

Improving Schooling by Combating Corruption: Evidence from a Newspaper Campaign in Uganda

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What are the most effective ways to increase primary school enrollment and student learning? We argue that innovations in governance of social services may yield the highest return since social service delivery in developing countries is often plagued by inefficiencies and corruption. We illustrate this by using data from an unusual policy experiment: a newspaper campaign in Uganda aimed at reducing capture of public funds by providing schools (parents) with information to monitor local officials' handling of a large education grant program. The campaign was highly successful and the reduction in capture of funds had a positive effect on enrollment and student learning. (JEL: D73, I22, O12)

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1 Introduction

The Millennium Development Goals call for universal primary school enrollment by 2015. Yet many would argue that the large literature on schooling in developing countries provide few reliable insights about what governments in developing countries should prioritize to raise educational attainment. This lack of knowledge does not reflect a lack of interest in the economic profession but, as argued by Glewwe (2002), that most of the contributions suffer from methodological shortcomings.

In recent years, new methods, in particular evaluations based on randomized assignments, have been successfully applied to deal with one of these concerns; i.e., the problem of identifying causal effects.¹ While much remains to be done, the new literature has provided important insights on which specific school programs and inputs work and which don't. However, similar to the conventional literature, most of the contributions study schooling through a somewhat narrow lens; i.e., they assess impact of policy interventions at the school level. In many cases, this information alone is not enough to guide government policy in developing countries.^{2,3} When scaling-up a specific program found to work in a controlled randomized experiment, it is crucial also to have an understanding of the whole delivery chain; from the institutional constraints that affect central government policy decisions, through the incentive constraints that influence different layers of government agencies and officials implementing a given policy, to the actions and incentives of the end-producers (schools) and beneficiaries (students and parents).⁴ As the total impact (when scaling up a program to the national level) depends on all the elements in the service delivery chain, interventions that focus on improving governance in general and governance of social services in particular may be the most cost-effective way to increase school

¹As briefly reviewed in Banerjee et al. (2004), three types of policy interventions have been analyzed: (i) reducing the cost of schooling (examples include Duflo, 2001; Glewwe et al., 2002, and Vermeersh, 2002); (ii) rewarding students for doing well on tests (as in Angrist et al., 2002), (iii) improving the quality of education (examples include Banerjee et al., 2003; Glewwe et al., 2002, 2004; Miguel and Kremer, 2004)

²We are not aware of any study that explicitly test how changes (improvements) in governance in general and governance of social services in particular affect school-specific outcomes (enrollment and student learning). There are however several studies on policy formation at the local level in developing countries (see for example Duflo and Chattopadhyay, 2003) and on education policies and the financing of schools in particular (e.g. Kremer et al., 2002).

³For projects run by non-governmental organizations or aid donors; i.e. projects not intended to be scaled-up (by the government), this concern is of less importance.

⁴Thus, the issue we raise is about external validity. However, the problem is different from what is typically discussed in the literature on randomized evaluations. In fact, the external validity concern raised in the literature stems from the fact that the context of a specific intervention and time duration often matter to its outcomes, thus confounding interferences for scaling up from randomized trials. General equilibrium effects further complicates extrapolation.

enrollment and student learning.

We illustrate these points using data from an unusual policy experiment: a newspaper campaign in Uganda aimed at reducing capture of public funds by providing schools (parents) with information to monitor local officials' handling of a large education grant program. The program in question - a capitation grant to cover primary schools' nonwage expenditures - is a fairly standard one in developing countries. Like many other spending programs, this was a program that appeared to work well, based on central government budget data. Funds were disbursed by the Ministry in charge on a regular basis, and a benefit incidence analysis carried out by the World Bank suggested that benefit incidence of public spending was neutral (World Bank, 1996). However, like in many other spending programs in low-income countries, the situation on the ground was completely different from the official statistics.

A public expenditure tracking survey (PETS) to gauge the extent to which public resources actually filtered down to the schools showed that in the mid-1990s, the average school received only around 20 % of central government spending on the program (Reinikka and Svensson, 2004a).⁵ Most schools received nothing and the bulk of the grants was captured by local government officials (and politicians) in charge of disbursing the grant to the schools. The data also revealed large variations in grants received across schools. Contrary to the findings of the benefit incidence analysis, data on actual public spending reaching the schools showed schools in poorer communities to suffer significantly more from capture.⁶

The government initiated a newspaper campaign in response to the problems identified in the first PETS. As an anticorruption program it differs from the standard package. Traditionally, anticorruption programs target corruption in service delivery primarily through capacity building of the legal and financial institutions - judiciary, police, and financial auditors - in charge of enforcing accountability in the public sector. This is a top-down approach where some government agencies are assigned to monitor and control others. The problem is that in many poor countries the legal and financial institutions are weak and among the most corrupt. More resources to these agencies may therefore not be the best solution. For these reasons, the government decided to take the bottom-up route of citizen enforcement and began to publish data on monthly transfers of capitation grants to districts in the national newspapers (and their local language editions).

In Reinikka and Svensson (2004b) we show that public access to information can indeed be a powerful deterrent to capture of funds at the local

⁵For a conceptual discussion on public expenditure tracking and other similar surveys, see Dehn, Reinikka and Svensson (2003).

⁶While Uganda in the mid-1990s may have been somewhat of an extreme case with respect to local capture and corruption, available data indicate that the situation is similar in other Sub-Saharan African countries (see Reinikka and Svensson, 2004a).

level. Specifically, we show that head teachers in schools closer to a newspaper outlet are more knowledgeable of the rules governing the grant program and the timing of releases of funds by the central government. These schools also managed to claim a significantly larger part of their entitlement after the newspaper campaign had been initiated.

In this paper, we take these results as a starting point to explore the effects of improved “client power” on school enrollment and performance. Specifically, we use distance to the nearest newspaper outlet as instrument for exposure (knowledge) to the information campaign. We then relate distance to funding and funding to changes before and after the newspaper campaign started in enrollment and student learning (test scores). We find that the reduction in capture had a positive effect on both enrollment and student learning.⁷

The past few years has seen a small but growing body of research focusing on identifying and quantifying corrupt behavior.⁸ A subset of this body of research also addresses the questions: What can explain corruption? How can it be tackled? Here we take the logical next step and relate changes in measured corruption to changes in socioeconomic outcomes.⁹

The next two sections describe the situation before the newspaper campaign and lay out the key components of the reform in the late 1990s. Section 4 describes the survey data used in the empirical analysis and the method used to quantify capture. Section 5 then presents the empirical evidence in three steps. First, we discuss the first-stage relationship, i.e. that between distance and reduction in capture of funds. Second, we present the second-stage relationship between reduction in capture and enrollment. Finally, we present the relationship between reduction in capture and student learning. Section 6 concludes.

⁷Our findings complement those of Björkman (2004). Björkman’s study is based on data on all Grade 7 students in Uganda, rather than survey data, combined with district specific information on newspaper circulation. In line with the results in Reinikka and Svensson (2004b), Björkman exploits the fact that exposure to information about the per-student capitation grant, and thus funding, varied by district. Specifically, she uses the variation in newspaper circulation per district as an instrument of exposure and finds that pupils in districts which were highly exposed to the per-capitation grant program scored 0.42 standard deviations better in the Primary Leaving Exam than pupils in districts that were less exposed to the program.

⁸For a discussion of this literature, see Svensson (2005). Contributions dealing with public corruption include Di Tella and Schargrodsky (2003), Hsieh and Moretti (2005), Olken (2003), Reinikka and Svensson (2004a), and Svensson (2003).

⁹There is a large cross-country literature on corruption that looks at the relationship between subjective measures of corruption and economic outcomes (growth and poverty). See Svensson (2005) for references.

