The Impact of Eyeglasses on the Academic Performance of Primary School Students: Evidence from a Randomized Trial in Rural China

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December, 2010

Abstract:

About 10% of primary school students in developing countries have poor vision, yet in virtually all of these countries very few children wear glasses. There has been almost no research on the impact of poor vision on school performance in developing countries, and simple OLS estimates are likely to be biased because students who study more often are likely to develop poor vision faster. This paper presents results from the first year of a randomized trial in Western China that began in the summer of 2004. The trial involves over 19,000 students in 165 schools in two counties of Gansu province. The schools were randomly divided (at the township level) into 103 schools that received eyeglasses (for students in grades 4-6) and 62 schools that served as controls. The results from the first year indicate that, after one year, making eyeglasses available increased average test scores by 0.09 to 0.14 standard deviations (of the distribution of the test scores). For those students who accepted the glasses, average test scores increased by 0.12 to 0.22 standard deviations.

Data collection for the Gansu Survey of Children and Families was supported by grants from The Spencer Foundation Small and Major Grants Programs (wave 1), by NIH Grants 1R01TW005930-01 and 5R01TW005930-02 (wave 2), and by a grant from the World Bank (wave 2). Travel was supported by CIFAP. We would like to thank Kate Arnold and Andrew Zeitlin for comments on earlier drafts of this paper.

I. Introduction

Most economists agree that higher levels of education increase economic growth (Barro, 1991; Mankiw, Romer and Weil, 1992; Hanushek and Kimko, 2000; Krueger and Lindahl, 2001; Sala-i-Martin, 2004; Hanushek and Woessmann, 2008), raising incomes and, more generally, the quality of life. Economists' support for education is matched by strong support from international development agencies. Two of the eight Millennium Development Goals (MDGs) adopted at the United Nations Millennium Summit in 2000 focus on education: first, all children should complete primary school, and second, gender equality should prevail at all education levels.

Yet school enrollment may have little effect on economic growth and individuals' incomes if few academic skills are obtained when children are in school. While economists and other social scientists have learned what education policies are effective at increasing school enrollment, much less is known about what policies are most effective in increasing student learning (Glewwe and Kremer, 2006). This paper examines a specific intervention that may increase student learning in developing countries that, to date, has received little attention: providing eyeglasses to primary school students with vision problems.

Approximately 10% of primary school age children in developing countries have vision problems, and although in almost all cases these problems can be corrected with properly fitted eyeglasses, very few children in developing countries wear eyeglasses. This paper presents results from a randomized trial in Western China that offered eyeglasses to children in grades 4, 5 and 6. More specifically, it presents estimates of the impact of being offered eyeglasses and, because about one third of those offers were turned down, estimates of the impact of wearing eyeglasses.

The rest of this paper is organized as follows. Section II provides information on education in China and vision problems among school-age children, and reviews the small literature on the impact of vision problems on student performance in developing countries. Section III describes the randomized trial and the data available from it. Section IV describes the estimation methodology, and Section V presents the results. Section VI presents checks of the robustness of the results, and Section VII presents exploratory estimates of factors that explain why some children did not accept the free eyeglasses. A final section summarizes the results and provides recommendations for further research.

II. Background and Literature Review

This section provides an overview of primary education in rural China and a brief literature review of the extent of vision problems among primary school students in developing countries and the impact of those problems on student performance.

A. Primary Education in Rural China. China has achieved nearly universal primary school enrollment. According to the 2000 census, only four percent of adults aged 25 to 29 had not attended any formal schooling (Hannum et al., 2008). The Law on Compulsory Education passed in 1986 mandates that all children complete nine years of schooling—six years of primary school and three years of lower secondary school. However, the rural poor and some minority populations continue to face difficulties in meeting this compulsory schooling goal.

In rural areas of Western China, nearly all children attend the nearest public primary school, located in their own village or in a nearby village. A typical primary school has one or two classes per grade level. Teachers are allocated to schools within the county by the county educational bureau, and their salaries are paid by the county government. Thus, disparities in primary school quality within counties are generally fairly modest (Li et al, 2009). This also helps explain why few students attend school while living away from home.

Each county in China has a Center for Disease Control office, which conducts regular physical exams of all students, including eye exams. In principle, health exams should be conducted every year for all students, but because of budgetary and staff constraints, in many areas schools conduct physical exams only once every two or three years. The results of the physical exams are given to the school's teachers, who convey the information to parents.

B. Vision Problems and School Performance. Very little data exist on vision problems among school-age children in developing countries. Bundy et al (2003) report that about 10% of school-age (5-15 years old) children have refraction errors (myopia, hypermetropia. strabismus, amblyopia, and astigmatism), which account for about 97% of the vision problems among those children. Almost all refraction errors can be corrected with properly fitted eyeglasses, but most children with refraction problems in low income countries do not have glasses. In China, a study by Zhao et al. (2000) in one district in Beijing found that 12.8% of children age 5-15 years had vision problems, of which 90% were due to refraction errors. Only 21% of the children with vision problems had glasses. In rural areas, children with vision problems are even less likely to wear glasses, as will be seen below. In China, a commonly held (but mistaken) view is that wearing eyeglasses causes children's vision to deteriorate faster.

Given the lack of data on vision problems among school-age children in developing countries, there has been very little research on the impact of poor vision on students' academic performance. Only one published study exists; Gomes-Neto et al. (1997) found large negative impacts of poor vision on primary school children in Northeast Brazil. In particular, they found that children with compromised vision (less than 90 on the Sneller chart) had a 10 percentage point higher probability of dropping out of school, an 18 percentage point higher probability of repeating a grade, and scored about 0.2 to 0.3 standard deviations lower on achievement tests. Yet these estimates could be biased. First, to the extent that some of these children wore glasses their vision could be correlated with unobserved factors that determine school performance, such as parental preferences for educated children. Second, even if none of these children wore glasses, students' vision can be affected by their home environment (e.g. lighting quality) and by their daily activities, including time spent studying and completing homework. Thus their vision could be correlated with unobserved factors that have a direct impact on school performance (e.g. hours spent studying), leading to biased estimation results.

III. Project Description and Data Available

The lack of rigorous studies on the impact of providing eyeglasses to students with visual impairments in developing countries led to the implementation of the Gansu Vision Intervention Project in 2004 in Gansu Province in northwest China. This section describes the project and the data available to evaluate its impact.

A. The Gansu Vision Intervention Project. In 2004, a team of Chinese and international researchers, in cooperation with the Ministries of Health and Education in Gansu Province, implemented a randomized trial to examine the impact of providing eyeglasses to primary school students with poor vision in two counties, Yongdeng and Tianzhu. The project covered all students in grades 4-6 in all primary schools from each of these two counties.

Gansu Province is located in northwestern China. Its geography is quite diverse, including areas of the flat Loess Plateau, the Gobi desert, mountainous and hilly areas, and vast grasslands. In the year 2000, its population was 25.6 million, 76 percent of whom reside in rural areas (Gansu Bureau of Statistics, 2001). Estimates of rural per capita disposable income in 2004, the year of the intervention, place Gansu at a rank of 30 out of 31 provinces, with only Tibet showing lower incomes (National Bureau of Statistics, 2005). Using per capita income data and official poverty lines, a World Bank report found that 23 percent of the rural population in Gansu is poor, compared to 6.5 percent for China as a whole (World Bank, 2001).

Yongdeng and Tianzhu are adjacent counties that were selected as study sites because they are typical rural counties in Gansu, are located within several hours drive to the northwest of Lanzhou, the provincial capital (which enabled the project to be closely monitored by the provincial Center for Disease Control (CDC) under the Ministry of Health), and have capable county CDC staff to implement the project effectively. Tianzhu is a Tibetan minority autonomous district under the jurisdiction of Wuwei municipality. It had a population of 213,000 in 2006, 17% of which were in urban areas. According to the 2000 population census, 63% of Tianzhu's population were Han

Chinese and 30% were Tibetan. Yongdeng is a much more populous county than Tianzhu, despite having a similar land area, and is part of Lanzhou Municipality. It had a population of 476,000 in 2006, of which 15% were in urban areas and nearly all of whom were Han Chinese. Being near Lanzhou, among counties in Gansu, both Tianzhu and Yongdeng are relatively well-off; their GDP per capita ranked 21st and 23rd, respectively, among 87 county-level units (including urban districts) in 2008 (**citation??**).

Yongdeng County is divided into 23 townships, of which 18 participated in the program. These 18 townships have 155 primary schools. Nine of these 18 townships were randomly chosen to participate in the eyeglasses intervention in 2004, and the remaining nine were assigned to the control group. Tianzhu county is divided into 22 townships, of which 19 participated in the program. These 19 townships have 101 primary schools. Ten of Tianzhu's 19 townships were randomly chosen to participate in the program in 2004, and the remaining nine were assigned to the control group. In both counties, excluded townships included the county seat, which are the main urban centers where incomes are higher and eyeglasses are easily accessible, and a few townships that were in remote locations that would have made program administration very costly.

