Children's care policy and inequality: Evidence from a health screening program in rural China

August 19, 2024

Abstract

This paper examines the distributional effect of vision care screenings on school-aged children's vision, educational and behavioural outcomes using a combination of randomized control trials (RCT) data and longitudinal data on student cohorts in rural China. We first analyze data from a pilot RCT study with multiple treatment interventions. Our results show that screenings, when coupled with follow-up actions, improve children's vision, generate significant impacts on their math test scores and behavioral outcomes, and reduce the social gradient in health. We then study the introduction of a large-scale vision care program, and leverage the exogenous variations in the timing of the program's rollout for the cohort difference-in-differences identification. Our analysis confirms the external validity of the RCT findings and finds that sustained health screenings enhance compliance with prescribed treatment. These findings support care policy implications of equigenesis that access to screenings can buffer social-health gradient inequalities in a low-cost manner.

Keywords: Inequalities; care policy; rural; children
JEL classification: I12, H40, O15

1 Introduction

Health screening is a prominent care policy tool for addressing neglected health issues such as vision loss among rural children. There are evidently significant disparities in health, academic achievements, behavior, and crucial life outcomes among children from rural, developing regions (Engle *et al.*, 2011, Gwatkin, 2017). The potential of routine health screenings to alleviate these disparities is of great policy and scientific interest.

Vision care screening has widely been recognized as an overlooked but critical healthcare instrument in rural areas of developing countries (Wong *et al.*, 2009). Globally, the expenditures related to vision for children are substantial. Vision loss is an important health impairment influencing education achievement, and many aspects of quality of life for school-aged children (Bourne *et al.*, 2017, Halla *et al.*, 2016, Glewwe *et al.*, 2016, WHO, 2019, Pruckner *et al.*, 2021). More than half of world's rural populations suffer from limited vision healthcare access, with the most severe conditions prevalent in Africa, where over 80% of rural inhabitants lack access to essential healthcare services (Scheil-Adlung, 2015). Recent evidence from Asia shows that myopia comprises about 48% of disability among children aged 5 to 9 years in India, and has become a significant and common eye problem for more than 12 million children aged between 5 and 15 years in China (Ma *et al.*, 2014). However, it remains unclear whether routine vision screening leads to improved vision outcomes.

In this paper, we assess the distributional effects of the care-based screening program on school-aged children's vision, educational and behavioural outcomes using a combination of randomized controlled trials (RCT) data and longitudinal cohort data. Our assessment carries out in two empirical settings within vast but understudied rural contexts of China. We first examine a pilot RCT study conducted in 2012-2013, involving 251 schools where grade 4-5 students with myopia were randomly assigned to five treatment groups with different types of vision screening programs, as well as a control group. We find that vision screenings have significant effects on vision, math test scores, and behavioral outcomes. We use a machine learning approach to quantify the disparities in screening effects with respect to children and home environment characteristics. We next assess a scaled-up version of the vision screening program, which has provided routine vision screenings to grade 4-9 students in 149 schools since 2014. We leverage the exogenous variations in the timing of the program's rollout for the cohort difference-in-differences identification, and find that the screenings significantly improve children's vision, which supports the external validity of the findings from the pilot RCT. The second empirical setting further allows us to identify the informational nudging channels in repeated rounds of vision screenings. The policy implications of this study are important and directly informative for assessing whether healthcare resources are allocated effectively, and for understanding the role of health screenings in shaping inequalities for hundreds of millions of children in rural areas of the developing world who are exposed to overlooked health risks (Bharadwaj *et al.*, 2013, Falk *et al.*, 2021, Hong *et al.*, 2019, Krämer *et al.*, 2021, Okeke and Abubakar, 2020).

Non-random sorting of individuals into care policy assignment is an important challenge for empirical assessment in many previous studies, as it can result in correlation between unobserved individual characteristics and outcomes through non-causal channels. This sorting makes it difficult to disentangle whether differences in individual outcomes are attributable to care programs, or to differences between individuals. This is critical because the characteristics of children are closely linked with their families, who choose where to live and which schools to attend on the basis of residential preferences for healthcare services, amenities and potential constraints. Further, there remains concerns about unobserved differences between schools. We address these challenges by implementing randomization in the pilot RCT and leveraging plausibly exogenous variations in the timing of the scaled-up vision screening program's roll-out.

China provides a unique setting for studying the effects of health screening. About half of childhood vision problems worldwide occur in China (Yi *et al.*, 2015). In our empirical settings, we have precise measurement on vision provided by trained ophthalmologists and rich information on individual-level outcomes. Before the implementation of our program, children in the study area had not undergone vision screenings, and they were unaware of this research during their vision screening process. Through vision screening, accurate identification of myopia enables one to obtain appropriate eyeglasses, which is shown to slow down the further progression of myopia (Sylvia *et al.*, 2022). By providing nudge-type healthcare information interventions, vision screenings generate significant and far-reaching impacts on outcomes related to quality of life (Hong *et al.*, 2019, Sunstein, 2022).

The first empirical setting examines results from the pilot RCT study. With the support of the education administration bureau from the local government, this RCT study covers myopic students in grade 4-5 from primary schools in the study area. Five different treatment specifications are considered, including combinations of health education, and for students lacking appropriate eyeglasses, providing vouchers redeemable for free eyeglasses, or providing free eyeglasses immediately. The RCT also collects detailed individual level information which enables us to study the heterogeneity in treatment effects.

In the second empirical setting, our vision care screening program dataset covers grade 4-9 students from all eligible primary and secondary schools in the study area. An important feature is that we can make use of the exogenous variations in the timing of the program's rollout. Our assessment therefore provides new evidence on the external validity of the RCT findings through scaling up the vision screening program. We also take advantage of the program's multiple survey waves and follow-up intervention variations to decompose early and sustained screening effects as underlying informational nudging channels at work. To our knowledge, our analysis is conducted with the most comprehensive vision care data file ever compiled on school-aged children in rural settings of a developing country (Guan *et al.*, 2024).

This study extends the work on the social science of care policies in rural contexts in the developing world by quantifying the beneficial effects of early health screening. There is a substantial literature evaluating the relationship between health screenings and important life outcomes (Falk *et al.*, 2021, Krämer *et al.*, 2021, Okeke and Abubakar, 2020). However, previous studies have largely only been reported in observational studies that often lack strong causal identification (Williams *et al.*, 2002). A causal identification is difficult to uncover from observational studies due to confounding factors and reverse causation. For example, in the vision screening context, many observational-based studies are likely to confound vision with unobserved determinants of eye health that are correlated with screenings. In the most closely related papers to our study, Glewwe *et al.* (2016) use the RCT experiment design to reveal the significant impact of providing free eyeglasses on academic outcomes of primary school children in western China. Glewwe *et al.* (2018) provide RCT evidence on the impact of vision screening and free eyeglass provision on educational achievement in US neighborhood contexts. We contribute to the literature by using accurate measures of visual acuity and health-related behavioral outcomes, focusing on the social gradient in health, and incorporating both RCT and cohort difference-in-differences estimation as complementary empirical strategies for causal identification. We elaborate on these points in the following paragraphs.

This study contributes to the existing literature on the social gradient in health within rural care policy frameworks. For example, Abu-Qarn and Lichtman-Sadot (2022) find that people from disadvantaged backgrounds benefit more from easier access to health screenings. Mahler and Yum (2024) show that when public and private educational input are substitutes, greater public spending on education can decrease educational inequalities among children. We use machine learning techniques to quantify the heterogeneities in the impact of vision screening based on individual characteristics (Chernozhukov *et al.*, 2017). Our study confirms and extends findings from recent studies that health screening can reduce the social gradient in health.

This study is related to the literature evaluating various aspects of the external validity of RCT studies. Conventional RCT experiments like ours typically have a relatively small sample size, but they can provide the most convincing causal evidence (Mathers *et al.*, 2010, Sylvia *et al.*, 2022). In our first RCT of vision screening, treatment was randomized to specific grades of primary schools in sampled rural counties. Our rich data allows us to take advantage of the variation in multiple treatment interventions to explore the distributional screening effects. However, this laboratory-based experiment cannot establish the operational dynamics of daily healthcare infrastructure. Given the massive RCT expenditures for scientific research, it is important that the impact of rural childcare screenings on important life outcomes be evaluated through a sustainable program in addition to the one-time off experiment. As such our second empirical setting incorporates data and infrastructure refinements to address limitations in previous research (Williams *et al.*, 2002). Specifically, our rural childcare screening center has been established to operate a sustained care program providing multiple rounds of vision screening. In doing so we obtain a unique and longitudinal screening cohort data for school-aged children in rural China. Our analysis confirms the external validity of the RCT findings by exploiting a scaled-up vision care program in rural schools through the application of a cohort difference-in-differences strategy. Our longitudinal screening cohort data also provide useful guidance to care policymakers responsible for rural healthcare resource allocations, suggesting that health screening should be combined with follow-up treatment to achieve more long-lasting health benefits.

Finally, this study is important to furthering our understanding of how children health inequalities can be reduced through informational nudging channels. Poor vision and myopia progression in rural areas of developing countries are much more severe problems than in developed countries (Matsumura *et al.*, 2019). Our mechanism analysis shows that screening decreases health inequalities through informational nudging channels. First, we find that the observed effects are driven by the early screening mechanism. In particular, the results could be accounted by the dose-response relationship found in the literature (Mallah *et al.*, 2022), where health screening performs as a salient nudge-type learning information channel. Second, sustained vision screenings can enhance compliance with prescribed treatment. Previous literature has shown that imperfect compliance with medical advice reduces the effectiveness of healthcare programs (Sylvia *et al.*, 2022), and we show that compliance with prescribed treatment can be enhanced with sustained health screenings. These findings relate to the theoretically motivated discussions in the care policy literature, and serve to emphasize the importance of the complementary role between public investment and private investment in social and health care resource allocations.

The remainder of this paper proceeds as follows. Section 2 describes the data. Section 3 layouts the empirical strategy. Section 4 presents the results. Section 5 concludes.

2 Data

Our study is built upon a long-term and large-scale survey project called "Seeing is Learning" conducted in rural areas of Shaanxi and Gansu provinces located in Northwest China. The project aims to mitigate the deterioration of children's vision and promote human capital accumulation among rural school students. It is administered by the Center for Experimental Economics in Education at Shaanxi Normal University. Launched in 2013, the project has continuously tracked the vision health of students in rural Northwest China, gradually expanding from a randomized controlled trial (RCT) study to a sustained vision screening program conducted regularly over the years. By collaborating with local government agencies and relevant organizations, the project established the Children Vision Center in each county, providing healthcare services such as vision screenings and follow-up services, including eyeglasses for myopic children. The resulting database is the most comprehensive and accessible record on school-aged children's vision and myopia progression in rural China, with identifiers available at the individual-by-grade-by-school level (Guan *et al.*, 2024). The purpose and workflow of the project are illustrated in Figure 1. The timeline of the two data settings is shown in Figure 2.

Our analysis focuses on the impact of vision screening on children's vision, and educational and behavioral outcomes. The data richness allows us to use a combination of the RCT approach and a cohort DiD (Difference-in-Differences) strategy for the empirical assessment. The RCT data is used to examine the effects on vision from screenings, and the individual panel data for the cohort DiD strategy is used to explore the dynamic screening effects. In particular, the longitudinal cohort data with multiple survey waves enables us to assess the external validity of the RCT evidence and document the informational nudging channels from sustained interventions. As far as we know, this is the first type of application in rural contexts from the developing world.

The dataset has several unique and attractive features. First, vision is measured by professionally trained staff which should be more accurate than those based on self-report. This precision is important for our fieldwork-based, quasi-experimental study, which relies largely on comparisons between cohorts that undergo different numbers of screening due to variations in the timing of the screening program's roll-out. Data such as the China Education Panel Survey also collect information on self-reported vision status (Huang *et al.*, 2022). An important assumption in previous studies examining children's healthcare access is that unbiased or complete information regarding vision is available. However, this assumption becomes questionable when vision data relies solely on self-reporting, especially considering that visual acuity among children tends to be fluid and subject to change. Second, there is no other concurrent treatment that could confound the effects. Rural school-aged children cohorts in our sample had not undergone vision screenings from schools or other agencies before the program's implementation, and they were not aware of this research at the time when they screened their vision.

2.1 The RCT data

During 2012 and 2013, the RCT study was conducted in Tianshui and Yulin prefectures, located in Gansu and Shaanxi provinces of northwestern China. 20,594 responses from students in the fourth and fifth grades have been collected in this cluster RCT program, which is designed to evaluate the effectiveness of treatment-control interventions addressing myopia (see Ma *et al.* (2014) for details). Specifically, the RCT dataset offers the advantage with respect to the randomization and the availability of a rich measure of behavior outcomes toward vision health. The RCT design includes a baseline survey wave and an evaluation survey wave, and the program implementation unfolds as follows.

