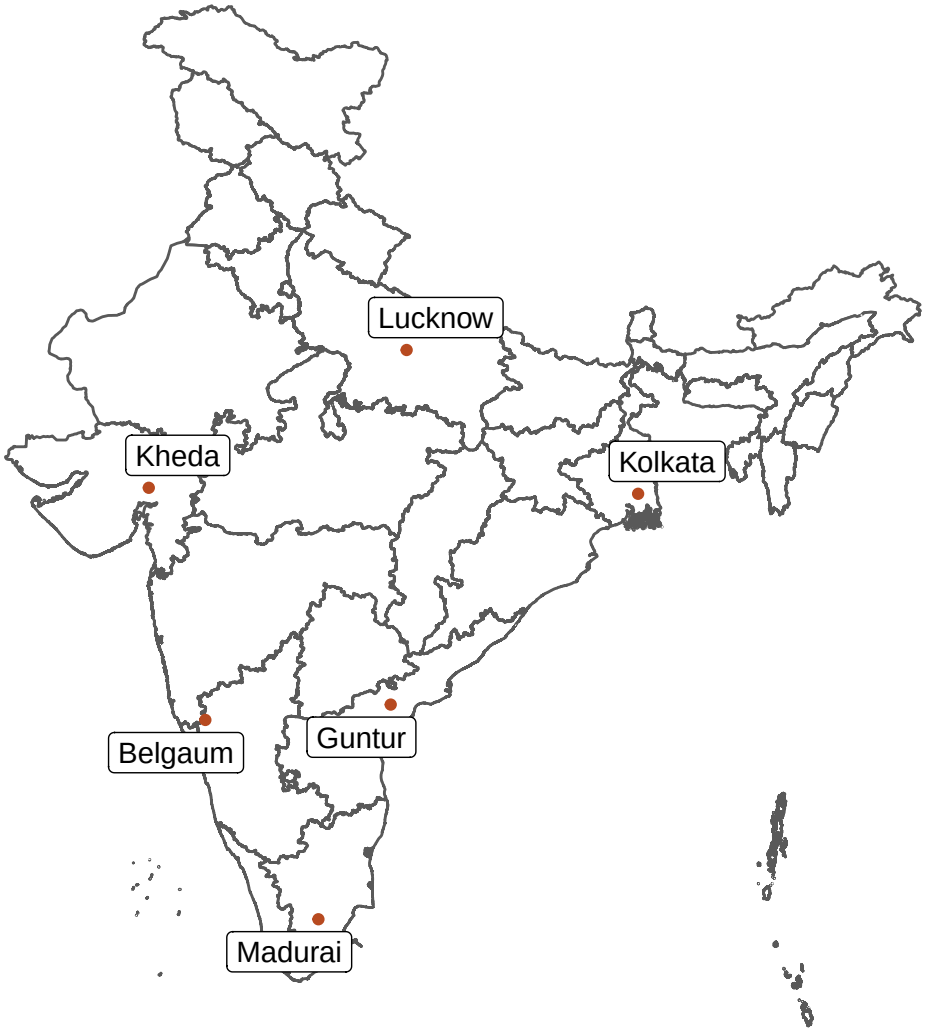
Claim	Research Design	Data Source
Clientelistic benefits are politically targeted but brokers make extra effort to canvass beneficiaries. Less mistargeting but weaker credit claiming in programmatic distribution.	Fixed effects models, Close elections regression discontinuity design.	National Election Studies 2019 ($n = 24,236$).
Housing program generates support for the ruling party but does not undercut the logic of ethnic voting.	Naturally occurring regression discontinuity design.	Face-to-face surveys ($n = 530$ households) in 57 villages across 3 districts of Bihar province.
Voters recognize good performance, and modestly reward efficient implementation. Core supporters of the ruling party do not punish it for benefiting out-partisans and ethnic out-groups.	Survey Experiments	Online survey ($n = 1,047$), Telephone survey in 12 different languages ($n = 5,350$).

Intuitions from Exploratory Fieldwork

These essays also draw on over two years of exploratory fieldwork in India. Figure 1.1 shows the six field sites where I conducted over 65 interviews and focus group discussions between 2017 and 2020. These conversations involved voters, politicians, bureaucrats, local journalists and academics. They build on prior fieldwork in five of these sites by Myron Weiner in the early 1960s and Atul Kohli in the late 1980s. Weiner and Kohli's fieldwork resulted in books, Weiner (1967) and Kohli (1990), that

provide a rich description of local politics, political organizations and their functioning, and electoral mobilization. My interviews were informed by these descriptions, and probed similar questions with the aim of creating a qualitative time series.

Figure 1.1: Field Work in India (2017 to 2020)



Note: A map of India that shows the six field sites where I conducted interviews and focus group discussions between 2017 and 2020. 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/.

The interviews yielded three intuitions that motivate the essays: first, politicians think it is vital for them to deliver benefits to their constituents to remain

electorally competitive; second, voters and politicians continue to value brokers and local intermediaries; and third, ethnic preferences or expectations of favoritism persist among voters. In the remainder of this chapter, I dwell on each of these insights, bringing in voices or anecdotes from the field to substantiate claims.

In my interviews, politicians consistently highlight the importance of delivering benefits to their constituents if they want to remain electorally competitive. Being an accessible and effective problem-solver is a necessary though not sufficient condition to win elections. For example, a multiple term elected representative in Guntur described their core work as:"*main iska aur uska bhi karunga. Han, apne ka thoda zyada kar dunga par sabka kar dunga... officer ke aage sar jhukata hoon janta ka kaam karwane ke liye*" (I will do this or that person's work. Yes, I might do slightly more for my own people but I will do everyone's work... I bend before bureaucrats to get people's work done). The politician's son, who manages the election campaign, adds that "relations [with people]" and "work done in last five years" are most important. The point is illustrated later during their door-to-door campaign, which I joined to better understand voter-politician interactions. An old woman, visibly upset, stopped the politician in one of the *galis* (alleys) and scolded him, "look there [pointing 200 feet to her left], there is a street light there but not in our *gali* (alley). It gets dark here and we need lights too." The politician promises to "look into it". The woman says, "you have not in five years. We came to you several times but nothing happened. We have 10 votes in this house. They will only go to you if the street light is put". The candidate signals to an aide that the work be done, then asks for her support. "Only if the street light is put" says the woman. The politician's son turns to me and says, "this is how voters are today. They want performance and work to be done. Otherwise no vote". A municipal level politician in Kolkata makes a similar assessment: "*voter apne vote ki vakat jaanta hai. Log sochne lage: kya ye candidate meri madad karega, kaam karne wala hai? Chahe dakait ho, agar*

kaam karega toh jeet jayega. Service bahot important ho gayi hai" (voters know the value of their vote. People think: will this candidate help me, will they do work? Even if they are a dacoit, if they do work they will win. Service has become very important). A parliamentarian concurs: "if you don't behave well, meet [people], do work, you will lose". In far away Belgaum, a local politician says the same thing: "*theek se baat kare, hastey hastey mile ... 50 percent kaam isme hee ho jata hai ... phir main is kaam karna chahiye*" (you talk properly to people, meet them with a smile ... 50 percent of the work is done if you do this much... after this the main thing is you must do work). In Kheda, a candidate points out that "*log badi badi batein nahi samajh pate*" (people don't understand big or complicated talk), they are "only interested in *pani, bijli, rasta, aur rozgar*" (water provision, electricity supply, road construction, and employment generation). Back in Guntur, a politician from the rival party provides a similar assessment: "now politics is all about welfare schemes. Earlier only 15 percent of [the] budget was spent on such schemes, now 50 percent is spent on this [and] not infrastructure or planned expenditure".

Brokers help citizens claim these benefits, and politicians get credit for them. As a former parliamentarian put it, a "strong booth level organization" is vital to win elections. Politicians need workers who "will get voters [to the polling booth], convince them, impress them with their arrangements". Politicians need to identify influential brokers ("*jinki chalti hai*") who will serve as "contact points for future work, and will get work done with the member of parliament". For citizens, the broker reduces the cost of interfacing with the state. A politician in Lucknow describes the broker as someone "*jo rozmarrah ki zindagi aasaan karta hai: kachahri mein, thane mein, haspatal mein, aur bacchon ka school mein admission karane ke liye*" (who makes everyday life easier: in courts, police stations, hospitals, and getting children admission in school). The broker's key attribute is that they "can stand up to the state ... speak with confidence and without fear". The journalist in Belgaum

offers a similar characterization: every street has "*yuvak sangathan*" (youth organizations) that help locals ("shoulder burdens...help out during a wedding, donate blood in the hospital, deal with everyday problems of households, and organize religious events") and seek benefits from politicians in exchange for canvassing for them. In Kolkata, an interviewee tells me, "there is a club culture. *Para* (neighborhood) club is important. [The] government gives them money. Clubs keep local youths occupied. They organize social events, do helpful tasks for residents. At the time of elections, they campaign for a party". The village headman or *mukhya* performs a similar role in rural communities: arbitrating disputes, resolving issues, helping people deal with the state (particularly police stations, courts, and hospitals).

Identities, especially caste, religion and language, also continue to play an important role in social, economic and political life. Even as ethnic groups are spatially mixed in urban, and increasingly, rural contexts, ethnicity is correlated with socioeconomic affluence producing very different modal living conditions in different groups. As a retired Indian Administrative Service (IAS) officer told me in an interview, "Scheduled Caste *bastis* (settlements) are the dirtiest. The entire village's sewage water flows into the *basti*. It's a marshland. People think Dalits are unclean, uneducated, and lazy". This kind of stereotyping and discrimination has been shown to affect economic interactions (Deshpande 2011; Jodhka 2010; Madheswaran and Attewell 2007; Deshpande and Newman 2007). It also fuels expectations of ethnic favoritism in politics. A journalist in Kheda explained the rationale of ethnic voting this way: "*apna naati hai, yeh kaam karega... kaam turant ho jata hai*" (he or she is our kin, they will work for us ... work happens immediately or speedily). Another journalist in Lucknow put it similarly: "*hamara hai, hamare liye kaam karega*" (he or she is one of us, they will work for us). A social activist in the same field site described caste-based politics as the "easiest mode — all you have to do is give a call in the name of the group and you will get votes... issue-based politics and mobilization

is harder ... you have to launch agitations, fight on the streets, and this is difficult work".

Three factors, in particular, seem to explain ethnicity's continued salience in politics: ethnic quotas, how parties select candidates, and ethnic associations. Ethnic quotas in education and public recruitment support instrumentalist reasons for ethnic voting. A politician in Lucknow put it this way, "*jati svaarth se judi hai, koi adarsh se nahi. Sarkari bharti mein preference milegi*" (caste is linked to self-interest, not some ideology. People think they will get preference in public recruitment [if a coethnic politician controls it]). While voters' material self-interest is one factor, how parties select candidates and mobilize support is another. In Belgaum, a politician made this point: "caste is not declining, it continues to influence politics...they put it in the voter's mind that this politician is from your caste and this influences the voter's thinking. Powerful lobbies that want to capture power still think in community terms. They think that if a constituency has a lot of Lingayat [ethnic group] voters, we should give the party ticket to a Lingayat". A journalist in the town echoes the same sentiment: "politicians say vote for me so that our community remains dominant and influential... *parties caste ka afeem logon ko sunghati hain, politician log isse drops mein logon ko dete hain*" (parties intoxicate people with the caste drug, politicians drip-feed it to the people). Parties or politicians are often helped in this by influential and resource-rich ethnic associations that run educational institutions, hospitals, banks, and places of worship. These associations fund candidates, mobilize support for them, and lobby the government for favorable distributive policies. The favored party or politicians, in turn, protect the association's economic and social interests. Or as a politician, who is not from the dominant caste group, explained why that caste is politically influential: "they control sugar factories and cooperatives ...[that] have large money backing and captive vote banks" and "societies, *muths* (temples), *swamijis* (religious leaders) that help pass on the message".

Contribution

The three essays improve our understanding of the politics of development in several ways. The first essay develops a typology of distributive decisions based on politicians' strategic considerations. The framework explains why it is optimal for politicians to distribute through brokers and rule-based, non-contingent, direct transfers. A more realistic and nuanced picture of distributive politics emerges in this discussion, one in which clientelism and programmatic distribution coexist, and one does not displace the other (Larreguy, Marshall, and Trucco 2018). The essay also points to an important reason why clientelism persists even when politicians are able to cheaply and directly communicate with voters: politicians still need brokers to claim credit for some welfare programs, disseminate their ideology, and diversify risk. Clientelism serves a utility beyond programmatic signaling (Mares and Young 2019). There are also implications for party brands, which will be less coherent and consistent: part clientelistic and part programmatic. Party platforms will also converge or be less distinct from one another. The coherence and convergence of party platforms affect partisanship (Lupu 2016).

The second essay 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. This paper joins a burgeoning literature on cross-ethnic political appeals (Gadjanova 2021; Arriola et al. 2020; Thachil 2014; Ichino and Nathan 2013). It also points to two less prominent mechanisms through which welfare programs shape political preferences: gratitude and sociotropic considerations. People that benefit from the program recognize that the incumbent has done something for them, and are more likely to say that some people voted for the incumbent out of gratitude. Saturating communities with a private (excludable) benefit also generates support among those that do not get that benefit. The yet-to-benefit look to their

neighbors, and evaluate the incumbent by observing social outcomes, not just personal gain. Even so, the pay-off from programmatic distribution is limited (Zucco Jr. 2013). 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. Ethnicity can continue to be an obstacle for democratic accountability (Adida et al. 2017). On the positive side, programmatic distribution does seem to improve last-mile delivery: beneficiaries are highly satisfied with the program, fewer instances of bribing, and more "deserving" people get the benefit (less mistargeting). Crucially, beneficiaries are aware of the programmatic features of distribution.

Finally, the third essay evaluates conditions under which programmatic distribution is electorally viable. Politicians will find it profitable to distribute benefits via rule-based, non-contingent direct transfers only when voters reward good performance, value less leakages and discretion, and supporters do not punish cross-ethnic distribution. These conditions build on prior work that focuses on structural factors like socioeconomic development and party competition (Wilkinson 2007; Levitsky 2007; Magaloni, Diaz-Cayeros, and Estvez 2007; Weitz-Shaprio 2012; Stokes et al. 2013; Mares and Young 2019). The presence or absence of some types of voter behavior help explain why some programs win votes (Manacorda, Miguel, and Vigorito 2011; Labonne 2013; Zucco Jr. 2013; Larreguy, Marshall, and Trucco 2018) but others do not (Kadt and Lieberman 2017; Imai, King, and Rivera 2020).

House Versus Cooking Gas Cylinder: Assessing the Political Impact of Two Benefits

Governments distribute a variety of benefits to win votes. Why do some benefits have greater electoral impact than others? This paper provides descriptive evidence that a $10 cooking gas cylinder and $2000 house have comparable electoral impact in India. This motivates a typology in which distributive decisions can be organized on two dimensions: the cost of a benefit, and how it is distributed. Politicians face two key trade-offs: first, given a finite budget, they can widely distribute a cheap benefit or give an expensive benefit to fewer voters; and second, they can either distribute the benefit through brokers or as a rule-based, non-contingent, direct transfer. Clientelism skews distribution in favor of party loyalists but provides effective credit claiming. Programmatic distribution provides better targeting but worse credit claiming. Using data from India's National Election Studies, I show that there is political targeting of the cooking gas cylinder but not the house. Cooking gas cylinder recipients are also more likely to be contacted by the ruling party broker before elections but not house recipients. The evidence suggests that party elites pursue a mixed strategy of distribution: relying on brokers to deliver cheap benefits and government programs to deliver expensive benefits. Brokers make up for the value difference in benefits through effective canvassing.

Introduction[1]

Governments routinely distribute benefits that range from homes to home appliances (De La O 2013; Manacorda, Miguel, and Vigorito 2011; Nazareno, Stokes, and Brusco 2006; Goyal 2019; Kumar 2021*a*,*b*; Bueno, Nunes, and Zucco Jr. 2017; Barnhardt, Field, and Pande 2015; Pop-Eleches and Pop-Eleches 2009; Nair 2020). Some of these benefits win a lot of votes, others less so. Why is it that some benefits win more votes and move political preferences more than others? The distributive politics literature assumes, sometimes implicitly, that more expensive benefits have greater impact on preferences (Lindbeck and Weibull 1987; Dixit and Londregan 1996; Heath and Tillin 2018). Yet, as this paper shows, a cheap benefit can have as much political impact as an expensive benefit. What explains this variation?

To motivate this study, I compare the electoral impact of two, one-time benefits distributed by India's government: a $10 cooking gas cylinder, and $2000 cash assistance to build a house. India is an interesting case because it is a populous, developing democracy, with considerable welfare spending, political parties with clientelistic infrastructure, and state capacity to support programmatic distribution.

A regression analysis shows that receiving either of the benefits is associated with similar changes in support for the ruling party, after controlling for a variety of factors that affect selection into benefits and political preferences. Gas cylinder recipients, on average, are 5 percentage points more likely to vote for the ruling party in a parliamentary election, while home recipients are 4 percentage points more likely to vote for the ruling party. This empirical pattern is observed for a variety of measures: satisfaction with the government, performance evaluations, incumbent's re-election prospects, and voting for opposition parties. In other words, the partial correlation between receiving a benefit and supporting the ruling party does not

[1]My sincere thanks to Sanjay Kumar and the Lokniti team at the Centre for the Study of Developing Societies for providing the National Election Studies dataset.

depend on the monetary value of the benefit.

The distributive politics literature does not adequately address this empirical anomaly. First, voting models predict a positive relationship between benefit size and preference change (Lindbeck and Weibull 1987; Dixit and Londregan 1996; Heath and Tillin 2018). Second, most studies fix the benefit and evaluate the distributive strategy (Kramon and Posner 2013; Larreguy, Marshall, and Trucco 2018), thereby side-stepping the question of what happens if we vary the benefit size. In practice, politicians simultaneously distribute many benefits, using different distributive strategies. What are the electoral consequences of these choices?

I propose a parsimonious framework to understand distributive decisions, and their electoral consequences. I argue that in developing democracies, politicians face two trade-offs while making such decisions. When it comes to the benefit, they have a finite budget which implies that they can give a cheap benefit to several voters, or an expensive benefit to fewer voters. When it comes to distributive strategies, politicians can either engage party brokers or apolitical bureaucrats.[2] Clientelism skews distribution in favor of party loyalists but provides effective credit claiming and monitoring of voters (Stokes et al. 2013; Muralidharan et al. 2021; Banerjee et al. 2020; Muralidharan, Niehaus, and Sukhtankar 2016). Programmatic distribution leads to potentially more efficient socio-economic targeting but weaker credit claiming and voter monitoring. Crucially, the loyalist-skew in clientelism tends to be more acute for an expensive benefit (which can only be given to a few people). In contrast, weak credit claiming and monitoring is a feature of all programmatic distribution, irrespective of benefit size.

These trade-offs explain why some material benefits have greater political impact than others. For example, a cheap benefit, distributed through brokers, can win more votes than an expensive programmatic benefit. There are two reasons for this:

[2] I classify countries where the bureaucracy is packed with party brokers as a case of clientelism, not programmatic distribution.

the cheaper benefit reaches a more persuadable or easily mobilizable audience, or it is reinforced with better credit claiming and voter monitoring.

I evaluate these explanations in the context of two benefits distributed by the Indian government. I show that the cooking gas cylinder was distributed with the help of brokers, while the house was not. Using a fixed effects model, I show that past vote, specifically support for the ruling party in the previous parliamentary election, strongly predicts getting the clientelistic benefit but not the programmatic benefit. I then employ a regression discontinuity design to understand the distributive consequences of a narrow election win for the ruling party, treating a coin-flip win as an exogenous shock to clientelistic resources. This analysis shows that the probability of receiving a cooking gas cylinder sharply increases in constituencies where the ruling party narrowly wins in the previous election, compared to where it narrowly loses. Furthermore, the probability that a party loyalist receives the cooking gas cylinder also increases significantly at the cut-point. This is evidence of political targeting and the loyalist skew expected under clientelism (Stokes et al. 2013). Neither of these patterns appear for the housing program. Finally, a regression analysis shows that cylinder recipients are more likely to be contacted by the ruling party's canvassers but this is not the case for house recipients.

Overall, the evidence points to the pivotal role of brokers. Distributing benefits can generate good will but brokers are needed to convert that latent good will into votes. This is why a cheap clientelistic benefit can match the impact of an expensive programmatic benefit.

These findings have important implications for the politics of development. The paper focuses on the strategic considerations that inform politicians' distributive decisions. It develops a theoretical framework to understand how politicians distribute benefits of varying value using different delivery channels. Most prior work focuses on one benefit, and evaluates how it is distributed or how some intervention can increase

efficiency. I engage with the possibility that politicians simultaneously distribute several benefits of varying value, and simultaneously use different distributive strategies. There is a mixing of distributive strategies in which brokers distribute some benefits and government programs are used to give out others. Clientelism and programmatic distribution coexist, rather than one displacing the other (Larreguy, Marshall, and Trucco 2018). This echoes Weghorst and Lindberg (2013)'s findings in Ghana where programmatic appeals do not weaken the efficacy of clientelism. It is also different from Mares and Young (2019)'s explanation for why clientelism persists. In Eastern Europe, politicians use different types of clientelism for programmatic signaling (i.e. support for or opposition to workfare programs), and voters are less likely to punish clientelistic practices when they perceive policy alignment (Mares and Young 2019). In contrast, this paper posits that clientelism persists even when politicians are able to cheaply and directly communicate with voters because they still need brokers to claim credit for some welfare programs, disseminate their ideology, and diversify risk.

The mixing of distributive strategies also has interesting implications for partisanship. If parties distribute some benefits through brokers and directly transfer other benefits using publicized rules and without conditions, voters will remember them for partially tolerating rent seeking and leakages but also some efficient programs. Party brands will be less coherent or consistent: part clientelistic and part programmatic. They will also converge or be less distinct from one another. The coherence and convergence of party platforms affect partisanship (Lupu 2016). On the empirical side, this is the first of its kind comparative evaluation of two, large-scale welfare programs in India using individual level data.

The remainder of the paper is structured as follows: I motivate the study with an example from India where two benefits, unequal in monetary value, have an equal impact on political preferences; then survey the existing literature, describe my argument, and present three pieces of empirical evidence in support of that argument.

Puzzle

India's federal government distributes a variety of benefits, some cheap, others expensive. This paper focuses on two flagship welfare programs of the BJP government: *Ujjwala* and *Awas Yojana*. *Ujjwala* provided a free liquefied petroleum gas (LPG) cylinder worth $10 to nearly 72 million households.[3] The *Awas Yojana* provided $2000 to poor households in rural areas to build a *pucca* (cement) house. Between April 2015 and December 2019, 8.8 million houses were constructed under the program, with 10 states accounting for 93% of the houses and 91% of eligible beneficiaries.[4] According to contemporary media reports and research papers, these schemes, in particular the two benefits described above, won votes for the BJP in elections. For example, see Attri and Jain (2019); Mukherjee and Waghmare (2020); Deshpande, Tillin, and Kailash (2019).

I probe this claim further by using data from the National Election Studies 2019 (details in the data section), and focusing on rural areas in 10 states where a large number of houses were built. I employ an ordinary least squares regression with the following specification in `lm_robust`:

$$\mathrm{Y}_{i,t} = \alpha_0 + \beta_1(\text{Got a house})_{i,t-1} + \beta_2(\text{Got a cylinder})_{i,t} + \rho\mathbf{X}_{i,t} + \sum_{j=1}^{J} \gamma_j \text{Booth}_j$$

where $Y_{i,t}$ captures electoral support for the BJP using a variety of survey measures[5], $\mathbf{X}$ is a vector of control variables like past vote choice, ethnicity, mean-centered age,

[3]Data from the Ministry of Petroleum and Natural Gas, Government of India. Accessible here. Figure as of May 22, 2019. India's parliamentary elections were conducted between April 11 and May 19, 2019, and the votes were counted on May 23, 2019.

[4]Data from the Ministry of Rural Development, Government of India.

[5]Appendix 2 explores the association between receiving a benefit and political ideology. I find little evidence that receiving a house or gas cylinder changes political ideology.

Table 2.1: Political Impact: House v. Gas Cylinder

	Voted BJP (0/1)	Voted NDA (0/1)	Voted Ethnic Party (0/1)	Re-elect Incumbent (0/1)	Govt Satisfication (-2 to +2)	BJP works for poor (0,0.5,1)
Got a house (β_1)	0.035^{**}	0.040^{**}	-0.016	0.033^{*}	0.043	0.016
	(0.012)	(0.013)	(0.010)	(0.014)	(0.042)	(0.011)
Got a cylinder (β_2)	0.046^{***}	0.037^{**}	-0.022^{*}	0.047^{***}	0.181^{***}	0.027^{**}
	(0.011)	(0.012)	(0.009)	(0.012)	(0.037)	(0.010)
Adj. R^2	0.567	0.523	0.548	0.461	0.363	0.333
Num. obs.	6019	6019	6019	5777	6507	6237
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Booth FE	Yes	Yes	Yes	Yes	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

gender (female or not), education, and monthly household expenditure (binned). The specification include polling booth fixed effects to adjust for any confounding due to time invariant factors at the precinct level. β_1 and β_2 are the parameters of interest. β_1 captures the partial correlation between getting a house and supporting the BJP. β_2 captures the same relationship for the cooking gas cylinder. Going into the analysis, I have two expectations: (i) receiving a benefit should increase support for the BJP ($\beta_1 > 0$ and $\beta_2 > 0$); and (ii) the expensive benefit should have greater impact than the cheaper benefit ($\beta_1 > \beta_2$).

Table 2.1 reports $\widehat{\beta_1}$ and $\widehat{\beta_2}$ from this analysis. The full results are reported in Table A2.1. Table 2.1 reports the coefficient estimates for a variety of measures: voting for the BJP in the parliamentary election, voting for the BJP-led National Democratic Alliance, voting for ethnic parties (most of which are in opposition to the BJP), pro-incumbency sentiment, satisfaction with the government, and evaluation of its performance. Home recipients are 3.5 percentage points more likely to vote for the BJP, 4 percentage points more likely to vote for the NDA, and 3.3 percentage points more likely to support re-electing the incumbent government. Cylinder recipients are

4.6 percentage points more likely to vote for the BJP, 3.7 percentage points more likely to vote for the NDA, 2.2 percentage points less likely to vote for an ethnic opposition party, 0.18 scale units more satisfied with the government, and 2.7 percentage points more likely to think the BJP works for the poor.

In terms of our initial expectations: (i) $\widehat{\beta_1}$ is statistically distinguishable from 0 in three out of six cases, $\widehat{\beta_2}$ is distinguishable from 0 in all six cases; and (ii) $\widehat{\beta_1} < \widehat{\beta_2}$ in five out of six cases but we fail to reject the null hypothesis of $\beta_1 = \beta_2$ in every case. On balance, there is little evidence to suggest that the expensive benefit had greater "impact" than the cheaper benefit.

This finding is robust to alternative specifications. In the appendix, I estimate ordinary least squares regressions separately for each benefit. The specification includes the same control variables and fixed effects. Table A2.4 reports the coefficients for the housing program. Table A2.5 for the cooking gas cylinder scheme.[6] Again, the partial correlations appear stronger for the cooking gas cylinder scheme. This continues to be the case if the model includes an interaction term between receiving a benefit and ethnic categories.[7] I get similar results if the analysis includes all rural respondents (Table A2.2), or all survey respondents with an additional control variable for ruralness (Table A2.3). In fact, the point estimate for β_2 is typically twice the size of the point estimate for β_1 in these specifications. In summary, there is pretty consistent evidence, at least observationally, that the expensive benefit does not have greater political "impact" than the cheap benefit. Why might this be the case?

[6]For comparison, Table A2.6 reports the results from a model in which the explanatory variable is an index of whether the respondent has benefited from six flagship programs of the BJP government (0 = not benefited from any of the programs, 1 = benefited from all of them). Compared to a voter who does not benefit from any government program, a voter who benefits from six major programs is 15 percentage points more likely to vote for the BJP, 17 percentage points more likely to vote for NDA, 8 percentage points less likely to vote for an ethnic opposition party, 18 percentage points more likely to say the government should be re-elected, 0.6 scale units more satisfied with the government, and 12 percentage points more likely to think the BJP works for the poor.

[7]Specifically three interaction terms: (Benefited × Muslim), (Benefited × Dalit), and (Benefited × Tribal).

Existing Literature

In this section, I review the distributive politics literature, and point out that it does not adequately address the empirical puzzle. I then turn to the clientelism literature for some insights that help understand the electoral implications of distributive decisions.

The distributive politics literature provides a framework to evaluate programs but it falls short in two respects. First, vote choice models imply that benefit size is positively associated with the magnitude of preference change (Heath and Tillin 2018). The larger the benefit, b_i, the more it can compensate for ideological or policy disutility for voter i. Second, most studies focus on a single benefit of standardized value, and often for simplicity, do not engage with the possibility that politicians *simultaneously* distribute many benefits of different value, using different distributive strategies (Kramon and Posner 2013). As Nath (2014) observes, "the composition of spending has not been studied much" (Nath 2014:3), nor have the "*portfolio* choices of politicians" (emphasis not added). By fixing the benefit in question, these studies end up evaluating a single distributive strategy, typically clientelism, and view other strategies from a linear, Progressive prism. That is to say, there is an implied hierarchy of distributive strategies, and a focus on the conditions leading to the transition from clientelism to programmatic politics. There has been less engagement with the idea that clientelism and programmatic distribution can co-exist, and politicians can employ mixed strategies.

Only three exceptions come to mind: Mares and Young (2019) examine why Eastern European politicians employ different types of clientelism, Magaloni, Diaz-Cayeros, and Estvez (2007) discuss "portfolio diversification" in the case of Mexico, and Levitsky (2007) in the case of Argentine Peronists. Mares and Young (2019) point out that politicians employ different types of clientelism to signal their support for

or opposition to workfare programs. They rely on coercion or negative inducements in localities where social policy benefits are politicized to signal a "tough on welfare" position. They use positive inducements in localities where demand for social policy benefits is high and there are no distributive conflicts to signal "paternalism, benevolence, personal generosity" and support for welfare programs (Mares and Young 2019:7). Clientelism co-exists with programmatic distribution because voters are less likely to punish clientelistic practices that signal policy alignment. This paper offers a different explanation, namely that parties still need brokers to claim credit for welfare programs, convert latent good will from such programs into votes, and to disseminate their ideology and diversify distributive risks.

Magaloni, Diaz-Cayeros, and Estvez (2007) helpfully point out that electoral returns are uncertain when politicians distribute public goods, compared to when they distribute private goods. Formally, Magaloni, Diaz-Cayeros, and Estvez (2007) say that the distribution of public goods yields an electoral return $E[X]$ with variance σ^2 while the distribution of private goods yields a return Y without any uncertainty. Their model assumes that $E[X] > Y$. However, there are two limitations of this approach. The public-private goods comparison does not comprehensively capture the difference between programmatic distribution and clientelism. Private goods, like a house, can be delivered programmatically, namely in a rules-based, non-contingent way. Furthermore, Magaloni, Diaz-Cayeros, and Estvez (2007) do not engage as much with benefit size, and assume that a programmatic benefit (public good) reaches more people than a clientelistic benefit (private good). As I show, the opposite is possible when a cheap benefit is distributed through brokers, and an expensive benefit using a government program.

Similarly, Levitsky (2007) helpfully points out that Argentine Peronists used clientelistic transfers to win over poor voters, while making programmatic appeals to middle class voters. Though Peronists use two distributive strategies simultaneously,

they do so for different sections of the population. I show that there is an incentive to mix distributive strategies for the same section of the population, namely poor voters.

The starting point is the voter's utility function, which typically draws on Downs (1957)'s spatial competition model and Riker and Ordeshook (1986)'s "calculus of voting":

$$U_i(b_i, \sigma_i, \sigma_P) = -(\sigma_i - \sigma_P)^2 + b_i - c_i \tag{2.1}$$

Voter i's utility from voting for party P depends on three things: the squared distance between i's ideological or policy ideal point and party P's ideological or policy position, i.e. $(\sigma_i - \sigma_P)^2$; the expected benefit $b \in \{0, b\}$ if party P comes to power, and the costs of voting $c \in (0, 1)$. As Lindbeck and Weibull (1987) and Dixit and Londregan (1996) show, it is electorally optimal for parties to target benefits at swing voters to compensate them for some (or all) of the disutility arising from policy differences $(\sigma_i - \sigma_P)^2$. Note that in this formulation, the value of the benefit is fixed (b or 0), and material gain (b_i) can compensate for ideological or policy differences. This is what gives rise to the idea that the bigger the benefit, the better. A sufficiently large b_i can theoretically compensate for any disutility arising from ideological or policy differences.[8] And for the same voter, a larger benefit will increase their utility from voting for party P, resulting in a stronger preference for party P, keeping constant all other factors. Heath and Tillin (2018), for example, show that efficient public goods provisioning makes people less responsive to vote buying when it involves a cheap benefit like free vegetables. However, this "institution effect" goes away when politicians seek to buy votes using more expensive benefits like paying for medical expenses, giving a free water pump, or getting a family member a job. In most cases, however, the benefit value is capped at b.

[8] This continues to be the case even if we stipulate diminishing returns to a benefit.

On the empirical side, this manifests itself in the form of single program or benefit evaluations. For example, does a cash transfer (Imai, King, and Rivera 2020; Zucco Jr. 2013; De La O 2013; Manacorda, Miguel, and Vigorito 2011), unemployment benefit (Nazareno, Stokes, and Brusco 2006), road (Goyal 2019), house (Kumar 2021*a*,*b*; Bueno, Nunes, and Zucco Jr. 2017; Barnhardt, Field, and Pande 2015), land titling program (Larreguy, Marshall, and Trucco 2018), infrastructural investment in basic services (Kadt and Lieberman 2017), or some other freebie (Pop-Eleches and Pop-Eleches 2009; Nair 2020) change political preferences? For a comprehensive list, see Table 1 in Kramon and Posner (2013).

Invariably, program evaluations lead to the question of distributive efficiency. The move then is to fix the benefit b, and evaluate a distributive strategy or interventions that reduce inefficiencies in that distributive strategy (Muralidharan et al. 2021; Banerjee et al. 2020; Muralidharan, Niehaus, and Sukhtankar 2016).

Formally, this leads to the inclusion of a "dead-weight loss" term, $\theta_{i,P} \in (0, 1)$, in models. Now, the optimal strategy for parties is to target benefits not just at swing voters but those to whom it can deliver it most efficiently, i.e. "core supporters" for whom $\theta_{i,P} \approx 0$ (Cox and McCubbins 1986). As Stokes et al. (2013) put it, the probability of receiving a benefit is now maximum when $(\sigma_i)^2 = 0$ (i.e. swing voters), or when $\theta_{i,P} = 0$ (core constituents).[9] Logically, what follows is a discussion of who is the the "core constituent"? In the clientelism literature, the argument made is that party brokers embedded in communities reduce inefficiencies because of their situated knowledge, ability to monitor voters and punish reneging (Stokes 2005). Accordingly, a party can efficiently deliver benefits to some voter i, irrespective of their ideological beliefs, if they are in community j which is part of the party broker's clientelistic network.

The debate then shifts to an empirical anomaly: Stokes et al. (2013) find that

[9]Formally, $p_i(b_{i,P} = b|\sigma_i, \theta_{i,P}) = \Phi[-\theta_{i,P} \times (\sigma_i)^2]$, where $\Phi[\cdot]$ is a distribution symmetric around 0.

"too many loyal supporters receive benefits, too few swing or uncommitted voters [do]" (73-74). Note that loyalists are "proximate to a party in ideological or partisan terms" (i.e. $(\sigma_i - \sigma_P)^2$ is small), as distinct from core supporters "who are network-proximate to a party" (Stokes et al. 2013:34). Their explanation for this reveals a principal-agent problem in clientelistic strategies. Party leaders lack information about the voter's type (loyalist, swing or opposition supporter), and imperfectly observe the broker's competence through network size. Brokers, on the other hand, know the voter's type and have an incentive to build the largest possible network with the fewest possible resources, siphoning-off the rest as rent. Owing to this information asymmetry, there is dealignment of incentives: the party wants to maximize electoral support, the broker wants to maximize rent. For any finite budget, the broker wants to spend as little of it on securing support, keeping the rest for themselves. Of course, the broker must build a network larger than their competition's to retain the party's favor. From the broker's perspective then, the "cheapest" voters are loyalists. A smaller benefit (b) can buy their support, so the broker can maximize network size (or support) by channeling benefits to loyalists. In practice, there may not be enough loyalist votes to carry an election, so the broker's network has to be "ideologically heterogenous" (Stokes et al. 2013:95) but with a preponderance of loyalists. There is robust evidence that clientelism leads to mistargeting or the partisan targeting of benefits (Bardhan et al. 2020; Shenoy and Zimmermann 2021; Marcesse 2018; Azulai 2017).

Despite mistargeting or a loyalist skew in distribution, brokers are indispensable. They are needed by the government as local partners to implement schemes (Krishna 2007; Mookherjee and Nath 2021)[10] and provide public goods (Baldwin 2019, 2013), by citizens to make claims on the state (Auerbach 2020; Kruks-Wisner 2018), and by

[10] Mookherjee and Nath (2021) find that brokers are better able to identify deserving households relative to programmatic distribution which relies on low quality information available to higher levels of government.

parties to mobilize or persuade voters because they have credibility and influence in the neighborhood which they use to shape political preferences (Harding and Michelitch 2019; Auerbach 2016; Baldwin 2013). Hidalgo and Nichter (2016) shows that benefits distributed through brokers can also be used to "import outsiders" into the electorate. Critically, when brokers are excluded from the distributive process, there may be less mistargeting but also weaker credit claiming and monitoring of voters.

