# How Do Voters Respond to Welfare vis-à-vis Public Good Programs?

## Theory and Evidence of Political Clientelism*

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**Abstract**

Using rural household survey data from West Bengal, we find that voters respond positively to excludable government welfare benefits but not to local public good programs, while reporting having benefited from both. Consistent with these voting patterns, shocks to electoral competition induced by exogenous redistricting of villages resulted in upper-tier governments manipulating allocations across local governments only for excludable benefit programs. Using a hierarchical budgeting model, we argue these results provide credible evidence of the presence of clientelism rather than programmatic politics.

Keywords: Clientelism; Public goods; Welfare programs; Voting
JEL Classification: H40, H75, H76, O10, P48.

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# 1 Introduction

Political clientelism is commonly believed to undermine the functioning of democracy in many middle and low-income countries.[1] Voters are effectively coerced to express political support for current incumbents by the threat of losing access to excludable government benefits, thereby diluting incentives of elected officials to be accountable and to deliver non-excludable public goods. Like most other forms of corruption, it is hard to detect, and it is equally difficult to provide reliable estimates of its systemic consequences.

The key defining characteristic of clientelism is that excludable benefits are allocated by incumbents to individual citizens *conditional* on their political support (**?**, Stokes et al. (2013)).[2] This is in contrast to programmatic politics, where, owing to strict eligibility based rules, delivery of private benefits to individual citizens is not subject to political discretion. As its name indicates, programmatic politics still leaves room for elected officials to exercise discretion over allocation of infrastructure programs across regions for electoral advantage or on the basis of past voting patterns (allowing 'pork-barrel politics' to prevail).

In this paper, we provide evidence of implicit *quid pro quo* arrangements between parties and individual citizens under clientelism by examining the pattern of benefit allocation across villages and the subsequent voting behavior of households upon receipt of these benefits. This approach is in contrast to various forms of indirect evidence provided in the comparative politics literature, such as the existence of local party brokers that mediate such trades on the basis of their personal connections with individual citizens (Stokes (2005), Kitschelt and Wilkinson (2007), Björkman (2014)), or positive correlations between citizen access to benefits and proximity to party networks and ideology (Stokes (2005), Stokes et al. (2013), Dunning and Nilekani (2013), Calvo and Murillo (2013)). While these forms of evidence are suggestive, they are also consistent with non-clientelistic forms of vote mobilization. For instance, Zarazaga (2014) conducts surveys of party brokers in Argentina and shows that many of their activities are consistent with programmatic politics, such as organizing political rallies and canvassing individual voters to advertise party policy platforms, or communicating information regarding voter preferences to party officials.[3] More-

[1] See Stokes (2005), Kitschelt and Wilkinson (2007), **?**, Stokes et al. (2013), Bardhan and Mookherjee (2020).

[2] This also distinguishes clientelism from vote-buying (unconditional pre-election gifts or bribes intended to influence votes via persuasion, gratitude or feelings of reciprocity) and turnout-buying (wherein party brokers subsidize or facilitate turnout of voter groups expected to vote in favor of their party).

[3] Schneider (2014) conducts surveys of party brokers in an Indian state and finds that they are not able to guess voters' political preferences over and above information contained in observable household attributes,

over, proximity of voter connections and ideology to parties are unlikely to be exogenous determinants of their access to government benefits, being subject to concerns for reverse causality or omitted variable bias. Attempts to overcome these identification problems using randomized field experiments are limited. A notable exception is Wantchekon (2003), which consists of an experiment in Benin that randomized policy platforms announced by competing Presidential candidates that differed with regard to promised private rather than public goods to different voter audiences. While the experiment showed that private good promises generated higher votes, this could be consistent with programmatic politics if voter preferences were biased in favor of private goods.

We focus on a context (rural West Bengal, in India) where prior research has provided evidence of benefits generated by public goods. For instance, Asher and Novosad (2020) use all-India data to show rural roads enabled citizens to gain better access to non-agricultural labor markets located outside their local areas. Chattopadhyay and Duflo (2004) show that in West Bengal, 85% of citizen complaints regarding local government programs concerned public works such as roads, irrigation and access to drinking water. We conduct surveys of household heads and ask them to name specific programs administered by local governments that they benefitted from in recent years. The list of government programs included a wide array of private (excludable) benefits and a number of public good programs, such as roads and irrigation. During 2004-2011, benefits reported per household were higher for public good programs than for private good programs, with roads being the single most reported benefit followed by employment programs. We also conduct a poll in which household heads privately cast ballots to express their political support for alternative political parties contesting elections in the local area. This data allows us to examine the relation between self-reported benefits received and political support expressed for incumbents, using an array of methods to isolate the causal impact of self-reported benefits on political support. Our approach therefore complements the analysis of Wantchekon (2003). The advantage is that it relies on direct evidence of the relation between (self-reported) private and public benefits and political support at the household level. In particular, by directly using data on self-reported benefits, it does not rely on any assumption regarding the relative valuation of private and public benefits. On the other hand, not being based on

contrary to what is assumed in accounts of clientelism. Auerbach and Thachil (2020) interview slum leaders in India and find that they have limited ability in monitoring co-partisan and co-ethnic voters and hence select clients who are better connected overall within the slum. This benefits the leaders' general reputation and is consistent with program politics where party brokers play an information gathering role within local communities.

a randomized controlled experiment, it is subject to causal identification concerns.

We address these concerns in two ways. First, we exploit a plausibly exogenous change in electoral competition owing to a redrawing of boundaries of electoral constituencies that occurred in 2006 and use a difference-of-difference analysis to examine how this resulted in a reallocation of public and private program budgets for local governments by higher level officials (reflecting perceptions of their relative effectiveness in generating votes). Second, we study the relation between benefits received and political support expressed at the individual household level, using instrumental variables and household fixed effects to control for potential endogeneity of benefits.

The analysis is grounded in a theoretical model of electoral competition that distinguishes between clientelism and programmatic politics on the basis of whether incumbents can make delivery of private benefits to individual voters conditional on their past political support. The model generates testable predictions, which help distinguish clientelism from programmatic politics. First, votes of individual citizens are affected only by receipt of current private benefits under clientelism, whereas in programmatic politics, they are affected by receipt of both private and public benefits. Second, in a hierarchical system of local government, clientelism affects the specific way in which incumbents are politically motivated to manipulate program grants to local governments in response to changes in political competition. Our model predicts that an exogenous increase in political competition will motivate upper tier incumbents to expand budgets to *aligned* local governments controlled by the same party and contract it for those controlled by the opposing party. The hypothesis of clientelism-based distortions then translates into a prediction that only private benefit programs will be manipulated in this fashion.

These predictions are tested using data collected from two rounds of a household survey in rural West Bengal, including retrospectively reported benefits received from various government programs in past years. Most of the analysis is based on data from the 2011 survey round, using benefits reported over seven previous years spanning 2004-2011. In West Bengal, as in most other states in India, the lowest tier of local government is the *gram panchayat* (GP) and the next upper-tier is the *panchayat samiti* (PS). The GP is responsible for allocating various private benefit programs to villages and households within their jurisdiction, in addition to the planning and administration of local infrastructure projects. The PS provides budgetary and technical approvals for these projects. This top-down hierarchical system provides considerable discretionary power to PS officials in project approvals and allocation of funds for different programs across GPs. This applies equally to (private)

welfare and infrastructure (local public good) programs. The welfare programs include different private benefits: employment, subsidized loans, farm inputs, low-income housing, sanitation and food items. The infrastructure programs involve construction of local public goods: primarily roads, and also irrigation and water programs. After reporting benefits received from various programs in past years, each respondent marked their preferred choice on a ballot containing the symbols of competing political parties in a private room and and dropped it into a sealed box. We show that the household ballot responses are positively correlated with actual vote shares of rival parties (aggregated at the corresponding constituency level for state assembly elections held the same year), suggesting they are a reasonable proxy for how households actually voted. The richness of data on receipt of different types of benefits as well as on proxy voting behavior allows us to test the relative effectiveness of private versus local public goods in generating votes for incumbents.

Our empirical analysis is carried out in two steps. In the first exercise, we examine variation in local-government program scales resulting from changes in political competition. Following Nath (2015), we isolate exogenous variation in political competition by utilizing the redrawing of boundaries between state legislative-assembly constituencies implemented in 2007 (and announced in December 2006) by a politically neutral State Delimitation Commission composed of members of the national judiciary. The Indian Constitution imposes many restrictions on the process to ensure that redistricting cannot be manipulated by political parties to extract partisan benefits, which Iyer and Reddy (2013) verify using data from two other Indian states. We find similar evidence for West Bengal using our data.

In our context, the redistricting information we use is whether and how a GP in our sample was reassigned from one assembly constituency to another. We begin our analysis by defining two treatment groups. Villages in both groups were redistricted to more competitive constituencies but varied in alignment. We focus on the electoral term 2004-2008, since alignment did not change over this period. To test the theoretical predictions, we use a difference-in-differences (DID) approach to compare changes in the treatment GPs with corresponding changes in the control group. To provide justification for the DID specification, we show that neither the treatment groups nor the control group differs significantly with respect to relevant village characteristics or variables reflecting possible motives for political manipulation (incumbency, representation in the Delimitation Commission or caste-based quotas). Moreover, we verify the absence of pre-2007 trend differences in benefit distributions, and check robustness with respect to controls for pre-trends.

With 'treatment' defined as redistricted to a more competitive constituency, and aggre-

gating receipts of different benefits, our results show that after 2007, villages in treated-nonaligned GPs experienced a 1.32 standard deviation (s.d.) smaller change in scale of private benefit programs compared to the control group, with a p-value of 0.11. At the same time, the gap between the changes in the two treated groups varying by alignment grew by 2.25 s.d (p-value 0.10). For employment benefits, the corresponding estimates of these two treatment effects are -1.88 s.d. (p-value .04) and 2.32 s.d. (p-value 0.03). For public benefits, in contrast, the corresponding differences were negligible (less than 0.01 s.d.) with p-values of 0.99. We also test a more demanding set of predictions of the theory with four treatment groups defined by redistricting through different combinations of competition changes and alignment. We check the robustness of these results to alternative definitions and measures of benefits, using clustering methods, and conducting placebo tests validating the underlying identification assumptions.

Our second empirical exercise examines how political support at the household level responded to the private and public benefits households received. An OLS regression of household support for the GP incumbent party in the 2011 survey shows that a one standard deviation increase in private benefits during the previous three years was associated with a 2% higher likelihood of supporting the incumbent party at the GP level, significant at the 10% level. On the other hand, reported household benefits (standardized) from a local road program in those years were associated with a statistically insignificant 1% decrease in support.

The OLS results are subject to possible reverse causality bias resulting from unobserved heterogeneity both within and across villages. For example, anticipated voting patterns can affect the allocation of benefits as incumbents could target loyal supporters (generating a positive bias), or they could target swing voters (generating a negative bias). To address these concerns, we provide two sets of results. First, we provide IV estimates for the 2011 survey round data using a 'supply-side' instrument for the scale of programs at the GP level: the average program scale in other villages in the same district, in the spirit of Levitt and Snyder Jr (1997). We interact these with fixed household characteristics such as, caste, landlessness, education, and religion (significant determinants of within-GP targeting), to predict the delivery of benefits to individual households. The resulting IV estimate of the effect of private benefits on household support for the incumbent turns out to be substantially larger than the OLS effect, amounting to a 13% higher likelihood of support for a one standard deviation increase in benefit (p-value less than .05). The corresponding IV estimate of the voting effect of a road benefit is negative and statistically

insignificant.

Second, we utilize an additional prior round of the household survey. This round was conducted in 2004 and provides data on political support expressed by household heads in 2004 and self-reported benefits over 2001-2004. As the same set of households were covered in the two survey rounds (with an attrition rate less than 1%), we pool data across the two survey rounds and re-examine the benefits-vote relationship while controlling for household fixed effects to address the possibility of endogenous targeting of benefits. The previous OLS results from the 2011 survey round turn out to be robust with respect to inclusion of household fixed effects.

In summary, the results at the household level mirror the patterns of manipulation of program scales at the GP level, in line with the predictions of the clientelistic model.

The rest of the paper is organized as follows. Section 2 describes related literature in more detail, while Section 3 describes the institutional context and data used. Section 4 presents the theoretical model. Section 5 presents the empirical results for the GP benefit scale and effects on household votes. Section 6 discusses the plausibility of alternative explanations, while Section 7 concludes.

## 2 Related Literature

In the previous section, we discussed how our paper is related to the literature on clientelism in comparative politics. This section focuses on related papers in political economy. Our focus on political clientelism contrasts with those studying social clientelism involving patronage of poor households by traditional elites, rather than political incumbents, in India and Pakistan (Anderson et al. (2015), Beg (2020)). It is also distinguished from studies of vote-buying involving unconditional pre-election gifts from political candidates to voters in the hope of swaying their votes (Gonzalez-Ocantos et al. (2012), Gonzalez Ocantos et al. (2014), Khemani (2015), Leight et al. (2019) and Vicente and Wantchekon (2009)). Our focus is on the public expenditure allocation consequences of political clientelism, rather than underlying enforcement mechanisms utilizing local brokers (Finan and Schechter (2012), Larreguy et al. (2016)).

The main contribution of our paper is to provide evidence for political clientelism by showing that voters respond differentially to the delivery of welfare programs than infrastructure programs. There is an extensive literature looking at political manipulation of funds for local infrastructure (Solé-Ollé and Sorribas-Navarro (2008), Brollo and Nannicini

(2012), Finan and Mazzocco (2016), Levitt and Poterba (1999), Stashko (2018)). Another set of papers examines the effects of specific private benefits programs on voter behavior in middle and low income countries (De La O (2013); Labonne (2013); Manacorda et al. (2011); Pop-Eleches and Pop-Eleches (2012); Brollo et al. (2020)). However, none of these papers compare voter responsiveness *across* private and public program benefits.

