

The Power of Information in Public Services: Evidence from Education in Uganda

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In this paper we argue that innovations in governance of social services is 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 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 capture of funds at the local level and that the reduction in capture of funds that resulted had a positive effect on school enrollment and learning outcomes. (JEL: D73, I22, O12)

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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 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 to start building consensus of what works and what does not. 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. And since social 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 school 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 percent 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 survey data and administrative data from the Ugandan Ministry of Education, we link school enrollment and average primary leaving exam scores with data on capture of funds and distance to 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 capture of funds at the local level and that the reduction in 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

¹We are not aware of any other studies that explicitly test how changes (improvements) in governance affect school-specific outcomes (enrollment and student learning). There are however several studies on policy formation at the local level in developing countries (see, for example, Duflo and Chattopadhyay, 2004) and on education policies and the financing of schools in particular (see, for example, Kremer et al., 2002).

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 a survey see Glewwe, 2002, and Kremer and Glewwe, 2006) as well as a small but growing microempirical literature on corruption.² It adds to the former by focusing on governance of social services and by exploring 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.

The next section describes the data used in the empirical analysis and the method used to quantify 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 reduction in capture of funds on school enrollment and learning results. Section 6 concludes.

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 2002.³ Both surveys collected detailed information on grant receipts (the 1996 survey collected information for the period 1991-1995 and the 2002 survey collected data for 2001) and enrollment. The 2002 survey also collected data on knowledge of the grant program by administering a test to head teachers, the means to acquire information on the grant program, and other variables that may influence the bargaining position of individual schools.

In both surveys, the grant data collected at the school level appear to adequately reflect what schools actually received. The data were collected

²For a discussion of this literature, see Svensson (2005). Contributions dealing with public corruption include Di Tella and Schargrotsky (2003), Hsieh and Moretti (2006), Olken (2006, 2007), Reinikka and Svensson (2004), and Svensson (2003).

³For a discussion of the 1996 public expenditure tracking survey, see Reinikka (2001) and Reinikka and Svensson (2004). A more general discussions of these types of service provider surveys can be found in Dehn et al. (2003) and Gauthier and Reinikka (2007).

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

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

The 2002 survey expanded the sample with an additional 170 schools from 9 of the original 18 districts.⁴ 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.⁵ We thus have a sample of 218 schools for which survey data are available for the years 1991–95 and 2001, and a sample of 388 (218 + 170) schools for which survey data are available in 2001.

The school-specific measure of 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

⁴The sampling frame for these additional schools was based on the 2001 school census, and the sampling procedure was similar to that of the 1996 survey. Specifically, a stratified random sample was chosen where each district was weighted according to size (number of schools). Thereafter, one district was randomly chosen from each region. Two additional districts were then selected from the two largest regions. The number of schools to be sampled from each of these 9 districts was proportional to the number of schools in the district.

⁵An additional complication was that since the 1996 survey, four districts had been split, thus yielding a sample of 22 districts.

number of enrolled students according to district records.⁶ In 1995 the grant formula allocated 2,500 Ugandan shillings (USh) a year for each student in grades P1–P3 and 4,000 USh for each student in grades P4–P7. In 2001 the amounts were 5,000 USh for grades P1–P3 and 8,100 USh for grades P4–P7.⁷ Records from the Ministry of Finance indicate that the entitlement rule was followed unless districts did not submit the required quarterly documentation, in which case funds could be delayed or withheld in the following months. These records show that in fiscal year 2000/01, 93 percent of the approved funds were released, although some districts received significantly less (for example, the central government withheld 49 percent of the funds to Kyenjojo and 25 percent to Kayunga, both newly established districts). The actual amounts disbursed by the central government were confirmed by the public expenditure tracking survey at the district level. To adjust for the withholding effect, a school’s entitlement was scaled down by the share of funds actually released by the center to the district.

Summary statistics on capitation grants received as share of entitled grants are reported in table 3. As evident, the situation has improved dramatically since the mid-1990s. Schools, which had received only 24 percent on average of the total yearly grant from the central government in 1995 (in the pre-campaign period), received more than 80 percent in 2001 (post-campaign period).⁸ More striking, while the median school received nothing in the mid-1990s, it received 82 percent of its entitlement in 2001. Thus the extent of diversion fell dramatically. However, diversion is still a problem for many schools. On average, 20 percent of school entitlements do not reach the schools, and about 30 percent of schools receive less than two-thirds of their entitlements.

We focus on enrollment of grade 7 students (P7). There are several rea-

⁶A concern is that the survey data could be inflated, although attempts have been made to ensure this was not the case. Note though that if the incentives to misreport are constant across schools, this is less of a concern since we look at the difference across schools. It is possible that the more informed schools are the more likely they are to inflate enrollment numbers (because they realize the total grant is a function of number of students enrolled). However, if the district officials are at least partly aware of these incentives, this would tend to work against finding an effect of the newspaper campaign. The informed schools would then have high reported enrollment numbers but also suffer from extensive capture (if the district disburse the grant based on the actual rather than the reported numbers).

