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Information Reduces Corruption and Improves Enrolment (But Not Schooling): A Replication Study of a Newspaper Campaign in Uganda

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ABSTRACT *In the mid-1990s, Ugandan primary schools received only one-fifth of intended government capitation grants. A seminal study shows that a grassroots newspaper campaign substantially reduced grant capture and improved educational outcomes. We replicate these results, confirming that the campaign reduced corruption and increased enrolment. The latter outcome is only robust with an improved enrolment measure introduced in later work by the authors of the original study. We cannot, however, support the authors' conclusion that lower capture enhanced learning. Finally, we show that the newspaper campaign allowed for a fairer allocation of teachers across schools, a result absent in the original papers.*

1. Introduction

The provision of education in developing countries is typically the purview of the government. Yet a government-run programme may not achieve the same level of effectiveness as a programme implemented by an external organisation. For example, Bold, Kimenyi, Mwabu, Ng'ang'a, and Sandefur (2013) investigate the scale-up of a successful scheme to increase educational attainment through the provision of contract teachers in Kenya. The programme was successful when implemented by an international non-governmental organisation. In the hands of the Kenyan government, however, the programme produced no discernible effects.

When government service provision falls short of its goals, it is crucial to understand why. In many cases, resource capture plagues public service delivery. Public expenditure tracking surveys (PETS) are one method for identifying the source of such leakages, the 1996 Ugandan PETS being the first of its kind. Relying on this PETS, Reinikka and Svensson (2004) reported the now infamous statistic that, in the mid-1990s, only one-fifth of government capitation grants for primary schools reached their intended recipients (p. 687). The rest found its way into the pockets of local officials or funded local patronage politics.

The Ugandan government took immediate action. To counter rent-seeking, the government introduced a newspaper campaign at the end of 1997. Instead of tackling corruption from the top, the campaign took a grassroots angle: national newspapers published information on the timing and amount of capitation grants disbursed by the central government to school districts. The government hoped that providing citizens with better information would curb elite capture.

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Reinikka and Svensson (henceforth RS (2005)) examine the effects of this anti-corruption newspaper campaign. Their contributions are two-fold. First, relying on a difference-in-differences analysis, RS (2005) evaluate how the campaign improved the receipt of capitation grants by primary schools. To do so, they regress the change in the share of capitation grants reaching schools between 1995 and 2001 on a school's distance to a newspaper outlet in 1997. The lower this distance, they surmise, the greater the school's exposure to the newspaper campaign and therefore the higher the share of capitation grants received. Supplementing the difference-in-differences analysis with an instrumental variables approach, the authors then examine how the resulting increase in funding influenced educational outcomes.

RS (2005) find that the share of capitation grants reaching schools skyrocketed due to the newspaper campaign. Moreover, 'the reduction in capture had a positive effect on enrolment and student learning' (see RS's [2005] abstract). The authors submit their findings to a wider range of robustness checks in Reinikka and Svensson (henceforth RS (2011)) and arrive at the same conclusions. Subsequent studies draw similar lessons. For example, Francken, Minten, and Swinnen (2009) show that access to information about the elimination of tuition fees in public primary schools in Madagascar reduced the capture of public education expenditures, though they do not study educational outcomes. In Brazil, the publication of audits on the use of federal funds reduced the re-election of corrupt mayors (Ferraz and Finan (2011), and schools with fewer financial resources due to corruption experienced poorer schooling outcomes (Ferraz, Finan, and Moreira (2012). The Tanzanian government pursued a similar information campaign to publish allocations of pro-poor spending programmes, but this initiative was not so systematic as to be evaluable, even though initial results appeared positive (Gauthier, 2010).

There are several reasons why RS (2005) is an important candidate for replication. First, Reinikka and Svensson's body of work on this topic is widely cited as evidence on information for accountability reform at the national level. In a review of social accountability initiatives, Fox (2015) suggests that the authors' work influenced the 2004 World Development Report, 'Making Services Work for Poor People'. Second, the Ugandan experience with leakage is not unique. After the first PETS in Uganda, more than a dozen countries followed suit. The tracking surveys revealed that leakage was far from an isolated incident. For example, leakage of non-wage educational expenditures amounted to 41 per cent in Tanzania and 50 per cent in Ghana (Gauthier, 2010). The Ugandan government stands out for its swift and decisive reaction of instituting reforms to counter the information asymmetries responsible for the missing funds. By contrast, even though tracking surveys revealed similar information challenges in other countries, governments did not succeed in translating PETS information to actionable policy due in part to insufficient dissemination and lack of political will (Gauthier, 2010; Reinikka and Svensson, 2004). A third motivation for the replication of RS (2005) relates to the consequences for the Ugandan population as a whole. Partially in response to the success of the newspaper campaign and institutional reforms, the Ugandan government passed legislation to improve citizens' information access. Finally, RS (2005, 2011) also contribute to understanding the policies which promote both attendance and learning, an ongoing challenge (see Kremer, Brannen, and Glennerster, 2013).

We pursue two objectives in this study. First, we conduct a pure replication of RS's (2005) results by recreating their dataset from the raw data and replicating their exact empirical approach. We then provide additional analysis. This analysis first consists of a Measurement and Estimation Analysis (MEA) (Brown, Cameron, & Wood, 2014) which investigates the sensitivity of RS's (2005) findings beyond the checks originally performed. Notably, we 'update' RS (2005) with the authors' later work from RS (2011). The remaining tests are absent from RS (2005, 2011) as well as from their working paper last updated in 2006 (Reinikka and Svensson, 2006) on which RS (2005, 2011) heavily relied. We also conduct a Theory of Change Analysis (TCA), which builds on the original analysis by investigating new educational outcomes likely to be influenced by the newspaper campaign, notably teacher supply. This analysis can help to illuminate the results of the original study in terms of enrolment and learning.

Recognising the potential for contention surrounding replication studies, we raise the threshold under which a result can be deemed as statistically significant from $p = 0.10$ to $p = 0.15$. We also adhere to a replication plan designed prior to the start of this study. The replication plan,[1] developed without access to the raw data, discusses the motivation for the study and the disambiguation of RS (2004, 2005, 2006, and 2011). It presents an overview of the MEA and TCA. However, much of the proposed analysis, specifically all that involved merging the main dataset with more detailed educational surveys, was incompatible with the raw data. In the end, the TCA we pursue relies only on additional variables in the PETS data. Though not specified explicitly in the replication plan, we believe that this analysis is in keeping with the spirit of the plan and our overall goal to limit the replication only to tests which are as straightforward as possible.

Our findings confirm that the newspaper campaign reduced elite capture. Moreover, we endorse the conclusion that the receipt of capitation grants increased enrolment, although this result emerges only if we rely on a more accurate enrolment measure introduced in RS (2011). It fails to hold when we replicate RS (2005). In contrast, we cannot support RS's (2005, 2011) assertion that lower capture due to the newspaper campaign improves learning outcomes. Finally, we try to better illuminate the theory of change behind these results. Specifically, we document an additional effect of the newspaper campaign – a more balanced allocation of teachers across schools – a result absent from the original papers.

The paper proceeds as follows: in Section 2, we conduct a pure replication. We perform simple MEA in Section 3, with additional analysis in Section 4. Section 5 presents the TCA. Section 6 discusses our results, and Section 7 concludes.

2. Pure replication

2.1. RS's (2005) identification strategy

RS (2005) rely on an instrumental variables (IV) approach. The first-stage estimates the effect of the newspaper campaign on the share of capitation grants received by schools. The second-stage estimates the impact of the share of capitation grants reaching schools due to the newspaper campaign on educational outcomes (enrolment and learning[2]).

