JICA Research Institute

JICA-RI Working Paper

Impact Evaluation Analyses for the JICA Projects

Do Community-Managed Schools Facilitate Social Capital Accumulation? Evidence from the COGES Project in Burkina Faso

Yasuyuki Sawada and Takaharu Ishii

No. 42

March 2012

JICA Research Institute

JICA Research Institute

JICA Research Institute
10-5 Ichigaya Honmura-cho
Shinjuku-ku
Tokyo 162-8433, JAPAN
TEL: +81-3-3269-3374
FAX: +81-3-3269-2054

Do Community-Managed Schools Facilitate Social Capital Accumulation?

Evidence from the COGES Project in Burkina Faso

Yasuyuki Sawada* and Takaharu Ishii†

Abstract

In this paper, we investigate the role of a School Management Committee (COGES) in facilitating social capital among community members and teachers. We employ unique data from Burkina Faso, where the COGES project was recently introduced. To determine the individual level of social capital of each community member and teacher, we conduct public goods games, one of the standard artefactual field experiments, with monetary rewards. Using instrumental variable and propensity score matching methods, we obtain several findings. First, we find that the COGES project increases the level of social capital significantly. This finding is robust across different econometric specifications and methodologies. According to our point estimates, the amount of voluntary contribution to public goods increases by 16% to 27%. Second, the social capital facilitation effect of COGES varies based on the characteristics of the participant: while those who are more educated tended to have a lower level of social capital, Muslims have a higher level of social capital with COGES. Third, our qualitative results are maintained if we use the subjective assessment data of social capital based on the General Social Survey (GSS) questions.

Keywords: School Management Committee (COGES), community participation, social capital, Burkina Faso, field experiments, public goods game.

* Visiting Fellow, JICA Research Institute, and Professor, Faculty of Economics, University of Tokyo (Sawada.Yasuyuki@jica.go.jp)

† Graduate School of International Cooperation Studies, Kobe University

This paper has been prepared as a part of a research project of the Japan International Cooperation Agency Research Institute (JICA-RI) titled “Impact Evaluation Analyses for the JICA Projects” and led by Yasuyuki Sawada. We would like to thank our collaborators at Institut National de la Statistique et de la Démographie (INSD), JICA Burkina Faso office, JICA Research Institute, and Hiromi Asaoka for their valuable cooperation in implementing our survey and experiments and Yasuhiko Sato, Chikako Yamauchi, Robert Garlik and other the session participants of 2012 CSAE conference at Oxford University, and two anonymous referees of the JICA Working Paper version for their constructive comments. The usual disclaimers apply.

1. Introduction

Achieving universal primary education is identified as one of the most important development objectives in global campaigns such as the Millennium Development Goals (MDGs). Yet progress towards this objective has been slow in Sub-Saharan Africa and South Asia (UNDP, 2010). To facilitate child schooling, a broad array of policy interventions on both the supply and demand sides have been proposed and implemented (Kremer 2003; Duflo and Kremer 2003; Banerjee and Duflo 2006; Duflo, Glennerster, and Kremer 2008; Glewwe 2002). On the demand side, conditional cash transfers (CCT) have attracted wide attention as an effective policy tool to directly stimulate child schooling among poor families. The success of the Oportunidades (formaly Progresa) program in Mexico is well known among both academic researchers and policymakers (Schultz 1994; World Bank 2009; Behrman et al. 2009). As for supply side interventions, Hanushek (2003) provides a review of the US and international evidence on the effectiveness of increasing inputs and resources in schools. He found that input-driven policies are not systematically related to school quality improvements. Alternatively, a recent innovation in the provision of education is performance incentive policies through decentralizing responsibility from governmental agencies or departments to communities or schools (Jimenez and Sawada 1999, 2011; Sawada and Ragatz 2005; World Bank 2003; King and Orazem 1999; Vegas 2005; Vegas and Umansky 2005; Kartik 2010; Bruns, Filmer, and Patrinos 2011). Also, from the viewpoint of imperfect information theories, bringing decision-making power and accountability closer to an autonomous entity involving beneficiaries with better information will mitigate agency costs and make schools more efficient (Bardhan 2002, 2004; Bardhan and Mookherjee 2005; Bowles and Gintis 2002; Conning and Kevane 2002). In this respect, the role of community participation has attracted significant attention in recent research (Rai 2002; Stiglitz 2002).

However, existing empirical results on the role of community- or school-based management (SBM) in improving the quality of education are mixed (Kremer and Holla 2008; Beasley and Huillery 2012). Encompassing evidence from 20 countries in Central America, Africa, Asia, and Middle East, Barrera-Osorio et al. (2009), Gertler et al. (2006; 2007), Bruns, Filmer, and Patrinos (2011) found that decentralized schools had a positive impact on educational outcomes. Using the method of the randomized control trials (RCT) in Kenya, Duflo, Dupas, and Kremer (2009) found that community participation improves the quality of education. In contrast, using RCT, Banerjee et al. (2010) evaluate the impact of the current Indian government's flagship program on universal primary education through the participation of beneficiaries in the monitoring of public services. The program delegates powers over school resource allocation, along with the monitoring and management of school performance, to committees of locally elected leaders and the parents of children enrolled in public schools. They find that the role of these committees has been somewhat limited. Moreover, De Laat, Kremer, and Vermeersch (2008) use RCT in Kenya to evaluate the effectiveness of community involvement in education. They found that encouraging school committees to report on teacher performance to the district administration did little to improve absentee rates.

While existing empirical results are mixed, there are two remaining problems in these studies.[1] First, the existing papers rarely examine the direct impact of community participation on outcomes. Rather, indirect channels such as the impact of training to encourage beneficiaries' participation through committees are investigated (Banerjee et al. 2010). It would not necessarily be satisfactory to investigate the impact of such training programs to identify the causal impact of school decentralization. Second, the existing studies use student- or teacher-level observed outcomes as criteria to evaluate the impact of community participation. While the ultimate outcome variable of interest can be student learning outcomes, the mechanisms of the

1. Beasley and Huillery (2012) propose a theoretical model which explains some of those mixed results by predicting that returns to participation will vary by community characteristics.

impact of the policy may entail intermediate outcomes of interest in the short and medium terms.

In this paper, we aim to tackle these two remaining issues by evaluating the School Management Committee or Committees de Gestion Scolaires (hereafter COGES) project in Burkina Faso.[2] COGES is a committee in each primary school whose members are elected in a democratic manner from among the parents of the students and community members.[3] COGES has a central role in setting and executing an annual school activities plan. For the first remaining issue, we study a pilot project of initiating a large-scale COGES project in Burkina Faso, in which COGES have been established for the first time. Since the COGES project has not been randomly assigned in the pilot project, we will handle selection bias carefully when we estimate the program effects. With respect to the second remaining issue, we investigate the role of COGES in directly facilitating social capital among parents, teachers, and COGES management committee members. In areas where market incompleteness and underdevelopment are serious and where the legal enforcement framework is weak, social capital among community members can play an important role in mitigating a variety of agency and enforcement problems (Durlauf and Fafchamps 2005). To accurately determine the level of social capital, we employ the standard technique of artefactual field experiments—more specifically, the public goods game (Levitt and List 2007; Cardenas and Carpenter 2009; Camerer et al. 2004). We also use information from the dictator game to control for the effect of altruism (Cox 2004) and data from the General Social Survey (GSS) questions to check the robustness of our results.

We believe that our study will contribute in the following three aspects by performing a rigorous evaluation of the COGES project. First, it is very important to understand the process of social capital accumulation in a country like Burkina Faso, where market underdevel-

2. COGES programs have been implemented in several countries in Western Africa, including Niger, Senegal, Burkina Faso, and Mali. See Beasley and Huillery (2012) for evaluation of COGES in Niger.
3. The selection process of COGES members is democratic. COGES members are selected by votes of parents of all students and community members.

opment is serious obstacle to economic development. Second, in a developing country, multiple ethnic groups and religions frequently co-exist. It would be valuable to evaluate the heterogeneities in program effects across different types of schools in this kind of multi-cultural situation. Burkina Faso gives us a desirable situation because Franco-Arabic Muslim schools and non-religious schools coexist in the same area. Finally, it is important in practical terms to identify the key factors in promoting a democratization process in a country with weak governance. According to Freedom House (2009), Burkana Faso ranks among the bottom countries in terms of political rights and civil liberty. Hence, it will be valuable to evaluate precisely the impact of democratic policies on public behavior.

