Can Mobile Phones Improve Learning? Evidence from a Field Experiment in Niger

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Abstract: The returns to educational investments hinge on whether such investments can improve the quality and persistence of educational gains. This has been a challenge in adult education programs, which are typically characterized by rapid skills depreciation. We report the results from a randomized evaluation of an adult education program (Project ABC) in Niger, in which adult students learned how to use simple mobile phones as part of a literacy and numeracy class. Overall, students demonstrated substantial improvements in writing and math skills. Students in ABC villages achieved additional literacy and numeracy gains, with test scores that were .19-.25 standard deviations higher than those in non-ABC villages. There are persistent impacts of the program: seven months after the end of classes, average math test scores are still higher in ABC villages. These effects are driven by the effectiveness of mobile phones as a motivational tool, primarily through their ability to increase the value of skills learned in class. These results suggest that simple and cheap information technology can be harnessed to improve educational outcomes among rural populations.

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JEL Codes: D1, I2, O1, O3

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1. Introduction

Despite decades of investment in education programs, nearly 18 percent of adults worldwide are unable to read and write in any language (UNESCO 2008).[1] Adult education programs have the potential to bridge this gap, but they are often characterized by low enrolment, high drop-out rates and rapid skills depreciation (H. Abadzi 1994, D. Ortega and F. Rodríguez 2008, J. Oxenham, A. Diallo, A. Katahoire, A. Petkova-Mwangi and O. Sall 2002, R. Romain and L. Armstrong 1987). The failure for adult literacy gains to persist may be due to the irrelevancy of such skills in daily life or limited opportunities to practice such skills in an individual's native language.

The widespread growth of mobile phone coverage in many developing countries offers an incentive to obtain, and facilitate the acquisition of, literacy and numeracy skills by illiterate adults. By teaching students how to use mobile phones, adult learners may be able to practice their literacy skills outside of class by sending and receiving short message services (SMS), making phone calls and using mobile money (m-money) applications, all of which require basic fluency with the numbers, symbols and letters on mobile phone keypads. Mobile phone technology could also affect returns to education by allowing households to use

[1]Literacy is defined as the skills of: 1) "recording information of some kind in some code understood by the person making the record and possibly by other persons in some more or less permanent form; and (2) decoding the information so recorded." Similarly, numeracy is defined as "the skill of using and recording numbers and numerical operations for a variety of purposes" (J. Oxenham et al. 2002). The data in the UNESCO report uses data from "around" 2000, which could be as early as 1995 and as recent as 2005 for particular countries.

the technology for other purposes, such as obtaining price and labor market information and facilitating informal private transfers.[2]

We report the results of a randomized adult education program in Niger, where a mobile phone-based component was added to an otherwise standard adult education program (Project *Alphabétisation de Base par Cellulaire,* or ABC). Implemented in 113 villages in two regions of Niger, all students followed the same basic adult education curriculum, but those in half of the villages also learned how to use a simple mobile phone.[3] Overall, our results provide evidence that the mobile phone technology substantially improved learning outcomes: Adults' writing and math test scores were .19-.25 standard deviations (s.d.) higher in ABC villages immediately after the program, with a statistically significant effect. There were no strong effects by region, gender or age. While these skills depreciated in both groups after the end of the program, the relative educational improvements in ABC villages seem to persist over time, particularly for math. These effects do not appear to be driven by differential attrition or differences in teacher quality, but are partially explained by increased student effort within and outside of the classroom.

Prior evidence on the impact of adult education programs is limited. Existing studies on the impact of such programs on educational outcomes often rely upon self-reported literacy or numeracy measures or do not have a

[2]The widespread penetration of mobile phones and the relatively low cost of Short Message Service (SMS) as compared to voice calls in many developing countries provide a powerful economic incentive to use SMS as the preferred communication platform.
[3]The experiment provided simple mobile phones – which primarily have voice and SMS capability– as opposed to smart or multimedia phones - which often have internet or video capability.

convincing identification strategy (G. Carron 1990, D. Ortega and F. Rodríguez 2008).[4] This paper overcomes these shortcomings by using a randomized experiment, combined with student-level test score and attendance data, as well as data on teacher quality and household socio-economic characteristics.

Our finding that information technology leads to an improvement in skills acquisition contributes to a debate on the effectiveness of computer-assisted learning in other contexts. While Linden (2008) and Osario and Linden (2009) find that computers have either no or mixed effects on learning outcomes, Banerjee, Cole, Duflo and Linden (2007) found that computers increased students' math scores and were equally effective for all students. They also found that these gains were short-lived, with only limited persistence over time. Barrow, Markman and Rouse (2009) find that students randomly assigned to a computer-assisted program obtained significantly higher math scores, primarily due to more individualized instruction. Yet our experiment is unique in that is used a relatively low-cost technology, did not require specialized instruction or software and focused on adult learners.

The remainder of the paper is organized as follows. Section II provides background on the setting of the research and the research design. Section III describes the different datasets and estimation strategy. Section IV discusses the results, whereas Section V addresses the potential mechanisms. Section VI

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[4] N. Blunch and C. Pörtner (2011) provide the only recent study to analyze the effects of literacy programs on welfare. Due to the non-experimental nature of their study, they rely on community fixed effects to deal with endogeneous program placement, and instrument for participation within the village using the time since adult literacy programs were available interacted with individual and household characteristics.

discusses alternative explanations, and Section VII provides a simple cost-effectiveness analysis. Section VIII concludes.

2. Research Setting and Design

Niger, a landlocked country located in West Africa, is one of the poorest countries in the world. With a per capita GNP of USD$ 230 and an estimated 85 percent of the population living on less than USD$2 per day, Niger is one of the lowest-ranked countries on the United Nations' Human Development Index (UNDP 2010). The country's education indicators are particularly striking: 71.3 percent of the population over the age of 15 was classified as illiterate in 2007 (INS and Macro International 2007). The problem of illiteracy is even more pronounced in our study regions, where close to 90 percent of adults are unable to recognize letters or numbers in any language.

2.1. Adult Education and Mobile Phone Interventions

Starting in February 2009, an international non-governmental organization, Catholic Relief Services, implemented an adult education program in two rural regions of Niger. The intervention provided eight months of literacy and numeracy instruction over a two-year period to approximately 6,700 adults across 134 villages. Courses were held between February and June of each year, with a break between June and January due to the agricultural planting and harvesting season.[5] All classes taught basic literacy and numeracy skills in the native language of the village (either Zarma or Hausa), as well as functional literacy topics.[6] Conforming to the norms of the Ministry of Non-Formal Education, each village had two literacy classes (separated by gender), with a maximum of twenty-five students per class. 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.

The additional intervention (ABC) was a variant of the basic adult education program. Participants in the ABC villages followed the same curriculum as those in non-ABC villages, but with two principal modifications: 1) they learned how to use a simple mobile phone, including turning on and off the phone, recognizing numbers and letters on the handset, making and receiving calls and writing and reading SMS; and 2) a mobile phone was provided to groups of literacy participants (one mobile phone per group of five people).[7] The mobile phone module of the program was introduced three months after the start of the adult education program (at the end of April, with classes starting in February), and neither students, teachers nor CRS field staff were informed of which villages were selected prior to the module. As one day per week was

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[5] Adult education courses in Niger cover a two-year period, for four months per year. Thus, each participant received a total of eight months of literacy and numeracy between 2009 and 2010 (2009 cohort) or 2010 and 2011 (2011 cohort).

[6] The primary local languages spoken in the program regions are Hausa, Zarma and Kanuri, although only Hausa and Zarma were the languages of instruction. Participants in predominately Kanuri villages were provided with the choice of instruction (Kanuri or Hausa), and all villages chose Hausa.

[7] While the shared mobile phones could potentially have a wealth effect, the effect would be 1/5th the price of the mobile phone, or USD$2.

allocated to reviewing previous material, teachers in ABC villages were instructed to teach the mobile phone module during this class. Thus, ABC students did not have additional class time and had less than six weeks of in-class practice with mobile phones (between the end of April and early June). Compared to the basic intervention, the ABC group allows us to disentangle the additional effect of having a mobile phone from the effect of the adult education program.

2.2. Experimental Design

Prior to the introduction of the program, CRS identified 140 intervention villages across two regions of Niger, Dosso and Zinder. Of these, some villages had an ongoing adult education program administered by a different organization or did not have mobile phone coverage, thereby reducing the sample size to 113 eligible villages.[8] Among these villages, we first stratified villages by regional and sub-regional administrative divisions. Due the inability of the NGO to implement the program everywhere during the first year, villages were then randomly assigned to a cohort (to start classes in 2009 or 2010), with half of the villages starting in 2009. Within each year cohort, villages were then assigned to either the basic (non-ABC) or the basic plus mobile phone intervention (ABC). In all, 58 villages were assigned to the ABC group and 55 to the non-ABC group.[9] A

[8]Of the 27 villages excluded from the randomization, 6 villages already had an ongoing adult education program and 21 villages did not have mobile phone coverage at the time of the village selection process. CRS implemented the adult education program in a total of 134 villages, 113 of which were included in our sample.

[9]When there was an even number of villages in a sub-region, villages were equally assigned to the ABC intervention. If there were an odd number of villages in a sub-region, a random draw was used to decide whether the number of ABC villages would be greater or less than the number of non-ABC villages.

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, eligible students were identified for both cohorts during the baseline. Individual-level eligibility was determined by three primary criteria: 1) membership in a formal or informal village-level producers' association; 2) illiteracy, as confirmed by an on-site diagnostic test; and 3) willingness to participate in the program. If there were more than fifty eligible applicants in a village, students were randomly chosen from among all eligible applicants in a public lottery.