Levitt and Snyder Jr (1997) is the only paper we are aware of that compares voter responsiveness to delivery of welfare and infrastructure programs. They provide an IV estimate of US federal spending on votes in House districts, using as an instrument the level of spending in all other districts in the same state. We use a similar instrument in our household-level analysis and, like them, find a large discrepancy between OLS and IV effects. They find that a $100 increase in per capita spending on 'high variation programs' (including local infrastructure) resulted in a 2% increase in votes for the incumbent, while spending on 'low variation' programs (consisting of private transfer programs involving direct payments to citizens) resulted in a 0.2% reduction. This pattern is exactly the opposite of what we find. The discrepancy can be explained by the difference in institutional settings: in the US most private transfers are based on strict individual eligibility rules, and elected politicians exercise discretion mainly over inter-jurisdictional allocation of infrastructure funds. Hence there is no scope for the kind of clientelistic practices we argue prevailed in West Bengal.

Our result concerning the heterogenous impact of political competition on the allocation of private benefits across politically aligned and non-aligned regions in India is consistent with evidence found in Dey and Sen (2016), Gupta and Mukhopadhyay (2016) and Shenoy and Zimmerman (2020). In the context of intergovernmental transfer of funds between central and state/municipal governments, the importance of alignment in close elections is documented in Arulampalam et al. (2009) for India, in Brollo and Nannicini (2012) for Brazil, and in Corvalan et al. (2018) for Chile. There is also a related set of papers that provide evidence of ethnic favoritism or home bias of elected officials (Burgess et al. (2015), Hodler and Raschky (2014), Hoffmann et al. (2017)). These papers, however, focus on personal motives of upper-level officials rather than political incentives.

# 3 Context and Data

**Political Environment and Government Hierarchy.** During the period of our study (2004-2011), there were two principal political parties competing in West Bengal: the

**Table 1: Official Election Results and Post-Election Poll Responses**

| Party Vote Shares (%) | Official Election Results* | | Results from Poll Responses | |
|---|---|---|---|---|
| | 2006 | 2011 | 2004 | 2011 |
| TMC | 25.4 | 36.7 | 11.2 | 43.0 |
| Left Front | 51.4 | 43.4 | 57.7 | 32.1 |
| INC | 12.9 | 9.9 | 18.5 | 11.2 |
| Others | 10.3 | 10.0 | 4.6 | 1.5 |
| Voter Turnout (%) | 84.8 | 86.3 | | |
| Didn't Respond | | | 8.0 | 12.2 |

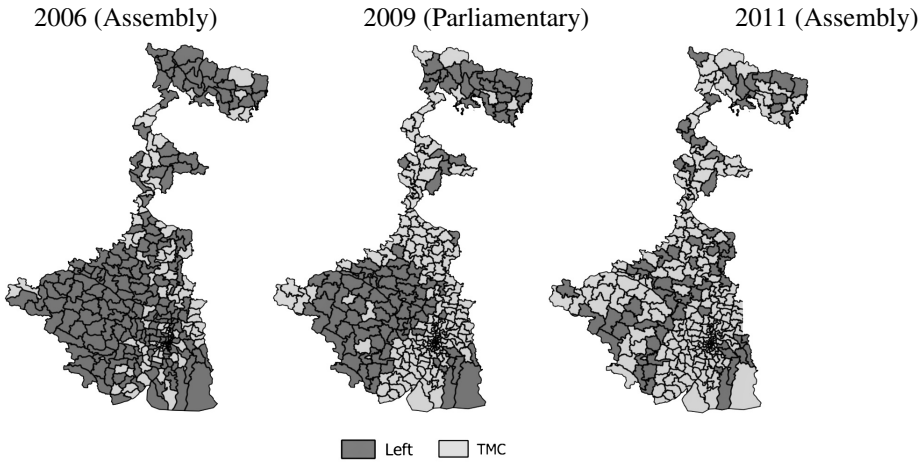
**Notes:** This table compares the changes in share of votes between 2004 and 2011 for main parties in the post-election straw survey poll with the changes in official vote shares between 2006 and 2011 assembly elections. The vote shares from the post-election survey polls are aggregated at the assembly-constituency level. The official election results are reported only for constituencies in which the survey was conducted.

Left Front (LF) coalition led by the Communist Party of India (Marxist) and the All India Trinamool Congress (TMC). The Left Front dominated elected offices corresponding to village, district, and state governments from 1977 to 2011 and lost its majority in the state assembly to the TMC in 2011. Table 1 presents official voting outcomes for the constituencies included in our sample. Between the 2006 and 2011 state assembly elections, the Left Front's vote share dropped from 51% to 43%, while the TMC's share rose from 25% to 37%.

Figure 1 shows how Left Front dominance progressively gave way to TMC dominance across successive elections in 2006, 2009 and 2011 at the assembly, parliamentary and assembly elections, respectively. Hence, it is reasonable to characterize our context as featuring electoral competition between two political parties.

Next we describe the structure of government. India is a federal state with legislative, administrative, and executive powers divided between the central and state governments. Each state has a hierarchy of administrative ministries and elected local government councils. A large range of benefit programs are administered, with upper-level governments raising the funds to pay for them and devolving spending authority to lower level governments. Program budgets flow down the hierarchy. District-level governments, *zilla parishads* (ZPs), allocate funds to middle-tier governments at the 'block' level, which comprises an elected body *panchayat samiti* (PS) and appointed bureaucrats in the Block Development Offices. The middle tier then allocates funds to bottom-tier *gram panchayats* (GPs) within its block. Finally, the elected GP bodies distribute benefits to households

**Figure 1: Changes in Electoral Outcomes for the Left Front**




**Note.** This figure plots voting outcomes at the assembly constituency level for three different elections, as indicated at the top of each map. The constituencies in dark gray were won by the Left Front party and the ones in light gray were won by the TMC party. The figure shows how Left Front dominance progressively gave way to TMC dominance across successive elections.

across and within villages in their jurisdiction. Whereas allocation of public goods across local governments in the US is discretionary and private benefits are based on strict eligibility criteria, both local public goods allocation and private benefits are discretionary in West Bengal and most other Indian states.

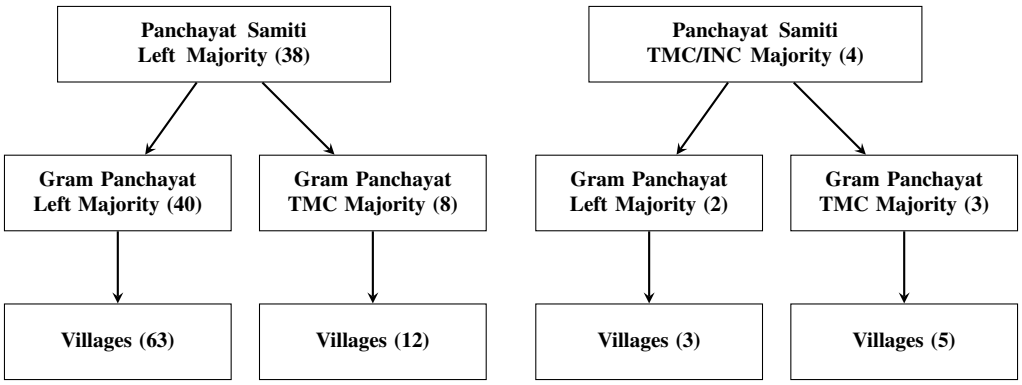
Our analysis focuses on the bottom two tiers: the PS at the block level, and the GP at the village level (see Figure 2). There are approximately 20 PSs in each district; each PS oversees roughly 10 GPs, and each GP allocates benefits among 10-15 villages. Each village in turn includes about 200-400 households. Council members and their chairpersons (Pradhans) are directly elected in each PS and GP. The area covered by a PS coincides or overlaps to a high degree with a state-assembly constituency, which elects a member of the Legislative Assembly (MLA) every five years. Figure A1 in the appendix overlays the map of PS boundaries with the map of AC boundaries. The median of the area overlap between a PS and GP is 87%, and the mean is 71%. During the period 2004-2011, state assembly elections were held in 2006 and 2011, and local government (PS,GP) elections in 2003 and 2008. Elections to the national Parliament were held in 2009. All elections are first-past-the post. Incumbency at the PS and MLA levels is positively correlated; for 70% of GPs in our sample, the corresponding MLA was from the same party that controlled the

PS. As Figure 2 shows, there are 45 PSs in our data, of which 39 were controlled by the LF. Political control at the PS tends to be positively correlated with political control at the GP level, but this correlation is not perfect. This results in variations in alignment of political control between the two tiers. Owing to the high overlap between assembly constituencies and PS jurisdictions in both area and political control, we treat them interchangeably; that is, we measure electoral competition by vote share differences at the assembly constituency level, and political alignment between the PS and GP levels.

During the period of study, GPs have very little autonomy over selection of development or welfare projects. Most programs they administered were 'centrally sponsored programs' on specific types of benefit programs which were created and largely funded by the central government, which filtered down from the central government to the state government and then down through the panchayat hierarchy at the district and block levels. GPs could request specific projects within the ambit of these programs to the relevant PS/ZP, but the ultimate authority for administrative, technical and financial approval was vested entirely in the PS/ZP. These administrative procedures are laid out in the Government of West Bengal Panchayat Accounts and Finance Rules of 2003. The lack of devolution of project choice to GPs has been noted by various State Finance Commissions as well as the World Bank. Further details of these are provided in Section A of the online Appendix. However, it is important to also note that the allocation of benefits across households within the GP was entirely devolved to the GP. These institutional details will be incorporated into the theoretical model in the next section.

**Survey Data.** We obtain information on benefits received by households and a proxy measure of their voting behavior from a household survey carried out in 2011. It included 2402 households from 89 villages in 57 GPs spread through all districts of West Bengal, except Calcutta and Darjeeling (owing to the relative lack of agricultural occupations in these two districts). The same households were surveyed in an earlier round in 2004. Most of the analysis in the paper relies only on data from the 2011 round, though some of the household level analysis will use data from both rounds. The villages were selected by the state department of agriculture to carry out cost of cultivation surveys between 1980-96, using a stratified random sampling scheme. Blocks were randomly selected within each district, and then two villages were selected randomly within each block (the first one was drawn randomly and then the second one was selected randomly from all villages within a 8 km radius of the first one). Within each of the selected villages our investigators carried out a complete listing of households and the amount of cultivable land they owned in 2003-

**Figure 2: Bottom Tiers of Local Government Hierarchy**




**Note.** This figure depicts the hierarchy of local elected bodies and jurisdictions. The *Panchayat Samiti* comprises of an elected body at the middle-tier level of government. The middle-tier allocates funds to bottom-tier *gram panchayats (GPs)* within its block. The elected GP bodies distribute benefits across and within villages in their jurisdiction. The *Majority* variables are defined according to 2003 panchayat election results.

04. Within each village, approximately 25 households were randomly selected (stratified by landownership). Table 2 provides a summary of the demographic characteristics for the 2,402 households. Half the households own no land; three out of four own less than 1.5 acres of agricultural land and have heads of household with less than 8 years of schooling.[4]

We now describe the major benefit programs. Administrative data on the programs we study does not provide the level of disaggregation we need. Moreover, there are concerns about the reliability of administrative data for these programs (Niehaus and Sukhtankar (2013)). In order to obtain information on benefits at the household level, we surveyed heads of households and asked them to report the major benefits they received from the local governments. Specifically, they were asked if their household had received benefits from different programs in each year between 2004-2011. Many of the programs listed created benefits that were clearly private (i.e., household-specific) in nature, including (a) *employment* in programs such as Sampoorna Grameen Rozgar Yojana, MGNREGA and MPLADS; (b) *minikits* providing farmers with seeds and fertilizers at highly subsidized

[4]In Appendix figure A3, we show the comparison between our sample and the publicly available Socio Economic and Caste Census (SECC) data at the village level for proportion of households SC/ST, proportion of households landless, and proportion of households with illiterate adults. Despite some differences in how the variables are defined in our sample and in the SECC data, these variables are positively correlated: correlation coefficient (p-value) is 0.69 (0.00) for SC/ST, 0.22 (0.04) for landless, and 0.57 (0.00) for illiteracy.

**Table 2: Summary Statistics: Demographics**

| Agri Land Owned (acres) | No. of Households | Characteristics of Head of Households | | | | |
|---|---|---|---|---|---|---|
| | | Avg. Age | % Males | Years of Schooling | % SC/ST | % in Agriculture |
| Landless | 1214 | 45 | 88 | 6.6 | 37.4 | 26 |
| 0-1.5 | 658 | 48 | 88 | 7.8 | 38.9 | 65 |
| 1.5-2.5 | 95 | 56 | 92 | 10.8 | 22.4 | 82 |
| 2.5-5 | 258 | 58 | 93 | 11.1 | 27.1 | 72 |
| 5-10 | 148 | 60 | 89 | 12.5 | 26.1 | 66 |
| $> 10$ | 29 | 59 | 100 | 13.9 | 30.9 | 72 |
| All | 2402 | 49 | 89 | 8.0 | 35.4 | 47 |

**Note.** This table provides demographic characteristics of the head of households (who were the main respondents to the survey) in 2004. *% Agriculture* refers to percentage of household heads whose primary occupation is agriculture. The numbers in this table are taken from the paper "Local Democracy and Clientelism: Implications for Political Stability in Rural West Bengal" by Bardhan et al. (2009).

rates; (c) *subsidized credit*; (d) *land reforms* consisting of receipt of land titles or registration of tenancy contracts; (e) *house, toilet* - lumpsum transfer to households for house or toilet construction; and (f) *Below Poverty Line (BPL)* cards, which identify poor households and entitle them to subsidized food grains and other household items. Road and irrigation projects comprised local public goods. Some programs could be classified as either private or public. For example, drinking water taps located in public spaces, the status of which depends on the relative distance of different households from the location of the tap. Our default classification is to consider them private. We later check the robustness of the results when they are classified as public benefits. For more details on these individual programs, see Appendix Table A2.

