Estimating the Effects of Educational System Contraction: The Case of China's Rural School Closure Initiative

Emily Hannum, Xiaoying Liu, Fan Wang*

April 13, 2018

Abstract

We estimate the impact of educational infrastructure consolidation on educational attainment using the case of China's rural primary school closure policies in the early 2000s. The rationale for consolidation policies is that economies of scale will enable higher quality education, but distance to school increases for children in villages with school closures. We use data from a large household survey covering 728 villages in 7 provinces in China, and exploit variation in villages' year of school closure and children's ages at closure to identify the causal impact of school closure. For children exposed to closure during their primary school ages, we find an average decrease of 0.60 years of schooling for girls after age 15, but no significant effect for boys. Negative effects strengthen with time since closure. There is no evidence of a differential trend in educational attainment between individuals in villages with school closure and those in villages without, as indicated by an analysis using older cohorts beyond primary school age when closure occurred. Different effects by gender may be related to greater sensitivity of girls' enrollment to distance and greater responsiveness of boys' enrollment to quality. (JEL I25, J16, O15, O18, R53)

*Emily Hannum: Population Studies Center and Department of Sociology, University of Pennsylvania; Xiaoying Liu: Population Studies Center, University of Pennsylvania; Fan Wang (*corresponding*): Department of Economics, University of Houston, Houston, TX 77204 (email:fwang26@uh.edu). We gratefully acknowledge support from Penn's University Research Foundation and School of Arts and Sciences Research Opportunity Grant Program, and additional support from Grand Challenges Canada (PI: Jere Behrman). Thanks also go to Jere Behrman, Aimee Chin, Raymond Fisman, Anthony Howell, Yinghua He, Ruixue Jia, Elaine Liu, Matthew Wiswall, Ma Xiang and Hongliang Zhang for suggestions and comments.

Educational infrastructure consolidation has been a long-standing policy response to declining student populations in high-income countries, with the rationale that economies of scale will enable higher quality education to be delivered in an efficient manner. However, the pressure to consolidate is expanding beyond high-income country settings, as more middle-income countries experience demographic trends of declining fertility, population aging, and rural outmigration.[1] In low- and middle-income countries, past research has addressed the impact of school expansion (Duflo 2001; Breen 2010; Andrabi, Das, and Khwaja 2013; Burde and Linden 2013; Kazianga et al. 2013) and programs to improve access to school (Muralidharan and Prakash 2017) on school enrollment and attainment. However, to date, relatively few studies have considered the impact of school system contraction on student outcomes, outside of high-income countries. To our knowledge, this paper is one of the first to estimate the impact of school consolidation on educational attainment[2] in a developing country.[3] In the context of rural villages in developing countries, school consolidation could significantly increase the cost of school enrollment by increasing the travel cost of attendance, but might also increase the perceived return to education due to improvements in school quality.

China is at the vanguard of the consolidation trend. Nationally, China faced dramatic declines in school-aged cohorts in the early 2000s due to fertility reduction.[4] While fertility is generally somewhat higher in rural areas than urban areas, depopulation through unprecedented rural-urban migration has occurred in these settings.[5] The State Council in 2001 initiated a massive national push to consolidate educational infrastructure. The school consolidation initiative, implemented by county level officials, intended to address sparse demand, inefficiencies in provision, and perceived quality problems in rural education (Mei et al. 2015). Consolidation has happened extremely rapidly in China. By some estimates, the total number of primary schools decreased by 53 percent between 2001 and 2012 (Ding and Zheng 2015; Ma 2017).

1. School consolidation is also emerging as an important policy response to changing demographics in middle income countries with large rural populations such as Thailand (Saengpassa 2017) and India (Malik 2013; Chowdhury 2017). Recent news reports and press releases mention current or planned consolidation efforts in Brazil, South Africa, Malaysia and Indonesia (Setiawati 2010; Ortellado 2015; Harun, Yunus, and Yusof 2017; Tawie 2017).

2. By educational attainment, we mean the highest level of education that an individual has completed. Each year of additional grade completed adds to the current educational attainment of a child. We consider both educational attainment for children who are still going to school and for children who have completed schooling.

3. One important exception is a study of high school closures at the end of the Cultural Revolution in China, which found large declines in high school completion rates and significant negative long term labor market outcomes among individual who were exposed to high school closures (Zhang 2018).

4. For example, China's 0 to 14 population dropped from 321,937,264 in 2000, to 266,616,527 in 2005, to 240,183,007 in 2010, to 233,556,402 in 2015 (United States Bureau of the Census 2017).

5. The urban population in China increased from 26.44 percent in 1990 to 49.23 percent in 2010 (United Nations Population Division 2017).

We analyze the impact of these changes on educational attainment using the 2011 China Household Ethnic Survey (CHES 2011), which is a household and village survey implemented in 728 villages in 7 provinces in western China with substantial minority populations. Driven by the national directive to consolidate schools, 215 of the villages in the sample experienced village primary school closure between 1999 and 2010.[6] Typically, closure decisions were made by county administrators who eliminated village schools and required students to attend schools that were farther away–generally located in township centers–but better appointed. In the CHES survey, compared to villages that had not experienced a school closure, villages that had experienced school closures are on average 3.8 km further away from the closest primary school. Compared to schools in villages without a closure, schools serving villages that had experienced closure have better buildings and technological equipment. We find that villages with and without school closure in our survey have similar income levels, a similar fraction of agricultural and migrant workers, and similar gender compositions. Villages with closure have, on average, 13 percent fewer households (415 vs 469 households per village), indicating that county administrators tended to close primary schools in smaller villages.

Following Duflo (2001), we consider individuals who were already beyond primary school age at year of school closure as potentially unexposed to the effects of the policy. Similar to Behrman, Parker, and Todd (2011), we estimate both age-at-exposure effects and duration-since-exposure effects (hereafter age effects and duration effects, respectively).[7] Behrman, Parker, and Todd (2009, 2011) rely on panel data to identify both age and duration effects of the Progresa/Oportunidades conditional cash transfer program in Mexico on grade progression and attainment as children age. In this paper with cross-sectional data, we exploit the differential calendar years for village-level closure policy initiation to analyze both age and duration effects using a cross-sectional dataset from 2011.

Specifically, to identify the effects of the school consolidation policy, within each province, we first compare the difference in educational attainment (number of grades completed by 2011) between those from closure villages who were exposed to closure to individuals of the same age cohorts from non–closure villages. We also compare

6. We use 1999 as the empirical cut-off year because school closures were sporadic before this date in the data. Even though the central directive for school consolidation was officially issued in 2001, from our data, it seems that the policy was in place in some counties before the nation-wide policy announcement (also see Dai et al. (2017)).

7. The age effect is determined by the age of exposed children at year of closure. For example, given a primary school cycle of 6 years, an 8 year old child who was in 3rd grade in the year of closure is potentially exposed to the consolidated primary school for 3 years. The duration effect is the time since policy initiation and is determined by the calendar year of school closure. For example, children in villages that experienced school closure in the year 2000 have been exposed to closure for 11 years by the year 2011, when the CHES survey took place.

the difference in educational attainment between unexposed individuals from closure villages and individuals of the same age cohorts from non-closure villages. We interpret the difference in these differences as the impact of the policy. We estimate the effects of the policy first over subgroups based on age at year of closure, and then subgroups based both on age at year of closure and the number of years since closure. We interpret results for years since closure as short-, medium- and long-run impacts of the policy on educational progression conditional on age at year of closure.

We find that the school closure policy had a significant negative impact on educational attainment for children exposed to closure. For example, for children who were between age 10 and 13 in the year of closure, we find that school closure reduced grades completed by 0.42 years by 2011, when children are on average 17 years of age. Analyzing girls and boys separately for this subset of children, we find that the reduction in attainment for girls is much greater at 0.60 years, while the reduction for boys is insignificant at 0.24 years. Dividing individuals into subgroups based on age at year of closure as well as the number of years since closure, we also find that the negative effects strengthen with time since closure. For example, for girls who were 6 to 9 years of age at year of closure, there is no significant impact of the policy on their grades completed in the 3 years after closure. However, 4 to 6 years after closure, grades completed is reduced by 0.56 years, and 7 to 12 years after closure, grades completed is lowered by 0.77 years.

To understand the mechanisms that drive these results, we look at the relationship between enrollment and school distance and facility quality. Focusing on children who are between 5 and 12 years of age in 2011, we find that each additional kilometer to school is associated with 1.1 percentage point lower school enrollment for girls. Boys' enrollment is also negatively associated with distance to school, but not significantly so. Additionally, we find that boys' enrollment is higher when the closest primary school to village has better school facilities, but girls' enrollment does not respond to differences in school facility features. By extension, it is possible that increased distance associated with closure tended to impede the education of girls, while improved quality of facilities tended to encourage the education of boys.

The remainder of this paper is organized in the following sections. Section 1 provides background on school consolidation policies, in comparative perspective and in China. In Section 2, we describe the data. Section 3 presents our estimation strategy and estimation equations. The first part of section 4 presents results from a regression model that differentiates the impact of the policy on children who were in different age groups in the year of closure. The second part of section 4 shows differential short-, medium- and long-term impacts of the policy for children in different age groups in the year of closure. Section 5 provides a discussion of mechanisms, with a focus on the potential impact of school quality and distance on school enrollment. Section 6 concludes.

1 Literature and Background

Global background and significance School closures have been a common policy response to declining student populations in sparsely populated rural communities in many countries, with the rationale that economies of scale will enable higher quality education to be delivered in an efficient manner (Howley 2011). For example, in the United States, a trend of consolidating small schools during much of the 20th century reduced the total number of public schools: in 1929 to 30, there were approximately 248,000 public schools, compared to 98,000 in 2013 to 14 (United States Department of Education 2016a, Table 214.10).[8] While rural population decline has created formidable challenges to maintaining rural schools (Blauwkamp, Longo, and Anderson 2011), closure policies have also emerged in urban areas in many countries in recent decades (e.g., Chiu, Joh, and Khoo 2016; Lee and Lubienski 2016). For example, the third largest school district in the United States, Chicago Public Schools, announced in the spring of 2013 a plan to close 54 primary schools with the expectation of saving 43 million USD annually (Lee and Lubienski 2016).[9] The closure decision may be based on a combination of declining enrollments and low achievement, with the idea that economies of scale would enable a higher quality educational experience for those who experience a closure (Kirshner, Gaertner, and Pozzoboni 2010; Engberg et al. 2012).

Beyond the United States, school district mergers and school closures have been widespread, well-documented phenomena in many countries (Bard, Gardener, and Wieland 2006; Kearns et al. 2009; Bartl 2013; Slee and Miller 2015).[10] In Chile, between 2000 and 2012, the educational system changed significantly: 1,651 schools closed, constituting about one-sixth of the contemporary stock, and 3,029 new schools were established–most of which were private-voucher schools (Grau, Hojman, and Mizala 2017). In the Netherlands, consolidation reforms implemented in the 1990s reduced the

8. While in recent years, the number of public schools has held relatively stable, with closures balanced by openings, consolidation remained a non-trivial phenomenon: in 2013 to 14, for example, there were 1,737 school closures, affecting approximately 274,000 students who had been enrolled in the prior school year (2012 to 13) (United States Department of Education 2016b).

9. In the United States, this pressure was tied in part to policy pressures to turn around or close "failing schools" (Deeds and Pattillo 2015).

10. Scholars have also investigated the implications of population aging for school finance and expenditures (for example, Poterba 1998; Ladd and Murray 2001; Gradstein and Kaganovich 2004; Ohtake and Sano 2010; Figlio and Fletcher 2012).

number of primary schools by about 15 percent in just a few years (De Haan, Leuven, and Oosterbeek 2016). In Hong Kong, student enrollment per school started falling around the turn of the millennium, and over 36 percent of primary schools were closed in the decade that followed a closure policy established in 2003 (Chiu, Joh, and Khoo 2016). Press reports suggest that school consolidation is emerging as an important policy response to changing demographics in middle income countries with large rural populations.[11]

Impact of closures Literature on the impact of school closures on affected students shows inconsistent results. Some studies suggest negative effects on performance and outcomes. Grau, Hojman, and Mizala (2017) estimate that school closure increases the probability of high-school dropout between 46 and 62 percent (1.7 and 2.3 percentage points). The authors also identify large causal effects of school closure on grade repetition in primary school. In the United States, one study of the experiences of Latino and African American students in an anonymous urban high school in the year following the closure of their school showed declines in the transition cohort's academic performance after transferring to new schools (Kirshner, Gaertner, and Pozzoboni 2010). A study in another anonymous urban school district showed that students displaced by school closures can experience adverse effects on test scores and attendance, but these effects can be minimized when students move to higher quality schools (Engberg et al. 2012). The same study showed that a negative effect on attendance for students displaced by school closures disappears after the first year in the new school. A study of school consolidations in Denmark from 2010 to 2011 showed that school consolidation had adverse effects on achievement in the short run, but effects appeared to weaken over time, suggesting that part of the effect was due to disruption (Beuchert et al. 2016).

Other studies do not show negative effects. A study of closing poor performing primary schools in Amsterdam showed no negative impacts on student performance (De Witte and Van Klaveren 2014). Another study in the Netherlands indicated that consolidation reforms led to increased student achievement on a nationwide exit examination (De Haan, Leuven, and Oosterbeek 2016). One study of over 200 school closings in Michigan found, on average, no persistent detrimental effect on the achievement of displaced students, and that students displaced from relatively low-performing

11. For example, Thailand's Ministry of Education recently announced a plan to merge thousands of small schools with fewer than 120 students each with other schools within a six-kilometer radius (Saengpassa 2017). In Punjab, India, new reports indicate that of 51,602 primary and middle public schools in the province, about 5,500 primary schools have been merged (Malik 2013); in Rajasthan, in 2014, the government merged 17,000 of the over 80,000 government schools in the state with other schools, with more mergers planned (Chowdhury 2017).

schools experience achievement gains (Brummet 2014). An analysis of closure of charter schools in Ohio indicated that closing low-performing charter schools led to longer-term achievement gains of around 0.2 to 0.3 standard deviations in reading and math for students attending these schools at the time they were identified for closure (Carlson and Lavertu 2016).

Hypothesized mechanisms of impact The inconsistencies in observed impact described in the preceding section could stem from differences in context, or from which among the disparate mechanisms of impact of school closings on student outcomes dominates. By design, school closure typically implies three changes for students in affected communities: they experience disruption, they must attend schools farther away from home, and they attend schools that are larger and better-resourced than the schools that were shuttered. A literature on the disruptive effects of moving schools suggests negative effects from switching schools, but a literature on school and teacher quality suggests the possibility of improvement associated with moving from lower-performing to higher-performing schools (Sacerdote 2012).

Under conditions of mobility associated with closure, students' emotional reactions to the change—anger or disenchantment at school closing, and experiences of stress in a new school and peer context—may impede student achievement and persistence (for example, see Kirshner, Gaertner, and Pozzoboni 2010). One study of student mobility in Texas unrelated to school closing indicated that while cross-district moves tended to be associated with improvements in school quality, within district moves did not, and were associated with short-run achievement costs (Hanushek, Kain, and Rivkin 2004). Sacerdote (2012) investigated the impact of displacement due to Hurricanes Katrina and Rita on long-term academic performance and college going for students in New Orleans. Analyses showed a short-term decline in academic performance, but long-term improvement, with gains concentrated among students initially in the lowest quintiles of the test score distribution. However, evacuees did not show gains in college-going relative to earlier cohorts from the same pre-hurricane high schools.

