"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 134 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 financial incentive component to the program. The monitoring intervention dramatically affected student performance: Reading and math test scores were .07-.18 s.d. higher in monitoring villages than in non-monitoring villages, with stronger effects during the second year. We provide evidence on the potential 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 134 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 0.07–0.18 standard deviations (SD) higher in the mobile monitoring villages immediately after the program, with a statistically significant impact. The effects were stronger during the first year, although were still positive during the second year. 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 higher 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.[1] Our

[1] Most recently, de Ree et al (2016) estimate the impact of an unconditional doubling of teachers' salaries in Indonesia, finding an improvement in teachers' job satisfaction but no impact on teacher effort or students' learning outcomes.

experiment is somewhat unique in that it did not provide any explicit financial incentives.[2]

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

Over a two-year period (2014 and 2015), 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

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

agricultural planting and 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.[3] 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.[4] Since men's and women's classes differed by both gender and class size, we are unable to disentangle the differential effects of gender 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.[5] 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 if the respondent had any additional information to share. The mobile monitoring component was introduced two months after the start of

[3] 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.
[4] 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.
[5] 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.

the adult education program, and neither students, teachers, nor CRS field staff were informed of which villages were selected prior to the calls. [6]

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 over the two-year period. 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. In all, 23 percent of teachers were replaced between the first and second years, with no correlation between the monitoring intervention and the probability of firing.[7]

B. Experimental Design

In 2013, CRS identified over 500 intervention villages across two regions of Niger, Maradi and Zinder. Of these, we randomly sampled 140 villages as part of the research program. Among these 140 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 (originally supposed to start classes in 2016).[8] Among the adult education villages, villages were then assigned to either the monitoring or no monitoring intervention. In all, 120 villages were assigned to the adult education program and 20 villages were

[6] 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).
[7] 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.
[8] Due to funding constraints, CRS could not introduce adult education into the control villages in 2016, but had planned to do so in 2017.

assigned to the pure control group.[9] Among the adult education villages, 60 villages were assigned to monitoring and 60 to the no monitoring condition.[10] Nevertheless, the final sample in this paper is 131 villages, 20 in the control group and 111 in the adult education program.[11] A timeline of the implementation and data collection activities is provided in Figure 1.

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

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[9] 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 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. In addition, while we originally started with 132 villages, one village had to be removed from the program (and the research) due to internal village issues.

[10] In 2016, 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).

[11] During the baseline, several villages initially assigned to the program and research were removed from the program, primarily due to the NGO's ability to register adult education participants in a timely manner. Two villages were removed due to internal village conflicts between two village chiefs. These were primarily in the Zinder region. Thus, the final sample was 131 villages, with 111 villages in the adult education program, 58 in the monitoring condition and 53 in the no monitoring condition.

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 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:

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

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:

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

Whether or not teacher's effort ($\boldsymbol{e}$) 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.[12] Assuming that the teacher accepts the NGO's offer, the teacher will then choose effort to maximize his/her expected utility.

[12] 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

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 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:

$$\begin{aligned} U^{NS} &= w_{NGO} - e \\ U^{S,i} &= (1-\theta_T)w_{NGO} + \theta_T p_m (w^i_m - e_m) \end{aligned} \tag{3}$$

Thus, the optimal w^{i*}_m 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:

$$y_i = y(e^t_i) \begin{cases} y(0) if\ e = 0 \\ y(1) if\ e = 1 \end{cases} \tag{4}$$

where e^t_i 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:

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

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 $\frac{\partial \bar{y}}{\partial p_m} < 0$.[13]

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 five primary sources. First, we conducted individualized math and reading tests and use these scores to measure the

[13] 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.

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. Fourth, we collected student attendance data from the centers. And finally, we conducted modified Stallings classroom observations in a subset of villages. Before presenting our estimation strategy, we discuss each of these data sources in detail.

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 140 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).[14] 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 the non-baseline villages in August 2014 and 2015, 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

[14]To choose the baseline villages, we stratified by region, sub-region and treatment status and selected a random sample of villages for the baseline.

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).

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)."[15] 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

[15]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).

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 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 rounds in 2014 and 2015. 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 and a follow-up sample in all villages in December 2015. 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, we conducted a survey of all teachers in adult education villages, which included an intrinsic motivation inventory (IMI), in 2014 and 2015. 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.[16] We applied one of the versions of the IMI to our specific context, namely, teachers' experience in teaching the adult education program.

[16]Although the overall questionnaire is called the IMI, the interest/enjoyment subscale is the only one that assesses intrinsic motivation. "The perceived choice and perceived competence concepts are theorized to be positive predictors of both self-report and behavioral measures of intrinsic motivation, and pressure/tension is theorized to be a negative predictor of intrinsic motivation." http://selfdeterminationtheory.org/intrinsic-motivation-inventory/.

In addition to data on teachers' characteristics, motivation and outside employment options, at the end of the adult education courses in May 2016, we also conducted classroom observations in five villages, using the modified Stallings classroom observation tool. While the sample size is too small to conduct statistical tests, we use these data to provide some supporting evidence as to whether the monitoring calls affected teaching quality.

D. Student Attendance

The final data are monthly student attendance data collected from a subset of intervention villages from CRS in 2015. These data are used to provide a "check" on teacher self-reported attendance, as well as to understand whether the interventions affected students' attendance within the classroom.

E. 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 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.[17]

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 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

[17] 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.

or math test score attained by student i in village v immediately after the program in 2014 and 2015.[18] $adulted_v$ 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, included as a robustness check. We pool observations across the two years and estimate the following specification:

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

The coefficients of interest are β_1 and β_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, and add in a linear time trend as a robustness check.

