School Grants and Education Quality: Experimental Evidence from Senegal

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The effect of increasing school resources on educational outcomes is a central issue in the debate on improving school quality. In this paper we use a randomized experiment to analyse the impact of a school grants programme in Senegal, which allowed schools to apply for funding for pedagogic improvements of their choice. We find positive effects on test scores at lower grades that persist for at least two years. These effects are concentrated among schools that focused funds on human resources improvements rather than school materials, reinforcing that teachers and principals are a central determinant of school quality.

INTRODUCTION

In the last 50 years, primary school enrolment has increased dramatically in the developing world. Even in the poorest areas of Sub-Saharan Africa, gross enrolment rates in primary school are approaching 80% (e.g. Glewwe and Kremer 2006). There is, however, widespread evidence that the quality of education in developing countries remains very low. As a result, increases in school enrolment may not translate into corresponding increases in productivity and wellbeing. This is consistent with recent evidence suggesting that education quality, not quantity, matters most for growth (e.g. Hanushek and Woessmann 2011; Glewwe et al. 2013).

We address the following question: is it possible to improve the quality of under-resourced schools by providing them with cash transfers? The appeal of this idea lies in its simplicity. The assumption behind it is that local decision-makers, such as principals and community leaders, are likely to have a deeper understanding of the needs of their schools than have central education authorities, and are therefore in the best position to put these resources to their most efficient use.

We study a primary school grants programme in Senegal from 2009 to 2011, which was developed to decentralize at least a small part of the country’s education budget. Through this programme, every primary school in Senegal could apply for funds for a specific school project that seeks to improve the quality of learning and teaching, with the best proposals being selected through a competitive process. The maximum amount that a school could receive for a project amounted to US$3190, which corresponded to 7% of the total annual school budget of a typical school (inclusive of teacher salaries). Of the almost 8000 primary schools in Senegal, around 10% submitted proposals for a grant, and 633 schools (90% of those that applied) were selected to receive funding. This approach to school funding is interesting because it places the responsibility on the school to identify its educational needs, and disburses funding based on this. It is also of general interest because—if it is successful—such a model can be replicated broadly in developing countries.

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To evaluate the effects of school grants, we randomly allocated schools whose proposals were selected to three funding cohorts over the course of two years. This randomization is critical: it allows us to causally estimate differences in school outcomes based on when schools received grants. Over the course of these two years, we administered language and mathematics examinations to students in grades 2 and 4, and collected survey information from parents, teachers and school principals. We find large and statistically significant effects on test scores one year after the start of the intervention for children who benefited from school grants when they were in second grade. These effects are larger for girls and in schools in the south of the country, where projects tended to focus on training human resources (teaching and management), compared to the north, where priority was placed on the acquisition of school materials (e.g. textbooks). We do not observe similar programme impacts for children in fourth grade. The point estimates are very similar in the second follow-up for the same children, pointing to persistent effects. In addition, we show that our main conclusions survive multiple-hypothesis testing (using a step-down procedure proposed by Romano and Wolf 2005). We also conduct back-of-an-envelope calculations to assess the cost-effectiveness of these grants, given our results, and find that this particular grants programme was more cost-effective than some smaller grants programmes that typically find no evidence on student performance, but that grants are much more costly than other types of interventions that have been shown to improve student outcomes in developing countries.

This paper makes several contributions. First, it adds to a growing literature that uses randomized experiments to understand how to improve the quality of education in developing countries. The evidence on the effect of school resources on primary school student achievement is, at best, mixed (see Glewwe et al. (2013) and Murnane and Ganimian (2016) for recent reviews, and Evans and Popova (2016) for a review of reviews). While much of the literature studies interventions that uniformly target specific resources, such as textbooks and flip charts (Glewwe et al. 2004, 2009) or computer-aided instruction (Banerjee et al. 2007), this paper studies an educational intervention that, by administering grants as opposed to targeted resources, allows schools to choose which resources to devote funding to. In addition, it demonstrates the use of delayed disbursement of funds as a method that allows for randomized evaluation while still (eventually) allocating funds equally. This type of evaluation still exploits randomization, but may be a more palatable avenue of ‘evidence-based decision-making’ for politicians and policymakers.

Second, and relatedly, this paper contributes to a smaller literature on decentralization in schooling decisions (for a review, see Galiani and Perez-Truglia (2013)), and school-based management (e.g. Bruns et al. (2011)). If local decision-makers can target resources better than a central authority, then school grants (and other ways of decentralizing funding) could help to boost the effect of school resources by targeting funds toward efficient uses of resources, particularly in settings with strong societal and economic institutions (Hanushek et al. (2013)). On the other hand, decentralization could lead to negative distributional consequences (Galiani et al. (2008); Blimpo et al. (2014)), or worse, local capture by other entities (Reinikka and Svensson (2004)).

School grant programmes in particular exist in several developing countries, and there is an important debate about their potential effectiveness (e.g. Bruns et al. (2011)). Nevertheless, there are very few rigorous evaluations of these programmes. Das et al. (2013) use an experimental design to study grants in India and Zambia, and find that while unanticipated grants are effective in increasing student performance, anticipated grants are not, because they are fully offset by a decline in parental investments in their child’s education. Blimpo et al. (2015) study an intervention that pairs school grants with

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training of community members and school staff for better management and use of these grants. They find some effects of the combined grants and training treatment on teacher and student absenteeism but not on test scores, and no effect of the cash-grant-only intervention. Closely related to this paper, Beasley and Huillery (2017) experimentally study a programme in Niger that gave grants to school committees with the aim of increasing parent involvement in primary education, and find that the grants increased parental involvement, but had no impact on student learning objectives and even a negative impact on teacher absenteeism. This grant programme, however, differed from the grants in our study in three important ways: first, it was not tied to pedagogical objectives, but instead was often used for infrastructure, festivals, and even agricultural production. Second, schools did not have to apply for the Niger grant; instead, it simply arrived, with one month’s notice. Finally, the size of the grant was much smaller (around US$200 per school as opposed to over US$3000 in our study).

None of these papers analyses a programme such as the one studied here, where schools receive funding if their grant application for a specific project is judged to be high quality. While most other grant programmes restrict somewhat the use of grant funding, they are not usually tied to a successful grant application of this type. Furthermore, both in Das et al. (2013) and in Blimpo et al. (2015), the grant amounts are between $500 and $700 per school, which is much lower than the amount provided by the school grants programme in Senegal (and even lower for Beasley and Huillery (2017)). Therefore the approach followed in Senegal is quite different than that followed in several other countries, by restricting grant attribution only to those schools that present a valuable and viable plan for using grant funds, and by providing very sizeable grants.

The paper proceeds as follows. In Section I we describe the school grants programme in Senegal and the randomized experiment design. In Section II we describe our data, and Section III describes our empirical approach. In Section IV we present our main results and examine potential mediating factors through which the impact of the programme may have operated. Section V concludes.

I. DESCRIPTION OF THE PROGRAMME AND EVALUATION

Primary schooling in Senegal consists of six years of education and is funded through a mix of government, foreign aid and household resources.¹ Gross enrolment rates in primary schools increased dramatically over the ten years prior to our study, from 67% in 2000 to 92% in 2009. Despite this large increase in enrolment, in 2009 only 60% of students completed primary school. In an effort to increase the quality of primary education, Senegal’s Ministry of Education initiated this school grants programme.

School grants in Senegal

Senegal uses (and used well before this experimental study) school grants as a tool to fund improvements in education quality, based on the premise that school-level actors are in the best position to identify a school’s unique deficiencies and the most workable solutions to address them. Instead of providing general funding for all schools, funds were targeted towards problems identified by the school as major obstacles to quality, and identified by a government evaluation committee as eligible for funding based on district-level and system-wide priorities. The idea behind this local-level process was that decentralized decision-making would allow more efficient and effective use of funds.
Generally, the programme worked as follows. The Ministry of Education issued a call for proposals, based on the available grant funding, priority areas and eligible activities (and sometimes eligible regions). Schools that decided to apply for funding completed a grant proposal for a school project addressing a particular issue faced by the school that fit with the priorities of the call. Another important component of the programme was its role in promoting strong community participation in schools. As a result, grant proposals were prepared by a committee of parents, teachers and local officials. We next describe the particular process in 2009 through which grants were sought, approved and allocated.

