

# Willing but Unable? Short-Term Experimental Evidence on Parent Empowerment and School Quality\*

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September 2, 2015

## Abstract

Giving communities power over school management and spending decisions has been a favored strategy to increase school quality, but its effectiveness may depend on local capacity. We examine the short-term responses of a grant to school committees in Niger. We find that parents increased participation and responsibility, but these efforts did not improve quality on average. Enrollment at the lowest grades increased and school resources improved, but teacher absenteeism increased, and there was no measured impact on test scores. To understand this, we examine heterogeneous impacts and spending decisions. Our findings suggest that programs based on parent participation should take levels of community capacity into account: even when communities are willing to work to improve their schools, they may not be able to do so. However, the generalizability of this result is limited by the short-term nature of the experiment.

## 1 Introduction

The dramatic expansion of access to schools in the last two decades is the result of an unprecedented effort to increase education in poor countries. However, the quality of education is often

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\*Cornelia Jesse led the implementation of this project and contributed substantially to its design, and we are deeply indebted to her for her leadership. We thank Yann Algan, Bruno Crépon, Esther Duflo, Pascaline Dupas, Pierre de Galbert, Emeric Henry, Cornelia Jesse, Florian Maynéris, Miguel Urquiola and the seminar participants at J-PAL Europe, Sciences Po, Columbia University, Oxford University, UCL, Paris I and the Journées d’Economie Publique Louis-André Gérard-Varet for helpful comments and discussions. We also thank several anonymous referees for thoughtful and detailed suggestions. We thank Adama Ouedraogo for his support throughout the project, and are grateful to Pierre de Galbert for excellent project management and Gabriel Lawin for data collection and management, and also Elizabeth Linos, Andrea Lepine and Hadrien Lanvin for research assistance. We thank the Government of Niger and the staff of the Ministry of Education for their collaboration, in particular Amadou Tchambou, Yacouba Djibo Abdou, Salou Moussa, and Damana Issaka. Mathieu Brossard was central in the initial conception and design of the project. Much of the project was carried out when Elizabeth Beasley was at J-PAL Europe and she thanks J-PAL for their support. Finally, and most importantly, we gratefully acknowledge the parents, staff and pupils of the schools for the time and information they shared with us. This work was supported by the World Bank and the donor partners of the Education for All Fast Track Initiative through the Education Program Development Fund. All errors remain our own, and the opinions expressed in this paper are ours alone and should not be attributed to the institutions with which we are affiliated, the World Bank or the Government of Niger.

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low.. One common strategy to improve quality is through improved management and oversight, and in particular by increasing involvement of parents and the community (World Bank, 2004). Community-based management policies have been widely adopted throughout the world over the past decade (see Barrera-Osorio et al, 2009 for an overview).<sup>1</sup> Grants to school committees, that is, putting money under the control of parents, are one potential way to increase school quality directly, by increasing school resources, and indirectly, by spurring parent participation. For this to work, parents must have the time, energy, and capacity to participate in school management effectively. Given the heavy investment in such programs, it is important to understand whether, and under which circumstances, they can actually work.

This paper provides evidence from a field experiment on the short term impact of a program to encourage parent participation in school management through grants to school committees in a context of low parent authority and capacity. In Niger, levels of education among adults are extremely low: 70% of Nigeriens aged 15-44 in 2010 had no education,<sup>2</sup> and the system for education is very hierarchical and centralized. In a pilot program to improve school quality, the Ministry of Education of Niger, in partnership with the World Bank, gave grants to school committees that had been trained in school management with the aim of increasing parent involvement.. A randomized evaluation was incorporated into the pilot project to provide information for scale-up. We use detailed data from 1000 schools (500 treatment) to assess the impact of the grant on parent empowerment, school management, and school quality. An important limitation of the study is that it provides only short-term evidence on behavioral responses: the first grant arrived late 2007 and was meant to continue several years, but the evaluation ended in 2009. The survey was administered during April and May of 2008 and administrative data was collected at the beginning of the 2008-2009 school year. This paper thus documents the short-term dynamics of an anticipated long-term program.

On average, we find that parents were willing to increase their participation in school management, but educational quality did not improve in a meaningful way as a result of this participation.

There is an overall positive impact of the grant program on parents' involvement and responsibility:

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<sup>1</sup>School-based management programs have been implemented in Argentina, Australia, Bangladesh, Canada, Guatemala, Honduras, Hong-Kong, India, Lebanon, Lesotho, Macedonia, Madagascar, Mexico, Nicaragua, the Philippines, Senegal, Serbia, Sri Lanka, the Gambia, the United Kingdom, and the United States (Duflo et al, 2015).

<sup>2</sup>World Development Indicators, World Bank, source: International Institute for Applied Systems Analysis (IIASA)

communities with the grant participated more and took on more responsibilities than those without the grant, although the average community did not engage in supervising teacher presence. Parents did not reduce their own contributions in response to the grant.

The impact on school management is mixed: cooperation between school stakeholders improved, but overall accountability did not change, and spending shows both expected and unexpected changes: there was more spending in infrastructure, but also school festivals and playground equipment, and, most unexpectedly, investment in agricultural projects which were probably non-educational but intended to make a profit.

Finally, school quality did not improve with these changes, at least in the short term. There were subsequent improvements in infrastructure and health resources, as well as an increase in participation at the lowest grades: fewer dropouts in 2007/2008 and increased enrollment in grade 2 in 2008/2009, but there is no evidence of a change in test scores (note that we cannot exclude the possibility of a downward bias in the estimate of test score impact due to differential dropouts, but the lack of change in test scores at levels that had no participation changes supports the finding of no impact on test scores). We also observe a decrease in teacher effort in response to the grant, which we attribute to the fact that some teachers have a preference for a centralized government and might be reluctant to collaborate with parents, especially when parents do not spend the money on projects that make the teacher's life easier.

We examine heterogeneous effects along several different dimensions, and here we highlight three interesting patterns. First, we find that in situations where the school committee is educated or has experience in another community organization - both of which we take as proxies for real authority - parents increased monitoring teacher attendance in response to the grant (though this did not mitigate the negative effect of the grants on teachers). Second, in small (one-teacher) schools, school committees spent on items that benefited the teacher, and teacher attendance increased in response to the grant in these schools. These results together suggest that teachers' response to parent participation depends on whether parents are acting in opposition to, or alliance with, the teachers. Third, we find that rural schools used some of the grant to invest in agricultural opportunities, and urban schools did not but invested in school infrastructure instead.

This study is related to two strains of the economics literature: parent participation and school resources. Previous evidence on the effectiveness of programs to increase quality via increased par-

ent participation is mixed. Banerjee et al (2010) report that providing information to parents about the school committee and training the community to measure educational performance in India had no impact on the activity of school committees, and therefore no impact on education outcomes. Duflo, Dupas and Kremer (2015) find that a training to empower parents helped mitigate the negative response of regular teachers to the addition of a contract teacher. In Madagascar, Lassibille et al (2010) found that facilitating community/school interactions, combined with streamlining management practices had positive impacts on attendance and learning. Other studies have supplied evidence that empowering the community to manage schools improves school quality, though these papers generally do not include random variation in treatment assignment and so the identification is weaker. Bryk et al. (1998) and Hess (1999) have argued that student achievement improved in Chicago after the implementation of reform involving the community in school management and Di Gropello (2006) overviews four school-based management programs in Latin America and concludes that school-based management models have led generally to greater community empowerment and teacher effort. Participation in school management may also be linked to social capital more generally: Sawada and Ishii (2012) employ matching and instrumental variable approaches to measure the impact of the COGES program itself in neighboring Burkina Faso, and find increases in social capital measured using several different tools, including field experiments.

