ABC, 123: 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 often 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, achieving a first-grade level within eight months of classes. Students in ABC villages achieved additional literacy and numeracy gains, with test scores 9-20 percent higher than those in non-ABC villages. Evidence suggests that there are persistent impacts of the program: seven months after the end of classes, average test scores are still higher in ABC villages. These effects do not appear to be driven by differences in class time devoted to students, teacher characteristics or teacher attendance. Rather they are largely explained by the effectiveness of mobile phones as a motivational and educational tool: students in ABC villages used mobile phones in more active ways and showed a higher interest in education. 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; literacy; information technology; program evaluation; Niger
JEL Codes: D1, I2, O1, O3

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1. Introduction

Despite decades of investment in primary, secondary and adult education in developing countries, nearly 18 percent of adults worldwide are unable to read and write in any language (UNESCO 2008).[1] Decades of economic research have produced compelling evidence that education can improve productivity, earnings, health outcomes and social networks (G. Akerlof and R. Kranton 2002, A. Case 2006, E. Hanushek 1995, E. Hanushek and L. Woessmann 2008, A. Krueger and M. Lindahl 2001). In the stochastic livelihood settings of the poor, these gains can improve an individual's ability to "deal with disequilibria" (T. Schultz 1975) by refining one's ability to acquire and process information and to discover and benefit from new technologies (e.g., A. Foster and M. Rosenzweig 1996). Thus, identifying policies that improve educational outcomes for children and adults is crucial for welfare in developing countries.

While a majority of countries in Africa have ensured access to primary education, expanding the quantity of education is insufficient in practice. The returns to such investments hinge crucially on whether such interventions improve the quality and persistence of education gains. There is considerable debate over whether educational inputs (e.g., textbooks, flipcharts, computers), teacher training and incentives (Duflo et

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[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.

al), conditional cash transfers, scholarships or medication (Miguel and Kremer) can be effective in improving enrolment, attendance and the quality of learning (P. Glewwe et al. 2004, M. Kremer 2003).

Many of these educational investments typically target school-aged children, but adult literacy programs are often promoted as a means of reaping more immediate returns (N. Blunch and C. Pörtner forthcoming). Yet large-scale adult literacy programs are often characterized by low enrollment and high dropout rates and a rapid loss of acquired skills (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).[2] This failure for adult literacy gains to persist may be due to the irrelevancy of basic literacy skills for targeted populations and the lack of opportunities to practice literacy and numeracy skills in an individual's native language.

This paper uses data from a randomized experiment to test whether a new technology – mobile phones -- can be harnessed to overcome these constraints and improve the quality and persistence of adult literacy gains. Partly due to their rapid spread throughout the developing world, mobile phones have drawn substantial attention for their potential impacts on economic development (J.C. Aker and I.M. Mbiti 2010, A. Bhavnani et al. 2008, M. Castells and et al. 2007, S. Corbett 2008, J. Donner 2008). One of the demonstrated channels through which mobile phones improve the welfare of the poor is increased market efficiency (J. Aker 2010, R. Jensen

[2]Abadzi (2003) notes that during the 1970s, literacy programs surveyed had success rates of 12.5 percent, though in the 1980s these rose to a median of about 60 percent of adults acquiring basic literacy skills.

2007, J. Labonne and R. Chase 2009). While mobile telephony could have an indirect effect on education via improved earnings, it may have an even more direct effect on education by providing an incentive for adults to use their literacy skills in their native languages (e.g., via Short Message Service, or SMS) and in livelihood-relevant settings.[3] Indeed, the motivation for this project was based on preliminary observations of this incentive at work in our research setting: the large cost difference between SMS and voice services in Niger motivated some previously illiterate grain traders to teach themselves how to read and write simple SMS.

In this context, teaching adults how to use mobile phones may enable adults to practice their newly acquired literacy skills outside of class by sending and receiving SMS in their native language. The technology could also affect the returns to education by allowing households to use the technology for other uses, such as obtaining price and labor market information and using them for informal insurance mechanism.

We estimate the impact of a mobile phone adult education program in Niger (Project *Alphabétisation de Base par Cellulaire,* or ABC), a country where more than 71.3 percent of the population over 15 was classified as illiterate in 2007 (INS and Macro International 2007). Implemented by Catholic Relief Services (CRS), the project was conducted in 117 villages in two regions of Niger, each with separate randomizations into treatment and control groups. While students in ABC and non-

[3] 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 and social incentive to use SMS as the preferred communication platform.

ABC villages followed the same basic adult education curriculum, those in ABC villages also learned how to use a simple mobile phone.[4] By contrast, the randomized nature of our intervention allows us to identify the impact of mobile phones on educational outcomes over time.

The results suggest that adult education classes were successful in improving adults' educational outcomes, allowing students to obtain the equivalent of a second-grade math education within an eight-month period. We also find evidence that mobile phones increased this learning more quickly: Adults' writing and math z-scores were 10-20 percent higher in ABC villages, suggesting that students achieved an additional year of primary school education. These results are robust to a variety of specifications, including alternative measures of the dependent variable. While both groups exhibit a strong depreciation in skills after the end of the program, we find that the rate of skills depreciation was smaller for ABC students at the upper end of the distribution. This suggests that mobile phones might improve the persistence of education gains.

We explore three potential mechanisms by which mobile phones might improve adult education outcomes, including increasing teachers' and students' effort within the classroom and student effort outside of the classroom, by enabling students to use their newly-acquired skills. We find evidence that the latter two mechanisms are the most compelling. To assess student effort, we use both student attendance and a behavioral

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[4]We distinguish between simple mobile phones – those which primarily have voice and SMS capability – from smart or multimedia phones, which often have internet or video capability. The ABC program did not provide any additional educational content or games via mobile phones.

measure of student interest in education (a call-in hotline for which network charges occur) and find that students in ABC villages were 12 percentage points more likely to call the hotline than those in non-ABC classes. In addition, students in ABC villages used mobile phones more frequently than their non-ABC counterparts, and were more likely to send and receive SMS, primarily to communicate with friends and family.

Prior evidence on the effectiveness of adult education programs is limited. While Blunch and Portner (2010) assess the impacts of adult literacy programs on economic outcomes, other studies on the impact of such programs on educational outcomes (N. Blunch and C. Pörtner forthcoming, G. Carron 1990, D. Ortega and F. Rodríguez 2008) often rely upon self-reported literacy scores or do not have a convincing identification strategy. This paper overcomes these shortcomings by using a randomized experiment in a large number of villages, combined with data on externally-administered and standardized writing and math tests, teacher quality, attendance and household-level data.

Our findings that information technology leads to an improvement in skills acquisition is in line with studies of the impact of computer-assisted learning in other contexts. For example, Banerjee, Cole, Duflo and Linden (2007) found that computers increased students' math scores, and was equally effective for all students. They also found that these gains were short-lived, with some persistence over time. Barrow, Markman and Rouse (2009) find that students randomly assigned to a computer-assisted program score significantly higher in math scores, primarily due to more individualized instruction. Yet our experiment is unique in that is uses a relatively low-cost technology and one that did not require specialized instruction or software.

The remainder of the paper is organized as follows. In the next section, we provide the background on the setting of the research, as well the randomized intervention. Section 3 describes some key features of the data. Section 4 outlines our estimation strategy. Section 5 discusses the results. Section 6 addresses alternative explanations and the potential mechanisms, whereas Section 7 provides a simple cost-effectiveness analysis. Section 8 concludes.

