The power of information in public services: Evidence from education in Uganda

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ABSTRACT

In this paper we argue that innovations in governance of social services are an effective way to improve outcomes such as attainment of universal primary education. To test this hypothesis we exploit an unusual policy experiment: a newspaper campaign in Uganda aimed at reducing the capture of public funds by providing schools (parents) with systematic information to monitor local officials' handling of a large education grant program. Combining survey and administrative data, we show that public access to information can be a powerful deterrent to the capture of funds at the local level and that the reduction in the capture of funds that resulted had a positive effect on school enrollment and learning outcomes.

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1. Introduction

The literature on education policy in developing countries focuses predominately on the last link in the service delivery chain; i.e., using a variation across schools or students to estimate the impact of various programs and inputs at the school level. A growing literature based on randomized trials have also helped start building consensus of what works, what doesn't, and why. However, a country's ability to improve education outcomes is not only determined by what happens at the school level, but by the behavior of different actors and agencies involved in the design and implementation of education policy. As service delivery in many developing countries is often plagued by inefficiencies and corruption, interventions that focus on improving governance in general and governance of social services in particular can be an important complement to more traditional school-based interventions to increase enrollment and student learning. 1

To examine this hypothesis we exploit an unusual policy experiment. Towards the end of 1997, the Ugandan government began to publish systematic public information on monthly transfers of capitation grants to districts in the national newspapers. The newspaper campaign came in response to evidence of extensive capture and corruption in the education sector — in 1995 schools received on average only 24% of the total yearly capitation grant from the central government (Reinikka and Svensson, 2004). The campaign was intended to enhance head teachers' and parents' ability to monitor the local administration and to voice complaints if funds did not reach the schools.

Using the survey data and administrative data from the Ugandan Ministry of Education, we link school enrollment and average primary leaving exam scores with data on the capture of funds and distance to the nearest newspaper outlet. A school's exposure to the newspaper campaign is determined both by the timing of the campaign and schools' (parents') access to newspapers. Exploiting variation over time (before and after the newspaper campaign was initiated) and across space (distance to a newspaper outlet), we find that public access to information can be a powerful deterrent to the capture of funds at the local level and that the reduction in the capture of funds was associated with an increase in enrollment. We also find positive, albeit weaker, effects on student learning (test scores). The results suggest that the effect on the quantity of education 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.

This paper links to a large literature on education policy in developing countries (for surveys see Glewwe, 2002; Glewwe and...
## 2. Data

The data used in this paper come from two sources: survey data on capture/corruption from two public expenditure tracking surveys, and enrollment and test score data from administrative records. Summary statistics are reported in Tables 1–3.

Public expenditure tracking surveys were conducted in 1996 and 2002.2 Both surveys collected detailed information on grant receipts and enrollment and test score data from administrative records.

The next section describes the data used in the empirical analysis and the method used to quantify the capture. Section 3 describes the situation before the newspaper campaign and lays out the key components of the reforms in the late 1990s. In Section 4 we discuss the empirical strategy. Section 5 presents the empirical evidence of changes in measured corruption to changes in socioeconomic outcomes.

The previous literature has focused on the effects of making untied funds available to schools. It adds to the latter by relating changes in measured corruption to changes in socioeconomic outcomes.

Table 3: Summary information on capitation grants received as share of entitled grants (percent). Source: Authors’ calculations based on 1996 and 2002 surveys and Ministry of Education; see text for details.

<table>
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<td>3.6</td>
<td>49.3</td>
<td>73.4</td>
</tr>
</tbody>
</table>

2002 survey collected data for 2001) and enrollment. The 2002 survey expanded the sample with an additional 170 schools from 9 of the original 18 districts. At the same time, not all of the original 250 schools 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.3 We thus have a sample of 218 schools for

and that it was representative of the population of schools in the selected districts.

The 2002 survey expanded the sample with an additional 170 schools from 9 of the original 18 districts. At the same time, not all of the original 250 schools 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.3 We thus have a sample of 218 schools for


which the survey data are available for the years 1991–95 and 2001, and a sample of 388 (218 + 170) schools for which the survey data are available in 2001.

