

Pro-poor targeting and accountability of local governments in West Bengal

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Accepted 24 January 2006

Abstract

A commonly alleged pitfall of decentralization is that poverty, socio-economic inequality and lack of political competition allow local elites to capture local governments. This hypothesis is empirically examined using a longitudinal sample of 89 West Bengal villages concerning targeting of credit, agricultural input kits, employment programs and fiscal grants spanning the period 1978–98. Higher poverty, land inequality and low caste composition of the poor was associated with negligible adverse effects on targeting of private goods to the poor *within* villages, but with lower employment generation out of allotted funds, and significantly lower allocation of resources to the village as a whole. Political competition or literacy levels among the poor were not systematically related to targeting.

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JEL classification: D73; H11; H42; H72; O17

Keywords: Poverty; Targeting; Accountability; Decentralization of service delivery; Corruption; West Bengal

1. Introduction

Decentralization of service delivery to local governments has recently been embraced by a large number of developing countries in order to increase responsiveness and accountability to poor and vulnerable groups (see, e.g., the [2004 World Development Report](#)). The principal concern with such initiatives is the danger that local governments may be subject to ‘capture’ by

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local elites, wherein targeting performance and responsiveness to the needs of the poor and minorities may deteriorate.¹ [Dreze and Sen \(1989\)](#) express this concern as follows:

“The extent of economic distress experienced by different individuals is, to a great extent, a matter of common knowledge within a given rural community. An apparent solution to the selection problem would take the form of making the selection process rely on local institutions to allocate public support according to individual needs. Would this method work in practice? The leaders of a village community undoubtedly have a lot of information relevant for appropriate selection. But in addition to the informational issue, there is also the question as to whether the community leaders have strong enough motivation – or incentives – to give adequately preferential treatment to vulnerable groups. Much will undoubtedly depend on the nature and functioning of political institutions at the local level, and in particular on the power that the poor and the deprived have in the rural community. Where the poor are also powerless – as is frequently the case – the reliance on local institutions to allocate relief is problematic, and can end up being at best indiscriminate and at worst blatantly iniquitous, as numerous observers have noted in diverse countries.” ([Dreze and Sen, 1989](#), p. 107)

These dangers have been emphasized particularly in rural communities characterized by high levels of poverty, inequality in land, social status, literacy or political power. Accordingly, it is widely believed that effective political competition and a reasonable level of asset equality and literacy are necessary preconditions for decentralization to achieve improved accountability.

Most of this discussion has been based, however, on anecdotal accounts and case studies. More systematic evidence based on larger samples are conspicuous by their absence, owing partly to the paucity of available data.² The aim of this paper is to examine how pro-poor targeting varied with poverty, inequality and political composition of local governments (*panchayats*) in the state of West Bengal, India. The West Bengal experience with decentralized implementation of development programs is unique insofar as it has spanned over a quarter century to allow estimation of long run effects of changing patterns of poverty, landownership and demographics on targeting. Our analysis is based on a longitudinal data set we have assembled covering over 80 villages for selected years spanning the period 1978–98, which witnessed four successive sets of elected local governments.

The principal responsibilities devolved to the West Bengal panchayats were the selection of beneficiaries of subsidized farm inputs and credit, implementation of land reforms, local infrastructure projects and welfare programs. We exclude an analysis of the land reform program, since this is the topic of a companion paper ([Bardhan and Mookherjee, 2003](#)). This paper examines the distribution of credit (under the Integrated Rural Development Program (IRDP)), agricultural minikits (containing seeds, fertilizers and pesticides), employment programs geared towards construction of local infrastructure, and fiscal grants covering a host of development and welfare programs. We examine targeting both within and across villages, reflecting resource allocation decisions at different levels of local government.

¹ See [Bardhan \(1996, 2002\)](#), [Bardhan and Mookherjee \(2000, 2005, 2006\)](#), [Bird \(1995\)](#), [Crook and Manor \(1998\)](#), [Dreze and Sen \(1989\)](#), [Lieten \(1996\)](#), [Manor \(1999\)](#), [Mathew and Nayak \(1996\)](#), [Mookherjee \(2004\)](#), [Prud'homme \(1995\)](#), [Tanzi \(1996\)](#), and the 2004 World Development Report.

² Notable exceptions are [Galasso and Ravallion \(2005\)](#) in the context of an education program in Bangladesh, and [Ravallion and van de Walle \(2002\)](#) for a land redistribution program in Vietnam.

Our empirical strategy is to examine changes over time of targeting with measures of local poverty, inequality and political competition, controlling for productivity differences (between the poor and non-poor, and between villages) and other village characteristics. If increased poverty (measured, e.g., by the proportion of households that owned little or no land) did not adversely affect the political power of the poor, nor their relative productivity, then per capita allocations to the poor should not decline (controlling for the overall scale of the program). In that case the share of resources delivered to the poor within a village should increase at least proportionately to their demographic weight. Similarly, if poverty rose within a village while the average productivity of residents of that village did not change, an equity-sensitive allocation process should devolve more resources to that village. Consequently, an observed failure of the poor's share to rise with their demographic share within villages (or a reduction in the per capita allotment to a village when it became poorer on average but no less productive) must reflect a reduction in the political power of the poor as their poverty grows, a phenomenon sometimes referred to as 'elite capture'. To test the 'elite capture' hypothesis, we therefore examine correlations of observed targeting with measures of economic and social vulnerability of the poor, controlling for productivity differences and other relevant village characteristics. We also examine the effect of differences in political composition of the panchayats, which reflect the role of electoral competition and differences in political ideology between the principal political parties in the state.

Our principal finding is that intra-village allocations of private goods (credit and minikits) reveal relatively little evidence of elite capture. Shares of the poor in the allocation of credit or kits were high on average, close to their demographic shares and larger than their land shares. While there is some evidence that greater poverty and land inequality worsened intravillage targeting in these programs, the quantitative magnitude of these effects were small compared with corresponding inter-village targeting biases.³ An increase in the proportion of poor (defined as landless or marginal landowners) that were of low caste status (i.e., belonging to scheduled castes or tribes (SC/ST)) by 5%, or in land inequality (with land shares of big or medium landowners rising by 10%), was associated with a (statistically significant) reduction in (per household) minikits or grants allocated to the village by approximately 25%. On the other hand, we find no significant association of targeting with illiteracy rates among the poor, or composition of local panchayats (within villages). We argue in Section 4 that the observed correlations between targeting and poverty or inequality most likely reflect patterns of political power within local governments, as the regressions control for plausible channels of reverse causality or omitted variable bias.

The main implication of our findings is that contrary to concerns expressed most frequently in the decentralization literature, 'elite capture' in the credit or kit distribution programs did not manifest itself *within* villages. The weakness of decentralization in West Bengal seems to have been instead in the administration of local employment generating programs, and in the process of inter-village allocation. Local governments selected local projects that generated *less* employment for the poor when landlessness and land inequality within the village became greater. Hence administration of local public good programs displayed a greater tendency towards elite capture, compared with the allocation of private goods, possibly because public good programs are inherently less transparent. The inter-GP allocation of resources operated

³ The only exception to this concerns the intra-village share of scheduled castes and tribes in the IRDP credit subsidy, which worsened significantly when land inequality rose.

with a bias against villages with high land inequality and large incidence of low caste status among the poor. This possibly reflected the lack of transparency of the inter-GP allocation process at the block level, where grant allocations were the outcome of political discretion, in combination with lobbying by representatives of different villages. These results are consistent with the view that under the Left Front state government in West Bengal, medium and small landowners were politically favored or more vocal in their dealings with higher levels of government, compared with big or marginal landowners, landless, or members of scheduled castes or tribes.⁴ The effect was to discriminate against villages whose need for the resources were greater.⁵ This suggests that formula-bound inter-village allocations of fiscal grants would have significantly improved targeting, as has already been observed in the recent decentralization experiences of Bolivia and South Africa.⁶

Section 2 describes the institutional background to the West Bengal *panchayat* system in operation since 1978. Section 3 explains the conceptual framework underlying ‘elite capture’ hypotheses and their testable implications. Section 4 discusses the assumptions underlying our identification strategy, and why we believe they are reasonable. Section 5 describes the nature of the data used, and possible sources of measurement error. Section 6 presents the main empirical results, while Section 7 concludes.

2. Institutional background

Article 40 of the original Indian Constitution states that “the State should take steps to organise village panchayats and endow them with such powers and authority as may be necessary to enable them to function as units of self-government”. However this article was in the nature of a directive principle for state policy, responsibility for the implementation of which was devolved to state governments. In 1957, the Balwantarai Mehta Committee of the government set out a detailed set of suggestions for establishment of a three-tier system. Following this West Bengal passed a Panchayat Act in 1957 and a subsequent Zilla Parishad Act in 1963. However, these panchayats were devolved few responsibilities, financial support was lacking, elections were not held regularly, and involved little or no popular participation.