How does the size of the benefit affect this distributive trade-off? Should politicians rely on brokers to distribute some benefits but not others? What are the electoral implications of different distributive choices? I propose a parsimonious framework to evaluate these choices in the next section.

Argument

The distributive decisions that politicians make can be organized on two dimensions: benefit size or value (how cheap or expensive is the benefit)[11], and distributive strategy (how it is being given out). The electoral implications of these choices stem from a third factor: who gets the benefit. To start with, I present a set of stylized facts about each of these dimensions:

1. **Benefit value:** As the benefit b_i becomes more valuable, we should expect the voter to get greater utility from supporting the party that gives that benefit. The utility gains could be subject to the law of diminishing returns (i.e. $u_i'(b) > 0$ and $u_i''(b) < 0$). That is to say, beyond a point, a unit increase in the value of a material benefit may not translate into any additional utility for the voter. This removes the possibility that a substantially large benefit can compensate for *any* ideological or policy disutility.

[11] I *assume* that the monetary value of a benefit is positively correlated with its use value (or utility) to the voter. Another way of saying this is that I treat the monetary value of a benefit as a good empirical measure of its use value. While noting that these are theoretically distinct concepts, I use them interchangeably in the remainder of the paper.

2. **Distributive strategies:** As the literature in the prior section suggests, clientelism produces mistargeting but provides robust credit claiming and voter monitoring. In contrast, programmatic distribution offers more objective targeting but weaker credit claiming and voter monitoring. Presented with a choice, politicians have to pick between distributing a benefit disproportionately to loyalists but ensuring *most* beneficiaries know the benefit was given by them and are encouraged to turn out and vote, or distributing the benefit to voters who need that benefit the most but potentially not claim as much credit or be able to monitor the beneficiary's political behavior.

3. **Beneficiary characteristics:** Assume voters exist on a single ideological dimension ranging from loyalists ($\sigma_i = k$) to opposition supporters ($\sigma_i = -k$), with swing voters in the middle ($\sigma_i = 0$). As ideological distance $(\sigma_i - \sigma_P)^2$ increases, the voter's disutility from voting for party P increases. Since the marginal utility from benefits also diminishes, there are two types of beneficiaries: (i) those who switch to party P because their ideological disutility can be compensated through benefits ($\sigma_i \geq k_0$ where $k_0 \in (0, -k)$); and (ii) those who get greater utility from voting for party P but their vote choice does not change as a result of receiving the benefit. For voters with $\sigma_i > 0$, their preference for party P becomes stronger, and for those with $\sigma_i < k_0$, their preference for the opposition party becomes weaker.

Against this backdrop, politicians decide what benefits to distribute, and how to distribute them. I assume politicians are office-seeking, and distribute benefits to win votes and elections. They face two trade-offs when making distributive decisions:

Trade-off 1: Given a finite budget Ω, a cheap (low value) benefit can be given to more voters or an expensive (high value) benefit to fewer voters. I *assume* that how much a benefit costs is positively correlated with its use value.

Trade-off 2: When deciding how to distribute a benefit, clientelism produces mistargeting (skews distribution in favor of loyalists) but provides more effective credit claiming and monitoring of voters; programmatic distribution provides more objective targeting but weaker credit claiming and voter monitoring. Critically, the loyalist skew in clientelism is more acute when there are fewer benefits to distribute, notably with an expensive or high value benefit. Programmatic distribution's limitations when it comes to credit claiming and voter monitoring is not dependent on benefit size.

Putting together, I propose a two-by-two that captures the distributive choices and their implications (see Table 2.2):

Table 2.2: What to distribute, and how to distribute it?

	Cost of the benefit	
Distributive Strategy	Cheap	Expensive
Clientelism	Benefit can saturate broker's heterogeneous network of loyalists and swing voters. E.g.: BJP's free cooking gas cylinder scheme or zero balance bank accounts	Benefit cannot be given to everyone in the broker's network. Loyalists entirely or disproportionately benefit. E.g.: Congress' housing scheme (Indira Awas Yojana)
Programmatic	Benefit is distributed to a large, ideologically heterogeneous population, including those inside the party broker's network and those outside that network supporting the opposition party. E.g.: Farm loan waivers	Benefit is distributed to fewer people but typically party supporters and opposition voters. E.g.: BJP's housing scheme (PM Awas Yojana)

Focusing on cheap benefits, Table 2.2 suggests that if the party adopts clientelism, it can distribute benefits to a large portion of the broker's network, including swing voters and weakly opposed voters. At the time of elections, it would also bene-

fit from the broker's local embeddedness and monitoring of voters. In contrast, if the party decides to go down the programmatic route, it can reach a slightly larger set of voters, including those outside its clientelistic network. However, credit-claiming and monitoring will be weaker because the party by-passes the broker. These differences must also be seen in another light — a small benefit does not generate an overwhelming amount of utility for the voter, so the need for credit claiming and monitoring is greater. From an electoral perspective, it makes more sense to distribute the cheaper benefit through brokers.

Now consider expensive or high value benefits. Table 2.2 suggests that if the party adopts clientelism, it will end up distributing benefits in an electorally inefficient manner, primarily to its loyalists. On the up side, brokers will mobilize these voters at the time of elections.[12] In contrast, if the party adopts a programmatic approach, the benefit reaches more than just its loyalist base but this happens at the expense of credit claiming and voter monitoring. Furthermore, an expensive or high value benefit gives the voter a lot of utility, potentially enough to compensate for weak credit claiming and election time monitoring. As a result, I contend that the party is more likely to distribute expensive benefits programmatically.

There are a few other reasons why a party would distribute small benefits through brokers, and large benefits programmatically. For party elites, there is an incentive to diversify their distributive strategy so that they are not overly reliant on brokers or bureaucrats. In so doing, party elites balance competing considerations: they want to efficiently deliver benefits to pivotal voters (explained earlier), and keep brokers engaged and happy. This means involving brokers in *some* distributive processes that generate rents for them. The best mixed strategy here is to distribute cheap benefits through brokers and expensive benefits through programs. Such a strategy minimizes the electoral impact of mistargeting (or suboptimal targeting),

[12] Nichter (2008) explains how parties can target benefits at loyalists to ensure they turnout to vote.

creates work for brokers, and permits just-enough leakage through small rents to keep the party machine well-oiled.

Taking a step back, Table 2.2 provides a framework to compare different strategies as well. For example, why might a cheap benefit distributed through brokers have as much (or greater) political impact as a program that delivers an expensive benefit? This can happen when brokers make up for the value deficit through credit claiming and voter monitoring. Some of this effect will also be on account of the fact that the broker delivered benefit reaches a more persuadable audience (namely, within-network or ideologically more proximate voters). In evaluating the pivotal role of brokers, it would be important to control for this selection bias. Conversely, when an expensive benefit is politically more impactful we can conclude that its value compensated for weaker credit claiming. This suggests that material utility or value, at some level, can compensate for local embeddedness and weaker canvassing.

Turning to the two benefits that motivate this study, Table 2.2 offers clear predictions or explanations. These are:

1. The BJP should distribute the cheap benefit using brokers, and the expensive benefit through a government program. As evidence of this mixed strategy, there should be mistargeting of the cheap benefit but not the expensive benefit. This should be the case both geographically and in terms of partisan characteristics of beneficiaries (party loyalist skew in distribution).

2. For the cheaper benefit to have greater impact (after adjusting for differential selection into benefits), there must be asymmetric contact before elections. Cylinder recipients should be more likely to be contacted by BJP canvassers, compared to non-recipients, controlling for other attributes that determine vote choice and who gets the benefit (e.g. past vote choice and ethnicity). House recipients should *not* be more likely to be contacted by BJP canvassers, compared to non-recipients. In effect, brokers work harder to claim credit for the

clientelistic benefit and mobilize support but they do not make such an effort for the programmatic benefit.

Empirical Strategy

The empirics in this paper map onto the two observable implications: establishing that the housing program did not engage in political targeting but the cooking gas cylinder scheme did; and as a consequence of this, BJP canvassers were more likely to contact cylinder recipients before an election but not house recipients. In support of the first claim I report qualitative evidence, correlational evidence (conditioning on observables) and results from a better identified close elections regression discontinuity design. In support of the second claim I present correlational evidence using survey data and a fixed effects model. In the remainder of this section I describe the data sources, measures, and estimation strategy.

Data

The attitudinal measures for this paper come from the National Election Studies (NES). These face-to-face surveys have been conducted since 1996 by the Center for the Study of Developing Societies (CSDS), a reputed research institute. There is a pre-election and post-election survey, both a random sample drawn from the voters' list. They are nationally representative on demographic, geographic, and political parameters. Here, I use data from the post-election survey in 2019, focussing on rural areas in 10 Indian states that account for 93% of the houses built under the *Awas Yojana*, and 91% of eligible beneficiaries ($n = 9745$).[13] These ten states have similar levels of housing deprivation (which serves as a proxy for socioeconomic development in rural areas), and broadly comparable political competition (insofar as the BJP

[13]More details on survey's methodology are available here: https://www.lokniti.org/media/PDF-upload/1565073104_34386100_method_pdf_file.pdf

contests many seats and has an organizational presence). These states are Bihar, Uttar Pradesh, Madhya Pradesh, Rajasthan, Jharkhand, Chhattisgarh, Odisha, West Bengal, Assam, and Maharashtra.

The NES measures exposure to government schemes, vote choice in the 2019 national elections, past vote choice, and political attitudes. For the RD analysis, I supplement survey data with publicly available administrative data to construct the forcing variable: BJP's margin of victory in the (previous) 2014 parliamentary election. India's election commission puts out election results at the parliamentary constituency level. It also disaggregates this information at the state constituency level. State constituencies are perfectly nested within parliamentary constituencies. At the state level, chief election officers disaggregate the results further at the polling booth level (popularly called "Form 20 data"). However, this information is reported in different formats, often without party names and different spellings of candidate names, sometimes even in local languages. This makes it exceedingly laborious to scrape and systematize the data, something reputed data repositories have also not finished doing for the 2014 parliamentary election. For this reason, I use results at the parliamentary and state constituency level for my analysis.

Measures

The appendix describes the measures used in various analyses, including survey questions, (re-) coding decisions, and aggregation of measures into an index.

Estimation

To show that there is political targeting of the cheap benefit but not the expensive benefit, I use a fixed-effects model with the following specification in `lm_robust`:

$$\text{Benefited}_{i,t} = \alpha_0 + \beta_1 \text{Past Vote}_{i,t-5} + \beta_2 \text{Muslim}_{i,t} + \beta_3 \text{Dalit}_{i,t} + \beta_4 \text{Tribal}_{i,t} + \rho \mathbf{X}_{i,t} + \sum_{j=1}^{J} \gamma_j \text{Booth}_j$$

where `Benefited` is a dummy variable that takes a value of 1 if the respondent says they received a benefit, otherwise 0. `Past Vote` is a dummy variable that takes a value of 1 if the respondent says they voted for the BJP in the 2014 parliamentary election, else 0. I control for ethnicity through three indicator variables: one for lower caste Hindus (`Dalits`), one for `Tribals`, and a third one for `Muslims`. $\mathbf{X}$ is a vector of demographic controls, namely mean-centered age, gender (female or not), education, and monthly household expenditure (binned). I specify fixed effects at the polling booth level to account for any confounding due to time invariant local factors.

To assess whether narrowly winning (or losing) in the previous parliamentary election affects getting a benefit, I use a close-elections regression discontinuity design. The forcing variable is operationalized as BJP's margin of victory or defeat (-100 to $+100$) in the prior parliamentary election. I use `rdrobust` in `R` to estimate the difference at the cut-point. I specify the following:

$$\text{rdrobust}(y = \text{benefited}, x = \text{bjp\_margin\_pct}, p = 1, \text{kernel} = \text{“triangular”}, \text{bwselect} = \text{“mserd”}, \text{cluster} = \text{constituencyID}, \text{all} = T)$$

where `bjp_margin_pct` is BJP's margin of victory or defeat in a parliamentary or state constituency in the 2014 election. All standard errors are heteroskedasticity-robust (HC2), and clustered at the parliamentary or state constituency level.

When it comes to contact at the time of elections, I again use an ordinary least

squares regression with fixed effects. This takes the specification:

$$\mathrm{Y}_{i,t} = \alpha_0 + \beta_1(\text{Got a house})_{i,t-1} + \beta_2(\text{Got a cylinder})_{i,t} + \rho\mathbf{X}_{i,t} + \sum_{j=1}^{J} \gamma_j \text{Booth}_j$$

where $Y_{i,t}$ is an indicator variable that equals 1 if the respondent is contacted by the BJP before an election, otherwise 0. $\mathbf{X}$ is a vector of control variables like past vote choice, ethnicity, mean-centered age, gender (female or not), education, and monthly household expenditure (binned).

Results

Before focusing on *Ujjwala* and the *Awas Yojana*, I present some descriptive statistics on government programs. Table 2.3 reports the percentage of an ethnic group that receives a benefit (columns 2 to 7), the percentage of the total survey sample that receives that benefit (column 8), and the percentage of beneficiaries that credit the Modi government for that benefit. The first three programs predate the current government, and have survived in some shape and form over decades. For example, the subsidized food program reaches an estimated 45% of the population, particularly benefiting the lower castes (SCs) and tribals (STs). Pension and employment guarantees benefit nearly a quarter of the population, though Muslims benefit at lower rates despite being socioeconomically backward. Lower caste Hindus and tribals, who are also socioeconomically vulnerable, benefit at slightly higher rates than the population average.

Of particular interest to us are programs started by the current government (reported in rows 4-9). As table 2.2 predicts, the government is able to distribute

Table 2.3: Descriptive Statistics

	Hindus				Minority		
Scheme	GEN	OBC	SC	ST	Muslim	Entire Sample	Credit Center
Food (PDS)	39.1	49.0	50.9	51.5	43.3	44.9	29.1
Pension	24.0	29.5	25.2	27.1	22.7	25.8	32.9
Employment/MNREGA	17.1	22.5	28.1	31.7	17.8	23.4	57.4
Free gas cylinder	26.4	34.4	35.8	46.8	29.6	31.9	76.6
Housing scheme	11.3	19.9	23.7	31.5	17.4	19.0	53.6
Zero balance bank acc	19.2	22.0	23.5	24.3	15.7	19.6	79.9
Health insurance	15.2	17.8	20.5	25.0	14.6	17.2	57.4
Farmer income support	12.0	15.0	10.9	12.6	9.9	11.9	48.0
Farm loan waiver	10.1	13.3	10.3	14.0	8.0	10.7	37.9

Note:
Columns 2-6 report the percentage of an ethnic group that receives a benefit. Column 7 reports the percentage of the entire sample that receives a benefit. Column 8 reports the percentage of beneficiaries that credit the Modi government for that benefit Data: National Election Studies 2019

a cheap benefit (like a gas cylinder) to many people (nearly a third of the sample), but more expensive benefits like a house or health insurance to fewer people (approximately 17-19% of the sample). An anomaly here is the zero-balance bank account — a relatively costless benefit that is cheaply deliverable but only reaches 20% of the sample.

Focusing on distributive strategies, I do observe a loyalist skew in the distribution of cooking gas cylinders but not houses. To see this, consider ethnicity as an imperfect proxy for political ideology: upper caste Hindus ("General" voters) strongly support the BJP's economic and social policies ($\sigma = k$), intermediate and backward castes (OBCs) are less supportive, tribals and lower castes are weakly opposed or swing voters (depending on the constituency's demographics), and Muslims are strongly opposed ($\sigma = -k$). This ordering of social groups is also negatively correlated with status and material affluence. 26% of "general" voters get a free cooking gas cylinder, despite being the most socioeconomically affluent group. In contrast,

only 11% of them get a house, which was specifically targeted at poor households using census data.

When it comes to credit for programs, a few things stand out. As one would expect, the current government gets relatively less credit for long running programs and more credit for its own flagship programs. The most striking difference is between the broker delivered cooking gas cylinder and the programmatically distributed house or farm loan waiver. Nearly 77% of survey respondents who get a cooking gas cylinder credit the BJP government for it. In contrast, only 54% of those that get a house credit the BJP government for it. Similarly, only 38% of those whose agricultural loan was waived by the government actually credit the BJP government for this benefit. The zero balance bank account is an interesting case: a programmatic good on first appearance that is overwhelmingly credited to the central government. Nearly 80% of those that got such a bank account credit the BJP government for it. It turns out that like the cooking gas cylinder, these accounts were opened with the help of party brokers. In interviews with bureaucrats involved with the program I confirm the role of party mobilization: brokers identified voters without a bank account, took them to the bank branch, and got their account opened. It is worth noting that this benefit was not easily available directly at the bank because banks had a strong incentive to not open zero balance accounts, which are commercially unviable. In summary, there is robust descriptive evidence that credit claiming is stronger for clientelistic goods, and weaker for programmatic ones. Credit claiming is complicated by the fact that some states have BJP governments, while others have opposition parties in power. This can lead to "credit hijacking", particularly when benefits cannot be distributed through non-state organizations in opposition governed areas (Bueno 2018). This is less of a concern when the same party controls both tiers of government.

Finally, these descriptive findings need to be appropriately caveated. For one, there is a lot of political and socioeconomic variation *within* ethnic groups, making

ethnicity a less than perfect predictor of either political ideology or deservingness. Moreover, most government programs have both programmatic and clientelistic features, making it hard to draw black-and-white contrasts. It is precisely for this reason that I focus on two government programs that sharply capture some of these contrasts. The cooking gas cylinder, valued at $10, is predominantly delivered through party brokers. Money to build a house, valued at $2000, is one of the best specimens of programmatic distribution in the Indian system. Beneficiaries were identified using socioeconomic indicators from the 2011 census, assigned a household deprivation score, rank ordered from most to least deprived, and given the benefit in that order with the village ranking made public before disbursement started. Since this housing program has an urban and rural component, with considerably stronger programmatic features in the rural component, my analysis focuses on rural areas. Since 93% of houses and 91% of beneficiaries are located in 10 Indian states, my analysis focuses on rural areas in these provinces. For a more empirically robust examination, I now turn to regression analysis, using a direct measure of partisanship, adjusting for a range of observable characteristics that affect selection into benefits, and restricting comparisons to within-precinct.

Political Targeting: Evidence from a Fixed Effects Model

Observationally, BJP supporters are more likely to get a cooking gas cylinder and benefit from six flagship programs but are not more likely to receive a house. Table 2.4 reports the coefficient estimates from a fixed-effects model in which the outcome is receiving a benefit (house or cooking gas cylinder), and the predictors of interest are the respondent's vote choice in the prior parliamentary election, and their ethnicity. The model includes a variety of demographic control variables, and restricts comparisons to within-precinct (polling booth) to account for any confounding due to local, time invariant factors.

Table 2.4: Political Targeting of Benefits? Partisanship and Ethnicity

	DV: Benefited from		
	Housing Program	Cooking gas cylinder	All BJP schemes
Voted BJP in 2014	0.018	0.048***	0.030***
	(0.012)	(0.013)	(0.006)
Muslim	−0.021	−0.072**	−0.042***
	(0.022)	(0.025)	(0.011)
Dalit	0.062***	0.050**	0.012
	(0.016)	(0.017)	(0.007)
Tribal	0.116***	0.077**	0.028**
	(0.022)	(0.024)	(0.010)
Adj. R^2	0.261	0.320	0.495
Num. obs.	6638	6638	6638
Dem. Controls	Yes	Yes	Yes
Booth FE	Yes	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 2.4 confirms the loyalist skew in clientelistic distribution: respondents that voted for the BJP in 2014 are 5 percentage points more likely to receive a cooking gas cylinder, 3 percentage points more likely to benefit from *six flagship* BJP schemes, but not more likely to receive a house ($\widehat{\beta} = 0.018$, $s.e. = 0.012$). Moreover, ideologically opposed voters (i.e. Muslims) are considerably less likely to get a cooking gas cylinder or benefit from flagship programs but are not less likely to receive a house. As Table 2.4 shows, Muslims are seven percentage points less likely than upper and backward caste voters to get a cooking gas cylinder, and four percentage points less likely to benefit from the flagship programs of the BJP government. Crucially, Muslims are *not* less likely to get a house ($\widehat{\beta} = -0.021$, $s.e. = 0.022$). There is some evidence of swing voter targeting as well. Dalits and tribals are more likely to get a house. Dalits are approximately six percentage points more likely to get a house compared to upper and backward caste voters ($\widehat{\beta} = 0.062$, s.e.= 0.016). Tribals are 11 percentage points more likely to get a house ($\widehat{\beta} = 0.116$, $s.e. = 0.022$ for tribals). Similarly,

Dalits are five percentage points more likely to get a cooking gas cylinder ($\widehat{\beta} = 0.05$, s.e.= 0.017), and Tribals are seven percentage points more likely to get a cylinder ($\widehat{\beta} = 0.077$, s.e.= 0.024).

Evidence from a Close Elections RD

Does clientelism lead to greater geographic mistargeting of benefits than programmatic distribution? For a design-based test, I turn to regression discontinuity analysis using survey responses from rural areas in ten Indian states. I leverage the fact that parliamentary election results are publicly available at lower levels of aggregation. India's Election Commission reports the results at the parliamentary constituency (PC) level, and for every state assembly constituency (AC) nested within the parliamentary constituency. This allows us to construct the forcing variable (`bjp_margin_pct`) for each survey respondent based on their parliamentary and state assembly constituency location.[14] The outcome in this analysis is whether the respondent reports receiving a benefit (house, cooking gas cylinder, or an index of six benefits distributed through flagship government programs). The expectation is that the BJP will reward areas that voted for it in the parliamentary election, and that the clientelistic benefit can be more precisely targeted towards areas and people that voted for the party than the programmatic benefit.

This is exactly what we find. At the parliamentary constituency level, the probability of receiving a cooking gas cylinder sharply increases when the BJP narrowly wins an election, compared to when it narrowly loses. There is much weaker evidence, if any, that the BJP distributes more houses in constituencies it narrowly wins. Table 2.5 reports a 43.7 percentage point (s.e.= 0.145, $p = 0.003$) increase in the probability of receiving a cooking gas cylinder when the BJP narrowly wins a parliamentary con-

[14]Following Cattaneo, Idrobo, and Titiunik (2019)'s guidance, I report the results of the McCrary density test, visualize the frequency distribution of the forcing variable, check for discontinuous changes in covariates at the cut-off, and sensitivity of results to the exclusion of observations near the cut-off in the Appendix.

stituency.[15] This difference is both substantively large and statistically significant. I get a similar result when the RD bandwidth is manually set to 5% ($n = 1255$). The probability of receiving a cooking gas cylinder increases by 26 percentage points (s.e.= 0.12, $p = 0.035$) at the cut-point. The estimate is smaller and statistically insignificant when the bandwidth is set to 3% ($n = 558$). There is only a three and a half percentage point increase (s.e.= 0.089, $p = 0.69$) in the probability of receiving a cooking gas cylinder at the cut-point. The main result is also robust to the exclusion of observations near the cut-off. Table A2.15 reports the difference at the cut-point for a variety of donut hole RD specifications. These estimates are always positive, and typically 20 percentage points or higher.

There is less conclusive evidence that the probability of receiving a house increases when the BJP narrowly wins a parliamentary constituency. Table 2.5 reports a 13 percentage point (s.e=0.114, $p = 0.25$) increase at the cut-point but it is not statistically distinguishable from zero. The point estimate is negative when the bandwidth is manually selected: $\widehat{\tau_{RD}} = -0.28$ (s.e.= 0.07, $p < 0.001$) when the bandwidth is 5%, and $\widehat{\tau_{RD}} = -0.31$ (s.e. $= 0.11$, $p = 0.005$) when the bandwidth is 3%. However, the difference at the cut-point is positive when observations near the cut-point are excluded from the analysis (see donut hole RD estimate in Figure A2.6 and Table A2.15 in the Appendix).

Within parliamentary constituencies, it is even clearer that the clientelistic benefit is targeted towards areas that voted for the BJP but this is not the case for the programmatic benefit. Table 2.6 shows that the probability of receiving a cooking gas cylinder increases by 24 percentage points (s.e.= 0.11, $p = 0.029$) when the BJP narrowly wins in a state assembly segment of the parliamentary constituency, compared to when it narrowly loses. There is a small, though statistically insignificant, decrease in the probability of receiving a house at the cut-point ($\widehat{\tau_{RD}} = -0.038$, s.e.= 0.11,

[15] The RD plots are reported in the Appendix.

Table 2.5: Benefit Targeting (Parliamentary Constituency Level)

Benefited from	RD (MSE optimal BW)				
DV	Coef	SE	p	n	BW (L,R)
Housing Scheme	0.129	0.114	0.254	2441	9.68,9.68
Free gas cylinder	0.437	0.145	0.003	1833	7.23,7.23
All BJP schemes	0.071	0.057	0.209	2169	8.61,8.61

Note:
The difference at the cut-point was estimated using rdrobust in R, specifying a first-order polynomial (p=1), triangular kernel weights, and MSE-optimal bandwidths. We report the robust, bias-corrected estimate and HC2 robust standard error (clustered at the parliamentary constituency level). Data from National Election Studies 2019, Election Commission of India 2014

$p = 0.73$). The results are similar if the RD bandwidth is manually set to 5% or 3%. When the bandwidth is set to 5%, the probability of receiving a cooking gas cylinder increases by 29 percentage points (s.e.= 0.14, $p = 0.04$) at the cut-point. When it is set to 3%, the probability of getting a cylinder increases by 37.6 percentage points (s.e.= 0.25, $p = 0.144$) at the cut-point. In both specifications, there is no statistically significant change in the probability of receiving a house at the cut-point: $\widehat{\tau_{RD}} = 0.06$ (s.e.= 0.22, $p = 0.77$) when the bandwidth is 5%, and $\widehat{\tau_{RD}} = 0.098$ (s.e. $= 0.28$, $p = 0.73$) when the bandwidth is 3%. The results for the cooking gas cylinder and house are robust to the exclusion of observations near the cut-off, using a variety of donut hole RD specifications (see Figure A2.3 and Table A2.11 in the Appendix).

The next part of this analysis shows that in areas that voted for the BJP, party loyalists specifically are more likely to receive the clientelistic benefit but not the programmatic benefit. In this RD analysis, the outcome is the probability of being a BJP voter *and* receiving a benefit (cooking gas cylinder or house). The outcome is coded as 1 if a survey respondent receives a benefit *and* reports voting for the BJP in the 2014 parliamentary election, otherwise 0.[16] The forcing variable (margin of

[16]Three types of respondents are coded 0: loyalists who do not receive the benefit, voters that

Table 2.6: Benefit Targeting (Assembly Constituency Level)

Benefited from	RD (MSE optimal BW)				
DV	Coef	SE	p	n	BW (L,R)
Housing Scheme	-0.038	0.112	0.734	3655	12.12,12.12
Free gas cylinder	0.240	0.110	0.029	3769	12.77,12.77
All BJP schemes	-0.041	0.081	0.616	4442	15.53,15.53

Note:
The difference at the cut-point was estimated using rdrobust in R, specifying a first-order polynomial (p=1), triangular kernel weights, and MSE-optimal bandwidths. We report the robust, bias-corrected estimate and HC2 robust standard error (clustered at the assembly constituency level). Data from National Election Studies 2019, Election Commission of India 2014

victory) is defined at the state assembly constituency level.

Table 2.7 reports the difference at the cut-point for this outcome. Strikingly, the probability of a BJP loyalist getting a cooking gas cylinder increases by 13 percentage points (s.e.= 0.07, $p = 0.06$) when the party narrowly wins in an assembly segment. There is no increase in the probability of a loyalist receiving a house ($\widehat{\tau_{RD}} = -0.03$, s.e. $= 0.08$, $p = 0.71$). In other words, clientelistic benefits are specifically targeted at loyalists in areas where the party wins in a parliamentary election.

The evidence from the fixed effects model and the RD analysis point in the same direction. Prior support to the BJP improves ones chances of getting a cooking gas cylinder but not a house. Qualitative information about these programs explain why this might be the case: party brokers distribute the cooking gas cylinder but the housing program by-passes these intermediaries. As a politician told me in an interview, whenever development work or social welfare schemes are launched, "cadres want the party to be the distributing unit ... [they] want to be part of the distribution network to get tenders to make money, and to give benefits only to supporters".

support some other party and receive the benefit, and voters that support some other party and do not receive the benefit.

Table 2.7: Loyalists Benefit?

Loyalist and benefited from	RD (MSE optimal BW)				
DV	Coef	SE	p	n	BW (L,R)
Housing Scheme	-0.030	0.08	0.705	2781	12.74,12.74
Free gas cylinder	0.132	0.07	0.060	2117	9.31,9.31

Note:
The difference at the cut-point was estimated using rdrobust in R, specifying a first-order polynomial (p=1), triangular kernel weights, and MSE-optimal bandwidths. We report the robust, bias-corrected estimate and HC2 robust standard error (clustered at the assembly constituency level). Data from National Election Studies 2019, Election Commission of India 2014

Contact By Brokers Before Elections

Does the broker's involvement (or not) in the distribution of a benefit affect credit claiming? To understand this, I study contact by parties during the parliamentary election. Table 2.8 reports the results from an ordinary least squares regression in which the dependent variable is whether a survey respondent reports being contacted by a party, and the explanatory variables are receiving a house or cooking gas cylinder. As before, the specification includes control variables (ethnicity, past vote choice, age, gender, education, and income), and precinct (polling booth) fixed effects.

Column 2 of Table 2.8 confirms the central prediction of my theoretical framework: people who get a house are no more likely to be contacted by BJP canvassers than those that do not get a house ($\widehat{\beta_1} = 0.012$, s.e.$= 0.011$) but cooking gas cylinder recipients are four percentage points more likely to be contacted by BJP canvassers ($\widehat{\beta_2} = 0.039$, s.e.$= 0.01$). The full results are reported in Table A2.1 of the Appendix. The same results are observed when the analysis includes all rural respondents (Table A2.2) or the full survey sample with an additional control variable for ruralness (Table A2.3). In each of those cases, cooking gas cylinder recipients are three to four percentage points more likely to be contacted by BJP canvassers compared to people

Table 2.8: Contact by Brokers

	Contacted by BJP (0/1)	Contacted by Cong (0/1)	Contacted by Reg Parties (0/1)
Got a house (β_1)	0.012	−0.002	0.018
	(0.011)	(0.010)	(0.009)
Got a cylinder (β_2)	0.039***	0.017	0.023**
	(0.010)	(0.009)	(0.007)
Adj. R^2	0.397	0.409	0.416
Num. obs.	6398	6345	6399
Controls	Yes	Yes	Yes
Booth FE	Yes	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

that have not got the benefit. In contrast, there is no association between receiving a house and being contacted by the BJP. These findings are also robust to alternative specifications (see Tables A2.4 and A2.5).

What about contact by other political parties? Columns 3 and 4 in Table 2.8 report the partial correlation between receiving a benefit and being contacted by the Congress party or regional parties. Once again we see that receiving a house is not associated with greater pre-election contact by opposition parties, whether that be the Congress or other regional parties. Cooking gas cylinder recipients are more likely to be contacted by regional parties ($\widehat{\beta}_2 = 0.023$, s.e.= 0.007) but not the Congress party. Cylinder recipients are more likely to be contacted by both if we look at all rural respondents (Table A2.2), or all survey respondents (Table A2.3). In other words, opposition parties reach out to cylinder recipients and compete for their votes but they make no additional effort to contact beneficiaries of the housing program.

Discussion

In May 2022, *The Economist* ran a piece titled, "India's politicians have figured out how to turn welfare into votes". That article concludes:

> For poor Indians, the benefits are clear. Not only does the central government have a bigger incentive to improve their lives; states also feel the need to compete with it in munificence. It is better, too, for Indian democracy for politicians to pitch for votes based on the services they provide rather than on the grievances they stoke. Despite the BJP's constant drumbeat of Hindu majoritarianism, it is the party's record of providing basic goods that appeals to many more voters, including Muslims. (Economist 2022*b*)

But how exactly do Indian politicians distribute welfare benefits, and why do some benefits have greater electoral impact than others? This paper offers a typology of distributive decisions that sheds light on the strategic considerations informing politicians' decisions. The upshot is that rule-based, non-contingent, direct transfers do not displace clientelism. Programmatic distribution and clientelism coexist because politicians have an incentive to use brokers to distribute cheap benefits, and government programs to distribute expensive benefits. An interesting electoral consequence of this is that cheap, clientelistic benefits (like a $10 cooking gas cylinder) can end up having as much impact as expensive, programmatic benefits (like a $2000 house).

But there are other, substantive implications for the politics of development. While prior work in distributive politics focuses on a benefit and evaluates its impact, how it is distributed, or how it can be distributed more efficiently, I look at how politicians allocate resources for several benefits and employ different delivery mechanisms. In every country, politicians simultaneously distribute several benefits of varying value, and use different distributive strategies. Clientelism and programmatic distribution tend to coexist, rather than one displace the other (Larreguy,

Marshall, and Trucco 2018). For example in Ghana programmatic appeals do not erode clientelism (Weghorst and Lindberg 2013). In Eastern Europe, clientelism is used for programmatic signaling, and voters are more tolerant of clientelistic inefficiencies when there is programmatic alignment (Mares and Young 2019). In India, clientelism persists because politicians have one eye on political survival, the other on welfare maximization. Even when they are able to cheaply and directly communicate with voters, and claim credit for the benefits they distribute, they entrust brokers with distributing some benefits while delivering others through government programs. They do this because they still need brokers to claim credit for welfare programs that distribute cheap benefits, to disseminate their ideology, and to diversify risk. Brokers are especially useful when distributing cheap benefits because their effective credit claiming and canvassing makes up for the low value of the benefit.

The mixing of distributive strategies also has interesting implications for partisanship. Parties that distribute some benefits through brokers and directly transfer other benefits through publicized rules and without conditions, develop less coherent brands. On the one hand voters see the party tolerate rent seeking and leakages. On the other hand they observe efficient last-mile delivery of benefits through flagship programs. The party brand is part clientelistic, part programmatic. If all the major parties develop similar reputations, they are less distinct from each other. As we know, the coherence and convergence of party platforms affects partisanship (Lupu 2016).

Finally, the findings in this paper suggest that politicians may not be optimally allocating resources when they distribute expensive benefits. If cheaper benefits can end up having as much electoral impact, why use scarce resources on high value benefits? This, in turn, has implications for poverty alleviation and economic development. If politicians are incentivized to distribute cheap goods, public investments in more expensive goods are likely to suffer. Yet, expensive benefits like a house

are critical for reducing physical and material insecurity, improving productivity and overall standard of living. How can distribution of expensive benefits be made electorally viable? Future work can help identify demand and supply side conditions that make it more attractive for politicians to distribute expensive benefits.

Appendix A: Study Measures

Outcomes and Explanatory Variables

Variable(s)	Source, Original Measure	Recoding
Voted BJP, Voted NDA, Voted for an ethnic party	(Survey) Who did you vote for? I am giving you this slip which has names and election symbols of the candidates and parties that you saw on the voting machine. On this slip please put a mark in front of the same symbol against which you pressed the button	Three dummy variables that take a value of 1 if the respondent voted for BJP, otherwise 0. Another one if they voted for BJP or any of its allies, otherwise 0. A third dummy variable that equaled 1 if the respondent voted for any ethnic party, otherwise 0.
Re-elect incumbent	(Survey) Should the BJP-led NDA government at the Centre get another chance after the coming Lok Sabha election?	Yes = 1, No = 0
Satisfaction with govt.	(Survey) Are you satisfied or dissatisfied with the performance of the BJP-led NDA government at the Centre over the last five years?	Fully satisfied = 2, Somewhat satisfied = 1, Somewhat dissatisfied = -1, Fully dissatisfied = -2
BJP works for poor	(Survey) People have different opinions about the development that has taken place in the country in the last 5 years. Some believe it has only been for the rich, others say it has been for all people, and some others say that there has been no development at all. What?s your opinion?	No development at all = 0, Only for rich = 0.5, For all people = 1

Variable(s)	Source, Original Measure	Recoding
Election involvement	(Survey) Did you do the following? (1) Attend election meetings/ rallies? (2) Participate in processions/nukad natak etc.? (3) Participate in door to door canvassing? (4) Contribute or collect money? (5) Distribute election leaflets or put up posters?	Yes = 1, No = 0, Index is an average of the dummy variables, excluding missing data.
Contacted by BJP, Congress, regional parties	(Survey) Did a candidate/party worker of the following parties come to your house to ask for your vote in the last one month? And, Were you or any of your family members contacted by the following parties through a phone call or recorded voice or SMS or WhatsApp in the last one month?	For each party, two dummy variables created that took a value of 1 if the respondent was contacted by that party (or those parties), else 0. One variable captured physical canvassing, the other digital contact. Then an average of those two variables was taken for each party.
Majoritarianism	(Survey) Please tell me whether you agree or disagree with each [statement]? (1) Even if it is not liked by the majority, the government must protect the interests of the minorities. (2) The Muslim community in India has been victimized under Narendra Modi?s government. (3) Minorities should adopt the customs of the majority community. (4) Only my religion is correct, not of anyone elses.	Each item/statement is coded from -2 to $+2$, with higher values indicating greater support for majoritarian ideas. The index is computed by averaging the items, excluding any missing data.