The school-specific measure of the grant diversion, “Share of grants received”, is grants received 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. As a measure of the entitled number of students, we take the average of the number of enrolled students (in grades P1–P3 and P4–P7) from the public expenditure tracking surveys and the number of enrolled students according to the district records. In 1995 the grant formula allocated 2500 Ugandan shillings (USh) per year for each student in grades P1–P3 and 4000 USh for each student in grades P4–P7. In 2001 the amounts were 5000 USh for grades P1–P3 and 8100 USh for grades P4–P7.9 Records from the Ministry of Finance indicate that the entitlement rule was followed, except when districts did not submit the required quarterly documentation. In this case funds could be delayed or withheld in the following months. These records show that in fiscal year 2000/01, 93% of the approved funds were released, although some districts received significantly less (for example, the central government withheld 49% of the funds to Kyenjojo and 25% 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, a school’s entitlement was scaled down by the share of funds actually released by the center to the district.

A concern with our measure of grant diversion is that the survey data could be inflated even if attempts were made to ensure that this was not the case. Note that if the incentives to misreport remain constant, this is less of a concern since we look at differences across schools. It is possible that the more informed a school is the more likely it is to inflate its enrollment figures (because the school knows that the total grant is a function of a number of students enrolled). However, if the district officials are (at least partly) aware of these incentives, this would tend to work against finding an effect of the newspaper campaign, because the informed schools would have higher reported enrollment figures but also suffer from the extensive capture (assuming that the district disburse the grant based on the actual rather than the reported figures).

The summary statistics on capitation grants received as share of the entitled grants are reported in Table 3. As evident, the situation has improved dramatically since the mid–1990s. Schools, which had received only 24% on average of the total yearly grant from the central government in 1995 (in the pre-campaign period), received more than 80% in 2001 (post-campaign period).7 More strikingly, while the median school received nothing in the mid-1990s, it received 82% of its entitlement in 2001. Thus the extent of the diversion had fallen dramatically. However, the diversion is still a problem for many schools. On average, 20% of school entitlements do not reach the schools, and about 30% of schools receive less than two-thirds of their entitlements.

We focus on the enrollment of grade 7 students (P7). There are several reasons for this choice. First, this is the cohort of students that started school in 1995 (when the survey data reveal extensive capture) and, with no grade repetition, graduated from primary school in 2001 (when the survey data reveal a dramatic drop in capture). Second, for grade 7 students we can derive enrollment numbers that are less likely subject to misreporting. Specifically, we exploit the Primary Leaving Exam (PLE) records from the Uganda National Examination Bureau. The PLE data contain information on scores on standardized tests (i.e., the Primary Leaving Exam) in Math, English, Science, and Social Studies conducted yearly on all grade 7 students. Our measure of grade 7 enrollment is the number of grade 7 students taking this test.

We also derive our measure of cognitive skills from the Primary Leaving Exam records. Our baseline measure adds the standardized test scores in Math, English, Science, and Social Studies into an aggregate score, averaged across grade 7 students in the school. Each individual subject score goes from 0 to 9. One advantage with using the Primary Leaving Exam scores is that almost all grade 7 students take the test (this is also the reason for why it is an appropriate measure of enrollment). A second advantage is that passing the test is a requirement for acceptance into secondary school, so students have incentives to do their very best on the test. Enrollment and test scores for 1991, 1995, 1997, 2001, and 2002 are collected for all 388 schools for which the survey data are available in 2001.

Enrollment has been increasing over time in Uganda (see Table 2). Average PLE scores, however, show little improvement over time.

Data on distance to the nearest newspaper outlet is taken from the school survey. We denote by distance, the distance (in logs) in kilometers to the nearest newspaper outlet as reported by the head teacher in school j. During piloting we compared the head teacher’s assessment of distance to the nearest newspaper outlet with the measured (by enumerators) distance. We conclude that in most cases the two measures were similar.8

3. Institutional setup

3.1. Pre-campaign period

For more than two decades, 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. The 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 the years, funds for the capitation grant program are disbursed by the central government nine times per year.

