Ordeal Mechanisms and Information in the Promotion of Health Goods in Developing Countries: Evidence From Rural China

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Abstract: The cost-effectiveness of policies that provide subsidized health goods is often compromised by the fact that many individuals do not use the goods that are provided to them. Cost-sharing strategies can improve targeting efficiency by inducing self-selection, but have been shown to significantly dampen overall take-up (which is often the primary policy goal). As a potential solution, applying an *ordeal* mechanism to the distribution of subsidized health goods has been proposed as a way to balance the dual goals of improving targeting efficiency while at the same time maintaining take-up. Another commonly used policy to promote health goods is health training. Despite the work that has been done (separately) on ordeal mechanisms and health training programs, there is a gap in the literature: that is, the effect of ordeal mechanisms without and without health training programs. In this paper, we report the results of a randomized field experiment designed to provide eyeglasses to myopic students of primary schools in rural western China. We test the performance of an ordeal mechanism with and without health training. We emphasize three findings. First, both with and without health training, the ordeal modestly improved targeting efficiency and reduced program costs compared to direct free distribution. Second, in the short run, the training program caused the ordeal mechanism to screen out some individuals who would have used eyeglasses if provided for free. Third, in the medium run, the ordeal mechanism only screened out individuals who would not have used eyeglasses both with the training program and without.

Keywords: Ordeal Mechanism; Information; Health; Developing Countries

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1. Introduction

Policy makers seeking to promote health goods through subsidization in developing countries are often faced with the dual challenge of a) increasing the overall take-up rate among individuals who can benefit from a good and b) doing so in a cost effective way. While for some goods, the issue of cost-effectiveness is almost moot since private and social benefits easily outweigh the costs (vaccinations, for example); for other health goods, however, higher costs of provision make cost-effectiveness a more salient consideration. This is particularly so when the provision of an otherwise effective health good does not necessarily lead to benefits, such as when recipients simply do not use the good provided. The literature on health goods in developing countries provides numerous such examples: insecticide-treated mosquito nets (Cohen and Dupas, 2010), water purification technologies (Dupas et al 2013) or improved cookstoves (Miller and Mobarak, 2013). For these technologies, cost-effectiveness can be improved by targeting these subsidized goods to individuals who are most likely to use—and therefore, benefit—from them.

One way to potentially improve the cost-effectiveness of providing a health good is through cost-sharing (Cohen and Dupas 2010, Ashraf et al. 2010). Cost-sharing can improve targeting in two ways, through a.) a selection effect (or the effect by which a higher price skews the distribution of "buyers" towards individuals with a greater propensity to use the good); and b.) a sunk cost effect (the a psychological effect whereby individuals are more likely to use a good because they paid a positive price for it, regardless of the intrinsic welfare the good brings).

The empirical evidence on the positive aspects of cost-sharing (i.e., increased use and, therefore, reduced waste) is not very encouraging (Cohen and Dupas 2010, Ashraf et al. 2010). There is also little evidence that higher prices lead to greater use. Indeed, charging even a modest price considerably dampens the purchase of health goods and screens out a large number of individuals who would otherwise use them (Cohen and Dupas 2010, Ashraf et al. 2010- which, of course, compromises another of the primary goals of policy makers).

A recently-proposed method of balancing these dual goals in developing countries

(increasing overall take-up and doing so cost-effectively) is through a so-called *ordeal* mechanisms (Alatas et al., 2013; Dupas et al., 2013). Traditionally, ordeal mechanisms have been used to target government transfers such as welfare and unemployment (Nicols et al., 1971; Nicols and Zeckhauser, 1982; Besley and Coate, 1992; Alatas et al., 2013). These mechanisms attempt to cheaply target resources to the needy by imposing different costs on targeted and non-targeted individuals. This approach may then lead individuals to self-select, with targeted individuals selecting themselves into the program (and non-targeted individuals not selecting themselves in). This self-selection thereby eliminates the need for costly (or impossible) collection of information for targeting. Because the utility cost imposed by the ordeal is different for rich and poor (poor people may be more liquidity constrained and less time constrained than relatively better-off people), requiring that applicants undergo such an ordeal (which may be as simple as requiring applicants to go through time-consuming application procedures) can deter the relatively wealthy from applying for benefits thus reserving government resources for the needy. Putting a mechanism in place that leads individuals to self-select may not only be cheaper than alternative targeting approaches but might also lead to more efficient targeting (Alatas et al., 2013).

The potential cost of ordeal mechanisms, however, is that they can screen out individuals who would have otherwise demanded the good. Of course, this negative effect is similar to what has been observed for price instruments (Cohen and Dupas 2010, Ashraf et al. 2010). If the ordeal mechanism reduces access to those that need the good, then it could be counter productive to the other primary goal of the program—increasing overall take-up of a health good among those who could benefit from it.

How well ordeal mechanisms function to screen out only individuals who would not use a good if acquired for free depends on a number of factors. Critical among these is individuals' perceptions of the relative benefits and costs of using a particular health good. Because take-up and use occur sequentially, these perceptions are not only important at the time individuals choose to acquire a good but also how these perceptions evolve with experience. Particularly in the case of

experience goods (such as eyeglasses or bednets), perceptions may differ at these two points in time because: a.) the two decisions are made under two different information sets; b.) individuals gradually update their private perceptions of benefit and cost. If individuals systematically undervalue a health good relative to their realized net benefit ex-post (after having acquired a good) the cost of an ordeal may screen out too many people. Indeed, incomplete or imperfect information that leads individuals to undervalue a good is often cited as a major factor contributing to surprisingly low take-up and use rates of health technologies in developing countries, even when they are provided for free (Dupas, 2010).

Given evidence that individuals tend to underestimate the value of many health goods, a common approach to increase overall take-up (independent of targeting and cost-effectiveness) is through health training or "social marketing" campaigns. One part of the sizeable literature on health education and training shows that it can effectively increase take-up and even increase the effect that subsidies have on the take-up of health goods (Jalan and Somanathan 2008, Luoto et al. 2012, Ashraf et al., 2013). On the other hand, some research has shown health education and training campaigns to be largely ineffective (Shi et al 2012).

Despite the work that has been done on ordeal mechanisms and health training programs separately, as far as we know, there is no research on how health training programs affect the functioning of an ordeal mechanism. Specifically, does training help to keep the overall rate of take-up from being dissuaded by the ordeal imposed? Does training help to screen out those who would tend to not use the good anyhow and induce those who would use the health good to be more decisive in doing so? Given that ordeal mechanisms are often (or easily) implemented together with health training programs, understanding how they interact with one another to influence the take-up and use of health goods can inform how to design policy approaches that can more effectively address the dual goals of increasing overall take-up and doing so in a cost-effective way.

