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Election, Implementation, and Social Capital in School-Based Management: Field Experimental Evidence from a Developing Country

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Abstract

We investigate the effect of School Management Committees (COGES) on social capital formation in Burkina Faso adopting a hybrid evaluation method consisting of a randomized controlled trial combined with a large-scale incentivized artefactual field experiment on public goods. We find that the COGES project significantly increased social capital in the form of voluntary contributions to public goods, especially among community members that are connected vertically. Combining the experimental findings with observed real-world decisions suggests that community management projects have the potential to improve local cost recovery by increasing contributions to local public goods, potentially leading to better fiscal sustainability.

JEL Codes: C93, H41, I25

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I. Introduction

To achieve universal primary education in developing countries, a variety of policy interventions have been proposed on both the supply and demand sides. These have ranged from the expansion and improvement of school infrastructure to deworming students, information sharing, free school lunches and uniforms, and (un)conditional cash transfers (Kremer 2003; Miguel and Kremer 2004; Jensen 2010, Duflo and Kremer 2003; Banerjee and Duflo 2006; Duflo, Glennerster, and Kremer 2008; Glewwe 2002; Kazianga et al. 2016). School-Based Management (SBM), which is a decentralization of decision-making powers and budgetary control from the central government to the school, is also seen as another potential way to deliver effective educational services (Barrera-Osorio, Fasih, and Patrinos 2009; Westhorp et al. 2014; Mbiti, 2016). However, estimated policy effects of SBM are mixed; while some studies have found positive impacts (Barrera-Osorio et al. 2009; Gertler et al. 2006, 2007; Blimbo, Evans, and Lahire 2011; Bruns, Filmer, and Patrinos 2011; Pradhan et al. 2011; Duflo, Dupas, and Kremer 2015), whereas other report negligible effects (Banerjee et al. 2010; De Laat, Kremer, and Vermeersch 2008; Kremer and Holla 2009).

An important related issue in developing countries is the sustainability of the voluntary provision of local public goods. Although local public goods can be underprovided, governments can often correct this type of market failure. However, the failures of government in developing countries are also fairly common. To tackle this joint failure, international development strategies designed to deliver local public goods have, in the last few decades, shifted from top-down central government driven strategies to decentralization strategies under which budgets and decisions are delegated to local communities to sustainably provide their own public goods (Miguel and Kremer 2007). The hope is that bringing decision-making power and accountability closer to those who benefit will make the service delivery system more efficient, effective, and sustainable (Mansuri and Rao 2013) and improve quality (Bardhan 2002, 2004;

Bardhan and Mookherjee 2005). While the reasoning is compelling, evidence on the effectiveness of decentralization is only now beginning to emerge. This is partly due to the difficulty in implementing rigorous evaluations of the decentralization policies designed to facilitate the voluntary provision of public goods. In a set of small-scale interventions in Kenya, Kremer and Miguel (2007) found that a number of interventions, such as local cost-sharing and verbal commitments, were all ineffective, and concluded that it may be difficult for a one time infusion of external assistance to promote sustainable provision. However, it is still unanswered whether and in what contexts institutional interventions, such as delegating decision-making rights to communities, can be effective in delivering local public goods in developing countries.[1]

This paper aims at filling part of the gaps in the existing understanding by rigorously evaluating an SBM program in the elementary education sector of Burkina Faso called the *Comites de Gestion dans des Ecoles Primaires* (COGES) project. In COGES schools, school management committees, which included elected members from each community, set and implemented annual school plans. We adopt a hybrid evaluation method consisting of a randomized controlled trial of the COGES project itself combined with a large-scale artefactual field experiment on public good contributions with monetary rewards.

There are three novel aspects of our study. First, we provide the first evidence on an SBM program per se, as opposed to existing studies which investigate subcomponents of SBM programs (Pradhan et al. 2014; Barr et al. 2012; Beasley and Huillery 2012; Blimpo, Evans, and Lahire 2013). Because there are only a few rigorous evaluations of SBM in developing countries (Westhorp et al. 2014), we believe our paper makes an important contribution to the understanding of these programs. Second, while our evaluation focuses on the reduced-form causal impacts of COGES on social capital in the form of voluntary contribution to public goods (Anderson et al. 2004), we also use the timing of different

[1] A closely related literature on "community driven development" examines the effects of strengthening local institutions on measures of social trust and cohesion (Fearon et al. 2009; Casey, Glennerster and Miguel, 2012; Nguyen and Rieger, 2016). This literature also finds some similar issues of fade-out of impacts over time. Our intervention focuses specifically on school governance structures, which has echoes of the debate in the US on school decentralization.

components of the intervention and the panel structure of the experimental data to understand how different features of the program affected the formation of social capital. More specifically, we estimate differentiated treatment effect in each of the project phases; one impact for the effect of the COGES elections and another impact for the effect of the COGES project implementation. Third, according to our pre-analysis plan, we purposely conducted the public goods game experiment with different configurations of community members. This allows us to investigate how COGES affected different forms of social capital (bonding, bridging and linking) and provides insight into the mechanisms behind the impact.

To preview our results, we find that the COGES project increased social capital significantly. In the schools treated with the COGES project, the average amount of voluntary contributions to public goods increased by 8.0 to 10.2%. Most of the effect can be explained by the implementation of the COGES project. However, for groups composed of a school principal, a teacher, and parents, the average contribution increased by 11.0 to 17.2% from the implementation of the COGES project and by additional 12.7 to 24.1% from the democratic election of COGES members. This result implies that linking social capital, which vertically connects people who are in different power relationships (Szreter and Woolcock. 2004), was an important channel for the impact. A potential implication of these results is that community managed projects may enable local cost recovery as community members are more likely to contribute to public goods in the form of the school when they have a voice in the decision-making process. This could lead to better fiscal sustainability for schools with School-Based Management in place. Consistent with this idea, we find that COGES had positive impacts on real-world decisions related to public goods that we observe in the schools.

The remainder of this paper is organized as follows. Section II discusses the COGES project, the experiments, the data collection and the identification strategy. Section III gives our empirical results and Section IV concludes.

