Journal of Public Economics 262 (2026) 105762

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/pubec

Impacts of a partial welfare reform on targeting and political competition ☆

Pushkar Maitra [a,*], Sandip Mitra [b], Dilip Mookherjee [c], Sujata Visaria [d]

[a] Department of Economics, Monash University, Caulfield, Australia
[b] Sampling and Official Statistics Unit, Indian Statistical Institute, Kolkata, 700108, India
[c] Department of Economics, Boston University, Boston, MA 02215, USA
[d] Faculty of Finance, Bayes Business School, City St. George's, University of London, London, UK

HIGHLIGHTS

- We use longitudinal household survey data from West Bengal to evaluate the effects of recent welfare reforms on pro-poor targeting and political competition.
- Welfare programs became better targeted toward poorer and socially disadvantaged households between the periods 2010–2013 and 2014–2018.
- The welfare reforms do not explain why the BJP's vote share increased.
- BJP's electoral gains from their new nationwide programs were outweighed by increased support for the TMC from recipients of new state and pre-existing benefits.

ARTICLE INFO

JEL classification:
H31
H42
H75
P16

Keywords:
Fiscal decentralization
Welfare benefits
Targeting
Political competition
India

ABSTRACT

Over the past decade, India's BJP-led national government has undertaken various reforms to welfare programs, aiming to reduce the scope for local officials to manipulate beneficiary selection or divert funds. Nevertheless state and local governments retained considerable discretion over beneficiary selection and the local delivery of central government funded welfare benefits, and have maintained exclusive control over state-funded programs. We use longitudinal household survey data from the state of West Bengal to evaluate the effects of these reforms on pro-poor targeting of welfare programs, and political competition between the BJP and the TMC, the incumbent political party in the state. We find that welfare programs were better targeted toward poorer and socially disadvantaged households in the period 2014–2018 than in 2010–2013. However the welfare reforms do not explain why the BJP's vote share increased, as its electoral gains from the new nationwide programs were outweighed by their losses, stemming from the increased support for the TMC from recipients of new state and pre-existing benefits.

☆ We thank Toke Aidt, Pranab Bardhan, Sascha Becker, Siddharth George, Rafat Mahmood, Kaveh Majesi, Paul Raschky, Kunal Sen, Ajay Shenoy and seminar and conference participants at the Australian Development Economics Workshop, Australian Public Choice Conference, Australian National University, City, University of London, Delhi School of Economics/IEG, Hitotsubashi University, Monash University, Presidency University, Rishi Bankim Chandra College, Symposium in honour of Martin Ravallion and University of Hong Kong for their comments and suggestions. We have also benefitted from detailed comments and suggestions made by two anonymous referees and the co-editor Thomas Fujiwara. Funding was provided by the Australian Agency for International Development, the International Growth Centre, United States Agency for International Development, the Hong Kong Research Grants Council (GRF Grant Number 16503014) and the HKUST Institute for Emerging Market Studies (Grant Number IEMS15BM05). Jingyan Gao, Arpita Khanna, Clarence Lee, Daijing Lv, Foez Mojumder, Moumita Poddar and Nina Yeung provided exceptional research assistance. We received Internal Review Board clearance from Monash University, Boston University and the Hong Kong University of Science and Technology.

* Corresponding author.
Email addresses: Pushkar.Maitra@monash.edu (P. Maitra), Sandip@isical.ac.in (S. Mitra), dilipm@bu.edu (D. Mookherjee), Sujata.Visaria@citystgeorges.ac.uk (S. Visaria).

https://doi.org/10.1016/j.jpubeco.2026.105762
Received 25 January 2026; Received in revised form 12 August 2026; Accepted 17 August 2026

Available online 1 September 2026

1. Introduction

Over the course of economic development, governments have tended to reform welfare programs so that beneficiary selection follows pre-determined rules and benefit delivery is automated, thereby limiting the discretionary power of local officials (Stokes, 2005; Stokes et al., 2013; Bardhan et al., 2020). Such programmatic reforms require large investments in centralized databases on household measures of need, as well as direct delivery mechanisms that rely on well-functioning automated payment systems and a high degree of financial inclusion. Notable recent examples include conditional cash transfer schemes in Brazil and Mexico, land reforms in Mexico, the electronic food voucher program (BPNT) in Indonesia, and the formula-based cash transfer program (BISP) in Pakistan. The reforms in Indonesia and Pakistan appear to have substantially improved the programs' targeting of the poor and limited the advantages of personal connections with local officials (Banerjee et al., 2023; Haseeb and Vyborny, 2022). In Brazil and Mexico, the reforms increased political competition and lowered clientelism (de Jainvry et al., 2014; Larraguy et al., 2015; Frey, 2019).

This paper examines the case of India, where various welfare reforms over the past decade have also aimed to limit the scope for local discretion, although they have been less comprehensive than the examples above. These were initiated in 2013 by the coalition centre-left government as the *Direct Benefit Transfer* scheme, but gained momentum after the Narendra Modi-led Bharatiya Janata Party (BJP) formed the national government in 2014. The central government introduced *new nationwide programs*, including the in-house toilets scheme (*Swachchh Bharat Yojana*), universal bank accounts scheme (*Jan Dhan Yojana*), and a new method for disbursing subsidies for cooking fuel (*Ujjwala Yojana*). For these, as well as for *pre-existing schemes*, they established enhanced oversight and selection mechanisms requiring biometric verification of the beneficiary's identity, and direct deposits of monetary benefits into the beneficiary's bank account. The new National Food Security Act in 2013 also led many states to streamline and revise their beneficiary lists for the public distribution system (PDS) of foodgrains (Drèze et al., 2019).[1] For many schemes, eligibility criteria were redefined or rearticulated more clearly. Concurrently, the central government cut their funding to some pre-existing schemes that were administered by state and local governments, causing them to contract.[2] Finally, many state governments devised and launched *new state programs*: self-funded benefit programs for their own residents that were not subject to central government restrictions or oversight.

However, the changes to the Indian welfare system did not go the full distance of centralizing beneficiary selection, or implementing pre-specified proxy-means formulae to determine entitlements. Local officials continued to have a role in identifying beneficiaries and in delivering benefits to households. Thus, the changes represent a partial welfare reform which reduced but did not eliminate the scope for local discretion. Such partial reforms required smaller investments in state capacity and less overreach by the national government over lower levels of administration. This Indian experience is therefore likely to be of interest to policymakers in other developing countries assessing trade-offs between partial and comprehensive welfare reforms. To this end, this paper provides evidence on the impact of these reforms on pro-poor targeting and political competition.[3]

Our specific context is the state of West Bengal, where there is active political competition between the parties governing at the central and state levels. Since 2014, the central government has been led by the BJP, whereas after the 2011 state legislature elections, the state government has been controlled by the Trinamool Congress (TMC), a regional party. Whereas the BJP had a negligible electoral presence in the state until 2014, both its vote and seat shares increased dramatically in the 2019 national election. Our analysis attempts to understand how much of this increase can be attributed to changes in the scale and composition of welfare benefits.[4]

We use data from the 2013 and 2018 rounds of a longitudinal survey of 3500 rural households residing in two districts in West Bengal, conducted a few months before the 2014 and 2019 parliamentary elections. We collected data on a comprehensive set of welfare benefits received by the households during the periods 2010–2013 and 2014–2018. We classify the multitude of welfare programs into three categories: (i) "pre-existing programs": those that existed before 2014 and continued to operate during 2014–2018; (ii) "new nationwide programs": those that were introduced by the central government after 2014, usually branded as Prime Minister Narendra Modi's schemes, and (iii) "new state programs": those that were introduced after 2014 by the TMC-led state government, branded as schemes provided by West Bengal's Chief Minister Mamata Banerjee.

Overall, we find that the welfare programs became better targeted to the poor and socially disadvantaged after 2014. New nationwide program benefits (by construction, these are only observed in the 2014–2018 period), were distributed in a more progressive manner than the pre-existing or new state program benefits during 2014–2018. Also, the distribution of pre-existing programs became more progressive after 2014. The share of the poorest households (i.e., those who owned no land) increased by 3–5 percentage points, a modest improvement in comparison with the changes caused by the Pakistan and Indonesian reforms.[5]

Next, we investigate how changes in benefit distribution affect voter support for the different political parties. It is natural to hypothesize

[1] The ration card that allows households to access the PDS also serves as an official indicator of their poverty status, with those who have a "below the poverty line" (or BPL) card eligible for larger entitlements than those "above the poverty line" (APL). Since many other schemes use possession of a BPL card as a selection criterion, revision of the list could affect distribution patterns of other welfare programs as well.

[2] The most prominent program to be downsized was the national rural employment guarantee scheme (NREGS), which is said to have provided employment to 50 million households per year between 2008 and 2012 (Government of India, 2020).

[3] There is some evidence that has shown that biometric verification reduced leakages in the national employment program (NREGS) by reducing the number of 'ghost' beneficiaries (Muralidharan et al., 2016, 2023). Case studies and media reports have claimed that the new nationwide welfare programs contributed to the BJP's growing vote share in the country after 2014. See, for example, Joshi et al. (2022). Reporting on the BJP's strong gains in four state elections in 2023, Mashal and Kumar (2023) wrote:

Mr. Modi, projecting himself as an ambitious champion of development as well as Hindu interests, also has a strong pull with voters across the country. His government has used the resources of the top-heavy and unequal Indian economy for well-targeted welfare schemes, handed out often in his name.

Deshpande et al. (2019) show that compared to earlier elections, voters in 2019 were more likely to credit the central government rather than state governments or local politicians for many nationwide welfare programs. Sen (2019) argues that targeted welfare schemes were a key driver of the BJP's landslide victory in the 2019 Parliamentary elections.

[4] Since many different aspects of the welfare programs were modified around the same time, we are unable to isolate the effect of any single dimension of the reforms.

[5] Haseeb and Vyborny (2022)'s findings indicate that the Benazir Income Support Program (BISP) in Pakistan increased the poor's share of benefits from 23 to 48% (our calculations based on estimates from Haseeb and Vyborny (2022), Table A4, where a household is classified as poor if it does not have solid flooring in its residence). In Indonesia, the government's BPNT electronic voucher program increased the share of the food subsidy going to poor households from 53 to 70% (our calculations based on estimates from Banerjee et al. (2023)).

that voters reward the party that they believe was responsible for delivering their benefit to them. In our context, this attribution is likely to depend on: (*a*) which party controlled the central government when the program was first launched, (*b*) advertisements and media descriptions about the forces behind the program, and (*c*) which party is currently in the state / local government, and therefore controls the local distribution of benefits. The new nationwide programs were introduced after the BJP formed the national government in 2014. Media campaigns branded them as reflecting the benevolence of Prime Minister Modi. If the campaign was convincing, the recipients of a new nationwide benefit are likely to have credited this to the BJP. Conversely, recipients of new state benefits are likely to have credited the TMC, because the programs were explicitly marketed as funded and delivered by the TMC-controlled state government. The pre-existing programs, on the other hand, were created under the auspices of previous national governments generally dominated by the Indian National Congress (INC), and continue to retain the names and images of former INC leaders such as Mahatma Gandhi and Indira Gandhi. Mamata Banerjee, the Chief Minister of West Bengal during 2011–2026, belonged to the INC before she broke away to form the TMC. Additionally, although they are funded by the central government, state governments have traditionally been responsible for the delivery of these pre-existing benefits. Since ordinary households tend to know less about funding sources than about last-mile delivery agents, they are likely to have attributed these benefits to the TMC.