Call for proposals, grant selection, and randomization design

In 2009, the Ministry of Education in Senegal released a call for proposals for school grants for all primary schools, directly informing education authorities of each district of deadlines and the emphasis of the grants. At that point, the government also sought technical and financial support from the World Bank to rigorously evaluate the programme. Because school grants had existed in previous years, many schools were likely aware of the call, but to better ensure that all schools had the same level of information about the programme, we sent field officers to each regional authority to ensure that they received the call. The emphasis of this particular call was on pedagogic issues, in contrast to previous calls that emphasized strengthening the physical environment. The grants totaled 1,500,000 CFA francs per school (approximately US $3190), which represented a roughly 7% increase in expenditures per student in a typical school, inclusive of teacher salaries, which comprise over 90% of the budget (based on collected self-reports from principals and teachers in our sample).

As a result of this call for proposals, 712 schools submitted a proposal, usually around 20 pages long. The winning proposals were selected over the course of a seven-day workshop by a twelve member committee, consisting of nine members from the Ministry of Education, two members from the national statistical agency, and one World Bank consultant. During this workshop, the committee judged projects based on the extent to which their objectives were to: (a) raise French, mathematics and science achievement, (b) reduce grade repetition and dropout, and (c) increase effective hours of learning; the committee adopted a project analysis grid based on these objectives. The committee used this rubric to judge the proposals, and discarded low-quality and ineligible proposals for projects that were inconsistent with the outlined goals, such as construction of offices, health huts, shops or gardening areas. The remaining proposals were then grouped into two categories. The first consisted of very good proposals that were eligible for financing. The second consisted of strong proposals with potential, but which needed revision. These were sent back to schools with comments from the evaluation committee, then re-submitted. Figure 1 provides a graphical representation of this process.

This process resulted in the selection of 633 projects to fund (an acceptance rate of 89%). Of those, 65 (almost 10%) were sent back for minor revisions, and all 65 of them were eventually accepted. Table 1 shows the characteristics of these projects, as reported in the grant applications (left-hand panel) and from a sample of projects surveyed in the second follow-up (right-hand panel). Input into the applications was provided by school principals and teachers in virtually all applications, as well as management boards and parents associations in most applications, and students in around 60% of applications. The objective of projects was mainly to improve student outcomes and teacher

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effectiveness, and the majority of projects revolved around improving courses (through, for example, teacher training and course development) and increasing resources (e.g. purchasing textbooks).

To evaluate the effectiveness of the school grants programme, these 633 projects were randomly allocated to three funding cohorts. A third of the schools (211) were selected randomly as the first cohort, to receive funding at the end of the school year in June 2009. This funding could be executed only at the beginning of the following school year (October/November). Of the remaining schools, 211 were to receive funding in June 2010, and another 211 were to receive funding in June 2011. In practice, the disbursement of the second round of grants did not occur until the first trimester of 2011. The locations of the schools that received grants are shown in Figure 1 of the Online Appendix.

This randomization process results in comparable treatment and control schools: schools in the first cohort received school grants during the 2009–10 school year, while schools in the second and third cohorts did not and therefore can be used as a comparison group for the schools in the first cohort. Analogously, since the third cohort did not receive funds until the 2011–12 school year, we can compare the first cohort (treatment) and third cohort (control) to examine effects of the grant two years out (see Figure 2). Unfortunately, we do not have any evidence about when the second and third cohorts were notified that they had won a grant, nor when they were told they would receive the funds. To the extent that these later cohorts changed their behaviour in anticipation of future funding, the results of this experiment may constitute a lower bound of the true effect of school grant resources.

The randomization among eligible schools is critical for our study: it ensures that the three successive cohorts are statistically comparable, which in turn ensures unbiased estimates of the effect of the programme. In other words, our analysis does not include
any ineligible schools. The control group consists of only schools that were judged as eligible but were not selected to receive funding by the randomization process until a later date.

Which schools apply for a grant?

Before moving on to the analysis of the experiment, we first investigate the selection margin of schools applying for a grant. Since only 10% of schools applied,
understanding the characteristics of schools that correlate with a grant application will speak to the external validity of the experimental results.

To do this, we use data from the 2008 census of primary schools (EMIS), which contains information on demographic and infrastructure characteristics. We match the names of the schools that applied for a grant in 2009 to the names in EMIS, and compare the means of various characteristics of schools that applied and did not apply for a school grant. We were able to match all but 24 schools, and the results are reported in Table 2. Schools that applied for a grant were, on average, more likely to be public schools and located in urban areas. They were also more likely to have more infrastructure features, such as toilets, electricity and running water, a cafeteria, a library and computers, and sports fields and playgrounds. Interestingly, schools that applied for a grant were also much more likely (by 25 percentage points, or 59% off a base of 43% among non-applicant schools) to have a management board.

These differences between schools that applied and did not apply for a school grant suggest a degree of self-selection into the grant programme: schools that applied appear to have more infrastructure resources (toilets, cafeteria, electricity) as well as more pedagogical resources (library, computers), and appear to be more organized, as suggested by the fact that they are more likely to have a management board. To the extent that these resources correlate with better leadership, engagement and/or student performance, these results suggest that the school grants programme allocates funds to schools that are at the higher end of the distributions of resources, leadership and engagement.

These results also have implications for external validity of our results. Given the design of our experiment, our results speak directly only to the population of schools that applied for a grant. Since these schools appear to be a selected sample of schools in Senegal, it is harder to extrapolate to schools with less resources and less organization.

|                          | Applicants (1) | Non-applicants (2) | Difference (3) |
|--------------------------|----------------|--------------------|----------------|
| Public school            | 0.994          | 0.886              | 0.109*** (0.012)|
| Rural school             | 0.737          | 0.791              | -0.055*** (0.016)|
| School has running water | 0.430          | 0.317              | 0.114*** (0.019)|
| School has toilets       | 0.757          | 0.547              | 0.211*** (0.020)|
| School has sinks         | 0.132          | 0.129              | 0.003*** (0.013)|
| School has cafeteria     | 0.477          | 0.330              | 0.147*** (0.019)|
| School has library       | 0.176          | 0.136              | 0.040*** (0.014)|
| School has computers     | 0.189          | 0.120              | 0.069*** (0.013)|
| School has sports fields | 0.552          | 0.504              | 0.048*** (0.020)|
| School has playgrounds   | 0.936          | 0.878              | 0.058*** (0.013)|
| School has electricity   | 0.406          | 0.280              | 0.125*** (0.018)|
| School has management board | 0.677       | 0.426              | 0.251*** (0.020)|
| Observations             | 688            | 6997               |                |

Notes
Data from the 2008 EMIS census of schools in Senegal. Columns (1) and (2) report means of variables among schools that applied to the grants programme and those that did not apply, respectively. We were unable to match 24 schools that had applied to the grants programme to EMIS. Column (3) reports the differences in means, with standard errors in parentheses.

*, **, *** indicate $p < 0.10, p < 0.05, p < 0.01$, respectively.

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the other hand, while the differences in means are statistically significant, they are not always economically very different: among pedagogical resources, for example, there is only a 4 percentage point difference in whether the school has a library, and a 7 percentage point difference in whether the school has computers. In addition, we stress that this does not invalidate the internal validity of our experiment, and still provides important lessons for the effectiveness of grant programmes for more highly tooled schools.