In this paper, we report the results of a randomized field experiment designed to test the performance of an ordeal mechanism with and without an additional education campaign. We

emphasize three findings. First, both with and without health training, the ordeal only modestly improved targeting efficiency and reduced program costs compared to direct free distribution. Second, in the short run, despite the overall rise in targeting efficiency, the training program caused the ordeal mechanism to screen out some individuals who would have used eyeglasses if provided for free. This appears to be due to the health training program having had a much larger effect on use when the good was directly distributed for free compared to when individuals needed to go through the ordeal to acquire the good. Third, targeting due to the ordeal mechanism was more efficient in the medium run. Both with and without the training program, the ordeal only screened out individuals who would not have used the good if freely delivered.

Our study makes several contributions to the existing literature. First, we contribute to the nascent literature on the application of ordeal mechanisms to the take-up of health goods in developing countries (the only other similar study that we are aware of is Dupas et al, 2013). Second, we present the first evidence of the role of information in determining the "success" of ordeal mechanisms. Third, we contribute to the literature on the role of education campaigns in increasing the take-up of under-utilized health technologies, specifically how information affects the demand curve defined on non-monetary cost (distance).

The rest of the paper is organized as follows. Section 2 describes the research context, presents the experimental design and the data collection. Section 3 presents the results. Section 4 concludes.

2. Context and Experimental Design

2.1 Context

A series of World Health Organization-supported studies suggest that approximately 10 to 15 percent of school-aged children in the developing world are have common vision problems (Maul et al., 2000; Murthy et al., 2002; He et al., 2007). In most cases children's vision problems can be easily corrected by timely and proper fitting of quality eyeglasses (World Health

Organization [WHO], 2006). Unfortunately, studies in a variety of developing countries document that 35 to 85 percent of individuals with refractive error do not have eyeglasses (Bourne et al., 2004, Ramke et al., 2007).

In rural areas of China the prevalence of vision problems among children is among the highest in the world (He, Huang, Zheng, Huang, & Ellwein, 2007; He et al., 2004). However, recent investigations in rural China demonstrate that fewer than one third of children needing glasses have and wear glasses (Li et al., 2010).

Although the inability of many families to afford eyeglasses is likely a part of the reason, two significant factors contributing to such low use rates in China are lack of awareness and mis-information (Li et al., 2008). In many cases, people are not aware of their vision problems since there are very access of vision check (Li et al., 2010; Yi et al., 2013). There are also commonly held (but mistaken) views in many countries, including China, that wearing eyeglasses will harm one's vision, i.e., it cause one's vision to deteriorate even faster (Li et al., 2010; Yi et al., 2013). In our survey, for example we find around half of the students were not aware their vision problems; in addition, when students being asked "do you believe wearing glasses will harm your vision," more than 40% of the students answered "Yes." (See Appendix Table 2 for details)

2.2 Sampling

Our experiment took place in two adjoining provinces of western China: Shaanxi and Gansu.[4] In each of the provinces, one prefecture was included in the study. A map of these regions is provided in Figure 1. From each prefecture, a list of all rural primary schools was obtained. This list formed the sampling frame and 252 schools were randomly selected for inclusion in the study. To minimize the possibility of contamination, we first selected townships

[4] Shaanxi's GDP per capita of USD6108 was ranked 14th among China's administrative regions in 2012, and was very similar to that for the country as a whole (USD 6091) in the same year, while Gansu was the second-poorest province in the country (per capita GDP USD3100) (China National Statistics Bureau, 2012).

and selected only one school per township. Within schools, our data collection efforts (discussed below) focused on 4th and 5th grade students. From each grade, one class was randomly selected and surveys and eye exams were given to all students in these classes.

2.3 Experimental Design

Following the baseline survey and vision tests, schools were randomly assigned to one of the six cells in a 3 by 2 experimental design (shown in Figure 2). Schools were first randomized into one of three *provision* groups (free distribution; ordeal and control). Half of the schools assigned to each provision group were then assigned to receive a health training program. To improve power, randomization was stratified by county and by the number of children in the school found to need eyeglasses. In total, this yielded a total of 45 strata. Our analysis takes this randomization procedure into account (Bruhn and McKenzie 2009).

The three provision groups are as follows:

Free distribution: In this group each student diagnosed with poor vision was given a free pair of eyeglasses as well as a letter to their parents informing their prescription. This pair of glasses was delivered to the hands of students by a team of one optometrist and two enumerators.

Ordeal: In this group, each student diagnosed with poor vision was given a voucher. Their prescription was also printed in the voucher. This voucher was redeemable for one pair of free glasses at an optical store that was in the county seat. The distance from each students home and the county seat varied in great deal among our sample, ranging from 1 kilometer to 105 kilometers (average distance of 1st distance quartile is 9 kilometers and that of 4th distance quartile is 62 kilometers, most 7 times further). In most cases in order to undertake the ordeal, the parent and student would have to pay for a round trip bus ticket and take the time to pick up the glasses. To eliminate the opportunity for arbitrage, student information (student name, school name and county name) was printed on each voucher and students were required to show their ID to redeem the voucher.

Control: Students in the control group were given a letter to their parents informing their myopia status and prescription, but without a free delivered eyeglasses or a voucher.

In each of these groups, half of the schools were assigned to receive an training program:

Training program*:* The training program involved a short documentary-type film, a set of cartoon-based pamphlets for students (regardless of whether they had poor vision or not), and a lecture/handout for the parents and teachers. All these materials address the importance of wearing glasses (correct vision problem and help school performance) and correct some of most commonly wrong briefs (wearing glasses will harm one's vision; doing eye exercise can help treating myopia).

2.4 Data Collection

Survey Data

A baseline survey was conducted in September 2012. The baseline survey collected detailed information on schools, students and households. The school survey collected information on school infrastructure and characteristics of school administrators and teachers. A student survey was given to all students in selected 4^{th} and 5^{th} grade classes. This student survey collected information on basic background characteristics of students as well as information relevant to vision health and eye glasses (whether or not have eye glasses, knowledge about myopia, etc). Household surveys were also given to these same students, which they took home and filled out with their parents. The household survey collected information on households (e.g., parents education levels) that children would likely have difficulty answering.

Nearly identical surveys were conducted at endline in May 2013.

Eye Exams

At the same time as the school survey, a two-step eye exam was administered to all students in the randomly selected classes in all sample schools. First, visual acuity screenings (or E-chart exams) were administered by a team of two trained staff. Visual acuity was tested separately for each eye at a distance of 4 meters using Early Treatment Diabetic Retinopathy

Study (ETDRS) charts (Precision Vision, La Salle, IL, USA) in a well-lighted, indoor area of the school. Students who failed the visual acuity screening test (cutoff is defined by VA of either eye less than or equal to 6/12 or 20/40) were enrolled in a second vision refraction test which was carried out at each school 1-2 days after the first. This second vision test was conducted by a team of one optometrist, one nurse and one assistant staff. Cycloplegic automated refraction with subjective refinement was underwent to determine prescriptions for children needing glasses.