Variable(s)	Source, Original Measure	Recoding
Hindu nation	(Survey) I will read out two statements. Please tell me which one do you agree with? (1) India primarily belongs to only Hindus; (2) India belongs to citizens of all religions equally, not just Hindus.	Dummy variable that equals 1 if the respondent selected statement 1, else 0
Muslim patriotism	(Survey) According to you how nationalist are the following religious communities - highly nationalist, somewhat nationalist, not much nationalist or not at all nationalist?	Coded from -2 (not at all nationalist) to $+2$ (highly nationalist).
Benefited from housing scheme, cooking gas cylinder scheme, all BJP schemes (Outcome and explanatory variable)	(Survey) Please tell me in the last five years, have you or someone from your family benefited from these government schemes? (1) Housing scheme/ Awas Yojana, (2) Rozgar guarantee scheme (MNREGA), (3) Scheme to provide free hospital treatment up to 5 lakh rupees per family, (4) Pension money (old age, widow, disabilities etc.), (5) PDS, (6) Income support scheme for farmers, (7) An agricultural loan waiver scheme, (8) Ujjwala Yojana, (9) Jan Dhan Yojana.	For each item, we create a dummy variable that equals 1 if the respondent claims to have benefited, otherwise 0. For the index on BJP schemes, we average the responses for the following schemes: (1), (3), (6), (7), (8), and (9). The index takes values between 0 and 1, where 1 = they have benefited from all the schemes they know about, 0 = they have benefited from none of the schemes they know about.

Control Variables

Variable(s)	Source, Original Measure	Recoding
Past vote for BJP	(Survey) Can you tell me which party did you vote for in the 2014 Lok Sabha election held five years ago?	If the respondent says BJP we code them 1, any other party 0. No response/ did not vote = NA.
Ethnicity variables: Muslim, Dalit, Tribal	(Survey) What is your religion? And, What caste group do you belong to?	Muslim takes a value of 1 if the respondent's religion is "Muslim", otherwise 0. Dalit takes a value of 1 if the respondent's caste category is "SC", otherwise 0. Tribal takes a value of 1 if the respondent's caste category is "ST", else 0. It is possible that some Muslims are Dalits ($n = 131$), and tribals ($n = 81$).
Mean-centered age	(Survey) What is your age? (in completed years)	We apply the following transformation: age_centered $= age_i - \overline{age}$ where $\overline{age}$ is the average age in the sample.
Female	(Survey) Gender: Male, Female, Other	Dummy variable that equals 1 if the respondent answers female.
Education	(Survey) Up to what level have you studied?	9 point scale where 0 is non-literate and 8 is professional degree or higher research (higher values indicate more education)
Rural	(Survey) Locality (Rural / Urban)	Dummy variable that equals 1 if the locality is reported as rural
Monthly expenditure (numeric)	(Survey) In normal circumstances, what is your monthly household expenditure? (10 categories or bins starting with "up to 1,000" and ending with "over 50,000")	. We take the mid-point value for each bin. For the last bin ("over 50,000"), we use the previous bin width ("30,001 to 50,000") and add half that to the lower value, $50000 + \frac{20000}{2} = 60000$.

Appendix B: Electoral Impact

Regression Analysis

In puzzle section, I specify the following regression:

$$\mathrm{Y}_{i,t} = \alpha_0 + \beta_1(\text{Got a house})_{i,t-1} + \beta_2(\text{Got a cylinder})_{i,t} + \rho\mathbf{X}_{i,t} + \sum_{j=1}^{J} \gamma_j \text{Booth}_j$$

where $Y_{i,t}$ captures electoral support for the BJP using a variety of survey measures, $\mathbf{X}$ is a vector of control variables like past vote choice, ethnicity, mean-centered age, gender (female or not), education, and monthly household expenditure (binned). The specification include polling booth fixed effects to adjust for any confounding due to time invariant factors at the precinct level.

Here, I report the full results (see Table A2.1); the results when the sample includes all rural respondents (Table A2.2); and the results with the full sample and an additional control variable for ruralness (Table A2.3).

Table A2.1: Electoral Impact: House v. Gas Cylinder (Full Results)

	Voted BJP (0/1)	Voted NDA (0/1)	Voted Ethnic Party (0/1)	Re-elect Incumbent (0/1)	Govt Satisfication (-2 to +2)	BJP works for poor (0,0.5,1)	Election involvement (Index)	Contacted by BJP (0/1)	Contacted by Cong (0/1)	Contacted by Reg Parties (0/1)
Got a house (β_1)	0.035^{**}	0.040^{**}	-0.016	0.033^{*}	0.043	0.016	0.002	0.012	-0.002	0.018
	(0.012)	(0.013)	(0.010)	(0.014)	(0.042)	(0.011)	(0.007)	(0.011)	(0.010)	(0.009)
Got a cylinder (β_2)	0.046^{***}	0.037^{**}	-0.022^{*}	0.047^{***}	0.181^{***}	0.027^{**}	0.026^{***}	0.039^{***}	0.017	0.023^{**}
	(0.011)	(0.012)	(0.009)	(0.012)	(0.037)	(0.010)	(0.006)	(0.010)	(0.009)	(0.007)
Muslim	-0.194^{***}	-0.212^{***}	0.149^{***}	-0.324^{***}	-0.856^{***}	-0.182^{***}	-0.004	-0.127^{***}	0.035^{*}	0.010
	(0.023)	(0.023)	(0.022)	(0.025)	(0.081)	(0.020)	(0.012)	(0.018)	(0.017)	(0.016)
Dalit	-0.094^{***}	-0.088^{***}	0.104^{***}	-0.039^{*}	-0.148^{**}	-0.045^{***}	-0.015	-0.023	0.005	0.009
	(0.016)	(0.016)	(0.013)	(0.017)	(0.050)	(0.013)	(0.008)	(0.012)	(0.011)	(0.010)
Tribal	-0.050^{*}	-0.059^{**}	0.034^{*}	-0.092^{***}	-0.139^{*}	-0.035	0.018	0.007	0.015	-0.001
	(0.021)	(0.022)	(0.014)	(0.023)	(0.067)	(0.018)	(0.011)	(0.017)	(0.017)	(0.014)
Adj. R^2	0.567	0.523	0.548	0.461	0.363	0.333	0.285	0.397	0.409	0.416
Num. obs.	6019	6019	6019	5777	6507	6237	6542	6398	6345	6399
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Booth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A2.2: Electoral Impact: House v. Gas Cylinder (Full Rural Sample)

	Voted BJP (0/1)	Voted NDA (0/1)	Voted Ethnic Party (0/1)	Re-elect Incumbent (0/1)	Govt Satisfication (-2 to +2)	BJP works for poor (0,0.5,1)	Election involvement (Index)	Contacted by BJP (0/1)	Contacted by Cong (0/1)	Contacted by Reg Parties (0/1)
Got a house (β_1)	0.015	0.019	−0.009	0.014	0.060	0.023*	0.015*	0.002	−0.007	0.011
	(0.009)	(0.010)	(0.007)	(0.012)	(0.034)	(0.009)	(0.006)	(0.009)	(0.008)	(0.007)
Got a cylinder (β_2)	0.038***	0.029**	−0.014*	0.049***	0.163***	0.020*	0.028***	0.041***	0.016*	0.020***
	(0.009)	(0.010)	(0.006)	(0.010)	(0.030)	(0.008)	(0.005)	(0.008)	(0.007)	(0.006)
Muslim	−0.163***	−0.189***	0.121***	−0.237***	−0.648***	−0.116***	−0.004	−0.099***	0.015	0.018
	(0.016)	(0.018)	(0.015)	(0.020)	(0.061)	(0.016)	(0.011)	(0.014)	(0.013)	(0.011)
Dalit	−0.068***	−0.064***	0.068***	−0.026*	−0.076*	−0.026*	−0.010	−0.012	−0.000	0.002
	(0.011)	(0.012)	(0.008)	(0.013)	(0.038)	(0.010)	(0.007)	(0.009)	(0.009)	(0.007)
Tribal	−0.029	−0.027	0.024*	−0.048*	−0.062	−0.024	0.004	0.007	0.026	0.006
	(0.017)	(0.018)	(0.010)	(0.020)	(0.055)	(0.015)	(0.010)	(0.015)	(0.014)	(0.011)
Adj. R^2	0.611	0.551	0.550	0.460	0.423	0.360	0.284	0.411	0.454	0.530
Num. obs.	10157	10157	10157	9658	11164	10548	11361	10933	10657	11140
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Booth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A2.3: Electoral Impact: House v. Gas Cylinder (Full Sample With Ruralness Control)

	Voted BJP (0/1)	Voted NDA (0/1)	Voted Ethnic Party (0/1)	Re-elect Incumbent (0/1)	Govt Satisfication (-2 to +2)	BJP works for poor (0,0.5,1)	Election involvement (Index)	Contacted by BJP (0/1)	Contacted by Cong (0/1)	Contacted by Reg Parties (0/1)
Got a house (β_1)	0.020^{*}	0.020^{*}	-0.008	0.008	0.062^{*}	0.021^{**}	0.028^{***}	0.004	-0.011	0.002
	(0.008)	(0.009)	(0.006)	(0.010)	(0.030)	(0.008)	(0.006)	(0.008)	(0.007)	(0.006)
Got a cylinder (β_2)	0.044^{***}	0.038^{***}	-0.016^{**}	0.051^{***}	0.176^{***}	0.036^{***}	0.023^{***}	0.034^{***}	0.021^{**}	0.017^{**}
	(0.008)	(0.009)	(0.005)	(0.009)	(0.027)	(0.007)	(0.005)	(0.007)	(0.007)	(0.005)
Muslim	-0.151^{***}	-0.183^{***}	0.104^{***}	-0.223^{***}	-0.638^{***}	-0.119^{***}	-0.012	-0.082^{***}	0.007	0.017
	(0.014)	(0.015)	(0.012)	(0.017)	(0.050)	(0.013)	(0.009)	(0.012)	(0.011)	(0.009)
Dalit	-0.065^{***}	-0.068^{***}	0.057^{***}	-0.023^{*}	-0.116^{***}	-0.028^{**}	-0.012^{*}	-0.006	-0.001	0.007
	(0.010)	(0.011)	(0.007)	(0.011)	(0.033)	(0.009)	(0.006)	(0.008)	(0.008)	(0.006)
Tribal	-0.024	-0.014	0.023^{**}	-0.025	-0.082	-0.025	-0.001	-0.006	0.017	0.010
	(0.015)	(0.016)	(0.009)	(0.018)	(0.049)	(0.014)	(0.009)	(0.013)	(0.012)	(0.010)
Adj. R^2	0.610	0.543	0.533	0.459	0.432	0.356	0.278	0.401	0.456	0.530
Num. obs.	13471	13471	13471	13048	14962	14164	15198	14724	14384	14949
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Booth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Alternative Specification

I run regressions separately for each benefit (house and cooking gas cylinder) with the following specification:

$$\begin{aligned}\mathrm{Y}_{i,t} = \alpha_0 + \beta_1 \mathrm{Benefited}_{i,t-1} + \beta_2 \mathrm{Muslim}_{i,t} + \beta_3 \mathrm{Dalit}_{i,t} + \beta_4 \mathrm{Tribal}_{i,t} \\ + \rho \mathbf{X}_{i,t} + \sum_{j=1}^{J} \gamma_j \mathrm{Booth}_j\end{aligned}$$

Where $Y_{i,t}$ captures electoral support for the BJP, $\mathbf{X}$ is a vector of control variables like mean-centered age, gender (female or not), education, monthly household expenditure (binned), and past vote choice. The models also include polling booth fixed effects to adjust for any confounding due to time invariant factors at the precinct level.

Table A2.4 reports the results for the housing program, Table A2.5 for the cooking gas scheme, and Table A2.6 for an index of six flagship programs of the BJP government.

Table A2.4: Housing Scheme and Electoral Preferences (Full Results)

	Voted BJP (0/1)	Voted NDA (0/1)	Voted Ethnic Party (0/1)	Re-elect Incumbent (0/1)	Govt Satisfication (-2 to +2)	BJP works for poor (0,0.5,1)	Election involvement (Index)	Contacted by BJP (0/1)	Contacted by Cong (0/1)	Contacted by Reg Parties (0/1)
Benefited	0.042^{***}	0.045^{***}	-0.020^{*}	0.041^{**}	0.073	0.020	0.006	0.018	0.000	0.021^{*}
	(0.012)	(0.013)	(0.009)	(0.014)	(0.041)	(0.011)	(0.007)	(0.011)	(0.010)	(0.009)
Muslim	-0.198^{***}	-0.215^{***}	0.151^{***}	-0.327^{***}	-0.868^{***}	-0.184^{***}	-0.006	-0.130^{***}	0.034	0.008
	(0.023)	(0.023)	(0.022)	(0.025)	(0.081)	(0.020)	(0.012)	(0.018)	(0.017)	(0.016)
Dalit	-0.092^{***}	-0.086^{***}	0.103^{***}	-0.037^{*}	-0.140^{**}	-0.044^{***}	-0.014	-0.021	0.006	0.010
	(0.016)	(0.016)	(0.013)	(0.017)	(0.050)	(0.013)	(0.008)	(0.012)	(0.011)	(0.010)
Tribal	-0.048^{*}	-0.057^{*}	0.032^{*}	-0.090^{***}	-0.128	-0.033	0.020	0.009	0.016	0.001
	(0.021)	(0.022)	(0.014)	(0.023)	(0.067)	(0.018)	(0.011)	(0.017)	(0.017)	(0.014)
Adj. R^2	0.565	0.522	0.547	0.459	0.360	0.332	0.283	0.396	0.409	0.415
Num. obs.	6019	6019	6019	5777	6507	6237	6542	6398	6345	6399
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Booth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A2.5: Free Gas Cylinder Scheme and Electoral Preferences (Full Results)

	Voted BJP (0/1)	Voted NDA (0/1)	Voted Ethnic Party (0/1)	Re-elect Incumbent (0/1)	Govt Satisfication (-2 to +2)	BJP works for poor (0,0.5,1)	Election involvement (Index)	Contacted by BJP (0/1)	Contacted by Cong (0/1)	Contacted by Reg Parties (0/1)
Benefited	0.051^{***}	0.042^{***}	-0.024^{**}	0.052^{***}	0.187^{***}	0.029^{**}	0.027^{***}	0.040^{***}	0.017	0.026^{***}
	(0.011)	(0.012)	(0.009)	(0.012)	(0.036)	(0.010)	(0.006)	(0.009)	(0.009)	(0.007)
Muslim	-0.195^{***}	-0.213^{***}	0.150^{***}	-0.325^{***}	-0.856^{***}	-0.183^{***}	-0.004	-0.127^{***}	0.035^{*}	0.009
	(0.023)	(0.023)	(0.022)	(0.025)	(0.081)	(0.020)	(0.012)	(0.018)	(0.017)	(0.016)
Dalit	-0.093^{***}	-0.085^{***}	0.103^{***}	-0.037^{*}	-0.146^{**}	-0.044^{***}	-0.015	-0.022	0.005	0.010
	(0.016)	(0.016)	(0.013)	(0.017)	(0.050)	(0.013)	(0.008)	(0.012)	(0.011)	(0.010)
Tribal	-0.046^{*}	-0.054^{*}	0.032^{*}	-0.088^{***}	-0.134^{*}	-0.033	0.019	0.008	0.015	0.001
	(0.021)	(0.022)	(0.014)	(0.023)	(0.067)	(0.018)	(0.011)	(0.017)	(0.017)	(0.014)
Adj. R^2	0.566	0.522	0.547	0.460	0.363	0.333	0.285	0.397	0.409	0.416
Num. obs.	6019	6019	6019	5777	6507	6237	6542	6398	6345	6399
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Booth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A2.6: BJP Schemes (Index) and Electoral Preferences (Full Results)

	Voted BJP (0/1)	Voted NDA (0/1)	Voted Ethnic Party (0/1)	Re-elect Incumbent (0/1)	Govt Satisfication (-2 to +2)	BJP works for poor (0,0.5,1)	Election involvement (Index)	Contacted by BJP (0/1)	Contacted by Cong (0/1)	Contacted by Reg Parties (0/1)
Benefited	0.147^{***}	0.170^{***}	-0.078^{***}	0.181^{***}	0.595^{***}	0.115^{***}	0.078^{***}	0.130^{***}	0.044^{*}	0.099^{***}
	(0.025)	(0.026)	(0.021)	(0.027)	(0.087)	(0.023)	(0.016)	(0.024)	(0.022)	(0.019)
Muslim	-0.192^{***}	-0.208^{***}	0.148^{***}	-0.320^{***}	-0.846^{***}	-0.180^{***}	-0.003	-0.125^{***}	0.036^{*}	0.012
	(0.023)	(0.023)	(0.022)	(0.025)	(0.081)	(0.020)	(0.012)	(0.018)	(0.017)	(0.016)
Dalit	-0.091^{***}	-0.085^{***}	0.103^{***}	-0.037^{*}	-0.143^{**}	-0.044^{***}	-0.014	-0.022	0.005	0.010
	(0.016)	(0.016)	(0.013)	(0.017)	(0.050)	(0.013)	(0.008)	(0.012)	(0.011)	(0.010)
Tribal	-0.047^{*}	-0.056^{*}	0.032^{*}	-0.090^{***}	-0.137^{*}	-0.034	0.019	0.008	0.015	0.000
	(0.021)	(0.022)	(0.014)	(0.023)	(0.067)	(0.018)	(0.011)	(0.017)	(0.017)	(0.014)
Adj. R^2	0.567	0.525	0.548	0.463	0.365	0.335	0.286	0.399	0.409	0.418
Num. obs.	6019	6019	6019	5777	6507	6237	6542	6398	6345	6399
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Booth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Appendix C: Ideological Impact

Is receiving a material benefit associated with shifts in ideological beliefs? In classical voting models, the benefit b_i can compensate for ideological disutility, $(\sigma_i - \sigma_P)^2$, but it does not fundamentally change ideological positions. Formally speaking, we do not think that $\sigma_i = f(b_{i,t-1})$ (current ideology is a function of past benefits received).

In this appendix, I present correlational evidence from a fixed effects model with the specification:

$$\mathrm{Y}_{i,t} = \alpha_0 + \beta_1(\text{Got a house})_{i,t-1} + \beta_2(\text{Got a cylinder})_{i,t} + \rho\mathbf{X}_{i,t} + \sum_{j=1}^{J} \gamma_j \text{Booth}_j$$

Where $Y_{i,t}$ is a measure of political ideology, $\mathbf{X}$ is a vector of control variables like ethnicity, mean-centered age, gender (female or not), education, monthly household expenditure (binned), and past vote choice. The models also include polling booth fixed effects to adjust for any confounding due to time invariant factors at the precinct level.

The dependent variable captures ideological beliefs using three measures: (1) does the respondent think India is a Hindu nation; (2) how patriotic or unpatriotic are Muslims?; and (3) agreement with majoritarian statements (for instance: only my religion is correct, not of anyone else's; minorities should adopt the customs of the majority community; even if it is not liked by the majority, the government must protect the interests of the minorities (reverse coded); the Muslim community in India has been victimized under Narendra Modi's government (reverse coded).

Overall, receiving a benefit, big or small, is not associated with holding more majoritarian beliefs, or thinking that India is a Hindu nation. Cylinder recipients,

if anything, are *less* likely to say India is a Hindu nation ($\widehat{\beta}_2 = -0.041$, s.e.=0.015). Cylinder recipients are also not more prejudiced towards Muslim. Home recipients, on the other hand, think think Muslims are less patriotic ($\widehat{\beta}_1 = -0.194$, s.e.=0.064). Muslims, unsurprisingly, are less likely to hold majoritarian beliefs and think India is a Hindu nation, and more likely to say their ethnic group is patriotic. In sum, material benefits may compensate for ideological disutility but they do not seem to shape ideological beliefs as such.

Table A2.7: Material Benefits and Political Ideology

	Majoritarian Beliefs (Index, -2 to +2)	India is a Hindu Nation (0/1)	How patriotic are Muslims? (-2 to +2)
Got a house (β_1)	0.007	0.026	-0.194^{**}
	(0.037)	(0.018)	(0.064)
Got a cylinder (β_2)	0.010	-0.041^{**}	-0.020
	(0.034)	(0.015)	(0.058)
Muslim	-0.214^{***}	-0.153^{***}	1.249^{***}
	(0.065)	(0.030)	(0.125)
Dalit	0.017	0.017	0.083
	(0.042)	(0.019)	(0.074)
Tribal	-0.026	-0.050	0.199
	(0.055)	(0.028)	(0.104)
Age (Mean Centered)	-0.000	0.000	-0.002
	(0.001)	(0.000)	(0.002)
Female	0.002	0.012	-0.109^{*}
	(0.029)	(0.013)	(0.048)
Education	0.001	0.002	-0.020
	(0.009)	(0.004)	(0.015)
Monthly Expend.	-0.000	0.000	0.000^{*}
	(0.000)	(0.000)	(0.000)
Past Vote=BJP	0.078^{*}	0.011	-0.171^{**}
	(0.032)	(0.015)	(0.058)
Adj. R^2	0.353	0.357	0.527
Num. obs.	2928	3378	2612
Booth FE	Yes	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Appendix D: Design Tests (State Assembly Constituency Level)

McCrary Density Test

The McCrary density test is performed using `rddensity` package in `R`, using the default specifications: a local quadratic approximation (p=2), triangular kernel, and MSE optimal bandwidth.

Table A2.8: McCrary Density Test

Diff. in Densities	t statistic	p
0.011	4.246	0

Note: The McCrary test suggests there is a discontinuous change in the density of the forcing variable at the cut-point ($x = 0$). This can happen because of two reasons: (1) close elections were manipulated in favor of (or against) the BJP; or (2) the survey company sampled more respondents on one side of the cut-point than the other. The first situation poses a problem for identification because it falsifies the "as-if randomness" or "coin-flip" logic of close elections. The second situation can arise with random sampling of constituencies and respondents, or even when there is asymmetric non-contact of respondents. This poses a problem for identification if respondent characteristics, or other predictors of the outcome, also discontinuously change at the cut-point.

To rule out these possibilities, I perform the McCrary density test on the official election results ($n = 4053$ assembly constituencies in a parliamentary election where the BJP or its ally fielded candidates). Table A2.9 reports the summary statistics from this test. We fail to reject the null hypothesis of no difference in densities at the cut-point. This can be seen visually in the section below, where I report the

frequency distribution (see figure A2.2). Finally, I check for covariate balance within the survey sample, and do not find any discontinuous changes.

Table A2.9: McCrary Density Test (All Assembly Constituencies)

Diff. in Densities	t statistic	p
0.004	1.443	0.149

Frequency Distribution

The top figure reports the frequency distribution of the forcing variable for survey respondents. The bottom figure reports the same for all assembly segments (restricted to parliamentary constituencies where the BJP or its allies fielded a candidate).

Figure A2.1: Forcing Variable Frequency Distribution (Survey Respondents, $n = 9654$)

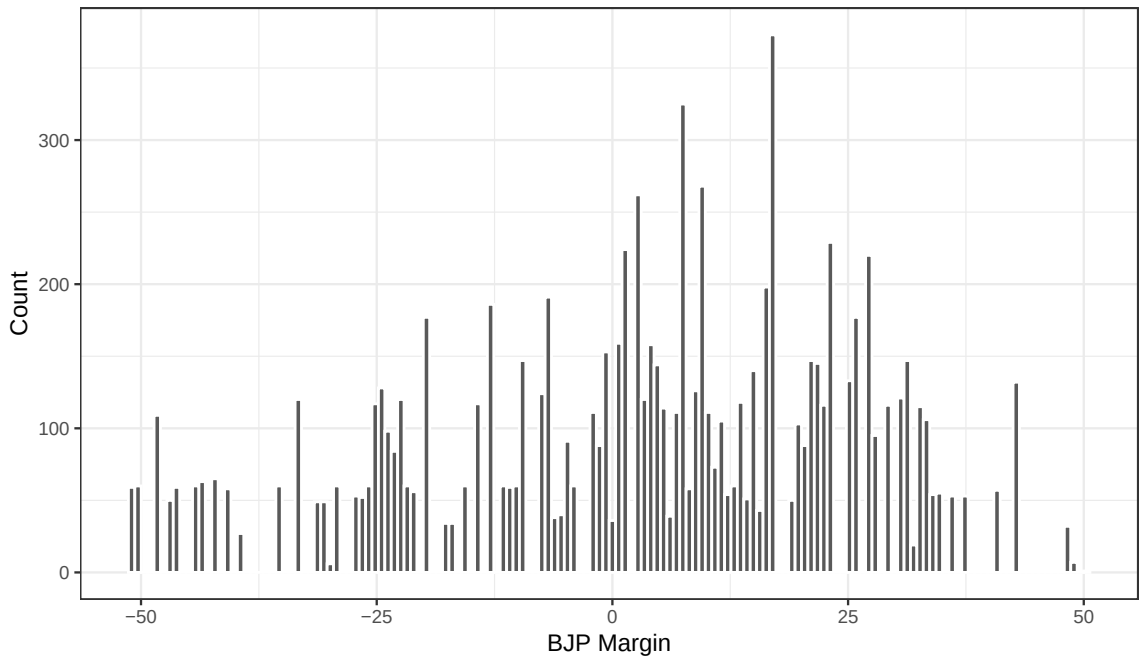
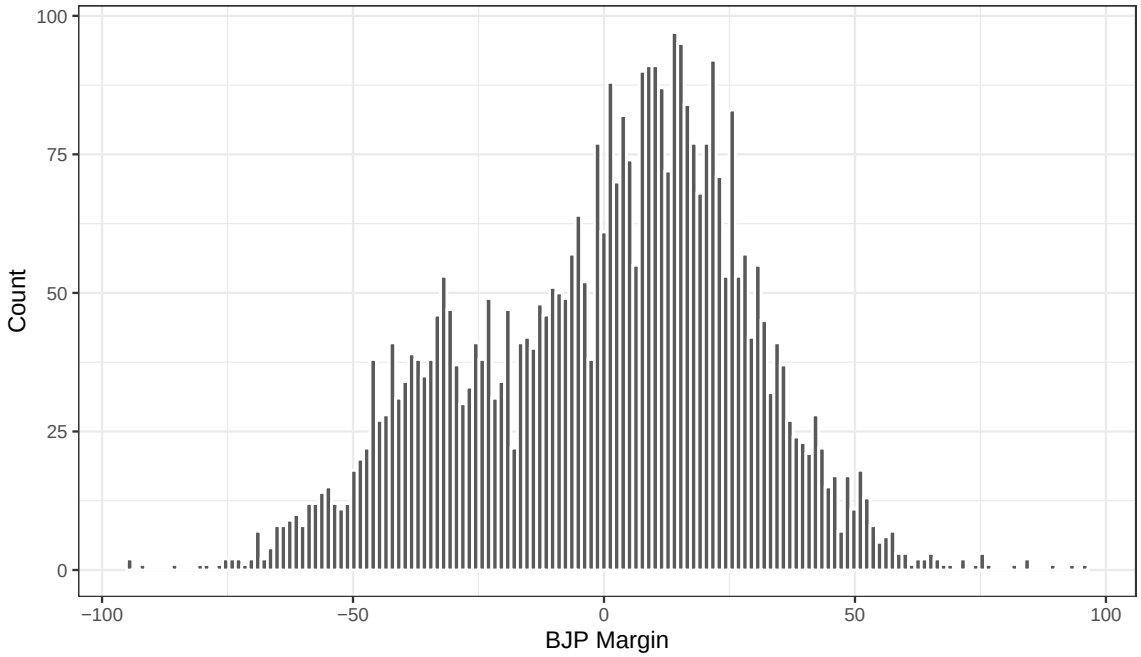


Figure A2.2: Forcing Variable Frequency Distribution (4053 Assembly Constituencies)



Covariate Balance

The table below checks for any discontinuous change in covariates at the cut-point, using exactly the same specification as the primary outcome analysis.

Table A2.10: Covariate Balance (AC Level RD)

Covariate	RD (MSE optimal BW)				
DV	Coef	SE	p	n	BW (L,R)
Hindu	0.074	0.165	0.651	3358	10.65,10.65
Muslim	-0.152	0.165	0.356	3187	10.24,10.24
Low Status Group	-0.065	0.055	0.241	2518	7.77,7.77
Age (Mean Centered)	-0.435	1.680	0.796	3994	13.49,13.49
Female	0.061	0.050	0.225	3893	12.91,12.91
Education	-0.452	0.375	0.228	4327	15.06,15.06
Monthly Expenditure	191.965	1110.087	0.863	3296	10.95,10.95
Monthly Income	404.875	1655.364	0.807	3789	13.39,13.39
Past Vote = BJP	0.008	0.126	0.948	2425	10.51,10.51
Landless	0.035	0.131	0.788	2948	9.52,9.52
Ineligible for Housing Scheme	0.012	0.099	0.903	4243	16.01,16.01

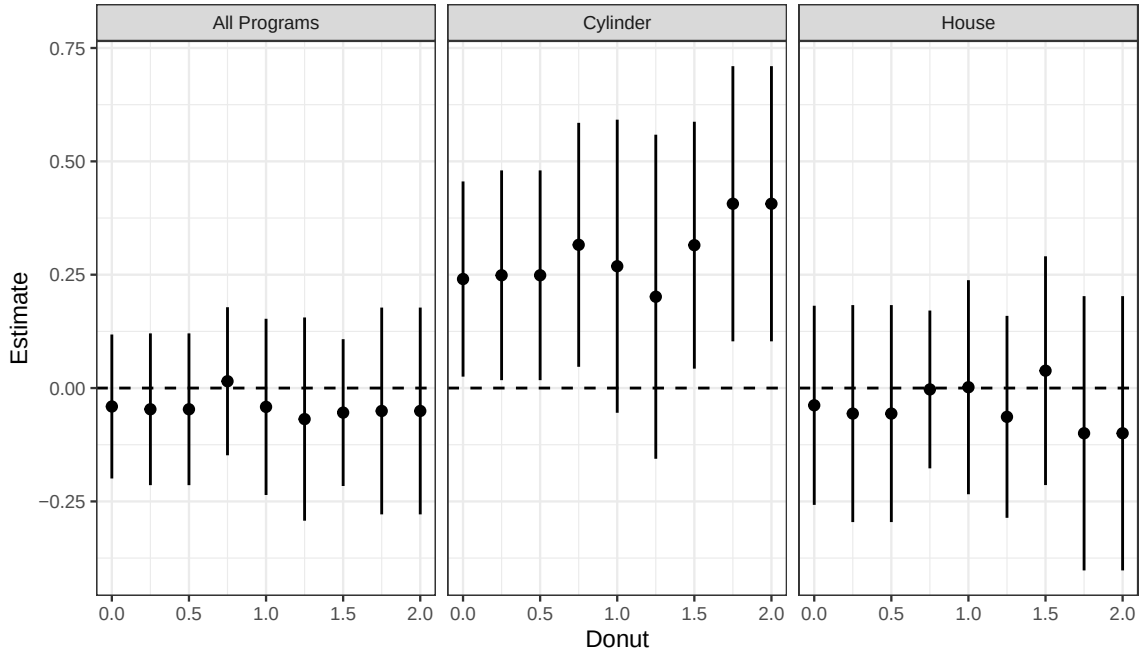
Note:
The difference at the cut-point was estimated using rdrobust in R, specifying a first-order polynomial (p=1), triangular kernel weights, and MSE-optimal bandwidths. We report the robust, bias-corrected estimate and HC2 robust standard error (clustered at the assembly constituency level). This is identical to the primary outcome specification in the paper. Data from National Election Studies 2019, Election Commission of India 2014

Donut Hole RD Estimates

In this section, I evaluate how sensitive the results are to the inclusion of observations near the cut point. I report the results from a "donut hole" regression discontinuity design. Cattaneo, Idrobo, and Titiunik (2019) explain the utility of such an approach:

> If systematic manipulation of score values occurred, it is natural to assume that the units closest to the cutoff are those most likely to have engaged in manipulation. The idea behind this approach is to exclude such units and then repeat the estimation and inference analysis using the remaining sample. (Cattaneo, Idrobo, and Titiunik 2019:103)

Figure A2.3: Donut Hole RD Estimates (Assembly Constituency Level)



Note: This figure shows the RD estimate and 95% confidence interval after excluding observations within the donut radius around the cut point. Data for this figure is presented in Table A2.11.

Table A2.11: Donut-Hole Approach (AC Level RD)

Donut	BW	DV	Estimate	SE	p	CI (L)	CI (H)	Dropped
0.00	15.53	All Programs	-0.04	0.08	0.62	-0.20	0.12	0
0.25	16.53	All Programs	-0.05	0.09	0.58	-0.21	0.12	36
0.50	16.53	All Programs	-0.05	0.09	0.58	-0.21	0.12	36
0.75	10.85	All Programs	0.01	0.08	0.86	-0.15	0.18	183
1.00	10.20	All Programs	-0.04	0.10	0.68	-0.24	0.15	306
1.25	10.54	All Programs	-0.07	0.11	0.55	-0.29	0.16	408
1.50	6.52	All Programs	-0.05	0.08	0.51	-0.22	0.11	544
1.75	6.87	All Programs	-0.05	0.12	0.66	-0.28	0.18	660
2.00	6.87	All Programs	-0.05	0.12	0.66	-0.28	0.18	660
0.00	12.77	Cylinder	0.24	0.11	0.03	0.03	0.46	0
0.25	13.42	Cylinder	0.25	0.12	0.04	0.02	0.48	36
0.50	13.42	Cylinder	0.25	0.12	0.04	0.02	0.48	36
0.75	10.51	Cylinder	0.32	0.14	0.02	0.05	0.58	183
1.00	11.86	Cylinder	0.27	0.16	0.10	-0.05	0.59	306
1.25	13.44	Cylinder	0.20	0.18	0.27	-0.16	0.56	408
1.50	7.37	Cylinder	0.32	0.14	0.02	0.04	0.59	544
1.75	8.28	Cylinder	0.41	0.15	0.01	0.10	0.71	660
2.00	8.28	Cylinder	0.41	0.15	0.01	0.10	0.71	660
0.00	12.12	House	-0.04	0.11	0.73	-0.26	0.18	0
0.25	12.25	House	-0.06	0.12	0.64	-0.30	0.18	36
0.50	12.25	House	-0.06	0.12	0.64	-0.30	0.18	36
0.75	13.47	House	0.00	0.09	0.97	-0.18	0.17	183
1.00	11.49	House	0.00	0.12	0.99	-0.23	0.24	306
1.25	14.22	House	-0.06	0.11	0.58	-0.29	0.16	408
1.50	7.37	House	0.04	0.13	0.77	-0.21	0.29	544
1.75	6.97	House	-0.10	0.15	0.52	-0.40	0.20	660
2.00	6.97	House	-0.10	0.15	0.52	-0.40	0.20	660

Note:
The difference at the cut-point was estimated using rdrobust in R, specifying a first-order polynomial (p=1), triangular kernel weights, and MSE-optimal bandwidth. Observations within the donut radius are excluded. We report the robust, bias-corrected estimate and HC2 robust standard error (clustered at the assembly constituency level). This is identical to the primary outcome specification in the paper. Data from National Election Studies 2019, Election Commission of India 2014

Appendix E: Design Tests (Parliamentary Constituency Level)

McCrary Density Test

The McCrary density test is performed using `rddensity` package in `R`, using the default specifications: a local quadratic approximation (p=2), triangular kernel, and MSE optimal bandwidth.

Table A2.12: McCrary Density Test

Diff. in Densities	t statistic	p
-0.007	-4.116	0

Note: The McCrary test suggests there is a discontinuous change in the density of the forcing variable at the cut-point ($x = 0$). This can happen because of two reasons: (1) close elections were manipulated in favor of (or against) the BJP; or (2) the survey company sampled more respondents on one side of the cut-point than the other. The first situation poses a problem for identification because it falsifies the "as-if randomness" or "coin-flip" logic of close elections. The second situation can arise with random sampling of constituencies and respondents, or even when there is asymmetric non-contact of respondents. This poses a problem for identification if respondent characteristics, or other predictors of the outcome, also discontinuously change at the cut-point.

To rule out these possibilities, I perform the McCrary density test on the official election results ($n = 537$ parliamentary constituencies where the BJP or its ally fielded candidates). Table A2.13 reports the summary statistics from this test. We fail to reject the null hypothesis of no difference in densities at the cut-point. This can be seen visually in the section below, where I report the frequency distribution (see

figure A2.5). Finally, I check for covariate balance within the survey sample, and do not find any discontinuous changes.

Table A2.13: McCrary Density Test (All Parliamentary Constituencies)

Diff. in Densities	t statistic	p
-0.002	-0.284	0.776

Frequency Distributions

The top figure reports the frequency distribution of the forcing variable for survey respondents. The bottom figure reports the same for all parliamentary constituencies in which the BJP, or its allies, fielded candidates.