The random assignment was done as follows. In each county, all included townships were ranked by rural income per capita in 2003, and starting with the first two townships, one township was randomly assigned to be a treatment township while the other was assigned to the control group. In Tianzhu, the 19th township (the poorest) was not paired with any other township, so a random draw assigned it to the group that received eyeglasses. The primary schools within each township were either all assigned to the treatment group or all assigned to the control group.[1]

A baseline survey that collected data on student characteristics, academic test scores, and visual acuity was conducted at the end of the 2003-2004 school year (i.e. in the summer of 2004). This survey included both the treatment and the control schools, and covered all students finishing grades 1-5 in June of 2004. The students with poor vision in treatment schools who would be entering grades 4-6 in the fall of 2004 were offered free eyeglasses. Later that summer, in each county, an optometrist contracted by the project traveled to each township to conduct more in-depth eye tests for students who accepted the offer (with the permission of their parents) and, if poor vision was confirmed, to prescribe appropriate lenses. Students were given a limited choice of colors and styles for their eyeglass frames. The Gansu Province CDC then ordered all of the eyeglasses from a company with an established reputation. The fall semester of 2004 began on August 26th, and most eligible and consenting students received their eyeglasses by mid-September. At the end of 2004-2005 academic year (late June or early July of 2005), grades for the fall semester 2004 and spring semester 2005 were collected to evaluate the impact of the eyeglasses on test scores.

Unfortunately, there were a few cases where control townships were provided with eyeglasses because, after providing the eyeglasses in the treatment townships, the money remaining in the budget was used to provide eyeglasses to students with poor vision in the control schools. This occurred in two of the control townships in Yongdeng

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[1] Primary schools with less than 100 students were excluded from the project to avoid high travel costs to a small number of very remote schools. Students in such schools account for only 6 percent of primary students in the two counties.

and in two control townships in Tianzhu.[2] Also, another of the control townships in Yongdeng was incorporated into a township that received eyeglasses, so that control group was also compromised. In all five cases where the control township was provided with eyeglasses, both that township and the treatment township with which it was paired were dropped from the analysis. Finally, in one pair of townships in Tianzhu no one in the treatment township was offered glasses while about one third of children in the control township were offered glasses, so it appears that there was a "role reversal" in this pair of townships. Because this reversal may have been done deliberately, this pair is also dropped from the analysis. This leaves six pairs of townships in Yongdeng and six pairs (plus the poorest township, which was randomly assigned to the treatment group) in Tianzhu for which the randomization was carried out according to the plan. Most of the regression analysis below is limited to these 25 townships, which together contain about 19,000 students spread across 165 schools (103 of which were received eyeglasses for children in grades 4-6 and 62 of which were controls).[3]

B. Data Used in the Analysis. The data used in the analysis are from three sources: 1. School records on basic student characteristics and academic grades before and after the intervention; 2. Results of health exams, including vision tests, conducted by the county Center for Disease Control in each primary school before eyeglasses were provided; and 3. Information from optometrists' records on students who were fitted for eyeglasses. The basic information in the school records include the grade the student was

[2] In a third control township in Tianzhu, four children in the control group received glasses, but three of these four did not have poor vision. This control township is retained in the analysis, after dropping the four children who received eyeglasses. Excluding this township, and its matched pair, has very little effect on the results **[need to check! Albert things we should leave these 4 kids in]**.

[3] The reason why 62% of schools (and 65% of students) are in the treatment group is that the two largest townships (which together have 25% of the students) were, by chance, assigned to the treatment group.

in during the 2003-04 school year, the students' sex, ethnicity and birthdate, and the occupation and education level of the head of the household (usually the father) in which the student lives. The school academic performance data include scores on exams given at the end of each semester in each grade since the student enrolled at that school (usually grade 1).[4] Separate scores are available for three subjects: Chinese, mathematics and science.

There is one important characteristic of the grade variables which has important implications for the analysis: in many, if not most, cases different exams were used in different schools, so the grades are not comparable across schools. Given random assignment of townships to the treatment and control groups, this non-comparability of exams across schools does not result in biased estimates. However, it does add noise to the data, similar to a school random effect, which must be addressed in estimation. The implications for estimation are further discussed in Section IV.

The school health data include whether the student wears glasses (and if so, the grade the student was in when he or she started to wear glasses), the student's height, weight and hemoglobin count, and at least one measurement of vision for each eye (students who were provided glasses have additional measurements for the purpose of fitting them with eyeglasses). In China, doctors usually conduct eye exams by asking a patient to read (with the other eye covered) a standard eye chart from 5 meters away. The chart is similar to eye charts used elsewhere. It has 12 rows of the letter E facing in different directions; the top row of the chart has very large E's, and each subsequent row has smaller E's. If the patient can read the 10^{th} row, the normal level, his/her eyesight is

[4] In some schools, the exam grades are averages over two or more exams, including an end of semester exam, given in that semester.

coded as 5.0. If the patient cannot read the first row, corresponding to the worst eyesight, his or her vision is coded as 4.0. If he or she can read the first row but cannot read the second row, his or her vision is coded as 4.1, and so forth. A patient who can read all 12 rows is coded as 5.2. The information from the optometrists, which exists only for children who were offered eyeglasses, includes whether the child was fitted for eyeglasses, and if not, the reason eyeglasses were not provided (some students had eye conditions that could not be corrected with eyeglasses, and others declined the offer to receive eyeglasses). **[Add description of GSCF-1 and GSCF-2 data if we use it for some analysis.]**

C. Descriptive Statistics. Table 1 presents descriptive statistics for the sample. The data consist of 18,915 students in grades 4-6 in 2004-05 in Tianzhu and Yongdeng counties in the 25 townships where the randomization was correctly implemented. Of these students, 2,528 (13.4%) had poor vision in the sense that either the left eye or the right eye (or both) had a visual acuity score of less than 4.9.[5] Only 2.3% of the children in the two counties with vision problems (59 out of 2,528) had eyeglasses before the project began. Students without vision problems had slightly higher scores than children with vision problems for all three subjects (78.9% vs. 78.2% for Chinese, 79.1% vs. 78.5% for mathematics, and 80.8% vs. 80.6% for science) at the end of the spring 2004 semester (a month or two before the program began).

[5] Although children with a visual acuity score of 4.9 in one or both eyes were also offered eyeglasses, only 6.8% (17 out of 249) accepted. In contrast, 56.5% of children (109 out of 193) with a visual acuity score of 4.8 in one or both eyes accepted the glasses that were offered to them. Since the exact cutoff point between good and poor vision is somewhat arbitrary, this suggests that the cutoff point for poor vision should be below 4.9, as opposed to below 5.0. Indeed, the low take-up rate for children with a visual acuity score of 4.9 makes it impossible to estimate the impact of providing eyeglasses to those children.

The test score data in Table 1 suggest that vision problems have little effect on students' academic performance. Indeed, simple t-tests show that, for both counties as a whole and for each county separately, none of the above-mentioned small differences in test scores is significant. But this conclusion is likely to be misleading because school performance can affect eyesight. In particular, medical studies (e.g. Angle and Wissmann, 1980; Lu et al., 2007) have shown that doing "near-work", that is spending long amounts of time doing activities with the eyes focused on objects about 1 meter from one's eyes) can cause myopia. This implies that students who spend more time studying are more likely to develop myopia, the most common refractive eye problem.

Indeed, the data available before the Gansu Vision Intervention Program was implemented suggest that studying does harm students' vision. The first thing to realize is that, among this sample of children, very few grade 1 students have poor vision (only 2.9% are classified as having a visual acuity score below 4.8 in both eyes), but this increases dramatically as children spend more time in school (7.0% of grade 3 students and 15.5% of grade 5 students). Thus children's test scores in grade 1 are unlikely to be seriously affected by vision problems but presumably do reflect, in part, time spent studying. OLS regressions of mean (over both eyes) visual acuity on average test scores (over Chinese, math and science) in grade 1, controlling for school fixed effects, grade level, parents' education and occupation (on the sample children in grades 3-5 in the 2003-04 school year) show a ***negative*** impact that is statistically significant at the 10% level. This suggests that visual acuity is negatively affected by increased study, so that simple comparisons of test scores across students with good vision and students with poor vision are likely to underestimate the negative impact of vision on student performance (because students with good vision, on average, study less). **[Meng checked the GSCF-2 data to see whether it shows that students who studied more in 2000 are more likely to have bad eyesight in 2004. She found that it does; see her log file 1/08/10.]**

Table 2 presents information on how the Gansu Vision Intervention Project was implemented for the 2,528 students with poor vision. These statistics exclude the township pairs for which the randomization was not properly implemented. Of these, 1,528 were in the program schools and thus were offered eyeglasses (those who already had eyeglasses were offered new ones), while 1,000 were in the control group and were not offered glasses. Of the 1,528 students who were offered glasses, 1,066 (69.8%) accepted them and the other 462 declined. The main reasons for turning down the offer were the objection of household head (145 cases) and refusal on the part of the child (80 cases).