This paper is organized as follows. In Section 2, we overview the COGES project in Burkina Faso. Section 3 presents the evaluation strategy, and is followed by a description of the data in Section 4. Section 5 shows the empirical results and Section 6 provides concluding remarks with policy implications.

2. Education system and the COGES project in Burkina Faso

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.[4] In 2000, the government adopted the Poverty Reduction Strategy Papers (hereafter PRSP) and stated that one of the most important goals of the PRSP is to "guarantee that the poor have access to basic social services." To achieve this goal, Ministry of Basic Education and Literacy or Ministere de l'Enseignement de Base et de l'Alphabetisation (hereafter MEBA) draw up the Basic Education Ten-Year Development Plan or Plan decennal de developpement de l'education de base (hereafter PDDEB) from 2000 which comprised Phase I until 2006 and

4. According to Sano (2009), around 28% of classrooms are multi-grade classrooms.

Phase II from 2007 until 2010. In the latter phase, strong emphasis has been placed on improving the quality of basic education by decentralizing the education system.

During Phase II, the government issued a presidential decree in July 2007 to mandate tuition-free primary and lower middle education. The government also adopted the Education Policy Law (Lettre de politique educative) in July 2008 to specify concrete strategies to achieve the MDGs in the education sector. In the decentralization process, each district is divided into the lowest administrative levels for basic education or Circonscription d'education de base (hereafter CEB). Each CEB has an office, staffed with inspectors (inspecteur) to facilitate teacher training programs. In 2009, the government issued a decree (2009-106) to delegate the right to manage infrastructure in preschool, basic education, and literacy programs to CEB.[5]

Since the initiation of PDDEB, enrollment at public primary schools has increased 9.7% annually, but discrepancies between boys and girls are widening, especially in the poorer regions. Moreover, dropouts and grade repetitions are still major constraints to achieving universal completion of a full course of primary schooling. To tackle these problems, the government enacted a decree to initiate COGES in May 2008. In 2009, with technical assistance from Japan International Cooperation Agency, MEBA started the "School for All" project or, more formally, "Support for the Improvement of School Management through a Community Participation Project" to improve the quality of basic education in Burkina Faso.

COGES pilot project

The pilot component of the School for All Project was ran for seven months from November 2008 until June 2009 at 35 primary schools in Oubritenga province in the Plateau-

5. As a pilot program, MEBA issued a ministerial decree (2009-116/MEF/MATD/MEBA) to delegate a budget for building and rehabilitating classrooms and distributing stationary to 49 communes in the urban areas.

Central region. These schools for the pilot project comprise 20 schools from the Nagréongo department and 15 schools from the Ourgou Manéga department. MEBA and the JICA COGES project team undertook selection of the COGES pilot project using a two-step procedure. First, the pilot schools were selected from a list of all schools located in a rural area within one hour of Ouagadougou that do not receive significant support from other donors. In the second step, within Oubritenga province, the Nagréongo and Ourgou Manéga departments were selected as departments with average low and middle scores, respectively, according to a nationwide examination, the Primary Education Certificate examination or Certificat d'etudes primaires (CEP) in 2008. In these pilot COGES schools, COGES have been established by a democratic secret ballot of community members. In each school, elected COGES members have received intensive initial training by trained inspectors who are responsible for monitoring COGES activities. Then, an annual school action plan has been constructed by COGES members. On average, 4.9 activities were planned and implemented in each school—popular activities include the construction of toilets and introducing school lunch programs (Sano 2009).

3. Evaluation strategy

In this study, we estimate the Average Treatment Effects on the Treated (ATT) of the COGES project on the level of social capital *Y*. We define a binary treatment variable *D* that takes one if COGES is placed and zero otherwise. The level of social capital with and without COGES is denoted by Y^1 and Y^0, respectively. Our purpose is then to quantify the ATT, i.e., $E(Y^1\text{-}Y^0|D=1)$. We impose the ignorability assumption,, i.e., $(Y^1,Y^0) \perp D \mid X$ where *X* is a set of covariates. If we approximate ATT by a linear conditional expectation function, we can estimate ATT by the following linear regression model:

(1) $$Y_i = \alpha + \beta D_i + X_i\gamma + u_i,$$

where i stands for an individual and β represents ATT. We first estimate equation (1) by OLS. Then, to mitigate endogeneity bias arising from selection on unobservables, we use the instrumental variables method. We use the size and location information of each school in the previous year. Finally, we relax the linearity of the conditional expectation function by employing the propensity score matching method.

Artefactual field experiments

The outcome variable Y, the level of social capital, is elicited by the public goods game, which is one of the most popular artefactual experiments (Cardenas and Carpenter 2008; Levitt and List 2005; Carmerer et al. 2009). In this game, each anonymous participant is placed incommunicado in a group containing N members and given an initial endowment, E. Each participant has to decide the amount of Y_i, that is how much of this endowment to contribute to make public goods. The total contributions are then calculated and multiplied by a factor ρ where $1<\rho<N$ by the experimenter. The final contribution amount is divided equally among the group members. Hence the final payoff of each group member becomes:

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

Note that $\partial\pi_i/\partial Y_i=-1+(\rho/N)<0$ when $1<\rho<N$. Since the zero-contribution strategy, i.e., $Y_i=0$, is a dominant strategy, the Nash equilibrium is a situation where $Y_i=0$ for all i. Hence, the actual amount Y_i represents the deviation from the individually rational Nash equilibrium and we can interpret Y_i as a measure of a participant's propensity for voluntary cooperation. In our actual

experiments, we designated groups of four members, i.e., N=4, and an initial endowment of 500FCFA or 1,000FCFA, i. e., E=500FCFA or 1,000FCFA. We set ρ＝2 and doubled the collected amount.

The public goods game is the generalization of the prisoner's dilemma game in that N group members decide simultaneously how much to invest in the public good. Hence, the invested amount, which is the deviation from the Nash equilibrium, can be interpreted as social capital in the form of conditional reciprocity, i.e., reciprocated expected cooperation (Anderson et al. 2004; Levitt and List 2005; Carmerer et al. 2009). However, a voluntary contribution in the public goods game may be influenced by the degree of altruism rather than a voluntary contribution to public goods. To separate the effect of pure altruism, we follow Cox (2004) to use the results of the dictator game and control for the effects arising from altruism. The dictator game is conducted as a hypothetical question. Initially, each participant is randomly matched by another person randomly chosen. The participant is then asked for the amount of transfers without a repayment obligation out of the initial endowment of 500FCA from the list of possible transfers, {0, 100, 200, 300, 400, 500FCFA}.

To check the robustness of the results, we also employed General Social Survey (GSS) type survey questions. More specifically, we employ the answers to the following three GSS questions on fairness, help, and trust:

GSS Fair: Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair? 1=Would take advantage of you; 2=Would try to be fair

GSS Help: Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves? 1= Try to be helpful; 2= Look out for themselves

GSS Trust: Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people? 1= Most people can be trusted; 2= Can't be too careful.

A response to each question is converted into a binary variable representing the qualitative response of social capital. For example, the GSS fair measure of social capital takes one if the answer to the GSS Fair question is 1 and zero otherwise.

4. Data

In this paper, we use a unique data set collected by the Japan International Cooperation Agency (JICA) Research Institute in November 2009 and November 2010. As already noted, the pilot component of the School for All Project was run for seven months from November 2008 until June 2009 at 35 primary schools in Oubritenga province, in the Plateau-Central region. Our evaluation study, which includes a public goods experiment as its main component, was conducted at five COGES schools and four non-COGES schools in Oubritenga province in November 2009. In November 2010, we also conducted an additional study at two COGES schools and one non-COGES school in Ganzourgou province.[6] In our study, we have a total of seven COGES schools surveyed at different times, but each COGES school was studied after one year of COGES activities. Hence, our study is an evaluation study of the COGES project over one year. In our public goods experiments, we designated four participants as a unit of experiment. As subjects of the experiments, we invite mothers and fathers of randomly chosen students, teachers and all COGES executive members. In total, we conducted 62 groups of experiments with 248 participants in total.[7]

Table 1 shows the definitions and descriptive statistics of the variables used in our study. These statistics are shown separately for COGES and non-COGES schools with the two-sample t-test for differences in the means of the two groups. Since there are two amounts of initial endowment, we divided the contribution amount in the case of 1,000 FCFA by two to

6. The non-COGES schools which are similar to the pilot COGES schools in terms of the school size are chosen by the local education bureau.
7. Detailed instructions are given to the subjects before the experiments. The experiment protocol is available from the corresponding author upon request.

normalize the outcome variable. To capture the non-linearity arising from differences in the initial stake, we employ a dummy variable, coin10, which takes one for the case of an initial endowment of 1,000FCFA and zero for 500FCFA endowments. With respect to the basic characteristics of experiment participants, we include a dummy variable for sex, years of education, and age. For the school type, we employ a dummy variable for Muslim schools. As Cox (2004) argues, individual voluntary contribution amounts to public goods may be affected by altruistic behavior. We therefore include the results from the dictator game in hypothetical questions as an additional control variable. In our public goods experiments, some participants play the game multiple times. Since Andreoni (1988) and Croson (1996) found that the amount of contribution in the public goods game will change in step with the number of repeated plays, we added a variable, orderi, which shows the order of play for each person: For example, if a person is playing for the first time, the variable order1 takes one and zero otherwise. If he/she is playing for the second time, the variable order2 takes one; and zero otherwise. Also, since we have various group types, we constructed five group type dummy variables: membertype1, a default variable, is a dummy that takes one in the case of four fathers and zero otherwise; membertype2 takes one if all four members are mothers and zero otherwise; membertype3 is a dummy that takes one for two fathers and two mothers and zero otherwise; membertype4 is a dummy that takes one for two teachers including a school principal, one father, and one mother and zero otherwise; and membertype5 is a dummy that takes one for a group consisting of COGES members.