II. A COGES Project Experiment

II.A. Background

Burkina Faso lags behind much of the rest of the world in achieving universal primary education.[2] To address this deficiency, the government of Burkina Faso adopted a Poverty Reduction Strategy in 2000, which stated that one important goal was to "guarantee that the poor have access to basic social services." To achieve this goal in the education sector, the Ministry of Basic Education and Literacy (MEBA) drew up a Basic Education Ten-Year Development Plan (PDDEB), which was divided into Phase I (2000-2006) and Phase II (2007-2010).[3] In the second phase, strong emphasis was placed on improving both access to and the quality of basic education by decentralizing the education system. For example, a presidential decree in July 2007 mandated tuition-free primary and lower middle education. The government also adopted the Education Policy Law (*Lettre de Politique Educative*) in July 2008 that specified concrete strategies to achieve the MDGs in the education sector. During the ensuing decentralization, each district was divided into the lowest administrative levels for basic education or *Circonscription d'Education de Base* (CEB). Each CEB had an office staffed with inspectors to facilitate teacher training programs with each office overseeing approximately 13 to 14 elementary schools. In 2009, the government issued an

[2] The education system of Burkina Faso comprises three years of preschool, six years of primary, four years of lower secondary, and three years of upper secondary education, followed by tertiary education. Multi-grade classrooms are also common, especially in rural schools.

[3] The official acronyms are based on the French names, which we have translated into English.. MEBA refers to *Ministère de l'Enseignement de Base et de l'Alphabétisation* and PDDEB refers to *Plan Decennal de Développement de l'Education de Base*..

additional decree (2009-106) that delegated to each CEB the right to manage preschool infrastructure, basic education, and literacy programs.

Since these reforms, enrollment at public primary schools increased by 9.7% annually. However, the gap between boys' and girls' enrollment actually widened, especially in poorer regions. Furthermore, dropouts and grade repetitions are still major constraints to achieving universal completion of primary school in Burkina Faso. To tackle these problems, the government, with technical assistance from the Japan International Cooperation Agency, started the "School for All" project in 2008. More formally the project was called "Support for the Improvement of School Management through a Community Participation Project" to improve the quality of basic education in Burkina Faso. Hereafter, we call this project the "COGES project."[4]

II.B. The COGES Project

COGES involved setting up a school management committee in each primary school whose members had a central role in setting and implementing an annual school action plan. The idea was for COGES to use input and ideas from the local community about how to improve the schools and a distinctive feature used to facilitate this idea was that new COGES members would be democratically elected by secret ballot voting within the community. Although some COGES members had been previously appointed by government decree, the newly elected COGES members had important roles including the presidency of COGES as well as members in charge of community participation, girls' enrollment, monitoring, accounting, and auditing.[5]

[4] Officially, the COGES project is called PACOGES (*Projet d'Appui aux Comités de Gestion des Ecoles*).

[5] Previously appointed COGES members included the local mayor, the Presidents of the Parents' and Mothers' Associations, the school principal, as well as teacher, NGOs and union representatives. Parents' Associations (APE) and Mothers' Associations (AME) among parents of students have also existed as school councils in Burkina Faso since the 1960s but they had limited roles in actual school management.

After the elections, each COGES organized meetings in which any community members within the school district were eligible to participate. The agenda of the first meeting was to discuss problems facing the school and to make an action plan for the subsequent school year. Typical action plans included things like providing separate toilets for female students, constructing or repairing school facilities (e.g., classrooms, desks, and chairs), providing school lunch for students, arranging housing for teachers, and purchasing learning materials. After the action plan was proposed, a second community meeting was then held to discuss and approve the action plan. Because most schools could not expect external resources from the central government, COGES could also mobilize financial and non-financial resources within the community. Further meetings were held to monitor the ongoing action plan and then to evaluate the previous year's action plan. The same cycle was then repeated every year; at the beginning of the new school year COGES and the community members would make a new action plan, including a procedure to implement, monitor, and evaluate the action plan using their own resources.

II.C. RCT-Based Evaluation

To identify the causal effect of the COGES project, we conducted a randomized controlled trial (RCT) in the form of a randomized "roll-out" of the COGES project in all elementary schools in the Ganzourgou Province, Burkina Faso. Using a list of all schools in the province provided by MEBA, we first partitioned all 279 schools in the province into 30 strata: 10 educational districts (CEB) by 3 school types (public schools, private Islamic schools, and private Catholic schools). The strata are displayed in Table 1. Using random assignment within each stratum, 141 schools were assigned to be first-year COGES schools (treatment group) and 138 schools were assigned to be second-year COGES schools (control group). In the first-year COGES schools, the COGES project was offered during the 2009-10 academic year while the second-year COGES schools received a delayed offer of treatment of the COGES project during the

subsequent 2010-11 academic year. During data collection, we discovered that some schools either did not exist or had been closed. This reduced the number of the schools to 134 and 132 for the first-year and second-year COGES schools, respectively.

[Insert Table 1 Here]

We conducted detailed surveys of all the major stakeholders in the school: the school principal, a randomly selected teacher from each grade, five randomly selected students of each randomly selected teacher, and the household head of each of the five randomly selected students. The first-round baseline surveys were conducted in December 2009 and January 2010. The second-round endline surveys were conducted in January and February of 2011. For the artefactual field experiments described below, we first randomly selected subsets of the schools in first-year and second-year COGES schools. We then recruited participants within the schools belonging to different groups, such as COGES members, teachers, parents and community members. During the baseline field experiment in February 2010, there were 43 and 40 schools in the first-year and second-year COGES groups, respectively. For the endline experiments in November and December of 2010, we selected a random subset of the baseline schools. Among these schools, there were 21 first-year and 21 second-year COGES schools.[6]

Table 2 reports tests of pre-treatment balance in observables by treatment status. The results indicate that we cannot reject the null hypothesis of no mean differences in the pre-treatment covariates between the treatment and control groups.

[6] In our experiments, we have a larger number of schools covered during baseline (83 schools) than endline (42 schools). However, in baseline, the experimental protocol for the subgroup formation was only followed by a subset of the 83 baseline schools. We restrict our analysis to this subset, which gives us a smaller sample size. To check for a potential sample selection problem, we regressed an indicator for remaining in the sample on observed characteristics and found only age was statistically significant at a 1% level. Because of this, we control for age in all the regression analyses below.

