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VISUALIZING DEVELOPMENT: EYEGLASSES AND ACADEMIC PERFORMANCE IN RURAL PRIMARY SCHOOLS IN CHINA

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Visualizing Development: Eyeglasses and Academic Performance in Rural Primary Schools in China

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Abstract:

About 10% of primary school students in developing countries have poor vision, but very few of them wear glasses. Almost no research examines the impact of poor vision on school performance, and simple OLS estimates are likely to be biased because studying harder often adversely affect one's vision. This paper presents results from a randomized trial in Western China that offered free eyeglasses to 1,528 rural primary school students. The results indicate that wearing eyeglasses for one year increased average test scores of students with poor vision by 0.15 to 0.22 standard deviations, equivalent to the learning acquired from an additional 0.33-0.50 years of schooling, and that the benefits are greater for under-performing students. A simple cost-benefit analysis suggests very high economic returns to wearing eyeglasses, raising the question of why such investments are not made by most families. We find that girls are more likely to refuse free eyeglasses, and that lack of parental awareness of vision problems, mothers' education, and economic factors (expenditures per capita and price) significantly affect whether children wear eyeglasses in the absence of the intervention.

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1. Introduction

Most economists agree that higher levels of education increase economic growth (Barro, 1991; Mankiw et al., 1992; Hanushek and Kimko, 2000; Krueger and Lindahl, 2001; Sala-i-Martin et al., 2004; Hanushek and Woessmann, 2008), raising incomes and the quality of life. Support for education is also strong among the international development community. Two of the eight Millennium Development Goals from the United Nations Millennium Summit in 2000 focus on education: all children should complete primary school, and gender equality should prevail at all education levels.

Yet school enrollment may have little effect on economic growth and individuals' incomes if children do not learn effectively while they are in school. Although economists and other social scientists have identified education policies that increase school enrollment, much less is known about how to increase student learning (Glewwe and Kremer, 2006). Recently, randomized control trials conducted by development economists similar to the one reported in this paper have begun to produce valuable evidence on the effect of specific interventions on student learning (see for example, Duflo, Hanna, and Ryan, forthcoming; Glewwe, Kremer, and Ilias, 2010; Glewwe, Kremer, and Moulin, 2009; Banerjee et al, 2007). These interventions, and most education policy reforms, have focused on improving the quality of schools and teachers—the supply side of education.

Much less attention has been paid to increasing students' capacity and motivation to learn, which often reflects decisions that parents make on their children's behalf. Researchers have found that health interventions, such as school meals and deworming programs, increase attendance and enrollment (Afridi, 2011; Vermeersch and Kremer, 2004; Miguel and Kremer, 2004), but did not find evidence that these school-level inter-ventions increase learning as measured by test scores. One study did find that reducing iron-deficiency among children in a poor region of China raised math test scores (Luo et al., forthcoming). If learning can be improved significantly with low-cost investments, then it is important for policy formulation to understand why these investments are not made. One possibility is lack of information; Jensen (2010) finds that simply informing students about the likely returns to further education increases years of schooling; our study also finds an important potential role of lack of information in explaining apparently suboptimal behavior.

This paper examines a specific health-related intervention with the potential to increase student learning in developing countries that, to date, has received little attention: providing eyeglasses to primary school students with vision problems. About 10% of primary school age children in developing countries have vision problems. In almost all cases these problems can be corrected with properly fitted eyeglasses, but very few of these children actually wear eyeglasses. This paper presents results for a randomized trial in Western China that offered free eyeglasses to children in grades 4, 5 and 6. It estimates the impact of being offered eyeglasses and, because one third of those offers were turned down, it also estimates the impact of wearing eyeglasses. We find that offering free eyeglasses to students with poor vision increases average test scores by 0.11 to 0.15 standard deviations, and actually wearing eyeglasses for one year increases average test scores by 0.15 to 0.22 standard deviations, equivalent to the learning acquired from an additional 0.33-0.50 years of schooling. An increase of time in school of this magnitude leads to higher life cycle wages, the present value of which easily exceeds the cost of the glasses.

These findings imply that households fail to make high-return investments, which raises an important policy-relevant question: What explains this failure? We study the determinants of children accepting the free eyeglasses offered by the project and also use a richer dataset on rural children from the same province to examine the determinants of wearing glasses absent the intervention. We find that both information failures, i.e., lack of awareness of vision problems, and credit constraints appear to be important factors.

The rest of the paper is organized as follows. Section 2 provides background information on education in China and vision problems among school-age children, and reviews the small literature on vision problems and student performance in developing countries. Section 3 describes the randomized trial and the data collection. The next two sections present the methodology used to estimate program impacts and the results, respectively. Section 6 checks the robustness of the results, and Section 7 investigates whether the results vary by student characteristics. Section 8 presents estimates that explore why some children did not accept the free eyeglasses, and more generally why most children with poor vision do not wear eyeglasses absent the intervention. A final section summarizes the results and provides recommendations for further research.

2. Background and Literature Review

This section introduces relevant aspects of primary education in rural China, and reviews the literature on the extent of vision problems among primary school students in developing countries, and how those problems affect students' academic performance.

A. Primary Education in Rural China. China has achieved nearly universal primary school enrollment. In the year 2000, only four percent of adults aged 25 to 29 did not have 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. Yet the rural poor and some minority populations continue to face difficulties in meeting this schooling goal (Hannum et al., 2008, Hannum, Park, and Cheng, 2007).

In rural areas of Western China, nearly all children attend the nearest public primary school, located in their village or a nearby village. Teachers are allocated to schools within the county by the county educational bureau, and the county government pays their salaries. Thus, disparities in primary school quality within counties are usually modest (Li et al., 2009), reducing the incentive to bypass the local school. In China, the Center for Disease Control under each county's Health Bureau is charged with conducting physical exams of all students, including eye exams. In principle, these exams should be conducted annually for all students, but budgetary and staff constraints cause many schools to conduct physical exams only once every two or three years. The results of these exams are given to teachers, who are expected to convey the information to parents.

B. Vision Problems and School Performance. Very little data exist on the vision problems of 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 constitute about 97% of these children's vision problems. Almost all refraction errors can be corrected with properly fitted eyeglasses, but most children with these problems in low income countries do not have glasses. Zhao et al. (2000) found that, in one district in Beijing, 12.8% of children age 5-15 years had vision problems, of which 90% were refraction errors. Only 21% of the children with vision problems had glasses. In China, He et al. (2007) found that 36.8% of 13-year-olds and 53.9% of 17-year-olds in middle schools in a county in Guangdong, a rich southern province, had myopia. Less than half had eyeglasses. Rural children with vision problems living in poor, remote areas and attending primary schools are even less likely to have glasses, as will shown below. In China, a commonly held (but mistaken) view is that wearing glasses causes children's vision to deteriorate faster.

The lack of data on children's vision problems in developing countries has led to very little research on the impact of poor vision on students' academic performance. Only two published studies exist. First, Gomes-Neto et al. (1997) found that primary school children in Northeast Brazil with vision problems had a 10 percentage point higher probability of dropping out, an 18 percentage point higher probability of repeating a grade, and scored 0.2 to 0.3 standard deviations lower on achievement tests. Yet these estimates could be biased; to the extent that some of these children wore glasses, their vision could be correlated with unobserved factors that affect learning, such as parental preferences for educated children. Also, even if none of them wore glasses, their vision can be affected by their home environment (e.g. lighting quality) and their daily activities, including time spent studying and doing homework. Thus their vision may be correlated with unobserved factors that directly affect school performance (e.g. hours studying), leading to biased estimates. Second, Hannum and Zhang (2008), using data from the Gansu Survey of Children and Families (described more below) and propensity score matching, find that for children with poor vision aged 13-16, wearing eyeglasses increases math and literacy test scores significantly (by 0.27 and 0.43 standard deviations) but does not increase language scores. Unfortunately, they could not fully address the problem of self-selection in wearing glasses; indeed, they show that wearing glasses tends to be associated with higher socio-economic status and greater academic achievement and engagement.

3. 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 Center for Disease Control of Gansu's Bureau of Health, implemented a randomized trial to examine the impact of providing eyeglasses to primary school students with poor vision in Yongdeng and Tianzhu counties. The project covered nearly all grade 4-6 students in primary schools in these two counties.

Gansu Province is in northwestern China. It is geographically diverse, including areas of the Loess Plateau, the Gobi desert, mountainous areas, and vast grasslands. In 2004, the year of the intervention, its population was 25.4 million, about three fourths of whom live in rural areas (National Bureau of Statistics, 2005). In 2004, Gansu ranked 30th out of 31 provinces in rural per capita disposable income, with only Tibet having a lower mean income (National Bureau of Statistics, 2005). Using official poverty lines, a World Bank report found that 22.7 percent of Gansu's rural population was poor in 1996, compared to 6.3 percent for China as a whole (World Bank, 2000).

Yongdeng and Tianzhu counties were chosen for the study because they are typical rural counties in Gansu, are located within several hours drive of Lanzhou (the provincial capital), which facilitated close monitoring by Gansu's Center for Disease Control (CDC), and have CDC staff who could implement the project effectively. Tianzhu is a Tibetan minority autonomous district under the Wuwei Municipality. Its population was 217,000 in 2004, 15% of whom were in urban areas. In the 2000 population census, 63% of

Tianzhu's population were Han Chinese and 30% were Tibetan. Yongdeng is more populous than Tianzhu, despite having a similar land area, and is part of Lanzhou Municipality. Its population was 514,000 in 2004, of whom 13% were in urban areas. Nearly all were Han Chinese. Among counties in Gansu, Yongdeng and Tianzhu both fall in the middle in terms of economic development, ranking 30th and 48th by GDP per capita among 87 county-level units (including urban districts) in 2004.[1]

Yongdeng County consists of 23 townships, (including the county seat) of which 18 participated in the program. These 18 townships have 155 primary schools. Nine of the 18 townships were randomly assigned to the eyeglasses intervention in 2004, and the other nine were assigned to the control group. Tianzhu consists of 22 townships, of which 19 participated in the program. These 19 townships have 101 primary schools. Ten of Tianzhu's 19 townships were randomly assigned to the program in 2004, and the other nine were the control group. In both counties, the excluded townships include the county seat (the counties' main urban centers, where incomes are higher and eyeglasses are easier to obtain) and a few townships in sparsely populated remote locations.