Another group of studies point to heterogeneity in the performance of participatory programs, and in the effect of decentralization more generally. Blimpo and Evans (2011) find no impact of a training for school committees on learning except in schools where school committee members were educated. Pradhan et al. (2014) find that an intervention to empower parents is effective only when combined with an intervention fostering the ties between the school committee and a local governing body. Decentralization of secondary school management in Argentina led to higher test scores in provinces with higher managerial capacity, and lower test scores in provinces with lower managerial capacity (Galiani et al., 2002). Using panel estimation on PISA data, Hanushek et al (2013) estimate that increasing school autonomy is associated with lower student performance in countries with generally lower performance, and with higher student performance in countries with generally higher performance. While context of rural Niger is likely to be substantially different from these contexts, there is good reason to anticipate that there may be heterogeneous impacts of parent participation.

Previous studies on increasing school resources have found that it may crowd out the contributions of other actors. For example, parents in Romania decreased time spent on homework when their child was admitted to a better school (Pop-Eleches and Urquiola, 2013). In Zambia and India, households decreased spending for education when they anticipated an increase in school funding (Das et al., 2013). In Kenya, civil-servant teachers decreased presence at school when school committee hired an extra teacher (Duflo, Dupas and Kremer, 2015).

Our findings contribute specifically to the literature on heterogeneity by showing that authority and capacity are important prerequisites for parents to undertake the more difficult aspects of management and that cooperation between parents and teachers (rather than confrontation) may be key.

An overall message is that parents will not always or even generally make optimal spending and management decisions to increase quality. It may be costly and time-consuming, parents may not have good information about how schools work and thus may not make optimal decisions, and it may be very difficult to put pressure on teachers to improve service quality. It may be particularly difficult since capacity depends on parent power vis-à-vis teachers, or “real authority” in the terms of Aghion and Tirole (1997), who underscore the fact that formal authority (the right to make decisions) need not imply real authority (effective control over decisions).<sup>3</sup>

A major limitation of the paper is the short-term nature of the findings. Long-term follow up was impossible, so we cannot discount the possibility that different results would have emerged after one or two more years. However, we believe that the results presented here are still useful, as, first, they give evidence about the barriers that communities may face at the beginning of participatory programs, and, second, the richness of the data we use to analyze spending decisions, contributions, involvement and responsibility, and link them to community characteristics gives some insight into the mechanisms at work within communities when making school management decisions.

The remainder of the paper is as follows. Section 2 presents some background information on education in Niger, and describes the school grant experiment. Section 3 presents the data and our estimation strategy and Section 4 the empirical results. Section 5 concludes.

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<sup>3</sup>Policies of *de jure* autonomy do not always lead to *de facto* autonomy (King and Ozler, 2004), and so participation may not be meaningful if communities have no actual power and even increase inequality by “leaving the poor behind” (Galiani et al, 2008).

## 2 Experimental Set-Up

### 2.1 Background on Education in Niger

Niger had made remarkable progress in education access in the decade prior to this evaluation: the number of children enrolled in primary school had more than doubled from 656,000 in 2000 to 1,554,102 in 2008, and net enrollment had risen from 27% to 49% in the same period. However, only 44% of children who begin primary school finished all grades, and only 43% of the sixth graders who took the national exam at the end of primary school passed it.<sup>4</sup>

The education system in Niger has traditionally been fairly hierarchical and rigid. Inherited from French colonization, the system replicates the French education system : highly centralized, with little, if any, room for local community participation. Unlike other systems, where the school might be supervised by a local governmental body, at the time of the evaluation there was generally no way for the local community to determine school policy or practice. Schools depended entirely on the hierarchical chain that originated in the Ministry of Education (except for some local fundraising , but these efforts were undertaken only when needs were not provided for by the Ministry).

In 2006 the Ministry of Education in Niger introduced school committees in all primary public schools in order to improve quality. These school committees (called the COGES) were designed to involve parents and community members in the school, improve accountability, improve management, and thus enhance access to and quality of education.<sup>5</sup> As discussed in the introduction, the establishment of local community groups for the purpose of improving public service provision via community participation is a strategy that many country governments and civil society organizations advocate. In many respects, the circumstances of Niger make a strong case for school-based management: low population density, vast distances and limited transportation, information, and communications infrastructure makes supervision of primary schools by the central government (or

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<sup>4</sup>The situation has continued to improve in terms of access to education: in 2011, net enrollment in primary school was 62%, and primary completion rates had risen to 46%.

<sup>5</sup>These school committees consist of 6 representatives, including the school director, who serves as secretary, and parent representatives. The parents are supposed to elect the representatives, who may also be the leaders of the Parent Association (APE), which includes all parents, and the Mother's Association (AME), which includes all mothers. In practice, the composition of the COGES varies by school. School committees are supposed to be responsible for the management of people working at the school (e.g. monitoring of teacher attendance and performance), financial resources (e.g. school meal funds) and material resources (e.g. purchase and management of textbooks, supplies and supplies). One of the school committee's central tasks is to draft an annual school improvement plan that includes its projects, activities, budget, and timelines to guide its work for the school year. The school committee works parallel to the APE and AME. Additional details and background are given in the Online Appendix, Section 1.

its regional structures) very costly, and the timely transmission of information to and from the central authorities for planning purposes is challenging.

In the districts where this program was carried out, the COGES were trained by several different organizations in financial management, governance (elections), and project planning. In 2006, many of the newly created and trained school committees were not actively engaged in school matters, nor did they develop an school improvement plan for the year. To spur school committee involvement and activity, the Ministry of Education introduced school grants to give the committees an incentive to meet, plan and undertake activities. The grants were expected to improve school management through increased parental participation and accountability, to improve school infrastructure and the quality of education, and to potentially increase enrollment rates and learning. The pilot project was carried out as a randomized evaluation in order to provide reliable information on impact prior to national scale-up.

The Ministry selected the regions of Zinder and Tahoua because the COGES there were already functional and had received basic training on planning and financial management, whereas COGES in the other six regions of Niger had not been trained yet. However, the context of these two regions is specific, even relative to the rest of Niger. The Zinder region is culturally similar to Northern Nigeria, with a relatively conservative Muslim population that has lower rates of formal schooling. On the other hand, the Tahoua region is a nomadic region where formal education poses a challenge because the nomadic population (the Tuareg and the Fulani) may often rely on children for herding. In both cases, one may expect parents to adhere less to formal schooling than in other regions in Sub-Saharan Africa.

## **2.2 Experimental Design**

The evaluation design included 1,000 schools in Tahoua and Zinder, randomly selected out of the 2,609 total public primary schools in those districts. Once these 1,000 schools were determined to be representative of the total pool of schools in those districts, half were randomly assigned to receive the grants and became the treatment group. The other 500 schools served as a control group. Both randomizations were stratified on inspection (a geographical administrative unit), existing support for the school committee (e.g. existing programs or sponsorship by NGOs), and whether the school was indicated as being in a rural or urban area in administrative data. Strata were constructed by

grouping the schools into inspection, then within each inspection into whether or not the school had existing support, and then within each of those groups, whether the school was in an rural or urban area. This gave 50 strata. Schools were assigned a random number between 0 and 1, and within each strata they were sorted by this random number, with the first half being assigned to treatment and the second to control. Data from the Administrative School Census in 2005-2006 (the school census is described below) was used to confirm balance between control and treatment schools along various observable characteristics (data from 2006-2007 was not yet available at the time of sampling in August 2007). Table 1 gives the balance check for the randomization and shows p-values for the test of equality of means across control and treatment, and shows no statistically significant differences.

The original project plan called for recurrent grants to schools for three consecutive school years, to be distributed at the beginning of each school year to support COGES activities. The Ministry of Education and the Ministry of Finance jointly worked out the grant transfer mechanism, consisting of a direct release of funds from the national treasury into the accounts of the two regional education authorities (i.e., one hierarchical level down from the national government). The funds were then transferred to the inspection level and then to the COGES. The transfers from the regional authorities and below took place as transfers of cash, which were recorded using signed receipts which were submitted to the Ministry of Finance.

In the first year, rather than receiving the grants at the beginning of the year as planned, the 500 COGES received the grants during December 2007 and January 2008, with the school year already in full swing, due to logistical difficulties with the transfer. The grants were not immediately distributed during the 2008-2009 school year, due to problems with the transfer mechanism <sup>6</sup>. Due to these issues and political disruptions in 2009, the evaluation was terminated after only one year. As a consequence, this evaluation evaluates only one year of the grant (the 2008/2009 grant was eventually distributed to some schools, after the evaluation was terminated).

