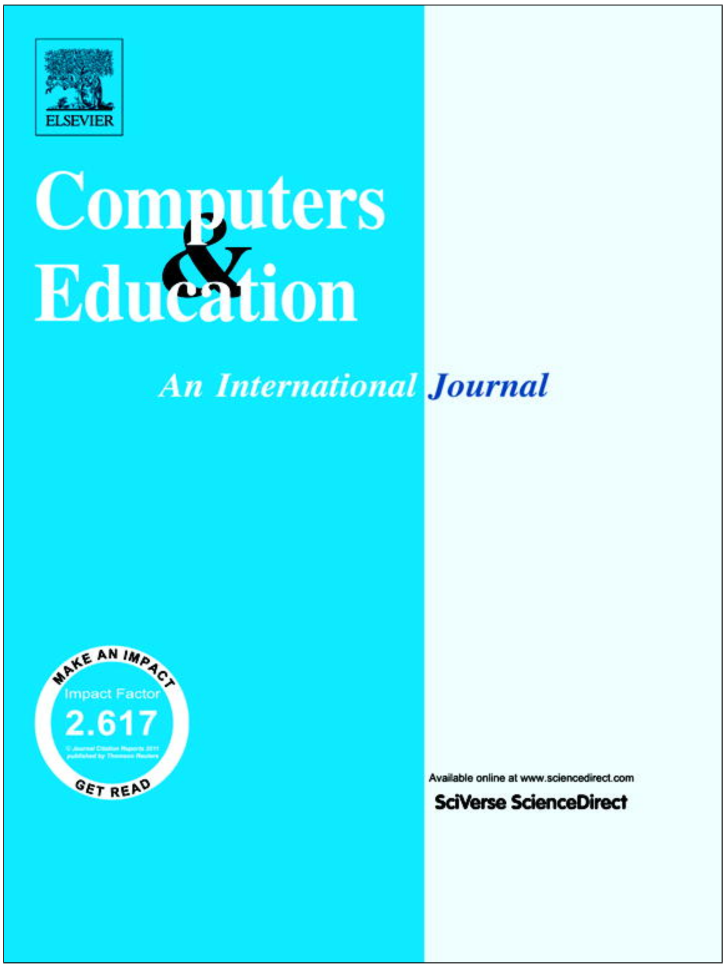

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Computers and the academic performance of elementary school-aged girls in China's poor communities

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A B S T R A C T

Experts agree that computers and computing play an important role in education. Since the 1980s there has been a debate about gender as it relates to computers and education. However, results regarding gender differences concerning computer use in education are not consistent. In particular there is little work done in China on this issue. Therefore, the overall goal of this paper is to demonstrate whether girls and boys can gain equally from computer-based education in China's elementary schools. To do so we analyze results from three randomized field experiments of a Computer Assisted Learning (CAL) program and One Laptop Per Child (OLPC) program. The field experiments are carried out in three kinds of schools: Shannxi rural public schools; Qinghai minority public schools; and Beijing migrant schools. Although CAL and OLPC have been considered cost effective means to improve learning outcomes, it is not known whether the programs impact girls differently than boys. Our analysis shows that, in fact, there were no differences between female and male students in either the improvement in standardized math test scores or Chinese test scores with either the CAL or OLPC programs. Our study suggests that among disadvantaged students in China's rural areas and migrant communities, there is reason to believe that computer based learning can benefit both girls and boys equally. This finding has possible implications for China's ongoing efforts to integrate computers and computing technologies into the nation's underserved schools.

1. Introduction

The rapid diffusion of computers has changed nearly every aspect of contemporary life, from work to education and health (Levine & Donitsa-Schmidt, 1998). As a consequence, today's education systems are charged with teaching children how to use computers as part of the overall education process (Baouendi & Wilson, 1989). In addition (and perhaps more importantly), it is thought that children in school can use computers as an effective learning tool and as a way to improve their overall education (Castro & Alves, 2007). In these ways, the literature suggests that computers are making an ever-increasing impact on many aspects of cognition and learning (Volman, Eck, Heemskerk, & Kuiper, 2005).

Since the 1980s, gender has been the focus of research on computer learning (Tengku Faekah, 2005). According to Kay (1993), users must have a certain level of will and recognize the utility of computers in order to successfully learn how to use them. As a consequence, the attitudes of students toward computing and their computer knowledge and skills are two factors that are closely related to the successful use of computers in education (Woodrow, Mayer-Smith, & Pedretti, 1996). For this reason, researchers have explored the differences between males and female students regarding computer attitudes, knowledge and skills (Janssen & Plomb, 1997).

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Are there differences across genders regarding student computer skills and attitudes toward computers? Insofar as computer skills are concerned, the evidence from existing research is consistent: female (in general) significantly lag behind male when judging their computer skills (Tengku Faekah, 2005).

The literature, however, is more nuanced when examining differences between genders with respect to attitudes toward computers. A large body of research, such as Barba and Mason (1994), Yaghi (1997) and Bovée, Voogt, and Meelissen (2007), shows that male students have more favorable attitudes toward computers. According to Bovée et al. (2007), in general, female students appear to be more anxious with computers and regard the computer as more difficult to deal with than male students. Most female students (in this case, secondary students) feel less confident with the use of computer than their male counterparts. Other studies indicate that there are differences in the ways that male and female students "like" computers or perceive their usefulness (Robertson, Calder, Fung, Jones, & O'Shea, 1995).

In contrast, other research teams have found no significant differences in attitudes toward computers with respect to gender. Hunt and Bohlin (1993) discovered that females and males did not display significant disparities in terms of any of the four types of attitudes toward computers (i.e., liking, comfort, confidence and perceived usefulness). This is true in both developed and developing countries. Bovée et al. (2007) found in a study of 240 students from South Africa that there were no gender differences in the attitudes of boys and girls regarding computers. Neither male nor female students were anxious toward the computers. Both enjoyed working with computers and regarded computers as useful tools for their lives.

In a small minority of studies researchers have found that females have a more positive attitude toward computers than males. For example, according to McGrath and Thurston (1992), female students were found to have more favorable attitude toward computers compared with the male students. Female students also were more interested in computers than male students. Tengku Faekah (2005) also found one case in which female students, more than male students, believed computers would be more useful in their future lives. This literature has raised the question: Does the fact that researchers have detected differences between genders regarding student computer skills and attitudes toward computers have implications for whether boys and girls can gain equally from computer-based education?

Perhaps surprisingly there is little work done in China on this particular issue. This is unexpected because the actions of the government demonstrate that putting computers in the classroom as a way to enhance learning is a priority in the coming years. In the 12th Five Year Plan, for example, the government will spend billions of dollars putting computers in every classroom (IDC, 2011). Computers are one of the key platforms of the Ministry of Education's strategic plan. At the same time there is concern about achieving gender equality in China. As both a millennium goal and a matter of basic national policy, China's leaders are committed to gender equality in education (Guo, 2011). Despite this, Pang, Zeng, and Rozelle (in press) has shown that there is still considerable gender inequality in China—especially regarding educational attainment.

The overall goal of this paper is to demonstrate whether there is any gender differential in terms of how girls are able to learn when using computers in China's elementary schools. In most general terms, we are seeking to answer two broad questions. Are there differences between young boys and young girls regarding student computer skills and attitudes toward computers? If so, do these differences have implications for whether boys and girls can gain equally from computer-based education?

