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The impact of grants in combination with school-based management trainings on primary education: A cluster-randomized trial in Northern Nigeria☆

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Abstract
Grant disbursals and school-based management interventions are expensive interventions that have recently received growing attention from policy-makers despite their mixed success at delivering improvements in educational outcomes in a cost-effective way. This paper reports results from a large-scale, cluster randomized controlled trial that evaluated two components of the Nigerian Partnership for Education Project (NIPEP) in Sokoto state, Nigeria. School-based management committees received both a training and a grant to improve access to and quality of primary school education, especially for girls. One year after implementation, the intervention had no impact on schools' infrastructure, educational attainment or learning outcome measures. Our results show the importance of understanding the context-specific constraints inhibiting the delivery and uptake of primary school education to avoid spending 100 million USD on a program with no discernable impact.

Keywords: Education, school-based management, student learning, impact evaluation, RCT, Nigeria, developing countries
Classification codes: I21, I28, H52, O15

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Introduction

Education has featured strongly in development efforts in the past decades, with particular attention to increase access to basic education and, more recently, improving its quality to ensure learning. 59 million children of primary school age remained out of school in 2018 (UNESCO, 2019) and on average, children spend 11.2 years in school. Yet, their learning achievements only correspond to those of 7.9 years in school, so children spend on average 3.3 years in school without learning (World Bank, 2020). Determining what works in improving the delivery of education has therefore received growing attention among practitioners and academics but remains a highly context-specific issue. Two prominent approaches have produced mixed results at improving the supply of quality education: First, providing schools with grants to improve their learning environment, e.g. infrastructure or working materials, thereby utilizing communities' insights into which local conditions act as binding constraints to educational attainment, has not proven effective (Newman et al., 2002; Olken, Onishi & Wang, 2014; Das et al., 2003). Second, empowering local school communities by providing management trainings has worked in some contexts (Lassibille et al., 2010) under certain conditions (Pradhan & De Ree, 2014; Blimpo, Evans & Lahire, 2015) but not always (Banerjee et al., 2010; Santibañez, Abreu-Lastra & O'Donoghue, 2014; Glewwe & Maïga, 2011).[1] However, only two of these evaluations (both in Mexico) concerned large-scale interventions involving more than 10,000 primary schools (Garcia-Moreno, Gertler & Patrinos, 2019; Santibañez, Abreau-Lastra & O'Donoghue, 2014).

Here, we assess the joint impact of a grant disbursal plus training program for school-based management committees of Nigerian primary schools. We hypothesize that the combination of empowering local communities in identifying constraints in the supply of quality primary education as well as providing the financial means to alleviate these will improve the educational attainment as well as learning outcomes of primary school students. We do this in a rural, high-poverty setting with a poorly functioning primary school system.

This paper uses a large field experiment with 128 primary schools in rural Nigeria. Half the schools were randomly selected into a treatment where each school-based management committee (SBMC) received a leadership and school management training as well as a school improvement grant. Half of the treatment schools (n = 32) received the normal amount of 250,000 NGN (approx. PPP-adjusted int-$ 2,272) as per NIPEP guidelines while the other half received twice that amount, so 500,000 NGN. The normal grant amount in the rural Sokoto context is enough to pay 10 qualified teachers their entry-level salary for a year, provide 120 students with school uniforms or construct two toilet buildings. Our analysis is based on surveys with headmasters, teachers, SBMC members and 6,000 primary school students. We tested students' literacy and numeracy skills as well.

We find that the intervention had no discernable impact on schools' infrastructure or equipment, enrolment, student or teacher attendance and learning achievements, regardless of high or normal grant amount treatment status. Anecdotally, some schools that had no toilets prior to the intervention seem to have used the grant money to build some

[1] For a good, if slightly outdated, overview see Barrera-Osorio et al. (2009).

toilets. We postulate five potential reasons for the zero result. First, challenges in the implementation may have meant that schools never received any grants, which is potentially corroborated by the low reporting of the intervention – only 50% of SBMC members and headmasters at treatment schools reported receiving an intervention at endline.
Second, schools may have received the grants but then decided to use the grants for school-unrelated matters, which is anecdotally supported by our data showing no improvements in the areas that respondents claimed to have spent the grant money on.
Third, schools' infrastructure was so wanting that the grant amount could be insufficient to alleviate this binding constraint as an input factor into the schooling production function. Even though 83% of sample schools had some sort of permanent structure, these were often in dire condition and / or used for other purposes such as storing harvests.
Fourth, educational attainment may not have been limited by the learning environment realities but by teachers' absenteeism. At 45% of schools, there was no teacher present upon the arrival of enumerators as part of the endline survey, and at 74% no learning was taking place.[2] Improving the school-based management committee's managerial and financial capacity may therefore not have addressed the core issue that constrains educational attainment in the study context.
Fifth, SBMC's capacity may be insufficient for the training and grant disbursal to be converted into primary schools' improvements. Following Blimpo, Evans and Lahire (2015), we estimate heterogeneous treatment effects of the intervention by the literacy rate of schools' SBMCs. We find no differential treatment effect by SBMC capacity, potentially due to the pervasive low baseline capacity with SBMCs' mean literacy being at 44%.

This study contributes to a growing body of literature of school-based management trainings in combination with grants (Beasley & Huillery, 2017; Blimpo, Evans & Lahire, 2015; Garcia-Moreno, Gertler & Patrinos, 2019; Gertler, Patrinos & Rubio-Codina, 2012; Khattri, Ling & Jha, 2012; Yamauchi, 2014). Khattri, Ling and Jha (2012) and Yamauchi (2014) observe positive impacts of school-based management (SBM) reforms in the Philippines, and Gertler, Patrinos and Rubio-Codina (2012) detect reduced grade failure and grade repetition in response to a SBM reform in rural Mexico, though the positive impact vanishes in extremely poor communities. Beasley and Huillery (2017) observe improvements in enrollment and schools' resources in Niger but, with a simultaneous increase in teacher absenteeism, detect no impact on learning outcomes. Blimpo, Evans and Lahire (2015) observe a reduction in student and teacher absenteeism but no impact on student test scores.