To measure the impact of the adult education program, we could have exploited the randomized phase-in of the program to collect data from the 2010 cohort during the first year. While this was the original intention of the research design, unanticipated uncertainty regarding program funding prevented us from collecting a second round of pre-program data from the 2010 cohort in January 2010, before they started the program. In addition, using the village-level lottery to estimate the spillover effects on eligible non-participants (and bound treatment effects for the adult education program) was impossible due to funding constraints. Hence, while we can estimate the causal effect of the mobile phone module as compared to the standard adult education intervention, we cannot estimate the causal impact of the adult education program.

3. Data and Estimation Strategy

The data we use in this paper come from three primary sources. First, we conducted several rounds of math and writing tests and use these scores to measure the impact of the program on educational outcomes. Second, we conducted detailed surveys about relevant student and household characteristics. Third, we collected information about the teachers in the program. Before presenting our estimation strategy, we discuss each of these data sources in detail.

3.1. Test Score Data

As students were identified for both cohorts in January 2009, writing and math tests were administered to all fifty students in each village prior to the start of courses, providing a baseline sample of over 5,600 students for the 2009 and 2010 cohorts. We administered follow-up tests with the 2009 cohort in June 2009 and with both cohorts in June 2010, thereby allowing us to estimate the immediate impacts of the program.[10] We also administered tests seven months after the end of classes in January 2010 and January 2011. The comparison of the June and January test results enables us to detect the persistence of initial gains potentially due to the ABC program.

The writing and math tests were developed in collaboration with the Ministry of Non-Formal Education and were identical in structure and difficulty for both languages (Hausa and Zarma) and all survey rounds. For writing, each

[10]We originally intended to administer tests to the 2009 and 2010 cohorts during each round of data collection to exploit the randomized phase-in of the program. Administering tests with the 2010 cohort in June 2009 or January 2010 (before they had started classes) proved to be unfeasible, and so data for the 2010 cohort are only available in January 2009, June 2010 and January 2011.

student was asked to participate in a dictation exercise, and the Ministry of Non-Formal Education staff then assigned scores from Level 0 ("beginner") to Level 6. Level 0 corresponds to being "completely illiterate" (not being able to recognize or write any letters of the alphabet correctly), whereas Level 1 implies that the student can correctly write letters and syllables of the local language alphabet. Level 6 implies that the student can correctly write two complete sentences with more complex word patterns. The levels are similar for the numeracy test, ranging from Level 0 (complete "innumeracy") to Level 1 (simple number recognition) and a maximum of Level 6 (math word problems involving addition, subtraction, multiplication and division).[11]

While attrition is typically a concern in adult education classes, we did not observe differential drop-out or absenteeism between ABC and non-ABC villages. First, all villages were provided with an enrolment incentive, whereby students who attended at least 80 percent of classes each month received a food aid ration. Second, drop-out typically occurred within the first month of classes. As the ABC module began three months after classes began and teachers and students were not informed of the ABC program in advance, it is unlikely that drop-out was correlated with the ABC program. Similarly, once a student missed several weeks' of classes, the teacher would not allow him or her to re-enter the class, as

[11]The different levels of the writing and math tests can be roughly compared to primary school grades in Niger. For math scores, Level 2 corresponds roughly to first grade, Level 3 to second grade and Levels 4 and 5 to third grade. The comparison with writing test scores is more difficult, as the language of instruction in primary schools in Niger is French or Arabic. Nevertheless, writing scores of 2 and 3 would roughly correspond to first grade, whereas scores of 4 and 5 would roughly correspond to second grade. In the actual dataset, a seventh level is included, ".5", signifying that a student could identify some letters and numbers correctly.

they had fallen behind in the curriculum. For this reason, students who dropped out of the course before the ABC module was introduced could not re-enter the program later or rejoin the class the following year. Nevertheless, as tests were administered after the end of classes, students could have been absent the day of the test, either due to seasonal-migration or agricultural activities.

Table A1 formally tests whether there is differential dropout or absenteeism at different periods in the program. Average dropout during the last two months of classes (after the introduction of the ABC module) was 5 percent, with no statistically significant difference between the ABC and non-ABC villages (Panel A). This suggests that the ABC program did not prevent student drop-out. Average absenteeism the day of the test immediately after the program was 18 percent, with a slightly higher rate of absenteeism in ABC villages. However, there is no statistically significant difference between the two (Panel B). Absentees were slightly younger and more likely to be female in ABC villages. The former would likely bias our treatment effect downwards, whereas the latter would bias the treatment effect upwards. Absenteeism during the January test rounds was higher, with 30 percent of students absent on the day of the test (Panel C). This is unsurprising, as the tests were unannounced and occurred before classes had begun for the year. Nevertheless, there was no statistically significant difference in absenteeism between ABC and non-ABC villages, or in the demographic composition of absentees.

3.2. Student and Teacher Data

The second primary dataset includes information on student and household characteristics. We conducted a household survey with 1,038 adult education students across 100 villages, who were randomly chosen from among all selected male and female students in that village. A baseline household survey was conducted in January 2009, with a follow-up survey in January 2010. Each survey collected detailed information on household demographics, assets, production and sales activities, access to price information, migration and mobile phone ownership and usage. We also obtained data on each student's attendance record, which was collected by the teachers. While the attendance incentive could have encouraged teachers to inflate attendance records (Shastri and Linden 2009), we would not expect this to be different across ABC and non-ABC villages.

The third dataset is comprised of teacher-level characteristics for each class and each year, in particular the highest level of education obtained, age, gender and village residence.

3.3. Pre-Program Balance of ABC and Non-ABC Villages

Table 1 suggests that the randomization was successful in creating comparable groups along observable dimensions. Differences in pre-program household characteristics are small and insignificant (Table 1). Average household size was eight, and a majority of respondents were members of the Hausa ethnic group. Less than 8 percent of respondents had any form of education (including coranic school), and only 27 percent of children between the ages of 7 and 15 had some primary schooling. Thirty percent of households in the sample owned a mobile phone, with 55 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, with less than 4 percent writing and receiving SMS. A higher percentage of respondents reporting *receiving* calls (as compared with making calls), as calling in Niger is quite expensive (equivalent to USD$.35 per minute, whereas receiving a call is free).[12] Furthermore, making a phone call requires being able to recognize numbers on the handset and therefore some number recognition.

Panel B presents a comparison of means of teacher characteristics across both years of the program. Overall teacher characteristics are well-balanced between ABC and non-ABC villages. Teachers were 32 years old and attended school for 8.5 years, equivalent to secondary school in Niger. Roughly one-third of the teachers were female, implying that men were teaching women's classes. More than two-thirds of teachers were from the same village. As the Ministry of Non-Formal Education and CRS were able to choose new teachers after the first year of the program, they could have selected better-quality teachers for ABC villages in the second year, which could undermine our identification strategy. A comparison of teacher characteristics by year suggests that this was not the case (Table A2).

Table 2 provides further evidence of the comparability of the ABC and non-ABC villages for writing and math z-scores. Test scores are normalized using the contemporaneous non-ABC test score mean and standard deviation for

[12]Households primarily received calls from migrants residing in other others of Niger or West and North Africa.

that round in that region.[13] Overall, non-normalized baseline writing and math scores were close to zero for both ABC and non-ABC villages, suggesting that the project selected participants who were illiterate and innumerate prior to the start of the program. The average normalized test scores for both writing and math were slightly higher in non-ABC villages, although we cannot reject the equality of means.

3.4. Estimation Strategy

To estimate the impact of mobile phones on educational outcomes, we use a difference-in-differences specification. Let $test_{icvt}$ be the normalized writing or math test score attained by student i in class c in village v during round t. ABC_v is an indicator variable for whether the village v is assigned to the adult education plus mobile phone intervention (ABC=1) or simply the basic adult education program (ABC=0). $post_t$ takes on the value of one in the June post-treatment tests (June 2009 or 2010) and zero for the baseline, $cohort_v$ is a binary variable equal to one if the village started in the 2010 cohort, 0 otherwise. θ_R are geographic fixed effects at the regional and sub-regional levels (the level of randomization). X'_{iv} is a vector of student-level baseline covariates, primarily gender, although we include age in some specifications. We estimate the following specification:

(1) $$test_{icvt} = \alpha + \beta_1 ABC_v + \beta_2 post_t + \beta_3 ABC_v * post_t + X'_{iv}\gamma + \delta cohort_v + \theta_R + \varepsilon_{ivt}$$

[13]The results are robust to using alternative methods of normalization, namely the baseline non-ABC test score.

where ABC_v*post_t is the interaction between being assigned to the ABC treatment and a post indicator variable (the June test score rounds). The coefficient of interest is β_3, which captures the average immediate impact of the mobile phone education program as compared with the basic adult education program, and is estimated by pooling across cohorts and years.[14] 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 (1) is our preferred specification for two reasons. First, the DD specification will control for potential pre-program differences in means between ABC and non-ABC villages. Second, the DD specification enables us to control for village-level fixed effects. As an alternative to this preferred approach, we also estimate the results using simple difference and value-added specifications, as well as testing whether the effects of the program differ across years.

4. Results

Figure 3 depicts the mean raw (non-normalized) test scores for ABC and non-ABC villages for both cohorts before, immediately after and seven months after the end of classes. Overall, writing and math scores were higher in both the ABC and non-ABC villages immediately after the program. Relative to the January 2009 baseline test scores, students reached a first-grade level in writing and a second-grade level in math. This suggests that adult education students

[14]The primary estimating equation pools test score data from the June 2009 and June 2010 rounds for the 2009 cohort, and the June 2010 test score data for the 2010 cohort. The results for equation (1) are robust to including only the immediate results for both cohorts (i.e., June 2009 for the 2009 cohort and June 2010 for the 2010 cohort).

moved from a "beginner" level (no letter or number recognition) to being able to correctly write letters, syllables and solve simple math problems, although the absence of a pure comparison group does not allow us to identify this as a causal effect.

The ABC program helped students to achieve additional gains: Average test scores in ABC villages were 17 percent higher for writing and 8 percent higher for math, respectively. Yet despite these strong initial gains, both groups experienced depreciation in writing and math skills after the end of classes. Test scores in ABC villages were still 8-13 percent higher after the end of the program, suggesting that the immediate additional gains due to the ABC program persisted.