At the baseline survey wave, students underwent a preliminary visual acuity screening at their respective schools, administered by one ophthalmologist and two ophthalmic nurses who have been certified as qualified refractionists by China's Ministry of Labor and Social Security. To rigorously evaluate vision, vision screening tests examine eyes without refraction using the Early Treatment Diabetic Retinopathy Study (ETDRS) charts (Camparini *et al.*, 2001). In particular, children who wear eyeglasses are required to be tested both with and without eyeglasses during the vision screening process. Visual acuity is tested separately for each eye without refraction at 4-meter using ETDRS charts (Precision Vision) in a well-lighted indoor area. Students owning eyeglasses are requested to bring them to school, and their visual acuity is tested with and without correction. To avoid potential staff member specific measurement errors, ophthalmologists and nurses employed in the vision center have followed a standardized vision screening check procedure through completing a one-month training program at China's leading ophthalmology institution, Zhongshan Ophthalmic Center in Guangzhou. Visual acuity for an eye is defined as the lowest line on which 4 of 5 optotypes are read correctly. If the top line could not be read at 4-meter, the participant is tested at 1 meter, and the measured visual acuity is divided by 4. Within the scope of ophthalmology practices, we have adopted the conventional categorization and reporting of visual acuity (VA) results.

Subsequent to the baseline survey and vision screening but preceding the refractive assessment, a total of 3,177 myopic students located in 251 primary schools, as identified based on the results of the baseline visual acuity screening, are selected via randomization at the school level to participate in the following interventions. The first intervention is the provision of free eyeglasses, wherein students receive free eyeglasses based on the student's measured refractive condition which are dispensed at school by the study optometrist. Parents of these students receive notifications regarding the free eyeglass intervention, along with their child's prescription details. The second intervention is the provision of the free eyeglasses voucher, which are non-transferable and non-marketable. Students are mandated to present their school identification for voucher redemption. Parents are responsible for covering the transportation expenses associated with redeeming the eyeglasses, which are available at the county hospital located at a median distance of 30 kilometers from the students' respective townships (with a range spanning from 1-105 kilometers). The third intervention is the provision of education focusing on vision health, wherein professional optometrists are engaged to encourage myopic students to utilize eyeglasses. Based on the combination of the aforementioned interventions, five distinct treatment groups are delineated as follows: (1) the sole education group, (2) the sole free eyeglasses voucher group, (3) the sole free eyeglasses group, (4) the group that receives both the free eyeglasses voucher and education and (5) the group that receives both the free eyeglasses and education. The control group, in contrast, does not receive any form of intervention. At the evaluation survey wave, students participating in the cluster randomized controlled trial program undergo a subsequent visual acuity screening, followed by the completion of the baseline questionnaire once more. In doing so the randomization provides a valid counterfactual.

Turning onto the outcome of interest, our key outcome is vision. Following the convention in the science of ophthalmology and optometry (Yi *et al.*, 2015), the Minimum Angle of Resolution (MAR) that reflects the inverse of visual acuity is defined as a continuous measurement scale for vision. This scale has been widely used in the literature by taking the logarithm transformation: e.g., LogMAR = $\log_{10}$ MAR. Put simply, the constant increment of LogMAR is approximately 0.1, and each increment represents an increase of one line in the ETDRS. LogMAR values range from -0.3 to 1.6 and we determine LogMAR using the uncorrected visual acuity of the better eye. A higher LogMAR value indicates poorer vision. As suggested by the ophthalmology literature, myopia is a binary variable that equals to one when an individual's LogMAR is greater than or equal to 0.3 in the screening tests. This study defines normal vision as having LogMAR < 0.3.

The RCT program collects rich data on individual behavior variables, including knowledge about vision health, habits, attitudes, and parenting traits. For vision health knowledge, we have collected questionnaire responses related to knowledge about myopia and eye care. In terms of habits and attitudes towards vision health, the data include eye use duration, eye care practices, and attitudes towards wearing glasses. We focus, in particular, on whether parenting plays a role in interacting with screenings that influence vision outcomes. Parenting-related indicators include the willingness to take their children for vision screening. These are outcomes that have been previously examined in the health literature (Glewwe *et al.*, 2016). As there are many correlated indicators and multiple variable scales, we construct a single, standardized measure for each type of behavior category and perform the empirical analysis (Braghieri *et al.*, 2022). The explanations regarding the data composition can be found in Appendix Table A6.

The human capital outcomes in the RCT data consist of the standardized scores in mathematics tests and mental health assessments conducted both before and after the implementation of various follow-up interventions. The standardized math test was administered and supervised by the survey team within the classroom, allowing students 25 minutes to complete it. This test was adapted from a subset of the Trends in International Mathematics and Science Study (TIMSS) (Martin and Kelly, 1996). The Mental Health Test, created by Prof. Bucheng Zhou of East China Normal University, is derived from the General Anxiety Test by Kiyoshi Suzuki in Japan (Zhou, 1991) which is a globally standardized assessment for children's anxiety, widely utilized in the United States, China, and other developed and developing countries (Reynolds and Richmond, 1997, Zhang *et al.*, 2013). Higher mental health test scores represent higher levels of anxiety.

2.2 The longitudinal cohort data

The longitudinal cohort data is an individual-level database of the universe of students from grades 4 to 9 of all primary and secondary schools in four rural counties in Shaanxi and Gansu Province in northwestern China. The implementation of vision screenings is proceeded as follows. At the baseline screening wave in 2014, we obtain a list of all primary and secondary schools from the Bureau of Education of the four rural counties. These counties have similar geographical and climate characteristics since they are spatially adjacent. In terms of demographic characteristics, they had all been listed as nationally designated poor counties at the beginning of the program. We conduct a vision screening for all enrolled students in grades 4 to 9 in these rural counties from 2014 to 2019. There are 368 schools in four rural counties, covering 53,737 students in total. Each county has undergone at least two screening rounds. For each screening round, the vision screening test is carried out by the vision center in each county. The operations of the vision center's screening tests are identical to the RCT program outlined above.

The timeline of vision screenings has been implemented on a random rolling basis. For each week, the vision center will randomly select 2-4 schools from a county to proceed the screening checks for all eligible students at grade 4-9. On average, the completion of a screening wave for an entire county's schools takes a year. After the first round of the vision screening in a county, the vision center will start to organize the follow-up round of vision screenings in the subsequent year with the same procedure. As part of the screening program, children respond to survey questions about their personal characteristics and home environment. Students who do not meet the visual acuity criteria are referred to the county-level vision center for follow-up screening interventions. To be specific, if a rural student, or a student from the county town of Ningshan, is identified as myopic and lacking appropriate eyeglasses, the vision center will offer a free eyeglass voucher. Students with vouchers are encouraged to visit the center to use the voucher and receive vision care services. It should be noted that only students living in rural villages will receive free care services. There is no need for students and their families to invest any monetary resources other than the time to visit the vision center to receive additional treatment care services. However, students living in county town centers, except those in Ningshan County, will need to pay for follow-up screening actions if needed. Variations in the provision of free eyeglass vouchers are used in our model setups to identify the moderating role of follow-up screening interventions on the effects of screenings.

Our empirical strategy leverages variations in the timing of the implementation of the vision screening program between counties. We identify the exact timeline of multiple screening waves from the longitudinal vision screening data. After sample selection based on the cohort difference-in-differences design (Figure A1), the final sample includes 19,084 students located in 149 schools in three counties from 2018 to 2019.

2.3 Summary statistics

Table 1 presents summary statistics for the outcomes of interest and main covariates in both the RCT and the longitudinal cohort data. The former dataset includes 3,177 myopic students from grades 4 to 5, while the latter dataset consists of 19,084 students from grades 4 to 9. In both datasets, students' vision is defined as a continuous variable as measured by the LogMAR, whereas the presence of myopia refers to a binary indicator that equals to one if the student's LogMAR is above 0.3. In the vision screening data, the sample average for LogMAR is 0.238, while the corresponding value in RCT data is 0.754. The average myopia rate in the vision screening data is 39.6%, which is significantly higher than the global average of 33.8% as reported by the World Health Organization in 2019, but slightly lower than the national average in 2017.[1] Regarding key covariates, a consistent pattern emerges across both datasets in terms of individual and home environment, owing to the surveys being conducted within adjacent provinces. Notably, parents of students exhibit relatively low educational attainment, with an average education level between primary and

[1]Report on the quality of compulsory education in China, http://www.moe.gov.cn/jyb_xwfb/moe_1946/fj_2018/201807/P020180724685827455405.pdf.

secondary school. Additionally, approximately half of the students have fathers who have migrated, indicating that the students in our sample receive limited parental support and guidance.

3 Research design

We begin with a pilot study using an RCT design to identify the average and distributional impact of vision screening (Subsection 3.1). After the pilot study, the screening program was rolled out to more locations. Based on comparisons between cohorts and counties that differ in the timing of program roll-out or the extent of program coverage, and adopting a difference-in-differences strategy, we corroborate the findings from the RCT using the observational data (Subsection 3.2). Analyzing data from both research designs allows us to harness the strengths of each empirical approach.

3.1 RCT

The analysis using the RCT data consists of the following two main components. Firstly, we quantify the average effects of vision screening on children's vision, learning and behavioral outcomes. The empirical model is

$$Y = D\alpha + X\beta + FE(\text{county}) + \epsilon,$$

where Y is a series of outcomes, including LogMAR, math score, mental health score, and vision health-related behaviors. $D = I(\text{treatment group})$ and the parameter of interest is α.

Secondly, we utilize the rich information on children's home environment and apply a recently developed machine learning tool to examine treatment effect heterogeneities in a systematic way (Chernozhukov *et al.*, 2017), shedding light on the impact of vision screening on vision health inequalities. The algorithm consists of the following steps. The sample comprises children who were randomly allocated to either one of the treatment groups or the control group. The sample is then randomly split into an auxiliary sample and a main sample of equal sizes. We then estimate the conditional average treatment effect function $S(X) = E(Y|D = 1, X) - E(Y|D = 0, X)$ using one of the three machine learning algorithms: random forest, elastic net, and support vector machine. The estimation uses the auxiliary sample only. Finally, we use the estimated $\hat{S}(X)$ to assign individuals in the main sample into groups that are predicted to be most affected or least affected by the treatment, and examine whether these two groups differ in the observable characteristics. An advantage of this method is that the validity of the statistical inference does not depend on type of machine learning algorithm used.

3.2 A cohort difference-in-differences model

First, we quantify the effect of vision screening and assess the external validity of the RCT study. Because vision is not observed before children participated in the vision screening, we cannot use conventional causal inference methods based on comparing changes in individual outcomes before and after the vision screening. Instead, we take advantage of the variations in the timing of the implementation of the vision screening program between counties and use a difference-in-differences strategy based on comparison between cohorts that underwent varying numbers of rounds of vision screening. Let c, t denote the county and cohort, respectively.

$$Y_{ct} = I(\text{early screening county}) \times I(\text{cohort} \geq 1)\tau + X\beta + \text{FE}(\text{school, cohort, wave}) + \epsilon_t.$$

The red dotted lines of Figure A1 in the appendix indicate the sample used in this analysis and how cohorts are numbered. Y_{ct} is an outcome of interest, such as LogMAR or an indicator of myopia. X is a vector of control variables. School, cohort, and survey wave fixed effects are included.

Let cohort s denote children who were at grade $4 + s$ in 2018 (Figure A1). In Huachi

County and Pingli County, which are denoted as *early screening counties*, the first round of vision screening was carried out in 2017, while in Ningshan County (*late screening county*), the first round was in 2018. In 2018, children from grades 5 and above, specifically cohorts 1 to 4, in the early screening counties, underwent an extra round of vision screening in comparison to their counterparts in the late screening county. Similarly in 2019, children in cohorts 1 to 4 in the early screening counties participated in 3 rounds of screening, which was one more round than children in the same cohorts in the late screening county. Since vision screening is only available to children in grades 4 and up, those in cohort 0 received the same number of rounds in both early and late screening counties in 2018 and 2019. The effect of one more round of vision screening can be identified by comparing the outcomes of children in cohorts 1 to 4 and cohort 0, and between early and late screening counties.

Under the assumption that in the absence of one more round of vision screening, the difference in vision of children in older and younger cohorts would be the same between the early and late screening counties, a cohort difference-in-differences strategy can be used. The key parameter of interest is τ, which identifies the effect of early vision screening on vision.