5. Empirical results

Basic results using OLS

Table 2 reports the results of estimating equation (1) using OLS. Specification (I) is a basic specification and (II) includes member type variables. Specifications (III) and (IV) in-

clude the results of the dictator game as an additional explanatory variable. In Table 2, the estimated coefficients for the COGES variables are all positive and statistically significant, which is consistent with the hypothesis that COGES facilitates social capital accumulation. With respect to the individual characteristics, while sex dummy (female) and years of education (education) are not statistically significant, the age variable (age) is negative and statistically significant in specifications (I), (III), and (IV). The dummy for the initial endowment of 1,000FCFA, coin10, is negative and statistically significant, suggesting non-linearity or decreasing marginal voluntary contribution to public goods. The Muslim school dummy (muslim) is negative and significant in specifications (I) and (II), which may reflect the unobserved systematic heterogeneity in the case of these schools. The estimated coefficients on the order of experiment variable (orderi) are positive and statistically significant for the third time and the fifth time. These findings are contrast quite sharply with those obtained by Andreoni (1988) and Croson (1994), who found in their laboratory experiments that as the public goods game is played repeatedly, contributions decreased toward the free-riding outcome.

In specifications (II) and (IV), the COGES member group variable (membertype5) shows a positive and statistically significant coefficient. This implies that the positive COGES effect shown by the positive COGES coefficient is even stronger among the very COGES committee members.

In the augmented specifications (III) and (IV), the coefficient on the dictator game variable (dictatorgame) is positive and significant, suggesting that the positive COGES effect may be explained at least partially by individual altruism. According to a comparison of specification (I) and (II), around 40 to 50 percent of the COGES effect may be attributed to altruism. Nonetheless, even with the dictator game variable, the statistical significance of the positive COGES coefficients remains, and thus it may be safely said that there is a positive COGES effect in facilitating social capital accumulation that cannot be explained by altruism.

Handling endogeneity problem using the instrumental variable method

Since COGES schools are not randomly assigned in the pilot project, the estimation results reported in Table 2 would involve an endogenity bias arising from the correlation between the COGES variable and the error term in equation (1). According to the pilot project document by Sano (2009), the COGES committees in Oubritenga province were placed in departments with relatively low average scores of CEP in 2008.[8] Also, the scale of a school affects the placement of COGES—schools with more students and teachers may be better able to accommodate COGES. Communities with a larger capacity may have a higher probability of receiving COGES schools. In fact, we find a significant difference in individual and school characteristics between COGES and non-COGES schools (Table 1). Hence, it will be important to handle a potential endogeneity bias in COGES placements before deriving conclusions based on Table 2.

The instrumental variable method is a natural choice to deal with the endogeneity bias. We employ the following four variables as instruments for the COGES placement variable: an interaction variable of the number of classrooms and the distance between the school and CEB office; the distance between the school and the nearest drinking water point; the distance between the school and the nearest health clinic; and a dummy variable that takes one if these pieces of information are missing. Basically, we use information on physical distance to capture the remoteness of each school as instruments. We also use the number of classrooms variable, which is based on 2008 data.[9] While a school with many classrooms may be better able to accommodate COGES to improve the quality of education and thus has a higher probability of COGES placement, school size in 2008 has been determined by historical factors other than COGES initiation.

8. In 2009, COGES schools in Ganzourgou province were placed randomly within each CEB by the evaluation project of JICA Research Institute.
9. If the 2008 data was missing, we used information in 2009.

Table 3 reports estimation results using the IV method. The coefficients of COGES are still all positive and significant and the other qualitative results are also maintained. The point estimate of the COGES variable falls into the range between 0.497 and 0.836. Since the average amount of contribution in non-COGES schools is around 3.116, we can say that the COGES project increases the voluntary contribution to public goods by 16% to 27%.

To check the validity of our identification strategy, we perform three tests: First, the joint F test of the excluded instruments; second, the test of weak instruments following Hahn and Hausman (2002) and the Wald test approach of Anderson and Rubin (1949); and finally, the over-identification restriction test of Sargan (1958). All of these test results support the validity of our IV estimates.

Handling the endogeneity problem using the propensity score matching method

As a part of robustness checking, we relax the assumption of the linear conditional expectation function of equation (1) and estimate ATT using the propensity score matching (PSM) method. To do this, we estimate the propensity score of COGES project treatment using individual and school level variables as covariates. More specifically, as individual level covariates, we include years of education and its squared variables, the missing dummy variable for years of education, age of the participant and its squared variables, and the missing variable of age. For the school level covariates, we use the number of teachers at each school. We also tried specifications with the distance between each school and CEB, but the variable did not pass the balancing test. Hence, we exclude the distance variable from the main estimation model of the propensity score. Then, at the second stage, we match each COGES observation with a non-COGES observation using three matching methods, i.e., one-to-one matching, caliper matching, and kernel matching methods.

In all matching methods, we impose a common support condition by dropping COGES observations with a propensity score higher than the maximum or less than the minimum propensity score of non-COGES observations. We also drop COGES observations with a propensity score lower than the non-COGES observation.

Also, to check whether the matching procedure balances the distribution of the covariates between COGES and non-COGES schools, we employ a two-sample *t*-test for differences in covariate means for the two groups. The results are presented in Table 4. We cannot reject the null hypothesis of the same means for all variables. We then follow Sianesi (2004) and rerun a probit model using the matched samples. If the matching is made properly, covariates are supposed to be statistically insignificant. According to the second block from the bottom of Table 4, with matched samples, we cannot reject the null hypothesis in which coefficients of all covariates are jointly zero. These test results support the validity of ATT estimated by PSM, which are reported in Table 4. Regardless of the different matching methods, ATT are positive and statistically significant based on the usual *t*-statistics. The estimated standard errors using the bootstrapping methods are 0.170, 0.152, and 0.176, respectively, in the case of one-to-one, caliper, and kernel matching methods with *p*-values of 0.457, 0.404, and 0.471. Yet, as Imbens (2004) stated, "[i]f one is interested in the average treatment effect for the sample, bootstrapping is clearly inappropriate." Hence our inference is based on conventional *t*-statistics. According to the results reported in Table 4, the COGES effect in terms of the voluntary contribution amount in public goods games ranges between 80FCFA and 102FCFA, equivalent to increases of 16% to 20.4%. In sum, the estimation results of our model indicate that COGES has a positive and statistically significant impact in facilitating social capital accumulation and that the magnitude is substantial.

Heterogeneities in the COGES effect[10]

Since COGES may affect different individuals and schools differently, we allow heterogeneities in the COGES effect by individual or school type. More specifically, we include the interaction variables of COGES, i.e., female×COGES, education×COGES, and Muslim×COGES. According to specification (I) and (II) with the female×COGES interaction variable, the social capital enhancement effect of COGES is smaller for women than that for men. In specification (III) and (IV), we include the interaction variable, education×COGES. Intriguingly, we find that a person with more years of education has a smaller social capital accumulation effect induced by COGES. In specification (V) and (VI), we include muslim×COGES and its coefficient is entirely positive and statistically significant. This means that the impact of a COGES project is larger in Muslim schools, which are often constructed in poor, remote areas. In the other specifications, the basic estimation results are maintained.