4.1. Immediate Impact of the ABC Program

Table 3 pools the data across cohorts and rounds and presents the results of equation (1). Using the simplest specification, the ABC program increased students' writing test scores by .19 s.d., with a statistically significant effect at the 5 percent level (Panel A, Column 1). This effect is robust to the inclusion of region, gender and cohort fixed effects (Panel A, Column 2), sub-regional fixed effects to account for the randomization process (Panel A, Column 3) and village-level fixed effects (Panel A, Column 4). Overall the results suggest that the ABC program increased students' writing scores by .19-.20 s.d.

The results are stronger in magnitude and statistical significance for math: the ABC program increased math z-scores by .25 standard deviations

(Panel B, Column 1). These results are robust to the use of region, gender and cohort fixed effects (Panel B, Column 2), sub-regional fixed effects (Panel B, Column 3) and village-level fixed effects (Panel B, Column 4).

The results in Table 3 are also robust to using a simple difference and value-added specification controlling for baseline test scores (Table A3).[15] Compared to the DD estimation, the simple difference and value-added specifications suggest that ABC program increased writing z-scores by .13-.16 s.d. (Panel A, Columns 1 and 3) and math z-scores by .13-.19 s.d. While the magnitude of the effect is lower as compared with the DD estimation results, this is unsurprising, as math and writing z-scores were slightly higher in non-ABC villages prior to the program.

4.2. Heterogeneous Effects of the ABC Program

We would expect greater learning benefits among subpopulations for whom complementarities between education and technology are stronger, such as those who are more engaged in entrepreneurial activities, migration and relatively younger populations. Table 4 tests for heterogeneous impacts of the ABC program by the student's residence, gender and age.

[15]The DD specification imposes the restriction that the coefficient on the baseline test score in the value-added specification is equal to one. Andrabi et al (2011) show that value-added specifications are not appropriate in situations where baseline skills depreciate rapidly and where students start off with very different baseline skills. This is not the case with the baseline test scores in our context, as almost all students were illiterate and innumerate prior to the start of the program. As a result, remaining skills are likely to be very persistent over the period of time measured by our tests. While value-added specifications often lead to more precise estimates, this is not the case in our context.

The Dosso region is relatively closer to the capital city (Niamey) and Nigeria, with a stronger density of agricultural markets and higher percentage of households engaged in agricultural trade (57 percent of households in Dosso, as compared with 38 percent in Zinder). The ABC program could therefore be more useful in the Dosso region, as students might have a stronger incentive to use the mobile phone to obtain price information, especially via cheaper SMS. Columns 1 and 3 report the results of a triple difference-in-differences (DDD) regression that tests for differential effects of the ABC program by region. The triple interaction term is not statistically significant for writing or math z-scores, suggesting that the ABC program did not have a differential impact by region.

In light of different socio-cultural norms governing women and men's household responsibilities and social interactions, the ABC program could have had different impacts by gender. As women of particular ethnic groups (e.g., the Hausa) are permitted to travel outside of their home village less frequently than men, the mobile phone could have served as a substitute for face-to-face communications, thereby strengthening the incentive to use the technology. Conversely, if the intensity of mobile phone usage increases with the size of an individual's social networks outside of the village, then we would expect a stronger impact of the ABC program for men. Columns 2 and 4 report the results of the ABC program by gender. On average, women's writing and math z-scores were lower than men's immediately after the program. Yet the coefficient on the triple interaction term is not statistically significant, suggesting that the ABC program had similar impacts for women and men.

Finally, the ABC program might also have had a differential impact by age. Younger students might be better positioned to learn new material or a new technology, implying that ABC might have a stronger effect on younger students. Alternatively, older adults might have more established social networks, thereby creating a more powerful incentive for them to use mobile phones as a means of communication. Columns 3 and 6 report the results of the ABC program by age, with "young" defined as younger than 40 years of age.[16] While younger students had higher average writing and math test scores, the coefficient on the triple interaction term is not statistically significant. Thus, this suggests that the ABC program did not have a differential impact by age.[17]

While evidence of strong heterogeneous effects appears to be inconclusive in Table 4, moderate amounts of heterogeneity cannot be detected given the imprecision of the estimates.

4.3. Effects on the Distribution of Post- Test Scores

While previous regressions estimate the average effects of the program, we consider that the ABC program might affect a student's ability to reach certain levels more easily. The presence of a new technology in the classroom might allow weaker students to learn more quickly by allowing them to have access to an alternative educational aide. On the other hand, mobile phones might only be useful for students at the higher ends of the test score distribution,

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[16]The average student age was 37 years, with a standard deviation of 12 years.
[17]Table A4 shows the results by the first and second years of the program for the 2009 cohort, the only cohort for which data are available for both years. An F-test for equality of the coefficients on the first and second years fails to reject that the effects were the same across both years.

as manipulating the mobile phone requires at least some number and letter recognition.

Figures 4a and 4b provide suggestive evidence that the ABC program increased the probability of students achieving higher test scores. The graphs show the coefficient from logit regressions in which the dependent variable was having obtained a particular (raw) test score level in writing or math. For writing scores, the ABC program is associated with a higher proportion of students achieving the top levels, although the effect is not statistically significant for math at higher test score levels.[18]

4.4. Persistent Impacts of the ABC Program

Empirical evidence suggests that unused labor market or education skills are lost more easily when they cannot be used on a regular basis (A. De Grip and J. Van Loo 2002). For example, Banerjee et al (2007) find that computers allowed short-term gains to persist for school-aged students after the end of classes. While we find that the ABC program can reinforce immediate skills acquisition, we wish to test whether mobile phones can improve the persistence of educational gains.

To test for potential persistent impacts of the program, we use the baseline, immediate (June) and persistent (January) test scores across both cohorts and years in the following specification:

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[18]Other research has tested whether a particular program has had differential effects according to baseline test scores. Since over 95 percent of students in our sample had a raw writing and math test score of zero (complete illiteracy and innumeracy) prior to the program, this does not provide sufficient variation to test for differential impacts according to baseline test scores.

$$(2)\ test_{icvt} = \alpha + \beta_1 ABC_v + \beta_2 post_t + \beta_3 ABC_v{*}post_t + \beta_4 post\text{-}jan_t + \beta_5 ABC_v{*}post\text{-}jan_t + X'_{iv}\gamma + \delta cohort_v + \theta_R + \varepsilon_{ivt}$$

where the coefficients of interests are β_3 and β_5. The specification is analogous to equation (1), although it includes a binary variable for the January test score round ($post\text{-}jan_t$) and an interaction term between the ABC program and the January test score round ($ABC_v{*}\ post\text{-}jan_t$).

Table 5 presents the regression results for equation (2). As the tests conducted during the January rounds were administered seven months' after the end of classes and were not announced in advance, neither students nor teachers were able to prepare for the tests. Both writing and math scores fell after the end of classes, dropping by 13 percent for writing and 8 percent for math. Writing z-scores were .13 s.d. higher in ABC villages after the end of the program, but not statistically different at conventional levels. Yet math z-scores were .19 s.d. higher in ABC after the end of the program, with a statistically significant difference at the 5 percent level. The results are similar if we construct parametric bounds for the treatment effect to account for non-random attrition (Table A5).[19]

The results in Table 5 show whether the short-term effects of the ABC program persisted, but do not tell us whether there was differential depreciation between the two groups. We do not find a statistically significant difference

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[19]The results are similar if we exclude the January 2011 tests for the 2009 cohort, as they might have been aware that tests would be administered in January. Excluding these observations, the persistent impact of the ABC program is significant for both writing and math.

between the June and January test score rounds for either writing or math, suggesting that the ABC program did not affect the rate of skills depreciation during the class "break" (between June and January). This could potentially change over the longer-term, as students achieve higher skill levels and are able to increase mobile phone usage. Overall, the results in Table 5 present some evidence that the immediate effects of the ABC program persisted, although primarily for math.

5. Potential Mechanisms

There are a variety of mechanisms through which the ABC program could affect students' immediate and persistent learning. First, when used effectively, technology can potentially lead to increased teacher effort, thereby improving teaching efficacy and the effectiveness of the overall adult education curriculum. In this sense, mobile phones might provide a pedagogical platform for teaching adult education, similar to educational inputs such as textbooks, flipcharts and visual aids (Hanushek 2003, Glewwe et al. 2004, Glewwe, et al. 2009). Second, as technology and education skills are often complementary, the presence of mobile phones can increase students' effort and incentives to learn, reflected by increased class participation and attendance. Thus, having access to mobile phones can increase the private returns to education by facilitating communication with social networks. While such communication can occur by voice, SMS prices are substantially cheaper than voice prices in many countries in sub-Saharan Africa (including Niger), thereby providing a powerful financial incentive to learn to read and write.[20] Finally, the mobile phone can facilitate learning outside of the classroom, both during and after classes are in session. We discuss each of these mechanisms in turn.

5.1. Teacher Effort

The presence of mobile phones or a new curriculum could have increased teacher effort within or outside of the classroom, thereby improving students' performance. As we are unable to directly observe teacher effort, we provide an observable 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. We therefore use the number of classes taught as a proxy for teacher effort. Teachers taught an average of 54 classes during the first year of the program (Table 6, Panel A), without a statistically significant difference in the number of classes taught between ABC and non-ABC villages. The number of classes did not change over time, with similar number of courses taught each month over the program period and during the second year of the program (not shown). This suggests that teachers in ABC villages were not teaching more classes, thereby improving test scores. Note, however, that we are unable to rule out unobservable, qualitative changes in teacher motivation due to the introduction of the ABC mobile phone module.

5.2. Student Effort and Motivation

[20]Kim et al. (2010) find evidence that SMS and voice are (weak) substitutes.

