NBER WORKING PAPER SERIES

MOBILE PHONES, CIVIC ENGAGEMENT, AND SCHOOL PERFORMANCE IN PAKISTAN

Minahil Asim
Thomas Dee

Working Paper 22764
http://www.nber.org/papers/w22764

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 2016

The authors would like to Zubair Bhatti and Ali Inam at the World Bank, staff at the School Education Department in Punjab, Pakistan, and seminar participants at Stanford University, UC Davis, and the Association for Education Finance and Policy for helpful comments. The view expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Mobile Phones, Civic Engagement, and School Performance in Pakistan
Minahil Asim and Thomas Dee
NBER Working Paper No. 22764
October 2016
JEL No. I2,O1

ABSTRACT

The effective governance of local public services depends critically on the civic engagement of local citizens. However, recent efforts to promote effective citizen oversight of the public-sector services in developing countries have had mixed results. This study discusses and evaluates a uniquely designed, low-cost, scalable program designed to improve the governance and performance of primary and middle schools in the Punjab province of Pakistan. The School Council Mobilization Program (SCMP) used mobile-phone calls to provide sustained and targeted guidance to local school-council members on their responsibilities and authority. We examine the effects of the SCMP on school enrollment, student and teacher attendance, and school facilities using a "difference in difference in differences" (DDD) design based on the targeted implementation of the SCMP. We find that this initiative led to meaningful increases in primary-school enrollment, particularly for young girls (i.e., a 12.4 percent increase), as well as targeted improvements in teacher attendance and school facilities, most of which were sustained in the months after the program concluded.

Minahil Asim
University of California, Davis
School of Education
1 Shield Ave
Davis, CA 95616
masim@ucdavis.edu

Thomas Dee
Stanford University
520 Galvez Mall, CERAS Building, 5th Floor
Stanford, CA 94305-3084
and NBER
tdee@stanford.edu

1. Introduction

The informed oversight of citizens can promote effective governance of their local public services by mitigating the moral hazard that can exist in the presence of information asymmetry and a divergence between the goals of individuals operating government agencies and the public interest (Azfar et al. 1999; Mansuri and Rao 2014). That is, informed civic engagement can support accountability of public sector workers, tailor public services to the unique needs of particular communities, improve poverty targeting, and, in general, increase the demand for good governance. A growing body of empirical evidence however, suggests that the manner in which citizens are given information and the opportunities to participate in the delivery of public services, influences the impact of civic engagement on the quality of local governance (Olken 2007; Bjorkman and Svensson 2009; Banerjee et al. 2010; Duflo et al. 2014; Blimpo 2015).

Our paper contributes to the literature on strengthening local governance to improve public service delivery in developing countries by studying a novel and low-cost intervention. Specifically, we examine the School Council Mobilization Program (SCMP), a unique program piloted in five districts in Pakistan's largest province Punjab. The SCMP focused on providing sustained and targeted guidance to school council (SC) members (i.e., parents, community members, the head-teacher) on their civic responsibilities through regular, low-cost engagement over mobile phones. The provincial government hired a call center for 17 months and, every month, calling agents provided information to SC members on their roles and responsibilities. These design features (i.e., a one-to-one, low-cost and sustained engagement mechanism between the provincial government and the SCs to encourage citizen participation in improving school governance) have not, to our knowledge, been evaluated in local governance settings. Moreover, Pakistan provides a unique cultural and political setting to evaluate this impact where public services are plagued by under-provision and corruption.

We use publically available school-level administrative data for 26 districts, 5 of which were exposed to SCMP, collected regularly by the Program Monitoring and Implementation Unit (PMIU) of the School Education Department in Punjab. We organized these school-level panel data into four distinct time periods. The earliest observations consist school-level data from the months just prior to the implementation of the SCMP. The next two periods correspond to a time-period when the SCMP was actively engaging SC members in the field and the fourth provides us data in the months shortly after SCMP activity had concluded. Schools are

segregated by gender in Pakistan. Within the 5 program districts, only primary and middle schools were chosen for SCMP. The eligibility criteria, whereby only schools with median or higher enrollment within each district-by-level-by-gender cell were intended to participate, informs our quasi-experimental research design. We leverage the existence of school-by-period panel data across districts with and without the intervention in "difference in differences" (DD) and "difference in difference in differences (DDD) designs. Using data from districts in which the SCMP was piloted, our DD identification strategy compares the changes in outcomes across treatment-eligible schools to the contemporaneous change observed in treatment-ineligible schools. We also examine the key identifying assumption of the DD specifications (i.e., common trends across treatment-eligible and ineligible schools) in DDD specifications that include panel data from districts in which the SCMP was *not* piloted.

We find that the program increased student enrollment by 5.7 percent relative to baseline enrollment. However, the impact was statistically significant for primary schools only. Furthermore, we find that the SCMP also reduced student attendance modestly (i.e., 1.2 percent), suggesting that the marginal student enrollee was comparatively unlikely to attend school as consistently as other students. Interestingly, the increase in enrollment and the decline in attendance were the highest for female primary schools (i.e., girls aged 5-12). Specifically, we find that the SCMP increased the enrollment of young girls by 12.4 percent (i.e., roughly 13 girls per school). We also find that the SCMP increased teacher attendance by roughly 2 percent relative to baseline attendance. The SCMP also increased the likelihood a school had functional facilities but this impact statistically significant for male-primary schools only. In general, we find that these effects grew during the duration of the program and continued in the months after the program ceased operations.

Overall, our results suggest that the engagement mechanism informed council members and encouraged them to participate in school governance that improved school outcomes. We speculate that continuous engagement with calling agents, who were of the same sex as the members, and the fact that it was spearheaded by the provincial government, assisted their engagement and added credibility to the calls. Moreover, we think that members, either through passive oversight, or proactive engagement and monitoring of the school were able to impact outcomes in their community's schools. Our results also provide broader evidence on improving public services through proactive citizen participation in low-cost and highly scalable ways (e.g.,

compared to in-person training) via continuous engagement mechanisms between the state and its citizens.

The remainder of the paper is organized as follows: section 2 provides a discussion on the theoretical framework and prior literature, section 3 describes the School Council Mobilization Program, section 4 and 5 include a description of the data and identification strategies respectively. Section 6 describes our results and section 7 provides a discussion and conclusion to the paper.

2. Theoretical Framework and Prior Literature

A broad and long-standing concern, both among policy-makers and in diverse academic literatures, involves the question of whether (and when) representative government agencies are ineffective in carrying out their core functions. The problem of poor representative governance is widely viewed as a particularly critical impediment to improving the delivery of much-needed public services within developing countries (World Bank 2004). The general theoretical frame for understanding how such governance failures may persist in any context can be explained by asymmetric information (i.e., a principal-agent problem). Funders and voters cannot easily or efficiently observe the behavior of their representative government agents. Moreover, the individuals operating government agencies may have private goals that diverge from the public interest with regard to their own effort as well as the goals of the public agency. In these circumstances, public services may be misaligned, underprovided, or even characterized by outright corruption.

An institutional design that may attenuate such problems involves the devolution of authority for public services from centralized to local governments. For example, the literature on fiscal federalism suggests that the local financing and provision of public goods, combined with residential mobility (Tiebout 1956), can impose competitive pressures that may improve public-sector performance. Furthermore, a more localized authority for the provision of public services can enhance the relative capacity for direct democratic engagement and oversight by concerned local citizens. Stiglitz (2002) stresses the unique policy relevance of such local engagement in developing countries, noting that because community members are those who benefit from a program, they have better incentives to monitor compared to the central-government bureaucrats. However, the fact that the financing for public services in developing

countries is often centralized may weaken the incentives for local oversight and citizen engagement.