Table 3 provides average levels of different benefit programs: column 1 shows the average number of benefits reported per household over the entire 2004-2011 period. The next three columns provide the same number for three sub-periods: the pre-redistricting (2004-06) period, and two post-redistricting sub-periods (2007-08, 2009-11) separated by panchayat elections held in 2008, which changed patterns of party alignment. As much of our focus is on GP program budgets approved by higher level officials, we estimate the scale of these programs by the per-village average of these reports in any given year. For public benefits (village roads and irrigation), benefits are imputed to all households in a village in any given year when at least one household reported benefitting from the corresponding

**Table 3: Summary Statistics: GP-Disbursed Benefits**

| | Average Per-Household Benefits | | | |
|---|---|---|---|---|
| | (2004-2011) | Sub-periods | | |
| | | (2004-06) | (2007-08) | (2009-11) |
| **Any Benefit** | 4.16 | 0.85 | 1.58 | 1.73 |
| **Private Benefits** | 0.73 | 0.28 | 0.21 | 0.24 |
| Employment* | 0.34 | 0.13 | 0.12 | 0.09 |
| Minikits | 0.09 | 0.05 | 0.01 | 0.03 |
| House or Toilet | 0.07 | 0.04 | 0.01 | 0.02 |
| BPL Cards | 0.14 | 0.04 | 0.04 | 0.06 |
| Drinking Water | 0.07 | 0.01 | 0.03 | 0.02 |
| Credit | 0.01 | 0.00 | 0.00 | 0.00 |
| **Public Benefits** | 0.77 | 0.25 | 0.13 | 0.39 |
| Road | 0.70 | 0.18 | 0.13 | 0.39 |
| Irrigation | 0.07 | 0.07 | 0.00 | 0.00 |
| **Other Benefits** | 0.03 | 0.02 | 0.01 | 0.01 |

**Employment* includes panchayat-provided employment, MNREGA, and MPLAD employment.
**Note.** This table presents the per-household benefits averaged for 2004-2011 and for three sub-periods: 2004-2006 (pre-redistricting), 2007-2008 (post redistricting), and 2009-2011 (post 2008 panchayat elections). For private benefits, we show the average number of benefits reported per household. Public benefits are imputed from survey responses as follows: a village is assumed to have had a road/irrigation project built in any given year if at least one household in that village reports benefitting from such a project that year, and all households in the village are then assumed to have received such a project in that year. *Other Benefits* includes *barga* (property rights), *patta* (registration of tenants), training, and relief. Appendix Table A2 provides details of government programs corresponding to the benefits listed in this table.

type of benefit in that year. This is reasonable, as Table A1 in the online Appendix shows that in 81% of village-years, more than 95% of household pairs in the sample reported the same benefit from a public program. Moreover, Table A8 in the Appendix shows the village level results are robust when we use actual (rather than imputed) reports of each household for whether it benefitted from public goods.

Table 3 shows that over the 2004-2011 period, average per-household reported benefits from public good programs were slightly higher than for private programs. Roads recorded the highest reported benefits, followed by employment programs. In this table, we also provide average number of 'other benefits,' which comprise *barga* (sharecroppers' rights to share of produce and protection against eviction), *patta* (deeds assigning property rights to land), flood relief, and vocational training. These constitute a small portion of overall benefits received by households, and we include them under private benefits in robustness checks.

We construct political support data from ballots cast by heads of households in a 'straw' poll. Investigators visited each household head the day prior to the survey, explained to

them details of the survey and in particular of the straw poll and how it was designed to preserve secrecy. They were given the option to participate in the main survey, and separately in the straw poll. At the end of the regular survey, those agreeing to participate in the straw poll were given a ballot paper containing signs of the principal parties competing in the local area, and a ballot box containing ballots cast by previous respondents as well as some dummy ballots. Each ballot paper was marked by a household ID on the back, which had been randomly assigned by the PIs and not disclosed to anyone else. Each participant was asked to go to a corner of the room, privately mark the sign of the party they supported, fold the ballot, insert it into the box, shake the box to mix up all the ballots, and return the box to the investigator who then sealed it before moving on to the next interview. Data entry operators entered the party name selected by each respondent against the assigned household ID, and did not know the name or address of any specific respondent. More than 99% of households in the sample agreed to participate in the straw poll in 2011. Further details of the straw poll process are provided in Section B of the Online Appendix.

The survey was conducted shortly after assembly elections in 2011. We compared the result of survey voting in 2011 (and in the 2004 survey round) to official voting outcomes at the Assembly Constituency level over this period. As seen in Table 1, vote shares in our survey ballots shifted in favor of the TMC in a similar way, though this shift was larger in magnitude than the observed shift in actual vote share. This difference in magnitude is not surprising, since the sample (third and fourth columns in Table 1) covers a small fraction of the population voting in the corresponding electoral constituencies (represented in the first two columns).[5]

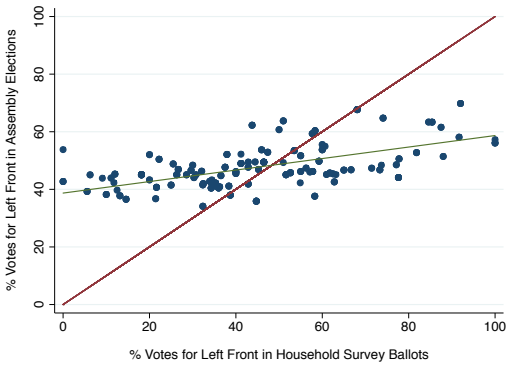
One concern with using household ballots is that voters may systematically misrepresent their voting choices. To check the data's reliability, we compare the share of votes for the Left Front from the survey data with the official Election Commission data for assembly elections. The vote shares for the Left Front from household ballots are aggregated at the assembly-constituency level. We pool the two rounds of survey data and two assembly-election results in 2006 and 2011. Figure 3 plots vote share aggregated from survey data against the corresponding actual shares in the assembly elections.[6] The correlation is 0.57 and significant at the 1% level. Since data on actual votes at the individual level is not

[5]The state legislative assembly consists of approximately 200 rural constituencies, with a constituency corresponding roughly to 50,000 households. Our sample only has 2400 households across all rural constituencies.

[6]These are comparable to the results for Sierra Leone in Casey (2015), which also uses poll survey responses as a proxy for votes.

available, we will use ballot responses of household heads as a proxy for how they actually voted.

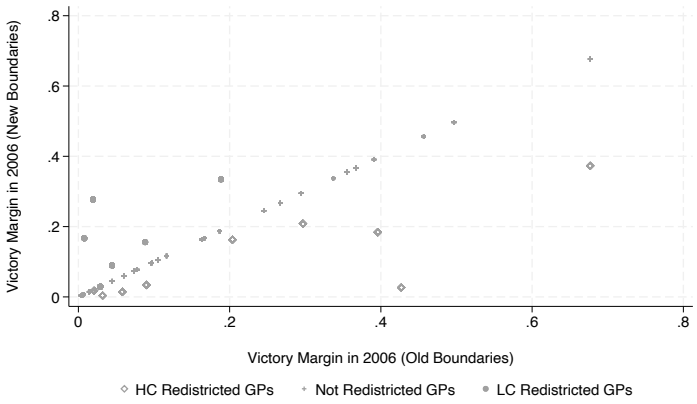
**Figure 3: Vote Share for the Left Front: Household Ballots vs. Assembly Elections**




**Note.** This scatter plot compares the share of votes for the Left Front in the household survey ballots (x-axis) with the official Election Commission data for assembly elections (y-axis). The vote shares for the Left Front from the household ballots are aggregated at the assembly-constituency level. We pool two rounds of survey data and two assembly-election results (2006 and 2011). The correlation coefficient is 0.57 and significant at the 1% level.

**Redistricting.** To isolate exogenous variations in political competition at the GP/village level, we utilize information about the redistricting of assembly constituencies that caused some GPs to be assigned to a different constituency in 2007. Electoral constituency boundaries for parliamentary and state assembly elections are periodically redrawn in order to equalize the population sizes of constituencies. This was the case in all Indian states following the 2001 census, after which redistricting took place based on changes in census population figures between 1981 and 2001. The previous redistricting took place three decades earlier. The Election Commission of India set up a three member Delimitation Commission for each state, comprising a retired chief justice, a member of the Election Commission of India, and the state election commissioner. An advisory committee consisting of five MPs and five state-assembly representatives representing different political parties provided input into the process. The state redistricting commission follows transparency and fairness rules concerning the redistricting process, including holding public hearings and addressing complaints. The new boundaries went into effect in West Bengal in late 2006. We therefore treat 2004-2006 as pre-redistricting years and 2007-2011 as post-redistricting years. Iyer and Reddy (2013) studied redistricting in two other Indian states and found no evidence of violation of the mandated rules. They also found that

**Figure 4: Change in Competition Due to Redistricting**




**Note.** This figure shows the pattern of changes in competitiveness at the GP level generated by redistricting in our sample. The horizontal axis represents the victory margin (difference in vote share between the winner and runner-up in the 2006 Assembly elections) in the original constituency to which a GP/village was assigned prior to 2007, while the vertical axis represents the victory margin in the newly defined constituency following 2007. Non-redistricted GPs are represented by the plus symbol markers lying along the 45 degree line, since they were assigned to the same constituency. *High Competition (HC) Redistricted* GPs are denoted by diamond shaped markers, which all lie below the line of equality, and *Low Competition (LC) Redistricted* GPs by the circular dots lying above.

the outcomes were politically neutral, with few exceptions (which concerned redrawing constituency boundaries for incumbents serving on the advisory committee).

In our sample, 26 out of 89 villages were redistricted. We classify the redistricted villages in our sample across jurisdictions classified by political control of the PSs and GPs (in the 2003 panchayat elections) and whether the redistricting was to a more or less competitive constituency (measured by difference in vote shares between the winner and runner-up in the 2006 Assembly election). Of the villages that were redistricted, 13 were 'moved' to a more competitive constituency and 7 were 'moved' to a less competitive constituency. In section 5, we will refer to these villages as High Competition Redistricted (HCR) and Low Competition Redistricted (LCR) respectively. We lack information on competition for rest of the 6 villages as they were assigned to a newly created assembly constituency. We exclude these six villages from our analysis. Figure 4 shows the pattern of changes in competitiveness (at the GP level) generated by redistricting in our sample.

In our subsequent analysis, we partition redistricted villages into different 'treatment' groups depending on alignment and change in competitiveness, and test predictions of the theoretical model concerning differences in benefit flows between them and the residual

control group. Within the HCR villages, 11 are in the aligned group (where the same party is in power at the GP and the *Panchayat Samiti* levels) and 2 are in the non-aligned group. Within the LCR villages, 6 are in the aligned group and 1 is in the non-aligned group.

# 4 Model

We focus on two tiers in the local government hierarchy: the higher tier is a block managed by a PS, which corresponds to an assembly constituency in the elections.[7] A representative constituency $C_i, i = 1, 2, ..$ has a jurisdiction consisting of GPs that distribute benefits in villages $v \in C_i$. To simplify the exposition, we assume the jurisdiction of a GP consists of a single village. Let $n_v$ denote the share of village $v$ in the population of $C_i$.

The analysis pertains to a single election, which occurs at the end of some period $t$. Elections involve competition between two political parties L and T. We take as given incumbents occupying office during period $t$ (who were elected at the end of period $t-1$), who make benefit-allocation decisions during period $t$. The election at the end of period $t$ results in a new government which will occupy office in period $t+1$ at each tier. Citizens vote retrospectively: they form expectations of policies to be pursued by the two parties in period $t+1$ conditional on winning the election, based on the actual policy of the incumbent party and policy platform of the challenger respectively during period $t$. The details of how these expectations are formed are explained below.

Incumbency patterns during period $t$ are denoted as follows. Constituency $C_i$ is controlled by either the L party ($I_i = 1$) or the T party ($I_i = -1$) as a result of the outcome of the election at the end of $(t-1)$. Elected officials at the assembly level follow the mandate of their party in allocating budgets for various programs to GPs. At $t$, village $v$ has an incumbent GP that is controlled by either the L party ($I_v = 1$) or the T party ($I_v = -1$). Let $\eta_i$ denote $\sum_{v' \in C_i} n_{v'} I_{v'}$, which is positive (resp. negative) if the L (resp. T) party has above-average control of the villages in the constituency.

Households within any village belong to different socio-economic groups $g = 1, \ldots, G$. The demographic share of group $g$ in village $v$ is denoted by $\mu_{vg}$. Members of each group have identical preferences for benefits. There are $K$ different benefit programs; some deliver public (non-excludable) goods, while others distribute private goods. Benefits are indivisible: each resident receives either one unit or none. Receipt of benefit $k$ generates

[7] A previous version of this paper showed how the analysis of the two-tier model can be extended to three tiers while generating similar results.

a utility of $\beta_{kg}$ for a member of $g$. Budgeting is top-down: for each program $k$, in period $t$, the GP is assigned a budget or per capita program scale of $B_{kv}$ units by the upper tier constituency $C_i$. If the benefit is a public good, every resident receives the same number of units ($B_{kv}$).

While private benefits could be recurring or one-time, we will initially ignore this distinction; assume for now that all private benefits are recurring and randomly distributed via lottery within socio-economic groups.[8] The decision made by the GP then reduces to allocating the assigned budget across different groups (represented by $\pi_{kg}$, the fraction of each group $g$ that receives benefit $k$). For the incumbent party, these decisions pertain to actual budgets for allocations in the current year, rather than a commitment to a post-election policy platform. Hence we do not make any Downsian assumption of pre-commitment to policy platform for the incumbent. For the challenger, however, the decision corresponds to announcement of a future policy it promises to enact if elected.

Let $\mathcal{P}, \mathcal{R}$ denote the set of public and private benefits, respectively. In period $t$, the incumbent party $p = L, T$ controlling the GP selects a policy $\pi^p_{kg}$, the fraction of group $g$ residents that will receive benefit $k = 1, \ldots, K$, satisfying the feasibility conditions $\pi^p_{kg} = B_{kv}$ for all $k \in \mathcal{P}$, and $\sum_g \mu_{vg}\pi^p_{kg} = B_{kv}$ for each $k \in \mathcal{R}$.

Given an allocated program budget $B_{ki}, k = 1, \ldots, K$ from the district government at the third tier, the party controlling constituency $C_i$ at the upper tier selects an allocation $B_{kv}$ across villages in its jurisdiction, satisfying the budget constraint $\sum_{v \in C_i} n_v B_{kv} = B_{ki}, k = 1, \ldots, K$. We take as given the budgetary allocation across constituencies.

As mentioned previously, budgeting is top-down: in the first stage of the game, the party controlling each constituency receives a budget from the district and allocates it among different GPs in its jurisdiction. This allocation determines the distribution of public benefits across villages. For private goods, at the second stage of the budgeting game, each GP allocates the assigned budget among different socio-economic groups within the village.