Random assignment was implemented as follows. In each county, all included townships were ranked by income per capita in 2003. Starting with the first two town-ships (the two wealthiest), one was randomly assigned to be a treated township and the other was assigned to the control group; this was repeated for all subsequent township pairs. In Tianzhu, the 19th township (the poorest) was not paired with any other township;

[1] All figures in this paragraph except Tianzhu census data are from Gansu Statistical Yearbook (2005). Tianzhu census data are from Wikipedia: "Tiannzhu Tibetan Autonomous County" (accessed Nov. 23, 2011).

it was randomly assigned to the treatment group. In each township primary schools were either all assigned to the treatment group or all assigned to the control group.[2]

A baseline survey was conducted at the end of the 2003-04 school year (June of 2004) to collect data on student characteristics, academic test scores, and visual acuity. Data were collected from both treatment and control schools from all students finishing grades 1-5 in June of 2004. The treatment school students who would be entering grades 4-6 in the fall of 2004 and had poor vision were offered free eyeglasses. In each county, an optometrist was hired later that summer to travel to each township to conduct more in-depth eye tests for students who accepted the offer and had permission from their parents. If poor vision was confirmed, they were prescribed appropriate lenses. Students had a limited choice of colors and styles for their eyeglass frames. The Gansu CDC then ordered all of the eyeglasses from a company with an established quality reputation. The fall semester of 2004 began on August 26th, and most of the eligible students who accepted the offer received eyeglasses by mid-September. At the end of the 2004-05 school year (late June or early July of 2005), exam scores for the 2004 fall semester and the 2005 spring semester were collected.

Unfortunately, in a few cases students in control townships were given eyeglasses because, after providing the eyeglasses in the treatment townships, the remaining funds were used to buy eyeglasses for students with poor vision in the paired control township. This occurred in two control townships in Tianzhu[3] and three control townships in Yongdeng. Finally, in one township pair in Tianzhu no one in the treatment township

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[2]Primary schools with less than 100 students were excluded from the project to avoid high travel costs to a few very remote schools. Students in such schools are only 6% of primary students in the two counties.
[3] 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. Excluding it and its matched pair (or including them but dropping these four students), has very little effect on the results.

was offered glasses while about one third of children with poor vision in the control township were offered glasses, so that there was a "role reversal" in this pair. Because this reversal may have been deliberate (though we have no evidence of this), this pair is also dropped from the analysis. This leaves six pairs of townships in Yongdeng and six pairs (plus the poorest township, the one randomly assigned to be treated) in Tianzhu for which the randomization was done according to the plan. Most of the regression analysis is limited to these 25 townships, which together contain about 19,000 students from 165 schools (103 in treatment townships and 62 in control townships).[4] Several robustness checks are presented that include the townships where the randomization was compromised.

B. Data. Four sources of data are used in the analysis: 1) school records on basic student characteristics and exam scores before and after the intervention; 2) results of health exams, including vision tests, conducted by the county CDC in each primary school before eyeglasses were provided; 3) information from optometrists' records on the students fitted for eyeglasses; and 4) data from the Gansu Survey of Children and Families (GSCF), a longitudinal study of children in rural Gansu that is separate from the Gansu Vision Intervention Project. The basic information in the school records include students' grades during the 2003-04 and 2004-05 school years, students' sex, ethnicity, and birthdate, and the occupation and education level of the head of each students' household (usually the father). Scores on exams (Chinese, math and science) given at the end of each semester in each grade were also collected.[5]

[4] 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.
[5] In some schools, these exam scores are averages of several exams, including an end of semester exam.

One important characteristic of the test score variables has important implications for the analysis: in many cases schools design their own exams, so the test scores are not always comparable across schools. Given random assignment of townships to the treatment and control groups, this non-comparability of exams across schools does not cause biased estimates, but it does add noise to the data, akin to a school random effect, that must be addressed in estimation. This is discussed in detail in Section 4.

The school health data include whether a student wears glasses (and if so, the student's grade when he or she first wore them), the student's height, weight and hemoglobin count, and at least one measurement of vision for each eye (students who received glasses have additional measurements related to fitting them with eyeglasses). In China, doctors usually conduct eye exams by asking patients to read (with one eye covered) a standard eye chart from 5 meters away. The chart has 12 rows of the letter E facing in different directions; the top row has very large E's, and each subsequent row has smaller E's. If the patient can read the 10th row, the normal level, his/her eyesight is 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 not the second, 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 exists only for children who were offered eyeglasses; it includes whether the child was fitted for eyeglasses, and if not, the reason eyeglasses were not provided (about one third declined the offer of eyeglasses, and some had vision problems that could not be corrected with eyeglasses).

Lastly, this paper also uses data from the Gansu Survey of Children and Families (GSCF), which was conducted in rural areas of Gansu province. The GSCF was first conducted in 2000 for a random sample of two thousand children aged 9-12. A second wave was conducted in 2004; only 131 of the original 2000 children were not re-interviewed.[6] In addition, 886 oldest younger siblings of the original 2000 children, if they were 8 years old or older, were also interviewed in 2004. The GSCF collected detailed information on vision and wearing eyeglasses from the both sets of children and their parents, as well as data on lighting conditions at home and at school, the cost and availability of eyeglasses, and many household and village characteristics. In addition to self-reported vision data, the 2004 GSCF also collected objective measurements of each child's eyesight through an eye exam, for both the originally sampled children and the 886 younger siblings, conducted by professionally trained staff from the Gansu CDC.

C. Descriptive Statistics. Table 1 presents descriptive statistics for the 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, 2,529 (13.4%) had poor vision in the sense that either the left eye or the right eye (or both) had a visual acuity score below 4.9.[7] Only 2.3% of the students in these counties with vision problems (59 out of 2,529) already had eyeglasses. Those with vision problems had slightly lower scores than those without problems for all three subjects (78.2% vs. 78.9% for Chinese, 78.5% vs. 79.1% for mathematics, and 80.6% vs. 80.8% for science) at the end of the spring 2004 semester (1-2 months before the program began).

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[6] The reasons children were not reinterviewed include: 108 had moved out of the counties where they had resided in 2000; 8 died; 4 were seriously ill; 2 had parents who divorced; 1 household refused to be re-interviewed; and 8 children were not reinterviewed for unknown reasons.

[7] 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. 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 data in Table 1 suggest that vision problems have little effect on students' academic performance. Indeed, simple t-tests show, for both counties separately and when combined, that none of the above-mentioned small differences in test scores is significant. But this conclusion may be misleading because study habits 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 many hours doing activities with the eyes focused on objects about 1 meter away) can cause myopia. Thus, students who study more 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. Among the grade 1 children in the data, very few have poor vision (only 2.9% are classified as having a visual acuity score below 4.8 in one or both eyes), but this increases dramatically as children continue in school (7.0% of students in grade 3, and 15.5% in grade 5). Thus children's grade 1 test scores are unlikely to be seriously affected by vision problems because so few have poor vision, but differences in visual acuity among older students would reflect, in part, time spent studying. OLS regressions of current mean (over both eyes) visual acuity on average test scores (over Chinese, math and science) in grade 1, controlling for school fixed effects, current grade, parents' education and occupation on the sample children in grades 3-5 in the 2003-04 school year show a *negative* impact that is significant at the 10% level. This suggests that visual acuity is negatively affected by increased study, so 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 (since students with good vision tend to study less). Similarly, for the GSCF data, a probit regression of poor vision in 2004 on study habits in 2000 (that (controls for sex, age, parental education and expenditure), yields a significantly (10% level) negative impact of studying on visual acuity four years later.

Table 2 presents information on how the Gansu Vision Intervention Project was implemented for the 2,529 students with poor vision. These figures exclude the township pairs for which the randomization was improperly implemented. Of these, 1,528 were in the program schools and so were offered eyeglasses (those who already had eyeglasses were offered new ones), while the 1,001 in the control group were not offered glasses. Of the 1,528 students offered glasses, 1,066 (69.8%) accepted them and the other 462 declined. The main reasons given for declining the offer were objection of the household head (145) and refusal by the child (80).

4. Methodology

Almost all primary school age children in Gansu province are in school; the GSCF data from the year 2000 show that only 1.4% of children age 9-12 were not enrolled in school. Thus, provision of eyeglasses cannot raise 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 greatly simplifies analysis of the impact of the project on student learning. To ease interpretation, all estimates in this paper use standardized test scores as the dependent variable; test scores are standardized by subtracting the mean and then dividing by the (student level) standard deviation, using the control schools' mean and standard deviation, separately for each subject and grade.

A. Estimation of the Impact of the Offer of Eyeglasses. The simplest estimate of the program impact on children in grades 4-6 with poor vision is a t-test that compares the mean test scores of the children with poor vision enrolled in the program schools with the same mean for the children with poor vision in the control schools. Technically, this estimates the impact of the *offer* to receive eyeglasses (intention to treat effect), not the impact of the eyeglasses themselves.

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

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

where u is a residual that is uncorrelated with P due to randomized program assignment. Reflecting the sample design, all regressions include a dummy variable for each pair of townships within which randomization was done (not shown in equation(1)). See Bruhn and McKenzie (2009) for a justification of this approach.

