Local Capture and the Political Economy of School Financing*

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Abstract

According to official statistics, Uganda spent roughly 8 percent of GNP on primary education in the mid 1990s. One of the key public programs in education was a per-student capitation grant to cover the schools' non-wage expenditures. Using panel data from a unique survey of primary schools, we assess the extent to which the grant actually reached the intended end-user (i.e., the schools). The survey data reveal that during the period 1991-95, the schools on average received only 13 percent of the grants. Most schools received nothing. The bulk of the school-grant was captured by local officials (and politicans). The data also reveal considerable variation in grants received across schools, suggesting that rather than being passive recipients of flows from the government, schools use their bargaining power to secure greater shares of funding.

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1. Introduction

Official budget data are typically the only source of information on public spending in low-income countries. Unfortunately, such information poorly predicts what the intended beneficiaries actually receive in terms of resources and services. This is particularly the case in countries with weak institutions. Uganda is a case in point. According to official statistics, Uganda spent roughly 8 percent of GNP on primary education in the mid-1990s. Anecdotal evidence, however, suggested that most public schools only received limited public support.

In this paper, we describe and analyze the results of an innovative survey tool to gauge the extent to which public resources actually filter down to the intended end-users. We focus on one of the key public programs in education in Uganda - a per-student capitation grant to cover schools' non-wage expenditures. This is a national program financed and run by the central government, using district offices as distribution channels. The survey compared disbursed flows from the central government (intended resources) with the resources actually received by 250 primary schools over a five-year period (1991-95). This unique panel data set lets us study the level and determinants of local capture.

The results of the survey are striking. On average, schools received only 13 percent of what the central government spent on the program. Most schools received nothing. The bulk of the grants was captured by local government officials (and politicians). The data also reveal large variations in grants received across schools. We find that a small set of school-specific variables can explain a significant part of this variation. Specifically, larger schools receive a larger share of the intended funds (per student). Schools with children of wealthier parents experience a lower degree of capture, while those with a higher share of unqualified teachers receive less funds. After addressing potential selection and measurement issues, we show that these school characteristics have a quantitatively large impact on the degree of local capture.

The contribution of this paper is twofold. First, we develop and implement a new, yet simple, empirical methodology to quantitatively assess local capture in basic service delivery systems in a poor developing country.[1] By primarily focusing on the frontline service provider of a public service (i.e., schools), and their fiscal (and political) relationship with the government bureacracy, we depart from the traditional empirical literature on public economics, which is either based

[1] On quantifying corruption, see Svensson (2003) and references given therein.

on household data or has a more aggregate focus on the allocation of resources across sectors. Our finding that a large part of the schools' entitlement is captured (subsequent studies and ongoing work suggest that the Uganda experience is not Uganda-specific, see Reinikka and Svensson [2002]), has obvious implications for a large and active research agenda. For example, it highlights the identification problem in attempting to evaluate the efficacy of public capital or services with official budget data. Such an exercise will severely underestimate any potential positive effect of public capital or services actually created by public funds. Even if one only cares about the marginal effect of additional spending (or foreign aid), the standard estimators will be biased if the determinants of capture are not explicitly taken into account.[2] Based on existing cross-country work, the effects of government spending (or foreign aid) on growth and social development outcomes are ambiguous.[3,4] An important explanation for this lack of correlation is poorly functioning systems of service delivery in many developing countries, implying that increased spending (foreign aid) does not necessarily translate into increased output and services.[5] More generally, the findings stress the need to focus more on issues related to the supply side of the delivery system in developing countries, in order to get a better understanding, and in the end improve, the impact of public spending.[6]

Second, we show that a large part of the variation in local capture can be explained by studying the interaction between local officials and end-providers (schools) as a bargaining game. In the empirical work, we exploit the fact that the local institututions are similar across districts. We can therefore use the (exogenous) variation across districts to identify the school-specific effects. Our finding that the schools' socio-political endowment matters for the degree of local

[2] Pritchett (1996) and Reinikka and Svensson (2001) make a somewhat similar argument.

[3] On the relathionship between government expenditures and growth, see Alesina (1997), Barro (1991), Kormendi and Mequire (1985), Landau (1986), Levine and Renelt (1992), and Ram (1986).

[4] On the relationship between aid and growth and social development outcomes, see Boone (1996), Burnside and Dollar (2000), Hansen and Tarp (2001), and Svensson (1998).

[5] The empirical growth literature is abundant with explicit (and implicit) attempts to separate productive spending from expenditures that have no direct effect on productivity (for example by *ex ante* determining what types of spending are likely to be productive, see Barro, 1991). Unfortunately, partitioning expenditure categories does not address the core problem—that public funds may not reach the intended end-user.

[6] There is an emerging theoretical literature with this focus, see e.g., Bardhan and Mookherjee (1999, 2000).

capture suggests that schools use their bargaining power vis-à-vis other parts of government to secure greater shares of funding. Local capture has thus obvious equity implications.

The paper is also related to the large literature on fiscal federalism, although it differs in three important aspects. First, although local (district) offices handled part of the administrative operation of the program, this was a centrally run program aimed at financing instructional materials and other kinds of non-wage spending at the primary schools. Second, contrary to the methodology predominant in the fiscal federalism literature, where budget data at the local level are used to explain the cross-sectional variation in grants received and used by different states and localities, we do not rely on budget data. Instead, we compare actual disbursements with spending outcomes on the frontline. Finally, we do not focus on the allocation of public resources across sectors, but the actually disbursement across frontline service providers.[7]

The survey findings had a direct impact on policy by providing a spark for (public) action. As evidence of the degree of capture became public knowledge, the central government enacted a number of changes: it began publishing the monthly transfers of public funds to the districts in newspapers, broadcasting the transfers on radio, and requiring primary schools to post information on inflows of funds. An initial (and internal) assessment of these reforms a few years later shows that the flow of funds improved dramatically (Republic of Uganda, 2000).[8] The large improvement suggests that provision and dissemination of information can play a crucial role in improving outcomes.

The rest of this paper is structured as follows. The next section briefly reviews the institutional setting for school finance and decision-making in Uganda. Section 3 discusses the survey and the measurement of local capture. Section 4 sets out a simple bargaining model to guide the empirical work on the relationship between the schools' socio-political endowment and their potential to claim entitled funds.

[7] Rosenzweig and Wolpin (1986) also deal with how resources within a given sector or program are allocated across localities. The key issue in Rosenzweig and Wolpin (1986) is evaluation. If the allocation of public resources across localities (e.g., schools) is systematically related to factors determining the outcome, and these factors are unobserved by the researchers but known to the local provider, simple cross-sectional estimates will produce misleading conclusions about the program effectiveness. We do not deal with the effect of the grant on educational outcomes.

[8] The empirical approach used in the follow-up study in 1999 differed somewhat from that used in our study. Furthermore, the sample of schools hardly overlapped, making a simple comparison of numbers impossible.

Section 5 describes the empirical specification we use to examine capture across schools and discusses the data. The results are presented in section 6. Section 7 concludes.

2. Institutional setting

It is commonly held that Uganda had a well-functioning public service delivery system in the 1960s. The government response to the political and military turmoil of the 1970s and early 1980s was de facto to retreat from funding and providing public services. In primary education, parents gradually took over the running of the public schools. The survey data indicate that by 1991, this situation had not changed a great deal. Parent-teacher associations (PTA) were the primary decision-makers at the school level, and funding by parents was, on average, the most important source of income.

While the subsequent economic recovery increased public spending relatively rapidly, institutional reforms were much slower to come. In particular, the central government exercised weak supervision over the execution of most public programs.

During the survey period (1991-95), the central government's financial contribution to primary education was threefold. First, the Ministry of Education paid the salaries of primary school teachers either directly, if the teacher had a bank account, or most often through the district education officer and/or the headmaster. Second, there was a national policy of financing instructional material and other non-wage spending at primary schools through a capitation grant (per student). The grant was a nationally set allocation per student and year and was intended to go directly to the schools. The district offices were used as distribution channels. Third, the central government provided funding for capital expenditure through the Ministry of Local Government. This funding was almost entirely limited to rehabilitation. In fact, since the 1970s, the central government had virtually abandoned its responsibility for classroom construction. The provision of classrooms thus became the responsibility of the local governments, which passed it on to the parents.[9]

[9]In addition, the central government is responsible for a share of the cost of donor-financed development projects (about 10 percent of the total project cost). It also incurs expenditures on teacher training, examinations, and school inspection. The last was almost non-existent during

The central government's total contribution to the funding of primary schools more than doubled between 1991 and 1995 in real terms, albeit from a negligible base (Table 2.1). In practice, the entire increase was used to raise teachers' salaries, which had eroded to extremely low levels (equivalent to a few U.S. dollars a month) during the institutional and economic collapse of the 1970s and 1980s. The capitation grant was retained at the same nominal level throughout the survey period and, therefore, its real value actually declined. There was an increase in spending on rehabilitation and school construction toward the end of the survey period.

The central government's stated policy was to disburse capitation grants in full to the schools, using district education offices as distribution channels. In 1991, the grant was set at the nominal rate of Ush 2,500 per child enrolled in grades one to four and Ush 4,000 per child enrolled in grades five to seven.