This paper makes an important contribution to understanding what does not work in education policy. The intervention evaluated was part of the larger 'Nigerian Partnership for Education Project' (NIPEP), a 100 million USD program funded by the Global Partnership of Education and the World Bank. From 2015 to 2020, more than 28,000 primary schools in five states in Northern Nigeria received School Improvement Grants, so that - excluding administrative costs - the primary school grant component of NIPEP alone already cost approximately 7 billion NGN or 21.6 million USD;[3] money that could have achieved

[2] Consequently, at 29% of schools, teachers were present but not teaching.
[3] The School Improvement Grants were delivered to 28,049 primary schools (World Bank, 2021). Administering 250,000 NGN per school yields a total amount of grant money disbursed of 7.012 billion NGN. Using the 2019

substantial learning achievements if spent on projects that deliver impact in a cost-effective way (e.g. Kenya's national literacy program Tusome (Piper, Destefano, Kinyanjui & Ong'ele, 2018)).

The remainder of this paper proceeds as follows: Section 2 describes the setting, sample, experimental design, data and estimation strategies while Section 3 presents the main results. Potential reasons for our null results are outlined in Section 4 before we conclude in Section 5.

2. Experimental design and data collection

2.1. Study setting

The study took place in nine rural and peri-urban Local Government Areas (LGAs) in Sokoto State in the north-west of Nigeria. Sokoto state is the state with the highest poverty headcount rate, with 87.73% of the population living on less than $1,90 a day at 2011 PPP international prices in 2019 (NBS 2020 Poverty and Inequality in Nigeria).

Sokoto performs similarly on socio-demographic dimensions in comparison to nationwide averages. Only 41% of Sokoto's population is literate (Nigeria Living Standards Survey 2018-2019) and of all children aged 5-16 years only 21.8% were literate and 10.6% were numerate (National Population Commission, 2016). Primary school enrolment is less than 60% and primary school attendance only amounts to 40%. Fertility is still high with women bearing on average 7.3 children and only 40% of households have access to electricity (see Table 1).

Table 1
Sociodemographic comparison of Sokoto state and Nigeria

	Sokoto	Nigeria
Average household size	5.93	5.06
Fertility rate	7.3	5.8
Literacy in any language (in %)	40.9	63.2
Gross primary school enrollment rate (% of school age population)	59.6	88.6
Net primary school attendance (% school age population)	40.4	65.8
Access to electricity (% total number of households)	40.1	63.7

Fertility rate data taken from Nigeran National Bureau of Statistics and UNICEF (2017); remaining data taken from Nigerian National Bureau of Statistics (2020).

2.2. Sampling

The sample of 128 primary schools was constructed by selecting nine Local Government Areas (LGAs) of Sokoto state where (a) the program had not yet been implemented and (b) the security situation in June 2018 was deemed safe enough for surveys to take place. The

World Bank's DEC alternative conversion factor of 325,0 results in a total grant amount disbursed to primary schools of 21.576 million USD.

nine LGAs were Binji, Bodinga, Goronyo, Ilela, Kware, Silame, Taumbuwal, Wamakko and Wurno. Schools included in the sampling frame had to be eligible for NIPEP[4] and had to have between 35 and 160 registered Grade 2 students.

Surveys were conducted with the headmaster, additional teachers, available SBMC members and up to 25 Grade 2 and 25 Grade 3 pupils – as part of the endline survey, Grade 4 pupils were also interviewed.[5] Additionally, surveyed pupils were also given short tests in mathematics and Hausa, the local language.

2.3. The intervention: NIPEP

The Nigerian Partnership for Education Project (NIPEP) was a program funded by the Global Partnership for Education, developed by the World Bank and implemented by the Federal Ministry of Education in five states in Northern Nigeria. Its aim was to 'improve access and quality of basic education […], with particular attention to girls' participation.' (World Bank, 2015). The entire program consisted of three major components:

Component 1: Promoting School Effectiveness and Improved Learning Outcomes
(a) School Improvement Grants to Primary Schools
(b) School Improvement Grants to Pre-Primary Schools
(c) Support to Teachers' Professional Development

Component 2: Increasing Access to Basic Education for Out-of-School Children with focus on Girls
(a) Scholarships for Girls
(b) Scholarships for Female Teachers
(c) Community Mobilization and SBMC Training

Component 3: Strengthening Planning and Management Systems including Learning Assessment and Capacity Development
(a) Management and Implementation Support (for the Federal Ministry of Education)
(b) Monitoring, Evaluation and Learning Assessment

Grant and SBMC Training components

The evaluation of the intervention presented here focused on Components 1(a) and 2(c) above, so that schools in the sample only received School Improvement Grants and the training program for the School-Based Management Committee (SBMC). The School Improvement Grants (SIG) amounted to 250,000 Nigerian Naira (approx. PPP-adjusted int-$

[4] Primary schools were eligible for receiving a School Improvement Grant (SIG) if they had (i) a functioning SBMC, (ii) received SBMC training in the administration of SIGs, (iii) a School Improvement Plan (SIP), and (iv) established a functioning bank account (World Bank, 2015).
[5] In most schools, less students were enrolled or present on the day of the survey, so that simply all Grade 2 and 3 students were interviewed. If more than 25 students were available per grade, at baseline a random sample was supposed to have been drawn but field observations showed these were unsuccessful. Therefore, at endline, a convenience sampling methodology was officially adopted.

2,272) and were intended for 'non-salary expenditures related to improving school effectiveness, and the quality of learning and teaching' (World Bank, 2015, pg. 34). As part of the evaluation, half the 64 treatment schools received this amount ('normal') whereas the other half received twice the amount, 500,000 Nigerian Naira ('high'). The SBMC training contained leadership and school management skills as well as the importance of community involvement when taking decisions. The SBMC was to draw up a School Improvement Plan before receiving the grant to determine the priority areas in which improvements were deemed necessary for the school.