[Insert Table 2 Here]

II.D. The Sequence of the COGES Project

[Insert Figure 1 Here]

To help facilitate the COGES election and the development and implementation of the action plan, several types of training were conducted for stakeholders. The sequence of training is described in Figure 1. First, in order to establish the system the school principals in the first-year COGES schools attended two days of training in January 2010 on how to organize community meetings and hold elections. After returning home, two community meetings were held in the same month; the first for sharing information about the upcoming COGES and the second for the election of COGES members. After the election, the school principals, the COGES president and accountant, and representatives from the municipal offices participated in two additional days of training on making an action plan including its implementation, monitoring and evaluation. These events were followed by actual implementation and monitoring of the school activities developed in the action plan. Because the project was designed as a randomized roll-out project, it provided the same sequence of training and elections for the second-year COGES schools starting from November 2010.

II.E. A Hybrid Experiment

We adopted a hybrid evaluation consisting of a randomized controlled trial combined with artefactual field experiment on the public goods game played by the school principal, teachers, parents, and elected COGES members. We conducted the baseline public goods games in February 2010 after the election of

the first-year COGES school.[7] The endline public goods games were conducted in November and December of 2010 after the COGES elections in the second-year COGES schools (Figure 1). The public goods game is a standard laboratory experiment used to measure voluntary cooperation among subjects (Levitt and Fehr 2004; Camerer and Fehr 2004; Cardenas and Carpenter 2008) and is regarded as a way to elicit a measure of social capital (Anderson et al. 2004).

In our public goods games, each participant was placed in a group containing N nonanonymous members and given an initial endowment, E. Each participant then had to decide on an amount Y_i of their endowment to secretly contribute to the public good. The contributions were then totaled, multiplied by a factor ρ (with $1<\rho<N$), and divided equally among the N group members. The group members did not observe the contributions of the other members but only the amount returned to them. The final payoff for each group member was given by:

$$\pi_i = (E - Y_i) + \frac{\rho}{N}\sum_{i=1}^{N} Y_i. \quad (1)$$

When $1<\rho<N$ note that $\partial\pi_i/\partial Y_i=-1+(\rho/N)<0$ so that $Y_i=0$ is a dominant strategy for each participant. Therefore, a pure-strategy Nash equilibrium is $Y_i=0$ for all i and any amount $Y_i > 0$ represents a deviation from the individually rational Nash equilibrium. Following the literature we interpret Y_i as a measure of participant i's social capital.

In our actual experiments, we designated groups of four members $N = 4$, an endowment $E = 500$ FCFA and set $\rho = 2$, so that the collected amount was doubled before dividing it.[8] We formed five different types

[7] One reason for holding the public goods game experiment after the COGES elections was that we needed to know who the elected COGES members were in order to specifically include them in the public goods game. A second reason for this timing is to separately identify different impacts of the COGES elections and the implementation of the COGES action plan (see the discussion below).

[8] On January 21, 2016, 1 US dollar was equivalent to 602 FCFA. FCFA refers to the *Franc Communauté Financière Africaine*, which is a currency backed by the French Treasury and used in Burkina Faso and many other West African Francophone countries. To understand the magnitude of these transfers note that the official minimum wage rate in Burkina Faso is 1,050 FCFA per day. However, it is common to set a daily wage rate at 300 to 500 FCFA in rural agricultural and urban

of groups; the fathers of students (Group 1), the mothers of the students (Group 2), either four men or four women from the community who did not send children to the school (Group 3), a group consisting of the school principal, one teacher, one father, and one mother (Group 4), and a group with the four elected COGES members (Group 5). [9] Each group played the public goods game twice with an immediate monetary reward after each round. The repeated play was to check whether, similar to existing experimental findings, public goods contributions would decline towards the free riding Nash equilibrium over time (Andreoni 1988).

Although typically interpreted as a measure of social capital or their propensity for voluntary cooperation (Anderson et al. 2004; Levitt and List 2005; Carmerer et al. 2009), public goods game contributions could be driven by altruism rather than by contributions to public goods per se. To separate potential effects of altruism, we follow Anderson et al. (1998) and use the results from a dictator game to control for the effects of altruism. To do so, we conducted a hypothetical dictator game (without monetary incentives) among our public goods game participants. Each participant was randomly matched with another group member from their experimental session and asked for a hypothetical transfer amount out of their endowment of 500 FCFA. The choice set for the transfer was {0, 100, 200, 300, 400, 500} FCFA. Because there is no self-interested reason for the sender to transfer money, a positive transfer is usually interpreted as a measure of altruism. However, other potential interpretations, such as self-image construction, are possible (Camerer and Fehr 2004; Levitt and List 2007).

service sectors. So keeping the entire transfer and contributing nothing would be the equivalent of approximately one day of work for many individuals in our sample. The average payout for the two round was 1600.581 FCFA (1st round: 784.2 FCFA, 2nd round: 812.7 FCFA) for the first-year COGES schools, and 1655.5 FCFA (1st round: 815.3 FCFA, 2nd round: 837.3 FCFA) for the second-year COGES schools.

[9] For Group 3, if the school id was an even number, we chose four women participants. Otherwise we chose four men participants.

II.F. Econometric Model

We estimate the impact of the COGES project on the level of social capital *Y* as measured by voluntary contributions made in the public goods game. Because the COGES project involved a particular sequence of interventions, experiments and data collections, the timing of events is important for interpreting what is being identified in the econometric model. Note that we first conducted the public goods experiments in February 2010 right after the COGES elections in the first-year COGES schools. The second public goods experiment was then conducted in November and December of 2010 after the COGES elections in the second-year COGES schools (Figure 1). Table 3 gives the summary statistics of the "before" and "after" data.