Uganda implemented cash budgeting in 1992 which, in many cases, produced volatile monthly releases of funds from the Treasury. However, as part of the World Bank's structural adjustment programs, non-wage recurrent expenditures for primary education were given a priority program status, which protected schools from within-year budget cuts. Available records show that capitation grants were fully released by the center on a monthly basis.

The central government policy regarding the capitation grant was not well-known to parents, particularly outside the capital. Even if parents knew about the policy in principle, many similar policy statements were not implemented in practice at that time. Little information was available to the public, for example on the spending items protected within the cash budget system. Local officials (and politicians) could take advantage of the gap in information about school funding; they could reduce disbursements or procure little for non-wage items to schools because they knew such actions would not attract political attention. In contrast, the failure to pay teachers' salaries attracted much more attention as, not surprisingly, teachers knew what their salaries were.

As Table 2.1 shows, parental contributions toward primary education consisted of PTA levies for investment and recurrent costs, top-ups to teachers' salaries, and tuition fees. The PTA fees and top-ups to teachers' salaries were entirely school-specific and set by each school's PTA, depending on the parents' ability to pay and the needs of the school. Parental contributions were clearly the mainstay of finance in government-aided primary schools. On average, parental contributions

the survey period.

accounted for over 60 percent of total school income during the sample period. In per-student terms, parents' average contribution increased by 35 percent in real terms during the sample period. Interviews at primary schools indicated that parents who were not able to pay the agreed PTA fees were often alienated and even forced to take their children out of the school.

In theory, the tuition fee per student was set at the same level as the capitation grant by the central government. It was left to each district to determine how the funds raised through tuition fees should be redistributed among the schools. In some districts, the schools were allowed to retain a certain percentage or a fixed amount of the tuition fee collected per student, with the balance transferred to the district education officer. In other districts, all tuition fees collected were remitted to the district headquarters and subsequent onward disbursements to schools, either in cash or in-kind, may or may not have taken place. The efficiency of tuition fee collection was very low in 1991, but improved somewhat in subsequent years. Interviews at the schools suggested that low collection efficiency was due to adverse incentives: most schools were neither allowed to keep the collected funds, nor benefited from them in any other way.

Teacher recruitment was carried out by district education service committees on behalf of the national teacher service commission. Recruitment was supply driven, as all new teachers graduating from the primary teacher collages were usually hired. Although teachers were hired by the districts, their payroll was maintained by the central government. As a result, and contrary to non-wage spending, the central government provided some supervision for teacher recruitment and salaries through the maintenance of the national payroll. Once recruited, the district education officer posted the teacher to a specific school. Hence, teachers had little opportunity to choose the school where they taught. If the demand for teachers exceeded the supply of training colleges, district education service committees recruited additional "licensed" teachers, who were often unqualified.

The PTA derived its authority from parents. The influence of the PTA over district officials depended on their competence in articulating their case. A typical PTA was run by an executive committee with about six members elected at a general meeting, and the headmaster.

Most students had few schools within walking distance, particularly in rural areas. This lack of choice can be traced back to the tumult of the 1970s and early 1980s and the central government's gradual abandonment of school construction which the local governments were not able to pick up. The school choice was also limited by Uganda's preference for "complete schools" (one school offering all seven grades), dating back to the colonial times.

3. Quantifying capture

A. Survey Design

Ideally, the public accounting system provides timely information about actual spending on various budget items and programs, and the reports accurately reflect what the intended users receive. This is not often the case in low-income countries. Typically, the accounting system functions poorly, institutions enhancing local accountability are weak, and there are few (if any) incentives to maintain adequate records at different levels of government. Consequently, little is known about the process of transforming budget allocations into services within most sectors.

These observations formed the basis for designing a new survey tool—a quantitative service delivery survey[10]—to gauge the extent to which public resources actually filtered down to the intended facilities. A survey of 250 government primary schools was implemented in 1996, covering the period 1991-95 (see Reinikka, 2001, for details on survey design). At the time of the survey, about 8,500 government primary schools were supposed to receive a large proportion of their funding from the central government via district administrations.[11]

The objective of the survey was twofold. First, to measure the difference between intended resources (from the central government) and resources actually received (by the school). Second, to collect quantitative data on service delivery at the frontline (i.e., the schools).

The initial intention was to track all main spending categories through the entire delivery system; i.e., the central government, districts, and schools. However, this turned out not to be possible due to several deficiencies in the system. First, at the central government level, data were not available on salaries paid to primary school teachers either by district or school. The only data available at the time of the survey were the aggregate salary payments, lumping together payments to teachers at primary, secondary, and tertiary levels, as well as to non-teaching staff. This made a systematic comparison between budget allocations and ac-

[10]For a conceptual discussion on Quantitative Service Delivery Surveys (QSDS) and reference to ongoing survey work, see Reinikka and Svensson [2002].

[11]The 1,500 private or community schools were not included in the survey.

tual spending at the school level with respect to teachers' salaries impossible.[12] Because salary data were lacking or incomplete, we used data on per-student capitation grants for non-wage spending available at the central government level as our core variable on intended funds. Second, the district-level records were much worse than those at the central government level. The quality of the available information both on transfers from the center and disbursements to schools was so poor that some districts simply had to be excluded from the expenditure tracking exercise. Unlike primary schools, some districts were also quite uncooperative during the survey. School records, on the other hand, were relatively comprehensive. Thus, a detailed comparison of budgetary allocations and actual spending could be made between the central government outlays for non-wage spending on instructional material and other running costs and the equivalent school income.

B. Sample

In order to bring out regional differences in the sample more clearly, the traditional four regions (North, East, West and Central) were reconfigured into seven regions (Northwest, North, Northeast, East, Central, Southwest and West). For each region, two or three districts were randomly chosen, together with the capital city, Kampala, to yield a sample of 18 districts, as illustrated in Figure 1.[13]

In the selected districts, the number of schools visited ranged from 10 to 20. Bushenyi had the largest number of primary schools (399 in 1994), while Bundibugyo had the smallest number (59). In the districts with less than 100 government schools, the enumerators visited 10 randomly chosen schools. Where the number of schools was between 100 and 200, 15 schools were randomly selected for visits, and 20 schools in the districts with more than 200 schools.

Enumerators were trained and closely supervised by a local research team and survey experts from the World Bank to ensure quality and uniformity of data collection and standards for assessing record-keeping at the schools. Standardized forms were used. In addition, interviewers made qualitative observations to supplement the quantitative data.

C. Defining capture

[12] A separate teachers' payroll clean-up revealed that in 1993, about 20 percent of the salaries were captured through "ghosts" on the payroll (Reinikka, 2001).

[13] The following 18 districts were selected: Arua, Moyo (Northwest); Apac, Gulu (North); Soroti, Moroto, Kapchorwa (Northeast); Jinja, Kamuli, Pallisa (East); Kampala, Mukono, Mubende (Central); Bushenyi, Kabale (Southwest); and Kabarole, Hoima, Bundibugyo (West).

Our school-specific measure of capture is,

$$\frac{\text{capitation grants received}}{\text{intended capitation grants from the center}} \tag{3.1}$$

where a low value indicates extensive capture.

In theory, the denominator in (3.1), the intended capitation grants from the center, should be the product of the number of students in the school and the per-student capitation grant. A closer examination of records at the Ministry of Education, however, revealed two sources of discrepancy from this formula. First, the growth in enrollment at the school level differed considerably from the central government statistics (see Reinikka, 2001, for a detailed discussion). Second, for the entire survey period (1991-95), the capitation grant was determined on the basis of the 1991 enrollment. Thus, the growth in enrollment observed at the school level over the period did not result in increased "intended capitation grants from the center" for the schools. For these reasons, we derive the denominator in (3.1), using 1991 enrollment data.

For several reasons, we believe the capitation grant data at the school level to adequately reflect what the schools received. First, the data was collected directly from the school records, which were kept for the schools' own needs. The school records were not submitted to any district or central authorities and did not consitute the basis for current or future funding. Thus, there were no obvious incentives to misrecord the data. The concern that headmasters might have underreported the school income in order to extract resources for themselves was allayed after interviews during the survey work, which did not support this claim. This is not surprising since the PTA was typically the principal decision-maker (and responsible for raising most of the income) at the school. Furthermore, parents who contributed the majority of school income presumably demanded financial information and accountability from the school (or PTA).

All available evidence, including World Bank records, also suggests that the capitation grants were fully released by the central government on a monthly basis. In the Ugandan treasury system, central ministries or other agents or individuals were unlikely to be able to capture central releases since they were subject to relatively elaborate pre-audit procedures.

D. Quantifying Capture

Did public resources reach the intended schools? Table 3.1 depicts information on the capture variable, the *share of intended capitation grants received*. Strikingly, on average, only 13 percent of the total yearly capitation grant from the central government reached the school. Eighty-seven percent either disappeared for private gain or was used for purposes unrelated to education. A majority of schools received nothing. Based on yearly data, 73 percent of the schools received less than 5 percent, while only 10 percent of the schools received more than 50 percent of the intended funds.

The picture looks slightly better when constraining the sample to the last year of the sample period. Still, only 22 percent of the total capitation grant from the central government reached the schools in 1995. Thus, in 1995, for every dollar spent on nonwage education items by the central government, roughly 80 cents were captured by local government officials (and politicians).