After vision screening, some myopic children who did not have proper eyeglasses were offered vouchers that they could use to redeem free eyeglasses. Using differences in program coverage across rural and urban areas, we can further identify the impact of giving the eyeglasses voucher, conditioning on children undergoing vision screening, which is an intent-to-treat event.

Second, we perform analysis on the distributional and dynamic effects of vision screening. We assess the effect of vision screening on vision health inequalities. The individual level data are aggregated by classes and the outcome variables include class-level standard deviations and the interquartile range of LogMAR values. In areas where vision screening is integrated with subsequent interventions, children diagnosed with myopia but lacking eyeglasses were offered vouchers redeemable for free eyeglasses at county vision centers. We identify the date when vouchers were redeemed, and construct a class by month variable measuring the proportion of redeemed vouchers. The effect of subsequent rounds of vision screening on voucher take-up can be identified by a difference-in-differences strategy leveraging the differences in timing of the subsequent screening rounds.

The cohort DID approach not only demonstrates the external validity of the RCT findings in wider spatial contexts, but also provides three complementary sets of results. First, only myopic children participated in the RCT study, while the regular vision screening program also covers children with normal vision, which enables us to assess whether earlier vision screening contributes to prevent the onset of myopia. Second, the regular vision screening program is conceived as an ongoing health service featuring a streamlined procedure that is more straightforward and cheaper than the treatments employed in the RCT. By assessing the effectiveness of the regular vision screening program, we can evaluate the program's scalability. Third, as the vision screening program operates on a routine basis, we can study the dynamic effects of the program over an extended time period.

4 Results

4.1 Results from the RCT setting

The pilot RCT of the vision screening programs provides a clean identification on the treatment effects. The RCT data includes detailed information on health-related behavior and outcomes. A variety of interventions are implemented for the treated units, including combinations of vision health education, voucher provision or free eyeglasses provision. We compare the children assigned to various interventions with those in the control group who receive no treatment, thereby quantifying the impact of the interventions. Table 2 reports three main findings. First, as shown in columns (1)-(3), we confirm the findings in the previous literature that vision screenings improve vision (Glewwe *et al.*, 2016, 2018, Ma *et al.*, 2014). The effects are stronger when free eyeglasses or vouchers redeemable for free eyeglasses are provided. Better vision contributes to the improvement in math scores. The effects on mental health are generally not statistically significant.

Second, the effects of vision screenings, and health screenings in general, work through many channels. They can identify health conditions and facilitate timely treatment, known as secondary prevention. They can also change people's health-related behavior. Among the combinations of different types of interventions, health education is most effective in improving recipients' health-related attitudes, knowledge and their parents' awareness (columns 4-7 of Table 2).[2] The findings suggest that a combination of medical treatment and education yields optimal outcomes.

Third, providing eyeglasses vouchers or free eyeglasses decreases the likelihood of parents taking their children to a vision screening (Table 2 Column 7). The provision of eyeglasses vouchers or free eyeglasses substitutes the family's health-related input, which suggests that for vision screening, public provision of the service substitute the private input. A policy implication of the finding is that public provision of health screening can mitigate health disparities caused by inequalities in the socioeconomic conditions of the children's families (Amaral *et al.*, 2024). In a related context of public and private investment in children's education, Mahler and Yum (2024) discuss the implications of the degree of substitution between public and private input on children's inequalities in academic achievement and show that the achievement inequalities can be reduced with more public investment when public and private input are substitutes.

To provide further evidence on the effect of vision screening on health disparities, we apply the machine learning techniques (Chernozhukov *et al.*, 2017) to systematically assess if the effects of vision screening vary along the observed characteristics. In this analysis, individuals are considered to belong to the treatment group if they receive any of the five possible combinations of treatments as indicated in Table 2. The sample is first split into a main sample and an auxiliary sample. Using the auxiliary sample, the machine learning algorithms provide an estimate on the treatment effect of vision screening on individuals with specific

[2] Appendix Table A6 describes the items that are used to construct the measures of vision-related behaviors

characteristics. We then sort the main sample by the predicted treatment effect (Appendix Figure A2) and contrast the attributes of those belonging to the top 20% most impacted and the bottom 20% least impacted individuals. Table 3 shows that characteristics associated with larger treatment effects are poorer baseline vision, attendance at boarding schools, a history of repeating a grade, and maternal absence. These factors are statistically significant at 90% taking into account the uncertainty associated with first stage machine learning estimates on the treatment effects and the results hold across three types of machine learning algorithms used. The findings indicate that children with disadvantaged home environment benefit more from the vision screening program, which, coupled with the substitution of public for private health input, contributes to a reduction in vision disparity among children.

4.2 Results from the cohort DID setting

We now proceed with analyzing the observational data gathered from the vision screening program following its expansion to additional areas. Using the cohort DID approach, Table 4 presents the estimation results of the effect of the vision care program. The dependent variable is a measure of visual acuity (LogMAR) which is relevant to both children with normal vision and myopic children, or a binary indicator on whether the child is myopic. We find that children who receive one more round of vision screening at younger ages have better vision (Column 2), which is an improvement of about 0.25 line on the visual acuity chart. The effect is stronger for children in areas where vision screening is coupled with providing eyeglass vouchers to those children lacking appropriate eyeglasses ("Follow-up" sample), which reports an improvement of 0.32 line on the visual acuity chart (Column 3). Early vision screening also reduces the incidence of myopia, with stronger effects in the follow-up sample as well.

In Appendix A, we report results from supplementary analysis showing that vision screening has a greater impact on vision health when combined with providing easier access to treatment and when participants comply with doctors' advice.

Robustness: The identification of the effect of the vision care program is based on the assumption that had the early screening counties conducted the screening at the same time as other counties, the average differences in vision between cohort 1-4 and cohort 0 would be the same between these counties (Figure A1). While we cannot conduct the traditional tests on parallel pre-trends as there is only one time period before treatment (cohort 0), we provide suggestive evidence that there are no systematic between-cohort differences between these counties in the absence of differences in the rounds of vision screening by checking whether there are post-treatment heterogeneities between cohorts 1-4 across early and late screening counties. The results are reported in Appendix Table A3. In these tests, the cohort indicated in the column is the placebo treatment cohort while the cohort indicated in the row is the control, and each cell reports the coefficient of a separate DID regression. We find that most of the coefficients are not statistically significant, providing evidence that there are no systematic between-county differences between cohorts that receive the same number of vision screening.

To further assess whether there exists unobserved confounding factors that drive the results, we estimate the model using random subsamples. In each iteration, 10 schools from the baseline sample are randomly dropped, and the process is repeated 1,000 times. Figure 3 plots the distribution of the 1,000 estimates. The estimates from the smaller samples cluster tightly around the estimates from the full sample.

Distributional effects: We perform the analysis on class level variables to assess the distributional effects of vision screening. Consistent with individual level results, class average LogMAR decreases by 0.026 in early screening classes (Table 6 Column 2). Moreover, our findings indicate that vision screening mitigates vision disparities among students within classes, as demonstrated by a decrease in the standard deviation of LogMAR scores, a narrowing of the gap between the 90th and 10th percentiles, and a reduction in the difference between the 75th and 25th percentiles (Table 6 Columns 3-6). Mechanisms by which vision screening contributes to the reduction of vision disparities is the substitution of private health expenditure with public health expenditure and the larger program effects for children from disadvantaged backgrounds, as discussed in Subsection 4.1.

The effectiveness of vision screenings varies depending on pre-existing vision conditions. Early detection of vision problems through screening can raise awareness among children and their parents, enabling timely corrective measures and potentially preventing further vision deterioration. This mechanism suggests that early vision screening should be most beneficial for children who are at risk of developing myopia. We test this hypothesis in two ways. First, we replace the dependent variable by indicators measuring if the LogMAR is greater than a cutoff that ranges from -0.1 to 0.8. Figure 4 displays the estimates from each of the regressions. We find that early vision screening significantly reduces the proportion of children who just become myopic, which is consistent with our hypothesis. Second, we control for the vision measured in the previous round of vision screening and find that the program effect is greater among children who are close to the threshold of myopia or in areas where vision screening is coupled with follow-up interventions (Table 5). In this analysis, in order to control for the vision in the preceding round, we need to limit our sample to children who have completed at least two rounds of vision screening. In early screening counties, older cohorts underwent 3 rounds of vision screening and younger cohorts underwent 2 rounds, whereas in late screening counties, both cohorts underwent 2 rounds of screening. The effect of early vision screening can be identified using similar cohort DID approach as in the main analysis.

4.3 Dynamic effects from sustained screenings

A fundamental mechanism by which health screenings lead to improved health outcomes is that they facilitate timely treatment of health conditions, which in turn depends on how individuals adhere to the advice provided by doctors. Compliance with doctor prescriptions can be imperfect due to factors such as behavioral biases. Indeed, in our context, children in areas where vision screening is combined with providing eyeglasses vouchers to those lacking appropriate eyeglasses, unfortunately a large share of children who received eyeglasses vouchers did not redeem them. Figure 5 plots the average voucher take-up rates in our sample of children who received vouchers. One year after the first round of vision screening, the take-up rate remains significantly below 40%. An important conjecture is that can sustained vision screenings encourage the compliance with treatment, thereby leading to improved health outcomes? We explicitly test for this conjecture.

In areas where vision screening was paired with the distribution of vouchers, myopic children lacking appropriate eyeglasses were provided with vouchers that could be redeemed for free eyeglasses at the respective county's vision care center. We link the vision screening data with a data file that records the date when vouchers were used. Define binary variable Y_{it} with 1 denoting that student i has used the voucher by month t, and 0 otherwise. $t = 0$ denotes the month of the first round of vision screening. We use a staggered DID model to quantify the impact of repeated vision screening on voucher redemption rates:

$$Y_{it} = I(t \geq \text{SecondRound}_i)\tau + \text{FE}(i) + \text{FE}(t) + \epsilon_{it}, \tag{1}$$

where SecondRound_i is the time of i's second round of vision screening, which varies between locations. The estimation results are reported in Table 7. Among children who receive vouchers, the second round of vision screening increases voucher redemption rates by 25.4%, which is economically significant given that the average voucher redemption rate is below 40% prior to the second round of vision screening (Figure 5). The effect remains statistically significant at 8.8% when including controls and replacing date fixed effects with school by date fixed effects.[3] When we further link data with voucher redemption after the third round of vision screening, we find that the third round of vision screening has little impact on further increasing the voucher redemption rate. Using an event study specification of (1), Figure 6 shows that the voucher redemption rate increases substantially after one more round of vision

[3] We examine whether the effect varies based on baseline characteristics and find little effect heterogeneity (Appendix Table A5).

screening. The redemption rate increases by about 10% in the month immediately following the second round of vision screening and reaches approximately 20% five months later. The coefficients for time periods before the event month are close to zero and not statistically significant, supporting the assumption of parallel trends between locations that conducted second round of vision screening at different time. It is also likely that similar results also apply to other health conditions, that regular health screenings foster better adherence to doctors' recommendations and consequently result in improved health outcomes.

Compared to the one-time RCT study, data from sustained vision screenings also enables us to assess whether early vision screening has longer-term effects. We follow the setup in the baseline cohort DID specification. During the screenings conducted in 2018 (2019), the early screening cohort underwent two (three) rounds of vision screening, whereas others only experienced one (two) round. Exploiting this variation, we can quantify the dynamic effects of early vision screening by separately analyzing the data from the screenings conducted in 2018 and 2019. The estimation results are reported in Table 8. Two main findings emerge. First, children continue to benefit from early vision screening two years after their first round, particularly when it is coupled with follow-up actions such as the provision of eyeglass vouchers to those lacking appropriate eyeglasses. From Columns 3 and 6 of Table 8, the positive effect on vision for children in areas with follow-up actions and for the incidence of myopia are statistically significant and stronger in 2019 than in 2018. Second, in areas where only vision screenings are conducted, the beneficial effects of early vision screening appear to be temporary and fade within two years. From Column 2 of Table 8, the effect from early screening on LogMAR in those areas is statistically significant in 2018, and it becomes statistically insignificant in 2019. The results are consistent with the literature that providing health information alone can have limited long-term health effects (Glewwe *et al.*, 2018).

To conclude this subsection, we find evidence supporting the combination of health screening with follow-up treatment for larger and more long-lasting health benefits, and that sustained health screening can enhance compliance with prescribed treatment.

5 Conclusion

Addressing the rising concern of health inequality in rural contexts in the developing world requires substantial expenditure on care policy. In order to assess the efficiency of healthcare resource allocation, it is important to evaluate the implications of public investments on the evolution of health inequalities over time. However, rigorous analysis has been poorly quantified in rural contexts in less developed regions due to the lack of the RCT experimental setting and the absence of longitudinal cohort-based data to account for the external validity of RCTs. This paper addresses these limitations to assess the distributional effects of children's care policies.

