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Mobile Phones, Civic Engagement, and School Performance in Pakistan

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A B S T R A C T

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, and student and teacher attendance, 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 (i.e., a 4.0 percent increase), and the improvements were sustained in the months after the program concluded.

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, 2012). 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 (Banerjee et al., 2010; Björkman and Svensson, 2009; Blimpo et al., 2015; Casey, 2018; Duflo et al., 2015; Mbiti, 2016; Olken, 2007; Pradhan et al., 2014).

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 out of thirty-six 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 under-provided, and often misappropriated.

We use school-level administrative data for 26 districts in Punjab, five of which were exposed to the SCMP, collected regularly by the Program Monitoring and Implementation Unit (PMIU) of the School Education Department. The school-level data are available for each of 17 months. The earliest observations (i.e., 5 monthly observations) consist of school-level data from the months just prior to the implementation of the SCMP. The next period corresponds to a time-period when the SCMP was actively engaging SC members in the field (8 months) and the last period provides us data in the months shortly after SCMP activity had concluded (4 months). Schools are segregated by sex in Pakistan. Within the five program districts, only primary and middle schools were chosen

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for the SCMP. The eligibility criteria, whereby only schools with median or higher enrollment within each district-by-level-by-sex 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 a "difference-in-difference-in-differences (DDD) design.[1]

Specifically, we identify schools as having an intent-to-treat based on a 3-way interaction (i.e., being in a treatment district in a post-treatment period and having above-median baseline enrollment). Our DDD specifications condition on unrestrictive fixed effects for each 2-way interaction between districts fixed effects, time fixed effects, and a fixed effect for being in the treatment-eligible group (i.e., above-median enrollment). We complement this analysis with the supporting evidence from event-study specifications. We also examine semi-dynamic specifications that allow the impact of the SCMP to vary over time and consider treatment heterogeneity by school (i.e., primary or middle, girls or boys).[2] While our core findings focus on the impact of program eligibility on the outcome measures (i.e., reduced-form or intent-to-treat estimates), we also discuss the impact of program eligibility on program participation both to demonstrate that the intent-to-treat created a treatment-control contrast, and to facilitate scaling the reduced-form estimates into "treatment-on-treated" estimates of the impact of program participation.

We find that SCMP eligibility increased student enrollment by 2.3 percent. With a take-up rate of 58 percent, we find that SCMP participation increased student enrollment by 4.0 percent (i.e., .023/0.58). The estimates suggest that the gains in school enrollment are meaningfully sized relative to the comparatively low-cost of the intervention (i.e., USD 50 per school for a yearlong engagement). Additionally, we find that the SCMP eligibility reduced student attendance modestly (i.e., 0.58 percentage points) and there was no statistically significant effect on teacher attendance.

We also find that the effects of SCMP eligibility on student enrollment grew in magnitude over time (i.e., in the months after the program ended), while student attendance fell sharply in the months after the program ended. These results suggest the challenge of sustaining attendance among the marginal enrollees or that there could be fade-out once the program ended (i.e., students may remain officially enrolled, but stop attending). Interestingly, the impacts of SCMP were statistically significant for primary schools only, where the increase in enrollment was the highest for female primary schools (i.e., girls aged 5-12) and the decrease in attendance was driven by boys' primary schools (i.e., boys aged 5-12). Specifically, we find that SCMP eligibility increased the enrollment of young girls by 6.5 percent. The treatment-on-treated estimates imply that SCMP participation increased the enrollment among girls by more than 12 percent (i.e., roughly 14 additional enrollees per school). On the other hand, attendance in boys' schools fell, albeit modestly, by 1.5 percentage points. Together the dynamic and heterogenous effects of the SCMP by school level and sex, and relatively low-cost of the intervention indicate that a more permanent change may need a longer and sustained effort with school councils.

Overall, our results suggest that the engagement mechanism informed council members and encouraged them to participate in school governance that improved school enrollment, especially for young girls. 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 directly targeted through the SCMP calls 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 two provides a discussion on the theoretical framework and prior literature, section three describes the School Council Mobilization Program, section four and five include a description of the data and identification strategies respectively. Section six describes our results and section seven 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 (The 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. The individuals operating government agencies, on the other hand, may have imperfect information on local preferences and may lack the capacity to respond to them (Bardhan and Mookherjee, 2006). They 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 (Eisenhardt, 1989). In these circumstances, public services may be misaligned, underprovided, or misappropriated.

An institutional design that may attenuate such problems involves the devolution or decentralization of authority for public services from centralized to local governments (Connerley et al., 2010; O'dwyer and Ziblatt, 2006; Smoke, 2015). 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 (2000, 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 (The 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 (Azfar et al., 1999; Mansuri and Rao, 2012; Patrinos et al., 2009). This may be done through oversight, monitoring, participation, or engagement in public service delivery, mechanisms which are beneficial in supporting the accountability of public sector workers who are rarely

[1] We considered but rejected a regression-discontinuity design. The lack of a crisp "first-stage" jump around the relevant enrollment thresholds weakened the credibility (and statistical power) of that design for this context. We also rejected a conventional "difference in differences" (DD) specification because of evidence we present that the SCMP, which began at the beginning of the academic year, coincided independently with enrollment and attendance trends unique to the larger schools eligible for treatment. We explain this in detail in the next sections.

[2] Additionally, we find no evidence that the effects we document (e.g., enrollment gains) are due to already enrolled students moving from treatment-ineligible to treatment-eligible schools.

held answerable for their absences or corrupt practices (Chaudhury et al., 2004; Duflo et al., 2012; Guerrero et al., 2013; Mbiti, 2016; Muralidharan et al., 2017). Local engagement may also support the tailoring of public services to the unique needs of particular communities, improve targeting of resources to alleviate constraints to delivery, improve management of the services, and overall, increase demand for good governance (Cilliers et al., 2018; Lemos et al., 2021; Glewwe and Maïga, 2011).