These concerns have motivated an increased interest in promoting the prevalence and quality of local engagement in the provision of public services (World Bank 2004). In particular, citizen participation in public sector delivery is one external mechanism that may mitigate the problems of information asymmetry and moral hazard between the goals of individuals operating government agencies and public interest, through oversight and engagement (Azfar et al. 1999; Mansuri and Rao 2014). Participation may, for example, support accountability of public sector workers who are rarely held accountable for their absences or corrupt practices.[1] Local engagement may also support the tailoring of public services to the unique needs of particular communities, improve poverty targeting, and, overall, increase demand for good governance. However, the efficacy of increased citizen engagement is, by no means, certain; community members may have poor information on their rights and responsibilities with respect to local governance as well as on the goals and challenges involved in the delivery of public services. Grassroots monitoring may be prone to capture by local elites or free-rider problems (Bardhan 2002). Mansuri and Rao (2014) argue that absence of strong institutions at the center may also constrain the ability of citizens to be fully engaged in service provision.

A recent and growing empirical literature provides mixed evidence on how local communities can be engaged to participate in improving public-sector performance in developing countries. For example, Bjorkman and Svensson (2009) conducted a randomized field experiment in Uganda in which localized NGOs informed communities about the status of health facilities and encouraged them to hold their local providers accountable for performance. The intervention provided information on the quality of services while also reducing the risk of elite capture. It also addressed the participation constraint by involving large number of community members and by encouraging them to develop a monitoring plan. They found the intervention generated large increases in utilization of services and improved health outcomes as measured by child mortality and child weight. However, Banerjee, Deaton and Duflo (2004) designed an RCT in Udaipur in India where a member of the community was paid to monitor clinics for 8 months and to take action using the collected information on absenteeism. They

[1] Chaudhry et al. (2004) document high absence rates among publically funded health workers and school teachers in six developing who are not held accountable for their absences.

found that absence rates were the same in program and control facilities. The authors argue that the key reason for this is that the community member did not manage to use his or her information on absenteeism to invoke community participation. Similarly, Olken (2007) found little impact of grassroots participation through increased attendance at community meetings and issuance of anonymous comment forms to villagers on reducing corruption overall in over 600 Indonesian village road projects.[2] The experiments sought to enhance participation at village level meetings in which project officials account for how they spend project funds. Inviting more members to monitoring meetings reduced only missing labor expenditures with no impact on missing material expenditures. Issuing anonymous comment forms reduced missing expenditures only if the comment forms were distributed via schools in the village, effectively bypassing village officials. Because the entire village gains from reducing corruption in materials, the authors suggest that grassroots monitoring can be effective in circumstances where local civic engagement is robust.

In studies focusing specifically on education, empirical evidence on the impact of strengthening citizen participation to improve service delivery is also mixed. For example, in Kenya, Duflo et al. (2014) found that giving school councils (SCs) the autonomy and funds to hire an extra contract teacher in schools over whom the committee had direct control led to an improvement in student test scores. The effects were larger when the SCs received school-based-management (SBM) empowerment training on how to select job applicants, monitor and assess teachers' effort and performance and review the performance to renew teachers' contracts. Civil-service teachers were more likely to be present in class and teaching and relatives of civil services teachers were less likely to be hired. Similarly, preliminary results from a randomized control trial of 610 villages across three states in India, show that that structured information campaigns about community roles and responsibilities in school management and services available to schools, conducted through repeated village meetings over two months, led to a significant and positive impact on community participation, provision of school entitlements, and teacher effort (Goyal et al. 2008).[3] In Gambia, also, results of an experiment in which principals,

[2] An anonymous invitation form was distributed along with the invitations to attend meetings, providing villagers an opportunity to relay information about the project without fear of retaliation. The results on the comment forms were discussed in the meetings.

[3] In one state, Karnataka, the community was also given information on the economic benefits of schooling and there was explicit advocacy in campaign meetings to monitor learning in schools however there was no additional

teachers and members of the communities received comprehensive training in developing school management plans as complements to a grant, improved teacher and student attendance but did not have an impact on test scores (Blimpo et al. 2015).

On the other hand, in Kenya, Kremer and Vermeerch (2005) found no effect of empowering school committees by increasing the frequency of meetings with school administration at the sub-district level and making them responsible for evaluating teacher performance. Providing voice and control over resources did not reduce teacher absenteeism or improve children's performance on tests. Similarly, Banerjee et al. (2010) found that providing information to villagers in India about the Village Education Committee and the status of education in their villages, and pedagogical training for teaching basic reading skills to the communities did not improve school performance, as measured by community participation in schools, teacher and student attendance and learning outcomes.[4] In both studies, information on performance on outcomes was not relayed to SC members.

These contrasting results for interventions aimed at improving service delivery via citizen engagement suggest that the context and the way citizens are given a chance to participate in the process of service delivery are imperative in predicting whether or not interventions will work to improve public services. Our paper contributes in several ways to this literature on strengthening local governance. From a design standpoint, the School Council Mobilization Program (SCMP) has several uniquely compelling features. In particular, it uses a one-to-one, low-cost and sustained engagement mechanism between the provincial government and the School Councils (SCs) to encourage citizen participation in improving school governance. These design features (i.e., provincial government directly engaging with leading citizens on a sustained and individual basis) may play an important role in terms of influencing their behaviors. However, interventions with these features have not, to our knowledge, been evaluated in local governance settings.

Moreover, Pakistan provides a unique cultural and political setting to evaluate this impact. Public services are plagued by under-provision and corruption. However, with growing mobile and internet usage, several ICT-based citizen engagement initiatives are being piloted and

---

impact of this information. The study also measures the impact only 2-3 months after the intervention was administered and the results are preliminary.

[4] Village Education Committees in India consist of the elected head of the village government, the head teacher of the local school, and three parents who are nominated by their community. These committees are responsible for monitoring school performance, allocating school resources and hiring additional contract teachers in the event of overcrowding.

scaled up in the Punjab province to improve performance of services.[5] SCMP, in particular, adds to our understanding of whether this engagement mechanism improves school performance through SC members who are autonomous but often unaware of their responsibilities. Also, it provides broader evidence on improving public services through proactive citizen participation through mechanisms spearheaded by a strong center. Lastly, our quasi-experimental identification strategy helps us estimate a credible causal impact of the program that has important policy implications.

3. The School Council Mobilization Program (SCMP)

The School Council Mobilization Program (SCMP) was a pilot project conducted for 17 months and situated in the Punjab province of Pakistan. This program, which is described in more detail below, focused on providing sustained and targeted guidance to school council members on their civic responsibilities through low-cost engagement over mobile phones. The Punjab province in which this pilot was situated comprises of almost 60% of the total population of Pakistan. Approximately, 44% of Punjab's population are children aged fewer than 18 years. The province contains approximately 54,000, primary, middle (lower secondary), high (upper secondary), and religious public schools spread across a total of thirty-six districts (PMIU 2012).[6] Compared to other provinces in the country, Punjab has performed better in improving key education indicators such as enrollment, student and teacher attendance, infrastructural development and performance on test scores. However, the government is still struggling to provide universal access to quality education. The net enrollment rate for example, is 62% at the primary level and only 25% at the secondary level with a higher proportion of out-of-school girls than boys (PSLM 2014).[7] The World Bank is currently funding the Punjab Education Sector Reform Program (PESRP), a highly visible province-wide program endorsed by the head of the provincial government to improve access, quality and governance in the education sector. The SCMP pilot was a component of the PESRP initiative.

School Councils in Punjab

[5] However, none of these programs (Bhatti et al. 2015; Masud 2015) has a timing and placement such that they would confound this study's inferences.

[6] Religious schools or Madrassas are usually situated within mosques and have their own religious curriculum instead of the one prescribed by the provincial government.

[7] The net enrollment rate refers to the number of age-appropriate students enrolled in a level of education divided by the total number of age-appropriate children for that level of education.