This leads us to test the following hypotheses that relate benefit receipts to political support. *First*, the receipt of a new nationwide program benefit during 2014–2018 would increase voter support for the BJP, whereas the receipt of a pre-existing or new state program benefit would increase support for the TMC. *Second*, since local officials presumably had less discretion in allocating pre-existing benefits after 2014, these benefits would be less effective at generating support for the TMC in 2018 than in 2013. *Finally*, household responses to changes in the scale and composition of welfare benefits help explain or are consistent with the observed changes in support for BJP and TMC between 2013 and 2018.

We test these hypotheses using household reports on the benefits they received, and their support for political parties as expressed in a confidential poll.[6] In this context, cross sectional regressions do not identify the causal effect of receiving a benefit, because government officials may strategically distribute benefits across villages and households based on their assessments of how households will vote. We therefore run separate cross-sectional instrumental variable regressions for each of the two survey rounds. Benefits received over the previous four years are instrumented by two versions of a “leave-out” instrument, which depend in different ways on the benefits received by households in other villages during the same time period (see Levitt and Snyder, 1997; Bardhan et al., 2024; Dang and La, 2019; Sedai et al., 2020; Maitra et al., 2023; Ellis and Jasperson, 2024).

We find that the receipt of a benefit from a new nationwide program had a statistically insignificant effect on the average household’s support for the BJP in 2018. The point estimates range from 3 to 9 percentage points, but are imprecisely estimated. In contrast, new state benefits increased support for the TMC by a statistically significant 18–28 percentage points (or 32–49% of the overall mean). We do not find evidence that pre-existing benefits became less effective at generating support for the TMC after 2013: one additional pre-existing benefit increased support for the TMC by a statistically significant 2.7–5.6 percentage points in 2018 (5–10% of the overall mean), in contrast to the imprecisely estimated 1.6–2.4 percentage point effects in 2013.

Combining these point estimates with the proportion of households receiving benefits of different types, we calculate the resulting aggregate effect on political support for each party in 2013 and 2018. Our results imply that changes in the distribution of benefits and in voters’ responses would have resulted in a 2.1–8.8 percentage point increase in support for the TMC, in contrast to an actual decline of 5.6 percentage points. This is explained mainly by the increased popularity of the TMC among recipients of the new state programs, and the increased pro-TMC responsiveness among recipients of pre-existing benefits, which outweighed the shrinkage in their scale. Conversely, our estimates imply that welfare changes would have led to a decline in the support for the BJP between 3.9 and 12.3 percentage points, whereas their support actually increased by 17 percentage points. Hence, in contrast to the popular narrative, the evidence does not support the claim that welfare reforms explain the observed rise in support for the BJP. Instead, it appears that they enabled the TMC to raise its vote share by maintaining control of the local distribution of benefits, and utilizing their constitutional authority to introduce new state programs.

2. Data and institutional details

2.1. Data

Our longitudinal survey covers 3500 households residing in 72 randomly selected villages in the potato-growing *talukas* (sub-divisions) of the Hugli and Pashchhim Medinipur districts of West Bengal.[7] The surveys were conducted in 2013 and 2018, and include information about sample households’ demographic and socio-economic characteristics and the welfare program transfers they received from the government (in the three or four years immediately preceding the survey).[8]

In both survey rounds, respondents also participated in a poll by casting a “secret ballot” for the political party they supported. The interviewer gave them a paper ballot with the names and election symbols for the major political parties in the area, asked them to mark the symbol of their preferred party, fold and place the ballot in a locked box, shake it and return it to the interviewer.[9] The respondents were assured that their votes would remain confidential, and that participation in the exercise was voluntary. All households in our sample consented to participate in the poll in 2013; less than 1% selected the option *None of the Above* on the ballot. In 2018, 8.1% of households refused to participate in the poll and 1.2% of those who participated selected the *None of the Above* option. However, refusal to participate in 2018 was uncorrelated with the party they cast the ballot for in 2013.

[6] Casey (2015) uses data from a similar confidential straw poll in Sierra Leone, and Bardhan et al. (2024) use data from such polls in West Bengal. The Lokniti-CSDS national election surveys conducted across India regularly include such a question.

[7] The sample was drawn for previous, unrelated projects: see Maitra et al. (2017); Mitra et al. (2018); Maitra et al. (2022, 2024). Sample villages were selected randomly from a stratified (by village council) list of villages, with the requirement that they be at least 8 kilometers apart from each other. As per the 2011 Census of India, the population of Hugli was 3.4 million and that of Pashchhim Medinipur was 5.2 million. Fig. A4 in the Supplementary Appendix shows that changes in vote shares in these two districts were similar to those in West Bengal as a whole. Our sample is purposively selected and households were weighted to ensure representativeness. See Supplementary Appendix C1 for a discussion of the sampling and the weighting process. Fig. A1a in the Supplementary Appendix shows the location of the two districts within the state, and Fig. A1b marks the locations of the sample villages.

[8] There was no attrition in our sample across the two rounds. Household composition did change in some households, with particular individuals leaving due to household splits, migration, death or marriage, but all households from the 2013 round were found in the same village during the 2018 round. The household respondent from 2013 also responded to the survey in 2018. If households had split between the two survey rounds, then in 2018 we interviewed the head of the original (parent) household from 2013.

[9] The box contained some dummy ballots and ballots cast by previously surveyed households. The ballot papers had no identifying information except for the household code number assigned by the PIs. None of the interviewers, respondents or data entry operators could link the household code number back to the actual household.

Table 1
Selected descriptive statistics: household characteristics.

	Authors' survey		National sample survey†	
	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)
Male Headed Household	0.943	0.231	0.917	0.276
SC Household	0.255	0.436	0.248	0.432
ST Household	0.044	0.206	0.090	0.286
Non Hindu Household	0.173	0.378	0.154	0.361
General Caste/OBC Household	0.528	0.499	0.509	0.500
Recent Migrant Household	0.002	0.042		
Household Size	5.708	2.706	3.970	1.825
Household Head Married	0.901	0.299	0.911	0.285
Household Head: More than Primary Schooling	0.402	0.490	0.444	0.497
Household Head Occupation: Cultivator	0.487	0.500		
Household Head Occupation: Labour	0.279	0.448		
Household Head Occupation: Business	0.066	0.249		
Landless	0.187	0.390	0.100	0.300
Landholding 0–0.5 acres	0.377	0.485	0.739	0.439
Landholding 0.5–1 acres	0.222	0.416	0.101	0.302
Landholding > 1 acres	0.214	0.410	0.059	0.236
Lives in a Kuchha (non-permanent) House	0.552	0.497		

Notes: † : National Sample Survey data from the districts of Hugli and Pashchim Medinipur are pooled from the rural sample of the 66th (2009-10) and 68th (2011-12) rounds. Means for survey data are weighted to ensure representativeness.

2.2. Household characteristics

Columns 1 and 2 of Table 1 present statistics about the characteristics of our sample households. In line with the population in this region, 94% of households were headed by men, and 83% were Hindu.[10] More than one half of the households belonged to either the General Castes or Other Backward Castes, and just over a quarter of households belonged to the Scheduled Castes. Only about 5% reported belonging to the Scheduled Tribes. Nearly all households had lived in the village for at least 10 years.

The majority of the households were of low economic status. This is evidenced by their low landholding: 19% owned no agricultural land, and 78% owned less than one acre. Sixty percent of household heads were not educated beyond primary school. Fifty-five percent of dwellings were non-permanent structures (*kuchcha*), usually built of mud or tin. About 77% of household heads were occupied in cultivation or casual labour.[11]

2.3. Benefits: Institutional details and descriptive statistics

As explained in Section 1, we classify the different welfare programs into three categories. *Pre-existing* programs include programs that existed as of 2013 and continued to exist in 2018. These include the national rural employment guarantee scheme (NREGS), separate schemes for village councils to provide housing, toilets or drinking water facilities, BPL cards and a range of other benefits like pensions, widow allowances, irrigation facilities, flood relief and vocational training. *New nationwide* programs were introduced by the BJP-led national government after 2014, and include household grants to construct in-house toilets (*Swachchh Bharat Yojana*), no-fee "zero-(minimum)-balance" bank accounts (*Jan Dhan Yojana*) and direct transfers of subsidies for cooking gas into beneficiaries' bank accounts (*Ujjwala Yojana*). Finally, *new state programs* were introduced by the TMC-led state government, which included medical assistance from the village council and a conditional cash transfer program aimed at increasing the educational attainment of girls (*Kanyashree*). The complete list of welfare programs is provided in Panels B, C and D of Table 2.

All benefit schemes, including the new nationwide programs, are administered through a similar hierarchy of central, state and local government officials, which include both elected representatives and appointed bureaucrats at all levels. Financial and other approvals are granted by higher level bodies, while delivery is delegated to block (sub-district) councils, block development offices and village councils. Despite the increased transparency and verification procedures created by the 2014 reforms, state and local governments continue to be either directly or indirectly involved in the last-mile implementation of the programs. For instance, the *Swachchh Bharat* and *Ujjwala* programs are restricted to households with "below poverty line" (BPL) status. This status is validated through a BPL card, whose distribution is delegated to local governments. Other eligibility criteria for specific schemes, such as the requirement that beneficiaries be poor or marginal farmers, disabled, pregnant, or households with girl children, can only be verified by local governments. While the *Ujjwala* subsidy is paid directly into beneficiaries' bank accounts, state and local governments are responsible for delivering *Swachchh Bharat* subsidies in the form of either cash or credit vouchers for construction materials.[12]

Within each of the three categories, we measure the total count of benefits received by any household rather than their monetary value because the benefits are all very different, making it difficult to measure their relative value. For example, BPL ration cards give beneficiaries access to subsidised consumables such as rice, oil and sugar; NREGA job cards give access to workfare over a long horizon; the housing scheme gives access to materials to build a house; and community toilets are shared with several other households. There is no "market" for many of these benefits. Moreover, a household may benefit from access even if they do not avail themselves of it to the maximum extent. We check if our main conclusions are robust to an alternative specification where the aggregate count is replaced by a vector of indicator variables for receipt of different specific benefits.