Estimates of β in equation (1) use only students with poor vision. To obtain more precise estimates of β one can be use an estimation method that adds students with good eyesight. Intuitively, this "double difference" method 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. The equation to be estimated is:

$$T = \alpha + \pi PV + \tau P + \beta PV{*}P + u \qquad (2)$$

where PV is a dummy variable indicating poor vision. In this specification the program's impact on students with good vision (PV = 0) will be τ, which one would expect to be zero, and the program's impact on students with poor vision will be τ + β, which equals β if, as expected, τ equals zero. The τ coefficient is also a check on the randomization; if the schools that participated in the program were better (worse) than average, then τ would be positive (negative).[8] Finally, the estimate of π measures the impact of poor vision on test scores, which one would expect to be negative. Yet this estimate will be biased toward zero because students who study more are likely to have worse vision. Fortunately, correlation between u and PV does not lead to bias in the estimate of the program impact (β),[9] and neither does random measurement error in PV (see Appendix I).

For both equations, in principle adding explanatory variables, such as child (e.g. sex) and parental characteristics, leads to more precise estimates. Several child and parent variables were tried, but none increased precision, so they are excluded from the analysis.

A more promising set of covariates for increasing precision is baseline test scores; students' test scores in the spring of 2004, before eyeglasses were provided, are highly correlated with test scores in 2005 and are uncorrelated with the program variable. As seen below, they have strong explanatory power, and adding them increases the precision of the estimates of the program's impact. Note that conditioning on pre-intervention test scores is a generalization of a regression in which the dependent variable is a change in test scores over time – the latter essentially forces the coefficient on the pre-intervention score to be one, while conditioning on that score in effect moves it to the right side of the regression equation and so does not constrain its coefficient.

A third possible set of covariates that could increase precision are school fixed effects, which can "soak up" variation in school quality and in differences in the tests across schools. This is feasible only for equation (2), since in equation (1) school fixed effects would be perfectly correlated with the program variable (P). This is also the case

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[8] 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 6.

[9] One way to see this is to assume that the correlation takes the form $u = \theta PV + \varepsilon$, where ε is uncorrelated with both PV and P. Then equation (2) becomes $T = \alpha + \pi PV + \tau P + \beta PV^*P + \theta PV + \varepsilon = \alpha + (\pi+\theta)PV + \tau P + \beta PV^*P + \varepsilon$; this regression will not yield unbiased estimates of π, but the estimate of β is still unbiased. More generally, in equation (2) u is not correlated with PV*P after conditioning on PV.

for equation (2), but the program effect in that equation is measured by the interaction of the program and poor vision dummy variables, which varies within schools. By focusing on within-school variation, this specification is also less subject to bias from imperfect randomization of treatment across schools, as all unobserved school differences are absorbed into the fixed effect. Non-random assignment causes bias in this context only if treated and untreated schools differ systematically in the differential performance of children with good and poor vision. When pre-program test scores are added as controls, bias occurs only if treatment status is correlated with differences in *changes* in student performance across children with good and poor vision. This is checked in Section 6.

A final issue is obtaining correct standard errors for the estimates of program effects. Standard errors should allow for heteroscedasticity of unknown form, as well as for correlation in the error term (u) across children in the same schools, and even children in different schools in the same townships. Indeed, as explained above schools often use their own tests (or township-specific tests used by all schools in the same township), as opposed to county-wide or province-wide tests; this generates correlation of test scores, and thus correlation in the error term, across students in the same school. More generally, unobserved school or township characteristics could lead to correlation of error terms for students in the same school or township.

The best approach to address this correlation is to use covariance matrices that allow for "clustering" of the error terms (see Wooldridge, 2010, Chapter 20). Yet for this paper the standard clustering formula have two disadvantages. First, OLS estimation of equations (1) and (2) that allows for correlation of unknown form at the township level "loses" information, leading to less precise estimates. This is because these covariance matrices do not distinguish between students in the same school and students in different schools in the same township. The correlation of the error terms is likely to be much stronger for the first set of students. To account for this differential correlation, we estimate specifications with school random effects, which distinguish between students in the same school and students in different schools, *and* we also allow for correlation of unknown form for the error terms of students in the same township. This specification is consistent even if the error terms in equations (1) and (2) do *not* follow the "classical" random effects form (see Wooldridge, 2010, pp.866-67). The only estimates in this paper without school random effects are those using school fixed effects; both sets of estimates allow for heteroscedasticity and correlation of unknown form at the township level.

The second problem is that covariance matrices that allow for clustering of the error terms are valid only as the number of clusters, i.e. the number of townships, goes to infinity. Our preferred estimates, which drop township pairs for which the randomization was improperly implemented, are based on 25 townships. Several authors have shown that these covariance matrices can be misleading when there are 30 or fewer clusters (see Cameron et al., 2008). To check whether our estimates have this problem, we also present p-values estimated using the wild bootstrap, as Cameron et al. (2008) suggest.

B. IV Estimates of the Impact of Providing Eyeglasses. The methods presented thus far estimate the impact of being offered eyeglasses, not the impact of having them. In general, the former impact will be less than the latter because students who are offered eyeglasses but do not accept them will not benefit from the offer. OLS estimates of the benefit of receiving eyeglasses may be biased because parents and/or students who accept the eyeglasses may differ in unobserved ways from those who decline the offer. For example, parents of students who take up the offer may have a more favorable opinion of education and so may do other things that raise their children's test scores.

Instrumental variable (IV) estimation can be used to obtain consistent estimates. In particular, one can estimate the impact of receiving eyeglasses (impact of the treatment on the treated) using the same equations presented above, replacing P (the offer of eye-glasses) with "G", actually receiving eyeglasses.[10] While G may be correlated with the residual, P can be used as an instrument 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 accepted the glasses in the program schools but also for the few students who already had glasses, in either the program schools or the control schools.

IV estimates of equation (1) are straightforward; one need only replace P with G and use P as an instrument for G. Yet there is one complication with IV estimates of equation (2). To see the problem, note that replacing P with G in that equation yields $T = \alpha + \pi PV + \tau G + \beta PV*G + u$. Although one can be in a program school if one does not have poor vision, it makes little sense to wear glasses if one does not have poor vision, so that G = 0 whenever PV = 0, and thus G and PV*G are perfectly correlated. This correlation is not exactly 1 in the data (it is 0.86), but this is the case only because a very small percentage of students report wearing classes even though they have good vision. Thus the IV estimates of equation (2) exclude the τG term.

IV estimation is valid even if the randomization was not implemented as planned. As long as the *plan* was randomized then the instrument is uncorrelated with all possible confounding factors and so is valid if it has explanatory power for having eyeglasses (which will be the case the program was implemented at least partially according to plan).

[10] 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 to wear eyeglasses. Yet since very few students had eyeglasses before the program, LATE estimates are very close to the impact of receiving eyeglasses on those who actually received them (impact of the treatment on the treated).

A final complication that arises with IV estimation is that the findings of Cameron et al. (2008) regarding the wild bootstrap have not been verified for IV estimation, so as yet there are no recommendations on how to implement IV estimation to correct for poor performance of clustered covariance matrices when there are less than 30 clusters. Thus, we do not present wild boostrap p-values for our IV estimates, and rely on the differences in the OLS results with and without wild boostrapping to provide an indication of the likely bias in statistical precision when the number of clusters is small.

5. 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 semester of 2005. Thus these results measure the impact of the project after one academic year. As explained above, all test scores have been normalized separately for each subject and grade.

Before examining the impact of the program, the data were examined to see whether the offer of eyeglasses was in fact randomly assigned across townships. This was done by estimating equations (1) and (2) using test scores from the spring of 2004, before the glasses were provided. These results are shown in Table 3. As explained above, all estimates use either school random effects or school fixed effects.

The estimates of equation (1) in the top panel 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 difference in the mean score on the Chinese test across these two sets of schools is very small (less than 0.05 standard deviations). The differences in the mean mathematics and science scores are also close to zero, -0.04 and 0.001, respectively. These differences are all statistically insignificant. Averaging across all three subjects gives an insignificant difference of 0.003 standard deviations. Thus estimates of equation (1) support the claim that randomization was correctly implemented in the 25 townships.

The second and third panels of Table 3 present estimates of equation (2) using the 2004 data; the second uses school random effects and the third uses school fixed effects.[11] Recall that estimates of equation (2) add students without vision problems and so should be more precise; indeed, the standard errors of the estimates of β are lower.

Consider first the school random effects estimates. Comparing students without vision problems (i.e. examining the "treatment township" coefficient), the differences in mean test scores for students without vision problems are small, and all differences are far from significant; the difference of the averaged scores is only 0.05 standard deviations and completely insignificant. Focusing on the (more precise) estimates of differences across students with poor vision (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 (-0.05) and statistically insignificant.

The last set of estimates in Table 3 adds school fixed effects to equation (2). As with the other two sets of estimates, the estimated program effects are far from significant. Indeed, they are very close to the school random effects estimates of equation (2). This is not surprising, for two reasons. First, since the offer of glasses was randomly assigned, both fixed and random effects estimates are consistent, so there should be no systematic difference. Second, as Wooldridge (2010, pp.326-27) explains, fixed and random effects

[11] Estimates of equation (1) classify students whose worst eye has a visual acuity score of 4.9 as having good vision. Yet recall that such children were offered glasses, and 17 out of 249 accepted them. Those 17 are excluded from the regression. Dropping all 249 of these children from the sample does not affect the results.

estimates give similar results when the number of observations per group (in this case the school) is large; this is the case here as there are 18,602 students in 165 schools.

Overall, the results in Table 3 support the hypothesis that the randomization was properly implemented in the township pairs where the distribution of eyeglasses was not corrupted. In addition, there are no significant differences in average pre-program scores between treatment and control schools for all township pairs, including those where eyeglasses were provided to some students in the control schools (see Appendix Table A.1); this suggests that the problem in the corrupted pairs amounted to not following the planned randomization, as opposed to a problem with the randomized plan.