⁷The 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).

⁸A handful of schools had missing financial data in 1995 but 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 1995 data.

sons 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 Primary Leaving Exams (PLE) records from the Uganda National Examination Bureau. The PLE data contain information on scores on standardized tests (primary leaving exams) in Math, English, Science, and Social Studies conducted yearly on all grade 7 students. Our measure of grade 7 enrollment is number of grade 7 students taking this test. We complement the grade 7 enrollment numbers with enrollment data for grades 1-6 from the Uganda Ministry of Education’s Education Management Information System (EMIS).

We also derive our measure of cognitive skills from Primary Leaving Exams 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 Primary Leaving Exams 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 survey data are available in 2001.

Enrollment has been increasing over time in Uganda (see table 2). Average primary leaving exam scores, however, show little improvement over time.

Data on distance to the nearest newspaper outlet is taken from the school survey. We denote by $distance_j$ the distance (in logs) in kilometers to the nearest newspaper outlet as reported by the head teacher in school j . During piloting we compared head teachers’ assessment of distance to nearest newspaper outlet with measured (by enumerators) distance. We concluded that in most cases the two measures were similar.⁹

⁹A newspaper outlet is defined as an outlet/shop where they regularly sell newspapers. In a predominately rural country like Uganda, regular access to newspaper is mainly determined by logistic 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 (2006a) reports that the distribution of newspapers across districts has been stable during the period 1996 to 2002.

3 Institutional setup

3.1 Pre-campaign period

For a long time, Uganda has had a national policy of financing instructional material and other non-wage spending at primary schools through a capitation grant. The grant is a nationally set annual allocation per student and is intended to go to the schools, either in-kind or as a direct financial transfer. 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 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 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 officers 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. Case study evidence of district political financing and corruption in Uganda also points in the same direction.¹⁰ The diversion of funds was also facilitated by the ignorance about the disbursements of capitation grant in most schools.

¹⁰Thomas (1998, 1999) argues that power in district governments is concentrated among a small elite, connected by common schooling, marriage, friendships, ethnicities, and religion. Sustaining this power balance is costly. Public funds are fueling a system of patronage politics, in which patrons give clients material rewards for their political loyalty and services (see also Bayart 1993). The patronage system takes different forms. Politicians divert public resources to finance their own campaigns and those of friends and family or to finance local and private causes, including distribution of private goods such as salt, sugar, and beer to overcome voter dissatisfaction. Political parties, or in Uganda, the National Resistance Movement, must also supply patronage goods to its members. In rural areas an effective political organization depends on a personal presence in the area, which means a well-staffed institutional hierarchy all the way down to the village level. This model requires substantial resources, and diversion of public resources is often the only source of funding available.

3.2 The information campaign

The possibility to use the main media outlets, and specifically newspapers, as instruments to strengthen relationships of accountability in the education sector was discussed in the policy circles in Uganda in response to the findings of the initial public expenditure tracking survey (see Collier, 2007). Towards the end of 1997, the government began to systematically publish data in the national newspapers on the monthly transfers of capitation grants to districts. The main newspapers used were the *The New Vision* and *The Monitor*. Apart from detailed data on transfers of education funds to the districts, newspapers published information on school entitlements and responsibilities, and on occasions also carried stories on misuse of the capitation grant funds.¹¹

The main objective of the newspaper campaign was to provide information on the grant program, including when funds were being transferred to the districts, so as to enable head teachers and parents to monitor the local administration and 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 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 decisionmaking in the form of school management committees, consisting of parents and the head teacher. Finally, communities have different ways to sanctions public officials/politicians, ranging from informal forms of social pressure (verbal complaints) to formal ones, such as local electoral sanctions (local politicians fear of losing an election) to career concern (public officials fear of losing their job

¹¹The information campaign also involved sending out leaflets on the workings of the capitation program to schools and informing schools and parents through radio. However, the radio stations did not systematically report information on when 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.

or not getting promoted). In Uganda in the late 1990s, district politicians were elected in (semi) competitive elections. Local politicians, in turn, had discretion over remuneration, hiring and firing decisions of education and accounting officers at the district level. In addition, by publicly informing beneficiaries of their entitlements, the central government signaled strengthened oversight (to voters and local officials) and the priority it accorded to education (Stasavage 2003). Thus, as a community became better informed, district officials may have rationally believed that the threat of punishment increased if funds were captured and, consequently, had incentives to reduce capture of funds intended for the school.