The presence of the ABC program could have encouraged greater student effort within the classes, as measured by student attendance. On average, students attended 74 percent of classes during the first year of the program. The high attendance rate is unsurprising, as students were provided with a food ration based upon their monthly attendance record. While average attendance rates were higher in the ABC villages, we do not find a statistically significant impact of the program on overall attendance rates or after the introduction of the ABC module (Table 6, Panel B).[21]

While the results in Table 6 suggest that overall student effort did not increase in response to the ABC program, there could be differential effects by teacher quality. Mobile phones could have served as a complement for "higher-quality" teachers, who were better able to use the technology to motivate students. Alternatively, mobile phones could have functioned as a substitute for "lower quality" teachers. Table 7 presents the results of a regression of student attendance rates on a binary variable for the ABC program, a proxy variable for teacher quality and the interaction term between the two. On average, student attendance prior to the ABC module (during the first two months of the program) was lower in classes taught by more highly educated teachers, with no statistically significant effect (Column 1). After the introduction of the ABC module, student attendance remained lower in classes taught by more highly educated teachers, but was higher in ABC classes taught by such teachers

[21]The quality of the student attendance data in 2010 was poorer than in 2009, as Niger was hit by a devastating drought that affected the NGO's ability to closely monitor teachers' attendance records. However, there is no statistically significant difference in the availability of attendance data between ABC and non-ABC villages.

(Column 2). These results suggest that while students might have exerted less effort in classes taught by better-educated teachers, the ABC program increased effort in such classes.

Experimental measures of student effort provide additional evidence that students in ABC villages were more enthusiastic about learning, although primarily in classes taught by more educated teachers. In January 2011, students in all villages were invited to call a "hotline" to express their support for the adult education program.[22] Students were informed that the village with the highest number of calls would receive education "kits", comprised of chalk, small blackboards and notebooks. These materials are provided free by CRS and primary and secondary schools in Niger, and so have little market value and no alternative uses. Since students had to pay for the calls, we interpret the "hotline" participation as a reliable measure of students' interest in education.[23]

Table 8 presents the results of a regression of this hotline experiment. While the interpretation of the coefficient on the ABC variable simultaneously captures students' interest in the adult education *program* as well as the education *materials,* the results provide suggestive evidence of the impact of the ABC program on students' interest in education. Individuals in ABC villages were 12 percentage points more likely to call the hotline than their non-ABC

[22]Call-in-hotlines (or their predecessor, the "mail-in-comments"), have been used to measure the salience of topics, in particular in "education for social change" contexts (P.W. Vaughan et al 2000).

[23]Table A6 provides some insights into the characteristics of those who called the hotline. Hotline callers were primarily from the Zinder region (80 percent), male (83 percent) and from the 2009 cohort (57 percent). The average non-normalized math and writing test scores of student callers were 3.9 and 3.4, respectively, suggesting that callers could write simple sentences and do more complicated addition and subtraction. Only 25 percent of callers were students, suggesting that non-students also called the hotline. More non-students called the hotline in ABC villages.

counterparts (Column 1), although these results are not statistically significant at conventional levels. When we split the sample by the teacher's level of education, those living in ABC villages with more highly educated teachers were 21 percentage points more likely to call the hotline, with a statistically significant difference at the 10 percent level (Column 2). While those living in ABC villages with less-educated teachers were also more likely to call the hotline, the results are not statistically significant at conventional levels (Column 3). Yet it is difficult to detect an effect given the imprecision of the estimates.[24]

The results in Table 8 do not appear to be solely correlated with a higher density of mobile phones within ABC villages, as mobile phone ownership and access was relatively high prior to the program, and there was not a statistically significant difference in respondents' mobile phone ownership and access after the program (Table 9). In addition, hotline callers were required to pay the cost of the call, which was the same for those living in ABC and non-ABC villages. Finally, a significant percentage of callers were non-students, suggesting that the ABC program could have affected interest in education within the village. Taken together with the results in Table 7, these results provide suggestive evidence that the ABC program increased student effort and motivation, although primarily in classes taught by more highly educated teachers, suggesting that mobile phones and teacher quality are complementary.

[24] While hotline data are available for all of the villages where CRS implemented the program (134 villages), the results in Table 8 only include observations from the randomized sample. If all village observations are included, the results in Column 1 are strongly positive and statistically significant at the 5 percent level (not shown).

5.3. Mobile Phone Usage Outside of Class

The previous results suggest that one mechanism through which ABC affected learning was to increase students' interest and effort. Table 9 tests whether the program had an impact on student learning outside of the classroom by affecting mobile phone usage. The ABC program did not affect the respondents' private (non-group) mobile phone ownership or access to a mobile phone. The program also did not lead to more passive usage of mobile phones, such as receiving calls. However, students in ABC villages used mobile phones more frequently and used phones in more "active" ways, particularly by making calls, writing SMS, "beeping"[25] and sending airtime credit, all of which require more advanced letter and number recognition. While households in both ABC and non-ABC villages used mobile phones primarily for social communications (28 percent of households used mobile phones to communicate news of a shock), households in ABC villages were 4.6 percentage points more likely to use the mobile phone to communicate with commercial contacts and 4 percentage points more likely to search for price information. Overall, these results suggest that mobile phones enabled students to practice the skills acquired outside of class by using the mobile phone in more active (and less expensive) ways, especially for commercial transactions.

6. Alternative Explanations

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[25]Beeping (or "please call me") is a widespread phenomenon in Africa, whereby a person with little or no credit will dial another number and let the phone ring once or twice before hanging up. The interlocutor is expected to call back, bearing the costs of the call.

There are two potential threats to the interpretation of the above findings. First, there might be differences in observable and unobservable characteristics in teacher quality across ABC and non-ABC villages. If the Ministry of Non-Formal Education or CRS chose better-quality teachers for ABC villages or better-quality teachers self-selected into those villages, then any differences we observe in test scores might be due to differences in teachers' quality, rather than the presence of the ABC program. The means comparison of teacher characteristics between ABC and non-ABC for each year of the program suggests that differences in teacher quality are unlikely to explain the results.

A second potential confounding factor is different social interactions among students in ABC and non-ABC villages, or the "study group effect", as a result of the distribution of shared mobile phones. The mobile phone distribution could have encouraged students to form study groups outside of class, thereby facilitating learning and improving test scores. In this case, the improved test scores may be due to the study groups rather than learning on the mobile phones. While this effect would still be attributed to the ABC program, it would have different implications for replicating the program: one interpretation would suggest a "technology" effect, whereas the other would suggest a "study group" effect.

While we cannot test for this empirically, we provide qualitative evidence that such a "study group" effect is unlikely. Focus group discussions with the literacy teachers revealed that few students formed study groups or studied outside of class, given the relatively heavy workload of the adult education classes. Yet even among those students who formed study groups, there do not seem to have been systematic differences in the use of study groups across ABC and non-ABC villages. Therefore, it seems unlikely that assigning adult participants in ABC classes to groups of five can account for the improvements in test scores.

7. Cost Effectiveness Analysis

A natural question related to the use of a new approach is whether the expected benefits outweigh the additional costs. Annual government expenditure on education in Niger is among the lowest in the world; approximately 3 percent of the annual budget is spent on education (World Bank 2004). Thus, investing in mobile phone technology to improve adult education outcomes is one of many potential education interventions competing for scarce public resources. In this section, we explore whether a mobile phone-based adult education program should be a public policy priority for the poorest countries using a simple comparison of the benefits and costs of the ABC program.

A cost-benefit analysis of the ABC program would require estimates of the social and private returns to adult literacy (M. Kremer, E. Miguel and R. Thornton 2009). Instead, we conduct a *cost-effectiveness* analysis comparing the additional costs of the program with its educational impacts (Evans and Ghosh 2008). To measure the cost-effectiveness of the ABC program, we would ideally use a causal estimate of the impact of the adult education program on test scores and compare test scores and costs between ABC and non-ABC villages. In the absence of a pure comparison group for the basic adult education program, this is not possible, but we can still calculate whether the additional education gains due to the ABC program are worth the additional costs.

Over a two-year period, the per-student program cost was US$18.35 in non-ABC villages and US$21.30 in ABC villages. Thus, for an additional US$2.95 per student, students were able to increase their test scores by .19 s.d. for writing and .25 s.d. for math as compared to the standard adult education program.[26] More broadly, the relatively higher test scores in ABC villages suggest that the program could be cost beneficial for several reasons. First, average baseline test scores for adult students in Niger were close to zero before the program, suggesting that the corresponding s.d. increase resulted in a much higher percentage change in outcomes for ABC students as compared with other programs. Second, since the ABC program targeted adults, there could be more immediate private and social returns to education as compared to those of school-aged children.

8. 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, the relevance of literacy skills in an individual's daily life and dearth of easily accessible materials in indigenous languages. How to improve learning in these contexts is

[26] While this compares favourably to other interventions, such as those surveyed in Evans and Ghosh (2008), these effects are not directly comparable as they focus mostly on in-school interventions. Moreover, there is a trade-off between the more immediate returns on interventions targeted towards adults versus the shorter duration during which they accrue, due to the shorter remaining life expectancy.

not clear, and most studies on the impact of educational inputs in improving attendance and educational outcomes have primarily focused on school-aged children. The few studies that have assessed the impact of information technology have found mixed results.

This paper assesses the impact of an intervention that taught students how to use a simple information technology as part of an adult education class. We find that this substantially increased students' skills acquisition in Niger, suggesting that mobile telephones could be a simple and low-cost way to improve adult educational outcomes. The treatment effects are striking: the joint ABC and adult education program increased writing and math test scores by .19-.25 s.d. as compared with the standard adult education program. The impacts appear to operate through increasing student effort and motivation within the classroom, and enabling students to practice these skills outside of the classroom.