Given the large difference between the amount disbursed by the central government and the amount received by the schools, an obvious question is, what happened to the money? In other words, is the gap between central spending and school income really a measure of local capture? There are several reasons to believe that this is indeed the case. First, like most public programs at that time, the capitation grant was a national program in which local (district) offices were used as distribution channels. This gave local officials and politicians the opportunity to capture the funds. All available evidence also suggests that funds were regularly released by the center. Second, unlike other government programs, the capitation grant was a rare liquid money infusion into a local administrative and political system, which made is easier to capture the funds. Other public programs were primarily in-kind (for instance, health clinics were provided with drug kits). Third, qualitative information collected by the enumerators (Reinikka, 2001), and case-study evidence (McPake, et al., 1999), also suggest that funds were not redirected to other high-priority areas (from the households' perspectives), like health or infrastructure, thus refuting the claim that the observed gap was a simply a welfare-enhancing reallocation of funds to higher priority needs.

Although there is anecdotal evidence that part of the leakage was indeed corruption (theft), evidenced that several district educational officers were indicted after the survey results were made public, the available information points to the fact that the funds were largely used to finance the local political machinery. For example, there are many anecdotes about funds being used to increase allowances for councilors and bureaucrats, and that on the day funds actually arrived in the district, local politicians got together with the district officials to decide how they should be used. Given the lack of any recorded information at the local (district) level, it was not possible to quantify these mechanisms.

As illustrated in Table 3.1, there is variation in capture across regions, although the bulk of the variation is within regions. The standard deviation of capture (the *share of intended capitation grants received*) across regions is roughly one-third of the average standard deviation within regions.[14]In the next two sections, we attempt to account for this variation within (and across) regions.

4. A bargaining model of local capture

Below we set out a simple bargaining model to guide the empirical specification. The objective is to show that socio-political features of the school (parents and teachers) have implications for the equilibrium amount of leakage. The model assumes that the extent to which public funds reach the primary schools depends on the bargaining strength of the school vis-à-vis the district bureaucracy.

A. Basics

Consider a school i, $i \in I$, with n_i students. For simplicity, we assume each student (child) to belong to a separate household, h. Each (identical) household inelastically supplies one unit of labor and earns income y_i. Income is used to finance a private consumption good c_i, and educational services, e_i.

A household h with a child in school i has preferences, $U_{hi} = u(c_{hi}) + e_{hi}$, where e_{hi} is the (quantity and quality of) educational services provided to a student h in school i, and $u(.)$ is a standard utility function with $u' > 0$, $u'' < 0$.

We assume that e_i depends on both the amount of government-provided financial support, s_i, and the parents' own contribution, $\sum_h t_{hi}$. Thus, $e_{hi} = e_i = s_i + \frac{1}{n_i} \sum_h t_{hi}$. As an example, e_i could be text books or improved school facilities.

Each school is entitled to a grant g (per student). The grant program is executed at the local level by district officers. The district officials (or district bureaucracy - we will use both terms interchangeably) have discretion over the use of the funds and will disburse $s_i = g - x_i \geq 0$ per student to school i, where x_i is capture. While the district officials might care about education, we assume that they have more urgent needs for the funds (such as financing the local political machinery, increasing the wages of local administrators, or simply increasing their own consumption; i.e., corruption). Thus, the local officer will attempt to capture (in expected terms) as much of the public funds as possible. Formally, the district official maximizes, $EU^o = E \sum_{i=1}^{I} n_i x_i$.

[14]The results are similar when comparing within and across districts rather than regions.

The n_i households and teachers associated with school i form a parent-teacher association $(\text{PTA})_i$ that is the effective decision-maker at the school.[15] The PTA determines the contribution schedules $t_i = (t_{1i}, t_{2i}, ..t_{n_i i})$, and bargains for resources from the district official to maximize joint (household) welfare. Specifically, at the beginning of the game, the PTA receives an offer s_i. If it accepts, the game ends and educational services $e_i = s_i + \frac{1}{n_i} \sum_h t_{hi}$ (per student) are produced.

The problem for the PTA is that ex ante, g is not known; i.e., g is private information to the district official. Alternatively, we could assume that g is known, but that the school cannot determine (without a costly effort) if funds have been released by the central government. The PTA only knows that g is distributed on the interval $[0, \bar{g}]$, according to the distribution function $F(g)$.

The PTA can obtain information about g, for example by contacting the central government, but this is costly. Let θ be the school-specific cost of finding out the true g.

In case the PTA does not accept the offer, it can exercise its voice option.[16] Voice can take many forms (see Hirschman, 1970), including individual or collective petition and/or appeal to a higher authority, including local chiefs, or through various types of actions and protests. There is a cost κ, defined in per-student terms, to launch a protest. We can conceptualize κ in a variety of ways. In order to initiate a (successful) protest, the PTA must (most likely) disseminate the information about g to the parents; it must (most likely) build a coalition for action within the school, it might need to formulate an appeal to the Ministry of Education, and provide political contributions. All these actions are costly.

A protest is successful with probability π, in which case all intended funds (g) are disbursed to the school. With probability $1 - \pi$, the protest is not successful and the PTA will end up with s_i. $\pi \in (0, 1)$ is assumed to be exogenously given.

The timing of events are as follows. First, the PTA receives an offer s_i from

[15]By assuming that the PTA is the effective decisionmaker and can enforce its decisions (section 2 provides motivations for this assumption), we assume away free-riding problems. Given that most schools are fairly large (a median school has 429 students), we believe that while free-riding may be a problem in reality, it will not be an important variable in explaining differences in capture across schools. The reason is that the additional free-riding problem caused by increasing the school size from say 300 to 400 (or 500) students is not likely to be large. The free-riding problem may be important when comparing very small schools with large schools. In the empirical work, we control for school size.

[16]In reality, parents may also use their exit option; i.e., move their children to another school/district. See the discussion in section 6.2.

the district official and sets PTA fees t_i. The PTA can either accept or reject the offer. In case it rejects the offer, it can invest θ to find out the true g and, if optimal, exercise its voice option (launch a protest).

B. Equilibrium capture

How much of the intended funds will the district official transfer to the school, and what factors make leakage more likely? The problem can be solved by working backwards. Consider a PTA which has invested θ. Clearly, it will find it optimal to launch a protest if the expected gain, $\pi g + (1 - \pi)s_i - \kappa$, is larger than the certain payoff s_i. That is if,

$$g \geq \hat{g} \equiv s_i + \frac{\kappa}{\pi} \ . \tag{4.1}$$

Condition (4.1) can be re-stated as,

$$\pi \left(g - s_i\right) - \kappa \geq 0. \tag{4.2}$$

If the expected gain per student of a protest, the first term in (4.2), is larger than the expected cost, the PTA will launch a protest.

In the first stage, the PTA will decide to incur the information cost if its expected net benefit is nonnegative, that is,

$$\int_0^{\hat{g}} s_i f(g) dg + \int_{\hat{g}}^{\bar{g}} \left[\pi g + (1 - \pi)s_i - \kappa\right] f(g) dg - \theta / n_i \geq s_i \ . \tag{4.3}$$

The left-hand side (LHS) of (4.3) represents the expected income when θ is incurred, while the right-hand side is the (certain) level of funding per student in the absence of the information investment. Equation (4.3) can be rewritten as,

$$\int_{\hat{g}}^{\bar{g}} \left[\pi \left(g - s_i\right) - \kappa\right] f(g) dg \geq \theta / n_i \ , \tag{4.4}$$

which clearly illustrates the consequences of an unknown g. Only if the expected net gain per student of a protest is sufficiently large, (LHS) of (4.4), will the PTA incur the cost of acquiring information about public funding.

Equation (4.4) is a necessary condition for incurring the information cost. In addition, there is a liquidity constraint. The PTA must be able to afford the information investment and the protest cost. That is,

$$n_i \kappa + \theta \leq \sum_h t_{hi} \tag{4.5}$$

At the beginning of the game, the PTA chooses the contribution schedule t_i. All individuals in a school district are identical. With quasi-linear utility, equilibrium school funding is simply

$$t_{hi} = t_i = y_i - u_c^{-1}(1) \; . \tag{4.6}$$

Consider next the district official's problem. By choosing a s_i such that (4.4) binds, the official can ensure that no protest will be voiced by the PTA. This will be an optimal response provided that the upper bound on the expected grant g ($\bar{g}$) is not too large. Specifically, if $\bar{g} = g$, which is the case if g is known but the school cannot determine (without a costly effort) if the district has received all funds from the central government, it is optimal to choose s_i so that (4.4) binds. Extracting more resources will lead the PTA to invest θ and protest, which yields strictly lower expected utility for the district official. By extracting less, the official simply gives up rents to the school.

Proposition 1. *If π is sufficiently large, there exists an equilibrium without protest in which capture $(1 - s_i)$ is a non-decreasing function of the information cost θ and the protest cost κ, and a non-increasing function of average income y_i and the size of the school, n_i.*

Proof. See appendix. ■

The intuition for the results summarized in proposition 1 is straightforward. The cost of acquiring information and the cost of exercising voice have a direct bearing on the cost-benefit decisions in (4.2) and (4.4). A larger school will have lower per-student costs of acquiring information which has the same effect as a lower θ. Lower per student costs, in turn, imply that the district official must disburse more funds ex ante to avoid a protest. Parental income influences local capture through the liquidity constraint. Higher parental income implies that (4.2) is less likely to bind. The school can then threaten to initiate a protest.