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

3.2.1 Predicted impact

A reduction in capture or corruption, and hence an increase in funding, could impact schooling outcomes in various ways. First, it may enable to school management to raise the quality of education. Second, it might reduce costs, to the extent that household education expenditures and government non-salary grants to schools are substitutes as suggested by Das et al. (2004).¹² 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

¹²While there is some variation, the majority of schools report that the funding was used to fund scholastic materials and for school management.

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 $\text{distance} \times \text{timing}$ as an instrument for exposure (access to information). We assess the validity of this instrument procedure next.

4.2 Instrument validity

The IV method we employ; i.e., using *distance to nearest newspaper outlet after 1998* as instrument, makes the counterfactual assumption that absent the newspaper campaign, school funding and outcomes (enrollment and learning) would change at a rate independent of proximity to a newspaper kiosk. Although we cannot test this directly, we can look at the reduced-form relationship between distance to the nearest newspaper outlet on change in outcomes *prior* to the information campaign. Thus we estimate the following difference-in-differences specification

$$y_{jt} = \beta_1 x_{jt} + \beta_3 \sigma_{1995} \text{distance}_j + \sum_t \gamma_t \delta_t + \mu_j + \varepsilon_{jt}^y, \quad (1)$$

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, σ_{1995} is a indicator variable for year 1995, distance_j is the distance (log) in kilometers to the nearest newspaper outlet, δ_t is a time dummy, μ_j are school-specific fixed effects, and β_3 is the reduced-form estimate. In the base specification we include income as control.¹³

¹³Following Reinikka and Svensson (2004), we use national household survey data to derive mean consumption level across district-urban-rural locations to proxy for income.

The estimates of (??) are reported in table 4.¹⁴ Proximity to a newspaper kiosk is uncorrelated with changes in 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 exams 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).¹⁵ 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 construction, 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 the UPE reform primarily affected students that had not yet begun school. 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 is no variation in knowledge about the UPE reform (all head teachers know they are prohibited from charging school-fees), but as shown below, there is a fair amount of variation in knowl-

¹⁴Because of concerns with misreporting, we subject the data to one filtering rule. Specifically, for each specification, we determined the multivariate outliers (for all specified variables) according to the method of Hadi (1994) and the observations that were flagged as outliers were consequently excluded.

¹⁵Universal primary education (UPE) became a cornerstone in President Museveni's election manifesto in 1996 and many observers argue that it was the salient issues for voters in the 1996 election (see for instance Stasavage, 2003). Museveni won the election and in the fall of 1996 the Government of Uganda announced that, starting in 1997, primary school fees would be abolished. The reform prohibited schools 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 synonym to free primary schooling for all (Stasavage 2003).

edge about the grant program. Moreover, UPE was one of the salient issues for voters in the 1996 election (Stasavage, 2003). Finally, 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 head of the newspaper campaign. Changes in 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 figure 1. Figure 1 plots the point estimates from 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.

Figure 2 plots the point estimates from regressions of grade 3 enrollment on *distance*. Assuming no grade repetition, students in grade 7 in 2001 were in grade 3 in 1997. Again, there is no evidence of a differential effect of being closer to a newspaper outlet prior to the newspaper campaign.

Table 5 reports the regression versions of figures 1 and 2; i.e., the results of estimating (??), with σ_t taking the value 0 in 1995 and 1 in 1997. We look at enrollment in grades 3 and 7, and test scores (primary leaving exams). Note that because we do not have survey data for 1997, we cannot examine changes in capture.

In all specifications, the estimated "placebo" effects are insignificant and close to zero.

To constitute a valid instrument, *distance* must also be correlated with degree of exposure. As reported in the working paper version, *distance* is highly correlated with head teachers' access to newspapers. A head teacher in a school near a newspaper outlet is 35 percentage points more likely to report access to a newspaper than a head teacher in a school one standard deviation further away from such an outlet.

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:

$$q_j = \pi_0 x'_j + \pi_1 distance_j + \varepsilon_j^q, \quad (2)$$

where q_j is the score on the knowledge tests.

Head teachers serving in schools close to a newspaper outlet are better informed about the formula used for deriving the capitation grant (table 6, specification 1). They are also better informed about the timing of releases of funds by the central government (specification 2). When both of these are combined into an aggregate score (*info*), the results show that distance to the nearest newspaper outlet has a strong negative effect on head teachers' knowledge about the grant program in general (specification 3).

A concern with these results is that head teachers serving in schools close to a newspaper outlet may be more knowledgeable in general, and schools with more knowledgeable head teachers may suffer less from capture.¹⁶ 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 head teachers serving in schools further away (see specification 4 in table 6).¹⁷ But for head teachers' knowledge of local affairs¹⁸ and knowledge of general (sociopolitical) issues typically not reported in newspapers, there is no significant difference between head teachers in schools close to a newspaper outlet and those in schools farther from one (see specifications 5 and 6).¹⁹

¹⁶While 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.