Households in each village cast a vote for either party in elections at both levels, subject to beliefs specified below. Below, we describe alternative specifications of these electoral contests that correspond to programmatic politics and clientelism. In both versions, elected officials at either tier seek to maximize the probability of their party's victory in the next election.

[8] A household's entitlement and demand for a recurring benefit (such as employment or a loan) is the same at all dates, irrespective of receipts of the benefit at previous dates. Hence every household is potentially eligible to receive a recurring benefit. For a one time benefit (such as low income housing, or BPL cards), a household that has already received one in the past is not entitled to another unit in the future.

We study subgame perfect equilibria of the three-stage game (subject to the postulated behavioral restriction on voter beliefs). This approach requires us to work backward, starting with voting at the third stage.

## 4.1 Voting under programmatic politics

First consider a standard model of "programmatic politics" without clientelism (Dixit and Londregan (1995), Grossman and Helpman (1996)). Voting is retrospective: for the incumbent party, the current distribution pattern $\pi_{kg}$ is what voters would expect in period $t+1$ if it were to be re-elected. For its opponent, it is the electoral platform discounted by a "credibility" parameter $(1-\alpha)$ smaller than one, thus generating an electoral advantage for the current incumbent. This is because the platform of the challenger consists of a promise, which voters compare with what the incumbent is currently providing.

Households vote partly on the basis of the utility of the benefits they expect and partly on the basis of the loyalty they feel toward each party (based on historical attachment, identity, or candidate personality). Suppose L is the incumbent in the GP. Relative loyalty $\tilde{\theta}$ to the L party is uniformly distributed within group $g$ in village $v$ with constant "swing" density $s_{vg} > 0$ and mean $\theta_{vg}$. A member of group $g$ with L-loyalty $\tilde{\theta}$ will vote for L if

$$\tilde{\theta} + \sum_k \beta_{kg}\pi^L_{kg} > (1-\alpha)\sum_k \beta_{kg}\pi^T_{kg} \tag{1}$$

We assume that swing densities are small in the sense that

$$\frac{1}{2\bar{s}} > K\bar{\beta} + \bar{\theta} \tag{2}$$

where $\bar{s}$ denotes $\max_{v,g}\{s_{vg}\}$, $\bar{\beta}$ denotes $\max_{v,g}\{\beta_{vg}\}$, $\bar{\theta}$ denotes $\max_{v,g}\{|\theta_{vg}|\}$, and $K$ is the total number of benefits. This assumption ensures that vote shares are always in the interior of the unit interval.[9] In the period $t$ election, the L party's resulting vote share among village $v$ residents will be

$$\sigma^p_v = \bar{\theta}_v + \sum_g \mu_{vg}s_{vg}\sum_k \beta_{kg}\pi^L_{kg} - (1-\alpha)\sum_g \mu_{vg}s_{vg}\sum_k \beta_{kg}\pi^T_{kg} \tag{3}$$

where $\bar{\theta}_v \equiv \frac{1}{2} + \sum_g \mu_{vg}\theta_{vg}$ represents the mean popularity of party L in village $v$.

[9] This is because $K\bar{\beta}$ is an upper bound to the difference between $(1-\alpha)\sum_k \beta_{kg}\pi^T_{kg}$ and $\sum_k \beta_{kg}\pi^L_{kg}$, while the support of $\tilde{\theta}$ is $[\theta_{vg} - \frac{1}{2s_{vg}}, \theta_{vg} + \frac{1}{2s_{vg}}]$.

## 4.2 Voting under Clientelism

Now consider the implications of clientelism, based on the formulation in Bardhan and Mookherjee (2018).[10] In a clientelistic context, the incumbent party can withhold the distribution of private benefits to residents who did not vote for it in the previous election. The descriptive literature on clientelism describes the many ways that secret ballots can be circumvented and party officials can monitor how each citizen votes.[11]

In this setting, voting decisions additionally incorporate strategic considerations — if they vote for the party that ends up losing the election they will be punished by the winner and lose their access to private benefits. Each resident will compare the expected utility of voting for either party, incorporating beliefs regarding the winner of the election (denoted by $p_L$, the probability that L wins). Suppose that party L is the incumbent (the exact expressions below will be modified in a straightforward manner if it is the challenger). The expected utility of a member of group $g$ with preference $\tilde{\theta}$ for the L party in period $(t+1)$ upon voting for L is

$$\tilde{\theta} + p_L \sum_{k \in \mathcal{R} \cup \mathcal{P}} \beta_{kg} \pi^L_{kg} + (1-\alpha)(1-p_L) \sum_{k \in \mathcal{P}} \beta_{kg} \pi^T_{kg} \tag{4}$$

since if it wins, T will withhold distribution of private benefits to this household in the next period. Conversely, the household will obtain an expected utility of

$$p_L \sum_{k \in \mathcal{P}} \beta_{kg} \pi^L_{kg} + (1-\alpha)(1-p_L) \sum_{k \in \mathcal{P} \cup \mathcal{R}} \beta_{kg} \pi^T_{kg} \tag{5}$$

if it votes instead for T. Comparing (4) with (5), we see that the resident will vote for L if

$$\tilde{\theta} + \sum_{k \in \mathcal{R}} \beta_{kg} [p_L \pi^L_{kg} - (1-\alpha)(1-p_L)\pi^T_{kg}] > 0. \tag{6}$$

Therefore, *public goods distributed by either party no longer matter*: voting decisions depend only on a comparison of private benefits distributed by either party, weighted by

[10] There are two main differences between that model and the one in this paper. Here we will take voter beliefs regarding the probability of L winning as exogenous, whereas in the other paper we study the consequences of requiring those beliefs to be self-fulfilling and satisfy some local stability conditions. In that sense the model developed here is a special case. On the other hand, the current model extends the previous one by incorporating two layers of local governments, with upper layer officials strategically allocating program budgets of GPs under their jurisdiction. This enables us to study predictions for inter-GP allocations and how they depend on party alignment between the two layers.

[11] Even if such methods are not possible, residents' votes can be inferred from their expressions of public support (e.g., attendance in party rallies) on the eve of the election. Party operatives need only monitor attendance in these rallies and condition allocation of private benefits on attendance (e.g., provide the benefit only if the resident attended the rally organized by the party that won the last election, and did not attend the pre-election rally of the opponent party). Residents attending the rally of a party then have an incentive to vote for it.

their respective likelihoods of winning.

This generates a fundamental difference between programmatic politics and clientelism: in the latter, voters weigh the expected personal consequences of their voting decisions. If the candidate they vote for loses the election, they will be punished by the subsequent incumbent. This punishment consists of the denial of private benefits earmarked for their group. By the very nature of public goods, they cannot be excluded from what will be provided by the incumbent. Hence, only private transfers matter, not public goods. Voting no longer reflects citizens' comparative evaluation of the policies of competing candidates.

The resulting vote share of L in the village is

$$\sigma_v^c = \bar{\theta}_v + \sum_g \mu_{vg} s_{vg} \sum_{k \in \mathcal{R}} \beta_{kg} [p_L \pi_{kg}^L - (1-\alpha)(1-p_L)\pi_{kg}^T]. \tag{7}$$

## 4.3 Second Stage GP (Within-Village) Allocations

Elected officials controlling the GP allocate private benefits in period $t$ to maximize the vote share of their own party in the next election. Expressions (3) and (7) show that under both programmatic politics and clientelism, for any given private program $k$, officials in either party have a dominant strategy $\{\pi_{kg}^*\}$, which maximizes $\sum_g \mu_{vg} s_{vg} \beta_g \pi_{kg}$ subject to $\sum_g \mu_g \pi_{kg} = B_{kv}$. Hence, private benefit distribution policies of GP incumbents will be the same under programmatic politics and clientelism (though in the latter case, goods will be distributed only among those who vote in favor of the incumbent).[12] From these conditions, we can characterize within-village allocations and the resulting vote shares in the next election.

Consider any GP with village $v$ that receives a budget $B_{kv}$ for program $k \in \mathcal{P} \cup \mathcal{R}$. Under either programmatic politics or clientelism, private benefit $k$ will be allocated within the village by a GP as follows. Groups will be ranked in order of priority according to the distributional characteristic $\delta_{vg} \equiv s_{vg}\beta_{kg}$. Define $g^*$ as follows: it is the group $g$ with the lowest value $\delta_{vg}$ such that $B_{kv} \geq \sum_{\{g':\delta_{vg'} \geq \delta_{vg}\}} \mu_{vg'}$. Then $\pi_{kg}$ equals one for all groups $g$ with $\delta_{vg} > \delta_{vg^*}$ and zero for all groups with $\delta_{vg} < \delta_{vg^*}$, with $\pi_{kg^*} = \frac{B_{kv} - \sum_{\{g':\delta_{vg'} \geq \delta_{vg^*}\}} \mu_{vg'}}{\sum_{\{g:\delta_{vg} = \delta_{vg^*}\}} \mu_{vg}}$. The resulting vote share of the L party in programmatic politics will be

[12] However, in clientelism private benefits are denied to those who voted for the losing party, resulting in a budgetary surplus. This could potentially be used to provide more benefits to those that voted for the incumbent. We avoid this complication by assuming that benefits denied to those voting for the losing party are diverted for personal use by party members or disposed of. This simplifies the model without changing any of the qualitative conclusions that follow.

$$\sigma_v = \bar{\theta}_v + I_v\alpha \sum_{k\in\mathcal{R}}[\sum_{\{g:\delta_{vg}>\delta_{vg^*}\}} \mu_{vg}(\delta_{vg} - \delta_{vg^*}) + \delta_{vg^*}B_{kv}] + I_v\alpha \sum_{k\in\mathcal{P}}(\sum_g \mu_{vg}\delta_{vg})B_{kv} \quad (8)$$

and will thus respond to both private and public benefits allocated to the village. Under clientelism, the share will be

$$\sigma_v = \bar{\theta}_v + I_v[(2-\alpha)p - (1-\alpha)] \sum_{k\in\mathcal{R}}[\sum_{\{g:\delta_{vg}>\delta_{vg^*}\}} \mu_{vg}(\delta_{vg} - \delta_{vg^*}) + \delta_{vg^*}B_{kv}], \quad (9)$$

where $I_v = 1$ or $-1$ depending on whether the GP is controlled by the L or T party, respectively, and $p$ denotes voter beliefs that the current incumbent will be re-elected. Votes will respond only to the private benefits allocated.

The within-village allocation of a private benefit program $k$ will thus be as follows. Different voter groups will be ordered by their "swing-weighted" benefit $\delta_{vg} = s_{vg}\beta_{kg}$; the GP will allocate the benefit to groups with the highest priority until the budget is exhausted. Define $\nu_{kv} \equiv \frac{\partial \sigma_v}{\partial B_{kv}}$, the marginal vote-generating effectiveness of benefit $k$ in village $v$. In both programmatic politics and clientelism, $\nu_{kv}$ is proportional to $I_v\delta_{vg^*}$, positive for the incumbent and negative for the challenger. In programmatic politics, the factor of proportionality is $\alpha$, the incumbency advantage parameter; in clientelism, it is $[(2-\alpha)p - (1-\alpha)]$, which depends on voter beliefs that the incumbent will be re-elected.[13] Observe also that assumption (2) on swing density ensures that $\nu_{kv}$ always lies between $-1$ and $1$.[14]

*The key distinction between programmatic politics and clientelism is thus the effect of public benefits on voter support: $\nu_{kv}$ is positive under programmatic politics and zero in clientelism.* In addition, the vote-generating effectiveness of private benefits depends on the incumbency parameter $\alpha$ in programmatic politics and on voter beliefs $p$ that the incumbent

[13] These expressions are modified when private benefits are of a one-time nature rather then recurring. $\nu_{kv}$ will be smaller compared with recurring private benefits because households that have already received a one-time benefit are not eligible to receive it again. Hence, current distributions will not motivate current or past recipients. Only those who are yet to receive the benefit will be motivated by the likelihood of receiving it in the future, which they gauge by observing current distribution patterns. The marginal utility $\beta_{kg}$ will thus be weighted by the fraction of members of group $g$ who are yet to receive it. This adjustment will lower the distributional characteristic of a one-time benefit relative to a recurring benefit for any group. Hence one time private benefits will generate a smaller vote share response compared with that of a recurring private benefit. The adjustment would apply equally in both programmatic politics and clientelism.

[14] Intuitively, this is because vote shares being interior must change by less than a unit increase in per capita benefits. This is verified by checking that (2) ensures $\delta_{vg}$ lies in the unit interval, while $I_v[(2-\alpha)p-(1-\alpha)]$ always lies between $-1$ and $1$.

will be re-elected in clientelism.

## 4.4 First-Stage PS (Across-Village) Benefit Allocations

Now consider the decisions made by the government controlling $C_i$, given the budget allotment $B_{ki}$ that it has received from the government one tier above. The vote share of party L in $C_i$ is $\sigma_i \equiv \sum_{v \in C_i} n_v \sigma_v$. Anticipated village vote shares $\sigma_v$ depend in turn on benefit program budgets $B_{kv}$ allocated to the corresponding GPs, as described in (8) and (9).

As in standard models of probabilistic voting, we assume the probability that the party L candidate wins constituency $C_i$ equals $p(\sigma_i)$, a smooth monotone increasing function of its aggregate vote share. The function $p$ smooths the likelihood of winning, owing to possible randomness in turnout or vote-counting errors. Specifically, L wins if and only if

$$z_L \equiv \lambda \sigma_i + \epsilon_i > 0 \tag{10}$$

where $\epsilon_i$ represents election 'noise' resulting from randomness in turnout and vote counting errors. We assume $\epsilon_i$ has an i.i.d. distribution (which is independent of the realization of voter preferences $\tilde{\theta}$) represented by a smooth density $f$. $\lambda > 0$ is a parameter which represents the importance of underlying voter preferences relative to election noise. The function $p(\sigma_i)$ then equals $1 - F(-\lambda \sigma_i)$, where $F$ denotes the distribution function of $\epsilon_i$.