Experiments and GSS questions

To further check the robustness of our findings, we employ a subjective assessment of social capital based on the GSS-type trust, help, and fairness questions. We follow Glaeser et al. (2000) and Anderson et al. (2004) to test the consistency between experimental results and responses to subjective questions. We use the Tobit model to regress the contribution amount in the public goods game on the GSS dummy. In the results reported in Table 6, we cannot find a robust positive correlation between the public goods game results and GSS responses.

To control for other covariates, we use the GSS answer as the dependent variable and estimate equation (1), treating the COGES variable as an endogenous variable. According to the empirical results shown in Table 7, we find positive and statistically significant COGES

10. Note that our analysis is the case of heterogenous treatment effects without essential heterogeneity (Heckman, Urzua, and Vytlacil 2006).

effects and the results seem to be robust even if we include various controlling variables. Also, the qualitative results of other coefficients are largely the same as before.[11] We may conclude that our qualitative results regarding the effects of COGES are robust against the bias arising from endogeneity and specification error problems.

6. Concluding remarks

In this paper, we investigate the role of the School Management Committee (COGES) in facilitating social capital among community members and teachers. We employ unique data from Burkina Faso where the COGES project was recently introduced, with variations in the school-level availability of COGES. To elicit the individual level of social capital of each community member and teacher, the public goods game, one of the standard artefactual field experiments, was played with monetary rewards. Using instrumental variable and propensity score matching methods, we obtain three sets of findings. First, we found that the COGES project increases the level of social capital, with the finding is robust across different econometric specifications and methodologies. The amount of voluntary contribution to public goods increases by 16% to 27%. This suggests that the community management project will stimulate local cost recovery. Second, the social capital facilitation effect of COGES varies depending on the characteristics of the participant: while those who have more years of schooling tended to have a lower level of social capital, Muslims have a higher level of social capital. Since Muslim schools are located in comparatively poorer areas, our results may suggest that the social capital facilitation effect of COGES is greater in poorer communities. Finally, our qualitative results are maintained if we use the subjective assessment of social capital based on the General Social Survey (GSS) questions.

11. Carpenter et al.(2003) conducted the experiments first, followed by subjective GSS questions. In contract, Glaeser et al.(2000) conducted the GSS questions first. In our study, we follow Glaeser et al.(2000).

In future studies, three issues should be explored. First, it will be imperative to identify channels through which COGES can facilitate social capital accumulation. For example, we need to investigate each activity component of a COGES project closely. Barr et al. (2012) combined field and lab experiment in Ugandan primary school to investigate the channels of community-monitoring interventions. Such an approach will be promising.

Second, we need to design and implement a stricter empirical strategy to identify the causal impact of a COGES project. The method of randomized control trials (RCT) will be a natural choice to achieve this identification. In fact, an RCT-based evaluation study of the COGES project has been conducted in Burkina Faso (Sawada et al. 2012).

Third, the external validity of the COGES project in Burkina Faso should be shown explicitly. Since JICA has been supporting a series of COGES projects in West African countries such as Niger, Senegal, and Mali, comparisons of the estimated program effects can be a step towards external validation of a COGES project.

References

Anderson, Lisa R., Jennifer M. Mellor, Jeffrey Milyo. 2004. Social capital and contributions in a public-goods experiment. *American Economic Review* 94 (2): 373-76.

Anderson, T. W., and H. Rubin. 1949. Estimators of the parameters of a single equation in a complete set of stochastic equations. *Annals of Mathematical Statistics* 21: 570-82.

Andreoni, James. 1988. Why free ride? Strategies and learning in public goods experiments. *Journal of Public Economics* 37: 291-304.

Banerjee, Abhijit, and Esther Duflo. 2006. The economic lives of the poor. *Journal of Economic Perspectives* 21 (2): 141-67.

Banerjee, Abhijit, Rukmini Banerji, Esther Duflo, Rachel Glennerster, Stuti Khemani. 2010. Pitfalls of participatory programs: Evidence from a randomized evaluation in education in india. *American Economic Journal: Economic Policy* 2 (1): 1-30.

Bardhan, Pranab. 2002. Decentralization of governance and development. *Journal of Economic Perspectives*16 (4): 185-205.

Bardhan, Pranab. 2004. Review article: A powerful, but limited, theory of development. *Economic Development and Cultural Change* 52 (2) : 475-86.

Bardhan, Pranab, and Dilip Mookherjee. 2005. Decentralizing antipoverty program delivery in developing countries. *Journal of Public Economics* 89 (4): 675-704.

Barr, Abigail, Frederick Mugisha, Pieter Serneels, and Andrew Zeitlin. 2012. Information and collective action in the community monitoring of schools: Field and lab experimental evidence from Uganda. Paper presented at CSAE Conference 2012 at Oxford University in March 19.

Barrera-Osorio, Felipe, Tazeen Fasih, Harry Anthony Patrinos, and Lucrecia Santibanez. 2009. *Decentralized decision-making in schools: The theory and evidence on school-based management*. Washington, DC: World Bank.

Beasley, Elizabeth, and Elise Huillery. 2012. Do local communities allocate funds optimally? Evidence from a randomized experiment in Niger. Paper presented at CSAE Conference 2012 at Oxford University in March 20.

Behrman, Jere R., Susan W. Parker, Petra E. Todd. 2009. Schooling impacts of conditional cash transfers on young children: Evidence from Mexico. *Econ Dev Cult Change* 57 (3): 439–77.

Bhardhan, P., and C. Udry. 1999. *Development microeconomics*. Oxford University Press.

Bowles, S., and H. Gintis. 2002. Social capital and community governance. *Economic Journal* 112: F419-36.

Bruns, Barbara, Deon Filmer, and Harry Patrinos. 2011. *Making schools work: New evidence on accountability reforms*. World Bank.

Camerer, Colin F., and Ernst Fehr. 2004. Measuring social norms and preferences using experimental games: A guide for social scientists. In *Foundations of human sociality: Economic experiments and ethnographic evidence from fifteen small-scale societies*, ed. Joseph Henrich, Robert Boyd, Samuel Bowles, Colin Camerer, Ernst Fehr, and Herbert Gintis. Oxford University Press.

Cardenas, Juan Camilo, and Jeffrey Carpenter. 2008. Behavioural development economics: Lessons from field labs in the developing world. *Journal of Development Studies* 44 (3): 311-38.

Carpenter, Jeffrey P., Amrita G. Daniere, and Lois M. Takahashi. 2003. Cooperation, trust, and social capital in Southeast Asian urban slums. Working paper, Middlebury College.

Conning, Jonathan, and Michael Kevane. 2002. Community-based targeting mechanisms for social safety nets: A critical review. *World Development* 30 (3): 375-94.

Croson, Rachel T. A. 1996. Partners and strangers revisited. *Economics Letters* 53: 25-32.

Cox, J. C. 2004. How to identify trust and reciprocity. *Games and Economic Behavior* 46 (2): 260-81.

