# The Power of Information*

## Evidence from a campaign to reduce capture

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**Abstract**

This paper evaluates an unusual policy experiment: an information campaign in Uganda aimed at reducing capture by empowering schools (parents) to monitor local officials' handling of a large school-grant program. The situation has improved dramatically since the information campaign started. Local capture has been reduced from 80 percent on average in 1995 (i.e., for every dollar spent by the government the schools received 20 cents) to 20 percent in 2001. Exploiting the difference in access to newspapers across schools, we show that the more extensively treated schools (i.e., schools with newspapers) suffered 14 percentage points less in 2001. Test score data from head teachers suggest that newspaper access rather than ability accounts for this effect. Finally, we show that if district officials cannot distinguish between informed and uninformed schools and/or if information creates positive externalities for the control group, then comparing newspaper-schools with schools lacking them will severely underestimate the impact of the newspaper campaign. Taking the group effects into account, we find that the newspaper campaign can explain nearly 75 percent of the reduction in local capture since 1995.

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# 1 Introduction

A common thread in most anticorruption programs is the reliance on legal and financial institutions - judiciary, police, financial auditors - for better policies of accountability. However, in many poor countries, these institutions are weak and among the most corrupt. Not surprisingly, there are few recent examples of successful efforts to combat corruption and capture in public programs.[1]

A complementary approach takes the expected users of public services as a starting point. Rather than increasing accountability from above, the idea is to empower citizens at the bottom of the public service delivery chain by providing them with information on the workings of public programs from which they are suppose to benefit. This way, citizens will be empowered to demand certain standards and monitor and challenge the abuse of institutions, systems, and officials with whom they interact in their daily life. Dissemination of information plays a crucial role in this bottom-up strategy. However, although "information", "knowledge" and "empowerment" have become buzzwords in the current policy debate on development, there is little quantitative evidence on the impact of these policy measures (Banerjee and He, 2003). This paper attempts to reduce this gap.

We evaluate an unusual policy experiment: an information campaign in Uganda aimed at reducing capture by empowering schools (parents) to monitor local officials' handling of a large school-grant program.

In the mid 1990s, a survey in Uganda to gauge the extent to which a grant intended for primary schools actually filtered down to the intended end-user

[1]The legal approach is more severe in developing countries, since legal processes and systems to control public sector mismanagement rely on the existence of a trustfull (benevolent) legal machine (judges, court personell, police, etc.) that can investigate and enforce existing rules. For many countries, in particular those characterized by systemic corruption, such a legal machine does not exist. The financial management approach suffers from the same dependence on a credible institutional framework. An efficient financial system relies on a functioning enforcement mechanism and the ability to delegate the reviews to trustfull auditors. In addition, while well-functioning legal and financial systems minimize obvious cases of mismanagement (such as theft of public funds), rules and accounting systems only partially constrain the discretionary power of public sector managers and employees. The complexity of the tasks performed by a typical public sector unit and its informational advantage relative to customers effectively hinder the design of legal and accounting measures to address all types of inefficiencies. Thus, less obvious measures of mismanagement (such as shirking, budget prioritization favoring personnel instead of customers, certain procurement procedures, political considerations, etc.) will typically not be captured. Finally, auditing reports and legal procedures are often difficult to interpret for a layman, and therefore often go unnoticed unless the agent who commissioned the audit acts on them.

revealed that for every dollar spent by the central government, the schools only received about 20 cents on average (Reinikka and Svensson, 2002). Case study evidence and other survey data showed that the school funds were not going to other prioritized (by the central government) sectors either. The disbursements were rarely audited or monitored, and most schools and parents had little or no information about their entitlements to the grants. Most of the funds were used for purposes unrelated to education (e.g., to fund the local political machinery) or for private gain.

As evidence of the degree of capture became public, the central government enacted a series of policy changes. It began to publish data on monthly transfers of capitation grants to districts in newspapers and broadcast them on the radio. Furthermore, it required primary schools and district administration headquarters to post notices on all inflows of funds. This promoted accountability by giving schools access to the information needed to understand and monitor the workings of the program. We use a repeat survey to assess the impact of this information campaign.

The raw data suggests a large improvement. In 2001, schools on average received 80 percent of their entitlements. On average, capture has been reduced by 57.9 percentage points since 1995.

We test for the impact of the information campaign using three different approaches. First, we exploit the fact that we have data on the same schools both pre and post treatment (i.e., the information campaign). Controlling for a set of other potential explanatory variables, we find that the (policy) changes that have occurred since 1995, including the information campaign, can account for the significant reduction in local capture.

Second, we exploit differences in means to acquire information, or more precisely access to newspapers. A key component in the information campaign has been the publishing of data on monthly transfers of public funds to the local governments in newspapers. Thus, schools with access to newspapers have been more extensively exposed to the information campaign. The differences-in-differences estimates show that while the degree of capture was similar in the groups with and without access to newspapers in mid 1990s, the more extensively treated schools suffered 14 percentage points less of local capture in 2001.

Data from a simple knowledge test of head teachers provide additional support for the hypothesis that the newspaper campaign has been effective. We find that head teachers with access to newspapers on average have better knowledge about the rules of the grant program and the timing of releases of funds from the center. They are also more knowledgeable about news events currently reported in the media. However, on a test of local and general knowledge, head teachers with access to newspapers score as well as

those lacking access, suggesting that it is information on the grant program (disseminated through newspapers), rather than some unobserved characteristic (e.g., ability) correlated with newspaper access, that accounts for the observed effects.

We also study possible group effects. We set up a simple model and show that if district officials cannot distinguish between informed and less informed schools, then comparing schools with and without newspapers will severely underestimate the impact of the newspaper campaign. This effect is reinforced if newspaper access creates externality benefits by raising the knowledge of the grant program of untreated schools (no newspaper). Taking these group effects into account, we find that the newspaper campaign can explain up to 75 percent of the reduction in local capture since 1995.

This paper links to a broad range of areas. In the public economics literature, it is traditionally (and implicitly) assumed that allocated funds reach the intended beneficiary. Consequently, empirical research on developing countries has focused on the allocation of funds and demand for public services. This paper instead focuses on the supply system, in line with the work of Bardhan and Mookherjee (1999, 2000).

There is a small but growing literature on the role of mass media in shaping public policy. Strömberg (2001, 2003) considers how the press influences redistributive programs in a model of electoral policies. The role of the media is to raise voter awareness and thereby increase the sensitivity of turnout to favors granted. Besley and Burgess (2002) focus on the media's role in increasing political accountability, also in a model of electoral policies. In contrast, our focus is on how the execution of already determined policies (a capitation grant) is affected by having more informed citizens. Empirically, we use micro data rather than disaggregated national accounts data.[2]

While our earlier study (Reinikka and Svensson, 2002) explored the extent to which diverted funds were used for private gain (by local government officials), among other things, providing one of the first quantitative attempts to systematically measure capture and corruption in basic service delivery systems, we here focus on the role of information and voice in combating corruption. The causes and consequences of corruption, to date, constitute an area that has been studied using cross-country data almost exclusively.[3]

[2]For further references, see Strömberg (2001, 2003).

[3]For effects of corruption on investment and growth, see Mauro (1995). On the determinants of corruption, see Ades and Di Tella (1997, 1999), Persson, Tabellini, and Trebbi (2003), Svensson (2000), and Treisman (2000). A common theme in this literature is the use of subjective measures of corruption in a cross-country setting. Svensson (2003) and Di Tella and Schargrodsky (2003) are exceptions. They use quantitative micro-level data on corruption. Hellman et al. [2000a,b] also use firm-level data. The data is numerical but

The rest of the paper is organized as follows. Section 2 describes the pre-campaign situation and discusses the findings from the earlier study on capture. Section 3 lays out the key components of the information campaign and provide a simple model to guide the empirical work. Section 4 describes the survey data used in the empirical analysis and the method used to quantify (local) capture. Section 5 presents the empirical evidence and section 6 concludes.