IV. Methodology

Virtually all children of primary school age in Gansu province are enrolled in school; the Gansu Survey of Children and Families, which collected data on 2000 children age 9-13 in the year 2000, found that only 1.4% were not enrolled in school. Thus provision of eyeglasses cannot increase school enrollment; the sole impact is on academic performance. The random assignment of schools to participate or not participate in the Gansu Vision Intervention Project allows for straightforward analysis of the impact of the project on students' scores on academic tests. To ease interpretation, all estimates in the rest of this paper use standardized test scores as the dependent variable;

test scores are standardized by subtracting the mean and then dividing by the standard deviation, using the mean and standard deviation of the control group schools, separately for each subject and grade.

A. Estimation of the Impact of the Offer of Eyeglasses. The simplest estimate of the impact of the program on children in grades 3, 4 and 5 with poor vision is to compare the mean test scores of the children who were enrolled in the program schools with the mean test scores of the children who were enrolled in the control schools. Technically speaking, this estimates the impact of the *offer* to receive eyeglasses (the intention to treat effect), not the impact of the eyeglasses themselves, because (as explained above) about 30% of the children who were offered eyeglasses did not accept them.

This t-test can be calculated by regressing the (standardized) test score variable (T) on a constant term and a dummy variable that indicates enrollment in a program school (P):[6]

$$T = \alpha + \beta P + u \qquad (1)$$

where u is a residual term that is uncorrelated with P due to randomized program assignment. Note that children in the same school and the same grade within a school may have common unobserved factors. Indeed, as explained above schools often used their own tests, as opposed to county-wide or province-wide tests, which will generate correlation of test scores among students within the same school and so will cause the

[6] In addition, all regressions in the paper include dummy variables for each pair of townships, since the randomization was done within those "strata". See Bruhn and McKenzie (2009) for a justification of this approach.

error term (u) to be correlated across children in the same school. Thus any estimation method used must allow for such correlation among students within the same schools, and perhaps even within the same townships (since both tests and school quality may be similar within townships).

Two general methods are used in this paper to allow for correlation of u across students in the same schools and townships: 1. Error components (random effect) models, with separate error terms for each township, each school within a township, and each grade within a school; and 2. OLS estimates with "robust" variance-covariance matrices that allow for heteroscedasticity of unknown form, including correlation across observations within the same schools or townships. Each method has advantages and disadvantages. The error components method has the advantage that it can be used to test for correlation at the grade, school, township and county levels; if correlation is not found at a certain level, more precise estimates can be obtained using estimation methods (of either type) that rule out such correlation. The other main advantage is that error components models use information about the structure of the data that has implications for the extent of the correlation found. In this context, one would expect u to be more highly correlated for two children in the same grade in the same school, relative to two children in different grades in the same school, and for two children in the same school (but in different grades) in the same township, relative to two children in different schools (and different grades) in the same township. The main disadvantage of error components models is that they specify a particular structure that may not hold. **[Also, normality assumption for Stata (but not SAS) version.]** For example, the correlation between the test scores of two children in a given school must be the same for all schools.

The advantages and disadvantages of the second method, robust correlation matrices for OLS estimates, mirror those of the error components approach. The main advantage is that, as its name indicates, this method imposes no structure at all on the correlation of the error terms of observations within the groupings (grades, schools or townships) once the "highest level" at which correlation takes place has been specified. The disadvantages are that this method does not allow for testing of the level at which correlation exists, and it does not use the information available about the "closeness" of different observations (e.g. whether two students in the same township are also in the same school and the same grade), which leads to less precise estimates. Thus, in general, estimates using this method require no assumptions about the nature of the correlation across observations (other than the highest level at which such correlation is found) but at the cost of yielding less precise estimates of program impacts.

Estimates of β in equation (1) use only students who have poor vision. More precise estimates of that parameter can be obtained by using an estimation method that also includes students with good eyesight. The intuition for this "double difference" estimation method is that it compares the difference in test scores of children with poor vision across treatment and control schools with the same difference for children with good vision. This estimator is obtained from the following econometric specification:

$$T = \alpha + \pi PV + \tau P + \beta PV^{*}P + u \qquad (1')$$

where PV is a dummy variable indicating poor vision. In this specification the impact of the program on students with good vision (PV = 0) will be τ, which one would expect to equal zero, and the impact of the program on students with poor vision will be $\tau + \beta$, which should equal β since τ should equal zero. The τ coefficient also serves as a check on the random design of the intervention; if the schools that participated in the program were better (worse) than average, then τ would be positive (negative).[7] Finally, the estimate of π is a (biased) estimate of the impact of poor vision on test scores, which one would expect to be negative (ignoring the bias). The bias arises because students who study more are likely to have worse vision, as explained above.

Estimation of equation (1′) must also take account of correlation in the error term (u) for students in the same grade and in the same school but in different grades, and perhaps also for students in the same township and the same county. This will be done using the same two methods described above (error components specification and OLS estimation with a robust variance-covariance matrix).

Returning to equation (1), in principle more precise estimates can be obtained by adding additional explanatory variables, such as child characteristics (e.g. sex) and parental background. This was pursued but in the end it did not increase precision, so this will not be done here **[come back to this later]**.

Similarly, more precise estimates could be obtained by adding school fixed effects, which can "soak up" variation in school quality and in the difficulty of the tests across schools. This can only be done for equation (1′), since in equation (1) the set of school fixed effects would be perfectly correlated with the program variable (P). This is also the case for equation (1′), but the program effect in that equation is measured by the interaction of the program variable and the poor vision dummy variable, which varies

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[7] Even if randomization was perfectly implemented, τ could be different from zero if there were spillover effects of the program onto children with good vision. This is investigated in Section VI.

within schools. By focusing on within-school variation, this specification also is less subject to bias from imperfect randomization of treatment across schools, since all unobserved school differences are absorbed in the fixed effect. Non-random assignment only causes bias in this context if treated and untreated schools differ systematically in the differential performance of children with good and poor vision.

Another point is that the impact of providing eyeglasses could vary by child and family characteristics. **[Need to come back to this as well]**.

Another approach to increase the precision of estimated program impacts is to examine whether the *change* in students' test scores is affected by being offered eyeglasses. Consider equation (1). Assume that it holds for two time periods, before the program started (t = 0) and about one year later, after the program started (t = 1). Let T_0 be a student's test score at t = 0 and let T_1 be the test score at the end of the first school year in which eyeglasses were offered (t = 1). Then the difference in the two test scores is:

$$T_1 - T_0 = (\alpha_1 + \beta_1 P + u_1) - (\alpha_0 + u_0) = (\alpha_1 - \alpha_0) + \beta_1 P + (u_1 - u_0) \qquad (1'')$$

where subscripts on α, β and u allow them to vary over time periods, and there is no $\beta_0 P$ coefficient in the term for T_0 because P = 0 for all observations at time zero. Note that the β_1 in equation (1″) is the same β that is in equation (1). This estimation method removes unobserved heterogeneity that is fixed over time, an important example of which is school fixed effects; such effects are quite likely if each school designed its own test. Unobserved heterogeneity in student characteristics that is fixed over time will also be removed.

Equation (1′) can also be modified by replacing T with $T_1 – T_0$. It is still the case that β_1 will estimate the program impact, since the equation will be:

$$T_1 - T_0 = (\alpha_1 - \alpha_0) + (\pi_1 - \pi_0)PV + \tau_1 P + \beta_1 PV^*P + (u_1 - u_0) \qquad (1'')$$

where again τ_1 should equal zero. If the correlation between the error terms and PV in each time period is primarily due to a fixed effect (e.g. the propensity to study does not change) then the coefficient $\pi_1 – \pi_0$ may be a consistent estimate of the effect of poor vision on an additional year of learning.

B. IV Estimates of the Impact of Providing Eyeglasses. The methods presented in the previous subsection estimate the impact of being offered the eyeglasses, not the impact of receiving eyeglasses. In general, the impact of being offered eyeglasses will be less than the impact of receiving them because those students who are offered but do not receive eyeglasses do not benefit from the offer. Direct OLS estimation of the benefit of receiving eyeglasses may yield biased estimates because parents and/or students who take up the offer of eyeglasses may differ in unobserved ways from students for whom the offer is turned down. For example, the parents of students who take up the offer may have more favorable attitudes toward education and so may do other things that raise the test scores of their children.