The size of the grant was based on the size of the school (the number of classrooms), and the average was \$209 per school, or \$1.83 per student. The grant was a relatively modest amount that was determined by considerations of financial sustainability in view of a potential extension of the program by the government. For the purposes of comparison, the control schools raised a little over

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<sup>6</sup>The regional authorities were unable to obtain the actual funds from the local treasury due to a liquidity issue at the local treasury level.

\$0.60 per year per student from the parents on average, and had an overall budget of around \$199 including donations from private NGOs, and so the grant is relatively large compared to the usual fundraising and about equivalent to the annual amount of money available for school projects (note that in principle most school inputs such as teachers and books were provided in kind by the central government and so not included in this \$199 - if they were, the grant would be smaller than the overall operating budget of the schools). For an idea of the practical scale, the amount of the grant was not, except in the very largest schools, sufficient to build an additional classroom. This grant amount is smaller than grants provided to school committees in most other evaluations: Blimpo and Evans (2010) use a grant of \$500 per school in Gambia. Gertler, Patrinos, and Rodríguez-Oreggia (2010) use grants of US\$500 to US\$700 per school in Mexico, and Pradhan et al. (2014) evaluate a grant of US\$326 (to be completed soon with another US\$544) per school in Indonesia.

About a month before the grant arrived, all 500 treatment schools (and school committees) received a letter informing them of the grant program and its objectives, and the grant amount allocated to their school. It also included general guidelines on the use of the grants, but the specific activity to be supported by the grants was to be decided on by the school committee.<sup>7</sup> One copy of this letter was distributed to the school director and a second copy to the president of the school committee before the arrival of the grants. Compliance in this respect was satisfactory: the grants arrived in 498 schools of the 500 program schools, 492 in the exact amount allocated to them and six in an different amount (see Online Appendix, Section 2, for further details on compliance).

## 3 Data and Empirical Strategy

### 3.1 Data

#### 3.1.1 Sources

Data come from three sources: (i) administrative data on primary schools (the Ministry of Education's annual school census, also called administrative data), (ii) an evaluation survey administered to school staff and two members of the school committee at treatment and control schools, and (iii) a financial survey administered to one member of the school committee on a subset of treatment schools.

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<sup>7</sup>One randomly selected group of schools received a slightly more restrictive list of potential expenditures, and another group received a warning that their projects might be audited. Analysis of spending patterns did not show any difference between these groups.

The Ministry of Education in Niger administers an annual census of all primary schools, including community schools and madrassas (Koranic schools), which provide data on enrollment, teacher characteristics, school facilities and resources, and community characteristics. We use the 2006/2007, 2007/2008 and 2008/2009 censuses. Each census is collected in the fall of the school year (for example, the 2008/2009 census contains the information reported by the schools in fall of 2008).

In addition to the administrative data, the Ministry and the World Bank worked with a local NGO to prepare a detailed school survey to be administered to the 1,000 schools included in the experiment in April/May 2008, five to six months after grant distribution, to understand the immediate effects of the grant. This questionnaire included information on school infrastructure and resources, pupil enrollment and attendance, school improvement plan, school committee functioning and membership, and school activities. It also asked detailed questions about the level of education and personal wealth of the school committee members. Three tests were also administered at this time: a math test, a french test, and an oral exam. The oral exam was administered to the youngest (grades 1 and 2) pupils. Teacher's physical presence at that visit was also recorded. The visit was on a day when the school was supposed to be open, but was not announced in advance.

Finally, a financial survey was administered to 85 randomly selected treatment schools in January/February 2009, asking detailed questions about the receipt and spending of the grants, any problems with the administration of the grant, and use of the grants (including the existence of a receipt for each expenditure).

### **3.1.2 Use of the Grants**

The school committees used the grants in a variety of ways. Eighty-five schools were randomly selected for a detailed questionnaire on grant arrival and spending. The most common use was material inputs such as construction and office supplies, and other uses included investment projects, health and sanitation projects, and transportation. Overall, the largest share of spending of the grant was in construction, representing 32% of the total amount spent (Figure 1). Construction activities included building classrooms, but communities also constructed lodging for teachers, latrines, school enclosures, and other buildings. Other projects included electrification or producing copies of exams. Fourteen percent of schools surveyed used at least part of the grant on some sort

of agricultural investment project. It is unclear whether the loans or small business projects were profitable.

### 3.1.3 Outcomes

Since we are using many different indicators of parent participation, and we wish to be able to draw general conclusions about the experiment's impact, simplify interpretation, and to guard against cherry-picking of results, we present results for indices that aggregate information over multiple outcome variables (following Kling et al, 2007). The aggregation also improves statistical power to detect effects that go in the same direction within a domain. The summary index  $Y$  is defined to be the equally weighted average of z-scores of its components, with the sign of each measure oriented so that more beneficial outcomes have higher scores. The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation. Thus, each component of the index has mean 0 and standard deviation 1 for the control group. The index is the average of the non-missing components, as long as the school has a valid response to at least two components. If only one component is available (or if no components are available), the school is dropped. We present different types of outcomes calculated in this way: parent participation, school management, and school quality. For each outcome, we construct several indices. The details and full list of component variables for each index are given in the Online Appendix, Section 3.

For parent participation in school, we use indices of parent contributions (e.g. school fees), involvement (e.g. going to meetings), responsibility (e.g. in charge of supplies), and teacher oversight (e.g. monitoring teacher attendance). School management is measured by two indices, accountability (e.g. keeping records) and cooperation (e.g. reported conflicts), and also by total spending across eight possible spending categories (infrastructure, supplies and textbooks, pupil educational support (e.g. remedial courses), pupil health, teacher support (e.g. housing), COGES expenses (e.g. travel to regional meetings), school festivals and playground, and investments in agriculture). Finally, we construct four indices to describe the effect of the grant on school quality: infrastructure (e.g. number of desks), materials (e.g. textbooks), health resources (e.g. first aid kit), and teacher effort (e.g. teacher attendance). Data for infrastructure, materials and health resources come from the 2008/2009 annual administrative database, collected in the fall of 2008, and so reflect changes between 8 and 10 months after receipt of the grants.

We also use data on dropouts, enrollment and test scores in order to examine the ultimate objective of increasing pupil participation and learning. We have two data points for participation in education. We use the number of dropouts reported by the school to our surveyors at the April/May 2008 questionnaire, and the change in enrollment from fall 2007 to fall 2008 reported to the Ministry of Education in the annual administrative censuses.

We have two limited measures of actual learning. First, we use test scores from a test administered to pupils during the April/May 2008 questionnaire. The test was administered to three grades, 10 pupils per grade. The pupils were supposed to be sampled from those who were enrolled at the beginning of the year, but in practice the 10 pupils appear to have been selected from the pupils present on that day. There are further quality problems with the test scores - including identical copies submitted by some grades in some schools - that raise concerns about the quality of the test score data. There is no evidence that the problems are correlated with treatment, and appear instead to be related to insufficient oversight of the examiners, so it is possible that the quality problems only add noise. However, as discussed below, the fact that participation is higher in the treatment schools and test takers were sampled from those present on that day leads to concerns of attrition bias in the test scores (if more children stayed in school in the treatment group, then the impact on test scores may be biased downwards). We therefore present the results as second-order evidence.

The overall results are nonetheless informative about the general level of education in rural Niger, and we provide some examples here to help give the reader a better idea of the context. In general, after discarding duplicate and suspect observations, pupils got about 1/3 of questions correct. For example, the following questions were asked:

- Grade 1: The interviewer asked the pupils to pick up a red crayon and a blue crayon out of a pile containing pieces of chalk of different colors: three white, one red, one blue, one yellow, and one green. 45% of pupils were able to do this.
- Grade 4: Pupils were asked to place the following numbers in order, from smallest to largest: 807 ; 708 ; 788 ; 800. 24% of pupils were able to do this.
- Grade 6: Pupils were asked to change an adjective from the masculine to the feminine form (Un nouveau maitre ==> Une \_\_\_\_\_ maitresse). 29% of pupils were able to

do this.