In 1977 the Left Front alliance came to power at the state government, displacing the previous Congress (I) government. Since then, the Left Front has been reelected with an absolute majority in five successive elections to the state legislature. Upon assuming power at the state, the two top priorities of the Left Front government were land reforms and village democracy. With regard to the latter they created a three tier system of local governments (*panchayats*), along the lines of the earlier recommendations of the Balwantarai Mehta Committee. The three tiers were at the district (*zilla parishads (ZP)*), block (*panchayat samiti (PS)*) and village (*gram panchayat (GP)*)

⁴ Many political commentators on West Bengal (Nossiter, 1988; Lieten, 1992; Bhattacharya, 1999) have described the Left Front government pursuing a ‘politics of middleness’, discriminating against big landowners their traditional ‘class enemy’, but also deviating from its traditional ideology of representing the interests of the poor, especially when it conflicted with those of medium and small landowners.

⁵ The results cannot be explained by an expectation among higher level officials that villages with greater land inequality or low caste status among the poor would be susceptible to elite capture resulting in wasteful diversion of resources, both for the reason that there is little evidence for such elite capture within villages, and also because our inter-village targeting regression controls for intra-village targeting.

⁶ See, for example, Faguet (2006) and Wittenberg (2006) for an account of how the use of need-based formulae for interregional allocations resulted in significant improvements in equity in the recent decentralization programs in Bolivia and South Africa respectively.

levels. On average, a GP covers 8–15 villages and a population of around 12000. A system of mandatory elections once every five years to these governments started in 1978, with direct elections for seats in all three levels. Each government is comprised of a number of seats (between five and twenty-five) and operates as a council that makes decisions collectively. At the district and block levels, the councils include representatives from lower level governments, and corresponding officers of the state bureaucracy (e.g., the District Magistrate in the ZP). Each GP corresponds to between eight and ten constituencies on average electing two members each.

Reforms to the system were created in 1985 and 1993 to shift responsibilities to the village governments away from bureaucratic officials in concerned ministries of the state governments, and to create a bottom-up budgeting system. The principal responsibilities entrusted to the *panchayats* included implementation of land reforms, of the two principal poverty alleviation schemes (the IRDP credit program, and employment programs such as Food for Work (FFW), National Rural Employment Program (NREP), Rural Labour Employment Guarantee Program (RLEGP) in the 1980s which were merged into the *Jawahar Rozgar Yojana (JRY)* from 1989 onwards), distribution of subsidized agricultural inputs (in the form of minikits containing seeds, fertilizers and pesticides), local infrastructure projects (including roads and irrigation), and miscellaneous welfare schemes (old-age assistance, disaster relief, housing programs for the poor, etc.). The capacity of these local governments to raise local revenues is limited: approximately 80% of GP fiscal resources in our sample derived from various schemes sponsored by the central and state governments.⁷ The role played by the GPs therefore consisted mainly in selection of beneficiaries of various development programs handed down from the central or state government. They hardly played any role in the delivery of education or health services to residents: operation of primary or secondary schools and medical clinics still remain under the control of state government officials.

Below we describe in further detail some aspects of the three most important development programs implemented by West Bengal *panchayats*.

2.1. IRDP

The IRDP program started in 1978. From 1980 onwards, it covered all blocks in the country. It replaced a number of different programs with a single integrated package of technology, services and assets aimed at improving the earning capacity of the rural poor. The most important component was a loan offered to the recipient, a certain fraction of which was a subsidy which did not have to be repaid. The target groups were scheduled castes and tribes, agricultural workers, artisans, marginal and small farmers not owning more than 5 acres of land. The subsidy rate was highest (50%) for scheduled castes and tribes, and lower (ranging from 25% to 33%) for others depending on how much land they owned. A certain fraction was earmarked for women and scheduled castes and tribes. The loans were usually given to enable recipients to invest in assets required in service professions (such as artisan tools, retail shops or rickshaws), livestock and agricultural implements. The loans were channeled through ‘lead’ commercial banks located near the villages. The *panchayats* usually selected a number of loan applicants from within each village and forwarded their applications to the local lead bank, with the ultimate loan decision made in consultation between officers of the bank, block officials, and

⁷ This excludes the IRDP and agricultural minikit distribution programs, which are also sponsored by central and state governments.

officers of the District Rural Development Agency (DRDA), a nodal agency of the state government.

Within participating villages in our sample, the total volume of credit subsidy in any given year was Rs. 6700 (in 1980 prices), amounting to about Rs. 24 per household. The subsidy component comprised approximately 80–85% of the actual loan amount. The average subsidy component of an individual loan was Rs. 826, with eight households (out of a population of about three hundred) on average receiving a loan. Therefore, participation within the village was highly selective. Moreover, the scale of the program was not large: the amount of the subsidy for the average loan recipient amounted to three weeks wages for farm workers.

2.2. Agricultural minikits

An important component of agricultural policy comprised the distribution of minikits containing seeds of high yielding rice varieties, potatoes, mustard, sesame, vegetables, fruits and lentils, besides fertilizers and pesticides. These were distributed by the block offices of the state's Agriculture department, in consultation with panchayat officials. In the sample villages the bulk of these were accounted by rice, potato and oilseeds. In our sample, only a small fraction of villages (i.e., below 15%) were excluded from the program. But within villages the program was small and selective: approximately one in every seven households in participating villages received a kit.

2.3. Employment programs

Employment generation for the rural landless is a major instrument of policy for alleviating poverty in India. In 1980, the Food for Work program was replaced by the NREP and RLEGP whose objectives were to generate employment for the landless, with a preference for scheduled castes and women. The stated objective of the RLEGP was to provide at least one member of every rural landless labour household with upto a hundred days of employment in a year. The projects would involve construction of local infrastructure, especially roads and irrigation. In 1989, these various employment programs were merged into the JRY, a single comprehensive program. All these programs are sponsored by the central government, with matching contributions from the state government. In West Bengal, significant responsibility for implementing these projects was devolved to the panchayats, in contrast to other states. The programs were coordinated by the ZPs, while detailed selection of project, organization and supervision were delegated to the GPs. About 20% of the funds were retained by the ZP for funding district wide infrastructure projects, with the rest distributed across villages in a uniform fashion. Numerous restrictions concerning utilization of the funds were imposed on the GPs, especially with respect to the proportion of labor and material costs, and sometimes also with respect to the kinds of projects that could be selected. Additional problems included shortages and delays in receipt of funds, resulting in underutilization of officially sanctioned amounts. The problem originated often in shortages and delays in receipt of Central government funds by the state government, with further compounding in disbursements to lower level governments. Hence actual funds received and utilized frequently fell short of the allocations that the panchayats were entitled to.

The scale and range of the employment program was far more significant than the IRDP. In our sample, practically no village was excluded from the program in any year, and among those villages that received the grants, the amount received was approximately Rs. 195 per household

at 1980 prices. This was approximately eight times the scale of the IRDP. Yet the average support per landless household amounted to no more than 10 days’ wages for farm labor.

3. Conceptual issues

Since little is known about the exact process by which local governments select recipients, we focus instead on the implications or outcomes of those processes in the tradition of revealed preference theory. That is, we summarize the outcome of the political process by which resource allocations by local governments are conducted by the maximization of a weighted sum of utilities of different classes of citizens, with the pattern of political accountability summarized by the imputed welfare weights.

Consider first intra-village allocations decided by a local government, in a village partitioned into different landowning classes c with demographic weights β_c . The relative welfare weights ω_c on different classes that intra-village allocations reflect their respective roles in the political process: as voters, concerned citizens and pressure groups. These will depend ultimately on socio-economic inequality within the village and the nature of political competition. Specific political economy theories generate different predictions of how such weights are determined: examples of ‘elite capture’ mechanisms are provided in [Grossman and Helpman \(1996\)](#) and some of our earlier work ([Bardhan and Mookherjee, 2000, 2003](#)). Without subscribing to any specific mechanism, we merely summarize the pattern of political accountability by the dependence of these welfare weights on local poverty, land inequality, illiteracy and low-caste status among the poor, and on the political composition of local governments. Let λ_{vt} denote the vector of the latter variables that ultimately determine the pattern of these welfare weights. The ‘elite capture’ hypothesis can be stated as follows: the welfare weight assigned to the poor vis-à-vis the non-poor is declining in the extent of poverty (i.e., demographic share of the poor), their illiteracy and low-caste status, in the extent of land inequality within the village, and in the lack of political contestability of local elections.