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 2A and 2B depict the mean normalized reading and math test scores for the adult education villages with and without monitoring across both years. Test scores are normalized using the mean and s.d. of contemporaneous test scores in comparison

[18]There are a number of ways that raw EGRA and EGMA scores can be used and transformed for analysis, including the raw untimed scores, raw timed scores (especially for reading scores), untimed normalized scores and timed normalized scores. The results in the tables show the timed normalized scores for reading and the untimed normalized scores for math, as is the convention for EGRA and EGMA. Results are largely robust to using raw non-normalized scores.

villages.[19] The means of the control group are not shown for ease of exposition. Three things are worth noting. First, the adult education program increases reading and math scores significantly as compared to the control group, with relatively stronger effects on reading, although very few achieved the “threshold” reading level of 45-60 words per minute. Second, the impacts are stronger for simpler reading “decoding” tasks, i.e., letter, syllable or word recognition, as compared with reading phrases or reading comprehension. Yet the effects are stronger for slightly more difficult math tasks, such as addition, subtraction, multiplication and division.[20] And third, the difference in test scores between monitoring and non-monitoring villages equivalent to – or 60% percent of - the difference in test scores between the non-monitoring villages and the comparison group, especially for simpler reading and math tasks. This suggests important learning gains from the monitoring program.

A. Immediate Impact of the Program

Table 2 presents the results of Equation (3) for reading z-scores across both years of the program. Across all reading tasks, the adult education program increased students' reading test scores by .14-.30 s.d. over the two year period, with a statistically significant effect (Table 2, Panel A). Similar to Figure 2, the impacts are stronger for simpler decoding tasks and the composite reading score. The adult education impacts are

[19] Normalizing the z-scores by region also yields similar results.
[20]Students in adult education villages without mobile monitoring did worse in quantity comparison as compared with the control group. This task asks students to compare a set of two numbers with three digits that closely resemble each other. Psychology shows that individuals use simple cognitive shortcuts when processing information, such as “left digit bias”, or the tendency to focus on the left-most digit of a number while partially ignoring other digits (Lacetera et al 2012). Whereas those who are non-literate guess for this task (and are correct 50% of the time), those who are neo-literate try. However, if they are not yet literate enough to process all of the information provided, we posit that they left-most digit (ignoring other numbers) and thus may guess incorrectly. This task is designed to text for this, as most of the number start with the same left-hand digit.

relatively stronger in one region, Maradi (Panel C), as compared to Zinder (Panel B). Overall, baseline reading test scores were lower in Maradi.

The monitoring intervention increased reading test scores by .07-.19 s.d., with a statistically significant effect at the 10 percent levels for four of the six reading measures, including the composite reading score. The impact of the monitoring intervention is similar in magnitude across regions, although slightly stronger in Zinder, where the intervention was almost equal to the adult education gains (Panel B). These results are also robust to using an alternative specification of the dependent variable, namely, raw untimed reading scores (Table A2).

The results are similar for math z-scores (Table 3): the adult education program increased math z-scores by .13-.29 s.d. as compared with the control group (Panel B, Column 1), with statistically significant effects at the 1 and 5 percent levels for more difficult math tasks and the composite score.[21] The program was successful in moving students beyond number identification to more complicated mathematical tasks, namely addition, subtraction, multiplication and division. Again, the adult education impacts are stronger in the Maradi region (Panel C), although the differences between regions are not statistically significant. Overall, the monitoring intervention increased test scores by .07-.12 s.d., with statistically significant effects primarily for simpler tasks (Panel A).

B. Effects of the Program over Time

While the results in Tables 2 and 3 suggest that the monitoring intervention increased reading and math z-scores as compared with the standard adult education

[21]While math tasks in the EGMA tests are also timed, most analyses use untimed scores in the analysis. An interesting impact is the negative coefficient on the "quantity comparison" task; this task essentially involves asking students to compare similar three-digit numbers and note which one is larger. The adult education program was not successful in improving students' recognition of these numbers, potentially due to left-digit bias. However, the monitoring program mitigated these negative effects.

program, a key question is the dynamics of these effects over time, once teachers learned more about the monitoring intervention and adults achieved higher learning outcomes.

Tables 4 and 5 shows the results of the monitoring intervention on reading and math scores by year. Overall, two things are worth noting. First, both reading and math score *levels* were higher in the second year, although the learning effects of the adult education program are primarily stronger for math over time.[22] Second, the effects of the monitoring intervention are positive and statistically significant for reading and math in the first year, primarily for the simpler reading and math tasks and the composite scores (Tables 4 and 5, Panel A). These effects range from .06-.24 s.d. for reading and .00-.14 s.d. for math, with statistically significant effects at the 5 and 10 percent levels. Yet the coefficients for the monitoring intervention are not statistically significant for any of the reading tasks in the second year, and for only one of the math tasks.[23]

Does this lack of statistical significance in the second year suggest that the intervention was less effective due to teachers' learning about the intervention? While the coefficients are not statistically significant, they are all positive and of relatively large magnitude, representing 30-70% of the coefficients on the adult education program. Given the higher test scores during the second year, the learning effects due to monitoring would have had to increase significantly during the second year in order to detect an effect, which may difficult due to non-linearities in learning. In addition, as mentioned above, the monitoring intervention was modified slightly in the second year, so that a subset of villages received the full monitoring intervention, whereas the

[22]This is also the case if we pool the 2014 and 2015 data and include a time trend interacted with both the adult education and monitoring treatments. While the coefficient on the interaction terms for the adult education and time trend are positive, they are primarily statistically significant for math.
[23]Estimating the regressions jointly with an interaction between the adult education, monitoring and time fixed effects supports these results.

remaining monitoring villages only called the teacher. Restricting the sample to the full monitoring intervention, the coefficients on the adult education program are the same as in the full sample; however, all of the coefficients on the monitoring intervention are greater than or equal to the monitoring coefficients in the full sample (Tables 4 and 5, Panel C). For math in particular, the full monitoring intervention was successful in increasing multiplication and division z-scores and quantity comparisons by .20 s.d., with a statistically significant effect at the 5 and 10 percent levels. These were two tasks that are traditionally more difficult. Thus, these results suggest that the monitoring effects were not necessarily lower in the second year because of teachers' learning about the intervention; however, they suggest that full monitoring intervention was necessary to improve learning.