In their first-stage, RS (2005) rely on a difference-in-differences (DD) analysis. A school's distance to a newspaper outlet in 1997, just prior to the launch of the newspaper campaign, determines its probability of treatment. Equation (1) illustrates the first-stage:

$$s_{it} = \alpha_{0i} + \alpha_1 year_{1997} + \alpha_2 (distance_i \times year_{1997}) + u_{it}, \tag{1}$$

for $t = \{1995, 2001\}$. The dependent variable s_{it} denotes the share of capitation grants reaching school i at date t. Coefficient α_{0i} is a school-specific fixed effect, which captures the effect of all time invariant characteristics at the school level. It notably includes the effect of $distance_i$, which represents the distance of school i to a newspaper outlet in 1997.[3] Variable $year_{1997}$ is binary, taking the value 1 after 1997 (for year 2001) and the value 0 otherwise (for year 1995). Finally, the error term u_{it} captures the idiosyncratic shocks that affect the share of capitation grants reaching school i at date t.

In Equation (1), α_2 captures the treatment effect, provided that the control and treatment groups follow parallel trends prior to the newspaper campaign and that no omitted variables bias is at work after the campaign. The latter implies showing that the distance of school i to a newspaper outlet in 1997 remains statistically significant when controlling for correlates of distance to a newspaper outlet which may influence the share of capitation grants reaching school *after* the launch of the campaign.

We move in Equation (2) to RS's (2005) second-stage:

$$y_{it} = \beta_{0i} + \beta_1 year_{1997} + \beta_2 \widehat{s_{it}} + v_{it}, \tag{2}$$

for $t = \{1995,\ 2001\}$. The dependent variable y_{it} denotes an enrolment or learning outcome. β_{0i} is a school-specific fixed effect, and $\widehat{s_{it}}$ is the predicted value of s_{it} from Equation (1). Variable $year_{1997}$ is defined as in Equation (1). The error term v_{it} captures the idiosyncratic shocks that affect educational outcomes in school i at date t. Coefficient β_2 measures the impact of the share of capitation grants on educational outcomes. If there is no omitted variables bias, the distance of school i to a newspaper outlet in 1997 will influence y_{it} *only* through the newspaper campaign.

To eliminate school fixed effects, RS (2005) difference Equations (1) and (2), leading to the first-stage OLS estimates in Equation (3):

$$\Delta s_{it} = s_{i,2001} - s_{i,1995} = \gamma_0 + \gamma_i distance_i + \Delta u_i \tag{3}$$

Similarly, they compute the second-stage OLS estimates in Equation (4):

$$\Delta y_i = y_{i,2001} - y_{i,1995} = \delta_0 + \delta_1 \widehat{\Delta s_{it}} + \Delta v_i \tag{4}$$

where $\widehat{\Delta s_{it}}$ is the predicted value of Δs_{it} derived from Equation (3).

2.2. Dataset construction

We provide a detailed description of our dataset construction in the Supplementary Materials. RS (2005) use data from Uganda's Public Expenditure Tracking Survey (PETS) conducted among head teachers in 1996 and 2002. The original 1996 sample consists of 250 schools randomly drawn from 18 districts. However, the number of schools surveyed in both 1996 and 2002 is 218 (250 – 32).[4] RS (2005) rely on this group to generate their (first-difference) OLS estimates of Equation (3) and Equation (4). An additional 170 schools from nine of the original 18 districts were surveyed in 2002, yielding 388 (218 + 170) schools in 2002.

The share of capitation grants reaching the school in year t ($t = \{1995,\ 2001\}$) is defined as the total amount disbursed in year t (intended for year t) and received by a school in year t (or at the beginning of year $t + 1$) divided by the total amount intended for year t. The number of students in grades P1−P3 and P4−P7 determines the grant total. In 1995, the formula allocated 2,500 Ugandan shillings (Ush) per year per student in grades P1−P3 and 4,000 Ush in grades P4−P7. In 2001, the introduction of UPE at the beginning of year 1997 doubled the allocations: 5,000 Ush for grades P1−P3 and 8,100 Ush for grades P4−P7. RS (2006) note that funds were withheld if districts did not submit the required quarterly documentation. To adjust, RS (2005) scale a school's entitlement by the share of funds actually disbursed by the central government. We follow the same procedure.

Enrolment is the primary educational outcome assessed by RS (2005). The dependent variable Δy_i in Equation (4) is the change in total enrolment between 1995 and 2001. Because RS (2005) retain the maximum of the two measures of total enrolment in 2001, we check that results are robust to relying on the minimum of these two values in our additional analysis.

Distance to a newspaper outlet in 2001 proxies for distance to a newspaper outlet in 1997. This variable comes from question Q29 of the 2002 PETS, which asks the head teacher to report the 'distance to the nearest place to buy a newspaper (in km)'. As specified in RS (2006), RS (2005) use the natural logarithm of one plus this distance.

Note that the authors also include mean consumption at the district level as a control, based on national household survey data. Following their approach, we control for this variable in all regressions.

2.3. Pure replication of RS's (2005) results

2.3.1. Table 1. Table 1 *of RS (2005)* reports summary statistics on the share of capitation grants reaching schools in 1995 and 2001 from the original paper (Panel A) and from our replication (Panel

Table 1. Summary statistics on the share of capitation grants reaching school in 1995 and in 2001 (in %)

	Mean	Median	St. dev.	Obs.
Panel A: Original				
1995	23.9	0	35.1	229
2001	81.8	82.3	24.6	217
Panel B: Pure replication				
1995	23.9	0	35.1	229
2001	81.8	82.3	24.6	217

Table 2. Linking distance, head teacher's knowledge, and share of capitation grants reaching school

	Dep. var.		
	HT's knowledge in 2001	Δs	s_{2001}
	(1)	(2)	(3)
Panel A: Original			
Distance to a newspaper outlet	−0.10***		
	(0.03)		
HT's knowledge in 2001 (inst.)		65.88***	
		(23.50)	
		[25.47]	
Number of schools	388	199	
Panel B: Pure replication			
Distance to a newspaper outlet	−0.10***		
	(0.03)		
HT's knowledge in 2001 (inst.)		65.88***	
		(23.50)	
		[25.47]	
Number of schools	388	199	
Panel C: Modified replication			
Distance to a newspaper outlet	−0.12***		
	(0.03)		
HT's knowledge in 2001 (inst.)			41.54***
			(17.79)
F-statistic	13.35		
Number of schools	375		375

Notes: Δ*s* represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Consumption at the district level is included as a control. OLS standard errors are in parentheses, and bootstrapped standard errors are in brackets. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

B). The figures are exactly the same and indicate a substantial increase in the share of capitation grants received between 1995 and 2001.

2.3.2. Table 2. In Table 2, RS (2005) better document the mechanism in Equation (3): that proximity to a newspaper outlet in 1997 increases the share of capitation grants reaching a school by improving the head teacher's knowledge about the timing and amount of disbursed grants. Eliciting this missing link implies computing, in a first-stage, the first-difference OLS estimates of Equation (5):

$$\Delta know_i = know_{i,2001} - know_{i,1995} = \lambda_0 + \lambda_1 distance_i + \Delta w_{1i} \quad (5)$$

where $know_{i,t}$ is the head teacher's knowledge of the grant in school i at date t. The first-difference Equation (6) gives the second-stage:

$$\Delta s_i = s_{i,2001} - s_{i,1995} = \rho_0 + \rho_1 \widehat{\Delta know}_i + \Delta w_{2i} \quad (6)$$

where $\widehat{\Delta know}_i$ is the predicted value of $\Delta know_i$ derived from Equation (5).

Yet, the head teacher's grant knowledge is available only in 2001, so the first-difference approach is not an option. A cross-sectional approach is an alternative, which estimates Equations (5) and (6) based on the 2002 PETS:

$$know_{i,2001} = \phi_0 + \phi_1 distance_i + w_{3i,2001}, \quad (7)$$

$$s_{i,2001} = \xi_0 + \xi_1 \widehat{know}_{i,2001} + w_{4i,2001} \quad (8)$$

Contrary to the first-difference approach, the cross-sectional approach cannot neutralise the school fixed effects embedded in the error terms $w_{3i,2001}$ and $w_{4i,2001}$ of Equations (7) and (8). It therefore falls prey to an omitted variables bias. In the first-stage, distance of school i to a newspaper outlet in 1997 could be correlated to other school characteristics which themselves determine the head teacher's grant knowledge. In the second-stage, distance of school i to a newspaper outlet in 1997 might affect the share of grants reaching schools through channels other than the head teacher's knowledge. In the absence of further controls in Equations (7) and (8), the results stemming from the cross-sectional approach must therefore be interpreted with caution.