Unannounced Checks on Short and Medium Term Take-up and Use of Glasses

Two rounds of unannounced checks were conducted to collect information on the take-up and use of eyeglasses. The short term check was done in early November 2012 (approximately 3 weeks after glasses and vouchers were distributed). In this round, a team of two enumerators visited each of the 252 schools without any prior announcement of their visit. Once at the school, enumerators entered classes while classes were in session and recorded the number of students wearing glasses. One limitation of these data is that it was not recorded for individual students. Instead, enumerators only collected information on the number of students in each class wearing glasses. After this count was finished, students with poor eyesight were given a short survey that included questions about a) whether they owned eyeglasses and how they were acquired and b) if they owned eyeglasses how often and when they wore them.

A second round—medium term check—was conducted in line with the endline survey in May 2013 (seven months after eyeglasses and vouchers were distributed). As the endline survey was more involved and time consuming, school principals in each school were informed the arrival of the survey team. However, they were not told specifically that we would check to see if children were wearing eyeglasses. In line with the first round, a team of 2 enumerators was sent into schools in advance of the rest of the survey team to conduct classroom checks. In this second round of checks, enumerators were given a list of children diagnoses with poor vision to record individual-level information on the use of glasses for each student.

2.5 Baseline Characteristics and Balance Check

Baseline characteristics of sample students are shown in Table 1. Of the 19,934 students given eye examinations at baseline, 3,177 (16%) were found to be myopic and need glasses. Only these students are included in the analysis sample.

On average, only 15 percent (463 out of 3177) of students who needed glasses in had glasses at baseline. In line with the finding in literature, besides the liquidity constrain, misinformation plays an important role of this low rate. For example, nearly 40 percent of myopic children believed that wearing eyeglasses could deteriorate one's vision and less than half knew they were myopic. Few of these students–only about one third–had family members that wore glasses. Generally, the education level of parents was low: only 11% of mothers and only 16% of fathers had attended high school. Ten percent of students in the sample had parents who had both migrated elsewhere for work and 22% boarded at school.

The last two columns of Table 1 show that these characteristics are well-balanced across the treatment groups. The first part of the table shows tests across the provision groups (free distribution, ordeal and control) and the second part of the table shows tests between students in training program and no training program schools. These tests were conducted by regressing baseline characteristics on treatment arm dummy variables, controlling for randomization strata fixed effects and clustering errors at the school level. According to this table, the sample is well-balanced in both dimensions: only 2 out of 13 coefficients were significant at 10% or higher.

2.6 Attrition

The overall attrition rate after seven months was low (less than four percent of our sample students dropped between baseline and endline). Further, there is no differential attrition across treatment groups. Results of this check are in Appendix Table A.1.

3. Results

3.1 Take-up and Voucher Redemption

We distinguish take-up and use. Take-up is defined by ownership, i.e., if students own

one pair of eyeglasses (regardless of how it was obtained). Use is defined by whether students actually wear glasses. We begin the discussion of results by examining the effect of the ordeal (vouchers) on the take-up of eyeglasses. Because 100% of students in "free" group obtained glasses by design, we exclude this group from this part of the analysis.

In this section, we are interested in three main questions. First, what percent of voucher recipients redeemed vouchers? Another way of asking the question in this part of the analysis is that we seek to understand what percentage of individuals was screened out by the ordeal. Second, how did the training program affect voucher redemption? In this part of the analysis we seek to understand if information affects that impact of the ordeal. Third, how does take-up in the case of ordeal group compare to the case of no subsidy of any kind (the control group)?

3.1.1 Voucher Redemption Rates

Figure 4 plots short and medium terms take-up rates for each experimental cell. Panel A and B shows the data collected in short term and medium term, respectively. Beginning with the case of no training program (columns on the left side of each graph), we find that the rates of voucher redemption are high. In the short term (after only 3 weeks) 84% of students had glasses in the ordeal group. Surprisingly, the medium term data shows an take-up rate only slightly higher – suggesting that if individuals were going to redeem the voucher, they did so immediately. In the medium term, the take-up rate in the ordeal group with no training program was 89%. Therefore, in the case of our rural China voucher treatment arm, the ordeal only screened out 11% of those who would have acquired glasses if given for free.

With the training program, take-up rates in the ordeal group were not much higher. The short run rate in the ordeal group with training was 89%. The medium run rate was 92%. The training program therefore led to raw differences of only 5 percentage points in the short term and 3 percentage points in the medium term.

3.1.2 Determinants of Voucher Redemption

Table 2 looks more closely at the effect of training and distance on voucher redemption.

This table shows estimates from the following linear probability model (using only observations from the ordeal group):

$$\text{Voucehr Redemption}_i = \alpha + \beta_1 * \text{Training}_i + \beta_2 * \text{Distance}_i +$$

$$\beta_3 * \text{Distance}_i * \text{Training}_i + \text{Controls}_i + \text{Strata Fixed Effects}_i + \varepsilon_i \quad (1)$$

where $\text{Voucehr Redemption}_i$ is 1 if the student had redeemed the voucher. This is regressed on a dummy for being assigned to the training program group (Training_i), distance from the school to the county seat (Distance_i, where vouchers were redeemed), and the interaction between the training group dummy and distance) controlling for baseline characteristics of student (awareness of their myopia status, misinformation about wearing glasses) and both individual SES variables (gender, grade level, school boarding status) as well as family SES variables (parental migrant status, parental education and household wealth) as well as randomization strata fixed effects. Standard errors are clustered at the school level.

In the table, the first three columns show variants of this regression with voucher pickup by the short term check as the dependent variable. Columns (4) to (6) show estimates for pickup by the endline survey. In addition to estimating the effect of distance assuming linearity in columns (1) and (4), columns (2), (3) , (5) and (6) include dummies for the 2nd, 3rd and 4th quartiles of the distance variable.

In line with the take-up results plotted in Figure 4 above, we see that the average effect of the training program (irrespective of distance) has only a modest effect on voucher redemption (Table 2, columns 1 and 4, row 1). This effect is significant at the short term but fades away by the enline survey. Moreover, as would be expected, distance has a strong negative effect on voucher redemption. This correlation with distance also appears to be non-linear (columns 2 and 5): redemption rates decline significantly only when schools are in the 3rd or 4th quantiles of the distance measure. Interestingly, columns (3) and (6) show that training has a strong influence on those living the furthest away. For these individuals, the training program compensates almost fully for the reduction in demand due to the cost of the ordeal (distance). This effect of flattening

the demand curve (defined over distance) is consistent with Ashraf et al. (2013) who find training flattens the demand curve defined over prices.

3.1.3 Voucher Redemption vs. Self-purchase

In this section, we compare take-up in the ordeal group to take-up in the control group. This comparison provides an indication of how many more individuals received glasses with the voucher than would have otherwise (left to purchase glasses on their own). To do so, we run the following regression using observations from the ordeal and control groups:

$$\text{Take}-\text{up}_i = \alpha + \beta_1 * \text{Ordeal}_i + \beta_2 * \text{Training}_i + \beta_3 * \text{Ordeal}_i * \text{Training}_i + \text{Controls}_i + \text{Strata Fixed Effects}_i + \varepsilon_i \quad (2)$$

where $\text{Take}-\text{up}_i$ is 1 if an individual owns glasses and 0 otherwise; Ordeal_i is an indicator for assignment to the ordeal group; Training_i is the an indicator variable of whether the student is in the group of receiving the training program; Controls_i are the same as in equation (1).