To meet this goal and answer these questions, we draw on the results of three randomized controlled trials. Although the three studies were implemented in three different environments—migrant communities outside of Beijing; poor rural mountainous region in Shaanxi Province that is populated mostly by China's ethnic majority Han; and a poor region in Qinghai province that is populated mostly by non-Han ethnic minorities—in each place the intervention was nearly the same. Students in half of the study schools (the treatment schools) participated in a Computer Assisted Learning program (or CAL, for short) while students in the remaining half (the control schools) did not. Students in both treatment and control schools were given standardized math and/or Chinese language tests before and after the intervention. In this paper we look at how third, fourth and fifth grade female students performed in the treatment and control schools by looking at changes in standardized test scores between the baseline and endline surveys and compare their performance to the performance of their male students classmates in each grade.

We also examine gender differentials in a fourth experiment which randomly assigned laptop computers to 150 students and did not give anything to a set of 150 control students.

2. Sampling, intervention, data and methods

In this section we do four things. First, we briefly describe the sampling process and the final sample that is used in each of the three RCTs. This subsection is purposely because since there are detailed appendices describing the sampling for each of the three RCTs. The second subsection describes the intervention that was implemented in each of the three study locations. The third subsection describes the data collection. Although we report on findings from three different study sites, because the interventions and data collection were essentially the same, the description in these subsections is simplified. In the fourth subsection we review the common statistical approach that was used to analyze the data from all three studies.

2.1. Sampling and randomization

2.1.1. Sample in Beijing migrant communities (study 1)

The sample in China's migrant communities is located in the suburbs of Beijing. Of Beijing's 16 districts and two counties, we chose the three that had the largest number of migrant workers and their families (Lai, Luo, Zhang, Huang, & Rozelle, 2011). There are more than 200 private migrant elementary schools in these districts. Of the total number of schools in the three Beijing districts, 43 of the schools were large enough to have two classes of grade 3 students. Using the 43 schools as the sampling frame, we chose 24 schools to be in our sample. There were 54 classes of grade 3 students in the 24 schools.

Using this set of schools, we conducted a clustered (at the class level) RCT of a Computer-Assisted Learning program (or CAL—see the next subsection for a detailed explanation of the intervention) in the fall semester of 2010. Because of a limit on the number of computers available, we only implemented the program in one class in each school. The other classes in the sample schools were the control classes. Assignment to the treatment group was performed randomly.

In total there were 2514 students in the sample schools. Of the total number of students, 902 students were in the treatment classes and 1255 students were in the control classes. In Beijing migrant schools 45.5 percent of the students were female. In the treatment schools 46 percent of the students were females; in the control classes, 45 percent were females. This rate of female students (i.e., less than 50 percent) is common in China's schools (Maslak, Kim, & McLoughlin, 2010), because of the sex ratio imbalance that exists in most rural areas and migrant communities (Hannum, Kong, & Zhang, 2009).

For a complete, more detailed description of the sampling process and the sample in the Beijing migrant schools, see Appendix A. In this appendix it is shown the sample is mostly balanced between the treatment and control classes at the baseline. The appendix also describes the attrition that occurred in the sample between the baseline and endline and demonstrates that the characteristics of those that attrited (in the treatment and control classes) are unrelated to the status of the classes—either treatment or control.

2.1.2. Sample in Shaanxi Province (study 2)

The second RCT was carried out in a predominantly Han rural area, located in Ankang Prefecture in Shaanxi Province, a poor province in China's northwest. We randomly chose four of Ankang's nine poor counties. There are 72 elementary schools in these four poor counties. After excluding the schools in the county seat, we chose all 72 schools to be part of the RCT.

Using this set of schools, we conducted a clustered (at the school level) RCT of CAL in the spring semester of 2011. Because of a limit on the number of computers available, we only implemented the program in grades three and five. A total of 5942 grade three and five students were involved in our study. Among the total number of participants, 2613 students were boarding students (that is, for five days each week they ate all of their meals at school and slept in the dormitory at night). The main sample for our study (that is, the main set of treatment students and the main set of control students) was made up of boarding school students. We used boarding school students so the CAL intervention could be implemented after school and before bed time. In this way, the CAL program did not interfere with any class time. Girls accounted for 46 percent of the overall sample. This means that of the 5943 students in the sample, 2733 were girls. Of the 2613 boarding school students, 1218 were girls.

After choosing our sample, we randomly assigned 36 schools to be the treatment schools (and the boarding school students in the treatment schools participated in the CAL sessions). At the same time, 36 schools were assigned as control schools. In the treatment schools, 47 percent of the boarding school students were girls. In the control schools, 46 percent of the boarding school students were girls.

For a complete, more detailed description of the sampling process and the sample of the Shaanxi rural sample, see Appendix B. In the appendix it is shown the sample is mostly balanced between the treatment and control classes at the baseline. The appendix also describes the attrition that occurred in the sample between the baseline and endline and demonstrates that the characteristics of those that attrited (in the treatment and control classes) are unrelated to the status of the classes—either treatment or control.

2.1.3. Sample in Qinghai Province (study 3)

The third RCT was carried out in an ethnic minority-dominated area, located in Haidong Prefecture in Qinghai Province. Qinghai is a poor province in China's northwestern region. Of Haidong's six poor counties, we randomly chose three counties, Xunhua, Hualong and Huzhu Counties, to be in our sample. Each of these counties is primarily populated by four ethnic minorities (Tibetans, Hui, Tu and Salar).

Because the project focused on teaching third and fourth-grade students Chinese language, schools in Tibetan communities were excluded because the level of their Chinese language skills were so poor that the students were unable to use the CAL software. Of the more than 80 rural minority schools in the Hui, Tu and Salar communities, we randomly selected 57 ethnic minority elementary schools as our final sample schools. To implement the RCT, 26 schools were randomly assigned as treatment schools; the remaining 31 schools were assigned as control schools.

Using this set of schools, we conducted a clustered (at the school level) RCT in the spring semester of 2011. Because of a limit on the number of computers, we only implemented the program in grade three and grade four. A total of 1717 third and fourth grade students were involved in our study. Among the total number of participants, 680 students were in the treatment schools and 1037 students were in the control schools. Assignment to the treatment schools was done randomly. In the treatment schools 54 percent of the students were females; in the control schools, 51 percent were females. On average female students accounted for 52 percent of overall sample.

For a complete, more detailed description of the sampling process and the sample, see Appendix C. In this appendix it is shown the sample is mostly balanced between the treatment and control classes at the baseline. The appendix also describes the attrition that occurred in the sample between the baseline and endline surveys and demonstrates that the characteristics of those that attrited (in the treatment and control classes) are unrelated to the status of the classes—either treatment or control.

2.2. Experiment arms/interventions

The main intervention involved computer-assisted math (in Beijing and Shaanxi) or Chinese language (in Qinghai) remedial tutoring sessions which were designed to complement the regular in-class math/Chinese language curriculum during the semester-long CAL program. Under the supervision of two teacher-supervisors trained by our research group, the students in the treatment group had two 40-min CAL sessions per week either during lunch breaks or after school. The sessions were mandatory and attendance was taken by the teacher-supervisors. The content (instructional videos and games) of each session was exactly the same for all students in the treatment group and emphasized basic competencies in the uniform national math or Chinese language curriculum.

