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 .20-.26 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 both as an educational input and as a motivational tool. Mobile phones increase educational outcomes most for students with the worst teachers, thus providing evidence of their usefulness as an educational tool. We also provide evidence that mobile phones increase motivation because they increase the value that students attach to the skills learned in class. These results suggest that simple and cheap information technology can be harnessed to improve educational outcomes among rural populations.

Keywords: Education; education quality; educational inputs; adult literacy; information technology; program evaluation; Niger
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 opportunity to providevincentives 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

[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.

allowing households to use 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 .20-.26 standard deviations (s.d.) higher in ABC villages immediately after the program, and were statistically significant at the 5% level. 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 persist over time. These effects do not appear to be driven by differential attrition or differences in teacher quality. Rather, they are partially explained by increased student interest in education and effort, including more active use of mobile phones outside of the classroom, as well as improved learning in classroom with less-well educated teachers.

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[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.

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 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.

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[4] N. Blunch and C. Pörtner (forthcoming) 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.

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, after which Section V addresses the potential mechanisms. Section VI 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 by the Ministry of Non-Formal Education in the adult education methodology.

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

[5] Adult education courses in Niger cover a two-year period, for 4 months per year. Thus, each participant received eight months of training over a two-year period, either during 2009 and 2010 or 2010 and 2011.
[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.

which villages were selected for the project. As one day per week was allocated to reviewing previous material, teachers in ABC villages were instructed to teach the mobile phone module during the revision class. Thus, ABC students did not have additional class time and had less than six weeks of in-class practice with mobile phones (between end April and early June). Compared to the basic intervention, the ABC group will allow 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 region 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]

[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 adult education 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.

A map of the project areas is provided in Figure 1, and a timeline of the implementation and data collection activities is provided in Figure 2.

Within each village, 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 were to start 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 impact of the program on educational outcomes. Second, we conducted detailed surveys to collect information 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 7. Level 0 corresponds to being "completely illiterate" (not being able to recognize or write any letters of the alphabet correctly), whereas Level 2 implies that the student can correctly write letters and syllables of the local language alphabet. Level 7 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 2 (simple number recognition) and a maximum of Level 7 (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 is 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 they had fallen behind in the curriculum. For this reason, students who dropped

[11]The different levels of the writing and math tests can be roughly compared to primary school grades in Niger. For math scores, Level 3 corresponds roughly to first grade, Level 4 to second grade and Levels 5 and 6 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 3 and 4 would roughly correspond to first grade, whereas scores of 5 and 6 would roughly correspond to second grade.

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 for test scores immediately after the program was 19 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 29 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 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

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[12] Households primarily received calls from migrants residing in other others of Niger or Africa.
[13]The results are robust to using alternative methods of normalization, namely the baseline non-ABC test score.

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. However, as the difference in baseline math test scores has a p-value of .20, this suggests that a simple comparison of means for math scores might underestimate our results.

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:

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

where $ABC_v{*}post_t$ is the interaction between being assigned to the ABC treatment and 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.[16]

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

[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.

[16]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.

students 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 (Column 3) and village level fixed effects (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 round fixed effects (Panel B, Column 2), sub-region 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).[17] 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-.18 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 by student characteristics

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 region, gender and age.

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

[17] In many cases, value-added specifications lead to more precise estimates. This is not the case here, perhaps because there was very little variation in baseline literacy.

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 mobile phone. 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 upon 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 after the first year of 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.[18] 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. [19]

Overall, the ABC program did not seem to have heterogeneous impacts by student characteristics such as region, gender or age.[20]

4.3. Heterogeneous Effects of the ABC Program by teacher characteristics

However, the program might have also had different impacts depending on the quality of teaching that already occurred in class. Table 5 presents evidence that learning in terms of literacy increased predominantly in classrooms with less-qualified teachers. Teachers are categorized into two groups, teachers with above median levels of education, corresponding to 9 years of education, and those with median education and below.[22] Column 1 documents that the program itself has a large and statistically significant impact on writing

[18]The average student age was 37 years, with a standard deviation of 12 years.