RS (2005) rely on a mix of the first-difference and the cross-sectional approach. They estimate Equation (7) in their first-stage and Equation (6) in their second-stage, where $\widehat{\Delta know}_i$ is replaced by $\widehat{know}_{i,2001}$. Panel A of Table 2 reports the results from the original paper. We replicate the same results, which we show in Panel B, labelled 'Pure replication'.[5]

2.3.3. Table 3. In Table 3, RS (2005) present OLS estimates of Equations (3) and (4) where Δy_i is the change in total enrolment between 1995 and 2001. In column 1, the authors simply regress Δy_i on a constant to determine the sign of this change. In columns 2 and 3, they report the OLS estimates of Equations (3) and (4). In column 4, they estimate the reduced form of Equations (3) and (4) by regressing the change in enrolment from 1995–2001 on distance to a newspaper outlet in 1997. In column 5, they estimate this reduced form for the 1991–1995 period, rather than for 1995−2001. Doing so serves as a falsification test. Indeed, if distance to a newspaper outlet only captures exposure to the newspaper campaign, then the coefficient of distance to a newspaper outlet should be negative and statistically significant in column 4 (since year 1997 is included in the 1995−2001 period) but not statistically significant in column 5, which covers only the pre-campaign period.

Panel A of Table 3 reports the results from the original paper, and Panel B reports the results from our pure replication. Column 1 reveals a statistically significant increase in school total enrolment in both panels. Moreover, OLS estimates of Equation (3) confirm, in both panels, the negative and statistically significant correlation between distance to a newspaper outlet and difference in the share of capitation grants reaching schools between 1995 and 2001 (column 2 of Table 3). Finally, our pure replication results for the falsification test reported in column 5 coincide exactly with those stemming from the authors' original Stata do-file.

Finally, though it is not consequential since the coefficient of 'Distance to newspaper outlet' is not statistically significant, we note that the estimates from Panel A and Panel B do not coincide in column 5, possibly due to a typo. In Panel A, copied from the published version of the paper, the OLS estimate of the constant and its standard deviation are equal to 68.4** and 33.6 (against 100.01*** and 33.26 in column 5 of Panel B). Moreover, the OLS estimate of the coefficient of 'Distance to

Table 3. Impact of the newspaper campaign on school total enrolment

	Dep. var.				
	Δ tot. enrol.	Δ s	Δ tot. enrol.	Δ tot. enrol.	Δ tot. enrol.
	95–01	95–01	95–01	95–01	91–95
	(1)	(2)	(3)	(4)	(5)
Panel A: Original					
Constant	450.47***	74.13***	29.66	574.18***	68.4**
	(20.34)	(6.77)	(286.20)	(49.31)	(33.6)
Distance to a newspaper outlet		−5.74**		−37.74**	−4.7
		(2.45)		(17.94)	(12.1)
$\widehat{\Delta s}$			7.55[a]		
			(4.63)		
Number of schools	202	188	188	202	153
Panel B: Pure replication					
Constant	450.47***	74.13***	29.66	574.18***	100.01**
	(20.34)	(6.77)	(286.20)	(49.31)	(33.26)
Distance to a newspaper outlet		−5.74**		−37.74**	−4.45
		(2.45)		(17.94)	(11.60)
$\widehat{\Delta s}$			7.55[a]		
			(4.63)		
Number of schools	202	188	188	202	153
Panel C: Modified replication					
Constant	418.40***	75.76***	362.42*	522.48***	100.01**
	(21.99)	(6.79)	(186.89)	(53.36)	(33.26)
Distance to a newspaper outlet		−6.83***		−12.13	−4.45
		(2.43)		(19.15)	(11.60)
$\widehat{\Delta s}$			2.22		
			(3.05)		
F-statistic		7.93			
Number of schools	212	198	198	212	153

Notes: Δs represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

newspaper outlet' and its standard deviation are equal to −4.7 and 12.1 (against −4.45 and 11.60 in column 5 of Panel B).

2.3.4. Table 4. In Table 4, RS (2005) seek to rule out the possibility that the positive impact of the newspaper campaign on school total enrolment is not due to sorting. Indeed, it could be that RS's (2005) results in column 3 of Panels A and B of Table 3 are driven by the fact that students sort into schools with more resources with no effect of funding on enrolment at the aggregate level. To rule out this possibility, RS (2005) rely on external data from a working paper (now Björkman, 2007). In this paper, Björkman estimates the impact of the newspaper campaign on enrolment of P7 students at the district level. While it is possible that students move to higher resource schools nearby, this sorting is less likely if the unit of observation is not the school but the district. Unlike RS (2005), Björkman uses the number of newspaper circulation to identify treatment, dividing the number of newspapers by the number of schools in a given district. Hence, schools in a given district have the same probability of being treated. Moreover, Björkman does not rely on RS's (2005) two-stage IV

Table 4. Impact of the newspaper campaign on enrolment of P7 students: difference-in-differences analysis at the district level

	Dep. var.		
	enrol. of P7 students	enrol. of P7 students	enrol. of P7 students
	1995	2001	1995 & 2001
	(1)	(2)	(3)
Panel A: Original			
Newspapers per school	14.46	34.00*	14.05
	(11.51)	(18.29)	(12.02)
Newspapers per school x 1997			20.16***
			(3.18)
Number of schools	53	53	106
Panel B: Pure replication			
Newspapers per school	14.46	34.00*	14.05
	(11.51)	(18.29)	(12.02)
Newspapers per school x 1997			20.16***
			(3.18)
Number of schools	53	53	106
Panel C: Modified replication			
Newspapers per school	14.46	34.00*	
	(11.51)	(18.29)	
Newspapers per school x 1997			21.54***
			(1.74)
District fixed effects	No	No	Yes
Number of schools	53	53	106

Notes: Consumption at the district level is included as a control. OLS standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

approach. Instead, she uses a reduced form equation which equates to the following DD analysis:

$$y_{jt} = \psi_0 + \psi_1 year_{1997} + \psi_2 newsperschool_j + \psi_3 \left(newsperschool_j \times year_{1997}\right) + w_{5jt}, \quad (9)$$

for $t = \{1995,\ 2001\}$. The dependent variable y_{jt} denotes the number of students enrolled in grade P7 in district j at year t. Variable $newsperschool_j$ represents the number of newspapers per school in district j in 1997. Variable $year_{1997}$ is defined as in Equation (1). Coefficient ψ_3 captures the impact of the newspaper campaign on enrolment of P7 students at the district level.

Panels A and B of Table 4 report the results from the original paper and its pure replication. In columns 1 and 2, Björkman regresses y_{jt} on $newsperschool_j$ in 1995 and in 2001. If the number of newspapers per school in district j in 1997 is indeed a proxy for the degree of exposure to the newspaper campaign, then the coefficient of this variable should be positive and statistically significant only in column 2 (which concerns the post-treatment period), not in column 1 (which concerns the pre-treatment period). The results confirm this interpretation. Moreover, column 3 reports the OLS estimates of Equation (9) and reveals a positive and statistically significant coefficient ψ_3. This finding suggests that RS's (2005) results in column 3 of Panels A and B of Table 3 are not driven by sorting.

3. Measurement and estimation analysis: a simple approach

In this section, we make standard adjustments to the estimations reported in the pure replication.

3.1. Table 2

Since it is standard to maintain the same sample and specification in both stages of the 2-SLS, we present in Table 2, Panel C, called 'Modified replication', the OLS estimates of the cross-sectional approach (Equations [7] and [8]). We get these results thanks to the standard *ivregress 2sls* command in Stata. We clarified the decision to estimate the two-stages with the same sample with the authors, who were supportive.