2 Institutional setup

2.1 Pre-campaign period

For a long time, Uganda has had a national policy of financing instructional material and other non-wage spending at primary schools through a capitation grant. The grant is a nationally set annual allocation per student and is intended to go to the schools, either in-kind or as a direct financial transfer. District offices are used as distribution channels. Although there are some differences across years, funds for the capitation grant program are disbursed by the central government nine times per year.

In all governments, resources earmarked for particular uses flow within legally defined institutional frameworks. Typically, funds pass through several layers of government bureaucracy down to service facilities, which are charged with the responsibility of spending the funds. However, in developing countries, information on actual public spending at the frontline level or by program is seldom available. To remedy this problem, a so-called public expenditure tracking survey (PETS) was developed (Reinikka and Svensson, 2004a). A PETS is designed to follow the flow of resources through various strata of government to determine how much of the originally allocated resources reach each level.

The first PETS was implemented in Uganda in the mid-1990s. The study was motivated by the observation that despite a substantial increase in public spending on education, the official reports showed no increase in primary enrollment. Specifically, the hypothesis was that actual service delivery, proxied by primary enrollment, was worse than budgetary allocations implied because public funds were subject to capture (by local politicians and public officials) and did not reach the intended schools. To test this hypothesis, a survey was conducted of 250 randomly chosen primary schools. The survey collected five years of data on spending (including in-kind transfers), service outputs, and provider characteristics. These data were then linked to survey data from 18 local governments (districts) and detailed disbursement data from three central government ministries (see Reinikka and Svensson, 2004a for details).

The initial PETS revealed a rather gloomy picture of governance in the education sector. Specifically, on average, only 24 percent of the capitation grant from the central government was reaching the schools in 1995 (table 1). Thus, nearly 80 cents of every dollar spent on nonwage education items by the central government was being diverted by local government officials. Most schools received nothing. Poor students suffered disproportionately because schools serving poor students received less than schools in other areas (Reinikka and Svensson 2004a).

Where did the money go? As discussed in Reinikka and Svensson (2004a),

there was no evidence of increased spending in other sectors. There was indirect evidence of theft, from numerous newspaper articles about indictments of district education officers after the survey findings went public. But anecdotal evidence suggests that most of the funds were used for patronage politics and funding of political activities. Case study evidence of district political financing and corruption in Uganda also points in the same direction. Thomas (1998, 1999) argues that power in district governments is concentrated among a small elite, connected by common schooling, marriage, friendships, ethnicities, and religion. Sustaining this power balance is costly. Public funds are fueling a system of patronage politics, in which patrons give clients material rewards for their political loyalty and services (see also Bayart 1993). The patronage system takes different forms. Politicians divert public resources to finance their own campaigns and those of friends and family or to finance local and private causes, including distribution of private goods such as salt, sugar, and beer to overcome voter dissatisfaction. Political parties, or in Uganda, the National Resistance Movement, must also supply patronage goods to its members.¹⁰ In rural areas an effective political organization depends on a personal presence in the area, which means a well-staffed institutional hierarchy all the way down to the village level. This model requires substantial resources, and diversion of public resources is often the only source of funding available. The diversion of funds was facilitated by the ignorance about the capitation grant in most schools.

2.2 Information campaign and voice

Hard evidence of corruption or capture is difficult for governments to simply brush aside. The Uganda case illustrates this well. When the extent of district government diversion of education funds became known in 1996, the central government reacted swiftly. Rather than taking the standard approach of yet another reform project on the supply side to improve the financial management system, the government decided to engage the citizenry.¹¹ Led by the Ministries of Local Government and Finance, the central government began to publish data in the national newspapers on the monthly transfers of capitation grants to districts. The main newspapers used were

¹⁰The National Resistance Movement is not formally a party, although it operates like one and its institutional structure is similar to other dominant party structures in other parts of Sub-Saharan Africa, with a national executive committee, a secretariat, and district, division, subcounty, town, parish, and village movement committees.

¹¹With few exceptions, anticorruption programs rely on legal and financial institutions - judiciary, police and financial auditors - to enforce and strengthen accountability in the public sector. The tacit assumption is that more and better enforcement of rules and regulations will reduce corruption. However, in many poor countries, the legal and financial institutions are weak and often corrupt themselves. In such a setting, providing more resources to enforcement institutions may not be the right solution to the problem of corruption.

the *The New Vision* (and its local language editions) and *The Monitor*. Apart from detailed data on transfers of education funds to the districts, newspapers published information on school entitlements and responsibilities under the universal primary education program, and on occasions also carried stories on misuse of the capitation grant funds.

The main objective of the newspaper campaign was to provide information on the grant program, including when funds was being transferred to the districts, so as to enable head teachers and parents to monitor the local administration and voice complaints if funds did not reach the schools. Hirschman (1970) identifies a set of conditions under which voice is the preferred mode of action for dissatisfied customers among available choices (voice, exit, and loyalty). Individuals must be able to compare outcomes. In the case of the school grant program this implies that they must be aware of the amount of their entitlement and how much the school actually receives and when it should receive it. Second, the expected return to voice must be higher than to the alternatives - to exit or not to act (loyalty). The relative returns depend on both the availability of an exit option and on the community's ability to take collective action. The propensity to resort to the voice option also depends on the readiness of a population to complain and on the existence of institutions and mechanism to transmit complaints cheaply and effectively. Finally, it depends on citizens' ability to either directly or indirectly sanction the district officers and/or local politicians.

These conditions suggest that the information campaign could be highly successful in reducing fund diversion. First, schools being exposed to the newspaper campaign should be aware if funds are being diverted, so that they can make an informed choice about whether to protest. Second, most households in Uganda, particularly in rural areas where most of the schools in the sample are located, have no easily available alternative to the local public school. While private schools exist, they are located in larger urban centers. This lack of an exit option increases the likelihood of voice as the response of choice to dysfunctional services (Hirschman 1970). Third, the collective action problem is likely to be a less important constraint in primary education than in other social sectors. Parents and school staff interact every day, and all schools have the institutions to handle collective decisionmaking in the form of school management committees, consisting of parents and the head teacher. Finally, communities have different ways to sanctions public officials/politicians, ranging from informal forms of social pressure (verbal complaints) to formal ones, such as local electoral sanctions (local politicians fear of losing an election) to career concern (public officials fear of losing their job or not getting promoted or getting a lower wage or wage increase). In Uganda in the late 1990s, district politicians were elected in (semi) competitive elections. Local politicians, in turn, had discretion over remuneration, hiring and firing decisions of education and accounting officers at the district level. In addition, by publicly informing

beneficiaries of their entitlements, the central government signaled strengthened oversight (to voters and local officials) and the priority it accorded to education (Stasavage 2003). Thus, as a community became better informed, district officials may have rationally believed that the threat of punishment increased if funds were captured and, consequently, had incentives to reduce capture of funds intended for the school.

The survey data indicate that the voice mechanism is indeed at play. Half of the schools reported that they did not receive the full amount of the capitation grant in 2001. Of these schools, 47 percent complained or protested to some formal or informal authority that could transmit the complaints onwards or act on them. These included central government officials and politicians, school inspectors, village or other local officials, village elders, and tribal leaders. Importantly, since both actual protest and the threat of voice may discourage the local political elite from diverting resources intended for the schools, in equilibrium, there is no reason to believe the incidence of voice and local diversion of funds (or better information) should be correlated. District officials may capture only as much as to ensure that a protest is not initiated. Thus, when a school/community becomes better informed, and hence its monitoring ability increases, the district officials may respond by reducing capture so as to ensure that a costly protest is not initiated.