An implication of proposition 1 is that if income is too low or the cost of acquiring information is too high, the school may end up with no funding. In this case, condition (4.2) may still hold with strict inequality, implying that a well informed school would initiate a protest (net gain of protest > 0). However, because the cost of acquiring information may be too high, the school chooses not to invest θ. As a result, the district officials can capture all funds. An example of such an outcome is illustrated in Figure 4.1.

This simple model illustrates two crucial points. First, information on allocation of central government spending can be misleading in explaining outcomes,

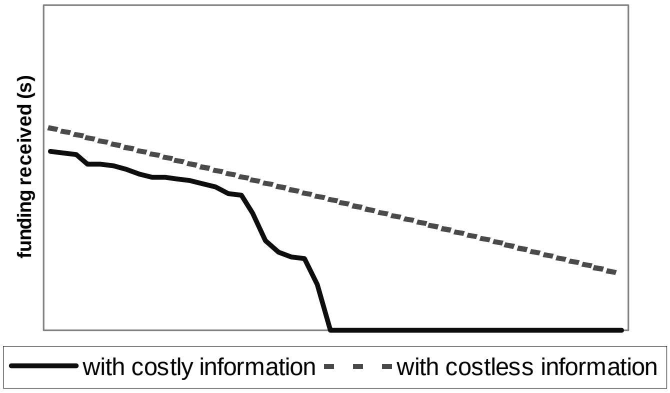

Figure 4.1: Funding received (s_i^*) as a function of the protest cost (κ_i)

in particular when institutions and oversight in the public sector are weak. That is, g and s may differ substantially. Second, the equilibrium amount of capture x_i^* is a function of the school's relative bargaining strength vis-à-vis the (district) bureaucracy.

5. Specification

The stylized model identifies four explanatory variables, n_i, κ_i, θ_i, and y_i. Generically, our empirical model can thus be stated as

$$x_{ijt}^* = X(n_{ijt}, \theta_{ijt}, \kappa_{ijt}, y_{ijt}) + \varepsilon_{ijt} \ , \tag{5.1}$$

where subscripts i, j, t refer to school, district, and year, respectively, and ε_{ijt} is an error term. Below, we discuss how we attempt to measure the variables in (5.1), and our baseline specification. In the next section, we present the initial results and thereafter, we consider relaxing some of the simplifying assumptions in the model underlying (5.1).

Only one of the explanatory variables in (5.1) is directly observable, namely the number of students (n_{it}). Thus, n_{it} is measured as the number of students in primary school (P1-P7) i at time t, denoted by *school size*. We do not have data on parental income. However, we do have information on parents' financial involvement in the school. *PTA income* is the average (per student) contribution by parents to the school. In the simple model of section 4, there is a one-to-one relationship between y and t. Thus, increased income implies larger contributions to the school at the margin. The investment costs κ_i and θ_i capture the costs of acquiring information and voicing a complaint, respectively. κ_i and θ_i are composite measures determined by various school-specific factors, such as the quality of the school leadership, the social cohesion in the school/community, distance to district headquarters, whether the school is located in an area that supported the (local) government, and access to media. It is plausible that a school with skilled leadership will require less resources to acquire information and initiate a protest. The social network determines the cost of agreeing, coordinating, and minimizing free-riding problems in case of a protest.[17] Distance to the headquarter, whether it is political or geographical, should have a direct effect on the size of κ_i and θ_i.

We use two variables to proxy for κ_i and θ_i. The first proxy is the quality of the school/PTA leadership, defined as the number of qualified teachers in relation to the total number of teachers in the school (the *share of qualified teachers*). This is a suitable proxy if formal education signals competence and competence determines the amount of resources that must be invested to acquire information and voice a complaint. The second proxy is a time-invariant, school-specific effect, η_i. Several plausible determinants of κ_i and θ_i can (in the short run) be treated as fixed. A detailed description of all variables is provided in appendix 2.

Obviously, when estimating the determinants of x_{it}, it is necessary to somehow scale the extent of local capture. Thus, we use the share of grants received by school i at time t in relation to what the school should have received $(s/g)_{ijt}$ as dependent variable. Log-linearizing (5.1), our empirical model is,

$$\log\left(\frac{s}{g}\right)_{ijt} = \beta_0 + \beta_1 \log \textit{qualified teachers}_{it} + \beta_2 \log \textit{PTA income}_{it}$$

[17] Studies on the role of social networks in overcoming coordination problems and reducing transaction costs in developing countries include Narayan and Pritchett (1999) and Wade (1988). To the extent that ethnic ties proxy for social networks, Miguel (2000) argues that ethnically diverse communities are less able to ensure enough social pressure for sustaining primary school contributions in rural western Kenya. In related work, Gugerty and Miguel (2000) show that higher ethnic diversity is associated with lower community participation in school meetings. Anecdotal evidence suggests that similar mechanisms apply to most parts of Uganda.

$$+\beta_3 \log students_{it} + \eta_i + \mu_t + \varepsilon_{ijt} \ , \tag{5.2}$$

where μ_t is a time-specific effect and ε_{ijt} is an error term. We allow for district and year-specific random effects. Thus, $\varepsilon_{ijt} = \bar{\varepsilon}_{it} + \hat{\varepsilon}_{jt}$, where $\bar{\varepsilon}_{it}$ is an idiosyncratic error term and $\hat{\varepsilon}_{jt}$ is a random district (j) and year (t) effect. The model suggests that $\beta_1,\ \beta_2,\ \beta_3 > 0$. Note that $\left(\frac{s}{g}\right)_{ijt}$ is censored from below; i.e., $s \geq 0$. For further references, let $\mathbf{z}_{it} = [\log \, qualified \ teachers_{it}, \log PTA \ income_{it}, \log \, students_{it}]$.

6. Results

A. Sample Data

Before proceeding, it is useful to take an initial look at the sample. Some schools did not report data for all five years, either due to missing records or because the school was not operational in the earlier years. Excluding a handful of misrecorded observations, we ended up with roughly 950 observations for 239 school.

Descriptive statistics are reported in Tables 7.1a and 7.1b, and Figure 7.1. In the sample, the average school size is 492 students. There are large variations, however, with the smallest school having 35 students and the largest roughly 100 times as many. The distribution is illustrated in Figure 7.1. The average student/teacher ratio is 32 students per teacher, with 68 percent of the teachers being qualified. Thirty-four schools (14 percent) reported that they did not have any qualified teachers for at least one year during the sample period. Only 13 schools had only qualified teachers at least one year during the sample period, and only one school had only qualified teachers during the whole sample period.

Parents contributed on average US$10 (in 1990 prices) to school expenditures. The data, however, again reveal large variations. Twenty-seven schools (11 percent) reported no supplementary income from parents in any year in which data were reported, while there are 44 school-year observations (5 percent) with *PTA income per student* above US$50. The median yearly contribution per student is US$1.60.

B. Basic findings

We start by looking at the simple relationship between local capture and the school characteristics, recognizing that there are several econometric issues that have not yet been addressed. We deal with these concerns in the following subsections.

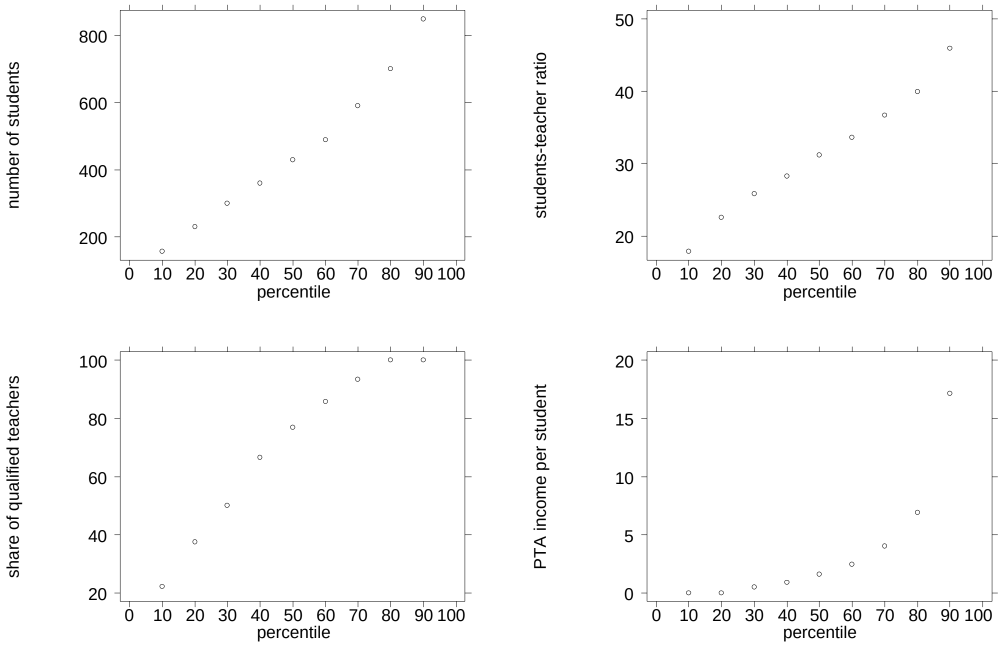

Figure 6.1: Cumulative distribution of explanatory variables

As a reference point, Table 7.2, column 1 reports a cross-section regression; i.e., equation (5.2) without school-specific fixed-effects. The *share of qualified teachers* enters significantly and with a positive sign. *PTA income per student* also enters with a positive sign, but is insignificant at standard significance levels. The variable *school size* enters with a negative sign. These results provide some weak support for the bargaining hypothesis. However, given that we do not control for any (time invariant) school/community characteristics, the results should be viewed accordingly. If the school-specific effects are correlated with the vector $\mathbf{z}_{it}$, the coefficients suffer from omitted variable biases.