2. Project ABC

Project ABC was implemented by Catholic Relief Services, an international non-governmental organization, in two rural regions of Niger, Dosso and Zinder. Niger is one of the poorest countries in the world and the lowest-ranked country on the UN's Human Development Index (HDI). 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: Close to 90 percent of adults in the regions are illiterate, unable to recognize letters or numbers.

While both regions are located in similar agro-climatic zones, they are over 500 km apart and exhibit distinct ethnic and environmental differences. Dosso is approximately 240 km from the capital city (Niamey), is primarily populated by the Zarma and Hausa ethnic groups and depends upon rainfed agriculture and small ruminants. Zinder, in the far east of the country, is located 750 km from the capital, is primarily populated by the Hausa and Kanuri ethnic groups and depends upon rainfed agriculture and both small and large ruminants. Due to these differences, random assignment to treatment status was conducted separately by region.

Of the 140 CRS intervention villages across the two regions, only 117 were eligible for our study.[5] The randomization first stratified villages by region and then by administrative divisions within each region. Randomization into program and comparison groups was then carried out separately within each stratum using a random number generator. Approximately half of the villages (55) were selected to participate in the first year of classes in 2009, with half of these were selected to participate in the ABC program. The same approach was followed for the 2010 cohort (Figure 1). We therefore present tests for the equality of means for the entire sample and by region.

The first phase of the adult literacy program began in February 2009. 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 slots per class. An individual was considered to be eligible for the literacy program if he/she was: 1) a member of a formal or informal producers' association within the village; 2) unable to read or write letters or numbers in any language; and 3) willing to participate

[5] A number of CRS' intervention villages were excluded from the randomized evaluation. At CRS' request, we excluded villages that already had an ongoing adult literacy program administered by a different organization. We also excluded villages in which there was no mobile phone coverage at the time of the village selection in December 2008 (though these villages did receive the literacy program).

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.

The adult education intervention covered eight months of literacy and numeracy instruction over a two-year period. Courses start in February of each year and continue until June, with a seven-month break between June and February due to the agricultural planting and harvesting season. Thus, the 2009 cohort started classes in February 2009 and finished in June 2010. 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]

In partnership with CRS, the authors developed a mobile phone module to incorporate into the traditional literacy and numeracy curriculum. Participants in ABC villages therefore followed the same curriculum as those in non-ABC villages, but with two modifications: 1) participants 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) the project provided mobile phones to groups of literacy participants (one mobile phone per group of five people).[7] The mobile phone module began three months after the start of the literacy courses each year, and neither students, teachers nor CRS staff were informed which villages were selected for the ABC project until two weeks prior to the start of

[6]The primary local languages spoken in the Dosso and Zinder regions of Niger are Hausa, Zarma and Kanuri, although the language of instruction was only in Hausa and Zarma.
[7] Although the provision of mobile phones to groups of five could potentially have a wealth effect, as the phones did not belong to one specific individual, the wealth effect would be 1/5th the price of the mobile phone, or USD$2. Moreover the households were not allowed to sell the phone.

the module.[8] Students in ABC villages were not given additional class time, as the mobile phone module was integrated into their regular weekly class schedule.

3. Data

3.1 Test Score Data

The study timeline is presented in Figure 2. Literacy and numeracy tests were administered to all fifty students in each village prior to the start of courses, providing a baseline sample of over 4,000 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, which allows us to estimate the immediate impacts of the program for both cohorts.[9] 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 due to continued mobile phone use. The design of the program also diffuses concerns about differential attrition between treatment and control villages.[10]

[8]Students in ABC villages had less than 6 weeks of practice using mobile phones during class time each year. Literacy courses are held for five days per week for three hours per day. As one day per week was allocated to revision of previous material, teachers in ABC villages were instructed to teach the mobile phone module during this class.

[9]At the onset of the program, the original intention was to collect literacy data from the 2009 and 2010 cohorts during each phase of data collection. Conducting literacy tests in June 2009 with the 2010 cohort proved to be unfeasible, and so data for the 2010 cohort are available in January 2009, June 2010 and January 2011.

[10]Attrition in literacy classes typically occurred within the first month of the course. As mobile phone activities did not begin until two months after the start of the course, and literacy teachers and students were not informed of the ABC program or their treatment status in advance, the decision to drop out of the literacy course could not have been based upon the ABC program. Similarly, once a student misses several weeks' of classes, the teacher would not allow him or her to re-enter the class, as they have fallen

The literacy and numeracy 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 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). The literacy and numeracy tests are provided in Figures A2.[11] Test scores are our primary data for analyzing the impacts of the program on educational outcomes.

3.2. Student and Teacher Data

As test score data do not provide insights into the mechanisms of the program, we collected additional data on student and teacher-level characteristics. At the

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behind in the curriculum. For this reason, students who dropped the course before the ABC module was introduced could not re-enter the program at a later time or rejoin the class the following year.

[11]The different levels of the literacy and numeracy tests can be compared to primary school grades in Niger. For math test scores, Level 2 corresponds roughly to first grade, Level 3 corresponds to second grade, and Levels 4 through 6 are third and fourth grades, respectively. The comparison with writing test scores is more difficult, as the language of instruction in primary schools in Niger is French or Arabic. Nevertheless, Levels 2 and 3 on the writing test would roughly correspond to first grade, whereas Levels 4 through 6 would correspond to second grade.

student level, we conducted a detailed household survey, interviewing a total of 1,038 literacy students across 100 villages. A baseline household survey was conducted in January 2009, with follow-up surveys 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 collected data on student-level attendance, teacher attendance and teacher-level characteristics for each class.

3.2. Pre-Program Characteristics of ABC and Non-ABC Students

Table 1 suggests that the randomization was successful in creating groups comparable along observable dimensions. Differences in pre-treatment household characteristics are small and insignificant (Table 1, Panel A). Average household size was eight. Children's educational achievements were similarly low: less than 10 percent of children aged 7-15 had ever attended primary school. Thirty percent of households in the sample owned a mobile phone prior to the start of the program, with eighty percent having access to a mobile phone within the village. Over 50 percent of respondents had used a mobile phone in the few months prior to the baseline, although almost exclusively for receiving calls.

Table 2 provides further evidence of the comparability of the program and comparison groups.[12] We cannot reject the equality of means for pre-program writing scores in the full sample (Panel A), although math test scores at the baseline are higher

[12]Results in Table 2 do not control for randomization fixed effects nor cluster the standard errors at the class level. Clustering improves the balances of baseline test scores.

in non-ABC villages. While the magnitude of the difference in math is relatively small (equivalent to one student in two non-ABC classes achieving a Level 1), the difference is statistically significant at the 1 percent level without clustering the standard errors. This suggests that a simple comparison of means might underestimate our results and that we should control for baseline test scores. The same patterns emerge when looking at test scores by region (Panels B and C). Overall, baseline writing and math scores for both program and comparison villages were close to zero, suggesting that the project selected participants who were illiterate and innumerate prior to the start of the program. The results are similar when using z-scores (Table A1).