2.3 UPE

Apart from the newspaper campaign, the education sector in Uganda has experienced at least one more major reform. In 1997, with the explicit aim to increase school enrollment, free-primary education (Universal Primary Education, UPE) was introduced. The reform prohibited schools to charge any type of school-fees, with the caveat that "only" four children per household could benefit from UPE in a given year.¹²

3 Predicted impact

Two major changes occurred in the primary education sector in Uganda in the late 1990s. First, school-fees were outlawed. Second, an information campaign was initiated to boost parents ability to monitor the local officials

¹²In practise, the announced UPE policy has evolved into one where all children receive free primary schooling (Stasavage 2003). There are several reasons for this. Families with fewer than four children have taken children of relatives to school. Moreover, the luganda (i.e. the language spoken by the Baganda - - the main ethnic/linguistic group in Uganda) equivalent of "four children going to school free" (abaana bana basome) was deliberately shifted to "abbaana bona basome", meaning all children going to school.

in charge of disbursing funds to the schools, with the primary objective to increase the share of entitlements actually reaching the schools. In appendix, we set up a simple model to help understand the possible effects on school enrollment and performance of these reforms. We highlight two mechanisms. First, lowering the cost of schooling increases the number of students that will complete primary school. However, compared to the counterfactual group that would complete primary school if schools were still allowed to charge fees, the increase in enrollment is made up of more "marginal" students, i.e. students with lower learning potential. As a result, the average skill of students falls. Second, under the assumption that increased funding to schools raise school quality, the marginal return to investing in education goes up. As a result, more students will complete primary school. This increase will again be made up of more marginal students, resulting in a fall in the average skill of students. However, the adverse effect is counter-balanced by the direct effect from higher quality to cognitive skills. Which effect dominates is unclear.¹³

4 Data

A public expenditure tracking survey was carried out in 2002 to assess the effects of improved access to public information. The survey replicated the 1996 survey, measuring the difference between the capitation grants disbursed by the central government and the resources actually received by the schools. In addition, it collected data on access to information and the means to acquire information on the grant program and other variables that may influence the bargaining position of individual schools.

The 1996 sample consisted of 250 schools, randomly drawn from 18 districts.¹⁴ To ensure that the sample had broad regional coverage (Northwest, North, Northeast, East, Central, Southwest and West) and that it was representative of the population of schools in the selected districts, schools were selected using a stratified random sample (see Reinikka 2001 for details). For each region two or three districts were drawn with a probability proportional to the number of schools in the district, and in each district 10–20 schools were visited, depending on the number of schools in the districts.¹⁵

¹³As we briefly discuss in the appendix, it is possible that an increase in the quality of education results in that "less" marginal students complete primary school. This could be the case if parents start sending their children to public instead of private schools following the change in quality of public education, or if high ability students (or their parents) are more responsive to changes in the quality of education.

¹⁴The selected districts were : 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).

¹⁵For both surveys (1996 and 2002) enumerators were trained and closely supervised

Not all schools in the original sample could be resurveyed in 2002 because of security concerns. Two districts (Moroto and Bundibugyo) were dropped, reducing the sample by 20 schools. One district (Gulu) experienced a major insurgency during the data collection phase, and an additional 11 schools had to be dropped. And one school in the original sample had closed, resulting in a final sample of 218 schools.¹⁶

The 2002 survey collected detailed information on receipt of funds and school enrollment and administered a knowledge test to head teachers.¹⁷

The school-specific measure of grant diversion, s_{jt} , is grants received (by school j in year t) as a share of grants disbursed by the central government to that school. A school's entitlement is based on the number of students in grades P1–P3 and P4–P7. In 1995 the grant formula allocated 2,500 Ugandan shillings (US\$) a year for each student in grades P1–P3 and 4,000 US\$ for each student in grades P4–P7. In 2001 the amounts were 5,000 US\$ for grades P1–P3 and 8,100 US\$ for grades P4–P7.¹⁸

Records from the Ministry of Finance indicate that this rule was followed unless districts did not submit the required quarterly documentation, in which case funds could be delayed or withheld in the following months. These records show that in fiscal 2000/01, 93 percent of the approved funds were released, although some districts received significantly less (for example, the central government withheld 49 percent of the funds to Kyenjojo and 25 percent to Kayunga, both newly established districts). The actual amounts disbursed by the central government were confirmed by the public expenditure tracking survey at the district level. To adjust for the withholding effect in deriving the diversion measure, a school's entitlement was scaled down by the share of funds actually released by the center to the district.

As with the 1996 survey data, the grant data collected at the school level appears to adequately reflect what schools actually received. The data were

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.

¹⁶An additional complication was that since the 1996 survey, four districts had been split, thus yielding a sample of 22 districts. The new districts are Adjumani, previously part of Moyo, Kyenjojo, previously part of Kabarole, Kayunga, previously part of Mukono, and Katakwi, previously part of Soroti.

¹⁷The 2002 survey also formed the basis for a related research project. An additional 170 schools from 9 of the original 18 districts were surveyed. The sampling frame for these additional schools was based on the 2001 school census, and the sampling procedure was similar to that of the 1996 survey. Specifically, a stratified random sample was chosen where each district was weighted according to size (number of schools). Thereafter, one district was randomly chosen from each region. Two additional districts were then selected from the two largest regions. The nine selected districts were Apac, Arua, Bushenyi, Kabale, Hoima, Kamuli, Pallisa, Mukono, and Soroti. The number of schools to be sampled from each of these 9 districts was proportional to the number of schools in the district.

¹⁸The grant has maintained its real value in U.S. dollar terms (the P1 to P3 entitlement for 1995 and 2001 was 2.9 U.S. dollars per student in 2001 prices).

collected directly from the school records, and in most cases the enumerators could double-check the information using copies of checks received. School records were kept for internal use only. They were not submitted to district or central authorities and were not used as a basis for funding. The risk that head teachers might have underreported the school income in order to divert funds for themselves was perceived as less serious, since each check had to be signed by at least two people (the head teacher and the chairperson of the school management committee).

School enrollment data were collected from school and district records. The numbers were very similar (the simple correlation is 0.97). The average of these two numbers was used to calculate each school's aggregate entitlement for the year (table 1).

Summary statistics indicate that the situation has improved dramatically since the mid-1990s (tables 1 and 2). Schools, which had received only 24 percent on average of the total yearly grant from the central government in 1995, received more than 80 percent in 2001. More striking, while the median school received nothing in the mid-1990s, it received 82 percent of its entitlement in 2001. Thus the extent of diversion fell dramatically. However, diversion is still a problem for many schools. On average, 20 percent of school entitlements do not reach the schools, and about 30 percent of schools receive less than two-thirds of their entitlements.

Test score data are collected from Uganda National Examination Bureau. Standardized tests (primary leaving exams, PLE) in Math, English, Science, and Social Studies are conducted yearly on all grade 7 students. Each of the subjects are graded from 1 to 9 and we use the sum of the four tests as our primary measure of cognitive skills. There are pros and cons with using PLE as a measure of cognitive skills. One advantage is that almost all students in the last grade of primary school (grade 7) take the test. Passing the test is a requirement for acceptance into secondary school, so students have incentives to do their very best on the test. A concern is that the test are censored both from below (0) and above (9). The censoring problem is mitigated by the fact that we look at school averages and the data reveal there is no school at either censoring point.

5 Improving Schooling by Combating Corruption

5.1 Identification

Availability of newspapers and year jointly determine a school's (community's) exposure to the information campaign. The information campaign was initiated in 1997 and broaden in the following year. Prior to 1997, schools' knowledge about the grant program was largely a function of own

effort and ability.

Access to newspapers varies greatly across schools in Uganda. In the survey data, roughly 50 percent of the head teachers reported access to a newspaper on a regular basis. Access to a newspaper, however, is partly endogenous. Moreover, even a head teacher without access to newspapers may be well-informed about the grant program if parents in the community have access to them. Similarly, parents may take action based on information published in newspapers they have access to, even if the head teacher does not have personal access to one. This “community effect” can be viewed as a measurement error problem. The variable of interest is the community’s, i.e. head teacher’s and parents’, exposure to information, or knowledge, about the grant program, but only the head teacher’s access to newspapers is observed.