The initial public expenditure tracking survey revealed a rather gloomy picture of governance in the education sector. Specifically, local government officials diverted nearly 80 cents of every dollar spent on the capitation grant program (Table 3). Most schools received nothing. Poor students suffered disproportionately because schools in poorer areas received on average less than schools in the relatively affluent ones (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 officials after the first survey findings went public. But anecdotal evidence suggests that funds were primarily used for patronage politics and funding of various political activities. The case study evidence of district political

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6 The 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).

7 A handful of schools had missing financial data in 1995 but had data for 1994. To increase the sample size, we use the 1994 observations for these schools. The results reported remain basically intact if we restrict attention to the sample of schools with the 1995 data.

8 A newspaper outlet is defined as an outlet/shop where newspapers are sold regularly. In a predominately rural country like Uganda, regular access to newspaper is mainly determined by logistics factors outside the control of the school. Newspapers are typically not available by mail in the rural parts of the country. Distance to a newspaper outlet is measured in the 2002 survey. Björkman (2006) reports that the distribution of newspapers across districts has been stable during the period 1996 to 2002.
financing and corruption in Uganda also points in the same direction. The diversion of funds was also facilitated by the ignorance about the disbursements of the capitation grant in most schools.

3.2. The information campaign

The possibility to use the main media outlets, and specifically newspapers, as instruments to strengthen the relationships of accountability in the education sector was discussed in the policy circles in Uganda in response to the findings of the initial public expenditure tracking survey (see, for example, Collier, 2007). Towards the end of 1997, the government began to publish the data in the national newspapers, on a regular basis, on the monthly transfers of the capitation grants to districts. In the campaign that has been sustained until today, the main newspapers used were The New Vision and The Monitor and their local language editions. Apart from the 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.

The information campaign also involved sending out leaflets on the workings of the capitation grant program to schools and informing schools and parents through radio. However, the radio stations did not systematically report the information on when or how much funds were being transferred to the districts or the amount schools were entitled to. All head-teachers have access to a radio in our sample. Here we focus on the effects of information about the program disseminated through newspapers. Thus, the paper should not be viewed as an evaluation of the entire information campaign. Instead we exploit the newspaper campaign in order to evaluate the effects of (better) access to information.

The main objective of the newspaper campaign was to provide information on the capitation grant program, including when and how much funds were being transferred to the districts, so as to enable head teachers and parents to monitor the local administration and to voice complaints if funds did not reach the schools.

There are several reasons to believe that the information campaign could be successful in reducing fund diversion in this particular setting. Clearly, schools being exposed to the newspaper campaign are more likely to be aware if funds are being diverted, and thus more likely to be able to make an informed choice about whether to voice a protest. Moreover, most households in Uganda, particularly in the rural areas where the schools in the survey sample are located, have no easily available alternative to the local public school as private schools are primarily 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). Furthermore, 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 regularly, and all schools have the institutions to handle collective decision making in the form of school management committees, consisting of parents and the head teacher. Finally, communities have different ways to sanction public officials/politicians, ranging from the informal forms of social pressure (verbal complaints) to the formal ones, such as local electoral sanctions (the local politician’s fear of losing an election) to career concern (the public official’s fear of losing his/her 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 and, consequently, district officials had an incentive to reduce the capture of funds intended for the community’s 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% 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 an actual protest and the threat of voice may have discouraged the local political elite from diverting resources intended for the schools, in equilibrium, there is no reason to believe that the incidence of voice and local diversion of funds should be correlated. Thus, when a school/community becomes better informed, and hence its monitoring ability increases, district officials may respond by reducing the capture so as to ensure that a costly protest is not initiated.

3.2.1. Predicted impact

A reduction in the capture or corruption, and hence an increase in funding, could impact schooling outcomes in various ways. First, it may enable the school’s management to raise the quality of education. Second, it might reduce costs to households to the extent that private education expenditures and government non-salary grants to schools are substitutes, as suggested by Das (2004). The first effect would most likely raise the marginal return to investing in education, while the second effect would lower the marginal cost. Both effects would tend to increase the number of students that will complete a given grade. The effect on the average skills of students is less clear. On the one hand there is a direct effect from higher quality to cognitive skills. On the other hand, the increase in enrollment that follows the reduction in cost and/or increase in marginal return could very well be made up of more “marginal” students; i.e., students with lower learning potential. This selection effect will tend to lower the average skill of students.