However, these welfare weights are not directly observable, and have to be inferred from actual targeting patterns. Consider therefore the implications of a given set of welfare weights on allocation of a subsidized resource by the village government among local residents. Suppose that allocation of input f_c to a representative resident of class c enables the latter to generate income y_c by the following profit function

$$y_c = \theta A_c f_c^\mu \tag{1}$$

where θ denotes village-specific productivity, A_c class-specific productivity, and $\mu \in (0,1)$ is the elasticity of income generated with respect to the concerned resource. The class-specific productivity A_c is increasing in the extent of land, education and other assets owned by class c farmers.

Next assume all households share a common homothetic, concave utility function defined over its income:

$$u_c = \frac{y_c^{1-\rho}}{1-\rho} \tag{2}$$

with $\rho > 0, \neq 1$. With a constraint on the aggregate supply of the resource available to the village denoted by \bar{f} , the allocation chosen by the government maximizes $\sum_c \beta_c \omega_c \frac{[\theta A_c f_c^\mu]^{1-\rho}}{1-\rho}$ subject to $\sum_c \beta_c f_c = \bar{f}$.

This generates the following expression for relative per capita deliveries of the input to different classes c, d :

$$\frac{f_c}{f_d} = \left(\frac{\omega_c}{\omega_d}\right)^{\frac{1}{1-\mu(1-\rho)}} \left(\frac{A_c}{A_d}\right)^{\frac{1-\rho}{1-\mu(1-\rho)}} \tag{3}$$

which implies the following expression for the targeting share of the poor within the village:

$$T_p \equiv \frac{\beta_p f_p}{\bar{f}} = \left[1 + \sum_{d \neq p} \left(\frac{\beta_d \omega_d}{\beta_p \omega_p}\right)^{\frac{1}{1-\mu(1-\rho)}} \left(\frac{A_d}{A_p}\right)^{\frac{1-\rho}{1-\mu(1-\rho)}} \right]^{-1} \tag{4}$$

Eq. (3) explains the problem of making inferences concerning the pattern of welfare weights ($\frac{\omega_c}{\omega_d}$) from relative per capita allocations ($\frac{f_c}{f_d}$). If the poor are less productive than the non-poor, and productivity is lower the greater the extent of poverty of the poor, this could justify a lower per capita allocation for the poor, if ρ lies between 0 and 1. If on the other hand ρ exceeds one, then equity considerations would dominate efficiency, and lower productivity among the poor would increase the per capita allocation to the poor. Hence, if ρ is less than one, even a perfectly unbiased utilitarian government would allocate less to the poor, generating a low observed level of targeting, and a negative poverty-targeting correlation. Inferences regarding accountability from observed targeting thus depend on ρ , a parameter of the utility function which is difficult to estimate.

Faced with this problem, we can: (i) estimate the targeting-poverty correlation after controlling for productivity differences between the poor and non-poor classes; (ii) examine targeting of poverty alleviation programs such as the IRDP which are explicitly and entirely earmarked for the poor; and (iii) focus on the correlation between targeting and demographic weight of the poor, because the identification problem is less serious with regard to demographic weights. To illustrate the latter point, suppose we assume that changing demographic patterns do not significantly alter the relative productivity of different size classes.⁸ Then a sufficient condition for the targeting ratio to increase (respectively decrease) in β_p , the demographic weight of the poor, is that the relative (overall) weight $\frac{\beta_p \omega_p}{\beta_d \omega_d}$ of the poor compared with any other class d , is increasing (respectively decreasing) in β_p . In words, the targeting share is increasing in the proportion of poor households as long as their political weight does not fall too fast (i.e., has an elasticity with respect to their political weight which exceeds -1).⁹ Hence, if the targeting ratio declines when there are more poor households, it must be the case that their political weight declines sufficiently.

The targeting Eq. (4) suggests the following regression specification for the intra-village targeting share of the poor:

$$T_{pvt} = \sum_c \delta_c \beta_{cvt} + \gamma \lambda_{vt} + \eta Q(LS_{vt}) + \theta \pi_{vt} + \mu \tilde{f}_{vt} + \nu C_{vt} + \alpha_v + \zeta_t + \epsilon_{vt} \tag{5}$$

⁸ Exceptions could arise in the presence of pecuniary externalities (e.g., through the effect of demographic weights on wage rates, which subsequently affect the marginal profitability of resources for different classes) or scale economies specific to each class (e.g., through learning effects within social networks that overlap substantially with land-ownership class). We shall discuss later how we control for either of these possibilities in our empirical work.

⁹ In the more general non-homothetic case, a sufficient condition for the targeting share to improve with a higher poverty headcount (in the case of two classes, poor and non-poor) is that the poor get less than the non-poor to start with, and their welfare weight is non-decreasing in their demographic weight. In this case, the per capita allocation to the poor increases (relative to the non-poor), so the targeting ratio increases more than proportionately than the poverty rate (Galasso and Ravallion, 2005).

where β_{cv_t} denotes the demographic share of class c in village v in year t ; λ_{v_t} is a vector of variables that determine the extent of elite capture (e.g., illiteracy, low caste status, land inequality), $Q(LS_{v_t})$ is a measure of concentration of local government seats between principal competing parties, π_{v_t} is a measure of relative productivity of the poor; \bar{f}_{v_t} is the per capita resource available to the village as a whole,¹⁰ C_{v_t} is a vector of controls, α_v is a village fixed effect, ζ_t is a common time effect, and ϵ_{v_t} is the residual. Following the discussion above, we infer elite capture if δ_c is significantly less than 1, γ is significantly less than 0, and η is such that targeting is significantly lower when the local government is more concentrated in favor of one party.

Extending the same approach to inter-village targeting, we can view the inter-village allocation decided at a higher level of government (at the district or state level) as the outcome of political discretion of elected officials at that level, in combination with bargaining strengths of representatives of different villages. Representing the outcome of this process by imputed welfare weights to different villages, we can obtain expressions analogous to (4) for the share of any given village as a function of demographic and welfare weights, and productivity of different villages. Since our data pertains to a sample rather than the population of all villages that compete for resources, we cannot estimate the targeting share of each village. Instead, we only observe the total amount of resource allocated to the sample villages. Hence the regression formulation (5) needs to be amended, with the dependent variable constituting the per capita resource \bar{f}_{v_t} allocated to a given village, expressed as a function of the scale of the program \bar{f}_t for the entire state as a whole, and the characteristics of village v in year t :

$$\bar{f}_{v_t} = \delta' \beta_{v_t} + \gamma' \lambda_{v_t} + \eta' Q(LS_{v_t}) + \theta' \pi'_{v_t} + \mu' \bar{f}_t + \nu' C_{v_t} + \psi I_{v_t} + \alpha'_v + \zeta'_t + \epsilon'_{v_t} \tag{6}$$

Here π'_{v_t} denotes the marginal profitability of the resource in village v as a whole, and I_{v_t} the (lagged) intra-village targeting ratio in village v .¹¹ We now interpret the role of the ‘capture’ variables λ_{v_t} as reflecting the political strength of the representatives of village v . If a village experiences an increase in poverty and in response receives a lower amount of resources, despite having the same population, productivity and political composition of the local governments, and despite the overall scale of the program remaining unchanged, it must reflect a decline in the political ‘voice’ of the representatives of that village, or the weight assigned to that village by higher officials.

4. Empirical identification strategy

Our interpretation of the (partial) correlation between targeting and poverty as reflecting the effect of poverty on targeting via its impact on the political power of the poor, relies on the assumption that conditional on the control variables included in the regression, poverty is exogenous with respect to targeting. This requires absence of a reverse impact of targeting on poverty, or of variables omitted from the regression that affect both targeting and poverty, and are unrelated to the political weight assigned by local government officials to the poor.

¹⁰ The scale would matter if the utility function were non-homothetic.
¹¹ The subsequent allocation of the resource within village v is likely to affect the preferences of both village representatives as well as higher level officials with regard to inter-village allocations. For instance, a higher level official may care about how the concerned village government distributes the allocated resource between the poor and the non-poor.

Our principal measures of poverty and inequality are based on the distribution of land – specifically the proportion of households that own no or little land, land shares of small and big landowners – besides measures of vulnerability of the poor – the proportion of landless or small landowning households that are illiterate, or of low caste. The distribution of land, literacy and caste are relatively slow to change, unlike measures of poverty based on income and consumption which are subject to greater volatility. Table 1 shows the results of a farm wage regression, using the farm village panel: the demographic share of landless, marginal and small landowners has a strong negative effect on the wage rate, controlling for literacy, caste, rainfall, nonagricultural occupations, and the land/household ratio (besides village fixed effects, year dummies and the cost of living index for agricultural workers in the nearest center where this index is available). Higher inequality of land shares and incidence of low caste households also depressed the wage, but these effects were statistically insignificant. Hence, demographic shares of different landowning classes can be taken to be a prime determinant of the extent of poverty among the landless.

