"Call Me Educated: Evidence from a Mobile Monitoring Experiment in Niger"

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Abstract. In rural areas of developing countries, education programs are often implemented through community teachers. While teachers are a crucial part of the education production function, observing their effort remains a challenge for the public sector. This paper tests whether a simple monitoring system, implemented via the mobile phone, can improve student learning as part of an adult education program. Using a randomized control trial in 160 villages in Niger, we randomly assigned villages to a mobile phone monitoring component, whereby teachers, students and the village chief were called on a weekly basis. There was no incentive component to the program. The monitoring intervention dramatically affected student performance: During the first year of the program, reading and math test scores were .11-.30 s.d. higher in monitoring villages than in non-monitoring villages. These effects were relatively stronger in the region where, absent our intervention, in-person monitoring was weakest. The effects were also stronger for teachers for whom the outside option was lowest. We provide more speculative evidence on the mechanisms behind these effects, namely, teacher and student effort and motivation.

JEL codes: D1, I2, O1, O3

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In rural areas of developing countries, public worker absence – of teachers, doctors, nurses or agricultural extension agents – is a widespread problem. In West Africa, teacher absenteeism is estimated between 27-40% (Transparency International 2013). Despite numerous interventions to overcome the monitoring problem, such as community-based monitoring, "para-teachers", audits or other incentives, teacher monitoring continues to be a significant challenge. This is particularly the case in countries with limited infrastructure and weak institutions, where the costs of monitoring are particular high.

The introduction of mobile phone technology throughout sub-Saharan Africa has the potential to reduce the costs associated with monitoring public employees, such as teachers. By allowing governments and organizations to communicate with remote villages on a regular basis, "mobile monitoring" has the potential to increase the observability of the agents' effort. Similarly, reductions in communication costs associated with mobile phone technology could potentially increase community engagement in the monitoring process, thereby providing the community with additional bargaining power.

We report the results of a randomized monitoring intervention in Niger, where a mobile phone monitoring component was added to an adult education program. Implemented in 160 villages in two rural regions of Niger, students followed a basic adult education curriculum, but half of the villages also received a monitoring component – weekly phone calls to the teacher, students and village chief. No other incentives or formal sanctions were provided in the short-term.

Overall, our results provide evidence that the mobile phone monitoring substantially improved learning outcomes. Adults' reading and math test scores were 0.11–0.30 standard deviations (SD) higher in the mobile monitoring villages immediately after the program, with a statistically significant impact. These effects were relatively higher in one region where monitoring was more difficult and were also stronger for teachers for whom the outside option was lowest. These effects do not appear to be driven by differential attrition or differences in teacher quality, but are partially explained by increased teacher effort and motivation, as well as some increased student motivation.

Our finding that monitoring leads to an improvement in skills acquisition contributes to a debate on the effectiveness of education monitoring in other contexts (Guerrero et al 2013). Using monitoring and financial incentives in a randomized experiment in India – specifically using cameras – Duflo, Hanna and Ryan (2012) find that teacher absenteeism fell by 21 percentage points and children's test scores increased by 0.17 s.d. Using a nationally representative dataset of schools in India, Muralidharan et al (2014) find that increased school monitoring is strongly correlated with lower teacher absence, but do not measure effects on learning. Using a matched design in Peru, Cueto et al (2008) find that a program of monitoring and financial incentives for teachers increased teacher attendance, though whether there are any impacts on learning outcomes is less clear. Using mobile phone monitoring linked to financial incentives, Cilliers et al (2014) find that the introduction of financial incentives increased teacher attendance and monitoring frequency, but similarly do not measure impacts upon learning. Our experiment is somewhat unique in that it did not provide any explicit financial incentives.[1]

The remainder of the paper is organized as follows. Section II provides background on the setting of the research and the research design, whereas Section III presents the model. Section IV describes the different datasets and estimation strategy, and Section V presents the results. Section VI addresses the potential mechanisms and Section VII discusses alternative explanations. Section VIII discusses cost-benefit analyses and Section IX concludes.

II. Research Setting and Experimental Design

With a gross national income per capita of $641, Niger is one of the lowest-ranked countries on the UN's Human Development (UNDP 2014). The country has some of the lowest educational indicators in sub-Saharan Africa, with estimated literacy rates of 15 percent in 2012 (World Bank 2015). Illiteracy is particularly striking among women and within our study region: It is estimated that only 10 percent of women attended any school in the Maradi and Zinder regions.

A. Adult Education and Mobile Monitoring Interventions

Starting in March 2014, an international non-governmental organization **(**NGO**)**, Catholic Relief Services, implemented an adult education program in two rural regions of Niger. The intervention provided five months of literacy and numeracy instruction to approximately 25,000 adults across 500 villages. Courses were held between March and July, with a break between July and January due to the agricultural planting and

[1] Our paper also contributes to the literature on community-based monitoring and inspection systems (Svensson 2007, Olken 2007, Bengtsson and Engstrom 2014).

harvesting season. All classes taught basic literacy and numeracy skills in the native language of the village (Hausa), as well as functional literacy topics on health, nutrition and agriculture. Each village was allocated 50 students for the adult education program, with spots for 35 women and 15 men.[2] These fifty students were taught in two literacy classes, separated by gender. Classes were held five days per week for three hours per day, and were taught by community members who were selected and trained in the adult education methodology by the Ministry of Non-Formal Education.[3] Since men's and women's classes differed by gender and class size, we are unable to disentangle the differential effects of gender and class size on learning outcomes.

The mobile monitoring component was implemented in a subset of the adult education villages. For this intervention, data collection agents made four weekly phone calls over a six-week period, calling the literacy teacher, the village chief and two randomly selected students (one female and one male). No phones were provided to either teachers or students.[4] During the phone calls, the field agents asked if the class was held in the previous week, the number of days and the number of hours per day, the number of students who attended and, when calling the teacher, if the teacher had any additional information to share. The mobile monitoring component was introduced two

[2] This breakdown differs from our previous study, whereby the 50 student slots were equally allocated between men and women. However, the donor for the program wanted to increase women's access to the adult education program, and thereby allocated more slots to women in each village.

[3] Unlike previous adult education programs in Niger, the same teacher taught both classes in the village. In addition, the differences in class size by gender makes it difficult for us to disentangle the learning effects by gender as compared with differences in the class size.

[4] Phone numbers for the students were obtained during the initial registration phase for the program. If the student's household did not have a phone, the number of a friend or family member was obtained, and this person was called to reach the student. For the first year, the same two students were called over the six-week period.

months after the start of the adult education program, and neither students, teachers, nor CRS field staff were informed of which villages were selected prior to the calls. [5]

While general information on the results of the monitoring calls were shared with CRS on a weekly basis, due to funding constraints, neither CRS nor the Ministry were able to conduct additional monitoring visits. In fact, the overall number of monitoring visits was extremely low for all villages in 2014. In addition, teachers were not formally sanctioned for less than contracted effort during the first year of the intervention; rather, teachers only learned whether they would be retained for the second year well after the end of classes.[6]

B. Experimental Design

In 2013, CRS identified over 500 intervention villages across two regions of Niger, Maradi and Zinder. Of these, we randomly sampled 160 villages as part of the research program. Among these 160 villages, we first stratified by regional and sub-regional administrative divisions, for a total of six strata. Villages were then randomly assigned to the adult education program **(**to start classes in 2014) or a comparison group (to start classes in 2016). Among the adult education villages, villages were then assigned to either the monitoring or no monitoring intervention. In all, 140 villages were assigned to the adult education program and 20 villages were assigned to the pure control group.[7] Among the adult education villages, 70 villages were assigned to monitoring and

[5] The experimental design was modified slightly during the second year of the study, with a subset of monitoring villages calling teachers only (as opposed to teachers, village chiefs and students).
[6] While CRS did have a policy for modifying salaries based upon attendance, as well as firing teachers after the first year, in practice, no formal sanctions for less than contracted effort were immediately applied: no one was fired, pay was not reduced, no follow-up visits, etc.
[7]While we only have 20 villages in the control group, our power calculations were based upon previous research in Niger on adult education outcomes. Aker, Ksoll and Lybbert (2012) find that a mobile phone-enhanced adult education program increased writing and math test scores by .20-.25 s.d. as compared with

70 to the no monitoring condition.[8] A map of the project areas is provided in Figure 1, and a timeline of the implementation and data collection activities is provided in Figure 2.