Research in a variety of contexts in the United States has indicated that the likelihood of attending a school declines as distance to the school increases, possibly due to higher costs such as those involving transportation (Schwartz, Stiefel, and Wiswall 2013). Press reports have raised concerns about distance and student safety in consolidating Chicago Public Schools for students who will need to traverse city neighborhoods (for example, see Chicago Tribune Editorial Board 2017). In developing countries, distance could also be an important determinant of school participation, particularly if long distances are involved or there are safety concerns (Kremer, Brannen, and Glennerster 2013). A multi-level analysis of survey data from 220,000 children in 340 districts of 30 developing countries estimated that parental decisions regarding children's enrollment were associated with distance from school, net of a host of other school, family and community characteristics (Huisman and Smits 2009). One study in Afghanistan implemented a randomized trial to estimate the effects of establishing village-based schools on enrollment and test scores for a sample of 1,479 boys and girls aged six to eleven in 31 villages in Afghanistan (Burde and Linden 2013). Results one year out showed significant enrollment effects, even more for girls than for boys, despite the non-significant observed correlation between distance to school and enrollment of children in the control group. Results also showed a sizeable achievement effect, as measured in math and language test scores. In a study from Pakistan, Jacoby and Mansuri (2015) found that Pakistani girls in lower castes were less likely to cross village boundaries to attend school. These findings illustrate a potential gender difference in the implications of distance for enrollment opportunities. However, this difference in distance effect for girls and boys is not consistently found: One multinational study found similar magnitudes of effect of distance on enrollment at ages 8 to 11 (Huisman and Smits 2009).

The quality of the new school environment may be an important factor conditioning the impact of closure on student outcomes. In the United States, a study in one anonymous urban school district showed that adverse effects of moving schools on test scores and attendance were minimized when students moved to higher quality schools (Engberg et al. 2012). Emerging literature in developing country contexts suggests that children who attend better quality schools are more likely to remain enrolled (Hanushek, Lavy, and Hitomi 2008). One multinational study found that parental decisions regarding children's education were associated with quality-related characteristics of the available educational facilities such as number of teachers (Huisman and Smits 2009).[12]

One caveat is important to mention. While the larger schools students transfer into may provide more resources, research in the United States about the impact of attending larger, presumably better-resourced schools, is inconsistent (Gershenson and Langbein 2015; Schwartz, Stiefel, and Wiswall 2013). One study using the 1980 census to estimate the effects of changes in school size indicated that students born in states where average school size increased obtained lower returns to education and completed fewer years of schooling (relative to the national population) than did earlier cohorts born in the same state (Berry and West 2010).

12. At the same time, while a key presumption of school consolidation is improved quality in larger, better–resourced schools, certain studies in the US have pointed to small schools as providing important benefits to students, and sometimes better outcomes (Bard, Gardener, and Wieland 2006; Schwartz, Stiefel, and Wiswall 2013).

School closure in China China's Compulsory Educational Law, promulgated in 1986, provided the legal foundation for nine years of compulsory education and established the principle that primary schools should be located in close proximity to rural children (Ministry of Education 1986; Dai et al. 2017). A legacy of this principle was a widely distributed network of schools across the country (Yang and Wang 2013). Schools included both complete and "incomplete" (early grades) primary schools. However, demographic changes were already exerting pressures on provision of education at the village-level in the 1990s (Cai, Chen, and Zhu 2017). Dai et al. (2017) report that consolidation experiments were piloted in some provinces in 1993.

National school consolidation policies commenced in 2001 (Dai et al. 2017). On May 29th, 2001, the State Council issued a document entitled "Decision on Basic Education Reform and Development" (General Office of the State Council 2001). This document required local governments to make reasonable adjustments to schools' geographic distribution to improve efficiency.[13] As seen elsewhere, the case for school closures is made in terms of quality and efficiency considerations (see, for example, Liu, Gaowa, and Wang 2013; Xie and Wu 2013). Resource constrained counties may have faced particular pressures toward consolidation (Fan 2013).

The number of rural schools decreased from 512,993 in 1997 to 210,894 in 2010, while teaching points (incomplete primary schools) dropped from 186,962 in 1997 to 65,447 in 2010. The number of students also decreased, from 95 million enrolled students in 1997 to 53.5 million in 2010 (Ministry of Education 1998–2015). However, the pace of school closures generally outstripped the pace of decline of students. Yang and Wang (2013) calculated an "average closure intensity parameter" as a ratio of the percent decline in number of schools and the percent decline in number of students during the same period to denote intensity of school consolidation in each province from 2000 to 2010. By this measure, 22 out of 27 provinces had average closure intensities greater than 1, and the highest reached 13.

Studies of county and provincial government policy documents have indicated that considerations about efficiency and economies of scale dominated decisions about school closures. As school consolidations rolled out across the nation, scholars and journalists raised concerns about the degree to which consolidation policies might be employed to avoid costs associated with compulsory school provision. Ding and Zheng (2015) analyze aggregate provincial educational expenditure data from 1996 and 2009 and find

13. Concurrently, two other national policies–national tax reform (which terminated an agricultural surtax) and compulsory school education policy adjustment reform (which emphasized the financial responsibility of county level government in providing compulsory education)–were issued that gave county level officials greater autonomy and also imposed more budgetary pressures on them (Ding and Zheng 2015).

that provinces with a greater rate of school consolidation significantly reduced their financial expenditure share on primary education. In 2008, the National Development and Reform Commission (NDRC) issued standards prescribing that at least one primary school should be planned in each town (cited in Dai et al. 2017, 3). In 2012, the Ministry of Education and then the General Office of the State Council issued documents calling for an end to consolidation (General Office of the State Council 2012), but persistent population decline in rural China continues to exert immense pressures toward further consolidation, and the number of schools continued to decrease after 2011.[14]

Scott Rozelle, Hongmei Yi and their co-authors have studied the implications of school consolidation policy for student achievement in three adjacent provinces in the north to northwestern part of China: Shanxi, Sha'anxi and Ningxia (Liu et al. 2010; Mo et al. 2012; Chen et al. 2014). Using data from ten counties in Sha'anxi Province and four in neighboring Ningxia Hui Autonomous Region, Liu et al. (2010) find that the primary school closure between 2002 to 2006 did not impact the academic performance of students in 2006, but the timing of mergers in students' lives mattered: higher-grade students' grades rose after merging, while grades of younger students fell. In three counties in Sha'anxi Province and one county in neighboring Shanxi Province, Mo et al. (2012) and Chen et al. (2014) find that elementary school students' academic performance improved when they transferred from less centralized schools to more-centralized schools.

By design, achievement studies must focus on students who remain in school to take tests. A limitation of this approach is the lack of attention to dropout, continuation, or attainment. Non-continuation might be expected to be a crucial mechanism of impact of consolidation. To understand the full implications of consolidation, including implications for performance, attention must be paid to short and longer-term implications for educational continuation and attainment. Presumably, quality improvements in primary schools attended could increase the chances of school continuation. At the same time, concerns about safety of children associated with traveling long distances or boarding at schools could detract from continuation, and it is possible that safety concerns might be more pronounced for girls than for boys.

A separate set of concerns have to do with distance and associated cost burdens to rural families. Using Chinese Household Income Project data from 2008 and 2009, Cai, Chen, and Zhu (2017) study the effects of the consolidation policy on 209 households and find that the compulsory school consolidation program increased educational expenditures, including expenditures on transportation and boarding due to greater dis-

14. The number of schools further decreased to 118,381 in 2015, and the number of students enrolled decreased further, to below 30 million in 2015 (Ministry of Education 1998–2015).

tance to school. Using data from one county in Guangdong province, Zhao and Barakat (2015) find that children from poorer families have difficulties paying for a bus or boarding at school and are more likely to endure longer commutes. It is possible that poor rural families would be more likely to shoulder costs for boys than girls: some research in China suggests that girls' educational attainment is more susceptible than boys' to poverty (Cherng and Hannum 2013; Liu and Hannum 2017).

In summary, school consolidation has been a major policy initiative in China, but the implications are not yet well understood. In particular, existing studies of impact on students have focused on important questions of impact on short-term school performance, but have not considered the impact on school continuation or attainment. In addition, existing studies of impact on students have had limited geographic coverage, collectively and individually, and have not distinguished short- and long-term consequences. The current study begins to address these limitations by applying a difference-in-difference design to investigate short and long-term implications of school consolidation for educational attainment using data from 728 villages across seven provinces.

2 Data

This paper utilizes data from the rural sample of the China Household Ethnic Survey (CHES 2011), which covers households and villages from 728 villages in 81 counties of 7 provinces with substantial minority populations in western China.[15] CHES 2011 sought to investigate the economic and social conditions of people in minority areas, and so utilized subsamples of the National Bureau of Statistics' Rural Household Survey (RHS) in the seven provinces with substantial minority populations. Household information was collected through diaries and single-round visits in 2011. Routinely-collected RHS data and purpose-designed questionnaires for the CHES project were included in the data.

Village closure information is taken from a village head survey, which was collected in conjunction with household surveys. Village heads were asked if the village currently had a primary school, and asked about the year of school closure if the village school had been closed. Based on the village heads survey, there are four categories of closure status. The first category includes 193 villages that did not have village schools in 2011 and experienced school closure between 1999 and 2010. In the second category, which included 22 villages, a school closure year between 1999 and 2010 was reported, but village heads also reported that the village currently had a school in 2011. In this case,

15. Appendix Section A.1.1 presents a map of CHES Survey prefectures (Howell 2017) and discusses distribution of survey villages across provinces.

it is plausible that new schools were built in these 22 villages after school closure.[16] In the third category, 430 villages had village schools in 2011 and did not experience school closure.[17] Finally, the fourth category includes 48 villages that had never had a primary school and 35 that do not currently have a school but had a village primary school at some point between 1954 and 1999. In the following analysis, we designate the first and second categories as school closure. The third and fourth categories are coded as non-closure.[18]

There is heterogeneity in the timing of school closure. Out of the 193 villages in the first category mentioned above, 14 experienced school closure between 1999 and 2001, 28 between 2002 and 2004, 80 between 2005 and 2007, and 71 between 2008 and 2010. School closure took place in all seven provinces in all the year ranges listed. In addition, the intensity of school closure also varied across provinces. Among the villages in this dataset, approximately 49 percent of the surveyed villages from Hunan province in south-central China and Inner Mongolia in north China reported village school closures between 1999 and 2010. Around 24 percent of surveyed villages from Ningxia and Xinjiang in northwestern China, and Guizhou in southwest China experienced village school closures between 1999 and 2010. Guangxi in south-central China and Qinghai in northwest China had the lowest ratio prevalence of closure. In these locations, around 18 percent of villages surveyed in 2011 by CHES reported having experienced closure between 1999 and 2010.

Comparing villages with and without school closure In Table 1, we compare village-level statistics between villages with and without closure across several sets of variables. All variables are from the village-head survey component of the CHES data. Summary statistics are organized in Panels A through C. The first column shows the overall averages for all villages. The second and third columns show the mean values for villages with and without closure respectively. Column four presents the p-value from a significance test of whether the means differ between non-closure and closure villages.[19] And column five tests, just for villages with closures, whether a linear trend exists for the variables across the year of closure (1999 to 2010).

16. Generally students went to schools in township centers after village school closure, but in these 22 villages, it is possible that a new consolidated school was built inside these villages.

17. We do not have survey information on the opening year of the schools. The vast majority of these schools should have been established in the 1980s and early 1990s when the central government aimed to have a school in each village to provide education to rural children.

18. We will test the robustness of regression results to dropping the second and fourth categories from the closure and non-closure groups.

19. We control for provincial fixed effects in these mean tests, but results are generally the same even if provincial fixed effects are not controlled for.

Table 1: Summary Statistics for Village Characteristics

	Villages with and without school closures				
	all	**group averages**		**p-values testing**	
	mean	*non-closure*	*closure*	*closure vs non-closure†*	*years of closure trend‡*
Panel A: Closest Primary School Physical Facility Measures					
Fraction with non-dilapidated buildings	0.82	0.79	0.90	0.00	0.40
Fraction with heating	0.25	0.18	0.44	0.00	0.38
Fraction with tap water	0.81	0.80	0.84	0.19	0.22
Fraction with kitchen	0.70	0.66	0.81	0.01	0.15
Fraction with shower	0.19	0.15	0.32	0.00	0.34
Fraction with sufficient desks	0.94	0.93	0.96	0.23	0.27
Fraction with library	0.76	0.72	0.85	0.00	0.49
Fraction with personal computers	0.53	0.47	0.68	0.00	0.73
Fraction with internet access	0.46	0.41	0.58	0.00	0.27
Panel B: Distance to Closest Primary School (km)					
Distance measure from village head survey	2.87	1.80	5.67	0.00	0.15
Panel C: Village Size and Demographics					
Per household arable land (mu, 1 acre = 6 mu)	11.59	10.15	15.09	0.00	0.58
Number of households	453.55	469.35	415.04	0.02	0.62
Fraction of Han in village	0.40	0.36	0.48	0.02	0.31
Fraction of women (all ages)	0.48	0.48	0.47	0.08	0.15
Fraction of 30 and younger (2011)	0.46	0.48	0.42	0.00	0.23
Fraction of women 30 and younger (2011)	0.46	0.46	0.46	0.37	0.13

Village Level Summary Statistics. All variables are from the village-head survey except for fraction of woman and fraction of individuals below age 30. † Column 4 shows results from testing if the mean gaps between closure and non-closure villages are statistically different: p-value results in column 4 controls for provincial fixed effects. ‡ Column 5 tests if values for each variable differs over the calendar year of closure for villages experiencing school closure.

In Panels A and B of Table 1, we compare distance to school and school facility measures. In both panels, village heads reported information for the complete primary school[20] that was closest to the village in 2011. For villages with school closure, the statistics reported are for the "replacement" school that village children attend, away from their own village.

Panel A compares nine kinds of school facilities (non-dilapidated buildings, heating, tap water, kitchen, shower, sufficient desks, library, personal computers, and internet access) for the closest primary school between closure villages and non-closure villages. The current schools for closure villages are more likely to have each of the nine kinds of physical facilities. In terms of technology, in current schools for closure villages, 68 percent of schools have computers and 58 percent have internet access. These frac-

20. Complete primary school is defined as a school that includes all the grades in primary school, i.e., grade 1 to grade 6.

tions are 47 percent and 41 percent for schools for non-closure villages. Regarding other kinds of facilities, in schools serving closure villages, 44 percent have heating, 84 percent have tap water, 81 percent have kitchens, and 32 percent have showers. Corresponding figures are 18 percent, 80 percent, 66 percent, and 15 percent in schools serving non-closure villages.[21] Column 4 shows that the provision of school facilities tends to differ in closure and non-closure villages: 7 out of 9 facilities measures have p-values close to 0. Moreover, for villages with closure, there is no linear trend in quality with the calendar year of school closure (as indicated by larger p values in column 5), which means that we do not find replacement schools' facilities to be systematically better for villages that closed schools more recently than those with earlier closures.

Panel B shows that the distance to school is significantly greater for villages with school closure than without. The average distance is 5.67 kilometers for villages with school closure,[22] compared to 1.80 kilometers for the latter. 65 percent of villages without closure report a 0 kilometer distance to the closest school.[23] In short, these findings suggest that schools serving closure villages, compared to non-closure villages, are better-resourced and more distant.

In Panels C of Table 1, we show that villages without school closure have, on average, 469 households, while villages with school closure have, on average, 415 households. Households in closure villages also have significantly more arable land per person and are more likely to be classified as non-minority (ethnic Han).[24]

In Panels A, B, C and D of Table 7 in the Appendix, we test how villages with and without school closure differ along several other dimensions, in terms of political connectedness (Panel A), income and labor market participation (Panel B), village expenditures (Panel C) and participation in other national policy schemes such as the "Grain

21. We do not have information on school facilities for the previously closed within-village schools from the closure villages. If these schools were similar or worse in facility measures to village-school facility measures for non-closure villages, then these data indicate that school closure might have brought about a significant improvement in school facility quality. This change would be consistent with the stated goal of the policy to improve quality through consolidation. We do not have information on teachers' quality in these schools.

22. The 25th percentile, median and 75th percentile in the distribution of distance to school are, respectively, 2 kilometer, 3.5 kilometer, and 8 kilometer.