C. Heterogeneous Effects by Gender and Teachers' Characteristics

We might expect greater impacts of the monitoring intervention among certain sub-populations, such as men and women, or according to teachers' characteristics, as predicted by our model. Table 6 tests for heterogeneous impacts of the program by the student's gender, while Table 7 tests for heterogeneous effects by teacher characteristics.

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 differential impacts by students' gender. As women belonging to particular ethnic groups in Niger travel outside of their home village less frequently than men, women may have had fewer opportunities to practice their newly-acquired skills outside of class. In addition, given the larger student-to-teacher ratio in women's classes in the program, this could have negatively affected women's learning outcomes. Table 6 presents the results by gender. Overall, women in the control group had significantly lower reading and math scores than men in control villages, confirming the “gender gap” in education skills in Niger. The adult education program increased men’s reading and math z-scores by .11-.56 s.d., with relatively stronger effects on for simpler reading tasks and math skills. While women’s reading and math z-scores were lower than men’s, the results are not statistically significant. The monitoring component had a positive impact on men’s test scores, primarily for reading, although these impacts are not statistically significant for most tasks. Overall, the monitoring effects do not differ by gender. This is perhaps unsurprising, as the same teacher taught both men’s and women’s classes in the village. At the same time, since women’s class sizes were also larger, it is difficult to disentangle the “gender” effect from the “class size” effect in these results.

Table 7 presents the impact of the monitoring program on reading and math composite z-scores by teachers’ characteristics, such gender, education, previous experience as an adult education teacher and whether the teacher lives within the village.[24] In many villages in the Maradi and Zinder regions of Niger, women often do not migrate, and therefore female literacy teachers might have had more constrained labor market options, thereby making the monitoring component more effective. On the other hand, teachers with higher levels of education should have better outside options, thereby reducing the effectiveness of monitoring component.

Similar to the previous results, mobile monitoring is associated with positive improvements in reading and math z-scores. Overall, the monitoring intervention is stronger for female teachers, for teachers with more experience and for “local” teachers – i.e., those based either in the village or within a 5-km radius. Although the differential

[24]All regressions are conditional on the presence of an adult education program in the village.

effect of monitoring by teachers' gender is not statistically significant, it is large in magnitude, and supports evidence that female teachers often have lower outside options, and therefore might be more responsive to monitoring. In the case of teacher experience, teachers' experience is negatively correlated with learning outcomes – thereby suggesting that such teachers are not putting in the same level of effort as newer teachers, and the monitoring intervention is mitigating this effect. Finally, while teachers' absence may be easier to observe within the village, the monitoring intervention appears to have made that absent more salient. Yet there are no other heterogeneous effects by other teacher characteristics, such as teacher education levels and their ability to find outside work.[25]

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, both in terms of the number of days taught and classroom pedagogy, 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 hence impact of the program. Third, having a more present and motivated teacher could potentially affect students' effort, leading to increased class participation and attendance. Fourth, as the monitoring component involved students, the calls could have motivated students independently, who in turn had spillover effects on their fellow learners. And finally, since the monitoring component also involved village chiefs, this could have increased their interest in community-level

[25]Looking at the correlates of teacher characteristics and monitoring in the adult education villages without monitoring ,

development programs, thereby motivating teachers and students. 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 and hence students' performance in two ways. First, it could have encouraged teachers to teach more classes. Second, it could have improved teachers' efficacy within the classroom, thereby improving student learning. Overall, 70% of teachers thought the calls were from CRS, whereas 29% from the Ministry, suggesting that they understood that the calls were from "higher-ups". In addition, teachers were generally aware of the other calls occurring: 80% of teachers knew the village chief was called, and 77% knew the students were called.

As we are unable to directly observe teacher observable effort across all classes, we assess these mechanisms using a number of proxies. To measure teachers' presence within the classroom, we use a self-reported measure as to whether teachers' stopped the class and the number of days stopped, as well as attendance measures that count the number of classes taught within the month and whether the teacher was replaced. Table 8 shows the results of the monitoring component on a variety of these indicators. While monitoring teachers reported "The…calls prevent us from missing courses", and that "Someone who works must be 'controlled'", the monitoring intervention did not appear to have a strong impact on teachers' presence within the classroom, even though these indicators were imperfectly measured. Teachers in monitoring villages were not less likely to stop teaching at any point as compared with their non-monitoring counterparts, although they reported being absent for 1.30 fewer days than the non-monitoring teachers, with a statistically significant difference at the 10 percent level (Panel A). The primary reasons cited for absence in both monitoring and non-monitoring villages were illness, funerals and agricultural work. While this suggests that the monitoring intervention may have increased the number of days that teachers were in the classroom, these indicators are self-reported, and the margin of this effect is quite small – only 1.30 days - and therefore could explained by improvements in learning.[26] This is confirmed, in part, by CRS' attendance records: teachers taught an average of 19.25 days in a month (out of 20 days), and monitoring teachers were not more likely to taught more classes (Panel B). In addition, there was no correlation between monitoring and the teacher's likelihood of being replaced between the first and second year. While this could, in part, be explained by the weakness of the administrative data used to monitor teachers' presence in the classroom, and the use of this information to sanction teachers, the monitoring intervention did not appear to have strong effects on the number of days taught.[27]

Other than teacher absence, the calls could have affected teachers' motivation, thereby making them more effective in class.[28] While often there is concern that external incentives (such as monitoring) may crowd out intrinsic motivation, teachers in our sample reported that the calls "prove that our work is important" and that they "give is courage". We proxy motivation in two primary ways: an observable measure of teacher

[26]Despite the fact that these data were self-reported, there was a high intra-village correlation of responses amongst teachers, village chiefs and students in monitoring villages, even when the teacher was absent. While this could be due to either collusion or a high degree of information-sharing amongst the stakeholders, CRS did not use the monitoring data to make firing decisions between the first and the second year.