Within each village, CRS identified eligible students in both the adult education and comparison villages prior to the baseline. Individual-level eligibility was determined by two primary criteria: illiteracy (verified by an informal writing test) and willingness to participate in the adult education program.

II. Model

A simple conceptual framework provides some intuition as to how monitoring might affect teachers' effort and student learning. A principal (the NGO or government) hires a short-term contractual teacher to teach an adult education program, but is unable to obtain complete information about the teachers' effort, related to imperfect supervision. Assuming that teachers believe they *may* be fired or penalized, monitoring should increase teachers' effort, which can vary with the intensity of monitoring and the cost of being fired.

Suppose that the NGO hires adult education teachers at a wage rate, w_{NGO}. Teachers can choose to exert some effort: e=1 (non-shirker) or e=0 (shirker). For simplicity, there are only two effort levels. Teachers who exert some effort will remain employed by the NGO for the duration of their contract. However, those who exert zero

a traditional adult education program. The non-experimental before-after comparison of the traditional adult education program in that experiment, and the basis of the power calculations for this paper, suggested an effect size of 5 s.d. as compared with the baseline scores. With this effect size, we determined that a sample of 20 villages in the control group was sufficient to determine the causal impact of the adult education intervention.

[8] In 2015, half of the villages receiving the adult education intervention will also receive the ABC program, which introduces a simple mobile phone module into the traditional adult education program. This is a replication of the experiment in Aker, Ksoll and Lybbert (2012).

effort (shirkers) risk being caught (and fired) probability θ. These teachers can find a new job with probability p_m and receive an outside wage w_m, which requires effort e_m.

Using this framework, the utility function for shirkers and non-shirkers is therefore:

$$\begin{aligned} U^{NS} &= w_{NGO} - e \\ U^{S} &= (1 - q)w_{NGO} + q p_m (w_m - e_m) \end{aligned} \tag{1}$$

In order to extract positive levels of effort from the teachers, the NGO will choose a wage rate which assures that $U^{NS} \geq U^{S}$, or that the non-shirking condition is satisfied:

$$w_{NGO} \geq p_m(w_m - e_m) + \frac{e}{\theta} \tag{2}$$

Whether or not teacher's effort (***e***) is influenced by the NGO wage rate (w_{NGO}), as in an efficiency wage model, would not affect the conclusions from our model. For simplicity, we abstract from this issue. The higher the teacher's outside option (outside wage net effort), the less likely he or she is to accept the NGO wage offer.[9] Assuming that the teacher accepts the NGO's offer, the teacher will then choose effort to maximize his/her expected utility.

Outside wage rates can vary by individual (w^{i}_{m}), as it might be more likely for teachers with outside experience to find a job or more likely for male teachers to find jobs, as women are traditionally restricted to the local labor market. This will modify the non-shirker's utility function (slightly) to an individual-specific one, $U^{S,i}$. This suggests that the NGO should tailor the wage and monitoring to the teacher's outside options, but

[9] In theory the NGO has two tools at its disposal to ensure teachers exert effort, namely $\boldsymbol{w}_{NGO}$ and θ, and the optimal combination of the two will be the outcome of the NGO's optimization process, including the cost of monitoring. Unless the wage is chosen such that no one shirks, the exact levels will not change any of our following results

in practice, the NGO can only set a single wage, which will not satisfy the non-shirking condition for every teacher. As a result, a proportion of the teachers will shirk.

A mobile phone monitoring intervention affects the teacher's probability of being caught and fired θ, with $\theta_T \in (\theta_L, \theta_H)$, where L corresponds to the default (low monitoring) state and H to the additional mobile phone monitoring. This leads to the following modifications to the teacher's decision problem:

$$U^{NS} = w_{NGO} - e \qquad (3)$$
$$U^{S,i} = (1-\theta_T)w_{NGO} + \theta_T p_m (w_m^i - e_m)$$

Thus, the optimal w_m^{i*} for which the teacher is indifferent between working and shirking will depend upon the level of monitoring. Again, since the NGO cannot set an individual-specific wage rate, a proportion $\tau(w_{NGO}, \theta)$ of teachers will shirk.

Student learning outcomes are characterized by the following education production function:

$$y_i = y(e_i^t)\begin{cases} y(0) if\ e = 0 \\ y(1) if\ e = 1 \end{cases} \qquad (4)$$

where e_i^t is the effort exerted by student i's teacher, and teacher effort positively affects learning outcomes. This model does not show complementarities or substitutes between teacher and student effort. The average student outcome will therefore be a function of the share of teachers providing effort:

$$\bar{y} = \tau_T y(0) + (1 - \tau_T) y(1) \qquad (5)$$

This leads to the following predictions with mobile phone monitoring:

- **Prediction 1.** As the probability of getting fired rises (θ_T), then $\frac{\partial U^S}{\partial \theta_T} < 0$, so $\frac{\partial \tau}{\partial \theta_T} >$ 0. This is true whenever the NGO wage is greater than the outside wage net effort option, but this needs to be the case for teachers to accept the post in the first place. Since student achievement rises in teacher effort, then $\frac{\partial \bar{y}}{\partial \theta_T} > 0$

- **Prediction 2.** If the attractiveness of the teacher's outside option rises, i.e. $\boldsymbol{p_m}$ or $\boldsymbol{(w^i_m - e_m)}$ rises, then the consequences of shirking become less severe and the proportion of teachers providing effort goes down: i.e. $\frac{\partial \tau}{\partial p_m} > 0$ and $\frac{\partial \tau}{\partial (w_m - e_m)} > 0$. This implies that students' learning outcomes will decrease with the attractiveness of teachers' outside options, so that $\frac{\partial \bar{y}}{\partial p_m} < 0$.[10]

While this model focuses on the probability of being fired, in practice, the NGO did not use the monitoring intervention to fire teachers between the first and the second year. Yet assuming that teachers believe they *may* be fired or penalized, additional monitoring should increase teachers' effort and student learning. Nevertheless, if there are no consequences between the first and second year, the effects may dissipate during the second year.

IV. Data and Estimation Strategy

The data we use in this paper come from three primary sources. First, we conducted individualized math and reading tests and use these scores to measure the impact of the program on educational outcomes. Second, we implemented household-level surveys. Third, we collected administrative and survey data on teachers, and use these data to better understand the mechanisms behind the effects. Before presenting our estimation strategy, we discuss each of these data sources in detail.

[10] This is not necessarily true when $\boldsymbol{p_m(w^i_m - e_m)}$ and teacher ability are correlated, as then a higher ability teacher might still teach better even when shirking than a present low ability teacher. Then locally, the above result holds, but not when you change outside options in a discrete way. At this point the fact that we have measures of teacher ability become important. Conditional on ability the above results hold.

A. Test Score and Self-Esteem Data

Our NGO partner identified students in all villages and for all cohorts in January 2014. While we had originally intended to implement the baseline in all 160 villages, the delayed start of the adult education program during the first year, as well as delays in funding, meant that we were only able to conduct the baseline in a subset of the sample (91 villages).[11] In these villages, we stratified students by gender and took a random sample of 16 students per village. We implemented reading and math tests prior to the start of courses (February 2014), providing a baseline sample of approximately 1,271 students. We administered follow-up tests in the same baseline villages (91) as well as a random sample of non-baseline villages (30 villages) in August 2014, thereby allowing us to estimate the immediate impacts of the program. This total sample was 1,926 students, excluding attrition.

To test students' reading and math skills, we used USAID's Early Grade Reading Assessment (EGRA) and Early Grade Math Assessment (EGMA) tests. These are a series of individual tasks in reading and math, often used in primary school programs. EGRA is a series of timed tests that measure basic foundational skills for literacy acquisition: recognizing letters, reading simple words and phrases and reading comprehension (Dubeck and Gove 2015). Each task ranges from 60-180 seconds; if the person misses four answers in a row, the exercise is stopped. EGMA measures basic foundational skills for math acquisition: number recognition, comparing quantities, word problems, addition, subtraction, multiplication and division (Reubens 2009).