23. Distance to school is not 0 for all villages without school closure. Households are located in various parts of a village, and the questionnaire did not specify if distance to school should be from the village center or from a village boundary. Some of the non-zero values possibly reflect the vantage point of the village head. It is also possible that a primary school exists in the village, but it is a teaching point rather than a full primary school with all 6 grades, and village heads reported distance to a full primary school further away.

24. In villages with closure, per household arable land is about 15.09 mu (1 acre=6 mu), in villages without closure, per household arable land size is only 10.15 mu. In non-closure villages, non-Han ethnic groups account for on average 64 percent of the village populations. In closure villages, non-Han ethnic groups account for 52 percent of the village population. These means are significantly different, but there are no mean trends in these variables for closure villages across closure years.

for Green" reforestation initiative, collectively-owned medical station initiatives, and the rural medical insurance scheme (Panel D). Controlling for provincial fixed effects, we generally do not find statistical differences between closure and non-closure villages along these dimensions.

Closure year, children and attainment As school closure took place in different years across villages, we are able to group children based on their ages in the year of school closure and their ages in 2011 with our cross-sectional data. Table 2 presents our data composition structure (with rows showing age at year of school closure and columns showing age in 2011). Table 2 illustrates which subset of children could have been impacted by the closure policy. Table 2 also shows average educational attainment for each group, as well as the sample size for each group.

The extent to which a given child is affected by the school closure policy depends on his or her age at the time of school closure and the duration of exposure (which is the difference between the age in 2011 and the age at time of school closure). First, the individuals in row groups A, B, C of Table 2 were between age 1 to 5, 6 to 9 and 10 to 13 respectively in the year of closure and could have been directly affected by the primary school closure policy. Those in row groups D and E were between age 14 to 21 and 22 to 29 in the year of closure and should not have been directly impacted (as students usually enter secondary school at age 14). Individuals in row group F are not exposed to the school closure policy. Second, for the column groups, individuals in the second to fourth column are between age 5 and 19 in 2011 and mostly still attending school. Individuals in columns 5 through 8 are between age 20 and 44 in 2011, and their 2011 educational attainment generally reflects their final attainment. Third, the group of individuals in each row and column cell were exposed to school closure at different starting ages, and experienced different durations of impact when we observe their educational attainment (number of grades completed) in 2011. To be specific, row group B shows that among individuals who were between 6 to 9 years of age in their village-specific school closure years, 98 were between 5 to 9 years of age, 211 were between 10 to 14, 69 were between 15 to 19, and 17 were between 20 to 24 in the year 2011.

In Table 2, we also compare educational attainment within each age-in-year-2011 group.[25] The educational attainment variable is based on years of schooling completed by each individual.[26] By simple comparisons of the mean, we find among those who

25. In Appendix Section A.1.3, we also compare educational attainment and the proportion of individuals who complete middle schools by gender within each age-in-year-2011 group.

26. The survey asks individuals to report years of schooling completed. For example, 4 is recorded for someone who has completed 4th grade in primary school, 9 is recorded for someone who has completed

Table 2: Exposure Groups and Grades Completed by 2011

	Age at village-specific year of closure and 2011 age							
	Age in 2011		*Age in 2011*		*Age in 2011*		*Age in 2011*	
	0-4	**5-9**	**10-14**	**15-19**	**20-24**	**25-29**	**30-34**	**35-44**
Group A: age 1 to 5 at year of closure								
Mean grades completed	0.08	1.11	5.18	9.69				
Observations	303	333	126	16				
Group B: age 6 to 9 at year of closure								
Mean grades completed		1.98	5.51	9.80	10.7			
Observations		98	211	69	17			
Group C: age 10 to 13 at year of closure								
Mean grades completed			5.98	9.40	10.4	8.50		
Observations			133	224	117	2		
Group D: age 14 to 21 at year of closure								
Mean grades completed			7	9.80	10.2	9.05	8.59	
Observations			16	276	592	241	32	
Group E: age 22 to 29 at year of closure								
Mean grades completed					9.98	8.81	8.14	7.99
Observations					101	460	322	98
Group F: non-closure villages individuals								
Mean grades completed	0.09	1.37	5.55	9.33	9.71	8.44	7.37	6.81
Observations	783	1227	1521	1774	2237	1713	1420	1569

Tables 3, 4 and 5 columns 3 and 4 regression data

Table shows means. Individuals in Group A were fully exposed to consolidated primary schools. Group B individuals were 6 to 9 at year of closure and were exposed to consolidated primary school for more than half of their primary school years. Group C individuals were 10 to 13 at year of closure and transitioned from village schools to consolidated primary schools during the final years of primary school. Group D and E individuals are from villages that experienced closure, but were beyond primary school age at year of closure. Group F individuals are from villages without school closure.

are too old to be affected by school closure policy (i.e., groups D and E) from closure villages, their average educational attainment are uniformly higher than their counterparts in non-closure villages (group F), no matter what age group in 2011. However, for those who were at school ages at the time of school closure (groups B and C), their average educational attainment is not consistently higher or lower compared to their counterparts, at different ages in 2011. In the following sections, we proceed to study the causal impact of school consolidation policy on children's educational attainment in a regression framework.

middle school, and 10 is recorded for someone who has completed one year of high school.

3 Methods

Identification strategy We follow a difference in difference strategy to identify the effects of the school consolidation policy. Within each province, we first compare the difference in educational attainment (number of grades completed by 2011) between those from closure villages who were exposed to closure to individuals of the same age cohorts from non–closure villages. This first difference could be due to the school consolidation policy or could be due to existing differences in educational attainment across villages that would have occurred without the policy. Hence, we also compare the difference in educational attainment between individuals not impacted by the consolidation policy from closure villages and individuals of the same ages cohorts from non-closure villages. This second difference should not be related to school consolidation but measures only existing differences in educational attainment across villages. We allow this second difference to be village-specific. We interpret the difference in the first and second differences as the impact of the policy. We estimate the effects of the policy first over subgroups based on age in year of closure (age effects), and then subgroups based both on age at year of closure and the number of years since closure. We interpret results for years since closure as short-, medium- and long-run impacts of the policy on educational progression conditional on age at year of closure (duration effects).

Our age effects estimation strategy follows Duflo (2001). Similar to Duflo (2001), we exploit the fact that individuals were at different ages in the village-specific years of closure to distinguish between individuals who could be impacted and who should not be impacted by the school closure policy. Specifically, the policy could impact both children who were attending elementary school at the time of school closure, and children who were yet to attend elementary school.[27] Those who were above elementary school age (age 13) at the time of the school consolidation program are less likely to be affected by this policy.[28] Specifically, we classify individuals from villages that have experienced closure into cohorts who are fully exposed (0 to 5 years old at the time of primary school closure) or partially exposed (6 to 13 years old at the time of primary school closure) to school closure and the older cohorts who are not exposed (between 14 and 21 at the time of school closure).

Our joint age and duration effects analysis follows Behrman, Parker, and Todd

27. Chinese children normally attend primary school between the ages of 6 and 12. Nearly all children in underdeveloped rural Western areas attend school for some number of years. They generally start elementary school between age 5 and 7, and the vast majority finish elementary school between age 12 to age 14.

28. By age 14, in our data, more than 80 percent of the children are no longer in elementary school.

(2009, 2011). They study the effects of the Progresa/Oportunidades conditional cash transfer program on educational attainment (grades completed) and differentiate between the short term and long term effects of the policy on children who were in different ages at year of closure. Specifically, they study program effects for children who were exposed to the program when they were 9-10, 11-12, and 13-15 at the start of the program in 1997, and 6 years later in 2003, when these children were 15-16, 17-18, and 19-21. They rely on a panel dataset to analyze these effects.[29] Under the assumption that the closure policy is the same across calendar years, we are able to estimate the joint age and duration effects of the policy using a cross-sectional dataset because of the variations in the years in which closure took place.

The age at which school closure takes place determines the *age effect* of the policy–which corresponds to the potential number of years that a child attends a consolidated primary school. The gap between the survey year, 2011, and the year in which village specific closures took place determines the *duration effect* of the policy–which corresponds to the number of years since village specific closures. Closures closer to 2000 provide us with longer-duration effects of the policy on final attainment, and more recent closures provide us with shorter-duration effects of the policy on the number of grades completed for children who are still attending school. The effect of the policy will be conditional on both the starting age at exposure and the length of exposure for each individual. As discussed previously, Table 2 shows the distribution of children with different starting ages of exposure and lengths of exposure (by subtracting age at exposure from age in 2011) in the sample.

Overall, the primary underlying assumption of our strategy is that in the absence of the school closure policy, the change in educational attainment at a certain age of younger cohorts relative to older cohorts would not have been systematically different in treatment (closure) and control (non-closure) villages within the same province, i.e., educational attainment in both groups of villages follows a provincial common trend. Given that the closure policy took place at the village level, we have the advantage of including village-specific fixed effects to account for unobservable differences in educational attainment across villages that are common across cohorts considered. Additionally, the large number of villages experiencing closure and the large number of control villages means that the difference in difference estimates are averaged over a many groups that experience policy changes. This means that our estimates are more likely to be robust to the presence of age- and location- specific random effects (Conley

29. Duflo (2001), however, uses a cross-sectional dataset gathered 20 years after the start of a school construction program in Indonesia, and hence focuses on the long run effects of the program on final educational attainment.

and Taber 2011).

Our common trend assumption, however, would be violated if county administrators choose to close the schools in villages where educational attainment is already deviating from the provincial trend. In other words, we may suffer from endogeneity bias if the closed schools were getting worse over time relative to the average time trend. In order to test whether the identification assumption is valid, we compare the differences in educational attainment between the cohort groups between age 14 to 21 at the time of exposure (the age group in which individuals have finished primary school but may still be attending school), and cohort groups between age 22 to 30 (the age group in which almost no individuals attend school). If our "common trend" assumption stands, controlling for various fixed effects, the difference of educational attainment between age subgroups within these age ranges should not differ significantly across villages with and without closure in the same province.

In addition, our estimation may raise concerns about sample selection bias if school closure had led to increased outmigration either by individual laborers or by families (Liu and Xing 2016). The first of these possibilities—increased labor migration caused by school closure—is less of a concern for our estimates, as the survey gathered data on educational attainment for all members of households including those working as migrant workers. For the second possible migration issue, however, we have not seen evidence supporting the proposition that entire households out-migrate as a result of school closure. An additional concern related to sample selectivity could emerge if a child's birth location is endogenous with respect to school consolidation policy. That is to say, if parents chose where to live before children are born with consideration to the quality of schools, which may be related with the risk of school closure, sample selection would be a concern. However, in the rural China context, mandatory household registration and land allocation policies do not allow people to choose their registered household location freely, even in rural areas.

Regression model with only age effects In Equation 1, we generalize our estimation strategy first to a regression framework in which we assume that the policy has an immediate and constant impact on educational attainment. In the scenario without policy intervention, educational attainment, or the number of grades completed, E of a child i from village v in province p and whose age in 2011 is a could be decomposed into four parts: a village fixed effect β_v, a province-specific age fixed effect ρ_{pa}, and idiosyncratic terms including one part that can be explained by observed characteristic X_i and another unobserved error term ε_i. With school closure, the policy's effect is assumed to be additive and captured by $\tilde{\lambda}_z$ that is constant within age group z defined by age-at-closure t_i:

$$(1) \qquad \begin{aligned} E_{pvia} &= \phi + \beta_v + \rho_{pa} \\ &+ \tilde{\lambda_z} \cdot \mathbb{1}\{l_z \leq t_i \leq u_z\} \cdot c_v \\ &+ X_i \cdot \gamma + \varepsilon_i \end{aligned}$$

where, ϕ is a constant, and c_v is a binary variable indicating if individual i is from a village v with school consolidation (i.e. treatment village). We group children in villages with school closure into Z groups based on their age at closure t_i, with lower and upper bounds for each group l_z and u_z. Therefore $\tilde{\lambda_z}$ captures the average treatment effect for age group z.

This specification imposes two key assumptions. First, $\tilde{\lambda_z}$ is not specific to 2011 age a, which means $\tilde{\lambda_z}$ captures the average effect across the children who were exposed to closure starting at the same age but who have different ages in 2011. We relax this assumption in Equation 2 which has both age and duration effects. Second, $\tilde{\lambda_z}$ is not specific to the calendar year in which the closure policy was implemented.

Regression model with age and duration effects Besides the age at school closure, the impact of school closure on educational attainment may also differ by the number of years of exposure to the policy: short-run effects of closure on a child's educational attainment progression could be dampened or amplified over the medium and long run.[30] In order to identify both age and duration effects with our cross-sectional data, we exploit the variation in the year of school closure. Under the assumption that the impact of the policy is not specific to the calendar year of closure, we can estimate Equation 2 to obtain the impact of the policy as a function of both starting age and the length of exposure.

In Equation 2, we use similar notations as in Equation 1, the difference is that the policy's effects are now captured by $\hat{\lambda_{zl}}$ that varies by age-at-closure variable t_i and years-of-exposure variable τ_i:

$$(2) \qquad \begin{aligned} E_{pvia} &= \phi + \beta_v + \rho_{pa} \\ &+ \hat{\lambda_{zl}} \cdot \mathbb{1}\{(l_l \leq \tau_i \leq u_l) \cap (l_z \leq t_i \leq u_z)\} \cdot c_v \\ &+ X_i \cdot \gamma + \varepsilon_i \end{aligned}$$

30. After an individual completes schooling, duration effects will become constant. In studies with cross-sectional data taken long after a policy has been implemented, Duflo (2001) for example, the duration effect is irrelevant because all educational attainment data is observed long after sample individuals have completed schooling. In our data, a significant proportion of individuals have not completed schooling, allowing us to have meaningful duration effects.

where, as before, c_v is a binary variable indicating if individual i is from a village v with school consolidation (i.e. treatment village). As in Equation 1, we group children in villages with school closure into Z groups based on their age at closure, with lower and upper bounds for each group, l_z and u_z. To capture duration effects, we further divide each of the Z groups of children into L groups based on the length of exposure τ_i, defined as the gap between individual i's age in 2011 and i's age at year of school closure, t_i.[31] Each l length of exposure group includes those with τ_i falling within lower and upper bounds, l_l and l_u. The exposure groups allow us to separately estimate the short, medium and long run effects of the consolidation policy on educational attainment. There are $Z \cdot L$ groups of interest for this regression.[32]

4 Results

4.1 Age effects only results

Table 3 presents estimates of Equation 1. The first panel presents overall results, while the sex-specific results are shown in the second and third panels. In each panel, we compare three subsets of children below age 14 against baseline group – those between 14 and 21 at the time of school closure. Columns 1 and 2 include all individuals between 1 and 44 years of age in 2011, columns 3 and 4 restrict to individuals between 10 and 34, and columns 5 and 6 restrict further to individuals between 15 and 34 years of age. The even-numbered columns drop villages that never had a school from the villages without closure group (category 4 as defined in data section). All regressions include several individual and household controls.[33] All standard errors are clustered at the village-level. Column 1 contains our focal main result, other columns contain results for robustness checks which we discuss later.

The estimates in the Table 3 show that the school consolidation policy had a clear negative impact on educational attainment in terms of grades completed by 2011, but only for girls. Panel One of the first column shows that the policy decreased the educational attainment for children who were below age 6, between age 6 and 9, and between

31. $\tau_i = \min(a_i, a_i - t_i)$: τ_i is the gap between age in 2011 and t_i if individual i was borne before the year of closure, and it is the age of the child in 2011 if the child was borne after school closure.

32. Ideally, we would estimate the policy effects for each t_i and τ_i combination separately, but we have constructed the Z and L groups due to limited sample size.

33. All regressions include controls for households size, a dummy for if the individual is Han and a categorical variable for the relative wealth. The relative wealth variable is based on the survey question that asked households if they are better or worse off than village average. We do not have income measures for all families. The village income per capita variable shown in summary Table 1 is from the village-head survey and not based on household incomes.