[27]Of the 113 adult education villages, CRS had attendance records for a subset of villages.

[28]We attempted to collect classroom observation data using a modified Stallings observation tool in May 2016, after the end of the classes, we were only able to go to a small number of villages (5) before adult education classes ended for the season.

effort and the intrinisic motivation inventory (IMI). In Panel C, while 28% of teachers kept their own attendance logs, monitoring teachers were 18 percentage points more likely to keep their own attendance logs, with a statistically significant effect at the 5 percent level. As such logs were suggested – but not required or verified – but CRS, this suggests that monitoring teachers were more willing to invest in teaching preparation. When looking at the IMI scores, we use several sub-scales of the inventory, namely intrinsic motivation, perceived competence, perceived pressure and perceived choice. While the monitoring intervention did not have an impact on teachers' perceived competence, pressure of choice z-scores, monitoring teachers had intrinsic motivation z-scores that were .24 s.d. higher than their non-monitoring counterparts, with a statistically significant effect at the 10 percent level.

B. Student Effort and Motivation

The monitoring component could have encouraged greater student effort within the classes, as measured by student dropout, attendance and motivation. While we do not have reliable data on student attendance, we do have self-reported measures of student dropout, the reasons for dropping out and the duration of time in the course. Table 9 shows these results. Overall, 27% of students dropped out fo the course at some point in the time over the two-year period, and the monitoring component did not appear to affect the likelihood of student dropout (Table 9, Panel A). Since a majority of those who dropped out primarily did so for reasons outside of their control, namely, pregnancy, illness or a death in the family, the lack of an observed impact is perhaps not surprising. For those who stayed in the course, the monitoring intervention did not appear to affect how long students stayed in the course; on average, non-monitoring students stayed in the course for approximately two months each year, with a similar rate by monitoring students. Thus, this suggests that students were not necessarily spending more time in the course.

Similar to intrinsic motivation for teachers, we also measure students' motivation by using self-esteem and self-efficacy measures (Panel B). All scores are normalized with respect to the contemporaneous means in the pure control group. Overall, respondents' self-esteem and self-efficacy z-scores were .12-.17 s.d. lower in the adult education as compared to students control villages, with a statistically significant effect for self-efficacy scores (Panel B). The monitoring component seems to mitigate this effect: respondents in monitoring villages had z-scores that were .03-.08 s.d. higher, although these effects are not statistically significant at conventional levels.[29]

Nevertheless, there is some evidence that the monitoring component affected student learning via the mechanism of calling students themselves. Panel C 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 directly test this hypothesis.[30]

VII. Alternative Explanations

[29]While potentially surprising, these results mirror those found in Aker et al (2015), who found that students' perceptions of self-esteem changed over time, particularly when they experienced learning failures.

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

There are several 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 implementation of 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), primarily during the first year. Women are slightly more likely to remain within the sample in adult education villages (without monitoring) as compared to the control. Since women had lower reading and math z-scores overall, this may underestimate the effects of the adult education program alone. By contrast, there are fewer women within the monitoring villages (as compared with the non-monitoring villages), which could potentially overestimate the effects of the monitoring program as compared with the adult education program. As we are primarily concerned with this latter comparison, we use Lee bounds to correct for bias for differential attrition between the monitoring and non-monitoring villages. Table A3 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 that were previously significant.

Third, 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.

Finally, for some of the student and teacher survey measures, there could be concerns about non-classical measurement errors, as teachers and students systematically report in ways that would bias the results. While this is an obvious concern for self-reported attendance data, when possible, we attempted to verify these results with administrative data. For the student test score data, however, as these are timed tests that objectively measure students' learning, and cannot be manipulated with additional effort, we are less concerned about this potential issue.

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 3). 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.

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						
	(1)	(2)	(3)	(4)	(5)	(6)
	Comparison Group	**Monitoring**	**Adult Educ.**	**Difference**	**Difference**	p-value
	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)	Coeff (s.e)	Coeff (s.e.)	
Household Characteristics at Baseline				(2)-(1)	(3)-(1)	(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 2014-2015						
	(1)	(2)	(3)	(4)	(5)	(6)
	Letters	Syllables	Words	Phrases	Comprehension	Composite Score
Panel A: All Villages						
(1) Adult education	0.30***	0.23***	0.15*	0.15*	0.14*	0.23***
	(0.10)	(0.09)	(0.08)	(0.08)	(0.08)	(0.09)
(2) Adult education*monitor	0.18*	0.19**	0.14*	0.10	0.07	0.16*
	(0.09)	(0.09)	(0.08)	(0.08)	(0.08)	(0.09)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,481	3,482	3,482	3,478	3,482	3,482
R-squared	0.02	0.01	0.01	0.01	0.01	0.02
Total effect: Adult Education + Monitoring						
p-value (Adult education + monitor=0)	*.00****	*.00****	*.00****	*.01***	*.05***	*0.00****
Panel B: Zinder						
(1) Adult education	0.23*	0.17	0.10	0.11	0.07	0.17
	(0.12)	(0.11)	(0.09)	(0.09)	(0.09)	(0.10)
(2) Adult education*monitor	0.19	0.24*	0.17	0.11	0.08	0.17
	(0.13)	(0.14)	(0.11)	(0.10)	(0.10)	(0.12)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,794	1,795	1,795	1,792	1,795	1,795
R-squared	0.02	0.02	0.01	0.01	0.01	0.02
Total effect: Adult Education + Monitoring						
p-value (Adult education + monitor=0)	*.01***	*0.02***	*0.06**	*0.12*	*0.37*	*.02***
Panel C: Maradi						
(1) Adult education	0.44***	0.33**	0.23	0.23	0.27**	0.34**
	(0.15)	(0.13)	(0.14)	(0.14)	(0.13)	(0.15)