[11]To choose the baseline villages, we stratified by region, sub-region and treatment status and selected a random sample of villages for the baseline. We also used a similar process to add on the 30 villages for the first follow-up survey.

The EGRA and EGMA tests were our preferred survey instruments, as compared with the Ministry's standard, untimed battery of writing and math tests, for two reasons. First, most adult education programs are criticized for high rates of skills' depreciation. Yet these high rates of skills' depreciation may be simply due to the levels of reading achieved by the end of traditional adult education programs, which are often not captured in traditional untimed tests. For example, the short-term memory required to store deciphered material is brief, lasting 12 seconds and storing 7 items (Abadzi 2003). Thus, "Neoliterates must read a word in about 1-1.5 second (45-60 words per minute) in order to understand a sentence within 12 seconds (Abadzi 2003)."[12] Thus, the EGRA timed tests allow us to determine whether participants in adult education classes are attaining the threshold required for sustained literacy acquisition. Second, the tests offer a great deal of precision in terms of measuring the skills that contribute to reading acquisition, capturing more nuanced levels of variation in learning (Dubeck and Gove 2015).

During the reading and math tests, we also measured students' self-esteem and self-efficacy, as measured by the Rosenberg Self-Esteem Scale (RSES) and the General Self-Efficacy Scale (GSES). The RSES is a series of statements designed to capture different aspects of self-esteem (Rosenberg 1965). Five of the statements are positively worded, while the other five statements are negatively-worded. Each answer is assigned a point value, with higher scores reflecting higher self-esteem. The GSES is a ten-item psychometric scale that is designed to assess whether the respondent believes he or she is capable of performing new or difficult tasks and to deal with adversity in life (Schwarzer and Jerusalem 1995). The scale ranges in value from 12-60, with higher scores reflecting

[12]This speed corresponds to oral-reading U.S. norms for first grade children. However, this is often not attained in literacy classes. For example, studies in Burkina Faso indicate that most literacy graduates need 2.2 seconds to read a word and are correct only 80-87 percent of the time (Abadzi 2003).

higher perceived self-efficacy. We use these results to measure the impact of the program on participants' perceptions of empowerment.

Survey attrition is a concern in most studies, especially in populations that engage in seasonal migration. Table A1 formally tests whether there is differential attrition by treatment status for the follow-up survey round. The rate of attrition in the comparison group – which did not receive the literacy program - was 5 percent, with relatively higher attrition in the non-monitoring group and lower attrition in the monitoring group. This suggests that the monitoring program might have prevented student attrition. Non-attriters in the adult education villages were more likely to be female as compared with non-attriters in the comparison villages, although there were no statistically significant differences among other characteristics between the monitoring and non-monitoring villages. The difference in attrition by gender would likely bias our treatment effect downwards, as female students have lower test scores as compared with male students in adult education classes (Aker et al 2012).

B. Household Survey Data

The second primary dataset includes information on baseline household characteristics. We conducted a baseline household survey in February 2014 with 1,271 adult education students across 91 villages, the same sample as those for the test score data. The survey collected detailed information on household demographics, assets, production and sales activities, access to price information, migration and mobile phone ownership and usage. These data are primarily used to test for balance imbalances across the different treatments, as well as to test for heterogeneous effects.

C. Teacher Data

The third dataset is comprised of teacher-level characteristics and a measure of teachers' motivation. Using administrative data from CRS' teacher screening and training process, the dataset includes information on teachers' level of education, age, gender and village residence. In addition, in November 2014, we conducted a survey of all teachers in adult education villages, which included an intrinsic motivation inventory (IMI). The IMI is a multidimensional measurement instrument intended to assess participants' subjective experience related to a target activity, and has been used in several experiments related to intrinsic motivation and self-regulation (e.g., Ryan 1982, among others). The instrument assesses participants' interest/enjoyment, perceived competence, effort, value/usefulness, felt pressure and tension, and perceived choice while performing a given activity, thus yielding six subscale scores that are combined into an overall score.[13] We applied one of the versions of the IMI to our specific context, namely, teachers' experience in teaching the adult education program.

C. Pre-Program Balance

Table 1A shows the pre-program comparison of a number of student and household-level characteristics between the different treatments and control, controlling for the variables used for stratification (Bruhn and McKenzie 2009). Overall, the results suggest that the randomization was successful in creating comparable groups along observable dimensions. Differences in pre-program household characteristics are small and insignificant (Table 1, Panel A). Average age was 34, and a majority of respondents were members of the Hausa ethnic group. The average education level of household

[13] The interest/enjoyment subscale is considered the self-reported measure of intrinsic motivation; although the overall questionnaire is called the IMI, the interest subscale is the only one that assesses intrinsic motivation per se.

members was 2 years. Fifty-eight percent of households in the sample owned a mobile phone, with 61 percent of respondents having used a mobile phone in the months prior to the baseline. Respondents primarily used the mobile phone to make and receive calls. All respondents reporting *receiving* calls (as compared with making calls), as making a phone call requires being able to recognize numbers on the handset. While some baseline differences are statistically significant – such as asset and mobile phone ownership, which are related -- overall, we made over 100 baseline comparisons across the treatment groups and find statistically significant differences that are consistent with what one would expect of randomization. A formal statistical test supports these conclusions.[14]

Table 1B provides further evidence of the comparability across treatments for reading scores. Using non-normalized baseline reading scores for each task, students in comparison villages had low levels of letter, syllable, word or phrase recognition prior to the program, without a statistically significant between the treatment and control groups or between the monitoring and non-monitoring villages. Comparisons of baseline math scores (Table 1C), similarly suggest comparability across the different groups, with the exception of one math task. This suggests that the project successfully selected participants who were illiterate and innumerate prior to the start of the program.

Table 1D presents a comparison of teacher characteristics across the adult education villages. Overall teacher characteristics are well-balanced between the monitoring and non-monitoring villages. Teachers were 37 years old and approximately

[14] In particular, the results in Tables 1A-1D are robust to testing for joint orthogonality of the covariates, with p-values of .25, .48 .55 and .11, respectively. The dependent variable in these regressions is "monitor" and is only estimated on the subset of adult education villages, so in fact tests for the joint orthogonality of covariates with respect to assignment to the monitoring treatment.

37 percent had some secondary education. Roughly one-third of the teachers were female, and a strong majority were married.

D. Estimation Strategy

To estimate the impact of both the adult education program and monitoring on educational outcomes, we use a simple differences specification. Let $test_{iv}$ be the reading or math test score attained by student *i* in village *v* immediately after the program. $adulted_v$ is an indicator variable for whether the village *v* is assigned to the adult education intervention (*adulted*=1) or the control (*adulted*=0). $adulted*monitor_t$ takes on the value of one if the adult education village received the mobile monitoring intervention, and 0 otherwise. θ_R are geographic fixed effects at the regional and sub-regional levels (the level of stratification). $\mathbf{X}'_{iv}$ is a vector of student-level baseline covariates, primarily gender, although we include the baseline test score in some specifications. We estimate the following specification:

$$test_{iv} = \beta_0 + \beta_1 adulted_v + \beta_2 adulted_v * monitor_v + X'_{io} + \theta_S + \varepsilon_{iv} \quad (6)$$

The coefficients of interest are β_1 and $\beta_{2,}$ which capture the average immediate impact of the adult education program (without monitoring) and the additional impact of the mobile phone monitoring program. The error term ε_{iv} captures unobserved student ability or idiosyncratic shocks. We cluster the error term at the village level for all specifications.

Equation (6) is our preferred specification. As an alternative to this preferred approach, we also estimate the impact of the program using a value-added specification. However, this reduces our sample size, as we do not have baseline data for all villages.

V. Results

Figures 3A and 3B depict the mean normalized reading and math test scores for the adult education villages with and without monitoring immediately after the end of classes. Test scores are normalized using the mean and s.d. of contemporaneous test scores in comparison villages, so that the mean in the comparison villages is zero. The means of the comparison group are not shown for ease of exposition. Three things are worth noting. First, the adult education program seems to increase reading and math scores significantly as compared to the comparison group, with relatively stronger effects on reading, although no one achieved the "threshold" reading level (Abadzi 2003). Second, these effects are also stronger for "lower level" tasks, i.e., simple letter or syllable recognition and addition and subtraction. And third, the difference in test scores between monitoring and non-monitoring villages is almost equivalent to the difference in test scores between the non-monitoring villages and the comparison group, especially for lower-level tasks. This suggests powerful learning gains from the monitoring program.