What were the principal factors underlying changes in the land distribution within the West Bengal villages in our sample? As Table 2 shows, the principal changes were a rising incidence of landlessness (the proportion of landless households rose from 47% to 52%), and growing equality of land shares (a redistribution of the order of 17% of cultivable land from medium and big landowners to small and marginal landowners). These reflected a combination of demographic pressures (population growth and migration), and subdivision of landholdings (via land reform, market sales, and breakup of joint families). Each of these influences – demographic change, migration, land reform, market transactions and household division – could potentially be correlated with targeting. We shall now argue that our regression specification and choice of variables included in the regression are such that none of these potential channels of reverse causality or omitted variable bias are likely to constitute plausible alternative explanations of absence of a positive correlation between targeting and poverty. In the discussion that follows, we use ‘poverty’ to denote not just the incidence of landlessness or

Table 1
Farm wage rate regression

Observations, Groups, $W - R^2$	311, 77, 0.61	311, 77, 0.69
% HH landless	-23.80** (10.26)	-18.02 (11.16)
% HH small	-25.52** (11.41)	-19.22 (11.75)
% HH medium	-13.08 (17.19)	-20.35 (17.91)
% Land small	6.16 (3.70)	4.25 (4.62)
% Land big	-1.92 (7.33)	-6.01 (7.26)
% HH low caste	-2.05 (8.82)	0.08 (8.89)
Cost of living index	1.23*** (0.25)	
% Nonagricultural occupation	13.77*** (2.60)	13.26*** (2.23)
Land/Household ratio	4.73** (1.61)	5.48** (1.86)
Land/Household ratio Sq.	-0.69** (0.24)	-0.66** (0.27)
% Upto small illiterate	7.76** (3.45)	7.64* (4.22)

Dependent variable: wage rate for hired male labor.

Both regressions include rainfall, square, and interaction with North Bengal district.

Both regressions include village f.e., second column includes year dummies.

Robust standard errors in parentheses, clustered at district level.

* Significant at 10% level.

** Significant at 5% level.

*** Significant at 1% level.

Table 2

Village economic and demographic characteristics in sample villages, 1978 and 1998

	1978	1998
Number of households	228	398
Operational land/household ratio (acre/HH)	1.54	0.87
% Households landless	47.3	52.3
% Households marginal (0–2.5 acres)	35.2	39.1
% Households small (2.5–5 acres)	11.2	6.4
% Households medium (5–12.5 acres)	4.7	2.0
% Households big (12.5–acres)	1.6	0.3
% Land small and marginal	56.7	73.9
% Land medium	23.9	18.5
% Land big	19.5	7.6
% Poor households low caste	38.3	39.8
% Upto small households illiterate	44.1	31.9
% Big households illiterate	4.4	3.2
% Households in nonagricultural occupation	41.1	51.4
Population/bank ratio	41.6	23.1
	1981	1995
Farm yield (value added Rs./acre)	1009.22	5345.86
Nominal hourly farm wage (Rs./h)	1.17	4.21
Rice price, Rs./kg (aus,lcl)	1.00	5.46
Rice price, Rs./kg (aman,lcl)	1.19	4.35
Cost of living index (1974=100)	136.81	411.67
Farm yield (in 1974 Rs./acre)	737.69	1298.59
Hourly farm wage (in 1974 Rs./h)	0.85	1.02

‘Poor’ household is either landless or marginal landowner.

‘Upto small’ household is either landless, marginal or small landowner.

All land information pertains to distribution of cultivable non-*patta* land owned.

Source: Indirect household survey, except data on farm yield, rice prices and wages based on cost of cultivation farm surveys; and cost of living index (for agricultural workers) and population/bank ratio from West Bengal Economic Review, various years.

marginal landholding status, but more generally, measures of vulnerability of the poor, such as land inequality, illiteracy or low caste status. We also use ‘targeting’ to denote the share of the poor in the intra-village context, and the scale of resources devolved to a village in the inter-village context.

First, the direct effect of targeting on poverty is unlikely to be quantitatively significant: the two principal anti-poverty programs, IRDP and employment generation, yielded benefits on a per household basis (averaged across the entire village, not just among recipients) to be less than 10 days’ wages for farm work. Among IRDP recipients, the average subsidy constituted less than 20 days’ wages for farm work.¹² It is unlikely that the programs would augment the wealth of recipients sufficiently to allow them to purchase land.

Second, it is unlikely that targeting of programs administered between 1978 and 1998 would affect the measured distribution of land and caste over this period by affecting fertility patterns, because our data on landownership pertains to households defined by the identity of the head of household. Households with heads below 20 years of age are negligible. So even if the

¹² We do not have the data to assess the average benefit per participant in the JRY-type programs.

distribution of benefits by local governments had a big enough impact on fertility patterns, and thereby on landlessness or land shares, its effects would not show up in our measures of the land distribution. The effects would appear only on the demographic size and age composition of households, which we do not include. Nevertheless, an important reason for subdivision of lands was growth in the number of households, which could conceivably be correlated with both targeting (e.g., if the welfare function representing the local government's objective is non-homothetic) and poverty (by increasing population pressure on land and increasing labor supply). So we control for the number of households in the targeting regressions.

Third, could targeting affect the household land distribution via its impact on migration? For instance, is it conceivable that the poor migrate to villages with better targeting of programs administered by local governments? Such a 'welfare magnet' effect, if it existed, would impart a positive bias to the targeting-poverty correlation (by raising the proportion of poor households residing in high-targeting villages).¹³ It would imply that the true correlation is even smaller than measured, further reinforcing the evidence of absence of a positive correlation.¹⁴

Fourth, targeting could be correlated with land reforms carried out by the local government, as well as other redistributive programs. One would expect this correlation to be positive – local governments pursuing redistributive objectives for whatever reason (ideology, gaining a competitive edge with respect to rival parties in the next election, accommodating rising political awareness among the poor, or responding to pressures from NGOs, civil society or higher level governments) would use different policy instruments towards the same objective. This could explain a negative correlation between targeting and poverty (or inequality), with lower levels of poverty a consequence rather than cause of enhanced accountability of the local government to the poor.

We respond to this possibility in the following ways.

- (i) We exclude *patta* land – land distributed by the land reforms – in calculating landlessness or land shares of different size classes. This eliminates possible spurious correlation arising through the direct impact of accompanying land reform on the land distribution.
- (ii) As documented in our companion paper on the land reform implemented in these villages, *patta*-land accounted for no more than 3–4% of all land in the villages, part of which was in the nature of homestead land. So the direct impact of the land distribution program on the distribution of cultivable land was minor.
- (iii) This still allows for the possibility that land reforms exerted an important indirect effect on the distribution of cultivable non-*patta* land. For instance, the registration of sharecroppers or threat of expropriation of lands owned above the legal ceilings may motivate big landowners to sub-divide and sell part of their land. This issue is discussed in more detail below. In any case, our results turn out to be robust when we control for the extent of land reform (both the sharecropper registration and the *patta* programs) carried out in the concerned villages.
- (iv) We also control for the proportion of seats in the local government secured by members of the Left Front alliance, who may be expected to be more ideologically committed to

¹³ Conversely, measured land inequality would tend to be higher in villages with better targeting, inducing a positive inequality-targeting correlation, whereas we find the opposite.

¹⁴ Moreover, 'welfare magnet' types of migration seem to us implausible in the West Bengal context — the programs administered by local governments were so small and selective, that it is unlikely that new in-migrants would be eligible for any of these benefits over and above the claims of long standing residents.

redistribution than the principal opposition party, the Congress party. In addition this variable represents the (lack of) political contestability of local elections, as argued in our companion paper on the political economy of the land reforms (Bardhan and Mookherjee, 2003). The same motive could well extend to targeting of development programs. Nevertheless, the share of local government seats secured by the Left Front (hereafter referred to as the ‘Left share’) is likely to be jointly determined with targeting policies of the local governments, so it is necessary to use a suitable instrument for the Left share. As argued in our land reform paper, the share of Congress seats in the national Parliament is a suitable instrument for the Left share, reflecting national voter sentiment in favor of the Congress, the principal political adversary of the Left Front in the state.¹⁵

To throw further light on the determinants of changes in the land distribution, we are currently carrying out a household land survey in these villages. This consists of an exhaustive listing of landownership and land transactions (since 1967) of *all* households resident in these villages. Preliminary results from the survey indicate that less than 2% of land currently owned (in 2004) was received via the land reforms. The bulk of the change in the land distribution since the late 1960s occurred through market transactions: 24% of existing landholdings were acquired via purchases, and 5.6% via other means (transfers, gifts, etc.). Market transactions and inter-household transfers could of course have indirectly been influenced by land reforms or targeting activities.