[Insert Table 3 Here]

[Insert Table 4 Here]

The data from the public goods games can be classified into four cases as shown in Table 4. Let D_s take a value of 1 if a school *s* is assigned the COGES project during the first-year and 0 otherwise. If we employ the "before" data collected in February 2010, the outcome difference between the first-year and second-year COGES schools, $\bar{Y}_b^{D=1} - \bar{Y}_b^{D=0}$, identifies the impact of the COGES election. This is because the election had occurred in the first-year COGES schools (treatment schools) but it had not yet occurred in the second-year COGES schools (control schools). The COGES project itself had also yet to be implemented in either the treatment or the control schools. We call this an "election effect", which is defined as the effect arising from the randomization of the democratic elections. With the "after" data from November and December of 2010, the outcome difference between the first-year and second-year COGES schools, $\bar{Y}_a^{D=1} - \bar{Y}_a^{D=0}$, identifies the impact of the implementation of the COGES action plan in

the first-year COGES schools. This is because the second-year schools had then been exposed to the election, while the first-year schools had been exposed to both the election and to the implementation of the school action plan. We call this the "implementation effect," which is defined as the accumulated impact of the COGES implementation net of the direct election effect.[10] The total impact of the COGES project can be estimated by summing the election and the implementation effects.

We use the following linear regression model to estimate the Intent to Treat (ITT) impact of the COGES project,

$$Y_{ist} = \alpha_t + \beta_t D_s + u_{ist}, \tag{2}$$

where Y_{ist} is the voluntary contribution to public goods for individual i in school s at time t either "before" ($t = b$) or "after" ($t = a$). Given that D_s was randomly assigned, when $t=b$, the treatment effect β_b identifies the effect generated by the election. Alternatively, when $t=a$, the treatment effect β_a identifies the effect generated by the implementation of the COGES project (Table 4). Note that the conventional difference-in-difference estimator captures the difference between these two effects.[11] We also show the estimation results with and without covariates because their inclusion can potentially help increase the precision of the estimates. Finally, because 8 of the 43 first-year COGES schools did not conduct COGES projects due to their slow project adoption speed (never-takers), and 3 of the 40 schools assigned to second-year COGES schools implemented COGES projects during the first-year (always-takers), we also estimate equation (2) using treatment assignment as an instrumental variable for actual COGES implementation,

[10] An additional possibility is that there is fade-out of the election effects in the first-year COGES schools. In this case, the impacts in the after data are estimating the difference between the implement effect in the first-year COGES schools and the election effects in the second-year COGES schools. In this situation the "after" data impact serves as a lower bound on the true implement effect because it nets out the (presumably nonnegative) election effect. However, in our empirical results the election effects are mostly zero in the first-year COGES schools, so if the second-year COGES schools also have a zero election effect then the impact on the "after" data identifies the implementation effect.

[11] $(\bar{Y}_a^{D=1} - \bar{Y}_b^{D=1}) - (\bar{Y}_a^{D=0} - \bar{Y}_b^{D=0}) = (\bar{Y}_a^{D=1} - \bar{Y}_a^{D=0}) - (\bar{Y}_b^{D=1} - \bar{Y}_b^{D=0}) = \beta_a - \beta_b$

which identifies a local average treatment effect (LATE) on the subpopulation of compliers (Imbens and Angrist, 1994).

III. Estimation Results

[Insert Table 5 Here]

Table 5 summarizes the estimation results for the election and implementation effects. In columns (1), (2) and (3), we estimated equation (2) using the "before" data, which identifies the election effect. In every case, the coefficient on the treatment variable, D_s, is insignificant. This indicates that the community-wide democratic election of COGES members did not increase social capital on average. However, when we estimate equation (2) using the "after" data to identify the implementation effect of COGES on social capital, the estimates are large, positive and statistically significant. These results are shown in columns (4), (5), and (6) in which each column adds additional controls. With the implementation of the COGES project, the average amount of voluntary contributions to public goods increased by 8.0 to 10.2%. Because each participant played the public goods game twice, we report the estimation results using the combined contributions from the two rounds of the game. In all specifications, the second round public goods game actually stimulated a significantly larger amount of voluntary contribution to the public goods than the first round.[12] Although there was a publicly announced fixed ending time of the game, this finding is not necessarily in conflict with theoretical possibilities such as learning about free-riding or voluntary contribution arising from an infinitely repeated game (Andreoni 1988). Our results may also be driven by social norms or other-regarding preferences such as altruism and trust. Indeed, in columns (3) and (6) of Table 5, we find that adding our measure of altruism, captured by the amount sent in the dictator game, is

[12] Estimating the models separately for the first and second round public goods game data gives the same overall pattern of results.

a strong predictor of contributions of public goods game contributions. However, the impact estimate of COGES changes very little compared to the baseline specification.

Table 6 shows the results of Intent-to-Treat (ITT) effects based on the reduced form equation, with the random assignment of COGES schools as an independent variable. While the point estimates are slightly smaller, the magnitudes of the ITT estimates are comparable to the LATE estimates.

[Insert Table 6 Here]

[Insert Table 7 Here]

To further understand the mechanisms behind the impact, we estimate separate treatment impacts for each of the five groups that played the public goods game. This was to explore whether the impact estimates come through bonding social capital (fathers' and mothers' groups), bridging social capital (community group) or linking social capital (parents, teacher and principal group). Table 7 shows these estimation results. In a result of particular interest, we find that the estimated treatment effect for group 4, composed of the school principal, a teacher, and two parents, is positive and statistically significant for all the specifications. The point estimates indicate that in this "vertically connected" group, the average contribution increased by 12.7 to 24.1% from the democratic election effect of COGES members and by an additional 11 to 17.2% through the implementation of the COGES project. [13]

These results indicate that the COGES project increased social capital extensively, especially the linking social capital of Szereter and Woolcock (2004), which they define as norms of respect and networks of trusting relationships between people who are interacting across explicit, formal, or institutionalized

[13] Another interesting result is that the estimated treatment effects for the group composed only of COGES members (group 5) are also positive, large and significant, indicating the elected COGES members show particularly strong contributions to public goods. However, we cannot distinguish the effects arising from selection of motivated COGES members or enhanced motivation through the implementation of the COGES project.

power or authority gradients in society. This finding is particularly interesting in the current context because democratic institutions precisely link individuals in such "vertical" relationships and give less powerful individuals a connection to individuals who explicitly make such decisions in a community.