A. Immediate Impact of the Program

Table 2 presents the results of Equation (3) for reading test z-scores. Across all reading tasks, the adult education program increased students' reading test scores by .12-.27 s.d., with a statistically significant effect at the 5 percent level for reading letters and syllables (Table 2, Panel A, Columns 1 and 2) and the composite score (Column 5). These adult education impacts are relatively stronger in Maradi (Panel C) as compared to Zinder (Panel B).

The monitoring intervention increased reading test scores by .14-.30 s.d., with a statistically significant effect at the 5 and 10 percent levels across all reading measures. These results are primarily driven by villages in Zinder (Panel B), the region with the lowest achievement gains for the adult education program and with a larger geographic area over which to conduct in-person monitoring.

The results are similar, although with a lower magnitude, for math z-scores (Table 3): the adult education program increased math z-scores by .08-.23 s.d. (Panel B, Column 1), with statistically significant effects at the 5 and 10 percent levels. These results are primarily stronger in the Maradi region (Panel C). Overall, the monitoring component increased test scores by .08-.15 s.d., although the statistically significant effects are primarily for simpler math tasks (Panel A) and for the Zinder region (Panel B). The results in Table 3 are also robust to using value-added specifications, the latter of which controls for average baseline test scores at the village level (not shown).

A key interest in adult education programs is whether such programs affect student empowerment. We therefore measure the impact of the adult education program and the mobile monitoring component on self-esteem and self-efficacy, using the RSES and GSES (Table 4). Overall, self-esteem and self-efficacy scores were 2-3 percent lower in the adult education as compared to control villages, although only with a statistically significant effect for self-efficacy scores (Table 4, Panel A). These effects are relatively stronger in the Zinder region, where students achieved the lowest literacy gains (Panel B). The monitoring component seems to mitigate this effect: monitoring villages have higher levels of self-efficacy as compared with students in the non-monitoring adult education villages.

While potentially surprising, this seems to mirror the results found in Aker et al (2015), who found that students' perceptions of self-esteem changed over time, particularly when they experienced learning failures. Since students in the Zinder region attained lower levels of learning overall, they could have potentially felt less capable in the short-term, although the monitoring component mitigated this effect.

B. Heterogeneous Effects of the Program

We would expect greater learning benefits among certain subpopulations, such as men and women, or according to teachers' characteristics, as predicted by our model. Table 5 tests for heterogeneous impacts of the program by the student's gender, while Table 6 tests for heterogeneous effects by teacher characteristics, in particular proxies for outside options.

In light of different socio-cultural norms governing women's and men's household responsibilities and social interactions, the adult education and monitoring program could have different impacts by gender. As women of particular ethnic groups (e.g., the Hausa) travel outside of their home village less frequently than men, the adult education classes may have provided fewer opportunities for women to practice outside of class, thereby weakening their incentives to learn. In addition, given the differences in class size between men and women, women could have been disadvantaged by the larger student-to-teacher ratio. Table 5 presents the results by gender. On average, women's reading and math z-scores were lower than men's immediately after the program, similar to the results found in Aker et al (2012). The monitoring component had a stronger impact on men's reading test scores as compared with women's, even though the same teachers taught both courses. However, since women's class sizes were also larger, it is difficult to disentangle the mechanisms behind this effect.[15]

[15] For example, because men had smaller classes, the additional teacher effort due to monitoring might have been more effective.

Table 6 presents these results by teachers' characteristics, namely gender, education level (secondary or below) and previous experience as an adult education teacher. In many villages in the Maradi and Zinder regions of Niger, women often do not migrate, and therefore have more localized and constrained labor market options. Teachers with higher levels of education should have better outside options, thereby reducing the effectiveness of monitoring component. The results suggest that this is the case: While monitoring increases reading and math z-scores of adult education students regardless of the teacher characteristics, with relatively stronger impacts on reading, the impact of monitoring is stronger for female teachers and those with less experience, consistently with our model. As for teachers with less experience, monitoring is less effective for new teachers, primarily for reading. This might be due to the fact that newer teachers had better outside options, or were already exercising higher effort, so the additional monitoring did not have a strong effect.[16]

VI. Potential Mechanisms

There are a variety of mechanisms through which the monitoring component could affect students' learning. First, mobile monitoring can potentially lead to increased teacher effort, thereby improving the effectiveness of the overall adult education curriculum. Second, the phone calls could potentially increase teachers' intrinsic motivation, thereby increasing their teaching efficacy and the impact of the program. Third, having a more present and motivated teacher could potentially affect students' effort, leading to increased class participation and attendance. And finally, as the monitoring component involved students, the calls could have motivated students

[16] We could more formally test for this relationship by looking at the correlates between teacher experience and test scores in the non-monitoring group.

independently, who in turn motivated their fellow learners. While we have more speculative evidence on each of these, we present evidence on each of these mechanisms in turn.

A. Teacher Effort and Motivation

The mobile phone monitoring could have increased teacher effort within the classroom, thereby improving students' performance. As we are unable to directly observe teacher effort, we assess the impact on a self-reported proxy. CRS and the Ministry of Non-Formal Education provided norms for the number of classes to be taught during each month, yet the actual number of classes taught was at the discretion of each teacher. While we would prefer an external, objective measure of the number of classes' taught, for the short-term, we use teachers' self-reported measures of whether or not they stopped the class and the number of days stopped. Table 7 shows the results of the monitoring component on teachers' self-reported effort and measures of intrinsic motivation. While teachers in monitoring villages were not less likely to stop teaching at any point over the course, they were absent for 1.27 fewer days than the non-monitoring teachers, with a statistically significant difference at the 10 percent level (Panel A). This suggests that the observed improvements in test scores may have been due to increased duration of the course, although the margin of this effect is quite small. This is, in part, supported by qualitative data: Teachers reported that "The…calls prevent us from missing courses", and that "Someone who works must be 'controlled'". However, there was no correlation between monitoring and the teacher's likelihood of being replaced between the first and second year (Panel C).

In addition to affecting teacher absence, the calls could have also affected teachers' intrinsic motivation, thereby making them more effective in class. Teachers themselves reported that the calls "prove that our work is important" and that they gave them "courage". While monitoring did not appear to have an impact on an index of self-reported pressure, perceived competence or choice, it did appear to increase self-reported intrinsic motivation, as measured by a 10-point scale: teachers reported feeling more interested in the task, with a statistically significant effect at the 10 percent level (Table 7, Panel B). However, with only 140 teacher observations, we may be underpowered to detect such small effects.

B. Student Effort and Motivation

The monitoring component could have encouraged greater student effort within the classes, as measured by student attendance or motivation. While we do not have reliable data on student attendance, we do have measures of student dropout at some point during the course and the reason for dropout. Table 8 shows these results. Overall, the monitoring component did not appear to affect the likelihood of student dropout (Table 8, Panel A) nor the likelihood of a student dropping out for an endogenous reason (i.e., lack of time, lack of interest) as opposed to an exogenous shock (pregnancy, illness, death in the family).

Nevertheless, there is some suggestive evidence that the monitoring component affected student learning via the mechanism of calling students themselves. Panel B shows the results of a regression of test scores on a binary variable for students who were called, as well as the monitoring treatment and an interaction term between the two. While the "called" students only represents 8 percent of the total sample, the calls appeared to affect students' learning: called students had significantly higher reading and math z-scores as compared with non-called students in monitoring villages, as well as students in non-monitoring villages. It is possible that the called students' greater motivation passed to other students, although we cannot test this hypothesis.[17]

VII. Alternative Explanations

There are two potential confounds to interpreting the above findings. First, there might be differential in-person monitoring between monitoring and non-monitoring villages. If the Ministry of Non-Formal Education or CRS decided to focus more of their efforts on monitoring villages because they had better information, then any differences we observe in test scores might be due to differences in program implementation, rather than the monitoring component. Yet during the first year of the program, there was very little in-person monitoring, and no differential visits by treatment status.