Despite the change in procedure, the first-stage coefficient (column 1 of Panel C) associated with distance to newspaper outlet is very similar to the original first-stage results. It is negative and statistically significant at the 99 per cent confidence level, revealing that distance to a newspaper outlet in 1997 is negatively correlated with a head teacher's knowledge about the capitation grant. Moreover, although the magnitude of the point estimate associated with head teacher's knowledge in the second-stage decreases to 41.54 (column 3 of Panel C), it remains positive and statistically significant (at the 99% confidence level). Thus, the share of capitation grants reaching school is positively correlated with the head teacher's knowledge about the capitation grant. It is important to stress that these results hold if standard errors are robust. All in all, RS's (2005) results reported in Panel A of Table 2 are fully robust to a more standard approach (Panel C). They are consistent with the premise that a lower distance to a newspaper outlet in 1997 increases the share of capitation grants reaching school by improving the head teacher's knowledge about capitation grants.

3.2. Table 3

RS (2005) discard 10 schools that experienced a reduction in the student body due to 'idiosyncratic shocks' (footnote 9, p. 264). Yet we are uncomfortable with the decision to focus only on schools with an increase in total enrolment. If these shocks are truly idiosyncratic, they should not be systematically correlated with distance to a newspaper outlet – and keeping them in the analysis should be inconsequential.

Table 3, Panel C reports our replication when reintegrating these 10 schools in columns 1 through 4. (Column 5, using data from 1991 and 1995, is unchanged.) The original result in column 3 of Panel A reveals a weakly positive and statistically significant correlation (with the p-value equal to 0.103) between the increase in the share of capitation grants due to the newspaper campaign and the increase in school total enrolment between 1995 and 2001. This correlation loses statistical significance in Panel C.[6] Consequently, the negative coefficient of distance to a newspaper outlet in the reduced form equation in column 4 is also not statistically significant in Panel C, while it is statistically significant in Panels A and B. Note that results reported in Panel C do not change if standard errors are robust or if, instead of keeping the maximum value of total enrolment as a proxy, we use the minimum.[7]

A more straightforward approach than the one used by RS (2005) for columns 1 to 4 of Table 3 therefore supports their claim that the newspaper campaign reduces capture of public funds. However, it does not confirm that 'the reduction in capture [induced by the newspaper campaign] had a positive effect on enrolment'.

3.3. Table 4

Equation (9) does not include district-specific fixed effects, critical in mitigating the risk of omitted variables bias. Instead of estimating Equation (9) in column 3 of Panel C of Table 4, we therefore estimate Equation (10) by running an OLS district fixed effects estimation:

$$y_{jt} = \psi_{0j} + \psi_1 year_{1997} + \psi_2 \left(newsperschool_j \times year_{1997}\right) + w_{6jt}, \quad (10)$$

where ψ_{0j} is a district-specific fixed effect which notably absorbs the effect of $newsperschool_j$.

Our estimates show that Björkman's results in column 3 of Panels A and B are robust to controlling for district fixed effects. Not surprisingly, a first-difference OLS estimation provides results that are similar to the OLS district fixed effect estimation, given that the sample of districts is identical in 1995 and in 2001.[8] (Results are available upon request.) Thus, a more demanding approach confirms Björkman's findings.

4. Measurement and estimation analysis: additional tests

Our pure replication and simple MEA support RS's (2005) claim that the newspaper campaign increases the share of capitation grants reaching schools by improving the head teacher's knowledge. However, simple MEA does not show that the reduction in elite capture positively influenced enrolment, knowing that this result is already weakly statistically significant in RS (2005). Moreover, RS (2005) do not provide empirical support for their claim that the reduction in capture also has a positive impact on student learning – they only reference it.

In fact, enrolment provided by the school's head teacher is prone to misreporting. Since higher enrolment dictates a higher capitation grant, head teachers have more incentive to overestimate enrolment in 2001 (in order to induce a higher capitation grant in the future) in schools which experienced a modest reduction in capture between 1995 is 2001, compared to schools where the reduction in capture was larger. If so, this misreporting bias runs against finding a positive and statistically significant relationship between an increase in the share of received grants and an increase in this school's total enrolment between 1995 and 2001. To avoid this bias, RS (2011) rely on observed (not self-reported) enrolment figures: enrolment of P7 students, proxied by the number of students who took the Primary Leaving Exam (PLE) reported by the Uganda National Examination Bureau. In what follows, we rely on this enrolment proxy.

The purpose of our additional analysis in this section is first to test the robustness of the impact of the newspaper campaign on capitation grant capture. We then test the robustness of the newspaper campaign on enrolment and learning outcomes.

4.1. Effect of the newspaper campaign on capture

4.1.1. Testing the parallel trend assumption. For γ_1 in Equation (3) to measure the effect of the newspaper campaign on grant receipt, a school's distance to a newspaper outlet should not be correlated with the change in the share of received capitation grants before 1997. In other words, the impact of a change in time-varying characteristics before the newspaper campaign in schools with a high probability of exposure to the campaign (due to their proximity to a newspaper outlet) should be similar to that of schools with a low probability of treatment.

Columns 1 and 2 of Table 5 report the correlation between distance to a newspaper outlet and the change in the share of capitation grants received over the 1991−1995 and 1995−2001 periods. The data are supportive of the parallel trend assumption: the correlation is negative and statistically significant only after the launch of the newspaper campaign (and positive and not statistically significant during the pre-campaign period).

It is important to stress that RS (2011) also provide support for the parallel trend assumption in column 1 of their Table 4, which focuses on the 1991−1995 period. However, information on some of the schools in their sample is available for only one of two years (either 1991 or 1995). This explains why RS (2011) rely on a fixed effect estimation for 1991−1995. However, there is no guarantee that the results from a fixed effect estimation with unbalanced panel data coincide with those from a fixed effect estimation with balanced panel data (or, equivalently, a first-difference estimation similar to the one we implement in Table 5). While schools surveyed once and schools surveyed twice enter the

Table 5. Distance to a newspaper outlet and change in the share of capitation grants

	Dep. var.	
	Δs	Δs
	91–95	95–01
	(1)	(2)
Distance to a newspaper outlet	0.81	−6.77**
	(2.28)	(2.62)
Number of schools	147	199

Notes: Δ*s* represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

estimation, fixed effects for the former group are not identified, which may compromise the reliability of RS's (2011) test of the parallel trend assumption.

4.1.2. Controlling for competing channels. The coefficient γ_1 must also remain statistically significant when accounting for competing channels, chief of which is UPE. The UPE reform, which abolished tuition fees and parent-teacher association (PTA) charges, preceded the newspaper campaign by roughly 10 months. As such, γ_1 may simply reflect the impact of the UPE reform on capture. The loss of complementary funding channels could push local communities to better secure the receipt of capitation grants. However, the ability of communities to improve their share of funding is likely to be correlated negatively with distance to a newspaper outlet. This increases the risk that γ_1 will lose statistical significance when controlling for communities' ability and incentive to mobilise.[9]

To illustrate this claim, we first focus on the community's ability to secure grants. RS (2004) study the share of grants received during the pre-campaign period (1991–1995), showing that better-off communities manage to claim a higher share of their entitlements. This finding suggests that 'rather than being passive recipients of flows from the government, schools use their bargaining power to secure greater shares of funding' (p. 679), a behaviour likely to increase after the UPE reform. Yet, as shown by Table 6, the correlation between distance to a newspaper outlet and consumption at the district level in 1995 (a proxy for the local community's wealth prior to 1997) is negative and statistically significant. This suggests that γ_1 suffers from a downward bias, leading us to control for consumption in 1995. Similarly, Table 6 shows that the ability to communicate with local officials is negatively correlated to distance to a newspaper outlet using three proxies: (i) school proximity to an urban centre, (ii) presence of a local official or of a representative of the District Education Office (DEO) in the School Management Committee (SMC), and (iii) whether the school received discretionary financial support. The third variable captures whether the school received financial support from sources other than government or PTA, hence from local officials. Controlling for these additional variables in Equation (3) is therefore critical.