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 and low resources, or free-rider problems and capture by local elites may be associated with local monitoring (Bardhan, 2002; Cilliers et al., 2018; Casey, 2018; Olken, 2007). Absence of strong institutions at the central government level, lack of oversight mechanisms both formal (judiciary and legislature) and informal (civil society organizations) and inadequate administrative and technical expertise in local and national government officials may constrain the ability of citizens to be fully engaged in service provision (Azfar et al., 1999; Bardhan and Mookherjee, 2006). Also, the success of decentralization relies on an educated and politically aware citizenry and an absence of high inequality in economic and social status that inhibits political participation of the poor and minorities (Bardhan & Mookherjee, 2006; Beasley and Huillery, 2017; Leer, 2016).

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 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 (Björkman and Svensson, 2009). The impacts sustained over the long run (Bjorkman Nyqvist et al., 2017).

However, Banerjee, Deaton and Duflo (2004) in an experiment 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, found that absence rates were the same in program and control facilities. The key reason for no effects, according to the authors, 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 increased attendance at community meetings and issuance of anonymous comment forms to villagers to account for how project funds are spent, on reducing corruption overall in over 600 Indonesian village road projects. The author argues that only in circumstances where civic engagement is robust and the entire village gains from reducing corruption, grassroots monitoring can be effective.

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. (2015) 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 were empowered and received training on monitoring and reviewing teacher job applications, their contracts, and their effort. 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, results from an experiment in 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 (Panday, Goyal and Sundraraman, 2006). In Gambia, also, results of an experiment in which principals, 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). Pradhan et al. (2014) investigated the role of SCs in improving school quality in Indonesia through a large-scale experiment with two treatments. The first assisted democratic elections of SC members and the second linked SCs to the village council. The village council, which is a powerful entity, added legitimacy to the activities of the SCs on the ground and improved learning.

On the other hand, in Kenya, Kremer and Christel (2005) found that more frequent SC meetings with the school administration at the sub-district level or SCs having the responsibility to evaluate teacher performance did not reduce teacher absenteeism or improve children's performance on tests (Kremer and Christel, 2005). Similarly, Banerjee et al. (2010) found that providing information to villagers in India about the Village Education Committee (with autonomy and responsibility in school governance) 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. In both studies, information on performance on outcomes was not relayed to SC members. Furthermore, Beasley and Huillery (2017) found that grants to school committees in Niger increased participation among parents, however, teacher absenteeism increased and there was no measured impact on test scores. The authors explain that the capacity, education, resources, authority, and preferences of council members influenced why their investments did not translate into improved learning for students (Beasley and Huillery, 2017).

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 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, as opposed to NGO-led trainings) 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 because public services, especially education, are underprovided with high rates of teacher absenteeism. However, with growing mobile and internet usage, several ICT-based citizen engagement initiatives were were piloted and scaled up in the Punjab province to improve performance of services (Bhatti, Kusek, & Verheijen, 2014; Masud, 2014).[3] SCMP 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] None of these programs has a timing and placement such that they would confound this study's inferences.

Table 1
Structure, Content and Timeline of SCMP Calls.

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

Notes: In Phase I, calls were made to 5 program districts. 10 additional districts were added to the program in Phase II.
Source: Government of Punjab, 2014

3. The School Council Mobilization Program (SCMP)

The School Council Mobilization Program (SCMP) was a pilot project conducted for 16 months (April 2013-August 2014) 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 percent of the total population of Pakistan. Approximately 44 percent of Punjab's population is 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.[4] 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, was 62 percent and 73 percent at the primary level in 2012 and 2018 respectively, and only 25 percent and 45 percent at the secondary level in 2012 and 2018 respectively, with a higher proportion of out-of-school girls than boys (Government of Pakistan, 2014, Government of Pakistan, 2020).[5] The World Bank funded 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

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 percent 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 (Cambridge Education, 2014). 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 enrollment, 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 (I-SAPS 2010).[6]

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.[7] The trainings were held via community organizations in all schools between 2008 and 2011 and cost the government PKR 18,000 (USD 113) per school for a one-time group-based session (Cambridge Education, 2014). A descriptive study examined 800 SCs in the province and found that despite the capacity building program, 21 percent failed to conduct the required one meeting every month and 48 percent of the head teachers did not perceive the members to be aware of their responsibilities (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 was that, on average, 71 percent of Punjab's households at the time owned a mobile phone (Government of Punjab, 2009) and this average ownership rate was likely to be higher among those serving on school councils.

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.[8] These agents placed monthly, informational phone calls to individual SC members; each lasting approximately 6 minutes.[9] The agents added credibility to the calls by informing the members that the

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

[5] 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.

[6] Primary school councils are given PKR 20,000 (USD 125) and middle schools PKR 40,000 (USD 250) annually for spending on school maintenance, hiring of an extra teacher and providing refreshments in council meetings. The implementation review found that most school councils did not spend the money that they were allotted at the start of the fiscal year (Cambridge Education 2013).

[7] 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).

[8] Abacus Consulting, a private call center in Lahore, was hired to carry out the operations.

[9] 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.

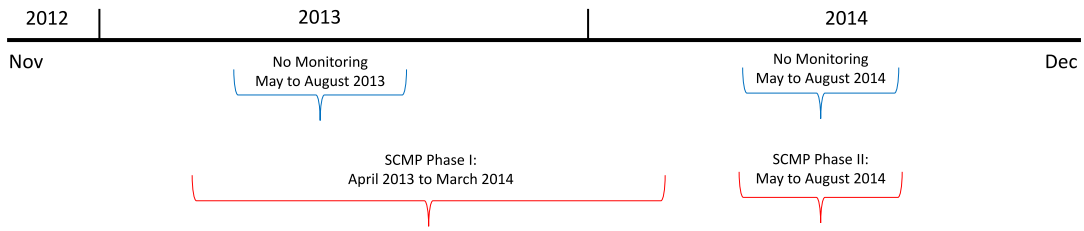

Fig. 1. Timeline of SCMP and Availability of Monthly Monitoring Data
Notes: Schools are monitored every month of the year, except May to August in Punjab, Pakistan. We have monitoring data from November 2012 to December 2014. No monitoring data were available for the summer months or December 2013. Schools were selected for the SCMP program in March 2013. The SCMP began operations in April 2013. Phase I operated between April 2013 to March 2014 and Phase II operated between May to August 2014. Annual examinations for Grade 5 and Grade 8 and official enrollments occur in March of every year and the new term begins in April.
Source: PMIU Monthly Monitoring Data, 2012-2014

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 15 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 were 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.[10] The agents also shared data on the number of out-of-school children in the district and province and discussed ways to improve enrollment numbers (i.e., through door-to-door campaigns or announcements at a local mosque) with the SC members.