By conducting a sustained children's care-based vision screening program in northwestern rural China in two empirical settings, we have established not just a pilot RCT experiment but also the healthcare infrastructure for assembling the longitudinal cohort data. We find a statistically significant beneficial effect of screenings on vision, academic performance and behavioral outcomes. We provide machine learning-based verification for statistical inferences about the heterogeneity in observed effects by children and home environment characteristics, suggesting the importance of social gradients in health under rural contexts. Our estimate is robust to modifications of model specifications. Our findings indicate that the early exposure to vision screening leads to substantial improvements in vision. More importantly, publicly provided vision screening programs mitigate vision health disparities through heterogeneous treatment effects and fostering substitution between public and private health related input. Our cohort-based difference-in-differences estimation framework not only confirms the external validity of the RCT findings but also reveals that sustained screenings enhance adherence to health recommendations. These findings provide a sound basis for policymakers who must plan integrated social care and complementary healthcare infrastructure in rural contexts from developing countries.

Our findings have important policy implications. First, our results provide insights into the distributional impacts of vision screening and how it can contribute to shaping the evolution of health inequalities. Findings of this study are important because they help to enrich the literature in the evaluation of care policy allocations (Altindağ *et al.*, 2022, Bharadwaj *et al.*, 2013, Currie and Zhang, 2023, Glewwe *et al.*, 2018), especially in the case of children healthcare screenings in rural contexts from the developing world. Vision screening facilitates secondary prevention, which refers to measures taken to slow or prevent the progression of a health condition. Indeed, although it is unlikely that myopia can be reversed, our findings show that its early detection through regular vision screening is valuable because screenings can facilitate timely treatment and positive behavioral changes. Second, we provide empirical evidence to policymakers on how to effectively scale up small-scale RCTs into sustainable and routine healthcare programs. Third, our results show that the benefits of health screenings for children are not limited to health outcomes alone. In our study, timely correction of myopia improves academic performance and affect behavioral outcomes. A full cost-benefit analysis of healthcare interventions for children should consider these indirect effects to ensure accurate evaluation of their overall impact. Finally, our analysis of the added benefits resulting from follow-up interventions, particularly the provision of vouchers for eyeglasses, shows that care-oriented screening interventions could be accompanied by nudge-type interventions such as offering vouchers for discounted medical treatment and information dissemination which could further improve health outcomes (Sunstein, 2022). This is particularly urgent considering the heavy global vision loss burden (Matsumura *et al.*, 2019). Recent global evidence shows that individuals devote massive healthcare resources to protecting eye health (Mathers *et al.*, 2010). Findings from our study underscore the necessity of implementing routine health screenings to enhance the health and well-being of school-aged children in rural communities.

This study has several limitations due to data constraints. First, we recognize that behavioral changes of teachers are not observed. Second, although findings from our cohort-based DID framework are consistent with those from the RCT, it should still be carefully evaluated when generalizing the results to other settings in different countries. It is important to take further research to develop a full understanding of the RCT external validity. Despite of potential limitations, we make extensive efforts in addressing data and identification challenges. Our study provides the impetus to understand the scientific relevance of care policies in rural contexts in developing countries and to implement integrated social-healthcare screening programs to address rising inequality concerns in the Global South.

References

Abu-Qarn, A. and Lichtman-Sadot, S. (2022) Can greater access to secondary health care decrease health inequality? Evidence from bus line introduction to Arab towns in Israel, *Economic Modelling*, **106**, 105695.

Altindağ, O., Greve, J. and Tekin, E. (2022) Public health policy at scale: impact of a government-sponsored information campaign on infant mortality in Denmark, *Review of Economics and Statistics*, pp. 1–36.

Amaral, S., Dinarte-Diaz, L., Dominguez, P. and Perez-Vincent, S. M. (2024) Helping families help themselves: The (Un) intended impacts of a digital parenting program, *Journal of Development Economics*, **166**, 103181.

Bharadwaj, P., Løken, K. V. and Neilson, C. (2013) Early life health interventions and academic achievement, *American Economic Review*, **103**, 1862–1891.

Bourne, R. R. A., Flaxman, S. R., Braithwaite, T., Cicinelli, M. V., Das, A., Jonas, J. B., Keeffe, J., Kempen, J. H., Leasher, J., Limburg, H., Naidoo, K., Pesudovs, K., Resnikoff, S., Silvester, A., Stevens, G. A., Tahhan, N., Wong, T. Y. and Taylor, H. R. (2017) Magnitude, temporal trends, and projections of the global prevalence of blindness and distance and near vision impairment: a systematic review and meta-analysis, *The Lancet Global Health*, **5**, e888–e897.

Braghieri, L., Levy, R. and Makarin, A. (2022) Social Media and Mental Health, *American Economic Review*, **112**, 3660–3693.

Callaway, B. and Sant'Anna, P. H. (2021) Difference-in-differences with multiple time periods, *Journal of econometrics*, **225**, 200–230.

Camparini, M., Cassinari, P., Ferrigno, L. and Macaluso, C. (2001) ETDRS-fast: Implementing psychophysical adaptive methods to standardized visual acuity measurement with ETDRS charts, *Investigative Ophthalmology and Visual Science*, **42**, 1226–1231.

Chen, S., Liu, W. and Song, H. (2020) Broadband internet, firm performance, and worker welfare: Evidence and mechanism, *Economic Inquiry*, **58**, 1146–1166.

Chernozhukov, V., Demirer, M., Duflo, E. and Fernández-Val, I. (2017) Generic Machine Learning Inference on Heterogenous Treatment Effects in Randomized Experiments, *arXiv*.

Currie, J. and Zhang, J. (2023) Doing more with less: Predicting primary care provider effectiveness, *Review of Economics and Statistics*, pp. 1–45.

Engle, P. L., Fernald, L. C., Alderman, H., Behrman, J., O'Gara, C., Yousafzai, A., De Mello, M. C., Hidrobo, M., Ulkuer, N., Ertem, I. *et al.* (2011) Strategies for reducing inequalities and improving developmental outcomes for young children in low-income and middle-income countries, *The Lancet*, **378**, 1339–1353.

Falk, A., Kosse, F., Pinger, P., Schildberg-Hörisch, H. and Deckers, T. (2021) Socioeconomic status and inequalities in children's IQ and economic preferences, *Journal of Political Economy*, **129**, 2504–2545.

Glewwe, P., Park, A. and Zhao, M. (2016) A better vision for development: Eyeglasses and academic performance in rural primary schools in China, *Journal of Development Economics*, **122**, 170–182.

Glewwe, P., West, K. L. and Lee, J. (2018) The Impact of Providing Vision Screening and Free Eyeglasses on Academic Outcomes: Evidence from a Randomized Trial in Title I Elementary Schools in Florida, *Journal of Policy Analysis and Management*, **37**, 265–300.

Guan, H., Zhang, Y., Du, K., Wang, Z. and Shi, Y. (2024) Revisit the one-time subsidies and long-run adoption of health products: Quasi-experimental evidence from providing free eyeglasses in rural china, *World Development*, **179**, 106627.

Gwatkin, D. R. (2017) Trends in health inequalities in developing countries, *The Lancet Global Health*, **5**, e371–e372.

Halla, M., Pruckner, G. J. and Schober, T. (2016) Cost savings of developmental screenings: Evidence from a nationwide program, *Journal of Health Economics*, **49**, 120–135.

Hong, K., Dragan, K. and Glied, S. (2019) Seeing and hearing: The impacts of New York City's universal pre-kindergarten program on the health of low-income children, *Journal of Health Economics*, **64**, 93–107.

Huang, J., Dang, H., Cai, Y., Liu, J. and Chen, Q. (2022) Myopia and Depression among Middle School Students in China—Is There a Mediating Role for Wearing Eyeglasses?, *International Journal of Environmental Research and Public Health*, **19**, 13031.

Krämer, M., Kumar, S. and Vollmer, S. (2021) Improving child health and cognition: Evidence from a school-based nutrition intervention in India, *Review of Economics and Statistics*, **103**, 818–834.

Ma, X., Zhou, Z., Yi, H., Pang, X., Shi, Y., Chen, Q., Meltzer, M. E., Cessie, S. L., He, M., Rozelle, S., Liu, Y. and Congdon, N. (2014) Effect of providing free glasses on children's educational outcomes in China: Cluster randomized controlled trial, *BMJ*, **349**, 1–12.

Mahler, L. and Yum, M. (2024) Aggregate and distributional effects of school closure mitigation policies: Public versus private education, *Economics Letters*, **235**, 111517.

Mallah, N., Orsini, N., Figueiras, A. and Takkouche, B. (2022) Income level and antibiotic misuse: a systematic review and dose–response meta-analysis, *The European Journal of Health Economics*, pp. 1–21.

Martin, M. O. and Kelly, D. L. (1996) *Third International Mathematics and Science Study. Technical Report. Volume I: Design and Development.*, ERIC.

Mathers, M., Keyes, M. and Wright, M. (2010) A review of the evidence on the effectiveness of children's vision screening, *Child: Care, Health and Development*, **36**, 756–780.

Matsumura, S., Ching-Yu, C. and Saw, S.-M. (2019) Global Epidemiology of Myopia, in *Updates on Myopia: A Clinical Perspective*, Springer, pp. 27–51.

Okeke, E. N. and Abubakar, I. S. (2020) Healthcare at the beginning of life and child survival: evidence from a cash transfer experiment in Nigeria, *Journal of Development economics*, **143**, 102426.

Olden, A. and Møen, J. (2022) The triple difference estimator, *The Econometrics Journal*, **25**, 531–553.

Pruckner, G. J., Halla, M., Pruckner, G. J. and Schober, T. (2021) Paying Adolescents for Health Screenings Works, *Working paper*, pp. 1–31.

Reynolds, C. R. and Richmond, B. O. (1997) What I think and feel: A revised measure of children's manifest anxiety, *Journal of abnormal child psychology*, **25**, 15–20.

Scheil-Adlung, X. (2015) Global evidence on inequities in rural health protection: new data on rural deficits in health coverage for 174 countries, Tech. rep., International Labour Organization.

Sunstein, C. R. (2022) The distributional effects of nudges, *Nature Human Behaviour*, **6**, 9–10.

Sylvia, S., Ma, X., Shi, Y. and Rozelle, S. (2022) Ordeal mechanisms, information, and the cost-effectiveness of strategies to provide subsidized eyeglasses, *Journal of Health Economics*, **82**, 102594.

WHO (2019) World report on vision, Tech. rep., World Health Organization.

Williams, C., Northstone, K., Harrad, R. A., Sparrow, J. M. and Harvey, I. (2002) Amblyopia treatment outcomes after screening before or at age 3 years: Follow up from randomised trial, *British Medical Journal*, **324**, 1549–1551.

Wong, H. B., Machin, D., Tan, S. B., Wong, T. Y. and Saw, S. M. (2009) Visual Impairment and Its Impact on Health-related Quality of Life in Adolescents, *American Journal of Ophthalmology*, **147**, 505–511.e1.

Yang, M., Zheng, S. and Zhou, L. (2022) Broadband internet and enterprise innovation, *China Economic Review*, **74**, 101802.

Yi, H., Zhang, L., Ma, X., Congdon, N., Shi, Y., Pang, X., Zeng, J., Wang, L., Boswell, M. and Rozelle, S. (2015) Poor vision among China's rural primary school students: Prevalence, correlates and consequences, *China Economic Review*, **33**, 247–262.

Zhang, L., Kleiman-Weiner, M., Luo, R., Shi, Y., Martorell, R., Medina, A. and Rozelle, S. (2013) Multiple micronutrient supplementation reduces anemia and anxiety in rural China's elementary school children, *The Journal of Nutrition*, **143**, 640–647.