Fortunately, instrumental variable (IV) estimation can be used to obtain consistent estimates. In particular, one can estimate the impact of actually receiving eyeglasses (impact of the treatment on the treated) using the same equations presented above, replacing P (the offer to receive eyeglasses) with "G", actually receiving the eyeglasses.[8] While G is likely to be correlated with the residual, P can be used as an instrumental variable for G; P is, by definition, uncorrelated with u and also has strong explanatory power for G. Note that G = 1 not only for students who agreed to accept glasses in the program school but also for the small number of students who wear their own glasses, either in the program schools or in the control schools.

While IV estimates for equations (1) and (1″) are straightforward in that one needs only to replace P with G and use P as an instrument for G, there is one complication with IV estimates of equations (1′) and (1‴). To see the problem, note that automatically replacing P with G in that equation yields $T = \alpha + \pi PV + \tau G + \beta PV*G + u$. Although it is possible to be in a program school if one does not have poor vision, it does not make sense to wear glasses if one does not have poor vision, which implies that G = 0 whenever PV = 0, and thus that G and PV*G are perfectly correlated. While this correlation is not exactly equal to 1 in the data (it is 0.86), this is only due to the fact that there are a very small percentage of students who report wearing classes even though they have good vision. Thus in IV estimates of (1′) and (1‴) the term τG is dropped. **[Also we really don't have a good IV for people who wear glasses and don't really need them.]**

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8 Strictly speaking, the IV estimates are local average treatment effects (LATE), i.e. estimates of the impact of wearing glasses for those students that were induced by the program (by the offer of free eyeglasses) to wear eyeglasses. Yet since very few students were wearing eyeglasses before the program, LATE estimates are almost identical to the impact of receiving eyeglasses on those who actually received them (impact of the treatment on the treated).

A final point to note about IV estimation is that it is valid even if the randomized trial was not strictly implemented according to the randomized plan. Quite simply, as long as the *plan* was randomized then the instrument is uncorrelated with all possible confounding factors and will be a valid instrument as long as it has explanatory power for the use of eyeglasses (which should be the case as long as the intervention was implemented to some extent according to the randomized plan).

V. Estimates of Program Impact

This section presents estimates of the impact of the Gansu Vision Intervention Project on the test scores of students in grades 4-6 in the spring of 2005. Thus these results measure the impact of the project after one year. As explained above, all test scores have been normalized separately for each subject and grade. The first subsection presents estimates of the impact of being offered eyeglasses, and the second presents IV estimates of the impact of receiving eyeglasses.

A. OLS Estimates of the Impact of Being Offered Eyeglasses. Before examining the impact of the Gansu Vision Intervention Project, the data must be examined to see whether the offer of eyeglasses was in fact randomly allocated across townships. This was done by estimating equations (1) and (1′) using test scores from the spring of 2004, before the project was implemented. These results are shown in Table 3.

Before examining those results, several error components models were estimated to determine whether correlation of the error terms in equation (1) is only at the school level, or also extends to the township level and perhaps even the county level. A likelihood ratio test of the hypothesis that the variance of the township level error term is zero (conditional on the existence of school level and grade level error terms) was rejected (p-value of 0.026). In contrast, an analogous test of the (non)existence of a county level error term (conditional on township, school and grade error terms) could not be rejected.[9] Thus all estimates in the paper allow for correlation up to the township level, but not up to the county level. Although, technically speaking, these tests apply only to the error components specification, the rejection of the lack of correlation at the township level for that specification suggests that there is some correlation at that level, even if it may not take the error components form, and so the more flexible (robust) estimation method should also allow for correlation at that level.

The two sets of estimates of equation (1) in the top half of Table 3 show no statistically significant difference in spring 2004 test scores across program and control schools, as indicated by the coefficients on the "treatment township" variable. More specifically, the differences in mean Chinese score across these two sets of schools are very small (less than 0.04 standard deviations of the distribution of test scores). Also, the differences in the mean mathematics and science scores are close to zero, ranging between -0.07 and 0.05, and these differences are not statistically significant. Averaging across all three subjects gives insignificant differences of 0.015 (error components specification) and -0.039 (OLS with robust errors specification) of the standard deviation of the (average) test score. Thus estimates of equation (1) support the claim that the randomization of the 25 townships was correctly implemented.

[9] This also holds for the spring 2005 test scores.

Estimates of equation (1′) are shown in the rest of Table 3.[10] Recall that these include both students without vision problems and students with vision problems and so should be more precise, and indeed it is the case that the standard errors of the estimates of β are lower.

Consider first the estimates of the error components specification. Comparing students without vision problems (i.e. examining the coefficient on "treatment township"), there is little difference in mean test scores for students without vision problems, and all differences are completely insignificant; the difference of the averaged scores is only 0.029 standard deviations and is far from significant. Focusing on the (more precise) estimates of differences across students with poor vision (i.e. the coefficient on "poor vision × treatment township), there are no significant differences in the impact on Chinese, math or science scores, and when all scores are averaged the impact is small (-0.041) and statistically insignificant. Note also that the standard errors of the impact of the program fall dramatically when the error components specification includes students with good eyesight.

Turning to the OLS specification with robust standard errors (but without school fixed effects), there is one surprising result. For the science test it appears that, for children with bad vision, the program had a negative impact (-0.12 standard deviations of the distribution of that test score) that is statistically significant. Yet the estimated negative impacts for the other two tests (-0.04 and -0.12) are not statistically significant, and neither is the negative impact (-0.11) for the average over all three tests. Given that

[10] Estimates of equation (1) classify students whose worst eye has a visual acuity score of 4.9 as having good vision. Recall, however, that such children were offered glasses, and that 17 out of 249 accepted them. Those 17 children are excluded from the regression. Note also that dropping all 249 of these children from the regressions does not change the results.

Table 3 presents 16 difference estimates of program "effects" on pre-test scores, even if the true values of these "effects" are all zero random chance is likely to yield one that appears to be statistically significant at the 5% level.

Finally, adding school fixed effects to equation (1′), as seen in the last three rows of Table 3, leads to estimated program "impacts" with poor vision that are completely insignificant, even though the standard errors are smaller than in the robust OLS estimates of equation (1′) that do not use school fixed effects. Overall, the results in Table 3 are consistent with the hypothesis that the randomization was properly implemented.

Next turn to estimates of the impact of being offered eyeglasses on test scores after a full academic year. Table 4 presents estimates of equations (1) and (1′), that is estimates with the (normalized) 2005 spring semester test score as the dependent variable, and Table 5 presents estimates of equations (1″) and (1‴), that is estimates for which the dependent variable is the change in the (normalized) test score from the spring of 2004 to the spring of 2005. Results are presented for both specifications, that is the error components method and OLS with robust standard errors.

The first two rows of Table 4 present results for equation (1), which uses data only for children with poor vision. The results show positive impacts, ranging from 0.07 to 0.16 standard deviations, for all three subjects, but none of these estimated impacts is statistically significant. It is not particularly surprising that the impacts are low, since the program had been in place for only one year.

To obtain more precise estimates of the impact of offering eyeglasses, turn to the estimates of equation (1′) in the rest of Table 4, which compare the difference in test scores of children with good vision across program and control schools with the same difference for children with bad vision. This comparison removes much of variation in test scores generated by the fact that different tests were used in different schools. That, along with the increased same size, greatly reduces the standard errors of the estimated impacts of the program.

The error components specification shows that the offer of eyeglasses increased students test scores by 0.06 to 0.09 standard deviations of a test score, and these impacts are statistically significant for two of the three subjects (Chinese and math) and for average test scores. In contrast, the OLS specification with robust standard errors finds smaller, statistically insignificant impacts. Recalling the results for this specification in Table 3, this reflects the negative, though mostly statistically insignificant baseline results, which imply that, by random chance, students with poor vision in treatment schools tended to have lower test scores than students with poor vision in control schools. As explained above, and seen for Table 3, adding school fixed effects removes random differences in schools, and so produces more plausible results **[Show algebra in a footnote?]**. In particular, when this is done the estimated impacts are positive and similar in size to those of the error components specification. Yet only one estimated impact, that for Chinese, is statistically significant, and that significance is only at the 10% level.

Overall, the results in Table 4 suggest that offering eyeglasses to children with poor vision increased their test scores (after about eight months) by 0.06 to 0.09 standard deviations, but the results are not precisely estimated. This leads to estimates that examine the changes in test scores over time, which are shown in Table 5.