¹⁷Respondents were asked to identify Winnie Byanyima (outspoken Member of Parliament and the wife of a former presidential candidate), Thabo Mbeki (president of South Africa), Bono (rock singer who was touring Africa at the time of the survey), George W. Bush (president of the United States), and Charles Onyango-Obbo (outspoken editor of *The Monitor*). Respondents were also asked to name the minister of education (Hon. Makubuya), minister of finance (Hon. Ssendaula), and prime minister (Hon. Nsibambi). Head teachers with access to newspapers were significantly more likely to respond correctly to each question (except the question on President Bush, for which there was no significant difference). The variable "knowledge about recent news events" is the average score on these eight questions, where a correct answer is coded 1 and an incorrect answer is coded 0.

¹⁸Respondents were asked to name the district education officer, chief administrative officer in the district, Local Council 5 chairman, Local Council 3 chairman, and their representative in Parliament. Head teachers in schools close and farther way from newspaper outlets were equally as likely to respond correctly to these questions. The variable "knowledge about local affairs" is the average score on these five questions, where a correct answer is coded 1 and an incorrect answer is coded 0.

¹⁹Respondents were asked the following questions: what is the (a) largest newspaper by circulation in Uganda (*The New Vision*), (b) location of East African Parliament (Arusha, Tanzania), (c) MTN (cellular/mobile phone provider), (d) month when the government's

Though the tests do not provide a comprehensive assessment of head teachers’ knowledge and abilities, the findings suggest that it is information on the grant program disseminated through newspapers and correlated with *distance* that accounts for the observed effects rather than some unobserved characteristic such as teachers’ ability. Three additional tests support this claim (results available upon request): The relationship between knowledge about the grant program and proximity to a newspaper outlet remains intact when controlling for differences in income and region fixed effects. Second, *distance* does not seem to proxy for remoteness, which may have an independent effect on a school’s ability to claim funds from the district. The strong relationship between *distance* and information about the grant program remains once controlling for distance to district headquarters (a proxy for distance to the district capital) and distance to the nearest bank branch (a proxy for distance to the nearest urban center). Finally, distance to the nearest newspaper outlet has an independent effect on *info* over and above increasing the likelihood that the head teacher has access to a newspaper. This result is consistent with the claim that a head teacher may be well-informed about the grant program even without having newspapers, if parents in the community where the school is located have access to them.

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

$$s_{jt} = \lambda_1 x_{jt} + \lambda_2 \sigma_{2001} distance_j + \sum_t \tau_t \delta_t + \mu_j + \varepsilon_{jt}^s \quad \text{for } t = \{95, 01\}, \quad (3)$$

where σ_{2001} is a binary variable taking the value 0 in year 1995 and 1 in year 2001, μ_j is a school-specific fixed effect, and δ_t is a time dummy.

The structural equation is:

$$grade\ 7\ enrollment_{jt} = \alpha_1 x_{jt} + \alpha_2 s_{jt} + \sum_t \theta_t \delta_t + \mu_j + \varepsilon_{jt}^e \quad \text{for } t = \{95, 01\}, \quad (4)$$

where we instrument for the share of funding reaching the school, s_{jt} , using the interaction term $\sigma_{01} distance_j$ as instrument. With two years of data we

budget is presented to Parliament (June), (e) number of districts in Uganda (56), and (f) number of members of Parliament (305). The variable “general political knowledge” is the average score on these six questions, where a correct answer is coded 1 and an incorrect answer is coded 0.

can difference equations (??)-(??) to eliminate the school fixed effects and thus estimate:

$$\Delta s_j = \lambda_1 \Delta x_j + \lambda_2 \text{distance}_j + \Delta \varepsilon_j^s \quad (3')$$

$$\Delta \text{grade 7 enrollment}_j = \alpha_0 + \alpha_1 \Delta x_j + \alpha_2 \Delta s_j + \Delta \varepsilon_j^e, \quad (4')$$

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

$$\text{grade 7 enrollment}_{jt} = \omega_0 x_{jt} + \omega_1 \text{distance}_j \sigma_{01,02} + \sum_t \phi_t \delta_t + \mu_j + \varepsilon_{jt}, \quad (5)$$

for $t = \{95, 97, 01, 02\}$, where $\sigma_{01,02}$ is a binary variable taking the value 1 in the post-campaign years 2001 and 2002 and 0 otherwise (in 1995 and 1997).

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 figure 3). As an example, a school close to a newspaper outlet suffers 25 percentage points less from capture as compared to a school one standard-deviation (in kilometers) further away from a newspaper outlet.

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 distance before and after the information campaign. The point estimate remains significant, albeit less precisely estimated.