Zhou, B. (1991) Mental health test (MHT): East China Normal University Press, *Shanghai, China.*

Figures

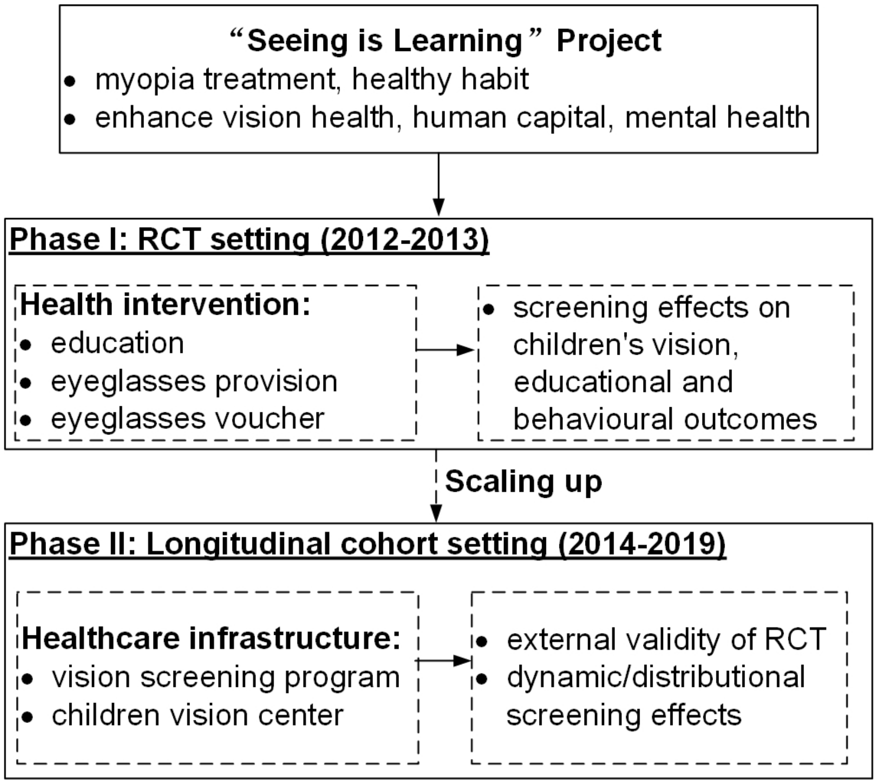


Figure 1: Workflow of the "Seeing is Learning" project

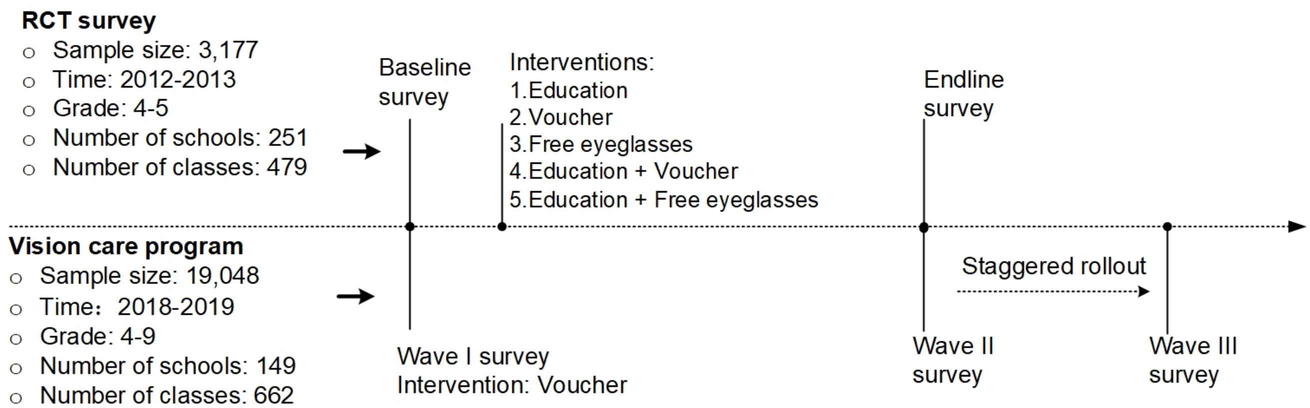


Figure 2: Survey timeline

Notes: This figure shows the timeline for the RCT surveys and the vision care program. The RCT survey is implemented in Tianshui and Yulin prefecture, as well as in Shaanxi province. The vision care program is carried out in three counties in Shaanxi province in China, namely Ningshan, Pingli, and Huachi.

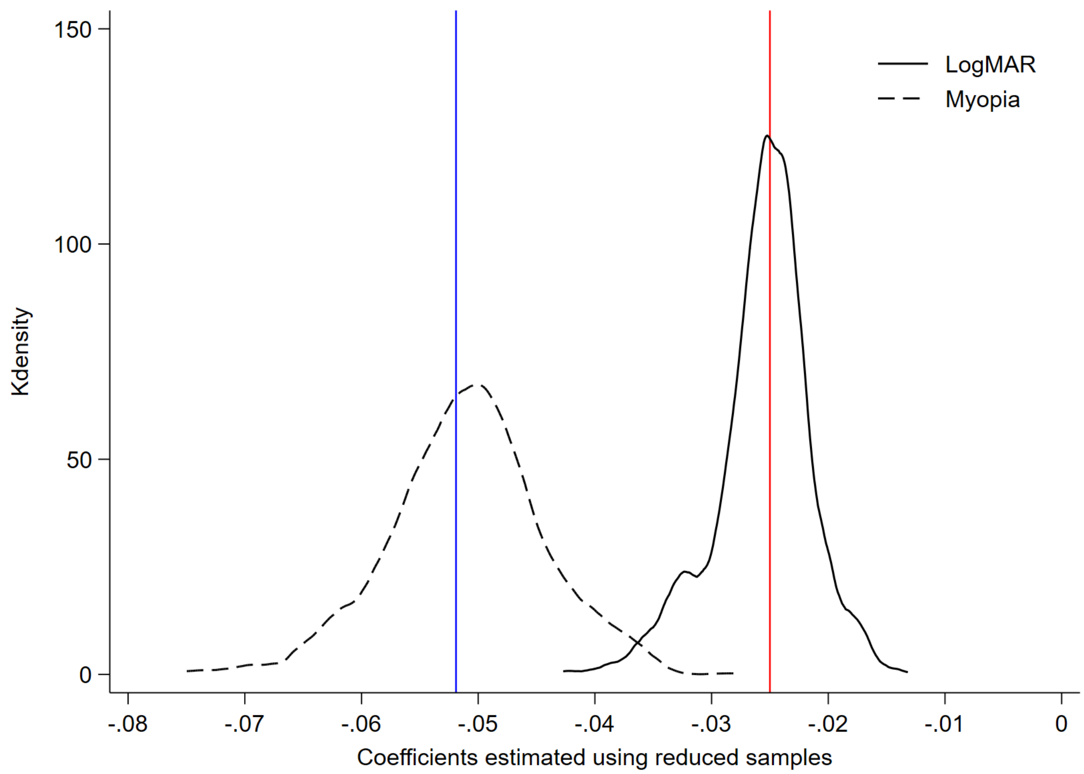


Figure 3: The distributions of coefficients estimated using reduced samples

Notes: This figure plots the distribution of the coefficients from 1,000 baseline regression replications. In each iteration, 10 schools from the baseline sample are randomly dropped. The vertical blue and red lines indicate the estimated coefficients for LogMAR and myopia from the baseline sample, respectively.

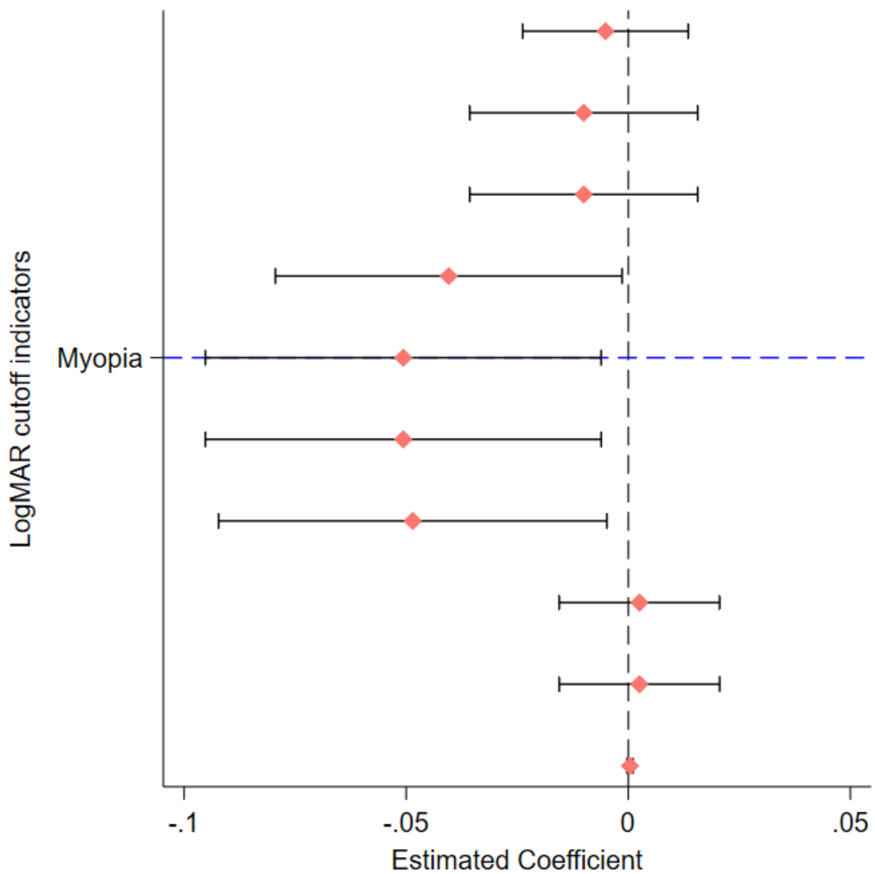


Figure 4: The distributional effects of the early screening

Notes: This figure shows the distributional effects of the vision screening. The dependent variable consists of ten indicators representing different cutoffs of LogMAR. Each indicator takes a value of 1 if the LogMAR is larger than the cutoff value. The cutoff values in the vertical line increase from bottom to top. The independent variable is the early screening cohorts. Red diamonds mark the estimated coefficients and short lines show 95% confidence intervals. The vertical blue dashed line indicates the cutoff value for myopia. Standard errors are clustered at the class level.

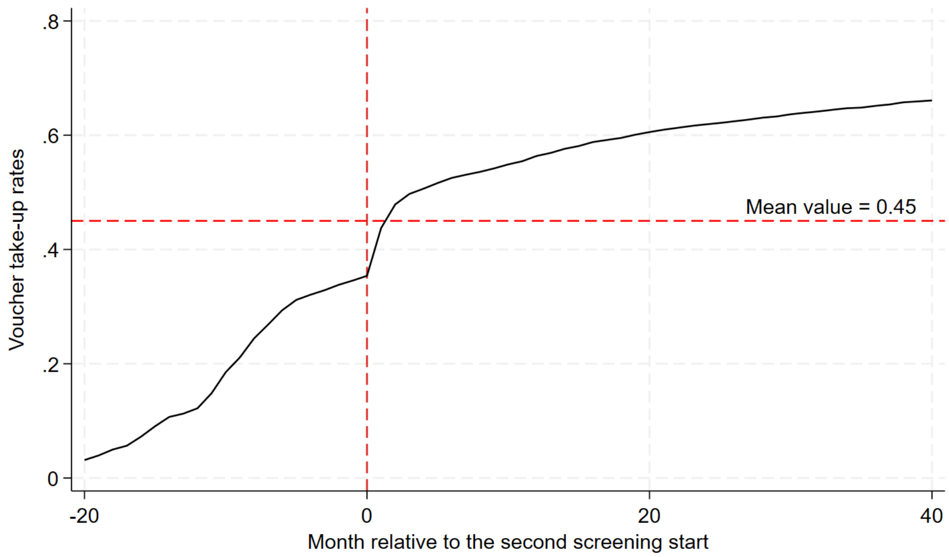


Figure 5: The trend on the voucher take-up rates

Notes: This figure shows the time trend in voucher take-up rates relative to the start month of the second round of vision screening indicated by the vertical red dashed line. The horizontal red dashed line represents the average voucher take-up rate across event months.