The party controlling $C_i$ is the party that controls the corresponding PS. Let $I_i = 1, -1$, depending on whether $C_i$ is controlled at $t$ by the L or the T party. The incumbent party selects an inter-village allocation $B_{kv}, k = 1, \ldots, K$ to maximize

$$I_i R p(\sum_{v \in C_i} n_v \sigma_v) - \frac{d}{2} \sum_{v \in C_i} \sum_k n_v (B_{kv} - B_{ki})^2, \tag{11}$$

subject to village-level vote-share equations (8) or (9) under programmatic politics and clientelism, respectively, and the budget constraint $\sum_{v \in C_i} n_v B_{kv} = B_{ki}, k = 1, \ldots, K$. Exogenous political rents of office are denoted by $R$, and $B_{ki}$ is the budget the PS receives from the next-highest tier at the district level. The first term in (11) represents the objective of enhancing re-election prospects, which motivates the incumbent party to bias inter-village allocations in favor of villages where benefit programs are likely to generate the most votes for the $C_i$ incumbent. Budget distortions impose a cost proportional to the variance of the resulting inter-village allocation, represented by the second term in (11).[15]

[15] This represents the cost of coping with complaints of unfair treatment from village-level representatives, media watchdogs, or auditors appointed by upper-level governments.

The parameter $d$ represents the cost imposed for deviations from the fair (equal) allocation.

In order to smooth the model sufficiently, $\lambda$ is assumed small enough (i.e., there is enough electoral noise) that

$$\max_{\sigma\in[0,1]}\{\lambda^2 f'(\lambda\sigma)\} < \frac{d}{R} \quad \text{and} \quad \max_{\sigma\in[0,1]}\{\lambda f(\lambda\sigma)\} < \frac{d}{R}B_{ki} \tag{12}$$

for all $k, i$. If electoral noise is uniformly distributed the first condition is always satisfied, while the second condition requires $\lambda U < \frac{d}{R}B_{ki}$ where $U$ denotes the constant density. If noise has a logistic distribution, these conditions simplify to $\lambda e^{\lambda} < \frac{d}{R}$ and $\lambda^2 e^{\lambda} < \frac{d}{R}B_{ki}$.

Condition (12) ensures that the objective function (11) is globally concave and that optimal budgetary allocations are interior, characterized by first-order conditions, and described as follows.[16]

**Proposition 1.** *The optimal inter-village allocation of program $k$ across GPs located in constituency $C_i$ satisfies*

$$B^*_{kv} = B_{ki} + \frac{R}{d}p'_i(\sigma^*_i)[\nu_{kv}I_iI_v - \sum_{v'} n_{v'}\nu_{kv'}I_iI_{v'}], \tag{13}$$

*where $B_{ki}$ denotes the per capita budget for the constituency and $\sigma^*_i$ denotes the resulting equilibrium vote share of the L party.*

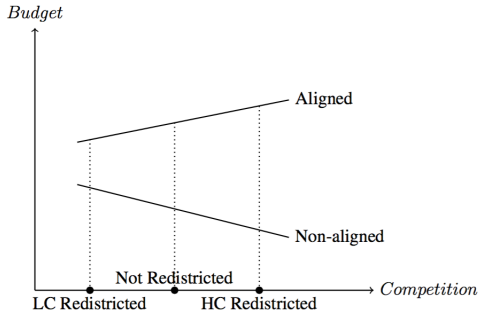
The inter-village allocation of benefit $k$ within constituency $C_i$ is biased in favor of village $v$ by an extent that depends on the following factors: (a) $\nu_{kv}$: how effective the benefit is in generating votes; (b) $p'_i$: how competitive the constituency is; and (c) $I_vI_i = 1$ or $-1$: whether political control is aligned between the two tiers. This yields the following corollary.

**Corollary 1.** *(a) In clientelism (resp. programmatic politics), public benefits will not (resp. will) respond to shocks in political competitiveness.*

*(b) For private benefits under either programmatic politics or clientelism, aligned GPs (where $I_iI_v = 1$) will receive higher (per capita) budgets than their non-aligned*

[16]The objective function (11) is globally concave if $Rp"(\sigma^*_i)[n_v\nu_{kv}]^2 < d$, which is ensured by the first condition in (12) since $p"(\sigma) = \lambda^2 f'(\lambda\sigma)$ and $n_v\nu_{kv}$ lies between -1 and 1. Solving the first order conditions while assuming the optimal allocation is interior yields expression (13) for the allocation. Since $\nu_{kv}$ lies between -1 and 1, the smallest value that the right hand side can take is $B_{ki} - \frac{R}{d}\max_{\sigma\in[0,1]}\{p'(\sigma)\}$, which is positive if the second condition in (12) holds.

**Figure 5: Theoretical Predictions**




**Note.** This figure outlines the main predictions of our model. Redistricting to a more competitive constituency will result in a larger (smaller) program scale in aligned (non-aligned) redistricted GPs, compared with non-redistricted GPs, thus resulting in a larger gap between the aligned and non-aligned GPs in this group. Within aligned GPs, those redistricted to more competitive constituencies will receive larger allocations than those redistricted to less competitive ones. The opposite will be the case for non-aligned GPs. In programmatic politics these patterns will appear for both public and private programs, whereas in clientelism, they will appear only for private benefit programs.

> *counterparts (where $I_i I_v = -1$). Non-aligned GPs will receive less in more competitive constituencies, while aligned ones will receive more. The opposite will be the case if the GP is redistricted to a less competitive constituency.*

Hence, alignment and competitiveness determine the direction and extent of the budgetary manipulation by the upper-tier government, as illustrated in Figure 5. We obtain the following testable predictions concerning the effects of redistricting: (1) redistricting to a more competitive constituency will result in a larger (smaller) program scale in aligned (non-aligned) redistricted GPs, compared with non-redistricted GPs, thus resulting in a larger gap between the aligned and non-aligned GPs in this group; (2) within aligned GPs, those redistricted to more competitive constituencies will receive larger allocations than those redistricted to less competitive ones, and the opposite will be the case for non-aligned GPs. Finally, in programmatic politics these patterns will appear for both public and private programs, whereas in clientelism, they will appear only for private benefit programs.

# 5 Empirical Results

## 5.1 Effects of Redistricting on Inter-Village Benefit Allocations

In this section, we empirically test the predictions of our model and make inferences about the prevalence of clientelism vis-a-vis programmatic politics. We use difference-in-differences analysis with time period 2004-2006 as the pre-redistricting years, and 2007-2008 as the post redistricting years. Since our treatment groups are defined partly by alignment of political control at the GP and PS levels, we restrict attention in this section to the years 2004-2008, since alignment did not change during this period.[17] The Appendix examines the results when the pre or post-periods are extended by a few years, while incorporating the changed patterns of alignment outside the 2004-08 period.

Our empirical strategy is illustrated in Figure 6. GPs are represented by black dots. The gray and black solid contours respectively define the old and new boundaries between different assembly constituencies C1 and C2. Some GPs remain in the same constituency (control group), while some (such as the black dot with a gray circle around it) are reassigned from C2 to C1. If C1 is a more contested constituency, changes in vote share in the redistricted GP will matter more in determining the winner of the subsequent assembly election.

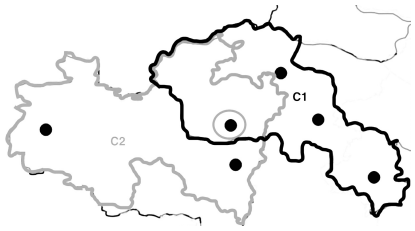
The party controlling the PS will therefore manipulate the budgetary allocation to the redistricted GP in a direction depending on political alignment and whether the GP is moved to a more or less competitive AC. If it is moved to a more competitive AC, and the GP is controlled by the rival (resp. same) party, the PS will reduce (resp. increase) the allocation. This applies only for benefit programs with a significant positive effect on voting patterns. Hence, we can infer which benefit programs are expected to affect votes by observing which ones are manipulated in the predicted directions.[18]

To assess the change in competition resulting from a given GP being redistricted, we compare the victory margin in the 2006 state Assembly election between the constituency it was redistricted into and that of the constituency to which it previously belonged. We use the 2006 election victory margins because they are the most recent signal available to

[17] Recall that local government elections were held in 2003 and 2008.

[18] We could potentially restrict our sample to only those villages that were close to the old boundary of the Assembly constituencies. These villages would have a higher likelihood of being redistricted than villages that were located in the center of the constituency. This would bring the empirical strategy closer to the ideal experiment such that the villages that do get redistricted would have been picked "more randomly". Unfortunately, we do not have sufficient data to carry out such an exercise.

**Figure 6: Illustrating Redistricting of Gram Panchayats**




**Note.** This figure illustrates redistricting of gram panchayats (GPs) in our sample. GPs are represented by black dots. The gray and black solid contours respectively define the old and new boundaries between different assembly constituencies C1 and C2. Some GPs remain in the same constituency (control group), while some (such as the black dot with a gray circle around it) were "moved" from C2 to C1.

the upper level officials of their party's competitive strength during the post-redistricting period.

In our sample, approximately 24% of GPs were redistricted. Population sizes and proportion of seats won by the Left Front did not vary significantly between redistricted and non-redistricted GPs (the respective t-statistics were 0.9 and 0.08). To examine more systematically how they differed on various dimensions, we provide in Table 4 linear probability regressions of the likelihood that any given village belonged to either the control group or one of the four treatment groups of redistricted villages (defined by alignment and competitiveness). In addition to a range of village characteristics representing distribution of landownership, occupation, caste, religion and immigration, the regressors reflect possible political motives of incumbents to manipulate the process: whether the Left party controlled the PS or GP; whether the Left party won the Assembly seat in 2006 elections; whether the constituencies were represented by an MP or MLA with a seat in the Delimitation Commissions; and whether the assembly constituency seat was reserved for Scheduled Caste (SC) or Scheduled Tribes (ST) candidates. Iyer and Reddy (2013) found that the last two regressors helped predict the likelihood of redistricting in Andhra Pradesh and Rajasthan. In contrast, for our sample in West Bengal, Table 4 shows that none of these variables are significant predictors of the likelihood of belonging to any of the treatment groups. The only exception is the proportion of Hindu households in column 2. We therefore include controls for this characteristic in the cross-sectional voting regressions (while it is subsumed in other regressions with village or household fixed effects).

We now explain how we measure benefit allocations at the village-year level. Since the theory predicts GP-level allocations chosen by the PS authorities, in the case of public

## Table 4: Predicting Redistricting

| | Dependent variable: Probability that village belongs to group $i$. | | | | |
|---|---|---|---|---|---|
| Group $i$: | Not Redistricted | HC Red. × Aligned | HC Red. × Non-Aligned | LC Red. × Aligned | LC Red. × Non-Aligned |
| | (1) | (2) | (3) | (4) | (5) |
| Left Panchayat Samiti | 0.14 | -0.08 | 0.11 | 0.05 | -0.22 |
| | (0.24) | (0.11) | (0.08) | (0.05) | (0.23) |
| Left Dominated GP | -0.01 | 0.06 | -0.13 | 0.05 | 0.03 |
| | (0.12) | (0.07) | (0.09) | (0.03) | (0.08) |
| Left Won 2006 Assembly | 0.02 | 0.00 | -0.05 | 0.07 | -0.04 |
| | (0.12) | (0.10) | (0.06) | (0.04) | (0.06) |
| Delimitation Commission Member | -0.15 | 0.22 | -0.01 | -0.08 | 0.02 |
| | (0.13) | (0.14) | (0.04) | (0.05) | (0.03) |
| Seat Reserved for SC/ST | -0.02 | 0.04 | -0.03 | 0.02 | -0.01 |
| | (0.17) | (0.10) | (0.04) | (0.05) | (0.02) |
| Proportion of HH Immigrated to Village Before 2004 | 0.11 | -0.30 | 0.02 | 0.17 | 0.00 |
| | (0.32) | (0.21) | (0.16) | (0.19) | (0.05) |
| Proportion of SC/ST HHs | 0.45 | -0.22 | 0.03 | -0.18 | -0.08 |
| | (0.19) | (0.14) | (0.06) | (0.12) | (0.07) |
| Proportion of Hindu HHs | -0.21 | 0.21 | -0.09 | 0.10 | -0.01 |
| | (0.14) | (0.11) | (0.07) | (0.07) | (0.07) |
| Proportion Cultivators | -0.17 | 0.15 | -0.08 | -0.10 | 0.19 |
| | (0.31) | (0.24) | (0.09) | (0.09) | (0.16) |
| Proportion Landless in 2004 | -0.06 | 0.00 | -0.08 | -0.06 | 0.20 |
| | (0.33) | (0.17) | (0.11) | (0.10) | (0.14) |
| Observations | 83 | 83 | 83 | 83 | 83 |
| Adjusted $R^2$ | -0.055 | -0.007 | 0.082 | -0.017 | 0.104 |
| Mean Dependent Variable | 0.76 | 0.13 | 0.02 | 0.05 | 0.04 |

**Note.** This table shows regressions for the likelihood that any given village belonged to the control group or to one of the four treatment groups of redistricted villages (defined by alignment and competitiveness effect). *HC Redistricted* refers to those cases where the village was redistricted to an assembly constituency with a smaller gap in vote share between winner and runner up. *LC Redistricted* refers to those cases where a village was redistricted to an assembly constituency with an equal or a larger gap in vote share between winner and runner up. *Aligned* is a dummy that takes value 1 if the same party is in power at the GP as well as at Panchayat Samiti. *Seat Reserved for SC/ST* refers to Assembly constituency seats. *Left Won 2006 Assembly* takes value 1 if the Assembly constituency the village belongs to was won by the Left Front in 2006. Robust standard errors are in parentheses, clustered at *panchayat samiti* level.

benefits, the relevant measure is whether a road or irrigation project was undertaken in a given village in a given year. For this we rely only on household reports, implying that actual allocations will have to be proxied based on these reports. If even a single household from village $v$ reported benefiting from a public project in year $t$, we infer that the project was approved and completed that year. This seems reasonable, since most road projects

constructed within a village are completed within a few months.[19] If no household reported benefitting from a public project, we assume no project was approved for that year. We refer to this variable as the 'imputed benefits' for public goods. We shall check the robustness of our results to an alternative way of measuring public benefits equal to the actual proportion of village households who reported benefitting from it in any given year. In the case of private benefits, we measure the village allocation as the per capita benefits distributed in the village in any given year. We shall also explore robustness to measuring it instead by the proportion of households in the village that reported receiving at least one private benefit of the stipulated kind in any given year.