As we see in Panel A of Table 2, in both survey rounds, about 76–80% of households reported receiving at least one welfare benefit in the previous 3–4 years. However, the average number of benefits received increased slightly from 1.6 to 2.3 from the 2013 to the 2018 round, and the composition of benefits changed. Fewer households (68%) received pre-existing benefits during 2014–2018 compared to 2010–2013 (80%). Nearly 40% of households in 2018 received a new nationwide benefit, while 11% received a new state benefit. While the pre-existing programs shrank in scale, they continued to be the largest category of benefits that households received. Moreover the scale of the new state programs was roughly similar in magnitude to the reduction in pre-existing programs, implying that their combined scale remained approximately the same.

3. Targeting comparisons

We start the empirical analysis by examining the extent to which the different programs reached the poor. We focus on ownership of agricultural land as a measure of household economic status, as it is closely

[10] All 17% of households who reported their head's religion as non-Hindu were Muslims.

[11] Table 1 compares the summary statistics from our sample with those from the National Sample Survey, a representative survey of Indian households conducted by the Indian government's Ministry of Statistics and Programme Implementation in 2009–2010 (66th round) and 2011–2012 (68th round). Unlike the NSS, our sampling frame includes only the potato-growing sub-districts of these two districts. However the proportions of Scheduled Castes, Muslims and general caste/OBC Hindus are very similar. Our sample includes a smaller percentage of Scheduled Tribe households, and the distribution of landholding in our sample exhibits thicker tails at both ends. Occupations were categorised differently in our survey, so we cannot compare our occupational status variable with that from the NSS.

[12] The rules governing the *Swachchh Bharat Yojana* state: "… *States shall have the flexibility to decide on the implementation mechanism to be followed in the state*." (MDWS, 2014, page 12).

Table 2
Households' receipt of different benefits during 2010–2013 and 2014–2018.

	2010–2013 (1)	2014–2018 (2)	Difference (3 = 2-1)
Panel A: *All Benefits*			
Total Number of Benefits	1.618	2.286	0.668
	(1.239)	(1.195)	[0.000]
Total Number of Benefits (excluding BPL cards)	1.584	1.927	0.343
	(1.217)	(1.737)	[0.000]
Proportion any Benefit	0.801	0.807	0.007
	(0.400)	(0.394)	[0.475]
Proportion any Benefit (excluding BPL cards)	0.799	0.762	−0.037
	(0.400)	(0.426)	[0.000]
Panel B: *New Nationwide Benefits*			
In-House Toilet (*Swachchh Bharat*)		0.146	
		(0.354)	
LPG (*Ujjwala*)		0.149	
		(0.356)	
Bank Account (*Jandhan*)		0.180	
		(0.384)	
Number New Nationwide Benefits		0.475	
		(0.656)	
Proportion any New Nationwide Benefits		0.390	
		(0.488)	
Panel C: *Pre-existing Benefits*			
Housing (*Panchayat*)	0.038	0.088	0.050
	(0.191)	(0.284)	[0.000]
Employment (NREGS)	0.500	0.425	−0.075
	(0.500)	(0.494)	[0.000]
Loans	0.050		
	(0.218)		
Toilet (*Panchayat*)	0.057	0.190	0.132
	(0.233)	(0.392)	[0.000]
Drinking Water (*Panchayat*)	0.302	0.251	−0.051
	(0.459)	(0.434)	[0.000]
Land Title (*Patta*)	0.001	0.007	0.005
	(0.037)	(0.081)	[0.000]
Road Building Work (non NREGS)	0.381	0.006	−0.375
	(0.486)	(0.079)	[0.000]
Pension	0.015	0.039	0.025
	(0.120)	(0.194)	[0.000]
Widow Allowance	0.013	0.021	0.008
	(0.113)	(0.144)	[0.009]
Irrigation	0.061	0.016	−0.045
	(0.239)	(0.125)	[0.000]
Flood Relief	0.119	0.145	0.026
	(0.324)	(0.352)	[0.001]
Self Help Group (SHG)	0.038	0.133	0.095
	(0.191)	(0.340)	[0.000]
Vocational Training	0.008	0.014	0.006
	(0.090)	(0.117)	[0.024]
BPL Card	0.033	0.359	0.325
	(0.180)	(0.480)	[0.000]
Number Pre-existing Benefits	1.618	1.693	0.075
	(1.239)	(1.522)	[0.023]
Number Pre-existing Benefits (excluding BPL cards)	1.584	1.335	−0.249
	(1.217)	(1.134)	[0.000]
Proportion any Pre-existing Benefits	0.801	0.746	−0.055
	(0.400)	(0.435)	[0.000]
Proportion any Pre-existing Benefits (excluding BPL cards)	0.799	0.679	−0.120
	(0.400)	(0.467)	[0.000]
Panel D: *New State Benefits*			
Medical Help (*Panchayat*)		0.081	
		(0.273)	
CCT (*Kanyashree*)		0.036	
		(0.188)	
Number New State Benefits		0.118	
		(0.334)	
Proportion any New State Benefits		0.114	
		(0.318)	

Notes: All benefits in Panel A provide the corresponding number of benefits and proportion of households receiving at least one, aggregated across all three types of benefits. New nationwide benefits and new state benefits did not exist before 2014. In Panels B–D, the initial set of rows shows the fraction of households who report receiving a specific benefit at least once during the periods 2010–2013 (column 1) and 2014–2018 (column 2) respectively. Subsequent rows show the total number of benefits of any type received per household, and the proportion of households receiving at least one of that type of benefit in the same period. Figures in parentheses denote standard deviations. Column 3 presents the difference between mean 2018 and mean 2013. Figures in square brackets denote the p-value from a Wald F-test for the difference in means. The sample is re-weighted to ensure representativeness (see Maitra et al., 2024) for details.

Table 3
Targeting of benefits: likelihood of receiving benefits and cumulative proportion of benefits accruing by land category. Aggregated benefits.

	2014–2018				2010–2013	Population
	All†	New nationwide	Pre-existing	New state	Pre-existing	Share
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: *Likelihood of Receiving (columns 1–5)*						
Landless (L1)	2.253***	0.639***	1.534***	0.080***	1.563***	0.195
	(0.133)	(0.063)	(0.098)	(0.019)	(0.124)	
Landholding 0—0.5 acres (L2)	2.317***	0.574***	1.605***	0.139***	1.631***	0.400
	(0.170)	(0.042)	(0.131)	(0.035)	(0.120)	
Landholding 0.5—1.0 acres (L3)	1.647***	0.385***	1.149***	0.113***	1.561***	0.230
	(0.128)	(0.034)	(0.116)	(0.026)	(0.116)	
Landholding 1.0—1.5 acres (L4)	1.340***	0.297***	0.932***	0.112***	1.593***	0.083
	(0.139)	(0.034)	(0.118)	(0.037)	(0.128)	
Landholding > 1.5 acres (L5)	1.145***	0.206***	0.816***	0.123***	1.504***	0.093
	(0.133)	(0.030)	(0.114)	(0.034)	(0.116)	
Number of Households	3500	3500	3500	3500	3500	
R-squared	0.581	0.377	0.533	0.114	0.629	
Panel B: *Share of Benefits Accruing*						
L1	0.224	0.256	0.220	0.133	0.192	
L1 + L2	0.696	0.728	0.693	0.604	0.603	
L1 + L2 + L3	0.889	0.910	0.887	0.824	0.829	
L1 + L2 + L3 + L4	0.946	0.961	0.944	0.903	0.912	
L1 + L2 + L3 + L4 + L5	1.000	1.000	1.000	1.000	1.000	

Notes: Panel A presents weighted OLS regression results. For discussion of the weights used, see Maitra et al. (2024). Panel B presents the implied (cumulative) benefit shares of households with less than the specified cultivable landholding, where the benefit share of land category j is calculated as $\frac{\omega_j \gamma_j}{\sum_j \omega_j \gamma_j}$, and γ_j denotes the estimated likelihood that a household in land category j receives the benefit, and ω_j is the proportion of sample households in land category j. The cumulative benefit share at category j is the sum of the benefit shares of all categories up to j. †: All Benefits 2014–2018 is the sum of new nationwide, pre-existing and new state benefits received during 2014–2018. Pre-existing Benefits exclude BPL card. Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

correlated with other measures of standard of living, such as education, assets, income and occupation.[13] Subsequently, we check that the results are robust to the use of other measures of economic status such as income, as well as targeting across social groups distinguished by caste and religion.

Panel A in Table 3 shows results of OLS regressions of the number of benefits received in each category on dummies for landownership groups, run separately for the two survey rounds. We cluster the standard errors at the village level and thus control for spatial correlation in benefits received. These estimates can be viewed as descriptive averages of the number of benefits received by households in each landholding category.

As we see, the coefficients decline as landholding increases, suggesting that benefits are generally distributed in a progressive manner. We can also use the estimated coefficients to calculate a Lorenz-based measure of progressivity (Biggs et al., 2009; Kakwani et al., 2021). Group j's benefit share is calculated as $\frac{\omega_j \gamma_j}{\sum_j \omega_j \gamma_j}$, where γ_j denotes the estimated number of benefits received by an average household in group j and ω_j is the fraction of sample households that are in group j. By the criterion of Lorenz-dominance, a distribution of benefits is deemed to be more progressive if poorer household groups receive a larger share of the benefits.

Panel B presents the corresponding cumulative benefit shares of households at different thresholds of landholding. New nationwide program benefits were distributed more progressively than benefits from new state programs or pre-existing programs after 2014: landless households, who are 19.5% of the sample, received 26% of all new nationwide benefits, whereas they received only 13% of new state benefits, and 22% of all pre-existing benefits. Moreover, pre-existing program benefit distribution became more progressive after 2014: whereas only 19% of landless households had received them during 2010–2013, 22% did so in 2014–2018.[14] When we consider all programs in aggregate, we continue to find that benefit distribution became more progressive during 2014–2018: compare columns 1 and 5; recall that during 2010–2013 only pre-existing benefit schemes were in operation.[15]

In Table 4, we use household social category as an indicator of social status. Households are classified as belonging to the Scheduled Tribes (ST) or Scheduled Castes (SC), Muslims or General Castes/Other Backward Castes. Once again, the estimates are descriptive averages of benefits received by each social group.[16] Panel B presents benefit

[13] Panel A of Table A1 in the Supplementary Appendix shows that landholding correlates positively with education, assets, income and farming occupation of the head. Among landless households there was only a 19% chance that the head had completed primary school, whereas this was 69% for households with more than 1.5 acres of land. Additionally, 62% of landless households resided in a *kuchcha* house compared to 38% of households owning more than 1.5 acres of land. Over the period 2010–2013, the average household income for households with more than 1.5 acres of landholding was above Rs. 100,000, more than twice the approximately Rs. 41,000 income of landless households.