II. DATA AND BALANCE

To evaluate the effect of the school grants programme, we administered three waves of surveys to students and their families, teachers and principals over the course of two academic years. We first conducted a baseline survey at the start of the 2009–10 academic year (in November), right as the first round of grants was able to be executed. While the funds were disbursed to treatment schools several months earlier, schools were not allowed to execute these funds until the beginning of the school year, at which point the baseline data were collected. Subsequent surveys took place in November 2010 at the beginning of the 2010–11 academic year (first follow-up), and in May 2011 at the end of the 2010–11 academic year (second follow-up).

At baseline, we administered written assessments in mathematics and French to a random sample of six children in each of grades 2 and 4, and an oral reading assessment (similar to Early Grade Reading Assessment) to a random sample of three of those six children in grades 2 and 4. In addition, we randomly selected two of the three children in each grade who took all three assessments, and conducted a household survey that included demographic and financial information on all household members. Finally, we collected classroom- and school-level information by surveying the school principals and the teachers of the students in our sample.

In the first follow-up, we re-surveyed and re-tested the same children and their households, teachers and principals. For most of these students, this corresponded to the start of grades 3 and 5, but for students who repeated their grade, it corresponded to grades 2 and 4 (and in the rest of the paper, we use the shorthands of grades 3 and 5 to refer to all of these students). Importantly, we administered the same tests to students across all waves so that we could measure academic progress. In the second follow-up, we re-surveyed and re-tested the same children who were tested at baseline and first follow-up, but did not collect general school and classroom information. In addition, we administered to children the Peabody Picture Vocabulary Test (PPVT), which is a widely used measure of cognitive outcomes for children that assesses a child’s receptive vocabulary (i.e. listening comprehension) by asking the child to point out pictures that best represent words spoken to them.

Of the 633 schools that won a grant, we sampled 525. We were able to contact 478 schools at baseline, among which 447 were successfully surveyed. At first follow-up we contacted 528 schools (which was more schools than we originally sampled because the enumerators accidentally went to an extra treatment and two extra control schools that we had not originally planned on sampling), among which 517 were successfully surveyed. Finally, at second follow-up we dropped cohort 2 schools due to budgetary constraints and contacted 340 schools from cohorts 1 and 3, among which 325 were successfully surveyed. See Table 1 in the Online Appendix for the corresponding number of student-level observations and attrition rates.

Table 3 shows descriptive statistics and balance between treatment and control schools, by grade. The ‘Control’ columns show means and standard deviations of
baseline characteristics in control schools, and the ‘Treat–Control’ columns report the differences in characteristics between treatment and control at baseline and their standard errors. Panel A reports test scores and shows that the resulting mean scores for the French, mathematics and oral tests (calculated as the proportion of correct responses on the exam) are around 20–40%. The same tests were administered at first follow-up, so these scores allowed room for noticeable improvement. The fourth row corresponds to an index of the three tests (which is the first principal component of these three tests, standardized to have unit variance across the grade).  

Panel B of Table 3 reports household characteristics of the students. On average, these students live less than a kilometre from the school and miss one day of school per month. Their households spend a fair amount of their income on education expenses as compared to food consumption, and around half of parents report involvement in school activities. Only 12% of the household interviews in the control were conducted in French, while almost all classroom instruction was conducted in French. Panel C reports school characteristics. The average school in our sample is not small: a school in the control group has a little fewer than 350 students and 10 teachers, 41% of whom hold a baccalaureate degree and almost half of whom participated in training in the five years preceding the intervention. The schools are varied in their resources: 57% of control schools have electricity, and 21% have a library. Three-quarters of principals have a baccalaureate degree.

Treatment and control schools are very well balanced, as shown in the ‘Treat–Control’ columns of Table 3. All but two differences (parental involvement in school and the percentage of teachers who report receiving training in the past five years, both for second grade) are insignificant at the 5% level, and the test of joint significance that all 42 differences equal zero cannot be rejected for either grade 2 or grade 4. This is the case despite the baseline survey taking place five months after the disbursement of funds. The execution of funds was not supposed to take place until the time of the baseline survey, but if grants were executed earlier than intended, then we may detect actual treatment effects in our balance test, so it is reassuring that there are very few differences. The exceptions are parental involvement and the percentage of teachers who report recent training, which may be leading indicators of treatment effects since they can change before the school year begins, so it is possible that funds were used earlier than intended. However, even if this were the case, as long as characteristics were balanced at randomization, the experimental effects should not be biased; the only change to the interpretation of our results would be in the persistence of our results (i.e. how long effects last).

In the Online Appendix, we explore various threats to balance and other concerns about our sample. To address the concern that some schools were missing at baseline (and hence not included in Table 3), Table 2 in the Online Appendix presents an analogous balance table at first follow-up for characteristics that are relatively constant over time. These characteristics are balanced across treatment and control schools. To address attrition concerns, Table 3 in the Online Appendix reports the balance table for students who did not leave the sample between baseline and first follow-up or second follow-up, respectively. Again, the sample is well-balanced.

III. EMPIRICAL APPROACH AND INFERENCE

We use a regression approach to estimate the impacts of the programme. Specifically, the impacts are the estimated $\beta_i$ coefficients from the regression

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### Table 3
**Baseline Descriptive Statistics and Balance, by Grade**

| Panel | Test scores | Household characteristics | School and teacher characteristics |
|-------|-------------|----------------------------|-----------------------------------|
|       | Grade 2     | Grade 4                    |                                   |
|       | Control     | Treat–Control              | Control                           |
|       |             |                            | Treat–Control                     |
| Panel A: Test scores | | | |
| Percent correct: French | 0.42 (0.22) | -0.01 (0.02) | 0.39 (0.17) | 0.00 (0.01) |
| Percent correct: Mathematics | 0.37 (0.23) | -0.00 (0.02) | 0.33 (0.19) | -0.01 (0.02) |
| Percent correct: Oral | 0.22 (0.17) | 0.01 (0.02) | 0.55 (0.24) | -0.01 (0.02) |
| Index score (standardized) | 0.00 (0.98) | -0.00 (0.09) | 0.02 (0.98) | -0.05 (0.09) |
| Panel B: Household characteristics | | | |
| Days of school missed last week | 0.17 (0.86) | 0.07 (0.07) | 0.16 (0.75) | -0.07* (0.04) |
| Student works after school | 0.01 (0.10) | 0.01 (0.01) | 0.02 (0.14) | -0.01* (0.01) |
| Household size | 9.26 (4.06) | 0.00 (0.32) | 9.07 (4.05) | 0.14 (0.33) |
| Number of children in household | 5.25 (2.61) | -0.03 (0.20) | 5.14 (2.77) | 0.21 (0.22) |
| Head has some education | 0.60 (0.49) | -0.02 (0.04) | 0.56 (0.50) | -0.06 (0.04) |
| Percent of adult females with some education | 0.37 (0.41) | -0.03 (0.03) | 0.31 (0.39) | -0.01 (0.03) |
| Distance to school (km) | 0.71 (0.91) | -0.07 (0.06) | 0.76 (2.56) | -0.03 (0.12) |
| Parent involved in school | 0.38 (0.49) | 0.09** (0.04) | 0.45 (0.50) | -0.07* (0.04) |
| Expenditure on household food (000s CFA) | 21.83 (15.45) | 1.26 (1.17) | 22.11 (15.50) | 0.51 (1.20) |
| Expenditure on uniform (000s CFA) | 2.43 (1.19) | 0.15 (0.36) | 2.28 (1.13) | 0.06 (0.35) |
| Expenditure on tuition (000s CFA) | 1.10 (1.18) | -0.01 (0.09) | 1.03 (1.01) | 0.04 (0.09) |
| Expenditure on supplies (000s CFA) | 3.85 (5.64) | -0.38 (0.29) | 4.34 (4.16) | -0.38 (0.27) |
| Student has tutor | 0.15 (0.36) | -0.01 (0.03) | 0.14 (0.35) | -0.01 (0.03) |
| Home has electricity | 0.47 (0.50) | 0.03 (0.04) | 0.45 (0.50) | 0.02 (0.04) |
| Home has modern toilet | 0.54 (0.50) | -0.01 (0.04) | 0.50 (0.50) | 0.01 (0.04) |
| Wealth index | -0.01 (1.00) | 0.04 (0.08) | -0.01 (1.03) | 0.01 (0.09) |
| Land owned (hectares) | 2.37 (3.47) | 0.43 (0.46) | 2.89 (9.11) | -0.50 (0.43) |
| Age of youngest infrastructure | 6.46 (11.01) | 0.26 (1.18) | 6.51 (11.05) | 0.24 (1.19) |