Figure A2.4: Forcing Variable Frequency Distribution (Survey Respondents, $n = 9658$)

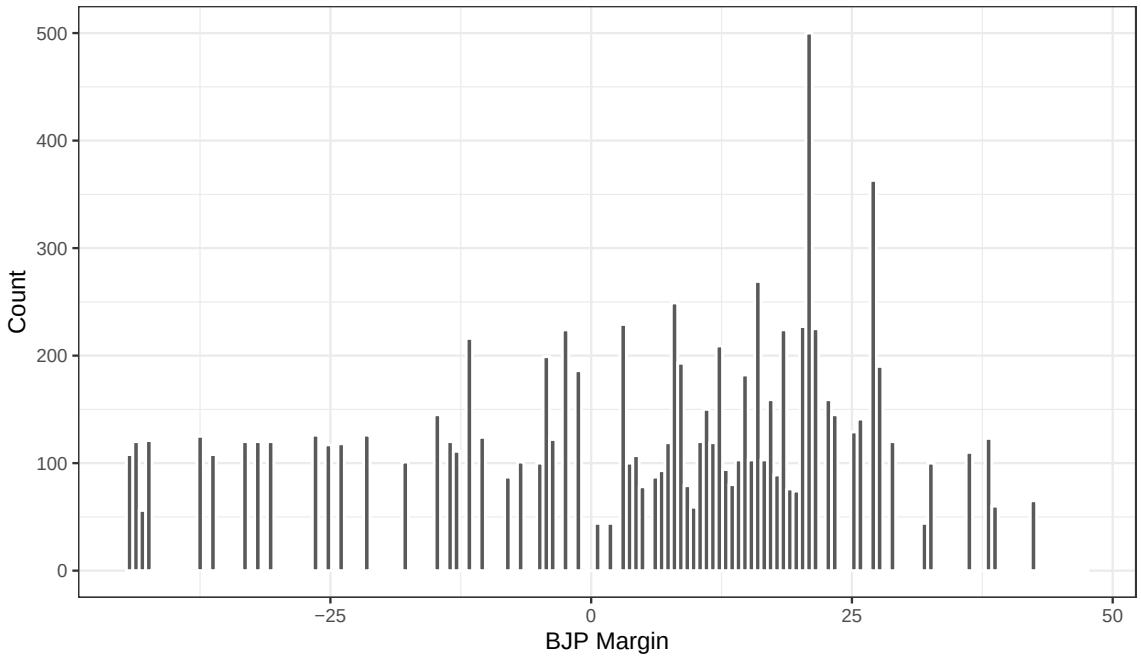
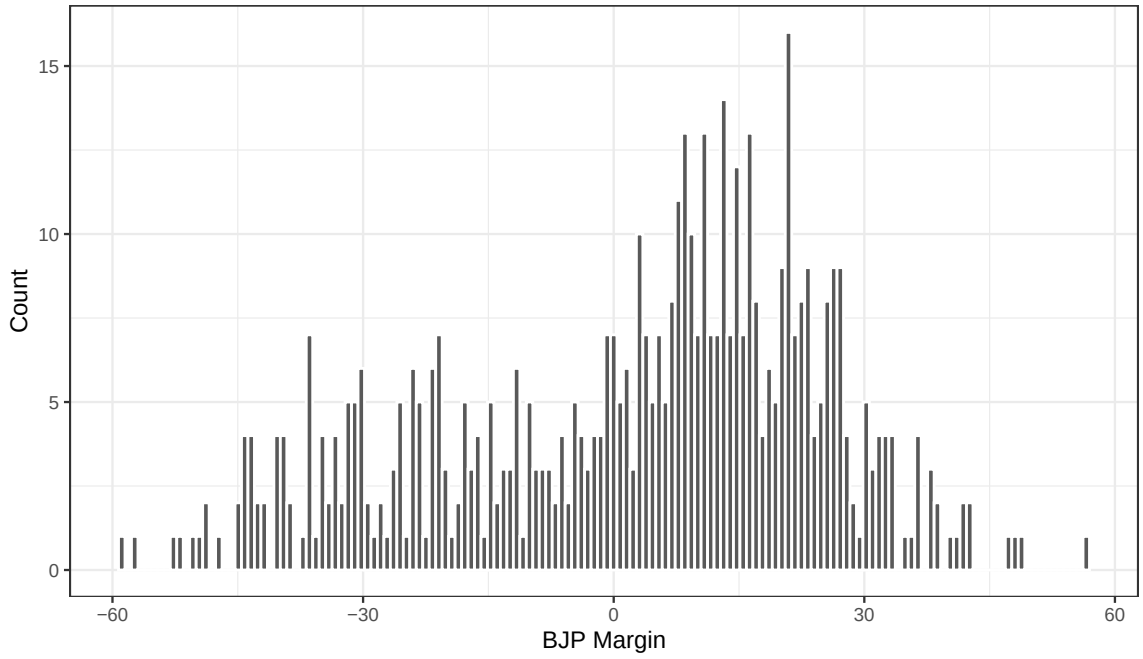


Figure A2.5: Forcing Variable Frequency Distribution (537 Parliamentary Constituencies)



Covariate Balance

The table below checks for any discontinuous change in covariates at the cut-point, using exactly the same specification as the primary outcome analysis.

Table A2.14: Covariate Balance (PC Level RD)

Covariate	RD (MSE optimal BW)				
DV	Coef	SE	p	n	BW (L,R)
Hindu	-0.059	0.138	0.671	2620	10.3,10.3
Muslim	-0.065	0.087	0.456	1833	7.37,7.37
Low Status Group	0.021	0.161	0.898	2742	10.62,10.62
Age (Mean Centered)	-2.464	2.683	0.358	2744	10.81,10.81
Female	0.138	0.071	0.051	1895	7.84,7.84
Education	0.161	0.456	0.725	3192	12.37,12.37
Monthly Expenditure	-426.269	1217.347	0.726	2349	9.26,9.26
Monthly Income	-1725.593	1965.691	0.380	2036	8.74,8.74
Past Vote = BJP	0.028	0.229	0.901	2392	12.43,12.43
Landless	0.030	0.137	0.829	2168	8.3,8.3
Ineligible for Housing Scheme	0.260	0.183	0.155	2579	10.4,10.4

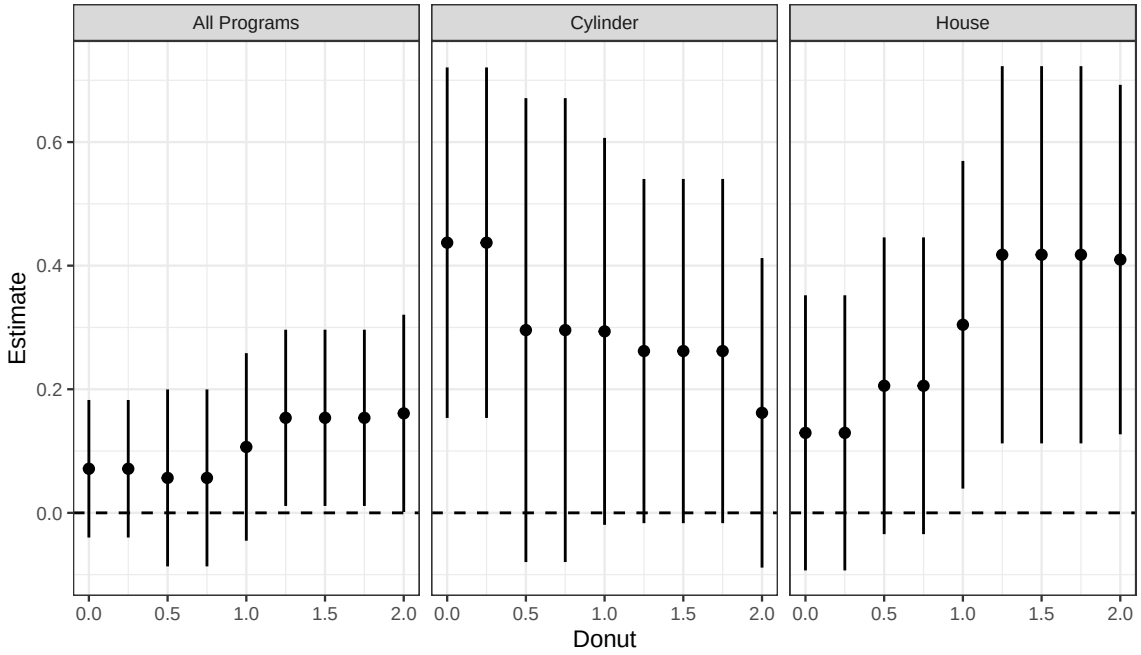
Note:
The difference at the cut-point was estimated using rdrobust in R, specifying a first-order polynomial (p=1), triangular kernel weights, and MSE-optimal bandwidths. We report the robust, bias-corrected estimate and HC2 robust standard error (clustered at the parliamentary constituency level). This is identical to the primary outcome specification in the paper. Data from National Election Studies 2019, Election Commission of India 2014

Donut Hole RD Estimates

In this section, I evaluate how sensitive the results are to the inclusion of observations near the cut point. I report the results from a "donut hole" regression discontinuity design. Cattaneo, Idrobo, and Titiunik (2019) explain the utility of such an approach:

> If systematic manipulation of score values occurred, it is natural to assume that the units closest to the cutoff are those most likely to have engaged in manipulation. The idea behind this approach is to exclude such units and then repeat the estimation and inference analysis using the remaining sample. (Cattaneo, Idrobo, and Titiunik 2019:103)

Figure A2.6: Donut Hole RD Estimates (Parliamentary Constituency Level)



Note: This figure shows the RD estimate and 95% confidence interval after excluding observations within the donut radius around the cut point. Data for this figure is presented in Table A2.15.

Table A2.15: Donut-Hole Approach (PC Level RD)

Donut	BW	DV	Estimate	SE	p	CI (L)	CI (H)	Dropped
0.00	8.61	All Programs	0.07	0.06	0.21	-0.04	0.18	0
0.25	8.61	All Programs	0.07	0.06	0.21	-0.04	0.18	0
0.50	9.84	All Programs	0.06	0.07	0.44	-0.09	0.20	44
0.75	9.84	All Programs	0.06	0.07	0.44	-0.09	0.20	44
1.00	11.73	All Programs	0.11	0.08	0.17	-0.05	0.26	164
1.25	10.31	All Programs	0.15	0.07	0.03	0.01	0.30	230
1.50	10.31	All Programs	0.15	0.07	0.03	0.01	0.30	230
1.75	10.31	All Programs	0.15	0.07	0.03	0.01	0.30	230
2.00	10.83	All Programs	0.16	0.08	0.05	0.00	0.32	274
0.00	7.23	Cylinder	0.44	0.14	0.00	0.15	0.72	0
0.25	7.23	Cylinder	0.44	0.14	0.00	0.15	0.72	0
0.50	8.32	Cylinder	0.30	0.19	0.12	-0.08	0.67	44
0.75	8.32	Cylinder	0.30	0.19	0.12	-0.08	0.67	44
1.00	9.06	Cylinder	0.29	0.16	0.07	-0.02	0.61	164
1.25	10.97	Cylinder	0.26	0.14	0.07	-0.02	0.54	230
1.50	10.97	Cylinder	0.26	0.14	0.07	-0.02	0.54	230
1.75	10.97	Cylinder	0.26	0.14	0.07	-0.02	0.54	230
2.00	11.50	Cylinder	0.16	0.13	0.21	-0.09	0.41	274
0.00	9.68	House	0.13	0.11	0.25	-0.09	0.35	0
0.25	9.68	House	0.13	0.11	0.25	-0.09	0.35	0
0.50	9.84	House	0.21	0.12	0.09	-0.03	0.45	44
0.75	9.84	House	0.21	0.12	0.09	-0.03	0.45	44
1.00	8.49	House	0.30	0.14	0.02	0.04	0.57	164
1.25	7.30	House	0.42	0.16	0.01	0.11	0.72	230
1.50	7.30	House	0.42	0.16	0.01	0.11	0.72	230
1.75	7.30	House	0.42	0.16	0.01	0.11	0.72	230
2.00	8.91	House	0.41	0.14	0.00	0.13	0.69	274

Note:
The difference at the cut-point was estimated using rdrobust in R, specifying a first-order polynomial (p=1), triangular kernel weights, and MSE-optimal bandwidth. Observations within the donut radius are excluded. We report the robust, bias-corrected estimate and HC2 robust standard error (clustered at the parliamentary constituency level). This is identical to the primary outcome specification in the paper. Data from National Election Studies 2019, Election Commission of India 2014

Appendix F: Regression Discontinuity Plots

Figure A2.7: Assembly Constituency Level Analysis of Survey Respondents

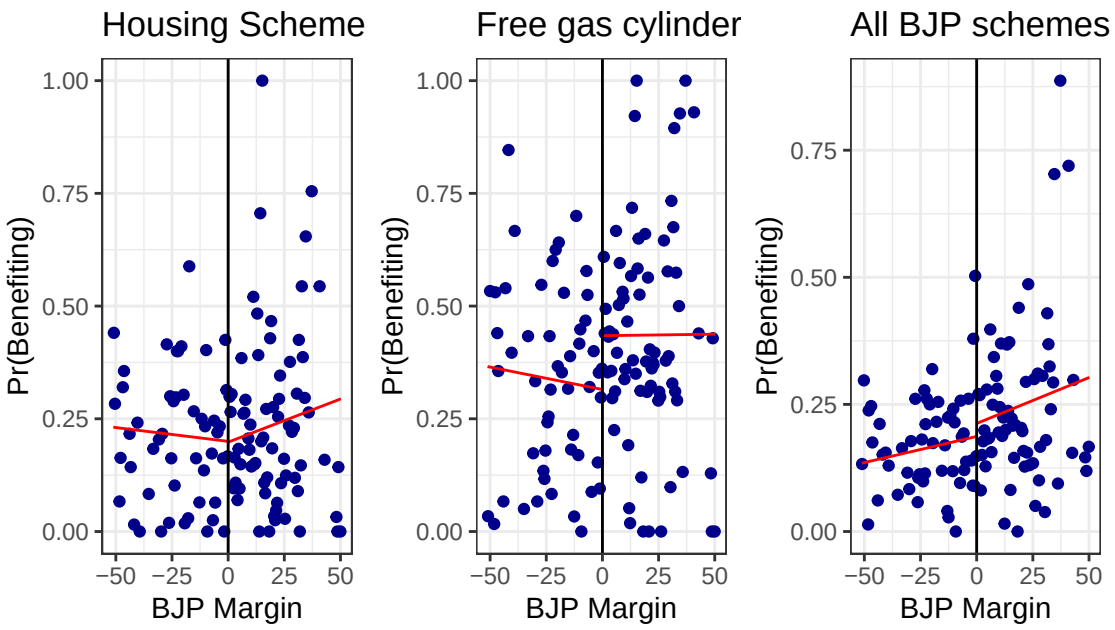
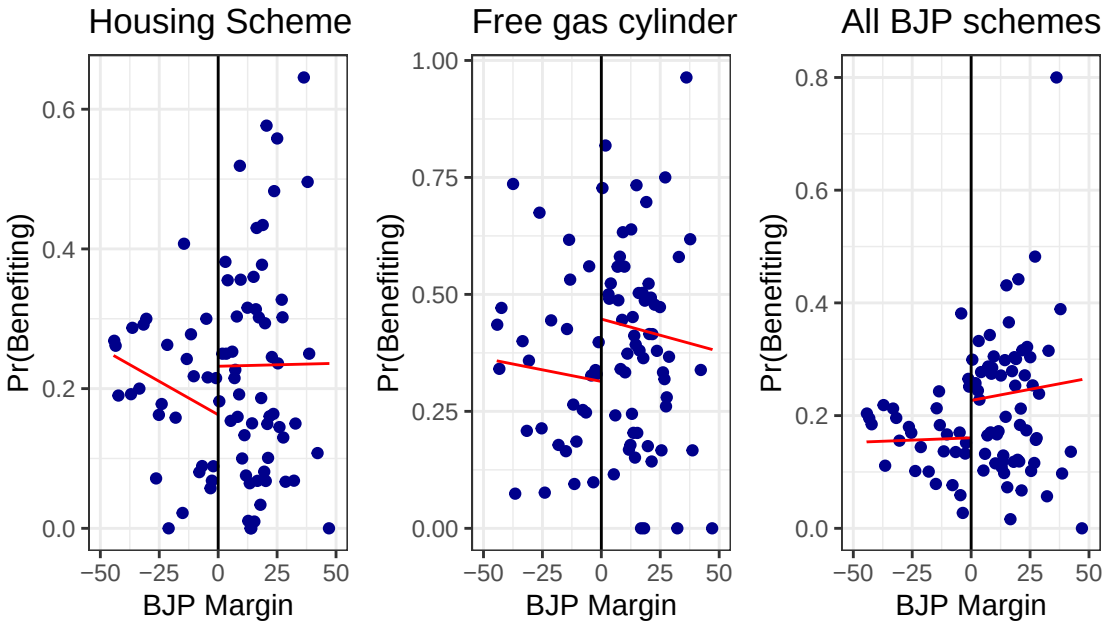


Figure A2.8: Parliamentary Constituency Level Analysis of Survey Respondents



Winning Support by Distributing Houses? Evidence from India

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.

Introduction

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

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

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

I focus on India's Bihar province, specifically three districts where Dalits are swing voters or weakly opposed to the ruling party. Conventionally, Muslims have strong ethnic reasons to oppose the current ruling party because of its Hindu majoritarian ideology and politically motivated violence against minorities (Wilkinson 2004; Nellis, Weaver, and Rosenzweig 2016; 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

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

[2] Chapter 2 in Thachil (2014) explains how the ruling party's ideology, position on key issues and spending priorities have an elite bias and do not appeal to subaltern groups. See also Chapter 4 in Jaffrelot (2021).

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, 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 3.1.

Importantly, the program does not reduce the distributive salience of ethnicity.

Table 3.1: 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.

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 multi-ethnic 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 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 $c \in (0, 1)$.

$$U_i(b_i, \sigma_i, \sigma_P) = -(\sigma_i - \sigma_P)^2 + b_i - c_i \tag{3.1}$$

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.

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

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

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

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

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

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.

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.

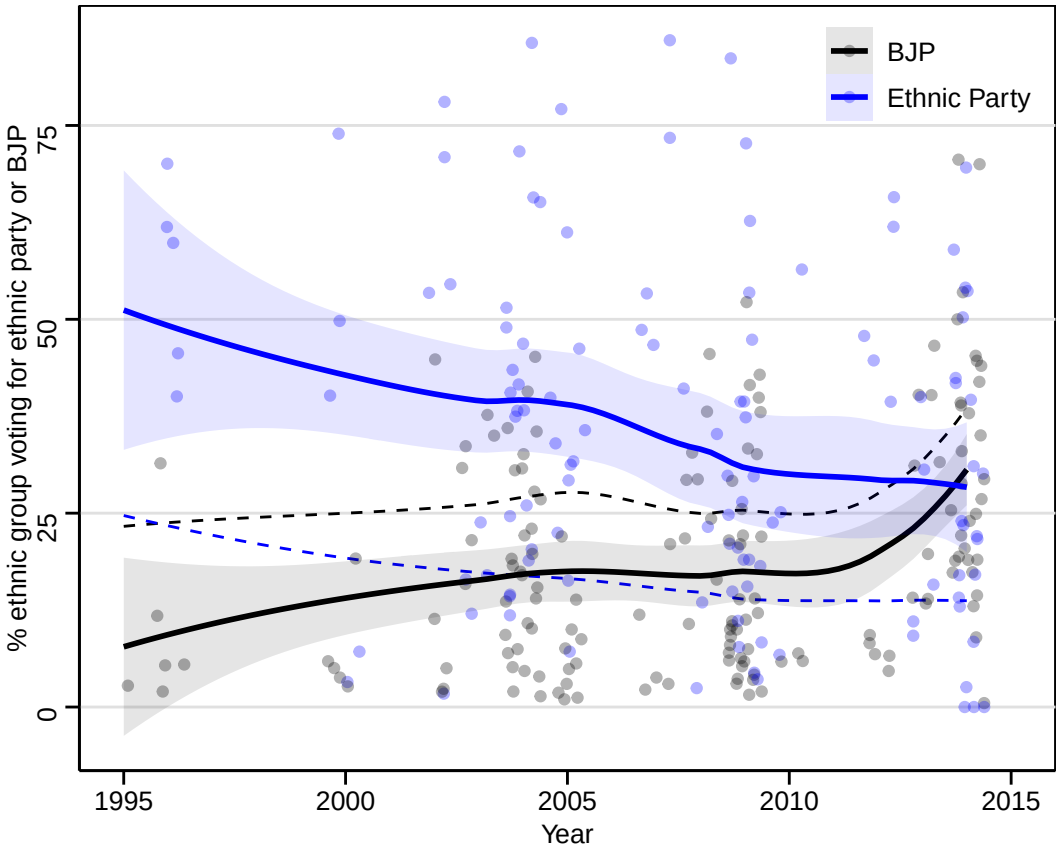
To see this, Figure 3.1 shows the proportion of an ethnic group voting for *its* ethnic party and the BJP in parliamentary and provincial elections since 1995. The analysis focuses on what Thachil and Teitelbaum (2015) call "narrow ethnic parties" that follow "patronage-based strategies *within* their 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

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

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

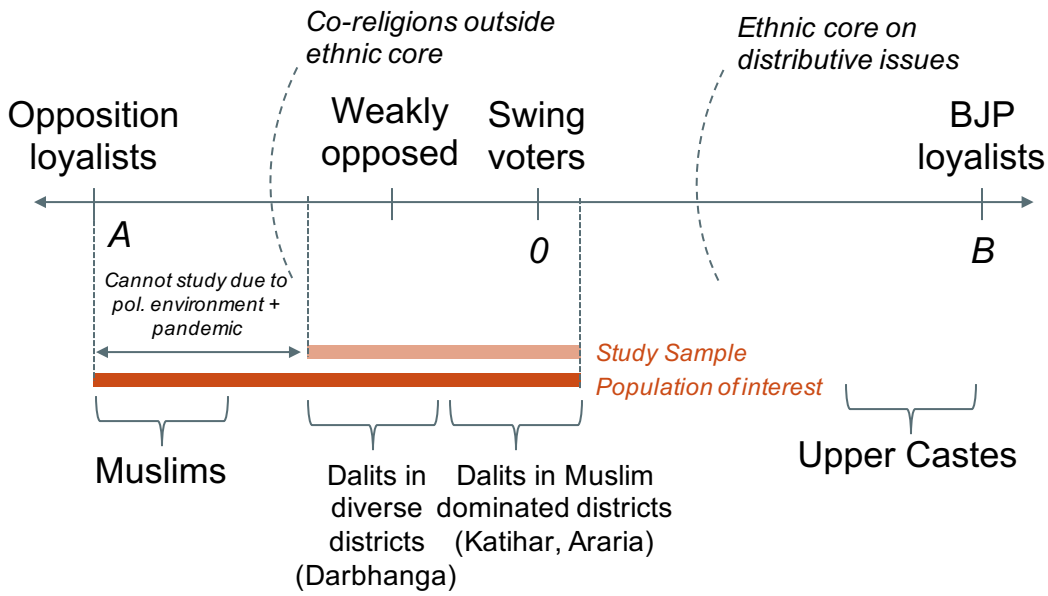
well-known ethnic parties between 1995 and 2014. This decline is not due to erosion in support among peripheral groups but hollowing of the core base. In the same period, there is a commensurate increase in support for the BJP. Clearly, the BJP increasingly appealed to voters outside its ethnic core 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?

Figure 3.1: 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).

Figure 3.2: 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.

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 3.2 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. 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 ethni-

cally 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]. Figure A3.1 in the appendices 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 3.2 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]

Figure A3.2 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 3.1. Could material benefits, delivered programmatically, have contributed to this realignment?

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

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

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

[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.

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

[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.

things must be true: (i) people should need the local leader's help to benefit from the program; and (ii) the local leader should get most of the credit for the program. For there to be clientelistic inertia, brokers should exert influence over political preferences, and control the supply of information to voters. This would typically imply low levels of awareness about the benefit, and high levels of misattribution.

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

$$\mathbb{E}(Y_i|\text{Offered a house}) - \mathbb{E}(Y_i|\text{Not offered a house}) \tag{3.2}$$

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*:

$$\mathbb{E}(Y_i(1) - Y_i(0)|Distance_i = 0) \tag{3.3}$$

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:

$$\text{Distance}_i = \frac{(-1) \times (\text{Rank}_i - [\text{Rank}_{\text{last beneficiary j}} + 0.5])}{n_{\text{village}}} \tag{3.4}$$

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 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

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

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). Table A3.2 in the Appendix 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 3.2 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 3.2 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 3.2: 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 A3.9 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

[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.

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 3.3 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 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 3.3 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%.

Table 3.3: 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.

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 3.4 reports the difference at the cut-point ($\widehat{\tau}$).[14] The appendices report 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

[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.

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 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. Table A3.1 in the Appendix 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 A3.1 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.

Table 3.4: 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 $\overline{Y_{Z=0}}$).

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. The appendices show 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

[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.

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 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 3.6). 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 3.3 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.

Figure 3.3: 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.

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

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 3.5, and figure A3.8 in the Appendix). 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.

Table 3.5: 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 3.4.

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 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. Figure A3.10 in the Appendix 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.

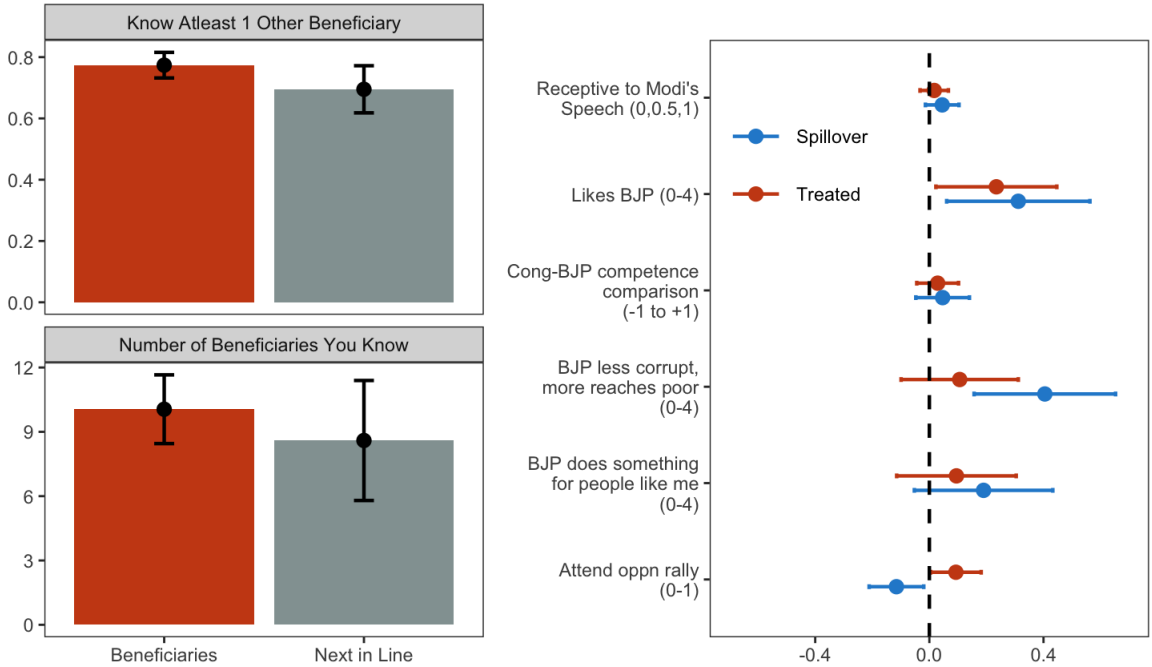
Network Exposure

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 3.4 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. Figure A3.11 in Appendix confirms that respondents around the cut-point report very similar network-level exposure to the program.

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

Figure 3.4: 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.

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]

$$Y_{i,j} = \beta_0 + \beta_1(\text{Benefited}_i) + \beta_2(\text{Exposed}_i) + \gamma(\text{Deprivation Score}_i) + \lambda_j \qquad (3.5)$$

The right panel in Figure 3.4 plots the coefficients from this model for treatment

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

($\widehat{\beta}_1$ 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 3.4). 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 3.6 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.

[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.

Table 3.6: 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	-0.29	0.18	0.10	386	-1.06	0.38	0.01	180	3.66
Skipped Meal	0.15	0.09	0.10	311	0.27	0.19	0.16	152	0.28
Monthly Income	-1605.05	1067.80	0.13	274	-3105.91	1700.80	0.07	152	6902.84
Recent Debt	2266.49	1565.74	0.15	305	7029.30	3143.90	0.03	152	5897.16

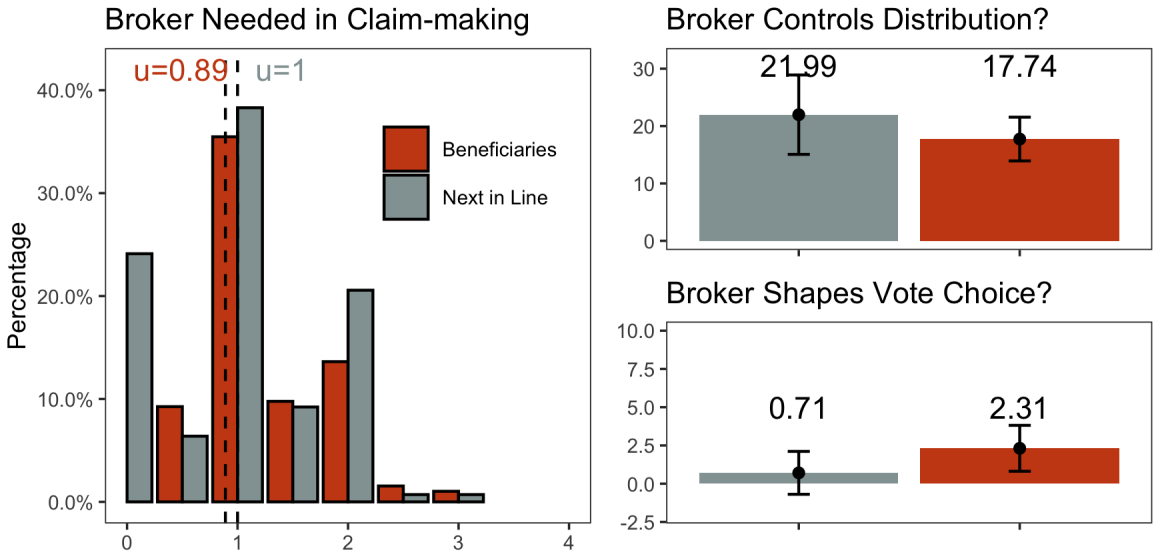
Note: See note to Table 3.4.

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 3.5 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.

Figure 3.5: 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.

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 3.7). 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 3.5 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.

Table 3.7: 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

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 3.5 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 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,

Table 3.8: 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 3.4.

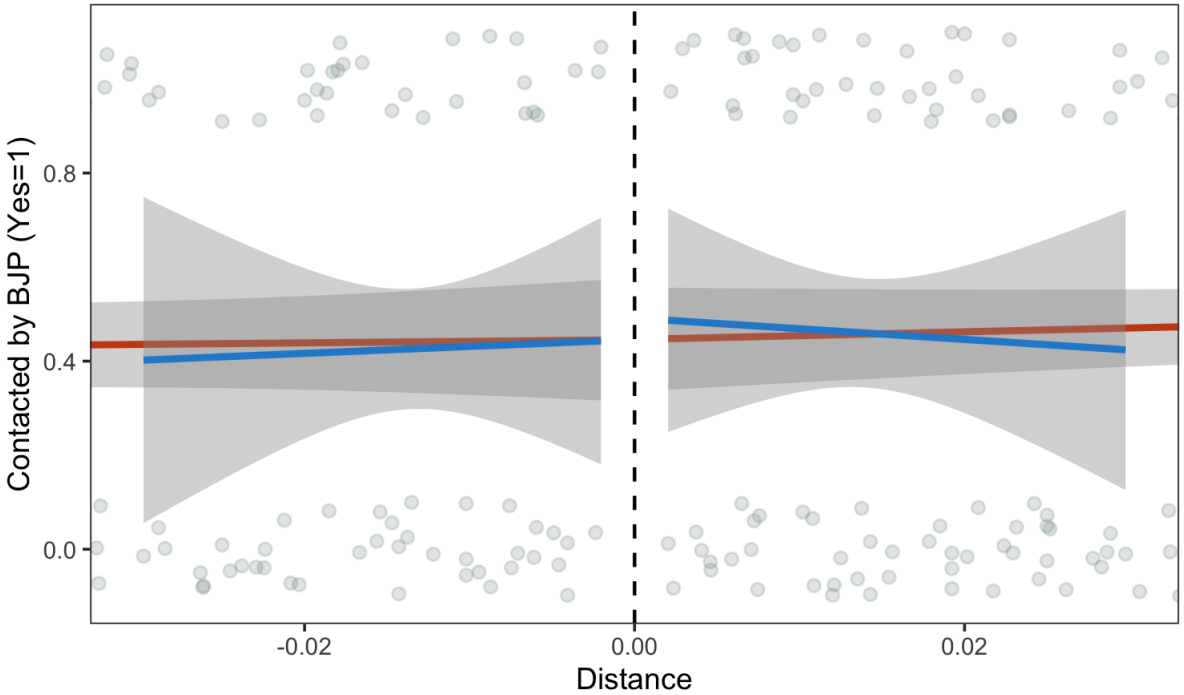
contact beneficiaries and monitor them during elections.

There is weak evidence for such a proposition. To begin with, brokers do not have much electoral influence (see bottom right panel in Figure 3.5). 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 3.8). There is no difference in contact rates at the cut-point (see Figure 3.6). 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

Figure 3.6: Canvassed by BJP?



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 (±3%), and shows estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth (±3%) and triangular kernel (in blue). 95% confidence intervals are depicted in gray.

anti-Muslim appeals. To check whether this is the case, I 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 the Appendix, 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 A3.12 in the Appendix).

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 3.9 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.

Table 3.9: 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.

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.

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 A3.13).[19] An even smaller percentage of subjects on either side of the cut-

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

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 A3.13 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 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 out-

see Table A3.1.

[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 ($Z_i = 0$).

side 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.

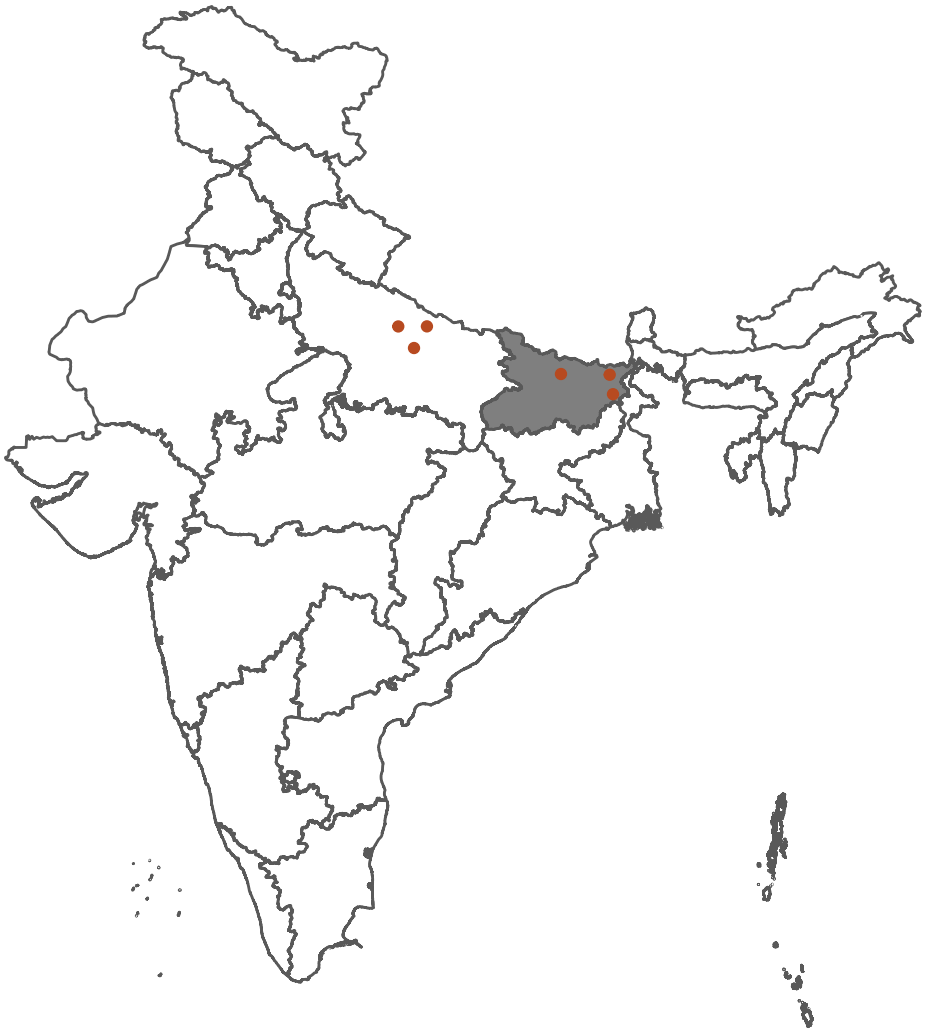
On the positive side, programmatic distribution does seem to improve last-mile delivery. The paper finds that beneficiaries are highly satisfied with the program, there are fewer reports of bribing, and more "deserving" people get the benefit (less mistargeting). Most importantly, beneficiaries know 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?