2.4. Experimental Design and Timeline

The 128 schools in the study sample were randomly assigned to either the treatment (receiving the grant and SBMC training) or control group (no intervention) with equal probability and the treatment schools were then again randomly divided into the normal grant and the high grant groups with equal probability. After a pilot study that tested the difficulty and wording of the student tests, the collection of baseline data took place in July and August 2018. Subsequently, the intervention was implemented and 14 months later, the endline data were collected in November and December 2019. Questions in the endline surveys were adjusted so that the correct school year (2018/19) was referred to. The study did not provide any monetary incentives for participation but rewarded students with a cookie for completing the survey.

2.5. Data

Baseline and endline data were collected via standardized questionnaires by enumerators fluent in the local dialect of Hausa. Answers were recorded by enumerators on tablets. Questions were drawn up in English, translated and back-translated to Hausa and available in Hausa on the tablets. Enumerators were recruited from local communities by the survey firm to ensure familiarity with the local dialect of the Hausa language. They were trained to create an encouraging, trusting and private environment for primary school children and to emphasize that their answers were confidential and would not impact their grades in school. Student interviews were conducted with both the enumerator and the child seated on a mat to make the child feel comfortable with the enumerator. At times, it proved difficult to create a private setting for the surveys, especially for the student tests, because interviews often had to be conducted outside in the shade of school buildings or trees where other curious children could easily pass or watch. Frequent reminders by the supervising team to the enumerators were given to ensure privacy with mixed success.

At each school, five different surveys were administered. First, the team leader of the enumerator group would fill out an observational questionnaire that collected impressions of the infrastructure, people present upon arrival and their activities as well as information copied from the school registries (enrolment and attendance records of students and teachers) if available. Furthermore, three separate questionnaires were designed and administered with each primary school's headmaster, additional teachers and members of their School-Based Management Committees (SBMC). Data on their demographics,

attitudes, their perception of the school's challenges, any interventions or trainings the school might have received in the past year, and activities and characteristics of the SBMC were collected.

The pupil surveys started with the numeracy and the literacy test and subsequently collected some information on their demographics and opinions on their primary school. The mathematics and Hausa questions started out with the easiest tasks – counting and recognizing single digits or letters – and became progressively more difficult. If a student answered a question incorrectly, a second question of the same difficulty level had to be answered correctly before moving on to the next difficulty level. Failing that, the questionnaire automatically moved on to the next sub-section to avoid frustrating the children with too many questions they were unable to answer.

At baseline, we interviewed 5,717 Grade 2 and 3 students, 88 headmasters, 181 teachers and 285 SBMC members while at endline, we interviewed 6,013 Grade 2, 3 and 4 students, 99 headmasters, 175 teachers and 348 SBMC members. It was not feasible to track and match students from the baseline to the endline survey, so we consider our data as independently pooled cross sections.

2.6. Estimation strategies

The random allocation of schools into treatment and control group allows us to establish a credible counterfactual so that any treatment effects we identify can be causally associated with the intervention. This assumes that randomization was successful at creating control and treatment groups that are balanced – we display the balance of some observables in Table 2 below.

First, we estimated the average effect of belonging to a school in the treatment group, the intent-to-treat effect (ITT), on each outcome variable Y, using endline data. In the cases where outcome variables were collected only once per school (observations, headmaster survey), the estimation was as follows:

$$Y_i = \alpha_0 + \alpha_1 T_i + \varepsilon_i$$

where T_i is a dummy variable for belonging to a treatment group school i and ε_i being an error term for school i. In the case of the outcome variable having multiple observations per school (students, teachers, SBMC members), standard errors were clustered at the unit of randomization, the school:

$$Y_j = \alpha_0 + \alpha_1 T_i + \varepsilon_{ij}$$

where j refers to an individual surveyed.

In the Appendix, we present two alternative specifications: First, we add the baseline values of the outcome variable as a control variable to our Intent-to-treat OLS estimation (ANCOVA). In the case of students' learning achievements where we have several

observations per school, standard errors were again clustered at the unit of randomization, the school. In other words, ε_i is replaced with ε_{ij}:

$$Y_i = \alpha_0 + \alpha_1 T_i + Y_{i(t-1)} + \varepsilon_i$$

where $Y_{i(t-1)}$ is the lagged outcome variable at baseline.

Second, we use a Differences-in-Differences estimator for which we assume (i) that treatment and control groups were on parallel trends before the introduction of the intervention and (ii) that participants were unable to select into treatment group schools. We only have baseline data from one point in time before the intervention, so we cannot test assumption (i). However, the balance of baseline observables gives an indication of whether treatment and control group are comparable along observed characteristics at one point in time. Assumption (ii) holds as the randomization of schools into treatment and control groups took place before but was not revealed (with the implementation of the intervention) until after the baseline survey so that respondents were unable to select into treatment schools.

$$Y_{it} = \alpha_0 + \alpha_1 T_i + \alpha_2 E_t + \alpha_3 T_i * E_t + \varepsilon_{it}$$

where E_t is an endline dummy variable equal to 1 if the data point was collected as part of the endline survey, i.e. after the intervention was implemented.

Given that many headmasters and SBMC members at treatment schools did not report receiving a grant at endline, we also run an Instrumental Variable analysis estimating the Complier Average Causal Effect (CACE). The first-stage regressions are:

$$X_i = \beta_0 + \beta_1 T_i + \epsilon_i$$

where X_i is either a dummy variable for if the headmaster or any SBMC member mentioned the school receiving any grant or a dummy variable for when anyone mentioned a training and ϵ_i is an error term for school i. As before, we cluster standard errors at the school level for all individual-level outcomes, such as student test scores.

