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Ritva Reinikka, The World Bank and Centre for the Study of African Economies
Jakob Svensson, IIES, Stockholm University, NHH and CEPR

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Centre for Economic Policy Research
90–98 Goswell Rd, London EC1V 7RR, UK
Tel: (44 20) 7878 2900, Fax: (44 20) 7878 2999
Email: cepr@cepr.org, Website: www.cepr.org

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ABSTRACT

The Returns from Reducing Corruption: Evidence from Education in Uganda*

What is the most effective way to increase primary school enrolment 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 examine this hypothesis by exploiting 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. Combining survey and administrative data, we show that the campaign was successful, and the reduction in capture of funds had a positive effect on enrolment and student learning.

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Ritva Reinikka
Development Research Group
The World Bank
1818 H Street NW
Washington DC 20433
USA
Email: rreinikka@worldbank.org

Jakob Svensson
Professor of Economics
Institute for International Economic
Studies (IIES)
Stockholm University
S-106 91 Stockholm
SWEDEN
Email: jakob.svensson@iies.su.se

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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 provides little guidance on 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), the fact that most 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 — the problem of identifying causal effects.¹ 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 restricted 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 of services (schools) and beneficiaries (students and parents). 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 enrollment and student learning.

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

²We are not aware of any study that explicitly tests 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).

³This does not imply that randomized evaluations cannot provide evidence on governance issues. They can do so directly (by evaluating interventions that focus on strengthening governance) and indirectly (by shedding light on political economy of service delivery in developing countries).

To examine this hypothesis we exploit an unusual policy experiment. Towards the end of 1997, the Ugandan government began to publish data on monthly transfers of capitation grants to districts in the national newspapers and their local language editions. The newspaper campaign came in response to evidence of extensive capture and corruption in the education sector. Specifically, survey evidence from schools combined with detailed disbursement data (public expenditure tracking survey) showed that, on average, only 20% of the funds for primary schools' nonwage expenditures (capitation grant) reached the schools in the mid-1990s (Reinikka and Svensson, 2004). 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. As an anticorruption program, the newspaper campaign was novel. Traditionally, anticorruption programs target corruption in service delivery primarily through capacity building of the legal and financial institutions — judiciary 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. Anchoring anti-corruption reforms in existing legal and financial agencies may be problematic, however, as in many poor countries the legal and financial institutions are weak and among the most corrupt. For this reasons, the Ugandan government decided to take the bottom-up route of enforcement by citizens and began publicizing information on amount and timing of disbursement of school grants.

Using data from a repeat public expenditure tracking survey and administrative data from the Uganda Ministry of Education, we link a school's enrollment and average primary leaving exam scores with survey data on capture and distance to nearest newspaper outlet. A school's exposure to the newspaper campaign is determined both by the timing of the campaign (rolled out at the end of 1997) and schools' (parents') access to newspapers. After controlling for school and time effects, the interaction between distance to a newspaper outlet and the timing of the reform is plausibly an exogenous variable that can be used to assess the impact of reducing corruption on school outcomes.

We find that public access to information can indeed be a powerful deterrent to capture of funds at the local level. 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, but not more knowledgeable in general. These schools also managed to claim a significantly larger part of their entitlement after the newspaper campaign had been initiated. Importantly, these relationships are not present in the data in the periods preceding the campaign.

Instrumenting for the share of funding reaching the school, we find that the reduction in capture had a positive effect on both enrollment (in grades 4-7) and student learning (particularly for girls). Distance to a newspaper outlet, however, is uncorrelated with changes in enrollment and test scores in the periods before the campaign was initiated.

We show that the impact of making more resources available at the schools (through reduced local government capture) is of the same order of magnitude as some of the more cost-effective school interventions that have been evaluated based on a randomized design. However, by focusing on governance of social services and the effects of making untied funds available to schools, this paper differs in important aspects from existing literature. The intervention (newspaper campaign) is also less expensive to implement than most others.

The past few years has seen a small but growing body of research focusing on identifying and quantifying corrupt behavior.⁴ 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.

The next section describes the situation before the newspaper campaign and lay out the key components of the reforms in the late 1990s. Section 3 describes the data used in the empirical analysis and the method used to quantify capture. In section 4 we discuss the empirical strategy used to estimate the effects of reduced capture. Section 5 presents the empirical evidence on enrollment and learning. Section 6 concludes.

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.⁵

⁴For a discussion of this literature, see Svensson (2005). Contributions dealing with public corruption include Di Tella and Schargrotsky (2003), Hsieh and Moretti (2005), Olken (2003, 2007), Reinikka and Svensson (2004a), and Svensson (2003).