The second measure of learning comes from the annual administrative censuses which report the number of candidates for the national end-of-primary school exam and the number who passed. We use results reported on the 2008/09 census, which were for the end of the 2007/08 school year.<sup>8</sup> On average, slightly over half of the schools presented at least one student for the end of 6th grade test (recall that most schools do not have all grades).

### 3.1.4 Interaction Variables

The sample size was chosen to be large enough to allow for testing for heterogeneous treatment effects along community characteristics, one of the initial objectives of this the study.<sup>9</sup>

The dimensions chosen for measurement of heterogeneous effects are those that are likely to affect parent response to the grant, or that we think have policy relevance : education, experience in other organizations, and wealth of the COGES, whether the school is in an urban or rural area, and whether it is a one-teacher school. Descriptive statistics and balance information for the interaction variables is given in Table 2.

Our intuition is that COGES with higher levels of education and experience in other organizations are likely to have higher capacity to manage schools. We wish to make sure that these dimensions are not merely proxying for wealth, and so we include wealth as an interaction term (and it is not impossible that wealthy communities might react differently, either because they have more real authority or because they can leverage a larger supplemental contribution from the community). The distinction between urban and rural schools is important for education planners in general, and also it is important to ensure that the other interaction terms are not just proxies for the urban rural divide. Finally, one-teacher schools present a unique situation in terms of the power dynamics between the teachers and the parents, and also these very small schools are of relevance to education planners. Further details on the construction of these interaction terms are given in the Online Appendix, Section 3.

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<sup>8</sup>Schools choose which of their 6th grade students would sit for the exam. There is no evidence that schools were punished in any way for a low pass rate.

<sup>9</sup>We did not register our analysis plan in a secure independent register in 2007 when the experiment was designed, as is best practice today.

### 3.1.5 Attrition

There is some attrition in the datasets. Each year, a handful of schools do not return the administrative data questionnaire or the questionnaires are improperly filled out, leading to missing data for 3% of the schools for the infrastructure index and 1.4% of the schools for 2008/09 enrollment. The April/May 2008 survey was conducted on the basis of unannounced visits, which meant that many schools were closed. In addition, some schools were not visited due to security concerns, and still others closed early that year because the summer rainy season began early and so many children went to the fields with their parents to work. As a result, data from the evaluation questionnaire is available for only 814 of the 1000 schools.

We test for differences by treatment group in the proportion of schools with missing outcome variables as a whole and sub-divided by district, urban and rural, and whether the school had external support (for example, NGO sponsorship) prior to the project. Results are reported in the Online Appendix, Table A1. Eighty-four tests on treatment and interaction between treatment and sub-groups yield one statistically significant differences (at the 10% level or higher), which is well within the amount that would be expected with random attrition. The comparability between treatment and control groups is thus intact. As to external validity, there are more schools missing in the region where security was a concern (Tahoua, in the north).

## 3.2 Empirical Strategy

**Average Treatment Effect** We estimate intent-to-treat effects as measured by the differences in the means of school outcomes between schools initially assigned to the treatment group and schools initially assigned to the control group. Let  $T$  be an indicator for treatment group assignment and let  $X$  be a matrix of stratification variables. Estimation of the intent-to-treat effect  $\beta$  is from the following equation:

$$Y_j = \beta T_j + X_j \gamma + \varepsilon_j \quad (1)$$

where  $Y_j$  is the outcome of school  $j$ . The covariates  $X_j$  are included to improve estimation precision and include whether the school is urban, the total proportion of girls in 2007/08, the total enrollment in 2007/08, whether the school was supported by an outside NGO in 2006/07, and the inspection

(a geographic/administrative unit). All regressions use robust standard errors.<sup>10</sup> The absolute magnitudes are in units of the outcome’s standard deviation (based on the control group), so the estimate shows the treatment effect in terms of standard deviations.

**Heterogeneous Treatment Effects Along Community Characteristics** In the second step, we estimate intent-to-treat effects with an interaction term to determine whether the average treatment effect on parent and teacher behavior varies with community characteristics. We run regressions of the form:

$$Y_j = \beta T_j + \theta(C_j T_j) + \sigma C_j + X_j \gamma + \varepsilon_j \quad (2)$$

where  $C_j$  denotes a given community characteristic. In this case  $\theta$  is the additional (or reduction of) impact for schools with characteristic  $C_j$ . We include an indicator for urban schools and the interaction of this indicator with the treatment assignment for each characteristic whose correlation with being located in an urban area is above 0.1, to disentangle the effect of this characteristic from the effect of being located in an urban area.

## 4 Results

### 4.1 Average treatment effects

On average, we observe that parents did not reduce their own contributions in response to the grant and increased their involvement in and responsibility over school management, although they did not go so far as to enforce rules on teacher attendance. At the same time, school committees increased investment in infrastructure (buildings and the school enclosure) and school festivals, and invested in agricultural projects. Accountability did not change, but reported cooperation with a number of school stakeholders improved as a result of the grant. All these effects did not create a path to school quality improvement. While infrastructure and health resources improved and pupil participation increased a bit among the youngest, teacher attendance declined on average, perhaps because of resentment over parent empowerment, and we find no impact on test scores. Impacts on

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<sup>10</sup>An alternative specification uses dummy variables for the strats used in random selection, which were defined using a dummy variable for urban, the total enrollment in 2005/06, and support by an outside NGO in 2005/06. This specification does not substantially change the results, but increases precision of some coefficient estimates and decreases precision of others.

the detailed components of each index are given in the Online Appendix, Section 4. We also provide a model in the Online Appendix, Section 5, that explains our results and the existing results in the literature.

#### 4.1.1 Parent Participation

Table 3 shows the impact of grants on parent contributions (column 1). The overall message is that the grants did not change parent contributions to schools. The contribution index mean of the treatment group is statistically and economically similar to the mean of the control group. The analysis of the component variables (funds collected per pupil, in kind donations, and official fees charged) shows that neither financial nor in-kind contributions were affected by the grant (Table A2). This result contrasts with previous studies showing that parents decreased their contributions in response to an increase in school resources (Das et al., 2013; Pop-Eleches and Urquiola, 2013).<sup>11</sup> Note that in general the amount of cash income available to schools is obtained through parental contributions. An important consequence of this is that, due to the increase in cash from the grant, cash on hand for schools increased and thus so did the possibility for investment.

Table 3, column 2 shows the impact of grants on the volume of parent involvement in school management. We see that all indicators of parent involvement increased, although no change in any individual component is significant: the number of meetings was higher, time elapsed since the last meeting was smaller, the number of topics addressed in the meetings is larger, and the presence at the last meeting is larger (Table A3). Overall, the mean of the parent involvement index in the treatment group is 0.06 standard deviations larger than the mean of the control group, and this effect is significant at the 10% level.

The impact of grants on parent responsibility in school management is reported in Table 3, column 3. The overall effect of the grants is positive: the mean of the index of the treatment group is almost 0.06 standard deviations above the mean of the control group. The analysis of detailed variables composing the index shows some small increases in the proportion of school committees in charge of infrastructure, collecting financial contribution and spending financial contributions, although none of these increases are statistically significant (although some of p-values are close to

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<sup>11</sup>An alternative interpretation would be that this result derives from the fact that we measure only the first year of the grant, and so parents did not have time to change their own contribution of inputs (see Das et al, 2013, where crowding out was greater when a school grant was anticipated than when it was unanticipated). We think this is unlikely since the parents were notified in advance of the grants arrival.

conventional significance), while the effect on the index itself is significant at the 10% level (Table A4).