# 2 Pre-campaign

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

These observations formed the basis for designing a new survey tool - a public expenditure tracking survey (PETS) - to gauge the extent to which public resources actually filtered down to the intended facilities.[4]

A survey of 250 government primary schools was implemented in 1996, covering the period 1991-95 (see the appendix for details on survey design). At the time of the survey, about 8,500 government primary schools were supposed to receive a large proportion of their funding from the central government via local government administrations.

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

Focusing on the capitation grant program had two advantages. First, like most public programs at that time, the capitation grant was a national program in which local (district) offices were used as distribution channels. This gave local officials and politicians the opportunity to capture the funds.

ordinal (based on multi-category responses to questions on corruption). In line with the cross-country literature, they explain corruption as a function of the political-institutional environment.

[4]For a conceptual discussion on Quantitative Service Delivery Surveys (QSDS) and references to ongoing survey work, see Dehn, Reinikka, and Svensson (2003) and Reinikka and Svensson (2003a).

Second, unlike other government programs, the capitation grant was a rare liquid money infusion into a local administrative and political system, which made is easier to capture the funds. Other public programs were primarily in-kind (for instance, health clinics were provided with drug kits).

Data were collected from the central ministries, the local government (districts), and schools. Detailed records were available at both the central level and at schools. At the district level, information on receipt from the central government was available, but the districts lacked reliable information on disbursements to individual schools.

There are several pieces of information suggesting that the capitation grant data at the school level adequately reflected what the schools received. First, the data were collected directly from the school records. These records were kept for the schools' own needs and were often of decent quality.[5] The school records were not submitted to any district or central authorities and did not constitute the basis for current or future funding. Thus, there were no obvious incentives to misrecord the data. At the same time, parents contributing the main part of school income demanded financial information and accountability from the school (or PTA), so school records were usually well-kept. Most of the public resources received were in-kind (textbooks, stationery, chalk, paper) and information on all these inputs was collected and subsequently valued (using market prices). The concern that headmasters might have underreported the school income in order to extract resources for themselves was allayed after interviews during the survey work, which did not support this claim. This is not surprising since the PTA was typically the principal decision-maker (and responsible for raising most of the income) at the school.

Records at the central ministries also revealed that the capitation grants were fully released by the central government on a monthly basis. In the Ugandan treasury system, central ministries or other agents or individuals were unlikely to be able to capture central releases, since they were subject to relatively elaborate pre-audit procedures. In addition, since the capitation grant program was given a priority program status as part of the World Bank structural adjustment programs, the releases from the center were also externally audited (by World Bank staff). Most importantly, records at the district level confirmed that the amounts disbursed were actually received by

[5] As discussed in Reinikka and Svensson (2003b), 5 schools of the 250 schools surveyed lacked any records and thus had to be dropped from the sample. Another 56 schools had missing records for at least one year in the survey period. As a group, these schools do no differ significantly in observable school characteristics, such as income or school quality (measured by the share of unqualified teachers), although there is some evidence that smaller schools are less likely to have records for the whole survey period.

the districts.

The results of the pre-campaign survey were striking (see table 1). In the mid 1990s, only 24 percent of the total capitation grant from the central government reached the schools.[6] Thus, for every dollar spent on nonwage education items by the central government, roughly 80 cents were captured by local government officials (and politicians). In fact, a majority of schools received nothing. As shown in Reinikka and Svensson (2002, 2003), poor students suffered disproportionately due to local capture, because schools catering to them received even less than others.

Where did the money end-up? Although there is anecdotal evidence that part of the leakage was indeed corruption (theft), as indicated by numerous newspaper articles about indictments of district education officers after the survey findings went public, case-study evidence suggests that funds were largely used to finance the local political machinery (Thomas, 1998, 1999). There was no evidence of increased spending in other sectors (Jeppson, 2001; McPake, et al., 1999). It is important to note that most schools (teachers and parents) did not know that they were entitled to capitation grants, which obviously facilitated the discretionary use of the grant in favor of local officials and politicians.

Another striking pattern in the pre-campaign data is that although there is some variation in capture across regions, the bulk of the variation is within regions. A variance decomposition shows that 98 % of the variation is due to variation within regions, while only 2 % is due to variation across regions.

# 3 Information campaign

As evidence of the extent of capture became public, the government reacted swiftly to reform the system. It began to publish data on monthly transfers of capitation grants to districts in newspapers and broadcast them on the radio. It required primary schools and district administration headquarters to post notices on all inflows of funds. It also initiated monitoring and supervision programs. This promoted accountability in two ways. First, the crackdown on corrupt district education officers signaled strengthened oversight by the central government. Thus, district authorities now knew they would be held accountable for any shortfall (or at least understood that the likelihood of this happening had increased). Second, by giving schools access to the information needed to understand the workings of the program,

[6] The value for 1995 is the 1995 value if data is available for that year, otherwise it is the value for the previous year.

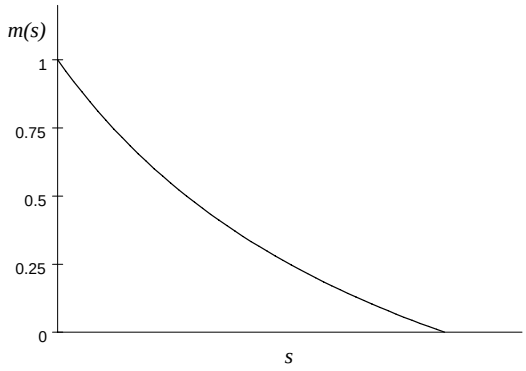


Figure 1: Probability of an investigation as a function of $s_j$.

schools (head teachers and parents) could now themselves monitor the local administration and voice complaints if funds did not reach the schools.

What is the expected impact of these policy changes for an individual school? To answer this question we need to understand what determines capture across schools. The following simple model illustrates that the public officials' ability to distinguish informed from less informed schools and/or information externalities are important determinants of the overall effects of the campaign.

## 3.1 A simple model of the effects of the campaign

Consider a public official in a district who receives funds $\bar{s}$ per school from the government. There are $J > 1$ schools in the district. The official is in charge of disbursing the funds to the schools. He may capture part (or all) of the funds, but then runs the risk of being investigated. If investigated, the official will be fired (conditional on parts of the fund being captured). A fired official cannot keep any captured funds. An investigation can be initiated by the central government. Assume there to be a probability $\rho$ that an audit (monitoring visit) will take place in school $j$ and conditional on the audit, an investigation will be initiated with probability $m(s_j)$, where $m'(s_j) < 0$, $m''(s_j) > 0$, $m(\bar{s}) = 0$, and $m(0) = 1$. We thus assume the likelihood of an investigation to be a function of how large part of the centrally disbursed funds reaches the school. The probability function is depicted in Figure 1.

Individual schools may also initiate an investigation (by voicing a complaint to higher authorities). The probability of a school voicing a complaint depends on both the amount of money the school is receiving and whether it

is aware of its entitlements. Specifically, we assume the likelihood of school $i$ initiating an investigation to be $\theta_j m(s_j)$, where $\theta_j \in [0,1]$ captures the school's knowledge about its entitlement.[7]

We start by assuming that the official can observe $\theta_j$.

The probability of an investigation of capture of funds from school $j$, $p_j$, is

$$p_j = \rho m(s_j) + (1-\rho)\theta_j m(s_j) = \gamma_j m(s_j), \tag{1}$$

where $\gamma_j \equiv \rho + (1-\rho)\theta_j$.[8]

The public official is an expected rent-maximizer. Thus, he maximizes capture taking (1) into account. The expected rents are

$$\Pi = (1-P)\left[J\bar{s} - \sum_{j=1}^{J} s_j\right], \tag{2}$$

where $(1-P) = \prod_{j=1}^{J}(1-p_j)$, $P$ is the likelihood of an investigation being initiated in at least one school, and the second term is aggregate capture.

Solving the official's maximization problem yields $J$ first-order conditions (for $s_1, \ldots s_J$),

$$-\gamma_j m'(s_j)\left(J\bar{s} - \sum_{i=1}^{J} s_i\right) - [1-p_j] = 0 \quad j = 1, 2 \ldots J. \tag{3}$$

The first-order conditions illustrate the trade-off facing the public official. Reducing capture to school $j$ by one unit increases the chances of no investigation taking place and thus that the official can keep his captured funds $\left(J\bar{s} - \sum_{i=1}^{J} s_i\right)$, the first term in (3), but also lowers the expected returns by $(1-p_j)$ units.