⁵The capitation grant is the second largest program in the overall school budget for primary schools, which is dominated by teachers salaries.

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, 2004). 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 in rural Uganda. 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, 2004 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 3). 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 2004).

Where did the money go? As discussed in Reinikka and Svensson (2004), 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 various political activities. Case study evidence of district political financing and corruption in Uganda also points in the same

direction.⁶ The diversion of funds was also facilitated by the ignorance about the capitation grant in most schools.

2.2 The reforms in late 1990s

Two major changes occurred in the primary education sector in Uganda in the late 1990s:

- (i) An information campaign aimed at reducing capture of public funds by providing schools (parents) with information to monitor local officials' handling of capitation funds was initiated;
- (ii) Primary school fees were abolished (the UPE reform).

In the following sub-sections we discuss these reforms in some detail.

2.2.1 Information campaign and voice

The possibility to use the main media outlets, and specifically newspapers, as instruments to strengthen relationships of accountability in the education sector was discussed in policy circles in Uganda in response to the findings of the initial public expenditure tracking survey. In the end of 1997, a decision was taken and the government began to systematically publish data in the national newspapers on the monthly transfers of capitation grants to districts. The main newspapers used were 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, and on occasions also carried stories on misuse of the capitation grant funds.

⁶Thomas (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. 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 main objective of the newspaper campaign was to provide information on the grant program, including when funds were 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 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). 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.

2.2.2 UPE

Universal primary education (UPE) became a cornerstone in President Museveni's election manifesto in early 1996 and many observers argue that it was the salient issues for voters in the 1996 election (see for instance Stasavage, 2003). Museveni won the election and in the fall of 1996 the Government of Uganda announced that, starting in 1997, primary school fees would be abolished. 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. In practise, this rule was not enforced and UPE became synonym to free primary schooling for all (Stasavage 2003).⁷

2.2.3 Predicted impact

In appendix, we set up a simple model to help understand the possible effects on school enrollment and performance of the reforms in the late 1990s. We highlight two mechanisms. First, lowering the cost of schooling increases

⁷There are several reasons for this. Families with fewer than four children could take 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 (Stasavage 2003).

the number of students that will complete a given grade in 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. Alternatively, or complementary, increased funding reduce costs, to the extent that household education expenditures and government non-salary grants to schools are substitutes as suggested by Das et al. 2004. As a result, more students will complete a given grade. The 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 counterbalanced by the direct effect from higher quality to cognitive skills. Which effect dominates is unclear.⁸

3 Data

We have school survey data on outcomes and financing in 1991-95 and 2001, and enrollment and test score data from administrative records for a larger sample of schools and more years (also 1997 and 2002). Summary statistics are reported in tables 1-2.

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 knowledge of the grant program by administering a test to head teachers, 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.⁹ To ensure that the sample had broad regional coverage (Northwest,

⁸As 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.

⁹The 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).

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.¹⁰

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.¹¹ The 2002 survey also formed the basis for a related research project. Therefore, an additional 170 schools from 9 of the original 18 districts were surveyed.¹² We thus have a final sample of 218 schools, for which survey data are available for the years 1991–95 and 2001, and a sample of 388 (218 + 170) schools for which survey data are available in 2001. For all 388 schools, we have administrative data on enrollment and test scores for 1991, 1995, 1997, and 2001.

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 (USh) a year for each student in grades P1–P3 and 4,000 USH for each student in grades P4–P7. In 2001 the amounts were 5,000 USH for grades P1–P3 and 8,100 USH for grades P4–P7.¹³

Records from the Ministry of Finance indicate that the entitlement rule

¹⁰For both surveys (1996 and 2002) 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.

¹¹An 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.

¹²The 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.

¹³The grant has maintained its real value in U.S. dollar terms (the P4 to P7 entitlement for 1995 and 2001 was 4.7 U.S. dollars per student in 2001 prices).

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 year 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 appear to adequately reflect what schools actually received. The data were 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 latter is the basis for the EMIS data). 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. Summary statistics are reported in table 1.

Summary statistics indicate that the situation has improved dramatically since the mid-1990s (table 3). 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.

Enrollment data, for each grade, were collected from the Uganda Ministry of Education's Education Management Information System (EMIS). These data are derived from district records and considered to be of fairly good quality. Test score data were 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.

4 Identification

4.1 Identification assumption

The identification strategy builds on two assumptions. First, prior to 1998 — the year the government began to systematically publish data on disbursement — schools’ knowledge about the grant program was largely a function of own effort and ability. Second, schools/communities closer to a newspaper outlet will be more exposed to information disseminated through newspapers. While both distance and timing (before and after 1998) may be correlated with other important variables, the key identification assumption we make is that the combination of the two is not. We assess the validity of this exclusion restriction next.