Since we test 13 different education outcomes (see Table 4), there is a heightened probability of falsely rejecting at least one null hypothesis (Anderson, 2008). Hence, we correct standard errors for multiple hypothesis testing using the Benjamini-Hochberg method and report a second set of statistical significance levels in Tables 2, 4, A1 and A3 where we present our main results (Benjamini & Hochberg, 1995).

3. Results

3.1. Summary statistics and Balance

In Table 2, we document our baseline results and the balance checks for whether randomization was successful. On average, schools in the study sample had 190 enrolled

students, 39% of which were female. Students' numeracy and literacy skills at baseline were very poor: Out of a maximum score of 20, Grade 2 students scored 6.2 points in mathematics and 1.6 points in Hausa, while Grade 3 students scored 8.8 points in mathematics and 3.8 points in Hausa. Upon enumerators' unannounced arrival on the day of the baseline survey, on average less than one teacher (0.71) was present and, according to school registries (only available at 108 of the 128 schools), 36% of pupils were absent.

The infrastructure and equipment of the school was also recorded and summarized in three indices ranging from 0 to 1 (shown in the first three lines of Table 2 below). Some examples of the more detailed measurements of the schools' learning environments, sanitation and facilities used in creating the indices are presented in Table 3. To create the indices, all measures were scaled to the same range and an unweighted mean was taken of all the measures. Table 3 corresponds to a list of all variables included in the indices.

Schools had on average 3 classrooms but for example working material and school uniforms were unavailable at the large majority of sample schools. Only 11% of schools had any kind of water supply, only 22% had any toilets and only 5% had access to electricity.

The randomization produced a treatment and control group that were on average balanced across outcome measures, with the exception of having any toilets available (higher likelihood at treatment schools, p-value 0.08) learning taking place (more likely upon enumerators' arrival in control than in treatment schools, p-value 0.04) and Grade 2 students' literacy (Control school students performed better on the Hausa test than their peers at treatment schools, p-value 0.06). Even though these significance levels do not hold up to multiple hypothesis testing, we choose to run specifications different to ITT OLS in these three cases. For the toilets and the learning taking place measures, we therefore apply an ANCOVA estimation strategy in Table 4. For the literacy outcome of Grade 2 pupils, we use mean test scores on the school level as the unit of analysis, which is balanced at baseline.

Table 2
Sample characteristics and balance checks

	Sample	Control		Treatment		Difference in means	p-value of difference in means
		N (1)	Mean (2)	N (3)	Mean (4)	(C-T) (5)	(6)
Summary indices							
Quality of the learning environment[1]	0.336	64	0.343	64	0.323	0.021	0.45
Sanitation[1]	0.219	64	0.240	64	0.188	0.052	0.27
School facilities[1]	0.152	64	0.157	64	0.150	0.007	0.82
Any toilets	0.220	63	0.286	64	0.156	0.129	0.08*
Pupil enrollment							
Total enrollment	190	64	166	64	214	-48	0.27
Female enrollment rate	0.394	54	0.397	54	0.391	0.006	0.82
Pupil attendance							
No. students observed in Grade 2 / 3	53	64	55	64	50	4.4	0.60
Pupil absence rate according to registry	0.356	54	0.310	54	0.403	-0.093	0.15
Teacher attendance							
Number of teachers present	0.711	64	0.859	64	0.563	0.297	0.22
Lessons							
Any learning taking place	0.234	64	0.313	64	0.156	0.156	0.04*
Learning achievement scores (out of 20)							
Grade 2 math	6.22	1,542	6.29	1,701	6.16	0.131	0.33
Grade 3 math	8.79	1,299	8.79	1,163	8.78	0.010	0.96
Grade 2 literacy	1.61	1,542	1.73	1,701	1.51	0.227	0.06*
Grade 2 school means literacy	1.58	59	1.71	62	1.46	0.251	0.471
Grade 3 literacy	3.77	1,299	3.84	1,163	3.70	0.215	0.52

[1]These variables are indices ranging from 0 to 1 with 0 representing a poor and 1 a good outcome.
Statistical significance levels (10%, 5%, 1%) based on naïve p-values represented with */**/*** and based on Benjamini-Hochberg adjusted p-values corresponding to a 5% significance level represented with ‡.

Table 3
Input variables selected for three summary indices

	Baseline mean (1)
Quality of the learning environment	
Condition of the school building[1]	2.52
Number of classrooms	3.04
Condition of classrooms[1]	2.49
Any blackboard in the classroom	0.489
Benches, chairs and tables[2]	0.36
Books[3]	3.87
Working material[3]	5.34
Any educational posters	0.052
Pupil uniforms[3]	4.92
Sanitation	
Any water supply	0.11
Any toilets	0.22
Any faeces around the compound	0.31
School facilities	
Any headmaster's office	0.50
Any staff room	0.14
Any storage room for learning materials	0.09
Any power supply	0.05
Number of observations	128

[1] Scale of 1 to 5 where 1 is very poor and 5 very good.
[2] Scale of 1 to 5 where 1 is sufficiently available for all pupils, 2 is 'All pupils seated but more children per chair/bench than designated, 3 is 'More than half of pupils sit on chairs/benches', 4 is 'Less than half …' and 5 is 'No chairs / benches available'.
[3] Scale of 1 to 6 where 1 is equivalent to 'more than ¾', 2 corresponds to '¾ to 1/2', 3 corresponds to 'One half', 4 corresponds to '¼ to ½', 5 corresponds to 'less than ¼' and 6 corresponds to 'None'.

3.2. Impacts on infrastructure, educational attainment and learning outcomes

Results of the outcomes on infrastructure, enrolment, attendance and learning outcomes are reported in Table 4. For each outcome we present the intent-to-treat estimates in column (1) and the instrumental variable estimates in columns (2) and (3). With one exception, the intervention had no discernable impact across outcomes and specifications. This proves robust to the alternative ANCOVA and Differences-in-Differences (DiD) specification (Appendix Table A1) and to differentiating between the normal and high grant amount treatment schools (Appendix Table A3).