Corruptibility of the head teacher could also be a concern if, for example, head teachers themselves colluded with local officials to benefit from local capture. If this were the case, knowledge would not necessarily be the primary mechanism explaining the reduction in grant capture – rather, these corrupt school officials could have been sanctioned for their behaviour (see Ferraz and Finan, 2011; and Ferraz et al., 2012). RS (2011) provide anecdotal evidence that misconduct typically occurred at the level of district authorities, rather than at the head teacher level. Even so, we also believe that testing for the proximity of an urban centre, the local official or representative of the DEO in the SMC and the discretionary financial support additionally proxy for the corruptibility of the head teacher. The

Table 6. Correlation between distance to a newspaper outlet and local community's ability and incentives to exert pressure on local officials

	Distance to a newspaper outlet
Ability to secure grants	
Wealth in 1995	−0.42***
Proximity to an urban centre	−0.58***
Local official or representative of the DEO in the SMC	−0.09*
Discretionary financial support	−0.18***
Incentive to secure grants	
Capture of capitation grants in 1995	−0.13***
Enrolment of P7 students in 1995	−0.31***
PLE scores in 1995	−0.31***

Notes: [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

Table 7. Distance to a newspaper outlet and change in the share of capitation grants when controlling for the UPE reform channel

	Dep. var.: Δs						
	95–01	95–01	95–01	95–01	95–01	95–01	95–01
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Distance to a newspaper outlet	−3.92[a]	−4.90[a]	−6.42**	−6.45**	−2.80*	−6.63**	−6.68**
	(2.78)	(3.03)	(2.66)	(2.64)	(1.66)	(2.89)	(3.06)
Wealth in 1995	0.000***						
	(0.00)						
Distance to an urban centre		−3.67					
		(3.37)					
Local official or representative of the DEO in the SMC			17.31[a]				
			(11.47)				
Discretionary financial support				8.33			
				(6.07)			
Share of capitation grants in 1995					−0.84***		
					(0.05)		
Enrolment of P7 students in 1995						0.16***	
						(0.06)	
PLE scores in 1995							0.98*
							(0.56)
Number of schools	199	199	199	199	199	176	176

Notes: Δ*s* represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

fact that the results hold with these tests (see Table 7 described below) suggests that the distance to a newspaper outlet does influence grant capture through information access rather than a competing disciplinary mechanism.[10]

We now focus on the community's incentives (beyond the mere existence of UPE reform) to secure the transfer of capitation grants. First, the greater the capture of funding in 1995, the greater the incentive to mobilise. Second, schools where enrolment is higher before the newspaper campaign

should also be more motivated to secure grants after UPE's abolition of tuition fees. Third, schools with higher test scores could have a greater incentive to secure capitation grants after UPE in order to maintain existing quality levels. In Table 6, all three variables (the degree of capture of capitation grants in 1995, the number of P7 students enrolled in 1995 and their PLE scores in 1995) are negatively correlated with the school's distance to a newspaper outlet. We should therefore ensure that γ_1 in Equation (3) remains statistically significant after controlling for these variables to purge γ_1 of its downwards bias.

Table 7 displays the OLS coefficients from regressing the change in the share of received grants between 1995 and 2001 on distance to a newspaper outlet in 1997 while holding constant, sequentially, each of the seven proxies for the local communities' mobilisation capabilities. The results show that γ_1 remains negative and statistically significant after controlling for the UPE reform channel.[11]

A community's awareness of UPE existence could serve as a final indicator of its incentive to mobilise. Though we lack a proxy for this characteristic, we doubt that it depends negatively on distance to a newspaper outlet. First, this information is far too important for head teachers to learn only through newspapers. And indeed, all of the 388 head teachers surveyed in 2002 were aware that his/her school was entitled to a UPE capitation grant. Second, the introduction of UPE was disseminated by other media,[12] such as radio (Deininger (2003). This reduces the probability of a negative correlation between distance to a newspaper outlet and awareness of UPE reform. We provide additional empirical support for this claim in Section 5.2.

It is important to stress that RS (2011) only control in columns 1–3 of their Table 9 for three of the seven proxies above: distance to an urban centre, if a local official is present in the SMC (though not a DEO representative), and if a school received discretionary financial support. While our approach is more exhaustive, it does not alter the negative and statistically significant coefficient of $distance_i$ in Equation (3). This additional analysis therefore supports RS's (2005, 2011) claim that this coefficient measures the effect of the newspaper campaign on capture of capitation grants.

4.2. Effect of the newspaper campaign on enrolment of P7 students

4.2.1. Estimating Equations (3) and (4). Table 8 reports estimates of Equations (3) and (4) when Δy_i is the change in enrolment of P7 students from 1995 to 2001. Results from the first-stage (column 1) confirm the negative and strongly significant relationship between 'Distance to a newspaper outlet' and Δs. Moreover, the second-stage (column 2) shows that an increase in the share of received capitation grants has a positive and statistically significant impact on the change in enrolment of P7 students. In the absence of reporting bias, empirical evidence supports that a reduction in capture of capitation grants and an increase of school resources due to the newspaper campaign improve enrolment outcomes.[13] Columns 3–8 of Table 8 show that these results are robust to controlling for the change in PLE scores and the change in the number of teachers.

However, results from the second-stage computed at the school level could be driven by sorting or a violation of the exclusion restriction. We address these issues in the following sections.

4.2.2. Testing for sorting. Table 9 is similar to columns 7 and 8 of Table 8, but we estimate Equations (3) and (4) at the district level using the mean values of the dependent and explanatory variables. Table 8's results are robust, although the point estimate for the coefficient of $\widehat{\Delta s}$ is lower at the district level. While this suggests that sorting may be at stake, the coefficient remains positive and statistically significant in Table 9. Sorting thus does not explain the entire effect of the results estimated in Table 8.

Although not a perfect test, it complements RS (2005). As explained in Section 2.3.4, RS (2005) test for sorting only with Björkman's (2004) (now Björkman, 2007) approach. Nor do they fully address sorting in their 2011 paper. As explained on p. 965, their '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 the primary school to attend.' RS (2011) do ensure that their results

Table 8. Impact of the newspaper campaign on enrolment of P7 students

	Dep. var.							
	Δs	Δ P7 enrol.	Δs	Δ P7 enrol.	Δs	Δ P7 enrol.	Δs	Δ P7 enrol.
	95–01	95–01	95–01	95–01	95–01	95–01	95–01	95–01
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance to a newspaper outlet	−8.43***		−8.45***		−8.36***		−8.39***	
	(3.03)		(3.03)		(3.06)		(3.05)	
$\widehat{\Delta s}$		0.68**		0.68**		1.09**		0.85**
		(0.33)		(0.34)		(0.50)		(0.34)
Δ PLE scores			−0.36	−0.80			−0.32	−1.00[a]
			(0.69)	(0.63)			(0.69)	(0.70)
Δ nb teach.					−0.21	1.63[a]	−0.18	1.92*
					(0.52)	(1.12)	(0.52)	(1.10)
Number of schools	171	171	171	171	170	170	170	170

Notes: Δ*s* represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

Table 9. Impact of the newspaper campaign on enrolment of P7 students: analysis at the district level

	Dep. var.	
	Δs	Δ P7 enrol.
	95–01	95–01
	(1)	(2)
Distance to a newspaper outlet	−22.47**	
	(9.02)	
$\widehat{\Delta s}$		0.74**
		(0.37)
Δ PLE scores	−1.41	−1.38**
	(1.20)	(0.67)
Δ nb teach.	−1.68	2.51
	(3.58)	(3.24)
Number of districts	21	21

Notes: Δ*s* represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

(those similar to columns 1 and 2 of our Table 8) hold when they control for the distance to the closest school students could attend. However, sorting can still occur even when it requires more effort to change schools. We therefore believe that estimating Equations (3) and (4) at the district level addresses this issue more thoroughly.