There is no overall impact on parent supervision of teachers (Table 3, column 4). We see small and insignificant changes in the proportion of school committees which discuss teacher behavior in school committee meetings, declare that they are active in increasing teacher attendance and improving education quality, declare that they monitor teacher attendance, and take remedial actions against teachers (Table A5). No trend emerges from these variables, and so there is no change in the teacher oversight index.

#### **4.1.2 School Management**

We observe no changes on accountability, but increased cooperation between school stakeholders and shifts in spending patterns.

While there is no impact of the grant on school accountability overall (Table 3, column 5), the analysis of the detailed components shows a 13% increase in the proportion of schools which could present a register for fund collection for examination, and a 21% increase in the proportion of schools which could present a register for fund expenses for examination, which might be simply the direct consequence of the fact that schools in the treatment group received money from the government and had something to record, rather than an overall change in accountability (Table A6). However, the grant did not change the use of other registers nor the frequency of minutes, which suggests that the increased involvement and responsibility of parents did not lead to a higher demand for transparency and record keeping.

Overall, we see an improvement in the cooperation between the school committee and different actors (Table 3, column 6): school committees are significantly more likely to report support from the community (+5 percentage points), from the teachers (+3 percentage points), from the parent committee (+5 percentage points) (Table A7). The proportions of school committees reporting support from local authorities, school administration, educational advisors and inspection are also consistently larger, although these differences are not significant. As a result, mean of the cooperation index for the treatment group is almost 0.07 standard deviations above the mean of the control group, significant at the 5% level. One explanation for the positive effect of grants on cooperation between school stakeholders and school committees is that giving resources under the

control of the school committee increased respect for its activities. The positive effect of the grant on the cooperation between the school committee and the different actors may be important when considering the short term nature of the experiment. It echoes the short term effect of a similar program on social capital observed in Burkina Faso (Sawada and Ichii, 2012).

Treatment schools increased spending on infrastructure, festivals and playground, and investments in agriculture. Figure 2 presents the absolute and percent differences in amounts budgeted for a given type of project in treatment schools compared to comparison schools (significant differences in dark grey, non-significant in light grey). The amount budgeted for a given type of project was significantly larger for infrastructure, festivals and playground, as well as investments in agriculture (Table 4): the amount budgeted for infrastructure was 20% larger in the treatment group (107,705 FCFA (215 USD) vs. 86,119 (172 USD) significant at the 5% level), the amount budgeted for festivals and playgrounds was sixfold greater than in the control group (1031 FCFA (about 2 USD) vs. 166 FCFA (0.33 USD), significant at the 1% level), and the amount budgeted for investments in agriculture was fourfold greater (2,416 FCFA (5 USD) vs. 583 FCFA (about 1 USD), significant at the 1% level). Note that the difference, while large relative to the amount spent in control schools on these activities, is small compared to the entire amount of the grant, so the bulk of the grant was not used on school festivals, playground and agricultural investments. The size of the increase in infrastructure spending in absolute terms (19,659 FCFA, or 40 USD)) is much larger than the increases in agriculture and festival and playground expenses (1,833 FCFA (a bit less than 4 USD) and 865 FCFA (almost 2 USD), respectively).

The investments in agriculture do not seem to have been done in the interest of one person, which might be considered a theft of resources, but rather as an investment on the part of the school (since they were recorded in the school ledger). One interpretation of the investment in agricultural projects is that credit in many areas of Niger is severely constrained. There may be profit opportunities from investment in agriculture (either in terms of raising crops or arbitraging prices for inputs or food products), but since isolated areas suffer from low levels of credit, these profitable opportunities are unexploited. If the COGES is aware of these opportunities, and they are patient, it may be most optimal for the long-term interest of the school to invest the windfall cash grant rather than spend it on educational inputs immediately. However, we cannot be sure that these investments were made for the profit of the school and have to consider the possibility

that it did not benefit to the pupils in any way.<sup>12</sup>

Finally, we see that school committees had spent just above a quarter of the grant at the time of the April/May 2008 questionnaire: the average increase in the total spending amount is 28,512 FCFA (57 USD), while the average grant is 104,500 FCFA (209 USD). This finding indicates that about 5 months after the grants arrived in treatment schools, the school committees have not yet budgeted any use of the remaining three-quarter grant. Together with the types of spending induced by the grant, our results suggest that the school budget constraint is not immediately binding: a large part of the grant is still unused, and some money is spent on leisure and agricultural spendings which seem non-essential for pure educational purposes. Also, the amount budgeted for teacher support is unchanged (the average amount in the treatment schools is even lower than in the control schools, although the difference is not significant), which is striking in a context where teachers suffer from long delays in the payment of their salary. Similarly, it seems surprising that the grant did not change the amount of money spent on supplies and textbooks, pupil educational support like remedial courses, or pupil health expenses, in a context where school equipment is very poor and pupils do not perform well at the primary school final exam. Overall, the impact of the grant on school expenses suggest that in the context of Niger, parents might not have sufficient information to make investments that are likely to improve school quality. Other explanations, which may be simultaneously be true, is that parents were saving the grant in the face of uncertain future cash flows (see Sabarwal, et al, 2014), that they were saving money in order to offset fees in the following year, or that they were saving money for lumpy investments

#### **4.1.3 School Quality**

We observe improvements only for infrastructure and health resources, alongside small increases in participation at the lowest grades. We find no improvement on materials, nor on teacher effort (on the contrary, there is a small decrease in teacher attendance. There is no evidence that test scores increased in response to the program.

In the slightly longer term (one year after the treatment) there is a small improvement in the infrastructure index of schools: a 0.04 standard deviation increase in the index for infrastructure

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<sup>12</sup>We would urge that future researchers examining local school management and activities collect data on school festivals, as well as school business investments, as potential targets of school spending. These expenditures were not foreseen and so detailed questions on these expenditures (for example, the number and type of school festivals, or the anticipated return of investment projects) were not included in the questionnaire, nor were questions about the local credit market.

quality (Table 3, column 7), significant at the 10% level. This is largely driven by increases in the number of classrooms and the construction of walls around the compound (columns 1 and 5 of Table A8).<sup>13</sup> The increase in the number of new classrooms amounts to 0.12 of a standard deviation, representing an additional 0.08 new classrooms per school in the treatment group over 0.28 new classrooms per school in the control group (a 29% increase). The increase in the proportion of schools with walls around the compound (enclosure) amounts to 0.18 of a standard deviation, with 9 percentage points more in the treatment group over 34% in the control group (a 26% increase). We find no overall impact on the materials available at the schools (books and classroom materials such as rulers, protractors and maps) (Table 3, column 8 and Table A9). There is a small (0.05 standard deviations) increase in the index of health resources (Table 3, column 9), significant at the 10% level. This increase is driven by increases in health information sessions (34% vs. 30% of schools), first aid kits (12% vs. 9% of schools), micronutrient supplementation (25% vs. 22% of schools), and deworming (64% vs. 62% of schools), though none of the individual components of the health index are significant alone (Table A10).

We find no effect of the grant of the number of days when class was cancelled because teachers were on strike, nor the opinion of the school committee on teacher assiduousness and punctuality, but we observe a decrease in teacher presence in the treatment group: around 4 percentage points less than the average of 76% presence in the control group, significant at the 10% level (Table 5). Teachers thus responded to increased resources under the control of parents with a reduction in their own inputs. Informal feedback from the field suggested that those teachers who felt the central government should make education decisions disliked that the communities were in charge of the grant, and they may have felt resentful that the grants undermined their authority (as representatives of the central government). In addition, the decreased teacher presence might also be related to the fact that the average school committee did not spend the grant on expenses supporting the teachers (teacher housing, furniture, supplies, guide books, and salary), even though school committees had not spent all of the grant at the time of the survey. As a consequence, teachers might have had the impression that parents were not capable of wisely investing the money allocated to them, and might have been resentful. Any such resentfulness might have been exacerbated by the ongoing pay disputes between the teachers and the government at that time (in many cases,

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<sup>13</sup>These items were also projects that were frequently reported by the schools as projects undertaken using the grant money.

teachers salaries had been substantially delayed or teachers had not been paid).