The mentioned exception is the dummy variable relating to whether schools had any toilets. Table 4 shows that the ANCOVA and CACE estimates are significant at the conventional 1% and 10% statistical significance levels, as are the DiD estimates (Table A1). However, this statistical significance disappears when correcting for multiple hypothesis testing with the exception of the ANCOVA specification where the positive impact of the intervention on school's toilets proves robust to the Benjamini-Hochberg correction. We therefore treat this treatment effect as anecdotal evidence.

Table 4
ITT and CACE estimates for outcomes of interest

	ITT OLS (1)	CACE IV (grant) (2)	CACE IV (training) (3)
First stage for IV estimators			
Treatment assignment		0.672***	0.377***
		(0.062)	(0.064)
F-statistic		118.93	35.12
Infrastructure and equipment			
Quality of the learning	0.036	0.061	0.109
environment[1]	(0.031)	(0.046)	(0.082)
Sanitation[1]	0.069	0.109	0.199
	(0.048)	(0.074)	(0.134)
School facilities[1]	-0.009	-0.023	-0.042
	(0.035)	(0.055)	(0.100)
Any toilets[2]	0.232***/‡	0.212*	0.386*
	(0.073)	(0.125)	(0.228)
Pupil enrollment			
Total enrollment	51.96	75.2	134
	(41.65)	(63.0)	(112)
Female enrollment rate	0.0094	0.017	0.028
	(0.041)	(0.054)	(0.090)
Pupil attendance			
Any students present	-0.059	-0.091	-0.249
	(0.084)	(0.085)	(0.236)
Teacher attendance			
Any teacher present	-0.073	-0.155	-0.282
	(0.090)	(0.137)	(0.253)
Any learning taking place[2]	0.020	-0.014	-0.025
	(0.081)	(0.123)	(0.223)
Normalized learning achievement scores			
Grade 2 numeracy	-0.069	-0.125	-0.233
	(0.100)	(0.166)	(0.314)
Grade 3 numeracy	-0.036	-0.140	-0.234
	(0.126)	(0.199)	(0.334)
Grade 2 school means literacy	-0.067	-0.137	-0.238
	(0.104)	(0.166)	(0.295)
Grade 3 literacy	-0.159	-0.316	-0.528
	(0.131)	(0.213)	(0.373)

[1]These variables are indices ranging from 0 to 1 with 0 representing a poor and 1 a good outcome.
[2]These variables were not balanced at baseline; the reported ITT OLS result is from running an ANCOVA specification.
Note: Standard errors in parentheses. Statistical significance levels (10%, 5%, 1%) based on naïve p-values represented with */**/*** and based on Benjamini-Hochberg adjusted p-values corresponding to a 5% significance level represented with ‡. The first-stage results of the instrumental variable specification in columns (2) and (3) were excluded from multiple hypothesis testing corrections.

4. Discussion of potential mechanisms

To understand why the intervention had no discernable impact on primary schools' learning environment or achievement, we explore five different channels to explain the null results. The auxiliary analyses are presented below.

4.1. Implementation challenges

First, the implementation of the grants and SBMC trainings may have been faulty from the responsible NIPEP office in Sokoto so that many treatment schools may have never received any money or trainings. We use the number of respondents that reported whether any intervention took place, a grant was disbursed or a training was offered as an indication for potential challenges in the implementation and find that the reporting of the intervention was very low.

At endline, at only 32 of the 64 treatment schools did any SBMC member report receiving an intervention, and at only 27 treatment schools did any SBMC member report receiving a grant. Similarly, only 26 of the 52 headmasters interviewed at treatment schools reported an intervention and only 17 reported receiving a grant. Many respondents also did not report any training for the SBMC committee (Table 5). The first stage of the instrumental variable estimations (Complier Average Causal Effect CACE), reported in Table 4 above, showed that at 66.7% of treatment schools at least one respondent reported an intervention and at 36.7% at least one respondent reported a training.

Table 5
Reporting of intervention components by headmasters and SBMC members

Subsample: Treatment schools at endline	Likelihood of reporting an **intervention** (1)	Likelihood of reporting a **grant** (2)	Likelihood of reporting a **SBMC training** (3)	Number of respondents (4)
Headmaster	50.0%	32.7%	38.5%	52
SBMC Chairman	50.0%	36.0%	26.0%	50
SBMC Vice Chair	28.6%	35.7%	35.7%	14
SBMC Secretary	61.5%	53.8%	15.4%	13
SBMC Treasurer	50.0%	50.0%	12.5%	8
SBMC Woman Leader	40.0%	27.3%	18.2%	11
SBMC Pupil representative	0.0%	0.0%	100.0%	1
SBMC Ordinary member	46.6%	32.8%	17.2%	64