Panel C: School and teacher characteristics

| Age of youngest infrastructure | 6.46 (11.01) | 0.26 (1.18) | 6.51 (11.05) | 0.24 (1.19) |
|                  | Grade 2                  |                     | Grade 4                  |                     |
|------------------|--------------------------|---------------------|--------------------------|---------------------|
|                  | Control                  | Treat–Control       | Control                  | Treat–Control       |
| Distance to nearest city (km) | 18.38 (25.01)            | -0.07 (2.18)        | 18.03 (24.56)            | 0.21 (2.20)         |
| Locality population (00,000s) | 1.38 (4.40)              | 0.04 (0.46)         | 1.41 (4.43)              | 0.03 (0.45)         |
| Locality has health centre | 0.71 (0.45)              | 0.03 (0.04)         | 0.71 (0.45)              | 0.03 (0.04)         |
| School located in south | 0.18 (0.39)              | -0.01 (0.04)        | 0.19 (0.39)              | -0.01 (0.04)        |
| School has electricity | 0.57 (0.50)              | 0.01 (0.05)         | 0.57 (0.50)              | 0.01 (0.05)         |
| Number of teachers | 9.68 (4.97)               | 0.44 (0.51)         | 9.74 (4.93)              | 0.57 (0.52)         |
| Number of pupils   | 341.11 (252.39)           | 28.47 (25.60)       | 343.65 (253.37)          | 35.57 (26.05)       |
| School has library | 0.21 (0.40)               | 0.08* (0.04)        | 0.21 (0.41)              | 0.08* (0.04)        |
| Number of books in library | 109.39 (442.71)         | 30.35 (39.18)       | 109.66 (443.70)          | 35.10 (39.64)       |
| Number of computers | 1.28 (4.39)              | -0.01 (0.40)        | 1.30 (4.39)              | 0.01 (0.40)         |
| Number of textbooks in classroom | 59.90 (45.18)           | 3.17 (4.58)         | 66.43 (51.96)            | 5.68 (5.40)         |
| Times per day asking for silence | 12.80 (14.47)           | 0.84 (1.36)         | 9.62 (11.94)             | 0.73 (1.15)         |
| Times per day punishing a student | 2.34 (6.34)             | 0.18 (0.62)         | 2.22 (3.26)              | 0.36 (0.54)         |
| Percent teachers female | 0.32 (0.24)              | 0.01 (0.02)         | 0.32 (0.23)              | 0.01 (0.02)         |
| Average teacher age | 33.12 (4.24)              | -0.13 (0.39)        | 33.26 (4.23)             | -0.10 (0.39)        |
| Percent of teachers with baccalaureate | 0.41 (0.23)             | -0.02 (0.02)        | 0.41 (0.22)              | -0.02 (0.02)        |
| Average teacher experience | 6.56 (3.69)              | 0.08 (0.35)         | 6.61 (3.69)              | 0.13 (0.35)         |
| Percent teachers with training in past 5 years | 0.47 (0.50)             | 0.10** (0.05)       | 0.47 (0.50)              | 0.01 (0.05)         |
| Percent of principals with baccalaureate | 0.74 (0.44)             | -0.05 (0.04)        | 0.74 (0.44)              | -0.06 (0.04)        |
| Wald test of joint equality: $\chi^2$ (p-value) | 46.4 (0.26)              |                     | 32.7 (0.82)              |                     |

**Notes**

‘Control’ columns report means and standard deviations of baseline characteristics in control schools for grades 2 and 4, respectively. ‘Treat–Control’ columns report differences in characteristics between treatment and control schools at baseline and their standard errors, clustered by school. Sample size grade 3/5: panel A $N = 2722/2744$ (oral $N = 1388/1362$); panel B $N = 931/918$; panel C $N = 2971/2924$.

*, **, *** indicate $p < 0.10$, $p < 0.05$, $p < 0.01$, respectively.
\[ Y_{ist}^k = z_i^k + \beta_i^k G_s + X_{ist}^k + \epsilon_{ist}^k, \]

where \( Y_{ist}^k \) is the proportion of correct answers in test \( k \), for student \( i \) in school \( s \) at follow-up \( t \) (1 or 2), \( G_s \) is a treatment indicator, \( X_{ist}^k \) are conditioning variables measured at baseline, and \( \epsilon_{ist}^k \) is an error term. Conditioning variables include household size, number of children, whether the head has any education, distance to school, a wealth index, the interview language, and the baseline scores of all tests. Since household interviews were conducted for only a random subsample of students, two-thirds of our sample have missing household characteristics (at random). In order to still include these observations, we assign zeros to conditioning variables if they are missing, and include dummies for observations with missing conditioning variables.

We report standard errors, clustered at the school level, and note significance at the 1%, 5% and 10% levels of standard single-hypothesis tests. In addition, since we are testing multiple hypotheses at once, we compute levels of significance for each coefficient in our main analysis using the step-down approach of Romano and Wolf (2005). In this way, we control for the family-wise error rate (FWE). The FWE is defined as the probability of incorrectly identifying at least one coefficient as significant, which becomes more likely as the number of hypothesis tests increases. The Romano–Wolf approach improves on more conservative classical methods such as the Bonferroni correction by applying a ‘step-down’ algorithm that accounts for the dependence structure of individual tests and thus improves power. Our approach is to control for an FWE of 5% and 10%, and mark each coefficient that is significant at each of these rates. However, testing too many hypotheses at once may reduce power to detect anything significant. We thus test multiple hypotheses in related groups (in our case, by the three main tests within a row) rather than for all effects reported in the paper, and also conduct these tests only for the main results on student outcomes.

**IV. RESULTS**

**Overall treatment effects**

We begin by showing the overall effect of the programme for students in grades 3 and 5 at first and second follow-up (who were in grades 2 and 4 at baseline). As explained above, at first follow-up we have measurements of student performance in written tests in French and mathematics, as well as an oral test that covers sound, letter and word recognition, and reading comprehension, but (for cost reasons) was administered to only a third of the students who took written tests. For each of these three tests we compute the proportion of correct answers given by each student. In addition, we use the first principal component as a summary index of these three tests, which is standardized to have mean zero and standard deviation one (within grade, across the entire sample). For the second follow-up, we also have scores from the PPVT, which is standardized to have mean zero and standard deviation one.