Column 3 reports the reduced-form relationship between 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 Δs_{jt} replaced by the instrument *distance_j*. 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 rely on the reduced form). The reduced form estimate is slightly smaller (in absolute values) than the estimate in the smaller sample (column 3) but precisely estimated. Combining the first-stage and the reduced form estimates, we also report "imputed" Wald/IV estimates for the large sample (reported in brackets).²⁰

The specification in column 6 restricts the sample to the post-UPE period (1997, 2001, and 2002). The estimate changes only marginally, consistent with the findings of the placebo experiment reported in table 5.

A back-of-the-envelope calculation suggests that the impact of making more resources available at the schools (through reduced local government capture) is of the same order of magnitude as some of the more cost-effective school interventions that have been evaluated based on randomized design. To see this note that in the average school (in 2001), 51 students completed grade 7 and since the school received approximately 80% of its entitlement, the total amount of non-salary inputs for the grade 7 cohort was US\$192 (US\$3.76 per student). If the school had received its full entitlement, it would have received almost one dollar more per student and the impact estimate (table 7, column 4), imply that 65 students would then have completed grade 7. Fully funded, the grade 7 cohort would in total have received US\$306 (US\$4.70 per student). In other words, one additional student completed grade 7 for each US\$8.1 that reached the school through reduced capture.²¹ Assuming that the additional funding became available when the newspaper campaign was rolled out in 1998, the cohort of grade 7 students in 2001 would have benefited from reduced capture for four years. Taking this into account, the estimate rises to US\$28.4.

Kremer, Miguel, and Thornton (2004) 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 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

²⁰It is an "imputed" Wald estimate since the two samples differ.

²¹ $(US\$306 - US\$192) / (65 - 51) = US\$8.1$.

the projects studied. The costs per additional year of school participation of a merit scholarship and child sponsorship program were US\$90 and US\$99, respectively. Thus, while the estimate of US\$8.1-US\$28.4 per additional student completing grade 7 is higher than the cost estimate for the deworming project, compared to other interventions that have been credibly evaluated, the results in table 7 suggest that schools used the additional funding that became available through reduced local government capture fairly effectively. Note though that the comparison with the cost estimates in Kremer, Miguel, and Thornton (2004) 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 (see further below).

5.2 Test scores

Table 8 presents evidence on the impact of the newspaper campaign on cognitive skills. We estimate (4') and (??) with the average primary leaving exam score for school j at time t as 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 *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 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 in the post-campaign period have higher scores, but the effect is only significant at the 10-percent level in the larger sample. Note though that the IV estimate in column (1) and the imputed Wald-IV estimate reported in brackets in column (4) are almost identical. The reduced form effect is somewhat larger (in absolute values) for girls, and more precisely estimated, although we cannot reject that the effect for boys are girls are equal.

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, average test scores are 1.1 points higher. Assuming the students only benefited from reduced capture in 2001, this implies a 0.1 standard deviations gain for each US\$0.47 per student that reached the school. Assuming the students benefited from reduced local government diversion beginning with the roll out of the newspaper campaign,

the estimate per 0.1 standard deviations gain is US\$1.88. As a reference point, the costs of the programs considered by Kremer, Miguel and Thornton (2004) range from US\$1.77-US\$4.01. The remedial education program in India, evaluated in Banerjee et al (2007), costs US\$0.67 per 0.1 standard deviations gain. It is worth emphasizing that since the point estimates are not very precisely estimated, the 90 percent confidence interval of our estimate also includes much lower effects and thus higher costs per 0.1 standard deviations gain.

5.3 Additional robustness tests and interpretation

Based on data from 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 knowledge about the grant program (see table 6). This suggests that at least from the head teachers' point of view, *distance* is not capturing 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.²² Distance may also proxy for some other geographical characteristic, such as remoteness, that could influence schools ability to attract both students and money. And if this effect becomes more important in the late 1990s, this may also bias our findings. To test for these alternative hypotheses, we augment equation (3'), (4'), and (??) with a proxy of remoteness: distance to the nearest bank branch (a proxy for distance to the nearest urban centre), interacted with the binary variable $\sigma_{post-campaign}$. We also experiment with two measures of 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 interaction terms enter significantly while the estimates in the first-stage, reduced-form, and IV specifications remain significant, albeit somewhat less precisely estimated.

²²Note that the results of the control experiments suggest that the value of politically connectedness was nil before the newspaper campaign started, so for this to be a concern political connectedness must have changed in the late 1990s.