The Government of Punjab established school councils (SCs) in 1994 in both primary and middle schools as part of province-wide school-based management (SBM) reforms. These SCs consist of a head-teacher (or principal) who serves as the chairperson and 7-15 elected members, including parents (at least 50% of the SC membership), and notable individuals from the community, such as shopkeepers. The members mostly belong to low-income backgrounds with little or no education and serve on the council for a year. The School Council Policy 2007 (i.e., the official government guidance document for SCs) states that members are required to meet monthly, keep records of their meetings and ensure two-thirds of the members attend them. The SC members are also responsible for monitoring teacher, staff, and student attendance, making efforts to increase enrolment, reducing dropouts, monitoring and assisting the provision of textbooks, hiring temporary teachers and staff, managing the SC Fund, planning infrastructural development, and keeping records of all transactions (Government of Punjab 2007).[8]

In 2007, Punjab's School Education Department initiated a capacity-building program for SCs to inform them of their role in local governance. The National Rural Support Program (NRSP) was contracted to conduct a three-day training in all primary and middle schools.[9] The trainings were held via community organizations in all schools between 2008 and 2011 and cost the government PKR 8,000 ($180) per school for a one-time group-based session (Cambridge Education 2014). A recent descriptive study examined 800 SCs in the province and found that despite the on-going capacity building program, 21% failed to conduct the required one meeting every month and 48% of the head teachers did not perceive the members to be aware of their responsibilities (GTZ & I-SAPS 2010). The fact that SC performance remained uneven, combined with the substantive implementation challenges (and comparatively high cost) associated with in-person training, provided an important motivation for the phone-based SCMP pilot. Another motivation is that, on average, 71% of Punjab's households own a mobile phone (MICS 2007-08) and this average ownership rate is likely to be higher among those serving on school councils.

[8] Primary school councils are given PKR 20,000 ($200) and middle schools PKR 40,000 ($400) annually for spending on school maintenance, hiring of an extra teacher and providing refreshments in council meetings. The implementation review found that most councils did not spend the money that they were allotted at the start of the fiscal year (Cambridge Education 2013).

[9] NRSP is a not-for-profit organization doing development and advocacy work in the country. It has a presence in 61 districts in all four provinces and works with 170,320 community organizations for rural development. (http://nrsp.org.pk).

Program Description

The SCMP began call-center operations in April of 2013 under the aegis of PESRP and with the financial assistance of the World Bank. The call center was located in the provincial capital, Lahore. A total of 15 individuals (i.e., 5 men and 10 women) trained as phone agents for the SCMP.[10] These agents placed monthly, informational phone calls to individual SC members; each lasting approximately 6 minutes.[11] The agents added credibility to the calls by informing the members that the call was being made directly from the provincial school education department. The members received calls from the same calling agent for the entire 17-month duration of the intervention. In light of the cultural context, SC members were assigned same-sex agents. In "Phase I" of the program (i.e., April 2013 to April 2014), the call center used a purposefully time-varying (but integrated) script to engage with SC members every month. The aim of the scripts was to discuss a unique SC responsibility each month and also to follow up with SC members about that responsibility at the next call. The timeline for the calls and more specific information on their content (e.g., the fourth call from July 15 to August 25 informed the members of the process of conducting the monthly meetings) are provided in Table 1.

In each call, the calling agent provided scripted information to the SCs on one area of responsibility but the scripts did not specifically address how those tasks could be achieved (Cambridge Education 2014). During some calls, SCs were also asked to give their feedback on the current state of school management for their respective schools. In "Phase II" of the program (i.e., May 2014 to August 2014), the order and content of the scripts was modified in response to feedback from the field and from centrally monitored process data. The Phase II script also emphasized the enrollment campaign to meet the Millennium Development Goal (MDG) of achieving universal primary education.[12] The agents also shared data on the number of out-of-school children in the district and province.

[10] Abacus Consulting, a private call center in Lahore, was hired to carry out the operations.
[11] Initially, these monthly calls were complemented by two text messages to each SC member. However, the text messages were discontinued owing to low-literacy levels of most council members who were unable to read them.
[12] The Chief Minister launched a province-wide awareness campaign to enroll every school-aged child in school to meet the MDG in 2015. Interventions under PESRP incorporated this campaign as part of their design. The existence of this province-wide effort implies a context that might conceivably attenuate the impact estimates we report.

This SCMP pilot was conducted among a subset of schools from 5 of Punjab's 36 districts (i.e., Attock, Chiniot, Jehlum, Lodhran, and Sargodha).[13] All of the schools in the province are segregated by gender and the school-council members typically share the gender of the students in the school they serve. Only primary schools (i.e., grades 1 to 5) and middle schools (i.e., grades 6 to 8) were chosen for this program. School eligibility for the pilot was also a function of school size. Specifically, within each of the 20 district-by-level-by-gender cells, only the schools with median or higher enrollment were intended to participate. This criterion, set by the World Bank, reflected both an interest in reaching more students and in increasing the likelihood that SC members had mobile phones. We define treatment eligibility based on this assignment rule. That is, the intent to treat (ITT) is a binary indicator equal to one for schools at or above median baseline school enrollment within their district-level-gender cell. However, there are several reasons that uptake of the "treatment" was sometimes inconsistent with this eligibility rule. In particular, accurate mobile phone numbers were available for most but not all of the SC members in eligible schools. Furthermore, to ensure that a fixed number of schools were called every month, the district governments were instructed to add schools to the sample that had SC members with valid phone numbers. To avoid the internal-validity threats created by such non-random take-up of the treatment, our panel-based design, which is described in more detail below, relies on the intent to treat implied by baseline enrollment and the intended eligibility rule.[14]

The fundamental goal of the SCMP was to utilize low-cost technology to inform and mobilize SC members through sustained and thoughtfully designed engagement with school governance and performance. Policy makers intended to combine the SCMP initiative with a reconstitution of SCs in eligible schools (i.e., election of new SC members and a modest increase of the minimum membership from 7 to 9). However, according to the SCMP implementation review (2013), no elections were held and the head-teachers mostly added approximately two members to the existing list of council members when needed.[15] Given this modest change in SC

[13] These 5 districts provided a geographically dispersed sample across the province (Cambridge Education 2013). The SCMP intervention expanded to 10 additional districts near the end of our study window (i.e., Phase II). We exclude these districts from our study.

[14] We carefully considered but rejected using this assignment rule in a regression-discontinuity design (RDD). The lack of a crisp "first-stage" jump around these thresholds weakened the credibility (and statistical power) of that design for this context.

[15] SCMP districts were given only three days to carry out the reconstitution before call center operations began hence elections were not held. Membership categories were revised and additional members added could be

membership, we view the treatment contrast created by SCMP eligibility as effectively defined by the call-center intervention. However, the modest increase in SC membership may be a relevant contextual factor.

The program cost the government PKR 5000 ($50) per school for a year-long engagement with SC members. The earlier NGO-delivered trainings, which were delivered in person, cost nearly 4 times as much per school. As noted earlier, the SCMP also has other distinctive design features. In particular, it provided more sustained and one-to-one engagement of the provincial government with SC members compared to a one-time NGO-led training. This type of continuous and personal engagement, spearheaded by a well-run center, may play an important role in terms of influencing behaviors. However, whether this intervention was actually effective in terms of influencing key school outcomes is ultimately an empirical question. In the next two sections, we turn to the data and research design that will take up that broad question.