The results are in Table 4. Panel A reports results from the first follow-up, which was administered at the start of grades 3 and 5, respectively, which was about a year after the disbursement of the project funds, while Panel B reports results from the second follow-up at the end of grades 3 and 5 for the same children. Columns (1)–(3) report effects on French, mathematics and oral test scores, and column (4) provides a summary measure by reporting the first principal component of these three tests. Column (5) reports PPVT scores, which were obtained only in the second follow-up.
In the pooled sample, the index shows an improvement equal to 0.08 of a standard deviation in the first follow-up, which is almost significant at the 5% level (p-value 0.052). This improvement is maintained and increases to 0.09 of a standard deviation in the second follow-up, which has a p-value of 0.072. Thus overall the programme improved outcomes in the schools. Turning to the individual tests, we find that all test scores improved in the first follow-up by similar amounts, and their p-values, adjusted for multiple-hypothesis testing, are less than 0.10. In the second follow-up the improvement

### Table 4

**Programme Impacts on Test Scores for Grades 3 and 5**

|                | French | Mathematics | Oral | Index | PPVT |
|----------------|--------|-------------|------|-------|------|
| **Panel A: Beginning of grade (first follow-up)** |        |             |      |       |      |
| Overall        | 0.021**† | 0.019**† | 0.019† | 0.080† |      |
|                | (0.010) | (0.010)    | (0.010) | (0.044) |      |
| Observations   | 5368    | 5361        | 2732  | 2679  |      |
| Control mean (SD) | 0.51 (0.23) | 0.49 (0.23) | 0.50 (0.27) |      |      |
| Grade 3        | 0.029**† | 0.027**† | 0.029† | 0.126**† |      |
|                | (0.014) | (0.012)    | (0.014) | (0.060) |      |
| Observations   | 2720    | 2718        | 1385  | 1350  |      |
| Control mean (SD) | 0.53 (0.25) | 0.54 (0.24) | 0.35 (0.22) |      |      |
| Grade 5        | 0.011   | 0.010       | 0.008 | 0.027 |      |
|                | (0.011) | (0.012)    | (0.013) | (0.053) |      |
| Observations   | 2648    | 2643        | 1347  | 1329  |      |
| Control mean (SD) | 0.48 (0.20) | 0.44 (0.20) | 0.64 (0.24) |      |      |
| **Panel B: End of grade (second follow-up)** |        |             |      |       |      |
| Overall        | 0.020*  | 0.005       | 0.026**† | 0.094**† | 0.057 |
|                | (0.012) | (0.011)    | (0.012) | (0.054) | (0.082) |
| Observations   | 3338    | 3327        | 1686  | 1620  | 1122 |
| Control mean (SD) | 0.63 (0.22) | 0.62 (0.22) | 0.58 (0.26) |      |      |
| Grade 3        | 0.035**† | 0.017       | 0.039**† | 0.160**† | 0.153 |
|                | (0.016) | (0.015)    | (0.018) | (0.077) | (0.096) |
| Observations   | 1732    | 1721        | 853   | 826   | 566 |
| Control mean (SD) | 0.66 (0.23) | 0.68 (0.23) | 0.45 (0.23) |      |      |
| Grade 5        | 0.003   | −0.008      | 0.007 | 0.013 | −0.060 |
|                | (0.012) | (0.013)    | (0.014) | (0.061) | (0.097) |
| Observations   | 1606    | 1606        | 833   | 794   | 556 |
| Control mean (SD) | 0.59 (0.20) | 0.57 (0.20) | 0.72 (0.21) |      |      |

**Notes**

Standard errors, clustered by school, are in parentheses. Conditioning variables: grade, gender, household size, number of children, education of head, distance to school, wealth index, interview language, baseline scores, missing dummies. The drop in student observations from the first follow-up to the second follow-up is due to the fact that cohort 2 schools were not surveyed in the second follow-up.

* *, **, *** indicate p < 0.10, p < 0.05, p < 0.01, respectively, corresponding to p-values from the usual single-hypothesis tests.

† indicates significance at the 10% level of Romano and Wolf (2005) p-values from joint tests of French, mathematics and oral (three tests each, by row), or to the index alone.
in mathematics disappears, but the improvements in French and the oral test remain and are both significant at least at the 0.10 level.

The overall improvement in test scores is largely driven by effects in grade 3 but not grade 5. Test scores increased by almost 3 percentage points across all tests in grade 3, which is a large effect in light of the means and standard deviations of test scores (e.g. the programme increased French scores by 0.09 standard deviations). When we look at the index of the three tests, the school grant increased grade 3 school performance by 0.13 of a standard deviation at the first follow-up. The effect on the index survives at 0.15 of a standard deviation up to the end of grade 3, indicating that the programme impacts persisted two years after the grant was disbursed to schools. However, these effects are absent for grade 5: the impacts are numerically close to zero and statistically insignificant by any criterion.9

Overall, it is interesting that a relatively small grant (relative to overall school expenditures) is able to improve children’s learning outcomes to this extent, particularly among third-graders, and that these impacts are sustained. While we cannot say what would have happened under other constraints, it appears that school grants can improve student outcomes when given to schools that are able to formulate viable and worthwhile grant applications for projects targeting pedagogical resources.

Cost-effectiveness

While these grants are small relative to the overall budgets of schools, they are relatively costly in comparison to other interventions that aim to increase school quality. Most studies have found that school resources do not improve student outcomes and are therefore quite cost-ineffective (Beasley and Huillery 2017; Blimpo et al. 2015). In contrast, the programme in Senegal gave schools around $3190 but improved the index of test scores by 0.08 standard deviations after one year for the full sample. In terms of cost-effectiveness, for the 340 students in the average Senegalese primary school, this implies a cost of US$9.40 per student to achieve that effect, or US$11.40 per student per tenth of a standard deviation. It is worth noting, however, that this includes only the size of the grant, and excludes other potential costs of running a grant programme, such as administrative or transaction costs. On the other hand, there are also potential benefits to future generations of students (e.g. from better teacher training or more classroom materials) that we are not accounting for in this simple calculation.

Other interventions have been able to improve student outcomes, at very different levels of cost-effectiveness. Using the Jameel Poverty Action Lab (J-PAL) metric of additional standard deviations in test scores per $100 spent per student, the Senegal grants programme provided an additional 0.87 standard deviations, while building new schools in areas where access to education was low provided an additional 2.13 standard deviations, giving scholarships to students provided a 1.38 standard deviation increase, and implementing computer-assisted learning provided a 1.55 standard deviation increase, while cash transfers to students, additional teachers, and textbooks and flip charts did not lead to any improvement in test scores.10 Thus while the grant programme is at the lower end of cost-effectiveness among interventions that had positive impacts on student test scores, it is not far off, and it may well be a suitable policy lever given a community’s political and social environment.

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Heterogeneous impacts

To better understand who benefited the most from the grants programme and why, we consider baseline characteristics of students and schools that have been shown to have differential impacts in other student achievement settings or that are particularly related to the school grants programme. For student characteristics, we examine heterogeneity by gender, baseline student achievement, and the education of the household head, because other studies have shown that females and higher achievers often benefit the most from school interventions, and because households who value education and/or are better resourced may be better able to take advantage of additional school resources. For school characteristics, we examine heterogeneity by whether the teacher has a higher education degree, whether the school has a library, and how often the students’ teacher asks for silence, because the grant programme was meant for pedagogical resources such as teacher training or materials (e.g. library books), and because schools additionally reported a desire to improve student behavioural problems in their applications. Finally, we examine treatment effects by region, because the southern regions (Ziguinchor and Kolda) are much poorer and had been beset by problems related to rebel activity, and therefore may have had a very different experience with the grants.

Table 5 reports results from regressions similar to those in equation (1) but augmented with an interaction term of the corresponding baseline characteristic (in addition to their level term). To be concise, we report only the index outcome variable and first follow-up results, and since the impact of the programme was largest for students in grade 3, we report only heterogeneous impacts for these students in the main text (see Table 6 in the Online Appendix for results from grade 5). Each column reports the treatment effect and the effect interacted with the baseline characteristic listed in the row, as well as the mean of the characteristic. For baseline student achievement and how often the teacher asks for silence at baseline, we convert the number into a ‘high’ (above median) or ‘low’ (below median) binary variable.