A second potential confounding factor could be due to differential attrition. The results in Table A1 suggest that attrition is higher in the adult education villages as compared with the comparison group and lower in the monitoring villages (as compared with non-monitoring villages). While it is difficult to predict the potential direction of this bias, we use Lee bounds to correct for bias for differential attrition between the monitoring and non-monitoring villages, our primary comparison of interest. Table A2 suggests that the upper bounds remain positive and statistically significant (unsurprisingly), and that the lower bounds for reading and math test scores are still positive and statistically significant for most of the primary outcomes.

[17] The main results are robust to excluding the "called" students from the sample, although the magnitudes of the coefficients are smaller (Table A5).

Finally, as we are conducting a number of comparisons across multiple outcomes, there is a risk that our results could be due to probabilistic equivalence, at least in part. Using a Bonferroni correction accounting for family-wise correlation, we modify the p-values to account for these multiple comparisons, with the results in Table A3. Overall, the results remain statistically significant for the reading outcomes and for those in the Zinder region.[18]

VIII. Cost-Effectiveness

A key question is the cost-effectiveness of the mobile intervention as compared to regular monitoring. While in-person monitoring visits were limited in the context of the first year of the study, we have data on per-monitoring costs for both in-person and mobile monitoring (Figure 4). On average, in-person monitoring costs are $6.20 per village, primarily including costs for the agent’s time and gas for the motorcycle. By comparison, the mobile monitoring intervention only costs $3.08 per village, including the costs of agents’ time and mobile phone credit. This suggests that per-village savings are $3, as compared with average gains of .20 s.d. in learning.

IX. Conclusion

Adult education programs are an important part of the educational system in many developing countries. Yet the successes of these initiatives have been mixed, partly due to the appropriateness of the educational input and the ability of governments and international organizations to monitor teachers’ effort.

[18] The small number of observations in the comparison group who did not receive the adult education intervention could raise concerns that our confidence intervals are too narrow (Cameron, Gelbach and Miller 2008). We therefore re-estimate our core results while using a bootstrap-t procedure for our standard errors (Table A4) and find similar results.

This paper assesses the impact of an intervention that conducted mobile monitoring of as part of an adult education intervention in Niger. We find that simply monitoring teachers substantially increased students' skills acquisition, suggesting that mobile telephones could be a simple and low-cost way to improve adult educational outcomes. The treatment effects are striking: the adult education program with monitoring increased reading and math test scores by .15-.25 s.d. as compared with the standard adult education program, amounting to a 75 percent increase in reading test scores, as well as an increase of almost 40 per cent for math test scores, although the latter is only marginally statistically significant. The impacts appear to operate through increasing teacher effort and motivation, although we are unable to clearly identify the precise mechanism at this time.

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Table 1A. Baseline Household Characteristics

Household Characteristics at Baseline	(1) **Comparison Group** Mean (s.d.)	(2) **Monitoring** Mean (s.d.)	(3) **Adult Educ.** Mean (s.d.)	(4) **Difference** Coeff (s.e) (2)-(1)	(5) **Difference** Coeff (s.e.) (3)-(1)	(6) p-value (2)=(3)
Age of Respondent	35.6	33.44	34.08	-1.26	-1.97	0.73
	(12.98)	(11.63)	(12.01)	(1.083)	(1.273)	
Gender of Respondent (1=Female, 0=Male)	0.685	0.677	0.683	0.01	-0.01	0.40
	(0.466)	(0.468)	(0.465)	(0.0121)	(0.0217)	
Average education level of household (in years)	1.787	2.112	2.069	0.12	-0.08	0.19
	(0.963)	(1.028)	(0.985)	(0.0811)	(0.0906)	
Number of asset categories owned by household	5.585	5.895	5.81	0.22*	-0.15	0.16
	(1.543)	(1.6)	(1.569)	(0.115)	(0.206)	
Household experienced drought in past year (0/1)	0.471	0.564	0.537	0.03	0.02	0.83
	(0.501)	(0.496)	(0.499)	(0.0400)	(0.0611)	
Household owns a mobile phone (0/1)	0.58	0.685	0.665	0.07**	0.00	0.33
	(0.496)	(0.465)	(0.472)	(0.0339)	(0.0519)	
Respondent used a cell phone since the last harvest	0.61	0.647	0.644	0.03	0.03	0.95
	(0.502)	(0.478)	(0.479)	(0.0330)	(0.0577)	
Used cellphone in past two weeks to make calls	0.737	0.722	0.703	0.04	-0.05	0.25
	(0.446)	(0.449)	(0.457)	(0.0338)	(0.0591)	
Used cellphone in past two weeks to receive calls	1	0.967	0.965	0.00	-0.05***	0.19
	(0)	(0.178)	(0.185)	(0.0165)	(0.0227)	

Note: This table shows the difference in means between the different treatment groups. "Comparison" is defined as villages assigned to no adult education treatment in 2014 or 2015. "Adult education" is defined as those villages that were assigned to adult education without monitoring, whereas "Monitoring" is defined as villages that were assigned to adult education with monitoring. Standard deviations are shown in parentheses. Columns (4) and (5) show the coefficients and s.e. from a regression of each characteristic on the treatments and stratification fixed effects. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 1B. Baseline Reading Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)
	Comparison Group Mean (s.d.)	**Monitoring** Mean (s.d.)	**Any Adult Educ.** Mean (s.d.)	**Difference** Coeff (s.e) (2)-(1)	**Difference** Coeff (s.e.) (3)-(1)	**p-value** (2)=(3)
Task 1: Total items correct	2.074	3.368	3.146	0.237	0.383	0.895
	(7.115)	(10.71)	(10.29)	(0.667)	(0.632)	
Task 2: Total items correct	1.2	2.745	2.483	0.387	0.712	0.727
	(5.532)	(9.754)	(9.362)	(0.611)	(0.480)	
Task 3: Total items correct	0.968	1.664	1.547	0.0762	0.155	0.914
	(5.17)	(7.277)	(7.299)	(0.446)	(0.427)	
Task 4: Total items correct	1.232	1.589	1.715	-0.416	0.603	0.352
	(7.185)	(7.851)	(8.574)	(0.568)	(0.737)	
Task 5: Total items correct	0.105	0.152	0.157	-0.00557	0.0353	0.658
	(0.592)	(0.764)	(0.769)	(0.0517)	(0.0587)	

Note: This table shows the difference in means between the different treatment groups. "Comparison" is defined as villages assigned to no adult education treatment in 2014 or 2015. "Adult education" is defined as those villages that were assigned to adult education without monitoring, whereas "Monitoring" is defined as villages that were assigned to adult education with monitoring. Standard deviations are shown in parentheses. Columns (4) and (5) show the coefficients and s.e. from a regression of each characteristic on the treatments and stratification fixed effects. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 1.C. Baseline Math Test Scores

	(1) **Comparison Group** Mean (s.d.)	(2) **Monitoring** Mean (s.d.)	(3) **Any Adult Educ.** Mean (s.d.)	(4) **Difference** Coeff (s.e) (2)-(1)	(5) **Difference** Coeff (s.e.) (3)-(1)	(6) **p-value** (2)=(3)
Task 1: Highest number correctly counted to	44.07	41.89	41.67	1.218	-0.963	0.677
	(23.75)	(24.24)	(23.95)	(1.576)	(4.832)	
Task 3: Total number correct (of 12)	4.135	4.414	4.342	0.122	0.217	0.899
	(5.32)	(5.268)	(5.202)	(0.294)	(0.645)	
Task 4: Total number correct (of 20)	5.708	5.791	5.747	-0.0105	0.105	0.906
	(8.168)	(8.137)	(8.094)	(0.495)	(0.691)	
Task 5: Total number correct (of 6)	4.236	4.244	4.248	-0.00818	0.0109	0.946
	(1.523)	(1.583)	(1.503)	(0.111)	(0.247)	
Task 6: Total number correct (of 4)	2.899	2.791	2.798	-0.0152	-0.0366	0.889
	(1.315)	(1.322)	(1.271)	(0.0837)	(0.111)	
Task 7: Total number correct (of 9)	7.708	7.547	7.606	-0.116	-0.126	0.977
	(1.914)	(2.143)	(2.061)	(0.152)	(0.272)	