Appendix A: Location of Field Sites

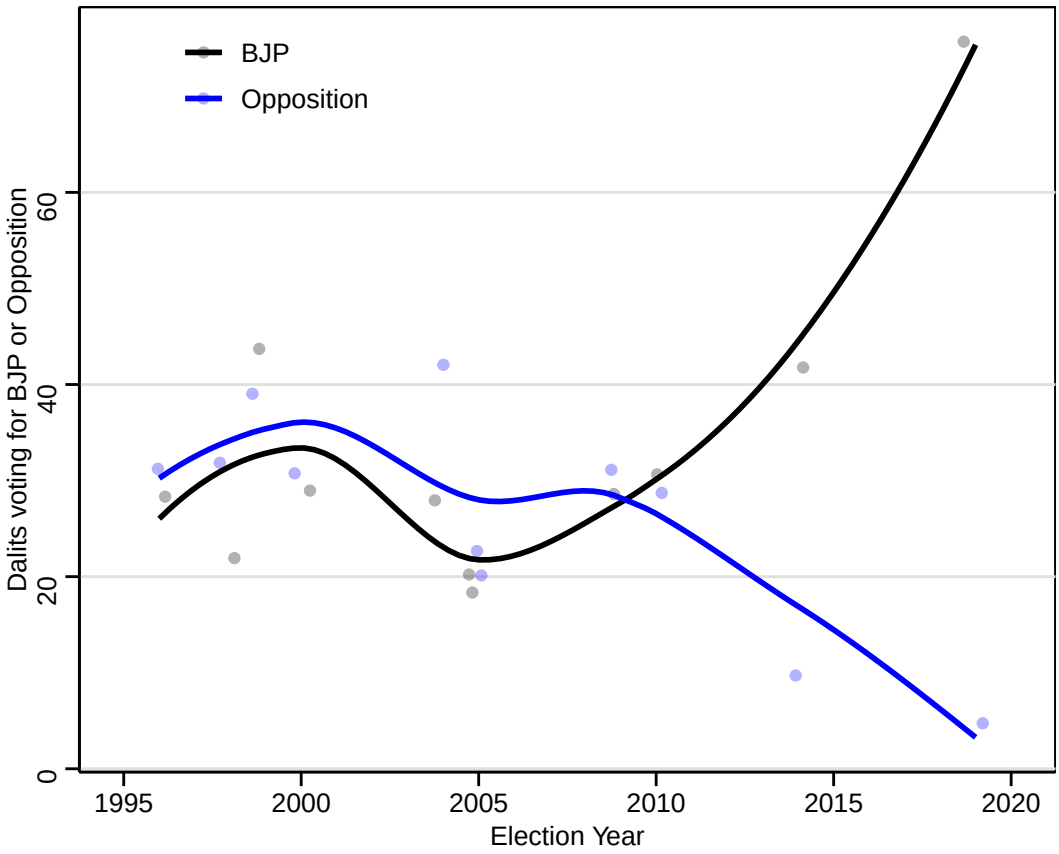
Figure A3.1: Location of Field Sites



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/.

Appendix B: Dalit Vote in Bihar

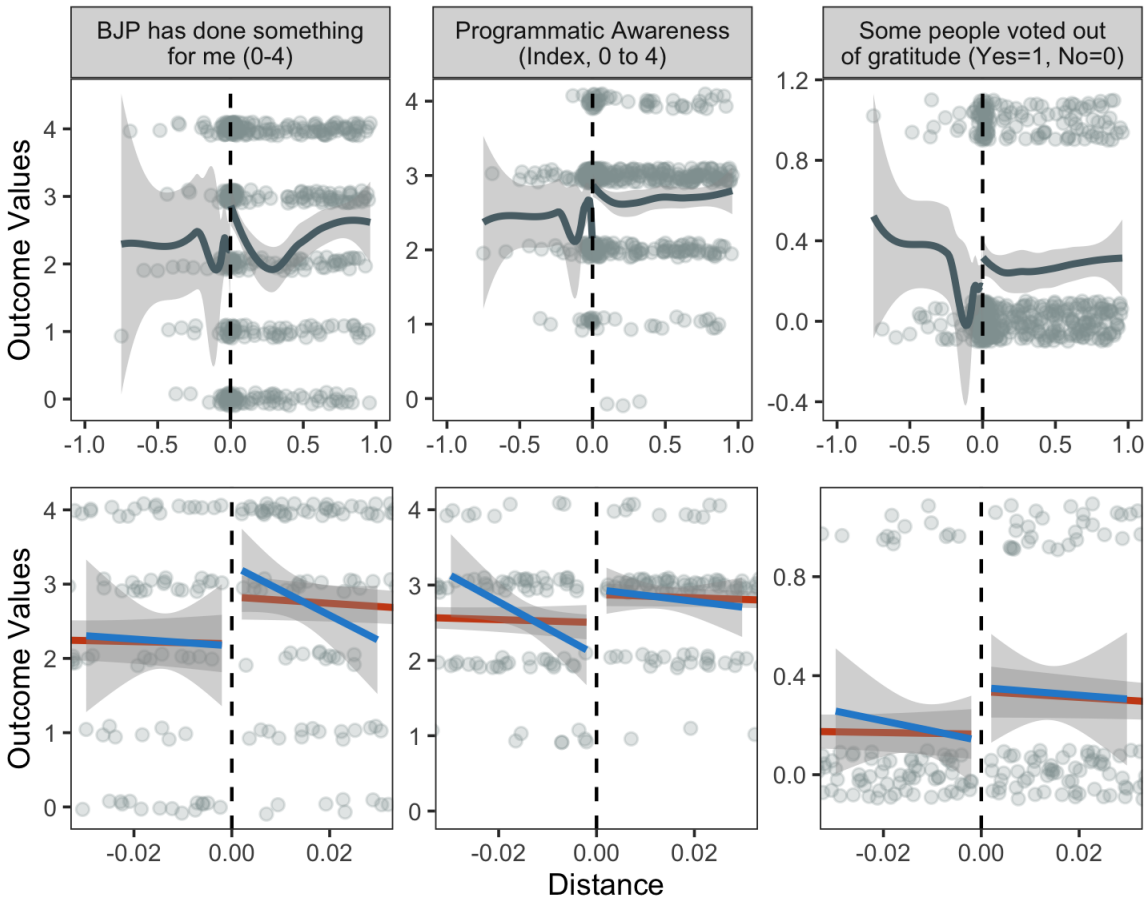
Figure A3.2: Dalit Vote in Bihar (1995-2019)



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).

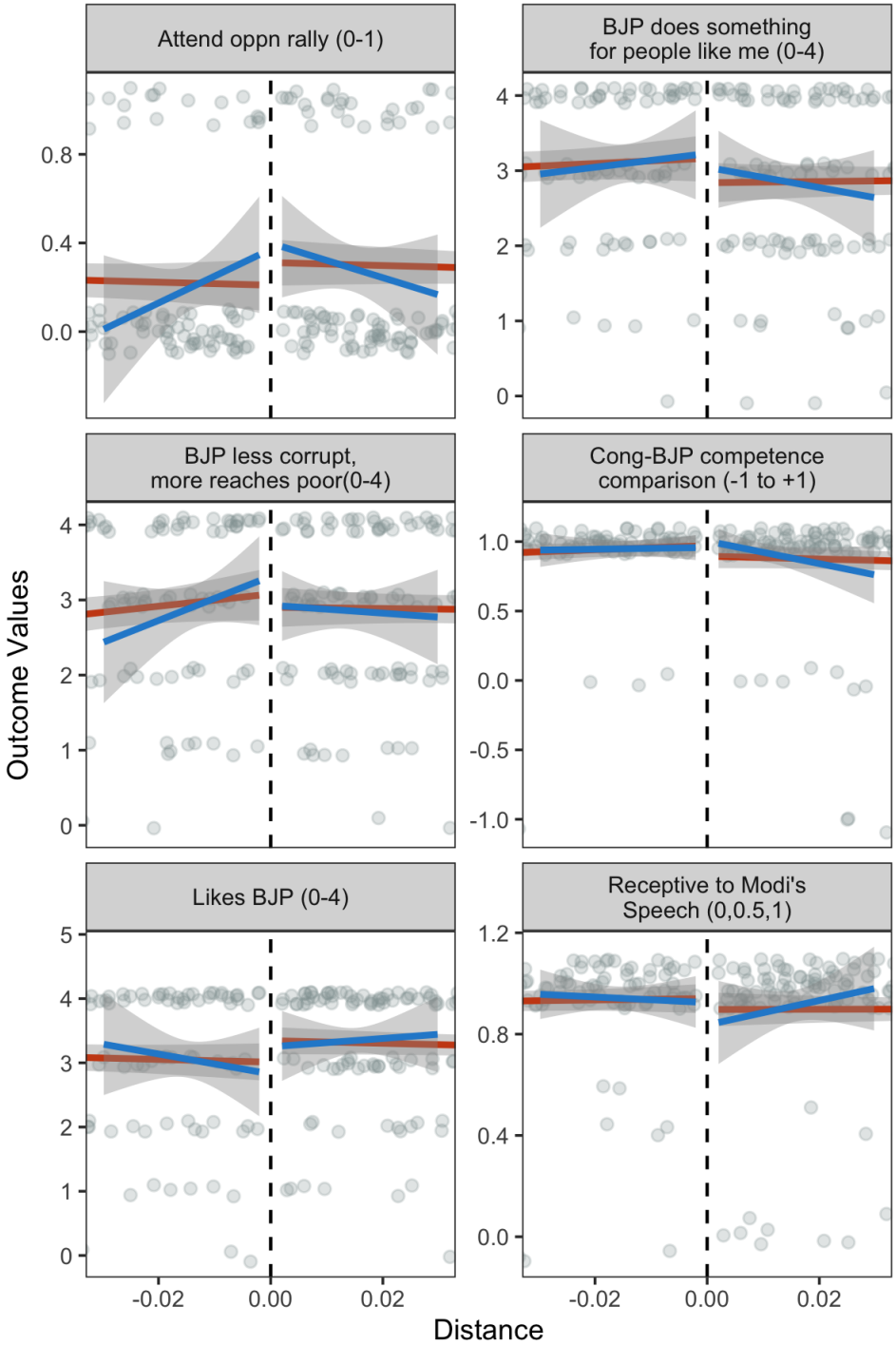
Appendix C: Regression Discontinuity Plots

Figure A3.3: Primary Outcomes



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

Figure A3.4: Secondary Outcomes



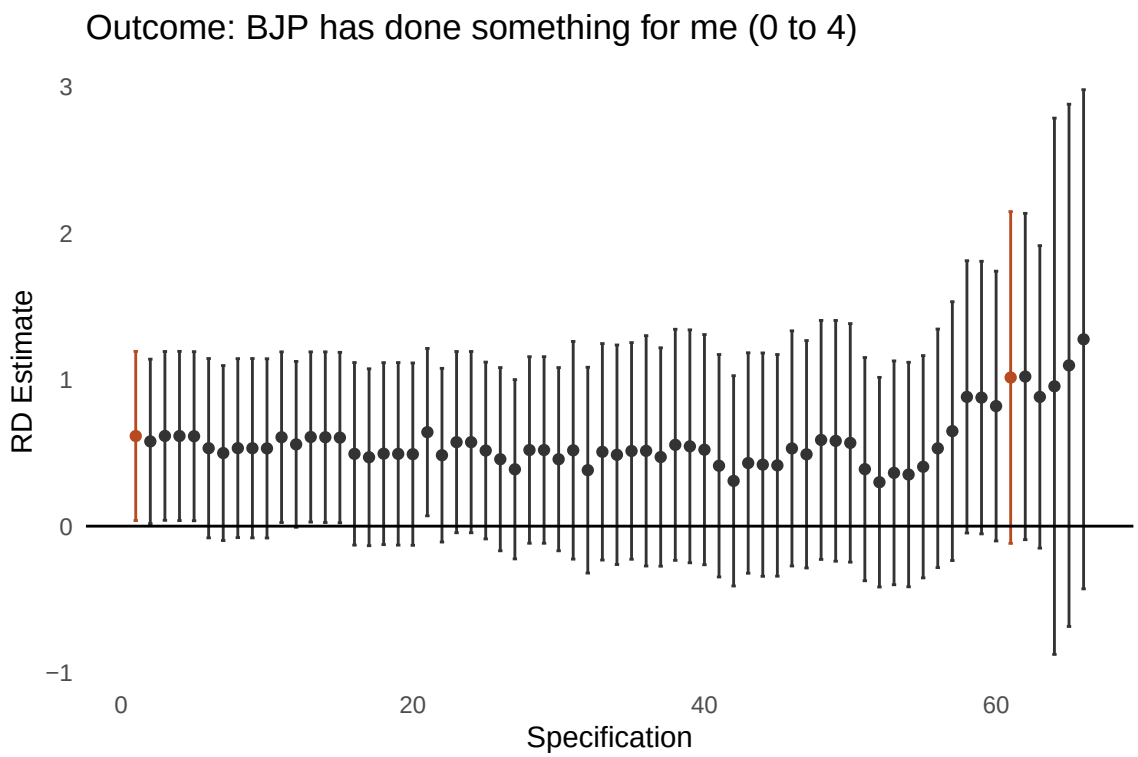
The figure shows outcomes at different values of the forcing variable. We zoom-in on data around the cut-point (±3%), and show estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear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

Appendix 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`).

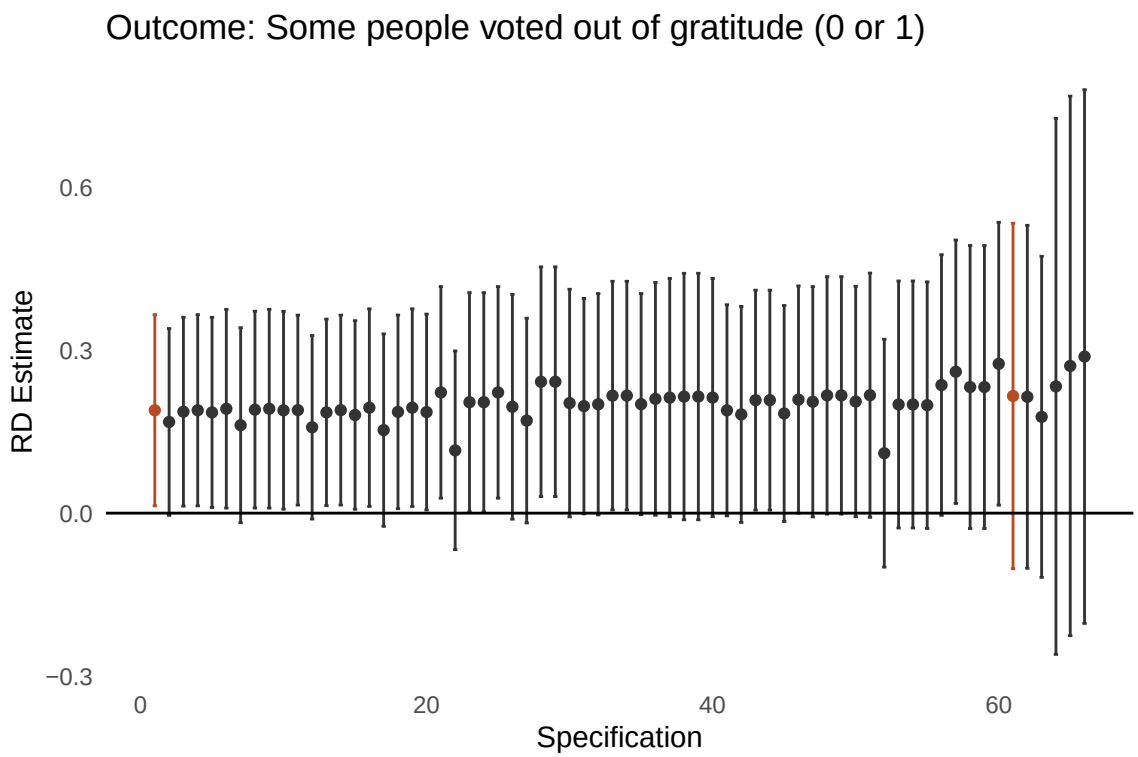
The following bandwidth selection procedures are used: manually selected and pre-registered bandwidth of 3%, one common MSE-optimal bandwidth selector (`mserd`), two different MSE-optimal bandwidth selectors (`msetwo`), one common MSE-optimal bandwidth selector for the sum of regression estimates (`msesum`),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

Figure A3.5: Specification Curve I



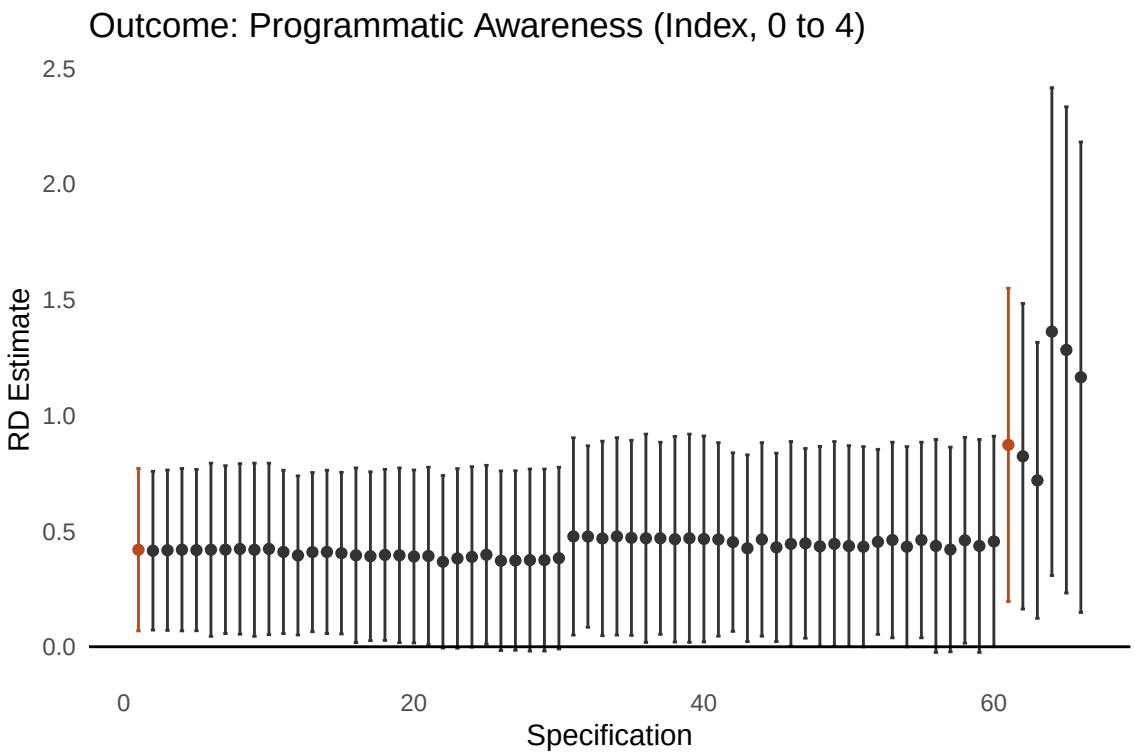
The figure reports the difference at the cut-point and 95% confidence intervals produced by `rdrobust` under different specifications, with the pre-registered specification in red.

Figure A3.6: Specification Curve II



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 A3.7: Specification Curve III



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.

Appendix E: Programmatic Awareness

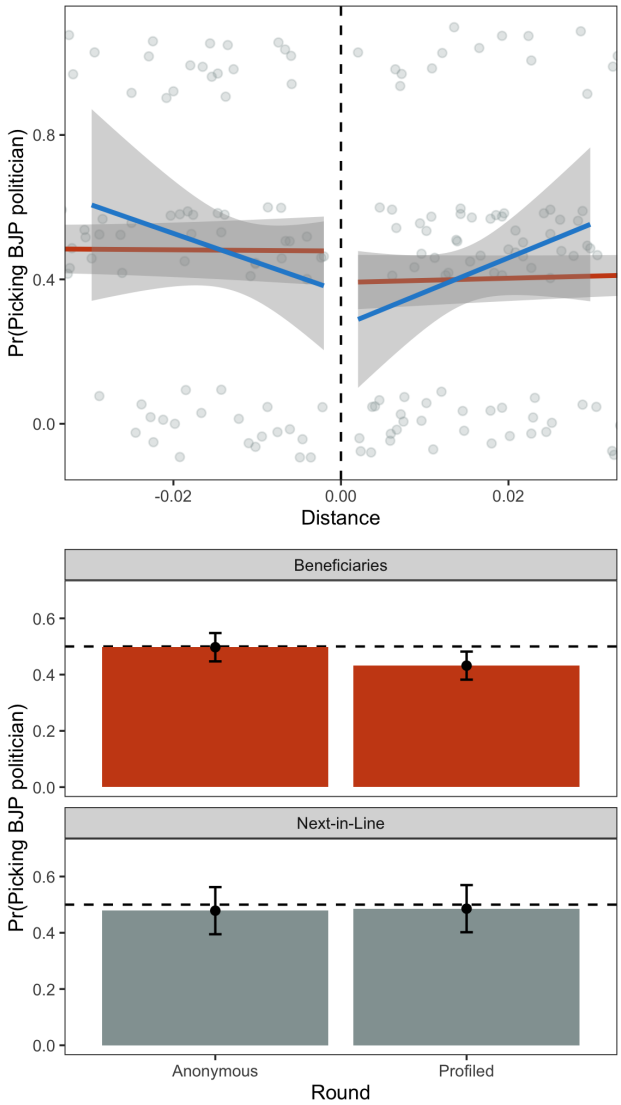
Table A3.1: Programmatic Awareness (Index and Components)

		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 3.4.

Appendix F: Distributive Salience of Ethnicity

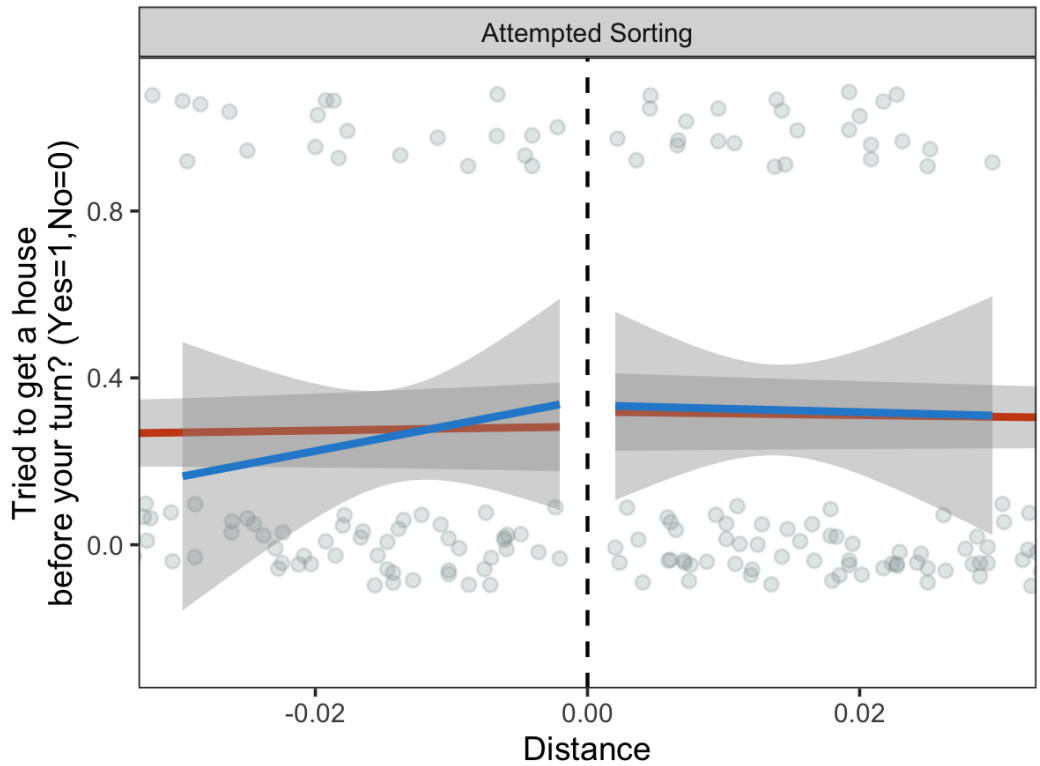
Figure A3.8: Choose Your Dictator Game



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

Appendix G: Attempts at Sorting

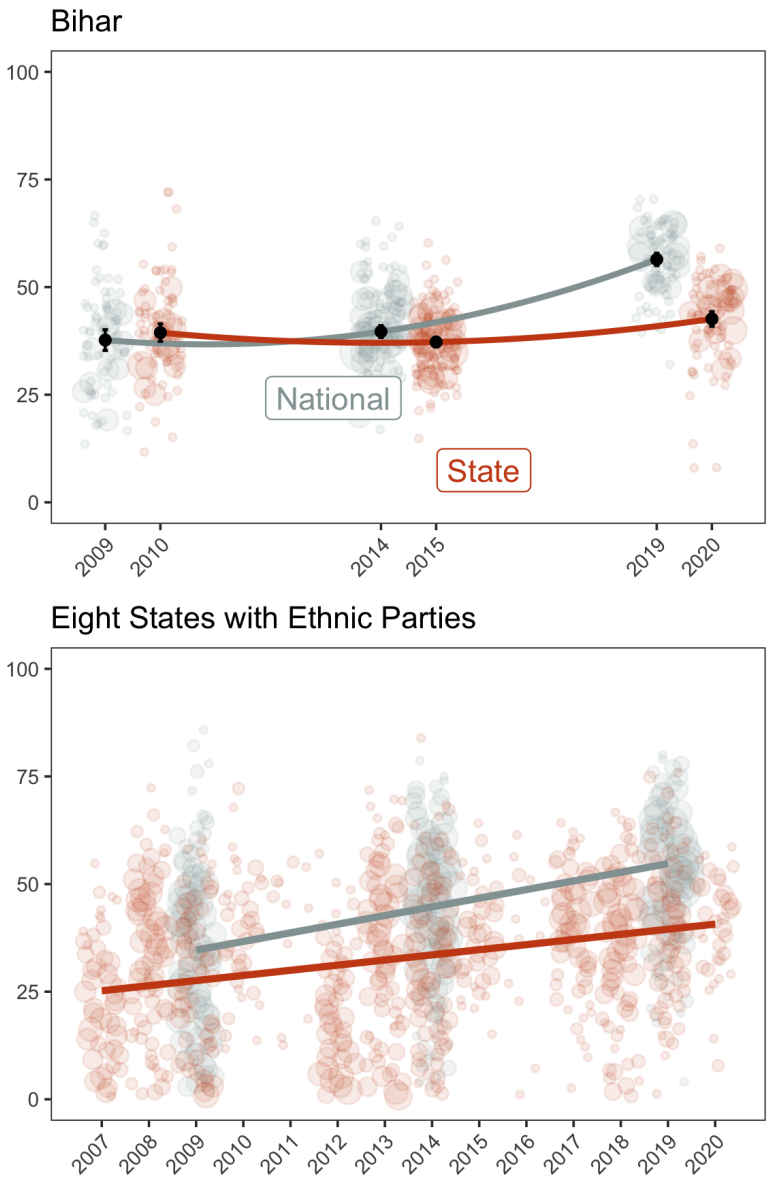
Figure A3.9: Attempts at Sorting



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

Appendix H: Reputational Spillovers

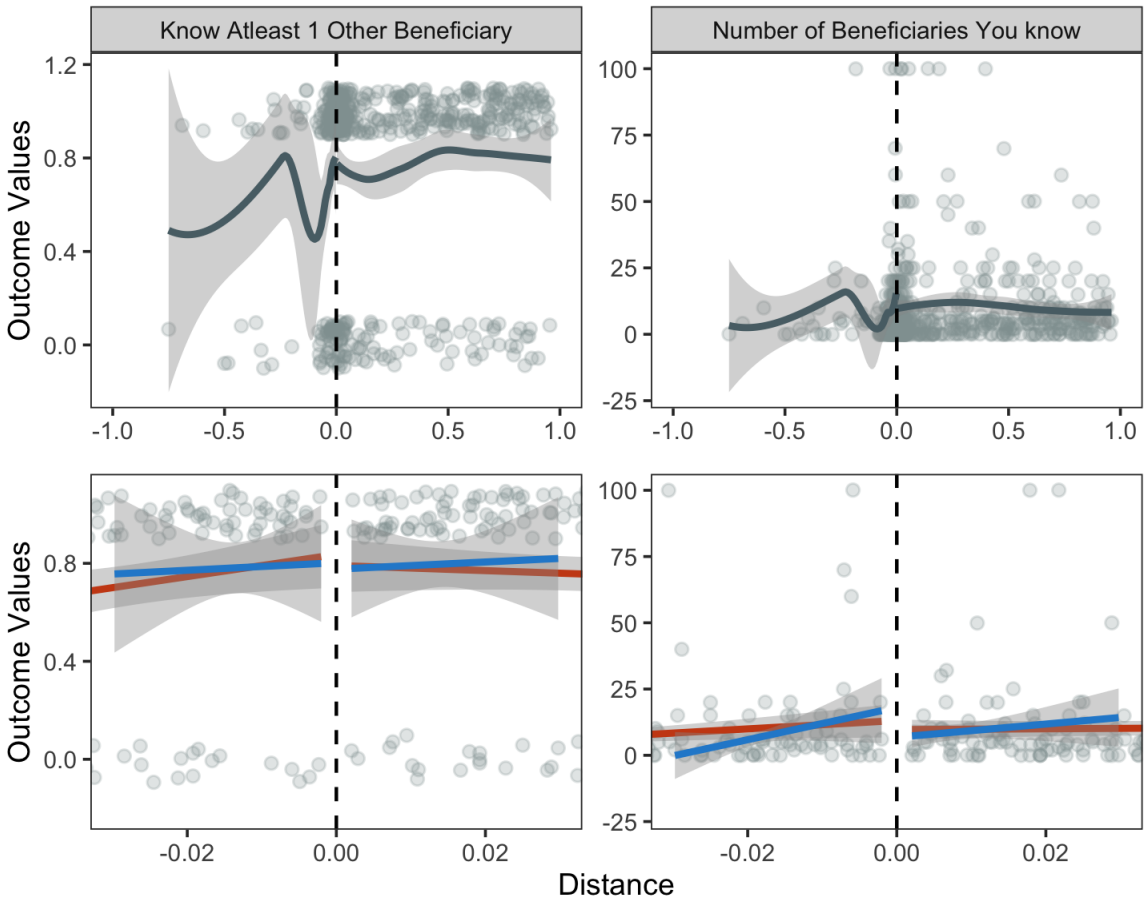
Figure A3.10: BJP Performance (National versus Local)



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 3.1). 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).

Appendix I: Exposure to the Scheme

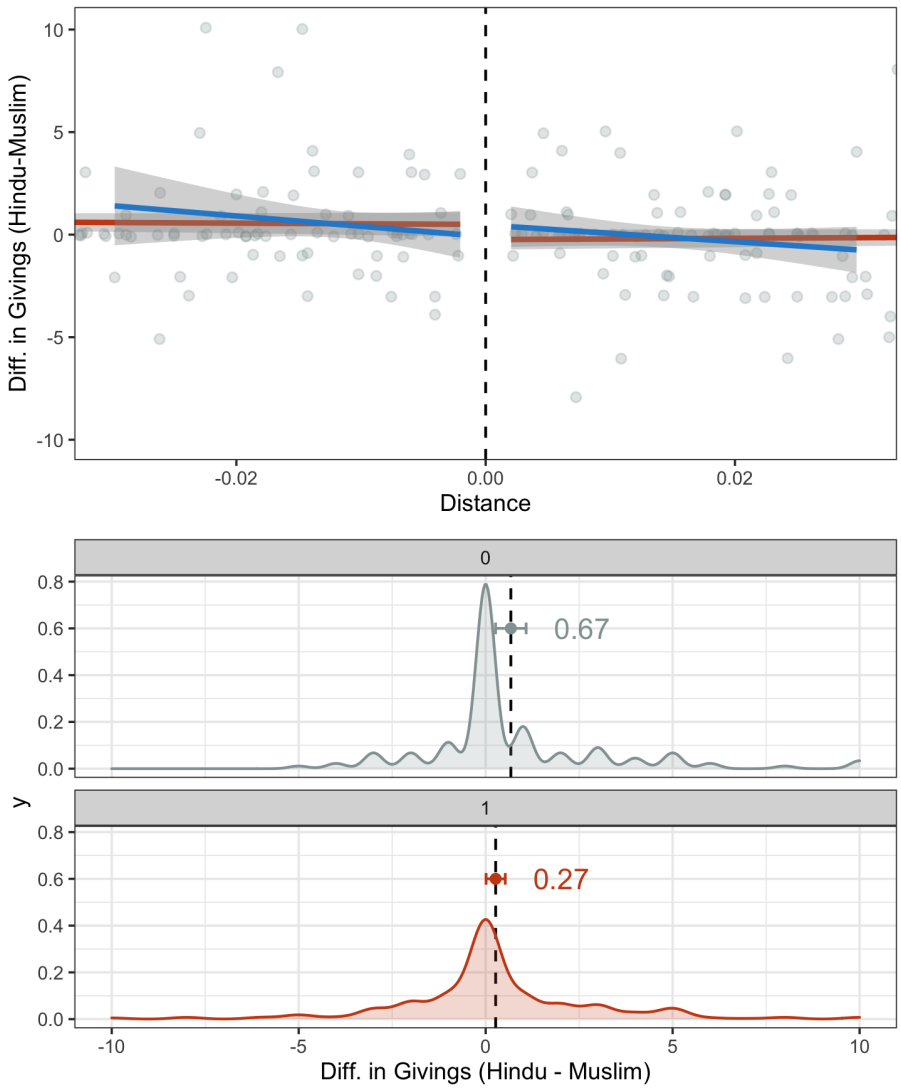
Figure A3.11: Exposure to the Scheme



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

Appendix J: Prejudice against Muslims

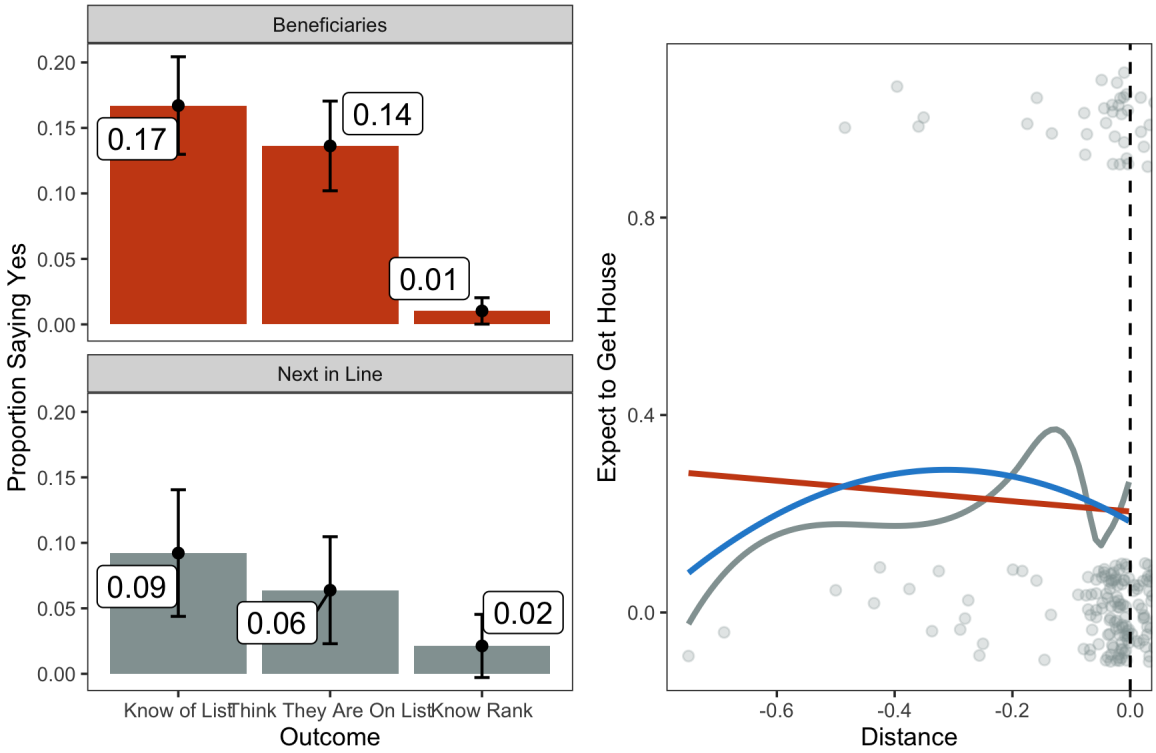
Figure A3.12: Prejudice Against Muslims



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

Appendix K: Anticipation Effects

Figure A3.13: Anticipation Effects



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.

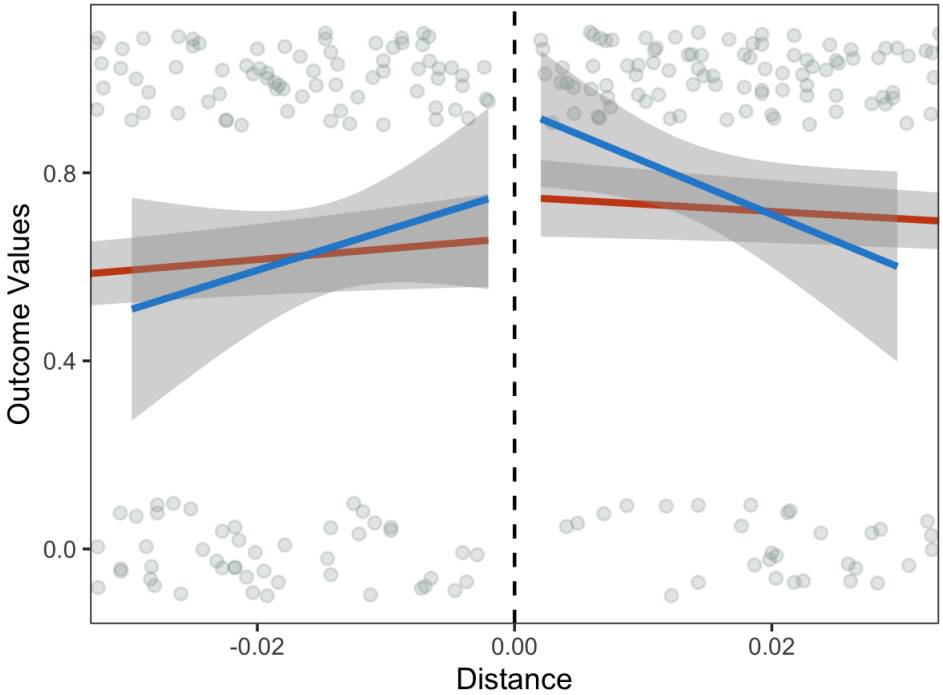
Appendix L: Contact Rates

Table A3.2: 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	No Diff	0.086	0.084	0.304	469	0.176	0.158	0.267	220

Note: See note to Table 3.4.