4.2 Instrument validity

The IV method we employ, i.e. using *distance to nearest newspaper outlet after 1998* as instrument, makes the counterfactual assumption that absent the newspaper campaign, school funding and outcomes (enrollment and learning) 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 outcomes *prior* to the information campaign. Thus we estimate the following difference-in-differences specification

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

where y_{jt} is outcome (share of funding reaching the school, enrollment, and average test scores) in school j at time t , σ_t is a binary variable taking the

value 0 in period 1991 and 1 in period 1995, $distance_j$ is the distance (log) in kilometers to the nearest newspaper outlet¹⁴, and β_3 is the reduced-form estimate.

A slightly more general specification allows for school-specific fixed effects, μ_j :

$$y_{jt} = \beta_0 x_{jt} + \beta_2 \sigma_{1995} + \beta_3 \sigma_t distance_j + \mu_j + \varepsilon_{jt} , \quad (2)$$

The results of estimating (1) and (2) are reported in table 4. Proximity to a newspaper kiosk is uncorrelated with changes in capture over the 5-year period prior to the reforms (column 1-2). Distance is also uncorrelated with changes in enrollment — both grade 7 enrollment (columns 3-4) and total enrollment (columns 5-6) — and average primary leaving exams of grade 7 students (columns 7-8) in the pre-campaign period.¹⁵ Thus, there are no differential trends across treatment (i.e. schools close to a newspaper outlet) and control groups in the pre-campaign (and pre-UPE) period.

While these falsification tests provide support for the main identification assumption, a concern is that the effects of the newspaper campaign may be confounded by the UPE reform in 1997. That is, even if there were no differential trends in the period up to 1995, the UPE reform may differentially affect schools closer to newspaper outlets. There are a variety of possible channels. For instance, parents living closer to a newspaper outlet may be more likely to know about free primary education and thus to enroll their children, although since UPE was one of the salient issues for voters in the 1996 election it is reasonable to assume most voters - close or not to a newspaper kiosk - knew about it. Alternatively, if newspapers are available in more urban areas, the marginal students (and size of the potential pool of students) affected by UPE may differ by areas (close or not to a newspaper kiosk). As newspaper outlets are not randomly placed in a given area, this is in general a concern when using distance as an instrument. However, in our sample of rural schools this is likely to be less of a problem. By construction, we do not have a mixed sample of urban schools (close to newspaper outlets) and rural schools (far from a newspaper outlet).

We cannot directly test for these alternative channels using the 1995-2001 comparison. However, as UPE was implemented one year ahead of the newspaper campaign, we can study the effects on enrollment and performance

¹⁴During piloting we compared head teachers' assessment of distance to the nearest newspaper outlet with measured (by enumerators) distance. We concluded that in most cases the two measures were similar.

¹⁵Enrollment numbers are collected from administrative records. The qualitative results remain intact when using survey data on enrollment (we then have data for less schools). As with grade 7 enrollment and total enrollment (columns 3-6, table 4), we cannot reject the hypothesis that $\beta_3 = 0$ when enrollment in grades 1-6 are used as dependent variables.

over the 1995-1997 period. By comparing outcomes in 1995 with 1997 we would capture the (initial) effect of the UPE reform. Observed changes between 1995 and 1997 cannot be attributed to the newspaper campaign. The results of estimating (2), with σ_t taking the value 0 in 1995 and 1 in 1997 are reported in table 5. We look at eight different outcomes: enrollment in grades 1-7 and test score (primary leaving exams). Note that because we do not have survey data for 1997, we cannot examine changes in capture.

Consistent with the official view that the UPE reform was a success, enrollment increased substantially for all grades (more than doubled in grade 1 for example). However, the increase is not significantly related to distance. The change in average test scores is also uncorrelated with the interaction term $\sigma_{97}distance_j$. Hence, in the sample of predominately rural school considered here, there is no evidence that the effects of the UPE reform (one year into the reform) is correlated with distance to nearest newspaper outlet, providing additional support for the key identification assumption.

To constitute a valid instrument, *distance* must also be correlated with degree of exposure. *Distance* is highly correlated with 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 (results available upon request).¹⁶

A more direct measure of knowledge is exploited in table 6. We use a simple knowledge test administered to head teachers to measure knowledge of the grant program in 2001. We can then estimate:

$$q_j = \pi_0 x'_j + \pi_1 distance_j + \varepsilon_j . \quad (3)$$

where q_j is the score on the knowledge tests.