4. Identification

4.1. Identification assumption

The identification strategy builds on two assumptions. First, prior to 1998—before 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. Controlling for time and school fixed effects, our strategy is thus to use distance × timing as an instrument for exposure (access to information). We assess the validity of this instrument procedure next.

9 Thomas (1998, 1999) argues that power in district governments is concentrated among a small elite, connected by common schooling, marriage, friendships, ethnicity, 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 at the time, must also supply patronage goods to their members. In the 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.

10 While there is some variation, the majority of schools reported that the capitation grants were used to fund scholastic materials and for school management.
4.2. Instrument validity

The IV method we employ; i.e., using distance to the nearest newspaper outlet after 1997 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 the distance to the nearest newspaper outlet on change in outcomes prior to the information campaign. Thus we estimate the following difference-in-differences specification

\[ y_{jt} = \beta_1 x_{jt} + \beta_2 \alpha_{1995} \text{distance}_j + \sum_{t} \gamma_t \delta_t + \mu_t + \epsilon_{jt}, \]

for \( t = \{91, 95\} \), where \( y_{jt} \) is outcome (share of funding reaching the school, enrollment, and average test scores) in school \( j \) at time \( t \), \( x_{jt} \) is a vector of controls, \( \alpha_{1995} \) is a indicator variable for year 1995, \( \text{distance}_j \) is the distance (log) in kilometers to the nearest newspaper outlet, \( \delta_t \) is a time dummy, \( \mu_t \) are school-specific fixed effects, and \( \beta_2 \) is the reduced-form estimate. In the base specification we include income as a control.\(^{11}\)

The estimates of Eq. (1) are reported in Table 4.\(^{12}\) Proximity to a newspaper kiosk is uncorrelated with changes in the capture over the 5-year period prior to the reform (column 1). The estimates are small and insignificant. Likewise, there is no evidence of differential trends in grade 7 enrollment (column 2) or in average Primary Leaving Exam scores (column 3) in the pre-campaign 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 other reforms in the later part of the 1990s. This is a valid concern because apart from the information campaign the education sector experienced another major change in the 1990s. In 1997 the Government of Uganda abolished primary school fees (the universal primary education, UPE, reform).\(^{13}\)

If newspaper exposure also influenced the effect of the UPE reform, our exclusion restriction will not hold. There are a number of potential mechanisms. For instance, if newspapers are more readily available in urban areas, the size of the potential pool of students affected by UPE may differ by area (close or not to a newspaper kiosk). As newspaper outlets are not randomly placed in a given area, this is in general a valid concern. However, in our sample of rural schools this is likely to be less of a problem. By design, we do not have a mixed sample of urban schools (close to newspaper outlets) and rural schools (far from a newspaper outlet). Moreover, we look at P7 students who started school before 1997 and thus benefitted relatively less from the UPE reform. Another argument would be that parents living closer to a newspaper outlet may be more likely to know about free primary education and thus to enroll their children. Similarly, access to a newspaper kiosk could have affected the degree to which the UPE reform was enforced at the school level. However, from the 2002 survey we know that there was no variation in knowledge about the UPE reform (all head teachers knew they were prohibited from charging school-fees), but as shown below, there was a fair amount of variation in the knowledge about the grant program. Moreover, UPE was one of the salient issues for voters in the 1996 election (Stasavage, 2003). Finally, the household survey data suggest that there is no relationship between newspaper exposure and school fees in the post-UPE period (results available upon request).

To test the exclusion restriction; i.e., that the effects are not influenced by the UPE reform, we exploit the fact that the UPE reform was implemented a year ahead of the newspaper campaign. Changes in the outcome in 1997, or between 1995 and 1997, cannot be attributed to the newspaper campaign but should be affected by the UPE reform. A graphical representation of the “parallel trend” assumption is presented in Fig. 1. Fig. 1 plots the point estimates from the regressions of grade 7 enrollment on distance for the years prior to the newspaper campaign, including the first year of the UPE reform. As evident, the point estimates are similar, suggesting that the parallel trend assumption holds. That is, there are no differential trends across treatment (i.e. schools close to a newspaper outlet) and control groups in the post-UPE but pre-campaign period. The data also reveal a break in the trend after the newspaper campaign became operational, consistent with the view that the newspaper campaign influenced the extent of corruption and thereby indirectly school enrollment.