To deal with these concerns, we use a two-step procedure. First, data from a simple knowledge test administered to head teachers are used to measure knowledge of the program in 2001. Second, distance to the nearest newspaper outlet, denoted *distance*, is used to instrument for exposure to the information campaign.

For distance to the nearest newspaper outlet to serve as a legitimate instrument, it must affect a school’s (head teacher’s and parents’) exposure to information about the grant program but have no direct effect on its ability to claim funds from the district. In Reinikka and Svensson (2004b), we provide a battery of tests on the validity of the instrument. We discuss these tests below.

5.2 Instrument validity

The IV method we employ, i.e. using *distance* as instrument, makes the counterfactual assumption that absent the newspaper campaign, funding to schools would change at a rate independent of proximity to a newspaper kiosk. Although we cannot test this directly, we can look at the reduced-form relationship between distance to the nearest newspaper outlet on change in diversion of funds *prior* to the information campaign. Thus we estimate

$$s_{jt} = \beta_0 x_{jt} + \beta_1 distance_j + \beta_2 \sigma_t + \beta_3 \sigma_t distance_j + \mu_j + \varepsilon_{jt} , \quad (1)$$

where σ_t is a dummy taking the value 0 in period 1991 and 1 in period 1995 and where β_3 is the reduced-form estimate. The school-specific effects can be differenced away, so that

$$\Delta s_j = constant + \beta_0 \Delta x_j + \beta_3 \Delta distance_j + \Delta \varepsilon_j . \quad (2)$$

This control experiment is reported in table 3, column 1. As evident, being located near a newspaper outlet has no effect on a school’s ability

to claim funds from the center. Thus, proximity to a newspaper kiosk is uncorrelated with the changes in capture over the 5-year period prior to the campaign.

To constitute a valid instrument, *distance* must also be correlated with degree of exposure. We turn to this next. As reported in Reinikka and Svensson (2004b), *distance* is highly correlated with the head teachers' access to newspapers. A head teacher in a school near a newspaper outlet is 35 percentage points more likely to report access to a newspaper than a head teacher in a school one standard deviation further away from such an outlet. The result remains intact when differences in income is controlled for.¹⁹

Table 4 reports a set of regressions where the dependent variables, scores on knowledge tests administered to head teachers, are regressed on distance to the nearest newspaper outlet

$$q_j = \delta_0 x'_j + \delta_1 distance_j + \varepsilon_j . \quad (3)$$

The regressions show that head teachers serving in schools close to a newspaper outlet are better informed about the formula used for deriving the capitation grant (table 4, specification 1). They are also better informed about the timing of releases of funds by the central government (specification 2). When both of these are combined into an aggregate score (*info*), the results show that distance to the nearest newspaper outlet has a strong negative effect on head teachers' knowledge about the grant program in general (specification 3).

A concern with these results is that head teachers serving in schools close to a newspaper outlet may be more knowledgeable in general, and schools with more knowledgeable head teachers may suffer less from capture. While a valid concern, note that the control experiment reported in table 3 suggests that these potentially more knowledgeable head teachers did not manage to claim more funds prior to the newspaper campaign. A way to test if proximity to a newspaper outlet is correlated with head teachers' general ability is to compare the head teachers' knowledge in other areas. On tests of knowledge about news events and people covered in newspapers at the time of the survey in 2002, head teachers serving in schools close to a newspaper outlet are, as would be expected, significantly more likely to score higher than head teachers serving in schools further away (see specification 4 in

¹⁹Following Reinikka and Svensson (2004a) the mean consumption level across district-urban-rural locations is used as a measure of income (denoted *income*). The mean consumption level is derived from national household survey data. Note that the district-urban-rural location has no administrative or political boundaries. This will mitigate the danger of the variable picking up processes at the district level that could have a direct bearing on the degree of local fund diversion, rather than income per se.

table 4).²⁰ But for head teachers' knowledge of local affairs²¹ and knowledge of general (sociopolitical) issues typically not reported in newspapers, there is no significant difference between head teachers in schools close to a newspaper outlet and those in schools farther from an outlet (see specifications 5 and 6 in table 4).²²

Though the tests do not provide a comprehensive assessment of head teachers' knowledge and abilities, the findings suggest that it is information on the grant program disseminated through newspapers and correlated with *distance* that accounts for the observed effects rather than some unobserved characteristic such as teachers' ability. Two additional tests support this claim (see Reinikka and Svensson, 2004b).²³

The relationship between knowledge about the grant program and proximity to a newspaper outlet remains intact when *income* is added as an explanatory variable. Although richer communities are likely to demand more newspapers and may therefore be closer to a newspaper outlet, in our random sample of almost exclusively rural schools, proximity to a newspaper outlet is insignificantly correlated with income. Second, *distance* does not seem to proxy for remoteness, which may have an independent effect on a school's ability to claim funds from the district. The strong relationship between *distance* and information about the grant program remains

²⁰ Respondents were asked to identify Winnie Byanyima (outspoken Member of Parliament and the wife of a former presidential candidate), Thabo Mbeki (president of South Africa), Bono (rock singer who was touring Africa at the time of the survey), George W. Bush (president of the United States), and Charles Onyango-Obbo (outspoken editor of *The Monitor*). Respondents were also asked to name the minister of education (Hon. Makubuya), minister of finance (Hon. Ssendaula), and prime minister (Hon. Nsibambi). Head teachers with access to newspapers were significantly more likely to respond correctly to each question (except the question on President Bush, for which there was no significant difference). The variable "knowledge about recent news events" is the average score on these eight questions, where a correct answer is coded 1 and an incorrect answer is coded 0.

²¹ Respondents were asked to name the district education officer, chief administrative officer in the district, Local Council 5 chairman, Local Council 3 chairman, and their representative in Parliament. Head teachers in schools close and farther way from newspaper outlets were equally as likely to respond correctly to these questions. The variable "knowledge about local affairs" is the average score on these five questions, where a correct answer is coded 1 and an incorrect answer is coded 0.

²² Respondents were asked the following questions: what is the (a) largest newspaper by circulation in Uganda (*The New Vision*), (b) location of East African Parliament (Arusha, Tanzania), (c) MTN (cellular/mobile phone provider), (d) month when the government's budget is presented to Parliament (June), (e) number of districts in Uganda (56), and (f) number of members of Parliament (305). The variable "general political knowledge" is the average score on these six questions, where a correct answer is coded 1 and an incorrect answer is coded 0.

²³ Although access to information is measured as the head teachers' knowledge about the grant program, the results should not be interpreted as the effect of informing only the head teachers about the program but more general the effects of informing the community about entitlements and corruption in the school grant program.

once controlling for distance to district headquarters (a proxy for distance to the district capital) and distance to the nearest bank branch (a proxy for distance to the nearest urban center). Finally, distance to the nearest newspaper outlet has an independent effect on *info* over and above increasing the likelihood that the head teacher has access to a newspaper. This result is consistent with the claim that a head teacher may be well-informed about the grant program even without having newspapers, if parents in the community where the school is located have access to them.

Estimates of equation (3) are of intrinsic interest because they assess the impact of the newspaper campaign on knowledge about the grant program. But they also represent the first stage of a two-stage least squares procedure to estimate the impact of improved access to public information as a tool to reduce diversion of grant funds. Table 3, column 2, presents reduced-form coefficients of distance to the nearest newspaper outlet on change in diversion of funds *over* the information campaign period. That is, estimates of equation (1) with the treatment dummy variable σ_t taking the value 1 in the post-campaign year 2001 and 0 in the pre-campaign year 1995. For the campaign experiment there is a strong relationship between distance to the nearest newspaper outlet and reduction in grant fund diversion, in sharp contrast to the control experiment reported in column 1.