[19]The ABC program could have had different effects during the first and second years of the program. Unreported results, which are available upon request, however, show that this is not the case. When we allow for different coefficients by year of attendance, we fail to reject that that the effects were the same across both years.

[20] It is worthwhile to note that - given the imprecision of the estimates in Table 4 - we have low power to detect small and moderate amounts of heterogeneity.

[22] Results are qualitatively similar, if we take the mean level of education as the cutoff (8 years of education), or alternative cutoffs (from 6 years of education).

Z-scores for the students in class-rooms with below median educated teachers. The ABC program increases test-scores by 0.25 standard deviations. For the better half of teachers, the impact is about half as large and we cannot reject that the impact is zero. For math, the mobile phones had an impact across all groups of teachers. The impact is positive, large and significant for the class-rooms with worse teachers, increasing test scores by 0.27 standard deviations, and just slightly lower at 0.24 for the better teachers. Since the program did not increase attendance for students in classrooms with worse teachers (see below) or of teachers this suggests that, in terms of literacy, mobile phones are an education tool that can compensate for poor teaching.

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.

Table 6 estimates a specification similar to equation (1), using baseline, immediate (June) and persistent (January) z-scores across both cohorts and years in the following specification:

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

where the coefficients of interests are β_3 and β_5. $ABC_v{*}\, post\text{-}june_t$ is the interaction between being assigned to the ABC treatment and a post indicator variable for the June test scores (which corresponds to Table 3). $ABC_v{*}\, post\text{-}jan_t$ is the interaction between being assigned to the ABC treatment and a post indicator variable for the January test score round.

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. 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.[23][24]

The results in Table 6 present some evidence that the effects of the ABC program persisted, mainly for math.

[23]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.

[24]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. Because throughout the paper, we use Z scores standardized to the contemporaneous control group, we cannot directly test for differential depreciation. Using the raw scores, we do find no statistically significant difference for either writing or math test scores. This suggests that the ABC program does 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.

5. Potential Mechanisms

There are a variety of mechanisms through which the ABC program could affect students' immediate and persistent learning. First technology can potentially lead to increased teacher effort, thereby improving teaching efficacy and the effectiveness of the overall adult education curriculum. Second, we had already seen in Table 5 that it can increase the effectiveness of the teacher in teaching. Mobile phones might provide a pedagogical platform for teaching adult education, similar to educational inputs such as textbooks, flip charts and visual aids (Hanushek 2003, Glewwe et al. 2004, Glewwe, et al. 2009). Third, 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.[25] 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.

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

5.1. Teacher Motivation

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 program period (Table 7, Panel A), without a statistical 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 over the four-month period. This suggests that teachers in ABC villages were not teaching more classes, thereby improving test scores, and this is true for both better and worse teachers. 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 Motivation for learning

We provide evidence that mobile phones increased student motivation. In January 2011, students in all villages were invited to call a "hotline" to express their support for adult education classes.[27] Students were informed that the

[27]Call-in-hotlines (or their predecessor, the "mail-in-comments"), have been previously used to measure the salience of topics, in particular in "education for social change" contexts. An example of this was a mixed-method evaluation of a radio soap opera "Twende na wakati" ("Let's go with the times") focused on HIV and

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 and motivation for education. Table 7B 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 23 percentage points more likely to call the hotline than their non-ABC counterparts (Column 1). In addition, individuals in ABC villages called the hotline more frequently, calling an average of six more times per village (Column 2). These results 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 the ABC program did not appear to affect respondents' mobile phone access and frequency of usage after the program (Table 8).