Table 3: Effect of School Closure on Educational Attainment

	Outcome: grades completed by year 2011					
			$10 \leq$ 2011 Age ≤ 34		$15 \leq$ 2011 Age ≤ 34	
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline group: Child was 14-21 years old at village primary school closure year						
Panel Male and Female						
Age at closure 0-5	-0.24	-0.30				
	(0.17)	(0.19)				
Age at closure 6-9	-0.29*	-0.38**	-0.39**	-0.54***		
	(0.16)	(0.18)	(0.18)	(0.20)		
Age at closure 10-13	-0.42***	-0.47***	-0.46***	-0.53***	-0.49***	-0.55***
	(0.14)	(0.16)	(0.14)	(0.15)	(0.15)	(0.16)
Age at closure 22-29	0.11	0.18	0.026	0.12	-0.0077	0.079
	(0.17)	(0.18)	(0.17)	(0.19)	(0.17)	(0.19)
Observations	18804	15918	12072	10289	9998	8538
Panel Female						
Age at closure 0-5	-0.43*	-0.61**				
	(0.23)	(0.25)				
Age at closure 6-9	-0.49**	-0.65***	-0.58**	-0.78***		
	(0.22)	(0.25)	(0.26)	(0.29)		
Age at closure 10-13	-0.60***	-0.69***	-0.60**	-0.69***	-0.60**	-0.65**
	(0.23)	(0.25)	(0.23)	(0.26)	(0.28)	(0.30)
Age at closure 22-29	0.19	0.27	0.051	0.12	0.067	0.11
	(0.23)	(0.23)	(0.25)	(0.25)	(0.26)	(0.26)
Observations	8869	7466	5664	4790	4658	3946
Panel Male						
Age at closure 0-5	-0.067	0.0034				
	(0.20)	(0.22)				
Age at closure 6-9	-0.042	-0.060	-0.096	-0.20		
	(0.20)	(0.22)	(0.23)	(0.25)		
Age at closure 10-13	-0.24	-0.21	-0.27	-0.28	-0.33	-0.34
	(0.19)	(0.20)	(0.19)	(0.20)	(0.22)	(0.23)
Age at closure 22-29	0.15	0.20	0.14	0.28	0.051	0.18
	(0.22)	(0.24)	(0.24)	(0.27)	(0.24)	(0.27)
Observations	9935	8452	6408	5499	5340	4592
Exclusions and controls:						
Village and province-age FE and controls†	Yes	Yes	Yes	Yes	Yes	Yes
Exclude villages that never had schools‡		Yes		Yes		Yes

†Includes village fixed effects, province-specific age FEs, controls for ethnicity, household size and relative household wealth.
‡Odd columns check robustness by dropping category 2 closure and category 4 non-closure status villages discussed in Section 2.
Statistical significance:* 0.10 ** 0.05 *** 0.01. Robust standard error clustered at village level. Each column (in each panel) represents a separate regression. Compare children with different starting age of exposure (and length of exposure) to children in baseline group who should not be impacted by school closure. Columns 3 to 6 restrict regressions to subsets of individuals who are closer together in 2011 age, testing the robustness of results to excluding younger and older individuals (see Table 2). Regression sample includes villagers who were below 29 at year of closure for villages that experienced closure between 1999 and 2010. Individuals included in regressions from all columns are below 45 years of age in year 2011.

age 10 and 13 in the year of closure by 0.24 (s.e. 0.17), 0.29 (s.e. 0.16), and 0.42 (s.e. 0.14) years, respectively. The effects for these three age ranges are small and insignificant for boys, but large for girls with reductions of 0.43 (s.e. 0.23), 0.49 (s.e. 0.22), and

0.60 (s.e. 0.23) years. Results are consistent across all columns.

Different age effects reflect the different possible mechanisms by which children under different age groups are affected by the school consolidation policy. Children who were below age 6 and not enrolled in any school yet at the year of closure could be affected by delaying entry into primary schools that are much further away for the concern of safety. Children who were between age 6 and 9 at the year of closure faced transitioning from village school to consolidated schools and the possible disruption of school life, as well as much longer traveling distance. Finally children who were between age 10 and 13 at year of closure attended consolidated schools in their final primary school years. These individuals are on average 16.8 years old in 2011. For these individuals, closure took place during final years of primary school when school transition was potentially the most disruptive and the opportunity cost of travel time was higher. The 0.60 year reduction in grades completed for girls in this group is due to the cumulative effects of consolidation during and after primary school.

4.2 Age and duration effects results

Each age-at-closure group includes individuals of different ages in 2011 as shown in Table 1, our estimates from Table 3 show the weighted average effect of children exposed to closure starting at the same age group but with different durations. The effects of school closure on educational attainment may not be homogeneous across children with different durations of exposure to the policy. The impacts of closure could amplify or weaken as children progress through school.

In order to capture the duration effects, we next allow the estimated impact of the policy to vary in the short, medium and long term. For our main results, we consider five age-at-closure groups ($Z = 5$), and for those under 13 we further divide each age group into 3 subgroups according to the number of years since closure ($L = 3$): 0 to 3, 4 to 6 and 7 to 12 years, representing short, medium and long-term exposure separately[34]. As before, age group 14 to 21 serves as reference group. By separating children into these Z and L groups, we are following, in principle, a similar strategy as Behrman, Parker, and Todd (2009, 2011) who analyze the effects of policy changes on grades completed by looking at both what they call *exposure differential* as well as *time since program initiation*. In our analysis, children who were at younger ages in the year of closure had greater *exposure differential* to consolidated primary schools, and children from villages that experienced closure closer to 1999 have had longer *time since program*

34. Individuals who were 10 to 13 at the time of closure and experienced 7 to 12 years of exposure are on average about 21 years old in 2011, hence we can interpret the impact here as the impact of policy on final attainment.

initiation.[35]

Given our earlier findings on differential gender effects, we estimate the model for girls and boys separately, with the results presented in Table 4 for girls and Table 5 for boys. All standard errors are clustered at the village-level. Column 1 presents our main results. The other columns provide robustness checks, to which we return following presentation of main results. The structure of the tables is the same as in Table 3. We again focus on column 1.

Impact on girls For girls, results from Table 4 are consistent with our finding from Table 3 in that school consolidation had a large and significantly negative impact on girls. Across all affected age groups, the coefficients are all small and insignificant in the first three years after exposure, but more negative and significant after a longer duration. The first column in Table 4 shows that for the 0 to 5 age-at-closure group, the impact of policy was negative but insignificant at -0.15 (s.e. 0.26) years of education within the first 3 years after school closure. The lack of a strong impact here is expected because most of the children in this group are still too young to attend primary school. After 4 to 6 years and 7 to 12 years, the closure policy decreases average grades completed by 0.55 (s.e. 0.29) and 0.68 (s.e. 0.36) years, respectively. Most of the children in these two subgroups are still attending or just finishing up with primary school in 2011. These negative effects of school closure for girls could be driven by delayed entry into primary school due to potential safety and cost concerns given longer travel distance. Once children start primary school, continued safety and cost concerns could make regular attendance more difficult and longer travel distance could reduce time available for studying. With only 16 children in the 0 to 5 age-at-closure group at age above 15 in 2011 (see Table 2), we do not know the full effects of closure on final attainment for individuals in the 0 to 5 age-at-closure group, however, the overall pattern here indicates that longer duration amplifies the negative effects of closure on girl's educational attainment.

The second group of coefficients in column 1 of Table 4 shows the impact of the policy on girls who were 6 to 9 when school closure took place. For this group of children, the impact of the policy one to three years after closure is insignificant at -0.22 (s.e. 0.33) years. This indicates that girls who were in earlier primary school grades were

35. Behrman, Parker, and Todd (2011) focus on short and long run time-since-program-initiation effects of short policy exposure, and they also analyze the long run time-since-program-initiation effects of longer policy exposure. Here, given variation in school closure years, we have a continuous measure of time-since-program-initiation (duration-effects) which we group into short, medium and long duration sub-categories. And we analyze effects for three subsets of individuals with different lengths of policy exposures (age-effects subgroups).

Table 4: Effect of School Closure on Female Educational Attainment

	Female Outcome: grades completed by year 2011					
			$10 \leq 2011$ Age ≤ 34		$15 \leq 2011$ Age ≤ 34	
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline group: Child was 14-21 years old at village primary school closure year						
Child was 0-5 years old at village primary school closure year						
x (0-3 years since closure)	-0.15	-0.29				
	(0.26)	(0.30)				
x (4-6 years since closure)	-0.55*	-0.68**				
	(0.29)	(0.31)				
x (7-12 years since closure)	-0.68*	-0.94**				
	(0.36)	(0.38)				
Child was 6-9 years old at village primary school closure year						
x (0-3 years since closure)	-0.22	-0.38	-0.25	-0.64		
	(0.33)	(0.41)	(0.51)	(0.65)		
x (4-6 years since closure)	-0.56*	-0.76**	-0.63	-0.83**		
	(0.34)	(0.34)	(0.38)	(0.39)		
x (7-12 years since closure)	-0.77**	-0.86**	-0.76**	-0.80*		
	(0.32)	(0.34)	(0.38)	(0.42)		
Child was 10-13 years old at village primary school closure year						
x (0-3 years since closure)	-0.53	-0.71*	-0.53	-0.71*		
	(0.35)	(0.39)	(0.38)	(0.43)		
x (4-6 years since closure)	-0.59*	-0.72**	-0.63*	-0.78**	-0.66	-0.81*
	(0.33)	(0.33)	(0.37)	(0.37)	(0.42)	(0.41)
x (7-12 years since closure)	-0.76**	-0.74*	-0.65*	-0.59	-0.58	-0.51
	(0.35)	(0.41)	(0.38)	(0.45)	(0.40)	(0.47)
Child was 22-29 years old at village primary school closure year						
All years since closure	0.20	0.28	0.074	0.12	0.078	0.10
	(0.22)	(0.23)	(0.25)	(0.26)	(0.26)	(0.27)
Observations	8869	7466	5664	4790	4642	3932
Exclusions and controls:						
Village FE, province-age FE and controls†	Yes	Yes	Yes	Yes	Yes	Yes
Exclude villages that never had schools‡		Yes		Yes		Yes

†Includes village fixed effects, province-specific age FEs, controls for ethnicity, household size and relative household wealth.
‡Odd columns check robustness by dropping category 2 closure and category 4 non-closure status villages discussed in Section 2.
Statistical significance:* 0.10 ** 0.05 *** 0.01. Robust standard error clustered at village level. Each column (in each panel) represents a separate regression. Compare children with different starting age of exposure (and length of exposure) to children in baseline group who should not be impacted by school closure. Columns 3 to 6 restrict regressions to subsets of individuals who are closer together in 2011 age, testing the robustness of results to excluding younger and older individuals (see Table 2). Regression sample includes villagers who were below 29 at year of closure for villages that experienced closure between 1999 and 2010. Individuals included in regressions from all columns are below 45 years of age in year 2011.

successfully transferred from village schools to consolidated schools without immediate disruption to grade progression. It is possible that student achievement is negatively affected by school closure and transition in the short-run (Hanushek, Kain, and Rivkin 2004; Sacerdote 2012), but lower achievement might not have a clear impact on grade progression in the short-run. Interestingly, we do find that the impact of consolidation amplifies over the medium and long run for this subset of girls. The policy reduces

Table 5: Effect of School Closure on Male Educational Attainment

	Male Outcome: grades completed by year 2011					
			10 ≤ 2011Age ≤ 34		15 ≤ 2011Age ≤ 34	
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline group: Child was 14-21 years old at village primary school closure year						
Child was 0-5 years old at village primary school closure year						
x (0-3 years since closure)	-0.030	0.17				
	(0.25)	(0.26)				
x (4-6 years since closure)	-0.17	-0.20				
	(0.27)	(0.28)				
x (7-12 years since closure)	-0.0044	0.0088				
	(0.26)	(0.29)				
Child was 6-9 years old at village primary school closure year						
x (0-3 years since closure)	-0.47	-0.57*	-0.59	-0.83*		
	(0.29)	(0.32)	(0.43)	(0.46)		
x (4-6 years since closure)	-0.030	-0.063	-0.11	-0.16		
	(0.31)	(0.33)	(0.35)	(0.37)		
x (7-12 years since closure)	0.28	0.31	0.22	0.14		
	(0.28)	(0.31)	(0.35)	(0.38)		
Child was 10-13 years old at village primary school closure year						
x (0-3 years since closure)	-0.48*	-0.54*	-0.54*	-0.58		
	(0.26)	(0.32)	(0.29)	(0.36)		
x (4-6 years since closure)	0.047	0.064	0.034	0.016	-0.12	-0.15
	(0.27)	(0.27)	(0.29)	(0.28)	(0.31)	(0.30)
x (7-12 years since closure)	-0.30	-0.26	-0.32	-0.34	-0.36	-0.38
	(0.30)	(0.32)	(0.32)	(0.33)	(0.34)	(0.35)
Child was 22-29 years old at village primary school closure year						
All years since closure	0.14	0.20	0.12	0.25	0.038	0.17
	(0.22)	(0.24)	(0.24)	(0.27)	(0.25)	(0.28)
Observations	9935	8452	6408	5499	5321	4578
Exclusions and controls:						
Village FE, province-age FE and controls†	Yes	Yes	Yes	Yes	Yes	Yes
Exclude villages that never had schools‡		Yes		Yes		Yes

†Includes village fixed effects, province-specific age FEs, controls for ethnicity, household size and relative household wealth.
‡Odd columns check robustness by dropping category 2 closure and category 4 non-closure status villages discussed in Section 2.
Statistical significance:* 0.10 ** 0.05 *** 0.01. Robust standard error clustered at village level. Each column (in each panel) represents a separate regression. Compare children with different starting age of exposure (and length of exposure) to children in baseline group who should not be impacted by school closure. Columns 3 to 6 restrict regressions to subsets of individuals who are closer together in 2011 age, testing the robustness of results to excluding younger and older individuals (see Table 2). Regression sample includes villagers who were below 29 at year of closure for villages that experienced closure between 1999 and 2010. Individuals included in regressions from all columns are below 45 years of age in year 2011.

attainment by 0.56 (s.e. 0.34) years in 4 to 6 years after closure, when girls are due to attend middle school; and the policy reduces attainment by 0.77 (s.e. 0.32) years in 7 to 12 years after closure, when it is about time for them to finish high school. These findings suggest that after exposure to consolidated schools in the second half of primary school, girls on average experience slower progression through middle school. Some of the long run effects are due to continued slower path of progression in high school and some are possibly due to failure to enter high school. In a separate set of regressions, we do indeed find that the high school completion rate is up to 8 percentage points lower for girls in this age-at-closure group 7 to 12 years after school closure.[36]

In the third group of coefficients in the first column of Table 4, we show estimates for the impact of closure on children who were 10 to 13 years old at the time of closure. The policy reduces attainment by 0.53 (s.e. 0.35) years in the short run. The negative impact of the policy on girls is significantly amplified 4 to 6 years and 7 to 12 years after the policy, reducing attainment by 0.59 (s.e. 0.33) years and 0.76 (s.e. 0.35) years, respectively. The magnitude of the short run impact of the policy here is larger than the two younger age-at-closure groups, indicating that perhaps there is a more immediate effect of school closure for children who were in the 4th, 5th and 6th grade at the time of closure. The closure policy could be more disruptive for these children who need to learn more difficult material and prepare for middle school entry. Children in this age-at-closure cohort are on average 17 years old 4 to 6 years after the policy, and 21 years old 7 to 12 years after the policy. The similar coefficient on the medium and long run impact indicates that the negative effects of the policy persist until the end of high school age: consolidation has a potentially a persistent negative effect on the final attainment for girls.