During each session, two students shared one computer and played math/Chinese language games designed to help students review and practice the basic material that was being taught in their regular school classes. If a student had a math- or Chinese language-related question, he/she was encouraged to discuss with his/her teammate (the one with whom he/she shared the computer). The students were not supposed to consult the other teams or the teacher-supervisor. According to our protocol, the teachers were only allowed to help students with scheduling, computer hardware issues and software operations.

Our research team took great care in preparing the necessary hardware, software, CAL curriculum and program implementation protocol in a way that would both facilitate smooth implementation of the CAL program and prevent confounding influences that might bias our results. To meet the hardware requirements of the CAL program, we acquired (by way of donation) brand new identical computers. Our CAL software package was installed on these machines. We then removed all pre-installed software that would not be used during the CAL intervention. We also disabled the Internet and USB functions on all of the computers. By doing so, we were able to prevent school teachers or other students from using the program computers for other purposes that might affect the operation of the regular CAL program. It also was done so that our evaluation of the program effects would not be capturing any other confounding influences (spillovers) if students were able to learn from (or be distracted by) other sources of information (that might be accessed by the Internet). This also avoided the situation that might occur if teachers/students from the control classes were able to copy our CAL software onto other computers.

In choosing the math or Chinese language questions to include in the CAL software, we consulted experienced elementary school math and Chinese language teachers in both urban and rural public schools, as well as committee members of the Center for Examination of Beijing, an institute that designs uniform tests for elementary schools in Beijing. By combining the commercial software and the CAL software, we had enough content and exercise games to cover the math/Chinese language course materials for the entire period of time in which CAL was implemented. The material was sufficient to provide 80 min of remedial tutoring per week (2 sessions per week times 40 min per session).

With both software and hardware ready, we then designed a detailed implementation protocol. The implementation protocol was written up for use in a manual for the students in the different CAL grade levels. Each manual, which was given to the teacher-supervisor as a bound, printed-out booklet, contained detailed instructions. The manual contained four main sections: a.) the detailed CAL curriculum; b.) CAL classroom rules for both students and teacher-supervisors; c.) the responsibilities of the teacher-supervisors when supervising the CAL sessions (what to do and what not do to); and d.) tutorials (in both words and graphics) on basic computer operations, CAL software use, and troubleshooting.

To ensure that the protocol would be properly implemented, we requested that each school assign two teachers to supervise all of the CAL sessions according to the protocol. The teacher-supervisors' five main responsibilities included: a.) taking attendance; b.) making sure that the CAL curriculum in each session was matched to the curriculum being taught in the students' math/Chinese class; c.) managing the CAL classrooms so that order was maintained; d.) providing immediate assistance when students experienced difficulty in computer and/or math/Chinese game software operations (but they were not supposed to instruct the students in math); and e.) taking care of the CAL computers and keeping close contact with our research group/volunteers regarding technical support or CAL management questions. Because this work was clearly beyond the scope of their normal classroom duties, we compensated the teacher-supervisors with a monthly stipend of 400 yuan (approximately 60 USD).

To further ensure that the teacher-supervisors (and the students under their supervision) strictly followed the protocol, we recruited volunteers from universities in Beijing, Xi'an and Xining and directed them to pay a visit to the program schools during the implementation of CAL. During the visit, the volunteers were instructed to attend the CAL sessions and observe whether the protocol was being strictly implemented. The volunteers did not provide notice to the schools before their visits. They also were instructed to avoid all unnecessary interactions with students and teachers so that they would not interrupt the sessions or provide additional assistance to CAL session management, which might confound the program effect.

Finally, we also provided technical support and free computer repairs and maintenance for the entire semester. We offered what we called a "24/7 consultation hotline" to answer all CAL-related questions, ranging from computer and CAL software operations to classroom management. In addition to monitoring the CAL sessions, our program volunteers also conducted basic on-site computer maintenance during their monthly school visits.

2.2.1. CAL control groups

Students that were assigned to the control group in each of the Beijing, Shaanxi and Qinghai samples did not receive any CAL intervention. The control schools in Shaanxi and Qinghai were fundamentally "blind" to the study (that is, they did not know that there was a study and during interviews at the time of the endline visit, only a small fraction said that they had even heard of the CAL programs in other schools in their county; none had ever visited a CAL school). In the Beijing schools, the third-grade students and their teachers in the 30 control classes did know that another class in their schools was part of the CAL program. This possibly might cause a Hawthorne Effect for the treatment group students and control group students. If the Hawthorne Effect was strong enough, it might affect the relative performance of the two groups and confound the estimates of the CAL intervention effect. However, according to an analysis by Lai et al. (2011), there is no evidence that the Hawthorne effect accounted for the measured CAL impacts (see Lai et al., 2011 paper for more details).

2.3. Data collection

The research group conducted two rounds of surveys in the three RCT sample control and treatment schools/classes. The first-round survey in each of the sites was a baseline survey before any implementation of the CAL program had begun. The second-round survey was a final evaluation survey conducted at the end of the CAL program that coincided with the end of the semester.

In each round of the survey, the enumeration teams visited each school and conducted a two-block survey. In the first block students were given a standardized math test and a standardized Chinese test. The math test included 29–31 questions. The Chinese test included 27–42 questions. Students were required to finish the test in each subject in 25 min. All students took the math test first and then they took the Chinese test. Our enumeration team monitored the test and strictly enforced the time limits and tried to make sure there was no cheating. We use the scores of the students on the math and Chinese tests as our measures of student academic performance.

In the second block enumerators collected data on the characteristics of students and their families. From this part of the survey we are able to create demographic and socioeconomic variables. The dataset includes measures of each student's *age* (measured in years), gender, grade, county, whether one is the only child of his or her family, father's education level (*father has at least high school degree*), mother's education level (*mother has at least high school degree*), whether their parents are still farmers or work off the farm (*family off-farm*) and poverty status (*whether one receives a poverty subsidy at school*). To create indicators of parental care, during the survey the students were also asked whether their parents migrated to some other place or whether they stayed at home for most of the time during the semester (*both parents at home*).

Importantly, during the endline survey the teams spent time matching survey forms and creating lists of those students that were present during both the baseline and endline surveys. The students that were present during both surveys become part of the sample that is used in assessing the impact of CAL on standardized test scores. We also created a variable called *attrit*. Regardless of the reason for not completing the endline survey (after successfully completing the baseline survey), the attrit variable equals one if a student took the baseline survey but did not take the endline survey. The variable equals zero if the student completed both surveys. Students that did not complete a baseline were not included in the analysis.