To explore the linking social capital results further, we note that the impact estimate does not inform us whether the impact comes from a compositional effect, in which the groups comprise different types of people who would play the game differently no matter who they played with, or a relational effect in which the game, when played among such a mixed group, captures effects on relational capital. In an attempt to disentangle these two ideas, we examined heterogeneous treatment impacts for fathers, mothers, teachers, and school directors within group 4. These results are displayed in Table 8. The findings suggest that within group 4 fathers showed a disproportionately positive and significant election effect whereas mothers displayed a strong implementation effect. Although exploratory, these results suggest interesting impact heterogeneity both through composition and through how the treatment impacts members differently.

[Insert Table 8 Here]

A potential implication of these findings is that School-Based Management may improve cost recovery by increasing community members' willingness to contribute to local public goods. This could lead to better fiscal sustainability in schools in which local stakeholders have more control. The overall impact estimate of the COGES project is consistent with a preceding quasi-experimental study of the COGES pilot project in Burkina Faso (Sawada and Ishii 2012). That study used data from 248 public goods game participants in 7 COGES schools and 5 non-COGES schools in Oubritenga province, and found a 16.0 to 27.0% increase in voluntary contributions to public goods from the introduction of the COGES pilot project. The results are also consistent with four interventions in Indonesia by Pradhan et al. (2013), which

found that the democratic election of school management committee members was effective at raising awareness of the school committee, parental supports, and teacher efforts.

III.A. Real World Decisions

Although the public goods game experiment gives a measure that is comparable across studies, it is perhaps an artificial situation for participants from these rural communities. Therefore, we also checked the consistency of our public goods game results using real world decisions related to the schools from the school director and project records data.

From the school director data, we considered the following variables; tuition fee paid per year in FCFA, annual textbook fee per student or family, annual financial contributions to the school, whether the school provided school meals, the frequency of school meals per month, and the availability of functional toilets at the school. Unlike the public goods game data, the school director data have a true baseline collected prior to both the election and implementation of COGES so we use a canonical difference-in-differences model. However, the coefficient on the COGES indicator now represents the combined election and implementation effect because we can no longer utilized the timing of data collection to untangle the two effects. These estimation results are displayed in Table 9. The results show that COGES increased both the amount of tuition fee payments and the availability of school meals. For these two variables, the experimental results are consistent with real world behavior.

[Insert Table 9 Here]

[Insert Table 10 Here]

From the project records, we also considered as outcomes the number of school projects and the amount of the COGES activity budget. For these data we have a midline and an endline but not a baseline so we

adopt a variant of the difference-in-difference model. The estimation results are displayed in Table 10. The results show that COGES generated 3.3 school projects on average and also caused a large and statistically significant increase in the annual school budget of FCFA 108,500 (approximately 180 USD), an almost 400% increase in the school budget compared to second-year COGES schools. Given that all children attending school arguably benefit from improved school services, these results indicate that COGES improved contributions to public goods not only in the laboratory setting.

A final piece of supporting evidence comes from a complementary paper in our research group. Using the same RCT, Todo et al. (2016) find that the COGES project increased the use of *Tontine*, which are rotating savings and credit associations (ROSCAs) common in Burkina Faso. Because ROSCAs require social capital to self-select reliable participants and enable mutual monitoring (Zeller 1998; Armendáirz and Morduch 2010), the results are consistent with the idea that COGES generated real-world increases in social capital in a broader sense beyond both the laboratory and the school setting.

IV. Concluding Remarks

In Burkina Faso, market underdevelopment is a serious obstacle to economic development and the country has a very low ranking in terms of political rights and civil liberties (Freedom House 2009). In such an environment, it is invaluable to be able to evaluate precisely the impact of democratic policies and to understand the formation of social capital, which can correct both market and government failures (Hayami 2009; Mansuri and Rao 2013). To fulfill this aim, we investigated the role of COGES in facilitating voluntary contributions to public goods among community members in rural Burkina Faso. We adopted a hybrid evaluation method consisting of a randomized controlled trial of the COGES project combined with an artefactual field experiment on the public goods game. We found that the COGES project significantly increased social capital in form of voluntary contributions to public goods. Most of

the impact came through the COGES project implementation but for groups connected by linking social capital, the effects on public goods contributions existed for both the COGES project implementation and for the election of COGES members. Although long-term follow-up in these data is not possible because of the randomized roll-out, the experimental findings were supported by positive impacts on real world decisions in the schools. In addition, a companion paper showed that the COGES program in Burkina Faso increased student enrollment, decreased repetition, and decreased teacher absence (Kozuka et al. 2016), which suggests that public goods contributions through the schools paid immediate dividends for students and teachers. Taken together, these complementary findings suggest that such community management projects have the potential to improve local cost recovery by increasing the voluntary provision of public goods, leading to better fiscal sustainability.

These findings are important in identifying how promoting democratic structures and community participation in a country with otherwise weak governance can improve the local provision of services. But we can also derive broader implications regarding the role of the local community where market mechanisms for resource allocation are generally underdeveloped. Market failures are a particularly serious binding constraint for educational investments because of uncertainty, irreversibility, externalities, and long gestation periods. To correct such market failures, governments often provide other mechanisms to force people to adjust their resource allocations. However, the government itself can also fail, especially in developing countries, because politicians and bureaucrats instead pursue self-interested objectives. The community however is one such mechanism that can be used counteract government failures and which relies on social capital to help promote voluntary cooperation and the provision of local public goods. Social capital thus plays a critical role in correcting both market and government failures (Hayami 2009). In fact, the complementarity between the market and social capital is highlighted by the public goods game used in this paper. It is a canonical example of market failure where the behavior of self-interested

individuals leads to socially suboptimal outcomes. In actual experiments however, behavior rarely corresponds to the non-Pareto efficient Nash equilibrium and the extent to which voluntary contributions to public goods deviate from the socially inefficient Nash equilibrium towards the socially optimal outcome can measure exactly this complementarity between market mechanisms and community-based social capital. Our empirical results indicate that such a complementarity can be strengthened by an SBM project.

In future studies, the external validity of our findings should be carefully examined. Although our results are consistent with the COGES pilot study (Sawada and Ishii, 2012) and a study on Indonesia (Pradhan et al. 2013), further validation of these results in various contexts is important. Given that JICA has been supporting similar COGES projects in West Africa (Niger from 2004; Senegal from 2007; and Mali from 2008), careful investigation of the effectiveness of COGES projects in these countries can generate important evidence on SBM projects more generally.