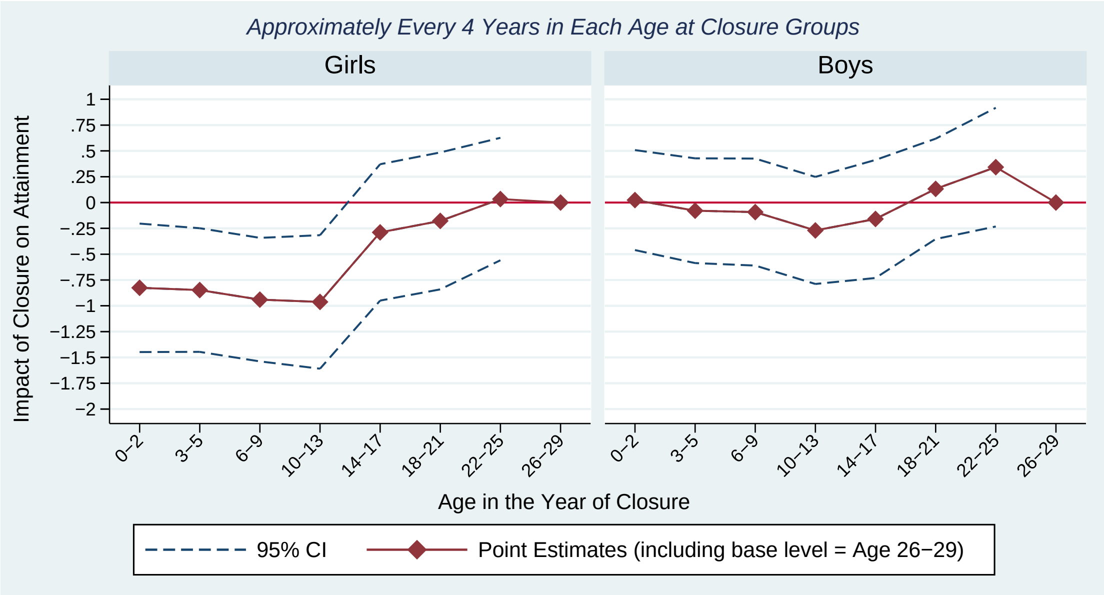
Overall, the results here show that the closure policy impacts both girls who are already attending primary school and girls who were yet to enter primary school. We find a strengthening of the negative impact of closure over time for all girls' age-at-closure groups. For the two younger age-at-closure cohorts, we do not know the effect of the policy on final attainment. However, given that impacts persist even until 12 years after closure, our long run estimates are likely lower bound for the effect of the policy on final attainment for these girls in the younger year-at-closure groups. Results do not differ significantly across columns.

Impact on boys We again see a large difference between boys and girls when we compare Table 5 for boys with Table 4 for girls: while we see medium and long run negative impacts of school closure on different subgroups of age-at-closure for girls, most of impacts on boys are not significant.

For boys in the 0 to 6 age-at-closure group, column 1 in Table 5 shows that the impact of school consolidation is very close to zero from 0 years up to 12 years after closure. For boys in the 6 to 9 and 10 to 13 age-at-closure groups, the impacts of the policy are again mostly insignificant. The exception is for boys who were 10 to 13 at the time of closure. For this group, the closure policy reduces grades completed by

36. Results for high school completion are available by request from the corresponding author.

Figure 1: Effect of School Closure on Educational Attainment (Number of Grades Completed by 2011) by 8 Age-at-Closure Group.



Each *a*–*b* group shows impact of closure on grades completed by 2011 for children who were between *a* and *b* years of age at the time of school closure. These results correspond to results from column 1 of Table 3 which had 5 age-at-closure groups (see Section 4.1), but use finer age-at-closure groupings. All coefficients are from estimating Equation 1.

0.48 years in the short–run (s.e. 0.26). This finding matches up with our finding from Table 4, where the policy has similar magnitude of impact on girls in the same age subgroup. These results indicate that perhaps the policy is disruptive to both boys and girls who are in the higher grades of elementary school. This effect could be due to difficulty of completing–in the process of school transition–the relatively heavy school workload. While girls' attainment in this age-at-closure group worsens 4 to 12 years after closure, we see no impact of the policy on boys 4 to 12 years after. Perhaps boys catch up quickly afterwards, as parents continue to support their educational efforts and they eventually succeed in transitioning. Girls, however, are unable to overcome this difficulty and the negative impacts persist.

4.3 Robustness and common trend

Common trend As discussed earlier, our estimation relies on the "common trend" assumption, which assumes in the absence of the policy, the trend in educational attainment across cohorts are the same in the treated and control villages. We check the common trend assumption for our specification with only age effects in each regression table and in Figure 1. In Table 3, 4 and 5, we test the assumption by including another group – those who were 22 to 30 years-of-age at the time of school closure – and should not be affected by the policy to compare with the reference group. As shown in the final row for each table, the coefficients on this age group's interaction with the closure policy are generally slightly positive but not significantly different from the baseline group (i.e. age-at-closure between 14 and 21). These coefficients are generally closer to zero when we exclude younger and older individuals by 2011 age in columns 3 through 6 of the tables. If villages that were selected for closure were already on a significant downward trajectory compared to provincial trend, we would expect to see a positive and significant coefficient here.

Following Duflo (2001), we test parallel trends with a finer set of age subgroups in Figure 1. In Figure 1, we show regression coefficients when we estimate Equation 1 by considering eight age-at-closure groups, 0 to 2, 3 to 5, 6 to 9, 10 to 13, 14 to 17, 18 to 21, 22 to 25, and 26 to 29. Age-at-closure group 26-29 is the base group. The regression is the same as the one from Column 1 of Table 3 except with the addition of more age-at-closure groups and a change in the base group. For the four groups that were above 13 years of age at the time of school closure,[37] the policy effects, with the 26-29 age-at-closure group as base group, are not significantly different from zero. For the four age groups lower than 14, the coefficients are significantly negatively deviating from the pre-existing trend. The figure also shows that for boys, the trend line is flat and insignificant from zero for all age-at-closure ages. Figure 3 in the Appendix shows similar results as Figure 1 with even finer age-at-closure breakdowns.

Robustness checks Our results are robust to several checks: 1) excluding younger and older age groups who have only completed a part of formal education at schools by 2011 or whose educational attainment might be systematically different from that of younger cohorts, 2) dropping villages that never had a school from the villages without school closure group, and 3) testing results across different age-at-closure groups and with different cutoffs ages.

First, although the inclusion of young age-at-closure groups allows us to see the impact of closure on children who started school after school closure, many of these individuals are in still early schooling years in 2011. These very young individuals and also older individuals are potentially ill-suited to be estimated jointly in an environment with village fixed effects. For Tables 3, 4 and 5, in columns three and four, we restrict the sample to individuals who are included inside the red solid box as shown in Table 2: only individuals who are between 10 and 34 years of age in 2011 and who were at least

37. In villages without closure, there is no "age-at-closure", i.e., the variable for the x-axis. These individuals in the villages without closure are matched to their counterparts in the treated villages by current age in 2011. The correspondence between age-at-closure and current age is shown in Table 2.

age 6 in the year of school closure among those in villages with closure. In columns five and six of these tables, we further restrict the sample and include only those who are between 15 and 34 years of age in 2011, and who were above age 10 in the year of closure among those in closure villages. For these tables, there are no significant differences in coefficients as we go from columns 1 and 2 to the more restricted age samples of later columns, although standard errors tend to increase due to significant drops in sample size.

Second, as reported in the data section, there is a subset of villages that report never having had a school or as having only had a school at some point between 1949 and 1999. For the odd-numbered columns in the regression tables, we include these villages along with villages that currently have a school and did not experience closure. We drop them in the even-numbered columns. For Table 3, columns 2, 4, and 6 have slightly more negative coefficients than columns 1, 3 and 5, perhaps due to a more precise comparison between villages with and without closure. We also seem to have a similar strengthening of coefficient magnitude in the even columns of Tables 4 and 5, but the results are less clear there.

Third, we run the same regression model but with different subgroups, cutoff ages, and groupings for age-at-closure and years-since-closure. Some of these results are shown in Figure 1 discussed earlier, and also shown in Figure 3 in the Appendix.[38] All these analyses show the robustness of our main estimation results.

5 Mechanism–Distance, Quality and Enrollment

As noted earlier, school closures typically imply disruption, greater distance, and better quality school facilities for affected students. Our prior analyses showed that the impact of closure on attainment is not of short-term duration, indicating that there is limited short-term disruption effects of closure on attainment. To investigate mechanisms behind the impacts of school closure on educational attainment, we analyze how the two variables distance to school and quality of school are linked to enrollment status at the time of the survey. Here, we focus only on children between 5 to 12 years of age in 2011, ages in which nearly all children are currently attending primary school in 2011.[39] For these children, we have information about distance to primary school and

38. Appendix section A.2 shows some addition tables and discusses some of these results. More results are available upon request from the corresponding author.

39. We include children who were age 13 at year of closure in the previous attainment regressions as children who could potentially be exposed to school closure because 13 year-olds could still be attending primary school. For the enrollment regression here, we only include ages in which children could only be attending primary school to focus on primary school enrollment.

primary school quality.[40] We use children from both villages with and without school closure in these regressions. The data for these analyses come from columns 2 and 3 of Table 2.

We make use of the information from village survey on school facilities and distance to closest primary school, which we summarized in Panel A and B in Table 1. For the distance to school variable, we use both the continuous version of the variable shown in Table 1, and also group the values into three categories: 0 kilometers to school, (0 to 3) kilometers to school (medium distance), and greater than 3 kilometers (long distance). The median distance is 2 kilometers and 7 kilometers for the second and third category, respectively. Schools of these three distance categories serve 46, 32 and 22 percent of the sample villages. We create an index for school facility quality by summing up the nine facility dummy variables: the value for this variable ranges from 0 to 9. We also divide the index value into three groups for a categorical version of the variable indicating the number of facilities that a school has: 0 to 3, 4 to 6, or 7 ot 9. Schools of these three facility categories serve 19, 48 and 33 percent of the sample villages.

Primary school enrollment in 2011 is high but not full. For children at 5 years of age, the enrollment rate is 10 percent. At age 6 and 7, the enrollment rate increases to 47 percent and then 83 percent. Enrollment peaks at 94.4 percent at age 11.[41] In the following regressions, we analyze the relationship between distance, quality and school enrollment.

We regress enrollment on distance to closest primary school and quality of these schools. Regressions control for county fixed effects, province-specific age fixed effects, village per capita income, village per capita land size, village population size, household relative wealth, the number of household members and household ethnicity. Despite the controls, the coefficients we obtain for distance to school and quality of school would not be causal if there are unobserved village-level attributes that affect enrollment and that are also correlated with distance and quality. Our inclusion of village-level controls and county fixed effects, however, seeks to reduce the risk of omitted variables bias our estimates.

We present the enrollment regression results for children from 5 to 12 years of age in Table 6. We run similar regressions in Table 12 in the Appendix, where we allow effects of distance and quality to differ for age subgroups 5 to 8 and 9 to 12. Panel A of Table 6 presents results where we regress enrollment on the continuous distance to school variable and the continuous aggregate school facility quality variable. In panel B of

40. For older individuals in the survey who are included in the earlier attainment regressions, we do not have measures for the quality of school when they were attending primary school.

41. After primary school, at age 13, 15 and 17, enrollment rates drops to 92, 82, and 55 percent respectively.

Table 6: Linear Probability Model of School Enrollment, Age 5 to 12

	Outcome: enrolled in school or not in 2011					
	All Age 5 to 12		Girls Age 5 to 12		Boys Age 5 to 12	
	all villages	no teaching points	all villages	no teaching points	all villages	no teaching points
Panel A: Continuous Distance and Quality Measures						
Distance (km) to Primary School	-0.0047**	-0.0060**	-0.0056*	-0.011***	-0.0035	-0.0019
	(0.0023)	(0.0028)	(0.0031)	(0.0039)	(0.0029)	(0.0042)
Number of Primary School Facilities	0.0028	0.00017	-0.0048	-0.0025	0.0094*	0.0033
	(0.0044)	(0.0047)	(0.0067)	(0.0078)	(0.0055)	(0.0057)
Observations	2460	2033	1130	942	1330	1091
Panel B: Categorical Distance and Quality Measures						
Categorical Distance (Compare to 0 km)						
$0 < x \leq 3$ $(median \approx 2)$ *km*	-0.022	-0.025	-0.032	-0.048	-0.028	-0.020
	(0.017)	(0.019)	(0.025)	(0.030)	(0.024)	(0.026)
$3 < x \leq \max$ $(median \approx 7)$ *km*	-0.063***	-0.082***	-0.084***	-0.14***	-0.053*	-0.039
	(0.023)	(0.030)	(0.032)	(0.042)	(0.030)	(0.041)
Categorical Quality (Compare to 0-3)						
4 to 6 Facilities	0.021	0.014	-0.0094	-0.013	0.049*	0.038
	(0.023)	(0.025)	(0.028)	(0.031)	(0.028)	(0.031)
7 to 9 Facilities	0.035	0.023	0.0029	0.0077	0.067**	0.041
	(0.025)	(0.027)	(0.033)	(0.038)	(0.031)	(0.034)
Observations	2460	2033	1130	942	1330	1091

Statistical significance:* 0.10 ** 0.05 *** 0.01. Standard Errors clustered at village level. Each column is a separate regression. Distance to closest primary school and school facility information are from village head survey. School facilities include if the school has pipe water, library, computers, non-dilapidated buildings and others shown in Table 1. All regressions include county fixed effects, province-specific age fixed effects, controls for village per capita income, village per capita land, village population size, household relative wealth, the number of household members and household ethnicity.

Tables 6, we show results for regressing school enrollment on the categorical variables for school distance and facility quality. In the first two columns of the table, we show results for both boys and girls between age 5 and 12, in columns three and four, we show results for girls between age 5 and 12, and in columns five and six, we show results for boys between age 5 and 12. In columns 1, 3 and 5, we show results including all villages. In columns 2, 4, and 6, we drop villages that contain a partial primary school (teaching-point) but not a complete primary school.[42]

Overall, Panel A in Table 6 shows that longer distance to school is linked to lower enrollment, especially for girls. School facility quality does not have an impact on girls' enrollment, but boys are more likely to attend schools with better physical facilities. For all children between age 5 and 12, columns 1 and 2 show that a kilometer increase in school distance is associated with an 0.47 (s.e. 0.23) and 0.60 (s.e. 0.28) percentage

42. Village heads were asked to answer distance and quality questions for the closest full primary school to the village. But in villages with primary school teaching-point, most children between age 5 and 12 should be attending the village school and only attend the full primary school at the end of primary school age for the 5th and 6th grades. The distance and school quality variables reported for this type of schools might not, therefore, reflect the actual distance to and quality of the relevant school for the vast majority of children between age 5 and 12 in these villages.

point reduction in school enrollment respectively. Columns 3 and 5 show that the reduction is -0.56 (s.e. 0.31) percentage points for girls and -0.35 (s.e. 0.29) percentage points for boys; excluding villages with primary school teaching-points, columns 4 and 6 show that the reduction is -1.1 (s.e. 0.39) percentage points for girls and -0.19 (s.e. 0.42) percentage points for boys. There is no overall significance in the impact of school facility quality on enrollment, but for boys, columns 5 and 6 shows that for the 0 to 9 valued school facility aggregate measure, each additional facility increases school enrollment by 0.94 (s.e. 0.55) percentage points and 0.33 (s.e. 0.57) percentage points respectively.