Column 3 depicts the estimates of the structural regression,

$$\Delta s_{jk} = \text{constant} + \gamma_0 \Delta x_{jk} + \gamma_2 \widehat{\text{info}}_j + \eta_j, \quad (4)$$

where the predicted school-specific outcomes, $\widehat{\text{info}}_j$, are attributed to *distance*_{*j*} and hence reflect factors outside the school's (community's) control.²⁴

Schools that are more exposed to the newspaper campaign - therefore more informed - experience a significantly larger reduction in district government diversion of funds after the campaign starts.^{25,26}

The quantitative effect of improved access to public information is large. The instrumental variable (IV) estimate implies that a 1 standard deviation increase in *info* results in a 1.1 standard deviation increase in spending reaching the schools (a 44.2 percentage point increase in funding reaching the school between 1995 and 2001).

²⁴All sample information (388 observations) reported in table 5 is used to derive the generated regressor $\widehat{\text{info}}_j$.

²⁵The fit of the first-stage regression (equation 2, with *info* as the dependent variable) is good. The *t*-test of the significance of *distance* in the first-stage regression is 3.73.

²⁶Both ordinary least squares (OLS) standard errors and bootstrapped standard errors are reported in table 8. The OLS standard errors are biased because they ignore the uncertainty in the estimated effect of *distance*_{*j*} on *info*_{*j*} in the first-stage regression. In practice, this does not appear to be much of a problem since the two standard error estimates are close.

5.3 Specification

With a valid instrument of exposure, we can link the degree of exposure to changes in capture, and then link the predicted changes in capture that are attributed to exposure to changes in enrollment and student learning. The structural model then takes the following form:

$$info_j = \delta_0 x_j + \delta_1 distance_j + \varepsilon_j \quad (5)$$

$$s_{jt} = \beta_0 x_{jt} + \beta_1 info_j + \beta_2 \sigma_{2001} + \beta_3 \sigma_{2001} info_j + \mu_j + \eta_{jt} \quad (6)$$

$$y_{jt} = \gamma_0 x_{jt} + \gamma_1 s_{jt} + \gamma_2 \sigma_{2001} + \gamma_3 \sigma_{2001} s_{jt} + \mu_j + \theta_{jt} \quad (7)$$

where $info_j$ is a measure of the head teachers' knowledge about the grant program, s_{jt} is share of the grant reaching the school, σ_{2001} is a treatment dummy taking the value 1 in the post-campaign year 2001 and 0 in the pre-campaign year 1995, y_{jt} is school outcome (enrollment or average test scores), μ_j is a school-specific fixed effect and ε_j , η_{jt} , θ_{jt} are iid error terms. We can difference away the school-specific effects. Thus, equations 6 and 7 can be restated as

$$\Delta s_j = constant + \beta_0 \Delta x_j + \beta_3 info_j + \Delta \eta_j, \quad (6')$$

$$\Delta y_j = constant + \gamma_0 \Delta x_j + \gamma_3 \Delta s_j + \Delta \theta_j. \quad (7')$$

Note that using $distance$ or the predicted school-specific outcomes, \widehat{info} , attributed to $distance$ in 6' yields identical results as the model is exactly identified.

5.4 Improving Schooling by Combating Corruption: Enrollment

Did the reduction in capture have an effect on enrollment and student learning? We look at these issues next.

Table 1 report summary statistics on enrollment. From 1995 to 2001, average school size has almost doubled.²⁷ One possible explanation for this dramatic change is the newspaper campaign, but clearly the removal of school-fees in 1997 has likely had a large impact as well.

²⁷Due to different idiosyncratic events in the period in-between the two surveys (1995 and 2001), such as a reduction in the number of classrooms or grades (due to adverse shocks or lack of maintenance), some schools have experienced structural shifts in their ability to accept new students. While there is no reason to believe that these idiosyncratic events are correlated with distance, due to a relatively small sample size, this may still be problematic. We can partly deal with this problem by removing the schools that reported adverse shocks during the period. Specifically, a handful of schools (10) experienced a dramatic fall in attendance during the period 1995 to 2001 due to different (exogenous) circumstances. These schools were consequently dropped from the sample.

Specification 1, Table 5, reports estimates of equation 7', where $\Delta students_j$ is the change in number of enrolled students in grade 1 to 7 between 1995 and 2001, and where the predicted school-specific change in funds reaching the school, $\Delta \hat{s}_j$, are attributed to \widehat{info}_j (or $distance_j$). The estimated effect is economically large. A one standard deviation increase in Δs_j results in a 0.66 standard deviation increase in school enrollment (i.e., 297 more students per school).

The results in columns 1 and 2 may be partly driven by sorting. That is, it may be the case that school enrollment in aggregate does not increase due to the newspaper campaign but that students sort into schools with more resources. While problematic, in reality there are reasons to believe that this problem is not so severe. Our sample consists almost exclusively of rural schools and the pool of potential students served by these schools does not typically have much choice with respect to primary school. More important, if there are schools close by, the result in previous sub-section suggest that the exposure to the campaign may be similar (since the distance to a newspaper outlet will be similar). Specification 2, which adds the distance to the closest school the students could attend as explanatory variable, also does not support the claim that sorting is an important issue.

Specifications 3 and 4 depict the reduced-form coefficients of distance to the nearest newspaper outlet on the change in school enrollment. Specification 3 shows the estimate of *distance* over the period (1995-2001). There is a strong relationship between distance to the nearest newspaper outlet and increase in school enrollment since the newspaper campaign started. In contrast, the control experiment reported in specification 4 shows that there is no relationship between enrollment and *distance* in the five-year period prior to the campaign (1991-1995). That is, being located near a newspaper outlet had no impact on school enrollment prior to the campaign, but a large effect once the campaign had started.

Specifications 5 and 6 provide complementary evidence. We were able to assemble test scores data from the Uganda National Examination Bureau for the sample of schools four years prior to the information campaign (1991, 1993, 1994, 1995) and two years after the campaign started (2001, 2002). From this data set we can derive school-specific information on number of grade 7 students. Combined with the survey data on distance to newspaper outlet, we can then estimate (the reduced form)

$$grade\ 7\ students_{jt} = \gamma_0 \mathbf{x}_{jt} + \gamma_1 distance_j \sigma_t + \mu_j + \delta_t + \varepsilon_{jt} , \quad (8)$$

where σ_t is a treatment dummy taking the value 1 in the post-campaign years 2001 and 2002 and 0 otherwise.

Apart from being able to estimate the impact based on a larger sample of schools and years, using grade 7 data derived from the Primary Leaving Exams (PLE) records have one additional benefit. One concern is that the

survey data could be inflated, although attempts have been made to ensure this was not the case.²⁸ Inflating the PLE data is much more difficult since these are based on submitted tests.

The number of enrolled grade 7 students are significantly higher in schools exposed to the information campaign. The effect is large. In the post campaign years 2001 and 2002, a school near a newspaper outlet has 0.49 standard deviations more grade 7 students (18 students) enrolled than a school one standard deviation further away from such an outlet. As reported in specification 6, the effects remain highly significant when we use the more conservative Huber-White standard errors clustered at the school level (as suggested by Bertrand, Duflo, and Mullainathan, 2004).