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TABLE 1—NUMBER OF SCHOOLS

	Second-year COGES Schools (Control Group)				First-year COGES Schools (Treatment Group)			
CEB	Public	Private	Franco Arab	Total	Public	Private	Franco Arab	Total
Boudry I	14	0	3	17	14	0	2	16
Boudry II	11	0	7	18	12	0	8	20
Kogho	6	0	0	6	6	0	0	6
Meguet	11	0	0	11	11	0	1	12
Mogtedo	16	1	7	24	16	2	7	25
Salogo	7	0	0	7	6	0	1	7
Zam	13	0	3	16	14	1	3	18
Zorgho I	13	0	3	16	12	0	2	14
Zorgho II	7	1	0	8	7	0	1	8
Zoungou	7	0	2	9	8	0	3	11
Total	105	2	25	132	106	3	28	137

TABLE 2—TESTS OF PRE-TREATMENT BALANCE IN OBSERVABLES ACROSS INTERVENTION

(ONLY ONE INTERVENTION)

	The second-year COGES (control)		The first-year COGES (treatment)		*t*-statistics for the null hypothesis of the same mean
	Observations	Mean	Observations	Mean	
Panel A: All Sample					
Age (years)	321	40.277	302	38.877	1.223
Male %	321	0.533	302	0.543	-0.258
Years of schooling	321	2.109	302	2.232	-0.382
Director %	321	0.044	302	0.046	-0.165
Teacher %	321	0.053	302	0.053	-0.001
Mothers' Associations					
AME %	321	0.031	302	0.033	-0.138
Parents' Associations					
APE %	321	0.047	302	0.053	-0.358
Mobile phone %	321	0.327	302	0.275	1.420
Dictator game contribution					
(FCFA)	321	2.637	302	2.631	0.075
Panel B: Only for the Schools in both 2009 and 2010					
Age (years)	185	40.357	202	38.485	1.326
Male %	185	0.546	202	0.545	0.027
Years of schooling	185	1.886	202	2.450	-1.327
Director %	185	0.049	202	0.059	-0.466
Teacher %	185	0.059	202	0.064	-0.199
AME %	185	0.027	202	0.035	-0.431
APE %	185	0.032	202	0.050	-0.841
Mobile phone %	185	0.341	202	0.267	1.567
Dictator game contribution					
(FCFA)	185	2.514	202	2.599	-0.802

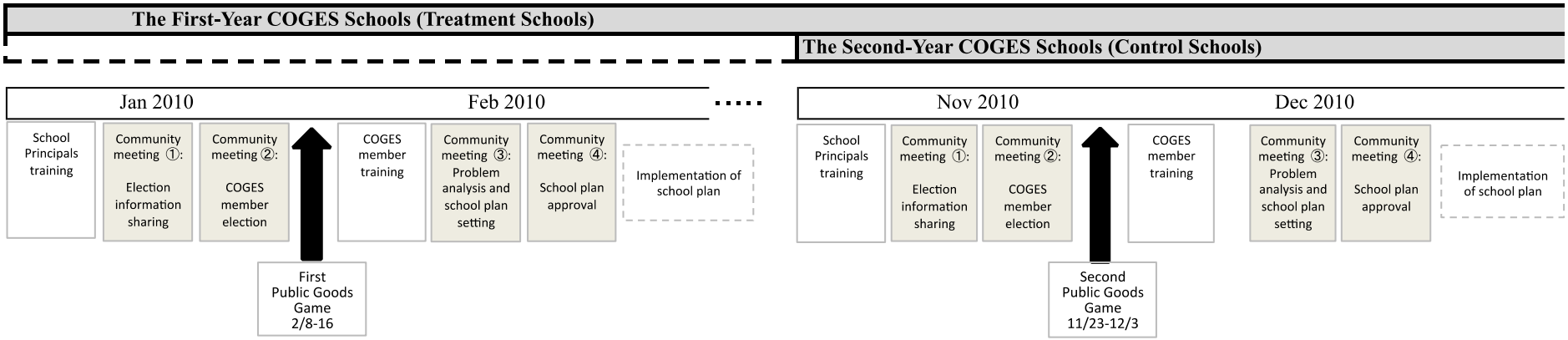

FIGURE 1. THE SEQUENCE OF EVENTS

TABLE 3—SUMMARY STATISTICS

	"Before" data			"After" data		
	Count	Mean	S.D.	Count	Mean	S.D.
1st round contribution (FCFA)	623	276.244	122.857	819	340.781	133.954
2nd round contribution (FCFA)	623	306.742	136.750	819	353.846	138.695
COGES implementation % (*D*)	623	0.485	0.500	819	0.495	0.500
COGES random assignment %	623	0.453	0.498	819	0.470	0.499
Age (years)	623	39.599	14.284	819	41.161	13.020
Male %	623	0.538	0.499	819	0.559	0.497
Years of schooling	623	2.169	4.003	819	2.446	4.420
Director %	623	0.045	0.207	819	0.051	0.221
Teacher %	623	0.053	0.224	819	0.050	0.218
Mothers' Associations (AME) %	623	0.032	0.176	819	0.049	0.216
Parents' Associations (APE) %	623	0.050	0.218	819	0.050	0.218
Dictator game	623	2.634	1.038	819	2.945	1.192
Group 1 % (father group)	623	0.159	0.366	819	0.203	0.402
Group 2 % (mother group)	623	0.162	0.369	819	0.200	0.400
Group 3 % (community group)	623	0.482	0.500	819	0.201	0.401
Group 4 % (mixed group)	623	0.197	0.398	819	0.203	0.402
Group 5 % (COGES members)	623	0	0	819	0.193	0.395

TABLE 4—CLASSIFICATION OF OBSERVATIONS

	Before (February 2010)	After (November/December 2010)
First-year COGES Schools	$Y_b^{D=1}$	$Y_a^{D=1}$
Second-year COGES Schools	$Y_b^{D=0}$	$Y_a^{D=0}$

TABLE 5—ESTIMATED COGES ELECTION AND IMPLEMENTATION EFFECTS (LOCAL AVERAGE TREATMENT EFFECT)