The ABC program relies upon simple mobile phones, rather than smart or multimedia phones, and does not require a specific program or software. These factors suggest that the program is easily scalable and replicable in other contexts. The effectiveness of the program in other contexts, however, will depend upon existing telecommunications infrastructure, the pricing structure of voice and SMS services and the availability of reading and writing materials in local languages. Nevertheless, given widespread mobile phone coverage and the introduction of mobile money services in many developing countries – which depend upon SMS or PIN codes – there are reasons to think that simple communication technologies can be effective learning tools in these contexts.

Programs to train adults in the use of mobile phones may bring important dynamic benefits as well. Such efforts may also increase adult students' motivation to continue to learn, just as the ABC program appears to have stimulated interest in learning. With the basic skills needed to use mobile phones and – perhaps – a greater curiosity and desire to learn, graduates of such programs may be able to tap into an array of services and information available by mobile phone. We are only able to assess the persistence of education gains over a one-to-two year period, but evidence from around the world increasingly suggests that mobile phones might be able to open new opportunities and build new skills. Over a longer horizon, mobile phone fluency among the poor may do much more than just increase educational gains.

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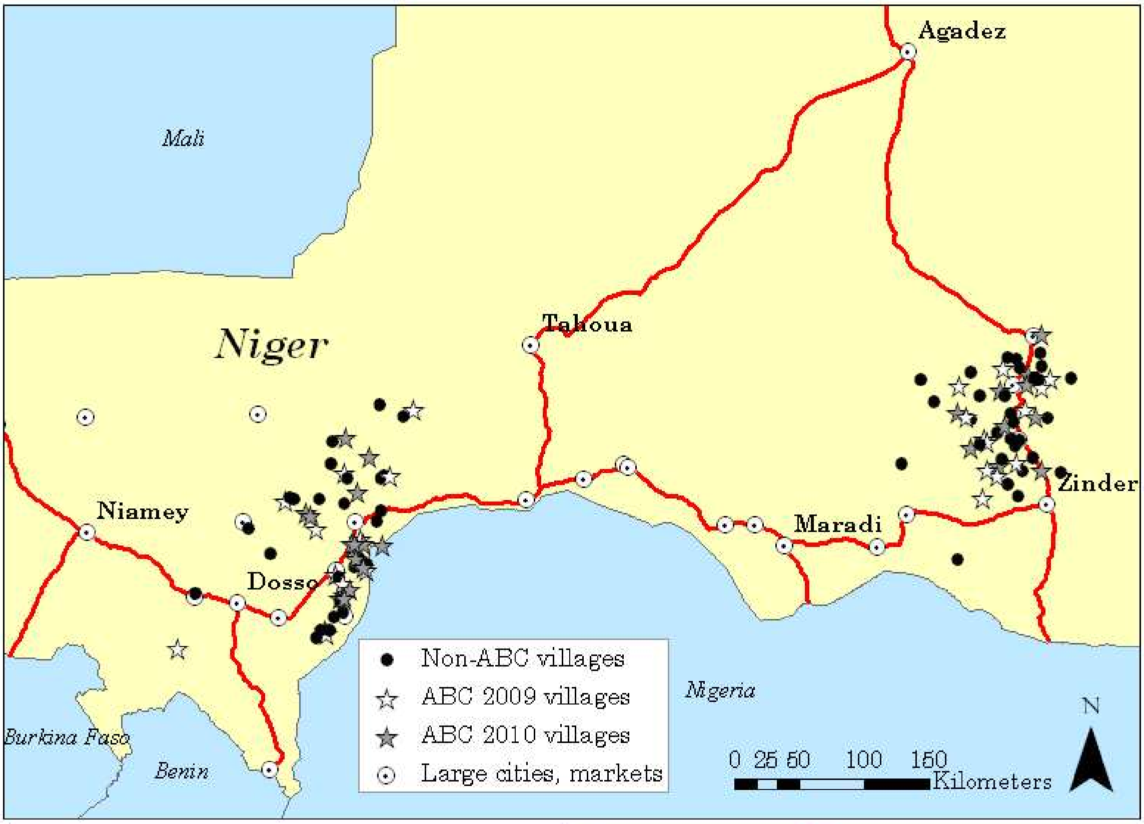
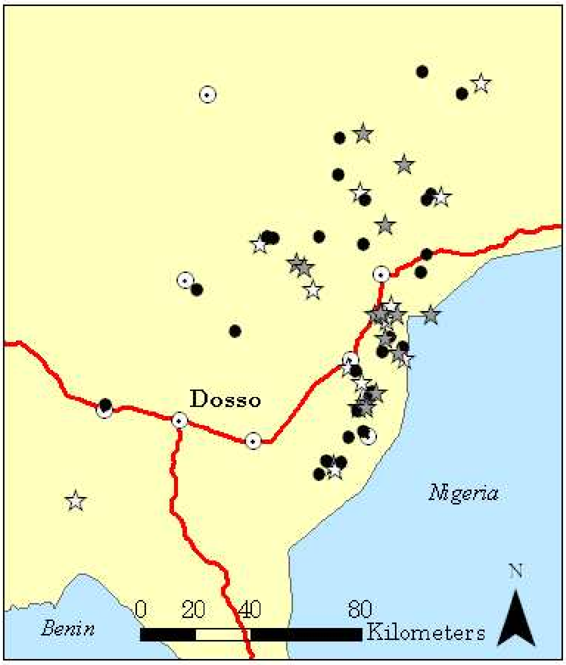
Figure 1. Map of Project Areas





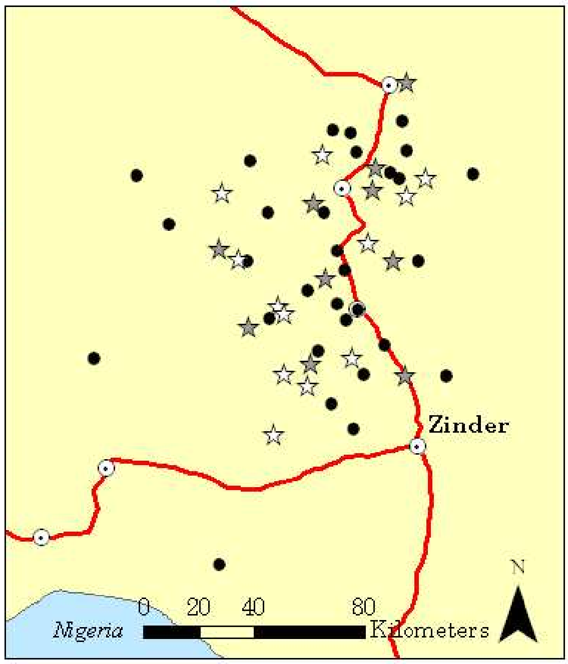
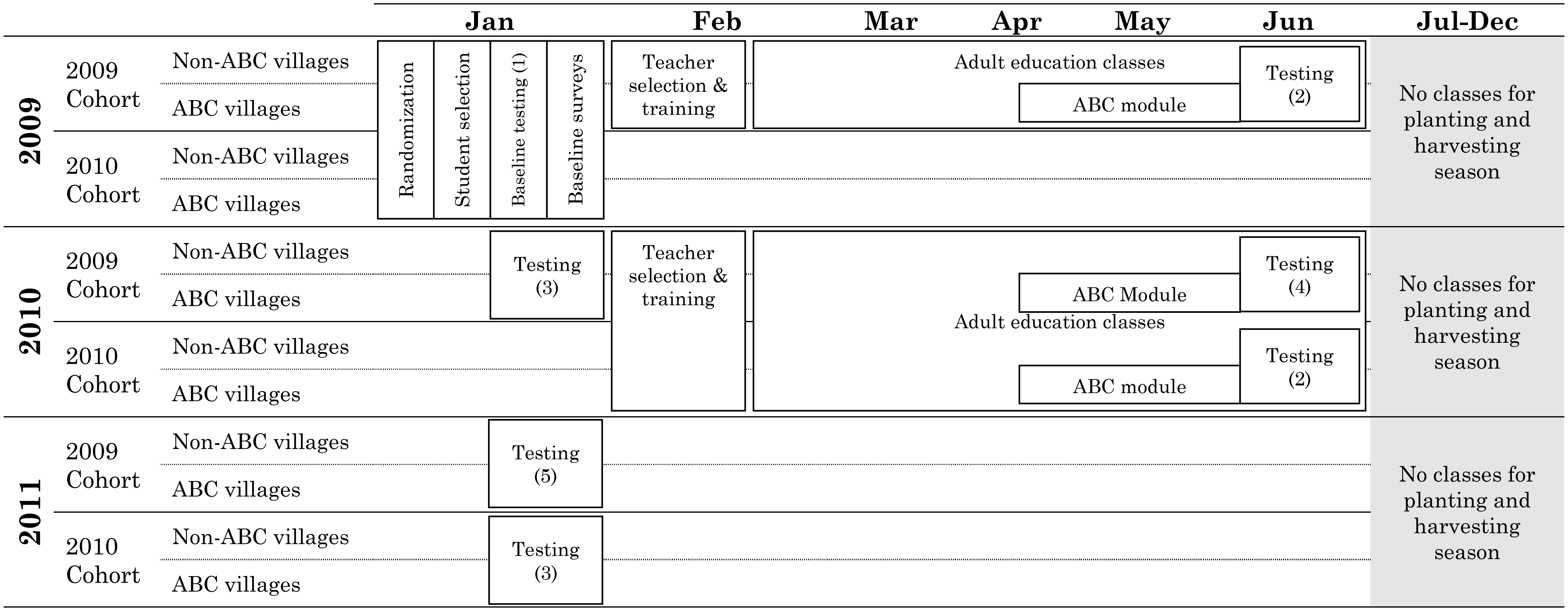


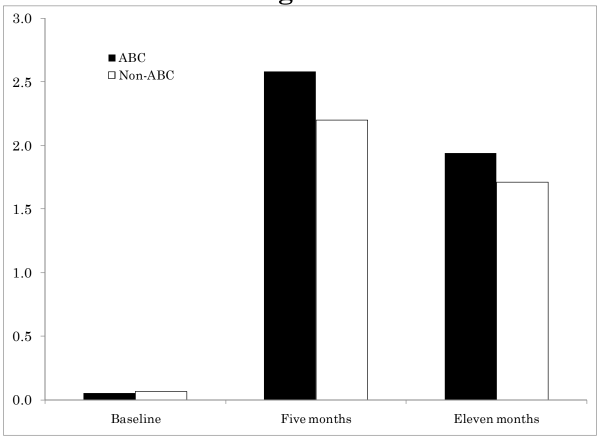
Figure 2. Timeline of Data Collection and Adult Education Activities



Notes: This figure represents the timeline for the adult education program, the ABC module and the data collection. "Testing" (1, 2, 3, 4, 5) etc. refers to the test round taken by the specific cohort.