Next we describe the regression specification. Villages redistricted to more (resp. less) competitive constituencies (smaller (resp. larger) victory margins in the 2006 state assembly election) are referred to as HCR (resp. LCR) villages. Let $B_{vt}$ denote the benefit variable described above (measured in standard deviation units) for village $v$ in year $t$. In the simpler specification we focus only on HCR (and its interaction with alignment) as the treatment variable:

$$B_{vt} \quad = \quad \alpha_0 + \alpha_1 \text{P}_t\text{*HCR}_v\text{*A}_v + \alpha_2 \text{P}_t\text{*HCR}_v + \beta X_{vt} + F_v + \tau_t + \epsilon_{vt}, \qquad (14)$$

where $HCR_v$ is a dummy for (HCR) villages, $A_v$ is a dummy for *'Aligned'*, that is, control by the same party at both the PS and GP levels, and $P_t$ is a dummy for the post-2007 years; $X_{vt}$ includes each of these three variables and pairwise interactions, and dummies for representation on the delimitation commission by the MLA or MP of the original constituency; and $F_v$ and $\tau_t$ are village and year fixed effects, respectively. The error term is given by $\epsilon_{vt}$; standard errors are clustered at the PS level. The theory predicts $\alpha_1 > 0, \alpha_2 < 0, \alpha_1 + \alpha_2 > 0$ for any benefit program that affects household votes positively, and zero for benefits that do not affect voting patterns.

The full specification involves four different treatment groups, involving both HCR and

[19] We use administrative data for road projects from the MPLAD program to get an estimate of the time taken to construct village road projects comparable to the ones we examine in our analysis. There are 26 MPLAD projects in West Bengal between 2004 and 2008, for which we have data for time taken to complete the project from the day it was approved. Only one road project took more than 360 days to complete. On average, the projects were completed in 154 days, and the standard deviation was 85 days.

LCR:

$$\begin{aligned} B_{vt} &= \alpha_0 + \alpha_1 \text{P}_t\text{*HCR}_v\text{*A}_v + \alpha_2 \text{P}_t\text{*HCR}_v + \alpha_3 \text{P}_t\text{*LCR}_v\text{*A}_v + \alpha_4 \text{P}_t\text{*LCR}_v \\ &+ \beta X_{vt} + F_v + \tau_t + \epsilon_{vt}, \end{aligned} \tag{15}$$

where $LCR_v$ denotes a dummy for an LCR village. Here the control group comprises non-redistricted villages. The theoretical predictions now are $\alpha_1 > 0 > \alpha_2, \alpha_1 + \alpha_2 > 0, \alpha_3 < 0, \alpha_4 > 0, \alpha_3 + \alpha_4 < 0$ for programs that affect votes and zero otherwise.

Prior to reviewing the results of these regressions, we turn to Figure 7, which shows the corresponding 'event study' results separately for each of the treatments and its corresponding interaction effect with different years, both before and after the year of redistricting, relative to 2006, the year of redistricting.

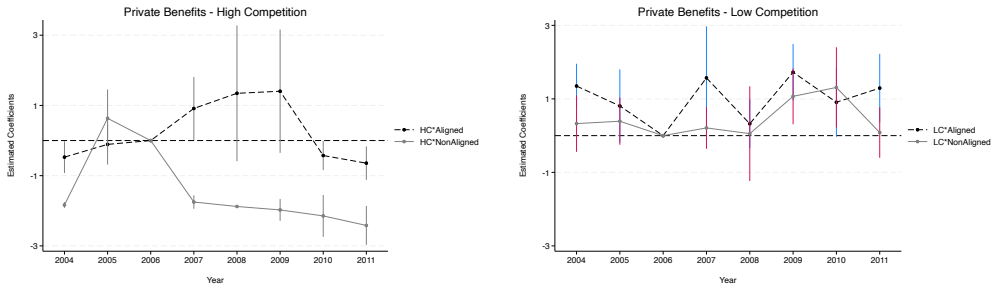
The dependent variable in each graph is the (deviation from the 2006 level) of the standardized measure of either aggregate private or public benefits at the village-year level. There do not appear to be any significant differences in pre-redistricting trends between aligned and non-aligned GPs in any of the cases. For the high-competition-nonaligned treatment, we see a statistically significant negative post-redistricting coefficient for private benefits for each of the four years following 2006, resulting in a post-redistricting decline that is in sharp contrast to a (possibly slightly upward) pre-redistricting trend. This decline is also strikingly different from the positive impacts on high-competition-aligned GPs for the first three years following redistricting and the absence of any clear trend over 2006-2011 as a whole. As a consequence, the difference between aligned and non-aligned HC constituencies is significantly negative for each year after 2006, but not in any year prior to 2006. For the other private benefit treatments, the signs of the post-redistricting coefficients in each year are in line with the theoretical predictions, though they are statistically insignificant. In the case of public benefits, none of the post-redistricting years display any significant impacts.[20]

Relative to the event study, our main regression specification (15) collapses the different year-interaction effects post-and pre-redistricting into an average difference between post- and pre-redistricting years, besides incorporating multiple treatments in the same regression. Our main regression covers the period 2004-08, in order to ensure balance between

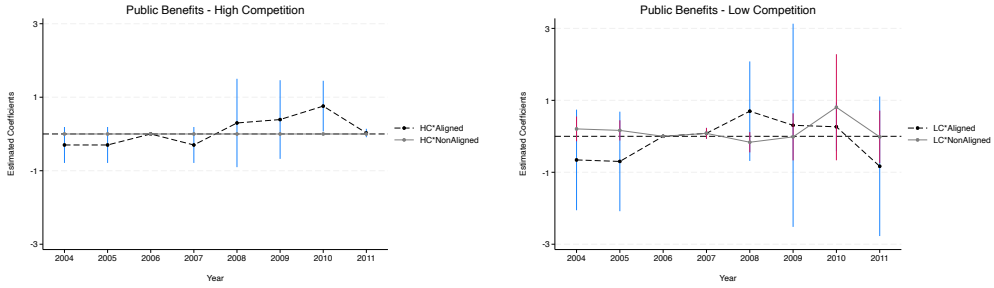
[20] Appendix Figure A2 presents robustness of the event study when the pre-treatment period includes years 1998-2003. We do not have data for HC Redistricted $\times$ Non-Aligned villages over the period 1998-2003 to check for parallel trends between the two HC treatment groups and hence we exclude them from this robustness exercise.

## Figure 7: Event Study

### Panel [a] Private Benefits




### Panel [b] Public Benefits




**Note.** These figures plot the estimated treatment effects from the event study regressions with dependent variable being standardized annual per household benefits. Each of the eight graphs plot estimates from separate regressions. *Private benefits* include MNREGA, MPLAD, IRDP credits, agricultural minikits, ration cards, houses, toilets, and drinking water. *Public benefits* refer to roads and irrigation projects that households reported benefitting from. The per household road benefits are imputed from survey responses using the following procedure: if even a single household reports receiving benefits from roads, that village is considered to have had a road built for that year. *Aligned* means that the same party is in power at both the *panchayat samiti* and *gram panchayat* levels. *HC Redistricted* refers to those cases where the village was redistricted to an assembly constituency with a smaller gap in vote share between winner and runner up. *LC Redistricted* refers to those cases where a village was redistricted to an assembly constituency with an equal or a larger gap in vote share between winner and runner up. The treatment effect is normalized to be zero for 2006.

pre- and post-redistricting phases, besides ensuring that political alignment remained the same before and after.[21] The results for the 2004-08 period are shown in Table 5. Standard errors are clustered at the PS level. Since the 'treated' group constitutes only 27% of all villages in the sample and is further subdivided into four treated subgroups that are predicted to be impacted differently, we show under each coefficient estimate the wild-cluster bootstrapped p-value (in square brackets) as well as the asymptotic standard errors

[21] Appendix Tables A9 and A10 show that similar results obtain when the post-redistricting period is extended to 2011 and when pre-redistricting period is extended back to 1998 respectively.

**Table 5: Effect of Competition and Alignment on Benefits Distributed**

| | Effect of High Competition | | Effect of Alignment (Given Competition) | | Effect of Alignment (HCR and LCR Villages) | |
|---|---|---|---|---|---|---|
| | Private | Public | Private | Public | Private | Public |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post × HC Redistricted | 0.60 | 0.01 | -1.32 | -0.00 | -1.21 | -0.00 |
| | (0.66)[0.37] | (0.33)[0.97] | (0.27)[0.11] | (0.14)[0.99] | (0.30)[0.10] | (0.18)[0.94] |
| Post × HC Redistricted × Aligned | | | 2.25 | 0.01 | 2.15 | 0.06 |
| | | | (0.72)[0.10] | (0.41)[0.99] | (0.74)[0.10] | (0.42)[0.88] |
| Post × LC Redistricted | | | | | 0.50 | 0.00 |
| | | | | | (0.32)[0.25] | (0.18)[0.99] |
| Post × LC Redistricted × Aligned | | | | | -0.26 | 0.83 |
| | | | | | (0.56)[0.67] | (0.69)[0.34] |
| Observations | 415 | 415 | 415 | 415 | 415 | 415 |
| Adjusted $R^2$ | 0.036 | 0.179 | 0.059 | 0.176 | 0.055 | 0.182 |
| Test: (Post × HC Redistricted × Aligned) + (Post × HC Redistricted) = 0 | | | | | | |
| t-Statistic | | | 1.35 | 0.02 | 1.36 | 0.17 |
| Wild cluster bootstrap p-value | | | [0.19] | [0.98] | [0.19] | [0.87] |
| Effect of Competition (Given Alignment) | | | | | | |
| Test: (Post × HC Redistricted × Aligned) = (Post × LC Redistricted × Aligned) | | | | | | |
| t-Statistic | | | | | 3.06 | -0.99 |
| Wild cluster bootstrap p-value | | | | | [0.05] | [0.42] |
| Test: (Post × HC Redistricted) = (Post × LC Redistricted) | | | | | | |
| t-Statistic | | | | | -8.57 | -0.24 |
| Wild cluster bootstrap p-value | | | | | [0.03] | [0.75] |

**Note.** This table presents estimates for equations 14 and 15 of section 5.1. Observations are at the village-year level, 2004-2008. Post takes value 1 for years 2007 and onwards. The dependent variable is standardized measure of annual per-HH benefits for each village. *HC Redistricted* refers to those cases where the village was redistricted to an assembly constituency with a smaller gap in vote share between winner and runner up. *LC Redistricted* refers to those cases where a village was redistricted to an assembly constituency with an equal or a larger gap in vote share between winner and runner up. *PS* refers to *panchayat samiti*, and *Aligned* means same party is in power at both the PS and GP levels. *Private benefits* include panchayat-provided employment, MNREGA, MPLAD, IRDP credits, agricultural minikits, ration cards, houses, toilets, and drinking water. *Public benefits* refer to roads and irrigation. The per household road/irrigation benefits are imputed from survey responses using the following procedure: if even a single household reports receiving benefits from roads/irrigation, that village is considered to have had a road/irrigation project built for that year. All specifications include other interaction terms, whether MLA/MP was part of delimitation committee, and village and year fixed effects. Robust standard errors clustered at *panchayat samiti* level are in parentheses. Wild bootstrapped *p*-values clustered at *panchayat samiti* level are in square brackets. The standardized mean (std. dev.) is 0.75 (0.13) for per household private benefits and 0.26 (0.30) for imputed public goods.

(in parentheses).

For private and public benefits, columns 1 and 2 show the results we get when we combine the two treatment groups into a single treatment group comprising GPs redistricted to more competitive constituencies irrespective of alignment. We see no significant differences between the combined treatment group and the control group. Columns 3 and 4 then show results for the specification dictated by the theory, focusing only on the HCR treatment. As is consistent with the model predictions, the difference in private benefit allocation between the aligned and non-aligned treatment groups relative to the control group increased to a large extent (2.25 s.d), with a wild bootstrap p-value of 0.10. For the GPs in the non-aligned treatment group, the program scales contracted by 1.32 s.d, with a p-value of 0.11. For public benefits, on the other hand, the results are consistent only with

the clientelism model: the differential effects are negligible (within $+/-$ 0.01 s.d.) and statistically insignificant. The difference in statistical significance cannot be attributed to greater imprecision of the public benefit estimates, as their coefficients have lower standard errors.

The last two columns of Table 5 show results for the more demanding specification (equation 15) involving both HCR and LCR. For public benefits (column 6), none of the treatment effects are significant. For private benefits (column 5) and HC redistricted villages, we continue to see the same results as before ($\alpha_1 > 0, \alpha_2 < 0, \alpha_1 + \alpha_2 > 0$). The estimated coefficients for the two HC redistricted treatment groups are similar. For the LC redistricted villages, the estimated coefficients are consistent with the model's predictions ($\alpha_3 < 0, \alpha_4 > 0$) but statistically insignificant. The bottom panel of the table shows that within the aligned group, we can reject the hypothesis that the LCR-aligned effect is the same as the HCR-aligned effect ($\alpha_4 - \alpha_2 = 0$) with a p-value of 0.05. We can also reject the hypothesis that LCR-nonaligned effect is the same as HCR-nonaligned effect ($\alpha_3 - \alpha_1 = 0$) with a p-value of 0.03.

**Robustness Checks.** We carry out a series of robustness checks such as adding additional controls, extending the time period of analysis, using alternative measures of outcome variables, and examining the results for each benefits separately. The results of these exercises show that our results are robust, with the only exception being that the sign of $\alpha_3$ is sometimes positive. The Appendix Table A4 shows that the main results are robust to the inclusion of pre-2007 trends specific to each separate treatment group and the control group. Appendix Table A9 shows robustness when the post-redistricting period is extended to 2011, which helps allay concerns regarding possible time lags between approval and completion of road projects. Appendix Table A10 shows corresponding results are similar when the pre-redistricting period is extended back to 1998 for a restricted subsample for which alignment data is available for 1998-2003.

The preceding results aggregated different types of private benefits into a single category of benefits, an approach that may raise concerns about aggregation biases and interpretation. Table 6 shows the corresponding results for employment programs (columns 1 and 2), which are estimated more precisely with p-values below 0.05. Columns 3 and 4 provide corresponding regressions for all private benefits, excluding employment programs. The results are similar, but less precise, compared to the results found in Table 5. Columns 5 and 6 show that the absence of significant effects on the allocation of public benefits continues to hold when they include drinking water access. Appendix Table A6

shows more detailed results separately for each type of benefit and shows that $\alpha_3 > 0$ in column 4 of Table 6 is driven largely by BPL cards and drinking water.