Panel B of Table 6 uses the categorical variables for distance and quality we constructed and matches up with the results from Panel A. For all regressions, the comparison villages have 0 kilometers for distance to school and 0 to 3 school facilities. In terms of distance, from column 3, for girls, there is a 3.2 (s.e. 2.5) and an 8.4 (s.e. 3.0) percentage point reduction in enrollment associated with attending medium distance (0<distance<=3 kilometers) and long distance (distance>3 kilometers) schools. For boys, from column 5, the respective reductions are 2.8 (s.e. 2.4) and 5.3 (s.e. 3.0) percentage points. As in Panel A, the coefficients in the even columns are more negative for the girls and closer to zero for the boys. In terms of quality, columns 3 and 5 of Panel B of Table 6 shows that higher school facility measures do not impact school enrollment for girls but do for boys: having a school with 4 to 6 facilities and 7 to 9 facilities are associated with a 4.9 (s.e. 2.8) and a 6.7 (s.e. 3.1) percentage point increase in enrollment for boys, respectively. As in Panel A, the quality coefficients are still positive for the boys but less significant in column 6 of Panel B. In combination, a village with a primary school that is more than 3 kilometers away and that has 6 to 7 facilities might see a significant reduction in enrollment for girls, but minimal effects for boys, as the potential positive facility quality impact and negative distance impact for boys largely cancel out.

Village parents determine school enrollment for young boys and girls based on the costs and benefits of enrollment. The school consolidation policy potentially changed both the costs and benefits concurrently, but differentially for boys and girls. Longer travel distance in difficult terrains might involve more transportation costs for both girls and boys, leading to lower school enrollment. The larger negative effects associated with girls might be due to potential additional parental concerns for girls' safety during longer travel, and higher opportunity cost of school enrollment for girls who otherwise could help out with household chores. Improved school facilities in consolidated schools could potentially increase the value of schooling for all children. However, the results here indicate that parents might have only perceived gains for boys in attending schools with better facilities, but did not value as much the gain in school quality for girls. The resistance towards school enrollment for young girls when distance to school increases is likely one of the contributing factors to the persistent negative effect of closure on girls' educational attainment that we find in earlier sections. We did not find a significant effect of the consolidation policy on boys' attainment, possibly because the positive effects from better quality and negative effects from longer distance cancel out.

6 Conclusion

To our knowledge, this paper is one of the first to estimate the impact of school consolidation on educational attainment in China, which is at the vanguard of a trend emerging in rural areas of many large middle–income countries. Specifically, we use multi-province data to estimate the impact of school consolidation on school progression and attainment in China. We find that children's educational outcomes are negatively affected, overall, by this policy. Our analyses indicate that children under 14 years of age at year of school closure experienced on average 0.24 to 0.42 fewer years of school attainment by 2011. This negative effect is not a temporary disruption: negative effects appear to strengthen with time since closure. Moreover, there is a striking contrast between boys and girls: while boys are less affected by this policy, girls exposed to the policy experience on average up to 0.60 fewer years of grades completed by 2011.

Our empirical results are consistent with certain possible mechanisms. The first mechanism, and probably the most important one is the much greater distance to schools following closure, and the corresponding increase in travel costs. This change could impede families in sending children to school. Media outlets report families delaying entry for young children and leaving school-aged children in boarding schools or rented apartments in town centers with parents or grandparents (Hui 2009). Such strategies will obviously increase financial costs for families who are exposed to school closure, and for those who have difficulty affording such strategies, children are likely to drop out of school earlier. A second possible mechanism of impact is the change of schooling environment and education quality. A core rationale for consolidation has been the expectation that more centrally located schools provide better quality education and thus improve students' performance, and some studies are consistent with this idea, though the effects may be partially offset by boarding (Mo et al. 2012; Chen et al. 2014).[43] As families and youth make decisions about educational continuation, the greater costs and

43. However, a counter-narrative has emerged in news reports highlighting that schools were overcrowded and underprepared for an influx of children (Yu and Jiang 2010; Wu 2014), and academic studies have provided inconsistent evidence (Liu et al. 2010; Chen et al. 2014).

risk associated with attending school at a distance must be weighed against the potential benefits of attending a better-resourced school. These calculations may differ for girls and boys.

Underlying the observed patterns of gender difference is a decision process in which parents weigh the benefits and costs of attending school for children–differently for boys and girls. Even though the cost of attending school increases for households who lose access to within-village primary education, the perceived benefit of boys attending school appears large enough to counteract the extra cost brought by school consolidation. On the other hand, the instrumental benefit associated with educating daughters is likely perceived to be less than that for sons by parents, on average. Although previous studies estimate higher rates of return to schooling for girls than boys in the 1980s-1990s (Zhang et al. 2005), a tradition of patrilocal marriage in most parts of rural China means that girls' returns may be viewed as unlikely to flow to natal households. In addition, attending schools outside the villages is likely more risky and costly for girls than for boys. As a result, the school-consolidation policy may have pushed a fraction of households over the margin from sending girls to school to not sending them.

Beyond China, in an age of global population aging and large-scale migration, sparse school-aged populations in rural communities are common. Designing education supply policies that appropriately balance efficiency and equity concerns in such contexts is a difficult challenge. School consolidation initiatives are emerging as a common response in many middle-income countries with large rural populations, with recent media reports describing closure initiatives in countries including India, Thailand, Malaysia, Indonesia, South Africa, and Brazil (for example, see Chowdhury 2017; Malik 2013; Harun, Yunus, and Yusof 2017; Ortellado 2015; Saengpassa 2017; Setiawati 2010; Tawie 2017)

Yet, the likely implications of these initiatives for educational access and inequality are poorly understood. Results presented here indicate that the school consolidation policy in rural China has had a negative impact on girls' educational attainment, but not boys'. While the impact, and gender differences in the impact, of consolidation are not likely be identical across nations, these findings highlight a significant case where consolidation has affected access and inequality and suggest the need for further scholarly attention to an emerging policy response to global demographic change.

References

Andrabi, Tahir, Jishnu Das, and Asim Ijaz Khwaja. 2013. "Students today, teachers tomorrow: Identifying constraints on the provision of education." *Journal of Public Economics* 100 (Supplement C): 1 –14.

Bard, Joe, Clark Gardener, and Regi Wieland. 2006. "Rural School Consolidation: History, Research Summary, Conclusions, and Recommendations." *Rural Educator* 27 (2): 40–48.

Bartl, Walter. 2013. "Economisation of the Education System in Shrinking Regions? The Demographic Responsiveness of Education Demand and Supply at Different Levels of the Education System." *Comparative Population Studies* 39 (2).

Behrman, Jere R., Susan W. Parker, and Petra E. Todd. 2009. "Medium-Term Impacts of the Oportunidades Conditional Cash Transfer Program on Rural Youth in Mexico." In *Poverty, Inequality, and Policy in Latin America,* edited by Stephan Klasen and Felicitas Nowak-Lehmann, 219–270. Cambridge, MA: MIT Press.

———. 2011. "Do Conditional Cash Transfers for Schooling Generate Lasting Benefits? A Five-Year Followup of PROGRESA/Oportunidades." *The Journal of Human Resources* 46 (1): 93–122.

Berry, Christopher, and Martin West. 2010. "Growing Pains: The School Consolidation Movement and Student Outcomes." *Journal of Law, Economics, and Organization* 26, no. 1 (April 1): 1–29.

Beuchert, Louise Voldby, Maria Humlum, Helena Nielsen, and Nina Smith. 2016. *The Short-Term Effects of School Consolidation on Student Achievement: Evidence of Disruption?* IZA Discussion Papers 10195. Institute for the Study of Labor (IZA).

Blauwkamp, Joan M., Peter J. Longo, and John Anderson. 2011. "School consolidation in Nebraska: Economic efficiency vs. rural community life." *Online Journal of Rural Research & Policy* 6 (1): 1.

Breen, Richard. 2010. "Educational Expansion and Social Mobility in the 20th Century." *Social Forces* 89, no. 2 (December 1): 365–388.

Brummet, Quentin. 2014. "The effect of school closings on student achievement." *Journal of Public Economics* 119 (November): 108–124.

Burde, Dana, and Leigh L. Linden. 2013. "Bringing Education to Afghan Girls: A Randomized Controlled Trial of Village-Based Schools." *American Economic Journal: Applied Economics* 5, no. 3 (July): 27–40.

Cai, Weixian, Gong Chen, and Feng Zhu. 2017. "Has the compulsory school merger program reduced the welfare of rural residents in China?" *China Economic Review* 46 (December): 123–141.

Carlson, Deven E., and Stephane Lavertu. 2016. "Charter school closure and student achievement: Evidence from Ohio." *Journal of Urban Economics* 95:31–48.

Chen, Xinxin, Hongmei Yi, Linxiu Zhang, Di Mo, James Chu, and Scott Rozelle. 2014. "Do poor students benefit from China's Merger Program? Transfer path and educational performance." *Asia Pacific Journal of Education* 34, no. 1 (January 2): 15–35.

Cherng, Hua-Yu Sebastian, and Emily Hannum. 2013. "Community poverty, industrialization, and educational gender gaps in rural China." *Social forces* 92 (2): 659–690.

Chicago Tribune Editorial Board. 2017. "The case for a new Englewood High School." *Chicago Tribune* (Chicago, IL) (June 15).

Chiu, Mingming, Sungwook Joh, and Lawrence Khoo. 2016. "The effects of school closure threats on student performance: Evidence from a natural experiment." *B.E. Journal of Economic Analysis and Policy* 16 (4).

Chowdhury, Shreya Roy. 2017. "Cramped classrooms, long commutes, dropouts: How Rajasthan's school mergers have hurt students." The Scroll. (Delhi, India) (May 3).

Conley, Timothy G., and Christopher R. Taber. 2011. "Inference with "Difference in Differences" with a Small Number of Policy Changes." *The Review of Economics and Statistics* 93 (1): 113–125.

Dai, Te-qi, Liang Wang, Yu-chao Zhang, Cong Liao, and Zheng-bing Liu. 2017. "The Cost of School Consolidation Policy: Implications from Decomposing School Commuting Distances in Yanqing, Beijing." *Applied Spatial Analysis and Policy* (September): 1–14.

De Haan, Monique, Edwin Leuven, and Hessel Oosterbeek. 2016. "School Consolidation and Student Achievement." *Journal of Law, Economics, and Organization* 32, no. 4 (November): 816–839.

De Witte, Kristof, and Chris P.B.J. Van Klaveren. 2014. "The influence of closing poor performing primary schools on the educational attainment of students." *Educational Research and Evaluation* 20 (4): 290–307.

Deeds, Vontrese, and Mary E. Pattillo. 2015. "Organizational "Failure" and Institutional Pluralism: A Case Study of an Urban School Closure." *Urban Education* 50 (4): 474–504.

Ding, Dong, and Fengtian Zheng. 2015. "Dismantling teaching points: integrate education resources or reduce investment in education." *China Economic Quarterly* 14 (2): 603–622.

Duflo, Esther. 2001. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." *American Economic Review* 91, no. 4 (September): 795–813.

Engberg, John, Brian Gill, Gema Zamarro, and Ron Zimmer. 2012. "Closing schools in a shrinking district: Do student outcomes depend on which schools are closed?" *Journal of Urban Economics* 71, no. 2 (March): 189–203.

Fan, Xianzuo. 2013. "The Reasons, Motivation, and Selection of Approach to the Consolidation of Primary and Secondary Schools in Rural Areas." *Chinese Education & Society* 46, no. 5 (September 1): 9–20.

Figlio, David N., and Deborah Fletcher. 2012. "Suburbanization, demographic change and the consequences for school finance." *Journal of Public Economics* 96, no. 11 (December): 1144–1153.

General Office of the State Council. 2001. "Directive 21 (2011) of the General Office of the State Council of the People's Republic of China on Basic Education Reform and Development." (Beijing, China).

———. 2012. "Directive 48 (2012) of the General Office of the State Council of the People's Republic of China on Rural Compulsory Education and School Reorganization." (Beijing, China).

Gershenson, Seth, and Laura Langbein. 2015. "The Effect of Primary School Size on Academic Achievement." *Educational Evaluation and Policy Analysis* 37, no. 1 (May 1): 135S–155S.

Gradstein, Mark, and Michael Kaganovich. 2004. "Aging population and education finance." *Journal of Public Economics* 88 (12): 2469–2485.

Grau, Nicolas, Daniel Hojman, and Alejandra Mizala. 2017. *School Closure and Educational Attainment: Evidence from a Market-based System.* University of Chile, Department of Economics, January.

Hanushek, Eric A., John F. Kain, and Steven G. Rivkin. 2004. "Disruption versus Tiebout improvement: the costs and benefits of switching schools." *Journal of Public Economics* 88, no. 9 (August): 1721–1746.

Hanushek, Eric A., Victor Lavy, and Kohtaro Hitomi. 2008. "Do Students Care about School Quality? Determinants of Dropout Behavior in Developing Countries." *Journal of Human Capital* 2, no. 1 (March): 69–105.

Harun, Hana Nax, Arfa Yunus, and Teh Athira Yusof. 2017. "Schools with low enrolment of students to be merged into one." *New Straits Times* (Kuala Lumpur, Malaysia) (July 31).

Howell, Anthony. 2017. "Impacts of Migration and Remittances on Ethnic Income Inequality in Rural China." *World Development* 94 (C): 200–211.

Howley, Craig Johnson. 2011. *Consolidation of Schools and Districts: What the Research Says and What It Means.* Boulder, CO: National Education Policy Center, February.

Hui, Ma. 2009. "School Closure and Consolidation does not Match Local Needs in Western China." *21st Century Business Herald* (Beijing, China) (June 30).

Huisman, Janine, and Jeroen Smits. 2009. "Effects of Household- and District-Level Factors on Primary School Enrollment in 30 Developing Countries." *World Development* 37, no. 1 (January): 179–193.

Jacoby, Hanan G., and Ghazala Mansuri. 2015. "Crossing boundaries: How social hierarchy impedes economic mobility." *Journal of Economic Behavior & Organization* 117 (C): 135–154.

Kazianga, Harounan, Dan Levy, Leigh L. Linden, and Matt Sloan. 2013. "The Effects of "Girl-Friendly" Schools: Evidence from the BRIGHT School Construction Program in Burkina Faso." *American Economic Journal: Applied Economics* 5, no. 3 (July): 41–62.

Kearns, Robin A., Nicolas Lewis, Tim McCreanor, and Karen Witten. 2009. "'The status quo is not an option': Community impacts of school closure in South Taranaki, New Zealand." *Journal of Rural Studies* 25, no. 1 (January): 131–140.

Kirshner, Ben, Matthew Gaertner, and Kristen Pozzoboni. 2010. "Tracing Transitions: The Effect of High School Closure on Displaced Students." *Educational Evaluation and Policy Analysis* 32, no. 3 (September 1): 407–429.

Kremer, Michael, Conner Brannen, and Rachel Glennerster. 2013. "The Challenge of Education and Learning in the Developing World." *Science* 340, no. 6130 (April 19): 297–300.

Ladd, Helen F., and Sheila E. Murray. 2001. "Intergenerational conflict reconsidered: County demographic structure and the demand for public education." *Economics of Education Review* 20 (4): 343–357.

Lee, Jin, and Christopher Lubienski. 2016. "The Impact of School Closures on Equity of Access in Chicago." *Education and Urban Society* 49, no. 1 (February 23): 53–80.

Liu, Chengfang, Linxiu Zhang, Renfu Luo, Scott Rozelle, and Prashant Loyalka. 2010. "The effect of primary school mergers on academic performance of students in rural China." *International Journal of Educational Development* 30, no. 6 (November): 570–585.

Liu, Haiming, Naren Gaowa, and Shu Wang. 2013. "Closures and Consolidation: The Effective Path to Improving the Overall Quality of Basic Education." *Chinese Education & Society* 46, no. 5 (September 1): 36–55.

Liu, Jing, and Chunbing Xing. 2016. "Migrate for education: An unintended effect of school district combination in rural China." *China Economic Review* 40:192–206.

Liu, Xiaoying, and Emily Hannum. 2017. "Early poverty exposure predicts young adult educational outcomes in China." *China Economic Review* 44:79–97.

Ma, Xiang. 2017. *Economies of Scale and Heterogeneity in the Provision of Public Goods: Evidence from School Consolidation in China.* Jinan University, Department of Economics, March 15.

Malik, Mansoor. 2013. "Apprehensions about primary schools merger policy." *DAWN* (Karachi, Pakistan) (February 4).