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).[11] All of the schools in the province are segregated by sex 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-sex 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. However, the take-up of the SCMP intervention was not in full compliance with the eligibility rules. This occurred for several reasons. 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 with lower enrollment to the sample because they had SC members with valid phone numbers.

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). Membership categories were revised, and additional members added could be grandparents or siblings of existing students, local mosque representatives, retired teachers, and local elected leaders However, according to the SCMP implementation review (2013), districts were given only three days to carry out the reconstitution before call center operations began, hence no elections were held. The head-teachers mostly added approximately two members to the existing list of council members when needed. We do not have formal evidence if elections were held in 2013 or the following year. We also do not have data on if, how many, and who the added members were. Given the modest change in SC 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 8000 (USD 50) per school for a yearlong engagement with SC members. The earlier NGO-delivered trainings, which were delivered in person, cost nearly twice 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 school-level administrative data collected regularly by the Program Monitoring and Implementation Unit (PMIU) of the School Education Department in Punjab. Approximately 900 monitoring and evaluation assistants (MEAs), hired by Punjab's provincial government, administer a monthly survey in all

[10] 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.

[11] 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.

Table 2
Descriptive Statistics.

VARIABLES	Program Districts	Non-Program Districts	p-value
Student Enrollment	119.08	137.74	0.000
	(91.14)	(118.13)	
Student Attendance	87.75	86.65	0.000
	(12.51)	(13.81)	
Teacher Attendance	89.70	89.02	0.000
	(19.79)	(20.84)	
SCMP Eligible	0.52	0.55	-
	(0.50)	(0.50)	
SCMP Participant	0.44	_	-
	(0.50)		
Female School	0.52	0.52	0.190
	(0.50)	(0.50)	
Primary School	0.83	0.83	0.195
	(0.38)	(0.37)	
N= total districts	5	21	
N= total schools	5,252	26,891	
N= school by month observations at baseline	22,673	117,848	
N= school by month observations in the overall sample	77,366	399,069	

Notes: The data are taken from the monthly monitoring reports of the Program Monitoring and Implementation Unit, School Education Department in Punjab, Pakistan. The table shows conditional means by district type (program and non-program) using school-level data. Standard deviations are in parenthesis. The statistics are from an unbalanced school-by-month sample for 5 months of monitoring pre-SCMP. The p-value tests level differences in means at baseline.

36 districts in approximately 54,000 public schools.[12] The MEAs fill out the survey by adding enrollment numbers to it from the school records, and record teacher attendance and student attendance, as a percentage of teacher and student totals, on the day of their random visit to the school every month.

We use data for 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 but excludes 10 districts because they were added to the pilot near the end of our study window. Our monthly data from these 26 districts span the period from November 2012 to December 2014. We merge these monthly school records overtime and use the natural log of the enrollment variable for our analysis (i.e., given the skewness in school sizes) and use percentages for student and teacher attendance as reported by the MEAs as our outcome measures.

The timeline for the intervention and corresponding data collection is summarized in Fig. 1. It should be noted that the new school term begins in April and in 2013, this start to the school term coincided with the onset of the SCMP. There is no monitoring in June and July and less than 50 percent of the schools are monitored in May and August because of summer vacations from mid-May to mid-August. We therefore exclude the summer months as we construct our data. Our analysis includes 17 time periods in total: 5 months of pre-SCMP data (November 2012 to March 2013) and 12 months of post-intervention data (April 2013 to December 2014).[13] This data structure is an appropriate one for examining dynamic and post-treatment effects. That is, the 8 months of monitoring data correspond to a time-period when the SCMP was actively engaging SC members in the field and 4 months of monitoring data give 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 relied on monthly monitoring reports of schools collected by the Program Monitoring and Implementation Unit in Punjab to identify eligible schools and to select program participants in the five program districts.[14] In these five program districts, a school was identified as SCMP eligible if it had median or higher enrollment in its district-by-level-by-sex cell. 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-sex cells.

Our final analytical sample consists of 32,143 unique schools and 476,435 school-by-month observations. Table 2 presents conditional means by district type (program and non-program) using school-level data, along with p-values for baseline differences. By design, slightly more than 50 percent of the schools were eligible to receive the program in program districts (or would have been eligible in non-program districts). Forty-four percent of the schools in the 5 program districts participated in the SCMP, i.e., received calls in the first month of the intervention (April 2013).[15] There are roughly the same numbers of male and female schools in the sample. However, roughly 82 percent of these schools are primary rather than middle schools. We also see that program districts have slightly smaller schools (approximately 119 students compared with 138 students per school) and somewhat higher percentage of student attendance (approximately 87.8 percent compared with 86.7 percent). These differences reflect both baseline differences across these districts as well as the program impact we study. In the next section, we describe more formally the research designs we use to examine such questions.

Identification Strategies

Our quasi-experimental approach to identifying the impact of the SCMP on school outcomes leverages both the existence of school-by-month panel data across districts with and without the intervention and our knowledge of school eligibility for the treatment. Program eligibility within participating districts was determined at the *school* level with the intent-to-treat (ITT) being based on school size. Specifically, within each district, schools were organized into four cells (i.e., primary or middle levels crossed by sex). Within those district-specific 2 $\times$ 2 cells, only the schools with median or higher enrollment were eligible to receive phone-calls through the SCMP. We define treatment eligibility based on this assignment rule. That is, the ITT is a binary

[12] 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.

[13] The data file for December 2013 was corrupted and, therefore, excluded from our analysis.

[14] From this sample of 5,269 schools, first, we excluded schools that had no reported enrollment at baseline (n=10) in program districts. None of these schools participated in the SCMP pilot. We believe these to be schools that closed prior to our study window, but had residual entries in the publicly available data files. Second, we deleted schools that had zero enrollment in each of the five time periods prior to the implementation of the SCMP program (n = 7 in the program districts). These schools opened after the SCMP began operations. They were, therefore, ineligible to participate in the program. We also note a relevant edit to the data preceded our sample construction. We found that some schools in the monitoring data (i.e., n = 135 school-by-month observations in program districts) report zero enrollment while also reporting positive student or teacher attendance rates. In these instances, we believe that the enrollment is not a true zero and we converted the reported enrollment from zero to missing values.