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 reduced capture had an impact, so sorting is primarily an interpretational concern. Moreover, there are reasons to believe that sorting may be less of a concern here. The sample consists almost exclusively of rural schools and the pool of potential students served by these schools typically does not have much choice with respect to primary school to attend. As we focus on grade 7 students, it is also the case that students started school in 1995 (or earlier). More important, if there are schools close by, the result in previous section suggests that the exposure to the campaign may be similar (since the distance to a newspaper outlet will be similar). As an additional robustness check, distance to the closest school the students could attend was added as explanatory variable in tables 7 and 8. Distance to the closest school the students could attend enters insignificantly and does not qualitatively affect our main results (results available upon request).

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 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.²³

Capitation grant (to cover primary schools' nonwage expenditures) is a fairly common spending program in developing countries, and public expen-

²³The 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, Dupas, and Kremer, 2009).

diture tracking surveys have been implemented in numerous countries, often followed by information dissemination or other social accountability interventions (Gauthier and Reinikka, 2007). In this respect, the results in the paper have some external validity. However, they need not generalize to other contexts. First, parents have for a long time been organized and played a role in the management and control of primary schools in Uganda. This is not the case in other sectors or many other developing countries. Second, the UPE reform, and the political campaign surrounding it, may have created a context particularly favorable to the campaign's success. Survey data also suggest that most households prioritize education over other social services (Stasavage 2003).

The impact 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, differ in two important aspects from most of the existing literature. First, the paper focuses on governance of social services, rather than the impact of specific school inputs. Second, we study the effects of making untied funds available. Schools were free to spend the additional resources that became available as a result of the campaign on whatever nonwage items they needed, be it textbooks, school meals, school uniforms, or flipcharts (or even to boost wages since funds generated by the schools themselves are fungible).²⁴

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 capture and corruption diverted scarce human and other resources away from productive uses (as suggested in Reinikka and Svensson, 2004), it is possible that "social cost effectiveness" estimates would be markedly lower.

Publicity cannot solve all the problems of corruption and diversion of funds in the provision of local services. And improving schooling requires more than to improve governance in general and governance of social services in particular. However, one conclusion we draw from the Uganda case is that experimentation and evaluation of new tools to enhance accountability should be an integral part in the research agenda on improving outcomes of social services.

²⁴Yet another difference is that we look at the compounded effect over a few years.

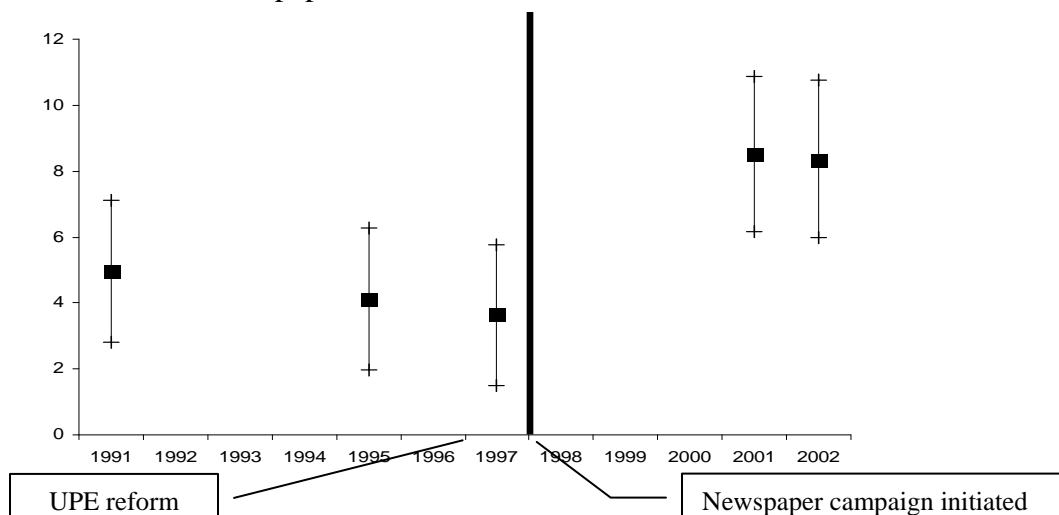
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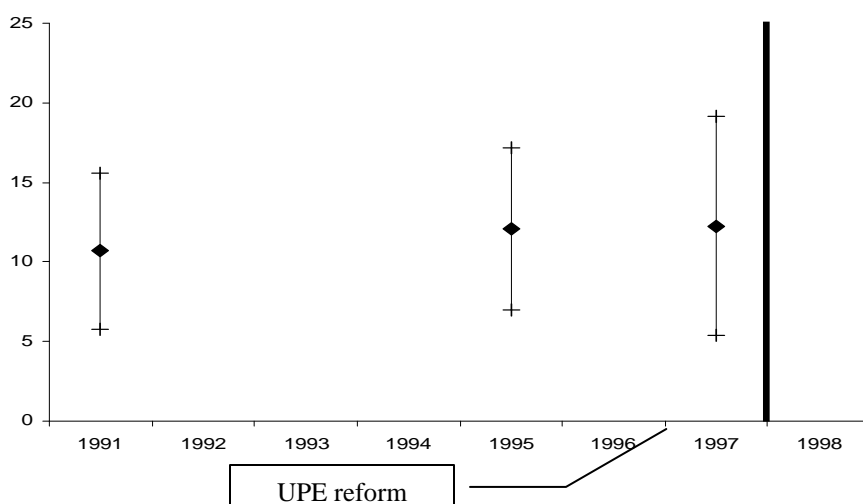
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Figure 1: Estimated impact on number of grade 7 students of being 1 std closer to a newspaper outlet