Head teachers serving in schools close to a newspaper outlet are better informed about the formula used for deriving the capitation grant (table 6, 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).

¹⁶Following Reinikka and Svensson (2004) 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.

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 4 suggests that these potentially more knowledgeable head teachers did not manage to claim more funds prior to the newspaper campaign.

To test if proximity to a newspaper outlet is correlated with head teachers' general ability, we compare the head teachers' knowledge in other areas. The results are reported in columns 4-6. 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 table 6).¹⁷ But for head teachers' knowledge of local affairs¹⁸ 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 one (see specifications 5 and 6).¹⁹

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

¹⁷Respondents 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.

¹⁸Respondents 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.

¹⁹Respondents 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.

distance that accounts for the observed effects rather than some unobserved characteristic such as teachers' ability.²⁰ Three additional tests support this claim (results available upon request): The relationship between knowledge about the grant program and proximity to a newspaper outlet remains intact when *income* is added as an explanatory variable. 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 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 (see footnote 22).

Table 7 depicts the first stage of a two-stage procedure to estimate the impact of improved access to public information as a tool to reduce diversion of grant funds on school outcomes. We estimate (1) and (2) with σ_t taking the value 1 in the post-campaign year 2001 and 0 in the pre-campaign year 1995. Schools that are more exposed to the newspaper campaign; i.e., closer to a newspaper outlet, experience a significantly larger reduction in district government diversion of funds after the campaign starts. A one standard deviation change in *distance* is associated with a 0.21 standard deviation reduction in capture. As the effect is non-linear (*distance* is the logarithm of distance in kilometers to the nearest newspaper outlet), a change in distance has a larger effect the closer the school is to a newspaper outlet (see figure 1). As an example, a school close to a newspaper outlet suffers 25 percentage points less from capture as compared to a school one standard-deviation (in kilometers) further away from a newspaper outlet.

4.3 Specification

With a valid instrument of exposure, we can study the main question of the paper: Did the reduction in capture have an impact on school outcomes? To this end we estimate:

²⁰Although 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.

$$y_{jt} = \gamma_0 \mathbf{x}_{jt} + \gamma_1 s_{jt}^{INS} + \sum_t \alpha_t \delta_t + \mu_j + \theta_{jt} \text{ for } t = \{95, 97, 01, 02\}, \quad (4)$$

where δ_t is a time dummy, s_{jt}^{INS} is the fitted value based on the first stage regression (Table 7, equation 2), using the interaction term $\sigma_{01,02} distance_j$ as instrument, and including the same vector of controls x as covariates, μ_j is a school-specific fixed effect and θ_{jt} is an iid error term. The disturbance term is adjusted to allow for correlations within schools. We look at two outcomes (y_{jt}): average primary leaving exam score and average school enrollment.

The reduced form is:

$$y_{jt} = \omega_0 \mathbf{x}_{jt} + \omega_1 distance_j \sigma_{01,02} + \sum_t \alpha_t \delta_t + \mu_j + \varepsilon_{jt}, \quad (5)$$

where $\sigma_{01,02}$ is a binary variable taking the value 1 in the post-campaign years 2001 and 2002 and 0 otherwise (in 1995 and 1997).

5 Results

5.1 Enrollment

We start by looking at the reduced-form relationship between distance to the nearest newspaper outlet and school enrollment; i.e. the estimates of (5). As reported in table 8, panel A, there is no robust relationship between distance in the post-campaign period and enrollment in grades 1-3. The estimated coefficients are also small (for example, the Beta coefficient on $distance_j \sigma_{01,02}$ is -0.038 in column 1). For grades 4-7, however, there is a significant relationship and the effect increases with grades.²¹ A one standard

²¹The results in table 8 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 (of course this would still imply that reduced capture had an impact). In reality there are reasons to believe that this problem is not so severe. The 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 section suggests that the exposure to the campaign may be similar (since the distance to a newspaper outlet will be similar). As an additional robustness check distance to the closest school the students could attend was added as explanatory variable. However, distance to the closest school the students could attend enters insignificantly and does not qualitatively affect the results reported in table 8.

deviation reduction in *distance* is associated with a 0.16 standard deviation increase in grade 7 students.

IV estimates are reported in panel B. They mirror the reduced-form estimates.²²

The finding that the enrollment effects are small (and insignificant) for lower grades but significant (and larger) for higher grades is consistent with the simple model in appendix 1. Even with free primary education, the marginal cost of schooling is likely to increase by age. As the child becomes older, his/her contribution to the household's income is likely to become more important and thus the child's alternative cost of education increases. Therefore changes in the quality of education (or the cost to the extent that household education expenditures and government non-salary grants to schools are substitutes) are likely to have a larger marginal effect for children in higher grades.