Figure A3.14: Contact Rates



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 (±3%), and shows estimates at the cut-point using two pre-registered specifications: a linear specification ($p = 1$) using an MSE-optimal bandwidth and triangular kernel (in orange); and a linear specification ($p = 1$) using the pre-registered, manually selected bandwidth (±3%) and triangular kernel (in blue). 95% confidence intervals constructed using heteroskedasticity-robust standard errors are depicted in gray.

Do Voters Reward Programmatic Distribution? Evidence from Survey Experiments in India

Governments in developing countries spend a considerable amount of money distributing material benefits to their citizens. Some of these benefits are distributed through brokers, others as rule-based, non-contingent, direct transfers. Governments are less likely to adopt programmatic distribution if voters do not prioritize efficient implementation, namely less leakage and more accurate targeting. Since rule-based, non-contingent, direct transfers can end up benefiting out-partisans and ethnic out-groups, supporters of the ruling party should not punish their party for benefiting non-supporters. To assess whether voter behavior incentivizes programmatic distribution, I conduct two pre-registered studies in India: an online survey experiment and a telephone-based survey experiment fielded in 12 languages. Indian voters reward good distributive performance, but are more focused on outcomes than efficient implementation. They place a modest premium on distributive efficiency. Strikingly, ruling party supporters do not punish their party for benefiting ethnic out-groups. These findings suggest there are strong incentives for politicians to deliver benefits, though not entirely as rule-based, non-contingent direct transfers.

Introduction

India's government, like so many governments in developing countries, distributes a wide variety of benefits to their citizens. It operates nearly 300 schemes that benefit 950 million people and in spending terms account for 3% of the gross domestic product in the last year (Economist 2022*a*). Some of these benefits are distributed through brokers, who exercise discretion and make receipt of the benefit contingent on political support. Others are distributed through rule-based, direct transfers that reduce discretion of brokers and are not contingent on political support. In multiethnic societies, discretion and patronage typically lead to ethnic favoritism in the distribution of benefits (Chandra 2004). However, rule-based, non-contingent, direct transfers end up benefiting people outside a party's ethnic core. Programmatic distribution of this kind is also more efficient, in terms of leakages and needs-based targeting of benefits, compared to the corruption and mistargeting associated with clientelism (Stokes et al. 2013; Putnam 1993; Chubb 1982). But do voters reward efficient implementation, creating incentives for politicians to distribute through rule-based, non-contingent, direct transfers? This paper focuses on how voter behavior can incentivize politicians to engage in programmatic distribution, building on work that looks at why some politicians opt for clientelism and others programmatic distribution (Wilkinson 2007; Levitsky 2007; Magaloni, Diaz-Cayeros, and Estvez 2007; Weitz-Shaprio 2012; Stokes et al. 2013; Weghorst and Lindberg 2013; Mares and Young 2019).

Voters can shape distributive policy because office-seeking politicians have a strong incentive to focus on issues that resonate with their supporters and swing voters, and are likely to win their votes. How voters, particularly core supporters of a party, react to different components of a distributive policy is strategically important and actionable information for politicians that can shape future choices. It can create electoral incentives for the politician or party to pursue some distributive strategies

instead of others. This paper contends that programmatic distribution (i.e. rule-based, non-contingent, direct transfers) is electorally viable when four conditions are jointly satisfied: (1) voters recognize and reward good distributive policies and performance, and punish bad distributive outcomes[1]; (2) voters reward programmatic efficiency, specifically less discretion, arbitrariness, and leakages; (3) core supporters of the ruling party do not punish distributive policies that end up benefiting people outside the party's ethnic core; and (4) voters outside the party's ethnic core reward the ruling party for channeling resources or opportunities to their group.

I evaluate these conditions for cross-ethnic, programmatic distribution in India, where the ruling party has experimented with rule-based, non-contingent, direct transfers that disproportionately benefit people outside its ethnic core. This paper assesses the impact of performance information on evaluations of the incumbent and donations to the ruling party using two pre-registered studies: an online survey experiment ($n = 1,047$) fielded primarily on core supporters of the ruling party, and a more nationally representative telephone-based survey experiment ($n = 5,350$) conducted in 12 different languages.

These experiments show that voters recognize good performance and reward distribution of benefits but are more focused on outcomes than efficient implementation. This generates strong incentives for parties to deliver benefits, though not entirely as ruled-based, non-contingent direct transfers. Specifically, I find that participants in the study evaluate the ruling party much more favorably when they read or listen to positively framed performance information (government programs benefited many people), and less favorably when they read or listen to negative performance information (growing income inequality, historic unemployment, rising prices). The effect size ranges between 8.5% to 11% of the total scale. Similarly, donations to the ruling

[1]This is not always the case. For example, ethnic or partisan motivated reasoning can skew how voters evaluate politicians' performance (Adida et al. 2017; Carlson 2016; Bolsen, Druckman, and Cook 2014) and whether they selectively blame or credit politicians for outcomes (Graham and Singh 2022).

party increase by 25 to 50 paisa (quarter to half a rupee) when respondents learn that government programs benefited many people. This effect is approximately 2% to 4.5% of the total budget available to respondents. In India's 2019 parliamentary election, political parties received donations worth $910 million.[2] Figure 4.1 and Appendix J show that India's national parties actively seek and compete for small donations from voters. Even a 2% reallocation of donations in favor of the ruling party because of its distributive policies amounts to approximately $18.2 million, equivalent to the legally prescribed campaign budget for 166 parliamentary candidates.

Figure 4.1: India's Ruling Party Seeks Donations

Note: An example of publicity material used by India's ruling party to elicit donations from voters. Appendix J illustrates this point further with a Tweet from the prime minister encouraging donations to the ruling party, and donation web pages of the two national parties.

Even as voters recognize and reward distributive performance, they seem to place a modest premium on efficiency. Study participants who are told about specific steps taken by the government to reduce fake claim-making, corruption and discretion in the distribution of benefits evaluate the ruling party's performance as slightly better than those who are told government programs benefited many people. The additional reward for efficiency is 0.17 scale units in the telephone survey and 0.3 scale units in

[2]For more, see the Association for Democratic Reform's report, *Analysis of Funds Collected and Expenditure Incurred by Political Parties during Lok Sabha Elections, 2019*, available here. Conversions from Indian Rupee to US Dollar rely on the Internal Revenue Service's Yearly Average Exchange Rate for 2019 provided here.

the online survey. This is approximately 1.5% to 3% of the total performance scale. When it comes to donations to the ruling party, respondents who are told about the steps taken to reduce fake claims, corruption and discretion donate 0.26 rupees more to the ruling party in the telephone survey, and 0.46 rupees more in the online survey. This efficiency reward is roughly 2% to 4% of the total budget available for donations.

Neither study supports the claim that supporters of the ruling party punish it for distributing benefits to people outside the ethnic core. This suggests that political parties are not constrained by ethnic considerations of their core supporters. In neither study do voters outside the ruling party's ethnic core *additionally* reward it for channeling benefits to their ethnic group. This finding points to the difficulty in weakening ethnic priors through programmatic distribution, particularly among ethnically opposed voters.

The paper improves our understanding of the politics of development in several ways. It proposes and experimentally evaluates specific types of voter behavior (demand-side conditions) that generate electoral incentives for politicians to pursue programmatic distribution (i.e. activate supply-side conditions). I provide an individual-level explanation or micro-foundation for when distributive politics transitions from clientelistic to programmatic mode, which links up with prevailing accounts that focus on structural factors like socioeconomic development and party competition (Wilkinson 2007; Levitsky 2007; Magaloni, Diaz-Cayeros, and Estvez 2007; Weitz-Shaprio 2012; Stokes et al. 2013; Mares and Young 2019). The paper also helps understand why voters reward targeted, non-discretionary program spending in some cases (Manacorda, Miguel, and Vigorito 2011; Labonne 2013; Zucco Jr. 2013; Larreguy, Marshall, and Trucco 2018) but not others (Kadt and Lieberman 2017; Imai, King, and Rivera 2020).

Ethnicity and partisanship are known to shape how voters perceive distributive performance (Bolsen, Druckman, and Cook 2014; Kahan 2016; Carlson 2016; Adida

et al. 2017). Here, I evaluate that relationship using different types of performance information, varying outcomes, who benefits from distributive policies, and how efficiently benefits reach intended recipients. The findings place limits on but also point to the stubborn persistence of politically or ethnically motivated reasoning.

These findings also reveal that the material value of a benefit does not really affect how voters perceive distributive performance. This has implications for the policy choices that politicians have to make with a finite budget, and how researchers model voters' utility and political preferences.

In the following sections of this paper, I develop a set of hypotheses drawing on the existing literature, describe the research design, present the findings, and explore their implications.

Theory and Hypotheses

Are voters receptive to performance information?

When voters in democracies recognize and reward good performance, politicians have an incentive to deliver benefits for their constituents and produce better policies. This becomes particularly important when the distribution of benefits is not contingent on political support, and politicians cannot monitor voters and enforce transactions. In other words, programmatic politics is electorally viable only when voters' evaluations of politicians are responsive to performance information, particularly on distributive issues. Specifically, we expect voters to reward an incumbent when there are good outcomes or policies, and punish them when there are bad outcomes or failed policies. Yet, as we know, political preferences depend on a variety of considerations, the incumbent's performance only one amongst them. Several factors can erode democratic

accountability, chief among them *politically motivated reasoning*[3]. As Kahan (2016) explains, this is "identity protective" behavior or "the formation of beliefs that maintain a person's status in affinity group united by shared values" (Kahan 2016:3)[4]. In several contexts, ranging from Uganda to the United States, scholars have argued that partisan identity skews how voters evaluate performance information, particularly politically inconvenient information (Carlson 2016; Bolsen, Druckman, and Cook 2014). Voters selectively blame or credit politicians for outcomes, "disproportionately crediting their party for positive developments and blaming opponents for negative developments" (Graham and Singh 2022). In multiethnic democracies, ethnic identity can play a similar role, shaping how voters process performance information. As Adida et al. (2017) find in Benin, voters "reward good-performing incumbents only if they are coethnics, and punish bad performers only if they are noncoethnics".

This literature motivates the first hypothesis of the study that evaluates whether the ground conditions for programmatic politics exist in the Indian case. Specifically, I evaluate whether Indian voters are receptive to performance information.

H_1: Voters will donate more money to the ruling party and rate its performance as better when they are exposed to positively framed performance information, compared to when they are exposed to negatively framed performance information.

Crossethnic distribution

A feature of programmatic distribution is that opposition supporters or ethnically opposed voters are not excluded from private benefits or public goods. This means that sections of the population that are eligible for assistance but outside the ruling

[3]For a broader discussion of directional motivated reasoning, see Kunda (1990)'s seminal work on this.

[4]See also Taber and Lodge (2006).

party's ethnic core can also receive material benefits.[5] In fact, political parties may deliberately appeal to voters outside their ethnic core and target material benefits at them whether to expand their base, diversify sources of support, or because institutions require them to do so (Arriola 2013; Thachil 2014; Adida et al. 2016; Arriola et al. 2020; Gadjanova 2021). What is less clear is how voters respond to crossethnic distribution, namely a program that intentionally or accidentally benefits a large number of people outside the party's ethnic core.

In the instrumentalist account, ethnic voting is the result of ethnic favoritism in the distribution of material benefits and opportunities (Chandra 2004; Posner 2005; Burgess et al. 2015; Kramon and Posner 2016; McClendon 2016; Ejdemyr, Kramon, and Robinson 2018). Ethnicity is a readily available, costless signal of politicians' distributive intent in low information environments. But performance information and who ends up benefiting from distributive policies can weaken the salience of ethnicity (Conroy-Krutz 2013; Ichino and Nathan 2013). More specifically, when core supporters of a party learn that its distributive policies considerably benefit people outside the party's ethnic core, they are less likely to think that this party solely champions their material interests and well-being. In other words, information on crossethnic distribution might lower support for a ruling party among its core supporters. Conversely, people outside the party's ethnic core (henceforth non-supporters) might be more likely to think this party will do something for them, and hence more supportive of it when they encounter information on crossethnic distribution.

However, a social identity based account of ethnic voting can yield different predictions. These theories suggest that group membership shapes cognition, evaluation of information, and emotional responses (Tajfel 1974; Fiske, Cuddy, and Glick 2007; Taber and Lodge 2006). If so, information on crossethnic distribution can prime eth-

[5]This is possible under clientelism when broker networks are ethnically heterogeneous (Auerbach and Thachil 2018; Auerbach 2020). However, generally speaking, clientelism provides more precise targeting of benefits, whether to co-ethnics or co-partisans, compared to programmatic distribution.

nic identity in the voter's mind or trigger considerations about intergroup competition for resources. Ethnically motivated reasoning might lead core supporters and non-supporters to disregard inconvenient information, and maintain their prior beliefs, attitudes and preferences. In other words, supporters would not punish crossethnic distribution, and non-supporters would not reward it. However, priming resource competition between groups might make core supporters of the party less supportive of it, and non-supporters more supportive of it. In summary, the ethnic politics literature makes contrasting predictions about how voters would respond to crossethnic distribution.

Parties, however, are more likely to engage in programmatic distribution when supporters do not punish it for policies that benefit people outside the ethnic core, and non-supporters reward such policies. I contend that two factors jointly shape the party's distributive choice, with core supporters' behavior acting as the primary constraint. This yields two hypotheses:

H_2: Core supporters will punish the ruling party for diverting resources to other ethnic groups. Specifically, treated subjects (core supporters who are informed that government programs have benefited a large number of people outside the ruling party's ethnic core) will donate less money to the ruling party and be more critical of its performance compared to control subjects (core supporters who are given generic information about government programs).

H_3: Non-supporters will reward the ruling party for diverting resources to their ethnic groups and away from core supporters. Specifically, treated subjects (non-supporters who are informed that government programs have benefited a large number of people outside the ruling party's ethnic core) will donate more money to the ruling party and be less critical of its performance compared to control subjects (non-supporters who are given generic information about government programs).

Furthermore, these effects should be sensitive to cost heuristics. Cost heuristics refer to information about the quantity of resources being channeled outside the ethnic core. In low information environments, there are typically two types of information that are readily available and highly visible: how expensive a material benefit or project is, and how much budgetary resources are allocated to the program. Since public resources are scarce, voters are more likely to reward or punish a party for distributing expensive benefits outside the ethnic core, compared to cheap benefits. This then yields a fourth hypothesis:

H_4: Core supporters will punish the party more for distributing an expensive benefit to people outside the ethnic core compared to when the party distributes a cheap benefit. Non-supporters will reward the party more for distributing an expensive benefit compared to a cheap benefit.

Rewarding efficiency

A distinct advantage of programmatic distribution is that it reduces discretion and leakages. For such distribution to be electorally viable, voters should reward efficient implementation. Every ruling party has the option to engage its brokers and intermediaries in the distributive process. Earlier in the dissertation, I have shown that brokers play a pivotal role in converting latent good will from material benefits into votes. To keep brokers happy, the party needs to generate rents for them or what can be colloquially called "oiling the wheels of the machine". In contrast, rule-based, low discretion, direct transfers can be vote-winners because more of the intended benefit reaches people and others observe this efficient transaction and reward it (what can be thought of as a "bystander effect").

However, it is far from clear if voters reward programmatic efficiency. For example, Manacorda, Miguel, and Vigorito (2011) in Uruguay, Pop-Eleches and Pop-Eleches (2012) in Romania, Labonne (2013) in Philippines, De La O (2013) in Mexico,

Zucco Jr. (2013) in Brazil, and Larreguy, Marshall, and Trucco (2018) in Mexico find that voters reward incumbents for targeted, non-discretionary program spending. Interestingly, Kadt and Lieberman (2017) find a negative relationship between improvements in service provision and support for the incumbent, ostensibly due to voter "concerns about corruption" and "ratcheting [up of] preferences for service delivery". However, Imai, King, and Rivera (2020) evaluate two programs using large-scale randomized control trials and conclude that "programmatic policies have no measurable effect on voter support for incumbents".

I contend that parties are likely to pursue programmatic distribution when voters, especially core supporters of the party, reward efficient implementation. When this happens, programmatic distribution makes electoral sense: the party can win over new supporters by distributing material benefits to them, and consolidate existing support through reputation building (i.e. claiming to reduce corruption, leakages, and improved last-mile delivery).

On the other hand, if core supporters do not reward or prioritize efficiency, clientelism makes more sense as a distributive strategy. The party is better off engaging brokers because they can maximize the impact of the material benefit through effective credit claiming, and the party can keep them happy by "oiling the machine" using public resources. This discussion then generates the following hypothesis:

H_5: Voters will reward efficiency. Specifically, treated subjects (voters who are given information on program implementation) will donate more money to the ruling party and be less critical of its performance compared to control subjects (voters who are given generic information about government programs).

Once again, I expect the magnitude of effects to vary by cost heuristics. Voters are more likely to reward efficiency in the distribution of expensive benefits with large budgetary allocations, compared to cheap benefits with small budgetary outlays.

H_6: Voters are more likely to reward the party for programmatic distribution when the benefit is expensive and has a large budgetary allocation compared to when the benefit is cheap and has a small budgetary allocation.

Finally, from the ruling party's perspective we can see that expensive benefits are higher cost – higher reward propositions. Core supporters are more likely to punish the party for distributing an expensive benefit to people outside the ethnic core but non-supporters are more likely to reward the party for such distribution. The reputational gains from programmatic distribution are also larger for expensive benefits. In contrast, cheap benefits are lower cost – lower reward propositions: core supporters are less likely to punish their distribution among non-supporters, non-supporters are less likely to reward the party for channeling these benefits to them, and the electorate is less likely to reward efficiency in the distribution of such benefits.

Putting this together, programmatic distribution becomes more likely when the rewards from efficiency and crossethnic distribution significantly exceed the punishment for distributing benefits outside the ethnic core. It should be noted that the party faces this trade-off only among core supporters, who likely punish the diversion of resources to other ethnic groups. Consequently, the decision hinges on whether core supporters *net reward* the party for programmatic distribution that might end up benefiting people outside the ethnic core. This, then, yields a final prediction:

H_7: Among core supporters of a party, the reward for programmatic efficiency significantly exceeds the punishment for distributing benefits outside the ethnic core.

Research Design

Data

To evaluate these hypotheses, I fielded an online (n =1,047) and telephone-based (n =5,350) survey experiment in India. These studies were approved by Yale's IRB[6], pre-registered with the Open Science Foundation, and have a publicly available pre-analysis plan (see Appendix).[7] The online survey experiment was fielded on Lucid between April 20 and 27, 2022. Online survey takers tend to be younger, educated, middle class, more urban, and from high status groups (see Table A4.1). Conventionally, these characteristics are positively associated with support for the ruling party, the BJP. Indeed 74% of online respondents report voting for the BJP in the prior parliamentary election. In effect, an online survey is a good way to study core supporters of the BJP.

I followed a rigorous, multi-step quality control process for the online survey. The survey was open to only those located in India and 18 years or older. For representativeness, I placed census-based quotas on gender, age, and region based on screening questions available on Lucid Marketplace. To deter bots and reduce low quality, inattentive respondents, I included a Captcha verification at the start of the survey, turned on Qualtrics' security feature that prevents multiple submissions from the same IP address and blocks search engines from including the survey in their search results, and included a pre-randomization attention check question that screened-out respondents who failed to answer correctly. 1,870 people entered the survey from Lucid, and 1,064 (or 56.9%) crossed these quality control measures. I dropped another 17 observations because they were either multiple submissions from

[6]The study was deemed exempt by Yale's Institutional Review Board (Protocol Number 2000032215, Determination Date: February 8, 2022).

[7]The pre-analysis plan is available here: https://osf.io/3hr5p.

the same IP address (6 observations) or left the survey more than once while answering pre-treatment questions prior to randomization (11 observations). These conditions were specified in the pre-analysis plan, prior to data collection.

The telephone-based survey experiment was fielded by CVoter, a reputed Indian polling firm that has collected data for several published academic studies. CVoter added my treatment vignettes and questions to their periodic omnibus surveys that use random digit dialing and computer assisted telephone interviewing (CATI). The study was conducted in over 25 Indian states in 12 different languages between April 18 and May 5, 2022 and yielded a more nationally representative sample (see Table A4.1). For example, 69% of online survey respondents reported living in a city (population exceeding 500,000) while 71% of respondents in the telephone survey reported living in a village. Similarly, 90% of online survey takers were highly educated, compared to 20% in the telephone survey. 81% of online respondents reported middle class incomes (households earning more than 20,000 rupees a month)[8] compared to just 14% in the telephone sample. Politically, 91% of online respondents reported voting in the last parliamentary election, compared to 76% in the telephone sample. 74% of online respondents supported the BJP in that election, compared to 57% of telephone respondents. There is also considerable underrepresentation of marginalized social groups in the online sample (7% Scheduled Castes (SCs), 1% Scheduled Tribes (STs), 15% religious minorities). The telephone sample is more representative: 15% SCs, 5% STs, and 19% religious minorities.

The pre-analysis plan describes in greater detail the data collection procedures, sample size rationale based on power calculations from a pilot study, and stopping rule.

[8]The Center for Monitoring Indian Economy (CMIE) classifies households as middle class if their annual income is greater than or equal to 200,000 rupees. This roughly translates to a monthly household income of 17,000 rupees or more. The surveys use a coarsened measure in which a monthly household income of 20,000 to 50,000 rupees is coded as 5. All respondents coded as 5 or higher are considered middle class.

Experiment Design

The survey experiment randomly assigns respondents to read or listen to information about the ruling party's performance. The treatment vignettes are identical in the online and telephone surveys. Figure 4.2 shows the survey design. Respondents are randomly assigned, with equal probability, to one of seven experimental conditions.

In the *baseline* condition, respondents are given negatively framed performance information about the ruling party. They are told that "India's economy is not doing well" and that "the income of 84% of households declined [last year] but rich people became richer". Furthermore, "unemployment is the highest in 30 years" and "prices are also rising, with 1 liter of petrol costing close to 100 rupees" (which is a historic high). The Appendix provides the exact text of the treatments.

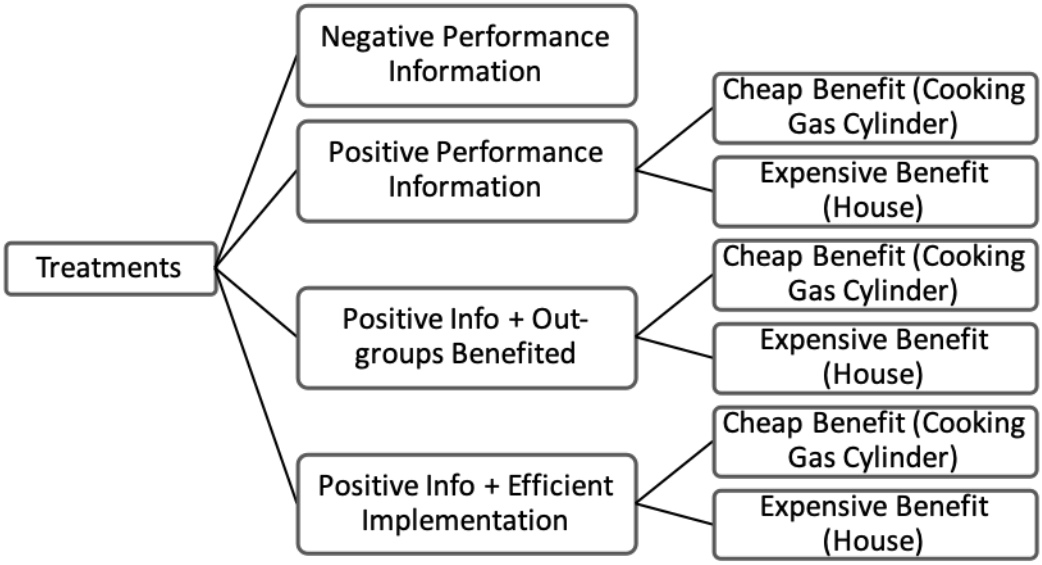
In the first treatment condition, respondents are given *generic* positively framed performance information. Among respondents randomly assigned to this condition, I further randomize whether the information is about a government program that distributes a cheap benefit ($10 cooking gas cylinder) or an expensive benefit ($2000 house). The *generic* condition only mentions the number of people that have benefited from each government program.

A second treatment condition, *out groups benefited*, includes the generic, positively framed performance information from above but also informs respondents that government programs have benefited a large number of people outside the ruling party's ethnic core, namely Muslims, Scheduled Castes (SCs) and Scheduled Tribes (STs). Once again, I further randomize whether this information is about a cheap or expensive benefit.

Finally, in the *efficiency* condition, respondents get the generic, positively framed performance information and details about the specific steps taken by the government to reduce fake claim-making (biometric authentication of beneficiaries),

corruption and discretion (direct transfer of money to bank accounts). Again, respondents can be randomly assigned to receive such information about the cooking gas cylinder scheme or the housing program. The exact text of these information vignettes is provided in the Appendix.

Figure 4.2: Experiment Design



Note: Information vignettes in the survey experiments. Subjects are randomly assigned, with equal probability, to one of seven experimental conditions.

Immediately after reading or hearing the information vignette, respondents answer two outcome questions: an attitudinal measure of satisfaction with the ruling party's performance (0 to 10 scale), and a behavioral measure that captures how respondents split 10 rupees between major political parties, where I am interested in the amount donated to the ruling party, BJP (0 to 10 scale). For the donation question, the online survey offered three options: BJP (national party), Congress (national party), and Other. The telephone survey measured this outcome slightly differently. Respondents were provided the option of two main national parties (BJP and Congress), and major state parties in their state. Depending on the state, there could either be no state party option, one major state party option, or two major

state party options. There was also an option "will not donate to any party" that was not read out by the enumerator but available to respondents should they express a desire to not donate at all. Since I am interested in the amount donated to the ruling party, BJP, I code respondents as 0 if they either donate all the money to other parties, or refuse to donate money to any party. The online survey also contained an open-ended question on "how [participants] decide who to vote for in national elections?". A research assistant blind to the study's hypotheses and respondent's treatment assignment status coded these responses as 1 if they only mention performance, developmental work, and distributive policies, 0.5 if they mention these and other considerations, and 0 if they only mention other factors. The exact text of these outcome questions is provided in the Appendix.

Figures A4.2 and A4.3 in the Appendix report the results of a pre-registered randomization check. In the online and telephone survey experiment, covariates are not jointly predictive of treatment assignment. Table A4.13 in the Appendix checks whether missingness in any outcome is predicted by treatment assignment. There is no missingness in outcomes in the online survey, and only 2% missingness in the attitudinal outcome in the telephone survey. Treatment assignment does not predict missingness in that outcome.

Heterogeneity

Some of the hypotheses developed in the previous section distinguish between *core supporters* of the ruling party and *non supporters*. I use ethnicity as the primary measure for identifying core supporters. Respondents that identify as Hindu General/ Upper Caste, Hindu OBC and Jain are coded as "core supporters", while those from all other ethnic categories (Hindu SC, Hindu ST, Muslim, Sikh, Christian, Parsi, and Buddhist) are coded as "non-supporters". For robustness, I pre-registered an alternative, multidimensional measure that considers social status and economic affluence

(see the Appendix for more details).

Estimation

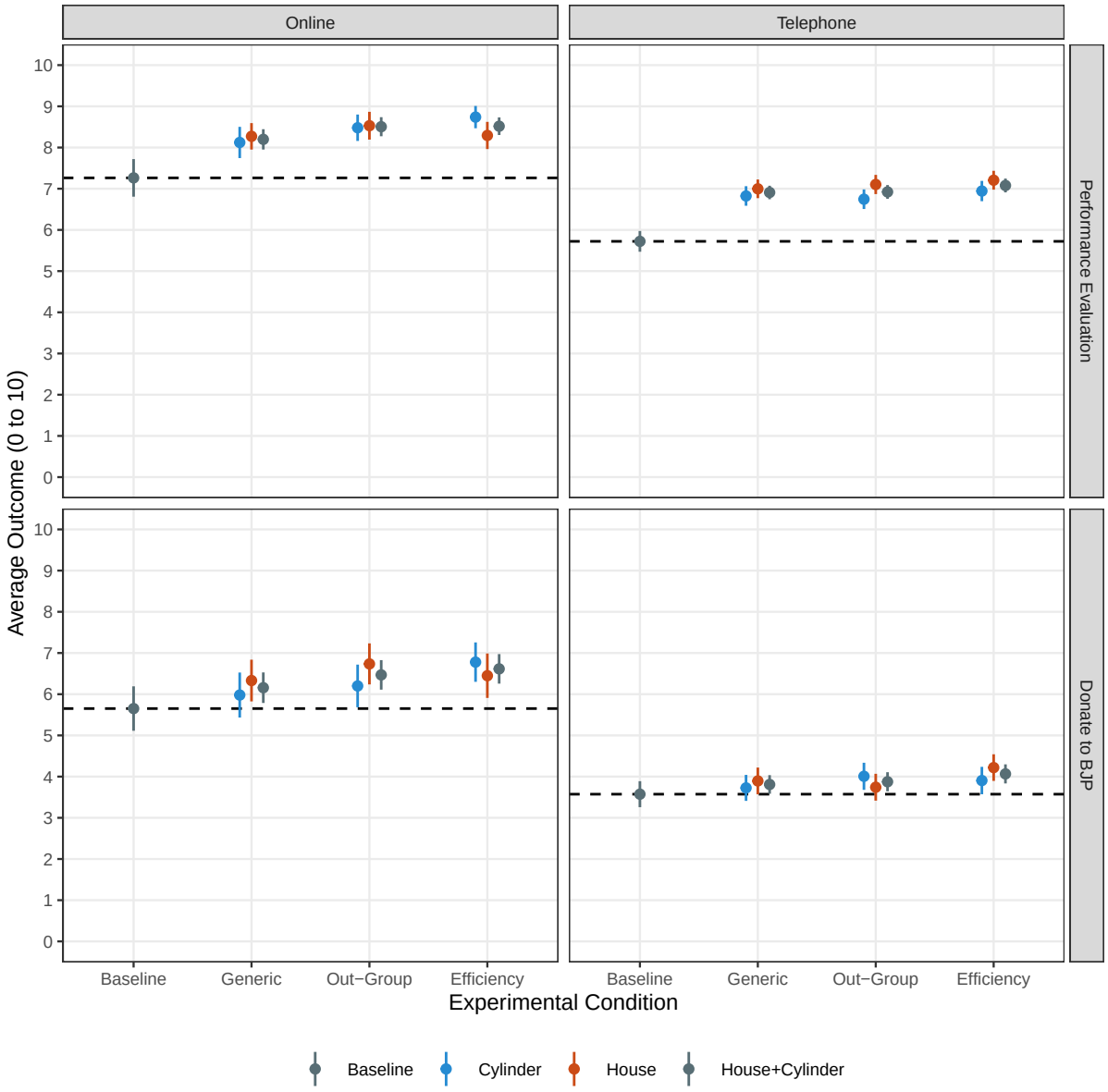
I evaluate the hypotheses by comparing different experimental conditions and estimating difference-in-means. Table A4.15 describes the test for each hypothesis, specifically the experimental groups being compared. I use ordinary least squares (OLS) regressions with heteroskedasticity-robust (HC2) standard errors. Column 3 of Table A4.15 provides the regression specification and parameters of interest for each hypothesis. I reject the null hypothesis of no effect if the two-tailed p value is less than 0.05.

Findings

The survey experiments find that voters are responsive to performance information (H_1), supporters of party do not punish it for distributing benefits to people outside the ethnic core (H_2), non-supporters recognize good performance but do not additionally reward the ruling party for channeling benefits to them (H_3), and there is some evidence of a very modest reward for efficient distribution (H_5). The magnitude of these effects is not larger for the expensive benefit relative to the cheap benefit (H_4 and H_6). Taken together, there are strong incentives for politicians to perform and deliver benefits to voters but not necessarily through rule-based, non-contingent, direct transfers that reduce broker discretion.

Figure 4.3 shows the average performance evaluation (in the top panel) and money donated to the BJP (in the bottom panel) for each experimental condition. For the treatment conditions, I show the average outcome for respondents randomly assigned to read or hear information about the housing program in orange, cooking gas cylinder scheme in blue, and the combined estimate in gray. Focusing on the at-

Figure 4.3: Group Mean Estimates for Different Experimental Conditions



Note: The figure shows the average outcome along with 95% confidence intervals in different experimental conditions. The top panel reports this information for the attitudinal measure (performance evaluation, 0 to 10 scale). The bottom panel reports this information for the behavioral measure (money donated to the BJP, 0 to 10 scale). For the three treatment groups (*generic, out-groups benefited* and *efficiency*), the combined estimate is shown in gray, the estimate for when respondents read/hear information about the housing program (expensive benefit) in orange and the cooking gas cylinder scheme (cheap benefit) in blue. The data plotted in this figure is reported in Table A4.2.

titudinal measure (top panel), I find that respondents who were exposed to negative performance information (*baseline* condition) rate the government 7.26 (s.e.= 0.23) out of 10 in the online survey, and 5.72 (s.e.= 0.128) out of 10 in the telephone survey. In the bottom panel, I find that respondents who were exposed to negative performance information, on average, donate 5.65 out of 10 rupees (s.e.= 0.273) to the BJP in the online survey, and 3.57 (s.e.= 0.16) out of 10 rupees in the telephone survey. These baseline figures suggest that the incumbent is pretty popular, even when voters are primed to think about growing income inequality, historic unemployment, and high inflation. Interestingly, online survey respondents in the baseline condition evaluated the ruling party's performance as 1.5 scale units higher than telephone respondents in the same condition, and donated approximately 2 rupees more than telephone respondents in that condition. This expressive support for the ruling party is likely due to the demographic composition of the online sample: younger, more educated, more affluent and urban people also tend to be more supportive of the BJP. Furthermore, 44% of respondents in the telephone survey refused to donate money to any political party. These respondents oppose the ruling party, and are sufficiently dissatisfied with all other opposition parties so as to not donate money to any of them. I can reasonably infer this from observable covariate data on voting intention in the last parliamentary elections. Table A4.14 in the Appendix shows the proportion of respondents that voted for the ruling party in the prior parliamentary election separately for those that donated money to any party, and those that refused to donate money to any party. In every experimental group, respondents that did not donate to any party consistently supported the ruling party at lower rates than those that donated money to any party. In the baseline condition, for example, 28% (s.e.= 3.7) of people that did not donate money to any party in the experiment claimed to vote for the ruling party in the prior parliamentary election. In contrast, 62% (s.e.= 2.4) of those that donated money to any party claimed to vote for the

ruling party in the prior election.

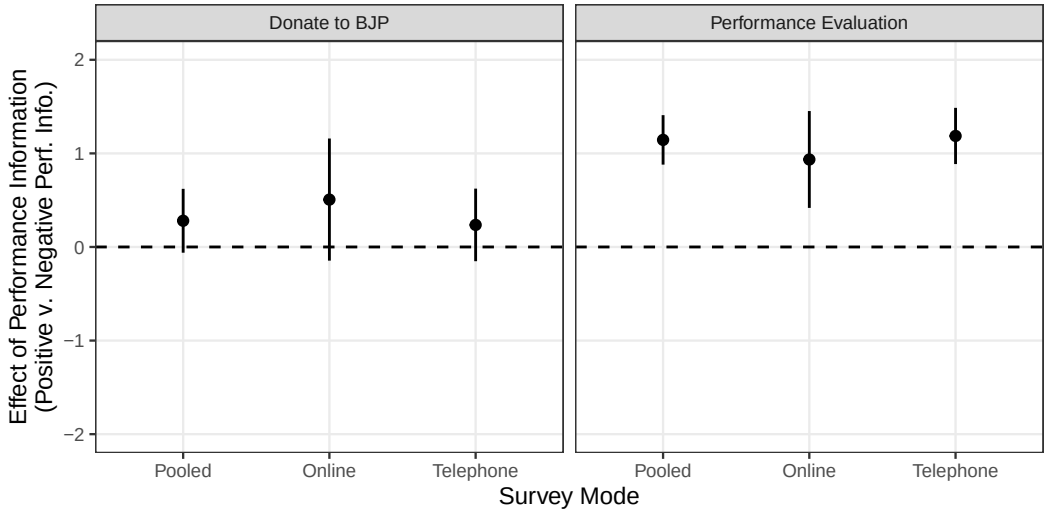
In both experiments there is a large, statistically and substantively significant improvement in performance evaluations and increase in donations to the BJP when respondents read or listen to *any* information about distributive programs. In the top panel, I find that evaluations of the ruling party's performance improve by over 1 scale point, equal to about 9% of the 0 to 10 scale, when respondents read or listen to any type of positively framed performance information. This is the case whether the information is about a government program that distributes a cheap benefit or an expensive benefit. In the bottom panel, I show that donations to the BJP increase by about 0.5 rupees or roughly 4.5% of the total budget when respondents read or listen to any type of positively framed performance information. Once again, this is the case whether the information is about a cheap or expensive benefit. In the remainder of this section, I compare different experimental groups to evaluate the hypotheses described earlier in the paper.