4. Data

The main source of data for our analysis is publically available school-level administrative data collected regularly by the Program Monitoring and Implementation Unit (PMIU) of the School Education Department in Punjab. This dataset includes administrative information on total enrollment in schools, teacher and student attendance, district-administrator visits per month, the provision of text books, their monthly expenditure, and the functionality of school facilities such as toilets, electricity, and water and the presence of a boundary wall around the school.[16] Approximately 900 monitoring and evaluation assistants (MEAs), hired by Punjab's provincial government, administer the monthly survey (except for June, July and August) in all 36 districts (54,000 public schools).[17]

We use data for the primary and middle schools in 26 out of the 36 districts in the province. This data set includes the 5 districts that were exposed to Phases I and II of the SCMP

---

grandparents or siblings of existing students, local mosque representatives, retired teachers, and local elected leaders.

[16] Functional facilities refer to running tap water or availability of utensils to store water in schools, a toilet that is in order, functioning electricity connections, and a complete boundary wall around the school.

[17] The schools monitored by individual agents are rotated to attenuate the risk for intentional misreporting. We take up the question of whether our findings might reflect, to an unknown extent, policy-endogenous misreporting in the data. We argue that our pattern of results is inconsistent with this concern.

but excludes 10 districts added to the pilot near the end of our study window.[18] Our monthly data from these 26 districts span the period from November 2012 to December 2014. It should be noted that this panel data structure makes it possible to implement "difference in difference" (DD) designs. For example, we can compare the change in outcomes across treatment eligible and ineligible schools in the five districts where treatment occurred. Alternatively, we can compare the outcome changes in treatment-eligible schools (i.e., sufficiently large schools) across districts that did and did not participate in the pilot. Combining these data also allows us to adopt a "difference in difference in differences" (DDD) approach. As we describe in the next section, these data make it possible to examine (and, under certain assumptions, correct for) violations of the identifying assumptions of the DD approach.

The timeline for the intervention and corresponding data collection is summarized in Table 2. It should be noted that there is no monitoring in June and July and only 50% of the schools are monitored in May and August because of summer vacations from mid-May to mid-August. Given that and the fact that not every school is surveyed in every month, we use the averaged school data from four distinct time periods: November 2012 to April 2013, September to November 2013, January to April 2014 and September to December 2014.[19] This data structure is an appropriate one for our analysis. The first time period captures school-level data from the period just prior to the implementation of the SCMP. The next two periods correspond to a time-period when the SCMP was actively engaging SC members in the field. And the third period provides us monitoring data in the months shortly after SCMP activity had concluded. This data period is an important one because it gives us some window into whether the effects of the SCMP persist once direct engagement has ceased. We explore the possible treatment heterogeneity by time period explicitly in our analysis.

To identify our "intent to treat" (ITT) population and to construct our analytical sample, we first relied on the baseline PMIU school-census data for 2012 that were utilized by the SCMP team to identify eligible schools and to select program participants in the five program districts.[20] In these five program districts, a school was identified as SCMP eligible if it had median or higher enrollment in each of the 20 district-by-level-by-gender cells. We matched this baseline

[18] The data are not yet available to study this expanded pilot but this is a compelling opportunity for follow-up research.
[19] The publicly available data file for December 2013 was corrupted and, therefore, excluded from our analysis.
[20] From this sample of 5,592 schools, we excluded schools that had no reported enrollment at baseline (n=53); none of these schools participated in the SCMP pilot.

sample of schools to their corresponding PMIU monitoring data from the one pre and three post-treatment periods described above. Our analytical sample also includes similarly constructed school-period panel data drawn from the PMIU data files for schools in the 21 other districts where the SCMP was *not* available. In particular, we identified schools in these 21 districts that would have been SCMP eligible (i.e., if SCMP had been available) as those with median or higher enrollment in each district-by-level-by-gender cells.

Our final analytical sample, collapsed by time-period consists of 123,235 school-by-period observations from 32,309 unique schools. However, this school-by-period panel data is a somewhat unbalanced one, reflecting the fact that some schools in our baseline intent-to-treat (ITT) population failed to participate in the PMIU monitoring in one or more of the follow-up periods (or possibly merged into other schools or closed). Specifically, the rate of missingness among baseline schools in the data increases from 5.1% in the first post-treatment period to 7.1% in the final period. This missingness in the administrative data implies a modest external-validity caveat.[21] However, it also raises more substantive internal-validity concerns. That is, our estimated impact of the SCMP eligibility could be biased if SCMP eligibility influenced the likelihood of participating in the subsequent school-level monitoring. In fact, an auxiliary DD regression in which missingness is the dependent variable indicates that SCMP-eligible were modestly but significantly less likely to be missing from the post-treatment school monitoring. That is, over this study period, schools with larger enrollments (i.e., SCMP eligible) increased their monitoring participation relative to schools with lower-baseline enrollments (i.e., by roughly 2 to 3 percent). Fortunately, there is a similar pattern among the schools in the 21 *non-program* districts, suggesting that these trends in missingness were related to school size. An auxiliary DDD specification in which missingness is the dependent variable indicates that missingness is unrelated to SCMP eligibility. This evidence of "missingness at random" is one of the reasons that we view the DDD specification as our preferred one. However, as a complement to the concerns raised by missingness, we also report both DD and DDD results based on a "balanced" sample of schools (i.e., each school observed in each time period).

Table 3 presents key descriptive statistics for the unbalanced sample. By design, slightly more than 50% of the schools were eligible to receive the program in program districts (or would

[21] The rate of missingness is higher for primary schools (5.0%) compared to middle schools (3.0%). It is also higher for male schools (7.2%) compared to female schools (2.2%)

have been eligible in non-program districts). Forty-four percent of the schools in the 5 program districts (i.e., 2,159 out of 4,909 schools) participated in the SCMP. Treated schools are identified as schools that received calls in the first month of the intervention (April 2013). However, schools that received calls in the first month subsequently received all calls in the proceeding months for the duration of the program. There are roughly the same number of male and female schools in the sample. However, roughly 82% of these schools are primary rather than middle schools.

Table 3 also provides suggestive evidence on how the key school outcomes (e.g., average student enrollment, student and teacher attendance, functioning school facilities) change over the study period. In general, we see that these measures improved over time in both the program and non-program districts. For example, average student enrollment shows an increase of 14 students in the program districts, post-treatment. On average, these gains are somewhat larger in the program districts than in the non-program districts, suggesting the existence of treatment effects. In the next section, we describe more formally the research designs we use to examine such questions.

5. Identification Strategies

Our quasi-experimental approach to identifying the impact of the SCMP on school outcomes leverages both the existence of school-by-period panel data across districts with and without the intervention and our knowledge of school eligibility for the treatment. That is, we effectively view a school i's treatment *eligibility*, T_i, (i.e., whether its baseline enrollment was at or above the median in its district-level-gender cell) interacted with being observed in the post-treatment period as an instrumental variable (IV) for whether it actually participated in the SCMP treatment. Our initial approach to estimating the effect of this "intent to treat" (ITT) is based on a "difference in differences" specification of the following form:

$$Y_{it} = \alpha_i + \lambda_t + \beta(T_i \times POST_t) + \varepsilon_{it} \quad (1)$$

in which Y_{it} is an outcome for school i observed in period t, α_i represents school fixed effects, λ_t, represents period fixed effects and the coefficient of interest, β, identifies the effects unique to being a treatment-eligible school in the post-treatment period. The term, ε_{it}, is a mean-zero error that accommodates school-level clustering (Liang and Zeger 1986; Bertrand et al. 2004). We use the same general approach to estimate the "first-stage" effect of treatment eligibility on treatment

uptake. The ratio of the "reduced-form" and "first-stage" estimates identifies the effect of taking up the SCMP intervention on school outcomes (i.e., the "treatment on treated" estimate). The outcomes of interest are school enrollment, school-level student and teacher attendance and the functionality of school facilities (toilets boundary wall, drinking water and electricity).[22]

The impact estimate based on this DD approach (and the data from the five treatment districts) controls for time-invariant traits unique to each school and time-varying determinants shared by all schools. The estimated impact effectively compares the change observed among treatment-eligible schools (i.e., schools with above-median enrollment) to the contemporaneous change observed among treatment-ineligible schools (i.e., schools with below-median enrollment). The critical identifying assumption in this approach is that the change observed over time among the treatment-ineligible districts provides a valid counterfactual for the changes that would have been observed in the treatment-eligible districts if the treatment had never occurred.