A. Estimates of the Impact of Being Offered Eyeglasses. Now turn to estimates of the impact of being offered eyeglasses on test scores after one academic year. Estimates of equations (1) and (2) with the (normalized) 2005 spring semester test score as the dependent variable suggest that offering eyeglasses raises students' test scores, but most of the estimates are statistically insignificant, including those averaged over all three subjects (these results are shown in Appendix Table A.2). To increase precision, estimates are presented in Table 4 that condition on initial test scores. The estimates based only on children with poor vision (top panel) show positive impacts for all three subjects, ranging from 0.09 to 0.19 standard deviations, and the estimated impact for science is significant at the 1% level, with a wild bootstrap p-value of 0.02. Averaging over all three scores yields an impact of 0.16 that is significant at the 5% level, although the wild bootstrap p-value (0.21) does not indicate statistical significance at conventional levels.[12]

[12] The wild boostrap yields higher p-values when the sample is limited to students with poor vision, so that identification comes solely from between-township variation in treatment status. It does not greatly alter p-values when good vision students are added and identification uses within-township variation in eligibility.

The remaining estimates in Table 4 include both good vision and poor vision students. Adding students with good vision increases statistical precision, but it also reduces the estimates somewhat. For the random effects specification, the subject-specific impacts range from 0.07 to 0.12 standard deviations, and that for Chinese is significant at the 5% level (with a wild bootstrap p-value of 0.096). Averaging over all three scores, the estimated impact is 0.11, which is significant at the 5% level, and the wild bootstrap p-value is 0.034. The fixed effects estimates in the third panel of the table are very similar to those in the second panel. The results in Table 4 are our preferred estimates for the impact of offering eyeglasses to the students in our sample.

B. IV Estimates of the Impact of Wearing Eyeglasses. Table 5 presents IV estimates of the impact of wearing eyeglasses for one year on student test scores.[13] As explained above, random selection into a program school, conditional on having bad eyesight, is the instrumental variable. This instrument has strong explanatory power; in the regressions including only children with poor vision the R^2 of the first stage regression is 0.495 and the t-statistic for the program township variable is 19.41.

When the sample is restricted to students with poor vision, the estimated impact of having eyeglasses is positive for all three subjects, ranging from 0.13 to 0.26 standard

[13] Some students had worn eyeglasses for more than one year; of the 1,245 children with glasses, 199 had obtained them on their own, of whom 94 obtained them one year ago, 85 obtained them two years ago, and 20 obtained them 3 or 4 years ago, so only 105 of the 1,245 children had them for over one year. Recall that only 59 children in the sample with bad vision had glasses; thus 140 of the 199 children who report having obtained eyeglasses on their own do not appear to have had bad vision. This could reflect a mis-diagnosis that led their parents to obtain glasses for them, or measurement error either in the visual acuity variables or the variable indicating wearing eyeglasses. Measurement error in reported wearing eyeglasses does not imply inconsistency since that variable is instrumented. Measurement error in visual acuity could lead to selection bias in the regressions that include only children with poor vision, but the direction of bias is ambiguous; children with poor vision who wear glasses and were mistakenly dropped from the sample probably have relatively mild vision problems, so excluding them removes children whose benefit from having glasses is modest, leading to upward bias in the estimated impact of glasses, yet including children with good vision who do not wear glasses will increase the test scores of children without glasses, leading to downward bias. Measurement error in visual acuity (PV) is unlikely to cause bias in estimates that have both poor vision and good vision children; that is, the analysis in Appendix I extends to IV estimation.

deviations, and the estimate of 0.26 for science is significant at the 1% level. Averaging over all subjects, the estimated impact is 0.22, and significant at the 5% level (though this significance may be exaggerated, as the wild bootstrap p-values indicated in Table 4).

The remaining estimates in Table 5 add students with good vision. The random effects estimates range from 0.10 for math (insignificant) to 0.11 for science (significant at 10% level) to 0.16 for Chinese (significant at 5% level). Averaging over all subjects, the impact of having eyeglasses is 0.15 standard deviations, which is significant at the 5% level. As above, the school fixed effects estimates are quite similar.

In summary, our estimates indicate that wearing eyeglasses for 8-9 months raises grade 4-6 students' test scores by 0.15 to 0.22 standard deviations of the distribution of test scores, which is a large impact for such a short time. One can express this effect in terms of an equivalent gain from additional time in school. The 2000 GSCF administered identical Chinese and math tests to children in grades 4, 5 and 6. Relatively few of the children were in grade 6, so we focus on grades 4 and 5. The mean test scores of grade 5 students were 0.37 standard deviations higher in Chinese and 0.51 standard deviations higher in math than the mean scores of grade 4 students. Comparing the average gains on these two tests (0.44) with the gains of 0.15 to 0.22 from wearing glasses, the impact of wearing glasses is equivalent to an additional one third to one half of a year in school. Put another way, providing glasses raises learning per year of school by 33 to 50 percent.

6. Robustness Checks

The estimates in Section 5 rely on assumptions that could be challenged. First, the estimates that compare children with poor vision to those with good vision assume that providing the former eyeglasses does not affect the latter's test scores. Second, all estimates that compare children with good and poor vision assume that changes in test scores over time would have been similar for both groups in the absence of the program. Finally, all Section 5 estimates assume that, after dropping the township pairs in which the randomization was compromised, the remaining township pairs are not affected by any selection bias. This section checks the validity of these assumptions.

Consider first whether children with good vision were affected by the program. They could have benefited if their teachers spent less time helping students with poor vision, or if they learned from their now better performing classmates with poor vision. If this were the case, estimates of equation (2) would underestimate the true impact of the program on students with poor vision, since comparing students with poor vision to those with good vision ignores positive spillovers of the program onto the latter. Conversely, teachers may have been distracted from general teaching by the need to monitor students who were given glasses, or may have given more attention to those now better-motivated students. This would lead to overestimation of the program's impact on the students who were offered glasses. If the intervention affects students with good vision, these spillover impacts should be included when evaluating the total social benefits of the intervention.

Table 6 presents estimates similar to those for equation (1) in Table 4, except that the sample includes only students with good vision, instead of students with poor vision. Estimates are presented both with and without conditioning on 2004 test scores. None of the eight estimated program impacts is either large or statistically significant; they range from -0.065 to 0.048, none has a t-statistic above 1.1, and the wild bootstrap p-values are 0.48 are larger. Averaging over all three tests, the estimated effects are very small, -0.001 without conditioning on 2004 scores and -0.022 when conditioned on those scores. The latter, the more precisely estimated of the two, yields a 95% confidence interval ranging from -0.148 to 0.104, ruling out impacts of 0.11 or above. Finally, estimates that allow the program's impact to vary by 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 received eyeglasses) also show no effect of any kind (not shown in Table 6). We conclude that there is no evidence of sizeable spillover effects, and thus that the estimates in Section 5 are unlikely to be biased due to spillovers.

Next, one could argue that the checks for pre-program differences in Table 3 are insufficient for estimates that compare 2005 test scores, conditional on 2004 scores, across students with good vision and poor vision, because even if the test score *levels* in the spring of 2004 were similar across the treatment and control groups it is possible that the *changes* over time differ across those groups. This possibility is examined in Table 7, which re-estimates the random effects results in Table 4 that include students with poor vision and with good vision, but does so using data from one year earlier. If the relative changes in test scores for these two groups of students were sufficiently different in the treatment and control schools, one would find a "program effect" even before the program was implemented. Yet there is no evidence of such an effect; using the average over all three tests the estimated "program effect" is only -0.017, and completely insignificant.

Turn last to the possibility that the estimates in Section 5 may be biased because the township pairs in which the program was implemented correctly are not a random sample of the original set of township pairs. This is difficult to check for the estimates of being offered glasses in Table 4 since the program was improperly implemented in the townships that were excluded from the estimates in those tables. More specifically, one could estimate the impact of being in a township in which students with vision problems *should have been offered eyeglasses* (each of which should have been paired with a township that did not offer eyeglasses, which occurred in a little over half of the township pairs), but this is not the treatment effect estimated in Table 4.

Yet one can use instrumental variable methods to estimate the impact of wearing eyeglasses, that is re-estimate Table 5 using a larger sample, if the initial assignment to receive eyeglasses has strong predictive power for wearing eyeglasses, since the initial assignment was randomized and thus is a valid instrument. This is done in Table 8, which not surprisingly has less precise estimates than those in Table 5. The estimates in the first panel, which are based only on students with poor vision, are much lower than in Table 5 – indeed the average impact is slightly negative – but the standard errors are so large that these results are uninformative. For example, the 95% confidence interval for the average over all three scores ranges from -0.37 to 0.27. The preferred estimates in the second and third panels of Table 8, which include both students with good vision and poor vision and so have lower standard errors, are similar to those in Table 5. While only two of the eight estimates are statistically significant (for the random and fixed effects specifications the estimated impacts on Chinese scores are significant at the 10% and 5% levels, respectively), the results are broadly similar to the analogous results in Table 5. For example, the random effects estimate of the impact on average scores in Table 8 is 0.13 standard deviations, while that in Table 5 is 0.15.

7. Heterogeneous Treatment Effects

The impact of providing eyeglasses may vary for different types of students. This section examines such variation by students' visual acuity and initial (2004) test scores.

Perhaps the most obvious dimension along which the impact of eyeglasses would vary is by students' visual acuity; students with particularly bad vision should benefit the most from this intervention. This is examined in the first column of Table 9. Among students with poor vision (visual acuity below 4.9) are students with very poor vision, which we define as visual acuity below 4.4. Using this definition, about 20% of students with poor vision have very poor vision. The first set of results in Table 9 includes only children with poor vision; it finds a positive program impact but no additional impact on children with very poor vision. Indeed, the additional impact point estimate is negative, though it is far from significant. Adding students with good vision to the regression (i.e. estimating equation (2)) gives a similar result. Thus there is no evidence that children with very poor vision benefit more from the program.