We provide some insights into the characteristics of those who called the hotline (Table A5). Those who called the hotline were primarily from the Zinder region (80 percent) and male (83 percent). The 2009 cohort made up a larger

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AIDS behavioral change in Tanzania (P.W. Vaughan et al. 2000). In the political economics literature, Vicente, Aker and Collier (2011) used a call-in-hotline in the context of an election-monitoring campaign.

proportion of calls (57 percent). The average math and writing test scores of student callers was 3.9 and 3.4, respectively, suggesting that callers could write simple sentences and do more complicated addition and subtraction. Yet only 25 percent of all callers were students, suggesting that non-students also called the hotline. More non-students called the hotline in ABC villages, suggesting that the ABC program had spillover effects on community interest in education.

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 in education. Table 8 tests whether the program had an additional impact on student learning outside of the classroom by affecting mobile phone usage. The ABC program did not affect household's private (non-group) mobile phone ownership, access to a mobile phone or their frequency of usage since the past harvest. The program also did not lead to more passive usage of mobile phones, such as making or receiving calls. However, students in ABC villages used mobile phones in more "active" ways, particularly by writing and receiving SMS, "beeping"[28] 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), there was no statistically significant difference in the reasons for using a mobile phone. Overall, these results suggest

[28]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.

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.

5.4. Changing the value of skills learned in class

The presence of the ABC program could have encouraged greater student effort within the classes, as measured by student attendance. On average, students attended 80-85 percent of classes. The high attendance rate is unsurprising, as students were provided with a food ration based upon their monthly attendance record. While average attendance rates somewhat higher in the ABC villages, there no statistically significant impact of the program – on average – on overall attendance rates or after the introduction of the ABC module (Table 7, Panel B).[30] In particular, for the students with worse teachers, Table 9 presents evidence that there is no evidence of an increase in attendance, if anything there was a small decrease in attendance. Yet these are the students for whom test scores increased substantially. This provides evidence that for the students in class-rooms with worse teachers, that mobile phones were able to (partially) compensate for poor teaching.

However, mobile phones did increase attendance in class rooms with better teachers. Table 9 shows that there was a significant increase in attendance in classrooms with better teachers. After the introduction of mobile

[30]The quality of the student attendance data in 2010 was poorer than in 2009, as Niger was hit by a devastating drought that affected CRS' 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.

phones, attendance of students in ABC classes with better teachers increase by 7.8 percentage points relative to non-ABC classes with better teachers (Column 1), which is significant at the 5% level of significance.[31] This provides evidence that the quality of teaching and mobile phones are complements, directly or indirectly, in motivating students. Mobile phones are direct complements to the quality of teaching within the classroom (as in Linden, 2008), when better teachers use the mobile phones better in their teaching or there is a feedback loop in that the skills practiced on the phone help in understanding lessons. However, the results in Table 5 which show that the impacts are larger for worse teachers provide evidence that mobile phones are not direct complements to the quality of teaching within the classroom.

Alternatively, the observed complementarity between teaching quality and effort can be due to skills taught in the classroom being complementary to mobile phones in producing output (Aker and Ksoll, 2011). Put differently, mobile phones motivate students in class-rooms with better teachers more, but the size of the impact on test scores is smaller than in classrooms with worse teachers, due to decreasing returns to effort in acquiring skills. This suggests that students in the classrooms with better teachers view mobile phones as complements for the skills taught in class.[32]

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[31] Column 2 contains a placebo test: we study impacts of ABC and teacher quality on attendance in the two months *before* students were aware of the mobile phone project.

[32] Two alternative interpretations are variants of this argument, namely that good teachers are better able to explain the usefulness of mobile phones to their students. This would still suggest that the skills that students now perceive as more useful are driving them to exert more effort. Moreover, good teachers might also be teaching other mobile related skills during class time.

6. Alternative Explanations

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

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Again, this would suggest that these new skills that teachers are teaching are complimentary to mobile phones, though this would not necessarily show up in literacy and numeracy test scores.

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 literacy classes. Yet even among those students who formed study groups, based on the limited number of focus 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 an average of .21 and .26 s.d. for writing and math, respectively, as compared to the standard adult education program. [33]

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

[33] 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).

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 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 studies 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 .20-.26 s.d as compared with the standard adult education program. The impacts operate both through substituting for poor quality of teaching in the classroom and increasing student motivation.