There is no change in enrollment or dropout overall (Table 7), but there is a positive impact at the lowest grade levels. The grant program reduced dropouts from grade 1 at the end of the 2007/2008 school year (2% vs. 3% in the control schools) (column 4 of Table 7b), a finding which is matched by an increase in enrollment in grade 2 at the beginning of the 2008/2009 school year (33 vs. 30 pupils in the controls schools) (column 5 of Table 7A).

The fact that participation increases only for the youngest pupils suggests that participation is more elastic when the child is young. This might be because the opportunity cost of time is higher for older children.<sup>14</sup>

We find no impact on the number of candidates presented for the end of primary school exam at the end of the 2007/2008 school year, the pass rate for the end of primary school exam, or any of the math, french or oral tests administered during the April/May 2008 questionnaire visit (Table 6). Since participation increased (or fewer children dropped out) in the lowest grades, we cannot rule out a downward bias due to attrition. However, the fact that test scores remained unchanged in the higher grades where there was no change in participation supports the finding that there was no improvement in learning.

## 4.2 Heterogeneous Treatment Effects

We now examine the different dimensions identified above to identify heterogeneous effects. Due to space limitations, we do not present the detailed regression tables in the paper, but they are available from the authors upon request. There are two overall messages from this analysis. The first is that the most difficult management task - monitoring teachers - was undertaken only by educated COGES or those with experience in other organizations, that is, those with higher capacity. The second is that, in one-teacher schools, there was a greater threat of teacher strikes, more of the grant was spent on items that benefited the teachers in some way, and, perhaps as a consequence of spending on items that benefited teachers, teacher presence increased slightly.

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<sup>14</sup>We also take the fact that only younger grades were impacted as evidence that the change in enrollment is not due to intentional misreporting by grant schools. In addition, the finding is replicated across two different types of data collections and at two different periods.

## Education of the COGES

Communities where the school committees were educated increased their supervision of teacher attendance in response to the grant. Educated school committees are 9 percentage points more likely to supervise teacher presence if the school was treated, significant at the 10% level. However, the increased monitoring did not reduce teacher absenteeism, suggesting that parents were not able to effectively confront teachers.

In terms of spending, educated COGES who received grants focused investments on infrastructure, perhaps to the detriment of other types of spending.<sup>15</sup>

COGES without educated members, on the other hand, increased spending on Health Resources and Pupil Educational Support.<sup>16</sup> The negative impact of the grant on money for Pupil Educational Support and the health resources index might reflect that educated COGES increased expenses in infrastructure, which are generally lumpy investments, and might have required the school to spend less on other items.

There is also a negative impact of the grant on math and french test scores in schools with educated COGES (about one-third of a standard deviation, significant at the 5% level for math and 10% level for french). This negative impact of the grant on learning in schools with educated COGES, who focused spending on infrastructure, echoes the findings in the literature that providing more-of-the-same educational inputs typically has no impact on learning, whereas interventions such as remedial education and rewards are more effective at increasing learning (Kremer et al, 2013). Educated COGES may not have made the optimal choice, because they decreased spending on pupil educational support, perhaps to finance the lumpy infrastructure investments.

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<sup>15</sup>Note that while educated COGES budgeted more money for infrastructure (58,755 FCFA (117 USD), significant at the 5% level), the increases in infrastructure in the following year were felt primarily in schools with non-educated COGES: the coefficient on the interaction term of treatment and education is negative (-0.08 SD) and significant at the 5% level. One possible reason, if the data on spending is accurate, is that the projects undertaken by educated COGES in response to the grant might have been larger and taken more time, so that they were not yet completed at the time that data on infrastructure was collected.

<sup>16</sup>For Health Resources, the treatment coefficient for the non-educated COGES is 0.06 SD, significant at the 10% level, while the coefficient for the interaction term is -0.12, significant at the 10% level, suggesting zero or negative impact of the grants on health resources in the educated COGES. For Pupil Educational Support, schools with non-educated COGES increased spending (3,639 FCFA (7 USD), significant at the 5% level), but no impact (or a possibly negative impact) for schools with educated COGES (the coefficient on the interaction term is -8,215 FCFA (16 USD), significant at the 5% level).

## Experienced COGES

Schools where the COGES has at least one member who is also a member in another community organization increased monitoring of teacher attendance in response to the grant. These schools are also those that enjoyed the increases in the cooperation index, whereas schools with no member that is also a member of another community organization had no increases.<sup>17</sup>

## Wealth of the COGES

We find that parent responsibility increased more in wealthy communities.<sup>18</sup>

We note that the results for wealth are different from the results for educated and experienced COGES, showing that the effects we find for education and experience are not merely proxies for wealth.

## One-Teacher Schools

One-teacher schools seem to have made a different choice than larger schools, with important effects: they budgeted more money for expenses related to Teacher Support.<sup>19</sup> This may be because there was more threat of striking from the teachers: one teacher schools in the treatment group lost 1.3 days more to teacher strikes than one teacher schools in the control schools (significant at the 10% level).

Perhaps as a result, one-teacher schools are the only schools to not suffer from the negative impact of the grants on teacher attendance on the day of the visit.<sup>20</sup> In fact, the size of the coefficient on the interaction term suggests that teacher attendance actually increased in one-teacher schools. This suggests that by transferring some of the grant to teachers - or at least to investments that benefit teachers - the one teacher schools limited the reduced teacher attendance associated with the grant in other schools.

At the same time, infrastructure in one-teacher schools did not improve, in contrast to other schools.<sup>21</sup>

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<sup>17</sup>For monitoring teacher attendance, the coefficient on the interaction term is 0.11, significant at the 5% level, and for cooperation, the coefficient is 0.07, significant at the 10% level.

<sup>18</sup>Each standard deviation increase in wealth is associated with an additional 0.05 standard deviation increase in the parent responsibility index in response to the grant, significant at the 5% level

<sup>19</sup>The coefficient on the interaction term is 8,985 FCFA (18 USD), significant at the 5% level.

<sup>20</sup>The coefficient on the interaction term is 0.17, significant at the 5% level, and the coefficient on the treatment term is -0.06, significant at the 5% level.

<sup>21</sup>Infrastructure may have even degraded - the coefficient on the interaction term is -0.17, significant at the 1% level, while coefficient on the treatment variable is 0.06, significant at the 5% level. Note that since the grant was

## Urban vs Rural Schools

Increases in in-kind contributions came from parents in urban schools.<sup>22</sup> The increase in the parent responsibility index is also driven by increases in urban rather than rural schools.<sup>23</sup>

Only schools located in rural areas increased their spending on agricultural investments. Urban schools did not increase spending on agricultural investments.<sup>24</sup> We speculate that this is because credit constraints may be less severe in urban areas, but we have no data to confirm this.

## 5 Conclusion

The short run impact of grants to school committees in Niger was to increase cooperation and participation along several dimensions without crowding out parent financial contributions. The implication of this finding is that one way to potentially avoid the crowding out due to increased inputs found in other experiments is to involve parents in the management of the funds. We also find that increased parent participation came with a small increase in young pupil participation.

However, more pessimistically, while the parents were willing to try to improve quality by participating, they were not able to do so, at least in the short run. One possible reason for this is that, in this context, parents (the majority of whom did not go to school) do not have sufficient information to make investments that are likely to improve quality. In particular, most investments focused on buildings, rather than extra lessons or materials, and these investments did not translate into improved learning. We also find that, on average, teachers decreased their effort in response to the grant to the COGES. This finding reinforces other evidence in the literature of negative teacher reactions to participatory programs, and highlights the importance of taking this potential reaction into account in policy planning.

The heterogeneous impact analysis, while second-order, yields potentially helpful insights for understanding the impact of the program and considering future programs. The most difficult type of participation - monitoring teachers - was attempted only by educated or experienced school committees. This suggests that participation initiatives need to take the capacity and authority

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based on the size of the school, one-teacher schools received smaller grants. They may then have been pushed away from investment in infrastructure since the lump sum was not enough to start a project.