(2) Adult education*monitor	0.17	0.15	0.11	0.10	0.06	0.15
	(0.13)	(0.13)	(0.11)	(0.11)	(0.11)	(0.12)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,686	1,687	1,687
R-squared	0.02	0.01	0.01	0.01	0.01	0.02
Total effect: Adult Education + Monitoring						
p-value (Adult education + monitor=0)	*.00****	*.00****	*.08**	*.09**	*.03***	*.00****

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 for 2014 and 2015. 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), 2014-2015

	(1)	(2)	(3)	(4)	(5)	(6)
	Number Identification	Quantity Comparison	Addition and Subtraction (Simple)	Addition and Subtraction (Difficult)	Multiplication and Division	Composite Score
Panel A: All Villages						
(1) Adult education	0.13*	-0.04	0.29***	0.23***	0.20**	0.20**
	(0.07)	(0.05)	(0.09)	(0.08)	(0.08)	(0.08)
(2) Adult education*monitor	0.11*	0.07*	0.12	0.09	0.09	0.12
	(0.06)	(0.04)	(0.08)	(0.08)	(0.07)	(0.07)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,462	3,470	3,478	3,480	3,480	3,455
R-squared	0.01	0.00	0.02	0.01	0.01	0.02
Total effect: Adult Education + Monitoring						
p-value (Adult education + monitor=0)	*.00****	*0.21*	*.00****	*.00****	*.00****	*.00****
Panel B: Zinder						
(1) Adult education	0.10	-0.07	0.26**	0.20**	0.19*	0.17
	(0.09)	(0.06)	(0.12)	(0.10)	(0.10)	(0.11)
(2) Adult education*monitor	0.13	0.03	0.13	0.09	0.04	0.13
	(0.09)	(0.07)	(0.12)	(0.12)	(0.11)	(0.11)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,780	1,785	1,788	1,789	1,789	1,777
R-squared	0.01	0.00	0.03	0.01	0.02	0.01
Total effect: Adult Education + Monitoring						
p-value (Adult education + monitor=0)	*.04***	*0.54*	*.00****	*.02***	*.05***	*.03***

Panel C: Maradi						
(1) Adult education	0.19	0.05	0.32**	0.29**	0.24	0.26**
	(0.12)	(0.08)	(0.15)	(0.13)	(0.16)	(0.13)
(2) Adult education*monitor	0.09	0.11**	0.11	0.09	0.12	0.11
	(0.08)	(0.05)	(0.10)	(0.11)	(0.10)	(0.10)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,682	1,685	1,690	1,691	1,691	1,678
R-squared	0.01	0.00	0.01	0.01	0.01	0.01
Total effect: Adult Education + Monitoring						
p-value (Adult education + monitor=0)	*.06**	*.05***	*.02***	*.01***	*.08**	*.02***

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 for 2014 and 2015. 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. Reading Timed Z-Scores by Year						
	(1)	(2)	(3)	(4)	(5)	(5)
	Letters	Syllables	Words	Phrases	Comprehension	Composite Score
Panel A: 2014						
(1) Adult education	0.27***	0.23**	0.13	0.14*	0.14	0.22**
	(0.10)	(0.09)	(0.08)	(0.09)	(0.09)	(0.10)
(2) Adult education*monitor	0.20**	0.24**	0.15*	0.12	0.06	0.19**
	(0.10)	(0.10)	(0.08)	(0.09)	(0.08)	(0.09)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,760	1,760	1,760	1,759	1,760	1,760
R-squared	0.02	0.02	0.01	0.01	0.01	0.02
Total effect: Adult Education + Monitoring						
p-value (Adult education + monitor=0)	*.00****	*.00****	*.00****	*.02***	*0.11*	*.00****
Panel B: 2015						
(1) Adult education	0.33***	0.24***	0.17**	0.15*	0.13*	0.24***
	(0.10)	(0.09)	(0.08)	(0.08)	(0.08)	(0.09)
(2) Adult education*monitor	0.15	0.13	0.12	0.08	0.09	0.12
	(0.10)	(0.09)	(0.08)	(0.07)	(0.08)	(0.09)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,721	1,722	1,722	1,719	1,722	1,722
R-squared	0.02	0.01	0.01	0.01	0.01	0.02
Total effect: Adult Education + Monitoring						
p-value (Adult education + monitor=0)	*.00****	*.00****	*.00****	*.02***	*.03***	*.00****
Panel C: 2015 for Joint Monitoring						
(1) Adult education	0.33***	0.24***	0.16**	0.15*	0.14*	0.24***
	(0.10)	(0.09)	(0.08)	(0.08)	(0.08)	(0.09)

(2) Adult education*monitor	0.18	0.14	0.16	0.13	0.16	0.16
	(0.13)	(0.12)	(0.11)	(0.10)	(0.11)	(0.12)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,334	1,335	1,335	1,333	1,335	1,320
R-squared	0.03	0.02	0.02	0.02	0.02	0.03
Total effect: Adult Education + Monitoring						
p-value (Adult education + monitor=0)	*.00****	*.00****	*.02***	*.02***	*.03***	*.00****