Columns (1) and (2) in Table 3 report the estimates based on short term data. Among students given no training program, the voucher intervention is estimated to boost the take-up of glasses by 58 percentage points from the base of 24 percent in pure controlled group (almost a 250% increase). Absent the voucher, training program has a small effect on take-up similar to the effect seen on voucher redemption. In the short term, training has a positive influence on the increase in take-up due to the voucher, though this is not significant.

The data collected in the medium term shows some interesting differences with the short run (column (3) to (4) in Table 2). First, the magnitude of impact of the voucher alone on improving glasses take-up decreases by 12 percentage points (or 21%) (the $\widehat{\beta_1}$ decreases from 0.58 to 0.46). This is because students in control group have increased their glasses ownership by 19 percentage points (an 80% increase) instead of that in ordeal group drops over time. The impact of training alone also slightly increased about 3 percentage points (or 60%) after one semester (the $\widehat{\beta_2}$ increases from 0.05 to 0.08).

3.2 Use

In this section, we compare the use rates across all treatment groups, in which use is defined by whether students actually wear glasses. Comparing short- and medium-run use rates, particularly between the free and ordeal groups provides an indication of how well the ordeal mechanism functioned. If use rates are higher in the free group than in the ordeal group, this suggests that the ordeal screened out individuals who would have used glasses if given for free. If they are similar, however, this suggests that the ordeal mechanism screened out only those individuals who would not have used eyeglasses if given for free.

Figure 5 shows the use rates based on the unannounced checks for each of the experimental groups in the short term (Panel A) and in the medium term (Panel B). In the short term data, we see that the functioning of the mechanism varies with and without the training program. With no training, use in the ordeal group is actually slightly *higher* than the free group. One possible explanation for this is that the cost of the ordeal (travelling to redeem the voucher) induces a sunk cost effect (Thaler 1980; Arkes and Blumer 1985). With the training program, however, we see that this result is reversed. With training, use in the free group is higher than in the ordeal group. This implies that, with training, the ordeal screens out individuals who would have used glasses if given for free. Comparing this set of outcomes with the outcomes from the no training group, the reverse in results appears to be due to the fact that training had larger impact in free group than in the ordeal group. In Panel (B), the pattern found in the medium-term results is similar to the short term but overall use rates are smaller.

To compare use rates more formally, we estimate the following model:

$$\text{Use}_i = \alpha + \beta_1 * \text{Training}_i + \beta_2 * \text{Ordeal}_i + \beta_3 * \text{Free}_i + \beta_4 * \text{Training}_i * \text{Ordeal}_i + \beta_5 * \text{Training}_i * \text{Free}_i + \text{Controls}_i + \text{Strata Fixed Effects}_i + \varepsilon_i \quad (3)$$

where the right hand side is the same as above. Use_i , however, differs between the short and medium run results due to the protocol change in data collection. For the short term estimates, because the data is collected in school level, Use_i is the proportion of myopic children in a school

wearing glasses at the time of the short term check. For medium-run estimates, since data is counted in individual level, Use_i is 1 if student I was wearing glasses at the time of the medium-run check and 0 otherwise. $Controls_i$ are the same as in equation (1).

Columns (1) and (2) of Table 4 show the short run use estimates. First, comparing the coefficients on Ordeal and Free shows that with no training, use in the ordeal group is indeed higher than use in the free group but this difference is not significant. We estimate this difference to be 8.0 percentage points (difference between β_2 and β_3 in column 2, p-value: 0.18) when covariates are included. With the training program, this relationship is reversed. We estimate that the training program boosted use in the free group by 16.6 percentage points (the sum of β_1 and β_5, p-value: 0.00) while it had no effect in the ordeal group. As a result, in the presence of the training, use in the ordeal group is 9.4 percentage points (p-value: 0.09) lower than the free group. Thus, in the short run, the ordeal does not screen out individuals who would have used eyeglasses when there is no training program but does when jointly implemented with an training program.

Turning to the medium-run results, we find that without the training program use in the voucher and free groups remains comparable. In the medium run, however, the impact of the training program on use in the free group is gone. As a result, the difference between the ordeal and free groups with the training program becomes insignificant (5.8 percentage points; p-value 0.21).

Overall, the results suggest that in the short run, the training program caused the ordeal mechanism to screen out some individuals who would have used eyeglasses if provided for free. In the medium run, however, the ordeal mechanism only screened out individuals who would not have used eyeglasses both with the training program and without.

4. Conclusion

Ordeal or self-selection mechanisms have been proposed as a way to improve the

cost-effectiveness of programs that provide subsidized health goods in developing countries (Dupas et al 2013). By sorting out individuals who would have not used a good even if provided for free, ordeal mechanisms can save considerable program resources. There is potential, however, that such mechanisms can sort out too many individuals, as is found in the empirical evidence of cost-sharing strategies, a result that can be counter productive to increasing overall take-up.

In this paper, we examined how a simultaneously-implemented health training program--in addition to subsidization, another commonly-used policy tool to boost take-up rates of health goods in developing countries--affected the performance of an ordeal mechanism. To do so, we implemented a cluster randomized experiment in which schools were first randomized into one of three groups: a.) a control group, b.) an ordeal group in which children with poor eyesight were given vouchers redeemable for free eyeglasses in the county seat, or c.) a group in which all children were delivered free eyeglasses. Half of the schools in each group were then randomly assigned into a cross-cutting health training program that provided information on the benefits of wearing glasses and addressed common misinformation related to wearing glasses.

We find that—in the short run—the ordeal mechanism worked well when not combined with a training program, that is, it only screened out individuals who would not have used the eyeglasses if given them for free. When paired with the training program, however, the ordeal screened out individuals in the short term who would have used glasses if provided them for free. In the medium run, the ordeal performed well (only screening out those who would not have used glasses) both with and without the training program.

While it is difficult to say what lead to these results, we do observe that the training program had a much larger effect on use when eyeglasses were provided for free (at least in the short run). This suggest that results may have been driven by differences in how individuals respond to product-specific health training program when that product was provided to them freely or if they incurred a cost to acquire it.