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 worked through the perception of increased value of the skills learned in class. 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

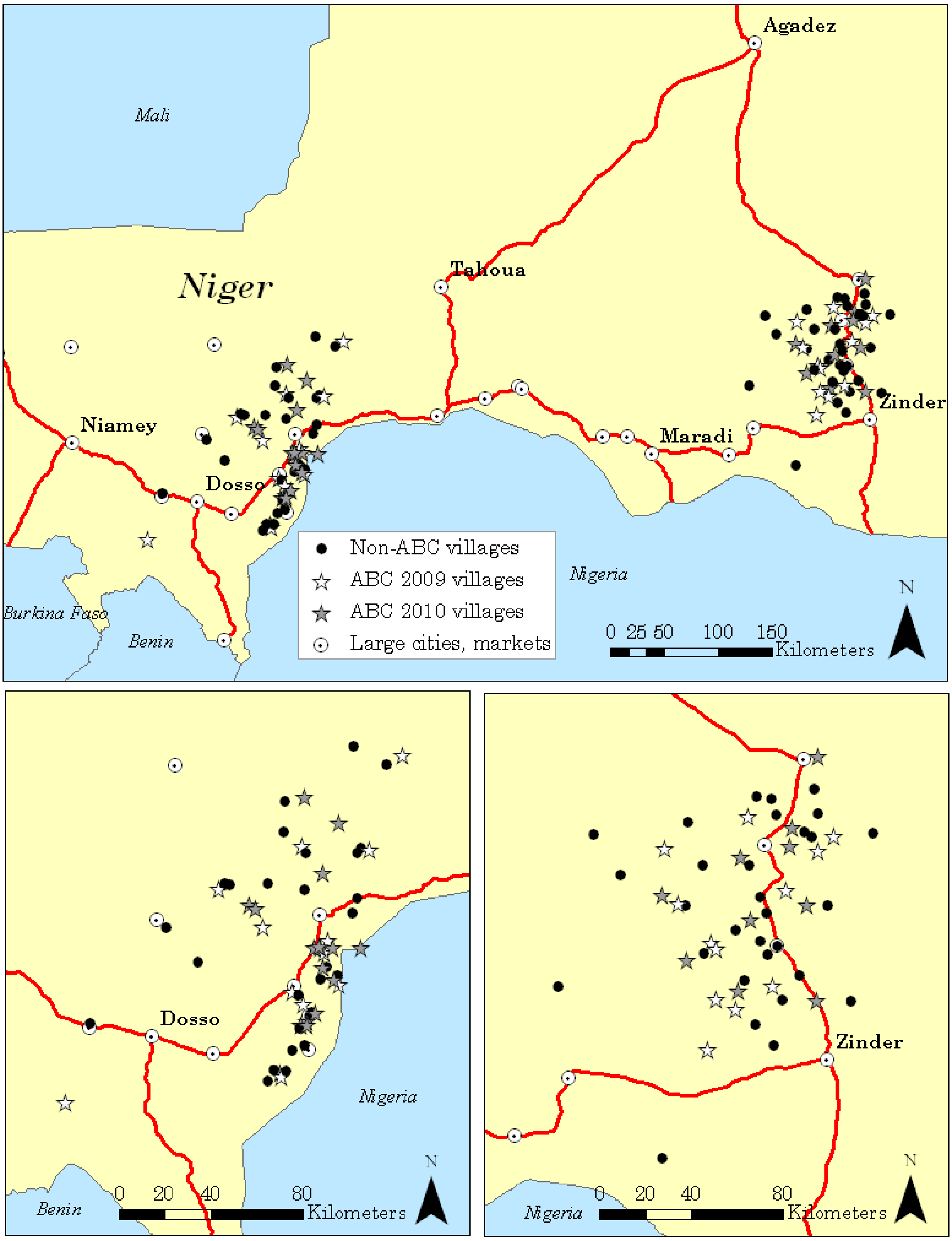
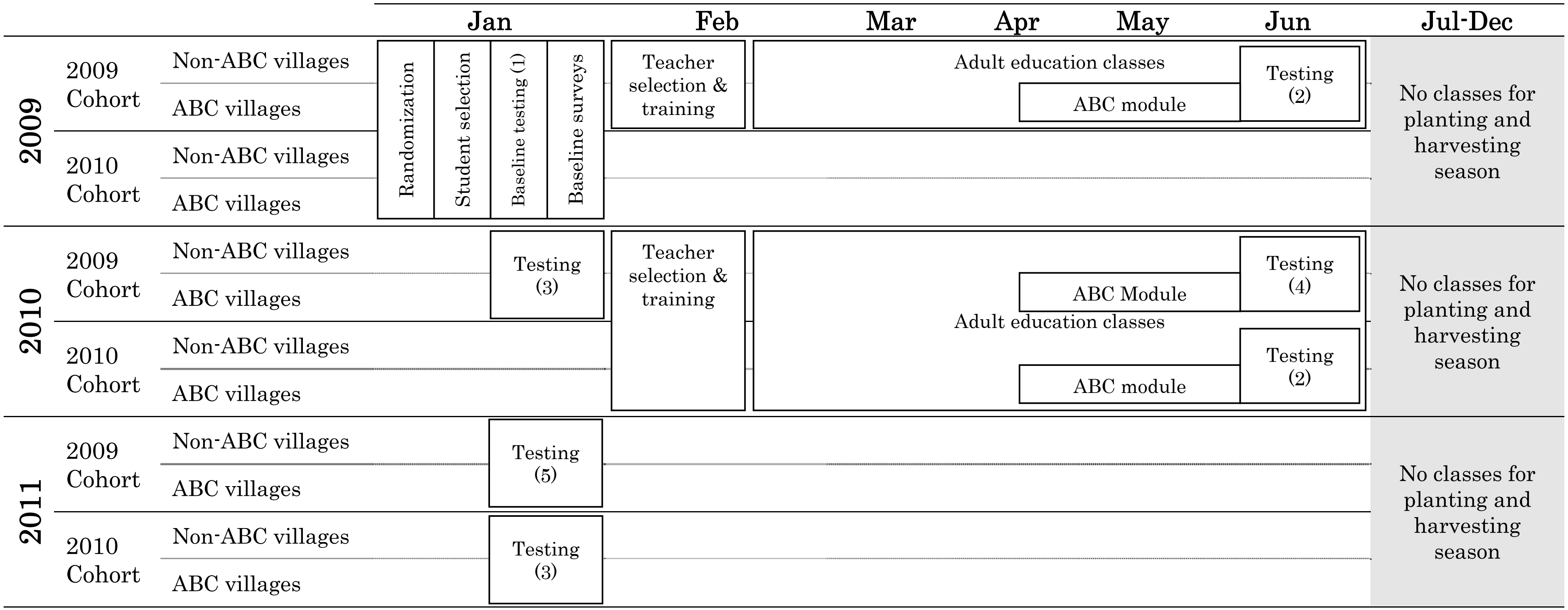