This version of the model illustrates both the individual effects and the possibility of negative spillovers in information accusation. First, local capture of funding to school $j$ is a negative function of external supervision (or audit) $\rho$, and a negative function of the school-specific effect $\theta_j$. These are the direct effects. Note that all schools will receive funding (even $\theta_j = 0$

[7] Note that this assumption implies that for a fully informed school (i.e., $\theta_j = 1$), the likelihood of an investigation is the same as for an audited school. It is straightforward to relax the assumption that the probability, $m(s_j)$, that the government initiates an investigation is the same as the probability a school will. The qualitative results remain intact.

[8] Note that if an audit takes place, we assume that the school will not initiate its own investigation.

schools), but more informed schools will receive more money.[9] Second, an increase in school $j$:s knowledge of the grant program reduces the funding to school $k$, even if school $k$ is informed. The intuition is as follows. If a school becomes more informed, this forces the official to increase that school's funding. As a result, total capture, $\left(J\bar{s} - \sum_{i=1}^{J} s_i\right)$, falls. This, in turn, reduces the return to honest behavior (first term in (3)) and, as a result, more funds will be captured from school $k$.

Consider instead a situation in which the official do not know to what extent each individual school is aware of its entitlement, i.e., $\theta_j$ is private information, but the official has knowledge about the overall distribution of informed schools. We can capture this by assuming there are two types of schools, more informed schools (type $\bar{\theta}$ schools) and less informed schools (type $\underline{\theta}$ schools), where $\bar{\theta} > \underline{\theta}$. The official knows that a share $\alpha \leq 1$ of the schools are of type $\bar{\theta}$.

The probability of an investigation of capture of funds from school $j$, $p_j$, is now

$$p_j = \gamma m(s_j), \tag{4}$$

where $\gamma \equiv \rho + (1-\rho)\left[\alpha\bar{\theta} + (1-\alpha)\underline{\theta}\right]$. Solving the official's maximization problem yields first-order conditions,

$$-\gamma m'(s_j)\left(J\bar{s} - \sum_{i=1}^{J} s_i\right) - [1 - p_j] = 0 \quad \text{for } j = 1, 2...J \tag{5}$$

In this second version of the model, external supervision (or audit) has a similar effect as in the full-information version. However, a school's individual knowledge about its entitlements $\theta_j$ does not influence the outcome. Instead, funding to school $i$ depends on the share of schools being informed. It is straightforward to show that $ds_j/d\alpha > 0$; i.e., the more informed schools in a district the more funding to each individual school. In Manski's (1993) terminology, local capture is driven by an exogenous (contextual) group effect.[10]

So far we have assumed that there are no externalities in learning about the grant program. However, it seems reasonable to assume that school staff

[9] All claims are proven in appendix 3 and hold provided that the first-order conditions bind.

[10] There is an exogenous (contextual) group effect since there is a causal relationship between the characteristics of the peer group members and outcome (average access to newspaper affect the official's maximization program and thus each school's funding).

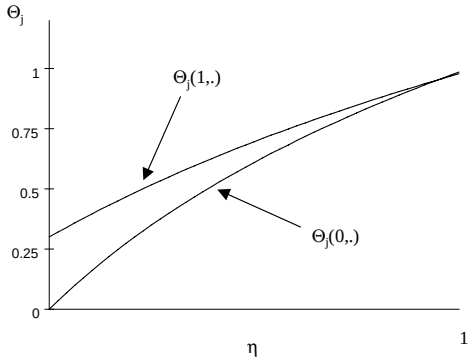


Figure 2: Knowledge $\Theta_j\left(\eta_j, \eta\right)$ as function of $\eta$.

learn from their peers or friends.[11] A simple way to introduce externalities in information across schools is to assume that $\theta_j$ is a non-negative function of both a school-specific effect $\eta_j$ (e.g., access to a newspaper) and the stock of knowledge in the district (e.g., average newspaper access in the district) $\eta$. Specifically, let $\eta_j = \{0, 1\}$ where $\eta_j = 1$ (0) indicates that the school has (has not) access to a newspaper and $\bar{\eta} = (1/J)\sum_{i=1}^{J} n_i$. A school's knowledge about its entitlement is now $\theta_j = \Theta_j\left(\eta_j, \eta\right)$, where $d\Theta_j/d\eta_j \geq 0$, $d\Theta_j/d\eta \geq 0$, and $\Theta_j\left(1, \eta\right) \geq \Theta_j\left(0, \eta\right)$. The knowledge function $\Theta_j\left(\eta_j, \eta\right)$ is illustrated in Figure 2.

If $\theta_j$ is private information, introducing externalities will not change the relative distribution of school grants across schools, although it will positively affect the amount each school receive. That is, $s_j = s$ for all schools, but the amount received will be higher as aggregate knowledge increases. In this case, local capture is driven both by an exogenous (contextual) group effect and an endogenous group effect (Manski, 1993).

If local officials can partly or fully observe each school's knowledge about the grant, introducing externalities will affect, and possibly change the sign of, $ds_k/d\theta_j$. In the example considered above, if $\theta_j = \Theta_j\left(\eta_j, \eta\right)$, an increase in school $j$:s knowledge about the grant may now (if the externalities are strong enough) even increase funding to school $k$. In this case, there are two countervailing forces. As before, if a school becomes more informed this forces the official to increase that school's funding. As a result, total capture, $\left(J\bar{s} - \sum_{i=1}^{J} s_i\right)$, falls. This in turn reduces the return to behaving

[11] There are several studies from developing countries that have shown that learning externalities are quantitatively important in, for example, the adoption of new technologies. See for instance Besley and Case (1994) and Foster and Rosenzweig (1995).

honestly (first term in (3)) and as a result more funds will be captured from school $k$. However, a higher $\eta_j$ also raises the marginal return of reducing capture (first term in (3)) and reduces the expected marginal loss of channeling more funds to the schools (second term in (3)). Both these forces will tend to increase funding to school $k$.

More generally, if the official observers a signal of each school's knowledge, for instance the official observes if the school has access to a newspaper or not, and learning spillovers are important, he will rationally induce that all schools' knowledge about the grant in the district have increased. If the knowledge function is as depicted in figure 2, the higher $\eta$, the more funding to each individual school and the smaller the difference between $\eta_j = 1$ and $\eta_k = 0$ schools.

# 4 Assessing the information campaign: Data

The objective of the repeat survey was twofold. First, to replicate the tracking exercise carried out in 1996; i.e., to measure the difference between the resources disbursed by the central government (intended for primary schools) and the resources actually received by the schools. Second, to collect data on access to information (and access to means for acquiring information) on the grant program, and other variables that may influence the bargaining position of individual schools.

The original sample (1996 sample) consisted of 250 schools, randomly drawn from 18 districts. Two general criteria governed the choice of procedure in selecting the sample of schools to be surveyed from the set of eligible (i.e., government operated) schools (see Reinikka [2001] for details). First, the sample should have a broad regional coverage. Second, it should be representative of the population of schools in the specified districts. A stratified random sample was chosen to account for these considerations. Specifically, it was decided that two or three districts (depending on the number of schools) should be drawn from each of the seven main regions (Northwest, North, Northeast, East, Central, Southwest and West). For each region, two (or three) districts were drawn with a probability proportional to the number of schools in the district, yielding a sample of 18 districts.[12,13] In the selected

[12] The capital Kampala was treated as a district.

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

districts, the number of schools visited ranged from 10 to 20, once more depending on the number of schools in the districts.[14]

Due to security concerns, not all schools in the original sampled could be re-surveyed. Specifically, it was decided that two districts (Moroto and Bundibugyo) should be dropped, which reduced the sample by 20 schools. After the data collection had been initiated, one of the districts (Gulu) experienced a major insurgency, and an additional 11 schools had to be dropped. One additional school belonging to the original 1996 sample had ceased to exist, resulting in a final sample of 218 schools.[15]

In addition to collecting detailed information on receipts and enrollment data, a short test of head teachers was also carried out in the 2002 survey.