[14] We find comparable patterns when we examine the corresponding cumulative proportion of benefits by income quantile in Table A3, in the Supplementary Appendix.

[15] Unfortunately, we do not observe the inclusion rules for each program, and just as importantly, do not have enough information about each household to determine their eligibility for each program. Therefore we cannot directly assess whether the improved targeting was the result of reduced elite capture or expanded coverage. Our analysis remains valid under the assumption that households care about the receipt or non-receipt of benefits, rather than the reasons for it.

[16] Social and economic status are positively correlated. As shown in Panel B of Table A1, ST/SC groups are the most deprived with respect to landownership, house type, education and occupation of the household head, while Muslims' status lies between ST/SC groups and all others. There is no clear ranking of

Table 4
Targeting of benefits: likelihood of receiving benefits and benefit shares accruing by social group. Aggregated benefits.

	2014–2018				2010–2013	Population
	All†	New nationwide	Pre-existing	New state	Pre-existing	Share
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: *Likelihood of Receiving (columns 1–5)*						
ST	2.483***	0.652***	1.685***	0.146*	1.665***	0.0477
	(0.299)	(0.121)	(0.193)	(0.075)	(0.196)	
SC	2.847***	0.742***	1.966***	0.139***	1.708***	0.2635
	(0.155)	(0.049)	(0.109)	(0.045)	(0.136)	
Non Hindu	1.250***	0.435***	0.725***	0.091***	1.399***	0.1626
	(0.131)	(0.065)	(0.097)	(0.022)	(0.237)	
General Caste/OBC	1.656***	0.344***	1.199***	0.114***	1.578***	0.5262
	(0.149)	(0.027)	(0.132)	(0.030)	(0.117)	
Number of Households	3500	3500	3500	3500	3500	
R-squared	0.603	0.388	0.559	0.113	0.631	
Panel B: *Share of Benefits Accruing*						
ST	0.061	0.065	0.060	0.059	0.050	
SC	0.386	0.409	0.385	0.310	0.284	
Muslim	0.105	0.148	0.088	0.125	0.143	
General Caste/OBC	0.448	0.378	0.468	0.507	0.523	

Notes: Panel A presents weighted OLS regression results. For discussion of the weights used, see Maitra et al. (2024). Panel B presents the weighted proportion of households in social group g that receive benefits of each type, given by $\frac{\omega_g \gamma_g}{\sum_g \omega_g \gamma_g}$ where γ_g is the raw proportion of benefits accruing to households in social group g and ω_g is the proportion of sample households in social group g. †: All Benefits 2014–2018 is the sum of new nationwide, pre-existing and new state benefits received during 2014–2018. Pre-existing Benefits exclude BPL card. Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

shares, derived from a regression of household benefits on social group dummies in Panel A. During 2014–2018 the SC, ST and Muslim groups received larger shares of the new nationwide program benefits than of the new state or the pre-existing benefits. The SC and ST benefit shares of pre-existing programs were higher in 2014–2018 than in 2010–2013, although the benefit shares of Muslims were lower.

We thus arrive at similar conclusions both when examining targeting by land ownership and by social groups. Targeting improved after 2014 in two ways. *First*, the new nationwide programs were (on average) better targeted than pre-existing programs. *Second*, pre-existing programs became better targeted to SC and ST groups after 2014 (Muslims are the sole exception). At the same time, the new state programs were less successful at targeting the poor than either the pre-existing or the new nationwide programs.[17]

4. Impacts on political competition

In the rest of the paper, we examine how households' support for different political parties responded to their receipt of different types of benefits, and subsequently use these estimates to calculate net induced changes in the overall support for the BJP or TMC. To place the analysis in context, we start by describing the political background in West Bengal over the past forty years.

4.1. Background: Political competition in West Bengal

Between 1977 and 2011, West Bengal politics was characterized by competition between the INC/TMC and the Left Front. However the scales shifted progressively in favour of the TMC in the first two decades of this century. The TMC won an absolute majority in the state legislature for the first time in 2011, and gained control of most district and village councils in 2013. Historically, the BJP has had a negligible presence in the state: it received 6% of votes and won only one of the 42 parliamentary seats from West Bengal in the 2009 parliamentary elections (see Fig. A2 in the Supplementary Appendix). However that has been changing since. In the 2014 parliamentary elections, the BJP won two seats and its vote share increased to 17%. The striking change occurred in the 2019 parliamentary elections when the BJP won 18 seats and more than 40% of votes, thereby becoming the second-most important political party in the state.[18]

The rising popularity of the BJP has been accompanied by a decline in the fortunes of the Left Front, whose vote share fell from 30% in 2014 to under 10% in 2019. Fig. A3c in the Supplementary Appendix shows that the BJP's large vote shares in 2019 were concentrated in areas where the Left Front was dominant in the 2009 parliamentary elections (Figure A3a). This change has sometimes been described as *Baam theke Ram* ([movement] from the Left to Ram, the Hindu God). When we consider the three parliamentary elections between 2009 and 2019, we see that this movement occurred in two steps: support shifted from the Left Front to the TMC in 2014 (compare Figs. A3a and A3b), and then from the TMC to the BJP in 2019 (compare Figs. A3b and A3c). However, the longitudinal household data we discuss below show more complex shifts at play; in particular, there were similarly large flows of voter support from the Left to the TMC as from the TMC to the BJP.

4.2. Survey data on support for different political parties

In Fig. 1 we plot the shares of households supporting each party in the straw polls conducted in the 2013 and 2018 survey rounds. Consistent with the pattern in the state as a whole, our sample shows that support for the Left Front decreased, and support for the BJP increased. The increase in the BJP's share from 4 to 21% was similar to, although smaller than, the increase in the actual vote share in the polling booths in our sample villages between the 2014 and 2019 parliamentary elections

deprivation between the SC and ST groups: whereas on average SC households have lower household incomes, ST households are more likely to live in a non-permanent (*kuchcha*) house. The average Muslim household in our sample is better off than the SC/ST groups: both in terms of housing material and in terms of land ownership. General Caste/OBC households are the least deprived group in our sample.

[17] When we examine the targeting patterns of the individual benefits in Table A4 in the Supplementary Appendix, we find that there are specific exceptions to each of these two general conclusions.

[18] The BJP has gone on to win the 2026 state assembly elections, and currently runs the West Bengal state government.

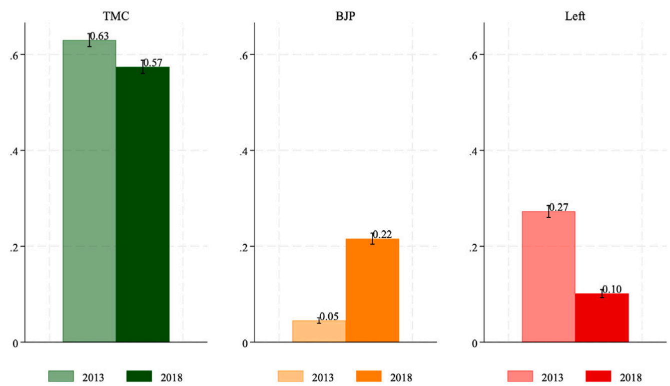

Fig. 1. Support for different parties in the survey poll. **Notes:** Proportion of households supporting the Left, BJP and TMC in the straw polls conducted in 2013 and 2018 with a 90% confidence interval presented. The sample is weighted (see Maitra et al., 2024) to ensure representativeness.

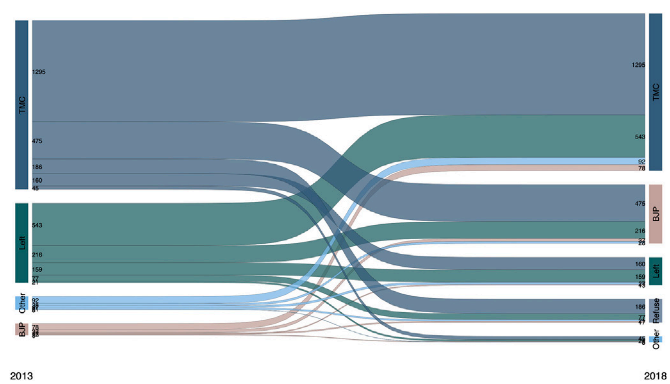

Fig. 2. Household level change in support for different political parties in survey poll 2013–2018. **Notes:** Sample is weighted (see Maitra et al., 2024) to ensure representativeness.

(35 percentage point increase, Fig. A4c), in our sample districts (31 percentage point increase, Fig. A4b), and in West Bengal more broadly (23 percentage point increase, Fig. A4a). The change in our household sample could be smaller because our polls were conducted a few months before the actual elections, with voters' support continuing to evolve in-between.

The longitudinal nature of our dataset allows us to examine how individual voters' political leanings changed between 2013 and 2018. The Sankey diagram in Fig. 2 shows that the shift in political fortunes was not merely caused by Left Front supporters switching their support to the BJP. In fact, a larger fraction of Left Front supporters switched their support to the TMC. Part of the BJP's gains also came from former TMC supporters. Thus the (relatively) stable share of the TMC masks both losses to the BJP and gains because Left supporters switched allegiance to the TMC. This becomes relevant when we consider the role of changes in welfare benefit programs in the changing support for the different political parties.

Table 5 presents OLS regressions of the likelihood that households shifted their political support between the 2013 and 2018 survey rounds, on changes in the number of benefits they received in the four years prior. The dependent variable measures the likelihood that household i that voted for party p in 2013 continued to support p in 2018 (or changed its support to p' in 2018). We consider the following possibilities: continuing to support the TMC, continuing to support the Left Front, switching support from the Left Front to the BJP, switching support from the TMC to the BJP, and switching support from the Left Front to the TMC. We ignore other possibilities because only about 10% of households supported the Left Front in 2018, and less than 5% supported the BJP in 2013.

The results in Table 5 suggest that the delivery of new state benefits created large gains for the TMC: households that received these benefits were 21.7 percentage points more likely to maintain support for the TMC than those who did not. They were also less likely to switch support from the TMC to the BJP. If they were formerly Left Front supporters, they were less likely to maintain that support, but were also less likely to switch support to the BJP. Interestingly, households that received an additional new nationwide benefit were (weakly) more likely to maintain support for the TMC.

Finally, the receipt of an additional pre-existing scheme benefit increased the likelihood that households switched support from the Left Front to the TMC by a statistically significant 2.1 percentage points, and reduced the likelihood that they switched from the TMC to the BJP by 1.3 percentage points.