Fig. 2 plots the point estimates from the regressions of grade 3 enrollment on distance. Assuming no grade repetition, students in grade 7 in 2001 were in grade 3 in 1997.\(^{14}\) Again, there is no evidence of a differential effect of being closer to a newspaper outlet prior to the newspaper campaign.

<table>
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<th>Specfication</th>
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<th>Share of grants received</th>
<th>Grade 7 enrollment</th>
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</table>

Note: robust standard errors clustered by school in parenthesis.

*** Denotes statistically significant at 1% level.

** Denotes statistically significant at 5% level.

* Denotes statistically significant at 10% level.

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11 Following Reinkikka and Svensson (2004), we use national household survey data to derive mean consumption level across district-urban-rural locations to proxy for income.

12 Because of concerns about misreporting, we subject the data to a filtering rule. Namely, for each specification, we determine the multivariate outliers (for all specified variables) according to the method of Hadi (1992). The observations that were flagged as outliers were excluded. Including outliers, the estimates and standard errors increase in most specifications.

13 Universal primary education (UPE) became a cornerstone in President Museveni’s election manifesto in 1996 and many observers argue that it was the salient issue for voters in the 1996 election (see Stasavage, 2003). Museveni won the election and in December 1996 the Government of Uganda announced that, starting in 1997, primary school fees would be abolished. The reform prohibited schools from charging any type of school-fees, with the caveat that “only” four children per household could benefit from UPE in a given year. In practice, this rule was not enforced and UPE became synonymous to free primary schooling for all (Stasavage, 2003).

14 The grade 3 enrollment numbers are from the Uganda Ministry of Education’s Education Management Information System (EMIS).
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.

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 then estimate:

$$q_j = \pi_0 x_j' + \pi_1 \text{distance}_j + \epsilon_j,$$

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 the distance to the nearest newspaper outlet has a strong negative effect on the head teachers’ knowledge about the grant program in general (specification 3).

A concern with these results is that head teachers serving in schools close to a newspaper outlet may be more knowledgeable in general, and schools with more knowledgeable head teachers may suffer less from the 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 assess this hypothesis, 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 the head teachers serving in schools further away (see specification 4 in Table 6). But for the head teachers' knowledge of local affairs and knowledge of general (sociopolitical) issues typically not reported in newspapers, there is no significant difference between the head teachers in schools close to a newspaper outlet and those in schools farther from one (see specifications 5 and 6).

Though the tests do not provide a comprehensive assessment of the head teachers' knowledge and abilities, the findings suggest that it is information on the grant program disseminated through newspapers that head teachers in schools close to a newspaper outlet are, as would be expected, significantly more likely to access 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.

4.3. Specification

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

We start by looking at the first-stage relationship; i.e., the relationship between the share of funding reaching the school, \( s_{jt} \), and distance in the post-campaign period. That is we estimate

\[
s_{jt} = \lambda_1 x_{jt} + \lambda_2 \sigma_{2001}\text{distance}_j + \sum_t \tau_t \delta_t + \mu_t + \epsilon_{jt}^f \quad \text{for } t = (95, 01),
\]

where \( \sigma_{2001} \) is a binary variable taking the value 0 in year 1995 and 1 in year 2001, \( \mu_t \) is a school-specific fixed effect, and \( \delta_t \) is a time dummy. The structural equation is:

\[
\text{grade 7 enrollment}_j = \alpha_0 + \alpha_1 \text{distance}_j + \alpha_2 \sigma_{2001}\text{distance}_j + \Delta \epsilon_{jt}^f \quad \text{for } t = (95, 01).
\]

where we instrument for the share of funding reaching the school, \( s_{jt} \), using the interaction term \( \epsilon_{jt}^f \cdot \text{distance} \), as instrument. With two years of data we can difference Eqs. (3) and (4) to eliminate the school fixed effects and thus estimate:

\[
\Delta \epsilon_{jt}^f = \lambda_1 \Delta x_{jt} + \lambda_2 \text{distance}_j + \Delta \epsilon_{jt}^f
\]

\[
\text{grade 7 enrollment}_j = \alpha_0 + \alpha_1 \Delta x_{jt} + \alpha_2 \text{distance}_j + \Delta \epsilon_{jt}^f.
\]