The estimates reported in table 8 are also consistent with the aggregate evidence on school enrollment. Since the removal of school-fees most children in Uganda start primary school, although not always in time. The net [gross] intake rate was 75% [170 %] in 2003 (Republic of Uganda, 2006).²³ However, there are high dropout rates in grades 4-7 and only a fraction of the students that starts primary school completes it. The official survival rate to grade 5 (percentage of a pupil cohort actually reaching a grade), for example, stands at 52% in 2003 (Republic of Uganda, 2006). In our sample of rural schools the estimated survival rate to grade 5 is 29% in 2001. Thus, there is a relatively small window of opportunity to increase enrollment in lower grades, but a much larger potential in higher grades.

A back-of-the-envelope calculation suggests that the impact of making more resources available at the schools (through reduced local government capture) is of the same order of magnitude as some of the more cost-effective school interventions that have been evaluated based on randomized design. To see this note that up to grade 3 there is no statistically significant difference in outcomes. However, more students complete grade 4 in schools closer to a newspaper outlet in the post-campaign periods. In the average school (in 2001), 125 students completed grade 4 and since the school received approximately 80% of its entitlement, the total amount of non-salary inputs for the grade 4 cohort was US\$470 (US\$3.76 per student). If the school had

²²The enrollment results for boys and girls separately are qualitatively similar to those reported in Table 6.

²³Net intake rate is new entrants to primary grade 1 who are of the official primary school entrance age as percentage of the population of official entry age. Gross intake rate is new entrants in primary grade 1 regardless of age as a percentage of the population of official entry age.

received its full entitlement, it would have received almost one dollar more per student and the impact estimate (table 8, panel B, column 4), imply that 148 students would then have completed grade 4. Fully funded, the grade 4 cohort would in total have received US\$704 (US\$4.70 per student). The cost per additional student completing grade 4 is $(148-125)/(US\$704-US\$470) = US\$10.2$. Assuming that the additional funding became available when the newspaper campaign was rolled out in 1998, the cohort of grade 4 students in 2001 would have benefited from reduced capture for four years. Taking this into account, the cost per additional student completing grade 4 rises to US\$31.6.

Kremer, Miguel, and Thornton (2004) compare the cost effectiveness of various ways to spend money to keep children in school based on randomized evaluations. Some of these interventions had no effect on school participation, implying infinite cost per participation gain, but others did.²⁴ Deworming, at an average cost of only US\$3.50 per additional year of school participation (Miguel and Kremer, 2004) was found to be the most cost effective intervention of the projects studied. The costs per additional year of school participation of a merit scholarship and child sponsorship program were US\$90 and US\$99, respectively. Thus, while the estimate of US\$10.2-US\$31.6 per additional student completing grade 4 is higher than the cost estimate for the deworming project, compared to other interventions that have been credibly evaluated, the results in table 8 suggest that schools used the additional funding that became available through reduced local government capture fairly effectively.

5.2 Test scores

Table 9 presents evidence on the impact of the newspaper campaign on cognitive skills. We estimate (4)-(5) with the average primary leaving exam score for school j at time t as dependent variable.

The reduced-form estimates are reported in panel A and the IV estimates in panel B. The first column depicts the average score, while columns 2 and 3 report the estimates for boys and girls separately. Students in schools closer to a newspaper outlet in the post-campaign period have higher scores, but the effect is insignificant at standard levels of significance (p-value = 0.18). The average score, however, masks the difference between girls and boys. There is a significant relationship between reduced capture and average test

²⁴By the same logic, there is an infinite cost per participation gain for grades 1-3 of making more resources available.

scores for girls. The effect is also economically important. A one standard deviation reduction in *distance* leads to a 0.09 standard deviation increase in average test scores. The estimated effect for boys, on the other hand, is small and insignificant.²⁵

A comparison with recent randomized evaluations again suggests that, at least for girls, funds have been put in productive use.²⁶ Comparing learning outcomes in a school (in 2001) that received 80% of its entitlement with that of a school that received its full entitlement, average test scores of girls would be 1.4 points higher. Assuming the students only benefited from reduced capture in 2001, this yields a cost per 0.1 standard deviations gain of girls of US\$0.84. Assuming the students benefitted from reduced local government diversion beginning with the roll out of the newspaper campaign, the cost per 0.1 standard deviations gain is US\$3.38. As a reference point, the costs of the programs considered by Kremer, Miguel and Thornton (2004) range from US\$1.77-US\$4.01. The remedial education program in India, evaluated in Banerjee et al (2005), costs US\$0.67 per 0.1 standard deviations gain.