Similar to the literature on education interventions in developing countries (Kremer and Holla 2009), we find large differences in the impact of the programme by gender: the programme had no discernible effect for boys, but increased the test score index by 0.18 standard deviations for girls. There were no significant differences in treatment effect by baseline achievement or the education of the household head, although the point estimate of the treatment effect for high achieving students is quite large. For school characteristics, although the point estimate for educated teachers is quite large, there is no discernible differential effect on index scores by teacher education or whether there is a library in the school. However, there is a large differential effect for scores of students whose teacher has to ask for silence in their classroom an above average number of times (although this unsurprisingly does not hold up to multiple-hypothesis testing with the other school characteristics), suggesting that additional resources may be particularly useful to help teachers to better manage their classrooms. Finally, there are dramatic differences in programme impacts depending on whether the school is located in the southern region of the country (which is poorer and has worse school results) or in the northern region. There are no significant impacts of the programme in the north, whereas the grant increased third grade index scores by almost one-third of a standard deviation—an effect that persisted through the end of the grade (second follow-up, not shown).

The south–north differences in estimates of the impact of school grants are striking. In the remainder of the paper we examine whether there are differences between what school principals, teachers and parents did in response to the availability of school...
### Table 5
**Programme Impacts on Grade 3 Test Index by Student and School Characteristics**

| Overall (1) | Student characteristics (2) | (3) | (4) | School characteristics (5) | (6) | (7) | Region (8) |
|-------------|----------------------------|-----|-----|---------------------------|-----|-----|-----------|
| Treat       | 0.126**                    | 0.041 | -0.019 | 0.088                     | -0.003 | 0.080 | 0.006 | 0.066 |
|             | (0.060)                    | (0.073) | (0.081) | (0.092)                  | (0.080) | (0.071) | (0.075) | (0.067) |
| × female    | 0.176**                    | 0.155 |     |                            |       |      |          |          |
|             | (0.083)                    | (0.102) |       |                            |       |      |          |          |
| × high ability |                          | 0.003 |     |                            |       |      |          |          |
|             | (0.124)                    | (0.124) |       |                            |       |      |          |          |
| × head educated |                        | 0.185 |     |                            |       |      |          |          |
|             | (0.117)                    | (0.117) |       |                            |       |      |          |          |
| × teacher educated |                   | -0.024 | 0.227* |                            |       |      |          |          |
|             | (0.124)                    | (0.124) |         |                            |       |      |          |          |
| × library   |                            |       |      |                            |       | 0.324** |          |
|             |                            |       |      |                            |       | (0.121) |          |
| × silence students |                      |       |      |                            |       |      |          |          |
|             |                            |       |      |                            |       | (0.117) |          |
| × south     |                            |       |      |                            |       |      | 0.324** |          |
|             |                            |       |      |                            |       |      | (0.121) |          |

Interaction mean 0.50 0.50 0.59 0.50 0.23 0.32 0.20 0.32

N 1350 1350 1156 666 1229 1280 1229 1350

**Notes**

Standard errors, clustered by school, are in parentheses.
Conditioning variables: grade, gender, household size, number of children, education of head, distance to school, wealth index, interview language, baseline scores, missing dummies.

*, **, *** indicate $p < 0.10$, $p < 0.05$, $p < 0.01$, respectively, corresponding to $p$-values from the usual single-hypothesis tests. There were no significant $p$-values from multiple-hypothesis tests.
grants, to help shed light on the sources of regional differences and grade differences in the impacts of the programme on the performance of students.

Understanding the differences by grade and region

Why were the effects stronger for grade 3 and students in the south? To shed light on these questions, Table 6 reports the effect of the grant programme on household characteristics (panel A), teacher characteristics (panel B), and school characteristics (panel C). We present separate estimates of programme impacts for grade 3 (column (1)), grade 5 (column (2)) and the difference (column (3)), as well as impacts in the south (column (4)), north (column (5)) and the difference (column (6)).

While other studies in developing countries have shown that households may act as either substitutes or complements to school investments (Das et al. 2013), we find virtually no effect of school grants on parental expenditure on schooling (tuition, uniforms, supplies) for either grade or either region, nor any effect on whether the student has a tutor. Students also do not appear to substitute away from or towards labour as measured by whether they work after school or miss school (though there is a small marginally significant effect on missing school in the south). It is noteworthy that unlike Das et al. (2013), we do not find strong evidence of a parental response, even in the short run. Furthermore, it is unlikely that these grants were ever fully unanticipated by parents since, in theory, parent committees should have been consulted in the grant application process. Das et al. (2013) argue that for the programmes that they study, it is precisely in situations where grants are fully anticipated that we should expect a strong compensatory response from parents.

In contrast, we find meaningful effects of school grants on teacher characteristics, as well as differences in these effects by grade and region. Teachers are significantly more likely to have had training in the past five years as a result of the grant (by 8 percentage points for grade 3, for example), but these effects, while significant on their own, are not statistically significant across grades or regions. On the other hand, grade 3 teachers and teachers in the south are significantly less likely to have to ask for silence in the classroom, and these differences are significantly different from grade 5 teachers and teachers in the north. On the whole, the teacher effects in combination with the heterogeneity results on student behaviour in the classroom suggest that one channel through which the grant impacted student achievement was by improving teacher management of classrooms, particularly in classrooms that had behavioural issues at baseline. If it is harder to improve behavioural issues as children age, then this may help to explain the differential effects by grade.

Among school characteristics, there was an increase in the number of students in the north, which was not matched by an equally large increase in the number of teachers, and which could have led to a dilution of treatment effects on student outcomes. Schools that received a grant in either the north or the south were more likely to have a library at first follow-up, and spent more on textbooks, tutoring and teacher training, but these effects were not significantly different in the north compared to the south. Finally, school grants significantly decreased teacher turnover in the south. Given that teachers are likely to be one of the most important inputs in the schooling production function, the fact that in the south the programme significantly affected the amount of training that they received, and how likely they were to remain in the school from one year to the next, is consistent with the finding of strong programme impacts on student performance in this region of the country.
### TABLE 6
**Programme Impacts on Household, Teacher and School Characteristics by Grade and Region, First Follow-Up**