One possible indirect effect of the land reform on market transactions in land could have operated through the threat of land reform, which created an incentive for big landowning families to sell off part of their land before it was vested by the land reform authorities. Such a threat could have existed only on those households owning more land than the legally permitted ceilings. Our current survey reveals that only 13.3% of all land sold was sold by households previously owning more land compared with the land ceiling. So the bulk of the market transactions could not have been motivated by the threat of expropriation by the land reform authorities.

Delivery of credit and farm inputs by local governments to small landowners could also have indirectly affected market transfers or household subdivision by raising the relative profitability of small plots of land. To address this possibility we shall examine the targeting-poverty correlation while controlling for farm yields on small farms relative to those in medium and large farms.

Better connectivity of the village with the outside world is another factor which may induce a spurious negative correlation between targeting and poverty. Improved roads and communication can reduce local poverty (by generating greater non-agricultural employment, raising local wages and generating higher prices for products sold by small producers), and raise the value of IRDP credit or agricultural kits to small producers (owing to improved marketing infrastructure), inducing in turn improved targeting by a responsive local government. Table 2 indicates that nonagricultural employment rose significantly between 1978 and 1998. We control for this by including the proportion of household heads engaged in nonagricultural occupations and also the farm wage rate in the targeting regression.

¹⁵ The fortunes of the Congress in the National Parliament are unlikely to depend much on events in rural West Bengal, a state which altogether accounts for only about 8% of all seats in the Parliament. It turns out to be a significant predictor of voter sentiment and the Left share in West Bengal local government.

Finally, as explained in the previous section, we control for the intra-village targeting ratio in the inter-village targeting regression. This is another variable which could be correlated with both poverty and (inter-village) targeting: if poor villages tend to achieve lower intra-village targeting on average, higher level government officials may allocate less to poor villages for that reason (even if they are themselves motivated to reach the poor), expecting high rates of leakage to the non-poor. And we control for the scale of the program for the village as a whole in the intra-village targeting regression, in case the welfare objective of local governments happens to be non-homothetic.¹⁶

In summary, the variables we control for include the extent of land reform, the Left share of local government seats, the number of households in the village, relative profitability of small farms, the proportion of non-agricultural employment, and local farm wages. These include the most likely channels for reverse causality or omitted variable bias in inferring the causal role of the land distribution on targeting via its effect on political power of the poor. We seek supplementary confirming evidence from the correlation of targeting with the proportion of poor households that have low caste status, a variable that is not subject to most of these possible competing channels of correlation.¹⁷

5. Data

Our sample consists of 89 villages drawn from 57 different GPs located in 15 districts of the state.¹⁸ The selected villages are those for which we could obtain farm-level production records from cost of cultivation surveys carried out originally by the state's agriculture department using a stratified random sampling frame.

Our empirical analysis is based on matching three different datasets for this village sample: (a) an (indirect) household survey of land, occupation, literacy and caste for the years 1978 and 1998; (b) successive 5-year panels of farm production from a (stratified random) sample of eight farms per village; and (c) a village panel for four to five years interspersed between 1978 and 1998, which includes details of local government composition, budgets, major expenditure items, IRDP credit and agricultural minikits distributed to village recipients. We explain each of these in further detail below.

¹⁶ Indeed, the inter- and intra-village targeting may be interdependent, with the intra-village targeting depending on the scale of the program at the village level (owing to non-homotheticity), and the latter depending on the former through the inter-village targeting process. We therefore instrument for the village scale of the program in the intra-village targeting, using the scale of the program for the state as a whole as a predictor of the scale of the program at the village level. This is based on the assumption that the state-wide scale of the program depends on the fiscal condition for the state government, which is independent of village specific preferences for redistribution. Specifically, we use a first-stage inter-village targeting regression which excludes the intra-village targeting ratio but includes the state-wide scale of the program, to predict the village scale. We then use the predicted village scale as a control in the intra-village targeting regression at the second stage. Finally, we re-run the inter-village regression, including the intra-village targeting ratio as control.

¹⁷ Since caste is acquired by birth, it cannot be influenced by market transactions or household division. Migration and fertility patterns represent the only way that this variable could be affected by transfers or omitted variables, and we have argued above that neither of these can account for absence of a positive correlation between targeting and incidence of low caste among the poor.

¹⁸ Calcutta and Darjeeling were excluded owing to the paucity of agriculture in those districts: Calcutta is primarily urban while Darjeeling is a mountainous region dominated by tea plantations. District boundaries within Dinajpur have changed within the period being studied so we aggregate all the data for Dinajpur villages. We therefore end up with data for 15 districts.

5.1. Household survey

Data on the distribution of land for individual villages in our sample are not available from any existing source. The most disaggregated information available concerns the distribution of operational holdings at the district level from the state Agricultural Censuses (once every 5 years), and at the state level from the National Sample Survey (once every 10 years, the most recent one available pertaining to 1991–92). We therefore conducted an ‘indirect survey’ of the concerned villages, using a voter list for the 1998 elections, and also the voter list from an election in 1978 or 1983 wherever available (which happened to be the case for 20 GPs). Three or four village elders in each village helped identify different voters belonging to the same household, and provided details of their land owned, leased or cultivated (area, irrigation status, mode of acquisition for owned land, *barga* registration status for tenants), caste, occupation and literacy status.¹⁹ In the case of villages for which separate voter lists were available for 1998 and either 1978 or 1983, this exercise was carried out separately for 1998 and 1978 based on the respective voter lists. For the others, it was carried out on the basis solely of the 1998 voter list (thereby creating the risk of some attrition in the sample whence households that resided at the beginning of the sample period may have left the village or died, who would remain unrepresented in the 1978/1983 distribution). For years in-between 1978 and 1998, we estimated the land distribution by interpolating the distribution between the two end years.

This ‘indirect’ household survey procedure has the advantage of eliciting rich community information concerning the distribution of land, and avoiding problems stemming from reluctance of individual households from declaring their assets to outside surveyors.²⁰ It could however suffer from lapses of knowledge or memory of third-party informers. We compared the size distribution of holdings compiled in this manner for 1978 and 1998 against the state Agricultural Censuses for 1980 and 1995, and the National Sample Survey for 1981–82 and 1991–92. These estimates are provided in Table 3, and show that the information from the three different sources for the state as a whole match quite closely.

5.2. Farm production surveys

The source is a ‘cost of cultivation’ survey carried out by the Socio-economic Evaluation Branch (SEEB) of the Department of Agriculture, Government of West Bengal. Every five years the SEEB selects a stratified random sample of villages from the entire state, according to a sampling plan where each village (*mouza*) is paired with another within a 8-mile radius, which lie within the same block. Blocks are selected randomly within each district, approximately two or three blocks per district (a district on average has about twenty blocks). Within each village, the SEEB selects a random sample of eight farms from different size classes, each of which is followed for between 3 and 5 years each. For each farm the data available includes a detailed cropwise breakdown of outputs and inputs, as well as wage rates and other factor and product prices. For each farmer we also have data on their annual incomes by different sources. Altogether about 2800 farm-years data (i.e., approximately 740 farms, covered for an average of 4 years each) is available for the mid-70s, and between 1981 and 1995. We use this data set to

¹⁹ The information provided was cross-checked across different elders.

²⁰ Several land experts in West Bengal, including Debu Bandyopadhyay, the state Land Reform Commissioner during the late 1970s and the early 1980s, advised us to carry out an indirect rather than direct survey for this reason.

Table 3
Land distribution data from different sources

Source	Year	0–2.5 acres, % land	2.5–5 acres, % land	5 acres and more, % land
Agricultural Census	1980	28	32	39
	1995	43	29	27
NSS	1981	29	29	42
	1991	40	31	29
Indirect survey	1978	28	28	43
	1998	46	28	26

Source: West Bengal Agricultural Censuses, NSS Operational Land Survey 1991–92, Indirect Survey Census.

NSS data pertain to operational holdings.

Indirect Survey pertains to cultivable non-*patta* land owned.

calculate the village wage rate for hired labor, and distribution of farm profitability (value added per acre) for different size classes, in each year.