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Figure 1. Study Region

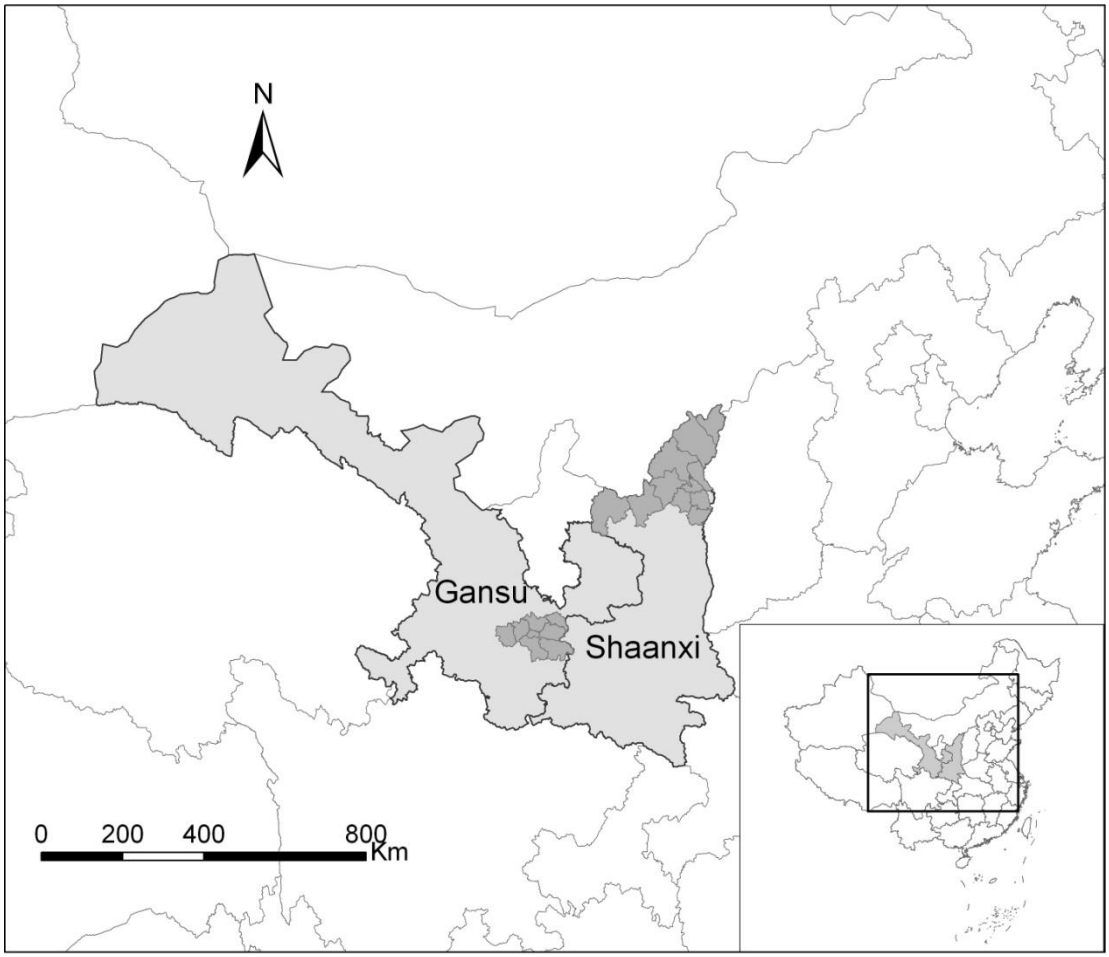


Figure 2: Experimental Design

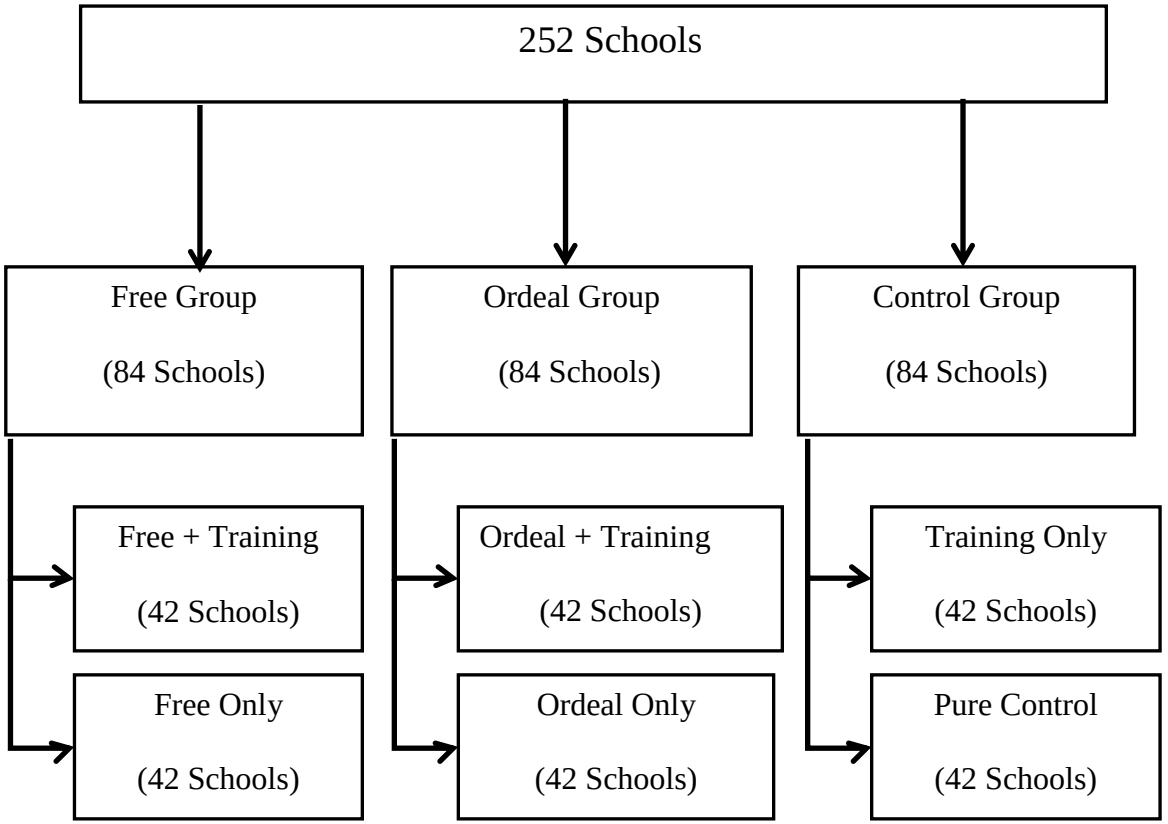


Figure 3: Data Collection and Intervention Timeline