|                        | By grade | By region |
|------------------------|----------|-----------|
|                        | Grade 3 (1) | Grade 5 (2) | Difference (3) | South (4) | North (5) | Difference (6) |
| **Panel A: Household characteristics** | | | | | |
| Days of school missed last week | 0.106 | 0.010 | 0.096 | 0.171* | 0.031 | 0.139 |
| (0.075) | (0.055) | (0.086) | | (0.090) | (0.058) | (0.107) |
| Student works after school | −0.008 | −0.016 | 0.008 | −0.008 | −0.012 | 0.005 |
| (0.012) | (0.014) | (0.014) | | (0.017) | (0.013) | (0.022) |
| Parent involved in school | 0.037 | 0.026 | 0.011 | −0.015 | 0.042 | −0.057 |
| (0.038) | (0.038) | (0.039) | | (0.058) | (0.037) | (0.068) |
| Expenditure on schooling (000s CFA) | 0.121 | −0.162 | 0.283 | 0.248 | −0.085 | 0.332 |
| (0.456) | (0.401) | (0.481) | | (1.044) | (0.360) | (1.104) |
| Student has tutor | −0.027 | −0.009 | −0.017 | −0.024 | −0.017 | −0.007 |
| (0.026) | (0.029) | (0.031) | | (0.020) | (0.027) | (0.033) |
| **Panel B: Teacher characteristics** | | | | | |
| Teacher has baccalaureate | −0.052 | 0.018 | −0.070 | −0.140* | 0.010 | −0.150* |
| (0.047) | (0.046) | (0.064) | | (0.076) | (0.038) | (0.085) |
| Teacher had training in past 5 years | 0.083** | 0.101** | −0.018 | 0.136* | 0.083** | 0.053 |
| (0.039) | (0.047) | (0.056) | | (0.072) | (0.036) | (0.081) |
| Minutes spent preparing lesson | 2.115 | 0.061 | 2.054 | 0.506 | 1.261 | −0.756 |
| (1.726) | (1.614) | (2.023) | | (2.503) | (1.541) | (2.939) |
| Number of textbooks | 6.209 | 6.493 | −0.284 | 5.656 | 6.404 | −0.839 |
| (4.231) | (4.964) | (5.296) | | (6.496) | (4.414) | (7.854) |
| Number of chairs | 0.018 | 0.071** | −0.053 | 0.093 | 0.032 | 0.061 |
| (0.038) | (0.035) | (0.041) | | (0.072) | (0.033) | (0.080) |
| Times per day asking for silence | −1.638** | 1.109 | −2.374*** | −3.139** | 0.463 | −3.01** |
| (0.686) | (0.830) | (0.931) | | (1.538) | (0.627) | (1.661) |
| Panel C: School characteristics | By grade | By region |
|--------------------------------|----------|----------|
|                                 | Grade 3  | Grade 5  | Difference | South (4) | North (5) | Difference (6) |
| Number of teachers             |          |          |            |           |           |                |
|                                | (1)      | (2)      | (3)        | (4)       | (5)       | (6)           |
|                                | −0.833   | 0.813    | −1.646     | (1.120)   | (0.560)   | (1.253)       |
|                                | (0.833)  | (0.813)  | (1.646)    | (1.120)   | (0.560)   | (1.253)       |
| Number of students             |          |          |            |           |           |                |
|                                | (1)      | (2)      | (3)        | (4)       | (5)       | (6)           |
|                                | 0.215**  | 0.116**  | 0.099      | (0.089)   | (0.049)   | (0.101)       |
|                                | (50.387) | (28.896) | (58.085)   | (50.387)  | (28.896)  | (58.085)      |
| School has library             |          |          |            |           |           |                |
|                                | (1)      | (2)      | (3)        | (4)       | (5)       | (6)           |
|                                | 24.489** | 21.291** | 3.199      | (10.720)  | (10.041)  | (14.688)      |
| Amount spent on textbooks      |          |          |            |           |           |                |
|                                | (1)      | (2)      | (3)        | (4)       | (5)       | (6)           |
|                                | 30.435*  | 13.627*  | 16.809     | (17.174)  | (7.705)   | (18.823)      |
| Amount spent on tutoring       |          |          |            |           |           |                |
|                                | (1)      | (2)      | (3)        | (4)       | (5)       | (6)           |
|                                | 27.636** | 26.919*  | 0.717      | (13.831)  | (14.206)  | (19.827)      |
| Amount spent on teacher training|          |          |            |           |           |                |
|                                | (1)      | (2)      | (3)        | (4)       | (5)       | (6)           |
|                                | 31.148   | 77.389*  | −46.241    | (43.392)  | (44.444)  | (62.114)      |
| Teacher composition changed in past year |          |          |            |           |           |                |
|                                | (1)      | (2)      | (3)        | (4)       | (5)       | (6)           |
|                                | −0.230***| −0.061   | −0.169*    | (0.086)   | (0.042)   | (0.096)       |

Notes
Columns (3) and (6) report the difference between the impact of the programme in grade 3 and grade 5, and between the south and the north, respectively. Standard errors, clustered by school, are in parentheses. Amounts in CFA. Sample size grade 3/grade 5/south/north: panel A \(N = 961/938/268/1531\); panel B \(N = 3026/2978/1205/4799\); panel C \(N = 3102/3044\).

*, **, *** indicate \(p < 0.10, p < 0.05, p < 0.01\), respectively.
To further understand why the programme differentially affected students, teachers and schools in the south compared to the north, Table 7 reports regional differences in test scores and household characteristics in untreated samples and project characteristics.

### Table 7
**Regional Differences, Grades 2 and 3**

| Panel A: Test scores at beginning of grade 2 (baseline) | South (1) | North (2) | Difference (3) | Observations |
|--------------------------------------------------------|-----------|-----------|----------------|--------------|
| Percent correct: French                                 | 0.430     | 0.420     | 0.010 (0.027)  | 2722         |
| Percent correct: Mathematics                           | 0.325     | 0.373     | −0.048** (0.023)| 2752         |
| Percent correct: Oral                                  | 0.154     | 0.242     | −0.088*** (0.016)| 1388         |
| Index score (standardized)                             | −0.239    | 0.049     | −0.288** (0.112)| 1359         |

| Panel B: Test scores at beginning of grade 3 (first follow-up, control schools) | South (1) | North (2) | Difference (3) | Observations |
|---------------------------------------------------------------------------------|-----------|-----------|----------------|--------------|
| Percent correct: French                                                         | 0.411     | 0.564     | −0.153*** (0.022)| 1805         |
| Percent correct: Mathematics                                                    | 0.434     | 0.569     | −0.136*** (0.021)| 1801         |
| Percent correct: Oral                                                           | 0.233     | 0.383     | −0.150*** (0.022)| 917          |
| Index score (standardized)                                                      | −0.629    | 0.100     | −0.730*** (0.100)| 894          |

| Panel C: Household characteristics (first follow-up, control schools)           | South (1) | North (2) | Difference (3) | Observations |
|---------------------------------------------------------------------------------|-----------|-----------|----------------|--------------|
| Household size                                                                  | 8.625     | 10.216    | −1.591*** (0.412)| 643          |
| Number of children in household                                                 | 5.050     | 5.551     | −0.501* (0.276)  | 643          |
| Head has some education                                                         | 0.550     | 0.401     | 0.149*** (0.050) | 641          |
| Percent of adult females with some education                                    | 0.261     | 0.224     | 0.038 (0.043)    | 638          |
| Wealth index                                                                    | −0.654    | 0.137     | −0.792*** (0.092)| 642          |
| Interview conducted in French                                                   | 0.175     | 0.090     | 0.085** (0.041)  | 643          |

| Panel D: Project characteristics (second follow-up, treatment schools)          | South (1) | North (2) | Difference (3) | Observations |
|---------------------------------------------------------------------------------|-----------|-----------|----------------|--------------|
| Months since project began                                                      | 15.914    | 23.479    | −7.564*** (1.144)| 152          |
| Students helped to draft application                                            | 0.800     | 0.547     | 0.253*** (0.082) | 152          |
| Project included textbooks                                                     | 0.800     | 0.895     | −0.095 (0.074)   | 149          |
| Project included computer materials                                            | 0.029     | 0.121     | −0.092** (0.042) | 151          |
| Project included teacher training                                              | 0.914     | 0.752     | 0.162** (0.062)  | 152          |
| Project included management training                                           | 0.629     | 0.368     | 0.261*** (0.093) | 152          |
| Project included building courses                                              | 0.971     | 0.821     | 0.151*** (0.046) | 152          |
| Project included improving general education                                    | 0.563     | 0.456     | 0.106 (0.100)    | 146          |
| Project included improving educational outputs                                  | 0.114     | 0.129     | −0.015 (0.063)   | 151          |
| Amount spent on principal (000,000s CFA)                                        | 0.082     | 0.034     | 0.048*** (0.014) | 147          |
| Amount spent on teachers (000,000s CFA)                                         | 0.317     | 0.278     | 0.039 (0.058)    | 147          |
| Amount spent on management (000,000s CFA)                                       | 0.128     | 0.041     | 0.087*** (0.022) | 147          |
| Amount spent on students (000,000s CFA)                                         | 0.505     | 1.025     | −0.520*** (0.092)| 148          |

**Notes**
Columns (1) and (2) show variable means for schools in the south and in the north (respectively), and column (3) shows the differences in means (south–north) and standard errors, clustered by school, in parentheses. Column (4) shows the number of observations in each row.

*, **, *** indicate $p < 0.10$, $p < 0.05$, $p < 0.01$, respectively.
of the treated sample. Panel A shows that students in southern schools performed worse on almost all baseline tests than students in the north, and panel B shows that among control schools, this performance gap is even wider after a year. Panel C compares household characteristics of students in the south and in the north. Because of the missing schools at baseline, we take characteristics measured in the first follow-up among students in the control schools. A few interesting patterns emerge. Households in the south are poorer but have fewer children and better educated heads, suggesting that southern households may place more emphasis on education despite having fewer resources.