[15] Schools that received calls in the first month subsequently received all calls in the proceeding months for the duration of the program, however we have participation data for the first month only.

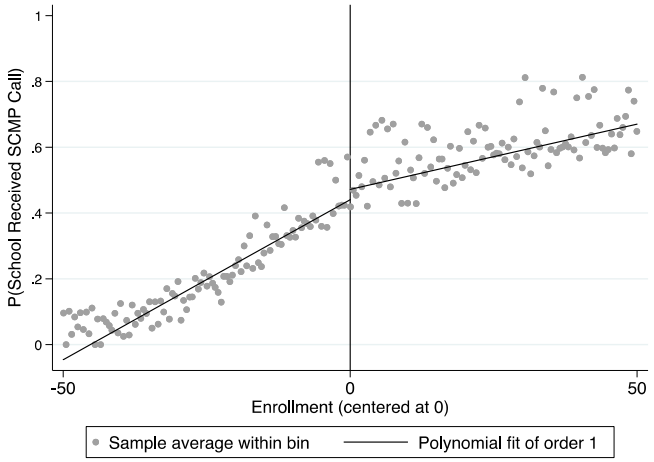

Fig. 2. Probability of Receiving the SCMP, First Stage Regression Discontinuity Estimates.

indicator equal to one for schools at or above median baseline school enrollment within their district-level-sex cell observed within a treatment district and in the post-treatment period.

As our first design choice, we extensively explored a (fuzzy) regression discontinuity (RD) design. We used school enrollment as the forcing variable and the median enrollment (for each district-level-sex cell) as the assignment variable to compare schools on either side of the threshold. Fig. 2 shows the lack of a crisp "first-stage" jump around the relevant enrollment thresholds, which weakened the credibility (and statistical power) of the fuzzy RD design in this context. While the probability of treatment increased in baseline enrollment, as expected, it did not jump discontinuously at the relevant threshold.

This paper, therefore, estimates the effect of the ITT on both SCMP take-up and on key school outcomes using a DDD ("difference-in-difference-in-differences") design that leverages (1) the pre/post panel nature of the available data from monthly school monitoring reports, (2) the definition of the ITT, and (3) available data from other districts in the province where the SCMP was not active. The DDD relies on the change *within* SCMP-eligible schools relative to neighboring schools that are not SCMP-eligible, and leverages the parallel data from districts without the SCMP to purge this estimate from biases due to possible parallel-trends violations. More specifically, the DDD approach is based on the following specification:

$$Y_{igt} = T_t + P_g + \lambda_t + (T_i \times \lambda_t) + \left(T_t \times P_g\right) + \left(P_g \times \lambda_t\right) + \beta\left(T_i \times P_g \times POST_t\right) + \varepsilon_{igt} \quad (2)$$

Where Y_{it} is an outcome for school i observed in period t, T_i, is treatment eligibility for school i (i.e., whether its baseline enrollment was at or above the median in its district-level-sex cell), P_g is a dummy variable that identifies schools in the five program districts where the intervention was fielded, and λ_t, represents period fixed effects. In the DDD approach, the coefficient of interest β 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 fixed effects unique to each 2-way interaction (i.e., district-by-time, district-by-eligibility, time-by-eligibility). These covariates indicate outcome trends across eligible and ineligible schools and about the level differences in outcomes across districts. We 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. Our preferred specification clusters standard errors at the school level to address the potentially spurious effects of serial correlation (Bertrand et al. 2004).[16,17]

Additionally, we estimate the take-up rate of the intervention, using the DDD specifications in which SCMP participation is the dependent variable. While we focus on the reduced form estimates in the analysis, we scale the effects by the compliance rate to get "treatment-on-treated" (TOT) estimates of program impact.[18]

Robustness Checks

Our data suggests that lower levels of student enrollment and attendance generally characterize the beginning of a school year.[19] If the enrollment and attendance changes at the beginning of the school year

[16] We also cluster standard errors at the district-month level to accommodate errors nested across all schools in a district at a moment in time. The results are presented in Table A6 in the Appendix.

[17] We acknowledge the new evidence that has emerged on using difference-in-differences designs in settings where there are multiple time-periods and variation in treatment timing. The evidence suggests that the traditional DD approach may not be appropriate if the treatment effect is not common across units and over-time and recommends ways to appropriately estimate impact (deChaisemartin and D'Haultfoeuille, 2019; Goodman-Bacon, 2018). We note that, in this application, all schools received the SCMP program at a single point in time and we present results that allow for dynamic treatment effects.

[18] We view a school i's treatment eligibility, Ti, interacted with being observed in the post-treatment period and in an SCMP district as an instrumental variable (IV) for whether it participated in the SCMP treatment. We scale the effects by the compliance rate to get "treatment-on-treated" (TOT) estimates of program impact. The TOT estimates rely on identifying assumptions, including the validity of the instrument. We show that our instrument, i.e., treatment eligibility, is correlated with the endogenous predictor, i.e., participation in the SCMP in Table 3. And that the instrument and the residuals are uncorrelated, i.e., we have reason to believe that there is no direct path from the instrument to the outcome, except through the endogenous predictor. The IV estimates in this case summarize the behavior of compliers i.e., the schools that were eligible for the SCMP and received the SCMP calls, and schools that were not eligible and did not receive the SCMP calls.

[19] For example, the mean enrollment in our sample in April 2013 and April 2014 (i.e., the first month at the start of the school-year) is 132 and 135 students respectively, compared to 147 and 146 students in October 2013 and October 2015, when the school is in full-session.

Table 3
The Estimated Effects of SCMP Eligibility on SCMP Participation.

	Sample	
	Unbalanced	Balanced
SCMP Eligibility	0.583***	0.602***
	(0.013)	(0.021)

Notes: The dependent variable is a binary variable for whether the school participated in SCMP. Each cell is a separate DDD regression. We control for month-year effects unique to treatment eligible schools across all districts, month-year effects unique to the program status of a district, and school-fixed effects. Standard errors (in parentheses) are clustered at the school level. ***p<0.001, ***p<0.01, * p<0.05, ~ p<0.1; Balanced panel includes schools that report the outcome measure in all 17 time periods.