Columns 2-5 report the results of estimating (5.2) with fixed-effects least squares. The first three columns show the partial effect of *PTA income per student*, the *share of qualified teachers*, and *school size*, on the *share of intended capitation* *grant received*, controlling for other (time invariant) community characteristics. All three variables enter with predicted signs and are highly significant, suggesting that local socio-political factors influence the schools' bargaining powers and thus, the extent to which funds are captured. The baseline regression is depicted in column (4). The **z**-variables are both individually and jointly highly significant, and the estimated effects are quantitatively important. A 1-percent increase in school size reduces capture by 0.8 percent. Similarly, a 1-percent increase in PTA support (higher parental income) increases the amount of public funding that reaches the school by 0.3 percent, and a 1-percent increase in the share of qualified teachers raises the amount of public funding that reaches the school by 0.4 percent.

Table 7.2 also reports two specification tests. F is the F ratio for the null hypothesis that all school-effects (η_i) are equal. H is the Hausman (1978) test statistic for testing the hypothesis that η_i and $\mathbf{z}_{it}$ are uncorrelated; that is, a test for fixed or random effects. Both hypotheses can be soundly rejected, thus providing support for our choice of a fixed effects estimator.

The preliminary findings reported in Table 7.2 support the main hypothesis of the paper: the equilibrium amount of local capture is a function of the schools' relative bargaining strength. The bargaining power, in turn, is a function of (average) parental income, school size, the quality of the school leadership, and a set of (time invariant) community/school characteristics. In section 4, we provide a plausible explanation why these variables should be of importance. Acquiring information and initiating a protest are costly actions. Schools with students of relatively wealthy parents are more likely to be able to afford these costs. The skill-level of the school leadership determines how costly it is to acquire information and voice a complaint, and to the extent that some of the costs are independent of the number of students, the per-student cost is inversely related to school size. The school-specific effects capture fixed factors such as the degree of social cohesion, political access, and the distance to district headquarters. The data show that these fixed factors are also important.

In the following subsections, we show that these qualitative results are robust.

B. Robustness tests

Table 7.3 reports the same set of regressions estimated by maximum likelihood (MLE). With censored data, fixed-effects least squares is inconsistent. All coefficients remain highly significant. As expected, the MLE estimates are larger than the fixed-effects least squares estimates. A simple comparison, how-

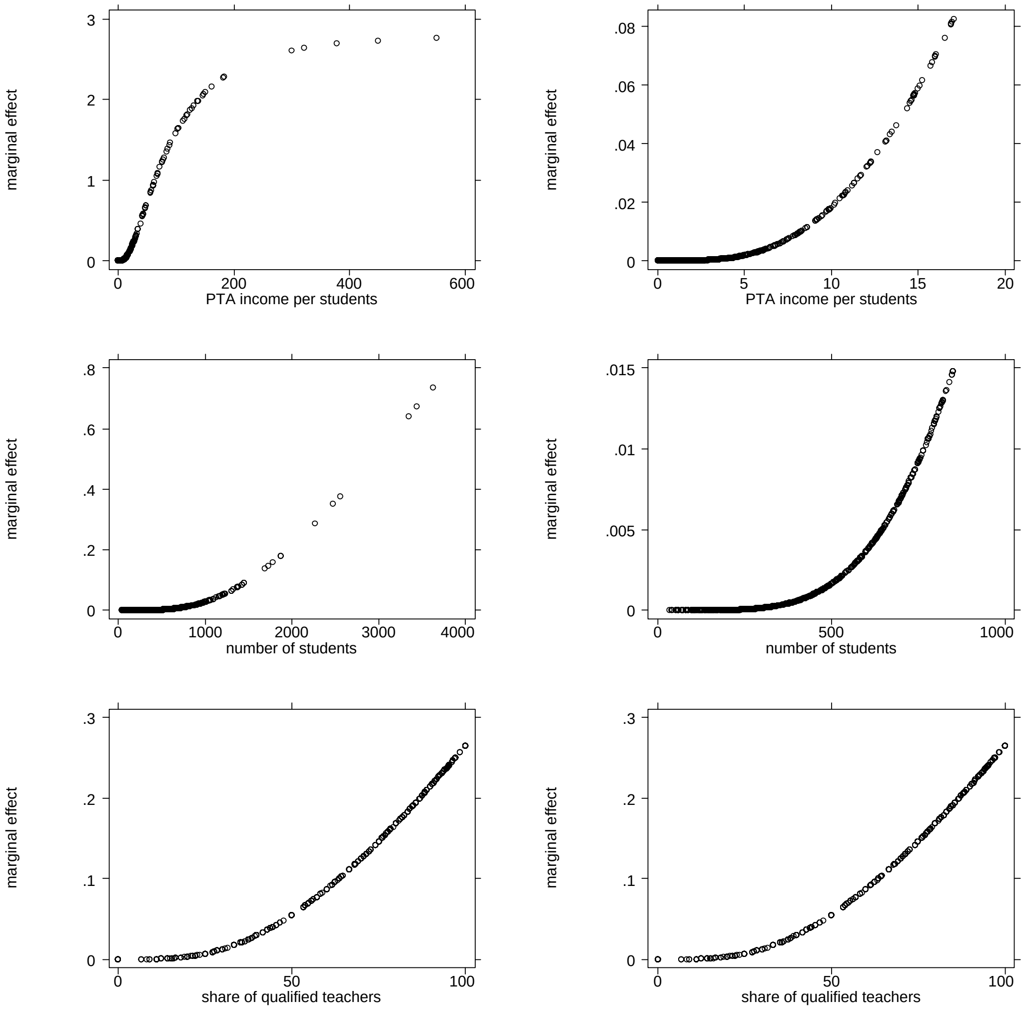

Figure 6.2: Marginal effects (in %) of changes in explanatory variables, $dE\left(\frac{s}{g}\right)_{it}/dz_{it}$. Left column: all sample. Right column: sample excluding top 10-percentile observations [Table 7.3, column (4)].

ever, is misleading since the unscaled coefficient vector ($\boldsymbol{\beta}^{MLE}$) only captures $\left[d\frac{s}{g}_{it}/dz_{it} \mid \frac{s}{g}_{it} > 0\right]$. The left-hand column of Figure 7.2 plots $dE\left(\frac{s}{g}\right)_{it}/dz_{it}$; that is, the expected marginal effect on grants received in relation to what should have been received of an increase in the explanatory variables. The right-hand column of Figure 7.2 plots the same derivatives $dE\left(\frac{s}{g}\right)_{it}/dz_{it}$ for all but the top 10-percentile observations. All derivatives are evaluated at the mean of the explanatory variables. For most schools; that is, for smaller schools, schools in poorer communities, and schools with relatively few qualified teachers, the marginal impact is small.

With school-specific fixed effects, $\boldsymbol{\beta}_z$ is identified from the deviation from school means. This identification strategy may be problematic if all variables have a common time trend. On the other hand, the data is noisy and including time effects places a strong restriction on the data. As shown in column (5), the effects remain intact when adding time effects. The coefficients are jointly highly significant, although the coefficient estimates on *PTA income per student* and the *share of qualified teachers* are smaller. With time effects, the *share of qualified teachers* becomes marginally insignificant (at the 10-percent level).

So far, we have relied on fairly restrictive assumptions to estimate the relationship between local capture and the schools' bargaining strength. For example, school size and parental income have been assumed to be fixed.

In reality, parents may not only use their voice option, but also their "exit option" to protest. Specifically, poorly financed schools (schools suffering from extensive capture) may not be able to attract many students. That is, students may self-select into well financed schools and/or may choose not to attend poorly funded schools. If this is the case, the estimated relationship between *school size* and the *share of intended capitation grant received* suffers from a sorting bias that would bias the coefficient on *school size* upward. On the other hand, in educational systems relying on local financing, more affluent communities can afford to build more schools (or support private schools), thereby suggesting fewer students per school in richer communities (cf. Duflo, 2000). Parents in these communities are likely to be more educated, have better political and bureaucratic access, and thus have a better chance of ensuring their share of local funding. This non-randomness in school construction would bias the coefficient on *school size* downward.

We deal with the potential sorting and non-randomness biases by instrumenting for *school size,* using district population data (denoted by *district population*). Generally, exploiting the variation across districts to identify the model is a reasonable identification strategy, given that the local institutional environment through which the grants are channeled is similar across districts. Specifically, *district population* is a potentially good instrument since, presumably, more densely populated districts will tend to have larger schools. Moreover, while there might be some sorting within districts, given the limited residential mobility in the presence of poorly functioning land markets, there is very limited mobility across districts. Likewise, to the extent that the variation in school construction intensity is mostly local, *district population* mitigates the potential non-randomness bias.