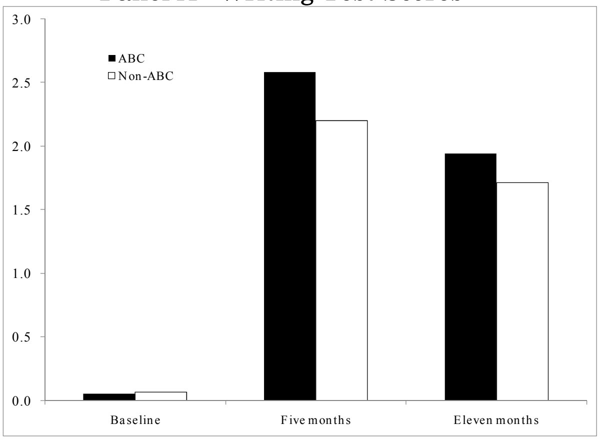
Figure 2. Timeline of Data Collection and Adult Education Activities



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

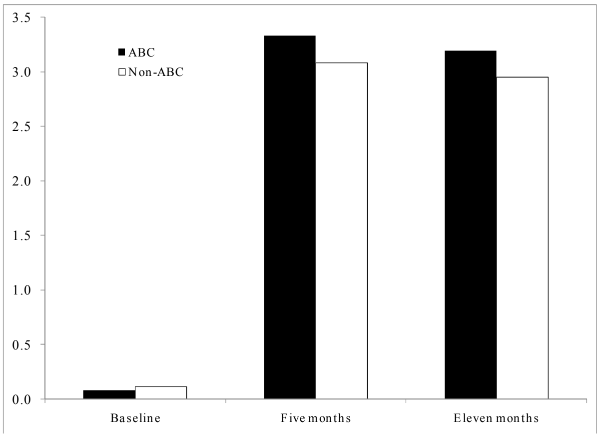


Table 1: Baseline Means Comparison

	ABC Mean (s.d.)	Non-ABC Mean (s.d.)	Difference Coefficient (s.e.)
Panel A: Student and Household-Level Characteristics (N=)			
Age of respondent	37.12	37.68	-0.57
	(11.75)	(13.03)	(1.25)
Respondent is household head (1=Yes, 0=No)	0.54	0.55	-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	8.32	8.35	-0.03
	(4.02)	(4.04)	(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	-0.02
	(1.56)	(1.60)	(0.12)
Household experienced drought in the past year	0.62	0.65	-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)	0.54	0.56	0.03
	(0.50)	(0.50)	(0.05)
Respondent has used mobile phone to make calls	0.72	0.69	0.02
	(0.45)	(0.46)	(0.04)
Respondent has used mobile phone to receive calls	0.88	0.85	0.02
	(0.34)	(0.36)	(0.04)
Panel B: Teacher-Level Characteristics (N=)			
Education (number of years)	8.55	8.30	.154
	(1.78)	(2.02)	(.213)
Age	32.62	32.77	-.148
	(8.02)	(8.97)	(1.13)
Gender (Female=1)	.371	.332	.062
	(.484)	(.472)	(.041)
Local (Teacher from village=1)	.678	.709	-.021
	(.469)	(.455)	(.049)

Notes: Column 1 presents the mean for ABC villages, Column 2 presents the mean for non-ABC villages for observations with non-missing information. 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 Mean (s.d.)	Non-ABC Mean (s.d.)	Difference Coeff (s.e.)
Panel A: Writing Z-scores			
Baseline Writing Test Z-score (both cohorts)	-.026	0	-.023
	(.886)	(1)	(.04)
N			
Panel B: Math Z-scores			
Baseline Math Test Z-score (both cohorts)	-.07	0	-.0592
	(.816)	(1)	(.0469)
N			

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

Panel A: Writing Z-Scores

	(1)	(2)	(3)	(4)
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 obs	13,402	12,823	12,823	12,823
R^2	0.006	0.060	0.085	0.130

Panel B: Math Z-Scores

	(1)	(2)	(3)	(4)
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, 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 by Student Characteristics and Region

	Writing Z-Scores			Math Z-Scores		
	(1)	(2)	(3)	(4)	(5)	(6)
ABC*Post	0.251	0.176*	0.159	0.456*	0.259**	0.268**
	(0.215)	(0.099)	(0.104)	(0.235)	(0.106)	(0.106)
ABC*Post*Dosso	-0.009			-0.041		
	(0.046)			(0.046)		
ABC*Post*Female		0.051			-0.001	
		(0.092)			(0.099)	
ABC*Post*Young			0.055			-0.033
			(0.108)			(0.112)
Main Effects and Interactions	Yes	Yes	Yes	Yes	Yes	Yes
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	0.086	0.098	0.089	0.090	0.091	0.090