4.2.3. Testing support for the exclusion restriction. The coefficient of distance to a newspaper outlet should be statistically significant only over the 1995−2001 period which is partly post-campaign. We

Table 10. Distance to a newspaper outlet and change in enrolment of P7 students

	Dep. var.: Δ P7 enrol.							
	91–95	95–01	91–95	95–01	91–95	95–01	91–95	95–01
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance to a newspaper outlet	−2.52*	−3.85**	−2.62*	−3.96**	−2.33	−5.60***	−2.33	−5.88***
	(1.51)	(1.84)	(1.51)	(1.86)	(1.94)	(1.99)	(1.95)	(1.95)
Δ PLE scores			−0.18	−0.34			−0.13	−1.40***
			(0.22)	(0.32)			(0.29)	(0.47)
Δ nb teach.					2.09***	1.54*	2.11***	1.66*
					(0.62)	(0.84)	(0.62)	(0.85)
Number of schools	233	281	233	281	117	181	117	181

Notes: Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

investigate the exclusion restriction by comparing the relationship between distance to a newspaper outlet and change in enrolment of P7 students over the 1991−1995 period (column 1 of Table 10) and the 1995−2001 period (column 2 of Table 10). We add controls step-wise in the remaining columns.

Table 10 shows a negative and statistically significant effect of distance to a newspaper outlet on the change in enrolment in both columns 1 and 2, violating the exclusion restriction. Working on balanced panel data does not allow us to support the results in column 2 of RS's (2011) Table 4, which show no significant relationship between distance to a newspaper outlet and change in enrolment during the pre-campaign period. However, as soon as we control for the change in the number of teachers (column 5), the coefficient of distance to a newspaper outlet loses statistical significance for the 1991−1995 period while it remains statistically significant for the 1995−2001 period. The inclusion of this control treats omitted variables bias since, as emphasised and shown in section 5.2, the change in the number of teachers is correlated to both distance to a newspaper outlet and the change in the number of P7 students. Treating this omitted variables bias is key to providing support for the exclusion restriction, as shown in columns 5–8 of Table 10.

4.3. Effect of the newspaper campaign on PLE scores of P7 students

4.3.1. Estimating Equations (3) and (4). Column 1 of Table 11 reports the IV estimates of Equation (4) when Δy represents the change in PLE scores of P7 students from 1995 to 2001. Results from the second-stage reveal no statistically significant impact of the change in the share of capitation grants reaching the school on the PLE scores of P7 students (the coefficient of $\widehat{\Delta s}$ is far from statistically significant, with a p-value equal to 0.885). Our results are thus inconsistent with those reported by RS (2011) in column 1 of their Table 8. But, as already noted, RS (2011) only demonstrate this effect of grant share on learning with a reduced form approach. This reduced form approach relies on a fixed-effect estimation with unbalanced panel data covering years 1995, 1997, 2001 and 2002 in which PLE scores of P7 students is regressed on distance to a newspaper outlet. The coefficient of distance to a newspaper outlet is negative and statistically significant (at the 90% confidence level) in this case.[14] However, the reduced form offers no guarantee that the channel driving these results is an increase in capitation grants due to the newspaper campaign.

There are three variables which, if added to Equations (3) and (4), could improve the statistical significance of the coefficient of $\widehat{\Delta s}$ in Equation (4). First, omitting the change in enrolment of P7 students may bias this coefficient downwards. While the change in enrolment is positively correlated with $\widehat{\Delta s}$, it could be negatively correlated with the change in PLE scores. An increase in the number of students might generate strain on resources and lower academic achievement. Alternatively, it

Table 11. Impact of the newspaper campaign on PLE scores of P7 students

	Dep. var.: PLE scores				
	95–01	95–01	95–01	95–01	95–01
	(1)	(2)	(3)	(4)	(5)
$\widehat{\Delta s}$	0.01	0.03	0.01	0.12*	0.16[a]
	(0.04)	(0.05)	(0.04)	(0.07)	(0.10)
Δ P7 enrol.		−0.03[a]			−0.05*
		(0.02)			(0.03)
Δ nb teach.			0.09*		0.08
			(0.05)		(0.12)
PLE score in 1995				−0.69***	−0.72***
				(0.13)	(0.16)
Number of schools	171	171	170	171	170

Notes: Δ*s* represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

could coincide with an influx of lower-achieving students. Second, omitting the change in the number of teachers in Equation (4) may also underestimate the positive effect of $\widehat{\Delta s}$ on the change in PLE scores (through its positive correlation with distance to a newspaper outlet documented in Section 5.2). The change in the number of teachers is negatively correlated with $\widehat{\Delta s}$, but it may have a positive relationship with the change in PLE scores. Finally, it seems critical to control for the PLE scores of P7 students in 1995. Contrary to the other dependent variables studied in this replication report − as well as in RS (2005, 2011) − PLE scores are bounded from above and cannot exceed 36. In this way, the higher the PLE score in 1995, the lower the increase in PLE scores between 1995 and 2001. (The data confirm that this negative correlation is equal to 59%, statistically significant at the 99% confidence level). Yet, PLE scores in 1995 are positively correlated with $\widehat{\Delta s}$. Consequently, this pattern is likely to bias the coefficient downwards, leading us to fail to reject the null.

Columns 2 through 5 of Table 11 report the second-stage OLS estimates when we control, sequentially, for the change in enrolment of P7 students (column 2), the change in the number of teachers (column 3) and the PLE score in 1995 (column 4). Column 5 controls for all of these variables together. The coefficient of $\widehat{\Delta s}$ becomes statistically significant only when we control for PLE scores in 1995 (see column 4 of Table 11). This result is robust to controlling for the change in enrolment of P7 students and their PLE scores, as shown in column 5 of Table 11. The p-value of the coefficient of $\widehat{\Delta s}$ is equal to 0.100 in this case. Note that all of the first-stage estimates confirm the statistically significant negative relationship between Δ*s* and distance to a newspaper outlet.

Yet, the results from column 5 of Table 11 could be due to sorting or a violation of the exclusion restriction. We address these issues in the following sections.

4.3.2. Testing for sorting. Table 12 reports the first- and second-stage estimates displayed in column 5 of Table 11 when these estimates are computed at the district level. The coefficient of $\widehat{\Delta s}$ grows in magnitude and becomes statistically significant compared to column 5 of Table 11. Results reported in Table 11 are therefore not driven by the sorting of higher-achieving students to schools receiving higher grant shares.

4.3.3. Testing support for the exclusion restriction. We investigate the exclusion restriction by comparing the relationship between distance to a newspaper outlet and change in PLE scores over the 1991−1995 period (column 1 of Table 13) and over the 1995−2001 period (column 2 of Table 13).

Table 12. Impact of the newspaper campaign on PLE scores of P7 students: analysis at the district level

	Dep. var.	
	Δs	Δ PLE scores
	95–01	95–01
	(1)	(2)
Distance to a newspaper outlet	−15.30**	
	(5.56)	
$\widehat{\Delta s}$		0.27**
		(0.11)
Δ P7 enrol.	0.26	−0.18*
	(0.32)	(0.09)
Δ nb teach.	−2.18	1.00
	(3.89)	(1.06)
PLE score in 1995	0.65	−0.61[a]
	(1.78)	(0.41)
Number of districts	21	21

Notes: Δ*s* represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

Table 13. Distance to a newspaper outlet and change in PLE scores of P7 students

	Dep. var.: Δ PLE scores									
	91–95	95–01	91–95	95–01	91–95	95–01	91–95	95–01	91–95	95–01
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance to a newspaper outlet	−0.60*	−0.34	−0.63*	−0.39	−0.04	−0.20	−1.12***	−1.23***	−0.73*	−1.02***
	(0.34)	(0.30)	(0.35)	(0.30)	(0.49)	(0.36)	(0.29)	(0.25)	(0.41)	(0.29)
Δ P7 enrol.			−0.01	−0.01					0.03[a]	−0.03***
			(0.02)	(0.01)					(0.02)	(0.01)
Δ nb teach.					0.13	0.09**			−0.03	0.03
					(0.11)	(0.04)			(0.11)	(0.05)
Initial PLE scores							−0.62***	−0.67***	−0.61***	−0.56***
							(0.06)	(0.05)	(0.10)	(0.07)
Number of schools	233	281	233	281	117	181	233	281	117	181

Notes: Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

Column 1 of Table 13 reports a negative and statistically significant effect of distance to a newspaper outlet on the change in PLE scores. As before, working on balanced panel data does not support the results reported by RS (2011) in column 3 of their Table 4. This persists even when we control simultaneously for the change in enrolment of P7 students, for the change in the number of teachers and for PLE scores in 1991 (column 9 of Table 13).[15] Put differently, the positive and statistically significant impact of $\widehat{\Delta s}$ on the change in PLE scores between 1995 and 2001 (column 5 of Table 11) could be driven by omitted variables correlated to distance.