4.3. Political support: specification and identification strategy

To build a conceptual framework for our empirical analysis, we draw upon the probabilistic-voting-cum-hierarchical-budgeting model of two party political competition in Bardhan et al. (2024). Consider the indicator variable S_{ivdt}, denoting the choice of the head of household i in village v in district d, to support a specific political party in an election in year t. This choice depends on a vector (denoted $\{B^k_{ivdt}\}_k$) of welfare benefits from different schemes (indexed by $k = 1, 2, ..$) that the household received between t and the year of the previous election $t' < t$. It also depends on which political party it attributes these benefits to. Thus we can write:

$$S_{ivdt} = \sum_k \beta_{kt} B^k_{ivdt} + \gamma_t \mathbf{X}_{ivd} + \epsilon_{ivdt} \quad (1)$$

where $\mathbf{X}_{ivd}$ denotes a set of observable household characteristics which are fixed over time. Note that the regression coefficients are allowed to be time-varying, to capture possible changes in responsiveness to benefits because of the different ways in which voters assign credit or differing party loyalties across different survey rounds.

The error term in Eq. (1) is given by

$$\epsilon_{ivdt} = \Psi_{ivd} + \tilde{\epsilon}_{ivdt} \quad (2)$$

where Ψ_{ivd} is a set of time-invariant latent household characteristics, which are unobservable to the researcher but may be observed by local government officials who allocate benefits. This includes the household's party loyalties, and its evaluation of policy attributes of competing parties on dimensions other than welfare benefits. The term $\tilde{\epsilon}_{ivdt}$ denotes an IID shock uncorrelated with benefits or household characteristics.

Since the unobserved household characteristics Ψ_{ivd} could be correlated with both the receipt of benefits and the household's voting propensity, OLS estimates of the regression (1) are likely to be biased. *A priori* the direction of this bias is hard to predict. It would be positive if benefits are delivered by incumbents primarily to loyal voters who would vote for them, regardless of whether they receive benefits. On the other hand, less secure incumbents may prioritize delivery to swing voters over loyal voters, in which case the bias would be negative.

If we additionally assume the regression coefficients do not vary across years ($\beta_{kt} = \beta_k, \gamma_t = \gamma$), we can estimate the causal impact of benefit receipt on political support by exploiting the household panel and estimating the first difference version of Eq. (1):

$$\Delta S_{ivdt} = \sum_k \beta_k \Delta B^k_{ivdt} + \Delta\tilde{\epsilon}_{ivdt} \quad (3)$$

where Δ denotes the difference between the two election rounds. The OLS estimate of β_k would be unbiased since the error term $\Delta\tilde{\epsilon}_{ivdt}$ no longer depends on households' latent characteristics, and is uncorrelated with the change in households' receipt of benefits between the two election years.

However, the assumption of time-invariant regression coefficients may be too strong. For example, it rules out the possibility that voters

Table 5
Changes in political support: OLS estimates.

	Stay TMC (1)	Stay left (2)	Left to BJP (3)	TMC to BJP (4)	Left to TMC (5)
Number of New Nationwide Benefits Received	0.036*	−0.013	0.006	0.009	0.008
	(0.022)	(0.010)	(0.009)	(0.013)	(0.011)
Number of New State Benefits Received	0.217***	−0.022*	−0.058***	−0.096***	−0.047
	(0.053)	(0.013)	(0.013)	(0.022)	(0.036)
Change in Number of Pre-existing Benefits Received	0.001	0.004	−0.002	−0.013**	0.021***
	(0.008)	(0.004)	(0.003)	(0.005)	(0.006)
Constant	0.432***	0.006	0.081**	0.183***	0.138***
	(0.065)	(0.029)	(0.034)	(0.051)	(0.043)
Number of Households	3500	3500	3500	3500	3500
R-squared	0.052	0.025	0.028	0.060	0.028
TMC support in 2013	0.617			0.617	
Left support in 2013		0.290	0.290		0.290
Equality of Effects:					
Change in New Nationwide *vs* Change in Pre-existing	0.035	−0.017	0.008	0.022	−0.013
	(0.025)	(0.012)	(0.010)	(0.014)	(0.014)

Notes: The table presents results from OLS regression on the voting transitions as discussed in Fig. 2. The dependent variable is either the likelihood that i who voted for party p in 2013 continued to support p in 2018 or switched support to a different party p' in 2018. The categories considered are: Staying with TMC, Staying with Left, Left to BJP, TMC to BJP and Left to TMC. Controls for religion, caste, migration status, gender, marital status, education and occupation of the household head, household size and an indicator for residence in Hugli district are also included. Regressions are weighted for representativeness (see Maitra et al., 2024). Standard errors in parentheses are clustered at the village level. Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. See Table A5 for a more aggregated version of these voting transitions.

became less responsive to benefits which they traditionally attributed to the TMC. This could be the consequence of aggressive political advertising by the national government claiming credit for these benefits themselves, or the reforms' restrictions on the (TMC-dominated) local governments' ability to opportunistically manipulate benefit allocations. Indeed, this paper attempts to examine whether this actually happened. Accordingly, we need to estimate two separate cross-section regressions for the two poll rounds, and find other ways of addressing the endogeneity problem.

For this we rely on the hierarchical benefit delivery model in Bardhan et al. (2024). Given a vector of per household project budgets $\{B^k_{vdt}\}_k$ allocated by the district d government to village v, the incumbent controlling the local government in v allocates benefits across households in the village to maximize its chances of being elected in the year t election. The resulting *intra-village benefit allocation* can be written as:

$$B^k_{ivdt} = \zeta_{1kt} B^k_{vdt} \times X_{ivd} + \zeta_{2kt} B^k_{vdt} \times \Psi_{ivd} + \mu^k_{ivdt} \tag{4}$$

where μ^k_{ivdt} denotes an error term uncorrelated with household characteristics or ϵ_{ivdt}. Conditional on the village project budgets, Eq. (4) shows one source of endogeneity bias: intra-village benefit allocations are correlated with unobserved household characteristics Ψ_{ivd}, which directly influence voting propensities.

An additional source of endogeneity arises from district government officials' ability to manipulate inter-village program budgets. Let X_{vd} and Ψ_{vd} denote village v aggregates of the observed and unobserved household characteristics respectively. The Bardhan et al. (2024) model shows the budget allocated to village v can be expressed as:

$$B^k_{vdt} = T^k_{vdt} + \eta^k_{dt} \tag{5}$$

where T^k_{vdt} depends on its village-level characteristics, observed and latent (X_{vd} and Ψ_{vd}, respectively) in conjunction with political alignment between parties controlling the district and local governments, and political competition in the relevant electoral constituencies. η^k_{dt} is related to the district average program budget B^k_{dt} as follows (where $n_{v'd}$ denotes the district population share of village v'):

$$\eta^k_{dt} = B^k_{dt} - \sum_{v' \in d} n_{v'd} T^k_{v'dt} \tag{6}$$

This is an accounting identity ensuring that the average village budget given by Eq. (5) equals B^k_{dt}. Note that T^k_{vdt} and hence the budget allocated to the village depends partly on its unobserved latent characteristics Ψ_{vd} which affect the voting propensities of its residents. This would bias OLS estimates of the benefit responsiveness parameters ν_{kt} in the inter-village version of Eq. (1), where village-level vote shares S_{vdt} are regressed on per capita village benefit allocations:

$$S_{vdt} = \sum_k \beta_{kt} B^k_{vdt} + \gamma_t X_{vd} + \epsilon_{vdt} \tag{7}$$

To address this bias in the inter-village regression given by Eq. (7), we follow Levitt and Snyder (1997) and construct a "leave-out" variable to instrument for the village v budget B^k_{vdt}. This is the per capita welfare budget of *other* villages in the district, and is given by:

$$L^k_{vdt} \equiv \frac{1}{1 - n_v} \sum_{v' \in d, v' \neq v} n_{v'd} B^k_{v'dt} \tag{8}$$

Following Levitt and Snyder (1997), this leave-out variable is a valid instrument under the assumptions that:

Assumption 1. T^k_{vdt} are uncorrelated across v.

and

Assumption 2. The district effect η_{dt} is uncorrelated with T^k_{vdt}.

If we include district fixed effects as controls, then Assumption 1 requires that, conditional on district fixed effects, the latent characteristics across villages are mutually independent. We discuss this assumption in more detail further below.

However, Assumption 2 is clearly violated. We can see this in Eq. (6), since village v is included in the averaging procedure in the term $\sum_{v' \in d} n_{v'd} T^k_{v'dt}$, with a weight equal to the village's population share n_{vd}. As Levitt and Snyder would argue, if the district has a large number of villages, then each village's population share is small, and the assumption is approximately valid, making the bias of the leave-out (LO) IV estimate of the parameters ν_{kt} in Eq. (7) "negligible". On the other hand however, as the number of villages per district grows arbitrarily large, the LO instrument converges to the district average budget, and so has limited power to predict cross-village budgetary allocations. Thus the reduced bias is counter-balanced by reduced precision, so that there is no clear ranking of the mean square error of the OLS and LO IV estimates. In practice, the first stage equations can be used to gauge the predictive power of the LO estimates.[19]

If the instrument is not weak, the LO IV estimate is an useful alternative to the OLS estimate because it is less biased. Moreover, we can infer the direction of the OLS bias by comparing the LO IV and OLS estimates. If political competition is weak, the secure incumbent is more likely to distribute benefits to loyal voters, so that the bias is positive (OLS > IV). Conversely, if political competition is strong, then the weak incumbent might distribute benefits to less loyal voters, hoping they would gain their support in this way. In that case, the bias would be negative (IV > OLS) and the IV estimate would be larger than the OLS estimate.

Accordingly, in our empirical analysis we present both OLS and LO IV estimates. As we run the voting regressions at the household level (as in Eq. (1)) rather than at the village level, we extend the LO IV to the household level by interacting the LO IV at the village level (defined in Eq. (8)) with X_{ivd} the set of observable (pre-determined) household characteristics. Since these characteristics directly affect household political support, we also include these as controls in the IV regression.

4.3.1. Adjustment of leave out instrument when district fixed effects are included

Assumption 1 states that within a district, village-level variation in the per-household welfare budget is independently distributed. However, as Ellis and Jasperson (2024) argue, if all villages in a district have the same population, the LO IV estimator of Eq. (7) reduces to the OLS estimator. To see this, observe that Eq. (8) implies that:

$$(1 - n_v)L^k_{vdt} + n_v B^k_{vdt} = B^k_{dt} \tag{9}$$

If all N_d villages in district d have the same population $n_v = \frac{1}{N_d}$, then it follows from Eq. (9) that inter-village differences in the leave-out variable can be written as a function of the differences in their budgets:

$$L^k_{vdt} - L^k_{v'dt} = -\frac{1}{N_d - 1}[B^k_{vdt} - B^k_{v'dt}] \tag{10}$$

By including district fixed effects when running the regression per Eq. (1), we ensure that the OLS and IV estimates depend only on inter-village differences within each district. Then, Eq. (10) implies that the OLS and LO IV estimates are equal. Although this argument relies on the assumption of equal population shares across villages, it highlights the possibility that the LO IV estimator does not reduce the bias by much.