<sup>22</sup>Urban schools were 17% more likely to have made in kind contributions, significant at the 10% level.

<sup>23</sup>The coefficient on the interaction term is almost 0.3 standard deviations, significant at the 1% level, whereas the coefficient on treatment alone in the interaction specification is near zero.

<sup>24</sup>Rural areas increased spending on agricultural investments by 2,046 FCFA (4 USD), significant at the 1% level, and the interaction term for urban schools is -1,755 FCFA (3.5 USD), significant at the 5% level.

of the intended participators into account. In addition, we find that one-teacher schools, which invested in the teacher's working conditions and/or made some type of transfer to the teacher, actually increased teacher attendance. We take this as evidence that teachers' negative reaction to parent participation might be reversed when parents are "on the side" of the teachers. Finally, we find that rural school committees as well as non-educated school committees invested a small part of the grant in agriculture, perhaps because they did not prioritize education or because they invested the money in order to get more funds for the school in the future. We highlight this finding so that future programs might be aware of it and collect more data to understand what schools might do with grants and the role that education preferences and credit constraints play in those decisions.

We emphasize that these findings are from a evaluation that ended prematurely. As such, their generalizability is limited even as they do give us some insight into what may be the immediate barriers to a community's ability to effectively leverage grant programs.

There are four key policy implications of our findings. First, on some measures, participatory programs can be successful: parents increased their participation in school management in response to the grant without immediately reducing their contributions. Second, on the other hand, there is no reason to assume that parents will make wise spending and management decisions. Third, capacity matters for difficult tasks, as in this case the parents with education or experience were those able to supervise teacher attendance. Finally, teachers may respond to parent empowerment by reducing effort, and avoiding this may require ensuring that teachers also benefit in some way.

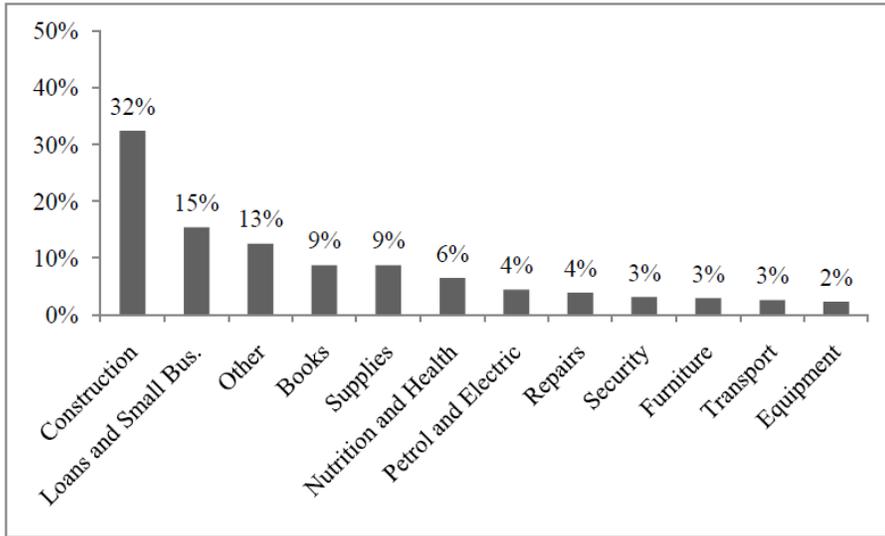
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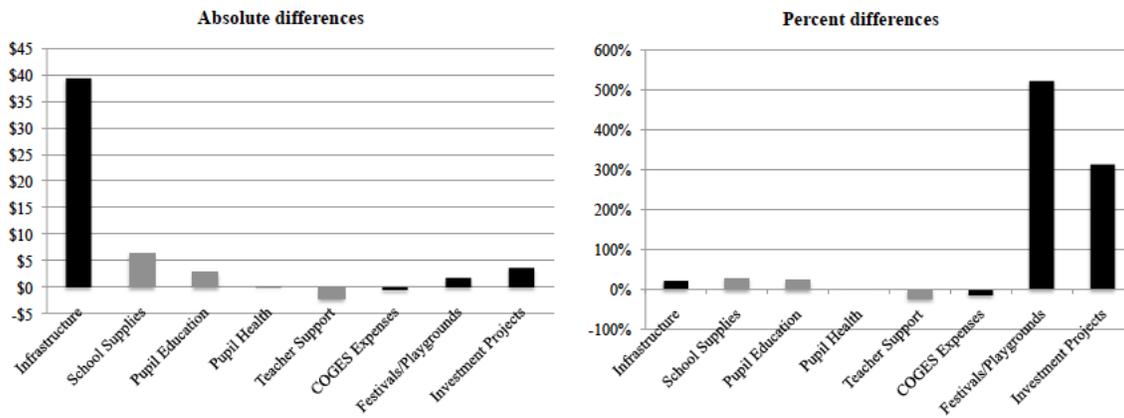
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Figure 1: Reported Use of Grant Money, by Total Amount Spent



Source: Financial Control over 85 randomly selected schools

Figure 2: Differences in spending between treatment and control groups



Source: World Bank Evaluation survey administered to control and treatment schools.

Table 1: Pre-program School Characteristics, by Treatment Group (Balance Check)

	(1)	(2)	(3)	(4)	(5)	(6)
	Control		Treatment		Difference	
	N	mean	N	mean	T-C	pvalue
<i>Pupil Characteristics</i>						
Enrollment 07/08	500	149,6	500	141,72	-7,88	0,28
% Girls in 07/08	500	0,38	500	0,38	-0,01	0,26
% Passed Exam in 07/08	262	0,45	224	0,42	-0,03	0,28
<i>Teacher Characteristics</i>						
Number of Teachers	490	3,87	494	3,55	-0,32	0,13
% of teachers civil servants	490	0,2	494	0,2	0	0,91
<i>Physical Infrastructure</i>						
Number of Buildings in 07/08	490	3,91	494	3,68	-0,23	0,17
Number of Latrines in 07/06	500	0,89	500	0,82	-0,08	0,55
Water Access in 06/07	500	0,09	500	0,11	0,01	0,53
Electricity in 06/07	500	0,01	500	0,02	0,01	0,22
<i>COGES Characteristics</i>						
COGES sponsored in 07/08	500	0,57	500	0,55	-0,01	0,70
COGES Exists in 06/07	500	0,88	500	0,9	0,02	0,32
<i>Location</i>						
Tahoua	500	0,52	500	0,51	-0,01	0,85
Distance to Inspection	500	41,1	500	38,59	-2,5	0,17
Distance to Health Center	476	8,24	461	8,95	0,7	0,61

*Data come from Ministry of Education Administrative Data. The data from 07/08 are reported in November (prior to the intervention) and are used when available, otherwise data from 06/07 is used. "Sponsored" COGES are those that have some sort of official sponsor or support group (such as an NGO).*

Table 2: Community Characteristics used for Heterogeneous Treatment Effect Analysis

	Control Obs.	Treatment Obs	p-value of difference in attrition	Control Mean	Treatment Mean	Difference in means (T-C)	p-value of difference in means
Educated COGES member	369	370	0.94	0.317	0.305	-0.01	0.73
Experienced COGES member	369	370	0.94	0.209	0.227	0.02	0.55
Average wealth of school committee (PCA)	329	320	0.89	1.146	1.154	0.01	0.42
One-Teacher School	499	497	0.34	0.122	0.145	0.02	0.24
Urban School	500	500		0.108	0.110	0.00	0.92

*Source: School survey conducted April-May 2008, except % of girls in 2007/08: 2007/08 administrative data.*

*Observations at school level. P-values are for tests of equality of the means across Treatment and Control.*

*Educated COGES member =1 if at least one member completed primary school. Experienced COGES member=1 if at least one member is also the member of another community organization.*

*The p-value of difference in attrition is calculated by creating a dummy variable equal to 1 if the data is missing for a particular school, and then calculating the p-value of the difference in this variable between groups.*