Voters are receptive to performance information

Do Indian voters recognize and reward good performance? These experiments show that voters donate more money to the ruling party and rate its performance as better when they are exposed to positively framed performance information, compared to when they are exposed to negatively framed performance information. For this analysis, I compare the *baseline* and *generic information* conditions. Figure 4.4 plots the difference in means estimate and 95% confidence interval for each outcome in the two survey experiments.

On the right, I show that the average performance evaluation improves by 1.14 scale units (s.e.= 0.135, $p < 0.001$) in the pooled data, 1.186 scale units (s.e.= 0.153, $p < 0.001$) in the telephone survey, and 0.935 scale units (s.e.= 0.263, $p < 0.001$) in the online survey when respondents learn about the number of people that have

Figure 4.4: Voter Evaluations and Behavior: Positive v. Negative Performance Information



Note: The figure shows difference in means estimates ($\overline{Y_{\text{Generic}}} - \overline{Y_{\text{Baseline}}}$) along with 95% confidence intervals. Hypothesis 1 predicts that these point estimates are positive and statistically distinguishable from 0. The data plotted in this figure is reported in Table A4.3.

benefited from the housing program or cooking gas cylinder scheme. The effect size is pretty similar in both surveys, and is between 8.5% to 11% of the total scale. These are large, substantively meaningful changes in voter beliefs.

On the left, I report that donations to the ruling party increase by 25 to 50 paisa (quarter to half a rupee) when respondents learn about how many people benefited from these government programs. In the pooled data, the difference in means estimate or $\widehat{\beta} = 0.28$ (s.e.= 0.174, $p = 0.108$). In the telephone survey, the difference in means estimate or $\widehat{\beta} = 0.24$ (s.e.= 0.198, $p = 0.23$). The effect on donations to the ruling party is even larger if we compare baseline to efficiency ($\widehat{\beta} = 0.49$, s.e.= 0.199, $p = 0.01$), and baseline to out-groups benefited ($\widehat{\beta} = 0.30$, s.e.= 0.199, $p = 0.13$).[9] In the online survey, the treatment effect is larger in magnitude but not statistically significant at the 0.05 level because it is estimated less precisely using a smaller sample ($\widehat{\beta} = 0.506$, s.e.= 0.331, $p = 0.127$). Once again, the effect on donations is

[9]These comparisons are not pre-registered.

even larger if we compare baseline to efficiency ($\widehat{\beta} = 0.96$, s.e.= 0.328, $p = 0.003$), and baseline to out-groups benefited ($\widehat{\beta} = 0.82$, s.e.= 0.328, $p = 0.01$). Across specifications and survey modes, the behavioral effect is smaller than the attitudinal shift, and approximately 2% to 4.5% of the total budget available to respondents in pre-registered specifications.

These are substantively meaningful changes in voters' behavior. In India's 2019 parliamentary election, political parties received donations worth $910 million, and as Figure 4.1 and Appendix J show, national parties actively seek and compete for small donations. Even a 2% reallocation of donations in favor of the ruling party because of its distributive policy successes amounts to approximately $18.2 million. This is equivalent to the legally prescribed campaign budget for 166 parliamentary candidates.

In summary, there is pretty consistent evidence that voters reward political parties for good performance and distributive outcomes. This creates an incentive for parties to deliver benefits, though not necessarily as ruled-based, non-contingent direct transfers. To probe this point further, I now turn to two other conditions that are necessary for programmatic politics to be electorally viable.

No punishment or reward for crossethnic distribution

Do supporters of the ruling party punish it for distributing benefits to people outside the party's ethnic core? And do people outside the ethnic core reward this party for channeling benefits to them? These experiments indicate that core supporters do not punish crossethnic distribution, and non-supporters do not reward it. For this analysis, I compare the *generic* and *out-groups benefited* conditions. The latter provides additional information on who benefits from the government program. For example, the *italicized* text in the treatment vignette below is the additional information included in the out-groups benefited condition:

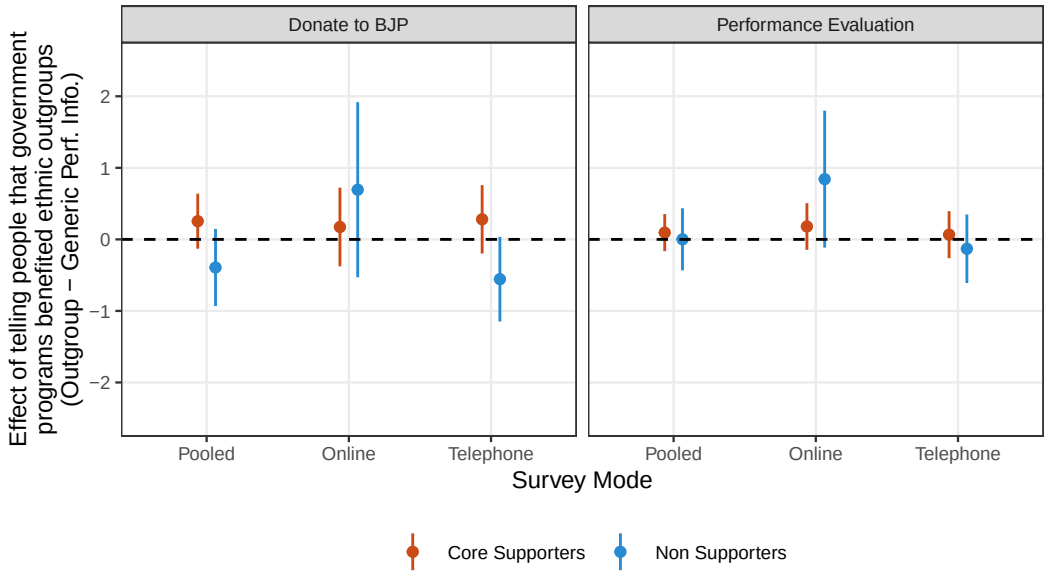
> The Modi government has distributed nearly 8 crore gas cylinders *to poor families. A large number of people who have got a cylinder are Muslims, Dalits (SCs), Adivasis (STs). Nearly 3 crore people from the minority community have got a cylinder. Over 3 crore Dalits and Adivasis have got a cylinder.*[10]

Figure 4.5 shows the impact of such information on voters' performance evaluation and donations to the ruling party. Core supporters do not punish crossethnic distribution. When it comes to performance evaluations, the difference in means estimate ($\widehat{\beta}$) is in the wrong direction and statistically insignificant (pooled data: $\widehat{\beta} = 0.095$, s.e.= 0.132, $p = 0.72$; telephone survey: $\widehat{\beta} = 0.066$, s.e.= 0.168, $p = 0.69$; online survey: $\widehat{\beta} = 0.18$, s.e.= 0.166, $p = 0.278$). Similarly for the behavioral outcome, the difference in means estimate is positive and insignificant (pooled data: $\widehat{\beta} = 0.254$, s.e.= 0.196, $p = 0.195$; telephone survey: $\widehat{\beta} = 0.28$, s.e.= 0.24, $p = 0.25$; online survey: $\widehat{\beta} = 0.17$, s.e.= 0.28, $p = 0.536$).

Voters outside the ruling party's ethnic core recognize good performance (see previous section) but do not additionally reward the party for channeling benefits to their ethnic group. There is some evidence of rewarding in the online survey experiment. Non-supporters that read about coethnics substantially benefiting from government programs evaluate the ruling party's performance as better ($\widehat{\beta} = 0.84$, s.e.= 0.48, $p = 0.08$) and donate more money to it ($\widehat{\beta} = 0.69$, s.e.= 0.62, $p = 0.26$). Even though these treatment effects are in the right direction, they are estimated using a small sample ($n = 128$) and are statistically indistinguishable from 0. In the telephone survey, which has a larger sample of non-supporters, the treatment effect estimates are in the wrong direction and statistically insignificant. For example, non-supporters who learn about government programs benefiting Muslims, Dalits and Tribals evaluate the ruling party's performance as 0.13 scale units worse (s.e.= 0.244,

[10] Crore is an Indian unit of measurement. 1 crore equals 10 million.

Figure 4.5: Punishment or Reward for Distributing Outside the Ethnic Core



Note: The figure shows difference in means estimates ($\overline{Y_{\text{Out-Groups}}} - \overline{Y_{\text{Generic}}}$) along with 95% confidence intervals for core supporters (in orange) and non-supporters (in blue). Hypothesis 2 predicts that the point estimates in orange are negative and statistically distinguishable from 0. Hypothesis 3 predicts that the point estimates in blue are positive and statistically distinguishable from 0. The data plotted in this figure is reported in Tables A4.4 and A4.5.

$p = 0.59$), and donate 55 paisa less to it (s.e.= 0.30, $p = 0.07$). Estimates from the pooled data paint a very similar picture. When non-supporters read or hear that government programs benefit Muslims, Dalits and Tribals, they evaluate the ruling party's performance in the same way ($\widehat{\beta} = 0.001$, s.e.= 0.221, $p = 0.996$) and donate about the same to the ruling party ($\widehat{\beta} = -0.393$, s.e.= 0.275, $p = 0.153$). There is no evidence of additional rewarding.

These findings are robust to an alternative, pre-registered measure of core supporters that factors social status and economic affluence. If anything, the point estimates move closer to zero for both sub-groups. Table A4.10 in the Appendix reports the difference in means estimates using this alternative measure. Figure A4.1 in the Appendix visualizes these results along the lines of Figure 4.5 above.

Contrary to the initial expectation, punishment and reward are not greater for the expensive benefit. When comparing a cheap and expensive benefit, supporters of

the ruling party do not punish their party more strongly for distributing an expensive benefit to people outside the ethnic core. The interaction term in Table A4.6 (columns 1 and 2) is statistically insignificant. Similarly, non supporters do not reward the ruling party more for distributing an expensive benefit to their ethnic group (see columns 3 and 4 in Table A4.6).

These results present a mixed picture for programmatic politics. Parties are more likely to adopt rule-based, non-contingent, direct transfers if their supporters do not punish the fact that such distributive policies can end up benefiting ethnically opposed groups. At the same time, parties are less incentivized to pursue such distribution if ethnically opposed voters do not reward it.

Modest reward for efficient implementation

Do voters sufficiently value distributive efficiency and reward it in elections? These experiments indicate that there is a very modest reward for efficiency. The analysis compares the *generic* and *efficiency* conditions. The latter describes specific steps taken by the government to reduce fake claim-making (biometric authentication of beneficiaries), corruption and discretion (direct transfer of money to the beneficiary's bank account). For example, the *italicized* text in the treatment vignette below is the additional information included in the efficiency condition:

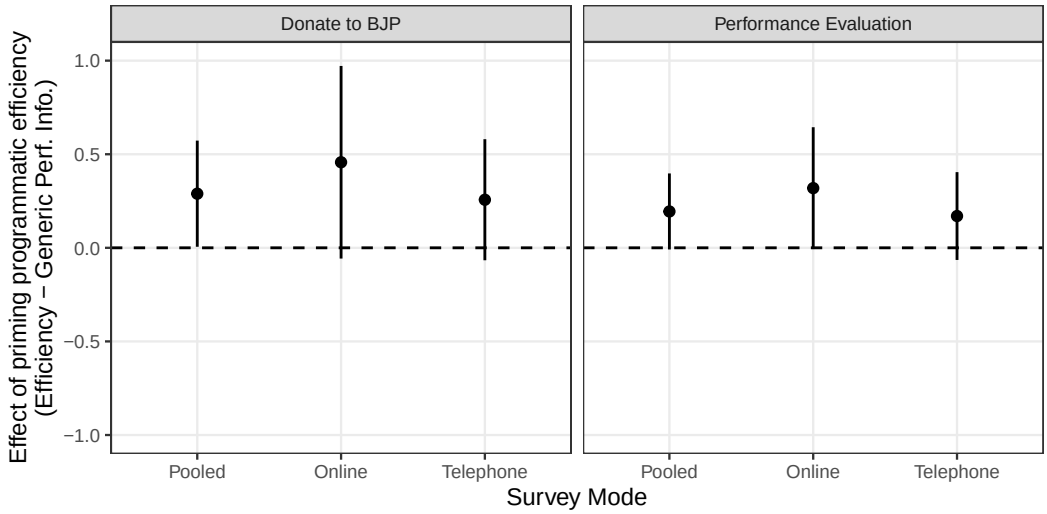
> The Modi government has distributed nearly 1 crore 70 lakh houses. *Before giving someone a house, the government verifies people's identity using their fingerprints (Aadhar card).*[11] *This ensures that the same person does not get a house more than once. It also stops some people from using a fake identity to get a house. To reduce corruption, the government directly transfers money for the house into people's bank account. This*

[11]This is a biometric identification issued by India's government to nearly all its residents, and used by it to identify beneficiaries and directly transfer benefits from social welfare programs.

money is given in installments, after the person proves that they are using the money to build a house by showing geotagged photographs.

The additional information consistently has a positive impact on voters' performance evaluation and donations to the ruling party (see Figure 4.6). In both surveys, the point estimate is positive, though statistically insignificant at the 5% level, indicating that there may be a small reward for distributive efficiency. In the pooled data, respondents who read or hear about efficient implementation of programs rate the ruling party's performance as 0.19 scale units (s.e.= 0.104, $p = 0.061$) better, and donate 0.29 rupees or 29 paisa (s.e.= 0.145, $p = 0.046$) more to the ruling party. In the telephone survey, respondents who hear about the steps taken by the government to reduce fake claim-making, corruption, and discretion rate the ruling party's performance as 0.17 scale units (s.e.= 0.12, $p = 0.155$) better, and donate approximately 0.26 rupees or 26 paisa (s.e.= 0.165, $p = 0.11$) more to the ruling party. These are not very large effects, approximately 1.5% of the total performance scale and 2% of the budget.

Figure 4.6: Rewards for Distributive Efficiency



Note: The figure shows difference in means estimates ($\overline{Y_{\text{Efficiency}}} - \overline{Y_{\text{Generic}}}$) along with 95% confidence intervals. Hypothesis 5 predicts that the point estimates are positive and statistically distinguishable from 0. The data plotted in this figure is reported in Table A4.7.

In the online survey, I find a larger effect. Respondents who read about the steps taken by the government to reduce fake claim-making, corruption and discretion rated the ruling party's performance as 0.319 (s.e.= 0.166, $p = 0.054$) scale units better, and donated nearly 45.7 paisa (s.e.= 0.262, $p = 0.08$) more to the ruling party. These effects are statistically significant at the 10% level. They are also substantively more meaningful: approximately 3% of the performance evaluation scale, and 4% of the total donation budget.

Do voters reward efficiency more when an expensive benefit is distributed? Table A4.8 in the Appendix shows that the interaction term, $\text{House}_i \times \text{Efficient}_i$, is inconsistently estimated and statistically insignificant. In other words, there is no evidence to suggest rewards for efficiency are greater for expensive benefits compared to cheap benefits.

These results show that there is a weak incentive for political parties to pursue direct transfers because voters do not adequately reward distributive efficiency. While voters are aspirational and seek developmental goods and tangible improvements in their living conditions, they are less discerning and concerned about how those benefits reach them. This generates strong incentives for politicians to deliver benefits, though not entirely or primarily as rule-based, non-contingent, direct transfers.

Discussion

A May 2022 article in the *The Economist* identifies four pillars undergirding India's economic transformation, one of them a "high-tech welfare safety-net for the hundreds of millions left behind". The publication notes:

> [T]he fourth pillar [is] digital welfare, with payments for some 300 schemes for needy Indians, from job support to fertiliser subsidies, sent straight to people's bank accounts. This cuts out bureaucrats and allows spending on

> a staggering scale. In the year to March, payments reached $81bn, or 3% of GDP, up from 1% four years earlier. Payments have totalled $270bn since 2017. Roughly 950m people have benefited, at an average of $86 per person per year. That makes a difference to struggling households: India's extreme poverty line is about $250 per person per year at market exchange rates. Mr Modi has not managed to initiate a national jobs boom, but he has created a national safety-net of sorts. (Economist 2022*a*)

While rule-based, non-contingent, direct transfers can fuel India's economic transformation, it is less clear how they will alter its political landscape. The findings in this paper point to a critical piece in any transition to programmatic politics: voter concern for *process* and *efficient implementation*, not just delivery of benefits. Until this happens, politicians will have weak electoral incentives to pursue programmatic distribution even in the presence of conducive structural factors like urbanization, economic affluence and mobility, greater political competition, direct communication with voters, and effective credit claiming. Future research in this area can look at why some voters reward distributive efficiency, and why others don't. Can educational interventions and awareness campaigns durably build public opinion against clientelism and in favor of rule-based, non-contingent, direct transfers? Do perceptual screens like ethnicity and partisanship determine how much importance voters give to distributive processes and efficient implementation? Does formalization and "paying into system" (i.e. through a wide direct taxpayer base) create a sense of stakeholdership necessary for voters to value process and distributive efficiency?

A second strand of research can probe implications for political parties. How do parties allocate finite resources to different kinds of benefits and delivery mechanisms? In a companion paper, I find that an optimal strategy involves distributing cheap benefits through brokers or party intermediaries, and directly transferring expensive benefits. This kind of mixed strategy assumes that parties need to keep brokers

engaged because they need them to disseminate their ideology, claim credit, or convert latent good will from distributive programs into votes. This, of course, leads to the question whether some parties are better placed to mix distributive strategies than others? Do ideology, mobilization structure, or candidate selection procedure constrain or enable mixing? These and many related questions can motivate future research in this area.

Appendix A: Survey Details

Overview

Online Survey

Platform: Lucid

Dates: April 20-27, 2022.

Sample size: 1,047.

Screeners: Captcha Verification, Attention Check.

Consent: Subjects read an IRB-approved consent form, then voluntarily consented to participate in a research study.

Compensation: $1 for completing a five minute survey. As the vendor does not set any standard rate, this amount was chosen to exceed the hourly minimum wage in India and the United States.

Treatments and outcomes: See Appendix B.

No deception: As indicated in the IRB protocol, no deception was involved.

Telephone Survey

Platform: CVoter

Dates: April 18-May 5, 2022.

Sample size: 5,350.

Screeners: None.

Consent: Enumerators read out an IRB-approved consent script and proceeded with the interview only if the subject verbally consented to participate in the study.

Compensation: The treatment vignettes and outcome questions were part of a larger, periodic omnibus survey. CVoter does not compensate respondents for participating in these surveys.

Treatments and outcomes: See Appendix B.

No deception: As indicated in the IRB protocol, no deception was involved.

Sample Characteristics

Table A4.1: Sample Characteristics

	Telephone(n=5350)				Online(n=1047)			
Variable	Mean	Min	Max	Missing	Mean	Min	Max	Missing
Age	39.327	18	90	0	35.203	19	86	0
Female	0.534	0	1	0	0.477	0	1	0
Education (0-7)	3.422	0	7	1366	5.626	0	7	0
Household Income (0-7)	2.994	0	7	1366	5.347	0	7	0
Urbanness (0 to 2)	0.579	0	2	0	1.605	0	2	0
Voted	0.765	0	1	1379	0.907	0	1	3
Voted BJP	0.572	0	1	1379	0.739	0	1	17
General	0.293	0	1	1368	0.625	0	1	19
OBC	0.325	0	1	1368	0.299	0	1	19
Minority	0.186	0	1	1368	0.152	0	1	0
SC	0.151	0	1	1368	0.067	0	1	19
ST	0.046	0	1	1368	0.010	0	1	19

Appendix B: Survey Questionnaire

Treatment Vignettes

Baseline Condition (Negatively Framed Performance Information)

India's economy is not doing well. Last year, the income of 84% of households declined but rich people became richer. The wealth of the 100 richest people in India increased from Rs. 23 lakh crore in March 2020 to Rs. 53 lakh crore in November 2021. The 100 richest people now have as much money as 55 crore poorest Indians. Over 7 out of 100 Indians do not have a job. Unemployment is the highest in 30 years. Prices are also rising, with 1 liter of petrol costing close to 100 rupees.

Generic Condition (Positively Framed Performance Information)

Cheap Benefit: The Modi government has distributed nearly 8 crore gas cylinders.

Expensive Benefit: The Modi government has distributed nearly 1 crore 70 lakh houses.

Out-groups Benefited Condition

Cheap Benefit: The Modi government has distributed nearly 8 crore gas cylinders to poor families. A large number of people who have got a cylinder are Muslims, Dalits (SCs), Adivasis (STs). Nearly 3 crore people from the minority community have got a cylinder. Over 3 crore Dalits and Adivasis have got a cylinder.

Expensive Benefit: The Modi government has distributed nearly 1 crore 70 lakh houses to poor families. A large number of people who have got a house are Muslims, Dalits (SCs) and Adivasis (STs). About 22 lakh Muslims have got a house. Nearly 40 lakh Dalits and 36 lakh Adivasis have got a house.

Efficiency Condition

Cheap Benefit: The Modi government has distributed nearly 8 crore gas cylinders. Before giving someone a cylinder, the government verifies people's identity using their

fingerprints (Aadhar card). This ensures that the same person does not get a free cylinder more than once. It also stops some people from using a fake identity to get a free cylinder. To reduce corruption, the government directly transfers money for refilling the cylinder into people?s bank account.

Expensive Benefit: The Modi government has distributed nearly 1 crore 70 lakh houses. Before giving someone a house, the government verifies people's identity using their fingerprints (Aadhar card). This ensures that the same person does not get a house more than once. It also stops some people from using a fake identity to get a house. To reduce corruption, the government directly transfers money for the house into people?s bank account. This money is given in installments, after the person proves that they are using the money to build a house by showing geotagged photographs.

Outcome Measures

Attitudinal Measure

How do you rate the performance of the Modi government on a scale of 0 to 10, where 0 is very poor and 10 is very good?

Behavioral Measure

If you had to donate 10 rupees to a political party, who would you give the money to: BJP, Congress, or [major state party/parties]? You can give all of the money to one party, or give some of the money to a party and the remaining money to other parties. How much will you give to each of these parties?

Open-ended Measure (Online Survey Only)

How do you decide who to vote for in national elections? In a few words tell us what factors or things matter the most to you when you decide who to support in the Lok Sabha elections.

Appendix C: Average Outcome Estimates

The table below reports estimates of the average outcome in each experimental condition. Column 1 describes the data source (online or telephone survey). Column 2 states the outcome variable name. Column 3 mentions the type of performance information to which respondents were randomly assigned. Column 4 mentions the government program about which performance information is randomly presented to the respondent. Columns 5 and 6 present the average value of the outcome in that experimental group, along with the HC2 robust standard error. Column 7 states the number of observations in each experimental condition.

Table A4.2: Group Means and Uncertainty Estimates

Mode	Outcome	Perf Info	Govt. Program	Average	SE	N
Online	Donate to BJP	Baseline	Baseline	5.651	0.273	152
Online	Donate to BJP	Efficiency	Cylinder	6.779	0.242	149
Online	Donate to BJP	Efficiency	House	6.449	0.273	147
Online	Donate to BJP	Generic	Cylinder	5.980	0.276	147
Online	Donate to BJP	Generic	House	6.331	0.256	151
Online	Donate to BJP	Out-Group	Cylinder	6.200	0.262	150
Online	Donate to BJP	Out-Group	House	6.735	0.252	151
Online	Perf. Evaluation	Baseline	Baseline	7.263	0.231	152
Online	Perf. Evaluation	Efficiency	Cylinder	8.738	0.137	149
Online	Perf. Evaluation	Efficiency	House	8.293	0.167	147
Online	Perf. Evaluation	Generic	Cylinder	8.122	0.192	147
Online	Perf. Evaluation	Generic	House	8.272	0.162	151
Online	Perf. Evaluation	Out-Group	Cylinder	8.480	0.162	150
Online	Perf. Evaluation	Out-Group	House	8.530	0.171	151
Telephone	Donate to BJP	Baseline	Baseline	3.573	0.161	765
Telephone	Donate to BJP	Efficiency	Cylinder	3.904	0.169	743
Telephone	Donate to BJP	Efficiency	House	4.218	0.165	785
Telephone	Donate to BJP	Generic	Cylinder	3.726	0.161	794
Telephone	Donate to BJP	Generic	House	3.895	0.165	768
Telephone	Donate to BJP	Out-Group	Cylinder	4.007	0.168	748
Telephone	Donate to BJP	Out-Group	House	3.742	0.166	747
Telephone	Perf. Evaluation	Baseline	Baseline	5.723	0.128	746
Telephone	Perf. Evaluation	Efficiency	Cylinder	6.944	0.126	730

Table A4.2: Group Means and Uncertainty Estimates *(continued)*

Mode	Outcome	Perf Info	Govt. Program	Average	SE	N
Telephone	Perf. Evaluation	Efficiency	House	7.206	0.116	771
Telephone	Perf. Evaluation	Generic	Cylinder	6.824	0.121	779
Telephone	Perf. Evaluation	Generic	House	6.997	0.115	745
Telephone	Perf. Evaluation	Out-Group	Cylinder	6.746	0.121	735
Telephone	Perf. Evaluation	Out-Group	House	7.103	0.119	720
Online	Donate to BJP	Efficiency	House+Cylinder	6.615	0.182	296
Online	Donate to BJP	Generic	House+Cylinder	6.158	0.188	298
Online	Donate to BJP	Out-Group	House+Cylinder	6.468	0.182	301
Online	Perf. Evaluation	Efficiency	House+Cylinder	8.517	0.108	296
Online	Perf. Evaluation	Generic	House+Cylinder	8.198	0.125	298
Online	Perf. Evaluation	Out-Group	House+Cylinder	8.505	0.118	301
Telephone	Donate to BJP	Efficiency	House+Cylinder	4.066	0.118	1528
Telephone	Donate to BJP	Generic	House+Cylinder	3.809	0.115	1562
Telephone	Donate to BJP	Out-Group	House+Cylinder	3.875	0.118	1495
Telephone	Perf. Evaluation	Efficiency	House+Cylinder	7.079	0.086	1501
Telephone	Perf. Evaluation	Generic	House+Cylinder	6.909	0.084	1524
Telephone	Perf. Evaluation	Out-Group	House+Cylinder	6.922	0.085	1455

Appendix D: Treatment Effect Estimates

Hypothesis 1

According to the pre-analysis plan, I evaluate this hypothesis by comparing respondents assigned to read/ listen to positively framed performance information (*generic* condition) and negatively framed performance information (*baseline* condition). I estimate the following ordinary least squares regression:

$$Y_i = \beta_0 + \beta_1(\text{Generic}_i) + \epsilon_i \tag{1}$$

where Hypothesis 1 predicts $\beta_1 > 0$. The results are presented in Table A4.3.

Table A4.3: Are voters receptive to performance information?

	Performance Evaluation			Donation to BJP		
	Pooled	Telephone	Online	Pooled	Telephone	Online
Treatment Effect	1.144***	1.186***	0.935***	0.280	0.236	0.506
	(0.135)	(0.153)	(0.263)	(0.174)	(0.198)	(0.331)
Control Group Mean		5.723***	7.263***		3.573***	5.651***
		(0.128)	(0.231)		(0.161)	(0.273)
Adj. R^2	0.050	0.027	0.030	0.036	0.000	0.003
Num. obs.	2720	2270	450	2777	2327	450

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Hypothesis 2

According to the pre-analysis plan, I evaluate this hypothesis by comparing core supporters assigned to the *out-groups benefited* condition and *generic information* condition. I estimate the following ordinary least squares regression:

$$Y_i = \beta_0 + \beta_1(\text{Out Groups Benefited}_i) + \epsilon_i \tag{2}$$

where Hypothesis 2 predicts $\beta_1 < 0$. The results are presented in Table A4.4.

Table A4.4: Do core supporters punish crossethnic distribution?

	Performance Evaluation			Donation to BJP		
	Pooled	Telephone	Online	Pooled	Telephone	Online
Treatment Effect	0.095	0.066	0.180	0.254	0.280	0.173
	(0.132)	(0.168)	(0.166)	(0.196)	(0.243)	(0.280)
Control Group Mean		7.619***	8.502***		5.704***	6.608***
		(0.119)	(0.111)		(0.174)	(0.199)
Adj. R^2	0.020	−0.001	0.000	0.007	0.000	−0.001
Num. obs.	1830	1370	460	1870	1410	460

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Hypothesis 3

According to the pre-analysis plan, I evaluate this hypothesis by comparing non-supporters assigned to the *out-groups benefited* condition and *generic information* condition. I estimate the following ordinary least squares regression:

$$Y_i = \beta_0 + \beta_1(\text{Out Groups Benefited}_i) + \epsilon_i \tag{3}$$

where Hypothesis 3 predicts $\beta_1 > 0$. The results are presented in Table A4.5.

Table A4.5: Do non-supporters reward crossethnic distribution?

	Performance Evaluation			Donation to BJP		
	Pooled	Telephone	Online	Pooled	Telephone	Online
Treatment Effect	0.001	−0.131	0.842	−0.393	−0.556	0.694
	(0.221)	(0.244)	(0.483)	(0.275)	(0.302)	(0.618)
Control Group Mean		7.035***	7.060***		4.368***	4.552***
		(0.167)	(0.380)		(0.209)	(0.442)
Adj. R^2	0.000	−0.001	0.015	0.004	0.003	0.002
Num. obs.	943	815	128	981	853	128

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Hypothesis 4

According to the pre-analysis plan, I evaluate this hypothesis by comparing respondents assigned to the *out-groups benefited* condition and *generic information* condition. I estimate the following ordinary least squares regression:

$$Y_i = \beta_0 + \beta_1(\text{House}_i) + \beta_2(\text{Out Groups}_i) + \beta_3(\text{House}_i \times \text{Out Groups}) + \epsilon_i \qquad (4)$$

where $\text{House}_i = 1$ for respondents assigned to information on the housing program and 0 if they are assigned to information on the cooking gas cylinder scheme; and $\text{Out-groups}_i = 1$ if the respondent is assigned to the *out-groups benefited* condition, and 0 if they are assigned to *generic* information. Hypothesis 4 predicts that $\beta_3 < 0$ for core supporters and $\beta_3 > 0$ for non supporters. The results are presented in Table A4.6.

Table A4.6: Benefit Size and the Reward or Punishment for Crossethnic Distribution

	Core Supporters		Non Supporters	
	Perf Eval	Donation	Perf Eval	Donation
House	0.227	0.130	0.587	0.358
	(0.187)	(0.281)	(0.306)	(0.383)
Outgroups Benefited	0.172	0.326	0.050	−0.353
	(0.193)	(0.277)	(0.315)	(0.382)
House x Outgroups Benefited	−0.152	−0.142	−0.126	−0.101
	(0.265)	(0.392)	(0.440)	(0.550)
Adj. R^2	0.019	0.006	0.004	0.003
Num. obs.	1830	1870	943	981
Study FE	Yes	Yes	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Hypothesis 5

According to the pre-analysis plan, I evaluate this hypothesis by comparing respondents assigned to the *efficiency* condition and *generic information* condition. I estimate the following ordinary least squares regression:

$$Y_i = \beta_0 + \beta_1(\text{Efficiency}_i) + \epsilon_i \tag{5}$$

where Hypothesis 5 predicts $\beta_1 > 0$. The results are presented in Table A4.7.

Table A4.7: Do voters reward efficient implementation?

	Performance Evaluation			Donation to BJP		
	Pooled	Telephone	Online	Pooled	Telephone	Online
Treatment Effect	0.194	0.170	0.319	0.289*	0.257	0.457
	(0.104)	(0.120)	(0.166)	(0.145)	(0.165)	(0.262)
Control Group Mean		6.909***	8.198***		3.809***	6.158***
		(0.084)	(0.125)		(0.115)	(0.188)
Adj. R^2	0.026	0.000	0.005	0.041	0.000	0.003
Num. obs.	3619	3025	594	3684	3090	594

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Hypothesis 6

According to the pre-analysis plan, I evaluate this hypothesis by comparing respondents assigned to the *efficiency* condition and *generic information* condition. I estimate the following ordinary least squares regression:

$$Y_i = \beta_0 + \beta_1(\text{House}_i) + \beta_2(\text{Efficiency}_i) + \beta_3(\text{House}_i \times \text{Efficiency}_i) + \epsilon_i \tag{6}$$

where $\text{House}_i = 1$ for respondents assigned to information on the housing program and 0 if they are assigned to information on the cooking gas cylinder scheme; and $\text{Efficiency}_i = 1$ if the respondent is assigned to the *efficiency* condition, and 0 if they are assigned to *generic* information. I expect $\beta_3 > 0$. The results are presented in Table A4.8.

Table A4.8: Benefit Size and the Reward for Efficiency

	Perf Eval	Donation
House	0.168	0.197
	(0.146)	(0.203)
Efficient Distrib.	0.203	0.280
	(0.151)	(0.204)
House x Efficient Distrib.	−0.024	0.012
	(0.207)	(0.289)
Adj. R^2	0.026	0.041
Num. obs.	3619	3684
Study FE	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Hypothesis 7

According to the pre-analysis plan, I evaluate this hypothesis by comparing respondents assigned to the *efficiency* condition and *out-groups benefited* condition. I estimate the following ordinary least squares regression:

$$Y_i = \beta_0 + \beta_1(\text{House}_i) + \beta_2(\text{Efficiency}_i) + \beta_3(\text{House}_i \times \text{Efficiency}_i) + \epsilon_i \tag{7}$$

where $\text{House}_i = 1$ for respondents assigned to information on the housing program and 0 if they are assigned to information on the cooking gas cylinder scheme; and $\text{Efficiency}_i = 1$ if the respondent is assigned to the *efficiency* condition, and 0 if they are assigned to *out-groups benefited* condition. I expect $\beta_2 > 0$ and $\beta_3 > 0$. The results are presented in Table A4.9.

Table A4.9: Do core supporters net-reward programmatic distribution

	Perf Eval	Donation
House	0.076	−0.013
	(0.187)	(0.274)
Efficient Distrib.	0.157	−0.106
	(0.185)	(0.272)
House x Efficient Distrib.	0.016	0.334
	(0.259)	(0.384)
Adj. R^2	0.017	0.009
Num. obs.	1857	1894
Study FE	Yes	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Appendix E: Alternative Measure of Core Supporters

This section presents results for Hypotheses 2 and 3 using an alternative measure of core supporters presented in the pre-analysis plan. This measure factors social status and economic affluence.

According to the pre-analysis plan:

A respondent is classified as a core supporter when:

They do not identify as Muslim, Christian, Sikh, Buddhist or Parsi (i.e. they identify as Hindu, Jain, No Religion or Other) AND any one of the following is applicable (depending on which of these are measured in the survey):

1. They belong to the following caste groups: General/ Upper Caste, OBC
2. They identify as middle or upper class
3. Their annual household income is greater than or equal to 200,000 rupees. The Center for Monitoring Indian Economy (CMIE) uses this income threshold to classify middle class. This roughly translates to a monthly household income of 17,000 rupees or more. I use a coarsened measure of monthly household income: respondents reporting a monthly household income of 20,000 to 50,000 (= 5 on the scale) or higher (6 or 7 on the scale) will satisfy this condition.
4. They are highly educated (i.e. graduate = 5, post graduate = 6, professional degrees and higher research = 7 on the education scale).

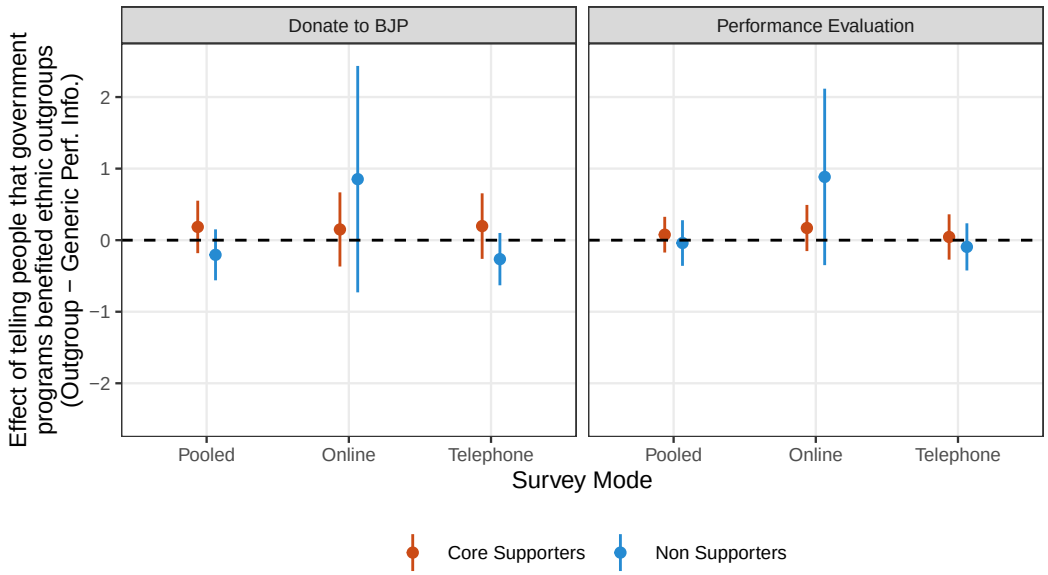
All others are classified as *non-supporters* or *periphery* voters.