4. Estimation Strategy

To estimate the impact of mobile phones on educational outcomes, we use a simple reduced form regression 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.[13] ABC_v is the treatment status indicator of village v, $round_t$ is an indicator variable for the test score round (January 2009, June 2009 or June 2010), $cohort_v$ is a binary variable equal to one of the village started in the 2009 cohort, 0 otherwise, and θ_R are geographic fixed

[13]It is common practice in education studies to normalize test scores (P. Glewwe, M. Kremer, S. Moulin and E. Zitzewitz 2004, M. Kremer et al. 2009), as often test instruments are not comparable across rounds. As our test has the same test structure during each period and has the same scoring structure, all results are robust to the use of the nominal test score.

effects at the regional or sub-regional level. X'_{iv} is a vector of student-level covariates, such as sex, ethnicity and age. We estimate a difference-in-differences specification:[14]

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

where $ABC_v{*}round_t$ is the interaction between being assigned to treatment and the particular round. The coefficient of interest is β_3, which captures the average impact of the mobile phone education program. The error term consists of μ_{cv}, a common class-level error component capturing common local or teacher characteristics, and ε_{iv}, which captures unobserved student ability or idiosyncratic shocks. As classes were separated by gender and taught by different teachers within the village, we cluster the error term at the class level. Nevertheless, we also cluster the standard errors at the village level as a robustness check.

5. Results

5.1. Short-Term Impact of the ABC Program

Figure 3 summarizes the key results of the paper. The graph shows the difference in mean writing (Panel A) and math (Panel B) test scores prior to the program, immediately after the program and seven months after the end of classes for the first year of classes. Overall, the adult education program strongly improved

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[14]Equation (1) is our preferred specification for two reasons. First, if the randomization of the ABC program was not perfect – as suggested by the difference in math scores at the baseline – the DD specification will control for potential pre-treatment differences in means between ABC and non-ABC villages. Second, the DD specification enables us to control for common village-level fixed effects across different rounds. The results are robust to a simple difference specification.

literacy and numeracy skills in both the ABC and non-ABC villages immediately after the program, suggesting that the curriculum was effective in helping students to achieve a first-grade level in writing and a second-grade level in math. The ABC program helped students to achieve additional gains: Average test scores in ABC villages were 20 percent higher for writing and 11 percent higher for math, respectively. Yet despite these strong initial gains, both groups experienced a depreciation in both writing and math skills after the end of classes, with relatively weaker depreciation for math skills.

Table 2 provides more detailed information on the evolution of test scores during the first and second years of the program for the overall sample (Panel A) and by region (Panels B and C).[15] After the first year of classes, students moved from a "beginner" level (i.e., not being able to recognize and write letters) to an average score of 1.88 and 1.56 in ABC and non-ABC villages, respectively. Participants in both classes were therefore able to correctly write letters and syllables, and average writing test scores in ABC villages were 21 percent higher. After the second year of the program, students in both ABC and non-ABC villages achieved a first grade writing level, with average test scores in ABC villages 9 percent higher. Without controlling for clustering at the class level, the effect of the ABC program is strongly statistically significant after both the first and second year of the program.

[15]"5 month test scores" refers to the June 2009 data for the 2009 cohort and the June 2010 data for the 2010 cohort. "17 month test scores" refers to the June 2010 data for the 2009 cohort.

Math test scores also increased considerably after the first year of the program, with average test scores of 2.41 and 2.18 in ABC and non-ABC villages, respectively. This suggests that the average math test scores in ABC villages were 10 percent higher after the first year of the program. By the second year of the program, average math test scores were 13 percent higher in ABC villages: This suggests that students in ABC villages were able to transition from an inability to recognize any numbers to successfully solving more complicated addition and subtraction problems, equivalent to a second grade math education in Niger. Overall, the results suggest that the effect of the program was primarily driven by the Dosso region. Table A1 shows that the results are similar when using normalized test scores.

Table 3 presents the results of equation (1) using standardized test scores and controlling for regional, demographic and round fixed effects. Overall, the results are similar as those in Table 2. The ABC program has a positive and statistically significant impact on writing z-scores (Panel A, Column 1), increasing students' test scores by .184 standard deviations. This effect is robust to the inclusion of regional, gender and cohort and round fixed effects (Column 2) and to the inclusion of sub-regional fixed effects to control for the randomization process (Column 3). The results are also robust to the inclusion of village-level fixed effects.

The results are stronger in magnitude and statistical significance for numeracy: the ABC program increases math z-scores by .25 standard deviations (Panel B, Column 1). This represents a 10-13 percent increase in math scores as compared with the comparison group. These results are robust to the use of region, gender and cohort fixed effects (Panel B, Column 2), the inclusion of sub-regional fixed effects to control for the randomization process (Panel B, Column 3) and village-level fixed effects (Panel B, Column 4).[16]

5.2. Heterogeneous Effects

We would expect greater learning benefits among subpopulations for whom complementarities between education and technology are stronger, such as those who are engaged in entrepreneurial activities and relatively younger populations. In this section, we turn to the results by geographic and demographic characteristics.

Table 4 presents the effects of the ABC program by region, gender and age. Column 1 tests for a differential impact of the program by region, containing a triple interaction between the ABC program, the Dosso region and round. While the ABC program had a relatively stronger impact on students' writing and math z-scores in the Dosso region, this effect is not statistically significant at conventional levels.

Column 2 assesses the impact of the program by gender. In general, test scores are lower for women; women's average writing test scores are one level lower as compared with men's test scores, with a similar difference for math test scores. The coefficient on the triple interaction term is not statistically significant at conventional

[16]The results in Table 3 are robust to estimating a simple difference specification between ABC and non-ABC villages, although the magnitude and statistical significance is weaker (Table A2).

levels, suggesting that the ABC program does not have a differential impact on men and women.[17]

Most empirical specifications of education production functions impose a quadratic age relationship based upon the assumption that educational achievement increases with age at a decreasing marginal rate. The average age of literacy participants is 37 years, with a standard deviation of 12 years. While there is not a statistically significant difference in the participants' ages across ABC and non-ABC villages, there is a statistically significant difference by region: participants are an average of five years younger in the Zinder region. Column 3 includes a triple interaction term between the ABC program, age and time, defining "young" as those younger than 45 years of age. The adult education program seems to be more useful for younger participants: those who are under age 45 have higher average writing and math z-scores as compared with those over 45. Yet the coefficient on the interaction term for ABC and age is not statistically significant at conventional levels. We therefore find no evidence that the ABC program has a differential impact by age on writing (Panel A, Column 3) or math z-scores (Panel B, Column 3). Similar regressions testing for heterogeneous impacts of the ABC program for the 2009 cohort by year also show that the program does not exhibit strong time effects (Table A3).

5.3. Distributional Effects

[17] Further disentangling the effect of the ABC program by gender and by language (Hausa or Zarma) suggests that the ABC program is relatively more effective for women as compared to men in Zarma villages.

While previous regressions estimate the average effects of the program, we consider that the ABC program might affect the distribution of educational gains. For example, while mobile phones might not be useful as an educational tool for students at lower skill levels it could be more useful for students once they have achieved a higher level.

Figures 4a and 4b provide suggestive evidence that the ABC program increased the proportion of students achieving higher test scores. The graphs show the coefficient from logit regressions in which the dependent variable was having obtained a particular level in writing or math test scores (Table A4). For writing scores, the ABC program seems to have increased the proportion of students achieving the top levels substantially, although we cannot reject the equality of means for math scores.

5.4. Persistent Impacts of the ABC Program

Widespread 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). While we find that the ABC program can reinforce skills acquisition in the short-term, we wish to test whether technology – in particular mobile phones – can improve the persistence of educational gains. Although no comparable evidence exists for adult students, mobile phones, computers and other devices have encouraged school-aged students to learn outside of the classroom (A. Banerjee et al. 2007, L. Linden et al. 2003).

Table 5 assesses the potential persistent impacts of the ABC program, using test score data from January 2010 for the first cohort and January 2011 for both cohorts (seven months' after the end of classes). The tests conducted during this period were not previously announced, so neither students nor teachers were able to prepare for the tests in advance.[18] Thus, there is potential non-random attrition during the January test rounds. If more ABC students with higher test scores were absent from the January tests, then this could be a lower bound on the effect. However, if more ABC students with lower test scores were absent from the ABC villages, then this could be an upper bound for the ABC effect.