Notes: Each Column represents a separate regression. Columns 1-3 present results with writing z-scores as the dependent variable. Columns 4-6 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 (4) include binary variables for Dosso, age and female; Columns (2) and (5) include binary variables for female and age; and Columns (3) and (6) include binary variables for young, 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: Heterogeneous Impacts by Teacher Quality

	Writing Z-Scores	Math Z-Scores
	(1)	(2)
ABC*Post	0.247**	0.270**
	(0.097)	(0.114)
ABC*Post*Good Teacher	-0.136	-0.031
	(0.165)	(0.169)
Student Gender, Cohort Interactions and Main Effects	Yes	Yes
	Yes	Yes
Sub-region fixed effects	Yes	Yes
Number of observations	12,823	12,840
R^2	0.088	0.090

Notes: All test scores are normalized to the contemporaneous non-ABC distribution. Test scores include data collected 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. "Good Teacher" is an indicator variable for whether the teacher's years of education are above the median. 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: 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 7: Impact of the ABC Program on Teacher and Student Attendance

	ABC Mean	Non-ABC Mean	Diff Coeff (s.e.)
Teacher Attendance (Number of Classes Taught)			
Year 1 Overall	53.87	56.61	-2.74
			(2.42)
N			
Year 1 First two months	35.57	37.23	-1.67
			(1.35)
Year 1 Last two months	26.77	27.31	-0.54
			(1.20)
Year 2 Overall			
Student Attendance Rate			
Year 1 Overall attendance	0.689	0.687	0.002
			(0.03)
Year 1 First two months	0.857	0.849	0.008
			(0.02)
Year 1 Last two months	0.853	0.843	0.01
Year 2 Overall			

Notes: Table displays the mean for ABC (Column 1) and non-ABC (Column 2) for 2009 and 2010, controlling for sub-region fixed effects. Column 3 reports the estimated difference. 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 7B. Effect of ABC on Student Interest in Education

Dependent variable:	*Called hotline*	*Number of calls*
	(1)	(2)
ABC	.228*** (.051)	6.26** (2.72)
Region fixed effects	Yes	Yes
Cohort fixed effects	Yes	Yes
Number of observations	139	139
R^2	0.12	0.13
Mean (s.d.) of non-ABC group	.413(.496)	5.24(15)

Notes: Data based upon results from the call-in hotline in January-March 2011. *, **, *** denote statistically significant at 10, 5 and 1 percent levels, respectively.

Table 8. Mobile Phone Usage after the Program

	ABC Mean (s.d.)	Non-ABC Mean (s.d.)	Difference Coefficient (s.e.)
Panel A: Mobile Phone Ownership			
Household owns a mobile phone	0.47	0.41	0.06
	(0.50)	(0.49)	(0.05)
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	0.85	0.00
	(0.36)	(0.36)	(0.04)
Used mobile phone since last harvest	0.75	0.68	0.07
	(0.44)	(0.47)	(0.04)
Made calls	0.80	0.73	0.07
	(0.40)	(0.45)	(0.05)
Received calls	0.91	0.90	0.01
	(0.29)	(0.30)	(0.04)
Wrote SMS	0.13	0.03	0.10***
	(0.35)	(0.17)	(0.03)
Received SMS	0.14	0.08	0.06*
	(0.35)	(0.28)	(0.03)
Send or received a beep	0.32	0.21	0.11**
	(0.47)	(0.41)	(0.05)
Transferred airtime credit	0.05	0.02	0.03*
	(0.21)	(0.13)	(0.02)
Received credit	0.16	0.12	0.04
	(0.37)	(0.33)	(0.04)
Panel B: Uses of Mobile Phones for Communications			
Communication with migrant since last harvest	0.35	0.33	0.01
	(0.47)	(0.47)	(0.07)
Communicate with family/friends inside Niger	0.80	0.75	0.05
	(0.40)	(0.43)	(0.05)
Communicate with commercial contacts inside Niger	0.12	0.08	0.04
	(0.33)	(0.28)	(0.03)
Used mobile phone to communicate death/ceremony	0.28	0.27	-0.01
	(0.44)	(0.45)	(0.07)
Used mobile phone to ask for help/support	0.17	0.20	-0.03
	(0.38)	(0.41)	(0.05)
Used mobile phone to ask for price information	.10	0.07	0.03
	(0.30)	(0.26)	(0.03)

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. 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.