The 2002 survey also formed the basis for a related research project. To this end, it was decided that an additional 170 schools from 9 of the original 18 districts should be surveyed.[16] 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.[17]

Our school-specific measure of capture is grants received (by school $j$ in year $t$) as a share of the grants disbursed by the central government to that school. A school's entitlement is a function of the number of P1 to P3 and P4 to P7 students. In 1995, the grant formula prescribed that each student in P1 to P3 was entitled to 2,500 USh per year, while each student in P4 to P7 was entitled to 4,000 USh. In 2001, the capitation grant was 5,000 USh per year for P1 to P3 students, and 8,100 USh per year for P4 to P7 students.

Records of releases by the Ministry of Finance (MoF) indicate that this

[14]The 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. In addition to collecting detailed information on financial and in-kind receipts and enrollment data, interviewers made qualitative observations to supplement the quantitative data.

[15]An 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.

[16]The total sample was set to 400 schools, of which 170 should be new schools. A replacement school was chosen for the school that had ceased to exist. Thus, in the end 171 new schools were surveyed.

[17]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 of the seven. Two additional districts were then selected from the two larger 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.

rule was followed, with one exception. If the districts did not submit documentation in each quarter, release(s) of funds in the following months could be withheld (postponed).[18] Information obtained by the MoF shows that in the fiscal year 2000/01, on average 93 percent of the approved funds were released, although some districts received significantly less (for example the government withheld 49 percent of the funds to Kyenjojo and 25 percent of the funds to Kayunga). The actual amount disbursed by the central government to districts was confirmed in the surveyed districts. In deriving the capture measure, we adjust for the withholding effect by scaling a school's entitlement with the share of funds actually released by the center to the district during that year.[19]

As with the 1996 survey data, we believe the grant data collected at the school level to adequately reflect what the schools actually received. The data was collected directly from the school records and most often, the enumerators could double-check this information from copies of bank cheques received. The concern that headmasters might have underreported the school income in order to extract resources for themselves was perceived as less likely, since each cheque had to be signed by at least two people (the headmaster and the school management committee chairman).[20]

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

Summary statistics are reported in Tables 1 and 2. As is evident, the situation has improved dramatically since the mid 90s. While only 24 percent of the total yearly grant from the central government reached schools on average in 1995, the average school received more than 80 percent of its entitlement 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 capture has been dramatically reduced. However, for many schools, local capture is still a problem. On average, 20 percent of the schools' entitlements do not end up in the schools and about 30 percent of the schools

[18] Within four weeks after the close of the quarter, the districts should submit (i) a progress report for the previous quarter, (ii) a district quarterly reports on monitoring visits, (iii) a cumulative progress report, and (iv) a budget request for the forthcoming quarter.

[19] Another source of discrepancy is that the Ministry of Education's (MoE) school enrollment numbers (per district) differed slightly from those reported by the district. We also adjusted the grant formula for this discrepancy; i.e., we scaled down the entitlement per student for those districts reporting higher enrollment rates than the MoE.

[20] Naturally, once cashed, the funds may have been used in a way that it was not intended.

receive less than 2/3 of their entitlements.

Table 1 also depicts the breakdown across regions. There are variation across regions. In particular, the eastern region and, to some extent, the west have more severe problems of capture. A striking difference from the 1995 data is that the bulk of the variation is now across regions. Specifically, a variance decomposition shows that 11 % of the variation in capture in 2001 is due to variation within regions, while 89 % is due to variation across regions.

# 5 Assessing the information campaign: Estimation

The fundamental problem in assessing the role and impact of information arises from the impossibility of observing what would happen to a school in both the state where it is informed of its entitlements and that where it is not. Impact evaluation methods therefore involve establishing a counterfactual state (in this case "not having access to information") against which the current state will be compared ("having access to information"). The evaluation problem is compounded by the fact that the information campaign is characterized as being non-exclusive. Thus, all schools in the 2001 sample could be viewed as treatment schools, although to a varying degree. In addition, potential group effects as discussed in section 3 make the evaluation problem more complex.

We evaluate the impact of the information campaign using a number of strategies. First, we look at the average impact by comparing school outcomes pre and post treatment. Second, we exploit the difference in newspaper access both within and across districts.

## 5.1 A pre-post evaluation

One way of evaluating the campaign is to compare schools' pre-campaign and post-campaign situations. A pre-post evaluation uses the pre-campaign data as a baseline, or counterfactual, implying that the 250 surveyed schools will serve as their own control group. The effect of the information campaign is determined by comparing funding to the schools in 1995 and 2001, respectively, accounting for the effect of other (exogenous) influences.

Formally, let $s_{jt}$ be funding to school $j$ in period $t$. The average treatment effect $\beta_1$ can be estimated as

$$s_{jt} = \beta_0 x'_{jt} + \beta_1 \sigma_t + \mu_j + \varepsilon_{jkt} \ , \tag{6}$$

where $x'_{jt}$ is a vector of observables variables that may influence the school's ability to acquire information and voice a complaint to higher authorities (see Reinikka and Svensson, 2002), $\mu_j$ is a school-specific effect, and $\sigma_t$ is the treatment dummy, taking the value 1 in the post-campaign year 2001 and 0 in the pre-campaign year 1995.

There are two advantages of evaluating the campaign by comparing pre- and post-campaign outcomes. First, there was no mass media based information dissemination prior to the first survey. Second, the information campaign was (is) non-exclusive in the sense that information about the workings of the grant program and releases of transfers of funds from the Ministry of Finance were published in national (daily) newspapers and broadcast on the national radio. However, the pre-post approach has some obvious shortcomings. First, it (only) provides an estimate of the average impact of the information campaign. Presumably, not all schools had the means (for instance, access to radio and newspapers) or the ability to acquire the necessary information on funding from the information campaign.[21] Thus, $\beta_1$ should be interpreted as a measure of the impact of this particular information campaign (or a campaign of this type), and not the impact of having more informed schools. Second, and most importantly, to identify a causal effect using pre-post data $x_{jt}$ must capture all time-varying factors that have influenced the relationship between schools and district officials, including policy changes that might have occurred since 1995. If this is not the case, the estimate of the effect of the campaign suffers from an omitted variable bias.

Table 3 reports the results from estimating (6) using data from 1995 and 2001. In Regression (1), we include the set of bargaining/search variables discussed in Reinikka and Svensson (2002). They show that controlling for school fixed effects, quality of school leadership (measured as the share of qualified teachers) and school size, actual spending is highly regressive: schools with children of wealthier parents experience a lower degree of capture. We follow Reinikka and Svensson (2002) and use the mean consumption level across district-urban-rural locations as a measure of income.[22]

[21] Thus if the intention is to evaluate the effect of empowerment (information) as a way to reduce local capture, the estimate suffers from a compliance bias.

[22] The mean consumption level across district-urban-rural is derived from household survey data. See appendix for details. Reinikka and Svensson (2002) use the mean consumption level across district-urban-rural locations as an instrument for a school-specific income measure (parents' average financial contribution to the school). This procedure could not be replicated as most schools in 2001, contrary to 1995, did not receive any financial contribution from parents (most likely due to the increase in funding reaching the school in 2001). The specification in Table 3 can thus be seen as a reduced-form specification. Reinikka and Svensson (2002) show that mean consumption level across

As is evident from Regression (1), the basic relationship between income and capture remains. Moreover, we can soundly reject the assumption that the school-specific effects are all equal (F-statistic is 1.65, with a p-value of 0.00), thus suggesting that there are important fixed school-specific effects.