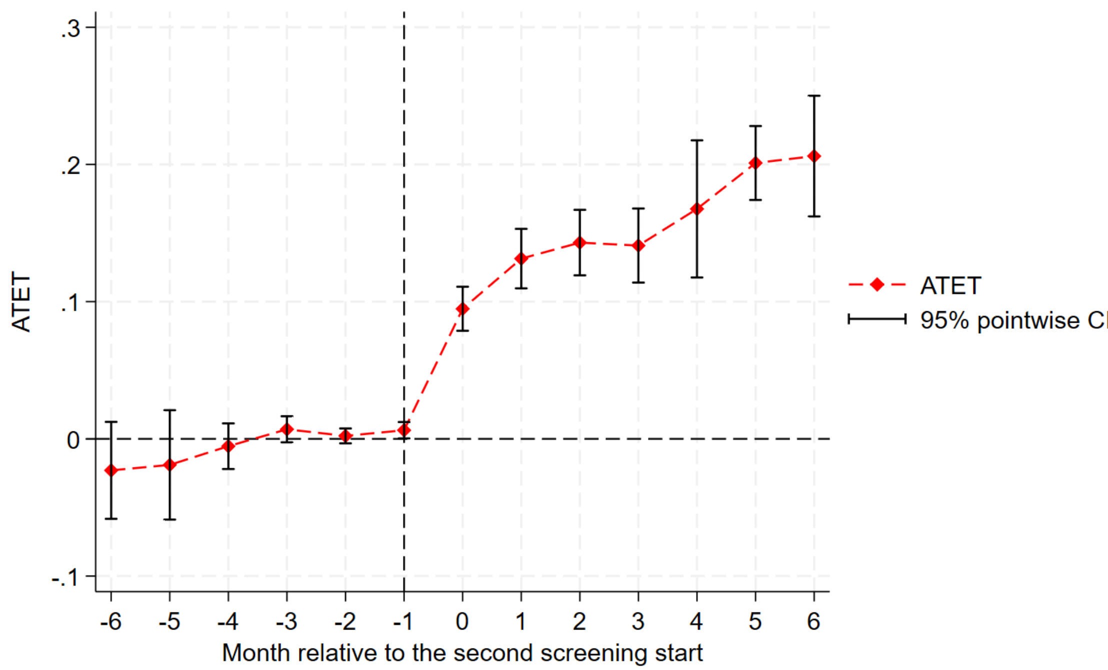


Figure 6: The dynamic screening effects on voucher take-up

Notes: This figure shows the dynamic effects of vision screening on the individual-level probability of voucher take-up, using the augmented inverse-probability weighting (AIPW) estimator proposed by Callaway and Sant'Anna (2021). The dependent variable is a binary indicator of whether a student used the free eyeglasses voucher from the first round of vision screening. The sample is restricted to students who received the free eyeglasses vouchers. The treatment is the second round of vision screening. Individual and survey month-fixed effects are controlled. Standard errors are clustered at the class level.

Tables

Table 1: Summary statistics

Variable	Description	Vision screening data (Sample size: 19,084)		RCT data (Sample size: 3,177)	
		Mean	SD	Mean	SD
LogMAR	Vision measurement, MAR = 1/(Vision), LogMAR = log10(MAR)	0.238	0.261	0.754	0.208
Myopia (0/1)	The dummy indicator measured by the LogMAR of the worse eye being greater than 0.3	0.396	0.489	0.990	0.101
Math score	Standardized math score	-	-	0.344	0.983
Mental health score	Standardized mental health score	-	-	-0.209	1.211
Gender	Male is assigned a value of 1 and otherwise 0	0.528	0.499	0.490	0.500
Boarding at school	Students boarding at school are assigned a value of 1 and otherwise 0	0.364	0.481	0.222	0.416
Paternal education	Paternal educational attainment assigned as a scale ranging from 1 to 6, corresponding to the following levels of education: no formal education, primary school, secondary school, junior high school or vocational school, junior college degree, college degree and above	2.813	1.000	2.751	0.985
Maternal education	Maternal educational attainment assigned as a scale ranging from 1 to 6, corresponding to the following levels of education: no formal education, primary school, secondary school, junior high school or vocational school, junior college degree, college degree and above	2.624	1.039	2.356	1.041
Paternal out-migration	Students whose father has migrated out assigned a value of 1 and otherwise 0	0.493	0.500	0.438	0.496
Maternal out-migration	Students whose mother has migrated out assigned a value of 1 and otherwise 0	0.270	0.444	0.140	0.347

Table 2: Screening effects: Health, educational and behavioral outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Health and educational outcomes			Behavioral outcomes			
Dependent variable	LogMAR	Math score	Mental health	Habit	Attitude	Knowledge	Parenting
Treatment: Screening	-0.034***	0.089**	-0.005	-0.046	-0.401***	-0.138***	0.544***
	(0.013)	(0.045)	(0.052)	(0.029)	(0.052)	(0.024)	(0.094)
Observations	2,902	2,945	2,789	2,793	2,942	2,947	2,946
Adjusted R-squared	0.399	0.363	0.364	0.268	0.074	0.068	0.067
Treatment 1: Education	-0.006	0.033	-0.037	-0.027	-0.591***	-0.200***	-0.297**
	(0.015)	(0.057)	(0.070)	(0.034)	(0.067)	(0.031)	(0.116)
Observations	969	975	921	925	975	976	976
Adjusted R-squared	0.426	0.422	0.386	0.322	0.125	0.121	0.070
Treatment 2: Voucher	-0.047***	0.074	-0.121*	-0.091***	-0.029	-0.007	0.770***
	(0.015)	(0.056)	(0.069)	(0.034)	(0.060)	(0.029)	(0.103)
Observations	914	927	883	879	928	929	929
Adjusted R-squared	0.439	0.409	0.372	0.294	0.089	0.027	0.142
Treatment 3: Voucher and Education	-0.038**	0.030	0.049	-0.023	-0.598***	-0.220***	0.836***
	(0.015)	(0.053)	(0.069)	(0.038)	(0.069)	(0.028)	(0.105)
Observations	927	934	880	889	933	935	934
Adjusted R-squared	0.405	0.428	0.348	0.246	0.158	0.126	0.163
Treatment 4: Free eyeglasses	-0.045***	0.103	-0.023	-0.053	-0.051	0.001	0.760***
	(0.014)	(0.065)	(0.060)	(0.035)	(0.060)	(0.030)	(0.111)
Observations	946	956	906	907	954	957	957
Adjusted R-squared	0.414	0.403	0.395	0.255	0.083	0.081	0.141
Treatment 5: Free eyeglasses and Education	-0.036***	0.126**	0.029	-0.051*	-0.363***	-0.128***	0.735***
	(0.014)	(0.054)	(0.051)	(0.030)	(0.059)	(0.028)	(0.097)
Observations	1,499	1,531	1,449	1,459	1,528	1,532	1,532
Adjusted R-squared	0.404	0.368	0.406	0.259	0.089	0.095	0.115
Controls	YES	YES	YES	YES	YES	YES	YES
County Fixed Effects	YES	YES	YES	YES	YES	YES	YES

Notes: This table presents the analysis results using the randomized controlled trial data. Each cell represents a separate regression. The focus of the table is to estimate the effects of screening intervention and its five subdivision types on students' vision and educational outcomes (columns 1-3) and eye-related behaviors (columns 4-7). The dependent variable in column 1 is students' LogMAR, while columns 2-3 represent students' standardized math score and mental health score, respectively. Eye-related behavioral outcomes encompass eye habits (column 4), attitudes towards eyeglasses-wearing behaviors (column 5), acquisition of vision knowledge (column 6), and parenting behaviors (column 7). All dependent variables are measured at wave II, with each cell controlling for the corresponding dependent variable at wave I. The five subdivision types of screening interventions include vision health education ("Education"), provision of a free eyeglasses voucher ("Voucher"), a combination of the free eyeglasses voucher provision and vision health education ("Voucher and Education"), provision of free eyeglasses ("Free eyeglasses"), and a combination of free eyeglasses provision and vision health education ("Free eyeglasses and Education"). All model specifications include individual characteristics and county-fixed effects. The individual controls include gender, ethnicity, Hukou (household registration), family size, number of older and younger siblings, boarding status, parental education level, parental migration status, and parental engagement in farmwork. Standard errors are clustered at class level and appear in parentheses.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3: CLAN of screening interventions

Machine Learners	Random Forest			Elastic network			Support vector machine		
Covariates	20% Most	20% Least	Difference	20% Most	20% Least	Difference	20% Most	20% Least	Difference
LogMAR at Wave I	0.700	0.533	0.166	0.659	0.553	0.105	0.699	0.534	0.161
	(0.676,0.727)	(0.510,0.558)	(0.130,0.204)	(0.632,0.687)	(0.530,0.576)	(0.070,0.140)	(0.672,0.727)	(0.512,0.555)	(0.128,0.195)
	/	/	[0.000]	/	/	[0.000]	/	/	[0.000]
Boarding	0.294	0.136	0.158	0.289	0.148	0.134	0.285	0.150	0.130
	(0.240,0.344)	(0.095,0.174)	(0.091,0.225)	(0.237,0.341)	(0.107,0.190)	(0.068,0.200)	(0.233,0.337)	(0.107,0.189)	(0.063,0.197)
	/	/	[0.000]	/	/	[0.000]	/	/	[0.000]
Repeater (Yes = 1)	0.454	0.288	0.154	0.522	0.143	0.395	0.472	0.264	0.202
	(0.396,0.511)	(0.234,0.340)	(0.075,0.232)	(0.465,0.580)	(0.101,0.181)	(0.324,0.465)	(0.041,0.528)	(0.211,0.313)	(0.125,0.279)
	/	/	[0.000]	/	/	[0.000]	/	/	[0.000]
Father migration	0.466	0.428	0.030	0.497	0.402	0.095	0.466	0.447	0.029
	(0.407,0.521)	(0.371,0.485)	(-0.051,0.110)	(0.437,0.552)	(0.344,0.456)	(0.014,0.176)	(0.403,0.518)	(0.388,0.502)	(-0.052,0.110)
	/	/	[0.000]	/	/	[0.018]	/	/	[0.473]
Mother migration	0.194	0.074	0.113	0.223	0.081	0.147	0.237	0.055	0.182
	(0.147,0.238)	(0.043,0.102	(0.058,0.168)	(0.172,0.267)	(0.048,0.110)	(0.086,0.205)	(0.188,0.286)	(0.029,0.081)	(0.126,0.236)
	/	/	[0.000]	/	/	[0.000]	/	/	[0.000]

Notes: This table presents the results of the Classification Analysis (CLAN) conducted using the different machine learners encompassing Random Forest, Elastic Network, and Support Vector Machine. The data used in this table is the randomized controlled trial data. The median confidence interval (90%) is provided in parentheses. The dependent variable is students' LogMAR at Wave II. The p-value, testing the null hypothesis that the parameter equals zero against a two-sided alternative, is enclosed in brackets. The terms “20% Most” and "20% Least" are the average characteristics of the most and least affected groups, respectively. The term "Difference" denotes the difference in the average characteristics between the most affected and least affected groups.

Table 4: Screening effects: cohort DID results

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	LogMAR			Myopia		
Sample	Full	Full	Follow-up	Full	Full	Follow-up
Early screening cohort	-0.027**	-0.025**	-0.032**	-0.052**	-0.051**	-0.066**
	(0.011)	(0.011)	(0.013)	(0.023)	(0.023)	(0.027)
Gender		-0.048***	-0.045***		-0.079***	-0.070***
		(0.004)	(0.005)		(0.008)	(0.009)
Boarding at school		-0.024***	-0.025***		-0.035***	-0.037***
		(0.005)	(0.006)		(0.009)	(0.010)
Paternal education		0.002	0.003		0.003	0.003
		(0.002)	(0.003)		(0.004)	(0.006)
Maternal education		0.006***	0.005*		0.014***	0.012**
		(0.002)	(0.003)		(0.004)	(0.005)
Paternal out-migration		0.007*	0.011**		0.011	0.015*
		(0.004)	(0.005)		(0.007)	(0.009)
Maternal out-migration		-0.008**	-0.005		-0.008	0.000
		(0.004)	(0.005)		(0.008)	(0.009)
School Fixed Effects	YES	YES	YES	YES	YES	YES
Cohort Fixed Effects	YES	YES	YES	YES	YES	YES
Survey Wave Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	19,084	19,084	12,500	19,084	19,084	12,500
Adjusted R-squared	0.105	0.116	0.138	0.085	0.093	0.111

Notes: This table shows estimation results on the screening effect on students' vision, measured by LogMAR (columns 1-3) and the incidence of myopia (columns 4-6). The independent variable, denoted as "Early screening cohort", is a binary indicator equal to one if a student has participated in the vision screening program more than once (2018 survey wave) or twice (2019 survey wave), and zero otherwise. Columns 3 and 6 restrict the analysis to students with follow-up interventions, while the remaining columns consider the full sample. Each model specification includes fixed effects for school, cohort, and survey wave. Columns 2-3 and columns 5-6 further control for individual characteristics, such as gender, boarding status, parental education level, and parental migration status. Standard errors are clustered at class level and appear in parentheses.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 5: The distributional screening effects: Two-wave panel regression

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	LogMAR at the current wave					
Sample	Myopic		Near-myopic		Non-myopic	
	Full	Follow-up	Full	Follow-up	Full	Follow-up
Early screening cohort	-0.077*	-0.147***	-0.007	-0.077**	0.008	0.002
	(0.040)	(0.053)	(0.025)	(0.033)	(0.008)	(0.011)
LogMAR at the last wave	0.628***	0.649***	0.935***	0.992***	0.832***	0.876***
	(0.041)	(0.043)	(0.038)	(0.050)	(0.029)	(0.035)
School Fixed Effects	YES	YES	YES	YES	YES	YES
Cohort Fixed Effects	YES	YES	YES	YES	YES	YES
Survey Wave Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	2,035	1,271	2,911	1,805	6,375	4,341
Adjusted R-squared	0.333	0.305	0.269	0.288	0.204	0.221