One compelling way to assess the "common trends" assumption underlying a DD approach (Angrist and Pischke 2008) is to examine the comparative trends across high and low-enrollment schools (i.e., treatment eligible and ineligible) in the neighboring districts that have *not* had the intervention (i.e., a "naïve" or "placebo" DD). The comparative data from schools in the *untreated* 21 districts provide information on the existence and potential direction of biases in DD inferences. Moreover, these data facilitate a "triple difference" (DDD) approach that isolates the impact of interest (under weaker assumptions than the DD), using the data from all 26 districts. More specifically, the DDD approach is based on the following specification:

$$Y_{igt} = T_i + P_g + \lambda_t + (T_i \times \lambda_t) + (T_i \times P_g) + (P_g \times \lambda_t) + \beta(T_i \times P_g \times POST_t) + \varepsilon_{igt} \qquad (2)$$

where P_g is a dummy variable that identifies schools in the five "program" districts where the intervention was fielded. In the DDD approach, the coefficient of interest reflects the *three-way* interaction of being a treatment-eligible school during the post-treatment period and in a district that offered the treatment. This approach controls unrestrictedly for several two-way interactions

[22] Our choice of outcome variables is constrained by the availability of administrative data. Prior studies that evaluated local governance in the education setting have examined teacher and student attendance and test scores as primary outcome measures. The literature also evaluates intermediate or process outcomes such as participation of community members at meetings to assess the mechanism of impact. In our analysis, we are unable to evaluate effects on SC attendance or test scores (which would reflect possible enrollment effects as well as student learning).

(e.g., period effects unique to treatment eligible schools across all districts and period effects unique to the program status of a district).[23]

The compelling feature of the DDD approach is that it provides a plausible way to correct for violations of the "common trends" assumption that may vex DD inferences. In particular, the DDD effectively controls for time effects that may be unique to treatment-eligible schools and to treated locations. One way to acknowledge this feature of the DDD approach is to recognize the parameter of interest, β, can be understood as the difference in two "difference in differences". That is, this DDD impact estimate is the difference between the "true" DD based on data from the five program districts and the "naïve" DD based on data from the 21 non-program districts. However, it should be noted that the DDD approach also embeds an identifying assumption. Specifically, it assumes that the comparative trends across low and high-enrollment in the non-program districts (i.e., the naïve DD) provides a valid counterfactual for the comparative changes that would have been observed across such schools in the program districts and in the absence of the program. Given the geographic proximity of the non-program districts and the fact that they share provincial governance, this assumption has some face validity to it. However, we also note that our DD impact estimates tend to be larger in absolute value than the corresponding DDD estimates. This implies that we could view the DDD estimates as conservatively small estimates of the effects of interest given the empirical evidence for the direction of the biases implied by violations of the common-trends assumption.

6. Results

We begin presenting our results by examining the effects of SCMP eligibility on school participation in the SCMP intervention. Specifically, Table 4 presents such "first-stage" estimates from DD and DDD specifications in which SCMP participation is the dependent variable. The results consistently indicate (i.e., across both types of specifications and balanced as well as unbalanced samples) that eligibility increased SCMP participation by a substantial amount: 58 percentage points. In Table 5, we begin examining whether SCMP eligibility (and the implied sharp uptake in SCMP participation) similarly influenced our outcome measures,

[23] As a practical matter, we continue to control for fixed effects specific to each school, which are perfectly collinear with a school's other fixed traits such as treatment eligibility and whether it resides in a program district.

using DD specifications based on schools in the "program" districts where the SCMP was implemented.

Columns (1) and (2) in Table 5 present reduced-form DD estimates (i.e., based on equation (1)) of the impact of being a treatment eligible school on outcomes for the unbalanced and balanced panels, respectively. These results suggest that student enrollment went up by approximately 5 students in SCMP eligible schools. Given the corresponding results in Table 4, the implied IV/2SLS estimates suggest that SCMP *participation* increased student enrollment by nearly 9 students (i.e., 5/0.58), an increase of roughly 7.5 percent relative to the pre-treatment baseline. However, the DD estimates in Table 5 also suggest that these marginal enrollees were less likely to attend school consistently, as SCMP eligibility implies a modest reduction in school attendance. The results in Table 5 also indicate that SCMP eligibility increased teacher attendance by 1 percentage point. This implies that SCMP participation increased teacher attendance by 1.7 percentage points (i.e., 1.0/0.58), an increase of roughly 2 percent relative to the baseline mean. The remaining DD results in columns (1) and (2) of Table 5 suggest that SCMP eligibility had mixed effects on aspects of school facilities (e.g., improving the water facility but reducing the likelihood that the school met standards for both toilets and electricity.

The internal validity of these DD results turns critically the identifying assumption that both SCMP-eligible and ineligible schools (i.e., schools with above vs. below-median enrollment) would have shared "common trends" in the absence of the treatment. One compelling, ad-hoc way to examine this assumption is to consider the comparative trends from schools in neighboring districts in the province where the SCMP was not implemented (i.e., "non-program" districts). More specifically, we imputed whether schools would have been eligible for the SCMP if their district had participated in the pilot (i.e., was their baseline enrollment at or above the median for their district-level-gender cell). Then, we used DD specifications to estimate the "effect" of SCMP eligibility on our key outcomes.

Columns (3) and (4) in Table 5 present the key results from these "naïve" or "placebo" DD specifications based on panel data from schools in the 21 districts that were not exposed to the program. We find that SCMP eligibility implied small but statistically significant and positive effects on student enrollment and attendance. We also find that SCMP eligibility implied consistently negative effects on the four school-facility measures. These results are consistent across both balanced and unbalanced samples. Overall, these results suggest that the DD results

are biased by independent trends in these outcome measures that are unique to higher-enrollment schools but unrelated to the SCMP intervention.

In light of this evidence, our preferred estimates are based on DDD specifications. These estimates are based on pooled data from schools in program and non-program districts and they control unrestrictively for time-varying determinants unique to higher and lower-enrollment schools (i.e., through fixed effects unique to each eligibility-period cell). These DDD estimates can be constructed by the "true" DD estimates in Table 5 from the "naïve" DD estimates (i.e., as the difference in two difference in differences). However, we report the key DDD estimates directly in Table 6, both for the balanced and the unbalanced samples. Across both samples, these results indicate that SCMP eligibility generated statistically significant increases in student enrollment, teacher attendance, and all four facilities measures. We also find consistent evidence of a modest but statistically significant reduction in student attendance, in all likelihood, reflecting the low attendance rates of marginal school enrollees.

The implied IV estimates suggest that these gains in school performance are meaningfully sized particularly relative to the comparatively low cost of the intervention. For example, our estimates imply that the SCMP increased school enrollment by 6.7 students (i.e., 3.905/0.584), an increase of 5.7 percent relative to the baseline mean. Similarly, our estimates imply that participation in the SCMP increased teacher attendance by 1.9 percentage points, an increase of 2.1 percent relative to the baseline mean. We find similarly sized gains for all four of the facility measures, though it should be noted that, even at baseline, most schools in the program districts met the requirements for toilets and water (i.e., roughly 99 percent). However, only 88 percent of schools met the standard for electricity at baseline and we find that SCMP participation increased this likelihood by 2.1 percentage points (i.e., 0.012/0.584) or 2.3 percent relative to the baseline mean.