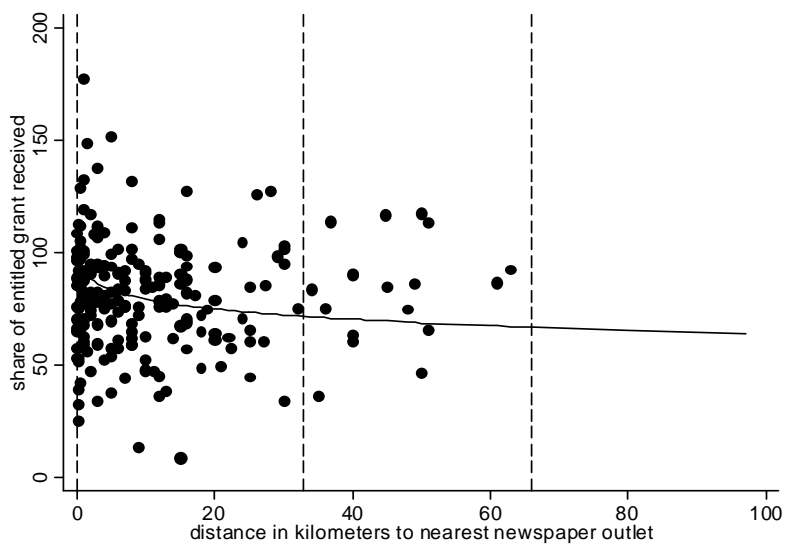
Note: Estimated impact (■) on number of grade 7 students of being 1 std closer to a newspaper outlet and 95 confidence intervals (vertical lines), derived from regressions of *grade 7 students* on *distance* in (1991, 1995, 1997, 2001, 2002). Bold vertical line indicates the year when the newspaper campaign began.

Figure 2: Estimated impact on number of grade 3 students of being 1 std closer to a newspaper outlet on



Note: Estimated impact (■) on number of grade 3 students of being 1 std closer to a newspaper outlet and 95 confidence intervals (vertical lines), derived from regressions of *grade 3 students* on *distance* in (1991, 1995, 1997, 2001, 2002). Bold vertical line indicates the year when the newspaper campaign began.

Figure 3. Estimated impact and scatter plot (table 7, column 1) of distance on (1-capture)



Notes: Distance between dotted lines is 1 STD

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

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

Source: Authors' calculations based on 1996 and 2002 survey; see text for details.

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

	Median	Mean	Standard deviation
<i>1995</i>			
School size (number of students), P7	34	42	37
Average primary leaving exam score	8.7	9.8	5.8
<i>1997</i>			
School size (number of students), P7	37	46	38
Average primary leaving exam score	10.7	11.1	5.3
<i>2001</i>			
School size (number of students), P7	45	51	32
Average primary leaving exam score	9.7	10.4	5.2
<i>2002</i>			
School size (number of students), P7	44	52	32
Average primary leaving exam score	10	10.6	5.3

Source: Ministry of Education (Education Management Information System/EMIS) and Uganda National Examination Bureau (Primary Leaving Exams records); see text for details.

Table 3. Summary Information on Share of Grants Received

	Mean	Median	Standard deviation	Maximum	Minimum	Number of observations
<i>All schools</i>						
1991	3.2	0.0	13.5	114.0	0.0	154
1995	25.4	0.0	35.8	109.8	0.0	200
2001	81.8	82.3	24.6	177.5	9.0	217
	1991	1995	2001			
<i>Regions</i>						
Central	6.1	24.3	92.8			
North	0.0	42.1	102.4			
Northwest	4.2	11.7	90.3			
West	0.0	18.4	71.6			
Southwest	0.6	19.0	83.3			
East	6.7	20.1	62.4			
Northeast	3.6	49.3	73.4			

Notes: Share of Grants Received is capitation grants received as share of entitled grants (in percent). Authors' calculations based on 1996 and 2002 surveys and Ministry of Education; see text for details.