Since we have data on enrollment and distance for more years and schools than we have data on the capture, we also look at the reduced form:

\[
\text{grade 7 enrollment}_j = \alpha_0 s_{jt} + \alpha_1 \text{distance}_j + \alpha_2 \sigma_{2001}\text{distance}_j + \epsilon_{jt} \quad \text{for } t = (95, 97, 01, 02),
\]

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

In the base specification we include income as control. In the robustness section we show that the results are robust to the inclusion of various other covariates.

5. Results

5.1. Enrollment

Table 7, column 1, depicts the estimates from the first-stage regression (3'). 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. 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 Fig. 3). As an example, a school close to a newspaper outlet suffers 25 percentage points less from the capture as compared to a school one standard-deviation (in kilometers) further away from a newspaper outlet.

In column 2 we add time-by-region fixed effects to take into account that there might be time varying differences across regions. A school's exposure to the information campaign is now identified by the within region variation in the difference in the distance before and after the information campaign. The point estimate remains significant.

Column 3 reports the reduced-form relationship between the distance to the nearest newspaper outlet and school enrollment in the smaller sample for which we have complete survey data (data on grant diversion in 1995 and 2001); i.e., the estimates of (4'), with \( \Delta \epsilon_{jt}^f \) replaced by the instrument \( \epsilon_{jt}^f \). The reduced form estimate is negative and fairly precisely estimated.
The IV estimate is reported in column 4. The reduction in corruption is associated with a statistically significant increase in enrollment. A one-standard deviation increase in the share of funding reaching the school is associated with a 0.48 standard deviation increase in grade 7 enrollment.

In column 5 we exploit the larger sample (at the cost of having to randomize design. To see this note that in the average school (in columns 5–7) in parenthesis. Imputed Wald/IV estimate in brackets. FD is first differencing, FE is fixed effects, and IV is instrumental variables.

Notes: (1) robust standard errors (in cols. 1–4) clustered by school (in cols. 5–7) in parenthesis. Imputed Wald/IV estimate in brackets. FD is first differencing, FE is fixed effects, and IV is instrumental variables.

*** Denotes statistically significant at 1% level.
** Denotes statistically significant at 5% level.
* Denotes statistically significant at 10% level.
Assuming that the additional funding became available when the newspaper campaign was rolled out, the cohort of grade 7 students in 2001 would have benefited from the reduced capture for four years. Taking this into account, the estimate rises to US$28.4.

Kremer et al. (2009) compare the cost effectiveness of various ways to spend money to keep children in school based on randomized trials. Some of these interventions had no effect on school participation, implying the infinite cost per participation gain, but others did. Deworming, at an average cost of 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, additional spending, but that a higher share of a given amount spent

5.2. Test scores

Table 8 presents evidence on the impact of the newspaper campaign on cognitive skills. We estimate (4′) and (7) with the average Primary Leaving Exam score for school j at time t as a dependent variable.

As discussed above, while it seems plausible to expect a positive relationship between more funding (as a result of a fall in corruption) and enrollment, the same is not true for the average test scores. Average test scores would improve if the positive effect of higher quality due to more/better inputs outweighs the potentially adverse effect due to a compositional change of students.