Another possible dimension of program heterogeneity is by students' initial performance; students with poor vision *and* relatively low academic performance may benefit more (in terms of improved learning) than students with poor vision whose academic performance is average or above average. This is examined in the second column of Table 9. When only students with poor vision are included, the impact is lower for students with higher initial (2004) test scores, but the negative coefficient on the interaction is insignificant. Yet when students with good vision are added to the regression the (triple) interaction effect is somewhat larger and more precisely estimated, so that it is statistically significant at the 1% level (with a bootstrapped p-value of 0.014). Recalling that the average 2004 test score was normalized to zero, these estimates imply that average students experience an increase of 0.11 standard deviations, while below average students (defined as those whose 2004 average test score was one standard deviation below the mean) had a gain of 0.27 standard deviations, and above average students (those whose 2004 average score was one standard deviation above the mean) experienced a small loss of 0.06 standard deviations.[14] Thus providing eyeglasses appears to lead to more equitable educational outcomes among students with poor vision.

8. If the Benefits Are So Large, Why Do Some Children not Wear Eyeglasses?

The eyeglasses provided by the Gansu Vision Intervention Project cost about 120 yuan (about $15 U.S.). As explained above, their estimated impact on learning after only one year of use is equivalent to one third to one half of a year of schooling, which should lead to higher wages when a student enters the workforce. De Brauw and Rozelle (2007) estimate that each year of schooling in rural China increases wages by about 9.3% for those less than 35 years old. Our own estimates of a Mincerian wage function using data on family members aged 15 to 35 from Wave 2 (in 2004) of the GSCF yield a much lower estimate: 4.56%. The GSCF data also indicate that a wage earner age 15-25 who completes lower middle school (grade 9) earned about 710 yuan per month, or about 8,520 yuan per year. Using the lower estimate of the impact of a year of schooling, and assuming that the program effect is equivalent to an increase of only one third of a year, the program should increase such a wage earner's annual income by 128 yuan per year (8520*0.33*0.0456). Assuming that this person finishes grade 9 and then works 40 years before retiring, the present discounted value (PDV) of this increase in wages easily exceeds the cost of glasses; even using a 10% discount rate the PDV will be 830 yuan, and using a more reasonable 5% discount rate yields a PDV of 1,834 yuan.

These large benefits from wearing eyeglasses, relative to their cost, combined with many refusals of free eyeglasses[15] and very infrequent wearing of eyeglasses absent

[14]These calculations use the fact that, for students with poor vision, the average 2004 test score was -0.16, and the standard deviation was 0.97. So the impact for an average student is 0.081 - 0.166×(-0.16) = 0.108.

the intervention, point to a failure to make what appears to be a high-return investment. A better understanding of the causes of this failure has important policy implications. Is the cost of obtaining good quality eyeglasses too high, especially for the poor who may be credit-constrained? Even if eyeglasses are offered at no cost, and at a nearby location, parents may hesitate because accepting the offer may be thought to entail an obligation to purchase new glasses in later years should the original pair be lost or broken, or should the child's prescription need to be updated.

Alternatively, parents may simply be unaware of their children's vision problems, or may incorrectly believe that eyeglasses will weaken their children's eyes or that poor vision has little effect on learning at a young age. Even if parents are advised that their children need eyeglasses, they may doubt this advice, or think that their children's vision problems are minor and not worth having their child fitted for eyeglasses. Alternatively, parents may view eyeglasses as useful only for schooling, and may have low educational aspirations for their children. Parents may also be influenced by community norms on the value of eyeglasses, or of education. To explore these possibilities, we investigate which children accept the eyeglasses offered by the project, and we use the 2004 GSCF data to estimate the determinants of wearing glasses in the absence of an intervention.

Table 10 presents probit estimates of the factors associated with accepting the eyeglasses offered in the project schools. The first variable to check is students' visual acuity; children with minor vision problems have less reason to wear glasses, while those with serious problems would benefit more. As expected, better visual acuity (average over both

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[15] As explained above, only 1066 (69.8%) of the 1528 students with poor vision in the program schools accepted the eyeglasses, even though they entailed no cost. The stated reasons for not accepting them are not very informative, the two most common being "child refused" and "household head refused" (see Table 2).

eyes) has a highly significant negative impact on accepting glasses.[16] The standard deviation of the visual acuity variable is 0.234, so raising visual acuity by a standard deviation reduces the probability of accepting eyeglasses by 11.6 percentage points (0.234×0.494).

One unexpected result is that girls are much less likely to accept eyeglasses than boys: 73.6% of boys accepted them, compared to 66.0% of girls. The regression results show that girls have an 8.2 percentage point lower probability of receiving glasses, a highly significant difference. The reasons for this are unclear. The stated reasons for not accepting eyeglasses are similar for boys and girls. Anecdotal evidence suggests that girls may worry more than boys that wearing glasses makes them less attractive.

Four other factors significantly affect the probability of accepting eyeglasses. First, those children with poor vision who already wore eyeglasses (49 of 1528) were more likely to accept new ones; such children were 17.7 percentage points more likely to accept them. This is unsurprising given that they were already convinced of the need for glasses, and many may have needed a new prescription. Two other results are that children in households headed by a schoolteacher or a village cadre were less likely to accept glasses; these effects are significant at the 1% and 10% levels, respectively. These effects are very large, with schoolteachers' children 22.4 percentage points less likely, and village cadres' children 35.2 percentage points less likely, to accept them. Perhaps these local authority figures decline program benefits to avoid being perceived as manipulating the program for personal benefit. Alternatively, it would be strange, and ironic, if these authority figures had more doubts about the merits of eyeglasses. Fourth, students in wealthier townships were more likely to accept the eyeglasses offered; a one standard

[16] Other specifications were tried. For example, parents may feel that a child whose average visual acuity is below 4.8 does not need glasses if one of the eyes has normal visual acuity. Yet regressions using the acuity of the best eye, or the worst eye, or the difference between the two eyes had no added explanatory power.

deviation increase in average township income raises 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 plausible factors had no significant impact on accepting the offered eyeglasses. First, and rather surprisingly, more educated parents were no more likely to accept them (indeed, the point estimate is slightly negative). Second, students' initial test scores had no effect. Third, the main ethnic minority in these two counties, Tibetans (about 14.5% of the students), were less likely to accept the eyeglasses, but this effect is statistically insignificant. Finally, there was no difference in acceptance by grade level.

Further insights can be obtained from examining the 2004 GSCF data. We examine 925 children who were in primary school (and between the ages of 8 and 15) in that survey, 413 of whom were the "index" children from the 2000 GSCF and 512 of whom were younger siblings of those index children. These data contain much more information, including vision-related information, than do the school records of the students who participated in the Gansu Vision Intervention Project.

Evidence for the hypothesis that many parents are unaware of their children's vision problems is seen in the 2004 GSCF. Mothers were asked to assess their children's vision using five categories, from very good to very bad. As seen in Table 11, the vast majority (86%) of mothers of children with good vision, as measured by trained optometrists, correctly report that their children had good or very good vision. Yet 82% of those whose children's vision was only fair, and 62% of mothers whose children's vision was poor, also stated that their children's vision was good or very good.

Similar findings are found when these children were asked if they had vision problems, as seen in Table 12. Children with good vision or with fair vision rarely report vision problems (difficulty seeing the blackboard in school, trouble doing homework due to poor vision, and eye pain at home when studying in dim light). Children with poor vision are much more likely to report problems – 30.4% cite difficulty seeing the blackboard in school, 26.1% report trouble doing homework due to poor vision, and 29.0% cite eye pain at home when studying in dim light – yet for each of these problems about 70% of students with poor vision report not experiencing the problem.

One can apply regression analysis to the 2004 GSCF data to examine almost all of the hypotheses presented at the beginning of this section. Of the 925 children in primary school who were 8-15 years old in that survey, 23 (2.5%) report wearing glasses. The following data in the 2004 GSCF survey are useful for assessing the these hypotheses: mothers' and fathers' assessments of their children's vision; mothers' estimates of the cost of eyeglasses and of the distance to the nearest locality where eyeglasses are sold; parents' reports of whether they wear eyeglasses; community literacy rates; and parental aspirations for their children's education.

Table 13 reports the results of five regressions. Given the very small share of children wearing glasses, the marginal effects of changes in the exp[lanatory variables are small in percentage terms (but large relative to the base percentage wearing glasses). Nonetheless, the results are highly suggestive regarding the factors affecting the decision to wear eyeglasses. We focus on the coefficients that are statistically significant, and report the marginal effects of the fullest specification in the last column of Table 13.[17]

The first regression (Column 1) is the most parsimonious. It shows that children's visual acuity has a highly significant negative impact on the probability of having glasses,

[17] Regressions that include parental aspirations are excluded from Table 13; adding those variables reduces the sample size, and the results are generally insignificant.

as expected. Unlike the Table 10 results, child sex has no effect, and older children are more likely to report having eyeglasses; while this latter result may seem to reflect that older children have more vision problems, the regression already controls for visual acuity and so may reflect more parental acceptance of eyeglasses for older children. Mothers', but not fathers', education has a strong positive impact on having eyeglasses. Finally, households with greater per capita expenditures are more likely to purchase eyeglasses for their children, conditional on these other factors. This is similar to the results of Hannum and Zhang (2008) who, using the GSCF data, find that household wealth levels in the year 2000 were significantly positively associated with wearing glasses at age 13-16 in 2004.

Column 2 in Table 13 considers whether lack of awareness of children's vision problems reduces their probability of having eyeglasses. Mothers who think that their children have poor vision are much more likely to obtain glasses for them, but fathers' assessments have no significant effect; the coefficient is positive but lower than that for mothers. Note that the estimated impact of the child's actual visual acuity weakens (from -1.33 to -0.86); this supports the finding that many mothers do not know their children's visual acuity, and suggests that mothers' perceptions matter more than actual acuity.[18]

The next set of results examines whether perceived price and distance dissuade some parents from obtaining eyeglasses for their child. Price has the expected negative effect, and is significant at the 10% level. In contrast, the effect of distance is small, insignificant, and in an unexpected direction. Adding an interaction between price and log per capita expenditure did not produce a statistically significant coefficient.