Notes: This table shows estimation results on the distributional screening effect on students' vision, measured by LogMAR at the current Wave. The data comprises two waves of survey information from the 2019 academic year. The independent variable, denoted as "Early screening cohort", is a binary indicator equal to one if a student has participated in the vision screening program more than twice, and zero otherwise. The sample is decomposed into three subsamples based on the LogMAR at the last wave: 1) the myopic sample for students with LogMAR greater than 0.4, 2) the non-myopic sample for students with LogMAR less than 0.1; and 3) the near-myopic sample for other students. Samples in odd columns represent the full sample, while samples in even columns narrow the analysis to students with follow-up interventions. Each model specification includes fixed effects for school, cohort, and survey wave, and control for LogMAR at the last wave, individual characteristics, such as gender, boarding status, parental education level, and parental migration status. Standard errors are clustered at class level and appear in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: The distributional screening effects: intra-class inequality

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	LogMAR: Mean		LogMAR: S.D.		LogMAR: P90-P10	LogMAR: P75-P25
Panel A: Full sample						
Early screening cohort	-0.023	-0.026*	-0.022*	-0.024**	-0.091***	-0.086***
	(0.015)	(0.014)	(0.013)	(0.012)	(0.029)	(0.024)
Observations	655	655	655	655	655	655
Adjusted R-squared	0.576	0.596	0.545	0.559	0.625	0.546
Panel B: Follow-up sample						
Early screening cohort	-0.019	-0.024	-0.025	-0.027*	-0.114***	-0.077***
	(0.020)	(0.018)	(0.017)	(0.016)	(0.039)	(0.029)
Observations	489	489	489	489	489	489
Adjusted R-squared	0.608	0.636	0.501	0.517	0.622	0.544
Class-level mean controls	NO	YES	NO	YES	YES	YES
School Fixed Effects	YES	YES	YES	YES	YES	YES
Cohort Fixed Effects	YES	YES	YES	YES	YES	YES
Survey Wave Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: This table shows estimation results on the vision screening effects on students' vision inequality within classes. Panel A uses the full sample, and Panel B restricts the analysis to samples with follow-up interventions. The measurements of students' vision inequality within classes include the class-level mean value of LogMAR (columns 1-2), the class-level standard deviation of LogMAR (columns 3-4), the difference between LogMAR at the 90th percentile and LogMAR at the 10th percentile within each class (column 5), and the difference between LogMAR at the 75th percentile and LogMAR at the 25th percentile within each class (column 6). The independent variable, denoted as "Early screening cohort", is a binary indicator equal to one if a student has participated in the vision screening program more than once (2018 survey wave) or twice (2019 survey wave), and zero otherwise. All model specifications include fixed effects for county, cohort, and survey wave. Columns 2 and 4 as well as columns 5-6 additionally control for individual characteristics such as gender, boarding status, parental education level, and parental migration status by considering their respective class-level mean values. Standard errors are clustered at class level and appear in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Dynamic screening effects: voucher take-up

	(1)	(2)	(3)	(4)
Dependent variable	Voucher take-up(0/1)			
2nd Screening	0.254***	0.088***	0.088***	0.082***
	(0.012)	(0.022)	(0.021)	(0.022)
3rd Screening				-0.025
				(0.022)
Controls	NO	NO	YES	YES
Individual Fixed Effects	YES	YES	YES	YES
Month Fixed Effects	YES	NO	NO	NO
School by Month Fixed Effects	NO	YES	YES	YES
Observations	201,298	201,298	201,298	201,298
Number of students	3,307	3,307	3,307	3,307
Adjusted R-squared	0.658	0.716	0.716	0.716

Notes: This table shows estimation results on the vision screening effects on the probability of voucher take-up, using individual-by-survey-month panel data and the two-way fixed effects model. The primary variation comes from the timing of the second or third survey wave across individuals. The sample is restricted to those who ultimately used the free eyeglasses vouchers. The dependent variable is a binary indicator of whether a student used the free eyeglasses voucher from the first round of vision screening. The variables "2nd Screening" and "3rd Screening" are dummy variables indicating if the survey month exceeds the timing of the second or third round of vision screening, respectively. The independent variable for columns (1)-(3) is "2nd Screening", and both the "2nd Screening" and "3rd Screening" are used for column (4). Columns (1) include individual and survey month fixed effects. Columns (2) replace survey month fixed effects with school-by-survey month fixed effects. Columns (3)-(4) further control for interaction terms between the time trend and individual or household factors, such as gender, boarding status, parental education level, and parental migration status. Standard errors are clustered at the class level and are reported in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Dynamic screening effects

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	LogMAR			Myopia		
Sample	Full	Without follow-up	Follow-up	Full	Without follow-up	Follow-up
Panel A: one year after screening (2018 sample)						
Early screening cohort	-0.023**	-0.037**	-0.012	-0.034	-0.055	-0.018
	(0.012)	(0.018)	(0.016)	(0.022)	(0.034)	(0.029)
Observations	14,190	4,560	9,630	14,190	4,560	9,630
Adjusted R-squared	0.113	0.071	0.133	0.094	0.058	0.110
Panel B: two years after screening (2019 sample)						
Early screening cohort	-0.031	0.002	-0.063**	-0.098**	-0.037	-0.163***
	(0.024)	(0.040)	(0.030)	(0.044)	(0.068)	(0.053)
Observations	4,894	4,894	2,870	4,894	2,024	2,870
Adjusted R-squared	0.124	0.124	0.144	0.090	0.061	0.112
Baseline controls	YES	YES	YES	YES	YES	YES
School Fixed Effects	YES	YES	YES	YES	YES	YES
Cohort Fixed Effects	YES	YES	YES	YES	YES	YES
Survey Wave Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: This table shows estimation results that separately analyze data from the screenings conducted in the 2018 and 2019 academic years. The independent variable, denoted as "Early screening cohort", is a binary indicator equal to one if a student has participated in the vision screening program more than once (Panel A) or twice (Panel B), and zero otherwise. Columns 1 and 4 consider the whole sample of each panel, columns 2 and 5 restrict the analysis to students without follow-up interventions, and columns 3 and 6 restrict the analysis to students with follow-up interventions. Each model specification includes individual characteristics and fixed effects for school, cohort, and survey wave. Standard errors are clustered at class level and appear in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix

Children's care policy and inequality: Evidence from a health screening program in rural China

Hongyu Guan **Wei Shi** **Wenjie Wu** **Yanwen Yun**

Appendix A contains supplementary analysis with the following findings: vision screening has a greater impact on vision health when combined with providing easier access to treatment, such as eyeglasses, when participants comply with doctors' advice, and when information is more accessible.

A Supplementary analysis

We assess if providing easier access to myopia treatment increases the beneficial effects of vision screening. In our sample, after vision screening, students in both rural and urban schools in Ningshan County (*enhanced coverage county*) were provided eyeglasses vouchers, while in Huachi County and Pingli County, only students in rural schools were provided eyeglasses vouchers. We merge the data from Ningshan County in 2019 with the data from Huachi and Pingli Counties in 2018, and adopt a triple differences design:

$$\begin{aligned} Y_{sct} = {} & I(\text{urban}) \times I(\text{enhanced coverage county}) \times I(\text{grade} \geq 5)\gamma \\ & + X\beta + \text{FE}(\text{urban, county, grade}) + \epsilon_t, \end{aligned} \tag{2}$$

where s, c and t denotes school, county and grade, respectively. The green dotted lines of Figure A1 indicate the sample used in this analysis. The fixed effects include pairwise interactions between the dummy variables of urban, counties, and grades. Under the standard parallel trends assumption for the triple difference estimator (Olden and Møen, 2022), i.e., in the absence of providing free eyeglasses vouchers, the differences between outcomes in urban and rural areas between students that first participated in vision screening and those already received one round of screening, would be the same between the enhanced coverage county and other counties. γ in Eq.(2) identifies the additional effect of providing eyeglasses vouchers.

Using the triple difference strategy, we find that providing eyeglasses vouchers to myopic children who do not have appropriate eyeglasses enhances the beneficial effect of early vision screening (Table A1). The effect size is significant: in addition to the effect of early vision screening, providing vouchers improves vision by 0.98 line on the visual acuity chart. The results indicate that early health screening is beneficial, and the effect can be enhanced by providing easy-to-access treatment to the identified health conditions. Furthermore, the effect of providing eyeglasses vouchers depends on the extent that children cooperate. Among the children who redeemed the vouchers, those who utilized them sooner after the initial vision screening round exhibited better vision during the follow-up screening (Appendix Table A2).

Finally, we examine whether an easier access to information has an impact on the effectiveness of vision screenings. The time period of our study coincides with the rollout of broadband access in rural areas of China as part of the "Broadband China" strategy. We determine the exact months in which each of rural towns (*xiang zhen*) in our study areas access to the universal telecommunication and internet. This is a longitudinal, place-based database containing the universe of access to the telecommunication and internet services administered by the Ministry of Industry and Information Technology of the People's Republic of China. We measure broadband access in a township by calculating the proportion of villages within that township that have access to broadband services before the vision screening program was implemented, and interact this variable with the binary variable indicating the early screening cohort. Appendix Table A4 in the appendix shows that the beneficial effect of vision screening is enhanced with easier access to information through better broadband connectivity. Our findings on the beneficial effects of easier information access on health contributes to the existing literature which demonstrates that high-speed internet enhances labor productivity and firm innovation (Chen *et al.*, 2020, Yang *et al.*, 2022).

B Appendix figure

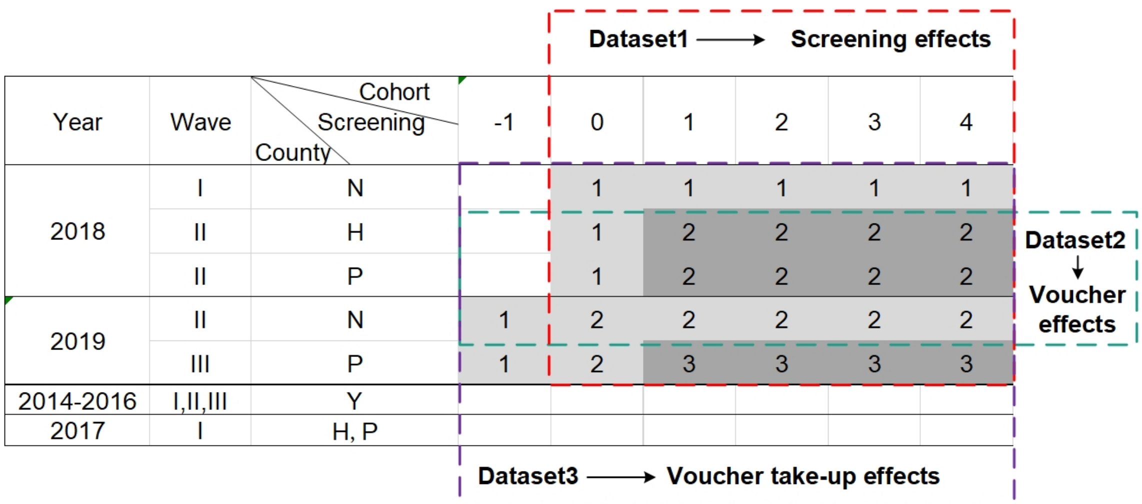
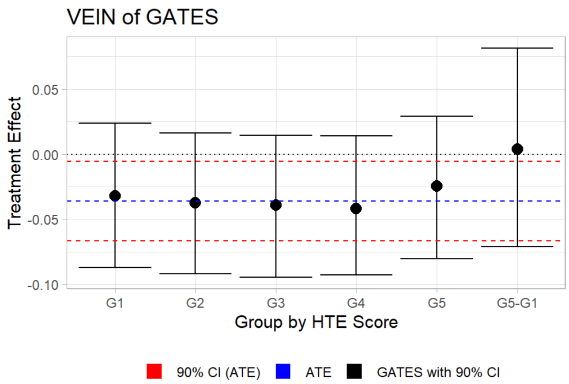
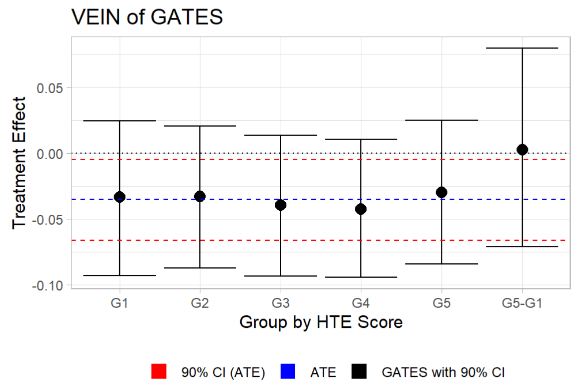


Figure A1: Data structure

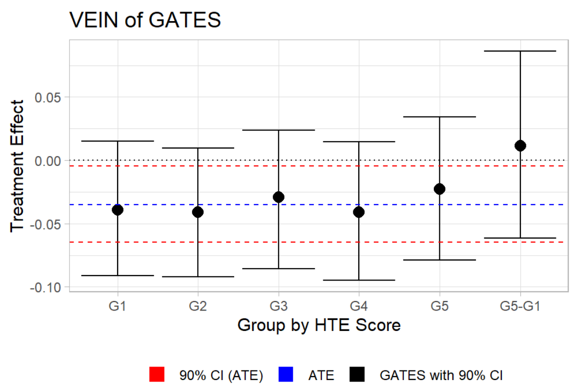
Notes: This figure shows the data structure in this study. N: Ningshan County. H: Huachi County. P: Pingli County. Y: Yongshou County.