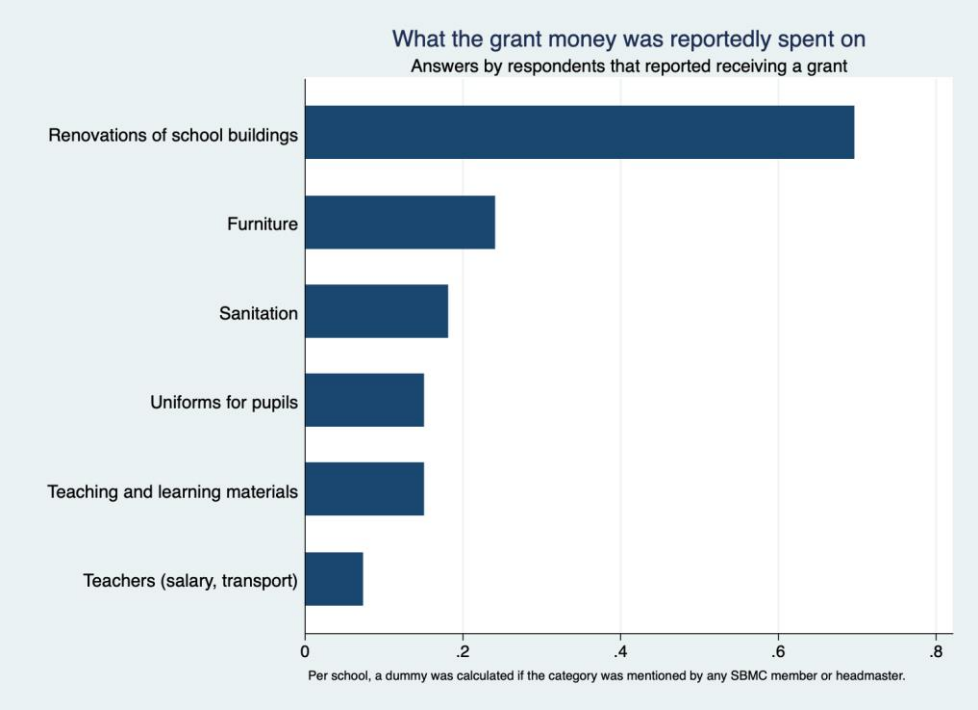
4.2. Spending challenges

Schools may have spent the grants on school-unrelated matters which could be another reason for the intervention to improve learning environments or students' learning achievements. To verify this explanation, we look at what SBMC members and headmasters claim to have spent the grant money on at the treatment schools where at least one person reported receiving a grant (displayed in Figure 1 below) and verify their reported usage of

the grant by testing for changes in the reported spending domain from base- to endline survey.

The most frequently cited use of the grant money was renovations (70%) and furniture (24%). These reported grant usages need to be interpreted with caution, though, as only 65 headmasters or SBMC members at treatment schools reported a grant in the endline survey, creating a non-random subsample of only 33 out of the 64 treatment schools.

Figure 1
Frequency of grant use mentions (at treatment schools where grant was reported)



To verify whether spending the grant money in the reported way was detected by our observational data, we run two sets of regressions. As our sample is non-random, we cannot use an ITT OLS specification as the treatment schools that report using the grant for renovations are likely to be systematically different to the treatment schools that do not, creating bias. Therefore, our first verification uses an OLS regression which includes the baseline value of the independent variable as a control variable (ANCOVA). Second, we use a Differences-in-Differences estimator:

Column (1) of Table 7 reports α_1: $Y_i = \alpha_0 + \alpha_1 X_i + Y_{i(t-1)} + \varepsilon_i$
Column (2) of Table 7 reports β_3: $Y_{it} = \beta_0 + \beta_1 X_i + \beta_2 E_t + \beta_3 X_i * E_t + \varepsilon_{it}$

where $Y_{i(t-1)}$ is the lagged outcome variable at baseline, X_i is the independent variable listed in Table 7's header cells and E_t is an endline dummy variable equal to 1 if the data point was collected as part of the endline survey, i.e. after the intervention was implemented.

The small number of schools left in this subsample make the below regression results merely suggestive as they are based on few observations, e.g. n=23 when the independent variable is 'reported using the grant for renovations'.

Table 7 does not give us confidence in the truthfulness of the headmasters and SBMC members' responses. Whenever respondents claimed for the school to have received a grant, their reported use of the grant money does not translate into improvements in the specified domain. For instance, if schools reported spending the grant on renovations, we do not observe any improvement in the condition of the school building, classrooms, doors and windows from base- to endline survey. One interesting exception is that schools that claimed to have used the grant to build toilets are in fact significantly more likely to have any toilet facility at the time of the endline survey.

Table 7
Comparison of reported grant uses with enumerator observations

Subsample: **Treatment schools where at least one respondent reported a grant**	ANCOVA (1)	DiD (2)
Independent variable: Reported using grant for renovations (dummy)		
General condition of school buildings / compound[1]	-0.124	0.079
	(0.103)	(0.129)
Condition of classrooms[1]	-0.073	0.050
	(0.091)	(0.120)
Condition of doors[1]	-0.090	0.053
	(0.100)	(0.142)
Condition of windows[1]	-0.022	0.081
	(0.103)	(0.145)
Independent variable: Reported using grant for learning materials (dummy)		
Availability of learning materials[1]	0.358	0.453
	(0.211)	(0.334)
Independent variable: Reported using grant for sanitation (dummy)		
Sanitation[1]	0.123	0.213
	(0.135)	(0.191)
Any toilet (dummy)	0.603***	0.631**
	(0.170)	(0.289)
Independent variable: Reported using grant for uniforms (dummy)		
Pupil uniforms[1]	-0.198	-0.375
	(0.405)	(0.368)

[1] These are indices ranging from 0 (very poor) to 1 (very good).
Note: Standard errors in parentheses. Statistical significance levels (10%, 5%, 1%) based on naïve p-values represented with */**/***.

4.3. Insufficient grant amount

If the amount of the grant was insufficient to eliminate the constraints to delivering a quality education, this could explain the zero impact of the intervention as well. Given the very lacking and, if existent, deficient infrastructure of our sample schools (see Table 3 in section 3.1.), the grant of approximately US$ 2,300 could not be enough to make a lasting improvement in the infrastructure such that educational attainment and learning outcomes

are affected. The treatment schools that received twice the grant amount, however, also recorded no improvements along infrastructural or educational dimensions (see Table A3 in the Appendix). This would suggest that even US$ 4,500 were not enough to significantly improve the deficient delivery of education.

4.4. Teacher absenteeism

Instead of schools' financial situation, the binding constraint in delivering quality primary education in the study context could also lie elsewhere: teacher absenteeism. Upon enumerators' arrival for the endline survey, there was no learning taking place at 74% of schools and at 45%, no teacher was even present. In other words, learning can take place anywhere, even in the shade of the tree, but without a teacher present and giving lessons, students are unlikely to learn. Disbursing grants to schools where teachers are regularly missing would therefore not translate into improvements in learning outcomes, either. However, this would not explain why we do not see any improvements along the other main outcome variables, such as infrastructure or learning materials, as the SBMC could have invested in these without teachers being present.