Figure 3. Average (Non-Normalized) Test Scores for ABC and Non-ABC Villages

Panel A: Writing Test Scores



Panel B. Math Test Scores

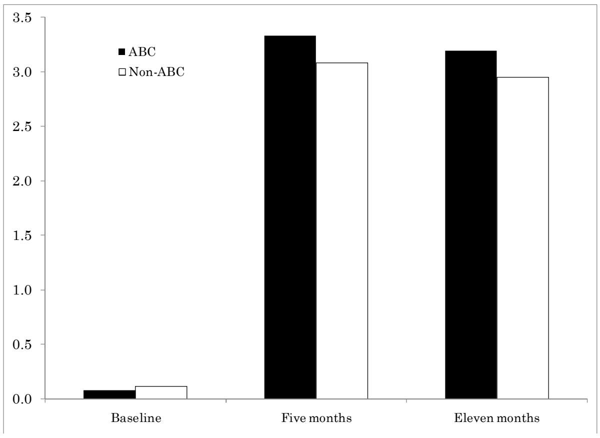
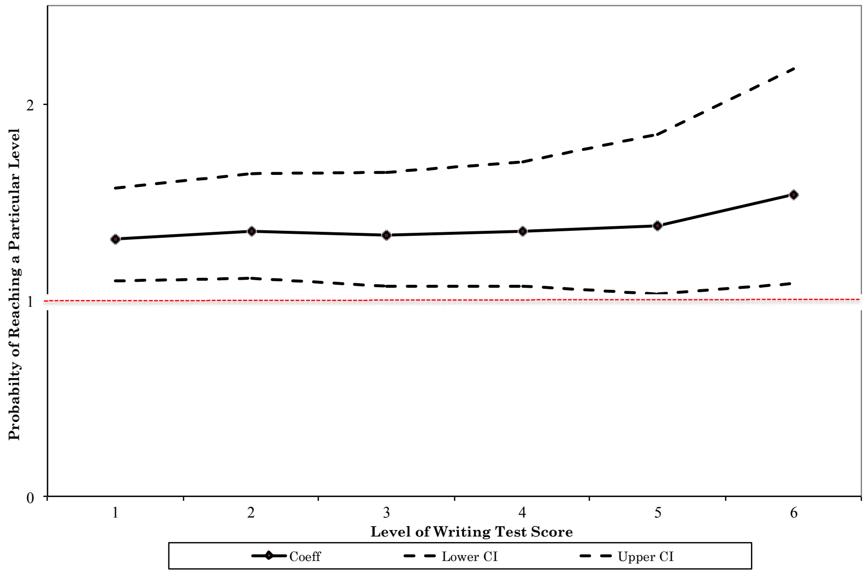


Figure 4. Impact of the ABC Program on Test Score Achievements

Panel A. Effect of ABC on the Probability of Achieving a Particular Level: Writing Non-Normalized Scores



Panel B. Effect of ABC on the Probability of Achieving a Particular Level: Math Non-Normalized Scores

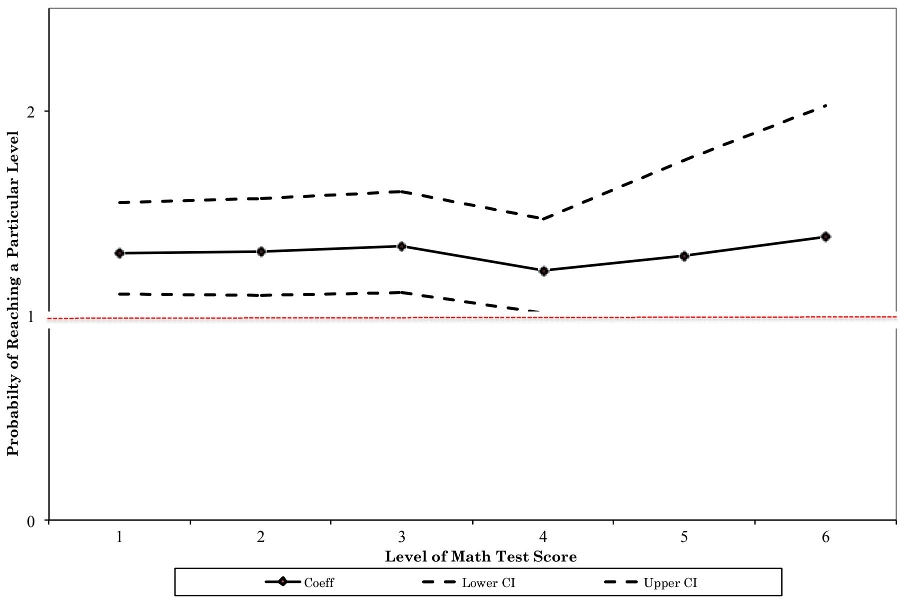


Table 1: Baseline Means Comparison

	ABC	Non-ABC	
	Mean (s.d.)	**Mean (s.d.)**	**Difference Coeff (s.e.)**
	(1)	(2)	(3)
Panel A: Student and Household-Level Characteristics			
Age of respondent	37.17	37.86	-0.69
	(11.75)	(13.03)	(1.25)
Respondent is household head (1=Yes, 0=No)	0.55	0.56	-0.01
	(0.50)	(0.50)	(0.02)
Respondent has attended some school (including coranic)	0.08	0.07	0.01
	(0.27)	(0.25)	(0.02)
Member of Hausa ethnic group	0.72	0.71	0.01
	(0.45)	(0.45)	(0.07)
Number of household members	8.42	8.32	-0.09
	(4.07)	(4.05)	(0.34)
Percentage of children (less than 15) with some education	0.27	0.28	-0.01
	(0.27)	(0.28)	(0.03)
Number of asset categories owned	4.98	5.00	-0.01
	(1.56)	(1.60)	(0.12)
Household experienced drought in the past year	0.61	0.64	-0.03
	(0.49)	(0.48)	(0.06)
Household owns mobile phone (1=Yes, 0=No)	0.29	0.30	-0.01
	(0.45)	(0.46)	(0.04)
Respondent has access to mobile (in HH or village)	0.79	0.76	0.04
	(0.40)	(0.43)	(0.04)
Respondent has used mobile phone since last harvest (1=Yes, 0=No)	0.57	0.54	0.03
	(0.50)	(0.50)	(0.05)
Respondent has used mobile phone to make calls	0.73	0.69	0.04
	(0.45)	(0.46)	(0.04)
Respondent has used mobile phone to receive calls	0.87	0.86	0.01
	(0.34)	(0.36)	(0.04)
Number of observations	519	519	1038
Panel B: Teacher-Level Characteristics			
Education (number of years)	8.57	8.32	.08
	(1.78)	(2.08)	(.216)
Age	32.71	33.06	-.308
	(8.07)	(9.16)	(1.185)
Gender (Female=1)	.368	.317	.06
	(.484)	(.467)	(.043)
Local (Teacher from village=1)	.682	.757	-.023
	(.467)	(.43)	(.051)
Number of observations	176	169	345

Notes: Column 1 presents the mean for ABC villages, Column 2 presents the mean for non-ABC villages. Column 3 reports the coefficient from a regression of the dependent variable on an indicator variable for ABC and sub-region fixed effects to account for randomization. Thus, Column (3) is not exactly equal to the difference between Columns (1) and (2). Huber-White standard errors clustered at the village level presented in parentheses. ***, **, * denote statistical significance at the 1, 5, 10 percent levels, respectively.

Table 2: Simple Difference in Mean Test Z-Scores between ABC and non-ABC Villages

	ABC	Non-ABC	
	Mean (s.d.)	**Mean (s.d.)**	**Difference Coeff (s.e.)**
	(1)	(2)	(3)
Panel A: Writing Z-scores			
Baseline Writing Test Z-score (both cohorts)	-0.03	0	-.023
	(.886)	(1)	(.04)
Number of observations	3046	2936	5982
Panel B: Math Z-scores			
Baseline Math Test Z-score (both cohorts)	-.07	0	-.059
	(.816)	(1)	(.047)
Number of observations	3046	2936	5982

Notes: Column 1 presents z-scores for ABC villages, Column 2 presents z-scores for non-ABC villages. Column 3 reports the coefficient from a regression of the dependent variable on an indicator variable for the ABC program and sub-region fixed effects to account for the level of randomization. Huber-White standard errors adjusted for clustering at the village level in parentheses. All test scores are normalized to the contemporaneous non-ABC distribution. ***, **, * denote statistically significance at 1, 5, 10 percent, respectively.