**Table 6: Robustness: Effect of Competition and Alignment**

| | Employment | | Private Benefits Without Employment | | Public Benefits With Water | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post × HC Redistricted | -1.88 | -1.83 | -0.72 | -0.71 | -0.01 | -0.01 |
| | (0.66)[0.04] | (0.67)[0.05] | (0.31)[0.16] | (0.35)[0.15] | (0.15)[0.69] | (0.19)[0.66] |
| Post × HC Redistricted × Aligned | 2.32 | 2.26 | 1.68 | 1.72 | 0.12 | 0.18 |
| | (0.77)[0.03] | (0.78)[0.03] | (0.80)[0.14] | (0.82)[0.14] | (0.42)[0.79] | (0.43)[0.70] |
| Post × LC Redistricted | | 0.23 | | 0.05 | | -0.01 |
| | | (0.21)[0.43] | | (0.31)[0.86] | | (0.19)[0.69] |
| Post × LC Redistricted × Aligned | | -0.31 | | 0.69 | | 0.82 |
| | | (0.39)[0.46] | | (0.51)[0.17] | | (0.76)[0.38] |
| Observations | 415 | 415 | 415 | 415 | 415 | 415 |
| Adjusted $R^2$ | 0.037 | 0.032 | 0.063 | 0.061 | 0.197 | 0.201 |
| Test: (Post × HC Redistricted × Aligned) + (Post × HC Redistricted) = 0 | | | | | | |
| t- Statistic | 1.10 | 1.05 | 1.29 | 1.35 | 0.29 | 0.44 |
| Wild cluster bootstrap p-value | [0.25] | [0.26] | [0.24] | [0.22] | [0.82] | [0.74] |
| Effect of Competition (Given Alignment) | | | | | | |
| Test: (Post × HC Redistricted × Aligned) = (Post × LC Redistricted × Aligned) | | | | | | |
| t-Statistic | | 3.50 | | 1.22 | | -0.77 |
| Wild cluster bootstrap p-value | | [0.01] | | [0.27] | | [0.51] |
| Test: (Post × HC Redistricted) = (Post × LC Redistricted) | | | | | | |
| t-Statistic | | -3.18 | | -2.48 | | 0.12 |
| Wild cluster bootstrap p-value | | [0.04] | | [0.19] | | [0.91] |

**Note.** This table estimates the same regression specifications as Table 5, but with alternative definitions of private and public goods. The dependent variable is a standardized measure of annual per-HH benefits for each village. Observations are at the village-year level, 2004-2008. Post takes value 1 for years 2007 and onwards. *HC Redistricted* refers to those cases where the village was redistricted to an assembly constituency with a smaller gap in vote share between winner and runner up. *LC Redistricted* refers to those cases where a village was redistricted to an assembly constituency with an equal or a larger gap in vote share between winner and runner up. *PS* refers to *panchayat samiti*, and *Aligned* means same party is in power at both the PS and GP levels. *Employment* consists of panchayat-provided employment, MNREGA and MPLAD employment. *Water* refers to drinking water. *Private benefits without employment* include IRDP credits, agricultural minikits, ration cards, houses, toilets, and drinking water. *Public benefits with water* consist of roads, irrigation and drinking water. All specifications include other interaction terms, whether MLA/MP was part of delimitation committee, and village and year fixed effects. Robust standard errors are in parentheses, clustered at *panchayat samiti* level. Wild bootstrapped *p*-values clustered at *panchayat samiti* level are in square brackets. The standardized mean (std. dev.) is 0.50 (0.10) for per household employment benefits, 0.53 (0.09) for per household private benefits without employment, and 0.28 (0.32) for imputed public good benefits with water.

Columns 3 and 4 of Table 7 present results from a placebo test using data for the period 2004-2006, with the redistricting date hypothetically moved ahead by one year to the end of 2005 (so 2006 constitutes a post-redistricting year, while 2004 and 2005 are prior years). These are contrasted with the 'correct' specification in columns 1 and 2, restricted also to a three year window (2005-07) around the actual year of redistricting (end of 2006, so 2007 is a post-redistricting year, while 2005 and 2006 constituted prior years). While results similar to previous tables hold in the correct specification, they fail to do so in the placebo columns. Columns 5 and 6 present results for a specification with placebo treatment groups. These groups are constructed as follows: we take the sub-sample of villages that were not redistricted in 2006. For all these villages, there was no change in competition at the

**Table 7: Placebo Tests**

| | Main Specification (*pre:* 2005-2006) (*post:* 2007) | | Placebo Shock (*pre:* 2004-2005) (*post:* 2006) | | Placebo Treatment (*pre:* 2004-2006) (*post:* 2007-2008) | |
|---|---|---|---|---|---|---|
| | Private | Public | Private | Public | Private | Public |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post × HC Redistricted | -1.80 | 0.21 | 0.28 | -0.49 | 0.22 | -0.73 |
| | (0.47)[0.14] | (0.14)[0.20] | (0.78)[0.69] | (0.81)[0.61] | (0.44)[0.73] | (0.82)[0.75] |
| Post × HC Redistricted × Aligned | 2.77 | -0.62 | -0.51 | 0.73 | -0.67 | 0.42 |
| | (0.85)[0.09] | (0.40)[0.16] | (0.89)[0.50] | (0.90)[0.46] | (0.59)[0.29] | (0.86)[0.86] |
| Post × LC Redistricted | 0.39 | 0.21 | -0.27 | -0.49 | -0.01 | -0.18 |
| | (0.45)[0.38] | (0.14)[0.19] | (0.82)[0.67] | (0.81)[0.62] | (0.47)[0.99] | (0.48)[0.57] |
| Post × LC Redistricted × Aligned | 0.83 | 0.35 | -0.88 | 1.26 | -1.28 | -0.61 |
| | (0.92)[0.36] | (1.03)[0.73] | (0.88)[0.27] | (1.23)[0.29] | (0.66)[0.06] | (0.65)[0.38] |
| Observations | 249 | 249 | 249 | 249 | 350 | 350 |
| Adjusted $R^2$ | 0.040 | 0.124 | 0.084 | 0.134 | 0.064 | 0.212 |
| Test: (Post × HC Redistricted × Aligned) = (Post × LC Redistricted × Aligned) | | | | | | |
| t-Statistic | 2.15 | -0.90 | 0.60 | -0.51 | 1.15 | 1.70 |
| Wild cluster bootstrap p-value | [0.11] | [0.42] | [0.53] | [0.62] | [0.26] | [0.11] |
| Test: (Post × HC Redistricted) = (Post × LC Redistricted) | | | | | | |
| t-Statistic | -13.09 | -0.03 | 1.10 | -0.22 | 0.66 | -1.23 |
| Wild cluster bootstrap p-value | [0.09] | [0.98] | [0.46] | [0.79] | [0.55] | [0.50] |

**Note.** This table provides two types of placebo tests for the main difference-in-differences specification. The first is the *Placebo Shock* test (columns 3-4), which uses data for the pre-redistricting period 2004-2006 and hypothetically moves the redistricting date ahead by one year (end of 2005). Post takes value 1 for 2006. *Redistricted* refers to cases where the GP was redistricted to an assembly constituency where the incumbent party has a lower likelihood of winning based on victory margins. The second is the *Placebo Treatment* test (columns 5-6), which creates placebo treatment groups (constructed randomly) using the sub-sample of villages that were not redistricted in 2006. The time period is 2004-2008. Post takes value 1 for years 2007 and onwards. For both tests, the dependent variable is a standardized measure of annual per-HH benefits for each village. Observations are at the village-year level. *PS* refers to *panchayat samiti*, and *Aligned* means same party is in power at both the PS and GP levels. *Private benefits* include panchayat-provided employment, MNREGA, MPLAD, IRDP credits, agricultural minikits, ration cards, houses, toilets, and drinking water. *Public benefits* refer to roads and irrigation. The per household road benefits are imputed from survey responses using the following procedure: if even a single household reports receiving benefits from roads, that village is considered to have had a road built for that year. All specifications include other interaction terms, whether MLA/MP was part of delimitation committee, and village and year fixed effects. Robust standard errors are in parentheses, clustered at *panchayat samiti* level. Wild bootstrapped *p*-values clustered at *panchayat samiti* level are in square brackets.

*panchayat samiti* level in the period 2004-2008. We then randomly assign a subset of villages into 'Placebo HCR' group and a subset of villages in 'Placebo LCR' group. The post period is 2007 onwards. The results show that there is no significant effect of placebo treatment groups on private and public benefit allocations.

The outcome variable used so far has been the standardized measure of annual per-HH benefits for each village. We also estimate equations 14 and 15 with an alternative dependent variable: the proportion of households receiving at least one benefit annually for each village. Appendix Table A5 shows the qualitative results are unaffected by (a) measuring private benefits by the proportion of households reporting receipt of at least one benefit and (b) measuring public benefits by the actual proportion of households that report having benefitted from a public program. Appendix Table A7 shows that the results of placebo regression exercises are similar to the ones in Table 7.

In summary, the results confirm the predictions of the clientelistic model: we see large effects on the program scales of private benefits and negligible, insignificant effects for public benefits; these results appear only after redistricting occurs. The short time span studied allows us to focus only on short run effects of the redistricting. For various reasons, this is not a problem. We are not interested in the effects of redistricting *per se* and use it only as a source of exogenous shock to political competition to infer the underlying mechanisms of how benefits of different kinds affect voting and how allocation of benefits are manipulated by upper tiers of the government in response. Moreover, we do not expect any long lasting effects, since alignment patterns changed after the 2008 panchayat elections. Political competition changed in the wake of the 2009 parliamentary election and then even more decisively after the 2011 state assembly election.

## 5.2 Household-Level Analysis: Effects of Benefits on Political Support

We now estimate the effects of benefits on political support at the household level. Since there was no survey conducted during the Panchayat term 2004-2008, we do not have data on political support immediately before the redistricting. We therefore examine cross-sectional differences in the likelihood of households expressing support for the GP incumbent in the 2011 household ballots. Column 1 of Table 8 reports OLS regression results for how the likelihood of heads of household voting for the incumbent party varied with number of private and public benefits their household received between 2009 and 2011. We restrict attention to benefits received during this period, because the previous GP elections were held in 2008, so there is a single well-defined incumbent at the GP level after 2008. The regression specification is

$$L_{iv} = \sum_k \nu_k b_{kiv} + \beta X_{iv} + \epsilon_{iv}, \tag{16}$$

where $L_{iv}$ is a dummy for whether the incumbent party was supported by household head $i$ in village $v$ in the 2011 household ballot, $b_{kiv}$ is a standardized measure of the number of benefits of type $k$ reported by the household over the 2009 - 2011 period, $X_{iv}$ is a vector of district fixed effects, household controls, and village controls.[22] In particular, controls include a range of household characteristics, including dummies for SC/ST, religion, landlessness, occupation, and whether the head of household is educated. They also include

[22]Table A11 in the Appendix shows robustness of results when district fixed effects are excluded.

the following GP characteristics: dummies for Left Front control of GP, dummies for Left Front control of PS, and alignment between GP and PS control. Standard errors are clustered at the village level; Table A12 in the Appendix shows the results when standard errors are clustered instead at the district level. Note in particular that the public benefits variable here is the actual report made by the household head, rather than an imputation based on reports made by other households in the same village in the corresponding year.

Column 1 of Table 8 shows a 2% OLS estimate of the effect of a one standard deviation increase in reported private benefits. The corresponding effect for public benefits is -1%. While the effect of receiving private benefits is statistically significant at 10%, the effect of public goods is statistically insignificant.

To address possible sources of OLS bias such as omitted variables (less popular incumbents were motivated to provide more benefits) or reverse causality (benefits targeted to loyal supporters rather than swing voters), we now provide IV estimates, using a strategy similar to that of Levitt and Snyder Jr (1997) to generate an instrument for the supply at the village level (GP budget allocated by the PS), which is then interacted with fixed household characteristics to represent the intra-village allocation. To explain this instrument, we return to the budgeting equation (13), applied to a specific program in a given constituency or district:

$$B_v = \bar{B} + \phi_v - \sum_{v'} n_{v'} \phi_{v'}, \tag{17}$$

where $B_v$ denotes the per-household benefit allocated to village $v$, $\bar{B}$ is the corresponding per-household benefit in the district, $n_v$ is the population share of village $v$ and $\phi_v$ is the 'political deservingness' of village $v$, representing the product of 'competitiveness' $\frac{Rp'}{d}$, alignment $I_i I_v$ and $\nu_v$ the vote generating effectiveness of the program in village $v$. Since the political deservingness of a village is related to the voting propensities of its residents, equation (17) shows the pattern of reverse causation that biases the OLS estimate of the effect of benefits on votes in regression (16).