5.5 Improving Schooling by Combating Corruption: Test scores

Table 6 presents evidence on the impact of the newspaper campaign on cognitive skills.²⁹ The base line specification is similar to equation 8, i.e.,

$$\text{average primary leaving exam score}_{jt} = \gamma_0 \mathbf{w}_{jt} + \gamma_1 \text{distance}_j \sigma_t + \mu_j + \delta_t + \varepsilon_{jt}, \quad (9)$$

In specification (1) we restrict attention to the pre-campaign year 1995 and the post campaign year 2001. We first estimate the reduced form relationship between test scores and distance to the nearest newspaper outlet (*distance*), controlling for income. The coefficient on *distance* is (economically) large but the coefficient is imprecisely estimated (marginally insignificant at the 10-percent level). Specification (2) depicts the IV-estimates following the procedure discussed above. The IV estimate enters with a positive sign and but is also imprecisely estimated. The coefficient estimate

²⁸If these incentives to misreport are constant across schools, this is less of a concern since we look at the difference across schools. It is possible that the more informed schools are more likely to inflate enrollment numbers (because they realize the total grant is a function of number of students enrolled). However, if the district officials are at least partly aware of these incentives, this would tend to work against finding an effect of the newspaper campaign. The informed schools would then have high reported enrollment numbers but also suffer from extensive capture (if the district disburse the grant based on the actual rather than the reported numbers). What we find in the data, however, is that informed schools suffer less from capture and increased enrollment faster than the less informed ones.

²⁹Note that we compare average student achievement across schools and time. The model suggest that the reduction in capture and hence increased spending at the school level should influence the pool of graduating students (i.e., more marginal students). Thus, our estimate is a reduced form one capturing the impact of the intervention on average achievement. It is not an estimate of the impact of individual learning.

suggests that a one-standard deviation change in s_j results in a 0.64 standard deviation increase in average test scores. For the average school, this would represent a 34 percent improvement (from 10.2 to 13.7).

In column (3), we report the results of estimating 9. The treatment variable, $distance_j\sigma_t$, enters highly significant and with a coefficient very similar to that in column (1). Taken together, these findings suggest that being closer to a newspaper outlet after the newspaper campaign started significantly increased spending to the school, which in turn had a large impact on school performance. Overall, i.e., across all primary schools in Uganda, test scores fell between 1995 and 2001 (Bjorkman, 2004). The results in Table 6 thus imply that schools that managed to increase funding, i.e., reduced capture, also managed to keep test scores basically unchanged during the period despite a large increase in enrollment driven both by increased funding (table 5) and reduced cost of schooling (UPE reform).

In specification (4), we allow for an arbitrary variance-covariance matrix, i.e. we cluster the standard errors by schools. The standard errors increase considerably but the treatment variable still enters significant.

The identifying assumption in the difference-in-differences model 4 is that without the campaign the reduction in grant diversion would not have been systematically different in the group of schools with and without access to newspapers, implying that test scores should have been uncorrelated with *distance* prior to the campaign. One way to test this assumption is to compare the change in outcomes before the campaign; i.e., during the period 1991-95. Table 7 presents this control experiment. We estimate equations similar to 9, but only use pre-campaign data. The treatment variable is $distance_j\sigma_t$ with $\sigma_t = 1$ in 1995 in specification (1), $\sigma_t = 1$ in 1994 in specification (2), $\sigma_t = 1$ in 1993 in specification (3), and $\sigma_t = 1$ in 1991 in specification (4). The difference-in-differences estimates are insignificant in the first three specification and marginally significant, but with a positive sign, in specification (4). Thus, if anything, the schools closer to a newspaper outlet appear to have performed worse in the pre-campaign period.

5.5.1 Discussion and additional robustness tests

We argue that distance to a newspaper outlet and year jointly determine a school's (community's) exposure to the information campaign. There are at least two concerns with this empirical approach. First, it is possible that distance captures some unobserved variable that is correlated with the school's ability to extract capitation funds. To the extent these unobserved characteristics are fixed, this will tend to work against finding an effect and thus will not be a major cause of concern.³⁰ The problem arises if

³⁰For example, if high-ability head teachers are more likely to be close to a newspaper outlet and also more able to make a claim for the grant independent of the newspaper

instead there are unobserved time-variant characteristics that are correlated with distance. For example, proximity to a newspaper outlet may be a correlate of a school’s ability to extract capitation funds once the leakage problem becomes public. While we cannot totally rule out this possibility, it is difficult to reconcile this alternative explanation with the results presented in tables 3-4. In addition, even if proximity to a newspaper outlet captures differences in ability to react to new information, rather than differences in knowledge about the program, as long as differences in the ability is not directly influencing enrollment and test scores, our empirical approach is still valid. That is, we still estimate the causal effects of increased funding, sparked by the newspaper campaign, on school outcomes.

A more serious concern would be if proximity to a newspaper outlet is a proxy for some unobserved characteristic that affects schooling. For example, “politically connected” schools or communities may be closer to a newspaper outlet and may be able to attract better (or more) teachers. Note that the results of the control experiments suggest that the value of politically connectedness was nil before the newspaper campaign started, so for this to be a concern political connectedness must have changed in the late 1990s. Distance may also proxy for some other geographical characteristic, such as remoteness, that may also influence schools ability to attract both students and teachers. And if this effect becomes more important due to the policy changes in the late 1990s, this may also bias our findings.

To test for these alternative hypotheses, we augmented equations 8 and 9 with two proxies of remoteness, distance to district headquarters (a proxy for distance to the district capital) and distance to the nearest bank branch (a proxy for distance to the nearest urban centre), interacted with the treatment dummy σ_t . We also experimented with two measures of political connectedness, a dummy variable indicating if the school has a local council official (a politician) in their school-management committee or if they have received discretionary support for school construction from the local government or other sources. Again we allow these variables to have a time-variant effect on school outcomes. The robustness results are reported in Table 8 and Table 9.

Panel B depicts the estimates of the four variables, interacted with the treatment dummy σ_t , when *not* controlling for schools’ (community’s) exposure to the information campaign. Panel A reports the augmented regressions results. In panel B, the remoteness proxies have a significant, and predicted, effect on school outcomes. That is, being closer to a urban centre or a district capital after the newspaper campaign was initiated results in a

campaign, $E[s_j | t = 2001, \textit{short distance}]$ and $E[s_j | t = 1996, \textit{short distance}]$ would both increase. However, the difference between them would likely fall since s_j is the share (in percent) of the grants reaching the school and a fully funded school has $s_j = 100$. Thus, the existence of fixed school-specific effects that are correlated with distance and the probability of claiming grants would result in a downward bias in the estimate of interest.

larger increase in enrollment and improved test results. However, once controlling for proximity to a newspaper outlet, these effects disappear (panel A). In the augmented specifications, our measure of a school's exposure to the information campaign ($distance_j\sigma_t$) remains highly significant, albeit somewhat less precisely estimated.³¹ The two measures of political connectedness have positive and significant effects on changes in both enrollment and learning between 1995 and 2001. However, they are uncorrelated with our measure of exposure and thus only marginally affect the estimate of $distance_j\sigma_t$.

6 Concluding remarks

What is the most effective way to increase primary school enrollment? In this paper we have argued that innovations in the governance of social services may yield the highest return since social service delivery in developing countries is often plagued by inefficiencies and corruption. We have illustrated this by using data from an unusual policy experiment. A newspaper campaign in Uganda aimed at reducing capture of public funds by providing schools (parents) with information to monitor local officials' handling of a large education grant program.

We find that both student enrollment and achievement increased substantially in schools that, thanks to the information campaign, managed to claim a higher share of their entitlements.

It is interesting to compare our findings with the recent studies on school achievement based on randomized trials. The policy experiment we study differ in two important aspects from the existing literature on schooling in developing countries: the level in the service delivery chain the intervention takes place and what resources are made available to the school. First, the intervention we study in the paper focuses on governance of social services, rather than the impact of specific school inputs. Second, as we have argued above, the end result of the information campaign was that more resources became available at the schools. Schools were free to spend their grants on whatever nonwage items they needed, be it textbooks, school meals, school uniforms, or flipcharts (or even to boost wages since funds generated by the schools themselves are fungible). In contrast, the studies by Miguel and Kremer (2004), Glewwe et al. (2002, 2004) and others, evaluate the impact of tied aid, i.e. the look at the impact of providing specific school inputs. Yet another difference is that we look at the compounded effect over a few years. Traditionally, impact has been evaluate only one year after the intervention.