Table 4
The Estimated Effects of SCMP Eligibility on School Outcomes.

OUTCOMES	Baseline Mean | ITT=1 (1)	Estimate (2)	N (3)
ln(Student Enrollment)	4.89	0.023**	471029
		(0.007)	
Student Attendance	88.5	-0.580**	471027
		(0.225)	
Teacher Attendance	89.2	0.169	459966
		(0.398)	
Eligibility X Month-Year FE		Yes	
Program District X Month-Year FE		Yes	
Eligibility X Program District FE		Yes	

Notes: Column (1) reports the pre-treatment means of the outcomes in program schools. Column (2) represents the Triple Difference estimates. Each cell is a separate regression. All specifications include school and month-year fixed effects. The standard errors (in parentheses) are clustered at the school level ***p<0.001, ***p<0.01, *p<0.05, ~p<0.1; Column (3) reports the sample size for the regression. The estimations are from an unbalanced panel.

Table 5
The Estimated Effects of SCMP Eligibility on Outcomes by Period.

OUTCOMES	Post Period 1 (1)	Post Period 2 (2)	p-value (3)
ln (Student Enrollment)	0.020**	0.031***	0.052
	(0.007)	(0.009)	
Student Attendance	-0.307	-1.175***	0.000
	(0.233)	(0.290)	
Teacher Attendance	0.213	0.072	0.739
	(0.420)	(0.493)	

Notes: Each cell is a separate DDD regression. We control for month-year effects unique to treatment eligible schools across all districts, month-year effects unique to the program status of a district, and school-fixed effects. The standard errors (in parentheses) are clustered at the school level. Post Period 1 includes 8 months of monitoring data when SCMP is actively engaged with schools (Apr 2013-Apr 2014, excluding summer months and Dec 2013 for which data are not available). Post period 2 includes 4 months after SCMP stops engaging with school council members (Sept-Dec 2014). The p-value tests if estimates across the two periods statistically differ from each other. ***p<0.001, ***p<0.01, * p<0.05, ~ p<0.1. The sample is unbalanced.

differ across larger and smaller schools, a conventional difference-in-differences (DD) design, based on data from five program districts only, would confound the true impact of the SCMP with factors unique to the start of the school year in larger schools. We examined event-study specifications to understand if a DD analysis would confound the true impact of the SCMP. Our unrestrictive event-study specifications use the basic two-way FE structure but have unrestrictive fixed effects for ITT=1 schools in each period before the onset of treatment and for each period after (i.e., with districts 5 months before treatment or never eligible as the reference category). We present the key results from these event-study specifications in the Appendix in Tables A1 and A2 and visually in Figs. A1–A3.

Critically, these event-study results suggest that the onset of the school year resulted in sharp changes in these outcomes, even in districts where the SCMP was not implemented. This collinearity (i.e., between the onset of the SCMP and the start-of-the-year changes unique to larger schools, even in non-SCMP districts) implies that a DD analysis is likely to result in biased estimates. We present reduced-form DD estimates of the impact of being a treatment-eligible school in a program-district (i.e., where the SCMP happened) in column (1) of Table A3, and DD estimates of the impact of being a treatment-eligible school in a non-program district (i.e., where the SCMP did not happen, but we use the assignment rule to estimate the effect of treatment eligibility) in column (2) of Table A3 in the Appendix. The results suggest that our outcome measures changed in not just SCMP eligible schools, but also among larger schools where the SCMP was not active. For these reasons, our analysis privileges the DDD results, which condition on unrestrictive fixed effects unique to the interaction between each time period and whether a school is larger or smaller.

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 schools 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, similar demographics, and the fact that they share provincial governance, this assumption has some face validity to it. The similarity the pre-treatment trends unique to larger schools in both program and non-program districts (see Figs. A1-A3) also suggests the validity of this maintained assumption.

Another internal-validity concern in our identification strategy arises from the fact that the school-by-month 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). Focusing on our core educational outcomes (student enrollment and attendance, and teacher attendance), we see that roughly 13.3 percent of the potential school-month observations from program districts have missing values. In the non-program districts, the rate of missingness averages 12.7 percent.[20] 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. Fortunately, auxiliary DD and DDD specifications in which missingness is the dependent variable indicate that missingness is unrelated to SCMP eligibility conditional on the other fixed effects (Table A4). As a complement to the concerns raised by missingness, we also report both DD and DDD results based on a smaller, "balanced" sample of schools (i.e., each school observed in each month) in Table A5 in the Appendix.

Results

We begin presenting our results by examining the effects of SCMP eligibility on school participation in the SCMP intervention. Specifically, Table 3 presents such "first-stage" estimates from DDD specifications in which SCMP participation is the dependent variable. The results

[20] We believe the missingness of some monthly reports reflects the limited capacity of the monitors who typically use motorbikes to visit 4 to 5 schools per day. Given the distances involved, these monitors are often unable to visit each school in each month.

[21] The rate of missingness is the same for middle (14.1percent) and primary schools (14.2percent). It is higher for male schools (16.7percent) compared to female schools (11.8percent).

Table 6
The Estimated Effects of SCMP Eligibility by School Level and Gender.

OUTCOMES	Primary			Middle		
	Girls	Boys	p-value	Girls	Boys	p-value
ln(Student Enrollment)	0.065***	-0.013	0.000	0.007	-0.010	0.108
	(0.010)	(0.009)		(0.011)	(0.012)	
Student Attendance	-0.265	-1.004***	0.008	-0.113	-0.937~	0.153
	(0.297)	(0.285)		(0.520)	(0.560)	
Teacher Attendance	0.212	0.765	0.288	0.971	-1.378~	0.538
	(0.549)	(0.518)		(0.730)	(0.720)	

Notes: We control for month-year effects unique to treatment eligible schools across all districts, month-year effects unique to the program status of a district, and school-fixed effects. The standard errors (in parentheses) are clustered at the school level. The p-value tests if estimates between girls' and boys' schools for each school level statistically differ from each other; ***p<0.001, ***p<0.01, *p<0.05, ~p<0.1.

consistently indicate (i.e., across balanced as well as unbalanced samples) that eligibility increased SCMP participation by a substantial amount: 58 percent. In Table 4, we begin examining whether SCMP eligibility (and the implied sharp uptake in SCMP participation) similarly influenced our outcome measures, using DDD specifications based on schools where the SCMP was implemented.