Finally, panel D of Table 7 shows the characteristics of projects being undertaken by schools with the school grant funds (i.e. treated schools), as reported by principals in the second follow-up. In the south, students were much more frequently named as participants in the drafting of the proposal. Although the extent of student input is unclear, this could indicate that principals were more sensitive to the needs of the students in the south.\textsuperscript{14} It is also significant that projects in the south started later. By the end of year 2 of the study, projects in the north had been running for 7.6 months longer than in the south. If results faded out quickly, then this could explain why we observe effects of the more recent projects than in the earlier projects. This is unlikely to be the case, however, given the persistence of programme impacts on student outcomes. Some of the more remarkable differences relate to the components of the project. The schools in the north were more likely to have components involving the purchase of computer-related materials, while schools in the south were much more likely to have components related to teacher training, building courses, managerial training, and spending on principals. At the same time, the northern schools reported spending more of the project funds on students. Thus there are clear differences in the characteristics of projects in schools in the north and the south, as stated by the principals of these schools. Schools in the south seem to be investing more in the teaching and management abilities of their human resources, while schools in the north invest more in equipment. Though suggestive, this may well be a force behind the large differences in programme impacts in these two sets of schools.

The resulting picture from this section is mixed. There are several differences between the south and the north: households are poorer yet more educated in the south, and projects in the south tended to focus on training and human resources, and less on information technology. However, when we examine the impact of the grant on various other outcomes, there are few significant differences between the north and the south. Nevertheless, the improvement in test performance in the south is remarkable, and one can expect that fewer classroom disruptions—perhaps due to teacher training—can help learning.

V. CONCLUSIONS

There is substantial debate about the importance of resources in schools for student performance. More often than not, increases in school resources are not associated with increases in student performance. One reason may be that central education authorities lack an understanding of the needs of schools. Principals, on the other hand, could have better information and could target resources more efficiently. The danger is that incentives to improve student performance may vary across school principals and there may be several sources of local pressures for alternative uses of these funds.
This paper studies the impact of a Senegalese school grants programme on student performance and on potential mechanisms that could underlie the change in school performance induced by such a programme. We find impacts of school grants on student learning, especially on girls and students with high ability levels at baseline, at a cost-effectiveness rate commensurate with other interventions that aim to improve education outcomes in low-income countries. Notably, these impacts persist over the two years of our evaluation. However, these impacts occur only in grade 3, not grade 5, and they are stronger in the south of the country. These results suggest that resources distributed in a decentralized manner can have positive impacts on students, at least at the scale of this experiment.

It is difficult to explain the grade differential in programme impacts. One conjecture is that principals focus on earlier grades because they see the foundations for future learning at that age. Alternatively, higher grades might already be well resourced relative to lower grades, so the marginal improvement in resources is less effective at higher grades. Future research examining the grade levels for which principals and teachers do and should focus their efforts and resources would help to answer this question.

We can say a little more, however, about the north–south difference in programme impacts, based on how we see principals spending their resources. While schools in the north emphasized information technology (IT) and other educational materials, schools in the south emphasized human resources, namely through the training of teachers and school administrators. Our results suggest that the latter type of investments, although perhaps less visible to the local community (and therefore less preferred by say, local politicians, or even local school authorities), may be more effective than the former type of investments.

Overall, while our experimental design allows us to cleanly isolate the impact of grants for primary schools that were selected to receive funds, we have limited ability to comment on schools that did not apply for a grant or schools whose proposals were rejected. Only 10% of Senegalese primary schools applied for a grant, and these applicants appeared to be better resourced and better organized than schools that did not apply for a grant. Thus the impact of a grant may be heavily dependent on the decision (and possible barriers) of schools to apply for funds, and the ‘success’ of a grants programme (in the eyes of policymakers) may depend on whether the targeted population of schools actually apply and whether they use resources efficiently. In addition, in alternative settings in which few proposals can be funded (unlike ours, in which 90% of proposals were funded), the impact of an intervention may also rely heavily on the ability of funders to distinguish between applicants. These questions related to the grant submission and selection processes are interesting avenues for future research.

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NOTES

1. Fees collected from parents represent around 10% of school funding in 2006 (PASEC 2007).
2. See Figure 2 of the Online Appendix for example test questions. The majority of questions on these exams were open-ended and hence unlikely to be simply measuring random guesses.
3. To examine the possibility that funds were disproportionately channelled to students preparing to enter secondary school, we also administered written assessments in mathematics and French to a random sample of children who were in grade 6 (or repeated grade 5) at first follow-up, and also surveyed their teachers.
4. The other schools were out of bounds due to inclement weather or rebel activity in the south. While this may have impacted the representativeness of the baseline sample, it did not affect the balance as accessibility was not correlated with treatment status, as we report later.
5. With the exception of the index score, we chose not to standardize the mathematics, French and oral scores. The tests were designed to appropriately measure the types of skills taught in the first years of primary school, and looking at the proportion of right answers in this test is a natural way to assess student knowledge in these subjects, and its progress over time. Furthermore, these scores are specific to Senegal, so standardization would not be useful for international comparisons. Even within sample, we show in Figures 3 and 4 of the Online Appendix that the distributions of scores are highly non-normal, so one standard deviation in test scores does not have the usual meaning. Nevertheless, for our main results we report standard deviations of control schools to convert results to standard deviations.
6. The wealth index is standardized to have unit variance and is defined as the first principal component of the following variables: the home has electricity, the home has plumbing, the home has a radio, the home has a television, the home has a telephone, the home has a computer, the home has a refrigerator, the home has gas, the home has an iron, the home has a bicycle, the home has an automobile, the home has a bed, the home has a modern toilet, the number of chickens, the number of sheep, the number of cows, the number of horses, the number of donkeys, the amount of land, savings, debt, food expenditure, child expenditure, other expenditure, wall material, ground material and roof material.
7. One interpretation of the individual tests is that they are noisy measurements of one underlying human capital factor. By using the first principal component of the three tests, we may improve precision.
8. Results without conditioning variables are presented in Table 4 in the Online Appendix and are almost identical, but less precise.
9. Therefore the lack of statistically significant results in grade 5 (but not in grade 3) does not appear to be due to a lack of power. If the point estimates for grade 5 were as large as those for grade 3, then it is likely that we would be able to reject that they were statistically equal to zero. When designing our study, we anticipated that with our sample we would be able to detect programme impacts of between 0.2 and 0.3 standard deviations, which is in line with what we find.
10. See https://www.povertyactionlab.org/node/7 (accessed 29 December 2018) for more examples and citations.
11. Conditioning on baseline characteristics unfortunately drops several schools that were missing at baseline. Table 5 in the Online Appendix shows that missing schools at baseline are mainly in the south and display worse student performance in the first follow-up than comparable non-missing schools. Fortunately, they are not disproportionately control or treatment schools.
12. This result barely withstands multiple-hypothesis testing with the two other regressions with student characteristics: the p-value is 0.102.
13. Table 7 in the Online Appendix shows that characteristics are balanced in both regions.
14. This is in line with a recent study in Indonesia, which found that school grants were effective in improving education quality only when coupled with significant community participation (Pradhan et al. 2014).

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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article:

Figure 1 Location of Schools in Sample
Figure 2 Sample test question from oral test
Figure 3 Distribution of Second/Third Grade Scores
Figure 4 Distribution of Fourth/Fifth Grade Scores
Table 1 Student Test Score Sample Sizes and Attrition
Table 2 First Follow-up Descriptive Statistics and Balance
Table 3 Difference in Baseline Characteristics among Non-Attriters
Table 4 Program Impacts on Grades 3 and 5 Test Scores, No Controls
Table 5 Characteristics of Schools by Baseline Missing Status, First Follow-Up
Table 6 Program Impacts on Grade 5 Test Index by Student and School Characteristics
Table 7 Baseline Descriptive Statistics and Balance (Grade 2), by Region

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