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 for 2014 and 2015. 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. Math Untimed Z-Scores by Year

	(1) Number Identification	(2) Quantity Comparison	(3) Addition and Subtraction (Simple)	(4) Addition and Subtraction (Difficult)	(5) Multiplication and Division	(6) Composite Score
Panel A: 2014						
(1) Adult education	0.08	-0.03	0.22**	0.16*	0.17**	0.15*
	(0.07)	(0.07)	(0.09)	(0.08)	(0.09)	(0.08)
(2) Adult education*monitor	0.14**	-0.00	0.17**	0.10	0.08	0.14*
	(0.06)	(0.06)	(0.08)	(0.08)	(0.08)	(0.07)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,758	1,751	1,759	1,761	1,761	1,751
R-squared	0.01	0.01	0.02	0.01	0.01	0.01
Total effect: Adult Education + Monitoring						
p-value (Adult education + monitor=0)	*.00****	*0.89*	*.00****	*.00****	*.01****	*.00****
Panel B: 2015						
(1) Adult education	0.18**	-0.05	0.34***	0.28***	0.21**	0.25***
	(0.08)	(0.06)	(0.10)	(0.09)	(0.10)	(0.09)
(2) Adult education*monitor	0.08	0.16***	0.10	0.12	0.12	0.11
	(0.07)	(0.05)	(0.08)	(0.09)	(0.08)	(0.08)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,704	1,719	1,719	1,719	1,719	1,704
R-squared	0.01	0.01	0.03	0.02	0.02	0.02
Total effect: Adult Education + Monitoring						
p-value (Adult education +	*0.00****	*.01****	*.00****	*.00****	*.00****	*.00****

monitor=0)						
Panel C: 2015 for Joint Monitoring						
(1) Adult education	0.18**	-0.04	0.34***	0.29***	0.22**	0.25**
	(0.08)	(0.06)	(0.11)	(0.09)	(0.10)	(0.10)
(2) Adult education*monitor	0.12	0.21***	0.15	0.18	0.23**	0.17
	(0.10)	(0.06)	(0.11)	(0.12)	(0.10)	(0.11)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,318	1,331	1,331	1,331	1,331	1,318
R-squared	0.02	0.02	0.04	0.03	0.03	0.02
Total effect: Adult Education + Monitoring						
p-value (Adult education + monitor=0)	*.01***	*.00****	*.00****	*.00****	*.00****	*.00****

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 for 2014 and 2015. 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 Gender

	Reading Z-Scores						Math Z-Scores				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Letters	Syllables	Words	Phrases	Comprehension	Reading Composite Score	Number Identification	Quantity Comparison	Addition and Subtraction (Simple)	Addition and Subtraction (Difficult)	Multiplication and Division
lucation	0.52**	0.44**	0.34	0.30	0.25	0.42*	0.12	0.19*	0.43***	0.56***	0.34**
	(0.23)	(0.22)	(0.21)	(0.21)	(0.21)	(0.22)	(0.10)	(0.10)	(0.16)	(0.17)	(0.16)
nonitor	0.27	0.35*	0.22	0.20	0.20	0.26	0.01	0.04	0.08	0.11	0.08
	(0.19)	(0.21)	(0.18)	(0.18)	(0.17)	(0.19)	(0.09)	(0.08)	(0.13)	(0.17)	(0.15)
lucation*female	-0.27	-0.27	-0.24	-0.19	-0.14	-0.23	0.08	-0.31**	-0.19	-0.45**	-0.18
	(0.23)	(0.23)	(0.22)	(0.23)	(0.22)	(0.23)	(0.12)	(0.13)	(0.15)	(0.17)	(0.16)
nonitor*female	-0.18	-0.29	-0.17	-0.18	-0.21	-0.19	0.10	0.03	0.04	-0.04	-0.02
	(0.19)	(0.21)	(0.19)	(0.18)	(0.17)	(0.19)	(0.10)	(0.09)	(0.13)	(0.17)	(0.15)
	-0.75***	-0.69***	-0.60***	-0.62***	-0.65***	-0.76***	-1.40***	-0.30***	-0.79***	-0.62***	-0.68***
	(0.19)	(0.19)	(0.18)	(0.18)	(0.18)	(0.18)	(0.10)	(0.11)	(0.12)	(0.12)	(0.10)
effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ns	3,480	3,481	3,481	3,477	3,481	3,481	3,462	3,470	3,477	3,479	3,479
	0.15	0.12	0.11	0.10	0.10	0.14	0.29	0.06	0.15	0.14	0.11

ble 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. s 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. Heterogeneous Effects by Teacher Characteristics

	Reading Z-Scores					Math Z-Scores				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Panel A: 2014										
(1) Monitor	0.12	0.43***	0.21**	0.15		0.10	0.26	0.14*	0.12	
	(0.11)	(0.16)	(0.09)	(0.12)		(0.09)	(0.16)	(0.08)	(0.11)	
(2) Monitor*teacher is female	0.37*					0.25				
	(0.21)					(0.17)				
(3) Monitor*teacher has secondary school		-0.30					-0.17			
		(0.19)					(0.18)			
(4) Monitor*teacher experience			0.02					0.03*		
			(0.01)					(0.01)		
(5) Monitor*teacher can find other work				0.14					0.11	
				(0.19)					(0.15)	
(6) Monitor*Local Teacher (<= 5 km from village)										
Number of observations	1,442	1,294	1,224			1,434	1,286	1,216		
R-squared	0.02	0.02	0.02			0.02	0.02	0.02		
Panel B: 2015										
(1) Monitor	0.14	0.15	0.12	0.05	-0.08	0.13	0.16	0.06	0.09	-0.07
	(0.10)	(0.11)	(0.10)	(0.10)	(0.14)	(0.09)	(0.11)	(0.09)	(0.10)	(0.13)
(2) Monitor*teacher is female	-0.02					-0.07				
	(0.23)					(0.20)				
(3) Monitor*teacher has secondary school		0.01					-0.08			
		(0.18)					(0.16)			