De Laat, Joost, Michael Kremer, and Christel Vermeersch. 2008. Teacher incentives and local partipation. Mimeo.
Duflo, Esther, Pascaline Dupas, and Michael Kremer. 2009. *Additional resources versus organizational changes in education: Experimental evidence from Kenya*. http://econ-www.mit.edu/files/4286.
Duflo, E., R. Glennerster, and M. Kremer. 2008. Using randomization in development economics research: A toolkit. *Handbook of development economics*. Elsevier.
Duflo, E., and M. Kremer. 2003. Use of randomization in the evaluation of development effectiveness. Paper prepared for the World Bank Operations Evaluation Department (OED) Conference on Evaluation and Development Effectiveness, Massachusetts Institute of Technology , Cambridge, Mass. in July 15-16.
Durlauf, Steven N., and Marcel Fafchamps. 2005. Social capital. In *Handbook of economic growth*, vol. 1B, eds. Aghion Philippeand and Steven Durlauf, 1639–99. Amsterdam: North-Holland.
Freedom House. 2009. Annual survey of freedom country scores 1972-73 to 2009-10. Database on-line available from Freedom House. Washington, DC: Freedom House.
Gertler, Paul, Harry Patrinos, and Marta Rubio-Bodina. 2006. Empowering parents to improve education: Evidence from rural Mexico. World Bank Policy Research Working Paper 3935.
———. 2007. *Impact evaluation for school-based management reform*. Washington, DC: World Bank.
Glaeser, Edward L., David I. Laibson, Jose A. Scheinkman, and Christine L. Soutter. 2000. Measuring trust. *Quarterly Journal of Economics* 115 (3): 811-46.
Glewwe. Paul. 2002. Schools and skills in developing countries: Education policies and socioeconomic outcomes. *Journal of Economic Literature* 40 (2): 436-82.
Girma, Sourafel, and Holger Görg. 2007. Evaluating the foreign ownership wage premium using a difference-in-difference matching approach. *Journal of International Economics* 72 (1) 97–112.
Hahn, Jinyoung, and Jerry Hausman. 2002. Notes on bias in estimators for simultaneous equation models. *Economics Letters* 75: 237-41.
Hahushek, Eric A. 2003. The failure of input-based schooling policies. *Economic Journal* 113: F64-F98.
Heckman, James J., Hidehiko Ichimura, and Petra Todd. 1997. Matching as an econometri evaluation estimator: Evidence from evaluating a job training programme. *Review of Economic Studies* 64 (4) 605–54.
Heckman, James J., Hidehiko Ichimura, Jeffrey A. Smith, and Petra Todd. 1998. Characterizing selection bias using experimental data. *Econometrica* 66 (5): 1017–98.
Heckman, James J., Sergio Urzua, and Edward Vytlacil. 2006. Understanding instrumental variables in models with essential heterogeneity. *Review of Economics and Statistics* 88 (3): 389–432.
Imbens, G. 2004. Nonparametric estimation of average treatment effects under exogeneity: A review. *Review of Economics and Statistics* 86 (1): 4-29.
Isham, J., D. Narayan, and L. Pritchett. 1995. Does participation improve performance? Establishing causality with subjective datam. *World Bank Economic Review* 9 (2): 175-200.
Jimenez, Emmanuel, and Yasuyuki Sawada. 1999. Do community-managed schools work? An evaluation of El Salvador's EDUCO program. *World Bank Economic Review* 13 (3): 415-41.
———. 2011. Does community management help keep children in schools? Evidence using panel data from El Salvador's EDUCO program. Mimeo.

Muralidharan, Karthik, and Venkatesh Sundararaman. 2010. The impact of diagnostic feedback to teachers on student learning: Experimental evidence from India. *Economic Journal* 120 (546): 187-203.
King, Elizabeth M., and Peter F. Orazem. 1999. Evaluating education reforms: Four cases in developing countries. *World Bank Econ Review*13 (3): 409-13.
Kremer, Michael. 2003. Randomized evaluations of educational programs in developing countries: Some lessons. *AEA papers and Proceedings* 93 (2): 102-06.
Kremer, Michael, and Holla, Alaka. 2008. Improving education in the developing world : What have we learned From randomized evaluations?" *Annual Review of Economics*1: 513-42.
Levitt, D.Steven., and John A. List. 2007. What do laboratory experiments measuring social preferences reveal about the real world? *Journal of Economic Perspectives* 21 (2): 153-74.
Rai, A.S. 2002. Targeting the poor using community information. *Journal of Development Economics* 69: 71-83.
Rosenbaum, Paul R., and Donald B. Rubin. 1983. The central role of the propensity score in observational studies for causal effects. *Biometrika* 70 (1): 41–55.
Rosenbaum, P. R., and D. B. Rubin. 1985. Constructing a control group using multivariate matched sampling methods that incorporate the propensity score. *American Statistician* 39 (1): 33–38.
Sano, Keiko. 2009. Completion report on the pilot COGES project in Bukina Faso. Japan International Cooperation Agency.
Sargan, J. D. 1958. The estimation of economic relationships using instrumental variables. *Econometrica* 26: 393-415.
Sawada, Yasuyuki, and Andrew B. Ragatz. 2005. Decentralization of education, teacher behavior and outcomes: The case of El Salvador's EDUCO program. In *Incentives to improve teaching: Lessons from Latin America*, ed. Emiliana Vegas, 255–306. Washington, DC: World Bank.
Sawada, Yasuyuki, Takeshi Aida, and Eiji Kozuka. 2012. Sustainability of community-managed schools: RCT-based evaluation of the school for all project in Burkina Faso. Mimeo. Japan International Cooperation Agency Research Institute.
Schultz, T. Paul. 2004. School subsidies for the poor: Evaluating the Mexican progresa poverty program. *Journal of Development Economics* 74: 199-259.
Sianesi, Barbara. 2004. An evaluation of the active labour market programmes in Sweden. *Review of Economics and Statistics* 86 (1): 133-55.
Smith, Jeffrey A., and Petra Todd. 2005. Does matching overcome LaLonde's critique of nonexperimental estimators? *Journal of Econometrics* 125 (1-2): 305–53.
Staiger, Douglas., and James. H. Stock. 2000. Instrumental variables regression with weak instruments. *Econometrica* 65 (3): 557-86.
Stiglitz, J. 2002. Patricipation and development: Perspectives from the comprehensive development paradigm. *Review of Development Economics* 6 (2): 163-82.
United Nations. 2010. *The Millennium Development Goals report 2010*. United Nations.
Vandenberghe, V., and S. Robin. 2003. Private, private government-dependent and public school: An international efficiency analysis using propensity score matching. *Public Economics*, no. 0308002, EconWPA.
Vegas, Emiliana. 2005. *Incentives to improve teaching: Lessons from Latin America*. World Bank.
Vegas, Emiliana, and Ilana Umansky. 2005. *Improving teaching and learning through effective incentives: What can we learn from education reforms in Latin America?* Washington, DC: World Bank.
World Bank. 2003. *World Development Report 2004: Making services work for poor people*. Washington, DC: World Bank.

World Bank. 2009. Conditional cash transfers: Reducing present and future poverty. *World Bank Policy Research Report*. Washington, DC: World Bank.
http://siteresources.worldbank.org/INTCCT/Resources/5757608-1234228266004/PRR-CCT_web_noembargo.pdf
———. 2010. *World Development Indicatior*.

Table 1. Participant characteristics by COGES and non-COGES

		Non-COGES			COGES			Mean difference
Variables		Obs	Mean	Std.dev.	Obs	Mean	Std.dev.	t-test
Dependent Variables								
Contribution	Individual Contribution (in # of 100FCFA coins)	112	3.116	1.286	136	3.242	1.458	-0.127
GSS Fair	GSS Fair Dummy	112	0.402	0.492	136	0.669	0.472	-0.267***
GSS Help	GSS Help Dummy	112	0.473	0.502	136	0.757	0.430	-0.284***
GSS Trust	GSS Trust Dummy	112	0.429	0.497	136	0.684	0.467	-0.255***
Explanatory Variables								
COGES	COGES Dummy	112	0	0	136	1	0	-
female	Female Dummy	112	0.491	0.502	136	0.316	0.467	0.175***
education	Educated Year	112	4.063	5.381	136	3.875	6.297	0.188
education_missing	Missing Dummy for “education”	112	0.089	0.286	136	0.015	0.121	0.075***
age	Age	112	38.545	17.966	136	42.985	14.816	-4.44**
age_missing	Missing Dummy for “Age”	112	0.107	0.311	136	0.015	0.121	0.092***
coin10	Dummy for an Experiment with 1000 FCFA	112	0.286	0.454	136	0	0	0.286***
muslim	Muslim Dummy	112	0.143	0.351	136	0.471	0.501	-0.328***
order1	Participation Frequency Dummy: 1st time	112	1	0	136	1	0	0
order2	Participation Frequency Dummy: 2nd time	112	0.241	0.430	136	0.243	0.430	-0.002
order3	Participation Frequency Dummy: 3rd time	112	0.080	0.273	136	0	0	0.080***
order4	Participation Frequency Dummy: 4th time	112	0.045	0.207	136	0	0	0.045**
order5	Participation Frequency Dummy: 5th time	112	0.009	0.095	136	0	0	0.009
membertype1	Group 1 Dummy: Members are 4 Fathers	112	0.321	0.469	136	0.324	0.470	-0.002
membertype2	Group 2 Dummy: Members are 4 Mothers	112	0.179	0.385	136	0.265	0.443	-0.086
membertype3	Group 3 Dummy: Members are 2 Fathers, 2 Mothers	112	0.393	0.491	136	0.294	0.457	0.099
membertype4	Group 4 Dummy: Members are 2 Teachers, 1 Mother, 1 Father	112	0.107	0.311	136	0.059	0.236	0.048
membertype5	Group 5 Dummy: Members are all COGES Executives	112	0	0	136	0.059	0.236	-0.059***
dictatorgame	Contribution in Dictator game (in # of FCFA coins)	112	2.036	1.780	136	2.971	1.531	-0.935***
dictator_missing	Missing Dummy for Dictator game	112	0.321	0.469	136	0.059	0.236	0.263***
year2010	Year Dummy for the Experiment (0 if 2009, 1 if 2010)	112	0.143	0.351	136	0.235	0.426	-0.092*
kadiogo	Regional Dummy: Kadiogo Prefecture	112	0.214	0.412	136	0	0	0.214***
bazega	Regional Dummy: Bazega Prefecture	112	0.107	0.311	136	0	0	0.107***
oubritenga	Regional Dummy: Oubritenga Prefecture	112	0.536	0.501	136	0.412	0.494	0.124*
ganzourgou	Regional Dummy: Ganzourgou Prefecture	112	0.143	0.351	136	0.118	0.323	0.025
IVs for PSM								
teacher_student	Ratio of number of teachers to number of students	112	0.034	0.018	136	0.026	0.010	0.008***
male_fem	Ratio of number of girls to number of boys	112	1.123	0.360	136	0.597	0.272	0.526***
number_class	Number of classes	112	4.179	1.821	136	4.353	1.575	-0.174
number_teacher	Number of teachers	112	5.750	2.223	136	4.000	1.167	1.75***
number_female	Number of girls	112	115.643	79.422	136	77.059	59.814	38.584***