The first two rows of Table 5 present estimates of equation (1″), which use changes in test scores as the dependent variable but are based only on students with poor vision. The estimated impacts are generally similar to those in Table 4, and on average are no more precise. All but one of the estimated impacts are statistically insignificant, the exception being the science impact estimated using OLS, that impact is 0.17 standard deviations and is significant at the 1% level.

The remaining estimates in Table 5 examine differenced results that include both students with good vision and students with poor vision. As in Table 4, including the students with good vision almost always increases the precision of the results. All estimated impacts are significant for the error components specification; the subject-specific impacts random from 0.09 to 0.12 standard deviations, and the average estimated impact is 0.125 standard deviations. The results of the OLS specification with robust standard errors are similar in magnitude, but only that for the average test score (0.144) is statistically significant, and only at the 10% level. However, when school fixed effects are added the OLS results are more precisely estimated; the estimated impacts are statistically significant for two of the three subjects (the exception being Chinese) and for the average over all subjects. The results are similar to those for the error components specification in that the range over the three subjects is from 0.08 to 0.12 standard deviations of the distribution of test scores, and the estimated average impact is 0.13.

[It may make sense that the strongest impact is on mathematics, since that subject may require more looking at the blackboard (which is harder to see for myopic

students) than do Chinese and science. Maybe look at GSCF pedagogy data to see if we can find this in the data.]

B. IV Estimates of the Impact of Wearing Eyeglasses. This subsection presents estimates of the impact of wearing eyeglasses for one year on student test scores. (A few of the students have worn eyeglasses for more than one year; of the 1,245 children with glasses, 199 had purchased them on their own, and of these 94 had purchased them about one year ago, 85 had purchased them two years ago, 18 had purchased them 3 years ago, and 2 had purchased them about 4 years ago, so only 105 out of the 1,245 children had them for more than one year.)[11] As explained above, random selection into the treatment school, conditional on having bad eyesight, is the instrumental variable. This IV has strong explanatory power; in the regressions that include only children with poor vision the R^2 of the first stage regression is 0.48 and the F-statistic is 277.3.

The results reported in Table 6 have the spring 2005 test score as the dependent variable. The first two rows present estimates of equation (1), which includes only students with poor vision, except that the sole explanatory variable is wearing eyeglasses instead of being in a program school. **[The error components model estimates are in fact simple random effects with school random effects; need to check whether anyone has written an error components model for IV estimation. If not, do the two stages manually using xtmixed and "block bootstrap" the standard errors. Note**

[11] Recall that only 59 children in the sample with bad vision had glasses, which implies that 140 of the 199 children who report having purchased eyeglasses on their own did not have bad vision as measured in the data. This could reflect a misdiagnosis that led their parents to purchase glasses for them, or measurement error either in the visual acuity variables or the variable that indicates wearing eyeglasses. Measurement error in reporting of wearing eyeglasses does not imply inconsistency since that variable is being instrumented. **[Need to think about measurement error in the visual acuity variable.]**

that comparison of error components model to school RE without IV (e.g. Tables 3 & 4) suggests that school RE underestimates the std. errors ("overestimates" precision) when looking only at kids with poor vision, but estimates that combine kids with good and poor vision are very similar for error components and school RE specifications.] The impacts are larger than the analogous results given in Table 4, which is to be expected since these results are estimates for the impact of actually having glasses, as opposed to only being offered eyeglasses, yet most of the estimated impacts are not significant. More precisely, the OLS estimates with robust standard errors are not statistically significant, and only the science score and the average score are significant for the error components specification, and the latter is significant only at the 10% level.

More precise estimates are obtained by adding students who do not have poor vision and estimating an equation similar to equation (1′), for which there are two explanatory variables, a dummy variable indicating poor vision and a dummy variable indicating that a student has poor vision and wears eyeglasses. These results are shown in the remaining rows of Table 6. As expected, the standard errors are much lower, but the estimated impacts are also somewhat lower. For the error components specification, the estimated impacts on Chinese and on the average test score are significant at the 5% level and moderately large (0.14 standard deviations for Chinese and 0.12 averaged over all scores). For OLS specification with robust standard errors, the estimated impacts are 0.09 for Chinese and math, and 0.12 for science, but none of these is significant; the estimated average effect is also 0.12, but also not significant. Finally, when school fixed effects are added to the OLS specification the results are very similar to those for the error components specification, except they are not as precisely estimated (the only impact that is significant even at the 10% level is that for Chinese).

Finally, Table 7 presents IV estimates of the impact of wearing glasses that use the differences in test scores from 2004 to 2005 as the dependent variable. Turning to the estimates based only on students with poor vision, the estimates for the two specifications are very similar, although the error components estimates are more precisely estimated. These estimates indicate that having eyeglasses can increase test scores from 0.07 standard deviations (for Chinese, but the estimated impact is not significant in either specification) to about 0.25 standard deviations (for math, and both estimates are significant). Averaging over all subjects, the error components model suggests that the impact is about 0.21 standard deviations, which is significant at the 5% level (the OLS estimate is 0.199, but with a t-statistic of only 1.34).

The remaining estimates in Table 7 add students with good vision. In the error components specification the impacts are very precisely estimated **[again, these are "simple" random effects models with school random effects]**. With impacts ranging from 0.11 for Chinese (not significant) to 0.18 for math and 0.17 for science (both significant at the 5% level). Averaging over all subjects yields an impact of having eyeglasses of 0.19 standard deviations, which is significant at the 1% level. For the OLS specification with robust standard errors, the estimated impacts are 0.08 for Chinese (not significant), 0.21 for math (not significant), and 0.19 for science (significant at the 10% level); averaging over all subjects also yields an impact of having eyeglasses of 0.19 standard deviations, but this impact is significant (t-statistic of 1.53).

Lastly, the OLS results with school fixed effects at the bottom of Table 7 show significant impacts that are very similar to those of the error components specification, except slightly less precisely estimated. To summarize this subsection, these estimates indicate that wearing eyeglasses for 8-9 months raises grade 4-6 student's test scores by 0.19 standard deviation of the distribution of test scores, which is an unusually large impact after such a short time. **[Check GSCF data to get an idea of what this implies in terms of additional time in school.]**

[We should compare our results with results based only on kids from the control group. We could show ordinary OLS estimates as well as IV estimates, perhaps using distance to the nearest place with an ophthalmologist as an IV].

VI. Robustness Checks

The estimates in the previous section are based on assumptions that could be challenged. In particular, the estimates that compare children with poor vision to children with good vision assume that providing eyeglasses to the former has no effect on the test scores of the latter, and all the estimates in Section V assumed that, after dropping the township pairs in which the randomization was compromised, the township pairs that remained did not suffer from any type of selection bias. This section checks whether these assumptions are valid.

Consider first the possibility that children with good vision were affected by the program. They could have been helped if their teachers devoted less time to helping students with poor vision, and/or if they learned from their now better performing classmates with poor vision. If this is the case, estimates based on equation (1′) and (1‴) would underestimate the true impact of the program on children with poor vision, since comparing students with poor vision to those with good vision overlooks the impact of the program on the latter. On the other hand, teachers may have been distracted from their general teaching duties by the need to monitor children who were provided glasses, or more generally if teachers devoted more of their attention to those children. This would lead to overestimation of the impact of the program on students who were offered glasses.

Table 8 presents estimates analogous to those for equations (1) and (1″), except that the sample is limited to children who had good vision, instead of to children who had poor vision. Estimates are presented for both the error components specification and the OLS specification with robust standard errors clustered at the township level. None of the 16 estimated program impacts is either large or statistically significant. The estimates range from -0.073 to 0.093, and none of them has a t-statistic larger than one. For the most precisely estimated impact on the average test score over all three subjects (the differenced OLS estimates with robust standard errors), the point estimate is -0.009 and the 95% confidence interval ranges from -0.106 to 0.089, ruling out estimated impacts of 0.09 or higher. Finally, estimates that allow the impact of the program to vary depending on the proportion of children with bad vision in a student's grade in his or her school (spillovers should be larger in classrooms where more children were provided eyeglasses) again show no effect of any kind (not shown in Table 8). Thus we conclude that no sizeable spillovers occurred, and thus the estimates presented in the previous section do not suffer from bias due to spillover effects.

Turn next to the possibility that the estimates presented thus far may be biased because the township pairs in which the program was implemented correctly may not be a random sample of the original set of township pairs. This is difficult to check for the estimates of being offered the program presented in Tables 4 and 5 because the program was not correctly implemented in the townships that were excluded from the estimates presented in those tables. More specifically, one could estimate the impact of being in a township in which all children with vision problems *should have been offered eyeglasses* (each of which should have been paired with a township that did not offer eyeglasses, which was the case in a little over half of the township pairs) but this is not the treatment effect that was estimated in Tables 4 and 5. However, one can still use instrumental variable methods to estimate the impact of wearing eyeglasses, as long as the initial assignment to receive eyeglasses has strong predictive power for wearing eyeglasses, since the initial assignment was randomized and this is a valid instrument. This is done in Tables 9 and 10.