Regression (2) adds $\sigma_{2001}$. $\sigma_{2001}$ enters highly significant. The point estimate suggests that the (policy) changes that have occurred since 1995, including the information campaign, has reduced local capture by almost 66 percentage points, controlling for income, school size and quality of school leadership. Interestingly, when including the treatment dummy $\sigma_{2001}$, actual spending is no longer regressive in nature but income neutral.[23]

There has been other changes in the education sector apart from the information and empowerment campaign since 1995, including improved monitoring and supervision by the central government, increased capitation grants (per student), and a reduction of (formal) school-fees. It is possible that these additional policy measures, or some other time-varying factor, have influenced the degree of capture. If that is the case, and since these other policy changes also were non-exclusive, the estimate of the information campaign suffers from an omitted variable bias. To the extent that these other policy measures had a positive impact on the share of entitled funds reaching the schools, $\beta_1$ constitutes a upper bound on the effect of the information campaign.[24]

## 5.2 Differences-in-differences

An alternative approach to estimate the effects of the campaign rests on the fact that there are differences in the means or infrastructure for acquiring information across schools, or more precisely, access to newspapers. One of the more important components of the campaign has been the publishing of monthly transfers of public funds to the districts in newspapers. Since 1997, the newspapers have also had numerous articles on cases of misuse of the

district-urban-rural locations is a significant predictor of school-specific income. 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 capture, rather than income per see.

[23]Note that income is constant across schools in a district-rural-urban location. We cannot tell if capture within such a location is regressive or not. Moreover, since income has increased in the late 1990s, including the time dummy $\sigma_{2001}$ will remove an important source of variation to identify the income effect.

[24]Although it is possible that some changes since 1995 may have reduced the schools' bargaining strength, in which case the pre-post estimate may not be the upper bound of the effect.

program and discussions about schools' entitlements and responsibilities.

Intuitively, schools with access to newspapers have been *more extensively exposed* to the information campaign. To explore the difference between the group of schools with and without access to newspapers, we employ a differences-in-differences model. The treatment group is thus schools (head teachers) reporting access to the main newspapers in 2001, and the control group is schools (head teachers) reporting that they did not have access to newspapers in 2001. Of course, this method will capture one component of the information campaign (i.e., the newspaper campaign), although most likely the most important component.

There are two concerns with this approach. One potential problem is that newspaper access is partly endogenous. Specifically, there may be some unobserved school characteristic correlated with both newspaper access and the efficiency in which the school can articulate its case to the district officials. In practise, there are reasons to believe that this is not a serious concern since schools do not necessarily buy their own paper.[25] In addition, as shown in Björkman (2003), newspaper penetration varies greatly across districts in Uganda. Furthermore, to the extent that these unobserved school characteristics are constant over time (at least in the period we are considering), we can control for them.

The second problem is possible group effects as discussed in section 3. If these effects are present, the differences-in-differences estimator will underestimate the effect of the newspaper campaign. We deal with these two concerns subsequently.

Let $m$ denote treatment group, with $m = 1$ if the schools in the group belong to the treatment group and $m = 0$ otherwise. Let $s_{j0}$ be funding to a school $j$ without access to newspapers and $s_{j1}$ funding to a school with access to newspapers. The average funding to a group of schools $m$ without newspapers reporting about the grant program in year $t$ is then $E[s_{j0}|\ t, m]$, and the average funding to a group of schools with newspapers in $t$ is $E[s_{j1}|\ t, m]$. We observe $E[s_{j1}|\ 2001, m = 1]$. The differences-in-differences estimator uses the control groups to estimate the counterfactual average, $E[s_{j0}|\ 2001, m = 1]$.[26]

Table 4, panel A, depicts the differences-in-differences estimates. The first column reports funding in 1995, the second column reports funding in 2001, and the third column reports the difference between them. The rows give

[25] Sharing of newspapers is not uncommon in poor countries. On average, each copy of the New Vision, one of the main newspapers in Uganda, is read by 10 people, according to the New Vision. School with access to a newspaper may not have access to it everyday but reported having access to a newspaper at least once a week. The median school had access to a newspaper 3 days per week.

[26] See for instance Angrist and Krueger (1999).

averages (and standard errors) for the treatment group, the control group, and the difference between them. As is evident, in 1995, the treatment and the control group suffered just as much from local capture. In fact, the treatment group received less funds than the control group, although the difference is not significant. This finding suggests that the treatment group did not have any other specific characteristics (apart from access to newspapers) which increased their capability to claim funds in 1995. From 1995 to 2001, there is a large drop in leakage in both groups, which is consistent with the pre-post findings reported above. However, the reduction in leakage is significantly higher in the treatment group. The differences-in-differences estimate is 13.8 and is significant at the 5 percent level. Thus, schools with access to newspapers and thus *more extensively exposed* to the information campaign, on average increased their funding by 13.8 percentage points more than the schools that lacked access to newspapers.

Table 5, Regression (1), depicts the regression version of the differences-in-differences method. The key identifying assumption in Regression (1) is that funding would have been the same in the two groups if no information on the grant program was reported in the papers. However, it is possible that the funding shares would have evolved differently across groups regardless of the content reported in the newspapers. As a robustness test, we therefore add the controls identified above to Regression (1). As is evident, the differences-in-differences estimate is robust to controlling for income, school size, and staff qualifications.

The differences-in-differences estimate can be interpreted as the causal effect of the newspaper campaign provided there are no group effects and under the assumption that in the absence of the campaign, the reduction in capture would not have been systematically different in the group of schools with and without access to newspapers. One way to test the second assumption is to compare the trend in outcomes before the campaign; i.e., during the period 1991-95.

In Table 4, Panel B, we present this control experiment. Although the amount of spending reaching the schools improved over the period, the trends do not differ systematically across the two groups. The differences-in-differences estimate is in fact even negative, although insignificantly different from 0. These results provide some suggestive evidence that the results reported in panel A are not driven by some unobserved variables that cause funding to schools to evolve differently across treatment and control groups.

## 5.3 Newspapers and knowledge

The results in the previous sub-section suggest that information published in the newspapers on the timing of disbursements and the workings of the program has a significant impact on reducing local capture. In this sub-section we provide some additional robustness test of the identification assumptions behind the differences-in-differences model. The data we use is based on a simple knowledge test of head teachers

If the differences-in-differences estimate should be interpreted as causal evidence of the impact of the newspaper campaign, schools with access to newspapers should be more informed about the program. In Table 6, we report a set of regressions where the explanatory variable is access to newspapers and the dependent variables are test scores indicating to what extent the head teacher correctly answered the question(s) reported in the top row. The second and third last rows in Table 6 report the range of possible scores and average test score in the sample population, respectively.

Regression (1) shows that schools with a newspaper are better informed about the formula for deriving the capitation grant. They are also better informed about the timing of releases of funds from the central government (Regression 2). The dependent variable in Regression 3, *info*, is the sum of the dependent variables in Regressions 1 and 2. As is evident, schools with access to newspapers are more informed in general about the workings of the grant program. These findings are consistent with the differences-in-differences estimates in tables 4 and 5.

One concern is that head teachers with access to newspapers are high ability types and thus more knowledgeable in general. A way to test this is to compare schools knowledge in other areas. This test is presented in Regressions (4)-(6). In Regression (4), the dependent variable is knowledge about news events and people that were discussed in the papers at the time of the survey in 2002. As expected, schools with a newspaper are significantly more likely to score high on this variable.[27] However, we find no significant

[27]We asked the respondents if they could identify the following individuals: (a) Winnie Byanyima (outspoken MP and wife of the former presidential candidate), (b) Thabo Mbeki (President of South Africa), (c) Bono (rock singer who was currently touring Africa), (d) George W. Bush, and (e) Charles Onyango-Obbo (outspoken editor of The Monitor). We also asked if the respondent could name the (f) Minister of Education (Hon. Makubuya), (g) Minister of Finance (Hon. Ssendaula), and (h) Prime Minister (Hon. Nsibambi). The head teachers with access to newspapers were significantly more likely to respond correctly to each individual question (except the question on President Bush for which we found no significant difference). The variable "Knowledge about recent news events" is the average score on these eight questions, where a correct answer is coded as 1 and an incorrect answer is coded as 0.

difference between the two types of schools (head teachers) in their knowledge of local affairs (Regression 5),[28] or knowledge of general (sociopolitical) issues typically not reported in the papers (Regression 6).[29]

While not providing any comprehensive information about head teachers' skills, the test scores finding reported in Table 6 suggest that it is information on the grant program disseminated through newspapers, rather than some unobserved characteristic (e.g., ability) correlated with newspaper access, that account for the observed effects.