Standard deviations reported in parentheses. *** denotes significance at the 1% level; ** at the 5% level, * at the 10% level
Values of t-test is Mean(Non COGES)-Mean(COGES)

Table 2. Determinants of contribution in the public goods experiment (OLS)

	(I)	(II)	(III)	(IV)
Estimation method	OLS	OLS	OLS	OLS
Dep.var.	Contribution	Contribution	Contribution	Contribution
COGES	0.810***	0.774***	0.494**	0.455**
	[0.243]	[0.196]	[0.178]	[0.147]
female	-0.021	0.009	-0.07	-0.042
	[0.173]	[0.171]	[0.157]	[0.166]
education	0.027	0.015	0.026	0.014
	[0.024]	[0.017]	[0.026]	[0.019]
education_missing	-0.365	-0.506**	-0.355	-0.494**
	[0.241]	[0.192]	[0.234]	[0.190]
age	-0.019*	-0.012	-0.015**	-0.010*
	[0.009]	[0.007]	[0.006]	[0.005]
age_missing	-1.056**	-0.446	-0.889**	-0.448**
	[0.394]	[0.253]	[0.306]	[0.199]
coin10	-0.727***	-0.713***	-0.674***	-0.643***
	[0.171]	[0.191]	[0.207]	[0.192]
muslim	-0.941**	-0.770**	-0.491	-0.187
	[0.311]	[0.269]	[0.323]	[0.259]
order2	-0.382	-0.335	-0.34	-0.275
	[0.358]	[0.374]	[0.340]	[0.364]
order3	1.754***	1.328*	1.844***	1.423*
	[0.474]	[0.693]	[0.508]	[0.731]
order4	0.011	0.113	0.038	0.147
	[0.086]	[0.209]	[0.104]	[0.213]
order5	0.890***	0.674	1.113***	0.872*
	[0.189]	[0.445]	[0.186]	[0.678]
membertype2		-0.501		-0.287
		[0.382]		[0.280]
membertype3		0.144		0.19
		[0.239]		[0.200]
membertype4		0.521		0.563
		[0.612]		[0.614]
membertype5		1.500**		1.366**
		[0.639]		[0.572]
dictatorgame			0.426***	0.397***
			[0.049]	[0.034]
dictator_missing			1.165***	1.254***
			[0.250]	[0.206]
oubritenga	0.676**	0.824***	0.531**	0.817***
	[0.239]	[0.218]	[0.175]	[0.150]
kadiogo	1.238***	1.261***	1.195***	1.209***
	[0.131]	[0.233]	[0.183]	[0.221]
ganzourgou	2.432***	2.271***	1.396**	1.170**
	[0.582]	[0.453]	[0.575]	[0.404]
year2010	-0.675*	-0.840***	-0.241	-0.232
	[0.319]	[0.258]	[0.252]	[0.162]
Constant	3.294***	2.947***	1.995***	1.587***
	[0.423]	[0.453]	[0.279]	[0.432]
Observations	248	248	248	248
Adjusted R-squared	0.26	0.31	0.38	0.41

Cluster-adjusted robust standard errors reported in parentheses.
*** denotes significance at the 1% level; ** at the 5% level, * at the 10% level

Table 3. Determinants of contribution in the public goods experiment (IV)

	(I)	(II)	(III)	(IV)
Estimation method	IV	IV	IV	IV
Dep.var.	Contribution	Contribution	Contribution	Contribution
COGES†	0.613***	0.729***	0.206**	0.252***
	[0.168]	[0.237]	[0.0918]	[0.0469]
female	-0.025	0.0395	-0.11	-0.0684
	[0.177]	[0.158]	[0.175]	[0.169]
education	0.0288	0.0176	0.0248	0.0135
	[0.0218]	[0.0161]	[0.0242]	[0.0173]
education_missing	-0.28	-0.410*	-0.380*	-0.497***
	[0.204]	[0.210]	[0.224]	[0.179]
age	-0.0185**	-0.0110*	-0.0153***	-0.0104**
	[0.00847]	[0.00666]	[0.00516]	[0.00486]
age_missing	-1.062***	-0.441**	-0.860***	-0.446**
	[0.351]	[0.215]	[0.279]	[0.181]
coin10	-0.795***	-0.749***	-0.764***	-0.696***
	[0.158]	[0.189]	[0.219]	[0.198]
muslim	-0.469**	-0.283	-0.189	0.0812
	[0.206]	[0.295]	[0.160]	[0.174]
order2	-0.391	-0.323	-0.345	-0.277
	[0.332]	[0.341]	[0.315]	[0.330]
order3	1.687***	1.291**	1.804***	1.385**
	[0.380]	[0.613]	[0.401]	[0.632]
order4	-0.0226	0.0811	-0.0071	0.124
	[0.0789]	[0.197]	[0.109]	[0.208]
order5	0.937***	0.745*	1.167***	0.898**
	[0.177]	[0.409]	[0.191]	[0.435]
membertype2		-0.503		-0.268
		[0.353]		[0.256]
membertype3		0.212		0.183
		[0.175]		[0.179]
membertype4		0.534		0.571
		[0.560]		[0.564]
membertype5		1.361***		1.422***
		[0.448]		[0.479]
dictatorgame			0.448***	0.414***
			[0.0530]	[0.0361]
dictator_missing			1.291***	1.385***
			[0.191]	[0.191]
oubritenga	1.066***	1.293***	0.675***	0.969***
	[0.175]	[0.285]	[0.104]	[0.156]
kadiogo	1.555***	1.742***	1.200***	1.219***
	[0.215]	[0.296]	[0.174]	[0.238]
ganzourgou	1.669***	1.450***	0.976**	0.811**
	[0.471]	[0.406]	[0.467]	[0.398]
Constant	2.998***	2.406***	1.958***	1.503***
	[0.441]	[0.485]	[0.214]	[0.330]
F Stat. for 1st stage instruments which use COGES as Dep.var.	2.89*	2.19*	8.82***	53.42***
Anderson and Rubin Wald test F	26.71***	58.17***	3.53**	8.67***
Anderson and Rubin Wald test Chi	125.73***	278.67***	16.77**	41.90***
Sargan	0.704	1.962	0.862	1.233
Observations	248	248	248	248
Adjusted R-squared	0.256	0.309	0.376	0.414

Cluster-adjusted robust standard errors reported in parentheses.
*** denotes significance at the 1% level; ** at the 5% level, * at the 10% level

† is endogeneous variables. We include four identifying instrumental variables: an interaction variable of the number of classrooms and the distance between the school and CEB office; the distance between the school and the nearest drinking water point; the distance between the school and the nearest health clinic; and a dummy variable that takes one if these pieces of information are missing. The number of classrooms is based on 2008 data. If 2008 data is missing, we used information from 2009. The Sargan statistic was reported using the estimation method with conventional standard errors.