Note: This table shows the difference in means between the different treatment groups. "Comparison" is defined as villages assigned to no adult education treatment in 2014 or 2015. "Adult education" is defined as those villages that were assigned to adult education without monitoring, whereas "Monitoring" is defined as villages that were assigned to adult education with monitoring. Standard deviations are shown in parentheses. Columns (4) and (5) show the coefficients and s.e. from a regression of each characteristic on the treatments and stratification fixed effects. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 1D. Balance Table of Teacher Characteristics

	(1) Comparison Schools		(2) Adult Education Only		(3) Adult Education + Monitoring		p-value (1)=(2)	p-value (1)=(3)	p-value (2)=(3)
Panel A. Teacher Characteristics	Mean	s.d	Mean	*s.d.*	Mean	*s.d.*			
Teacher Age			37.35	(8.67)	36.84	(9.37)			0.836
Teacher is female			0.33	(0.47)	0.34	(0.48)			0.816
Teacher is married			0.88	(0.33)	0.92	(0.27)			0.561
Teacher has some secondary education			0.35	(0.48)	0.39	(0.49)			0.569

Note: This table shows the difference in means between the different treatment groups. "Comparison" is defined as villages assigned to no adult education treatment in 2014 or 2015. "Adult education" is defined as those villages that were assigned to adult education without monitoring, whereas "Monitoring" is defined as villages that were assigned to adult education with monitoring. Standard deviations are shown in parentheses. Columns (4) and (5) show the coefficients and s.e. from a regression of each characteristic on the treatments and stratification fixed effects. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 2. Reading Timed Z-Scores

	(1) Letters	(2) Syllables	(3) Words	(4) Phrases	(5) Composite Score
Panel A: All Villages					
(1) Adult education	0.27***	0.22**	0.12	0.13	0.23**
	(0.10)	(0.10)	(0.08)	(0.09)	(0.10)
(2) Adult education*monitor	0.18*	0.30**	0.14*	0.14*	0.18**
	(0.09)	(0.13)	(0.08)	(0.08)	(0.09)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,773	1,773	1,773	1,773	1,773
R-squared	0.02	0.01	0.01	0.01	0.02
Total effect: Adult Education + Monitoring					
p-value (Adult education + monitor=0)	*.00****	*.00****	*.00****	*0.00****	*0.00****
Panel B: Zinder					
(1) Adult education	0.17	0.10	0.04	0.05	0.10
	(0.13)	(0.14)	(0.10)	(0.10)	(0.12)
(2) Adult education*monitor	0.22*	0.45*	0.19*	0.18*	0.24*
	(0.14)	(0.22)	(0.11)	(0.11)	(0.14)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	903	903	903	903	903
R-squared	0.02	0.01	0.02	0.01	0.02
Total effect: Adult Education + Monitoring					
p-value (Adult education + monitor=0)	*0.00****	*0.03***	*0.05***	*0.06**	*0.00****
Panel C: Maradi					
(1) Adult education	0.44***	0.37***	0.25*	0.27*	0.40**
	(0.15)	(0.13)	(0.14)	(0.15)	(0.16)
(2) Adult education*monitor	0.15	0.17	0.09	0.11	0.15
	(0.12)	(0.14)	(0.11)	(0.12)	(0.13)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	870	870	870	870	870
R-squared	0.02	0.01	0.01	0.01	0.02
Total effect: Adult Education + Monitoring					
p-value (Adult education + monitor=0)	*0.000*	*0.001*	*0.05*	*0.03*	*0.001*

Notes: This table presents the results from a regression of different reading outcomes on adult education (only), adult education plus monitoring and randomization fixed effects. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 3. Math Z-Scores (Untimed)

	(1) Number Identification	(2) Quantity Comparison	(3) Addition and Subtraction	(4) Multiplication and Division	(5) Composite Score
Panel A: All Villages					
(1) Adult education	0.13*	0.08	0.21**	0.17*	0.23***
	(0.07)	(0.07)	(0.09)	(0.09)	(0.08)
(2) Adult education*monitor	0.11*	0.14**	0.15*	0.08	0.09
	(0.06)	(0.06)	(0.08)	(0.08)	(0.07)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,773	1,773	1,773	1,773	1,773
R-squared	0.01	0.02	0.01	0.01	0.01
p-value (Adult education + monitor=0)	0.013	0.078	0.004	0.121	0.64
Panel B: Zinder					
(1) Adult education	0.06	0.09	0.14	0.09	0.10
	(0.09)	(0.10)	(0.12)	(0.10)	(0.11)
(2) Adult education*monitor	0.21**	0.13*	0.24**	0.08	0.23**
	(0.09)	(0.08)	(0.12)	(0.11)	(0.11)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	903	903	903	903	903
R-squared	0.02	0.03	0.02	0.02	0.03
p-value (Adult education + monitor=0)	0.079	0.021	0.045	0.316	
Panel C: Maradi					
(1) Adult education	0.20**	0.16	0.29**	0.33**	0.31***
	(0.10)	(0.13)	(0.14)	(0.15)	(0.11)
(2) Adult education*monitor	0.03	0.00	0.08	0.06	0.04
	(0.08)	(0.08)	(0.11)	(0.11)	(0.10)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	870	870	870	870	870
R-squared	0.01	0.01	0.01	0.01	0.01
p-value (Adult education + monitor=0)	0.041	0.313	0.044	0.303	

Notes: This table presents the results from a regression of different math outcomes on adult education (only), adult education plus monitoring and randomization fixed effects. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 4. Self-Esteem and Self-Efficacy

	(1) Self-Esteem	(2) Self-Efficacy
Panel A: All Villages		
(1) Adult education	-0.33	-0.92**
	(0.23)	(0.45)
(2) Adult education*monitor	0.06	0.41
	(0.16)	(0.35)
Strata fixed effects	Yes	Yes
Observations	1,773	1,773
R-squared	0.01	0.01
Mean of comparison group	20.73	29.03
p-value (Adult education + monitor=0)	0.36	0.778
Panel B: Zinder		
(1) Adult education	-0.51	-1.67***
	(0.31)	(0.56)
(2) Adult education*monitor	0.09	1.16**
	(0.28)	(0.57)
Strata fixed effects	Yes	Yes
Observations	903	903
R-squared	0.02	0.01
Mean of comparison group	21.05	32.19
p-value (Adult education + monitor=0)	0.252	0.513
Panel C: Maradi		
(1) Adult education	0.00	0.11
	(0.32)	(0.72)
(2) Adult education*monitor	0.04	-0.20
	(0.19)	(0.41)
Strata fixed effects	Yes	Yes
Observations	870	870
R-squared	0.02	0.00
Mean of comparison group	20.09	33.95
p-value (Adult education + monitor=0)	0.98	0.473

Notes: This table presents the results from a regression of different outcomes on adult education (only), adult education plus monitoring and randomization fixed effects. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 5. Heterogeneous Effects by Gender

	Reading Z-Scores					Math Z-Scores			Self-Esteem	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Letters	Syllables	Words	Phrases	Reading	Number Identification	Add and Subtract	Multiplication and Division	Self-Esteem	Self-Efficacy
Panel A: Women										
(1) Adult education	0.20***	0.12*	0.07	0.08	0.13**	0.22**	0.20**	0.14**	-0.26	-0.95*
	(0.07)	(0.06)	(0.05)	(0.05)	(0.06)	(0.09)	(0.09)	(0.07)	(0.24)	(0.51)
(2) Adult education*monitor	-0.01	0.04	0.01	0.02	-0.00	0.10	0.11	0.04	-0.11	0.27
	(0.07)	(0.07)	(0.05)	(0.05)	(0.06)	(0.07)	(0.08)	(0.06)	(0.20)	(0.38)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,231	1,232	1,232	1,232	1,232	1,232	1,232	1,232	1,232	1,232
R-squared	0.03	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.033	0.008
Panel B: Men										
(1) Adult education	0.54**	0.56**	0.36	0.42*	0.53**	0.05	0.37**	0.36*	-0.48	-0.77
	(0.25)	(0.28)	(0.23)	(0.25)	(0.25)	(0.09)	(0.18)	(0.19)	(0.34)	(0.69)
(2) Adult education*monitor	0.50**	0.71**	0.29	0.28	0.41*	0.01	0.13	0.07	0.43	0.47
	(0.21)	(0.34)	(0.21)	(0.22)	(0.23)	(0.07)	(0.14)	(0.17)	(0.27)	(0.54)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	534	534	534	534	534	534	534	534	534	534
R-squared	0.06	0.05	0.03	0.03	0.03	0.03	0.03	0.03	0.038	0.042
*p-value of adult education*female*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	0.609	0.362
*p-value of adult education*monitor*female*	*0.031*	*0.078*	0.301	0.285	0.129	0.608	0.997	0.649	0.115	0.392