Table 3: Impact of the ABC Program on Average Test Scores: Difference in Differences

	(1)	(2)	(3)	(4)
Panel A: Writing Z-Scores				
ABC*Post	0.190**	0.199**	0.205**	0.198**
	(0.087)	(0.087)	(0.088)	(0.090)
ABC	-0.027	-0.032	-0.053	
	(0.048)	(0.049)	(0.048)	
Post	0.000	-0.013	-0.016	-0.013
	(0.059)	(0.061)	(0.060)	(0.060)
2009 Cohort		0.061	0.077	
		(0.054)	(0.047)	
Female		-0.425***	-0.423***	-0.423***
		(0.033)	(0.033)	(0.032)
Age		-0.010***	-0.010***	-0.010***
		(0.001)	(0.001)	(0.001)
Dosso		0.109**		
		(0.055)		
Sub-region fixed effects	No	No	Yes	No
Village fixed effects	No	No	No	Yes
Number of observations	13,402	12,823	12,823	12,823
R^2	0.006	0.060	0.085	0.130
Panel B: Math Z-Scores				
ABC*Post	0.246***	0.259***	0.261***	0.258***
	(0.090)	(0.093)	(0.092)	(0.094)
ABC	-0.071	-0.072	-0.097*	
	(0.051)	(0.051)	(0.055)	
Post	-0.000	-0.027	-0.030	-0.028
	(0.066)	(0.069)	(0.068)	(0.069)
2009 Cohort		0.144***	0.150***	
		(0.053)	(0.045)	
Female		-0.380***	-0.379***	-0.376***
		(0.033)	(0.033)	(0.033)
Age		-0.009***	-0.009***	-0.008***
		(0.001)	(0.001)	(0.001)
Dosso		0.121**		
		(0.053)		
Sub-region fixed effects	No	No	Yes	No
Village fixed effects	No	No	No	Yes
Number of obs	13,420	12,840	12,840	12,840
R^2	0.009	0.059	0.087	0.139

Notes: Each column represents a separate regression. Panel A presents results with writing z-scores as the dependent variable. Panel B present results with math z-scores as the dependent variable. "ABC" is an indicator variable for whether a village was assigned to the ABC program, 0 otherwise. "Post" is an indicator variable equal to 1 after the cohort participated in the adult education program (the June test score rounds for both cohorts), 0 otherwise. All test-scores are normalized to the contemporaneous non-ABC distribution. The sub-region is the level at which the ABC program was randomized. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively. Huber-White standard errors clustered at the village level are in parentheses.

Table 4: Heterogeneous Impacts of the ABC Program on Test Score

Dependent variable:	**Writing Z-Scores**			**Math Z-Scores**		
	(1)	(2)	(3)	(4)	(5)	(6)
ABC*Post	0.162	0.176*	0.159	0.156	0.259**	0.268**
	(0.151)	(0.099)	(0.104)	(0.135)	(0.106)	(0.106)
ABC*Post*Dosso	0.070			0.186		
	(0.183)			(0.183)		
Dosso*Post	0.056			0.056		
	(0.131)			(0.139)		
ABC*Dosso	0.052			0.010		
	(0.100)			(0.100)		
ABC*Post*Female		0.051			-0.001	
		(0.092)			(0.099)	
Female*Post		-0.495***			-0.238***	
		(0.064)			(0.067)	
ABC*Female		-0.034			0.065	
		(0.069)			(0.076)	
ABC*Post*Young			0.055			-0.033
			(0.108)			(0.112)
Young*Post			0.204**			0.268***
			(0.082)			(0.089)
ABC*Young			0.024			0.063
			(0.063)			(0.066)
Sub-region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	12,823	12,823	12,384	12,840	12,840	12,403
R^2	0.061	0.098	0.089	0.062	0.091	0.090

Notes: Each Column represents a separate regression. Columns 1-4 present results with writing z-scores as the dependent variable. Columns 5-8 present results for math z-scores. All test-scores are normalized based on the contemporaneous non-ABC distribution. The sub-region is the level at which the ABC program was randomized. All regressions include binary variables for ABC and post. Columns (1) and (5) include binary variables for Dosso, age and female; Columns (2) and (6) include binary variables for female and age; Columns (3) and (7) include binary variables for young, age and female; and Columns (4) and (8) include binary variables for whether the teacher had above average years of education, age and female. "Young" is defined as being younger than 40 years of age. Huber-White standard errors clustered at the village level are in parentheses. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

Table 5: Persistent Effects of the ABC Program

	Writing Z-Scores	Math Z-Scores
	(1)	(2)
ABC*Post (June round)	0.208**	0.261***
	(0.088)	(0.092)
ABC*Post (January round)	0.127	0.186**
	(0.078)	(0.075)
Post (June round)	-0.009	-0.016
	(0.060)	(0.068)
Post (January round)	0.004	-0.006
	(0.048)	(0.051)
ABC	-0.058	-0.102*
	(0.052)	(0.056)
Gender, Age, Cohort	Yes	Yes
Sub-region fixed effects	Yes	Yes
Number of observations	18,774	18,819
R^2	0.111	0.107

Notes: All test scores are normalized to the contemporaneous non-ABC distribution. Results include data collected 7 months after the end of classes for the 2009 and 2010 cohorts. "ABC" is an indicator variable for whether the village was assigned to the ABC program, 0 otherwise. "Post" is an indicator variable equal to 1 if after the cohort participated in the program. The sub-region is the level at which the ABC program was randomized. Huber-White standard errors clustered at the village level in parentheses. ***, **, * denote statistically significance at 1, 5, 10 percent, respectively.

Table 6: Impact of the ABC Program on Teacher and Student Attendance

	ABC	Non-ABC	
	Mean (s.d.)	**Mean (s.d.)**	**Difference Coeff (s.e.)**
	(1)	(2)	(3)
Panel A: Teacher Attendance (Number of Classes Taught)			
Year 1 Overall	53.47	57.08	-4.02
	(16.03)	(18.07)	(3.54)
Pre-ABC Module	36.23	39.01	-3.17
	(9.24)	(7.72)	(1.95)
Post-ABC Module	27.79	29.09	-1.41
	(9.49)	(9.50)	(1.08)
Number of observations	109	98	207
Panel B: Student Attendance Rate Year 1			
Overall	.761	.729	.01
	(.331)	(.346)	(.027)
Pre-ABC Module	.868	.846	.011
	(.194)	(.212)	(.026)
Post-ABC Module	.856	.82	.021
	(.214)	(.252)	(.02)
Number of observations	4380	4200	8580
Panel C: Student Attendance Rate Year 2			
Overall	.578	.591	-.001
	(.444)	(.448)	(.039)
Number of observations	1512	1562	3074

Notes: Table displays the mean for ABC (Column 1) and non-ABC (Column 2) for 2009 and 2010. "Year 1" is the first year of the program for the specific cohort (2009 for the 2009 cohort, 2010 for the 2010 cohort). Year 2 is the second year of the program for the 2009 cohort. Column 3 reports the estimated difference, controlling for sub-region fixed effects. Huber-White standard errors clustered at the village level are in parentheses. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

Table 7: Heterogeneous Impacts of the ABC Program on Student Attendance

	(1)	(2)
Dependent variable: Student Attendance Rate	**Pre-ABC Module**	**Post-ABC Module**
ABC	-3.44	-3.83
	(4.44)	(3.26)
ABC*Teacher education	2.46	7.37*
	(4.66)	(4.30)
Teacher education	-2.15	-7.81**
	(3.38)	(3.24)
Gender, Cohort	Yes	Yes
Sub-region fixed effects	Yes	Yes
Number of observations	3,555	3,947
R^2	0.22	0.14

Notes: The dependent variable is the proportion of classes that the student attended out of the total classes taught. "ABC" is an indicator variable for whether the village was assigned to the ABC program, 0 otherwise. "Teacher education" is a binary variable equal to 1 if the teacher had more than the mean years of education (8 years), 0 otherwise. Column (1) estimates the regression using data from prior to the introduction of the ABC module (i.e., months 1 and 2 of the adult education program). Column (2) estimates the regression using data from after the introduction of the ABC module, i.e., Months 3 and 4 of the adult education classes. The sub-region is the level at which the ABC program was randomized. Huber-White standard errors clustered at the village level in parentheses. ***, **, * denote statistically significance at 1, 5, 10 percent, respectively.

Table 8. Effect of ABC on Student Interest in Education

Dependent variable: *Person from village called hotline*

	(1)	(2)	(3)
	Overall	Above Average Level of Education	Below Average Level of Education
ABC	0.12	.211*	0.08
	(0.09)	(.121)	(0.14)
Sub-region fixed effects	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes
Number of observations	112	66	40
R^2	0.32	0.41	0.66
Mean (s.d.) of non-ABC group	0.52	0.52	0.52

Notes: Data based upon results from the call-in hotline in January-March 2011. Column 1 reports the results for the whole sample. Column 2 reports the results for the sample of villages where teachers had above-average years of education. Column 3 reports results for the sample of villages where teachers had below average years of education. The number of observations in Column 1 is greater than the sum of the observations in Columns 2 and 3 due to missing data on teachers' levels of education. Huber-White standard errors are in parentheses. *, **, *** denote statistically significant at 10, 5 and 1 percent levels, respectively.