Table 4. Instrument validity: Control experiment 1991-1995

<i>Specification</i>	(1)	(2)	(3)
Dep. variable	Share of grants received	Grade 7 enrollment	Primary leaving exam score
Distance to newspaper outlet×1995	0.81 (3.54)	-0.49 (1.74)	-0.60 (.53)
1991	10.4 (10.0)	56.0 ^{***} (5.39)	11.0 ^{***} (1.87)
1995	41.9 ^{**} (17.4)	59.0 ^{***} (7.86)	9.92 ^{***} (2.42)
School fixed effects	Yes	Yes	Yes
Observations	354	552	558
Schools	207	323	325

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

Table 5. Instrument validity: Control experiment 1995-1997

<i>Specification</i>	(1)	(2)	(3)
Dep. variable	Grade 7 enrollment	Grade 3 enrollment	Primary leaving exam score
Distance to newspaper outlet×1997	-0.38 (1.26)	-3.27 (4.57)	-0.25 (.36)
1995	56.8 ^{***} (10.6)	193.2 ^{***} (36.5)	16.1 (2.92)
1997	62.7 (12.6)	254.4 ^{***} (42.5)	18.8 (3.56)
School fixed effects	Yes	Yes	Yes
Observations	583	558	597
Schools	311	300	315

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

Table 6. Distance and exposure

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

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

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

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

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

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

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

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

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

Table 7. Estimates of the effects of the newspaper campaign on grade 7 enrollment

<i>Specification</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. variable	Share of grants received	Share of grants received	Grade 7 enrollment	Grade 7 enrollment	Grade 7 enrollment	Grade 7 enrollment	Grade 7 enrollment
Sample	1995, 2001		1995, 2001	1995, 2001	1995, 1997 2001, 2002	1997 2001, 2002	1995, 1997 2001, 2002
Method	FD	FD	FD	FD-IV	FE	FE	FE
Distance $\times\sigma_{\text{post-campaign}}$	-7.99** (3.05)	-5.38* (2.98)	-4.53** (1.81)		-3.43** (1.40) [0.43]	-3.31** (1.57) [0.41]	-2.66* (1.41) [0.33]
Share of grants received				0.72** (.30)			
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-time effects	-	Yes	-	-	-	-	Yes
Observations	166	166	179	166	1303	1017	1303
Schools	166	166	179	166	372	372	372

Notes: (i) Robust standard errors (in cols. 1-4) clustered by school (in cols. 5-7) in parenthesis. *** [**] (*) denotes statistically significant at 1 [5] (10) percent levels, respectively. Income is included as control. Imputed Wald/IV estimate in brackets. FD is first differencing, FE is fixed effects, IV is instrumental variables.

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

<i>Specification</i>	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable	Average primary leaving exam score					
Sample years	1995, 2001			1995, 1997, 2001, 2002		
Method	Total	Boys	Girls	Total	Boys	Girls
FD-IV	FD-IV			FE		
Distance $\times\sigma_{01,02}$	0.05 (.08)	0.05 (.09)	0.03 (.08)	-0.50* (.29) [0.06]	-0.40 (.29) [0.05]	-0.52* (.31) [0.06]
Time effects	Yes	Yes	Yes	Yes	Yes	Yes
School effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects $\times\sigma_{01,02}$	Yes	Yes	Yes	Yes	Yes	Yes
Observations	171	169	167	1319	1305	1305
Schools	171	169	167	374	371	373

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

Table 9. Robustness

<i>Specification</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. variable	Share of grants received			Grade 7 enrollment			Grade 7 enrollment		
Sample	1995, 2001			1995, 2001			1995, 1997, 2001, 2002		
Method	FD			FD-IV			FE		
Distance to newspaper outlet $\times\sigma_{\text{post-campaign}}$	-5.99 [*] (3.51)	-7.84 ^{***} (3.03)	-7.91 ^{***} (3.05)				-3.65 ^{**} (1.79)	-3.41 ^{**} (1.40)	-3.31 ^{**} (1.40)
Share of grants received				1.02 [*] (.62)	0.73 ^{**} (.31)	0.71 ^{**} (.31)	0.51 (2.04)		
Distance to urban centre $\times\sigma_{\text{post-campaign}}$	-4.01 (3.82)			4.89 (5.56)			0.51 (2.04)		
Local council official in SMC $\times\sigma_{\text{post-campaign}}$		4.36 (8.10)			-2.33 (6.33)			1.06 (3.54)	
Receipt of discretionary support $\times\sigma_{\text{post-campaign}}$			7.82 (5.93)			5.27 (8.20)			4.66 (5.87)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	166	166	166	166	166	166	1303	1303	1303
Schools	166	166	166	166	166	166	372	372	372

Notes: (i) Robust standard errors (in cols. 1-6) clustered by school (in cols. 7-9) in parenthesis. *** [**] (*) denotes statistically significant at 1 [5] (10) percent levels, respectively. Income is included as control. FD is first differencing, FE is fixed effects, IV is instrumental variables.