2.4. Statistical methods

The first step in our analysis is to examine the impact of CAL on standardized test scores of the students in general (regardless of gender). We used both unadjusted and adjusted ordinary least squares (OLS) regression analyses to estimate how the standardized test score outcomes changed in the treatment group relative to the control group. Our unadjusted analysis regressed changes in the outcome variables (i.e., post-program outcome value minus pre-program outcome value) on a dummy variable of the treatment (CAL intervention) status. We used adjusted analyses as well to control for some systematic differences between the treatment and control groups, improve precision and test for heterogeneous treatment effects (we will describe these approaches in detail in the models below). In all regressions, we accounted for the clustered nature of our sample by constructing Huber-White standard errors corrected for class-level/school-level clustering (relaxing the assumption that disturbance terms are independent and identically distributed within classes/schools). The unadjusted model is:

$$\Delta y_{isc} = \alpha + \beta \cdot treatment_c + \varepsilon_{isc} \tag{1}$$

where is the change in the standardized test score during the program period for child *i* in school *s* (and class *c* in the case of Beijing), is a dummy variable for a student attending a class/school assigned to the treatment group (equal to one for students in the treatment group and zero otherwise), and is a random disturbance term (clustered at the class/school level). By construction, the coefficient of the dummy variable, is equal to the unconditional difference in the change in the outcome (Δy_{isc}) between the treatment and control groups over the program period.

To improve the efficiency of the estimation, we built on the adjusted model in Eq. (1) by including a set of control variables:

$$\Delta y_{isc} = \alpha + \beta \cdot treatment_c + \theta \cdot y_{0isc} + X_{isc}\gamma + \varepsilon_{isc} \tag{2}$$

where all the variables and parameters are the same as those in Eq. (1), except that we add a set of control variables. Specifically, we controlled for, the pre-program outcome value for student *i* in school *s* and class *c*, and, a vector of additional control variables. The variables in are student *and* family characteristics (*female*, *ethnicity*, *grade*, *county*, *only-child family*, *father's educational level*, *mother's educational level*, *family off-farm*, *both parents at home*, *poverty subsidies*, *ever used a computer*, *and access to other modern technology*). By including and as control variables, in Eq. (2) is an unbiased, efficient estimate of the CAL treatment effect.

In order to examine the main question of this paper, whether young girls learn as well from computer-based education as young boys, we build on the models in Eqs. (1) and (2) and create models to examine heterogeneous effects of gender. To do so, we create a new interaction term by multiplying the treatment variable and the female variable. The new variable, * *female*, is then added to Eqs. (1) and (2) and the sum of the coefficients on the and * *female* measures the impact of CAL on young female students.

3. Results

In this section we will do two things. First, we review the results of the previous CAL studies and demonstrate the effectiveness of the CAL intervention in three different schooling environments. Second, we will turn to the main questions of this paper: Do young female students perform as well as young male students when they participate in CAL programs?

3.1. Computer Assisted Learning and learning outcomes

The effectiveness of CAL in the schools of primary-aged students from communities of vulnerable populations—migrant communities outside of Beijing; poor rural mountainous regions in Shaanxi Province that are populated by ethnic Han; and poor regions in Qinghai province that are populated mostly by non-Han ethnic minorities—can be seen by examining both the intent to treat analysis and the multivariate regression analysis. In each place the intervention was nearly the same. Students in half of the study schools (treatment schools) were provided Computer Assisted Learning (or CAL, for short); the other half (control schools) was given nothing. Students in both treatment and control schools were given standardized math and/or Chinese language tests before and after the intervention.

Table 1
Ordinary least squares estimators of the impact of the CAL program on male/female math academic outcomes in Beijing migrant Schools.[a]

Dependent Variable: standardized post-CAL math test score-standardized baseline math test score	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treatment (1 = the treatment group; 0 = the control group)	0.11***[b] [0.04][c]	0.11 [0.08]	0.11** [0.05]	0.09 [0.06]	0.14*** [0.04]	0.12** [0.05]
(2) Treatment*female		0.01 [0.09]		0.04 [0.09]		0.05 [0.07]
(3) Female		0.03 [0.06]		0.02 [0.06]		−0.07* [0.04]
Control set[d]	No	No	No	No	Yes	Yes
School dummy variables	No	No	Yes	Yes	Yes	Yes
Observations	2157	2157	2157	2157	2157	2157
R-squared	0.004	0.004	0.05	0.05	0.31	0.31

[a] Source: Authors' survey.
[b] *Significant at 10%, ** significant at 5%, ***significant at 1%.
[c] Robust standard errors are in brackets, clustered at the class level.
[d] The control variable set includes student's baseline math score, baseline Chinese score, age, gender, ever used a computer, access to other modern technologies, whether living with father, whether living with mother, number of siblings and parents' educational level.

Table 2
Ordinary least squares estimators of the impact of the CAL program on male/female math academic outcomes in Shaanxi rural public schools.[a]

Dependent variable: standardized post-CAL test score - standardized baseline test score	(1)	(2)	(3)	(4)
(1) Treatment (1 = the treatment group; 0 = the control group)	0.12**[b] [0.06][c]	0.12** [0.05]	0.11* [0.07]	0.10* [0.06]
(2) Treatment*female			0.02 [0.07]	0.04 [0.06]
(3) Female		−0.12*** [0.03]	−0.00 [0.04]	−0.13*** [0.04]
(4) Control set[d]	No	Yes	No	Yes
(5) Observations	2613	2613	2613	2613
R-squared	0.01	0.26	0.01	0.26

[a] Source: Authors' survey.
[b] *Significant at 10%; ** significant at 5%; *** significant at 1%.
[c] Robust standard errors in brackets; standard errors are clustered at the class level.
[d] The control variable set includes the student baseline math score, baseline Chinese score, ethnicity, grade, gender, ever used a computer (yes = 1, no = 0), access to other modern technologies (yes = 1, no = 0), parent's educational level, family off-farm (yes = 1, no = 0), poverty subsidy (yes = 1, no = 0), only-child family (yes = 1, no = 0), both parents at home (yes = 1, no = 0), and county dummy variables.

According to the descriptive, intent-to-treat analysis, the scores of the students in all three sets of the CAL treatment schools rose by more than the scores of the students in the respective control schools. The increase in scores of students in the CAL Beijing migrant treatment schools rose by 0.11 standard deviations more than the students in the control schools. The increase of CAL treatment students in the rural Shaanxi schools was 0.12 standard deviations more relative to students in control schools. The increase was 0.14 standard deviations for students in CAL treatment schools in the Qinghai rural minority communities. In all cases, the rise in the scores of the students that received the CAL treatment was statistically significantly higher than those of the students in the control schools.

The impact is even more pronounced when looking at the results of the (more efficient) regression analysis (Table 1, row 1, column 5; Tables 2 and 3, row 1, column 2). In the case of all three sets of schools the rise in standardized test scores from the CAL treatment (relative to the control schools) ranges from 0.12 standard deviations to 0.19 standard deviations (0.14 standard deviations in Beijing migrant schools (Table 1, row 1, column 5); 0.12 standard deviations third graders and fifth graders in Shaanxi (Table 2, row 1, column 2); and 0.19 standard deviations in Qinghai (Table 3, row 1, column 2)). The levels of statistical significance also became more pronounced.

3.2. *Do young girls learn from computers?*

3.2.1. *Beijing migrant community schools*

The first step to assessing if young girls learned as well from CAL as young boys is to make sure that the starting math test scores of young girls and young boys were the same before the analysis. As can be seen from the data from the Beijing migrant schools, there was no gender difference in baseline math test scores (Table 4). In the treatment group at the baseline, the average standardized scores of boys (0.02) were the same as those for girls (0.02—row 1, columns 1 and 2). The *p*-value (0.902) shows that the difference was not significantly different from zero (row 1, column 3). The scores of boys and girls were also statistically the same in the control group during the baseline (row 1, columns 4–6).