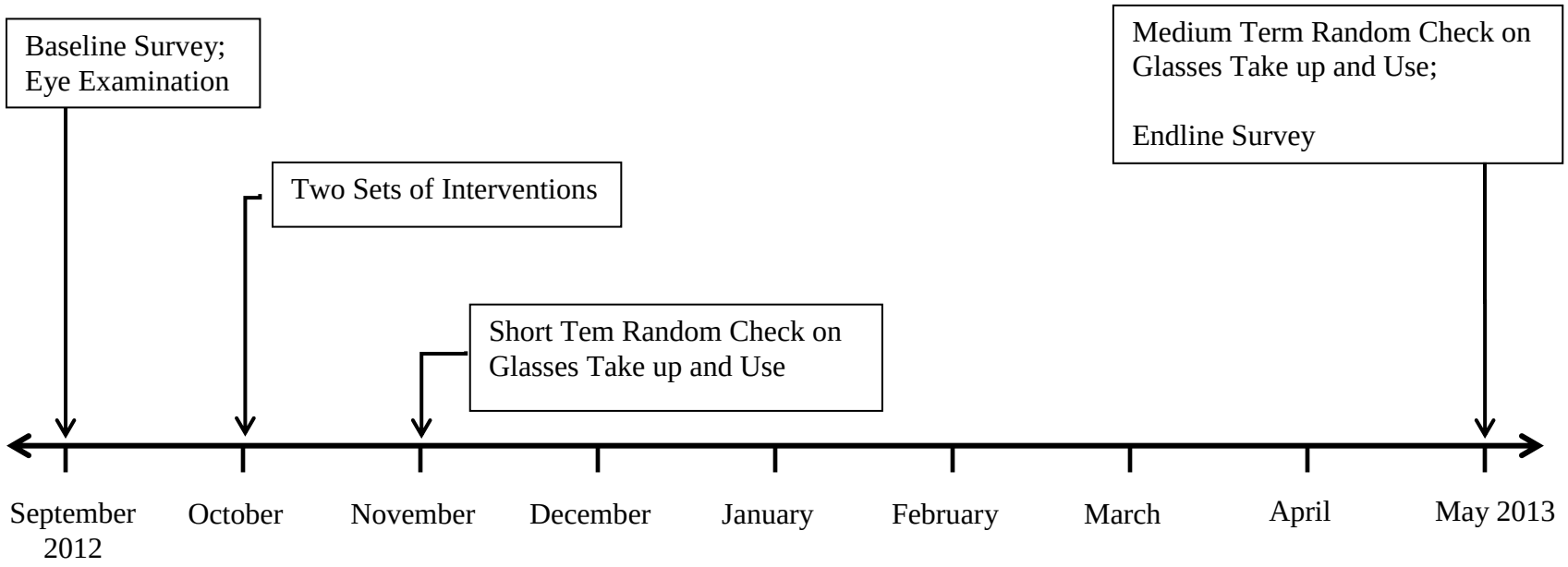
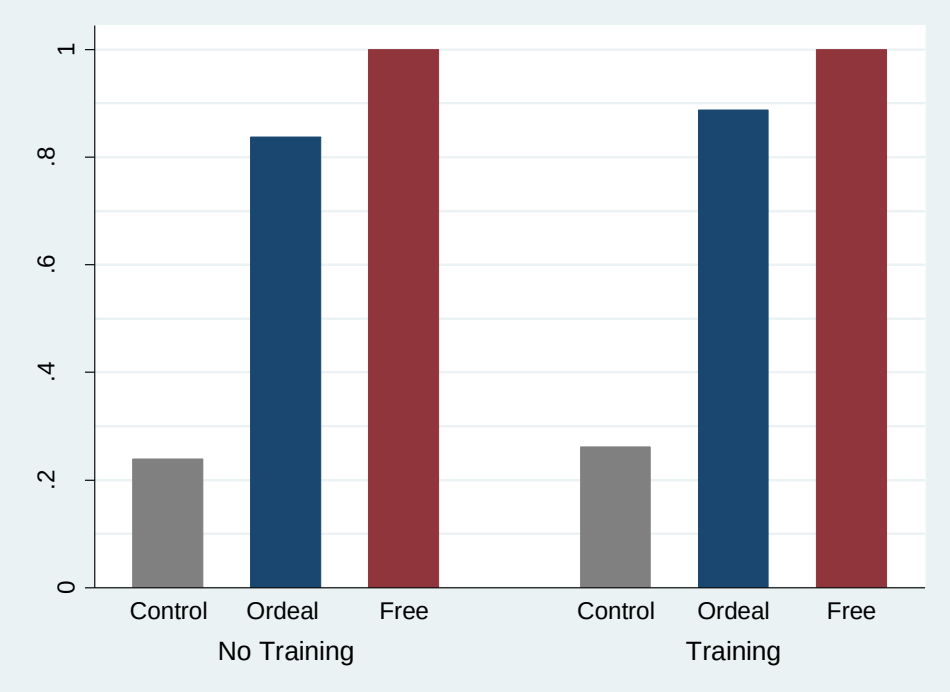
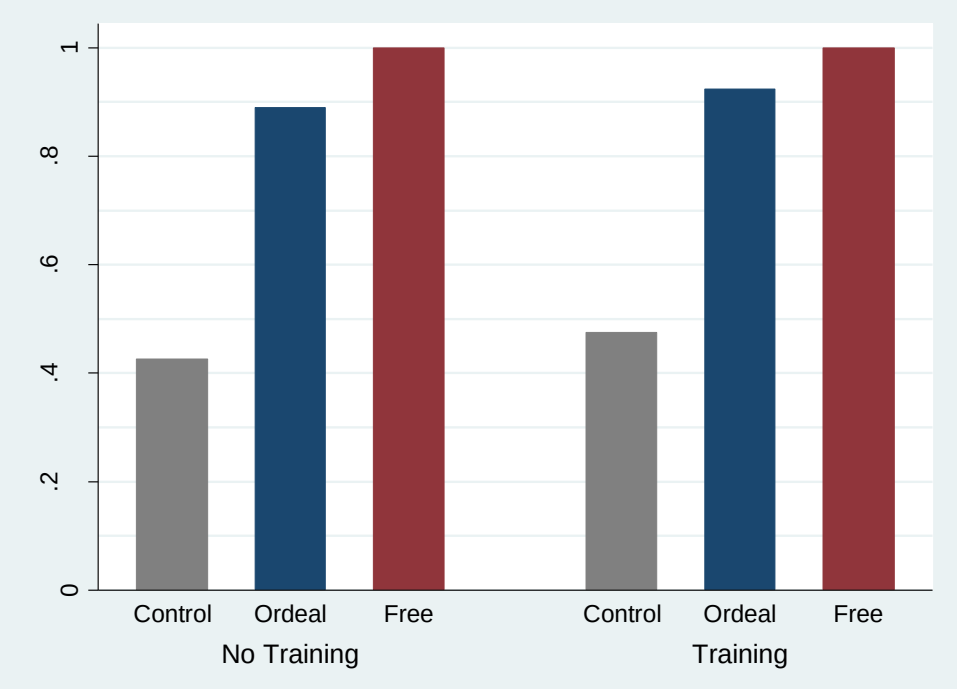


Figure 4: Glasses Take-up Across Treatment Groups Over Time

Panel A. Short Term (Three Weeks Follow up)