In response, we follow Ellis and Jasperson (2024)'s suggestion to modify the LO estimator to include a proper subset of villages in the same district, as well as villages from other districts. Our modified instrument includes an average of the per-household budget of other villages from the same block as the index village, and all villages from the other district in our dataset. By excluding certain villages from the same district, we ensure that inter-village differences in the instrument are no longer proportional to their respective budget differences. This is because the inter-village differences in the modified instrument are a convex combination of the corresponding differences in budget averages of the included and excluded groups of villages, which are uncorrelated with one another under Assumption 1. This ensures that this modified instrument has a smaller bias than the original leave-out instrument.

Since we include district fixed effects in our voting regressions, we also examine the robustness of the original leave-out estimates when they are replaced by the modified leave out estimates, which we term the EJ estimates.

5. Regression specification and hypotheses tested

As previously explained, households may attribute the benefits they received to different political parties at different times, so that the estimated effect of benefits on political support varies from one survey round to another. We allow this by running two separate cross-sectional regressions of political support. Based on Eq. (1), for each survey round t and party p, we estimate the following regression:

$$\begin{aligned} S_{ivpt} &= \beta_{npt}\text{New Nationwide Benefits}_{it} + \beta_{spt}\text{New State Benefits}_{it} \\ &\quad + \beta_{ept}\text{Pre-existing Benefits}_{it} + \gamma_{pt}\mathbf{X}_{iv} + \varepsilon_{ivpt} \\ &\text{for } t = \{2013, 2018\}; \text{and } p = \{\text{TMC, BJP, Left}\} \end{aligned} \tag{11}$$

The dependent variable $S_{ivpt} = 1$ if household i in village v supports party p in survey round t and 0 otherwise. The vector $\mathbf{X}_{iv}$ represents household socio-economic characteristics that affect intrinsic party-specific loyalties. Other idiosyncratic preferences are represented by the residual ε_{ivpt}. New Nationwide Benefits$_{it}$, New State Benefits$_{it}$ and Pre-existing Benefits$_{it}$ denote the number of benefits in each category that household i received during the periods 2010–2013 and 2014–2018. Note that by construction, the variables New Nationwide Benefits and New State Benefits take the value zero in the survey period 2010–2013.[20]

This allows us to test the following hypotheses.

Hypothesis 1.

1a Receipt of an additional new nationwide program benefit during 2014–2018 increased the average household's support for the BJP and decreased its support for the TMC in 2018.
1b Receipt of an additional new state program benefit during 2014–2018 increased the average household's support for the TMC and decreased its support for the BJP in 2018.
1c Receipt of an additional pre-existing program benefit during 2014–2018 increased the average household's support for the TMC and decreased its support for the BJP in 2018.

We have also described how the welfare reform after 2014 attempted to restrict local officials' discretion in allocating pre-existing state benefits. Accordingly, we hypothesise that:

Hypothesis 2.

Benefits from pre-existing schemes became less effective at generating support for the TMC between 2013 and 2018.

Finally, we hypothesise that the observed changes in political support for the BJP and TMC can be linked to the changes in welfare programs.

Hypothesis 3.

The observed changes in support for the BJP and TMC between 2013 and 2018 can be explained by changes in the scale and composition of welfare benefits received, and in households' responsiveness to these benefits.

[19] If our argument above is correct, then we would expect a household's benefits to correlate negatively with the benefits received by the average household in other villages in the same district. We see this pattern in the first stage regressions presented in Table B1, where the coefficients on the diagonal terms are negative and significant (see columns 1, 3 and 5). For instance, one additional new nationwide benefit received by the average household in other villages is associated with 0.58 fewer new nationwide benefits for the average household.

[20] Recall that to be eligible for some schemes, such as *Ujjwala* and *Swachchh Bharat*, households must have a BPL card, i.e., a card certifying that their income is below the poverty line. In this way, BPL cards are a gateway to other benefits, in addition to being a benefit in and of themselves. We examine the robustness of our findings to excluding BPL cards from the set of pre-existing benefits. The results (available on request), are both qualitatively and quantitatively similar to those in Table 6.

Table 6
Political support in 2013 and 2018. Cross-sectional regression results: aggregate benefits.

	TMC			BJP			Left		
	OLS	LO	EJ	OLS	LO	EJ	OLS	LO	EJ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: *Political Support 2018*									
New Nationwide 2014–2018	0.017	−0.029	−0.021	0.027	0.047	0.096	−0.009	−0.023	−0.077
	(0.020)	(0.053)	(0.086)	(0.017)	(0.045)	(0.079)	(0.012)	(0.042)	(0.055)
New State 2014–2018	0.181***	0.276***	0.195**	−0.160***	−0.219***	−0.260***	−0.010	−0.015	0.034
	(0.036)	(0.051)	(0.084)	(0.027)	(0.051)	(0.054)	(0.018)	(0.034)	(0.036)
Pre-existing 2014–2018	0.046***	0.056***	0.027	−0.035***	−0.061***	−0.020	−0.007	−0.008	−0.019
	(0.010)	(0.021)	(0.036)	(0.009)	(0.021)	(0.035)	(0.006)	(0.014)	(0.017)
Constant	0.521***	0.497***	0.555***	0.339***	0.388***	0.319***	0.007	0.011	0.031
	(0.076)	(0.084)	(0.094)	(0.067)	(0.078)	(0.085)	(0.041)	(0.049)	(0.057)
Number of Households	3500	3500	3500	3500	3500	3500	3500	3500	3500
R-squared	0.057	0.051	0.052	0.083	0.073	0.063	0.027	0.026	0.001
Mean Support in 2018	0.573			0.225			0.101		
First Stage F									
New Nationwide		79.56	24.12		79.56	24.12		79.56	24.12
		[0.00]	[0.00]		[0.00]	[0.00]		[0.00]	[0.00]
New State		1128.94	139.35		1128.94	139.35		1128.94	139.35
		[0.00]	[0.00]		[0.00]	[0.00]		[0.00]	[0.00]
Pre-existing		75.68	20.68		75.68	20.68		75.68	20.68
		[0.00]	[0.00]		[0.00]	[0.00]		[0.00]	[0.00]
Equality of Effects									
New Nationwide = Pre-existing	−0.029	−0.085	−0.048	0.062***	0.108***	0.116	−0.003	−0.015	−0.058
	(0.026)	(0.061)	(0.106)	(0.021)	(0.052)	(0.095)	(0.015)	(0.045)	(0.058)
Panel B: *Political Support 2013*									
Pre-existing 2010–2013	0.018	0.015	0.024	0.003	0.010	0.013	−0.028**	−0.042	−0.062**
	(0.012)	(0.035)	(0.034)	(0.003)	(0.008)	(0.010)	(0.012)	(0.033)	(0.031)
Constant	0.646***	0.652***	0.635***	0.028	0.013	0.009	0.294***	0.321***	0.361***
	(0.072)	(0.097)	(0.096)	(0.034)	(0.038)	(0.040)	(0.064)	(0.089)	(0.087)
Number of Households	3500	3500	3500	3500	3500	3500	3500	3500	3500
R-squared	0.039	0.039	0.038	0.028	0.026	0.025	0.045	0.044	0.038
Mean Support in 2013	0.617			0.044			0.290		
First Stage F									
Pre-existing		244.42	13.96		244.42	13.96		244.42	13.96
		[0.00]	[0.00]		[0.00]	[0.00]		[0.00]	[0.00]

Notes: Coefficients are estimated from year-specific regressions run following the specification in (11). In Panels A and B the dependent variables are the household's support for the TMC (Columns 1–3), the BJP (Columns 4–6) and the Left Front (Columns 7–9), as elicited in the poll in the 2018 and 2013 surveys, respectively. LO and EJ denote the "leave out" and the Ellis and Jasperson (2024) instruments described in the text. Controls for religion, caste, migration status, gender, marital status, education and occupation of the household head, household size and an indicator for residence in Hugli district are also included. Regressions are weighted (Maitra et al., 2024) for representativeness. Standard errors in parentheses are clustered at the village level. Significance: $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$. $p-values$ for first stage F-statistics are in square brackets.

6. Regression results: political support

In Table 6, we present OLS as well as the original (LO) and the Ellis-Jasperson modified (EJ) versions of the "leave out" IV estimates for Eq. (11). The results for the 2018 survey round are shown in Panel A and those for 2013 are in Panel B.[21] The first-stage F-statistics for all instruments exceed 10; thus, the instruments are not weak.[22] In most cases, the IV estimates are larger than the corresponding OLS estimates, indicating that the OLS estimates are biased toward zero, possibly reflecting strategic attempts by weak incumbents to direct benefits toward swing voters and away from loyal voters.

These regressions allow us to examine Hypothesis 1, which describes the contrasting effects of different types of benefits on support for the BJP and TMC. As we see in Panel A, in 2018 the receipt of nationwide benefits has a positive effect on support for the BJP, and ranges from 2.7 to 9.6 percentage points (12–42.7% of the average support for the BJP in 2018 in the sample), but is statistically insignificant in all specifications. The effects on TMC support are similarly insignificant, but range from −2.9 to 1.7 percentage points (−5.1 to 3% of the average support for the TMC in 2018). We do not find conclusive evidence in support of Hypothesis 1a. With regard to new state programs, there is precise evidence supporting Hypothesis 1b: the OLS estimate shows that an additional new state benefit increased the average household's support for the TMC by 18.1 percentage points (31.6%; LO estimate 27.6 percentage points or 48.2%; EJ estimate 19.5 percentage points or 34%), and decreased its support for the BJP by 16 percentage points or 71.1% (LO estimate 21.9 or 97.3%; EJ estimate 26 or 115.5%). All of these coefficient estimates are statistically significant ($p < 0.01$). Finally, we obtain partial evidence supporting Hypothesis 1c with respect to pre-existing benefits. The OLS estimates show that receipt of an additional pre-existing benefit increased the average household's support for the TMC by 4.6 percentage points or 8% (LO estimate 5.6 percentage points or 9.8%), mainly at the expense of a reduction in support for the BJP

[21] Table A7 in the Supplementary Appendix presents the OLS regression coefficients for the demographic and socio-economic controls that are included in the set of explanatory variables. The corresponding LO and EJ results are available on request.

[22] The corresponding first stage regressions are presented in Table B1 in the Supplementary Appendix.

Table 7
Predicted effect of aggregate benefits on political support implied by cross sectional regression estimates.