Notes: This table presents the results from a regression of different outcomes on adult education (only), adult education plus monitoring, gender, the separate interaction terms and randomization fixed effects. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 6. Heterogeneous Effects by Teacher Characteristics

	Reading Z-Scores			Math Z-Scores		
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Monitor	0.10	0.46***	0.40**	0.06	0.24	0.05
	(0.11)	(0.16)	(0.16)	(0.09)	(0.18)	(0.12)
(2) Monitor*teacher is male	-0.35*			-0.32*		
	(0.21)			(0.18)		
(3) Monitor*teacher has secondary school		-0.35*			-0.18	
		(0.20)			(0.19)	
(4) Monitor*teacher is new			-0.44**			-0.07
			(0.20)			(0.16)
Number of observations	1,402	1,402	1,402	1,402	1,402	1,402
R-squared	0.02	0.12	0.13	0.02	0.28	0.29

Notes: This table presents the results from a regression of different reading and outcomes on monitoring, its interaction with different teacher characteristics (gender, education and experience), the teacher characteristics (not shown) and randomization fixed effects. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 7. Teacher Effort and Motivation

	Mean Non-Monitoring Village	Monitoring Village
	Mean (s.d.)	Coeff (s.e.)
Panel A: Self-reported teacher attendance		
(1) Stopped course (Yes/No)	0.26	-0.05
	(0.08)	(0.08)
(2) Number of days stopped course	1.28	-1.27*
	(3.30)	(0.65)
Panel B: Teacher Motivation		
(3) Felt pressure or tension (z-score)	0	-0.20
	(1.00)	(0.19)
(4) Interest (self-reported motivation) (z-score)	0	0.32*
	(1.00)	(0.17)
(5) Perceived Competence (z-score)	0	0.25
	(1.00)	(0.19)
(6) Perceived choice	0	0.19
	(1.00)	(0.19)
Panel C: Teacher Replacement		
(7) Teacher was replaced	0.24	-0.04
	(0.43)	(0.07)
Number of observations		140

Notes: This table presents the results from a regression of teacher-level outcomes on a binary variable for monitoring, among the sample of adult education courses. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 8. Student Effort

	Monitoring Village
	(1)
Panel A: Student Dropout of Course	
(1) Stopped course (Yes/No)	-0.02
	(0.03)
(2) Stopped course for personal choice	-0.10
	(0.06)
Panel B: Learning Outcomes of Called Students (Compared with All Monitoring Students)	
(3) Reading z-score	0.58**
	(0.27)
(4) Math z-score	0.24
	(0.17)
Number of observations	1,773

Notes: This table presents the results from a regression of student-level outcomes on a binary variable for monitoring, among the sample of adult education villages.. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table A1 Attrition

	(1)	(2)	(3)
	Comparison	**Adult Education Only**	**Adult Education + Monitoring**
Panel A. Attrition	Mean (s.d.)	Coef (s.e.)	Coef (s.e.)
Attrition	0.05	0.04*	-0.04**
	(0.22)	(0.02)	(0.01)
Panel B. Characteristics of Non-Attriters			
Female	0.69	0.03*	-0.03
	(0.46)	(0.02)	(0.02)
Age	31.83	1.80	0.19
	(12.41)	(1.45)	(0.90)
Mayahi	0.31	0.00	0.00
	(0.46)	(0.00)	(0.00)

Notes: Panel A shows the results of a regression of a binary variable for attrition on adult education, monitoring and stratification fixed effects. Panel B shows the results of a regression of student characteristics among non-attriters on adult education, monitoring and stratification fixed effects. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table A2. Lee Bounds

	(1) Lower Bound	(2) Upper Bound
Panel A: Reading		
(1) Letters	0.13*	0.25**
	(0.07)	(0.10)
(2) Syllables	0.26**	0.42***
	(0.11)	(0.14)
(3) Words	0.12*	0.32***
	(0.07)	(0.10)
(4) Phrases	0.12*	0.32***
	(0.07)	(0.10)
(5) Composite Reading Z-Score	0.13*	0.33***
	(0.07)	(0.11)
Panel B: Math		
(6) Number identification	0.07	0.50***
	(0.05)	(0.06)
(7) Quantity Comparison	0.10*	0.13**
	(0.06)	(0.06)
(8) Addition and Subtraction	0.12**	0.24***
	(0.06)	(0.08)
(9) Multiplication and division	0.06	0.21**
	(0.06)	(0.09)
(10) Composite Math Z-Score	0.08	0.21***
	(0.06)	(0.07)

Notes: This shows the results of Lee bounds correcting for non-differential attrition between monitoring and non-monitoring villages. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level

Table A3. Bonferroni Corrections

	(1)	(2)	(3)	(4)	(5)	(6)
	Letters	Syllables	Words	Phrases	Comprehension	Composite Score
Panel A: Reading						
(1) Bonferroni-Corrected p-values	0.017**	0.008***	0.056*	0.076*	0.066*	0.027**
Panel B: Math	Number Identification	Quantity Comparison	Addition and Subtraction	Multiplication and Division	Word Problems	Composite Score
(1) Bonferroni-Corrected p-values	0.015**	0.473	0.007***	0.138	0.265	0.015**

Table A4. Bootstrapped Standard Errors

Panel A: Reading	(1)	(2)	(3)	(4)	(5)
	Letters	Syllables	Words	Phrases	Composite Score
(1) Adult education	0.249**	0.230**	0.120	0.138	0.22**
	(0.105)	(0.0970)	(0.0818)	(0.0883)	(0.100)
(2) Adult education*monitor	0.18*	0.28**	0.14*	0.14	0.17*
	(0.10)	(0.12)	(0.08)	(0.09)	(0.09)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,773	1,773	1,773	1,773	1,773
R-squared	0.02	0.01	0.01	0.01	0.02

Panel B: Math	(1)	(2)	(3)	(4)	(5)
	Number Identification	Quantity Comparison	Addition and Subtraction	Multiplication and Division	Composite Score
(1) Adult education	0.12	0.0840	0.205**	0.154*	0.128
	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)
(2) Adult education*monitor	0.11*	0.125**	0.151*	0.0866	0.165**
	(0.06)	(0.06)	(0.08)	(0.08)	(0.08)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,773	1,773	1,773	1,773	1,773
R-squared	0.02	0.01	0.02	0.01	0.02

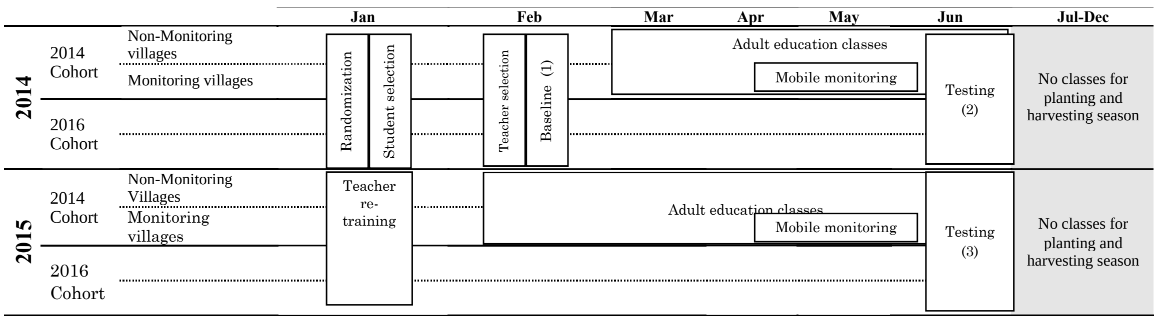
Notes: This table presents the results from a regression of different reading outcomes on adult education (only), adult education plus monitoring and randomization fixed effects. Bootstrap-t standard errors are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table A5. Excluding Called Students					
Panel A: Reading					
	(1)	(2)	(3)	(4)	(5)
	Letters	Syllables	Words	Phrases	Composite Score
(1) Adult education	0.27***	0.22**	0.13	0.14*	0.23**
	-0.1	(0.10)	(0.08)	(0.09)	(0.10)
(2) Adult education*monitor	0.16*	0.26**	0.11	0.10	0.16*
	-0.09	(0.13)	(0.08)	(0.08)	(0.09)
Strata fixed effects	Yes	-0.23	Yes	Yes	Yes
Observations	1,732	1,732	1,732	1,732	1,732
R-squared	0.02	0.01	0.01	0.01	0.02
Panel B: Math	(1)	(2)	(3)	(4)	(5)
	Number Identification	Quantity Comparison	Addition and Subtraction	Multiplication and Division	Composite Score
(1) Adult education	0.12*	0.08	0.21**	0.17*	0.13
	(0.07)	(0.07)	-0.09	(0.09)	(0.08)
(2) Adult education*monitor	0.10*	0.13**	0.14*	0.07	0.16**
	(0.06)	(0.06)	-0.08	(0.08)	(0.07)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,732	1,732	1,732	1,732	1,732
R-squared	0.02	0.01	0.02	0.01	0.02