The estimates in Table 9 are "level" estimates of the impact of wearing eyeglasses on test scores; they are the same as those in Table 6 except that the township pairs where the program was incorrectly implemented are included. With one weak exception, all estimates are statistically insignificant, which is not particularly surprising since most of the estimates in Table 6 were also statistically insignificant.[12] Yet some estimates in Table 9 are worrisome in that they are negative and of large size. For example, the estimated impact of equation (1) for the OLS specification with robust standard errors, averaged over all three subjects, is -0.322. Yet this estimate is not close to being

[12] The instrumental variable has strong explanatory power, with a t-statistic of 5.49 for the robust OLS specification and 15.07 for the error components specification.

statistically significant (the standard error is 0.299) and so the appropriate conclusion is that the estimated effects in Table 9 are uninformative. Indeed, the most precise estimates are usually positive, although still not statistically significant.

Finally, the estimates in Table 10 are "differenced" estimates. Several of these are statistically significant, and all those that are have the expected positive sign. Turning to estimates that average over all three subjects, the two with the most precise estimates (the error components specification and the robust OLS specification with school fixed effects, both for equation (1′)) show positive impacts of 0.17 and 0.18 standard deviations, which is very similar to the estimates for the restricted sample in Table 7. Thus it appears that restricting the sample to township pairs where the program was correctly implemented does not lead to biased estimates.

VII. Why Do Some Children Not Accept Eyeglasses?

As explained above, only 1066 (69.8%) of the 1528 students with poor vision in the program schools agreed to be fitted for eyeglasses, even though they were provided at no cost. The stated reasons for not accepting them are not very informative, the two most common reasons being "child refused" and "parents refused". This section presents simple probit estimates of the factors associated with accepting the eyeglasses. The results are exploratory, since the data available are somewhat limited.

Table 11 presents probit estimates of the factors associated with accepting the offer of free eyeglasses in the program schools. The most obvious variable to check is students' visual acuity; children whose eyesight is not very bad have less reason to wear glasses, while students with very bad eyesight have a greater need for them. As expected, better visual acuity (an average over both eyes) has a highly significant negative impact on accepting glasses. The visual acuity variable has a standard deviation of 0.234, which implies that a one standard deviation increase in visual acuity reduces the probability of accepting eyeglasses by 11.6 percentage points (0.234×0.494).

A more unexpected result is that girls have a much lower probability of accepting eyeglasses than boys: 73.6% of boys received eyeglasses while only 66.0% of girls received them. This is evident in the regression results, which show that girls have a 8.2 percentage point lower probability of receiving glasses, a highly significant difference. The reasons for this are not clear. The stated reasons for not accepting eyeglasses are very similar for boys and girls **[check for statistical significance]**.

Four other factors have significant impacts on the probability of accepting eyeglasses. First, the relatively few children with poor vision who were already wearing eyeglasses (49 out of 1528) were more likely to accept new ones; such children were 17.7 percentage points more likely to accept glasses. This is not surprising given that they were already convinced of the need for glasses, and many may have needed an updated prescription. Two other factors are that children from households headed by a schoolteacher or by a party cadre were less likely to accept glasses, and these effects were significant at the 1% and 10% levels, respectively. The sizes of these effects are very large, with children of schoolteachers 22.4 percentage points less likely to accept eyeglasses, and children of party cadres 35.2 percentage points less likely to accept them. It is quite strange, and ironic, that these authority figures seem to have doubts about the merits of eyeglasses. Fourth, students from wealthier townships were more likely to accept the eyeglasses offered; a one standard deviation increase in average township income increase the probability of accepting glasses by 7.1 percentage points. Perhaps the residents of wealthier townships are more accustomed to both children and adults wearing eyeglasses.

Finally, four other factors had no significant impact on acceptance of the eyeglasses offered. First, and somewhat surprisingly, more educated parents were no more likely to accept them (indeed, the point estimate is slightly negative). Second, the students' initial test scores had no effect. Third, the main ethnic minority group in these two counties, Tibetans (which constitute 14.5% of the students), where slightly less likely to accept the eyeglasses, but this effect is not statistically significant. Finally, there was no difference in acceptance by grade level.

[Perhaps add analysis of GSCF data, which are much richer, on the determinants of wearing eyeglasses among kids age 13-17. See what Emily has done. Check how close optometrists came to where kids lived, and if we have data on distances they had to travel to get the free glasses.]

VIII. Summary and Conclusion

Vision problems are a serious impediment to learning for about 10% of primary school age children in both developed and developing countries. Fortunately, almost all vision problems are easily corrected by providing children with correctly fitting eyeglasses. Virtually alll developed countries have in place a variety of programs to provide eyeglasses to children with vision problems **[need a source for this]**. In contrast, in most developing countries children with vision problems do not have eyeglasses, especially at the primary level.

This paper examines the impact of providing eyeglasses to children with poor vision in rural areas of Gansu province, one of China's poorest provinces. More specifically, a randomized control trial was implemented in 25 townships of two counties in that province, which included about 19,000 children in 165 schools, of which about 12% had poor vision. The results indicate that offering eyeglasses to children with poor vision increases their test scores (averaged over three subjects) by between 0.09 to 0.14 standard deviations of the distribution of those test scores, depending on the estimation method used. In fact, for about one third of these children, either they or their parents refused the offer of free eyeglasses, which implies that the impact of actually wearing the eyeglasses is about 50% higher than these estimates. Thus, as one would expect, instrumental variables estimates of the impact of wearing eyeglasses lead to estimates of between 0.12 and 0.22 standard deviations. These are rather large effects; similar tests given to children in grades 5 and 6 in Gansu province show that an addition year of schooling leads to an increase of 0.4 to 0.5 standard deviations of the distribution of test scores, which implies that these impacts are equivalent to one fourth to one half of a year of schooling. Thus providing eyeglasses is a relatively low cost and easily implementable intervention that could improve the academic performance of a substantial proportion of primary (and secondary) school students in developing countries.

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Table 1: Descriptive Statistics from Tianzhu and Yongdeng Counties

		Yongdeng	Tianzhu	Both Counties
Number of children in grades 4-6 in 2004-05		12,783	6,132	18,915
Children with vision problems		1,742 (13.6%)	786 (12.8%)	2,528 (13.4%)
Of which:				
Had glasses already		36 (2.1%)	23 (2.9%)	59 (2.3%)
Did not have glasses		1,706 (97.9%)	763 (97.1%)	2,469 (97.7%)
Test scores in spring 2004 (before intervention):				
Students without vision Problem				
Chinese		79.0	78.6	78.9
Mathematics		79.2	79.0	79.1
Science		80.8	80.6	80.8
Students with vision Problem				
Chinese		78.7	77.1	78.2
Mathematics		79.2	76.8	78.5
Science		80.8	80.2	80.6

Notes:

1. The data used in this table, and all following tables, exclude pairs of townships where the randomization plan was not correctly implemented.
2. Vision problem is defined as a visual acuity score < 4.9 in one or both eyes. As explained in the text, although the 249 children for whom one or both eyes had a score of 4.9 were offered glasses, only 17 (6.8%) accepted the glasses, so the analysis focuses on children for whom one or both eyes had a score of less than 4.9.

Table 2: Implementation of Gansu Vision Intervention Project

		Yongdeng	Tianzhu	Both Counties
Students in grades 4-6 in 2004-05 with vision problems		1,742	786	2,528
Of which:				
In control schools		889	111	1,000
In program schools		853	675	1,528
Students in program schools who:				
Accepted the offer to receive glasses		649	417	1,066
Did not accept the offer to receive glasses		204	258	462
Reasons given for not accepting glasses:				
Household head refused		91	54	145
Child refused		38	42	80
Cannot adjust to glasses		0	58	58
Eye disease 1		0	11	11
Optometrist not available		7	27	34
Eye disease 2		30	33	63
Eye problem cannot be corrected by glasses		0	5	5
Eye disease 3		0	1	1
Vision not correctable(?)		19	0	19
Child is handicapped		2	0	2
Missing		17	27	44

Notes:

1. The data used in this table, and all following tables, exclude pairs of townships where the randomization plan was not correctly implemented.
2. Vision problem is defined as a visual acuity score < 4.9 in one or both eyes.