Although the literature is not clear about whether young girls learn as much as young boys from computers, our data from Beijing migrant schools demonstrates that the rise in the scores of young girls matched those of young boys. In the intent to treatment analysis we see that the endline test scores were higher for both boys and girls in the CAL treatment schools (0.06–0.11) relative to those of boys and girls in the control schools (−0.07 to −0.05—Table 4, row 2; columns 1 and 2 and columns 4 and 5). Most importantly there was no statistical difference in the level of the endline scores of the young boys in the CAL treatment schools when compared to young girls (*p*-value = 0.439—row 2; column 3). In fact, when looking at both the baseline and endline scores together, we also find that the point estimate of the rise of the scores of the young girls in the treatment schools (0.11–0.02 = 0.09) versus the rise of the scores of the girls in the control schools (−0.05 − (−0.02) = −0.03), or 0.12 (that is: 0.09 − (0.03)) is higher than the relative rise of scores of boys in the treatment schools (0.06–0.02 = 0.04) relative to boys in the control schools (−0.07 − (−0.01) = −0.06), or 0.10 (0.04 − (−0.06)). Hence, because the difference in difference is insignificant in a statistical sense (and, in fact, the point estimates favor young girls), we can conclude from looking at the gender differences in the impact of the CAL treatment in the Beijing migrant schools is that young girls do not do worse than young boys.

Table 3
Ordinary least squares estimators of the impact of the CAL program on male/female Chinese academic outcomes in Qinghai minority public schools[a]..

Dependent variable: standardized post-CAL test score – standardized baseline test score	(1)	(2)	(3)	(4)
(1) Treatment (1 = the treatment group; 0 = the control group)	0.14*[b] [0.08][c]	0.19*** [0.07]	0.13 [0.09]	0.19** [0.07]
(2) Treatment*female			−0.03 [0.08]	−0.00 [0.06]
(3) Female		−0.12*** [0.03]	0.11** [0.05]	−0.12*** [0.04]
(4) control set[d]	No	Yes	No	Yes
(5) Observations	1717	1717	1717	1717
R-squared	0.01	0.24	0.01	0.24

[a] Source: Authors' survey.
[b] *Significant at 10%; ** significant at 5%; *** significant at 1%.
[c] Robust standard errors in brackets; standard errors are clustered at the school level.
[d] The control variable set includes the student baseline math score, baseline Chinese score, ethnicity, gender, ever used a computer (yes = 1, no = 0), access to other modern technologies (yes = 1, no = 0), parent's educational level, family off-farm (yes = 1, no = 0), only-child family (yes = 1, no = 0), both parents at home (yes = 1, no = 0), and county dummy variables.

Table 4
Difference in math test score between male and female in treatment group and control group in Beijing migrant schools.

Math score	Treatment group			Control group		
	Male	Female	*P*-value	Male	Female	*P*-value
Baseline	0.02	0.02	0.902	−0.01	−0.02	0.916
Endline	0.06	0.11	0.439	−0.07	−0.05	0.612
Did	0.04	0.09	0.422	−0.06	−0.03	0.53

We draw the same conclusion when we carry out the regression analysis for the improvement in standardized math test scores (by examining the magnitude and statistical significance of the coefficient of the interaction variable, Treatment*Female—Table 1, row 2). Regardless of the model that we use (with or without controls), the sign of the coefficient on the Treatment*Female is positive. Hence, the point estimates (ranging from 0.01 to 0.05) suggests that, if anything, young girls are doing better than young boys from the CAL treatment (row 2; columns 2, 4 and 6). However, in all cases, the standard errors are large relative to the magnitude of the coefficient. In other words, we conclude that young girls in the Beijing migrant schools learned as much as young boys.

3.2.2. Shannxi rural public schools

As in the case of the Beijing migrant community schools, the data from Shannxi rural public schools also demonstrate that during the baseline, there was no gender difference in baseline math test scores in either the treatment or the control schools (Table 5). The test scores of young boys (0.11) and young girls (−0.06) in the treatment schools were the same statistically (the *p*-value was 0.24). The same was true in the control schools. Hence, it is valid to continue with the analysis of the relative effect of CAL on young boys and girls.

According to the descriptive analysis (just as in the case of Beijing migrant communities), the test scores of young boys (girls) in the Shaanxi rural treatment schools rose by $-0.04-(-0.06)=0.02$ ($0.08-(-0.06)=0.14$) more than young boys (girls) in the Shaanxi rural control schools (Table 5, rows 2 and 3). There is no statistical difference in the relative rise of boys compared to girls (row 4, columns 3 and 6). In other words, young girls appear to be learning as much as young boys from the CAL intervention.

The regression analysis supports the descriptive analysis in the Shaanxi rural treatment and control schools (and also is consistent with the findings of the multivariate analysis in the Beijing migrant community sample—Table 2). Regardless of the specification that we use (with and without control variables), the analysis demonstrates that there is no difference between young boys and young girls. The coefficients of the Treatment*Female variable are all statistically insignificant from zero (row 2). Hence, there is no reason to suspect that young girls are not learning as much from the CAL program as young boys (and, in fact, the positive point estimates—in all models—means that there is possibly a slight tendency for young girls to learn more—though it is important to stress the size of the standard errors are large relative to the magnitude of the coefficients).

3.2.3. Qinghai minority rural schools

Although the starting Chinese language scores for young boys and young girls differed in the Qinghai schools in the minority communities during the baseline in both the treatment and control schools (in fact, young boys were scoring higher than young girls during the baseline in both treatment and control schools—Table 6, row 1), the intent to treat analysis shows higher rises (in this case) in Chinese language scores for both young boys and young girls in Qinghai minority in CAL treatment schools compared to young boys and young girls in control schools. Specifically, between the baseline and endline surveys young boys and young girls in the CAL treatment schools saw their scores rise by 0.15 and 0.12 standard deviations relative to young boy and young girls in the control schools (for young boys: $0.04-(-0.11)=0.15$; and for young girls: $0.13-(-0.00)=0.13$—Table 6, row 3), respectively. There is no statistically significant difference in these two figures. This finding from the intent to treat analysis in Qinghai, of course, supports the intent to treat findings from the Beijing migrant schools and the Shaanxi rural schools. Young girls are learning from computers at the same rates as young boys.

The multiple regression analysis also supports this finding. Focusing on the full model (the model in which we control for all control variables—Table 3, column 4), we find that the impact of the treatment is positive and significant (the coefficient is 0.19 and is significant at the five percent level of significance—column 4; row 1). The insignificant coefficient on the interaction variable (Treatment*Female) means that young girls, as well as boys, learned equally well from CAL.

3.3. Attrition

One explanation of our results is, in fact, young boys learn better from computers than young girls, but, that this result is being disguised by different rates of attrition of young boys and young girls. Specifically, if young girls, especially those that were not learning from the computers, were missing the CAL sessions at higher rates than young boys, it is possible that the absence of findings showing that the young girls were not learning from CAL is being covered up by an attrition bias.