	"Before" data (Election Effect)			"After" data (Implementation Effect)		
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Method	IV	IV	IV	IV	IV	IV
Strata FE	YES	YES	YES	YES	YES	YES
Control	NO	YES	YES	NO	YES	YES
D^+ (COGES dummy)	12.03	11.60	12.03	34.01**	33.61**	26.50**
	(15.84)	(15.01)	(13.51)	(16.07)	(15.64)	(13.25)
Group 2 dummy (mother group)		-19.82	-19.72		18.09	6.971
		(27.11)	(25.88)		(26.15)	(23.17)
Group 3 dummy (community group)		-6.943	-13.30		14.57	5.058
		(19.17)	(18.38)		(22.72)	(20.01)
Group 4 dummy (mixed group)		44.27*	28.32		39.53*	32.23
		(23.35)	(21.55)		(22.62)	(20.65)
Group 5 dummy (COGES members)					35.18	30.40
					(21.54)	(19.46)
Amount sent in dictator game			36.15***			41.54***
			(4.303)			(3.835)
2nd round dummy	30.50***	30.50***	30.50***	13.06***	13.06***	13.06***
	(7.027)	(7.027)	(7.027)	(3.633)	(3.633)	(3.633)
Constant	282.6***	282.1***	192.7***	358.3***	354.4***	232.3***
	(26.22)	(37.27)	(39.14)	(20.57)	(33.91)	(33.67)
Kleibergen-Paap rk Wald F statistic for the first stage regression	252.643	271.052	272.977	561.679	570.686	574.236
Observations	1,246	1,246	1,246	1,638	1,638	1,638
R-squared	0.084	0.125	0.199	0.050	0.076	0.205

Notes: The dependent variable is the amount contributed in the public goods game with the initial stake of 500FCFA; Robust standard errors are in parentheses; All standard errors are clustered at the school-group level; + indicates an endogenous variable where the first-year COGES assignment indicator is used as an instrumental variable; Control variables are age, years of schooling, and dummy variables for male, private school, Islamic school, school director, teacher, AME member, and APE member; *** p<0.01, ** p<0.05, * p<0.1.

TABLE 6—ESTIMATED COGES ELECTION AND IMPLEMENTATION EFFECTS (INTENTION TO TREATMENT EFFECT)

	"Before" data (Election Effect)			"After" data (Implementation Effect)		
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Method	OLS	OLS	OLS	OLS	OLS	OLS
Strata FE	YES	YES	YES	YES	YES	YES
Control	NO	YES	YES	NO	YES	YES
D (COGES dummy)	9.605	9.322	9.674	28.91**	28.59**	22.54**
	(12.80)	(12.26)	(11.01)	(13.47)	(13.17)	(11.14)
Group 2 dummy (mother group)		-20.37	-20.30		19.11	7.771
		(27.65)	(26.34)		(25.74)	(22.85)
Group 3 dummy (community group)		-7.809	-14.24		15.28	5.612
		(19.68)	(18.80)		(22.16)	(19.52)
Group 4 dummy (mixed group)		43.89*	27.85		40.12*	32.69
		(23.87)	(22.04)		(22.26)	(20.41)
Group 5 dummy (COGES members)					35.87*	30.94
					(21.36)	(19.32)
Amount sent in dictator game			36.35***			41.56***
			(4.363)			(3.858)
2nd round dummy	30.50***	30.50***	30.50***	13.06***	13.06***	13.06***
	(7.112)	(7.141)	(7.144)	(3.656)	(3.669)	(3.670)
Constant	283.4***	283.6***	193.8***	360.7***	354.3***	232.1***
	(26.39)	(38.01)	(39.82)	(20.27)	(33.53)	(33.52)
Observations	1,246	1,246	1,246	1,638	1,638	1,638
R-squared	0.083	0.124	0.199	0.068	0.094	0.219

Notes: The dependent variable is the amount contributed in the public goods game with the initial stake of 500FCFA; Robust standard errors are in parentheses; All standard errors are clustered at the school-group level; Control variables are: age, years of schooling, and dummy variables for male, private school, Islamic school, school director, teacher, AME member, and APE member; *** p<0.01, ** p<0.05, * p<0.1.

TABLE 7—ESTIMATED COGES ELECTION AND IMPLEMENTATION EFFECTS

(WITH GROUP-SPECIFIC HETEROGENEOUS TREATMENT EFFECT)

	"Before" data (Election Effect)			"After" data (Implementation Effect)		
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Method	IV	IV	IV	IV	IV	IV
Strata FE	YES	YES	YES	YES	YES	YES
Control	NO	YES	YES	NO	YES	YES
D x Group 1^+ (fathers)	33.31	42.64	46.13	2.495	8.976	11.34
	(34.30)	(36.28)	(34.01)	(23.54)	(25.44)	(22.03)
D x Group 2^+ (mothers)	-14.66	-17.99	-7.720	27.97	34.38	21.10
	(30.85)	(32.96)	(31.37)	(28.49)	(30.43)	(24.47)
D x Group 3^+ (community)	-13.12	-8.117	-6.722	24.44	26.75	11.30
	(17.44)	(17.50)	(16.34)	(26.68)	(26.47)	(20.37)
D x Group 4^+ (mixed)	76.24***	54.74**	41.58**	62.47***	44.82*	40.37*
	(23.07)	(23.62)	(20.16)	(23.86)	(24.83)	(21.45)
D x Group 5^+ (COGES)				54.04*	54.43*	49.54**
				(28.95)	(28.97)	(25.23)
Amount sent in dictator game			36.08***			41.58***
			(4.455)			(3.868)
2nd round dummy	30.50***	30.50***	30.50***	13.06***	13.06***	13.06***
	(7.027)	(7.027)	(7.027)	(3.633)	(3.633)	(3.633)
Constant	284.9***	294.2***	195.1***	359.2***	377.6***	248.7***
	(25.70)	(30.81)	(33.03)	(20.98)	(28.25)	(28.80)
Kleibergen-Paap rk Wald F statistic for the first stage	38.91	37.667	37.682	111.016	113.541	113.996
Observations	1,246	1,246	1,246	1,638	1,638	1,638
R-squared	0.099	0.113	0.189	0.065	0.077	0.206

Notes: The dependent variable is the amount contributed in the public goods game with the initial stake of 500FCFA; Robust standard errors are in parentheses; All standard errors are clustered at the school-group level; + indicates an endogenous variable where the first-year COGES assignment indicators interacted with group indicators are used as instrumental variables; Control variables are: age, years of schooling, and dummy variables for male, private school, Islamic school, school director, teacher, AME member, and APE member; *** p<0.01, ** p<0.05, *p<0.1.