[18] A related issue is whether some parents think that providing eyeglasses will increase the deterioration in their children's vision. There is no information in the 2004 GSCF on this attitude, but in the 2007 GSCF a new sample of mothers was asked, and about 25% opined that glasses would worsen their child's vision.

The mothers or fathers of 37 (4%) of the 925 children in the sample report that they themselves wear eyeglasses; such parents presumably understand their benefits. The fourth column in Table 13 shows that, controlling for all the variables discussed thus far, having a parent who wears eyeglasses has a strong positive effect on the probability that a child has eyeglasses. Indeed, when this variable is added the impact of the child's actual visual acuity falls (in absolute value) to -0.67 and becomes statistically insignificant.

Finally, the last column in Table 13 examines whether community characteristics have effects beyond those of parent and child characteristics. The community literacy rate, an indicator of the value placed on education by, and the general socio-economic status of, the community, has a significant positive impact on a child's probability of having glasses.

9. Summary and Conclusion

Vision problems create a potential barrier to learning for about 10% of primary school age children in both developed and developing countries. Fortunately, almost all childhood vision problems are easily corrected by correctly fitted eyeglasses. In developed countries such as the U.S., public programs such as Medicaid and the Children's Health Insurance Program pay for children's eye exams and some NGOs provide free eyeglasses to children from poor families.[19] In contrast, in developing countries very few children with vision problems have eyeglasses, especially at the primary level, and these children are rarely assisted by either public or private organizations.

This paper examines the impact of providing eyeglasses to school age children with poor vision in rural areas of Gansu province, one of China's poorest provinces. A

[19] In the U.S., NGOs providing free eye exams or glasses include Vision USA, Lions Clubs, New Eyes for the Needy, Sight for Students, Helen Keller Foundation International, and Essilor Vision Foundation. Helen Keller Foundation International runs programs on a very limited scale in a number of developing countries.

randomized control trial was implemented in 25 townships of two counties in Gansu, which included about 19,000 children in 165 schools, of whom 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 0.11 to 0.16 standard deviations of the distribution of those test scores, depending on the estimation method used.

For about one third of these children, either they or their parents refused the offer of free eyeglasses, which suggests that the impact of actually wearing eyeglasses is about 50% higher than these estimates. Indeed, instrumental variables estimates of the impact of wearing eyeglasses yield estimates of between 0.15 and 0.22 standard deviations. These are rather large effects, equivalent to one third to one half of a year of schooling. Simple calculations suggest that the benefits in terms of higher wages greatly outweigh the costs. Thus our results indicate that providing eyeglasses is a 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.

Perhaps the more important question is to understand why parents do not obtain eyeglasses for their children. Our estimates suggest that parental misperceptions, especially the impression that their children's eyesight is adequate, play a major role. There is also evidence that low income and perceived high prices may play a role. Yet more research, perhaps including additional randomized trials, is needed to understand parents' decision making with respect to providing glasses for their children, and to design policies that will ensure that school age children in developing countries who need eyeglasses will in fact have them.

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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%)	787 (12.8%)	2,529 (13.4%)
Of which:			
Had glasses already	36 (2.1%)	23 (2.9%)	59 (2.3%)
Did not have glasses	1,706 (97.9%)	764 (97.1%)	2,470 (97.7%)
Test scores in spring 2004 (before the intervention):			
Students without vision problems			
Chinese	79.0	78.6	78.9
Mathematics	79.2	79.0	79.1
Science	80.8	80.6	80.8
Students with vision problems			
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	787	2,529
Of which:			
In control schools	889	112	1,001
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			
Explanatory Variables	Chinese	Math	Science	Average
Equation (1): School Random Effects, Only Students with Poor Vision *N* = 2,491				
Treatment Township (β)	0.045	-0.042	0.001	0.003
	(0.094)	(0.089)	(0.074)	(0.095)
	[0.712]	[0.726]	[0.988]	[0.952]
Equation (2): School Random Effects, All Students *N* = 18,602				
Poor Vision (π)	0.040	0.089**	0.065*	0.077***
	(0.030)	(0.036)	(0.035)	(0.028)
Treatment Township (τ)	0.028	0.007	0.083	0.046
	(0.064)	(0.066)	(0.054)	(0.069)
Poor Vision×Treatment Township (β)	0.014	-0.076	-0.072	-0.053
	(0.044)	(0.058)	(0.053)	(0.048)
	[0.816]	[0.264]	[0.182]	[0.326]
Equation (2): School Fixed Effects, All Students *N* = 18,602				
Poor Vision (π)	0.043	0.089**	0.066*	0.078**
	(0.030)	(0.036)	(0.036)	(0.028)
Treatment Township (τ)	Not identified	Not identified	Not identified	Not identified
Poor Vision×Treatment Township (β)	0.015	-0.075	-0.072	-0.052
	(0.044)	(0.059)	(0.053)	(0.049)
	[0.790]	[0.268]	[0.194]	[0.338]

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

2. Standard errors in parentheses; wild bootstrap p-values in brackets. Random effects models include school random effects, and fixed effects models include school fixed effects. Both models allow for heteroscedasticity, including correlation (clustering) among observations within the same township, of unknown form for the individual level error term. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

Table 4: Estimated Program Effect After One Year: Conditional on 2004 Scores

	Dependent Variable			
Explanatory Variables	Chinese	Math	Science	Average
Equation (1): School Random Effects, Only Students with Poor Vision				***N* = 2,474**
2004 Chinese Score	0.172***	0.193***	0.162***	0.219***
	(0.047)	(0.042)	(0.041)	(0.045)
2004 Math Score	0.125***	0.169***	0.213***	0.212***
	(0.022)	(0.047)	(0.043)	(0.030)
2004 Science Score	0.138***	0.087***	0.134***	0.149***
	(0.022)	(0.031)	(0.032)	(0.028)
Treatment Township (β)	0.091	0.097	0.186***	0.156**
	(0.078)	(0.084)	(0.055)	(0.077)
	[0.374]	[0.454]	[0.016]	[0.214]
Equation (2): School Random Effects, All Students *N* = 18,504				
2004 Chinese Score	0.200***	0.169***	0.126***	0.206***
	(0.029)	(0.026)	(0.016)	(0.023)
2004 Math Score	0.111***	0.184***	0.209***	0.209***
	(0.014)	(0.031)	(0.045)	(0.027)
2004 Science Score	0.159***	0.109***	0.194***	0.192***
	(0.021)	(0.018)	(0.023)	(0.019)
Poor Vision (π)	-0.043	-0.020	0.011	-0.022
	(0.038)	(0.028)	(0.034)	(0.030)
Treatment Township (τ)	-0.059	-0.008	0.037	-0.013
	(0.060)	(0.050)	(0.061)	(0.063)
Poor Vision×Treatment	0.117**	0.072	0.071	0.108**
Township (β)	(0.051)	(0.055)	(0.044)	(0.049)
	[0.096]	[0.234]	[0.132]	[0.034]
Equation (2): School Fixed Effects, All Students *N* = 18,504				
2004 Chinese Score	0.199***	0.169***	0.125***	0.206***
	(0.029)	(0.027)	(0.016)	(0.023)
2004 Math Score	0.110***	0.185***	0.208***	0.209***
	(0.014)	(0.031)	(0.045)	(0.027)
2004 Science Score	0.156***	0.108***	0.191***	0.189***
	(0.021)	(0.018)	(0.023)	(0.018)
Poor Vision (π)	-0.043	-0.021	0.011	-0.022
	(0.038)	(0.029)	(0.034)	(0.030)
Treatment Township (τ)	Unidentified	Unidentified	Unidentified	Unidentified
Poor Vision×Treatment	0.116**	0.072	0.070	0.107**
Township (β)	(0.051)	(0.056)	(0.044)	(0.049)
	[0.024]	[0.238]	[0.170]	[0.052]

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

2. Standard errors in parentheses; wild bootstrap p-values in brackets. Random (fixed) effects models include school random (fixed) effects. All models allow for heteroscedasticity, including correlation (clustering) among observations within the same township, of unknown form for the individual level error term. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

Table 5: Effect of Eyeglasses After One Year: IV Results Conditional on 2004 Scores

	Dependent Variable			
Explanatory Variables	Chinese	Math	Science	Average
Equation (1): School Random Effects, Only Students with Poor Vision				***N* = 2,474**
2004 Chinese Score	0.171***	0.193***	0.162***	0.218***
	(0.046)	(0.040)	(0.038)	(0.043)
2004 Math Score	0.123***	0.169***	0.211***	0.210***
	(0.022)	(0.047)	(0.044)	(0.030)
2004 Science Score	0.137***	0.088***	0.136***	0.149***
	(0.022)	(0.031)	(0.031)	(0.028)
Has Eyeglasses (β)	0.130	0.137	0.261***	0.221**
	(0.109)	(0.120)	(0.073)	(0.109)
Equation (2): School Random Effects, All Students			***N* = 18,503**	
2004 Chinese Score	0.200***	0.169***	0.126***	0.206***
	(0.029)	(0.026)	(0.015)	(0.023)
2004 Math Score	0.110***	0.184***	0.209***	0.209***
	(0.013)	(0.030)	(0.044)	(0.027)
2004 Science Score	0.159***	0.109***	0.195***	0.192***
	(0.020)	(0.018)	(0.022)	(0.018)
Poor Vision (π)	-0.041	-0.021	0.008	-0.023
	(0.038)	(0.029)	(0.033)	(0.030)
Has Eyeglasses (β)	0.162**	0.103	0.107*	0.154**
	(0.074)	(0.080)	(0.060)	(0.071)
Equation (2): School Fixed Effects, All Students			***N* = 18,503**	
2004 Chinese Score	0.199***	0.169***	0.126***	0.206***
	(0.028)	(0.026)	(0.016)	(0.023)
2004 Math Score	0.110***	0.185***	0.208***	0.209***
	(0.013)	(0.030)	(0.044)	(0.026)
2004 Science Score	0.156***	0.108***	0.191***	0.189***
	(0.020)	(0.018)	(0.022)	(0.018)
Poor Vision (π)	-0.044	-0.022	0.010	-0.023
	(0.038)	(0.028)	(0.034)	(0.030)
Has Eyeglasses (β)	0.166**	0.103	0.100	0.153**
	(0.074)	(0.080)	(0.063)	(0.071)

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

2. Standard errors in parentheses; pairs cluster bootstrap p-values in brackets. Random (fixed) effects models include school random (fixed) effects. All models allow for heteroscedasticity, including correlation (clustering) among observations within the same township, of unknown form for the individual level error term. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

3. The instrumental variable for having eyeglasses is a dummy variable that indicates that one has been selected into the program *and* has poor vision.