Our data suggest in all three of our study locations—Beijing migrant communities; Shaanxi rural communities; and Qinghai minority communities—that attrition patterns are not driving our results. Table 7 summarizes the findings of the attrition analysis for the three

Table 5
Difference in math test score between male and female in treatment group and control group in Shannxi rural public schools.

Math score	Treatment group			Control group		
	Male	Female	*P*-value	Male	Female	*P*-value
Baseline	0.11	−0.06	0.24	0.06	−0.03	0.0911
Endline	0.07	0.02	0.42	0.00	−0.09	0.100
Did	−0.04	0.08	0.653	−0.06	−0.06	0.996

Table 6
Difference in Chinese test score between male and female in treatment group and control group in Qinghai minority public schools.

Math score	Treatment group			Control group		
	Male	Female	*P*-value	Male	Female	*P*-value
Baseline	0.22	−0.19	0.000	0.19	−0.19	0.000
Endline	0.26	−0.06	0.000	0.08	−0.19	0.000
Did	0.04	0.13	0.222	−0.11	−0.00	0.0038

Table 7
Difference in attrition rates between young boys and young girls in Beijing migrant schools, Shaanxi rural schools and Qinghai minority schools (units: percentage).

School	Attrition rate	Male	Female	Difference
Beijing migant schools	Treatment group	6.50	4.60	−1.900
	Control group	7.60	4.80	−2.800
Shannxi rural public schools	Treatment group	4.80	3.40	−1.40
	Control group	2.30	1.10	−1.20
Qinghai minority public schools	Treatment group	12.70	9.43	−3.27
	Control group	8.26	7.39	−0.87

studies. On average, only between 2.3 and 12.7 percent of young boys and 1.1 and 9.4 percent of young girls attrited from the study between the baseline and endline. These low numbers give the first evidence that attrition is not affecting our results. Even if they do affect our results, as young girls attrited at lower rates than young boys in all three cases, it could not be the case that a CAL effect in favor of boys was obscured by an attrition bias (i.e., young girls, especially those who were not learning from the computers were missing the CAL sessions at higher rates than young boys).

4. Learning on one's own laptop: do young girls learn as well as young boys?

It is quite clear from the analyses in the previous section that when young girls are exposed to Computer Assisted Learning in a school setting (in any one of many different school settings), young girls learn as well as young boys. However, this leaves open the question of whether a similar outcome is possible if the computers were available at home (instead of school).

In this section we examine the gender impacts of a One Laptop per Child (OLPC) project that we conducted ourselves in 2011 (see Mo et al., 2011). OLPC is a high profile initiative (implemented in many countries around the world) that attempts to narrow the inequality of access to technology and improve educational performance between the rich and poor (Buchele & Owusu-Aning, 2007). We implemented an OLPC project in China—as a randomized controlled trial—starting in December of 2010 and ending in the fall of 2011. The study involved 300 third grade students in 13 migrant schools in Beijing. In the study, after conducting a baseline survey, we randomized across individuals (not classes or schools). Using the results from the baseline survey, we divided the students into two groups, a treatment group and a control group, and because we prebalanced, the average math test scores of the students in each of the groups were statistically the same. We then gave (permanently) half of the students (the 150 treatment students) a laptop that included a number of preinstalled pieces of learning software. The other half (the 150 control students) did not receive a laptop (or anything else).

The results from our OLPC project showed that, on average, giving students access to computers (with preinstalled learning software) at home helped raise standardized math test scores (Mo et al., 2011 and Table 8 in this paper, column 1, row 1). The regression results (controlling for control variables and school dummy variables) revealed that after a time period of six to eight months after the laptop was given to the treatment students (immediately after the baseline and six to eight months before the endline) the standardized math scores of the treatment students rose by 0.17 standard deviations more than those of the control students. Interestingly, the magnitude of impacts of the in-the-home OLPC intervention and the in-the-school CAL intervention were similar. There was no effect on Chinese language scores (Table 8, column 3, row 1).

Was there a gender effect? In this paper we report in Table 8 (columns 2 and 4) the results of a set of regressions that are exactly the same as those used in Mo et al. (2011), except that we include an additional interaction variable: Treatment*Female. The coefficient on the new variable can help us test the hypothesis that young girls learn from computers as well as young boys when they are at home (as opposed to the results in the previous section that seeks measure gender effects of computer learning at school).

According to our findings, there were no gender differences associated with the OLPC project (Table 8, row 2). The coefficient on the interaction variables, Treatment*Female variable, is insignificant from zero. This result indicates that young girls are learning from the computer at the same rates as young boys. The rates of increase of standardized math test scores (and Chinese language scores) are the same for young girls who get a laptop in their home and for young boys (columns 2 and 4).

5. Conclusion

In this paper we present the gender difference results from a randomized field experiment of a Computer Assisted Learning (CAL) program and OLPC program involving three kinds of schools (Beijing migrant schools, Shannxi rural public schools and Qinghai minority public schools). The total number of students involved in our survey is 9356, Beijing migrant schools account for 44 percent, Shannxi rural public schools account for 28 percent, Qinghai minority rural public schools account for 28 percent.

The main intervention for Beijing migrant school students, Shannxi rural public schools was a math program and the main intervention for Qinghai rural public schools students was a Chinese program. All the sample students were offered 40 min of shared computer time after school, twice a week. During the sessions the students played computer-based games that required them to practice relatively simple problem solving skills using their math and Chinese knowledge. The CAL program was tailored to the regular school math and Chinese curricula and was remedial in nature, providing the students with drills and exercises that were related to the material that they were learning in class. There was also an animation-based tutoring session that reviewed the lessons of the week.

To evaluate the effectiveness of the program we randomly chose schools from the entire sample frame as treatment schools and, in the case of the Beijing schools only, randomly chose a class within each program school to receive the CAL intervention. As a result all students were randomly divided into treatment groups and control groups. The remaining non-program schools were never told about the CAL program.

The data from Beijing migrant schools, and Shannxi rural public schools show there was no gender difference in baseline math test scores but the data from Qinghai minority public schools indicate there was a gender difference in baseline Chinese test scores not only for the treatment group but also for the control group. After the CAL intervention, the treatment groups improved more in math (for Beijing migrant schools and Shaanxi rural public schools) and Chinese (for Qinghai rural public schools) test scores than did the respective control groups. But there were no gender differences in improvement in standardized math and Chinese test scores between female and male students according to our regression results.

All the regression results from Beijing migrant schools, Shaanxi rural public schools and Qinghai rural public schools indicate the CAL intervention had no heterogeneous program effects on the math and Chinese test scores of students for the student gender characteristic.

Our study suggests that among many disadvantaged students in China, there is reason to believe that computer based learning can benefit both girls and boys equally. Our findings of no gender difference in the improvement of the math and Chinese learning with the computer assistance in our study support some the conclusions of Hunt and Bohlin (1993) to a certain extent. The findings also have possible implications for China's ongoing efforts to integrate computers and computing technologies into the nation's underserved schools. In particular, there is no reason to worry (at least at the elementary school level) that curriculum need to be target individually between young boys and young girls (since both appear to be learning equally well).

However there are limitations to what we can reasonably extrapolate from our work. First, our empirical evidence on gender difference in improvement in math and Chinese standardized test score is only for students in elementary schools. This admittedly is only one segment of China's education population. Second, while our sample in China's rural public schools and migrant private schools are relatively large, the samples are not representative of China as a whole. That means we must be careful in claiming external validity of our results. Certainly, given the importance of the topic, more research is certainly needed.