(4) Monitor*teacher experience			0.03**					0.03*		
			(0.01)					(0.02)		
(5) Monitor*teacher can find other work				0.04					0.01	
				(0.14)					(0.13)	
(6) Monitor*Local Teacher (<=5 km from village)					0.29*					0.22*
					(0.17)					(0.14)
Number of observations	1,228	1,215	1,205	1,215	1,228	1,219	1,206	1,196	1,206	1,219
R-squared	0.02	0.02	0.03	0.02	0.03	0.02	0.02	0.02	0.02	0.03

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 8. 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)	0.53	-0.03
	(0.50)	(0.06)
(2) Number of days stopped course	2.06	-1.30*
	(4.12)	(0.67)
Panel B: Teacher Performance		
Number of classes teachers taught (attendance lists)	19.25	-0.85
	(2.12)	(0.92)
Teacher was replaced	0.24	-0.04
	(0.43)	(0.07)
Panel C: Teacher Motivation		
Teacher kept an attendance log	0.28	0.18**
	(0.45)	(0.08)
Intrinsic motivation z-score	0	0.24*
	(1.00)	(0.13)
Perceived competence z-score	0	-0.02
	(1.00)	(0.14)
Perceived pressure z-score	0	0.08
	(1.00)	(0.12)
Perceived choice z-score	0	0.14
	(1.00)	(0.12)
Number of observations		240

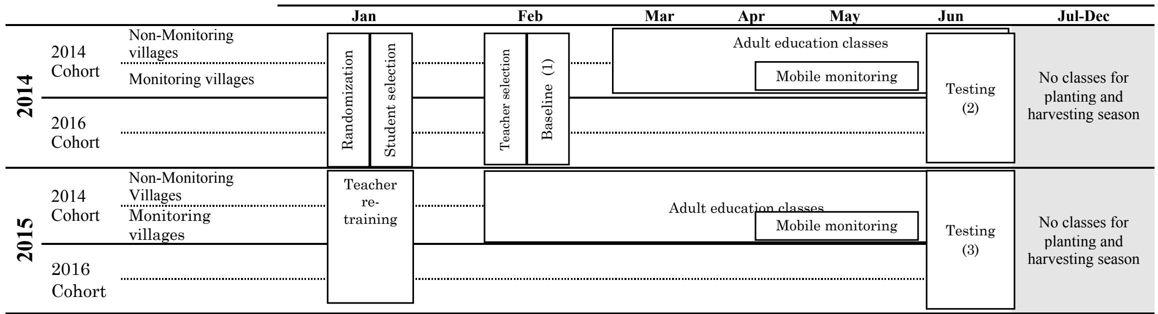
Notes: This table presents the results from a regression of teacher-level outcomes on a binary variable for monitoring, among the sample of adult education courses. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant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

Table 9. Student Effort

	Adult Education Village (Non-Monitor)	Adult Education Village (Non-Monitor)	Adult Education*Monitor
	Mean (s.d.)	Coeff (s.e.)	Coeff (s.e.)
Panel A: Student Drop-Out of Course			
Stopped course (Yes/No)	0.27		-0.02
	(0.44)		(0.02)
Stopped course for personal choice	0.11		-0.01
	(0.31)		(0.02)
Length of time in course	1.92		0.05
	(1.23)		(0.08)
Panel B: Self-Esteem and Self-Efficacy			
Self-esteem z-score		-0.12	0.03
		(0.09)	(0.06)
Self-efficacy z-score		-0.17**	0.08
		(0.08)	(0.07)
Panel C: Learning Outcomes of Called Students (Compared with All Monitoring Students)			
Reading z-score			0.58**
			(0.27)
Math z-score			0.24
			(0.17)
Number of observations			1,773