The DDD estimates in Table 4 are based on pooled data from schools in program and non-program districts controlling unrestrictedly for time-varying determinants unique to higher and lower-enrollment schools (i.e., through fixed effects unique to each eligibility-period cell). Column (1) reports the pre-treatment means of the outcomes in program schools, column (2) reports the estimates and column (3) reports the sample size for an unbalanced panel. These results indicate that SCMP eligibility generated a statistically significant increase of 2.3 percent in student enrollment, and a reduction of 0.58 percentage points in student attendance (or 0.65 percent from baseline student attendance). There is no estimated effect of SCMP on teacher attendance. If we scale these estimates by the take-up rate to estimate the impact of treatment-on-treated, we find that the SCMP increased school enrollment by 4.0 percent (i.e., 0.023/0.58) and reduced student attendance by 1 percentage points (i.e., 0.58/0.58) or 1.1 percent from baseline student attendance. The estimates suggest that the gains in school enrollment are meaningfully sized particularly relative to the comparatively low-cost of the intervention.[22]

These full-sample impact estimates may mask several forms of treatment heterogeneity. For example, there are 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. Additionally, 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 two post-treatment periods (Phase 1 from April 2013-April 2014 when SCMP is actively engaged with SC members, and Phase II from September 2014-December 2014, four months after the operations ceased). We report the key results from these specifications in Table 5.

The F-tests reported in column (3) of Table 5 indicate that we cannot reject the hypothesis that the effects are the same across these periods for student enrollment and attendance. The results indicate that the effects of the SCMP were smaller in the first post-treatment period. However, we find that the enrollment effects persist in period 2 and in fact, are larger in magnitude from the first post-treatment period (2.0 percent in period 1 compared to 3.1 percent in period 2). These findings indicate that the effects of the SCMP grew over time and did not immediately fade out. But the estimated effect on student attendance fell sharply (1.2 percentage points). This pattern suggests that sustaining the gains in student enrollment, in particular, may require a sustained or redesigned effort.

There are also reasons to speculate that the effects of the SCMP intervention may vary by school level (i.e., primary vs. middle school) and by the sex of the students served at the school. We examine this question by presenting our key DDD estimates in Table 6 for samples defined by the school level served and the sex of the students. Interestingly, we find, for middle schools, no statistically significant effects of the SCMP on any of our outcome measures. This is striking because the net enrollment rate (NER) for middle schools in the Punjab province at the time (i.e., 25 percent) was substantially *lower* than the NER for primary schools (i.e., 62 percent; Government of Pakistan, 2014). 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.

Though the effects of the SCMP are concentrated in primary schools, the results in Table 6 also indicate that they varied by whether the school served boys or girls. The F-test reported in column (3) suggest that differences in effects in student enrollment and attendance were statistically significant between boys and girls. SCMP eligibility increased school's enrollment of girls by 6.5 percent (while SCMP participation increased enrollment by 12.0 percent),[23] but SCMP eligibility had no effect on enrollment for boys. Interestingly, the increase in girls' enrollment was not accompanied by a statistically significant decline in student attendance, suggesting that the enrolled young girls were attending school for an appreciable amount of time. However, the results in Table 6 also indicate that the declines in student attendance were concentrated among young boys. The implied IV estimate indicates that SCMP participation reduced the student attendance rate at boys' schools by 1.5 percentage points (i.e., -1.00/0.66). While the magnitude is small, the results underscore the challenge of promoting sustained school engagement among young boys in the region. The results should also be understood in light of the dynamic treatment effects, which suggest that attendance for boys fell rapidly after the SCMP program ceased operations. Together these results indicate the need for a more sustained, and perhaps redesigned effort for boys' schools.

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

[22] For completeness, we briefly describe what the covariates (time fixed-effects, district-eligibility fixed effects, and time-eligibility fixed-effects) indicate in our DDD estimation. The effects indicate negative significant trends in enrollment across eligible and ineligible schools, but modestly positive and significant effects in student and teacher attendance. Additionally, the covariates indicate positive and significant level differences in enrollment and student attendance across districts.

[23] The first stage estimates suggest that the rate of compliance for the intervention is 54 percentage points for girls' primary schools and 66 percentage points for boys' primary schools.

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 (Bardhan and Mookherjee, 2006; Beasley and Huillery, 2017; Mansuri and Rao, 2012; Pradhan et al., 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 study, 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 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 decentralization 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 (Casey, 2018). 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 modestly reduced student attendance. The impacts were statistically significant for primary schools only. The treatment-induced increases in enrollment were the highest for female primary schools, who have a lower net enrollment rate to begin with. Specifically, we find that SCMP eligibility increased the enrollment of young girls by 6.5 percent, while attendance in boys' schools fell, albeit modestly, by 1.5 percentage points. The treatment-on-treated estimates imply that SCMP participation increased the enrollment among girls by roughly 14 additional enrollees per school. 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 enrollment effects appeared to persist in the months after the program ceased operations, while attendance dropped suggesting that a longer intervention may be required for permanent change in outcomes. In all, the results suggest that the engagement mechanism did induce behavioral changes among council members that resulted in appreciable, though targeted improvements in school performance. Through proactive engagement, oversight, or monitoring, the council members were able to improve enrollment.

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 enrollment), at least three 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 with focus on a single task to improve enrollment. The persistence of this engagement may be central to creating and sustaining the performance benefits we found in this study. Third, in the second phase of the program, the SCMP calls provided information on performance (i.e., the provincial and district enrollment figures which needed improvement), and ways to improve them (i.e., through door-to-door campaigns or announcements at a local mosque). The specificity of information shared may be key to inducing a change in behavior among the SCs. We anticipate that these three features combined may have induced the SCMP to work on the intensive margin i.e., mobilize SC members and make them perform better. We cannot, however, rule out the possibility that the SCMP pilot may have worked on the extensive margin as well. That is, the SCMP effort may have led to SCs being formed or establishing full membership once the government started engaging with schools to collect phone numbers for the SC pilot. However, we have limited information on how and when the SCs were formed, their composition, and the total number of members at the time of the SCMP pilot to validate this claim.