Using instrument techniques also addresses another significant estimation issue; i.e., the impact of "noisy" data. The problem with non-sampling measurement errors is a general concern when using micro-level data. While there are no strong incentives for the school to misreport the number of students in its own records, measurements or recording errors can still be expected. The district-level population data should serve to mitigate the effects of measurement error, since we generally think of these errors as being largely idiosyncratic to the school.

In principle, the estimated relationship between the *share of qualified teachers* and the *share of intended capitation grant received* suffers from a similar sorting bias: qualified teachers might self-select into well-financed schools. However, teachers could not shop around for jobs themselves because the appointments during the sample period were made by the districts. Teachers thus had a limited choice about choosing which schools to work in within a district. Good (qualified) teachers could try to get into private primary schools, but since our sample only consists of public schools, this selection problem is of less concern. The allocation of (quality) teachers across schools within a district may partly be determined by the relative bargaining strength of the schools.[18] However, to the extent that the **z**-variables and the school-specific effects capture the relative bargaining strength of schools, this will not cause a problem. Only to the extent that there are time-variant school-specific effects that influence both the allocation of teachers across schools and the *share of intended capitation grant received* will the coefficient on the *share of qualified teachers* be affected. We therefore choose to treat the *share of qualified teachers* as exogenous. This strategy also has empirical backing. If sorting is a common phenomenon, one would tend to see more qualified teachers in

[18]It is worth noting that with respect to the hiring of teachers, the central government (the Ministries of Education and Public Service and the Teacher Service Commission) clearly exercised some oversight over the district educational officers.

schools with better financial positions; that is, in schools with children of wealthy parents. This is not the case, as shown in Table 7.1b. The simple correlation between the *share of qualified teachers* and the *PTA income per student* is 0.08, suggesting that teachers are randomly assigned to schools to a large extent.

So far, we have (implicitly) assumed that the *PTA income per student* is solely driven by parental income. In reality, parents' contributions depend on both income and the amount of funds received. For a given y_j, well-financed schools (low capture) will receive less contributions from parents (substitution effect). This endogenous response will tend to mask the positive relationship between *PTA income per student* and the *share of intended capitation grant received*, and thus work against us. In addition, the *PTA income per student* may also be measured with error. These problems may again be mitigated by instrumenting using district based data. Our instrument (denoted by *mean consumption*) is created using household expenditure data.[19] Specifically, we used the 1992 Integrated Household Survey data to derive district level data on the mean consumption levels in 1992. Subsequent household surveys were used to derive annual district growth rates over the period, broken down by urban and rural. Combining these data, we could derive our instrument: the mean consumption levels across district-urban-rural location in 1991-95. The presumption is that wealthier districts on average also have wealthier parents that contribute more to the schools.

Table 7.4a depicts the first-stage regressions. The instruments perform well. *Mean consumption* [*district population*] is a significant predictor of *PTA income per student* [*school size*]. In both regressions, the instruments pick up roughly 3 percent of the variation in the explanatory variables. The reduced firm regressions are reported in Table 7.4b. The instruments enter with the predicted signs and are both individually and jointly significant.[20]

To deal with the censoring and the selection/measurement problems, we estimate the model by conditional maximum likelihood.[21] The results are given in Table 7.5. The IV-estimates are significantly larger than the ML-results given in Table 7.3. The large coefficient on *school size* suggests that selection issues are of less concern, but that the ML-results suffer from a measurement error bias and

[19] We wish to thank Simon Appleton for providing some of these data.

[20] When both instruments enter simultaneously, *mean consumption* is not significantly different from zero, although the instruments are still jointly highly significant. Multicolinearity in the instruments are one potential explanation for this.

[21] The conditional log-likelihood function for a simultaneous limited dependent variable model is given in Smith and Blundell (1986).

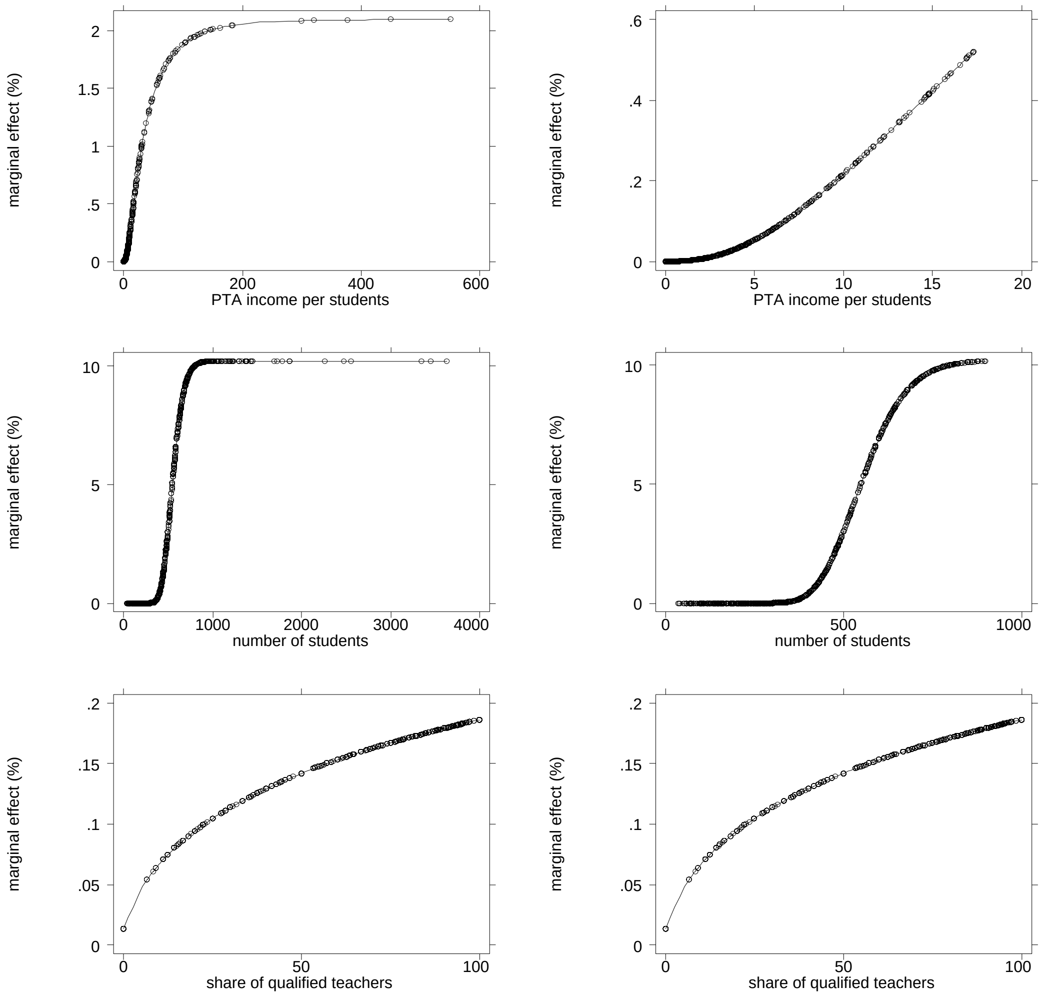

Figure 6.3: Marginal effects (in %) of changes in explanatory variables (IV-estimates), $dE\left(\frac{s}{g}\right)_{it}/dz_{it}$. Left column: all sample. Right column: sample excluding top 10-percentile observations [Table 7.5, column (2)].

possibly a bias due to non-randomness in school construction. Under plausible assumptions, both these types of biases push the estimate towards zero. Similarly, measurement and simultaneity problems mask the relationship between income and the *share of intended capitation grant received* in Table 7.3. These problems are mitigated when instrumenting for the *PTA income per student*.[22] As is evident from column (2), the results remain intact when including time-effects, although the coefficient estimates are smaller.[23]

The simultaneous limited dependent variable estimates are quantitatively large, also for smaller schools and schools with less wealthy parents. Figure 7.3 (left-hand column) again plots $dE\left(\frac{s}{g}\right)_{it}/dz_{it}$, with the right-hand column depicting the derivatives for all but the top 10-percentile observations. A 1-percent increase in school size (evaluated at the mean of all explanatory variables) reduces capture by 2 percentage points. A 1-percent increase in PTA support increases the amount of public funding that reaches the school by 0.25 percentage points, and a 1-percent increase in the share of qualified teachers reduces leakage by 0.27 percent.

We ran a number of additional robustness tests on the results reported above. One concern is outliers. So far, we have taken an extremely conservative approach with respect to outliers: only a few observations, which quite clearly seem to be a result of misrecording, have been dropped. However, some fairly serious outliers remain. In particular, there are 17 [3] observations on *PTA income per student* [*school size*], taking values of more than 3 standard deviations above the mean. While there is no theoretical justification for deleting these observations, it would be of considerable concern if our results were completely driven by them. To examine this possibility, we dropped all observations on *school size* and *PTA income per student* with values larger than 3 standard deviations above the mean. The results are similar to those reported above.

We added additional controls, including the student-teacher ratio and the tuition fee per student. Adding these variables did not change the results. Only the tuition fee per student had some explanatory power, as can be seen from Table 7.6, column (1). All other variables remain unchanged.