Mei, Hong, Quanbao Jiang, Yuanyuan Xiang, and Xiaoping Song. 2015. "School Consolidation: Whither China's Rural Education?" *Asian Social Work and Policy Review* (March): 138150.

Ministry of Education. 1986. "Compulsory Education Law of the People's Republic of China (Adopted at the Fourth Session of the Sixth National People's Congress April 12, 1986 and promulgated by Order No. 38 of the President of the People's Republic of China on April 12, 1986)." (Beijing, China).

———. 1998–2015. "China Education Statistics Year Book." http://en.moe.gov.cn/Resources/Statistics/.

Mo, Di, Hongmei Yi, Linxiu Zhang, Yaojiang Shi, Scott Rozelle, and Alexis Medina. 2012. "Transfer paths and academic performance: The primary school merger program in china." *International Journal of Educational Development* 32, no. 3 (May): 423–431.

Muralidharan, Karthik, and Nishith Prakash. 2017. "Cycling to School: Increasing Secondary School Enrollment for Girls in India." *American Economic Journal: Applied Economics* 9, no. 3 (July): 321–50.

Ohtake, Fumio, and Shinpei Sano. 2010. "The Effects of Demographic Change on Public Education in Japan." In *The Economic Consequences of Demographic Change in East Asia, NBER-EASE Volume 19,* 193–219. University of Chicago Press.

Ortellado, Pablo. 2015. "Brazil's Students Occupy Their Schools to Save Them." *The New York Times* (New York, NY) (December 15).

Poterba, James. 1998. "Demographic change, intergenerational linkages, and public education." *The American Economic Review* 88 (2): 315.

Sacerdote, Bruce. 2012. "When the Saints Go Marching Out: Long-Term Outcomes for Student Evacuees from Hurricanes Katrina and Rita." *American Economic Journal: Applied Economics* 4, no. 1 (January): 109–135.

Saengpassa, Chularat. 2017. "Ministry's plan to merge schools criticised: Ministry predicts improved quality of education, but NGO fears policy failure." *The Nation* (Bangkok, Thailand) (January 2).

Schwartz, Amy Ellen, Leanna Stiefel, and Matthew Wiswall. 2013. "Do small schools improve performance in large, urban districts? Causal evidence from New York City." *Journal of Urban Economics* 77 (September): 27–40.

Setiawati, Indah. 2010. "Schools merger to boost efficiency." *The Jakarta Post* (Jakarta, Indonesia) (March 1).

Slee, Bill, and Dave Miller. 2015. "School Closures as a Driver of Rural Decline in Scotland: A Problem in Pursuit of Some Evidence?" *Scottish Geographical Journal* 131, no. 2 (April 3): 78–97.

Tawie, Sulok. 2017. "Sarawak to start merging low enrolment schools next year." *Malay Mail* (Kuala Lumpur, Malaysia) (September 6).

United Nations Population Division. 2017. "World Population Prospects." https://esa.un.org/unpd/wpp/.

United States Bureau of the Census. 2017. "International Data Base." https://www.census.gov/programs-surveys/international-programs/data/tools.html.

United States Department of Education. 2016a. "Chapter 2: Elementary and Secondary Education." In *Digest of Education Statistics, 2015.* Washington, D.C.: National Center for Education Statistics.

———. 2016b. "Closed Schools." National Center for Education Statistics. https://nces.ed.gov/fastfacts/display.asp?id=619.

Wu, Yong. 2014. "Tingting Shang Xue Neng Bu Neng Jin Yi Dian [Can Tingting go to a closer school–An investigation from Inner Mongolia]." *People's Daily* (Beijing, China) (June 25): 23.

Xie, Ailei, and Zhihui Wu. 2013. "Consolidating Rural Schools in China: Policy, Issues, and Debates: Guest Editors' Introduction." *Chinese Education & Society* 46, no. 5 (September 1): 3–8.

Yang, Dong-ping, and Shuai Wang. 2013. "From Extending Teaching to Remote Areas and Running School in Various Forms to Canceling Teaching Centers and Merging Schools – A trade-off between equity and efficiency of the policy on rural compulsory education." *Tsinghua Journal of Education* 34, no. 5 (October): 25–34.

Yu, Lei, and Shuai Jiang. 2010. "An American Chinese Teacher Investigates Chinese School Consolidation Program." *China Youth Daily* (Beijing, China) (February 22).

Zhang, Junsen, Yaohui Zhao, Albert Park, and Xiaoqing Song. 2005. "Economic returns to schooling in urban China, 1988 to 2001." *Journal of Comparative Economics,* Symposium: Poverty and Labor Markets in China, 33, no. 4 (December 1): 730–752.

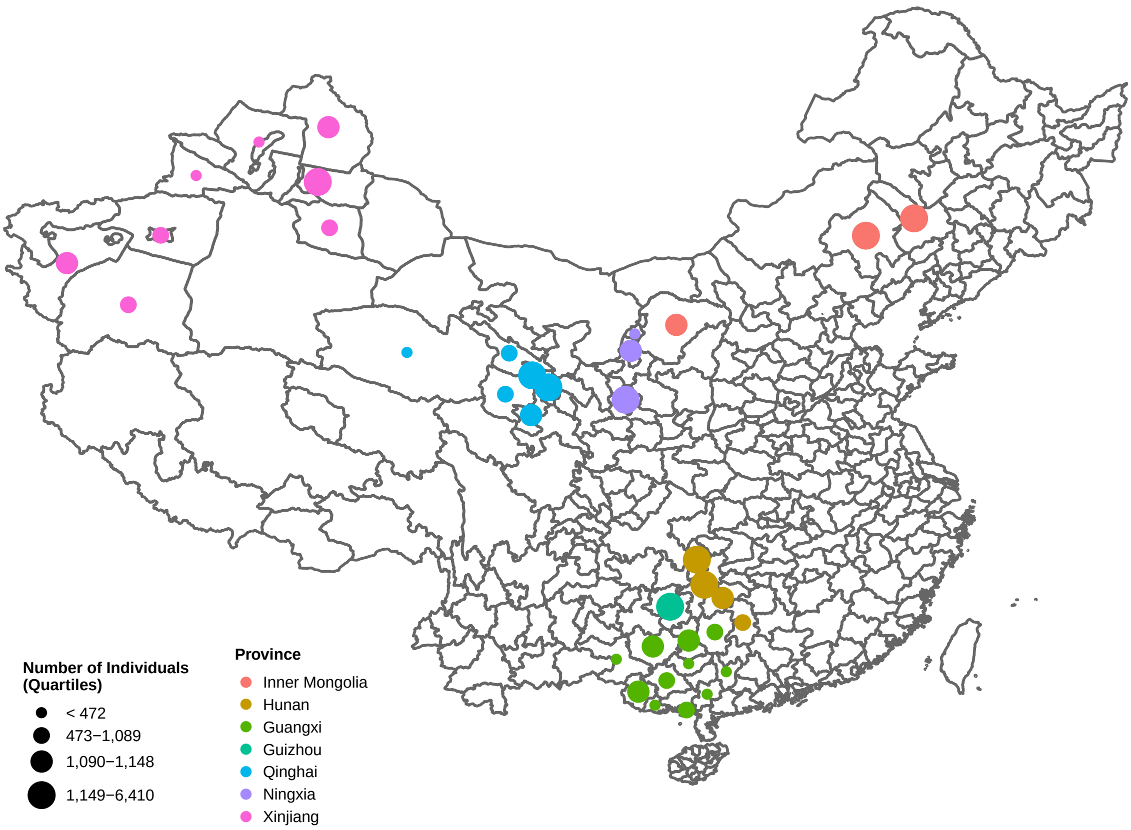
Zhang, Shuang. 2018. "The Effects of High School Closure on Education and Labor Market Outcomes in Rural China." *Economic Development and Cultural Change* forthcoming.

Zhao, Dan, and Bilal Barakat. 2015. "The increasingly long road to school in rural China: the impacts of education network consolidation on broadly defined schooling distance in Xinfeng County of rural China." *Asia Pacific Education Review* 16, no. 3 (September): 413–431.

A Appendices (For Online Publication)

A.1 Additional Data Details

Figure 2: CHES Survey Prefectures Containing Survey Counties (Howell 2017)



A.1.1 Location of closures

This paper utilizes data from the rural sample of the China Household Ethnic Survey (CHES 2011), which covers households and villages from 728 villages[44] in 81 counties of 7 provinces with substantial minority populations in western China: Qinghai Province (119 villages surveyed); Ningxia Hui Autonomous Region (97 villages surveyed); Xinjiang Uygur Autonomous Region (94 villages surveyed); Inner Mongolia Autonomous Region (100 villages surveyed); Qiandongnan Miao and Dong Autonomous Prefecture in Guizhou Province (120 villages surveyed); Hunan Province (101 villages surveyed); and Guangxi Zhuang Autonomous Region (103 villages surveyed). Figure 2 presents a map of CHES Survey Prefectures (Howell 2017).

44. There are 751 unique village IDs in the survey, but 17 villages do not have school closure information, and 6 villages report closure without a closure year.

A.1.2 Additional summary statistics

In Panels A, B, C and D of Appendix Table 7, we test how villages with and without school closure differ along several other dimensions. We discuss other summary statistics variables in Table 1.

Table 7: Additional Summary Statistics for Village Characteristics

	Villages with and without school closures				
	all	**group averages**		**p-values testing**	
	mean	*non-closure*	*closure*	*closure vs non-closure†*	*years of closure trend‡*
Panel A: Leadership					
Village has contact at above county level	0.62	0.63	0.58	0.40	0.86
There are villagers who became officials at county level	0.56	0.61	0.45	0.00	0.06
Panel B: Income, Wage, Migration					
Per capita annual net income (Yuan) 2011	4445.89	4421.79	4503.87	0.97	0.16
Local temp work wage (Yuan) 2011	83.08	82.19	85.24	0.32	0.34
Fraction of village labor force in agriculture	64.16	63.71	65.28	0.17	0.39
Fraction of migrant worker in total population	0.24	0.23	0.26	0.13	0.40
Fraction of male migrant worker in total population	0.15	0.15	0.16	0.20	0.14
Panel C: Village Expenditures					
Per capita village budget spending (Yuan) 2011	69.03	63.13	83.95	0.20	0.47
Per capita spending on education (Yuan) 2011	1.26	1.62	0.38	0.40	0.38
If village has education spending in 2011	0.17	0.18	0.15	0.21	0.39
Panel D: Other Policies					
Village implemented Grain for Green	0.69	0.66	0.74	0.35	0.54
Village has been consolidated since 1999	0.14	0.14	0.14	0.19	0.22
Village has collective-owned medical station	0.65	0.66	0.64	0.64	0.50
Village started rural medical insurance scheme after 2006	0.50	0.52	0.45	0.08	0.99

Village Level Summary Statistics. † Column 4 shows results from testing if the mean gaps between closure and non-closure villages are statistically different: p-value results in column 4 controls for provincial fixed effects. ‡ Column 5 tests if values for each variable differs over the calendar year of closure for villages experiencing school closure.

Panel A shows that non-closure villages are more likely to have someone originally from the village working at the county or higher level of government. The first variable shows if the village has any contacts in governments above the county level, and there is no statistical differences between closure and non-closure villages for this measure controlling for provincial fixed effects. For the second variable, however, we find that 45 percent of villages with closure have someone from the village at county or above county level governments, but 61 percent of the villages without closure do. The difference is significant.

In Panel B, we do not find any statistically significant differences between closure and non-closure villages for income and labor market variables. Villages that have closure have 2 percent higher average net annual income (4503 Yuan vs 4421 Yuan in

2011), and 4 percent higher average daily wage than villages without closure (85.24 Yuan vs 82.19 Yuan).[45] These differences are not statistically different. Additionally, villages with closure have 26 percent of the population working as migrant workers, compared to 23 percent in villages without closure. Males migrant workers account for 16 and 15 percent of the population in closure and non-closure villages respectively. The vast majority of those not working as migrant workers are agricultural workers/farmers, accounting for 65.3 percent and 63.7 percent of the labor force in villages with and without closure respectively.

In Panel D, we check if there is a relationship between the implementation of the closure policy and three other village-level policies. The fraction of villages with collectively-owned medical stations among closure and non-closure villages is 64 percent and 66 percent. 74 percent of villages with closure implemented the Grain for Green (Grain for Green) policy,[46] while 66 percent of villages without closure had. 45 percent of villages with closure had implemented cooperative medical insurance after 2006[47], and 52 percent of villages without closure had. Controlling for provincial fixed effects, the differences between these variables in closure and non-closure villages are not significant. Overall, in our sample of villages, it seems that the closure decision is unrelated to the level of village economic development and other socio-economic policies, but might be partly driven by the size of villages (or school size) as discussed in Section 2 of the paper.

45. 1 Dollar = 6.5 Yuan in 2011.

46. This was a policy to convert cultivated land back into forest, as these lands were converted from forest to cultivated land before, which led to decrease in forest coverage, and caused flood, and soil erosion.

47. The cooperative medical insurance policy started in 2004. By the end of 2006, 51 percent of villages in our sample had this insurance program. 99 percent of our sample villages had the program by the end of 2009.

A.1.3 Grades completed by 2011 for males and females

Table 2 shows average grades completed by 2011 for all individuals. Appendix Table 8 presents information on grades completed by 2011 for males and females separately.

Table 8: Summary Statistics for Educational Attainment

	Age at village-specific year of closure and 2011 age							
	Age in 2011		*Age in 2011*		*Age in 2011*		*Age in 2011*	
	0-4	**5-9**	**10-14**	**15-19**	**20-24**	**25-29**	**30-34**	**35-44**
Group A: age 1 to 5 at year of closure								
Number of grades completed: *Female*	0.083	1.00	5.37	9.64				
Number of grades completed: *Male*	0.082	1.20	5	9.80				
Fraction completed middle school: *Female*	0	0	0.032	0.91				
Fraction completed middle school: *Male*	0.0058	0	0.031	0.80				
Observations	303	333	126	16				
Group B: age 6 to 9 at year of closure								
Number of grades completed: *Female*		2.24	5.58	10	8.80			
Number of grades completed: *Male*		1.66	5.43	9.60	11.5			
Fraction completed middle school: *Female*		0	0.018	0.74	0.80			
Fraction completed middle school: *Male*		0	0.010	0.69	0.83			
Observations		98	211	69	17			
Group C: age 10 to 13 at year of closure								
Number of grades completed: *Female*			5.85	9.27	10.4	8		
Number of grades completed: *Male*			6.09	9.49	10.3	9		
Fraction completed middle school: *Female*			0.081	0.68	0.80	0		
Fraction completed middle school: *Male*			0.042	0.71	0.84	1		
Observations			133	224	117	2		
Group D: age 14 to 21 at year of closure								
Number of grades completed: *Female*			7.33	9.90	10.1	8.97	7.69	
Number of grades completed: *Male*			6.57	9.71	10.4	9.11	9.21	
Fraction completed middle school: *Female*			0.22	0.80	0.78	0.66	0.46	
Fraction completed middle school: *Male*			0.14	0.75	0.82	0.71	0.68	
Observations			16	275	590	241	32	
Group E: age 22 to 29 at year of closure								
Number of grades completed: *Female*					9.75	8.41	7.81	7.82
Number of grades completed: *Male*					10.2	9.11	8.41	8.13
Fraction completed middle school: *Female*					0.75	0.59	0.48	0.42
Fraction completed middle school: *Male*					0.84	0.68	0.61	0.51
Observations					101	460	322	98
Group F: individuals from non-closure villages								
Number of grades completed: *Female*	0.11	1.47	5.58	9.31	9.48	8.01	6.64	5.88
Number of grades completed: *Male*	0.070	1.28	5.52	9.35	9.92	8.80	7.98	7.71
Fraction completed middle school: *Female*	0.0057	0	0.048	0.71	0.71	0.52	0.41	0.27
Fraction completed middle school: *Male*	0.0047	0	0.040	0.73	0.75	0.63	0.54	0.47
Observations	783	1227	1521	1774	2237	1713	1420	1569