Overall, the results in Table 5 show that the education gains persisted after the end of the project. Test scores are approximately 25 percent lower seven months after the end of classes, suggesting that there is some depreciation for both math and writing. However, writing and math z-scores are higher in ABC villages after the program, with a statistically significant difference between the two.

It is important to note that these results do *not* hold if we include the 2009 cohort from Zinder (Table A5). The Zinder region experienced a devastating drought in 2009 with over 50-90 of households' harvests destroyed. The depreciation of skills in the seven months after the drought was severe. The severity of the incident justifies – in our view – treating these observations differently, as this event substantially altered the pattern of normal depreciation in these villages. The pattern of depreciation of

[18]The results are similar if we exclude the June 2011 tests for the 2009 cohort, as they might have been aware that tests might happen.

skills in the 2010 Zinder villages does not seem different from the 2009 or 2010 cohort in Dosso. However, this implies in terms of external validity that the additional gains from ABC might not be persistent in the face of massive unanticipated shocks.

5.5. Alternative Explanations

One concern with the previous results is that 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. While this concern is highly unlikely due to the randomized nature of the intervention – and the fact that the teachers were selected well before the ABC villages were announced – we formally test for this explanation.

We collected additional data on teacher-level characteristics for each year, including the highest level of education obtained, age, gender and village residence. Our data were also verified with the teacher data collected by the Ministry of Non-Formal Education. Table 6 presents a comparison of means between ABC and non-ABC villages by education, age, gender and "local" status (i.e., whether the teacher is from the community) for each year, as new teachers were added and some teachers were replaced between 2009 and 2010. Overall teacher characteristics are balanced between ABC and non-ABC villages across all characteristics for both years of the program. On average, literacy teachers were 32 years old and teachers had an average of 8.5 years of education, equivalent to secondary school in Niger. Roughly one-third of the literacy teachers were female, implying that male teachers were teaching women's classes. Approximately two-thirds of teachers were from the village where the education classes were held. Overall, these results suggest that observable differences in teacher quality are not driving the results.

A second potential confounding factor is different social interactions among students in ABC and non-ABC villages, or the "study group effect". Since the ABC program provided mobile phones to groups of five students, this self-group formation 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 the mobile phones. While we currently 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 students spend little time studying in these study groups outside of class given the relatively heavy workload of the literacy classes. In addition, among classes that did encourage study groups, there were no systematic differences in the use of study groups across ABC and non-ABC villages. Therefore, it seems unlikely that adult participants in ABC classes were organizing into extra-class study groups, thereby increasing test scores.

6. Mechanisms

There are a variety of mechanisms through which the ABC program could affect student learning in the short- and long-term. 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 the broader discussion of educational inputs such as textbooks, flip charts and visual aids. 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. This pricing scheme may provide a powerful financial incentive to learn to read and write text messages.[19] Finally, the mobile phone can facilitate learning outside of the classroom – both during the course and after the end of courses – thereby serving as a dynamic learning tool. We explore these mechanisms in this section.

6.1. Teacher Motivation

It is plausible that the presence of mobile phones or a new curriculum increased teacher motivation and effort within or outside of the classroom, thereby improving students' performance. As we are unable to directly observe teacher effort, we provide an observable (although admittedly imperfect) proxy. CRS and the Ministry of Non-Formal Education provided norms for the number of classes to be taught during each

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

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 22 classes per month (Table 6), and there was not a statistically significant difference in the number of classes taught between program and comparison villages. This provides suggestive evidence that teachers in ABC classes were not teaching more classes and hence improving test scores.

6.2. Student Motivation

The presence of the ABC program could have encouraged greater student effort, which might have occurred if the returns to effort within class were higher because teachers had access to a new educational tool. Increased student effort could also have occurred if students perceived that the skills in class were more useful due to the mobile phone. While we cannot observe in-class student effort, we use student attendance as a proxy for effort. We find that student attendance was three percentage points higher in ABC villages, with a statistically significant difference between the two (Table 6).[20] The results are relatively stronger in Zinder, where student attendance was 7 percentage points higher. These results are robust to the inclusion of regional and sub-regional fixed effects, but not to the inclusion of teacher-level controls. The attendance records provide some evidence that student effort increased in response to the ABC program.

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[20]The high rate of student attendance in ABC and non-ABC villages is somewhat surprising, as students were provided with a monthly food ration based upon their monthly attendance record. Therefore, conditional on receiving food aid, there was still a statistically significant difference in student attendance between ABC and non-ABC villages.

Experimental evidence on student effort also provides evidence that students in ABC villages exerted more effort and are more the enthusiastic about learning. In January 2011, students in all villages were invited to call a "hotline" to express their support for adult education classes.[21] 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 the CRS and local primary and secondary schools, and so have little market value and no alternative use outside of education. Since students had to pay for the calls, we interpret the "hotline" participation as a more reliable measure of students' interest in education than attendance or a hypothetical survey measure. The leaflet promoting the hotline is depicted in Figure 5.

Table 7 presents the results of a regression of the ABC indicator variable on two outcome measures, namely, whether at least one person in the village called the hotline and the number of calls received per village. While the interpretation of the coefficient on the ABC binary variable is bundled – in other words, capturing students' interest in the education program and education materials -- the results provide supporting evidence of the program's impact on upon students' interest in education. As compared with a mean of 41 percent in non-ABC villages, ABC villages were 23 percentage points more likely to call the hotline than their non-ABC counterparts (Column 1), and is

[21] Call-in-hotlines or its predecessor 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 AIDS behavioral change in Tanzania (P.W. Vaughan et al. 2000). In the political economics literature, Vicente, Aker and Collier used a call-in-hotline in the context of an election-monitoring campaign (2010).

robust to the use of a probit specification (Column 2). ABC villages were also more likely to call the hotline more frequently, calling an average of 6 more times per village as compared to the calls received in non-ABC villages (Column 3). This represents an increase of 200 percent.

We provide some insights into the characteristics of those who called the hotline (Table A6). Those who called the hotline were more likely to be from the Zinder region (80 percent of all calls), male (83 percent) and have participated in the 2009 cohort (57 percent). In addition, while students who called the hotline had test scores that ranged from 0 to 6, the average math and writing test scores of callers was 3.5. This suggests that callers correctly write sentences and do more complicated math calculations, perhaps making it easier to use the mobile phone. Finally, as approximately 25 percent of callers were able to be linked to the student class lists, this suggests that a significant number of non-student village members called the hotline. This rate was relatively higher in ABC villages, suggesting that both the adult education classes and the ABC program increased community interest in education.

6.3. Mobile Phone Usage Outside of Class

The previous results suggest that the primary mechanism through which ABC affects learning is practice outside of the classroom. Table 8 shows the results of a regression of a variety of outcomes related to mobile phone usage in ABC and non-ABC villages in January 2010, after one year of the program. The ABC program did not appear to affect household's mobile phone ownership, access to a mobile phone or their frequency of usage since the past harvest. Furthermore, the ABC program did not appear to lead to more passive usage of mobile phones, such as calls. However, students in ABC villages used mobile phones in more "active" ways, particularly by writing and receiving SMS, "beeping" [22] and sending airtime credit, all of which require more advanced letter and number recognition. They also used mobile phones to remain in contact with friends and family members more frequently. In both ABC and non-ABC villages, mobile phones were primarily used for social communications: over 25 percent of households used mobile phones to communicate news of a funeral or to request aid. Overall, these results suggest that mobile phones affect learning by enabling students to practice the skills acquired both during and outside of class, mainly by strengthening communications within familial networks.