Rewards or Punishment for Crossethnic Distribution

Table A4.10: Hypotheses 2 and 3 (Alternative Measure of Core Supporters)

Mode	Outcome Type	Subgroup	DIM	SE	p	CI(L)	CI(H)
Online	Donate to BJP	Core Supporters	0.150	0.264	0.570	-0.368	0.668
Online	Donate to BJP	Non Supporters	0.853	0.795	0.287	-0.730	2.435
Online	Perf. Evaluation	Core Supporters	0.170	0.164	0.301	-0.153	0.493
Online	Perf. Evaluation	Non Supporters	0.884	0.620	0.158	-0.349	2.117
Telephone	Donate to BJP	Core Supporters	0.197	0.233	0.400	-0.261	0.655
Telephone	Donate to BJP	Non Supporters	-0.265	0.186	0.155	-0.631	0.101
Telephone	Perf. Evaluation	Core Supporters	0.045	0.161	0.782	-0.272	0.361
Telephone	Perf. Evaluation	Non Supporters	-0.094	0.168	0.577	-0.423	0.236
Pooled	Donate to BJP	Core Supporters	0.185	0.187	0.323	-0.182	0.552
Pooled	Donate to BJP	Non Supporters	-0.205	0.182	0.259	-0.561	0.151
Pooled	Perf. Evaluation	Core Supporters	0.077	0.127	0.547	-0.173	0.326
Pooled	Perf. Evaluation	Non Supporters	-0.040	0.162	0.805	-0.359	0.279

Figure A4.1: Punishment or Reward for Distributing Outside the Ethnic Core



Note: The figure shows difference in means estimates ($\overline{Y_{\text{Out-Groups}}} - \overline{Y_{\text{Generic}}}$) along with 95% confidence intervals for core supporters (in orange) and non-supporters (in blue). Hypothesis 2 predicts that the point estimates in orange are negative and statistically distinguishable from 0. Hypothesis 3 predicts that the point estimates in blue are positive and statistically distinguishable from 0. The data plotted in this figure is reported in Table A4.10.

Appendix F: Randomization Check

The statistical models section in the pre-analysis plan specifies the procedure to check for randomization. The basic idea is to check whether covariates jointly predict treatment assignment using a multinomial regression. This involves estimating a goodness of fit measure (like the Akaike Information Criterion or AIC) for a model in which the categorical treatment assignment variable Z_i is regressed on covariates like female, age, education, urbanness, ethnicity, monthly household income, turnout in parliamentary election, and vote choice in the prior parliamentary election. I then conduct randomization inference on this test statistic to obtain a p-value that tells us the probability of observing this test statistic or more extreme under the null hypothesis that covariates are not jointly predictive of treatment assignment. If randomization is done correctly, we will most likely *fail* to reject this null hypothesis (i.e. $p > 0.05$).[12]

Parametric Test

I conduct an analysis of variance (ANOVA) test that compares the fit of two models: a multinomial regression in which Z_i (treatment assignment vector) is regressed on covariates, and a multinomial regression in which Z_i is regressed on a constant.

Model 1: $Z_i \sim \sum_{k=1}^{K} \beta_k X_k$

Model 2: $Z_i \sim 1$

ANOVA compares the two regression models, and evaluates the joint predictive power of all the covariates $X_1 \cdots X_K$. The null hypothesis is that $\beta_1 = \beta_2 = \cdots = \beta_K = 0$ (i.e. the covariates do not jointly predict any variation in Z_i).

[12] A p-value less than 0.05 does not necessarily imply that treatment is not randomly assigned. In 5% of cases covariate imbalance can occur by chance, even when the researcher randomizes treatment assignment.

Randomization Inference

I obtain the distribution of the test statistic (AIC) under the null hypothesis using 5,000 hypothetical random assignments generated by `randomizr` in `R`. Let the observed test statistic or AIC in Model 1 above be $\widehat{\text{AIC}}_{\text{obs}}$. I calculate a two-tailed p-value which is $2 \times min(p^{\text{lower}}, p^{\text{upper}})$. Note that the lower tail p-value here is approximately the same as the p-value from the ANOVA test.

Online Survey

Table A4.11 reports the results from the Analysis of Variance (ANOVA). I fail to reject this null hypothesis ($p = 0.805$).

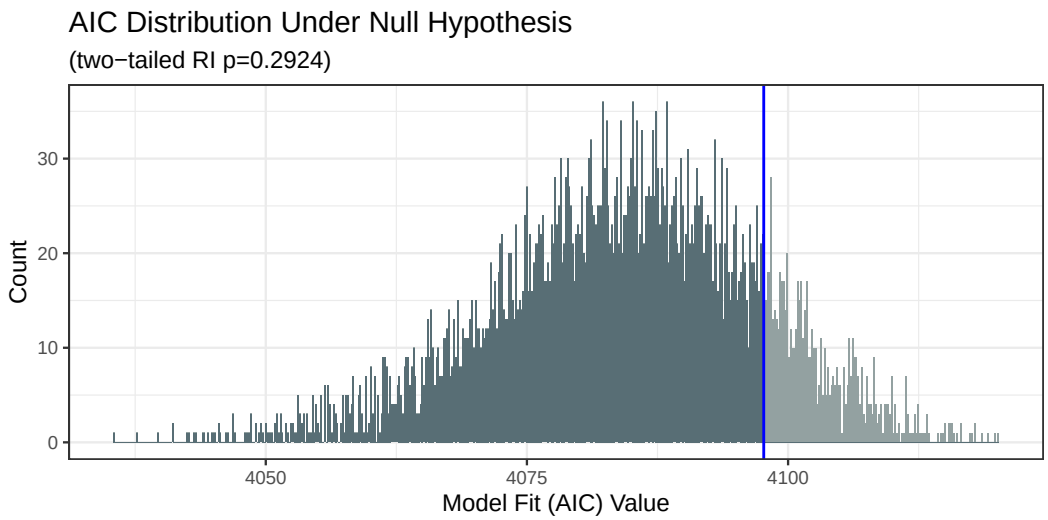
Table A4.11: Online Survey: ANOVA (Z $\sim$ Covariates v. Z $\sim$ 1)

Likelihood Ratio	Degrees of Freedom	Pr(Chi)
67.125	78	0.805

Randomization Inference

Figure A4.2 shows the distribution of the test statistic under the null hypothesis using 5,000 hypothetical random assignments. The blue line shows the observed test statistic or AIC in the data. I report a two-tailed p-value which is $2 \times min(p^{\text{lower}}, p^{\text{upper}})$. This p-value is 0.29. I am unable to reject the null hypothesis.

Figure A4.2: Randomization Check (Online Survey)



Note: Frequency distribution of the test statistic (AIC). The observed test statistic, $\widehat{\text{AIC}}_{\text{obs}}$, is shown using the blue line. The density below the blue line (p^{lower}) is shaded dark gray. The density above the blue line (p^{upper}) is shaded light gray.

Telephone Survey

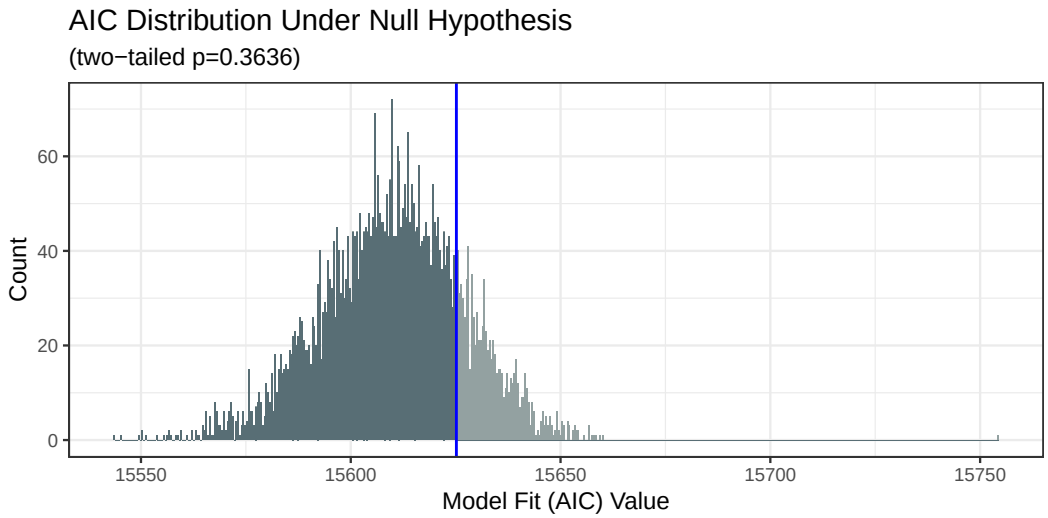
Table A4.12 reports the results from the Analysis of Variance (ANOVA). I fail to reject this null hypothesis ($p = 0.853$).

Table A4.12: Telephone Survey: ANOVA (Z $\sim$ Covariates v. Z $\sim$ 1)

Likelihood Ratio	Degrees of Freedom	Pr(Chi)
126.27	144	0.853

Figure A4.3 shows the distribution of the test statistic under the null hypothesis using 5,000 hypothetical random assignments. The blue line shows the observed test statistic or AIC in the data. I report a two-tailed p-value which is $2 \times min(p^{\text{lower}}, p^{\text{upper}})$. This p-value is 0.36. I fail to reject the null hypothesis.

Figure A4.3: Randomization Check (Telephone Survey)



Note: Frequency distribution of the test statistic (AIC). The observed test statistic, $\widehat{\text{AIC}}_{\text{obs}}$, is shown using the blue line. The density below the blue line (p^{lower}) is shaded dark gray. The density above the blue line (p^{upper}) is shaded light gray.

Appendix G: Missingness in Outcomes

The pre-analysis plan specifies that missingness in any outcome will be dealt with in the following way:

> We will check if missingness in the outcomes is correlated with treatment assignment. If the F-statistic in the ordinary least squares regression $\text{Missing}_i \sim Z_i$ is statistically significant ($p \leq 0.05$), we will estimate extreme value bounds for that outcome. In this regression specification, Missing_i is an indicator variable that takes a value 1 if there is missing data for that outcome for subject i, otherwise 0. Z_i is a categorical variable with seven levels that indicates the respondent's treatment assignment.

If missingness is uncorrelated with treatment assignment, the pre-analysis plan states that we "drop observations from a regression analysis if they have missing values for any variable in that regression specification".

Table A4.13 reports the F-statistic and associated p-value from an ordinary least squares regression, $\text{Missing}_i \sim Z_i$, for each outcome in the online and telephone survey. As Table A4.13 shows, there is missing data for only one outcome (attitudinal measure in the telephone survey). Treatment assignment is *not* jointly predictive of missingness in that outcome.

Table A4.13: Testing for Asymmetric Missingness in Outcomes

Mode	Outcome	Missing	F Statistic	p value
Telephone	Perf Eval	124	1.526 (df=6,5343)	0.165
Telephone	Donation	0	NA (df=6,5343)	NA
Online	Perf Eval	0	NA (df=6,1040)	NA
Online	Donation	0	NA (df=6,1040)	NA

Appendix H: Donating to Parties and Support for Ruling Party

This section explores why 44% of respondents in the telephone survey refused to donate money to any political party. I contend that respondents who do not want to donate to any political party are a particular type of opposition voter: they do not support the ruling party but are dissatisfied with all the opposition parties. To probe this point further, I estimate the proportion of BJP voters among those that donated money to any party, and those that refused to donate money to any party. I use a covariate that measures vote choice in the prior parliamentary election for this purpose. I do so separately for each experimental condition. Table A4.14 shows that fewer than 30% of those that refused to donate money to any party report voting for the BJP in the prior parliamentary election. Over 60% of those that donated to any party report voting for the BJP in the prior election. This difference in support for the ruling party is observed across experimental conditions.

Table A4.14: Donating to Parties and Support for Ruling Party

Donation	Treatment	Average	SE	CI(Low)	CI(High)	N
Donated to Parties	Baseline	0.622	0.024	0.575	0.668	423
Donated to Parties	Efficiency	0.661	0.016	0.630	0.693	871
Donated to Parties	Generic	0.677	0.016	0.645	0.708	851
Donated to Parties	Out-Group	0.679	0.016	0.647	0.711	810
Refused to Donate	Baseline	0.282	0.037	0.209	0.355	149
Refused to Donate	Efficiency	0.274	0.027	0.221	0.328	270
Refused to Donate	Generic	0.329	0.028	0.275	0.383	292
Refused to Donate	Out-Group	0.315	0.027	0.262	0.367	305

Appendix I: Pre Analysis Plan

Crosswalk to Pre-Analysis Plan

Table A4.15: Hypotheses, Pre-Registered Tests, Results

Hypothesis	Comparison	Empirical Specification	Result
H_1	Generic (positively framed performance information) v. Baseline (negatively framed performance information)	$Y_i \sim \beta_0 + \beta_1(\text{Generic})$, where $\beta_1 > 0$	Table A4.3
H_2, H_3	Out-groups Benefited v. Generic	$Y_i \sim \beta_0 + \beta_1(\text{Out Groups})$, where $\beta_1 < 0$ for core supporters and $\beta_1 > 0$ for non supporters.	Tables A4.4, A4.5
H_4	Out-groups Benefited v. Generic	$Y_i \sim \beta_0 + \beta_1(\text{House}) + \beta_2(\text{Out Groups}) + \beta_3(\text{House} \times \text{Out Groups})$, where *House* = 1 if assigned to information on the housing program and 0 for information on the cooking gas cylinder scheme; and *Outgroups* = 1 if assigned to the *out-groups benefited* condition, and 0 if assigned to *generic* information. $\beta_3 < 0$ for core supporters and $\beta_3 > 0$ for non supporters.	Table A4.6
H_5	Efficiency v. Generic	$Y_i \sim \beta_0 + \beta_1(\text{Efficiency})$, where $\beta_1 > 0$.	Table A4.7
H_6	Efficiency v. Generic	$Y_i \sim \beta_0 + \beta_1(\text{House}) + \beta_2(\text{Efficiency}) + \beta_3(\text{House} \times \text{Efficiency})$, where *House* = 1 if assigned to information on the housing program and 0 if assigned to information on the cooking gas cylinder scheme; and *Efficiency* = 1 if assigned to the *efficiency* condition, and 0 if assigned to *generic* information. I expect $\beta_3 > 0$.	Table A4.8
H_7	Efficiency v. Out-groups Benefited (core supporters only)	$Y_i \sim \beta_0 + \beta_1(\text{House}) + \beta_2(\text{Efficiency}) + \beta_3(\text{House} \times \text{Efficiency})$, where *House* = 1 if assigned to information on the housing program and 0 if assigned to information on the cooking gas cylinder scheme; and *Efficiency* = 1 if assigned to the *efficiency* condition, and 0 if assigned to *out-groups benefited* condition. I expect $\beta_2 > 0$ and $\beta_3 > 0$	Table A4.9

Voter Perceptions of Cross-ethnic Programmatic Distribution

Study Information

Hypotheses

H1: Voters will donate more money to the ruling party and rate its performance as better when they are exposed to positively framed performance information, compared to when they are exposed to negatively framed performance information.

H2: Core supporters will punish the ruling party for diverting resources to other ethnic groups. Specifically, treated subjects (core supporters who are informed that government programs have benefited a large number of people outside the ruling party's ethnic core) will donate less money to the ruling party and be more critical of its performance compared to control subjects (core supporters who are given generic information about government programs).

H3: Non-supporters will reward the ruling party for diverting resources to their ethnic groups and away from core supporters. Specifically, treated subjects (non-supporters who are informed that government programs have benefited a large number of people outside the ruling party's ethnic core) will donate more money to the ruling party and be less critical of its performance compared to control subjects (non-supporters who are given generic information about government programs).

H4: Core supporters will punish the party more strongly for distributing an expensive benefit to people outside the ethnic core compared to when the party distributes a cheap benefit. Non-supporters will reward the party more for distributing an expensive benefit compared to a cheap benefit.

H5: Voters will reward efficiency. Specifically, treated subjects (voters who are given information on program implementation) will donate more money to the ruling party and be less critical of its performance compared to control subjects (voters who are given generic information about government programs).

H6: Voters are more likely to reward the party for programmatic distribution when the benefit is expensive and has a large budgetary allocation compared to when the benefit is cheap and has a small budgetary allocation.

H7: Among core supporters of a party, the reward for programmatic efficiency significantly exceeds the punishment for distributing benefits outside the ethnic core.

Design Plan

Study type

Experiment - A researcher randomly assigns treatments to study subjects, this includes field or lab experiments. This is also known as an intervention experiment and includes randomized controlled trials.

Blinding

For studies that involve human subjects, they will not know the treatment group to which they have been assigned.

Is there any additional blinding in this study?

No response

Study design

Survey experiment, administered online or via telephone, that randomly assigns participants to see information vignettes. In the baseline condition (Z0), participants read/hear negative valence performance information about the ruling party. In two placebo control groups, they read/hear generic information about one of two government programs: one that distributes a $10 cooking gas cylinder (Z1), another that gives a $2000 house (Z4). In the "out-groups benefited" condition, respondents read/hear generic information about a government program (cylinder in Z2, houses in Z5) and are also informed that this program disproportionately benefits people outside the ruling party's ethnic core, namely Muslims, Dalits (SCs), and Tribals (STs). Finally, respondents in the "efficiency" condition receive generic information about a program and implementational details that describe specific steps taken by the government to reduce fake claim-making (biometric authentication of beneficiaries), and corruption (direct transfer of money to bank accounts). Z3 provides such information for the cooking gas cylinder scheme, Z6 for the housing program.

Immediately after this, we ask respondents to rate the government's performance, and donate 10 rupees to political parties (BJP, Congress, major state party/parties in respondent's state). Participants can give all the money to one party or split it between the parties. We are interested in the amount of money donated to the ruling party, BJP. The online survey ends with an open-ended question on "how [participants] decide who to vote for in national elections?". A research assistant blind to the study's hypotheses will code responses as 1 if they only mention performance, developmental work, and distributive policies; 0.5 if they mention these considerations but also other factors; and 0 if they only mention other factors.

The survey questionnaire and randomization scheme is attached.

- Survey Questionnaire.pdf (https://osf.io/project/3hr5p/files/osfstorage/625f728e6ac2a703174a9f48)

Randomization

Each respondent is assigned with equal probability to one of seven experimental conditions. For the online survey, I will check "evenly present elements" in Qualtrics' randomizer function.

Sampling Plan

Existing Data

Registration prior to creation of data

Explanation of existing data

No response

Data collection procedures

For the online survey:

Subjects will be recruited on Lucid. We will pay $1 for every completed survey response. Actual compensation

(money or points) will depend on the survey vendor through which the participant enters the survey. Eligibility will be limited to respondents located in India. In order for the sample to be representative of the population, we will

place gender, age, and region quotas based on screening questions available in Lucid Marketplace. We will "screen out" respondents who: (1) fail the Captcha verification process; (2) fail a pre-randomization attention check question; (3) select "I do not live in India" in the "which state do you currently reside in" question; and (4) do not consent to participate in the study. All other respondents will advance to the survey, and be randomly assigned to one of seven information vignettes.

For the telephone survey:

I will engage a survey company, CVoter, that conducts telephone surveys in India using the random digit dialing method. CVoter will add my questions to their omnibus surveys and make available a dataset containing responses to these questions and subjects' background information. A detailed note on CVoter's methodology (random digit dialing and computer assisted telephone interviewing or CATI), along with their quality control and enumerator training procedures are provided below. Like the online survey, respondents will be randomly assigned to hear one of seven information vignettes and then asked two outcome questions.

- CVoter CATI Methodology Note.pdf (https://osf.io/project/3hr5p/files/osfstorage/625f728e6ac2a703174a9f4c)
- Power Calculations.pdf (https://osf.io/project/3hr5p/files/osfstorage/625f728e6ac2a703174a9f4a)

Sample size

1,050 for the online survey. 3,000 for the telephone based survey.

Sample size rationale

A budget constraint determined the sample size. Within that constraint, I ensured that the research design is adequately powered to detect effects from a pilot survey experiment conducted in March 2022. The power calculation figure, along with an explanation, is attached.

Stopping rule

For the online survey: I will field the survey on Lucid Marketplace, allowing people in batches to take the survey, excluding previous respondents from new launches. I will keep the survey link active till approximately 1,050 people have completed the survey.

For the telephone survey: CVoter will provide approximately 3,000 responses to my survey questions. The exact sample size will be contingent on the contact rate.

Variables

Manipulated variables

The study randomly assigns respondents to read/listen to information vignettes. In the baseline condition (Z0), participants read/hear negative valence performance information about the ruling party. In two placebo control groups, they read/hear generic information about one of two government programs: one that distributes a $10 cooking gas cylinder (Z1), another that gives a $2000 house (Z4). In the “out-groups benefited” condition, respondents read/hear generic information about a government program (cylinder in Z2, houses in Z5) and are also informed that this program disproportionately benefits people outside the ruling party's ethnic core, namely Muslims, Dalits (SCs), and Tribals (STs). Finally, respondents in the “efficiency” condition receive generic information about a program and implementational details that describe specific steps taken by the government to reduce fake claim-making (biometric authentication of beneficiaries), and corruption (direct transfer of money to bank accounts). Z3 provides such information for the cooking gas cylinder scheme, Z6 for the housing program. The survey questionnaire contains each information vignette.

- Survey Questionnaire.pdf (https://osf.io/project/3hr5p/files/osfstorage/625f728e6ac2a703174a9f48)

Measured variables

Outcome Measures (H1 to H7):

1) How do you rate the performance of the Modi government on a scale of 0 to 10; where 0 is very poor and 10 is very good?

2) If you had to donate 10 rupees to a political party, who would you give the money to: BJP, Congress, or [major state party/parties]? You can give all of the money to one party, or give some of the money to a party and the remaining money to other parties. How much will you give to each of these parties? (Numeric variable capturing the amount given to the BJP, defined between 0 and 10)

3) How do you decide who to vote for in national elections? In a few words tell us what factors or things matter the most to you when you decide who to support in the Lok Sabha elections. (A research assistant blind to the study's hypotheses will code responses as 1 if they only mention performance, developmental work, and distributive policies; 0.5 if they mention these consideration but also other factors; and 0 if they only mention other factors.)

Covariates:
1) What is your age? / What year were you born in?
2) What is your gender? (1 = Male, 2 = Female)
3) Up to what level have you studied? (0 = Not Literate (Cannot read or write), 1 = Non-formal education (can read and write), 2 = Up to primary level, 3= Up to high school, 4 = Higher Secondary, 5 = Up to graduation, 6 = Up to Post-Graduate, 7 = Professional Degree)
4) What is your religion? (Hindu, Muslim, Christian, Sikh, Buddhist, Jain, Parsi, No religion, Others)
5) What caste group do you belong to? (SC, ST, OBC, General)
6) What is your main occupation? (1 = Student/ Unemployed, 2 = Housewife, 3 = Land owning farmer, 4 = Landless agricultural labor, 5 = Semi government/ Contractual government jobs, 6 = Government service, 7 = Private Sector Service, 8 = Business/ Self-employed, 9 = General Labor, 0 = Others)
7) Many people talk about class nowadays, and use terms like lower class, middle class or upper class. In your opinion, compared to other households, the household you live in currently belongs to which class – lower class, middle class, or upper class? (Lower class, Middle class, Upper class).
8) Do you live in a village, town, or city? (Village, Town (50,000 to 5 lakh people), City (More than 5 lakh people))
9) What is your family's income in one month? (1= Less than 3000, 2 = 3000 to 6000, 3 = 6000 to 10,000, 4 = 10,000 to 20,000, 5 = 20,000 to 50,000, 6 = 50,000 to 1 Lakh, 7 = More than 1 Lakh, 0 = Can't Say)
10) Did you vote in the 2019 Lok Sabha elections? (1 = I am sure I voted, 0 = I did not vote)
11) Which political party did you support in the 2019 Lok Sabha elections? (1 = BJP, 0 = Any other party or prefer not to answer).
(Telephone version: Whom did you vote for in the 2019 Lok Sabha Elections? Coded from party list, 0 = Did not vote, 98 = Not listed in voter list, 99 = Don't know/ Can't say, 999 = NOTA. To be recoded as 1 = BJP, 0 = Any other party or did not vote or not listed in voter list or don't know/ can't say or NOTA)
12) Location (Assembly Constituency, State, Region).

- Survey Questionnaire.pdf (https://osf.io/project/3hr5p/files/osfstorage/625f728e6ac2a703174a9f48)

Indices

Identifying core supporters:

The primary measure for identifying core supporters will be ethnicity: respondents that identify as Hindu General/ Upper Caste, Hindu OBC and Jain will be coded "core supporters", respondents from all other ethnic categories (Hindu SC, Hindu ST, Muslim, Sikh, Christian, Parsi, and Buddhist) will be treated as "non-supporters".

For robustness, I will also use a multidimensional measure that considers social status and economic affluence. In this measure, a respondent is classified as a core supporter when:

1. They do not identify as Muslim, Christian, Sikh, Buddhist or Parsi (i.e. they identify as Hindu, Jain, No Religion or Other)

AND any one of the following is applicable (depending on which of these are measured in the survey):

1. They belong to the following caste groups: General/ Upper Caste, OBC
2. They identify as middle or upper class
3. Their annual household income is greater than or equal to 200,000 rupees. The Center for Monitoring Indian Economy (CMIE) uses this income threshold to classify middle class. This roughly translates to a monthly household income of 17,000 rupees or more. I use a coarsened measure of monthly household income: respondents reporting a monthly household income of 20,000 to 50,000 (=5 on the scale) or higher (6 or 7 on the scale) will satisfy this condition.
4. They are highly educated (i.e. graduate = 5, post graduate = 6, professional degrees and higher research = 7 on the education scale).

All others are classified as “non-supporters” or “periphery” voters.

No files selected

Analysis Plan

Statistical models

The statistical tests of our hypotheses will use ordinary least squares regression, with heteroskedasticity-robust (HC2) standard errors. Most of our tests are for differences in means. We will visualize the results of these tests by plotting the means themselves.

For H1: Y = b0 + b1*Z, where Z is a treatment indicator that takes a value of 1 for respondents assigned to generic information about government programs (Z1 and Z4 conditions) and 0 for respondents assigned to read negative performance information (Z0). Y is the outcome variable (performance evaluation or money donated to the BJP). The data will be subsetted to respondents in three experimental conditions (Z0, Z1, Z4). We are interested in b1, the estimate of the average treatment effect.

For H2 and H3: Y = b0 + b1*Z, where Z is a treatment indicator that takes a value of 1 for respondents assigned to the "out-groups benefited" condition (Z2 and Z5), and 0 for those assigned to receive generic information about government programs (Z1 and Z4). Y is the outcome variable (performance evaluation or money donated to the BJP). The data will be subsetted to respondents in four experimental conditions (Z1, Z2, Z4, Z5). We are interested in b1. For H2, I will subset to core supporters and expect b1 < 0. For H3, I will subset to non-supporters (all respondents not considered "core supporters") and expect b1 >0.

For H4: Y = b0 + b1*(House) + b2*(Outgroups Benefited) + b3*(House)*(Outgroups Benefited), where House = 1 for respondents assigned to information on the housing program (Z4 and Z5) and 0 if they are assigned to information on the cooking gas cylinder scheme (Z1 and Z2); Outgroups Benefited =1 if the respondent is assigned to the "out-groups benefited" condition (Z2 and Z5), and 0 if they are assigned to generic information (Z1 and Z4). Y is the outcome variable. I will subset the data to respondents in four experimental conditions (Z1, Z2, Z4, Z5), and run the analysis separately for core supporters and non-supporters. We are interested in b3. Per H4, b3 < 0 for core supporters and b3 > 0 for non-supporters (i.e. the penalty or reward for cross-ethnic distribution is larger for the expensive benefit).

For H5: Y = b0 + b1*Z, where Z is a treatment indicator that takes a value of 1 for respondents assigned to the "efficiency" condition (Z3 and Z6), and 0 for those assigned to receive generic information about government

programs (Z1 and Z4). Y is the outcome variable (performance evaluation or money donated to the BJP). I will subset the data to respondents in four experimental conditions (Z1, Z3, Z4, Z6). We are interested in b1 and expect b1 > 0

for core supporters and non-supporters.

For H6: Y = b0 + b1*(House) + b2*(Efficiency) + b3*(House)*(Efficiency), where House = 1 for respondents assigned to information on the housing program (Z4 and Z6) and 0 if they are assigned to information on the cooking gas cylinder scheme (Z1 and Z3); Efficiency =1 if the respondent is assigned to the "efficiency" condition (Z6 and Z3), and 0 if they are assigned to generic information (Z1 and Z4). Y is the outcome variable. I will subset the data to respondents in four experimental conditions (Z1, Z3, Z4, Z6). We are interested in b3, and expect b3 >0 for core supporters and non-supporters (i.e. both groups reward efficiency in distribution more when there is an expensive benefit).

For H7: Y = b0 + b1*(House) + b2*(Efficiency) + b3*(House)*(Efficiency), where House = 1 for respondents assigned to information on the housing program (Z5 and Z6) and 0 if they are assigned to information on the cooking gas cylinder scheme (Z2 and Z3); Efficiency =1 if the respondent is assigned to the "efficiency" condition (Z6 and Z3), and 0 if they are assigned to the out-groups benefited condition (Z2 and Z5). Y is the outcome variable. I will subset the data to core supporters, and respondents in four experimental conditions (Z2, Z3, Z5, Z6). H7 posits that b2 > 0 and b3 >0 for core supporters (i.e. core supporters reward programatic efficiency more than they punish distribution to out-groups; and that net rewards are greater for the expensive benefit).

Pooled Analysis: For each hypothesis above, I will pool data from the online and telephone surveys. This analysis will have the same regression specification as above but with the addition of study fixed effects (Telephone = 0, Online = 1) so that all comparisons are within-study.

Covariate for Precision: To get more precise estimates, I will include the respondent's state or province as a covariate in each specification.

Randomization Check: I will check whether covariates jointly predict treatment assignment using a multinomial regression. This involves the following steps:

Step 1: Estimate a goodness of fit measure (like the AIC) using a multinomial regression (multinom function in the nnet package). The categorical treatment assignment variable Z (which has 7 levels) is regressed on covariates: female, age, education (0 to 7), social class (0,1 or 2), urbanness (0, 1 or 2), religion and caste community, monthly household income (0 to 7), turnout in prior parliamentary election (0 or 1), and ruling party supporter or voter in prior parliamentary election (0 or 1).

Step 2: Conduct randomization inference on this test statistic to get a p value (i.e. the probability of obtaining the test statistic from Step 1 or more extreme under the null hypothesis that covariates jointly do not predict treatment assignment).

Step 3: I will not reject the null hypothesis if the p value from randomization inference is larger than 0.05.

No files selected

Transformations

No response

Inference criteria

Each hypothesis will be evaluated using a two-tailed t-test. We will use p = 0.05 as the threshold to determine statistical significance. The t-statistic and associated p value will be generated using lm_robust or difference_in_means from the estimatr package in R.

Data exclusion

We will drop observations from a regression analysis if they have missing values for any variable in that regression specification. In the online survey, we will also exclude any respondents who leave the survey two or more times during the pre-treatment questions, as detected by an embedded JavaScript.

Missing data

We will check if missingness in the outcomes is correlated with treatment assignment. If the F-statistic in the ordinary least squares regression Missing ~ Z is statistically significant (p <= 0.05), we will estimate extreme value bounds for that outcome. Here, Missing is an indicator variable that takes a value 1 if there is missing data for that outcome, else 0. Z is a categorical variable with 7 levels that indicates the respondent's treatment assignment.

Exploratory analysis

No response

Other

Other

No response

Appendix J: Parties Seeking Small Donations

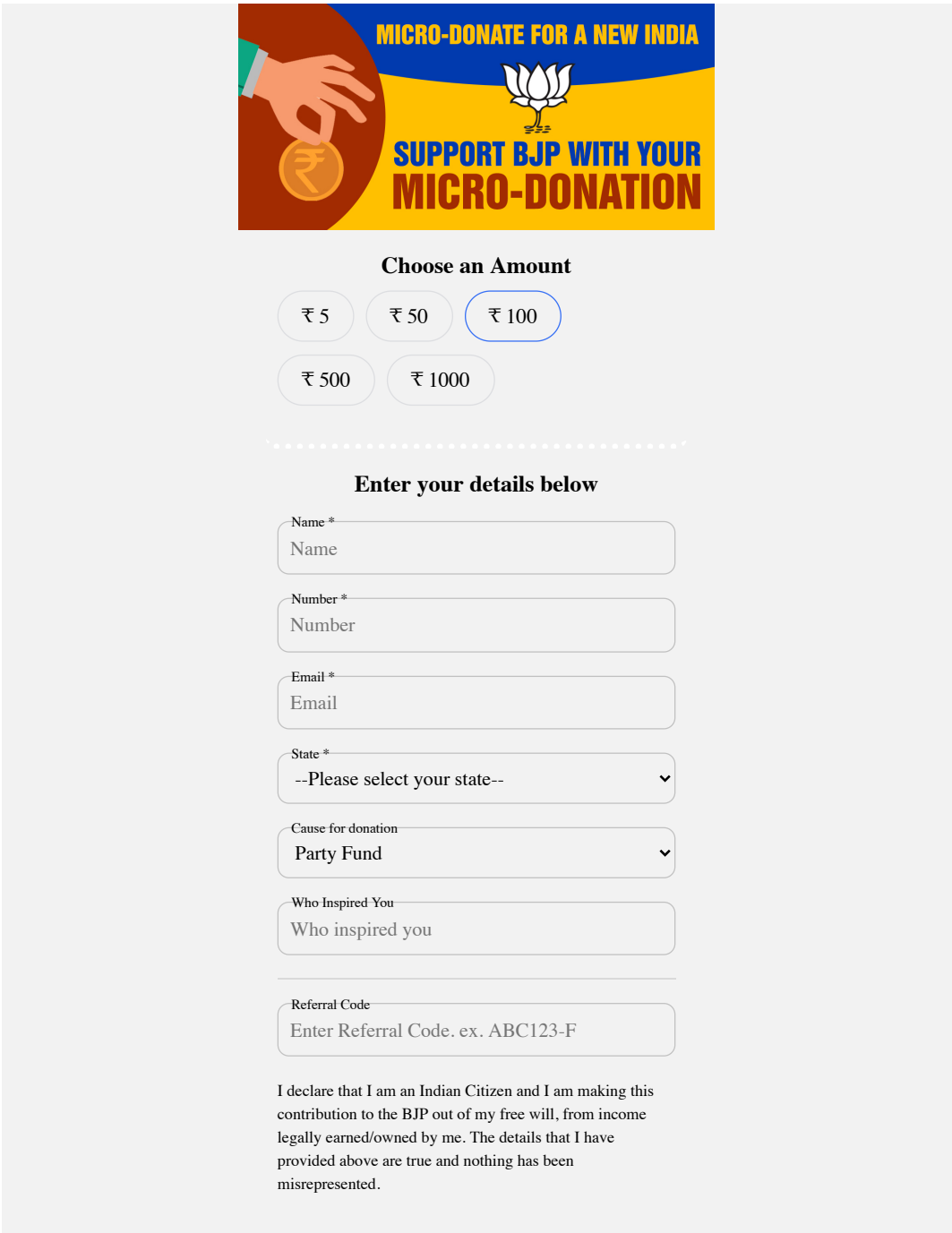
Narendra Modi's Tweet

On December 25, 2021, BJP leader and India's Prime Minister, Narendra Modi encouraged citizens to donate money to his party by sharing a screenshot of his own small donation to the party fund.

Figure A4.4: Modi's Donation to the Party Fund

Donation Web Pages

India's two main parties, the BJP and Congress, have setup websites to facilitate small donations to the party. Below are the websites on which Indian citizens can donate to these parties.



Indian National Congress

Donate to the party

Join Indian National Congress movement for change. Contribute for better future of the nation.

Share this on:

Terms & Conditions:

AGREE TO TERMS:

I ACCEPT THE FOLLOWING TERMS AND CONDITIONS:

- I Declare that I am a citizen of India.
- I am above the age of 18 years.
- I am making this contribution voluntarily from my own personal bank account or credit card and out of funds generated legally owned by me in India.
- I further declare that the said contribution is being made by me on my own volition without any undue influence, coercion, promise, or solicitation of any kind.
- I also declare that my voluntary contribution is fully compliant with norms and stipulations of the Foreign Contribution (Regulation) Act, 2010 (FCRA).
- The details I have provided above are true to the best of my knowledge and nothing has been concealed or misrepresented.
- I agree to be contacted by the Congress Party, it's frontal organizations, affiliates, and partners.
- I have read and accept the above Terms & Conditions.

FOR CHEQUE PAYMENT:

Payment Details

Amount

₹ Enter Amount

Email

Phone

Name

City

PAN Number

Address

Pin Code

State

--Select--

Date of Birth *

The cheque has to be drawn in favour of “President, All India Congress Committee” and sent to: The Office of the Treasurer, All India Congress Committee, 24, Akbar Road, New Delhi – 110011.

Please note: Your voluntary contribution to Indian National Congress, a political party is 100% deductible under the section 80GGB / 80GGC of the Income Tax, 1961.

You agree to share information entered on this page with Indian National Congress (owner of this page) and Razorpay, adhering to applicable laws.

Select Date

UPI VISA RuPay

Pay ₹ 0.00

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