Table 6: Estimated Program Effects for Students with Good Vision

	Dependent Variable			
Explanatory Variables	Chinese	Math	Science	Average
Equation (1): School Random Effects, Only Students with Good Vision				***N* = 16,031**
Treatment Township (β)	-0.046	-0.002	0.048	-0.001
	(0.077)	(0.066)	(0.088)	(0.091)
	[0.736]	[0.950]	[0.734]	[0.960]
Equation (1): School Random Effects, Only Students with Good Vision				***N* = 16,030**
2004 Chinese Score	0.206***	0.164***	0.120***	0.204***
	(0.029)	(0.026)	(0.015)	(0.021)
2004 Math Score	0.108***	0.187***	0.208***	0.208***
	(0.013)	(0.032)	(0.046)	(0.028)
2004 Science Score	0.164***	0.112***	0.205***	0.199***
	(0.022)	(0.018)	(0.023)	(0.019)
Treatment Township (β)	-0.065	-0.015	0.029	-0.022
	(0.060)	(0.050)	(0.063)	(0.064)
	[0.480]	[0.808]	[0.776]	[0.856]

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

2. Standard errors in parentheses; wild bootstrap p-values in brackets. All models include school random effects. All models allow for heteroscedasticity, including correlation (clustering) among observations within the same township, of unknown form for the individual level error term. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

Table 7: Falsification Test: "Effect" of Program in 2003-04 School Year

	Dependent Variable (2004 test scores)			
Explanatory Variables	Chinese	Math	Science	Average
Equation (2): School Random Effects, All Students			***N* = 18,600**	
2003 Chinese Score	0.387***	0.215***	0.125***	0.286***
	(0.012)	(0.016)	(0.010)	(0.008)
2003 Math Score	0.178***	0.355***	0.121***	0.257***
	(0.014)	(0.024)	(0.015)	(0.011)
2003 Science Score	0.130***	0.111***	0.425***	0.260***
	(0.012)	(0.016)	(0.025)	(0.018)
Poor Vision (π)	-0.019	0.036	0.030	0.018
	(0.019)	(0.032)	(0.029)	(0.015)
Treatment Township (τ)	-0.017	-0.037	0.027	-0.010
	(0.032)	(0.032)	(0.022)	(0.028)
Poor Vision×Treatment Township (β)	0.055	-0.046	-0.053	-0.017
	(0.036)	(0.046)	(0.041)	(0.030)
	[0.186]	[0.388]	[0.228]	[0.576]

Notes: Same notes as those given for Table 6.

Table 8 Effect of Eyeglasses After One Year: IV Results Using Full Sample

	Dependent Variable			
Explanatory Variables	Chinese	Math	Science	Average
Equation (1): School Random Effects, Only Students with Poor Vision				***N* = 4,293**
2004 Chinese Score	0.160***	0.176***	0.162***	0.191***
	(0.042)	(0.040)	(0.038)	(0.046)
2004 Math Score	0.116***	0.160***	0.211***	0.199***
	(0.022)	(0.036)	(0.043)	(0.029)
2004 Science Score	0.154***	0.092***	0.136***	0.165***
	(0.023)	(0.023)	(0.031)	(0.025)
Has Eyeglasses (β)	-0.078	-0.082	0.026	-0.051
	(0.138)	(0.160)	(0.126)	(0.161)
Equation (2): School Random Effects, All Students *N* = 32,587				
2004 Chinese Score	0.208***	0.185***	0.128***	0.217***
	(0.027)	(0.026)	(0.015)	(0.025)
2004 Math Score	0.111***	0.179***	0.201***	0.204***
	(0.017)	(0.029)	(0.039)	(0.031)
2004 Science Score	0.155***	0.103***	0.205***	0.192***
	(0.015)	(0.014)	(0.022)	(0.015)
Poor Vision (π)	-0.041	-0.028	0.024	-0.020
	(0.043)	(0.041)	(0.043)	(0.043)
Has Eyeglasses (β)	0.158*	0.106	0.033	0.126
	(0.088)	(0.103)	(0.094)	(0.096)
Equation (2): School Fixed Effects, All Students *N* = 32,587				
2004 Chinese Score	0.206***	0.184***	0.127***	0.216***
	(0.027)	(0.026)	(0.015)	(0.025)
2004 Math Score	0.110***	0.179***	0.200***	0.203***
	(0.018)	(0.029)	(0.039)	(0.031)
2004 Science Score	0.153***	0.101***	0.201***	0.190***
	(0.015)	(0.014)	(0.022)	(0.015)
Poor Vision (π)	-0.056	-0.035	0.020	-0.029
	(0.045)	(0.041)	(0.044)	(0.044)
Has Eyeglasses (β)	0.184**	0.119	0.037	0.140
	(0.092)	(0.104)	(0.096)	(0.098)

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

2. Standard errors in parentheses; pairs cluster bootstrap p-values in brackets. Random (fixed) effects models include school random (fixed) effects. All models allow for heteroscedasticity, including correlation (clustering) among observations within the same township, of unknown form for the individual level error term. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

3. The instrumental variable for having eyeglasses is a dummy variable that indicates that one has been selected into the program *and* has poor vision.

Table 9: Interaction Effects Between Program, Visual Acuity and 2004 Test Scores

Explanatory Variables	*Dependent Variable: Average Test Score*	
Equation (1): School Random Effects, Only Students with Poor Vision		***N* = 2,474**
2004 Chinese Score	0.219***	0.246***
	(0.046)	(0.034)
2004 Math Score	0.212***	0.241***
	(0.029)	(0.035)
2004 Science Score	0.148***	0.174***
	(0.028)	(0.030)
Treatment Township (β)	0.171**	0.136*
	(0.077) [0.130]	(0.079) [0.236]
Very Poor Vision	0.052	--
	(0.040)	
Very Poor Vision × Treatment Township	-0.082	--
	(0.069) [0.282]	
Avg. Test Score 2004 × Treatment Township	--	-0.106
		(0.092) [0.284]
Equation (2): School Random Effects, All Students	***N* = 18,478**	
2004 Chinese Score	0.208***	0.200***
	(0.023)	(0.029)
2004 Math Score	0.209***	0.202***
	(0.027)	(0.030)
2004 Science Score	0.191***	0.185***
	(0.019)	(0.024)
Poor Vision (π)	-0.025	-0.012
	(0.033)	(0.029)
Treatment Township (τ)	-0.011	-0.007
	(0.063)	(0.061)
Poor Vision × Treatment Township (β)	0.119**	0.081*
	(0.051) [0.054]	(0.043) [0.092]
Very Poor Vision	0.030	--
	(0.053)	
Very Poor Vision × Treatment Township	-0.064	--
	(0.077) [0.410]	
2004 Avg. Test Score × Treatment Township	--	0.033
		(0.079)
2004 Avg. Test Score × Poor Vision	--	0.068**
		(0.032)
2004 Avg. Test Score × Poor Vision		-0.166***
× Treatment Township		(0.055) [0.014]

Notes: 1. Constant terms and strata dummy terms are not shown (to reduce clutter).
2. Standard errors in parentheses; wild bootstrap p-values in brackets. All models allow for heteroscedasticity, including correlation (clustering) among observations within the same township, of unknown form for the individual level error term. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

Table 10: Probit Estimates of Factors Associated with Accepting Eyeglasses

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

Table 11: Mother's Assessment of Vision and Actual Visual Acuity (children age 8-15 who were enrolled in primary school in 2004)

Measured Acuity	*Mother's Assessment*					
	Very Bad	Bad	Fair	Good	Very good	Don't Know
Good (≥ 5.0)	1	4	92	251	367	4
Fair (4.8-4.99)	0	0	18	29	52	0
Poor (< 4.8)	1	7	17	14	29	1

Source: Gansu Survey of Children and Families.

Table 12: Children's Reports of Vision Problems, by Actual Visual Acuity (children age 8-15 who were enrolled in primary school in 2004)

Measured Visual Acuity	*Child Reports of Vision Problems*		
	Difficulty seeing blackboard (%)	Trouble doing homework due to poor vision (%)	Felt pain in eyes when studying at home in dim light (%)
Good (≥ 5.0)	8.5	6.7	19.1
Fair (4.8-4.99)	7.1	7.1	21.2
Poor (< 4.8)	30.4	26.1	29.0

Source: Gansu Survey of Children and Families.