6. 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 literacy 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

[22]Beeping 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.

priority for the poorest countries using a simple comparison of the benefits and costs of the ABC program.

While 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), we conduct a *cost-effectiveness* analysis, thereby focusing only on the costs of the program as compared to its educational impacts. Evans and Ghosh (2008) assess the cost-effectiveness of different education interventions in developing countries using three outcomes (enrollment, attendance and test scores), concluding that adult literacy programs are cost-effective in increasing enrollment. To measure the cost-effectiveness of ABC, we follow the approach outlined in Ortega and Rodriguez (2008) define "benefits" as the writing or math z-scores. We then compare ABC and non-ABC centers to isolate the additional costs and benefits associated with introducing mobile phones into adult literacy training, implicitly netting out broader livelihood gains from increased literacy.

Figure 6 shows the cost per student in the normal and mobile phone literacy programs. Over a two-year period, the per-student program cost was US$18.35 per student in non-ABC villages and US$21.30 in ABC villages. Thus, for an additional US$2.93 per student, students were able to increase their test scores by an average of .17 and .25 standard deviations for writing and math, respectively.

Compared with most of the interventions assessed by Evans and Ghosh (2008) – including blackboards, furniture, teacher training and teacher and student incentives -- the ABC program is relatively more expensive. With a per-student cost of $1.5 for each

.1 s.d. increase, the program is relatively less expensive than most other interventions, and more expensive than school inputs (blackboards, workbooks and furniture), remedial education (India), teacher training (Honduras) and a capitation grant. Yet such a simple comparison probably underestimates the potential cost effectiveness of the program for several reasons. First, the ABC intervention combined an educational input (the mobile phone) with teacher training in how to use the mobile phone, rather than a one-off intervention. Second, average baseline test scores for adult students in Niger was effectively zero before the program, suggesting that the corresponding s.d. increase resulted in a much higher percentage change in outcomes for ABC as compared with other programs. And finally, as the ABC program targets adults, it is possible that there could be more immediate private and social returns to the program as compared with those that target school-aged children.

Finally, the ABC program relies upon simple (and locally available) mobile phones, rather than smart or multimedia phones, and does not require a specific program or software. Consequently, this suggests that the program is easily scalable and replicable in other contexts.

7. 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 (e.g., textbooks and flipcharts), the relevance of literacy skills in an individual's daily life and dearth of easily accessible materials in indigenous languages. Furthermore, studies on the impact of educational inputs in improving attendance and educational outcomes have primarily focused on school-aged children, and even fewer have assessed the impact of information technology.

An intervention that taught students how to use a simple information technology increased students' skills acquisition in the short-term in Niger. This suggests that mobile telephony could be a simple and low-cost way to promote adult's educational outcomes. The treatment effects treatment are striking: the program increases test scores by 9-20 percent, translating into a .20-year increase in schooling for writing and a one-year increase in schooling for math. An additional advantage of the technology-based program such as the one in Niger is that it could reduce skills' depreciation after the end of classes. It may also result in adult students who are motivated to learn, as it appears to stimulate interest in learning itself.

Nevertheless, the mobile phone-based education intervention may be limited in its scope and applicability. First, it will only be effective in cases where telecommunications infrastructure currently exists, so that adult populations can use the technology. Second, the effect of such technology-based programs will depend upon the existing adult education infrastructure, which varies greatly across countries. Furthermore, the benefits could depend upon the pricing structure of voice and SMS functions for mobile phones. If the ratio of voice and SMS costs is near one, then the financial incentive to use writing-based functions could be diminished, although this might be less of a concern with the introduction of mobile money in many developing countries. Finally, the approach may not be as effective for adult education programs that teach in languages that already have widespread writing materials available (e.g., French, English, Swahili and Portuguese in sub-Saharan Africa).

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Figure 1. Map of Project Areas

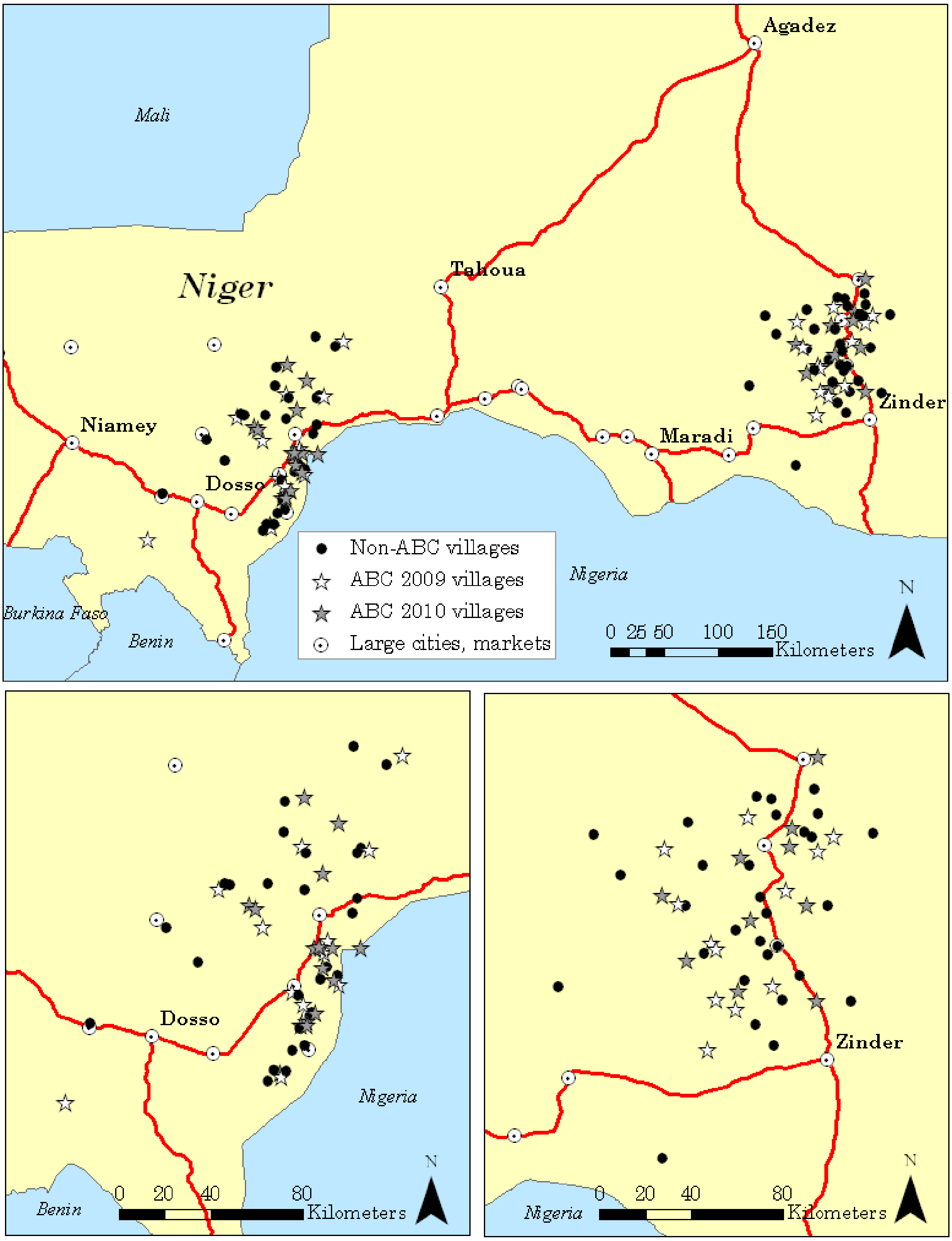
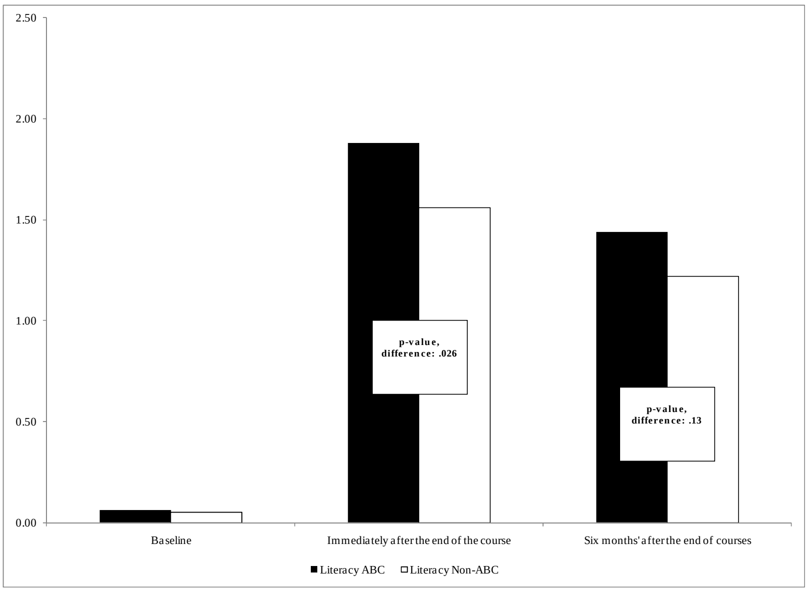