4.5. Capacity of SBMC

Following Blimpo, Evans and Lahire's (2015) argument of the importance of SBMC's local capacity, we assess whether the average literacy[6] of the SBMC members interviewed limited the potential impact of the intervention by interacting it with a treatment assignment dummy.

$$Y_i = \gamma_0 + \gamma_1 T_i + \gamma_2 L_i + \gamma_3 T_i * L_i + \varepsilon_i$$

where T_i is a dummy variable for belonging to a treatment group school i, L_i is the average literacy rate of interviewed SBMC members interviewed at school i, and ε_i is an error term. As before, we cluster standard errors at the school level for all learning achievement outcomes such that the above standard error ε_i becomes ε_{ij} where j refers to a student surveyed. [7]

Table 8 below displays the estimates of the coefficients of interest, γ_3. We find no heterogeneous effects of the intervention by SBMC capacity. In fact, both pupil and teacher attendance even show up as being negatively impacted by higher SBMC literacy rates and receiving the NIPEP intervention. However, the other measure of teacher attendance being a precisely estimated zero coefficient as well as the pupil attendance measure only being significant at the 10% level has us inclined to disregard these statistically significant results. Blimpo, Evans and Lahire (2015) estimated that a minimum of 45% adult literacy was

[6] This measure was constructed as the percent of interviewed SBMC members that confirmed being able to read a letter.

[7] Using an alternative specification (not shown) where we include the baseline values of the outcome variable as a control variable (Ancova) yields the same null results, with the same two exceptions of negative correlations as in the endline OLS specification reported in Table 8.

needed for their intervention to show effects on students' learning outcomes. As only 44% of SBMC members interviewed at endline said they were able to read a letter, SBMC capacity in our study context may indeed fall below that critical literacy threshold. Therefore, we do not disprove Blimpo, Evans and Lahire's (2015) finding but are simultaneously unable to confirm the importance of local capacity for the delivery of education in our study context.

Table 8
Interacting SBMC literacy with treatment assignment

	(1)
Infrastructure and equipment	
Quality of the learning environment[1]	-0.049
	(0.083)
Sanitation[1]	0.008
	(0.133)
School facilities[1]	-0.022
	(0.090)
Pupil enrollment	
Total enrollment	72.69
	(113.6)
Female enrollment rate	-0.079
	(0.113)
Pupil attendance	
Any students present	-0.385*
	(0.223)
Teacher attendance	
Number of teachers present	0.174
	(1.56)
Learning taking place	-0.519**
	(0.211)
Normalized learning achievement scores	
Grade 2 numeracy	-0.125
	(0.161)
Grade 3 numeracy	0.075
	(0.192)
Grade 2 school means literacy	0.249
	(0.288)
Grade 3 literacy	-0.003
	(0.371)

Note: Standard errors in parentheses. Statistical significance levels (10%, 5%, 1%) based on naïve p-values represented with */**/***.

5. Conclusion

We examined the impact of the grant and school-based management components of the Nigerian Partnership of Education Project (NIPEP) on primary school's infrastructure, educational attainment and learning achievement in Sokoto state, Nigeria. NIPEP was a USD

100 million project funded by the World Bank and Global Partnership for Education from 2015 to 2019 in 5 states in Northern Nigeria with the goal of improving access to quality primary school education with a special focus on girls. The grant disbursal to primary school component alone cost approximately USD 21.1 million.

Other school-based management interventions in combination with grant disbursals have had a mixed success at improving educational attainment and learning outcomes (Banerjee et al., 2010; Glewwe & Maïga, 2011; Blimpo, Evans & Lahire, 2015; Lassibille et al., 2010; Pradhan & De Ree, 2014; Santibañez, Abreau-Lastra & O'Donoghue, 2014). Further, few studies evaluate large-scale interventions like ours: Garcia-Moreno, Gertler and Patrinos (2019) and Santibanez, Abreau-Lastra and O-Donoghue (2014) evaluate large-scale school-based management interventions plus grant disbursals in Mexico, while the remaining studies found evaluated small pilot studies or their own experiment. Here, we present novel insights from a large-scale intervention in a lower-middle income and sub-Saharan African context, Nigeria, that proved entirely ineffective at delivering impact. One year after implementation, the intervention appears to have had no impact on schools' infrastructure, educational attainment or learning outcome measures.

There are several possible explanations for this lack of impact. First, the implementation of the intervention may have been faulty from the responsible state's education authorities, which is corroborated by the low level of reporting an intervention by respondents at treatment schools. Second, schools may have spent the grant on school-unrelated matters while reporting untrue uses of the grant at the endline survey. Anecdotal evidence from a subsample of treatment schools shows that no improvements could be detected in the domains that respondents claimed to have invested the grant in. Third, the grant amount of 250,000 Nigerian Naira (approx. PPP-adjusted int-$ 2,272) may have been insufficient to alleviate the infrastructural and working material deficits of the primary schools in the sample. Fourth, the binding constraint in educational attainment and learning achievement could be teacher absenteeism, rather than a lack of money. Upon arrival for the endline survey, there were no teachers present at 45% of sample schools and no learning taking place at 74%. Fifth, the local capacity of SBMC members may have been limiting the committees' ability to transform the training's lessons and grant into measurable school improvements. Our proxy measure of SBMC capacity, members' mean literacy level, proved quite low (44% of interviewed SBMC members at endline reported being able to read a letter) but was also uncorrelated with the success of the intervention at treatment schools.