5.3. Village surveys

The village surveys were carried out subsequent to creation of the farm database. Our survey teams visited the same villages included in the farm database, and gathered information concerning political party composition of GPs; yearly budgets, employment schemes and infrastructure projects. In addition, our survey teams obtained data from (a) the corresponding local ‘lead’ bank concerning details of every *Integrated Rural Development Program (IRDP)* loan to village residents, (b) the local Agriculture Department office which delivered free ‘minikits’ containing seeds and fertilizer to farmers within the village, and (c) the Block Land Reforms Office (BLRO) containing all land reform activity in these villages since 1971. These two databases are supplemented by Census data on villages, monthly rainfall records, district allocations of principal development program assistance, and results of elections to the Lok Sabha and the state legislative assembly for all election years between 1977 and 1998.

Data from the village surveys concerning disbursement of credit, kits, and GP budgets were collected for a selection of years between 1978 and 1998, typically for one year from each five year term of an elected GP body.²¹ The names of the recipients of these forms of assistance was matched against the household survey to identify their landholding class. This was used to construct the intravillage targeting share for each year, and the total amount of resource allocated to each village for that year, the dependent variable of the two key regressions. The targeting regressions thus include an average of four observations per village or GP, spaced apart by 5-year intervals.

There are two principal sources of measurement error in the data, with regard to the distribution of land, literacy and caste. First, there could be error arising from interpolation between the two endpoints for intervening years. Since demographics and land distributions typically change gradually, this is unlikely to be a serious source of error. Moreover, since this is a form of classical errors-in-variables, it will result in downward bias in estimated absolute values of coefficients. Hence a negative poverty-targeting correlation would represent an understatement of the strength of the correlation.

²¹ In most cases, this data was collected for the election year, if this was available from the local government records, otherwise it was the previous or following year.

The second source of error could result from attrition of households, the result of out-migration (or in rare instances, where the household head and spouse died, leaving no heirs). This would be comparatively insignificant for the subset of villages for whom separate voter lists for 1978 and 1998 were used to construct the distributions at the two endpoints. Unfortunately, this sub-sample of 20 GPs (contained in five specific districts) is both nonrandom and too small to check the robustness of our results. However, the consistency between the land distribution estimates from the indirect survey with those from the state Agricultural Census or the National Sample Survey (not subject to similar attrition problems) suggests that attrition is not a serious problem with respect to the land distribution.

6. Empirical results

Tables 4 and 5 provide the intra-village and inter-GP targeting regression results, respectively. The former use data for different years for individual villages, and the latter for averages at the

Table 4
Intra-village targeting

	IRDP credit subsidy			Minikits	Employment
	% Share Landless	% Share Upto small	% Share SC/ST	% Share Upto small	
Mean (S.D.)	0.45 (0.40)	0.97 (0.14)	0.45 (0.42)	0.98 (0.08)	0.024 (0.095)
Ratio of Mean share to Demographic share	1.38	2.09		2.08	
Ratio of Mean share to Land share		1.43		1.50	
Observation, Groups, $W-R^2$	386, 72, 0.09	386, 72, 0.16	414, 75, 0.25	259, 77, 0.10	207, 73, 0.30
% HH landless	-0.18 (3.38)	0.27 (1.92)	-4.85 (4.22)	0.02 (0.56)	0.04 (0.11)
% HH marginal	-1.83 (2.27)	0.36 (1.39)	-6.53** (2.98)	-0.14 (0.34)	0.07 (0.10)
% HH small	0.97 (2.19)	1.27 (1.17)	-4.54 (2.99)	-0.69* (0.36)	-0.09 (0.12)
% HH medium	-3.30 (6.73)	-1.05 (3.14)	-0.41 (4.95)	-0.10 (0.64)	0.43** (0.20)
% Land Upto small	0.26 (0.36)	0.01 (0.44)	1.88* (0.93)	-0.06 (0.15)	0.03 (0.04)
% Land big	-0.40 (1.21)	-0.70 (0.49)	1.77 (1.14)	4.56e-3 (4.16e-2)	-0.05*** (0.01)
% Illiterate among upto small HH	-0.73 (0.70)	-0.12 (0.19)	-0.66 (0.69)	0.08 (0.12)	-0.01 (0.03)
% SC/ST among poor HH	0.75 (0.97)	-0.20 (0.40)	0.08 (1.27)	-0.02 (0.24)	0.03 (0.05)
%LS in GP (predicted)	0.97 (0.85)	0.11 (0.12)	-0.81 (0.48)	-0.18 (0.16)	-2.88e-4 (3.81e-2)
%LS in GP Sq. (predicted)	-0.82 (0.80)	-0.09 (0.09)	0.42 (0.37)	0.07 (0.12)	4.97e-4 (2.93e-2)
%LS in ZP	0.16 (2.12)	0.98 (0.77)	-1.05 (1.57)	-0.14 (0.27)	0.16 (0.12)
%LS in ZP Sq.	0.44 (1.50)	-0.66 (0.47)	0.47 (1.00)	0.12 (0.19)	-0.10 (0.09)

Employment-dependent variable: days employment generated per rupee employment grant exp.

All regressions control for % HH heads nonagriculturally employed, number of households, village/GP f.e., time block dummies and (predicted) village (per HH) resource received.

Credit, kit regressions control for relative yield of small farms, farm wage.

Employment regression controls for (predicted) real wage, average farm yield.

Robust standard errors in parentheses, clustered at district level.

* Significant at 10% level.

** Significant at 5% level.

*** Significant at 1% level.

Table 5
Inter-GP targeting

	IRDP credit subsidy (per HH) (1980 Rs.)	Minikits grant (per HH) (no. kits)	Employment (per HH) (1980 Rs.)	Total grant (per HH) (1980 Rs.)
Mean (S.D.)	23.56 (66.24)	0.09 (0.11)	194.89 (365.92)	463.93 (1247.39)
Observations, Groups, $W-R^2$	292, 46, 0.14	183, 50, 0.27	215, 47, 0.38	216, 47, 0.27
% HH landless	237.04 (190.61)	-1.26 (1.09)	-582.74 (638.48)	-362.52 (1075.10)
% HH marginal	189.24 (171.91)	-0.58 (0.90)	-843.56**** (554.64)	-920.38 (1113.61)
% HH small	-258.27**** (179.15)	0.78 (1.58)	-617.65 (969.20)	2152.85 (1689.45)
% HH medium	698.43 (564.82)	0.87 (1.81)	103.33 (1635.42)	3003.71**** (2085.00)
% Land upto small	-60.90 (74.68)	0.40 (0.28)	553.45* (281.04)	1098.80** (502.95)
% Land big	-165.30** (72.48)	-0.22 (0.29)	-104.32 (159.64)	-364.99 (362.52)
% Upto small HH illiterate	-95.70 (85.86)	0.41 (0.26)	-364.86 (345.20)	-247.05 (531.62)
% Poor HH SC/ST	78.14 (116.71)	-1.56*** (0.47)	-771.16**** (567.64)	-2509.72*** (763.42)
% LS in GP (predicted)	-70.56 (59.57)	0.49* (0.28)	-832.61*** (288.19)	-991.74 (1266.52)
% LS in GP Sq. (predicted)	32.12 (42.02)	-0.26 (0.17)	178.39 (164.96)	8.35 (652.81)
% LS in ZP	274.06* (159.24)	0.45 (0.86)	-85.66 (541.35)	-895.44 (1629.81)
% LS in ZP Sq.	-170.82* (100.52)	-0.35 (0.55)	39.33 (383.66)	347.16 (1111.71)

Controls: % HH heads nonagriculturally employed, no. households, average farm yield, all-GP average subsidy/grant per HH, GP fixed effect, time block dummies.

Credit, kit regressions added controls: farm wage, intravillage targeting.

Credit: added control for population/bank ratio in district.

Grant regressions added control: (predicted) real wage.

Robust standard errors in parentheses, clustered at district level.

* Significant at 10% level.

** Significant at 5% level.

*** Significant at 1% level.

**** Significant at 20% level.

level of individual GPs.²² Data was collected for an average of one year per elected GP administration in the given village or GP, typically the election year, so we have approximately data for four years per village or GP.

For the IRDP program, the names of the loan beneficiaries were matched with the indirect survey, enabling us to identify their landholding status. We were successful in identifying the recipients of approximately 92% of the total loan amounts disbursed, and correspondingly restrict our targeting analysis to the loans that were identified thus. Other loan details included the amount, duration and purpose of the loan, the interest rate, and the subsidy component. We computed the following measure of financial subsidy f for each loan: $f=l[s+(1-s)v]$ where l denotes the loan amount, s the proportion of it that was the subsidy, and v the difference between the interest rate on the loan and market interest rates. Data on the market rate was unavailable, so we constructed f based on different assumptions regarding the value of v . The results with different values of v were qualitatively similar, so we report the results corresponding to $v=50\%$. For the inter-village targeting regression (the first column of Table

²² A complicating feature is that each GP covers between 8–15 villages, and we do not have data for all villages in the GP's concerned. So in the inter-GP credit and kit regressions we compute weighted (per household) averages for the villages in a given GP that we do have data on.