(a) Random Forest



(b) Elastic Network



(c) Support Vector Machine

Figure A2: The Sorted Group Average Treatment Effects (GATES)

Notes: This figure plots the Sorted Group Average Treatment Effects (GATES) by using different machine learners including Random Forest (a), Elastic Network (b), and Support Vector Machine (c). The data used in this figure is the randomized controlled trial data. Black circles represent the estimated coefficients, while the short lines depict the 90% confidence intervals at different quantile cutoffs (G1-G5). The term "G5-G1" denotes the difference in the effects of vision screening on LogMAR between the first and the fifth quantiles.

C Appendix table

Table A1: Voucher effects: cohort DDD results

	(1)	(2)	(3)	(4)
Dependent variable	LogMAR		Myopia	
Voucher	-0.102***	-0.098***	-0.112*	-0.102
	(0.037)	(0.036)	(0.066)	(0.064)
Gender		-0.042***		-0.074***
		(0.005)		(0.009)
Boarding at school		-0.004		-0.010
		(0.006)		(0.011)
Paternal education		0.003		0.005
		(0.002)		(0.005)
Maternal education		0.006***		0.013***
		(0.002)		(0.005)
Paternal out-migration		0.004		0.009
		(0.004)		(0.009)
Maternal out-migration		-0.008		-0.013
		(0.005)		(0.010)
County Fixed Effects	YES	YES	YES	YES
Cohort Fixed Effects	YES	YES	YES	YES
Urban-Rural Type Fixed Effects	YES	YES	YES	YES
Interaction Fixed Effects	YES	YES	YES	YES
Observations	14,808	14,808	14,808	14,808
Adjusted R-squared	0.080	0.088	0.073	0.080

Notes: This table shows estimation results on the effect of the voucher interventions on students' vision, measured by LogMAR (columns 1-2) and the incidence of myopia (columns 3-4). The independent variable, denoted as "Voucher", is a binary indicator equal to one if a student has received the free eyeglasses voucher in the vision screening program, and zero otherwise. Each model specification includes county-, cohort-, and urban-rural type fixed effects, as well as interaction fixed effects for county by cohort, county by urban-rural type, and cohort by urban-rural type. Columns 2 and 4 further control for individual characteristics, such as gender, boarding status, parental education level, and parental migration status. Standard errors are clustered at class level and appear in parentheses.
* p <0.1, ** p <0.05, *** p <0.01.

Table A2: Voucher take-up effects: early versus later

	(1)	(2)	(3)	(4)
Dependent variable	LogMAR		ΔLogMAR	
Voucher take-up	-0.015		-0.088***	
	(0.009)		(0.012)	
Duration: from voucher take-up to the follow up screening		-0.003		-0.017***
		(0.002)		(0.002)
LogMAR at Wave I	0.411***	0.412***	/	/
	(0.021)	(0.021)	/	/
Baseline controls	YES	YES	YES	YES
Class Fixed Effects	YES	YES	YES	YES
Survey Wave Fixed Effects	YES	YES	YES	YES
Observations	3,315	3,315	3,315	3,315
Adjusted R-squared	0.388	0.388	0.137	0.140

Notes: This table presents the estimation results of the effects of voucher take-up during Wave I and Wave II (earlier group) compared to voucher take-up intervention after Wave II (later group) on students' vision at Wave II, utilizing an alternative dataset comprising two rounds of individual vision surveys. The analysis is restricted to students who received voucher take-up intervention during the vision screening program. The dependent variables are LogMAR (in columns 1-2), and the difference in LogMAR between the two survey waves (in columns 3-4). In the odd-numerated columns, the independent variables are voucher take-up, measured by a binary indicator equal to one if a student received a free eyeglasses voucher during wave I and subsequently exchanged it for eyeglasses within the vision screening program, and zero otherwise. In the even-numerated columns, the independent variables are the time duration between voucher take-up and the sequential vision screening, and assigned as 0 if a student received a free eyeglasses voucher at Wave II. All model specifications include individual characteristics, and fixed effects for county and survey waves, and columns 1-2 further control the LogMAR at wave I. Standard errors are clustered at class level and appear in parentheses.
* p <0.1, ** p <0.05, *** p <0.01.

Table A3: Heterogeneous cohort trend

Sample group	Cohort 2	Cohort 3	Cohort 4	Cohort 5
Panel A: outcome = LogMAR				
Cohort 1	0.002	0.056***	0.004	-0.077***
	(0.015)	(0.015)	(0.021)	(0.026)
Cohort 2		-0.019	-0.034	-0.085***
		(0.026)	(0.023)	(0.015)
Cohort 3			-0.059***	-0.070**
			(0.021)	(0.035)
Cohort 4				0.035
				(0.040)
Panel B: outcome = Myopia				
Cohort 1	0.049*	0.050	0.031	-0.036
	(0.028)	(0.033)	(0.037)	(0.035)
Cohort 2		-0.019	-0.060	-0.041
		(0.041)	(0.039)	(0.028)
Cohort 3			-0.064*	-0.039
			(0.035)	(0.052)
Cohort 4				0.039
				(0.059)

Notes: This table follows the baseline specification but focuses on a subsample generated by considering cohort pairs, with each cell defined by the combination of a row cohort and a column cohort. In this subsample, all cohorts are categorized as part of a post-treatment group, with classification based on the school year and survey timing related to the vision screening program. The dependent variables are LogMAR (Panel A) and the incidence of myopia (Panel B). The independent variable is a binary indicator equal to one if a student has participated in the vision screening program more than once or twice, and zero otherwise. All model specifications include individual characteristics and fixed effects for county, cohort, and survey wave. Standard errors are clustered at class level and appear in parentheses. * p <0.1, ** p <0.05, *** p <0.01.

Table A4: The effect of information availability on vision

	(1)	(2)	(3)	(4)
Dependent variable	LogMAR		Myopia	
Telecom	-0.074***	-0.015	-0.128***	-0.085*
	(0.016)	(0.023)	(0.028)	(0.047)
Telecom # Early screening cohort		-0.091***		-0.065
		(0.028)		(0.054)
Early screening cohort		0.024		0.010
		(0.021)		(0.038)
Baseline controls	YES	YES	YES	YES
County Fixed Effects	YES	YES	YES	YES
Cohort Fixed Effects	YES	YES	YES	YES
Survey Wave Fixed Effects	YES	YES	YES	YES
Observations	18,269	18,269	18,269	18,269
Adjusted R-squared	0.064	0.065	0.066	0.066

Notes: This table shows the estimation results on the effects of the township-level telecommunication penetration on students' vision, measured by LogMAR (columns 1-2) and the incidence of myopia (columns 3-4). The independent variable, denoted as "Telecom", is quantified by the proportion of villages with access to telecommunications within a township before the vision screening program was implemented. In columns 2 and 4, we augment the model specification by including an interaction term between township-level telecommunication penetration and the screening and voucher interventions. All model specifications include individual characteristics and fixed effects for county, cohort, and survey wave. Standard errors are clustered at class level and appear in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Heterogeneous screening effects: voucher take-up

	(1)	(2)	(3)	(4)	(5)
Dependent variable	Voucher take-up(0/1)				
	Baseline LogMAR	Gender	Boarding at school	Parental education	Parental out-migration
2nd Screening	0.086***	0.089***	0.091***	0.078**	0.090***
	(0.026)	(0.022)	(0.022)	(0.031)	(0.023)
2nd Screening × Column variables	0.004	-0.004	-0.020	0.003	-0.004
	(0.032)	(0.014)	(0.018)	(0.007)	(0.014)
Observations	201,298	201,298	201,298	201,298	201,298
Adjusted R-squared	0.716	0.716	0.716	0.716	0.716

Notes: This table follows the model specification in Table 7 by adding interaction terms between "2nd Screening" and various children and household characteristics, including baseline LogMAR, gender, boarding status, parental education level, and out-migration status. The term "2nd Screening" is a dummy indicating whether the survey month occurs after the second round of screening survey. The sample is restricted to those who ultimately used the free eyeglasses vouchers. The dependent variable is a binary indicator of whether a student used the free eyeglasses voucher from the first round of vision screening. All columns include controls for individual and school-by-survey-date fixed effects, as well as interaction terms between the time trend and individual or household characteristics. Standard errors are clustered at the class level and are reported in parentheses.
* p <0.1, ** p <0.05, *** p <0.01.

Table A6: Items used to construct the measures of vision-related behaviors

Variable	Survey Question
Habit index	(1) Do you ever rub your eyes? 1=Never; 2=Occasionally; 3=Often. (2) Do you share a towel with your family for face washing? 1 = Never; 2 = Occasionally; 3 = Often. (3) Do you read while lying in bed? 1 = Never; 2 = Occasionally; 3 = Often. (4) Do you read under the sunshine? 1 = Never; 2 = Occasionally; 3 = Often. (5) How much time do you use a computer every day? 1 = Never use a computer; 2 = Less than half an hour; 3 = Half an hour to one hour; 4 = One to two hours; 5 = Two to three hours; 6 = Three hours or more. (6) How much time do you spend on your phone each day? 1 = Don't use the phone; 2 = Less than half an hour; 3 = Half an hour to one hour; 4 = One to two hours; 5 = Two to three hours; 6 = Three hours or more. (7) How much time do you watch TV each day? 1 = Don't watch TV; 2 = Less than half an hour; 3 = Half an hour to one hour; 4 = One to two hours; 5 = Two to three hours; 6 = Three hours or more. (8) Do you sit close to the TV when watching it? 1 = Don't watch TV; 2 = Never; 3 = Occasionally; 4 = Often. (9) Do you usually do eye exercises? 1 = Yes; 2 = No.
Attitude index	(1) Do you think wearing glasses looks good? 1 = Attractive; 2 = Average; 3 = Unattractive.
Knowledge index	(1) Should students regularly check their eyesight? 1 = Agree; 2 = Disagree; 3 = Don't know. (2) Myopia means not being able to see things up close clearly. 1 = Agree; 2 = Disagree; 3 = Don't know. (3) Myopia is caused by a deformation in the shape of the eye. 1 = Agree; 2 = Disagree; 3 = Don't know. (4) Doing eye exercises can help solve nearsightedness. 1 = Agree; 2 = Disagree; 3 = Don't know. (5) Myopia can be corrected by wearing eyeglasses. 1 = Agree; 2 = Disagree; 3 = Don't know. (6) Myopia but not wearing eyeglasses can affect learning. 1 = Agree; 2 = Disagree; 3 = Don't know. (7) There's no need to wear eyeglasses when a child is myopic. 1 = Agree; 2 = Disagree; 3 = Don't know. (8) Wearing eyeglasses can cause worsening vision. 1 = Agree; 2 = Disagree; 3 = Don't know. (9) Primary students should not wear eyeglasses. 1 = Agree; 2 = Disagree; 3 = Don't know.
Parenting index	(1) Has your parents taken you for a vision screening? 1 = Yes; 2 = No.

Notes: The construction process of each index is as follows: first, we orient all variables that compose an index in such a way that a higher value consistently signifies that the corresponding behavior is associated with unhealthier vision outcomes; second, we standardize those variables using the mean and standard deviations at baseline survey; third, we take an equally weighted average of the index components and exclude from the analysis observations in which any of components are missing. Furthermore, if an index comprises only one component, we designate the standardized variable as the corresponding index.