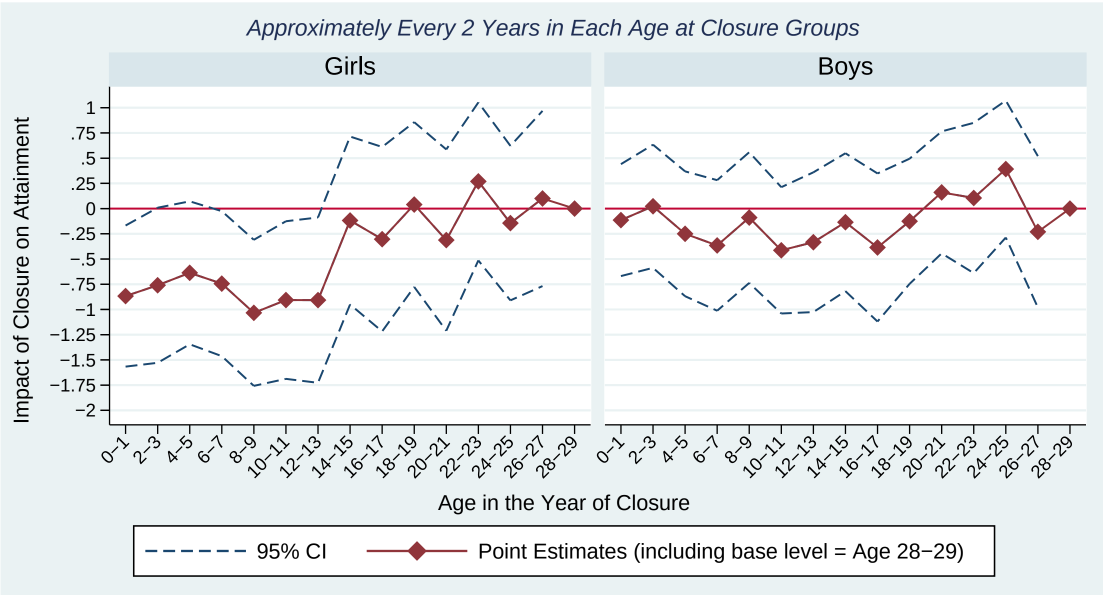
Table shows means. Individuals in Group A were fully exposed to consolidated primary schools. Group B individuals were 6 to 9 at year of closure and were exposed to consolidated primary school for more than half of their primary school years. Group C individuals were 10 to 13 at year of closure and transitioned from village schools to consolidated primary schools during the final years of primary school. Group D and E individuals are from villages that experienced closure, but were beyond primary school age at year of closure. Group F individuals are from villages without school closure.

A.2 Additional Attainment Regression Results

A.2.1 Age effects with finer age-at-closure breakdowns

Appendix Figure 3 shows the same style of graph as Figure 1 except now with even finer age breakdowns, grouping every 2 closure-year ages together on the x-axis. There is more instability in the coefficients due to the smaller sample size for each group, but we see the same pattern as in Figure 1. For girls, there is a significant drop in the trend line at closure-age 12 to 13, coefficients are insignificant from 0 above this closure-age, but significantly negative below this closure-age group. For boys, there is a flat trend along the x-axis.

Figure 3: Effect of School Closure on Educational Attainment (Number of Grades Completed by 2011) by 15 Age-at-Closure Group.



Each *a*–*b* group shows impact of closure on grades completed by 2011 for children who were between *a* and *b* years of age at the time of school closure. These results correspond to results from column 1 of Table 3 which had 5 age-at-closure groups (see Section 4.1), but use finer age-at-closure groupings. All coefficients are from estimating Equation 1.

A.2.2 Attainment regression for girls in younger cohorts

The panels of Appendix Table 9 show results for regression samples that only include women below 35, 30 and 25 years of age in 2011. We also exclude individuals who were 0 to 5 or 22 to 30 years-of-age at the time of school closure from villages with closure. Results are similar across panels.

Table 9: Effect of School Closure on Educational Attainment for Restricted Age Cohorts

	Outcome: grades completed by year 2011					
			$10 \leq 2011$ Age $\leq X$		$15 \leq 2011$ Age $\leq X$	
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline group: Child was 14-21 years old at village primary school closure year						
Panel Female Below 35 in 2011						
Age at closure 6-9	-0.54**	-0.62**	-0.54**	-0.65**		
	(0.25)	(0.27)	(0.28)	(0.30)		
Age at closure 10-13	-0.55**	-0.61**	-0.54**	-0.60**	-0.54*	-0.59*
	(0.24)	(0.27)	(0.25)	(0.28)	(0.30)	(0.33)
Observations	6485	5472	5316	4492	4310	3648
Panel Female Below 30 in 2011						
Age at closure 6-9	-0.59**	-0.66**	-0.58**	-0.66**		
	(0.25)	(0.28)	(0.28)	(0.31)		
Age at closure 10-13	-0.59**	-0.62**	-0.58**	-0.62**	-0.63**	-0.62*
	(0.24)	(0.27)	(0.25)	(0.28)	(0.30)	(0.33)
Observations	5769	4879	4758	4029	3752	3185
Panel Female Below 25 in 2011						
Age at closure 6-9	-0.59**	-0.73**	-0.58*	-0.74**		
	(0.28)	(0.31)	(0.32)	(0.35)		
Age at closure 10-13	-0.51**	-0.63**	-0.50*	-0.62**	-0.49	-0.59
	(0.26)	(0.29)	(0.27)	(0.30)	(0.34)	(0.36)
Observations	4900	4138	3889	3288	2883	2444
Exclusions and controls:						
Village FE, province-age FE and controls	Yes	Yes	Yes	Yes	Yes	Yes
Exclude villages that never had schools		Yes		Yes		Yes

†Includes village fixed effects, province-specific age FEs, controls for ethnicity, household size and relative household wealth.
‡Odd columns check robustness by dropping category 2 closure and category 4 non-closure status villages discussed in Section 2.
Statistical significance:* 0.10 ** 0.05 *** 0.01. Robust standard error clustered at village level. Each column (in each panel) represents a separate regression. Compare children with different starting age of exposure (and length of exposure) to children in baseline group who should not be impacted by school closure. Columns 3 to 6 restrict regressions to subsets of individuals who are closer together in 2011 age, testing the robustness of results to excluding younger and older individuals (see Table 2). Regression sample includes villagers who were below 21 at year of closure for villages that experienced closure between 1999 and 2010. Individuals included in regressions from all columns are below 35, 30 and 25 years of age in 2011 in each of the three Panels.

A.2.3 Richer and poorer villages

Table 3 shows results for all females from estimating Equation 1. With an average per capita income of 4446 Yuan in 2011, the CHES survey villages are all poor by Chinese standards. The panels of Appendix Table 10 show results for females in villages with below and above 4000 yuan (1 Dollar = 6.5 Yuan in 2011) per-capita income in 2011. Standard errors are larger when we divide females into separate village groups. The two panels of Appendix Table 10 both show negative effects of closure on grades completed by 2011. The negative effects are generally weaker for females from relatively richer villages of the CHES survey.

Table 10: Effect of School Closure on Educational Attainment by Village Income

	Outcome: grades completed by year 2011					
			$10 \leq 2011$ Age ≤ 34		$15 \leq 2011$ Age ≤ 34	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel Female in Villages with Per Capita Income Below 4000 Yuan						
Age at closure 0-5	-0.42	-0.60*				
	(0.30)	(0.33)				
Age at closure 6-9	-0.72**	-0.74**	-0.83**	-0.90**		
	(0.29)	(0.33)	(0.38)	(0.42)		
Age at closure 10-13	-0.60*	-0.61*	-0.75**	-0.79**	-0.70*	-0.75*
	(0.31)	(0.35)	(0.33)	(0.38)	(0.42)	(0.44)
Age at closure 22-29	0.13	0.26	0.015	-0.0002	-0.018	-0.021
	(0.31)	(0.29)	(0.34)	(0.32)	(0.34)	(0.33)
Observations	4495	4066	2910	2620	2403	2164
Panel Female in Villages with Per Capita Income Above 4000 Yuan						
Age at closure 0-5	-0.33	-0.48				
	(0.35)	(0.39)				
Age at closure 6-9	-0.35	-0.67*	-0.44	-0.84*		
	(0.36)	(0.40)	(0.39)	(0.43)		
Age at closure 10-13	-0.56	-0.76**	-0.45	-0.67*	-0.47	-0.60
	(0.34)	(0.38)	(0.35)	(0.39)	(0.39)	(0.44)
Age at closure 22-29	0.29	0.25	0.17	0.26	0.27	0.25
	(0.32)	(0.35)	(0.36)	(0.39)	(0.39)	(0.42)
Observations	4374	3400	2754	2170	2255	1782
Exclusions and controls:						
Village FE, province-age FE and controls†	Yes	Yes	Yes	Yes	Yes	Yes
Exclude villages that never had schools‡		Yes		Yes		Yes

†Includes village fixed effects, province-specific age FEs, controls for ethnicity, household size and relative household wealth.
‡Odd columns check robustness by dropping category 2 closure and category 4 non-closure status villages discussed in Section 2.
Statistical significance:* 0.10 ** 0.05 *** 0.01. Robust standard error clustered at village level. Each column (in each panel) represents a separate regression. Compare children with different starting age of exposure (and length of exposure) to children in baseline group who should not be impacted by school closure. Columns 3 to 6 restrict regressions to subsets of individuals who are closer together in 2011 age, testing the robustness of results to excluding younger and older individuals (see Table 2). Regression sample includes villagers who were below 29 at year of closure for villages that experienced closure between 1999 and 2010. Individuals included in regressions from all columns are below 45 years of age in year 2011.

A.2.4 Han and non-Han majority villages

Table 3 shows results for all females from estimating Equation 1. The panels of Appendix Table 11 show results for females in villages where Han individuals are the minority (top panel) and where Han individuals are the majority (bottom panel). Standard errors are larger when we divide females into separate village groups. The two panels of Appendix Table 11 generally show similar results. In our dataset, there are more villages where Han individuals are in the minority, giving us slightly tighter standard errors for the estimates in the top panel of Appendix Table 11.

Table 11: Effect of Closure on Educational Attainment by Ethnicity

	Outcome: grades completed by year 2011					
			$10 \leq 2011$ Age ≤ 34		$15 \leq 2011$ Age ≤ 34	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel Female Villages Han is Minority						
Age at closure 0-5	-0.013	-0.14				
	(0.29)	(0.34)				
Age at closure 6-9	-0.40	-0.45	-0.47	-0.61		
	(0.30)	(0.35)	(0.37)	(0.43)		
Age at closure 10-13	-0.56*	-0.60*	-0.60*	-0.64*	-0.63*	-0.70*
	(0.30)	(0.35)	(0.33)	(0.38)	(0.36)	(0.41)
Age at closure 22-29	0.16	0.28	0.15	0.19	0.15	0.21
	(0.30)	(0.30)	(0.33)	(0.32)	(0.34)	(0.33)
Observations	5677	4758	3631	3032	2984	2490
Panel Female Villages Han is Majority						
Age at closure 0-5	-0.37	-0.37				
	(0.32)	(0.36)				
Age at closure 6-9	-0.26	-0.38	-0.36	-0.52		
	(0.31)	(0.34)	(0.35)	(0.40)		
Age at closure 10-13	-0.51	-0.49	-0.45	-0.48	-0.58	-0.54
	(0.34)	(0.38)	(0.36)	(0.41)	(0.45)	(0.48)
Age at closure 22-29	0.17	0.25	-0.11	-0.031	-0.098	-0.13
	(0.34)	(0.34)	(0.40)	(0.40)	(0.42)	(0.43)
Observations	3192	2708	2033	1758	1674	1456
Exclusions and controls:						
Village FE, province-age FE and controls†	Yes	Yes	Yes	Yes	Yes	Yes
Exclude villages that never had schools‡		Yes		Yes		Yes

†Includes village fixed effects, province-specific age FEs, controls for ethnicity, household size and relative household wealth.
‡Odd columns check robustness by dropping category 2 closure and category 4 non-closure status villages discussed in Section 2.
Statistical significance:* 0.10 ** 0.05 *** 0.01. Robust standard error clustered at village level. Each column (in each panel) represents a separate regression. Compare children with different starting age of exposure (and length of exposure) to children in baseline group who should not be impacted by school closure. Columns 3 to 6 restrict regressions to subsets of individuals who are closer together in 2011 age, testing the robustness of results to excluding younger and older individuals (see Table 2). Regression sample includes villagers who were below 29 at year of closure for villages that experienced closure between 1999 and 2010. Individuals included in regressions from all columns are below 45 years of age in year 2011.

A.3 Additional Enrollment Regression Results

In Appendix Table 12, we expand upon the results from Table 6. We find that for 5 to 8 year old girls, the presence of medium and long distance schools are associated with larger reductions in enrollment than for boys. We also find that for 5 to 8 year old boys, better facility schools are associated with increases in enrollment. For children between 9 and 12, there are no enrollment differences for girls when schools are less than 3 kilometers away, but more distant schools are associated with a reduction in girls' enrollment. For this age group, schools with better facilities are associated with small increases in enrollment for both boys and girls, but not significantly.

Table 12: Linear Probability Model of School Enrollment by Age Subgroups

	Outcome: enrolled in school or not in 2011					
	All Age 5 to 12		Girls Age 5 to 12		Boys Age 5 to 12	
	all villages	no teaching points	all villages	no teaching points	all villages	no teaching points
Dummy for age 5 to 8 interactions						
Categorical Distance (Compare to 0 km)						
x $0 < x \leq 3$ $(median \approx 2)$ km	-0.048*	-0.061**	-0.055	-0.095**	-0.070*	-0.065*
	(0.027)	(0.029)	(0.039)	(0.045)	(0.036)	(0.039)
x $3 < x \leq \max$ $(median \approx 7)$ km	-0.076**	-0.094**	-0.084*	-0.15**	-0.075	-0.046
	(0.034)	(0.042)	(0.045)	(0.058)	(0.046)	(0.060)
Categorical Quality (Compare to 0 to 3)						
x 4 to 6 Facilities	0.027	0.014	-0.035	-0.059	0.085**	0.073*
	(0.031)	(0.032)	(0.042)	(0.047)	(0.040)	(0.043)
x 7 to 9 Facilities	0.033	0.013	-0.027	-0.029	0.098**	0.066
	(0.038)	(0.039)	(0.053)	(0.059)	(0.047)	(0.049)
Dummy for age 9 to 12 interactions						
Categorical Distance (Compare to 0 km)						
x $0 < x \leq 3$ $(median \approx 2)$ km	0.0035	0.014	-0.012	-0.0011	0.013	0.028
	(0.018)	(0.019)	(0.027)	(0.032)	(0.024)	(0.025)
x $3 < x \leq \max$ $(median \approx 7)$ km	-0.049**	-0.066**	-0.082**	-0.13**	-0.026	-0.025
	(0.024)	(0.034)	(0.037)	(0.050)	(0.030)	(0.044)
Categorical Quality (Compare to 0 to 3)						
x 4 to 6 Facilities	0.013	0.013	0.016	0.034	0.0093	-0.0045
	(0.026)	(0.030)	(0.030)	(0.035)	(0.036)	(0.043)
x 7 to 9 Facilities	0.037	0.032	0.030	0.045	0.037	0.017
	(0.025)	(0.028)	(0.030)	(0.037)	(0.036)	(0.043)
Observations	2460	2033	1130	942	1330	1091

Statistical significance:* 0.10 ** 0.05 *** 0.01. Standard Errors clustered at village level. Each column is a separate regression. Distance to closest primary school and school facility information are from village head survey. School facilities include if the school has pipe water, library, computers, non-dilapidated buildings and others shown in Table 1. All regressions include county fixed effects, province-specific age fixed effects, controls for village per capita income, village per capita land, village population size, household relative wealth, the number of household members and household ethnicity.