Table 4. Balancing tests and estimated ATT by propensity score matching

Variables used for estimating propensity score	Sample before matching	Sample after one to one matching	Sample after caliper matching	Sample after kernel matching
education				
Mean(treatment)	3.875	3.421	3.421	3.421
Mean(control)	4.0625	2.974	2.973	3.143
t test(p value)	0.804	0.597	0.597	0.742
educationXeducation				
Mean(treatment)	54.375	39.395	39.395	39.395
Mean(control)	45.205	34.737	34.737	35.885
t test(p value)	0.567	0.666	0.666	0.746
education_missing				
Mean(treatment)	0.147	0	0	0
Mean(control)	0.089	0.026	0.026	0.008
t test(p value)	0.006***	0.157	0.157	0.428
number_teacher				
Mean(treatment)	4	5	5	5
Mean(control)	5.75	5	5	5
t test(p value)	0***	1	1	1
age				
Mean(treatment)	42.985	44.487	44.487	44.487
Mean(control)	38.545	45.711	45.711	44.635
t test(p value)	0.034**	0.553	0.553	0.941
ageXage				
Mean(treatment)	2065.6	2116.6	2116.6	2116.6
Mean(control)	1805.6	2269.3	2269.3	2155
t test(p value)	0.125	0.444	0.444	0.847
age_missing				
Mean(treatment)	0.015	0	0	0
Mean(control)	0.107	0	0	0
t test(p value)	0.002***	1	1	1
N(treatment)	136	76	76	76
N(control)	112	112	112	112
Propensity score estimation results				
Pseudo R2	0.243	0.011	0.011	0.003
LR test (p value)	0.000***	0.688	0.688	0.955
Estimated ATT				
Effect of participation (ATT)		1.02	1.02	0.805
t value		2.36	2.36	3.58

*** denotes significance at the 1% level; ** at the 5% level, * at the 10% level

Table 5. Influence of school type and individual characteristics to contribution

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)
Method	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
Dep.var.	Cont.	Cont.	Cont.	Cont.	Cont.	Cont.	Cont.	Cont.	Cont.	Cont.
COGES†	0.177	0.655	0.292	0.542**	0.467***	0.306***	0.614	2.050**	0.402**	0.547**
	[0.369]	[0.499]	[0.215]	[0.262]	[0.0879]	[0.0730]	[1.170]	[0.828]	[0.204]	[0.269]
female	-0.142	0.367	-0.114	-0.0887	0.774***	0.172	0.159	1.309	0.816***	0.0433
	[0.368]	[0.688]	[0.168]	[0.168]	[0.196]	[0.184]	[0.839]	[0.820]	[0.221]	[0.220]
education	0.0251	0.00924	0.044	0.0821	0.0343	0.0157	0.0563	0.116***	0.0177	0.0776
	[0.0239]	[0.0203]	[0.0511]	[0.0580]	[0.0210]	[0.0171]	[0.0851]	[0.0442]	[0.0638]	[0.0602]
education_missing	-0.375	-0.573**	-0.333	-0.286	-0.13	-0.442***	-0.343	-0.384	-0.161	-0.274
	[0.250]	[0.224]	[0.270]	[0.259]	[0.0973]	[0.135]	[0.264]	[0.374]	[0.170]	[0.242]
age	-0.0154***	-0.00976**	-0.0151***	-0.0106**	-0.00497	-0.00727	-0.0145***	-0.00857	-0.00472	-0.0089
	[0.00489]	[0.00486]	[0.00514]	[0.00511]	[0.00699]	[0.00577]	[0.00476]	[0.00700]	[0.00722]	[0.00615]
age_missing	-0.870***	-0.306	-0.827***	-0.409**	-0.641*	-0.325	-0.717**	0.07	-0.661*	-0.347
	[0.283]	[0.249]	[0.246]	[0.160]	[0.332]	[0.263]	[0.344]	[0.496]	[0.361]	[0.218]
coin10	-0.766***	-0.677***	-0.745***	-0.631***	-0.736***	-0.710***	-0.717***	-0.523***	-0.752***	-0.644***
	[0.215]	[0.186]	[0.166]	[0.189]	[0.197]	[0.206]	[0.164]	[0.177]	[0.253]	[0.185]
muslim	-0.193	0.13	-0.133	0.253	-9.312*	-2.728	-0.0526	0.53	-9.763*	-1.287
	[0.180]	[0.199]	[0.182]	[0.166]	[5.224]	[2.870]	[0.379]	[0.385]	[5.340]	[3.030]
order2	-0.345	-0.273	-0.364	-0.364	-0.603*	-0.371	-0.376	-0.413	-0.598*	-0.408
	[0.314]	[0.335]	[0.341]	[0.321]	[0.353]	[0.312]	[0.371]	[0.371]	[0.362]	[0.306]
order3	1.808***	1.313**	1.618***	0.825	1.583***	1.348**	1.43	0.201	1.738***	0.851
	[0.402]	[0.539]	[0.496]	[0.856]	[0.377]	[0.636]	[0.935]	[0.633]	[0.618]	[0.859]
order4	-0.00779	0.135	0.00267	0.145	0.00696	0.116	0.0164	0.195	-0.00107	0.139
	[0.107]	[0.202]	[0.0830]	[0.175]	[0.0985]	[0.210]	[0.0818]	[0.155]	[0.127]	[0.177]
order5	1.144***	1.209	1.156***	0.888**	1.531***	1.013**	1.346**	1.889**	1.557***	0.951***
	[0.351]	[0.799]	[0.167]	[0.390]	[0.180]	[0.396]	[0.622]	[0.739]	[0.205]	[0.366]
membertype2		-0.276		-0.247		-0.364		-0.257		-0.301
		[0.255]		[0.294]		[0.259]		[0.292]		[0.299]
membertype3		0.183		0.0964		0.139		0.0385		0.0798
		[0.173]		[0.154]		[0.176]		[0.166]		[0.160]
membertype4		0.577		0.438		0.521		0.364		0.422
		[0.560]		[0.411]		[0.537]		[0.377]		[0.409]
membertype5		1.367***		1.322***		0.668		1.074*		0.921
		[0.503]		[0.505]		[1.087]		[0.634]		[1.081]

Cluster-adjusted robust standard errors reported in parentheses. *** denotes significance at the 1% level; ** at the 5% level, * at the 10% level

† is endogeneous variables. We include four identifying instrumental variables: an interaction variable of the number of classrooms and the distance between the school and CEB office; the distance between the school and the nearest drinking water point; the distance between the school and the nearest health clinic; and a dummy variable that takes one if these pieces of information are missing. The number of classrooms is based on 2008 data. If 2008 data is missing, we used information from 2009. The Sargan statistics are reported using the estimation method with conventional standard errors.

Table 5. Influence of school type and individual characteristics to contribution (continued)

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)
Method	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
Dep.var.	Cont.	Cont.	Cont.	Cont.	Cont.	Cont.	Cont.	Cont.	Cont.	Cont.
femaleXcoges†	0.0614	-0.852					-0.539	-2.762*		
	[0.798]	[1.032]					[1.686]	[1.422]		
educationXcoges†			-0.0273	-0.0932			-0.0486	-0.158*	0.0242	-0.0855
			[0.0541]	[0.0846]			[0.118]	[0.0887]	[0.0679]	[0.0865]
muslimXcoges†					8.864*	2.615			9.254*	1.42
					[5.190]	[2.592]			[5.297]	[2.771]
dictatorgame	0.447***	0.422***	0.452***	0.431***	0.151	0.332***	0.460***	0.468***	0.134	0.385***
	[0.0556]	[0.0422]	[0.0500]	[0.0332]	[0.0919]	[0.0858]	[0.0599]	[0.0381]	[0.113]	[0.0970]
dictator_missing	1.282***	1.511***	1.299***	1.421***	0.341	1.044**	1.389***	1.857***	0.292	1.233***
	[0.229]	[0.309]	[0.157]	[0.132]	[0.297]	[0.422]	[0.273]	[0.405]	[0.305]	[0.419]
oubritenga	0.670***	1.026***	0.702***	1.044***	0.750***	0.869***	0.765**	1.281***	0.729***	0.983***
	[0.137]	[0.176]	[0.150]	[0.156]	[0.0859]	[0.233]	[0.313]	[0.313]	[0.106]	[0.216]
kadiogo	1.204***	1.160***	1.191***	1.164***	1.305***	1.183***	1.151***	0.936***	1.317***	1.149***
	[0.158]	[0.235]	[0.135]	[0.217]	[0.161]	[0.244]	[0.137]	[0.226]	[0.200]	[0.230]
ganzourgou	0.983**	0.718*	0.964**	0.775***	5.863	2.288	0.894**	0.449*	6.088	1.58
	[0.480]	[0.395]	[0.431]	[0.298]	[3.906]	[1.741]	[0.433]	[0.253]	[4.015]	[1.666]
year2010	1.982***	1.175***	1.849***	1.216***	1.950***	1.681***	1.551	-0.0468	2.046***	1.336***
	[0.306]	[0.423]	[0.221]	[0.457]	[0.389]	[0.350]	[1.002]	[0.822]	[0.521]	[0.497]
Constant	0.0614	-0.852					-0.539	-2.762*		
	[0.798]	[1.032]					[1.686]	[1.422]		
F Stat. for 1st stage instruments which use COGES as Dep.var.	8.82***	53.42***	8.82***	53.42***	8.82***	53.42***	8.82***	53.42***	8.82***	53.42***
F Stat. for 1st stage instruments which use educationXCOGES as Dep.var.			18.01***	66.10***			18.01***	66.10***	18.01***	66.10***
F Stat. for 1st stage instruments which use femaleXCOGES as Dep.var.	43.31***	26.82***					43.31***	26.82***		
F Stat. for 1st stage instruments which use muslimXCOGES as Dep.var.					0.58	0.5			0.58	0.5
Anderson-Rubin Wald test F	3.53***	8.67***	3.53*	8.67***	3.53**	8.67***	3.53**	8.67***	3.53**	8.67***
Anderson-Rubin Wald test Chi	16.77***	41.90***	16.77***	41.90***	16.77***	41.90***	16.77***	41.90***	16.77**	41.90***
Sargan	0.864	1.067	0.866	0.832	0.016	1.024	0.835	0.154	0.01	0.782
Observations	248	248	248	248	248	248	248	248	248	248
Adjusted R-squared	0.375	0.361	0.393	0.447	-0.658	0.371	0.378	0.126	-0.773	0.437