The IV and reduced-form estimates are reported in columns 1–3 and 4–6, respectively. We report both pooled results and the results separately for boys and girls. Students in schools closer to a newspaper outlet and may be able to attract more funding.18

A comparison with recent randomized evaluations again suggests that funds have been put in productive use. Comparing learning outcomes in a school (in 2001) that received 80% of its entitlement with those of a school that received its full entitlement, the average test scores are 1.1 points higher. Assuming the students only benefited from the reduced capture in 2001, this implies a 0.1 standard deviation gain for each US$0.47 per student that reached the school. Assuming the students benefited from the reduced local government capture fairly effectively with the roll out of the newspaper campaign, the estimate per 0.1 standard deviation gain is US$1.88. As a reference point, the costs of the programs considered by Kremer et al. (2009) range from US$1.77 to US$4.01. The remedial education program in India, evaluated in Banerjee et al. (2007), costs US$0.67 per 0.1 standard deviation gain. It is worth emphasizing that since the point estimates are not very precisely estimated, the 90% confidence interval of our estimates also includes much lower effects and thus higher costs per 0.1 standard deviation gain.

5.3. Additional robustness tests and interpretation

Based on data from the head teachers in the 2002 survey, there is no variation in knowledge about the UPE reform (all head teachers know they are prohibited from charging school-fees), but there is a fair amount of variation in the knowledge about the grant program (see Table 6). This suggests that at least from the head teachers’ point of view, distance is not capturing the differential knowledge about free primary education.

Additional support for the identifying assumption is provided in Table 9. “Politically connected” schools or communities may be closer to a newspaper outlet and may be able to attract more funding.18

Distance may also proxy for some other geographical characteristic, such as remoteness, that could influence the 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 augment Eqs. (3′), (4′), and (5) with a proxy of remoteness: distance to the nearest bank branch (a proxy for distance to the nearest urban center), interacted with the binary variable $\sigma_{post-campaign}$. We also experiment with two measures of

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**Table 8** Estimates of the effects of the newspaper campaign on test scores.

<table>
<thead>
<tr>
<th>Specification</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dep. variable</td>
<td>Average primary leaving exam score</td>
<td>Average primary leaving exam score</td>
<td>Average primary leaving exam score</td>
<td>Average primary leaving exam score</td>
<td>Average primary leaving exam score</td>
<td>Average primary leaving exam score</td>
</tr>
<tr>
<td>Sample</td>
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<td>Boys</td>
<td>Girls</td>
<td>All</td>
<td>Boys</td>
<td>Girls</td>
</tr>
<tr>
<td>FD-IV</td>
<td>FD-IV</td>
<td>FD-IV</td>
<td>FD-IV</td>
<td>FE</td>
<td>FE</td>
<td>FE</td>
</tr>
<tr>
<td>Share of grants received</td>
<td>0.05</td>
<td>0.05</td>
<td>0.03</td>
<td>(0.08)</td>
<td>(0.09)</td>
<td>(0.08)</td>
</tr>
<tr>
<td>Distance × $\sigma_{01,02}$</td>
<td>−0.50$^*$</td>
<td>−0.40$^*$</td>
<td>−0.51$^*$</td>
<td>(0.29)</td>
<td>(0.29)</td>
<td>(0.31)</td>
</tr>
<tr>
<td>Time effects</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>School effects</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Controls</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Region fixed effects × $\sigma_{01,02}$</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
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<td>169</td>
<td>167</td>
<td>1319</td>
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<td>1305</td>
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<tr>
<td>Schools</td>
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<td>169</td>
<td>167</td>
<td>374</td>
<td>371</td>
<td>373</td>
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</tbody>
</table>

Notes: (i) robust standard errors clustered by school in parenthesis. Income is included as control. Imputed Wald/IV estimate in brackets.

*** Denotes statistically significant at 1% level.
** Denotes statistically significant at 5% level.
* Denotes statistically significant at 10% level.
political connectedness; binary variables indicating if the school has a local council official (a politician) in their school-management committee; and if the school has received discretionary support for school construction from the local government or other sources. We allow these variables to have a time-variant effect on school outcomes.

Once controlling for proximity to a newspaper outlet, none of the interaction terms enter significantly while the estimates in the first-stage, reduced-form, and IV specifications remain significant, albeit somewhat less precisely estimated.

The enrollment and test score results 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 the reduced capture had an impact, so sorting is primarily an endogenous variable.

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 a high 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 the capture of public funds by providing schools (parents) with information to monitor local officials’ handling of a large education grant program.