## 5.4 Group effects

An additional concern with the differences-in-differences model is that it will underestimate the true treatment effects of the newspaper campaign if there are positive information spillovers across schools and/or if the officials cannot distinguish if a school is informed (has a newspaper) or not about its entitlement. As discussed in section 3, if these two complementary mechanisms are present, local capture will be influenced both by an exogenous (contextual) group effect and an endogenous group effect. On the other hand, if school staff do not learn from their peers and if the officials can distinguish informed from uninformed schools, an increase in the average knowledge in the district may reduce funding to uninformed schools.

In this section we attempt to test for possible group effects. We do that by estimating the following regression

$$s_{jkt} = \beta_0 x_{jkt} + \beta_1 \sigma_t + \beta_2 \sigma_t m_{jk} + \beta_3 \sigma_t M_k + \mu_j + \varepsilon_{jkt} \ , \qquad (7)$$

where $s_{jkt}$ is funding to school $j$ in district $k$ at time $t$, $x_{jkt}$ is a vector of school-specific controls, $\sigma_t$ is the treatment dummy that takes the value 1 in

[28] We asked if the respondents could name the (a) District Education Officer, (b) Chief Administrative Officer (in the district), (c) LC 5 Chairman (Local Council 5 chairman), (d) LC 3 Chairman, and (e) their representative in Parliament. The two types of schools were as likely to repond correctly to each of these questions. The variable "Knowledge about local affairs" is the average score on these five questions, where a correct answer is coded as 1 and an incorrect answer is coded as 0.

[29] We asked the respondents if they could tell what is the (a) Largest newspaper (circulation) in Uganda (The New Vision), (b) Location of East African Parliament (in 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), (f) Number of members in Parliament (305). The two types of schools were as likely to repond correctly to each of these questions. The variable "General political knowledge" is the average score on these six questions, where a correct answer is coded as 1 and an incorrect answer is coded as 0.

the post-treatment year $t$ and 0 in $t-1$, $m_{jkt}$ is a binary (0,1) variable indicating access to newspapers, $M_{kt}$ is a measure of how well informed schools are on average in the district, $\mu_j$ is a school-specific fixed effect and $\varepsilon_{jkt}$ is an iid error term.

The full-information version of the model with no spillovers suggests that $\beta_2 > 0$ and $\beta_3 < 3$. The limited-information version (with or without spillovers) instead predicts that $\beta_2 \geq 0$ and $\beta_3 > 0$. The measure of $M_k$ is based on survey data and is the average number of schools in the district with access to newspapers.

We can difference away the school-specific effects. Thus we estimate

$$\Delta s_{jk} = \beta_0 \Delta x_{jk} + \beta_1 \sigma_{2001} + \beta_2 m_{jk} + \beta_3 M_k + \Delta \varepsilon_{jk} \ , \tag{8}$$

where $\Delta s_{jk} = s_{jkt} - s_{jkt-1}$ and $\sigma_{2001}$ is a constant.

Table 7, panel A, depicts the results of estimating (8). In Regression (1), no school-specific controls are included. The results reported in column 1 is consistent with the limited information and/or spillover models. There is a large and highly significant effect of average newspaper access, but no significant impact of individual schools' access to newspapers ($\beta_2$). In Regression (2), we add the vector of controls $\Delta x_{jk}$. The results remain intact.

We can derive an estimate of the impact of the newspaper campaign by comparing a school with the average value of $M_k$ to a school with $M_k = 0$. The estimates in Regression (2) imply that the newspaper campaign reduced capture by 35.9 percentage points. Thus, the newspaper campaign accounts for 62 percent of the reduction in capture experienced by the average school since 1995.[30]

Individual access to a newspaper has no impact on capture in Regressions (1)-(2). This is consistent with the incomplete information model where the official only observes the distribution of informed schools. It is also consistent with the information spillover model, if these spillovers are large. Presumably the spillover effects are larger; i.e., the difference in knowledge between a school with access to a newspaper and a school without, the more schools have access to newspapers, as illustrated in Figure 2. One way to test for this is to differentiate between schools with access to a newspaper in districts where most schools have access to one from schools with access to a newspaper in districts where most schools do not. Let $\hat{M}_k = 1$ if at least 50 % of the surveyed schools in district $k$ has access to a newspaper and $\hat{M}_k = 0$ otherwise. To test for a differential effect, we add $\beta_4 m'_{jk}$ to equation

[30] According to Table 1, the average school experienced a reduction in capture of 57.9 percentage points.

(8), where the variable $m'_{jk}$ takes the value 1 if $m_{jk} = 1$ and $\hat{M}_k = 0$, and 0 otherwise.

The results are reported in Regressions (3)-(4). The point estimate on $m'_{jk}$ is large, implying that in a district where a majority of schools do not have access to a newspaper, individual access increases funding by 22 percentage points ($\beta_2 + \beta_4$ in Regression (4)), however the effect is imprecisely estimated (p-value in Regression (4) is 0.142).

As with the differences-in differences approach, the estimates in Table 7, panel A, can be interpreted as the causal effect of the newspaper campaign under the assumption that in the absence of the campaign, the reduction in capture would not have been systematically different across groups. As a control experiment, we compare the trend in outcomes before the campaign; i.e., during the period 1991-95. The results are presented in Table 7, panel B.

Before the information campaign started, there is no significant effect of individual access to newspaper on local capture. Furthermore, there is no evidence of any group effects or that the individual effect ($m'_{jk}$) varies with the share of schools having access to a newspaper. These findings suggest that the results reported in panel A are not driven by inappropriate identification assumptions.

## 5.5 Linking newspapers, information, and capture

In the previous subsection, access to a newspaper was a proxy of a school's knowledge about its entitlements. Thus, the previous regression framework can be seen as a reduced form attempt to estimate the impact of the newspaper campaign. In this section, we combine the findings from sections 5.2-5.4, and estimate the entire chain of causal effects. Specifically, we estimate the following model

$$info_{jk} = \gamma_0 \Delta x_{jk} + \gamma_1 \sigma_{2001} + \gamma_2 m_{jk} + \gamma_3 M_k + \Delta \upsilon_{jkt} \ . \tag{9}$$

$$\Delta s_{jk} = \beta_0 \Delta x_{jk} + \beta_1 \sigma_{2001} + \beta_2 info_{jk} + \Delta \varepsilon_{jk} \ , \tag{10}$$

where $info_{jk}$, defined in section 5.3, measures to what extent school $j$ in district $k$ is informed about the grant program. The key identifying assumption in the model (9)-(10) is that the error term in (10) is uncorrelated with the newspaper variables $m_{jk}$ and $M_k$.

Table 8 reports the results. Regression (1) depicts the first-stage regression; i.e., equation (9). Consistent with the group effects finding reported above, we find a large and highly significant effect of average newspaper access ($\gamma_3$). The explanatory power of the two excluded instruments, $m_{jk}$ and

$M_k$, is satisfactory. The $F$-statistic of their joint significance in the first-stage regression is 7.2 and is highly significant.

Regression (2) reports the second-stage regression (10). As is evident, $\widehat{info}_{jk}$ has a positive and significant effect on the share of entitled grants reaching the schools. Moreover, we cannot reject the null hypothesis that the instruments satisfy the orthogonality conditions; i.e., that the instruments are uncorrelated with the error process in (10).

The test score results on the other three knowledge measures defined in section 5.3, "knowledge about recent news events", "knowledge about local affairs", and "general political knowledge", provide further evidence in favor of the identifying assumption. As reported in Table 9, neither individual access ($m_{jk}$) to a newspaper nor average number of schools in the district with access to newspapers ($M_k$) explain schools' knowledge of local affairs (Regression 1) and/or information about general (sociopolitical) issues typically not reported in the papers (Regression 2). However, both explanatory variables have a significantly positive effect on schools' knowledge about current news events.[31] These test score findings suggest that it is information on the grant program disseminated through newspapers, rather than some unobserved characteristic correlated with $m_{jk}$ or $M_k$ that account for the findings reported in Table 8.

We can derive the impact of the newspaper campaign using the same procedure as in section 5.4. The estimates suggest that the information campaign can explain 37.5 percentage points of the reduction in local capture since the mid 1990s, i.e., 65 percent of the reduction in capture experienced by the average school since the campaign started.