		OLS				LO				EJ			
	Mean	Unit effect		Total effect		Unit effect		Total effect		Unit effect		Total effect	
	Q_t	TMC $\hat{\beta}_{t,TMC}$	BJP $\hat{\beta}_{t,BJP}$	TMC $Q_t \times \hat{\beta}_{t,TMC}$	BJP $Q_t \times \hat{\beta}_{t,BJP}$	TMC $\hat{\beta}_{t,TMC}$	BJP $\hat{\beta}_{t,BJP}$	TMC $Q_t \times \hat{\beta}_{t,TMC}$	BJP $Q_t \times \hat{\beta}_{t,BJP}$	TMC $\hat{\beta}_{t,TMC}$	BJP $\hat{\beta}_{t,BJP}$	TMC $Q_t \times \hat{\beta}_{t,TMC}$	BJP $Q_t \times \hat{\beta}_{t,BJP}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
New Nationwide 2018	0.475	0.017	0.027	0.008	0.013	−0.029	0.047	−0.014	0.023	−0.021	0.096	−0.010	0.046
New State 2018	0.118	0.181	−0.160	0.021	−0.019	0.276	−0.219	0.032	−0.026	0.195	−0.260	0.023	−0.031
Pre-existing 2018	1.693	0.046	−0.035	0.077	−0.060	0.056	−0.061	0.094	−0.103	0.027	−0.020	0.046	−0.034
Pre-existing 2013	1.618	0.018	0.003	0.029	0.005	0.015	0.010	0.025	0.017	0.024	0.013	0.039	0.021
Predicted Effect on Change in Vote Share													
New Nationwide				0.008	0.013			−0.014	0.023			−0.010	0.046
New State				0.021	−0.019			0.032	−0.026			0.023	−0.031
Pre-existing				0.048	−0.064			0.070	−0.120			0.008	−0.054
Total Predicted Effect (All Benefits)				0.077	−0.071			0.088	−0.123			0.021	−0.039
Actual Change				−0.056	0.171			−0.056	0.171			−0.056	0.171

Notes: $t = 2013, 2018$; so Q_j^{2013} and Q_j^{2018} denote the average number of benefits of type j received during the periods 2010–2013 and 2014–2018 respectively. Actual change in support for TMC and BJP from the poll, reported in Fig. 1. Predicted Effect on change in support for TMC is given by $\hat{\beta}_{TMC}^{2018} \times Q^{2018} - \hat{\beta}_{TMC}^{2013} \times Q^{2013}$; Predicted Effect on change in support for BJP is given by $\hat{\beta}_{BJP}^{2018} \times Q^{2018} - \hat{\beta}_{BJP}^{2013} \times Q^{2013}$. Predicted effects are computed using the OLS, LO and EJ estimates from Table 6.

by 3.5 percentage points or 15.6% (LO estimate 6.1 percentage points or 27.1%). These coefficients are statistically significant ($p < 0.01$). The corresponding EJ estimates are smaller in magnitude and have the same sign, but are imprecisely estimated.[23]

Although we do not obtain precise evidence of the effect of new nationwide benefits on BJP support, in the OLS and LO specifications we are able to reject the hypothesis that this effect was identical to that of the pre-existing benefits (see “Equality of effects” in Table 6, Panel A). In summary, we obtain partial evidence in support of Hypothesis 1.

Hypothesis 2 states that in 2018, an additional pre-existing benefit would generate a smaller increase in political support for the TMC than it did in 2013. To investigate this, we compare the coefficients on pre-existing benefits in 2018 (Panel A) with those in 2013 (Panel B). In contrast to the hypothesis, we see that the positive effect of pre-existing benefits on support for the TMC in 2018 (OLS estimate 4.6 percentage points; LO estimate 5.6; EJ estimate 2.7) was not smaller than in 2013 (OLS estimate 1.8 percentage points; LO estimate 1.6; EJ estimate 2.4). For a statistically rigorous comparison, in unreported regressions we run a stacked specification that pools data from 2013 and 2018, and estimate a difference-in-differences regression to examine whether the effect of pre-existing benefits is different in 2018 than in 2013. Using a one-sided test, we reject the null hypothesis that the effect is smaller in 2018 ($p = 0.07, 0.11$ and 0.16 in OLS, LO and EJ specifications respectively). Thus, the evidence does not support Hypothesis 2: there is no statistical support for the claim that pre-existing benefits became less effective at generating support for the TMC.

6.1. Predicted effects of welfare reforms on political support

Next, we examine Hypothesis 3, which posits that observed changes in the political support for the BJP and TMC can be explained by the changes in welfare programs. Whereas we have seen that pre-existing benefits became more effective at generating support for the TMC after 2014, it is also a fact that the scale of these programs shrank. Most notably, the proportion of households receiving NREGS fell from 50% to 42%. To account for these changes in opposing directions, we use the regression results from Table 6 to calculate overall effects, which are presented in Table 7. Relying first on the OLS estimates from Table 6, we find that changes in pre-existing programs raised support for the TMC by 4.8 percentage points and reduced support for the BJP by 6.4 percentage points between 2013 and 2018. The new state benefits increased support for the TMC by 2.1 percentage points, largely at the expense of the BJP. Finally, the new nationwide programs generated relatively small benefits for both the TMC (0.8 percentage points) and the BJP (1.3 percentage points). Aggregating across the three programs, the OLS estimates imply that the changes in welfare programs increased support for the TMC by 7.7 percentage points and reduced support for the BJP by 7.1 percentage points.

If instead we use estimates based on the LO instrument, the implied changes in support for the TMC are similar (an 8.8 percentage point increase) while the estimated reduction in support for the BJP is even larger (12.3 percentage points). The corresponding predictions based on the EJ estimates are more muted: a 2.1 percentage point increase in support for the TMC and a 3.9 percentage point decrease in support for the BJP.

As noted above, some of the point estimates on which these calculations are based are imprecisely estimated. To address concerns about the statistical validity of the prediction, we simulate the estimation procedure over 10,000 iterations using a cluster bootstrap method.[24] Fig. A5 in the Supplementary Appendix shows the kernel density estimates and the corresponding histograms of the combined effects predicted by the OLS, LO and EJ specifications. For the OLS estimates, the simulated predicted effects are *always* greater than the actual change in political support for the TMC, and *always* smaller than the actual change in support for the BJP between 2013 and 2018. Correspondingly, for the LO estimates, the predicted effects are larger than the actual change in support for the TMC in 96.7% of the simulations, and always smaller than the actual change in support for the BJP. The corresponding proportions for the EJ estimates are 84% and 99.9% respectively. Moreover, using the OLS, LO and EJ estimates, we predict that the welfare programs would have caused support for the BJP to decline in 100%, 99.8% and 80.6% of simulations, and support for the TMC to increase in 99.5%, 87% and 63.3% of the simulations respectively. In reality, support for

[23] The LO and EJ approaches make very different choices about what to leave out. There is, therefore, no nested estimation specification that allows us a statistical comparison of the different estimates. However, we compute the Bhattacharyya coefficients (Bhattacharyya, 1946) of distributional similarity using the bootstrapped OLS, LO and EJ estimates (with 10,000 replications). The coefficients are generally above 0.84, indicating high similarity. The two exceptions are the coefficients for the effects of pre-existing benefits in 2018 on political support for the Left (0.65) and the BJP (0.33).

[24] This involves re-sampling entire villages, and then re-running the analyses in Table 6, on each bootstrap sample, with village-level clustered standard errors. The resampling procedure corresponds to the “pairs cluster bootstrap” described in Cameron et al. (2008). Since our sample consists of 72 villages, we have sufficient clusters.

the BJP increased between 2013 and 2018, and support for the TMC decreased. It therefore seems unlikely that the actual changes in political competition that occurred after 2014 can be explained by the changes in the welfare programs.

6.2. Heterogeneity and robustness

In order to better understand underlying mechanisms and to examine the robustness of our conclusions, we also consider alternative specifications. Below we provide a summary of these findings. The empirical results are in the Supplementary Appendix. Since the OLS as well as the IV (both LO and EJ) results were qualitatively similar, hereafter, we only report findings from the OLS specification.

6.2.1. Heterogeneity of local government control

In addition to the party in power in the national and the state government, households may also be influenced by the political party that controls their *local* government, because it influences beneficiary selection and is in charge of the last-mile delivery of program benefits. After the 2013 village council elections, the TMC gained control in the local governments of 68 of our 72 sample villages. However, during 2010–2013 there was more heterogeneity: the Left Front was in charge in the majority of local governments in our sample, and only a minority were controlled by the TMC. This allows us to examine whether households were more responsive to pre-existing benefits when their local government was also controlled by the TMC. We do not find evidence for this (see Table A8 in the Supplementary Appendix) the effect of pre-existing benefits on support for the TMC in 2013 was not statistically different between areas where the local government was controlled versus not controlled by the TMC. In contrast, in 2018 both pre-existing benefits and new state benefits increased support for the TMC only in TMC-controlled areas, and not in areas controlled by the Left Front.

6.2.2. Heterogeneity by household landholding

Our main analysis considers the effect of benefit receipt on the average household. However, the same benefits may increase support for political parties more among less privileged households. If so, a program that is better targeted to the underprivileged would generate greater pay-off for the political party that delivers the benefits. To investigate this, in Fig. A6 in the Supplementary Appendix, we plot OLS estimates and corresponding 90% confidence intervals following the specification in Eq. (11), run separately for five different land categories. Indeed, in the case of pre-existing benefits during 2014–2018 (Figure A6c), we do confirm this pattern: these benefits increased support for the TMC and decreased support for the BJP to a statistically significant extent only among land-poor households (those with either no land or less than 0.5 acres). In all the other cases (Figures A6a, A6b and A6d) we find a non-monotonic pattern. When they received either new nationwide or new state benefits in 2014–2018, or pre-existing benefits in 2010–2013, it was mainly the recipients in intermediate land categories who increased support for the TMC, and decreased support for the BJP. This suggests that the TMC benefited from its focus on the non-poor, particularly in the case of new state benefits.

We also re-estimate the predicted changes in support for the different parties using OLS estimates that incorporate this heterogeneity. As with our previous results, this predicts an increase in support for the TMC of 5.9 percentage points during 2014–2018, and a reduction in support for the BJP of 6.1 percentage points, contrary to what occurred in reality. We obtain similar results when we instead incorporate heterogeneity across different social groups or income categories (see Figs. A7 and A8 in the Supplementary Appendix).