Table 14. Impact of the newspaper campaign on PLE scores of P7 students: controlling for competing channels

	Dep. var.: Δ P7 scores	
	(1)	(2)
$\widehat{\Delta S}$	0.21	0.11
	(0.19)	(0.11)
Δ P7 enrol.	−0.06[a]	−0.05*
	(0.04)	(0.03)
Δ nb teach.	0.05	0.07
	(0.12)	(0.10)
PLE score in 1995	−0.72***	−0.71***
	(0.20)	(0.13)
Wealth in 1995	−0.00	
	(0.00)	
Distance to an urban centre		−0.69
		(0.77)
Number of schools	170	170

Notes: Δ*s* represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

Obvious candidates are those factors which capture the level of development and/or remoteness of the area in which a school is located, such as the district's consumption level in 1995 and a school's distance to an urban centre. In Table 14, we test whether the results reported in column 5 of Table 11 are robust to controlling for both of these characteristics. The coefficient of $\widehat{\Delta s}$ is no longer statistically significant. Consequently, we cannot support RS;s (2005, 2011) claim that an increase in the share of received capitation grants due to the newspaper campaign improves learning outcomes.

5. Theory of change analysis

This section investigates if proximity to a newspaper outlet influences other educational characteristics. It amounts to a limited TCA, in that we explore only a subset of additional relationships that may help to explain the results of the original study. In our replication plan, we proposed investigating a wider range of additional educational characteristics relevant to the interpretation of the results. Though this is not feasible due to data constraints, we instead use variables available across both PETS: PTA contributions, teacher salaries, and the number of teachers.

5.1. Speculating on outcomes

The introduction of UPE led to the abolition of tuition fees and PTA charges for primary education.[16] Could distance to a newspaper outlet influence the overall decrease in PTA fees between 1995 and 2001? For example, parents in less remote schools may receive better information, making them more likely to be aware of the fact that they must no longer contribute to the PTA. However, a variety of media outlets disseminated information about the UPE. We therefore suspect parents' UPE awareness to be orthogonal to their distance. The decrease in PTA contributions between 1995 and 2001 should therefore not be systematically different in schools closer to a newspaper outlet.

We turn to the change in teacher salaries between 1995 and 2001. On one hand, the newspaper campaign could reduce salaries, since it aimed to create greater transparency with respect to capitation grants but not other transfers. The 2002 PETS shows that, of 388 schools, 84 per cent

publicly display UPE capitation grants received from the district, whereas only 30 per cent do the same for teacher salaries. Local officials discouraged from grant capture may therefore turn to salary capture. On the other hand, the newspaper campaign might induce treated schools to mobilise in order to receive *all* of the funds to which they are entitled (teacher salaries included). In other words, the newspaper campaign could positively affect salaries. But it is also possible that teacher salaries were not among the public funds monitored by local officials. As RS (2004) describe, the central government was responsible for the teacher payroll, implying a higher level of oversight. Moreover, teachers were assuredly better informed about the value of their own salaries than were head teachers about the value of their capitation grants. If so, we should expect orthogonality between the change in teacher salaries from 1995 to 2001 and distance to a newspaper outlet.[17]

For teachers recruited by the central government, the district managed hiring and placement decisions. In theory, a teacher had no choice about assignment. Prior to the campaign, the number of teachers in a given school was negatively and significantly correlated with distance to a newspaper outlet, even when accounting for the number of students enrolled. This suggests that schools closer to a newspaper outlet had more than their fair share of teachers. These schools are not among the poorest (as we show in Table 6), implying that the unequal allocation was not to help disadvantaged schools.

This inequality rather suggests that, prior to the newspaper campaign, district officials in charge of placement may have sorted teachers due to other reasons: pressure of teachers anxious to avoid remote postings or pressure of schools to acquire as many teachers as possible, with less remote schools better able to lobby, bribe, and so forth. Could the newspaper campaign have changed this pattern? We know that the newspaper campaign substantially reduced local officials' ability to capture public funds. This might have induced the most corrupt officials (those the most prone to cede to bribes/pressures) to change their behaviour or relocate. If so, we could observe that the newspaper campaign allowed for a more balanced allocation of teachers across schools. More precisely, the change in the number of teachers between 1995 and 2001 would depend positively on distance to a newspaper outlet.[18]

In the following section, we test three hypotheses. The first is whether distance to a newspaper outlet is orthogonal to the change in PTA contributions between 1991 to 1995 and 1995 to 2001. The second and third hypotheses tests if distance to a newspaper outlet influences the change in teacher salaries and the change in the number of teachers over the same two time periods, respectively.

5.2. Testing the outcomes

Columns 1 and 2 of Table 15 report the OLS coefficients of an estimation which regresses the change in PTA contributions on distance to a newspaper outlet. Column 1, over the 1991–1995 period, shows that distance to a newspaper outlet is orthogonal to the change in PTA fees prior to the UPE reform. Column 2, over the 1995–2001 period, reveals that this relationship is unaffected by the UPE reform: the decrease in PTA contributions between 1995 and 2001 is no different in schools closer to a newspaper outlet. This result provides additional evidence that parents' knowledge of the UPE reform is orthogonal to distance.

In columns 3 and 4 of Table 15, we analyse the effect of distance to a newspaper outlet on the change in teachers' salaries from the central government. Prior to the newspaper campaign (1991 −1995), there is a positive relationship between the change in teacher salaries and distance to a newspaper outlet (column 3). This relationship becomes negative (though not statistically significant) after the campaign from 1995−2001 (column 4). If anything, the newspaper campaign induced treated schools to mobilise in order to receive all of the funds to which they are entitled (teacher salaries included), thereby countering the positive relationship between the change in teacher salaries and distance to a newspaper outlet observed prior to the campaign.

In columns 5 and 6 of Table 15, we analyse the impact of distance to a newspaper outlet on the change in the number of teachers over the 1991−1995 and 1995−2001 periods, respectively. Distance

Table 15. Distance to a newspaper outlet and complementary school characteristics

	Dep. var.					
	Δ PTA charges	Δ PTA charges	Δ teach. sal.	Δ teach. sal.	Δ nb teach.	Δ nb teach.
	91–95	95–01	91–95	95–01	91–95	95–01
	(1)	(2)	(3)	(4)	(5)	(6)
Distance to a newspaper outlet	−0.22	−0.43	0.86*	−0.05	−0.22	0.80**
	(0.38)	(0.45)	(0.49)	(0.37)	(0.34)	(0.37)
Robust to controlling for						
Δ nb teach.	yes	yes	yes	yes	no	no
Δ PTA charges	no	no	no	no	yes	yes
Δ teach. sal.	no	no	no	no	yes	yes
Δ P7 enrol.	no	no	no	no	yes	yes
Number of schools	153	212	153	212	152	212

Notes: Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, **, and *** indicate statistical significance at the 85 per cent ($p < 0.15$), 90 per cent ($p < 0.10$), 95 per cent ($p < 0.05$), and 99 per cent ($p < 0.01$) confidence levels, respectively.

to a newspaper outlet is orthogonal to the change in the number of teachers prior to the newspaper campaign (column 5). However, after the campaign, the relationship becomes positive and statistically significant (column 6). This suggests that the newspaper campaign generated a more balanced allocation of teachers across schools since schools closer to a newspaper outlet had more than their fair share of teachers prior to the campaign.

6. Discussion

This replication study confirms the key finding of RS (2005): that the newspaper campaign launched in Uganda in 1997 reduced the capture of government capitation grants transferred to schools by increasing information and awareness among head teachers. We find that a one standard deviation (2.16 km) decrease in distance to a newspaper outlet increases the share of funding that reaches a school by 0.24 standard deviations or roughly 10 percentage points (Table 8, column 7). In their column 1 of Table 7, RS (2011) document an increase in funding of 0.23 standard deviations or roughly nine percentage points. This result shows that citizen-oriented campaigns can reduce corruption in public service delivery.