These full-sample impact estimates may mask several forms of treatment heterogeneity. For example, there are several reasons to suspect that the impact of the SCMP varies by time period. School-council members may become more effective as their engagement with the call center accumulated. Furthermore, the structured engagement of the call center (Table 1) indicates that more explicit guidance around enrollment, teachers, and the use of funds began only in the latter half of Phase 1. Furthermore, it may be that the effects of the SCMP intervention faded once Phase II concluded. We examined these questions by modifying our DDD specification to

allow our impact estimates to vary by each of the three post-treatment periods described in Table 2. We report the key results from these specifications in Table 7.

The F-tests reported in column (4) of Table 7 indicate that we cannot always reject the hypothesis that the effects are the same across these periods. Nonetheless, these results indicate that the effects of the SCMP were consistently smaller and often statistically insignificant in the first post-treatment period (i.e., midway through Phase 1). However, we also find that the effects are larger in period 2 (i.e., near the end of Phase 1) and that they persist in period 3 (i.e., the four months after operations ceased). These findings indicate that the effects of the SCMP grew over time and did not immediately fade out.[24] However, at least two caveats are appropriate. First, as more data become available, a longer, post-treatment time window might find more evidence for the existence of fade-out. Second, in period 3, the estimated effect of the SCMP on student enrollment fell somewhat and the estimated effect on student attendance fell more sharply. This pattern suggests that sustaining the gains in student enrollment, in particular, may require a sustained or redesigned effort. The design of the SCMP calls did not inform SC members about how enrollment could be increased or students could be encouraged to attend school. Qualitative evidence suggests that the members either visited households door-to-door to encourage parents to send out-of-school children to school or made announcements via the local mosque, efforts that may have abated when the SCMP concluded (Cambridge Education 2014).

There are also multiple reasons to speculate that the effects of the SCMP intervention may vary by school level (i.e., primary vs. middle school) and by the gender of the students served at the school. We examine this question by presenting our key DDD estimates in Table 8 for samples defined by the school level served and the gender of the students. Interestingly, we find, for middle schools, no statistically significant effects of the SCMP on any of our 7 outcome measures. This is striking because the net enrollment rate (NER) for middle schools in the Punjab province (i.e., 25%) is substantially *lower* than the NER for primary schools (i.e., 62%; PSLM 2012-2013). This pattern also suggests that the SCMP's emphasis on meeting the Millennium Development Goal of universal primary education may have narrowed its impact towards early grades.

[24] One exception is that the teacher attendance gains are no longer statistically significant when the program concluded. However, we cannot reject the hypothesis of a common treatment effect across all periods.

Though the effects of the SCMP are concentrated in primary schools, the results in Table 8 also indicate that they varied by whether the school served boys or girls. For boys' primary schools, the effect of the SCMP on student enrollment is more modest. SCMP participation increased enrollment by 4.9 students (i.e., 3.275/0.670), an increase of 4.6 percent relative to the baseline mean. Interestingly, the increase in boys' enrollment was not accompanied by a statistically significant decline in student attendance. In boys' primary schools, the SCMP did lead to statistically significant increases in teacher attendance and in three of the four facility measures. In contrast, the SCMP only increased one of the four facility measures in girls' schools (i.e., water). This heterogeneity in facilities improvement may reflect cultural factors. The school council members for girls' schools are typically woman, who may be culturally restricted from facilitating improvements in school construction (e.g., engaging contractors, negotiating prices, etc.).[25]

However, the results in Table 8 indicate that the SCMP was particularly successful in increasing the enrollment of young girls. Specifically, these estimates imply that SCMP participation increased a school's enrollment of girls by 12.9 students (i.e., 6.816/0.527), an increase of 12.4 percent relative to baseline enrollment in girls' primary schools. However, the results in Table 8 also indicate that the declines in student attendance were concentrated among these young girls. The implied IV estimate indicates that SCMP participation reduced the student attendance rate at girls' schools by 2.1 percentage points (i.e., -0.011/0.527).

Given this pattern of enrollment and attendance results, a reasonable question is whether the gains in girls' enrollment are meaningfully attenuated by the corresponding decline in student attendance. Some simple back-of-the-envelope calculations allow us to engage this question. To begin, consider designating the girls observed in these schools as either (i) those already enrolled or (ii) those newly enrolled because of the SCMP. Then consider the empirical implications of the conjecture that the newly enrolled students actually had attendance rates of zero. Specifically, how would the overall student attendance rate change if the SCMP did not influence of already enrolled students but each of the newly enrolled 12.9 students had an attendance rate of zero? It is straightforward to show that the attendance rate would fall from 88.6 percent to 78.8 percent, a

[25] Provision of devices for storing water is incorporated in the definition of functional water facilities and these that may easily be provided by females that could explain why we may see a positive impact on this facility for female schools.

decline of 9.8 percentage points.[26] The fact that the observed reduction in student attendance attributable to the SCMP is substantially smaller than this suggests that the newly enrolled young girls were indeed attending school for an appreciable amount of time.

However, an alternative interpretation for our overall finding is that newly enrolled students had little to no school attendance while already enrolled students experienced sharp *increases* in attendance. But how large would the growth in student attendance among the already enrolled have to be in order to explain our findings? It is straightforward to show that, when newly enrolled students have zero attendance, the already enrolled students would need a 8.6 percentage point increase in attendance rates (i.e., from 88.6 percent to 97.2 percent) to explain the modest overall attendance declines we observe. The implausibility of such large and targeted effects suggests that the young girls who became newly enrolled because of the SCMP had meaningful rates of school attendance. Nonetheless, the fact that we do observe modest declines in overall school attendance also underscores the challenge of promoting sustained school engagement among young girls in this region.

7. Discussion and Conclusion

Local participation in the delivery of public services is a promising way to improve poverty targeting, build community-level social capital, increase demand for good governance, and improve outcomes for public services. However, local governance may not be effective because of low levels of literacy among the community members, information asymmetry, lack of incentives, and collective action that constrain the ability of citizens to be fully engaged in service provision (Mansuri and Rao 2014). Various interventions involving local participation have relaxed some of these constraints in order to understand the mechanisms of community engagement that do improve public service delivery. The empirical evidence is mixed and, as pointed out by Banerjee et al., (2010), it is difficult to disentangle if the mixed findings are driven by differences in the details of the interventions or context or both.

In this paper, we examine the same approach to governance reform (i.e., strengthening local participation by informing community members of their roles and responsibilities) through an intervention that has unique and compelling features. SCMP utilized low-cost mobile

[26] This calculation is based on the fact that, at baseline, girl's primary schools in the program districts had an average enrollment of 104.3 students and an average student attendance rate of 88.6 percent.

technology to regularly engage with autonomous school council members in public schools through scheduled calls via a call center. The provincial government that actively advocates the devolution of school management responsibilities to the SCs initiated the calls and the SCs have historically followed the mandate set by the government. The calls provided information on the responsibilities of the SC in Phase I of the intervention (Table 1) to encourage direct participation. This focus on participation is typical for most Community Driven Development (CDD) programs in which participation is facilitated but information on performance on outcomes is not provided (Bjorkman and Svensson 2009). In Phase II of the intervention, specific information on enrollment rates in the province, and the need to enroll out-of-school children was relayed.

Overall, the program increased student enrollment but not attendance. The impacts were statistically significant for primary schools only. The treatment-induced increases in enrollment (and the decline in attendance) were the highest for female primary schools. Females have a lower net enrollment rate to begin with, and the results suggest that girls enrolled through the program had a low propensity to attend. Teacher attendance and the likelihood of functional facilities increased but the impact was statistically significant for male-primary schools only. The novelty in program design (i.e., continuous engagement as opposed to a one-time training) appeared to relate to the change in outcomes. The impact of the SCMP grew as the cumulative experience with the phone calls grew and as the advice conveyed by the call agents became more specific. Moreover, these effects appeared to persist in the months after the program ceased operations. In all, the results suggest that the engagement mechanism did induce behavioral changes among council members that results in appreciable, though targeted, improvements in school performance. Through proactive engagement, oversight, or monitoring, the council members were able to improve enrollment (but not attendance among girls, especially), deter male teachers from absenteeism, and improve facilities for schools for young boys.