Assuming that the political deservingness of different villages are drawn from an i.i.d. distribution conditional on a district-specific parameter, we can take a random sample $I$ of *other* villages in the district. For any such village $v'$ in $I$, the same budget equation (17) applies; hence,

$$B_{v'} = \bar{B} + (1 - n_{v'})\phi_{v'} - n_v \phi_v - \sum_{v'' \neq v, v'} n_{v''} \phi_{v''}. \tag{18}$$

As the population share of each village within the district goes to zero, equation (18) im-

**Table 8:** Effect of Benefits on Votes for Incumbent in 2011

| | OLS | IV Regression | | |
|---|---|---|---|---|
| | | First Stage | | Second Stage |
| | | Private | Public | |
| | (1) | (2) | (3) | (4) |
| Private Benefits | 0.02 | | | 0.13 |
| | (0.01)[0.09] | | | (0.06)[0.04] |
| Public Benefits | -0.01 | | | -0.08 |
| | (0.02)[0.45] | | | (0.10)[0.43] |
| $S_{d(v)}$ | | -0.87 | -0.47 | |
| | | (0.21)[0.00] | (0.18)[0.01] | |
| $S_{d(v)}$ × SC/ST | | 0.14 | -0.17 | |
| | | (0.08)[0.06] | (0.09)[0.06] | |
| $S_{d(v)}$ × Landless | | 0.03 | -0.04 | |
| | | (0.06)[0.65] | (0.06)[0.55] | |
| $S_{d(v)}$ × No Education | | 0.19 | 0.14 | |
| | | (0.06)[0.00] | (0.07)[0.05] | |
| $S_{d(v)}$ × Hindu | | -0.11 | -0.13 | |
| | | (0.14)[0.42] | (0.13)[0.32] | |
| Observations | 2383 | 2383 | 2383 | 2383 |
| Adjusted $R^2$ | 0.174 | 0.239 | 0.424 | 0.129 |
| District FE | YES | YES | YES | YES |
| F-Test of excluded instruments | | 21.24 | 5.50 | |
| [p-value] | | [0.00] | [0.00] | |
| Rank Test [p-value] | | | | 12.30 [0.02] |
| Weak-Instrument-Robust Tests: | | | | |
| Conditional Likelihood Ratio test [p-value] | | | | 5.55 [0.09] |
| *J*-Overidentification test [p-value] | | | | 4.23 [0.24] |

**Note.** This table presents OLS estimates for equation 16 and IV estimates for equation 19 in section 5.2. The dependent variable is whether the respondent voted for the incumbent party in majority at the GP. *Private* and *public* benefits are standardized and aggregated over the period 2009-2011. All specifications include district fixed effects, household (HH) characteristics and GP characteristics. HH Characteristics include SC/ST, religion, landlessness, occupation, and level of education of household head. GP characteristics include dummy for left GP, dummy for left *panchayat samiti* (PS) and dummy for alignment between GP and PS. Endogenous variables: private and public benefits. Excluded instruments: standardized aggregate per capita total benefits ($S_{d(v)}$) and $S_{d(v)}$×HH characteristics. HH characteristics used for instruments are SC/ST, landless, no education and religion dummies. Robust standard errors are in parentheses, clustered at village level. P-values clustered at village level are in square brackets. The mean proportion of households voting for incumbent party in majority at the GP is 0.52 and the standard deviation is 0.50.

plies the cross-village correlation between $B_{v'}$ and $\phi_v$ goes to zero, while the correlation of $B_{v'}$ with its own deservingness $\phi_{v'}$ is bounded away from zero. Hence, for any given village $v$, the average per household benefit of other villages in the district is approximately orthogonal to $\phi_v$, besides helping predict $B_v$ (using (17)), making it an asymptotically valid instrument for $B_v$. Even if the population shares of each village are not close to zero, they are typically less than $\frac{1}{2}$, so the bias in the IV estimator will be smaller than that of the OLS estimator.[23]

Hence, we instrument program scale by program scales in other villages in the same district, interacted with dummies for fixed household characteristics $H_{iv}$ such as caste, landlessness, education, and religion (significant determinants of within-GP targeting) to predict the delivery of benefits to individual households. We include controls for these characteristics to capture their direct effects on voting propensities. The first-stage and second-stage regression specifications are as follows:

$$\begin{aligned} \textit{First Stage:} \quad & b_{ivk} = \tau_1 S_{d(v)} * H_{iv} + \tau_2 S_{d(v)} + \tau_3 H_{iv} + \tau_4 X_{iv} + \eta_{ivk} \\ \textit{Second Stage:} \quad & L_{iv} = \sum_k \nu_k \bar{b}_{ivk} + \rho_1 H_{iv} + \rho_2 X_v + \epsilon_{iv}, \end{aligned} \tag{19}$$

where $\bar{b}_{ivk}$ denotes predicted benefits of type $k$ received by the household, obtained from the first-stage regression, and $S_{d(v)}$ denotes per capita benefit across all sample villages in the same district level after excluding village $v$.

The F-statistics of the first stage regressions (columns 2 and 3 in Table 8) for private and public good benefits are 21 and 6, respectively. The less-than-full-rank test for identification is rejected with a p-value of 0.02; hence, the instruments provide enough independent variation in the two endogenous variables. In the bottom panel, we present two weak-instrument-robust tests: the conditional likelihood ratio (CLR) test statistic for joint significance of coefficients of the two endogenous variables (which is significant at the 10% level) and the Hansen *J* test of overidentifying restrictions (which is not rejected at the 10% level).[24]

Column 4 shows a 13% IV estimate for effect of private benefits on the likelihood of the household head voting for incumbent, much larger than the OLS estimate. A $z$-test for

[23] The coefficient of $\phi_v$ in expression (18) for $B_{v'}$ equals $n_v$, whereas its coefficient in expression (17) for $B_v$ equals $1 - n_v$.

[24] Andrews et al. (2019) show that in the single regressor over-identified case, the CLR is superior to other tests and they explicitly recommend using CLR if errors are homoskedastic. Our context is more complicated compared to these cases as we have two endogenous regressors, besides non-homoskedastic errors. There appears to be no consensus on what inference procedures to use in this context.

**Table 9:** Pooled Voting Regressions with Household Fixed Effects

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Private Benefits | 0.03 | 0.03 | 0.03 | 0.03 |
| | (0.01)[0.02] | (0.01)[0.02] | (0.01)[0.02] | (0.01)[0.02] |
| Public Benefits | -0.02 | -0.02 | -0.02 | -0.02 |
| | (0.02)[0.15] | (0.01)[0.14] | (0.02)[0.15] | (0.01)[0.17] |
| Left GP | | -0.22 | | -0.23 |
| | | (0.05)[0.00] | | (0.06)[0.00] |
| Left PS | | 0.00 | | -0.04 |
| | | (0.05)[0.98] | | (0.07)[0.61] |
| Aligned GP and PS | | 0.08 | | 0.06 |
| | | (0.05)[0.15] | | (0.06)[0.27] |
| Observations | 4766 | 4766 | 4766 | 4766 |
| Adjusted $R^2$ | 0.004 | 0.033 | 0.004 | 0.035 |
| Time Period Dummy | NO | NO | YES | YES |

**Note.** This table reports the results from pooled OLS regressions with household fixed effects. There are two time periods: 2004 and 2011 (corresponding to the two survey rounds). The dependent variable is whether the respondent voted for the incumbent party in majority at the GP. *Private* and *public* benefits are standardized and aggregated over 2001-2003 for the first period and over 2009-2011 for the second period. Specifications in columns (2) and (4) include GP characteristics: dummy for left GP (i.e., if a majority of seats in a GP are held by Left party), dummy for left *panchayat samiti* (PS) and dummy for alignment between GP and PS. Robust standard errors clustered at village level are in parentheses. P-values clustered at village level are in square brackets. The mean proportion of households voting for incumbent party in majority at the GP is 0.52, and the standard deviation is 0.50.

equality of OLS and IV estimates of effects of private benefits is rejected with a p-value of 0.04. Even if the IV is not unbiased, it is likely to be less biased than the OLS estimate, so the contrast between the OLS and IV estimate indicates the OLS bias is negative. This is consistent with the hypothesis that weaker incumbents provide more benefits and with the targeting of benefits to swing rather than loyal voters. In contrast, the effect of receipt of public benefits is negative and statistically insignificant. So the evidence in favor of clientelism continues to be upheld at the household level, and the responsiveness of voters to private and public benefits mirrors the pattern of manipulation of GP budgets by upper tiers shown in the previous section.

Since the instruments for benefits in the preceding results included interactions of predicted program scales with household characteristics, it may give rise to concerns for potential endogeneity of intra-village targeting patterns of benefits. Note however that the regression controlled for the household characteristics to capture their direct effect on vot-

ing propensities. This still leaves open the possibility that benefits were selectively targeted by the incumbent to its loyal supporters and observed household characteristics are imperfect proxies of political loyalties. To partially address this concern, we pool the data from the 2011 survey round with the previous round conducted in 2004 with the same set of households. In the 2004 survey, the same household heads were asked similar questions about benefits they had received in past years from various GP programs and also asked to privately cast a ballot among competing political parties. This allows us to regress an indicator for vote cast for the incumbent in the two survey rounds on benefits received in the past three years and control for household fixed effects. Additional controls include dummies for Left Front majority in the GP and PS. The results are shown in Table 9. We see a 3% effect of private benefits on the likelihood of voting for the incumbent, which is statistically significant at the 5% level. The corresponding effect of public benefits is -2% (p-value .15). These estimates are therefore similar to the OLS results from the 2011 survey round.[25]

# 6 Addressing Possible Concerns or Alternative Explanations

We now consider possible concerns with our analysis, including alternative explanations that do not rely on the prevalence of clientelism. At the outset, we reiterate that by using data on self-reported benefits, our approach does not rely on the assumption that voters value public benefits at least as much as private benefits. It relies instead on the assumption that those who report benefitting from public goods value them to some extent. The key evidence is that despite a large proportion of households reporting that they benefitted from public benefits, they did not reciprocate with their political support for local incumbents. This is mirrored in the corresponding lack of responsiveness of program budgets for public good programs at the village level in response to changes in electoral competition induced by redistricting.[26]

[25] The reader may wonder about the robustness of the cross-sectional results of Table 8 when applied to the 2004 survey data. The OLS estimates turn out to be similar, while the IV analysis could not be replicated, owing to failure of the rank identification test and the over-identification tests. For this reason, we do not report the results of the cross-sectional analysis for the 2004 survey data.

[26] Admittedly, this still leaves open the possibility that our tests to detect significance for public benefits lack statistical power. However, standard errors associated with public good effects are not larger than those for private goods.

A potential alternative explanation for different effects for private and public benefits in a programmatic politics setting could arise from corruption, the scope for which is greater in public good programs (e.g., because citizens are less well-informed about the costs of building local infrastructure, while they know what to expect in terms of private benefits based on stated policy platforms). This would imply that the composition of government expenditure is distorted in favor of public good programs. Competitive pressure and re-election motives would then induce politicians to reallocate budgets in favor of private benefits in aligned constituencies. However, we should also expect to see significant reductions in the supply of public benefits in such constituencies, which we do not see. Moreover, under programmatic politics, citizen votes should respond positively to both private and public good benefits – a prediction that is contrary to our results.

Consider next the possibility of measurement error in benefits, arising from potential recall bias of households owing to surveys being conducted retrospectively. Could the results of Table 8 be due to greater recall bias with respect to public benefits? On the face of it, this seems implausible in the West Bengal context, as most local road projects (which constitute the vast majority of public benefits) implemented by the local government are clearly marked with a permanent sign recording the date of construction and the role of the GP (and the funding source) in building the road. Moreover, this explanation cannot account for the results in Table 8, where households that recalled benefitting from public goods did not respond with their political support in the same way as when they recalled benefitting from private goods. To further check the possibility of recall bias for road benefits, Table A1 in the Appendix checks the extent of within-village clustering of household responses to road benefits. It shows a very high within-village-year clustering of household responses of road benefits: in 81% of village-years, the proportion of household-pairs reporting the same benefit exceeded 95%.[27]

The motive for household heads to cast a vote in our straw ballot may be questioned — if they believed the poll was anonymous and private, they should not fear any consequences from local parties, and so clientelistic forces should no longer affect how they vote. We agree with this concern, but note that it would, if anything bias our results against finding evidence of clientelism. The straw ballot was conducted within a few months of the state assembly election, and we think it is plausible they cast a vote in the same way in the two polls.

A third possible explanation of our findings is that there are long gestation lags between

[27] In comparison, the same was true for private benefits in 45% of village years.

approval and completion of road projects. In that case budget approvals in any given year would not translate into roads completed in the same year (or next one or two years). Since our results in Table 5 are based on aggregating household reports of benefits from (presumably completed) road projects in a single year, could it explain why this measure of public benefits does not react to contemporary shocks in electoral competition? We do not think so, for the following reasons. First, using administrative data for roads constructed under the MPLAD program in West Bengal, we find that from the day of approval, the average time taken to complete construction was 154 days, and the standard deviation was 85 days. Hence, most village programs are completed well within one year. Second, it would not explain the results in Tables 8 and 9, which show household votes' lack of responsiveness to receipt of benefits from a road project completed that year.

Another concern with our explanation could be the underlying assumption of nonexcludability of local public goods. If certain groups of residents could be prevented from using them, discretion could be used by political incumbents in the allocation of access based on voting patterns. While this may be partially true in the case of some public amenities (such as drinking water taps), we think it is less plausible in the case of roads, since Appendix Table A1 shows a high degree of clustering of household responses concerning road benefits within any village-year. Moreover, it would also not explain the results of Table 8: household heads' votes do not respond to the public goods they did report benefitting from in the same way that they respond to private benefits.

It is also conceivable that the logic of clientelistic politics could apply at the group (village or neighborhood) level, where an entire village or neighborhood that votes against the incumbent can be punished collectively if the incumbent is re-elected, by cutting back funds for local public goods. Compared with individual sanctions, such group sanctions would be less likely to be effective in controlling voting behavior. While our model abstracted from the possibility of such group sanctions, we concede that they could be operative. However, we would then expect to see some significant effects for public benefits at the village or individual household level. As the evidence does not show such effects, we infer that group sanctions were either not operative or not effective.

# 7 Conclusion

This paper shows that under clientelism, private benefits are effective in generating votes, but public goods are not. The empirical evidence for this is provided in two different

ways. One examines changes in the allocation of local-government program benefits across villages as a result of exogenous shocks to political competition. The other studies how the political support expressed by individual heads of household responded to variations in benefits they received, instrumented by variations in average program scale at the district level. The results corroborate each other in a manner predicted by a theoretical model of politically manipulated budgets.

Identifying the patterns of resource allocation consistent with political clientelism is an important first step towards assessing its implications for development. Clientelism can potentially lead to three main distortions. First, since voters are less responsive to receiving benefits from infrastructure projects, there could be an under-provision of public goods as a consequence of clientelism. Second, since inter-village allocation of benefits depends on political alignment across the tiers of governments, clientelism is a source of inequality in resource allocation *across* regions. Third, it is possible that the discretion allowed to local politicians could result in resources being diverted or misused for corrupt purposes. However, on the other hand, clientelism could possibly lead to better targeting of resources *within* local jurisdictions. Local political brokers have better information about potential beneficiaries, which can be exploited by elected officials for redistribution of private benefits or provision of insurance against shocks. If the distortions generated by clientelism are bigger than the gains from better targeting of resources, switching from discretionary allocation of programs benefits to rule-based allocation may be desirable.[28] The potential gains of adopting such alternative policies and the welfare implications of clientelism in general are left for future work.

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