Publicity cannot solve all the problems of corruption and diversion of

³¹This is to be expected since newspapers are likely to be available in both district capitals and urban centres.

funds in the provision of local services. And improving schooling requires more than to improve governance in general and governance of social services in particular. However, one conclusion we draw from the Uganda experiment is that since traditional approaches to improve governance have produced weak results in most developing countries, experimentation and evaluation of new tools to enhance accountability should be an integral part in the research agenda on improving outcomes of social services.

7 Appendix

Assume that parents make decisions for their children. Each family i has, for simplicity, one child. There is a continuum of families, $i \in [0, 1]$, that potentially could send their children to school. There are two periods. In period 1, the child either works at home, goes to school, or both. In the second period, the child is an adult and works for a wage. In period 1, the parents derive direct benefit from the child's work (at home), while in period 2 the parents benefit from transfers from their child (now an adult).

Parents' utility function is

$$U_i = c_1^i + \delta c_2^i, \quad (\text{a1})$$

where c_t^i is i th parents' consumption in period t , δ is a discount factor.³²

Cognitive skills, a , are acquired according to

$$a^i = \alpha^i q(r) h(s^i), \quad (\text{a2})$$

where α^i is learning efficiency of child i (which compose of many factors, such as innate ability, child motivation, parental motivation, etc.), $q = q(r)$ denotes school quality as function of (public) resources provision r to the school, and s is fraction of time in period 1 spent in school ("years of schooling"). The functions $q(\cdot)$ and $h(\cdot)$ are increasing and concave. The parameter α^i is drawn from a uniform distribution $U[0, 1]$.

Parents' consumption in each time period is given by

$$c_1^i = y_1 - ps^i + \eta(1 - s^i) \quad (\text{a3})$$

and

$$c_2^i = y_2 - \gamma y_c^i, \quad (\text{a4})$$

where y_t is parental income (exogenous), p is the price of schooling, $\eta(1 - s)$ is the income generated from home production by the child in period 1, y_c^i is the child's income when working as an adult in period 2, and γy_c^i is the share of the child's income transferred to her parents.

Equation (5) completes the model, relating child cognitive skills to child income,

$$y_c^i = \omega a^i \quad (\text{a5})$$

In this simple model, parents cannot borrow or save. The only way to shift income between periods is to alter the investment in children's education.³³

Maximizing (a1), subject to the budget constraints (a3)-(a4), yields the first-order condition

$$-(p + \eta) + \alpha^i \theta q(r) h'(s^i) \leq 0, \quad (\text{a6})$$

³²One could also assume that the parents care about education per se (parental tastes for educated children). This would provide yet another reason for sending their children to school (see for instance Glewwe, 1999).

³³Introducing savings and borrowing would reduce parents' incentives to invest in education but would not eliminate them. Specifically, if one assumes that all investments are risky, parents would diversify their investments along several different alternatives, including children's education (Glewwe, 2002).

where $\theta \equiv \delta\gamma\omega$. The first-order condition states that for a given ability of the child, α^i , parents will choose to invest in education up to the point where the marginal cost of more schooling, taking the form of higher school fees (p) and reduced time for home production, is equal to the marginal gain, taking the form of increased transfer from a more educated and hence higher paid child.

Assume a child completes primary school if she has s^p years of schooling. The first-order condition (a6) then tells us that

$$\mu^p = 1 - F(\alpha^p) = 1 - \alpha^p \quad (\text{a7})$$

students will complete primary school, where $\alpha^p = \frac{(p + \eta)}{\theta q(r)h'(s^p)}$.

Average cognitive skills of the students completing primary school, a^p , i.e., at the time they complete primary school, is then

$$a^p = E[a^i | \alpha^i > \alpha^p] = q(r)h(s^p) \int_{\alpha^p}^1 \alpha^i f(\alpha^i | \alpha^i \geq \alpha^p) = \frac{1}{2}q(r)h(s^p)(1 + \alpha^p) \quad (\text{a8})$$

Differentiating (a7) and (a8) yields the following results.

Result 1: $\frac{d\mu^p}{dp} < 0$; $\frac{da^p}{dp} < 0$. That is, lower cost of schooling increases the number of students that will complete primary school. However, since the increase is made up of more "marginal" students, the average skill of students falls.

Result 2: $\frac{d\mu^p}{dr} > 0$; $\frac{da^p}{dr} \leq 0$. That is, increased funding to the school and hence higher school quality increases the marginal return to investment in education. As a result, more students will complete primary school. This increase will again be made up of more marginal students, resulting in that average skills of students fall. However, the adverse effect is counterbalanced by the direct effect from higher quality to cognitive skills. Which effects dominates is unclear.

Thus we have two effects of the reforms in the late 1990s. Both reforms will tend to increase enrollment. The information campaign, to the extent it results in higher spending reaching the school, may also result in an increase in average cognitive skills. It should be noted that the model is highly stylized. For example, parents only invest in the length of schooling. Moreover, school characteristics like class size are considered fixed. It is possible, that the increase in enrollment may lead to increased class size with potentially adverse effect on the learning environment. Also, different economic groups, with different skill distributions, may have been differentially affected by the reforms.

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Table 1. Summary Statistics on School Characteristics, 1995 and 2001 Surveys

	Median	Mean	Standard deviation
<i>1995</i>			
School size (number of students)	450	530	367
Student-teacher ratio	37	37	11
Income (Ugandan shilling)	7,315	7,785	3,612
Ratio of qualified to total teachers	0.88	0.79	0.25
<i>2001</i>			
School size (number of students)	855	949	469
Student-teacher ratio	55	56	16
Income (Ugandan shilling)	9,001	10,322	5,078
Ratio of qualified to total teachers	1	0.91	0.17
Distance to newspaper outlet (kilometers)	9	15.3	33.3

Source: Authors' calculations based on 2001 survey and Ministry of Education and individual school records; see text for details.

Table 2. Summary Information on Capitation Grants Received as Share of Entitled Grants, 1995 and 2001 Surveys (percent)

	Mean	Median	Standard deviation	Maximum	Minimum	Number of observations
<i>All schools</i>						
1995	23.9	0.0	35.1	109.8	0.0	229
2001	81.8	82.3	24.6	177.5	9.0	217
	1995	2001				
<i>Regions</i>						
Central	24.3	92.8				
North	26.7	102.4				
Northwest	11.2	90.3				
West	24.0	71.6				
Southwest	21.1	83.3				
East	20.1	62.4				
Northeast	36.0	73.4				

Source: Authors' calculations based on 2001 survey and Ministry of Education and individual school records; see text for details.

Table 3. Instrument validity

<i>Specification</i>	(1)	(2)	(3)
Regression	Control experiment: reduced form	Campaign experiment: reduced form	2 nd stage
Dep. variable	Δs	Δs	Δs
Distance to nearest newspaper outlet	0.62 (2.55)	-6.77** (2.62)	
2001		75.7*** (7.74)	2.30 (21.1)
1995	18.6** (7.70)		
Info			65.9*** (23.5) [23.6]
Schools	147	199	199

Notes: Income is included as control.

Robust standard errors in parenthesis and bootstrapped standard errors in square brackets.

*** [**] (*) denote statistically significant at 1 [5] (10) percent levels, respectively.

Source: Reinikka and Svensson (2004b).