Notes: This table presents the results from a regression of different reading outcomes on adult education (only), adult education plus monitoring and randomization fixed effects. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

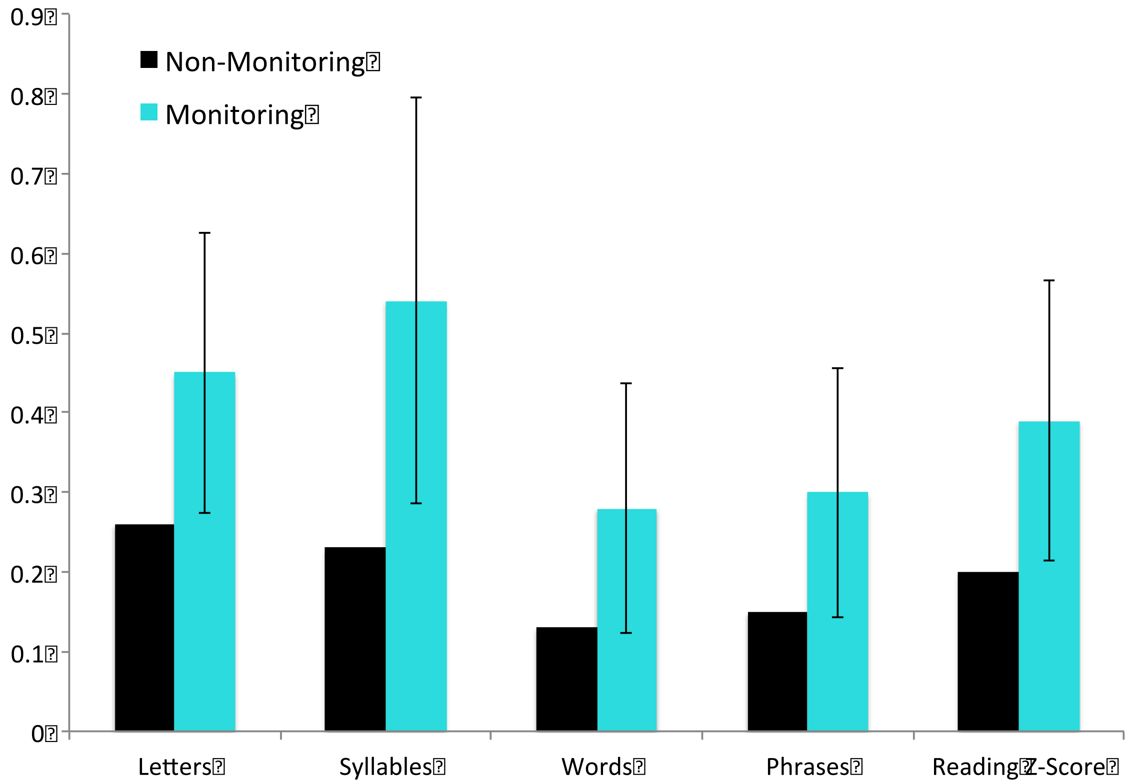
Figure 1. Map of Intervention Areas

Figure 2. Timeline of Activities



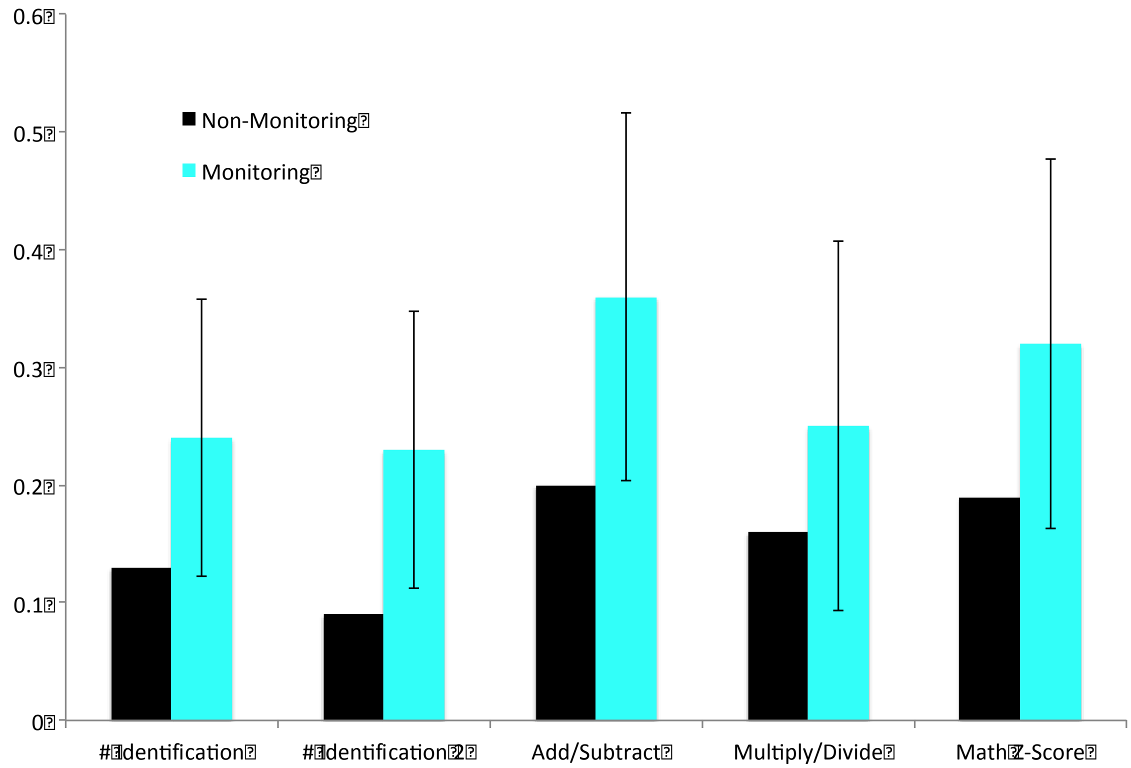
Note: Figure shows the timeline of activities for the different groups in our study. The 140 villages receiving adult education classes either did not receive extra monitoring attention (Non-monitoring villages) or received the mobile phone-based monitoring (Monitoring villages). The 2016 cohort is the group of 20 comparison villages, in which no adult education program was implemented in 2014, and which serve to estimate the impacts of the literacy program in concurrent research.

Figure 3A. Impact of Monitoring on Reading Timed Z-Scores



Notes: This figure shows the mean reading z-scores of different reading tasks of the monitoring and non-monitoring villages, controlling for stratification fixed effects. Reading scores are normalized according to contemporaneous reading scores in comparison villages. Standard errors are corrected for heteroskedasticity and clustering at the village level.

Figure 3B. Impact of Monitoring on Math Z-Scores



Notes: This figure shows the mean math z-scores of different math tasks of the monitoring and non-monitoring villages, controlling for stratification fixed effects. Math scores are normalized according to contemporaneous math scores in comparison villages. Standard errors are corrected for heteroskedasticity and clustering at the village level.

Figure 4. Cost effectiveness of the Mobile Monitoring Intervention