Figure 2. Calendar of Data Collection and Project Activities

	January	February	April	June
2009	Select villages and students. Literacy and numeracy tests for both cohorts. Baseline household survey for both cohorts.	Literacy classes begin for 2009 cohort.	ABC module begins in half of 2009 cohort villages.	Classes end. Literacy and numeracy tests for 2009 cohort (1).
2010	Literacy and numeracy tests for 2009 cohort (2). Second round of household survey for both cohorts	Literacy classes begin for 2009 and 2010 cohorts.	ABC module begins in half of 2010 cohort villages and restarts for the 2009 cohorts.	Classes end. Literacy and numeracy tests for 2009 cohort (3). Literacy and numeracy tests for 2010 cohort (1).

Figure 3. ABC and Non-ABC Test Scores for the 2009 Cohort in January 2009, June 2009 and January 2010

Panel A: Writing Test Scores



B. Math Test Scores

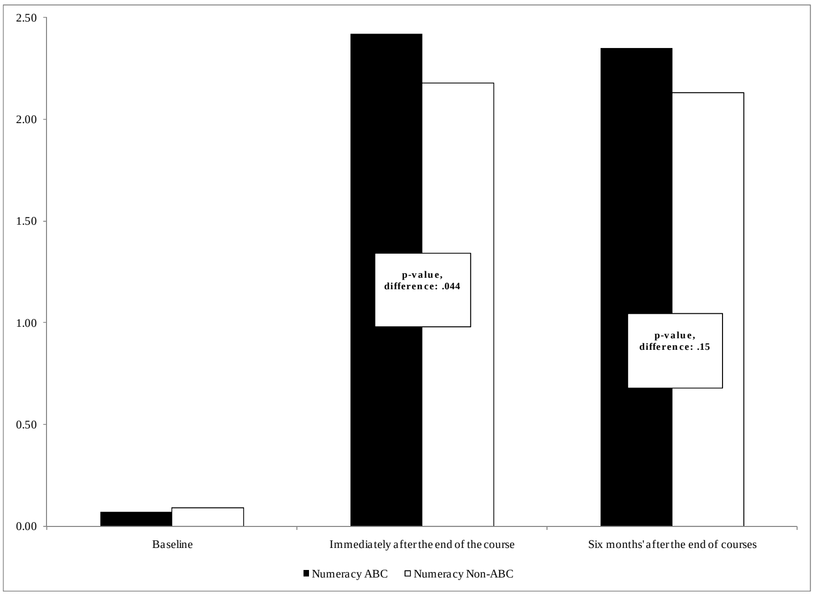
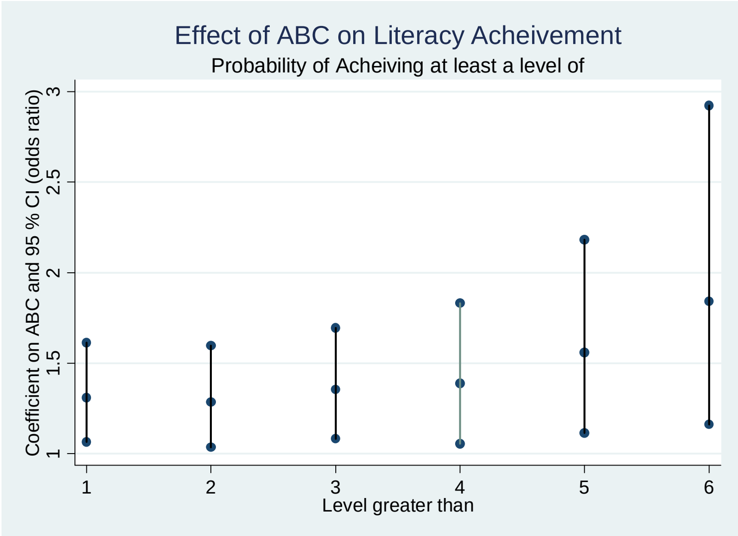


Figure 4. Impact of the ABC Program on the Distribution of Test Scores

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



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

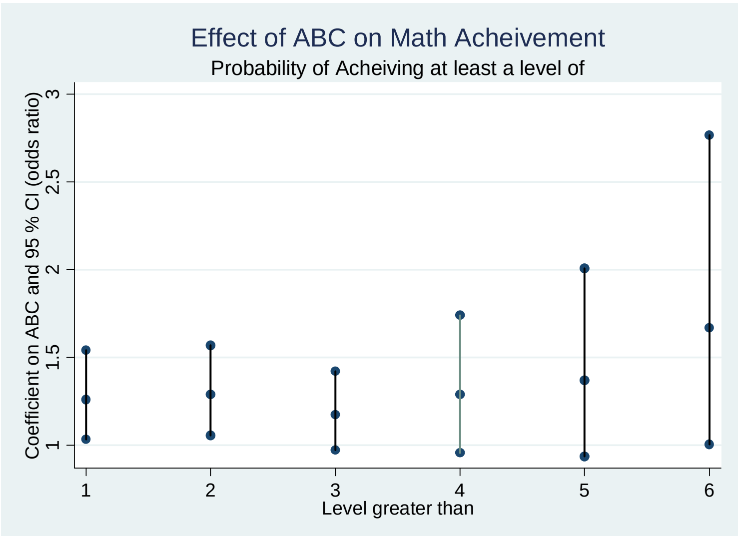
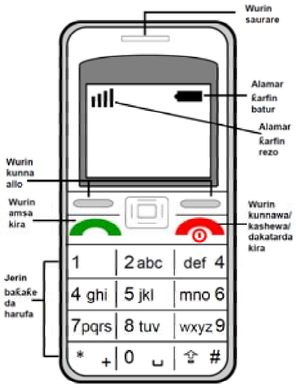


Figure 5. Leaflet for the Call-In Hotline

YAKI DA JAHILCI TAMKAR HANYAR SADARWA CE!