5), the dependent variable is the average (per household) subsidy accruing to a given village in any given year. For the intra-village regression, it is the share of the subsidy accruing to differing landowning and caste groups. The first three columns of [Table 4](#) report regressions for the share of the landless, the ‘upto small’ category comprising landless, marginal and small landowners, and the SC/ST (scheduled castes and tribes) category.

For the minikits, we count the number of kits allocated to different categories within a village in any given year, and use the shares of the ‘upto small’ category in the second column of [Table 4](#).²³ The second column of [Table 5](#) likewise regresses the average (per household) number of kits distributed in a village in any given year. The dependent variable in the fourth column of [Table 4](#) is the number of days of employment generated per rupee of expenditure out of employment grants. We use this as a measure of the intra-village pro-poor targeting of employment grants based on the argument that the landless rely on employment as the principal source of their earnings, so would prefer the local projects selected in these programs that maximize the employment generated. Medium and big landowners in contrast would prefer projects that raise the value of their land and farm operations the most. As [Foster and Rosenzweig \(2001\)](#) argue, road projects are more employment-intensive than irrigation projects, so the poor and non-poor may have conflicting preferences between road and irrigation projects if irrigation projects raise the profitability of medium or large farms more than do road projects.

In the inter-GP regression, we use the extent of employment grants spent by each GP (per household) as the dependent variable in the third column.²⁴ The last column of [Table 5](#) uses as the dependent variable all grants received by the GP (per household), calculated as the difference between total income of the GP and local revenues raised by the GP, divided by the number of households. This includes employment grant allotments, as well as grants covering all other central and state government sponsored development and welfare programs. As noted previously, these account for approximately 80% of all funds at the disposal of GPs.

All regressions include village (in the case of the intra-village regressions) or GP (in the case of the inter-GP regressions) fixed effects, besides dummies for the time blocks (i.e., corresponding to different 5-year GP administration spans). Control variables used in each regression are listed at the bottom of each table. The extent of (1-year lagged) cumulative land reform implemented was not used as a control in the regressions reported. However, the results were virtually unchanged when those additional controls were included, and so these supplementary results are not reported to conserve space.

The Left share of GP seats is likely to be jointly determined along with targeting policies, so we use instead the Left share predicted by a vector of instruments that includes the share of Congress seats in the national Parliament, the lagged (i.e., in the previous 5-year administration) Left share, the rate of inflation in the price index for agricultural workers in the nearest center,²⁵ the rate of growth of factory employment in the concerned district, and interactions between these. This (first-stage) regression is reported in our companion paper on the West Bengal land

²³ We do not examine the share of the landless since most of them are unlikely to operate any land on their own, so have little use for a minikit.

²⁴ We use actual expenditures rather than allotted grants because of the reason (explained previously) that grant allotments are frequently made too late in the fiscal year by higher level governments for the funds to be actually usable by the GP. To capture the timeliness of grant allotments, which seems crucial from the comments of GP officials we have interviewed, we use actual expenditures rather than grants allotted.

²⁵ This price index is reported by the West Bengal government for four centers: Calcutta, Asansol, Raniganj and Jalpaiguri.

reform (Bardhan and Mookherjee, 2003, Table 13).²⁶ The identification assumption is that changes in the fortunes of the Congress in national politics are uncorrelated with time-varying shocks affecting redistribution preferences of voters and officials in specific West Bengal villages.²⁷ The standard errors reported in Tables 4 and 5 are not corrected for the first-stage prediction errors, so likely to represent an understatement of the true standard errors.²⁸

Other control variables which are predicted owing to potential endogeneity problems include: (a) Scale of the program at the GP level in the intravillage regressions, which is predicted (from a first-stage inter-GP regression) by the scale of the program for the state as a whole, which we assume is uncorrelated with village-specific time-varying shocks concerning redistribution preferences. The scale of the program for the state as a whole is estimated using the average across all GPs in the sample for the amount received by each GP. (b) Farm wage rate, in the employment and grant regressions. This is estimated from the first-stage regression shown in Table 1. The instruments thus include rainfall, and the cost of living index in the nearest center.

The intra-village targeting results in Table 4 show that average levels of targeting of credit and kits to the poor and low caste groups were high, consistently exceeding the corresponding demographic weights and land shares. Hence average per capita and per-land-area allotments to the poor (respectively low caste groups) exceeded those to the non-poor (respectively non-low-caste groups). The leakage rate of the IRDP (i.e., proportion flowing to those not entitled, i.e., medium or big landowners) was on average less than 4%. The credit regression shows that only the targeting share of the SC/ST group was sensitive to land inequality in a statistically significant fashion. The targeting share of the poor in the kits program was also lowered significantly when there was a higher fraction of big rather than small landowning households.

Tables 6 and 7 indicate the implied magnitude of the changes in targeting shares associated with changes in the land distribution of roughly the order of those experienced during the two decades (2–2.5% shift in demographic shares and 10% shift in land shares between poor and non-poor groups). It also shows the effects of a hypothetical 5% change in the caste composition of poor households, and a 10% change in the political composition of local governments. The effects of these changes on intra-village targeting to the poor (defined by landownership status) were quantitatively insignificant. The shares of landless, or ‘upto small’ categories did not change much when poverty or inequality rose.

However, this assessment must be qualified in two ways. First, targeting to low caste groups was adversely affected when the land distribution became more unequal: the share of the SC/ST group fell by about 30% if the demographic weight of the marginal landowners rose by 2% relative to big landowners.²⁹

Second, the employment intra-village targeting regression (last column of Table 4) shows that higher land inequality and poverty was associated with significantly worse targeting. The first

²⁶ We use an Arellano–Bond specification with GP fixed effects for estimating the Left share across five successive GP elections between 1978 and 1998, owing to the use of lagged Left share as an argument. The correlation between the observed and predicted values of Left share is 0.31.

²⁷ Moreover, the Arellano–Bond specification is not rejected by the data, so the lagged Left share is also a valid instrument.

²⁸ They turn out to be insignificant based on the reported standard errors (with few exceptions, such as the inter-GP kits and employment regressions), so are likely to continue to be insignificant if the standard errors were to be corrected.

²⁹ The incidence of poverty among SC/ST categories is much higher on average than the non-SC/ST population: for instance, in our sample, 59% of SC/ST households were landless, as against 39% among non-SC/ST households, and their average landholding was 0.86 acres, compared with 1.58 acres for the rest. Hence the decline in the share of the SC/ST group was also likely to be associated with a decline in pro-poor targeting.

Table 6
Implications for targeting of IRDP credit subsidies

	(Intra-village) Landless, % share	(Intra-village) Upto small, % share	(Intra-village) SC/ST, % share	(Inter-GP) GP per HH
Mean (S.D.) at 1980 prices	45.0 (39.7)	96.5 (13.8)	45.4 (42.0)	23.56 (66.24)
2.5% Households switch: medium to landless	7.8	3.3	−11.1	−11.54
2% Households switch: big to medium	−6.6	−2.1	−0.8	13.97
2% Households switch: big to marginal	−3.7	0.7	−13.1**	3.8
10% Cultivable land shifts: small to big	−6.6	−7.1***	−1.1	−10.44
5% Poor households switch non-SC/ST to SC/ST	3.7	−1.0	0.4	3.9
Left share of ZP seats rise: 86% to 96%	10.9	−4.1*	−0.7	−8.5****
Left share of GP seats rise: 66% to 76%	−7.5	−0.8	0.7	−0.31

* Significant at 10% level.

** Significant at 5% level.

*** Significant at 1% level.

**** Significant at 15% level.

column of Table 8 shows that rising landlessness (to the tune of 2.5% households who move from medium to landless categories) and land inequality (2% households move from medium to big category) were associated with reduced employment generation (out of given employment grants) by approximately 40%.

Turning now to the inter-GP regressions shown in Table 5, there was a strong negative relation between low caste status among the poor and the amount of kits or grants received by a village. Higher land inequality was also associated with significantly lower allotment of kits and grants. Table 7 indicates that a 5% rise in the proportion of poor households that belonged to SC/ST groups, or a 10% shift in land from small to big landowners was associated with a 70% reduction in kits received, a change which dwarfed the corresponding effects on intra-village targeting. In the case of credit, greater landlessness or land inequality was associated with decreased allocations of the order of 40% or more, but this effect was not measured precisely (so failed to be statistically significant). But in terms of quantitative magnitude the effect was much greater than the corresponding changes in the intra-village targeting ratio. Only in the case of the employment program were the adverse effects of greater landlessness and land inequality on intra- and inter-village targeting of similar orders of magnitude (one-third to one-fourth declines).