5.3 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 approach. First, it is possible that distance captures some unobserved variable (apart from UPE) 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.²⁷ The problem arises if instead

²⁵Differences in marginal costs and gain of schooling may shed light on the findings on achievement. Bjorkman (2006b), using exogenous rainfall shocks to proxy for income changes, finds that households in Uganda respond to income shocks by varying the quantity and quality of girls' education while boys are to a large extent sheltered. She also finds that income shocks have larger marginal effects on test scores of girls in the post UPE period. Complementary evidence on test scores is provided in Bjorkman (2006a). Bjorkman looks at differences across districts in average test scores conditional on district differences in newspaper circulation.

²⁶It should be noted that this improvement in the quality of education (or for boys at least no worsening of quality) comes about although average school size (table 8) increased.

²⁷For 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 campaign, $E[s_j | t = 2001, \text{short distance}]$ and $E[s_j | t = 1996, \text{short distance}]$ 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

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 4-6. 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 more funding. Note that the results of the control experiments suggest that the value of political 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 money. And if this effect becomes more important in the late 1990s, this may also bias our findings.

To test for these alternative hypotheses, we augmented equation (5) 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 binary variable $\sigma_{01,02}$. We also experimented with two measures of political connectedness, a binary 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, using grade 7 enrollment as dependent variable, are reported in table 10.²⁸

Once controlling for proximity to a newspaper outlet, none of interaction terms enter significantly. The measure of a school’s exposure to the information campaign ($distance_j\sigma_{01,02}$) remains significant, albeit somewhat less precisely estimated.²⁹

probability of claiming grants would result in a downward bias in the estimate of interest.

²⁸The results for the other dependent variables used in table 8 are qualitatively the same as those reported in table 10 (full results available upon request).

²⁹This is to be expected since newspapers are likely to be available in both district

6 Discussion and Conclusion

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 exploit an unusual policy experiment to examine this hypothesis: 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 (in grades 4-7) and achievement (for girls) increased in schools that, as a result of the information campaign, managed to claim a higher share of their entitlements. A number of specification checks support the causal interpretation of these estimates.

Capitation grant (to cover primary schools' nonwage expenditures) is a fairly common spending program in developing countries. In this respect, the results in the paper have some external validity. However, they need not generalize to other contexts. First, parents have for a long time been organized and played a role in the management and control of primary schools in Uganda. This is not the case in other sectors. Second, the UPE reform, and the political campaign surrounding it, may have created a context particularly favorable to the campaign's success. Survey data also suggest that most households prioritize education over other social services (Stasavage 2003).

The impact of making more resources available at the schools (through reduced local government capture) is of the same order of magnitude as some of the more cost-effective school interventions that have been evaluated based on randomized design. The policy experiment we study, however, differ in two important aspects from most of the existing literature. First, the paper focuses on governance of social services, rather than the impact of specific school inputs. Second, we study the effects of making untied funds available. Schools were free to spend the additional resources that became available as a result of the campaign 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).³⁰ It should be noted, though, that the comparison of cost estimates is not straightforward since the intervention we exploit did not

capitals and urban centres. The simple correlation between distance to a newspaper outlet and distance to district headquarters [distance to the nearest bank branch] is 0.51 [0.48]. Distance to the nearest newspaper outlet is orthogonal to the two measures of political connectedness.

³⁰Yet another difference is that we look at the compounded effect over a few years.

result in additional spending, but that a higher share of a given amount spent reached schools. A full cost-effectiveness analysis needs to include the cost of the newspaper campaign and how the money that previously had been captured had been utilized. To the extent capture and corruption diverted scarce human and other resources away from productive uses (as suggested in Reinikka and Svensson, 2004), it is possible that "social cost effectiveness" estimates would be markedly lower.

Publicity cannot solve all the problems of corruption and diversion of 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.³¹

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 $q()$ and $h()$ 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.³²

³¹One 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).

³²Introducing 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).

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

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

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.