In order to situate our findings, it is important to understand the context and the nature of the interventions in prior literature that have evaluated improving service delivery through local civic engagement. First, there is some evidence that either providing information on public-sector performance or supporting civic participation is, in isolation, largely ineffective. For example, in field experiments in Uganda, Bjorkman and Svensson (2009) found that health care outcomes improved when efforts to enhance participation of community members were linked to

performance data. Similarly, Duflo et al. (2014) found that linking school-based management (SMB) training to the collection of performance data meaningfully improved school outcomes. In contrast, Banerjee et al. (2010) found that, in India, providing training and information to Village Education Committees through non-governmental organizations (NGOs) was ineffective.

In light of this literature (and the qualified success of the SCMP in improving school outcomes), at least two SCMP design features should be underscored. One is that the effectiveness of the SCMP may be due in part to the fact that it had the imprimatur of the government rather than being organized by an NGO. Second, unlike other interventions, the SCMP fostered *sustained* engagement with local SCs. The persistence of this engagement may be central to creating and sustaining the performance benefits we found in this study. The other important and compelling features of using mobile phone technology to support civic engagement concern cost and scalability. The continuous engagement conducted under the SCMP cost only PKR 5000 ($50) per school. In contrast, a prior effort to conduct one-time training of SC members in Pakistan cost more than three times as much. Furthermore, the SCMP model is likely to be substantially easier to scale up with high fidelity than more time and labor-intensive training efforts. The qualified success of the SCMP in improving local oversight and school performance, at least at the primary level, suggest that these design features merit further replication and careful study.

References

Angrist, J. D., & Pischke, J. S. (2008). *Mostly harmless econometrics: An empiricist's companion*. Princeton university press.

Azfar, O., Kahkonen, S., Lanyi, A., Meagher, P., & Rutherford, D. (1999). Decentralization, governance and public services: the impact of institutional arrangements. *Centre for Institutional Reform and the Informal Sector*.

Banerjee, A. V., Banerji, R., Duflo, E., Glennerster, R., & Khemani, S. (2010). Pitfalls of Participatory Programs: Evidence from a randomized evaluation in education in India. *American Economic Journal: Economic Policy*, 1-30.

Banerjee, A., Deaton, A., & Duflo, E. (2004). Health, health care, and economic development: Wealth, health, and health services in rural Rajasthan. *The American economic review*, *94*(2), 326.

Bardhan, P. (2002). Decentralization of governance and development. *Journal of Economic perspectives*, 185-205.

Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How Much Should We Trust Differences-in-Differences Estimates?. *Quarterly Journal of Economics*, *119*(1).

Bhatti, Z. K., Kusek, J. Z., & Verheijen, T. (2014). *Logged On: Smart Government Solutions from South Asia*. World Bank Publications.

Björkman, M., & Svensson, J. (2009). Power to the People: Evidence from a Randomized Field Experiment on Community-Based Monitoring in Uganda. *The Quarterly Journal of Economics*, *124*(2), 735–769.

Blimpo, M., Evans, D. K., & Lahire, N. (2015). Parental human capital and effective school management: evidence from The Gambia. *World Bank Policy Research Working Paper*, (7238).

Cambridge Education. (2014). *Review of Implementation of School Council Policy 2013*

Chaudhury, N., Hammer, J., Kremer, M., Muralidharan, K., & Rogers, F. H. (2004, October). Provider absence in schools and health clinics. In *Northeast Universities Development Consortium Conference, HEC Montreal, October*.

Duflo, E., Dupas, P., & Kremer, M. (2015). School governance, teacher incentives, and pupil–teacher ratios: Experimental evidence from Kenyan primary schools. *Journal of Public Economics*, *123*, 92-110.

Kremer, Mi., & Christel, V.(2005). *School Committee Empowerment: Preliminary Notes.* Mimeo, Harvard University

Liang, K. Y., & Zeger, S. L. (1986). Longitudinal data analysis using generalized linear models. *Biometrika*, *73*(1), 13-22.

Mansuri, G., & Rao, V. (2012). *Localizing development: does participation work?*. World Bank Publications.

Masud, E., 2014, Calling the Public to Empower the State: Pakistan's Citizen Feedback Monitoring Program, 2008-2014, Innovations for Successful Societies, Princeton University, http://successfulsocieties.princeton.edu/

Olken, B. A. (2007). Monitoring Corruption: Evidence from a Field Experiment in Indonesia. *Journal of Political Economy*, *115*(2).

Social, P. (2014). Living Standards Measurement Survey (PSLM) 2012-13. *Statistics Division, Government of Pakistan, Statistics Division, Pakistan Bureau of Statistics, Islamabad*.

Pandey, P., Goyal, S., & Sundararaman, V. (2006). Community Participation in Public Schools - The Impact of Information Campaigns in Three Indian States. *Policy Research Working Paper, The World Bank, Impact Evaluation Series No. 26*(4776), 1-40.

Tiebout, C. M. (1956). A pure theory of local expenditures. *The journal of political economy*, 416-424.

Stiglitz, J. E. (2002). Participation and development: Perspectives from the comprehensive development paradigm. *Review of development economics*, *6*, 163-182.

World Bank. (2004). *Making Services Work for Poor People*. World Development Report 2004.

Table 1: Structure, Content and Timeline of SCMP Calls

Call	Year	Month calls are made	Call Content
		Phase I	
1	2013	Apr 15 – May 15	Introduction to the program
2	2013	May 15 – Jun 15	Introduction to the new School Council policy
3	2013	Jun 15 – Jul 15	School council meeting
4	2013	Jul 15 – Aug 15	Process of conducting the meeting
5	2013	Aug 15 – Sep 15	Procedure of changing SC membership
6	2013	Sep 15 – Oct 15	Managing the bank account
7	2013	Oct 15 – Nov 15	Enrollment and attendance
8	2013	Nov 15 – Dec 15	Hiring of temporary teachers
9	2013/2014	Dec 15 – Jan 15	Utilization of funds and audit
10	2014	Jan 15 – Feb 15	School planning
11	2014	Feb 15 – Mar 15	Record keeping
12	2014	Mar 15 – Apr 15	Advocacy (This call was not made)
		Phase II	
1	2014	May	Introduction to the program
2	2014	Jun	
3	2014	Jul	Emphasis on Millennium Development Goal of achieving Universal Primary Enrollment
4	2014	Aug	

Source: Government of Punjab, 2014

Table 2: Timeline of SCMP and Availability of Monthly Monitoring Data

Year	Month	Calls	Data Availability
2012	Nov		Pre-treatment Period
	Dec		
	Jan		
	Feb		
	Mar		
	Apr	Phase I	
	May		
2013	Jun		
	July		
	Aug		
	Sep		Post-treatment Period 1
	Oct		
	Nov		
	Dec		
	Jan		Post-treatment Period 2
	Feb		
	Mar		
	Apr		
	May	Phase II	
2014	June		
	July		
	Aug		
	Sep		Post-treatment Period 3
	Oct		
	Nov		
	Dec		

Source: PMIU Monthly Monitoring Data, 2012-2014

Table 3: Descriptive Statistics

	Program Districts				Non-Program Districts			
	Pre-Treatment		Post-Treatment		Pre-Treatment		Post-Treatment	
VARIABLES	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Student Enrollment	117.152	88.665	131.141	100.026	135.792	116.245	145.400	124.686
Student Attendance	0.874	0.082	0.891	0.081	0.861	0.100	0.858	0.112
Teacher Attendance	0.895	0.119	0.914	0.129	0.889	0.125	0.907	0.137
Toilet Facility	0.984	0.109	0.987	0.098	0.956	0.179	0.979	0.121
Boundary Wall	0.928	0.230	0.955	0.191	0.912	0.257	0.929	0.236
Water Facility	0.998	0.024	0.991	0.072	0.987	0.076	0.991	0.069
Electricity	0.880	0.308	0.911	0.271	0.806	0.372	0.844	0.345
SCMP Eligible	0.516	0.500	0.524	0.499	0.501	0.500	0.505	0.500
SCMP Participant	0	0	0.445	0.497	0	0	0	0
Female School	0.507	0.500	0.544	0.498	0.509	0.500	0.522	0.499
Primary School	0.830	0.376	0.809	0.393	0.833	0.373	0.823	0.382
Sample Size	5,250		14,344		27,059		76,582	

Notes: The data are taken from the monthly monitoring reports of the Program Monitoring and Implementation Unit, School Education Department in Punjab, Pakistan. The estimation sample consists of school-level panel data from 26 districts monitored between November 2012 and December 2014. These statistics are from a mildly unbalanced school-by-period sample. We show results for both balanced and unbalanced samples.