Table 4. Measuring and instrumenting for exposure

Dependent variable	Specification					
	(1)	(2)	(3)	(4)	(5)	(6)
	Knowledge about grant formula ^a	Knowledge about timing ^b	Information about grant program ^c	Knowledge about news events ^d	Knowledge about local affairs ^e	General political knowledge ^f
Distance to nearest newspaper outlet	-0.063*** (.021)	-0.040** (.020)	-0.103*** (.029)	-0.039*** (.010)	-0.001 (.004)	-0.013 (.010)
Range of scores	[0,1]	[0,1]	[0,1,2]	[0,1]	[0,1]	[0,1]
Average test score	0.65	0.24	0.89	0.65	0.75	0.57
Number of schools	388	388	388	388	388	388

** Significant at the 5 percent level. *** Significant at the 1 percent level.

a. A binary variable 1,0 indicating correct (=1) or incorrect (=0) knowledge about grant formula.

b. A binary variable 1,0 indicating correct (=1) or incorrect (=0) knowledge about timing of releases of the grant.

c. The sum [0,2] of “Knowledge about grant formula” and “Knowledge about timing.”

d. Average score [0,1] on eight questions on recent news events, where correct answers are coded 1 and incorrect answers are coded 0.

e. Average score [0,1] on five questions on local affairs where correct answers are coded 1 and incorrect answers are coded 0.

f. Average score [0,1] on six questions on general political knowledge, where correct answers are coded 1 and incorrect answers are coded 0.

Note: Numbers in parentheses are robust standard errors. See text for details of the regression.

Source: Reinikka and Svensson (2004b).

Table 5. Estimates of the effects of the newspaper campaign on enrollment

<i>Specification</i>	(1) ^{ii,iii}	(2) ^{ii,iii}	(3) ⁱ	(4) ⁱ	(5) ⁱ	(6) ^{iv}
Dep. variable	Enrollment	Enrollment	Enrollment	Enrollment	Enrollment grade 7	Enrollment grade 7
Years	95, 01	95, 01	95, 01	91, 95	91, 93, 94, 95, 01, 02	91, 93, 94, 95, 01, 02
Method	IV, first difference	IV, first difference	Reduced form, diff	Reduced form, diff	OLS	OLS
2001	162 (105)	12.1 (173)	557 ^{***} (49)			
1995				68.4 [*] (35.1)		
Distance			-30.6 ^{**} (15.7)	-4.65 (12.0)		
Share of funding reaching school	5.33 ^{**} (2.73) [2.61]	7.23 ^{***} (2.78) [2.69]				
Income	-0.01 (.01)	-0.003 (.01)	-0.02 ^{***} (.01)	0.002 (.02)	1.4E-4 (4E-4)	1.4E-4 (6E-4)
Distance to nearest school		1.51 (.96)				
Distance* post-campaign periods					-5.21 ^{***} (.79)	-5.21 ^{***} (1.58)
Time effects					Yes	Yes
School effects					Yes	Yes
Schools	187	182	187	153	377	377
Observations	187	182	187	153	1829	1829

Notes: (i) Robust standard errors in parenthesis. (ii) The change in share of funding reaching school is instrumented for by distance to nearest newspaper outlet. (iii) Bootstrapped standard errors in brackets. (iv) Standard errors clustered by school in parenthesis. *** [**] (*) denotes statistically significant at 1 [5] (10) percent levels, respectively.

Table 6. Estimates of the effects of the newspaper campaign on test scores of grade 7 students

<i>Specification</i>	(1) ⁱ	(2) ^{i,iii}	(3) ⁱ	(4) ⁱⁱ
Dep. variable	Test scores	Test scores	Test scores	Test scores
Years	95, 01	95, 01	91, 93, 94, 95, 01, 02	91, 93, 94, 95, 01, 02
Method	OLS, first difference	IV, first difference	OLS	OLS
Distance*post-campaign periods	-0.477 (.399)		-0.532*** (.177)	-0.532** (.252)
Share of funding reaching school		0.059 (.059)		
Income	2.9E-4 (2.1E-4)	8.8E-5 (2.5E-4)	-1.4E-4 (8.3E-5)	-1.4E-4 (1.1E-4)
Time effects	-	-	Yes	Yes
School effects	-	-	Yes	Yes
Schools	188	172	376	376
Observations	188	172	1824	1824

Notes: (i) Standard errors in parenthesis; (ii) Standard errors clustered by school in parenthesis; (iii) IV regression with distance*2001 as instrument.

*** [**] (*) denotes statistically significant at 1 [5] (10) percent levels, respectively.

Table 7. Control experiment

<i>Specification</i>	(1) ⁱ	(2) ⁱ	(3) ⁱⁱ	(4) ⁱⁱ
Dep. Variable	Test scores	Test scores	Test scores	Test scores
Years	91, 93, 94, 95	91, 93, 94, 95	91, 93, 94, 95	91, 93, 94, 95
Distance*1995	-0.073 (.260)			
Distance*1994		-0.152 (.212)		
Distance*1993			-0.268 (.211)	
Distance*1991				0.532 (.342)
Income as control	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes
School effects	Yes	Yes	Yes	Yes
Schools	329	329	329	329
Observations	1117	1117	1117	1117

Notes: Standard errors clustered by school in parenthesis.

*** [**] (*) denotes statistically significant at 1 [5] (10) percent levels, respectively.

Table 8. Robustness: Enrollment

<i>Specification</i>	(1)	(2)	(3)	(4)
Dep. variable		Enrollment grade 7		
Years		91, 93, 94, 95, 01, 02		
<i>Panel A</i>				
Distance to newspaper outlet *post-campaign periods	-4.80 ^{***} (.90)	-4.95 ^{***} (.94)	-5.21 ^{***} (.79)	-4.82 ^{***} (.80)
Distance to district capital *post-campaign periods	-0.97 (1.01)			
Distance to urban centre *post-campaign periods		-0.53 (1.02)		
Local council official in SMC*post-campaign periods			-0.58 (2.25)	
Receipt of discretionary support*post-campaign periods				10.1 ^{***} (3.10)
<i>Panel B</i>				
Distance to district capital *post-campaign periods	-3.54 ^{***} (.90)			
Distance to urban centre *post-campaign periods		-3.41 ^{***} (.87)		
Local council official in SMC*post-campaign periods			0.39 (2.28)	
Receipt of discretionary support*post-campaign periods				12.9 ^{***} (3.10)
Controls	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes
School effects	Yes	Yes	Yes	Yes
Schools	377	377	377	377
Observations	1829	1829	1829	1829

Notes: Robust standard errors in parenthesis.

*** [**] (*) denotes statistically significant at 1 [5] (10) percent levels, respectively.

Table 9. Robustness: Test scores

<i>Specification</i>	(1)	(2)	(3)	(4)
Dep. variable		Test scores		
Years		91, 93, 94, 95, 01, 02		
<i>Panel A</i>				
Distance to newspaper outlet *post-campaign periods	-0.444** (.202)	-0.396** (.201)	-0.523*** (.177)	-0.462*** (.179)
Distance to district capital *post-campaign periods	-0.204 (.222)			
Distance to urban centre *post-campaign periods		-0.268 (.223)		
Local council official in SMC*post-campaign periods			1.36*** (0.49)	
Receipt of discretionary support*post-campaign periods				1.78*** (.68)
<i>Panel B</i>				
Distance to district capital *post-campaign periods	-0.437** (.195)			
Distance to urban centre *post-campaign periods		-0.496*** (.189)		
Local council official in SMC*post-campaign periods			1.39*** (.492)	
Receipt of discretionary support*post-campaign periods				2.04*** (.67)
Controls	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes
School effects	Yes	Yes	Yes	Yes
Schools	376	376	376	376
Observations	1824	1824	1824	1824

Notes: Robust standard errors in parenthesis.

*** [**] (*) denotes statistically significant at 1 [5] (10) percent levels, respectively.