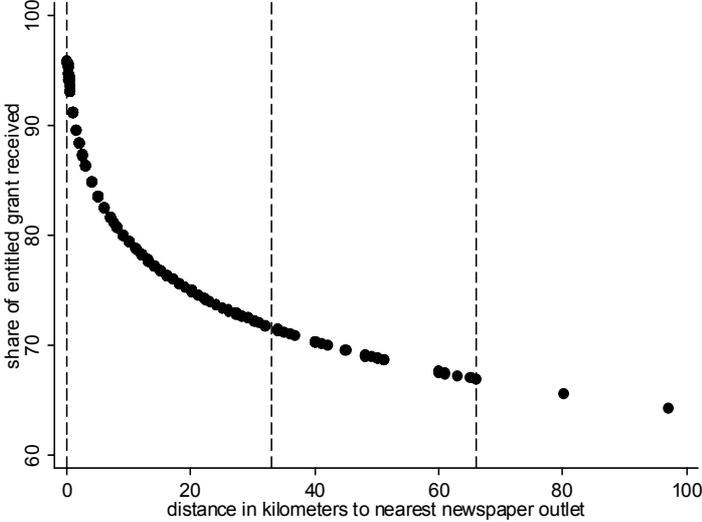
Thus we have two effects of the reforms in the late 1990s. Both reforms will tend to increase enrollment. The information campaign, to the extent it results in higher spending reaching the school, may also result in an increase in average cognitive skills. It should be noted that the model is highly 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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Figure 1. Estimated impact (table 7, column 2) of distance on (1-capture)



Notes: Distance between dotted lines is 1 STD

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 1996 and 2002 survey; see text for details.

Table 2. Summary Statistics on School enrollment and test scores, 1995, 1997, 2001 and 2002.

	Median	Mean	Standard deviation
<i>1995</i>			
School size (number of students), P1	125	156	108
School size (number of students), P2	84	102	71
School size (number of students), P3	79	80	70
School size (number of students), P4	66	81	60
School size (number of students), P5	56	68	52
School size (number of students), P6	45	55	47
School size (number of students), P7	34	42	37
Average primary leaving exam score	8.7	9.8	5.8
<i>1997</i>			
School size (number of students), P1	373	430	272
School size (number of students), P2	144	171	111
School size (number of students), P3	123	147	99
School size (number of students), P4	93	111	77
School size (number of students), P5	73	87	63
School size (number of students), P6	54	66	54
School size (number of students), P7	37	46	38
Average primary leaving exam score	10.7	11.1	5.3
<i>2001</i>			
School size (number of students), P1	176	207	172
School size (number of students), P2	117	142	107
School size (number of students), P3	119	140	98
School size (number of students), P4	110	126	82
School size (number of students), P5	89	101	64
School size (number of students), P6	65	75	47
School size (number of students), P7	45	51	32
Average primary leaving exam score	9.7	10.4	5.2
<i>2002</i>			
School size (number of students), P1	177	212	159
School size (number of students), P2	118	138	95
School size (number of students), P3	115	136	87
School size (number of students), P4	108	124	77
School size (number of students), P5	92	106	64
School size (number of students), P6	71	80	49
School size (number of students), P7	44	52	32
Average primary leaving exam score	10	10.6	5.3

Source: Ministry of Education (Education Management Information System/EMIS); see text for details.

Table 3. Summary Information on Capitation Grants Received as Share of Entitled Grants (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 1996 and 2002 surveys and Ministry of Education; see text for details.

Table 4. Instrument validity: Control experiment 1991-1995

<i>Specification</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. variable	Share of grants received	Share of grants received	Grade 7 enrollment	Grade 7 enrollment	Total enrollment	Total enrollment	Primary leaving exam score	Primary leaving exam score
Distance to newspaper outlet*1995	0.48 (.81)	-0.02 (3.94)	1.90 (1.25)	-2.61 (2.04)	-16.2 (12.0)	-8.94 (19.7)	0.14 (.24)	0.15 (.35)
Distance to newspaper outlet	1.90 (2.04)		-4.00 (3.38)		-62.6 (52.6)		-1.12 (.87)	
1991	7.65 ^{***} (2.04)		33.8 ^{***} (8.89)		496 ^{***} (81)		9.70 ^{***} (.89)	
1995	25.9 ^{***} (6.31)	25.4 ^{***} (11.0)	31.1 ^{***} (10.1)	2.83 (5.75)	525 ^{***} (102)	29.3 ^{***} (48.9)	9.12 ^{***} (1.00)	-0.17 (.77)
School fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	334	334	566	566	566	566	566	566
Schools	204	204	308	308	308	308	308	308

Notes: Income is included as control. Robust standard errors clustered by school in parenthesis. *** [**] (*) denote statistically significant at 1 [5] (10) percent levels, respectively.