Cluster-adjusted robust standard errors reported in parentheses. *** denotes significance at the 1% level; ** at the 5% level, * at the 10% level

† is endogeneous variables. We include four identifying instrumental variables: an interaction variable of the number of classrooms and the distance between the school and CEB office; the distance between the school and the nearest drinking water point; the distance between the school and the nearest health clinic; and a dummy variable that takes one if these pieces of information are missing. The number of classrooms is based on 2008 data. If 2008 data is missing, we used information from 2009. The Sargan statistics are reported using the estimation method with conventional standard errors.

Table 6. Response regarding GSS and contribution in the public goods experiment

Attitudinal Measures of Trust		Mean	(I)		(II)	
Most people can be trusted	ALL	0.569	-0.424	(0.324)	-0.351***	(0.333)
	Non-COGES	0.429	-0.295	(0.550)	-0.251**	(0.099)
	COGES	0.684	-0.665***	(0.245)	-0.539***	(0.152)
Most people try to be fair	ALL	0.548	-0.552*	(0.305)	-0.564***	(0.273)
	Non-COGES	0.402	-0.451	(0.533)	-0.486***	(0.086)
	COGES	0.670	-0.750***	(0.171)	-0.711***	(0.140)
Most people try to be helpful	ALL	0.629	-0.428	(0.316)	-0.399***	(0.381)
	Non-COGES	0.473	-0.322	(0.482)	-0.314***	(0.102)
	COGES	0.758	-0.630*	(0.371)	-0.545***	(0.147)

Cluster-adjusted robust standard errors reported in parentheses.
*** denotes significance at the 1% level; ** at the 5% level, * at the 10% level
(I) is the marginal effect using Tobit estimation. Dependent variable is contribution. Explanatory variables is Constant, each GSS dummy.
(II) is marginal effect using Tobit estimation. Dependent variable is contribution. Explanatory variables is Constant, each GSS dummy, coin10, order2-5

Table 7. Determinants of GSS and contribution in the public goods experiment

	(I)	(II)	(III)	(IV)	(V)	(VI)
Method	IV	IV	IV	IV	IV	IV
Dep.var.	GSS_Fair	GSS_Fair	GSS_Help	GSS_Help	GSS_Trust	GSS_Trust
COGES†	0.167**	0.133***	0.482***	0.195***	0.52	0.111**
	[0.0676]	[0.0351]	[0.156]	[0.0517]	[0.325]	[0.0480]
female	0.0824*	0.0773*	0.172**	0.107	0.177**	0.0801
	[0.0482]	[0.0453]	[0.0695]	[0.0693]	[0.0717]	[0.0582]
education	0.00368	0.00324	0.0086	0.00267	-0.00253	-0.0107*
	[0.00344]	[0.00329]	[0.00759]	[0.00614]	[0.00586]	[0.00581]
education_missing	0.01	-0.00124	-0.188	-0.341*	0.261*	0.0433
	[0.0725]	[0.0687]	[0.248]	[0.188]	[0.135]	[0.0739]
age	-0.000105	-6.44E-05	0.00325	0.00245**	0.00197	0.000429
	[0.00150]	[0.00139]	[0.00204]	[0.00122]	[0.00313]	[0.00187]
age_missing	-0.569***	-0.562***	-0.00413	0.0281	-0.366***	-0.337***
	[0.113]	[0.115]	[0.226]	[0.180]	[0.127]	[0.126]
muslim	0.000446	0.0251	-0.194	0.111	-0.0548	0.373***
	[0.0440]	[0.0229]	[0.143]	[0.117]	[0.245]	[0.0927]
dictatorgame		0.0199		0.0348		-0.00272
		[0.0261]		[0.0249]		[0.0128]
dictator_missing		0.0654		0.475***		0.546***
		[0.0602]		[0.150]		[0.0928]
oubritenga	-0.697***	-0.712***	-0.481***	-0.391***	-0.467**	-0.286***
	[0.0427]	[0.0568]	[0.125]	[0.0987]	[0.221]	[0.0865]
kadiogo	0.0121	-0.0157	0.173	-0.195	0.209	-0.314***
	[0.0768]	[0.0653]	[0.162]	[0.124]	[0.286]	[0.101]
ganzourgou	-0.135***	-0.179***	0.274	0.0521	0.147	-0.105
	[0.0423]	[0.0493]	[0.234]	[0.156]	[0.251]	[0.0637]
Constant	0.806***	0.771***	0.390*	0.382***	0.358	0.492***
	[0.113]	[0.105]	[0.233]	[0.120]	[0.422]	[0.0908]
F Stat. for 1st stage instruments which use COGES as Dep.var.	1540.82***	1320.62***	1540.82***	1320.62***	1540.82***	1320.62***
Anderson and Rubin Wald test F	27.53***	8.86***	416.65***	547.48***	105.14***	47.14***
Anderson and Rubin Wald test Chi	163.29***	53.16***	2470.83***	3284.87***	623.49***	282.82***
Sargan	2.564	2.66	24.613***	23.025***	7.144	4.37
Observations	188	188	188	188	188	188
Adjusted R-squared	0.433	0.432	0.173	0.285	0.132	0.345

Cluster-adjusted robust standard errors reported in parentheses.
*** denotes significance at the 1% level; ** at the 5% level, * at the 10% level

† is endogeneous variables. We include four identifying instrumental variables: an interaction variable of the number of classrooms and the distance between the school and CEB office; the distance between the school and the nearest drinking water point; the distance between the school and the nearest health clinic; and a dummy variable that takes one if these pieces of information are missing. The number of classrooms is based on 2008 data. If 2008 data is missing, we used information from 2009. The Sargan statistics are reported using the estimation method with conventional standard errors.

Abstract (in Japanese)

要約

本研究では、初等教育への住民参加プロジェクトである学校運営委員会(COGES)の活動が、地域住民や教員らの間のソーシャル・キャピタル(社会関係資本)を向上させるか否かを検証した。分析に用いたデータは、近年 COGES プロジェクトが開始されたブルキナ・ファソのパイロット事業において収集した、公共財実験のデータである。公共財実験は、実験経済学分野では標準的手法となっている実験室型の実験であるが、我々は公共財実験を公共財への自発的供給量という側面でのソーシャル・キャピタルの水準を測定するために実施した。COGES の政策評価を数量化するための推定方法として、操作変数法やプロペンシティ・スコア・マッチング法を用いた。分析の結果、以下の実験結果が得られた。第一に、COGES の活動は社会関係資本の水準を有意に上昇させることが確認された。この結果は、異なる計量経済学の手法に対して頑健な結果である。点推定の結果によると、COGES の活動は公共財実験の貢献額を 16%から 27%上昇させることが示された。第二に、COGES の活動がソーシャル・キャピタルに与える効果は、参加者の属性によって異なることが確認された。例えば、教育年数の長い人は、教育年数の短い人と比べてソーシャル・キャピタルの蓄積が小さくなりがちであり、イスラム校の地域住民や教員は非イスラム校の地域住民や教員よりもソーシャル・キャピタルの蓄積が大きいことが見出された。第三には、公共財実験と主観的質問である General Social Survey(GSS)の結果が整合的であることが確認され、我々の結果が頑健であることを示唆している。