Lastly, 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 8000 (USD 50) per school. In contrast, a prior effort to conduct one-time training of SC members in Pakistan cost more than twice as much. The SCMP model is likely to be substantially easier to sustain in the long-run and scale up with high fidelity than more time and labor-intensive training efforts for a lasting improvement in outcomes. 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.

Authorship Contributions

We certify that we have participated in the concept, design, analysis, writing and revision of the manuscript and that this material or similar material has not been and will not be submitted to or published in any other publication before hearing back from the Economics of Education Review.

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Appendix

Table A1
Event Study Estimates for Program Districts.

Variable	ln(Student Enrollment)	Student Attendance	Teacher Attendance
lead4	-0.010	-0.235	1.107
	(0.007)	(0.441)	(0.802)
lead3	-0.021*	-0.320	1.034
	(0.008)	(0.459)	(0.824)
lead2	-0.032***	-2.049***	-0.838
	(0.010)	(0.487)	(0.927)
lead1	-0.021*	-2.518***	-0.606
	(0.010)	(0.592)	(0.917)
lag0	-0.051***	-0.478	-0.471
	(0.010)	(0.530)	(0.881)
lag1	-0.038***	-0.666	0.870
	(0.011)	(0.470)	(0.869)
lag2	-0.038***	-0.949~	0.968
	(0.011)	(0.526)	(0.907)
lag3	-0.042***	-1.209*	0.604
	(0.011)	(0.470)	(0.889)
lag4	-0.043***	-0.794~	2.733**
	(0.011)	(0.460)	(0.891)
lag5	-0.046***	-1.990***	0.957
	(0.012)	(0.490)	(0.961)
lag6	-0.048***	-2.421***	-0.386
	(0.012)	(0.614)	(0.953)
lag7	-0.068***	-2.511***	0.976
	(0.012)	(0.515)	(0.934)
lag8	-0.089***	-1.163*	1.632~
	(0.012)	(0.516)	(0.871)
lag9	-0.085***	-1.585**	0.430
	(0.012)	(0.529)	(0.866)
lag10	-0.085***	-1.806***	1.013
	(0.012)	(0.471)	(0.842)
lag11	-0.088***	-1.720***	1.446
	(0.012)	(0.504)	(0.910)
N	76,835	76834	74897

Notes: The standard errors (in parentheses) are clustered at the school level. ***p<0.001, ***p<0.01, * p<0.05, ~ p<0.1; The estimations include school and month-year fixed effects.

Table A2
Event Study Estimates for Non-Program Districts.

	ln(Student Enrollment)	Student Attendance	Teacher Attendance
lead4	0.006*	-0.398*	-0.126
	(0.003)	(0.199)	(0.376)
lead3	0.011***	-0.164	-0.177
	(0.003)	(0.214)	(0.384)
lead2	0.004	-0.665**	-1.689***
	(0.003)	(0.226)	(0.423)
lead1	0.002	-1.299***	-1.054**
	(0.003)	(0.239)	(0.407)
lag0	-0.034***	-0.516*	-0.123
	(0.004)	(0.237)	(0.414)
lag1	-0.040***	0.222	0.467
	(0.004)	(0.224)	(0.412)
lag2	-0.044***	0.416~	-0.106
	(0.004)	(0.245)	(0.414)
lag3	-0.041***	0.212	-0.335
	(0.004)	(0.232)	(0.394)
lag4	-0.037***	-0.368	-0.082
	(0.004)	(0.232)	(0.399)
lag5	-0.044***	-1.369***	-0.370
	(0.005)	(0.244)	(0.431)
lag6	-0.046***	-1.506***	-1.212**
	(0.005)	(0.295)	(0.419)
lag7	-0.060***	-0.980***	-0.243
	(0.005)	(0.234)	(0.401)
lag8	-0.094***	0.505*	0.251
	(0.005)	(0.239)	(0.413)
lag9	-0.092***	0.100	0.359
	(0.005)	(0.252)	(0.414)
lag10	-0.095***	0.110	0.011
	(0.005)	(0.225)	(0.393)
lag11	-0.097***	0.056	0.294
	(0.005)	(0.232)	(0.403)
N	394194	394193	385069

Notes: The standard errors (in parentheses) are clustered at the school level. ***p<0.001, ***p<0.01, * p<0.05, ~ p<0.1; The estimations include school and month-year fixed effects.

Table A3
The Estimated Effects of SCMP Eligibility on School Outcomes, Difference-in-Differences

	Difference in Differences (DD): Program Districts (1)		Difference in Differences (DD): Non-Program Districts (2)	
OUTCOMES	Est.	N	Est.	N
ln (Student Enrollment)	-0.041***	76835	-0.065***	394194
	(0.007)		(0.003)	
Student Attendance	-0.284	76834	0.298**	394193
	(0.201)		(0.101)	
Teacher Attendance	0.747*	74897	0.563***	385069
	(0.361)		(0.167)	

Notes: Each cell is a separate regression. All specifications include school and month-year fixed effects. The standard errors (in parentheses) are clustered at the school level; ***p<0.001, ***p<0.01, *p<0.05, ~p<0.1.

Table A4
The Estimated Effects of SCMP Eligibility on Missingness in Outcomes.

OUTCOMES	Difference in Differences (DD) Program Districts (1)	Difference in Differences (DD) Non-Program Districts (2)	Triple Difference (DDD) (3)
Overall	-0.005	0.003	-0.008
	(0.007)	(0.003)	(0.007)
Student Enrollment	-0.007	0.002	-0.009
	(0.007)	(0.003)	(0.007)
Student Attendance	-0.007	0.002	-0.009
	(0.007)	(0.003)	(0.007)
Teacher Attendance	-0.010	-0.002	-0.007
	(0.007)	(0.003)	(0.008)
N	89,284	457,130	546,414

Notes: Each cell is a separate regression. All specifications include school and month-year fixed effects. The standard errors (in parentheses) are clustered at the school level. ***p<0.001, ***p<0.01, * p<0.05, ~ p<0.1; The dependent variable is a binary variable for "missingness" for each outcome after edits to the data in both program and non-program districts.