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Appendix A. Sampling and the sample in Beijing migrant communities (study 1)

We conducted a clustered (at the class level) RCT of Computer-Assisted Learning (CAL) in Beijing migrant schools in the fall semester of 2010. A total of 4103 students in 98 classes of Beijing migrant schools were involved in our study. Among the participants, 2154 students in 54 classes of 24 schools constituted the main sample for our study. The other schools/classes served as additional controls to check for intra-school spillovers and Hawthorne Effects.

In choosing our sample we first obtained a comprehensive list of all migrant schools in Beijing. Unlike public schools, no official list of Beijing migrant schools was available. To collect a comprehensive list of migrant schools in Beijing, we contacted all educational and research institutes and non-profit organization in the great Beijing area that might have contact information for Beijing migrant schools. We then called each school to confirm that the school was still open. During each phone call we also asked the principal of each school if there were any other schools in their area. By proceeding in this way we established a relative complete database of Beijing migrant schools, a total of 230 elementary schools were on our list.

We selected our sample schools using the comprehensive list that we assembled. To focus on our study on districts where migrant schools were most concentrated, (which made implementation somewhat easier by reducing inter-school transportation time), we restricted our sample to three districts in Beijing. These districts were among the areas of Beijing most densely populated by migrants and migrant schools. Of the 230 schools in the database, 69 schools were in these three districts. We then proceeded to exclude schools that had only one class per grade at the grade 3 level. We did this as part of our strategy to test the CAL's impact. We also excluded all schools which did not use text books in their math classes that were based on China's "uniform national math curriculum." This exclusion criterion was used because our CAL program provided remedial tutoring material that was centered on the uniform national math curriculum.

In total, 43 schools having two or more grade 3 classes and using the national math curriculum were eligible to be included in the study. Our power calculations required only 24 classes in each of the treatment group and control group. Therefore, of the 43 schools in the sample frame, we randomly chose 24 schools as our sample schools. The rest of 19 schools excluded from being part of the CAL intervention were kept as an additional control group which we use to check the robustness of our empirical results to possible Hawthorne Effects and/or other spillovers (described more below).

Because the math CAL software we used was only available at the third grade level, we only examined all third-grade students in our study and only one class in each school received the CAL intervention. In total, 2514 third-grade students were in 54 classes (more than 48 classes). Approximately 47 students per class were in the 24 core sample schools.

Although the core sample at the baseline survey included a total of 24 schools and 2514 students, there was some attrition by the end of the study. For various reasons (mainly because of school transfers and extended absences due to illness or injuries), by the time of the evaluation survey we were only be able to follow up with 902 students in the treatment classes and 1255 students in the control classes in

24 sample schools. In other words, 2157 out of the initial 2154 students were included in our evaluation survey and were part of the subsequent statistical analysis. The 14% attrition during the four months of the study, while perhaps seemingly high, is normal and can be thought of as symptomatic of migrant education in China, where parents are often moving from place to place in search of employment. Boys, older students and students with lower math test scores attrited at higher rates than students did not. Compared with students that did not attrite, attrition students were more likely to have never used a computer and had more access to some other types of electronic technology, such as cell phone.

Fortunately, the attrition seems to be independent of the assignment of the CAL intervention, thus unlikely to either reduce the validity of our research design or bias our results. The attrition is evenly distributed between the treatment and control groups. The attrition rate is 15% for the treatment group and 13% for the control group. The difference in the attrition rate between these two groups is not statistically significant. In other words, students did not leave the sample because they were or were not assigned to the treatment/control groups. Furthermore, when comparing the attrition students in the treatment group to those in the control group, we found they had similar characteristics, with the exception of *gender* and the indicator of *father has a high school degree* (differences only weakly significant). This suggests that, in general, the factors leading to attrition were largely the same for both treatment and control groups.

After choosing the 24 schools for the core sample, the first step of our study was to randomly select one class in each sample school to receive the CAL intervention. Hence, our research team randomly selected one third grade class in each of the 24 program schools to receive the CAL intervention (treatment group), leaving the rest of the third grade classes in each school to serve as the control group. In this way, the baseline student characteristics were balanced between students in the control classes and those in the treatment classes. The raw and within-school differences between the treatment and control groups were not only statistically insignificant for all student characteristics but also small in magnitude in most cases.

The randomization design, a random assignment clustered at the class level within each school, has two advantages. First, by random assignment within each school, we accounted for the potential school-specific difference between students who received the intervention and those who did not, thus improving the efficiency of our estimates. Second, intervention at the class level (as opposed to the individual level) minimizes both the CAL program's interference with the regular school instruction and the potential spillovers to students in the control group.

Appendix B. Sampling and the sample in Shaanxi Province (Study 2)

We conducted a clustered (at the school level) RCT of Computer-Assisted Learning (CAL) in Shaanxi rural schools in the Spring semester of 2011. A total of 5943 students in 177 classes of 72 Shaanxi rural schools were involved in our study. Among the participants, 2613 students in the 177 classes who were boarding students constitute the main sample for our study.

Choosing the sample consisted of several steps. First, to focus our study on poor rural students, we restricted our sample frame to four counties randomly selected out of the ten counties in Ankang Prefecture, the prefecture that covers one of the poorest areas in the southern part of Shaanxi Province. Shaanxi Province is a large (a population of nearly 40 million), rural (more than 60 percent of the population live in rural areas) and poor province in northwestern China. The average per capita income of these four counties is only around 4000 RMB (around $600) per year in 2011, which is far below rural China's average per capita income of 6977RMB in the same year (CNBS, 2011). Three out of the four sample counties are nationally-designated poverty counties in China.[1]

After choosing the counties, we obtained a comprehensive list of all *wanxiao* (or all elementary schools with six full grades, grade one through grade six) in each of the four counties from the Department of Education of Ankang Prefecture. We used two criteria to choose our sample schools. We called each school to confirm whether the school was a boarding school for both third and fifth-grade students and excluded schools with too few boarders (i.e., less than 16 boarding students in either grade). We excluded all schools if they did not use text books in their math classes that were based on China's "uniform national math curriculum." This exclusion criterion was used because these schools would not meet the requirements of our CAL program (which provided remedial tutoring material that was centered on the uniform national math curriculum). Eventually, we included all 72 schools that met the above two criteria in our sample.

After the baseline survey on the 72 schools, we randomly chose 36 schools as the treatment groups, leaving the other 36 schools as control groups. Because the math CAL software we used was only available at the third and fifth grade level, we could only implement CAL program to third-grade and fifth-grade students in our main sample schools. Therefore we offered CAL sessions to the third and fifth grade boarding students in the 36 treatment schools, and the third and fifth grade boarding students in the other 36 schools served as control group. The rest students (non-boarding students) in grade 3 and grade 5 of the 72 sample schools, while excluded from being part of the CAL intervention, were kept in our study to check for possible spillover effects.

All of the third-grade and fifth-grade boarding students in the 72 sample schools were included in the study (although only the boarding students in treatment schools received the CAL intervention). In total, there were 1155 third-grade boarding students in 44 classes and 1571 fifth-grade boarding students in 51 classes, approximately 16 students per class in the 72 sample schools.