6.2.3. Heterogenous impacts across welfare programs

In the preceding analysis we measured benefits as simple counts of the number of schemes that households reported benefiting from. This implicitly assumes that households place equal value on the different benefits within a category, and attribute them to political parties in the same way. In Table A9 in the Supplementary Appendix we consider ten different benefit schemes separately (this includes three nationwide, two new state and five pre-existing programs). The regression coefficients vary considerably across programs in each round. In the 2018 survey round, housing benefits generated larger impacts on TMC political support than NREGS workfare and the receipt of housing benefits, while other programs had smaller, statistically non-significant impacts. Once again, we do not find support for Hypothesis 3.

6.2.4. Controls for changes in household economic circumstances

In the global context, it has been argued that the recent increase in political support for populist and right-wing nationalist parties is the result of increased economic insecurity (see, for example, Colantone and Stanig, 2018, 2019; Autor et al., 2020; Oliveira, 2022). To examine whether a similar force is at play in our sample, we investigate whether households recently affected by a negative income shock that received welfare benefits increased their support for the BJP more. However, as Tables A10 and A11 in the Supplementary Appendix show, the effect of welfare benefit receipt does not vary by the change in a household's economic circumstances, measured variously by changes in farm income, non-farm income, employment income, total household income, landholding or large livestock ownership.

6.2.5. Shy BJP supporters?

Whereas all sample households participated in the 2013 straw poll, 8.1% of households did not participate in the 2018 poll. It is possible that the refusal was driven by a hesitation to reveal support for the BJP. As a robustness exercise, we re-code non-participants as supporting the BJP, and re-estimate the OLS and IV regressions. As we see in Table A12 in the Supplementary Appendix, this does not materially affect our findings.

7. Concluding comments

To summarize, our analysis shows that the reforms changed how welfare programs were delivered in West Bengal: the new nationwide programs introduced after 2014 were better targeted than the programs that previously existed, and the pre-existing programs also became better targeted to the poor. However, we find no evidence that these changes contributed to the observed increase in households' support for the BJP. If anything, the opposite was true – they increased support for the TMC at the expense of the BJP. This was despite the introduction of large-scale new nationwide programs attributed to the BJP. Our analysis indicates this was partly because the TMC responded by introducing new state programs and received credit for them. In addition, households receiving pre-existing program benefits became more likely to support the TMC in 2018. This occurred despite reduced discretion of state and local officials, and a downsizing of these programs.

Our findings are consistent with a number of recent articles on West Bengal politics that draw attention to the TMC's political strategies. Dey and Sen (2020); Mahadevan and Shenoy (2023); Shenoy and Zimmermann (2026) all provide empirical evidence of political clientelism in West Bengal after the TMC gained majority control of the state government in 2011 and of local governments in 2013. Bhattacharya and Dasgupta (2023) suggest that in an environment of fluctuating and uncertain earnings, the structural characteristics of West Bengal's rural labour market and the low female labour force participation rate ensure that the electorate continues to rely on transfers from the state government to stabilize household incomes, and this allows clientelism to perpetuate. Nath (2022) provides data and ethnographic evidence that since 2011, the TMC has increasingly wooed the minority vote by encouraging religious and cultural celebrations and the use of traditional community-based dispute settlement mechanisms.

If welfare program changes do not explain the increasing vote share of the BJP after 2014, what does? We are unable to address this question satisfactorily with the data at our disposal. However when we examine whether the support for different political parties grew differently among households with different characteristics, we find suggestive evidence that ideological and social identity considerations played a role. For instance, even after controlling for welfare benefits received and household economic circumstances, general caste Hindu households were much more likely than non-Hindu households to support the BJP in the 2018 survey round. Data limitations prevent us from examining other possible sources of anti-TMC-incumbency, such as citizens blaming the TMC's leaders and party workers for corruption, or for governance failures with regard to security or dispute settlement.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary Appendix

Supplementary Appendix to this article can be found online at doi:10.1016/j.jpubeco.2026.105762.

Data availability

Data will be made available upon request.

References

Autor, D., Dorn, D., Hanson, G., Majlesi, K., 2020. Importing political polarization? The electoral consequences of rising trade exposure. Am. Econ. Rev. 110 (10), 3139–3183.

Banerjee, A., Hanna, R., Olken, B., Satriwan, E., Sumarto, S., 2023. Electronic food vouchers: evidence from an at-scale experiment in Indonesia. Am. Econ. Rev. 113 (2), 514–547.

Bardhan, P., Mookherjee, D., 2020. Clientelistic politics and economic development: an overview. In: Baland, J.-M., Borguigno, F., Platteau, J.-P., Verdier, T. (Eds.), The Handbook of Economic Development and Institutions. Princeton University Press.

Bardhan, P., Mitra, S., Mookherjee, D., Nath, A., 2024. How do voters respond to welfare vis-à-vis public good programs? Theory and evidence of political clientelism. Quant. Econ. 15 (3), 655–697.

Bhattacharya, S., Dasgupta, I., 2023. Economics and politics of some emerging fault lines in rural West Bengal. Econ. Polit. Wkly. LVIII (16), 25–28.

Bhattacharyya, A., 1946. On a measure of divergence between two multinomial populations. Sankhyā: Indian J. Stats. 401–406.

Biggs, A.G., Sarney, M., Tamborini, C.R., 2009. A progressivity index for social security. Issue Paper, (2009-01).

Cameron, A.C., Gelbach, J.B., Miller, D.L., 2008. Bootstrap-based improvements for inference with clustered errors. Rev. Econ. Stat. 90 (3), 414–427.

Casey, K., 2015. Crossing party lines: the effects of information on redistributive politics. Am. Econ. Rev. 105 (8), 2410–2448.

Colantone, I., Stanig, P., 2018. The trade origins of economic nationalism: import competition and voting behavior in Western Europe. Am. J. Polit. Sci. 62 (4), 936–953.

Colantone, I., Stanig, P., 2019. The surge of economic nationalism in Western Europe. J. Econ. Perspect. 33 (4), 128–151.

Dang, D.A., La, H.A., 2019. Does electricity reliability matter? Evidence from rural Vietnam. Energy Policy 131, 399–409.

de Jainvry, A., Gonzalez-Navarro, M., Sadoulet, E., 2014. Are land reforms granting complete property rights politically risky? Electoral outcomes of Mexico's certification program. J. Dev. Econ. 110, 216–225.

Deshpande, R., Tillin, L., Kailash, K.K., 2019. The BJP's welfare schemes: did they make a difference in the 2019 elections? Stud. Indian Polit. 7 (2), 219–233.

Dey, S., Sen, K., 2020. Do Political Parties Practise Partisan Alignment in Social Welfare Spending? Evidence From Village Council Election in India. Technical report, University of Warwick.

Drèze, J., Gupta, P., Khera, R., Pimenta, I., 2019. Casting the net: India's public distribution system after the food security act. Econ. Polit. Wkly. LIV (6), 36–47.

Ellis, C., Jasperson, J., 2024. Fixed effects and the “leave-out”. Instrument. Technical report, Working Paper. Tippie College of Business, University of Iowa.

Frey, A., 2019. Cash transfers, clientelism, and political enfranchisement: evidence from Brazil. J. Public Econ. 176 (C), 1–17.

Government of India, 2020. MGNREGA Sameeksha 2012. Technical report., Ministry of Rural Development.

Haseeb, M., Vyborny, K., 2022. Data, discretion and institutional capacity: evidence from cash transfers in Pakistan. J. Public Econ. 205 (2).

Joshi, B., Ranjan, A., Sircar, N., 10 Mar 2022. A Fundamental Shift in Electoral Behaviour. Technical report, Hindustan Times.

Kakwani, N., Wang, X., Xu, J., Yue, X., 2021. Assessing the social welfare effects of government transfer programs: some international comparisons. Rev. Income Wealth 67 (4), 1005–1028.

Larraguy, H., Marshall, J., Trucco, L., 2015. Breaking Clientelism or Rewarding Incumbents? Evidence From the Urban Titling Program in Mexico. Technical report, Department of Government, Harvard University.

Levitt, S.D., Snyder Jr, J.M., 1997. The impact of Federal spending on house election outcomes. J. Polit. Econ. 105 (1), 30–53.

Mahadevan, M., Shenoy, A., 2023. The political consequences of resource scarcity: targeted spending in a water-stressed democracy. J. Public Econ. 220, 104842.

Maitra, P., Mitra, S., Mookherjee, D., Motta, A., Visaria, S., 2017. Financing smallholder agriculture: an experiment with agent-intermediated microloans in India. J. Dev. Econ. 127, 306–337.

Maitra, P., Mitra, S., Mookherjee, D., Visaria, S., 2022. Evaluating the distributive effects of a micro-credit intervention. J. Dev. Econ. 158 (102896).

Maitra, P., Miller, R., Sedai, A., 2023. Household welfare effects of ROSCAs. World Dev. 169, 106287.

Maitra, P., Mitra, S., Mookherjee, D., Visaria, S., 2024. Decentralized targeting of agricultural credit programs: private versus political intermediaries. J. Eur. Econ. Assoc. 22 (6), 2648–2699.

Mashal, M., Kumar, H., 4 Dec 2023. With big state victories, modi expands his dominance in India. The New York Times.

MDWS, 2014. Guidelines for Swachchh Bharat Mission (Gramin). Technical report, Ministry of Drinking Water and Sanitation, Government of India.

Mitra, S., Mookherjee, D., Torero, M., Visaria, S., 2018. Asymmetric information and middleman margins: an experiment with West Bengal potato farmers. Rev. Econ. Stat. C (1), 1–13.

Muralidharan, K., Niehaus, P., Sukhtankar, S., 2016. Building state capacity: evidence from biometric smartcards in India. Am. Econ. Rev. 106, 2895–2929.

Muralidharan, K., Niehaus, P., Sukhtankar, S., 2023. Identity verification standards in welfare programs: experimental evidence from India. Rev. Econ. Stat. 1–46.

Nath, S., 2022. Politics of cultural misrecognitions and the rise of identity consolidations in post-left West Bengal. Econ. Polit. Wkly. LVII (40), 58–66.

Oliveira, W.F., 2022. Unpacking the Effect of Immigration on Elections. Technical report, Pontifical Catholic University of Rio de Janeiro (CPI/PUC-Rio).

Sedai, A.K., Nepal, R., Jamasb, T., 2020. Flickering lifelines: electrification and household welfare in India. Energy Econ. 104975.

Sen, R., 2019. Indian Elections 2019: Why the BJP Won Big. Technical report, Institute of South Asian Studies, National University of Singapore.

Shenoy, A., Zimmermann, L.V., 2026. Party organizations and complementary political control. J. Eur. Econ. Assoc. jvag041.

Stokes, S., 2005. Perverse accountability: a formal model of machine politics with evidence from Argentina. Am. Polit. Sci. Rev. 99 (3), 315–325.

Stokes, S., Dunning, T., Nazareno, M., Brusco, V., 2013. Brokers, Voters and Clientelism. Cambridge University Press, New York.