Table 3: Check for Pre-Program Differences across Treatment and Control Groups (Differences in Spring 2004 Scores)

	Dependent Variable			
	Chinese	Math	Science	Average
Explanatory Variables				
Equation (1): Error Components Specification ***N* = 2,490**				
Treatment Township (β)	0.038 (0.126)	-0.055 (0.148)	0.046 (0.114)	0.015 (0.139)
Equation (1): OLS with Robust Covariance Matrix ***N* = 2,490**				
Treatment Township (β)	0.028 (0.099)	-0.058 (0.095)	-0.070 (0.072)	-0.039 (0.099)
Equation (1′): Error Components Specification ***N* = 18,598**				
Poor Vision (π)	0.037 (0.032)	0.069** (0.032)	0.037 (0.031)	0.056* (0.029)
Treatment Township (τ)	0.016 (0.098)	-0.028 (0.116)	0.077 (0.087)	0.029 (0.111)
Poor Vision×Treatment Township (β)	-0.005 (0.041)	-0.060 (0.042)	-0.038 (0.040)	-0.041 (0.038)
Equation (1′): OLS with Robust Covariance Matrix ***N* = 18,598**				
Poor Vision (π)	0.001 (0.030)	0.085** (0.041)	0.067 (0.040)	0.060** (0.026)
Treatment Township (τ)	0.061 (0.064)	0.047 (0.065)	0.088 (0.059)	0.077 (0.070)
Poor Vision×Treatment Township (β)	-0.042 (0.082)	-0.122 (0.072)	-0.116** (0.050)	-0.110 (0.065)
Equation (1′): OLS w/ Robust Covariance Matrix & School Fixed Effects ***N* = 18,598**				
Poor Vision (π)	0.042 (0.030)	0.090** (0.036)	0.064* (0.036)	0.077** (0.028)
Treatment Township (τ)	Not identified	Not identified	Not identified	Not identified
Poor Vision×Treatment Township (β)	0.015 (0.044)	-0.075 (0.059)	-0.070 (0.053)	-0.051 (0.049)

Notes: 1. Constant terms and strata dummy variables are not shown (to reduce clutter).

2. Standard errors are in parentheses. Error components specifications include township, school and grade error terms. OLS specification allow for both heteroscedasticity and clustering at the township level of unknown form. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

Table 4: Estimated Program Effect: Level Results, without Covariates

	Dependent Variable			
	Chinese	Math	Science	Average
Explanatory Variables				
Equation (1): Error Components Specification *N* = 2,473				
Treatment Township (β)	0.095 (0.126)	0.116 (0.125)	0.164 (0.110)	0.154 (0.129)
Equation (1): OLS with Robust Covariance Matrix *N* = 2,473				
Treatment Township (β)	0.074 (0.093)	0.078 (0.086)	0.097 (0.090)	0.103 (0.096)
Equation (1′): Error Components Specification *N* = 18,501				
Poor Vision (π)	0.010 (0.031)	0.012 (0.031)	0.042 (0.031)	0.027 (0.029)
Treatment Township (τ)	-0.059 (0.109)	0.003 (0.085)	0.036 (0.112)	-0.011 (0.119)
Poor Vision×Treatment Township (β)	0.085** (0.040)	0.067* (0.040)	0.056 (0.040)	0.087** (0.037)
Equation (1′): OLS with Robust Covariance Matrix *N* = 18,501				
Poor Vision (π)	-0.006 (0.045)	0.021 (0.030)	0.058 (0.038)	0.030 (0.036)
Treatment Township (τ)	0.001 (0.072)	0.065 (0.053)	0.093 (0.096)	0.066 (0.085)
Poor Vision×Treatment Township (β)	0.061 (0.064)	0.012 (0.058)	0.008 (0.057)	0.033 (0.060)
Equation (1′): OLS w/ Robust Covariance Matrix & School Fixed Effects *N* = 18,501				
Poor Vision (π)	-0.014 (0.029)	0.010 (0.030)	0.048 (0.032)	0.018 (0.023)
Treatment Township (τ)	Not identified	Not identified	Not identified	Not identified
Poor Vision×Treatment Township (β)	0.099* (0.050)	0.054 (0.063)	0.041 (0.050)	0.081 (0.055)

Notes: 1. Constant terms and strata dummy variables are not shown (to reduce clutter).

2. Standard errors are in parentheses. Error components specifications include township, school and grade error terms. OLS specification allow for both heteroscedasticity and clustering at the township level of unknown form. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

Table 5: Estimated Program Effect: Differenced Results, without Covariates

	Dependent Variable			
	Chinese	Math	Science	Average
Explanatory Variables				
Equation (1″): Error Components Specification N = 2,473				
Treatment Township (β)	0.058 (0.133)	0.160 (0.165)	0.130 (0.109)	0.132 (0.117)
Equation (1″): OLS with Robust Covariance Matrix N = 2,473				
Treatment Township (β)	0.048 (0.114)	0.113 (0.129)	0.170*** (0.059)	0.143 (0.101)
Equation (1‴): Error Components Specification N = 18,500				
Poor Vision (π)	-0.026 (0.037)	-0.054 (0.037)	0.004 (0.037)	-0.028 (0.029)
Treatment Township (τ)	-0.074 (0.089)	0.013 (0.087)	-0.028 (0.074)	-0.037 (0.081)
Poor Vision×Treatment Township (β)	0.091* (0.047)	0.120** (0.047)	0.097** (0.047)	0.125*** (0.037)
Equation (1‴): OLS with Robust Covariance Matrix N = 18,500				
Poor Vision (π)	-0.006 (0.062)	-0.064 (0.049)	-0.010 (0.070)	-0.030 (0.051)
Treatment Township (τ)	-0.059 (0.059)	0.019 (0.052)	0.004 (0.062)	-0.011 (0.050)
Poor Vision×Treatment Township (β)	0.104 (0.088)	0.130 (0.079)	0.128 (0.081)	0.144* (0.072)
Equation (1‴): OLS w/ Robust Covariance Matrix & School Fixed Effects N = 18,500				
Poor Vision (π)	-0.056 (0.052)	-0.078* (0.043)	-0.017 (0.060)	-0.058 (0.041)
Treatment Township (τ)	Not identified	Not identified	Not identified	Not identified
Poor Vision×Treatment Township (β)	0.083 (0.069)	0.123** (0.060)	0.116* (0.067)	0.130** (0.054)

Notes: 1. Constant terms and strata dummy terms are not shown (to reduce clutter).

2. Standard errors are in parentheses. Error components specifications include township, school and grade error terms. OLS specification allow for both heteroscedasticity and clustering at the township level of unknown form. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

Table 6: Effect of Eyeglasses: Level IV Results, without Covariates

	Dependent Variable			
	Chinese	Math	Science	Average
Explanatory Variables				
Equation (1): Error Components Specification ***N*** **= 2,473 [NOT QUITE RIGHT]**				
Treatment Township (β)	0.128 (0.124)	0.144 (0.113)	0.265** (0.129)	0.224* (0.127)
Equation (1): OLS with Robust Covariance Matrix ***N*** **= 2,473**				
Treatment Township (β)	0.103 (0.132)	0.109 (0.124)	0.135 (0.121)	0.144 (0.134)
Equation (1′): Error Components Specification ***N*** **= 18,500 [NOT QUITE RIGHT]**				
Poor Vision (π)	-0.014 (0.033)	0.008 (0.033)	0.046 (0.034)	0.017 (0.032)
Poor Vision×Treatment Township (β)	0.139** (0.061)	0.078 (0.061)	0.063 (0.061)	0.116** (0.058)
Equation (1′): OLS with Robust Covariance Matrix ***N*** **= 18,500**				
Poor Vision (π)	-0.007 (0.054)	-0.012 (0.048)	0.011 (0.055)	-0.003 (0.055)
Poor Vision×Treatment Township (β)	0.089 (0.107)	0.090 (0.107)	0.117 (0.111)	0.122 (0.113)
Equation (1′): OLS w/ Robust Covariance Matrix & School Fixed Effects ***N*** **= 18,501**				
Poor Vision (π)	-0.015 (0.029)	0.009 (0.031)	0.059 (0.070)	0.017 (0.024)
Poor Vision×Treatment Township (β)	0.142* (0.073)	0.077 (0.089)	0.048 (0.032)	0.115 (0.080)

Notes: 1. Constant terms and strata dummy terms are not shown (to reduce clutter).

2. Standard errors are in parentheses. Error components specifications include township, school and grade error terms. OLS specification allow for both heteroscedasticity and clustering at the township level of unknown form. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

3. The instrumental variable for having eyeglasses is a dummy variable for being selected into the program and having poor vision.