Table 3: Grant Impact on Participation, Management, and Quality Indices

	(1) Parent Contribution Index	(2) Parent Involvement Index	(3) Parent Responsibility Index	(4) Teacher Oversight Index	(5) Accountability Index
Treatment	-0.0117	0.0600*	0.0586*	0.0266	0.0127
	0.0490	0.0321	0.0353	0.0389	0.0351
Constant	-0.141	-0.00756	-0.0889	0.335**	-0.219*
	0.167	0.117	0.129	0.159	0.124
Observations	782	922	780	778	806
R-squared	0.056	0.059	0.051	0.110	0.124
Control Group					
Mean	-0.00709	-0.0355	-0.0191	0.00229	0.00325

	(6) Cooperation Index	(7) Infrastructure Index	(8) Materials Index	(9) Health Index	(10) Teacher Effort Index
Treatment	0.0661**	0.0414*	-0.0439	0.0469*	-0.0237
	0.0306	0.0236	0.0350	0.0270	0.0435
Constant	-0.220**	-0.454***	-0.402**	-0.396***	0.484***
	0.103	0.0936	0.171	0.114	0.158
Observations	777	978	826	933	784
R-squared	0.078	0.164	0.174	0.238	0.213
Control Group					
Mean	-0.00756	-2.98e-09	-0.00411	1.26e-08	-0.00712

*Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ . Regressions control for whether the school is in a rural or urban area, total enrollment in 07/08, proportion of girls in 07/08, whether the school had NGO support prior to the grant and inspection fixed effects. Details on the component variables and the impact of treatment on each component variable for each index are given in the Appendix.*

Table 4. Impact on Spending Decisions

	Dependent Variable: Amount of money Spent on....								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Infrastructur e and Equipment	Supplies and Textbooks	Pupil Educational Support	Pupil Health	Teacher Support	COGES Expenses	School Festivals and Playground	Investments in Agriculture	Total Amount
Treatment	21,586**	3,222	1,435	1,253	-1,086	32.14	864.8***	1,833***	28,512***
	9,121	1,981	1,369	2,154	1,331	300.6	285.5	658.5	9,993
Constant	-24,197	836.7	-763.1	-13,404*	1,489	524.5	-1,599**	-861.4	-34,994
	38,103	8,622	4,031	8,062	4,576	1,046	765.0	1,098	41,928
Observations	726	733	734	734	734	738	736	731	698
R-squared	0.127	0.156	0.087	0.051	0.019	0.039	0.039	0.047	0.157
Control Group Mean	86119	11631	6058	8711	4352	782.7	165.8	582.9	115898

*Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ . Regressions control for whether the school is in a rural or urban area, total enrollment in 07/08, proportion of girls in 07/08, whether the school had NGO support prior to the grant, and inspection fixed effects. Dependent variable is the amount in FCFA spent by COGES in the corresponding category of activities, as declared by the president of COGES in the April/May 2008 survey. Infrastructure and Equipment includes expenses related to classrooms, desks, chairs, blackboards, school enclosure and security, and cleaning. Supplies and Textbooks includes expenses for notebooks, pens, and textbooks. Pupil Educational Support includes expenses like additional courses, awareness campaigns to increase enrollment, and academic rewards. Pupil Health includes expenses related to nutrition and health like drinkable water, meals, latrines and drugs. Teacher support includes expenses benefitting to teachers like teacher housing, furniture, supplies, guide books, and salary. COGES Expenses includes expenses related to COGES meetings, contributions to "COGES communal" and inspector visits. Schools festivals and Playground includes expenses like graduation ceremonies, parties, and soccer balls. Investments in Agriculture includes fields, crops and livestock, unrelated to education activities.*

Table 5. Impact on Teacher Effort

	(1)	(2)	(3)	(4)
	Days on strike	Teacher is Present	COGES opinion of Teacher Effort	Teacher Effort Index
Treatment	-0.541	-0.0382*	-0.0220	-0.0237
	0.490	0.0227	0.0253	0.0435
Constant	-2.071	0.937***	3.656***	0.484***
	2.292	0.0738	0.0932	0.158
Observations	706	799	734	784
R-squared	0.127	0.248	0.134	0.213
Control Group Mean	4.592	0.760	3.617	-0.00712

*Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ . Regressions control for whether the school is in a rural or urban area, total enrollment in 07/08, proportion of girls in 07/08, whether the school had NGO support prior to the grant and inspection fixed effects. Days on strike is the number of days that the school was closed due to teachers striking in 2007/2008. Teacher is present is the school average of the dummy variable indicating 1 if a teacher is physically present at the day of visit (on a day when the school was supposed to be open). If the school was closed, all teachers were counted as absent. The Teacher Effort Index is the average of the z-scores of the variables in columns (1) to (3).*

Table 6. Impact on Test Scores

	(1)	(2)	(3)	(4)
	Oral	Math	French	End Primary Pass Rate
Treatment	-0.101	-0.0351	-0.0338	-0.0244
	0.0749	0.0588	0.0586	0.0227
Constant	-0.0252	-0.159	0.0648	0.525***
	0.261	0.209	0.221	0.0706
Observations	499	763	739	557
R-squared	0.200	0.200	0.251	0.177
Control Group Mean	0.00828	0.00545	0.0145	0.614

*Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ . Regressions control for whether the school is in a rural or urban area, total enrollment in 07/08, proportion of girls in 07/08, whether the school had NGO support prior to the grant and inspection fixed effects. Oral, Math and French test scores come from normalized test scores from the World Bank administered exam in the spring of 2008. Oral test scores were given only to pupils in grades 1 and 2. The End Primary Pass Rate is the percent of students from the school who passed the exam at the end of grade 6 at the end of 2008 (administrative data).*

Table 7. Impact on Dropout and Enrollment

A: Dependent Variable: Dropout as reported at school visit in Spring 2008									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Total	Total Girls	Total Boys	Grade 1	Grade 2	Grade 3	Grade 4	Grade 5	Grade 6
Treatment	-0.00559	-0.206	-0.00469	-0.0136*	-0.00646	-0.00791	-0.00778	0.00264	0.00139
	0.00520	0.212	0.00609	0.00758	0.0107	0.00582	0.0100	0.00849	0.00987
Constant	0.0723***	0.775	0.0908***	0.0366**	0.0613**	0.0678***	0.143**	0.115**	0.0891**
	0.0165	0.662	0.0224	0.0183	0.0291	0.0240	0.0570	0.0455	0.0384
Observations	748	754	753	531	434	525	454	381	466
R-squared	0.059	0.036	0.055	0.038	0.042	0.046	0.090	0.068	0.104
Control Group Mean	0.0359	0.366	0.0379	0.0296	0.0328	0.0295	0.0364	0.0313	0.0508
B: Dependent Variable: Enrollment as reported in 2008/09 Administrative Data									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Total	Total Girls	Total Boys	Grade 1	Grade 2	Grade 3	Grade 4	Grade 5	Grade 6
Treatment	1.366	0.505	0.862	-0.604	3.256**	-0.471	-0.541	0.366	-0.639
	2.445	1.254	1.654	1.502	1.376	1.174	1.190	1.019	0.962
Constant	37.56**	-21.01***	58.57***	34.47***	-1.052	5.214	1.546	-1.388	-1.225
	15.14	7.562	9.652	6.267	6.441	4.881	4.534	3.911	3.925
Observations	988	988	988	988	988	988	988	988	988
R-squared	0.901	0.880	0.866	0.470	0.545	0.546	0.484	0.520	0.540
Control Group Mean	160.3	65.70	94.63	40.09	29.95	23.87	26.22	20.98	19.22

Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ . Regressions control for whether the school is in a rural or urban area, total enrollment in 07/08, proportion of girls in 07/08, whether the school had NGO support prior to the grant and inspection fixed effects. Table 15A gives the impact of the treatment on dropout rates in the spring of 2008. Schools that do not have a particular grade level are missing. Note that some schools did not provide breakdowns by sex. Table 15B gives the impact of treatment on enrollment in the fall of 2008 (the academic year following the treatment). Schools that have zero enrollment at a given grade level (because they are missing a particular level) are counted as zeros.