We find that student enrollment increased in schools that, as a result of the information campaign, managed to claim a higher share of their entitlements. We also find positive, albeit weaker, effects on achievement. A number of specification checks support the causal interpretation of these estimates.

The capitation grant (to cover primary schools’ nonwage expenditures) is a fairly common spending program in developing countries, and at least 60 public expenditure tracking surveys and related quantitative service delivery surveys have been implemented in around 30 countries, including often followed by information dissemination or other social accountability interventions, such as citizen report cards (Gauthier and Reinikka, 2007). In this respect, the results in the paper have some external validity. A similar newspaper campaign has been implemented over the past decade, for example, in Tanzania whose education system has many similar characteristics. Kenya has also used a variance of the Uganda experience in its UPE program. While there has not been careful impact evaluations of the Tanzanian and Kenyan information campaigns on school enrollment or learning outcomes, it is likely that similar results hold there, too. However, for the Uganda results to have some external validity. A similar newspaper campaign has been implemented over the past decade, for example, in Tanzania whose education system has many similar characteristics. Kenya has also used a variance of the Uganda experience in its UPE program.

Table 9

<table>
<thead>
<tr>
<th>Dep. variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
<th>(9)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Share of grants received</td>
<td>FD</td>
<td>FD-IV</td>
<td>FE</td>
<td>FD</td>
<td>FD-IV</td>
<td>FE</td>
<td>FD</td>
<td>FD-IV</td>
<td>FE</td>
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<tr>
<td>Distance to newspaper outlet × c₁0₁,0₂</td>
<td>–5.59⁎⁎⁎</td>
<td>–7.84⁎⁎⁎</td>
<td>–7.91⁎⁎⁎</td>
<td>1.02⁎⁎</td>
<td>0.73⁎⁎</td>
<td>0.71⁎⁎⁎</td>
<td>–4.05⁎⁎⁎</td>
<td>–3.41⁎⁎⁎</td>
<td>–3.31⁎⁎⁎</td>
</tr>
<tr>
<td>Distance to urban center × c₁0₁,0₂</td>
<td>–4.01</td>
<td>4.89</td>
<td>5.56</td>
<td>1.23</td>
<td>2.05</td>
<td>1.06</td>
<td></td>
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<tr>
<td>Local council official in SMC × c₁0₁,0₂</td>
<td>4.36</td>
<td>2.33</td>
<td>–6.33</td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Receipt of discretionary support × c₁0₁,0₂</td>
<td>7.82</td>
<td>5.27</td>
<td>8.20</td>
<td>4.66</td>
<td></td>
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<tr>
<td>Time effects</td>
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</tbody>
</table>

Notes: robust standard errors (in cols. 1–6) clustered by school (in cols. 7–9) in parenthesis. Income is included as control. FD is first differencing, FE is fixed effects, and IV is instrumental variables.

⁎⁎⁎ Denotes statistically significant at 1% level.
⁎⁎ Denotes statistically significant at 5% level.
⁎ Denotes statistically significant at 10% level.

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This may not the case in other sectors or many other countries. Second, the UPE reform, and the political campaign surrounding it, may have created a context particularly favorable to the information campaign’s success. Survey data also suggest that most households prioritize learning and whether a campaign sustained over longer duration generates greater impact on the school outcomes.

19 The fact that more funding resulted in improvements in the quantity of education but had less of an impact (or at least a less precisely estimated impact) on the quality of education is consistent with the recent literature on randomized evaluation (see for instance Glewwe and Kremer, 2006; Duflo et al., 2009).
education over other social services (Stasavage, 2003) which may not be the case in all countries.

The impact on quantity of education 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, differs in two important aspects from most of the existing literature. First, the paper focuses on the 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). Yet another difference is that we look at the compounded effect over a few years.

It should be noted that the comparison of cost estimates is not straightforward since the intervention we exploit did not 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 the capture and diversion of funds had been reduced as a result of the campaign on whatever goods or services 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), yet another difference is that we look at the compounded effect over a few years.

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 case is that experimentation and evaluation of new tools to enhance accountability should be an integral part in the research agenda on improving the outcomes of social services.

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