Table 7: Effect of Eyeglasses: Differenced IV Results, without Covariates

	Dependent Variable			
	Chinese	Math	Science	Average
Explanatory Variables				
Equation (1): Error Components Specification ***N*** **= 2,473 [NOT QUITE RIGHT]**				
Treatment Township (β)	0.060 (0.125)	0.197 (0.150)	0.264** (0.124)	0.214** (0.109)
Equation (1): OLS with Robust Covariance Matrix ***N*** **= 2,473**				
Treatment Township (β)	0.067 (0.162)	0.185 (0.189)	0.236*** (0.075)	0.199 (0.148)
Equation (1′): Error Components Specification ***N*** **= 18,500 [NOT QUITE RIGHT]**				
Poor Vision (π)	-0.049 (0.039)	-0.080** (0.040)	-0.017 (0.039)	-0.057* (0.031)
Poor Vision×Treatment Township (β)	0.113 (0.071)	0.179** (0.072)	0.166** (0.070)	0.186*** (0.057)
Equation (1′): OLS with Robust Covariance Matrix ***N*** **= 18,500**				
Poor Vision (π)	0.022 (0.080)	-0.075 (0.066)	-0.015 (0.062)	-0.027 (0.062)
Poor Vision×Treatment Township (β)	0.082 (0.151)	0.207 (0.150)	0.187* (0.108)	0.193 (0.126)
Equation (1′): OLS w/ Robust Covariance Matrix & School Fixed Effects ***N*** **= 18,501**				
Poor Vision (π)	-0.057 (0.053)	-0.079* (0.043)	-0.018 (0.061)	-0.059 (0.042)
Poor Vision×Treatment Township (β)	0.120 (0.099)	0.176* (0.088)	0.165* (0.097)	0.187** (0.080)

Notes: 1. Constant terms and strata dummy terms are not shown (to reduce clutter).

2. Standard errors are in parentheses. Error components specifications include township, school and grade error terms. OLS specification allow for both heteroscedasticity and clustering at the township level of unknown form. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

3. The instrumental variable for having eyeglasses is a dummy variable for being selected into the program and having poor vision.

Table 8: Estimated Program Effects for Students with Good Vision

	Dependent Variable			
	Chinese	Math	Science	Average
Explanatory Variables				
Level Estimates: Error Components Specification ***N*** **= 16,045**				
Treatment Township (β)	-0.060 (0.109)	-0.005 (0.085)	0.032 (0.115)	-0.017 (0.121)
Level Estimates: OLS with Robust Covariance Matrix ***N*** **= 16,045**				
Treatment Township (β)	-0.000 (0.072)	0.063 (0.052)	0.093 (0.096)	0.065 (0.085)
Differenced Estimates: Error Components Specification ***N*** **= 16,044**				
Treatment Township (τ)	-0.073 (0.083)	0.008 (0.087)	-0.036 (0.080)	-0.040 (0.080)
Differenced Estimates: OLS with Robust Covariance Matrix ***N*** **= 16,044**				
Treatment Township (τ)	-0.053 (0.057)	0.020 (0.048)	0.002 (0.061)	-0.009 (0.047)
Maybe Add Results Based on Proportion of Kids with Bad Vision				

Notes: 1. Constant terms and strata dummy variables are not shown (to reduce clutter).

2. Standard errors are in parentheses. Error components specifications include township, school and grade error terms. OLS specification allow for both heteroscedasticity and clustering at the township level of unknown form. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

Table 9: Effect of Eyeglasses: Level IV Results Using Full Sample

	Dependent Variable			
	Chinese	Math	Science	Average
Explanatory Variables				
Equation (1): Error Components Specification ***N*** **= 4,293 [NOT QUITE RIGHT]**				
Treatment Township (β)	-0.170 (0.145)	-0.185 (0.139)	-0.080 (0.149)	-0.171 (0.156)
Equation (1): OLS with Robust Covariance Matrix ***N*** **= 4,293**				
Treatment Township (β)	-0.232 (0.230)	-0.266 (0.263)	-0.275 (0.252)	-0.322 (0.299)
Equation (1′): Error Components Specification ***N*** **= 32,588 [NOT QUITE RIGHT]**				
Poor Vision (π)	-0.023 (0.041)	0.001 (0.041)	0.064 (0.042)	0.015 (0.039)
Poor Vision×Treatment Township (β)	0.148* (0.080)	0.078 (0.080)	-0.016 (0.082)	0.092 (0.076)
Equation (1′): OLS with Robust Covariance Matrix ***N*** **= 32,588**				
Poor Vision (π)	0.187 (0.129)	0.183 (0.134)	0.196 (0.120)	0.236 (0.155)
Poor Vision×Treatment Township (β)	-0.237 (0.229)	-0.253 (0.247)	-0.243 (0.231)	-0.306 (0.280)
Equation (1′): OLS w/ Robust Covariance Matrix & School Fixed Effects ***N*** **= 32,588**				
Poor Vision (π)	-0.031 (0.046)	-0.004 (0.045)	0.060 (0.051)	0.011 (0.050)
Poor Vision×Treatment Township (β)	0.163 (0.101)	0.089 (0.116)	-0.010 (0.112)	0.100 (0.117)

Notes: 1. Constant terms and strata dummy terms are not shown (to reduce clutter).

2. Standard errors are in parentheses. Error components specifications include township, school and grade error terms. OLS specification allow for both heteroscedasticity and clustering at the township level of unknown form. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

3. The instrumental variable for having eyeglasses is a dummy variable for being selected into the program and having poor vision.

Table 10: Effect of Eyeglasses: Differenced IV Results Using Full Sample

	Dependent Variable			
	Chinese	Math	Science	Average
Explanatory Variables				
Equation (1): Error Components Specification *N* = 4,293 [NOT QUITE RIGHT]				
Treatment Township (β)	0.023 (0.133)	0.170 (0.158)	0.081 (0.142)	0.096 (0.123)
Equation (1): OLS with Robust Covariance Matrix *N* = 4,293				
Treatment Township (β)	0.021 (0.158)	0.202 (0.182)	0.019 (0.159)	0.077 (0.155)
Equation (1′): Error Components Specification *N* = 32,587 [NOT QUITE RIGHT]				
Poor Vision (π)	-0.024 (0.047)	-0.138*** (0.048)	0.004 (0.047)	-0.061 (0.038)
Poor Vision×Treatment Township (β)	0.086 (0.092)	0.281*** (0.094)	0.060 (0.093)	0.172** (0.075)
Equation (1′): OLS with Robust Covariance Matrix *N* = 32,587				
Poor Vision (π)	0.018 (0.083)	-0.103 (0.080)	0.033 (0.082)	-0.007 (0.075)
Poor Vision×Treatment Township (β)	0.063 (0.167)	0.244 (0.173)	0.025 (0.166)	0.112 (0.152)
Equation (1′): OLS w/ Robust Covariance Matrix & School Fixed Effects *N* = 32,587				
Poor Vision (π)	-0.033 (0.057)	-0.143** (0.060)	-0.000 (0.063)	-0.067 (0.052)
Poor Vision×Treatment Township (β)	0.096 (0.118)	0.288** (0.124)	0.066 (0.127)	0.181* (0.103)

Notes: 1. Constant terms and strata dummy terms are not shown (to reduce clutter).

2. Standard errors are in parentheses. Error components specifications include township, school and grade error terms. OLS specification allow for both heteroscedasticity and clustering at the township level of unknown form. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

3. The instrumental variable for having eyeglasses is a dummy variable for being selected into the program and having poor vision.

Table 11: Probit Estimates of Factors Associated with Accepting Eyeglasses

Variable	Mean	Coefficient	Marginal Effects
Average visual acuity	4.551	-1.467*** (0.546)	-0.494*** (0.197)
Female	0.500	-0.242*** (0.059)	-0.082*** (0.019)
Had glasses before program began	0.032	0.662* (0.379)	0.177* (0.077)
Household head is a teacher	0.016	-0.594*** (0.232)	-0.224*** (0.094)
Household head is village leader (cadre)	0.016	-0.923* (0.484)	-0.352* (0.182)
Township per cap. income, 2003 (yuan/yr)	1511.5	0.00045** (0.00019)	0.00015** (0.00006)
Head years of schooling	8.58	-0.012 (0.024)	-0.004 (0.008)
Test score, spring 2004 (avg. for 3 subjects)	-0.187	-0.012 (0.074)	-0.004 (0.025)
Tibetan	0.145	-0.038 (0.140)	-0.013 (0.048)
Grade in 2003-2004 (3, 4 or 5)	4.27	-0.078 (0.127)	-0.026 (0.043)
Observations		1497	

Notes: 1. Constant term is not shown (to reduce clutter).

2. Standard errors are in parentheses. The specification allows for both heteroscedasticity and clustering at the township level of unknown form. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

3. The sample consists of all children in the program schools in grades 4-6 in 2004-05 who were deemed to have poor vision (one or both eyes with visual acuity below 4.9).

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