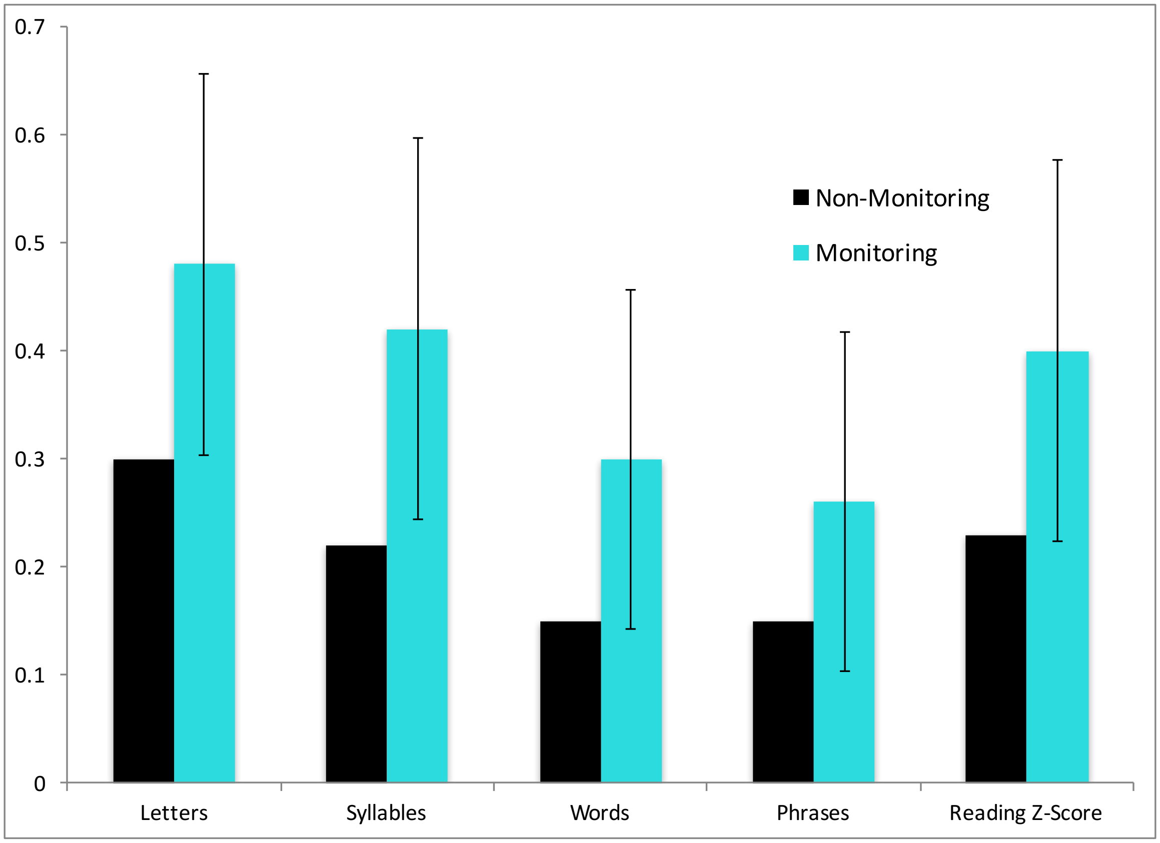
Notes: This table presents the results from a regression of student-level outcomes on a binary variable for monitoring, among the sample of adult education villages.. Huber-White standard errors clustered at the village level are provided in parentheses. *** significant at the 1 percent level, ** significant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

Figure 1. Timeline of Activities



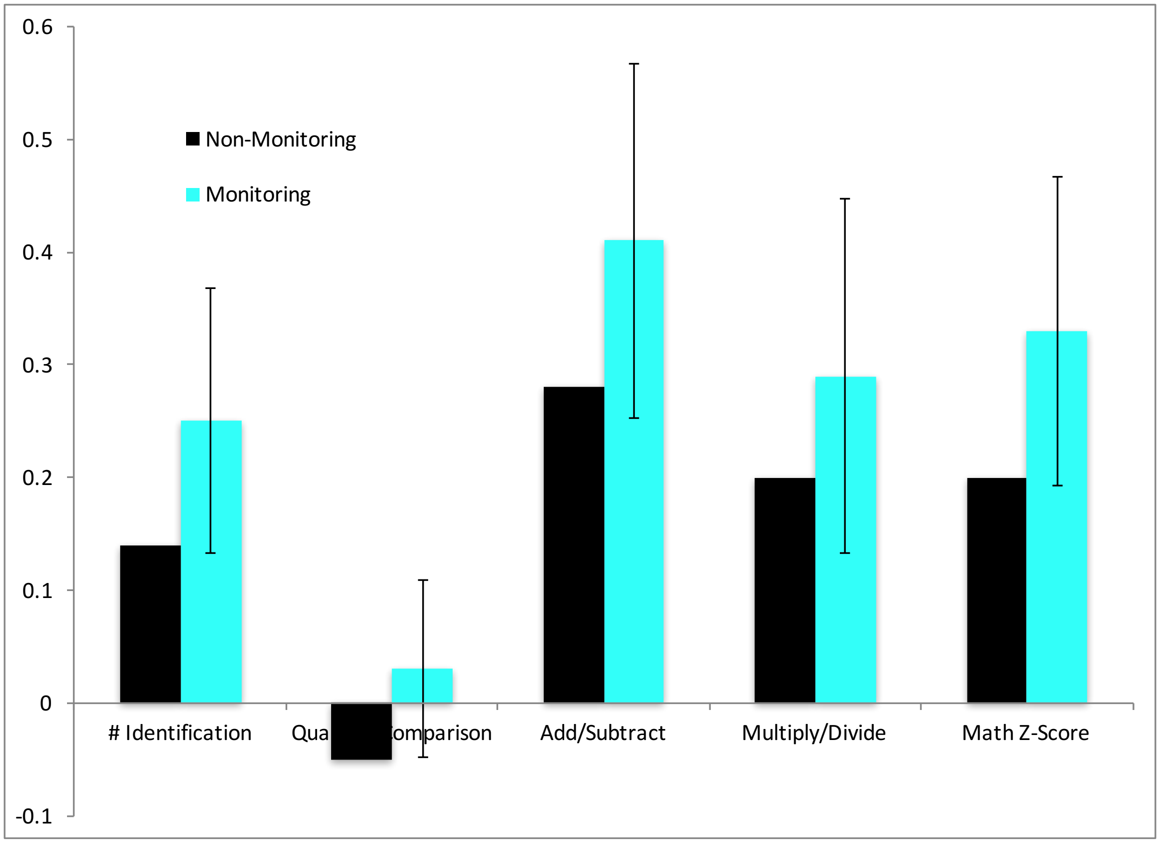
Note: Figure shows the timeline of activities for the different groups in our study. The 140 villages receiving adult education classes either did not receive extra monitoring attention (Non-monitoring villages) or received the mobile phone-based monitoring (Monitoring villages). The 2016 cohort is the 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 2A. Impact of the Monitoring Program over Both Years



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

Figure 2B. Impact of Monitoring on Math Z-Scores over Both Years



Notes: This figure shows the mean math z-scores of different math tasks for students in 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 clustered at the village level.

Figure 3. Cost effectiveness of the Mobile Monitoring Intervention