Table A5
The Estimated Effects of SCMP Eligibility on School Outcomes, Balanced Panel.

	Difference in Differences (DD): Program Districts (1)		Difference in Differences (DD): Non-Program Districts (2)		Triple Difference (DDD) (3)	
OUTCOMES	Est.	N	Est.	N	Est	N
ln (Student Enrollment)	-0.059***	22032	-0.078***	99671	0.024~	122364
	(0.013)		(0.006)		(0.014)	
Student Attendance	-0.408	22032	-0.159	93585	-0.249	115617
	(0.372)		(0.198)		(0.421)	
Teacher Attendance	0.865	21233	-0.119	96118	0.984	117351
	(0.628)		(0.316)		(0.703)	
Eligibility X Month FE	No		No		Yes	
Program District X Month FE	No		No		Yes	
Eligibility X Program District FE	No		No		Yes	

Notes: Each cell is a separate regression. All specifications include school and month-year fixed effects. The standard errors (in parentheses) are clustered at the school level. ***p<0.001, ***p<0.01, *p<0.05, ~ p<0.1; The estimations are from a balanced panel, which includes schools that report the outcome measure in all 17 time periods.

Table A6
The Estimated Effects of SCMP, Standard Errors Clustered at the District and District-Month Level.

OUTCOMES	(1)	(2)
ln(Student Enrollment)	0.023~	0.023***
	(0.013)	(0.006)
Student Attendance	-0.580	-0.580
	(0.404)	(0.365)
Teacher Attendance	0.169	0.169
	(0.472)	(0.428)

Notes: Each cell is a separate regression. All specifications include school and month-year fixed effects. The standard errors (in parentheses) in column (1) are clustered at the district level and in column (2) at the district-month level. ***p<0.001, ***p<0.01, * p<0.05, ~ p<0.1.

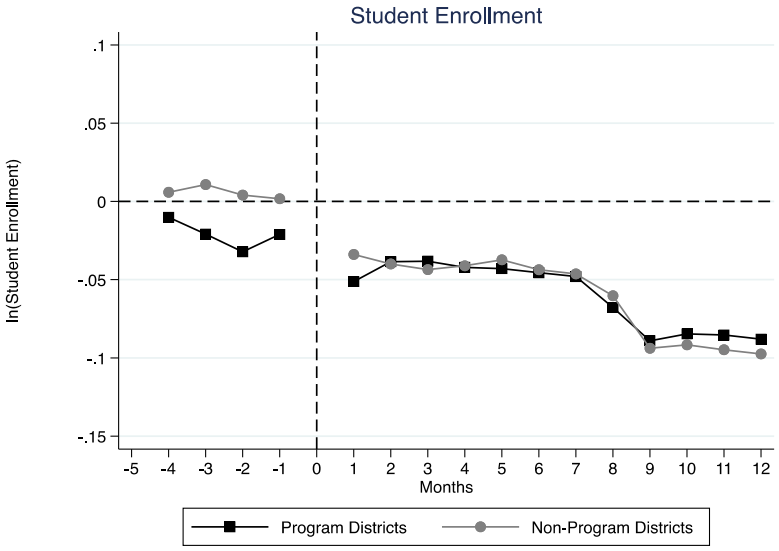

Fig. A1. Event Study Estimates for Student Enrollment.

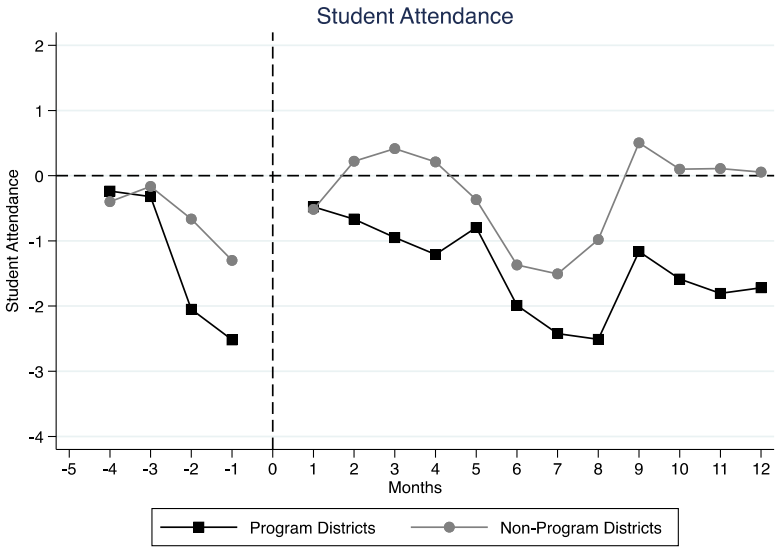

Fig. A2. Event Study Estimates for Student Attendance.

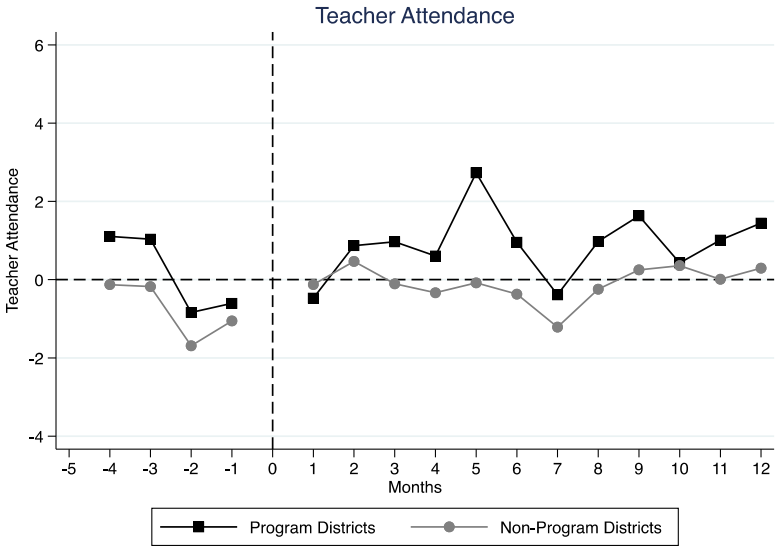

Fig. A3. Event Study Estimates for Teacher Attendance.

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