Table 5. Instrument validity: Control experiment 1995-1997

<i>Specification</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. variable	Grade 1 enrollment	Grade 2 enrollment	Grade 3 enrollment	Grade 4 enrollment	Grade 5 enrollment	Grade 6 enrollment	Grade 7 enrollment	Primary leaving exam score
Distance to newspaper outlet*1997	-27.1 (17.1)	-8.91 (6.38)	-7.33 (5.50)	-4.43 (4.11)	-2.43 (3.15)	-1.21 (2.43)	-0.46 (1.63)	0.15 (.35)
1997	336 ^{***} (48.4)	83.3 ^{***} (19.8)	59.5 ^{***} (17.0)	34.4 ^{***} (12.9)	20.5 ^{**} (9.8)	10.8 (7.4)	2.6 (5.6)	-0.17 (.77)
Percentage increase	176	68	50	37	28	22	11	12
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	570	570	570	570	570	570	597	570
Schools	301	301	301	301	301	301	315	301

Notes: Income is included as control. Robust standard errors clustered by school in parenthesis. *** [**] (*) denote statistically significant at 1 [5] (10) percent levels, respectively. Percentage increase is the percentage increase in unconditional averages between 1995 and 1997.

Table 6. Distance and exposure

<i>Specification</i>	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable	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 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

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

** 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.

Table 7. First-stage 1995-2001

<i>Specification</i>	(1)	(2)
Dep. variable	Share of grants received	Share of grants received
Distance to newspaper outlet*2001	-5.30** (2.26)	-6.87** (3.42)
Distance to newspaper outlet	2.76 (2.15)	
1995	28.3*** (6.2)	
2001	97.1*** (5.8)	74.7*** (11.3)
School fixed effects	No	Yes
Observations	417	417
Schools	218	218

Notes: Income is included as control. Robust standard errors clustered by school in parenthesis. *** [**] (*) denote statistically significant at 1 [5] (10) percent levels, respectively.

Table 8. Estimates of the effects of the newspaper campaign on enrollment

<i>Specification</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. variable	Grade 1 enrollment	Grade 2 enrollment	Grade 3 enrollment	Grade 4 enrollment	Grade 5 enrollment	Grade 6 enrollment	Grade 7 enrollment
PANEL A							
Distance* $\sigma_{01,02}$	-5.95 (9.21)	-7.18 (5.05)	-7.83 (4.79)	-8.01** (4.02)	-7.39** (3.16)	-5.94** (2.33)	-4.60*** (1.64)
PANEL B							
Share of funding reaching school	0.87 (1.34)	1.06 (0.73)	1.14 (.70)	1.16** (.58)	1.08** (.46)	0.86** (.34)	0.70*** (.24)
Time effects	Yes						
School effects	Yes						
Controls	Yes						
Observations	1253	1253	1253	1253	1253	1253	1280
Schools	364	364	364	364	364	364	364

Notes: (i) Robust standard errors clustered by school in parenthesis. *** [**] (*) denotes statistically significant at 1 [5] (10) percent levels, respectively. Income is included as control.

Table 9. Estimates of the effects of the newspaper campaign on test scores

<i>Specification</i>	(1)	(2)	(3)
Dep. variable	Average primary leaving exam score		
	Total	Boys	Girls
PANEL A			
Distance* $\sigma_{01,02}$	-0.36 (.27)	-0.19 (.27)	-0.49* (.29)
PANEL B			
Share of funding reaching school	0.05 (.04)	0.03 (0.04)	0.07* (.04)
Time effects	Yes	Yes	Yes
School effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	1319	1305	1305
Schools	374	371	373

Notes: (i) Robust standard errors clustered by school in parenthesis. *** [**] (*) denotes statistically significant at 1 [5] (10) percent levels, respectively. Income is included as control..

Table 10. Robustness

<i>Specification</i>	(1)	(2)	(3)	(4)
Dep. variable		Grade 7 enrollment		
<i>Panel A</i>				
Distance to newspaper outlet* $\sigma_{01,02}$	-4.42** (1.95)	-4.29** (1.93)	-4.21*** (1.54)	-4.59*** (1.54)
Distance to district capital* $\sigma_{01,02}$	-0.36 (2.25)			
Distance to urban centre* $\sigma_{01,02}$		-0.70 (2.20)		
Local council official in SMC* $\sigma_{01,02}$			0.70 (3.82)	
Receipt of discretionary support* $\sigma_{01,02}$				11.7 (8.04)
Controls	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes
School effects	Yes	Yes	Yes	Yes
Schools	364	364	364	364
Observations	1280	1280	1280	1280

Notes: Robust standard errors clustered by school in parenthesis. *** [**] (*) denotes statistically significant at 1 [5] (10) percent levels, respectively.