TABLE 8—ESTIMATED COGES ELECTION AND IMPLEMENTATION EFFECTS

(WITH GROUP-SPECIFIC HETEROGENEOUS TREATMENT EFFECT & LINKING SOCIAL CAPITAL)

	"Before" data (Election Effect)			"After" data (Implementation Effect)		
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Method	IV	IV	IV	IV	IV	IV
Strata FE	YES	YES	YES	YES	YES	YES
Control	NO	YES	YES	NO	YES	YES
D x Group 1^{+} (father)	-19.05	-20.53	-9.210	10.45	8.124	12.63
	(40.41)	(41.55)	(40.49)	(36.26)	(37.05)	(31.42)
D x Group 2^{+} (mother)	-72.86	-77.99*	-46.17	-56.72	-57.91	-61.46
	(46.71)	(46.71)	(44.19)	(40.41)	(39.76)	(38.71)
D x Group 3^{+} (community)	-14.68	-7.268	-5.921	24.46	27.32	11.67
	(17.40)	(17.63)	(16.42)	(26.68)	(26.59)	(20.47)
(Group 4 = mixed group)						
D x Group 4 x father^{+}	50.30*	63.45**	56.66*	-7.921	-2.279	-4.289
	(29.21)	(30.53)	(31.26)	(31.91)	(32.97)	(28.23)
D x Group 4 x mother^{+}	56.44	61.20	38.56	84.73**	95.82***	85.61**
	(38.29)	(38.53)	(35.45)	(33.22)	(33.42)	(35.32)
D x Group 4 x teacher^{+}	49.94	-43.95	-33.30	86.82***	46.93	38.82
	(35.12)	(44.94)	(42.45)	(28.57)	(39.40)	(34.37)
D x Group 4 x director^{+}	107.0***	66.25	47.61	92.54***	44.78	49.23
	(41.00)	(58.19)	(52.27)	(23.51)	(36.29)	(33.50)
D x Group 5^{+}				54.13*	53.34*	48.42*
				(28.97)	(29.08)	(25.37)
Amount sent in dictator game			35.62***			41.58***
			(4.482)			(3.868)
2nd round dummy	30.50***	30.50***	30.50***	13.06***	13.06***	13.06***
	(7.027)	(7.027)	(7.027)	(3.633)	(3.633)	(3.633)
Constant	286.3***	291.1***	194.7***	360.2***	375.1***	246.9***
	(25.93)	(30.59)	(32.48)	(21.21)	(28.60)	(29.00)
Kleibergen-Paap rk Wald F statistic for the first stage regression	22.00	5.486	5.40	69.22	14.774	14.629
Observations	1,246	1,246	1,246	1,638	1,638	1,638
R-squared	0.100	0.121	0.193	0.076	0.085	0.213

Notes: The dependent variable is the amount contributed in the public goods game with the initial stake of 500FCFA; Robust standard errors in parentheses; All standard errors are clustered at the school-group level; + indicates an endogenous variable where the first-year COGES assignment indicators interacted with group indicators are used as instrumental variables; Control variables are: age, years of schooling, and dummy variables for male, private school, Islamic school, school director, teacher, AME member, and APE member; *** p<0.01, ** p<0.05, * p<0.1.

TABLE 9—ESTIMATED IMPACTS OF COGES ON REAL WORLD DECISIONS

(DIFFERENCE-IN-DIFFERENCE ESTIMATION)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Tuition Fee	Textbook Fee	Financial Contribution (FCFA)	School Meal (dummy)	School Meal Frequency (per month)	Functional Toilet (dummy)
Strata FE	YES	YES	YES	YES	YES	YES
After x D^+	456.8*	-41.04	339.2	0.0807*	-0.708	0.0108
	(234.0)	(46.32)	(890.6)	(0.0466)	(1.190)	(0.0510)
D^+	-118.9	7.252	95.59	-0.0213	0.141	0.0667
	(162.6)	(14.02)	(624.1)	(0.0564)	(1.127)	(0.0487)
After	542.6	72.50	1,549	0.365***	0.264	0.0664**
	(477.8)	(71.85)	(1,600)	(0.0868)	(1.282)	(0.0304)
Constant	-305.5	-29.62	1,118	0.594***	19.10***	0.591***
	(211.5)	(22.15)	(917.9)	(0.0475)	(0.872)	(0.0294)
Kleibergen-Paap rk Wald F for the first stage regression	221.079	252.2	193.721	220.405	214.301	219.675
Observations	503	513	428	519	494	517
R-squared	0.521	0.135	0.180	0.283	0.085	0.401

Notes: Robust standard errors are in parentheses; + indicates an endogenous variable where the first-year COGES assignment indicator as well as the same variable interacted with an "after" indicator variable are used as instrumental variables; *** p<0.01, ** p<0.05, * p<0.1.

TABLE 10—ESTIMATED IMPACTS OF COGES ON REAL WORLD DECISIONS

(DIFFERENCE-IN-DIFFERENCE ESTIMATION)

	(1)	(2)	(4)	(5)
Sample	All	Before	All	Before
VARIABLES	Number of projects	Number of projects	Amount spent (FCFA)	Amount spent (FCFA)
(1 - *D*) x after	3.278***		108,571***	
	(0.252)		(14,037)	
(1 - *D*)	-3.273***		-110,039***	
	(0.225)		(13,955)	
After	0.0979		-136,287***	
	(0.154)		(12,695)	
Treat		3.273***		110,039***
		(0.225)		(13,962)
Constant	4.469***	1.196***	137,754***	27,716***
	(0.145)	(0.173)	(12,604)	(5,978)
Observations	1,361	469	1,361	469
R-squared	0.124	0.291	0.211	0.073

Notes: Robust standard errors are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.