In Regression (3), Table 8, we again differentiate between schools with access to a newspaper in districts where most peers also have access to one from schools with access to a newspaper in districts where most peers do not. In the first-stage regression, both average newspaper access $M_k$ and $m'_{jk}$ enter significantly positive. Thus, individual access to a newspaper is a significant predictor of knowledge about the grant program, but only if most of the other schools in the same districts lack access. The estimated effects are large. In a district where most schools lack access to a newspaper,

[31] It is interesting to note that while individual access has no explanatory power in the knowledge about the grant (Regression 1, Table 8), it is an important determinant of knowledge about current news events. A school with access to a newspaper has a 0.3 standard deviation higher test score on "knowledge about recent news events". A one standard deviation increase in $M_k$ improves the same test score with 0.17 standard deviation. These results are consistent with the hypothesis that the learning spillovers between peers are relatively small with respect to information that is unrelated to the school compared to spillovers in information on the school grant program.

having access to one increases test score (*info*) with 0.9 standard deviations. A school lacking own access to a newspaper would, everything else equal, increase its test scores (*info*) with 1.2 standard deviations if reallocated from a district with the lowest to the highest average newspaper penetration in the sample.

Regression (4) presents the second-stage results. The information effect $\hat{\beta}_2$ is slightly smaller but is still highly significant. Moreover, we still cannot reject the null hypothesis of the validity of the instruments.

The estimates in regressions 3 and 4 suggest that the impact of the newspaper campaign is conditional on the share of schools with access to a newspaper. For example, if only one school, school $j$, in district $k$ has access to a newspaper, the estimates imply that the newspaper campaign increased funding (or reduced local capture) to that school by 34 percentage points. A school with the average value of $M_k$ (and $m_{jk}$), increased its funding as a result of the newspaper campaign with 43.2 percentage points, or 3/4 of the total reduction in capture experienced by the average school.

# 6 Conclusion

Through a relatively inexpensive policy action - the provision of mass information - Uganda has managed to dramatically reduce capture of a public program aimed at increasing primary education. Because poor people were less able than others to claim their entitlement from the district officials before the campaign, but just as likely in 2001, they benefited most from it.

The data suggest a reduction in capture from the mid 1990s of, on average, 58 percentage points.

We exploit differences in access to newspapers to identify the effects of what is likely the most important component of the information campaign - the publishing of monthly transfers of public funds to the districts in newspapers. The differences-in-differences estimates reveal that information disseminated through newspapers played an important role in informing and empowering schools. Schools with access to newspapers; i.e., the more extensively treated schools, on average increased their funding by 14 percentage points more than the schools that lacked access to newspapers. Consistent with this interpretation, we find that head teachers with access to newspapers on average have better knowledge about the rules and timing of releases of funds from the center, but score as well as those lacking newspapers on a test of local and general knowledge, suggesting that newspaper access does not simply capture ability. Moreover, the trend in outcomes do not differ

systematically across the two groups in the five year period before the campaign was initiated. That is, the sharp difference in local capture between the group of schools with and without access to newspapers took place after the campaign started.

However, the comparison of schools with and without newspapers attenuate an important effect, the spillover effect from informed to uninformed schools. In the paper, we discuss two complementary effects that may influence local capture: an exogenous group effect (if the local official cannot distinguish informed from uninformed schools) and an endogenous group effect (learning spillovers). Taking these into account, we find that the information campaign can explain up to 75 percent of the reduction in local capture since the mid 1990s.

At present, several countries run, or are in the process of initiating, a similar capitation grant program as in Uganda (for example Tanzania and Kenya). The results in this paper suggest that policies to inform and empower the end users (parents and school staff) should be (and already is in some countries) an integral part of the school funding program.

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# 7 Appendix

## 7.1 Data description

$\sigma_{2001}$ = treatment dummy taking the value 1 in the post-information period 2001 and 0 in the pre-information period 1995.
*M'* = average number of surveyed schools in the district with access to national newspaper (source: survey data).
*M"* = circulation per school in the district of New Vision (source: The New Vision).
*mean consumption* = mean consumption level in the district-urban-rural location (source: constructed using data from the 1995 and 1999/2000 Uganda National Household Survey).
*newspaper* = dummy taking the value 1 if the school has access to a national newspaper, 0 otherwise (source: survey data).
*school size* = number of students in P1-P7 (source: survey data).
*share of intended capitation grant received* = capitation grant received as a share of what should have been received, adjusted for withheld funds from the MoF and lower enrollment rates at the center (source: survey data, official statistics MoF)
*share of qualified teachers* = share of qualified teachers to total number of teachers (source: survey data).
*supervision* = dummy taking the value 1 if the school has received a supervision/support visit from the MoE during the year, 0 otherwise (source: survey data).

## 7.2 Proofs of claim in section 3

Without loss of generality, assume $J = 2$. Let the first-order conditions (3) be given by the functions $F$ and $G$.

$$F(s_{1,}s_2;\theta_2) = -\gamma_1 m'(s_1)\left(2\bar{s} - \sum_{j=1}^{2} s_j\right) - [1 - \gamma_1 m(s_1)] = 0, \tag{11}$$

$$G(s_{1,}s_2;\theta_2) = -\gamma_2 m'(s_2)\left(2\bar{s} - \sum_{j=1}^{2} s_j\right) - [1 - \gamma_2 m(s_2)] = 0. \tag{12}$$

Total differentiate $F$ and $G$, we can derive

$$\frac{ds_2}{d\theta_2} = -\frac{1}{\Delta}\left[F_{s_1}G_{\theta_2} - F_{\theta_2}G_{s_1}\right] \tag{13}$$

$$\frac{ds_1}{d\theta_2} = -\frac{1}{\Delta}\left[F_{\theta_2}G_{s_2} - F_{s_2}G_{\theta_2}\right] \tag{14}$$

Note that $F_{s_1} < 0$ and $\Delta > 0$ (by SOC) and that

$$G_{\theta_2} = -(1-\rho)m'(s_2)\left(2\bar{s} - \sum_{j=1}^{2} s_j\right) + (1-\rho)m(s_2) > 0$$

and that $F_{\theta_2} = 0$. Thus,

$$\frac{ds_2}{d\theta_2} = -\frac{1}{\Delta}F_{s_1}G_{\theta_2} > 0$$

Further note that

$$F_{s_2} = \gamma_1 m'(s_1) < 0$$

Thus,

$$\frac{ds_1}{d\theta_2} = \frac{1}{\Delta}F_{s_2}G_{\theta_2} < 0$$

**Table 1**. Summary information on capture (in percent)

| | Mean | Median | St. dev. | Max | Min | Obs |
|---|---|---|---|---|---|---|
| *All schools* | | | | | | |
| 1995 | 23.9 | 0 | 35.1 | 109.8 | 0 | 229 |
| 2001 | 81.8 | 82.3 | 24.6 | 177.5 | 9.0 | 217 |
| | Mean (1995) | Mean (2001) | | | | |
| *Regions* | | | | | | |
| 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 | | | | |

a. Grants received as share of entitled grants.

**Table 2**. Summary statistics on school characteristics

| | School size | Income | Share of qualified teachers |
|---|---|---|---|
| 1995 | | | |
| Mean | 531 | 7,785 | 0.79 |
| Median | 449 | 7,315 | 0.88 |
| Std. dev. | 375 | 3,612 | 0.25 |
| 2001 | | | |
| Mean | 952 | 10,322 | 0.91 |
| Median | 855 | 9,001 | 1 |
| Std. dev. | 477 | 5,078 | 0.17 |

**Table 3**. Pre-post evaluation of the information campaign

| *Specification* | (1) | (2) |
|---|---|---|
| Income | 0.006*** | -0.002 |
| | (.004) | (.002) |
| Share of qualified staff | 14.9 | -23.5 |
| | (16.0) | (13.4) |
| School size | 0.061*** | -0.005 |
| | (.002) | (.009) |
| $\sigma_{2001}$ | | 65.7*** |
| | | (6.47) |
| School fixed effects | yes | yes |
| $R^2$ (overall) | 0.05 | 0.47 |
| Schools | 247 | 247 |
| Observations | 444 | 444 |

a. Dependent variable is grants received as share of entitled grants.
b. Robust standard errors in parenthesis.
c. *** [**] (*) denote significance at the 1 [5] (10) percent levels.