Table 9. Mobile Phone Usage after the Program

	ABC	Non-ABC	
	Mean (s.d.)	Mean (s.d.)	Difference Coeff (s.e.)
	(1)	(2)	(3)
Panel A: Mobile Phone Ownership			
Respondent owns a mobile phone	0.40	0.40	-0.00
	(0.49)	(0.49)	(0.08)
Respondent has access to a mobile phone	0.85	0.83	0.02
	(0.35)	(0.36)	(0.04)
Used mobile phone since last harvest	0.72	0.66	0.06**
	(0.44)	(0.47)	(0.03)
Made calls	0.76	0.69	0.07*
	(0.40)	(0.45)	(0.04)
Received calls	0.91	0.90	0.01
	(0.29)	(0.30)	(0.03)
Wrote SMS	0.10	0.03	0.07***
	(0.35)	(0.17)	(0.02)
Received SMS	0.12	0.08	0.04
	(0.35)	(0.28)	(0.03)
Send or received a beep	0.38	0.29	0.09**
	(0.47)	(0.41)	(0.04)
Transferred airtime credit	0.06	0.03	0.03**
	(0.21)	(0.13)	(0.02)
Received credit	0.14	0.10	0.04
	(0.37)	(0.33)	(0.03)
Panel B: Uses of Mobile Phones for Communications			
Communication with migrant since last harvest	0.76	0.72	0.04
	(0.43)	(0.45)	(0.05)
Communicate with family/friends inside Niger	0.80	0.74	0.06
	(0.40)	(0.43)	(0.04)
Communicate with commercial contacts inside Niger	0.13	0.08	0.046**
	(0.33)	(0.28)	(0.02)
Used mobile phone to communicate death/ceremony	0.29	0.28	0.01
	(0.44)	(0.45)	(0.05)
Used mobile phone to ask for help/support	0.19	0.19	0.00
	(0.38)	(0.41)	(0.03)
Used mobile phone to ask for price information	.10	0.06	0.038*
	(0.30)	(0.26)	(0.02)
Number of observations	502	512	1014

Notes: Data based upon the household survey data collected in January 2009 and January 2010 including 1,038 observations. Column 1 presents the mean of the 2009 cohort in ABC villages, Column 2 is the mean of the 2009 cohort in non-ABC villages, and Column 3 is the unconditional difference in means. "Beeping" is using a ring without completing a call to signal another individual to call. The total number of observations is 1014, although the means for cell phone usage are conditional upon the respondent having used a mobile phone since the previous harvest (639 respondents out of 1014). Huber-White standard errors clustered at the village level are presented in parentheses. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

Appendices

Table A1: Attrition and Test Absenteeism

	ABC	Non-ABC	
	Mean (s.d.)	**Mean (s.d.)**	**Difference Coeff (s.e.)**
	(1)	(2)	(3)
Panel A: Drop-Out			
Pre-ABC Module	.042	.035	-.01
	(.2)	(.184)	(.015)
Post-ABC Module	.036	.06	-0.02
	(.186)	(.238)	(.02)
Panel B: June Test Rounds (Immediate)			
Absenteeism (absent day of test=1)	0.19	0.17	-0.018
	(.4)	(.394)	(.018)
Age of absentee	35.43	37.48	-1.32
	(11.26)	(12.088)	(0.92)
Gender of absentee (female=1)	0.49	.356	0.14***
	(.5)	(.479)	(0.03)
Panel C: January Test Rounds (Persistent)			
Absenteeism (absent day of test=1)	0.31	0.29	-0.018
	(.463)	(.454)	(.018)
Age of absentee	34.51	36.06	-0.94
	(11.67)	(12.61)	(0.94)
Gender of absentee (female=1)	0.43	.401	0.03
	(.496)	(.49)	(0.02)

Notes: Column 1 presents the mean for ABC villages, Column 2 presents the mean for non-ABC villages. Column 3 reports the coefficient from a regression of the dependent variable on an ABC indicator variable and sub-region fixed effects to account for randomization, and so does not exactly equal the difference between Columns (1) and (2). Huber-White standard errors clustered at the village level presented in parentheses. ***, **, * denote statistical significance at the 1, 5, 10 percent levels, respectively.

Table A2: Comparison of Teacher Characteristics by Year			
	ABC	Non-ABC	
	Mean (s.d.)	**Mean (s.d.)**	**Difference Coeff (s.e.)**
	(1)	(2)	(3)
Panel A: Teacher-Level Characteristics in 2009			
Education (number of years)	8.86	8.25	.263
	(1.315)	(2.286)	(.321)
Age	32.25	33.07	-.246
	(6.65)	(9.626)	(1.825)
Gender (Female=1)	.345	.254	.094
	(.479)	(.439)	(.083)
Local (Teacher from village=1)	.667	.763	-.061
	(.475)	(.429)	(.085)
Number of observations	60	59	119
Panel B: Teacher-Level Characteristics in 2010			
Education (number of years)	8.431	8.362	-.015
	(1.957)	(1.972)	(.241)
Age	32.94	33.048	-.251
	(8.697)	(8.929)	(1.23)
Gender (Female=1)	.379	.352	.026
	(.487)	(.48)	(.041)
Local (Teacher from village=1)	.69	.755	-.024
	(.465)	(.432)	(.049)
Number of observations	116	110	226

Notes: Column 1 presents the mean for ABC villages, Column 2 presents the mean for non-ABC villages. Column 3 reports the coefficient from a regression of the variable on an indicator variable for ABC, but does not include sub-region fixed effects to account for randomization due to a limited number of observations. Huber-White standard errors clustered at the village level presented in parentheses. ***, **, * denote statistical significance at the 1, 5, 10 percent levels, respectively.

Table A3: Impact of the ABC Program on Test Scores: Simple Difference and Value Added Specifications

Panel A: Writing Z-Scores	**Simple Difference**		**Value Added**	
	(1)	(2)	(3)	(4)
ABC	0.149*	0.132*	0.157*	0.142*
	(0.079)	(0.073)	(0.080)	(0.074)
Baseline Test Z-score			0.100***	0.087***
			(0.018)	(0.018)
2009 Cohort	-0.058	0.000	-0.073	-0.013
	(0.085)	(0.077)	(0.085)	(0.078)
Female	-0.649***	-0.653***	-0.638***	-0.644***
	(0.044)	(0.045)	(0.043)	(0.043)
Age	-0.015***	-0.016***	-0.015***	-0.016***
	(0.002)	(0.002)	(0.002)	(0.002)
Sub-region fixed effects	No	Yes	No	Yes
Number of observations	7,148	7,148	6,912	6,912
R^2	0.123	0.174	0.133	0.182
Panel B: Math Z-Scores				
ABC	0.172**	0.129*	0.185**	0.144**
	(0.086)	(0.069)	(0.085)	(0.068)
Baseline Test Z-score			0.076***	0.063***
			(0.020)	(0.016)
2009 Cohort	0.041	0.081	0.025	0.071
	(0.084)	(0.069)	(0.084)	(0.069)
Female	-0.501***	-0.506***	-0.490***	-0.500***
	(0.044)	(0.044)	(0.044)	(0.045)
Age	-0.013***	-0.015***	-0.013***	-0.015***
	(0.002)	(0.002)	(0.002)	(0.002)
Sub-region fixed effects	No	Yes	No	Yes
Number of observations	7,165	7,165	6,928	6,928
R^2	0.085	0.156	0.092	0.161

Notes: Each column represents a separate regression. Panel A presents results with writing test scores as the dependent variable. Panel B present results for math. All test-scores are normalized based on the contemporaneous non-ABC distribution. The sub-region is the level at which the ABC program was randomized. Huber-White standard errors cluster at the village level presented in parentheses. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

Table A4. Effects of the ABC Program by Year

	(1)	(2)
Panel A: Writing Z-Scores		
ABC*Post (1 year treatment)	0.222**	0.232**
	(0.102)	(0.101)
ABC*Post (2 year treatment)	0.147	0.139
	(0.111)	(0.110)
Post (1 year treatment)	-0.001	-0.005
	(0.070)	(0.070)
Post (2 year treatment)	-0.009	-0.047
	(0.074)	(0.079)
ABC	-0.051	-0.051
	(0.047)	(0.048)
Gender, Age, Cohort	No	Yes
Sub-region fixed effects	Yes	Yes
Number of observations	13,402	12,823
R^2	0.033	0.086
Panel B: Math Z-Scores		
ABC*Post (1 year treatment)	0.228**	0.244**
	(0.105)	(0.108)
ABC*Post (2 year treatment)	0.297**	0.293**
	(0.134)	(0.133)
Post (1 year treatment)	-0.002	-0.008
	(0.0790)	(0.0805)
Post (2 year treatment)	-0.010	-0.078
	(0.088)	(0.093)
ABC	-0.095*	-0.096*
	(0.0548)	(0.0545)
Gender, Age, Cohort	No	Yes
Sub-region fixed effects	Yes	Yes
Number of observations	13,420	12,840
R^2	0.039	0.088

Notes: Each column represents a separate regression. Panel A presents results with writing z-scores as the dependent variable. Panel B present results with math z-scores as the dependent variable. All test-scores are normalized to the contemporaneous non-ABC distribution. The sub-region is the level at which the ABC program was randomized. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively. Huber-White standard errors clustered at the village level are in parentheses.

Table A5: Persistent Effects of the ABC Program: Bounding

Panel A: Writing Z-Scores	**Lower Bound**	**Upper Bound**
	(1)	(2)
ABC*Post (January round)	0.069	0.200**
	(0.078)	(0.077)
Gender, Age, Cohort	Yes	Yes
Sub-region fixed effects	Yes	Yes
Number of observations	18615	18626
R^2	0.11	0.122
Panel B: Math Z-Scores		
ABC*Post (January round)	0.129*	0.272***
	(0.073)	(0.073)
Gender, Age, Cohort	Yes	Yes
Sub-region fixed effects	Yes	Yes
Inverse Mills' Ratio	Yes	Yes
Number of observations	18660	18660
R^2	0.104	0.108

Notes: All test scores are normalized to the contemporaneous non-ABC distribution. Results include data collected 7 months after the end of classes for the 2009 and 2010 cohorts. The upper bound is constructed by dropping the highest test scores from the January round in the non-ABC villages. The lower bound is constructed by dropping lowest test scores from the January round in the ABC villages. All regressions include controls for the ABC program, the June test score round ("post"), the January test score round ("January post") and the interaction between the ABC program and the June test score round. The sub-region is the level at which the ABC program was randomized. Huber-White standard errors clustered at the village level in parentheses. ***, **, * denote statistically significance at 1, 5, 10 percent, respectively.

Table A6. Characteristics of Hotline Participants

	Mean (s.d.)	Min	Max
Region (0=Zinder, 1=Dosso)	.187(.39)	0	1
Gender (0=Male, 1=Female)	.167(.37)	0	1
Cohort (0=2010, 1=2009)	.575(.49)	0	1
Writing Test Score	3.88(2.10)	0	6
Math Test Score	3.40(1.37)	0.5	6

Notes: Regressions include data from the call-in hotline between January and March 2011.