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[22] We note a similar pattern by comparing the fixed-effects least squares results in Table 7.3, with the two-stage, fixed-effects least squares estimation (results available upon request).

[23] It is worth noting that if the *share of qualified teachers* is also measured with error, the resulting attenuation bias, under plausible assumptions, pushes the estimate toward zero. Thus, the estimates in Table 7.5 are most likely to constitute a lower bound on the effects of a more qualified teaching staff.

In column (2), we add the district-based instrument variables *mean consumption* and *district population* to the basic regression. When instrumenting, the parameters are solely identified based on variation across districts. One might expect that there are processes at the district level, rather than at the school level, influencing the degree of leakage and thus explaining our results. Specifically, it is plausible that our instruments, district income and size, could directly influence the officials' possibilities to divert funds; that is, they have an independent effect on s/g. However, once controlling for the set of school-specific characteristics, the evidence suggests that these district characteristics are unimportant. The proxy for district income (district mean consumption level) even enters with a negative sign. The finding that the *share of intended capitation grant received* does not appear to be driven by these district-specific variables is important and suggests that they are indeed suitable as instruments. The result also supports the maintained assumption of the paper: the focus on school/community characteristics.

7. Conclusion

In this paper we have, to our knowledge, provided the first quantitative assessment of local capture in a large public expenditure program in a developing country. Even though the institutional environment in Uganda is not identical to other low-income (or Sub-Saharan African) countries, we believe that our estimate of local capture can nevertheless be viewed as a first approximation of similar programs elsewhere. In fact, initial results from subsequent studies (see Reinikka and Svensson, 2002, for a review) indicate that this is the case. For example, a public expenditure tracking study in Tanzania revealed that local councils captured a large part (57 percent) of the funds for school expenditures disbursed by the central government. In Ghana, a similar study indicated that only about 20 percent of non-wage public health expenditure and 50 percent of non-wage education expenditure reached the frontline facilities. Ongoing survey work in other parts of the world suggests that (local) capture is indeed an important and common phenomenon.

We have also argued that resource flows are endogenous to schools' sociopolitical endowment. Rather than being passive recipients of flows from the government, schools use their bargaining power vis-à-vis other parts of the government to secure larger shares of funding. Resources are therefore not allocated according to the rules underlying government budget decisions, with substantial equity and efficiency implications. One implication of this finding is that an understanding of the local political economy is required in order to estimate the actual budget allocation across end-users (in this case schools). In the case of school funding in Uganda, we have argued that this involves studying the bargaining game between the intended user (the school) and the administrator of the program (the district officers). Three variables seem important in explaining the variation in capture across schools: school size, income, and the extent to which teachers are qualified. Our results also indicate that a large part of the variation in local capture can be explained by (time invariant) school/community characteristics. Identifying what characteristics matter is an important area for future research.

As an example, anecdotal evidence indicates that the headmaster's relationship with district officials was an important factor in obtaining funding from the local government. Similarly, academically well-performing schools were often favored by district officials because they projected a positive image of them and the district as a whole. Well-performing schools attracted visitors from the center. Local officials, in turn, rewarded them by transferring more capitation grants. These anecdotes are consistent with the school survey data which show that, despite overall dismal spending outcomes, some schools were able to obtain most of their intended capitation grants.

The contribution of this paper is not only empirical. A methodological contribution is the design of a new survey tool—the quantitative service delivery survey—that can be used to gather data on government resource flow and frontline service delivery, including quantifying capture. Similar quantitative service provider surveys are presently being implemented in several other developing countries. In countries with poor accounting systems, such a survey can provide policymakers with valuable information both on inputs and outputs of the service delivery system. It also provides a new set of data for empirical research. In addition, information disseminated directly to the public can play a critical role in improving spending outcomes. In fact, the findings of the Uganda survey prompted a strong response from the central government. It began to publish monthly transfers of public funds to districts in newspapers and broadcast them on the radio. It also required primary schools to post notices on all inflows of funds. On the one hand, these measures aimed at empowering the users by lowering the cost of information and strengthening the schools' bargaining position vis-à-vis the local officers, whereas on the other hand, they aimed at changing the nature of the game by signaling a strengthened oversight by the central government. An initial assessment of these reforms suggests markedly improved outcomes (Republic of Uganda, 2000).

8. Appendix

8.1. Proof of proposition 1

Let $\tilde{s}_i^* = g - \frac{\kappa}{\pi}$: that is, $\tilde{s}_i^*$ is the s_i such that (4.2) binds. Consider the case when the school has made the information investment, θ. If $s_i \leq \tilde{s}_i^*$, the PTA will choose to initiate a protest and the official's expected payoff is $(1-\pi)(g-s_i)$. If $s_i > \tilde{s}_i^*$ the PTA will not protest and the official's expected payoff is $g - s_i$. Clearly the official will then either choose $s_i = 0$ (in which case the school will protest), or $s_i = \tilde{s}_i^*$, thereby avoiding a protest. Ensuring no protest by providing funding $\tilde{s}_i^*$ is optimal if

$$E\left[x_i \mid no\ protest\right] - E\left[x_i \mid protest\right] = \pi g - \frac{\kappa}{\pi} \geq 0\ . \tag{8.1}$$

Condition (8.1) is most likely to hold when π is large. Thus, for a sufficiently high π, the official will ensure enough funding so that no protest will be initiated.

Consider now the situation before the PTA makes its choice of whether to acquire information about g. Let $\hat{s}_i^*$ be the cutoff value of s_i implicitly defined by (4.3). That is,

$$\int_{\hat{s}_i^*+\frac{\kappa}{\pi}}^{\bar{g}} \left[\pi\left(g-\hat{s}_i^*\right)-\kappa\right] f(g)dg - \theta/n = 0 \tag{8.2}$$

Comparing (8.2) and (4.3), it is obvious that $\hat{s}_i^* < \tilde{s}_i^*$. Thus, if the district official offers $s_i < \hat{s}_i^*$, the PTA will invest θ (per student) and once g is known, will also initiate a protest. If π is sufficiently high this will result in the expected payoff $(1-\pi)(g-s_i)$, which is strictly lower than $g - \hat{s}_i^*$. Thus, provided that the credit constraint (4.5) does not bind, the equilibrium capture is given by $x_i^* = g - \hat{s}_i^*$.

Differentiating (8.2) yields,

$$\frac{ds}{dn} = \frac{\theta}{n^2\Lambda} \geq 0$$

$$\frac{ds}{d\kappa} = \frac{-\left[\pi\left(g-\hat{s}_i^*\right)-\kappa/n\right] f(\hat{g})\pi^{-1} - \int_{\hat{g}}^{\bar{g}} f(g)dg}{\Lambda} \leq 0$$

$$\frac{ds}{d\theta} = -\frac{1}{n\Lambda} \leq 0$$

where

$$\Lambda = \left[\pi\left(g - \hat{s}_i^*\right) - \kappa\right] f(\hat{g}) + \int_{\hat{g}}^{\bar{g}} \pi f(g) dg > 0 \ .$$

Substituting (4.6) into the credit constraint (4.5), yields

$$\kappa + \theta/n_i \leq y_i - u_c^{-1}(1) \ . \tag{8.3}$$

Clearly (8.3), holds for a wider range of parameter values κ and θ the larger average income y_i.

8.2. Data description

average share of teachers = average share of qualified teachers to total number of teachers in the district-urban-rural location.
district population = district population (source: Bureau of Statistics, Republic of Uganda).
mean consumption = mean consumption level in the district-urban-rural location (source: constructed using the 1992-1995 Uganda Household Surveys data).
PTA income per student = real PTA total income in US 1990 dollars/number of students (adjusted for inflation using end of year calendar data from the Department of Statistics).
school size = number of students in P1-P7.
share of intended capitation grant received = capitation grant received as a share of what should have been received. The amount that should have been provided is based on the number of students in 1991 (or the first year it was recorded), scaled by the ratio between the number of students in the school according to the survey and the number of students in the school according to the official statistics in 1991.
share of qualified teachers = share of qualified teachers to total number of teachers.
students-teacher ratio = students-teacher ratio.

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Table 2.1. School income data, 1991–95
(1991 prices in millions of U Sh)

	1991	*1992*	*1993*	*1994*	*1995*
Government	539.8	437.1	606.9	1,017.7	1,202.9
Teacher salaries	213.9	214.7	381.3	748.6	914.6
Capitation grants*	252.1	159.9	152.0	150.4	141.2
Rehabilitation and other	73.8	62.5	73.6	118.7	147.1
Parents (PTA)	772.3	840.5	1,087.8	1,371.8	1,649.9
PTA levies	591.1	609.6	775.2	934.9	1,032.7
Teacher salaries	125.8	134.1	196.0	300.7	475.9
Tuition fees	55.4	96.8	116.6	136.2	141.3
Total	1,312.1	1,277.61	1,694.7	2,389.5	2,852.8
(percent)					
Government	100	100	100	100	100
Teacher salaries	40	49	63	74	76
Capitation grants*	47	37	25	15	12
Rehabilitation and other	13	14	12	11	12
Parents (PTA)	100	100	100	100	100
PTA levies	77	73	71	68	63
Teacher salaries	16	16	18	22	29
Tuition fees	7	11	11	10	8
Total	100	100	100	100	100
Government	41	34	36	43	42
Parents (PTA)	59	66	64	57	58

*****Capitation grants based on what schools should have received; tuition fees are those actually collected from parents; other items are actual receipts by the schools.