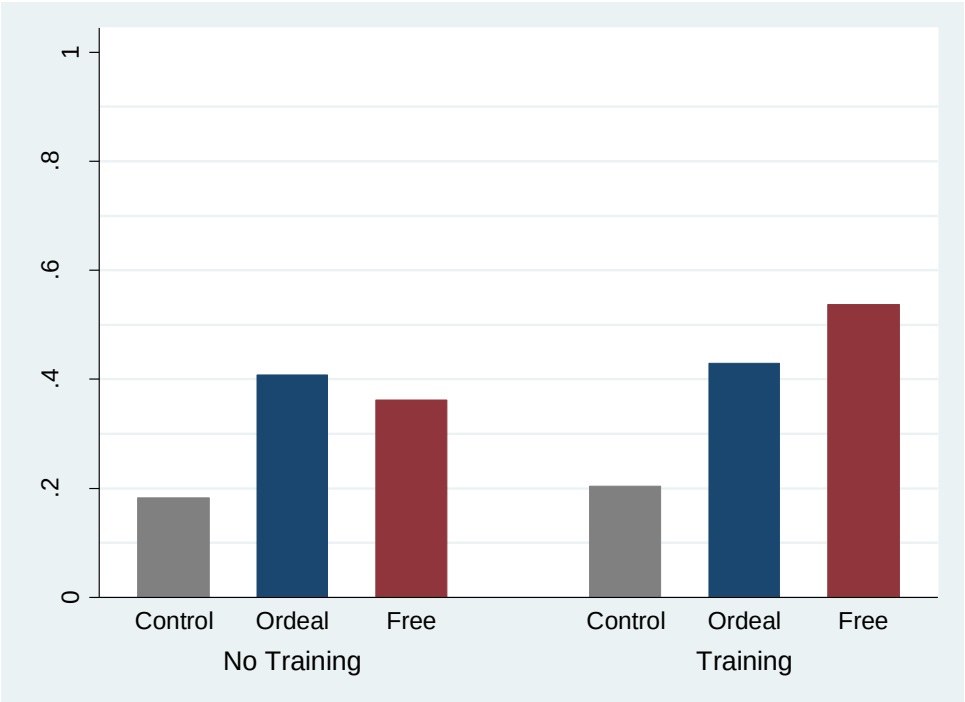
Panel B. Medium Term (Seven Months Follow Up)



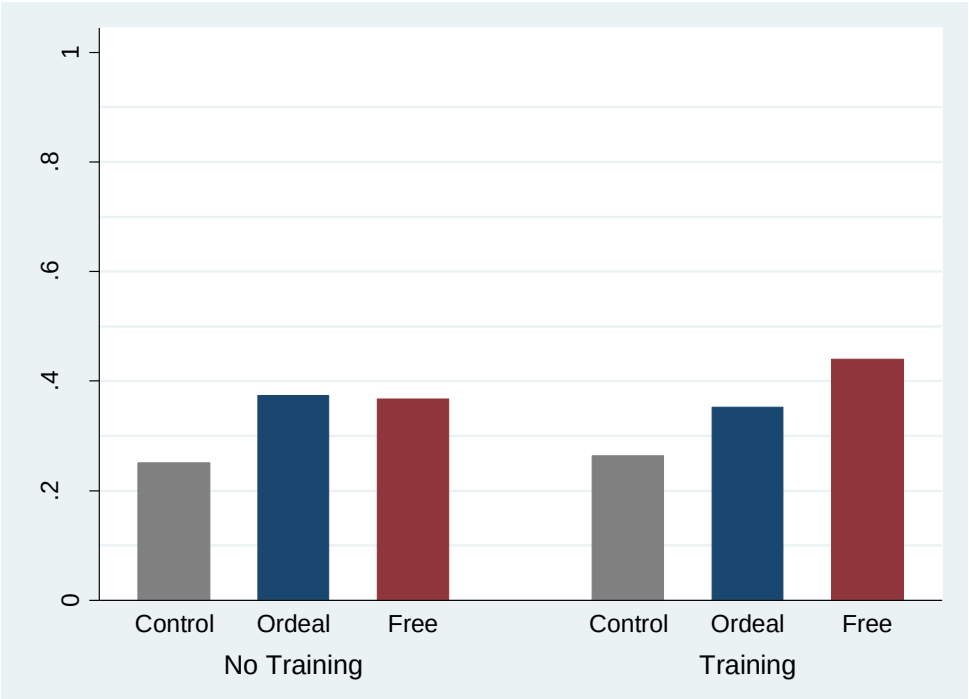
NOTES: Figures show mean values of glasses take-up. Estimated treatment effects on glasses take-up are reported in Table 2.

Figure 5: Glasses Use Across Treatment Groups Over Time

Panel A. Short Term (Three Weeks Follow up)



Panel B. Medium Term (Seven Months Follow Up)



NOTES: Figures show mean values of glasses use. Estimated treatment effects on glasses use are reported in Table 4.

Table 1. Baseline Characteristics Among Treatment Arms

	Total (1)	Control (2)	Ordeal (3)	Free (4)	P value (2) = (3)	P value (2) = (4)
Baseline own glasses (0/1)	0.15	0.14	0.14	0.16	0.98	0.42
	(0.35)	(0.35)	(0.35)	(0.36)		
Baseline severity of myopia	0.56	0.57	0.57	0.54	0.91	0.20
	(0.50)	(0.50)	(0.50)	(0.50)		
Believes Wearing Glasses Will	0.40	0.41	0.37	0.42	0.17	0.69
Harm Vision (0/1)	(0.49)	(0.49)	(0.48)	(0.49)		
Awareness of myopia (0/1)	0.46	0.45	0.47	0.46	0.56	0.87
	(0.50)	(0.50)	(0.50)	(0.50)		
Family glass (0/1)	0.34	0.34	0.32	0.36	0.31	0.51
	(0.48)	(0.48)	(0.47)	(0.48)		
Number of classmates wearing	1.64	1.36	1.47	2.04	0.71	0.10*
glasses	(2.04)	(1.74)	(1.70)	(2.45)		
Gender (0/1)	0.49	0.50	0.48	0.49	0.35	0.68
	(0.50)	(0.50)	(0.50)	(0.50)		
Grade5 (0/1)	0.60	0.60	0.62	0.60	0.39	0.80
	(0.49)	(0.49)	(0.49)	(0.49)		
Boarding (0/1)	0.22	0.23	0.19	0.25	0.41	0.66
	(0.42)	(0.42)	(0.39)	(0.43)		
Parent migration (0/1)	0.10	0.11	0.10	0.09	0.68	0.31
	(0.30)	(0.31)	(0.30)	(0.29)		
Father high school (0/1)	0.16	0.16	0.14	0.17	0.38	0.47
	(0.36)	(0.36)	(0.35)	(0.38)		
Mother high school (0/1)	0.11	0.10	0.08	0.14	0.29	0.03**
	(0.31)	(0.30)	(0.28)	(0.35)		
Household asset	0.00	0.04	0.04	0.06	0.99	0.30
	(1.00)	(0.97)	(0.99)	(1.03)		
Observation	3177	1036	988	1153	2024	2189

Notes: All standard deviations in parentheses. Tests for equality between study arms account for clustering at the school level.