The Ugandan case can be compared to similar changes in educational financing in Madagascar: in 2002, the government of Madagascar overhauled education financing by covering the tuition fees of all students in public primary schools. A number of monitoring devices accompanied this policy change. The government (i) required schools to post the amount of money received (though only 29% of schools complied) and submit budget plans, (ii) imposed district-level audits (though anecdotal evidence suggests those schools affected were closest to the capital), and (iii) promoted the campaign through mass media (newspapers, radio, and television). Francken et al. (2009) show that remoteness (and therefore less top-down monitoring) increased the amount of capture while access to information for bottom-up monitoring was important for reducing corruption. Though the authors do not study the response of educational outcomes to changes in capture, these results are in line with the conclusions of RS (2005, 2011).

Whether additional funding improves educational outcomes is a more complicated issue. We provide support for RS's (2005, 2011) conclusion that increasing the share of funding allocated to schools improves enrolment. RS (2011) find that a one standard deviation increase in the share of

funding increases enrolment of P7 students by 1.2 standard deviations or 29 students (their column 4 of Table 7). Our results are extremely similar: we find an increase in enrolment of 1.1 standard deviations or 24 students (Table 8, column 8). We do not, however, document a positive impact of an increase in the share of capitation on student test scores.

Taken in the context of the existing literature on student learning, the weakness or absence of an effect of funding on educational outcomes is not surprising. A simple increase in school resources does not necessarily translate into learning gains for the average student (for example, Glewwe, Kremer, and Moulin, 2009). Of the 43 high-quality studies (13 of which are randomised evaluations) from the period 1990–2010, Glewwe, Hanushek, Humpage, and Ravina (2013) show that most school inputs matter little for improving educational outcomes. Combined with the findings on enrolment outcomes, they are in line with Kremer et al. (2013) who conclude based on a summary of education-oriented RCTs that (i) reducing the costs of school attendance tends to increase enrolment but has little to no impact on learning gains and that (ii) the provision of resources themselves, unless augmented by additional reforms, does not improve average achievement outcomes.

Finally, we show that the newspaper campaign allows for a more balanced allocation of teachers across schools. More precisely, our analysis reveals that schools closer to a newspaper outlet lose teachers to the benefit of remoter schools after the launch of the newspaper campaign, suggesting that those schools closer to a newspaper outlet had more than their fair share of teachers before the campaign. Relying on column 6 of Table 15, we find that a one standard deviation (2.16 km) increase in distance to a newspaper outlet increases the number of teachers by 0.15 standard deviations, hence by roughly one teacher. This pattern may be due to the change in behaviour or departure of the most corrupt district officials. This reallocation could explain the absence of an effect of the campaign on learning, depending on the quality of teachers who relocate and on whether grants and teacher quality are complements or substitutes. If grants improve learning by compensating for low-quality teachers, the absence of an effect on learning could result from lower quality teachers relocating to distant schools. If, on the contrary, grants and teacher quality are complements, the absence of effect could be explained by the relocation of higher quality teachers instead. We do not have information on teacher quality, so we unfortunately cannot explore this relationship.

7. Conclusion

Reinikka and Svensson (2005, 2011) show that a newspaper campaign in Uganda reduced the capture of primary school capitation grants which, in turn, improved educational outcomes. This study replicates Reinikka and Svensson's (2005) results and examines their robustness. Our replication confirms the overall positive effect of the newspaper campaign. First, its effect of reducing capture is fully robust. Moreover, we find that the additional funding increased enrolment, although we cannot support the authors' finding that it also improves learning outcomes. Finally, our results suggest that the newspaper campaign allowed for a more balanced allocation of teachers across schools. Our contribution therefore calls for the continued application of information campaigns in order to stem corruption and improve service delivery in developing countries.

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Notes

1. We provide the replication plan as part of our Supplementary Materials. These Supplementary Materials also include our Stata data and do-files as well as further information on our data and additional results.
2. RS (2011) explicitly study learning and, although presented as preliminary, RS (2005) do report it in their abstract: 'the reduction in capture had a positive effect on enrolment *and student learning*' (p. 259).
3. RS (2006) discuss issues related to using distance as an instrument in more detail on p. 12.
4. The authors note that, due to security concerns and closures, not all of the original 250 schools were resurveyed in 2002.
5. RS (2005) use two questions to determine the head teacher's knowledge of the capitation grant. The first is 'Do you know the school's entitlement of UPE capitation grant per student in 2001?' (Q58), asked separately for P1−P3 and P4−P7. If head teachers are correct about both groups, we code this variable as one and zero otherwise. Further communication with the authors indicated that they allowed for an error of $\pm 5\%$ in the responses. We do the same. The second question is 'Do you know when the district receives funds for UPE from the Ministry of Finance?' (Q63). Again, we code this variable as one if the head teacher knows when the district receives funds and zero otherwise. The final knowledge variable sums the two previous variables, yielding a value of 0, 1, or 2.
6. It also loses significance if we integrate only the two schools which experienced the smallest decrease (less than 5% of their total enrolment in 1995).
7. Nor do the results change substantively when relying separately on start-of-year and end-of-year enrolment figures.
8. This approach consists in differencing Equation (10) in order to eliminate district-specific fixed effects. A first-difference OLS estimation therefore amounts to Equation (11):

$$\Delta y_j = y_{j,2001} - y_{j,1995} = \chi_0 + \chi_1 newsperschool_j + \Delta w_{6j}.$$

9. We might also expect the effect of the newspaper campaign to be weaker in areas with lower literacy. We test this conjecture in Supplementary Materials Table A2 and show that the interaction between distance to a newspaper outlet and literacy levels is indeed negative and significant in explaining the change in grant capture. We thank an anonymous referee for highlighting the importance of this test.
10. We thank an anonymous referee for making us aware of this second interpretation of these proxy tests.
11. Another potentially competing channel is the potential change in top-down measures to improve institutional quality over the 1995 to 2001 time-period, which could happen to be more pronounced in districts where schools are closer to a newspaper outlet. To test this channel, we use the Afrobarometer survey to control for the change in the perception of corruption of local officials between 2002 and 2000 at the district level (the latter year marks the earliest available Afrobarometer round). Our results, which we provide in Supplementary Materials Table A1, show no correlation between this change and distance to a newspaper outlet. Consequently, the impact of the newspaper campaign on grant capture is unaffected when controlling for the change in the perception of corruption. We thank an anonymous referee for this suggestion.
12. While radio disseminated some information on capitation grants, it was not systematic. Instead, detailed information on the amount and timing of capitation grants appeared in only two national newspapers (The Monitor and The New Vision), not through radio.
13. Results reported in columns 1 and 2 of Table 8 are similar to those reported by RS (2011) in columns 1 and 4 of their Table 7. RS (2011) rely on the *hadimvo* command in Stata in order to identify outliers which leads them to drop more observations than we do. (They estimate Equations (3) and (4) on 166 observations while we rely on all 171 observations.)
14. Note that, for this statistically significant result to emerge, RS (2011) control for region fixed-effects, an approach which is not the default in the rest of their paper.
15. The same is true when we control only for the change in enrolment of P7 students (column 3 of Table 13) or for initial PLE scores (column 7 of Table 13).

16. PTA charges were introduced during the 1970s to complement low teacher salaries. Bategeka and Okurut (2006) explain that 'collections from PTA charges were used as an incentive for teachers and also for the general running of a school. Parents and teachers of respective schools would agree on the amount, which varied from school to school' (p. 2).
17. We ensure in Section 5.2 that our results on the change in teacher salaries between 1995 and 2001 are robust when controlling for the change in the number of teachers.
18. Obviously, the change in the number of teachers may be indirectly affected by distance to a newspaper outlet through the change in PTA fees, teacher salaries, and enrolment. Our results are robust when we control for these variables.

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