Table 7
Implications for targeting of minikits

	(Intra-village) Upto Small, % share	(Inter-GP) GP average per HH
Mean (S.D.)	97.7 (8)	0.085 (0.114)
2.5% Households switch: medium to landless	0.3	−0.053
2% Households switch: big to medium	−0.2	0.017
2% Households switch: big to marginal	−0.3	−0.012
10% Cultivable land shifts: small to big	0.6	−0.062*
5% Poor households switch: non-SC/ST to SC/ST	−0.1	−0.078***
Left share of ZP seats rises: 86% to 96%	1.2	−0.029
Left share of GP seats rises: 66% to 76%	−0.3	−0.007

***, **, * Significant at 1,5,10% level.

Table 8
Implications for targeting of grants

	(Intra-village) Employment days generated per rupee employment	(Inter-GP) GP average per HH employment grant	(Inter-GP) average per HH all grants
Mean (S.D.)	0.024 (0.095)	195 (366)	464 (1247)
2.5% Households switch: medium to landless	−0.010**	−17	−84*
2% Households switch: big to medium	0.009**	2	60
2% Households switch: big to marginal	0.001	−17	−18
10% Cultivable land shifts: small to big	−0.008*	−66*	−146**
5% Poor households switch: non-SC/ST to SC/ST	0.001	−39	−125***
Left share of ZP seats rise: 86% to 96%	−0.005	−0.31	−16
Left share of GP seats rise: 66% to 76%	0.000	−46**	−97***

* Significant at 10% level.

** Significant at 5% level.

*** Significant at 1% level.

Our interpretation of these results is that intra-village targeting of credit and kits takes place within a village or GP in a context of rich information shared within the community concerning relative entitlements and allotments of different claimants. The allocation of private goods involves a fairly straightforward distributional assessment. The choice of public infrastructure projects is more complicated, as these involve public rather than private goods, besides knowledge of technology and procurement costs of related raw materials. So the scope for elite capture of local public good programs is greater than for private goods. Similarly, the inter-GP allocation is decided as the outcome of allocations made by officials of higher level governments (at the block, district and state level), influenced by the lobbying power and negotiating skills of GP-representatives. This process involves considerably less transparency and shared information about entitlements and allotments to competing claimants. Our enquiries with state government officials indicate there was no formula-bound process at the block level or below for allocating grants, kits or credit across different GPs. Presumably, the inter-GP allocations reflect the fact that lobbying strength of GP-representatives depend on their land and caste status. If medium or small non SC/ST landowners are politically more vocal (than big, landless or SC/ST households) in the inter-GP allocation process, then higher land inequality, poverty (e.g., a shift out of small/medium landowning categories to landless or big categories) or low-caste status would result in a GP being less successful in securing resources from higher level governments.

Note, however, that the inter-GP allocations do not show any evidence that targeting was systematically related to the strength of local political competition.³⁰ Nor was there any evidence of political clientelism or favoritism by the Left Front state government. For if competition mattered for targeting, we would expect to see an inverted-U with respect to the Left share of GP or ZP seats (as elaborated further in Bardhan and Mookherjee, 2003): targeting would improve when local elections became more contested and the political composition of the GPs became more evenly divided between the Left and the Congress. A statistically significant inverted-U is observed only in the case of the inter-GP allocation of IRDP subsidies (with respect to the

³⁰ This statement should be interpreted with caution. The lack of any correlation with political composition of GPs may reflect a tendency for convergence among policies pursued by officials from different parties, the result of (Downsian) political competition. So the lack of a targeting-political composition correlation may reflect the operation of vigorous political competition.

political composition of the ZP). There was only one other instance where the political composition of the GP was statistically significant: in the allocation of employment and fiscal grants across GPs, a higher Left share was throughout associated with a significantly *lower* allocation, rather than any U-shaped pattern (see last two columns of Table 8). This is inconsistent with either hypothesis of a positive correlation between political competition and targeting, or the existence of favoritism by the Left Front state government to GPs dominated by the Left: both would predict a greater allocation when the Left share in the GP rose from below 50% to 50%.³¹

7. Summary and concluding comments

In summary, we do not find much evidence that targeting patterns of private goods such as credit or agricultural minikits in West Bengal were vulnerable to ‘elite capture’ within local communities. The principal problem lay elsewhere: in the selection of local infrastructure projects under employment generation programs, and the process of allocating resources *across* GPs by higher levels of government. In these respects there was a perverse tendency to generate less employment out of allotted funds, and allocate less rather than more resources (in the form of minikits, employment and other fiscal grants) to villages when poverty, land inequality or frequency of low caste households within those villages rose. This result suggests that greater attention needs to be devoted to processes governing allocation of resources across different local governments, rather than within local communities. Formula-bound inter-government allocations (where allocations are based on publicly available measures of size and poverty) would enhance the transparency and equity of the process.

We also found little evidence that inequality of political competition or political favoritism mattered for targeting. Neither did we find any evidence that levels of literacy among the poor mattered. On the other hand, caste did matter, both within and across villages. Higher poverty and land inequality lowered the share of SC/ST groups within villages, and a higher incidence of low caste status among the poor lowered what the corresponding local government managed to secure from upper levels.

One may question the significance of inter-village allocative biases, when intra-village targeting performance levels are so high. If one is concerned about resources flowing to the poor, and they do go mostly to the poor within any given village, how does it matter whether one village is favored over another? Obvious horizontal equity concerns aside, there may be a variety of reasons from the standpoint of vertical equity. First, suppose village *A* has 95% of its population that are poor, while another village *B* has 50% poor. Suppose also that within each village poor and non-poor receive the same per capita allocation. Then a shift in the total allocation from village *A* to *B* causes the overall targeting ratio (fraction received by the poor, averaging across the two villages) to decline, because 95% of resources in village *A* reach the poor, as against 50% in village *B*. From the standpoint of vertical equity, a larger fraction of resources should go to village *A*. But our evidence points to the opposite: rising poverty within a village resulted in it receiving less. Second, the relative extent of poverty of the poor may differ

³¹ One might be inclined to interpret the monotone decreasing pattern of GP-level grants with the local Left share, as allocation of greater resources by the Left Front (which dominated more ZPs apart from the state government) in areas where its base was weak, as part of a long-term strategy of building up its support base in those areas. But if the local government was controlled by the Congress, the opposition party would then obtain discretion over utilization of the allocated funds within the village. It is unclear how the Left would then be able to benefit politically from allocating more to those GPs.

across villages. As the evidence from [Table 1](#) shows, *ceteris paribus* wage rates in village *A* are likely to be substantially lower than village *B*. From the standpoint of vertical equity, this should have accentuated the resources going to village *A* rather than *B*. Yet the evidence suggests precisely the opposite. From the standpoint of targeting based on poverty gaps rather than headcount ratios, then, there was an additional source of targeting failure.

We now mention a number of qualifications to our analysis, which stem from the nature of data available. First, our use of the indirect survey inevitably gives rise to measurement error in the key village characteristics, a problem which can be rectified only if direct household surveys are carried out to estimate changes in landholding patterns, literacy or caste more precisely. However, problems of attrition would continue to remain in such surveys (of current village residents). Second, we analyzed targeting on the basis only of landholding status of recipients, rather than gender or political affiliation. It has been argued that targeting performance of the West Bengal panchayats on the other dimensions was far weaker (see, e.g., [Webster, 1992](#)). Third, data limitations prevented us from examining biases in the allocation of irrigation, a crucial determinant of agricultural performance. We are currently conducting direct household surveys to remedy these problems.

In future research, we plan to extend our analysis of targeting to examine the effect of reservation policies for GP positions for women and SC/STs (which have been in place since 1993), and examine the implications of targeting for growth of farm productivity.

Acknowledgements

We thank the MacArthur Foundation Inequality Network, National Science Foundation Grant No. SES-0418434 for funding the data collection. Mookherjee thanks the John Henry Simon Guggenheim Foundation for funding a sabbatical year (2001–2002). None of our funding agencies are responsible for any of the views expressed in this paper. We are grateful to Alfredo Cuecuecha and, especially Monica Parra Torrado, for outstanding research assistance. For data collection, we are indebted to Sankar Bhaumik and Sukanta Bhattacharya of the Department of Economics, Calcutta University who led the village survey teams, and Bhaswar Moitra and Biswajeet Chatterjee of the Department of Economics at Jadavpur University who led the teams that collected the farm management data. The paper has benefited from the comments of Tim Besley, an anonymous referee, and from presentations at the October 2004 Kennedy School of Government conference on development economics, the Center for Global Development, Washington DC, and participants of a seminar at ISI Delhi.

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