Despite its poor record at generating measurable, meaningful and sustainable improvements in education outcomes, school-based management interventions in combination with grant disbursals to schools have recently gained popularity with funders and policy-makers. Our evidence underlines the importance of further research needed to understand the determinants of SBM interventions' success. For example, identifying the complementary input factors, such as sufficient ministerial and SBMC capacity or satisfactory teacher attendance and motivation, and context-specific characteristics that make SBM and grant disbursal interventions with positive impact possible is important to understand for researchers as well as policy-makers.

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Appendix

Table A1
ANCOVA and Differences-in-Differences estimates for outcomes of interest

	ANCOVA (1)	Diff-in-Diff (2)
Infrastructure and equipment		
Quality of the learning environment[1]	0.047	0.063
	(0.028)	(0.041)
Sanitation[1]	0.084*	0.070*
	(0.048)	(0.048)
School facilities[1]	-0.006	0.001
	(0.030)	(0.048)
Any toilets	0.232***/‡	0.275**
	(0.073)	(0.111)
Pupil enrollment		
Total enrollment	23.96	6.93
	(29.87)	(61.0)
Female enrollment rate	-0.012	0.016
	(0.044)	(0.723)
Pupil attendance		
Any students present	-0.048	0.051
	(0.084)	(0.106)
Teacher attendance		
Any teacher present	-0.048	0.109
	(0.091)	(0.123)
Any learning taking place	0.020	0.176
	(0.081)	(0.110)
Normalized learning achievement scores[2]		
Grade 2 numeracy	-0.043	
	(0.094)	
Grade 2 school means numeracy		0.015
		(0.141)
Grade 3 numeracy	-0.028	
	(0.111)	
Grade 3 school means numeracy		0.098
		(0.157)
Grade 2 school means literacy	-0.016	-0.015
	(0.075)	(0.146)
Grade 3 literacy	-0.041	
	(0.064)	
Grade 3 school means literacy		-0.021
		(0.159)

[1]These variables are indices ranging from 0 to 1 with 0 representing a poor and 1 a good outcome.
[2]We were unable to match students from baseline to endline, so in column (1) the baseline control variables for the learning achievement scores are baseline school-level averages and in column (2) Differences-in-Differences were estimated using school level averages at both base- and endline.
Note: Standard errors in parentheses. Statistical significance levels (10%, 5%, 1%) based on naïve p-values represented with */**/*** and based on Benjamini-Hochberg adjusted p-values corresponding to a 5% significance level represented with ‡.

Table A2
Baseline balance of normal and high grant treatment groups

	Normal		High		Difference in means	p-value of difference in means
	N (1)	Mean (2)	N (3)	Mean (4)	(N-H) (5)	(6)
Summary indices						
Quality of the learning environment[1]	31	0.338	33	0.309	0.030	0.43
Sanitation[1]	31	0.151	33	0.222	-0.072	0.29
School facilities[1]	31	0.144	33	0.154	-0.010	0.84
Any toilets	31	0.161	33	0.152	0.010	0.92
Pupil enrollment						
Total enrollment	31	177	33	248	71	0.34
Female enrollment rate	25	0.392	29	0.390	0.002	0.95
Pupil attendance						
No. students observed in Grade 2 / 3	31	50	33	51	-0.126	0.99
Pupil absence rate according to registry	25	0.528	29	0.295	0.233	0.02**
Teacher attendance						
Number of teachers present	31	0.129	33	0.970	-0.841	0.02**
Any learning taking place	31	0.097	33	0.212	-0.115	0.21
Learning achievement scores (out of 20)						
Grade 2 math	829	6.09	872	6.22	-0.126	0.48
Grade 3 math	541	8.56	622	8.97	-0.411	0.13
Grade 2 literacy	829	1.14	872	1.85	-0.711	0.00***/‡
Grade 2 school means literacy	30	1.11	32	1.78	-0.666	0.14
Grade 3 literacy	541	3.22	622	4.11	-0.891	0.00***/‡
Grade 3 school means literacy	31	2.97	32	3.58	-0.608	0.33

[1]These variables are indices ranging from 0 to 1 with 0 representing a poor and 1 a good outcome.
Statistical significance levels (10%, 5%, 1%) based on naïve p-values represented with */**/*** and based on Benjamini-Hochberg adjusted p-values corresponding to a 5% significance level represented with ‡.

Table A3

ITT estimates of normal and high grant treatment groups

	Normal SIG (1)	High SIG (2)	p-value of F-Test (1) = (2) (3)
Summary indices			
Quality of the learning environment	0.033	0.038	0.90
	(0.039)	(0.038)	
Sanitation	0.038	0.099	0.37
	(0.060)	(0.059)	
School facilities	-0.020	0.002	0.66
	(0.043)	(0.042)	
Any toilets	0.048	0.227**	0.13
	(0.101)	(0.100)	
Pupil enrolment			
Total enrolment	12.82	89.87*	0.19
	(51.06)	(50.52)	
Female enrolment rate	0.041	-0.019	0.29
	(0.051)	(0.049)	
Pupil attendance			
Any students present	0.097	-0.210**	0.009***
	(0.101)	(0.100)	
Teacher attendance			
Number of teachers present	-0.4	0.429	0.97
	(0.736)	(0.828)	
Any learning taking place	0.065	-0.039	0.36
	(0.098)	(0.097)	
Normalized learning achievement scores			
Grade 2 numeracy	-0.192*	0.042	0.09*
	(0.116)	(0.123)	
Grade 3 numeracy	-0.082	0.020	0.52
	(0.144)	(0.154)	
Grade 2 school means literacy	-0.094	-0.052	0.78
	(0.135)	(0.127)	
Grade 3 school means literacy	-0.118	-0.178	0.72
	(0.144)	(0.144)	

Note: Initially, balance in outcomes between normal and high treatment schools was tested and confirmed (see Table A2).
Standard errors in parentheses. Statistical significance levels (10%, 5%, 1%) based on naïve p-values represented with */**/*** and based on Benjamini-Hochberg adjusted p-values corresponding to a 5% significance level represented with ‡.