Table 3.1. Share of intended capitation grant received
(in percent)

	Mean	*Median*	*St. dev.*	*Maximum*	*Minimum*	*Observations*
All schools						
1991–95	12.6	0	26.7	115.9	0	944
1995	21.9	0	33.7	108.9	0	208
Regions						
North	11.5	0	22.8	104.4	0	136
West	11.8	0	25.4	109.8	0	143
Southwest	8.1	0	23.7	101.6	0	131
Northwest	7.6	0	22.8	105.9	0	101
East	11.4	0	25.6	107.2	0	137
Northeast	17.5	0	27.2	108.9	0	146
Central	18.3	0	34.3	115.9	0	150
Region-year average	11.8	0	9.2	36.8	0	35

Table 7.1a. Descriptive statistics

Variable	*Mean*	*Med.*	*St. dev.*	*Max.*	*Min.*	*Obs.*
Students	492	429	350	3,828	35	942
Student-teacher ratio	32.0	31.2	12.3	110	6	942
Percent qualified teachers	68.4	76.9	29.9	100	0	938
PTA income per student [real 1990 US$]	10.1	1.6	36.4	550.7	0	942

Table 7.1b. Correlation matrix

Variable	*School size*	*PTA income per student*	*Share of qualified teachers*
School size	1		
PTA income per student	0.26	1	
Share of qualified teachers	0.40	0.08	1

Table 7.2. Explaining leakage across schools

Equation	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
Time	*1991-95*	*1991-95*	*1991-95*	*1991-95*	*1991-95*
Method	*OLS*	*FE-LS*	*FE-LS*	*FE-LS*	*FE-LS*
PTA income per student	0.133	0.421			0.336
	(.107)	(.096)			(.092)
	[.216]	[.000]			[.000]
School size	–0.332		0.828		0.827
	(.114)		(344)		(.324)
	[.005]		[.018]		[.012]
Share of qualified teachers	0.093			0.449	0.397
	(.054)			(.118)	(.124)
	[.088]			[.000]	[.002]
Wald					8.87
					[.000]
F		3.59	3.49	3.54	3.68
		[.000]	[.000]	[.000]	[.000]
H		12.35	21.72	16.74	43.65
		[.000]	[.000]	[.000]	[.000]
No. schools	239	239	239	239	239
No. obs.	938	942	942	938	938
Adj. R^2	.02	.39	.39	.39	.42

a. The dependent variable is the share of intended capitation grant received.
b. Estimation by OLS (column 1) and fixed-effects least squares (cols. 2-5), with district and year clustered errors.
c. Standard errors in parenthesis and p-values in brackets.
d. All variables in logarithms.
e. Wald is the test statistic for the null hypothesis that the coefficients on PTA income per student, school size, and share of unqualified teachers are zero, with p-values reported in brackets.
f. F is the F-ratio for the null hypothesis that all fixed effects are equal, with p-values reported in brackets.
g. H is the Hausman (1978) test statistic for the null hypothesis that the fixed effects are uncorrelated with the explanatory variables (z), with p-values reported in brackets.

Table 7.3. Explaining leakage across schools: Limited dependent variable estimation

Equation	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
Time	*1991–95*	*1991–95*	*1991–95*	*1995–95*	*1995–95*
Method	*MLE*	*MLE*	*MLE*	*MLE*	*MLE*
PTA income per student	3.061			2.756	0.932
	(.423)			(.423)	(.356)
	[.000]			[.000]	[.009]
School size		3.421		3.043	2.754
		(.780)		(.713)	(.607)
		[.000]		[.000]	[.000]
Share of qualified teachers			3.387	2.559	0.559
			(.550)	(.511)	(.361)
			[.000]	[.000]	[.122]
σ	2.515	2.648	2.551	2.343	1.840
Proportion y > 0	0.26	0.26	0.25	0.25	0.25
LR				120.8	41.23
				[.000]	[.000]
Time effects	No	No	No	No	Yes
No. schools	239	239	239	239	239
No. obs.	942	942	938	938	938

a. The dependent variable is the share of intended capitation grant received.

b. Estimation by maximum likelihood, with standard errors in parenthesis and p-values in brackets.

c. LR is the likelihood ratio test statistic for the null hypothesis that the coefficients on *PTA income per student*, *school size*, and *share of unqualified teachers* are zero, with p-values reported in brackets.

Table 7.4a. First-stage regressions

Equation	*(1)*	*(2)*	*(3)*	*(4)*
Time	*1991–95*	*1991–95*	*1991–95*	*1991–95*
Dep. Variable	*PTA income per student*	*School size*	*PTA income per student*	*School size*
Method	*FE-LS*	*FE-LS*	*FE-LS*	*FE-LS*
Mean consumption (district)	1.889	–0.355	1.753	–0.345
	(.632)	(.262)	(.622)	(.263)
	[.003]	[.176]	[.005]	[.191]
Population (district)	2.6E-5	2.1E-6	-4.4E-6	1.9E-6
	(1.4E-6)	(5.7E-7)	(2.0E-6)	(8.4E-7)
	[.053]	[.000]	[.025]	[.023]
Time effects	No	No	Yes	Yes
No. schools	239	239	239	239
No. obs.	942	942	942	942
Adj. R^2	0.82	0.90	0.83	0.90

a. Estimation by fixed-effects least squares, with standard errors in parenthesis and p-values in brackets.

Table 7.4b. Reduced form regressions

Equation	*(1)*	*(2)*	*(3)*
Time	*1991–95*	*1991–95*	*1991–95*
Method	*MLE*	*MLE*	*MLE*
Mean consumption (district)	25.70		0.751
	(2.75)		(4.77)
	[.000]		[.875]
PTA income per student		1.329	
		(.376)	
		[.000]	
Population (district)		5.8E-5	6.4E-5
		(6.0E-6)	(1.1E-5)
		[.000]	[.000]
School size	2.680		
	(.663)		
	[.000]		
Share of qualified teachers	1.544	.981	1.074
	(.406)	(.387)	(.389)
	[.000]	[.011]	[.006]
σ	2.164	2.067	2.103
Proportion y > 0	0.26	0.26	0.26
LR	170.2	201.3	188.4
	[.000]	[.000]	[.000]
No. schools	239	239	239
No. obs.	938	938	938

a. The dependent variable is the share of intended capitation grant received.

b. Estimation by maximum likelihood, with standard errors in parenthesis and p-values in brackets.

c. LR is the likelihood ratio test statistic for the null hypothesis that the coefficients on *mean consumption* (*PTA income per student*), *population* (*school size*), and *share of unqualified teachers* are zero, with p-values reported in brackets.

Table 7.5. Explaining leakage across schools: Instrument techniques

Equation	*(1)*	*(2)*
Time	*1991–95*	*1991–95*
Method	*Conditional MLE*	*Conditional MLE*
PTA income per student	5.320	2.055
	(1.432)	(1.239)
	[.000]	[.098]
School size	24.76	10.15
	(6.213)	(4.586)
	[.000]	[.027]
Share of qualified teachers	0.971	0.577
	(.373)	(.351)
	[.009]	[.101]
σ	2.027	1.839
Proportion y > 0	0.25	0.25
LR	207.2	35.71
	[.000]	[.000]
Time effects	No	Yes
No. schools	239	239
No. obs.	938	938

a. The dependent variable is the share of intended capitation grant received.

b. Estimation by conditional maximum likelihood (Smith and Blundell [1986]), with standard errors in parenthesis and p-values in brackets.

c. LR is the likelihood ratio test statistic for the null hypothesis that the coefficients on *PTA income per student, school size,* and *share of unqualified teachers* are zero, with p-values reported in brackets.

Table 7.6. Explaining leakage across schools: Additional robustness tests

Equation	*(1)*	*(2)*
Time	*1991–95*	*1991–95*
Method	*MLE*	*MLE*
PTA income per student	2.351	0.989
	(.425)	(.360)
	[.000]	[.006]
School size	3.186	2.754
	(.704)	(.610)
	[.000]	[.000]
Share of qualified teachers	2.386	0.575
	(.507)	(.365)
	[.000]	[.116]
Tuition fee per student	1.676	
	(.412)	
	[.000]	
Mean consumption (district)		–6.916
		(6.318)
		[.274]
Population (district)		1.6E-5
		(1.4E-5)
		[.272]
LR1	99.42	41.38
	[.000]	[.000]
LR2		1.45
		[.484]
Time effects	No	Yes
No. schools	239	239
No. obs.	938	938

a. The dependent variable is the share of intended capitation grant received.

b. Estimation by maximum likelihood, with standard errors in parenthesis and p-values in brackets.

c. LR1 is the likelihood ratio test statistic for the null hypothesis that the coefficients on PTA income per student, school size, and share of unqualified teachers are zero, with p-values reported in brackets.

d. LR2 is the likelihood ratio test statistic for the null hypothesis that the coefficients on mean consumption and population are zero, with p-values reported in brackets.

Appendix Figure 1.

Districts Included in the Primary School Survey (1996)