Table 1 Cont. Baseline Characteristics Among Treatment Arms

	No Training (1)	Training (2)	P value (1) = (2)
Baseline Glasses (0/1)	0.16	0.13	0.17
	(0.37)	(0.34)	
Baseline Severity of Myopia	0.56	0.55	0.59
	(0.50)	(0.50)	
Believes Wearing Glasses Will Harm Vision (0/1)	0.39	0.41	0.30
	(0.49)	(0.49)	
Awareness of Myopia (0/1)	0.48	0.45	0.22
	(0.50)	(0.50)	
Family Member Wearing Glass (0/1)	0.34	0.35	0.44
	(0.47)	(0.48)	
Number of Classmates Wearing Glasses	1.64	1.65	0.99
	(1.98)	(2.10)	
Gender (0/1)	0.50	0.48	0.40
	(0.50)	(0.50)	
Grade5 (0/1)	0.61	0.60	0.34
	(0.49)	(0.49)	
Boarding (0/1)	0.21	0.23	0.70
	(0.41)	(0.42)	
Parent Migration (0/1)	0.10	0.10	0.83
	(0.30)	(0.30)	
Father Education	0.16	0.16	0.86
	(0.37)	(0.36)	
Mother Education	0.10	0.12	0.12
	(0.30)	(0.33)	
Household asset	(0.03)	0.03	0.44
	(1.01)	(0.99)	
Observation	1529	1648	3177

Notes: All standard deviations in parentheses. Tests for equality between study arms account for clustering at the school level.

Table 2. Determinants of Voucher Redemption in Ordeal Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Short Term			Medium Term		
Dep. Variable	Voucher Redemption (1 = Yes)			Voucher Redemption (1 = Yes)		
Training	0.051*	0.019	-0.029	0.037	-0.001	-0.038
	(0.028)	(0.034)	(0.044)	(0.024)	(0.031)	(0.036)
Distance	-0.003***			-0.003***		
	(0.001)			(0.001)		
2nd Distance Quartile		0.051	0.018		0.059	0.047
		(0.060)	(0.104)		(0.046)	(0.099)
3rd Distance Quartile		-0.144**	-0.176***		-0.151***	-0.179***
		(0.056)	(0.056)		(0.036)	(0.044)
4th Distance Quartile e		-0.134**	-0.269***		-0.082**	-0.179***
		(0.054)	(0.071)		(0.035)	(0.055)
Training * 2nd Distance Quartile			0.023			0.003
			(0.124)			(0.114)
Training * 3rd Distance Quartile			-0.054			-0.015
			(0.114)			(0.136)
Training * 4th Distance Quartile			0.221***			0.161**
			(0.070)			(0.062)
Controls Included	YES	YES	YES	YES	YES	YES
Observation	947	947	947	947	947	947
*Mean of Dep. Var.*in No Training Ordeal Group		0.84			0.89	
adj. R^2	0.133	0.136	0.141	0.122	0.131	0.133

Notes: Standard deviations clustered at school level are reported in parentheses. Strata fixed effects are included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3. Impact of Treatments on Take-up (Ordeal and Control Groups)

	(1)	(2)	(3)	(4)
	Short Term		Medium Term	
Dep. Variable	Take-up (1 = Own Glasses)		Take-up (1 = Own Glasses)	
Training	0.024	0.050*	0.055	0.076**
	(0.029)	(0.026)	(0.040)	(0.037)
Ordeal	0.585***	0.581***	0.456***	0.459***
	(0.031)	(0.029)	(0.036)	(0.033)
Ordeal*Training	0.035	0.041	-0.019	-0.016
	(0.043)	(0.043)	(0.052)	(0.051)
Controls Included	NO	YES	NO	YES
Observation	1950	1950	1950	1950
Mean of Dep. Variable in Pure Control Group	0.24		0.43	
adj. R^2	0.407	0.540	0.264	0.358

Notes: Standard deviations clustered at school level are reported in parentheses. Strata was included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4. Impact of Treatment on Glasses Use

	(1)	(2)	(3)	(4)
	Short Term		Medium Term	
Dep. Variable	Use (1 = Wear Glasses)		Use (1 = Wear Glasses)	
Training	0.012	0.019	0.027	0.036
	(0.037)	(0.038)	(0.036)	(0.036)
Ordeal	0.229***	0.245***	0.123***	0.127***
	(0.053)	(0.053)	(0.044)	(0.040)
Free	0.153***	0.164***	0.109***	0.105***
	(0.050)	(0.052)	(0.038)	(0.037)
Ordeal*Training	-0.009	-0.027	-0.038	-0.038
	(0.072)	(0.070)	(0.062)	(0.060)
Free*Training	0.169**	0.147**	0.049	0.042
	(0.067)	(0.066)	(0.056)	(0.057)
Controls Included	NO	YES	NO	YES
Observation	252	252	3054	3054
Mean of Dep. Variable in Pure Control Group	0.18	0.18	0.25	0.25
adj. R^2	0.280	0.320	0.089	0.148

Notes: Standard deviations clustered at school level are reported in parentheses. Strata fixed effects are included.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A1. Attrition Between Baseline and Endline

	(1)
Dep. Variable	Attrition (1 = Yes)
Training Only Dummy	-0.015
	(0.011)
Ordeal Only Dummy	0.005
	(0.013)
Ordeal + Training Dummy	0.006
	(0.013)
Free Only Dummy	0.006
	(0.014)
Free + Training Dummy	0.011
	(0.013)
Baseline Glasses (0/1)	0.004
	(0.010)
Baseline Severity of Myopia	-0.001
	(0.002)
Believes Wearing Glasses Will Harm Vision	0.001
	(0.009)
Awareness of Myopia (0/1)	-0.011
	(0.007)
Family Member Wearing Glass (0/1)	-0.006
	(0.007)
Number of Classmates Wearing Glasses	-0.001
	(0.002)
Gender (0/1)	-0.008
	(0.007)
Grade5 (0/1)	0.004
	(0.008)
Boarding (0/1)	0.034***
	(0.011)
Parent Migration (0/1)	0.011
	(0.011)
Father Education	0.003
	(0.008)
Mother Education	0.003
	(0.008)
Household asset	0.000
	(0.000)
N	3177
adj. R^2	0.004

Notes: Standard deviations clustered at school level are reported in parentheses. Strata fixed effects are included.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2 Baseline Awareness and Misinformation

	(1) Understand what is myopia (1= Yes)	(2) Awareness of one's myopia status (1= Yes)	(3) Believe wearing glasses harm one's vision (1= Yes)	(4) Believe eye exercise treats myopia (1 =Yes)
Training Only Group	-0.051	-0.027	0.030	-0.030
	(0.045)	(0.047)	(0.044)	(0.040)
Ordeal Only Group	0.032	0.027	-0.046	-0.047
	(0.051)	(0.041)	(0.048)	(0.047)
Ordeal + Training Group	0.008	-0.017	-0.025	-0.057
	(0.047)	(0.042)	(0.042)	(0.041)
Free Only Group	0.035	0.008	0.004	-0.006
	(0.048)	(0.041)	(0.043)	(0.044)
Free + Training Group	0.018	-0.011	0.038	-0.051
	(0.042)	(0.043)	(0.037)	(0.040)
N	3177	3177	3177	3177
Mean of Dep. Var. in Pure Control Group	0.429	0.465	0.400	0.551
Mean of Dep. Var. in Full Sample	0.437	0.461	0.402	0.519
adj. R^2	0.002	-0.000	0.002	0.000

Notes: Standard deviations clustered at school level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$