Although the sample at the baseline survey included a total of 72 schools and 2726 students, there was some attrition by the end of the study. For various reasons (mainly because of school transfers and extended absences due to illness or injuries) by the time of the evaluation survey we were only be able to follow up with 2613 boarding students in the 72 sample schools. In other words, 2613 out of the initial 2726 students were included in our evaluation survey and were part of the subsequent statistical analysis. There were 31 attrited students from the third grade and 82 attrited students from the fifth grade. Older students, students who were the only child of their family (only weakly significant at the 10% level) and those who had lower Chinese test scores (for third-graders only) were more likely to leave the sample.

We do not consider the attrition to be a serious problem for our study for two reasons. First, the attrition rate was as low as 4%, and thus is unlike to have any substantial influence on our subsequent analysis. Second, when comparing the attrited students in the treatment group to those in the control group, we found they had similar characteristics. This suggests that, in general, the factors leading to attrition were

[1] There are 592 national designated poverty counties among the more than 2000 county-level jurisdictions in China. The Leading Group of the Alleviation of Poverty gave counties the designation in the 1990s based on the severity of the level of poverty in the county.

largely the same for both groups. After choosing the 72 schools for our sample, we randomly chose 36 schools from these 72 schools to receive the CAL intervention. As the CAL intervention only engaged third- and fifth-grade boarding students, the 1275 boarding students of the third and fifth grades in the 36 treatment schools constitute the treatment group. Among these students, 553 are third-grade students and 722 are fifth-grade students. The 1451 boarding students of the same grade (602 from the third grade and 849 from the fifth grade) in the other 36 schools served as the control group. Due to attrition, there were 2613 students left in our final analytic sample, among whom, 1205 were from the 36 treatment schools, and 1408 were from the control schools. We used a set of student characteristics to check the validity of the random assignment, and found that with the exception of the dummy variable of *both parents at home* (significant at the 5% level for the whole sample, and 10% level for the sample of fifth-grade students), the differences between the treatment and control groups were not only statistically insignificant for all student characteristics but also small in magnitude in most cases.

Appendix C. Sampling and the sample in Qinghai Province (study 3)

We conducted a clustered (at the school level) RCT of Computer Assisted Learning (CAL) in Qinghai rural schools in the spring semester of 2011. A total of 1889 students in 57 elementary schools in poor rural minority areas in China's Qinghai Province participated in our study. We adopted a three-step process to arrive at this sample.

First, we chose our sample counties. To focus our study on poor rural minority students and the impact of CAL Chinese program on improving the Chinese language skills of students in rural minority areas, we first chose Qinghai Province, a large (total area 724,000 square kilometers), rural (more than 60 percent of the population live in rural areas), and poor province in northwestern China vastly populated by ethnic minorities (ethnic minorities take up around 43 percent of the total population in Qinghai). The average per capita income of Qinghai is only around 2683 RMB, 35% below the mean national income per year in 2010 (CNBS, 2010). We then chose Haidong Prefecture, which covers one of the poorest areas in the east part of Qinghai Province. Finally, we restricted our sample frame to three minority autonomous counties in Haidong Prefecture. Two out of the three sample counties are nationally-designated poverty counties (Xunhua and Hualong) in China and the remaining one is a provincially-designated poverty county (Huzhu).[2] Among these three counties, Xunhua is a Salar minority autonomous county, Hualong is a Hui minority autonomous county, and Huzhu is a Tu minority autonomous county.

Second, after choosing the counties, we sampled 57 schools. To do so, we obtained a comprehensive list of all *wanxiao* (or all elementary schools with six full grades, that is grades one to six) in each of the three counties from the Department of Education of Haidong Prefecture. We used three criteria to choose our sample schools. We called each school to confirm the number of minority students in that school and excluded schools with less than 20% minority students. We excluded all schools that did not use text books in their Chinese classes that were based on China's uniform national Chinese curriculum.[3] This exclusion criterion was used because these schools would not meet the requirements of our CAL program (which provided remedial tutoring material that was centered on the uniform national Chinese curriculum). For implementation convenience we also excluded schools that were either too big (more than 150 third-grade students) or too small (less than 20 third-grade students). There were 70 schools that met all the three criteria above. Because of our limited computer provision, we then randomly chose 57 schools for our final sample.[4]

Finally, because the Chinese CAL software we used was only available at the third-grade level, we only examined third-grade students in our sample schools. All of the third-grade students in the 57 sample schools were included in the study. In total, there were 1889 third-grade students included in the sample, among whom 86% belonged to a minority ethnic group.

After choosing the 57 schools for our sample, we randomly chose 26 schools from these 57 schools to receive the CAL intervention. The 764 third-grade students in the 26 treatment schools constitute the treatment group. The 1125 third-grade students in the other 31 schools served as the control group. We used a set of student characteristics to check the validity of the random assignment, and found that the differences between the treatment and control groups were not only statistically insignificant for all student characteristics but also small in magnitude in most cases.

Although at the time of the baseline survey, the main sample included a total of 57 schools and 1889 third-grade students, there was some attrition by the end of the study and some students were not included in our analysis. For various reasons (mainly because of school transfers and extended absences due to illness or injuries) by the time of the evaluation survey we were only be able to follow up with 1717 third-grade students in the 57 sample schools. In other words, 1717 out of the initial 1889 students were included in our evaluation survey and were part of the subsequent statistical analysis. Among the 764 treatment students, 680 remained in our sample at final evaluation. Among the 1125 control students, 1037 remained in our sample at final evaluation. On average, boys, younger students, students with lower baseline Chinese test scores, those who had used a computer before, and those whose mother is illiterate were more likely to attrite from the sample. These patterns are not strong (statistically significant only at the 10% level). Students of Hui minority left the sample significantly more than other ethnic groups (significant at the 1% level).

However, we do not consider the attrition is a serious problem in our case for two reasons. First, the attrition rate is lower than 10%, and thus is unlikely to have any substantial influence on our subsequent analysis. Second, when comparing the attrition students in the treatment group to those in the control group, we found they had similar characteristics. This suggests that, in general, the factors leading to attrition were largely the same for both groups.

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[2] There are 592 national designated poverty counties among the more than 2000 county-level jurisdictions in China. The Leading Group of the Alleviation of Poverty gave counties the designation in the 1990s based on the severity of the level of poverty in the county.

[3] This curriculum is published by the People's Education Press, a publishing organ under the Ministry of Education that produces subject matter curricula for use in China's schools.

[4] We only had 200 computers for all of the treatment schools, which was not enough to accommodate students of the same grade (or even the same class) simultaneously in 35 out of the 70 schools (treatment schools often amount to half of the schools in the whole sample). Therefore, the students of the same grade needed to break into several groups and took turns in using the computer for CAL sessions. Even so, the program could not cover all schools. The CAL protocol requested each student should have had two 40-min sessions per week, yet in most schools, school was over around 4:30 pm and students had to leave for home immediately as they live far away from school. Hence there was only a limited amount of time for the schools to run CAL program. As a result, it was infeasible for the CAL program to accommodate all of the 35 schools, and thus we randomly chose 57 schools out of the 70 schools. 18.

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