Table 4: First-Stage Estimates - The Estimated Effects of SCMP Eligibility on Participation

	Difference in Differences (DD)		Triple Difference (DDD)	
	Unbalanced	Balanced	Unbalanced	Balanced
INDEPENDENT VARIABLE	(1)	(2)	(3)	(4)
SCMP Eligibility	0.577***	0.584***	0.577***	0.584***
	(0.013)	(0.014)	(0.013)	(0.012)
Eligibility X Period FE	No	No	Yes	Yes
Program District X Period FE	No	No	Yes	Yes
Eligibility X Program District FE	No	No	Yes	Yes
Observations	19,594	17,645	123,235	109,834

Notes: The dependent variable is a binary variable for whether the school participated in SCMP. All specifications include school and time-period fixed effects. Standard errors (in parentheses) are clustered at the school level.
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Difference-in-Differences Reduced-Form Estimates - The Estimated Effects of SCMP Eligibility on School Outcomes

	Program Districts		Non-Program Districts	
	Difference in Differences (DD)		Naïve Difference in Differences (DD)	
	Unbalanced	Balanced	Unbalanced	Balanced
OUTCOMES	(1)	(2)	(3)	(4)
Student Enrollment	5.000***	4.868***	0.876*	0.963*
	(0.933)	(0.964)	(0.470)	(0.493)
Student Attendance	-0.004*	-0.004*	0.003**	0.002*
	(0.002)	(0.002)	(0.001)	(0.001)
Teacher Attendance	0.010**	0.010**	0.000	-0.002
	(0.004)	(0.004)	(0.002)	(0.002)
Toilet Facility	-0.006**	-0.006**	-0.016***	-0.016***
	(0.003)	(0.003)	(0.002)	(0.002)
Boundary Wall	0.000	-0.000	-0.010***	-0.009***
	(0.004)	(0.004)	(0.002)	(0.002)
Water Facility	0.008***	0.007***	-0.004***	-0.003***
	(0.002)	(0.002)	(0.001)	(0.001)
Electricity	-0.017***	-0.014**	-0.029***	-0.026***
	(0.006)	(0.006)	(0.003)	(0.003)

Notes: Each cell is a separate regression. All specifications include school and time-period fixed effects. The standard errors (in parentheses) are clustered at the school level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 6: Triple-Difference Reduced-Form Estimates - The Estimated Effects of SCMP Eligibility on Outcomes

	Triple Difference (DDD)	
	Unbalanced	Balanced
OUTCOMES	(1)	(2)
Student Enrollment	4.066***	3.905***
	(1.041)	(1.083)
Student Attendance	-0.007***	-0.006**
	(0.003)	(0.003)
Teacher Attendance	0.010**	0.011**
	(0.005)	(0.005)
Toilet Facility	0.010**	0.009**
	(0.004)	(0.004)
Boundary Wall	0.010**	0.008*
	(0.004)	(0.005)
Water Facility	0.012***	0.010***
	(0.002)	(0.002)
Electricity	0.012*	0.012*
	(0.007)	(0.007)
Eligibility X Period FE	Yes	Yes
Program District X Period FE	Yes	Yes
Eligibility X Program District FE	Yes	Yes

Notes: Each cell is a separate regression. All specifications include school and time-period fixed effects. The standard errors (in parentheses) are clustered at the school level. The R^2 ranges from 0.427 to 0.976 in the unbalanced sample and from 0.393 to 0.975 in the balanced panel.
*** p<0.01, ** p<0.05, * p<0.1.

Table 7: Triple-Difference Reduced-Form Estimates - The Estimated Effects of SCMP Eligibility on Outcomes by Period

	Post-period 1	Post-period 2	Post-period 3	p-value
OUTCOMES	(1)	(2)	(3)	(4)
Student Enrollment	3.485***	4.731***	4.014***	0.163
	(1.076)	(1.054)	(1.268)	
Student Attendance	-0.007**	-0.002	-0.012***	0.005
	(0.003)	(0.003)	(0.003)	
Teacher Attendance	0.009	0.014**	0.008	0.568
	(0.006)	(0.006)	(0.006)	
Toilet Facility	0.005	0.015***	0.011**	0.021
	(0.004)	(0.004)	(0.005)	
Boundary Wall	0.004	0.015***	0.012**	0.063
	(0.005)	(0.005)	(0.006)	
Water Facility	0.016***	0.013***	0.006***	0.004
	(0.003)	(0.003)	(0.002)	
Electricity	0.002	0.017**	0.018*	0.076
	(0.006)	(0.008)	(0.010)	
Eligibility X Period FE	Yes	Yes	Yes	-
Program District X Period FE	Yes	Yes	Yes	-
Eligibility X Program District FE	Yes	Yes	Yes	-

Notes: Each cell is a separate regression. All specifications include school and time-period fixed effects. The standard errors (in parentheses) are clustered at the school level. There was no exposure to treatment in Period 3. The p-value in column 4 tests if estimates across the three periods statistically differ from each other. The sample is mildly unbalanced.
*** p<0.01, ** p<0.05, * p<0.1.

Table 8: Triple-Difference Reduced-Form Estimates - The Estimated Effects of SCMP Eligibility by School Level and Gender

	Primary		Middle	
	Male	Female	Male	Female
OUTCOMES	(1)	(2)	(3)	(4)
First Stage: SCMP Participation	0.670***	0.527***	0.527***	0.464***
	(0.020)	(0.021)	(0.051)	(0.046)
Student Enrollment	3.275**	6.816***	-7.930	0.323
	(1.296)	(1.588)	(4.892)	(3.557)
Student Attendance	-0.005	-0.011***	0.003	-0.007
	(0.004)	(0.004)	(0.007)	(0.007)
Teacher Attendance	0.015**	0.013	-0.007	-0.006
	(0.007)	(0.009)	(0.010)	(0.011)
Toilet Facility	0.021***	0.002	0.001	0.004
	(0.008)	(0.006)	(0.005)	(0.003)
Boundary Wall	0.025***	0.006	-0.027	0.003
	(0.009)	(0.005)	(0.017)	(0.007)
Water Facility	0.014***	0.014***	0.005	0.002
	(0.004)	(0.004)	(0.005)	(0.004)
Electricity	0.013	0.017	-0.006	0.003
	(0.012)	(0.011)	(0.013)	(0.012)
Eligibility X Period FE	Yes	Yes	Yes	Yes
Program District X Period FE	Yes	Yes	Yes	Yes
Eligibility X Program District FE	Yes	Yes	Yes	Yes

Notes: Each cell is a separate regression. All specifications include school and time-period fixed effects. The standard errors (in parentheses) are clustered at the school level.
*** p<0.01, ** p<0.05, * p<0.1.