KU NUNA GOYON BAYANKU ZUWA GA KARATUN YAKI DA JAHILCI NA CIKIN GARINKU



Ku yi kira

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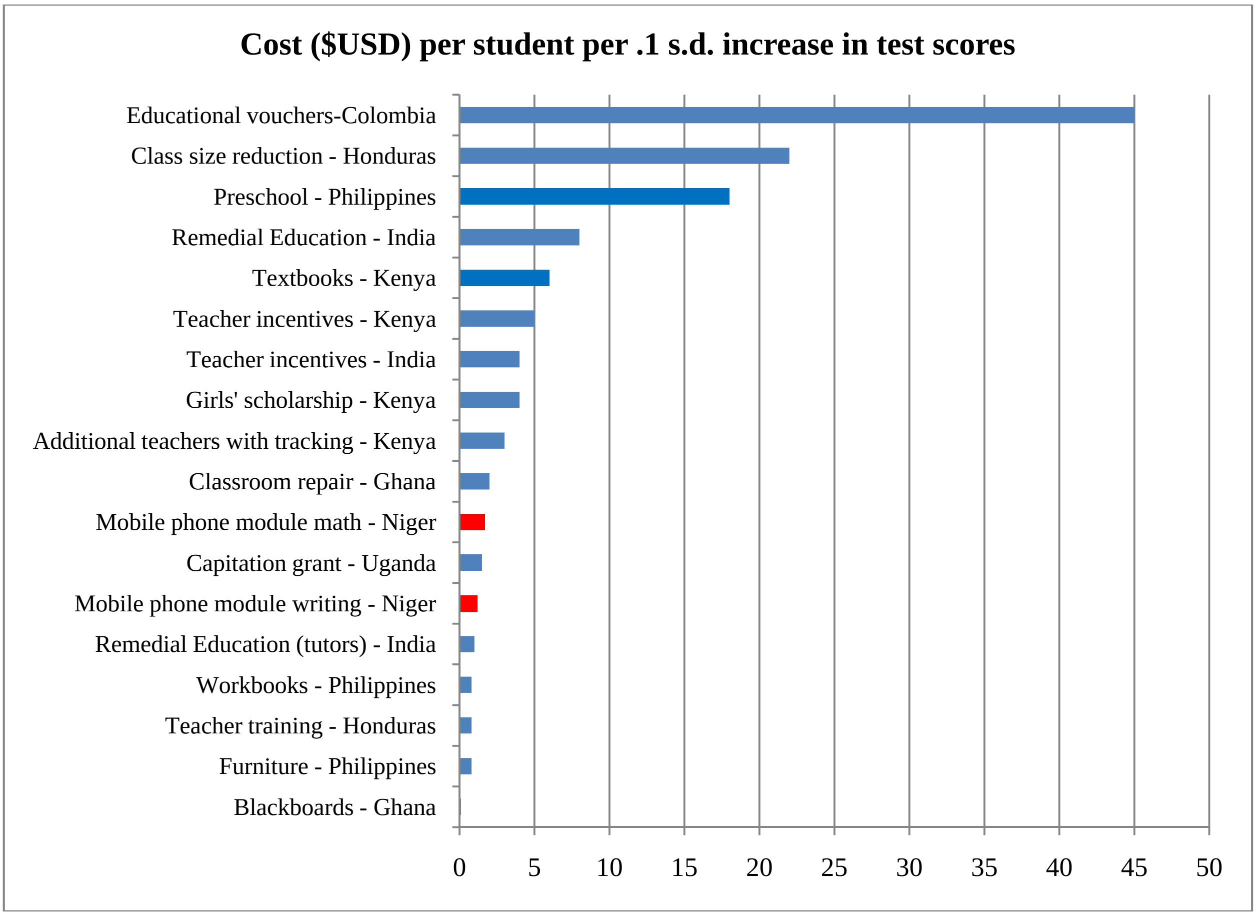
Sunan mai kira - Sunan gari

Misali: Ambarka Gero Lillato

CRS CATHOLIC RELIEF SERVICES

Tufts UNIVERSITY

Figure 6. Cost Effectiveness of the ABC Program



Notes: Data on the cost-effectiveness calculations of other education interventions from Evans and Ghosh (2008). Cost-effectiveness of the ABC program based upon the authors' calculations and data from Catholic Relief Services.

Table 1: Baseline Household Descriptive Statistics (by Treatment Status)			
	ABC Mean	Non-ABC Mean	Diff (s.e.)
Panel A: Pooled Sample			
Age	37.86	37.18	0.69 (.77)
Head of Household (1=Yes, 0=No)	0.56	0.55	0.01 (.03)
Farmer is respondent's main occupation	0.80	0.79	0.01 (.03)
Housewife is respondent's main occupation	0.18	0.19	-0.01 (.02)
Number of household members	8.42	8.33	0.09 (.25)
Percent Children <15 with some primary education	0.10	0.09	0.01 (.01)
Number of asset categories owned	4.97	4.99	-0.01 (.11)
Number of houses owned	3.18	3.12	0.06 (.13)
Own mobile phone (1=Yes, 0=No)	0.30	0.30	0.0 (.03)
Respondent has access to mobile (in HH or village)	0.79	0.76	0.03 (.02)
Used mobile phone since last harvest (1=Yes, 0=No)	0.54	0.57	-0.03 (.03)
Number times used mobile phone since last harvest	6.67	7.26	-0.59 (.47)

Notes: Table displays summary statistics for treatment (Column 1) and control group (Column 2). Column 3 reports the difference. ***, **, * denote statistical significance at the 1, 5, 10 percent levels, respectively.

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

	ABC Mean	Non-ABC Mean	Coeff (s.e.)
Panel A: Pooled Sample			
Baseline writing test score	0.03	0.04	-.01 (.01)
Baseline math test score	0.04	0.06	-.02 (.01)***
5-month writing test score	1.88	1.57	.31 (.05)***
5-month math test score	2.41	2.19	.23 (.04)***
17 month writing test score	2.32	2.11	.21 (.09)**
17 month math test score	3.13	2.78	.35 (.07)***
Panel B: Dosso			
Baseline writing test score	0.03	0.04	0.0 (.01)
Baseline math test score	0.05	0.07	-0.02 (.01)**
5-month writing test score	1.70	1.35	0.35 (.06)***
5-month math test score	2.48	2.13	0.35 (.05)***
17 month writing test score	2.37	1.91	0.46 (.11)***
17 month math test score	3.33	2.77	0.56 (.1)***
Panel C: Zinder			
Baseline writing test score	0.03	0.04	-0.01 (.01)
Baseline math test score	0.03	0.05	-0.02 (.01)**
5-month writing test score	2.10	1.88	0.22 (.08)***
5-month math test score	2.33	2.27	0.06 (.06)
17 month writing test score	2.25	2.40	-0.15 (.13)
17 month math test score	2.89	2.79	0.1 (.09)

Notes: Table displays summary statistics for ABC (Column 1) and non-ABC (Column 2). Column 3 reports the difference. 5-month results are the June 2009 test scores for the 2009 cohort and the June 2010 test scores for the 2010 cohort. 17-month test scores are the June 2010 test scores for the 2009 cohort. Standard errors in parenthesis do not adjust for clustering at the class level. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively. Summary statistics are for respondents with non-missing information