**Table 4**. Differences-in-differences estimates of the effects of the information campaign on capture and control experiment

| Group | Year | | |
|---|---|---|---|
| *Panel A: Campaign experiment* (no. observations: 444) | 1995 | 2001 | 2001-1995 difference |
| Access to newspapers | 24.5*** | 83.7*** | 59.2*** |
| | (2.87) | (1.94) | (3.46) |
| No access to newspapers | 29.6*** | 75.0*** | 45.4*** |
| | (5.40) | (3.11) | (6.22) |
| Access-no access difference | -5.12 | 8.68** | 13.8** |
| | (6.10) | (3.66) | (7.13) |
| *Panel B: Control experiment* (no. observations: 417) | 1991 | 1995 | 1991-1995 difference |
| Access to newspapers | 3.30** | 24.5*** | 21.2*** |
| | (1.30) | (2.87) | (3.14) |
| No access to newspapers | 2.94 | 29.6*** | 26.7*** |
| | (1.93) | (5.40) | (5.73) |
| Access-no access difference | 0.36 | -5.12 | -5.48 |
| | (2.32) | (6.10) | (6.61) |

a. Average funding received (in percent), with robust standard errors in parenthesis.
b. *** [**] (*) denote significance at the 1 [5] (10) percent levels.

**Table 5**. Conditional differences-in-differences estimates of the effects of the information campaign

| *Specification* | (1) | (2) |
|---|---|---|
| 1995 | 29.6*** | 49.2*** |
| | (5.4) | (7.3) |
| 2001 | 75.0*** | 100.7*** |
| | (3.1) | (7.5) |
| Newspaper | -5.12 | -2.18 |
| | (6.1) | (6.3) |
| Newspaper*2001 | 13.8** | 14.0** |
| | (7.1) | (7.2) |
| Controls | no | yes |
| $R^2$ | 0.80 | 0.81 |
| Schools | 218 | 218 |
| Observations | 417 | 415 |

a. Dependent variable is grants received as share of entitled grants.
b. Robust standard errors in parenthesis.
c. *** [**] (*) denote significance at the 1 [5] (10) percent levels.
d. The controls are "income", "share of qualified staff", "school size".

**Table 6**. Head teacher test

| *Specification* | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dep. var. | Knowledge about grant formula | Knowledge about timing | Info | Knowledge about news events | Knowledge about local affairs | General political knowledge |
| Newspaper | 0.110** | 0.090** | 0.200*** | 0.094*** | -0.001 | 0.023 |
| | (.056) | (.045) | (.073) | (.025) | (.012) | (.021) |
| Range of scores | (0,1) | (0,1) | (0,1,2) | [0,1] | [0,1] | [0,1] |
| Average test | 0.65 | 0.24 | 0.89 | 0.65 | 0.75 | 0.57 |
| Schools | 389 | 388 | 388 | 388 | 388 | 388 |

a. The dependent variables are in: (1) a binary 1,0 variable indicating correct (=1) [incorrect (=0)] knowledge about grant program, (2) a binary variable 1,0 indicating correct (=1) [incorrect (=0)] knowledge about timing of releases of the grant, (3) the sum [0,2] of "Knowledge about grant formula" and "Knowledge about timing", (4) average score [0,1] on five sub-questions on news events, where a correct [incorrect] answer to each sub-question is coded as 1 [0], (5) average score [0,1] on five sub-questions on local affairs, where a correct [incorrect] answer to each sub-question is coded as 1 [0], (6) average score [0,1] on six sub-questions on general political knowledge, where a correct [incorrect] answer to each sub-question is coded as 1 [0]. See text for details.

b. Average test is average test score.

c. Robust standard errors in parenthesis.

d. *** [**] (*) denote significance at the 1 [5] (10) percent levels.

**Table 7**. Group effects

| *Specification* | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Panel A: Campaign experiment (1995-2001)* | | | | |
| 2001 | 20.3** | 28.1** | 14.8 | 21.5 |
| | (9.9) | (11.8) | (12.1) | (13.7) |
| Newspaper ($m_{jk}$) | 2.1 | 2.5 | -0.8 | -1.0 |
| | (6.8) | (7.0) | (7.6) | (7.6) |
| $M_k$ | 44.0*** | 45.6*** | 53.4*** | 57.4*** |
| | (13.8) | (14.5) | (17.7) | (18.4) |
| $m'_{jk}$ | | | 18.7 | 23.1 |
| | | | (15.0) | (15.7) |
| Controls | no | yes | no | yes |
| Adj. R2 | 0.06 | 0.09 | 0.07 | 0.10 |
| Schools | 199 | 197 | 199 | 197 |
| *Panel B: Control experiment (1991-1995)* | | | | |
| 1995 | 14.2 | 13.1 | 17.2 | 16.6 |
| | (10.7) | (12.1) | (12.1) | (13.7) |
| Newspaper ($m_{jk}$) | -3.5 | -1.3 | -2.0 | 0.4 |
| | (8.5) | (8.6) | (9.1) | (9.2) |
| $M_k$ | 17.1 | 10.5 | 12.1 | 4.9 |
| | (15.9) | (17.0) | (18.7) | (19.8) |
| $m'_{jk}$ | | | -18.5 | -20.2 |
| | | | (11.7) | (12.4) |
| Controls | no | yes | no | yes |
| Adj. R2 | 0.01 | 0.02 | 0.01 | 0.03 |
| Schools | 147 | 145 | 147 | 145 |

a. Dependent variable is grants received as share of entitled grants.
b. $m_{jk}$ is a binary 1,0 variable indicating access (1) [no access (0)] to a newspaper, $M_k$ is the average number of surveyed schools in district $k$ with access to newspapers, $m'_{jk}$ is a binary 1,0 variable taking the value 1 if school $j$ has access to a newspaper and less than 50 % of the surveyed schools in the district ($k$) has access to a one, and 0 otherwise.
c. Robust standard errors in parenthesis.
d. *** [**] (*) denote significance at the 1 [5] (10) percent levels.

**Table 8.** Linking newspaper access, information, and capture

| *Specification* | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Regression | 1st stage | 2nd stage | 1st stage | 2nd stage |
| Dep. variable | $info_{jk}$ | $s_{jk}$ | $info_{jk}$ | $s_{jk}$ |
| 2001 | 0.32 | 9.4 | 0.14 | 14.3 |
| | (.20) | (19.5) | (.22) | (17.8) |
| Newspaper ($m_{jk}$) | 0.02 | | -0.7 | |
| | (.12) | | (.13) | |
| $M_k$ | 0.82*** | | 1.15*** | |
| | (.25) | | (.30) | |
| $m'_{jk}$ | | | 0.66** | |
| | | | (.32) | |
| $info_{jk}$ | | 57.1*** | | 52.1*** |
| | | (19.2) | | (17.4) |
| Controls | yes | yes | yes | yes |
| F-test | 7.2 | | 6.3 | |
| | [.00] | | [.00] | |
| Hansen | | 0.01 | | 0.73 |
| | | [.91] | | [.69] |
| Schools | 197 | 197 | 197 | 197 |

a. For variable definition, see notes of tables 6 and 7.
b. F-test is the test statistic of a F-test of the joint significance of the excluded instruments.
c. Hansen is Hansen (1982) *J* statistic.
d. Robust standard errors in parenthesis.
e. *** [**] (*) denote significance at the 1 [5] (10) percent levels.

**Table 9**. Head teacher test

| *Specification* | (1) | (2) | (3) |
|---|---|---|---|
| Dep. var. | Knowledge about local affairs | General political knowledge | Knowledge about news events |
| Newspaper ($m_{jk}$) | 0.01 | 0.04 | 0.11** |
| | (.01) | (.06) | (.04) |
| $M_k$ | 0.02 | 0.02 | 0.15* |
| | (.04) | (.03) | (.09) |
| Schools | 218 | 218 | 218 |

a. For variable definition, see notes of table 6.
b. Robust standard errors in parenthesis.
c. *** [**] (*) denote significance at the 1 [5] (10) percent levels.