Table 13: Determinants of Student Wearing of Eyeglasses
(from 2004 Gansu Survey of Children and Families)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	*Coefficient Estimates from Probit Specification*					*Marginal Effects*
Visual acuity	-1.329***	-0.858**	-0.852**	-0.670	-0.783*	-0.0196
(best eye)	(0.374)	(0.412)	(0.420)	(0.423)	(0.417)	(0.0120)
Female	-0.228	-0.210	-0.228	-0.145	-0.155	-0.00396
	(0.169)	(0.177)	(0.173)	(0.173)	(0.173)	(0.00417)
Age (years)	0.0849**	0.0902***	0.0861**	0.101***	0.101***	0.00253**
	(0.0339)	(0.0341)	(0.0346)	(0.0375)	(0.0386)	(0.00106)
Father's education	-0.0210	-0.0114	-0.0104	-0.000136	0.00228	5.69e-05
	(0.0325)	(0.0341)	(0.0334)	(0.0365)	(0.0375)	(0.000930)
Mother's education	0.0919***	0.0895***	0.0936***	0.0851**	0.0722*	0.00180*
	(0.0316)	(0.0326)	(0.0331)	(0.0364)	(0.0372)	(0.00105)
Log p.c. expend.	0.553***	0.515***	0.530***	0.497**	0.463**	0.0116**
	(0.184)	(0.190)	(0.189)	(0.204)	(0.201)	(0.00562)
Mother assessment of child's vision		0.898***	0.887***	0.858***	0.809***	0.0508
		(0.326)	(0.329)	(0.307)	(0.298)	(0.0364)
Father assessment of child's vision		0.603	0.605	0.558	0.542	0.0247
		(0.380)	(0.385)	(0.357)	(0.364)	(0.0251)
Estimated cost of glasses			-0.00366*	-0.00375*	-0.00451**	-0.000113
			(0.00215)	(0.00218)	(0.00225)	(7.37e-05)
Estimated distance to buy glasses			0.00164	0.000704	0.000869	2.17e-05
			(0.00422)	(0.00392)	(0.00388)	(9.75e-05)
Parent wears glasses				0.955***	0.962***	0.0678
				(0.322)	(0.318)	(0.0439)
Village literacy rate					1.300**	0.0325*
					(0.645)	(0.0171)
Constant	-0.392	-2.710	-2.668	-3.679	-3.890	
	(2.360)	(2.732)	(2.751)	(2.731)	(2.898)	
Observations	921	921	921	921	921	921

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

2. Estimated cost of glasses and distance to buy glasses are medians of parental reports.

Appendix I: Impact of Measurement Error in the Poor Vision (PV) Variable

This appendix shows that random measurement error in the poor vision variable will lead to underestimation of program effects in estimation of equation (2). Let PV* be the (unobserved) true indicator of whether a child has poor vision, and let PV denote the observed value of that variable. Thus $PV = PV^* + \varepsilon$, where ε is the measurement error. Since both PV and PV* are dummy variables, ε will clearly be correlated with PV*, so this is not classical measurement error.

Assume that both types of measurement error (PV = 0 when PV* = 1 and PV = 1 when PV* = 0) occur with the same frequency, denoted by θ. Thus there are three possibilities:

Frequency	*Value of PV**	*Value of PV*	*Value of ε*
$1-2\theta$ (no error)	1 or 0	Same as PV*	0
θ	0	1	1
θ	1	0	-1

The assumption that both types of errors occur with the same frequency is plausible if the error in the underlying visual acuity variable is random and the density of the distribution of that variable is similar on both sides of the cutoff point (4.8), and the latter is approximately correct.

Measurement error alters equation (2) as follows:

$$T = \alpha + \pi PV^* + \tau P + \beta PV^*P + u$$

$$= \alpha + \pi(PV - \varepsilon) + \tau P + \beta(PV - \varepsilon)^*P + u$$

$$= \alpha + \pi PV + \tau P + \beta PV^*P + (u - \pi\varepsilon - \beta\varepsilon P)$$

Bias in OLS estimation of β will be primarily determined by whether the interaction term PV*P is correlated with the composite error term $(u - \pi\varepsilon - \beta\varepsilon P)$. Focusing on bias due to measurement error, this will be determined by whether PV*P is correlated with $\pi\varepsilon + \beta\varepsilon P$. Covariance formulas imply $Cov(\pi\varepsilon + \beta\varepsilon P, PV^*P) = \pi Cov(\varepsilon, PV^*P) + \beta Cov(\varepsilon P, PV^*P)$. The following derivations show that $Cov(\varepsilon, PV^*P) = Cov(\varepsilon P, PV^*P) = \theta E[P]$:

$$\begin{aligned}
Cov(\varepsilon, PV^*P) &= E[(\varepsilon - E[\varepsilon])(PV^*P - E[PV^*P])] \\
&= E[\varepsilon(PV^*P - E[PV^*P])] \\
&= E[\varepsilon PV^*P] - E[\varepsilon]E[PV^*P] \\
&= E[\varepsilon PV^*P] \\
&= Prob[\varepsilon = 0]\times E[\varepsilon PV^*P|\ \varepsilon = 0] + Prob[\varepsilon = -1]\times E[\varepsilon PV^*P|\ \varepsilon = -1] + Prob[\varepsilon = 1]\times E[\varepsilon PV^*P|\ \varepsilon = 1] \\
&= (1 - 2\theta)\times 0 + \theta\times(-1\times 0\times E[P]) + \theta\times(1\times 1\times E[P]) \\
&= 0 + 0 + \theta E[P] > 0
\end{aligned}$$

$$\begin{aligned}
Cov(\varepsilon P, PV^*P) &= E[(\varepsilon P - E[\varepsilon P])(PV^*P - E[PV^*P])] \\
&= E[(\varepsilon P - E[\varepsilon]\times E[P])(PV^*P - E[PV^*P])]
\end{aligned}$$

$$
\begin{aligned}
&= E[\varepsilon P(PV^*P - E[PV^*P])] \\
&= E[\varepsilon PV^*P^2 - E[\varepsilon P]E[PV^*P])] \\
&= E[\varepsilon PV^*P - E[\varepsilon]E[P]E[PV^*P])] \\
&= E[\varepsilon PV^*P] \\
&= \theta E[P] > 0
\end{aligned}
$$

Thus we have:

$$Cov(\pi\varepsilon + \beta\varepsilon P, PV^*P) = \pi Cov(\varepsilon, PV^*P) + \beta Cov(\varepsilon P, PV^*P) = (\pi + \beta)\theta E[P]$$

This derivation has two implications. First, as measurement error decreases ($\theta \rightarrow 0$), this correlation goes to zero and so bias goes to zero. Second, it is reasonable to assume that $\pi = -\beta$. Quite simply, if we expect a child with poor vision to score lower on tests by a factor of π (note that $\pi < 0$), then providing that child with glasses should remove the problem, which implies an impact of the same magnitude but in the opposite direction ($\beta > 0$). Thus implies that the two terms in the bias tend to cancel each other out. Note that the fact that π may be estimated with bias does not matter for this derivation, which is based on the true underlying value of π, not an estimate of π.

Appendix II: Additional Tables

Table A.1: Check for Pre-Program Differences across Treatment and Control Groups, Including Township Pairs where Randomization Was not Correctly Implemented (Differences in Spring 2004 Scores)

	Dependent Variable			
Explanatory Variables	Chinese	Math	Science	Average
Equation (1): School Random Effects, Only Students with Poor Vision ***N* = 4,453**				
Treatment Township (β)	-0.129	-0.172*	-0.071	-0.144
	(0.103)	(0.098)	(0.091)	(0.109)
	[0.476]	[0.262]	[0.584]	[0.398]
Equation (2): School Random Effects, All Students ***N* = 33,679**				
Poor Vision (π)	0.025	0.085***	0.041	0.059**
	(0.022)	(0.024)	(0.027)	(0.026)
Treatment Township (τ)	-0.100	-0.069	-0.018	-0.072
	(0.084)	(0.077)	(0.078)	(0.093)
Poor Vision×Treatment Township (β)	0.010	-0.091**	-0.031	-0.044
	(0.035)	(0.041)	(0.045)	(0.038)
	[0.804]	[0.070]	[0.516]	[0.268]
Equation (2): School Fixed Effects, All Students ***N* = 33,679**				
Poor Vision (π)	0.025	0.085***	0.041	0.060**
	(0.022)	(0.025)	(0.028)	(0.026)
Treatment Township (τ)	Not identified	Not identified	Not identified	Not identified
Poor Vision×Treatment Township (β)	0.010	-0.091**	-0.032	-0.045
	(0.035)	(0.042)	(0.045)	(0.039)
	[0.806]	[0.076]	[0.508]	[0.276]

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

2. Standard errors in parentheses; wild bootstrap p-values in brackets. Random effects models include school random effects, and fixed effects models include school fixed effects. Both models allow for heteroscedasticity, including correlation (clustering) among observations within the same township, of unknown form for the individual level error term. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.

Table A.2: Estimated Program Effect After One Year
(not conditional on 2004 test scores)

	Dependent Variable			
Explanatory Variables	Chinese	Math	Science	Average
Equation (1): School Random Effects, Only Students with Poor Vision				***N* = 2,474**
Treatment Township (β)	0.097	0.098	0.191**	0.161
	(0.094)	(0.089)	(0.083)	(0.098)
	[0.462]	[0.458]	[0.152]	[0.294]
Equation (2): School Random Effects, All Students *N* = 18,505				
Poor Vision (π)	-0.015	0.010	0.047	0.018
	(0.029)	(0.030)	(0.032)	(0.023)
Treatment Township (τ)	-0.039	0.005	0.056	0.008
	(0.076)	(0.064)	(0.085)	(0.088)
Poor Vision×Treatment Township (β)	0.100**	0.054	0.043	0.081
	(0.050)	(0.062)	(0.049)	(0.055)
	[0.060]	[0.404]	[0.414]	[0.174]
Equation (2): School Fixed Effects, All Students *N* = 18,505				
Poor Vision (π)	-0.014	0.010	0.047	0.018
	(0.029)	(0.030)	(0.032)	(0.023)
Treatment Township (τ)	Not identified	Not identified	Not identified	Not identified
Poor Vision×Treatment Township (β)	0.099*	0.054	0.042	0.081
	(0.050)	(0.063)	(0.050)	(0.055)
	[0.058]	[0.418]	[0.414]	[0.182]

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

2. Standard errors in parentheses; wild bootstrap p-values in brackets. Random effects models include school random effects, and fixed effects models include school fixed effects. Both models allow for heteroscedasticity, including correlation (clustering) among observations within the same township, of unknown form for the individual level error term. Asterisks denote statistical significance: * 10% level, ** 5% level, *** 1% level.