Table 9: Impact on Attendance by Teacher's Education

Dependent Variable: Percent classes attended	Months 3 and 4	(Placebo) Months 1 and 2
	(1)	(2)
ABC	-2.91	0.00
	(1.80)	(0.02)
ABC*Good Teacher	7.80**	0.01
	(3.50)	(0.03)
Good Teacher	-7.87***	0.01
	(2.91)	(0.02)
Gender, Cohort	Yes	Yes
Sub-region fixed effects	Yes	Yes
Number of observations	6,022	5,976
R^2	0.12	0.12

Notes: The dependent variable in Column (1) is what percentage of the classes the student was present during months 3 and 4 of classes. For Column (2) this refers to months 1 and 2, i.e. before the start of the mobile phone intervention. "ABC" is an indicator variable for whether the village was assigned to the ABC program, 0 otherwise. "Good Teacher" indicates that the teacher's years of education are above the median level of education (9 years). 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.

Appendix

Table A1: Attrition andTest Absenteeism

	ABC Mean (s.d.)	Non-ABC Mean (s.d.)	Difference Coefficient (s.e.)
Panel A: Drop-Out			
Pre-ABC Module	.042	.035	-.009
	(.2)	(.184)	(.015)
Post-ABC Module	.036	.06	-.015
	(.186)	(.238)	(.02)
Panel B: June Test Rounds (Immediate)			
Absenteeism (absent day of test=1)	.199	.192	0
	(.4)	(.394)	(.024)
Age of absentee	34.13	37.48	-1.33
	(11.804)	(12.088)	(.912)
Gender of absentee (female=1)	.492	.356	.141***
	(.5)	(.479)	(0.03)
Panel C: January Test Rounds (Persistent)			
Absenteeism (absent day of test=1)	.293	.289	.001
	(.455)	(.454)	(.023)
Age of absentee	34.25	36.06	-.941
	(11.74)	(12.61)	(.941)
Gender of absentee (female=1)	.441	.401	.036
	(.497)	(.49)	(.023)

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 Mean (s.d.)	Non-ABC Mean (s.d.)	Difference Coefficient (s.e.)
Panel A: Teacher-Level Characteristics in 2009			
Education (number of years)	8.86	8.254	.263
	(1.32)	(2.29)	(.321)
Age	32.25	33.07	-.246
	(6.65)	(9.63)	(1.83)
Gender (Female=1)	.345	.254	.094
	(.479)	(.439)	(.083)
Local (Teacher from village=1)	.667	.763	-.061
	(.475)	(.429)	(.085)
Panel B: Teacher-Level Characteristics in 2010 (N=)			
Education (number of years)	8.41	8.32	.099
	-1.95	-1.89	(.231)
Age	32.79	32.63	.001
	(8.62)	(8.67)	(1.158)
Gender (Female=1)	.383	.367	.03
	(.488)	(.484)	(.039)
Local (Teacher from village=1)	.683	.685	-.025
	(.467)	(.466)	(.048)

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 and sub-region fixed effects to account for randomization. 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	Simple Difference		Value Added	
	(1)	(2)	(3)	(4)
ABC	0.172**	0.129*	0.179**	0.141**
	(0.086)	(0.069)	(0.085)	(0.068)
Baseline Test Z-score			0.083***	0.065***
			(0.017)	(0.014)
2009 Cohort	0.041	0.081	0.029	0.074
	(0.084)	(0.069)	(0.084)	(0.069)
Female	-0.501***	-0.506***	-0.497***	-0.506***
	(0.044)	(0.044)	(0.044)	(0.044)
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	0.094	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

Panel A: Literacy

	(1)	(2)
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: Numeracy

	(1)	(2)
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. 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.