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

Panel A: Writing	**Writing Z-Score**			
	(1)	(2)	(3)	(4)
ABC*Time	0.184**	0.173**	0.184**	0.170**
	(0.0753)	(0.0738)	(0.0756)	(0.0773)
ABC	-0.0292	-0.0241	-0.0485	
	(0.0440)	(0.0467)	(0.0471)	
Time	-0.0938*	-0.131*	-0.135*	-0.127*
	(0.0533)	(0.0713)	(0.0711)	(0.0711)
2010 Cohort		-0.0881**	-0.102**	
		(0.0433)	(0.0394)	
Female		-0.371***	-0.371***	-0.370***
		(0.0420)	(0.0356)	(0.0211)
Dosso		-0.0944**		
		(0.0425)		
Sub-region fixed effects	No	No	Yes	No
Village fixed effects	No	No	No	Yes
Number of observations	13,479	13,479	13,479	13,479
R^2	0.003	0.042	0.066	0.112

Panel B: Math	Math Z-Score			
	(1)	(2)	(3)	(4)
ABC*Time	0.250***	0.241***	0.242***	0.245***
	(0.0742)	(0.0741)	(0.0744)	(0.0768)
ABC	-0.0755	-0.0644	-0.0914*	
	(0.0505)	(0.0469)	(0.0500)	
Time	-0.127**	-0.177**	-0.177**	-0.169**
	(0.0539)	(0.0768)	(0.0767)	(0.0765)
2010 Cohort		-0.136***	-0.140***	
		(0.0478)	(0.0433)	
Female		-0.343***	-0.341***	-0.339***
		(0.0453)	(0.0389)	(0.0222)
Dosso		0.0793*		
		(0.0438)		
Sub-region fixed effects	No	No	Yes	No
Village fixed effects	No	No	No	Yes
Number of observations	13,497	13,497	13,497	13,497
R^2	0.005	0.039	0.066	0.121

Notes: Regressions include data for the 2009 cohort in June 2009 and for the 2010 cohort in June 2010, immediately after the courses. Sub-regional fixed effects control for the level of randomization across cohorts and ABC villages. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively. Robust standard errors clustered at the class level.

Table 4: Impact of the ABC Program by Region, Gender and Age

Panel A: Writing	Writing Z-Score		
	(1)	(2)	(3)
ABC*Time*Dosso	-0.0143		
	(0.0381)		
ABC*Time*Female		0.0781	
		(0.132)	
ABC*Time*Young			0.0882
			(0.0996)
Sub-region fixed effects	Yes	Yes	Yes
Round and Cohort fixed effects	Yes	Yes	Yes
Number of observations	13,479	13,479	12,298
R^2	0.078	0.089	0.091

Panel B: Math	Math Z-Score		
	(1)	(2)	(3)
ABC*Time*Dosso	-0.0513		
	(0.0356)		
ABC*Time*Female		0.0109	
		(0.141)	
ABC*Time*Young			-0.0108
			(0.103)
Sub-region fixed effects	Yes	Yes	Yes
Round and Cohort fixed effects	Yes	Yes	Yes
Number of observations	13,497	13,497	12,317
R^2	0.083	0.085	0.097

Notes: The results are for the 2009 and 2010 cohorts. All regressions include a binary variable for ABC, time and ABC*time. Column (1) includes a binary variable for Dosso and Dosso*time; Column (2) includes a binary variable for "female" and female*time; and Column (3) includes a binary variable for "young" and young*time. "Young" is defined as younger than 45 years of age. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively. Robust standard errors clustered at the class level.

Table 5: Persistent Effects of ABC Program

Panel A: Writing	Writing Z-Scores			
	(1)	(2)	(3)	(4)
ABC	0.203**	0.193**	0.190**	0.184**
	(0.0797)	(0.0789)	(0.0788)	(0.0802)
ABC 7 months later	0.159**	0.150**	0.162**	0.162**
	(0.0751)	(0.0750)	(0.0760)	(0.0789)
Region fixed effects	No	Yes	No	No
Gender fixed effects	No	Yes	Yes	Yes
Cohort fixed effects	No	Yes	Yes	No
Sub-region fixed effects	No	No	Yes	No
Village fixed effects	No	No	No	Yes
Number of observations	14,069	14,069	14,069	14,069
R^2	0.008	0.060	0.093	0.136
Panel B: Math	**Math Z-Scores**			
	(1)	(2)	(3)	(4)
ABC	0.270***	0.260***	0.246***	0.256***
	(0.0863)	(0.0869)	(0.0845)	(0.0855)
ABC 7 months later	0.203**	0.194**	0.195**	0.201**
	(0.0780)	(0.0781)	(0.0784)	(0.0801)
Region fixed effects	No	Yes	No	No
Gender fixed effects	No	Yes	Yes	Yes
Cohort fixed effects	No	Yes	Yes	No
Sub-region fixed effects	No	No	Yes	No
Village fixed effects	No	No	No	Yes
Number of observations	14,112	14,112	14,112	14,112
R^2	0.006	0.077	0.110	0.162

Notes: Results include data collected 8 months after the end of classes for the 2009 and 2010 cohorts. "ABC" denotes test scores immediately after the program, whereas "ABC 7 months' later" denotes test scores seven months after the program. ***, **, * denote statistically significance at 1, 5, 10 percent, respectively. Robust standard errors clustered at the class level.

Table 6: Teacher and Student Characteristics and Attendance

	ABC Mean(s.d.)	Non-ABC Mean(s.d.)	Diff Coeff (s.e.)
Teacher Characteristics (2009)			
Education (number of years)	8.86(1.3)	8.24 (2.3)	.62(.40)
Age	32.09 (6.7)	33.24 (9.6)	1.15(1.80)
Sex (1=Woman, 0=Men)	.33 (.44)	.25 (.47)	.08(.08)
Local (1=Yes, 0=No)	.656(.479)	.775(.420)	-.12(.11)
Teacher Characteristics (2010)			
Education (number of years)	8.38(1.95)	8.34(1.89)	.05(.25)
Age	32.73(8.61)	32.69(8.67)	.03(1.20)
Sex (1=Woman, 0=Men)	.388(.489)	.362(.483)	.03(.05)
Local (1=Yes, 0=No)	.678(.469)	.690(.464)	-.01(.07)
Teacher Attendance (Number of Classes Taught)			
Overall	56.45(16.9)	60.7(11.4)	-4.24(2.72)
Doutchi	58.6(6.5)	63.2(3.2)	-4.57(3.15)
Zinder	53.4(16.9)	57.4(16.3)	-3.99(4.66)
Student Attendance Rate			
Overall	.84(.28)	.81(.23)	.03(.01)***
Doutchi	.86(.23)	.85(.25)	.015(.007)**
Zinder	.82(.25)	.75(.31)	.06(.01)***

Notes: Table displays summary statistics for ABC (Column 1) and non-ABC (Column 2) for 2009 and 2010 program years. Column 3 reports the difference with robust standard errors in parenthesis. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

Table 7. Effect of ABC on Student Interest in Education

Dependent variable:	*Called hotline*		*Number of calls*	
	(1) OLS	(2) Probit	(3) OLS	(4) Poisson
ABC	.228*** (.051)	.241*** (.056)	6.26** (2.72)	.739** (.331)
Region fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Number of observations	139	139	139	139
R^2	0.12		0.13	
Pseudo-R^2		0.09		
Wald chi-squared				32.09
Mean (s.d.) of non-ABC group	.413(.496)	.413(.496)	5.24(15)	5.24(15)

Notes: Data based upon results from the call-in hotline in January-March 2011. The coefficient for the probit regressions is the marginal effect. Robust standard errors clustered at the sub-regional level are in parentheses. *, **, *** denote statistically significant at 10, 5 and 1 percent levels, respectively.